NBER WORKING PAPER SERIES

DO HUMAN CAPITAL DECISIONS RESPOND TO THE RETURNS TO EDUCATION? EVIDENCE FROM DACA

Elira Kuka
Na'ama Shenhav
Kevin Shih

Working Paper 24315
http://www.nber.org/papers/w24315

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2018

We would like to thank Anna Aizer, Catalina Amuedo-Dorantes, Marcella Alsan, Elizabeth Cascio, Aimee Chin, Ethan Lewis, Dan Millimet, Marianne Page, Bruce Sacerdote, Doug Staiger, and participants at the Barcelona GSE Summer Forum Migration meeting, UC Davis Alumni Conference, WEAI Conference, and SEA Conference for helpful comments and suggestions. We are also grateful to Alex Magnuson for excellent research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2018 by Elira Kuka, Na'ama Shenhav, and Kevin Shih. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Do Human Capital Decisions Respond to the Returns to Education? Evidence from DACA
Elira Kuka, Na'ama Shenhav, and Kevin Shih
NBER Working Paper No. 24315
February 2018
JEL No. I20,I26,J1,J13

ABSTRACT

This paper studies the human capital responses to a large shock in the returns to education for undocumented youth. We obtain variation in the benefits of schooling from the enactment of the Deferred Action for Childhood Arrivals (DACA) policy in 2012, which provides work authorization and deferral from deportation for high school educated youth. We implement a difference-in-differences design by comparing DACA eligible to non-eligible individuals over time, and we find that DACA had a significant impact on the investment decisions of undocumented youth. High school graduation rates increased by 15 percent while teenage births declined by 45 percent. Further, we find that college attendance increased by 25 percent among women, suggesting that DACA raised aspirations for education above and beyond qualifying for legal status. We find that the same individuals who acquire more schooling also work more (at the same time), counter to the typical intuition that these behaviors are mutually exclusive, indicating that the program generated a large boost in productivity.

Elira Kuka
Department of Economics
Southern Methodist University
3300 Dyer Street, Umphrey Lee Center
Dallas, TX 75275
and NBER
ekuka@smu.edu

Kevin Shih
Department of Economics
Russell Sage Laboratory
Rensselaer Polytechnic Institute
110 8th Street
Troy, NY 12180
shihk2@rpi.edu

Na'ama Shenhav
Department of Economics
Dartmouth College
6106 Rockefeller Hall
Hanover, NH 03755
naama.shenhav@dartmouth.edu

1 Introduction

Whether individuals respond to incentives for schooling is a central question for understanding gaps in educational attainment. The canonical model of human capital predicts that individuals respond to returns to education, as with any investment (Becker, 1967). However, individuals may have difficulty accessing accurate information about these returns or may be liquidity constrained, preventing them from pursuing the optimal schooling behavior.[1] Disadvantaged students may be particularly susceptible to these pitfalls; yet to date, there has been little work documenting how this population responds to schooling incentives.

In this paper, we study the human capital responses of undocumented youth to a salient shock in their returns to education, which resulted from the institution of the Deferred Action for Childhood Arrivals program (DACA). Announced in June 2012, and in effect from August 2012 to October 2017, DACA granted temporary relief from deportation and work authorization – two years, initially, subject to renewal – for undocumented youth that were in school or had completed high school, and met other criteria based on age and year of arrival. DACA's explicit education requirement coupled with work authorization, could have made schooling attractive along two major dimensions. First, DACA receipt led to a 100% decline in deportation risk, roughly 7.3 (1.5) percentage points for men (women) ages 18 to 39.[2] Second, based on prior estimates of the labor market effects of legalization, DACA led to an expected increase in wages of between 6 and 14 percent as well as higher wage returns to years of schooling.[3]

Undocumented youth are a population of interest for several reasons. First, similar to other under-resourced populations, they face several disincentives to acquire human capital, such as lack of information about college applications, uncertainty over the costs and returns to schooling, as well as reduced access to credit markets (Amuedo-Dorantes and Bansak, 2006; Osili and Paulson, 2009). Hence, our results may elucidate the education choices of other low-income youth. Second, the fact that undocumented children have persistently low rates of high school graduation is worthy of attention in itself, as high school dropouts fare worse along multiple measures of health, family life, and economic success. Since this likely reflects, in part, uncertainty over employment and lower wage returns to education, policies that target these barriers could improve a constellation of behaviors (Borjas, 2017). Third, undocumented youth have been frequently at the center of the intense policy debates

[1] See Bleemer and Zafar (2015); Brown, Karl Scholz and Seshadri (2011); Jensen (2010).

[2] Details of calculation in Section 3.

[3] Borjas (2017); Bratsberg, Ragan and Nasir (2002); Rivera-Batiz (1999); Kossoudji and Cobb-Clark (2002) empirically estimate the effect of legal residence utilizing cross-sectional differences and exploiting the 1986 Immigration Reform and Control Act (IRCA).

on immigration reform. Fully understanding the effects of DACA is therefore crucial for evaluating the benefits and costs of future DACA-like policies.

We navigate several empirical challenges to identify the causal response to DACA. First, to our knowledge, there are no available data over this period that contains information on legal status and education for a large sample of youth. As a result, we follow the literature and rely on the absence of U.S. citizenship combined with country of origin as a second-best measure of undocumented status.[4] Second, while non-eligible undocumented youth might ex-ante be a sensible comparison group, we show that the early age of arrival (before 16) and year of arrival (before 2007) required for DACA make eligible youth significantly more predisposed to stay in school relative to non-eligible undocumented youth. Instead, we use foreign-born citizens with identical age and year of arrival profiles as our comparison group. Third, we limit our attention to individuals that arrived before age 10 to address compositional issues, which we clarify in our empirical methodology.

Hence, our difference-in-difference framework compares immigrant non-citizens (treated) to immigrant citizens (comparison) over time using the 2005 to 2015 American Community Surveys (ACS). This empirical design is similar in spirit to other recent policy evaluations that identify treatment effects by utilizing counterfactuals that vary along demographic traits, such as income, nationality, age, and/or year of arrival (Jackson, Johnson and Persico, 2016; Kleven et al., 2013; Marie and Zölitz, 2017). The data provide strong support for the identifying assumptions. First, we show that the average school attendance and high school completion of the treated and control groups tracked each other closely for seven years prior to DACA, and that there is an apparent closing of the gap in these outcomes after 2012. Second, we demonstrate that a large set of observable characteristics do not predict a differential improvement in schooling of the eligible population after DACA. Third, we show that our results are largely insensitive to using alternative control groups or specifications, including propensity score methods.

We find that DACA had a significant impact on adolescents' schooling, work, and fertility decisions. Our preferred estimates analyzing Hispanics show that DACA led to a 3.3 percentage point (p.p.) increase in the school attendance of 14 to 18 year olds and an 11.4 p.p. increase in the high school completion of 19 year olds, relative to a mean of 75 percent. Our results imply that more than 49,000 additional Hispanic youth obtained a high school diploma because of DACA. This large response coupled with the fact that the effects are significantly larger for males, who have an elevated risk of deportation, suggests that teenagers value the benefits of DACA and remain in school to attain eligibility.

Moreover, despite the fact that post-secondary schooling was not required for DACA,

[4]See Kaushal (2006), Pope (2016) and Amuedo-Dorantes and Antman (2016).

we also find an increase along this margin, particularly among young Hispanic women. The college enrollment rate of 19 year old Hispanic females increased by 9.8 p.p., a 25 percent relative to the mean. The effects are smaller, though still positive, for individuals further from the typical college enrollment age, indicating that momentum may play a role in these decisions. We interpret these effects as evidence that young adults responded to the future wage returns to education offered by DACA, and not just the near-term benefits of qualifying for the program.

At the same time, we also observe increases in working among individuals that attend more school. This is not necessarily surprising, since students may need to work in order to afford schooling or may try to reconcile competing incentives for work and school by doing both. However, it runs counter to the typical modeling choices and empirical implementation, which often treat work and schooling as mutually exclusive choices (see e.g. Charles, Hurst and Notowidigdo (2015)). Eligible individuals show a marked decline in being "idle" – neither in school nor working – indicating that the program generated a large boost in productivity.

Finally, we consider whether higher returns to education could also influence the desire to have children. We find that DACA leads to lower rates of pregnancy among high-school-aged women (ages 15 to 18). This group of teenagers is 1.7 p.p. less likely to give birth after DACA, which corresponds to a 45 percent reduction. Intuitively, the decline in fertility is concentrated among women that were on the margin of a first birth, whose future employment and education decisions are most sensitive to the presence of an additional child.

To gain additional insight into these results, we show the effect of DACA on test scores and youth sexual behavior by exploiting variation in the geographic concentration of eligible youth. Using administrative data from California, we show that the undocumented also exerted greater effort to learn required concepts, as demonstrated by a higher share of students passing a mandatory high stakes test for graduation (the California High School Exit Exam). Additional evidence from the Youth Risk Behavior Factor Surveillance Survey (YRBSS) indicates that eligible teens were more likely to use pregnancy prevention methods, revealing that the reduction in fertility was a result of concerted efforts to delay motherhood.

Our findings speak to central questions in education and immigration policy.

First, we provide compelling evidence that a large share of the gap in the high school graduation, college attendance and teenage pregnancy of undocumented students and their peers is attributable to the uncertain and limited returns to schooling. Previous evidence has made clear that an increase in the opportunity cost of schooling can exacerbate dropout rates contemporaneously (Black, McKinnish and Sanders, 2005; Cascio and Narayan, 2015; Charles, Hurst and Notowidigdo, 2015; Atkin, 2016; Shah and Steinberg, 2017). However, it is not at all clear that responses to higher wage returns in the future should be expected to

mirror these effects, since obtaining a degree, unlike dropping out, requires individuals to put forth effort, patience, and to be sufficiently forward-looking. Prior work finds evidence for this behavior by exploiting novel, though often context- or skill-specific interventions, such as foreign firm entry, communal income-sharing, or experimental information treatments (Oster and Steinberg, 2013; Abramitzky and Lavy, 2014; Jensen, 2010; Wiswall and Zafar, 2014). We move these findings to a more general, national policy setting, and produce direct policy implications for raising the human capital of a large population of disadvantaged youth.

We also provide novel evidence of the response to a *conditional* and *temporary* amnesty, whereas the majority of the literature focuses on unconditional amnesties. Unconditional amnesties have been shown to improve labor market outcomes (Rivera-Batiz, 1999; Kossoudji and Cobb-Clark, 2002; Kaushal, 2006; Steigleder and Sparber, 2017), increase college enrollment (Cortes, 2013)[5], decrease crime (Pinotti, 2017), and increase EITC receipt and payroll tax payments (Cascio and Lewis, 2016; Monras, Vázquez-Grenno and Elias Moreno, 2017). Our results document that individuals are willing to overcome significant hurdles – in this case educational requirements – to obtain legal status, even when the duration of the status remains uncertain.

Relative to previous research on DACA, we show that the program had large positive effects on high school attainment and college attendance. Earlier studies use high school graduation as a criteria for DACA eligibility, which precludes them from examining effects on high school enrollment or graduation, and find zero or negative effects on post-secondary schooling (Pope, 2016; Amuedo-Dorantes and Antman, 2017; Hsin and Ortega, 2017). We make two important distinctions relative to these studies. First, we focus on a more narrow range of ages around typical high school graduation and college enrollment, who we show are more responsive to DACA. Second, we capture the total impact of DACA on college attendance, which includes the effect on the rate of college-eligibility (i.e graduating from high school), attendance conditional on eligibility, and persistence. Earlier studies omit effects on at least one of these margins. These new findings should inform the current debate on immigration policy, which has until now ignored the role for a path to legalization in producing an educated immigrant workforce.

We also enrich existing evidence linking schooling decisions and teenage pregnancy, showing a meaningfully larger connection between these choices than prior estimates. Current studies focus on the impact of mandatory schooling requirements on fertility decisions - holding returns constant - and find mixed results (McCrary and Royer, 2011; Black, Devereux

[5]Additionally, Liscow and Woolston (2016) analyze the impact of legalization on teenage schooling by exploiting variation in sibling citizenship within mixed-citizen families. However, the effect of being born in the U.S (or citizenship) may be quite different than DACA's temporary amnesty, and mixed-citizen families are not representative of DACA-eligible youth.

and Salvanes, 2008). Our results show that fertility responds strongly to the perception of future opportunities. Hence, we provide causal evidence to support earlier claims that the prevalence of teenage births among disadvantaged communities is a reaction to lack of opportunity (Kearney and Levine, 2014, 2012).

The paper continues as follows. We provide further detail regarding the implementation and institutional details of DACA in Section 2. In Section 3 we examine the incentives of DACA and generate empirical predictions for education and teenage fertility responses. We discuss our data and empirical strategies in Sections 4 and 5. Section 6 presents results on schooling attendance, fertility, and working, and Section 7 provides evidence on mechanisms, including exit exam performance and sexual behavior. We include sensitivity exercises in Section 8 and conclude in Section 9.

2 DACA's Legislative History and Take-up

Prior to DACA, there were multiple attempts at federal legislation to create a unifying policy for undocumented students (Olivas, 2004). The DREAM Act put forth in 2001 was the most prominent of these efforts, proposing a pathway to legalization for undocumented childhood immigrants conditional on meeting minimum education requirements. Momentum for the DREAM Act dissipated in 2010, however, after opposing political parties failed to come to a resolution. This legislative inaction led to the enactment of DACA by Executive Order in June 2012.[6]

DACA provides two types of benefits to recipients. First, deportation is deferred for two years, allowing beneficiaries to reside legally in the U.S. Second, beneficiaries receive an Employment Authorization Document (EAD), commonly referred to as a work permit, which grants recipients work authorization. Possession of an EAD also allows individuals to apply for a Social Security number, which opens the possibility of obtaining a state identification card or driver's license (in many states), and can reduce the frictions to applying for a credit card, bank account, or loan.

Application requests are initially granted for two years, but recipients may request an extension through a renewal process.[7] A recent estimate finds that roughly 93% of recipients applied for renewal after the initial two-year period (Hipsman, Gómez-Aguiaga and Capps, 2016). The prevalence of renewals could reflect an expectation among recipients that the

[6]Note that while DACA was officially announced and enacted in June 2012, applications were only accepted beginning in August 2012.

[7]Renewal applicants must satisfy the same criteria as initial applications, although the renewal form does not contain questions about schooling completion as in the initial application.

program would persist beyond two years.[8] Efforts to expand the reach of DACA, though never passed, could have further added to expectations of the program's longevity.[9]

DACA applicants must meet a suite of immigration, education, and criminal requirements for approval. The first set of requirements are based on age and date of arrival in the U.S. and age at the time of DACA's enactment. We use these criteria to determine individual treatment status in our empirical analysis: (i) under 31 by June 15, 2012, (ii) entered the U.S. before age 16, (iii) continuous residence in the U.S since June 15, 2007, and present at the time of application. Applicants must also be at least 15 years old, though we do not use this restriction in order to assess young teenagers who may age into eligibility.[10] The remaining two requirements screen out applicants convicted of a serious crime[11] and select highly educated individuals. In particular, applicants must currently be in school, have graduated high school or obtained a general education development (GED) certificate.[12] The application also requires a $465 fee.

U.S. Citizenship and Immigration Services (USCIS) began accepting applications for DACA on August 15, 2012, which was met by an immediate surge in applications. Figure 1 displays total initial applications and initial approvals by quarter from implementation through 2016. USCIS received nearly 150,000 applications in the fourth quarter of 2012, and 525,000 applications within 1 year – roughly 30% of the estimated eligible population of 1.7 million (Passel and Lopez, 2012). The rate of applications slowed beginning in 2013; USCIS received a total of 901,000 applications by the end of 2016. On September 5, 2017, President Trump ordered an end to DACA, leading to an immediate halt in the acceptance of new applications and renewals.[13]

The geographic distribution of DACA applications reflects the concentration of undocumented populations in a handful of states. Appendix Figure A.1 displays cumulative initial DACA applications through 2016 by state. California alone accounts for over 237,000 DACA

[8] An interview with a DACA recipient in 2015 revealed that she "wasn't concerned that DACA is only ... temporary ... since recipients can ... renew every two years" (Nevarez, 2015).

[9] In November 2014, President Obama announced an expanded DACA program which would make individuals residing in the United States prior to January 1, 2010 eligible, but that version of the program was never implemented due to legal challenges. See https://obamawhitehouse.archives.gov/the-press-office/2014/11/20/remarks-president-address-nation-immigration.

[10] Note that individuals under 16 facing deportation proceedings may be exempt from the minimum age requirement at the time of application.

[11] Specifically, applicants may not have been convicted of a felony, significant misdemeanor, or three or more other misdemeanors, or pose a threat to public security.

[12] Applicants may substitute veteran status for this requirement. See https://www.uscis.gov/humanitarian/consideration-deferred-action-childhood-arrivals-daca#guidelines for a full description of DACA eligibility criteria.

