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

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This paper studies human capital responses to the availability of the Deferred Action for Childhood Arrivals (DACA) program, which provides temporary work authorization and deferral from deportation for undocumented, high-school-educated youth. We use a sample of young adults that migrated to the United States as children to implement a difference-in-difference design that compares noncitizen immigrants ("eligible") to citizen immigrants ("ineligible") over time. We find that DACA significantly increased high school attendance and high school graduation rates, reducing the citizen-noncitizen gap in graduation by 40 percent. We also find positive, though imprecise, impacts on college attendance. (*JEL* H52, I21, I26, J13, J15, J24)

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 widens, the high school and college completion rates of many communities remain low (Bailey and Dynarski 2011, Murnane 2013). Undocumented youth, who account for 1.5 percent of the population of US minors, stand out in particular in this regard, with between 15 and 40 percent of young adults not having completed high school.[1] 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, a high opportunity cost of schooling, or misperceptions of the returns to education. In this paper, we examine how the availability and design of legalization policies impact youth investments in human capital.

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

We take advantage of an ongoing policy "experiment" in the United States, the institution of the Deferred Action for Childhood Arrivals program (DACA), which granted temporary legal status for undocumented youth. Enacted in August 2012, DACA extended temporary relief from deportation and work authorization—two years, initially, subject to renewal—to undocumented youth who were in school or had completed high school, and met other criteria based on age and year of arrival. DACA thus generates a discrete increase in the benefits associated with completing high school, and could also affect incentives for higher education. We discuss the range of direct and indirect benefits of participation in detail in Section I. Despite this, previous studies of DACA's impact on schooling have ignored its effect on high school attainment.

The responses of undocumented youth to immigration policy is of interest for several reasons. First, given the large migrant and refugee populations in the United States and Europe, and the intense policy debates on immigration reform, fully understanding the effects of DACA can assist in evaluating the benefits and costs of future immigration policies. 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). Finally, similar to other under-resourced populations, they face several disincentives to acquire human capital, such as lack of information about college applications, uncertainty over the costs and returns to schooling, and reduced access to credit markets (Amuedo-Dorantes and Bansak 2006, Osili and Paulson 2007). Understanding the role of expected benefits of schooling for this population may inform the education choices of other low-income youth.

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 in our preferred specification rely on the absence of US citizenship combined with Hispanic ethnicity as a second-best measure of undocumented status (Amuedo-Dorantes and Antman 2016, Kaushal 2006, Pope 2016). Second, while ineligible 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) requirements for DACA make eligible youth significantly more predisposed to stay in school relative to ineligibale 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 who arrived by age ten to address mechanical changes in sample composition that are tied to the year of arrival criteria for eligibility.

Hence, our difference-in-difference framework primarily compares Hispanic immigrant noncitizen child arrivals (treated) to immigrant citizen child arrivals (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. 2014; 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 comparison 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 improvement in schooling of the eligible population after DACA. As a result, our findings are largely insensitive to using alternative comparison groups or specifications, including propensity score methods.

We find that DACA had a significant impact on adolescents' educational investments. Our preferred estimates for Hispanics show that DACA led to a 2.2 percentage point (pp) increase in the school attendance of 14- to 18-year-olds, a 2.5 percent increase, which narrowed the gap in attendance between citizens and noncitizens in this age range in our sample by 55 percent. This rise in school attendance arguably includes many students who were on the margin of high school completion. We find that DACA increased high school completion of 19- to 22-year-olds by 5.9 pp, a 7.5 percent increase. 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 citizen and noncitizen youth in our sample closed by 40 percent. Effects are smaller, though still positive, for individuals further from the typical high school enrollment age.

We find less precise evidence of impacts on post-secondary schooling, which was not required for DACA. Estimates from our main specification show that attainment of some college for 19- to 22-year-old Hispanics increased by 1.3 pp, but this effect is imprecisely estimated and varies across alternative specifications. We interpret these results as suggestive, but not definitive, evidence of increases in college going.

As a secondary identification strategy, we use administrative data from California and leverage variation in the geographic concentration of eligible youth across counties. We analyze the impact of DACA on Hispanic high school enrollment and performance on the California High School Exit Exam (CAHSEE), a mandatory test required for graduation. We find a 4 pp increase in high school enrollment and a 2 pp increase in the number of CAHSEE test takers, which corroborates well the ACS results on attendance. We also find increases in the pass rate among twelfth-grade Hispanic students approaching their final opportunity to graduate, which indicates that DACA also induced greater schooling effort.

How large is the effect of DACA? Unpacking our results, we find that DACA had twice the impact on high school graduation of men (7.7 pp) as for women (3.5 pp). To interpret these magnitudes, we scale these impacts by the expected lifetime benefits of DACA, which we calculate with a simple model of lifetime expected earnings. This combines estimates of the reduction in the sex-specific average risk of deportation, skill-specific earnings in the United States and abroad, and the difference between legal and nonlegal earnings in the United States. We obtain elasticities of high school graduation to expected lifetime earnings around 0.05 for both men and women, which implies that the differential DACA response by gender was proportional to the expected benefits of the program. Further, we calculate that the semi-elasticity of high school graduation is 0.25. This is roughly 60 percent of the semi-elasticity in Abramitzky and Lavy (2014), who study an increase in the return to schooling in Israeli kibbutzim. This suggests that DACA-eligible youth may be less sensitive to future returns, although these results could also be rationalized by misperceptions of the returns from DACA.

Our findings speak to central questions in education and immigration policy. First, we provide compelling evidence that a large share of the gap in the high school graduation of noncitizen youth and their citizen peers is attributable to the uncertain and limited returns to schooling. Previous papers show that *reductions* in the labor market incentives to stay in school, commonly induced through improvements in the low-skilled labor market, increase the likelihood of dropping out of high school and reduce college enrollment (Atkin 2016; Black, McKinnish, and Sanders 2005; Cascio and Narayan 2017; Charles, Hurst, and Notowidigdo 2018; Shah and Steinberg 2017). However, responses to *increases* in future wage returns 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 (Abramitzky and Lavy 2014, Jensen 2010, Oster and Steinberg 2013, Wiswall and Zafar 2014).[2] 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. Among this literature, Cortes (2013) is the only study, that we are aware of, that examines the effects of an unconditional amnesty (Immigration Reform and Control Act of 1986 (IRCA)) on education,[3] and finds that college attendance substantially increases with amnesty. However, since that study does not account for changes in the sample age or age of immigration, the conclusions of that study are difficult to interpret.

We also contribute to a growing literature on the impacts of DACA, which finds that DACA improves health among children and adults (Giuntalla and Lonsky 2018, Hainmueller et al. 2017), reduces teenage pregnancy (Kuka, Shenhav, and Shih 2019), and improves adult labor market outcomes (Amuedo-Dorantes and Antman 2017, Pope 2016). Closer to this paper, Pope (2016), Amuedo-Dorantes and Antman (2017), and Hsin and Ortega (2017) analyze impacts on school attendance, focusing on older populations that have already completed high school and therefore are already eligible for the program. We build on this prior work in several respects.

[2] 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).

[3] The IRCA provided legal status to undocumented immigrants that entered the United States before 1982 and met other criteria. In a similar vein, Liscow and Woolston (2017) and Felfe, Rainer, and Saurer (2016) analyze the impact of citizenship on childhood and teenage schooling. However, the effect of citizenship may be quite different than a temporary or permanent amnesty, and the mixed-citizen families in Liscow and Woolston (2017) are not necessarily representative of all undocumented youth.

First, we study impacts on high school graduation, asking whether youth at an earlier stage of schooling are more responsive to DACA, perhaps because they want to *become* eligible. We provide a careful and transparent analysis of school attendance and high school completion that yields new insights about the comparability of schooling outcomes between citizens and noncitizens in this population. Second, in our analysis of higher education, we do not control for high school graduation, contrary to all prior studies of DACA on schooling. Consistent with selection bias, we show in sensitivity analyses that controlling for this covariate meaningfully reduces the estimate on school attendance for this sample.[4] Third, we provide a two-pronged approach to analyzing schooling impacts, utilizing both individual-level survey data and county-level administrative records from California. The similarity of our estimates across these disparate sources helps reinforce our conclusions. These new findings 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.

The paper continues as follows. We provide further detail regarding the institutional details of DACA in Section I. In Section II we examine the incentives of DACA and generate empirical predictions for schooling decisions. We discuss our data and empirical strategies in Sections III and IV. Section V presents results on schooling attendance, high school graduation, and college. Section VI provides evidence on exit exam performance. We discuss mechanisms and calculate implied schooling elasticities in Section VII, and conclude in Section VIII.

I. Institutional Background and Take-Up of DACA

Prior to DACA, there were multiple attempts to create a unifying federal policy for undocumented youth (Olivas 2004). The Development, Relief and Education for Alien Minors (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 memorandum in June 2012, with the first applications being accepted in August 2012.

