Dreamers and Beyond: Examining the Broader Educational Effects of DACA

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Abstract

This paper examines the direct effects and spillover effects of the Deferred Action for Childhood Arrivals (DACA) on human capital investments during high school. DACA significantly increased the returns to schooling for undocumented youth, while leaving the returns for their US-born peers unchanged. I estimate the impacts of DACA using detailed administrative data from Los Angeles Unified School District. 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

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1 Introduction

The introduction of Deferred Action for Childhood Arrivals (DACA) in 2012 provided legal protection for over 650,000 undocumented youth living in the US who were enrolled in school or had completed high school. This was one of the largest immigration reforms in recent years and has remained at the forefront of immigration policy debates. Recent studies find that gaining DACA eligibility after high school improved employment, health, and housing outcomes (e.g. Pope, 2016a; Amuedo-Dorantes & Antman, 2017; Hsin & Ortega, 2018; Giuntella & Lonsky, 2020; Christopher, 2022). However to date, the literature on the impact of becoming DACA-eligible during high school is limited; furthermore, to my knowledge, no study has investigated DACA's spillovers on in-eligible high school students. Given the importance of high school years where educational decisions can have long-lasting and potentially irreversible consequences, DACA-eligibility and spillovers during these formative years could have been even more impactful than DACA exposure at older ages. Better understanding how DACA impacted pre-college human capital investments has important implications for understanding the extent to which the absence of legal status directly and indirectly inhibits educational investments and later adulthood success for the over 3.2 million undocumented youth currently enrolled in US schools (who are mostly ineligible for DACA) and their peers.[1]

In this paper, I examine the direct and spillover effects of DACA on high school students using detailed administrative data from Los Angeles schools, a setting where educational attainment lagged significantly behind the national average at the time of DACA's introduction (Hansen, 2017). Due to minimum education eligibility requirements, DACA provides an ideal natural experiment to better understand how high school students respond to an increase in the returns to schooling. Prior work shows that nationally, undocumented youth were more likely to complete high school after DACA's enactment (Kuka, Shenhav, & Shih, 2020). Little is known, however, about the extent to which high school effort increased beyond what was required to meet DACA's minimum

[1] Most currently enrolled undocumented students do not qualify for DACA. As will be discussed in Section 2 where I describe DACA's eligibility requirements in more detail, this is driven by the fact that DACA eligibility requires arrival to the US by 2007, before most currently enrolled K-12 students were born.

education requirements. Moreover, while DACA led to a discrete change in the returns to schooling for undocumented youth, it left the returns for their US-born peers unchanged. On the one hand, increases in effort among undocumented students could have inspired their US-born peers to study more. For instance, prior research has shown that adolescents are highly responsive to changes in social norms regarding educational investments (Bursztyn & Jensen, 2015; Bursztyn, Egorov, & Jensen, 2018), and it is plausible that DACA led to changes in peer cultures that encouraged additional educational investments in areas with high shares of undocumented youth. On the other hand, with no added benefit to completing high school, US-born students may have chosen to make similar educational investment decisions after DACA, or could have been negatively affected if DACA induced lower achievers to stay enrolled in school. To date, prior studies have ignored the possibility of spillovers on US-born. Given plausibly exogenous changes in the returns to schooling among *existing* peer networks are rarely observed, DACA provides an ideal opportunity to better understand how responsive educational investments are to changes in peer educational investments.

I use administrative data from Los Angeles Unified School District (LAUSD) together with administrative data on DACA applicants from the US Citizenship and Immigration Services (USCIS). 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. Given take-up of DACA was high in Los Angeles, these data allow me to create proxies for students' legal status that minimize measurement error. To determine the direct effects of DACA, I compare changes in educational outcomes of foreign-born students who are more and less likely to be DACA-eligible based on the concentration of DACA applicants in their residence zip code, 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 with higher and lower shares of DACA-eligible high school peers, before and after DACA's introduction.

I find that DACA led to significant increases in targeted students' educational investments. High school graduation increased by 7 p.p. (or 12%) 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 these effects 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 0.15 standard deviations after DACA's enactment. Finding significant improvements in performance, serve as a useful proxy for determining whether undocumented youth increased high school effort and were more engaged in school after DACA.

These increases in educational investments of targeted students had positive spillover effects on undocumented students' US-born peers: at the average campus, where 3.4 percent of students were likely undocumented, DACA's introduction leads to a 2 p.p. (or 4 percent) increase in US-born students' probability of high school completion. These increases are driven by low-achievers, who are most on the margin of dropping out of high school. US-born students' ELA achievement during high school also increases by 0.06 standard deviations after DACA's enactment. Gains in achievement occurred for US-born students across the baseline achievement distribution. These positive spillovers are consistent with direct peer-to-peer influences (i.e. increases in effort among undocumented students inspiring their US-born peers to study harder) or changes in classroom dynamics due to academic improvements among undocumented students.

Ultimately, I cannot identify the precise channel for the positive spillovers; however several pieces of evidence point to peer-to-peer influences as being the primary driver of the results, rather than changes in classroom dynamics. Importantly, spillovers are largest for closer contacts.[2] If other changes in school-wide instruction were driving the positive spillovers, there is no reason for closer contacts to have a stronger impact. Moreover, the heterogeneous results suggest that the direct and spillover effects occurred for students who shared similar pre-determined characteristics. This provides further evidence consistent with peer-to-peer interactions, as several studies demonstrate

[2]As will further be justified in Section 5.3, 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 youth forge the closest bonds with those who share similar characteristics (e.g. Moody, 2001). Finally, I consider possible neighborhood and family changes and find that any increases in home resources or neighborhood improvements due to DACA are unlikely to be the primary mechanism behind the positive spillovers on US-born students.[3]

My primary contribution is to empirically show that policies such as DACA that increase the returns to school for vulnerable subgroups can have important positive spillovers. 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 that tests whether such policies spillover to non-eligible peers (Abramitzky, Lavy, & Perez, 2021), who find that a pay reform change that improved educational outcomes among kibbutz members in Israel also increased educational attainment for non-kibbutz peers. My project builds upon this recent work by addressing whether similar spillovers due to an increase in the returns to schooling exist for students in a large low-performing US school district.

In addition, I contribute to the emerging literature on DACA. To date, most studies have focused on understanding how the policy affected undocumented 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 undocumented youth who experienced DACA during high school. 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. To my knowledge, I am the first to use administrative K-12 schooling data to explore the direct impacts of DACA on a rich set of schooling outcomes. Importantly, this allows me to ascertain whether DACA impacted achievement, as a useful proxy for high school effort, as well as determine DACA's impact on different types of students, such as those initially higher or lower achieving. This has important implications for understanding

[3]Appendix 5.4 includes details on several additional robustness checks that help rule out other alternative explanations. On the whole, DACA was the major policy change affecting undocumented youth at this time. The one exception is that California extended state financial aid eligibility to undocumented youth. Online Appendix C.1 includes details on compelling evidence consistent with DACA driving the results. Nonetheless, it is important to acknowledge that part of my effects could also be driven by increases in college affordability which also increases the returns to schooling.

whether lack of legal status inhibits educational investments and subsequent adult outcomes.[4] In addition, I use a novel approach to approximate the undocumented population that uses zip-code level variation in the concentration of DACA applicants to proxy for each foreign-born student's likely undocumented status. In the context of Los Angeles (with high take-up), this allows me to estimate DACA eligibility with minimal measurement error.[5] Finally, to my knowledge, no study has investigated DACA's spillovers on in-eligible high school students as most studies on DACA utilize national survey data making it difficult to connect undocumented students to peer groups.

My results also speak to several strands of the peer effects literature. Most related is to the growing literature that explores how peer personality and peer quality affect educational investments. 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. In this paper, I show that an increase in peer effort, as proxied by increases in academic achievement, leads to sizeable positive impacts on educational investments during high school.[6] My context is unique within this literature, as I am able to demonstrate the importance of a plausibly exogenous increase in peer effort among *existing* peer groups. 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) in a high school where educational investments are low.

In addition, this paper contributes to the literature on the spillover effects of immigration on the educational outcomes of US-born students. While this topic is typically difficult to study since immigrants are not randomly assigned to schools, several recent have been able to overcome

[4]Only one prior study has considered whether DACA affected high school achievement. Using county-level aggregate data from California, Kuka et al. (2020) find that DACA increased the the high school exit exam pass rate among Hispanic seniors approaching their final opportunity to pass in counties with more undocumented youth.

[5]Most prior literature relies on the absence of US citizenship and Hispanic ethnicity as a second best measure for undocumented status (e.g. Kuka et al., 2020; Pope, 2016a; Amuedo-Dorantes & Antman, 2017). However, this is measured with noise, as non-citizens include green card holders and temporary visa holders. As discussed in Section 3, there is likely to be less noise in my measure. However, my results are robust to other proxies (including non-citizens).

[6]It is important to note that I cannot determine whether these increases are driven by a change in one's intrinsic personality or improvements in mental health or family environments perhaps driven by a reduction in fear of deportation.

these selection concerns. These studies have focused on the impacts of specific immigrant groups such as refugees (Figlio & Özek, 2019; Morales, 2020; Van der Werf, 2021) or the overall impact of immigrants on US-born students (Figlio, Giuliano, Marchingiglio, Özek, & Sapienza, 2021). In general these studies find that immigrant students have a small positive influence on US-born peers. This paper adds to this literature on the peer effects of immigration, which has until now ignored the role of legal status on driving any spillover effects associated with immigrant children.

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) 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 2012 - 2014 in Los Angeles county by the 111,000 youths estimated to be immediately eligible for DACA yields a take-up rate of 65% (Batalova, Hooker, & Capps, 2014). For high school students ages 15-19 in Los Angeles during the same time period, dividing the 29,593 initial applications received by the 42,672 youth estimated to be immediately eligible yields a take-up rate of 69%. The higher take-up in Los Angeles can likely be attributed to the strong presence of pro-immigration

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

rights groups who undertook extensive outreach activities immediately after DACA's enactment.[8]

The key benefits of DACA receipt include temporary protection from deportation and a work permit (or an Employment Authorization Document). However, DACA-recipients also experience other benefits. For instance, applying for a work permit allows an individual to apply for a social security number, making it possible to apply for a driver's license in most states and eases access to credit. In addition, as will be discussed in more detail in Section 2.1, many states including California expanded state financial aid eligibility to undocumented youth around the time of DACA's enactment.[9] Several pieces of evidence point to DACA, rather than the expansions in financial aid access occurring during this period as being the primary driver of my results.[10] Nonetheless, the results of this study will combine the impacts of DACA's key benefits (i.e. temporary deportation protection and a work permit), as well as other associated benefits and other policies introduced at the time, such as the CDA, geared towards easing burdens typically faced by undocumented youth.

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).

[8] For instance, pro-immigration rights groups such as the Coalition for Humane Immigrant Rights (CHIRLA) were instrumental in advertising DACA and providing legal and financial application assistance. Also, local Spanish media and the Catholic Church played a critical role in advertising and encouraging DACA-eligible youth to apply across Los Angeles (information from https://www.lamayor.org/community_based_organizations and www.daca100.org).

[9] According to the Higher Ed Immigration portal as of 2022, in 19 states (including California) undocumented youth are eligible for state financial aid or scholarships. These states are home to 72% of the undocumented population.

[10] Online Appendix C.1 provides details on the role of state financial aid eligibility and why I view it as unlikely that state financial aid eligibility expansions for undocumented youth played a significant role in California.

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.[11] 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 than wage offered in undocumented youth's country of origin at all education levels.[12]

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 DACA status. However, even if undocumented youth do not consider the change to lifetime earnings, they may still choose to complete high school if they prefer living in the US and value the temporary protection from deportation or the other associated benefits DACA offers. Consistent with this model, Kuka et al. (2020) find that nationally likely undocumented youth were significantly more likely to complete high school after DACA.

The prediction of Kuka et al. (2020)'s model on college enrollment is less clear. College enrollment was not tied to eligibility, but DACA participation likely eased several burdens faced by undocumented youth in higher education – from increasing the certainty and expected earnings after college graduation to being able to work legally while attending college. At the same time, the opportunity cost of college enrollment increases since undocumented youth who benefit from DACA are now able to earn the legal wage. Kuka et al. (2020) find that nationally likely undocumented youth exposed to DACA experienced positive (but imprecise) increases in college enrollment.

[11] 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).

[12] 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.

Finally, while the above mentioned model considers the benefits of DACA that arise through increased wages, it is also important to acknowledge other changes for undocumented youth occurring during this period that could have influenced educational investments. For instance, undocumented youth in California became eligible for merit-based state financial aid in January of 2013 with the implementation of the California Dream Act (CDA). While California's undocumented youth were eligible for in-state tuition since 2001, eligibility for merit-based financial aid packages such as the California Grant (Cal Grant) additionally reduced the expected college costs by 38 percent, and could have further increased the returns and appeal to college.[13]

2.2 Expected Impacts on US-born Students

The expected impacts of DACA on US-born students is less clear. While DACA's introduction increased the returns to schooling for undocumented youth, it did not introduce any direct changes in the returns to schooling for US-born students. Importantly, however, for US-born students, the policy changed the educational investments of some students (who were undocumented) in their *existing* peer networks. On the one hand, increases in effort among undocumented students could have inspired their US-born peers to study more. Prior research has shown that adolescents are highly responsive to changes in social norms regarding educational investments (Bursztyn & Jensen, 2015; Bursztyn et al., 2018), and it is plausible that DACA led to changes in peer cultures that encouraged additional educational investments in areas with high shares of undocumented youth. On the other hand, with no added benefit to completing high school, US-born students may have chosen to make similar educational investment decisions before and after DACA.

In addition to the spillovers driven by peer interactions just discussed, it is important to acknowledge that US-born students could have also been affected by other school-level changes occurring in response to DACA. For instance, improvements in undocumented students' outcomes could have freed up teachers' time to improve instruction. However, if DACA induced lower-achieving students to remain enrolled in school, this may have taken up teachers time (or school

[13] See Online Appendix C.1 for a detailed discussion of the role of the CDA.

level resources in general) to the disadvantage of their US-born peers. I investigate these possibilities in greater detail in Section 5.3, when I explore the possible mechanisms behind the spillover results.

2.3 Undocumented Population in Los Angeles

Los Angeles provides an ideal setting to study the educational effects of DACA. 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 (and continues to be) very popular among students in the city (Stewart et al., 2019). 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). In addition to broad support among students, there was 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 increase enrollment among high school students who were eligible (Singer, Svajilenka, & Wilson, 2015). 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, before DACA's enactment, educational attainment in Los Angeles was low relative to the rest of the US. In 2012, only roughly 60% of high school seniors graduated from high school on-time (i.e. within 4 years of starting 9th grade). And among those who were undocumented and met all of the age and date of arrival DACA criteria, 30% already dropped out of high school, and among high school graduates, over 70% did not pursue higher education (McHugh, 2014).

In terms of spillovers, Los Angeles provides a unique setting since undocumented youth share 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 Mexican-born parents. Moreover, due to DACA's age/date of arrival criteria, DACA-eligible youth are not recent immigrants. The majority of DACA-eligible youth have spent the majority of their schooling in LAUSD, thereby increasing the likelihood that they were well integrated with their US-born peers at the time the policy was introduced.[14]

3 Data

My analysis relies on detailed administrative data from LAUSD. These data track key yearly academic and behavioral outcomes, 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, yearly enrollment and high school completion.[15] Importantly, these data also includes each student's country of birth, date of arrival to the US (if foreign-born), and residence zip-code, which are critical for my approach to approximating undocumented status.

These data are advantageous as they provide a large number of demographics and outcomes for the second largest school district in the US, serving over 600,000 students annually. However, one drawback is that I cannot follow those who leave LAUSD. Restricting the sample to those observed in 9th grade minimizes the possibility that these students left LAUSD for other reasons beyond high school drop-out. Moreover, I find that DACA increased enrollment in LAUSD, suggesting that attrition out of LAUSD to other districts is uncorrelated with policy exposure.[16]

While these data allow me to identify foreign-born students and their country of origin, like other studies' I cannot directly observe whether a foreign-born student is undocumented in LAUSD data. 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

[14] DACA-eligible youth had to spend at least 5 years of US residence at the time of DACA's enactment. The median age of US entry among DACA-eligible youth was 6 and the most common age was 3 (Parlapiano & Yourish, 2018).

[15] Although I observe math and ELA exam scores, I primarily focus on ELA. This is because beginning in 8th grade students select their math exam version, making the scores across versions within the same grade not easily comparable.