[13] To assist with the transition, renewals continued to be accepted until Oct 5, 2017 for individuals whose benefits would expire by March 2018.

applications, with Texas in second at 138,000. Illinois, New York, and Florida each account for roughly 40,000 applicants. These five states constitute 52% of the total number of applications.

The majority of applicants are from Latin America, with 600,000 applications from Mexico alone. Behind Mexico, El Salvador, Guatemala and Honduras are the next-highest applicant countries. Outside of Latin America, the largest sources of applicants are in Asia (South Korea and the Philippines) and the Caribbean (Jamaica and the Dominican Republic), although each of these countries contributed less than 5,000 total applications.[14]

3 DACA Education and Fertility Incentives

Typical models of human capital decisions focus on wages as the only relevant benefit of schooling. However, increasingly scholars have recognized broader benefits of schooling, which may also be incorporated in the schooling consideration of individuals (Oreopoulos and Salvanes, 2011). We consider the incentives of DACA for adolescent decisions in light of this more holistic view of the returns to schooling.

With this lens, we consider two channels by which DACA may raise the payoff to schooling and influence education choices. First, work authorization, which allows undocumented immigrants to have legal standing in the labor market, provides access to higher paying occupations in the formal sector and may also raise wages in the informal sector. Prior research suggests that legal immigrants earn a higher wage at every level of education - a legal status premium - and also obtain greater returns to education relative to undocumented immigrants. Estimates vary, but legal immigrants may experience as much as a 1.8% higher wage for each additional year of education (Borjas, 2017).[15] Appendix Figure A.2 illustrates this shift in the earnings-education relationship: the level increase in wages that occurs at high school completion with DACA, and a steeper gradient in wages with post-secondary schooling.

Work authorization thus raises the payoff associated with high school completion, but also raises the opportunity cost of post-secondary education. This generates an unambiguous prediction that individuals increase high school completion, but has less clear implications for post-secondary schooling. Whether college enrollment increases will depend on multiple factors: liquidity constraints, the wedge between high school and college earnings, and

[14]Qualitative evidence suggests that DACA application rates among Asians were low due to significant stigma associated with undocumented status, distrust towards authorities and the uncertain nature of the program, and lack of information about DACA through ethnic media (Singer, Svajlenka and Wilson, 2015).

[15]We have not encountered the wage difference for high school completion, but Rivera-Batiz (1999) finds a 5% increase in wages associated with completion of 10 years of schooling.

psychic costs associated with schooling.

Second, the guarantee of deferral from deportation provides an additional benefit to completing high school. Anecdotal evidence suggests that undocumented immigrants often strongly identify as Americans and have a preference for remaining in the U.S (see, e.g. Vargas (2012)). This desire could then generate an education response to qualify for DACA. Even so, it is not clear how largely this factor would weigh in education decisions, as current statistics do not report removals separately for child arrivals, and so the actual risk of deportation is unknown. Moreover, it is possible that the risk of removal is already lower for high school graduates, such that DACA may not create a significant additional incentive for schooling.

We gauge the potential magnitude of this benefit by calculating the risk of deportation for young individuals using tabulations of removals by age and gender in 2012 from the Department of Homeland Security (Simanski and Sapp, 2012).[16] Due to data limitations, we assume that the deportation risk only varies along these two dimensions and that DACA recipients forecast their risk based on the population ages 18-39. On average, the deportation risk is 5%.[17] There is significant variation in the risk across gender, however, as men account for almost 90% of all deportations. This implies that the deportation risk is closer to 1.5% for women and 7.3% for men, taking differences in the size of the respective populations into account.[18] We should thus expect adolescent boys to exhibit a larger education response to DACA, if the reduction in deportation risk is similarly valued across genders.

Incentives for increased schooling may in turn reduce teenage fertility by raising the cost of having a child (Black, Devereux and Salvanes, 2008). First, while in school, young women have reduced opportunity or desire for engaging in risky behavior, including sexual activity, an "incarceration" effect. Second, schooling under DACA increases the current human capital and wage trajectory of young women, which raises the opportunity cost of a teenage birth. Recent studies suggest that this may be the most important factor for reducing teen pregnancy (Kearney and Levine, 2014, 2012). The accumulation of human capital may also include gaining information about how to practice "safe sex." Third, there may be reduced demand for childbearing due to expectations about future schooling choices.

To summarize, we make the following three testable predictions regarding the impact of DACA and the relevant channels for undocumented youth:

[16] Tabulations from 2011 would be ideal, but only more aggregate statistics were available for that year.

[17] Calculated as 341,448 removals divided by an estimated population of 6.6 million (56%) of 11.9 million undocumented immigrants.

[18] Calculated as the rate of 18 to 39 removals (81.4%) times the share of male (female) deportations, 89.3% (10.7%), times 419,384 alien removals - a total of 304,851 (36,527) deportations - divided by an estimated population of 4.1M (2.5M), 35% (21%) of 11.9 million unauthorized immigrants (Passel and Cohn, 2009; Passel, 2005).

Predictions:

1. The rate of high school completion increases, with a larger effect for boys if the education response is proportional to the reduction in deportation risk.
2. The rate of post-secondary education increases, if individuals respond to higher post-secondary returns.
3. The fertility rate for teenage girls declines.

4 Data

We use data from the IPUMS ACS (Ruggles et al., 2015) for the period 2005 through 2015 to examine the education and fertility of eligible and ineligible individuals. The ACS is a yearly survey that collects demographic educational, and, for ages 16 and up, employment information for a 1 percent representative sample of the U.S. population. Included among these variables are year of immigration and citizenship status, which we use together with year of birth to determine eligibility for DACA, as discussed in Section 5.[19] Since age is not reported in the survey, we assign current age as the difference between survey year and year of birth.

Importantly, the ACS collects information on all households living in the U.S., irrespective of their citizenship or legal status. Pope (2016) details that the sampling procedure for the ACS draws from the universe of addresses, and is therefore likely to be representative of the unauthorized immigrant population. As discussed in Liscow and Woolston (2016), the Census Bureau takes several steps to encourage responses to the ACS. The Bureau is not permitted to share personal information with other government agencies, and communicates this confidentiality policy in the survey. It also performs outreach to Hispanic organizations, and makes the survey available in Spanish.

To further enrich our analysis we utilize alternative data sources that allow deeper insight into changes in education and fertility. As California has the largest undocumented population among states, we obtain administrative data on student achievement from the California Department of Education (CA DOE), which contain aggregate results from the California High School Exit Examination (CAHSEE). The data include average test scores, the number of test takers, and the number of students passing the exam by test subject, county and race/ethnicity. To hone in on DACA treatment effects, we focus on Hispanic

[19] Year of immigration comes from the response to the question, "When did this person come to live in the United States?" Redstone and Massey (2004) show that the ambiguity in the wording of this question leads to various interpretations in reporting, which may cause us to misassign treatment in some cases. We assume that this mis-interpretation is not discontinuous after 2012.

students and utilize data from the full span of the CAHSEE, which began in 2006 and was retired in 2016.[20]

We also examine teenage sexual behavior using the Youth Risk Behavior Surveillance System (YRBSS) data. The YRBSS is fielded biennially by the Centers for Disease Control and Prevention for a nationally representative sample, and asks high school students about sexual behaviors, alcohol and other drug use, tobacco use, unhealthy dietary behaviors, and inadequate physical activity. The national sample is not representative of particular states, however; therefore, we obtained data for 22 states that separately administered the survey from 2005 to 2015.[21]

5 Empirical Strategy

Ideally, we would like to be able to randomly assign eligibility for DACA within undocumented youth, and then track a rich set of subsequent outcomes after the enactment of DACA. Of course, this is infeasible. But, in this spirit, we identify the effect of DACA in the ACS by comparing the education and fertility outcomes of DACA-eligible youth to a control group of non-eligible immigrants using a difference-in-differences strategy. We first sketch the outline of this approach and later discuss several refinements required for identification in practice. Our difference-in-difference framework is implemented with the estimating equation,

$$Y_{igst} = \alpha_0 + \alpha_1 Eligible_g + \alpha_2 (Eligible_g * Post_t) + X_{ig} + \gamma_{st} + \phi_{ag} + \epsilon_{igst} \quad (1)$$

where $Eligible_g$ is an indicator for whether an individual is among the eligible group (indexed by g) and $Post_t$ is an indicator that equals 1 beginning in the year 2012. We include a vector of individual controls (dummies for gender, age, race/ethnicity, and citizenship), X_{ig}, state-by-year fixed effects γ_{st}, and age-of-arrival-by-eligibility fixed effects, ϕ_{ag}, to account for potential differences in the composition of the eligible and control group populations that may influence schooling decisions. All regressions are weighted using sampling weights and standard errors are clustered at the state level.

To account for differential linear pre-trends in the outcome by eligibility status, we model and adjust for such trends prior to estimating Equation 1. This first step fits a linear time

[20] Publicly available educational records from other states with large undocumented populations, such as New York, Florida and Texas, do not provide separate information for Hispanics, and are thus not feasible for studying DACA.

[21] These states include Alaska, Alabama, Arkansas, Arizona, Connecticut, Delaware, Florida, Kentucky, Maryland, Maine, Michigan, Montana, North Carolina, North Dakota, New Hampshire, New York, Oklahoma, Rhode Island, South Carolina, Tennessee, West Virginia, Wyoming.

trend for eligible and non-eligible for each outcome and control using only the pre-period (2005-2011), and then estimates residuals for the full sample period. Equation 1 is then estimated on the de-trended data, adjusting standard errors to account for the estimated parameters in the first step. We favor this approach, rather than including a linear trend in Equation 1, because in the presence of dynamic treatment effects, this two-step approach performs better (Wolfers, 2006; Lee and Solon, 2011; Borusyak and Jaravel, 2016; Goodman-Bacon, 2016), and because the identification of the pre-trend is more transparent. Our qualitative results are unchanged by including these trends, as discussed in Section 8.2 (see Appendix Table A.4 and Appendix Figure A.7).

The interaction between *Eligible* and *Post*, captured by α_2, provides the average effect of DACA after 2012. If individuals are unable to adjust education decisions immediately, this estimate will provide an attenuated estimate of the policy effect. Therefore, our preferred specification replaces $Post_t$ with indicators for each year to estimate dynamic treatment effects. This event study approach additionally allows us to visually observe any differences between the eligible and ineligible groups before and after the policy went into effect, which provides a strong test of the identification strategy.

We conduct our analyses on various subgroups of youth, reflecting the distinct ages at which different decisions are taken. We perform the analysis of current school attendance on children aged 14 to 22, high school completion and college enrollment on young adults aged 19 to 22, fertility choices on teenagers aged 15 to 18, and labor market decisions on teenagers aged 16 to 22.

5.1 Determining Eligibility

Our analysis tracks the behavior of likely-DACA-eligible youth. The eligible group we focus on consists of foreign-born non-citizens that arrived by age 10 and by year 2007. In what follows we motivate how this focus helps us obtain identification.[22]

5.1.1 Undocumented Status

A primary task to assign DACA eligibility is to identify undocumented youth. Since information on legal status is not available in the ACS or other surveys, we identify likely undocumented youth using the absence of citizenship. Eligibility is thus measured with noise, as non-citizens also include green card holders and temporary visa holders[23]. This

[22]Note that our focus on youth always satisfies the eligibility criteria that individuals must be under 31.

[23]Since visa holders generally migrate to the U.S. as adults, we are unlikely to confuse visa holders as DACA eligible.

causes our estimated effects of DACA eligibility to be a "scaled-down" estimate of the true intent to treat (ITT) effect.

To get closer to the true ITT effect, we separately analyze groups that are likely to have a higher share of undocumented individuals among those that we assign eligibility. First, we restrict to Hispanics, who have the highest DACA take-up rates and comprise a large proportion of undocumented immigrants. Second, we classify individuals as "high take-up" if they were born in countries that have a DACA take-up rate above 30%.[24] While there is substantial overlap between our Hispanic and high take-up samples, these two groups are not identical. Among foreign born Hispanics ages 14 to 22, 86% of respondents come from high take-up countries, and among individuals born in high take-up countries, 93% are Hispanic.

5.1.2 Age of Arrival

In addition to undocumented status, DACA also required individuals to have arrived in the U.S. by age 16 and by year 2007. Analyzing youth subject to these constraints requires a further refinement – we restrict our analysis to foreign-born individuals that arrived in the U.S. at age 10 and prior. This restriction helps correct for a mechanical compositional shift, whereby moving forward in survey time causes the age of arrival distribution among eligible youth to shift towards younger ages.

To fix ideas, consider the sample of eligible 18 year olds in the ACS. In 2011, everyone in this group would have immigrated by age 14 (in order to have arrived by 2007), while by 2015, everyone in this group would have immigrated by age 10. As age of arrival has been shown to be an important factor in educational and fertility decisions (Bleakley and Chin, 2010), failure to account for this compositional issue may confound treatment effects.

This identification restriction implies that our baseline estimates omit any effect on undocumented teens that immigrated at older ages, who account for roughly 40% of 14 to 18 year old non-citizens. This may be problematic for drawing policy inferences if the incentives of DACA differentially impacted older immigrants. In sensitivity analyses, we show that the results are qualitatively similar, with slightly larger treatment effects, when we include individuals that arrived between the ages of 11 and 16.

[24] These countries are El Salvador, Mexico, Uruguay, Honduras, Bolivia, Brazil, Peru, Ecuador, Jamaica, Guatemala, Venezuela, Dominican Republic, and Colombia. Statistics are based on the Migration Policy Institute's (MPI) estimates of the DACA-eligible population and application rates by country, available at http://www.migrationpolicy.org/programs/data-hub/deferred-action-childhood-arrivals-daca-profiles (accessed 8/16/2017).

5.2 Comparison Group

We select the comparison group based on two criteria. First, we restrict the sample to individuals born outside of the contiguous U.S., including Hawaii and Alaska, to avoid the strong cultural, institutional, and structural divisions between natives and immigrants (LaLonde and Topel, 1992; Borjas, 1985, 2017). Second, we limit the comparison group to individuals that arrived before age 10 and before 2007, matching the composition of eligible individuals. Our control group is thus composed of immigrants that arrived by 2007 and by age 10, and are ineligible for DACA due to current legal status (proxied by citizenship).

To give a sense of the demographic make-up of this group, Appendix Table A.1 provides descriptive statistics of the Hispanic treatment and control groups at baseline (from 2005 to 2011). Roughly 24% of the control group were born in U.S. territories, primarily Puerto Rico, 19% were born abroad to American parents, and 57% gained citizenship through naturalization. Relative to the treatment group, high school aged youths in the control group are more likely to have health insurance coverage, English fluency, and parental college, and are less likely to be in poverty. However, the control group is more likely to have a single mother and similarly likely to have a recent birth.

Importantly for our identification strategy, these characteristics do not predict changes in schooling after DACA, as we discuss in the next section. Moreover, in Section 8.1, we show that balancing these characteristics using propensity score methods does not alter our results.

It is worth noting that in addition to matching the immigration patterns of the eligible group, this comparison group matches the schooling trends of eligible youth better than other feasible immigrant comparison groups. Appendix Figure A.3 shows that other ineligible immigrants, namely non-citizens not meeting the age and/or year of arrival criteria, each exhibit differential pre-trends when compared to eligible individuals. This stands in stark contrast to our preferred control group, which exhibits no differential trends in outcomes prior to DACA, as will be shown in the following section.

5.3 Descriptive Evidence and Validity of Identification

The key identification assumptions for the difference-in-difference model are that (i) the control group should be a reasonable counterfactual for the treatment group (no differential pre-trends), and (ii) there can be no confounding shocks with treatment.

We provide two pieces of evidence in favor of these assumptions. First, while the control and treated groups differ in the levels of a few observable characteristics at baseline, this relationship is stable over time and does not predict an increase in schooling after 2012. We

demonstrate this by regressing outcomes (e.g. school attendance and high school completion) on a large number of demographic characteristics for the 2005-2011 pre-period.[25] We then generate fitted values for the whole period.

Panels (a) and (b) of Figure 2 present the coefficients from event study regressions of Equation 1 where the outcomes are these fitted values. They show that based on observables eligible individuals are not expected to change the likelihood of being in school or of completing high school after 2012. This suggests that our estimated effects are not due to a change in the *composition* of the sample, but rather a change in *behavior* among non-citizens relative to citizens.

As further evidence of the parallel trends in schooling across these groups, Figure 3 plots average school attendance of our eligible and control groups among Hispanic teens. We include a vertical line demarcating the implementation of DACA. It clearly shows that the educational trajectories of these two groups tracked each other closely from 2005 to 2011, with a constant gap of roughly 4 percentage points over this period. Strikingly, after 2012, the difference narrows by half, as attendance of the eligible group increases by over 2 percentage points. These patterns provide support for common trends as well as suggestive evidence of a DACA treatment effect on education decisions. Moreover, our event study analyses in the next section show that these patterns are unchanged by the introduction of control variables.