DACA provides two types of benefits to recipients. First, deportation is deferred, allowing beneficiaries to reside legally in the United States. 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 ages 18 and 39 by sex in 2012 from the Department of Homeland Security (Simanski and Sapp 2013). On average, the annual deportation risk is 5 percent, however, there is significant variation in the risk across sex, as men account for almost 90 percent of all deportations. This implies that the deportation risk is closer to 1.5 percent for women and 7.3 percent for men, taking differences in the size of the respective

[4] For a more detailed discussion of this analysis as well as of these earlier works, see online Appendix F.

populations into account.[5] It is worth noting that for both men and women, the perceived risk of deportation may be much higher than the actual risk, as recent surveys found that 59 percent of foreign-born Hispanics are somewhat or significantly concerned about the risk of deportation (Lopez et al. 2013).

Second, recipients may obtain an Employment Authorization Document (EAD), which grants recipients work authorization. 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 frictions in 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 percent of recipients applied for renewal after the initial two-year period (Hipsman, Gómez-Aguiñaga, and Capps 2016). The prevalence of renewals could reflect an expectation among recipients that the program would persist beyond two years (Nevarez 2015). Efforts to expand the reach of DACA, though never passed, could have further added to expectations of the program's longevity.[6]

DACA applicants must meet a suite of immigration, education, and criminal requirements and pay a \$465 fee for approval. The first set of requirements are based on age and date of arrival in the United States. We use these criteria to determine treatment status in our empirical analysis: (i) under 31 by June 15, 2012, (ii) entered the United States before age 16, (iii) continuous residence in the United States since June 15, 2007, and present at the time of application. Applicants must also be at least 15 years old, though we do not use this restriction in our analysis 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.[7] We do not use this last criterion to determine treatment, as schooling is the potential outcome of the program that we focus on.

US Citizenship and Immigration Services (USCIS) began accepting applications for DACA on August 15, 2012, which was met by an immediate surge in applications. Figure 1, panel A 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 one year—roughly 30 percent 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

[5] 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 the Pew Center estimated population of 6.6 million (56 percent of 11.9 million undocumented immigrants) (Passel 2005, Passel and Cohn 2009). Deportation rates by gender are calculated as the rate of 18 to 39 removals (81.4 percent) times the share of male (female) deportations, 89.3 percent (10.7 percent), times 419,384 alien removals—a total of 304,851 (36,527) deportations—divided by an estimated population of 4.1 million (2.5 million), 35 percent (21 percent) of 11.9 million unauthorized immigrants.

[6] President Obama announced an expanded DACA program in 2014 that would have extended eligibility to youth that arrived in the United States by January 1, 2010; however, that version was never enacted.

[7] 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 percent did not respond to the question) (Wong et al. 2016).

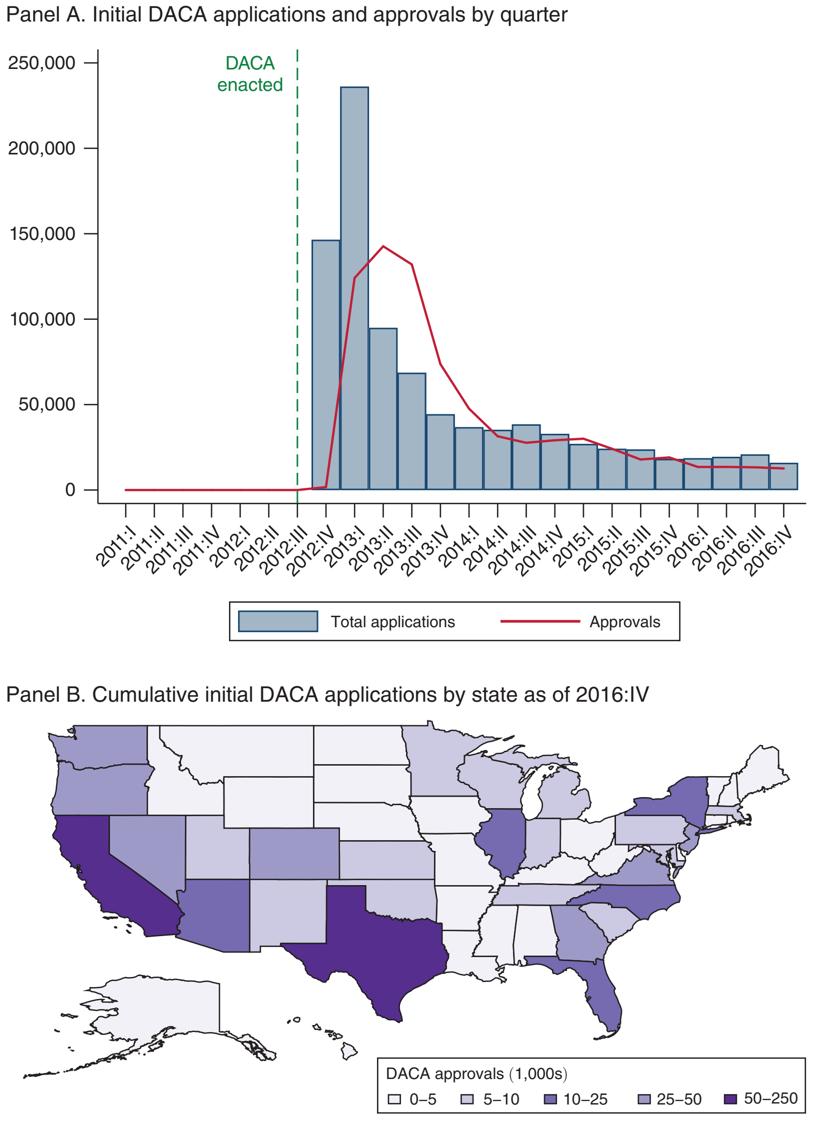

Figure 1. DACA Applications—Years 2011–2016

Notes: Panel A shows first-time DACA application counts and the number approved in each quarter through 2016. Panel B shows first-time DACA application counts across states as of the fourth quarter of 2016. Data come 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.

Trump ordered an end to DACA, leading to an immediate halt in the acceptance of new applications and renewals. However, ongoing court challenges have resulted in a continuation of renewals.

The geographic distribution of DACA applications reflects the concentration of undocumented populations in a handful of states. Figure 1, panel B displays cumulative initial DACA applications through 2016 by state. California and Texas account for over 237,000 and 138,000 DACA applications, respectively. Illinois, New York, and Florida each account for roughly 40,000. These five states alone constitute 52 percent of the total number of applications. Moreover, the majority of applicants are from Latin America, with 600,000 applications from Mexico alone, with El Salvador, Guatemala, and Honduras as the next highest applicant countries. Outside of Latin America, the largest sources of applicants are from 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.[8]

II. DACA Incentives for Education

To motivate our empirical analysis, we use a simple human capital investment framework to examine how DACA might impact school attendance, high school completion, and college attendance of undocumented youth. For brevity, we present the basic intuition here and include further details in online Appendix Section C.

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 annual wage corresponding to his current legal status and schooling level. 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."[9] 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 United States illegally, he may be deported to his country of origin at any time. If he is deported, he may either work in his country of origin or attempt a return to the United States.[10] We assume that the probability of successful return is low for simplicity.[11]

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, lifetime earnings are given by the discounted sum of the US wages earned over the expected years in the United States after completing his schooling and the discounted earnings 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 United States and in his country of origin, and how many years he expects to remain in the United States. It is worth highlighting that the expected duration in the United States depends on his estimation of unobserved risks of deportation and successful reentry, which may be imperfect. For example, survey evidence in Section I showed that youth may overestimate deportation risk. This would lead to biased perceptions of the expected years in each country and

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

[9] Works on this topic, including Borjas (2017), Kossoudji and Cobb-Clark (2002), and Rivera-Batiz (1999), find that legalization raises wages between 6 to 14 percent.

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

[11] Bazzi et al. (2019) provides a nice summary on the risk of apprehension, with estimates of the probability of apprehension conditional on attempted return ranging from 40 to 60 percent. We assume that the unconditional probability is likely to be much smaller, since not all migrants will attempt return.

cause education choices at baseline to differ from decisions taken under correct information.

By granting work authorization and reducing the risk of deportation, DACA raises the expected wage earned in the United States, from the nonlegal to the legal wage, and increases the expected number of years in the United States. 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 net of the probability of successful return—which extends the number of years one expects to spend in the United States—and to the wage gain from remaining in the United States without legal status, instead of returning to one's country of origin.[12] 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 dropout wage) and the probability of return is unchanged, this generates a clear prediction that high school completion will increase after DACA.

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-school-educated workers, which has been documented in cross-sectional data (Borjas 2017, Rivera-Batiz 1999). It could also increase if the return to college in the United States is higher than in the country of origin, since DACA ensures that more years will be spent in the United States reaping those returns. Empirically, returns to college are roughly 50 percent higher *on average* in typical countries of origin for DACA youth (e.g., Mexico);[13] however, the returns to experience for college-educated workers are twice as high in the United States (Lagakos et al. 2018). Thus, over the life cycle, college-educated workers may still reap higher returns in the United States. If these conditions are satisfied, DACA should encourage college attendance.

