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Uncertain Futures: How Did the Threat of Rescinding DACA Affect Eligible Immigrants' Outcomes?

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The latest version is available here.

Abstract

Since its introduction in 2012, Deferred Action for Childhood Arrivals (DACA) has been one of the most contested U.S. immigration policies. This article examines how the uncertainty arising from the 2017 attempted rescission of the policy impacted the labor market outcomes of eligible immigrants. Using American Community Survey data, I implement a difference-in-differences research design that exploits the sharp eligibility cutoffs of the policy to define treatment and comparison groups. I find that the threat to end the program had statistically significant negative effects on eligible immigrants' employment, labor force participation, and total income. I further investigate how state-level support for DACA recipients mitigated these effects and explore heterogeneity by sex, age, and education. My results are robust across a range of different specifications, samples, and undocumented proxies, and pass placebo tests. My paper demonstrates that the outcomes of DACA-eligible workers respond to legislative uncertainty, strengthening the argument for a more permanent legal structure.

JEL Codes: J15, J61, J68, K37

Keywords: Immigration Policy, Undocumented Immigrant, Liminal Legality, Labor Market, Policy Uncertainty, DACA

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1 Introduction

Unauthorized immigration has long been a polarizing issue in the United States. Both the flow of new unauthorized immigrants and the treatment of the already settled undocumented population that makes up over a quarter of the at least 40 million foreign-born individuals in the U.S. (Adams and Boyne 2015) are widely debated. Of all questions revolving around immigrants with no legal status, a particularly pivotal one is the matter of those commonly referred to as "Dreamers".[1] Dreamers arrived as young children with their parents through unauthorized channels and have resided in the U.S. ever since. Having grown up in the U.S. and gone through the education system—every year, an estimated 65,000 of them graduate from American high schools (Adams and Boyne 2015)—they likely adopted the language and culture and started their assimilation. Their supporters argue that apart from their legal status, they are no different from any second-generation immigrant born on U.S. soil. However, unlike that group, they face the constant danger of deportation—back to countries they have barely lived in and have since likely forgotten. To address their unique situation and improve their economic and social outcomes, on June 15, 2012, President Obama announced the Deferred Action for Childhood Arrivals (DACA) executive memorandum.

By granting eligible individuals two-year renewable work authorizations and a reprieve from deportation, DACA proved to be hugely successful in improving the economic well-being of undocumented immigrants. It was demonstrated to increase employment (Pope 2016), reduce poverty (Amuedo-Dorantes and Antman 2016), and improve a wide range of related outcomes for eligible immigrants (Giuntella and Lonsky 2020; Wang, Winters, and Yuan 2022; Gihleb, Giuntella, and Lonsky 2023) while also contributing to the increased growth of the U.S. economy (Ortega, Edwards, and Hsin 2018). Despite its success and lack of adverse effects on natives' labor market outcomes (Battaglia 2023), DACA was widely scrutinized, and its legal construction as an executive memorandum did not guarantee its longevity. After the Democrats lost control of the White House following the 2016 presidential election, Donald Trump, who previously expressed his disdain for DACA, began work to end the policy. Eventually, on September 5, 2017, the Department of Homeland Security announced that the policy was ending and set the timeline for deportations to begin within six months. In response to the rescission announcement, lawsuits and injunctions were filed almost immediately, halting the proceedings. The ensuing legal back-and-forth lasted for nearly three years and only ended in June 2020 when the U.S. Supreme Court blocked the rescission.

[1] Named after a 2001 policy proposal designed to address their situation.

While DACA was never repealed and recipients never actually lost their authorization, the period between the rescission announcement and the Supreme Court ruling highlighted the vulnerabilities of the policy and tempered the optimism that previously surrounded the program (Patler, Hamilton, and Savinar 2021). During this time, the threat of deportation loomed large, and consequently, recipients experienced high levels of volatility regarding their lives, livelihoods, and futures in the U.S. However, since this uncertainty was unique to DACA, the intent to end the program had no direct effect on the status of ineligible undocumented immigrants, creating a divergence between their experiences and those of DACA recipients. In this paper, I explore how the threat of deportation and the anticipated loss of work permits arising from the attempt to overturn DACA affected eligible immigrants' economic outcomes compared to those of their ineligible counterparts.

There are several mechanisms through which the attempted rescission of the program could have impacted the labor market outcomes of DACA recipients. The threat of losing workers to revoked work permits or deportation and the fear of retaliation for employing DACA recipients likely lowered firms' willingness to hire and invest in the training of eligible immigrants. Additionally, the uncertainty could have forced DACA recipients to seek employment under the table to forgo the need for work authorizations. Since such jobs pose an increased risk, it is plausible that the marginal workers who gained employment thanks to the original policy left their jobs and dropped out of the labor force. Conversely, it is also possible that eligible workers temporarily increased their work hours in anticipation of future deportation, unemployment, and loss of earnings, thus making the matter theoretically ambiguous and warranting an empirical assessment.

To answer the research question, I construct a difference-in-differences model around the rescission announcement using the 1% samples of the American Community Survey from 2006 to 2019. My identification strategy—in line with the literature standard (Pope 2016)—exploits the arbitrary age-at-arrival discontinuity of the DACA eligibility requirement to establish treatment and comparison groups. Based on this approach, I measure the intent-to-treat effects of the attempted repeal—while controlling for a range of individual and local labor market characteristics.

It is important to note that due to the reporting limitations of the ACS, I use the residual proxy approach outlined by Liu and Song (2020) to identify undocumented immigrants in my sample. While this approach is frequently used in the economic literature, the eligibility requirement coded with this method is still less restrictive than the actual criteria on account of the lack of available information on exact legal status and criminal history. Therefore, my model estimates the lower bound of the intent-to-treat effects of the rescission threat.

Applying my empirical specification to the data, I identify statistically significant and robust reductions of 3.93 and 4.83 percentage points in DACA-eligible immigrants' labor force participation and employment, respectively. The magnitudes of these effects are considerable, given that eligible immigrants are typically of prime working age, demonstrating high labor force participation and employment rates. I also document an increase in self-employment likelihood and a decline in total income. These results, together with the lack of meaningful changes in employed immigrants' weekly work hours, rule out the possibility of a rightward labor supply shift and strengthen the argument that the effects of the rescission threat were overwhelmingly negative. Lastly, I find suggestive evidence of a reduction in hourly wage, which could be indicative of negative labor demand effects or of DACA recipients moving towards lower-paying, under-the-table jobs. However, presumably due to sample size limitations, these coefficients fail to be significant. Beyond my main results, I investigate the role state-level support for DACA plays in mitigating the consequences of the uncertainty, and I show that the outcomes of eligible immigrants residing in states involved in either of the two anti-rescission lawsuits were less severely impacted. I also explore effect heterogeneity by sex, age, and education, and I supplement my main results with a range of robustness checks and placebo tests.

My paper makes a number of contributions to the existing economics literature. Most importantly, I add to the evidence on the impacts of the threat to end DACA on eligible immigrants' outcomes between 2017 and 2020. To my knowledge, my paper is among the first to document the labor market effects of the rescission attempt, with only one other, parallel working paper investigating the question (Zaiour 2024). My study offers the most comprehensive examination of the intended repeal of the program, surveying the widest set of outcomes. I also further our understanding of the events by presenting the first-ever exploration of the role state-level action plays in mitigating the harms of the rescission threat and by conducting a detailed heterogeneity analysis that has not been performed in this context previously. Additionally, I validate my use of the residual proxy and demonstrate the robustness of my results using specifications that exploit eligibility criteria different from the standard age-at-arrival discontinuity. These features are unique in the literature, and together with the novel placebo tests included in the paper, help me create the most rigorous and complete analysis of the attempted repeal of the policy.

Research on the effects of the rescission threat is otherwise limited. In their recent articles, Ortega and Connor (2024) investigate the projected macroeconomic impacts the repeal could have on the U.S., and Amuedo-Dorantes and Wang (2024) discuss the role of intermarriage in mitigating the threat of uncertainty. Beyond these studies, previous research mainly documents adverse effects stemming from anxiety surrounding the attempted policy change, such as worsened health outcomes (Patler, Hamilton, Meagher, and Savinar

2019) and changing sleep patterns (Giuntella, Lonsky, Mazzonna, and Stella 2021). As the exploration of labor market effects in the aftermath of the rescission announcement is still a novel direction, my analysis improves our understanding of the topic and meaningfully contributes to the relevant body of work.

Next, my paper adds to the literature on the effects of DACA on eligible immigrants. Previous studies showed that DACA increased the economic well-being of recipients by increasing employment and reducing poverty while lowering their probability of school attendance (Pope 2016; Amuedo-Dorantes and Antman 2016; Amuedo-Dorantes and Antman 2017). These improvements subsequently boosted their likelihood of homeownership (Wang, Winters, and Yuan 2022) and of living independently (Gihleb, Giuntella, and Lonsky 2023). Besides the economic effects, DACA favorably affected eligible immigrants' health outcomes (Hainmueller, Lawrence, Martén, Black, Figueroa, Hotard, Jiménez, Mendoza, Rodriguez, Swartz, and Laitin 2017; Giuntella and Lonsky 2020) and reduced teenage pregnancies (Kuka, Shenhav, and Shih 2019). My study furthers the understanding of these effects by exploring the evolution of the economic outcomes of DACA recipients, particularly by expanding the analysis beyond the end of the Obama administration and demonstrating that the rescission threat largely erased the original benefits of the policy.

Lastly, the paper makes contributions to other segments of the literature on immigrant workers' outcomes. Regarding the impacts of immigration enforcement, previous studies have found that increased enforcement leads to a decline in unauthorized immigrants' labor market outcomes—such as employment, hours worked, and earnings (Orrenius and Zavodny 2009). Similar effects are detected when asymmetric, sector-specific enforcement policies redistribute undocumented workers from heavily monitored, better-paying industries to more weakly monitored, low-paying ones (Bansak and Raphael 2001; Dávila and Pagan 1997). As the literature mainly focuses on enacted policies rather than on the consequences of uncertain interventions, demonstrating that the threat of increased enforcement also changes the outcomes of unauthorized immigrants adds to the relevant body of work. By documenting the mitigating effects of anti-rescission lawsuits and other state legislation, I also contribute to our understanding of the interplay of federal and state-level immigration policy. Other papers in this segment of the literature document how the Arizona immigration law, SB 1070, which was halted by federal policymakers before full implementation, had minimal effects on the share of likely undocumented immigrants in the state (Amuedo-Dorantes and Lozano 2015) but reduced the flow of new undocumented workers from Mexico (Hoekstra and Orozco-Aleman 2017).

Besides its contributions to the economics literature, my paper also carries important policy implications. While the 2020 Supreme Court ruling at least temporarily stabilized

DACA, the executive memorandum came under scrutiny again when the Biden administration attempted to codify it in 2021. With recent rulings finding those efforts unlawful, the policy is now exposed to the possibility of being gradually phased out (Montoya-Galvez 2025). Moreover, another flagship liminal legality policy, Temporary Protected Status (TPS), also faced the threat of rescission and was recently terminated for recipients from several countries. These events cast doubts over the future of temporary authorization programs and raise questions regarding their viability. My findings underscore that if our aim is the economic integration of unauthorized immigrants who have lived in the U.S. for an extended period of time, policymakers should aim to construct more permanent and comprehensive legal packages. By offering a pathway to citizenship or permanent residency, such interventions could perpetuate the associated economic benefits and shield recipients from legislative uncertainty.

The rest of the paper proceeds as follows. Section 2 offers a brief background on DACA and discusses the timeline of the legal action following the rescission announcement. Section 3 develops a conceptual framework for the analysis. Section 4 presents the dataset, the identification strategy, and the primary empirical specification. Section 5 details the main results and offers relevant analysis that is then supplemented by extensions and robustness checks in Sections 6 and 7, respectively. Section 8 concludes.

2 Background

The future of immigrants who arrived as children in the U.S. through unauthorized channels, but since then integrated into American society both culturally and linguistically, has long generated a significant debate in the U.S. While these young people virtually grew up as any other legal U.S. resident, they lack the same rights and legal status, which limits their economic integration and exposes them to a constant risk of deportation.

To protect these individuals, the Development, Relief, and Education for Alien Minors (DREAM) Act was proposed in 2001, which aimed to introduce various protections and a pathway to citizenship to resolve the matter. Despite the initial momentum, the original DREAM Act failed to pass into legislation, and the future of "Dreamers" remained unresolved.

Highlighting the importance of the matter in question, various versions of the DREAM Act were proposed several times since the original proposal, but the bill failed to garner enough support every time—most recently in 2011. This latest 2011 failure prompted President Obama to bypass the usual legislative process and release the Deferred Action for

Childhood Arrivals (DACA) presidential memorandum on June 15, 2012.[2]

The DACA program provided Dreamers with a two-year renewable work permit and reprieve from deportation without creating a pathway to citizenship or any sort of more long-term accommodations. Thus, it was initially considered merely a temporary solution preceding a more permanent legislative framework.

Eligibility for the program was established using an easy-to-follow set of criteria with fairly arbitrary cutoffs (Napolitano 2012). Eligible applicants must be immigrants with no lawful status. They must have been under the age of 31 and were required to be physically present in the U.S. as of June 15, 2012. They must have entered the U.S. before their 16th birthday and must have been continuously residing in the U.S. since June 15, 2007. They were required to be either still in school or have a high school diploma or equivalent, or have been honorably discharged from the U.S. Armed Forces, all while not having any felony convictions or more than three misdemeanors.

The application required the submission of various records and forms as well as a $495 registration fee, but was otherwise considered relatively simple and accessible. The U.S. Citizenship and Immigration Services (USCIS) started accepting applications on August 15, 2012, and by June of 2016 received 844,931 initial applications, approximately 88% of which were approved. It is worth noting that initial applications gradually trailed off, and following the second anniversary of the program in 2014, renewals started dominating the pool of filings. This change in trends suggests that the vast majority of eligible individuals who intended to file an application did so prior to 2016 (USCIS 2016).

