Online Appendix

DACA and Human Capital Investments

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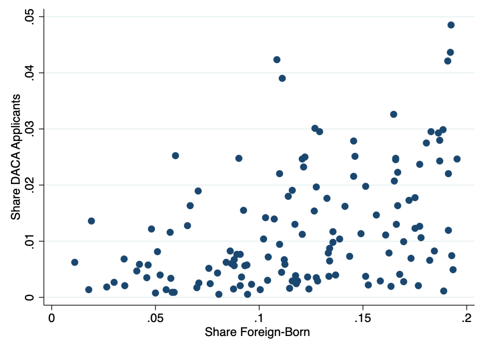
A Supplementary Figures and Tables

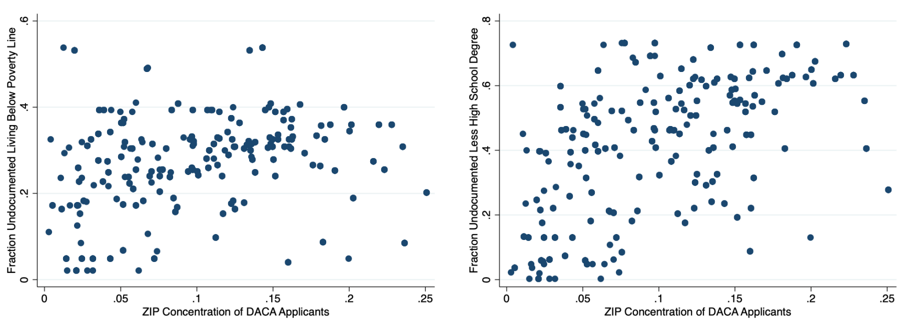
Figure A.1: Correlation Between Zip Share of Foreign-born Youth and DACA applicants



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

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

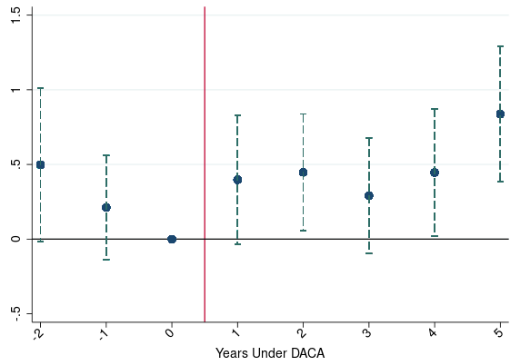
(a) Poverty Rate **(b)** Less than High School Degree



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

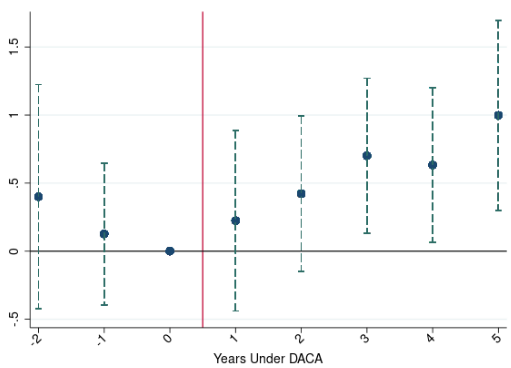
Figure A.3: Direct Impact of DACA on 12th Grade Enrollment

(a) All

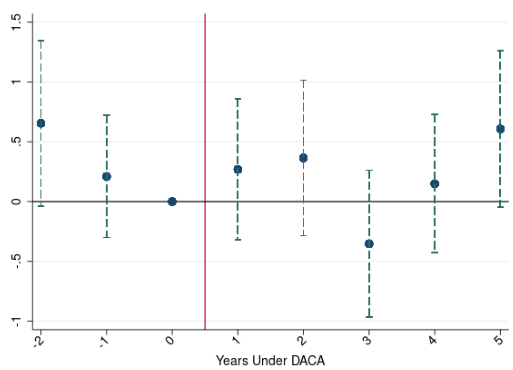


Baseline Achievement Distribution

(b) Bottom 50th Percentile



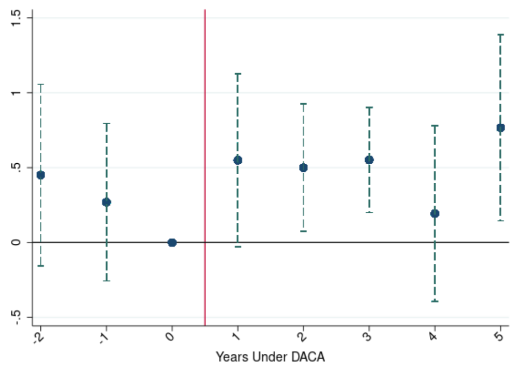
(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 3 and 95% confidence intervals. The dependent variable is an indicator for *expected* 12th grade enrollment (defined as enrollment 4 years after 9th grade). The sample includes Mexican immigrants who arrived to the US by age 9 in 9th grade cohorts between 2006-07 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 9th grade cohort from 2008-09 is omitted, so estimates are relative to that unexposed cohort. See Table 3 for more detail on the full set of controls. Standard errors are clustered by residence zip-code.