While we have thus far focused on the benefits of DACA that arise through expected wages, we acknowledge there are likely significant nonpecuniary benefits of DACA. For example, undocumented youth may have an incentive to finish high school just to avoid deportation if they have a preference for remaining in the United States (Vargas 2012).

III. Data

We use data from the Integrated Public Use Microdata Series (IPUMS) ACS (Ruggles et al. 2018) for the period 2005 through 2015 to examine the education decisions of eligible and non-eligible individuals. We use year of immigration and

[12] Since youth also now have short-term certainty over deportation risk, this also reduces the possibility of "mistakes" based on short-term misestimation of the deportation risk, which could increase individual and social welfare (see, e.g., Finkelstein and Notowidigdo (2018) and Bernheim and Taubinsky (2018)).

[13] We calculate the ratio of the return to college in countries of origin and the return to college in the United States by taking the ratio of column 5 and column 6 from table 5 of Clemens, Montenegro, and Pritchett (2016).

citizenship status, together with current age, to determine eligibility for DACA.[14] Since age of arrival and year of birth are not reported in the survey, we assign age of arrival as the difference between year of arrival and the survey year minus current age.

Our main analysis focuses on a sample of immigrant youth ages 14 to 22 that arrived to the United States by age 10 and by 2007. We focus on individuals born outside of the 50 US states to avoid the strong cultural, institutional, and structural divisions between natives and immigrants (Borjas 1985, 2017, LaLonde and Topel 1992). We justify the age of immigration criteria in Section IV. We conduct our analyses on various subgroups of youth, reflecting the distinct ages at which different decisions are taken. We use a sample of teens between the ages of 14 to 18 to examine high school attendance, and a sample of young adults between the ages of 19 and 22 to examine post-secondary school attendance and high school completion.

Importantly, the ACS collects information on all households living in the United States, 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. Moreover, the Census Bureau also takes several steps to encourage responses to the ACS (Liscow and Woolston 2017). The Census Bureau is not permitted to share personal information with other government agencies and communicates this confidentiality policy in the survey. Particularly relevant for our context, the Census also performs outreach to Hispanic organizations, and makes the survey available in Spanish.

As a secondary source of schooling outcomes, we turn to administrative data from California. California has the largest undocumented population among all states and accounts for nearly 30 percent of DACA recipients. We obtain county-level administrative data on Hispanic high school enrollment from the California Department of Education (CA DOE) and county-level Hispanic performance on the California High School Exit Examination (CAHSEE), including average test scores, the number of test takers, and the number of students passing the exam by test subject. CAHSEE data is available starting in academic year 2005–2006, hereafter 2005, when the CAHSEE was launched, through 2014, after which the CAHSEE was suspended. Enrollment data are available for the entire period, from 2005 to 2015.

We provide further detail on the sample construction and variable definitions in both these data sources in the online data Appendix.

IV. Empirical Strategy Using Individual Eligibility

Our empirical strategy identifies DACA-ineligible immigrant youth whose education decisions follow similar patterns to eligible youth prior to DACA, and uses

[14] 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) shows that the ambiguity in the wording of this question leads to various interpretations in reporting that may cause us to misassign treatment in some cases. We assume that this misinterpretation is not discontinuous after 2012.

a difference-in-difference strategy to measure the impacts of the policy. We use the estimating equation,

$$(1)\qquad Y_{igast} = \alpha_0 + \alpha_1 Eligible_g + \alpha_2 (Eligible_g \times Post_t) + \beta X_{ig} + \gamma_{st} + \gamma_{rt} + \gamma_{at} + \epsilon_{igast},$$

where Y_{igast} is the outcome for an individual i, who has eligibility status g (eligible or not eligible), and is currently age a and living in state s in year t. The term $Eligible_g$ is a function of whether an individual is (i) not a citizen and (ii) meets DACA's age and year of arrival requirements. We impose the latter criteria by restricting our sample to immigrant youth who arrived by 2007 and by age 10. This also corrects for a mechanical shift in sample composition whereby moving forward in survey time reduces the maximum age of arrival of eligible youth, which could have an independent effect on our estimates (Bleakley and Chin 2010).[15] As a result, $Eligible_g$ is simply an indicator for being a noncitizen. The variable $Post_t$ is an indicator that equals 1 beginning in the year 2012.

We account for fixed individual characteristics by including a vector of controls, X_{ig}, that include dummies for sex, year of immigration, birth region,[16] as well as age of immigration-by-eligibility and age-by-eligibility fixed effects. We include state-by-year (γ_{st}) and race-by-year fixed effects (γ_{rt}) to absorb state- and race-specific shocks, and age-by-year dummies (γ_{at}) to account for cohort effects. We use sampling weights in all regressions.

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 also allows us to visualize any difference between the eligible and ineligible groups before and after the policy went into effect as a test of the identification assumption.

We obtain standard errors for our estimates using two methods. First, we cluster our standard errors at the state level, as is commonly done for difference-in-differences with a federal policy (e.g., Hoynes, Miller, and Simon 2015), which we report in all tables. Second, we obtain *p*-values using permutation tests that compare our estimates of the effect of DACA to estimates of placebo policies, following Conley and Taber (2010) and Agarwal et al. (2015). Since our main analysis covers four "treated" years, we assign a placebo DACA policy to four randomly chosen years drawn without replacement, allowing the remaining seven years to serve as the pre-period. We then estimate the effect of the placebo DACA policy. We construct *p*-values by comparing our estimate to the distribution of estimates from 1,000 placebo DACA simulations.

[15] Without this adjustment, the sample of eligible 18-year-olds in 2011 would include those that arrived by age 14, but in 2015 would only include those that arrived by age 10.

[16] We assign countries of birth into five groups: Mexico, Central and South America, United Kingdom and Europe, Asia, and rest of the world.

A. *Intent-to-Treat (ITT) Interpretation*

Our measure of eligibility is measured with noise, as noncitizens 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 ITT effect due to the possible inclusion of individuals who are not undocumented. Drawing on a number of sources summarized in online Appendix B, we calculate that 55 percent of all US noncitizens and 45 percent of noncitizens ages 18–24 are *legal* residents. As a result, our difference-in-difference estimates for the whole sample are likely to underestimate the true effect of DACA by 45 percent.

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 percent of undocumented immigrants and that 72 percent of all Hispanic noncitizens are undocumented. Given our estimates for all noncitizens above, this is likely to be a lower bound on the share undocumented among noncitizen Hispanic youth.

Second, we analyze individuals from countries that have a DACA take-up rate above 30 percent ("high take-up"), which have a high share of undocumented due to overlap with the Hispanic subgroup but may have greater familiarity with DACA.[17] 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 percent of respondents come from high take-up countries, and among individuals born in high take-up countries, 93 percent are Hispanic.

Our baseline estimates are also "local" in the sense that they omit any effect on undocumented teens who immigrated after age 10, who account for roughly 40 percent of 14- to 18-year-old noncitizens in the ACS. We find slightly larger treatment effects when we include individuals who arrived between the ages of 11 and 16 (see Section VC).

B. *Baseline Sample Characteristics*

Online Appendix Table A.1 provides descriptive statistics of the Hispanic eligible and comparison groups at baseline, from 2005 to 2011. Roughly 24 percent of the comparison group was born in US territories, primarily Puerto Rico, 19 percent was born abroad to American parents, and 57 percent 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. As a sensitivity

[17] 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).

exercise, we use propensity score methods to generate balance in demographics across our treated and comparison groups (see Section VC.)

Note that while other ineligibale immigrants could in theory serve as a comparison group, namely noncitizens who do not meet the age and/or year of arrival criteria, online Appendix Figure A.1 shows that the common trends assumption does not hold for these groups. 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.

C. *Evidence on Identifying Assumptions*

Our identification relies on the assumption that in the absence of DACA, citizen child arrivals would have exhibited similar trends to noncitizen child arrivals. We examine the plausibility of this assumption in two ways. First, we ask whether the gap in *predicted* schooling, based on observable characteristics, was constant around the timing of DACA. To obtain predicted schooling outcomes, we generate fitted values for the entire sample period from a regression of schooling outcomes on a large number of demographic characteristics from 2005 to 2011.[18] Online Appendix Figure A.2 presents event study estimates of equation (1) for predicted school attendance and high school completion. In favor of our strategy, the coefficients are generally insignificant and show no upward trend in predicted schooling. Hence, any change in education outcomes of noncitizens relative to citizens post DACA must come from a change in *behavior*, not compositional changes.

Second, we examine observed trends in schooling of our treatment and comparison groups. Panel A of Figure 2 shows that the school attendance 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. Panel B shows a similar, unexpected closure of the gap in high school completion in 2012. These patterns provide support for common trends as well as suggestive evidence of a DACA treatment effect on education decisions. Further, the few examples where the trends appear to deviate in the raw means seem to be explained by small changes in observable characteristics that we control for in our regressions.[19]

We also require there to be no other simultaneous policies targeting undocumented youth. DACA was the only such national policy implemented during our sample period; we examine the role of local-level policies in Section VC.

