Does Peer Motivation Impact Educational Investments? Evidence From DACA

Briana Ballis*

February 27, 2021

Latest version available at: https://brianaballis.weebly.com

Abstract

Despite the significant influence that peer motivation is likely to have on educational investments during high school, it is difficult to test empirically since exogenous changes in peer motivation are rarely observed. In this paper, I focus on the 2012 introduction of Deferred Action for Childhood Arrivals (DACA) to study a setting in which peer motivation changed sharply for a subset of high school students. DACA significantly increased the returns to schooling for undocumented youth, while leaving the returns for their peers unchanged. I find that DACA induced undocumented youth to invest more in their education, which also had positive spillover effects on ineligible students (those born in the US) who attended high school with high concentrations of DACA-eligible youth.
JEL Codes: I26, H52, J15

* University of California, Merced. Department of Economics. 5200 N. Lake Rd., Merced, CA 95343. Phone: (631) 697-4036. Email: bballis@ucmerced.edu.

I thank Scott Carrell, Paco Martorell, Marianne Page, Marianne Bitler and Michal Kurlaender for many useful discussions and suggestions. I would also like to thank Umut Ozek, as well as seminar participants at the Association for Education Finance and Policy (AEFP) 44th annual conference; the UC Davis Alumni Conference, Western Economic Association International (WEAI) 94th annual conference, University of Hawaii at Manoa, Association for Public Policy Analysis & Management (APPAM) 41st Annual Conference; Fall Migration Research Cluster Workshop (UC Davis); Southern Economic Association (SEA) 89th annual conference, the 5th IZA Workshop: The Economics of Education, the University of South Carolina, and . The conclusions of this research do not necessarily reflect the opinions or official position of Los Angeles Unified School District.

1 Introduction

A substantial literature documents the importance of peer influences as an input to economic mobility (Sacerdote, 2011). However, the existing empirical literature mostly focuses on estimating the existence of peer effects rather than on the influence of specific peer attributes. For example, the motivation of one's high school peers is believed to have a strong influence on long-run trajectories. Despite this belief, little is known about the exact degree to which peer motivation impacts schooling investments during adolescence, if at all. Better understanding how specific attributes of peers, such as peer motivation, influence schooling investments, will likely yield important insights in understanding the root causes of educational underachievement and for corrective policy design.

This paper uses the 2012 introduction of DACA as a natural experiment that changed the returns to schooling among some high school students, without changing the incentives for others. Under DACA, undocumented youth who completed high school could receive temporary protection from deportation and work authorization.[1] Thus, DACA dramatically increased the incentives for undocumented youth to complete high school. Indeed, prior work suggests that the introduction of DACA significantly increased the likelihood that undocumented youth completed high school, by as much as 7.5 percent (Kuka, Shenhav, & Shih, 2020). In this paper, I add to the literature on DACA by first showing that DACA also led to improvements in achievement among undocumented youth suggesting motivation likely increased in response to the policy, and ultimately provide the first evidence of educational spillovers on US-born students due to DACA. Studying the impact DACA had on US-born students (who were not DACA-eligible) provides an ideal natural experiment to better understand the responsiveness of educational investments to changes in peer motivation.

Beyond the contributions this paper makes to the peer effects literature, understanding the spillover effects of DACA also has important policy implications for the DACA program itself. DACA is an important immigration reform that has remained at the forefront of public discourse

[1] DACA also required undocumented youth to meet specific age/date of arrival criteria and to have never committed a felony. Section 2 provides more detail on these other DACA-eligibility criteria.

and current immigration policy debates. Previous studies on DACA have focused exclusively on the direct impacts DACA had on undocumented youth, but these studies have ignored the possibility of spillovers on US-born. As the program continues to be contested politically, fully accounting for the costs and benefits of this program are crucial for current and future policy debates.[2]

I use administrative data from Los Angeles Unified School District (LAUSD) together with administrative data on DACA applicants from the U.S. Citizenship and Immigration Services (USCIS). These data allow me to create cleaner proxies for students' legal status than have been used in the past and reduces measurement error. Specifically, I combine information from the LAUSD on students' country of birth and current zip-code of residence with the USCIS information on DACA applications by zip code to determine each students' likely eligibility. To identify the direct impact of DACA on undocumented youth, I compare changes in educational outcomes of foreign-born students living in zip-codes with higher concentrations of DACA-eligible youth (who were more likely to be undocumented) to those with lower concentrations (who were likely foreign-born citizens), before and after the introduction of DACA. To identify the spillover effects of DACA, I compare changes in the educational outcomes of US-born students in high schools with higher concentrations of DACA-eligible peers to those in high schools with lower concentrations.

I find that DACA led to significant increases in targeted students' educational investments. High school graduation increased by 6 percentage points (or 10 percent) among youth who were likely undocumented. The effects are driven by males and students who were initially low achievers. These groups are typically at risk of dropping out of high school and would have been more likely to respond to DACA's educational incentives. The magnitude of this effect are similar to Kuka et al. (2020), who focus on a national sample. In addition, I find that DACA led to significant improvements in English Language Arts (ELA) achievement and GPA among likely undocumented youth, whose ELA achievement increased by 0.14 standard deviations after DACA's enactment. As students would have had to exert additional effort in order to experience these performance

[2] As will be discussed in more detail in Section 2, the Supreme Court recently ruled against an attempt by the Trump administration to terminate DACA in June 2020. However, uncertainty over DACA's future persists (Totenberg, 2020).

improvements, these results suggest that undocumented youth increased effort in response to DACA. Then, I show that this increased effort had positive spillover effects on undocumented students' US-born peers: at the average campus, where approximately 1 percent of students were likely to be undocumented, DACA's introduction leads to a 2 percentage points (or 4 percent) increase in US-born students' probability of graduating from high school. These results are driven by low-achieving US-born students. Achievement on ELA exams during high school also increased by 0.06 standard deviations after DACA's enactment for US-born. Gains in achievement occurred for all US-born students, regardless of baseline achievement.

These spillovers are consistent with several possible mechanisms. First, US-born students may have been affected by direct peer-to-peer influences: increased effort among DACA-eligible students may have inspired their US-born peers to study harder. Second, improvements in undocumented youths' motivation may have freed up teachers' and administrators' time for other instructional improvements. Finally, the introduction of DACA may have led to other administrative changes at the school level. For instance, if schools trained guidance counselors to better understand the process of college admissions for DACA-eligible students, this training could have spilled over to their US-born peers.[3] To shed light on which mechanism is most likely, I investigate whether the spillovers are driven by closer contacts.[4] If peer-to-peer influences are driving these results, closer contacts should have a larger influence. However, if school-wide changes in instruction are the primary mechanism, closer peers should not necessarily have a stronger influence. Consistent with peer-to-peer influences, I find that the spillovers are driven by closer contacts.

This paper contributes to three key literatures. First, it adds to the small but growing literature on spillover effects of policies that increase the returns to schooling. While there is an existing literature that estimates the direct impact of increasing the returns to education for specific student groups (Kuka et al., 2020; Abramitzky & Lavy, 2014), I am aware of only one other study

[3]It is also important to acknowledge that since DACA induced lower-achieving students to stay enrolled in school, this may have taken up teachers time (or school level resources in general) to the disadvantage of their US-born peers. Given the pattern of results I document (i.e. positive spillovers), it is unlikely that this is the primary mechanism.

[4]As will further be justified in Section 5.2, to proxy for closer contacts I focus on students from the same middle school (as they are more likely to have stronger ties and longer-lasting friendships).

that tests whether such policies spillover to non-eligible peers (Abramitzky, Lavy, & Perez, 2021), who find that a pay reform change that improved high school outcomes among kibbutz members in Israel also increased educational attainment for non-kibbutz peers. However, Abramitzky et al. (2021) can only address whether there are spillover effects on the margin of college enrollment because high school completion was so high in their setting (over 95 percent were completing). My project builds upon this recent work by addressing whether policy spillovers exist on the margin of high school completion among students in a large low-performing school district in the US.

Second, it adds to the small literature that analyzes the impact of peer personality traits on educational outcomes. Two recent studies have shown that having more persistent peers in university (Golsteyn, Non, & Zölitz, 2020) and more motivated elementary school peers (Bietenbeck, 2020) lead to improvements in contemporaneous performance, but that these peer personality traits do not influence long-run outcomes. My context is unique within this literature, as I am able to focus on a plausibly exogenous increase in peer motivation (driven by policy) among existing peer groups (where established friendships are expected). Moreover, I focus on a critical time during adolescence both when conforming to social norms may be especially important and also a time right before critical human capital decisions are typically made (i.e. high school completion and college enrollment). While I cannot formally distinguish whether the increase in achievement that I document among undocumented youth is driven by a change in their intrinsic personality or an improvement in one's family environment perhaps driven by a reduction in fear of deportation, I clearly find that DACA led these youth to invest more in their education. It is the spillovers from this exogenous change in peer motivation to do well in school that I identify in this paper.

Finally, I contribute to the emerging literature on the impacts of DACA. To date, most studies have focused on understanding how the policy affected DACA-eligible students who completed high school, and focus on the policy's impact on their labor market and college outcomes (Pope, 2016a; Amuedo-Dorantes & Antman, 2017; Hsin & Ortega, 2018). Only one other study has focused on DACA-eligible youth who experienced DACA during high school (Kuka et al., 2020). Kuka et al. (2020) use the American Community Survey (ACS) and find high school graduation rates increased by 2.2. to 7.5 percent for DACA-eligible youth. I am able to make three important contributions to the literature on DACA. First, I am able to examine intermediate outcomes, which allows me to test whether DACA led to increased effort in school. Second, I am able to consider the educational spillover effects of this policy. Third, using zip-code level variation in the concentration of DACA applicants to approximate the undocumented population allows me to estimate DACA-eligibility with less measurement error than prior studies that largely rely on the absence of citizenship as a proxy for undocumented status.[5]

2 Policy Background

Signed into law under an executive order in June 2012 by former President Barack Obama, DACA provides temporary protection from deportation, and a work permit for undocumented youth who entered the US as children. DACA eligibility requires that individuals meet a series of age/date of arrival criteria (i.e. arrival to the US before age 16 and by June 2007)[6] and minimum education requirements.[7] Specifically, to be program eligible, undocumented youth are required to complete high school, earn a general educational development (GED) certificate (or equivalent), or currently be enrolled in school. To continue receiving benefits, recipients must re-apply every two years.

To apply for DACA, individuals have to fill out the application forms, pay a processing fee of $465 and provide documentation to demonstrate that all of the eligibility criteria are met. There was an immediate surge in applications once the US Citizenship and Immigration Services (USCIS) began accepting applications on August 15, 2012. Roughly 30% of the of the estimated eligible population of 1.7 million applied within the first year (Passel and Lopez, 2012). In Los Angeles, take-up of DACA was even higher. Dividing the 72,180 initial applications received in

[5]Using foreign-born non-citizens is the most common way to approximate the undocumented population in the literature on DACA (e.g. Pope (2016a); Kuka et al. (2020); Amuedo-Dorantes and Antman (2017)), however, this is measured with noise, as non-citizens include green card holders and temporary visa holders.

[6]These age/date of arrival criteria require undocumented youth to reside in the US for at least 5 years. Thus, DACA-eligible youth are not recent immigrants. Because DACA eligible youth had already been living in the US for a significant amount of time when the policy was implemented, they were likely to be well integrated with their peers.

[7]They also were unable to commit a felony. The number of eligible youth with felonies is likely small (Patler, 2018).

2012 - 2014 in Los Angeles county by the 111,000 youths estimated to be immediately eligible for DACA (Batalova, Hooker, & Capps, 2014) yields a take-up rate of 65%.[8] The significantly higher take-up in Los Angeles can likely be attributed to the strong presence of pro-immigration rights groups who undertook extensive outreach activities immediately after DACA's enactment.[9]

Since DACA's introduction in 2012 it has been contested politically and has faced several legal challenges. The first major attack on DACA occurred in August 2016, with the presidential campaign of Donald Trump during which he promised to terminate the program if elected president (Chishti, Bolter, & Pierce, 2017). In 2017, shortly after being elected, the Trump administration argued that DACA was unlawful, and announced plans to terminate the program (Ruiz Soto & Capps, 2017). By 2018, the federal government was no longer accepting new applications, and was only accepting renewals. While the Supreme Court blocked the Trump administration's attempt to terminate DACA in June 2020, the future of the policy remains unclear (Totenberg, 2020).

2.1 Education Incentives for Undocumented Youth

A human capital investment model proposed by Kuka et al. (2020) illustrates how DACA likely incentivized undocumented youth to invest more in their education. To briefly summarize this model, Kuka et al. (2020) consider undocumented youth choosing a level of education (high school drop-out, high school completion, or college) based on expected lifetime earnings. DACA recipients experience an increase in expected lifetime earnings for two reasons. First, DACA recipients receive a work permit. This increases the expected wage at all education levels from the non-legal to the legal wage.[10] Second, DACA temporarily eliminates the risk of deportation. This increases the number of years undocumented youth expect to live and earn US wages, which are typically higher

[8]Counts of DACA applicants in Los Angeles county are based on the author's calculations using USCIS data described in more detail in Section 3.

[9]Pro-immigration rights groups in Los Angeles such as CHIRLA (The Coalition for Humane Immigrant Rights) were instrumental in advertising DACA and providing legal and financial DACA application assistance to help applicants acquire the correct documents to prove residency. It is also important to note that local Spanish media (TV and radio), as well as the Catholic Church played a critical role in advertising and encouraging those eligible to sign-up for DACA in Los Angeles (information from https://www.lamayor.org/community_based_organizations and www.daca100.org).

[10]Undocumented individuals face a "wage penalty" in the US. Prior literature finds that legalization raises wages between 6 to 14 percent (Rivera-Batiz, 1999; Kossoudji & Cobb-Clark, 2002; Borjas, 2017).

than wage offered in undocumented youth's country of origin at all education levels.[11]

Because high school completion is tied to DACA eligibility, the model predicts that undocumented youth will be incentivized to complete high school to benefit from the increase in expected lifetime earnings associated with becoming a DACA recipient. However, even if undocumented youth do not consider the change to expected lifetime earnings driven by DACA, they may still choose to complete high school if they prefer living in the US, and value the temporary protection from deportation DACA offers.[12] Since the returns to college will also increase with legalization due to DACA, undocumented youth may also be incentivized to enroll in college.[13] Consistent with this model, Kuka et al. (2020) find that nationally likely undocumented youth experienced significant increases in high school completion in response to DACA, and positive (but imprecise) increases in college enrollment.

2.2 Undocumented Population in Los Angeles

Los Angeles provides an ideal setting to study the effects of DACA on student outcomes. Los Angeles is home to the largest percentage of DACA-beneficiaries in the US, accounting for 14 percent of all beneficiaries (Parlapiano & Yourish, 2018). As previously noted take-up of DACA was high in Los Angeles, and was very popular among students in the city. In fact, the introduction of DACA was in part motivated by a decade long student-led movement based in Los Angeles that had been advocating for a path to citizenship (Nicholls, 2013).[14] In addition to broad support among students, there was also a lot of DACA outreach done in LAUSD high schools. DACA application clinics were set-up in high schools as part of the DACA100 campaign that aimed to

[11] Kuka et al. (2020) assume that at every level of education, undocumented youth will earn more in the US relative to their country of origin. For the typical country of origin, Mexico, this assumption is plausible.

[12] Moreover, students enrolled in high school may have been additionally incentivized to become DACA-eligible in order to obtain a driver's license and to have the ability to work a part time job while in high school. Only as of January 1, 2015 were undocumented immigrants in California able to obtain a driver's license.

[13] Undocumented residents in California had been eligible for in-state tuition since 2002. However, they only became eligible for state financial aid through the introduction of the California Dream Act in 2012. Therefore, at the time of DACA's introduction, undocumented youth also experienced increases in college affordability, thereby, increasing the incentives to enroll in college at this time. I turn to a discussion of this policy change in more detail in Section 6.

[14] DACA has remained popular among Los Angeles high school students who have continued to protest for DACA as it has been challenged at the federal level in recent years (Stewart, Silverio-Bautista, Moran-Perez, & Parsley, 2019).

increase enrollment among high school students who were eligible (Singer, Svajilenka, & Wilson, 2015).[15] In other parts of the country where less outreach was targeted towards high school students, the immediate benefits of DACA among high school students may have been less salient.

Moreover, prior to DACA's enactment, educational attainment in Los Angeles was low relative to the rest of the US. In 2012, only roughly 60% of all high school students graduated from high school on-time.[16] Among those who were undocumented who met all of the age and date of arrival DACA criteria 30% of had already dropped out of high school (McHugh, 2014), and for those who completed high school, most (slightly over 70%) did not pursue higher education.[17]

In terms of spillovers, Los Angeles provides a unique setting since undocumented youth share very similar ethnicity and socio-economic backgrounds with their US-born peers. Over 86% of DACA applicants in California come from Mexico (Svajlenka & Singer, 2013), and roughly 60% of children living in Los Angeles have parents who were born in Mexico. Moreover, as previously noted, DACA-eligible youth are not recent immigrants.[18] Most have spent the majority of their schooling in LAUSD, thereby increasing the likelihood that DACA-eligible youth were well integrated with their US-born peers at the time the policy was introduced.

3 Data

I leverage administrative data from the Los Angeles Unified School District (LAUSD), and focus on students entering 9th grade between 2007 and 2014.[19] The data track key academic and behavioral

[15]In addition to high school students, DACA100 targeted parents of elementary school students, and those in the surrounding community to sign-up for DACA (information from www.daca100.org). Because of the age/date of arrival criteria for DACA, middle and elementary school students were ineligible for DACA.

[16]Based on author's calculations, roughly 60 percent of entering 9th graders in 2009 completed high school within four years of entering 9th grade. This estimate does not adjust for the possibility that these students may have enrolled in another district, or subsequently received a GED. However, restricting the sample to those observed in 9th grade minimizes the possibility that these students left LAUSD for any other reason than deciding to drop-out of high school.

[17]In 2012, only 20% of potentially eligible youth who completed high school were enrolled in college and 7% completed a college degree in Los Angeles (McHugh, 2014).

[18]In 2012, DACA-eligible youth were required to have immigrated to the US before 2007. The median age of US entry among DACA-eligible youth was 6 while the most common age was 3 (Parlapiano & Yourish, 2018).

[19]This includes 9th grade cohorts who were unexposed (2007-2009), partially exposed (2010-2012) and fully exposed (2013-2014) to DACA during high school. Appendix Table A.1 shows DACA exposure by each 9th grade cohort.

outcomes yearly, including attendance rates, state standardized exam scores (which I normalize to have a mean zero and standard deviation one at the grade-year level), disciplinary actions, semester GPA, the California High School Exit Exam (CAHSEE), SAT scores, yearly enrollment indicators and whether a student graduated from high school. Importantly, LAUSD data also includes each student's country of birth, date of arrival to the US (if foreign-born), and current zip-code of residence. To estimate the spillover effects of DACA, I focus on students who were born in the US, who are unlikely to be affected by DACA except through policy spillovers. The final sample I use to estimate the spillover effects of DACA consists of 238,781 US-born students.

However, like other studies' I cannot directly observe whether a foreign-born student is undocumented. Instead, I combine information on whether a student is foreign-born together with the concentration of DACA applicants in their zip-code of residence, to approximate undocumented status. The more foreign-born residents who applied to DACA in a students zip-code of residence, the higher the corresponding likelihood that a student is undocumented.

