

ABSTRACT

Title of Dissertation: Essays in the Economics of Immigration

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Chapter 1 sets the stage for Chapters 2 and 3, which involves the empirical analyses of the effects of two prominent immigration policies: Deferred Action for Childhood Arrivals (DACA) and the Immigration Reform and Control Act (IRCA). This chapter begins with a review of the history of modern US immigration policy and relevant empirical evidence regarding it. It then focuses on three special topics: immigration and labor markets, immigration and crime, and the effects of enforcement policy. These topics are chosen for their contextual relevance for DACA and IRCA, as well as for marriage.

Chapter 2 examines the impact of Deferred Action on Childhood Arrivals (DACA) on the marriage outcomes of its recipients. DACA, an immigration policy introduced by President Barack Obama in 2012, provides temporary benefits to unauthorized immigrants who arrived in the US as children. By analyzing data from the American Community Survey (ACS), the study examines the effects of DACA eligibility on the probability of being married and the types of individuals

DACA recipients marry. The findings suggest that DACA eligibility increased the likelihood of marriage by approximately 2 percentage points, with deportation relief being a key driver for women and work authorization playing a more prominent role for men. The analysis also reveals that DACA recipients are more inclined to marry US natives, emphasizing the desire for assimilation, and tend to choose spouses who are fluent in English, indicating the influence of DACA on language-related assimilation.

Chapter 3 investigates the impact of the legalization provision of the Immigration Reform and Control Act of 1986 (IRCA) on marriage rates. The IRCA offered a pathway to citizenship for unauthorized immigrants. Using data on unauthorized immigrants that were legalized under the IRCA from the Legalized Population Survey (LPS) and a comparison group of US natives from the National Longitudinal Survey of Youth (NLSY79), the study implements an individual fixed effects strategy to estimate the changes in marriage rates as a result of the IRCA legalization. The findings reveal a statistically and economically significant increase in marriage rates for both men and women following IRCA legalization. Men experienced a 6.51 percentage point increase, while women saw an 8.29 percentage point increase. Unlike the effects observed in Chapter 2 for DACA, the permanent nature of the IRCA contributed to a stronger impact on marriage rates. The study explores potential mechanisms but finds inconclusive evidence regarding labor market outcomes and education as drivers of the marriage effect resulting from immigration liberalization.

ESSAYS ON THE ECONOMICS OF IMMIGRATION

by

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Preface

Family is a foundation of the US immigration system. A common way through which a family is formed is marriage. Marriage is not only a fundamental life investment, it also has a substantial effect on the well-being of children born within it (Kearney and Levine, 2018). The marriage rate in a community is likely to affect the opportunities that the broader community offers (Chetty and Hendren, 2018). Despite a secular decline, marriage rates in the US remain high by international standards. In 2016, marriage rates in the US were only surpassed by Turkey and Lithuania among members of the OECD (OECD, 2018). There is a strong cultural emphasis on marriage and family in the US. Because of that emphasis, and the significance of family and marriage behaviors in immigration policy, one cannot adequately understand US immigrant populations without understanding those behaviors.

The cultural emphasis on family and population characteristics of the US are reflected in the evolution of the nation's immigration policy. In the Civil Rights era, when discrimination by race and its manifestation in national origin was being combated by policy-makers, the Immigration and Nationality Act of 1965 was passed. The act replaced a system of nation-based quotas with one that provides a strong preference for family members of US citizens and permanent residents. In 2017, 66% of green card recipients were sponsored by family members (Department

of Homeland Security, 2018). For decades, there has been extensive debate in the US about policies that would provide unauthorized immigrants with some combination of deportation relief, work authorization, and paths to lawful permanent residence or citizenship. Given the sizeable population of unauthorized immigrants in the United States and its family-based immigration system, any policy that affects the legal rights, and by extension marriage prospects, of unauthorized immigrants is expected to have broad implications for the future composition of American society.

In this dissertation, I present three essays on the effect of immigration policy on marriage – with a particular focus on Deferred Action for Childhood Arrivals (DACA) and the Immigration Reform and Control Act (IRCA), which expanded the legal rights of unauthorized immigrants.

Chapter 1 sets the stage for Chapters 2 and 3, which involves the empirical analyses of the effects of two prominent immigration policies: Deferred Action for Childhood Arrivals (DACA) and the Immigration Reform and Control Act (IRCA). This chapter begins with a review of the history of modern US immigration policy and relevant empirical evidence regarding it. It then focuses on three special topics: immigration and labor markets, immigration and crime, and the effects of enforcement policy. These topics are chosen for their contextual relevance for DACA and IRCA, as well as for marriage.

Chapter 2 examines the impact of Deferred Action on Childhood Arrivals (DACA), an immigration policy implemented by President Barack Obama in 2012, on the marriage outcomes of its recipients. DACA provides temporary benefits, including deportation relief and work authorization, to unauthorized immigrants

who arrived in the US as children. I explore how DACA eligibility affects the probability of being married and the types of individuals DACA recipients marry, considering factors such as gender, relative wages, social norms, educational pursuits, and fertility timing.

To measure the effects of DACA, I use data from the American Community Survey (ACS) to identify eligible and ineligible Hispanic individuals. A difference-in-differences strategy is employed, comparing outcomes before and after the implementation of DACA, to estimate the intent-to-treat effects on marriage probability and the likelihood of marrying individuals with different immigration statuses. However, the available data in the ACS only indicate whether an individual is a citizen or not, leading to contamination of the treatment group with authorized immigrants. This contamination likely attenuates the estimated effects.

The findings suggest that DACA eligibility increased the probability of being married by approximately 2 percentage points compared to a base rate of 31% in the eligible sample. The analysis examines the mechanisms behind these effects, focusing on deportation relief and work authorization provided by DACA. The results suggest that deportation relief is a key driver of the marriage outcomes for women, while the work authorization provision plays a more important role for men. However, increased labor market opportunities resulting from DACA may lead some women to substitute away from marriage.

The chapter also investigates the marital choices of DACA recipients. The findings indicate that DACA pushes individuals towards marriages that are more assimilative rather than solely motivated by legal benefits. DACA-eligible indi-

viduals are more likely to marry US natives and less likely to marry foreign-born citizens. This pattern reflects a preference for assimilation, as marrying a US native offers greater assimilative benefits. Additionally, DACA recipients are more likely to choose spouses who are fluent in English, suggesting the influence of DACA on language-related assimilation.

In Chapter 3, I examine the impact of the legalization provision of the Immigration Reform and Control Act of 1986 (IRCA) on marriage rates. The IRCA offered a pathway to citizenship for unauthorized immigrants and implemented stricter enforcement measures.

The key conceptual difference between the IRCA and the Deferred Action for Childhood Arrivals (DACA) program lies in the permanent nature of the IRCA's legalization. The chapter discusses how this permanence may increase the effects on marriage rates in comparison to the temporary DACA.

This chapter draws on Kossoudji and Cobb-Clark (2002) and Amuedo-Dorantes *et. al.* (2007), who use the same data and strategy as in this chapter, but study the effect of the IRCA on labor markets. The conceptual framework used in Chapter 1, which considers the costs and benefits of marriage for individuals with different immigration statuses, also informs this analysis.

To estimate the effect of IRCA legalization on marriage rates, I use the Legalized Population Surveys (LPS) and the National Longitudinal Survey of Youth (NLSY79). Using an individual fixed effects approach, I compare the treatment group (IRCA legalization applicants) with a comparison group of Hispanic US natives in the same age range. While controlling for time-invariant unobserved indi-

vidual heterogeneity, I examine the changes in marriage rates before and after the IRCA's legalization. Alternative comparison groups are also considered to ensure the robustness of the results.

The findings indicate that the IRCA legalization had a statistically and economically significant impact on increasing marriage rates for both men and women. In the my preferred sample, men experience a 6.51 percentage point increase in marriage rates, while women saw an 8.29 percentage point increase. These effects contrast with the lower-bound effects observed in Chapter 2 for DACA, a temporary policy. The study also explores potential mechanisms by examining labor market outcomes and education but finds imprecise results and no strong evidence to support these factors as important drivers of the marriage effect resulting from immigration liberalization.

This dissertation begins by developing a theoretical framework for examining the effects of immigration policy on marriage. The two subsequent chapters utilize this framework to inform their empirical analysis of arguably the two most significant immigrant policies since 1965: Deferred Action for Childhood Arrivals (Chapter 2) and the Immigration Reform and Control Act (1986). The theoretical framework suggests that both policies have the potential to increase marriage rates by reducing deportation risk, improving labor market prospects, and fostering assimilation.

In both empirical chapters, I show that the two policies robustly increase marriage rates among their recipients. I provide evidence that suggests that the permanent policy (IRCA) has larger marriage effects than the temporary policy (DACA). In Chapter 2, which uses data that allow for me to adequately analyze

the mechanisms, I find that decreased deportation risk and increased labor market prospects are key mechanisms through which the marriage results are driven. I also find that DACA eligibility increases assimilative marriages, consistent with the assimilation mechanism.

Dedication

This dissertation is dedicated to Rachel and Monica. They give each day of my life meaning.

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I would like to express my deepest gratitude and appreciation to the individuals who have played significant roles in my academic journey. Their guidance, support, and inspiration have been invaluable, and I owe a great deal to each and every one of them.

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while also caring for a newborn. They selflessly took care of me, my daughter, and my wife when we needed it the most. I am grateful for their warmth and loyalty, and I am delighted that my wife brought them into my life.

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As the previous paragraph hinted, I conclude by thanking my Lord for the countless gifts he has bestowed upon me – including those used in my economics career. To quote Saint Ignatius of Loyola, “Whatever I have or hold, You have given me; I restore it all to You and surrender it wholly to be governed by Your will.”

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Chapter 1: Modern US Immigration Policy

A History and Key Topics

Abstract

This chapter sets the stage for Chapters 2 and 3, which involves the empirical analyses of the effects of two prominent immigration policies: Deferred Action for Childhood Arrivals (DACA) and the Immigration Reform and Control Act (IRCA). This chapter begins with a review of the history of modern US immigration policy and relevant empirical evidence regarding it. It then focuses on three special topics: immigration and labor markets, immigration and crime, and the effects of enforcement policy. These topics are chosen for their contextual relevance for DACA and IRCA, as well as for marriage.

1.1 Introduction

Immigration policy encompasses a variety of things. These include the criteria for establishing residence and citizenship and determining who is eligible to enter the country. It also includes regulations governing migrants' engagement in the labor market, the political process, and participation in civic institutions. Additionally, immigration policy determines the extent to which migrants can access government services. Further, it includes the enforcement of immigration laws, which includes the detection of violations and the resulting consequences, as well as the methods employed to achieve these objectives (Perez, 2015).

The original empirical contributions of this dissertation are about the effect of immigration policy on marriage. The policies whose marriage effects are investigated in Chapters 2 and 3 are Deferred Action for Childhood Arrivals (DACA) and the Immigration Reform and Control Act (IRCA). They focus on unauthorized immigrants. Unauthorized immigrants are a population whose marriage behaviors are important to study apart from those of other immigrants. They can legally marry, but nevertheless face substantial barriers in marriage markets. Examples of such barriers include the threat of deportation, uncertain future immigration policy, lack of work authorization, the inability to obtain driver's licenses and difficulty in accessing higher education.

These barriers are the result of immigration policy. For decades, there has been extensive debate in the US about policies that would provide unauthorized immigrants with some combination of deportation relief, work authorization, and

a pathway to lawful permanent residence and/or citizenship. Given the sizeable population of unauthorized immigrants in the United States, and the US's family-based immigration system (which I discuss in this chapter), any policy that affects the legal rights, and by extension marriage prospects, of unauthorized immigrants is expected to have broad implications for the future composition of American society.

But unauthorized immigrants and the policies that focus specifically on them cannot be thought of in isolation. They operate in a broader immigration system with many facets and history. The purpose of this chapter is to set the stage for the empirical studies in Chapters 2 and 3. It does so by providing a history of modern US immigration policy, accompanied by a review of the work economists have done to determine the effects those policies that I review.

Section 2 of this chapter reviews the history of US immigration policy since the 1960's, and addresses a selection of relevant studies regarding the policies reviewed in the history. Section 3 focuses on three specific areas of immigration policy and economics that I deem important to understanding the economics of immigration, with unauthorized immigrants in-mind. These "Special Topics" are "Immigration and the Labor Market," "Immigration and Crime," and "The Effects of Immigration Enforcement Policy." They are chosen for their relevance in understanding DACA and IRCA, as well as for immigration and marriage.

1.2 History of Modern US Immigration Policy, 1965-Present

This section provides an overview of US immigration policy from 1965 to the present day. During this period, much of the current policy infrastructure was established. Like any policy, immigration policy reflects broader national and geopolitical trends, a significant theme in this history. Donato and Amuedo-Dorantes (2020) and Watson and Thompson (2021) are key resources used to construct this historical account and deserve special recognition.

Donato and Amuedo-Dorantes (2020) describe federal immigration policy before 1965 (1925-1964) as primarily focused on limiting entry, implemented through legislation and narrow executive actions. The policy regime featured nation-specific quotas favoring immigration from Western Europe and was generally restrictive. From 1965 to 1990, policy shifted towards liberalization, mostly implemented through legislation and narrow executive actions. Actions aligned with US foreign policy, particularly in protecting immigrants from countries aligned with the Soviet Union, such as Cuba, Vietnam, and China. A significant policy during this period was the Immigration Reform and Control Act of 1986 (IRCA), which combined a large-scale amnesty for unauthorized immigrants with increased enforcement measures. This was in response to the large increases in migration from Mexico during this time period, which included a relative shift from cyclical migration around farming seasons to permanent migration (Abramitzky and Boustan, 2017).

Between 1991 and 2005, policy became more restrictive. With some exceptions it was typically implemented through legislation and narrow executive actions.

These policies often focused on enhancing the US government’s enforcement capabilities and limiting immigrants’ access to government benefits. The 1996 Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) is a notable example of the former type, while the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) limited immigrant access to government benefits as part of a broader US social safety net reform. The terrorist attacks of September 11, 2001, played a pivotal role during this era, leading to increased security measures such as the USA Patriot Act of 2001.

Since 2006, policy has typically been more restrictive and implemented through broad executive actions rather than legislation. One such restrictive action was Secure Communities, which increased cooperation between local law enforcement and federal immigration enforcement. An exception to the overall restrictiveness of this era is Deferred Action for Childhood Arrivals (DACA), an executive order issued by the Obama Administration that provided deportation relief and work authorization to unauthorized immigrants who arrived in the US as children.

1.2.1 The Second Great Migration

This history of modern American immigration policy begins with the Immigration and Nationality Act of 1965, which introduced the foundation of the US immigration system today. The act, passed during the Civil Rights movement, eliminated the country-specific quota system that was implemented in the 1920s. This previous system is generally recognized to be governed by racial and ethnic prefer-

ences for Northern and Western Europeans over Southern and Eastern Europeans, Asians, Africans, and Middle-Easterners (Chishti *et al.*, 2015). The country-specific allocation was replaced by a system that implemented preferences for close family members of US citizens and legal permanent residents, as well as for workers with specific skills. There were uniform caps for each country (20,000 annually at the time), but immediate family members such as spouses, parents, and children did not apply to those caps.¹ This act ushered in what has been referred to as the Second Age of Mass Migration (Abramitzky and Boustan, 2017).

Immigration had drastically decreased since the implementation of the quotas during the 1920s, and after the 1965 act, began to drastically rise. The US foreign-born share of the population increased from 5 percent in 1970 to 14 percent in 2010 (Abramitzky and Boustan, 2017). And in this new Age of Mass Migration, the composition of immigrants changed as well. In 1960, 74.5% of the foreign-born population was from Europe, 19.1% was from the Americas, and 5% was from Asia. In 2010, the numbers were 12.1%, 55.1%, and 28.2%, respectively (Batalova and Gelatt). These two trends are visualized well in two figures. One is Figure 1b from Abramitzky and Boustan (2017), which shows the percentage of the US population that was foreign-born between 1850 and 2010. The second is from Batalova and Gelatt, which shows the share of US immigration by region birth between 1960 and 2021. The figure is titled, “Regions of Birth, 1960-2021.” Additionally, immigration from Mexico became subject to the same numerical limits other countries faced. This

¹These country caps initially only applied to the Eastern Hemisphere, but they were applied to Western Hemisphere countries with an amendment in 1976.

came despite Mexico being a neighbor, and increasingly economically integrated with the US. This created a large incentive for unauthorized immigration from the country (Massey, 2015). Legal immigration from Mexico dropped by half shortly after the 1965 Act (Donato and Amuedo-Dorantes, 2020). By the mid-1980s, there were an estimated four million unauthorized immigrants in the United States (Watson and Thompson, 2021), and this eventually increased to over 11 million (27% of the total stock of immigrants) by 2011 (Abramitzky and Boustan, 2017).

1.2.1.1 Key Episode: The Immigration Reform and Control Act of 1986 (IRCA)

The most significant policy response to the rise in unauthorized immigration came in 1986 when President Ronald Reagan signed the Immigration Reform and Control Act (IRCA). This multifaceted law sought to address the rise in unauthorized immigration by offering a pathway to citizenship for unauthorized immigrants already residing in the country, while concurrently strengthening border controls and enforcing penalties against employers who hired unauthorized workers. This represented a compromise between factions that emphasized humanitarian concerns and those that emphasized rule-of-law and border security (Donato *et al.*, 1992).

The key provisions of the law include a legalization program that offered a pathway to citizenship for unauthorized immigrants who had been living in the United States continuously at least since the beginning of 1982. These immigrants were required to pass a background check, pay a fee, and demonstrate knowledge

of the English language and US history to meet eligibility criteria. The IRCA also imposed employer sanctions, making it illegal for employers to knowingly hire unauthorized workers. Employers were required to verify their employees' work eligibility by examining certain documents, such as a Social Security card or a green card, and violators of these provisions were subject to fines and penalties. Additionally, the IRCA allocated funds for increased border enforcement, including hiring additional Border Patrol agents and constructing new border fencing. The law also made it illegal to produce or use false documents such as fake Social Security cards or green cards. Finally, the IRCA established a separate amnesty program for certain farm workers who had worked in the US for at least 90 days during the previous year.

Labor market effects of the IRCA have been extensively studied. The estimated effects of the IRCA on wages range from increases of 6–13 percent from two papers: Rivera-Batiz (1999) and Kossoudji and Cobb-Clark (2002). They both use the Legalized Population Survey (LPS), a survey of unauthorized immigrants legalized by the IRCA that had waves at the time of the 1987 authorization, and a follow-up in 1992. The first paper compares them to a sample of mostly authorized Mexican immigrants in the 1990 Census. The latter improves up on this by comparing the men in the LPS population to a similarly-aged sample of Hispanics in the NLSY79, using a difference-in-differences strategy. Amuedo-Dorantes *et al.* (2007) use a similar research design to Kossoudji and Cobb-Clark (2002) and find that employment rates fell among the newly legalized immigrants because men became more selective about the jobs they were willing to hold, while women exited the

labor force. The authors argue this is potentially related to increased government benefits increasing the outside option to employment. However, Pan (2010) uses the fact that individuals must have arrived by 1982 and implements a regression discontinuity design, finding that the IRCA raised female employment rates. Thus, the employment effects are sensitive to the variation used. Cascio and Lewis (2019) use administrative records from California and find that the IRCA legalization led to increases in income tax filing and uptake of the EITC.

Investigations into the impacts of the IRCA have also been extended to social outcomes. Baker (2015) uses state-by-state variation in IRCA legalizations and finds that the IRCA led to decreases in crime – particularly property crimes. Cascio and Lewis (2023) find that the IRCA led to more immigrants bringing their families into the US. Another example is Chapter 3, which uses a similar difference-in-differences strategy to Amuedo-Dorantes *et al.* (2007) and finds that the IRCA legalization led to an increase in marriage rates among its recipients.

1.2.2 Intertwining Immigration Policy with Foreign Policy

Congress and the executive branch often acted in response to refugee crises that were related to US foreign-policy objectives. This typically involved accommodating refugees from countries that were battlegrounds in the US’s fight against communism. This is an important theme of immigration policy from the 1960’s through the early 1990’s. Krogstad (2019) presents a useful figure showing refugee admissions to the US by source, from 1975 to the present, entitled ”The shifting

origins of refugees to the U.S. since 1975).” It labels key policy events (many of which are covered here) on the timeline.

A key milestone during this era was in 1980, when the Refugee Act was signed by President Jimmy Carter, leading to a significant expansion in refugee resettlement. This act adopted the United Nations definition of refugee: someone seeking protection from persecution or the fear of persecution based on factors such as race, religion, nationality, membership in a particular social group, or political opinion. While initially establishing an annual limit of fifty thousand refugee entries, the act granted presidents the authority to set annual caps. It also recognized the right to asylum, allowing individuals to seek protection upon arrival in the United States, regardless of the legality of their arrival. If approved, asylees, like refugees, were able to pursue permanent residency. The policy implications of the Refugee Act became immediately apparent, as the Carter administration shortly thereafter took in around 150,000 Cuban and Haitian refugees who had arrived in the Mariel Boatlift, an episode which I discuss in detail below. But to understand this era better, we must circle back to the early 1960’s.

An important policy early this era occurred in 1961 – when the Cuban Revolution induced many Cuban refugees. This strained state governments and local volunteer organizations. President John F. Kennedy tasked his Secretary of Health, Education, and Welfare to conduct an investigation into the activities of Cuban refugees under the purview of the executive branch. The objective was to address the growing demand for assistance among asylum seekers effectively. This led to the establishment of the Cuban Refugee program, which streamlined and enhanced the

provision of aid to the refugees. Congress formalized and solidified this effort with the Migration and Refugee Assistance Act of 1962, which provided aid to refugees – particularly those fleeing communist countries (USCIS).

In 1965, President Lyndon B. Johnson announced that the United States would open its doors to all Cubans who were seeking sanctuary, leading to a large influx of Cubans via small boats. To facilitate a safer and more efficient process for bringing Cubans to the United States, the federal government initiated an airlift program on December 1, 1965. Under this program, Cubans who were already in the US had the opportunity to apply for the admission of their relatives into the country. The screening process involved Cuban refugees being assessed in Cuba, subsequently flown to Miami, and then subjected to further screening at special processing centers by the Immigration and Naturalization Service (INS) and other inspection agencies. Recognizing the need to address the large influx of Cubans facilitated by the airlift program, Congress passed the Cuban Adjustment Act on November 2, 1966. This act aimed to provide a legal framework for allowing Cuban refugees who had entered the US under the attorney general's parole authority to attain lawful permanent resident status after two years of residency.

A climactic event in this era of Cuban inflows is known as the Mariel Boatlift, which involved mass emigration from Cuba to the US by boat. This occurred between April and October of 1980. In April of 1980 – during a period of great unrest in Cuba – a substantial number of Cubans sought asylum from the Castro regime at the Peruvian embassy. In an attempt to quell the growing unrest, Castro made the decision to open the port of Mariel, allowing any residents who wished

to leave the country to do so. Over the span of six months, approximately 125,000 Cuban immigrants (often referred to as “Marielitos”) crossed the Straits of Florida. Many settled in the Miami area – which is geographically close to Cuba and already had a notable Cuban presence. The Marielitos were eligible to obtain permanent residence in the US under the provisions of the Cuban Adjustment Act. This event has been of great interest to immigration economists, so I discuss it in further detail in Section 3.1.3.

There were similar actions to those toward Cuban refugees toward refugees from other regions as well. Significant examples come from the Vietnam War era. In April of 1975, President Ford ordered Operation Babylift, which allowed for the evacuation of Vietnamese orphans to the US, who were then put up for adoption. In May of that year, the Indochina Migration and Refugee Act was enacted, which was meant to support the resettlement of Vietnamese and Cambodian refugees in the US. The act provided \$405 million to the assistance of over 130,000 individuals for various purposes; including transportation, processing, reception, and resettlement costs. The Carter Administration issued an executive order that doubled the number of Southeast Asian refugees that were authorized to enter the US. In 1982 and 1988, President Reagan signed the Amerasian Immigration Act and the Amerasian Homecoming Act, respectively. These acts gave immigration priority to those born in Cambodia, Korea, Laos, Thailand, or Vietnam with an American father during the period of US military involvement in those countries. The cumulative scale of these actions ended up being quite large. By 1995, nearly 500,000 Vietnamese immigrants had entered the US (Donato and Amuedo-Dorantes, 2020).

Intertwined with the US government opposition to the Marxist Sandinista Junta of National Reconstruction government in Nicaragua, the Reagan Administration shielded roughly 200,000 Nicaraguan refugees from deportation (Desilver, 2014). Another episode is related to the US government support of the El Salvadorian government in Salvadoran Civil War – which caused the US to be initially reluctant to accept Salvadorean refugees. But then the US government changed course after a lawsuit and pressure from advocacy groups (Menjivar and Gomez Cervantes, 2018). By executive order, the George H.W. Bush and Clinton Administrations protected roughly 200,000 Salvadoran refugees from deportation (Desilver, 2014).

Additionally, the US implemented a provision in the 1990 Immigration Act called Temporary Protected Status (TPS), which permits foreign nationals from specific countries to reside and work in the US if they are unable to safely return to their home country due to conflict or natural disasters. The initial beneficiaries of TPS were individuals from El Salvador – about 290,000 individuals (Orrenius and Zavodony, 2015). TPS was initially designed as a temporary measure, with a duration of up to 18 months (although country designations can be extended). TPS does not offer a direct path to permanent residency. Orrenius and Zavodony (2015) study this policy with a difference-in-differences strategy that compares Salvadoran migrants (who were eligible for TPS) to Mexican migrants (who were ineligible for TPS) and find that TPS leads to higher employment rates for women and higher earnings for men.

Another notable example of intertwining immigration and foreign policy is the

US response to the Tiananmen Square protests. These were a series of student-led demonstrations against the Chinese Communist Party (CCP) that took place in Beijing, China in 1989. The protests spanned over several months and the CCP forcefully suppressed the demonstrators in various parts of the city, including Tiananmen Square. The death toll of the incident ranges from several hundred up to 10,000 (BBC News, 2017). The US took various steps to safeguard Chinese individuals residing in the US that feared harm after those events. Initially, the US halted any compulsory departures for Chinese nationals already present in the country as of June 1989, and subsequently granted them permission to work legally. The Chinese Student Protection Act, enacted in October 1992, extended eligibility for lawful permanent resident status to these Chinese nationals. There were about 80,000 Chinese nationals present in the US as of June 1989 – on student visas, other temporary visas, or were of unauthorized status. Orrenius *et al.* (2012) look at the labor market outcomes of Chinese nationals that were likely beneficiaries and compare them to two other groups. One was immigrants from Hong Kong, Taiwan, and South Korea. The other was Chinese immigrants who did not arrive in time to benefit from the actions. They find that the actions increased the earnings and employment of the affected Chinese nationals.

1.2.3 1990-2005: “Law and Order” Across the Border

The policy environment from 1965 through the early 1990’s can be characterized as one where protections for migrants were extended and restrictions on

migrants were liberalized. A policy that capped off that era occurred in 1990, when Congress passed the Immigration Act. The legislation was meant to “open the front door wider to skilled immigrants of a more diverse range of nationalities” (Simpson, 1990). The act expanded and revised the 1965 framework – increasing immigration (permanent residence) and several categories of temporary visas for workers. The act forms the foundation immigration law today (Chishti and Yale-Loehr, 2016).

1.2.3.1 Key Episode: Immigration Act of 1990 and the H-1B Visa Program

The law increased immigration (permanent residences) and temporary migration. It created a cap of 700,000 total that is virtually guaranteed to increase every year.² It set the allocations within that total across family-sponsorship, employment-based, diversity visas (described later), and other smaller categories. Importantly, it specified that immediate relatives of US citizens (spouses, parents, and non-adult children) are not subject to a cap. The act also expanded employment sponsorship as an overall percentage of total immigration. It created the diversity lottery category, which aims to diversify the incoming immigrant population by allocating 55,000 visas (revised to 50,000 in 1997) to individuals (and certain family members) in countries that experienced relatively low immigration into the US within the past 5 years.

One notable change to temporary visas was the law’s revision to the H1 visa

²Immigration had previously only been capped by the country-specific caps. But in practice, immigration still increased with the introduction of this cap.

program for highly skilled workers. It divided it into H1-A (for nurses) and H1-B (for other specialty occupations) categories and expanded the latter (Chishti and Yale-Loehr, 2016). The H1-B program is an important means of acquiring high-end talent for US businesses – particularly in STEM-related fields. STEM occupations account for approximately 60% of H-1B admissions, and the program is large enough that growth in the H-1B program can be impactful to the overall growth of foreign-born STEM employment (Kerr and Lincoln, 2010).

An important topic that researchers have investigated regarding the impact of the H-1B program is innovation. I summarize several different approaches to this research question. First, I discuss Kerr and Lincoln (2010). They exploit the fact that during the period they study (1995-2008), the cap on H-1B visas varied greatly: ranging from as low as 65,000 per year to as high as 195,000 per year. They look at cities and firms disproportionately affected by the H-1B program and use data on patent grants and applications, finding that in cities with 10% higher H-1B growth, there is an increase of 1-4% in patents with Indian and Chinese names. There is no evidence of crowding out patents with names outside of those ethnicities. When they perform a similar exercise on a panel of 77 publicly listed companies, they find that the relatively more H-1B impacted firms are similarly more innovative.

Bound *et al.* (2018) construct a macroeconomic model to analyze the impacts of the H-1B program on the technology sector. They calibrate their model using data from 1994-2001. Because of their effects on innovation, H-1B workers were found to have lowered prices and raised output of information technology goods by 1.9% and 2.5%, respectively – while also increasing the profits of the sector. On the

other hand, without the H-1B program, employment for US-born computer science workers would have been up to 10.8% higher in 2001.

Khanna and Lee (2018) study the impact of the H-1B program on innovation in the following manner. They create a firm-level dataset that runs through 2006-2015 that contains: firm-level H-1B worker applications, products the firms sell (Nielsen Retail Scanner Data), and other firm characteristics (Compustat database). They measure innovation as product reallocation: the entry of new products and the exit of outdated products. They first use the panel structure of the data to establish a positive relationship between product reallocation and higher firm revenue growth. They then link applications for H-1B workers to product reallocation. Thus, they argue that they provide evidence linking the H-1B program to innovation in the form of product reallocation.

1.2.3.2 1990-2005: “Law and Order” Across the Border, Continued

After the 1990 Act, the US entered a political climate characterized by its emphases on “law and order” – reflected in policies that emphasized security and public safety, as well as “personal accountability” with respect to the utilization of government services. The major branches of the federal government moved with this change, with the seizure of Congress by the Republicans in 1994 and the related second-term pivot towards the political center of President Bill Clinton. With respect to immigration-related legislation, 1996 was a pivotal year, with the passage of three major laws that had important implications for immigrants.

This was a time where Democratic President Bill Clinton, conceding to Republican congressional victories, famously declared, “The era of big government is over... Self-reliance and teamwork are not opposing virtues; we must have both” (Clinton, 1996). The stage was set for the passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA). While the legislation’s main focuses and impacts extend far beyond immigrants – emphasizing tying government benefits to work requirements – the relevant part of this discussion are the substantial changes in eligibility for safety net programs for immigrants.

Prior to PRWORA, legal immigrants were eligible for the same benefits as citizens, but the act eliminated their access to certain safety net programs, including cash welfare assistance (Aid to Families with Dependent Children/Temporary Assistance for Needy Families or TANF), food stamps (now Supplemental Nutrition Assistance Program or SNAP), Medicaid, State Children’s Health Insurance Program (SCHIP), and Supplemental Security Income (SSI). Subsequent legislation at the federal and state levels led to a complex patchwork of eligibility rules varying by immigrant status, arrival year, and program (Bitler and Hoynes, 2011). As it pertains to immigrants, there has been a sizable literature investigating the effects of welfare reform on immigrants. Below, I review a sample of that literature.

Borjas (2002) examines the impact of the welfare reform legislation on welfare use in immigrant households. The study highlights a significant decline in welfare participation rates among immigrants in California, whereas immigrants residing outside California experienced similar declines to natives. States outside of California often responded to PRWORA by offering state-funded alternatives that included

eligibility for immigrants. And immigrants often responded to the reform by becoming citizens. In another study, Borjas (2003) uses a similar strategy (comparing states that offered alternatives to those that did not) to explore the link between Medicaid cutbacks and health insurance coverage in the immigrant population. The analysis shows that the cutbacks in the Medicaid program did not reduce health insurance coverage rates among targeted immigrants. Instead, immigrants responded by increasing their labor supply, thereby increasing the probability of being covered by employer-sponsored health insurance.

East (2018) investigates the impact of welfare reform on immigrants' labor supply, focusing on access to Food Stamps. Using state-level variation in eligibility impacts, the study reveals that program access significantly affects the labor supply of immigrants. While single women experienced a decrease in employment rates of approximately 6%, married men continued to work at similar rates, but reduced their hours by 5%. The findings align with traditional labor supply theory, showcasing the labor supply disincentives associated with means-tested programs. Using a similar empirical design, East (2020) focuses on the effects of the Food Stamp program on children's health. She estimates the medium-run health effects on US-born children of immigrants. The results indicate that the loss of parental eligibility has significant effects on program receipt, and each additional year of parental eligibility leads to improvements in health outcomes for children at ages 6-16.

There was also an increased taste in the American public for policies focused on security, "tough on crime" approaches, and public safety more generally. The 1996 Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) was

an important immigration-focused law that reflected the political attitudes of the time. The enforcement-focused law dedicated vast resources to the Immigration and Naturalization Service – doubling the budget of the agency between 1994 and 1998. The size of the agency, as measured by the number of employees, also doubled during a similar time-frame (Watson and Thompson, 2021). Many of these employees were Border Patrol agents (Donato and Amuedo-Dorantes, 2020).

The law increased employer sanctions for hiring unauthorized immigrants to harsher levels than the IRCA. The law also simplified the removal of unauthorized migrants and disallowed their entry into the US for up to ten years. It also made it such that US resident sponsors of immigrants are legally responsible for the sponsored. It also increased the income threshold that one must obtain in order to be a sponsor (Donato and Amuedo-Dorantes, 2020).

A key feature of the law was to shift many immigration-related offenses from civil to criminal. These came with harsher punishments, and also provided justification for restrictionist politicians and activists to bolster their cases (Watson and Thompson, 2021). The law made human smuggling (assisting migrants to enter or stay in a country illegally) and the use of fraudulent immigration-related documents criminal offenses. The law also took a set of crimes that would have previously been minor offenses (such as smaller-scale drug crimes) and made them grounds for deportation without a hearing.

A complementary law passed in 1996 was the Anti-Terrorism and Effective Death Penalty Act. This law gave immigration enforcement authorities the ability to deport any non-citizen who had ever committed a crime, and limited judicial

review of deportation orders.

In correspondence with both laws, immigration-related arrests skyrocketed. Between 1994 and 2000, interior arrests increased nearly 120%, rising from 63,000 annually to 138,000 annually. Similarly, border arrests drastically rose by roughly 70%, going from about 1 million in 1994 to 1.7 million in 2000 (Watson and Thompson, 2021).

