Do Human Capital Decisions Respond to the Returns to Education? Evidence from DACA

Elira Kuka*
Southern Methodist University,
IZA, and NBER

Na'ama Shenhav†
Dartmouth College

Kevin Shih‡
Queens College
City University of New York

June 14, 2018§

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 between 4 and 11 percentage points (p.p.), while teenage births declined by 1.7 p.p. Further, we find that college attendance increased by nearly 10 p.p. among women, suggesting that DACA raised aspirations for education above and beyond qualifying for legal status. We also find that the same individuals who acquire more schooling also work more, counter to the typical intuition that these behaviors are substitutes.

Keywords: Returns to education, schooling, fertility, amnesty, undocumented immigrants
JEL Classification: I20, I26, J13, J1

*Elira Kuka, Department of Economics, Southern Methodist University, Email: ekuka@smu.edu

†Na'ama Shenhav, Department of Economics, Dartmouth College, E-mail: naama.shenhav@dartmouth.edu

‡Kevin Shih, Department of Economics, Queens College, City University of New York, E-mail: kevinyang-shih@gmail.com

§We would like to thank Anna Aizer, Catalina Amuedo-Dorantes, Marcella Alsan, Elizabeth Cascio, Aimee Chin, Chloe East, Hilary Hoynes, Chris Karbownik, Melissa Kearney, Ethan Lewis, Dan Millimet, Matt Notowidigdo, Francesc Ortega, Marianne Page, Bruce Sacerdote, Diane Schanzenbach, and Doug Staiger, as well as seminar participants at Northwestern MPES, University of Connecticut, OSU, the US Census Bureau, and conference participants at the Barcelona GSE Summer Forum Migration meeting, UC Davis Alumni Conference, WEAI Conference, SEA Conference, and SOLE Annual Meeting for helpful suggestions and feedback. We are also grateful to Alex Magnuson for excellent research assistance.

1 Introduction

The canonical model of human capital predicts that individuals respond to returns to education, as with any investment (Becker, 1964). However, even as the earnings gap between college- and non-college educated workers continues to grow, the high school and college completion rate of many communities remains low.[1] Undocumented youth, who account for 1.5% of the population of US minors, stand out in particular in this regard, with between 15% and 40% of young adults not having completed high school.[2] Survey responses of this population suggest that the absence of legal status may inhibit investments in education (Wong et al., 2016), but also at play may be liquidity constraints, high opportunity costs of schooling, or misperceptions of the returns to education.[3] In this paper, we examine how the availability and design of legalization policies impact youth investments in human capital. As countries grapple with an increasing presence of non-permanent residents like the undocumented population, it is critical to discern how the design of legal protections shapes the education of the labor force.

We take advantage of an ongoing policy "experiment" in the US, the institution of the Deferred Action for Childhood Arrivals program (DACA), which granted temporary legal status for undocumented youth and in doing so provided a salient shock to the returns to education. Enacted in August 2012, DACA extended temporary relief from deportation and work authorization – two years, initially, subject to renewal – to undocumented youth that were in school or had completed high school, and met other criteria based on age and year of arrival. DACA receipt thus generates a discrete increase in the benefits associated with completing high school and higher education. By lowering the deportation risk nearly to 0 – from roughly 7.3 (1.5) percentage points for men (women) in a year – DACA increases the number of years an individual could expect to earn US wages.[4] Additionally, work authorization provides access to higher wages and possibly greater returns to education earned by those with legal status.[5]

Undocumented youth are a population of interest for several reasons. First, similar to

[1] See, e.g. Bailey and Dynarski (2011); Murnane (2013).

[2] Estimates of completed education vary by due to different methods of identifying undocumented youth. A range of estimates are provided in Erisman and Looney (2007) and Passel (2003).

[3] For misperceptions see Bleemer and Zafar (2015); Jensen (2010); Hastings, Neilson and Zimmerman (2015); for liquidity constraints see the review in Deming and Dynarski (2009), and discussion in Section 7.

[4] We calculate deportation risk as the total number of deportations over our estimated number of undocumented individuals, for ages 18 to 39. See Section 2 for details.

[5] Borjas (2017); Bratsberg, Ragan and Nasir (2002); Rivera-Batiz (1999); Kossoudji and Cobb-Clark (2002) empirically estimate the labor market effects of legalization utilizing cross-sectional differences and exploiting the 1986 Immigration Reform and Control Act (IRCA), and find that legalization raises wages between 6 to 14 percent and raise the wage returns to years of schooling.

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, and reduced access to credit markets (Amuedo-Dorantes and Bansak, 2006; Osili and Paulson, 2009). Understanding the role of expected benefits of schooling for this population 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 returns could improve a constellation of behaviors (Borjas, 2017). Third, given the large size of the migrant and refugee population both in the US and in Europe and the intense policy debates on immigration reform, fully understanding the effects of DACA is crucial for evaluating the benefits and costs of future immigration policies.

We navigate several empirical challenges to identify the causal response to DACA. First, there are no available data over this period that contain 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 US citizenship combined with country of origin as a second-best measure of undocumented status.[6] 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. 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. We then demonstrate that a large set of observable characteristics do not predict a differential

[6] Related work by Kaushal (2006), Pope (2016) and Amuedo-Dorantes and Antman (2016) similarly use non-citizen Hispanic or Mexicans to approximate the undocumented population.

improvement in schooling of the eligible population after DACA. As a result, our findings 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, fertility, and work decisions. Our preferred estimates for 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, with larger point estimates for Hispanic men. Our results imply that more than 49,000 additional Hispanic youth obtained a high school diploma because of DACA, and that the gap in high school graduation between citizens and non-citizens narrowed by 40%. Corresponding to these effects on teenage schooling, we also find that young women between the ages of 15 and 18 also were 1.7 p.p. less likely to give birth after DACA.

Moreover, despite the fact that post-secondary schooling was not required for DACA, 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 also exhibit increases in schooling. 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 substitutes. Eligible individuals show a marked decline in being "idle" – neither in school nor working – indicating that the program generated a large boost in economic activity.

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 DACA may have induced the undocumented to exert greater schooling effort, as demonstrated by a higher share of high school students passing a mandatory high stakes test for graduation (the California High School Exit Exam). This allows us to rule out that the increases in schooling that we observe were only a response on the part of teachers or schools to graduate more students after DACA. 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.

Why are youth so responsive to DACA? Overall, the data are most consistent with youths responding proportionally to the magnitude of the expected lifetime benefits of DACA, which we calculate using estimates of the reduction in the sex-specific average risk of deportation, skill-specific earnings in the US and abroad, and the difference between legal and non-legal earnings in the US. In particular, while the point estimates for high school completion appear larger for men than women, scaling by the estimated wage benefits of DACA reveals elasticities of high school that are quite similar for men and women (0.11), and comparable to estimates from other studies (Black, McKinnish and Sanders, 2005; Abramitzky and Lavy, 2014). The elasticity of college attendance of women (0.67) is noticeably larger than that of men (0.15), which is consistent with the college-going decision of women being more sensitive than men (Carrell and Sacerdote, 2017; Evans et al., 2017; Angrist, Lang and Oreopoulos, 2009). Moreover, we show that our effects are not concentrated among youth who live in states with very high baseline deportation rates, suggesting that the response was not solely driven by immediate risks of deportation.

Our findings speak to central questions in education and immigration policy. First, we provide compelling evidence that a large share of the gap in high school graduation, college attendance and teenage pregnancy of undocumented youth is attributable to the uncertain and limited returns to schooling. Previous papers show increases 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, responses to higher wage returns in the future would not necessarily mirror these effects, since obtaining a degree, unlike dropping out, requires individuals to put forth effort, patience, and be sufficiently forward-looking (Oreopoulos and Salvanes, 2011). 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).[7] 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 youth.

We also provide novel evidence of the response to a *conditional* and potentially *temporary* amnesty, whereas the majority of the literature focuses on *unconditional* amnesties, such as IRCA. Unconditional amnesties have been shown to improve labor market outcomes (Rivera-

[7] Changing the cost of college, and, hence, the returns, through financial aid has also been found to increase post-secondary attainment; but there has been little evidence that aid enters into longer-term planning, such as by affecting the high school graduation decision (Deming and Dynarski, 2009).

Batiz, 1999; Kossoudji and Cobb-Clark, 2002; Kaushal, 2006; Steigleder and Sparber, 2017), decrease crime (Pinotti, 2017), and increase EITC receipt and payroll tax payments (Cascio and Lewis, 2016; Monras, Vázquez-Grenno and Elias Moreno, 2017). Among this literature, Cortes (2013) is the only study, that we are aware of, that studies the effects of an unconditional amnesty (IRCA) on education,[8] and finds that college attendance substantially increases with amnesty. However, the conclusions drawn in that study are difficult to interpret, as potential biases from changes in selection into migration, or differences in respondent age and age of immigration pre- and post-IRCA remain unresolved.[9] We identify and address a variety of threats to estimation, allowing us to document that the legal protections of DACA encouraged individuals to undertake human capital investments, even when the duration of these protections remained uncertain.

Relative to previous research on DACA, we show that the program had significant 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). However, in light of our findings that DACA strongly increases high school completion, a potential concern is that these findings could be contaminated by selection bias. 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, an age group that we show is 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.

Further, our research enriches past work that has shown links between mandatory school-

[8]Additionally, Liscow and Woolston (2016) and Felfe, Rainer and Saurer (2016) analyze the impact of citizenship on childhood and teenage schooling by exploiting variation in sibling citizenship within mixed-citizen families in the US and the introduction of birthright citizenship in Germany, respectively. However, the effect of citizenship may be quite different than a temporary or permanent amnesty, and mixed-citizen families are not necessarily representative of all undocumented youth.

[9]These issues stem from using year of immigration for variation in IRCA eligibility, which has a mechanical relationship with age and age of immigration. More generally, it is difficult to find a credible design to study the causal effect of IRCA on education with available data. After the policy went into effect, many of the legalized immigrants became citizens (Cascio and Lewis, 2016), which then makes this group difficult to distinguish in survey data from immigrants that have legal status for other reasons. Additionally, there are no large datasets that track the outcomes of formerly undocumented youth immediately before and after the policy.

ing requirements and fertility decisions (McCrary and Royer, 2011; Black, Devereux and Salvanes, 2008; Geruso and Royer, 2018). Our results show that fertility also 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 at least in part a reaction to a lack of incentive to remain in school (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. Sections 6 and 7 presents results on schooling attendance, fertility, and working, and Section 8 provides evidence on exit exam performance and sexual behavior. We include sensitivity exercises in Section 9, discuss implied schooling elasticities in Section 10, and conclude in Section 11.

2 Institutional Background and Take-up of DACA

Prior to DACA, there were multiple attempts at federal legislation to create a unifying policy for undocumented youth, frequently referred to as DREAMers (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, with the first applications being accepted in August 2012.

DACA provides two types of benefits to recipients. First, deportation is deferred for two years initially, allowing beneficiaries to reside legally in the US. Since there are no available estimates of the deportation risk for undocumented youth, we try to approximate the size of this benefit using tabulations of removals for the population between age 18 and 39 by sex in 2012 from the Department of Homeland Security (Simanski and Sapp, 2012). On average, the annual deportation risk is 5%, however, there is significant variation in the risk across sex, 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.[10] It is worth noting that for both men and women, the perceived

[10] Tabulations on deportations from 2011 would be ideal, but only more aggregate statistics were available for that year. The overall annual deportation risk is calculated as 341,448 removals divided by an estimated population of 6.6 million (56%) of 11.9 million undocumented immigrants. Deportation rates by gender are calculated as the rate of 18 to 39 removals (81.4%) times the share of male (female) deportations, 89.3%

risk of deportation is likely to be much higher than the actual risk. While we estimate the aggregate risk to be 5%, a recent survey found that 59% of foreign-born Hispanics are somewhat or significantly concerned about the risk of deportation (Lopez et al., 2013).

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. During our sample period, 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 program would persist beyond two years.[11] Efforts to expand the reach of DACA, though never passed, could have further added to expectations of the program's longevity.[12]

DACA applicants must meet a suite of immigration, education, and criminal requirements for approval, in addition to paying a $465 fee. The first set of requirements are based on age and date of arrival in the US and age at the time of DACA's enactment. We use these criteria to determine treatment status in our empirical analysis: (i) under 31 by June 15, 2012, (ii) entered the US 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 as a condition for our analysis sample since young teenagers may age into eligibility. Second, applicants are not eligible if they have been convicted of a serious crime. Third, applicants must currently be in school, have graduated high school or obtained a general education development (GED) certificate.[13] We do not use these last two criteria to determine treatment as they are potential outcomes of the program.

US Citizenship and Immigration Services (USCIS) began accepting applications for DACA

(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).

[11] 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). Note that 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.

[12] 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.

[13] Applicants may substitute veteran status for this requirement, though in practice this seems to be rare, as a survey of DACA recipients revealed 100 percent had at least a high school diploma (2.9% did not respond to the question) (Wong et al., 2016).

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.[14] However, ongoing challenges in court, which continue to be under way as of the writing of this paper, have resulted in a continuation of renewals.

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 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.[15]

3 DACA Incentives for Education and Fertility

To motivate our empirical analysis, we utilize a simple human capital investment framework to examine how DACA might impact high school completion, college attendance, and childbearing of undocumented youths. For brevity, we present the basic intuition here and include further details in Appendix Section C. The key insight of the framework is that human capital decisions for undocumented youth are not only a function of the wages they face in the US; they also reflect the risk of deportation and the wages they face if deported.[16]

We consider the schooling decision of an undocumented young adult in his final year of high school prior to DACA. If he leaves school, he will work full time and obtain an

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

[15] 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).

[16] Similar models that examine human capital decisions under uncertainty include Altonji (1993), Altonji, Blom and Meghir (2012) and Jayachandran and Lleras-Muney (2009).

annual wage corresponding to his current legal status and schooling level.[17] Given that he is undocumented, he will earn a lower wage relative to a worker with legal status that has the same skill level because of an unauthorized "wage penalty."[18] If he remains in school, he foregoes current earnings, but will earn a higher wage with every year of additional schooling completed. Since undocumented youth are in the US illegally, he may be deported at any time, in which case he would be required to return to his country of origin, and would instead work there.

Given these options, he chooses the level of education - dropout, high school graduate, or some college - that maximizes his lifetime expected earnings. For each level of education, this is given by the discounted sum of the US wages earned over the years in the US after completing his schooling and the discounted total earnings of in his country of origin earned over the remainder of his working years. His optimal education choice will therefore depend on the wage for each schooling level in the US, the wages in his country of origin, and how many years he expects to remain in the US.

By conferring legal status and reducing the risk of deportation, DACA raises the expected wage earned in the US – from the non-legal to the legal wage – and increases the expected number of years in the US. Since eligibility depends on having a high school diploma, this generates a discrete jump in the earnings function when one attains a high school degree. The size of this jump increases proportionally to the reduction in the deportation risk – which extends the number of years one expects to spend in the US – and to the wage gain from remaining in the US, without legal status, instead of returning to one's country of origin.[19] It also increases with the unauthorized wage penalty among high-school-educated workers, since this determines the size of the increase in wages when one obtains legal work authorization through DACA. Assuming that the opportunity cost of high school (i.e. the drop-out wage) is unchanged, this generates a clear prediction that high school completion will increase.

