Does Peer Motivation Impact Educational Investments? Evidence From DACA

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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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I Introduction

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

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

Beyond the contributions this paper makes to the peer effects literature, understanding the spillover effects of DACA also has important policy implications for the DACA program itself. DACA is an important immigration reform that has remained at the forefront of public discourse and current immigration policy debates. Previous studies on DACA have focused exclusively on the

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

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

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

I find that DACA led to significant increases in targeted students' educational investments. High school graduation increased by 6 percent among youth who were likely undocumented. The effects are driven by males and students who were initially low achievers, whose likelihood of graduating increased by 10 percent and 12 percent, respectively. 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 is 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 undocumented youth. As students would have had to exert additional effort in order to experience these performance improvements, these results suggest that undocumented youth were also more motivated after DACA's enactment. Then, I show that

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

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

These findings 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 additional investments in school with higher shares of undocumented youth. For instance, if schools trained guidance counselors to better understand the process of college admissions for DACA-eligible students, this training could have spilled over to their US-born peers.[3]

This paper contributes to two key literatures. First, it adds to the small but growing literature on spillover effects of policies that increase the returns to schooling. While there is an existing literature that estimates the direct impact of increasing the returns to education for specific student groups (Kuka et al., 2020; Abramitzky & Lavy, 2014), I am aware of only one other study that tests whether such policies spillover to non-eligible peers (Abramitzky, Lavy, & Perez, 2018), who find that a pay reform change that improved high school outcomes among kibbutz members in Israel also increased educational attainment for non-kibbutz peers. However, Abramitzky et al. (2018) can only address whether there are spillover effects on the margin of college enrollment because high school completion was so high in their setting (over 95 percent were completing). My project builds upon this recent work by addressing whether policy spillovers exist on the margin of

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

high school completion among a very different sample of students in a large low-performing school district in the US.

Second, I contribute to the emerging literature on the impacts of DACA. To date, most studies have focused on understanding how the policy affected DACA-eligible students who completed high school, and focus on the policy's impact on their labor market and college outcomes (Pope, 2016; Amuedo-Dorantes & Antman, 2017; Hsin & Ortega, 2018). Only one other study has focused on DACA-eligible youth who experienced DACA during high school (Kuka et al., 2020). Kuka et al. (2020) use the American Community Survey (ACS) and find high school graduation rates increased by 2.2. to 7.5 percent for DACA-eligible youth. I am able to make three important contributions to the literature on DACA. First, I am able to examine intermediate outcomes, which allows me to test whether DACA led to increased effort in school. Second, I am able to consider the educational spillover effects of this policy. Third, using zip-code level variation in the concentration of DACA applicants to approximate the undocumented population allows me to estimate DACA-eligibility with less measurement error than prior studies that largely rely on the absence of citizenship as a proxy for undocumented status.[4]

II 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 they were 16 and by June 2007)[5] and minimum education requirements.[6] Specifically, to be program eligible, undocumented youth were required to complete high school, earn a general educational development (GED) certificate or equivalent

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

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

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

state-authorized exam, or currently be enrolled in school. To continue receiving benefits, DACA 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, the setting of this study, take-up of DACA was even higher. Dividing the 72,180 initial applications received in 2012 - 2014[7] in Los Angeles county by the 111,000 youths estimated to be immediately eligible for DACA (Batalova, Jeanne and Hooker, Sarah and Capps, Randy, 2014) yields a take-up rate of 65%.[8]

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).

II.A 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

[7] Author's calculations using USCIS data described in more detail in Section III.

[8] While take-up in Los Angeles was high relative to the national average, there are reasons for incomplete take-up. For instance, undocumented youth may be hesitant to provide information on legal status to the federal government.

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.[9] 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.[10]

Because high school completion is tied to DACA eligibility, the model predicts that undocumented youth will be incentivized to complete high school to benefit from the increase in expected lifetime earnings associated with becoming a DACA recipient. However, even if undocumented youth do not consider the change to expected lifetime earnings driven by DACA, they may still choose to complete high school if they prefer living in the US, and value the temporary protection from deportation DACA offers. Since the returns to college will also increase with legalization due to DACA, undocumented youth may also be incentivized to enroll in college.[11]

II.B 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). Moreover, before DACA's enactment educational attainment of likely DACA-eligible youth in Los Angeles was low. At the time of policy introduction, 30% of potentially DACA eligible youth who met all of the age and date of arrival criteria had already dropped out of high school (McHugh, Margie, 2014), and for those who completed high school, most (slightly over 70%) did not pursue higher education.[12]

[9] 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).

[10] 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.

[11] In addition, undocumented youth in California became eligible for state financial aid through the introduction of the California Dream Act in 2012. Thus, undocumented youth in California also experienced increases in college affordability around this time.

[12] 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, Margie, 2014).

Undocumented youth in Los Angeles also share much in common with their US-born peers. At the time of DACA's introduction, US-born in LAUSD also had low levels of educational attainment (roughly 60 percent graduated high school). Moreover, US-born and likely undocumented youth share similar ethnicity and socio-economic backgrounds. Over 86% of DACA applicants in California come from Mexico (Svajlenka, Nicole Prchal and Singer, Audrey, 2013), and roughly 60% of children living in Los Angeles have parents who were born in Mexico. Finally, as previously noted, DACA-eligible youth are not recent immigrants.[13] Most have spent the majority of their schooling in LAUSD, thereby increasing the likelihood that DACA-eligible youth were well integrated with their US-born peers at the time the policy was introduced.

III Data

I leverage administrative data from the Los Angeles Unified School District (LAUSD), and focus on students entering 9th grade between 2007 and 2014.[14] The data track key academic and behavioral outcomes yearly, including attendance rates, state standardized exam scores, disciplinary actions, semester GPA, the California High School Exit Exam (CAHSEE), SAT scores, yearly enrollment indicators and whether a student graduated from high school. Importantly, LAUSD data also includes each student's country of birth, date of arrival to the US (if foreign-born), and current zip-code of residence. To estimate the spillover effects of DACA, I focus on students who were born in the US, who are unlikely to be affected by DACA except through policy spillovers. The final sample I use to estimate the spillover effects of DACA consists of 238,781 students.

However, like other studies' I cannot directly observe whether a 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

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

[14] 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.I shows DACA exposure by each 9th grade cohort.

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 (ages 15-31) who applied to DACA immediately after DACA's enactment as follows:

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

where the numerator is constructed from USCIS data and the denominator from the ACS.[15] For each foreign-born student, I use this measure to proxy for their likelihood of being undocumented. As illustrated in Figure I, there is significant variation in this measure across Los Angeles zip-codes.

Importantly, since take-up of DACA was high in Los Angeles county (over 65%), this measure is likely to estimate the undocumented population with minimal measurement error. Nevertheless, Equation 1 will undercount the undocumented population living in a zip-code. However, as long as take-up of DACA across zip-codes was uncorrelated with trends in educational outcomes, this undercounting is unlikely to confound my estimates. While I am not able to test this assumption directly,[16] event-study plots presented in Section IV.C demonstrate that educational outcomes in zip-codes with different concentrations of DACA-applicants (the variation used in this paper) had similar trends prior to DACA's enactment (in particular for low-achievers who were most impacted by the policy). Moreover, I show in Section VI that using other measures to approximate the underlying undocumented population yields similar results.[17]

[15] In 2012, DACA applicants were ages 15-31 due to the different age/date of arrival restrictions. In order to focus on high school aged DACA applicants (i.e. ages 15-19) I take the total number of DACA applicants in a zip-code and multiply by 0.40, since 40% of DACA applicants in Los Angeles were ages 15-19 (USCIS, 2014). Then, I divide by the number of foreign-born who were ages 15-19 using data from the ACS. Results using this measure yield similar results.

[16] While I observe the number of DACA applicants by zip-code, I do not observe the number of undocumented youth by zip-code. This makes it impossible to compute the take-up of DACA by zip-code.

[17] For instance, I draw similar conclusions when approximating the undocumented population using the fraction of

Finally, I use one's country of origin and age of US arrival to identify individuals likely to be DACA-eligible. In California, over 95% of DACA applicants are Hispanic, with the vast majority born in Mexico (86%) (Svajlenka, Nicole Prchal and Singer, Audrey, 2013). Therefore, to estimate the direct impacts of DACA I limit my focus to Hispanic foreign-born students only.[18] In addition, DACA applicants had to have lived continuously in the US since June 15, 2007. This imposes a different maximum age of US arrival for different 9th grade cohorts. As an example, 9th grade students from 2007 (the oldest cohort in my sample) were 14 in 2007, while 9th grade cohorts from 2014 (the youngest) 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.

III.A Summary Statistics

Table I 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 ELA baseline achievement, but have very similar levels of math baseline achievement. 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 I compare foreign-born students by ethnicity and age of arrival to the US. 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 learner and be a Free-Lunch participant.

undocumented foreign-born individuals in a PUMA (MPI) or the fraction of foreign-born non-citizens by zip-code. Using foreign-born non-citizens is the most common way to approximate the undocumented population in this literature (Kuka et al., 2020; Pope, 2016; Amuedo-Dorantes & Antman, 2017). While the results using these other measures are sometimes less significant, the direction and magnitude are always consistent with our preferred specification which approximates undocumented status using the share of DACA applicants.

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

Hispanics and Mexicans who arrived to the US before age 9, have similar baseline achievement to all foreign-born students, but lower achievement relative to US-born students. Despite these differences in baseline achievement, educational attainment is similar across all subgroups shown in Table I.

Table II presents summary statistics that compare high school campuses with more vs. less likely undocumented students. Students in campuses with higher fractions of DACA-eligible youth are more likely to be Hispanic, English language learners (ELL), receiving free or reduced price lunch (FRL), and have lower standardized exam performance at baseline. While all campuses have similar shares of foreign-born students, foreign-born students in campuses with higher concentrations of DACA-eligible youth are more likely to have been 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 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 VI that my results are robust to the inclusion of time trends interacted with campus demographics.

IV Direct Impacts

IV.A Empirical Strategy

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

noted, this strategy is infeasible because I cannot directly observe a students' legal status.[19]

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

$$Y_{izc} = \delta_0 + \delta_1(\text{ShareEligible}_z * \text{Exposed}_c) + \lambda_1 Z_i + \gamma_s + \gamma_z + \phi_c + \varepsilon_{izc} \quad (2)$$

where Y_{izc} is an indicator for high school completion for foreign-born student i in 9th grade cohort c living in zip-code z. ShareEligible_z is the fixed concentration of DACA applicants in a student's zip-code of residence as defined in Equation 1, and is interacted with an indicator for whether a student attended high school after DACA's enactment.[20] I control for zip-code (high school campus) γ_z (γ_s) fixed effects to account for fixed cross-sectional differences across zip-codes (high school campuses), and cohort controls ϕ_c to account for trends in high school completion that could affect all students in Los Angeles. Z_i includes individual characteristics that include age of arrival to the US, gender and disability status, all measured in 9th grade, as well as 8th grade ELA test scores.[21] Finally, Z_{sc} accounts for school by cohort demographics that include the fraction of students who are

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

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

[21]I do not control for free-lunch status. Parents must apply to receive free-lunch, and parents who are undocumented may be less likely to apply. I also do not include an indicator for whether a student was classified as an English Language Learner (ELL) in 9th grade. Across this time, the fraction of students classified as EL in 9th grade significantly declined due to an increase in pressure to reclassify ELL students in LAUSD. Finally, I do not condition on 8th grade math test scores, since students can choose which version of the 8th grade math test to take in California.

male, by racial group (Hispanic, White, and Black), and receiving special education, all measured as of 9th grade. The main variable of interest, δ_1, identifies the average impact of DACA on the outcomes of likely undocumented youth.