The terrorist attacks of September 11, 2001 – including the perception that a cause of the tragedies was inadequate security in entrances to the country, led to an even greater emphasis on security in immigration policy. The aforementioned Immigration and Naturalization services was placed into the newly established Department of Homeland Security (DHS) and broken up into three sub-departments: Immigrations and Custom Enforcement (ICE), Customs and Border Protection (CBP), and the US Citizenship and Immigration Services (USCIS). Separating enforcement functions into separate agencies with their own organizational structures complemented the machinery introduced by the IIRIRA and in practice represented a large expansion in immigration enforcement intensity (Watson and Thompson, 2021).

1.2.4 2006-Present: Presidential Power: Executive Actions Drive Immigration Policy

Congress and the political parties have become increasingly polarized on the subject of immigration. And to pass the Senate, most potential Congressional action regarding immigration would require a filibuster-proof majority of at least 60 votes.

In the 2006 Congressional elections, the Democrats took control of both chambers of Congress. Since then, in absence of the necessary consensus for Congress to act on immigration, federal immigration policy has mostly relied on executive actions from the president. In this subsection, I highlight some major executive actions that I believe to be representative of the particular administrations' stances towards immigration. This period spans the second half of George W. Bush's second term, and the presidencies of Barack Obama and Donald Trump.

While George W. Bush did support legislation that would have led to a pathway to citizenship for many unauthorized immigrants, his executive actions placed an emphasis on increased immigration enforcement. His major actions and general approach towards enforcement were continued through Barack Obama's first term.

One such action by the Bush Administration was the implementation of the Consequence Delivery System (CDS). It was a set of sanctions applied to migrants attempting to cross into the US via the border with Mexico in order to deter them from attempting to enter the US again. The sanctions included increased difficulty in obtaining a legal US visa, relocation of the migrant far away from the point of capture, and prosecution in US courts. This action was continued through 2012 by the Obama Administration. The rollout was staggered over time at different locations across the border – this created an avenue for researchers to determine the effect of the policy. Bazzi *et al.* (2021) use this policy variation and administrative records on apprehended Mexican nationals to estimate the effect of the policy on recidivism. They find that exposure to the CDS reduced the 18-month reapprehension rate of men by 19-25%.

Another important enforcement initiative was Secure Communities (SC), which was effective from 2008-2014. The program made it so the immigration status of anyone arrested by local law enforcement agencies was checked against an ICE database, with DHS and the FBI being informed of any unauthorized immigrants that had been arrested. When someone is arrested, their fingerprints are typically taken and run against an FBI database. Under SC, this was also done against an immigration database. If a match was determined and probable cause for removal was determined, ICE would then issue a detainer on the individual, which allowed for 48 hours for ICE to assume custody of the individual and enter them into the immigration enforcement system (Alsan and Yang, 2022). Between 2008 and 2014; 454,000 individuals were removed under the policy (East *et al.* 2022). Beginning in 2008, the program was rolled out by county, until it was active throughout the entire US by 2013. The variation in the timing of country-level activation of SC has been used by researchers to evaluate the effects of the program.

Given that the program operates through law enforcement, a natural question to ask is if it affected crime rates. Miles and Cox (2014) find that SC had no detectable effect on overall crime rates. East *et al.* (2022) find that SC decreased employment of likely unauthorized immigrants. They also find negative spillover effects to the employment and hourly wages of US-born individuals – perhaps due to an increase in labor costs and/or local consumption. Alsan and Yang (2022) investigate the spillover effects of SC on the participation of Hispanic citizens in Supplemental Security Income (SSI) and the Supplemental Nutrition Assistance Program (SNAP). The theory is that the presence of harsher immigration enforcement chills

participation in government programs even for citizen members of the community, perhaps out of a belief that their entanglement with the government endangers their unauthorized family and friends. They find that SC decreases SNAP participation by 2.1 percentage points and SSI participation by 1.7 percentage points.

Like Bush, President Obama had an ambition to sign legislation that protected unauthorized immigrants who arrived in the US as children, known as the DREAMers. In the lead-up to the 2012 presidential election, it became clear that Obama did not have the political capital to get such a bill through Congress. He then opted to act on behalf of this population through executive order, which resulted in Deferred Action for Childhood Arrivals (DACA).

1.2.4.1 Key Episode: Deferred Action for Childhood Arrivals (DACA)

Deferred Action for Childhood Arrivals (DACA) is an executive order announced by President Obama in 2012 and implemented in 2013 to address the situation of unauthorized immigrants who arrived in the US as children. DACA provides recipients with temporary benefits, including relief from deportation and work authorization. Work authorization is administered through giving recipients social security numbers. This also allows them to open bank accounts and access credit. States have introduced complementary policies to further assist DACA recipients. For instance, in 48 states and Washington DC, DACA recipients are eligible to obtain a driver's license. Some states also grant recipients access to in-state tuition and state financial aid at public colleges and universities. DACA also permits

recipients to travel internationally for educational, employment, or humanitarian purposes.

DACA applicants are required to pay a fee of \$495, renewable every two years. To be eligible for DACA, applicants must meet certain criteria, such as being age 30 or younger as of June 15, 2012, having entered the US at age 15 or younger, being physically present in the US at the time of application, being 15 years or older when applying, having completed high school or its equivalent, and having no lawful status as of the same date. Additionally, they must not have a criminal record and must have lived continuously in the US since June 15, 2007.

It is important to note DACA's temporary nature, with the implication that it can be rescinded by the presidential administration in power. The Trump Administration announced plans to phase out DACA in 2017, but legal challenges and subsequent actions by President Biden have led to its current status, where it is in place but no longer accepts new applications.

As of August 31, 2018, 699,350 individuals had received DACA benefits, while there were approximately 1,302,000 immediately eligible and 1,724,000 potentially eligible individuals. Take-up of DACA has remained relatively stable. Reasons why eligible individuals did not enroll include lack of information, fear of sharing identifying information with the government, and financial barriers such as the application fee and paperwork requirements (Zatz and Rodriguez, 2015).

In economics and other social sciences, there is a large and growing literature on DACA, studying a wide-range of outcomes. Pope (2016) uses the American Community Survey, comparing eligible and ineligible individuals before and after

the policy was implemented to study labor market outcomes such as employment, earnings, and hours worked. He finds that DACA eligibility had positive effects on labor market outcomes. Most other studies of DACA rely on a similar difference-in-differences strategy of comparing eligibles and ineligibles before and after the policy. Gonales *et al.* (2014) find that DACA led to greater participation in the labor market, use of healthcare, use of banking, and driver's license attainment.

There is also growing literature with mixed results on DACA's effect on educational attainment. Kuka *et al.* (2020) find that DACA robustly increased high school graduation and, more suggestively, college enrollment. Amuedo-Dorantes and Antman (2017) and Hsin and Ortega (2018) look at older samples who already met DACA's high school education requirement and find that circumstances exist in which DACA causes people to substitute away from education and into work. These circumstances reflect outside options and the flexibility schools offer their students to work and attend at the same time.

Social outcomes have been studied as well. Kuka *et al.* (2019) find that DACA caused a substantial decline to teenage births. Chapter 2 looks at the effect of DACA on its recipients marriage outcomes. He finds that DACA eligibility increases marriage rates by a lower-bound of 2 percentage points. He also finds that DACA increases marriage for the purposes of assimilation – as reflected by DACA eligibile individuals being more likely to marry native-born US citizens and fluent English speakers.

Research also assesses DACA's effects on mental health. Venkataramani *et al.* (2017) find that DACA had positive effects on mental health for recipients, and

Hainmueller *et al.* (2017) find that DACA improved its recipients’ children’s mental health.

1.2.4.2 Presidential Prowess: Executive Actions Drive Immigration Policy, Continued

After continuing many of the Bush Administrations stricter policies in his first term, the end of Obama’s presidency generally moved the US towards a softer enforcement regime. After that, the presidency of Donald Trump, who campaigned heavily on immigration restriction and enforcement, took a sharp U-turn towards restrictionism.

Trump began his presidency in 2017 by quickly issuing three notable immigration executive orders. The first order focused on constructing a border wall, expanding detention facilities, and increasing expedited removals. The second order restricted federal funds to so-called “sanctuary” jurisdictions – who refuse to enforce federal immigration law they deem unjust or overly punitive. The order also restarted cooperative agreements between ICE and local law enforcement agencies that had been previously scaled back. Finally, it also expanded the list of types of individuals that would be given priority for deportation. The third order – resembling what he campaigned on as a “Muslim Ban” – suspended immigrant visas for citizens of certain countries, including Iran, Libya, North Korea, Somalia, Syria, Yemen, and some from Venezuela, and later added Nigeria, Myanmar, Eritrea, Kyrgyzstan, Sudan, and Tanzania to the list of affected countries (Amuedo-Dorantes

and Donato, 2020).

One of the most restrictive and divisive immigration policies of the Trump Administration was its 2018 “Zero Tolerance Policy” (ZTP) which aimed at criminally prosecuting all unauthorized immigrant adults who crossed the border – regardless of whether they traveled with children or sought asylum. Critics argued that the harsh conditions it imposed were inhumane, while defenders argued they realistically engaging with a crisis. This controversial policy was also known in the popular press as the “family separation” policy. The stated justification for the policy was the surge of minors and families that attempted to cross border, with the ZTP serving as a deterrent. Most of the surge in migrants is attributed to individuals traveling from the Northern Triangle: El Salvador, Guatemala, and Honduras (Amuedo-Dorantes and Bucheli, 2023).

Amuedo-Dorantes and Bucheli (2023) examine the effects of the ZTP. They do so by comparing migration flows of minors from the Northern Triangle to those from Mexico before and after the policy was implemented. They estimate that the ZTP increased the number of unaccompanied children through family separations by 48 percent and reduced the odds of family reunification by 49 percent.

One of the final major immigration executive actions of the Trump Administration was its use of Title 42 – an emergency health authority – in response to the COVID-19 pandemic. Typically, migrants can enter the US illegally, ask for asylum, and enter the US. Title 42 allowed for the US to turn away migrants at the US-Mexico border with a justification of limiting the spread of COVID-19. This policy continued into the administration of Joe Biden, until it was allowed to expire

on May 11, 2023 (Long, 2023).

This section took us through eras of immigration liberalization and restrictionism in modern immigration policy. A consistent theme was that immigration policy reflected broader phenomena in the US sociopolitical landscape – such as foreign policy such as communism or terrorism and domestic social concerns such as crime and the welfare state. This section also described the means (legislation and executive actions) which also reflect the broader political norms of their time. This section is meant to lay a groundwork for later sections in this chapter, and later chapters to place them all in proper historical context.

1.3 Special Topics

1.3.1 Immigration and the Labor Market

As documented in Section 2, immigration to the United States has surged over the past half century. The US foreign-born share of the population increased from 5 percent in 1970 to 14 percent in 2010 (Abramitzky and Boustan, 2017). For assessing the impact of immigrant inflows on labor markets, it is important to note that immigrants disproportionately have low levels of education. While making up 6.2% of the overall population in 1980, immigrants were about 11 percent of US residents without a high school degree between the ages 25-64. ³ In 2000, while only growing to 11% of the overall population, immigrants represented 46% of US residents without high school degrees. 31 percent of immigrants at that time had less

³It is also true, however, that immigrants are over-represented among those with advanced degrees.

than a high school education (Watson and Thompson, 2021; Batalova and Gelatt).

This is important for at least two reasons. The first is that immigrants are a disproportionately disadvantaged group so their economic prospects are of policy interest. The second is that this skill composition of immigrants presents a worry that they could compete with lower-skilled Americans, and thus potentially worsen the labor market outcomes of those natives.

In this section, I provide an overview of immigration and the labor market. I first discuss the economic motivations of immigrants into the US. Then, I review the effects of a sample of immigration policies. Finally, I discuss the effect of immigration on the wages of US natives with less education. The insights on immigration and the labor market presented in this section are crucial to understanding for Chapters 2 and 3, which are studies of the effects of major immigration policies on marriage outcomes. As I discuss in those chapters – both conceptually and empirically – labor market outcomes are significant channels through which immigration policies affect immigrants’ marriage rates and their partner choice.

1.3.1.1 Economic Motives for Immigration

This is a broad consensus in the economic literature that a main motivation for migration to the US is better job opportunities (Hanson, 2006). For example, Hanson and Spilimbergo (1999) document that a 10 percent decrease in wages led to a 6-8 percent increase in apprehensions at the US border. More evidence comes from Cadena and Kovak (2016), who document that in comparison to US-born

men, Mexican-born men's internal migration patterns are much more responsive to local economic downturns. Immigrants also have much to gain from moving to the US, Clemens *et al.* (2009) estimate that a marginal moderate-skill immigrant from a typical developing country would experience an earnings premium of roughly \$10,000 per year from moving to the US off of a base of roughly \$4,900 per year in 2007.⁴

1.3.1.2 The Effect of Immigration Policy on Immigrants' Labor Market Outcomes

While covering the history of modern immigration policy in Section 2, I have reviewed the evidence on the effects of these immigration policies on various outcomes. In this section, I discuss in more detail the evidence on the effect of immigration policy on labor market outcomes.

I cover two major immigration policies that gave long-term unauthorized immigrants work authorization and deportation relief. The first policy is the Immigration Reform and Control Act of 1986 (IRCA). As I note in Section 2, Rivera-Batiz (1999), and Kossoudji and Cobb Clark (2002) report estimated wage effects of the IRCA legalization that range from 6-13 percent. Amuedo-Dorantes *et al.* (2007) find that employment rates fell among the legalized immigrants because men became more selective about the jobs they were willing to hold while women exited the labor force. They all use the Legalized Population Survey (LPS), a survey of unauthorized immigrants legalized by the IRCA that had waves at the time of the

⁴Clemens *et al.* (2009) use the GDP per capita of those countries as a proxy for the base.

1987 authorization, and a follow-up in 1992. Rivera-Batiz (1999) makes inferences by comparing them to a sample of mostly authorized Mexican immigrants in the 1990 Census. Kossoudji and Cobb Clark (2002) and Amuedo-Dorantes *et al.* (2007) improve on this by comparing them to similarly-aged sample of Hispanics in the NLSY79, that has waves annually (and hence during the same years as the LPS). Pan (2010) uses the fact that individuals must have arrived by 1982 and implements a regression discontinuity design and finds that the IRCA raised female employment rates. Thus, the employment effects are sensitive to the variation used.

The second policy of this type – albeit a temporary one – is Deferred Action for Childhood Arrivals (DACA). As I also note in Section 2, Pope (2016) finds that DACA eligibility increased the likelihood of working by 3.7–4.8 percentage points and the number of hours worked per week by 0.9–1.7 hours. He also finds that for individuals below the median income, DACA also has positive earnings effects. He uses American Community Survey data to proxy for the key eligibility requirements of DACA (being age 30 or under and resided in the US for at least 5 years at DACA’s announcement, and arrived in the US at age 15 or under) to construct a treatment group and comparison groups of similar, but ineligible individuals.

Next, I review the effects of policies that protect refugees. Orrenius and Zavodony (2015) study Temporary Protected Status (TPS) with a difference-in-differences strategy that compares Salvadoran migrants (who were eligible for TPS) to Mexican migrants (who were ineligible for TPS) and find that TPS leads to higher employment rates for women and higher earnings for men. Another example is Orrenius *et al.* (2012), who estimate the labor market effects of The Chinese Student

Protection Act of 1992. They compare the labor market outcomes of Chinese nationals that were likely beneficiaries of the policy to two other groups. One group was immigrants from Hong Kong, Taiwan, and South Korea. The other was Chinese immigrants who did not arrive in time to benefit from the actions. They increased earnings and employment of the affected Chinese nationals.

In my history, I also present evidence on the impact of benefits access on immigrants' labor market outcomes. As discussed in Section 2, the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) eliminated immigrants' access to certain safety net programs, including Food Stamps. And some states responded to PRWORA by offering state-funded alternatives that included eligibility for immigrants. East (2018) exploits this state-level variation in the response to PRWORA to investigate the impact of welfare reform on immigrants' labor supply, focusing on access to Food Stamps. She reports that single women experienced a decrease in employment rates of approximately 6%, while married men reduced their hours by 5%.

Finally, I discuss the effects of immigration enforcement on labor market outcomes. The best evidence on this subject comes from the Secure Communities program (SC), which required the biometrics of those arrested by local law enforcement to be checked against the ICE database. East *et al.* (2022) use variation from the county-by-county rollout of SC and finds that it decreased the employment of likely unauthorized immigrants.

A broad conclusion to draw from these studies is that, as it pertains to labor market outcomes, the immigration policies reviewed are – in at least one sense –

effective. Policies that were meant to help migrants such the IRCA legalization, DACA, the refugee protections had positive effects on labor market outcomes. Enforcement, which is at least in part intended to serve as a deterrent to potential unauthorized immigrants, has negative employment effects. And the justification for the immigrant-specific parts of PRWORA was to deter immigration caused by access to benefits and generate incentives to work for those immigrants that do come. As East (2018) notes, the work incentives had the effects that standard labor supply theory suggest they would, as intended. An important caveat is that, as I document in Sections 2 and 3.3, enforcement policies can have negative spillover effects to authorized immigrants and even US-born citizens (e.g. Alsan and Yang, 2022). As did PRWORA on immigrants' US-born children (East, 2020).

1.3.1.3 The Effect of Immigration on US Natives' Labor Market Outcomes

An important question regarding immigrant inflows is their effect on natives' labor market outcomes. Indeed, this has been one of the most heavily discussed questions in immigration economics. As a theoretical matter, it may seem at first straightforward: the increase in labor supply from immigrant inflows should lead to a decrease in wages and/or employment of similar workers, because they are potentially substitutes for each other. Native workers with less education are plausibly similar to the less-educated immigrants coming into the US. But incoming immigrants also buy products and start businesses. They may also have skills that are

different enough from natives such that they actually complement native workers (and thus make the natives more productive) rather than directly compete with them.

There are also disagreements about how to properly estimate the effects of immigrant inflows on labor markets. There are two main approaches to estimating these effects. The first involves comparisons across geography – comparing outcomes in labor markets with different levels of immigration. The second involves comparisons across skill groups – comparing workers more similar to the incoming immigrants to workers that are less similar (Watson and Thompson, 2021). Within these two approaches there are a lot of important judgements researchers need to make, which the discussion in this section will highlight.

A concern with the cross-geography approach is that cities may be experiencing immigrant inflows for reasons related to the outcome in a way that could bias estimates. For example, cities with booming labor markets may attract more immigrants, and thus native workers may also have better labor market outcomes than comparison cities for reasons unrelated to immigration.

One way to deal with this issue is featured in the seminal work of Card (2001). Card uses settlement patterns of immigrants that predate the current labor market conditions to predict immigrant inflows during the time period studied. This prediction from older settlement patterns is then used as an instrumental variable for current inflows. Card’s analysis implies that a 10 percent increase in immigration reduces the wages of natives in similar occupations by 1-2 percentage points, which is thought to be a relatively small effect (Borjas, 2003).

Abramitsky *et al.* (2023) uses a similar strategy of relying on previous settlement patterns in local labor markets, but also relies on policy variation from the implementation of strict, country-specific quotas imposed in the 1920's. In this case, immigrant supply is restricted, so native wages would be predicted to go up in areas with high exposure to the quotas. But the authors fail to find a difference in wage effects across high and low exposure areas. They find that high exposure urban areas relied on internal North American migration to adjust, and rural areas relied on substitution towards better farming equipment.

A different local labor market approach comes from Orrenius and Zavodny (2003). The inflow of immigrants they use is from the awarding of spousal green cards. They argue that these immigrants are unlikely to choose their location based on their own labor market opportunities. They find negative effects only for blue-collar natives, and these effects are quite small.

Borjas (2003) argues that the local labor market model assumed by the previously mentioned papers is inferior to a national labor market model. He argues that the national labor market model better accounts for internal migration and substitution across technologies and locations. For example, employers or industries with operations across different locations can respond to immigrant inflows in location A by moving part of its operations to A from B because the immigrants make labor costs in A relatively cheap. Thus, immigrants in location A can be competing with natives in location B. His approach is reflective of the second approach discussed earlier: comparisons across skill groups. He looks at the effect across education and experience groups and finds that a 10% increase in the labor supply via immigration

leads to a decline in wages of 3-4%, which is notably larger than those estimated with local labor market approaches.

Monras (2020) constructs a spatial model, and uses a low-skill immigration shock from the Mexican Peso Crisis of 1994, which involved migration to the US driven by the sudden devaluation of the peso relative to the dollar. This is thought to involve a more sophisticated version of Borjas' (2003) argument about the national labor market. His analogous result to the others reviewed here (the short-run response) is that 1 percent increase labor supply because of immigration reduces low-skilled wages by approximately 0.7-1.4%.

I conclude with another local labor market approach, which I address on its own, because researchers have dedicated a lot of attention to it in a contested literature. This natural experiment comes from the Mariel Boatlift, which I briefly discussed in Section 2. This involved mass emigration from Cuba to the US by boat and occurred between April and October 1980. Recall that over the span of six months, approximately 125,000 Cuban immigrants crossed the Straits of Florida. Figure 1 in Borjas (2017) is illustrative of the massive spike in Cuban Migration in 1980. Many settled in the Miami area – which is geographically close to Cuba and already had a notable Cuban presence. The Marielitos were eligible to obtain permanent residence in the US under the provisions of the Cuban Adjustment Act.”

This event is one of the most studied in immigration economics. It involves the nearly sudden and hence surprising influx of many workers – disproportionately with lower-levels of education – into the Miami labor market. This has been thought of by scholars as a natural experiment that helps answer questions about immigrant

inflows in a local labor market. In particular, the question about the effect of such an inflow on the labor market outcomes of natives with less education (a group of great interest to policy-makers). The purpose of this subsection is not take a stance on the true effect of the Mariel Boatlift, but to summarize some key contributions to the debate that provide a representative picture of the key issues of contention.

Studies of the Mariel Boatlift have been published for decades in a contested literature. In a series of dueling papers, many theoretical insights and empirical tools have been applied to the same basic question of the event's effect on US natives' labor market outcomes.

There is still ample room for disagreement in the proper way to empirically approach this question (which also involves conceptually grounded decisions and assumptions). Since the Mariel Boatlift most directly affected labor markets in the Miami area, virtually any credible empirical strategy involves the selection of other labor markets to serve as a comparison to Miami. Thus, some pertinent questions include the following. Which other labor markets have similar trends in the labor market outcomes for potentially affected native workers? And is it possible that those labor markets are also affected by the influx of workers into Miami? Moreover, which workers within those labor markets are most likely to be affected by the Marielitos? The affected workers are both those that are most similar and complementary to the Marielitos. These are among the issues that have been intensely debated by immigration economists.

The seminal paper in this literature is Card (1990), who uses a difference-in-differences strategy in Current Population Survey (CPS) data to compare the

Cubans and non-Cubans in Miami to those in Atlanta, Los Angeles, Houston, and Tampa-St. Petersburg; before and after the Mariel Boatlift. He concludes that despite increasing the size of the Miami labor force by 7%, there was no effect on the wages or employment of either non-Cubans or incumbent Cubans.

This classic study has been met with high-profile critiques. Angrist and Krueger (1999) show the danger of arbitrarily choosing a small number of treated and comparison units as Card (1990) did. In 1994, Castro announced another potential opportunity for Cubans to migrate, but ended up diverting it to naval base in Guantanamo Bay. Angrist and Krueger use this non-event as a “placebo” inflow, and show that if they implement the same analysis (with the same group of cities) as Card (1990) did, but centered around 1994 rather than 1980, they are able to generate a fake treatment effect of 6.3 percentage points for the unemployment rate of Black workers in Miami. They argue that in research with so few studied units, spurious results such as that one are too likely to happen to generate confidence in its results.

Another important critique is Borjas (2017), who argues that Card (1990) did not adequately select a sample of workers similar to the Marielitos. Approximately 60% of the Marielitos did not finish high school, whereas roughly a quarter of the incumbent workers in Miami did not. In comparison to the aforementioned increase in the Miami labor force, the boatlift increased the number of high school dropouts by nearly 20% (Borjas, 2017). Thus, Borjas argues, Card (1990) made a key error in not focusing on a sample of high school dropouts. He is also critical of Card’s (1990) choice of comparison cities – arguing that they did not have similar labor market trends to Miami prior to the Mariel Boatlift. He chooses different cities that

he argues have better trends. He finds that – using a sample of non-Hispanic high school dropouts – that the wages of relevant native workers in Miami decreased by 10-30%, depending on the sample.⁵

Borjas (2017) also elicited critiques and refinements that further dissected the data and introduced newer methodologies. Peri and Yasenov (2019) use the synthetic control method to select a convex combination of comparison cities for Miami in a data-driven manner. Essentially, the synthetic control method uses pre-treatment trends in the outcome and other observable characteristics to assign weights (all adding up to 1) to non-Miami cities and combines them to construct a counterfactual “synthetic” Miami. Though there are certainly decisions the researcher needs to make in implementing synthetic control, the data-driven approach is thought to reduce the amount of subjectivity and researcher biases in selecting a counterfactual. Thus, this paper addresses the critiques of Borjas (2017) and Angrist and Krueger (1999) about arbitrary control group choice. The authors report results consistent with Card (1990) – they do not report any effects of the boatlift on wages or employment. They then set their sights on Borjas’ claim that Card (1990) did not choose an appropriate subsample of comparable workers to the Marielitos. By replicating Borjas’ (2017) result and then examining a longer period of pre-Boatlift data (1972-1979 rather than 1977-1979) for all 27 possible combinations of high school dropouts across genders (male, female, both), ethnicity (non-Cuban Hispanic, non-Hispanic, and all non-Cubans), and age (prime, young-old, and all working age),

⁵However, all studies reviewed in this section, including Borjas (2017), have confirmed a non-negative employment effect.

they argue that the time series for each of these subsamples in the shorter time-frame is volatile and not reflective of the long-term trend. Thus, selecting a proper comparison group for the Marielitos based on the shorter time-frame can lead to inferences based on measurement error. And that is what they conclude happened with Borjas (2017), because selecting a similarly trending comparison group led to them again confirming Card (1990). Peri and Yasenov also point out differences in Borjas' results when a combination of the May CPS and CPS Merged Outgoing Rotating Groups (MORG) is used rather than the March CPS, with the latter having a larger effect. They attribute this difference the smaller sample size in March (and thus a greater vulnerability to measurement error) and recall bias (the March CPS asks for earnings in the past year, the other data ask for earnings in the past week).

Another critique of Borjas (2017) comes from Clemens and Hunt (2019). The thrust of their criticisms come from two main points. First, they expand on the sample-size critique of Peri and Yasenov (2019), pointing out that Borjas' sample excludes women, Hispanics, workers under 25 and over 59, and workers with a high school education or higher. This omits 91% of the observations per year during the period where he finds the largest effects (1983-1987). This amounts to an average of only 17 observations per year during those years. Secondly, and at much greater length, they argue that the March CPS results are an artifact of the Census Bureau increasing their sample coverage of Miami Blacks – who have lower wages than non-Blacks – in the same year that the Mariel Boatlift occurred. Thus, the relative decrease in non-Hispanic wages after the event is because there is a higher proportion of lower-wage Blacks in that group.

1.3.2 Immigration and Crime

On June 16, 2015 Donald Trump launched his presidential campaign and infamously said, “When Mexico sends its people, they’re not sending their best. . . They’re sending people that have lots of problems, and they’re bringing those problems to us. They’re bringing drugs. They’re bringing crime.” (Phillips, 2017). While expressed in a bombastic manner that many believe to be prejudiced against Mexicans, Trump was expressing a belief that is not unique throughout US history – a belief that immigration causes crime (Lee, 2019).

In what follows, I review the research that brings evidence to the underlying premises that immigration increases crime. The research reviewed in this section is focused on immigrant populations that are disproportionately unauthorized and lower-income, so it should be viewed with that in-mind. It is also likely that people who speculate about the link between immigration and crime, such as Trump in the above quote, also have those immigrant populations in-mind.

Key questions include the following. Do immigrants commit crime at a higher rate relative to natives? Regardless of whether they themselves commit crime, do they affect crime rates? And what effect does immigration policy have on crime? Key sources for this section were Orrenius and Zavodny (2019) and Doleac (2017). I emphasize here that this is only meant to be a brief overview, and those wishing for a more in-depth review should refer to those sources.

This relates to Chapters 2 and 3 in the following manner. As I discuss at multiple points in Chapters 2 and 3, my view is that marriage is deeply related to

the broader community. Crime is generally thought to be a key indicator of stability at the community-level, and thus can have important implications for marriage markets. Given the gender imbalance of crime, there is also reason to believe that it can impact marriage markets through that channel as well.

When considering whether immigrants commit crime at a higher rate relative to natives, I begin with a related question: does economic theory predict that they would? As Orrenius and Zavodny (2019) discuss, economic theory gives countervailing predictions. In the classic theory of crime presented in Becker (1968), potential criminals consider the costs and benefits when deciding whether or not to commit crime.

One potential cost of committing crime is the opportunity cost. This is higher for those with greater labor market opportunities that could be lost if caught committing a crime. Unauthorized immigrants – the ones typically accused of greater criminality – have lower earnings than otherwise equivalent natives, so their opportunity cost is lower *vis-a-vis* labor market returns.⁶ But there are other opportunity costs. So conversely, immigrants face harsher punishments for many crimes (particularly those that are unauthorized or have not naturalized). In addition to the statutory penalties that non-immigrants also face, immigrants convicted of crimes may face deportation, and being barred from reentry or the ability to naturalize. Moreover, immigrants – particularly ones that are demographically more likely to be unauthorized – face greater scrutiny from law enforcement and thus have a higher

⁶However, the argument could be made that because of their incomes relative to their options in their origin country, their opportunity cost is higher.

likelihood of detection. This further increases their cost of committing crime, and represents a deterrence effect.

The evidence I present in this section suggests that the deterrence effect is potentially important. *Prima facie* evidence is that the incarceration rate among immigrants is 25% of the rate of US natives (National Academies of Sciences, Engineering, and Medicine; 2015). But what if the low incarceration rate is due to the fact that immigrants are being deported rather than incarcerated? Butcher and Piehl (2007) present evidence that is inconsistent with deportation explaining low immigrant incarceration rates. Indeed, their evidence suggests that the immigration process selects individuals who have a lower propensity to commit crime or be responsive to deterrence. They track immigrants over time and find that immigrants became less likely to be institutionalized overtime, and new arrivals were involved with criminal activity at very low rates. The selection mechanism that appears to be at work here adds an additional potential explanation for immigrants' lower criminality outside of a Beckerian analysis.

A complementary piece of evidence that deterrence is an important part of the story comes from the fact that the US-born children of immigrants converge to US natives' higher crime rates (Bersani, 2014). Second generation immigrants likely have some cultural similarities to their parents, but face different punishments for committing crime. This argument is buttressed by the fact that second generation immigrants drastically outperform their parents in the labor market and education – so they face a greater opportunity cost for committing crime native (National Academies of Sciences, Engineering, and Medicine; 2015). Thus, this multigenera-

tional evidence is consistent with deterrence from the threat of immigrant-specific legal penalties being important.

If immigrants themselves do not commit crime at a higher rate, do they nevertheless cause crime rates to increase (perhaps due to their effects on natives)? The answer appears to be no. While there are many correlational studies that make the same point (Ousey and Charis, 2018), there also is a small piece causal evidence that suggests immigrant inflows do not affect crime rates. Chalfin (2013) uses variation in rainfall across regions in Mexico combined with persistent migration patterns from those Mexican regions to different cities in the US. He finds that Mexican migration does not affect violent or property crime in receiving cities.

I finish this section by reviewing the effect on crime of some major immigration policies discussed in this chapter. These include enforcement policies, the IRCA’s legalization and employer sanctions, and DACA.

Two prominent enforcement policies have been studied. They might be hypothesized to affect crime rates because they both utilize federal cooperation with local law enforcement. The first is 287(g) agreements. Forrester and Nowrasteh (2018) use a staggered adoption of 287(g) agreements throughout the state of North Carolina. They find no effect of apprehensions through 287(g) agreements – which are agreements between local law enforcement and ICE to collaborate – on crime rates and police clearances. The second enforcement policy under consideration is Secure Communities, which is an administrative action that requires the biometrics of those arrested by local law enforcement to be checked against the ICE database. Miles and Taylor (2014) exploit the county-by-county rollout of Secure Communities

from 2008-2013 and find no effect on overall crime rates.

The Immigration Reform and Control Act of 1986 (IRCA) has at least two potential channels through which it could affect crime. One is the amnesty program. Baker (2015) exploits county-level variation in IRCA legalizations and finds that a one percentage point increase in the number of IRCA legalizations per-capita is associated with a fall in overall crime-rates of 4.5 percent. The other channel through which the IRCA could affect crime is through its sanctions on employers who hire unauthorized immigrants. The non-legalized unauthorized immigrants whose labor market prospects were harmed by the sanctions could turn to crime as an alternative. Freedman *et al.* (2018) find an increase in felony charges against those most likely to be affected by the IRCA's employment regulations.

Finally, Gunadi (2020) examines the effect of DACA on crime. Using a differences-in-differences strategy, he fails to find an effect at the individual-level of DACA eligibility on the likelihood of incarceration. Then, using state-level variation in DACA applications approved, he finds that an increase of one DACA approval per 1000 population is associated with a 1.6% decline in the property crime rate – suggesting the economic effects of DACA provided an alternative to crime for some individuals.

1.3.3 The Effects of Immigration Enforcement Policy

Most mainstream political parties and ideologies in modern times deem maintaining national borders as a legitimate use of state power. Any concept of national

borders requires the governments to make determinations of who is authorized to be in a country, for how long, and what they are allowed to do when within the country. For any set of rules that governments choose to make, they also determine the consequences of violating those rules and the amount of resources to be put into detention and enforcement.

The benefits of immigration enforcement potentially include creating a sense of trust that the laws of society will be duly enforced, ensuring public safety, regulating access to publicly provided goods and services, and allowing a society to craft a culture and identity that it collectively desires to have. It also allows countries to regulate the number of various types of immigrants they allow in. But as economists, we must weigh those potential benefits against the costs. This section focuses on the effects of immigration enforcement on unauthorized immigrants and their surrounding communities – which often include US citizens and lawful permanent residents.

Enforcement is inherently an aggressive action. It has the word “force” in it, and is often meant to have a deterrent effect, which implies negative consequences for potential violators by-design. Thus, the empirical results discussed in this section will generally be considered harmful to unauthorized immigrants and those close to them. I wish to make the point that this section makes no argument about the morality of immigration enforcement nor does it make any policy prescriptions about the optimal level of immigration enforcement. I merely offer a summary of important costs of immigration enforcement, after I previously acknowledged the potential benefits in the previous paragraph.

First, I provide a brief overview of how immigration enforcement has evolved over recent decades, and the types of enforcement that are currently used. Then, I discuss evidence on the effect of enforcement on outcomes of immigrants and their communities. These outcomes include labor market and educational outcomes, family structure, utilization of public services, political participation, and interactions with the criminal justice system. I note that the evidence in this section comes from studies that are relatively new and thus have not received the scrutiny that older studies have. It is possible that as this literature and related ones evolve, some conclusions may be altered.