In addition, DACA increases expected college earnings, but since the opportunity cost of college also rises, as discussed above, the effect on the *return* to college and college attendance is ambiguous. All else equal, the lifetime return to a college education will increase after DACA if the unauthorized wage penalty for college-educated workers is larger than for high-

[17]In our data, 65% of young (age 16 to 22), dropout, Hispanic non-citizen males were employed, typically working 30 hours per week, prior to DACA; similar women have lower rates of working and hours of work.

[18]See, Rivera-Batiz (1999); Kossoudji and Cobb-Clark (2002); Borjas (2017), among others.

[19]Conditional on education, Clemens, Montenegro and Pritchett (2016) show the PPP-adjusted wages are higher in the US compared to 42 developing countries (Table 5 of the paper). They do not analyze the wage differential for undocumented workers, but given the costs of migration, it is not unreasonable to think a similar principle would hold.

school-educated workers, which has been documented in cross-sectional data (Borjas, 2017),[20] It could also increase if the return to college in the US is higher than in the country of origin, since DACA ensures that more years will be spent in the US reaping those returns.[21] If these conditions are satisfied, DACA should encourage college attendance.

Changes in schooling decisions then could have direct effects on fertility, as evidence suggests that greater economic opportunity may in fact be the most important factor for reducing teen pregnancy (Kearney and Levine, 2014, 2012). In raising the economic benefit of completing high school, DACA also significantly increases the opportunity cost of a teenage birth. If fertility is responsive to these benefits, as we hypothesize, then DACA will cause teenage fertility to decline.

While we have thus far focused on the benefits of DACA that arise through expected wages, we acknowledge there are likely significant non-pecuniary benefits of DACA. For example, undocumented youth may have an incentive to finish high school just to avoid deportation, as they may strongly identify as Americans and have a preference for remaining in the U.S. (Vargas, 2012). Moreover, while completing high school young women may learn "safe sex" practices or have reduced opportunities to engage in risky behavior, including sexual activity, often referred to as an "incapacitation" effect (Black, Devereux and Salvanes, 2008). Also, relief from deportation may reduce urgency of having children to guarantee their US citizenship. Note, however, that if the non-pecuniary benefits of avoiding deportation were the only incentives of DACA, observing any effects on the college margin would be unlikely.[22]

4 Data

We use data from the IPUMS ACS (Ruggles et al., 2017) 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 US population. Included among these variables are year of immigration and citizenship status, which we use together with

[20] Cross-sectional estimates suggest legal status raises the returns to education by 1.8% (Borjas, 2017; Rivera-Batiz, 1999).

[21] While in general the *percent* increase in earnings for an additional year of education are often higher in the origin countries of the DACA-eligible population (Hanson, 2006; Clemens, Montenegro and Pritchett, 2016), the *level* increase in earnings is almost certainly much higher in the US. We show this is the case for Mexico in Appendix Section C.

[22] Avoiding deportation could lead to increased college only if remaining in high school through graduation provided new information that the net benefits of college were larger than previously believed.

year of birth to determine eligibility for DACA, as discussed in Section 5.[23] 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 US, 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 all states and accounts for nearly 30% of DACA-recipients, 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 utilize variation across counties in the share eligible among the Hispanic youth population and focus on the county average outcomes of Hispanic students and from the full span of the CAHSEE, which began in 2006 and was retired in 2016.[24]

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 of high school students, and asks about sexual behaviors, alcohol and other drug use, tobacco use, unhealthy dietary behaviors, and inadequate physical activity. To obtain state-representative estimates, we obtained data for 22 states that separately administered the survey from 2005 to 2015.[25] Since the survey is only given to high school students, we note that the composition of students that respond may be affected if DACA raises the share of youths in school. However, this type of selection

[23] 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 misinterpretation is not discontinuous after 2012.

[24] 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.

[25] 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.

would likely bias our results towards finding increased risky behaviors.[26]

5 Empirical Strategy

The infeasible, ideal identification would randomly assign eligibility for DACA within undocumented youth, and then track a rich set of subsequent outcomes after the enactment of DACA. In this spirit, we identify the effect of DACA in the ACS by comparing the education and fertility outcomes of DACA-eligible immigrant youth to the outcomes of non-eligible immigrant youth using a difference-in-differences strategy. We first sketch the outline of this approach and later discuss several refinements that we make to achieve cleaner identification. Our difference-in-difference framework is implemented with the estimating equation

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

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 sex, 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.[27]

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 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 will perform 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

[26]Other data sources like the National Survey of Family Growth also contain information on sexual behavior and responses are not contingent on being in school, but there are too few teens in the survey to provide power for our analysis.

[27]As an alternative approach to constructing p-values, we follow Agarwal et al. (2015) and Conley and Taber (2010) and compare our estimates of the effect of DACA with placebo policies implemented at randomly specified years. Since in our main analysis we have four "treated" years, we assign a placebo DACA policy to four randomly chosen years drawn without replacement, and the remaining seven years serve as the pre-period. The results, shown in Appendix A, confirm that our effects on schooling are statistically significant.

qualitative results are unchanged by including these trends, as discussed in Section 9.2 (see Appendix Table A.4 and Appendix Figure A.6).

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 outcomes of 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 who arrived by age 10 and by year 2007. In what follows we motivate how this focus helps us obtain identification.[28]

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 include green card holders and temporary visa holders. This causes our estimated effects of DACA eligibility to be a "scaled-down" estimate of the true intent to treat (ITT) effect due to the possible inclusion of individuals who are not undocumented.

To estimate the scope of this mismeasurement, we perform back-of-the-envelope calculations to obtain the share of non-citizens – particularly non-citizen youth – that are undocumented. Source numbers are in Sections 10 and Appendix B. This yields that 55% of all US non-citizens and 45% of young adults between the ages of 18 and 24 are legal residents. As a result, our difference-in-difference estimates for the whole sample are likely to under-estimate the true effect of DACA by 45%.

[28] Our focus on youth always satisfies the eligibility criteria that individuals must be under 31.

To get closer to the true ITT effect, we separately analyze groups that have a higher share of undocumented individuals among those that we assign eligibility. We first analyze treatment effects among Hispanics. Our best estimates suggest that Hispanics comprise 78% of undocumented immigrants and that 72% of all Hispanic non-citizens are undocumented, which, given our earlier estimates, is likely to be a lower bound on the share undocumented among non-citizen Hispanic youth. Second, we analyze individuals from countries that have a DACA take-up rate above 30% ("high take-up"), which, due to the overlap with the Hispanic subgroup, have a high share of undocumented, but may have greater familiarity with DACA.[29] 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.

In Section 8, we use a complementary approach which assigns eligibility at the geographic area, and therefore does not rely on the demanding requirement of accurately assigning individual eligibility. Additionally, we utilize administrative data on local school enrollment and testing, which improves our measurement of schooling outcomes.

5.1.2 Age of Arrival

In addition to undocumented status, DACA also required individuals to have arrived in the US 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 who arrived in the US 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 incen-

[29] 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).

tives 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 who arrived between the ages of 11 and 16.

5.2 Comparison Group

We select the comparison group based on two criteria. First, we limit the sample to individuals born outside of the fifty US states 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 who arrived before age 10 and before 2007, matching the composition of eligible individuals. Our control group is thus composed of immigrants who arrived by 2007 and by age 10, and are ineligible for DACA due to current legal status (proxied by citizenship). For subgroup analyses, we stratify the comparison group along the same dimensions as the treatment group.

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 US 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 comparison group are more likely to have health insurance coverage, English fluency, and parental college, and are less likely to be in poverty; but are also more likely to have a single mother and similarly likely to have had a recent birth.

Importantly for our identification strategy, these characteristics do not predict changes in schooling after DACA. Moreover, using propensity score methods to generate balance in demographics across our treated and comparison groups does not alter our results – see Sections 5.3 and 9.4 for more detail.

As a final note, we point out that while other youth immigrants could feasibly serve as a comparison group, the schooling behavior of such immigrants is vastly different than eligible population. Appendix Figure A.2 shows that other ineligible immigrants, namely non-citizens not meeting the age and/or year of arrival criteria, each exhibit different trends in schooling than eligible immigrants. This reinforces our intuition that the age and year of arrival of immigrants strongly influence school attendance and that the comparison group should match these characteristics of the eligible population.

5.3 Descriptive Evidence and Validity of Identification

Our identification relies on the assumption that in the absence of DACA, the comparison youth (citizen immigrants) would have exhibited similar trends to the treated group (non-citizen immigrant youth).[30] We provide two pieces of evidence in favor of this assumption. 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 the covariates do not predict an increase in schooling after 2012. We demonstrate this by regressing outcomes (school attendance and high school completion) on a large number of demographic characteristics for the 2005-2011 pre-period.[31] We then generate fitted values for the whole period. By pooling these predictors together, instead of testing them individually, we avoid multiple hypothesis testing, give appropriately more weight to factors that are more predictive of schooling, and increase the power of our test.

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 will not reflect a change in the *composition* of the sample, but rather a change in *behavior* among non-citizens relative to citizens.

Second, 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.

Nonetheless, there are a few years in which the trends in the groups appear to deviate, such as the relative increase in non-citizen high school completion between 2010 and 2011. This would be a concern for our identification if the reason that the groups diverged was

[30] We also require there to be no other simultaneous policies targeting undocumented youth. We have not come across any such policies.

[31] We divide these characteristics into two subsets. The first includes indicators for age, race, sex, 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.

due to unobservable factors, such as changes in preferences. However, as can be seen in Figure 2, the 2010-11 rise in non-citizen high school graduation is predictable by our set of observable characteristics. This gives us further confidence that, once we condition on individual covariates, the schooling trends of non-citizens and citizens evolved in parallel prior to DACA.[32]

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 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. To give a sense of the magnitude, these point estimates are slightly higher than the within-sibling effect of citizenship status on school attendance (Liscow and Woolston, 2016). 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

[32] As another example, while Panel (b) shows high school graduation rates among non-citizens fall relative to citizens between 2009-10, this reflects unexpectedly negative selection into the survey in that year (see Panel (b) of Figure 2), which we control for in the event studies. Once controls are included there is essentially no change in the gap in Hispanic high school graduation between 2009-10 or 2010-11.

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.[33] 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.[34] 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

[33] 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.

[34] In Appendix D, 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 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.

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 - which is roughly the same when we adjust for observable characteristics - narrowed by 40%.

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.4 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).

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 offer an incomplete picture of the effect on post-secondary schooling. For instance, Pope (2016) measures no effect on average school attendance, but studies an older population of high school graduates, ages 18 to 35, and shows evidence of differential pre-trends for this outcome. Hsin and Ortega (2017) find increases in dropping out of four-year colleges within a set of urban universities, but they do not study entry into college. Instead, our strategy hones in on young adults – whose schooling decisions appear to be more malleable to this intervention – and captures the total effect of DACA on the college attendance, which includes increased college-eligibility from high school graduation, enrollment conditional on high school graduation, and dropping out.

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 3 provides estimates for all, Hispanics and the high take-up sample, and we include the corresponding event studies for Hispanics in Figure 6.[35] 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. We note that the 95% confidence interval allows for an effect as small as 0.5 p.p., a 14 percent effect, which is still an economically meaningful reduction. 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 "incapacitation" 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 22, 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 reflect an intertemporal substitution in order to complete high school, consistent with an incapacitation effect. The absence of an increase in childbearing among women ages 19 to 22 provides weak evidence against this form of short-term substitution, 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 cannot 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 4 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

[35] The event studies for the high take-up sample look very similar and therefore for brevity are included in the Appendix, Figure A.5.

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 Heterogeneous Responses to DACA

Having demonstrated a strong educational response to DACA in line with our expected effects, we now intend to test three more nuanced hypotheses: (1) Given the stark differences in deportation risk across men and women, does the schooling of men rise more than the schooling of women? (2) Do educational responses align with local deportation risk? (3) Are schooling costs a factor in college attendance?

7.1 Differential Effects on Men and Women

The effects for high school and college attendance are quite distinct when we stratify the sample by sex. Table 5, 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.[36] These differences are quite striking, and warrant further investigation.

7.1.1 Opportunity Cost of High School and Returns to Legal Status

There are several possible explanations for these gender patterns. First, these effects are consistent with the disparity across men and women in the national deportation risk, which in turn affects the returns to schooling. Another possibility is that the opportunity cost of attending high school for male dropouts is lower than for women. If this was the case, we might expect that men would substitute from relatively low-opportunity-cost activities (leisure or idleness) towards high school. We would expect to see a smaller decline among men in the likelihood of dropping out to work, since average annual earnings are 2.6 times larger for males ($6,005) than females, assuming that leisure is valued similarly across sexes.[37]

Figure 7, which presents difference-in-difference estimates for men and women, suggests this is an unlikely explanation. It shows that DACA reduced the likelihood of being idle

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

[37] Annual earnings estimated using Hispanic non-citizens in the ACS. See Section 10 for more detail.

by roughly 5 percentage points for both men and women, but only men also reduced the likelihood of working as dropouts. Hence, men perceived the gains of DACA to be valued at least as highly as the lost wages from less time in the labor market, while women were drawn primarily from non-market work or leisure into schooling. Further analysis shows that these effects are not driven by men that in the absence of DACA would have been employed in a subset of occupations or industries, such as construction, agriculture, cleaning, and food preparation. This is more consistent with men obtaining greater benefits from DACA than having a lower opportunity cost. In further support of this, we find that when we take into account the magnitude of labor market benefits from DACA, we obtain similar elasticities of high school for men and women. We discuss the details of this calculation in Section 10.

7.1.2 Explaining College Attendance

But then, why don't men continue to college at the same rates as women? We join a string of recent studies in making the observation that the entry and persistence of women in post-secondary schooling may be more responsive to costs, broadly defined (Carrell and Sacerdote, 2017; Evans et al., 2017; Angrist, Lang and Oreopoulos, 2009). A candidate explanation for this in our setting is that undocumented men experience lower wage returns to completing some college and therefore perceive college to be less important for advancing one's career (Carrell and Sacerdote, 2017). However, even when we condition on the expected returns to college post-DACA, we find that men attend college less than women (estimates in Section 10). There may also be a delay in men's college attendance, which would attenuate our estimates. Figure 7 shows that the impact of DACA on male high school attendance is fairly constant from age 16 to age 23, which are less typical high school ages. The impact on male college attendance is therefore lagged, positive from age 20 on and statistically significant at ages 22 to 23.

7.2 Threat of Deportation

Earlier we hypothesized that youth may remain in school due to the non-negligible national deportation risk, which no doubt shapes the risk perceived by youth. A further question is whether this schooling response varies with the *likely* or *immediate* threat of deportation, which we measure using the deportation rate in one's state of residence. To explore this possibility, we estimate a flexible regression that allows the difference-in-difference coefficient to vary for each state and then plot the coefficients against the pre-DACA deportation rate in the state from Immigration and Customs Enforcement,[38] together with the

[38] Details regarding this data are available in Appendix B.

best-fit line.

Figure B.2 shows no systematic relationship between the state deportation risk and the DACA-induced increase in schooling. For instance, we find similar-sized effects on teenage schooling in Louisiana and Alabama – which have comparable numbers of undocumented youth – despite the fact that we estimate the deportation risk to be over 30 times higher in Louisiana. This result is robust to alternative specifications, such as a linear interaction between the impact of DACA and the deportation rate.

One possible interpretation of this is that undocumented youth are likely to be responding to the *perception* of deportation risk rather than actual risk. This would be consistent with the fact that the prevalence of concern over deportations do not appear to mirror the deportation threat, as we discussed earlier. Another possibility is that youth place little value on the reduction in deportation risk from DACA and instead are responding only to the expected wage increase from legal status. This seems less likely given the stated concerns over deportation, but the available data do not allow us to fully rule this out.

