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Abstract

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

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1 Introduction

A substantial literature documents the importance of peer influences as an input to economic mobility (Sacerdote, 2011). In particular, peer characteristics such as gender, race and achievement have all been shown to have a strong influence on educational investments (e.g. Hoxby, 2000; Imberman, Kugler, & Sacerdote, 2012; Carrell, Hoekstra, & Kuka, 2018). However, significantly less is known about *why* these peer characteristics generate significant spillovers on long-run trajectories. One possible explanation is that peers with these characteristics have different personality traits or have other non-cognitive skills, and there has been a growing interest in better understanding how these attributes explain one's own human capital accumulation decisions. Yet, there has been little evidence on the extent to which personality traits or non-cognitive skills influence peer outcomes.

In this paper, I examine the spillover effects of one important aspect of peer personality - academic motivation. To do so, I use the 2012 introduction of DACA as a natural experiment that changed the returns to schooling among some high school students, without changing the incentives for others. Under DACA, undocumented youth who completed high school could receive temporary protection from deportation and work authorization.[1] DACA's eligibility criteria introduce clear incentives for undocumented youth to invest more in their education. Indeed, prior work suggests that DACA's introduction significantly increased the likelihood that undocumented youth completed high school, by as much as 7.5 percent (Kuka, Shenhav, & Shih, 2020). Using this abrupt change in educational investments among undocumented youth, I test whether exposure to DACA-eligible peers impacted US-born students (who were not DACA-eligible). As plausibly exogenous changes in academic motivation among *existing* peer networks is rarely observed, DACA provides an ideal setting to study the responsiveness of educational investments to changes in peer motivation.[2]

Beyond the contributions this paper makes to the peer effects literature, understanding

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

[2] As will further be discussed in Section 2, DACA-eligible youth were required to live in the US for at least 5 years. Therefore, DACA-eligible youth were not recent immigrants and were likely well integrated with their US-born peers.

the spillover effects of DACA also has important policy implications for the DACA program itself. DACA is an important immigration reform that has remained at the forefront of public discourse and current immigration policy debates. Previous studies on DACA have focused exclusively on the impacts DACA had on undocumented youth, but these studies have ignored the possibility of spillovers on US-born (e.g. Pope, 2016a; Kuka et al., 2020; Amuedo-Dorantes & Antman, 2017).[3] 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.

I use administrative data from Los Angeles Unified School District (LAUSD) together with administrative data on DACA applicants from the U.S. Citizenship and Immigration Services (USCIS). 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 whether DACA directly affected Los Angeles students, I compare changes in educational outcomes of foreign-born students living in zip-codes with higher concentrations of DACA-eligible youth (who were more likely to be undocumented) to those with lower concentrations (who were likely foreign-born citizens), before and after the introduction of DACA.[4] To identify the spillover effects of DACA, I compare changes in the educational outcomes of US-born students in high schools with higher shares of DACA-eligible peers to those in high schools with lower shares.

I find that DACA led to significant increases in targeted students' educational investments. High school graduation increased by 6 percentage points (or 10 percent) among youth who were

[3]The lack of attention to spillovers can likely be explained by the fact that most studies on DACA utilize national survey data (such as the ACS) which make it difficult to connect undocumented students to peer groups.

[4]The direct impacts analysis conducted in this paper shares similarities with Kuka et al. (2020), however, it differs in at least two important ways. First, I establish the direct impact of DACA on educational attainment and on student effort in Los Angeles only. This is critical to determine before turning to the estimation of spillovers (the primary goal of this paper) since the impacts on educational attainment in Los Angeles could be different than those estimated nationally. Second, there has been less attention placed on understanding to what extent DACA increased effort during high school, which I am able to address using rich administrative K-12 schooling records. Better understanding DACA's impact on effort in high school is important for understanding DACA's impact on undocumented youth, and for the purposes of this paper, critical for interpreting and understanding the mechanisms driving DACA's spillovers.

likely undocumented. The effects are driven by males and students who were initially low achievers. These groups are typically at risk of dropping out of high school and would have been more likely to respond to DACA's educational incentives. The magnitude of this effect are similar to Kuka et al. (2020), who focus on a national sample. In addition, I find that DACA led to significant improvements in English Language Arts (ELA) achievement and GPA among likely undocumented youth, whose ELA achievement increased by 0.14 standard deviations after DACA's enactment.[5] As students would have had to exert additional effort in order to experience these performance improvements, these results suggest that undocumented youth increased effort in response to DACA.

These increases in educational investments had positive spillover effects on undocumented students' US-born peers: at the average campus, where 1 percent of students were likely undocumented, DACA's introduction leads to a 2 percentage point (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. To investigate these possibilities, I estimate whether close contacts have a differential impact, and find that spillovers are larger for closer contacts.[6] Spillovers being larger for close contacts is consistent with the positive spillovers being driven by peer-to-peer influences. If other changes in school-wide instruction were driving the positive spillovers, there is no reason for closer contacts to have a stronger impact.

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

[5] In this paper, I primarily focus on ELA achievement. After 8th grade students can choose what version of the math exam they take during each grade so I do not focus on math scores as a main outcome. However, I do find evidence that conditional on the math exam students took, math scores improved both for undocumented youth and their peers.

[6] 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).

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. However, Abramitzky et al. (2021) only address whether there are spillover effects on the margin of college enrollment because high school completion was high in their setting (over 95 percent were completing). My project builds upon this recent work by addressing whether policy spillovers exist on the margin of high school completion among students in a large low-performing US school district.

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

More broadly, 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

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.[7]

Finally, I contribute to the emerging literature on the impacts of 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). 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. My primary contribution to this literature is to show that DACA also generated positive spillovers on the US-born population. In addition, to my knowledge, this is the first paper to identify the impacts of DACA using administrative K-12 schooling data. This allows me to carefully track DACA's impact on a wide range of intermediate schooling outcomes to ascertain whether DACA impacted overall effort, as well as for different types of students (i.e. high vs. low achievers).[8] Finally, 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 students likely undocumented status. In the context of Los Angeles (with high take-up), this allows me to estimate DACA-eligibility with minimal measurement error.[9]

[7]While this paper contributes to this literature on immigration spillovers, it is important to re-emphasize that DACA-eligible youth are not recent immigrants due to the eligibility criteria detailed in Section 2. It is also important to acknowledge that along with legalization, DACA introduced clear incentives to increase high school completion. Therefore, I am not able to isolate the causal impact of legalization alone, but rather legalization along with other educational incentives. However, for high achievers, who were not incentivized to complete high school (they likely would have completed regardless of DACA) other educational incentives were likely less important.

[8]To my knowledge, only Kuka et al. (2020) has investigated whether DACA improved high school performance by focusing on data from California's exit exam. They identify improvements on the pass rate among likely undocumented seniors approaching their final opportunity to pass the exam, suggesting effort improved for some.

[9]Using immigrant non-citizens is the most common way to approximate the undocumented population (e.g. Pope, 2016a; Kuka et al., 2020; Amuedo-Dorantes & Antman, 2017), however, this is measured with noise, as non-citizens

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)[10] and minimum education requirements.[11] 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 (Batalova, Hooker, & Capps, 2014) yields a take-up rate of 65%.[12] The significantly higher take-up in Los Angeles can likely be attributed to the strong presence of pro-immigration rights groups who undertook extensive outreach activities immediately after DACA's enactment throughout the city of Los Angeles.[13]

include green card holders and temporary visa holders. As will be detailed in Section 3, there is likely to be less noise in my measure. However, my results are robust to other proxies (including the fraction on non-citizen immigrants).

[10] In 2012, DACA-eligible youth were required to have immigrated to the US before 2007. These age/date of arrival criteria require at least 5 years of US residence. Thus, DACA-eligible youth are not recent immigrants. The median age of US entry among DACA-eligible youth was 6 and the most common age was 3 (Parlapiano & Yourish, 2018).

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

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

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

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

2.1 Education Incentives for Undocumented Youth

A human capital investment model proposed by Kuka et al. (2020) illustrates how DACA likely incentivized undocumented youth to invest more in their education. To briefly summarize this model, Kuka et al. (2020) consider undocumented youth choosing a level of education (high school drop-out, high school completion, or college) based on expected lifetime earnings. DACA recipients experience an increase in expected lifetime earnings for two reasons. First, DACA recipients receive a work permit. This increases the expected wage at all education levels from the non-legal to the legal wage.[14] 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.[15]

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 expected lifetime earnings, they may still choose to complete high school if they prefer living in the US, and value the temporary protection from deportation DACA offers.[16]

[14]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).

[15]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.

[16]Moreover, students enrolled in high school may have been additionally incentivized to become DACA-eligible in

Since the returns to college will also increase with legalization due to DACA, undocumented youth may also be incentivized to enroll in college.[17] Consistent with this model, Kuka et al. (2020) find that nationally likely undocumented youth experienced significant increases in high school completion in response to DACA, and positive (but imprecise) increases in college enrollment.

2.2 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, 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 before and after DACA.

2.3 Undocumented Population in Los Angeles

Los Angeles provides an ideal setting to study the effects of DACA on student outcomes. Los Angeles is home to the largest percentage of DACA-beneficiaries in the US, accounting for 14 percent of all beneficiaries (Parlapiano & Yourish, 2018). As previously noted take-up of DACA was high in Los Angeles, and was very popular among students in the city. In fact, the introduction of DACA was in part motivated by a decade long student-led movement based in Los Angeles

order to obtain a driver's license and to have the ability to work a part time job while in high school. Only as of January 1, 2015 were undocumented immigrants in California able to obtain a driver's license.

[17] Undocumented residents in California had been eligible for in-state tuition since 2002. However, they only became eligible for state financial aid through the introduction of the California Dream Act in 2012. Therefore, at the time of DACA's introduction, undocumented youth also experienced increases in college affordability, thereby, increasing the incentives to enroll in college at this time. I turn to a discussion of this policy change in more detail in Section 5.4.

that had been advocating for a path to citizenship (Nicholls, 2013).[18] In addition to broad support among students, there was also a lot of DACA outreach done in all 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).[19] In other parts of the country where less outreach was targeted towards high school students, the immediate benefits of DACA among high school students may have been less salient.

Moreover, prior to DACA's enactment, educational attainment in Los Angeles was low relative to the rest of the US. In 2012, only roughly 60% of all high school students graduated from high school on-time.[20] Among those who were undocumented who met all of the age and date of arrival DACA criteria 30% of had already dropped out of high school (McHugh, 2014), and for those who completed high school, most (slightly over 70%) did not pursue higher education.[21]

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, as previously noted, 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 DACA-eligible youth were well integrated with their US-born peers at the time the policy was introduced.

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

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

[20]This is calculated for 2009 9th graders, where on-time graduation is measured within four years of 9th grade.

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

3 Data

I leverage administrative data from the LAUSD, and focus on students entering 9th grade between 2007 and 2014.[22] The data track key academic and behavioral outcomes yearly, including attendance rates, state standardized exam scores (which I normalize to have a mean zero and standard deviation one at the grade-year level), disciplinary actions, semester GPA, yearly enrollment indicators and whether a student graduated from high school. Importantly, LAUSD data also includes each student's country of birth, date of arrival to the US (if foreign-born), and current residence zip-code.

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

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

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

where the numerator is constructed from USCIS data and the denominator is constructed from the ACS.[23] For each foreign-born student, I use this measure to proxy for their likelihood of being

[22] This includes 9th grade cohorts who were unexposed (2007-2009), partially exposed (2010-2012) and fully exposed (2013-2014) to DACA during high school. Appendix Table A.1 shows *expected* DACA exposure by each 9th grade cohort, where *expected* exposure is defined during the four years following 9th grade enrollment.