This subsection on enforcement helps contextualize Chapters 2 and 3 in the following manner. Both DACA and the IRCA, the policies studied in those chapters, involve relief from the threat of deportation. One consequence of that is that they represent relief from the direct effects of enforcement policies. I show in Chapter 2 that relief from enforcement policies is a key mediator for the marriage effects that I report. Thus, understanding these policies and their broader effects are important to understanding the policies analyzed in the rest of this dissertation.

1.3.3.1 An Overview of Enforcement Policy

It is important to distinguish enforcement at the border and enforcement in the interior of the country. Border enforcement is a type of immigration enforcement in which a country enforces its immigration laws with a particular focus on the areas at or near the border and ports of entry. Methods include dispatching border

patrol agents to those areas, the construction of walls or other physical barriers at key areas, and electronic surveillance systems (Guerette and Clarke, 2005). Border enforcement is currently undertaken by US Customs and Border Protection (CBP).

Interior enforcement is the primary focus of the literature reviewed in this section. Interior enforcement involves enforcing immigration laws away from the border and ports of entry. It focuses on the removal of unauthorized immigrants already living in the US. Interior enforcement can include raids of dwellings or businesses where unauthorized immigrants are suspected to be, agreements with law enforcement agencies, and verification systems for businesses' employees (E-Verify) as well as sanctions for employers who hire unauthorized immigrants. Interior enforcement is currently undertaken by US Immigration and Customs Enforcement (ICE).

The cooperation between federal immigration authorities and local law enforcement comes in several forms. One is 287(g) agreements between ICE with state and local law enforcement agencies. These agreements provide law enforcement agencies with personnel that is trained to work with ICE, aiding in detection and removal of unauthorized immigrants that are encountered in regular law enforcement activity (Amuedo-Dorantes *et al.*, 2015).

Another type of cooperation is broad federal policy that creates an infrastructure with requirements for coordination between local law enforcement and federal immigration authorities. A key example is Secure Communities (SC) which was effective from 2008-2014, deactivated in 2015, and activated again in 2017. Under this program, the immigration status of anyone arrested by local law enforcement

agencies is checked against an ICE database. If an arrested unauthorized immigrant is identified with probable cause for removal (i.e. is arrested for an offense that could lead to removal), ICE then issues a detainer on the individual, which allows 48 hours for ICE to assume custody of the individual in order to start removal proceedings (Alsan and Yang, 2022). SC also attempts to prioritize enforcement actions to target the most dangerous unauthorized immigrants (Amuedo-Dorantes *et al.*, 2015). The Obama Administration replaced SC with the Priority Enforcement Program (PEP), which maintained cooperation between the federal and local governments, but targeted a specific set of crimes in response to criticisms that SC was too indiscriminate. The Trump Administration, after campaigning on more stringent immigration enforcement, restored SC in 2017 (Amuedo-Dorantes *et al.*, 2015).

A final type of law enforcement-based policy are state-level omnibus laws. These laws enable and encourage police officers to attempt to verify the immigration status at lawful police stops. A controversial example of this was Arizona’s SB-1070, often referred to in the media as the “Show Me Your Papers” law. It had various provisions, but the controversial one was what made it an omnibus law – it encouraged police officers to attempt to determine the immigration status of anyone suspected to be unauthorized during a lawful stop. If an immigrant lacked proper documentation, they could be charged with a misdemeanor that could come with a \$100 fine (plus legal fees) and up to 20 days in prison (Amuedo-Dorantes and Lozano, 2014).

1.3.3.2 The Effects of Enforcement on Immigrant Communities

This section describes the existing evidence on the effects of immigration enforcement on a variety of key outcomes for unauthorized immigrants and the members of their communities. It is worth bearing in mind that many of these community members are US-born or in the US legally. I focus on a selection of outcomes that I believe are broad enough to provide an illustrative landscape of the multidimensional impacts of enforcement.⁷

It is logical to start with the effects of enforcement on classically studied outcomes of microeconomists: labor market outcomes and education. East *et al.* (2022)

⁷Though not exclusively, this section draws a lot of the work of Catalina Amuedo-Dorantes and her co-authors. They constructed a useful, time-varying index of local immigration enforcement intensity at the metropolitan statistical-area level and quasi-experimentally tested it with event studies and placebo tests with respect to a wide variety of economic and social outcomes. Because many of the papers discussed in this section rely on this index, I describe it before proceeding further. This version of the index is from Amuedo-Dorantes *et al.* (2020).

The index tracks five major interior enforcement policies: local 287(g) agreements, state-level 287(g) agreements, Secure Communities, omnibus immigration laws, and E-Verify.

The index is constructed as follows. For each of the five aforementioned policies, a measure of exposure is taken at the county level, and then aggregated to the MSA level, weighted by each county's population and the portion of months in a year that a policy was active in the county. For policy k , MSA m , year t , and all counties c in m :

$$EI_{m,t}^k = \frac{1}{N_{m,2000}} \sum_{c \in m} P_{c,2000} \frac{1}{12} \sum_{j=1}^{12} \mathbb{I}(E_{c,j,t}^k) \quad (1.1)$$

where $\mathbb{I}(E_{c,j,t}^k)$ is an indicator function that equals 1 if policy k was active in county c in month j during year t , $P_{c,2000}$ is the population of county c during 2000, and $N_{m,2000}$ is the population of the MSA during 2000. Populations are taken from the 2000 Census.

Indices for each policy are summed at the MSA-year level, yielding an index that proxies for the overall immigration enforcement intensity to which an individual living in MSA m during year t is exposed. This index (below), ranges from zero (i.e., no policy was active in that MSA-year) to 5 (i.e., all policies were active for the entire year).

$$TotalEnforcement_{m,t} = \sum_{k \in K} EI_{m,t}^k \quad (1.2)$$

use the county-by-county rollout of Secure Communities and estimate a decrease in employment among likely unauthorized immigrants. They also find negative spillover effects to the employment and hourly wages of US-born individuals – perhaps due to an increase in labor costs and/or a reduction in local consumption. Rubalcaba *et al.* (2022) study the effect of immigration enforcement on the teenage children of potential unauthorized immigrants. The idea is that in times of heightened enforcement risk of parents working, families with unauthorized parents will use their US citizen children’s labor to smooth consumption. To overcome the endogeneity of immigration enforcement to these outcomes, they use an indicator variable identifying the months in which the level of arrests exceeds the MSA-specific trend as their regressor of interest. The authors report that such a surge in arrests increases labor force participation and hours worked of these teenagers by 27% and 20%, respectively. East and Velásquez (2022) look at the effect of SC on college educated US-born women with young children’s labor supply. They find a reduction in the probability of working of 1% and in hours worked by 1.5% relative to their means. They attribute these effects to the negative impact of SC on women’s ability to outsource home production to immigrants affected by enforcement measures.

With respect to education, immigration enforcement has proven to be salient as well. Dee and Murphy (2020) find that local 287(g) agreements reduce the number of Hispanic student enrolled in the local public schools by 10% within 2 years. Amuedo-Dorantes and Lopez (2017) examine the effects of intensified immigration enforcement on schooling for the children of likely unauthorized immigrants. They find that increased enforcement increases the probability of repeating a grade for

children ages 6-13 by 14% and the dropout rate for students ages 14-17 by 18%. While labor market and education outcomes are important, it is also important to look at broader measures of community well-being.

One key proxy for the overall well-being of a community is reflected in the family structure within it (Chetty *et al.*, 2014). One way a family is formed is through marriage. Amuedo-Dorantes *et al.* (2020) report that increased immigration enforcement causes Mexican non-citizens to marry US citizens at higher rates. This is hypothesized to be because the instability enforcement introduces induces non-citizens to seek the greater long-term security that marriage to a citizen provides. This is consistent with my finding in Chapter 2, that DACA – which provides relief from deportation – induces its recipients to move away from marriages to citizens that are more likely to be for security via legal benefits.

Children’s resources, health, and living situations are thus key indicators of family well-being. The impacts of immigration enforcement can be detected at birth. Amuedo-Dorantes *et al.* (2022) use birth records data to study the effect of exposure to intensified immigration enforcement during pregnancy of likely unauthorized immigrant mothers on infant birth weight. They find that enforcement (as measured by the enforcement index) reduces birth weight, and the effect is largest for exposure during the third trimester.

Then, it is important to consider the effect of enforcement on children’s circumstances once they are born. Using the ACS from 2005-2011, Amuedo-Dorantes *et al.* (2018) report that a one standard deviation increase in the enforcement index has the following effects on US-born children with at least one likely unauthorized

parent: it raises the likelihood of living in poverty by 4%, lowers their household incomes by 19%, and increases their participation in the Food Stamps program by 7%. Amuedo-Dorantes and Arenas-Arroyo (2018) use 2005-2015 data to look at the effect of enforcement on the living arrangements of US-born Hispanic children. They find that the average MSA-level increase in enforcement over that period leads to 19% increase in the likelihood the children live in a household without their parents and a 20% increase in the likelihood of them living with a likely unauthorized mother with an absent father.

Individuals in immigrant communities may also respond to an increase in immigration enforcement by engaging less with public services. This is often referred to as “chilling effects” (Watson, 2014). The level of engagement with public services is indicative of the level of trust and sense of belonging in the society that immigrants call home (Jiminez *et al.*, 2021). Watson (2014) looks at the effect of enforcement intensity⁸ on Medicaid participation among the children of non-citizens. She finds that most of the decline in Medicaid participation in the time surrounding welfare reform can be explained by a spike in enforcement intensity. Alsan and Yang (2022) estimate the effect of Secure Communities on participation of Hispanic citizens in the Supplemental Security Income (SSI) and the Supplemental Nutrition Assistance Program (SNAP). They report a decrease in SSI and SNAP of 1.7 and 2.1 percentage points, respectively.

Another important public service whose relationship with the local community is indicative of broader societal trust is the police. Muchow and Amuedo-Dorantes

⁸A moving average of the ratio of deportable aliens to the number of non-citizens.

(2020) use a Google Trends index as a measure of awareness of immigration enforcement, and find that Los Angeles neighborhoods with higher Latino non-citizen concentration experienced declines in per capita domestic violence calls as awareness of enforcement increased.

Dhingra *et al.* (2022) use variation in enforcement from two policy changes to examine the effect of enforcement on willingness to report crime. The first is the Trump Administration’s 2017 executive orders to drastically increase enforcement intensity. They use the FBI Uniform Crime Reporting data to measure reporting and the National Crime Victimization Survey to measure the underlying level of crime. They find that in counties with higher Hispanic concentrations and high levels of ICE cooperation, reporting of crime went down but underlying crime did not. They then replicate these results using the Secure Communities rollout.

It is important to consider the effects of enforcement on interactions with the criminal justice system more generally. Since enforcement is so entangled with law enforcement, a natural question to ask is about its effect on crime. Miles and Cox (2014) find that Secure Communities had no detectable effect on the overall county crime rate. Nevertheless, law-enforcement-based immigration enforcement still leads to more immigrants being detained, potentially putting a burden on the capacity of the criminal justice system. Amuedo-Dorantes and Lopez (2022) use 2006-2018 data from the Annual Survey of Jails and look at the impact of immigrant detention on jail conditions. They find that increases in detainees held for ICE lead to increased non-citizen jailed populations and have no effect on citizen jailed populations. They use 287(g) and Secure Communities as instruments to determine that this higher

level of non-citizen inmates leads to higher reports of jails being overcrowded and under-staffed, longer stays in jail, and more physical assaults within jails.

Measures of political participation also provide important information about the civic engagement of immigrant communities. Amuedo-Dorantes and Lopez (2017) look at the effect of immigration enforcement on the political participation of US citizens that live in households with non-citizens. They find that intensification of immigration enforcement lowers voter registration among that group by 5.3%. They also look at broader civic engagement finding an increase in volunteering in activities such such as community, immigrant, political, and advocacy groups by 17-24%. Amuedo-Dorantes and Bucheli (2022) look at participation from the candidate side. Using Congressional district-level data from 2008-2018, they find that immigration enforcement decreased the percentage of Hispanic candidates in congressional elections. They hypothesize that enforcement increases the costs of mobilization of Hispanic candidates' potential Hispanic supporters that would have allowed them to secure nominations and may also stigmatize Hispanics to other voters.

1.4 Conclusion

As stated in Section 1, the history and the special topics reviewed in this chapter are meant to set the stage for the policy-specific analyses I undertake in Chapters 2 and 3.

Section 2 provided a broad overview of modern immigration policy – specifi-

cally discussing Deferred Action for Childhood Arrivals and the Immigration Reform and Control Act, and where they fit in the broader context of history.

The evidence reviewed the “Special Topics” are important for understanding the empirical work undertaken in Chapters 2 and 3, which are studies of the effects of major immigration policies on marriage outcomes. As I have argued, labor market outcomes are significant channels through which immigration policies affect immigrants’ marriage rates and their partner choice. Crime is a key determinant of social stability, and thus feeds directly into marriage markets. And both DACA and the IRCA give their recipients deportation relief, and therefore represent a relief from the direct effects of enforcement policy. Thus, understanding the effects of enforcement policies are helpful in understanding the policies covered in Chapters 2 and 3.

Chapter 2: The Impact of Immigration Status on Marriage

Evidence from Deferred Action for Childhood Arrivals

August 3, 2023

Abstract

In June 2012, the Obama Administration announced the Deferred Action for Childhood Arrivals (DACA), a program that grants work authorization and deportation relief to unauthorized immigrants who entered the US as children. I estimate the effect of DACA on marriage. I focus on the probability of being married and, conditional on marriage, the probability of being married to a US citizen or US native, and the spouse's English fluency. Proxying for DACA eligibility using the American Community Survey, and focusing on Hispanic immigrants, I use a difference-in-differences strategy, finding that DACA eligibility increases the probability of being married by approximately 2 percentage points, an estimate that is likely a lower bound. I provide evidence that the expanded labor market opportunities offered by DACA amplify its effect on marriage among men and attenuate it among women. I also find that DACA's relaxation of deportation risk increases the incentives to marry, more conclusively among women. For those who do marry, I find that DACA induced individuals to marry more assimilatively, as captured by more frequent marriages to US-born citizens and fluent English speakers.

2.1 Introduction

The US has a large immigrant population. In 2017, the US held 18% of the world's immigrant population (United Nations, 2017). One feature of the US immigrant population is the large number of unauthorized immigrants, estimated to be 12.1 million (3.8% of the US population) (Baker, 2014). Such unauthorized immigrants often have strong, enduring ties to the US; 62% have lived in the US for at least 10 years, and 21% for at least 20 (Gelatt and Zong, 2017), and it is common for unauthorized immigrants to be brought to the US as children by their immediate or extended family. A recent estimate suggests 1.1 million unauthorized immigrant children living in the country (Passel and Taylor, 2010). The family structure of immigrants has broad implications for their lives in the US and their interactions with broader society.

Unauthorized immigrants represent a population who face substantial barriers to participation in American life, and hence face many challenges compared to those with lawful status. These barriers and challenges include the threat of deportation, uncertain future immigration policies, lack of work authorization, the inability to obtain a driver's license, and difficulties with accessing higher education.¹ These obstacles often limit them from achieving their full potential in many domains: including employment and education. One important domain where the barriers unauthorized immigrants face may be particularly constraining is marriage.

In this chapter, I examine the effects of an immigration policy, the Deferred

¹Unauthorized immigrants do not qualify for federal aid, and often do not qualify for state aid or in-state tuition.

Action on Childhood Arrivals (DACA), on recipients' marriage outcomes. DACA is an executive order issued by President Obama that targets unauthorized immigrants who arrived in the US as children. Following its announcement on June 15, 2012, DACA offered recipients a collection of temporary benefits, including deportation relief and work authorization. DACA potentially affects recipients' marriage opportunities in multiple ways. For example, deportation relief increases recipients' appeal to potential partners by reducing uncertainty. Work authorization improves recipients' income prospects and exposure to marriage markets. It might even affect the opportunity cost of dating and marriage. Effects may differ by gender for a variety of reasons, including differences in relative wages, social norms, propensities to pursue higher education, and the importance of fertility timing. I therefore address two questions. 1) How does DACA eligibility affect the probability of being married?, and 2) When people do get married, whom do they marry (e.g., US citizens, US natives, or another immigrants)?

Primary eligibility requirements for DACA center on the age of the individual when the policy was announced, the timing of arrival into the US, and educational attainment. These can be measured using data obtained from the American Community Survey (ACS). I use a difference-in-differences strategy, comparing eligible and ineligible Hispanic individuals before and after implementation of DACA, to obtain intent-to-treat estimates of the effect of DACA on the probability of being married and the probability of marrying individuals of various types, conditional on being married. The ideal dataset would contain information on individuals' legal statuses, but since the best immigration status information in the ACS is whether

an individual is a citizen, the treatment group is contaminated with authorized immigrants.^{2 3}

I find that DACA eligibility increases the probability of being married by a lower-bound of approximately 2 percentage points, in comparison to a base of 31% in the eligible sample. My preferred point estimates for men are larger than those for women, but I cannot statistically reject the estimates are the same. When I explore mechanisms for these effects, I primarily focus on the two main provisions of DACA—deportation relief and work authorization. Using a measure of state-level deportation risk and an MSA-level measure of immigration enforcement intensity as proxies for how salient the deportation relief provision of DACA would be, my findings suggest that deportation relief is a driver of marriage results among women. The result is less clear among men. To test the salience of the work authorization provision, I use two approaches. The first is controlling for labor market outcomes that are affected by DACA [Pope (2016)], and then examining how DiD estimates change. The second is interacting the DiD variable with a local employment propensity for similarly aged Hispanic citizens during years before DACA. Between these two approaches, I demonstrate that the work authorization provision is a driver of

²There is no theoretical reason for which DACA should affect these authorized immigrants directly, and estimates can be interpreted as lower-bounds [Pope (2016)]. If DACA affects authorized immigrants, the magnitude of the effect is certainly less than those directly affected by the policy. There are, then, two cases. The first is that effects on authorized immigrants are of the same sign as DACA recipients, but of a lesser magnitude. The other case is that effects on authorized immigrants are the opposite sign as DACA recipients. In either case, such contamination would attenuate estimates relative to what they would be using complete status data.

³This interpretation also includes the highly plausible assumption that the DACA population is sufficiently small for the policy’s effect on marriage rates of ineligible to be negligible. This plausibility is corroborated by results being robust to using any of several comparison groups. Some groups’ overall marriage markets, such as the large pool of US native citizens, are less likely to be measurably affected by DACA than those of smaller comparison groups, such as foreign-born citizens.

the effect among men, but it diminishes the effect among women. Among some women, the increased labor market opportunities that DACA provides cause them to substitute away from marriage, on the margin. After directly focusing on the two main provisions of DACA, I conduct an analysis that incorporates a group of variables that are thought to be outcomes affected by DACA that are relevant to marriage (labor market, education, and family structure). This mediation analysis describes what percentage of DACA's effect on marriage can be explained by DACA's effect on these mediators. Labor market and family structure outcomes explain a substantial portion of the marriage effect for both sexes. The gendered differences in labor market outcomes follow a similar pattern as the previous analysis (for marriage, opportunities are facilitators for men and substitutes for women). And while the overall variation explained by education is small, the signs of the effects follow a similar pattern as well.

I also provide results for whom DACA causes recipients to marry, conditional on being married. Findings accord with DACA pushing people toward marriages that are more assimilative, and less for legal benefits. I find the DACA eligible are more likely to marry US natives and less likely to marry foreign-born citizens. The assimilative benefit of marrying a US native is greater than that of marrying a foreign-born citizen, but the legal benefits of marrying a native are the same as those of marrying a foreign-born citizen. This marriage pattern is thus consistent with DACA recipients placing greater value on assimilation relative to the legal protections of marrying any citizen. When individuals marry, DACA induces them to marry spouses who are more likely fluent in English.

This chapter contributes to two bodies of literature. The first is the growing literature on the influence of DACA. The effects of DACA have been studied on a variety of outcomes. For the current study, such literature is important to understand because marriage is perhaps the most multi-dimensional decision people make during their lives. Nearly all other important outcomes affect and are affected by marriage. Pope (2016) uses a similar identification strategy, finding that DACA eligibility had positive effects on labor market outcomes. Gonaes *et al.* (2014) find that DACA led to greater participation in the labor market, use of healthcare, use of banking, and driver's license attainment. There is also growing literature with mixed results on DACA's effect on educational attainment. Amuedo-Dorantes and Antman (2017) and Hsin and Ortega (2018) find that circumstances exist in which DACA causes people to substitute away from education and into work. Kuka *et al.* (2020) find that DACA robustly increased high school graduation and, more suggestively, college enrollment. The same authors find in Kuka *et al.* (2019) that DACA caused a substantial decline to teenage births. Research also assesses DACA's effects on mental health. Venkataramani *et al.* (2017) find that DACA had positive effects on mental health for recipients, and Hainmueller *et al.* (2017) found that it improved DACA recipients' children's mental health.

I also contribute to literature on the relationship between immigration and marriage in three ways. First, most studies of immigration and marriage are about inter-group marriage to citizens or other ethnic groups. This chapter also focuses on overall rates of marriage. Second, other papers do not focus on unauthorized immigrants, whose legal statuses make marriage decisions different from those of autho-

rized immigrants. Third, most studies on immigration and marriage do not assess responses to specific policies. One exception to all of these is Amuedo-Dorantes *et al.* (2020), who examine the effects of various state and local immigration enforcement policies on intermarriage and marriage rates. They find that a one standard deviation increase to an enforcement policy index raises the likelihood that unauthorized immigrants marry a US citizen by 3 to 6 percent. Some studies examine the effects of gender ratios on immigrant marriage markets, such as Angrist (2004) and Lafortune (2013). Researchers have also assessed the effect of immigrant intermarriage on labor market outcomes, including Meng and Greogry (2005) and Furtado and Theodoropoulos (2010). Bleakley and Chin (2010) examine the effects of English skills on marriage rates and intermarriage, and Chiswick and Houseworth (2010) assess the effects of arrival age on intermarriage. Furtado and Theodoropoulos (2011) examine the trade-offs between assortative mating on both education and ethnicity among immigrants. This chapter focuses on overall marriage rates and intermarriage, assessing unauthorized immigrants' marriage behaviors and identification of responses to a specific and prominent policy.

This chapter informs policy in several ways. Firstly, it contributes to the evaluation of an ongoing policy that could potentially remain in place for a long time. This research also informs future uses of prosecutorial discretion that provides groups of unauthorized immigrants similar types of benefits, or takes existing benefits away. For example, after enacting DACA, President Obama attempted to expand its benefits to larger populations. In contrast, President Trump attempted to eliminate DACA. Although those actions were struck down in court, it is possible

that similar actions will pass. My results thus provide insights into policy moves in both directions. Perpetual court cases and proposed legislation involve the legal statuses of unauthorized immigrants, and this study informs discussions regarding them. Also, for decades, there has been extensive debate in the US regarding permanent policies that would provide unauthorized immigrants with some combination of deportation relief, work authorization, and paths to lawful, permanent residence or citizenship. While it is beyond the scope of this chapter to speculate how the effects may differ for a permanent policy from those of DACA, it could very well be a good place to start one's thinking.

Section 2 of this chapter provides background information on DACA and its target population. Section 3 discusses a conceptual framework that motivated the empirical analyses that follow. Section 4 describes the data used and dataset construction. Section 5 outlines the empirical strategy used and the sample selection decisions involved when implementing it. Section 6 reports results, and Section 7 concludes and discusses the broader implications of the chapter.

2.2 Background

2.2.1 DACA's benefits

DACA provides a variety of benefits to recipients, the most prominent of which are deportation relief and work authorization. The latter involves receiving a social security number, which allows opening of bank accounts and obtaining credit. States have enacted complementary policies to further help DACA recipients. The

Real ID Act of 2005, enacted as part of the War on Terror, made it impossible for unauthorized immigrants to obtain a driver's license. In 48 states and Washington DC, DACA recipients are able to obtain a driver's license. Some states also allow recipients to access in-state tuition and state financial aid at public colleges and universities. DACA also allows recipients to travel for educational, employment, or humanitarian purposes. As I discuss in the Conceptual Framework section, these benefits might affect DACA recipients' marriage decisions.

2.2.2 Applying for DACA and eligibility requirements

The Department of Homeland Security (DHS) began accepting applications for DACA on August 15, 2012, which took 4 to 6 months to process. The initial application required a \$495 fee, which must be paid every two years for renewal. Proof of eligibility must be submitted, along with other eligibility requirements. First, applicants must have been physically present in the US both on June 15, 2012 and at the time of application. Second, they must be 15 years or older when applying. Third, they must have completed high school or a GED, or are currently enrolled in school, or have been honorably discharged from the Armed Forces or Coast Guard. Fourth, they must have had no lawful status as of June 15, 2012. Fifth, they must not have been convicted of a felony or serious misdemeanors, or three or more other misdemeanors, and do not otherwise pose a threat to national security or public safety. Sixth, they must have arrived in the US at age 15 or younger. Seventh, they must have lived continuously in the US since June 15, 2007,

five years before President Obama’s announcement. Eighth, they must have been age 30 or younger on June 15, 2012.

I again highlight an essential feature of DACA—its temporary nature. Since it was implemented by executive order, the presidential administration in power can rescind it at any time. ⁴

2.2.3 DACA recipients

The post-DACA treatment period in this study is 2013 to 2017. I first present a snapshot of all DACA recipients as of August 31, 2018. 699,350 people have received DACA benefits, in comparison to 1,302,000 who were immediately eligible, and 1,724,000 who were potentially eligible on that date. The latter number includes those who could be eligible were they enrolled in an education program before submitting their applications (356,000), and those who will be eligible once they become older (66,000) (Migration Policy Institute, 2018). The take-up of DACA remained relatively stable throughout DACA’s existence to that point, which I present in Figure 1. Researchers attribute eligible people not applying to lacking information, or fear of giving identifying information to the government (Zatz and Rodriguez, 2015).⁵ Additionally, the \$495 application fee, substantial amounts of paperwork, and the need to submit proof of meeting requirements represent potential

⁴On September 5, 2017, the Trump Administration announced a plan to phase-out DACA, calling for a legislative solution for its recipients that tied with a border wall. This action was slowed by legal challenges, and then eventually President Biden issued another order reinstating DACA. That action by President Biden was then stopped by another court order. As of this writing, DACA is in place and accepts renewals, but it no longer accepts new applications.

⁵This is despite it being illegal for enforcement authorities to use information obtained from DACA applications.

obstacles to applying for unauthorized immigrants, who often lack documentation and resources.

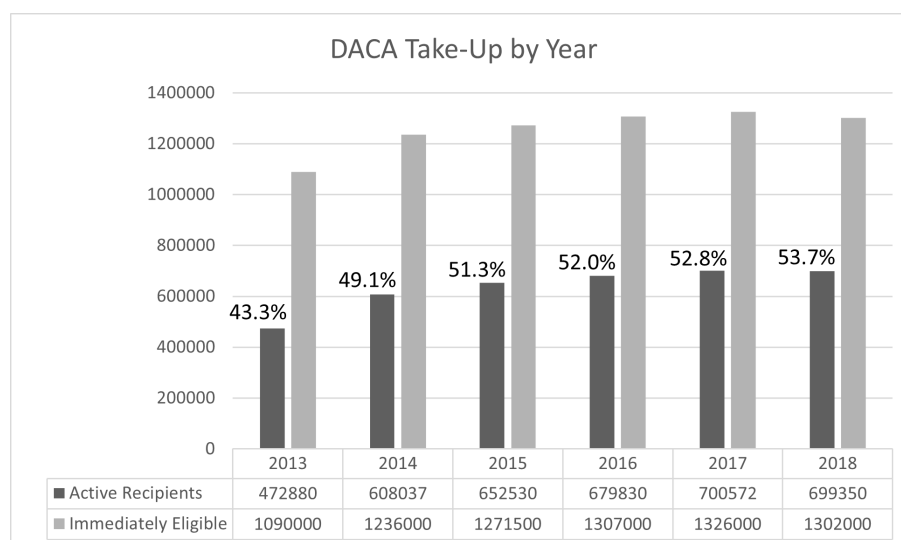


Figure 2.1: DACA Take-Up by Year

Notes: This figure shows a comparison of active DACA recipients and an estimate of immediately eligible individuals. Data on active recipients are from United States Citizenship and Immigration Services (compiled in the Federal Register) and eligibility estimates are from the Migration Policy Institute (MPI) data hub and reports. To my knowledge, MPI did not release an estimate of eligibility for 2015, so a constant growth rate is assumed from 2014-2016.

80% of recipients (558,050) were born in Mexico, suggesting a great deal of shared cultural traits among applicants. A more detailed country-of-origin breakdown appears in Figure 2, panel (a). There is a skewness in gender, with the education requirement as a possible driver, toward females (367,980; 53%), which underscores the importance of considering how the incentives that DACA creates potentially differ for women. A large portion of recipients are from California (200,150; 28.6%) and Texas (115,290; 16.4%). More information about state of residence appears in Figure 2, panel (b). This amplifies the need to test results for robustness

to the inclusion of state fixed effects and trends. The age distribution of recipients is: 16–20: 183,070 (26.2%); 21–25: 255,290 (36.5%); 26–30: 172,030 (24.6%); and 31–36: 86,680 (12.4%). This indicates that a substantial portion of DACA recipients passed through plausible ages for marriage during the study period. Relatedly, at the end of the study period, 560,540 (80.2%) recipients were single, 127,320 (18.2%) were married, and 9,150 (1.3%) were divorced. The magnitude of the latter two numbers makes the probability of being married a significant outcome of DACA to test.

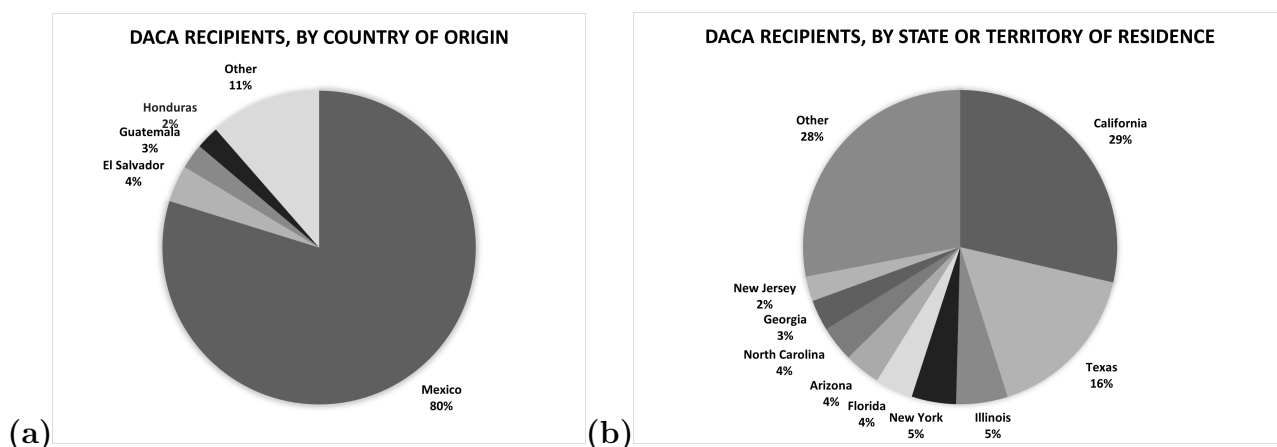


Figure 2.2: DACA Recipients: By Country of Origin and State of Residence

Notes: This figure shows the geographic distribution of DACA recipients. Panel (a) shows country of origin, and Panel (b) shows state of residence. Data are from United States Citizenship and Immigration Services, and are as of August 31, 2018.

Qualitative examination of the DACA target population draws attention to one of the most important features of DACA recipients—they do not appear to have a natural marriage market.⁶ They share characteristics and experiences common to

⁶For detailed qualitative analysis of Dreamers, see Zatz and Rodriguez (2015).

both unauthorized immigrants⁷ (e.g., their parents and other community members) and those who were born and raised in the US⁸ (their US-born siblings and peers). As of 2010, one million children are unauthorized immigrants whose parents are unauthorized immigrants. Of this population, about 400,000 have one or more siblings with birthright citizenship (Taylor *et al.*, 2011). DACA recipients know only American society, yet in some important ways, they are excluded from full participation in it.

2.3 Conceptual Framework

This section describes how DACA potentially affects marriage decisions. Given DACA's provisions, my conceptual framework centers on the roles that reduced deportation risk, expanded labor market opportunities, and greater chances for cultural integration play in inducing more marriages and marriages to people with different immigration statuses. This conceptual framework largely applies to Chapter 3 as well.

In reality, people arrive at marriage decisions by considering many dimensions of their potential partners. And DACA granting benefits as fundamental to an individual's life as deportation relief and work authorization means that it can affect one's characteristics and preferences in various and complicated ways. Hence, any

⁷For example, their parents, who are typically unauthorized immigrants themselves, instinctively avoid contact with authorities, opting not to enroll them in preschool or apply for government programs for which they might be eligible. Similarly, their parents might be fearful of enrolling them in activities that require paperwork, such as field trips or after-school sports. Such parents, with their uncertain work situations, might be forced to move often, switching their children's schools (Suárez-Orozco *et al.*, 2011).

⁸The Supreme Court ruled in *Plyer v. Doe* that all children in the US, of any immigration status, have the right to attend public schools.

modeling of DACA's effect on marriage will necessarily make highly contestable assumptions. There are also many different and countervailing potential effects. Thus, my conceptual framework is merely meant to provide some ideas about the potential effects, rather than taking a strong stance on what the effects actually should be. The ways that DACA might affect marriage and the direction and magnitude of its effect on marriage is ultimately an empirical question.

My framework is one in the style of Becker (1973), where individuals consider the relative costs and benefits of remaining unmarried and marrying people of various immigration statuses. I analyze how DACA potentially changes an individual's ranking of these alternatives. And also – because marriage is a two-sided market – how receiving DACA may change a recipient's marriage value to her potential spouses. The two-sided nature of marriage also implies a distribution of the “surplus” from the marriage divided between the two partners, governed by their relative bargaining power. That being said, the empirical strategy used in this chapter and Chapter 3, as well as much of the framing in this section treat marriage as a one-sided problem. So that is a limitation to this analysis to be acknowledged.

The first provision of DACA that I consider is deportation relief. Clearly, relieving the threat of deportation increases the expected return from marriage. Deportation of one partner effectively ends or at least severely damages a marriage, and from the perspective of potential partners, this is also true. A partner with DACA (i.e., someone highly unlikely to be deported⁹) provides higher expected returns than does a non-citizen partner without DACA. It is possible that less fear

⁹Individuals with DACA can still be deported if they commit a crime, for example.

of deportation causes DACA recipients to participate more in society, exposing them to a greater quantity and variety of potential marriage partners. Exposure to more individuals in the “mainstream” of society potentially changes their characteristics and preferences in a way that makes them more attractive to a larger pool of people, and makes them more attracted to people in that pool. There are strong reasons to believe that deportation relief increases the probability of being married among DACA recipients. Recipients of relief should also tend more toward partners in the US mainstream.