Despite its success in improving eligible immigrants' labor market and socioeconomic outcomes (Pope 2016; Amuedo-Dorantes and Antman 2016; Gihleb, Giuntella, and Lonsky 2023; Wang, Winters, and Yuan 2022), the policy was widely contested from the first days of its establishment, mainly by Republican political leaders. When the Obama administration attempted to expand the policy to the parents of Dreamers in November 2014 through the proposed DAPA memorandum (Johnson 2014), these anti-DACA voices intensified, and a number of, primarily conservative, states filed an injunction against the expansion, which was later upheld by the U.S. Supreme Court in June 2016.

Following the failed expansion attempt, some raised concerns that should the Democratic Party lose control of the White House, the executive memorandum could be overturned and its recipients could face deportation. These worries, however, were largely just speculative until Donald Trump's unexpected presidential victory in November 2016.

From the beginning of his presidential campaign, Donald Trump was a vocal critic of DACA and swore to repeal the policy upon his election, but since he was not considered

[2]For a visual depiction of the timeline of events described in this section, consult Figure 1.

a favorite during the election cycle, many ignored these threats. However, shortly after he took office in January 2017, these concerns intensified until eventually, on September 5, 2017, Acting Secretary of the Department of Homeland Security (DHS), Elaine Duke, announced the rescission of the program via a memorandum based on the orders of Attorney General Jeff Sessions (Duke 2017). The rescission announcement presented a timeline that would uphold DACA recipients' permits for a six-month period but would cancel them after that. The goal of this transition window was solely to prepare for the deportation and enforcement scheduled to begin on March 5, 2018. The rescission memorandum also ended first-time DACA applications; however, it did allow for renewal applications for immigrants whose permits expired during the six-month transition period. The announcement was soon followed by legal action from Democratic and nonpartisan lawmakers as 15 states and Washington, D.C. almost immediately filed a lawsuit to block the rescission (Kopan 2017). California and three other states followed shortly in a separate filing.[3]

These lawsuits were the beginning of a lengthy legal process during which the fate of the policy and the futures of its recipients were constantly under threat. In early 2018, various judges ruled in favor of upholding DACA, in response to which the Department of Justice appealed to the Supreme Court for an expedited ruling to ensure that deviation from the original deportation schedule was minimized. However, when the appeal was rejected, the matter was forced to go through the regular judiciary and appeals process, which greatly extended the timeline (Gomez 2018). The administration modified its new deportation target date to late 2018, and many blue states passed rulings in favor of restoring DACA. While during this period the policy was never actually rescinded and deportations never began, the status of the program, previously perceived as largely stable, was in constant limbo, and the lives of its recipients were spent in seemingly never-ending uncertainty. Every time the previously announced target date for deportations arrived—convinced of its ability to still execute the plans—the administration simply pushed the target date back by a couple of months.

Eventually, following a drawn-out appeals process, the Supreme Court began its hearings in November 2019. The final vote, which was announced on June 18, 2020, ruled 5-4 in favor of blocking the rescission. It is important to note, however, that in its rulings, the Supreme Court stated that the deliberation was made purely on procedural grounds. This caveat means that future attempts to repeal the policy are not prevented by the decision (de Vogue, Cole, and Ehrlich 2020), which raises the questions of whether the uncertainty

[3]The complete list of states that were part of either of the lawsuits is the following: California, Connecticut, Delaware, Hawaii, Illinois, Iowa, Maine, Maryland, Massachusetts, Minnesota, New Mexico, New York, North Carolina, Oregon, Pennsylvania, Rhode Island, Vermont, Virginia, Washington, and Washington, D.C. A map of these areas is presented in Figure 4.

period ever really ended.

Following the ruling, the Department of Homeland Security issued an amendment that withdrew the rescission attempt and upheld DACA recipients' status, but blocked new first-time applications. While this amendment was later vacated and the original policy was fully reinstated, it served as an indication that the legal battles surrounding DACA were far from over.

Following the 2020 elections, when the Democrats reclaimed control of the White House, President Biden reinstated DACA to its full legal capacity and promised a more permanent legal solution, further highlighting the issue's partisan nature (Biden 2021). However, shortly after the reinstatement on July 16, 2021, Judge Andrew Hanen of the U.S. District Court for the Southern District of Texas filed an injunction, which was affirmed by the U.S. Court of Appeals Fifth Circuit on October 14, 2022, to block new DACA applications on the grounds that the program bypassed congressional approval. While this injunction effectively neutralized the reinstatement of the policy, it still constituted an improvement in terms of recipients' legal status as it allowed for the permit renewal of already approved DACA recipients, thus upholding the benefits for those who received their status before the 2017 rescission announcement.

It is worth noting that even today, DACA is a major legal battleground. On January 17, 2025, the U.S. Court of Appeals for the Fifth Circuit narrowed Judge Hanen's DACA Final Rule that prohibits the processing of initial applications (DHS 2022) to the state of Texas, thus suspending any action against current DACA recipients elsewhere. Following this development, on September 29, 2025, USCIS announced in a court filing that it has made arrangements to accept new applications outside the state of Texas (USCIS 2025). However, the implementation of this plan is on hold for now due to the fact that the appellate court's January ruling also found the Biden administration's attempt to codify DACA unlawful. With that decision, the court effectively placed the future of the policy back in the hands of Judge Hanen, who is yet to release a formal order on the matter but is facing pressure from Republican states to phase out the program and allow Congress to decide on the fate of its recipients (Montoya-Galvez 2025).

3 Conceptual framework

To better understand how the possibility of rescission changed eligible immigrants' behavior, one must first take into account what benefits DACA entails.

When DACA was introduced, the two most important components of the policy were the renewable work permits and the promise that recipients would not be deported, thus allowing eligible immigrants to enter the mainstream labor market and society. Thanks to these program features, the experiences of eligible immigrants started deviating from their ineligible—still fully undocumented—counterparts and started showing similarities to naturalized immigrants, as evidenced by the trendlines presented in Figure 2.

The policy eliminated the limitations to seeking employment through official channels, thus providing a pathway for DACA recipients to participate in the mainstream labor market. Simultaneously, the work authorizations also allowed firms to hire workers who previously would have been off-limits due to their undocumented status. Therefore, DACA increased both the labor supply and the labor demand for DACA recipients, contributing to an increase in their hiring.

These effects are well-documented in the economic literature. Employment and labor force participation of eligible workers increased together with their income (Pope 2016), which contributed to reduced levels of poverty (Amuedo-Dorantes and Antman 2016). These developments, together with the fact that DACA permits also allowed immigrants to access services such as banking or healthcare, also contributed to changes in other aspects of quality of life related to labor market outcomes, such as health and housing (Giuntella and Lonsky 2020; Wang, Winters, and Yuan 2022; Gihleb, Giuntella, and Lonsky 2023).

As the policy lowered the barriers to employment and labor market success, the opportunity cost of schooling increased; thus, DACA recipients' decisions regarding their schooling also adjusted. As potential foregone earnings increased, DACA recipients gradually substituted away from post-secondary education. As the literature demonstrates, undocumented immigrants' high school completion rates rose (Kuka, Shenhav, and Shih 2020) to gain and maintain eligibility for DACA, but participation in education beyond the required level fell (Amuedo-Dorantes and Antman 2017; Hsin and Ortega 2018; Okura, Hsin, and Aptekar 2023).

When, in September 2017, the first Trump administration announced its intention to end DACA, these benefits tied to the authorization were all in jeopardy. Due to the various lawsuits and injunctions, recipients never actually lost their status during the uncertainty period leading up to the Supreme Court ruling, but the perception of stability and permanence that surrounded the program during the Obama administration crumbled. While this legislative uncertainty did not directly affect their ineligible counterparts, as they never stopped behaving as and being undocumented immigrants, DACA recipients had to face the unique volatility of their situation and their exposure. To mitigate the threat of losing their work permits and to cope with the possibility of deportation, they were forced to alter their behavior. Simultaneously, due to the uncertainty surrounding these workers, firms were also pushed to confront the new costs and risks associated with hiring DACA-eligible immigrants.

I hypothesize that the possibility of DACA recipients losing their work authorization and potentially being deported increased the opportunity cost of employing such workers. If the rescission had actually succeeded, the resources that firms had invested in the hiring and training of such workers would have been wasted. Additionally, the narratives surrounding the program could have also given rise to fear of retaliation for employing DACA-eligible immigrants, resulting in further chilling effects. The combination of these risks suggests that firms likely substituted away from DACA recipients by pursuing the hiring of workers with more certain work authorizations instead, thus lowering the labor demand for workers with DACA authorizations.

At the same time, the supply side effects are theoretically less clear. On the one hand, if eligible workers had indeed accepted that they would have to leave the country soon, they could have increased their labor supply and work hours to maximize their earnings in the interim period, thus preparing for the financial blow of the deportation. On the other hand, it is highly unlikely that DACA recipients would simply accept deportation or even preemptively leave the country and pursue return migration to cope with the uncertainty, given the typical demographic and social characteristics of eligible immigrants.[4] This hypothesis would imply that instead they could have attempted to lower their visibility for immigration enforcement by substituting authorized employment with self-employment or work through unofficial channels, which would have also minimized their loss of earnings by shortening the time to transition had the rescission passed. However, it is important to note that such changes would have posed an increased risk due to the nature of the jobs typically available in the informal sector, so it is plausible that the marginal workers whom DACA originally enticed to work dropped out of the labor force in response to the uncertainty.

As more adverse labor market conditions ensued for DACA recipients during the uncertainty period, the net benefit associated with pursuing higher education could have increased. Since human capital translates more universally, schooling could have represented a viable strategy to improve future outcomes even if the deportations had begun. Additionally, in the wake of the rescission announcement, several colleges and universities pledged their support to undocumented students (Rock 2017), so these places could have been perceived as safe havens for recipients. However, due to the age distribution of DACA recipients, I consider it highly unlikely to see any meaningful changes in eligible immigrants' school attendance. While originally DACA indeed targeted younger immigrants by setting its eligibility requirement at under 31 years of age, by the time of the rescission announcement, over 5 years

[4]DACA recipients don't fit the typical profile of return migrants, as they are young, they have resided in the U.S. for an extended period of time, have built their entire life there, and presumably have very limited familiarity with their countries of origin. This argument is further supported by the fact that the proportion of DACA-eligible immigrants in the data doesn't vary notably between the pre- and post-treatment periods.

had passed since the original implementation of the policy. Therefore, typical recipients at this point would likely be in their late 20s or early 30s, which makes reenrollment in post-secondary education a much less feasible coping mechanism.

Similarly, an argument could be made for relocation as a way of lowering visibility and mitigating exposure to potential enforcement. However, this behavior is even less likely than schooling due to the associated fixed costs and implicit rigidities of this process that would be further exacerbated by potential earnings losses and the threat of revoked authorizations.[5]

To summarize, the threat of rescinding DACA could have decreased or increased eligible immigrants' labor force participation, employment, and total income. Additionally, depending on underlying labor market dynamics, it could have also had effects on employed DACA workers' work hours or hourly wages. Lastly, there is a possibility of increased school attendance, but I consider such a change highly unlikely.

The trendlines presented in Figure 2 provide support for my hypothesis and motivation for the empirical exploration of the research questions. While the relevant outcomes of naturalized immigrants and ineligible undocumented immigrants evolve virtually in parallel, the trends for the eligible group are altered twice. First, in 2012, at the time of DACA's original release, the eligible group started outpacing their ineligible counterparts and closing the gap to the naturalized group. Second, in 2017, following the rescission announcement, their outcomes began falling and showing similarities to those of undocumented immigrants again. To conduct a rigorous examination of these changes, in the remainder of the paper, I propose an identification strategy, describe the dataset and model used for the analysis, and present my results.

4 Data, and methods

To create an empirical framework for the analysis of the events, I construct a difference-in-differences research design that exploits the fact that the eligibility cutoffs of DACA were exogenously determined. My identification strategy relies on the fact that the volatility arising from the rescission threat was unique to DACA recipients' status—ineligible immigrants experienced none of this uncertainty as they remained undocumented the entire time.

To assemble the dataset required for my identification strategy, I use the 1% samples of the American Community Surveys from 2006 to 2019 (Ruggles, Flood, Sobek, Backman, Chen, Cooper, Richards, Rodgers, and Schouweiler 2024). These representative individual-

[5] In an early version of this paper, I tested the effects of the rescission threat on a range of housing and migration-related outcomes. As expected, I failed to demonstrate any meaningful, significant changes. While the corresponding analysis is not included in this paper, these results are available upon request.

level datasets not only include the variables necessary to model most of the eligibility criteria but also provide a rich collection of labor market indicators and relevant controls.

Using the birth year and birth quarter variables, I construct a variable for age in June 2012 from which I am able to infer whether a given individual passes the age requirement for DACA. Combining the birth information with the year of arrival, I then code the other criteria of DACA, such as age at arrival or years in the U.S. Then, from the educational attainment variable, I am able to derive the requirement for a high school diploma or equivalent.

While the depth of the ACS to model DACA eligibility is superior, there is one distinct shortcoming of these datasets. To encourage honest responses and protect respondents, the Census Bureau does not collect information on individuals' legal status, which prevents the direct identification of unauthorized immigrants in the sample. To circumvent this issue, I utilize the residual method to construct a proxy for undocumented status. This strategy was validated by Liu and Song (2020) for the purposes of DACA-related analysis and showed that when compared to the official USCIS records, it outperforms other approaches such as the use of the Hispanic proxy that uses ethnicity and citizenship status to find immigrants who are likely without legal status.[6]

The residual proxy starts with the universe of the entire foreign-born population and gradually eliminates individuals based on their responses to various questions, with the leftover group plausibly constituting the group of undocumented respondents. From the initial pool, I first eliminate citizens and immigrants who arrived in the U.S. before 1980, given that the Immigration Reform and Control Act naturalized all immigrants who arrived before 1980. Next, I rule out respondents with likely asylee or refugee status based on country of origin[7]. Then, I exclude individuals who are veterans or are currently in the military, those who receive welfare benefits from the government, and those who are employed in the government sector. Lastly, I drop all individuals whose profession requires lawful status or licensing.[8] All remaining respondents are then classified as undocumented.