The main identification assumption is that likely undocumented youth had similar counterfactual trends to likely citizens. In order to test this assumption, I estimate an event-study specification that replaces Exposure_c from Equation 2 with 9th grade cohort indicators. This event-study allows me to visually detect any differences in outcomes between likely undocumented youth and likely citizens before and after DACA's enactment. These event-study results are presented in Section IV.C and provide evidence in favor of this parallel trends identification assumption.

IV.B Results

I begin by establishing whether DACA increased high school enrollment and completion among likely undocumented youth. Difference-in-differences estimates are presented in Table III. I find that likely undocumented youth were significantly more likely to be enrolled during grades 11 through 12 and complete high school after DACA's enactment.[22] Starting with a model that only includes 9th grade cohort indicators, school fixed effects, and zip-code fixed effects, I successively add controls. The estimated effects are largely stable to the choice of specification. These results suggest that foreign-born youth who lived in the average zip-code (where 14 percent of foreign-born youth had applied to DACA), were 2.5 p.p. (or 3.2 percent) more likely to be enrolled in 12th grade and 3.5 p.p. (or 6 percent) more likely to complete high school after DACA's enactment.[23] In order to account for multiple inference (Kling, Liebman, & Katz, 2007), I also examine the impact of DACA on a summary index of educational attainment, which is computed as the equally weighted average of the z-scores of high school completion and enrollment in each grade. The results using this summary measure also indicate an improvement in the educational attainment of

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

[23] Specifically, this is effect size is computed by multiplying the coefficient by the mean of ShareEligible_z, which was 0.14.

likely undocumented youth.

Intermediate Outcomes – Next, I investigate whether DACA led to changes in behavior and achievement. On the one hand, it is possible that these increases in educational attainment were accompanied by increases in effort. This could either be because additional effort was required in order to be able to graduate, or because likely undocumented youth became more motivated after DACA's enactment. On the other hand, it is possible that DACA induced students to simply remain enrolled in school (to obtain a diploma), but was not accompanied by any changes in effort.[24] The extent to which any increases in educational attainment among likely undocumented youth would spillover to US-born peers will depend on which of these two scenarios was more likely.

Table IV presents difference-in-differences estimates from a slightly modified version of Equation 2 using yearly outcomes as the outcome variables.[25] Specifically, I focus on yearly attendance rates, an indicator for whether a student was suspended within the year, ELA achievement, and cumulative GPA. Starting with a model that only includes campus-grade, year-grade, and zip-code fixed effects, I successively add controls. The estimated effects are largely stable to choice of specification. DACA did not impact attendance rates, increased the likelihood of being suspended, increased cumulative GPA, and increased ELA performance. In the fully specified model, these estimates suggest that foreign-born students who lived in the average zip-code (with 14 percent of foreign-born students applying to DACA) are 1.4 p.p. more likely to be suspended, experience an improvement in GPA of 0.07 points (off of a mean of 2.26) and experienced a 0.07 standard deviation increase in ELA standardized test performance. In addition, the results using a summary index of academic achievement also indicate an improvement in performance of likely undocumented youth.

[24] This scenario could occur if prior to DACA those on the margin of high school completion dropped out because they no longer wanted to be enrolled in school, as opposed to dropping out because they were not meeting high school graduation standards.

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

$$Y_{istgz} = \beta_0 + \beta_1(\text{ShareEligible}_z \times \text{Post}_t) + \lambda_1 Z_i + \lambda_2 Z_{sc} + \phi_{sg} + \alpha_{tg} + \gamma_z + \varepsilon_{stgz} \quad (3)$$

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

One important caveat of these findings is that DACA induced undocumented youth to stay enrolled in school, as shown in Table III. Thus, these estimates of yearly outcomes which focus on grades 9 through 12 are subject to compositional changes due to the policy. Since, lower-achieving students were induced to stay enrolled in school due to the policy. If anything, this is likely to bias me against finding a positive effect of DACA on intermediate outcomes. The fact that I identify improvements in achievement even despite this compositional change, provides compelling evidence that the effort among undocumented youth was likely to have improved in response to DACA.

Heterogeneous Responses – I next stratify the sample by gender, country of origin, and baseline achievement (as of 8th grade). Tables V focuses on the impacts of DACA on educational attainment across these subgroups. The effects on educational attainment are driven by men, larger for those of Mexican origin, and larger for those in the bottom half of the baseline achievement distribution. These are precisely the groups who are typically at risk of dropping out of high school and would have been more likely to respond to DACA's educational incentives.

Table VI focuses on heterogeneity for yearly outcomes. I estimate similar increases in achievement across gender and country of origin. By baseline achievement, I find that the increases in ELA performance were larger for the top half of the achievement distribution at baseline. Again, to interpret the impacts of DACA on achievement, especially for those at the bottom half of the baseline achievement distribution, it is important to consider that this group was induced to stay enrolled in school due to DACA.[26] On the margin of high school GPA, however, I find that the effects are driven by students who were lower achieving at baseline. The increases in the likelihood of ever being disciplined are entirely driven by those who were lower achieving at baseline.

The heterogeneous responses by baseline achievement provide suggestive evidence that DACA impacted two different groups of undocumented students: lower-achieving students on the margin of high school completion and higher achieving students on the margin of college

[26] In fact, in Section IV.C I show that those in the bottom half of the achievement distribution were significantly less likely to have missing standardized exam scores during high school after DACA's enactment. If the lowest-achieving students within this group were induced to stay enrolled in school due to DACA and are now more likely to appear in the test-taking sample, as is likely to be the case, then the underlying ability of this group was likely declining overtime.

enrollment. For low-achievers, DACA led to significant increases in high school completion. As outlined in Section II.A, these are precisely the students who were likely incentivized to complete high school in order to receive the benefits of DACA. They also increased effort, as measured by ELA performance and GPA. These increases in effort were either driven by necessity (i.e. in order to be able to graduate they had to work harder), or because DACA led to increases in their academic motivation. For high-achievers, DACA did not impact high school completion (as they likely would have graduated regardless of DACA), but it did lead to significant increases in achievement. These higher-achieving students were likely incentivized to work harder during high school in order to be eligible for the new merit-based financial aid opportunities in California that were tied to high school performance, or in order to gain access to more competitive colleges or degree programs.

IV.C Evidence for the Main Identification Assumption

This analysis rests on the assumption that likely undocumented youth had similar counterfactual trends to likely citizens. In order to provide evidence in support of this assumption, I next examine the relationship between the likelihood of being undocumented (ShareEligible_z) and educational attainment for each each cohort separately using an event-study specification. Figure II plots event-study estimates where the outcome is a summary index of educational attainment.[27]

For the full foreign-born and Mexican-born samples (Panels A and B of Figure II), I estimate a small downward pre-policy trend in educational attainment for likely undocumented youth relative to likely citizens. Importantly, this trend is in the opposite direction of the effects I estimate post-policy. If anything, this would bias me against finding a positive impact of DACA on educational attainment. Moreover, this downward pre-policy trend does not exist for those in the bottom half of the baseline achievement distribution who were most impacted by DACA (Panel C of Figure II). For this lower-achieving sample, consistent with the identification assumption – that likely undocumented youth had similar counterfactual trends to likely citizens – for 9th grade

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

cohorts expected to graduate before DACA's enactment there was little differences in educational attainment across those who were more vs. less likely to be undocumented. However, for cohorts exposed to DACA during high school, likely undocumented youth were significantly more likely to complete high school relative to likely citizens. For high-achieving students (Panel D of Figure II), who were unlikely to be impacted by DACA's graduation incentives, there is little relationship between the likelihood of being undocumented and completing high school across cohorts.

Similarly, I estimate the relationship between the likelihood of being undocumented (ShareEligible_z) and yearly outcomes in each calendar year separately. Figure III plots event-study estimates where the outcome is a summary index of achievement.[28] This plot demonstrate similar patterns across all subgroups. Before DACA's enactment in 2012, there was little difference in achievement between those who were more and less likely to be undocumented. However, after 2012 likely undocumented students experienced significant improvements in achievement.

I also show that observables do not predict a differential improvement in outcomes for likely undocumented relative to likely citizens after DACA's enactment. Columns 2-7 of Appendix Table A.II show that there were no trends in demographics among likely undocumented youth relative to likely citizens leading up to DACA's introduction. In addition, I use all of covariates (excluding treatment) to generate predicted high school completion based on foreign-born students during the pre-policy period. Column 1 of Appendix Table A.II show that conditional on cohort, campus, and zip-code fixed effects, there were no trends in predicted outcomes across students more and less likely to be undocumented. Taken together, it is unlikely that the underlying ability of students more likely to be undocumented was increasing at the time of DACA's introduction, such that compositional changes among likely undocumented youth (relative to likely citizens) are driving these results.

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

V Spillover Effects

V.A Empirical Strategy

Next, I leverage the introduction of DACA to determine whether the increased returns to schooling experienced by undocumented youth affected their US-born peers' outcomes. Specifically, I focus on the 2012 introduction of DACA, wherein the control group consists of US-born students without DACA-eligible peers, and the treatment effect varies across US-born students in the fraction of their peers who were DACA-eligible. As previously noted, I do not observe a student's legal status so I focus on the share of a student's peers who were *likely* DACA-eligible defined as follows:

$$\text{DACAShare}_{sc} = \text{FBShare}_{sc} \times \left(\frac{\sum_{z=n}^{N} \text{n}_{scz} \times \text{ShareEligible}_{z}}{\text{n}_{sc}} \right)_{sc} \tag{4}$$

where FBShare_{sc} is the fraction of Hispanic foreign-born youth who arrived to the US by age 9 in a campus-cohort, rescaled by the second term which captures the likelihood that these foreign-born peers were undocumented. Specifically, this second term is the weighted average of the zip-code concentration of DACA applicants as defined in Equation 1 (see Section III) across the residence zip-codes of the foreign-born students in a campus-cohort. Within a campus-cohort, n_{sc} indicates the number of foreign-born students overall, and n_{scz} indicates the number living in a particular zip.