Specifically, I use administrative data on the number of DACA applications by zip-code and year provided by the U.S. Citizenship and Immigration Services (USCIS), together with estimates of the number of foreign-born residents by age, zip-code and year provided by the ACS. Then, for each zip-code, I construct an estimate of the share of foreign-born youth who applied to DACA immediately after DACA's enactment as follows:

$$\text{ShareEligible}_z = \left(\frac{\text{Total DACA Applicants (July 2012- December 2013)}}{\text{Foreign-Born Youth (CY 2014)}} \right)_z \quad (1)$$

where the numerator is constructed from USCIS data and the denominator is constructed from the ACS.[20] For each foreign-born student, I use this measure to proxy for their likelihood of being

[20] As just noted, the numerator counts the number of DACA applicants by zip-code between July 2012- December 2013. Given DACA's eligibility criteria, these applicants were ages 15-30 as of June 2012. In the denominator, I focus on the total number of foreign-born youth in a zip-code who were of similar ages across a similar period. Specifically, the denominator counts the foreign-born population ages 15-29 in each zip-code using 5-year ACS estimates from 2014.

undocumented based on their zip-code of residence (which I observe in LAUSD data).[21] As illustrated in Figure 1, there is significant variation in this measure across Los Angeles zip-codes.[22]

Importantly, since take-up of DACA was high in Los Angeles county (over 65%), this measure likely estimates the undocumented population with minimal measurement error. Nevertheless, Equation 1 will undercount the undocumented population living in a zip-code. However, as long as take-up of DACA across zip-codes was uncorrelated with trends in educational outcomes, this undercounting is unlikely to confound my estimates. While I am not able to test this assumption directly, event-study plots presented in Section 4.3 demonstrate that educational outcomes of foreign-born Hispanic youth in zip-codes with different concentrations of DACA-applicants had similar trends prior to DACA's enactment (especially for low-achievers who were most impacted).[23] Moreover, in Section 6 I show that my results are similar using other measures to approximate undocumented status that do not select on the DACA application decision (e.g. the share of foreign-born non-citizens). It is therefore reassuring that I find quantitatively similar results either way.

In addition to using Equation 1 to identify likely undocumented status, I also use country of origin and age of US arrival to identify foreign-born youth who were more likely to be DACA-eligible. In California, over 95% of DACA applicants are Hispanic, with the vast majority born in Mexico (86%) (Svajlenka & Singer, 2013). Therefore, to estimate the direct impacts of DACA I limit my focus to Hispanic foreign-born students.[24] In addition, DACA applicants had to have lived

[21]As a robustness check in Section 6, I focus on an alternate measure that focuses on DACA-eligible youth who were high-school aged. Specifically, for each zip code, I take the total number of DACA applicants and multiply by 0.40, since 40% of DACA applicants in Los Angeles county were ages 15-19 according to official USCIS estimates (USCIS, 2014), and then divide by the number of foreign-born ages 15-19 in a zip-code using 5-year ACS estimates from 2014. Reassuringly, using this alternate measure yields very similar results.

[22]Appendix Figure A.1 decomposes how much variation there is in the numerator and denominator of Equation 1.

[23]While I observe the number of DACA applicants by zip-code, I do not observe the number of undocumented youth by zip-code. Therefore, I cannot compute DACA take-up by zip-code. To provide some evidence that take-up was likely similar across zip-codes, I show in Appendix Figure A.2 that there is no correlation between the fraction of DACA applicants (measured by Equation 1) and the fraction of the non-citizen population over 18 years old living in poverty or with less than a high school education. This suggests that poverty rates and educational attainment (factors that could affect DACA take-up) were relatively similar across zip-codes with different concentrations of DACA applicants. One caveat here is that these educational and economic measures focus on foreign-born non-citizen population over 18 which does not correspond to the DACA-eligible population who were ages 15-30 as of June, 2012.

[24]This sample restriction does not drop many students. Of all foreign-born youth who arrived to the US by age 9 in 9th grade cohorts between 2007 and 2014, 83% are Hispanic.

continuously in the US since June 15, 2007. This imposes a different maximum age of US arrival across 9th grade cohorts. As an example, 9th grade students from 2007 (the oldest cohort) were 14 in 2007, while 9th graders in 2014 (the youngest cohort) were 9 in 2007. Therefore, I also limit my focus to Hispanic foreign-born students who arrived to the US by age 9. This final restriction ensures that any foreign-born youth in my sample would have been eligible for DACA if they were undocumented regardless of their cohort. The final sample I use to estimate the direct impacts of DACA consists of 21,139 students.

Finally, my measure of DACA exposure for US-born students builds upon Equation 1. Specifically, I approximate the share of a student's peers who were *likely* DACA-eligible as follows:

$$\text{DACAShare}_{sc} = \text{FBShare}_{sc} \times \left(\frac{\sum_{z=n}^{N} \text{n}_{scz} \times \text{ShareEligible}_z}{\text{n}_{sc}} \right)_{sc} \tag{2}$$

where s and c represent high school campus and 9th grade cohort respectively. FBShare_{sc} is the share of Hispanic foreign-born youth (who arrived to the US by age 9) in each student's campus as of 9th grade, rescaled by the second term which captures the likelihood that these foreign-born peers were undocumented.[25] Specifically, this second term is the weighted average of the zip-code DACA application measure defined in Equation 1 across the residence zip-codes of Hispanic foreign-born youth (who arrived to the US by age 9) *enrolled* in each student's campus as of 9th grade.[26]

3.1 Summary Statistics

Table 1 presents summary statistics for 9th grade cohorts enrolled between 2006-07 and 2013-14. Columns 2 vs. 3 compares US-born students to foreign born students in LAUSD. The vast majority of US-born and foreign-born students are Hispanic (roughly 77 percent) and participate in Free-Lunch (roughly 65 percent). Foreign-born students are slightly more likely to be classified as an

[25] This measure is a school-level share that varies by 9th grade cohort, however, estimates that focus on cohort-level shares yield nearly identical results. For high school students who take classes and interact with students across different grades, this school-level share is likely to more accurately capture relevant peer groups.

[26] Here, n_{sc} indicates the number of Hispanic foreign-born students (who arrived by age 9) overall, and n_{scz} indicates the number of Hispanic foreign-born students (who arrived by age 9) living in a given zip-code in each student's campus as of 9th grade.

English Learner and have slightly lower baseline ELA scores, but performed similarly at baseline on the math exams. The similar ethnicity and economic background of US-born and foreign-born students in Los Angeles suggest that spillovers due to DACA were likely.

Columns 3-6 of Table 1 compare foreign-born youth by ethnicity and age at US arrival. Relative to all foreign-born youth, those of Hispanic ethnicity are lower achieving at baseline, but are equally likely to be classified as an English language learner (ELL) and be receiving free or reduced price lunch (FRL). Hispanics and Mexicans who arrived to the US before they were 9, have similar baseline achievement to all foreign-born students, but lower achievement relative to US-born students. Despite these differences in baseline achievement, on-time high school completion (defined within four years of 9th grade), is similar across all the subgroups shown in Table 1.[27]

Table 2 presents summary statistics that compare high school campuses with different concentrations of likely undocumented students. Students in campuses with higher fractions of likely undocumented youth are more likely to be Hispanic, participating in ELL programs, receiving FRL, and are lower performing at baseline. While all campuses have fairly similar shares of foreign-born students, foreign-born students in campuses with higher concentrations of likely undocumented youth are more likely to be born in Mexico. It is important to note that while my peer effects identification strategy does not require that the fraction of likely undocumented youth in a school be uncorrelated with school characteristics, it does require that the fraction of undocumented youth is uncorrelated with changes in outcomes that occur for any reason other than the introduction of DACA. So while these differences do not pose a direct threat to my identification strategy, it is important to rule out the possibility that these demographic differences do not introduce a later divergence in trends. Reassuringly, I demonstrate in Section 6 that my results are robust to the inclusion of time trends interacted with baseline campus demographics.

[27] In this paper, I focus on on-time graduation in order to include more recent 9th grade cohorts.

4 Direct Impacts

4.1 Empirical Strategy

The first objective of this paper is to determine whether the increased returns to schooling due to DACA impacted educational investments of undocumented youth in Los Angeles. If I could directly observe legal status then I could compare changes in educational investments of undocumented youth who exogenously experienced an increase in returns to schooling in 2012, to changes in educational investments among foreign-born citizens who were not eligible. However, as previously noted, this strategy is infeasible because I cannot directly observe a students' legal status.[28]

Instead, I leverage differences across foreign-born youth in their *likelihood* of being undocumented by exploiting the concentration of DACA applicants in their zip-code of residence as defined in Equation 1 and whether they were enrolled in high school after DACA's enactment. Again, the more foreign-born residents who applied to DACA in a students zip-code of residence, the higher the corresponding likelihood that a foreign-born student was undocumented, thus any effect of DACA should be increasing with the concentration of DACA applicants in ones zip-code of residence. My estimation equation thus takes the following form:

$$Y_{izc} = \delta_0 + \delta_1(\text{ShareEligible}_z * \text{Exposed}_c) + \lambda_1 Z_i + \gamma_s + \gamma_z + \phi_c + \varepsilon_{izc} \tag{3}$$

where Y_{izc} is an indicator for high school completion on-time (i.e. within 4 years of 9th grade) for foreign-born student i in 9th grade cohort c living in zip-code z. ShareEligible_z is the fixed concentration of DACA applicants in a student's zip-code of residence as defined in Equation 1, and is interacted with an indicator for whether a student attended high school after DACA's enactment.[29]

[28] This challenge is not unique to this paper. To my knowledge, there are no available datasets that contain information on undocumented status and educational outcomes for a large representative sample. Most of the prior literature has relied on the absence of US citizenship and Hispanic ethnicity as a second best measure for undocumented status (Kuka et al., 2020; Pope, 2016a; Amuedo-Dorantes & Antman, 2017; Kaushal, 2006).

[29] Results are qualitatively and quantitatively similar if instead I interact ShareEligible_z with the number of years each 9th grade cohort was expected to be enrolled in high school after DACA's enactment.

I control for zip-code (high school campus) γ_z (γ_s) fixed effects to account for fixed cross-sectional differences across zip-codes (high school campuses), and cohort controls ϕ_c to account for trends in high school completion that could affect all students in Los Angeles. Z_i includes individual characteristics that include age of arrival to the US, gender and disability status, all measured in 9th grade, as well as ELA standardized test scores measured in 8th grade.[30] Finally, Z_{sc} accounts for school by cohort demographics that include the fraction of students who are male, by racial group (Hispanic, White, and Black), and receiving special education, all measured as of 9th grade. The main variable of interest, δ_1, identifies the average impact of DACA on the outcomes of likely undocumented youth.

The main identification assumption is that educational outcomes would have evolved similarly for foreign-born Hispanic students in zip-codes with different fractions of DACA applicants in the absence of DACA. In order to test this assumption, I estimate an event-study specification that replaces Exposure_c from Equation 3 with 9th grade cohort indicators. This event-study allows me to visually detect any differences in outcomes between likely undocumented youth and likely citizens before and after DACA's enactment. These event-study results are presented in Section 4.3 and provide evidence in favor of this parallel trends identification assumption. In addition, I consider other education and immigration policies during this period in Section 6. Overall, I do not find evidence that there are any other contemporaneous policy shocks that could have differentially impacted foreign-born students living in zip codes with different fractions of DACA applicants.

4.2 Results

I begin by establishing whether DACA increased high school enrollment and completion among likely undocumented youth. Difference-in-differences estimates are presented in Table 3. I find that likely undocumented youth were significantly more likely to be enrolled during grades 11

[30] I do not control for free or reduced price lunch status. Parents must apply to receive free-lunch, and parents who are undocumented may be less likely to apply. I also do not include an indicator for whether a student received ELL services at baseline. Across this time, the fraction of 9th grade students participating in ELL was declining. Prior research has found that being re-classified during high school has no impact on academic performance (Pope, 2016b). Nonetheless I explore several robustness checks in Section 6 that demonstrate it is unlikely that this change in ELL classification are driving these results. Reassuringly, results are robust to including these control variables.

through 12 and complete high school after DACA's enactment.[31] Starting with a model that only includes 9th grade cohort indicators, school fixed effects, and zip-code fixed effects, I successively add controls. The estimated effects are largely stable to the choice of specification. These results suggest that foreign-born youth who lived in the average zip-code (where 14 percent of foreign-born youth had applied to DACA), were 2.5 p.p. (or 3.2 percent) more likely to be enrolled in 12th grade and 3.5 p.p. (or 6 percent) more likely to complete high school after DACA's enactment.[32] In order to account for multiple inference (Kling, Liebman, & Katz, 2007), I also examine the impact of DACA on a summary index of educational attainment, which is computed as the equally weighted average of the z-scores of high school completion and enrollment in each grade. The results using this summary measure also indicate an improvement in the educational attainment.

Intermediate Outcomes – Next, I investigate whether DACA led to changes in behavior and achievement. On the one hand, it is possible that these increases in educational attainment were accompanied by increases in effort. This could either be because additional effort was required in order to be able to graduate, or because likely undocumented youth became more motivated after DACA's enactment. On the other hand, it is possible that DACA induced students to simply remain enrolled in school (to obtain a diploma), but was not accompanied by any changes in effort.[33] The extent to which any increases in educational attainment among likely undocumented youth would spillover to US-born peers will depend on which of these two scenarios was more likely.

Table 4 presents difference-in-differences estimates from a slightly modified version of Equation 3 using yearly outcomes as the outcome variables.[34] Specifically, I focus on yearly attendance rates, an indicator for whether a student was suspended within the year, ELA achievement,

[31] I do not find significant increases in 10th grade enrollment. As students are required to be enrolled in school until they are 16 (which will occur for most students in 11th grade), a non-significant relationship for 10th grade enrollment is consistent with students waiting to drop-out until they are legally able to do so.

[32] Foreign-born students living in the zip-code with the maximum share of DACA applicants (25 percent), were 4.5 p.p. (or 5.7 percent) more likely to be enrolled in 12th grade and 6.2 p.p. (or 11 percent) more likely to complete high school after DACA's enactment. For a foreign-born student living in a zip-code where 100 percent of foreign-born youth took up DACA, the effect size is likely to be even larger, however, there are no such zip-codes in Los Angeles.

[33] This scenario could occur if prior to DACA those undocumented students on the margin of high school completion dropped out because they no longer wanted to be enrolled in school, as opposed to dropping out because they were unable to meet the high school graduation requirements.

[34] Specifically, I estimate the following difference-in-difference specification:

and cumulative GPA. Starting with a model that only includes campus-grade, year-grade, and zip-code fixed effects, I successively add controls. The estimated effects are largely stable to choice of specification. DACA did not impact attendance rates, but increased the likelihood of being suspended, cumulative GPA, and ELA performance. In the fully specified model, these estimates suggest that Hispanic foreign-born students who lived in the average zip-code (with 14 percent of foreign-born students applying to DACA) are 1.4 p.p. more likely to be suspended, experience an improvement in GPA of 0.07 points (off of a mean of 2.26) and experience a 0.07 standard deviation increase in ELA performance.[35] In addition, the results using a summary index of academic achievement also indicate an improvement in performance among likely undocumented youth.

One caveat for these findings is that DACA induced undocumented youth to continue in school (as shown in Table 3). Thus, estimates of yearly outcomes that focus on students enrolled throughout high school may be subject to compositional changes. While I will later demonstrate in Section 4.3 that overall, there was not a significant change in the observable composition of likely undocumented students enrolled throughout high school, statistically insignificant compositional changes or those based on unobservables is still possible. If anything, this is likely to bias me against finding a positive effect of DACA on yearly outcomes. The fact that I identify improvements in achievement despite possible compositional changes, provides compelling evidence that effort among undocumented youth likely improved in response to DACA.

Heterogeneous Responses – I next stratify the sample by gender, country of origin, and baseline ELA achievement (as of 8th grade). Tables 5 focuses on the impacts of DACA on educational attainment across these subgroups. The effects on educational attainment are driven by men, larger

---

$$Y_{istgz} = \beta_0 + \beta_1(\text{ShareEligible}_z \times \text{Post}_t) + \lambda_1 Z_i + \lambda_2 Z_{sc} + \phi_{sg} + \alpha_{tg} + \gamma_z + \varepsilon_{stgz} \tag{4}$$

where Y_{istgz} is a yearly outcome from grade g in which the student was enrolled during year t. Now I interact the fixed concentration of DACA applicants in a student's zip-code of residence with a post-policy indicator, Post_t, which equals 1 if the outcome was measured after DACA's enactment in 2012. ϕ_{sg} and α_{tg} are school-grade and year-grade fixed effects, and all other control variables measured at baseline (i.e. 9th grade) are as previously defined.

[35]Foreign-born students living in the zip-code with the maximum share of DACA applicants (25 percent), were 2.6 p.p. more likely to be suspended, experience an improvement in GPA of 0.13 points (off of a mean of 2.26) and experience a 0.14 standard deviation increase in ELA performance.

for those of Mexican origin, and larger for those in the bottom half of the baseline achievement distribution. These are precisely the groups who are typically at risk of dropping out of high school and would have been more likely to respond to DACA's educational incentives.

Table 6 focuses on heterogeneity for yearly outcomes. I estimate similar increases in achievement across gender and country of origin. By baseline achievement, I find that the increases in ELA performance were larger for the top half of the achievement distribution at baseline.[36] On the margin of high school GPA, however, I find that the effects are driven by students who were lower achieving at baseline. The increases in the likelihood of ever being disciplined are entirely driven by those who were lower achieving at baseline.[37] Again, to interpret the impacts of DACA on achievement, especially for those at the bottom half of the baseline achievement distribution, it is important to consider that this group was induced to stay enrolled in school due to DACA.

The heterogeneous responses by baseline achievement provide suggestive evidence that DACA impacted two different groups of undocumented students: lower-achieving students on the margin of high school completion and higher achieving students on the margin of college enrollment. For low-achievers, DACA led to significant increases in high school completion. As outlined in Section 2.1, these are precisely the students who were likely incentivized to complete high school in order to receive the benefits of DACA. They also increased effort, as measured by ELA performance and GPA. These increases in effort were either driven by necessity (i.e. in order to be able to graduate they had to work harder), or because DACA led to increases in their motivation. For high-achievers, DACA did not impact high school completion (as they likely would have graduated regardless of DACA), but it did lead to significant increases in achievement. These higher-achieving students were likely incentivized to work harder during high school in order to be eligible for the

[36] I do not focus on math achievement as a main outcome. Beginning in 8th grade students can choose the sequence of math courses they take. Therefore, in a given grade students may be taking a different version of the math exam. With this caveat, Appendix Table A.2 shows that conditional on the type of math exam a student took, math achievement improved for high-achieving students. For the other subgroups, the coefficients are positive (suggesting a possible improvement), but these effects are not statistically significant.

[37] This suggests that the overall increases in discipline among undocumented youth is likely to be driven by compositional changes in unobservables.

new merit-based financial aid opportunities in California that were tied to high school performance, or in order to gain access to more competitive colleges or degree programs.

4.3 Evidence for the Main Identification Assumption

This analysis rests on the assumption that educational outcomes would have evolved similarly for foreign-born Hispanic students in zip-codes with different fractions of DACA applicants. In order to provide evidence in support of this assumption, I next examine the relationship between the likelihood of being undocumented (ShareEligible$_z$) and educational attainment among Hispanic foreign-born students for each 9th grade cohort separately using an event-study specification. Figure 2 plots event-study estimates where the outcome is a summary index of educational attainment.[38]

For the Hispanic and Mexican foreign-born samples, I estimate a small downward pre-policy trend in educational attainment for likely undocumented youth relative to likely foreign-born citizens in Panels A and B of Figure 2. This trend is in the opposite direction of the effects I estimate post-policy, so if anything this is likely to to bias me against finding a positive impact of DACA on educational attainment. Importantly, this pre-policy trend does not exist for lower-achieving students who were most impacted by DACA's high school graduation incentives (Panel C, Figure 2). Consistent with the identification assumption, cohorts expected to graduate before DACA's enactment there was little differences in educational attainment across low-achieving foreign-born students with different likelihoods of being undocumented. However, cohorts exposed to DACA during high school, those with higher likelihoods of being undocumented had significantly higher educational attainment. For high-achieving students (Panel D, Figure 2), there is little relationship between the likelihood of being undocumented and attainment across cohorts.