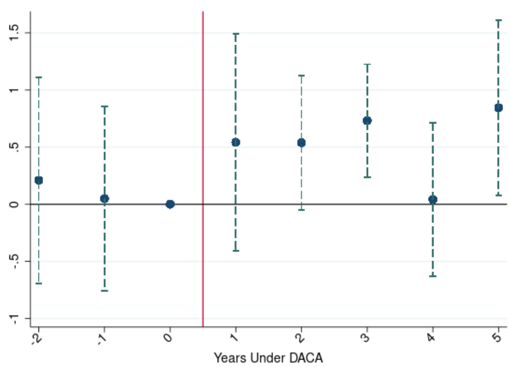
Figure A.4: Direct Impact of DACA on High School Completion

(a) All

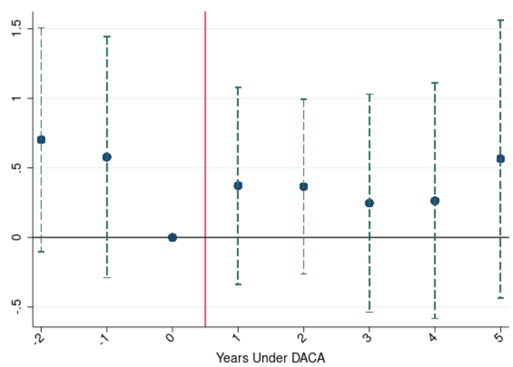


Baseline Achievement Distribution

(b) Bottom 50th Baseline Percentile



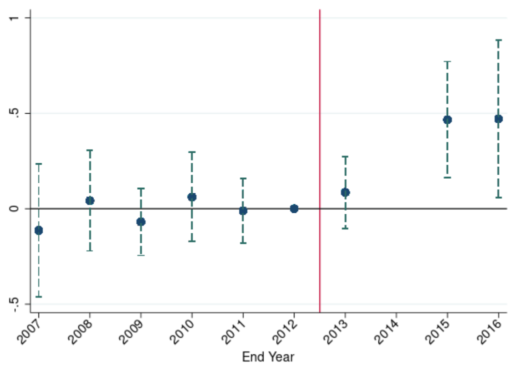
(c) Top 50th Baseline Percentile



Note: These figures plot coefficients from Equation 3 and 95% confidence intervals. The dependent variable is an indicator for high school completion, which is an indicator equal to one if a student graduated from high school within four years of 9th grade. The sample includes Mexican immigrants who arrived to the US by age 9 in 9th grade cohorts between 2006-07 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 9th grade cohort from 2008-09 is omitted, so estimates are relative to that unexposed cohort. See Table 3 for more detail on the sample and the full set of controls. Standard errors are clustered by residence zip-code.

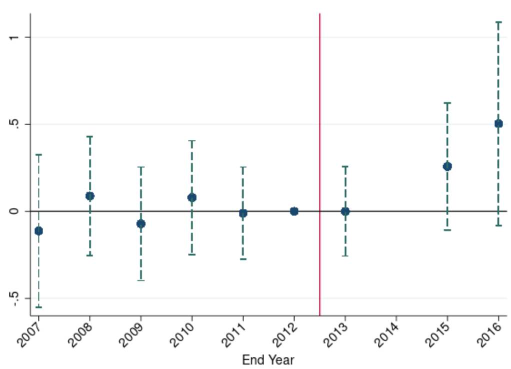
Figure A.5: Direct Impact of DACA on ELA Performance

(a) All

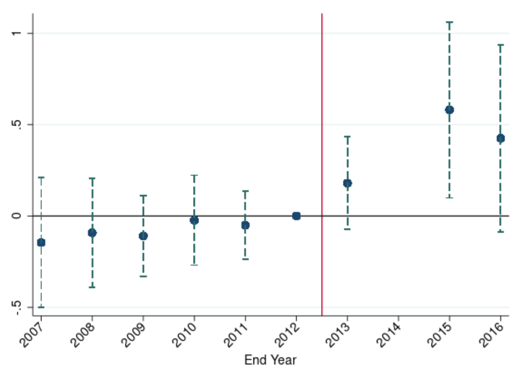


Baseline Achievement Distribution

(b) Bottom 50th Percentile



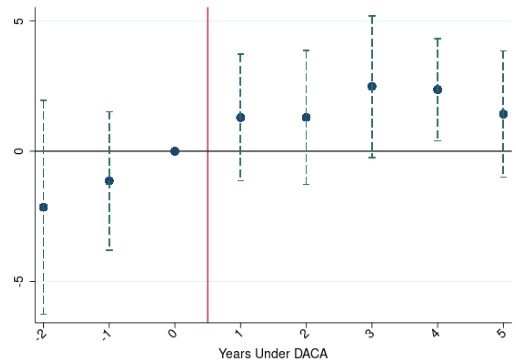
(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 5 and 95% confidence intervals. The dependent variable is performance on the ELA standardized exam. The sample includes Mexican immigrants who arrived to the US by age 9 in 9th grade cohorts between 2006-07 to 2013-14, and focus on yearly outcomes between 9-11th grade. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table 4 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors are clustered by zip-code.

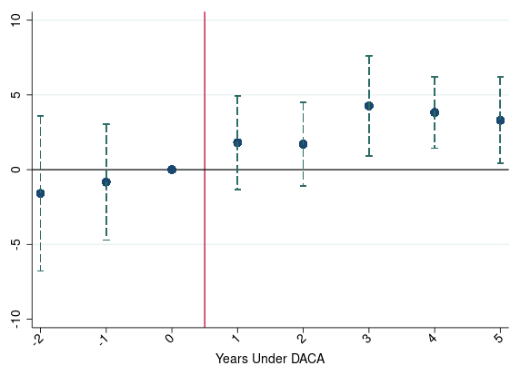
Figure A.6: Spillover Effects of DACA on 12th Grade Enrollment