[23] As just noted, the numerator counts the number of DACA applicants by zip-code between July 2012- December 2013. Given DACA's eligibility criteria, these applicants were ages 15-30 as of June 2012. In the denominator, I focus

undocumented based on their zip-code of residence (which I observe in LAUSD data).[24] As illustrated in Figure 1, there is significant variation in this measure across Los Angeles zip-codes.[25]

Importantly, since take-up of DACA was high in Los Angeles county (over 65%), this measure likely estimates the undocumented population with minimal measurement error.[26] Nevertheless, Equation 1 will undercount the undocumented population living in a zip-code. However, as long as take-up of DACA across zip-codes was uncorrelated with trends in educational outcomes, this undercounting is unlikely to confound my estimates. While I am not able to test this assumption directly (as I do not observe undocumented youth by zip-code), event-study plots presented in Section 4.2 demonstrate that educational outcomes of Hispanic immigrants in zip-codes with different concentrations of DACA-applicants had similar trends prior to DACA's enactment (especially for low-achievers who were most affected). Moreover, I show that DACA take-up is uncorrelated with measures of poverty rates and educational attainment (factors that could affect DACA take-up) at the zip-code level.[27] Finally, in Section 5.4 I show that my results are similar using other measures to approximate undocumented status that do not select on the DACA application decision (e.g. share of foreign-born non-citizens). It is reassuring that I find similar results either way.[28]

In addition to using Equation 1 to identify likely undocumented status, I also use country of origin and age of US arrival to identify foreign-born youth who were more likely to be DACA-eligible. In California, over 95% of DACA applicants are Hispanic, with the vast majority born in Mexico (86%) (Svajlenka & Singer, 2013). Therefore, to estimate the direct impacts of DACA I limit my focus to Hispanic foreign-born students.[29] In addition, DACA applicants had to have lived

on the total number of foreign-born youth in a zip-code who were of similar ages across a similar period. Specifically, the denominator counts the foreign-born population ages 15-29 in each zip-code using 5-year ACS estimates from 2014.

[24] As a robustness check in Section 5.4, I focus on an alternate measure that focuses on DACA-eligible youth who were high-school aged. Reassuringly, this alternate measure yields very similar results.

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

[26] Compared to the share of non-citizen immigrants, which Kuka et al. (2020) estimate to be 55 percent undocumented, my measure likely comes closer to the undocumented population.

[27] Specifically, Appendix Figure A.2 demonstrates that there is no correlation between the fraction of DACA applicants (Equation 1) and non-citizen population over 18 years old living in poverty or with less than a high school education.

[28] In addition, one advantage of focusing on one school district is that outreach and other DACA assistance (such as providing documents to verify eligibility) was organized at the district level in LAUSD. Thus, DACA was likely to be equally salient across all regions of Los Angeles, making the possibility of differential take-up across areas less likely.

[29] This sample restriction does not drop many students. Of all foreign-born youth who arrived to the US by age 9 in

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

To estimate the spillover effects of DACA, I focus on students who were born in the US, who are unlikely to be affected by DACA except through policy spillovers. The final sample I use to estimate the spillover effects of DACA consists of 238,781 US-born students. My measure of DACA exposure for US-born students builds upon Equation 1. Specifically, I approximate the share of a student's peers who were *likely* DACA-eligible as follows:

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

where s and c represent high school campus and 9th grade cohort respectively. FBShare_{sc} is the share of Hispanic foreign-born youth (who arrived to the US by age 9) in each student's campus as of 9th grade, rescaled by the second term which captures the likelihood that these foreign-born peers were undocumented.[30] Specifically, this second term is the weighted average of the zip-code DACA application measure defined in Equation 1 across the residence zip-codes of Hispanic foreign-born youth (who arrived to the US by age 9) *enrolled* in each student's campus as of 9th grade.[31]

Finally, these administrative data from LAUSD are advantageous both in terms of the number of outcomes they track, my ability to observe country of birth, and the large sample. Yet one drawback of using administrative data from a single district is that I cannot track people who

9th grade cohorts between 2007 and 2014, 83% are Hispanic.

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

[31] Where n_{sc} denotes the number of Hispanic immigrants (arriving by age 9) overall, and n_{scz} denotes the number of Hispanic immigrants (arriving by age 9) living in a given zip-code, in each student's campus as of 9th grade.

leave LAUSD. Restricting the sample to those observed in 9th grade minimizes the possibility that these students left LAUSD for any other reason than deciding to drop-out of high school. Moreover, I will show that DACA predicted enrollment in LAUSD (rather than drop-out), suggesting that any possible attrition from LAUSD (to other school districts) is uncorrelated with policy exposure.[32]

3.1 Summary Statistics

Table 1 presents summary statistics for 9th grade cohorts enrolled between 2006-07 and 2013-14. Columns 2 vs. 3 compares US-born students to foreign born students in LAUSD. The vast majority of US-born and foreign-born students are Hispanic (roughly 77 percent) and participate in Free-Lunch (roughly 65 percent). Foreign-born students are slightly more likely to be classified as an English Learner and have slightly lower baseline ELA scores, but performed similarly at baseline on the math exams. The similar ethnicity and economic background of US-born and foreign-born students in Los Angeles suggest that spillovers due to DACA were likely.

Columns 3-6 of Table 1 compare foreign-born youth by ethnicity and age at US arrival. Relative to all foreign-born youth, those of Hispanic ethnicity are lower achieving at baseline, but are equally likely to be classified as an English language learner (ELL) and be receiving free or reduced price lunch (FRL). Hispanics and Mexicans who arrived to the US before they were 9, have similar baseline achievement to all foreign-born students, but lower achievement relative to US-born students. Despite these differences in baseline achievement, on-time high school completion (defined within four years of 9th grade), is similar across all the subgroups shown in Table 1.[33]

Table 2 presents summary statistics that compare high school campuses with different concentrations of likely undocumented students. Students in campuses with higher fractions of likely undocumented youth are more likely to be Hispanic, participating in ELL programs, receiving FRL, and are lower performing at baseline. While all campuses have fairly similar shares of foreign-born

[32]To further corroborate this claim, my results in Section 4.2 and 5.2 show that the decreases in drop-out are driven only by low-achievers (who are on the margin of high school drop-out). This pattern is consistent with drop-out rather than students enrolling in other district (since it unlikely that *only* low-achievers would be enrolling elsewhere).

[33]In this paper, I focus on on-time graduation (i.e. measured within 4 years of 9th grade enrollment) in order to include more recent 9th grade cohorts.

students, foreign-born students in campuses with higher concentrations of likely undocumented youth are more likely to be born in Mexico. It is important to note that while my peer effects identification strategy does not require that the fraction of likely undocumented youth in a school be uncorrelated with school characteristics, it does require that the fraction of undocumented youth is uncorrelated with changes in outcomes that occur for any reason other than the introduction of DACA. So while these differences do not pose a direct threat to my identification strategy, it is important to rule out the possibility that these demographic differences do not introduce a later divergence in trends. Reassuringly, I demonstrate in Section 5.4 that my results are robust to the inclusion of time trends interacted with baseline campus demographics.

4 Direct Impacts

4.1 Empirical Strategy

Before turning to spillovers, it is important to determine whether the increased returns to schooling due to DACA impacted educational investments among undocumented youth in Los Angeles.[34] If I could directly observe legal status then I could compare changes in educational investments of undocumented youth who exogenously experienced an increase in returns to schooling in 2012, to changes in educational investments among foreign-born citizens who were not eligible. However, as previously noted, this strategy is infeasible because I cannot observe a students' legal status.[35]

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,

[34] As previously noted, Kuka et al. (2020) use a similar empirical strategy to identify the direct impacts of DACA on educational attainment using a national sample. My direct impacts analysis differs because I am able to determine impacts of DACA on Los Angeles undocumented students only (this is necessary to establish, before turning to the spillover effects on US-born students in Los Angeles) and explore a wider set of intermediate outcomes (such as behavioral outcomes and GPA) across a wide range of student characteristics (such as baseline achievement) to better ascertain whether DACA impacted student effort (and for what *types* of undocumented students).

[35] This challenge is not unique to this paper. Most of the prior literature has relied on the absence of US citizenship and Hispanic ethnicity as a second best measure for undocumented status (Kuka et al., 2020; Pope, 2016a; Amuedo-Dorantes & Antman, 2017; Kaushal, 2006). Given the high take-up rate of DACA in Los Angeles, my measure that depends on the geographic concentration of DACA applicants is likely measured with minimal measurement error.

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 high school completion and enrollment in each high school grade (outcomes that only occur once), I estimate the following event-study equation on the sample of foreign-born Hispanic 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 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.[36] I control for zip-code (high school campus) γ_z (γ_s) fixed effects to account for fixed cross-sectional differences across zip-codes (high school campuses), and cohort 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.[37] 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 the high school completion (or enrollment) of likely undocumented youth for each 9th grade cohort separately.[38]

[36] Again, each 9th grade cohort varied in their exposure to DACA, based on the number of years they were *expected* to be enrolled in high school after 9th grade as shown in Appendix Table A.1. For 9th cohorts expected to graduate before the policy, there should be no impact of DACA on outcomes. However, the impacts should be increasing across 9th grade cohorts who had more years of exposure to DACA during high school (i.e. the four years following 9th grade).

[37] I do not control for free or reduced price lunch status. Parents must apply to receive free-lunch, and parents who are undocumented may be less likely to apply. Reassuringly, results are robust to including this control variable. Moreover, I do not control for 8th grade math achievement because as previously noted by the time of 8th grade students can take different versions of the math exam.

[38] For table estimates, I also estimate a pre-post version of Equation 3:

$$Y_{izsc} = \vartheta_0 + \vartheta_1(\text{ShareEligible}_z * \text{Exposed}_c) + \lambda_1 X_{izsc} + \lambda_2 Z_{sc} + \gamma_s + \gamma_z + \phi_c + \varepsilon_{izsc} \quad (4)$$

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

$$Y_{izstg} = \beta_0 + \sum_{j=2005}^{2016} \beta_j \mathbb{1}\{j = t\} \times \text{ShareEligible}_z + \lambda_1 X_{izsc} + \lambda_2 Z_{sc} + \phi_{sg} + \alpha_{tg} + \gamma_z + \varepsilon_{izstg} \quad (5)$$

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 zip-code of residence with yearly dummy variables. ϕ_{sg} and α_{tg} are school-grade and year-grade fixed effects, and all other control variables are as previously defined.The variables of interest, β_j, trace out the impact of DACA on the academic and behavioral outcomes of likely undocumented youth for each year separately.[39]

The main identification assumption is that educational investments would have evolved similarly for foreign-born Hispanic students residing in zip-codes with different fractions of DACA applicants in the absence of DACA. In favor of this parallel trends assumption, event-study estimates presented in Section 4.2 rule out the possibility that likely undocumented youth were increasingly more likely to invest in their education relative to likely foreign-born citizens leading up to DACA.

I also find that observables do not predict a differential improvement in outcomes for likely undocumented youth after DACA's enactment. Specifically, I use all covariates (excluding treatment) to generate predicted high school completion based on students during the pre-policy period. These results are shown in Appendix Table A.2, where Panel A focuses on the full sample and Panel B limits the sample to those who were enrolled throughout all four years of high school. Column 1 shows that conditional on cohort, campus, and zip-code fixed effects, Hispanic immigrants living in

where Exposed_c is an indicator for whether a student was *expected* to attend high school after DACA's enactment. This indicator variable is equal to one for 9th grade cohorts entering 9th grade after 2009-10, and 0 otherwise. All other variables are as previously defined.