However, when discussing whom a DACA recipient marries, there is a potential countervailing effect to the one described above—decreased legal benefits of marriage. Although a difficult process in some cases, it is possible for an unauthorized immigrant to obtain lawful permanent residence by marrying a US citizen.¹⁰ One benefit of becoming a permanent resident is deportation relief, and thus there is a degree of substitutability between receiving DACA and marrying a citizen. Hence, after receiving DACA, there is less legal incentive for an unauthorized immigrant to marry a US citizen, which contradicts the previously hypothesized move toward more assimilative marriages.

Work authorization might affect DACA recipients’ marriage prospects in a number of ways. First, by expanding their employment opportunities, it could make them more attractive to potential partners through higher and more stable earn-

¹⁰If the unauthorized immigrant entered the US illegally, the process could require exiting the US and coming back years later, or applying for a waiver that demonstrates sufficient hardship to their citizen spouse that would result from deportation. An unauthorized immigrant who entered the US legally and overstayed a visa can marry and obtain a green card through the standard process.

ings, or expected earnings. The next two hypothesized effects are similar to those mentioned for deportation relief. One is that working in legally authorized jobs increases exposure to, and thus lowers search and matching costs in, marriage markets of US natives and authorized immigrants (potentially leading to more assimilative marriages). Another is that such exposure changes the cultural preferences of the DACA recipient, and how potential partners perceive the recipient culturally. These factors might lead to a prediction of DACA increasing the probability of being married, and the probability of marrying more assimilated types of people. However, increased work opportunities also raise the opportunity cost of time spent dating and time-consuming aspects of married life. Similar to deportation relief, becoming a permanent resident through marriage to a citizen yields work authorization, so there is additional substitutability between DACA and marrying a US citizen. These features create countervailing effects, predicted to decrease the probability of being married and the probability of marrying a US citizen.

With work authorization comes a social security number that gives DACA recipients access to banking and credit, which might increase attractiveness to potential partners through the ability to accumulate wealth and borrow against future income. Since marriage is potentially a lifetime decision that requires many upfront (e.g., wedding cost) and future investments (e.g., home purchase and children's education), access to banking and credit allows DACA recipients to optimize intertemporally. Access to banking and credit thus makes marriage more attractive and feasible, and the predicted effect of these benefits is to increase the probability of being married.

Some states include in-state tuition and financial aid for higher education among DACA benefits, which affect a recipient's marriage market opportunities in multiple ways. First, the ability to acquire more human capital increases earnings potential, making a recipient more desirable. Second, acquisition of human capital makes recipients more attractive to other educated individuals, and it might also change their own preferences. More broadly, going to school exposes them to different marriage markets—potentially the American mainstream. These factors should increase both the probability of being married and more assimilative types of marriages. Contrarily, and like work authorization, they increase the opportunity cost of dating and marriage, which decreases the probability of being married. The ability to obtain a driver's license also affects marriage outcomes. An increased ability to transport oneself increases work opportunities, again possibly increasing attractiveness. Increased mobility also decreases search and matching costs in marriage markets, a provision that likely pushes people toward marriage and expands the marriage markets in which they operate.

In combination with responses to DACA above, there are reasons to expect that the effects of DACA are different for men and women. In most economic frameworks of marriage [e.g., Becker (1973), Bertrand *et. al.* (2018), and Shenhav (2021)], relative wages of the sexes affect the returns to marriage through specialization and bargaining power. For example, if women earn less than their potential male marriage partners, the stability from DACA might create an opportunity for them to marry and specialize in home production. Some facts corroborate this tendency. 64% of male DACA recipients are employed, in comparison to 48% of females; 44%

of female DACA recipients are not in the labor force, in comparison to 27% of males (Zong *et al.*, 2017). These facts could alternatively or complementarily be explained by social norms. Following Shenhav (2021), as I do in Chapter 1, these could be modeled in a Becker-style framework as the sexes gaining different utility from spousal income (e.g. men valuing a female spouse’s labor market income less than their own, while women value income from both partners equally). Traditional gender norms have eroded, but disparities remain regarding expectations by sex in the US. In a 2017 survey, 76% of Americans agreed that men face much pressure to support their families financially, and 68% of Americans agreed that they also experience pressure to be successful in their job or career. The percentage of Americans answering *yes* to these same questions regarding women were 40% and 44%, respectively (Parker *et al.*, 2017). Alternatively, expectations of greater responsibilities in home-production might make the trade-off between work and marriage more salient among women. The marriage effect of the increased economic prospects offered by DACA might be diminished for women, and they might be stronger for men. Another potential reason for disparate effects across the sexes is biological constraints; timing of fertility is more salient among women, and thus the cost of delaying marriage is higher for them.

Women might be more risk-averse than men are [Borghans *et al.*, (2009)], and women might prefer, or be expected, to take more responsibility for children if a marriage dissolves by deportation. Hence, it is possible that women are more responsive to deportation risk [Berger Cardoso *et al.*, (2018)]. Alternatively, men are more likely to be deported than women are, so they might, in-turn, be more

responsive to the deportation environment [Kuka *et al.*, (2020)].

A recent trend in advanced countries has been that women have a higher propensity to pursue higher education than men have. This is also true among DACA recipients. In 2017, 20% of female recipients were attending college, in comparison to 15% of males (Zong *et al.*, 2017). Since DACA has been linked to higher educational attainment [Kuka *et al.*, (2020)], this might cause substitutes away from or delay marriage, or place women in different marriage markets than men are in.

2.4 Data

The dataset I constructed is a repeated cross-section of US Hispanics¹¹ of marriage age. It includes their marital status and spousal characteristics, and broader demographics that serve as measures of eligibility for DACA and controls. The dataset also links them to their geographic locations, such as state and MSA, to leverage the policy environment, such as immigration enforcement, and the civil society in which they reside.

The main data source I use is the American Community Survey (ACS), acquired through the Integrated Public Use Microdata Series (IPUMS; Ruggles *et al.*, 2019). The years from the survey I use are 2005 through 2017.¹² These data do not

¹¹Given concerns regarding the validity of potential comparison groups, I limit the samples to only Hispanics.

¹²My sample ends in 2017 because on September 5, 2017, the Trump Administration announced the repeal of DACA. Due to legal challenges of Trump's repeal and the election of President Biden, the repeal did not go through. Nevertheless, uncertainty concerning the program potentially changed behaviors and made DACA a different treatment. Therefore, I remain conservative and avoid post-2017 data.

indicate directly whether an individual applied for DACA. However, a range of information is available that allows researchers to proxy DACA eligibility closely. The data also contain comprehensive information on individuals' marriage behaviors, spousal characteristics, and a rich set of controls.

I proxy for eligibility using several variables. The variable that measures an individual's age at arrival is constructed using data on individuals' age and the number of years they have resided in the US. The variable that indicates whether an individual has lived continuously in the US since June 15, 2007 is constructed using the year of the survey and the number of years they have resided in the US. The variable that measures age as of June 15, 2012 is calculated using year of birth and quarter of birth data. Finally, I proxy for the education requirement using data on individuals' educational attainment.

In addition to variables used to measure eligibility, other variables in the ACS are used as controls, for further sample construction, or to test mechanisms. These can be grouped into several categories. One is demographics, which includes sex, race, ethnicity, and urban status. Another is labor market outcomes, which includes usual weekly hours, whether an individual was employed during the past year, whether an individual was employed at the time of the survey, and annual income. I also use data on individuals' highest level of education and family structure: including the number of children they have and whether they live in a multigenerational household. To construct fixed effects and trends, I use survey year and the state in which a respondent resides.

To address concerns, which I detail in the Empirical Strategy section, regard-

ing local civil society influencing the effectiveness of DACA’s roll out in places that have more eligible individuals, I compiled a list of state and state-year-level controls that proxy social capital levels that can be used in lieu of state fixed effects and trends, since one concern is that they absorb useful identifying variation. These social capital proxies derive from several data sources. The first is the CPS’s civil engagement supplement, which includes state-level estimates of organization membership, meeting participation, protests, news-following, and political discussions with friends and family. From the 2010 US religion census, I construct state-level estimates of congregations per capita and adherents per capita, and both of those measures for Catholics, since Hispanics are mostly Catholic (ASARB, 2011). The Catholic Church and the Catholic Charities USA are among the largest and most influential non-profit service providers for immigrants, operating in concert with local Catholic congregations to provide services (Mooney, 2009). I also include the state-level Social Capital Index, developed by the Social Capital Project of the United States Congress Joint Economic Committee (JEC, 2018). The Social Capital Project also provides a formatted version of the December 2015 Internal Revenue Service Business Master File, which includes state-level estimates of membership organizations per 1000 people, and non-religious nonprofits per 1000 people. Finally, I use ACS data to construct state-year levels of the percentage of non-citizens and marriage rates for Hispanics age 18 to 35.

The primary outcome of interest, an indicator that equals 1 if the individual is married, is constructed using data on respondents’ marital statuses. One advantage to using this stock variable, as opposed to a flow variable, is that it captures both

inflows and outflows of marriage. Since the population is young, sufficient unmarried individuals are in the sample on whom DACA could have a substantial influence on the stock of married individuals.

During mechanisms analysis, I proxy for individuals' deportation risk, and I then assess how the marriage effect of DACA differs from deportation risk. I proxy for deportation risk in two ways. First, I construct a state-level measure that is similar to Kuka *et al.* (2020). I use state-wide interior removals from the pre-period of 2005 to 2011, and take the ratio of that to the Hispanic population age 18 to 35 during that period. This represents how many people are removed relative to the Hispanic population, calculated from the ACS. Removal data are from the Transactional Records Access Clearinghouse, maintained at Syracuse University.¹³ Second, I follow a series of papers¹⁴ when constructing an MSA-level index of immigration enforcement intensity. Data used to construct this index were obtained from the Urban Institute's collection state and local immigration policy resource, which provides activation and deactivation dates for state and local policies. These policies include local 287(g) agreements, state-level 287(g) agreements, Secure Communities, omnibus immigration laws, and E-Verify.¹⁵

The index is constructed following [Amuedo-Dorantes *et al.* (2020)]. For each of the five aforementioned policies, a measure of exposure is taken at the county level, and then aggregated to the MSA level, weighted by each county's population

¹³Data were obtained from <http://trac.syr.edu/phptools/immigration/removehistory/>.

¹⁴Including Amuedo-Dorantes *et al.* (2020), Amuedo-Dorantes *et al.* (2018), Amuedo-Dorantes and Arenas-Arroyo (2019), and Amuedo-Dorantes and Arenas-Arroyo (2021).

¹⁵Information on when these policies were active across locations was obtained from <https://www.urban.org/features/state-immigration-policy-resource>.

and the portion of months in a year that a policy was active in the county. For policy k , MSA m , year t , and all counties c in m :

$$EI_{m,t}^k = \frac{1}{N_{m,2000}} \sum_{c \in m} \frac{1}{12} \sum_{j=1}^{12} \mathbb{I}(E_{c,j}^k) P_{c,2000} \quad (2.1)$$

where $\mathbb{I}(E_{c,j}^k)$ is an indicator function that equals 1 if policy k was active in county c in month j during year t , $P_{c,2000}$ is the population of county c during 2000, and $N_{m,2000}$ is the population of the MSA during 2000. Populations are taken from the 2000 Census.

Indices for each policy are summed at the MSA-year level, yielding an index that proxies for the overall immigration enforcement intensity to which an individual living in MSA m during year t is exposed. This index (below), ranges from zero (i.e., no policy was active in that MSA-year) to 5 (i.e., all policies were active for the entire year).

$$TotalEnforcement_{m,t} = \sum_{k \in K} EI_{m,t}^k \quad (2.2)$$

I also study spouses' characteristics, conditional on an individual being married. These characteristics include citizenship status, US nativity, being a foreign-born citizen, and English fluency, constructed using information on spousal characteristics in the ACS on IPUMS (Ruggles *et al.*, 2019). Data described in this section

are used to implement a difference-in-differences strategy, which I detail in the next section.

2.5 Empirical Strategy and Diagnostic Results

The treatment of interest in this study is DACA eligibility, and the strategy used to identify the effect of this treatment is difference-in-differences. The ideal treatment group are DACA recipients, whom I cannot identify directly, given data constraints. The primary task, therefore, is constructing a treatment group whose inclusion criteria best proxy DACA eligibility over time. The task that follows, then, is constructing an appropriate comparison group. Since the DACA population has salient features that make its members simultaneously similar and dissimilar to both other immigrants and US natives, often in conflicting ways, there is no obvious comparison group for them. Left unresolved, these sample selection issues would render formal implementation of the empirical strategy useless. I summarize how I chose the comparison group, the full reasoning for which appears in Appendix C. The conclusion I reach is that no one way of evaluating comparison groups is flawless, but in their totality, the best comparison group is foreign-born citizens. I show in the Results section that most results in this paper are qualitatively similar across comparison groups.

When considering DACA requirements, the most important unobservable, and therefore the one that must be proxied, is unauthorized status. I proxy for this by including only non-citizens in the treatment group. At the June 15, 2012 announce-

ment of the DACA policy, a person must have been under 31 years old and had been residing in the US for at least 5 years to be eligible. Additionally, that person must have entered the US before the age of 16, and have at least the equivalent of a high school education, or be in school.

Construction of the treatment group commenced as follows. To capture the age at announcement requirement, I limit the sample to individuals who are 30 or younger. I impose a lower-bound age of 18, since in most states, a person must be 18 or older to get married without parental consent. I proxy for the requirement of having resided in the US for 5 years at the announcement by limiting the treatment group to those who have resided in the US for at least 5 years by June of the year prior to their ACS survey year. The age and time-residing requirements are proxied in reference to the ACS survey year, rather than DACA's actual announcement, because coding the requirement in reference to DACA's announcement makes keeping comparisons between treatment and comparison groups equitable throughout the sample period infeasible. For example, it would be difficult to argue that individuals treated by DACA who were 10 years old in 2005 are comparable to treated individuals who were 35 in 2017. During survey years 2005 to 2007, there are individuals who would not have entered the US at the time those surveys were taken, but then would eventually enter the US and be eligible for DACA. Hence, the treatment group is the population of Hispanic non-citizens, age 18 to 30, who entered the US before the age of 16, have resided in the US for at least 5 years, and have at least a High School equivalent education, or are in school.

For this DiD strategy to be valid, I must select a comparison group that pro-

vides an accurate counterfactual regarding trends to the DACA-eligible group if its members did not receive treatment. As already mentioned, there is no obvious comparison group. Eligibility requirements regarding age, age of entry, and years residing in the US make potential non-citizen comparison groups older than and/or having entered the US later than the eligible group by construction. Using Hispanic citizens as a comparison group alleviates these mechanical problems, but there is good reason to believe that citizens are different in ways that affect marriage behaviors. Since marriage is a two-sided matching market, in which individuals might consider anything about potential matches, there are many channels through which the parallel trends assumption might be violated. Marriage markets can be affected by labor markets, financial markets, government policies, and aspects of social life that fall outside of those domains. During the years leading up to and after DACA, secular marriage trends changed rapidly, and those changes differed across age [Wang (2018)] and demographic groups [Raley *et al.* (2015)]. The US economy was in recovery from the Great Recession, which had heterogeneous influences across demographic groups [Hoynes *et al.* (2012)]. Therefore, the ideal comparison group is one whose members have similar ages, cultures, and economic opportunities to be DACA eligible. The comparison group I use during a preferred analysis is foreign-born citizens. Part of my rationale for using this group follows an argument from Kuka *et al.* (2019), who highlight that like DACA recipients, foreign-born citizens have legal rights to remain in the US and work, and to have access to immigrant and US citizen networks.

For the foreign-born citizens comparison group, I impose the same sample re-

strictions as for the eligible group. This group is then Hispanic foreign-born citizens, age 18 to 30, who entered the US before the age of 16, have resided in the US for at least 5 years, and have at least a High School equivalent education, or are in school.

The treatment and preferred comparison group described above are presented more succinctly in Table 1. I also include the alternative comparison groups I considered and use for alternative outcomes and robustness checks in the Results section. All groups are limited to Hispanics that meet the education requirements of DACA.

Group:	Eligible	Foreign-Born Citizens	Citizens	Ineligible by Age of Entry Only
Citizenship	No	Yes	Yes	No
Age	18-30	18-30	18-30	18-30
Age of Entry	0-15	0-15	No restriction	≥ 16
Years Residing in US	≥ 5	≥ 5	No restriction	≥ 5
Birthplace	Not US	Not US	No restriction	Not US
Education	$\geq$ HS or in school	$\geq$ HS or in school	$\geq$ HS or in school	$\geq$ HS or in school

Notes: This table describes the sample restrictions of the eligible group and each potential comparison group. All groups are Hispanics. Additionally, there is a RD-style version of “Non-Citizens, Ineligible by Age of Entry Only” that further limits the sample to those who entered the US between the ages of 12 and 19.

Table 2.1: Treatment and Comparison Groups, Sample Restrictions

In Table 2, I compare the observables of the eligible group and the foreign-born citizens comparison group. Their marriage rates are similar, regardless of stock (28.2% for the eligibles vs. 30.1% for foreign-born citizens) and flow (4.7% vs. 4.5%). Adding evidence to their similar behaviors regarding romance, the two groups also have similar unmarried cohabitation rates (6.1%).

Crucially, age and age of entry into the US are similar across the two groups. The age similarity (23.97 versus 24.582 years) is important because a person’s propensity to marry inherently differs throughout the lifecycle. Cohorts in the US have also had different trends in marriage during recent decades [Wang (2018)]. I

highlight age of arrival (7.84 versus 6.12 years) because it has been shown to be a determinant of individual assimilation, including a determinant of immigrant marriage behaviors. For example, Bleakley and Chin (2010) find that through its effect on English language skills, which is a measure of assimilation, an earlier US arrival age decreases the probability of marriage and increases the probability of inter-ethnic marriage.

An important difference between groups for the current empirical strategy concerns education. 56.5% of the eligible group's highest educational attainment is high school, in comparison to 32.6% for the foreign-born citizens group. It is well-documented that there have been diverging marriage trends across education levels [Parker and Stepler (2017)].

	(1) Eligible	(2) Foreign-Born Citizens	(3) Difference
Married	0.282 (0.002)	0.301 (0.002)	-0.019 (0.002)
Married during last year	0.047 (0.001)	0.045 (0.001)	0.002 (0.001)
Cohabiting	0.061 (0.001)	0.061 (0.001)	-0.000 (0.001)
Female	0.479 (0.002)	0.532 (0.002)	-0.052 (0.003)
Black	0.010 (0.000)	0.025 (0.001)	-0.015 (0.001)
Asian	0.003 (0.000)	0.008 (0.000)	-0.005 (0.000)
Age	23.970 (0.013)	24.582 (0.016)	-0.612 (0.021)
Age of entry into US	7.843 (0.016)	6.123 (0.020)	1.720 (0.026)
Hours per week	28.089 (0.063)	29.932 (0.076)	-1.843 (0.099)
Worked during past year	0.768 (0.001)	0.825 (0.002)	-0.057 (0.002)
Working	0.673 (0.002)	0.721 (0.002)	-0.048 (0.003)
Income	15536.516 (62.115)	20773.806 (96.991)	-5237.290 (109.771)
GED	0.037 (0.001)	0.025 (0.001)	0.012 (0.001)
High school diploma	0.565 (0.002)	0.326 (0.002)	0.239 (0.003)
Some college	0.377 (0.002)	0.482 (0.002)	-0.105 (0.003)
College degree	0.058 (0.001)	0.192 (0.002)	-0.134 (0.002)
<i>N</i>	84409	54575	138984

Notes: This table reports summary statistics for the eligible and comparison group with foreign-born citizens. Numbers reported are means, with standard errors in parentheses. The difference between the groups appears in Column (3). Restrictions for each sample are defined in Table 1.

Table 2.2: Summary Statistics by Eligibility

In addition to the observables, I conduct a pre-trends analysis. First, I plot regression-adjusted marriage rates for the eligible and ineligible groups over time. Second, I conduct an event-study analysis, estimating an effect for each year leading up to and after DACA (the event) and then showing the results both graphically and in table form. This provides insights into whether the treatment-comparison group combination satisfies the parallel trends assumption. There is also reason to suspect that men’s and women’s marriage trends, and corresponding responses to DACA, differ due to various social and economic influences.¹⁶ I thus conduct this analysis separately by sex.

I conduct a regression-adjusted mean analysis in the following way. I plot the mean of the marriage indicator for eligible and ineligible groups over time, with a line marking year 2012, the announcement year. I regression-adjust for a minimal set of controls—age and age of entry into the US. The former drives marriage strongly through lifecycle effects, and the latter proxies a great deal of unobserved heterogeneity that might exist between the two groups. For example, immigrants who arrived in the US at a younger age potentially has many shared cultural markers with native-born citizens that cannot be observed in the data. These controls are minimized to demonstrate that the validity of the DiD strategy is largely independent of controls. Further controls can thus be included to test for robustness.

On samples of each treatment-comparison combination, I estimate:

$$\text{Married}_i = \alpha + \beta_1 \text{Eligible}_i + \sum_{y=2005}^{2017} \beta_y \text{Year}_i + \sum_{y=2005}^{2017} \beta_y^E \text{Year}_i * \text{Eligible}_i +$$

¹⁶I discuss these in further detail in the Conceptual Framework, and later in this section.

$$\beta_A Age_i + \beta_{AE} Age of Entry_i + \varepsilon_i$$

I calculate the average for eligibles as:

$$Married_y^E = \hat{\alpha} + \hat{\beta}_1 + \hat{\beta}_y + \hat{\beta}_y^E + \hat{\beta}_A \bar{Age} + \hat{\beta}_{AE} Age of \bar{Entry} \quad (2.3)$$

I calculate the average for the comparison group similarly:

$$Married_y^I = \alpha + \beta_y Year_i + \beta_A Age + \beta_{AE} Age of \bar{Entry} \quad (2.4)$$

In Figures 3 and 4, panel (a), I show plots for men and women, respectively.

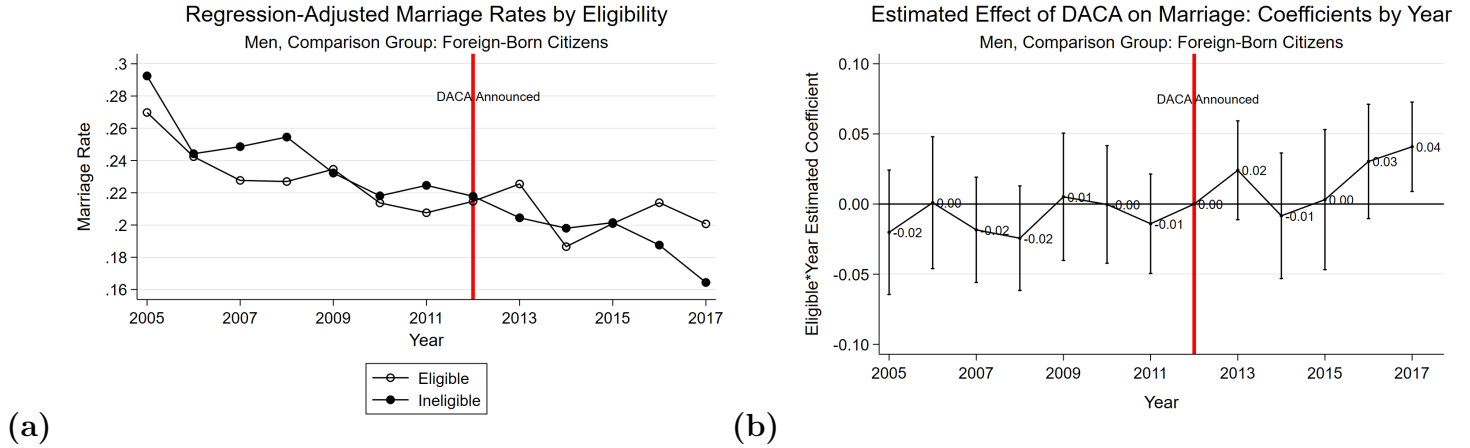
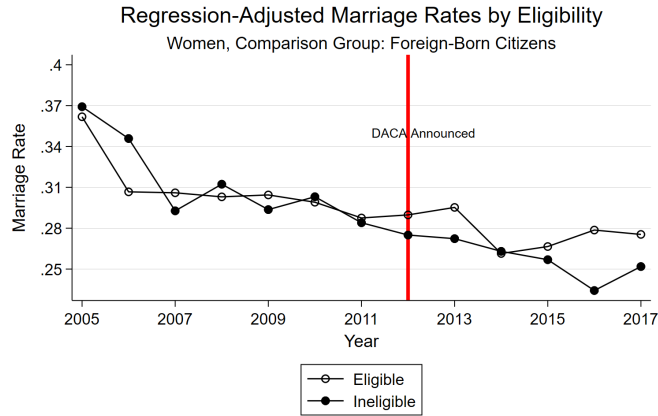
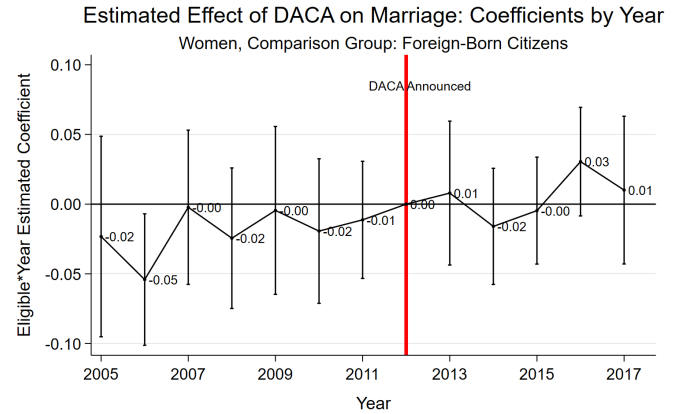


Figure 2.3: Men: Trends by Eligibility

Notes: This figure compares regression-adjusted (for age and age of entry into the US) marriage trends of the eligible group and comparison group that consist of foreign-born citizens. Panel (a) shows each group over time separately. Panel (b) plots the difference between the groups over time, with 95% confidence intervals.



(a)



(b)

Figure 2.4: Women: Trends by Eligibility

Notes: This figure compares regression-adjusted (for age and age of entry into the US) marriage trends of the eligible group and comparison group that consist of foreign-born citizens. Panel (a) shows each group over time separately. Panel (b) plots the difference between the groups over time, with 95% confidence intervals.

Among men, eligibles trend similarly to the foreign-born citizen comparison group, and during the post-DACA period, there appears to be a structural change operating through eligible individuals. In each year of the pre-DACA period, the eligible mean is below, but close to, the ineligible mean. After DACA is announced, the eligible mean is, with 2014 an exception, above the ineligible mean, and increasingly so over time. This accords with a change in marriage rates among eligibles that was caused by DACA. ¹⁷

Like among men, women eligibles trend similarly to the foreign-born citizen

¹⁷I report analogous results for alternative comparison groups in panel (a) Appendix Figures 12 and 14 for citizens and non-citizens comparison groups, respectively. The comparison group that uses all Hispanic citizens tracks to the eligibles similar to foreign-born citizens, but slightly and non-significantly above the eligible group during the pre-period. In the non-citizens sample, in which the comparison group is ineligible only by age of entry, it appears that throughout the pre-period, ineligible's marriage rate declines while eligibles' plateaus. In this sample, there appear to be pre-trends. Thus, this graphical analysis demonstrates foreign-born citizens being a better comparison group for DACA-eligible men.

comparison group during the pre-period. They then diverge from them during the post-period, and then rise above them during the post-DACA period ¹⁸

I extend this exercise to an event-study analysis, estimating:

$$Y_{it} = \beta_0 + \sum_{y=2005}^{2017} \beta_y Year_i + \sum_{y=2005}^{2017} \beta_y^E Year_i * Eligible_i + \beta_1 Eligible_i + \beta_2 A_{it} + \varepsilon_{it}$$

where A_{it} is a vector that includes age and age of entry controls. The omitted year is 2012, the announcement year. Results for foreign-born citizens are reported graphically for men and women in Figure 3 panel (b) and Figure 4 panel (b), respectively. Analogous results for comparison groups with all citizens and non-citizens appear in panel (b) of Appendix Figures 12 and 14 for men, and in panel (b) of Appendix Figures 13 and 15 for women. For each sex-comparison group combination, I plot the coefficient for $Year_i * Eligible_i$. The value-added of these figures, relative to panel (a) of Figures 3 and 4, is that they allow the annual effect size to be seen more easily, and they provide information on the estimates' precision. The estimates are noisy. Point estimates lead to the same conclusions as their panel (a) counterparts, but they highlight that these conclusions come with uncertainty, further highlighting the importance of multiple approaches when evaluating comparison groups. Table 3 reports these results in table form.

Shown in Table 3, Column (1), during the pre-period, estimates for men oscil-

¹⁸Among women, the comparison group that appears to most convincingly lack pre-trends is also foreign-born citizens. In each other sample [presented in panel (a) of Appendix Figures 13 and 15], and to varying degrees, it appears that the marriage rate of the ineligible group trends downward more rapidly than eligibles' marriage rate before DACA's announcement. This is a concern because such trends, were they to continue into the post-DACA period, would bias results away from zero. Graphical analysis thus suggests that foreign-born citizens represent the best comparison group for DACA-eligible women.

late at and below zero, before rising above zero, eventually significantly.¹⁹ Among women, shown in Column (2), coefficients for the foreign-born citizens comparison group remain steady below zero before DACA, and then increase after DACA, but are never statistically significant.

	(1) Men	(2) Women
Eligible*2005	-0.0201 (0.0221)	-0.0232 (0.0358)
Eligible*2006	0.000948 (0.0234)	-0.0541** (0.0235)
Eligible*2007	-0.0184 (0.0187)	-0.00225 (0.0276)
Eligible*2008	-0.0244 (0.0186)	-0.0245 (0.0251)
Eligible*2009	0.00514 (0.0226)	-0.00452 (0.0300)
Eligible*2010	-0.000297 (0.0209)	-0.0193 (0.0258)
Eligible*2011	-0.0140 (0.0176)	-0.0113 (0.0209)
Eligible*2013	0.0241 (0.0175)	0.00792 (0.0257)
Eligible*2014	-0.00838 (0.0223)	-0.0160 (0.0207)
Eligible*2015	0.00314 (0.0249)	-0.00466 (0.0191)
Eligible*2016	0.0304 (0.0203)	0.0305 (0.0194)
Eligible*2017	0.0408** (0.0159)	0.0101 (0.0264)
Age controls?	Yes	Yes
Age of entry controls?	Yes	Yes
Year fixed effects?	Yes	Yes
State fixed effects?	No	No
State trends?	No	No
Observations	66651	67060
R ²	0.121	0.110

Notes: This table contains an event study analysis of the likelihood of being married. The specification includes interactions between year indicators and Eligible. 2012 is the omitted interaction. The samples contain Hispanics of one sex with at least a high school diploma, age 18*-30. The treatment group is non-citizens who entered the US before the age of 16 and who have resided in the US for at least 5 years. The comparison group is foreign-born citizens who are otherwise the same as the treatment group. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3: The Effect of DACA Eligibility on Marriage: Event Study Analysis

I now discuss the main specification that I will be estimating. I estimate a treatment effect by comparing changes before and after the 2013 implementation of DACA for the outcome of interest in the treatment group and a particular

¹⁹ Across all tested comparison groups for both sexes, the coefficient for 2014 is negative.

comparison group. Since treatment begins for all treated individuals at the same time (i.e., 2013 implementation of DACA), the estimating equation is in a classic difference-in-differences form. My identifying assumption is a conditional parallel trends assumption: that in absence of the treatment, the treatment and comparison group would follow the same trends after conditioning on covariates. Though my treatment and comparison groups are constructed differently, my estimating equation is similar to Pope (2016). I estimate regressions:

$$Y_{it} = \beta_0 + \beta_1 \text{Eligible}_{it} * \text{After}_{it} + \beta_2 \text{Eligible}_{it} + \beta_3 \text{After}_{it} + \beta_4 X_{it} + \beta_5 A_{it} + \beta_6 S_s + \beta_7 P_{st} + \theta_t + \gamma_s + \gamma_s \times t + \varepsilon_{it}$$

where Y_{it} is the outcome of interest. In this paper, the main outcome is an indicator that equals 1 if an individual is married. I also estimate models in which the outcome is an indicator that equals 1 if an individual's spouse is of a certain type (e.g., a citizen or a US native). X_{it} is a vector of demographic, geographic, economic controls. A_{it} is a vector that controls for age at the time of the ACS survey, and age during the year the individual entered the US. For those born outside of the US, age of entry is the age during the year they immigrated. For US natives, the age of entry is zero. S_s and P_{st} are state-level and state-year level controls that address the civil society concerns discussed in the Data section. S_s is omitted when state fixed effects γ_s are included. The three terms that precede the error term are year fixed effects, state fixed effects, and state trends, respectively. The coefficient of interest is β_1 , the lower-bound intent-to-treat (ITT) effect of DACA eligibility.

β_1 represents an intent-to-treat effect because the variable proxied is DACA eligibility, rather than DACA enrollment. Discussed previously, DACA uptake is

unobserved. The effects of DACA should largely operate through those who enroll in the program. Thus, the treatment-on-the-treated effects are likely larger. I argue the effect is likely a lower-bound of the ITT because I observe only whether an individual is a citizen; I do not observe whether the individual is an unauthorized immigrant. There will thus be individuals in the treatment group who are untreated because they are authorized immigrants, biasing estimates toward zero. Pope (2016) scales estimates by considering this bias. He uses a combination of census estimates (Acosta *et al.*, 2014) of the non-citizen population and DHS estimates (Baker and Rytina, 2013) of the unauthorized population to determine that approximately 60% of non-citizens in his sample are unauthorized. This leads to a rough calculation that the true ITT effects are 1.6 times larger than those he obtains. However, my sample differs from his. First, mine contains individuals 18 to 30, and his also contains individuals 31 to 35. Second, mine contains only Hispanics, while his does not make that restriction. The data tables he uses do not contain information on my specific sample. However, the proportion of undocumented immigrants in an 18 to 30 group is not expected to differ markedly from an 18 to 35 group. Third, Latin American immigrants are unauthorized disproportionately in comparison to other groups (Acosta *et al.*, 2014; Passel and Cohn, 2019). This means that my sample is diluted less by authorized immigrants than is his. Thus, a better scaling of my estimates reasonably places the true ITT effect between 1 and 1.6 times the DiD estimates.

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²⁰In response to recent developments in difference-in-differences literature [e.g. Goodman-Bacon

I also estimate specifications in which *Eligible * After* is interacted with another variable, including other appropriate interactions to saturate the model. I do so for two reasons that are analyzed best with a similar functional form.

The first type of interaction specification is used to explore sex differences of results in a sample that pools the sexes. I interact *Eligible * After* with a female indicator variable. Doing so facilitates formal hypothesis testing of sex differences in a higher-powered specification.