(a) All

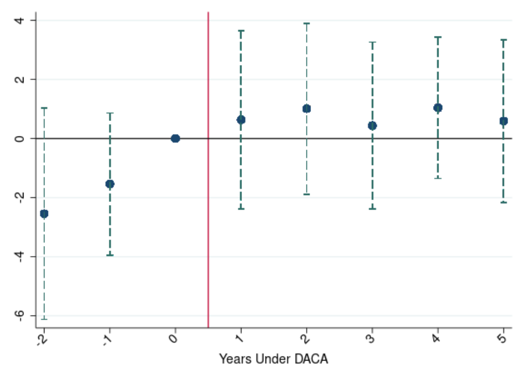


<u>Baseline Achievement Distribution</u>

(b) Bottom 50th Percentile



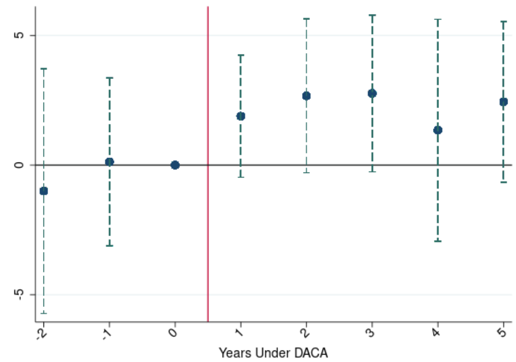
(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 7 and 95% confidence intervals. The dependent variable is an indicator for *expected* 12th grade enrollment (defined as enrollment 4 years after 9th grade). The subsample is shown in the sub-figure labels. Event time is computed by subtracting 12 from the grade each 9th grade cohort was expected to be enrolled in during the year right before the policy was implemented (or the 2011-12 school year). The sample includes US-born youth in 9th grade cohorts between 2006-07 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 9th grade cohort from 2008-09 is omitted, so estimates are relative to that unexposed cohort. See Table 4 for more detail on the sample and the full set of controls. Standard errors are clustered by high school.

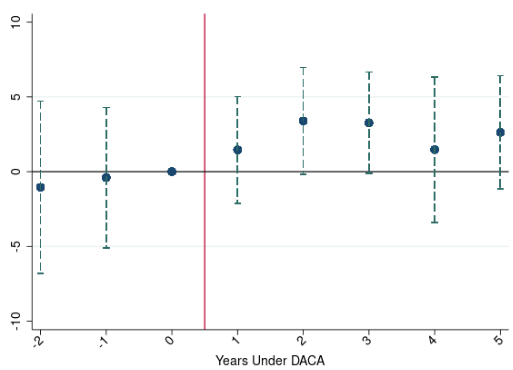
Figure A.7: Spillover Effects of DACA on High School Completion

(a) All

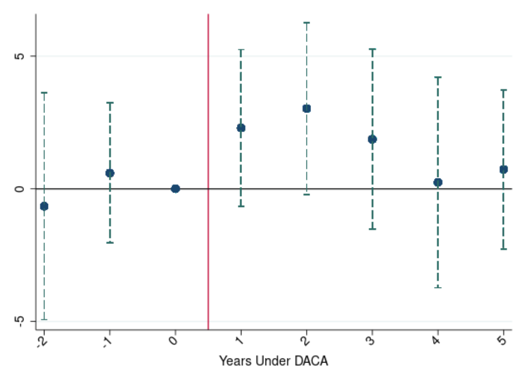


Baseline Achievement Distribution

(b) Bottom 50th Percentile



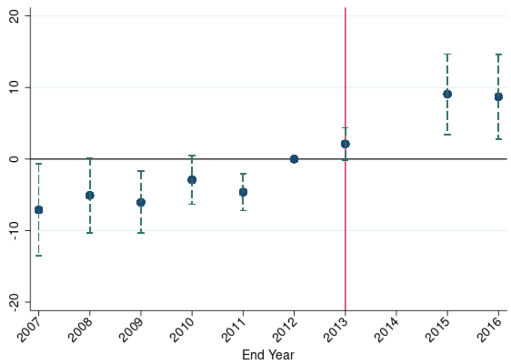
(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 7 and 95% confidence intervals. The dependent variable is an indicator for high school completion, which is an indicator equal to one if a student graduated from high school within four years of 9th grade. The subsample used is shown in the sub-figure labels. Event time is computed by subtracting 12 from the grade each 9th grade cohort was expected to be enrolled in during the year right before the policy was implemented (or the 2011-12 school year). The sample includes US-born youth in 9th grade cohorts between 2006-07 to 2013-14. The sub-sample is shown in the sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 9th grade cohort from 2008-09 is omitted, so estimates are relative to that unexposed cohort. See Table 5 for more detail on the sample and the full set of controls. Standard errors are clustered by high school.

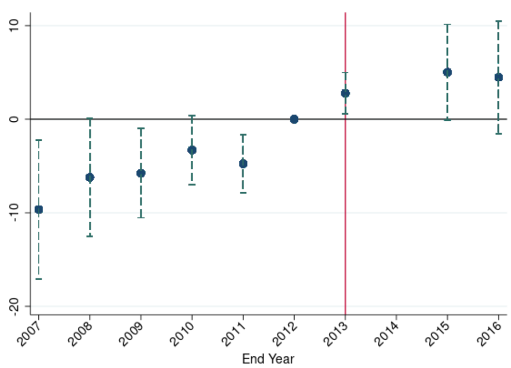
Figure A.8: Spillover Effects of DACA on ELA Performance

(a) All

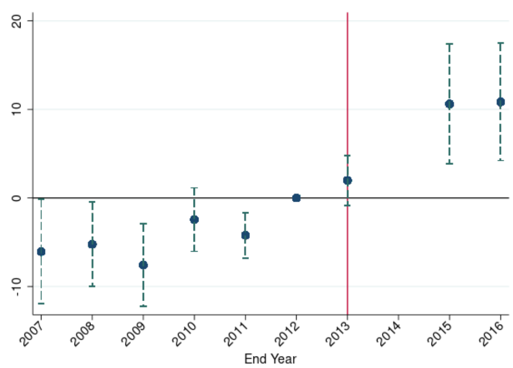


Baseline Achievement Distribution

(b) Bottom 50th Percentile



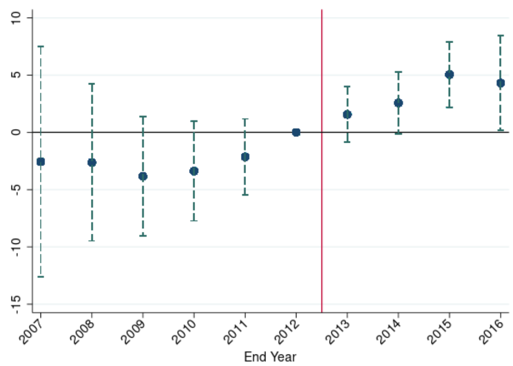
(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 9 and 95% confidence intervals. The dependent variable is performance on the ELA standardized exam. The sample includes US-born students in 9th grade cohorts between 2006-07 to 2013-14, and focus on yearly outcomes within 3 years of 9th grade. The sub-sample is shown in sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table 5 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors are clustered at the high school campus level.