My difference-in-difference estimating equation thus takes the form:

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

where Y_{isc} is an indicator for high school completion for US-born student i in 9th grade cohort c in high school s. DACAShare_{sc} is the fraction of students in a school and 9th grade cohort who I estimate to be DACA-eligible as just described, and is interacted with an indicator for whether a student attended high school after DACA's enactment. I control for high school campus γ_s fixed effects to account for fixed cross-sectional differences across high school campuses, and cohort

controls ϕ_c to account for trends in high school completion that could affect all students in Los Angeles. Z_i includes individual characteristics that include race, gender, gender-race interactions, special education status, and 8th grade ELA test scores.[29] Finally, Z_{sc} accounts for school by cohort demographics that include the fraction of students who are male, by racial group (Hispanic, White, and Black), and receiving special education, all measured as of 9th grade.

The coefficient of interest, α_1, represents the peer effects stemming from the share of one's peers estimated to be DACA-eligible. Again, I trace out the impacts for each cohort separately by replacing Exposure$_c$ with 9th grade cohort indicators. This specification will allow me to visualize any differences in outcomes between US-born students with higher concentrations of likely DACA-eligible peers and those with fewer concentrations of likely DACA-eligible peers before and after DACA's enactment, as a test of the parallel trends identification assumption. These event-study results are presented in Section V.B and provide evidence in favor of this parallel trends assumption.

V.B Results

I begin by documenting whether exposure to undocumented peers led to changes in educational attainment for US-born students after DACA's enactment. Difference-in-differences estimates are presented in Table VII. I find that US-born students with more undocumented peers were significantly more likely to enroll in grades 11-12 and complete high school after DACA's enactment.[30] Starting with a model that only includes 9th grade cohort indicators and high school campus fixed effects, I successively add controls. My estimated effects are largely stable to choice of specification. These results suggest that US-born students with the average number of undocumented peers (1 percent of their campus-cohort), 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.

[29] Again, I do not control for ELL status as of 9th grade given the downward trend in ELL participation over this period. I also do not control for an FRL indicator. However, when I control for both EL stats and FRL status the results are similar.

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

Intermediate Outcomes – Next, I examine whether exposure to higher concentrations of undocumented peers led to increases in achievement for US-born students after DACA's enactment. To do so, I estimate a slightly modified version of Equation 5 to account for yearly outcomes.[31] Difference-in-differences estimates from this specification are presented in Table VIII, where the outcomes include yearly attendance rates, an indicator for whether a student was suspended, ELA achievement and cumulative GPA. Starting with a model that only includes campus-grade and year-grade fixed effects, I successively add controls. The results are largely stable to the choice of specification. I find that exposure to more undocumented peers did not affect attendance rates or the likelihood of being disciplined. However, I do find that exposure to more DACA eligible peers led to significant increases in achievement. In the fully specified model, I find that US-born students with the average number of 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.

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

Table X focuses on heterogeneity for the yearly outcomes. By ethnicity, I find that the increases in ELA performance and GPA are driven by Hispanic US-born students. I estimate similar increases in achievement due to DACA spillovers across gender. I find that all US-born students

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

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

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

experienced increases in achievement, regardless of baseline achievement. For GPA, the positive spillovers are largest for US-born students in the bottom of the achievement distribution. While the increases in ELA performance are largest for those in the upper half of the distribution. Again, one caveat for these findings is that DACA induced US-born students to stay enrolled in school, which will lead to compositional changes, especially among those the lower half of the achievement distribution. As previously noted, if anything, this should bias me against finding positive spillovers on the yearly achievement of US-born students.

These heterogeneous results provide evidence consistent with spillover effects being driven by peer interactions. First, recalling that among likely undocumented youth, males and lower-achieving youth drove the increases in high school graduation due to DACA. Similarly, among US-born students, males and lower-achieving youth drove the increases in high school graduation. Because low-achieving students were more likely to interact with one another, this is precisely the group of students who would have been impacted by the increased motivation of their undocumented peers to complete high school. Second, recalling that among likely undocumented, high-achieving youth experienced the largest increases in ELA achievement due to DACA. Similarly, among US-born students, higher-achieving youth experienced the largest improvements in ELA achievement. As high-achieving students were more likely to interact with one another, this is precisely the group of US-born students expected to have the largest ELA score increases after DACA's introduction.

V.C Evidence for the Main Identification Assumption

To rule out the possibility that these results are driven by pre-trends, I next examine the relationship between educational attainment and the estimated fraction of undocumented peers (DACAShare_{sc}) for each cohort separately. Figure IV plots event-study estimates where the outcomes is a summary index of educational attainment.[32] Panel A presents estimates for the overall sample, while Panel B

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

(C) presents estimates for students in the bottom (top) quartile of the 8th grade ELA achievement distribution. For the overall sample, I estimate a small positive (but insignificant) pre-DACA trend in educational attainment. Importantly, for low-achievers who were on the margin of high school completion there are no pre-DACA trends. The plot in Panel B of Figure IV shows that for low-achieving 9th grade cohorts expected to graduate before DACA's enactment there was little difference in educational attainment between US-born students with more and less undocumented peers. However, for low-achieving US-born students who were expected to be enrolled in high school after DACA's enactment, those with higher concentration of undocumented peers were significantly more likely to stay enrolled until 12th grade and complete high school. While these patterns do not hold for high-achieving students, they were already likely to graduate from high school.

Similarly, I estimate event-study specifications for the yearly outcomes which plot the relationship between the estimated fraction of undocumented peers (DACAShare_{sc}) and outcomes of US-born students in each year separately. Figure V plots event-study estimates where the outcome is a summary index of academic achievement.[33] Before DACA's enactment in 2012, there was little difference in achievement between US-born students with more vs. fewer undocumented peers. After DACA's enactment in 2012, students with higher concentrations of undocumented peers experienced significant improvements in ELA achievement. While there does appear to be a positive trend in achievement (with those with higher concentrations of DACA-eligible peers being increasingly likely to do better relative to those with lower concentrations) between 2005 and 2008 for those in the top quartile, it largely appears to level of three years before DACA's introduction.

I also investigate whether observables predict a differential improvement in outcomes for US-born youth with higher concentrations of undocumented peers after DACA. Reassuringly, Columns 1-7 of Appendix Table A.III provide evidence that there was not a differential change in demographics for US-born students with more vs. fewer DACA-eligible peers after DACA's

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

enactment. In addition, I use all covariates to generate predicted high school completion based on students during the pre-policy period. Columns 8 of Appendix Table A.III show that conditional on 9th grade cohort and high school fixed effects, there were no trends in predicted outcomes for students with different concentrations of undocumented peers. Taken together these checks provide compelling evidence that parallel trends for students with different concentrations of undocumented peers was likely to continue in the absence of DACA.

VI Robustness

The measure I use to approximate undocumented status is likely measured with minimal measurement error (due to the high take-up of DACA). Nonetheless, one may worry that the share of DACA applicants in a zip may still introduce measurement error. To alleviate this concern, Appendix Table A.IV demonstrates that the direct impacts of DACA are largely robust to using several different measures to approximate undocumented status. Column 1 reports my baseline model that approximates the likelihood of being undocumented by using the fraction of foreign-born youth ages 15-31 in one's residence zip who applied to DACA using Equation 1. Column 2 approximates the likelihood of being undocumented by using the fraction of foreign-born youth ages 15-19 in one's residence zip who applied to DACA using a slightly modified version of Equation 1. Column 3 uses the fraction of foreign-born youth ages 0-18 who were estimated to be undocumented in one's residence PUMA.[34] Finally, Column 4 uses the fraction of foreign-born non-citizens ages 0-18 in one's residence zip-code.[35] In general, the main results all suggest improvements in educational attainment and achievement among likely undocumented youth regardless of which scaling measure is used. While the impacts on ELA achievement are always significant, the impacts on high school enrollment and completion are sometimes insignificant (but always positive).

Similarly, Appendix Table A.V shows that the spillover effects of DACA on US-born

[34]This is calculated by MPI. One downside of this measure, is that PUMAs are larger areas than zip-codes.

[35]This is computed using data from the ACS. Because some non-citizens have green cards or temporary visas, this measure includes youth who are not undocumented. However, for Hispanic foreign-born youth, Kuka et al. (2020) estimate that 72% of all Hispanic non-citizens are undocumented, suggesting that this also may be a reliable proxy.

students are robust to using different measures to approximate the fraction of undocumented peers. Column 1 reports my baseline model that scales the fraction of foreign-born peers by the zip-code DACA-application rate using Equation 1. Column 2 scales the fraction of foreign-born youth by the high-school aged DACA applicants. Column 3 scales the fraction of a campus-cohort who was foreign-born by the fraction of undocumented youth estimated to be living in a PUMA. Column 4 scales the fraction of a campus-cohort who was foreign-born by the fraction of non-citizens in a zip-code. Finally, Column 5 simply uses the fraction of foreign-born students in a campus-cohort to define peer exposure. Reassuringly, I come to similar conclusions regardless for how I scale the fraction of foreign-born to account for the likelihood of being undocumented (Columns 2-4). In addition, the much smaller and insignificant estimates in Column 5 provide compelling evidence that my estimates are not picking up the 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 youth are undocumented, suggest that I am instead able to capture the peer effects stemming from DACA.

Next, I employ an individual fixed effects approach to estimate the impact of DACA on outcomes that vary yearly. Specifically, I run an individual fixed effects model of the following form on the sample of US-born students:

$$Y_{isct} = \delta_0 + \delta_1 (\text{DACAShare}_{sc} \times \text{Post}_t) + \gamma_i + \phi_{gt} + \varepsilon_{isct} \tag{7}$$

where γ_i and ϕ_{gt} are individual and grade-year fixed effects respectively. For this analysis, I focus on the three years following 9th grade for all outcomes except for ELA achievement, where I focus on the two years following 9th grade. Appendix Table A.VI presents these results. In terms of ELA achievement, the results using an individual student fixed effects specification are consistent with the previous estimate that rely on the within-cohort and across campus variation to estimate the impact of DACA on peer outcomes presented in Section V.B. I find that exposure to DACA-eligible peers after DACA's enactment led to a statistically significant and economically meaningful increase in the ELA

standardized exams. The estimate of 4.68 in Column 8 implies that on average, for a student with 1 percent of DACA-eligible peers, after DACA ELA standardized exam scores increased by 0.05 of a standard deviation. While I find that exposure to DACA-eligible peers after DACA's enactment led to a statistically significant increase in the likelihood of being disciplined, a statistically significant decrease in semester GPA and a statistically significant decrease in attendance using the individual student fixed effect model, these effects are small and not economically meaningful. The estimates imply that DACA led to a 0.4 pp increase in the probability of being disciplined, a 0.01 point decrease in semester GPA (on a scale of 4), and a 0.05 pp decrease in average attendance rates for US-born students.

Next, I show that any campus-level population differences by the share of undocumented peers are unlikely to be driving my results. To do so, I re-estimate my models including time trends interacted with campus demographics at baseline. Appendix Table A.VII 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, average baseline ELA achievement measured in 8th grade, and total cohort size, measured in the 2011-12 school year. In terms of ELA Achievement (Panel C) the results are also robust to the inclusion of time trends interacted with the baseline fraction of students belonging to each racial grouping (Hispanic, Black, White, and Asian) at baseline. 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 students in a campus belonging to each racial group. However, the point estimates are positive and of similar magnitude to the baseline estimates, suggesting similar conclusions. Taken together, these results help to rule out the possibility that differential trends driven by demographic differences are driving my results.