7.3 Affordability of College

Another 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.[39] 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 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*.[40] 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.

[39]While our paper studies human capital responses to changes in the returns to education, related studies have examined whether affordability has affected schooling, specifically in the context of higher education. With respect to undocumented youth, several papers found positive effects on college enrollment from policies granting undocumented students eligibility for in-state tuition subsidies (e.g. Kaushal (2008); Flores (2010); Amuedo-Dorantes and Sparber (2014); Darolia and Potochnick (2015); Koohi (2017)). Note Chin and Juhn (2011) are a notable exception who do not find significant increases in enrollment. Conger and Turner (2017) also find strong declines in degree attainment of undocumented students following a price hike at the City University of New York Colleges.

[40]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).

8 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. This approach is similar to that of Cascio and Lewis (2016), who also utilize variation in the geographic concentration of unauthorized immigrants in the absence of individual-level information on legal status. 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. 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.[41] The geographic unit is the county of school attendance for the California analysis, and the state of residence for the YRBSS analysis.[42] 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.[43] We use the same two-step process for absorbing linear pre-trends.

We first validate this empirical approach by replicating our main results for school at-

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

[42] 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.

[43] 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. Note that dropouts are not counted in the CAHSEE as it only tracks test-takers.

tendance using county-level variation in eligibility in California. Appendix Figure E.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.

8.1 School Performance: Examining California

First, we analyze whether students put forth greater effort in response to incentives. If schools or teachers 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 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 E.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.

8.2 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, and not purely an "incapacitation" effect.

9 Robustness

9.1 Alternative Sample Restrictions

We now test the sensitivity of our main findings to alternative sample selection criteria and refinements. The first column of Table 8 presents our baseline results, for school attendance, high school completion, college attendance and fertility, in Panels (a) to (d), respectively. In columns (2) and (3) we restrict the sample to those who arrived in the US 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 consecutively add other individuals that are not eligible for DACA to the control group. We start by first adding foreign born individuals, including citizens and non-citizens, who arrived in the US after turning 16 in column (4), then we add individuals who arrived after 2007 in column (5). Again, the results are not sensitive to the inclusion of these individuals. While all our analyses thus far have restricted the sample to only foreign born individuals to avoid comparisons with natives, we relax this restriction in the sixth column and include individuals that were born in the US. 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 presence of legal immigrants in the eligible group, we refine our baseline definition of DACA eligibility by removing non-citizens that may be 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 in the last column of Table 8 are similar in magnitude to our baseline effects.

9.2 Accounting for Time Trends

In this section we consider two alternatives to the two-step method we use to control for differential time trends by eligibility status: not accounting for for differential trends by eligibility status and including linear time trends directly in Equation 1. Appendix Table A.4 shows the difference-in-differences estimates across these three different specifications, and Appendix Figure A.6 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 qualitative findings that DACA led to improvements in schooling and fertility are not particularly sensitive to whether or how we account for trends. The event studies for school attendance and high school graduation are particularly robust to these assumptions, as there is no visible trend in the outcomes of interest net of the control variables. We maintain that the two-step detrending procedure is preferable in our setting, however, since it allows us to absorb pre-existing differences across the treatment and comparison groups, but circumvents the concerns raised with including linear trends in the main specification.

9.3 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. If this was the case, we would expect to see a change in observable characteristics, which would be picked up in Figure 2. The event studies indeed show that changes in observables across eligible and non-eligible groups cannot explain our findings.

9.4 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.[44] We then re-estimate our regressions of schooling attendance and high school completion using inverse-propensity score weighting. Summary tables in Appendix Section F 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.

10 Discussion

In this section we convert our difference-in-differences estimates to the intent to treat (ITT) effects of DACA, and use these estimates to obtain the range of elasticities of schooling to lifetime earnings consistent with our results.

First, to recover the ITT effect of DACA we need to rescale our treatment effects to account for the fact that the eligible group includes unauthorized and authorized individuals. We do so using the strategy employed in Pope (2016) applied to our populations of interest. According to the Department of Homeland Security (Baker and Rytina, 2013), there were 1.4 million undocumented youth between the ages of 18 and 24 in 2012. At the same time, there were 2.55 million non-citizens of the same ages in the 2012 ACS (Acosta, Larsen and Grieco, 2014). This implies that 55% of our sample of eligible teens is likely to be unauthorized. Thus, the ITT effect of DACA for the whole sample is approximately 1.8 times as large as our difference-in-difference estimates. A similar calculation for the Hispanic sample, the details of which are in B, indicates that at least 72% of Hispanic youth are undocumented. Thus, the ITT effect of DACA for Hispanics is at most 1.39 times as large as our estimates.

To estimate the elasticity of schooling choices from these estimates, we need to consider what the wage benefits of DACA - both real and perceived - could be. To do this, we calculate the expected life time earnings for each level of education before and after DACA, using the formalization of our conceptual framework in Appendix C. We do not attempt to monetize the non-pecuniary benefits of DACA, such that our wage estimates are thus a lower bound of the total benefits of DACA, which makes the estimated elasticity an upper bound on the sensitivity of schooling to total schooling returns.

We calculate the change in lifetime earnings after DACA for high school and college as the

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

difference in the lifetime earnings for that level of schooling. We abstract from discounting to simpify notation, but account for this in our calculations of earnings, as we discuss in Appendix C. We denote the lifetime earnings before DACA ($\tau = 0$) as ω_0 and after DACA ($\tau = 1$) as ω_1. Lifetime earnings are given by the sum of earnings over the expected number of post-schooling years spent in the US, Y_τ^{US}, and the expected number of post-schooling years spent in country of origin O, Y_τ^O. Wages, w in the US are a function of legal status (n for non-legal/undocumented or ℓ for legal) and schooling level s, while wages in O only depend on s. Hence, we estimate the lifetime earnings before and after DACA are:

$$\omega_0^s = w^{O,s} \cdot Y_0^O + w^{n,s} \cdot Y_0^{US} \tag{3}$$

$$\omega_1^s = w^{O,s} \cdot Y_1^O + w^{\ell,s} \cdot Y_1^{US} \tag{4}$$

We draw on a variety of sources to calculate expected life time earnings before and after DACA, and discuss a range of estimates. To calculate the number of years working in the US, we subtract the cumulative probability of deportation for each age and sex from the total number of working years. We calculate this deportation risk for each age and sex prior to DACA using the number of removals from the US in 2012 (Simanski and Sapp, 2012) divided by the estimated population of undocumented immigrants. We conservatively assume the probability of deportation only declines to 0.5 p.p. after DACA.

We assign the US wages for each legal status and education as the average wages of foreign-born individuals who arrived in the US by age 10 and year 2007, and who are between the ages of 18 and 60 year old individuals in the 2009-2011 ACS. Since the vast majority of the DACA-eligible population was born in Mexico, we proxy the wage in the country of origin as the average wage in Mexico by education and sex calculated from the IPUMS 2010 Mexico Census (Minnesota Population Center, 2018).

Finally, we consider three time horizons over which eligible individuals may have expected DACA benefits to last; 4 years, 6 years, or permanent. Although there is no data on these expectations, we surmise that 4 years may be the minimum expectation, given that the Obama administration was re-elected for a 4 year term soon after the passage of DACA. We summarize these inputs in Appendix Table C.1.

Table 9 shows the elasticity of high school completion and college attendance estimates under each of these scenarios using the rescaled (ITT) schooling estimates for Hispanics. Intuitively, under a four year expectation, the elasticity is large, particularly for college, since the wage benefits over four years are moderate relative to the sizable educational response, especially when we consider that during the first to two years individuals enrolled in college cannot reap the benefits of DACA. Our preferred elasticities rely on six years of duration, the actual duration of DACA. Under these assumptions the elasticity of high school completion is around 0.11, while that of college is between 0.15 and 0.67. The high school estimates are in the ballpark but smaller in absolute value than estimates of the elasticity of high school non-enrollment with respect to dropout wages – -0.54 to 0.72 (Black, McKinnish and Sanders, 2005) – and the elasticity of high school completion in Israeli kibbutzes – 0.44 (Abramitzky and Lavy, 2014). Males and females exhibit similar responsiveness with respect to high school decisions, which means that the differential point estimates are proportional to differences in the change in high-school-educated earnings. The elasticities for college enrollment, instead, are larger for females relative to males, consistent with prior work showing that women are more likely to enroll in college.

11 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 to 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 between 4 to 11 p.p., an effect that was more pronounced among Hispanic men, and reduced teenage births by 1.7 p.p. Further, we estimate that DACA led to a 9.8 p.p. higher rate of college enrollment among young Hispanic women, and had a smaller impact on men's college decisions, at least over the short term that we are able to measure. Working also increased alongside schooling, counter to the typical modeling assumption that assumes these pursuits are mutually exclusive. Auxiliary analyses show that undocumented youth are likely to have increased effort in school, as measured by an increased share passing a high-stakes exit exam, and increased their utilization of pregnancy prevention methods.

These results have significant policy implications. First, they show that a substantial part of the gap in educational attainment between undocumented and citizen youth is due to the low benefits of schooling associated with lack of legal status. Hence, policies that increase real or perceived economic opportunities of disadvantaged youth may lead to a more educated workforce. Second, immigration policy is currently at the center of the public debate, both in the U.S. and in Europe, with many fearing 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 policies that include incentives for education and reduce uncertainty over employment can lead to improvements in each of these areas of concern.

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.

Acosta, Yesenia D, Luke J. Larsen, and Elizabeth M. Grieco. 2014. "Noncitizens Under Age 35: 2010-2012." United States Census Bureau.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel. 2015. "Regulating Consumer Financial Products: Evidence from Credit Cards *." *The Quarterly Journal of Economics*, 130(1): 111–164.

Altonji, Joseph G. 1993. "The demand for and return to education when education outcomes are uncertain." *Journal of Labor Economics*, 11(1, Part 1): 48–83.

Altonji, Joseph G, Erica Blom, and Costas Meghir. 2012. "Heterogeneity in human capital investments: High school curriculum, college major, and careers." *Annu. Rev. Econ.*, 4(1): 185–223.

Amuedo-Dorantes, Catalina, and Chad Sparber. 2014. "In-state tuition for undocumented immigrants and its impact on college enrollment, tuition costs, student financial aid, and indebtedness." *Regional Science and Urban Economics*, 49: 11–24.

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.

Angrist, Joshua, Daniel Lang, and Philip Oreopoulos. 2009. "Incentives and Services for College Achievement: Evidence from a Randomized Trial." *American Economic Journal: Applied Economics*, 1(1): 136–163.

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

Bailey, Martha J., and Susan M. Dynarski. 2011. "Inequality in Postsecondary Education." In *Whither Opportunity?.* , ed. Greg J. Duncan and Richard J. Murnane. Russell Sage Foundation.

Baker, Bryan, and Nancy Rytina. 2013. "Estimates of the Unauthorized Immigrant Population Residing in the United States: January 2012." Department of Homeland Security, Office of Immigration Statistics.

Becker, Gary Stanley. 1964. *Human Capital.* University of Chicago Press.

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.

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.

Chin, Aimee, and Chinhui Juhn. 2011. *Does reducing college costs improve educational outcomes for undocumented immigrants? Evidence from state laws permitting undocumented immigrants to pay in-state tuition at state colleges and universities.* Springer.

Clemens, Michael A., Claudio E. Montenegro, and Lant Pritchett. 2016. "Bounding the Price Equivalent of Migration Barriers." Social Science Research Network SSRN Scholarly Paper ID 2839518, Rochester, NY.

Conger, Dylan, and Lesley J Turner. 2017. "The effect of price shocks on undocumented students' college attainment and completion." *Journal of Public Economics*, 148: 92–114.

Conley, Timothy G., and Christopher R. Taber. 2010. "Inference with Difference in Differences with a Small Number of Policy Changes." *The Review of Economics and Statistics*, 93(1): 113–125.

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

Darolia, Rajeev, and Stephanie Potochnick. 2015. "Educational" When,"" Where," and" How" Implications of In-State Resident Tuition Policies for Latino Undocumented Immigrants." *The Review of Higher Education*, 38(4): 507–535.

Deming, David, and Susan Dynarski. 2009. "Into College, Out of Poverty? Policies to Increase the Postsecondary Attainment of the Poor." National Bureau of Economic Research Working Paper 15387.

Erisman, Wendy, and Shannon Looney. 2007. "Opening the Door to the American Dream: Increasing Higher Education Access and Success for Immigrants." *Institute for Higher Education Policy.*

Evans, William N., Melissa S. Kearney, Brendan C. Perry, and James X. Sullivan. 2017. "Increasing Community College Completion Rates among Low-Income Students: Evidence from a Randomized Controlled Trial Evaluation of a Case Management Intervention." National Bureau of Economic Research Working Paper 24150.

Felfe, Christina, Helmut Rainer, and Judith Saurer. 2016. "Why Birthright Citizenship Matters for Immigrant Children: Impacts on Parental Educational Choices."

Flores, Antonio. 2017. "How the U.S. Hispanic population is changing."

Flores, Stella M. 2010. "State dream acts: The effect of in-state resident tuition policies and undocumented Latino students." *The Review of Higher Education*, 33(2): 239–283.

Geruso, Michael, and Heather Royer. 2018. "The impact of education on family formation: quasi-experimental evidence from the UK." National Bureau of Economic Research.

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.

Hanson, Gordon H. 2006. "Illegal Migration from Mexico to the United States." *Journal of Economic Literature*, 44(4): 869–924.

Hastings, Justine, Christopher A. Neilson, and Seth D. Zimmerman. 2015. "The Effects of Earnings Disclosure on College Enrollment Decisions." National Bureau of Economic Research Working Paper 21300.

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.

Jayachandran, Seema, and Adriana Lleras-Muney. 2009. "Life Expectancy and Human Capital Investments: Evidence from Maternal Mortality Declines." *The Quarterly Journal of Economics*, 124(1): 349–397.

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.

Kaushal, Neeraj. 2008. "In-state tuition for the undocumented: Education effects on Mexican young adults." *Journal of Policy Analysis and Management*, 27(4): 771–792.

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.

Koohi, Shiva. 2017. "College prospects and risky behavior among Mexican immigrant youth: The effects of in-state tuition policies on schooling and childbearing." *Economics of Education Review*, 58: 162–174.

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?"

Lopez, Mark Hugo, Paul Taylor, Cary Funk, and Ana Gonzalez-Barrera. 2013. "On Immigration Policy, Deportation Relief Seen As More Important Than Citizenship."

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."

Minnesota Population Center. 2018. "Integrated Public Use Microdata Series, International: Version 7.0."

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

Murnane, Richard J. 2013. "U.S. High School Graduation Rates: Patterns and Explanations." *Journal of Economic Literature*, 51(2): 370–422.

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

NW, 1615 L. St, Suite 800 Washington, and DC 20036 USA202-419-4300 | Main202-419-4349 | Fax202-419-4372 | Media Inquiries. 2015. "Chapter 5: U.S. Foreign-Born Population Trends."

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. 2003. "Further demographic information relating to the DREAM Act." *Washington, DC: The Urban Institute.*

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."

Passel, Jeffrey S., and DVera Cohn. 2014. "Chapter 2: Birthplaces of U.S. Unauthorized Immigrants."