The foreign-born citizens group has more females than the eligible group does (53.2% versus 47.9%). Such differences could be resolved by simply controlling for sex, but there are other sex-related concerns that would not be resolved by doing so. Thus, in my preferred estimates, I estimate the model on male- and female-only samples. In the Conceptual Framework section, I highlight theoretical reasons for which the results might differ by sex, and thus they should be thought of as different parameters of interest. There are also econometric reasons to estimate the models separately by sex. When the sexes are pooled in the same sample, there is greater potential for spillover. Individuals in the treatment group might be marrying individuals in the comparison group. Relatedly, if an individual's spouse is also in the sample, error terms certainly correlate.²¹

(2021); de Chaisemartin and D'Haultfoeuille (2022); Sun and Abraham (2021); and Callaway and Sant'Anna (2021)], I clarify that the standard parallel trends assumption is sufficient in my case. First, it must be the case that groups' treatment can only increase over time and can change at most once. My treatment group is defined as Hispanic non-citizens, age 18 to 30, who entered the US before the age of 16, have resided in the US for at least 5 years, and have at least a High School equivalent education, or are in school. This treatment group becomes treated in 2013, and its treatment status never subsequently changes. Second, the treatment is binary, which DACA eligibility is. Third, there is no variation in treatment timing; treatment begins in 2013 for all eligible individuals [de Chaisemartin and D'Haultfoeuille (2022)].

²¹Same-sex marriage represents a sufficiently small portion of marriages in the US to be a negligible part of the sample, so separating samples by sex largely eliminates the problem of

Halving the sample incurs a significant loss to statistical power, and thus I also estimate models using samples that pool the genders, but I allow the effects of DACA to differ by gender. This still leaves some of the previously discussed issues unresolved, but it allows for estimation of a separate parameter for females and is a straightforward way to conduct a formal test of gender differences..

The second type of specification when interacting *Eligible * After* with other variables allows exploring the mechanisms through which marriage results are driven. The two main mechanisms I explore correspond to the two main benefits of DACA—deportation relief and work authorization. I do so using proxies for deportation risk and employment prospects.

To proxy for deportation risk, I use 1) state-level ratios of deportations to deportation-aged Hispanics and 2) a measure of local immigration enforcement intensity, a measure of which common immigration enforcement policies were active in the MSA of the individual.²² To proxy employment prospects, I use a propensity score of employment among Hispanic citizens during the pre-DACA period.²³²⁴ Since DACA grants recipients similar employment rights to a citizen, I use a measure of employment prospects for an individual who is an observationally equivalent

within-sample marriage.

²²See the Data section for more details.

²³The propensity score is generated from a probit with an employment dummy on the left-hand side. The right-hand side of the probit includes the age, age of entry, education, demographics, and social capital controls included in Equation (11). The right-hand side also includes state-year characteristics for the state in which the individual resided, including age 18 to 65 unemployment and labor force participation rates. It also includes shares of workers age 18 to 65 in agriculture, fishing or hunting, construction, and manufacturing. Finally, the right-hand side also includes the individual's household structure variables, comprising of the number of family members in the household, family size, non-family members in the household, and an indicator of whether they live in a multi-generational household.

²⁴Using only pre-period data to generate propensity scores precludes me from using year fixed effects or trends.

citizen during the pre-DACA period to proxy for exposure to potential employment effects of DACA for individuals in the sample. In these regressions, the specific variable I use is an indicator that equals 1 if an individual is above the median of the propensity score into employment.

The equation I estimate to explore mechanisms and pool the sexes in one model is:

$$Y_{it} = \beta_0 + \beta_1 \text{Eligible}_{it} * \text{After}_{it} + \beta_2 \text{Eligible}_{it} * \text{After}_{it} * \text{Mechanism}_{it} + \beta_3 \text{Eligible}_{it} * \text{Mechanism}_{it} + \beta_4 \text{After}_{it} * \text{Mechanism}_{it} + \beta_5 \text{Eligible}_{it} + \beta_6 \text{After}_{it} + \beta_7 \text{Mechanism}_{it} + \beta_8 X_{it} + \beta_9 A_{it} + \theta_t + \gamma_s + \gamma_{st} + \varepsilon_{it}$$

where Mechanism_{it} is the proxy for the mechanism being explored, or, in the case of testing for heterogeneity by sex, an indicator that equals 1 if the individual is female.

These standard DiD samples might be vulnerable to violations of the parallel trends assumption. I therefore estimate Equations (11) and (12) using samples that incorporate regression-discontinuity elements, as Pope (2016) does. The intuition for regression discontinuity-style estimation is that if individuals are otherwise eligible, those near the threshold of an eligibility requirement for DACA, on either side, should be similar in both observable and unobservable ways. By implication, I expect that parallel trends are more likely to hold near thresholds. Like in the standard non-citizens sample, individuals are ineligible only by age of entry.²⁵ I

²⁵Following Pope (2016), I do not condition on meeting the requirement that an individual resided continuously in the US for more than 5 years. This variable is subject to measurement error, and it is sufficiently small that approaching the threshold reduces the sample drastically. Since this contaminates the eligible group with ineligible people, this decision biases estimates toward zero.

refine the sample closer to the age of entry eligibility threshold. This sample includes Hispanic non-citizens, age 18 to 30, with at least a high school diploma, who entered the United States between the ages of 12 and 19. The RD-element comes from the fact that this sample includes only individuals who entered the US between the ages of 12 to 19, close to the age 16 threshold. I consider this a complementary analysis because it is underpowered relative to the baseline sample.

2.6 Main Results

2.6.1 Marital Status

Preferred estimates come from estimating Equation (11) separately for men and women. Discussed previously, these estimates use foreign-born citizens as the comparison group. I test their robustness across samples and specifications.

Results for men are reported in Table 4. My baseline DiD estimate of the lower-bound effect of DACA eligibility on the probability of being married, reported in Column (1), is 2.67 percentage points. Given the mechanical importance of age to marriage, and the importance of age of arrival to social outcomes, I include age and age of arrival controls. In Column (2), I include the state-year marriage rate for Hispanics age 18 to 35, which reduces the point estimate to 2.33 percentage points. In Column (3), inclusion of demographic (i.e., race and urban status) and social capital controls, other than the local marriage rate, including various measures of local and state political participation, religion, and non-citizen presence, reduces the coefficient to 2.21 percentage points. In Columns (4) and (5), I include year fixed

effects and state fixed effects, respectively. The effect size progressively reduces to 2.14 percentage points. In Column (7), I include state trends, which increases the effect to 2.4 percentage points. Results reported in Columns (2) through (6) suggest that they are stable to the inclusion of many controls. These lower-bound estimates range approximately between 2 and 3 percentage points. Since the dependent variable mean among eligibles is 23%, a 2 to 3 percentage point lower-bound ITT is sizable, reflecting at least a 9% to 13 % increase to marriage rates among those treated.

I report the same set of results in Table 5 for the analogous sample of women. Again, including only age and age of arrival controls in Column (1) yields an estimated effect of 2.23 percentage points. Including local marriage rate for Hispanics age 18–35 reduces the estimate to 1.91 percentage points [Column (2)]. Demographic and non-marriage-rate social capital controls in Column (3) reduce the estimate slightly to 1.84 percentage points. Estimates remain insensitive to other controls. In Columns (4) and (6) year fixed effects and state fixed effects are included sequentially, and the point estimate remains approximately 1.8 percentage points. The inclusion of state trends increases the estimate to 2 percentage points [Column (6)]. A 2 percentage point lower-bound ITT, on a base eligible marriage rate of 31%, represents a 6.45% increase.²⁶

To place the size of these effects into context, I discuss other marriage results from the literature [Table 6]. These estimates come from a variety of contexts,

²⁶A potential reason that the marriage rate among women is higher than among men in these samples is that during 2017, Hispanic men’s median age of first marriage was 29.8, in comparison to Hispanic women’s median of 27.5 (Payne, 2019). Recall that the samples have an upper bound of age 30.

	(1)	(2)	(3)	(4)	(5)	(6)
Eligible*After	0.0267*** (0.00804)	0.0233*** (0.00860)	0.0221** (0.00860)	0.0218** (0.00859)	0.0214** (0.00864)	0.0240*** (0.00893)
Age controls?	Yes	Yes	Yes	Yes	Yes	Yes
Age of entry controls?	Yes	Yes	Yes	Yes	Yes	Yes
Marriage Rate: Hispanics Age 18-35	No	Yes	Yes	Yes	Yes	Yes
Demographic and social capital controls?	No	No	Yes	Yes	Yes	Yes
Year fixed effects?	No	No	No	Yes	Yes	Yes
State fixed effects?	No	No	No	No	Yes	Yes
State trends?	No	No	No	No	No	Yes
Dep Var Mean	0.24	0.24	0.24	0.24	0.24	0.24
Dep Var Mean: Eligibles	0.23	0.23	0.23	0.23	0.23	0.23
Observations	66651	66651	66651	66651	66651	66651
R ²	0.119	0.128	0.132	0.132	0.134	0.135

Notes: This table has DiD results for the effect of DACA eligibility on the probability its recipients are married. The outcome variable is an indicator for whether the individual is married. The sample contains Hispanic women with at least a high school degree, ages 18-30. The treatment group is non-citizens that entered the US before the age of 16 and have resided in the US for at least 5 years. The comparison group is identical except for being foreign-born citizens. More controls are added in each column: they include age, age of entry in the US, state-year marriage rate for Hispanics age 18-35, race, urban, social capital variables, year fixed effects, state fixed effects, and state trends. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: The Effect of DACA Eligibility on the Probability of Being Married, Men

	(1)	(2)	(3)	(4)	(5)	(6)
Eligible*After	0.0223** (0.00884)	0.0191** (0.00832)	0.0184** (0.00838)	0.0183** (0.00830)	0.0179** (0.00789)	0.0200*** (0.00742)
Age controls?	Yes	Yes	Yes	Yes	Yes	Yes
Age of entry controls?	Yes	Yes	Yes	Yes	Yes	Yes
Marriage Rate: Hispanics Age 18-35	No	Yes	Yes	Yes	Yes	Yes
Demographic and social capital controls?	No	No	Yes	Yes	Yes	Yes
Year fixed effects?						
State fixed effects?	No	No	No	Yes	Yes	Yes
State trends?	No	No	No	No	Yes	Yes
Dep Var Mean	No	No	No	No	No	Yes
Dep Var Mean: Eligibles	0.33	0.33	0.33	0.33	0.33	0.33
Observations	0.31	0.31	0.31	0.31	0.31	0.31
R ²	67060	67060	67060	67060	67060	67060
r ²	0.108	0.119	0.124	0.124	0.127	0.128

Notes: This table has DiD results for the effect of DACA eligibility on the probability its recipients are married. The outcome variable is an indicator for whether the individual is married. The sample contains Hispanic women with at least a high school degree, ages 18-30. The treatment group is non-citizens that entered the US before the age of 16 and have resided in the US for at least 5 years. The comparison group is identical except for being foreign-born citizens. More controls are added in each column: they include age, age of entry in the US, state-year marriage rate for Hispanics age 18-35, race, urban, social capital variables, year fixed effects, state fixed effects, and state trends. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: The Effect of DACA Eligibility on the Probability of Being Married, Women

including access to contraception and abortion, women’s wages relative to men’s, male incarceration, and immigration enforcement. The magnitudes of these effect sizes range from a 0.36 to 5.1 percentage points effect on marriage. Despite being ITT lower bounds, current results lie within this range, and thus I argue them to be plausible and noteworthy in size by convention. However, my effects are larger when discussed in percentage terms in comparison to the population mean. Estimates in the literature range from 0.7% to 7.9%. My effect sizes are as large as 13% among men, which is larger than that reported in the literature, underscoring the economic significance of these effects, but they are not sufficiently large to render them implausible.

Paper	Effect/Policy of Interest	Population	Estimate PP	Percent terms
Angrist and Evans (1996)	3 years of exposure to liberalized abortion laws	Teen white women	-2.9	-5.2
Shenhav (2021)	10% increase in women’s wages relative to men	Women ages 18-64	-5.1	-7.9
Goldin and Katz (2002)	Pill access before age 21	Women ages 30-49	-0.8	-1.1
Goldin and Katz (2002)	Legalized abortion at age 18	Women ages 30-49	-2.9	-4.0
Charles and Luoh (2010)	1 SD increase of male incarceration in marriage-market	Women aged 18-25	-3.1	-5.3
Amuedo Dorantes <i>et al.</i> (2020)	1 SD increase in immigration enforcement index	Mexican non-citizens	0.36	0.7

Notes: This table reports headline estimates of the effects of other policies and economic phenomena on marriage. This provides a point of comparison for the preferred results of this paper—a lower-bound ITT of approximately 2 percentage points. For men, that is approximately a 9% increase in marriage. For women, that is approximately a 6.5% increase.

Table 2.6: Marriage Effects from Literature

2.6.1.1 Increasing marriage or shifting it earlier?

In this paper, I present evidence that DACA has increased marriage rates *within my sample* – which consists of individuals ages 18-30. A key question in the interpretation of my results is whether this is increasing DACA recipients’ lifetime marriage rates or is merely shifting forward marriages that would have occurred later in life. To my knowledge, it is impossible to determine the effects of DACA

on lifetime marriage rates using my data. However, I argue that the evidence that exists is consistent with an overall increase rather than a shift. In particular, I look at the age distribution of the effect *within* my sample. If DACA shifted the age of marriage forward outside the sample, one would expect it to also do so within the sample. And I do not find that it shifts marriage forward within the sample.

First, I examine whether or not there is a shift in the raw data. Figures 5 and 6 show the percent married by age before [panel (a)] and after [panel (b)] DACA. For men, the eligible men's marriage rates after DACA are higher relative to the ineligible's than before DACA at every age, by a similar magnitude. For women, this is true at every age aside from age 30. Hence, there is no evidence in the raw data that DACA is noticeably shifting the age of marriage within my sample.

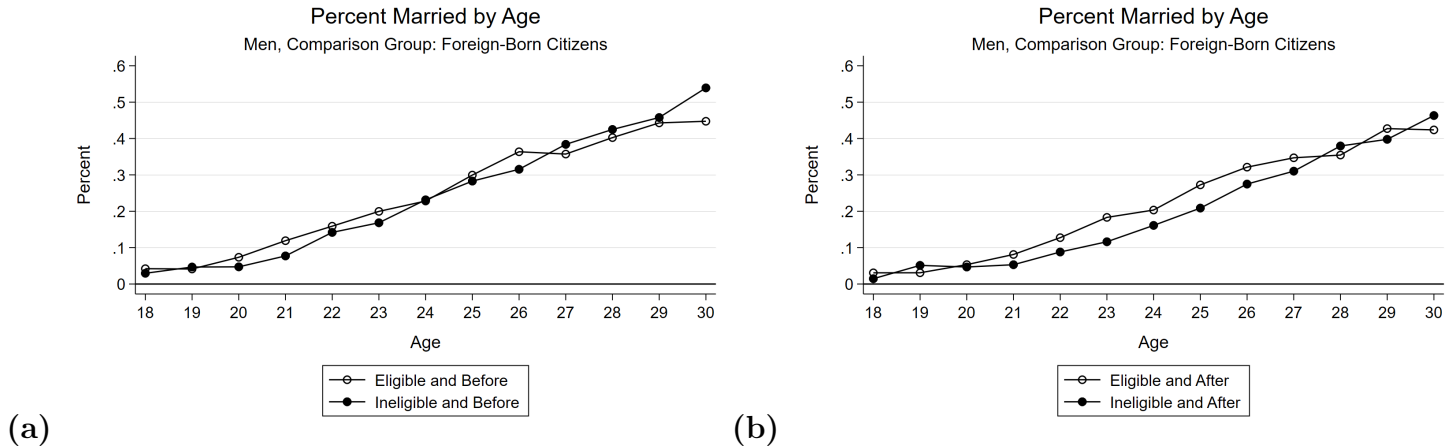
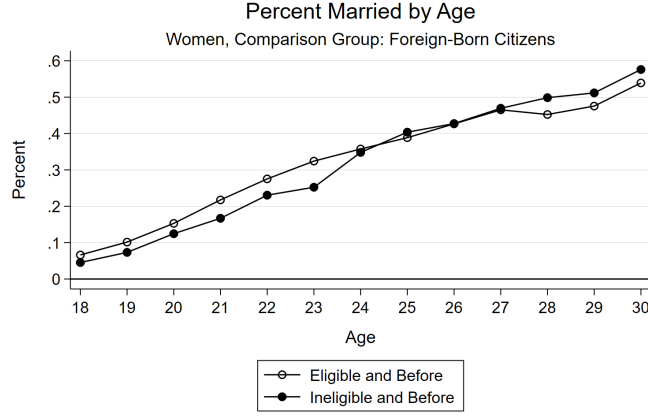
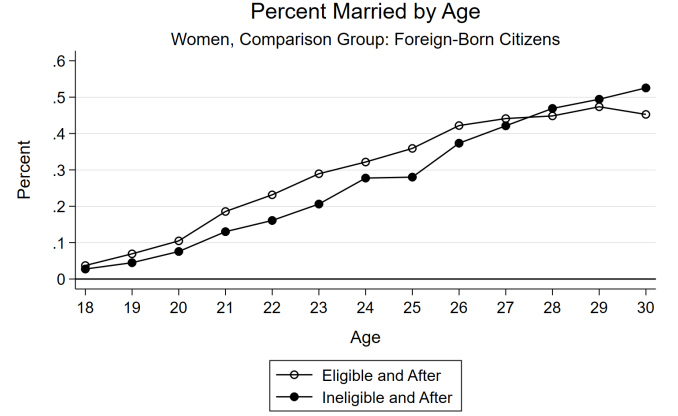


Figure 2.5: Marriage Age CDF, Men

Notes: This figure shows CDFs of men's marriage age before and after DACA. The ineligible group is the foreign-born citizens comparison group used throughout this paper.



(a)



(b)

Figure 2.6: Marriage Age CDF, Women

Notes: This figure shows CDFs of women's marriage age before and after DACA. The ineligible group is the foreign-born citizens comparison group used throughout this paper.

Second, I allow the effect of DACA eligibility on marriage to differ by age. The purpose of this to see if the effects are larger at younger ages, indicating that DACA is shifting marriages forward within the sample. I implement this by estimating the following regression, for both men and women:

$$Y_{it} = \beta_0 + \sum_{a=19}^{30} \beta_a Age_a + \sum_{y=19}^{30} \beta_a^E Age_a * Eligible_i + \sum_{y=19}^{30} \beta_a^A Age_a * After_t + \sum_{y=19}^{30} \beta_a^{EA} Age_a * Eligible_i * After_t + \beta_1 Eligible_{it} + \beta_2 After_t + \beta_3 Age of Entry_{it} + \varepsilon_{it}$$

I display the results in Figure 7. The results for men appear in panel (a), and appear in panel (b) for women. For men, The effect sizes grow at about age 22, and remain similar (with an upwards outlier at age 25 and a downwards outlier at 30) for the remaining ages. The pattern is similar for women, with downward outliers at ages 26 and 28. So if any shift is happening, this is consistent with marriage being shifting to later years in my sample.

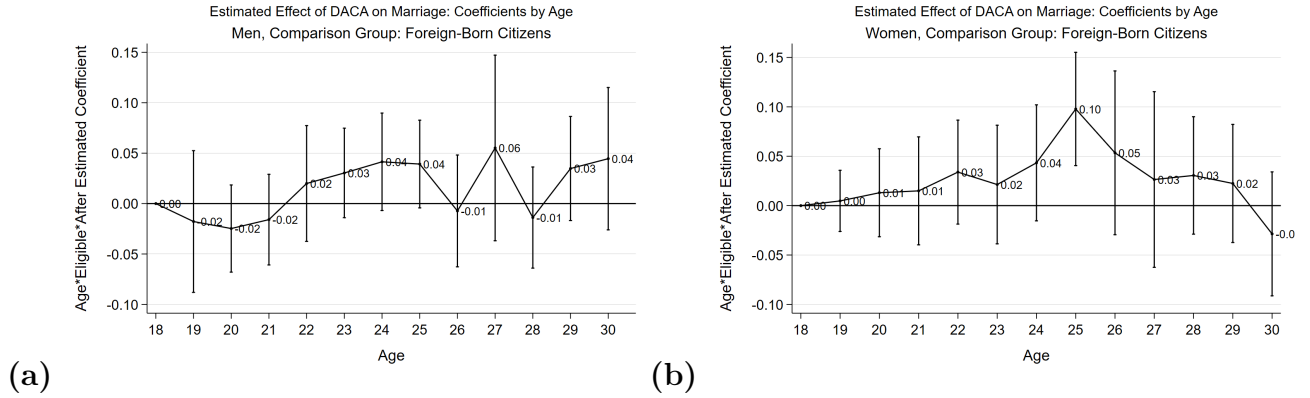


Figure 2.7: Age Heterogeneity

Notes: This figure compares the effect of DACA on marriage by age, with the base age as 18. The only additional control is age of entry. Panel (a) displays the results for men. Panel (b) displays the results for women.

2.6.1.2 Mechanisms

I now turn to evidence on the mechanisms underlying the consistent result found previously: DACA eligibility increases the probability of being married by a lower-bound of 2-3 percentage points, depending on sex and controls. The main two benefits of DACA are deportation relief and work authorization, and I discuss how they could potentially affect in the Conceptual Framework Section. Hence, I investigate how the extent of deportation relief and post-DACA employment prospects operate as potential channels through which the marriage effect could be driven. For deportation relief, I use geographic variation in immigration enforcement as a proxy for how valuable deportation relief would be for a DACA recipient. To explore the salience of the work authorization, I test the sensitivity of the estimates to using labor market outcomes as controls. I also construct a proxy for local employment

prospects for a DACA recipient, and analyze that in a similar manner as I analyze deportation relief. After analyzing the two main provisions of DACA, I also conduct a mediation analysis that incorporates a group of variables that are thought to be outcomes of DACA that are relevant to marriage (labor market, education, and family structure), and describes what percentage of the effect can be explained by DACA’s effect on these mediators.

As described in the Data Section, I proxy for an individuals’ deportation risk in two ways. For each way, I estimate Equation (12) with the deportation risk measure as the mechanism that is interacted throughout the model. The first measure is at the state-level, and I refer to it as the “deportation ratio.” Recall that it is the ratio of state-wide interior removals to the Hispanic population aged 18-35 in the pre-period of 2005-2011.²⁷ The second measure is at the MSA-year-level, and I refer to it as “enforcement index.” It ranges from 0-5 and is a measure of which of five types of policies (local 287(g) agreements, state-level 287(g) agreements, Secure Communities, omnibus immigration laws, and E-Verify) were active in counties within the MSA. This measure has the advantage of having been examined as quasi-experimental in several different contexts, including marriage during the same time period as this study (2005-2017). Amuedo Dorantes *et al.* (2020) subject this measure to event study analysis and placebo tests involving immigrants who are unlikely to be affected by enforcement. Hence, it is more plausible for these estimates to be viewed as causal. For example, this measure is less susceptible to a concern about immigration location within the US being endogenous to immigration policy. Addi-

²⁷When this measure is used, state fixed effects are omitted from the estimation.

tionally, unlike the deportation ratio, the enforcement index is time-varying.²⁸ The results for both men and women are presented in Table 7.

	Men		Women	
	(1) Deportation Risk	(2) Enforcement Index	(3) Deportation Risk	(4) Enforcement Index
Eligible*After	0.0127 (0.0129)	0.0424*** (0.0127)	0.000711 (0.00982)	0.00593 (0.0118)
Eligible*After*Deportation Risk	0.326 (0.257)		0.571*** (0.200)	
Eligible*After*Enforcement Index		-0.0234 (0.0154)		0.0373** (0.0154)
Effect of moving from min to max of measure (PP)	15.9	-11.7	27.8	18.7
Age controls?	Yes	Yes	Yes	Yes
Age of entry controls?	Yes	Yes	Yes	Yes
Demographic and social capital controls?	Yes	Yes	Yes	Yes
Labor market controls?	No	No	No	No
Education controls?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes
State fixed effects?	No	Yes	No	Yes
State trends?	Yes	Yes	Yes	Yes
Dep Var Mean	0.24	0.33	0.33	0.32
Dep Var Mean: Eligibles	0.23	0.25	0.31	0.31
Observations	66651	71419	67060	51770
R ²	0.139	0.199	0.133	0.139

Notes: This table reports DiD results for the effect of DACA eligibility on the probability its recipients are married that test the effect being driven by a deportation relief mechanism. The outcome variable is an indicator of whether an individual is married. For each sex, the first column includes an interaction with the DiD variable and a pre-period, state-level deportation risk measure. The second column interacts the DiD variable with a MSA-level measure of the enforcement policies in effect. Each sample contains Hispanics of one sex with at least a high school diploma, age 18–30. The treatment group is non-citizens who entered the US before the age of 16 and who have resided in the US for at least 5 years. The comparison group is identical, except they are foreign-born citizens. All specifications include controls for age, economic, sex, race/ethnicity, urban, education, and social capital variables. They also include year and state fixed effects, and state trends. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.7: Effect of DACA on Marriage, Deportation Relief Mechanism

First, I consider the men. These estimates lead to an ambiguous conclusion. The estimates using the deportation ratio in Column (1) is noisy. If taken at face-value, the point estimate on *Eligible*After*DeportationRisk* is larger than that on *Eligible * After*, suggesting an important role for deportation relief. The estimate suggests that moving from the the state with the least deportation risk to the highest deportation risk would result in a 15.9 percentage point increase in the likelihood of being married in response to DACA. This would represent a large push towards

²⁸I deem the Enforcement Index results more trustworthy, but include the Deportation Risk results as well because 1) the measure has been used in recent DACA literature and 2) still provides additional confirmation of the mechanism.

marriage in states where DACA provided substantial deportation relief. However, the noisiness of these estimates are casted further into doubt when compared with the more precise estimates when the enforcement index is used in Column (2). The coefficient on *Eligible * After* is statistically and economically significant, and the coefficient on the interaction with the enforcement index is negative and noisy. Taking this alone would suggest that deportation relief was not as big of a factor in men’s marriage response to DACA compared to other factors, and relative to women’s response.

The results for women are far less ambiguous. In Column (3), inclusion of *Eligible * After * DeportationRisk* – which has a large and highly significant coefficient – diminishes the size of the *Eligible * After* coefficient to a near-zero effect. The *Eligible * After * DeportationRisk* coefficient suggests that moving from the the state with the least deportation risk to the highest deportation risk would result in a 27.8 percentage point increase in the likelihood of being married. Similarly, using the enforcement index in Column (4) yields similar results. The coefficient on *Eligible * After* is diminished to a statistically insignificant 0.6 percentage points, while the coefficient on *Eligible * After * EnforcementIndex* implies that moving from none of the policies being active, to all of the policies being active would results in a marriage response to DACA that is 18.7 percentage points higher. Hence, it is much clearer that the deportation relief effects of DACA were salient to women. However, it should be noted that this does not necessarily mean women are responding to their *personal* deportation risk. Given that men are the vast majority

of deported individuals²⁹, it is more likely that women are responding to the deportation risk of their potential husbands of a similar age and age of arrival (and therefore DACA eligibility). As both a complementary and stand-alone point, it could also reflect that since marriage often comes with children, women – who are traditionally more attached to their children than men – feel safer making a commitment that could yield children after they have reason to think they or the child’s father will not be separated from those children.

Next, I consider the labor market mechanism, which I report in Table 8. Pope (2016) finds that DACA increased labor market outcomes for both sexes. So if including these outcomes as controls *increases* the estimated effect of DACA on marriage, this suggests those increased labor market opportunities accompanied a substitution away from marriage. And if including these outcomes as controls *decreases* the estimated effect of DACA on marriage, this suggests that those increased labor market opportunities facilitated marriage.

There are societal norms that increased labor market prospects makes men more marriageable. This would imply a facilitating effect of DACA’s labor market effects on marriage for men. There is also a notion that women’s increased supplying of labor (away from household production) could provide disutility to those with more traditional preferences. This would imply that increased labor market prospects for women would increase the opportunity cost of marriage. Indeed, the model presented in the Conceptual Framework section provides the possibility of

²⁹For example, they represented 94% of deportations in FY 2012 <https://trac.syr.edu/immigration/reports/350/>.

diverging effects by sex.

In Table 8, there are four columns for each sex. The first column is the same as my main results with foreign-born citizens. The second column includes a set of labor market outcomes used in Pope (2016) as controls.³⁰ I also include interactions of these controls with *After* and *Eligible*. In the third column, the employment prospects mechanism is explored by including an indicator that equals 1 if an individual is above the median of the employment propensity described in the Empirical Strategy section. The estimates for this are imprecise, so I estimate that same specification in the fourth column, but use all citizens as a comparison group rather than just foreign-born citizens as a means of increasing statistical power.

Among men, results accord with the marriagable men hypothesis, suggesting that DACA's work authorization provision increases eligible men's marriage prospects. In Column (1), the estimated effect of DACA is 2.3 percentage points. In Column (2), with labor market outcomes included as controls, the effect reduces to 1.3 percentage points, consistent with facilitation of marriage. In Column (3), the coefficient for *Eligible * After* reduces to 0.8 percentage points, and the point estimate for *Eligible * After * HighEmploymentPropensity* is 2.3 percentage points, suggesting that the marriage effect is larger among individuals with better employment prospects. However, these estimates are imprecise and therefore statistically non-significant. In Column (4), with the larger sample, previous estimates are corroborated qualitatively. The coefficient for *Eligible * After* is 0.6 percentage points,

³⁰The outcomes include: weekly hours, whether the individual worked in the last year, whether or not they were working at the time of the survey, and their income

	Men			Women		
	(1) No LM Controls	(2) LM Controls	(3) Emp Prop: FB Citizens	(4) Emp Prop: All Citizens	(5) No LM Controls	(6) LM Controls
Eligible*After	0.0230** (0.00892)	0.0132* (0.00784)	0.00836 (0.00767)	0.00572 (0.00508)	0.0198** (0.00752)	0.0265*** (0.00751)
Elig*After*High Employment Propensity			0.0226 (0.0138)	0.0304*** (0.00743)		
Age controls?	Yes	Yes	Yes	Yes	Yes	Yes
Age of entry controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and social capital controls?	Yes	Yes	Yes	Yes	Yes	Yes
Labor market controls?	No	Yes	No	No	No	No
Educational controls?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects?	Yes	Yes	No	No	Yes	No
State trends?	Yes	Yes	Yes	Yes	Yes	Yes
Dep Var Mean	0.24	0.24	0.24	0.18	0.33	0.33
Dep Var Mean: Eligibles	0.23	0.23	0.23	0.23	0.31	0.31
Observations	66651	66651	66651	359686	67060	67060
R ²	0.140	0.161	0.141	0.137	0.134	0.147

Notes: This table reports DiD results for the effect of DACA eligibility on the probability its recipients are married that test the effect being driven by a labor market mechanism. The outcome variable is an indicator of whether an individual is married. For each sex, the first two columns compare specifications with and without labor market controls. The third column interacts the DiD variable with a pre-period propensity score into employment. Each sample contains Hispanics of one sex with at least a high school diploma, age 18-30. The treatment group is non-citizens who entered the US before the age of 16 and who have resided in the US for at least 5 years. The comparison group is identical, except they are foreign-born citizens. All specifications include controls for age, economic, sex, race/ethnicity, urban, education, and social capital variables. They also include year and state fixed effects, and state trends. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Effects of DACA on Marriage, Labor Market Mechanism

and that for *Eligible * After * HighEmploymentPropensity* is a statistically significant 3 percentage points. All estimates for the men’s sample thus point in the same direction.

Among women, estimates are more consistent with labor market prospects reducing the marriage effect. In Column (5), the estimated effect of DACA on marriage without any labor market controls is 2 percentage points. When labor market controls are included in Column (6), the estimate increases to 2.7 percentage points. In Column (7), where employment propensity is included, the coefficient for *Eligible * After* is 3 percentage points. The coefficient for *Eligible * After * HighEmploymentPropensity* is -1.9 percentage points, though statistically non-significant. When native-born citizens are also included in the comparison group in Column (8), the coefficient for *Eligible * After* is a statistically significant 1.52 percentage points, and the coefficient for *Eligible * After * HighEmploymentPropensity* is -0.74 percentage points. All of these coefficients suggest that in contrast to men, the employment prospects afforded by DACA attenuate the marriage effect among women.

Both sexes have a robust increase to marriage rates in response to receiving DACA. I explore mechanisms related to the two major provisions of DACA—deportation relief and work authorization. Using multiple methods and/or measures for each mechanism, I arrive at two conclusions. First, the economic benefits of DACA appear to facilitate men’s marriage responses. There is also evidence that the increased economic opportunities of DACA reduce these effects among women, at least for the ages considered; it is possible that DACA only delays marriage among

some women. Clearer evidence suggests that women’s marriage rates are responsive to the deportation relief DACA provides.

Though I use the labor market variables as a means of proxying for the effect of DACA’s work authorization, they are also worthy of being considered as mediators independent of their close relationship to a particular provision. Alongside the labor market variables, I incorporate other potential mediators as well. They are in two domains: education and family structure. Education has been a heavily investigated set of outcomes for DACA [e.g Amuedo-Dorantes and Antman (2017), Hsin and Ortega (2018), and Kuka *et al.* (2020)] and is widely known to be a characteristic that people value and sort on in marriage markets. Marriage and educational attainment can also be thought of as opportunity costs of one-another. And since marriage is inherently a change in family structure and a vehicle for having children, I also consider family structure variables as potential mediators of marriage. To view these all together in a succinct manner, I use a formal mediation analysis as in Bolt et al. (2021) – whom I follow closely – and many other studies.

The goal is to decompose β_1 from Equation (7) into proportions explained by each of the potential mediators. Consider the model:

$$Y_{it} = \alpha_c + \alpha_L \mathbf{L}_{it} + \alpha_E \mathbf{E}_{it} + \alpha_F \mathbf{F}_{it} + \alpha_{DACA} Eligible * After_{it} + \alpha_X \mathbf{X}_{it} + v_{it}^Y$$

where $\mathbf{L}_{it}$ is a vector of labor market potential mediators, $\mathbf{E}_{it}$ is a vector of education potential mediators, $\mathbf{F}_{it}$ is a vector of family structure potential mediators, and $\mathbf{X}_{it}$ is a vector of other controls that ensure conditional parallel trends and fully saturate the model. For notational clarity, let $Eligible * After_{it} \equiv DACA_{it}$. Then, totally differentiating (10) with respect to $DACA_{it}$ yields:

$$dY_{it} \frac{dL_{it}}{dDACA_{it}} = \alpha_L \frac{dL_{it}}{dDACA_{it}} + \alpha_E \frac{dE_{it}}{dDACA_{it}} + \alpha_F \frac{dF_{it}}{dDACA_{it}} + \alpha_{DACA}$$

Then, I estimate how each potential mediator is individually related to $DACA_{it}$.

