The Impact of DACA on Juvenile Crime and School Safety

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Abstract

DACA (Deferred Action for Childhood Arrivals) provides temporary work authorization and deportation protection to individuals meeting strict age, residency, education, and criminal history criteria. Although not its primary focus, DACA's requirements shift young people's incentives, and this project investigates whether those shifts prompted changes in student misbehavior. Specifically, leveraging restricted-access, student-level data from Texas, I use a Difference-in-Differences (DiD) approach to assess DACA's impact on juvenile crime and school discipline. I compare likely undocumented students to all other students before and after DACA began, using administrative proxies for documentation status that is new to the immigration and broader literature. Relative to likely undocumented students' averages, DACA decreased in-school and out-of-school suspensions per student per year by 10.9% and 16.5%, respectively. Transfers to alternative education programs, a proxy for juvenile crime, also fell by 13.7% per student per year due to DACA. The effect sizes nearly double for students who were too young to apply to DACA right away, showcasing a deterrence effect, and were biggest for non-white students.These results have important implications for disciplinary policy, demonstrating how students' incentives can be shifted to deter juvenile crime and misbehavior.

JEL Codes: K42, K37, J13, J15, I26, J61

Keywords: Juvenile Crime, School Discipline, Juvenile Delinquency, DACA

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1 Introduction

In the 2021-2022 school year alone, there were over 1.3 million criminal incidents perpetrated by students at school[1], and over 60% of these incidents were violent crimes (Burr et al., 2024). Juvenile crime is linked to lower rates of graduation and labor force participation, and higher rates of recidivism into adulthood (Ullman et al., 2024), which means that not only is it an issue for the individual perpetrators but also for the broader society. Policies, then, are critical in deterring juvenile crime, and the Deferred Action for Childhood Arrivals (DACA) is such a program.

Predominantly an immigration policy, DACA provides temporary work authorization and protection from deportation to eligible individuals who arrived as undocumented immigrants before the age of 16. Along with age and residency criteria, DACA also requires that recipients must 1) be currently in school or graduated high school and 2) not have committed any felonies, multiple misdemeanors, or otherwise be deemed as a threat to national safety or security. These two requirements shift incentives for young undocumented individuals, which could lead to a shift in behaviors. This leads to the two main research questions in this paper: 1) Did DACA deter juvenile crime? and 2) Did DACA impact rates of student misbehavior?

Both questions are empirically ambiguous. Previous literature from Becker (1964, 1974) and Keane and Wolpin (1997) postulate that if the returns to crime(education) decrease(increase), then investment in these activities should also decrease(increase). This then would suggest that DACA leads to less juvenile crime and school discipline[2]. However, the cost(return) of crime(education) is already so high(low) for undocumented students, DACA may not lead to any measurable behavioral difference.

[1]This includes both on campus and at school-sponsored events.

[2]If investments in education increase, we should see less student misbehavior as their focus shifts to investing more in their academic success.

To explore whether DACA impacted student misbehavior, I use a Difference-in-Differences (DiD) methodology. The first difference is across proxied documentation status, and the second difference is time relative to DACA implementation. Using the Texas Education Agency's student-level administrative data, I proxy for documentation status using a combination of administrative flags to identify students who were born outside of the U.S. and do not have Social Security Numbers.[3] Thus, my treatment group are students who were born outside of the U.S. and do not have SSNs, while my control group consists of all other students. I also leverage student fixed effects and dynamic controls for age to analyze five main outcomes of interest: two measures of student misbehavior (In-school and Out-of-school suspensions), and three proxy measures for juvenile crime (Expulsions, Transfers to Disciplinary Alternative Education Programs (DAEPs), and Transfers to Juvenile Justice Alternative Education Programs (JJAEPs)).[4]

I find consistent evidence that DACA deterred juvenile crime and misbehavior. In-school suspensions dropped by 10.9% of the treatment groups' (immigrants students without SSNs) mean, and Out-of-school suspensions dropped by 16.5%. Transfers to Disciplinary Alternative Education Programs (DAEPs) dropped by 13.7%, and both Expulsions and Transfers to Juvenile Justice Alternative Education Programs (JJAEPs) consistently decrease albeit with less precision. These effects are largest for non-white students, and the treatment effect was almost double for students who were too young to apply to DACA immediately (i.e., they were 14 or younger in 2012), suggesting that DACA did have a deterrence effect on juvenile delinquency.

This paper makes two main contributions to the literature. It is the first, to my knowledge, to explore the causal impacts of DACA on juvenile crime and student misbehavior. It has been established that DACA has decreased poverty (Amuedo-Dorantes and Antman,

[3]See subsection 3.2 for further discussion of this method.
[4]See subsection 3.3 for further discussion.

2016), increased educational attainment and college going (Ballis, 2023; Amuedo-Dorantes and Antman, 2017; Pope, 2016; Kuka et al., 2020), and increased labor force participation (Pope, 2016). Gunadi (2020) examines whether DACA decreased *overall* crime rates or the likelihood of being institutionalized[5] for 18-35 year old "undocumented" men,[6] finding little evidence DACA had any impact outside of decreasing overall property crime rates. While Gunadi examines DACA and crime, there are several key differences between our work: 1) I focus on a younger age group that could have a quicker reaction to DACA (middle and high schoolers compared to individuals who are above 18) and 2) I look at individual-level crime/disciplinary outcomes. Ballis (2023) uses geographic variation in DACA applications to examine educational investments among high schoolers to determine if DACA had spillover effects on their U.S. citizen peers, finding that educational investment did increase while there was no significant evidence of changes in the likelihood of ever being disciplined.[7] The key difference between Ballis' work and mine is that I examine different disciplinary outcomes amongst likely undocumented students, while Ballis explores how DACA impacted the disciplinary outcomes of all students.

Another main contribution of this paper, and another differentiating factor from both Gunadi (2020) and Ballis (2023), is my unique administrative data. Given this data allows me to identify students who were both born outside of the U.S. and do not have SSNs, I can more accurately separate the students who were likely to be affected by DACA from those who were not. Gunadi (2020), like other works including Amuedo-Dorantes and Antman (2017); Pope (2016); Kuka et al. (2020); Borjas (2017), define undocumented individuals as Hispanic non-

[5]Gunadi uses the American Community Survey (ACS) data, which denotes if a respondent is living in an institution, which includes mental hospitals, elderly homes, jails, etc. Butcher and Piehl (1998) note that 70% of men between the ages of 18 and 35 that are living in institutions are incarcerated.