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, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. 2017. "Integrated Public Use Microdata Series: Version 7.0." *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.*

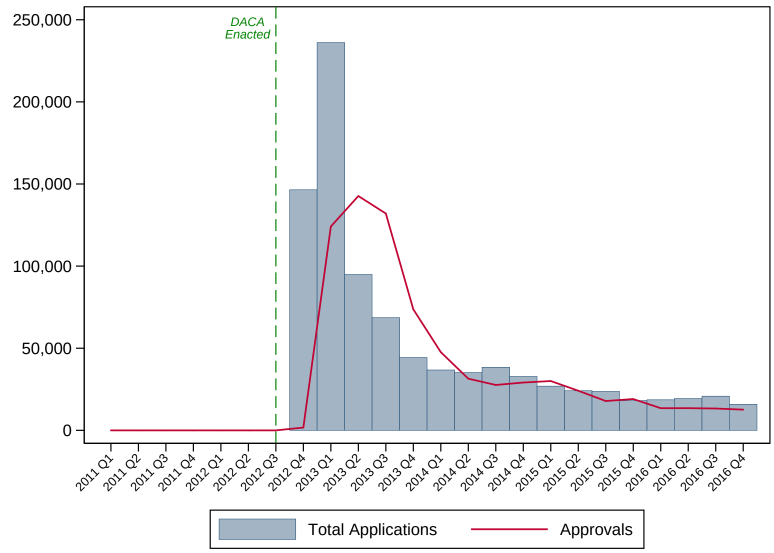
Wiswall, Matthew, and Basit Zafar. 2014. "Determinants of college major choice: Identification using an information experiment." *The Review of Economic Studies*, 82(2): 791–824.

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."

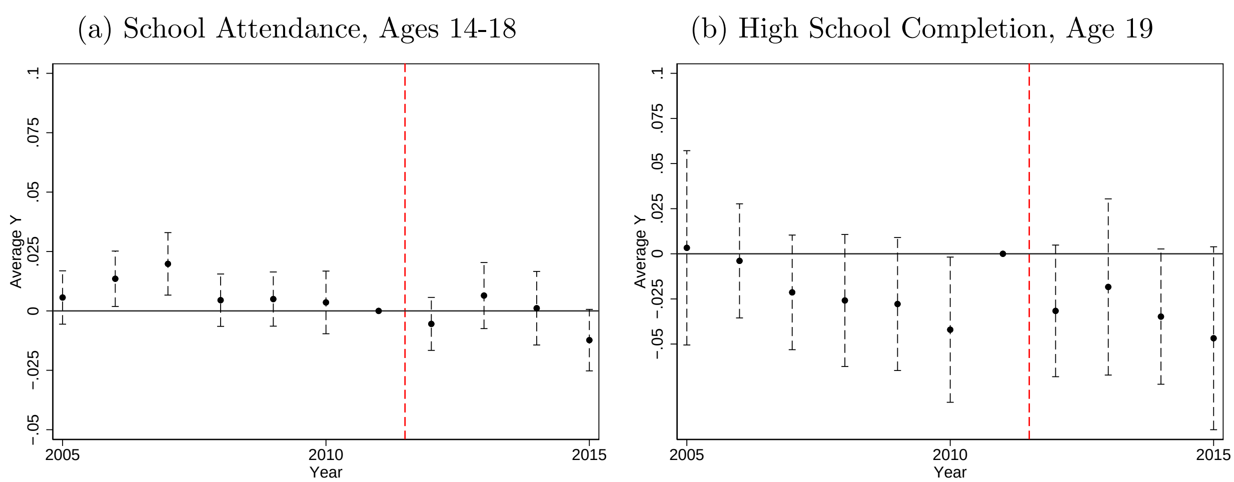
12 Figures

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.

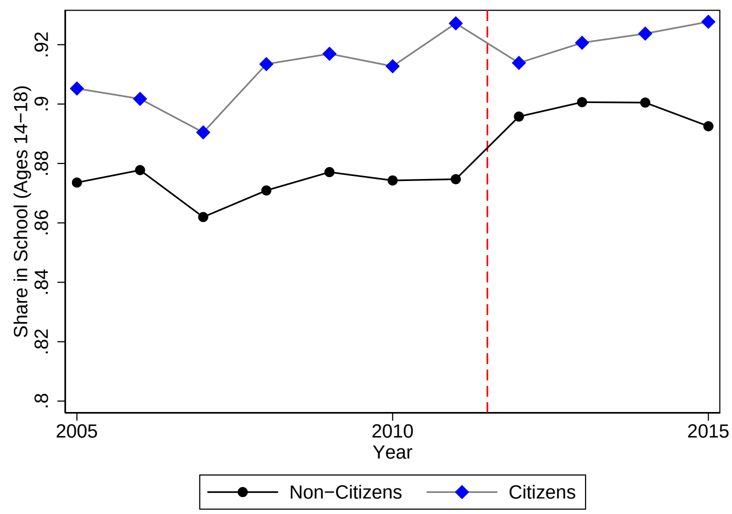
Figure 2: Effect of DACA on *Predicted* School Attendance and High School Completion, Hispanics



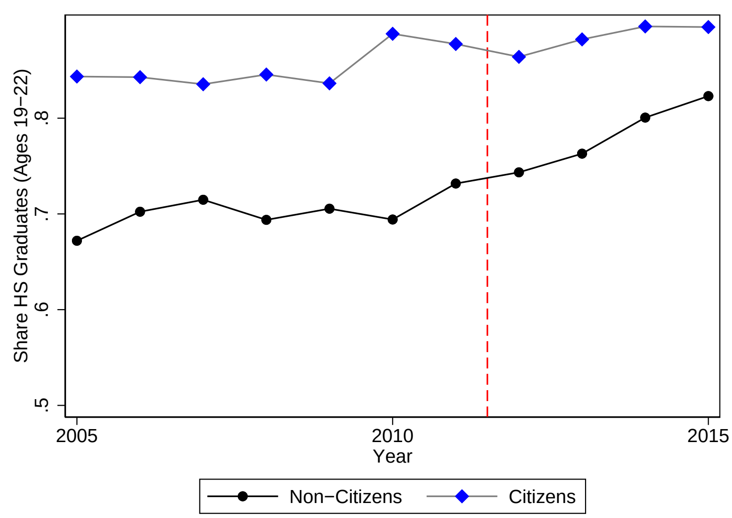
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born Hispanics that immigrated by age 10 and by 2007. Outcomes are the fitted values of likelihood of being in school and high school completion from a regressions on demographic variables using 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 who Arrived by Age 10 and by 2007

(a) Share in School, Ages 14-18



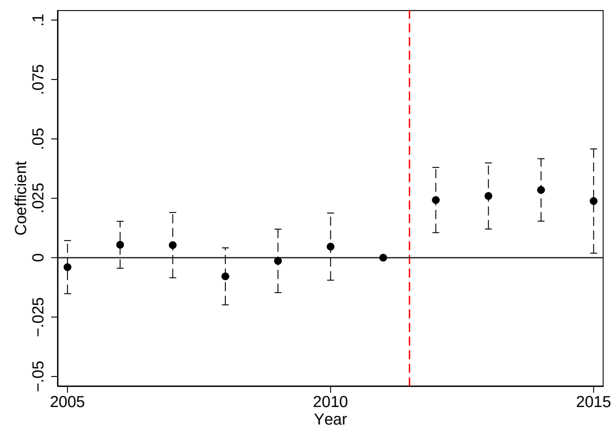
(b) High School Completion, Ages 19-22



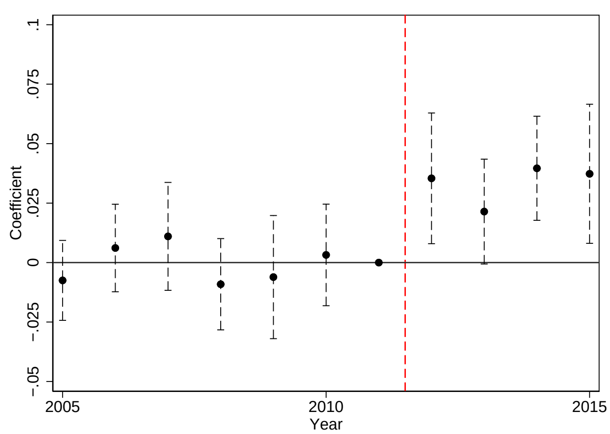
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born Hispanics that immigrated by age 10 and by 2007. This figure shows average school attendance and high school completion rates for Hispanic immigrants ages 14-18 and ages 19-22. The red line demarcates the implementation of DACA.

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

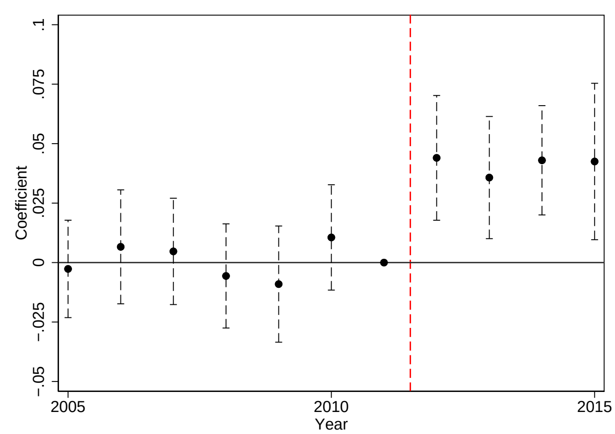
(a) All



(b) Hispanic



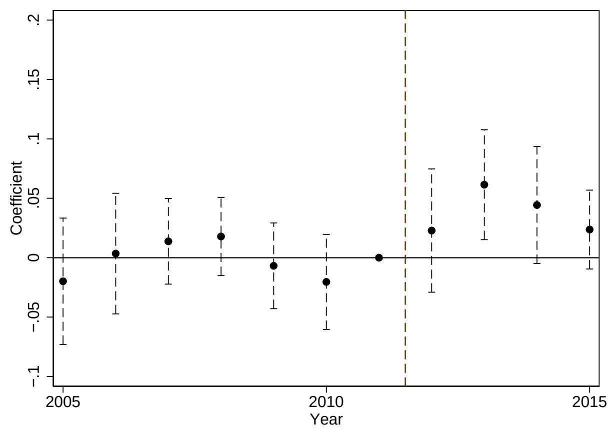
(c) High Take-Up



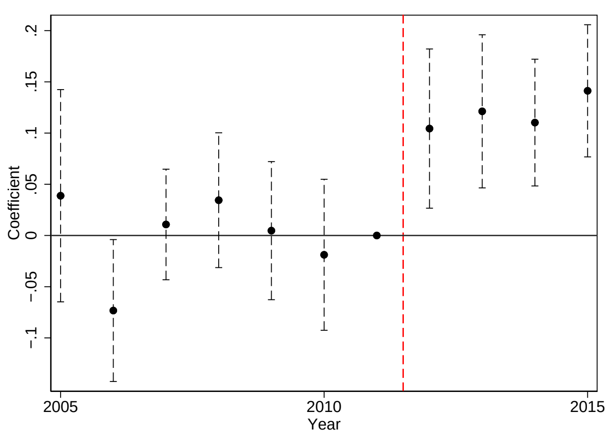
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. Each point represents a coefficient 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. 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 results are weighted by the survey sampling weights.

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

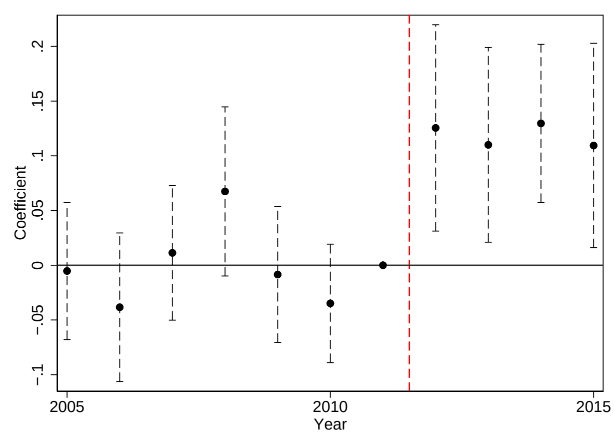
(a) All



(b) Hispanic

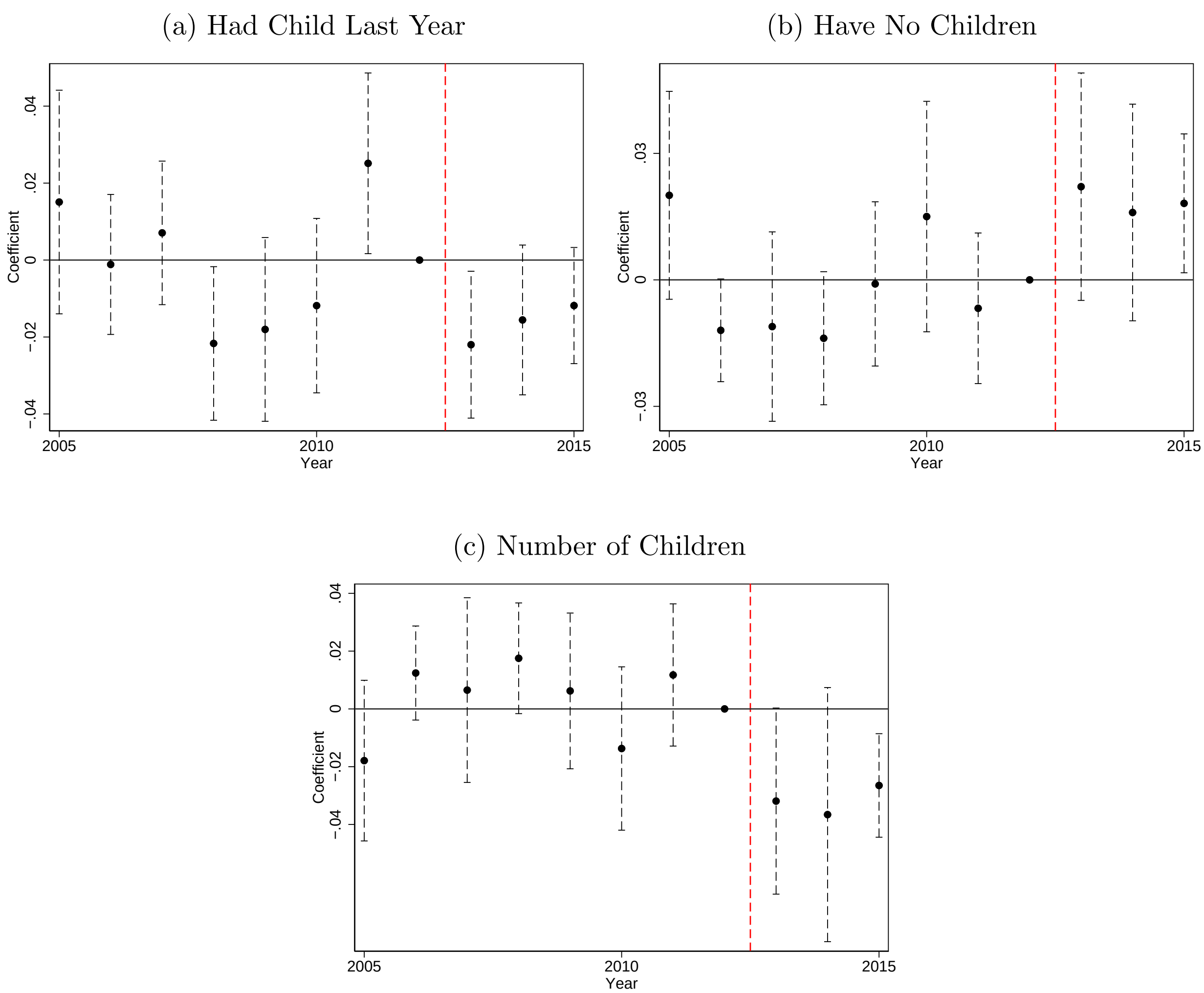


(c) High Take-Up



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. Each point represents a coefficient 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. 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 results are weighted by the survey sampling weights.