In addition to the core eligibility indicators and outcome and control variables, I also create two variables to describe local labor market trends. Using the available labor market information in the ACS, I compute employment and labor force participation rates that vary

[6]While the residual proxy was found superior to other alternatives such as the Hispanic proxy and the use of non-citizens, in the Robustness section I also conduct regressions using these alternative definitions to demonstrate that the choice of proxy does not meaningfully influence my findings.

[7]The list of these countries includes: Congo, Syria, Burma, Iraq, Somalia, Bhutan, Ukraine, Eritrea, Sudan, Kuwait, and Cuba.

[8]These occupations are the following: probation officer, clergy, various scientists, technicians, and engineers, architect, different healthcare practitioners, law enforcement officer, insurance agent, legal practitioners, teacher, securities sales agent, pilot, and air traffic controller.

by year and Public Use Microdata Area (PUMA). It is worth noting that since PUMAs are defined as areas with 100,000 residents, the Census Bureau updates their boundaries every decade. To eliminate any potential discrepancies between my earliest and latest sample years from the PUMA reorganization in 2010, instead of raw PUMAs, I opt to use the artificially constructed consistent PUMAs (C-PUMAs) that were created by IPUMS to ensure cross-decade comparability.

Following the variable creation, I restrict my dataset to undocumented (as identified with the residual proxy) high school graduates who arrived in the U.S. before 2007 and were between 22 and 30 years old in June 2012, and immigrated between the ages of 12 and 19. This ensures that all immigrants in my sample meet the age, years in the U.S., and education requirements, while I use the age-at-arrival eligibility discontinuity to create my identifying variation. Those immigrants who arrived between the ages of 12 and 15 meet all eligibility criteria, thus form my treatment group, while those who arrived between the ages of 16 and 19 fail the last requirement, thus creating my comparison group. It is worth noting that the choice of 22 years as the lower limit of age in June 2012 is intentional. Given the before-2007 arrival requirement for all individuals in the sample, if they were under the age of 21 in June 2012, it would have been impossible for them to have arrived after their 16th birthday. Therefore, had I included such immigrants, my setup would have yielded incomplete comparisons.

Next, I focus on determining the survey years to be included in my dataset. While I pursue event study estimations that use all years from 2006 to 2019, as well as a robustness test that compares the effects of the rescission threat to the original policy by using survey years between 2009 and 2019, for my main regressions, I use samples that only include survey years from 2014 to 2019.

My rationale for excluding 2012 and 2013 is to account for the time it took for the original policy effects to fully materialize. Following DACA's 2012 announcement, official records (USCIS 2016) reveal that initial applications were first received in 2012 and their numbers peaked in 2013. Beginning in 2014, the composition of applications shifted in favor of renewal applications, and the number of first-time applicants plummeted. Based on these official statistics, I argue that the economic effects of DACA likely didn't stabilize until renewal applications started dominating the pool of filings, indicating that all immigrants who intended to apply did so. Based on this reasoning, my pre-treatment years are 2014 through 2017.[9]

While Donald Trump had been a vocal critic of DACA even during his election campaign,

[9]Further evidence in support of this delayed onset of the original policy effects is provided via the event study plots presented in the Results section.

and an argument can be made that anticipatory behavior might exist among DACA recipients as early as 2016 and even more so in 2017, the actual rescission announcement was not made until September 2017. Therefore, as the rescission attempt was ended by the Supreme Court in early 2020, the treatment period runs from the fall of 2017 to the spring of 2020. Due to the economic effects and data collection issues encountered in 2020 during COVID-19, that year is not part of the sample. As the ACS data is collected throughout the calendar year and we do not have information on the month each recipient was surveyed, I choose to still treat 2017 as a pre-treatment year – leaving 2018 and 2019 as my post-treatment years. However, in the event study plots of the next section, we can see that some of the effects started appearing even in 2017, so while I run regressions using all survey years 2014 through 2019, my preferred composition of years will include 2014-2016 and 2018-2019.[10]

To summarize, my main regression sample consists of undocumented immigrants aged 22 to 30 in June 2012, who arrived in the U.S. before 2007 and immigrated between the ages of 12 and 19, who, when surveyed between 2014 and 2019, were high school graduates. For my preferred setup, I further exclude the 2017 survey year from this sample. I also run tests on the employed immigrants in the sample separately for some outcomes of interest, and later, I also create separate subsamples based on sex, age, and educational attainment. Pre-period (2014-2016) means for all relevant variables by eligibility status are presented in Table 1. These summary statistics indicate that my treatment and comparison groups are largely similar in terms of their composition, with meaningful differences only appearing in terms of average ages of arrival and years in the U.S., which is by design of the identifying variation.

I use my dataset to estimate the following regression equation:

$$Y_{itc} = \beta_0 + \beta_1 DACA_i + \beta_2 DACA_i \times Post_t + X_i \rho_1 + Z_{tc} \rho_2 + \gamma_t + \gamma_c + \epsilon_{itc} \quad (1)$$

where Y_{itc} is the outcome of interest—indicators for labor force participation, employment, self-employment, school attendance, and for neither being in the labor force nor in school, as well as continuous variables measuring real total income, usual weekly hours worked, and real hourly wage (the latter two of which are only tested for the employed subsample)—for immigrant i surveyed in year t residing in C-PUMA c. $DACA_i$ is an indicator for DACA eligibility and $Post_t$ is the indicator for survey years 2018 and 2019. The vector X_i contains binary controls for male, Asian, non-Hispanic black, and Hispanic, and

[10] While there is little indication of the aforementioned potential anticipatory effects in 2016 on the event study graphs, additional samples excluding 2016 and 2017 are also tested.

college graduate respondents, as well as continuous controls for age and years in the U.S. The Z_{tc} vector of controls includes the previously described C-PUMA-by-year labor force participation and employment rates.[11] Lastly, the model also includes survey year and C-PUMA fixed effects and clusters standard errors at the C-PUMA level. The coefficient of interest is β_2, which represents the intent-to-treat effect of the threat arising from overturning DACA.

The validity of my estimates hinges on the existence of parallel trends between my treatment and comparison groups. While it is impossible to guarantee that the two groups would have evolved in parallel without the rescission announcement, there are several aspects of the identification strategy and of the events that support this assumption. First, as demonstrated by the pre-period summary statistics in Table 1, the two groups are remarkably similar in terms of their observable characteristics. These similarities likely remain throughout the uncertainty period, as at this time only renewal applications were allowed and self-deportation was highly unlikely, as previously discussed. Second, the trendlines for the ineligible group are parallel to those for a similar group of naturalized immigrants during the sequence of events. These two features—together with the inclusion of C-PUMA-by-year labor market controls to guard against bias from time and location varying labor market shocks—strengthen the argument that deviations from the trend in the outcomes of eligible immigrants can be attributed to the uncertainty surrounding their status.

While the research design increases confidence in the validity of the estimates, it is important to recognize three key limitations of the analysis. Most notably, since I am using the residual proxy method to identify undocumented status and there are no available data regarding respondents' criminal history, the treatment group possibly includes individuals who, in reality, would fail the eligibility criteria. Next, it is plausible that the anti-immigrant sentiments prevalent during the first Trump administration affected undocumented immigrants more strongly than their DACA-eligible counterparts. While upon careful comparison of the trends for naturalized and undocumented immigrants, this possibility seems highly unlikely, it is important to acknowledge these concerns. Lastly, despite DACA's attempted rescission having no impact on the ineligible group's status, the existence of general equilibrium effects cannot be fully rejected. We could imagine a scenario that sees enough DACA recipients make a switch from the mainstream to the informal labor market to upset the equilibrium and alter ineligible workers' outcomes. As improbable as such a large shock might seem, it is impossible to rule it out due to our lack of available data on under-the-table employment.

These circumstances, however, do not invalidate the identification strategy, merely alter

[11] The inclusion of these C-PUMA level controls does not meaningfully influence my results. I also performed regressions that omitted them or used state-level time trends instead. The coefficients of interest from these alternative specifications were nearly identical to my primary estimates.

the interpretation of the coefficients. Since each of the aforementioned three concerns biases the estimates downwards, the coefficients of interest are simply to be interpreted as lower bounds of the effects of the rescission threat.

5 Results

5.1 Event studies

To explore the evolution of the differences between my treatment and comparison groups and provide further support for the parallel trends assumption required by my research design, I present event study graphs in Figure 3. The sample used for these estimations consists of undocumented immigrants aged between 22 and 30 in June 2012, who arrived in the U.S. before 2007 between the ages of 12 and 19 and were high school graduates when surveyed between 2006 and 2019. As outlined above, the eligible group consists of those immigrants who arrived between the ages of 12 and 15, while the ineligible group includes those who immigrated after their 16th birthday.

The event study specifications contain the same controls and fixed effects, and follow the same clustering as the main regression equation described above. I omit the interaction between the indicator for the 2011 survey year and the eligibility dummy; thus, the difference between the treatment and comparison groups in that year serves as my baseline. I illustrate the standard errors for each coefficient using gray shading, and I mark the original 2012 implementation and the 2017 rescission attempt with dark red reference lines in my graphs.

My coefficient plots present a clear trend. While we can see some fluctuations in the treatment-comparison difference (particularly before 2009), generally, the gap between the two groups is fairly consistent. However, as expected, we see a clear deviation from this established pattern following the 2012 implementation of DACA, most distinctly for labor force participation, employment, and real total income.

As suggested in previous sections, following the establishment of the program, eligible immigrants' outcomes gradually started outpacing those of their ineligible counterparts. The treatment-comparison difference starts growing in 2013, and the gap widens until it stabilizes in 2014 and 2015. As suggested earlier, this trend is due to the process of initial DACA applications and approvals. We see the stabilization of the difference as the share of initial applications shrinks, and the majority of the application pool consists of renewal filings.

In general, we can observe a relatively stable difference between the outcomes of eligible and ineligible immigrants between 2014 and 2016, particularly for the aforementioned three variables. While this consistency in the pre-rescission period does not guarantee the existence of parallel trends, it does provide strong evidence in support of the assumption, thus increasing the confidence in the validity of my research design.

In 2017, this trend was clearly disrupted by the attempted repeal of the program. As the ACS administers its data collection throughout the calendar year, but the rescission was only announced in September, the effects for 2017 vary across variables, likely driven by their different levels of responsiveness. The graphs demonstrate a dramatic drop in labor force participation, more moderate reductions in employment, while at the same time showing virtually no immediate effects for real total income. It can be argued that unemployed workers could suspend their job search efforts almost immediately, thus making labor force participation the most responsive of these three variables. At the same time, total annual income this late into the year would likely not change much, if at all.

While the effects of the rescission threat are not uniform across different labor market metrics, by 2018, the detrimental impacts are much more pronounced and universally noticeable. The treatment-comparison gap is much smaller in 2018 and 2019 and is dramatically different from the established pre-rescission trend. Labor force participation and employment fall and stabilize at a considerably lower level. Similarly, real total income also plummets by 2018, but unlike the previous two indicators, it decreases even further in 2019. Meanwhile, self-employment and the likelihood of an immigrant not being in the labor force or in school drastically increase and stabilize at this higher treatment-comparison difference by 2019.

It is also worth noting that the usual weekly hours worked and the hourly wage, which are examined for employed respondents in the sample, also show signs of increasing and decreasing, respectively. However, these changes are much less noticeable. In the case of work hours, this is likely due to the fact that most employed immigrants already work full-time, so the possible magnitude of the change is more limited. The same does not hold for hourly wage, which presents a change with a much more dramatic magnitude for 2018, but then rebounds in 2019 close to its 2016 level.

5.2 Main results

The previously included event study plots vividly illustrate the effects of both the original 2012 release of DACA and the consequences of its 2017 attempted repeal. The dramatic deviations from the established trend that followed the rescission announcement clearly motivate my difference-in-differences analysis. I present the coefficients of interest estimated for my outcomes of interest in Table 2.

The regressions providing the coefficients displayed in Column 1 use my sample of undocumented immigrants aged between 22 and 30 in June 2012, who arrived in the U.S. before

2007 between the ages of 12 and 19 and were high school graduates when surveyed between 2014 and 2019. Next, as my event study plots highlighted the variance that exists across the responsiveness of each outcome of interest, for the estimates in Column 2, I exclude the 2017 survey year from my sample. This modification eliminates any ambiguity stemming from the different timelines that the adjustment of certain outcomes followed. Lastly, in Column 3, I also account for potential anticipatory effects by omitting both 2016 and 2017. While these different sample year compositions do not yield meaningfully different results, going forward, the sample omitting 2017 will serve as my preferred option to ensure the rigor of my analysis.

As anticipated based on the event study plots, the effects on labor force participation, employment, and total income are the most dramatic. Using my preferred composition of sample years, I estimate the reduction of labor force participation rate to be 3.93 percentage points and the decrease in employment to be 4.83 percentage points. Both of these effects are highly statistically significant. The group of immigrants in question consists of prime working-age immigrants, most of whom work full-time. The pre-period labor force participation and employment of eligible immigrants were 82.2% and 78.1%, respectively; therefore, the effects of the rescission threat are considerable. This empirical evidence demonstrates that the hypothesis of negative employment effects was, in fact, the correct one. Furthermore, the fact that labor force participation decreased by less than employment also shows that several newly unemployed workers are still actively searching for jobs. This could mean that these eligible immigrants were terminated against their will, suggesting that changes in the labor demand could be driving the effects.

The magnitude of the drop in real total income is similarly noticeable. The 1,833 decrease measured in 1999 dollars constitutes an over 10% reduction based on the pre-period mean for the eligible group. The source of this change is likely a combination of workers losing their jobs and other workers having to take lower-paying jobs, possibly in the informal sector. However, the latter of these phenomena is mostly speculative as the real hourly wage penalty for employed workers—as well as the change in their usual weekly hours—fails to be statistically significant.

The results also indicate a 2.61 percentage point rise in self-employment, which is a nearly 38% jump based on the pre-period sample means. Such a massive change is likely due to the reduced dependence on employers associated with this choice, which can be highly desirable given the uncertainty resulting from the rescission threat. Another potential benefit prompting immigrants to pursue self-employment could be the less formal nature of these jobs, which could lower their visibility and exposure to immigration enforcement.