[6]While I use an individual-level proxy not seen in the literature before, Gunadi uses the both the residual method introduced by Borjas (2017) and the Hispanic non-citizen definition found in works like Amuedo-Dorantes and Antman (2017) and Pope (2016).

[7]Ballis defines the disciplinary outcome as "ever disciplined," representing an analysis of the extensive margin only while I examine both the intensive and extensive margins.

citizens in the ACS data, while Ballis (2023) using geographic variation in DACA applications to determine which schools have higher or lower populations of undocumented students. Alternatively, my method does not restrict based on race/ethnicity[8], age, or self-identified citizenship status. I also benefit from having the entire universe of Texas public school students, while the ACS is restricted to include only those who filled out the survey.

Beyond the contributions to the previous literature, this project also carries important policy implications. This analysis identifies and examines how an immigration policy like DACA affects young peoples' behaviors, and illuminates further benefits of DACA outside of those that were its explicit purpose (i.e., safety/security of undocumented childhood arrivals). This work also has broader implications for disciplinary policy, as it illustrates how student behavior can be shifted using incentives.

2 Policy & Institutional Background

The Deferred Action for Childhood Arrivals (DACA) program is an immigration program that provides temporary[9] protection from deportation and work authorization to eligible undocumented immigrants. Started via an executive order from President Obama, the program began accepting applications in August of 2012. To receive DACA, an individual must:

1. Have arrived in the U.S. before the age of 16

2. Have resided in the U.S. consecutively as an undocumented individual since June 2007

3. Be under the age of 31 in June 2012 and above the age of 15 at application[10]

[8] In 2023, about 85% of DACA recipients were originating from Latin American countries (USCIS, 2023).

[9] Recipients have to re-apply for the program every two years as long as they are eligible, and there is a $465 fee required to apply or renew (which has since increased $555 in 2025).

[10] An individual who is younger than 15 years old can apply for DACA if they are currently in the removal process.

4. Be currently in school, have graduated high school, earned a GED, or been honorably discharged from the military

5. Not have any felony convictions, multiple serious misdemeanor convictions, or be otherwise deemed as a threat to national safety/security.

In the first quarter of its existence, there were just over 367,000 DACA applications received by the U.S. Citizenship and Immigration Services (USCIS), and over 800,000 at its peak (USCIS, 2012; Batalova et al., 2014). In 2023, there are a little over 550,000 active DACA recipients (USCIS, 2023), and the total number of undocumented individuals in the United States is estimated to be around 12 million, roughly 3% of the U.S. population (Institute, 2019b). On a state level, Texas is home to the second largest population of both undocumented immigrants and DACA recipients, second only to California (USCIS, 2023).

3 Data

I use the Texas Education Agency's (TEA) student-level administrative data, accessed through agreement of the Texas Education Research Center (ERC).[11] These data include all K-12 students who attended public school in Texas from 2000-2024, which is rouhgly 5.2 million students every year.

3.1 Analytic Sample

While I have access to the entire panel of TEA data ranging from 2000 - 2024, I restrict the time period of my sample specifically to the years 2007 - 2016. Figure 1 shows the plotted average outcomes year over year for immigrant and no-immigrant disciplinary outcomes in

[11] The Texas ERC is housed within the University of Texas system, and I specifically work with the center at UT Austin. The ERC provides access to all TEA data, as well as several other large administrative Texas data sources, to researchers of approved projects.

Texas - and in most of the outcomes, the immigrant averages surpass the non-immigrant averages in 2007. While the reason this occurred is beyond the scope of this project, this shift does warrant adjusting the study's time frame to begin in 2007 to ensure that the parallel trends assumption is upheld. Similarly, the panel stops in 2016[12] because the Trump Administration's legal challenges to DACA began that year, which introduces uncertainty to DACA's existence, the effects of which also extend beyond the scope of this project.

In addition to the restrictions placed on the time frame being analyzed, I also narrowed my sample of students being analyzed to align better with the eligibility criteria of DACA. Specifically, any immigrant that appeared for the first time in my data at the age of 16 or later was excluded.[13] I also included only students that appeared in my data in 2007 or earlier, given that another requirement of DACA is that the individual must have resided in the U.S. since June 2007. These restrictions result in minimal sample size reductions (less than 1%).

3.2 Proxying for Documentation Status

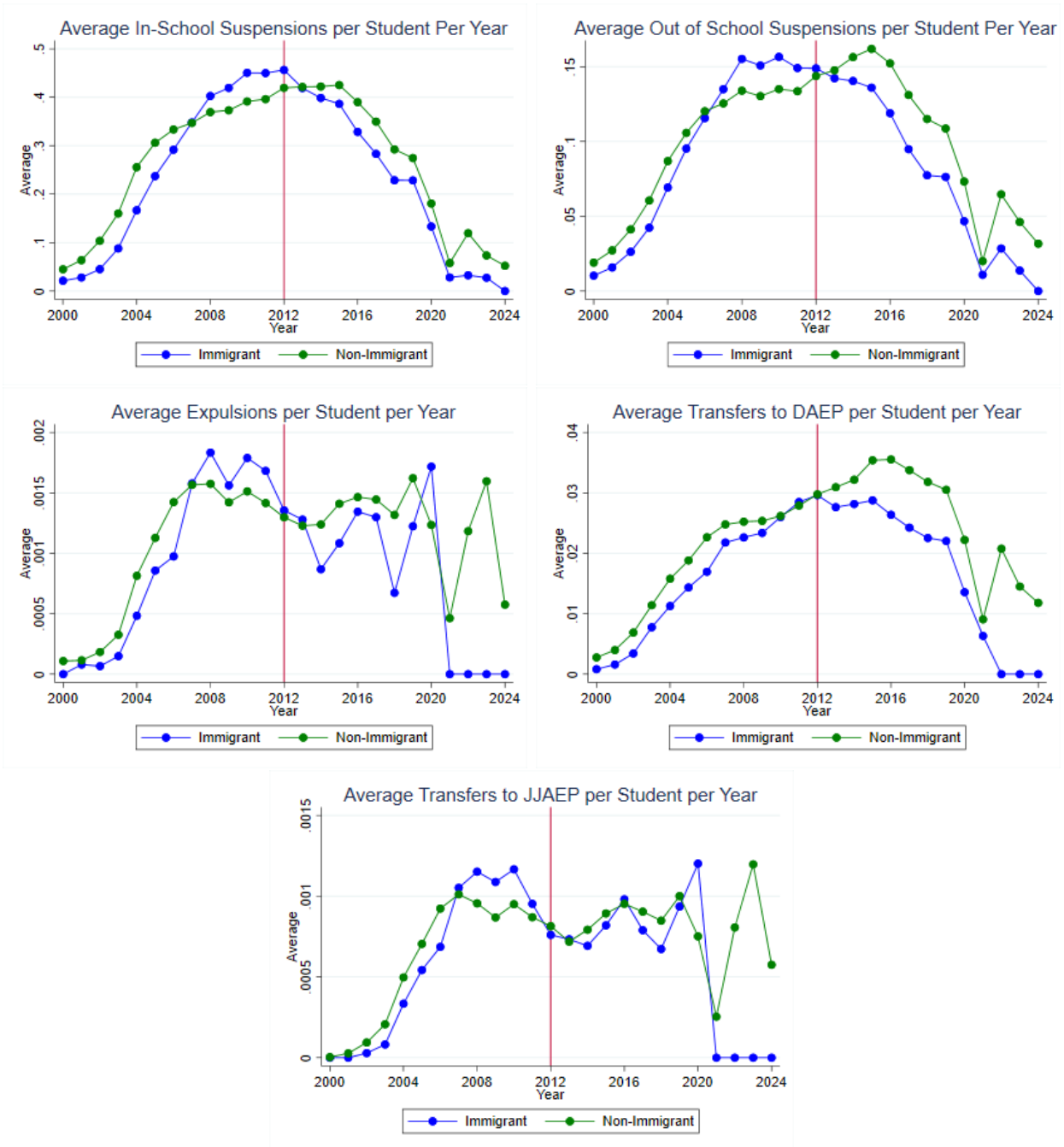
My identification strategy, as discussed further in section 4, relies on comparing students who are likely undocumented to those who are not. To build a proxy for undocumented status, I combine two administrative flags: 1) the "Immigrant" flag and 2) the "Missing Social Security Number" (SSN) flag. The "Immigrant" flag denotes students who were a) born outside of the U.S. and its territories and b) who have not attended a U.S. school for more than three years. The "Missing SSN" flag denotes students who did not provide a social security number.[14] Combining these two, I classify a student as likely undocumented