[16] As will be discussed in more detail in Section 5.4, changes in completion are only driven by those on the margin of high school completion decisions (i.e. low achievers). The lack of response for high-achievers and strong response for low-achievers is consistent with changes in high school completion decisions as opposed to changes in attrition.

and year provided by the US Citizenship and Immigration Services (USCIS), together with estimates of the number of foreign-born residents by age, zip-code and year provided by 5-year ACS estimates. Then, for each zip-code, I construct an "applicant share" estimated 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 counts the number of DACA applicants and the denominator counts the number of foreign-born youth immediately after DACA's enactment.[17] For each foreign-born student, I use the applicant share in their residence zip-code to proxy for their likely DACA-eligibility. As illustrated in Figure 1, there is significant variation in this measure across Los Angeles zip-codes.[18]

Importantly, since take-up of DACA was high in Los Angeles this measure will approximate the underlying DACA-eligible population closely. Nevertheless, Equation 1 will undercount the DACA-eligible 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 since I do not observe counts of undocumented youth by zip-code, event-study plots presented in Section 4.2 demonstrate that educational outcomes of foreign-born students in zip-codes with different concentrations of DACA-applicants had similar pre-trends. I also find that the DACA applicant share is uncorrelated with the zip-code level share of non-citizen youth living in poverty or with less than a high school degree, as shown in Appendix Figure A.2. This suggests that take-up across zip-codes is unlikely to be correlated with underlying socio-economic population differences of non-citizens, a close but imperfect approximation of the undocumented population. Furthermore, as previously noted, the broad outreach of organizations such as DACA100, suggests that DACA was salient throughout Los

[17] This measure captures take-up for the *full* DACA-eligible population since the numerator includes all DACA applicants ages 15-30 and the denominator includes foreign-born youth ages 15-29. As discussed in more detail in Online Appendix D, I focus on an alternate measure that approximates the applicant share for high school youth using county-level estimates of the take up of DACA by age. Reassuringly, this alternate measure yields very similar results.

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

Angeles making differential take-up across zip-codes less likely. Reassuringly, in Online Appendix D I show that my results are similar if I use other measures to approximate undocumented status that do not select on the DACA application decision (e.g. share of foreign-born non-citizens).

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

$$\text{DACAShare}_{sc} = \text{FBShare}_{sc} \times \left(\frac{\sum_{z=1}^{N} \text{n}_{scz} \times \text{ShareEligible}_z}{\text{n}_{sc}} \right)_{sc} \quad (2)$$

where s and c represent high school campus and 9th grade cohort, respectively. FBShare_{sc} is the share of immigrants in each student's campus as of 9th grade, rescaled by the second term which captures their likely DACA-eligibility.[19] Specifically, this is the weighted average of the DACA application share in Equation 1 across the residence zip-codes of immigrants enrolled in each student's 9th grade campus, where n_{sc} and n_{scz} denotes the number of immigrants overall and in a given zip-code, respectively. Finally, as will be discussed in Section 3.1, I construct this measure using a *likely* DACA-eligible sample of foreign-born Hispanics who arrived to US by age 9.

3.1 Sample Construction

To estimate the direct impacts of DACA, I focus on foreign-born students entering 9th grade between 2007 and 2014. This includes 9th grade cohorts who were unexposed (2007-2009), partially exposed (2010-2012) and fully exposed (2013-2014) to DACA during high school.[20] Following the literature on DACA, I further restrict the sample to populations more likely to be undocumented, such as Hispanics and Mexican-born.[21] Finally, I limit my sample to those who arrived to the US by age

[19] This is a school-level measure, however, estimates that focus on cohort-level shares yield nearly identical results. Because high school students are likely to take classes and interact with students across different grades, a school level measure is likely to more accurately capture relevant peer groups.

[20] Appendix Table A.1 shows *expected* DACA exposure by each 9th grade cohort, where *expected* exposure is defined during the four years following 9th grade enrollment.

[21] Over 95% of DACA applicants in California are Hispanic, and the vast majority are born in Mexico (86%) (Svajlenka & Singer, 2013). Since 83% of foreign-born LAUSD students are Hispanic, this sample restriction doesn't significantly reduce the sample. Mexicans serve as a high impact sample, as they are less likely to enter the US as refugees compared to immigrants from Central America who over this period faced natural disasters and civil wars that made them more likely to qualify for Temporary Protected Status (TPS) as refugees (Babich & Batalova, 2021).

9. Without this restriction, the age composition of DACA-eligible youth across cohorts would differ due to fact that DACA-eligible youth had to arrive to the US by June 15, 2007. Since one's age in 2007 determines the oldest age at US-arrival required to be DACA-eligible, this results in younger cohorts having to arrive to US earlier than older cohorts. That is, DACA-eligible youth in the 2013-14 cohort include those that arrived before age 9, while those in the 2006-07 cohort includes those that arrived before age 16. Restricting the sample to the age that the youngest cohort in my sample had to arrive to the US by ensures that all undocumented youth in my sample would be DACA-eligible regardless of their 9th grade cohort. Given one might expect immigrants who arrive to the US earlier to have better outcomes, not making this age adjustment could influence the estimates.[22] The final direct impacts sample consists of 21,139 students. For some results (such as the event-studies), I focus on Mexican born students given they are more likely to be undocumented. The final sample that restricts to Mexican-born students consists of 17,247 students.

To estimate the spillover effects of DACA, I focus on US-born students entering 9th grade between 2007 and 2014. Importantly, this subgroup is not eligible for DACA, and if impacted by DACA would only be through policy spillovers. The final sample I use to estimate the spillover effects of DACA consists of 238,781 US-born students.

3.2 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 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.

[22] Kuka et al. (2020) make a similar age adjustment.

Relative to all foreign-born youth, Hispanics 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 age 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 enrollment, is similar across all the subgroups shown in Table 1.

Table 2 presents summary statistics that compare campuses with different shares of DACA-eligible 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, those in campuses with higher concentrations of likely DACA-eligible 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 DACA-eligible youth be uncorrelated with school characteristics, it does require that the fraction of DACA-eligible 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 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, Appendix Table A.7 shows that my results are robust to the inclusion of time trends interacted with baseline campus demographics.[23]

4 Direct Impacts

4.1 Empirical Strategy

I first estimate the impact of DACA on the educational investments of undocumented high school students 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 the changes for foreign-born citizens who did not. However, as previously noted, this strategy is infeasible because I cannot observe a students' legal status.

[23] See Section 5.1 for a more detailed discussion of these results.

Instead, I leverage differences across foreign-born youth in their *likelihood* of being undocumented by exploiting differences in the concentration of DACA applicants in their residence zip-code as defined in Equation 1. Any impact of DACA should be increasing with this measure, since the more foreign-born who applied to DACA in a students zip-code, the higher the corresponding likelihood that a given foreign-born individual living in that zip-code was undocumented.

To identify the direct effects of DACA on educational attainment outcomes, I estimate the following event-study equation on the sample of foreign-born youth:

$$Y_{izsc} = \delta_0 + \sum_{j=2007}^{2014} \delta_j \mathbb{1}\{c = j\} \times \text{ShareEligible}_z + \lambda_1 X_{izsc} + \lambda_2 Z_{sc} + \gamma_s + \gamma_z + \phi_c + \varepsilon_{izsc} \quad (3)$$

where Y_{izsc} is an indicator for high school completion or enrollment in each grade for foreign-born student i in 9th grade cohort c attending high school campus s and 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 9th grade cohort dummies. I control for zip-code γ_z and campus γ_s fixed effects to account for fixed cross-sectional differences across zip-codes and campuses, respectively. I also control for cohort fixed effects ϕ_c to account for trends in high school completion that could affect all Los Angeles students. X_{izsc} includes individual characteristics that include age of arrival to the US, gender and disability status, all measured in 9th grade, as well as ELA test scores measured in 8th grade.[24] Finally, Z_{sc} accounts for school by cohort demographics that include the fraction of students who are male, by racial group, and receiving special education, all measured as of 9th grade. The main variables of interest, δ_j, identify the average impact of DACA on educational attainment of likely undocumented youth for each 9th grade cohort separately.

For yearly academic and behavioral outcomes, I estimate a slightly modified version of Equation 3 on the sample of foreign-born youth as follows:

[24] My preferred specification omits controls for free or reduced price lunch status (FRL) and lagged math test-scores. Since parents must apply for FRL, those who are undocumented may be less likely to apply. In addition, 8th grade students can choose their math test version. Reassuringly, results are robust to including these controls.

$$Y_{izstg} = \beta_0 + \sum_{j=2005}^{2016} \beta_j \mathbb{1}\{j = t\} \times \text{ShareEligible}_z + \lambda_1 X_{izsc} + \lambda_2 Z_{sc} + \phi_{sg} + \alpha_{tg} + \gamma_z + \varepsilon_{izstg} \quad (4)$$

where Y_{izstg} 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 residence zip-code with yearly dummy variables. ϕ_{sg} and α_{tg} are school-grade and year-grade fixed effects, respectively. All other control variables are as previously defined. The variables of interest, β_j, trace out the impact of DACA on academic and behavioral outcomes of likely undocumented youth in each year.

The main identification assumption is that educational investments would have evolved similarly for foreign-born students residing in zip-codes with different DACA applicant shares in the absence of DACA. In favor of this assumption, event-study estimates presented in Section 4.2 show that foreign-born students living in zip-codes with different DACA applicant shares had similar pre-trends. In addition, Appendix Table A.2 demonstrates that foreign-born students living in zip-codes with higher DACA applicant shares are not predicted to have higher high school completion rates, based on observable characteristics, at the time of DACA's introduction.[25] This is true for the full sample shown in Panel A, and for the sample used to estimate yearly outcomes, who remained enrolled *throughout* high school shown in Panel B.[26] This suggests that the change in educational investments after DACA can be attributed to behavioral changes among likely undocumented youth, rather than by compositional changes in zip-codes with higher DACA application shares.

Finally, the identification strategy requires that there were no contemporaneous shocks that differentially impacted undocumented youth. I assess the plausibility of this assumption more carefully in Section 5.4, where I consider several other policies during this period. Overall, I am able to rule out several alternative explanations for my findings. However, as previously noted, and

[25]Predicted high school completion is generated using all covariates (excluding treatment) and pre-policy students.

[26]On the whole, DACA also does not predict changes in individual covariates except for an increase in the likelihood of being male for those who remained enrolled throughout high school. However, this is consistent with the fact I document increases in high school enrollment for males who are typically on the margin of high school completion.

will be further discussed in Section 5.4, the introduction of the CDA which extended state financial aid eligibility to undocumented youth could impact educational investments during this time period. While I find several pieces of evidence consistent with DACA primarily driving my results, given the close timing of when these policies were implemented it is important to acknowledge that some of the results below could be driven by the CDA. Ultimately, both DACA and the CDA increase the returns to schooling. Moreover, as previously noted the vast majority of undocumented youth live in states that offer similar financial aid policies, which increases the generalizability of my findings.

4.2 Results

I begin by establishing whether DACA increased educational attainment for likely undocumented youth. I first examine the relationship between likely DACA-eligibility and educational attainment outcomes for each 9th grade cohort separately with an event-study analysis. Panel A of Figure 2 presents event-study estimates where the outcomes is a summary index of educational attainment.[27] Since DACA is expected to have a greater impact on educational attainment for students typically on the margin of high school drop-out decisions, such as low-achievers, I also explore the results separately for those above and below the median of baseline achievement in Panels B and C. Overall, these figures demonstrate compelling evidence in support of parallel trends, as there is little difference in educational attainment across students more and less likely to be DACA-eligible in cohorts expected to graduate high school before DACA's enactment.[28] While there is a slight downward trend in educational attainment for high-achievers, this trend is not statistically different from zero. Moreover, this is likely pushing in the opposite direction of my results that find DACA increased educational attainment and attenuating the estimated treatment effects. Reassuringly, for the low-achieving subgroup most affected by DACA, there is no evidence of a pre-trend. Cohorts exposed to DACA during high school, experienced significant increases in educational attainment,

[27]This summary index measure of educational attainment accounts for multiple inference (Kling, Liebman, & Katz, 2007), and is computed as the equally weighted average of the z-scores of high school completion and enrollment in each grade. 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 those in Figure 2.

[28]The x-axis in Figure 2 denotes the *expected* years of DACA exposure during school based on the year the student was enrolled in 9th grade. 9th grade cohorts to the left of the vertical line were expected to graduate before DACA's enactment. While those to the right of the vertical line were expected to graduate after DACA's enactment.

with the increases being driven purely by low-achieving students. There was not a response for high-achievers who would have likely graduated high school regardless of DACA.

Table 3 presents the difference-in-differences estimates from a pre-post version of Equation 3. In line with the event-study results, DACA increased *expected* 11th-12th grade enrollment, high school completion, and a summary measure of educational attainment for likely undocumented youth.[29] Again, the results are driven by low-achievers. The differences across initial achievement in a summary index of educational attainment is statistically significant, with a p-value of 0.07. Table 3 also reveals larger increases in educational attainment for "higher-impact" subgroups such as Mexicans who are more likely to be undocumented than Hispanics overall, and males who are typically at a higher risk of dropping out of high school than females. While the difference in a summary index of educational attainment for Mexicans is not statistically significant, with a p-value of 0.147, the differences by gender are with a p-value of 0.07.

These estimates suggest that for the average zip-code in Los Angeles, which had an applicant share of 14%, DACA's introduction increased 12th grade enrollment and high school completion by 2.5 and 3.4 p.p., respectively. As another way to contemplate magnitudes, I can consider the effect of increasing the likelihood of DACA-eligibility from 0-100%. As noted above, my measure of likely DACA-eligibility uses geographic variation in the DACA applicant share and will under-estimate DACA-eligibility. Therefore, to get closer to the true intent-to-treat (ITT) impacts of DACA-eligibility, I rescale the coefficients by 69% to account for incomplete take-up. In addition, to better capture the likely DACA-eligibility of a high school aged student, the population of interest, I further re-scale the coefficients by 40% to capture the difference in take-up of DACA for those ages 15-19.[30] Re-scaling the coefficients accordingly suggests that DACA-eligibility

[29] I define enrollment based on *expected* enrollment in that grade, i.e. 10th grade enrollment is defined one year, 11th grade enrollment is defined two years, and 12th grade enrollment is defined three years after 9th grade, respectively. Focusing on *expected* rather than *actual* enrollment in each grade ensures that students within a 9th grade cohort are assigned the same amount of policy exposure. This prevents more years of treatment being assigned to grade repeaters. Reassuringly, results are similar if I focus on enrollment in *actual* grades instead.

[30] Despite accounting for 16% of the foreign-born population between the ages of 15-30, 41% of DACA applicants in Los Angeles county were between the ages of 15-19 (USCIS, 2014). Therefore, to account for this difference in take-up among foreign-born high school youth, I further re-scale the point estimates by 40% ($\frac{1}{2.5}$).

increased 12th grade enrollment and high school completion by 5 and 7 p.p., respectively. These magnitudes compare closely to Kuka et al. (2020) who find on a national sample that DACA increased 12th grade enrollment and high school completion by 2.2 and 6.5 p.p., respectively.[31]

Intermediate Outcomes– An important question is whether these increases in educational attainment were accompanied by increases in effort. On the one hand, additional effort may have been required to meet graduation requirements or could have been exerted if undocumented youth were more motivated to attend college. On the other hand, it is possible that DACA induced students to remain enrolled in school, but additional effort was not exerted if it was not required to meet graduation requirements. Understanding which of these two scenarios occurred is critical for understanding potential mechanisms behind the spillovers on US-born students and also can provide insights into whether DACA increased long-run aspirations of likely undocumented youth.

Figure 3 presents event-study estimates where the outcome is a summary index of academic achievement, computed as the equally weighted average of the z-scores of ELA standardized test performance and GPA.[32] Overall, these figures demonstrate compelling evidence in support of parallel trends. Before DACA's enactment in 2012, there was little difference in achievement across students more and less likely to be DACA-eligible. However, after 2012 likely undocumented students experienced significant increases in achievement. This time, the plots reveal that the increases in achievement were fairly similar across baseline achievement levels. For low-achievers, these increases were either driven by necessity (i.e. working harder was required to meet graduation requirements) or due to increases in motivation during high school. Since high-achievers did not experience a changes in high school enrollment or completion, these increases are unlikely explained by a change in the high school completion decision. Rather, the increases could be related to the fact that college attendance became more appealing for undocumented youth after DACA's introduction.

[31] While it is possible that these enrollment increases could be driven by a reduction in deportations, a rough back of the envelope calculation using deportation data from Transactional Records Access (TRAC) Clearinghouse suggests that this is unlikely to be the case. For likely undocumented youth, the estimated increase in high school graduation of 6 percent yields 159 additional graduates in one cohort. Given the pre-DACA deportation risk for high school students was very low between 0.01% and 2.44%, this would only yield between 0 and 64 fewer deportations.

[32] Appendix Figure A.5 plots event-study estimates where the outcome is an indicator for ELA performance. These results present similar patterns to those in Figure 3.

As detailed above in Section 2.1, the new merit-based financial aid opportunities available through the CDA and legalization under DACA both serve to increase the return to a college degree.