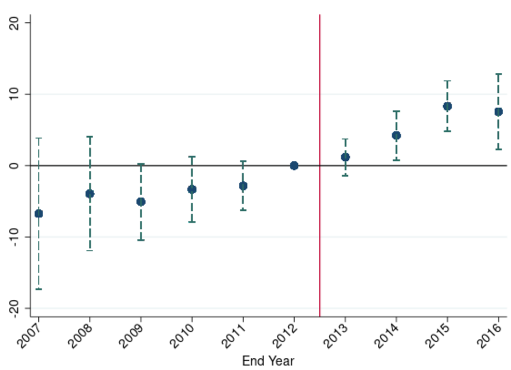
Figure A.9: Spillover Effects of DACA on Semester GPA

(a) All

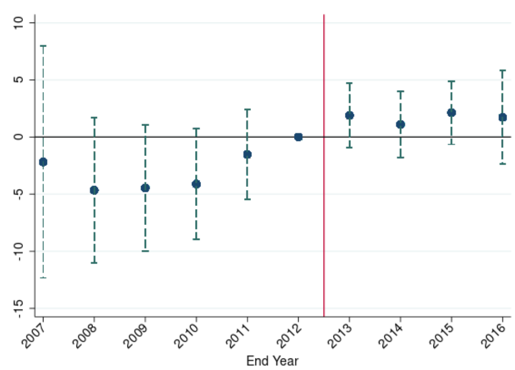


Baseline Achievement Distribution

(b) Bottom 50th Percentile

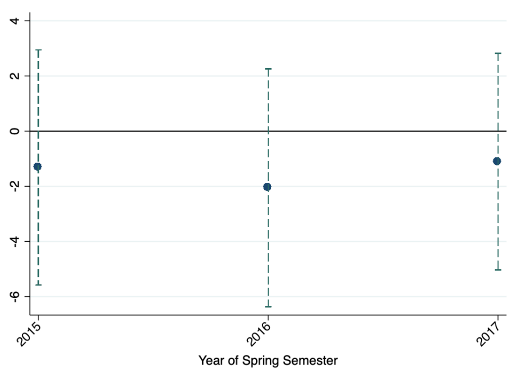


(c) Top 50th Percentile



Note: These figures plot coefficients from Equation 9 and 95% confidence intervals. The dependent variable is GPA. The sample includes US-born students in 9th grade cohorts between 2006-07 to 2013-14, and focus on yearly outcomes within 3 years of 9th grade. The sub-sample is shown in sub-figure labels. Baseline achievement percentiles are computed based on 8th grade ELA achievement. The 2012 calendar year is omitted, so estimates are relative to that pre-policy year. See Table 5 for more detail on the sample and the full set of controls. All regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors are clustered at the high school campus level.

Figure A.10: Event Study Estimates of Teacher Turnover



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

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

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

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

Table A.2: The Effect of DACA on Predicted High School Completion and Exogenous Student Characteristics, Foreign-born Hispanics

	Predicted HS Grad	Male	Age at US Arrival	Special Education	Mexican	Std ELA (G8)	Std ELA (G7)	Std Math (G7)
Panel A: Full Sample								
ShareEligible*	0.0302	0.0927	0.0751	-0.0362	0.0552	0.300	0.425*	0.425
Exposed	(0.0551)	(0.165)	(0.526)	(0.0975)	(0.108)	(0.287)	(0.243)	(0.302)
	[0.008]	[0.026]	[0.021]	[-0.010]	[0.015]	[0.083]	[0.117]	[0.117]
Mean (Y)	0.564	0.507	5.880	0.0720	0.816	-0.217	-0.193	-0.0775
N	21,139	21,139	21,139	21,139	21,139	21,139	20,169	20,157
Panel B: Full High School Enrollment Sample								
ShareEligible*	-0.0397	0.395**	0.189	-0.00427	0.0710	-0.0581	0.192	0.329
Exposed	(0.0598)	(0.174)	(0.461)	(0.0785)	(0.104)	(0.289)	(0.266)	(0.329)
	[-0.011]	[0.109]	[0.052]	[-0.001]	[0.020]	[-0.016]	[0.053]	[0.091]
Mean (Y)	0.512	0.506	5.856	0.0516	0.815	-0.145	-0.129	-0.00870
N	16,375	16,383	16,383	16,383	16,383	16,383	15,741	15,734

Note: This table contains results obtained from regressing predicted high school completion and student demographics on $(\text{ShareEligible}_z * \text{Exposed}_c)$. The sample for these regressions are foreign-born Hispanic students who arrived to the US by age 9 and were in 9th grade cohorts from 2006-07 to 2013-14. Panel A focuses on the full sample, while Panel B restricts the sample to those who were enrolled in high school for four years. All regressions include zip, cohort, and high school campus fixed effects. The impact of DACA-eligibility is shown in brackets, and is the coefficient multiplied by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

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

	(1)	(2)	(3)	(4)	(5)	(6)
					8th Grade ELA Score	
	All	Mexican	Female	Male	Bottom 50	Top 50
ShareEligible*Post	0.345	0.429	0.341	0.640	0.0249	1.231***
	(0.319)	(0.335)	(0.332)	(0.409)	(0.331)	(0.450)
	[0.095]	[0.1178	[0.094]	[0.177]	[0.007]	[0.340]
Mean (Y)	-0.0472	-0.0589	-0.0669	-0.0277	-0.354	0.299
Observations	37,957	31,367	18,798	19,159	20,235	17,722