[12]The TEA data is denoted by *fiscal years*, and each school chooses if they run their fiscal calendar from July to June or August to July. Thus, the analytic time frame looking at fiscal years 2007 to 2016 temporally runs from July 2007 to August 2017.

[13]A key requirement of DACA, as discussed in section 2, is that the undocumented individual arrived in the U.S. before the age of 16.

[14]U.S. Citizens are assigned a SSN shortly after birth, and folks who are on a visa and are authorized to work in the U.S. can apply for a SSN (Administration, 2024). Thus, students who are missing their SSN are

Figure 1: Averages of Each Outcome by Student by Year



Notes: Above shows the plotted averages of the key outcomes of interest for the immigrant (blue) and non-immigrant (green) groups in each year. Note that in 2007 and continuing for the rest of the pre-period (prior to DACA's start in 2012), the immigrant trends surpass the non-immigrant trends for four out of the five variables of interest. Because of this, I restrict the sample time frame to begin in 2007 in order to hold this shift constant across the pre-period and satisfy the parallel trends assumption.

if they both 1) were *ever* labeled as an immigrant and 2) were *ever* labeled as missing their SSN. Thus, the students I am denoting as likely undocumented are students who were born outside of the U.S., did not attend a U.S. school for three or more years, and are not in possession of a SSN.[15]

The summary statistics of the likely undocumented students compared to all others are in Table 1, using the base year of 2007. [16] On average, the likely undocumented students are slightly older than those who are not, and they have a higher proportion of Hispanic and students in Limited English Proficiency (LEP) programs. These students also have a higher proportion who have lower Socio-Economic Status (SES) and have less students with disabilities. Otherwise, the two groups are relatively similar.

3.3 Measuring Juvenile Crime and Student Misbehavior

There are five main outcomes of interest: two that measure student misbehavior, and three that proxy for juvenile crime. The two measures of student misbehavior I include are in-school and out-of-school suspensions. Both types involve removing a student from their regular classroom for alternative programming. The main difference between the two is that in-school suspensions are served on school campus, while out-of-school suspensions bar students from being on campus during their suspension period. While Out-of-school suspensions are assigned for more severe disciplinary infractions, both types of suspensions are most often assigned for infractions that are against the school's code of conduct but are not necessarily illegal.

I proxy for juvenile crime using three measures: expulsions, transfers to Disciplinary Alternative Education Programs (DAEPs), and transfers to Juvenile Justice Alternative

not U.S. citizens and are not authorized to work in the U.S.

[15] See Appendix B for an exploration of just using the "Immigrant" flag only for the documentation status proxy.

[16] Appendix A presents the summary statistics for base year 2012.

Table 1: Descriptive Statistics

	Likely Undocumented	Likely Documented
Age	11.318	10.683
	(2.956)	(3.253)
% At Risk of Dropping Out	0.76	0.57
	(0.43)	(0.50)
% Gifted And Talented	0.07	0.07
	(0.25)	(0.26)
% Male	0.51	0.51
	(0.50)	(0.50)
% Black	0.03	0.24
	(0.18)	(0.43)
% Hispanic	0.88	0.70
	(0.33)	(0.46)
% LEP	0.59	0.19
	(0.49)	(0.40)
% Lower SES	0.83	0.71
	(0.37)	(0.46)
% Disabled	0.06	0.12
	(0.24)	(0.33)
% High Attendance	0.94	0.91
	(0.24)	(0.28)
Students	182,810	2,182,447

Notes: Above are the descriptive statistics for both the likely undocumented and likely documented groups in the base year 2007. Each variable, outside of age, is measured via indicators, so all averages can be interpreted as percentages of the population. For example, the likely undocumented group is made up of 88% Hispanics in 2007, while the likely documented group is made up of 70%. The "At Risk of Dropping Out" variable is an administrative classification that is calculated based on family economic status, grade proficiency, and other factors. The "LEP" variable stands for "Limited English Proficiency," an administrative classification used for students who are not fully fluent in English. The "Black" and "Hispanic" variables are the percentage of the total group that are just that race/ethnicity only, excluding individuals who are two or more races. The "High Attendance" variable denotes the percentage of students who have above average attendance rates (i.e., attend over 80% of classes).All of these variables are added into the preferred specification as further controls.