[39] Again, for table estimates, I estimate a pre-post version of Equation 5:

$$Y_{izstg} = \upsilon_0 + \upsilon_1 (\text{ShareEligible}_z \times \text{Post}_t) + \lambda_1 X_{izsc} + \lambda_2 Z_{sc} + \phi_{sg} + \alpha_{tg} + \gamma_z + \varepsilon_{izstg} \quad (6)$$

where Post_t is an indicator variable which equals 1 if the outcome was measured after DACA's enactment in 2012. All other variables are as previously defined.

high DACA zips were not predicted to be more likely to graduate high school after DACA. Columns 2-7 demonstrate that overall there were similar trends in observables and baseline achievement among Hispanic immigrants living in zips with different shares of DACA applicants.[40]

4.2 Results

Figure 2 shows the impact of DACA on a summary index of educational attainment for Hispanic foreign-born students separated by baseline achievement, based on estimates from Equation 3.[41] Panel A demonstrates that for low-achievers, likely undocumented immigrants exhibit parallel trends in educational attainment to likely documented immigrants before DACA's introduction. However, educational attainment sharply increases for likely undocumented youth expected to be enrolled in high school after DACA's introduction.[42] Specifically, estimates based on Equation 4 presented in Table 3 suggest that DACA increased in *expected* 12th grade enrollment [43] and high school completion by 10 and 22 percent respectively, for low-achieving likely undocumented students.[44] In contrast, Panel B of Figure 2 demonstrates that for high-achievers immigrants there is little relationship between likely undocumented status and educational attainment across 9th grade cohorts. High-achievers are unlikely to be affected by DACA's graduation incentives since they are already very likely to graduate from high school regardless of DACA. Consistent with the event-study plots, Table 3 demonstrates that DACA did not predict changes in educational

[40]One exception is that I find that DACA predicts the likelihood of being male for the sample enrolled throughout high school shown in Panel B. This is consistent with the increases in high school enrollment among likely undocumented males presented in 4.2, and also correspond to a relatively small change of 1 percent. In addition, I document a small positive marginally significant increase in 7th grade ELA achievement.

[41]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.

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

[43]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.

[44]The effect size is computed for the fully exposed student residing in a zip-code where 25 percent of the foreign-born population applied to DACA, and is computed by multiplying the coefficient by 0.25.

attainment among high-achieving undocumented students.

Exploring differences by country of origin and gender, Table 3 also reveals that Mexicans and males experienced the largest increases in educational attainment. In addition to low-achievers, males are typically at risk of dropping out of high school and would have been more likely to respond to DACA's educational incentives. Moreover, the effects are slightly larger for Mexicans, who are more likely to be undocumented relative to the overall Hispanic population in Los Angeles.

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. On the other hand, it is possible that DACA induced students to remain enrolled in school, but was not accompanied by any changes in effort.[45] The extent to which any increases in educational attainment among likely undocumented youth would spillover to US-born peers will depend on which of these two scenarios was more likely.

Figure 3 shows the impact of DACA on a summary index of academic achievement for Hispanic foreign-born students separated by baseline achievement, based on estimates from Equation 5.[46] Now, across baseline achievement there are similar patterns. Before DACA's enactment in 2012, there was little difference in achievement for immigrant students with higher and lower likelihoods of being undocumented. However, after 2012 likely undocumented students experienced significant increases in achievement. Estimates presented in Table 4 based on Equation 6 suggest that ELA performance increased by 0.11 (0.226) of a standard deviation for low (high) achievers and cumulative GPA increased by 0.18 points (off of a mean 1.89) for low-achievers. Table 4 reveals that the increases in achievement were fairly similar across gender and country of birth.[47]

[45]This scenario could occur if undocumented students on the margin of dropping out of high school did so because they no longer wanted to be enrolled in school, as opposed to being unable to meet graduation requirements.

[46]This summary index is computed as the equally weighted average of the z-scores of ELA standardized test performance and GPA. 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. I extend the number of cohorts to include 9th grade cohorts between 2005 and 2014. The event studies look similar if instead I focus on the main analysis sample (i.e. 9th graders enrolled between 2007 and 2014). I do not observe high school completion data for the 2005 and 2006 9th grade cohorts, so cannot similarly extend the number of cohorts for the educational attainment outcomes.

[47]Again, I do not focus on math achievement as a main outcome. Beginning in 8th grade students can choose the

Taken together, these results suggest that DACA led to significant increases in effort among all undocumented students. For low-achievers, DACA led to significant increases in educational attainment and effort, as measured by ELA performance and GPA. These increases in effort were either driven by necessity (i.e. working harder was required to meet graduation requirements) or due to increases in motivation. Regardless of the mechanism, I conclude that low-achieving undocumented youth were academically more motivated after DACA. For high-achievers, DACA did not impact educational attainment, but lead to significant increases in effort, as measured by ELA achievement. One possible explanation for this increase could be related to financial aid changes around this time that may have incentivized high-achievers to work harder. Specifically, undocumented youth became eligible for new merit-based financial aid opportunities in California that were tied to high school performance. Additionally, they could have been responding to the increased returns to a college degree with legalization as detailed above in Section 2.1.

One caveat of these findings is that DACA induced low-achievers to complete high school (as shown in Table 3). Thus, estimates of these intermediate outcomes that focus on students enrolled throughout high school may be subject to compositional changes (especially for the low-achieving subsample). While Panel B of Appendix Table A.2 reveals that there was not a significant change in the observable composition of likely undocumented students enrolled throughout high school, statistically insignificant compositional changes or those based on unobservables is still possible. If anything, this is likely to bias me against finding a positive effect of DACA on achievement. The fact that I identify improvements in achievement despite possible compositional changes, provides compelling evidence that effort among undocumented youth likely improved in response to DACA.

Finally, I consider whether DACA impacted behavioral outcomes. Table 4 reveals that DACA predicts increases in the likelihood of being disciplined among likely undocumented low-achievers and males, but had little impact on attendance for any subgroup. This increase in

sequence of math courses they take. Therefore, in a given grade students may be taking a different version of the math exam. With this caveat, Appendix Table A.3 shows that conditional on the type of math exam a student took, math achievement improved for high-achieving students. For the other subgroups, the coefficients are positive (suggesting a possible improvement), but these effects are not statistically significant.

disciplinary actions could reflect changes in behavior among likely undocumented (who may have been less scared to act-up in class due to their change in legal status), changes by teachers (who may have been more likely to discipline undocumented due to their legal status change),[48] or could be driven by the compositional changes just noted. Regardless of the mechanism, more misbehavior among undocumented youth would, if anything, likely negatively impact their US-born born peers.

5 Spillover Effects

5.1 Empirical Strategy

Ultimately, the main objective of this paper is to estimate the responsiveness of educational investments to changes in peer motivation. To do so, I leverage the introduction of DACA to determine whether the increased returns to schooling experienced by undocumented youth affected their US-born peers' outcomes. Specifically, I focus on the 2012 introduction of DACA, wherein the control group consists of US-born students without DACA-eligible peers, and the treatment effect varies across US-born students in the fraction of their peers who were DACA-eligible. Thus, to identify the spillover effects of DACA on high school completion and enrollment (outcomes occurring once), I estimate the following event-study equation on the sample of US-born students:

$$Y_{isc} = \alpha_0 + \sum_{j=2007}^{2014} \alpha_j \mathbb{1}\{j = c\} \times \text{DACAShare}_{sc} + \lambda_1 X_{isc} + \lambda_2 Z_{sc} + \gamma_s + \phi_c + \varepsilon_{isc} \tag{7}$$

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

[48] 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). However, I view this response by teachers as relatively unlikely since it is unclear whether teachers would know a student's immigration status, as federal law prohibits school districts or teachers from asking for proof of a student's immigration status.

school campus γ_s fixed effects to account for fixed cross-sectional differences across high school 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.[49] 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.[50]

For outcomes that vary yearly (i.e. academic and behavioral outcomes), I estimate the following slightly modified version of Equation 7 on the sample of US-born students:

$$Y_{isctg} = \pi_0 + \sum_{j=2005}^{2016} \pi_j \mathbb{1}\{j = t\} \times \text{DACAShare}_{sc} + \lambda_1 X_{isc} + \lambda_2 Z_{sc} + \phi_{sg} + \alpha_{tg} + \varepsilon_{isctg} \tag{9}$$

where Y_{isctg} is a yearly outcome from grade g in which the student was enrolled during year t. Now I interact the fixed concentration of the fraction of likely DACA-eligible peers with yearly dummy variables. ϕ_{sg} and α_{tg} are school-grade and year-grade fixed effects, and all other control variables 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 for each year separately.[51]

[49] 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.

[50] For table estimates, I also estimate a pre-post version of Equation 7:

$$Y_{isc} = \zeta_0 + \zeta_1(\text{DACAShare}_{sc} * \text{Exposed}_c) + \lambda_1 X_{isc} + \lambda_2 Z_{sc} + \gamma_s + \phi_c + \varepsilon_{isc} \tag{8}$$

where Exposed_c is an indicator for whether a student was *expected* to attend high school after DACA's enactment. Again, this indicator variable is equal to one for 9th grade cohorts entering 9th grade after 2009-10, and 0 otherwise. All other variables are as previously defined.

[51] Again, for table estimates, I estimate a pre-post version of Equation 9:

$$Y_{isctg} = \rho_0 + \rho_1(\text{DACAShare}_{sc} \times \text{Post}_t) + \lambda_1 X_{isc} + \lambda_2 Z_{sc} + \phi_{sg} + \alpha_{tg} + \varepsilon_{isctg} \tag{10}$$

where Post_t is an indicator variable which equals 1 if the outcome was measured after DACA's enactment in 2012. All other variables are as previously defined.

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. To test this assumption, I preform analogous checks to those used to test the validity of a similar parallel trends assumption required for the direct impacts results. First, event-study estimates presented in Section 5.2 provide evidence that US-born students with different concentration of DACA-eligible peers had similar pre-trends. Second, I show that observables do not predict a differential improvement in educational attainment for US-born youth with higher concentrations of undocumented peers after DACA. For the full sample shown in Panel A, Appendix Table A.4, shows that conditional on cohort and campus fixed effects, US-born students with more undocumented peers are not predicted to have a higher likelihood of graduating, higher baseline performance, or other demographic changes.[52] For those enrolled during all four years of high school, Panel B shows that there were small declines in the likelihood of graduating from high school (a reduction of 1 percentage point) and declines in baseline performance among students enrolled throughout all four years of high school. If anything, these declines suggest that the estimates on yearly outcomes may be biased downwards.

5.2 Results

Figure 4 shows the impact of attending high school with higher shares of undocumented peers on a summary index of educational attainment for US-born students separated by baseline achievement, based on estimates from Equation 7.[53] For low-achievers shown in Panel A, US-born students with different shares of DACA-eligible peers exhibit parallel trends in educational attainment leading up to DACA's introduction. However, educational attainment abruptly increases for 9th grade cohorts with more undocumented peers who were expected to be enrolled in high school after DACA's introduction. In contrast, for high-achievers shown in Panel B, there is little relationship between the concentration of likely undocumented peers and educational attainment across all 9th grade cohorts.

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

[53]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.