Note: This table shows difference-in-differences estimates of the direct impact of DACA on yearly math achievement. Each column reports estimates of υ_1 from a separate regression of Equation 6. The sample for these regressions are foreign-born Hispanic students who were in 9th grade cohorts from 2006-07 to 2013-14 who arrived to the US by age 9. All regressions include zip-code, grade-year, and campus-grade fixed effects. Regressions also include the full set of individual and cohort level controls, as well as an indicator variable for which version of the math exam was taken. See Table 4 for more detail on the sample and the full set of controls. These results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment). All regressions are weighted by the inverse of the number of times a student is observed in the sample. The impact of DACA-eligibility is shown in brackets, and is the coefficient multiplied by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth. Standard errors in parentheses are clustered by residence zip-code. *p<0.10, ** p<0.05, *** p<0.01.

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

	Predicted HS Grad	Black	Hispanic	Male	Free-Lunch	Special Education	ELA (G8)	ELA (G7)	Math (G7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full Sample									
DACAShare*	-0.505	1.160**	-0.898	-0.324	-0.005	0.101	-1.290	-0.722	2.136
Exposed	(0.338)	(0.487)	(0.785)	(0.451)	(1.218)	(0.389)	(1.869)	(1.895)	(2.147)
	[-0.005]	[0.011]	[-0.008]	[-0.003]	[0.000]	[0.001]	[-0.012]	[-0.007]	[0.020]
Mean (Y)	0.547	0.103	0.781	0.510	0.695	0.087	-0.046	-0.008	0.049
N	238,781	238,781	238,781	238,781	238,781	238,781	238,781	224,625	224,701
Panel B: Full High School Enrollment Sample									
DACAShare*	-1.478***	-0.565	3.808***	0.613	0.952	-0.748	-8.680***	-7.955***	-6.422**
Exposed	(0.385)	(0.626)	(0.953)	(0.488)	(1.004)	(0.543)	(2.206)	(2.127)	(2.581)
	[-0.015]	[-0.006]	[0.039]	[0.006]	[0.010]	[-0.007]	[-0.088]	[-0.081]	[-0.065]
Mean (Y)	0.547	0.0867	0.797	0.506	0.702	0.177	0.0299	0.0584	0.125
N	184,170	184,170	184,170	184,170	184,170	184,170	184,170	176,071	176,167

Note: This table contains results obtained from regressing predicted high school completion and student demographics on DACAShare$_{sc}$ $\times$ Exposed$_c$. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. Panel A focuses on the full sample, while Panel B restricts the sample to those who were enrolled in high school for all four years. The demographic variables are measured as of 9th grade. All regressions include 9th grade campus and cohort fixed effects. See Table 5 for more detail on the sample. The effect size for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets. This is defined by first re-scaling the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth, and then multiplying the re-scaled coefficient by 0.034. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

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

							Baseline Achievement	
	All	Black	Hispanic	White	Female	Male	Bottom 50	Top 50
DACAShare*Post	5.710**	0.174	6.020**	-5.659	6.221**	5.177*	5.060**	7.853*
	(2.788)	(3.393)	(2.929)	(7.930)	(2.961)	(2.759)	(2.221)	(4.039)
	[0.056]	[0.002]	[0.059]	[-0.055]	[0.061]	[0.051]	[0.050]	[0.077]
Mean (Y)	0.0326	-0.223	-0.0249	0.585	0.0166	0.0483	-0.378	0.377
N	433827	38822	343937	26689	214496	219331	198927	234900

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on yearly math achievement. Each column reports estimates of ρ_1 from a separate regression of Equation 10. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. All regressions include campus-year and campus-grade fixed effects. Regressions also include the full set of individual, cohort level controls, and an indicator variable for which version of the math exam was taken. See Table 5 for more detail on the sample and the full set of controls. These results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment). All regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect size for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets. This is defined by first re-scaling the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school youth, and then multiplying the re-scaled coefficient by 0.034. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.6: The Effect of DACA on Attainment and Achievement using Quartile Cutoffs of DACA-eligible peer share, US-Born Students

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

Note: This table contains difference-in-difference estimates of the spillover effects of DACA on educational attainment outcomes (Columns 1-3) and on achievement outcomes (Columns 4-5). Each column reports results from a separate regression. In Panel A, Columns 1-3 shows estimates of ζ_1 from Equation 8 and Columns 4-5 shows estimates of ρ_1 from Equation 10. The effect size of DACA for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets, and is defined as the re-scaled coefficient multiplied by 0.034. See Table 5 for more detail on how the coefficients are re-scaled. Panel B shows a similar set of results where the share of DACA-eligible peers is split into quartiles (0-1.4%, 1.5-3.7%, 3.8-5.5%, 5.6-11%), where the indicator for having 0-1.4% DACA-eligible peers is excluded from the regression. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. All regressions include campus-year and campus-grade fixed effects. Regressions also include the full set of individual and cohort level controls. See Table 6 for more detail on the full set of controls. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table A.7: Peer Effects of DACA on Educational Attainment and Achievement – Accounting for Differences in Campus-Level Characteristics, US-born Students