6 Results

6.1 School Attendance

We first test the hypothesis that DACA implementation led to increased school attendance and high school completion. Figure 4 presents the event studies for school attendance of adolescents ages 14 to 18. The estimates for the whole sample, shown in Panel (a), do not indicate a pre-existing trend between our eligible and control groups: the difference prior to 2012 is small, statistically insignificant, and generally constant. After DACA's enactment in 2012, however, the eligible youth experience an immediate and persistent 2.5 p.p. increase in school attendance. Panels (b) and (c) show that when we analyze Hispanics and the high

[25] We divide these characteristics into two subsets. The first includes indicators for age, race, gender, age and year of immigration, citizenship status, birthplace, language, state, metropolitan status. The second includes health insurance coverage, presence of mother and father in the household, parental college attendance, family size, number of siblings, household poverty status, and the presence of a food stamp recipient in the household. While we use both sets of observables in our prediction to be as comprehensive as possible, we acknowledge that the second set could also be considered outcomes affected by DACA. The results are similar, and more precise, if we only use the first set of variables.

take-up sample, whose eligible individuals are more likely to resemble undocumented youth, we find a similar pattern of results with slightly larger increases in school attendance after DACA was implemented.

The difference-in-difference results appear in Panel (a) of Table 1. Confirming the event study estimates, we find that DACA led to statistically significant increases in school attendance, with a 2.6, a 3.3 and a 4.1 p.p. increase in school attendance among all, Hispanics and the high take-up sample, respectively, equivalent to between 2.8 and 4.6 percent effects relative to the means. The estimates indicate that populations most likely to apply for DACA saw large increases in school attendance, commensurate with the educational requirements of the program.

Panel (b) shows the effect of DACA among college-aged individuals, ages 19 to 22, who could have been affected by DACA through multiple channels. Similar to younger individuals, legalization could have increased their returns to schooling and incentivized them to go back to school in order to be eligible for DACA. On the other hand, DACA provides work authorization, which could incentivize young adults to drop out of school - for this group, likely post-secondary education - in order to work. Our results show that DACA led to increased school attendance for this group, with effects that are larger in size to those among teenagers.

6.2 High School Completion and College Attendance

We next examine whether increases in school-going resulted in a higher rate of high school completion and college attendance. We first focus on high school completion, defined by either earning a high school diploma or GED.[26] Our preferred specification focuses on 19 year olds because individuals age 20 or above may have already made the decision to drop out by the time DACA was enacted, and would arguably be less likely to return to complete high school compared with individuals that had not yet dropped out.

These results, presented in Figure 5 and Panel (a) of Table 2, show that DACA increased high school completion by 3.8 p.p. overall, with Hispanics and the high take-up sample experiencing an 11 p.p. increase in high school graduation. The event study indicates that the effects were immediate after DACA's enactment and fairly stable over the post period, though the confidence intervals are wide, allowing for an increase in the effects over time.[27]

[26] We would like to be able to separately estimate the effect on diploma and GED, but we only have information on the type of high school degree for a selected sample of individuals that have completed no more than high school.

[27] In Appendix B, we look into the plausibility of an effect on high school graduation following either DACA's announcement in June or enactment in August, by examining the incidence of obtaining a diploma between July and December. Using the National Longitudinal Study of Youth 1997 (NLSY97), we show

This represents a sizable increase in the likelihood of completing high school, both in absolute terms and relative to other interventions, particularly given the low 75% completion among eligible individuals.

Panel (b) of Table 2 shows that when we expand our sample to slightly older individuals, 19 to 22, to allow more time for high school completion, we find a smaller 5.9 p.p. impact among Hispanics and similar effects for the high take-up sample. This attenuation is reasonable given that this sample includes individuals that left high school prior to DACA (e.g. age 22 in 2012). Effects among 23 to 30 year olds, who are likely to have work or family commitments that would pose a barrier returning to school, are even more muted. We find a marginally significant 1.9 p.p. effect among Hispanics, and a statistically insignificant effect among the high take-up sample.

To put our findings into perspective, multiplying the 830,700 eligible Hispanics age 19-22 represented in the ACS by our estimated 5.9 p.p. increase in high school graduation implies that DACA led to more than 49,000 additional high school graduates. As a result, the 15 percentage point gap in high school completion between Hispanic undocumented youth and their citizen immigrant peers narrowed by 40%. The effects are consistent with recent evidence that high school interventions have the potential to alter the educational choices of youth (Carrell and Sacerdote, 2017).

We now examine impacts on college-going, which we define as including any post-secondary schooling, recalling that the theoretical effects on this margin of schooling are ambiguous. The last three columns of Table 2 and Appendix Figure A.5 show that a moderate share of young adults took up more college, despite the fact that this was not required for DACA eligibility. The effects are once again largest for 19 year olds, the most recent graduates of high school, indicative of a role for momentum in the college decision. College attendance of Hispanics in this group rose by 7.6 p.p., advancing their post-secondary attendance by 22 percent. Among 19 to 22 year old Hispanics, college-going increases 4.1 p.p, 10 percent of the mean. These effects accord with the results from surveys of DACA recipients, in which 43% of respondents report attending some post-secondary schooling, largely pursuing a bachelor's degree or advanced degree (Wong et al., 2016).[28]

By comparison, prior studies find zero or negative effects of DACA on college attendance (Pope, 2016; Amuedo-Dorantes and Antman, 2017; Hsin and Ortega, 2017). However, the estimates in these earlier studies condition on high school attainment, and therefore offer an incomplete picture of the effect on post-secondary schooling. For instance, Pope (2016)

that one quarter of students that complete high school in 5 years obtain a diploma in these months. Further, this could be a lower bound on the scope for completing in the first semester, if those who do not return to complete a degree are only deficient one semester of work.

[28] 83 percent of those attending school report working while in school, consistent with our results in 6.4.

measures no effect on college, but shows evidence of differential pre-trends in schooling in his sample of high school graduates, and therefore places less emphasis on these results.[29] While Hsin and Ortega (2017) find increases in dropping out of four-year colleges, they do not study entry into college. Instead, by not conditioning on high school completion our strategy captures the total effect of DACA on college attendance - on increased college-eligibility from high school graduation, enrollment conditional on high school graduation, and dropping out. Importantly, the event studies show no evidence of differential pre-trends, which we believe makes our estimates more representative of the full policy impact.

6.2.1 Differential Effects by Gender

The effects for high school and college attendance are quite distinct when we stratify the sample by gender. Table 3, which focuses on the most responsive subgroup, 19 year olds, shows that the effects for high school completion are almost four times as large for young men as for young women. In contrast, we find that women respond much more on the college attendance margin (up to 10 p.p.), and are unable to reject that men do not increase college attendance at all.[30]

There are several possible explanations for these gender patterns. As discussed in Section 3, men may experience a larger incentive to graduate due to the higher risk of deportation. Men also have a lower average rate of high school completion (72% versus 77% for women), which makes them less subject to a "ceiling effect," although this explanation seems unlikely. But then, why don't boys continue to college? Prior evidence suggests that men may experience lower wage returns to completing some college and perceive college to be less important for advancing one's career (Carrell and Sacerdote, 2017). This fact may be particularly relevant for undocumented men whom are often employed in occupations requiring manual-intensive labor.

6.2.2 The Role of Liquidity Constraints

An important factor that drives college enrollment are costs, and although DACA did not directly alter tuition fees or access to federal aid, our effects on college enrollment could differ depending on the affordability of college. To explore this, we allow effects to differ depending on whether the individual lives in a state that grants undocumented students eligibility for in-state tuition rates. We revise our main equation to include an indicator for

[29] Pope (2016) also analyzes a broader range of ages in the main analysis, 18 to 35, which we would expect to generate smaller and less precise estimates given that the largest effects on college were among the youngest of this group.

[30] These patterns by gender are very similar when we examine 19 to 22 year olds.

the presence of in-state tuition for undocumented students in the state of residence together with the interaction of the indicator with *Eligible* and *Post*.[31] Appendix Table A.2 shows that the effects of DACA tend to be larger in states that offered in-state tuition for the undocumented. Intuitively, the college response to DACA is more muted when college is less affordable.

6.3 Fertility

We next examine effects on teenage (ages 15 to 18) childbearing. Teenage motherhood among this population, and Hispanics in particular, has persistently been above that of other groups, and thus are of strong interest for policy (Kearney and Levine, 2012). Since there is a nine-month lag between changes in fertility behavior and observed childbearing, we redefine the "post" period to begin in 2013.

Table 4 provides estimates for all, Hispanics and the high take-up sample, and we include the corresponding event studies for Hispanics in Figure 6.[32] The results show that DACA led to a large decline in the likelihood of being teenage mother. We find a 1.7 p.p. decline in Hispanic females' likelihood of having a child in the previous year, a 45 percent reduction relative to the mean. The reduction in fertility appears to be concentrated among teens on the margin of a first birth, as we find a similar-sized 1.9 p.p. increase in the likelihood of having no children. This translates into a significant decrease in the number of children among teenage girls.

We gain information about the role of the "incarceration" effect of high school in Appendix Table A.3, which investigates whether fertility declines extended to women ages 19 to 30. This population was likely to have already completed high school, and therefore would have experienced similar employment incentives from DACA, but are not subject to the constraints of attending high school. We find no decline in childbearing among women ages 19 to 30, and find some *increases* in the number of children among women ages 23 to 30, which we suspect could be due to DACA-induced improvements in income.

The fact that we only find reductions in fertility among teenage girls is potentially explained by two possibilities. First, the decline in childbearing may be reflective of an intertemporal substitution in order to complete high school. The insignificant effect on the fertility of women ages 19 to 22 provides weak evidence against short-term substitution,

[31] We collect information on states that passed laws allowing undocumented students to pay in-state college tuition fees from Mendoza and Shaikh (2015). As of 2015, twenty states offered in-state tuition to unauthorized immigrants, including four states where the state University system offered in-state tuition (Hawaii, Michigan, Oklahoma and Rhode Island).

[32] The event studies for the high take-up sample look very similar and therefore for brevity are included in the Appendix, Figure A.6.

though the estimates are not precise enough to entirely rule this out. Second, DACA may have had a particularly strong effect on young women, which could have generated a more permanent shift in expectations. We would expect this to cause a decrease in fertility also at later ages if these expectations were maintained, although this seems doubtful given the halting of the program. Nonetheless, until more time has passed, this hypothesis can not be verified.

6.4 Idleness and Working

Given that DACA increased schooling among teenagers and young adults, we now assess how greater schooling interacts with time spent working and "idleness," not working and not attending school. In Table 5 we analyze detailed work and schooling choices among individuals ages 16 to 18 and 19 to 22 for the Hispanic and high take-up samples.

We find several striking results. First, we find a large decline in idleness for every subgroup. These effects were largest among high school ages, where DACA reduced the likelihood of being idle by 45 to 60 percent, but also substantial for college ages, with a 17 to 29 percent effect. Second, we find that these young adults instead attended school and worked more, often at the same time. This upends the notion that work and school must necessarily be substitutes when opportunity costs increase. In our setting, labor and human capital development are not "either/or", but "and." In response to DACA, individuals respond *both* to increasing work incentives and greater returns to schooling.

7 Additional Evidence using Geographic Variation

In this section we aim to further understand the reasons behind the estimated schooling and fertility adjustments. For example, did DACA also cause individuals to increase effort in school? Were declines in fertility simply due to a mechanical substitution of time towards schooling and away from sexual activity, or do they reflect changes in contraceptive behavior by young adults? We proceed by using datasets from the California Department of Education and the YRBSS to help shed light on these issues.

Since these data do not contain information on legal status (or citizenship), year of arrival, or age of arrival, we implement a variation on the previous empirical strategy which exploits geographic variation in the concentration of eligible youth. In particular, we analyze the change in outcomes of Hispanic youth in geographic areas that have high share of eligible Hispanics compared with outcomes in areas with a low share of eligible Hispanics. Specifically, we assign a binary indicator for "high-eligibility" to geographic areas where the

average share of eligible individuals among the Hispanic population ages 14 to 18 from 2005-2011 was above the median of the sample.[33] The geographic unit is the county of school attendance for the California analysis, and the state of residence for the YRBSS analysis.[34] The California analysis is restricted to the 34 counties that are identified in the ACS.

We use the estimating equation:

$$Y_{ict} = \alpha + \beta HiShareElig_c \times Post_t + \gamma_c + \gamma_t + \epsilon_{ct} \tag{2}$$

where $HiShareElig_c$ is an indicator for having above-median share eligible among Hispanics ages 14-18, roughly the schooling population of interest, in geographic area (county or state) c between 2005 and 2011. As before, we replace $Post_t$ with year indicators to estimate treatment effects over time. CAHSEE data provides student outcomes as county-aggregates, and so Y_{ct}, the share of the Hispanic population taking and passing the CAHSEE, replaces Y_{ict} in that analysis.[35] We use the same two-step process for absorbing linear trends.

We first validate this empirical approach by replicating our main results for school attendance using county-level variation in eligibility in California. Appendix Figure C.1 shows a positive and significant impact on school attendance among teenagers. This raises our confidence that this geographic variation can be used to examine the effect of DACA on other outcomes.

7.1 School Performance: Examining California

First, we analyze whether students put forth greater effort in response to incentives. If schools practice social promotion, high school enrollment could have translated into graduation with minimal student effort. We probe this possibility by analyzing student performance on a high-stakes state-level test required for graduation.

California provides a useful environment for studying this question because it has the largest DACA-eligible population and administered a mandatory examination for graduation, the CAHSEE, between 2006 and 2016. The CAHSEE exam consists of two parts, a mathematics test and an english language arts (ELA) test, and passing both is required to graduate. Students are able to take the CAHSEE (one or both sections) multiple times, if necessary. All students take the exam for the first time in 10^{th} grade, and those that do not

[33] Utilizing a binary indicator mitigates measurement error in the county- (or state-) level share Hispanic in an area.

[34] States with a high share of eligible Hispanics include: Alabama, Arizona, Arkansas, Delaware, Florida, Kentucky, Maryland, North Carolina, Oklahoma, Rhode Island, South Carolina, and Tennessee.

[35] Shares are defined using the average population of Hispanics ages 14-18 between 2005 and 2011 as the denominator, matching the relevant high school enrollment population.

pass take the exam again in 11^{th} and/or 12^{th} grades.

Table 6 presents the effects on CAHSEE test performance, which are corroborated by event studies in Appendix Figure C.4. The results show significant increases in the share of Hispanic students taking and passing the exam. Counties with a high share of eligible Hispanics saw a 0.8 p.p. increase in the share of 10^{th} grade test takers in both the Math and ELA exams, consistent with increased enrollment after DACA. Moreover, we also find increased passing rates – after DACA, the "treated" counties saw an increase in the share of Hispanics passing the test on both exams by 0.6 to 0.8 p.p. on a baseline pass rate of roughly 13%.

While the share of students passing improves, the effects on average test score performance are mixed. Among 10^{th} graders, DACA does not lead to significant changes in math scores but it leads to small decreases in ELA scores. This may suggest that marginal undocumented students – those induced to stay in school due to DACA – are on average less prepared for the exam and lower-scoring. Interestingly, average test scores rise for repeat test-takers in 12^{th} grade, who are are approaching their final opportunity to graduate high school. The positive results suggest that they increase effort to pass the exam.

7.2 Exploring Reasons for Changes in Fertility with YRBSS

Table 7 provides results of the effect of DACA on the sexual behavior of Hispanic high school students ages 14 to 18. DACA led to a 4.8 percentage point decline in unprotected sex among Hispanic high school students, a reduction of over 20%. Among specific pregnancy prevention methods asked about in the survey, students report an increase in use of condoms, decrease in IUD's/shots, and an increase in withdrawal. At the same time, we find no change in the likelihood of having sex. These results suggest that DACA led to a reduction in teenage pregnancy through a greater attention towards practicing safe sex.

8 Robustness

8.1 Inverse Propensity Score Reweighting

Although we have shown evidence of the validity of our identification strategy, there may remain concerns that differences in background characteristics still bias our results. As a result, we also present propensity score estimates as an additional method of controlling for omitted variable bias. We predict the likelihood of being eligible for each subsample and age group using a probit regression with the demographic characteristics from Equation 1, household poverty, and dummies for whether individual primarily speaks English, primarily

speaks Spanish, is fluent in English, and lives in a metropolitan area. For regressions of schooling attendance between ages 14 to 18 and teen fertility, we also include additional controls for family composition.[36] We then re-estimate our regressions of schooling attendance and high school completion using inverse-propensity score weighting. Summary tables in Appendix Section D show that summary statistics using this weighting produces balanced characteristics across eligible and ineligible individuals. Moreover, the regression estimates are generally the same as when we do not use this reweighting, though the standard errors are larger for fertility outcomes, indicating that the estimates are on the whole robust to this method of bias correction.