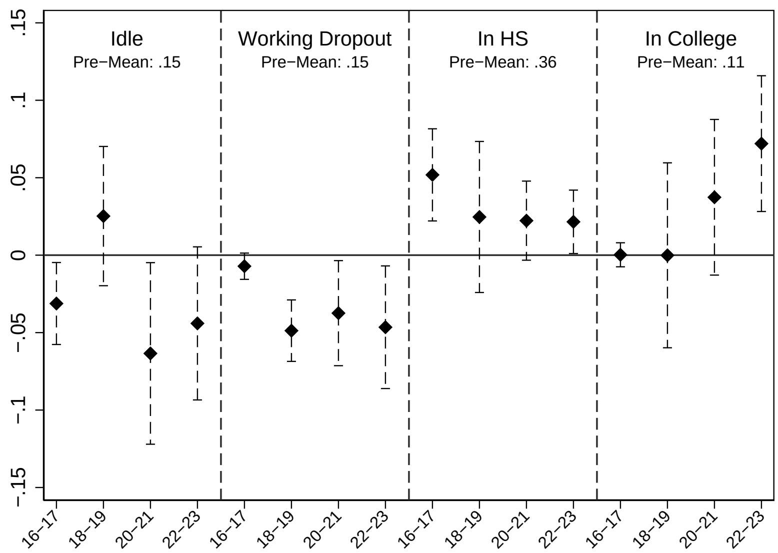
Figure 6: Effect of DACA on Fertility, Hispanics Ages 15-18



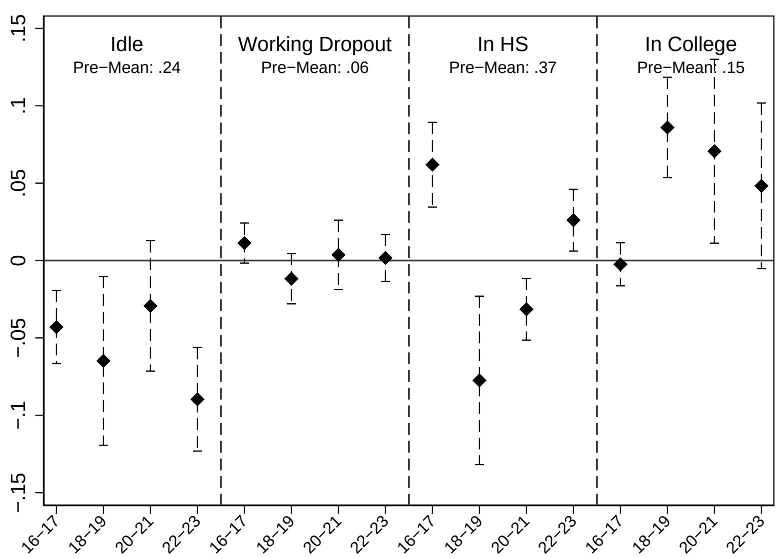
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born females that immigrated by age 10 and by 2007. Note that when analyzing fertility behavior, we consider 2013 and onward to be post treatment years. This is due to the fact that fertility decisions are made 9 months prior to reporting. Each point represents a coefficient 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. 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 results are weighted by the survey sampling weights.

Figure 7: Effect of DACA on Idleness, Dropping out to Work, and Schooling

(a) Hispanic Men



(b) Hispanic Women



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. Each point is the difference-in-difference coefficient from regressions run separately for each two-year age bin and gender. 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. 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 results are weighted by the survey sampling weights.

13 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 de-trended 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 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 4: Effect of DACA on Work and Idleness

	Overall 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 5: Effect of DACA on High School Completion and College Enrollment – By Sex

	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 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 Main Outcomes, 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: College Enrollment, Age 19							
Eligible*Post	0.076**	0.092**	0.089***	0.078***	0.047*	0.011	0.082**
	(0.032)	(0.042)	(0.022)	(0.023)	(0.024)	(0.011)	(0.035)
Mean Y	0.350	0.364	0.297	0.277	0.270	0.396	0.357
Individuals	10252	6932	15131	16823	19316	76508	9515
D: 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 who arrived after age 16; (5) adds foreign born individuals who arrived after 2007; (6) adds individuals born in the US 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.

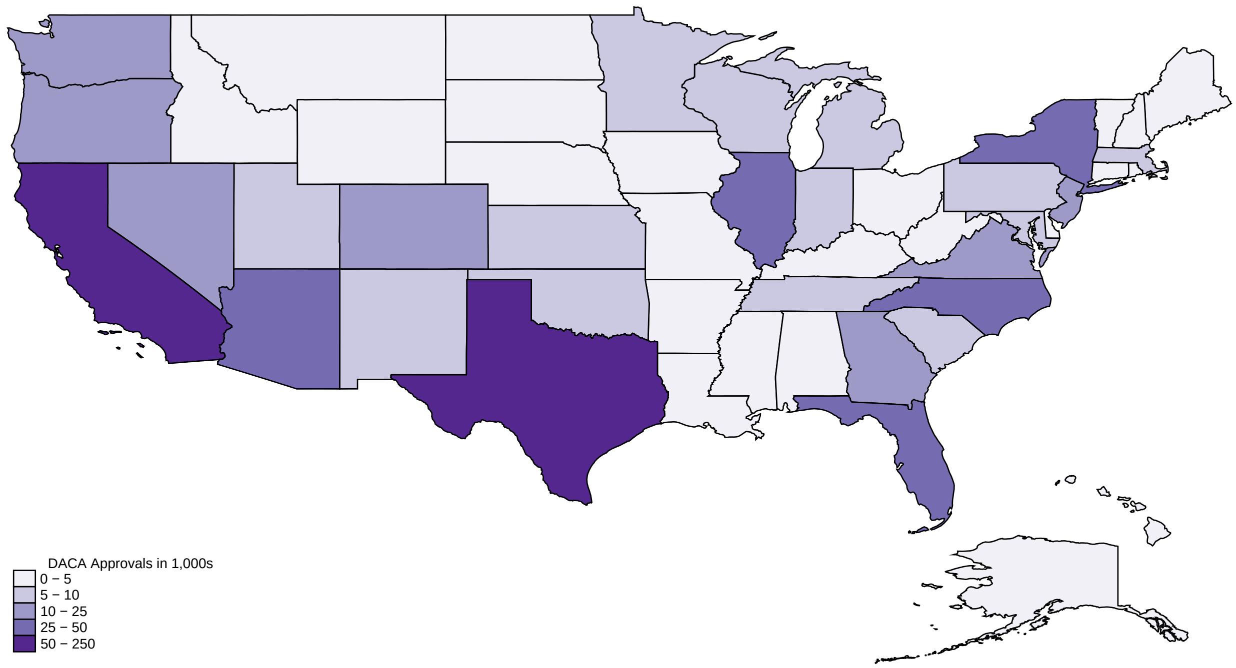
Table 9: Implied Elasticity of High School Graduation and College Enrollment to Wages

	HS, Age 19	College, Age 19-22
A: 4 Years Exp. Duration		
Elasticity - All	0.173	0.891
Elasticity - Males	0.210	0.330
Elasticity - Females	0.204	1.778
B: 6 Years Exp. Duration		
Elasticity - All	0.109	0.329
Elasticity - Males	0.114	0.145
Elasticity - Females	0.106	0.672
C: Permanent Exp. Duration		
Elasticity - All	0.037	0.052
Elasticity - Males	0.044	0.019
Elasticity - Females	0.038	0.120

Notes: Estimates of the elasticity of high school and college for all, males, and female DACA-eligible youth, under various expecations of the duration of DACA duration of DACA; 4 years, 6 years, and permanent. Elasticity calculated using (1) the implied ITT effects of DACA for Hispanics (see Section 10) and (2) estimates of the wage benefits of DACA using inputs from Table C.1 together with the framework for expected wages in Section C.

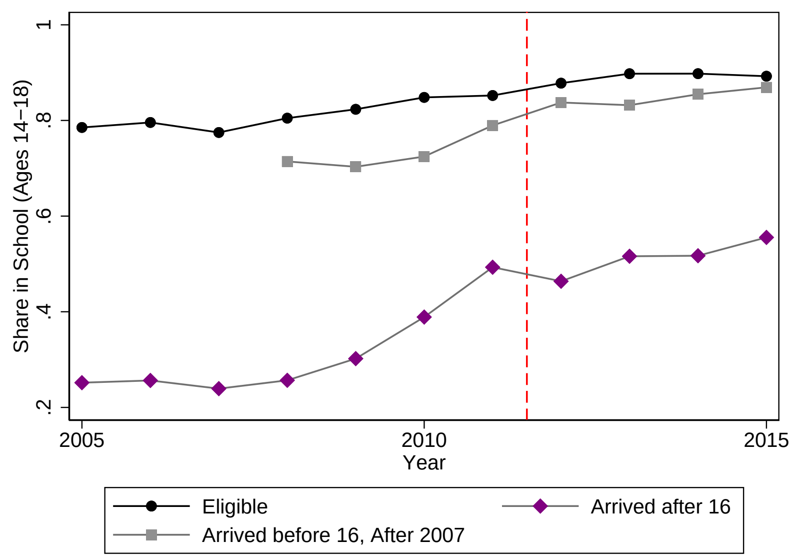
A Appendix: Supplementary Tables and Figures

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.

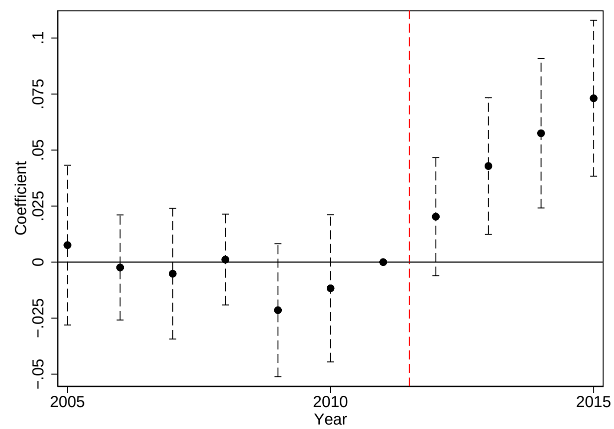
Figure A.2: Trends in School Attendance Among Non-Chosen Control Groups, Hispanic Immigrant Non-Citizens Ages 14-18



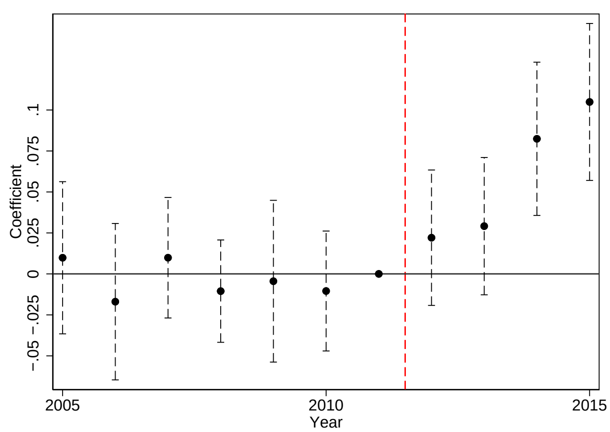
Notes: This figure shows school attendance rates for Hispanic immigrant non-citizens aged 14-18 that immigrated by age 10 and by 2007, or that immigrated after 16 or 2007, with statistics calculated from the 2005-2015 American Community Surveys. The red line demarcates the implementation of DACA.

Figure A.3: Effect of DACA on School Attendance, Ages 19-22

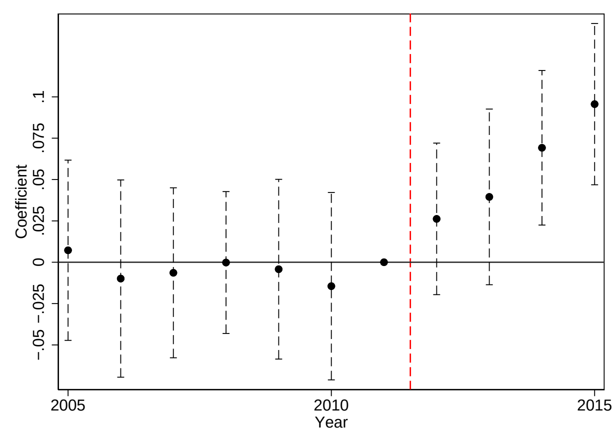
(a) All



(b) Hispanic



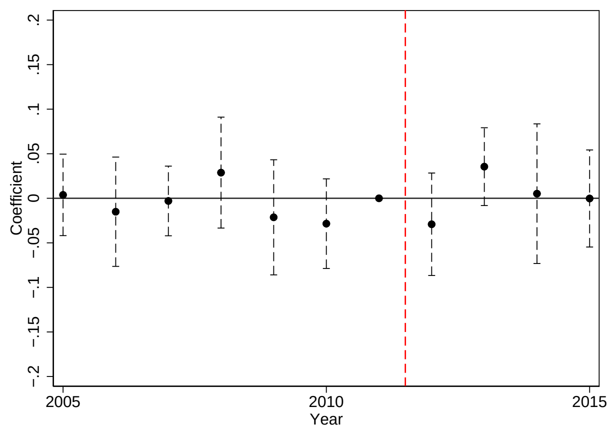
(c) High Take-Up



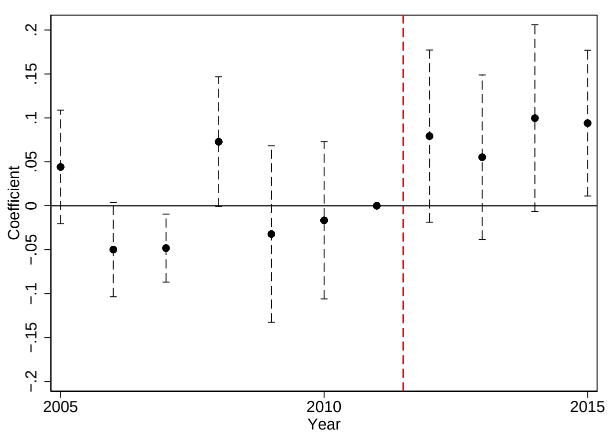
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step 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.4: Effect of DACA on College Enrollment, Age 19