Finally, I consider other policies affecting LAUSD public school students during this time. One policy change that occurred in 2015 was the elimination of the high school exit exam.[36] If schools with higher concentrations of DACA-eligible students were also most likely to be positively

[36]Under this policy, cohorts expected to graduate high school after 2015 were no longer required to pass the math and english high school exit exams in order to be able to graduate.

impacted by the elimination of the high school exit exam, then it is possible I may be misattributing the increases in high school completion to more motivated peers. To rule out this possibility, I estimate Equation 5 including time trends that vary by the fraction of students who were unable to pass the high school exit exam on their first attempt in 10th grade in 2012. Appendix Table A.VIII presents results for the high school outcome variable and demonstrate that the estimates are robust to the inclusion of such trends. This suggests that even after controlling for campuses that would have been more or less impacted by the elimination of the high school exit exam, I still find a positive and significant relationship between the concentration of DACA-eligible peers on high school completion among US-born students.[37]

VII Conclusion

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

My results indicate that DACA increased educational attainment of undocumented students and their in-eligible peers. I find that among likely undocumented youth DACA increased 12th grade enrollment by 3 percent, high school graduation by 6 percent, ELA achievement by 0.07 standard deviations, and GPA by 0.07 percentage points (off of a mean of 2.17). Among

[37] Similarly, the direct impacts results on high school completion are robust to the inclusion of time trends for the fraction of students in a school who passed the exit exam at baseline. These results are available upon request.

US-born students at the average campus, where approximately 1 percent of students were likely to be undocumented, I also find that DACA increased 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 has 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. Moreover, the results of this study 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. 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 the 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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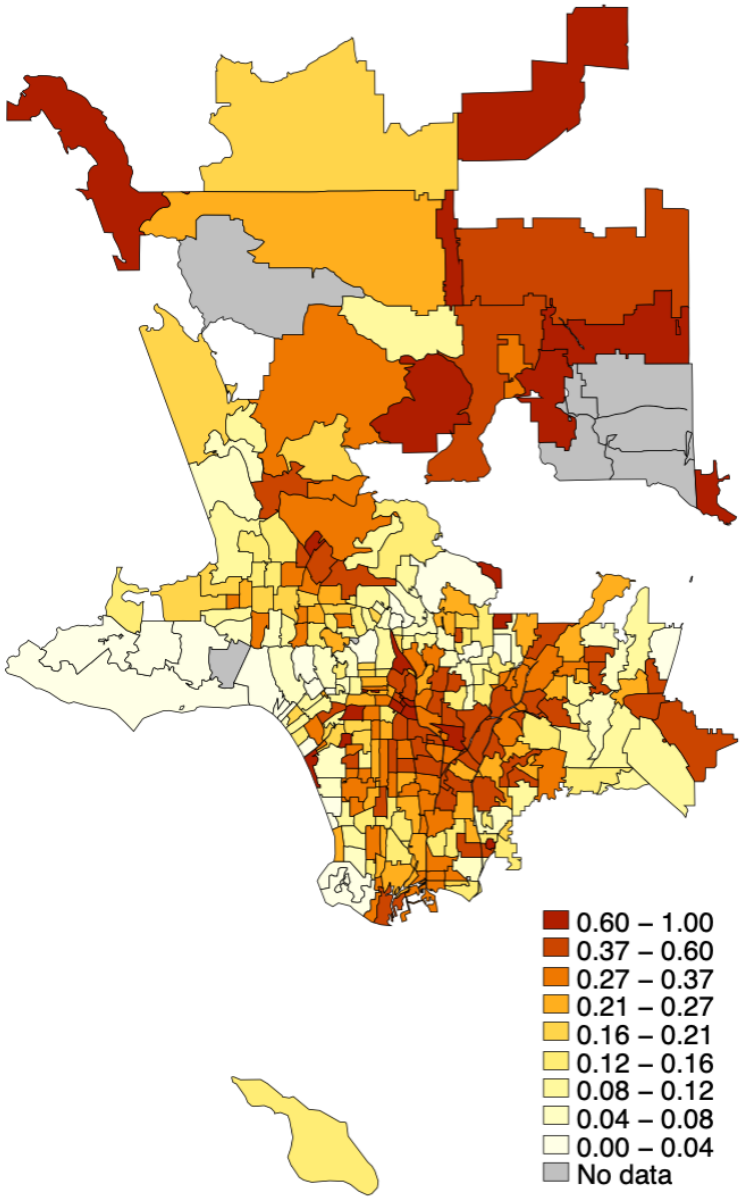
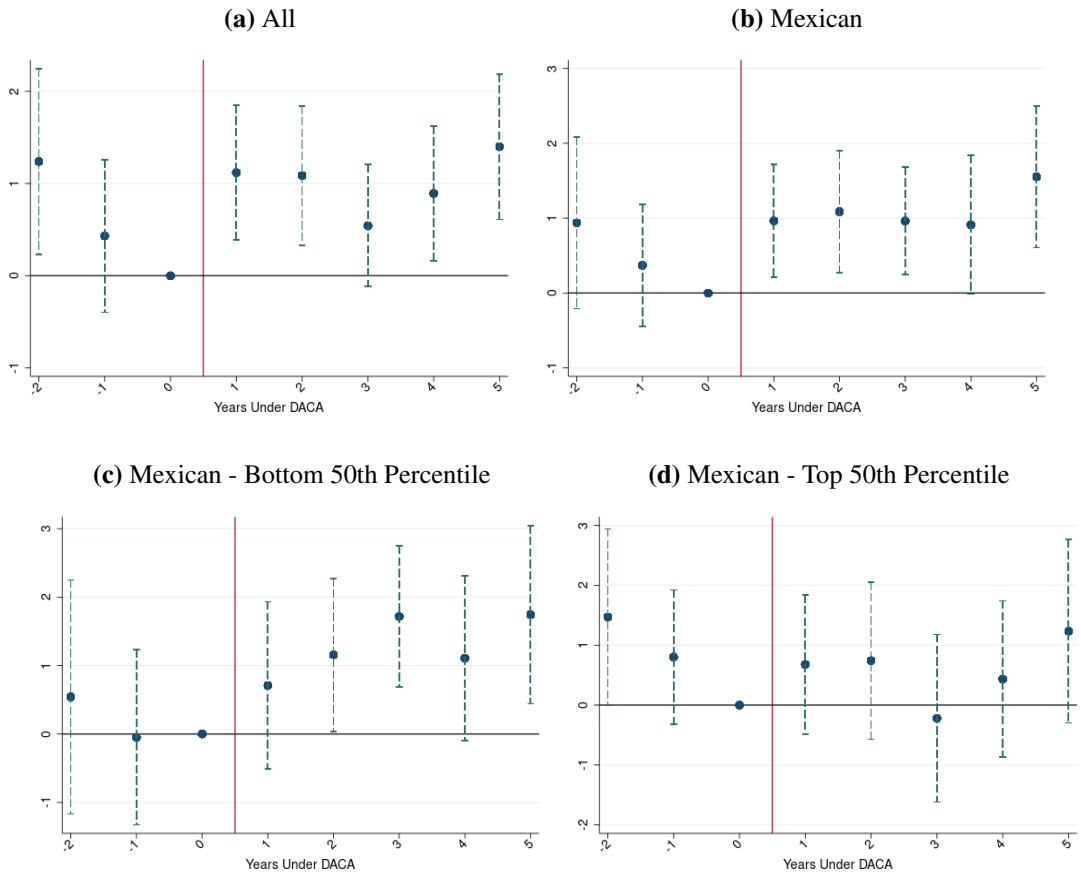
Figures/Tables

Figure I: Fraction of Foreign-Born Population Ages 15-19 who applied to DACA, 2012-2013



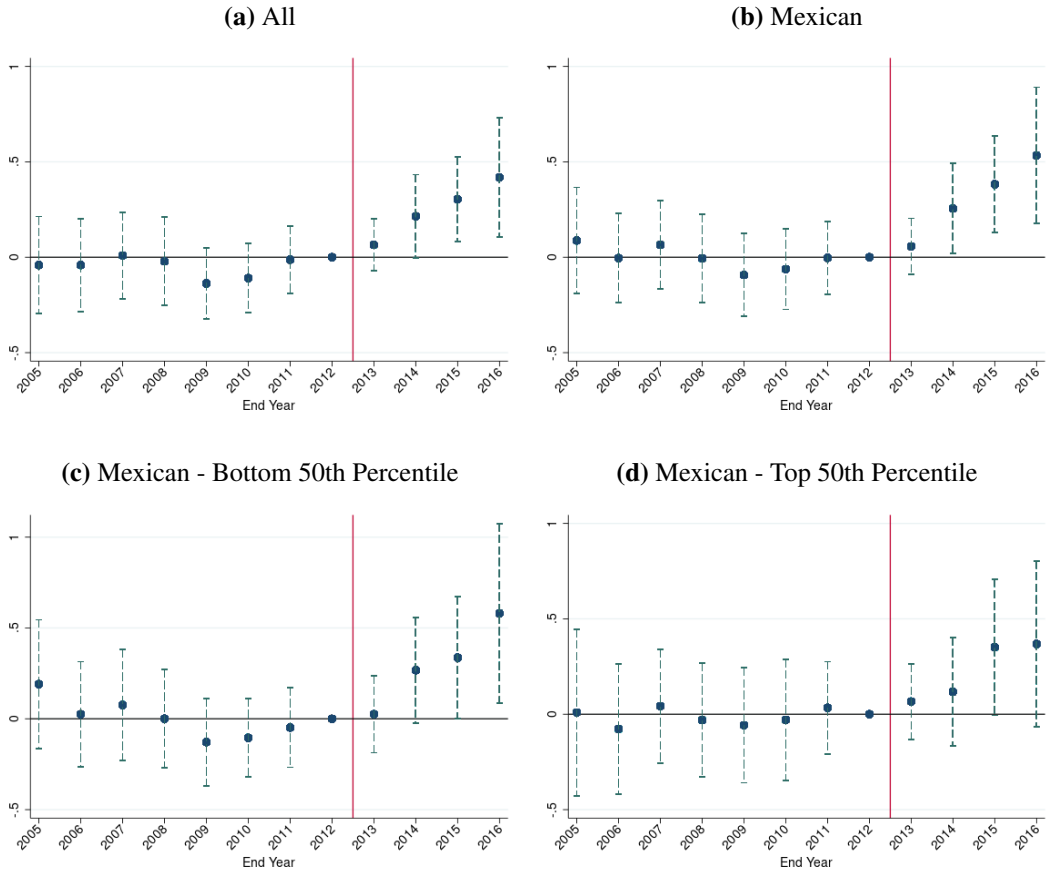
Note: This plot shows the share of foreign-born students (ages 15-19) who applied to DACA in each Los Angeles zip code (ShareEligible_z). This is computed using a slightly modified of Equation 1. For each zip-code, I take the total number of DACA applicants and multiply by 0.40, since 40% of DACA applicants in Los Angeles were ages 15-19 (USCIS, 2014). Then, I divide by the number of foreign-born who lived in the zip-code who were ages 15-19 using data from the ACS.