Education Programs (JJAEPs). Expulsions involve a student being removed from their original school setting for an indefinite amount of time. Sometimes, expulsions occur in tandem to transfers to alternative education programs - school settings that are meant to provide a more controlled space for learning for students who struggle with misbehavior or other learning difficulties. In Texas, there are two types of alternative education programs: DAEPs, which are run by the school district, and JJAEPs, which are run by the State. There are much more DAEPs than JJAEPs, but they otherwise function similarly. A student can be moved to one of these alternative education programs without being expelled, or can be expelled to one of these programs. The disciplinary section of the Texas Code of Student Conduct maintains that multiple factors are considered when assigning a student to these disciplinary actions, despite the offenses causing these actions being similar. Because of this variability and overlap, I use all three as proxies for juvenile criminal activity. See Appendix C for further discussion on the definition of each of these variables.

Table 2 depicts the average outcome levels of the likely undocumented students compared to all others in the base year of 2007.[17] In the base year 2007 (5 years before DACA began), all of the outcome levels remain similar across both groups.

4 Methodology

I employ a Difference-in-Differences (DiD) methodology to determine the impact of DACA on juvenile crime and student misbehavior. The first difference is proxied[18] documentation status, comparing students who are likely undocumented to those who are not. The second difference is temporal, comparing trends before and after DACA began in 2012.

[17] See Appendix A for the outcome levels in base year 2012.

[18] As discussed in subsection 3.2, I classify students as likely undocumented if they were 1) ever labeled as an immigrant AND 2) ever labeled as missing their SSN. I explore an alternative proxy, using just all students who were ever labeled as an Immigrant, in subsection 3.2.

Table 2: Outcome Means

	Likely Undocumented	Likely Documented
In-School Suspensions per student per year	0.399	0.455
	(1.416)	(1.534)
Out of School Suspensions per student per year	0.16	0.19
	(0.75)	(0.86)
Expulsions per student per year	0.00	0.00
	(0.04)	(0.05)
Transfers to DAEP per student per year	0.03	0.03
	(0.20)	(0.24)
Transfers to JJAEP per student per year	0.00	0.00
	(0.04)	(0.04)
Students	182,810	2,182,447

Notes: Above are the sub-population means for students who are likely undocumented or documented in the base year of 2007. DAEP standards for Disciplinary Alternative Education Programs, which are the ones run by the school districts, while JJAEP stands for Juvenile Justice Alternative Education Programs, the ones who are run by the state.

4.1 Differences-in-Differences Specification

Equation 1 shows the DiD specification. Given the use of student fixed effects (δ_i), I am identifying the treatment effect using within student variations in outcomes year over year. To control for the portion of variation in these outcomes that are due to the students growing older, I include birth-cohort[19] by year fixed effects, a much more aggressive control than a linear age variable or age fixed effects.[20] I also include school fixed effects[21] and a vector of time-varying[22] student controls[23] that are unlikely to be respondent to the treatment.[24]

[19]Cohort here is defined as the year in which each age group was born.

[20]See Appendix D for further discussion of these age controls.

[21]School fixed effects are included to account for differing disciplinary cultures in different schools.

[22]Because I am using student fixed effects, characteristics of each students that do not vary over time, like their race or gender, are colinear with the fixed effects and thus drop out of the specification.

[23]These controls specifically include: gifted and talented program participation, being an unschooled asylum seeker, being an unaccompanied minor, having Limited English Proficiency (LEP), being disabled, and transferring schools within the school year.

[24]This stipulation is made to avoid the use of "bad controls" in my specification (Wooldridge, 2024). These are variables like Socio-Economic Status (SES) and being labeled as "At-Risk" of dropping out, as DACA could affect both of these things. Including these as controls would create additional variation in my specification instead of controlling for it, which is why these, and any other variables that could be impacted by the treatment, were excluded.

$$y_{icst} = \beta_0 + \delta_i + \gamma_{ct} + \lambda_s + \beta_1(Post_t \times Undoc_i) + X_{it} + \epsilon_{icst} \quad (1)$$

Where:

y_{icst}: delinquency outcome y for student i in birth cohort c in school s at time t

δ_i: student fixed effects

γ_{ct}: birth cohort by year fixed effects

λ_s: school fixed effects

$(Post_t \times Undoc_i)$: Indicates likely undocumented student $(Undoc_i)$ in the periods after DACA $(Post_t)$

X_{it}: Vector of student-specific, time-varying controls

In order for β_1 to be an un-biased DiD coefficient, two assumptions must be upheld: 1) the Stable Unit Treatment Value Assumption (SUTVA) and the Parallel Trends Assumption (PTA). SUTVA is broken if the treatment (DACA) affected the control group (students who are not undocumented). Ballis Ballis (2023) shows that there is evidence of DACA affecting U.S. citizens through spillovers from their undocumented peers, which means that SUTVA is likely violated in my case. This means that, because the control group does have some benefits gained from DACA, any effect I find is likely attenuated. This means that my estimates are likely *lower bounds*, and the true causal impact of DACA is greater than what I am capturing.

The PTA dictates that, absent DACA, both the likely undocumented students and the likely documented students have similar trends in their outcomes. Figure 2 maps the trend in outcomes for both the treatment group (blue) and the control group (green) in order to determine if there is visual evidence that the trends of the two groups diverge. Visually, the pre-trends prior to 2012 seem to remain parallel in all outcomes, but an Event Study

analysis is also needed to confirm that this is the case.