8.2 Accounting for Time Trends

While our preferred method to account for differential time trends across eligibility groups is the two-step detrending procedure, in this section we analyze whether our baseline ACS results are sensitive to either not allowing for differential trends by eligibility status or including linear time trends in the main specification. Appendix Table A.4 shows the sensitivity of our main difference-in-differences estimates to these different specifications, and Appendix Figure A.7 shows a comparison of event study coefficients estimated with and without trends. Our results show that while the magnitude of the effects can vary across specifications, our main findings that DACA led to improvements in schooling and fertility are not sensitive to whether we account for trends. This is due to the fact that there is no strong pre-trend in our main outcomes of interest, net of the control variables. We conclude that adopting the two-step detrending procedure is the best way to adjust for differential pre-trends in our setting.

8.3 Alternative Sample Restrictions

Our baseline empirical analyses restrict the sample to foreign born individuals that migrated to the U.S. by 2007 and by age 10 to avoid changes in sample composition over time. We now test how sensitive are main findings are to this and other sample restrictions.

The first column of Table 8 presents our baseline results, for school attendance, high school completion and fertility, in Panels (a) to (c), respectively. In columns (2) and (3) we restrict the sample to those that arrived in the U.S. by age 6 and by age 16, respectively, and show that our baseline results are not sensitive to the age of arrival restriction. The fourth to sixth columns of Table 8 contain results when we consecutively add other individuals

[36] These include dummies for family size, number of siblings, whether mom is present, and whether dad is present.

that are not eligible for DACA to the control group. We start by first adding foreign born individuals, including citizens and non-citizens, that arrived in the U.S. after turning 16 in column (4), then we add individuals that arrived after 2007 in column (5). Again, the results are not sensitive to the inclusion of these individuals. While in all our analyses we restrict the sample to only foreign born individuals to avoid comparisons with natives, in the sixth column we relax this restriction and include also individuals that were born in the U.S. While the magnitudes are smaller, the pattern of the effects are similar to the baseline estimates.

Finally, to further ensure that our results are not affected by the behavior of legal immigrants, we refine our baseline definition of DACA eligibility to remove non-citizens that are more likely to have legal status. Hence, following Liscow and Woolston (2016), we restrict eligibility to non-citizens who do not live in households with veterans or with positive Social Security or welfare receipt. Reassuringly, the estimated effects from this refined analysis, shown in the last column of Table 8, are similar in magnitude to our baseline effects.

8.4 Survey Response and Population Changes

Since our analysis relies on survey data, one could be concerned that DACA might lead to changes in survey responses that could drive the estimated effects. For example, legalization and work authorization could decrease the likelihood that undocumented youth return to their country of origin, or change their willingness and ability to participate in the survey. If these changes in survey participation are correlated with educational outcomes, the measured effect of DACA on schooling may be biased by changes in sample composition. We address this concern with the event study of predicted schooling in Figure 2, which we described earlier. The event studies indeed show that changes in observables across eligible and non-eligible groups cannot explain our findings.

9 Conclusion

In this paper, we quantify the education and fertility response of undocumented youth to a large shock in the returns to education. We obtain variation in the returns to education from the enactment of DACA, which provided temporary deferral from deportation and work authorization this population. Using a difference-in-difference design, we show that DACA altered the education, work, and fertility behaviors of undocumented youth.

We find that this policy increased high school graduation rates by 15 percent and reduced teenage births by 45 percent. Further, we estimate that DACA led to a 22 percent higher

rate of college enrollment among young Hispanic women. Working also increased alongside schooling, counter to the typical modeling assumption that assumes these pursuits are mutually exclusive. This resulted in a decline in idleness, and generated a significant boost in productivity. Auxiliary analyses show that undocumented youth also exhibited mastery of required subjects, as measured by an increased share passing a high-stakes exit exam, and increased their utilization of pregnancy prevention methods.

These results have significant bearing for the direction of future immigration policy. In the wake of the recent termination of DACA, the fate of over 1.7 million unauthorized youth is precarious. In part, the controversy over this policy stems over fears that that undocumented immigrants may bring undesirable attributes to communities – for example, low levels of education and high levels of teenage births. Our findings suggest that immigration policy that includes incentives for education can lead to improvements in each of these areas of concern; a reversal of this policy may overturn those gains.

References

Abramitzky, Ran, and Victor Lavy. 2014. "How responsive is investment in schooling to changes in redistributive policies and in returns?" *Econometrica*, 82(4): 1241–1272.

Amuedo-Dorantes, Catalina, and Cynthia Bansak. 2006. "Money transfers among banked and unbanked Mexican immigrants." *Southern Economic Journal*, 374–401.

Amuedo-Dorantes, Catalina, and Francisca Antman. 2016. "Can authorization reduce poverty among undocumented immigrants? Evidence from the Deferred Action for Childhood Arrivals program." *Economics Letters*, 147: 1–4.

Amuedo-Dorantes, Catalina, and Francisca Antman. 2017. "Schooling and Labor Market Effects of Temporary Authorization: Evidence from DACA." *Journal of Population Economics*, 30(1): 339–373.

Atkin, David. 2016. "Endogenous Skill Acquisition and Export Manufacturing in Mexico." *The American Economic Review*, 106(8): 2046–2085.

Becker, Gary Stanley. 1967. *Human capital and the personal distribution of income: An analytical approach.* Institute of Public Administration.

Black, Dan A, Terra G McKinnish, and Seth G Sanders. 2005. "Tight labor markets and the demand for education: Evidence from the coal boom and bust." *ILR Review*, 59(1): 3–16.

Black, Sandra E, Paul J Devereux, and Kjell G Salvanes. 2008. "Staying in the classroom and out of the maternity ward? The effect of compulsory schooling laws on teenage births." *The Economic Journal*, 118(530): 1025–1054.

Bleakley, Hoyt, and Aimee Chin. 2010. "Age at arrival, English proficiency, and social assimilation among US immigrants." *American Economic Journal. Applied Economics*, 2(1): 165.

Bleemer, Zachary, and Basit Zafar. 2015. "Intended college attendance: evidence from an experiment on college returns and costs."

Borjas, George J. 1985. "Assimilation, changes in cohort quality, and the earnings of immigrants." *Journal of Labor Economics*, 3(4): 463–489.

Borjas, George J. 2017. "The Earnings of Undocumented Immigrants." National Bureau of Economic Research Working Paper 23236.

Borusyak, Kirill, and Xavier Jaravel. 2016. "Revisiting Event Study Designs."

Bratsberg, Bernt, James F Ragan, Jr, and Zafar M Nasir. 2002. "The effect of naturalization on wage growth: A panel study of young male immigrants." *Journal of Labor Economics*, 20(3): 568–597.

Brown, Meta, John Karl Scholz, and Ananth Seshadri. 2011. "A new test of borrowing constraints for education." *The Review of Economic Studies*, 79(2): 511–538.

Carrell, Scott, and Bruce Sacerdote. 2017. "Why Do College-Going Interventions Work?" *American Economic Journal: Applied Economics*, 9(3): 124–151.

Cascio, Elizabeth, and Ethan Lewis. 2016. "How Much Does Amnesty Stretch the Safety Net? Evidence from the Immigration Reform and Control Act of 1986."

Cascio, Elizabeth U, and Ayushi Narayan. 2015. "Who needs a fracking education? the educational response to low-skill biased technological change." National Bureau of Economic Research.

Charles, Kerwin Kofi, Erik Hurst, and Matthew J Notowidigdo. 2015. "Housing booms and busts, labor market opportunities, and college attendance." National Bureau of Economic Research.

Cortes, Kalena E. 2013. "Achieving the DREAM: The Effect of IRCA on Immigrant Youth Postsecondary Educational Access." *American Economic Review*, 103(3): 428–432.

Goodman-Bacon, Andrew. 2016. "The Long-Run Effects of Childhood Insurance Coverage: Medicaid Implementation, Adult Health, and Labor Market Outcomes." National Bureau of Economic Research Working Paper 22899.

Hipsman, Faye, Bárbara Gómez-Aguiaga, and Randy Capps. 2016. "DACA at Four: Participation in the Deferred Action Program and Impacts on Recipients."

Hsin, Amy, and Francesc Ortega. 2017. "The Effects of Deferred Action for Childhood Arrivals on the Educational Outcomes of Undocumented Students." IZA Institute of Labor Economics Working Paper 11078.

Jackson, C. Kirabo, Rucker C. Johnson, and Claudia Persico. 2016. "The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms." *The Quarterly Journal of Economics*, 131(1): 157.

Jensen, Robert. 2010. "The (perceived) returns to education and the demand for schooling." *The Quarterly Journal of Economics*, 125(2): 515–548.

Kaushal, Neeraj. 2006. "Amnesty programs and the labor market outcomes of undocumented workers." *Journal of Human Resources*, 41(3): 631–647.

Kearney, Melissa S., and Phillip B. Levine. 2012. "Why Is the Teen Birth Rate in the United States So High and Why Does It Matter?" *Journal of Economic Perspectives*, 26(2): 141–163.

Kearney, Melissa S., and Phillip B. Levine. 2014. "Income Inequality and Early Nonmarital Childbearing." *Journal of Human Resources*, 49(1): 1–31.

Kleven, Henrik Jacobsen, Camille Landais, Emmanuel Saez, and Esben Schultz. 2013. "Migration and wage effects of taxing top earners: Evidence from the foreigners tax scheme in Denmark." *The Quarterly Journal of Economics*, 129(1): 333–378.

Kossoudji, Sherrie A, and Deborah A Cobb-Clark. 2002. "Coming out of the shadows: Learning about legal status and wages from the legalized population." *Journal of Labor Economics*, 20(3): 598–628.

LaLonde, Robert J, and Robert H Topel. 1992. "The assimilation of immigrants in the US labor market." In *Immigration and the workforce: Economic consequences for the United States and source areas*. 67–92. University of Chicago Press.

Lee, Jin Young, and Gary Solon. 2011. "The Fragility of Estimated Effects of Unilateral Divorce Laws on Divorce Rates." *The B.E. Journal of Economic Analysis & Policy*, 11(1).

Liscow, Zachary, and William Gui Woolston. 2016. "Does Legal Status Affect Educational Attainment in Immigrant Families?"

Marie, Olivier, and Ulf Zölitz. 2017. "High Achievers? Cannabis Access and Academic Performance." *The Review of Economic Studies*, 84(3): 1210–1237.

McCrary, Justin, and Heather Royer. 2011. "The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth." *American Economic Review*, 101(1): 158–195.

Mendoza, Gilberto, and Noor Shaikh. 2015. "Tuition benefits for immigrants."

Monras, Joan, Javier Vázquez-Grenno, and Ferran Elias Moreno. 2017. "Understanding the Effects of Legalizing Undocumented Immigrants."

Nevarez, Griselda. 2015. "American Dream: After Qualifying For DACA, Young Immigrants Buy Homes."

Olivas, Michael A. 2004. "IIRIRA, the Dream Act, and undocumented college student residency." *Immigr. & Nat'lity L. Rev.*, 25: 323.

Oreopoulos, Philip, and Kjell G. Salvanes. 2011. "Priceless: The Nonpecuniary Benefits of Schooling." *Journal of Economic Perspectives*, 25(1): 159–184.

Osili, Una Okonkwo, and Anna L Paulson. 2009. "Immigrants access to financial services and asset accumulation." *Insufficient funds: Savings, assets, credit, and banking among low-income households*, 285–317.

Oster, Emily, and Bryce Millett Steinberg. 2013. "Do IT service centers promote school enrollment? Evidence from India." *Journal of Development Economics*, 104: 123–135.

Passel, Jeffrey, and Mark Hugo Lopez. 2012. "Up to 1.7 million unauthorized immigrant youth may benefit from new deportation rules." *Washington, DC: Pew Hispanic Center.*

Passel, Jeffrey S. 2005. "Estimates of the Size and Characteristics of the Undocumented Population."

Passel, Jeffrey S., and DVera Cohn. 2009. "A Portrait of Unauthorized Immigrants in the United States."

Pinotti, Paolo. 2017. "Clicking on heaven's door: The effect of immigrant legalization on crime." *The American Economic Review*, 107(1): 138–168.

Pope, Nolan G. 2016. "The Effects of DACAmentation: The Impact of Deferred Action for Childhood Arrivals on Unauthorized Immigrants." *Journal of Public Economics*, 143: 98–114.

Redstone, Ilana, and Douglas S. Massey. 2004. "Coming To Stay: An Analysis of the U.S. Census Question On Immigrants Year of Arrival." *Demography*, 41(4): 721–738.

Rivera-Batiz, Francisco L. 1999. "Undocumented workers in the labor market: An analysis of the earnings of legal and illegal Mexican immigrants in the United States." *Journal of Population Economics*, 12(1): 91–116.

Ruggles, Steven, K Genadek, R Goeken, J Grover, and M Sobek. 2015. "Integrated Public Use Microdata Series: Version 6.0." *University of Minnesota, Minneapolis.*

Shah, Manisha, and Bryce Millett Steinberg. 2017. "Drought of Opportunities: Contemporaneous and Long-Term Impacts of Rainfall Shocks on Human Capital." *Journal of Political Economy*, 125(2): 527–561.

Simanski, John F., and Lesley Sapp. 2012. "Immigration Enforcement Actions: 2012." *Department of Homeland Security*.

Singer, Audrey, Nicole Prchal Svajlenka, and Jill H Wilson. 2015. "Local Insights From DACA for Implementing Future Programs for Unauthorized Immigrants."

Steigleder, Quinn, and Chad Sparber. 2017. "The effect of legal status on immigrant wages and occupational skills." *Applied Economics Letters*, 24(2): 80–84.

Vargas, Jose Antonio. 2012. "How Obamas New Policy Affects Undocumented Youth – and Me." *Time*.

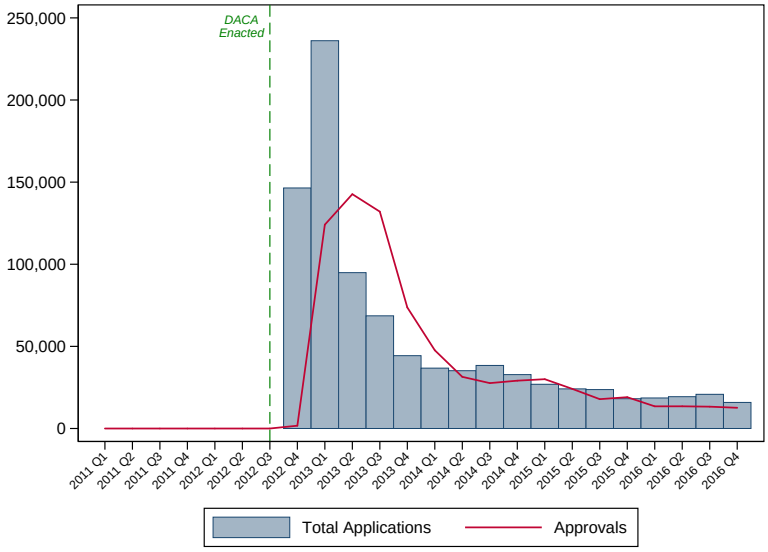
Wiswall, Matthew, and Basit Zafar. 2014. "Determinants of college major choice: Identification using an information experiment." *The Review of Economic Studies*, 82(2): 791–824.

Wolfers, Justin. 2006. "Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results." *American Economic Review*, 96(5): 1802–1820.

Wong, Tom K., Greisa Martinez Rosas, Adrian Reyna, Ignacia Rodriguez, Patrick OShea, Tom Jawetz, and Philip E. Wolgin. 2016. "New Study of DACA Beneficiaries Shows Positive Economic and Educational Outcomes."