Panel A: Enrolled in 12th Grade						
DACAShare*Exposed	2.625***	2.526**	3.426***	3.823***	2.336***	2.826
	(0.928)	(1.127)	(1.163)	(1.095)	(0.875)	(1.843)
	[0.025]	[0.024]	[0.032]	[0.036]	[0.022]	[0.027]
Mean (Y)	0.771	0.771	0.771	0.771	0.771	0.771
Panel B: Graduated from High School						
DACAShare*Exposed	2.418**	2.642**	3.450***	3.403**	2.220**	1.024
	(1.078)	(1.235)	(1.270)	(1.449)	(1.040)	(1.703)
	[0.023]	[0.025]	[0.032]	[0.032]	[0.021]	[0.010]
Mean (Y)	0.576	0.576	0.576	0.576	0.576	0.576
N	238781	238781	238781	238781	238781	238781
Panel C: Standardized Exam Performance (ELA)						
DACAShare*Post	6.539***	5.367***	5.164***	4.920***	6.404***	2.643*
	(1.302)	(1.661)	(1.684)	(1.402)	(1.294)	(1.380)
	[0.064]	[0.053]	[0.051]	[0.048]	[0.063]	[0.026]
Mean (Y)	0.0664	0.0664	0.0664	0.0664	0.0664	0.0664
Observations	490,051	490,051	490,051	490,051	490,051	490,051
Controls						
$f(t)\times$FL		X				
$f(t)\times$ G8 ELA			X			
$f(t)\times$ ELL				X		
$f(t)\times$ Cohort Size					X	
$f(t)\times$ Racial Composition						X

Notes: This table shows difference-in-differences estimates of the spillover effects of DACA on high school enrollment and graduation, as well as on yearly standardized test performance on ELA exams. These models use the full set of controls specified in Tables 5 and 6, as well as linear time trends that vary by the fraction of a campus that received free or reduced price lunch (FRL), average baseline ELA achievement, the fraction of the campus that was classified as an English Language Learner (ELL), the size of the cohort, and the fraction of the campus belonging to each of the largest racial groupings (Hispanic, black, white, and asian), all measured in 2012. In Panels A and B, each column reports estimates of ζ_1 from a separate regression of Equation 8. In Panel C, each column reports estimates of ρ_1 from a separate regression of Equation 10. See Table 5 for more detail on the full set of controls and sample in Panels A-B and see Table 6 for the full set of controls and sample in Panel C. In Panel C, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average high school student with 1 percent DACA-eligible peers are shown in brackets, and is defined as the coefficient multiplied by .01. Standard errors in parentheses are clustered at the high school campus level. *$p< 0.10$, **$p < 0.05$, *** $p< 0.01$.

Table A.9: The Effect of DACA on Campus Switching, US-Born Students

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

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on high school campus switching within LAUSD after expected 9th grade. Within each panel, each column reports estimates of ζ_1 from a separate regression of Equation 8. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14 who continue to be enrolled in 10th, 11th or 12th grade. All regressions include 9th grade cohort and campus fixed effects. Regressions also include the full set of individual and cohort level controls. See Table 5 for more detail on the full set of controls. The effect size for the average high school student with 3.4 percent DACA-eligible peers are shown in brackets. This is defined by first re-scaling the coefficients to account for undercounting in the share DACA-eligible peer measure by multiplying the coefficients by 0.69 to account for incomplete take-up and by 0.40 to account for the difference in take-up of high school students. Then the re-scaled coefficient is multiplied by 0.034. Standard errors in parentheses are clustered at the high school campus level. *$p<0.10$, ** $p<0.05$, *** $p<0.01$.

B Considering Other Pathways for Spillovers

B.1 Family Spillovers

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

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

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

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

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

B.2 Neighborhood Spillovers

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

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

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

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

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

Note: This table contains difference-in-difference estimates of the spillover effects of DACA on a summary index of educational attainment (Columns 1-2) and a summary index of academic achievement (Columns 3-4). Odd-numbered columns show the results for US-born students who speak Spanish at home, while the even-numbered columns show the results for US-born students who do not speak Spanish at home. Each column reports results from a separate regression. Columns 1-2 show estimates of ζ_1 from Equation 8 and Columns 3-4 shows estimates of ρ_1 from Equation 10. For Columns 3-4, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

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

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

Note: This table contains difference-in-difference estimates of the spillover effects of DACA on a summary index of educational attainment (Columns 1-4) and a summary index of academic achievement (Columns 5-8). Columns 1 and 5 show the results using the original sample of students, while columns 2 and 6 show the results using the restricted sample of schools that have students with at least two zip-codes that are home to at least 10% of students in a school. Columns 3 and 7 show the results where DACA peer exposure is defined at the zip-code level. Columns 4 and 8 show the results with both treatment variables (i.e. treatment defined at both the zip-code and school level). Each column reports results from a separate regression. Columns 1-4 show estimates of ζ_1 from Equation 8. and Columns 5-8 shows estimates of ρ_1 from Equation 10. For Columns 5-8, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. In the main analysis sample (Columns 1 and 5) there are 3.4% likely DACA-eligible peers at the average campus, in the restricted sample with enough variation in home zip-code (Columns 2 -4 and 6-8) there are 4.9% DACA-eligible peers at the average campus, and at the average zip-code there are 5.2% likely DACA-eligible youth. Shown below in square brackets are the effects at the average campus and in curly brackets at the average zip-code, and is defined as the re-scaled coefficient multiplied by the average DACA share. See Table 5 for more detail on how the coefficients are re-scaled. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

C Accounting for Contemporaneous Policy Changes

C.1 California Dream Act (CDA)

The CDA act was signed into law on October 8, 2011 (roughly a year before DACA's enactment), and implemented in January of 2013.[6] While California's undocumented youth had been eligible for in-state tuition (IST) since 2001, CDA's introduction further increased college affordability by allowing undocumented students to qualify for state financial aid. Conditional on meeting GPA and income criteria, undocumented students could qualify for the Cal Grant, California's primary financial aid program.[7] Cal Grant receipt can lower annual college costs by as much as 23-38%.[8] These cost reductions could have made college more attractive for undocumented students and incentivized effort during high school during this period in order to qualify for the Cal Grant.[9]

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

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

[6] 9th graders expected to graduate high school in 2013 were the first cohort to be able to qualify for the CDA and were also the first cohort to be exposed to DACA before expected high school graduation.