Similarly, I estimate the relationship between the likelihood of being undocumented (ShareEligible$_z$) and yearly outcomes in each calendar year separately. Figure 3 plots event-study

[38]Appendix Figures A.3 and A.4 plot event-study estimates where the outcome is an indicator for 12th grade enrollment and high school completion respectively. These results demonstrate similar patterns to the event-study results using the summary measure of educational attainment.

estimates where the outcome is a summary index of achievement.[39] This plot demonstrates similar patterns across all subgroups. Before DACA's enactment in 2012, there was little difference in achievement across foreign-born students with different likelihood of being undocumented. However, after 2012 likely undocumented students experienced significant improvements in achievement.

I also show that observables do not predict a differential improvement in outcomes for likely undocumented youth after DACA's enactment in Appendix Table A.3. For the full sample shown in Panel A, Columns 2-7 demonstrate that there were similar trends in observables among foreign-born Hispanics living in zip codes with different fractions of DACA applicants. In addition, I use all covariates (excluding treatment) to generate predicted high school completion based on students during the pre-policy period. Column 1 shows that conditional on cohort, campus, and zip-code fixed effects, foreign-born Hispanic students living in high DACA zip-codes were not predicted to be more likely to graduate high school after DACA's enactment. Panel B shows similar estimates, but limits the sample to those foreign-born Hispanic students who were enrolled throughout all four years of high school. Similar to the overall sample, I do not find strong evidence of observable changes in the composition of likely undocumented youth who were enrolled throughout high school.[40] Taken together, it is unlikely that improvements in the underlying ability of Hispanic foreign-born students living in zips with high fractions of DACA applicants are driving these results.

5 Spillover Effects

5.1 Empirical Strategy

Next, I leverage the introduction of DACA to determine whether the increased returns to schooling experienced by undocumented youth affected their US-born peers' outcomes. Specifically, I focus on the 2012 introduction of DACA, wherein the control group consists of US-born students without

[39] Appendix Figure A.5 plots event-study estimates where the outcome is an indicator for ELA performance and cumulative GPA respectively. These results present similar patterns to the results using the summary measure.

[40] The one exception is that I find those enrolled throughout high school in Panel B are significantly more likely to be male. I also find in Panel B that point estimates suggest a decline in baseline ELA achievement and the predicted likelihood of graduating high school, although these effects are not statistically significant. As previously noted, if anything, this is likely to bias me against finding a positive effect of DACA on yearly outcomes.

DACA-eligible peers, and the treatment effect varies across US-born students in the fraction of their peers who were DACA-eligible. My difference-in-difference estimating equation takes the form:

$$Y_{isc} = \alpha_0 + \alpha_1(\text{DACAShare}_{sc} \times \text{Exposure}_c) + \lambda_1 X_{isc} + \lambda_2 Z_{sc} + \gamma_s + \phi_c + \varepsilon_{isc} \tag{5}$$

where Y_{isc} is an indicator for high school completion for US-born student i in 9th grade cohort c in high school s. DACAShare_{sc} is the fraction of likely DACA-eligible peers as defined in Equation 2, and is interacted with an indicator for whether a student attended high school after DACA's enactment. I control for high school campus γ_s fixed effects to account for fixed cross-sectional differences across high school campuses, and cohort controls ϕ_c to account for trends in high school completion that could affect all students in Los Angeles. Z_i includes individual characteristics that include race, gender, gender-race interactions, special education status, and 8th grade ELA test scores.[41] Finally, Z_{sc} accounts for school by cohort demographics that include the fraction of students who are male, by racial group (Hispanic, White, and Black), and receiving special education, all measured as of 9th grade. The coefficient of interest, α_1, represents the peer effects stemming from the share of one's peers estimated to be DACA-eligible.

The main identification assumption is that educational outcomes would have evolved similarly for US-born students in schools with different fractions of likely undocumented peers. Again, I trace out the impacts for each cohort separately by replacing Exposure_c with 9th grade cohort indicators as a test of this parallel trends assumption. These event-study results are presented in Section 5.3 and provide evidence in favor of this parallel trends assumption.

5.2 Results

I begin by documenting whether exposure to undocumented peers led to changes in educational attainment for US-born students after DACA's enactment. Difference-in-differences estimates are

[41] Again, I do not control for ELL status as of 9th grade given the downward trend in ELL participation over this period. I also do not control for an FRL indicator. Reassuringly, results are robust to including these control variables.

presented in Table 7. I find that US-born students with more undocumented peers were significantly more likely to enroll in grades 11-12 and complete high school after DACA's enactment.[42] Starting with a model that only includes 9th grade cohort indicators and high school campus fixed effects, I successively add controls. My estimated effects are largely stable to choice of specification. These results suggest that US-born students at the average campus, where approximately 1 percent of students were likely to be undocumented, experienced a 2 p.p. (or 3 percent) increase in the likelihood of being enrolled in 12th grade and a 2 p.p. (or 4 percent) increase in the likelihood of high school completion. Results using a summary index also indicate an increase in educational attainment.

Intermediate Outcomes – Next, I examine whether exposure to higher concentrations of undocumented peers led to increases in achievement for US-born students after DACA's enactment. To do so, I estimate a slightly modified version of Equation 5 to account for yearly outcomes.[43] Difference-in-differences estimates from this specification are presented in Table 8, where the outcomes include yearly attendance rates, an indicator for whether a student was suspended, ELA achievement and cumulative GPA. Starting with a model that only includes campus-grade and year-grade fixed effects, I successively add controls. The results are largely stable to the choice of specification. I find no impact on attendance rates or the likelihood of being disciplined. However, I do find positive policy spillovers on achievement. In the fully specified model, I find that US-born students with the average number of likely undocumented peers (1 percent) experienced a 0.05 point increase in their GPA (off of a mean of 2.33) and a a 0.06 standard deviation increase in ELA achievement after DACA's enactment. In addition, results using a summary index of academic

[42] I do not estimate a significant relationship for 10th grade enrollment. Again, as students are required to be enrolled in school until they are 16 (which will occur for most students during 11th grade), a non-significant relationship for 10th grade enrollment is consistent with students waiting to drop-out until they are legally able to do so.

[43] Specifically, I estimate the following difference-in-difference specification:

$$Y_{isctg} = \gamma_0 + \gamma_1(\text{DACAShare}_{sc} \times \text{Post}_t) + \lambda_1 Z_i + \lambda_2 Z_{sc} + \phi_{sg} + \alpha_{tg} + \varepsilon_{iscgt} \qquad (6)$$

where Y_{isctg} is a yearly outcome from grade g in which the student was enrolled during year t. Now I interact the fixed concentration of likely-DACA eligible peers in a student's 9th grade cohort-campus with a post-policy indicator, Post_t, which equals 1 if the outcome was measured after DACA's enactment in 2012. ϕ_{sg} and α_{tg} are school-grade and year-grade fixed effects, and all other control variables measured at baseline (i.e. 9th grade) are as previously defined.

achievement also indicate an improvement in achievement.

Again, one caveat for these findings focusing on US-born students enrolled throughout high school is that DACA may have introduced compositional changes. Indeed, as will be further discussed in Section 5.3, DACA induced lower-achieving US-born students to continue throughout high school. However, this sort of compositional change if anything, should bias me against finding a positive spillover effect of DACA on yearly outcomes. The fact that I identify improvements in achievement despite this change, provides compelling evidence that US-born students' effort likely improved in response to their likely DACA-eligible peers being more invested in their education.

Heterogeneous Responses – I next stratify the sample by gender, race, and baseline achievement. Table 9 focuses on educational attainment among US-born students across these different groups. The spillover effects of DACA on high school enrollment are driven by Black, Hispanic, males, and lower-achieving students. In terms of high school completion, the positive spillover effects are driven by Black and lower-achieving students.

Table 10 focuses on heterogeneity for the yearly outcomes. US-born Hispanics experienced the largest increases in ELA performance and GPA. In terms of baseline achievement, the increases in ELA performance are largest for those in the top of the achievement distribution, while the increases in GPA are largest for those in the bottom of the achievement distribution.[44] Across gender, I estimate similar increases in ELA achievement and GPA. Again, one caveat for these findings is that DACA induced US-born students to stay enrolled in school, which will lead to compositional changes. As previously noted, if anything, this should bias me against finding positive spillovers on the yearly achievement of US-born students.

These heterogeneous results provide evidence consistent with spillover effects being driven by peer-to-peer interactions. In terms of educational attainment, the direct and spillover

[44] As previously noted, after 8th grade students can choose what version of the math exam they take during each grade so I do not focus on math scores as a main outcome. However, I do find evidence that conditional on the math exam they took, math scores among US-born students improved as a consequence of having more DACA-eligible peers in Appendix Table A.4. The increases are driven by the same subgroups that drove the increases in ELA achievement.

effects of DACA were driven by males and low-achievers. As low-achieving male students are more likely to interact with one another, this is precisely the group of US-born students expected to have experienced the largest spillovers in terms of attainment. For ELA achievement, the direct and spillover effects were largest for those who were higher achieving at baseline. As high-achieving students are more likely to interact with one another, this is precisely the group of US-born students expected to have experienced the largest positive spillovers in terms of ELA achievement.

To further investigate whether peer-to-peer interactions are driving these effects, I turn to estimating whether the spillover effects are larger for closer contacts. Specifically, I focus on whether there is a differential spillover effect for students coming from the same middle school. Students who come from the same middle school are likely to have closer ties and longer-lasting friendships. If peer-to-peer interactions are driving my results closer contacts should generate larger spillovers. However, if the spillovers are being driven by changes in the classroom or school-wide dynamics, closer contacts should not have a differential effect. The results in Appendix Table A.5 indicate that it is likely undocumented students from the same middle school that are driving the positive spillover effects on educational outcomes.[45] This provides additional evidence consistent with spillovers being primarily driven by peer -to-peer interactions.[46]

5.3 Evidence for the Main Identification Assumption

To rule out the possibility that these results are driven by pre-trends, I next examine the relationship between educational outcomes and the share of undocumented peers (DACAShare_{sc}) for each cohort separately. Figure 4 plots event-study estimates where the outcomes is a summary index of educational attainment.[47] Panel A presents estimates for all US-born students, while Panel B (C)

[45] For the average US-born student, 35% of their undocumented peers attended the same middle school as they did.

[46] In terms of investigating teacher turnover, I am limited by the fact that I can only track teacher turnover between 2013 and 2017 (which is only during the post-policy period). Nonetheless, Appendix Figure A.6 presents event-study estimates where the outcome is the fraction of teachers who left a high school campus in a given year. This plot shows no differential trend in teacher turnover in high school campuses with different concentrations of undocumented students. This provides further suggestive evidence inconsistent with school-wide dynamics driving the spillover effects.

[47] Appendix Figures A.7 and A.8 plot event-study estimates where the outcome is an indicator for 12th grade enrollment and high school completion, respectively. These results demonstrate similar patterns to the results using the summary measure.

presents estimates for those in the bottom (top) quartile of the baseline achievement distribution.

For the full US-born sample, I estimate a small positive (but insignificant) pre-DACA trend in educational attainment. Importantly, this small trend is driven by high-achievers who were less impacted by DACA's incentives to graduate. For low-achievers who were more impacted by DACA's graduation incentives (as they are more likely on the margin of dropping out) there are no pre-trends. The plot in Panel B of Figure 4 shows that for low-achieving 9th grade cohorts expected to graduate before DACA's enactment there was little difference in educational attainment between US-born students with different concentrations of undocumented peers. In contrast, those with higher concentration of undocumented peers in cohorts expected to graduate after 2012 were significantly more likely to continue until 12th grade and complete high school. While these patterns do not hold for high-achieving students, they were already likely to graduate from high school.

Similarly, I estimate event-study specifications for the yearly outcomes which plot the relationship between the estimated fraction of undocumented peers (DACAShare_{sc}) and outcomes of US-born students in each year separately. Figure 5 plots event-study estimates where the outcome is a summary index of academic achievement.[48] Before DACA's enactment in 2012, there was little difference in achievement between US-born students with more vs. fewer undocumented peers. After DACA's enactment in 2012, students with higher concentrations of undocumented peers experienced significant improvements in ELA achievement. While there does appear to be a positive trend in achievement between 2005 and 2008 for those in the top quartile, it largely appears to level of three years before DACA's introduction.

I also investigate whether observables predict a differential improvement in outcomes for US-born youth with higher concentrations of undocumented peers after DACA. To do so, I regress exogenous characteristics as well as a measure that predicts high school graduation on

[48] Appendix Figures A.9 and A.10 plot event-study estimates where the outcome is an indicator for ELA performance and cumulative GPA respectively. The results that focus on each outcome separately present similar patterns to the results that focus on the summary measure.

the DACA peer exposure variable, while conditioning on school and cohort fixed effects.[49] In Appendix Table A.6, I start with the full sample that was used to estimate educational attainment outcomes (Panel A) and then turn to the sample enrolled throughout high school used to estimate yearly achievement outcomes (Panel B). Reassuringly, for the full sample in Panel A there were no overall trends in baseline achievement and demographics.[50] Moreover, students with higher concentrations of undocumented peers were not predicted to have higher graduation rates. Thus, it is unlikely that compositional changes are driving the increases in educational attainment. Turning to the sample used to analyze yearly outcomes, Panel B shows that there were declines in the likelihood of graduating from high school and declines in baseline performance among students enrolled throughout all four years of high school. If anything, the declines in predicted graduation and baseline performance suggest that the estimates on yearly outcomes may be biased downwards.

6 Robustness

The measure I use to approximate undocumented status is likely measured with minimal measurement error due to the high take-up of DACA in Los Angeles. Nonetheless, one may worry that using share of DACA applicants in a zip may still introduce measurement error. To alleviate this concern, Appendix Table A.8 demonstrates that the direct impacts of DACA are largely robust to using several different measures to approximate undocumented status that do not select on the DACA application decision. Column 1 reports my baseline model that approximates the likelihood of being undocumented by using the fraction of foreign-born youth ages 15-30 in one's residence zip who applied to DACA using Equation 1. In order to get closer to the high-school aged DACA applicant population, Column 2 approximates the likelihood of being undocumented by using the fraction of foreign-born youth ages 15-19 in one's residence zip who applied to DACA using a slightly modified version of Equation 1.[51] Turning to measures that do not select on the DACA application decision, Column 3

[49]For the predicted high school completion measure, I use all covariates to generate predicted high school completion based on students during the pre-policy period.

[50]The one exception is a small positive trend in the likelihood of being Black. The increase corresponds to a 1 percentage point increase in the likelihood of being Black.

[51]As previously noted, for each zip code, I take the total number of DACA applicants and multiply by 0.40, since 40% of DACA applicants in Los Angeles county were ages 15-19 according to official USCIS estimates (USCIS, 2014),

uses the fraction of foreign-born youth ages 0-18 who were estimated to be undocumented in one's residence PUMA.[52] Finally, Column 4 uses the fraction of foreign-born non-citizens ages 0-18 in one's residence zip-code.[53] In general, the main results all suggest improvements in educational attainment and achievement among likely undocumented youth regardless of which scaling measure is used. While the impacts on ELA achievement are always significant, the impacts on high school enrollment and completion are sometimes insignificant (but always positive).

Similarly, Appendix Table A.9 shows that the spillover effects of DACA on US-born students are robust to using different measures to approximate the fraction of undocumented peers. Column 1 reports my baseline estimates that scale the fraction of Hispanic foreign-born peers (who arrived by age 9) by the zip-code DACA-application rate from Equation 1. Column 2 scales the fraction of foreign-born peers by the high-school aged DACA applicants as just detailed. Turning to the measures that do not select on the DACA application decision, Column 3 scales the fraction of foreign-born peers by the fraction of undocumented youth estimated to be living in a PUMA and Column 4 by the fraction of non-citizens in a zip-code. Finally, Column 5 simply uses the fraction of foreign-born peers to define peer exposure. Reassuringly, I come to similar conclusions regardless for how I proxy for likely undocumented status (Columns 2-4). In addition, the insignificant estimates in Column 5 provide compelling evidence that my estimates are not picking up peer effects stemming from having more foreign-born peers after 2012. The fact that the estimates in this table are only significant after proxying for the likelihood that these foreign-born peers are undocumented, suggest that I am instead able to capture the peer effects stemming from having more undocumented peers after DACA's enactment.

Next, I show that any campus-level population differences by the share of undocumented

and then divide by the number of foreign-born ages 15-19 in a zip-code using 5-year ACS estimates from 2014.

[52]This is calculated by MPI who estimate the undocumented population using ACS data by making a number of statistical adjustments to account for the fact that the undocumented population may be undercounted in the ACS (see https://www.migrationpolicy.org/about/mpi-methodology-assigning-legal-status-noncitizens-census-data for more detail). One downside of this measure is that PUMAs are larger areas than zip-codes.

[53]As previously noted, using foreign-born non-citizens is the most common way to approximate the undocumented population in the literature on DACA (e.g. Pope (2016a); Kuka et al. (2020); Amuedo-Dorantes and Antman (2017)). This measure over-counts the undocumented population in a zip-code, as non-citizens include green card holders and temporary visa holders.

peers are unlikely to be driving my results.[54] To do so, I re-estimate my models including time trends interacted with campus demographics at baseline (in the 2011-12 school year). Appendix Table A.10 demonstrates that my peer effect results on attainment and achievement are robust to the inclusion of time trends interacted with the baseline fraction of FRL students, ELL students, ELA achievement (measured in 8th grade), and total cohort size. In terms of ELA Achievement (Panel C) the results are also robust to the inclusion of time trends interacted with the baseline fraction of students belonging to each racial grouping (Hispanic, Black, White, and Asian). In terms of educational attainment (Panels A-B), the results are no longer significant with the inclusion of time trends interacted with the fraction of a campus belonging to each racial group. However, the point estimates are positive and of similar magnitude to the baseline estimates (shown in Column 1), suggesting a similar conclusion. Taken together, these results help to rule out the possibility that differential trends driven by demographic differences are driving my results.

Next, I consider other immigration and education policies impacting LAUSD students during this time period. In terms of immigration policies, the only major policy change that I am aware of is the introduction of the California Dream Act in 2012. As previously noted, this policy allowed undocumented students to participate in state-funded financial aid programs. This in part can likely explain why I document increases in achievement among those likely undocumented students who were higher achieving at baseline, and who had been likely to graduate high school regardless of the introduction DACA. Overall, this policy change (introduced in the same year as DACA) is similar to DACA in that it may make investing in higher education more attractive.

In terms of education policies, in the summer of 2013, LAUSD introduced policies that significantly reduced overall suspensions.[55] Beginning in 2015, students were no longer required to

[54]As previously noted, students in campuses with high concentrations of likely DACA-eligible students are more likely to be Hispanic, ELL, receiving FRL, and have lower standardized performance at baseline.

[55]Specifically, schools were encouraged to use restorative justice methods as an alternate to suspensions. Moreover, suspensions for willful defiance were banned. Willful defiance is a subjective category defined as defying teachers and other school staff, or disrupting school activity. Before the ban in 2013, they accounted for 54 percent of all suspension across the state (Pope & Zuo, 2020). These changes to discipline policy led to declines in suspensions from 9.8 percent to 1.4 percent between the 2007 and 2014 9th grade cohorts in my sample.

pass the high school exit exam in order to graduate.[56] And the introduction of online credit-recovery courses around this time has anecdotally been linked to increases in graduation rates.[57] If schools with higher concentrations of DACA-eligible students were also more likely to be impacted by these changes in discipline or graduation requirements, then I could be misattributing the increases in educational outcomes to DACA. Reassuringly, Appendix Table A.11 shows that the concentration of DACA-eligible students is uncorrelated with baseline discipline and graduation rates. Thus, it is unlikely that any policies impacting high schools with low graduation or high discipline rates would have had a stronger effect in campuses with higher fractions of likely DACA-eligible youth.[58]

To more formally rule out the possibility that alternate educational policies are driving my results, I estimate Equation 5 including campus-level time trends that vary by the fraction of students who were unable to pass the high school exit exam on their first attempt in 2012, who were suspended in 2012, and who graduated high school during the pre-policy period. Appendix Table A.12 presents spillover results that include these campus-level trends on the summary index of educational attainment (Panel A) and the summary index of academic achievement (Panel B). These results demonstrate that my results are robust to the inclusion of such trends. This suggests that even after controlling for the possibility that campuses more impacted by these other educational policies were trending differently, I still find a positive and significant relationship between the concentration of DACA-eligible peers and the educational outcomes of US-born students.