[18] These include (i) indicators for age, race, sex, age and year of immigration; citizenship status; birthplace; language; state; and metropolitan status and (ii) 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. The results are similar, and more precise, if we only use the first set of variables, which are less likely to be outcomes of DACA.

[19] For example, the rise in noncitizen high school graduation between 2010 and 2011 is mirrored in our prediction of high school completion in online Appendix Figure A.2.

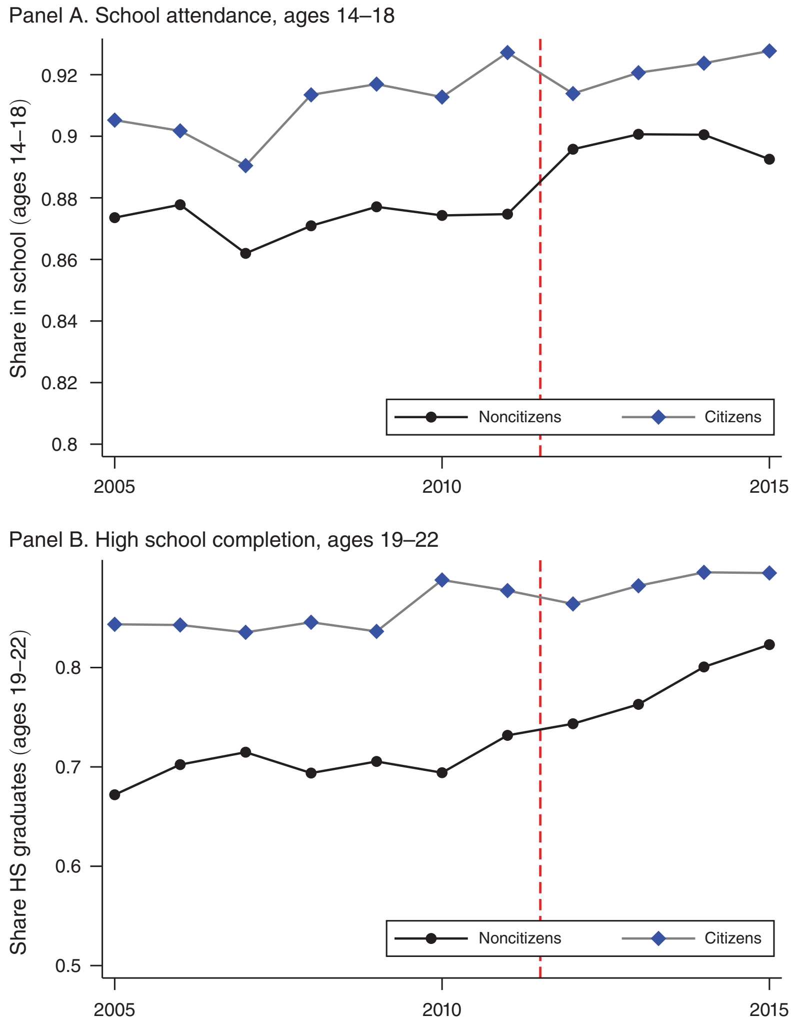

Figure 2. Average School Attendance and High School Completion, Hispanics

Notes: This figure shows the average school attendance and high school completion rates for Hispanic immigrants ages 14–18 and 19–22 for eligible youth (noncitizens) and ineligible youth (citizens). The vertical dashed line demarcates the implementation of DACA.

Source: 2005–2015 ACS. Sample is composed of foreign-born Hispanics who immigrated by age 10 and by 2007.

V. ACS Results

A. *School Attendance*

We first examine whether DACA led to increased school attendance. Figure 3 presents event studies for school attendance of adolescents ages 14 to 18. Estimates for the whole sample appear in panel A, while those for Hispanics and the high take-up sample appear in panels B and C. The figures show that there was not a preexisting trend between our eligible and comparison groups prior to 2012. After

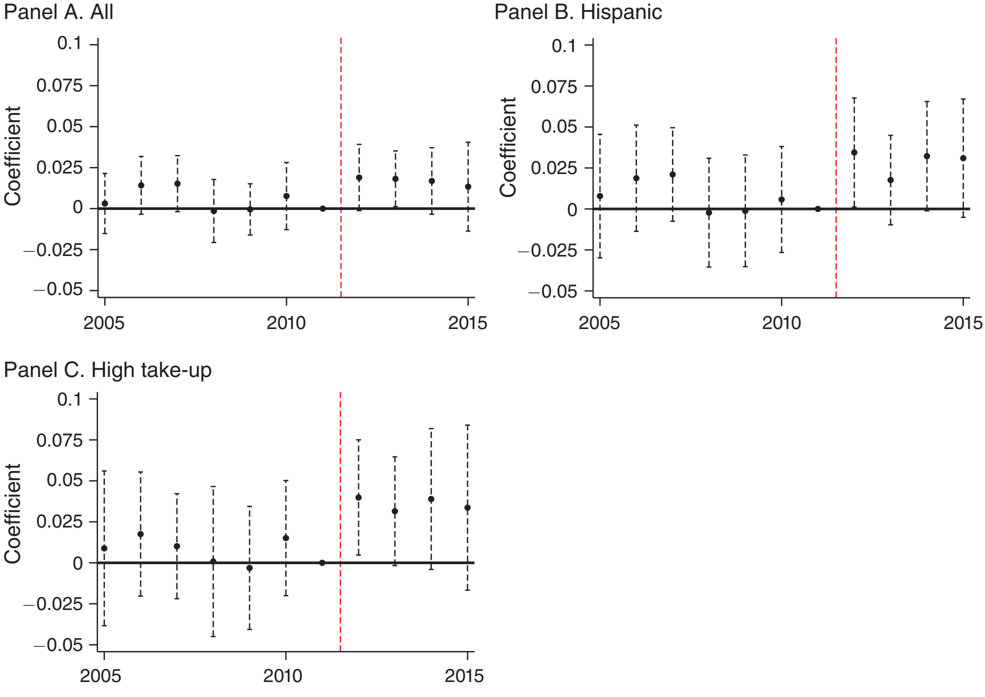

Figure 3. Effect of DACA on School Attendance, Ages 14–18

Notes: This figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interactions between year and eligibility indicators, where the outcome is an indicator for being in school, and year 2011 is the omitted category. The dashed vertical line indicates the enactment of DACA. Eligible individuals are defined as noncitizen immigrants, and the comparison group is comprised of citizen immigrants. All regressions control for the following fixed effects: sex, year of immigration, birth region, age of immigration by eligibility, age by eligibility, state-by-year, race-by-year, and age-by-year (see equation (1)). Standard errors are clustered by state, and regressions are weighted by the survey sampling weights. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30 percent—see text for details.

Source: 2005–2015 ACS. Sample is composed of foreign-born Hispanics between the ages of 14 and 18 who immigrated by age 10 and by 2007.

the enactment of DACA in 2012, eligible youth experience an immediate and persistent increase in school attendance.

The difference-in-difference results appear in panel A of Table 1. In line with the event studies, we find that DACA led to statistically significant increases in school attendance of 14–18-year-olds, with a 1.2, 2.2, and 2.9 pp increase among all immigrants, Hispanics, and the high take-up sample, respectively, which is equivalent to a 1.3 to 3.3 percent increase. To give a sense of the magnitude of this effect, the point estimates for Hispanics are slightly higher than the within-sibling impact of citizenship status on school attendance (Liscow and Woolston 2017). Panel B shows the effect of DACA among college-aged individuals, ages 19 to 22, with event study figures in online Appendix Figure A.3. We find positive, statistically insignificant point estimates for this age category.

Table 1—Effect of DACA on School Attendance

	All	Hispanic	High take-up
Panel A. Age 14–18			
Eligible × *Post*	0.012	0.022	0.029
	(0.005)	(0.008)	(0.008)
Mean *Y*	0.921	0.891	0.889
Individuals	114,453	54,015	48,359
Panel B. Age 19–22			
Eligible × *Post*	0.019	0.020	0.005
	(0.012)	(0.014)	(0.012)
Mean *Y*	0.547	0.405	0.401
Individuals	82,077	38,704	34,768

Notes: This table shows the difference-in-difference estimates of the impact of DACA on the school attendance of eligible youth. Eligible individuals are defined as noncitizen immigrants, and the comparison group is comprised of citizen immigrants. High take-up includes individuals born in countries that have a DACA-eligible take-up rate above 30 percent—see text for details. The outcome is current school attendance, and post is an indicator for 2012 or after. All regressions control for the following fixed effects: sex, year of immigration, birth region, age of immigration-by-eligibility, age-by-eligibility, state-by-year, race-by-year, and age-by-year (see equation (1)). Standard errors, shown in parentheses, are clustered by state, and regressions are weighted by the survey sampling weights.

Source: 2005–2015 ACS. Sample is composed of foreign-born individuals ages 14–18 (panel A) or 19 to 22 (panel B) who immigrated by age 10 and by 2007.

B. *High School Completion and College Attendance*

Next, we investigate whether increases in school going resulted in a higher rate of high school completion, defined as having earned either a high school diploma or GED.[20] We hypothesize that DACA will have the largest impact on youth that had not yet or just recently dropped out of school by DACA enactment (ages 19 to 22) but also examine impacts on 23 to 30-year-olds, all of whom would have likely left high school before DACA.