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

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

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

and the literature on whether financial aid eligibility enters college enrollment decisions during high school is mixed (Deming & Dynarski, 2009). A recent meta-analysis synthesizing all IST studies finds that the median increase in college enrollment due to IST policies is 2 p.p. (Carroll, Gamez, & Clasing-Manquain, 2022). Although imprecisely estimated, Kuka et al. (2020) find suggestive evidence that DACA increased college enrollment by as much as 6 p.p.. In the context of the Cal Grant specifically which was the financial aid package undocumented youth became eligible for under the CDA, Bettinger et al. (2019) find that Cal Grant eligibility does not increase college enrollment.[10] In addition, CDA's introduction did not lead to increases in educational attainment or achievement for the small subset of undocumented youth who were eligible for CDA but not for DACA due to arriving to the US at older ages.[11] Finally, turning to the ACS I replicate the results of Kuka et al. (2020) but separate the effects separately for California and the rest of the US to see if the results differ in California, perhaps driven by the CDA's introduction.[12] Overall, I find that the direct impacts of DACA were fairly similar in California and the rest of the US. Taken together, these results suggest that the CDA alone is unlikely to be the primary driver of the increases in effort.

C.2 Other Education Policy Changes

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

Around the time of DACA's introduction LAUSD students experienced changes in school

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

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

[12] These results are available upon request.

discipline and graduation requirements. In the summer of 2013, LAUSD introduced policies that significantly reduced overall suspensions.[13] Beginning in 2015, students were no longer required to pass the high school exit exam in order to graduate.[14] And the introduction of online credit-recovery courses around this time has anecdotally been linked to increases in graduation rates.[15] If schools with higher concentrations of DACA-eligible students were also more likely to be impacted by these changes in discipline or graduation requirements, then I could be misattributing the increases in educational outcomes to DACA. Reassuringly, Appendix Table C.1 shows that the concentration of DACA-eligible students is uncorrelated with baseline discipline and graduation rates. Thus, it is unlikely that any policies impacting high schools with low graduation or high discipline rates would have had a stronger effect in campuses with higher fractions of likely DACA-eligible youth.[16]

To more formally rule out the possibility that alternate educational policies are driving my results, I estimate Equation 8 including campus-level time trends that vary by the fraction of students who were unable to pass the high school exit exam on their first attempt in 2012, who were suspended in 2012, and who graduated high school during the pre-policy period. Appendix Table C.2 presents spillover results that include these campus-level trends on the summary index of educational attainment (Panel A) and the summary index of academic achievement (Panel B). These results demonstrate that my results are robust to the inclusion of such trends. This suggests that even after controlling for the possibility that campuses more impacted by these other educational

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

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

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

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

policies were trending differently, I still find a positive and significant relationship between the concentration of DACA-eligible peers and the educational outcomes of US-born students.

To further rule out the possibility that my spillover results are driven by changes in graduation requirements or discipline, I turn to exploring heterogeneity by the likelihood of graduating from high school and baseline discipline. Specifically, I use all covariates to predict the likelihood a student graduated from high school. Columns 1-3 of Appendix Table C.3 shows estimates for the summary index of educational attainment (Panel A) and the summary index of academic achievement (Panel B) across terciles of the predicted likelihood of high school graduation. As expected, the increases in educational attainment are driven by US-born students who were least likely to graduate. However, all US-born students experienced improvements in achievement. Because decreasing graduation requirements alone should not led to improvements in achievement, I view it as unlikely that changes to graduation requirements alone can explain the increases in educational investments that I document. Columns 4-5 of Appendix Table C.3 test whether there were differences across baseline discipline. These results indicate that there was little difference in educational attainment across baseline discipline, but that increases in achievement were driven by those who were not disciplined at baseline. Prior research finds that changes in discipline policy benefit the short-run outcomes of those at risk of being suspended, but negatively affect their peers who are unlikely to be suspended (Pope & Zuo, 2020). The patterns I document (i.e. larger positive impacts for those unlikely to be disciplined) are not consistent with a reduction in suspensions driving my results.[17]

Finally, it is important to note there was a decrease in the fraction of ELL students over this time period. The fraction of 9th grade US-born students participating in ELL decreased from 19 to 7.6 percent between 2007 and 2014. This decline was likely driven by a 2006 policy change that removed a math requirement for ELL re-classification (Betts et al., 2020), and also by a

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

strategic plan outlined by the district to reclassify more ELL students.[18] Prior work has found that being re-classified during high school has no impact on academic performance (Pope, 2016b), and descriptive studies also find that older students' performance is not affected by changes in the rigor of ELL re-classification criteria (Kim & Herman, 2014). Thus, it is relatively unlikely that changes to ELL reclassification alone would have had a large impact on high school students' educational outcomes. Nonetheless, as shown in Appendix Table C.1 high schools with higher concentrations of likely undocumented youth had higher fractions of ELL students and would have been more impacted by any changes in ELL re-classification practices.

To further rule out the possibility that my spillover results are being driven by the reduction in ELL participation, I reestimate Equations 8 and 10 including campus-level time trends that vary by the fraction of 9th grade students receiving ELL services in 2007. Column 7 of Appendix Table C.2 demonstrate that the estimates are robust to the inclusion of this trend. In addition, Columns 6-7 of Appendix Table C.3 demonstrate that the positive spillover effects of DACA are larger for students who were not enrolled in ELL programs at baseline. If it were the case that my results are driven by changes in ELL re-classification policies, then students in ELL programs should be most affected. I find larger effects for non-ELL students, which is not consistent with ELL policy changes driving my results. Finally, controlling for ELL status as of 8th grade has no impact on my estimates.