To further rule out the possibility that my spillover results are driven by changes in graduation requirements or discipline, I turn to exploring heterogeneity by the likelihood of graduating

[56]While this policy did reduce graduation requirements, the exit exam was generally not a barrier for high school graduation, as the majority (over 70 percent) of LAUSD students were able to pass on their first attempt.

[57]Credit-recovery programs (that enable students to take classes online that they failed in the classroom) have been shown to increase high school graduation rates, but decrease college-going. Therefore, whether online credit recovery programs improve student learning remains unclear (Heinrich & Darling-Aduana, 2020).

[58]While the concentration of DACA-eligible youth is slightly negatively correlated with the fraction of students able to pass the high school exit exam on their first attempt, as previously noted it is unlikely that the elimination of the high school exit exam led to meaningful changes in the rigor of high school graduation requirements. This claim is supported by the fact that despite initial differences in exit exam passing, there was eventually little difference in high school graduation rates for campuses with different concentration of DACA-eligible youth. In addition, the concentration of DACA-eligible youth is positively correlated with the fraction of ELL students. I investigate ELL policy changes in more detail later in this section. Overall, I do not find evidence that ELL policies are impacting my results.

from high school and baseline discipline. Specifically, I use all covariates to predict the likelihood a student graduated from high school. Columns 1-3 of Appendix Table A.7 shows estimates for the summary index of educational attainment (Panel A) and the summary index of academic achievement (Panel B) across terciles of the predicted likelihood of high school graduation. As expected, the increases in educational attainment are driven by US-born students who were least likely to graduate. However, all US-born students experienced improvements in achievement. Because decreasing graduation requirements alone should not led to improvements in achievement, I view it as unlikely that changes to graduation requirements alone can explain the increases in educational investments that I document. Columns 4-5 of Appendix Table A.7 test whether there were differences across baseline discipline. These results indicate that there was little difference in educational attainment across baseline discipline, but that increases in achievement were driven by those who were not disciplined at baseline. Prior research finds that changes in discipline policy benefit the short-run outcomes of those at risk of being suspended, but negatively affect their peers who are unlikely to be suspended (Pope & Zuo, 2020). The patterns I document (i.e. larger positive impacts for those unlikely to be disciplined) are not consistent with a reduction in suspensions driving my results.[59]

Finally, as previously noted, there was a decrease in the fraction of ELL students over this time period. The fraction of 9th grade US-born students participating in ELL decreased from 19 to 7.6 percent between 2007 and 2014. This decline was likely driven by a 2006 policy change that removed a math requirement for ELL re-classification (Betts et al., 2020), and also by a strategic plan outlined by the district to reclassify more ELL students. Prior work has found that being re-classified during high school has no impact on academic performance (Pope, 2016b), and descriptive studies also find that older students' performance is not affected by changes in the rigor of ELL re-classification criteria (Kim & Herman, 2014). Thus, it is relatively unlikely that changes to ELL reclassification alone would have had a large impact on high school students' educational

[59] It is also important to note that my peer effects results are unchanged if I control for baseline discipline (i.e. an indicator for whether a student was disciplined or the number of days they were disciplined in 8th grade) and the predicted likelihood of being disciplined in high school (which is constructed by using all covariates and baseline discipline to predict the likelihood of being disciplined in high school). These results are available upon request.

outcomes. Nonetheless, as shown in Appendix Table A.11 high schools with higher concentrations of likely undocumented youth had higher fractions of ELL students and would have been more impacted by any changes in ELL re-classification practices.

To rule out the possibility that my spillover results are being driven by the reduction in ELL participation, I estimate Equation 5 including campus-level time trends that vary by the fraction of 9th grade students receiving ELL services in 2007. Column 7 of Appendix Table A.12 demonstrate that the estimates are robust to the inclusion of this trend. In addition, Columns 6-7 of Appendix Table A.7 demonstrate that the positive spillover effects of DACA are larger for students who were not enrolled in ELL programs at baseline. If it were the case that my results are driven by changes in ELL re-classification policies, then students in ELL programs should be most affected. I find larger effects for non-ELL students, which is not consistent with ELL policy changes driving my results. Finally, controlling for ELL status as of 8th grade has no impact on my estimates.

7 Conclusion

In this paper, I present evidence on how DACA affects educational attainment. My identification strategy is based on the enactment of DACA in 2012, which increased the returns to a high school diploma for undocumented youth but left the returns for US-born students unchanged. First, I examine whether DACA led to increases in high school enrollment, completion, and effort among likely undocumented youth in Los Angeles. Then, I estimate whether the increases in peer motivation of undocumented youth due to DACA had any impact on their peers' educational investments. To estimate whether DACA had positive spillovers on US-born students, I leverage variation in the concentration of DACA-eligible youth across Los Angeles schools and compare the educational outcomes of US-born students in high schools with higher concentrations of DACA-eligible peers to those in high schools with lower concentrations before and after DACA's enactment.

My results indicate that DACA increased educational attainment among undocumented students and their in-eligible US-born peers. I find that among likely undocumented youth DACA increased 12th grade enrollment by 6 percent, high school graduation by 10 percent, ELA achievement by 0.14 standard deviations, and GPA by 0.127 percentage points (off of a mean of 2.26). Among US-born students at the average campus, where approximately 1 percent of students were likely to be undocumented, I also find that DACA increased 12th grade enrollment by 3 percent, high school graduation by 4 percent and ELA achievement by 0.06 standard deviations. These results are robust to a number of specification checks, including compositional changes and differences in trends across the types of campuses that have more or fewer concentrations of undocumented students.

This paper makes a novel contribution to the peer effects literature by isolating a plausibly exogenous increase in peer motivation due to DACA. My context is unique within this literature, as I am able to focus on a plausibly exogenous increase in peer motivation (driven by policy) among existing peer groups (where established friendships are expected). Moreover, I focus on a critical time during adolescence both when conforming to social norms may be especially important and also a time right before critical human capital decisions are typically made.

In addition to the contributions to the peer effects literature, these results have important policy implications for the DACA program itself. Previous studies on DACA have focused exclusively on the direct impacts DACA had on undocumented youth, but these studies have ignored the possibility of spillovers on US-born students. To my knowledge, my study is the first to account for the educational spillovers of DACA on US-born high school students. As the program continues to be contested politically, fully accounting for the costs and benefits of this program are crucial for current and future policy debates.

While this paper shows robust evidence on the positive direct and spillover effects DACA had on educational investments during high school, I am unable to assess whether the policy led to increases in college enrollment or improved labor market outcomes. Given that high school completion and achievement are strong predictors of adult success, it is likely that these longer-run outcomes were also likely to improve as a consequence of DACA.

References

Abramitzky, R., & Lavy, V. (2014, July). How Responsive Is Investment in Schooling to Changes in Redistributive Policies and in Returns? *Econometrica*, *82*(4), 1241-1272.

Abramitzky, R., Lavy, V., & Perez, S. (2021). The Long-Term Spillover Effects of Changes in the Return to Schooling. *Journal of Public Economics (forthcoming)*.

Amuedo-Dorantes, C., & Antman, F. (2017). Schooling and labor market effects of temporary authorization: evidence from daca. *Journal of Population Economics*, *30*(1), 339-373.

Batalova, J., Hooker, S., & Capps, R. (2014, August). DACA at the Two Year Mark: A National and State Profile of Youth Eligible and Applying for Deferred Action).

Betts, J., Hill, L., Bachofer, K., Hayes, J., Lee, A., & Zau, A. (2020). Effects of English Learner Reclassification Policies on Academic Trajectories. *NBER Working Paper, No. 28188*.

Bietenbeck, J. (2020). Own Motivation, Peer Motivation, and Educational Success. *IZA DP No. 13872*.

Borjas, G. J. (2017). The Earnings of Undocumented Immigrants. *NBER Working Paper, No. 23236*.

Chishti, M., Bolter, J., & Pierce, S. (2017). Will White House Immigration Wish List Tank Emerging DREAMer Momentum in Congress? *Migration Policy Institute*.

Golsteyn, B. H., Non, A., & Zölitz, U. (2020). The impact of peer personality on academic achievement. *Journal of Political Economy (forthcoming)*.

Heinrich, C. J., & Darling-Aduana, J. (2020). Does Online Course-taking Increase High School Completion and Open Pathways to Postsecondary Education Opportunities? *EdWorkingPaper: 20-47*.

Hsin, A., & Ortega, F. (2018, August). The Effects of Deferred Action for Childhood Arrivals on the Educational Outcomes of Undocumented Students. *Demography*, *55*(4), 1487-1506.

Kaushal, N. (2006). Amnesty Programs and the Labor Market Outcomes of Undocumented

Workers. *Journal of Human Resources*.

Kim, J., & Herman, J. L. (2014). Understanding Patterns and Precursors of ELL Success Subsequent to Reclassification. *CRESST Report 818. National Center for Research on Evaluation, Standards, and Student Testing*.

Kling, J. R., Liebman, J. B., & Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, *75*(1), 83-119.

Kossoudji, S. A., & Cobb-Clark, D. A. (2002, July). Coming out of the Shadows: Learning about Legal Status and Wages from the Legalized Population. *Journal of Labor Economics*, *20*(3), 598-628.

Kuka, E., Shenhav, N., & Shih, K. (2020, February). Do human capital decisions respond to the returns to education? evidence from daca. *American Economic Journal: Economic Policy*, *12*(1), 293-324.

McHugh, M. (2014, September). Diploma, Please: Promoting Educational Attainment for DACA- and Potential DREAM Act-Eligible Youth).

Nicholls, W. J. (2013). The DREAMers: How the Undocumented Youth Movement Transformed the Immigrant Rights Debate. *Stanford University Press*.

Parlapiano, A., & Yourish, K. (2018). A Typical Dreamer Lives in Los Angeles, Is From Mexico and Came to the U.S. at 6 Years Old. *The New York Times*.

Patler, C. (2018, May). To Reveal or Conceal: How Diverse Undocumented Youth Navigate Legal Status Disclosure. Sociological Perspectives. *Sociological Perspectives*, *61*(4), 857-873.

Pope, N. (2016a). The effects of dacamentation: The impact of deferred action for childhood arrivals on unauthorized immigrants. *Journal of Public Economics*, *143*(C), 98-114.

Pope, N. (2016b). The Marginal Effect of K-12 English Language Development Programs: Evidence for Los Angeles Schools. *Economics of Education Review*.

Pope, N., & Zuo, G. (2020). Suspending suspensions: The education production consequences of school suspension policies. *Working Paper*.

Rivera-Batiz, F. (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.

Ruiz Soto, A. G., & Capps, R. (2017). Trump Administration Makes Down Payment on Campaign Pledges to Address Illegal Immigration. *Migration Policy Institute*.

Sacerdote, B. (2011, June). Peer Effects in Education: How Might They Work, How Big Are They and How Much Do We Know Thus Far? In E. Hanushek, S. Machin, & L. Woessmann (Eds.), *Handbook of the Economics of Education* (Vol. 3, p. 249-277). Elsevier.

Singer, A., Svajilenka, N. P., & Wilson, J. H. (2015). Local Insights From DACA for Implementing Future Programs for Unauthorized Immigrants.

Stewart, R., Silverio-Bautista, K., Moran-Perez, G., & Parsley, M. (2019). Students unite in support of DACA as the Supreme Court decides its fate. *Daily Sundial*.

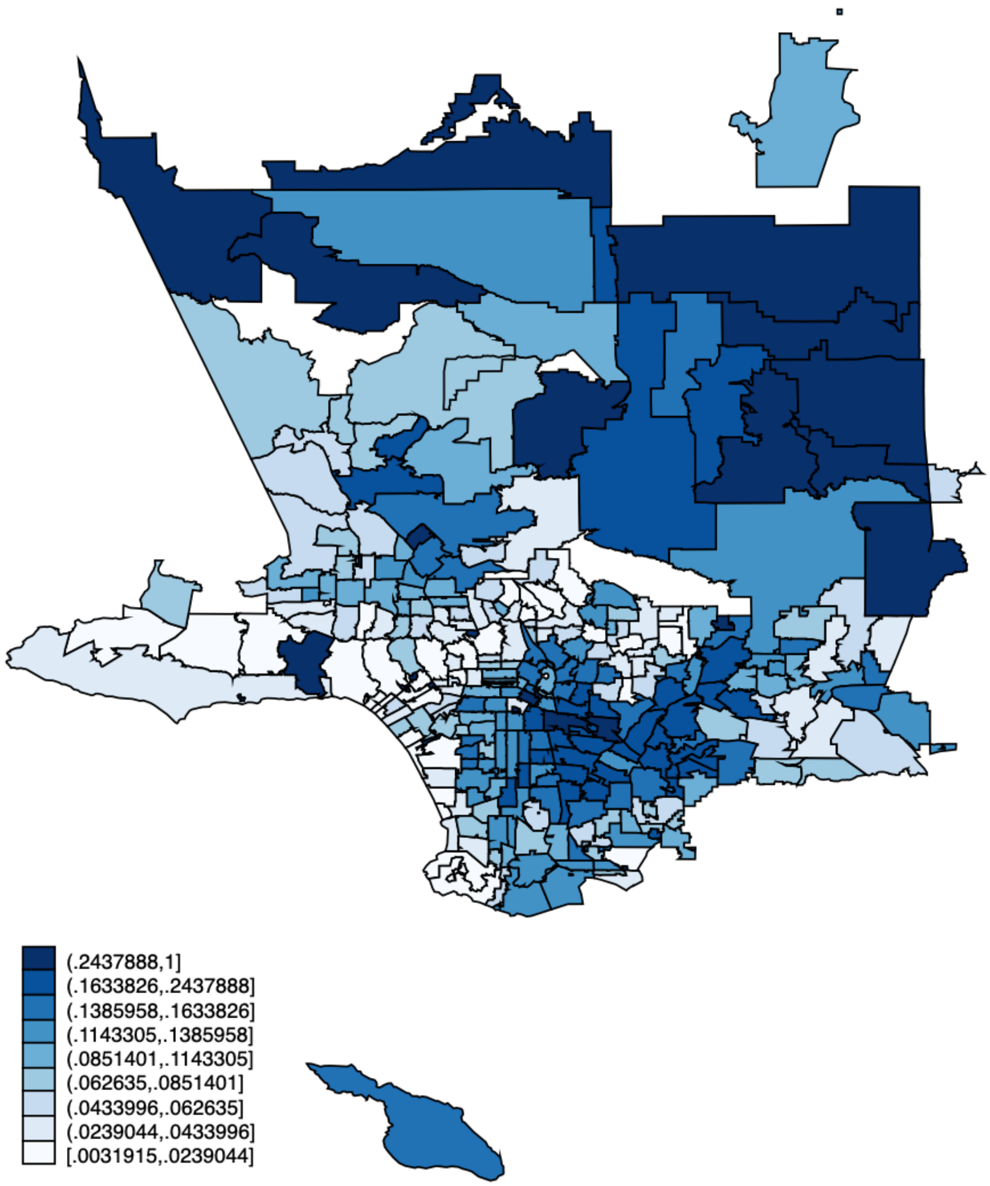
Svajlenka, N. P., & Singer, A. (2013). Immigration Facts: Deferred Action for Childhood Arrivals (DACA).

Totenberg, N. (2020). Supreme Court Rules for DREAMers, Against Trump. *NPR*.

USCIS. (2014). Characteristics of individuals requesting and approved for deferred action for childhood arrivals (daca). Retrieved from `https://www.uscis.gov/sites/default/files/USCIS/Humanitarian/Deferred\%20Action\%20for\%20Childhood\%20Arrivals/USCIS-DACA-Characteristics-Data-2014-7-10.pdf`

Figures/Tables

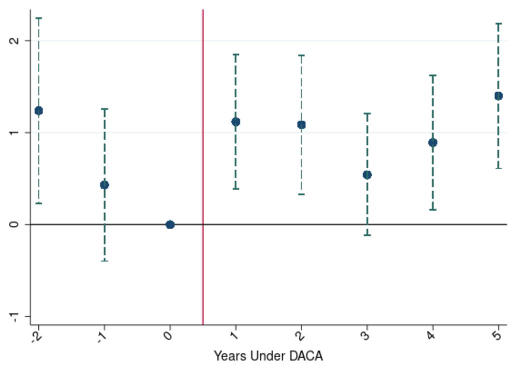
Figure 1: Fraction of Foreign-Born Population Ages who applied to DACA, 2012-2013



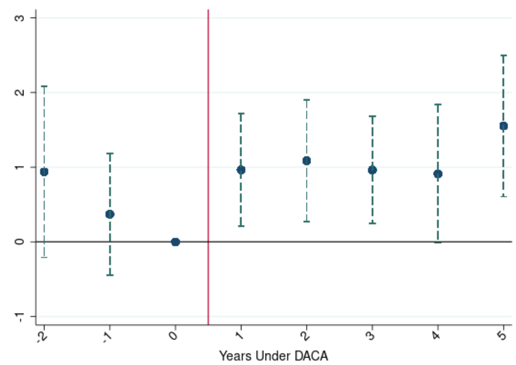
Note: This plot shows the share of foreign-born individuals who applied to DACA in each Los Angeles zip code using USCIS data. This is computed using Equation 1. For each zip-code, I take the total number of DACA applicants between July 2012-December 2013 and then, I divide by the number of foreign-born who lived in the zip-code who were ages 15-29 using data from the 5-year ACS estimates from 2014.

Figure 2: Event Study Estimates of the Direct Impact of DACA on Summary Index of Educational Attainment, Foreign-born Hispanics

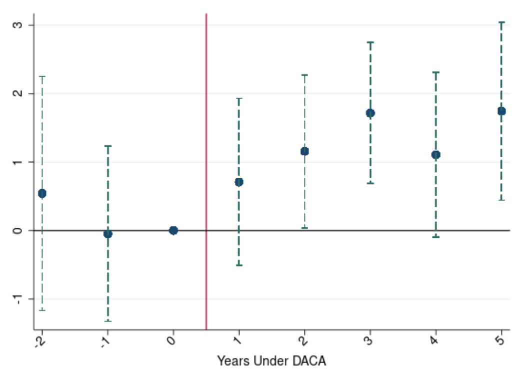
(a) All



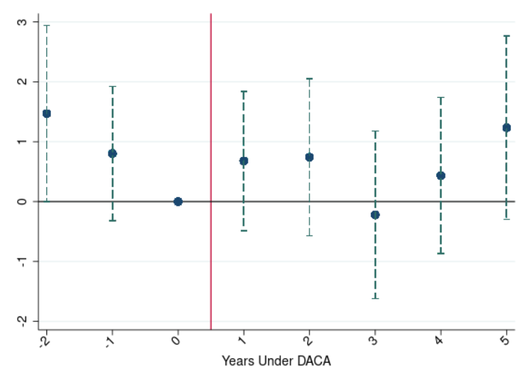
(b) Mexican



(c) Mexican - Bottom 50th Baseline Achievement Percentile



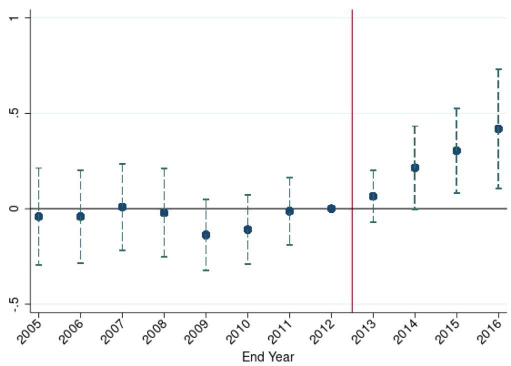
(d) Mexican - Top 50th Baseline Achievement Percentile



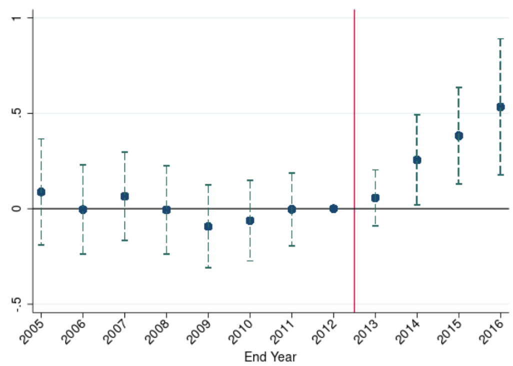
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between 9th grade cohort dummies and ShareEligible$_z$ (computed using Equation 1). The dependent variable is a summary index based on enrollment in grades 10-12 and high school completion. Event time is computed by subtracting 12 from the grade each 9th grade cohort was expected to be enrolled in during the year right before the policy was implemented (or the 2011-12 school year). The sample includes foreign-born Hispanic students who arrived to the US by age 9 in 9th grade cohorts between 2006-07 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 9th grade cohort from 2008-09 is omitted, so estimates are relative to that unexposed cohort. See Table 3 for more detail on the sample and the full set of controls. Standard errors are clustered by zip-code.