So for a variable v_{it} , to obtain a value for $\frac{dv_{it}}{dDACA_{it}}$, I estimate:

$$v_{it} = \kappa_c + \kappa_v DACA_{it} + \kappa_x^v \mathbf{X}_i t + v_{it}^v$$

Then, I use (11) and (12) to calculate the total impact of DACA through v_{it} to be $\frac{\partial Y_{it}}{\partial v_{it}} \frac{dv_{it}}{dDACA_{it}} = \alpha_v \times \kappa_v$. Let $\frac{dY_{it}}{dDACA_{it}} \equiv \rho$. Then the share of the effect of DACA on marriage that can be explained by v_{it} is $\frac{\alpha_v \times \kappa_v}{\rho}$. The set of labor market variables I use are same as previously used. For education, I use indicators for GED, some college, and college degree or higher, with high school degree being the baseline. For family structure, I use family size, the number of children, the number of children under 5, and an indicator for whether the individual lives in a multi-generational household. I report the results in Table 9.

Proportion of β_1 Explained	
Men	
<u>Labor Market</u>	<u>0.165</u>
Hours	0.137
Worked last year	-0.076
Working	0.020
Income	0.086
<u>Education</u>	<u>0.024</u>
GED	-0.003
Some College	-0.014
College degree or higher	0.045
Years of Education	-0.004
<u>Family Structure</u>	<u>0.227</u>
Family Size	0.009
Number of Children	0.163
Children Under 5	0.054
Multi-gen Household	0.000
Women	
<u>Labor Market</u>	<u>-0.209</u>
Hours	0.065
Worked last year	-0.183
Working	-0.091
Income	0.001
<u>Education</u>	<u>-0.036</u>
GED	-0.010
Some College	-0.046
College degree or higher	0.072
Years of Education	-0.052
<u>Family Structure</u>	<u>0.239</u>
Family Size	0.007
Number of Children	0.121
Children Under 5	-0.009
Multi-gen Household	0.120

Notes: This table reports the proportion of the effect of DACA on marriage explained by potential mediators. The labor market mediators include: weekly hours, whether the individual worked in the last year, whether or not they were working at the time of the survey, and their income. The education mediators include: indicators for GED, high school degree, some college, and college degree or higher. The family structure mediators include: family size, the number of children, the number of children under 5, and an indicator for whether the individual lives in a multi-generational household.

Table 2.9: Decomposition of DACA Marriage Effect

Consistent with the previous results, labor market variables explain a sizable proportion of the effect for both men (0.165) and women (-0.209). The results are qualitatively similar to those in Table 8. For men, the labor market variables explain a *positive* percentage of the overall effect, which reflects their role as a facilitator. That both hours (0.137) and income (0.086) both separately explain large pieces of the effect are consistent with separate roles for a “responsibility effect” (someone is reliable and works hard) and an income effect within a “marriageable man hypothesis” framework. For women, negative explanatory power of labor market outcomes is largely driven by employment measures. This is also consistent with women substituting between working and home production within a marriage.

For both sexes, it also seems to be the case that any change in educational attainment that results from DACA explains little of the marriage effect. For men, this is 2.4 percent. For women, this is -3.6 percent. But the signs for both sexes are again consistent with opportunities gained from DACA being facilitators of marriage for men and substitutes for women.

Unsurprisingly, the effect on family structure is deeply intertwined with the effect on marriage. Nearly a quarter of the effect is explained by DACA-induced changes in family structure for both sexes. For men, the marriage effect coincides with the number of children (0.163) and separately young children (0.054). For women, children are also part of the marriage package (0.112), but women’s effects are more closely tied to their broader families: reflected by in living in a multi-generational household (0.12).

Because I only have one source of exogenous variation, I cannot identify the

sequential relationship between these mediators and marriage. However, one can consider the incentives DACA creates to consider the plausibility of different sequences. For example, the method does not say whether DACA caused individuals to have children, which then leads them to get married rather than marriage coming first in the sequence. However, I extensively argue why DACA could have increased marriage. There are few, if any, theoretical reasons to believe that DACA increased out-of-wedlock births that led to “shot-gun marriages.” In fact, there is evidence that DACA decreased teen pregnancy [Kuka *et al.* (2019)].

2.6.2 Spousal Characteristics, Conditional on Marriage

Immigrants who marry into the mainstream of a host country is a common proxy for assimilation [e.g., Bleakley and Chin (2010), Furtado and Theodoropoulos (2011), and Meng and Gregory (2005)], and a natural question to ask about any immigration policy is how it affects measures of assimilation. Thus, I address the question of how DACA changes whom recipients marry, with a focus on how assimilative their spousal choice is. Since DACA increases marriage rates among recipients, results in the following section are conditional on being married.

2.6.2.1 English Fluency

I begin with a straightforward measure of how assimilative a DACA recipient’s marriage is, an indicator of whether the spouse speaks English fluently. Both the treatment and comparison groups consist of Hispanic immigrants, so we can presume

that Spanish is typically the first language of both groups. In contrast to outcomes analyzed in the next subsection, this measure of assimilation is unconnected to legal status, such as citizenship. I report results for both men and women in Table 10 and Figure 4. In Column (1) of Table 10, and correspondingly in panel (a) of Figure 8, I report a 7.5 percentage point increase to the likelihood of being married to a fluent English speaker, representing an 11% increase to the base of 68%. Among women, the effect is smaller but still practically and statistically significant. In Column (2) of Table 10, and correspondingly in panel (b) of Figure 8, I report a 3.2 percentage point effect, suggesting roughly a 5% increase among eligibles. Although estimates in Figure (4) are noisy, they provide less evidence for pre-trends in the male sample.

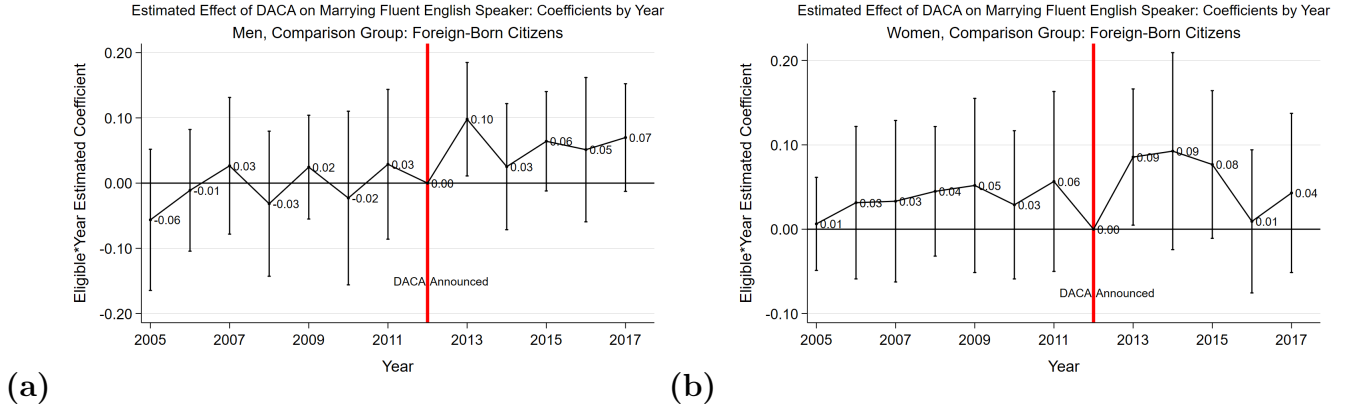


Figure 2.8: Trends in Spouse English Fluency

Notes: This figure compares regression-adjusted (for age and age of US entry) spouse-characteristic trends among eligibles and a comparison group. Panel (a) shows men's trends of marriage to a fluent English speaker, with a foreign-born citizens comparison group. Panel (b) shows the same among women.

	(1)	(2)
	Men	Women
Eligible*After	0.0752*** (0.0167)	0.0322*** (0.0158)
Age controls?	Yes	Yes
Age of entry controls?	Yes	Yes
Demographic and social capital controls?	Yes	Yes
Education controls?	Yes	Yes
Year fixed effects?	Yes	Yes
State fixed effects?	Yes	Yes
State trends?	Yes	Yes
Dep Var Mean	0.70	0.62
Dep Var Mean: Eligibles	0.68	0.60
Observations	13366	19203
R ²	0.0957	0.0755

Notes: This table reports DiD results for the effect of DACA eligibility on the English fluency of an individual's spouse, conditional on being married. The sample contains married Hispanics of either sex with at least a high school diploma, age 18–30. The treatment group is non-citizens who entered the US before the age of 16 and who have resided in the US for at least 5 years. The comparison group is foreign-born citizens. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.10: Effect of DACA on Spouse's English Fluency

2.6.2.2 Citizenship and Place of Birth

I now discuss a more common measure of assimilation—the citizenship of an immigrant's spouse. In the Conceptual Framework section, I predict that DACA induces recipients to marry citizens through the consequences of greater exposure to them. I also note a potential countervailing effect, that DACA reduces incentives to marry a citizen for legal benefits. Since DACA changes both an individual's legal status and potential integration into society, it is important to distinguish how eligibles respond to changes to legal incentives versus how they respond to DACA's increased opportunity for integration.

One way to distinguish assimilative from legal incentive effects of marrying a citizen is comparing the likelihood of marrying types of citizens, where relative

sizes of the two countervailing effects differ. I do this by comparing results when the outcome is marriage to native-born citizens to results when the outcome is marriage to foreign-born citizens. The legal benefit of marrying a citizen is the same regardless of birthplace. However, marrying a native-born citizen is more assimilative than marrying a foreign-born citizen. Thus, after running a model in which the outcome is married to a citizen, I estimate models in which the outcome variables are married to a foreign-born citizen and married to a US native. In essence, I decompose the effect found when the outcome is married to a citizen into separate effects in which incentives differ.

The comparison group for each estimate is chosen, as usual, to best meet the identifying assumptions of the DiD strategy. In the case of spouse composition, however, it is also chosen in a way that makes actions of the treatment and comparison groups equitable, particularly regarding whether a marriage decision is homogamous (i.e., marrying someone in the same group as oneself). It is inappropriate to use foreign-born citizens as a comparison group when the outcome is marrying a foreign-born citizen. Therefore, in this subsection, I typically use the non-citizens group described in Table 1 as a comparison group for DACA eligibles. For both eligible and ineligible non-citizens, marrying a US citizen or a foreign-born citizen is not homogamous.

I begin by reporting results for men, with regression results in each column of Table 11, and corresponding graphical results in panel (a)-(d) of Figure 9. In Column (1), when the outcome is the spouse's citizenship, the estimate is a noisy, near-zero coefficient of -0.0083. In panel (a), there is lack of evidence for pre-trends, with year-

by-year estimates consistently near zero, which might reflect countervailing effects of DACA's legal and assimilative benefits. The other results corroborate this. In Column (2) is an estimated 4.3 percentage point decrease to the probability of being married to a foreign-born citizen. In panel (b), this is corroborated by point estimates near zero before DACA and decreasing monotonically after DACA. In further corroboration of my theory, in Column (3), I report a 3.5 percentage point increase to the probability of being married to a native-born citizen. However, the estimate is imprecise, and in panel (c), point estimates increase during 2011 before reducing to zero during 2012,³¹ which is potential evidence of a pre-trend. To corroborate these countervailing effects, in Column (4), I report a similar but more precise 5.2 percentage point estimate using a comparison group of foreign-born citizens. For this outcome, foreign-born citizens represent a valid comparison group because marrying a native and marrying a fluent English speaker (recall that they are all born in Spanish-speaking countries) are not homogamous. The corresponding graphical analysis in panel (d) lacks evidence of pre-trends, suggesting that DACA induces men to marry more assimilatively. Combined with spousal English fluency results, these estimates suggest that eligible men move toward assimilative marriages and away from those influenced by legal incentives.

³¹The 2012 point is at zero by construction, since it is the omitted year.

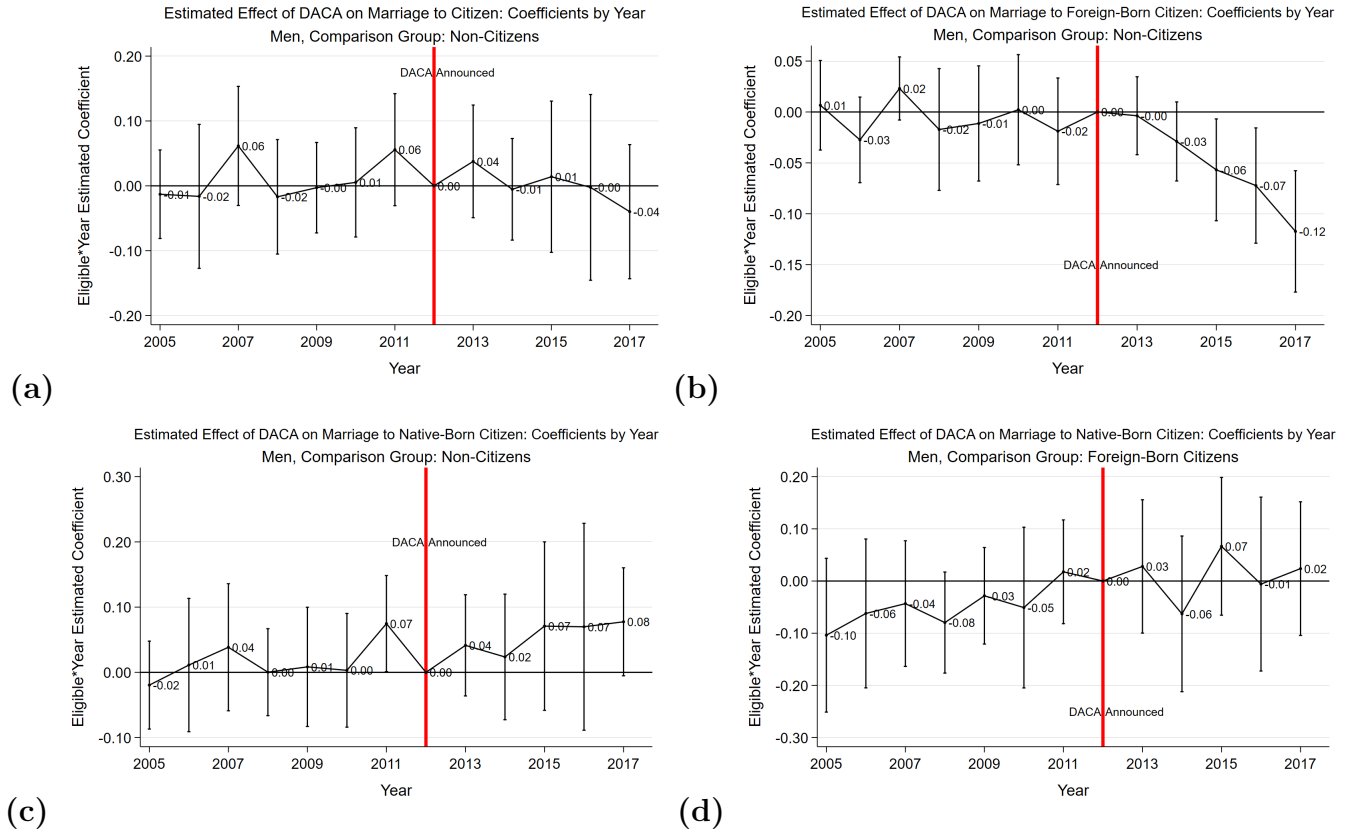


Figure 2.9: Men: Trends in Spouse Citizenship and Birthplace

Notes: This figure compares regression-adjusted (for age and age of US entry) spouse-characteristic trends for eligible men and a comparison group. Panel (a) shows the trend of marriage to a US citizen, using a non-citizens comparison group. Panel (b) shows the trend of marriage to a foreign-born citizen, using a non-citizens comparison group. Panel (c) shows the trend of marriage to a native-born citizen, using a non-citizens comparison group. Panel (d) shows the trend of marriage to a native-born citizen, using a foreign-born citizens comparison group.

	(1)	(2)	(3)	(4)
	Spouse is Citizen	Spouse is FB Citizen	Spouse is Native	Spouse is Native
Eligible*After	-0.00830 (0.0199)	-0.0433*** (0.0128)	0.0350 (0.0281)	0.0519** (0.0252)
Age controls?	Yes	Yes	Yes	Yes
Age of entry controls?	Yes	Yes	Yes	Yes
Demographic and social capital controls?	Yes	Yes	Yes	Yes
Labor market controls?	No	No	No	No
Education controls?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes
State fixed effects?	Yes	Yes	Yes	Yes
State trends?	Yes	Yes	Yes	Yes
Comparison group?	Non-citizens	Non-citizens	FB citizens	FB citizens
Dep Var Mean	0.49	0.12	0.37	0.49
Dep Var Mean: Eligibles	0.61	0.12	0.48	0.49
Observations	20934	20934	20934	13366
R ²	0.102	0.0232	0.110	0.101

Notes: This table reports DiD results for the effect of DACA eligibility on whom individuals marry, conditional on being married. The outcome variable is an indicator of a spouse characteristic. The sample contains married Hispanic men with at least a high school diploma, age 18–30. The treatment group is non-citizens who entered the US before the age of 16 and who have resided in the US for at least 5 years. The outcomes, in order by column, are indicators of an individual’s spouse being a citizen, a foreign-born citizen, and a US native (twice in a row). In Columns (1)–(3), the comparison group is non-citizens. In Column (4), the comparison group is foreign-born citizens. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.11: Effect of DACA on Spousal Citizenship and Place of Birth, Men

Analogous results among women appear in each column of Table 12, and each panel of Figure 10. The signs of the point estimates all point in the same direction as those for men, but they are typically less precise, of lesser magnitude, and, through graphical analysis, appear more susceptible to pre-trends. The point estimate when the outcome [Column (5)] is an indicator of marriage to a citizen is very imprecise, and it is a 1.9 percentage point effect, but it appears to be biased away from zero by pre-trends [panel (a)]. The Column (2) estimate is also an imprecise and small -0.1 percentage point effect on the probability of being married to a foreign-born citizen. The estimate of being married to a native-born citizen in Column (3) is 1 percentage point, but also potentially biased away from zero [panel (c)]. Thus, little can be concluded from these results. However, for outcomes that allow a foreign-born citizens comparison group, results are more similar to that among men. In

Column (4), I precisely estimate a 4.6 percentage point increase to the probability of being married to a US native.

In summary, the evidence for women is less convincing than that for men. This statement also applies to English fluency and citizenship results. However, some evidence that allows for inference typically points to DACA inducing women to also marry more assimilatively.

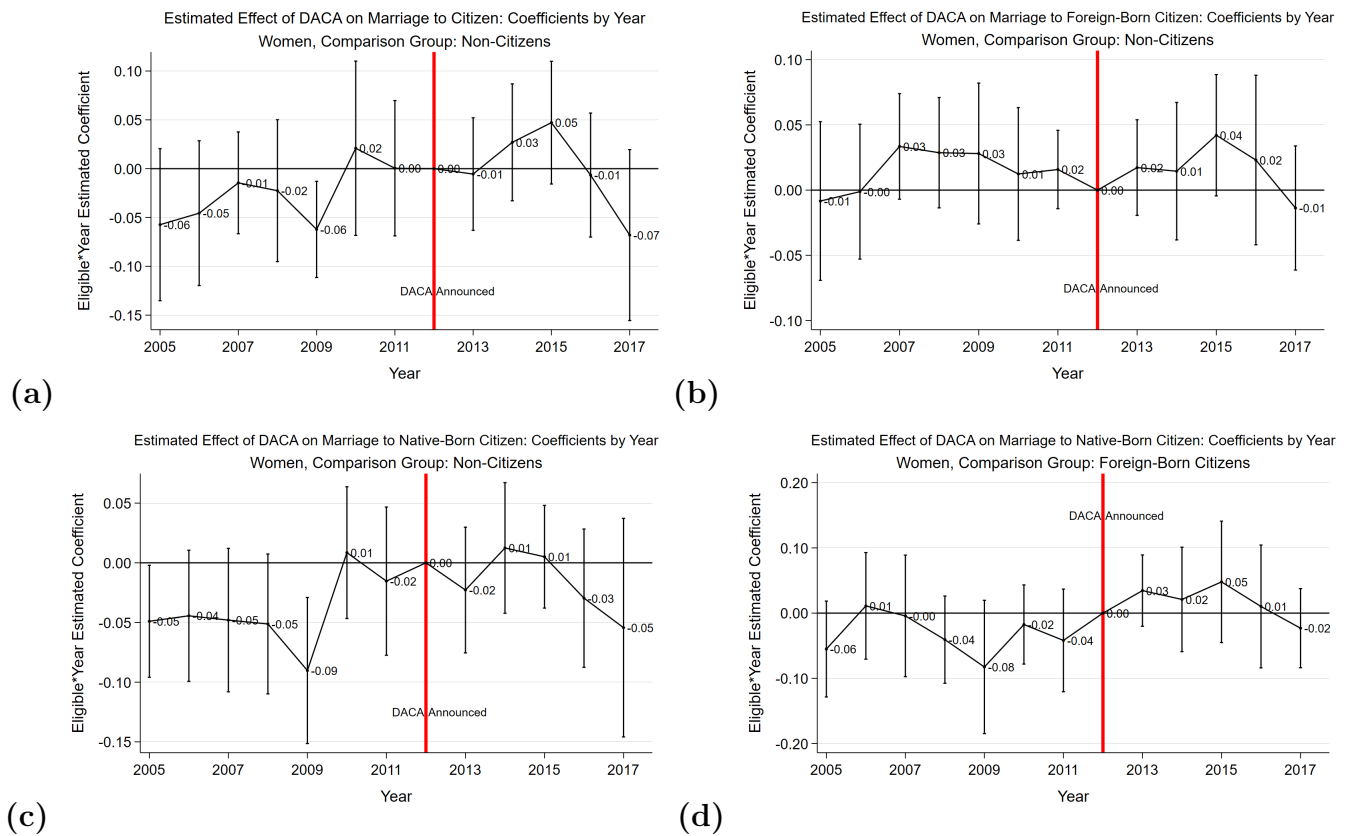


Figure 2.10: Women: Trends in Spouse Citizenship and Birthplace

Notes: This figure compares regression-adjusted (for age and age of US entry) spouse-characteristic trends for eligible women and a comparison group. Panel (a) shows the trend of marriage to a US citizen, using a non-citizens comparison group. Panel (b) shows the trend of marriage to a foreign-born citizen, using a non-citizens comparison group. Panel (c) shows the trend of marriage to a native-born citizen, using a non-citizens comparison group. Panel (d) shows the trend of marriage to a native-born citizen, using a foreign-born citizens comparison group.

	(1)	(2)	(3)	(4)
	Spouse is Citizen	Spouse is FB Citizen	Spouse is Native	Spouse is Native
Eligible*After	0.0192 (0.0132)	0.00769 (0.00864)	0.0115 (0.0108)	0.0462*** (0.0108)
Age controls?	Yes	Yes	Yes	Yes
Age of entry controls?	Yes	Yes	Yes	Yes
Demographic and social capital controls?	Yes	Yes	Yes	Yes
Labor market controls?	No	No	No	No
Education controls?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes
State fixed effects?	Yes	Yes	Yes	Yes
State trends?	Yes	Yes	Yes	Yes
Comparison group?	Non-citizens	Non-citizens	Non-citizens	FB citizens
Dep Var Mean	0.41	0.14	0.27	0.38
Dep Var Mean: Eligibles	0.50	0.13	0.37	0.37
Observations	24718	24718	24718	19203
R ²	0.0947	0.0246	0.114	0.0916

Notes: This table reports DiD results for the effect of DACA eligibility on whom individuals marry, conditional on being married. The outcome variable is an indicator of a spouse characteristic. The sample contains married Hispanic women with at least a high school diploma, age 18–30. The treatment group is non-citizens who entered the US before the age of 16 and who have resided in the US for at least 5 years. The outcomes, in order by column, are indicators of an individual’s spouse being a citizen, a foreign-born citizen, and a US native (twice in a row). In Columns (1)–(3), the comparison group is non-citizens. In Column (4), the comparison group is foreign-born citizens. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.12: Effect of DACA on Spousal Citizenship and Place of Birth, Women

2.6.3 Robustness

I test the robustness of results against other comparison groups, reporting results for both men and women in Table 13. There are three columns for each sex. The first uses Hispanic citizens, including US natives, as a comparison group. The second column uses a sample of non-citizens who are ineligible for DACA only because of their age of entry (i.e., they entered at age 16 or older). The third trims the aforementioned non-citizen sample to include only those who entered between ages 12 and 19. This brings the sample closer to the eligibility threshold, thus introducing a regression discontinuity-style logic. All of these samples use the full set of controls used in the final column of the main results tables.

Results for men are discussed first. Recall that the estimate for the foreign-born citizens comparison group is 2.3 percentage points [Table 4, Column (7)]. The

estimate using citizens as a comparison group is 2.9 percentage points [Column (1)]. The estimate for the non-citizens sample [Column (2)] is 2.56 percentage points. Both accord with estimates for the foreign-born citizens comparison group. The RD-style sample [Column (3)] yields a slightly smaller estimate, at 1.8 percentage points. The sample size is much smaller, and thus estimates are also less precise. Recall that since these estimates are ITT and lower bound, the estimate remains sizable in comparison to other marriage estimates in the literature. I therefore argue for economic significance. Qualitative results among men are robust to several alternative samples.

Results among women are also robust to these alternative samples, though they have a wider range. Recall that the estimate for the foreign-born citizens comparison group is 1.98 percentage points [Table 5, Column (7)]. With a citizens comparison group [Column (4)], the estimated lower bound is 1.4 percentage points. With non-citizens as a comparison group [Column (5)], the effect is 3.43 percentage points. Trimming the sample closer to the age of entry eligibility threshold [Column (6)] reduces the effect to 2.83 percentage points. Qualitatively, results among women are robust to several samples. Estimates from the most reliable sample with the foreign-born citizens comparison group lie in the middle of the other estimates.

	(1) Citizens	Men (2) Non-Citizens	(3) RD-Style	(4) Citizens	Women (5) Non-Citizens	(6) RD-Style
Eligible*After	0.0209*** (0.00466)	0.0256*** (0.00915)	0.0184 (0.0150)	0.0140** (0.00562)	0.0343*** (0.00811)	0.0283** (0.0121)
Age controls?	Yes	Yes	Yes	Yes	Yes	Yes
Age of entry controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demographic and social capital controls?	Yes	Yes	Yes	Yes	Yes	Yes
Labor market controls?	No	No	No	No	No	No
Education controls?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
State trends?	Yes	Yes	Yes	Yes	Yes	Yes
Dep Var Mean	0.18	0.34	0.33	0.25	0.43	0.44
Dep Var Mean: Eligibles	0.23	0.25	0.32	0.31	0.32	0.41
Observations	359686	71903	39567	376910	63695	30696
R ²	0.137	0.144	0.143	0.140	0.148	0.119

Notes: This table reports DiD results for the effect of DACA eligibility on the probability its recipients are married. The outcome variable is an indicator of whether an individual is married. Each sample contains Hispanics of one sex with at least a high school diploma, age 18–30. The treatment group is non-citizens who entered the US before the age of 16 and who have resided in the US for at least 5 years. Each comparison group is as defined in Table 1. All specifications include controls for age, economic, sex, race/ethnicity, urban, education, and social capital variables. They also include year and state fixed effects, and state trends. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.13: Effect of DACA on Marriage, Alternative Samples

It is also of interest to investigate the effect of DACA eligibility on cohabitation. There are at least two reasons to do so. One is for conceptual robustness: it it aides in the interpretation of the marriage results. It helps answer questions such as: does DACA actually cause more people to live with romantic partners? Or does it merely lead to individuals changing their marital status in a more legalistic sense? The second reason is to help ensure that the marriage results are not merely changes in reporting behavior induced by DACA. For example, unauthorized immigrants may list their partner as cohabiting before DACA, but feel more comfortable calling it marriage (with no legal change in marital status) after DACA gave them a greater sense of security.

There are three groups that encompass the entire population: 1) married people, 2) unmarried cohabiters, and 3) people who do not live with a romantic

partner.³² DACA could shift people across these groups. We already know that there is a roughly 2 percentage point increase of DACA on marriage for both sexes.

Ideally, one would want to know the extent to which the marriage effect is driven by individuals shifting from unmarried cohabiters or is being driven by people who are not living with a romantic partner. Conceptually, this would describe in a rich way how commitment and romantic living arrangements are changed by DACA. Unfortunately, in a reduced-form analysis, we can only say what the *net* changes in each category are. I estimate Equation (7) when the outcome variables are indicators for living with an unmarried partner and living with no romantic partner.

Cohabitation is not straightforward to measure. In the ACS, individuals listed as unmarried partners in the same household can be identified, but this may not be reported accurately. This could stem from errors in self-reporting, and even correctly reported data miss cohabiting relationships that do not involve the head of the household. Hence, I use a potential lower-bound measure of cohabitation, an indicator that equals 1 if an individual is the household head and has an unmarried partner listed, or is listed as the unmarried partner of the household head. And I also use a potential upper-bound measure: an indicator that equals 1 if the individual is unmarried and lives with another adult of the opposite sex. In my analysis, I refer to this latter measure as “Expanded Cohabitation.” I perform my analysis with both definitions of cohabitation. The results are reported in Table 14.

The results are similar for both sexes. Columns (1)-(4) are the results for men, and columns (5)-(8) are the results for women. In the first column for each

³²These groups abstract away from married people who do not live together.

sex, I use the lower-bound measure of cohabitation and find no significant change in cohabitation, with the point estimates very close to zero. A similar story is reported in the second column for each sex when I use the expanded definition of cohabitation. The point estimate is larger for women in this case, but is still statistically insignificant. In the third and fourth columns for each sex, I report the outcome of not living with a romantic partner using the two alternative definitions of cohabitation (in the same order as the first two columns). For both measures and sexes, this effect is a decrease of roughly 2 percentage points – corresponding to the roughly 2 percentage point increase in marriage that headlines this paper. This means that *on net*, people are moving from not living with a romantic partner to marriage.

This net effect could be entirely people moving from not living with a partner to marriage. It could also be individuals moving from not living with a partner to cohabitation, and also people moving at the same rate from unmarried cohabitation to marriage. This would lead to the symmetric effects in marriage and non-cohabitation, with a net zero change in unmarried cohabitation. Alternatively (and likely) it is some combination of these two scenarios. However, since it is not a merely a symmetric effect that appears to only be flows between cohabitation and marriage, this does not appear to be merely a change in how individuals are answering the survey. It *does* appear that DACA is inducing individuals to move into deeper forms of romantic commitment.

	Men				Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Cohabiting	Expanded Cohabit	Not living w/ partner	Not living w/ part (expand)	Cohabit	Expanded Cohabit	Not living w/ partner	Not living w/ part (expand)
Eligible*After	-0.00431 (0.00353)	0.000871 (0.0101)	-0.0197** (0.00867)	-0.0236* (0.0128)	0.00220 (0.00422)	-0.0119 (0.0101)	-0.0222** (0.00884)	-0.00997 (0.0109)
Age controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age of entry controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo and socap controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trends?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep Var Mean	0.05	0.23	0.71	0.55	0.07	0.25	0.61	0.46
Dep Var Mean: Eligibles	0.05	0.24	0.72	0.56	0.07	0.26	0.62	0.47
Observations	66651	66651	66651	66651	67060	67060	67060	67060
R ²	0.0201	0.00972	0.160	0.0993	0.0124	0.0111	0.142	0.0836

Notes: This table has DiD results for the effect of DACA eligibility on cohabitation and marriage. The sample contains married Hispanics of either sex, with at least a high school degree, ages 18-30. The treatment group is non-citizens that entered the US before the age of 16 and have resided in the US for at least 5 years. The comparison group is foreign-born citizens. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.14: The Effect of DACA Eligibility on Marriage and Cohabitation

As another robustness test, I follow Kuka *et al.* (2020) and subject my main estimates to, and calculate alternative p-values from, a permutation test. I assign a placebo DACA policy to five different years, following the number of post-DACA years in the actual analysis, and the others are the placebo “pre-treatment” period. These use the full set of controls in Equation 11. Results are reported as histograms in Figure 11. This is repeated 1000 times, and each point estimate is compared to the point estimate obtained in Column (7) of Tables 4 and 5. Results among men are reported in panel (a), and results for women in panel (b). For both sexes, results further demonstrate the validity of my results. For men in panel (a), most draws are to the left of the actual point estimate of 0.023. The p-value from this exercise is 0.012, very similar to the 0.013 when clustering at the state level. For women in panel (a), results are also corroborating. The p-value implied by the exercise is 0.024, in comparison to the 0.011 p-value when clustering at the state level. This is not as close as that for men, but p-values obtained from both methods are reasonably close and well below the 5% significance level.

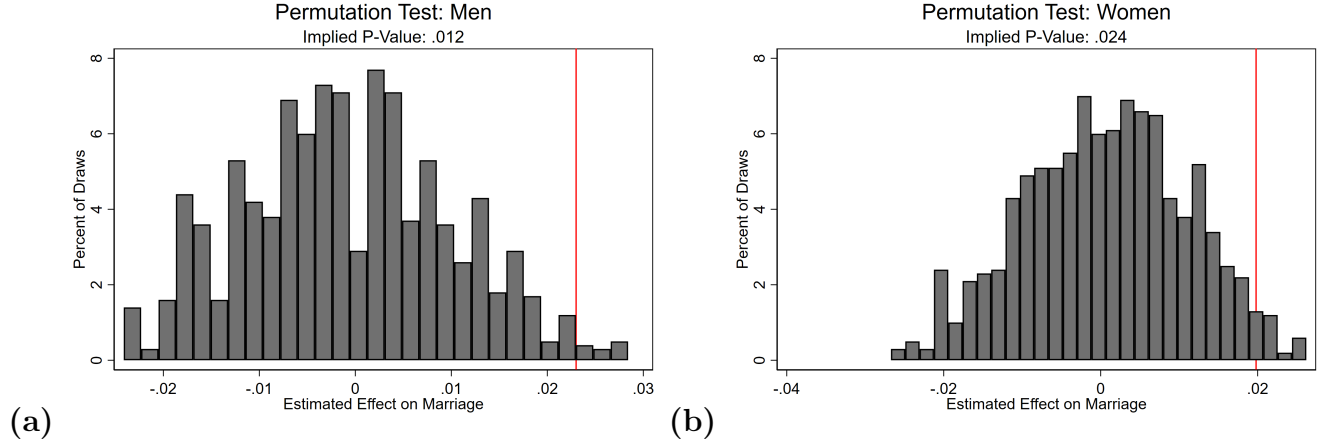


Figure 2.11: Permutation Test: Selecting Random “Treatment Years”

Notes: This figure reports results from permutation tests. I compare my preferred estimated effect DiD estimate (i.e., the vertical red line) to placebo estimates from 1,000 samples, during which I randomly assign five years as “treated” and the remaining years function as a placebo “pre-period.” The eligible and foreign-born citizen comparison groups, and the specifications, are as used in my preferred specifications. Panel (a) shows this among men, and Panel (b) among women.

Since my identification argument relies on a *conditional* parallel trends assumption (CPTA), it is useful to test the robustness of my main estimates when using an estimator that is formally shown to be consistent under this assumption. I use the doubly robust estimator (DRDID) of Sant’Anna and Zhao (2020). It is doubly robust because it provides a consistent estimate of the average treatment effect even if one of the two most common methods for DiD with covariates (a propensity score or a regression working model for outcome dynamics or a propensity score working model outcome model) is misspecified. To be conservative, I only include age and age of entry controls. I report these results in Table 15. Both the male and female estimates are qualitatively similar to the standard DiD estimates with the same controls.