Leading up to DACA's introduction, there appears to be a positive trend among high-achievers[54] with no difference in outcomes among those with different shares of DACA-eligible peers after DACA's enactment. The fact that high-achievers did not respond to DACA's graduation incentives is a-priori expected, as high-achievers were likely to graduate from high school regardless of DACA.

Consistent with these event-studies, difference-in-differences estimates based on Equation 8 presented in Table 5 suggest significant increases in high school enrollment and completion for US-born students with higher shares of DACA-eligible peers after DACA's enactment, with increases driven by low-achievers. Column 1 starts with a model that only includes 9th grade cohort indicators and high school campus fixed effects, and Columns 2-3 successively add controls. My estimated effects are largely stable to choice of specification. In the fully specified model, these results suggest that US-born students at the average campus, where approximately 1 percent of students were likely to be undocumented, experienced a 2 p.p. (or 3 percent) increase in the likelihood of being enrolled in 12th grade and a 2 p.p. (or 4 percent) increase in the likelihood of high school completion. Results using a summary index also indicate an increase in educational attainment.[55] Columns 4-7 show that the increases in attainment are significantly larger for those who were lower-achieving at baseline, who are most on the margin of dropping out of high school. 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 12th grade enrollment and high school completion, respectively.

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 shows the impact of DACA-eligible peers on a summary index of academic achievement for the sample of US-born students separated by baseline achievement, based on estimates from Equation 9.[56] For low-achievers shown in Panel A, before DACA's enactment in 2012 there was

[54]However, without more years of pre-policy data it is not possible to rule out the possibility that the coefficient for the 2006-07 9th grade cohort may be an outlier.

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

[56]This summary index is computed as the equally weighted average of the z-scores of ELA standardized test

little difference in achievement between US-born students with more vs. fewer undocumented peers. After DACA's enactment in 2012, students with higher shares of undocumented peers experienced significant improvements in academic achievement. The patterns are largely similar for the high-achievers shown in Panel B, however, there does appear to be a positive trend in achievement between 2005 and 2008 that largely appears to level off three years before DACA's introduction.

Consistent with these event-studies, difference-in-differences estimates based on Equation 10 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, I find that US-born students with the average number of likely undocumented peers (1 percent) experienced a 0.05 point increase in their GPA (off of a mean of 2.33) and a a 0.06 standard deviation increase in ELA achievement after DACA's enactment. In addition, results using a summary index of academic achievement also indicate an improvement in achievement.[57]

Columns 4-7 of Table 6 demonstrate increases in achievement for US-born students across the baseline achievement distribution. The increases in cumulative GPA are slightly larger for lower-achievers, and the increases in ELA are slightly larger for low-achievers. These were the same heterogeneity patterns observed for the direct impacts presented in Section 4.2. I find no impact on attendance rates or the likelihood of being disciplined for any subgroup of US-born students. Again, due to compositional changes (i.e. low-achieving US-born students were more likely to stay enrolled in high school), the fact that I identify improvements in achievement despite this change, provides compelling evidence that US-born students' effort likely improved in response to their undocumented peers investing more in their education.

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. The results that focus on each outcome separately present similar patterns to Figure 5.

[57] As previously noted, after 8th grade students can choose what version of the math exam they take during each grade so I do not focus on math scores as a main outcome. However, I do find evidence that conditional on the math exam they took, math scores among US-born students improved as a consequence of having more DACA-eligible peers in Appendix Table A.5. The increases are driven by the same subgroups that drove the increases in ELA achievement.

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 driven by Black, Hispanic and males. In terms of high school completion, the positive spillover effects are driven by Black students. Table 8 focuses on heterogeneity among US-born students for the yearly outcomes. Hispanics experienced the largest increases in ELA performance and GPA. Across gender, I estimate similar increases in ELA achievement and GPA.

5.3 Discussion

I find that exposure to more academically motivated undocumented peers (due to DACA) had positive spillovers on US-born students. This finding shows that policies such as DACA that increase the returns to schooling for one group can have important spillovers on unaffected groups. The size of the spillover effects on high school enrollment, completion, and performance are roughly 50, 40, and 40 percent of the direct effects on likely undocumented students.[58] These relative effect sizes are quite similar to Abramitzky et al. (2021), who find that the spillover effects of a pay reform change in Israel that increased the returns to schooling for kibbutz members on high school achievement of non-kibbutz members was 50 percent of the direct effects on kibbutz members.

Given these sizable spillovers it is important to consider what is responsible for driving these effects. The positive spillover effects of DACA on US-born students are consistent with several possible mechanisms. First, US-born students may have been affected by direct peer-to-peer influences: increased effort among DACA-eligible students may have inspired their US-born peers to study harder. Second, improvements in undocumented youths' motivation may have freed up teachers' and administrators' time for other instructional improvements. Finally, the introduction of DACA may have led to other administrative changes at the school level. For instance, if schools trained guidance counselors to better understand the process of college admissions for DACA-eligible students, this training could have spilled over to their US-born peers.

Overall, I find evidence consistent with the spillovers being driven by peer-to-peer interac-

[58] This is computed based on the average effect size for the direct and spillover effects.

tion, rather than other school level changes. First, the heterogeneous results suggest that the direct and spillover effects occurred for students who shared the same gender and baseline achievement. In terms of educational attainment, the direct and spillover effects of DACA were driven by males and low-achievers. As low-achieving male students are more likely to interact with one another, this is precisely the group of US-born students expected to have experienced the largest spillovers in terms of attainment. Similarly, for ELA (GPA) achievement, the direct and spillover effects were largest for those who were higher (lower) achieving at baseline. As high-achieving (low-achieving) students are more likely to interact with one another, this is precisely the group of US-born students expected to have experienced the largest positive spillovers in terms of ELA achievement.

I also find that the spillover effects are driven by closer contacts which is consistent with peer-to-peer interactions, rather than changes in the classroom or school-wide dynamics. If these other school-level changes were driving the spillovers, closer contacts should not have a differential effect. To define closer contacts I focus on students from the same middle school since they are likely to have closer ties and longer-lasting friendships. The results in Appendix Table A.6 indicate that it is likely undocumented students from the same middle school that are driving the positive spillover effects on educational outcomes.[59] This provides additional evidence consistent with spillovers being primarily driven by peer-to-peer interactions, rather than school-level changes.[60]

Finally, it is important to acknowledge that DACA predicted increases in high school enrollment for low-achieving undocumented students (as shown in Section 4.2). This could have harmed US-born students if the composition of peers enrolled throughout high school (after 9th grade) declined in a way that disrupted learning (e.g. Carrell et al., 2018). But these compositional changes could have also improved lower-achieving students' outcomes if they benefitted from having more students with similar ability in classrooms (for instance if teachers tailored their

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

[60]In terms of investigating teacher turnover, I am limited by the fact that I can only track teacher turnover between 2013 and 2017 (which is only during the post-policy period). Nonetheless, Appendix Figure A.10 presents event-study estimates where the outcome is the fraction of teachers who left a high school campus in a given year. This plot shows no differential trend in teacher turnover in high school campuses with different concentrations of undocumented students. This provides further suggestive evidence inconsistent with school-wide dynamics driving the spillover effects.

instruction in a way that benefitted low-achieving students (e.g. Banerjee et al., 2016). While these policy-induced compositional changes are important to acknowledge, I view it as unlikely that these compositional changes alone are driving the spillovers. First, the compositional changes predicted by DACA before high school completion were relatively small. Out of the 281,046 enrolled students in my sample, DACA induced 274 additional undocumented students to stay enrolled in expected 10th grade.[61] Second, by the time of high school, low and high achievers often receive instruction in separate classrooms (e.g. honors classes). Despite no compositional changes among undocumented high achievers, US-born achievers experienced positive increases in performance, suggesting that there were other mechanisms beyond compositional changes. Finally compositional changes in school, may not necessarily translate to changes in peer networks since high school students may have friends enrolled in high school and also others who have already dropped out.

5.4 Robustness

The measure I use to approximate undocumented status is likely measured with minimal measurement error due to the high take-up of DACA in Los Angeles. Nonetheless, to alleviate any potential concerns regarding measurement error, Appendix Table A.7 shows that the spillover effects of DACA on US-born students are robust to using different measures to approximate the fraction of undocumented peers. Column 1 reports my baseline estimates that scale the fraction of Hispanic foreign-born peers (who arrived by age 9) by the zip-code DACA-application rate (i.e. the second term in Equation 2). Column 2 scales the fraction of foreign-born peers by the high-school aged DACA applicants.[62] Turning to the measures that do not select on the DACA application decision, Column 3 scales the fraction of foreign-born peers by the fraction of non-citizens making a number of statistical adjustments to more accurately identify the undocumented population living in a

[61] The increases were larger later during high school. DACA induced 845 additional undocumented students to stay enrolled in LAUSD in expected 11th grade, and 951 additional undocumented students to stay enrolled in LAUSD in expected 12th grade.

[62] Specifically, for each zip code, I take the total number of DACA applicants and multiply by 0.40, since 40% of DACA applicants in Los Angeles county were ages 15-19 according to official USCIS estimates (USCIS, 2014), and then divide by the number of foreign-born ages 15-19 in a zip-code using 5-year ACS estimates from 2014.

PUMA[63] and Column 4 scales by the fraction of non-citizens in a zip-code.[64] Reassuringly, I come to similar conclusions regarding DACA's spillovers regardless for how I proxy for likely undocumented status.[65]

Finally, Column 5 of Appendix Table A.7 shows estimates that use the fraction of foreign-born peers without accounting for likely undocumented status to define peer exposure (i.e. I do not include second term in Equation 2). The insignificant estimates in Column 5 provide compelling evidence that my estimates are not picking up peer effects stemming from having more foreign-born peers after 2012. The fact that the estimates in this table are only significant after proxying for the likelihood that these foreign-born peers are undocumented, suggest that I am instead able to capture the peer effects stemming from having more undocumented peers after DACA's enactment.

The key identification assumption in this analysis is that US-students in campuses with greater shares of undocumented students had similar counterfactual trends relative to US-born students in campuses with fewer shares of undocumented students. I have already presented evidence in support of this assumption in Section 5.1 and with the event studies in Section 5.2. However, an additional concern is that population differences by campus share of undocumented students could have led to a later divergence in trends.[66] To rule out this possibility, I re-estimate my models including time trends interacted with campus demographics at baseline (in the 2011-12 school year). Appendix Table A.9 demonstrates that my peer effect results on attainment and achievement are robust to the inclusion of time trends interacted with the baseline fraction of FRL students, ELL students, ELA achievement (measured in 8th grade), and total cohort size. In terms of ELA Achievement (Panel C) the results are also robust to the inclusion of time trends interacted

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

[64]As previously noted, using foreign-born non-citizens is the most common way to approximate the undocumented population in the literature on DACA (e.g. Pope, 2016a; Kuka et al., 2020; Amuedo-Dorantes & Antman, 2017).

[65]Similarly, Appendix Table A.8 demonstrates that the direct impacts of DACA are largely robust to using several different measures to approximate undocumented status that do not select on the DACA application decision.

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

with the baseline fraction of students belonging to each racial grouping (Hispanic, Black, White, and Asian). In terms of educational attainment (Panels A-B), the results are no longer significant with the inclusion of time trends interacted with the fraction of a campus belonging to each racial group. However, the point estimates are positive and of similar magnitude to the baseline estimates (shown in Column 1), suggesting a similar conclusion. Taken together, these results help to rule out the possibility that differential trends driven by demographic differences are driving my results.