Figure 3: Event Study Estimates of the Direct Impact of DACA on Summary Index of Academic Performance, Foreign-born Hispanics

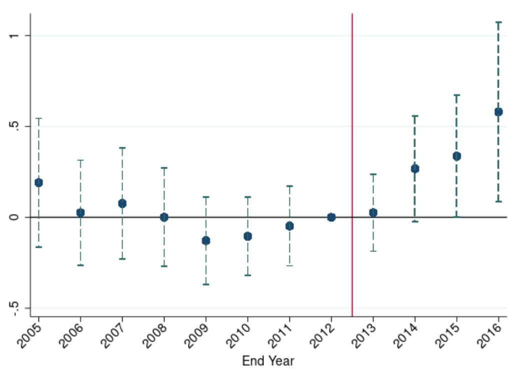
(a) All



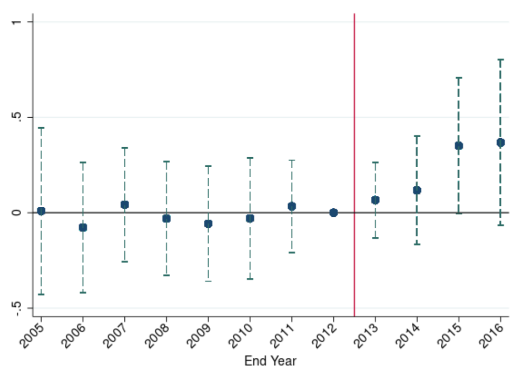
(b) Mexican



(c) Mexican - Bottom 50th Baseline Achievement Percentile



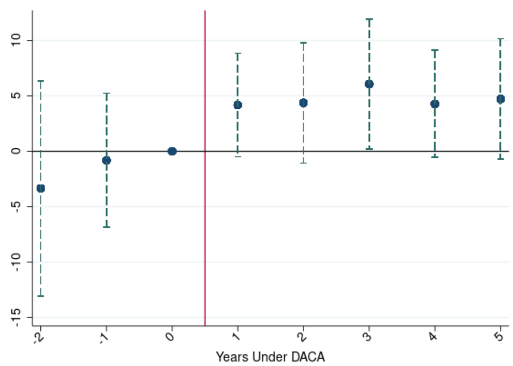
(d) Mexican - Top Baseline Achievement 50th Percentile



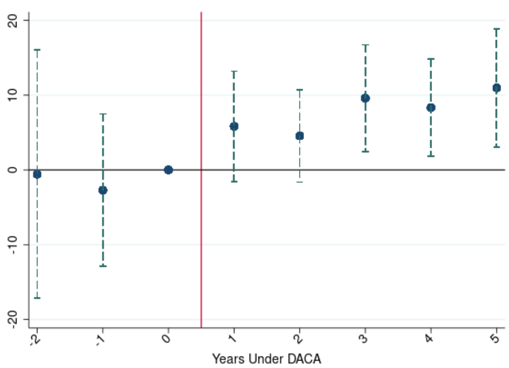
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and ShareEligible$_z$ (computed using Equation 1). The dependent variable is a summary index based on GPA and performance on the ELA standardized exam. The sample includes foreign-born Hispanic students who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table 3 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors are clustered by zip-code.

Figure 4: Event Study Estimates of the Direct Impact of DACA on Summary Index of Educational Attainment, US-born Students

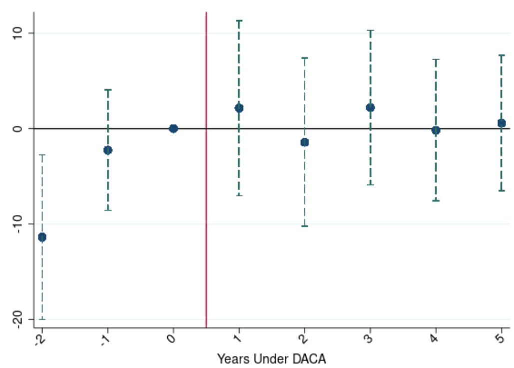
(a) All



(b) Bottom Baseline Achievement Quartile



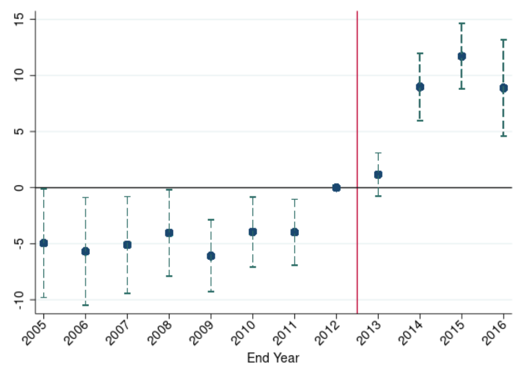
(c) Top Baseline Achievement Quartile



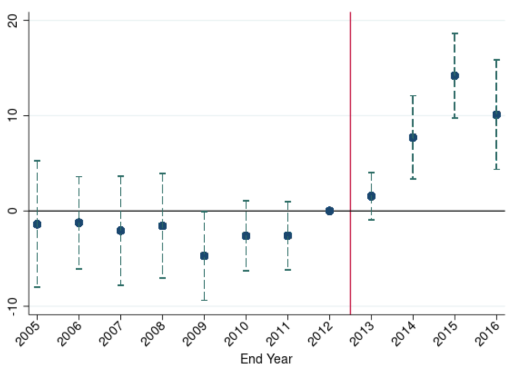
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between 9th grade cohort dummies and DACAShare_{sc} (computed using Equation 2). The dependent variable is a summary index based on enrollment in grades 10-12 and high school completion. Event time is computed by subtracting 12 from the grade each 9th grade cohort was expected to be enrolled in during the year right before the policy was implemented (or the 2011-12 school year). The sample includes US-born students in 9th grade cohorts between 2006-07 to 2013-14. The sub-sample is shown in sub-figure labels. Baseline achievement quartiles are computed based on 8th grade ELA achievement. The 9th grade cohort from 2008-09 is omitted, so estimates are relative to that unexposed cohort. See Table 7 for more detail on the sample and the full set of controls. Standard errors are clustered at the high school campus level.

Figure 5: Event Study Estimates of the Direct Impact of DACA on Summary Index of Academic Performance, US-born Students

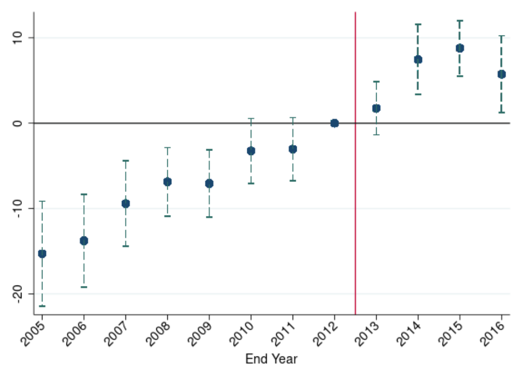
(a) All



(b) Bottom Baseline Achievement Quartile



(c) Top Baseline Achievement Quartile



Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and DACAShare$_{sc}$ (computed using Equation 2). The dependent variable is a summary index based on GPA and performance on the ELA standardized exam. The sample includes US-born students in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in sub-figure labels. Baseline achievement quartiles are computed based on 8th grade ELA achievement. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table 7 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors are clustered at the high school campus level.

Table 1: Summary Statistics - 9th Grade Cohorts Between 2007 - 2014

			Foreign-Born		US Arrival Before Age 9	
	Full	US-Born	All	Hispanic	Hispanic	Mexican
	(1)	(2)	(3)	(4)	(5)	(6)
DACA Applications By Zip						
ShareEligible_z - Ages 15-19	0.323	0.324	0.316	0.337	0.341	0.348
ShareEligible_z - Ages 15-31	0.131	0.131	0.127	0.138	0.139	0.143
Demographics (G9)						
Male	0.511	0.510	0.516	0.514	0.507	0.506
Black	0.090	0.103	0.014	0	0	0
Hispanic	0.780	0.781	0.773	1	1	1
White	0.063	0.064	0.055	0	0	0
Special Education	0.081	0.087	0.048	0.055	0.072	0.076
English Language Learner	0.184	0.156	0.338	0.386	0.272	0.283
Free-Lunch	0.654	0.655	0.648	0.668	0.678	0.676
Foreign-Born	0.150	0	1	1	1	1
Born in Mexico	0.086	0	0.571	0.738	0.816	1
Age US Arrival	-	-	7.834	7.583	5.880	5.767
Baseline Achievement						
Std ELA Score (G8)	-0.069	-0.046	-0.199	-0.378	-0.217	-0.252
Std ELA Score (G7)	-0.032	-0.008	-0.177	-0.359	-0.193	-0.228
Std Math Score (G7)	0.047	0.049	0.034	-0.187	-0.079	-0.108
Outcomes						
Graduated High School	0.572	0.576	0.552	0.514	0.564	0.556
Enrolled Expected G10	0.906	0.907	0.898	0.903	0.921	0.922
Enrolled Expected G11	0.845	0.848	0.831	0.832	0.860	0.859
Enrolled Expected G12	0.768	0.771	0.748	0.741	0.776	0.775
Std ELA Score (G11)	0.061	0.072	0.003	-0.168	-0.075	-0.096
Observations	281,046	238,781	42,265	32,381	21,139	17,247

Note: This table presents summary statistics for students in 9th grade cohorts between 2007 and 2014 enrolled in Los Angeles Unified school district. The first column includes the full sample, the second column includes those students born in the US, and the third column includes those students who were not born in the US. Columns 4-6 include foreign-born students separated by ethnicity and age of arrival to the US. Column 4 includes Hispanic foreign-born students, Column 5 includes Hispanic foreign-born students who arrived to the US before they were 9 years old, and Column 6 includes Mexican foreign-born students who arrived to the US before they were 9 years old. High school graduation is measured on-time, and is an indicator equal to one if a student graduated from high school within four years of 9th grade.

Table 2: Characteristics of Schools by the Concentration of Undocumented Peers

	(1)	(2)	(3)	(4)
		DACA Concentration - Percentile		
	Full	Bottom 25	25-75	Top 25
Share DACA-Eligible Peers				
DACAShare - Ages 15-19	0.023	0.010	0.022	0.037
DACAShare - Ages 15-31	0.009	0.004	0.009	0.016
Baseline Demographics (G9)				
Male	0.511	0.511	0.510	0.511
Black	0.090	0.194	0.072	0.022
Hispanic	0.780	0.548	0.805	0.960
White	0.063	0.135	0.055	0.006
Asian	0.040	0.073	0.041	0.005
Special Education	0.081	0.089	0.084	0.068
Free-Lunch	0.654	0.572	0.666	0.712
English Language Learner	0.184	0.114	0.195	0.230
Foreign-Born	0.150	0.135	0.158	0.151
Foreign-Born - Mexican	0.086	0.047	0.089	0.118
Baseline Achievement				
Std ELA Score (G8)	-0.069	0.151	-0.068	-0.292
Std ELA Score (G7)	-0.032	0.198	-0.036	-0.250
Std Math Score (G7)	0.047	0.208	0.058	-0.133
Outcomes				
Graduated High School	0.572	0.582	0.569	0.569
Enrolled Expected G10	0.906	0.897	0.906	0.915
Enrolled Expected G11	0.845	0.836	0.847	0.851
Enrolled Expected G12	0.768	0.766	0.769	0.767
Std ELA Score (G11)	0.061	0.205	0.039	-0.048
Number of Campuses	155	29	70	56
Average Cohort Size	524	624	558	391
Observations	281,046	68,923	153,493	58,630

Note: This table presents summary statistics for all students in 9th grade cohorts between 2007-2014 enrolled in Los Angeles Unified school district. The first column includes the full sample, Columns 2-5 separate students based on the fraction of one's peers estimated to be DACA-eligible using Equation 2. High school graduation is measured on-time, and is an indicator equal to one if a student graduated from high school within four years of 9th grade.

Table 3: The Effect of DACA on High School Attendance and Completion, Foreign-born Hispanics

	(1)	(2)	(3)	(4)
Panel A: Enrolled in Expected 10th Grade				
ShareEligible*Exposed	0.0693	0.0685	0.0664	0.0538
	(0.0838)	(0.0856)	(0.0855)	(0.0891)
	[0.017]	[0.017]	[0.017]	[0.013]
Mean (Y)	0.921	0.921	0.921	0.921
Panel B: Enrolled in Expected 11th Grade				
ShareEligible*Exposed	0.144*	0.144*	0.138*	0.161*
	(0.0825)	(0.0817)	(0.0811)	(0.0883)
	[0.036]	[0.036]	[0.035]	[0.040]
Mean (Y)	0.860	0.860	0.860	0.860
Panel C: Enrolled in Expected 12th Grade				
ShareEligible*Exposed	0.190*	0.196**	0.174*	0.179*
	(0.0970)	(0.0956)	(0.0957)	(0.0969)
	[0.048]	[0.049]	[0.044]	[0.045]
Mean (Y)	0.776	0.776	0.776	0.776
Panel D: Graduated from High School				
ShareEligible*Exposed	0.276**	0.286**	0.233**	0.248**
	(0.108)	(0.112)	(0.113)	(0.113)
	[0.069]	[0.072]	[.058]	[0.062]
Mean (Y)	0.564	0.564	0.564	0.564
Panel E: Summary Index				
ShareEligible*Exposed	0.529***	0.544***	0.481***	0.501***
	(0.181)	(0.178)	(0.174)	(0.178)
	[0.132]	[0.136]	[.120]	[0.125]
N	21,139	21,139	21,139	21,139
Controls				
Cohort & Zip FE	X	X	X	X
Campus FE	X	X	X	X
Demographics		X	X	X
8th Grade Std Test (ELA)			X	X
Campus-Cohort Demographics				X

Note: This table shows difference-in-differences estimates of the direct impact of DACA on high school enrollment and on-time graduation. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 3. The sample for these regressions are foreign-born Hispanic students who were in 9th grade cohorts from 2006-07 to 2013-14 who arrived to the US by age 9. Individual demographic controls include age of arrival to the US, country of origin indicators, gender, and whether a student received special education services. District demographic cohort controls include the percentage of students in the cohort belonging to each racial group, receiving special education, and who are male. The effect for the fully exposed student living in a zip-code where 25 percent of the foreign-born population applied to DACA is shown in brackets, and is defined as the coefficient multiplied by 0.25. Standard errors in parentheses are clustered at the zip-code level. *p<0.10, ** p<0.05, *** p<0.01.

Table 4: The Effect of DACA on Yearly Outcomes, Foreign-born Hispanics

	(1)	(2)	(3)	(4)
Panel A: Yearly Attendance Rate (Grades 9-12)				
ShareEligible*Post	-0.0155	-0.0162	-0.0174	-0.0135
	(0.0263)	(0.0260)	(0.0247)	(0.0248)
	[0.004]	[0.004]	[0.004]	[0.003]
Mean (Y)	0.936	0.936	0.936	0.936
Panel B: Ever Disciplined (Grades 9-12)				
ShareEligible*Post	0.106***	0.0992***	0.101***	0.104***
	(0.0340)	(0.0340)	(0.0345)	(0.0355)
	[0.027]	[0.025]	[0.025]	[0.026]
Mean (Y)	0.0334	0.0334	0.0334	0.0334
Panel C: Cumulative GPA (Grades 9-12)				
ShareEligible*Post	0.425	0.516*	0.459*	0.508**
	(0.283)	(0.286)	(0.249)	(0.242)
	[0.106]	[0.129]	[0.115]	[0.127]
Mean (Y)	2.262	2.262	2.262	2.262
Panel D: Standardized ELA Exam Performance (Grades 9-11)				
ShareEligible*Post	0.512*	0.537*	0.534**	0.553**
	(0.305)	(0.312)	(0.235)	(0.237)
	[0.128]	[0.134]	[0.133]	[0.138]
Mean (Y)	-0.0922	-0.0922	-0.0922	-0.0922
Panel E: Summary Achievement Index (Grades 9-11)				
ShareEligible*Post	0.820**	0.902***	0.794***	0.836***
	(0.321)	(0.338)	(0.264)	(0.261)
	[0.205]	[0.226]	[0.199]	[0.209]
Controls				
Zip FE	X	X	X	X
Grade-Year FE	X	X	X	X
Campus-Grade FE	X	X	X	X
Demographics		X	X	X
8th Grade Std Test (ELA)			X	X
Campus-Cohort Demographics				X

Note: This table shows difference-in-differences estimates of the direct impact of DACA on yearly attendance rates (Obs= 71,811), indicators for ever being disciplined (i.e. in or out of school suspensions only) (Obs=75,155), cumulative GPA (Obs=72,308), and standardized ELA test performance (Obs=43,153), as well as a summary index based on the outcomes in Panels C-D (N=56,910). Within each panel, each column reports estimates of β_1 from a separate regression of Equation 4. The sample for these regressions are foreign-born Hispanic students who were in 9th grade cohorts from 2006-07 to 2013-14 who arrived to the US by age 9. All regressions are weighted by the inverse of the number of times a student is observed in the sample. See Table 3 for more detail on the sample and control variables. The effect for the fully exposed student living in a zip-code where 25 percent of the foreign-born population applied to DACA is shown in brackets, and is defined as the coefficient multiplied by 0.25. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

Table 5: The Heterogenous Effects of DACA on Educational Attainment, Foreign-born Hispanics

	(1)	(2)	(3)	(4)	(5)	(6)
					8th Grade ELA Score	
	Full	Mexican	Female	Male	Bottom 50	Top 50
Panel A: Enrolled in Expected 10th Grade						
ShareEligible*Exposed	0.0538	0.108	0.00354	0.0685	0.276***	-0.209
	(0.0891)	(0.0933)	(0.140)	(0.115)	(0.0886)	(0.127)
	[0.013]	[0.027]	[0.001]	[0.017]	[0.069]	[-0.052]
Mean (Y)	0.921	0.922	0.917	0.926	0.913	0.932
Panel B: Enrolled in Expected 11th Grade						
ShareEligible*Exposed	0.161*	0.265***	0.0557	0.213	0.450***	-0.139
	(0.0883)	(0.101)	(0.149)	(0.160)	(0.122)	(0.134)
	[0.040]	[0.066]	[0.014]	[0.053]	[0.113]	[-0.035]
Mean (Y)	0.860	0.859	0.856	0.863	0.836	0.891
Panel C: Enrolled in Expected 12th Grade						
ShareEligible*Exposed	0.179*	0.247**	-0.0931	0.328*	0.278*	0.0326
	(0.0969)	(0.115)	(0.137)	(0.167)	(0.157)	(0.152)
	[0.045]	[0.062]	[-0.023]	[0.082]	[0.070]	[0.008]
Mean (Y)	0.776	0.775	0.778	0.774	0.728	0.838
Panel D: Graduated from High School						
ShareEligible*Exposed	0.248**	0.286**	0.0237	0.383**	0.394***	0.0426
	(0.113)	(0.119)	(0.169)	(0.165)	(0.139)	(0.228)
	[0.062]	[0.072]	[0.006]	[0.096]	[0.099]	[0.011]
Mean (Y)	0.564	0.556	0.612	0.518	0.446	0.720
Panel E: Summary Index						
ShareEligible*Exposed	0.501***	0.676***	-0.0175	0.822**	0.874***	-0.0247
	(0.178)	(0.198)	(0.284)	(0.319)	(0.282)	(0.336)
	[0.125]	[0.169]	[-0.004]	[0.206]	[0.219]	[-0.006]
N	21,139	17,247	10,424	10,715	11,996	9,143