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

Table C.1: Additional Educational Policy Pressures

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

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

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

Table C.2: Peer Effects of DACA on Educational Attainment and Achievement – Accounting for Other Educational Policies, US-born Students

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

Notes: This table shows difference-in-differences estimates of the spillover effects of DACA on a summary index of educational attainment (Panel A) and academic achievement (Panel B). In Panel A, each column reports estimates of ζ_1 from a separate regression of Equation 8. In Panel B, each column reports estimates of ρ_1 from a separate regression of Equation 10. These models use the full set of controls specified in Tables 5 and 6, as well as linear time trends that vary by campus level characteristics, including the fraction of 10th graders who passed the high school exit exam in 2012 are shown in, the discipline rate in 2012, the graduation rate for pre-policy 9th grade cohorts (i.e. those in 9th grade between 2007 and 2010), and the fraction of 9th grade ELL students. See Table 5 for more detail on the full set of controls and sample in Panels A-B and see Table 6 for the full set of controls and sample in Panel C. In Panel C, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors in parentheses are clustered at the high school campus level. *$p< 0.10$, **$p < 0.05$, *** $p< 0.01$.

Table C.3: The Heterogenous Effects of DACA By Predicted Likelihood of High School Graduation and Baseline Characteristics, US-born students

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

Note: This table shows difference-in-differences estimates of the spillover effects of DACA on a summary index of educational attainment and a summary index of educational achievement for students with different likelihoods of graduating high school and whether students were disciplined at baseline (in 8th grade). I use the full set of controls to predict the likelihood of graduating from high school. This likelihood is the split into three terciles, from the lowest likelihood in column (1) to the highest likelihood in column (3). In Panel A, each column reports estimates of ζ_1 from a separate regression of Equation 8. In Panel B, each column reports estimates of ρ_1 from a separate regression of Equation 10. The sample for these regressions are US-born students who were in 9th grade cohorts from 2006-07 to 2013-14. See Table 5 for more detail on the full set of controls and sample in Panel A and see Table 6 for the full set of controls and sample in Panel B. In Panel B, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

D Using Alternate Ways of Approximating DACA-eligibility

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

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

Turning to measures that do not select on the DACA application decision and will likely over-estimate the DACA-eligible population, Columns 3 and 4 proxy for likely DACA eligibility using the fraction of non-citizens adjusted to more accurately identify the undocumented population living in a PUMA and by the fraction of non-citizens in a zip-code, respectively.[19] Again, the effect sizes are shown in brackets, and are computed by taking the coefficient and multiplying by the

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

average of each measure. Reassuringly, the average effects and conclusions are nearly identical regardless for how I proxy for likely DACA-eligibility.[20] Finally, Column 5 shows that there are no spillovers driven by the share of foreign-born students. The fact that the estimates in this table are only significant after proxying for the likelihood that these foreign-born peers are undocumented, suggest that I am able to capture the peer effects stemming from having more undocumented peers after DACA's enactment rather than spillovers from having more foreign-born peers after 2012.

[20]Similarly, Appendix Table D.2 demonstrates that the direct impacts of DACA are largely robust to using several different measures to approximate undocumented status that do not select on the DACA application decision.

Table D.1: The Effect of DACA on Educational Investments of US-Born Students – Robustness of Results to Scaling of Foreign-Born Peer Measure

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

Note: This table contains difference-in-differences estimates where the fraction of undocumented peers is approximated in several different ways. Column 1 uses Equation 1 to approximate undocumented status of one's foreign-born hispanic peers (i.e. my main specification), Column 2 uses a modified version of Equation 1 that accounts for the fraction of DACA-applicants estimated to be high-school aged, Column 3 uses the fraction of the foreign-born population ages 1-18 who were non-citizens making a number of statistical adjustments to more accurately identify the undocumented population living in a PUMA done by the Migration Policy Institute (MPI), Column 4 uses the fraction of foreign-born non-citizens in a zip-code, and Column 5 focuses on the fraction of one's peers who were foreign-born. In Panels A and B, each column reports estimates of ζ_1 from a separate regression of Equation 8. In Panel C, each column reports estimates of ρ_1 from a separate regression of Equation 10. See Table 5 for more detail on the full set of controls and sample in Panels A-B and see Table 6 for the full set of controls and sample in Panel C. In Panel C, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average high school student is shown in brackets, and is defined as the coefficient multiplied by the mean estimated value of undocumented peers (shown in the last row of this table). Standard errors in parentheses are clustered at the high school campus level. *p<0.10, ** p<0.05, *** p<0.01.

Table D.2: The Effect of DACA on Educational Investments of Hispanic Foreign-Born Students – Robustness of Results to the Proxy Used to Approximate Undocumented Status

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

Note: This table contains difference-in-differences estimates where undocumented status is approximated in several different ways. Column 1 uses Equation 1 to approximate undocumented status (i.e. my preferred specification), Column 2 uses a modified version of Equation 1 that accounts for the fraction of DACA-applicants estimated to be high-school aged, Column 3 uses the fraction of the foreign-born population ages 1-18 who were non-citizens making a number of statistical adjustments to more accurately identify the undocumented population living in a PUMA done by the Migration Policy Institute (MPI), and Column 4 uses the fraction of foreign-born non-citizens in a zip-code. In Panels A and B, each column reports estimates of ϑ_1 from a separate regression of Equation 4. In Panel C, each column reports estimates of υ_1 from a separate regression of Equation 6. See Table 3 for more detail on the full set of controls and sample in Panels A-B and see Table 4 for the full set of controls and sample in Panel C. In Panel C, the results focus on yearly outcomes within 3 years of 9th grade enrollment (i.e. between 9th grade enrollment and *expected* 11th grade enrollment), and the regressions are weighted by the inverse of the number of times a student is observed in the sample. The effect of DACA for the average foreign-born student are shown in brackets, and is defined as the coefficient multiplied by the mean fraction of foreign-born estimated to be undocumented in a given zip-code (shown in the last row of this table). Standard errors in parentheses are clustered at the residence zip-code level. *p<0.10, ** p<0.05, *** p<0.01.