10 Figures

Figure 1: Initial DACA Applications and Approvals by Quarter

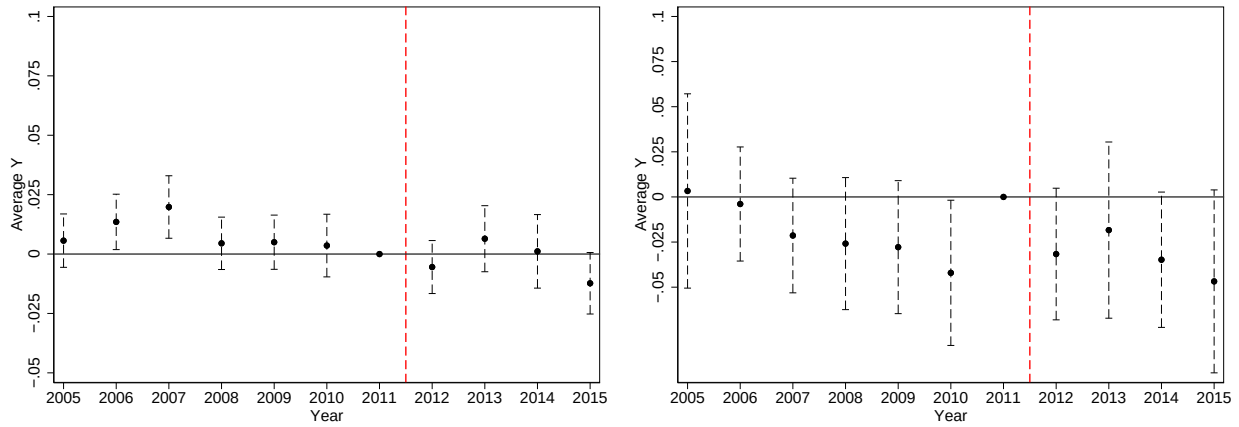


Notes: Figure shows first-time DACA application counts and the number approved in each quarter through 2016. Data comes from publicly available records from United States Citizenship and Immigration Services. See https://www.uscis.gov/tools/reports-studies/immigration-forms-data/data-set-form-i-821d-deferred-action-childhood-arrivals.

Figure 2: Effect of DACA on *Predicted* School Attendance and High School Completion, Hispanics

(a) School Attendance, Ages 14-18

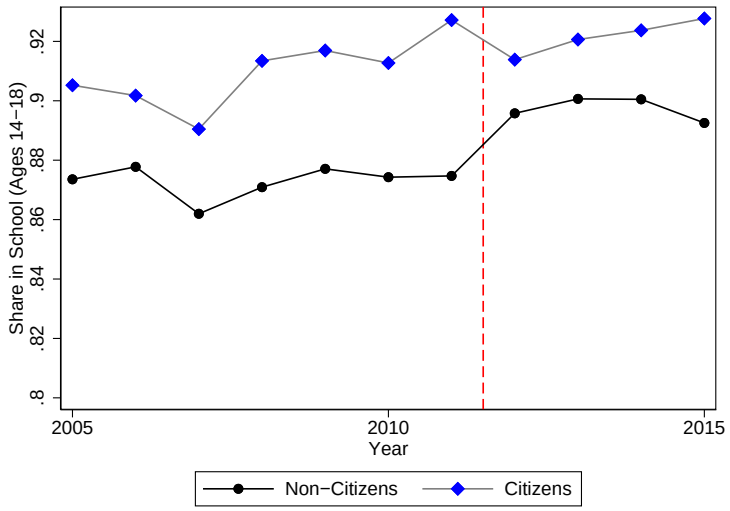
(b) High School Completion, Age 19



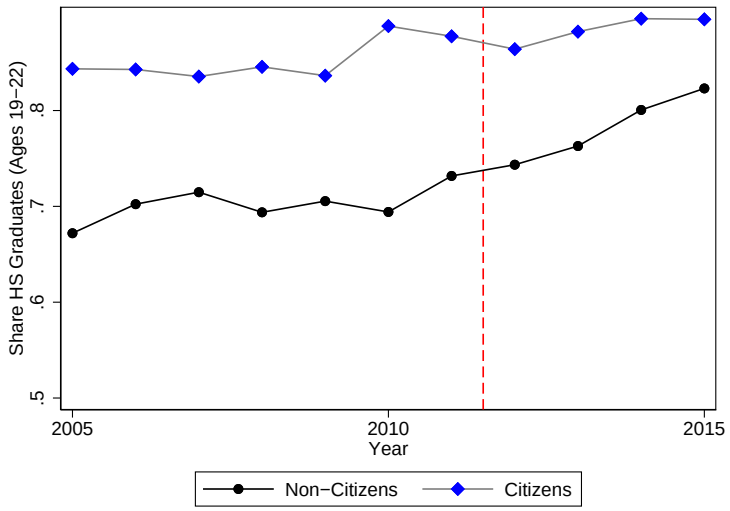
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born Hispanics that immigrated by age 10 and by 2007. Outcomes are the fitted values of likelihood of being in school and high school completion, which we estimate from a regression of school attendance on demographic variables using data from 2005 to 2011. See text for details. Each point represents coefficients from event study regressions that separately estimate interactions between year and eligibility indicators. Year 2011 is the omitted category, and the vertical dashed line indicates the enactment of DACA. We also provide 95% confidence intervals, calculated with standard errors that are clustered by state and adjusted for three additional degrees of freedom. The results are weighted by the survey sampling weights..

Figure 3: Average School Attendance and High School Completion, Hispanic Immigrants that Arrived by Age 10 and by 2007

(a) Share in School, Ages 14-18



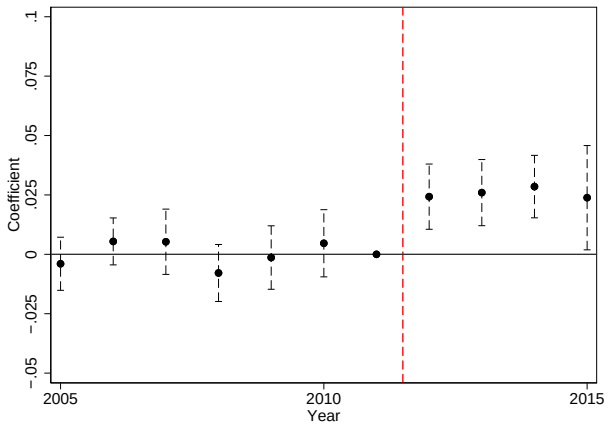
(b) High School Completion, Ages 19-22



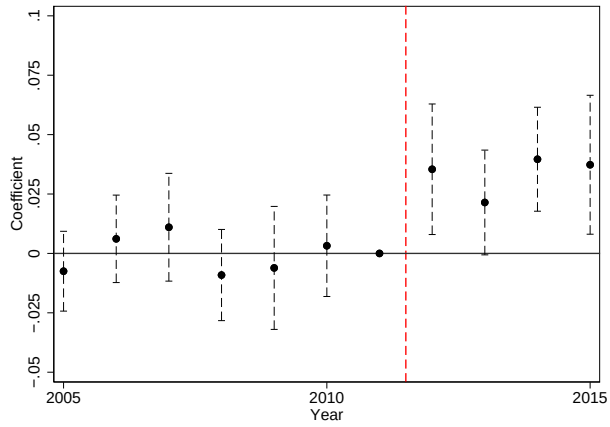
Notes: This figure shows average school attendance and high school completion rates for Hispanic immigrants ages 14-18 and ages 19-22, respectively, that immigrated by age 10 and by 2007, with statistics calculated from the 2005-2015 American Community Surveys. The red line demarcates the implementation of DACA.

Figure 4: Effect of DACA on School Attendance, Ages 14-18

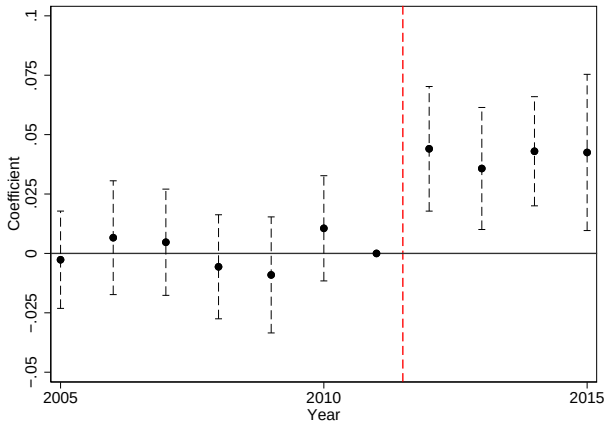
(a) All



(b) Hispanic



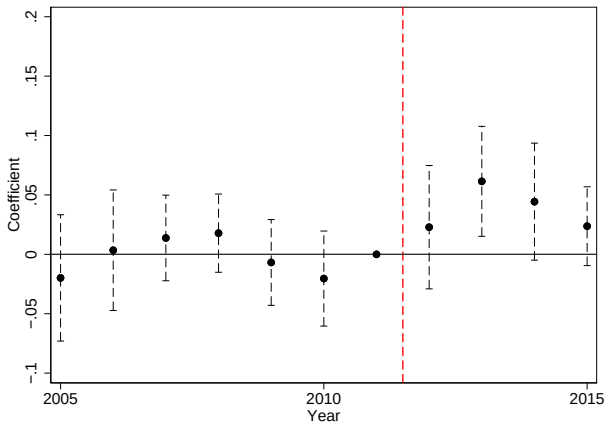
(c) High Take-Up



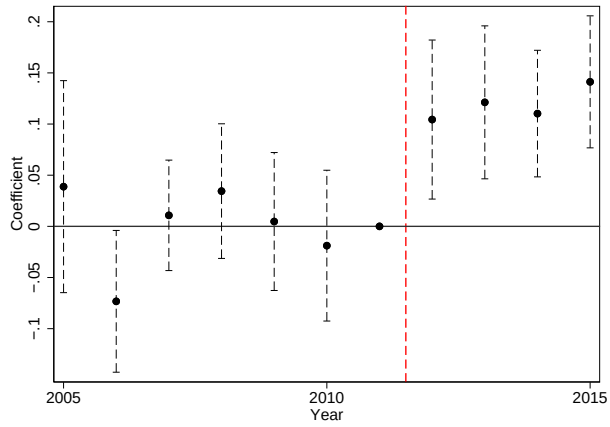
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Each point represents coefficients from event study regressions that separately estimate interactions between year and eligibility indicators. Year 2011 is the omitted category, and the vertical dashed line indicates the enactment of DACA. We also provide 95% confidence intervals, calculated with standard errors that are clustered by state and adjusted for three additional degrees of freedom. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights.

Figure 5: Effect of DACA on High School Completion, Age 19

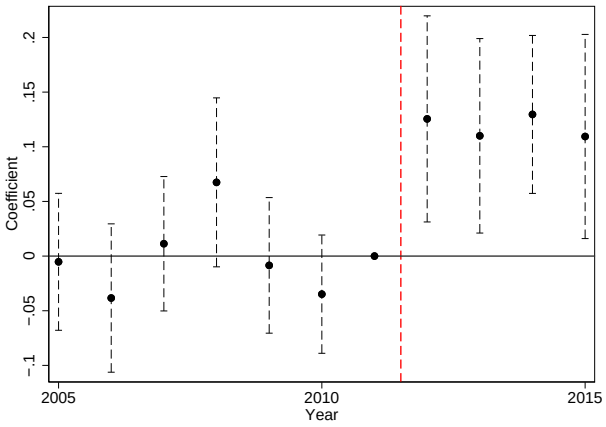
(a) All



(b) Hispanic



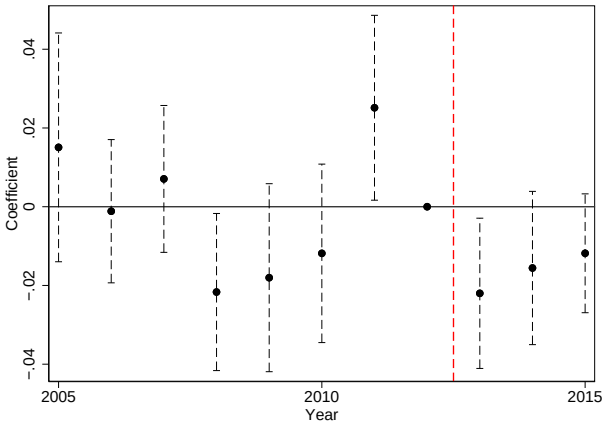
(c) High Take-Up



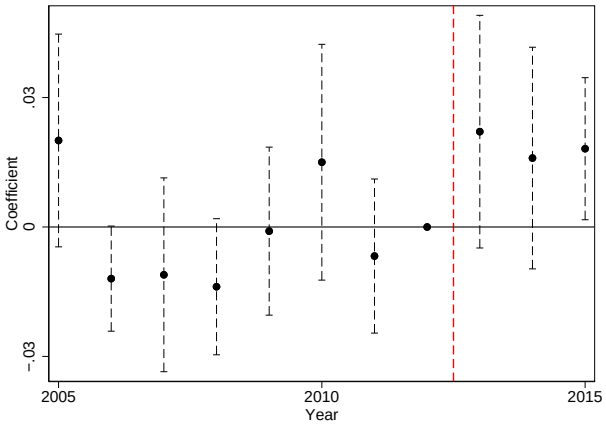
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Each point represents coefficients from event study regressions that separately estimate interactions between year and eligibility indicators. Year 2011 is the omitted category, and the vertical dashed line indicates the enactment of DACA. We also provide 95% confidence intervals, calculated with standard errors that are clustered by state and adjusted for three additional degrees of freedom. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights.

Figure 6: Effect of DACA on Fertility, Hispanics Ages 15-18

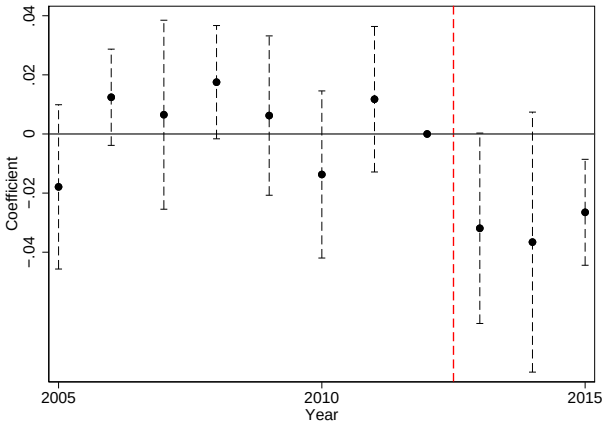
(a) Had Child Last Year



(b) Have No Children



(c) Number of Children



Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born females that immigrated by age 10 and by 2007. Note that when analyzing fertility behavior, we consider 2013 and onward to be post treatment years. This is due to the fact that fertility decisions are made 9 months prior to reporting. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2012) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Each point represents coefficients from event study regressions that separately estimate interactions between year and eligibility indicators. Year 2012 is the omitted category, and the vertical dashed line indicates the enactment of DACA. We also provide 95% confidence intervals, calculated with standard errors that are clustered by state and adjusted for three additional degrees of freedom. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights.

11 Tables

Table 1: Effect of DACA on School Attendance

	All	Hispanic	High Take-Up
A: Age 14-18			
Eligible*Post	0.026***	0.033***	0.041***
	(0.004)	(0.008)	(0.007)
Mean Y	0.921	0.891	0.889
Individuals	114453	54015	48359
B: Age 19-22			
Eligible*Post	0.046***	0.055***	0.055***
	(0.010)	(0.015)	(0.017)
Mean Y	0.547	0.405	0.401
Individuals	82077	38704	34768

Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the detrended data. The dependent variable is current school attendance, and post is an indicator for 2012 or after. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

Table 2: Effect of DACA on High School Completion and College Enrollment

	High School Completion			Some College		
	All	Hispanic	High Take-Up	All	Hispanic	High Take-Up
A: Age 19						
Eligible*Post	0.038**	0.114***	0.118***	0.001	0.076**	0.099***
	(0.014)	(0.025)	(0.029)	(0.018)	(0.032)	(0.033)
Mean Y	0.824	0.747	0.741	0.468	0.350	0.343
Individuals	22153	10252	9173	22153	10252	9173
B: Age 19-22						
Eligible*Post	0.041***	0.059***	0.063***	0.022*	0.041***	0.047***
	(0.007)	(0.010)	(0.009)	(0.012)	(0.015)	(0.015)
Mean Y	0.858	0.781	0.775	0.544	0.407	0.399
Individuals	82077	38704	34768	82077	38704	34768
C: Age 23-30						
Eligible*Post	0.022***	0.019*	0.005	0.044***	0.054***	0.044***
	(0.008)	(0.011)	(0.006)	(0.009)	(0.013)	(0.015)
Mean Y	0.875	0.785	0.782	0.634	0.466	0.460
Individuals	124184	54964	48042	124184	54964	48042

Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. The dependent variables are high school completion and enrollment in post-secondary schooling, and post is an indicator for 2012 or after. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

Table 3: Effect of DACA on High School Completion and College Enrollment – By Gender

	High School Completion			Some College		
	All	Female	Male	All	Female	Male
A: Hispanic						
Eligible*Post	0.114***	0.053	0.192***	0.076**	0.098***	0.050
	(0.025)	(0.037)	(0.025)	(0.032)	(0.034)	(0.048)
Mean Y	0.747	0.776	0.721	0.350	0.388	0.316
Individuals	10252	4888	5364	10252	4888	5364
B: High Take-Up						
Eligible*Post	0.118***	0.053	0.190***	0.099***	0.102***	0.062
	(0.029)	(0.036)	(0.031)	(0.033)	(0.037)	(0.052)
Mean Y	0.741	0.770	0.714	0.343	0.376	0.312
Individuals	9173	4388	4785	9173	4388	4785

Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of 19 year old foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. The dependent variables are high school completion and enrollment in post-secondary schooling, and post is an indicator for 2012 or after. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: Effect of DACA on Teenage Fertility, Ages 15-18

	Indicator		Continuous
	Child LY	No Children	Num. Children
A: All			
Eligible*Post	-0.011***	0.016***	-0.025***
	(0.004)	(0.003)	(0.004)
Mean Y	0.022	0.974	0.032
Individuals	45032	45148	45148
B: Hispanic			
Eligible*Post	-0.017***	0.019***	-0.032***
	(0.006)	(0.007)	(0.010)
Mean Y	0.035	0.957	0.051
Individuals	20768	20845	20845
C: High Take-Up			
Eligible*Post	-0.018***	0.027***	-0.043***
	(0.006)	(0.008)	(0.012)
Mean Y	0.035	0.955	0.055
Individuals	18544	18614	18614

Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born females that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. Note that when analyzing fertility behavior, we consider 2013 and onward to be post treatment years. This is due to the fact that fertility decisions are made 9 months prior to reporting. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2012) and removes this trend from the full data; and the second step performs estimation on the de-trended data. The dependent variables are indicators for having had a child last year, having no children, and total number of children, and post is an indicator for 2013 or after. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: Effect of DACA on Work and Idleness

	Total Effect					
	School	Work	Idle	School & Work	School Only	Work Only
A: Hispanic 16–18						
Eligible*Post	0.042***	0.021**	-0.041***	0.022**	0.020	-0.000
	(0.012)	(0.009)	(0.011)	(0.009)	(0.015)	(0.005)
Mean Y	0.843	0.200	0.091	0.134	0.709	0.066
Individuals	32888	32888	32888	32888	32888	32888
B: High Take-Up 16–18						
Eligible*Post	0.057***	0.036***	-0.054***	0.039***	0.018*	-0.003
	(0.010)	(0.008)	(0.008)	(0.009)	(0.010)	(0.007)
Mean Y	0.842	0.201	0.089	0.132	0.710	0.069
Individuals	29458	29458	29458	29458	29458	29458
C: Hispanic 19–22						
Eligible*Post	0.055***	0.084***	-0.061***	0.078***	-0.023**	0.006
	(0.015)	(0.009)	(0.013)	(0.013)	(0.009)	(0.015)
Mean Y	0.405	0.589	0.218	0.212	0.193	0.377
Individuals	38704	38704	38704	38704	38704	38704
D: High Take-Up 19–22						
Eligible*Post	0.055***	0.032***	-0.038***	0.049***	0.006	-0.017
	(0.017)	(0.011)	(0.009)	(0.017)	(0.009)	(0.018)
Mean Y	0.401	0.594	0.214	0.210	0.192	0.385
Individuals	34768	34768	34768	34768	34768	34768

Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Post is an indicator for 2012 or after. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Effect of DACA on CAHSEE Math and ELA Exams

	Math Test		Math Score		ELA Test		ELA Score	
	% Take	% Pass	Level	Log	% Take	% Pass	Level	Log
A: Grade 10								
High Share DACA Eligible * Post	0.008*	0.008**	0.242	0.001	0.008*	0.006	-0.931**	-0.002***
	(0.004)	(0.003)	(0.604)	(0.002)	(0.004)	(0.003)	(0.343)	(0.001)
Mean Y	0.181	0.135	374.196	5.925	0.182	0.134	370.634	5.915
Observations	340	340	340	340	340	340	340	340
B: Grade 11								
High Share DACA Eligible * Post	0.000	-0.001*	-0.495	-0.002	0.002	-0.000	-0.938	-0.003
	(0.001)	(0.001)	(0.461)	(0.001)	(0.001)	(0.000)	(0.591)	(0.002)
Mean Y	0.071	0.023	340.773	5.831	0.072	0.024	338.029	5.823
Observations	340	340	340	340	340	340	340	340
C: Grade 12								
High Share DACA Eligible * Post	0.006***	0.002***	0.864**	0.002**	0.004**	0.001**	0.136	0.000
	(0.001)	(0.000)	(0.380)	(0.001)	(0.002)	(0.000)	(0.482)	(0.001)
Mean Y	0.056	0.015	337.294	5.821	0.059	0.014	332.414	5.806
Observations	340	340	340	340	340	340	340	340
D: All Grades								
High Share DACA Eligible * Post	0.016**	0.009**	-0.305	-0.001	0.015**	0.007	-0.539	-0.002
	(0.007)	(0.004)	(0.462)	(0.001)	(0.006)	(0.004)	(0.543)	(0.001)
Mean Y	0.316	0.175	359.939	5.886	0.321	0.176	356.000	5.875
Observations	340	340	340	340	340	340	340	340

Notes: Data are from the California Department of Education and span 2006–2015. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. The share of Hispanics aged 10-30 that are eligible, defined according to the "age-eligible" criteria discussed in the text, represent our treatment, and post is an indicator for 2012 or after. The results are weighted by the average county Hispanic population aged 14-18 from the 2005-2011 ACS. Standard errors, shown in parentheses, are clustered by county and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

Table 7: Effect of DACA on the Sexual Behavior of High School Students, Ages 14 to 18

	Last Time Had Sex, Pregnancy Protection:					Had Sex	
	None	Pill	Condom	IUD/Shot	Withdraw/Oth.	Ever	Last 3 Mos.
High Share DACA Eligible * Post	-0.048**	-0.001	0.050***	-0.033***	0.031**	-0.009	0.004
	(0.022)	(0.017)	(0.017)	(0.011)	(0.012)	(0.018)	(0.020)
Eligible Mean	0.189	0.103	0.543	0.039	0.125	0.470	0.328
Individuals	29332	29332	29332	29332	29332	67913	67100

Notes: Data from the 2005-2015 Youth Risk Behavior Surveillance Survey. Sample includes survey participants ages 14 to 18. The dependent variables provide various measures of pregnancy protection and recent sexual activity. The share of Hispanics aged 10-30 that are eligible, defined according to the "age-eligible" criteria discussed in the text, represent our treatment, and post is an indicator for 2012 or after. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Regressions include state and year fixed effects. The results are weighted by survey weights provided by YRBSS. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

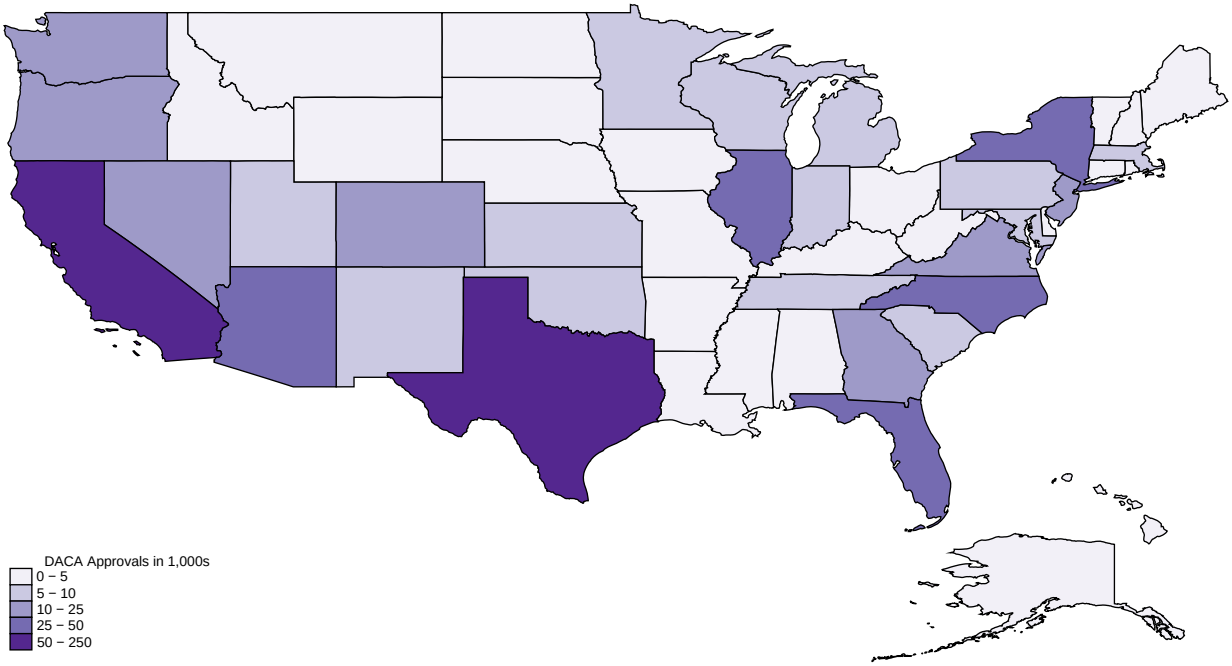
Table 8: Effect of DACA on School Attendance, High School Completion and Fertility, of Hispanics, Alternative Sample Restrictions

		Arrived By		No Restriction on		Add	Refine
	Baseline	Age 6	Age 16	Age Arrival	Year Arrival	Natives	Eligibility
A: School Attendance, Ages 14-18							
Eligible*Post	0.033***	0.031***	0.034***	0.036***	0.050***	0.028***	0.033***
	(0.008)	(0.009)	(0.008)	(0.009)	(0.008)	(0.004)	(0.009)
Mean Y	0.891	0.899	0.850	0.840	0.834	0.912	0.892
Individuals	54015	37393	66981	68048	77474	409095	50219
B: High School Completion, Age 19							
Eligible*Post	0.114***	0.119***	0.137***	0.128***	0.115***	0.058***	0.099***
	(0.025)	(0.035)	(0.016)	(0.016)	(0.015)	(0.013)	(0.023)
Mean Y	0.747	0.761	0.677	0.651	0.637	0.762	0.756
Individuals	10252	6932	15131	16823	19316	76508	9515
C: Have No Children, Ages 15-18							
Eligible*Post	0.019***	0.018**	0.029***	0.030***	0.029***	0.023***	0.021**
	(0.007)	(0.008)	(0.007)	(0.007)	(0.005)	(0.003)	(0.008)
Mean Y	0.957	0.961	0.949	0.948	0.948	0.966	0.958
Individuals	20845	14222	25997	26364	29820	157332	19318

Notes: Data are from the 2005–2015 American Community Survey (ACS). High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. Columns (2) and (3) adjusts the sample to include only individuals that arrived by age 6 (more restrictive) and by 16 (more expansive), respectively. Column (4) adds foreign born individuals that arrived after age 16; (5) adds foreign born individuals that arrived after 2007; (6) adds individuals born in the U.S. Column (7) refines the baseline specification, restricting eligibility to individuals that do not live in a household that receives government benefits or that has a veteran in it. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. The dependent variable is shown in the panel heading, and post is an indicator for 2012 or after. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

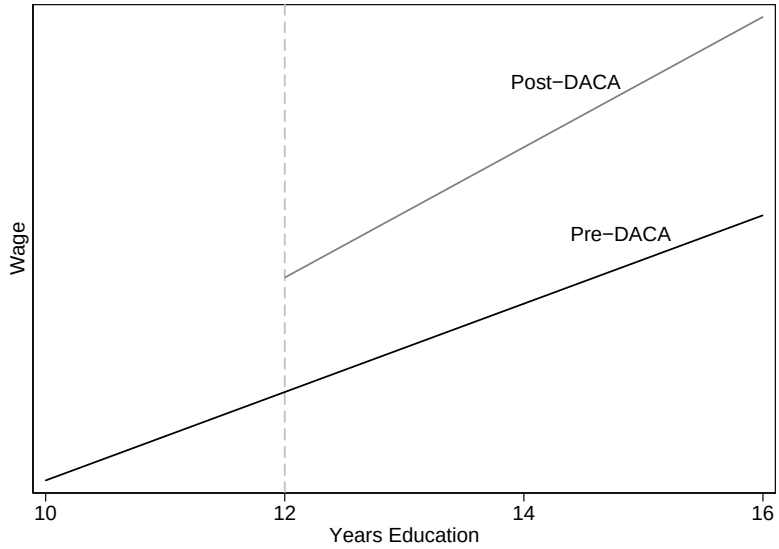
A Appendix: Further Results

Figure A.1: Cumulative Initial DACA Applications by State as of Q4 2016



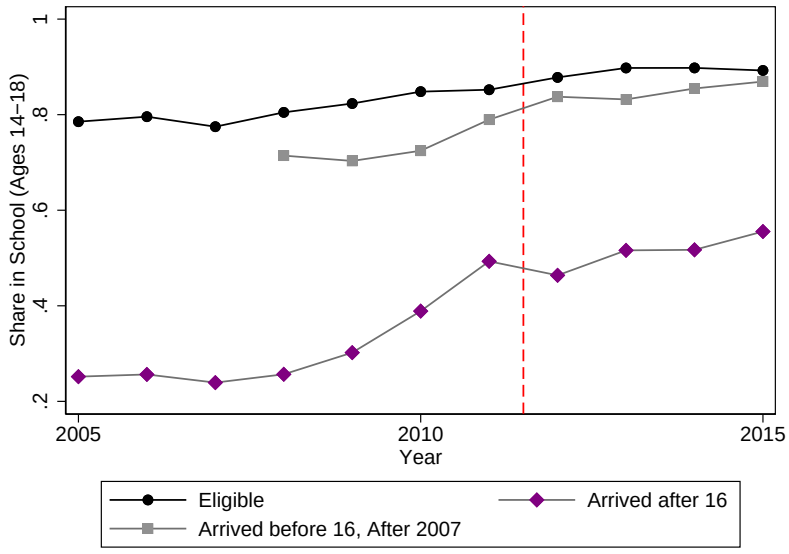
Notes: Figure shows first-time DACA application counts across states as of the fourth quarter of 2016. Data comes from publicly available records from United States Citizenship and Immigration Services. See https://www.uscis.gov/tools/reports-studies/immigration-forms-data/data-set-form-i-821d-deferred-action-childhood-arrivals.

Figure A.2: Returns to Education, Before and After DACA



Notes: Figure shows hypothetical changes in returns to education due to DACA. The vertical axis measures wage returns, while the horizontal axis measures years of education.

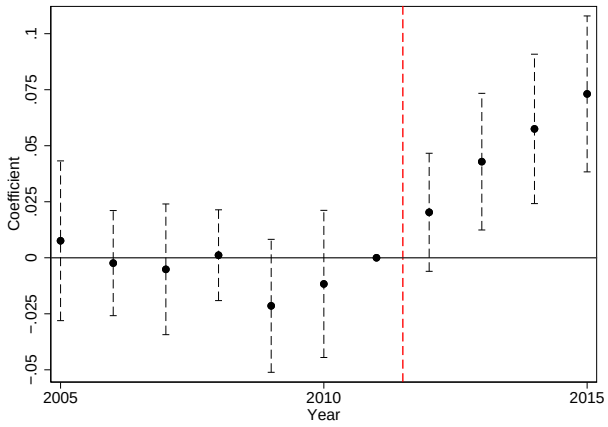
Figure A.3: Trends in School Attendance Among Non-Chosen Control Groups, Hispanic Immigrant Non-Citizens Ages 14-18



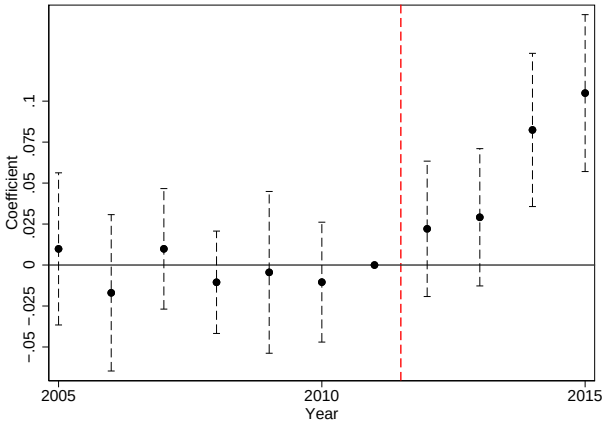
Notes: This figure shows school attendance rates for Hispanic immigrant non-citizens aged 14-18 that immigrated by age 10 and by 2007, or that immigrated after 16 or 2007, with statistics calculated from the 2005-2015 American Community Surveys. The red line demarcates the implementation of DACA.