Turning to the difference-in-differences results, estimates presented in Table 4 suggest that for the average zip-code in Los Angeles, which had an applicant share of 14%, DACA's introduction increased ELA performance by 0.077 of a standard deviation and increased cumulative GPA by 0.07 points (or 3 percent given a mean of 2.26). Re-scaling the estimates to reflect the impact of DACA-eligibility on academic achievement using the method previously discussed suggests that DACA-eligibility during high school increased ELA test-scores by 0.15 of a standard deviation and cumulative GPA by 0.14 (or 6 percent given a mean of 2.26). Consistent with the event-studies both high and low-achievers experienced increases in achievement. The increases in GPA were larger and only statistically significant for low-achievers and the increases in ELA achievement were slightly larger for high-achievers. However, neither of these differences are statistically significant. Table 4 reveals that the increases in achievement were fairly similar across gender and country of birth.[33]

One caveat of these findings is that DACA induced low-achievers to complete high school as shown in Table 3. Thus, these results that follow students throughout high school could be biased downwards by compositional changes since more at-risk students are continuing in high school. Reassuringly, Panel B of Appendix Table A.2 suggests that the composition of likely undocumented students enrolled throughout high school largely did not change significantly after DACA. There was only a small increase in the likelihood of being male, but this is consistent with the changes in male enrollment documented above. Moreover, even if there were compositional changes based on unobservables, this is likely to bias me against finding a positive effect of DACA on achievement. The fact that I identify improvements in achievement despite possible compositional changes, provides compelling evidence that effort among undocumented youth likely improved.

Finally, I consider whether DACA impacted behavioral outcomes. Table 4 reveals that

[33] Math scores are not a primary outcome for the aforementioned reasons. Nonetheless, Appendix Table A.3 shows that conditional on the type of math exam, math achievement improved for high-achievers. For other subgroups, the coefficients are positive (suggesting a possible improvement), but these effects are not statistically significant.

DACA had little impact on attendance for any subgroup, but led to increases in the likelihood of being disciplined for low-achievers and males. The difference in discipline across gender is not statistically significant, but the difference across high and low-achievers is statistically significant, with a p-value of 0.02. This increase in disciplinary actions could be driven by the possible compositional changes just discussed or could reflect changes in behavior among students and/or teachers who may have been responding to the possible reductions in consequences of bad behavior due to a legal status change.[34] Regardless of the mechanism, more misbehavior among undocumented youth would, if anything, likely negatively impact their US-born peers.

5 Spillover Effects

5.1 Empirical Strategy

Next, I estimate the spillover effects of DACA on the educational investments of US-born high school students in Los Angeles. To do so, I focus on the 2012 introduction of DACA, wherein the control group consists of US-born students without DACA-eligible peers, and the treatment effect varies across US-born students in the fraction of their peers who were DACA-eligible. Specifically, I estimate the following event-study model on the sample of US-born students:

$$Y_{isc} = \alpha_0 + \sum_{j=2007}^{2014} \alpha_j \mathbb{1}\{j = c\} \times \text{DACAShare}_{sc} + \lambda_1 X_{isc} + \lambda_2 Z_{sc} + \gamma_s + \phi_c + \varepsilon_{isc} \tag{5}$$

where Y_{isc} is an indicator for high school completion or enrollment in each grade 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 9th grade cohort dummies. I control for high school campus, γ_s, fixed effects to account for fixed cross-sectional differences across high school

[34] For instance, before DACA, teachers may have been less likely to report misbehavior among undocumented youth if they wanted to keep undocumented youth away from the legal system (for instance discipline in school is often dealt with by school resource officers, who are law enforcement agents). After DACA, teachers may have been more likely to report misbehavior among undocumented youth if teachers viewed there being a reduction in negative consequences for undocumented youth engaging in the legal system (i.e. now, they can no longer be deported).

campuses, and cohort fixed effects, ϕ_c, to account for trends in high school completion that could affect all students in Los Angeles. X_{isc} includes individual characteristics that include race, gender, gender-race interactions, special education status, and 8th grade ELA test scores.[35] 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 coefficients of interest, α_j, represent the peer effects stemming from the share of one's peers estimated to be DACA-eligible for each 9th grade cohort separately.

For outcomes that vary yearly, I estimate a slightly modified version of Equation 5 on the sample of US-born students as follows:

$$Y_{isctg} = \pi_0 + \sum_{j=2005}^{2016} \pi_j \mathbb{1}\{j = t\} \times \text{DACAShare}_{sgt} + \lambda_1 X_{isc} + \lambda_2 Z_{sc} + \phi_{sg} + \alpha_{tg} + \varepsilon_{isctg} \tag{6}$$

where Y_{isctg} is a yearly achievement or behavioral outcome from grade g in which the student was enrolled during year t. Now I interact the concentration of likely DACA-eligible peers in each student's school during the year t and grade g that the outcome was measured with yearly dummy variables. ϕ_{sg} and α_{tg} are school-grade and year-grade fixed effects, respectively. All other controls are as previously defined. The variables of interest, π_j, trace out the peer effects stemming from the share of one's peers estimated to be DACA-eligible in each year.

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. In support of this assumption, event-study estimates presented in Section 5.2 reveal that US-born students with different concentration of DACA-eligible peers had similar pre-trends. Moreover, Panel A of Appendix Table A.4 demonstrates that US-born students with more DACA-eligible peers are not predicted to have better schooling outcomes, based on observable characteristics,

[35] To be consistent with the direct impact results, I do not control for an FRL indicator in my main specification. Reassuringly, results are robust to including this control variable.

at the time of DACA's introduction. This suggests that any increases in educational attainment after DACA's introduction can be attributed to changes in behavior rather than compositional changes. For the sample of US-born students used to estimate yearly outcomes who remained enrolled *throughout* high school, Panel B of Appendix Table A.4 reveals small declines in predicted high school graduation and baseline performance. These declines are expected as DACA induced low-achieving US-born students to remain in school. If anything, this suggests that students with more undocumented peers are becoming negatively selected overtime, which would lead me to underestimate positive spillovers on yearly achievement outcomes. As a final check of the parallel trends assumption, Appendix Table A.7 demonstrates that the results are robust to the inclusion of time trends interacted with baseline campus demographics. This helps to rule out the possibility that my results are driven by trends introduced by differences in baseline campus demographics.[36]

5.2 Results

To determine whether exposure to DACA-eligible peers increased educational attainment for US-born students, I start by examining the relationship between the share of likely DACA-eligible peers and educational outcomes for each 9th grade cohort separately with an event-study analysis. Panel A of Figure 4 presents event-study estimates for the full sample, where the outcomes is a summary index of educational attainment.[37] Panels B and C explore the results separately for those below and above the median of baseline achievement, allowing me to capture the differences for students with different likelihoods of dropping out of high school. Overall, there is little evidence of a pre-trend, especially for low-achievers who are more likely to be on the margin of high school drop-out decisions. While there is a slight upward trend in educational attainment for high-achievers, this trend is not statistically significant. Reassuringly, this subgroup was not incentivized to complete high school after DACA, as they were already likely to graduate. In contrast, for low-achievers

[36]The one exception is that in terms of educational attainment, 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.

[37]This index is computed as the equally weighted average of the z-scores of high school completion and enrollment in each grade. Appendix Figures A.6 and A.7 show event-study estimates where the outcome is an indicator for 12th grade enrollment and high school completion, respectively. These results demonstrate similar patterns to Figure 4.

there is no evidence of a pre-trend, but an abrupt increase in educational attainment for cohorts exposed to DACA during high school with higher shares of DACA-eligible peers.

The difference-in-differences estimates based on a pre-post version of Equation 5 are presented in Table 5. The results for the full sample are shown in Columns 1-3 and the results broken out by quartiles of baseline achievement are shown in Columns 4-7. For the full sample, I start with a model that only includes 9th grade cohort indicators and campus fixed effects. The estimated effects are very stable as I successively add controls. In line with the event-study results, DACA increased *expected* 11th-12th grade enrollment, high school completion, and a summary index of educational attainment for those with higher shares of likely DACA-eligible peers.[38] Similar to the event-studies, the increases are largest for low-achievers who are more likely to be on the margin of high school drop-out decisions. The difference in a summary index of educational attainment for those in the top and bottom quartile of achievement is marginally significant, with a *p*-value of 0.12.

To contemplate magnitudes, I re-scale the estimates to account for the fact that my measure of peer exposure relies on the DACA applicant share and undercounts exposure to DACA-*eligible* peers. As discussed in more detail above, by accounting for the incomplete take-up of DACA I can recover the spillover effects of DACA *eligibility*, the policy parameter of interest. Specifically, I re-scale the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up for high-school youth. After making these adjustments, for the overall sample, the magnitude of the results indicates that for the average US-born high student exposed to 3.4% DACA-eligible peers, DACA increased the likelihood of 12th grade enrollment by 2 p.p. (or 3 percent) and high school completion by 2 p.p. (or 4 percent).[39] Low achievers at the average campus experienced a 5 p.p. (or 7 percent) and a 3 p.p. (or 9 percent) increase in the likelihood of

[38] I do not estimate a significant relationship for 10th grade enrollment. 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.

[39] The average exposure is computed by starting with the average of DACAShare_{sc}, which was 0.9% and making two adjustments to address undercounting in this measure. I multiply the average of DACAShare_{sc} by 1.44 (computed by $\frac{1}{.69}$) to account for the fact that 69% of eligible high school students applied to DACA and by 2.5 (computed by $\frac{41}{16}$) to account for differences in take-up for high school students. This yields 3.4% DACA-eligible peers on average.

12th grade enrollment and high school completion, respectively.[40] The magnitudes of these spillover effects are roughly half of the magnitude of the direct effects of DACA-eligibility documented above. This compares very closely to Abramitzky et al. (2021) who also find that the spillover effects of a pay reform in Israel that increased the returns to schooling were half of the direct effects.

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. Figure 5 presents event-study estimates where the outcome is a summary index of academic achievement.[41] Estimates for the full sample appear in Panel A, while those for students below and above the median of baseline achievement appear in Panels B and C. For the overall and low-achieving samples, the figures show that there was not a pre-existing trend in outcomes for those with higher and lower shares of undocumented peers before DACA. After DACA's enactment in 2012, those with higher shares of undocumented peers experienced significant improvements in academic performance. The patterns are largely similar for the high-achievers shown in Panel C, however, there does appear to be a small positive pre-trend in outcomes before DACA's enactment for those with higher shares of DACA-eligible peers. While there is still a discrete increase in achievement for those with higher shares of undocumented peers post-DACA in 2014, it is important to acknowledge that the increases for high-achievers could partially be explained by the small pre-existing positive trend.

Consistent with these event-studies, difference-in-differences estimates presented in Table 6 show that academic achievement for US-born students with more undocumented peers improved after DACA's enactment. Starting with a model that only includes campus-grade and year-grade fixed effects in Column 1, I successively add controls in Columns 2-3. The results are largely stable to the choice of specification. In the fully specified model, the results suggest that for the average US-born student, who was exposed to 3.4% DACA-eligible peers, DACA's introduction increased

[40] I also estimate a categorical specification using bins for different percentages of likely DACA-eligible peers (0-1.4%, 1.5-3.7%, 3.8-5.5%, 5.6-11%). Appendix Table A.6 demonstrate that the spillover effects increase with the share of likely eligible peers. For students in schools with 5.6-11% likely DACA-eligible peers, 12th grade enrollment increases by 3 p.p. (or 4 percent) and high school graduation increases by 4 p.p. (or 7 percent).

[41] This summary index is computed as the equally weighted average of the z-scores of ELA standardized test performance and GPA. Appendix Figures A.8 and A.9 plot event-study estimates where the outcome is an indicator for ELA performance and cumulative GPA respectively. These results present similar patterns to Figure 5.

ELA performance by 0.06 of a standard deviation and cumulative GPA by 0.05 points (off of a mean of 2.33). In addition, results using a summary index of academic achievement indicate an improvement in achievement. In line with the event studies, Columns 4-7 demonstrate that the increases in achievement occurred for those across the achievement distribution. The increases in cumulative GPA are slightly larger for lower-achievers and the increases in ELA are slightly larger for high-achievers. Both of these differences are statistically significant, with a *p*-value of roughly 0.10. These were the same patterns observed for the direct impacts presented in Section 4.2.[42]

Again, since DACA's spillovers induced US-born low-achievers to complete high school as shown in Table 5, it is important to acknowledge that these achievement estimates could be biased downwards due to compositional changes. Indeed, as previously noted, Panel B of Appendix Table A.4 shows that for those enrolled throughout high school there were declines in predicted high school graduation and baseline performance. The fact that I identify improvements in achievement despite compositional changes, provides compelling evidence that US-born students' effort likely improved in response to the increases in educational investments of their undocumented peers.

Turning to behavioral outcomes, I find that DACA led to little change in the attendance rates or discipline for US-born students with higher concentrations of likely DACA-eligible peers. While there are statistically significant increases in discipline for high-achievers, the magnitude of these effects for the average student are rather small and correspond to an increase in discipline by about 0.5 of a percentage point. The increases in discipline for this group could be related to the increases in discipline among undocumented youth post-DACA or to the aforementioned changes in the sample composition of US-born student who remained enrolled in high school after DACA.

Heterogeneous Responses – I next stratify the sample by gender and race. Table 7 focuses on educational attainment for US-born students across these subgroups. The positive spillover effects on high school enrollment are largest for Black, Hispanic and males. In terms of high school completion, the positive spillover effects are driven by Black students. Overall these differences are

[42] Appendix Table A.5 demonstrates that conditional on the version of the exam, math scores for those with higher DACA exposure also increased. The increases are driven by the same subgroups that drove the increases in ELA scores.

not statistically significant from one another, with the exception that the difference in educational attainment across Black and Hispanics is statistically significant with a *p*-value of 0.02. Table 8 focuses on heterogeneity among US-born students for the yearly outcomes. Hispanics experienced the largest increases in ELA performance and GPA. Moreover, based on a summary index of academic achievement the difference between Hispanic and white students is statistically significant, with a *p*-value of 0.02. Across gender, I estimate similar increases in ELA achievement and GPA.

5.3 Discussion

Given the large peer effects documented in the previous section, it is important to consider possible mechanisms behind these results. US-born students may have been positively affected by direct peer-to-peer influences: increased effort among DACA-eligible students may have inspired their US-born peers to study harder. However, DACA could have led to changes in classroom or school-wide dynamics. For instance, improvements in undocumented youths' outcomes may have freed up teachers' and administrators' time for other instructional improvements. In addition, campuses with higher concentrations of DACA-eligible youth may have trained guidance counselors to better understand the process of college admissions for DACA recipients. These additional investments targeted towards undocumented youth could have positively affected US-born students as well.[43]

To investigate these possibilities, I start by exploring whether the spillovers are driven by closer contacts. If peer-to-peer interactions are responsible, the spillover effects should be driven by peers who spend more time together. However, if school-level changes were responsible, there should be little difference in spillovers driven by those with closer and more distant relationships. To define closer contacts, I focus on students who attended the same middle school since they are likely to have closer ties and longer-lasting friendships. Specifically, for each student, I determine what fraction of their high school peers were likely undocumented and attended their middle school,

[43] Compositional changes of undocumented youth could have also played a role. For instance, higher shares of lower-achieving undocumented peers could have disrupted learning if these peers were more disruptive in the classroom (e.g. Carrell, Hoekstra, & Kuka, 2018). In addition, lower-achieving US-born students may have been benefitted from having more students with similar ability in classrooms, for instance teachers could have tailored their instruction in a way that benefitted low-achieving students (e.g. Banerjee et al., 2016). However, because I find positive spillovers for all US-born students (even higher achievers in terms of academic achievement), both of these possibilities are unlikely.

as well as the fraction of their high school peers who were likely undocumented and who attended a different middle school and denote these shares by $\text{DACACloseShare}_{sc}$ and $\text{DACADistantShare}_{sc}$, respectively.[44] Then, I estimate difference-in-differences models where the peer exposure treatment variable enters in separately for the share of close and distant DACA-eligible peers. The results in Table 9 demonstrate that closer contacts drive the positive spillover effects on US born students outcomes. Moreover, this difference is statistically significant. As those from the same middle school are likely to be closer friends, this provides suggestive evidence consistent with spillovers being primarily driven by peer-to-peer interactions, rather than school-level changes.

Moreover, the heterogeneous results suggest that the direct and spillover effects occurred for students with similar pre-determined characteristics. This provides further evidence consistent with peer-to-peer interactions, as several studies demonstrate that youth forge the closest bonds with those of similar characteristics (e.g. Moody, 2001). Specifically, in terms of educational attainment, the direct and spillover effects of DACA were driven by males and low-achievers. Similarly, the increases in GPA and ELA for likely undocumented youth and US-born students are driven by the same subgroups. The increases in GPA are largest for low-achievers, while the increases in ELA performance are largest for high achievers. Among US-born students, the increases in educational investments were largest for Hispanic and Black students, and had little impact on white students.

To more directly test whether DACA led to school-wide changes, I turn to investigating whether DACA led to changes in teacher turnover. If classroom conditions improved in response to DACA, this could have made teaching in the classroom more enjoyable which could have lead to decreases in teacher turnover. Appendix Figure A.10 presents event-study estimates where the outcome is the fraction of teachers who left a high school campus in a given year. Due to data limitations I can only track teacher turnover between 2013 and 2017. Nonetheless, this plot provides suggestive evidence that there was not a differential trend in teacher turnover in high

[44] More specifically, using a slightly modified version of Equation 2, I take the share of Hispanic foreign-born youth (who arrived to the US by age 9) in each student's campus as of 9th grade who attended their middle school or a different middle school, and rescale both of these measures by the likelihood of being undocumented.

school campuses with different concentrations of undocumented students. This provides further suggestive evidence inconsistent with school-wide dynamics driving the spillover effects.