Lastly, as expected, the attempt to overturn DACA had virtually no effect on eligible immigrants' school attendance. Given the age distribution and labor market characteristics of the sample, this came as no surprise. However, this also means that the likelihood of not being either in the labor force or in school also ended up increasing statistically significantly. This shows that without viable alternatives to employment following an unexpected job loss, many eligible immigrants simply dropped out of the labor force.

Overall, my results demonstrate that the rescission threat had strong and statistically significant negative effects on eligible immigrants' labor force participation, employment, and real total income, while also increasing the prevalence of behaviors—like self-employment or dropping out of the labor force—that are substitutes for traditional employment. Therefore, I find that the attempt to repeal DACA was highly detrimental to its recipients.

6 Extensions

6.1 Effects of state-level support

As previously discussed, the announcement to end DACA faced almost immediate pushback, with many states and public figures demonstrating support for the program and its recipients. It is plausible that these actions and statements helped mitigate eligible immigrants' and their employers' concerns regarding their futures, thus eliminating some of the negative labor market impacts.

To evaluate this possibility, I create three indicators, each based on a different display of support, and add them and their interactions to the base difference-in-differences specification. The first of these dummy variables is based on states' participation in either of the two anti-rescission lawsuits. As previously discussed, immediately after the rescission announcement, 15 states and Washington, D.C. filed a lawsuit blocking the rescission attempt, and then later California and three other states joined in a separate filing, raising the total number of participating states to 19, not including Washington, D.C. As the map provided in Figure 4 demonstrates, the set of participating states not only features both Democratic and Republican ruled areas but is also highly diverse geographically and economically. Therefore, any differential trends for DACA recipients residing in these states are likely strongly linked to involvement in the lawsuits.

While filing a lawsuit blocking the repeal of DACA is probably the most direct and targeted way a state can voice support for its DACA-recipient residents, there are other policy tools that have been used to aid the undocumented population more generally. One of these interventions is issuing driver's licenses to undocumented immigrants that would allow them to participate more freely in the mainstream society and even the labor market.

Amuedo-Dorantes, Arenas-Arroyo, and Sevilla (2020) demonstrated that these laws can increase weekly work hours of undocumented immigrants residing in such states and also change their commuting patterns. While this policy intervention does not specifically target DACA recipients facing the uncertainty of rescission, it can be used to infer how sympathetic a given state is in general towards undocumented immigrants. Therefore, my second indicator is based on the list of states that issued driver's licenses to undocumented immigrants in 2017 as compiled by Amuedo-Dorantes, Arenas-Arroyo, and Sevilla (2020) with a map of these areas being provided in Figure 5.

Lastly, allowing a state's undocumented residents to access in-state tuition at higher education institutions is another way of displaying support for this population. This is particularly notable in the context of DACA, as while it does not directly address the rescission announcement, many of these policies were originally designed to benefit DACA recipients and other, at the time, young immigrants. To create an indicator for this policy tool, I assessed the relevant laws for all states and compiled a list of the 16 that by 2017 had passed legislative action granting in-state tuition to undocumented students. The states that fit this requirement are presented on the map in Figure 6.[12] It is important to note that I explicitly restrict this list to states with formal legislative acts; therefore, I exclude areas like Washington, D.C., that had an executive order in place or Rhode Island, where a Board of Governors memo dictated the implementation.

Using these indicators one by one, I estimate the following specification:

$$\begin{aligned} Y_{itc} &= \beta_0 + \beta_1 DACA_i + \beta_2 DACA_i \times Support_c + \beta_3 Post_t \times Support_c + \\ &+ \beta_4 DACA_i \times Post_t + \beta_5 DACA_i \times Post_t \times Support_c + X_i\rho_1 + Z_{tc}\rho_2 + \gamma_t + \gamma_c + \epsilon_{itc} \end{aligned} \tag{2}$$

where $Support_c$ is the indicator for the given display of state-level support—involvement in anti-rescission lawsuits, driver's licenses, or in-state tuition for undocumented immigrants—and all other variables, controls, and fixed effects are as previously specified in Equation 1. My coefficients of interest are β_4 and β_5, which represent the effects of the rescission threat and the mitigating factor of a given supporting action, respectively. After estimating the above specification for each of the support indicators, I also combine the three into the same regression to compare their validity and increase the strength of the argument of causality for the involvement in anti-rescission lawsuits. The coefficients of interest from these regressions for each outcome of interest are presented in Tables 3 and 4. Below the estimates, I also

[12] The complete list consists of California, Colorado, Connecticut, Florida, Illinois, Kansas, Maryland, Minnesota, Nebraska, New Jersey, New Mexico, New York, Oregon, Texas, Utah, Washington.

provide p-values from the Wald tests for the null hypothesis of equivalence across the three support types from the combined specification.

The results for the mitigating effects of involvement in anti-rescission lawsuits are displayed in Column 1; the specifications using driver's licenses for undocumented immigrants and in-state tuition policies are in Columns 2 and 3, respectively. Column 4 presents the coefficients from the combined specification. The estimates indicate that residing in a state that participated in the lawsuits mitigated the effects of the labor force participation and employment effects of the rescission threat, but failed to erase them completely. These coefficients are already significant in the stand-alone specification, but are further strengthened when the other two policy tools are also added to the regression, demonstrating a distinct pro-DACA support effect even when controlling for other immigration-policy-related characteristics.

These findings suggest that employers or eligible immigrants, or both, could have perceived the rescission as less likely or less threatening if their states explicitly voiced support for DACA by participating in a lawsuit. This argument seems especially plausible when observing the estimates for labor force participation from the combined specification. These coefficients indicate that while in states without lawsuits the drop in labor force participation was 8.36 percentage points, this decrease was only 0.35 percentage points in those involved in legal action. The virtually negligible magnitude of this change, even though states filing lawsuits still experienced a net reduction of 2.5 percentage points in employment, is noteworthy. These findings could signal that the vast majority of these freshly unemployed workers remained in the labor force and were actively searching for jobs, thus hinting that DACA recipients viewed the labor market conditions in these states as more sympathetic.

Unlike the coefficients measured using the indicator for the lawsuits, the other two policy tools largely fail to produce any significant effects and do not seem to mitigate the effects of the rescission. Given the fact that they are much less targeted towards DACA and are much more of a representation of support towards the undocumented population in general, this result is not particularly surprising. Despite the lack of significant effects, testing them is still valuable, and their inclusion in the combined specification strengthens the validity and causality of the lawsuit participation estimates.

The sole difference from this trend is the coefficient for the usual weekly hours of employed workers for these two policy tools. While neither the general DACA effect nor the mitigating factor of the lawsuits is statistically significant for this outcome variable, DACA-eligible immigrants residing in states with driver's license or in-state tuition policies did experience a statistically significant increase of over 2 hours. The significance and magnitude of this change are present in the individually performed regressions for both policy tools, and for driver's license law states, it even survives the combined specification. While these results might very well be indicative of differential labor market trends, it is hard to say anything more concrete, given the multitude of underlying factors.

To summarize, this analysis provides evidence for the argument that state-level action can successfully combat the adverse effects of federal policy changes. Through the above regressions, I find that states that demonstrated support for DACA-eligible immigrants by filing an anti-rescission lawsuit could mitigate the detrimental consequences of the threat on labor force participation and employment. These effects are not only significant in the individual and combined specifications, but, as evidenced by the Wald tests, are also significantly different from the estimates produced by driver's license policies. While I fail to reject the null hypothesis that the mitigating labor force participation and employment effects of anti-rescission lawsuits are also significantly different from the coefficients produced using in-state tuition policies, this is likely due to sample size limitations and does not meaningfully weaken my argument for the validity of these findings.

6.2 Sex-based effect heterogeneity

Prior economics literature highlights disparate labor market trends of immigrant men and women that persist even after receiving authorization (Powers, Seltzer, and Shi 1998). Examining another major liminal legality program, TPS, Orrenius and Zavodny (2015) find that authorization increased eligible women's employment rates while boosting men's earnings. These findings suggest that immigrant men are already employed, thus authorization only moves them into better jobs, presumably on the mainstream labor market. Meanwhile, based on these results, one could hypothesize that immigrant women are the marginal workers for whom authorization policies could eliminate some of the opportunity costs and risks of working, thus moving them into employment.

Considering these trends, it is possible that the threat of ending DACA affected eligible men and women differently. To explore any existing heterogeneities, I run the main empirical specification described in Equation 1 for the male and female subsamples of my sample of undocumented immigrants aged 22 to 30 in June 2012 who arrived in the U.S. before 2007, aged between 12 and 19, and were high school graduates when surveyed between 2014 and 2019 (excluding 2017, as previously highlighted). The coefficients of interest from these regressions for each outcome are displayed in Table 5. Column 1 features the effects estimated using the male subsample, while Column 2 presents those created using the female subsample. The table also includes p-values from Wald tests on the null hypothesis of equivalence between the estimates from the two subsamples.

These results suggest that eligible men are driving the overall negative impacts of the rescission attempt. The reductions of labor force participation and employment are 5.61 and 7.32 percentage points, respectively, and are highly significant for the male subsample. Similarly, men also encounter a statistically significant 4.56 percentage point rise in the probability of dropping out of the labor force without attending school. These magnitudes far outpace the effects measured for the female subsample, which not only all stay below 2 percentage points but are also not statistically significant for any outcome.

Despite these considerable differences, it is important to note that none of the Wald tests produce significant p-values. While this is likely a result of sample size limitations, without the required statistical significance, it is hard to view the highlighted differences as anything more than suggestive of divergent outcomes between men and women.

6.3 Age-based effect heterogeneity

In line with prior literature on DACA that points to variation in authorization-induced earnings growth across different ages and education levels (Patler, Hale, and Hamilton 2021), I also investigate if similar differences exist for the effects of the rescission threat as well. In this subsection, I present results by age groups, and in the following, I perform regressions by educational attainment.

As the immigrants in my sample were between the ages of 22 and 30 at the time of the original release of DACA, at the time of the rescission, the youngest respondents are 27, and the oldest ones are 35. Considering this range, my first subsample includes immigrants aged at most 30 years at the time of the survey, while my second subsample consists of those older than 30 years. The coefficients for each variable of interest are collected into Table 6, with the estimates produced using the younger subsample displayed in Column 1 and those from the older subsample listed in Column 2. As before, I also include p-values from Wald tests for the equivalence between the subsample effects.

As expected based on earlier research, the effects are driven by the younger subsample. The adverse effects on their labor force participation and employment—10.6- and 12.4-percentage-point reductions, respectively—are strong and statistically significant. Similarly dramatic effects are documented for their real total income and their likelihood of not being in the labor force or in school. Even more notably, the real wage outcome for employed workers, which previously failed to yield significant coefficients, is now measured to be -$1.53 (expressed in 1999 dollars) and is significant at the 10% level. These highly significant and large effects demonstrate that the rescission attempt was particularly detrimental for younger immigrants. This is likely due to the fact that, unlike their older counterparts, when

DACA was originally released, they did not have a well-established standing on the labor market, and thus developed a much stronger dependence on the work permits provided by the program.

In line with this idea, the magnitude of the rescission effects for older immigrants is much smaller and statistically insignificant for almost all outcomes. The sole exception is school enrollment. This finding is somewhat surprising considering that these immigrants are past typical schooling age, but could be explained by sample limitations, as the proportion of them in the pre-period who are enrolled in school is barely 8%.

In general, likely due to small sample sizes, the Wald tests fail to yield significant p-values again. However, the findings of the previous literature and the significant and strong estimates for the subsample of immigrants aged at most 30 make a compelling argument for the possibility that the rescission impacted younger eligible immigrants more dramatically.

6.4 Education-based effect heterogeneity

As mentioned previously, this subsection investigates the existence of differential rescission threat effects by educational attainment. While admittedly there might be endogeneity concerns regarding this extension linked to DACA affecting college attendance, it is worth noting that the immigrants in my sample were aged at least 22 when DACA was released in 2012. This feature means that individuals in question were past typical schooling age when they received authorization, which should limit the probability of biased results.

From my main sample, I create two subsamples. The immigrants in the first one do not have a college degree, while those in the second one have graduated from college. The coefficients of interest from the regressions performed using these subsamples are presented in Table 7. Column 1 contains the estimates produced using non-college graduates, while Column 2 displays those from the college graduate subsample. As before, p-values for the Wald tests testing equivalence are also reported.

In general, both subsamples in question yield strong and statistically significant effects, albeit the magnitudes of these coefficients are typically much larger for college graduates—consider the 11.7-percentage-point reduction in employment versus the decrease of 3.69 percentage points for non-graduates—which is in line with previous literature. This can be because jobs available to college graduates are more likely to require formal employment authorizations like those provided by DACA and are less accessible for undocumented immigrants. Therefore, the threat of revoking these work permits is more likely to hurt college graduates who are plausibly employed in these positions on the mainstream labor market than their less educated counterparts, whose skill set is less likely to be tied to jobs requiring authorization. As before, however, the evidence for these differential trends is merely suggestive due to the lack of statistically significant Wald tests.

7 Robustness

7.1 Robustness to choice of pre-period

To address any concerns that the results are driven by the choice of pre-period as opposed to actual detrimental rescission effects, I conduct robustness checks using an alternative regression specification. In this setup, I assess the evolution of DACA-eligible immigrants over three periods: prior to the 2012 release of the policy, before the rescission announcement when DACA was relatively stable, and lastly after the rescission announcement.

For this analysis, the sample is similar to my main sample and consists of undocumented immigrants who were aged 22 to 30 in June 2012, immigrated to the U.S. prior to 2007 between the ages of 12 and 19, and were high school graduates when surveyed between 2009 and 2019. Using this sample, I perform regressions according to the following empirical specification:

$$Y_{itc} = \beta_0 + \beta_1 DACA_i + \beta_2 DACA_i \times Post2012_t + \beta_3 DACA_i \times Post2017_t + \\ + X_i\rho_1 + Z_{tc}\rho_2 + \gamma_t + \gamma_c + \epsilon_{itc} \quad (3)$$

where $Post2012_t$ is an indicator that is 1 for years 2013, 2014, 2015, 2016, and 2017, and 0 otherwise. Similarly, $Post2017_t$ is a dummy variable taking the value of 1 for 2018 and 2019, and 0 for all other years. All other variables and fixed effects are as specified above in Equation 1. My coefficients of interest are β_2 and β_3, which correspond to the intent-to-treat effects of DACA and its attempted rescission, respectively.