4.2 Event Studies

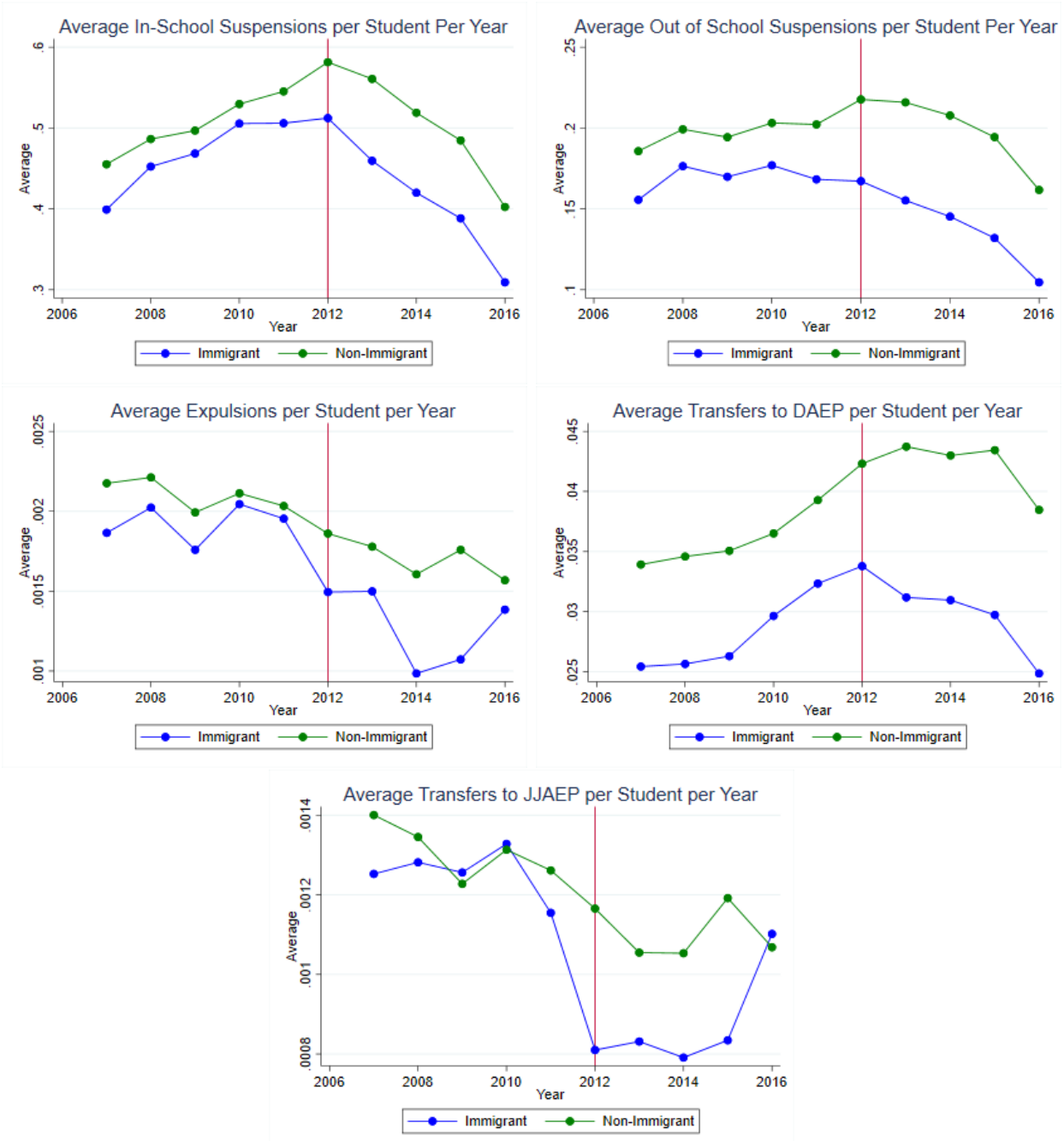
To further explore the extent in which the PTA is upheld, I explore event studies that examine how the DiD coefficient may change in every year relative to the treatment. Equation 2 depicts the event studies specification, which is similar to Equation 1 except that there is an individual coefficient for each year prior to DACA (which began in 2012) and after, except for in t_{-1}, which is the reference year (2011). The PTA would be broken if the event study shows pre-trends, i.e., if there are statistically significant impacts of DACA on periods prior to 2012.

$$y_{icst} = \beta_0 + \delta_i + \gamma_{ct} + \lambda_s + \left(Undoc_i \times \sum_{\substack{m=-5 \\ m \neq -1}}^{4} \beta_m \mathbb{1}[t - 2012 = m] \right) + X_{it} + \epsilon_{icst} \qquad (2)$$

4.3 Further Threats to Identification

Despite there being a saturation of controls, there is still the risk that the DiD coefficient, $beta_1$ in Equation 1, could be mis-identified. The main threat to identification would be if there is a temporal shock that only affects the treatment group (likely undocumented students), which could be the case if, for example, there is a change in cultural/social attitudes towards immigrants that affects their disciplinary outcomes. Typically, I would include an undocumented status by time fixed effect, which would control for average differences over time between the students of different documentation statuses. The issue with including this control, however, is that it is nearly perfectly collinear with my DiD coefficient. β_1 is identified by comparing 1) the difference across documentation status and 2) the difference across time, both of which would be subsumed in an undocumented times year fixed effect.

Figure 2: Averages of Each Outcome by Year



Notes: Above show the raw data trends for each outcome of interest, broken into the immigrant (treatment) group and the non-immigrant (control) group, year over year. The goal of these figures is to assess if the Parallel Trends Assumption (PTA) is likely to be upheld, i.e., to ensure prior to treatment in 2012, the trends of each group move in parallel. Visually, this looks to be the case in all of the outcomes except for in the Tranfers to JJAEPs outcome, which does show some convergence instead of parallelism.

5 Results

Table 3 depicts the main DiD results. Column 0 illustrates the DiD coefficients without any controls, and subsequent columns include more and more controls. The preferred specification is column 4, which includes all of the controls denoted in Equation 1. Standard errors are clustered at the student level.

There is a significant and sizable effect of DACA on three outcomes: in-school suspensions, out-of-school suspensions, and transfers to DAEPs. In-school suspensions decreased 10.9% relative to the treatment group's mean (the last column in Table 3), and out-of-school suspensions decreased by 16.4%. Transfers to DAEPs decreased by 13.7%. Expulsions and transfers to JJAEPs also decreased, but those estimates have less precision as more controls are added.