Note: This table shows difference-in-differences estimates of the direct impact of DACA on high school enrollment and on-time completion. Within each panel, each column reports estimates of δ_1 from a separate regression of Equation 3. The sample for these regressions are foreign-born Hispanic students who were in 9th grade cohorts from 2006-07 to 2013-14 who arrived to the US by age 9. All regressions include zip-code, 9th grade cohort, and 9th grade campus fixed effects. Regressions also include the full set of individual and cohort level controls. See Table 3 for more detail on the sample and control variables. The effect for the fully exposed student living in a zip-code where 25 percent of the foreign-born population applied to DACA is shown in brackets, and is defined as the coefficient multiplied by 0.25. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

Table 6: The Heterogenous Effects of DACA on Outcomes, Foreign-born Hispanics

	(1)	(2)	(3)	(4)	(5)	(6)
					8th Grade ELA Score	
	Full	Mexican	Female	Male	Bottom 50	Top 50
Panel A: Yearly Attendance Rate (Grades 9-12)						
ShareEligible*Post	-0.0135	-0.0228	0.0394	-0.0708*	-0.0385	0.0187
	(0.0248)	(0.0238)	(0.0319)	(0.0383)	(0.0346)	(0.0272)
	[-0.003]	[-0.006]	[0.010]	[-0.018]	[-0.010]	[0.005]
Mean (Y)	0.936	0.936	0.935	0.938	0.922	0.953
Observations	71,811	58,489	35,334	36,477	39,394	32,417
Panel B: Ever Disciplined (Grades 9-12)						
ShareEligible*Post	0.104***	0.101***	0.0727	0.137***	0.176***	0.0464
	(0.0355)	(0.0363)	(0.0482)	(0.0511)	(0.0521)	(0.0379)
	[0.026]	[0.025]	[0.018]	[0.034]	[0.044]	[0.012]
Mean (Y)	0.0334	0.0337	0.0218	0.0446	0.0423	0.0222
Observations	75,155	61,308	36,995	38,160	41,695	33,460
Panel C: Cumulative GPA (Grades 9-12)						
ShareEligible*Post	0.508**	0.589**	0.786***	0.323	0.727**	0.324
	(0.242)	(0.255)	(0.277)	(0.392)	(0.287)	(0.357)
	[0.127]	[0.147]	[0.197]	[0.081]	[0.182]	[0.081]
Mean (Y)	2.262	2.232	2.428	2.101	1.889	2.717
Observations	72308	58982	35644	36664	39728	32580
Panel D: Standardized ELA Exam Performance (Grades 9-11)						
ShareEligible*Post	0.553**	0.525**	0.615**	0.685**	0.444*	0.902***
	(0.237)	(0.256)	(0.238)	(0.286)	(0.225)	(0.326)
	[0.138]	[0.131]	[0.154]	[0.071]	[0.111]	[0.226]
Mean (Y)	-0.0922	-0.121	-0.0275	-0.156	-0.613	0.506
Observations	43,153	35,511	21,420	21,733	23,069	20,084
Panel E: Summary Achievement Index (Grades 9-11)						
ShareEligible*Post	0.836***	0.876***	1.056***	0.738*	0.924***	0.808**
	(0.261)	(0.273)	(0.263)	(0.387)	(0.298)	(0.370)
	[0.209]	[0.219]	[0.264]	[0.185]	[0.231]	[0.202]
Mean (Y)	-0.0354	-0.0647	0.103	-0.169	-0.494	0.542
Observations	56,910	46,435	27,955	28,955	31,727	25,183

Note: This table shows difference-in-differences estimates of the direct impact of DACA on yearly outcomes. Within each panel, each column reports estimates of β_1 from a separate regression of Equation 4. The sample for these regressions are foreign-born Hispanic students who were in 9th grade cohorts from 2006-07 to 2013-14 who arrived to the US by age 9. All regressions include zip-code, grade-year, and campus-grade fixed effects. Regressions also include the full set of individual and cohort level controls. See Table 3 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect for the fully exposed student living in a zip-code where 25 percent of the foreign-born population applied to DACA is shown in brackets, and is defined as the coefficient multiplied by 0.25. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

Table 7: The Effect of DACA on Enrollment and High School Graduation, US-Born Students

	(1)	(2)	(3)	(4)
Panel A: Enrolled in 10th Grade				
DACAShare*Exposed	0.957	0.979	1.005	0.762
	(0.761)	(0.755)	(0.749)	(0.737)
	[0.010]	[0.010]	[0.010]	[0.008]
Mean (Y)	0.907	0.907	0.907	0.907
Panel B: Enrolled in 11th Grade				
DACAShare*Exposed	1.757**	1.837**	1.934**	1.901**
	(0.813)	(0.814)	(0.794)	(0.818)
	[0.018]	[0.018]	[0.019]	[0.019]
Mean (Y)	0.848	0.848	0.848	0.848
Panel C: Enrolled in 12th Grade				
DACAShare*Exposed	2.486**	2.627***	2.707***	2.625***
	(0.982)	(0.989)	(0.971)	(0.928)
	[0.025]	[0.026]	[0.027]	[0.026]
Mean (Y)	0.771	0.771	0.771	0.771
Panel D: Graduated from High School				
DACAShare*Exposed	2.297*	2.427*	2.610**	2.418**
	(1.229)	(1.242)	(1.131)	(1.078)
	[0.023]	[0.024]	[0.026]	[0.024]
Mean (Y)	0.576	0.576	0.576	0.576
Panel E: Summary Index				
DACAShare*Exposed	5.608**	5.917**	6.142***	5.882***
	(2.240)	(2.260)	(2.175)	(2.065)
	[0.056]	[0.059]	[0.061]	[0.059]
N	238,781	238,781	238,781	238,781
Controls				
Cohort & Campus FE	X	X	X	X
Demographics		X	X	X
8th Grade Std Test (ELA)			X	X
Campus-Cohort Demographics				X

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on high school enrollment and graduation, as well as a summary index based on the outcomes in Panels A-D. Within each panel, each column reports estimates of α_1 from a separate regression of Equation 5. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. Individual demographic controls include gender, race, disability status and gender-race interactions. District demographic cohort controls include the percentage of students belonging to each racial group, enrolled in special education, and who are male. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table 8: The Effect of DACA on Yearly Outcomes, US-Born Students

	(1)	(2)	(3)	(4)
Panel A: Yearly Attendance Rate (Grades 9-12)				
DACAShare*Post	0.217	0.219	0.233	0.207
	(0.175)	(0.175)	(0.166)	(0.168)
	[0.002]	[0.002]	[0.002]	[0.002]
Mean (Y)	0.933	0.933	0.933	0.933
Observations	798,534	798,534	798,534	798,534
Panel B: Ever Disciplined (Grades 9-12)				
DACAShare*Post	0.329	0.313	0.304	0.264
	(0.259)	(0.252)	(0.248)	(0.253)
	[0.003]	[0.003]	[0.003]	[0.003]
Mean (Y)	0.0386	0.0386	0.0386	0.0386
Observations	841,929	841,929	841,929	841,929
Panel C: Cumulative GPA (Grades 9-12)				
DACAShare*Post	4.170***	4.258***	4.616***	4.572***
	(1.355)	(1.195)	(1.238)	(1.219)
	[0.042]	[0.043]	[0.046]	[0.046]
Mean (Y)	2.325	2.325	2.325	2.325
Observations	798,399	798,399	798,399	798,399
Panel D: Standardized ELA Performance (Grades 9-11)				
DACAShare*Post	4.977***	5.066***	6.469***	6.539***
	(1.751)	(1.557)	(1.280)	(1.302)
	[0.050]	[0.051]	[0.065]	[0.065]
Mean (Y)	0.0664	0.0664	0.0664	0.0664
Observations	490,051	490,051	490,051	490,051
Panel E: Summary Achievement Index (Grades 9-11)				
DACAShare*Post	7.903***	7.989***	8.335***	8.316***
	(1.368)	(1.202)	(1.165)	(1.134)
	[0.079]	[0.080]	[0.083]	[0.083]
Observations	631,098	631,098	631,098	631,098
Controls				
Demographics		X	X	X
8th Grade Std Test (ELA)			X	X
Campus-Cohort Demographics				X

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on yearly outcomes, as well as a summary index based on the outcomes in Panels C-D. Within each panel, each column reports estimates of γ_1 from a separate regression of Equation 6. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. See Table 7 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample, and include campus-year and campus-grade fixed effects. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. Standard errors in parentheses are clustered at the high school campus level.
*p<0.10, ** p<0.05, *** p<0.01.

Table 9: The Heterogenous Effects of DACA on School Attendance and High School Completion, US-born students

							8th Grade ELA Test Score Quartiles			
	Full	Black	Hispanic	White	Female	Male	(≤ 25)	(25 - 50)	(50 - 75)	≥ 75
Panel A: Enrolled in Expected 10th Grade										
DACAShare*Exposed	0.762	2.329	0.821	2.457	0.752	0.759	1.238	0.767	0.357	0.534
	(0.737)	(2.032)	(0.776)	(3.359)	(0.823)	(0.705)	(0.901)	(0.852)	(0.790)	(0.875)
	[0.008]	[0.023]	[0.008]	[0.025]	[0.008]	[0.008]	[0.012]	[0.008]	[0.004]	[0.005]
Mean (Y)	0.907	0.835	0.919	0.867	0.905	0.909	0.887	0.909	0.916	0.916
Panel B: Enrolled in Expected 11th Grade										
DACAShare*Exposed	1.901**	4.248**	1.577*	5.070	1.373	2.380***	2.511**	1.668**	2.280**	1.020
	(0.818)	(2.089)	(0.821)	(3.999)	(1.047)	(0.700)	(1.049)	(0.836)	(1.038)	(1.101)
	[0.019]	[0.042]	[0.016]	[0.051]	[0.014]	[0.024]	[0.025]	[0.017]	[0.023]	[0.010]
Mean (Y)	0.848	0.737	0.864	0.798	0.846	0.849	0.799	0.844	0.868	0.880
Panel C: Enrolled in Expected 12th Grade										
DACAShare*Exposed	2.625***	5.729***	2.031**	4.612	2.019**	3.202***	4.821***	2.286**	1.929*	1.778
	(0.928)	(1.812)	(0.957)	(4.648)	(1.004)	(1.035)	(1.515)	(1.030)	(1.001)	(1.126)
	[0.026]	[0.057]	[0.020]	[0.046]	[0.020]	[0.032]	[0.048]	[0.023]	[0.019]	[0.018]
Mean (Y)	0.771	0.646	0.787	0.722	0.777	0.765	0.673	0.763	0.809	0.841
Panel D: Graduated from High School										
DACAShare*Exposed	2.418**	4.636**	1.063	3.653	2.326**	2.502**	3.152**	2.623**	0.887	2.058
	(1.078)	(2.002)	(1.110)	(4.741)	(1.162)	(1.101)	(1.476)	(1.214)	(1.299)	(1.299)
	[0.024]	[0.046]	[0.011]	[0.037]	[0.023]	[0.025]	[0.032]	[0.026]	[0.009]	[0.021]
Mean (Y)	0.576	0.442	0.579	0.618	0.621	0.532	0.341	0.536	0.666	0.764
Panel E: Summary Index										
DACAShare*Exposed	5.882***	12.58***	4.060*	12.14	4.901**	6.813***	8.785***	5.665***	4.260*	4.184
	(2.065)	(3.885)	(2.058)	(10.28)	(2.294)	(2.108)	(3.026)	(1.906)	(2.444)	(2.696)
	[0.059]	[0.126]	[0.041]	[0.121]	[0.049]	[0.068]	[0.088]	[0.057]	[0.043]	[0.042]
N	238,781	24,689	186,570	15,265	117,085	121,696	60,442	58,528	61,039	58,772

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on high school enrollment and graduation, as well as a summary index based on the outcomes in Panels A-D. Within each panel, each column reports estimates of α_1 from a separate regression of Equation 5. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. All regressions include 9th grade cohort and campus fixed effects. Regressions also include the full set of individual and cohort level controls. See Table 7 for more detail on the sample and the full set of controls. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

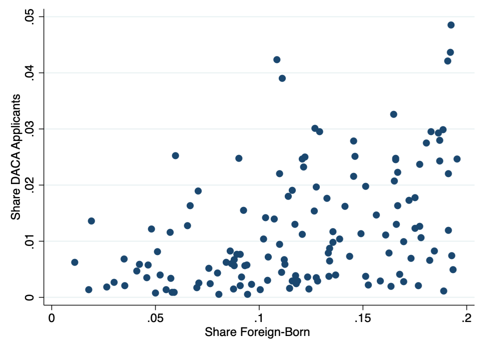
Table 10: The Heterogenous Effects of DACA on Yearly Outcomes US-born students

							8th Grade ELA Test Score Quartiles			
	Full	Black	Hispanic	White	Female	Male	($\leq$ 25)	(25 - 50)	(50 - 75)	$\geq$ 75
Panel A: Yearly Attendance Rate (Grades 9-12)										
DACAShare*Post	0.207	0.530	0.183	-0.433	0.236	0.176	0.315	0.119	0.237	0.152
	(0.168)	(0.370)	(0.181)	(0.446)	(0.182)	(0.183)	(0.224)	(0.223)	(0.210)	(0.145)
	[0.002]	[0.005]	[0.002]	[-0.004]	[0.002]	[0.002]	[0.003]	[0.001]	[0.002]	[0.002]
Mean (Y)	0.933	0.914	0.932	0.941	0.932	0.934	0.899	0.927	0.943	0.958
Observations	79,8534	72,414	634,081	48,934	392,147	406,387	187,657	194,126	209,905	206,846
Panel B: Ever Disciplined (Grades 9-12)										
DACAShare*Post	0.264	0.453	0.285	-0.422	0.255	0.259	0.530	0.754**	0.705***	0.380**
	(0.253)	(0.763)	(0.231)	(0.572)	(0.192)	(0.336)	(0.351)	(0.291)	(0.227)	(0.179)
	[0.003]	[0.005]	[0.003]	[-0.004]	[0.003]	[0.003]	[0.005]	[0.008]	[0.007]	[0.004]
Mean (Y)	0.0386	0.0934	0.0342	0.0293	0.0260	0.0509	0.0664	0.0422	0.0306	0.0172
Observations	841,929	79,443	665,972	51,922	413,215	428,714	203,040	205,793	219,330	213,766
Panel C: Cumulative GPA (Grades 9-12)										
DACAShare*Post	4.572***	2.996	4.345***	-0.205	5.022***	4.061***	6.830***	5.955***	3.038*	3.584**
	(1.219)	(2.295)	(1.328)	(4.465)	(1.418)	(1.268)	(1.465)	(1.388)	(1.727)	(1.600)
	[0.046]	[0.030]	[0.043]	[-0.002]	[0.050]	[0.041]	[0.068]	[0.060]	[0.030]	[0.036]
Mean (Y)	2.325	2.130	2.260	2.809	2.482	2.173	1.639	2.050	2.465	3.058
Observations	798,399	72,470	633,683	49,072	393138	405261	186016	194656	210514	207213
Panel D: Standardized ELA Performance (Grades 9-11)										
DACAShare*Post	6.539***	3.171	5.506***	-1.575	6.606***	6.476***	3.923***	7.333***	5.686***	7.727***
	(1.302)	(3.602)	(1.220)	(3.646)	(1.282)	(1.464)	(1.380)	(1.714)	(1.689)	(1.579)
	[0.065]	[0.032]	[0.055]	[-0.016]	[0.066]	[0.065]	[0.039]	[0.073]	[0.057]	[0.077]
Mean (Y)	0.0664	-0.138	-0.0150	0.752	0.159	-0.0242	-0.880	-0.359	0.205	1.082
Observations	49,0051	43,671	388,816	30,346	242,586	247,465	107,056	119,095	132,225	131,675
Panel E: Summary Index (Grades 9-11)										
DACAShare*Post	6.331***	3.044	6.213***	-3.019	7.318***	5.335***	5.993***	7.502***	5.148***	6.007***
	(0.890)	(1.839)	(0.958)	(3.901)	(0.964)	(1.077)	(1.201)	(1.045)	(1.358)	(1.289)
	[0.063]	[0.030]	[0.062]	[-0.030]	[0.073]	[0.053]	[0.060]	[0.075]	[0.051]	[0.060]
Observations	810,909	74,633	643,125	49,694	398,849	412,060	191,444	198,040	212,904	208,521

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on yearly outcomes, as well as a summary index based on the outcomes in Panels C-D. Within each panel, each column reports estimates of γ_1 from a separate regression of Equation 6. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. All regressions include campus-year and campus-grade fixed effects. Regressions also include the full set of individual and cohort level controls. See Table 7 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

A Appendix

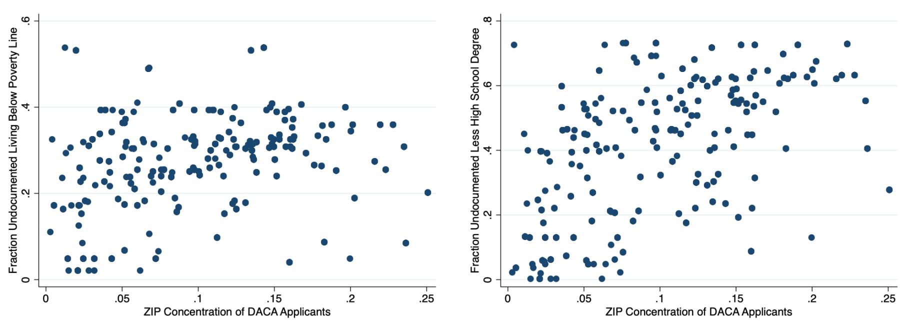
Figure A.1: Correlation Between Zip Share of Foreign-born Youth and DACA applicants



Note: Each dot of the scatter plots represents a zip code. The x-axis is the share of the population ages 15-29 who were foreign-born using using data from the 5-year ACS estimates from 2014. The y-axis is the share of the foreign-born population ages 15-29 who applied to DACA in each Los Angeles zip-code. DACA application data come from USCIS.

Figure A.2: Correlation Between Concentration of DACA Applicants and Zip Characteristics

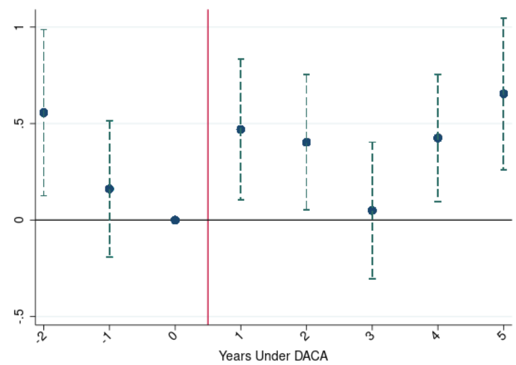
(a) Poverty Rate **(b)** Less than High School Degree



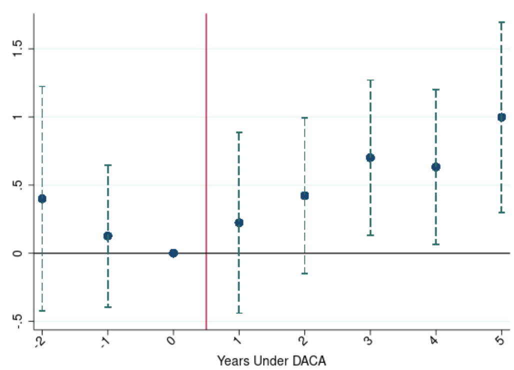
Note: Each dot of the scatter plots represents a zip-code. The x-axis is the share of foreign-born individuals who applied to DACA in each Los Angeles zip code (ShareEligible_z) computed using Equation 1. The y-axis is the share of the likely undocumented population (over 18 years old) living in a zip-code who were living below the federal poverty line (Panel A) or with less than a high school diploma. The data for the y-axis comes from a Migration Policy Institute (MPI) dataset that estimates characteristics of the underlying undocumented population at the PUMA level (which I then aggregate to the zip-code level).