Figure A.4: Effect of DACA on School Attendance, Ages 19-22

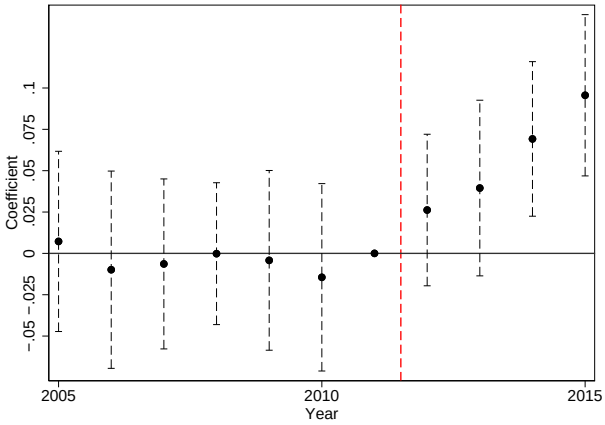
(a) All



(b) Hispanic



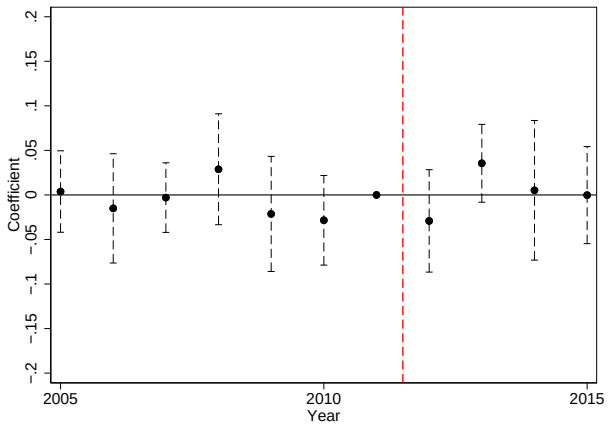
(c) High Take-Up



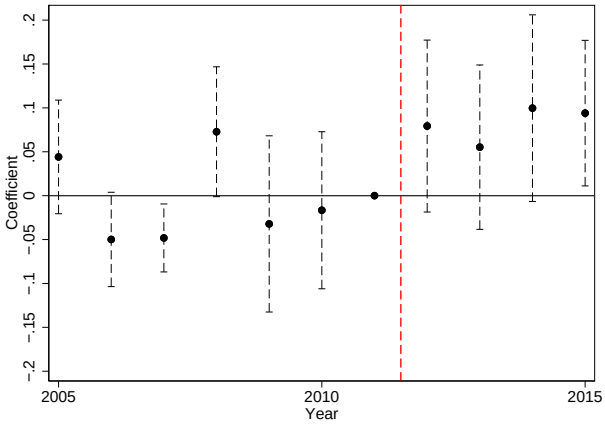
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Each point represents coefficients from event study regressions that separately estimate interactions between year and eligibility indicators. Year 2011 is the omitted category, and the vertical dashed line indicates the enactment of DACA. We also provide 95% confidence intervals, calculated with standard errors that are clustered by state and adjusted for three additional degrees of freedom. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights.

Figure A.5: Effect of DACA on College Attendance, Age 19

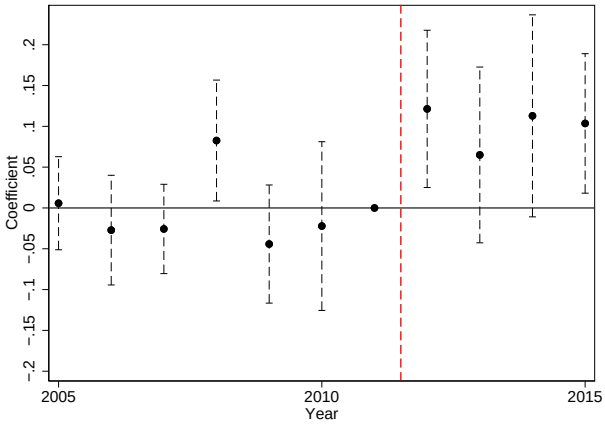
(a) All



(b) Hispanic



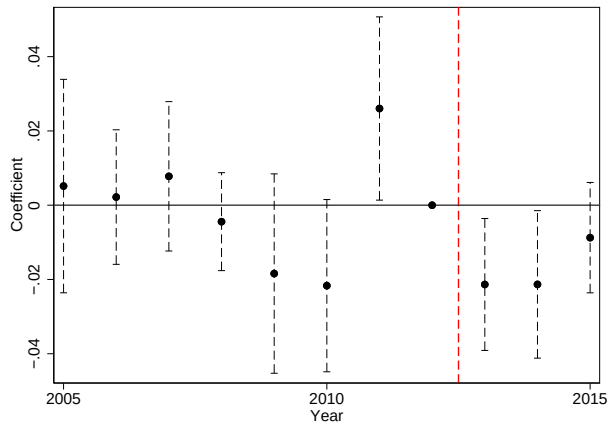
(c) High Take-Up



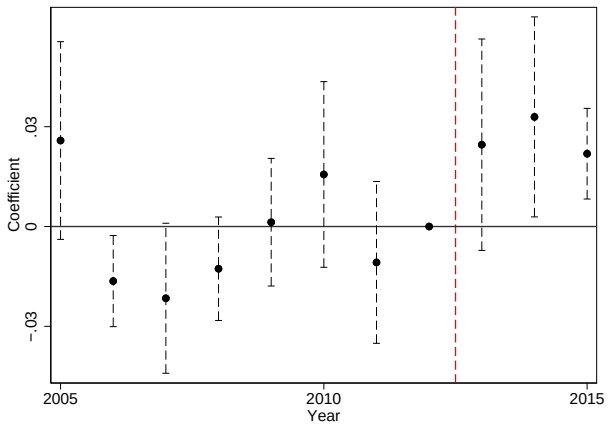
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Each point represents coefficients from event study regressions that separately estimate interactions between year and eligibility indicators. Year 2011 is the omitted category, and the vertical dashed line indicates the enactment of DACA. We also provide 95% confidence intervals, calculated with standard errors that are clustered by state and adjusted for three additional degrees of freedom. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights.

Figure A.6: Effect of DACA on Teenage Fertility, Ages 15-18 from High Take-Up Countries

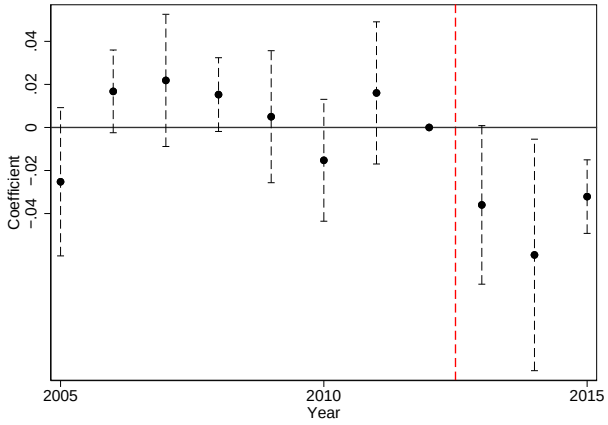
(a) Had Child Last Year



(b) Have No Children



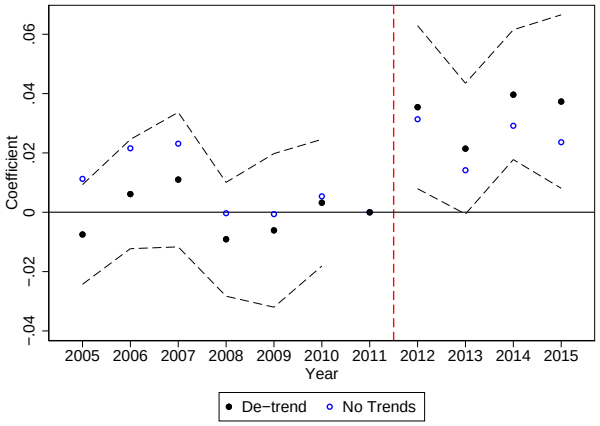
(c) Number of Children



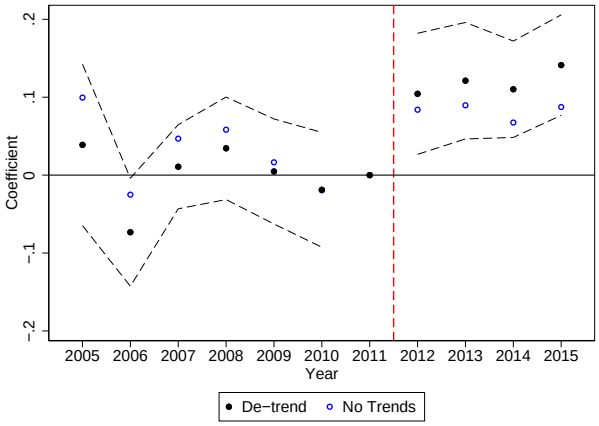
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born females that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2012) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Each point represents coefficients from event study regressions that separately estimate interactions between year and eligibility indicators. Year 2013 is the omitted category, and the vertical dashed line indicates the enactment of DACA. We also provide 95% confidence intervals, calculated with standard errors that are clustered by state and adjusted for three additional degrees of freedom. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights.

Figure A.7: Effect of DACA on School Attendance, High School Completion and Fertility, Hispanics – Sensitivity to Trends

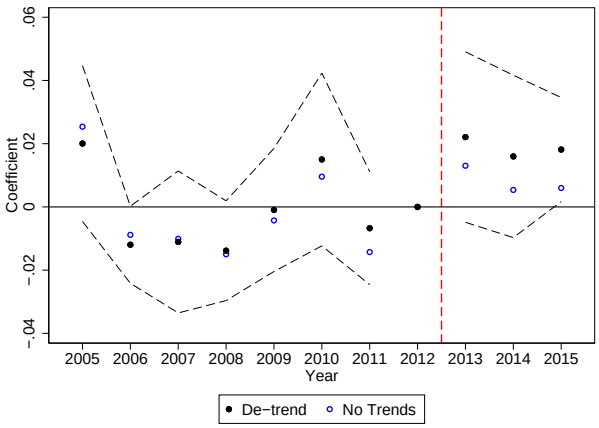
(a) School Attendance, Ages 14-18



(b) High School Completion, Age 19



(c) No Children, Females Ages 15-18



Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. Each point represents coefficients from event study regressions that separately estimate interactions between year and eligibility indicators. Year 2011 is the omitted category, and the vertical dashed line indicates the enactment of DACA. Note that when analyzing fertility behavior, we consider 2013 and onward to be post treatment years. This is due to the fact that fertility decisions are made 9 months prior to reporting. We also provide 95% confidence intervals for our baseline (de-trended) specification, calculated with standard errors that are clustered by state and adjusted for three additional degrees of freedom. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights.

Table A.1: Pre-DACA Characteristics of Hispanic Treatment and Comparison Groups, Ages 14-22

	Eligible	Control			
	(1)	(2)	(3)	(4)	(5)
	All	All	US Territories	US Parents	Naturalized
A: Individual Characteristics					
Female	0.47	0.49	0.48	0.50	0.50
Current Age	17.69	18.26	17.79	17.90	18.57
Age at Immigration	5.13	3.81	4.17	3.00	3.92
Year of Immigration	1995.57	1993.69	1994.62	1993.43	1993.39
Born in US Territory	0.00	0.24	1.00	0.00	0.00
Health Insurance	0.24	0.42	0.49	0.51	0.37
English Primary Language	0.03	0.16	0.11	0.36	0.12
Poor English	0.08	0.03	0.03	0.02	0.04
B: Family Characteristics					
Parent(s) in HH, Ages 14-17	0.92	0.93	0.92	0.95	0.94
Single Mother HH, Ages 14-17	0.18	0.26	0.41	0.21	0.20
Parent(s) College	0.07	0.19	0.14	0.24	0.19
Number of Siblings	1.54	1.17	1.19	1.08	1.20
In Poverty	0.32	0.22	0.36	0.16	0.18
Income to Poverty Ratio	1.64	2.26	1.82	2.60	2.35
Food Stamp Recipient in HH	0.18	0.19	0.37	0.12	0.13
C: Outcomes					
School Attendance, Ages 14-18	0.87	0.91	0.89	0.93	0.91
School Attendance, Ages 19-22	0.33	0.49	0.38	0.55	0.51
High School Completion, Ages 19-22	0.70	0.85	0.75	0.87	0.88
College Enrollment, Ages 19-22	0.31	0.51	0.37	0.57	0.55
Had Child in Year Prior, Ages 15-18	0.02	0.02	0.02	0.01	0.01
Number of Children, Ages 15-18	0.04	0.02	0.02	0.01	0.02
Obs.	39820	18714	4206	3633	10875

Notes: Data are from the 2005–2011 American Community Survey (ACS). The sample is composed of Hispanic foreign born individuals ages 14 to 22 that immigrated by age 10 and by 2007. Average characteristics for DACA-eligible appear in column (1), the complete control group in column (2), the control group born in U.S. territories in column (3), the control group born to American parents abroad in column (4), and the control group that gained citizenship through naturalization in column (5).

Table A.2: Effect of DACA on College Enrollment –
By Presence of In-State Tuition Policies

	Hispanic		High Take-Up	
A: Age 19				
Eligible*Post	0.076**	0.030	0.099***	0.070
	(0.032)	(0.056)	(0.033)	(0.053)
Eligible*Post*In State Tuition		0.084		0.053
		(0.071)		(0.060)
Individuals	10252	10252	9173	9173
B: Age 19-22				
Eligible*Post	0.041***	-0.001	0.047***	0.005
	(0.015)	(0.017)	(0.015)	(0.025)
Eligible*Post*In State Tuition		0.058**		0.055*
		(0.026)		(0.030)
Individuals	38704	38704	34768	34768

Notes: In-state tuition laws taken from Mendoza and Shaikh (2015). Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. The dependent variable is college enrollment, and post is an indicator for 2012 or after. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

Table A.3: Effect of DACA on Fertility After High School

	Hispanic			High Take-Up		
	Child LY	No Children	Num. Children	Child LY	No Children	Num. Children
A: Age 19-22						
Eligible*Post	-0.000	-0.000	0.026	0.003	0.000	0.023
	(0.010)	(0.016)	(0.024)	(0.011)	(0.015)	(0.023)
Mean Y	0.109	0.748	0.362	0.110	0.746	0.366
Individuals	18433	18501	18501	16510	16574	16574
B: Age 23-30						
Eligible*Post	-0.002	-0.022*	0.034	0.008	-0.051***	0.076**
	(0.011)	(0.013)	(0.041)	(0.010)	(0.012)	(0.033)
Mean Y	0.116	0.463	1.054	0.117	0.467	1.044
Individuals	27671	27736	27736	24170	24235	24235

Notes: Data are from the 2005–2015 American Community Survey (ACS). The sample is composed of foreign born females that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2012) and removes this trend from the full data; and the second step performs estimation on the de-trended data. The dependent variables are indicators for having had a child last year, having no children, and total number of children, and post is an indicator for 2013 or after. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: Effect of DACA on School Attendance, High School Completion and Fertility, Hispanics – Sensitivity to Trends

	Hispanic			High Take-Up		
	De-Trend	Linear Trend	No Trend	De-Trend	Linear Trend	No Trend
A: School Attendance, Ages 14-18						
Eligible*Post	0.033***	0.031*	0.016**	0.041***	0.041***	0.025***
	(0.008)	(0.018)	(0.007)	(0.007)	(0.015)	(0.008)
Mean Y	0.891	0.891	0.891	0.889	0.889	0.889
Individuals	54015	54015	54015	48359	48359	48359
B: High School Completion, Age 19						
Eligible*Post	0.114***	0.107*	0.061***	0.118***	0.120**	0.086***
	(0.025)	(0.058)	(0.022)	(0.029)	(0.059)	(0.024)
Mean Y	0.747	0.747	0.747	0.741	0.741	0.741
Individuals	10252	10252	10252	9173	9173	9173
C: Have No Children, Ages 15-18						
Eligible*Post	0.019***	0.019*	0.011	0.027***	0.027*	0.014*
	(0.007)	(0.011)	(0.008)	(0.008)	(0.014)	(0.008)
Mean Y	0.957	0.957	0.957	0.955	0.955	0.955
Individuals	20845	20845	20845	18614	18614	18614

Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by the survey sampling weights. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.

B Appendix: High School Graduation by Month in the NLSY97

The NLSY97 is a longitudinal survey of a nationally representative sample of roughly 9,000 youth that were between the ages of 12 and 16 by December 31, 1996. Respondents are surveyed on an annual basis on a range of topics, including educational progress. We use the NLSY97 to estimate the proportion of youth that receive a high school diploma in each month for individuals that graduate in 4, 5, or 6 years. We calculate the years of high school attended at the time of diploma as the ceiling of the difference between the year and month of diploma and the year and month that high school began. For simplicity, we assume the school year begins in September. Hence, graduating in September at the beginning of one's 4^{th} year is considered as graduating in four years. The statistics below are unweighted, and are unchanged when weighted,

Table B.1: Graduation by Month and Year

	Graduated in:		
	4 yrs	5 yrs.	6+ yrs.
Jan. to Jun.	0.975	0.757	0.824
Jul. to Aug.	0.019	0.025	0.049
Sep. to Dec.	0.006	0.218	0.127
Observations	6091	325	102

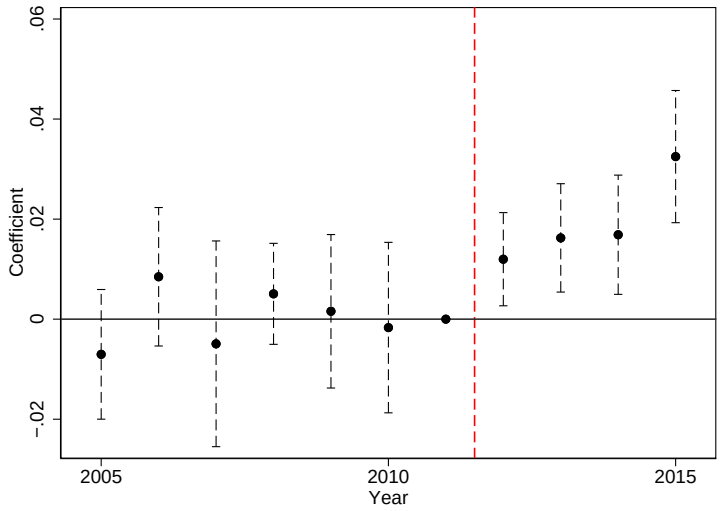
Notes: Data include individuals surveyed in the NLSY97. Statistics in each column represent the share of individuals that graduate in each set of months among those that graduate in a given number of years.