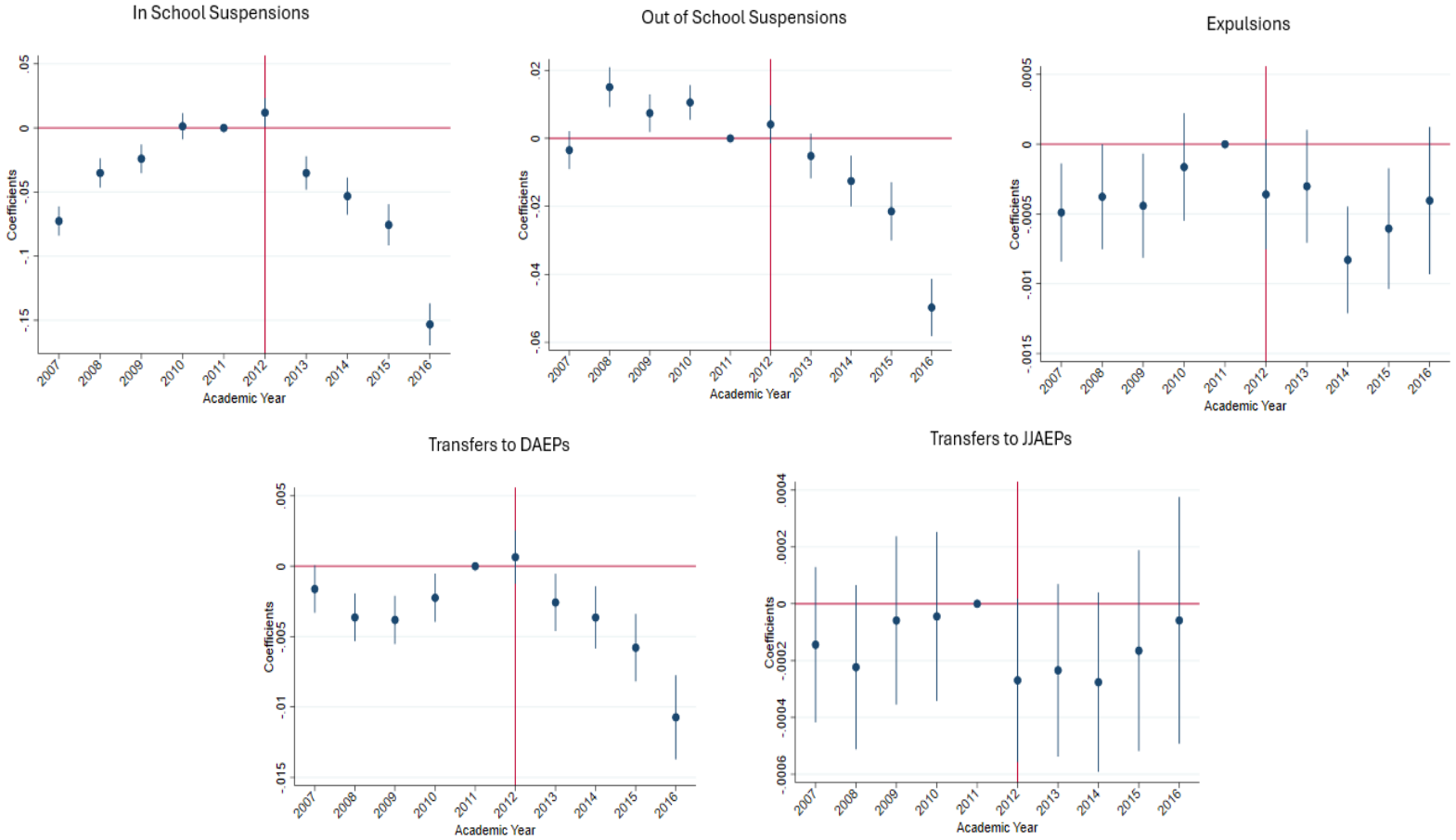
Figure 3 depicts the event studies following Equation 2 for each outcome. There is no noticeable pre-trends present in these graphs, which gives some suggestive evidence that the PTA is upheld.

Table 3: DiD Results

	(0)	(1)	(2)	(3)	(4)	Mean
Total In-School Suspensions Per Student Per Year	-0.07258***	-0.04344***	-0.03853***	-0.03853***	-0.05039***	0.46177
	(0.00496)	(0.00438)	(0.00437)	(0.00438)	(0.00440)	(1.52012)
Total Out of School Suspensions Per Student Per Year	-0.04374***	-0.02896***	-0.02710***	-0.02710***	-0.02750***	0.16715
	(0.00260)	(0.00230)	(0.00229)	(0.00230)	(0.00231)	(0.78111)
Total Expulsions Per Student Per Year	-0.00041***	-0.00022**	-0.00019**	-0.00019**	-0.00018*	0.00171
	(0.00010)	(0.00009)	(0.00009)	(0.00009)	(0.00010)	(0.04251)
Total Transfers to DAEP Per Student Per Year	-0.00391***	-0.00499***	-0.00444***	-0.00444***	-0.00390***	0.02853
	(0.00065)	(0.00061)	(0.00061)	(0.00061)	(0.00061)	(0.21083)
Total Transfers to JJAEP Per Student Per Year	-0.00026***	-0.00007	-0.00005	-0.00005	-0.00007	0.00112
	(0.00008)	(0.00008)	(0.00008)	(0.00008)	(0.00008)	(0.03391)
Controls:						
Student Fixed Effects		✓	✓	✓	✓	
Cohort by Year Fixed Effects			✓	✓	✓	
School Fixed Effects				✓	✓	
Additional Controls					✓	
Sample Size			**28,892,159**			

Notes: See above for the results of DiD specification comparing immigrant students who do not have social security numbers with all other students. The treatment group average is included in the far right column for reference. Additional controls are: gifted and talented program participation, being an unschooled asylum seeker, being an unaccompanied minor, having Limited English Proficiency (LEP), being disabled, and transferring schools within the school year. Estimates are rounded to 5 decimal places, and standard errors are clustered by student. *** is significant at the $\alpha = 0.01$ level, ** is significant at the $\alpha = 0.05$ level, and * is significant at the $\alpha = 0.10$ level.

Figure 3: DiD Event Studies



Notes: Above shows the event studies of each of the main outcomes of interest. The treatment group are immigrant students who do not have social security numbers, and the control group are all other students. These figures show how the DiD estimates change over time pre- and post-DACA. The point-estimates are represented by the dots, and the lines show the 95% confidence interval.

6 Heterogeneity Analysis

6.1 Comparing Results only within Race

In the main Difference-in-Difference results, I do not impose any restrictions on the sample outside of those made to ensure the other DACA eligibility requirements are met, as explained in subsection 3.1. However, this may mask treatment effect heterogeneity by race, especially given that most DACA recipients are Hispanic.

Table 4 examines how the DiD results change when adding in restrictions based on race. The first column reflects the same results shown in Table 3, i.e., the impact of DACA on the whole population irrespective of race. The second column shows the impact when restricting the sample to only have students who are non-white in both the treatment and control groups, and the last column restricts the sample to only specifically have only Hispanic students in both groups. All columns include all of the controls mentioned in Equation 1. For most outcomes, the effects are largest for the sample that includes only non-white students, and smallest for the sample that includes all students irrespective of race. This suggests that the impact of DACA on non-white students was the greatest, followed by Hispanic students.