Figure A.3: Event Study Estimates of the Impact of DACA on 12th Grade Enrollment, Foreign-born Hispanics

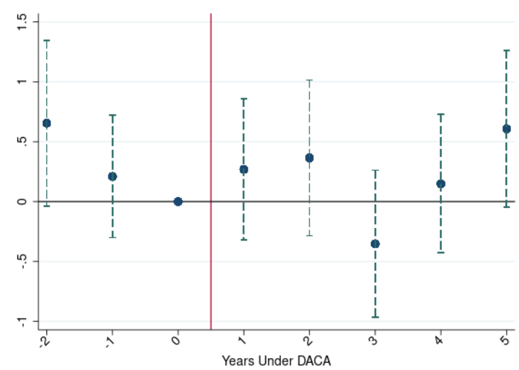
(a) All



(b) Mexican - Bottom 50th Baseline Achievement Percentile



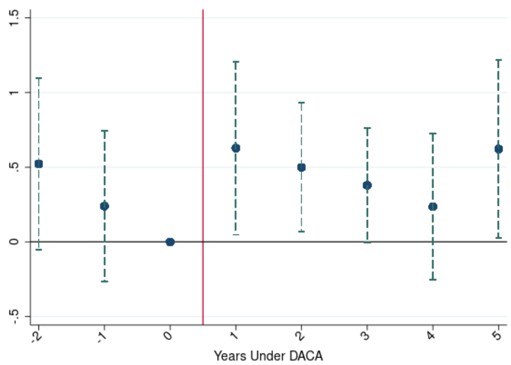
(c) Mexican -Top 50th Baseline Achievement Percentile



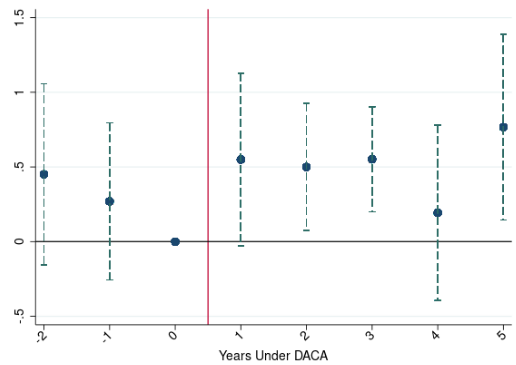
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and ShareEligible$_z$. The dependent variable is an indicator for 12th grade enrollment. The sample includes foreign-born Hispanic students who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table 3 for more detail on the sample and the full set of controls. Standard errors are clustered by zip-code.

Figure A.4: Event Study Estimates of the Impact of DACA on High School Completion, Foreign-born Hispanics

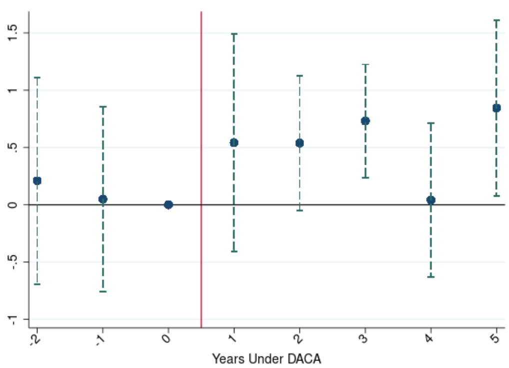
(a) All



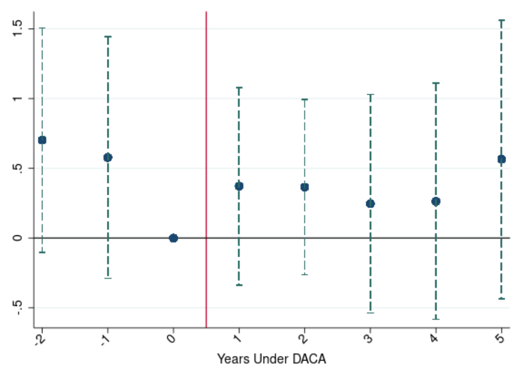
(b) Mexican



(c) Mexican - Bottom 50th Baseline Achievement Percentile



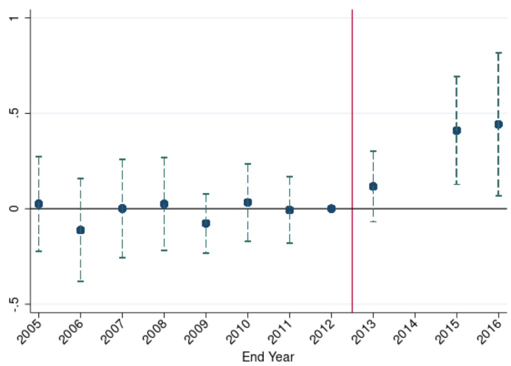
(d) Mexican - Top 50th Baseline Achievement Percentile



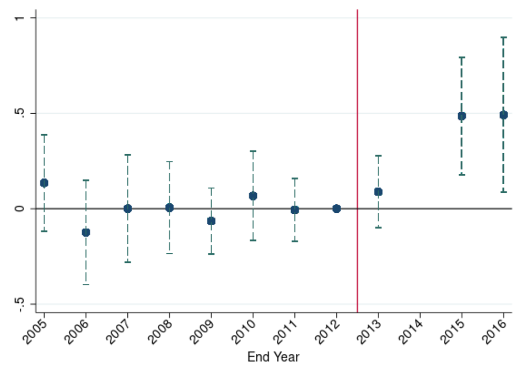
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and ShareEligible$_z$. The dependent variable is an indicator for high school completion. The sample includes foreign-born Hispanic students who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table 3 for more detail on the sample and the full set of controls. Standard errors are clustered by zip-code.

Figure A.5: Event Study Estimates of the Direct Impact of DACA on ELA Performance, Foreign-born Hispanics

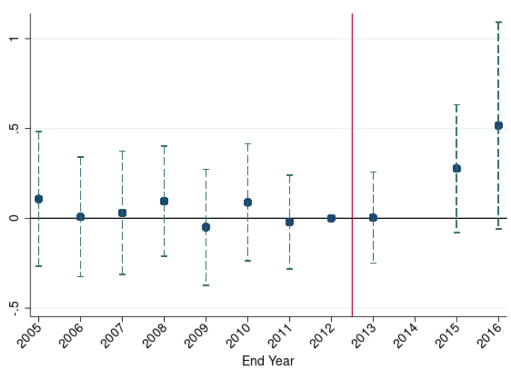
(a) All



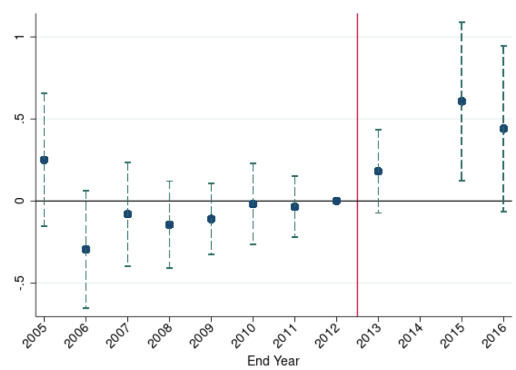
(b) Mexican



(c) Mexican - Bottom 50th Baseline Achievement Percentile

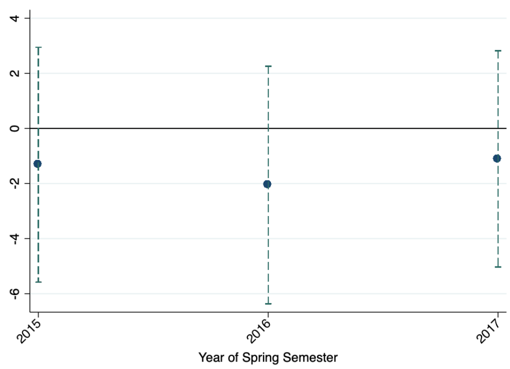


(d) Mexican - Top 50th Baseline Achievement Percentile



Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and ShareEligible_z. The dependent variable is performance on the ELA standardized exam. The sample includes foreign-born Hispanic students who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table 3 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors are clustered by zip-code.

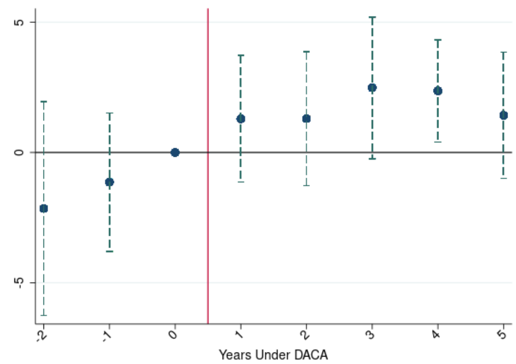
Figure A.6: Event Study Estimates of Teacher Turnover



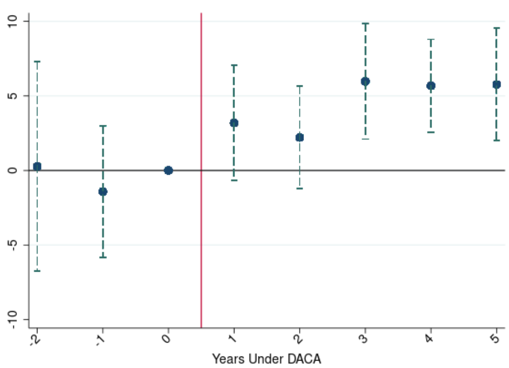
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between year dummies and DACAShare_{sc}. The dependent variable is the fraction of teachers who left a campus in a given year. The 2014 calendar year is omitted, so estimates are relative to that year. This regression controls for year and campus fixed effects. Standard errors are clustered by high school.

Figure A.7: Event Study Estimates of the Spillover Effects of DACA on 12th Grade Enrollment, US-born Students

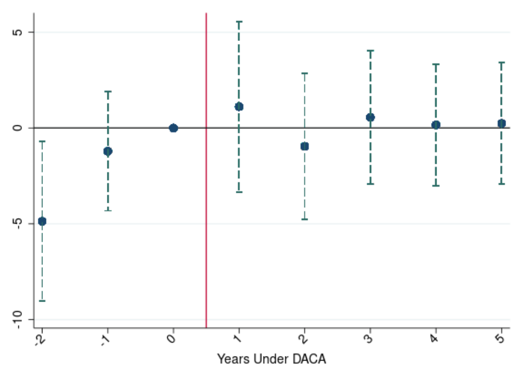
(a) All



(b) Bottom Baseline Achievement Quartile



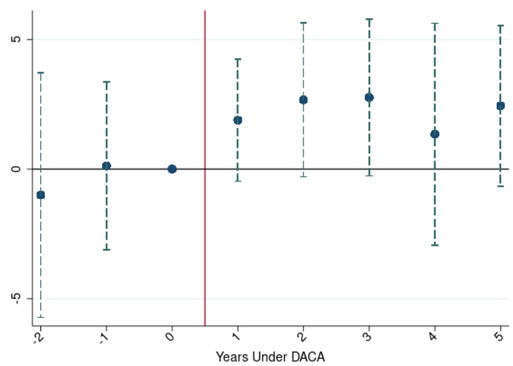
(c) Top Baseline Achievement Quartile



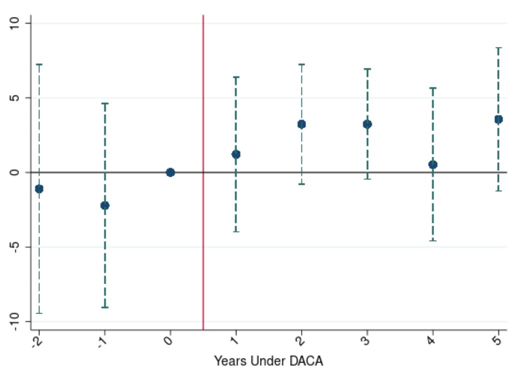
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between 9th grade cohort dummies and DACAShare$_{sc}$. The dependent variable is an indicator for whether a student was enrolled in 12th grade. The subsample is shown in the sub-figure labels. Event time is computed by subtracting 12 from the grade each 9th grade cohort was expected to be enrolled in during the year right before the policy was implemented (or the 2011-12 school year). The sample includes US-born youth in 9th grade cohorts between 2006-07 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 9th grade cohort from 2008-09 is omitted, so estimates are relative to that unexposed cohort. See Table 7 for more detail on the sample and the full set of controls. Standard errors are clustered by high school.

Figure A.8: Event Study Estimates of the Spillover Effects of DACA on High School Completion, US-born Students

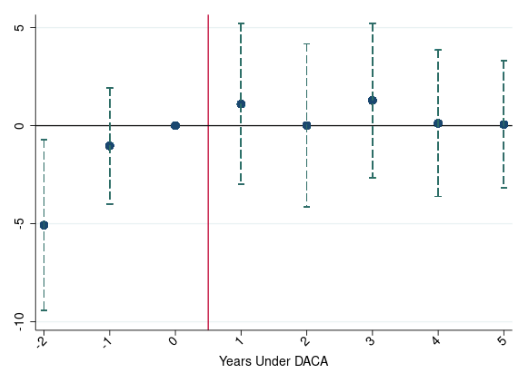
(a) All



(b) Bottom Baseline Achievement Quartile



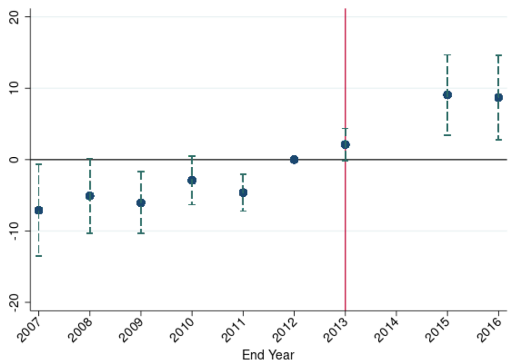
(c) Top Baseline Achievement Quartile



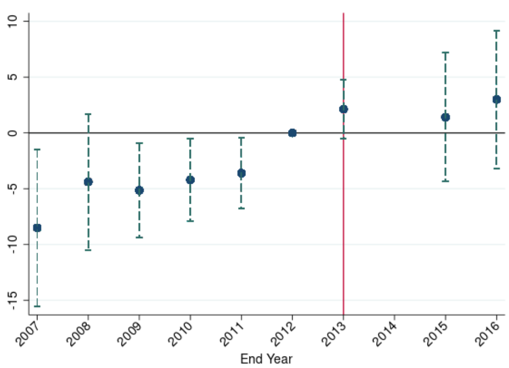
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between 9th grade cohort dummies and DACAShare$_{sc}$. The dependent variable is an indicator for whether a student completed high school. The subsample used is shown in the sub-figure labels. Event time is computed by subtracting 12 from the grade each 9th grade cohort was expected to be enrolled in during the year right before the policy was implemented (or the 2011-12 school year). The sample includes US-born youth in 9th grade cohorts between 2006-07 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 9th grade cohort from 2008-09 is omitted, so estimates are relative to that unexposed cohort. See Table 7 for more detail on the sample and the full set of controls. Standard errors are clustered by high school.

Figure A.9: Event Study Estimates of the Spillover Effects of DACA on ELA Standardized Test-Performance, US-born Students

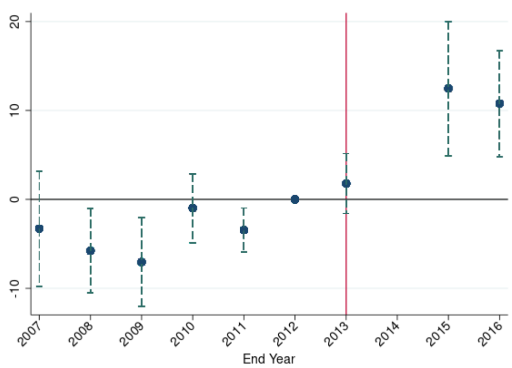
(a) All



(b) Bottom Baseline Achievement Quartile



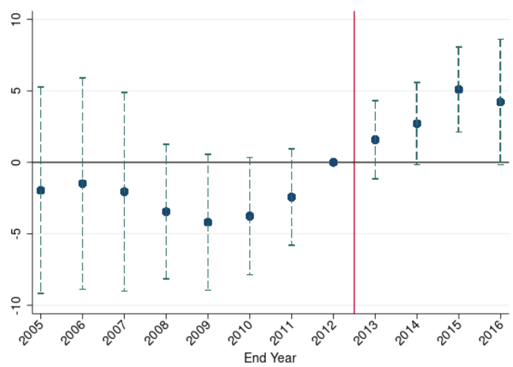
(c) Top Baseline Achievement Quartile



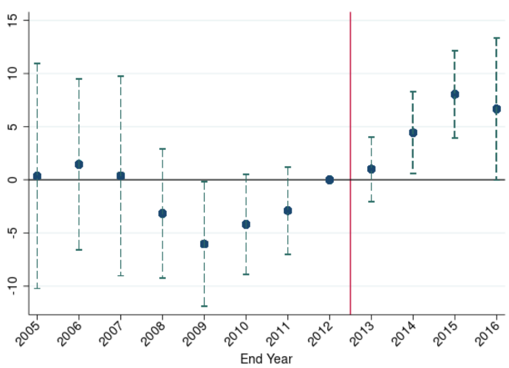
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and DACAShare$_{sc}$. The dependent variable is performance on the ELA standardized exam. The sample includes US-born students in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table 7 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors are clustered at the high school campus level.

Figure A.10: Event Study Estimates of the Impact of DACA on Semester GPA, US-born Students

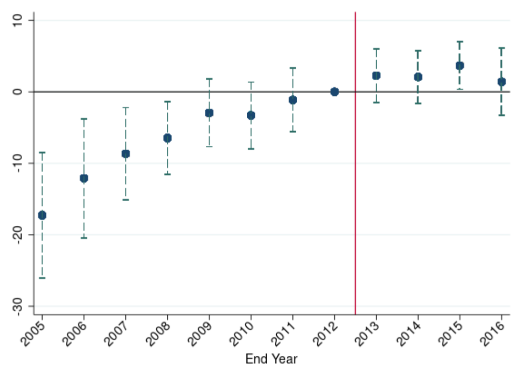
(a) All



(b) Bottom Baseline Achievement Quartile



(c) Top Baseline Achievement Quartile



Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and DACAShare$_{sc}$. The dependent variable is GPA. The sample includes US-born students in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table 7 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors are clustered at the high school campus level.

Table A.1: 9th Grade Cohorts and Share Exposed to DACA During High School

9th Grade Cohort	Policy Exposure by Year-Grade			$FracExposed_c$	Years Under DACA
	10	11	12		
2006-07	2007-08	2008-09	2009-10	0	0
2007-08	2008-09	2009-10	2010-11	0	0
2008-09	2009-10	2010-11	2011-12	0	0
2009-10	2010-11	2011-12	2012-13	0.25	1
2010-11	2011-12	2012-13	2013-14	0.50	2
2011-12	2012-13	2013-14	2014-15	0.75	3
2012-13	2013-14	2014-15	2015-16	1	4
2013-14	2014-15	2015-16	2016-17	1	5

Note: This table shows the cross-cohort variation in policy exposure by 9th grade cohort. The first school year after DACA's enactment was the 2012-2013 school year. 9th grade cohorts differed in the amount of time during high school that they were expected to be enrolled in school after DACA's enactment. For each 9th grade cohort, this table highlights each year-grade of expected exposure to DACA during high school.