C Appendix: Geographic Level Analysis

C.1 CA Enrollment and CAHSEE Results

Before turning to results obtained with geographic (county or state) level variation, we first show that the schooling results identified in our main school attendance analysis are also present when using cross-county variation in California. Figure C.1 below shows that counties with a high share of DACA-eligible Hispanics experienced increased school attendance after DACA implementation. This results thus suggest that it is reasonable to investigate test score performance with county-level variation. Moreover, Figure C.2 shows a similar pattern of results when estimating the enrollment impact of DACA, where the outcome of interest is county-level hispanic high school enrollment from the California Department of Education as a share of the ACS number of Hispanics aged 14 to 18 in the county.[37]

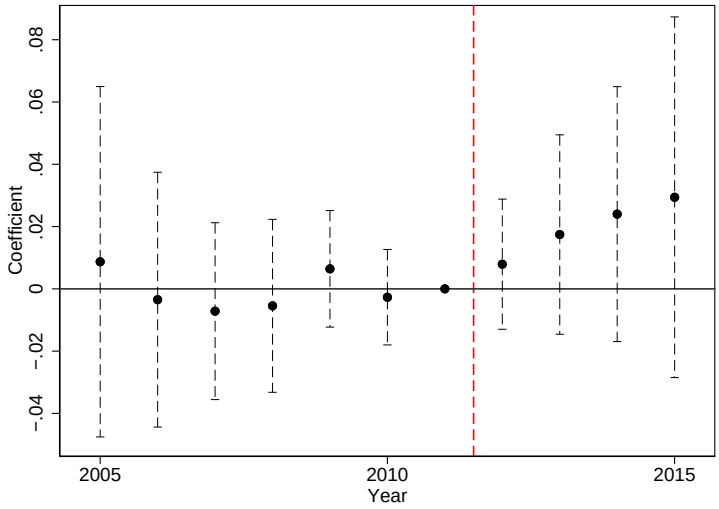
Figure C.1: Effect of DACA on School Attendance, Hispanics Ages 14-18 – California County-Level Variation



Notes: Data are from the American Community Survey (ACS) and cover years 2005–2015. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Each point represents coefficients from event study regressions that separately estimate interactions between year and an indicator for having above median share of eligible Hispanics, and 95% confidence intervals are provided for reference. The vertical dashed line indicates the enactment of DACA, and year 2011 is the omitted category. The results are weighted by the population in each cell. Standard errors are clustered by county and adjusted for three additional degrees of freedom.

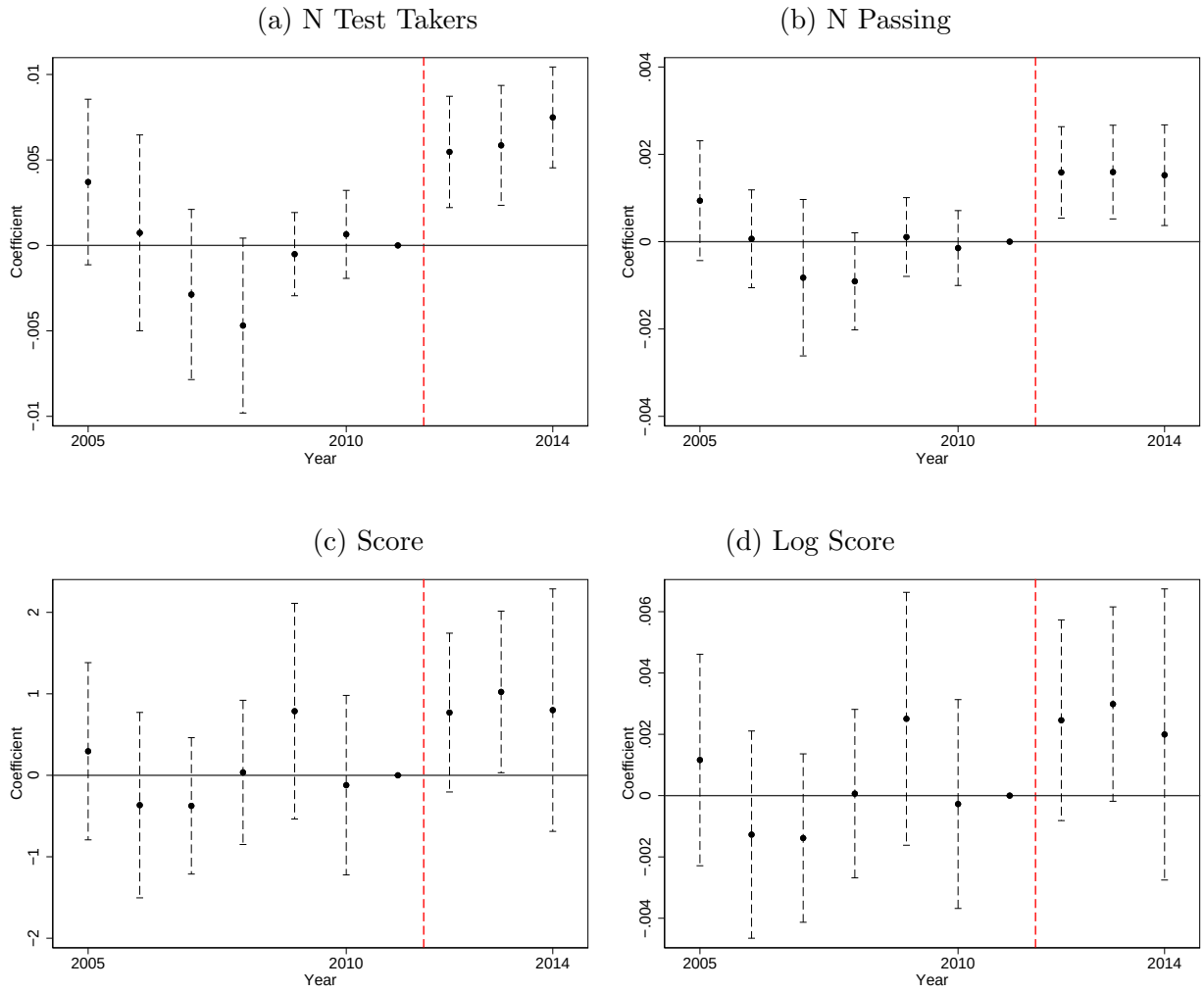
[37]Because the ACS only identifies 34 of California's 58 counties, we limit our analysis to these counties. Nonetheless, these 34 counties account for over 88% of total K-12 enrollment during the 2005-2015 period.

Figure C.2: Effect of DACA on High School Enrollment, Hispanics – California County-Level Variation with DOE Data



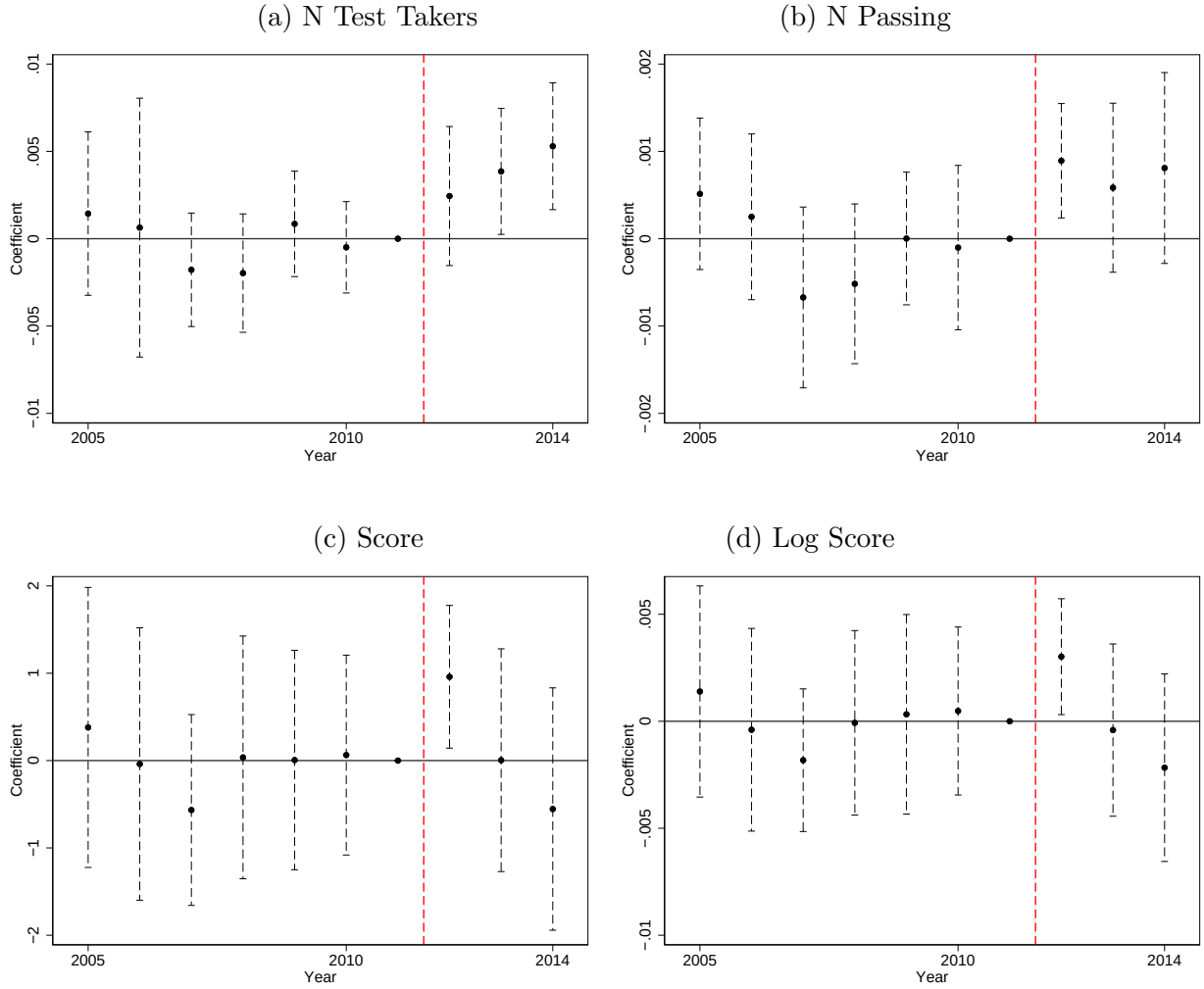
Notes: Data are from California's Department of Education, and cover years 2005–2015. The sample includes county-level hispanic enrollment in high schools. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Each point represents coefficients from event study regressions that separately estimate interactions between year and an indicator for having above median share of eligible Hispanics, and 95 confidence intervals are provided for reference. The vertical dashed line indicates the enactment of DACA, and year 2011 is the omitted category. The results are weighted by the average number of Hispanics aged 14 to 18 in the county in the 2005-2011 ACS. Standard errors are clustered by county and adjusted for three additional degrees of freedom.

Figure C.3: Effect of DACA on CAHSEE Math Exam, 12th Graders



Notes: Data are from California's Department of Education, and cover years 2005–2015. The sample includes county-level hispanic test takers. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Each point represents coefficients from event study regressions that separately estimate interactions between year and an indicator for having above median share of eligible Hispanics, and 95% confidence intervals are provided for reference. The vertical dashed line indicates the enactment of DACA, and year 2011 is the omitted category. The results are weighted by the average number of Hispanics aged 14 to 18 in the county in the 2005-2011 ACS. Standard errors are clustered by county and adjusted for three additional degrees of freedom.

Figure C.4: Effect of DACA on CAHSEE ELA Exam, 12th Graders



Notes: Data are from California's Department of Education, and cover years 2005–2015. The sample includes county-level hispanic test takers. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step performs estimation on the de-trended data. Each point represents coefficients from event study regressions that separately estimate interactions between year and an indicator for having above median share of eligible Hispanics, and 95% confidence intervals are provided for reference. The vertical dashed line indicates the enactment of DACA, and year 2011 is the omitted category. The results are weighted by the average number of Hispanics aged 14 to 18 in the county in the 2005-2011 ACS. Standard errors are clustered by county and adjusted for three additional degrees of freedom.

D Results with Inverse Propensity Score Weighting

Table D.1: Pre-DACA Characteristics of Hispanic Treatment and Comparison Groups Using Inverse Propensity Score Weighting, Ages 14-18

	Sample Weights		Propensity Score Weights	
	(1) Eligible	(2) Ineligible	(3) Eligible	(4) Ineligible
Female	0.48	0.50	0.48	0.48
Current Age	15.92	16.10	15.96	15.98
Age at Immigration	5.21	3.82	4.89	5.04
Year of Immigration	1997.39	1995.78	1997.04	1997.17
Current Year	2008.12	2008.11	2008.13	2008.13
English Primary Language	0.02	0.17	0.07	0.06
Spanish Primary Language	0.98	0.82	0.93	0.93
Poor English	0.06	0.03	0.05	0.06
Mother in HH	0.14	0.14	0.14	0.14
Father in HH	0.72	0.65	0.70	0.71
Number of Family Members	4.69	4.22	4.55	4.56
Number of Siblings	1.71	1.37	1.60	1.59
Income to Poverty Ratio	1.50	2.16	1.71	1.72

Notes: Data are from the 2005–2011 American Community Survey (ACS). The sample is composed of Hispanic foreign born individuals ages 14 to 18 that immigrated by age 10 and by 2007. Means are weighted by sampling weights in columns (1) and (2) and inverse propensity score weights multiplied by sampling weights in columns (3) and (4).

Table D.2: Pre-DACA Characteristics of Hispanic Treatment and Comparison Groups Using Inverse Propensity Score Weighting, Ages 19-22

	Sample Weights		Propensity Score Weights	
	(1) Eligible	(2) Ineligible	(3) Eligible	(4) Ineligible
Female	0.48	0.50	0.48	0.48
Current Age	15.92	16.10	15.96	15.98
Age at Immigration	5.21	3.82	4.89	5.04
Year of Immigration	1997.39	1995.78	1997.04	1997.17
Current Year	2008.12	2008.11	2008.13	2008.13
English Primary Language	0.02	0.17	0.07	0.06
Spanish Primary Language	0.98	0.82	0.93	0.93
Poor English	0.06	0.03	0.05	0.06
Mother in HH	0.14	0.14	0.14	0.14
Father in HH	0.72	0.65	0.70	0.71
Number of Family Members	4.69	4.22	4.55	4.56
Number of Siblings	1.71	1.37	1.60	1.59
Income to Poverty Ratio	1.50	2.16	1.71	1.72

Notes: Data are from the 2005–2011 American Community Survey (ACS). The sample is composed of Hispanic foreign born individuals ages 19 to 22 that immigrated by age 10 and by 2007. Means are weighted by sampling weights in columns (1) and (2) and inverse propensity score weights multiplied by sampling weights in columns (3) and (4).

Table D.3: Effect of DACA on School Attendance, High School Completion and Fertility, Hispanics – Inverse Propensity Score Weighting

	All	Hispanic	High Take-Up
A: In School, Age 14-18			
Eligible*Post	0.015***	0.018**	0.020**
	(0.004)	(0.008)	(0.009)
Individuals	114453	54015	48359
B: High School Completion, Age 19-22			
Eligible*Post	0.041***	0.083***	0.089***
	(0.007)	(0.010)	(0.008)
Individuals	82077	38690	34759
C: Have No Children, Age 15-18			
Eligible*Post	0.015**	0.015	0.033*
	(0.007)	(0.018)	(0.019)
Individuals	45148	20844	18613

Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. All regressions include flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, and state-by-year fixed effects. The results are weighted by inverse propensity score weights multiplied by sampling weights. Standard errors, shown in parentheses, are clustered by state and adjusted for three additional degrees of freedom. * p<0.10, ** p<0.05, *** p<0.01.