The other key assumption is that there were no contemporaneous shocks that differentially impacted high school campuses in a way that correlates with the share of undocumented students. To address this, I consider other immigration and education policies impacting LAUSD students during this time period. In terms of immigration policies likely to affect undocumented students, the only major policy change that I am aware of is the introduction of the California Dream Act in 2012. As previously noted, this policy allowed undocumented students to participate in state-funded financial aid programs. This in part can likely explain why I document increases in achievement among those likely undocumented students who were higher achieving at baseline, and who had been likely to graduate high school regardless of the introduction DACA. Overall, this policy change (introduced in the same year as DACA) is similar to DACA in that it may make investing in higher education more attractive.

In terms of education policies, in the summer of 2013, LAUSD introduced policies that significantly reduced overall suspensions.[67] Beginning in 2015, students were no longer required to pass the high school exit exam in order to graduate.[68] And the introduction of online credit-recovery courses around this time has anecdotally been linked to increases in graduation rates.[69] If schools

[67] 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.

[68] 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.

[69] 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).

with higher concentrations of DACA-eligible students were also more likely to be impacted by these changes in discipline or graduation requirements, then I could be misattributing the increases in educational outcomes to DACA. Reassuringly, Appendix Table A.10 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.[70]

To more formally rule out the possibility that alternate educational policies are driving my results, I estimate Equation 8 including campus-level time trends that vary by the fraction of students who were unable to pass the high school exit exam on their first attempt in 2012, who were suspended in 2012, and who graduated high school during the pre-policy period. Appendix Table A.11 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 A.12 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

[70]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.

likely to graduate. However, all US-born students experienced improvements in achievement. Because decreasing graduation requirements alone should not led to improvements in achievement, I view it as unlikely that changes to graduation requirements alone can explain the increases in educational investments that I document. Columns 4-5 of Appendix Table A.12 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.[71]

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.[72] 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 A.10 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.

[71] 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.

[72] 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.

To further rule out the possibility that my spillover results are being driven by the reduction in ELL participation, I reestimate Equations 8 and 10 including campus-level time trends that vary by the fraction of 9th grade students receiving ELL services in 2007. Column 7 of Appendix Table A.11 demonstrate that the estimates are robust to the inclusion of this trend. In addition, Columns 6-7 of Appendix Table A.12 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.

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 the increases in peer motivation of undocumented youth due to DACA had any impact on their peers' educational investments. To estimate whether DACA had positive spillovers on US-born students, I leverage variation in the concentration of DACA-eligible youth across Los Angeles schools and compare the educational outcomes of US-born students in high schools with higher concentrations of DACA-eligible peers to those in high schools with lower concentrations before and after DACA's enactment.

My results indicate that DACA increased educational attainment among undocumented students and their in-eligible US-born peers. I find that among likely undocumented youth DACA increased 12th grade enrollment by 6 percent, high school graduation by 10 percent, ELA achievement by 0.14 standard deviations, and GPA by 0.127 percentage points (off of a mean of 2.26). Among

US-born students at the average campus, where approximately 1 percent of students were likely to be undocumented, I also find that DACA increased 12th grade enrollment by 3 percent, high school graduation by 4 percent and ELA achievement by 0.06 standard deviations. These results are robust to a number of specification checks, including compositional changes and differences in trends across the types of campuses that have more or fewer concentrations of undocumented students.

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

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

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

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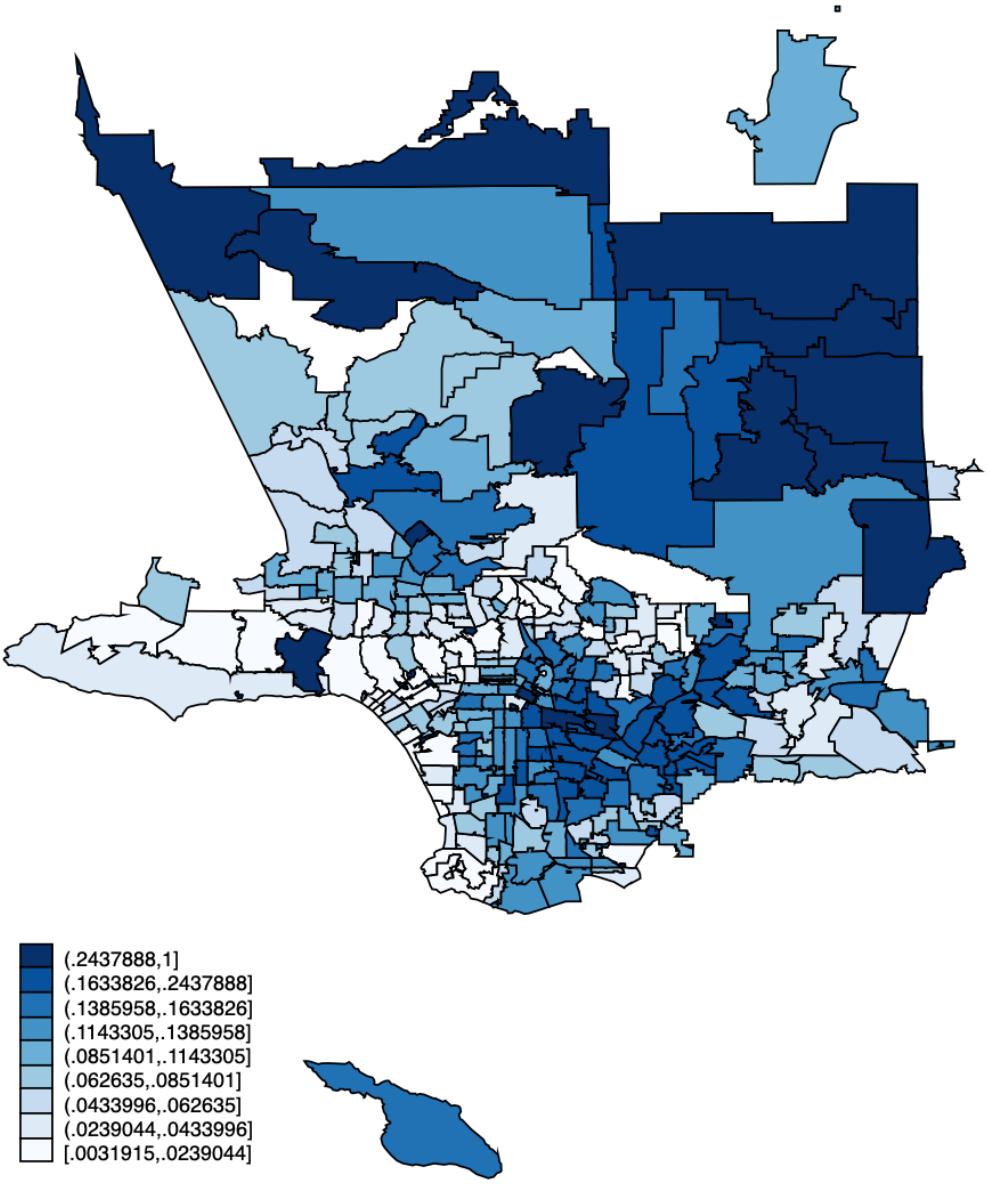
Figures/Tables

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

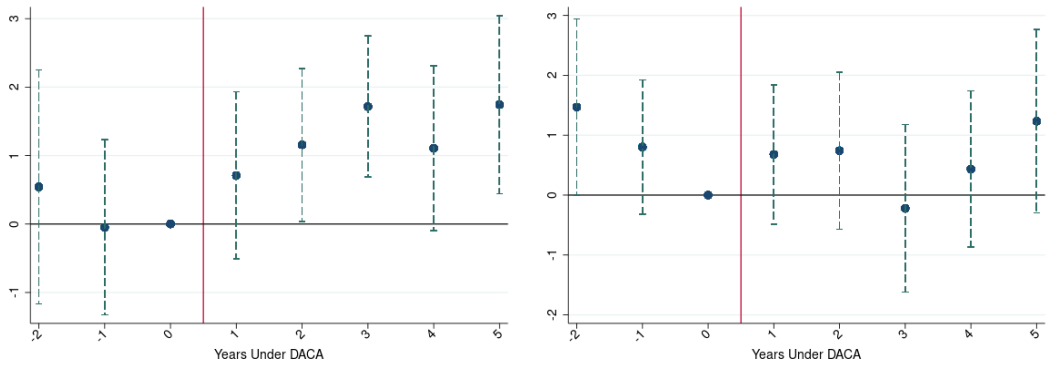


Note: This plot shows the share of foreign-born individuals who applied to DACA in each Los Angeles zip code using USCIS data. This is computed using Equation 1. For each zip-code, I take the total number of DACA applicants between July 2012-December 2013 and then, I divide by the number of foreign-born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

Baseline Achievement Distribution

(a) Bottom 50 Percentile **(b)** Top 50 Percentile

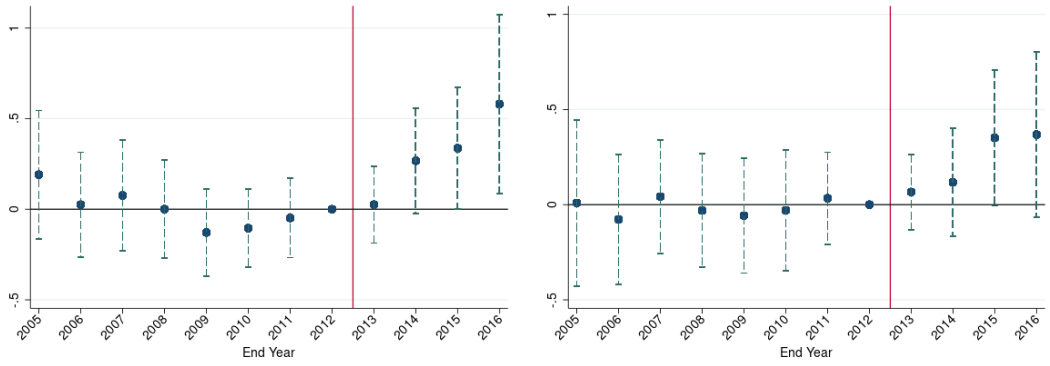


Note: These figures plot coefficients from Equation 3 and 95% confidence intervals. The dependent variable is a summary index based on *expected* enrollment in each grade and high school completion. Event time is computed by subtracting 12 from the grade each 9th grade cohort was expected to be enrolled 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

Baseline Achievement Distribution

(a) Bottom 50 Percent **(b)** Top 50 Percent

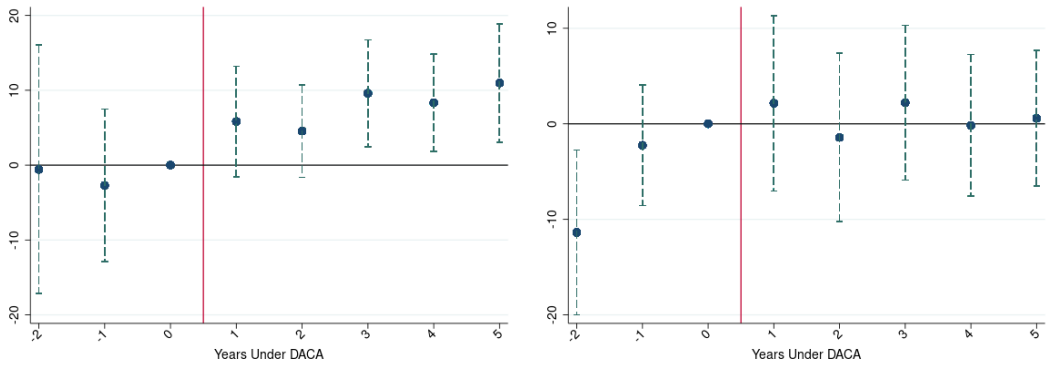


Note: These figures plot coefficients from Equation 5 and 95% confidence intervals. The dependent variable is a summary index based on GPA and ELA standardized exam performance. The sample includes Mexican immigrants who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14. Baseline 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