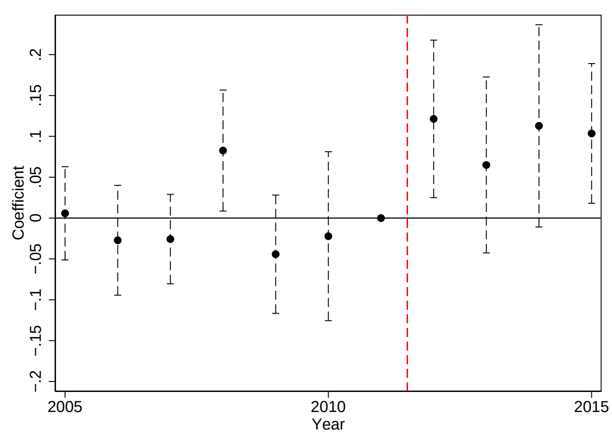
(a) All



(b) Hispanic



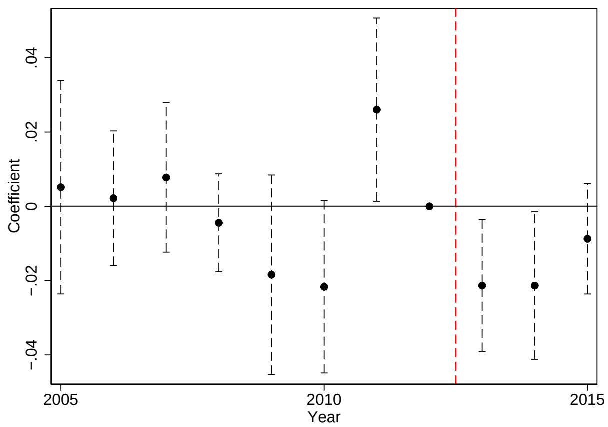
(c) High Take-Up



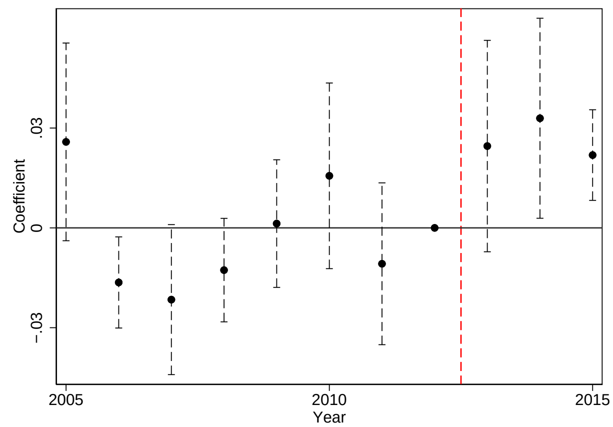
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step 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.5: Effect of DACA on Teenage Fertility, Ages 15-18
from High Take-Up Countries

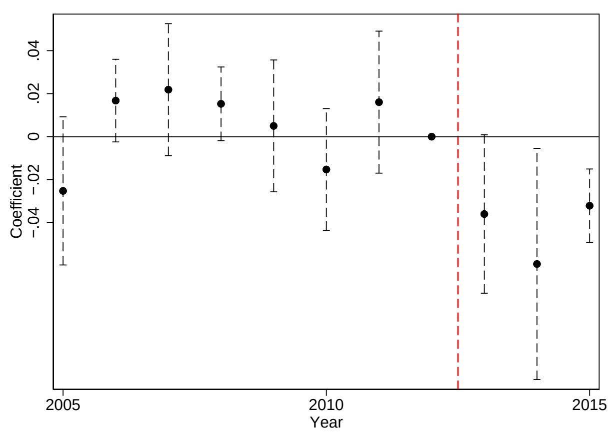
(a) Had Child Last Year



(b) Have No Children



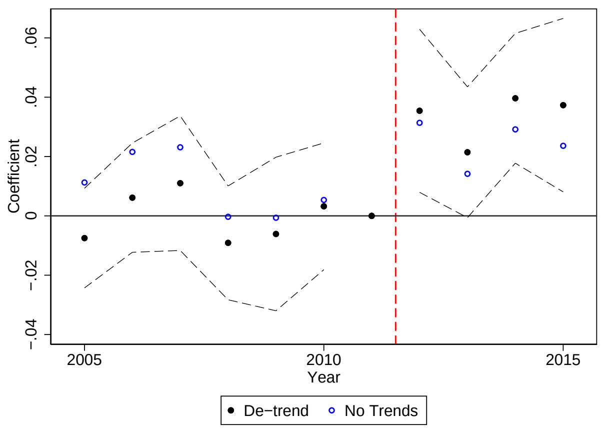
(c) Number of Children



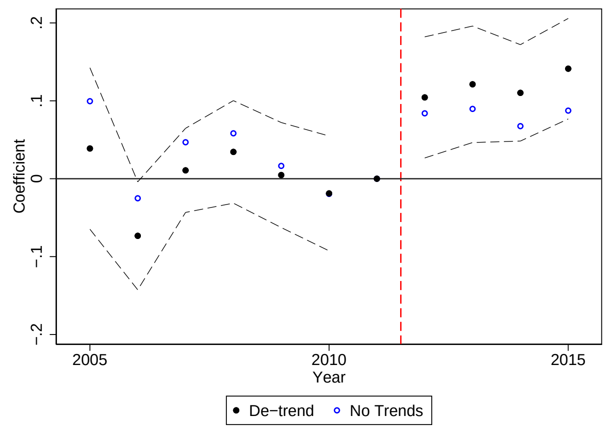
Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born females that immigrated by age 10 and by 2007. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30%. See text for details. We apply a two-step 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.6: Effect of DACA on Main Outcomes, Hispanics – Sensitivity to Trends

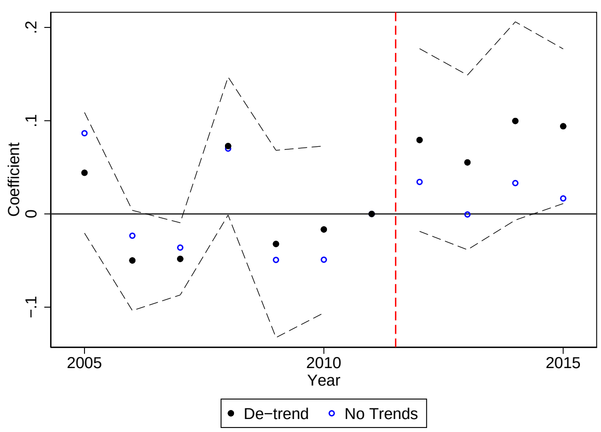
(a) School Attendance, Ages 14-18



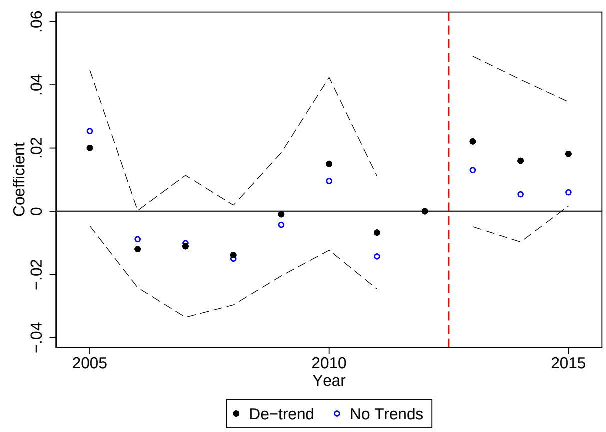
(b) High School Completion, Age 19



c) College Enrollment, Age 19



(d) No Children, Females Ages 15-18

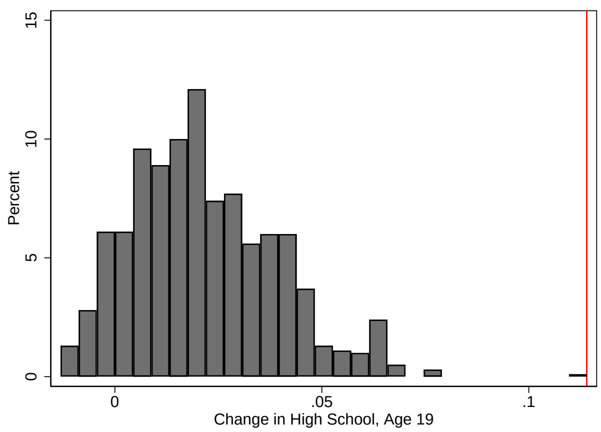


Notes: Data are from the 2005–2015 American Community Survey (ACS), and the sample is composed of foreign born individuals that immigrated by age 10 and by 2007. Each point represents coefficients from event study regressions that separately estimate interactions between year and eligibility indicators. 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.

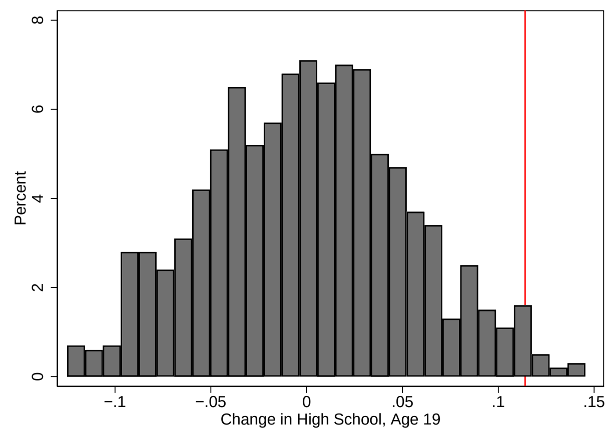
Figure A.7: Permutation Tests of High School Completion and College Enrollment

(a) High School Completion, Age 19

(i) Version 1: De-trend 2005-2011

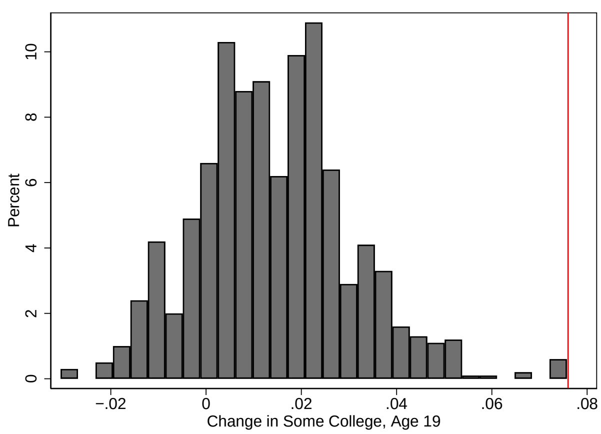


(ii) Version 2: De-trend Placebo Years

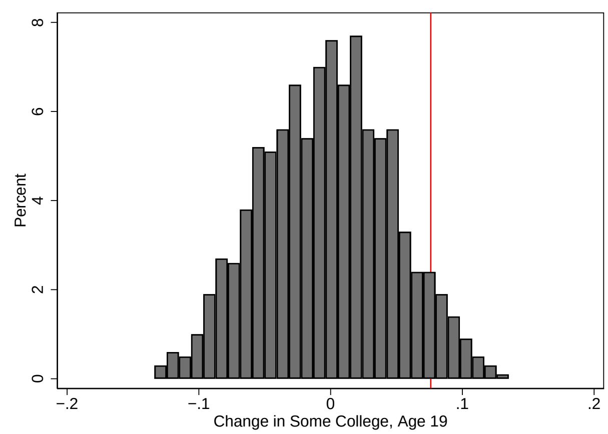


(a) College Enrollment, Age 19

(i) Version 1: De-trend 2005-2011



(ii) Version 2: De-trend Placebo Years



Notes: This figure shows two versions of permutation tests where we compare our estimated effect of DACA for the Hispanic sample (shown in the vertical red line) to placebo estimates from 1,000 samples where we randomly assign four years as "treated", and the remaining seven years as the pre-period. Version 1 estimates the real pre-trend using data from 2005-2011, removes this trend from the data, and then randomly assigns the four years of DACA. Version 2 first randomizes the four years of DACA, and then detrends the data using the seven years randomly assigned to be pre-treatment. The p-values from these two versions are 0.001 and 0.014 for high school completion, and 0.001 and 0.059 for college attendance.

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 US 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 detrended 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 Main Outcomes, 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: College Enrollment, Age 19						
Eligible*Post	0.076**	0.070	0.026	0.099***	0.100	0.057*
	(0.032)	(0.068)	(0.027)	(0.033)	(0.077)	(0.029)
Mean Y	0.350	0.350	0.350	0.343	0.343	0.343
Individuals	10252	10252	10252	9173	9173	9173
D: 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: Calculation of Undocumented Population and Deportation Risk Analysis

B.1 Back-of-the-Envelope Calculation of the Undocumented Population

Our goal in this section is primarily to understand the frequency with which non-citizens are undocumented among the Hispanic population. For context, we also calculate the share of non-citizens with legal status and the share of the undocumented that are Hispanic.

Unfortunately, data are not always available at the level of aggregation that we need for these calculations, therefore we must make a few approximations to get close to these statistics.

1. We use the number of non-citizen Hispanics from the fourteen most common countries of origin to approximate the total number of Hispanic non-citizens. Immigrants from these countries account for 95% of all Hispanics in the US (Flores, 2017; Passel and Cohn, 2014).

2. We use the share of undocumented among non-citizens from Latin America (LA) (Central America, South America, and the Caribbean) to approximate the share of undocumented among Hispanic non-citizens. We estimate that Latin American immigrants account for at least 94% of all Hispanic immigrants (Flores, 2017; Passel and Cohn, 2014).[45]

Using these estimates, we calculate:

◦ 72% of Hispanic non-citizens are undocumented, as: $\frac{\text{8.75M undocumented from LA}}{\text{12.2M non-citizens from LA}} = 0.72$. Source: Hispanic Origin Profiles table of Flores (2017) and Table 2.1 of Passel and Cohn (2014).

◦ 55% of non-citizens have legal status, as $1 - \frac{\text{11.2M undocumented}}{(\text{42.5M foreign born} - \text{17.8M citizens})} = 0.55$. Source: Figure 5.8 of NW, Washington and Inquiries (2015).

◦ 78% of the undocumented population are Hispanic, as $\frac{\text{8.75M undocumented from LA}}{\text{11.2M undocumented}} = 0.78$. Source: Table 2.1 of Passel and Cohn (2014).

[45] We use data from the fourteen most common countries of origin for Hispanic immigrants to calculate this. Among this group, 99% of Hispanic immigrants are from Latin America.

B.2 Deportation Risk Analysis

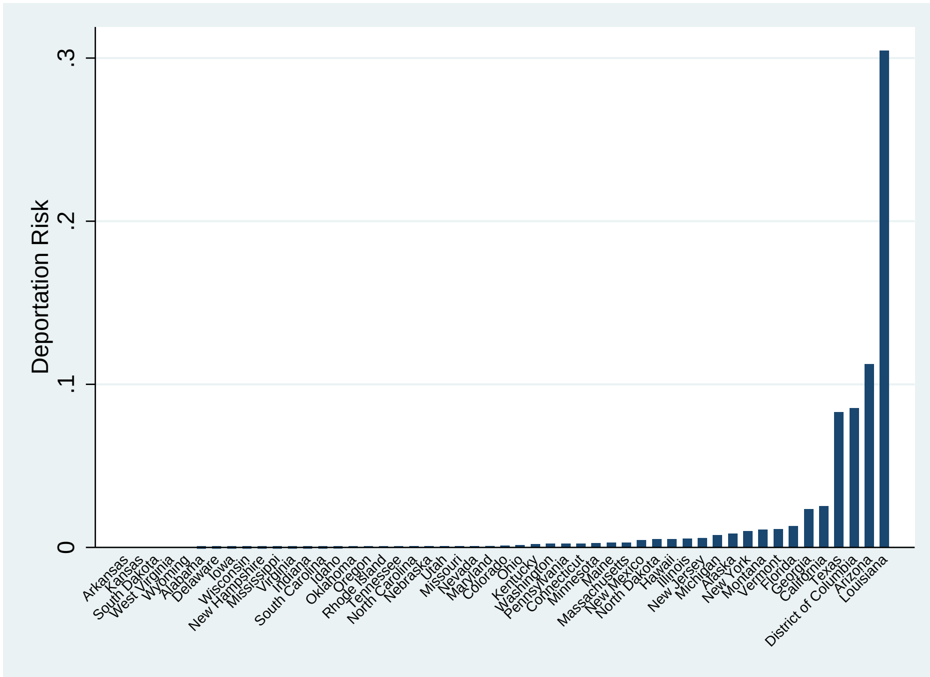
To measure the risk of deportation in each state, we obtained publicly available Immigration and Customs Enforcement data on aggregate deportations maintained by the Transactional Records Access Clearinghouse.[46] We obtain the annual deportation rate as the number of interior deportations by state of departure in each fiscal year from 2005-2011 divided by the noncitizen population aged 10-30 in each state, calculated from the ACS. We then take the average of the deportation rate over the 2005-2011 period to create a single pre-DACA measure of deportation risk that we assign to each individual in the ACS based on their current state of residence.