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



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

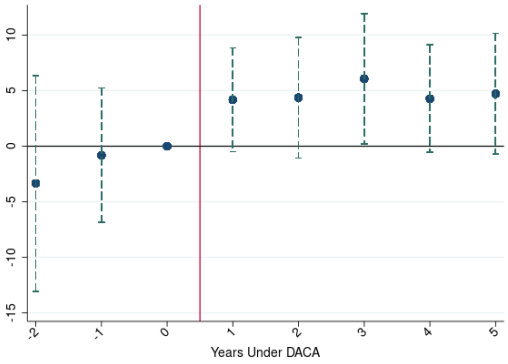
Figure III: Event Study Estimates of the Direct Impact of DACA on Summary Index of Academic Performance, Foreign-born Hispanics



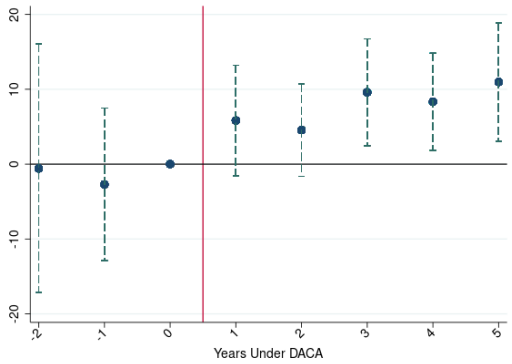
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and ShareEligible$_z$. The dependent variable is a summary index based on GPA and performance on the ELA standardized exam. The sample includes foreign-born Hispanic students who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in the sub-figure labels. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table III 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 IV: Event Study Estimates of the Direct Impact of DACA on Summary Index of Educational Attainment, US-born Students

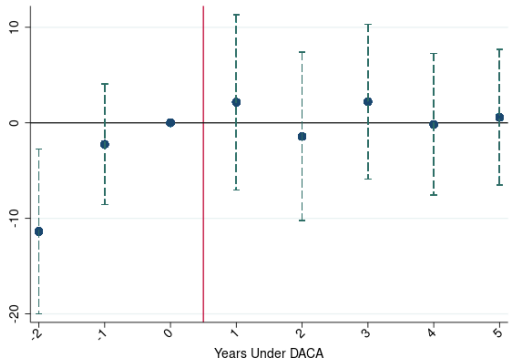
(a) All



(b) Bottom Quartile



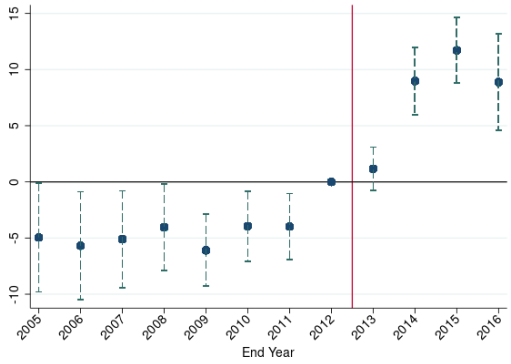
(c) Top Quartile



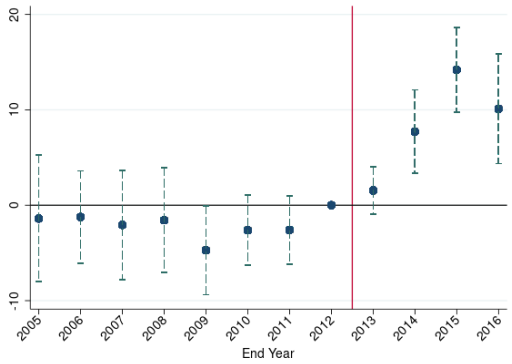
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between 9th grade cohort dummies and DACAShare$_{sc}$. The dependent variable is a summary index based on enrollment in grades 10-12 and high school completion. Event time is computed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

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

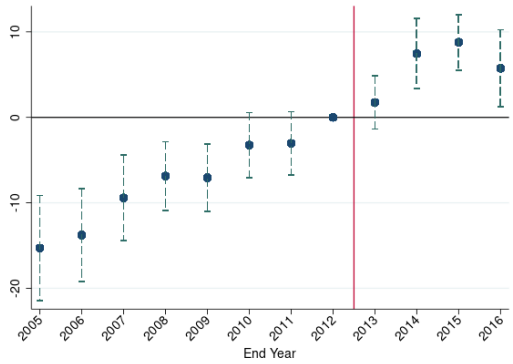
(a) All



(b) Bottom Quartile



(c) Top Quartile



Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and DACAShare$_{sgt}$. The dependent variable is a summary index based on GPA and performance on the ELA standardized exam. The sample includes US-born students in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in sub-figure labels. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table VII 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 I: 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 Applications By Zip						
ShareEligible$_z$ - Ages 15-19	0.323	0.324	0.316	0.337	0.341	0.348
ShareEligible$_z$ - Ages 15-31	0.131	0.131	0.127	0.138	0.139	0.143
Demographics (G9)						
Male	0.511	0.510	0.516	0.514	0.507	0.506
Black	0.090	0.103	0.014	0	0	0
Hispanic	0.780	0.781	0.773	1	1	1
White	0.063	0.064	0.055	0	0	0
Special Education	0.081	0.087	0.048	0.055	0.072	0.076
English Language Learner	0.184	0.156	0.338	0.386	0.272	0.283
Free-Lunch	0.654	0.655	0.648	0.668	0.678	0.676
Foreign-Born	0.150	0	1	1	1	1
Born in Mexico	0.086	0	0.571	0.738	0.816	1
Age US Arrival	-	-	7.834	7.583	5.880	5.767
Baseline Achievement						
Std ELA Score (G8)	-0.069	-0.046	-0.199	-0.378	-0.217	-0.252
Std ELA Score (G7)	-0.032	-0.008	-0.177	-0.359	-0.193	-0.228
Std Math Score (G7)	0.047	0.049	0.034	-0.187	-0.079	-0.108
Outcomes						
Graduated HS	0.572	0.576	0.552	0.514	0.564	0.556
Enrolled Expected G10	0.906	0.907	0.898	0.903	0.921	0.922
Enrolled Expected G11	0.845	0.848	0.831	0.832	0.860	0.859
Enrolled Expected G12	0.768	0.771	0.748	0.741	0.776	0.775
Std ELA Score (G11)	0.061	0.072	0.003	-0.168	-0.075	-0.096
Observations	281,046	238,781	42,265	32,381	21,139	17,247

Note: This table presents summary statistics for students in 9th grade cohorts between 2007 and 2014 enrolled in Los Angeles Unified school district. The first column includes the full sample, the second column includes those students born in the US, and the third column includes those students who were not born in the US. Columns 4-6 include foreign-born students separated by ethnicity and age of arrival to the US. Column 4 includes Hispanic foreign-born students, Column 5 includes Hispanic foreign-born students who arrived to the US before they were 9 years old, and Column 6 includes Mexican foreign-born students who arrived to the US before they were 9 years old.

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

	(1)	(2)	(3)	(4)
		DACA Concentration - Percentile		
	Full	Bottom 25	25-75	Top 25
Share DACA-Eligible Peers				
DACAShare - Ages 15-19	0.023	0.010	0.022	0.037
DACAShare - Ages 15-31	0.009	0.004	0.009	0.016
Baseline Demographics (G9)				
Male	0.511	0.511	0.510	0.511
Black	0.090	0.194	0.072	0.022
Hispanic	0.780	0.548	0.805	0.960
White	0.063	0.135	0.055	0.006
Asian	0.040	0.073	0.041	0.005
Special Education	0.081	0.089	0.084	0.068
Free-Lunch	0.654	0.572	0.666	0.712
English Language Learner	0.184	0.114	0.195	0.230
Foreign-Born	0.150	0.135	0.158	0.151
Foreign-Born - Mexican	0.086	0.047	0.089	0.118
Baseline Achievement				
Std ELA Score (G8)	-0.069	0.151	-0.068	-0.292
Std ELA Score (G7)	-0.032	0.198	-0.036	-0.250
Std Math Score (G7)	0.0467	0.208	0.0577	-0.133
Outcomes				
Graduated HS	0.572	0.582	0.569	0.569
Enrolled Expected G10	0.906	0.897	0.906	0.915
Enrolled Expected G11	0.845	0.836	0.847	0.851
Enrolled Expected G12	0.768	0.766	0.769	0.767
Std ELA Score (G11)	0.061	0.205	0.039	-0.048
Number of Campuses	155	29	70	56
Average Cohort Size	524	624	558	391
Observations	281,046	68,923	153,493	58,630

Note: This table presents summary statistics for all students in 9th grade cohorts between 2007-2014 enrolled in Los Angeles Unified school district. The first column includes the full sample, Columns 2-5 separate students based on the concentration of DACA-eligible youth in their 9th grade cohort. For each student, this is calculated using Equation 4.

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

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

Note: This table shows difference-in-differences estimates of the direct impact of DACA on high school enrollment and graduation. 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. Standard errors in parentheses are clustered at the zip-code level. *p<0.10, ** p<0.05, *** p<0.01.

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

	(1)	(2)	(3)	(4)
Panel A: Yearly Attendance Rate (Grades 9-12)				
ShareEligible*Post	-0.0155	-0.0162	-0.0174	-0.0135
	(0.0263)	(0.0260)	(0.0247)	(0.0248)
Mean (Y)	0.936	0.936	0.936	0.936
Observations	71,811	71,811	71,811	71,811
Panel B: Ever Disciplined (Grades 9-12)				
ShareEligible*Post	0.106***	0.0992***	0.101***	0.104***
	(0.0340)	(0.0340)	(0.0345)	(0.0355)
Mean (Y)	0.0334	0.0334	0.0334	0.0334
Observations	75,155	75,155	75,155	75,155
Panel C: Cumulative GPA (Grades 9-12)				
ShareEligible*Post	0.425	0.516*	0.459*	0.508**
	(0.283)	(0.286)	(0.249)	(0.242)
Mean (Y)	2.262	2.262	2.262	2.262
Observations	72,308	72,308	72,308	72,308
Panel D: Standardized ELA Exam Performance (Grades 9-11)				
ShareEligible*Post	0.512*	0.537*	0.534**	0.553**
	(0.305)	(0.312)	(0.235)	(0.237)
Mean (Y)	-0.0922	-0.0922	-0.0922	-0.0922
Observations	43,153	43,153	43,153	43,153
Panel E: Summary Achievement Index (Grades 9-11)				
ShareEligible*Post	0.820**	0.902***	0.794***	0.836***
	(0.321)	(0.338)	(0.264)	(0.261)
Observations	56,910	56,910	56,910	56,910
Controls				
Zip FE	X	X	X	X
Grade-Year FE	X	X	X	X
Campus-Grade FE	X	X	X	X
Demographics		X	X	X
8th Grade Std Test (ELA)			X	X
Campus-Cohort Demographics				X