Following the same rationale as for my main effects, I use three different sets of survey years. First, I estimate my regression equation using all sample years, then I omit 2012 and 2017 to address ambiguity potentially arising from midyear treatment implementation, and lastly, I also omit 2016 to account for any anticipatory effects. Columns 1, 2, and 3 of Table 8 contain coefficients from the regressions using each of these setups, respectively. Regardless of the years included, the sign, approximate magnitude, and statistical significance of the effects on all outcomes are the same, but as before, my preferred approach for the sake of analytical rigor will omit 2012 and 2017.

The results demonstrate that DACA had significant, positive effects of 3.42 percentage points and 4.55 percentage points on eligible immigrants' labor force participation and employment, respectively, while the post-rescission period is statistically indistinguishable from the pre-DACA period. These estimates are analogous to the findings of the main specification, effectively illustrating that the threat to end the program erased the original benefits of the policy. Even more notably, however, this empirical specification shows a significant increase of 1.027 hours in employed eligible immigrants' weekly work hours and a decrease of their hourly wage by $1.256 (expressed in 1999 dollars) compared to the pre-DACA period. These findings mean that the rescission actually left eligible immigrants worse off than they were before the program's introduction.

These estimates provide further support for the validity of my research design, underscoring that the attempt to repeal the policy had strong and statistically significant adverse effects on eligible immigrants' labor market outcomes, regardless of the choice of pre-period.

7.2 Robustness to undocumented proxy method

As mentioned previously, while the residual proxy is a validated and widely accepted tool to identify undocumented immigrants in the economics literature, some may be concerned that the results of the analysis might be influenced by its use. To demonstrate that detecting the effects of the rescission threat does not depend on the choice of proxy for undocumented status, in Table 9, I present regressions using three alternative proxy methods to identify undocumented immigrants in the ACS data.

First, for the regressions in Column 1, I further restrict my sample created using the residual proxy to Hispanic respondents based on the rationale that it is often argued that most undocumented immigrants are of Hispanic ethnicity (Krogstad and Passel 2024). On the one hand, making this adjustment decreases my sample size, thus only my coefficients measuring the effects of the rescission threat on labor force participation and employment retain statistical significance. On the other hand, the sign and approximate magnitude of all my estimated effects hold, signaling the robustness of my main results.

Next, I drop the use of the residual method and instead proxy for undocumented status using non-citizen Hispanic immigrants. While the approximate magnitudes and signs of the estimates presented in Column 2 remain consistent, strengthening the argument for robust findings, I generally fail to demonstrate any statistical significance using this proxy.

Lastly, I relax the assumption of Hispanic origin, and simply proxy for undocumented immigrants using non-citizen status, for which I present estimates in Column 3. Generally, these coefficients, in terms of their signs, magnitudes, and significance, closely resemble the original estimates, confirming that the results are robust to proxy selection.

7.3 Robustness to alternative analytical approaches

To illustrate that my results are not unique to creating treatment and comparison groups using the age-at-arrival discontinuity and increase confidence in their external validity, Table 10 features coefficients produced using alternative analytical approaches.

In Column 1, I present estimates by applying my main empirical specification to a sample that is designed to exploit the year-of-arrival cutoff of the eligibility criteria. This alternative sample includes undocumented immigrants aged 14 to 19[13] in June 2012, who arrived in the U.S. between 2001 and 2010 (not including 2007) before their 16th birthday and were high school graduates when surveyed between 2014 and 2019 (2017 omitted, as before). In this sample, immigrants who arrived between 2001 and 2006 make up the eligible group, while the ineligible group consists of those having arrived between 2008 and 2010.

The regressions performed with this sample yield coefficients consistent with the main results, with the exception of the self-employment effects, which in this case end up being negative. This difference is likely due to the fact that this is a considerably younger group of eligible immigrants than those of the main sample. This trait means that their networks and resources are likely less well-established, hindering their ability to switch to self-employment. This argument is further supported by the fact that, in terms of magnitude, the coefficients produced by this sample most closely resemble those estimated using the at most 30-year-old subsample of the main sample.

Next, for the estimates in Columns 2 and 3, I move away from the strict discontinuity-based sample design. Instead of allowing for variation around a single eligibility cutoff, I prepare a combined sample exploiting both the age-at- and year-of-arrival cutoffs. My sample consists of undocumented immigrants aged 16 to 30 in June 2012 who arrived in the U.S. between 2001 and 2010 (2007 excluded), aged 12 to 19, and were high school graduates when surveyed between 2014 and 2019 (2017, again omitted). This sample design means that those in the ineligible group failed at least one of the age-at-arrival and the years in the U.S. requirements, but potentially both.

When estimating the specification from Equation 1 on this sample, I also add age at immigration and year of arrival to my regular set of controls. The coefficients from these regressions are presented in Column 2, and in their signs, significance levels, and approximate magnitudes closely match those of the main specification—with the slight difference that, presumably due to the increased sample size, the wage penalty also becomes statistically significant.

[13] Due to the fulfillment of the age in June 2012 and the age-at-arrival requirements, immigrants older than 19 in June 2012 could not have arrived after 2007, thus relaxing the age restrictions of the sample would produce incomplete comparisons.

To strengthen the argument that the effects in this broader sample are still driven by eligibility as a whole, rather than one specific eligibility criterion, for the coefficients in Column 3, I estimate the following empirical specification:

$$\begin{aligned} Y_{itc} = \beta_0 + \beta_1 DACA_i + \beta_2 Post_t \times Age_i + \beta_3 Post_t \times YrImmig_i + \\ + \beta_4 Post_t \times ImmigAge_i + \beta_5 DACA_i \times Post_t + X_i \rho_1 + Z_{tc} \rho_2 + \gamma_t + \gamma_c + \epsilon_{itc} \end{aligned} \quad (4)$$

which interacts each of the three eligibility criteria variables—age, year of immigration, age at arrival—with the $Post_t$ indicator for the post-rescission period. This regression equation also includes age-at- and year-of-immigration in the vector of individual controls, but is otherwise identical to the primary specification displayed in Equation 1. As before, the coefficient of interest, β_5, is the intent-to-treat effect of the rescission threat. Its values reported in Column 3 remain similar to those of the main results, strengthening the robustness of my findings.

Lastly, in Columns 4 and 5, I present estimates using the largest sample that allows for variation in any of the three core eligibility cutoffs—age, age-at-arrival, years in U.S. It consists of undocumented immigrants aged 16 to 35 in June 2012 who arrived in the U.S. in any year except for 2007 and when surveyed between 2014 and 2019 (2017 excluded) were high school graduates. This sample construction allows for the ineligible group to fail any combination of the three eligibility cutoffs. To ensure the rigor of the identification despite the increase in the complexity of the comparisons between eligible and ineligible immigrants, I include controls for age at and year of immigration when I estimate Equation 1 to produce the results in Column 4. Then, to further strengthen the validity of my coefficients, for the estimates in Column 5, I perform regressions using the specification from Equation 4, which interacts the variables of the eligibility criteria with the post-period variable. This ensures that the effects are not associated with a specific criterion and instead capture the impacts of DACA eligibility as a whole.

The coefficients for this analytical approach not only closely resemble those of the main results but also benefit from the drastically increased sample size, thereby increasing statistical significance. These findings, together with the results of the prior alternative approaches, demonstrate that my findings are highly robust and valid even beyond the main sample's narrow window around the age-at-arrival cutoff.

7.4 Placebo test

To demonstrate that the measured results are unique to DACA-eligible immigrants and thus can be interpreted as the labor market effects of the uncertainty arising from the rescission threat, I conduct two sets of placebo tests, which are novel in the relevant literature.

To address the concerns that the detected effects might merely be a materialization of anti-immigrant narratives present during the first Trump presidency, I create a placebo DACA eligibility variable. I consider an individual placebo DACA eligible if they 1) are foreign-born, 2) are high school graduates, 3) arrived in the U.S. prior to their 16th birthday, 4) were younger than 31 in June of 2012, and 5) have been continuously residing in the U.S. since 2007. This definition essentially utilizes the age, age at arrival, and education requirements of DACA while replacing undocumented status with being an immigrant, which allows me to observe if specific anti-immigrant biases during the uncertainty period drive my results.

I replace the original empirical specification's DACA eligibility indicator with this placebo eligibility and proceed to estimate the effects using two samples. The first sample consists of naturalized citizens who were aged 17 to 30 in 2012, arrived in the U.S. between 2001 and 2010 (excluding 2007), between the ages of 12 and 19, who, when surveyed between 2014 and 2019 (2017 excluded), were high school graduates. The coefficients estimated for this sample are displayed in Column 1 of Table 11. My second set of regressions further restricts the first sample to only Hispanic immigrants to more closely model the characteristics of typical DACA recipients and the kind of anti-immigrant narratives they might face. The results for this sample are presented in Column 2 of Table 11.

Assessing the results of these placebo tests, one can see that, with the exception of school attendance, which is likely a by-product of the fact that the sample members aged over time, none of the coefficients are statistically significant, and they all differ both in magnitude and direction from the effects of the rescission threat that I estimated using the main regression specifications. Therefore, I conclude that my results pass placebo tests and they capture the effects of the uncertainty created by the attempt to repeal DACA, which are distinct from any effects created by anti-immigrant sentiments during the period.

8 Conclusion

My paper uncovers what happened to DACA recipients' labor market outcomes during the period of uncertainty arising from the attempted repeal of the policy. I present statistically significant and robust evidence that the threat of ending DACA was highly detrimental to eligible workers' labor market outcomes. My findings demonstrate that, facing the expected loss of work permits and the possibility of deportation, the outcomes of these immigrants adjusted to erase virtually all of the original benefits of the program.

While my paper uncovers strong, negative labor market effects for the DACA rescission attempt, there is still much that we do not understand regarding how the changes to liminal legality policies impact their recipients. A potential future direction is an examination of non-labor market outcomes following an attempted repeal. Recent articles discuss how homeownership or insurance coverage can be affected by gaining access to temporary authorization policies like DACA (Wang, Winters, and Yuan 2022; Gihleb, Giuntella, and Lonsky 2023) or TPS (Harris and Jerch 2025). However, not much is known about the evolution of these outcomes when policy recipients face the threat of losing their status. These measures can be indicative of immigrants' economic, social, and cultural integration; thus, understanding how policy uncertainty alters them could strengthen our understanding of the interaction between interventions and immigrants' assimilation.

Given ongoing policy discussions, a more in-depth examination of heterogeneous responses to immigration enforcement and legislative uncertainty represents another intriguing avenue of future research. My paper offers suggestive evidence that immigrants' sex, age, and education influence how they experience the reversal of liminal legality policies, but a more detailed exploration of the underlying mechanisms falls outside the primary scope of this paper. Uncovering these group- and trait-specific differences in immigrants' behavior could help us design more effective policy tools and thus remains a highly relevant research direction.

Similarly, state-level and local characteristics seem to have an ever-growing role in determining which immigrant communities' labor market outcomes are most responsive to federal policy. Recent developments—such as the impending revocation of the work permits of DACA recipients residing in Texas (Kramer 2025)—highlight that these state-level differences will soon go beyond just mitigating enforcement and uncertainty effects. The possibility of heterogeneously implementing previously uniform federal policies signals the dawn of an era during which immigration policy might be formulated in a more local context than ever before. While my paper only touches on the matter of state-level heterogeneities in the form of an extension to the main research question, given these political dynamics and trends, legislative interactions between federal and state governments should warrant further exploration, and their study should be considered vital.

All in all, there is still much to uncover regarding the behavior of immigrant workers and their role in the U.S. economy and the way immigration policy interacts with the labor market and production, but my paper offers notable evidence that the positive effects of temporary legalization policies are highly vulnerable to reversal or even uncertainty. As previous literature showed, DACA significantly benefited not just its target population (Pope 2016; Amuedo-Dorantes and Antman 2016; Amuedo-Dorantes and Antman 2017) but the U.S. economy as a whole, as well (Ortega, Edwards, and Hsin 2018). However, my results indicate that many of these gains were eliminated or reversed as eligible immigrants' outcomes respond to legislative uncertainty, making the economic effects of temporary authorization very fragile in the long run.

It is also important to recognize that the number of liminal legality recipients experiencing this volatility is on the rise. Another major program, TPS, also underwent a rescission attempt in 2018, and more recently, it was terminated for a number of recipients. Given the severity of the economic effects demonstrated above, the growing number of affected immigrant workers, and the considerable role they play in the U.S. economy, a rigorous evaluation of the merits and the future of liminal legality programs is now more crucial than ever before.

My paper demonstrates that DACA and similar programs expose their recipients to legislative uncertainty, thereby weakening the long-run benefits associated with temporary authorization. Based on my findings, I argue that in this era of growing political polarization, the effectiveness of liminal legality initiatives is waning. My research highlights that if our goal is the successful economic integration of these immigrant workers in a manner that equally benefits them and the U.S. economy as a whole, a more permanent and comprehensive legalization policy crafted through bipartisan collaboration is necessary.