	Men	Women
ATT	0.0273***	0.0258***
	(0.0083)	(0.0082)

Notes: This table reports Doubly Robust Difference-in-Differences estimates of the effect of DACA on marriage as derived in Sant’Anna and Zhao (2020). The comparison group is foreign-born citizens. The covariates include age and age of entry. Robust and asymptotic standard errors are estimated, using Influence Functions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.15: Doubly Robust Difference-in-Differences

Another concern is the potential for heterogeneous treatment effects. To investigate this, I use a more generalized version of the DRDID estimator that extends it to multiple time periods and multiple treatment groups [Callaway and Sant’Anna (2021)]. I only have one treatment group, but have multiple time periods. This estimator allows me to obtain a group-time average treatment effect for each period after DACA. However, because there is only one treatment group, I cannot distinguish between heterogeneous and dynamic treatment effects. However, since I show in Figure 1 that take-up of DACA is similar throughout the time-frame of my sample, I argue that any effect is more likely to be a dynamic effect rather than heterogeneous effects. I argue this because the stable take-up rate indicates that a statistically similar group of ACS individuals is being captured in every year. This estimator works by combining localized DRDID estimators: in the pre-period contiguous years are compared, and in the post-period each year is compared to the year before the treatment. I report my results for both men and women in Table 16. First, the estimates are quite noisy. But even if one wanted to take the point estimates at face-value, I argue there is no discernible pattern for either sex. For men, the estimated coefficients shrink between 2013 and 2014, then revert in 2015. The

point estimate then grows in 2016 and 2017. For women, the point estimate shrinks between 2013 and 2014, then returns to a similar size in 2015. Then spikes in 2016, and then reverts back in 2017. Hence, I see a lack of evidence for heterogeneous (or dynamic) effects.

	Men	Women
2005 v. 2006	-0.0224 (-0.0203)	-0.0469** (0.0216)
2006 v. 2007	-0.0004 (0.0181)	0.0382 (0.0202)
2007 v. 2008	0.0037 (0.0185)	-0.0152 (0.0193)
2008 v. 2009	-0.0061 (0.0187)	0.0188 (0.0194)
2009 v. 2010	0.0188 (0.0180)	0.0077 (0.0187)
2010 v. 2011	0.0009 (0.0168)	-0.0020 (0.0176)
2011 v. 2012	0.0092 (0.0159)	-0.0022 (0.0171)
2012 v. 2013	0.0210 (0.0155)	0.0146 (0.0166)
2012 v. 2014	-0.0028 (0.0146)	0.0090 (0.0162)
2012 v. 2015	0.0183 (0.0149)	0.0175 (0.0160)
2012 v. 2016	0.0238 (0.0149)	0.0511*** (0.0161)
2012 v. 2017	0.0345** (0.0151)	0.0183 (0.0163)

Notes: This table reports Doubly Robust Difference-in-Differences estimates of the effect of DACA on marriage as derived in Callaway and Sant’Anna (2021). The estimates allow for multiple time periods to be estimated separately. The comparison group is foreign-born citizens. The covariates include age and age of entry. Robust and asymptotic standard errors are estimated, using Influence Functions.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.16: Doubly Robust Estimation with Multiple Periods

2.7 Conclusion

Deferred Action for Childhood Arrivals (DACA) is one of the largest US immigration policies of the last six decades, addressing a fundamental question: how do the barriers faced by unauthorized immigrants impact their life paths and their relationship with society? My research reveals that DACA significantly increases marriage rates among both men and women. This chapter examines the specific provisions of DACA that drive these effects, suggesting that the program promotes marriages that align with assimilation, which is desirable for those who envision the US as a “melting pot.”

Furthermore, this chapter confirms the connection between immigration policy and marriage as discussed in Chapter 1, which operates through deportation risk, labor market opportunities, and opportunities for assimilation. The temporary nature of DACA positions it as a point of comparison to the policy discussed in Chapter 3, the Immigration Reform and Control Act (IRCA). DACA and IRCA share similarities in offering deportation relief and work authorization, but the crucial distinction lies in the permanence of the latter. The predicted implications of this difference are discussed in Chapter 1 and tested in Chapter 3.

2.8 Figures

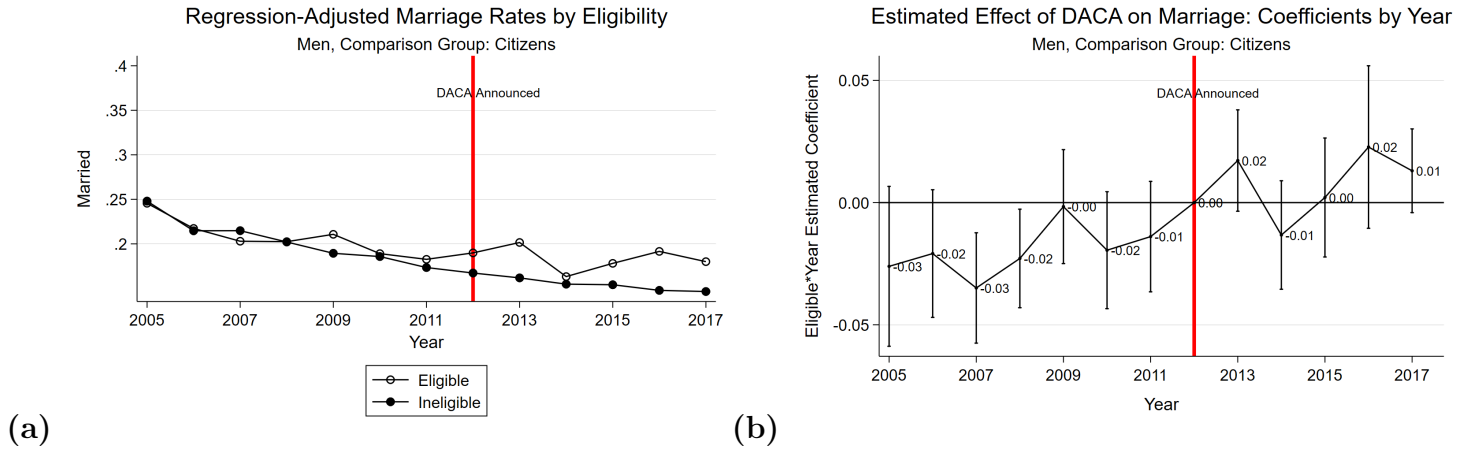
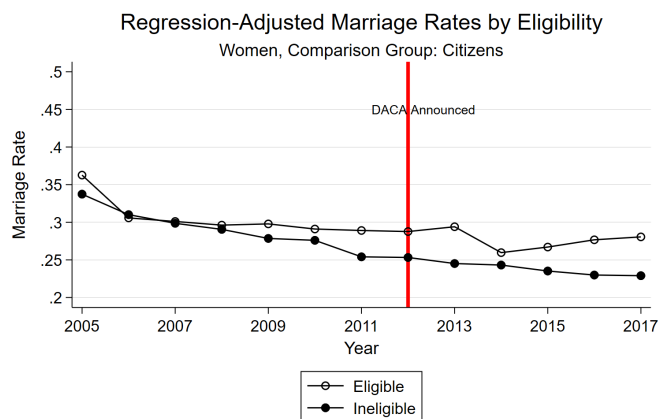
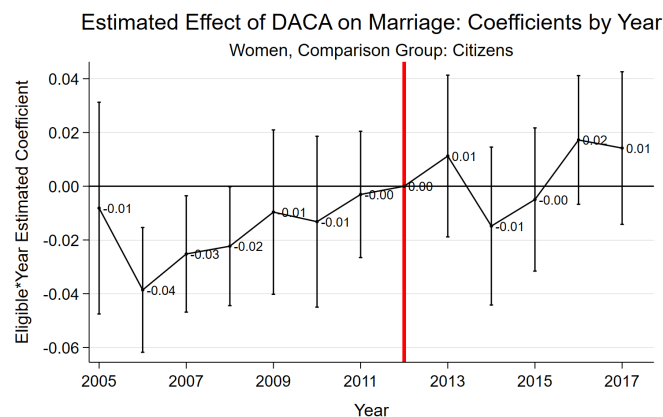


Figure 2.12: Men: Trends by Eligibility, Citizens Comparison Group

Notes: This figure compares the regression-adjusted (for age and age of US entry) marriage trends of the eligible group and the comparison group that consists of citizens. Panel (a) shows each group over time separately. Panel (b) plots the difference between the groups over time, with 95% confidence intervals.



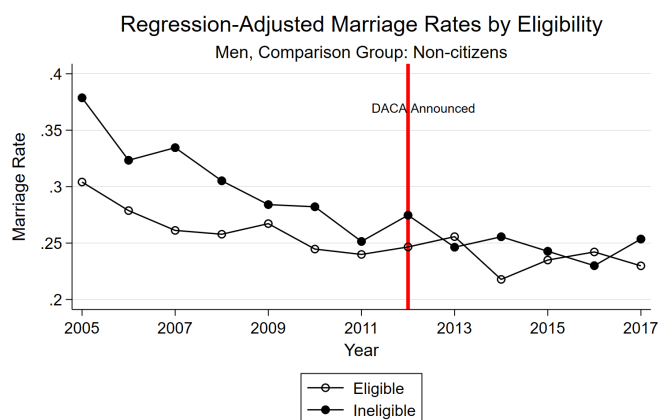
(a)



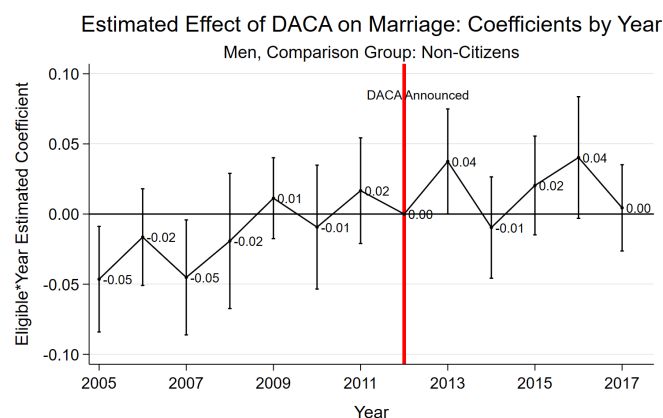
(b)

Figure 2.13: Women: Trends by Eligibility, Citizens Comparison Group

Notes: This figure compares the regression-adjusted (for age and age of US entry) marriage trends of the eligible group and the comparison group that consists of citizens. Panel (a) shows each group over time separately. Panel (b) plots the difference between the groups over time, with 95% confidence intervals.



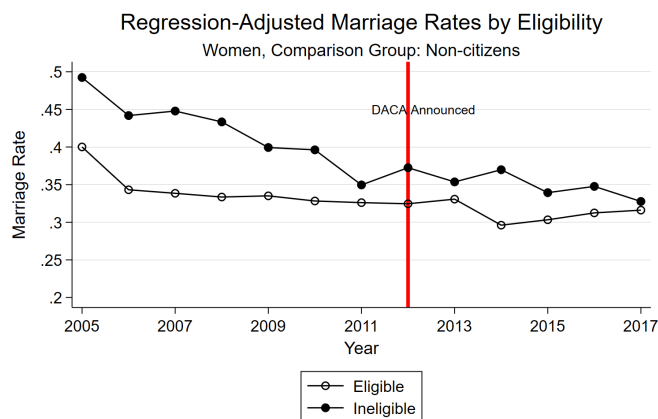
(a)



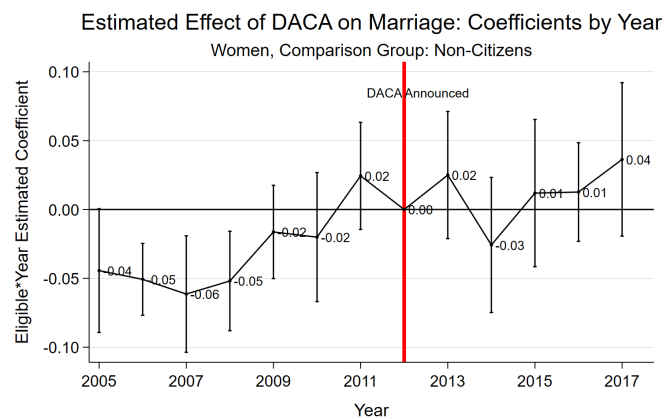
(b)

Figure 2.14: Men: Trends by Eligibility, Non-Citizens Comparison Group

Notes: This figure compares the regression-adjusted (for age and age of US entry) marriage trends of the eligible group and the comparison group that consists of non-citizens, where DACA eligibility variation derives only from age of entry. Panel (a) shows each group over time separately. Panel (b) plots the difference between the groups over time, with 95% confidence intervals.



(a)



(b)

Figure 2.15: Women: Trends by Eligibility, Non-Citizens Comparison Group

Notes: This figure compares the regression-adjusted (for age and age of US entry) marriage trends of the eligible group and the comparison group that consists of non-citizens, where DACA eligibility variation derives only from age of entry. Panel (a) shows each group over time separately. Panel (b) plots the difference between the groups over time, with 95% confidence intervals.

2.9 Tables

	(1) Citizens	(2) Foreign-Born Citizens	(3) Non-Citizens	(4) RD-Style
Eligible*After	0.0199*** (0.00470)	0.0223** (0.00973)	0.0263*** (0.00895)	0.0169 (0.0153)
Eligible*After*Female	-0.00502 (0.00689)	-0.00228 (0.0152)	0.00698 (0.0110)	0.0126 (0.0166)
Eligible*Female	0.0268*** (0.00479)	0.00384 (0.00726)	-0.0389*** (0.00721)	-0.0388*** (0.00867)
After*Female	-0.00234 (0.00309)	-0.00410 (0.0141)	-0.0114 (0.0108)	-0.0506*** (0.00907)
Female	0.0635*** (0.00323)	0.0874*** (0.00662)	0.123*** (0.00685)	0.150*** (0.00803)
Age controls?	Yes	Yes	Yes	Yes
Age of entry controls?	Yes	Yes	Yes	Yes
Demographic and social capital controls?	Yes	Yes	Yes	Yes
Labor market controls?	Yes	Yes	Yes	Yes
Education controls?	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes
State fixed effects?	0.22	0.28	0.38	0.38
State trends?	736596	133711	135598	70263
Dep Var Mean	0.142	0.142	0.150	0.138

Notes: This table reports DiD results for the effect of DACA eligibility on the probability its recipients are married. The outcome variable is an indicator of whether an individual is married. Each sample contains Hispanics with at least a high school diploma, age 18*-30. The treatment group is non-citizens who entered the US before the age of 16 and who have resided in the US for at least 5 years. Each comparison group is as defined in Table 1. All specifications include controls for age, economic, sex, race/ethnicity, urban, education, and social capital variables. They also include year and state fixed effects, and state trends. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.17: Marriage: Testing for Gender Heterogeneity

2.10 Comparison Group Selection

This section determines the best comparison group for the DACA-eligible group among potential candidates identified in Table 1. First, I compare various groups on observes, and how well they align with the treatment group. I then examine how well they each potentially adhere to the parallel trends assumption by analyzing pre-trends.

Table 17 reports summary statistics for each group. I draw particular attention to the following variables. Most straightforwardly, I compare groups on their

marriage rates, since it is the outcome, and since I am analyzing changes over time, it is helpful to also consider a flow measure of the outcome (i.e., whether a couple got married during the past year). I consider labor market outcomes, since they measure a person’s ability to marry, and attractiveness as a marriage partner. They also represent measures of the opportunity cost of dating and marrying instead of working more. I also include education, since it is well-documented that there have been diverging marriage trends across education levels [Parker and Stepler (2017)]. I also highlight age of entry because it has been shown to be a determinant of individual assimilation, especially a determinant of immigrant marriage behaviors. Bleakley and Chin (2010) found that through its effect on English language skills, which is a measure of assimilation, an earlier arrival age into the US decreases the probability of marriage and increases the probability of inter-ethnic marriage. Many differences across groups in the aforementioned observables might be attributed to differences in age composition. Marriage-, education-, and work-related variables follow a trajectory over the lifecycle. Different age groups in the US have also had different trends in marriage in recent decades [Wang (2018)].

Each potential comparison group is similar to the treatment group in some regard, and different in others. For example, citizens are closer to eligibles than other comparison groups in average age (23.6 versus 23.97 years), hours worked per week (27.39 versus 28.09 hours), employment rate (0.67 versus 0.673), and income (\$17,000 versus \$15,500). Citizens, however, have substantially more education than eligibles have, and unsurprisingly they entered the US at a much earlier age. Limiting the comparison group for foreign-born citizens brings the average age of

entry much closer to eligibles (8.555 versus 7.843), but it widens the gap substantially for labor market and education variables.

When considering the non-citizen comparison group, it is plausible that their age gap with eligibles, which exists mechanically because of DACA's requirements, drives a significant difference in other observables. The eligible group is, on average, 23.97 years old, and the non-citizens comparison group is 28.36 years old, on average. Groups with this large of an age gap unsurprisingly differ in marriage rates (eligibles: 0.282 , ineligible: 0.535), and labor market variables such as income (\$15,500 versus \$18,700) and hours per week (28.1 versus 30.6). They also differ in education, with the 57% of eligibles' highest education being high school graduation, and 6% having at least a college degree. Those same numbers are 66% and 10% for the non-citizens comparison group. Despite these observable differences, citizenship correlates with many unobservable characteristics. For example, it is possible that non-citizens' social networks overlap more with the DACA eligibles', and unlike the citizen group, both non-citizen comparison groups have unauthorized immigrants in them, which is a highly consequential feature of a person's life.

	(1)	(2)	(3)	(4)
	Eligible	Citizens	Foreign-Born Citizens	Non-Citizens
Married	0.282 (0.002)	0.209 (0.001)	0.301 (0.002)	0.535 (0.002)
Married in last year	0.047 (0.001)	0.038 (0.000)	0.045 (0.001)	0.045 (0.001)
Cohabiting	0.061 (0.001)	0.073 (0.000)	0.061 (0.001)	0.101 (0.001)
Female	0.479 (0.002)	0.515 (0.001)	0.532 (0.002)	0.454 (0.002)
Black	0.010 (0.000)	0.027 (0.000)	0.025 (0.001)	0.012 (0.000)
Age	23.970 (0.013)	23.595 (0.005)	24.582 (0.016)	28.356 (0.011)
Age of entry into US	7.843 (0.016)	0.876 (0.004)	6.123 (0.020)	19.282 (0.011)
Hours per week	28.089 (0.063)	27.387 (0.022)	29.932 (0.076)	30.593 (0.083)
Worked in past year	0.768 (0.001)	0.785 (0.001)	0.825 (0.002)	0.779 (0.002)
Working	0.673 (0.002)	0.670 (0.001)	0.721 (0.002)	0.704 (0.002)
Income	15536.516 (62.115)	17046.899 (26.082)	20773.806 (96.991)	18671.568 (89.928)
GED	0.037 (0.001)	0.038 (0.000)	0.025 (0.001)	0.040 (0.001)
High School diploma	0.565 (0.002)	0.380 (0.001)	0.326 (0.002)	0.658 (0.002)
Some college	0.377 (0.002)	0.484 (0.001)	0.482 (0.002)	0.236 (0.002)
College degree	0.058 (0.001)	0.136 (0.000)	0.192 (0.002)	0.105 (0.001)
<i>N</i>	84409	602888	54575	51189

Notes: This table reports summary statistics for the eligible and potential comparison groups. Numbers reported are means, with standard errors in parentheses. Restrictions for each sample are defined in Table 1.

Table 2.18: Summary Statistics by Group

Next, I compare groups using pre-trends analysis. The first method I use is the regression-adjusted mean analysis discussed in the Empirical Strategy section, equations (3)-(5). Results for the foreign-born citizens comparison group appear in Figures 2 (men) and 3 (women), panel (a).

Among men, the eligibles trend similarly to the foreign-born citizen comparison group, and during the post-DACA period, a structural change operating through eligible individuals is evident. During each year of the pre-DACA period, the eligible mean is below, but close to, the ineligible mean. After DACA is announced, the eligible mean is, with 2014 being an exception, above the ineligible mean, and

increasingly so over time. This accords with a change to marriage rates among eligibles that DACA caused. I report analogous results for alternative comparison groups in panel (a) Appendix Figures 8 and 10 for citizens and non-citizens comparison groups, respectively. The comparison group that uses all Hispanic citizens tracks to eligibles similarly to foreign-born citizens, but slightly and non-significantly trends above the eligible group during the pre-period. In the non-citizens sample, in which the comparison group is ineligible only by age of entry, throughout the pre-period, the ineligible's marriage rate declines while the eligibles' plateaus. In this sample, pre-trends are evident, and thus this graphical analysis evidences foreign-born citizens being a better comparison group for DACA-eligible men.

Among women, the comparison group that most convincingly lacks pre-trends is also foreign-born citizens. In each of the other samples (reported in panel (a) of Appendix Figures 9 and 11), to varying degrees, the marriage rate of the ineligible group trends downward more rapidly than eligibles' marriage rate before DACA's announcement. This is a concern, since such trends, continuing into the post-DACA period, would bias results away from zero. The graphical analysis thus suggests that foreign-born citizens are the best comparison group for DACA-eligible women.

I extend this to an event-study analysis, described in the Conceptual Framework section, particularly Equation (6). Results for foreign-born citizens are reported graphically for men and women in Figure 2 panel (b) and Figure 3 panel (b), respectively. Analogous results for comparison groups with all citizens and non-citizens are reported in panel (b) of Appendix Figures 8 and 10 for men and in panel (b) of Appendix Figures 9 and 11 for women. For each sex-comparison

group combination, I plot the coefficient for $Year_i * Eligible_i$. The value-added of these figures relative to panel (a) of Figures 2 and 3 is that they allow the annual effect size to be seen more easily, and they provide information on the precision of the estimates, which are noisy. Point estimates suggest the same conclusions as their panel (a) counterparts, but they highlight that these conclusions come with uncertainty, highlighting the importance of multiple approaches when evaluating comparison groups. To see these results in table form, see Table 18, which I discuss next.

Among men, I first discuss foreign-born citizens [Column (2)]. During the pre-period, estimates oscillate at and below zero before rising above zero, eventually significantly. The citizen comparison group [Column (1)] has all pre-period coefficients negative, and all but one of the post-period coefficients are positive.³³ The non-citizens comparison group appears to be weakest, and a rise in the magnitude of the coefficients throughout the pre-period is evident, which accentuates during the post-period, indicating potential bias away from zero.

Among women, all comparison groups also typically have negative coefficients during the pre-period, and positive coefficients during the post-period. However, for the comparison group with all citizens, coefficients steadily increase throughout the pre-period and into the post-period, and the non-citizens comparison group sample has a positive coefficient for 2011, the year before the announcement. Coefficients then typically continue to be positive after DACA's announcement. Similar to non-citizen men, this might suggest pre-trends that bias results away from zero.

³³Across all comparison groups for both sexes, the coefficient for 2014 is negative.

Coefficients for the foreign-born citizens comparison group remain steady below zero before DACA, and then increase after DACA.

(1)2-4(1)5-7	Men			Women		
	(1) Citizens	(2) Foreign-Born Citizens	(3) Non-Citizens	(4) Citizens	(5) Foreign-Born Citizens	(6) Non-Citizens
Eligible*2005	-0.0261 (0.0163)	-0.0201 (0.0221)	-0.0465** (0.0187)	-0.00815 (0.0196)	-0.0232 (0.0358)	-0.0443* (0.0224)
Eligible*2006	-0.0208 (0.0130)	0.000948 (0.0234)	-0.0165 (0.0172)	-0.0386*** (0.0116)	-0.0541** (0.0235)	-0.0507*** (0.0130)
Eligible*2007	-0.0349*** (0.0112)	-0.0184 (0.0187)	-0.0452** (0.0204)	-0.0252** (0.0108)	-0.00225 (0.0276)	-0.0614*** (0.0211)
Eligible*2008	-0.0229** (0.0100)	-0.0244 (0.0186)	-0.0192 (0.0240)	-0.0223** (0.0110)	-0.0245 (0.0251)	-0.0519*** (0.0180)
Eligible*2009	-0.00164 (0.0116)	0.00514 (0.0226)	0.0113 (0.0143)	-0.00963 (0.0152)	-0.00452 (0.0300)	-0.0163 (0.0169)
Eligible*2010	-0.0195 (0.0119)	-0.000297 (0.0209)	-0.00933 (0.0220)	-0.0132 (0.0158)	-0.0193 (0.0258)	-0.0201 (0.0233)
Eligible*2011	-0.0139 (0.0113)	-0.0140 (0.0176)	0.0167 (0.0188)	-0.00306 (0.0117)	-0.0113 (0.0209)	0.0244 (0.0194)
Eligible*2013	0.0172 (0.0103)	0.0241 (0.0175)	0.0375** (0.0186)	0.0112 (0.0150)	0.00792 (0.0257)	0.0250 (0.0230)
Eligible*2014	-0.0133 (0.0111)	-0.00838 (0.0223)	-0.00963 (0.0180)	-0.0148 (0.0146)	-0.0160 (0.0207)	-0.0258 (0.0244)
Eligible*2015	0.00209 (0.0121)	0.00314 (0.0249)	0.0204 (0.0175)	-0.00495 (0.0133)	-0.00466 (0.0191)	0.0119 (0.0266)
Eligible*2016	0.0227 (0.0166)	0.0304 (0.0203)	0.0403* (0.0216)	0.0172 (0.0119)	0.0305 (0.0194)	0.0127 (0.0178)
Eligible*2017	0.0130 (0.00853)	0.0408** (0.0159)	0.00440 (0.0153)	0.0142 (0.0141)	0.0101 (0.0264)	0.0364 (0.0277)
Age controls?	Yes	Yes	Yes	Yes	Yes	Yes
Age of entry controls?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	No	No	No	No	No	No
State fixed effects?	No	No	No	No	No	No
State trends?	No	No	No	No	No	No
Observations	359686	66651	71903	376910	67060	63695
R ²	0.125	0.121	0.129	0.123	0.110	0.133

Notes: This table contains an event study analysis of the likelihood of being married. The specification includes interactions between year indicators and Eligible. 2012 is the omitted interaction. The samples contain Hispanics of one sex with at least a high school diploma, age 18–30. The treatment group is non-citizens who entered the US before the age of 16 and who have resided in the US for at least 5 years. The comparison group for a column is the same as defined in Table 1. Robust standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.19: The Effect of DACA Eligibility on Marriage: Event Study Analysis

The strongest comparison group for both men and women is foreign-born citizens, and thus I focus my main analysis around them. All comparison groups have observables to which they are closer to the treatment group, but foreign-born citizens comparison groups appear to have pre-period behaviors that are most consistent with the parallel trends assumption holding.

Chapter 3: The Impact of Immigrant Legalization on Marriage

Evidence from the Immigration Reform and Control Act

Abstract

This chapter investigates the impact of the legalization provision of the Immigration Reform and Control Act of 1986 (IRCA) on marriage rates. The IRCA offered a pathway to citizenship for unauthorized immigrants. Using data on unauthorized immigrants that were legalized under the IRCA from the Legalized Population Survey (LPS) and a comparison group of US natives from the National Longitudinal Survey of Youth (NLSY79), the study implements an individual fixed effects strategy to estimate the changes in marriage rates as a result of the IRCA legalization. The findings reveal a statistically and economically significant increase in marriage rates for both men and women following IRCA legalization. Men experienced a 6.51 percentage point increase, while women saw an 8.29 percentage point increase. Unlike the effects observed in Chapter 2 for DACA, the permanent nature of the IRCA contributed to a stronger impact on marriage rates. The study explores potential mechanisms but finds inconclusive evidence regarding labor market outcomes and education as drivers of the marriage effect resulting from immigration liberalization.

3.1 Introduction

The policy covered in Chapter 2 – Deferred Action for Childhood Arrivals – is often referred to as the most important US immigration policy since 1986. That 1986 policy is the Immigration Reform and Control Act of 1986 (IRCA), signed by President Ronald Reagan at the end of that year. The act provided a pathway to citizenship for over 2.7 million unauthorized immigrants as well as increased enforcement measures. The legalization provision the IRCA likely lessened the substantial barriers to key institutions (such as marriage) that unauthorized immigrants face.

In this chapter, I analyze the question of how providing legalization of unauthorized immigrants through the IRCA affects marriage rates. I attempt to keep this analysis as similar as possible to the analysis in Chapter 2, but the opportunities the data provide are different. Thus, I also draw on Kossoudji and Cobb-Clark (2002) and Amuedo-Dorantes *et. al.* (2007), who use the same data and strategy as in this chapter, but study the effect of the IRCA on labor markets.

This chapter is informed by a similar conceptual framework used in Chapter 2 to analyze the costs and benefits of marriage for individuals with different immigration statuses. The removal of the threat of deportation, coupled with increased work opportunities and access to banking and credit through work authorization, could make marriage more attractive and feasible – both through exposure to a broader marriage market and by providing more resources. A potential countervailing force still exists, with increased work and educational opportunities possibly raising the

opportunity cost of dating and married life.

The key conceptual difference between the IRCA and DACA is the permanent nature of the legalization provided by the former. While permanent residents cannot easily be deported and deportation is impossible once citizenship is obtained, it is unclear whether this makes the hypothesized effects on marriage smaller or larger than those estimated in Chapter 2. On the one hand, the permanent nature of the IRCA legalization increases the expected returns to marriage even more. On the other hand, the temporary nature of DACA may incentivize individuals to move more quickly in the marriage market. Also, if DACA were to be rescinded, people with established families are a lower priority for immigration enforcement, potentially diminishing the importance the statutory permanence the IRCA offers.

I follow Kossoudji and Cobb-Clark (2002) by drawing from the Legalized Population Survey (LPS) – a dataset on IRCA legalization applicants with survey waves before and after the legalization – and the National Longitudinal Survey of Youth (NLSY79) to construct a comparison group of Hispanic Americans ages 21-36.

To estimate the effect of the IRCA legalization on marriage rates, I use the panel structure of the data and an individual fixed effects approach. I compare the treatment group with the comparison group before and after the IRCA’s legalization, while controlling for time-invariant unobserved individual heterogeneity with individual fixed effects. I then verify the robustness of the results by estimating them with alternative comparison groups that are chosen to match the unauthorized immigrants socioeconomic and cultural attributes.

I find evidence that the IRCA legalization increased marriage rates for both

men and women in a statistically and economically significant manner. In my preferred sample, obtaining legalization through the IRCA increased marriage rates by 6.51 percentage points for men. For women, I find a 8.29 percentage point effect on marriage rates. The effects of this permanent policy provide a contrast to the results for DACA – the temporary results in Chapter 2 – which were a lower-bound 2 percentage point effect for both sexes.

I use the two datasets' similar information available on labor market outcomes and education to provide insight on potential mechanisms. The estimates are imprecise so the conclusions for this section are very suggestive. For men, there is no evidence that labor market variables are an influential mechanism. For women, the coefficients move in the same direction as they do in the analogous Chapter 2 analysis of labor market mechanisms – suggesting that the women with the best labor market opportunities may be substituting away from or delaying marriage on the margin. Though the change in that coefficient is economically significant, it is not statistically significant. Finally, consistent with the DACA results in Chapter 2, there is little evidence that education for either sex is an important mechanism underlying the marriage effect to this immigration liberalization.

In addition to the studies of immigration and marriage outlined in Chapter 2, I contribute to the broader knowledge base on the effects of the IRCA's legalization. Labor market effects have been prominently studied. The estimated effects of the IRCA on wages range from increases of 6–13 percent (Rivera-Batiz, 1999; Kossoudji and Cobb-Clark, 2002). Amuedo-Dorantes *et al.* (2007) find that employment rates fell among the legalized immigrants because men became more selective about

the jobs they were willing to hold, while women exited the labor force. This is potentially related to increased government benefits increasing the outside option to employment. However, Pan (2010) finds that the IRCA raised female employment rates. Cascio and Lewis (2019) find that the IRCA legalization led to increases in income tax filing and uptake of the EITC.

Investigations into the impacts of the IRCA have also been extended to social outcomes. Baker (2015) finds that the IRCA led to decreases in crime – particularly property crimes. Cascio and Lewis (2023) find that the IRCA led to more immigrants bringing their families into the US.

This chapter seeks to analyze the effect of the policy on a key social outcome: marriage. It is structured as follows. Section 2 provides background on the policy. Section 3 explains the conceptual framework used to inform the empirical analysis. Section 4 describes the data used. Section 5 lays out the empirical strategy. Section 6 presents the empirical results. Finally, Section 7 concludes the chapter.

3.2 Background

The Immigration Reform and Control Act (IRCA) is a US federal law that was enacted on November 6, 1986. The law was a landmark piece of legislation that aimed to address the issue of unauthorized immigration into the United States. The purpose of the law was to provide a path to citizenship for unauthorized immigrants already living in the US, while also strengthening US border controls and enforcing penalties against employers who hire unauthorized workers – representing

a compromise between factions that emphasized humanitarian concerns and those that emphasized rule-of-law and border security.

The key provisions of the law include a legalization program that offered a pathway to legal status for unauthorized immigrants who had been living in the United States continuously prior to January 1, 1982. These immigrants were required to pass a background check, pay a fee, and demonstrate knowledge of the English language and US history to meet eligibility criteria. The IRCA also imposed employer sanctions, making it illegal for employers to knowingly hire unauthorized workers. Employers were required to verify their employees' work eligibility by examining certain documents, such as a Social Security card or a green card, and violators of these provisions were subject to fines and penalties. Additionally, the IRCA allocated funds for increased border enforcement, including hiring additional Border Patrol agents and constructing new border fencing. The law also made it illegal to produce or use false documents such as fake Social Security cards or green cards. Finally, the IRCA established a separate amnesty program for certain farm workers who had worked in the US for at least 90 days during the previous year. The key focus on this paper is the legalization.

To be eligible for legalization, unauthorized immigrants had to meet certain requirements: continuous residency in the United States since January 1, 1982, being of good moral character, and paying a fee. The legalization program was aimed at bringing unauthorized immigrants into the mainstream of American society and providing them with a legal status that would enable them to work and live without fear of deportation. Over 2.7 million unauthorized immigrants applied for

legalization between 1987 and 1988. This is in comparison to a total US population of about 240 million.¹ ²

3.3 Conceptual Framework

I consider individuals that behave as in Becker (1973), weighing the costs and benefits of marrying individuals with various immigration statuses or remaining unmarried. Similarly to Chapter 2, such a framework is appropriate for this chapter, where the key difference is that the amnesty offered by the IRCA is permanent.

Like DACA, the legalization provided by the IRCA removes the threat of deportation, increasing the expected return from marriage. And there is also a potential countervailing effect: decreased legal benefits of marriage to a US citizen. While the work authorization that comes with legalization makes recipients more attractive to potential partners due to higher earnings, increased work opportunities may raise the opportunity cost of dating and married life. Access to banking and credit through work authorization may make marriage more attractive and feasible. Additionally, greater exposure to a broader set of society stemming from the legalization could change the types of partners the newly legalized desire, as well as their desirability to a larger range of people.