Overall, these results suggest that classroom peer spillovers are likely responsible for the findings, however, it is important to acknowledge that my results are also consistent with improvements in family and neighborhood inputs. For instance, if older-siblings became DACA-eligible and experienced improvements in labor market outcomes (e.g. Pope, 2016a; Amuedo-Dorantes & Antman, 2017) this could have led to a positive income shock for their younger US-born siblings. Moreover, an abundance of research shows the importance of neighborhood quality for student outcomes (e.g. Chetty & Hendren, 2018; Ang, 2020). The enactment of DACA could have improved neighborhood quality if, for example, poverty rates declined due to increased employment. Such improvements could have positively impacted US-born students living in these communities.

Since I am not able to link siblings in my data, I cannot rule out the importance of sibling spillovers entirely. However, as will be discussed in more detail in Online Appendix B given the small share of students likely to have likely DACA-eligible siblings using the ACS and the fact that US-born students who are less likely to be growing up in immigrant families (i.e. those who speak English at home) also experienced positive spillovers due to DACA, I view it as unlikely that family spillovers are the primary mechanism. In addition, as will be shown in Online Appendix B there are positive spillovers driven by classroom and neighborhood peers, but in a horse-race specification the spillovers driven by classroom peers are significantly larger in magnitude. Taken together, these results point to the positive spillover effects being primarily driven by changes occurring at schools.

Finally, it is important to note that while the magnitudes of the effects are large, the results are broadly consistent with the existing literature on peer effects in education. Comparing my estimates to Abramitzky et al. (2021) who focus on spillovers stemming from a pay-reform in Israel that increased the returns to schooling for Kibbutzm, I find the spillover effects due to DACA are larger in absolute terms but very similar in relative terms when compared to the direct effects. In both contexts, the spillovers are about half of the direct effects. The larger impacts of DACA could be explained by differences in context. Abramitzky et al. (2021) focus on all Israeli schools, while I am focused on one large urban school district in the US, where educational lags behind the national average. In the case of Los Angeles, with a significant amount of marginal high school graduates, it is perhaps unsurprising that a policy change that incentivizes high school completion would have such a large impact. Furthermore, prior research demonstrates that LAUSD students respond strongly to established norms regarding educational investments (Bursztyn et al., 2018).[45] Hence, if DACA was effective at changing peer cultures from one that stigmatizes effort to one that rewards it, this could have had particularly profound impacts on pre-college human capital investments.

Additionally, I can place the magnitude of these effects by comparing to the peer effects literature that studies how ability and peer quality affects educational attainment. For example, Carrell et al. (2018) find that increasing the share of disruptive peers during elementary school by 5% decreases college enrollment by 1.4 p.p. and Gazze, Persico, and Spirovska (2021) find that increasing the share of lead exposed (who are more disruptive) peers during elementary and middle school decreases high school completion by 0.70 p.p.. Using the estimates presented in Table 5, I find that increasing the share of DACA-eligible peers during high school by 5% increases high school completion by 3 p.p. after DACA's enactment. Relative to these other studies, DACA's spillovers are slightly larger. This could be driven by the fact that high school peers rather than peers from earlier grades may be more influential for educational attainment decisions. For instance, high school peers are influencing educational attainment decisions contemporaneously and may play an especially influential role on peer decision making during a time when conforming to social norms may be particularly important. On the other hand, the magnitude of my effects are smaller than studies that focus on spillovers from friends or siblings. For example, Gadgete-Miranda (2022) find that having on extra friend who graduates from high school increases the likelihood of high school completion by roughly 10 p.p. and Goodman, Hurwitz, Smith, and Fox (2015) find that if an older sibling enrolls in college this increases the likelihood of college attendance for younger

[45] For example, Bursztyn et al. (2018) demonstrate that in Los Angeles schools, if the decision to sign-up for an SAT preparatory course is made public, sign-ups declined from 80 to 53 percent. The authors highlight that this stark reduction is consistent with students concerned with signaling a social type within a context where effort is stigmatized.

siblings by 15-20 p.p.. Given the strong effects that very close peers have on educational attainment decisions, it is plausible that DACA which significantly changed the educational investments of those in *existing* friendship networks would yield such large spillovers.

Turning to studies that focus on spillovers on academic achievement, I find effects on test-scores that are similar in magnitude to those obtained by Imberman, Kugler, and Sacerdote (2012). For example, the authors find that a 5 p.p. increase in high-achieving Katrina evacuees increases incumbent math test scores by 6% of a standard deviation in the bottom quartile and by 5% in the top quartile, and increases ELA test scores by 8% of a standard deviation in the bottom quartile and by 9% in the third quartile. Using the estimates presented in Table 6, I find that increasing the share of DACA-eligible peers during high school by 5% increases ELA test scores by 5% of a standard deviation in the bottom quartile and by 10% in the top quartile. On the other hand, my estimates are somewhat larger than those obtained by Carrell and Hoekstra (2010) who find that increasing the share of disruptive peers by 5% decreases test-scores by 3% of a standard deviation during elementary school and Lavy, Paserman, and Schlosser (2012) who find that a 5% increase in the proportion of grade repeaters (who are lower performing students on average) leads to a decrease in test-scores of 6% of a standard deviation during high school. For the overall population, increasing the DACA-eligible peers during high school by 5% leads to an increase in ELA test scores by 9% of standard deviation. While DACA's spillover effects on achievement are on the larger end, they are still broadly consistent with the current literature, especially with studies such as Imberman et al. (2012) which focus on large changes in peer achievement composition.

5.4 Robustness

The results presented so far indicate that DACA increased educational investments of those directly eligible and their US-born peers. A critical assumption of this analysis is that there were no contemporaneous shocks that differentially impacted campuses in a way that correlates with the share of undocumented students. In Online Appendix C.2, I consider several other education policy changes related to school discipline, graduation requirements, and ELL participation. Reassuringly, I find several pieces of evidence that help me rule out these other education policies as alternative explanations for my results. However, as previously noted in Section 2 undocumented youth became eligible for state financial aid in California through the CDA around this time. Hence, as previously noted the results should be interpreted as the impacts of DACA and related policies like the CDA that increased the returns to schooling for undocumented youth around this time.[46]

Another potential concern is that I do not observe undocumented status, and instead use a proxy to determine likely DACA-eligibility. Most of the prior literature on DACA 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). Given the high take-up rate of DACA in Los Angeles, my measure that depends on the geographic concentration of DACA applicants is likely to introduce less measurement error than the most commonly used alternative, the share of non-citizen immigrants. This is because 69% of eligible high school youth applied to DACA in Los Angeles, while Kuka et al. (2020) estimate that 55 % of non-citizens are undocumented. Reassuringly, Online Appendix D demonstrates that my results are robust to using several different proxies that approximate the likely DACA-eligible population.

Finally, one may be concerned that DACA could have led to differential attrition or campus switching, perhaps with students in campuses with higher shares of DACA-eligible youth being more likely to leave LAUSD or switch to campuses with fewer DACA-eligible students if they viewed DACA as leading to an influx of low-performing peers. As previously noted, my data does not allow me to follow students if they leave LAUSD. However, higher-achieving students serve as a useful placebo to check whether DACA increased moves out of the district. This is because if high-achievers leave the data it is more likely that they are enrolling in another district rather than dropping out of high school. Reassuringly, Table 5 demonstrates that this group experienced no change in the likelihood of graduating from high school in LAUSD. Moreover, even if DACA did

[46]As will be further detailed in Online Appendix C.1, several pieces of evidence point to DACA being the primary driver of my results. First, previous literature finds limited evidence that state financial aid eligibility in California changed college enrollment decisions (Bettinger et al., 2019). Second, the response to DACA in California was similar to other states that did not change financial aid policy using the ACS. Finally, there was no changes to the educational attainment for a small subset of likely undocumented youth eligible for the CDA, but not for DACA, during this period.

lead US-born students to leave LAUSD, this is likely to bias me against finding a positive spillover effect of DACA given attrition out of LAUSD in response to DACA is more likely to occur for students with more family resources (who would be expected to have better outcomes).

While excessive campus switching within LAUSD could attenuate estimates on educational attainment, it does not pose a threat to identification since I assign DACA peer exposure based on one's 9th grade campus before potentially endogenous campus switching can occur.[47] Nonetheless, I investigate whether more DACA peer exposure during 9th grade increased the likelihood of switching campuses in later grades in Appendix Table A.9. These results suggest that DACA's introduction increased 12th grade campus switching for US-born students with higher shares of DACA-eligible peers and that the increases are driven by transitions to continuation campuses. As continuation campuses primarily serve students at risk of high school drop-out, this pattern is consistent with compositional changes, as more "at-risk" students continued in high school, rather than a behavioral response to switch campuses to avoid DACA-eligible peers.

6 Conclusion

In this paper, I present evidence on how policies that increase the returns to schooling for certain subgroups can have important positive spillover effects. 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 these changes in educational investments of undocumented youth due to DACA had any impact on their peers' educational investments. To estimate whether DACA had spillovers on US-born students, I leverage variation in the concentration of DACA-eligible youth across high schools and compare the educational outcomes of US-born students with higher and lower concentrations of DACA-eligible peers before and after DACA's enactment.

[47] For yearly outcomes I assign DACA peer exposure during the year that the outcome is measured. Reassuringly, the results are similar when DACA peer exposure is assigned based on 9th grade enrollment before any differential attrition can occur. This suggests that the yearly estimates are unlikely to be biased and are available upon request.

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 12 percent, ELA achievement by 0.15 standard deviations, and GPA by 0.14 p.p. (or 6 percent given a mean of 2.26). Among US-born students at the average campus, where approximately 3.4 percent of students were likely to be DACA-eligible, 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 educational investments. My context is unique within the literature, as policy-driven changes in educational investments are rarely observed among *existing* peer networks. Moreover, I focus on a critical time during adolescence when conforming to social norms may be especially important and at a time right before critical human capital decisions are made.

In addition, these results have important policy implications for the DACA program itself. Previous studies on DACA have focused exclusively on the direct impacts DACA. 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.

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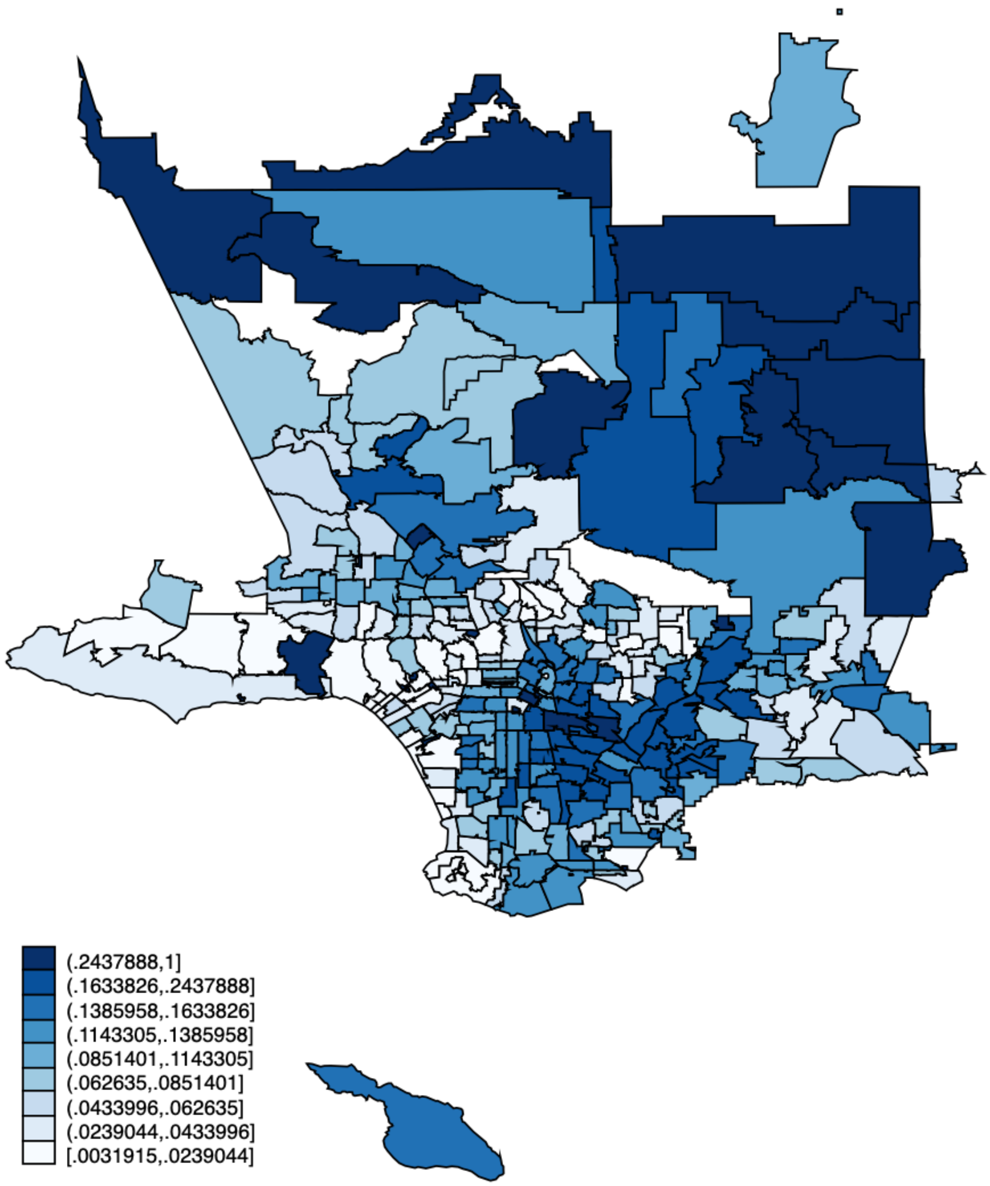
Figures/Tables

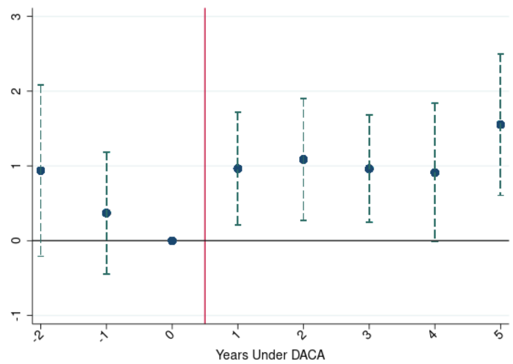
Figure 1: Fraction of Foreign-Born Population Ages who applied to DACA, 2012-2013



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.

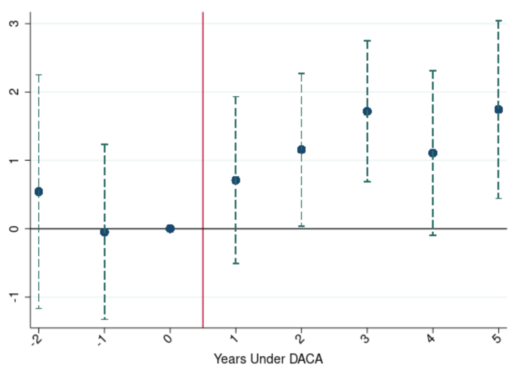
Figure 2: Direct Impact of DACA on a Summary Index of Educational Attainment

(a) All

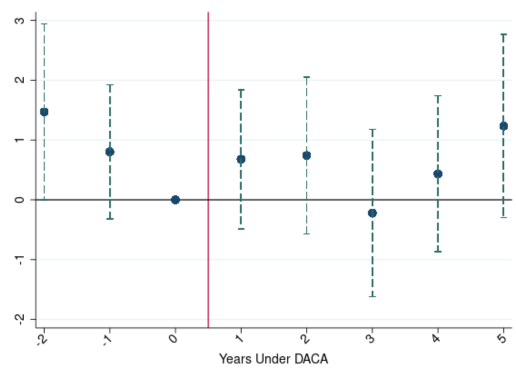


<u>Baseline Achievement Distribution</u>

(b) Bottom 50 Percentile



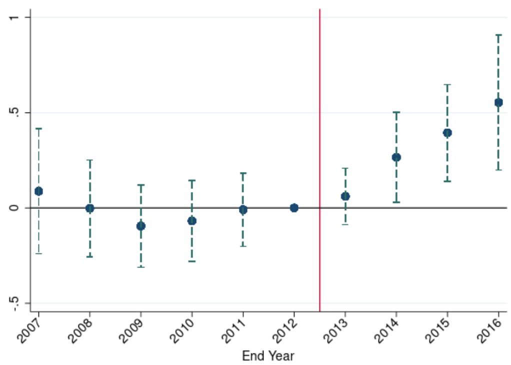
(c) Top 50 Percentile



Note: These figures plot coefficients from Equation 3 and 95% confidence intervals. The dependent variable is a summary index based on *expected* enrollment in each grade 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 before DACA was implemented (i.e. 2011-12 school year). The sample includes Mexican immigrants who arrived to the US by age 9 in 9th grade cohorts between 2006-07 to 2013-14. 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 full set of controls. Standard errors are clustered by zip-code of residence.