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A Tables and figures

Figure 1: The timeline of changes to DACA

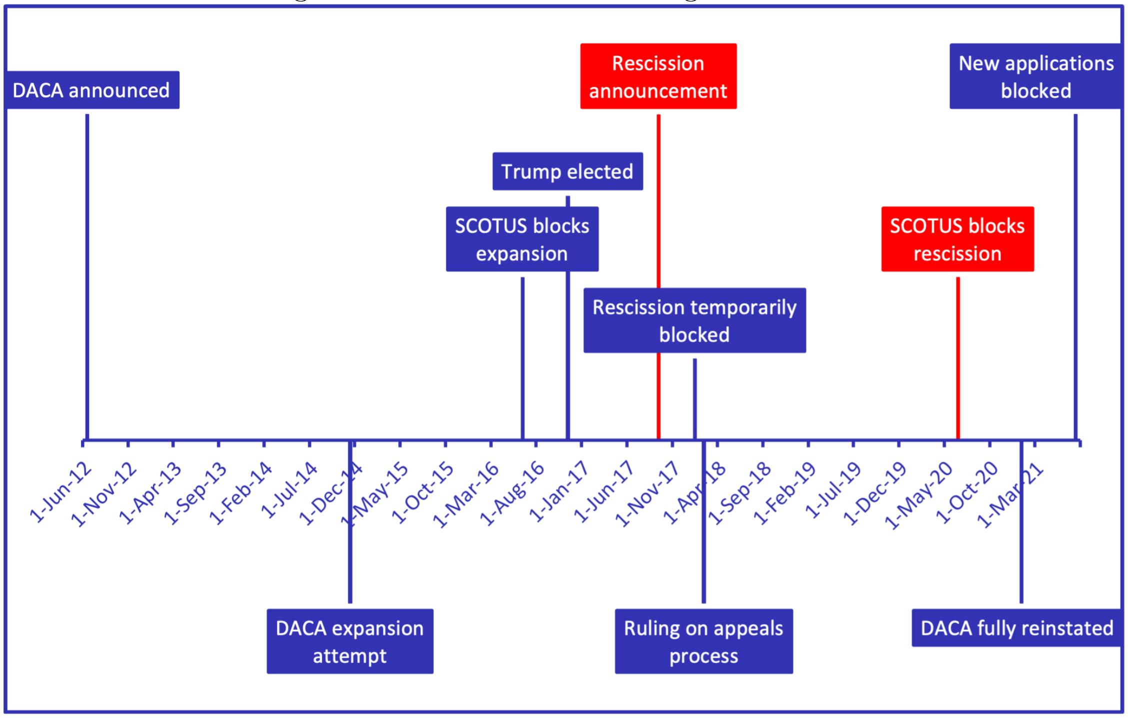
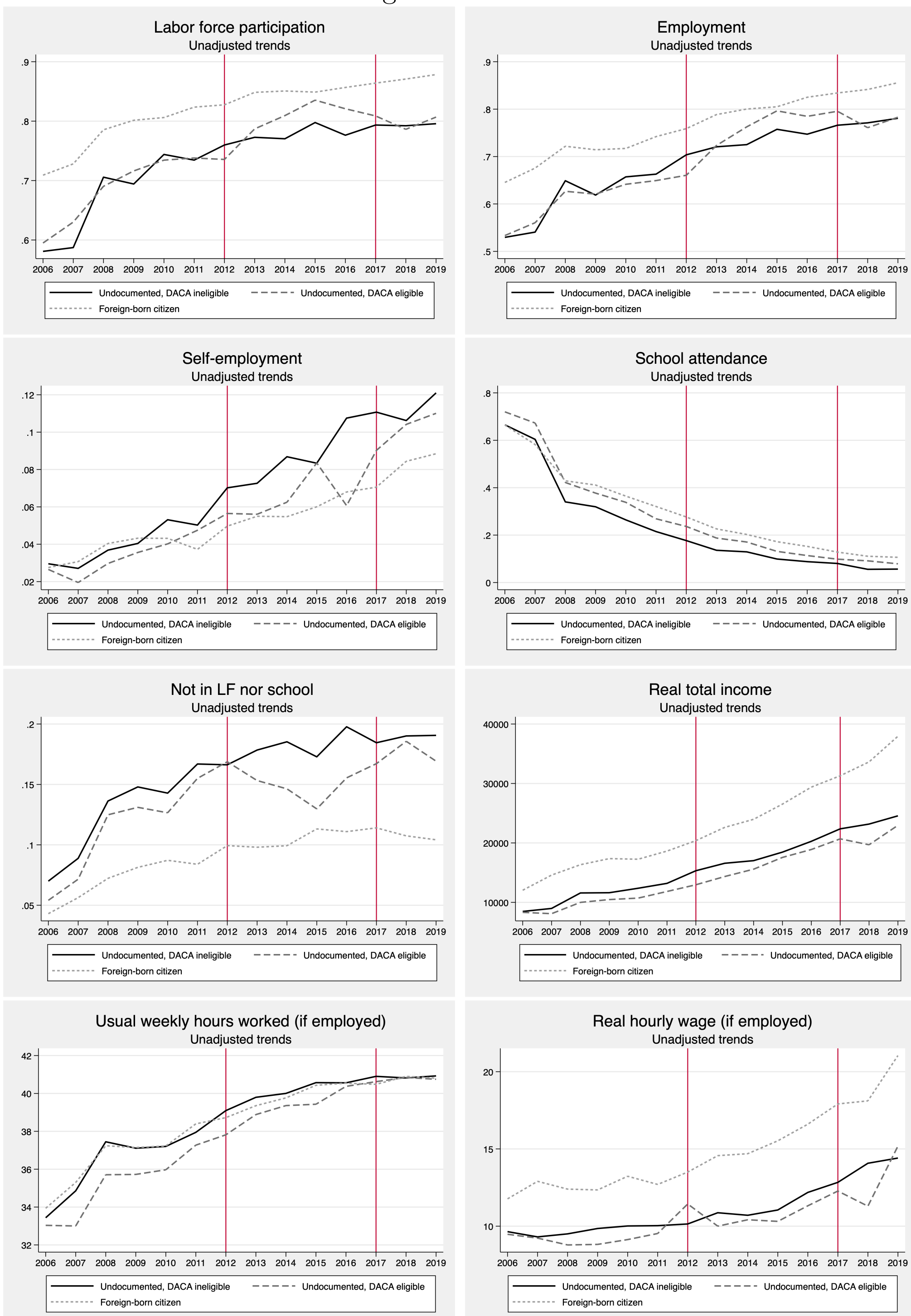


Figure 2: Trends



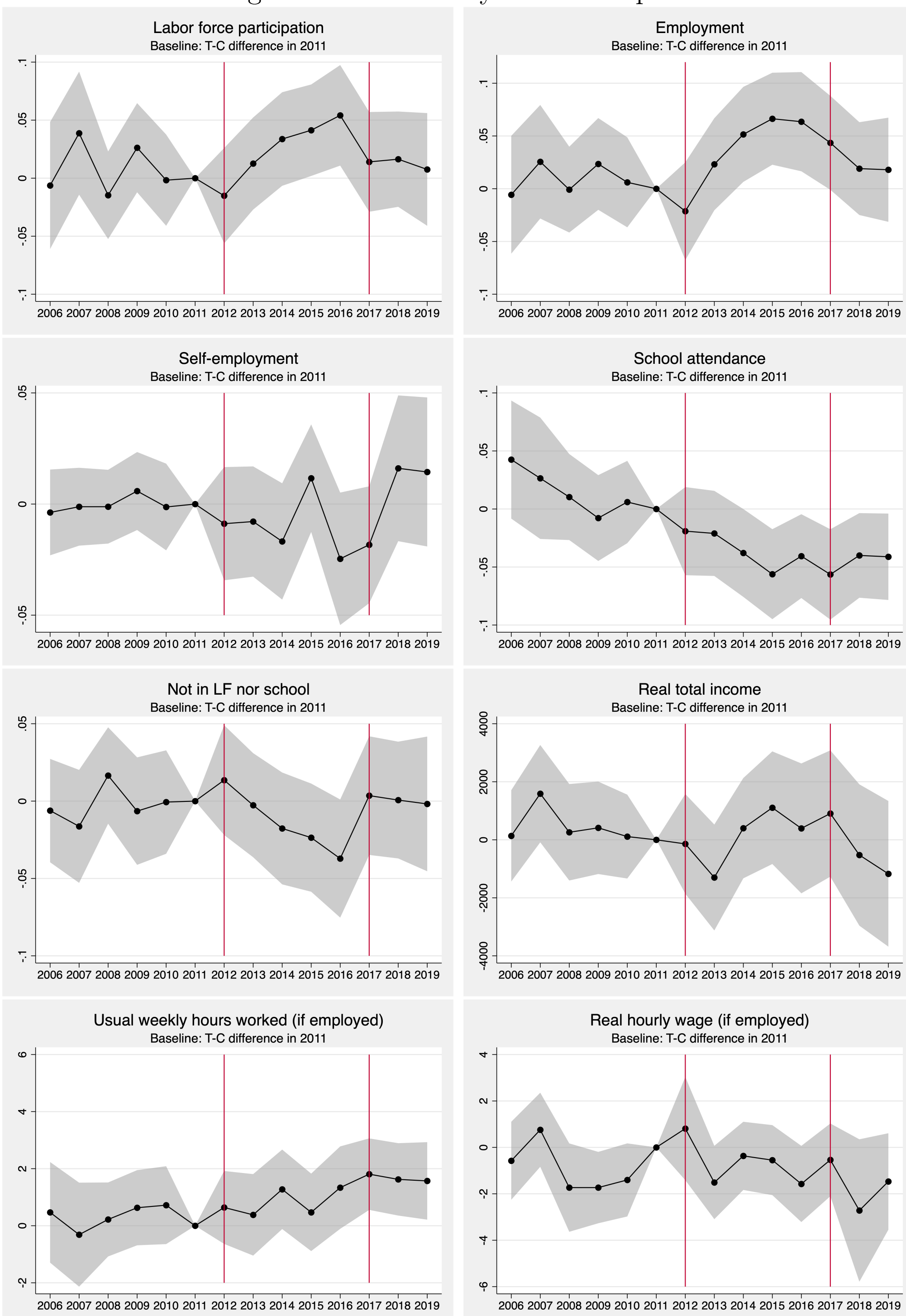
Notes: Sample: non-U.S.-born immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates when surveyed 2006-2019. DACA ineligible group: undocumented (residual proxy), arrived in the U.S. aged 16-19. DACA eligible group: undocumented (residual proxy), arrived in the U.S. aged 12-15. Baseline group: naturalized citizens.

Table 1: Summary statistics

	DACA ineligible		DACA eligible	
Variable	N	Mean	N	Mean
Age	6052	30.071	3552	28.870
		(0.032)		(0.044)
Male	6052	0.560	3552	0.567
		(0.006)		(0.008)
Asian	6052	0.143	3552	0.127
		(0.005)		(0.006)
Non-Hispanic Black	6052	0.072	3552	0.085
		(0.003)		(0.005)
Hispanic	6052	0.675	3552	0.690
		(0.006)		(0.008)
College graduate	6052	0.179	3552	0.158
		(0.005)		(0.006)
Years in U.S.	6052	12.513	3552	15.203
		(0.033)		(0.046)
C-PUMA LFPR	6052	0.761	3552	0.756
		(0.001)		(0.001)
C-PUMA ER	6052	0.936	3552	0.934
		(0.000)		(0.000)
Year of immigration	6052	2002.424	3552	1999.738
		(0.031)		(0.044)
Age at arrival	6052	17.558	3552	13.667
		(0.014)		(0.019)
In labor force	6052	0.781	3552	0.822
		(0.005)		(0.006)
Employed	6052	0.742	3552	0.781
		(0.006)		(0.007)
Self-employed	6052	0.092	3552	0.069
		(0.004)		(0.004)
In school	6052	0.107	3552	0.140
		(0.004)		(0.006)
Not in LF nor school	6052	0.185	3552	0.143
		(0.005)		(0.006)
Total income (1999 dollars)	6052	18479.346	3552	17252.564
		(311.633)		(299.678)
Usual weekly hrs. worked	6052	31.805	3552	32.802
		(0.243)		(0.298)
Hourly wage (1999 dollars)	4807	11.274	2962	10.587
		(0.173)		(0.192)

Notes: 2014-2016 group means and standard errors for treatment and comparison groups. Sample: undocumented (residual proxy) immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates at the time of survey. Eligible: aged 12-15 at arrival. Ineligible: aged 16-19 at arrival.

Figure 3: Event study coefficient plots



Notes: Coefficients of the event study interaction terms with controls for male, Asian, non-Hispanic black, Hispanic respondents, college degree, age, years in the U.S., and C-PUMA level employment and labor force participation rate. Fixed effects for survey year, C-PUMA. Sample: undocumented (residual proxy) immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates when surveyed 2006-2019. Eligible: aged 12-15 at arrival. Ineligible: aged 16-19 at arrival. Baseline: eligible-ineligible difference in outcome variable in 2011. C-PUMA level clustered standard errors shaded.