Baseline Achievement Distribution

(a) Bottom Quartile **(b)** Top Quartile

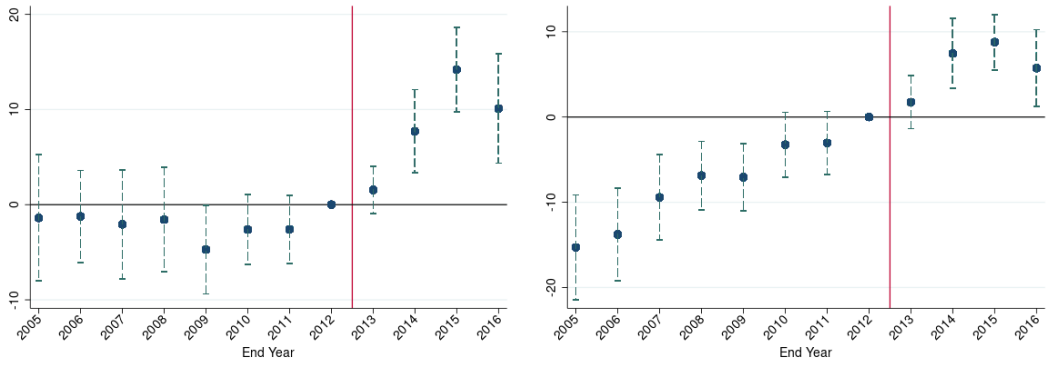


Note: These figures plot coefficients from Equation 7 and 95% confidence intervals. The dependent variable is a summary index based on enrollment in each grade and high school completion. Event time is computed by subtracting 12 from the grade each 9th grade cohort was expected to be enrolled in during 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

Baseline Achievement Distribution

(a) Bottom Quartile **(b)** Top Quartile



Note: These figures plot coefficients from Equation 9 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 2004-05 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

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

Note: This table presents summary statistics for students in 9th grade cohorts between 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
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 Los Angeles Unified school district. The first column includes the full sample, Columns 2-5 separate students based on the fraction of one's peers estimated to be DACA-eligible using Equation 2. 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 3: The Direct Impact of DACA on Educational Attainment, Foreign-born Hispanics

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

Note: This table shows difference-in-differences estimates of the direct impact of DACA on high school enrollment and on-time graduation. Within each panel, each column reports estimates of ϑ_1 from a separate regression of Equation 4. The sample for these regressions are foreign-born Hispanic students who were in 9th grade cohorts from 2006-07 to 2013-14 who arrived to the US by age 9. Individual demographic controls include age of arrival to the US, country of origin indicators, gender, and whether a student received special education services. District demographic cohort controls include the percentage of students in the cohort belonging to each racial group, receiving special education, and who are male. The effect for the fully exposed student living in a zip-code where 25 percent of the foreign-born population applied to DACA is shown in brackets, and is defined as the coefficient multiplied by 0.25. Standard errors in parentheses are clustered at the 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	
	Full	Mexican	Female	Male	Bottom 50	Top 50
Panel A: Yearly Attendance Rate (Grades 9-12)						
ShareEligible*Post	-0.0135	-0.0228	0.0394	-0.0708*	-0.0385	0.0187
	(0.0248)	(0.0238)	(0.0319)	(0.0383)	(0.0346)	(0.0272)
	[-0.003]	[-0.006]	[0.010]	[-0.018]	[-0.010]	[0.005]
Mean (Y)	0.936	0.936	0.935	0.938	0.922	0.953
Observations	71,811	58,489	35,334	36,477	39,394	32,417
Panel B: Ever Disciplined (Grades 9-12)						
ShareEligible*Post	0.104***	0.101***	0.0727	0.137***	0.176***	0.0464
	(0.0355)	(0.0363)	(0.0482)	(0.0511)	(0.0521)	(0.0379)
	[0.026]	[0.025]	[0.018]	[0.034]	[0.044]	[0.012]
Mean (Y)	0.0334	0.0337	0.0218	0.0446	0.0423	0.0222
Observations	75,155	61,308	36,995	38,160	41,695	33,460
Panel C: Cumulative GPA (Grades 9-12)						
ShareEligible*Post	0.508**	0.589**	0.786***	0.323	0.727**	0.324
	(0.242)	(0.255)	(0.277)	(0.392)	(0.287)	(0.357)
	[0.127]	[0.147]	[0.197]	[0.081]	[0.182]	[0.081]
Mean (Y)	2.262	2.232	2.428	2.101	1.889	2.717
Observations	72308	58982	35644	36664	39728	32580
Panel D: Standardized ELA Exam Performance (Grades 9-11)						
ShareEligible*Post	0.553**	0.525**	0.615**	0.685**	0.444*	0.902***
	(0.237)	(0.256)	(0.238)	(0.286)	(0.225)	(0.326)
	[0.138]	[0.131]	[0.154]	[0.071]	[0.111]	[0.226]
Mean (Y)	-0.0922	-0.121	-0.0275	-0.156	-0.613	0.506
Observations	43,153	35,511	21,420	21,733	23,069	20,084
Panel E: Summary Achievement Index (Grades 9-11)						
ShareEligible*Post	0.836***	0.876***	1.056***	0.738*	0.924***	0.808**
	(0.261)	(0.273)	(0.263)	(0.387)	(0.298)	(0.370)
	[0.209]	[0.219]	[0.264]	[0.185]	[0.231]	[0.202]
Mean (Y)	-0.0354	-0.0647	0.103	-0.169	-0.494	0.542
Observations	56,910	46,435	27,955	28,955	31,727	25,183

Note: This table shows difference-in-differences estimates of the direct impact of DACA on yearly attendance rates (Obs= 71,811), indicators for ever being disciplined (i.e. in or out of school suspensions only) (Obs=75,155), cumulative GPA (Obs=72,308), and standardized ELA test performance (Obs=43,153), as well as a summary index based on the outcomes in Panels C-D (N=56,910). Within each panel, each column reports estimates of υ_1 from a separate regression of Equation 6. 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. 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. See Table 3 for more detail on the control variables. The effect for the fully exposed student living in a zip-code where 25 percent of the foreign-born population applied to DACA is shown in brackets, and is defined as the coefficient multiplied by 0.25. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

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

				8th Grade ELA Test Score Quartiles			
	All Students			($\leq$ 25)	(25 - 50)	(50 - 75)	$\geq$ 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)
	[0.010]	[0.010]	[0.008]	[0.012]	[0.008]	[0.004]	[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)
	[0.018]	[0.019]	[0.019]	[0.025]	[0.017]	[0.023]	[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)
	[0.025]	[0.027]	[0.026]	[0.048]	[0.023]	[0.019]	[0.018]
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)
	[0.023]	[0.026]	[0.024]	[0.032]	[0.026]	[0.009]	[0.021]
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)
	[0.056]	[0.061]	[0.059]	[0.088]	[0.057]	[0.043]	[0.042]
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 estimates of ζ_1 from a separate regression of Equation 8. 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 and 9th grade 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 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 6: The Effect of DACA on Yearly Outcomes, US-Born Students

	All Students			8th Grade ELA Test Score Quartiles			
				($\leq$ 25)	(25 - 50)	(50 - 75)	$\geq$ 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]	[0.002]
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]	[0.008]	[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)
	[0.042]	[0.046]	[0.046]	[0.068]	[0.060]	[0.030]	[0.036]
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)
	[0.050]	[0.065]	[0.065]	[0.039]	[0.073]	[0.057]	[0.077]
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)
	[0.079]	[0.083]	[0.083]	[0.060]	[0.075]	[0.051]	[0.060]
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 estimates of ρ_1 from a separate regression of Equation 10. 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. 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 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 7: The Heterogenous Effects of DACA on School Attendance and High School Completion, US-born students

	Full	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)
	[0.008]	[0.023]	[0.008]	[0.025]	[0.008]	[0.008]
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)
	[0.019]	[0.042]	[0.016]	[0.051]	[0.014]	[0.024]
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)
	[0.026]	[0.057]	[0.020]	[0.046]	[0.020]	[0.032]
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)
	[0.024]	[0.046]	[0.011]	[0.037]	[0.023]	[0.025]
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)
	[0.059]	[0.126]	[0.041]	[0.121]	[0.049]	[0.068]
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 of ζ_1 from a separate regression of Equation 8. 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 of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

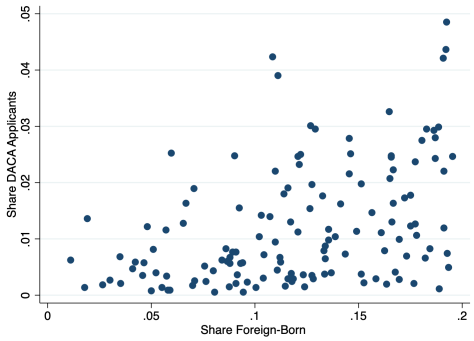
Table 8: The Heterogenous Effects of DACA on Yearly Outcomes US-born students

	Full	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]	[0.005]	[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)
	[0.046]	[0.030]	[0.043]	[-0.002]	[0.050]	[0.041]
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)
	[0.065]	[0.032]	[0.055]	[-0.016]	[0.066]	[0.065]
Mean (Y)	0.0664	-0.138	-0.0150	0.752	0.159	-0.0242
Observations	49,0051	43,671	388,816	30,346	242,586	247,465
Panel E: Summary Index (Grades 9-11)						
DACAShare*Post	6.331***	3.044	6.213***	-3.019	7.318***	5.335***
	(0.890)	(1.839)	(0.958)	(3.901)	(0.964)	(1.077)
	[0.063]	[0.030]	[0.062]	[-0.030]	[0.073]	[0.053]
Observations	810,909	74,633	643,125	49,694	398,849	412,060

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on yearly outcomes, as well as a summary index based on the outcomes in Panels C-D. Within each panel, each column reports estimates of ρ_1 from a separate regression of Equation 10. 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. All regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

A Appendix

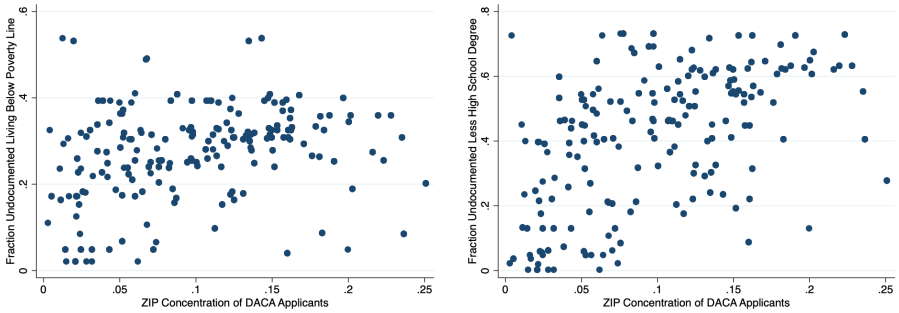
Figure A.1: Correlation Between Zip Share of Foreign-born Youth and DACA applicants