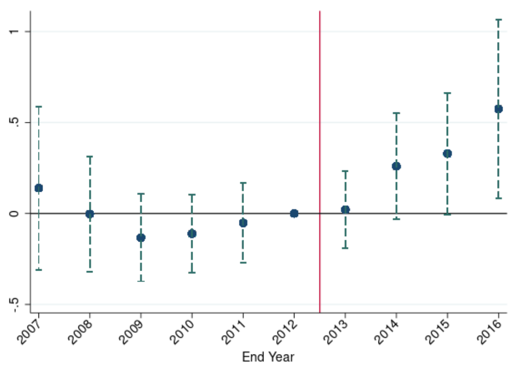
Figure 3: Direct Impact of DACA on a Summary Index of Academic Performance

(a) All

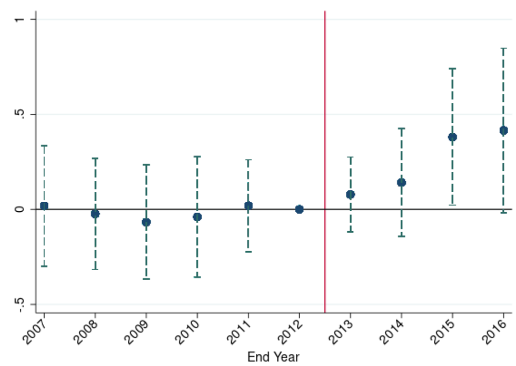


Baseline Achievement Distribution

(b) Bottom 50 Percentile



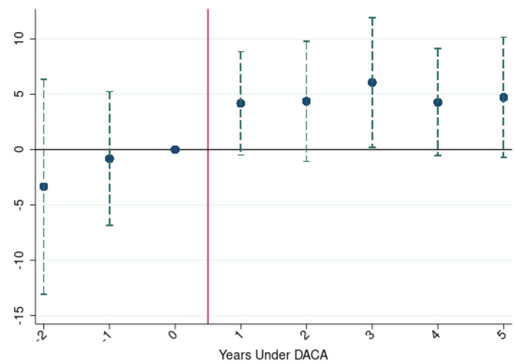
(c) Top 50 Percentile



Note: These figures plot coefficients from Equation 4 and 95% confidence intervals. The dependent variable is a summary index based on GPA and ELA standardized exam performance. The sample includes Mexican immigrants who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 2011-12 school year is omitted, so estimates are relative to that pre-policy year. See Table 4 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.

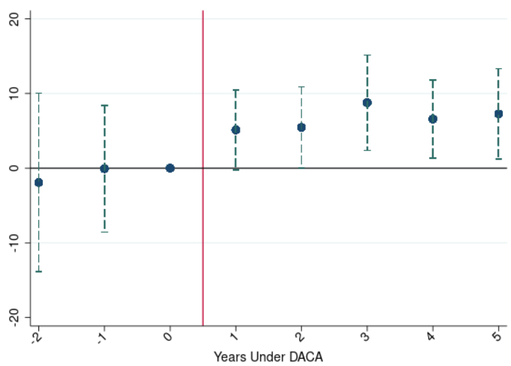
Figure 4: Spillover Effects of DACA on Summary Index of Educational Attainment

(a) All

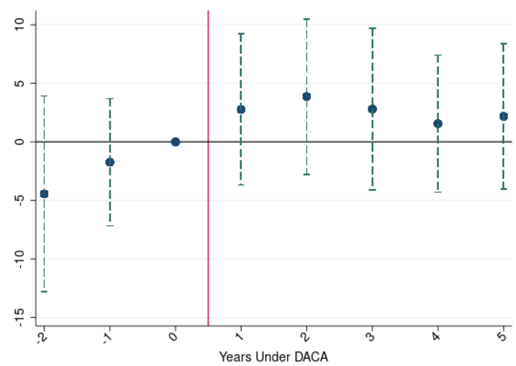


<u>Baseline Achievement Distribution</u>

(b) Bottom 50 Percentile



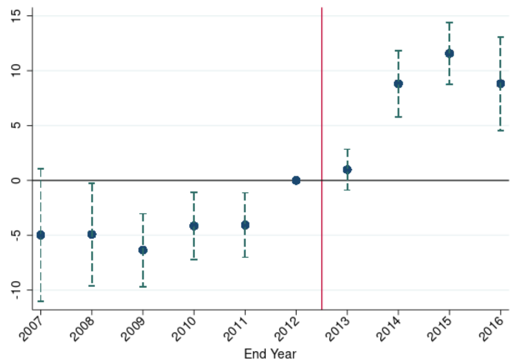
(c) Top 50 Percentile



Note: These figures plot coefficients from Equation 5 and 95% confidence intervals. The dependent variable is a summary index based on *expected* enrollment in each grade 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. 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 5 for more detail on the full set of controls. Standard errors are clustered at the high school campus level.

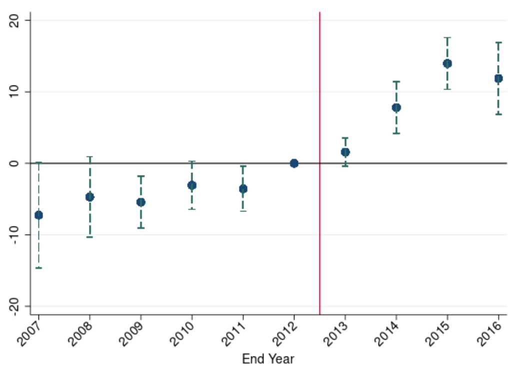
Figure 5: Spillover Effects of DACA on Summary Index of Academic Performance

(a) All

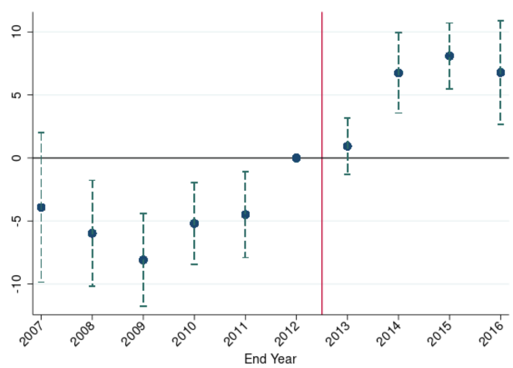


Baseline Achievement Distribution

(b) Bottom 50 Percentile



(c) Top 50 Percentile



Note: These figures plot coefficients from Equation 6 and 95% confidence intervals. 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 2006-07 to 2013-14. 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 6 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	
	All	US-Born	All	Hispanic	Hispanic	Mexican
	(1)	(2)	(3)	(4)	(5)	(6)
DACA Apps in Zip ($ShareEligible_z$)	0.131	0.131	0.127	0.138	0.139	0.143
<u>Demographics (G9)</u>						
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
<u>Baseline Achievement</u>						
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
<u>Outcomes</u>						
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 2006-07 and 2013-14 enrolled in LAUSD. 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. Expected enrollment in 10th grade is defined as enrollment one year after 9th grade, expected enrollment in 11th grade is defined as enrollment two years after 9th grade enrollment, and expected enrollment in 12th grade is defined as enrollment three years after 9th grade enrollment. 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 (DACAShare)	0.009	0.004	0.009	0.016
Re-Scaled	0.032	0.014	0.032	0.058
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 2006-07 and 2013-14 enrolled in LAUSD. 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. Expected enrollment in 10th grade is defined as enrollment one year after 9th grade, expected enrollment in 11th grade is defined as enrollment two years after 9th grade enrollment, and expected enrollment in 12th grade is defined as enrollment three years after 9th grade enrollment. 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. The second row shows the share of DACA-eligible peers ($DACAShare_{sc}$) re-scaled to account for the undercounting in this measure. Specifically, I multiply the average of $DACAShare_{sc}$ by 1.44 (computed by $\frac{1}{.69}$) to account for the fact that 69% of eligible high school students applied to DACA and by 2.5 (computed by $\frac{41}{16}$) to account for differences in take-up for high school students.

Table 3: The Direct Impact of DACA on Educational Attainment, Foreign-born Hispanics

	(1)	(2)	(3)	(4)	(5)	(6)
					8th Grade ELA Score	
	All	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)
	[0.007]	[0.015]	[0.000]	[0.010]	[0.038]	[-0.029]
	{0.015}	{0.030}	{0.001}	{0.019}	{0.076}	{-0.058}
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)
	[0.022]	[0.037]	[0.008]	[0.030]	[0.063]	[-0.019]
	{0.044}	{0.073}	{0.015}	{0.059}	{0.124}	{-0.038}
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)
	[0.025]	[0.034]	[-0.013]	[0.046]	[0.039]	[0.005]
	{0.049}	{0.068}	{-0.026}	{0.091}	{0.077}	{0.009}
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)
	[0.034]	[0.040]	[0.003]	[0.053]	[0.055]	[0.006]
	{0.068}	{0.079}	{0.007}	{0.106}	{0.109}	{0.012}
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)
	[0.070]	[0.094]	[-0.002]	[0.114]	[0.121]	[-0.003]
	{0.138}	{0.187}	{-0.005}	{0.227}	{0.241}	{-0.007}
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 graduation, as well as a summary index based on the outcomes in Panels A-D. Within each panel, each column reports the coefficient from a separate regression of a pre-post version of Equation 3, where ShareEligible$_z$ is interacted with an indicator for *expected* enrollment in high school after 2012. This indicator variable, $Exposed_c$, equals one for cohorts entering 9th grade after 2009-10, and 0 otherwise. The sample for these regressions are foreign-born Hispanics 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 the average zip-code, which had a DACA applicant share of 14 percent is shown in brackets, and is defined as the coefficient multiplied by 0.14. The impact of DACA-eligibility is shown in curly brackets, and is the coefficient multiplied by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up for high school youth. Standard errors in parentheses are clustered at the residence zip-code level. *p<0.10, ** p<0.05, *** p<0.01.

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

	(1)	(2)	(3)	(4)	(5)	(6)
					8th Grade ELA Score	
	All	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)
	[-0.002]	[-0.003]	[0.005]	[-0.010]	[-0.005]	[0.003]
	{-0.004}	{-0.006}	{0.011}	{-0.020}	{-0.011}	{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)
	[0.014]	[0.014]	[0.010]	[0.019]	[0.024]	[0.006]
	{0.029}	{0.028}	{0.020}	{0.038}	{0.049}	{0.013}
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)
	[0.070]	[0.082]	[0.109]	[0.045]	[0.101]	[0.045]
	{0.140}	{0.163}	{0.217}	{0.089}	{0.201}	{0.089}
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)
	[0.077]	[0.073]	[0.085]	[0.095]	[0.062]	[0.125]
	{0.153}	{0.145}	{0.170}	{0.189}	{0.122}	{0.249}
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)
	[0.116]	[0.121]	[0.146]	[0.102]	[0.128]	[0.112]
	{0.231}	{0.242}	{0.292}	{0.204}	{0.255}	{0.223}
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, as well as a summary index based on the outcomes in Panels C-D. Within each panel, each column reports the coefficient from a separate regression of a pre-post version of Equation 4, where ShareEligible$_z$ is interacted with a post-policy indicator that equals 1 if the outcome was measured after 2012, and 0 otherwise. The sample is restricted to the 4 years after 9th grade, except for Panels D-E focus which focuses on the 3 years after 9th grade. 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 the average zip-code, which had a DACA applicant share of 14 percent is shown in brackets, and is defined as the coefficient multiplied by 0.14. The impact of DACA-eligibility is shown in curly brackets, and is the coefficient multiplied by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up for high school youth. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01

Table 5: The Effect of DACA on Enrollment and High School Graduation, US-Born Students

				8th Grade ELA Test Score Quartiles			
		All Students		(≤ 25)	(25 - 50)	(50 - 75)	≥ 75
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Enrolled in 10th Grade							
DACAShare*Exposed	0.957	1.005	0.762	1.238	0.767	0.357	0.534
	(0.761)	(0.749)	(0.737)	(0.901)	(0.852)	(0.790)	(0.875)
	[0.009]	[0.009]	[0.007]	[0.012]	[0.007]	[0.003]	[0.005]
Mean (Y)	0.907	0.907	0.907	0.887	0.909	0.916	0.916
Panel B: Enrolled in 11th Grade							
DACAShare*Exposed	1.757**	1.934**	1.901**	2.511**	1.668**	2.280**	1.020
	(0.813)	(0.794)	(0.818)	(1.049)	(0.836)	(1.038)	(1.101)
	[0.017]	[0.018]	[0.018]	[0.024]	[0.016]	[0.021]	[0.010]
Mean (Y)	0.848	0.848	0.848	0.799	0.844	0.868	0.880
Panel C: Enrolled in 12th Grade							
DACAShare*Exposed	2.486**	2.707***	2.625***	4.821***	2.286**	1.929*	1.778
	(0.982)	(0.971)	(0.928)	(1.515)	(1.030)	(1.001)	(1.126)
	[0.023]	[0.025]	[0.025]	[0.045]	[0.021]	[0.018]	[0.017]
Mean (Y)	0.771	0.771	0.771	0.673	0.763	0.809	0.841
Panel D: Graduated from High School							
DACAShare*Exposed	2.297*	2.610**	2.418**	3.152**	2.623**	0.887	2.058
	(1.229)	(1.131)	(1.078)	(1.476)	(1.214)	(1.299)	(1.299)
	[0.022]	[0.025]	[0.023]	[0.030]	[0.025]	[0.008]	[0.019]
Mean (Y)	0.576	0.576	0.576	0.341	0.536	0.666	0.764
Panel E: Summary Index							
DACAShare*Exposed	5.608**	6.142***	5.882***	8.785***	5.665***	4.260*	4.184
	(2.240)	(2.175)	(2.065)	(3.026)	(1.906)	(2.444)	(2.696)
	[0.053]	[0.058]	[0.055]	[0.082]	[0.053]	[0.040]	[0.039]
N	238,781	238,781	238,781	60,442	58,528	61,039	58,772
Controls							
Demographics		X	X	X	X	X	X
8th Grade Std Test (ELA)		X	X	X	X	X	X
Campus-Cohort Demographics			X	X	X	X	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 the coefficient from a separate regression of a pre-post version of Equation 5, where $DACAShare_{sc}$ is interacted with an indicator for *expected* enrollment in high school after 2012. This indicator variable, $Exposed_c$, equals one for cohorts entering 9th grade after 2009-10, and 0 otherwise. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. All specifications include 9th grade cohort and campus fixed effects. 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 size for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets. This is defined by first re-scaling the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth, and then multiplying the re-scaled coefficient by 0.034. Standard errors in parentheses are clustered at the high school campus level. *$p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 6: The Effect of DACA on Yearly Outcomes, US-Born Students

	All Students			8th Grade ELA Test Score Quartiles (≤ 25)	(25 - 50)	(50 - 75)	≥ 75
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Yearly Attendance Rate (Grades 9-12)							
DACAShare*Post	0.217	0.233	0.207	0.315	0.119	0.237	0.152
	(0.175)	(0.166)	(0.168)	(0.224)	(0.223)	(0.210)	(0.145)
	[0.002]	[0.002]	[0.002]	[0.003]	[0.001]	[0.002]	[0.001]
Mean (Y)	0.933	0.933	0.933	0.899	0.927	0.943	0.958
Observations	798,534	798,534	798,534	187,657	194,126	209,905	206,846
Panel B: Ever Disciplined (Grades 9-12)							
DACAShare*Post	0.329	0.304	0.264	0.530	0.754**	0.705***	0.380**
	(0.259)	(0.248)	(0.253)	(0.351)	(0.291)	(0.227)	(0.179)
	[0.003]	[0.003]	[0.003]	[0.005]	[0.007]	[0.007]	[0.004]
Mean (Y)	0.0386	0.0386	0.0386	0.0664	0.0422	0.0306	0.0172
Observations	841,929	841,929	841,929	203,040	205,793	219,330	213,766
Panel C: Cumulative GPA (Grades 9-12)							
DACAShare*Post	4.170***	4.616***	4.572***	6.830***	5.955***	3.038*	3.584**
	(1.355)	(1.238)	(1.219)	(1.465)	(1.388)	(1.727)	(1.600)
	[0.041]	[0.045]	[0.045]	[0.067]	[0.058]	[0.030]	[0.035]
Mean (Y)	2.325	2.325	2.325	1.639	2.050	2.465	3.058
Observations	798,399	798,399	798,399	186016	194656	210514	207213
Panel D: Standardized ELA Performance (Grades 9-11)							
DACAShare*Post	4.977***	6.469***	6.539***	3.923***	7.333***	5.686***	7.727***
	(1.751)	(1.280)	(1.302)	(1.380)	(1.714)	(1.689)	(1.579)
	[0.049]	[0.063]	[0.064]	[0.038]	[0.072]	[0.056]	[0.076]
Mean (Y)	0.0664	0.0664	0.0664	-0.880	-0.359	0.205	1.082
Observations	490,051	490,051	490,051	107,056	119,095	132,225	131,675
Panel E: Summary Achievement Index (Grades 9-11)							
DACAShare*Post	7.903***	8.335***	8.316***	5.993***	7.502***	5.148***	6.007***
	(1.368)	(1.165)	(1.134)	(1.201)	(1.045)	(1.358)	(1.289)
	[0.077]	[0.082]	[0.081]	[0.079]	[0.087]	[0.060]	[0.073]
Observations	631,098	631,098	631,098	191,444	198,040	212,904	208,521
Controls							
Demographics		X	X	X	X	X	X
8th Grade Std Test (ELA)		X	X	X	X	X	X
Campus-Cohort Demographics			X	X	X	X	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 the coefficient from a separate regression of a pre-post version of Equation 6, where DACAShare$_{sc}$ is interacted with a post-policy indicator that equals 1 if the outcome was measured after 2012, and 0 otherwise. See Table 5 for more detail on the sample and the full set of controls. Panels A-C focus on yearly outcomes within 4 years of 9th grade enrollment. Panels D-E focus on yearly outcomes within 3 years of 9th grade enrollment. 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 size for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets. This is defined by first re-scaling the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth, and then multiplying the re-scaled coefficient by 0.034. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table 7: The Heterogenous Effects of DACA on School Attendance and High School Completion, US-born students