Table A.2: The Heterogenous Effects of DACA on Math Test Scores, Foreign-born Hispanics

	(1)	(2)	(3)	(4)	(5)	(6)
					8th Grade ELA Score	
	Full	Mexican	Female	Male	Bottom 50	Top 50
ShareEligible*Post	0.345	0.429	0.341	0.640	0.0249	1.231***
	(0.319)	(0.335)	(0.332)	(0.409)	(0.331)	(0.450)
	[0.086]	[0.107]	[0.085]	[0.160]	[0.006]	[0.308]
Mean (Y)	-0.0472	-0.0589	-0.0669	-0.0277	-0.354	0.299
Observations	37,957	31,367	18,798	19,159	20,235	17,722

Note: This table shows difference-in-differences estimates of the direct impact of DACA on yearly math achievement. Each column reports estimates of β_1 from a separate regression of Equation 4. The sample for these regressions are foreign-born Hispanic students who were in 9th grade cohorts from 2006-07 to 2013-14 who arrived to the US by age 9. All regressions include zip-code, grade-year, and campus-grade fixed effects. Regressions also include the full set of individual and cohort level controls, as well as an indicator variable for which version of the math exam was taken. See Table 3 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect for the fully exposed student living in a zip-code where 25 percent of the foreign-born population applied to DACA is shown in brackets, and is defined as the coefficient multiplied by 0.25. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

Table A.3: The Effect of DACA on Predicted High School Completion and Exogenous Student Characteristics, Foreign-born Hispanics

	Predicted HS Grad	Male	Age at US Arrival	Special Education	Mexican	Std ELA (G8)	Std ELA (G7)	Std Math (G7)
Panel A: Full Sample								
ShareEligible*	0.0302	0.0927	0.0751	-0.0362	0.0552	0.300	0.425*	0.425
Exposed	(0.0551)	(0.165)	(0.526)	(0.0975)	(0.108)	(0.287)	(0.243)	(0.302)
	[0.008]	[0.023]	[0.019]	[-0.009]	[0.014]	[0.075]	[0.106]	[0.106]
Mean (Y)	0.564	0.507	5.880	0.0720	0.816	-0.217	-0.193	-0.0775
N	21,139	21,139	21,139	21,139	21,139	21,139	20,169	20,157
Panel B: Subset of Full Sample completing four years of high school								
ShareEligible*	-0.0397	0.395**	0.189	-0.00427	0.0710	-0.0581	0.192	0.329
Exposed	(0.0598)	(0.174)	(0.461)	(0.0785)	(0.104)	(0.289)	(0.266)	(0.329)
	[-0.010]	[0.099]	[0.047]	[-0.001]	[0.018]	[-0.015]	[0.048]	[0.082]
Mean (Y)	0.512	0.506	5.856	0.0516	0.815	-0.145	-0.129	-0.00870
N	16,375	16,383	16,383	16,383	16,383	16,383	15,741	15,734

Note: This table contains results obtained from regressing predicted high school completion and student demographics on $(\text{ShareEligible}_z * \text{Exposed}_c)$. The sample for these regressions are foreign-born Hispanic students who arrived to the US by age 9 and were in 9th grade cohorts from 2006-07 to 2013-14. Panel A focuses on the full sample, while Panel B restricts the sample to those who were enrolled in high school for four years. All regressions include zip, cohort, and high school campus fixed effects. See Table 3 for more detail on the sample. The effect for the fully exposed student living in a zip-code where 25 percent of the foreign-born population applied to DACA is shown in brackets, and is defined as the coefficient multiplied by 0.25. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

Table A.4: The Heterogenous Effects of DACA on Math Test Scores, US-born students

							Baseline Achievement	
	Full	Black	Hispanic	White	Female	Male	Bottom 50	Top 50
DACAShare*Post	7.817**	1.914	8.436**	-8.361	8.914**	6.776**	7.536***	9.841**
	(3.281)	(3.890)	(3.518)	(9.088)	(3.508)	(3.240)	(2.672)	(4.588)
	[0.078]	[0.019]	[0.084]	[-0.084]	[0.089]	[0.068]	[0.075]	[0.098]
Mean (Y)	0.0326	-0.223	-0.0249	0.585	0.0166	0.0483	-0.378	0.377
N	433827	38822	343937	26689	214496	219331	198927	234900

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on yearly math achievement. Each column reports estimates of γ_1 from a separate regression of Equation 6. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. All regressions include campus-year and campus-grade fixed effects. Regressions also include the full set of individual, cohort level controls, and an indicator variable for which version of the math exam was taken. See Table 7 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.5: The Spillover Effects of DACA by Type of Undocumented Peer, US-Born Students

	Educational Attainment		Academic Achievement	
	(1)	(2)	(3)	(4)
ShareEligible	5.882***		7.594***	
	(2.065)		(1.432)	
	[0.059]		[0.076]	
ShareEligible - Same Middle School		15.91***		14.18***
		(2.301)		(1.752)
		[0.159]		[0.142]
ShareEligible - Diff. Middle School		-2.084		2.709*
		(2.212)		(1.369)
		[-0.021]		[0.027]
N	238,781	238,781	634,546	634,546

Note: This table contains difference-in-difference estimates of the spillover effects of DACA on a summary index of educational attainment (Columns 1-2) and a summary index of academic achievement (Columns 3-4). Each column reports results from a separate regression. Columns 1 and 3 show estimates of the impact of all undocumented peers (DACAShare$_{sc}$). Columns 2 and 4 show estimates of the impact of undocumented peers from the same and different middle school. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. See Table 7 for more detail on the sample and the full set of controls. For Columns 3-4, regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p$<$0.10, ** p$<$0.05, *** p$<$0.01.

Table A.6: The Effect of DACA on Predicted High School Completion and Exogenous Student Characteristics, US-Born Students

	Predicted HS Grad	Black	Hispanic	Male	Free-Lunch	Special Education	ELA (G8)	ELA (G7)	Math (G7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full Sample									
DACAShare*	-0.505	1.160**	-0.898	-0.324	-0.005	0.101	-1.290	-0.722	2.136
Exposed	(0.338)	(0.487)	(0.785)	(0.451)	(1.218)	(0.389)	(1.869)	(1.895)	(2.147)
	[-0.005]	[0.012]	[-0.009]	[-0.003]	[0.000]	[-0.001]	[-0.013]	[-0.007]	[0.021]
Mean (Y)	0.547	0.103	0.781	0.510	0.695	0.087	-0.046	-0.008	0.049
N	238,781	238,781	238,781	238,781	238,781	238,781	238,781	224,625	224,701
Panel B: Subset of Full Sample completing four years of high school									
DACAShare*	-1.478***	-0.565	3.808***	0.613	0.952	-0.748	-8.680***	-7.955***	-6.422**
Exposed	(0.385)	(0.626)	(0.953)	(0.488)	(1.004)	(0.543)	(2.206)	(2.127)	(2.581)
	[-0.015]	[-0.006]	[0.038]	[0.006]	[0.010]	[-0.007]	[-0.087]	[-0.080]	[-0.064]
Mean (Y)	0.547	0.0867	0.797	0.506	0.702	0.177	0.0299	0.0584	0.125
N	184,170	184,170	184,170	184,170	184,170	184,170	184,170	176,071	176,167

Note: This table contains results obtained from regressing predicted high school completion and student demographics on $\text{DACAShare}_{sc} \times \text{Exposed}_c$. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. Panel A focuses on the full sample, while Banel B restricts the sample to those who were enrolled in high school for all four years. The demographic variables are measured as of 9th grade. All regressions include 9th grade campus and cohort fixed effects. See Table 7 for more detail on the sample. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.7: The Heterogenous Effects of DACA By Predicted Likelihood of High School Graduation and Baseline Characteristics, US-born students

	Predicted Likelihood Graduation			Disciplined in G8		ELL in G8	
	Low	Medium	High	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Summary Index of Educational Attainment							
DACAShare*	9.440***	5.056**	3.245	5.707**	4.757**	4.361	5.338**
Exposed	(2.738)	(2.212)	(2.093)	(2.865)	(2.138)	(2.689)	(2.071)
	[0.094]	[0.051]	[0.032]	[0.057]	[0.048]	[0.044]	[0.053]
N	79,597	79,593	79,591	23,359	214,598	45,407	192,550
Panel B: Summary Index of Academic Achievement							
DACAShare*	4.516***	7.591***	7.416***	0.529	2.650***	3.392***	5.976***
Post	(1.249)	(1.029)	(1.241)	(0.853)	(0.431)	(1.288)	(1.041)
	[0.045]	[0.076]	[0.074]	[0.005]	[0.026]	[0.034]	[0.060]
Observations	248,393	273,641	284,254	68,039	738,249	148,450	657,838

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on a summary index of educational attainment and a summary index of educational achievement for students with different likelihoods of graduating high school and whether students were disciplined at baseline (in 8th grade). I use the full set of controls to predict the likelihood of graduating from high school. This likelihood is the split into three terciles, from the lowest likelihood in column (1) to the highest likelihood in column (3). In Panel A, each column reports estimates of α_1 from a separate regression of Equation 5. In Panel B, each column reports estimates of γ_1 from a separate regression of Equation 6. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. All regressions include campus-year and campus-grade fixed effects. Regressions also include the full set of individual and cohort level controls. See Table 7 for more detail on the sample and the full set of controls. In Panel B, the regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.8: The Effect of DACA on Educational Investments of Hispanic Foreign-Born Students – Robustness of Results to the Proxy Used to Approximate Undocumented Status

	DACA Apps Ages 15-30	DACA Apps Ages 15-19	Estimated Undoc	Non-Citizens
	(1)	(2)	(3)	(4)
Panel A: Enrolled in 12th Grade				
ShareEligible*Exposed	0.179*	0.0605	0.0309	0.184**
	(0.0969)	(0.0392)	(0.0255)	(0.0921)
	[0.0249]	[0.0206]	[0.0153]	[0.160]
Mean (Y)	0.776	0.776	0.776	0.776
Panel B: Graduated from High School				
ShareEligible*Exposed	0.248**	0.0832*	0.0119	0.167*
	(0.113)	(0.0487)	(0.0272)	(0.0967)
	[0.0344]	[0.0284]	[0.00588]	[0.145]
Mean (Y)	0.564	0.564	0.564	0.564
N	21,139	21,139	21,121	21,121
Panel C: Standardized Exam Performance (ELA))				
ShareEligible*Post	0.553**	0.227**	0.138***	0.414***
	(0.237)	(0.0875)	(0.0459)	(0.150)
	[0.0767]	[0.0775]	[0.0683]	[0.360]
Mean (Y)	-0.0922	-0.0922	-0.0922	-0.0922
Observations	43,153	43,153	43,109	43,109
Mean Proxy	0.139	0.341	0.495	0.870

Note: This table contains difference-in-differences estimates where undocumented status is approximated in several different ways. Column 1 uses Equation 1 to approximate undocumented status (i.e. my preferred specification), Column 2 uses a modified version of Equation 1 that accounts for the fraction of DACA-applicants estimated to be high-school aged, Column 3 uses the fraction of the foreign-born population ages 1-18 estimated to be undocumented by the Migration Policy Institute (MPI) at the PUMA, and Column 4 uses the fraction of foreign-born non-citizens in a zip-code. In Panels A and B, each column reports estimates of α_1 from a separate regression of Equation 5. In Panel C, each column reports estimates of γ_1 from a separate regression of Equation 6. The full set of controls and information on the sample is specified in Table 3. The regressions in Panel C are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average foreign-born student are shown in brackets, and is defined as the coefficient multiplied by the mean fraction of foreign-born estimated to be undocumented in a given zip-code (shown in the last row of this table). Standard errors in parentheses are clustered at the residence zip-code level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.9: The Effect of DACA on Educational Investments of US-Born Students – Robustness of Results to Scaling of Foreign-Born Peer Measure

	DACA Apps Ages 15-30	DACA Apps Ages 15-19	Estimated Undoc	Non-Citizens	None
	(1)	(2)	(3)	(4)	(5)
Panel A: Enrolled in 12th Grade					
DACAShare*Exposed	2.625***	1.152***	0.547**	0.427*	-0.0455
	(0.928)	(0.401)	(0.251)	(0.220)	(0.0867)
	[0.0246]	[0.0264]	[0.0182]	[0.0249]	[-0.00770]
Mean (Y)	0.771	0.771	0.771	0.771	0.771
Panel B: Graduated from High School					
DACAShare*Exposed	2.418**	1.261***	0.599**	0.454*	0.0704
	(1.078)	(0.464)	(0.292)	(0.236)	(0.122)
	[0.0227]	[0.0289]	[0.0199]	[0.0265]	[0.0119]
Mean (Y)	0.576	0.576	0.576	0.576	0.576
N	238,781	238,781	238,781	238,781	238,781
Panel C: Standardized Exam Performance (ELA)					
DACAShare*Post	6.539***	2.826***	1.565***	0.984***	0.0976
	(1.302)	(0.587)	(0.373)	(0.256)	(0.137)
	[0.0640]	[0.0677]	[0.0541]	[0.0600]	[0.0160]
Mean (Y)	0.0664	0.0664	0.0664	0.0664	0.0664
Observations	490,051	490,051	490,051	490,051	490,051
Mean DACA peers	0.010	0.024	0.034	0.060	0.165

Note: This table contains difference-in-differences estimates where the fraction of undocumented peers is approximated in several different ways. Column 1 uses Equation 1 to approximate undocumented status of one's foreign-born hispanic peers (i.e. our preferred specification), Column 2 uses a modified version of Equation 1 that accounts for the fraction of DACA-applicants estimated to be high-school aged, Column 3 uses the fraction of the foreign-born population ages 1-18 estimated to be undocumented by the Migration Policy Institute (MPI) at the PUMA, Column 4 uses the fraction of foreign-born non-citizens in a zip-code, and Column 5 focuses on the fraction of one's peers who were foreign-born. In Panels A and B, each column reports estimates of α_1 from a separate regression of Equation 5. In Panel C, each column reports estimates of γ_1 from a separate regression of Equation 6. The full set of controls and information on the sample is specified in Table 7. The regressions in Panel C are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average high school student is shown in brackets, and is defined as the coefficient multiplied by the mean estimated value of undocumented peers (shown in the last row of this table). Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.10: Peer Effects of DACA on Educational Attainment and Achievement – Accounting for Differences in Campus-Level Characteristics, US-born Students

Panel A: Enrolled in 12th Grade						
DACAShare*Exposed	2.625***	2.526**	3.426***	3.823***	2.336***	2.826
	(0.928)	(1.127)	(1.163)	(1.095)	(0.875)	(1.843)
	[0.026]	[0.025]	[0.034]	[0.038]	[0.023]	[0.028]
Mean (Y)	0.771	0.771	0.771	0.771	0.771	0.771
Panel B: Graduated from High School						
DACAShare*Exposed	2.418**	2.642**	3.450***	3.403**	2.220**	1.024
	(1.078)	(1.235)	(1.270)	(1.449)	(1.040)	(1.703)
	[0.024]	[0.026]	[0.034]	[0.034]	[0.022]	[0.010]
Mean (Y)	0.576	0.576	0.576	0.576	0.576	0.576
N	238781	238781	238781	238781	238781	238781
Panel C: Standardized Exam Performance (ELA)						
DACAShare*Post	6.537***	5.501***	5.169***	4.967***	6.414***	2.791**
	(1.300)	(1.657)	(1.684)	(1.404)	(1.292)	(1.372)
	[0.065]	[0.055]	[0.052]	[0.050]	[0.064]	[0.028]
Mean (Y)	0.0664	0.0664	0.0664	0.0664	0.0664	0.0664
Observations	490,051	490,051	490,051	490,051	490,051	490,051
Controls						
$f(t)\times$FL		X				
$f(t)\times$ G8 ELA			X			
$f(t)\times$ ELL				X		
$f(t)\times$ Cohort Size					X	
$f(t)\times$ Racial Composition						X

Notes: This table shows difference-in-differences estimates of the spillover effects of DACA on high school enrollment and graduation, as well as on yearly standardized test performance on ELA exams. These models use the full set of controls specified in Table 7 and also linear time trends that vary by the fraction of a campus that received free or reduced price lunch (FRL), average baseline ELA achievement, the fraction of the campus that was classified as an English Language Learner (ELL), the size of the cohort, and the fraction of the campus belonging to each of the largest racial groupings (Hispanic, black, white, and asian), all measured in 2012. In Panels A and B, each column reports estimates of α_1 from a separate regression of Equation 5. In Panel C, each column reports estimates of γ_1 from a separate regression of Equation 6. See Table 7 for the full list of controls and more information about the sample. The regressions in Panel C are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *$p < 0.10$, **$p < 0.05$, *** $p < 0.01$.

Table A.11: Additional Educational Policy Pressures

Panel A: Baseline Campus Measures by Concentration of DACA-eligible Peers					
Fraction Campus	Pass HS Exit First Attempt				
DACA-eligible	Math	Reading	Discipline Rate	Graduation Rate	ELL Rate
	(1)	(2)	(3)	(4)	(5)
1=Lowest	0.739	0.743	0.039	0.520	0.110
2	0.709	0.700	0.036	0.532	0.189
3	0.653	0.642	0.042	0.512	0.224
4=Highest	0.700	0.688	0.034	0.519	0.192

Panel B: Correlation b/w Concentration of DACA-eligible Peers and Baseline Campus Measures					
	Pass HS Exit First Attempt				
	Math	Reading	Discipline Rate	Graduation Rate	ELL Rate
	(1)	(2)	(3)	(4)	(5)
Correlation Coefficient	-0.132	-0.183	-0.083	0.065	0.315

Notes: This table shows several different campus measures related to other educational policies that occurred around the time of DACA's introduction. Panel A shows the fraction of students who passed the high school exit exam on their first attempt during 10th grade in 2012, the fraction of students who were suspended in 2012, the fraction of students who graduated high school during the pre-policy period (in 9th grade cohorts between 2007 and 2010) and the fraction of students receiving ELL services in 2012 across campuses grouped by the quartile of the concentration of a campus' undocumented peers. Panel B shows the raw correlation coefficient between the concentration of undocumented peers and the average rating in each of these other areas.

Table A.12: Peer Effects of DACA on Educational Attainment and Achievement – Accounting for Other Educational Policies, US-born Students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Summary Index of Educational Attainment							
ShareEligible*Exposed	5.882***	6.090***	6.207***	4.834**	3.517*	5.279**	6.979***
	(2.065)	(2.026)	(2.070)	(2.197)	(1.971)	(2.069)	(2.470)
	[0.059]	[0.061]	[0.062]	[0.048]	[0.035]	[0.053]	[0.070]
N	238,781	238,781	238,781	238,781	238,781	226,894	238,781
Panel B: Summary Index of Academic Achievement							
ShareEligible*Post	8.062***	8.256***	8.143***	7.350***	7.473***	7.982***	7.698***
	(1.031)	(1.064)	(1.066)	(1.127)	(1.000)	(1.181)	(1.033)
	[0.081]	[0.083]	[0.081]	[0.073]	[0.075]	[0.080]	[0.077]
Observations	634,546	634,546	634,546	634,546	634,546	603,255,	634,546
Controls							
$f(t)\times$ Pass Math Exit		X		X			
$f(t)\times$ Pass ELA Exit			X	X			
$f(t)\times$ Discipline Rate					X		
$f(t)\times$ Graduation Rate						X	
$f(t)\times$ ELL Rate							X

Notes: This table shows difference-in-differences estimates of the spillover effects of DACA on a summary index of educational attainment (Panel A) and academic achievement (Panel B). In Panel A, each column reports estimates of α_1 from a separate regression of Equation 5. In Panel B, each column reports estimates of γ_1 from a separate regression of Equation 6. These models use the full set of controls specified in Table 7 and also linear time trends that vary by campus level characteristics, including the fraction of 10th graders who passed the high school exit exam in 2012 are shown in, the discipline rate in 2012, the graduation rate for pre-policy 9th grade cohorts (i.e. those in 9th grade between 2007 and 2010), and the fraction of 9th grade ELL students. See Table 7 for the full list of controls and more information about the specifications that were run. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p< 0.10, **p < 0.05, *** p< 0.01.