The results, presented in the first three columns of Table 2, show that DACA increased high school completion for all age categories. Panel A shows completion rates of 19-year-olds increased by 4.6 pp overall, with larger effects for Hispanics and the high take-up sample who experienced increases of 6.5 and 8.5 pp, respectively. This represents a sizable increase, both in absolute terms and relative to other interventions, particularly given the low 75 percent completion among Hispanics.

Panel B of Table 2 shows the impact for 19- to 22-year-olds is somewhat smaller, ranging from 3.8 to 6.4 pp, though more precisely estimated due to the larger sample size. The effect for 23 to 30-year-olds is quite a bit more muted. We find a marginally significant 1.5 pp effect among Hispanics, and a similarly sized but statistically insignificant effect for the high take-up sample. This is perhaps not surprising since this group is likely to have work or family commitments that would pose a barrier to returning to school.

[20] We would like to be able to separately estimate the effect on diploma and GED, but the ACS only provides information on the type of high school degree for the subsample of individuals that completed exactly high school.

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
Panel A. Age 19						
Eligible × *Post*	0.046	0.065	0.085	0.003	0.034	0.057
	(0.016)	(0.026)	(0.027)	(0.025)	(0.029)	(0.028)
Mean *Y*	0.824	0.747	0.741	0.468	0.350	0.343
Individuals	22,153	10,252	9,173	22,153	10,252	9,173
Panel B. Age 19–22						
Eligible × *Post*	0.038	0.059	0.064	0.017	0.013	0.011
	(0.007)	(0.010)	(0.011)	(0.009)	(0.010)	(0.011)
Mean *Y*	0.858	0.781	0.775	0.544	0.407	0.399
Individuals	82,077	38,704	34,768	82,077	38,704	34,768
Panel C. Age 23–30						
Eligible × *Post*	0.013	0.015	0.013	0.008	−0.001	−0.000
	(0.005)	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)
Mean *Y*	0.862	0.767	0.761	0.613	0.443	0.435
Individuals	133,576	61,210	54,110	133,576	61,210	54,110

Notes: This table shows the difference-in-difference estimates of the impact of DACA on the high school completion and the attainment of some college of eligible youth. The outcomes are high school completion (GED or diploma) in columns 1–3 and completion of some college (more than 12 years of completed education) in columns 4–6, and post is an indicator for 2012 or after. See the notes of Table 1 for the definition of eligibility, high take-up, control variables, clustering, and sample weights.

Source: 2005–2015 American Community Survey. Sample is composed of foreign-born individuals ages 19 (panel A) or 19–22 (panel B) or 23–30 (panel C) who immigrated by age 10 and by 2007.

The event studies in Figure 4 allow us to examine the timing of the effects for the 19 to 22 sample. Mirroring the increase in school attendance, we see that the increase in high school completion began in the year following the DACA announcement, and that the gap in completion rates was steady prior to the policy.[21] There is also a slight upward trend in the point estimates over time, although the point estimates are noisy, and we cannot reject a constant effect.

To put our findings into perspective, multiplying the 830,700 eligible Hispanics age 19–22 represented in the ACS by our estimated 5.9 pp increase in high school graduation implies that DACA led to over 49,000 additional high school graduates. As a result, the 15 pp gap in high school completion between Hispanic noncitizen youth and their citizen immigrant peers in our sample, which is roughly the same when we adjust for observable characteristics, narrowed by 40 percent.

Finally, in the last three columns of Table 2 and online Appendix Figure A.4 we analyze impacts on college going, which we define as having attended any post-secondary schooling. In general, we find positive but imprecise effects on the

[21] To examine the plausibility of an effect on high school graduation in 2012, which had at most six months that were treated by DACA, we estimate the likelihood of obtaining a diploma between July and December. Using the National Longitudinal Study of Youth 1997 (NLSY97), we find that one quarter of students that complete high school in five years obtain a diploma between August and December (see online Appendix D). 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.

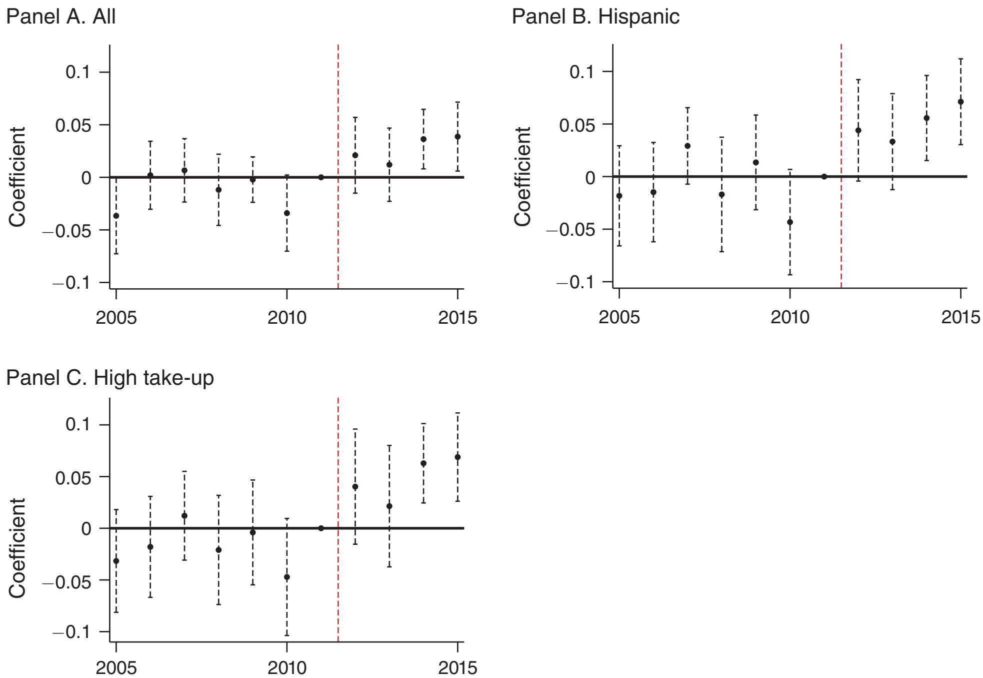

Figure 4. Effect of DACA on High School Completion, Ages 19–22

Notes: This figure shows the coefficients and 95 percent confidence intervals from event study regressions that estimate interactions between year and eligibility indicators, where the outcome is an indicator for being in school between the ages of 19 and 22, and year 2011 is the omitted category. The dashed vertical line indicates the enactment of DACA. See the notes accompanying Figure 3 for definition of eligibility, control variables, clustering, sample weights, and high take-up.

Source: 2005–2015 ACS. Sample is composed of foreign-born Hispanics between the ages of 19 and 22 who immigrated by age 10 and by 2007.

order of 1 to 3 pp for young adults ages 19 to 22. Even so, we are still able to rule out declines in college attendance larger than 2.2 pp for Hispanics. This lower bound is more positive than the upper bound of effects in Amuedo-Dorantes and Antman (2017) and Hsin and Ortega (2017), and similar to the mean effect in Pope (2016).[22] An exception to this is that we find a statistically significant 5.7 pp increase in college going for 19-year-olds in the high take-up sample, a 17 percent effect. This could reflect true heterogeneity in the treatment effect on college but may also have occurred in this subsample by chance.

A limitation of these results is that they do not allow for differential linear trends in college going between citizens and noncitizens, which may be important in part due to recent changes in tuition policies for undocumented students (Mendoza and Shaikh 2019). If we account for differential trends by eligibility, we find a significant 3 to 7 pp increase in college attendance (see Section VC for details).[23]

[22] We provide an in-depth comparison of the estimates across these studies in online Appendix Section F.

[23] In results not reported, we test whether the impact on college is tied to the cost of tuition by allowing for differential effects in states where undocumented individuals are eligible for in-state tuition. Intuitively, we find that

Overall, we view this as suggestive, though not definitive, evidence of increases in college, particularly with respect to the prior trend in undocumented attendance. Importantly, the fact that we find a consistent pattern across specifications should positively update and perhaps widen the confidence interval of college impacts from earlier studies.

C. *Robustness*

These results indicate that DACA had a significant impact on youth schooling decisions. We now perform sensitivity exercises to test alternative explanations for these findings and specifications for the analysis.

First, we examine whether our conclusions change when we calculate p-values using the permutation tests described in Section IV. Figure 5 shows the histogram of placebo estimates for Hispanic school attendance for ages 14 to 18 (panel A) and high school graduation for ages 19–22 (panel B), along with a vertical line representing our difference-in-difference estimate. The permutation tests yield p-values less than 0.01 for both outcomes, which suggests that our clustered standard errors for high school completion may in fact be too conservative.