Figure B.1 ranks states according to this measure of deportation risk. A select few states have very large deportation rates; Louisiana (31%) being the highest, followed by Arizona, Washington DC, and Texas (8% to 12%). The remaining states have deportation rates deportation rates that fall between 0% and 3%. Because the variation in deportation risk is concentrated in a handful of states, we use a flexible estimation strategy in which we impose no parametric relationship between deportation risk and the impact of DACA, instead visually inspecting for such a relationship.

Our difference-in-difference estimator extends our baseline regression model to allow the coefficient on PostxEligible to vary for each. We also include the two-way interactions of state and eligibility indicators. We then plot state-specific treatment effects in order of the state deportation risk, along with 95% confidence intervals in Figure B.2. Marker size is proportional to the size of the state's non-citizen population.

[46]Data retrieved from http://trac.syr.edu/phptools/immigration/removehistory/.

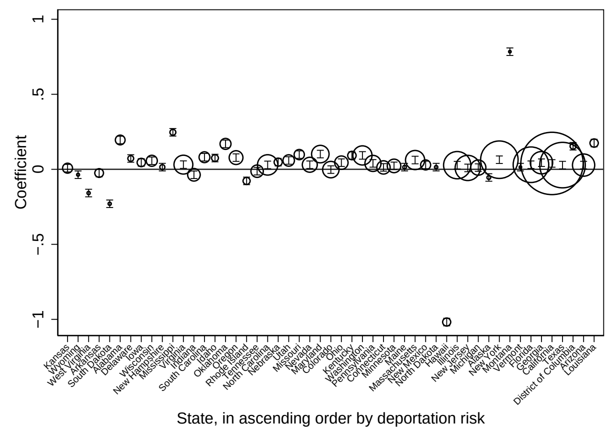
Figure B.1: Deportation Rate by State Prior to DACA



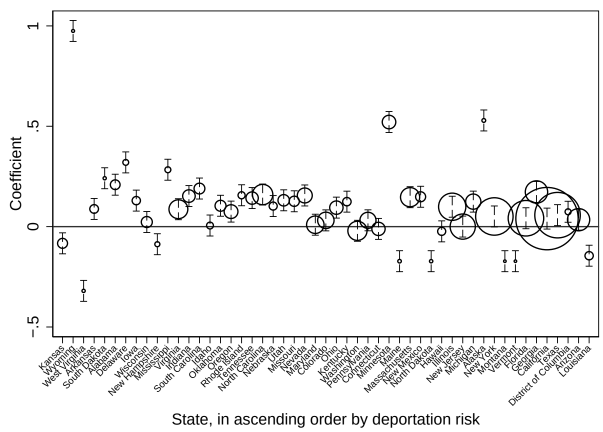
Notes: Figure shows the deportation rate within each state prior to DACA. We define the deportation rate as the number of interior deportations by state of departure in each fiscal year from 2005-2011, obtained from the Transactional Records Access Clearinghouse, divided by the noncitizen population aged 10-30 in each state, calculated from the ACS.

Figure B.2: Does DACA have Larger Effects on Hispanic Schooling in States with Higher Deportation Risk?

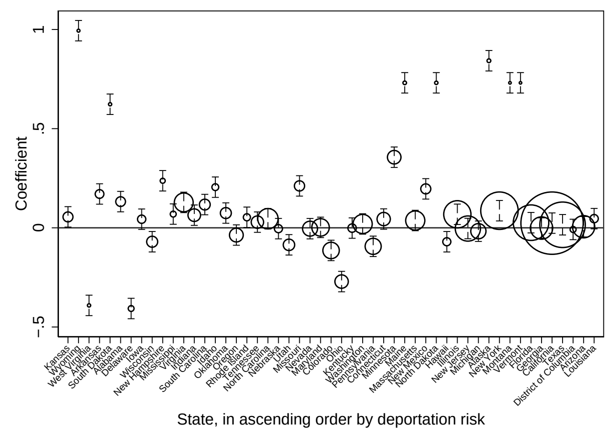
(a) In School, Ages 14-18



(b) HS Grad, Ages 19-22



(c) Some College, Ages 19-22



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. 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 difference-in-difference regressions that estimate the coefficient on the interaction between eligibility, post, and state of residence. States are placed in ascending ordered according to their baseline deportation risk, which is calculated as the number of interior deportations by state of departure in each fiscal year from 2005-2011, obtained from the Transactional Records Access Clearinghouse, divided by the noncitizen population aged 10-30 in each state, calculated from the ACS. We also provide 95% confidence intervals, calculated with standard errors that are clustered by state and adjusted for three additional degrees of freedom. Regression includes flexible controls for year of immigration, age of immigration-by-citizenship status, demographic characteristics, state-by-year fixed effects, and state by eligibility fixed effects. The results are weighted by the survey sampling weights.

C Appendix: Extended Conceptual Framework and Elasticity Estimation

In this section, we include a formal representation of the framework described briefly in Section 3 and derive implications of the framework for education decisions. Schooling levels are denoted by s and include high school drop out (D), high school diploma (HS), and some college (C), respectively. O represents the country of origin, and in the U.S. n indicates undocumented status, and ℓ indicates legal status.

C.1 Set-Up

Consider an undocumented youth deciding whether to drop out immediately, continue school for only one additional year and obtain a high school diploma, or to commit to enrolling in college after high school. Additionally, he anticipates two different states of the world during his working life: one in which he remains in the U.S. and another in which he is deported to his country of origin.

The expected lifetime earnings are the sum of yearly wages in the U.S. and yearly wages in their country of origin, weighted by the expected years of work in the U.S. versus expected years of work in the country of origin.[47] If an individual drops out of high school, he works the maximum number of years, equal to the difference between retirement age and his current age, T; otherwise, his working years are equal to $T - \alpha$, where α is the number of years spent in additional schooling. When we empirically estimate life time earnings, we assume $T = 43$, the difference between age 18 and 60, $\alpha = 1$ if an individual chooses to complete high school, and $\alpha = 2$ if an individual chooses to attend some college.[48]

Given a deportation risk, d, the expected number of years spent working in the US is the cumulative probability that they are not deported, given by $Y_\tau^{US} = \sum_{t=\alpha}^{T}(1 - d_\tau)^t$. The number of years spent working in one's country of origin is then $T - \alpha - Y_\tau^{US}$. For simplicity, we assume yearly wages are static and can hence describe expected lifetime earnings for different each status and schooling combination. We also abstract from discounting to have a more parsimonious model, but following the literature account for a 5% discount factor in our empirical estimates.[49] The expected lifetime earnings before ($\tau = 0$) and after ($\tau = 1$)

[47] It is worth mentioning that we have not explicitly included the nontrivial tuition costs of college in this framework, but to the extent college tuition remains unchanged after DACA, introducing a fixed college tuition cost would lead to the same result.

[48] We assign the wage associated with some college after one year of college to match our empirical work, where we will measure college attendance as having attended at least one year of college.

[49] For example, to incorporate a discount rate r for wages prior to DACA we set $\omega_0^s = \sum_{t=\alpha}^{T} \frac{w^{O,s}}{(1+r)^t} \cdot [1 - (1 - d_\tau)^t] + \frac{w^{n,s}}{(1+r)^t} \cdot (1 - d_\tau)^t$.

DACA are:

$$\omega_0^s = w^{O,s} \cdot Y_0^O + w^{n,s} \cdot Y_0^{US}$$

$$\omega_1^s = w^{O,s} \cdot Y_1^O + w^{\ell,s} \cdot Y_1^{US}$$

We assume that the policy affected the anticipated years of work in the U.S. and in the origin country by lowering the deportation risk ($Y_1^{US} > Y_0^{US}$), and also allowing individuals to earn higher wages associated with legal status in the U.S. ($w^{\ell,s} > w^{n,s}$). We ignore any general equilibrium changes in market wages for any education level in either the U.S. or abroad. Additionally, we assume high school dropouts do not see any change in deportation risk and cannot access legal wages since choosing to they are ineligible for DACA. Hence, the expected lifetime wages of a high school dropout are equivalent before and after DACA,

$$\omega_1^D = \omega_0^D = w^{O,D} \cdot Y_0^O + w^{n,D} \cdot Y_0^{US}$$

The youth arrives at his decision by comparing expected lifetime earnings under each schooling decision and status, and choosing the option that yields the highest net benefit. Specifically, he decides to finish high school if $\omega_\tau^{HS} - \omega_\tau^D > 0$. He then enrolls in college if $\omega_\tau^C - \omega_\tau^{HS} > 0$.

This setup allows us to conveniently analyze the expected impacts of DACA. First, DACA should increase the number of high school graduates if it increases the return to high school. In this simple framework, the return to high school is simply the difference between the dropout wage and the high school wage ($\omega_\tau^{HS} - \omega_\tau^D$). The *change* in the return to high school after DACA is,

$$\begin{aligned}
&(\omega_1^{HS} - \omega_1^D) - (\omega_0^{HS} - \omega_0^D) \\
&= (\omega_1^{HS} - \omega_0^D) - (\omega_0^{HS} - \omega_0^D), \textit{ following from equation C.1} \\
&= \omega_1^{HS} - \omega_0^{HS} \\
&= (w^{O,HS} \cdot Y_1^O + w^{\ell,HS} \cdot Y_1^{US}) - (w^{O,HS} \cdot Y_0^O + w^{n,HS} \cdot Y_0^{US})
\end{aligned}$$

To further simply the expression we add and subtract $Y_1^{US} \cdot w^{n,HS}$,

$$\begin{aligned}
&= w^{O,HS} \cdot (Y_1^O - Y_0^O) + w^{n,HS} \cdot (Y_1^{US} - Y_0^{US}) + (w^{\ell,HS} - w^{n,HS}) \cdot Y_1^{US} \\
&= (w^{n,HS} - w^{O,HS}) \cdot (Y_0^O - Y_1^O) + (w^{\ell,HS} - w^{n,HS}) \cdot Y_1^{US}
\end{aligned}$$

The resulting expression elucidates two potential ways in which DACA may incentivize individuals to attain a high school diploma:

1. By changing the deportation risk, DACA affects the number of anticipated work years spent in the country of birth, and hence the number of expected years that undocumented individuals can earn U.S. wages rather than home country wages. DACA will thus incentivize high school graduation if $Y_0^O - Y_1^O > 0$ and $w^{n,HS} - w^{O,HS} > 0$ – i.e. that individuals actually perceived a decline in deportation risk and decrease in expected work years abroad, and that the wages paid to undocumented high school graduates in the U.S. are greater than the wages they could earn as high school graduates abroad.
2. By providing work authorization, DACA allows individuals to earn the high school wages paid to those with legal status. This is a benefit that encourages high school graduation if $w^{\ell,HS} - w^{n,HS} > 0$.

Using the same framework, we can assess how DACA affects the decision to enroll in college. Specifically, we compare the returns to college – defined here as the difference between expected lifetime earnings associated with some college and a high school diploma – before and after DACA:

$$\begin{aligned}(\omega_1^C - \omega_1^{HS}) - (\omega_0^C - \omega_0^{HS}) &= (w^{O,C} \cdot Y_1^O + w^{\ell,C} \cdot Y_1^{US}) - (w^{O,HS} \cdot Y_1^O + w^{\ell,HS} \cdot Y_1^{US}) \\ &\quad - (w^{O,C} \cdot Y_0^O + w^{n,C} \cdot Y_0^{US}) + (w^{O,HS} \cdot Y_0^O + w^{n,HS} \cdot Y_0^{US}) \\ \\ &= (w^{O,C} - w^{O,HS}) \cdot Y_1^O + (w^{n,C} - w^{n,HS}) \cdot Y_1^{US} \\ &\quad - [(w^{O,C} - w^{O,HS}) \cdot Y_0^O + (w^{n,C} - w^{n,HS}) \cdot Y_0^{US}]\end{aligned}$$

Similar to before, we can further simplify the expression by adding and subtracting $(w^{n,C} - w^{n,HS}) \cdot Y_1^{US}$,

$$\begin{aligned}&= (w^{O,C} - w^{O,HS}) \cdot (Y_1^O - Y_0^O) + (w^{n,C} - w^{n,HS}) \cdot (Y_1^{US} - Y_0^{US}) \\ &+ [(w^{\ell,C} - w^{\ell,HS}) - (w^{n,C} - w^{n,HS})] \cdot Y_1^{US} \\ \\ &= [(w^{n,C} - w^{n,HS}) - (w^{O,C} - w^{O,HS})] \cdot (Y_0^O - Y_1^O) \\ &+ [(w^{\ell,C} - w^{\ell,HS}) - (w^{n,C} - w^{n,HS})] \cdot Y_1^{US}\end{aligned}$$

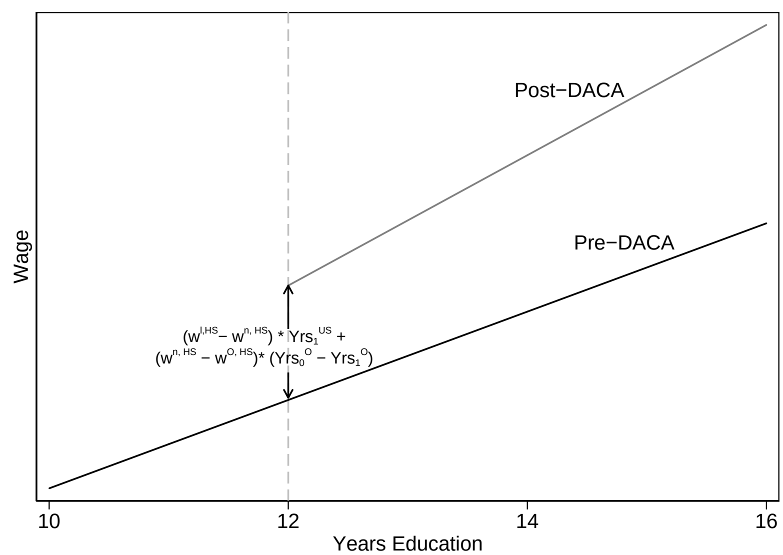
Hence, simplification gives us a similar expression as before, where the last line follows from the fact that $Y_1^{US} - Y_0^{US} = Y_0^O - Y_1^O$.