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

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

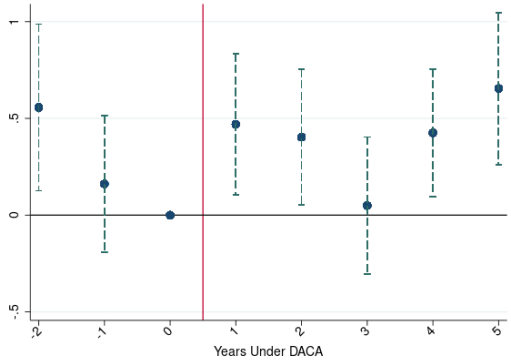
(a) Poverty Rate **(b)** Less than High School Degree



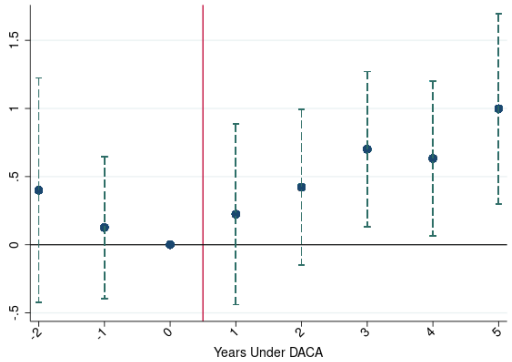
Note: Each dot of the scatter plots represents a zip-code. The x-axis is the share of foreign-born individuals who applied to DACA in each Los Angeles zip code (ShareEligible_z) computed using Equation 1. The y-axis is the share of the likely undocumented population (over 18 years old) living in a zip-code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

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

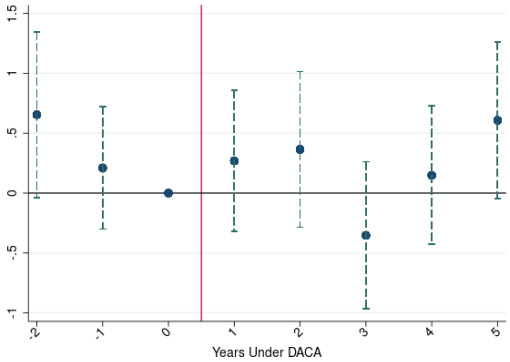
(a) All



(b) Mexican - Bottom 50th Baseline Achievement Percentile



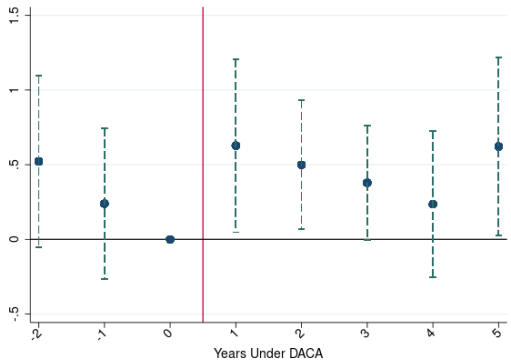
(c) Mexican -Top 50th Baseline Achievement Percentile



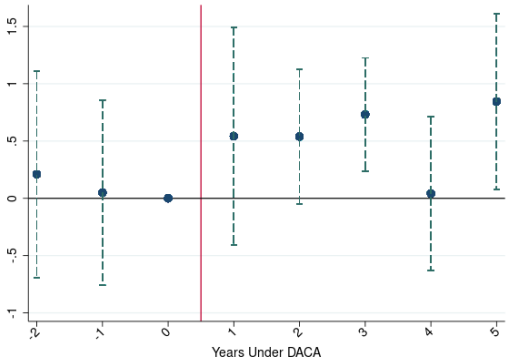
Note: These figures plot coefficients from Equation 3 and 95% confidence intervals. The dependent variable is an indicator for *expected* 12th grade enrollment (defined as enrollment 4 years after 9th grade). The sample includes foreign-born Hispanic students who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 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: Event Study Estimates of the Impact of DACA on High School Completion, Foreign-born Hispanics

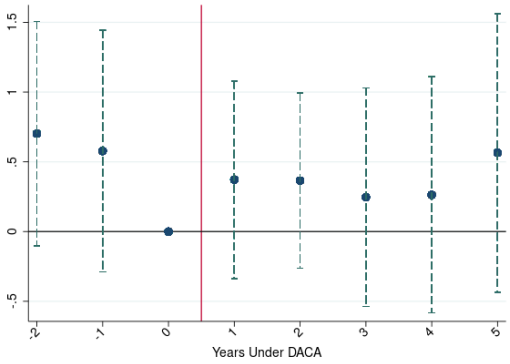
(a) All



(b) Mexican - Bottom 50th Baseline Achievement Percentile



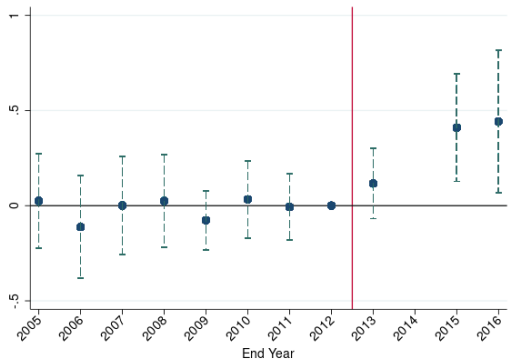
(c) Mexican - Top 50th Baseline Achievement Percentile



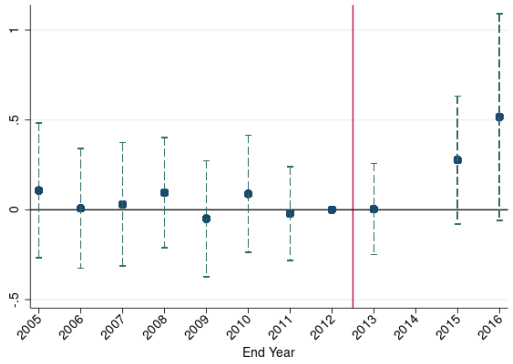
Note: These figures plot coefficients from Equation 3 and 95% confidence intervals. The dependent variable is an indicator for high school completion, which is an indicator equal to one if a student graduated from high school within four years of 9th grade. The sample includes foreign-born Hispanic students who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 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: Event Study Estimates of the Direct Impact of DACA on ELA Performance, Foreign-born Hispanics

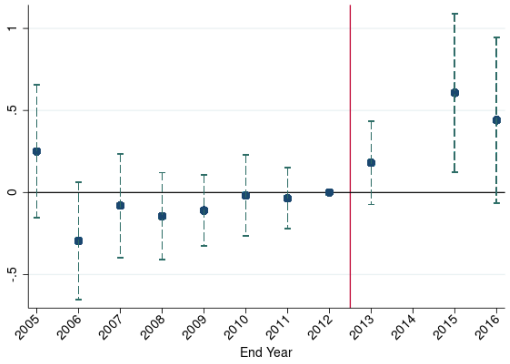
(a) All



(b) Mexican - Bottom 50th Baseline Achievement Percentile



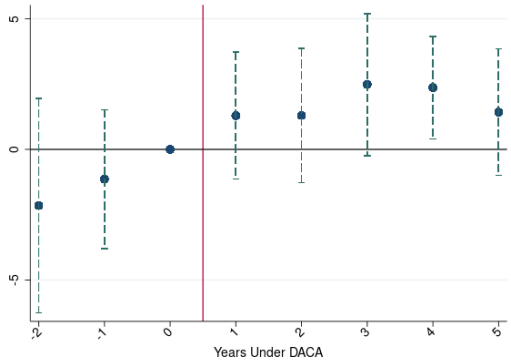
(c) Mexican - Top 50th Baseline Achievement Percentile



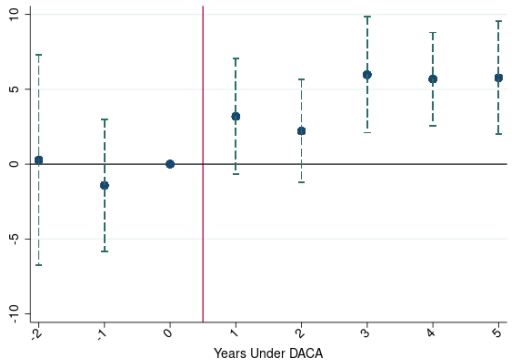
Note: These figures plot coefficients from Equation 5 and 95% confidence intervals. The dependent variable is performance on the ELA standardized exam. The sample includes foreign-born Hispanic students who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14, and focus on yearly outcomes within 3 years of 9th 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: Event Study Estimates of the Spillover Effects of DACA on 12th Grade Enrollment, US-born Students

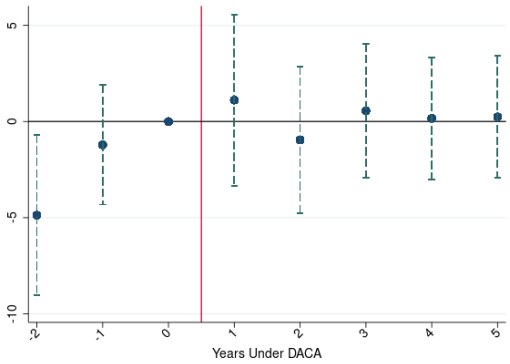
(a) All



(b) Bottom Baseline Achievement Quartile



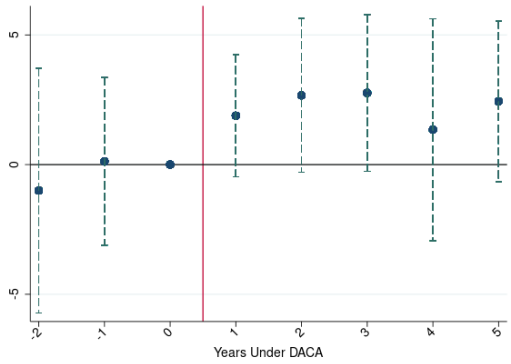
(c) Top Baseline Achievement Quartile



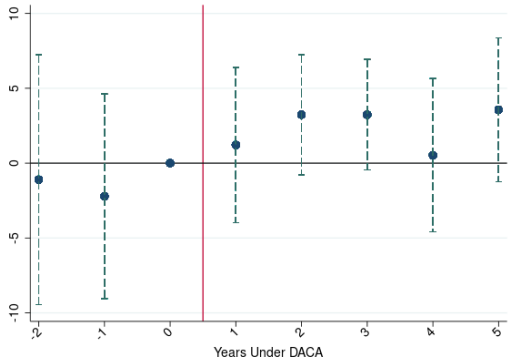
Note: These figures plot coefficients from Equation 7 and 95% confidence intervals. The dependent variable is an indicator for *expected* 12th grade enrollment (defined as enrollment 4 years after 9th grade). The subsample is shown in the sub-figure labels. Event time is computed by subtracting 12 from 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: Event Study Estimates of the Spillover Effects of DACA on High School Completion, US-born Students