	All	Black	Hispanic	White	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Enrolled in Expected 10th Grade						
DACAShare*Exposed	0.762	2.329	0.821	2.457	0.752	0.759
	(0.737)	(2.032)	(0.776)	(3.359)	(0.823)	(0.705)
	[0.007]	[0.022]	[0.008]	[0.023]	[0.007]	[0.007]
Mean (Y)	0.907	0.835	0.919	0.867	0.905	0.909
Panel B: Enrolled in Expected 11th Grade						
DACAShare*Exposed	1.901**	4.248**	1.577*	5.070	1.373	2.380***
	(0.818)	(2.089)	(0.821)	(3.999)	(1.047)	(0.700)
	[0.018]	[0.040]	[0.015]	[0.048]	[0.013]	[0.022]
Mean (Y)	0.848	0.737	0.864	0.798	0.846	0.849
Panel C: Enrolled in Expected 12th Grade						
DACAShare*Exposed	2.625***	5.729***	2.031**	4.612	2.019**	3.202***
	(0.928)	(1.812)	(0.957)	(4.648)	(1.004)	(1.035)
	[0.025]	[0.054]	[0.019]	[0.043]	[0.019]	[0.030]
Mean (Y)	0.771	0.646	0.787	0.722	0.777	0.765
Panel D: Graduated from High School						
DACAShare*Exposed	2.418**	4.636**	1.063	3.653	2.326**	2.502**
	(1.078)	(2.002)	(1.110)	(4.741)	(1.162)	(1.101)
	[0.023]	[0.044]	[0.010]	[0.034]	[0.022]	[0.024]
Mean (Y)	0.576	0.442	0.579	0.618	0.621	0.532
Panel E: Summary Index						
DACAShare*Exposed	5.882***	12.58***	4.060*	12.14	4.901**	6.813***
	(2.065)	(3.885)	(2.058)	(10.28)	(2.294)	(2.108)
	[0.055]	[0.118]	[0.038]	[0.114]	[0.046]	[0.064]
N	238,781	24,689	186,570	15,265	117,085	121,696

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 from a pre-post version of Equation 5. See Table 5 for more details on the specification. 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 5 for more detail on the full set of controls. The effect size for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets. This is defined by first re-scaling the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth, and then multiplying the re-scaled coefficient by 0.034. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table 8: The Heterogenous Effects of DACA on Yearly Outcomes US-born students

	All	Black	Hispanic	White	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Yearly Attendance Rate (Grades 9-12)						
DACAShare*Post	0.207	0.530	0.183	-0.433	0.236	0.176
	(0.168)	(0.370)	(0.181)	(0.446)	(0.182)	(0.183)
	[0.002]	[0.005]	[0.002]	[-0.004]	[0.002]	[0.002]
Mean (Y)	0.933	0.914	0.932	0.941	0.932	0.934
Observations	798,534	72,414	634,081	48,934	392,147	406,387
Panel B: Ever Disciplined (Grades 9-12)						
DACAShare*Post	0.264	0.453	0.285	-0.422	0.255	0.259
	(0.253)	(0.763)	(0.231)	(0.572)	(0.192)	(0.336)
	[0.003]	[0.004]	[0.003]	[-0.004]	[0.003]	[0.003]
Mean (Y)	0.0386	0.0934	0.0342	0.0293	0.0260	0.0509
Observations	841,929	79,443	665,972	51,922	413,215	428,714
Panel C: Cumulative GPA (Grades 9-12)						
DACAShare*Post	4.572***	2.996	4.345***	-0.205	5.022***	4.061***
	(1.219)	(2.295)	(1.328)	(4.465)	(1.418)	(1.268)
	[0.045]	[0.029]	[0.043]	[-0.002]	[0.049]	[0.040]
Mean (Y)	2.325	2.130	2.260	2.809	2.482	2.173
Observations	798,399	72,470	633,683	49,072	393138	405261
Panel D: Standardized ELA Performance (Grades 9-11)						
DACAShare*Post	6.539***	3.171	5.506***	-1.575	6.606***	6.476***
	(1.302)	(3.602)	(1.220)	(3.646)	(1.282)	(1.464)
	[0.064]	[0.031]	[0.054]	[-0.015]	[0.065]	[0.063]
Mean (Y)	0.0664	-0.138	-0.0150	0.752	0.159	-0.0242
Observations	490,051	43,671	388,816	30,346	242,586	247,465
Panel E: Summary Index (Grades 9-11)						
DACAShare*Post	8.316***	5.441**	8.224***	-1.876	8.885***	7.741***
	(1.134)	(2.100)	(1.219)	(4.419)	(1.173)	(1.328)
	[0.081]	[0.053]	[0.081]	[-0.018]	[0.087]	[0.076]
Observations	631,098	58,936	500,095	38,776	309,513	321,585

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 from a pre-post version of Equation 6. See Table 6 for more details on the specification. See Table 5 for more detail on the sample and full set of controls. Panels A-C focus on yearly outcomes within 4 years of 9th grade enrollment. Panels D-E focus on yearly outcomes within 3 years of 9th grade enrollment. 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 size for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets. This is defined by first re-scaling the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth, and then multiplying the re-scaled coefficient by 0.034. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table 9: The Spillover Effects of DACA by Type of Undocumented Peer, US-Born Students

	Educational Attainment			Academic Achievement	
	(1)	(2)		(3)	(4)
DACAShare* Exposed	5.882*** (2.065) [0.055]		DACAShare* Post	8.316*** (1.134) [0.080]	
DACACloseShare* Exposed		15.91*** (2.301) [0.053]	DACACloseShare* Post		14.35*** (1.623) [0.050]
DACADistantShare* Exposed		-2.084 (2.212) [-0.013]	DACADistantShare* Post		4.073*** (1.369) [0.025]
N	238,781	238,781	N	631,098	631,098

Note: This table contains difference-in-differences 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. Column 1 shows difference-in-differences estimates of a pre-post version of Equation 5 and Column 3 shows difference-in-differences estimates of a pre-post version of Equation 6. See Tables 5 and 6 for more specification details. Columns 2 and 4 show similar estimates but the treatment variable is separately defined by whether or not the likely DACA-eligible peers attended the same middle school as a given student. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. See Table 5 for more detail on the sample and the full set of controls. For Columns 3-4, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect size for the average student is shown below in brackets. This is computed by first re-scaling the coefficients to account for the fact that the share of DACA-eligible peers is an undercount, and is defined as the coefficient multiplied by 0.69 to account for incomplete take-up and 0.40 to account for the difference in take-up of high school students. Then, in columns 1 and 3, the effect size of DACA for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets, and is defined as the re-scaled coefficient multiplied by 0.034. In Columns 2 and 4, the effect effect size of DACA for the average high school student with 1.2 percent DACA-eligible close peers and 2.2 percent DACA-eligible distant peers are shown in brackets, and is defined as the re-scaled coefficient multiplied by 0.012 or 0.022, respectively. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

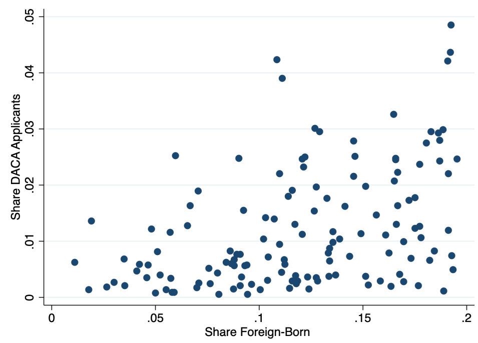
Online Appendix

DACA and Human Capital Investments

Briana Ballis

A Supplementary Figures and Tables

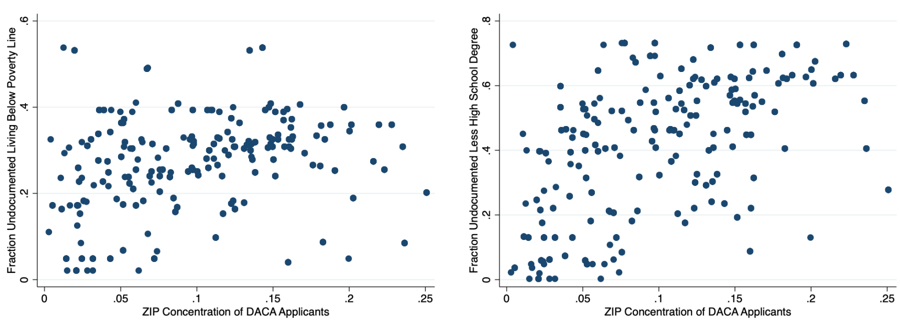
Figure A.1: Correlation Between Zip Share of Foreign-born Youth and DACA applicants



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

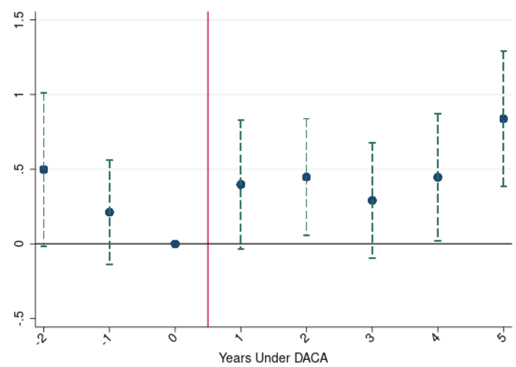
(a) Poverty Rate **(b)** Less than High School Degree



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).

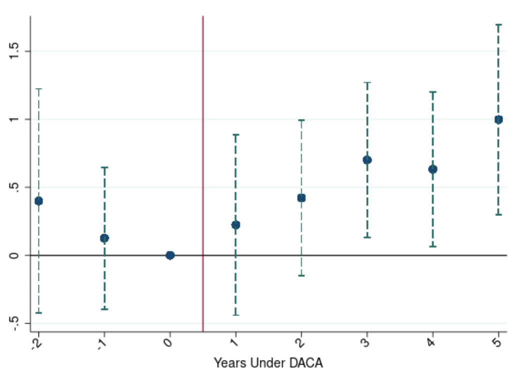
Figure A.3: Direct Impact of DACA on 12th Grade Enrollment

(a) All

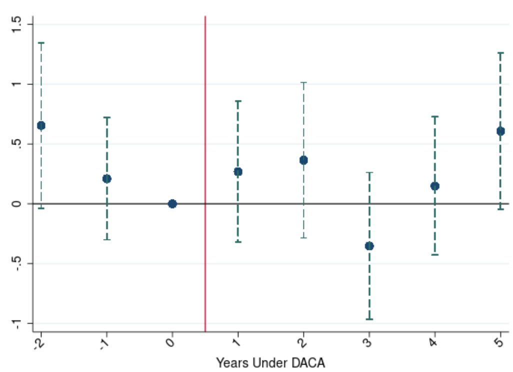


Baseline Achievement Distribution

(b) Bottom 50th Percentile



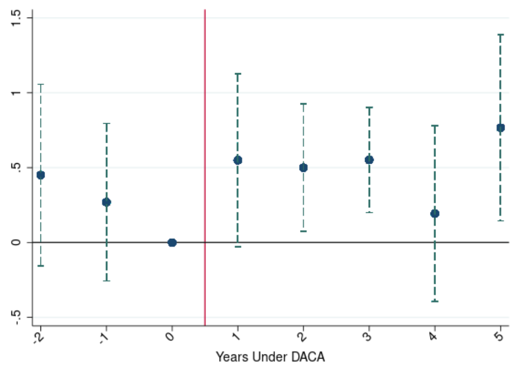
(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 3 and 95% confidence intervals. The dependent variable is an indicator for *expected* 12th grade enrollment (defined as enrollment 4 years after 9th grade). The sample includes Mexican immigrants 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 full set of controls. Standard errors are clustered by residence zip-code.

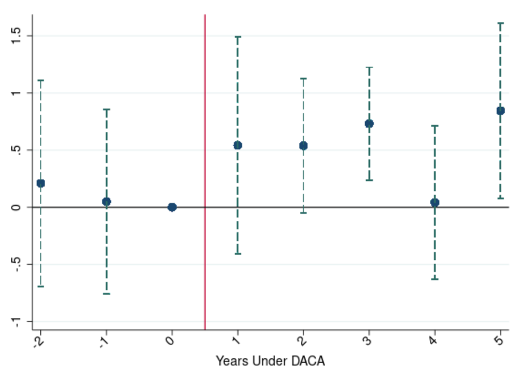
Figure A.4: Direct Impact of DACA on High School Completion

(a) All

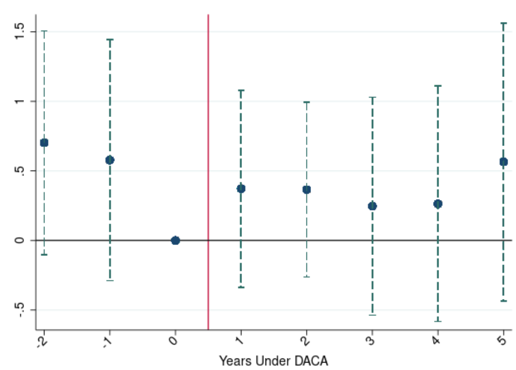


Baseline Achievement Distribution

(b) Bottom 50th Baseline Percentile



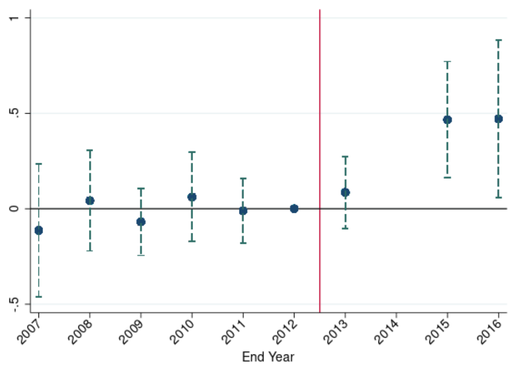
(c) Top 50th Baseline Percentile



Note: These figures plot coefficients from Equation 3 and 95% confidence intervals. The dependent variable is an indicator for high school completion, which is an indicator equal to one if a student graduated from high school within four years of 9th grade. The sample includes Mexican immigrants 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 residence zip-code.

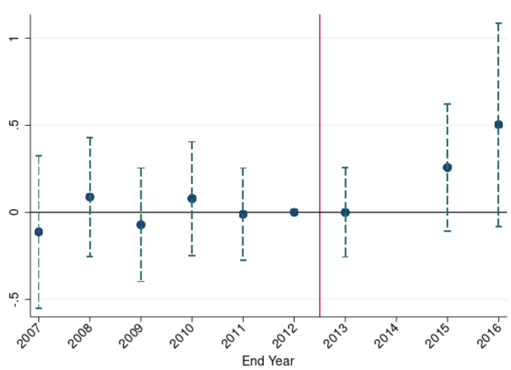
Figure A.5: Direct Impact of DACA on ELA Performance

(a) All

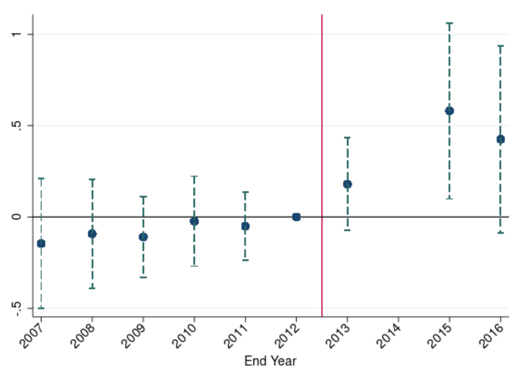


Baseline Achievement Distribution

(b) Bottom 50th Percentile



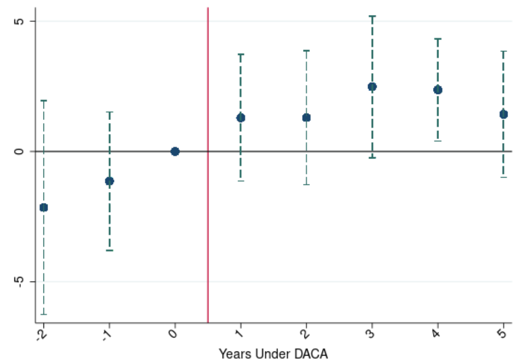
(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 4 and 95% confidence intervals. The dependent variable is performance on the ELA standardized exam. The sample includes Mexican immigrants who arrived to the US by age 9 in 9th grade cohorts between 2006-07 to 2013-14, and focus on yearly outcomes between 9-11th grade. 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 4 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.