The permanent nature of the IRCA's legalization creates a potential difference from the results in Chapter 2 for DACA. Permanent residents cannot easily be deported, and deportation is impossible once citizenship is obtained. Whether

¹<https://fred.stlouisfed.org/series/POPTOTUSA647NWDB>

²To my knowledge, there are no reliable estimates of the unauthorized immigrant population at that time.

this makes short-to-medium term effects smaller or larger than those estimated in Chapter 2 is ambiguous. On one hand, the permanent nature of the IRCA legalization increases the expected returns to marriage even more. In economist jargon, these returns include marriage-specific home production and surplus utility generated from a match. In plain English, these returns could include companionship, the pursuit of shared interests and values, and starting a family together. These things of value are destroyed, or at least severely compromised, by the deportation of a partner. I believe this to be the more likely effect. But, it is also conceivable that the temporary nature of DACA may incentivize individuals to move more quickly and shift marriages forward, creating a larger short-term effect. If DACA were to be rescinded, people with established families are a lower priority for immigration enforcement, potentially diminishing the importance of the permanence. While it is more straightforward for the permanent policy to have a larger effect, it is ultimately an empirical question.

Lastly, the effect of the IRCA may differ for men and women. As this policy is further back than DACA (in the 1980's vs the 2010's), a plausible prediction is that sex differences in the results could be even more driven by traditional gender norms. In June 2012, when DACA was announced, the labor force participation for Hispanic or Latino men and women (age 20 or older) were 81% and 60%, respectively.³ In June 1987, the year that the applications for the IRCA legalization were accepted, those same rates were 85% and 53%.⁴ Increased economic prospects stemming from

³<https://fred.stlouisfed.org/series/LNU01300035>

⁴<https://fred.stlouisfed.org/series/LNU01300034>

legalization could be an even greater facilitator of marriage for men. For women, a weaker norm of women working may mean that there is less of an incentive to substitute away from marriage in response to rising economic prospects, because there is less of a cultural expectation to take advantage of those prospects.

3.4 Data

There are two datasets I draw from. One for the treatment group, and the other for the comparison group. Those datasets are the Legalized Population Survey (LPS) and the National Longitudinal Survey of Youth (NLSY79), respectively. Kossoudji and Cobb-Clark (2002) and Amuedo-Dorantes *et. al.* (2007) draw from them in a similar manner.

The LPS is a survey conducted by the Immigration and Naturalization Service (INS) that had two waves. The first, conducted in 1989, asked respondents for information about when they first migrated to the US and the week before the amnesty application was filed in 1987. Two-thirds of the initial wave of respondents were resurveyed in 1992 about their current information. The NLSY79 is a longitudinal dataset created by the Bureau of Labor Statistics (BLS) that follows a sample of individuals born between 1957 and 1964.

To ensure comparability between the treatment and comparison groups, the variables used in the analysis are included in both datasets. These variables include basic demographic variables (age, number of children, and sex), labor market variables (employment, home production,⁵ being in school, weekly hours, and weekly

⁵In the LPS, this variable is the answer “keeping house” in response to the question: “What

earnings), and education variables (years of education and current enrollment in school).

For the LPS group, I impose most of the restrictions of Kossoudji and Cobb-Clark (2002). Most of these restrictions are made for the purpose of maintaining cultural similarity and targeting similar cohorts. First, I limit my sample to Mexican and Central Americans. I also limit my sample to those who arrived in 1975 or later. This is to limit the analysis to a smaller migration window (1975-1982) and thus only focus on similar cohorts. I also limit my sample to those who entered the US without inspection. I deviate from Kossoudji and Cobb-Clark (2002) in relaxing their restriction that the individuals needed to have worked prior to legalization, since the focus of their paper is labor markets, whereas mine focuses on marriage markets.

For the NLSY comparison group, I make the following sample restriction. Rather than follow Kossoudji and Cobb-Clark (2002) in allowing natives and immigrants to be in the comparison group, I opt to follow Amuedo-Dorantes *et al.* (2007) in restricting the sample to natives, because the IRCA has been found to affect non-IRCA unauthorized immigrants (Bansak and Raphael, 2001; Kaushal, 2006).⁶ However, I allow immigrants to be in some of my alternate comparison groups presented in Section 6.3 because, nevertheless, other immigrants may be a more valid comparison group due to similar time-variant unobservable characteristics, such as social capital and non-measurable human capital.

were you doing most of week before [applying for residence]?” In the NLSY, this variable is the answer “keeping house” in response to the employment status question.

⁶Ideally, I would use foreign-born citizens as I do in the previous chapter, however, that group is too small in these data.

I also deviate from Kossoudji and Cobb-Clark (2002) in restricting the sample to being a balanced panel and having the key variables available pre-IRCA and post-IRCA. This will allow me to implement the individual fixed effects strategy and keep the sample consistent across specifications. Summary statistics are reported in Table 1 for the treatment and comparison groups, before and after the IRCA legalization was implemented.

	(1)	(2)	(3)	(4)
	LPS Pre-IRCA	LPS Post-IRCA	NLSY Pre-IRCA	NLSY Post-IRCA
Age	26.050 (0.103)	31.050 (0.103)	25.655 (0.057)	30.886 (0.057)
Female	0.422 (0.017)	0.422 (0.017)	0.499 (0.013)	0.499 (0.013)
Married	0.614 (0.017)	0.763 (0.015)	0.476 (0.013)	0.561 (0.013)
Working	0.866 (0.012)	0.775 (0.014)	0.711 (0.011)	0.745 (0.011)
Home Production	0.092 (0.010)	0.117 (0.011)	0.114 (0.008)	0.125 (0.008)
In School	0.008 (0.003)	0.009 (0.003)	0.018 (0.003)	0.125 (0.008)
Weekly Hours	36.674 (0.536)	34.601 (0.589)	34.962 (0.446)	36.524 (0.459)
Weekly Earnings	207.570 (4.694)	287.494 (7.343)	262.866 (8.557)	373.892 (8.851)
Children	1.161 (0.044)	1.918 (0.050)	0.974 (0.030)	1.450 (0.035)
Years of Education	7.944 (0.120)	8.158 (0.121)	11.902 (0.063)	12.158 (0.067)
<i>N</i>	857	857	1577	1577

Notes: This table reports summary statistics for treatment and comparison groups before and after the IRCA. Numbers reported are means, with standard errors in parentheses.

Table 3.1: Summary Statistics by Treatment Status and Period

The DiD strategy requires the treatment and comparison group to have similar trends in marriage in absence of the treatment. The individual fixed effects strategy

requires time-variant unobservables to be uncorrelated with the treatment. Thus, summary statistics in levels do not provide direct evidence for the identifying assumptions in either case. However, to the extent that groups with similar pre-period characteristics might make two groups more likely to be similar in marriage trends and time-variant observables, the pre-period characteristics are worth noting.

Table 1 shows that the average age of IRCA recipients was about 26 years in the pre-IRCA period, which is similar to the average age of Hispanics in the NLSY (25.7). Given the strong relationship between age and marriage, this is an important variable to match on.

A weaker match is the overall marriage rates between the groups. The pre-period proportion of married individuals in the treatment group is 61.4% 1987 versus 47.6% in the comparison group. Despite this, they have similar numbers of children (1.16 vs. 0.974).

The proportion of individuals who were employed also has large differences. This pre-period share is 86.6% for the treatment group, while it is 71.1% for the comparison group. The average weekly earnings of IRCA recipients and the NLSY group are \$207.57 and \$262.53, respectively. The groups have similar rates of home production and being in school, and they also work similar hours.

There is a substantial difference in years of education between the treatment and comparison groups. Before the implementation of the IRCA, the average years of education for the LPS group was 7.94, while for the NLSY group it was 11.9. This difference is likely due to the fact that the NLSY sample largely faced mandatory education laws and lived in the wealthier US society, while the LPS sample consists

entirely of unauthorized immigrants, a group that is well-known to face barriers to education. Thus, I also test the robustness of my results by limiting the NLSY comparison group to individuals with less than a high school education.

While the two groups do share some similarities in age and fertility, differences in key levels of observables such as baseline marriage rates and education are a cause for concern. Thus, it will be important to use alternate comparison groups to test the robustness of the results.

3.5 Empirical Strategy

I explore the effect of IRCA on marriage in the following manner. First, I non-causally examine the data to search for evidence of structural changes in marriage markets after the IRCA legalization. Second, I compare results from two identification strategies: difference-in-differences and individual fixed-effects. Comparing the estimates from DiD (the identification strategy used in Chapter 2) to the estimates from individual fixed-effects (the identification strategy favored in this chapter) serves to contrast the two chapters. Only the data in this chapter allow for individual fixed-effects. Finally, I test the robustness of the individual fixed effects results to alternative comparison groups.

As mentioned, the first part is a descriptive test for structural changes in marriage markets. I run period-by-period marriage regressions for the treatment and comparison groups. The outcome variable is marriage, and the explanatory variables are various labor market and educational variables. This describes how

these various variables relate to marriage markets, before and after the IRCA. Should the coefficients change in sign, or in an economically significant magnitude without changing sign, that could signify a structural change brought about in the marriage market from legalization. The equations are also estimated for the comparison group to ensure that there are no broader structural changes happening at the same time. This analysis is analogous to Kossoudji and Cobb-Clark's (2002) analysis of structural changes in the labor market as a response to IRCA.

Second, in an effort to highlight the differences between this chapter and the previous chapter about DACA, I compare difference-in-differences (DiD) results to individual fixed-effects results. In this chapter, the set of variables I have access to is limited because they need to be comparable across the LPS and NLSY. In contrast, both the treatment and comparison groups in the DACA chapter are in the American Community Survey, which allows for a more expansive set of variables to be analyzed, such as age of arrival into the US, finer geographic variables, spouse characteristics, and family structure variables. Importantly, the annual repeated cross-section structure of the ACS allows for multiple pre-periods in the data, which allows for pretrends analyses to examine the plausibility of the main identifying assumption of DiD (parallel trends). The 2x2 structure of the dataset in this chapter does not allow for such analyses. However, the strength of these data, unlike the ACS data, is that they are longitudinal. This allows for an individual fixed-effects strategy that controls for time-invariant unobserved heterogeneity. Thus, I will describe these dual identification strategies in what follows.

IRCA legalization – the treatment of interest – is not randomly assigned in my

dataset, so I must use a strategy that allows me to use quasi-experimental variation. Because there is a pre-treatment period and a post-treatment period, I am able to use DiD. The treatment group is the LPS sample, which consists of immigrants eligible for legalization under the IRCA, while the comparison group is the NLSY sample, consisting of non-eligible US natives (in my preferred sample). The outcome variable of interest is a binary indicator that takes the value of one if the individual is married and zero otherwise. I implement the DiD by estimating the following equation:

$$Married_{i,t} = \gamma_0 + \gamma_1(Post_t \times IRCA_i) + \gamma_2 Post_t + \gamma_3 IRCA_i + X'_{i,t} \gamma_4 + \epsilon_{i,t} \quad (3.1)$$

where $Married_{i,t}$ is a binary indicator for the marital status of individual i in time period t , $Post_t$ is a dummy variable for post-treatment periods, $IRCA_i$ is a dummy variable for individuals in the treatment group, and $Post_t \times IRCA_i$ is the interaction term between post-treatment periods and the treatment group, $X'_{i,t}$ is a vector of individual-year-level covariates. The parameter of interest is γ_1 , which measures the differential change in the outcome variable between the treatment and comparison groups after the treatment. The key identifying assumption is that the outcomes for the two groups would evolve in a similar manner in the absence of the treatment.

Next, utilizing the panel structure of my data, I employ an individual fixed effects strategy to estimate the causal effect of the IRCA on marriage rates of indi-

viduals. I compare the treatment group with the comparison group before and after the IRCA's legalization while accounting for time-invariant unobserved individual heterogeneity with the fixed effects. The treatment group and comparison group are the same, as is the outcome of interest.

My estimating equation is:

$$Married_{i,t} = \beta_0 + \beta_1(Post_t \times IRCA_i) + \beta_2 Post_t + X'_{i,t} \beta_3 + \phi_i + \varepsilon_{i,t} \quad (3.2)$$

The main difference between Equation (1) and Equation (2) is that the latter excludes time-invariant terms and includes ϕ_i , which is the individual fixed effect. The coefficient of interest is β_1 , which measures the estimated effect of legalization on individual i . The identifying assumption is that there is no time-varying heterogeneity that is correlated with the treatment and affects marriage rates. I attempt to address this concern by estimating Equation (2) on samples with different comparison groups. Different comparison groups are likely to have different unobservable characteristics. Thus, if the results are consistent across different comparison groups, it is more likely that the the individual fixed effects are doing an adequate job of capturing potential sources of bias.

In all specifications, I control for age due to the mechanical relationship between marriage and age. And as in Chapter 2, I estimate the effects by sex separately. I present the results in the following section.

3.6 Results

In this section, I present and examine the results from the estimations outlined in the previous section. First, I do so for the correlational analysis of structural changes in marriage markets in response to the IRCA. Second, I compare the DiD and individual FE results. And finally, I present results that demonstrate the degree to which the results are robust to using alternative comparison groups.

3.6.1 The Relationship of the Marriage Market to Labor Market and Education

Here, I present the results on whether there is evidence of a suggestive change in the correlation of education and labor market outcomes and the marriage rate. I note that for this subsection, in contrast to the main analysis, I use a sub-sample in which all marriage, labor-market, and educational variables are available for everyone. A key difference, which will prove important, is that in the post-period are almost all men in the LPS are employed and almost no men in the LPS are in school. For each combination of sample, sex, and period, I regress marriage on variables such as age, employment, weekly earnings, current enrollment in school, and years of education. The results for men are presented in Table 2, while the results for the women are presented in Table 3. It is important to note that this analysis closely follows the approach taken by Kossoudji and Cobb-Clark (2002) in their analysis of labor markets.

	(1)	LPS (2)	(3)	(4)	NLSY (5)	(6)
	Pre-IRCA	Post-IRCA	Pooled	Pre-IRCA	Post-IRCA	Pooled
Age	0.0514*** (0.00805)	0.00969 (0.00719)	0.0298*** (0.00439)	0.0252** (0.0121)	0.00386 (0.0125)	0.0162*** (0.00598)
Employed	-0.0780 (0.168)	- (-)	-0.111 (0.209)	0.239*** (0.0674)	0.143 (0.107)	0.199*** (0.0589)
Weekly earnings	0.000720*** (0.000167)	0.000329*** (0.000101)	0.000430*** (0.0000878)	0.000311** (0.000135)	0.000339*** (0.0000939)	0.000344*** (0.0000769)
In School	-0.341** (0.172)	- (-)	-0.495** (0.213)	0.351* (0.179)	0.157 (0.264)	0.276* (0.158)
Years of Edu	-0.00609 (0.00663)	-0.0122* (0.00636)	-0.00935** (0.00473)	-0.0402*** (0.0117)	-0.0132 (0.0121)	-0.0254*** (0.00857)
Dep Var Mean	0.57	0.77	0.67	0.42	0.57	0.50
Observations	411	411	822	430	430	860
R ²	0.162	0.0328	0.121	0.0865	0.0489	0.0803

Notes: This table shows the conditional correlations between marriage and other outcomes for the men in the LPS and NLSY samples before and after the IRCA legalization occurred. It is a regression of marriage on age, employed, engaging in home production, weekly hours worked, weekly earnings, and years of education.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.2: Marriage Markets Before and After IRCA, Men

I first examine the results for men. For both the LPS and NLSY samples, age is a significant and positive predictor of marriage in the pre-period, but it becomes statistically insignificant (yet still positive) in the post-period. Since there is no theoretical reason to expect the IRCA to have influenced age as a predictor of marriage, I believe this finding is more likely due to the fact that the samples are older in the post-period, where age has less predictive power for marriage.

In terms of the treatment group, employment (conditional on earnings) does not appear to be a significant predictor of marriage in either period. Indeed, in the post-period it is not even estimable due to a lack of variation that is not explained by earnings. As mentioned previously mentioned, almost all men are employed in the LPS subsample in the post-period. So estimating a coefficient for employment on marriage is too demanding of the data. However, weekly earnings show a significant positive association with marriage in both periods for both groups. Notably, after

legalization, the coefficients on weekly earnings for the treatment group converge to a similar magnitude as those for the comparison group. This suggests that legalization recipients may have entered marriage markets similar to those of native Hispanics.

The results for education are as follows. Being in school has a negative effect for LPS men's marriage prospects in the pre-period. As mentioned previously, almost no men are in school in LPS subsample in the post-period. The effect of years of education is negative in the pre-period and more negative for men in the post-period. It is a positive predictor of marriage in the NLSY men's sample. Perhaps the unauthorized immigrant men who do access education have a larger incentive to focus on establishing themselves and delaying marriage.

	LPS			NLSY		
	(1)	(2)	(3)	(4)	(5)	(6)
	Pre-IRCA	Post-IRCA	Pooled	Pre-IRCA	Post-IRCA	Pooled
Age	0.0314*** (0.0110)	0.0188* (0.0107)	0.0216*** (0.00535)	0.0210* (0.0124)	-0.0157 (0.0110)	0.00841 (0.00670)
Employed	-0.0945 (0.0584)	-0.0413 (0.0920)	-0.0987** (0.0490)	-0.265** (0.134)	-0.581*** (0.126)	-0.354*** (0.0839)
Weekly earnings	-0.000162** (0.0000651)	-0.000368*** (0.0000821)	-0.000238*** (0.0000813)	0.000158 (0.000624)	0.000735*** (0.000239)	0.000752*** (0.000216)
In School	-0.133 (0.160)	-0.143 (0.104)	-0.172*** (0.0652)	-0.215 (0.329)	- (-)	-0.295 (0.326)
Years of Edu	-0.00986 (0.0129)	0.0191 (0.0117)	0.00404 (0.00886)	-0.0213** (0.00886)	-0.0181** (0.00906)	-0.0180*** (0.00657)
Dep Var Mean	0.50	0.55	0.53	0.63	0.73	0.68
Observations	467	467	934	207	207	414
R ²	0.0516	0.0407	0.0403	0.0868	0.0596	0.0605

This table shows the conditional correlations between marriage and other outcomes for the women in the LPS and NLSY samples before and after the IRCA legalization occurred. It is a regression of marriage on age, employed, engaging in home production, weekly hours worked, weekly earnings, and years of education.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.3: Marriage Markets Before and After IRCA, Women

Now, I discuss the results for women. Similar to men, age becomes a weaker predictor of marriage. Higher earnings becomes an even stronger negative predictor of marriage in the post-period. This contrasts with earnings being a positive predictor of marriage for NLSY women. In a Becker model, this could suggest stronger

preferences for female household production in unauthorized immigrant families. It may also reflect lower relative wages for women in unauthorized immigrant communities. Additionally, years of education consistently shows a negative association with marriage in the LPS sample for women.

3.6.2 Main Results

Table 4 presents the DiD and individual fixed effects results for both men [Columns (1)-(2)] and women [Columns (3)-(4)]. The point estimates are similar across the two identification strategies for both sexes. In the first column for each sex [Columns (1) and (3)], I present the DiD results. In the second column for each sex [Columns (2) and (4)], I present the fixed effects results. The DiD estimate for men is 6.98 percentage points (although imprecise), while the fixed effects estimate [Column (2)] is 6.51 percentage points. For women, the DiD strategy [Column (3)] yields an imprecise estimate of 7.62 percentage points, while the fixed effects estimate [Column (4)] indicates an 8.29 percentage point effect. These estimates are relatively large compared to the results presented for DACA in Chapter 2 – which is a lower-bound 2 percentage point effect for men and women. Thus, they are also substantial within the context of the marriage literature as a whole, as discussed in Chapter 2. Given that the IRCA is a permanent policy while DACA is a temporary one, the former should be expected to have a larger effect. An important caveat is that the DACA estimates are lower-bounds, while these estimates are meant to be point estimates. Another difference between Chapter 2 and Chapter 3 are the ages

of the samples. The Chapter 2 sample is restricted between 18-30 with a mean age of 24 years old in the treatment group. The Chapter 3 sample is restricted between 21-36 with a mean age of 28.6 years old in the treatment group. They possibly have different propensities to marry independent of differences in the effects of the policies. Bearing this all in mind, comparing the estimates across the chapters directly needs to be done with some degree of caution.⁷

	Men		Women	
	(1)	(2)	(3)	(4)
	DiD	FE	DiD	FE
Legalization*Post	0.0698 (0.0457)	0.0651* (0.0368)	0.0762 (0.0477)	0.0829** (0.0376)
Age controls?	Yes	Yes	Yes	Yes
Dep Var Mean	0.57	0.57	0.59	0.59
Observations	2136	2136	1922	1922
R ²	0.0330	0.733	0.0127	0.750

Notes: This table compares DiD and individual fixed effects results for whether IRCA legalization affected the probability of being married for its recipients. The treatment group is IRCA applicants, ages 21-36. The comparison group consists of Hispanics, ages 21-36 from the NLSY79.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4: The Effect of the Legalization on Marriage, DiD vs FE

3.6.3 Robustness

In this subsection, I examine the robustness of the results to a set of alternative comparison groups. Pre-period summary statistics for these groups are reported and compared to the LPS group in Table 5. The individual FE results with these alternate comparison groups are presented in Table 8 for men and Table

⁷In unreported results, I estimate the DiD specification on a sub-sample of individuals who began the sample period single. These estimates use more-similar identifying variation to the fixed effects strategy, and also are arguably better-targeted conceptually. But they yield implausibly large results, thus they require future research.

9 for women. The comparison groups consist of native-born Hispanics who have less than a high school education, non-citizens, foreign-born individuals, individuals with a foreign-born mother, and individuals with a mother who has less than a high school education. Native-born Hispanics who have less than a high school education and those with mothers with less than a high school education are used because of the degree to which the treatment group differs from all other comparison groups in years of education. The rest of the comparison groups are chosen for their potential to be closer culturally to the treatment group, since they consist of different groups of first and second generation immigrants.

One important observation from Table 5 is that the treatment group still exhibits significantly lower educational attainment compared to any of the comparison groups. The only comparison group that is relatively closer to the treatment groups in terms of education is the group of Hispanic natives with less than a high school education (Column (2), 9.53 years). However, it is worth noting that if there are negative spillovers from immigration liberalization to a comparison group, it may affect those who are more closely educated [Borjas (2003)]. Therefore, since this comparison group is the closest one to the treatment group is education, there is a possibility that this particular comparison group is negatively affected by the legalization, which could introduce bias away from zero in the estimates obtained from it.

	(1)	(2)	(3)	(4)	(5)	(6)
	LPS	Less than HS	Non-Citizens	Foreign-Born	Mom Foreign-Born	Mom Less than HS
Age	26.050 (0.103)	25.381 (0.131)	25.924 (0.137)	25.960 (0.114)	25.691 (0.090)	25.628 (0.066)
Female	0.422 (0.017)	0.477 (0.030)	0.482 (0.030)	0.464 (0.025)	0.459 (0.020)	0.505 (0.015)
Married	0.614 (0.017)	0.434 (0.030)	0.558 (0.030)	0.528 (0.025)	0.498 (0.020)	0.479 (0.015)
Working	0.866 (0.012)	0.552 (0.030)	0.676 (0.028)	0.719 (0.022)	0.750 (0.017)	0.687 (0.014)
Home Production	0.092 (0.010)	0.192 (0.024)	0.162 (0.022)	0.131 (0.017)	0.112 (0.012)	0.137 (0.010)
In School	0.008 (0.003)	0.007 (0.005)	0.007 (0.005)	0.012 (0.005)	0.014 (0.005)	0.016 (0.004)
Weekly Hours	36.674 (0.536)	30.960 (1.255)	34.101 (1.147)	35.671 (0.908)	36.206 (0.680)	34.189 (0.541)
Weekly Earnings	207.570 (4.694)	178.377 (14.135)	248.069 (23.668)	274.926 (18.164)	278.364 (12.178)	235.425 (9.497)
Children	1.161 (0.044)	1.441 (0.089)	1.129 (0.076)	0.988 (0.059)	0.912 (0.047)	1.082 (0.037)
Years of Education	7.944 (0.120)	9.527 (0.087)	10.554 (0.207)	11.203 (0.168)	11.782 (0.120)	11.442 (0.073)
<i>N</i>	857	281	278	405	645	1166

Notes: This table reports summary statistics for treatment and alternate comparison groups before the IRCA. Numbers reported are means, with standard errors in parentheses. The comparison groups consist of subsamples of the original NLSY sample. These include individuals who are native Hispanics who have less than a high school education, are non-citizens, are foreign-born, have a foreign-born mother, and have a mother with less than a high school education.

Table 3.5: Pre-Period Summary Statistics for Alternate Comparison Groups

I first discuss the robustness results for men. Recall the baseline estimate of 6.51 percentage points from Table 4. Across different comparison groups, the point estimates range from 3.88 percentage points to 8.04 percentage points – with one outlier that is 13.6 percentage points. That one is the comparison group with less than a high-school education, precisely the one with the recently mentioned potential upward bias. Overall, these results confirm a positive effect that is notably larger in point-estimate than the DACA estimates, but do not point towards a precise coefficient. Indeed, the estimates are possibly all similar given the lack of precision. However, given that it is a native-born citizen group with higher education, it is also least likely to be affected by negative spillovers in the labor and marriage markets.

Thus, it is my preferred comparison group.

For women, the baseline estimate from Table 4 is 8.29 percentage points. The point estimates yielded from the comparison groups range from 8.85 percentage points to 17.2 percentage points – all larger than the baseline. The 17.2 percentage point effect is also yielded from the comparison group with native-born Hispanics with less than a high school education, which might also face the upward bias from negative spillovers. The other estimates range from 8.85 to 13.2 percentage points – but the lack of precision makes them statistically similar. Again, given the lack of precision, providing a precise preferred estimate is not feasible here.

In summary, these robustness checks do provide evidence of a marriage effect that is larger than that for DACA. However, they still fall in a wide range, and thus do not give us confidence in any particular point estimate.

	(1) No HS	(2) Non-Citizen	(3) Foreign-Born	(4) Mom Foreign-Born	(5) Mom No HS
Legalization*Post	0.136** (0.0659)	0.0719 (0.0589)	0.0413 (0.0505)	0.0388 (0.0405)	0.0804** (0.0374)
Dep Var Mean	0.61	0.63	0.62	0.60	0.57
Observations	1271	1278	1424	1688	2144
R ²	0.687	0.742	0.741	0.755	0.713

Notes: This table has individual fixed effects results for whether the IRCA amnesty affected the probability of being married for its male recipients. The treatment group is IRCA applicants, ages 21-36. The comparison groups are altered from the baseline for robustness. They consist of subsamples of Hispanics, ages 21-36 from the NLSY79. They include individuals with less than a HS education, non-citizens, foreign_born, mother foreign-born, and mother with less than high school education.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6: Alternate Comparison Groups, Men

	(1) No HS	(2) Non-Citizen	(3) Foreign-Born	(4) Mom Foreign-Born	(5) Mom No HS
Legalization*Post	0.172** (0.0688)	0.132** (0.0580)	0.112** (0.0492)	0.0964** (0.0441)	0.0885** (0.0382)
Dep Var Mean	0.66	0.71	0.69	0.67	0.61
Observations	975	992	1100	1316	1902
R ²	0.741	0.751	0.770	0.755	0.746

Notes: This table has individual fixed effects results for whether the IRCA amnesty affected the probability of being married for its female recipients. The treatment group is IRCA applicants, ages 21-36. The comparison groups are altered from the baseline for robustness. They consist of subsamples of Hispanics, ages 21-36 from the NLSY79. They include individuals with less than a HS education, non-citizens, foreign-born, mother foreign-born, and mother with less than high school education.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.7: Alternate Comparison Groups, Women

3.6.4 Mechanisms

To provide insights into potential mechanisms, I also control for whether the individual is currently employed, weekly hours worked, weekly earnings, and whether the individual stays at home (labor market mechanisms). And then years of education and whether they are in school (education mechanisms). This is meant to be similar to the mechanisms analysis in Chapter 2. The labor market and education controls are surely outcomes of the IRCA. Thus, they are “bad controls” for the purpose of obtaining an estimate of the effect if the IRCA. But seeing how the estimate changes with their inclusion can provide insights into the underlying mechanisms of the effect. A limitation of the analysis in section is that the sample is limited only to individuals who have all the labor market and education variables.

The mechanism results for men are presented in Table 8. In Column (1), I present the specification where only age is controlled for. Though there is a loss of precision due to the smaller sample size, this yields an estimate of 6.5 percentage

points – which is virtually the same effect as in Table 4, Column (2). After controlling for labor market outcomes, the point estimate remains relatively stable in Table 8, Column (2), with a 6.85 percentage point effect. This finding is consistent with the results observed in the DACA study. While Pope (2016) finds across-the-board improvements in labor market outcomes in response to DACA, Amuedo-Dorantes *et al.* (2007) presents a more nuanced story. Specifically, they find decreases in labor force participation and employment for men, accompanied by increases in wages. Thus, with key labor market variables going in different directions for men, it does not come out as clearly as a mediator of marriage as it did in Chapter 2.

When introducing education controls in Column (3), the coefficient moves up slightly to 6.92 percentage points. It is statistically and economically indistinguishable from Columns (1) and (2). This is consistent with education not being an important mechanism for the effect of the IRCA legalization on marriage. This is similar to the DACA results for education in Chapter 2.

	(1)	(2)	(3)
Legalization*Post	0.0650 (0.0406)	0.0685* (0.0411)	0.0692* (0.0411)
Age controls?	Yes	Yes	Yes
Labor market controls?	No	Yes	Yes
Educaton controls?	No	No	Yes
Dep Var Mean	0.58	0.58	0.58
Observations	1682	1682	1682
R ²	0.741	0.743	0.743

Notes: This table has individual fixed effects results for whether IRCA amnesty affected the probability of being married for its recipients. The focus of this table is on mechanisms. The treatment group is IRCA applicants, ages 21-36. The comparison group consists of Hispanics natives, ages 21-36 from the NLSY79. Controls include number of children, employment, engaging in home production, weekly hours worked, weekly earnings, and years of education.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.8: The Effect of the Legalization on Marriage, Men

Table 9 presents the results for women. The structure of the table is similar to Table 8. In Column (1), I present the specification where only age is controlled for. The estimate of 7.95 percentage points is indistinguishable from the same specification in the larger sample in Table 4, Column (5). After controlling for labor market outcomes, the point estimate moves an economically significant but statistically insignificant amount up to 8.75 percentage points. Though the lack of precision means it should be taken with a grain of salt, the finding of an increased effect after controlling for labor market variables is consistent with the DACA results in Chapter 2. The economic interpretation of the results is that women with the strongest labor market opportunities in response to the IRCA are substituting away from or delaying marriage on the margin.

When introducing education controls in Column (3), the coefficient loses even more precision and moves down to 7.11 percentage points. If taken at face-value, it suggests that education could be a facilitator of marriage. But the results are far too imprecise to make that statement with any degree of confidence.

	(1)	(2)	(3)
Legalization*Post	0.0795* (0.0482)	0.0875* (0.0501)	0.0711 (0.0510)
Age controls?	Yes	Yes	Yes
Labor market controls?	No	Yes	Yes
Educaton controls?	No	No	Yes
Dep Var Mean	0.57	0.57	0.57
Observations	1348	1348	1348
R ²	0.749	0.753	0.756

Notes: This table has individual fixed effects results for whether IRCA amnesty affected the probability of being married for its recipients. The focus of this table is on mechanisms. The treatment group is IRCA applicants, ages 21-36. The comparison group consists of Hispanic natives, ages 21-36 from the NLSY79. Controls include number of children, employment, engaging in home production, weekly hours worked, weekly earnings, and years of education.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.9: The Effect of Legalization on Marriage, Women

3.6.5 Synthesizing Results

I begin this section with a conditional correlational analysis of the treatment and comparison groups before and after the legalization. The conditional correlations of marriage with variables like age and schooling seem to change over time that is consistent with the individuals being later in their lives in the later period, which is not very notable. What is more interesting, is that the conditional correlations between marriage and earnings become more similar in the post-period for men across the treatment and comparison group. But for women, the earnings are a negative conditional predictor of marriage for women in the treatment group, and a positive conditional predictor of marriage in the comparison group – possibly reflecting the economic and social circumstances in their communities.

Next, I compare compares the results obtained through the DiD approach and individual fixed effects strategy. The point estimates for the DiD and FE models

are similar for both men and women, indicating a positive effect of the IRCA on marriage rates.

I then proceed to examine the robustness of the results by considering alternate comparison groups. These groups include native-born Hispanics with less than a high school education, non-citizens, foreign-born individuals, individuals with immigrant mothers, and individuals whose mothers have less than a high school education. The point estimates obtained from these comparison groups vary, but they generally confirm a positive effect of the IRCA on marriage rates. However, due to the lack of precision in the estimates, it is difficult to determine a preferred comparison group or provide a precise point estimate. And it is difficult to statistically distinguish the estimates across comparison groups, despite there being economically significant differences between the point estimates.

Finally, I explore potential mechanisms behind the observed effects by controlling for labor market and education variables. The results show that labor market outcomes do not provide a clear explanation for the marriage effect for men, but perhaps show some substitution with marriage for women. Education, as in the DACA chapter, does not appear to be a significant mechanism for the IRCA's impact on marriage rates. However, the imprecise nature of the estimates limits the conclusions that can be drawn.

Overall, the findings suggest a positive effect of the IRCA on marriage rates, and this is robust to several comparison groups. And it does seem that the effects, as predicted, are larger for the IRCA than those found for DACA. But there are caveats to that. First, the IRCA estimates are imprecise. Second, the DACA results are a

lower-bound (but if you do the back-of the envelope scaling done in Chapter 2, the point estimates are still smaller for DACA in an economically significant manner). Third, the sample in this chapter is older than the one in Chapter 2. Finally, data limitations (especially power) provide less scope to analyze mechanisms than there is in Chapter 2.

3.7 Conclusion

The Immigration Reform and Control Act of 1986 contained the largest legalization in American history, granting more than 2.7 million unauthorized immigrants a pathway to citizenship. It is a life-changing policy for its recipients, and hence it is unsurprising that it changed an important life choice for many of its recipients: marriage.

The results of this chapter are for the IRCA, which is the most comparable to any mass legalization that has been a part of notable congressional proposals over the past few decades. Thus, the results from this chapter are arguably most applicable to large-scale immigration reforms that are within the current Overton window, rather than DACA. The caveat to that is that DACA is more recent, and thus the cohorts affected by that policy may be operating in a more similar environment than the cohorts affected by the IRCA.

The policy not only affects immigrants directly but also has implications for future immigration flows. Spouses of US citizens can obtain permanent residence without numerical limits imposed on other immigration categories. Granting unau-

thorized immigrants a pathway to citizenship, which was shown to increase marriage rates in the context of the IRCA, has significant consequences for future immigration composition. Indeed, Cascio and Lewis (2023) report that the IRCA resulted in higher utilization of family preference visas. Therefore, studies on marriage in relation to immigration such as this one should be thought of as offering insights into future immigration patterns.

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