Note: This table shows difference-in-differences estimates of the direct impact of DACA on yearly attendance rates, indicators for ever being disciplined (i.e. in or out of school suspensions only), cumulative GPA, and standardized ELA test performance, as well as a summary index based on the outcomes in Panels C-D. The sample for these regressions are foreign-born Hispanic students who were in 9th grade cohorts from 2006-07 to 2013-14 who arrived to the US by age 9. All regressions are weighted by the inverse of the number of times a student is observed in the sample. See Table III for more detail on the sample and control variables. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

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

	(1)	(2)	(3)	(4)	(5)	(6)
					8th Grade ELA Score	
	Full	Mexican	Female	Male	Bottom 50	Top 50
Panel A: Enrolled in Expected 10th Grade						
ShareEligible*Exposed	0.0538	0.108	0.00354	0.0685	0.276***	-0.209
	(0.0891)	(0.0933)	(0.140)	(0.115)	(0.0886)	(0.127)
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)
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)
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)
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)
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 completion. The sample for these regressions are foreign-born Hispanic students who were in 9th grade cohorts from 2006-07 to 2013-14 who arrived to the US by age 9. All regressions include zip-code, 9th grade cohort, and 9th grade campus fixed effects. Regressions also include the full set of individual and cohort level controls. See Table III for more detail on the sample and control variables. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

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

	(1)	(2)	(3)	(4)	(5)	(6)
					8th Grade ELA Score	
	Full	Mexican	Female	Male	Bottom 50	Top 50
Panel A: Yearly Attendance Rate (Grades 9-12)						
ShareEligible*Post	-0.0135	-0.0228	0.0394	-0.0708*	-0.0385	0.0187
	(0.0248)	(0.0238)	(0.0319)	(0.0383)	(0.0346)	(0.0272)
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)
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)
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)
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)
Mean (Y)	-0.0354	-0.0647	0.103	-0.169	-0.494	0.542
Observations	56,910	46,435	27,955	28,955	31,727	25,183

Note: This table shows difference-in-differences estimates of the direct impact of DACA on yearly outcomes. The sample for these regressions are foreign-born Hispanic students who were in 9th grade cohorts from 2006-07 to 2013-14 who arrived to the US by age 9. All regressions include zip-code, grade-year, and campus-grade fixed effects. Regressions also include the full set of individual and cohort level controls. See Table III 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 in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

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

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

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on high school enrollment and graduation, as well as a summary index based on the outcomes in Panels A-D. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. Individual demographic controls include gender, race, disability status and gender-race interactions. District demographic cohort controls include the percentage of students belonging to each racial group, enrolled in special education, and who are male. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

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

	(1)	(2)	(3)	(4)
Panel A: Yearly Attendance Rate (Grades 9-12)				
DACAShare*Post	0.217	0.219	0.233	0.207
	(0.175)	(0.175)	(0.166)	(0.168)
Mean (Y)	0.933	0.933	0.933	0.933
Observations	798,534	798,534	798,534	798,534
Panel B: Ever Disciplined (Grades 9-12)				
DACAShare*Post	0.329	0.313	0.304	0.264
	(0.259)	(0.252)	(0.248)	(0.253)
Mean (Y)	0.0386	0.0386	0.0386	0.0386
Observations	841,929	841,929	841,929	841,929
Panel C: Cumulative GPA (Grades 9-12)				
DACAShare*Post	4.170***	4.258***	4.616***	4.572***
	(1.355)	(1.195)	(1.238)	(1.219)
Mean (Y)	2.325	2.325	2.325	2.325
Observations	798,399	798,399	798,399	798,399
Panel D: Standardized ELA Performance (Grades 9-11)				
DACAShare*Post	4.977***	5.066***	6.469***	6.539***
	(1.751)	(1.557)	(1.280)	(1.302)
Mean (Y)	0.0664	0.0664	0.0664	0.0664
Observations	490,051	490,051	490,051	490,051
Panel E: Summary Achievement Index (Grades 9-11)				
DACAShare*Post	7.903***	7.989***	8.335***	8.316***
	(1.368)	(1.202)	(1.165)	(1.134)
Mean (Y)	-0.0384	-0.0384	-0.0384	-0.0384
Observations	631,098	631,098	631,098	631,098
Controls				
Campus-Year FE	X	X	X	X
Campus-Grade FE	X	X	X	X
Demographics		X	X	X
8th Grade Std Test (ELA)			X	X
Campus-Cohort Demographics				X

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on yearly outcomes, as well as a summary index based on the outcomes in Panels C-D. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. See Table VII 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 in parentheses are clustered at the high school campus level. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

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

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

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on high school enrollment and graduation, as well as a summary index based on the outcomes in Panels A-D. 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 VII for more detail on the sample and the full set of controls. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

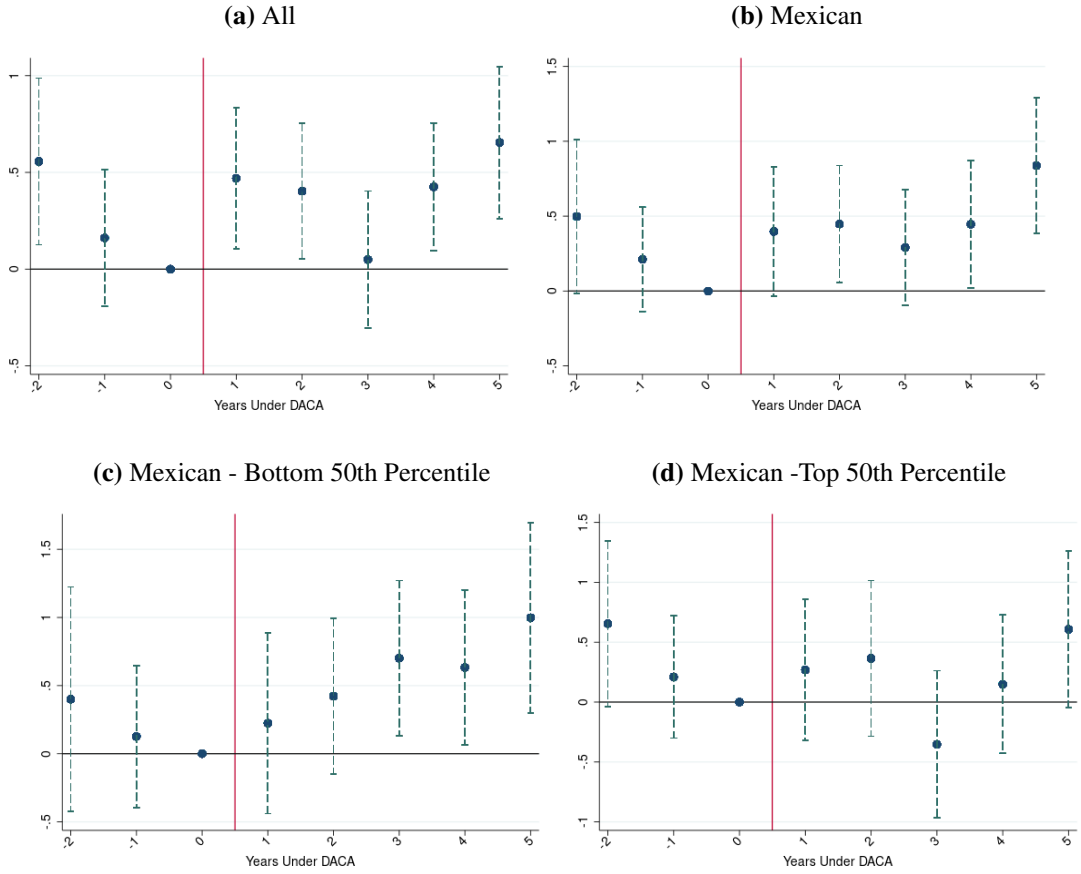
Table X: The Heterogenous Effects of DACA on Yearly Outcomes US-born students

							8th Grade ELA Test Score Quartiles			
	Full	Black	Hispanic	White	Female	Male	($\leq$ 25)	(25 - 50)	(50 - 75)	$\geq$ 75
Panel C: Semester GPA										
Panel A: Yearly Attendance Rate (Grades 9-12)										
DACAShare*Post	0.207	0.530	0.183	-0.433	0.236	0.176	0.315	0.119	0.237	0.152
	(0.168)	(0.370)	(0.181)	(0.446)	(0.182)	(0.183)	(0.224)	(0.223)	(0.210)	(0.145)
Mean (Y)	0.933	0.914	0.932	0.941	0.932	0.934	0.899	0.927	0.943	0.958
Observations	79,8534	72,414	634,081	48,934	392,147	406,387	187,657	194,126	209,905	206,846
Panel B: Ever Disciplined (Grades 9-12)										
DACAShare*Post	0.264	0.453	0.285	-0.422	0.255	0.259	0.530	0.754**	0.705***	0.380**
	(0.253)	(0.763)	(0.231)	(0.572)	(0.192)	(0.336)	(0.351)	(0.291)	(0.227)	(0.179)
Mean (Y)	0.0386	0.0934	0.0342	0.0293	0.0260	0.0509	0.0664	0.0422	0.0306	0.0172
Observations	841,929	79,443	665,972	51,922	413,215	428,714	203,040	205,793	219,330	213,766
Panel C: Cumulative GPA (Grades 9-12)										
DACAShare*Post	4.572***	2.996	4.345***	-0.205	5.022***	4.061***	6.830***	5.955***	3.038*	3.584**
	(1.219)	(2.295)	(1.328)	(4.465)	(1.418)	(1.268)	(1.465)	(1.388)	(1.727)	(1.600)
Mean (Y)	2.325	2.130	2.260	2.809	2.482	2.173	1.639	2.050	2.465	3.058
Observations	798,399	72,470	633,683	49,072	393138	405261	186016	194656	210514	207213
Panel D: Standardized ELA Performance (Grades 9-11)										
DACAShare*Post	6.539***	3.171	5.506***	-1.575	6.606***	6.476***	3.923***	7.333***	5.686***	7.727***
	(1.302)	(3.602)	(1.220)	(3.646)	(1.282)	(1.464)	(1.380)	(1.714)	(1.689)	(1.579)
Mean (Y)	0.0664	-0.138	-0.0150	0.752	0.159	-0.0242	-0.880	-0.359	0.205	1.082
Observations	49,0051	43,671	388,816	30,346	242,586	247,465	107,056	119,095	132,225	131,675
Panel E: Summary Index (Grades 9-11)										
DACAShare*Post	6.331***	3.044	6.213***	-3.019	7.318***	5.335***	5.993***	7.502***	5.148***	6.007***
	(0.890)	(1.839)	(0.958)	(3.901)	(0.964)	(1.077)	(1.201)	(1.045)	(1.358)	(1.289)
Observations	810909	74633	643125	49694	398849	412060	191444	198040	212904	208521

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. 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 VII 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 in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