Table 2: Main results

Outcomes	(1) All sample years	(2) W/o. 2017	(3) W/o. 2016 & 2017
In LF	-0.0348**	-0.0393**	-0.0310*
	(0.0149)	(0.0155)	(0.0167)
Employed	-0.0468***	-0.0483***	-0.0418**
	(0.0157)	(0.0164)	(0.0176)
Self-employed	0.0283**	0.0261**	0.0181
	(0.0124)	(0.0131)	(0.0137)
In school	0.00848	0.00688	0.00898
	(0.0109)	(0.0116)	(0.0128)
Not in LF nor school	0.0224	0.0263*	0.0176
	(0.0144)	(0.0148)	(0.0154)
R. tot. inc.	-1,903**	-1,833*	-1,525
	(925.3)	(967.8)	(976.3)
Observations	17,707	14,799	11,872
U. w. hrs. worked	0.261	0.383	0.826
	(0.424)	(0.475)	(0.531)
R. hrly. wage	-1.136	-1.065	-1.372
	(0.876)	(0.899)	(0.873)
Observations	13,494	11,227	9,003

Notes: Coefficients of the post-2017 and DACA eligible interaction term from Equation 1 for each outcome with controls included for male, Asian, non-Hispanic black, Hispanic respondents, college degree, age, years in the U.S., and C-PUMA level employment and labor force participation rate. Fixed effects for survey year, C-PUMA. Sample: undocumented (residual proxy) immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates when surveyed 2014-2019. Eligible: aged 12-15 at arrival. Ineligible: aged 16-19 at arrival. Post-period survey years: 2018, 2019. (1) All sample years included. (2) Survey year 2017 excluded. (3) Survey years 2016 and 2017 excluded. C-PUMA level clustered standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Figure 4: States involved in anti-rescission lawsuits

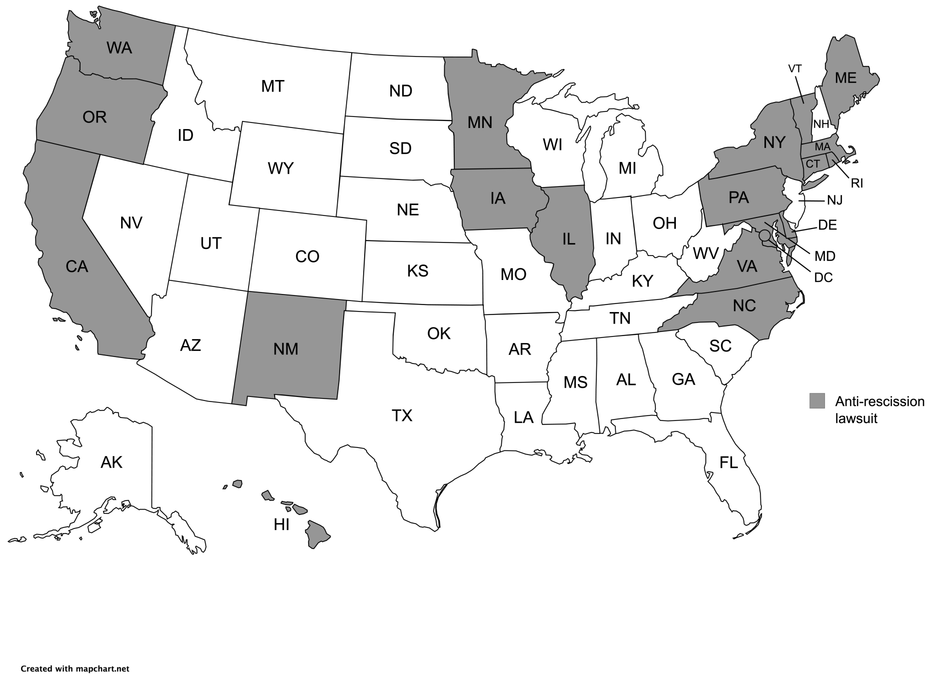


Figure 5: States offering driver's licenses to undocumented immigrants (2017)

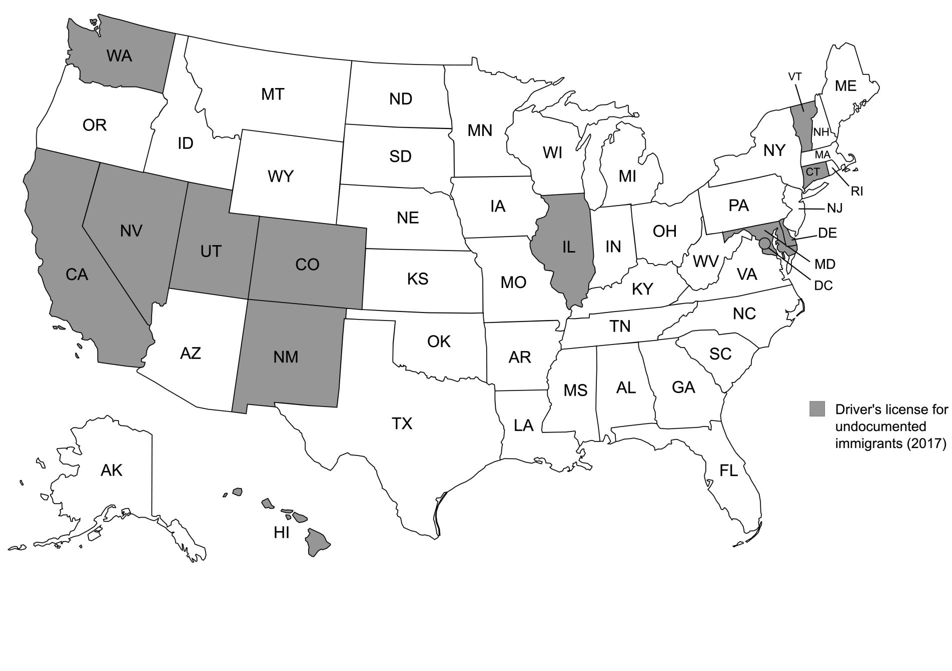


Figure 6: States offering in-state tuition to undocumented immigrants (2017)

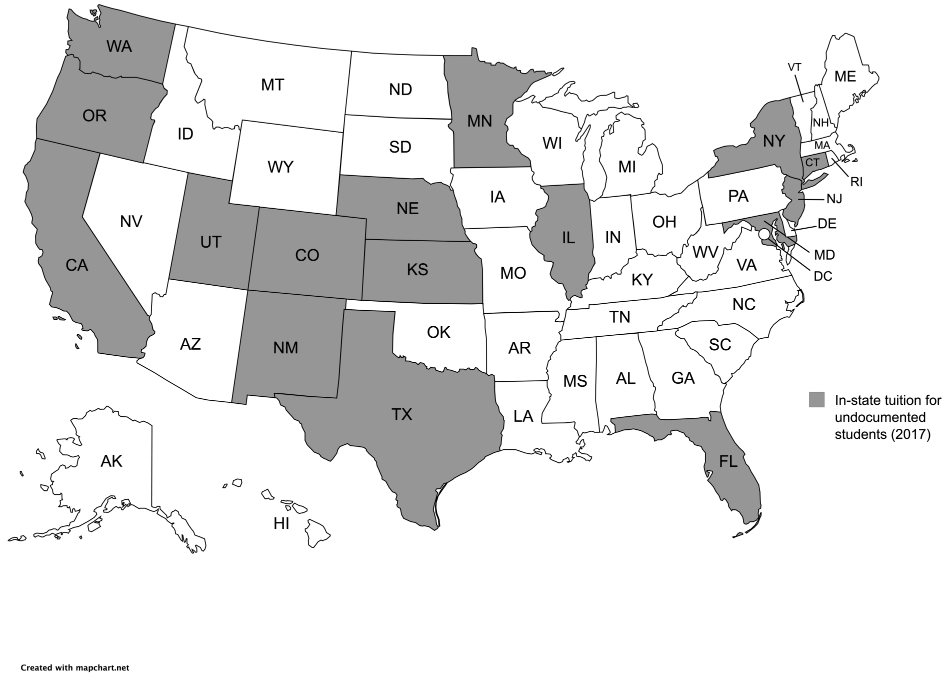


Table 3: State policy interactions

	(1) Lawsuit	(2) UDL	(3) UIST	(4) Combined
In LF				
Eligible x Post-2017	-0.0715***	-0.0417**	-0.0591*	-0.0836**
	(0.0225)	(0.0206)	(0.0318)	(0.0330)
Lawsuit x Eligible x Post-2017	0.0588*			0.0801**
	(0.0307)			(0.0401)
Undoc. DL x Eligible x Post-2017		0.00547		-0.0489
		(0.0313)		(0.0428)
Undoc. IS. Tuition x Eligible x Post-2017			0.0262	0.0274
			(0.0364)	(0.0394)
P-value: Lawsuit = UDL				0.0836*
P-value: Lawsuit = UIST				0.347
P-value: UDL = UIST				0.256
Employed				
Eligible x Post-2017	-0.0787***	-0.0464**	-0.0851***	-0.109***
	(0.0240)	(0.0215)	(0.0321)	(0.0345)
Lawsuit x Eligible x Post-2017	0.0557*			0.0840**
	(0.0328)			(0.0404)
Undoc. DL x Eligible x Post-2017		-0.00456		-0.0716
		(0.0333)		(0.0441)
Undoc. IS. Tuition x Eligible x Post-2017			0.0490	0.0588
			(0.0373)	(0.0408)
P-value: Lawsuit = UDL				0.0380**
P-value: Lawsuit = UIST				0.652
P-value: UDL = UIST				0.0643*
Self-employed				
Eligible x Post-2017	0.0199	0.0227	0.0113	0.00832
	(0.0209)	(0.0180)	(0.0246)	(0.0271)
Lawsuit x Eligible x Post-2017	0.0108			0.00849
	(0.0265)			(0.0336)
Undoc. DL x Eligible x Post-2017		0.00825		-0.00214
		(0.0260)		(0.0344)
Undoc. IS. Tuition x Eligible x Post-2017			0.0197	0.0182
			(0.0290)	(0.0314)
P-value: Lawsuit = UDL				0.861
P-value: Lawsuit = UIST				0.836
P-value: UDL = UIST				0.702
In school				
Eligible x Post-2017	0.0265	0.0150	0.0177	0.0310
	(0.0179)	(0.0158)	(0.0269)	(0.0290)
Lawsuit x Eligible x Post-2017	-0.0366			-0.0394
	(0.0234)			(0.0267)
Undoc. DL x Eligible x Post-2017		-0.0194		0.00635
		(0.0232)		(0.0277)
Undoc. IS. Tuition x Eligible x Post-2017			-0.0145	-0.00778
			(0.0297)	(0.0307)
P-value: Lawsuit = UDL				0.333
P-value: Lawsuit = UIST				0.405
P-value: UDL = UIST				0.767
Observations	14,799	14,799	14,799	14,799

Notes: Coefficients of the double differences estimator and its interactions with the policy indicators from Equation 2 for each outcome. Sample: undocumented (residual proxy) immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates when surveyed 2014-2019 (2017 excluded). Eligible: aged 12-15 at arrival. Ineligible: aged 16-19 at arrival. Post-period survey years: 2018, 2019. (1) Double differences specification with heterogeneity using an indicator for states participating in anti-rescission lawsuits. (2) Double differences specification with heterogeneity using an indicator for states issuing driver's licenses to undocumented immigrants in 2017. (3) Double differences specification with heterogeneity using an indicator for states offering in-state tuition to undocumented immigrants in 2017. (4) Double differences specifications with heterogeneities from previous columns combined into one equation. P-values from Wald tests for the equivalence of heterogeneity coefficients reported. C-PUMA level clustered standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 4: State policy interactions, cont'd

	(1) Lawsuit	(2) UDL	(3) UIST	(4) Combined
Not in LF nor school				
Eligible x Post-2017	0.0481**	0.0312	0.0370	0.0524*
	(0.0230)	(0.0202)	(0.0282)	(0.0294)
Lawsuit x Eligible x Post-2017	-0.0398			-0.0468
	(0.0299)			(0.0389)
Undoc. DL x Eligible x Post-2017		-0.0116		0.0180
		(0.0296)		(0.0412)
Undoc. IS. Tuition x Eligible x Post-2017			-0.0140	-0.00999
			(0.0330)	(0.0372)
P-value: Lawsuit = UDL				0.363
P-value: Lawsuit = UIST				0.512
P-value: UDL = UIST				0.665
R. tot. inc.				
Eligible x Post-2017	-2,658**	-2,768*	-432.6	-1,003
	(1,189)	(1,414)	(1,502)	(1,703)
Lawsuit x Eligible x Post-2017	1,554			802.9
	(1,861)			(2,777)
Undoc. DL x Eligible x Post-2017		2,298		2,694
		(1,874)		(3,158)
Undoc. IS. Tuition x Eligible x Post-2017			-1,877	-3,055
			(1,947)	(2,373)
P-value: Lawsuit = UDL				0.734
P-value: Lawsuit = UIST				0.198
P-value: UDL = UIST				0.245
Observations	14,799	14,799	14,799	14,799
U. w. hrs. worked				
Eligible x Post-2017	-0.161	-0.602	-1.447	-1.484
	(0.797)	(0.701)	(0.960)	(1.005)
Lawsuit x Eligible x Post-2017	0.986			-0.434
	(0.978)			(1.132)
Undoc. DL x Eligible x Post-2017		2.352**		2.037*
		(0.934)		(1.095)
Undoc. IS. Tuition x Eligible x Post-2017			2.444**	1.677
			(1.101)	(1.212)
P-value: Lawsuit = UDL				0.188
P-value: Lawsuit = UIST				0.252
P-value: UDL = UIST				0.843
R. hrly. wage				
Eligible x Post-2017	-1.759	-1.623	0.452	-0.0334
	(1.663)	(1.425)	(0.776)	(1.105)
Lawsuit x Eligible x Post-2017	1.286			0.895
	(1.871)			(2.161)
Undoc. DL x Eligible x Post-2017		1.336		1.762
		(1.609)		(2.261)
Undoc. IS. Tuition x Eligible x Post-2017			-2.043	-2.990
			(1.412)	(2.128)
P-value: Lawsuit = UDL				0.811
P-value: Lawsuit = UIST				0.245
P-value: UDL = UIST				0.241
Observations	11,227	11,227	11,227	11,227

Notes: Coefficients of the double differences estimator and its interactions with the policy indicators from Equation 2 for each outcome. Sample: undocumented (residual proxy) immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates when surveyed 2014-2019 (2017 excluded). Eligible: aged 12-15 at arrival. Ineligible: aged 16-19 at arrival. Post-period survey years: 2018, 2019. (1) Double differences specification with heterogeneity using an indicator for states participating in anti-rescission lawsuits. (2) Double differences specification with heterogeneity using an indicator for states issuing driver's licenses to undocumented immigrants in 2017. (3) Double differences specification with heterogeneity using an indicator for states offering in-state tuition to undocumented immigrants in 2017. (4) Double differences specifications with heterogeneities from previous columns combined into one equation. P-values from Wald tests for the equivalence of heterogeneity coefficients reported. C-PUMA level clustered standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect heterogeneity by sex

Outcomes	(1) Subsample of men	(2) Subsample of women
In LF	-0.0561***	-0.0151
	(0.0153)	(0.0336)
Heterogeneity p-value:		0.714
Employed	-0.0732***	-0.0195
	(0.0182)	(0.0339)
Heterogeneity p-value:		0.638
Self-employed	0.0274	0.0222
	(0.0187)	(0.0177)
Heterogeneity p-value:		0.672
In school	0.0101	-0.00465
	(0.0135)	(0.0213)
Heterogeneity p-value:		0.666
Not in LF nor school	0.0456***	0.00208
	(0.0140)	(0.0318)
Heterogeneity p-value:		0.531
R. tot. inc.	-1,667	-887.9
	(1,326)	(1,690)
Heterogeneity p-value:		0.977
Observations	8,218	6,344
U. w. hrs. worked	0.587	0.697
	(0.591)	(0.802)
Heterogeneity p-value:		0.645
R. hrly. wage	0.0345	-1.594
	(0.987)	(1.206)
Heterogeneity p-value:		0.212
Observations	7,083	3,909