Table 4: DiD Results by Race

	All Races	% Mean	Non-White Only	% Mean	Hispanic Only	% Mean
Total In-School Suspensions Per Student Per Year	−0.050 39***	-10.9%	−0.075 51***	-16.1%	−0.065 43***	-13.3%
	(0.00440)		(0.00450)		(0.00473)	
Total Out of School Suspensions Per Student Per Year	−0.027 50***	-16.5%	−0.041 97***	-24.5%	−0.031 65***	-17.7%
	(0.00231)		(0.00237)		(0.00249)	
Total Expulsions Per Student Per Year	−0.000 18*	-10.5%	−0.000 22**	-12.5%	−0.000 17	-9.1%
	(0.00010)		(0.00010)		(0.00010)	
Total Transfers to DAEP Per Student Per Year	−0.003 90***	13.7-%	−0.006 07***	-20.7%	−0.004 34***	-14.2%
	(0.00061)		(0.00063)		(0.00066)	
Total Transfers to JJAEP Per Student Per Year	−0.000 07	-6.25%	−0.000 10	-8.6%	−0.000 08	-6.6%
	(0.00008)		(0.00008)		(0.00008)	
Sample Size	28,892,159		19,345,062		14,220,741	

Notes: Above are the heterogeneity results by race for each outcome. These coefficients are also expressed in terms of percentages of the restricted treatment groups' mean, i.e., the mean of the treatment group given the race restriction. Racial restrictions were placed on both the treatment and controls groups such that these results. So, the "Non-White" column, for example, compares students who were likely undocumented and non-white to all other non-white students. All columns include student fixed effects, birth cohort by year fixed effects, school fixed effects, and the vector of time varying student-level controls. Standard errors are clustered on the student level. *** is significant at the $\alpha = 0.01$ level, ** is significant at the $\alpha = 0.05$ level, and * is significant at the $\alpha = 0.10$ level.

6.2 Comparing Results Across Age at DACA Implementation

Another potential area of heterogeneity that may be masked by looking at the full example is heterogeneity based on age. Specifically, how do students who are too young to apply to DACA right away (i.e., are 14 or younger in 2012) compare to those who could apply immediately? Table 5 examines how the DiD results differ based on age at DACA implementation.

The DiD effects were much larger for students who were too young to apply right away than those who weren't. For example, out-of-school suspensions decreased by 26.46% for the students who couldn't apply to DACA immediately, compared to 10.83% for students who could. A similar story can be seen with the transfers to DAEPs as well: the students who were too young in 2012 saw decreases of 20.25% whereas the students who could apply right away saw decreases of 10%. This suggests that DACA did have some deterrence effect on juvenile crime, since younger kids show effects sizes that are almost double those of the kids who were older.

Table 5: DiD Results by Age at DACA Implementation

Outcome	**Too Young**	**Not Too Young**
Total In-School Suspensions Per Student Per Year	-7.8%***	-1.59%
Total Out of School Suspensions Per Student Per Year	-26.46%***	-10.83%***
Total Expulsions Per Student Per Year	-19.17%	-7.95%
Total Transfers to DAEP Per Student Per Year	-20.25%***	-10.07%***
Total Transfers to JJAEP Per Student Per Year	-6.34%	-2.31%
Number of Observations	12,225,526	7,217,931

Notes: Above show how the DiD results differ for students who were too young to apply to DACA right away (i.e., were 14 or younger in 2012) compared with students who could apply immediately. Both columns include the same controls as seen in Equation 1: student fixed effects, cohort by year fixed effects, school fixed effects, and other student-level time varying controls. Further, both columns are results from restricting the sample to include only those within the age group. For example, the "Too Young" column shows the DiD estimates when comparing likely undocumented students who were too young to apply to DACA in 2012 (14 years old or younger) to students who were not likely undocumented but in the same age range. Both columns' are expressed in terms of percentages of the treatment group means, and standard errors are clustered by student. *** is significant at the $\alpha = 0.01$ level, ** is significant at the $\alpha = 0.05$ level, and * is significant at the $\alpha = 0.10$ level.

7 Conclusion

The Deferred Action for Childhood Arrivals (DACA) program provides temporary protection from deportation and work authorization for undocumented childhood arrivals who meet specific age, residency, education, and criminal history criteria. This project specifically seeks to address how young students react to these criteria, as the requirements for staying in school and not committing crimes shift their incentives.

Using student-level, restricted access data from Texas and a Difference-in-Differences (DiD) methodology, there are three key results that should be highlighted. First, across all specifications and robustness checks, there remains evidence of a significant and sizable decrease in in-school and out-of-school suspensions, as well as transfers to disciplinary alternative education programs (a proxy for juvenile crime). This suggests that DACA did cause a decrease in both juvenile crime and student misbehavior. This suggestion is further strengthened by the results from the expulsions and transfers to juvenile justice alternative education programs, as both consistently showed negative coefficients, albeit with less statistical precision than the other results (likely due to the rarity of these two disciplinary outcomes).

The second key finding of this study is that these results are robust to two different types of proxy methods, as well as different sample definitions. Further, the magnitude of the results, in terms of percentage of the treatment group's mean, is largest when restricting the sample to only looking at non-white students, but becomes slightly smaller when looking at only Hispanic students. For example, in Table 4, for students of all races, DACA decrease in-school suspensions by 10.9%, non-white students by 16.1%, and Hispanic students by 13.3% of each treatment group's mean. This suggests that DACA had the strongest effect on non-white students, but slightly less of an affect when narrowing to only include Hispanic students.

Thirdly, the effects of DACA differ across age groups, specifically in relation to when DACA was started. As seen in Table 5, when looking at just the students who were too young to apply to DACA right away (individuals who were 14 or younger in 2012), their outcomes decreased by much larger amounts than students who apply to DACA immediately. This suggests that DACA did have a deterrence impact on crime, as despite being younger, the students who were too young to apply right away had effect sizes that were nearly double those of the students who could apply immediately.[25]

These findings result in several important policy implications. First, they provide evidence that students do respond to incentives, especially in terms of their behavior (and misbehavior). This knowledge is particularly useful in designing future disciplinary policies, as shifting incentives can be leveraged to ensure stronger results. Second, these findings illuminate how, despite being predominantly an immigration policy, DACA had significant unexplored impacts on the broader society, which speaks to the potential for other federal-level policies to be leveraged to cause positive social change. Finally, this project adds to the growing evidence that policies aimed at assisting young undocumented individuals are helpful to other members of society as well, bolstering the push for an expansion of such policies in the future.

[25]See Figure 4 for the strong relationship between age and disciplinary outcomes. Given students who are 14 are on the upward slope, we should see smaller decreases to their outcomes than the kids who could apply right away. However, we see the opposite, hence why it is likely DACA did deter juvenile crime/misbehavior.