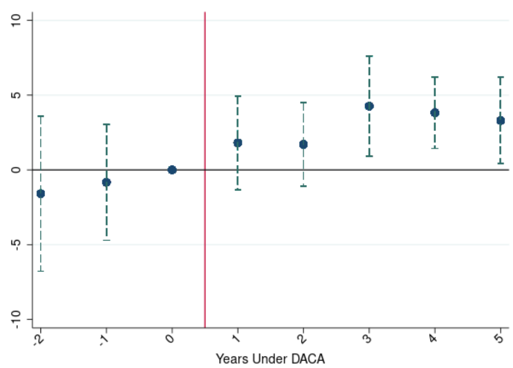
Figure A.6: Spillover Effects of DACA on 12th Grade Enrollment

(a) All

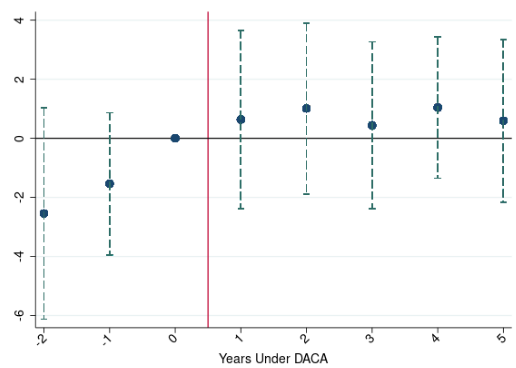


Baseline Achievement Distribution

(b) Bottom 50th Percentile



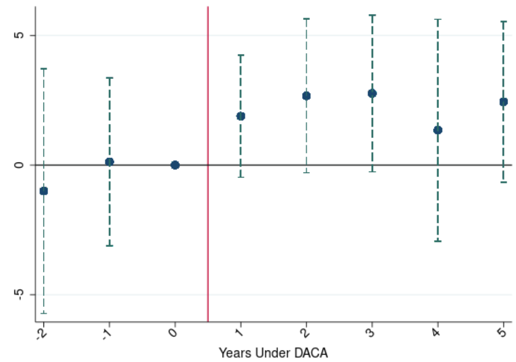
(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 5 and 95% confidence intervals. The dependent variable is an indicator for *expected* 12th grade enrollment (defined as enrollment 4 years after 9th 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 4 for more detail on the sample and the full set of controls. Standard errors are clustered by high school.

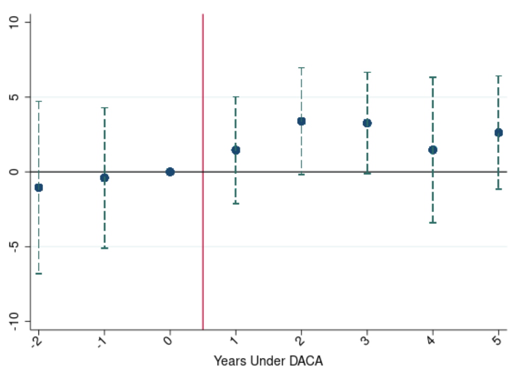
Figure A.7: Spillover Effects of DACA on High School Completion

(a) All

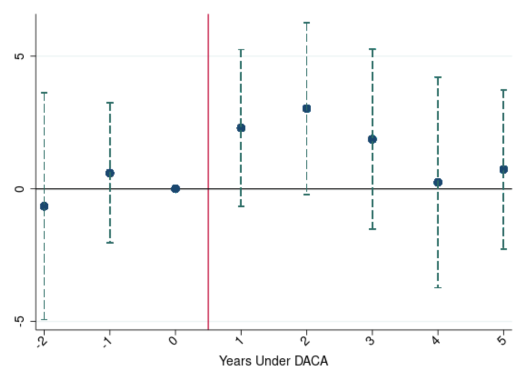


<u>Baseline Achievement Distribution</u>

(b) Bottom 50th Percentile



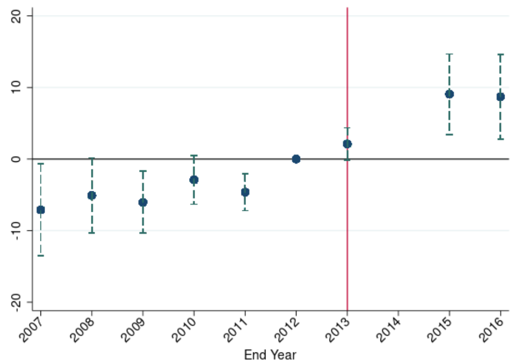
(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 5 and 95% confidence intervals. The dependent variable is an indicator for high school completion, which is an indicator equal to one if a student graduated from high school within four years of 9th grade. 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 5 for more detail on the sample and the full set of controls. Standard errors are clustered by high school.

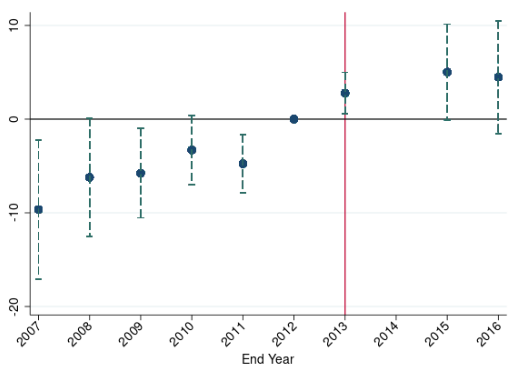
Figure A.8: Spillover Effects of DACA on ELA Performance

(a) All

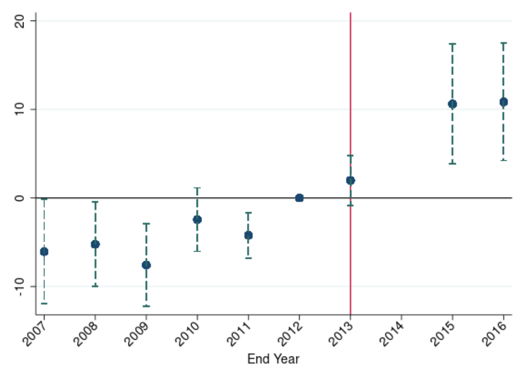


<u>Baseline Achievement Distribution</u>

(b) Bottom 50th Percentile



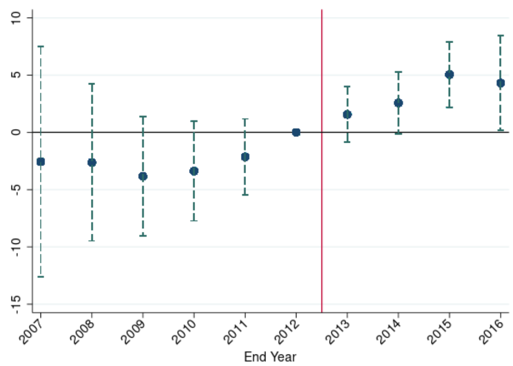
(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 6 and 95% confidence intervals. The dependent variable is performance on the ELA standardized exam. The sample includes US-born students in 9th grade cohorts between 2006-07 to 2013-14, and focus on yearly outcomes within 3 years of 9th grade. 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 5 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.

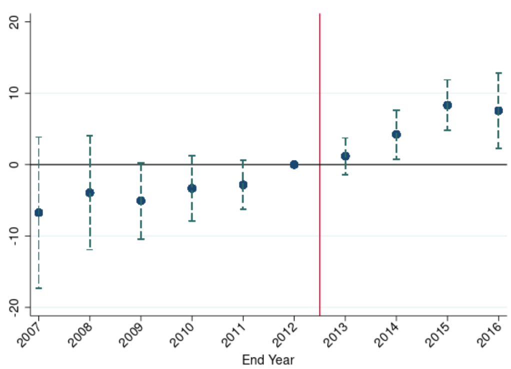
Figure A.9: Spillover Effects of DACA on Semester GPA

(a) All

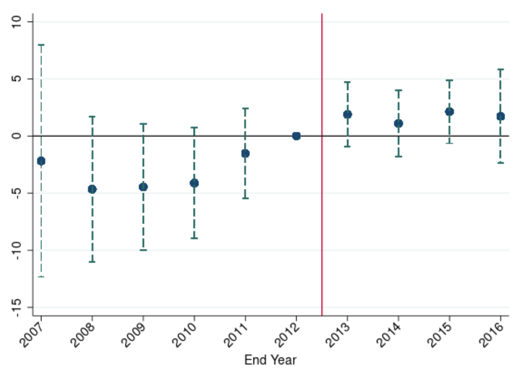


Baseline Achievement Distribution

(b) Bottom 50th Percentile

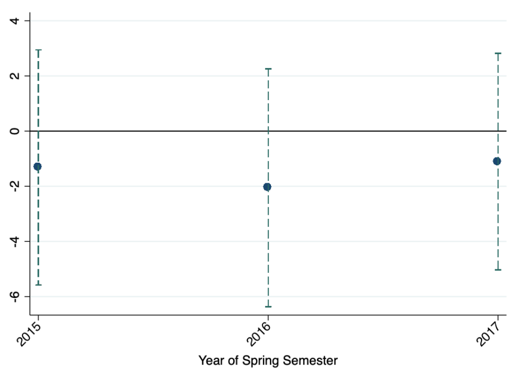


(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 6 and 95% confidence intervals. The dependent variable is GPA. The sample includes US-born students in 9th grade cohorts between 2006-07 to 2013-14, and focus on yearly outcomes within 3 years of 9th grade. 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 5 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 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.

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 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]	[0.026]	[0.021]	[-0.010]	[0.015]	[0.083]	[0.117]	[0.117]
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: Full High School Enrollment Sample								
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)
	[-0.011]	[0.109]	[0.052]	[-0.001]	[0.020]	[-0.016]	[0.053]	[0.091]
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 ShareEligible$_z *$Exposed$_c$, where $Exposed_c$ equals one for cohorts entering 9th grade after 2009-10, and 0 otherwise. 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. The impact of DACA-eligibility is shown in brackets, and is the coefficient multiplied by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

Table A.3: The Heterogenous Effects of DACA on Math Test Scores, Foreign-born Hispanics

	(1)	(2)	(3)	(4)	(5)	(6)
					8th Grade ELA Score	
	All	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)
	[0.095]	[0.1178	[0.094]	[0.177]	[0.007]	[0.340]
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. Within each panel, each column reports estimates from a pre-post version of Equation 4. See Table 4 for more details on the specification. 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 4 for more detail on the sample and the full set of controls. These results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment). All regressions are weighted by the inverse of the number of times a student is observed in the sample. The impact of DACA-eligibility is shown in brackets, and is the coefficient multiplied by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

Table A.4: 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]	[0.011]	[-0.008]	[-0.003]	[0.000]	[0.001]	[-0.012]	[-0.007]	[0.020]
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: Full High School Enrollment Sample									
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]	[0.039]	[0.006]	[0.010]	[-0.007]	[-0.088]	[-0.081]	[-0.065]
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 $\text{DACAShare}_{sc} \times \text{Exposed}_c$, where $Exposed_c$ equals one for cohorts entering 9th grade after 2009-10, and 0 otherwise. 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 Panel 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 5 for more detail on the sample. The effect size for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets. This is defined by first re-scaling the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth, and then multiplying the re-scaled coefficient by 0.034. 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 Heterogenous Effects of DACA on Math Test Scores, US-born students

							Baseline Achievement	
	All	Black	Hispanic	White	Female	Male	Bottom 50	Top 50
DACAShare*Post	5.710**	0.174	6.020**	-5.659	6.221**	5.177*	5.060**	7.853*
	(2.788)	(3.393)	(2.929)	(7.930)	(2.961)	(2.759)	(2.221)	(4.039)
	[0.056]	[0.002]	[0.059]	[-0.055]	[0.061]	[0.051]	[0.050]	[0.077]
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. Within each panel, each column reports estimates from a pre-post version of Equation 6. See Table 6 for more details on the specification. 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 5 for more detail on the sample and the full set of controls. These results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment). All regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect size for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets. This is defined by first re-scaling the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth, and then multiplying the re-scaled coefficient by 0.034. 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 Attainment and Achievement using Quartile Cutoffs of DACA-eligible peer share, US-Born Students

	Graduated High School	Enrolled 12th Grade	Summary Index Educational Attainment	ELA Score	Summary Index Achievement
	(1)	(2)	(3)	(4)	(5)
Panel A: Share of Undocumented Peers (Baseline Specification)					
DACAShare*Exposed	2.418**	2.625***	5.882***	6.539***	8.316***
	(1.078)	(0.928)	(2.065)	(1.302)	(1.134)
	[0.02]	[0.03]	[0.06]	[0.06]	[0.08]
Panel B: Categorical					
2nd Quartile * Exposed	0.0144	0.0145	0.00166	-0.0145	-0.00117
	(0.0159)	(0.0157)	(0.0290)	(0.0282)	(0.0158)
3rd Quartile * Exposed	0.0228	0.0280*	0.0175	0.0620*	0.0887***
	(0.0196)	(0.0167)	(0.0314)	(0.0339)	(0.0182)
4th Quartile * Exposed	0.0396**	0.0404**	0.0756**	0.104***	0.0915**
	(0.0167)	(0.0186)	(0.0337)	(0.0357)	(0.0352)
Mean (Y)	0.576	0.771	0.000	0.0664	-0.0384
N	238,781	238,781	238,781	490,051	631,098

Note: This table contains difference-in-differences estimates of the spillover effects of DACA on educational attainment outcomes (Columns 1-3) and on achievement outcomes (Columns 4-5). Each column reports results from a separate regression. In Panel A, Columns 1-3 shows difference-in-differences estimates of a pre-post version of Equation 5 and Columns 4-5 shows difference-in-differences estimates of a pre-post version of Equation 6. See Tables 5 and 6 for more specification details. The effect size of DACA for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets, and is defined as the re-scaled coefficient multiplied by 0.034. See Table 5 for more detail on how the coefficients are re-scaled. Panel B shows a similar set of results where the share of DACA-eligible peers is split into quartiles (0-1.4%, 1.5-3.7%, 3.8-5.5%, 5.6-11%), where the indicator for having 0-1.4% DACA-eligible peers is excluded from the regression. 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 6 for more detail on the full set of controls. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.7: 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)
	[0.025]	[0.024]	[0.032]	[0.036]	[0.022]	[0.027]
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)
	[0.023]	[0.025]	[0.032]	[0.032]	[0.021]	[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	6.539***	5.367***	5.164***	4.920***	6.404***	2.643*
	(1.302)	(1.661)	(1.684)	(1.402)	(1.294)	(1.380)
	[0.064]	[0.053]	[0.051]	[0.048]	[0.063]	[0.026]
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 Tables 5 and 6, as well as 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 difference-in-differences estimates of a pre-post version of Equation 5. In Panel C, each column reports ifference-in-difference estimates of a pre-post version of Equation 6. See Tables 5 and 6 for more specification details. See Table 5 for more detail on the full set of controls and sample in Panels A-B and see Table 6 for the full set of controls and sample in Panel C. In Panel C, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and 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.9: The Effect of DACA on Campus Switching, US-Born Students

	Moved High School	Moved to Continuation High School	Moved to Regular High School
Panel A: Switched Campus in 10th Grade			
DACAShare*Exposed	1.767	0.392*	1.375
	(2.196)	(0.232)	(2.192)
	[0.017]	[0.004]	[0.014]
Mean (Y)	0.114	0.007	0.107
N	216,608	216,608	216,608
Panel B: Switched Campus in 11th Grade			
DACAShare*Exposed	5.170	1.157*	4.013
	(3.594)	(0.684)	(3.502)
	[0.051]	[0.011]	[0.039]
Mean (Y)	0.189	0.0310	0.158
N	202,370	202,370	202,370
Panel C: Switched Campus in 12th Grade			
DACAShare*Exposed	7.510**	3.442***	4.068
	(3.710)	(0.934)	(3.490)
	[0.074]	[0.034]	[0.040]
Mean (Y)	0.211	0.0518	0.159
N	184,170	184,170	184,170

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on high school campus switching within LAUSD after expected 9th grade. Within each panel, each column reports estimates from a pre-post version of Equation 5. See Table 5 for more details on the specification. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14 who continue to be enrolled in 10th, 11th or 12th grade. All regressions include 9th grade cohort and campus fixed effects. Regressions also include the full set of individual and cohort level controls. See Table 5 for more detail on the full set of controls. The effect size for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets. This is defined by first re-scaling the coefficients to account for undercounting in the share DACA-eligible peer measure by multiplying the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school students. Then the re-scaled coefficient is multiplied by 0.034. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

B Considering Other Pathways for Spillovers

B.1 Family Spillovers

In this section, I investigate whether the positive spillover effects of DACA on US-born students can be explained by changes in family inputs. For instance, if older-siblings became DACA-eligible and experienced improvements in labor market outcomes (e.g. Pope, 2016a; Amuedo-Dorantes & Antman, 2017) this could have led to a positive income shock for their younger US-born siblings. Unfortunately, I do not observe siblings in my data so I cannot entirely rule out the possibility that part of the spillover effects could be driven by family spillovers. However, two things are worth noting that suggest sibling spillovers are unlikely to be the primary mechanism.