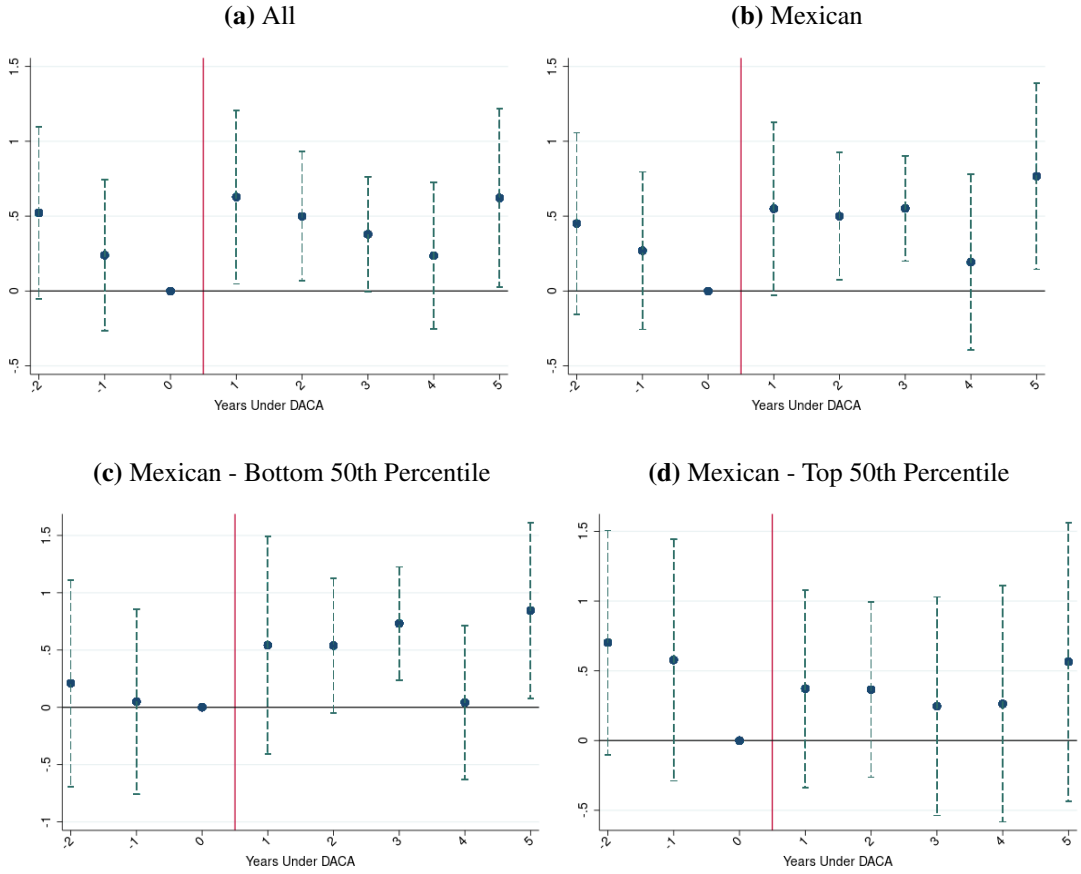
A Appendix

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



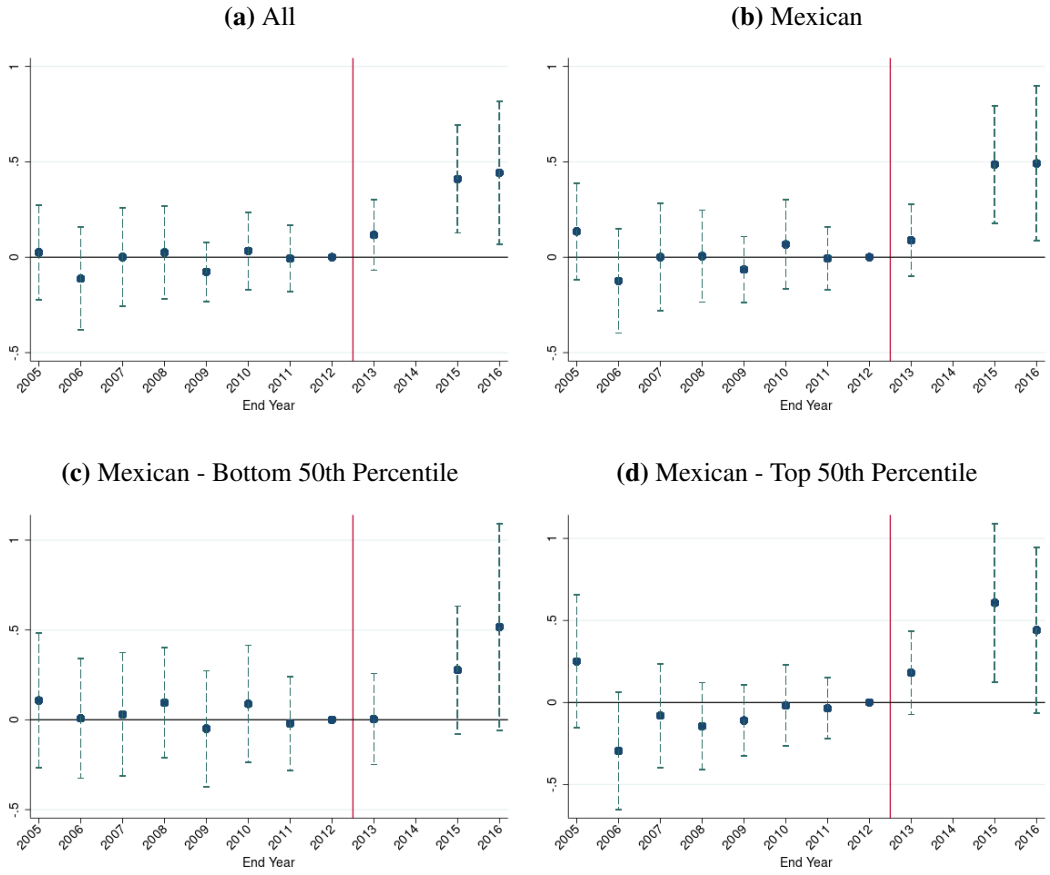
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and ShareEligible$_z$. The dependent variable is an indicator for 12th grade enrollment. The sample includes foreign-born Hispanic students who arrived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

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



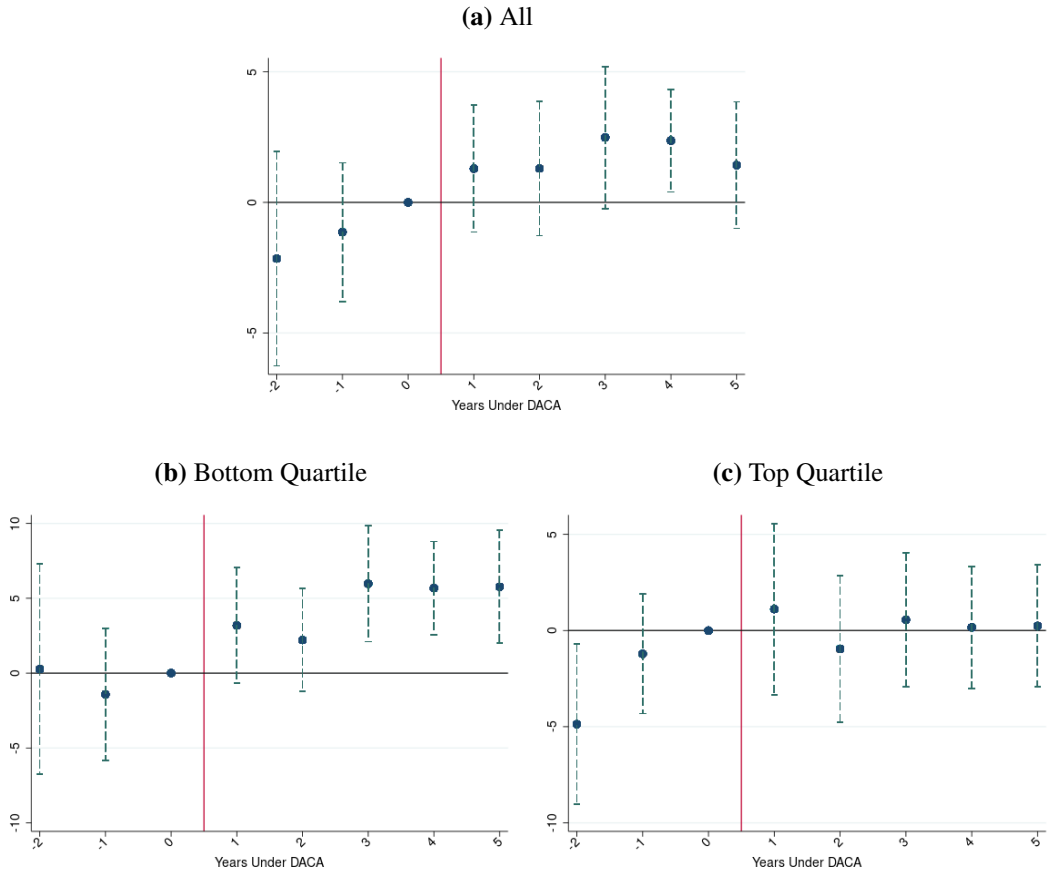
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and ShareEligible$_z$. The dependent variable is an indicator for high school completion. The sample includes foreign-born Hispanic students who arrived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

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



Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and ShareEligible$_z$. The dependent variable is performance on the ELA standardized exam. The sample includes foreign-born Hispanic students who arrived to the US by age 9 in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in the sub-figure labels. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table III 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.