Second, we test the sensitivity of our main findings to alternative sample selection criteria and refinements. The first column of Table 3 reprints our baseline results for school attendance, high school completion, and college attendance. In the following columns, we re-estimate results using alternative age-of-arrival restrictions, keeping only those that arrived by age 6 (column 2) or age 16 (column 3). We then expand the comparison group by sequentially adding in noncitizens and citizens who arrived in the United States after turning 16 (column 4), individuals who arrived after 2007 (column 5), and US-born individuals (column 6). The magnitude of our estimate is sometimes attenuated across these columns, but the precision and pattern of effects are very similar. In the last column we recode our eligibility indicator to exclude noncitizens that live in households with veterans or report positive social security or welfare receipt (Liscow and Woolston 2017). The estimated effects remain similar to our baseline effects.

Third, we account for potential differential linear pre-trends in the outcome by eligibility status in two different ways. Our less preferred approach is to control for the interaction between eligibility and year in equation (1). Instead, we favor a "two-step" approach in which we (i) regress each outcome and covariate on the interaction between eligibility and year using pre-period data, and obtain residuals; and then (ii) estimate equation (1) on the de-trended data, adjusting standard errors to account for the parameters in (i) (Goodman-Bacon 2016). We favor this approach because in the presence of dynamic treatment effects, directly controlling for linear trends can bias estimates (Borusyak and Jaravel 2017, Lee and Solon 2011, Wolfers 2006), and for transparency. Online Appendix Table A.2 shows our qualitative results are unchanged when we perform these adjustments, though we find larger and more precise effects on college going when using the "two-step" approach.

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the impacts on college are between 3 and 6 points higher in states with in-state tuition, but these differences are not statistically significant.

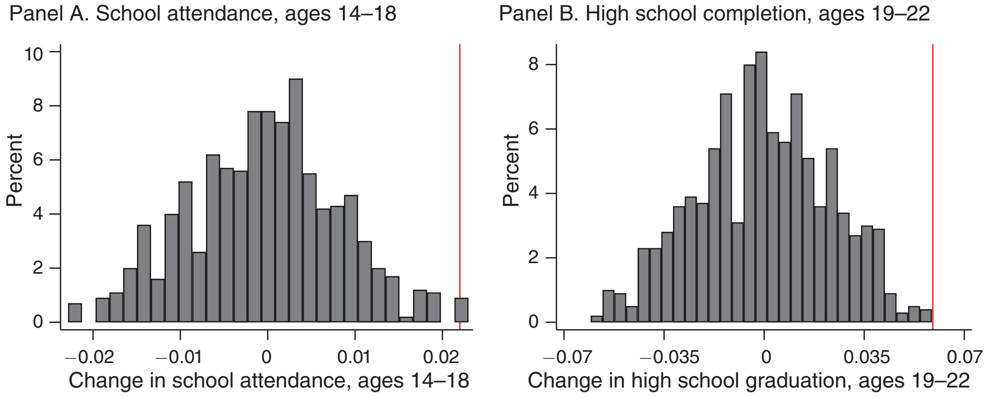

Figure 5. Permutation Tests of School Attendance and High School Completion, Hispanics

Notes: This figure shows results from permutation tests where we compare our estimated effect of DACA for the Hispanic sample to placebo estimates from 1,000 samples where we randomly assign four years as "treated" and the remaining seven years as the pre-period. We plot the distribution of placebo estimates, with our estimated effect represented by the vertical red line. See the notes of Figure 3 for definition of eligibility, control variables, clustering, and sample weights. The implied p-values are less than 0.01 for both panels.

Source: 2005–2015 ACS. Sample is composed of foreign-born Hispanics ages 14–18 (panel A) or 19–22 (panel B) who immigrated by age 10 and by 2007.

Table 3—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
Panel A. School attendance, ages 14–18							
Eligible × *Post*	0.022	0.018	0.018	0.017	0.025	0.025	0.025
	(0.008)	(0.009)	(0.007)	(0.007)	(0.007)	(0.006)	(0.008)
Mean *Y*	0.891	0.899	0.850	0.840	0.834	0.912	0.892
Individuals	54,015	37,393	66,981	68,048	77,474	409,095	50,219
Panel B. High school completion, ages 19–22							
Eligible × *Post*	0.059	0.050	0.037	0.029	0.034	0.031	0.059
	(0.010)	(0.015)	(0.008)	(0.008)	(0.010)	(0.010)	(0.012)
Mean *Y*	0.781	0.796	0.698	0.651	0.641	0.777	0.789
Individuals	38,704	25,393	61,550	76,235	87,132	291,278	35,920
Panel C. College enrollment, ages 19–22							
Eligible × *Post*	0.013	0.001	−0.005	−0.008	−0.005	0.007	0.012
	(0.010)	(0.016)	(0.010)	(0.010)	(0.011)	(0.008)	(0.009)
Mean *Y*	0.407	0.425	0.337	0.298	0.293	0.436	0.415
Individuals	38,704	25,393	61,550	76,235	87,132	291,278	35,920

Notes: This table shows difference-in-difference estimates of the impact of DACA on schooling decisions of eligible youth, with different restrictions. Column 1 contains baseline results from Tables 1 and 2. Columns 2 and 3 adjust the sample to include only individuals who arrived by age 6 (more restrictive) and by 16 (more expansive), respectively. Column 4 adds to the comparison group foreign-born individuals who arrived after age 16, column 5 adds foreign-born individuals who arrived after 2007, and column 6 adds individuals born in the United States. 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. The dependent variable is shown in the panel heading, and post is an indicator for 2012 or after. See the notes of Table 1 for the definition of eligibility, high take-up, control variables, clustering, and sample weights.

Source: 2005–2015 ACS. Sample composed of Hispanics ages 14–18 or ages 19–22.

Fourth, we consider other policies affecting undocumented immigrants during this period. While DACA was the only national-level policy, there were several local-level programs that were active, most notably Secure Communities, 287(g) agreements, and the Criminal Alien Program. These policies focused on detainment or deportation of undocumented criminals, which could have altered schooling decisions. Among these programs, Secure Communities stands out as a primary threat to identification as it experienced large expansions that coincided with DACA and influenced take-up of public assistance (e.g., Alsan and Yang 2019).[24] To address this potential confound, we control for the county-by-year rollout of Secure Communities using data from Alsan and Yang (2019), shown in online Appendix Figure A.5. Our estimates are unaffected by this control.

Fifth, since our analysis relies on survey data, one could be concerned that DACA might lead to changes in participation in the ACS, which could bias our estimated effects. Recall, however, that online Appendix Figure A.2 showed that we cannot predict our findings based on a large number of characteristics of survey participants. Hence, this explanation seems less plausible.

Finally, we also present propensity score re-weighted estimates as an additional method of controlling for omitted variable bias. This addresses potential concerns that the imbalance in background characteristics between eligible and ineligibale youth influences our results. We predict the likelihood of being eligible for each subsample and age group using a probit regression with a large number of individual and household covariates, described in the online Appendix. We then re-estimate our regressions using inverse-propensity score weighting. Online Appendix Table A.3 shows that the effects are the same as when we do not use this re-weighting.

VI. Schooling Evidence from California

Thus far, we have shown that DACA increased individual reports of high school attendance and completion. We use administrative data from California to extend these results in two ways. First, we examine whether DACA led to growth in high school attendance using official school reports, which addresses potential concerns about strategic reporting of school attendance. Second, we use data on the CAHSEE, a mandatory exam for graduation, to assess whether test performance changed alongside increases in attendance. The CAHSEE consists of two subject exams in mathematics and english language arts (ELA), and students must pass both in order to graduate. All students take the exam for the first time in tenth grade, and those who do not pass may take the exam again in eleventh and twelfth grades.

[24] Since 287(g) agreements took place before DACA (Watson 2013) that policy is less likely to contaminate our effects, and we find no evidence of significant responses in our pre-trend analysis. Furthermore, Dee and Murphy (2018) finds that 287(g) actually reduced high school enrollment, which implies that contamination from this program is likely to imply that our estimates are conservative. The Criminal Alien Program (CAP) also saw expansions many years prior to DACA. Recent changes dedicated additional funding to CAP circa 2014, after DACA was already implemented (Cantor, Noferi, and Martinez 2015). Nonetheless, we see treatment effects immediately after the implementation of DACA and before CAP funding in 2014.

A. *Empirical Strategy Using Geographic Eligibility*

Since administrative schooling data from California are available at the county level, we adapt our empirical strategy to exploit variation in the *concentration* of eligible youth across counties.[25] In particular, we compare the outcomes of Hispanic youth in counties that have a high share of eligible Hispanics relative to counties with a low share of eligible Hispanics, before and after DACA.[26]

We use the estimating equation

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

where Y_{ct} is a school performance measure for county c in year t and $HiShareElig_c$ is an indicator for having an above-median average share of eligible individuals among the Hispanic population ages 14 to 18 during the pre-DACA period between 2005 and 2011. We will refer to these as "high-undocumented" counties, and below-median-share counties as "low-undocumented" counties. We include county fixed effects, γ_c, to control for fixed differences in school quality and county composition, year fixed effects, γ_t, to control for statewide trends and changes in other state or federal policies, and county unemployment rates, U_{ct}, to account for differences in local economic conditions, especially during the Great Recession, that may influence schooling decisions. As before, we replace $Post_t$ with year indicators to estimate treatment effects over time. Standard errors are clustered at the county level.