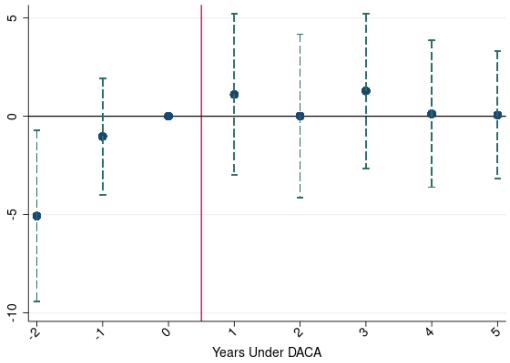
(a) All



(b) Bottom Baseline Achievement Quartile



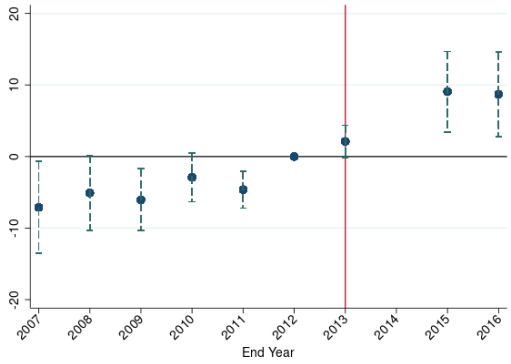
(c) Top Baseline Achievement Quartile



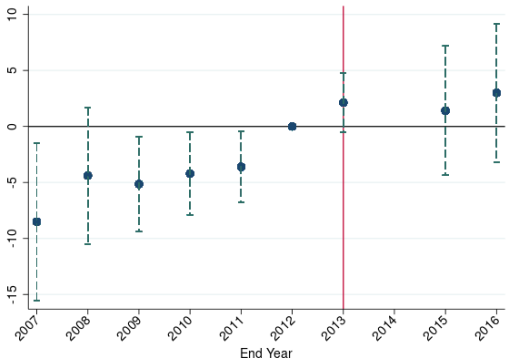
Note: These figures plot coefficients from Equation 7 and 95% confidence intervals. The dependent variable is an indicator for high school completion, which is an indicator equal to one if a student graduated from high school within four years of 9th grade. The subsample used is shown in the sub-figure 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: Event Study Estimates of the Spillover Effects of DACA on ELA Standardized Test-Performance, US-born Students

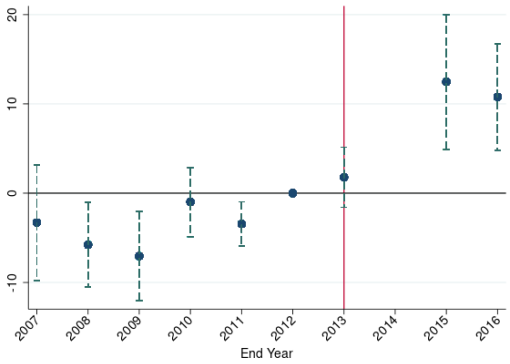
(a) All



(b) Bottom Baseline Achievement Quartile



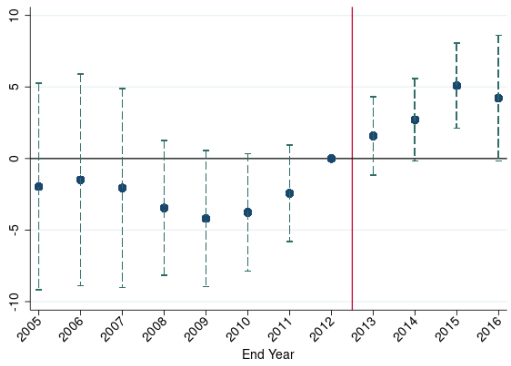
(c) Top Baseline Achievement Quartile



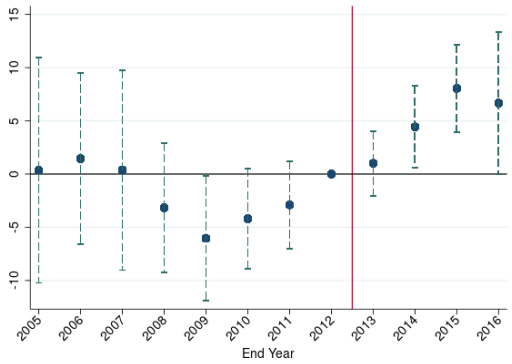
Note: These figures plot coefficients from Equation 9 and 95% confidence intervals. The dependent variable is performance on the ELA standardized exam. The sample includes US-born students in 9th grade cohorts between 2004-05 to 2013-14, and focus on yearly outcomes within 3 years of 9th grade. The sub-sample 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: Event Study Estimates of the Impact of DACA on Semester GPA, US-born Students

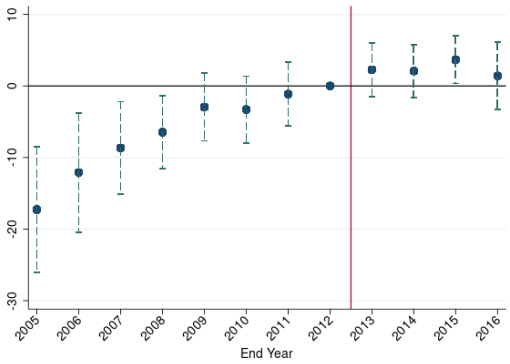
(a) All



(b) Bottom Baseline Achievement Quartile

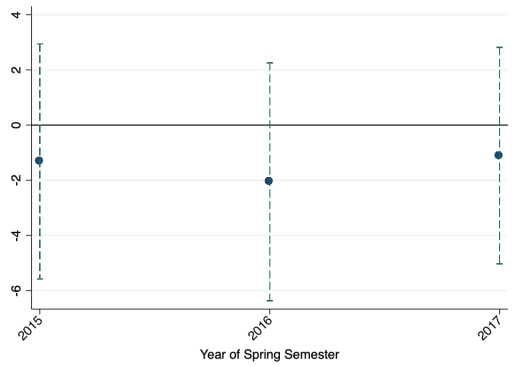


(c) Top Baseline Achievement Quartile



Note: These figures plot coefficients from Equation 9 and 95% confidence intervals. The dependent variable is GPA. The sample includes US-born students in 9th grade cohorts between 2004-05 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]	[0.023]	[0.019]	[-0.009]	[0.014]	[0.075]	[0.106]	[0.106]
Mean (Y)	0.564	0.507	5.880	0.0720	0.816	-0.217	-0.193	-0.0775
N	21,139	21,139	21,139	21,139	21,139	21,139	20,169	20,157
Panel B: 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)
	[-0.010]	[0.099]	[0.047]	[-0.001]	[0.018]	[-0.015]	[0.048]	[0.082]
Mean (Y)	0.512	0.506	5.856	0.0516	0.815	-0.145	-0.129	-0.00870
N	16,375	16,383	16,383	16,383	16,383	16,383	15,741	15,734

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

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

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

Note: This table shows difference-in-differences estimates of the direct impact of DACA on yearly math achievement. Each column reports estimates of υ_1 from a separate regression of Equation 6. 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 effect for the fully exposed student living in a zip-code where 25 percent of the foreign-born population applied to DACA is shown in brackets, and is defined as the coefficient multiplied by 0.25. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

Table A.4: The Effect of DACA on Predicted High School Completion and Exogenous Student Characteristics, US-Born Students

	Predicted HS Grad	Black	Hispanic	Male	Free-Lunch	Special Education	ELA (G8)	ELA (G7)	Math (G7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full Sample									
DACAShare*	-0.505	1.160**	-0.898	-0.324	-0.005	0.101	-1.290	-0.722	2.136
Exposed	(0.338)	(0.487)	(0.785)	(0.451)	(1.218)	(0.389)	(1.869)	(1.895)	(2.147)
	[-0.005]	[0.012]	[-0.009]	[-0.003]	[0.000]	[-0.001]	[-0.013]	[-0.007]	[0.021]
Mean (Y)	0.547	0.103	0.781	0.510	0.695	0.087	-0.046	-0.008	0.049
N	238,781	238,781	238,781	238,781	238,781	238,781	238,781	224,625	224,701
Panel B: 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]	[0.038]	[0.006]	[0.010]	[-0.007]	[-0.087]	[-0.080]	[-0.064]
Mean (Y)	0.547	0.0867	0.797	0.506	0.702	0.177	0.0299	0.0584	0.125
N	184,170	184,170	184,170	184,170	184,170	184,170	184,170	176,071	176,167

Note: This table contains results obtained from regressing predicted high school completion and student demographics on $\text{DACAShare}_{sc} \times \text{Exposed}_{c}$. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. Panel A focuses on the full sample, while Banel B restricts the sample to those who were enrolled in high school for all four years. The demographic variables are measured as of 9th grade. All regressions include 9th grade campus and cohort fixed effects. See Table 5 for more detail on the sample. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.5: The Heterogenous Effects of DACA on Math Test Scores, US-born students

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

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on yearly math achievement. Each column reports estimates of ρ_1 from a separate regression of Equation 10. 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 of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.6: The Spillover Effects of DACA by Type of Undocumented Peer, US-Born Students

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

Note: This table contains difference-in-difference estimates of the spillover effects of DACA on a summary index of educational attainment (Columns 1-2) and a summary index of academic achievement (Columns 3-4). Each column reports results from a separate regression. Columns 1 shows estimates of ζ_1 from Equation 8 and 3 shows estimates of ρ_1 from Equation 10. Columns 2 and 4 show similar estimates but the treatment variable is separately defined by whether or not the undocumented 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. For columns 1 and 3, the effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.7: The 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 estimates of ζ_1 from a separate regression of Equation 8. In Panel C, each column reports estimates of ρ_1 from a separate regression of Equation 10. 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 is shown in brackets, and is defined as the coefficient multiplied by the mean estimated value of undocumented peers (shown in the last row of this table). Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

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

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

Note: This table contains difference-in-differences estimates where undocumented status is approximated in several different ways. Column 1 uses Equation 1 to approximate undocumented status (i.e. my preferred specification), Column 2 uses a modified version of Equation 1 that accounts for the fraction of DACA-applicants estimated to be high-school aged, Column 3 uses the fraction of the foreign-born population ages 1-18 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 estimates of ϑ_1 from a separate regression of Equation 4. In Panel C, each column reports estimates of υ_1 from a separate regression of Equation 6. See Table 3 for more detail on the full set of controls and sample in Panels A-B and see Table 4 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 foreign-born student are shown in brackets, and is defined as the coefficient multiplied by the mean fraction of foreign-born estimated to be undocumented in a given zip-code (shown in the last row of this table). Standard errors in parentheses are clustered at the residence zip-code level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.9: Peer Effects of DACA on Educational Attainment and Achievement – Accounting for Differences in Campus-Level Characteristics, US-born Students

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

Notes: This table shows difference-in-differences estimates of the spillover effects of DACA on high school enrollment and graduation, as well as on yearly standardized test performance on ELA exams. These models use the full set of controls specified in 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 estimates of ζ_1 from a separate regression of Equation 8. In Panel C, each column reports estimates of ρ_1 from a separate regression of Equation 10. 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.10: 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 A.11: Peer Effects of DACA on Educational Attainment and Achievement – Accounting for Other Educational Policies, US-born Students

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

Notes: This table shows difference-in-differences estimates of the spillover effects of DACA on a summary index of educational attainment (Panel A) and academic achievement (Panel B). In Panel A, each column reports estimates of ζ_1 from a separate regression of Equation 8. In Panel B, each column reports estimates of ρ_1 from a separate regression of Equation 10. These models use the full set of controls specified in Tables 5 and 6, 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 are shown in, the discipline rate in 2012, the graduation rate for pre-policy 9th grade cohorts (i.e. those in 9th grade between 2007 and 2010), and the fraction of 9th grade ELL students. See Table 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.12: The Heterogenous Effects of DACA By Predicted Likelihood of High School Graduation and Baseline Characteristics, US-born students

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

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on a summary index of educational attainment and a summary index of educational achievement for students with different likelihoods of graduating high school and whether students were disciplined at baseline (in 8th grade). I use the full set of controls to predict the likelihood of graduating from high school. This likelihood is the split into three terciles, from the lowest likelihood in column (1) to the highest likelihood in column (3). In Panel A, each column reports estimates of ζ_1 from a separate regression of Equation 8. In Panel B, each column reports estimates of ρ_1 from a separate regression of Equation 10. 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. 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.