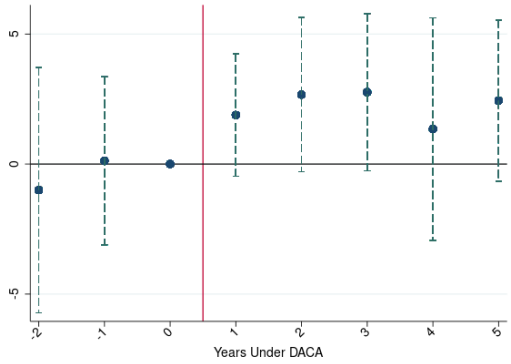
Figure A.IV: Event Study Estimates of the Spillover Effects of DACA on 12th Grade Enrollment, US-born Students



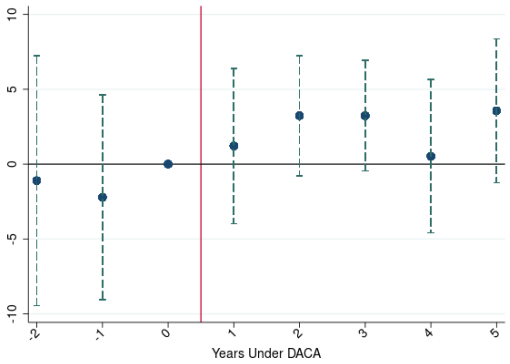
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between 9th grade cohort dummies and DACAShare$_{sc}$. The dependent variable is an indicator for whether a student was enrolled in 12th grade. The subsample is shown in the sub-figure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

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

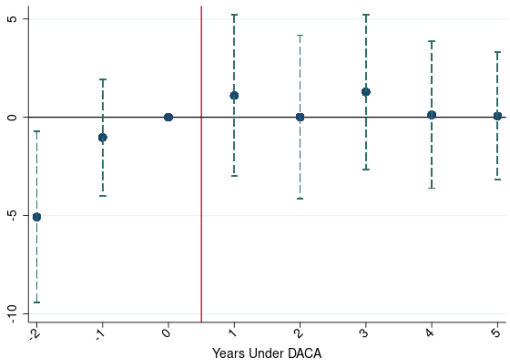
(a) All



(b) Bottom Quartile



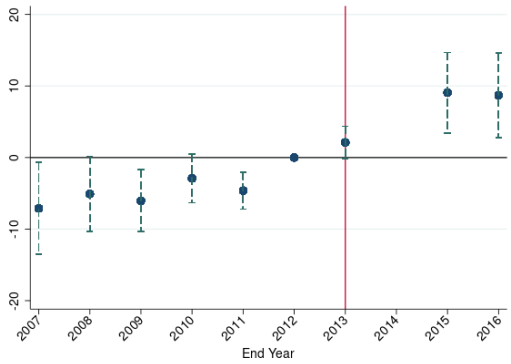
(c) Top Quartile



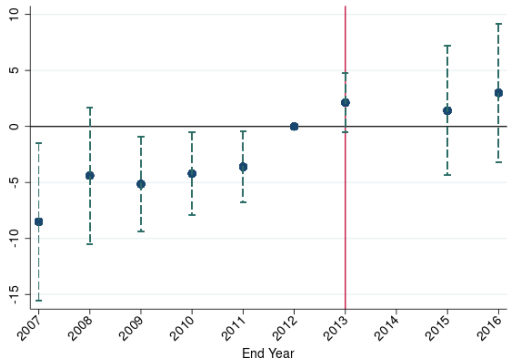
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between 9th grade cohort dummies and DACAShare_{sc}. The dependent variable is an indicator for whether a student completed high school. The subsample used is shown in the sub-figure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

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

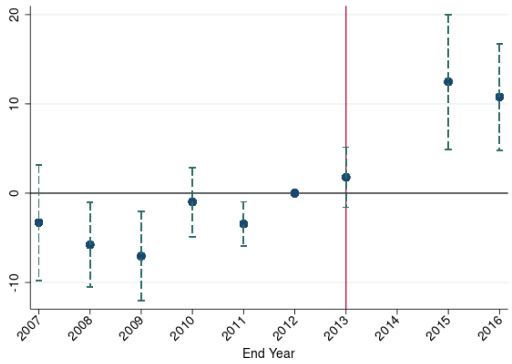
(a) All



(b) Bottom Quartile



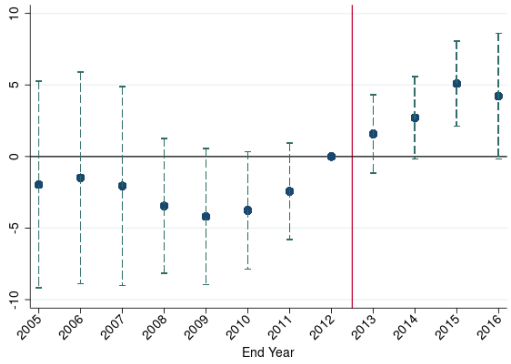
(c) Top Quartile



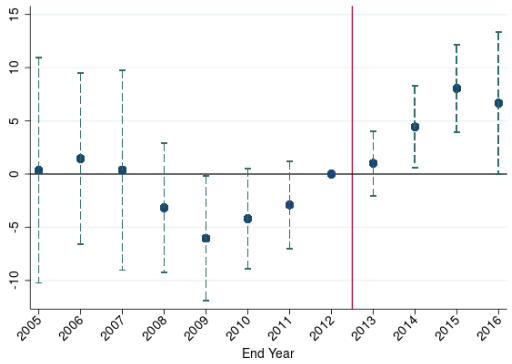
Note: These figures plot coefficients and 95% confidence intervals from event-study regressions that estimate interactions between calendar year dummies and DACAShare_{sgt}. The dependent variable is performance on the ELA standardized exam. The sample includes US-born students in 9th grade cohorts between 2004-05 to 2013-14. The sub-sample is shown in sub-figure labels. The 2012 calendar year is omitted, so estimates are relativeSee Table VII 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.VII: Event Study Estimates of the Impact of DACA on Semester GPA, US-born Students

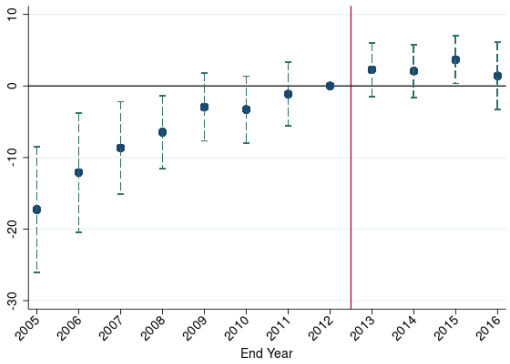
(a) All



(b) Bottom Quartile



(c) Top Quartile



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

Table A.I: 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.II: 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)
ShareEligible* Exposed	0.0302	0.0927	0.0751	-0.0362	0.0552	0.300	0.425*	0.425
	(0.0551)	(0.165)	(0.526)	(0.0975)	(0.108)	(0.287)	(0.243)	(0.302)
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

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. All regressions include zip, cohort, and high school campus fixed effects. See Table III for more detail on the sample. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

Table A.III: 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)
DACAShare* Exposed	-0.176	0.892**	-0.679	-0.332	3.027	0.166	-1.535	-1.014	1.226
	(0.206)	(0.385)	(0.605)	(0.377)	(2.439)	(0.312)	(1.280)	(1.422)	(1.747)
Mean (Y)	0.576	0.103	0.781	0.510	0.655	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

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. The demographic variables are measured as of 9th grade. All regressions include 9th grade campus and cohort fixed effects. See Table VII for more detail on the sample. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.IV: 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 15-31	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. our preferred specification), Column 2 uses a modified version of Equation 1 that accounts for the fraction of DACA-applicants estimated to be high-school aged, Column 3 uses the fraction of the foreign-born population ages 1-18 estimated to be undocumented by the Migration Policy Institute (MPI) at the PUMA, and Column 4 uses the fraction of foreign-born non-citizens in a zip-code. The full set of controls and information on the sample is specified in Table III. Standard errors in parentheses are clustered at the zip-code level. The regressions in Panel C are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average foreign-born student are shown in brackets, and is defined as the coefficient multiplied by the mean fraction of foreign-born estimated to be undocumented in a given zip-code (shown in the last row of this table). *p<0.10, ** p<0.05, *** p<0.01.

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

	DACA Apps Ages 15-31	DACA Apps Ages 15-19	Estimated Undoc	Non-Citizens	None
	(1)	(2)	(3)	(4)	(5)
Panel A: Enrolled in 12th Grade					
DACAShare*Exposed	2.625***	1.152***	0.547**	0.427*	-0.0455
	(0.928)	(0.401)	(0.251)	(0.220)	(0.0867)
	[0.0246]	[0.0264]	[0.0182]	[0.0249]	[-0.00770]
Mean (Y)	0.771	0.771	0.771	0.771	0.771
Panel B: Graduated from High School					
DACAShare*Exposed	2.418**	1.261***	0.599**	0.454*	0.0704
	(1.078)	(0.464)	(0.292)	(0.236)	(0.122)
	[0.0227]	[0.0289]	[0.0199]	[0.0265]	[0.0119]
Mean (Y)	0.576	0.576	0.576	0.576	0.576
N	238,781	238,781	238,781	238,781	238,781
Panel C: Standardized Exam Performance (ELA)					
DACAShare*Post	6.539***	2.826***	1.565***	0.984***	0.0976
	(1.302)	(0.587)	(0.373)	(0.256)	(0.137)
	[0.0640]	[0.0677]	[0.0541]	[0.0600]	[0.0160]
Mean (Y)	0.0664	0.0664	0.0664	0.0664	0.0664
Observations	490,051	490,051	490,051	490,051	490,051
Mean DACA peers	0.010	0.024	0.034	0.060	0.165

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

Table A.VI: The Effect of DACA on Yearly Outcomes – Individual Student Fixed Effect Model, US-born Students

	Attendance Rate		Ever Disciplined		Semester GPA		ELA	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DACAShare*Post	0.207	-0.483***	0.264	0.431***	4.572***	-1.084***	6.539***	4.676***
	(0.168)	(0.108)	(0.253)	(0.0983)	(1.219)	(0.240)	(1.302)	(0.562)
Mean (Y)	0.933	0.933	0.0386	0.0386	2.325	2.325	0.0664	0.0664
	[0.002]	[-0.005]	[0.003]	[0.004]	[0.045]	[-0.01]	[0.064]	[0.046]
Observations	798,534	798,534	841,929	841,929	798,399	798,399	490,051	490,051
Model	Baseline	Ind. FE	Baseline	Ind. FE	Baseline	Ind. FE	Baseline	Ind. FE

Note: This table contains the baseline results (see Table VIII for more detail) and results obtained from an individual fixed effects model (Equation 7) where the DACA-peer exposure variable is regressed on yearly attendance rates, a yearly indicator for whether students were disciplined, yearly Fall GPA, and yearly performance on the English standardized exam. See Table VII for the full list of controls and information about the sample. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors in parentheses are clustered at the high school campus level. I limit this yearly analysis to the three years after 9th grade. *p<0.10, ** p<0.05, *** p<0.01

Table A.VII: 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)
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)
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)
Mean (Y)	0.0664	0.0664	0.0664	0.0664	0.0664	0.0664
Observations	490,051	490,051	490,051	490,051	490,051	490,051
Controls						
$f(t)\times$FL		X				
$f(t)\times$ G8 ELA			X			
$f(t)\times$ ELL				X		
$f(t)\times$ Cohort Size					X	
$f(t)\times$ Racial Composition						X

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

Table A.VIII: The Effect of DACA on High School Graduation – Accounting for Differences in the Fraction of Students able to Pass the High School Exit Exam on their First Attempt in 2012, US-born Students

	(1)	(2)	(3)	(4)
DACAShare*Exposed	2.418**	2.573**	2.745**	2.268*
	(1.078)	(1.075)	(1.091)	(1.174)
Mean (Y)	0.576	0.576	0.576	0.576
N	238,781	238,781	238,781	238,781
Controls				
Full Set	X	X	X	X
$f(t)\times$ Fraction Passed Math Exit		X		X
$f(t)\times$ Fraction Passed ELA Exit			X	X

Notes: This table shows difference-in-differences estimates of the spillover effects of DACA on high school graduation. These models use the full set of controls specified in Table VII and also linear time trends that vary by the fraction of 10th graders who passed the high school exit exam in 2012. See Table VII for the full list of controls and more information about the specifications that were run. Standard errors in parentheses are clustered at the campus level. *p$<$ 0.10, **p $<$ 0.05, *** p$<$ 0.01.