B. *California Schooling Results*

We first analyze impacts on high school enrollment. We complement our main results by examining two different measures of attendance, high school enrollment and the number of CAHSEE test takers. To account for differences in population sizes across counties, we standardize all attendance measures by the average population of Hispanics ages 14–18 between 2005 and 2011. This serves two purposes: (i) it allows us to validate our earlier results using administrative data and slightly different identifying assumptions, and (ii) it allows us to establish a "first stage" on schooling attendance in this data before examining test scores.

Figure 6 presents the event study estimates for high school enrollment in panel A and the number of math and ELA test takers in panels B and C, respectively. Prior to 2012, the gap between high-undocumented counties and low-undocumented counties was relatively constant. All three figures show a clear rise in attendance in high-undocumented counties after DACA. The difference-in-difference estimates for these outcomes, included in online Appendix Table A.4 and in the notes below the figure, indicate that high-undocumented counties experienced a marginally

[25] This approach is similar to Cascio and Lewis (2018), who also utilize variation in the geographic concentration of unauthorized immigrants in the absence of individual-level information on legal status.

[26] Since we construct this treatment variable using the ACS, our analysis focuses on the 34 of California's 58 counties that are identified in the ACS that account for over 88 percent of total K–12 enrollment during the 2005–2015 period.

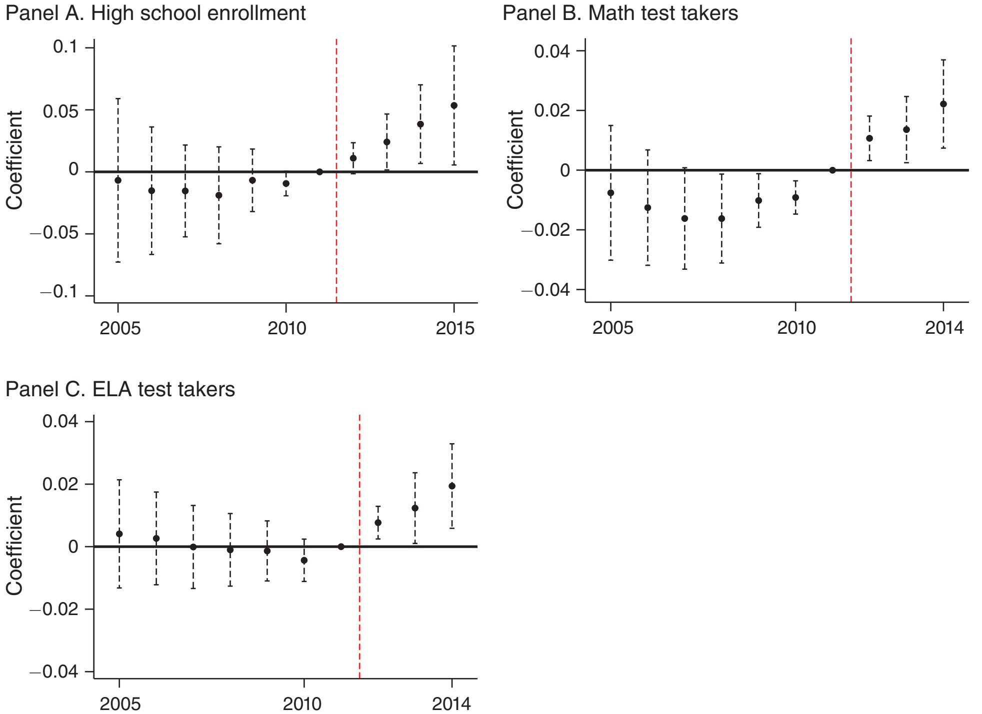

Figure 6. Effect of DACA on High School Attendance, California

Notes: This figure shows event study regressions that separately estimate interactions between year and an indicator for being a county with an above-median share of DACA-eligible Hispanics, where year 2011 is the omitted category. The outcomes are the share of youth that (i) are enrolled in high school (panel A), (ii) sat for the math CAHSEE (panel B), or (iii) sat for the ELA CAHSEE (panel C). The denominator for the shares is the average number of Hispanics aged 14 to 18 in the county between 2005–2011. Regressions include county fixed effects, year fixed effects, and a control for the county unemployment rate (see equation (2)). Regressions are weighted by the average number of Hispanics aged 14 to 18 in the county in the 2005–2011 ACS, and standard errors are clustered by county. The difference-in-difference estimate is 0.042 ($p = 0.108$) for enrollment, 0.024 ($p < 0.01$) for math test takers, and 0.013 ($p < 0.10$) for ELA test takers.

Source: Enrollment data for academic years 2005/06 to 2015/16 and CAHSEE data for 2005/06 to 2015/16, provided by the California Department of Education

significant 4.2 pp increase in high school enrollment and a significant 1.3 to 2.4 pp increase in ELA and math CAHSEE test takers, respectively.[27] Despite the different data source and identification, these estimates also show positive impacts on school attendance.[28]

Next, we analyze whether students put forth greater effort on exams. Online Appendix Table A.5 presents the effects on test performance for tenth, eleventh, and twelfth graders. Among first-time test takers in the tenth grade, DACA led to

[27] The p-value is 0.108 for high school enrollment; $p < 0.01$ for math test takers, and $p < 0.10$ for ELA test-takers.

[28] In online Appendix B, we calculate that the scaling factor to convert these estimates to ITT is between 9.7 and 16.3, which implies an order of magnitude larger enrollment effect relative to our ACS estimates. We speculate that this may be an overestimate of the ITT if noncitizens are underreported in high undocumented counties, although large effects are plausible if native Hispanics experienced spillover effects from the policy.

statistically significant decreases in performance on both the math and ELA exams. This decline should be interpreted in light of the increases in attendance, and hence the number of test takers. Marginal undocumented students—those induced by DACA to stay in school—are likely to be less prepared on average for the exam and lower scoring. Importantly, however, the magnitudes are quite small. The share of test takers passing each exam falls by 1 to 2 percentage points off of a baseline pass rate of 74 percent for both math and ELA. At the same time test scores decline by 1.6–3 points from average scores of around 370 in math and ELA.

Among repeat test takers in eleventh and twelfth grades, we find mixed results. The effect on eleventh grade performance is statistically insignificant, but average performance appears to rise for twelfth grade students. Pass rates for the ELA exam increase by a statistically significant 1.7 percentage points, which translates to a modest 7 percent of the mean. This suggests that undocumented students facing their final attempt to graduate and qualify for DACA may have increased effort, particularly with respect to English language.

VII. Discussion

Having focused on the average reduced-form effects on different education choices, we now separately consider the importance of deferral from deportation as an incentive for schooling. We then use our difference-in-difference estimates to construct a range of elasticities of schooling to changes in returns based on lifetime earnings and compare these to similar estimates in the literature.

A. *The Role of Deportation Risk*

We begin by analyzing differential schooling responses across men and women, recalling that the national deportation risk for men is over four times higher than the risk for women. Table 4, which focuses on 19–22-year-olds, shows that the effects for high school completion are more than twice as large for young men as for young women, and that these differences across sexes are statistically significant at the 10 percent level. We find no significant difference between men and women in college attendance. These results are consistent with youth responding to the differential national patterns in deportation risk across gender. Further, in the online Appendix, we rule out differences in opportunity costs as an alternative explanation.[29]

As a second test of the role of deportation risk, we ask whether schooling responses are correlated with *local* deportation risk, which we measure using the deportation rate in one's state of residence. We take a nonparametric approach, estimating difference-in-difference coefficients by state, and then plotting the coefficients, ranked in ascending order of state's deportation rate (see online Appendix E for data details). Online Appendix Figure E.2 shows no systematic relationship between the state deportation risk and the DACA-induced increase in schooling. For instance, we

[29] In particular, online Appendix Table A.6 shows that men substituted toward schooling from work and idleness, while women entirely substituted from idleness, perhaps due to a lower incidence of teenage births (Kuka, Shenhav, and Shih 2019). This suggests that men likely had a higher opportunity cost of schooling than women.

TABLE 4—EFFECT OF DACA ON HIGH SCHOOL COMPLETION AND COLLEGE ENROLLMENT, AGES 19–22: BY SEX

	High school completion			Some college		
	All	Female	Male	All	Female	Male
Panel A. Hispanic						
Eligible × *Post*	0.059	0.035	0.077	0.013	0.009	0.015
	(0.010)	(0.014)	(0.016)	(0.010)	(0.017)	(0.018)
Mean *Y*	0.781	0.812	0.754	0.407	0.456	0.363
p-value male = female			0.07			0.84
Individuals	38,704	18,501	20,203	38,704	18,501	20,203
Panel B. High take-up						
Eligible × *Post*	0.064	0.040	0.084	0.011	0.004	0.014
	(0.011)	(0.013)	(0.019)	(0.011)	(0.013)	(0.018)
Mean *Y*	0.775	0.806	0.747	0.399	0.446	0.357
p-value male = female			0.09			0.66
Individuals	34,768	16,574	18,194	34,768	16,574	18,194

Notes: This table shows the difference-in-difference estimates of the impact of DACA on the high school completion and attainment of some college of eligible youth ages 19–22. The outcomes are high school completion (GED or diploma) in columns 1–3 and completion of some college (more than 12 years of completed education) in columns 4–6, and post is an indicator for 2012 or after. See the notes of Table 1 for the definition of eligibility, high take-up, control variables, clustering, and sample weights.