Notes: Coefficients of the post-2017 and DACA eligible interaction term from Equation 1 for each outcome with controls included for Asian, non-Hispanic black, Hispanic respondents, college degree, age, years in the U.S., and C-PUMA level employment and labor force participation rate. Fixed effects for survey year, C-PUMA. Sample: undocumented (residual proxy) immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates when surveyed 2014-2019. Eligible: aged 12-15 at arrival. Ineligible: aged 16-19 at arrival. Post-period survey years: 2018, 2019. (1) Male subsample. (2) Female subsample. P-values from Wald tests for the equivalence of subsample coefficients reported. C-PUMA level clustered standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 6: Effect heterogeneity by age

Outcomes	(1) At most 30 years old	(2) Older than 30 years
In LF	-0.106***	-0.0202
	(0.0321)	(0.0225)
Heterogeneity p-value:		0.236
Employed	-0.124***	-0.0273
	(0.0338)	(0.0237)
Heterogeneity p-value:		0.235
Self-employed	0.0425	0.0198
	(0.0268)	(0.0206)
Heterogeneity p-value:		0.949
In school	-0.0378	0.0275*
	(0.0274)	(0.0148)
Heterogeneity p-value:		0.0237**
Not in LF nor school	0.0995***	0.0132
	(0.0290)	(0.0221)
Heterogeneity p-value:		0.181
R. tot. inc.	-3,878**	-51.79
	(1,677)	(1,731)
Heterogeneity p-value:		0.343
Observations	6,581	7,960
U. w. hrs. worked	0.919	0.469
	(1.064)	(0.717)
Heterogeneity p-value:		0.992
R. hrly. wage	-1.530*	-0.866
	(0.849)	(1.317)
Heterogeneity p-value:		0.942
Observations	4,869	6,108

Notes: Coefficients of the post-2017 and DACA eligible interaction term from Equation 1 for each outcome with controls included for male, Asian, non-Hispanic black, Hispanic respondents, college degree, age, years in the U.S., and C-PUMA level employment and labor force participation rate. Fixed effects for survey year, C-PUMA. Sample: undocumented (residual proxy) immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates when surveyed 2014-2019. Eligible: aged 12-15 at arrival. Ineligible: aged 16-19 at arrival. Post-period survey years: 2018, 2019. (1) At most 30 years old subsample. (2) Older than 30 years old subsample. P-values from Wald tests for the equivalence of subsample coefficients reported. C-PUMA level clustered standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 7: Effect heterogeneity by education

Outcomes	(1) Less than college	(2) At least college degree
In LF	-0.0292*	-0.0946**
	(0.0162)	(0.0472)
Heterogeneity p-value:		0.528
Employed	-0.0369**	-0.117**
	(0.0173)	(0.0490)
Heterogeneity p-value:		0.475
Self-employed	0.0279**	0.0327
	(0.0137)	(0.0320)
Heterogeneity p-value:		0.542
In school	0.00373	0.0110
	(0.0123)	(0.0402)
Heterogeneity p-value:		0.941
Not in LF nor school	0.0181	0.0716*
	(0.0157)	(0.0414)
Heterogeneity p-value:		0.503
R. tot. inc.	-718.9	-7,623
	(752.5)	(6,000)
Heterogeneity p-value:		0.318
Observations	12,241	2,329
U. w. hrs. worked	0.199	1.259
	(0.508)	(1.449)
Heterogeneity p-value:		0.628
R. hrly. wage	-0.943	-0.786
	(0.942)	(3.443)
Heterogeneity p-value:		0.777
Observations	9,189	1,817

Notes: Coefficients of the post-2017 and DACA eligible interaction term from Equation 1 for each outcome with controls included for male, Asian, non-Hispanic black, Hispanic respondents, age, years in the U.S., and C-PUMA level employment and labor force participation rate. Fixed effects for survey year, C-PUMA. Sample: undocumented (residual proxy) immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates when surveyed 2014-2019. Eligible: aged 12-15 at arrival. Ineligible: aged 16-19 at arrival. Post-period survey years: 2018, 2019. (1) Less than college subsample. (2) College graduate subsample. P-values from Wald tests for the equivalence of subsample coefficients reported. C-PUMA level clustered standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 8: Three-period results

	(1) All survey years	(2) W/o. 2012 & 2017	(3) W/o. 2012, 2016 & 2017
In LF			
Eligible x Post-2012	0.0342***	0.0342**	0.0277**
	(0.0110)	(0.0133)	(0.0137)
Eligible x Post-2017	0.0136	0.00908	0.00962
	(0.0152)	(0.0163)	(0.0162)
Employed			
Eligible x Post-2012	0.0523***	0.0455***	0.0411***
	(0.0126)	(0.0144)	(0.0151)
Eligible x Post-2017	0.0207	0.0129	0.0146
	(0.0167)	(0.0172)	(0.0171)
Self-employed			
Eligible x Post-2012	-0.00732	-0.00757	-0.00329
	(0.00687)	(0.00771)	(0.00776)
Eligible x Post-2017	0.0195*	0.0169	0.0165
	(0.0117)	(0.0117)	(0.0118)
In school			
Eligible x Post-2012	-0.0392***	-0.0421***	-0.0403***
	(0.0103)	(0.0119)	(0.0124)
Eligible x Post-2017	-0.0387***	-0.0445***	-0.0431***
	(0.0120)	(0.0129)	(0.0130)
Not in LF nor school			
Eligible x Post-2012	-0.0176**	-0.0158	-0.0106
	(0.00866)	(0.0103)	(0.0107)
Eligible x Post-2017	-0.00248	0.00211	0.000930
	(0.0129)	(0.0136)	(0.0135)
R. tot. inc.			
Eligible x Post-2012	224.5	112.5	-123.1
	(476.9)	(516.6)	(531.2)
Eligible x Post-2017	-1,040	-949.9	-1,035
	(846.8)	(869.7)	(867.3)
Observations	34,743	27,905	24,993
U. w. hrs. worked			
Eligible x Post-2012	0.445	0.294	0.162
	(0.312)	(0.381)	(0.410)
Eligible x Post-2017	1.058**	1.027**	1.156**
	(0.437)	(0.464)	(0.462)
R. hrly. wage			
Eligible x Post-2012	-0.587	-0.192	-0.0567
	(0.478)	(0.489)	(0.494)
Eligible x Post-2017	-1.789**	-1.256*	-1.397*
	(0.803)	(0.738)	(0.735)
Observations	25,026	20,059	17,845

Notes: Coefficients of the double difference estimators from Equation 3 for each outcome with controls included for male, Asian, non-Hispanic black, Hispanic respondents, college degree, age, years in the U.S., and C-PUMA level employment and labor force participation rate. Fixed effects for survey year, C-PUMA. Sample: undocumented (residual proxy) immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates when surveyed 2009-2019. Eligible: aged 12-15 at arrival. Ineligible: aged 16-19 at arrival. (1) All sample years included. (2) Survey years 2012 and 2017 excluded. (3) Survey years 2012, 2016, and 2017 excluded. C-PUMA level clustered standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 9: Robustness to alternative proxies

Outcomes	(1) Residual & Hispanic proxies	(2) Hispanic non-citizens	(3) Non-citizens
In LF	-0.0313*	-0.0233	-0.0275**
	(0.0168)	(0.0151)	(0.0130)
Employed	-0.0351*	-0.0178	-0.0270*
	(0.0187)	(0.0166)	(0.0143)
Self-employed	0.0124	0.0124	0.0222**
	(0.0152)	(0.0124)	(0.0107)
In school	0.0203	0.00842	0.00490
	(0.0133)	(0.0105)	(0.00950)
Not in LF nor schoo	0.0218	0.0210	0.0211*
	(0.0165)	(0.0154)	(0.0128)
R. tot. inc.	-1,112	-805.0	-1,779**
	(868.5)	(742.2)	(802.3)
Observations	10,183	14,400	21,669
U. w. hrs. worked	0.339	0.563	0.359
	(0.639)	(0.590)	(0.424)
R. hrly. wage	-1.101	-0.890	-1.204
	(1.071)	(0.952)	(0.809)
Observations	7,738	10,799	16,504

Notes: Coefficients of the post-2017 and DACA eligible interaction term from Equation 1 for each outcome with controls included for male, Asian, non-Hispanic black, Hispanic respondents, college degree, age, years in the U.S., and C-PUMA level employment and labor force participation rate. Fixed effects for survey year, C-PUMA. Sample: undocumented immigrants aged 22-30 in June 2012, arrived in the U.S. before 2007, aged 12-19 who were high school graduates when surveyed 2014-2019. Eligible: aged 12-15 at arrival. Ineligible: aged 16-19 at arrival. Post-period survey years: 2018, 2019. (1) Undocumented status coded as residual and Hispanic proxy. (2) Undocumented status coded as Hispanic non-citizens. (3) Undocumented status coded as non-citizens. C-PUMA level clustered standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

Table 10: Robustness to alternative analytical approaches

Outcomes	(1) Yr. of arr. cutoff	(2) Mixed cutoffs	(3) Mixed cutoffs	(4) DiD	(5) DiD
In LF	-0.113***	-0.0601***	-0.0732**	-0.0519***	-0.0461***
	(0.0324)	(0.0176)	(0.0319)	(0.00660)	(0.0103)
Employed	-0.0992***	-0.0526***	-0.0846**	-0.0500***	-0.0431***
	(0.0359)	(0.0189)	(0.0336)	(0.00709)	(0.0108)
Self-employed	-0.0254*	0.0226*	0.0271	-0.00591	-0.00128
	(0.0136)	(0.0123)	(0.0210)	(0.00437)	(0.00661)
In school	0.0219	0.0213	0.0402	-0.0134**	0.0467***
	(0.0352)	(0.0152)	(0.0287)	(0.00567)	(0.00778)
Not in LF nor school	0.0237	0.0206	0.0353	0.0220***	-0.0263***
	(0.0237)	(0.0153)	(0.0267)	(0.00570)	(0.00911)
R. tot. inc.	-807.0	-2,893***	1,927	-1,128***	-490.6
	(1,020)	(877.8)	(1,606)	(398.4)	(549.4)
Observations	6,547	16,254	16,254	146,012	146,012
U. w. hrs. worked	-1.656	0.344	0.728	1.008***	-0.346
	(1.189)	(0.557)	(1.085)	(0.210)	(0.328)
R. hrly. wage	0.183	-1.912**	1.504	0.112	-1.552**
	(0.850)	(0.948)	(1.485)	(0.443)	(0.760)
Observations	3,784	11,432	11,432	93,764	93,764
Interacted controls	No	No	Yes	No	Yes

Notes: (1) Coefficients of the double difference estimator from Equation 1 for each outcome. Sample: undocumented (residual proxy) immigrants aged 14-19 in June 2012, arrived in the U.S. before age 16 between 2001 and 2010 (2007 excluded) who, when surveyed 2014-2019 (2017 excluded), were high school graduates. Eligible: arrived before 2007. Ineligible: arrived after 2007. (2)-(3) Sample: undocumented (residual proxy) immigrants aged 16-30 in June 2012, arrived in the U.S. aged 12-19 between 2001 and 2010 (2007 excluded) who, when surveyed 2014-2019 (2017 excluded), were high school graduates. Eligible: arrived aged 12-15 before 2007. Ineligible: all other respondents. (2) Coefficients of the double difference estimator from Equation 1 for each outcome with added controls for age at and year of arrival. (3) Coefficients of the double difference estimator from Equation 4 for each outcome. (4)-(5) Sample: undocumented (residual proxy) immigrants aged 16-35 in June 2012, arrived in the U.S. aged 12-19 between 2001 and 2010 (2007 excluded) who, when surveyed 2014-2019 (2017 excluded), were high school graduates. Eligible: less than 30 years old in June 2012, arrived aged 12-15 before 2007. Ineligible: all other respondents. (4) Coefficients of the double difference estimator from Equation 1 for each outcome with added controls for age at and year of arrival. (5) Coefficients of the double difference estimator from Equation 4 for each outcome.

Table 11: Placebo tests

Outcomes	(1) Naturalized citizens	(2) Naturalized Hispanic citizens
In LF	0.00776	0.0143
	(0.0123)	(0.0240)
Employed	0.00519	0.0120
	(0.0139)	(0.0250)
Self-employed	-0.0105	0.000469
	(0.00821)	(0.0154)
In school	-0.0417***	-0.0469*
	(0.0148)	(0.0262)
Not in LF nor school	0.0138	0.00569
	(0.0105)	(0.0212)
R. tot. inc.	454.1	273.9
	(946.9)	(1,139)
Observations	20,935	5,912
U. w. hrs. worked	0.140	-0.0325
	(0.429)	(0.695)
R. hrly. wage	0.282	-0.666
	(0.622)	(0.873)
Observations	16,562	4,666

Notes: Coefficients of the placebo difference-in-differences estimator for each outcome with controls included for male, Asian, non-Hispanic black, Hispanic respondents, college degree, age, years in the U.S., C-PUMA level employment, and labor force participation rate. Fixed effects for survey year, C-PUMA. Survey years: 2014-2019, 2017 excluded. (1) Treatment group: Naturalized immigrants aged 17-30 in June 2012, who arrived in the U.S. between 2001 and 2006, aged 12-15, who were high school graduates at the time of survey. Comparison group: Naturalized immigrants aged 17-30 in June 2012, arrived in the U.S. 2001-2006 aged 16-19 or arrived in the U.S. 2008-2010 aged 12-15 or arrived in the U.S. 2008-2010 aged 16-19 who were high school graduates at the time of survey. (2) Treatment group: Naturalized Hispanic immigrants aged 17-30 in June 2012, who arrived in the U.S. between 2001 and 2006, aged 12-15, who were high school graduates at the time of survey. Comparison group: Naturalized Hispanic immigrants aged 17-30 in June 2012, arrived in the U.S. 2001-2006 aged 16-19 or arrived in the U.S. 2008-2010 aged 12-15 or arrived in the U.S. 2008-2010 aged 16-19 who were high school graduates at the time of survey. C-PUMA level clustered standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$