First, I estimate that few US-born students are likely to have DACA-eligible siblings. Turning to the ACS, among Los Angeles US-born youth ages 9-13 in 2007 (who would have been in high school in 2012), only 3% had older siblings who were likely DACA-eligible.[1] Second, I find that the positive spillovers of DACA also extend to US-born students who do not speak Spanish at home, who are less likely to be growing up in immigrant families.[2] Table B.1 demonstrates that all US-born students regardless of their home language experience positive DACA spillovers. In fact, the spillovers on educational attainment are larger for non-Spanish speakers, while the impacts on academic achievement are roughly equal across language spoken at home.[3] The fact that I document positive DACA spillovers for US-born students who speak English at home (whose siblings are less likely to be DACA-eligible) provides suggestive evidence that changes in family inputs are unlikely to be the primary mechanism behind the positive DACA spillovers.

[1]Ultimately, I am interested in the share of US-born students with likely DACA-eligible older siblings at the time of DACA's enactment in 2012. If I focused on high school students in the ACS in 2012, I would not be able to account for older siblings that left the household. Therefore, to get at the statistic of interest, I focus on 9-13 year olds in 2007 (who will be in high school during 2012) to capture a period before older siblings are likely to leave the household. To identify likely DACA-eligible youth in the ACS, I focus on foreign-born non-citizens who met the age/date of arrival DACA criteria (i.e. arrival before age 16 and by 2007) and who were enrolled in school or who completed high school (or have a high school equivalent) given DACA's minimum education requirements.

[2]Figlio et al. (2021) also use home language to identify those less likely to be growing up in immigrant families.

[3]Appendix Table C.3 demonstrates that the positive spillovers of DACA are slightly larger for those who are not ELL participants, another group who is less likely to be growing up in immigrant families.

B.2 Neighborhood Spillovers

In this section, I turn to exploring whether the positive spillovers on US-born students can be explained by changes in neighborhood inputs. As previously noted, the enactment of DACA could have improved neighborhood quality if, for example, poverty rates declined due to DACA enabling employment or crime reduced if crime reporting increased due to reduced concerns over interacting with police officers.[4] Table B.2 shows the results for the impacts of school and neighborhood peer exposure separately, as well as a specification that includes both exposure variables together in a "horse-race" specification.[5] In order to ensure that there is variation in neighborhood exposure within a school, I limit the analysis to schools that have students with at least two zip-codes with more than 10% of the student population.[6] Individually, the share of DACA-eligible peers in a school and in a zip-code positively impact the educational attainment and achievement outcomes of US-born youth. In the "horse-race" specification for educational attainment outcomes (Column 4), the positive spillovers are driven by school peers. In terms of academic achievement, school and neighborhood peers both contribute to the spillover effects (Column 8), but the magnitude of the effect for school peers is nearly double that of the effect for neighborhood peers. Overall, these results suggest both school and neighborhood peers matter, but that high school peers matter more.

[4]For example, Amuedo-Dorantes and Deza (2022) find that sanctuary policies reduce domestic violence. The most likely mechanism is that undocumented women may be less likely to report incidences of domestic violence as concerns over police officers asking about their immigration status would have reduced.

[5]Neighborhood DACA peer exposure is defined similarly to the school peer exposure variable. Specifically, for each student, I take the share of foreign-born students in their zip-code of residence and multiply this by ShareEligible_z to approximate the share of likely DACA-eligible neighborhood peers.

[6]It is important to note that this sample restriction does not impact the main spillover results. Table B.2 demonstrates that the results based on the original sample (Column 1) and those that focus on this restricted sample (Column 2) are very similar.

Table B.1: The Spillover Effects of DACA by Home Language, US-Born Students

	Educational Attainment			Academic Achievement	
	Spanish Speaker	Non-Spanish Speaker		Spanish Speaker	Non-Spanish Speaker
	(1)	(2)		(3)	(4)
DACAShare*	3.682***	10.03***	DACAShare*	5.727***	5.287***
Exposed	(1.359)	(3.101)	Post	(1.482)	(1.326)
N	144,966	93,815	N	516,989	289,299

Note: This table contains difference-in-differences 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). Odd-numbered columns show the results for US-born students who speak Spanish at home, while the even-numbered columns show the results for US-born students who do not speak Spanish at home. Each column reports results from a separate regression. Columns 1-2 show difference-in-differences estimates of a pre-post version of Equation 5 and Columns 3-4 shows difference-in-differences estimates of a pre-post version of Equation 6. See Tables 5 and 6 for more specification details. For Columns 3-4, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table B.2: Horse Race Results: The Spillover Effects of DACA driven by Schools vs. Neighborhoods, US-Born Students

	Educational Attainment				Academic Achievement			
	Original Sample	New Sample			Original Sample	New Sample		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DACAShare*	5.882***	6.164***		5.485**	8.316***	7.863***		6.438***
Exposed	(3.026)	(2.224)		(2.598)	(1.134)	(1.213)		(1.051)
DACAShareZip*			3.824***	1.32			6.243***	2.726**
Exposed			(1.206)	(1.429)			(1.355)	(1.072)
ES Avg Campus	[0.06]	[0.08]		[0.07]	[0.08]	[0.11]		[0.09]
ES Avg Zip			{0.05}	{0.02}			{0.09}	{0.04}
N	238,781	212973	212349	212349	631,098	564458	563840	563840

Note: This table contains difference-in-differences estimates of the spillover effects of DACA on a summary index of educational attainment (Columns 1-4) and a summary index of academic achievement (Columns 5-8). Columns 1 and 5 show the results using the original sample of students, while Columns 2 and 6 show the results using the restricted sample of schools that have students with at least two zip-codes that are home to at least 10% of students in a school. Columns 3 and 7 show the results where DACA peer exposure is defined at the zip-code level. Columns 4 and 8 show the results with both treatment variables (i.e. treatment defined at both the zip-code and school level). Each column reports results from a separate regression. Columns 1-4 show difference-in-differences estimates of a pre-post version of Equation 5 and Columns 5-8 shows difference-in-differences estimates of a pre-post version of Equation 6. See Tables 5 and 6 for more specification details. For Columns 5-8, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. In the main analysis sample (Columns 1 and 5) there are 3.4% likely DACA-eligible peers at the average campus, in the restricted sample with enough variation in home zip-code (Columns 2 -4 and 6-8) there are 4.9% DACA-eligible peers at the average campus, and at the average zip-code there are 5.2% likely DACA-eligible youth. Shown below in square brackets are the effects at the average campus and in curly brackets at the average zip-code, and is defined as the re-scaled coefficient multiplied by the average DACA share. See Table 5 for more detail on how the coefficients are re-scaled. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

C Accounting for Contemporaneous Policy Changes

C.1 California Dream Act (CDA)

The CDA act was signed into law on October 8, 2011 (roughly a year before DACA's enactment), and implemented in January of 2013.[7] While California's undocumented youth had been eligible for in-state tuition (IST) since 2001, CDA's introduction further increased college affordability by allowing undocumented students to qualify for state financial aid. Conditional on meeting GPA and income criteria, undocumented students could qualify for the Cal Grant, California's primary financial aid program.[8] Cal Grant receipt can lower annual college costs by as much as 23-38%.[9] These cost reductions could have made college more attractive for undocumented students and incentivized effort during high school during this period in order to qualify for the Cal Grant.[10]

Ultimately, I cannot rule out the possibility that part of the direct and spillover effects of DACA are also being driven by CDA's introduction since both policies were introduced at roughly the same time. Therefore, my results should be interpreted as the impacts of DACA and other related policies aligned with DACA (in the sense that they expand opportunities for undocumented youth) such as the CDA. However, I find several pieces of evidence consistent with DACA (as opposed to the CDA) being the primary driver of the results.

First, significantly larger changes in the cost of college through IST policies led to modest increases (and in some settings no change) in post-secondary attainment for undocumented youth,

[7] CDA eligibility requires three years of high school enrollment in California, while DACA requires arrival to the US by 2007. In 2012 there was close overlap in eligibility for the CDA and DACA for undocumented youth in California. 9th graders expected to graduate high school in 2013 were the first cohort to be able to qualify for the CDA and were also the first cohort to be exposed to DACA before expected high school graduation.

[8] The Cal Grant A is the most generous and competitive Cal Grant. The Cal Grant A covers tuition and fees at any University of California (UC) or California State University (CSU) campus. The Cal Grant A cannot be used to attend community college, but undocumented students could qualify for the California College Promise Grant (CCPG), formally known as the Board of Governors Fee Waiver (BOG), that waives tuition and fees at community colleges.

[9] Based on the author's calculation from the 2021-22 school year. Receiving the Cal Grant A decreases annual college costs at a UC by approximately $14,100 (or 38%) and at a CSU by 6,782 (or 23%). The estimated costs for attending a UC come from : https://admission.universityofcalifornia.edu/tuition-financial-aid/tuition-cost-of-attendance/. The estimated costs for Cal State LA come from: https://www.calstatela.edu/financialaid/2020-2021-cost-attendance. The estimated costs for a California Community College (CCC) come from: http://www.lacolleges.net/admissions/fees.asp.

[10] Undocumented youth do not qualify for federal aid (and DACA did not change this), so for California undocumented students the Cal Grant is the main possible source of financial aid.

and the literature on whether financial aid eligibility enters college enrollment decisions during high school is mixed (Deming & Dynarski, 2009). A recent meta-analysis synthesizing all IST studies finds that the median increase in college enrollment due to IST policies is 2 p.p. (Carroll, Gamez, & Clasing-Manquain, 2022). Although imprecisely estimated, Kuka et al. (2020) find suggestive evidence that DACA increased college enrollment by as much as 6 p.p.. In the context of the Cal Grant specifically which was the financial aid package undocumented youth became eligible for under the CDA, Bettinger et al. (2019) find that Cal Grant eligibility does not increase college enrollment.[11] In addition, CDA's introduction did not lead to increases in educational attainment or achievement for the small subset of undocumented youth who were eligible for CDA but not for DACA due to arriving to the US at older ages.[12] Finally, turning to the ACS I replicate the results of Kuka et al. (2020) but separate the effects separately for California and the rest of the US to see if the results differ in California, perhaps driven by the CDA's introduction.[13] Overall, I find that the direct impacts of DACA were fairly similar in California and the rest of the US. Taken together, these results suggest that the CDA alone is unlikely to be the primary driver of my findings.

Many states have similar financial aid policies for undocumented students in place. According to the Higher Ed Immigration portal, as of 2022, 18 other states also allow undocumented students to apply for state financial aid or scholarships. These states are home to 72% of the overall undocumented population. Given that the vast majority of undocumented youth live in states that offer similar financial aid policies, this serves to increase the generalizability of my results to most DACA-eligible youth in the US.

[11]Despite not finding that Cal Grant eligibility changed college enrollment decisions, Bettinger et al. (2019) find that Cal Grant eligibility did increase college persistence and improved longer-run labor market outcomes. These results are consistent with Conger and Turner (2017) who find that among those undocumented students who enroll in college, availability of financial aid increases college persistence.

[12]One caveat of these findings is that this is a much smaller group of undocumented individuals who arrived to the US between ages 12-14 (in grades 7-9). Undocumented students enrolled in high school during 2012 (the year DACA was enacted) arriving to the US during this age range would not have qualified for DACA since they arrived to the US too late to qualify. These results are available upon request.

[13]These results are available upon request.

C.2 Other Education Policy Changes

In this section, I consider other education policies affecting Los Angeles high school students during this time. For instance, I consider changes related to school discipline, graduation requirements, and ELL participation that roughly coincided with DACA's introduction. Overall, I find little evidence that these other policy changes are likely to significantly bias my results.

Around the time of DACA's introduction LAUSD students experienced changes in school discipline and graduation requirements. In the summer of 2013, LAUSD introduced policies that significantly reduced overall suspensions.[14] Beginning in 2015, students were no longer required to pass the high school exit exam in order to graduate.[15] And the introduction of online credit-recovery courses around this time has anecdotally been linked to increases in graduation rates.[16] 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 C.1 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.[17]

To more formally rule out the possibility that alternate educational policies are driving my

[14] 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.

[15] 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.

[16] 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).

[17] 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.

results, I estimate a pre-post version of 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 C.2 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 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 C.3 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 C.3 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.[18]

Finally, it is important to note 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.[19] 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 outcomes. Nonetheless, as shown in Appendix Table C.1 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 further rule out the possibility that my spillover results are being driven by the reduction in ELL participation, I re-estimate pre-post versions of Equations 5 and 6 including campus-level time trends that vary by the fraction of 9th grade students receiving ELL services in 2007. Column 7 of Appendix Table C.2 demonstrate that the estimates are robust to the inclusion of this trend. In addition, Columns 6-7 of Appendix Table C.3 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.

[18]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.

[19]Because of this decline in ELL participation my main direct impacts and peer effects specification does not include controls for ELL participation. Reassuringly both sets of results are robust to including controls for ELL participation.

Table C.1: 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 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 C.2: 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)
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)
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 difference-in-differences estimates of a pre-post version of Equation 5. In Panel B, each column reports difference-in-differences estimates of a pre-post version of Equation 6. See Tables 5 and 6 for more details on the specification. These models use the full set of controls, as well as linear time trends that vary by campus level characteristics, including the fraction of 10th graders who passed the high school exit exam in 2012, 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 5 for more detail on the full set of controls and sample in Panels A-B and see Table 6 for the full set of controls and sample in Panel C. In Panel C, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors in parentheses are clustered at the high school campus level. *p< 0.10, **p < 0.05, *** p< 0.01.

Table C.3: 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)
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)
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 measure is then split into three terciles, from the lowest likelihood in column (1) to the highest likelihood in column (3). In Panel A, each column reports difference-in-differences estimates of a pre-post version of Equation 5. In Panel B, each column reports difference-in-differences estimates of a pre-post version of Equation 6. See Tables 5 and 6 for more details on the specification. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. See Table 5 for more detail on the full set of controls and sample in Panel A and see Table 6 for the full set of controls and sample in Panel B. In Panel B, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

D Using Alternate Ways of Approximating DACA-eligibility

In this section, I consider alternate ways of approximating undocumented status to probe the robustness of the main results. As noted above in Section 3 of the paper, my measure of DACA-eligibility uses geographic variation in take-up of DACA which was high. Nonetheless, this will underestimate the true likelihood of DACA-eligibility. To assuage concerns that this is influencing my findings I consider several other measures that over and under-estimate likely DACA-eligibility, and reassuringly come to very similar conclusions.

Appendix Table D.1 presents the spillover results using a variety of alternate ways of approximating likely DACA-eligibility. Column 1 shows my baseline estimates that use the overall DACA take-up rate in one's zip code to determine likely DACA-eligibility. In Column 2, I slightly modify this proxy to capture the difference in take-up for high school students, the population of interest. While I do not observe the total number of DACA high school applicants in each zip-code, I observe the share of DACA-applicants who were less than 19 at the county level. Therefore, I multiply the total number of applications in each zip-code by the take-up rate of DACA for those ages 15-19 in Los Angeles county, which was 41%, and then divide by the total foreign-born population ages 15-19 in that zip-code using 5-year ACS estimates. Comparing the average effects, which are computed by taking the coefficients and multiplying by the average of each of these measures, reveal very similar conclusions.

Turning to measures that do not select on the DACA application decision and will likely over-estimate the DACA-eligible population, Columns 3 and 4 proxy for likely DACA eligibility using the fraction of non-citizens adjusted to more accurately identify the undocumented population living in a PUMA and by the fraction of non-citizens in a zip-code, respectively.[20] Again, the effect sizes are shown in brackets, and are computed by taking the coefficient and multiplying by the

[20] The estimates of the undocumented population by PUMA are calculated by MPI who make a number of statistical adjustments to ACS data to account for the fact that the undocumented population may be undercounted in the ACS. For more detail on the methodology see here: see https://www.migrationpolicy.org/about/mpi-methodology-assigning-legal-status-noncitizens-census-data. One downside of this measure is that PUMAs are larger areas than zip-codes.

average of each measure. Reassuringly, the average effects and conclusions are nearly identical regardless for how I proxy for likely DACA-eligibility.[21] Finally, Column 5 shows that there are no spillovers driven by the share of foreign-born students. 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 able to capture the peer effects stemming from having more undocumented peers after DACA's enactment rather than spillovers from having more foreign-born peers after 2012.

[21] Similarly, Appendix Table D.2 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.

Table D.1: 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. my main 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 who were non-citizens making a number of statistical adjustments to more accurately identify the undocumented population living in a PUMA done by the Migration Policy Institute (MPI), 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 difference-in-differences estimates of a pre-post version of Equation 5. In Panel C, each column reports difference-in-differences estimates of a pre-post version of Equation 6. See Tables 5 and 6 for more details on the specification, sample, and controls for Panels A-B and Panel C, respectively. In Panel C, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and 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 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 D.2: 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 who were non-citizens making a number of statistical adjustments to more accurately identify the undocumented population living in a PUMA done by the Migration Policy Institute (MPI), and Column 4 uses the fraction of foreign-born non-citizens in a zip-code. In Panels A and B, each column reports difference-in-differences estimates of a pre-post version of Equation 3. In Panel C, each column reports difference-in-differences estimates of a pre-post version of Equation 4. See Tables 3 and 4 for more details on the specification, sample, and controls for Panels A-B and Panel C, respectively. In Panel C, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and 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 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.