Source: 2005–2015 ACS. Sample is composed of foreign-born individuals ages 19–22 who immigrated by age 10 and by 2007.

find similar-sized effects on teenage schooling in Arizona and New Jersey despite the fact that we estimate the deportation risk to be over 12 times higher in Arizona.[30] We come to the same conclusion when we allow the effect of DACA to vary linearly with the state-level deportation risk.

One interpretation of these results is that youth do not value reductions in deportation risk. Considering the magnitude of the schooling response, this seems unlikely. Our calculations of the increase in lifetime earnings resulting from DACA, which we discuss in more detail below, reveal that the expected earnings benefits from lower deportation risk account for 37–50 percent of the total benefits of DACA. Therefore, removing this benefit would imply doubling the schooling responses for each dollar of earnings benefits. We cannot rule out this possibility, but it seems less plausible. Our preferred interpretation is that youth react to *perceived* deportation risk, which is likely to be different than actual measured risk. For example, youth in low-deportation-rate states may overestimate the likelihood of deportation, consistent with the survey evidence in Section I, or youth in high-deportation states may overestimate the probability of return to the United States conditional on deportation. These misperceptions could rationalize the similar schooling responses across states.

[30] Further, in results not reported, we find that states with a higher deportation risk do not have a lower "legal premium," which could offset the response to deportation risk.

B. *From Reduced Form Effects to Schooling Elasticities*

We now convert our difference-in-difference estimates to the ITT effects of DACA and use these estimates to obtain a range of elasticities of schooling to lifetime earnings.

First, to recover the ITT effect of DACA we rescale our treatment effects to account for the fact that the eligible group includes legal immigrants. Given that our best estimates indicate that roughly 72 percent of noncitizen Hispanics are undocumented, we rescale our treatment effects by a factor of 1.39 to recover the ITT effects. Since the share undocumented tends to be higher for youth, we take this as a lower bound of the share of noncitizen youths that are undocumented, and hence our ITT effects are an upper bound.[31]

Second, we calculate the percent change in expected returns to schooling from DACA by taking the difference in expected lifetime earnings before and after DACA. This can be written as a function of (i) the expected years working in the United States and Mexico pre- and post-DACA, and (ii) home country, undocumented US, and documented US wages for each education level (see online Appendix C for more detail). We do not attempt to monetize the nonpecuniary 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 draw on a variety of sources to estimate these inputs, summarized in detail in online Appendix Table C.1. To calculate the expected duration of years working in the United States, we subtract the cumulative probability of deportation from the total number of working years. In our preferred estimate, we calculate this deportation risk for each age and sex prior to DACA using the number of removals from the United States in 2012 (Simanski and Sapp 2013) divided by the estimated population of undocumented immigrants. We conservatively assume for most calculations that the probability of deportation only declines to 0.5 pp during DACA receipt, and for simplicity assume that the perceived probability of return is zero.

We also consider three time horizons over which eligible individuals may have expected DACA benefits to last: four years, six years, or permanent. Although there is no data on these expectations, we surmise that four years may be the minimum expectation, given that the Obama administration was reelected for a four year term soon after the passage of DACA.

We assign the US wages for each legal status (citizens, noncitizens) and education level as the average wages of foreign-born adults that meet the sample immigration requirements 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 (Ruggles et al. 1790–2018).

Column 1 of Table 5 shows the elasticity of high school completion under each of these scenarios using the rescaled (ITT) schooling estimates for Hispanics.

[31] In contrast, since only 55 percent of eligible teens in the whole sample are likely to be unauthorized, the ITT effect is roughly 80 percent larger than the difference-in-difference estimates for the whole sample.

Table 5—Implied Elasticity of High School Graduation to Wages

	Actual deportation risk	Perceived deportation risk	
	Age-based	0%	30%
Panel A. Four Years expected duration			
Elasticity—All	0.086	0.174	0.079
Elasticity—Males	0.080	0.141	0.085
Elasticity—Females	0.129	0.171	0.052
Panel B. Six Years expected duration			
Elasticity—All	0.054	0.109	0.052
Elasticity—Males	0.050	0.089	0.056
Elasticity—Females	0.082	0.108	0.034
Panel C. Permanent expected duration			
Elasticity—All	0.019	0.027	0.014
Elasticity—Males	0.017	0.022	0.016
Elasticity—Females	0.024	0.027	0.009

Notes: Estimates of the elasticity of high school for all males, and female DACA-eligible youth, under various expectations of the duration of DACA (four years, six years, and permanent) and the deportation risk. Elasticity calculated using (i) the implied ITT effects of DACA for Hispanics (see Section VII) and (ii) estimates of the wage benefits of DACA using inputs from online Appendix Table C.1 together with the framework for expected wages in Section C.

Intuitively, the elasticity is larger when we assume shorter expected durations of DACA, since the benefits of DACA accrue over a shorter period. Our preferred elasticities rely on six years of duration, the actual duration of DACA. Under these assumptions, the elasticity of high school completion is between 0.05 and 0.08. Men and women exhibit similar responsiveness, indicating that the varying magnitudes of the difference-in-difference estimates are proportional to the respective changes in expected high-school-educated lifetime earnings.[32]

The remaining two columns show the sensitivity of our elasticity calculations to misperceptions in the deportation risk. In column 2 we assume the perceived risk to always be zero, and hence that the only earnings benefit from DACA comes from the higher wages afforded by legal status. Since the increase in the return to high school is much lower under this assumption, our average elasticity estimate doubles to 0.109. In column 3 we assume a 30 percent deportation rate, the highest from our state deportation rate calculations. These elasticities are similar to the baseline estimates.[33]

Is the size of our DACA responses comparable to prior work? The most similar estimates to ours come from Abramitzky and Lavy (2011, 2014), who study the high school completion response to a change in the wage returns to schooling in Israeli kibbutzim. Abramitzky and Lavy (2011) argues that the near-zero initial return to schooling in the kibbutz makes a *semi-elasticity* of high school graduation to the return for a year of schooling understanding the magnitude, and calculates this to

[32] See online Appendix Table C.3 for the equivalent college enrollment elasticities.

[33] The elasticity for 30 percent deportation risk is typically lower than for the age-varying deportation risk, consistent with higher benefits. Occasionally the 30 percent elasticity is higher, reflecting a higher return to high school at baseline (which reduces the percent increase in the return).

be 0.43.[34] When we calculate the semi-elasticity to the return to high school completion from our estimates, we find an average of 0.25, roughly 60 percent of the response in Israel.[35] Hence, a 10 pp increase in the return to high school graduation leads to a 2.5 percent increase in high school completion.

Taking these estimates at face value suggests that the response to DACA among undocumented youth may be smaller than among Israeli teens. One potential reason for this is that the perceived wage differences between Mexico, US legal, and US undocumented individuals could be smaller than the ones we calculate, which would cause us to underestimate the semi-elasticity. Second, undocumented youth may expected to return to the United States if deported, or have lower expectations about the probability of deportation, which would also make the perceived benefit lower than our calculation. Finally, undocumented youth may be less responsive than Israelis because they face other barriers to schooling, such as liquidity constraints, norms about working or helping out at home, or early childbearing, to name a few reasons.

Finally, we note that our analysis has focused on the undocumented population living in the United States, holding constant migration decisions, as DACA explicitly required an established presence in the United States. However, in the long run, DACA's benefits might encourage new immigration if potential migrants expect the United States to continue to pass DACA-like policies. This point, as well as any general equilibrium responses to DACA, are left for future investigation.

VIII. Conclusion

In this paper, we quantify the education 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 decisions of undocumented youth.

The policy increased school attendance by 2.2 pp and high school graduation rates by 6 pp, an effect that was more pronounced among Hispanic men. These effects imply that DACA reduced the citizen-noncitizen gap in school attendance by 55 percent, and the gap in high school completion by 40 percent. We find some increases along the college margin as well, but cannot reject a null effect in our main specification. Auxiliary analyses show that DACA also induced greater effort in school, as we find increases in the pass rates of a mandatory exam for graduation among twelfth graders.

These results have significant policy implications. First, they show that a substantial part of the gap in educational attainment between noncitizen and citizen youth is due to the low benefits of schooling associated with lack of legal status. Hence, policies that increase the real or perceived economic opportunities of disadvantaged youth may lead to a more educated workforce. Second, immigration policy

[34] This is likely to be an upper bound on the elasticity to the change in the return to high school.

[35] See online Appendix Table C.3 for the semi-elasticity estimates.

is currently at the center of the public debate, with many fearing that undocumented immigrants may bring undesirable attributes to communities, such as low levels of education. 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.

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