We expect DACA to incentivize college enrollment in two distinct ways:

1. Similarly to above, DACA affects the number of expected years that undocumented individuals can earn the U.S. college wage premium. This will incentivize high school graduation if $Y_0^O - Y_1^O > 0$ and $(w^{n,C} - w^{n,HS}) - (w^{O,C} - w^{O,HS}) > 0$ – i.e. that individuals actually perceived a decline in deportation risk and decrease in expected work years abroad, and that the college wage premium paid to undocumented in the U.S. is greater than the college wage premium they could earn abroad.
2. By providing work authorization, DACA allows individuals to earn the college wage premium associated with legal status $(w^{\ell,C} - w^{\ell,HS})$, rather than the college wage premium associated with undocumented status $(w^{n,C} - w^{n,HS})$. This is a benefit that encourages college enrollment if $(w^{\ell,C} - w^{\ell,HS}) - (w^{n,C} - w^{n,HS}) > 0$.

To solidify this intuition, we illustrate the earnings-schooling profile before and after DACA in Figure C.1. This figure illustrates the discrete increase in the return to high school after DACA and assumes that the returns to college also increase.

Figure C.1: Returns to Education, Before and After DACA



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

C.2 Estimating the Elasticity of Schooling

In our estimation of the elasticity, we estimate life time earnings using this model with a few adjustments. First, we allow d to vary by age (18 to 24, 25 to 34, 35 to 44, 45 to 54, and 55 to 60) and sex based on the tabulations of deportations. Second, we calculate $w^{n,s}$, $w^{\ell,s}$ and $w^{s,O}$ as the expected annual earnings by multiplying annual earnings for each country, schooling, and legal status by the probability of working for that group. Table C.1 shows the inputs into the expected wages before and after DACA by sex. We pair these inputs with the implied ITT estimates of DACA for Hispanics that we calculate in Section 10 divided by the mean rate of schooling of Hispanics in our sample to obtain the percent increase in schooling. The resulting elasticity of schooling estimates are in Table 9.

Table C.1: Wages and Returns from DACA - Inputs into Elasticity Calculation

	All	Male	Female
A: Inputs for Calculation of Returns			
Dropout Wages - Mexico	1733	2751	821
HS Wages - Mexico	2631	3677	1566
Some College Wages - Mexico	5143	6844	3616
Dropout Wages - U.S. Noncitizens	4469	6005	2280
HS Wages - U.S. Noncitizens	5471	6864	3667
Some College Wages - U.S. Noncitizens	7143	8536	5778
Dropout Wages - U.S. Citizens	5270	6874	3518
HS Wages - U.S. Citizens	8355	10180	6480
Some College Wages - U.S. Citizens	15397	19552	12057
Deportation Risk, Ages 18-60 - Prior to DACA	0.035	0.056	0.008
B: Expected Years			
Years illegal in U.S. - Prior to DACA	15.358	12.069	25.851
Years in Mexico - Prior to DACA	27.642	30.931	17.149
Years legal in U.S. - Prior to DACA	0.000	0.000	0.000
Years in Mexico - 4 Year DACA	23.643	30.960	19.005
Years illegal in U.S. - 4 Year DACA	15.407	8.090	20.045
Years legal in U.S. - 4 Year DACA	3.950	3.950	3.950
Years in Mexico - 6 Year DACA	21.506	24.046	13.188
Years illegal in U.S. - 6 Year DACA	15.598	13.058	23.916
Years legal in U.S. - 6 Year DACA	5.896	5.896	5.896
Years in Mexico - Permanent DACA	4.415	4.415	4.415
Years illegal in U.S. - Permanent DACA	0.000	0.000	0.000
Years legal in U.S. - Permanent DACA	38.585	38.585	38.585
C: Returns to Schooling			
Return to HS - Prior to DACA	28466	23232	42730
Return to College - Prior to DACA	71108	87033	72534
Change in Return to HS - 6 Year DACA	49237	61991	38389
Change in Return to College - 6 Year DACA	28409	37818	20551
Change in Return to HS - Permanent DACA	162150	194818	127253
Change in Return to College - Permanent DACA	167497	232029	116924

Notes: Inputs to the calculation of the benefits of DACA for all, male, and female DACA-eligible youth under various assumptions of the duration of DACA; 4 years, 6 years, and permanent. Wages in Panel A are expected annual earnings are calculated for each country and education as the probability of being employed times the average annual earnings. Wage and employment data for Mexico are from the 2010 Census and for the US are from the 2005 to 2011 ACS. Expected years in Mexico and the U.S. in Panel B are calculated using the equations for Y^{US} and Y^{O} in Section C. In Panel C, the return to HS is the difference between the expected lifetime earnings for a high school graduate and a high school dropout, and the return to college is the difference between the expected lifetime earnings for an individual with some college and a high school graduate.

D 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 D.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

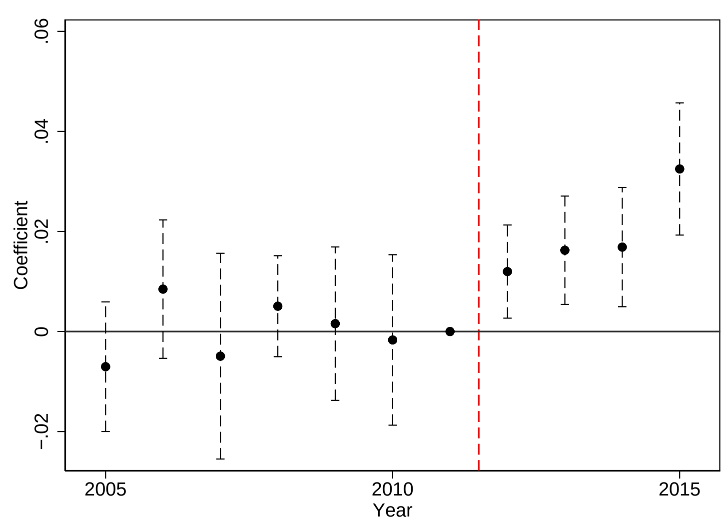
Notes: Data include individuals surveyed in the NLSY97. Statistics in each column represent the share of individuals that graduate in each set of months among those that graduate in a given number of years.

E Appendix: Geographic Level Analysis

E.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 E.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 E.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.[50]

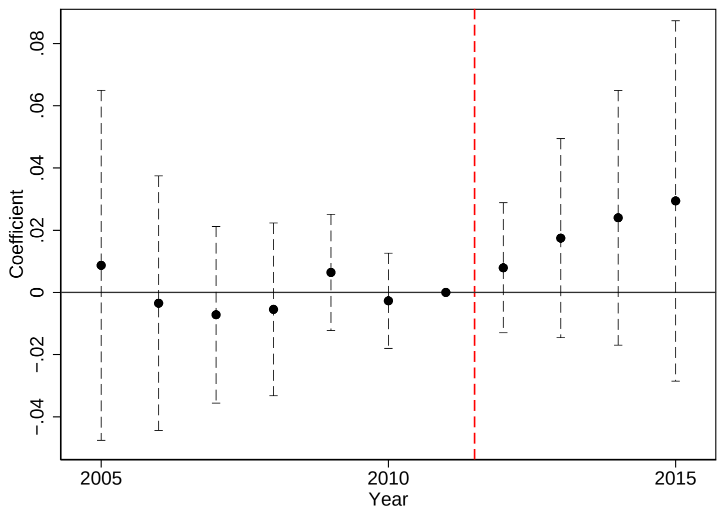
Figure E.1: Effect of DACA on School Attendance, Hispanics Ages 14-18 – California County-Level Variation



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.

[50]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.

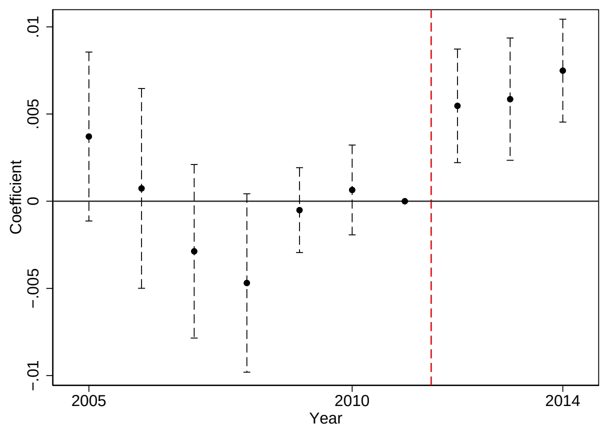
Figure E.2: Effect of DACA on High School Enrollment, Hispanics – California County-Level Variation with DOE Data



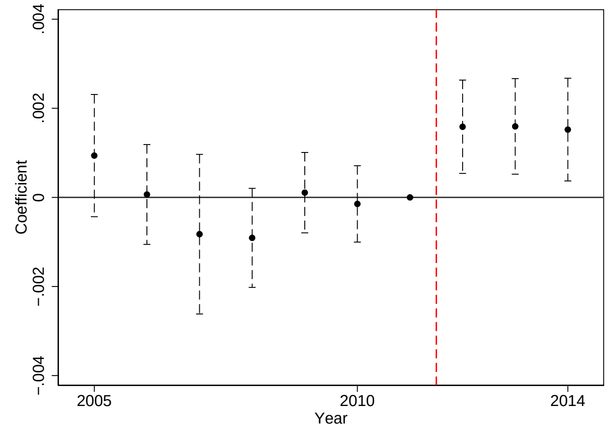
Notes: Data are from California's Department of Education, and cover years 2005–2015. The sample includes county-level hispanic enrollment in high schools. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and 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 E.3: Effect of DACA on CAHSEE Math Exam, 12th Graders

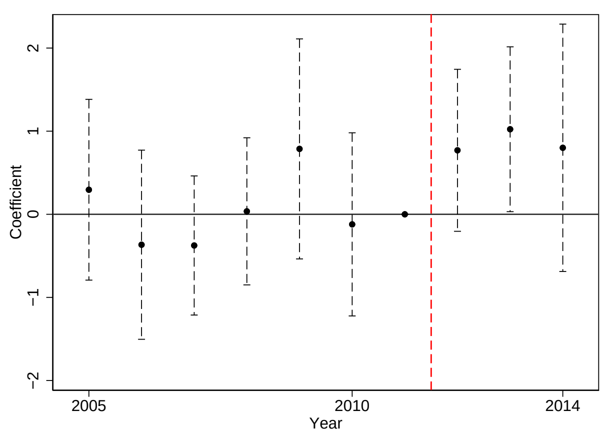
(a) N Test Takers



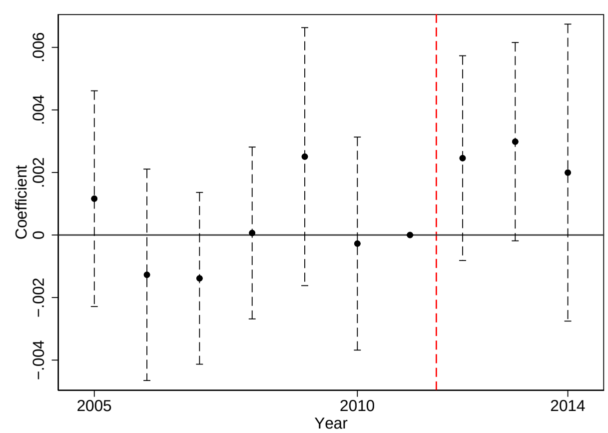
(b) N Passing



(c) Score



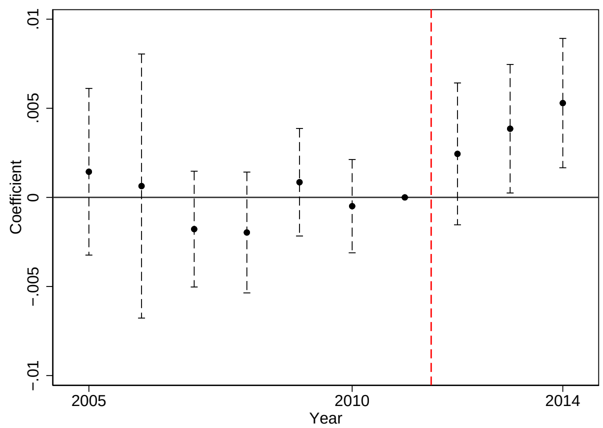
(d) Log Score



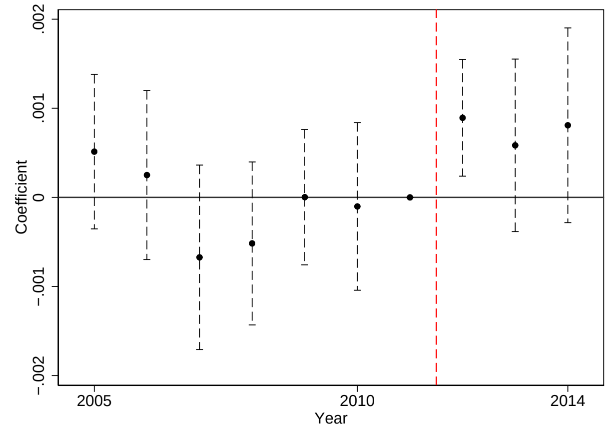
Notes: Data are from California's Department of Education, and cover years 2005–2015. The sample includes county-level hispanic test takers. We apply a two-step adjustment, where the first step fits a trend to the pre-period data (2005-2011) and removes this trend from the full data; and the second step 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 E.4: Effect of DACA on CAHSEE ELA Exam, 12th Graders

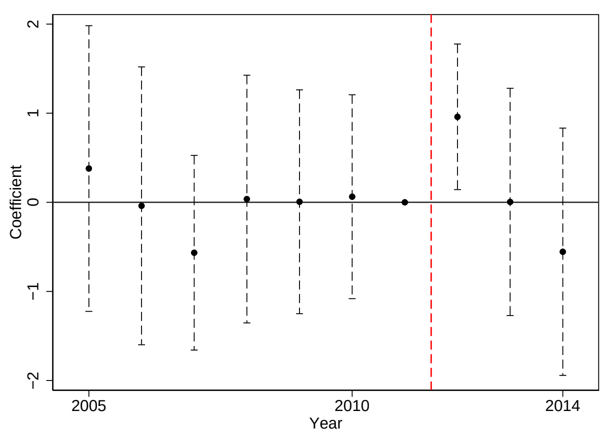
(a) N Test Takers



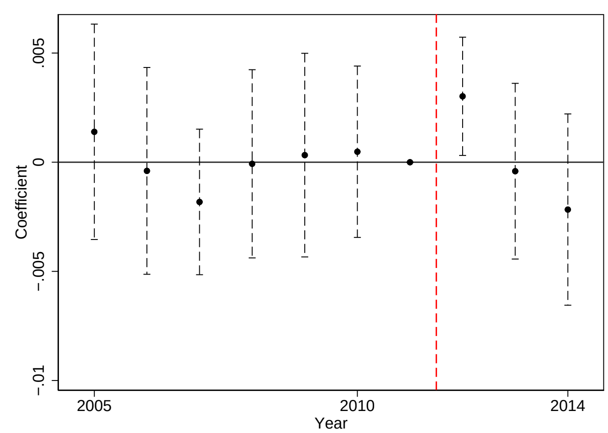
(b) N Passing



(c) Score



(d) Log Score



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.

F Results with Inverse Propensity Score Weighting

Table F.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 F.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 F.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: Some College, Age 19-22			
Eligible*Post	0.026*	0.038**	0.054***
	(0.014)	(0.018)	(0.017)
Individuals	82077	38690	34759
C: High School Completion, Age 19-22			
Eligible*Post	0.041***	0.083***	0.089***
	(0.007)	(0.010)	(0.008)
Individuals	82077	38690	34759
D: 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.