8 Appendix

Appendix A Summary Statistics using Base Year 2012

See below for the summary statistics of both the characteristics, and the outcome levels, of the likely undocumented and documented groups in the base year of 2012, as opposed to 2007 like shown in the main body of the paper.

Appendix B Alternate Proxy Method

The main results use a combination of the immigration and missing SSN administrative flags for documentation status, as explained in subsection 3.2. This method, however, could potentially be too narrow, as there could be students who have social security numbers but are still undocumented.[26] So, an alternative and broader proxy method is explored in this section: proxying documentation status by looking at all students who were ever marked as an immigrant.

Table 8 depicts the results from the DiD specification laid out in Equation 1 using the Immigrant flag only as the proxy for documentation status instead. Similar to the main results, DACA is shown to have a negative and significant impact on in-school and out-of-school suspensions, as well as transfers to DAEPs.

Table 9 compares the results using the different proxy methods. Two main differences stand out: 1) the immigrant only proxy method returns a significant expulsion result, and 2) the results for the immigrant only flag proxy are larger for all outcomes. This suggests that the main results are attenuated, and the real causal impact of DACA is larger given that broadening the proxy group results in a larger estimated impact.

[26]This could be due to several reasons including: lying about having a SSN, having a fake SSN, using a parent's or siblings SSN, etc.

Table 6: Descriptive Statistics

	Likely Undocumented	Likely Documented
Age	14.618	13.934
	(2.079)	(2.147)
At Risk of Dropping Out	0.57	0.51
	(0.49)	(0.50)
Gifted And Talented	0.09	0.08
	(0.29)	(0.28)
Male	0.51	0.51
	(0.50)	(0.50)
Black	0.03	0.22
	(0.17)	(0.41)
Hispanic	0.88	0.72
	(0.32)	(0.45)
LEP	0.38	0.14
	(0.49)	(0.35)
Lower SES	0.83	0.74
	(0.37)	(0.44)
Disabled	0.07	0.12
	(0.25)	(0.32)
High Attendance	0.89	0.88
	(0.32)	(0.33)
Students	103,683	1,392,337

Notes: Above are the descriptive statistics for both the likely undocumented and likely documented groups in the base year 2012. Each variable, outside of age, is measured via indicators, so all averages can be interpreted as percentages of the population. For example, the likely undocumented group is made up of 88% Hispanics in 2012, while the likely documented group is made up of 72%. The "At Risk of Dropping Out" variable is an administrative classification that is calculated based on family economic status, grade proficiency, and other factors. The "LEP" variable stands for "Limited English Proficiency," an administrative classification used for students who are not fully fluent in English. The "Black" and "Hispanic" variables are the percentage of the total group that are just that race/ethnicity only, excluding individuals who are two or more races. The "High Attendance" variable denotes the percentage of students who have above average attendance rates (i.e., attend over 80% of classes).All of these variables are added into the preferred specification as further controls.

Table 7: Outcome Means

	Likely Undocumented	Likely Documented
In-School Suspensions	0.512	0.581
	(1.606)	(1.710)
Out of School Suspensions	0.17	0.22
	(0.75)	(0.92)
Expulsions	0.00	0.00
	(0.04)	(0.04)
Transfers to DAEP	0.03	0.04
	(0.24)	(0.26)
Transfers to JJAEP	0.00	0.00
	(0.03)	(0.04)
Students	103,683	1,392,337

Notes: Above are the sub-population means for students who are likely undocumented or documented in the base year of 2012. DAEP standards for Disciplinary Alternative Education Programs, which are the ones run by the school districts, while JJAEP stands for Juvenile Justice Alternative Education Programs, the ones who are run by the state.

Table 8: DiD Results using Only Immigrant Flag

	(0)	(1)	(2)	(3)	(4)	Imm. Avg
Total In-School Suspensions Per Student Per Year	−0.083 29***	−0.062 66***	−0.057 07***	−0.068 32***	−0.076 05***	0.4376
	(0.00414)	(0.00365)	(0.00364)	(0.00365)	(0.00368)	(1.4718)
Total Out of School Suspensions Per Student Per Year	−0.044 15***	−0.035 61***	−0.033 49***	−0.033 49***	−0.033 84***	0.1558
	(0.00215)	(0.00189)	(0.00189)	(0.00189)	(0.00191)	(0.7515)
Total Expulsions Per Student Per Year	−0.000 42***	−0.000 26***	−0.000 23***	−0.000 23***	−0.000 22***	0.0017
	(0.00009)	(0.00008)	(0.00008)	(0.00008)	(0.00008)	(0.0420)
Total Transfers to DAEP Per Student Per Year	−0.004 77***	−0.006 42***	−0.005 79***	−0.005 79***	−0.005 28***	0.0275
	(0.00055)	(0.00052)	(0.00051)	(0.00052)	(0.00052)	(0.2064)
Total Transfers to JJAEP Per Student Per Year	−0.000 22***	−0.000 07	−0.000 05	−0.000 05	−0.000 06	0.0011
	(0.00007)	(0.00007)	(0.00007)	(0.00007)	(0.00007)	(0.0332)
Controls:						
Student Fixed Effects		✓	✓	✓	✓	
Cohort by Year Fixed Effects			✓	✓	✓	
School Fixed Effects				✓	✓	
Additional Controls					✓	
Sample Size			**28,892,159**			

Notes: See above for the results of DiD specification comparing immigrant students with non-immigrant students before and after DACA. The overall average of each outcome for the immigrant group can be seen in the far right column for comparison purposes. Additional controls are: gifted and talented program participation, being an unschooled asylum seeker, being an unaccompanied minor, having Limited English Proficiency (LEP), being disabled, and transferring schools within the school year. Standard errors are clustered on the student level, and star significant levels include: *** is significant at the $\alpha = 0.01$ level, ** is significant at the $\alpha = 0.05$ level, and * is significant at the $\alpha = 0.10$ level.

Table 9: DiD Results Compared Between Proxy Methods

Outcome	Immigrant Flag Only	Combo of Immigrant and SSN Flags
Total In-School Suspensions Per Student Per Year	-17.4%***	-10.9%***
Total Out of School Suspensions Per Student Per Year	-21.7%***	-16.5%***
Total Expulsions Per Student Per Year	-12.9%***	-10.5%*
Total Transfers to DAEP Per Student Per Year	-19.2%***	-13.7%***
Total Transfers to JJAEP Per Student Per Year	-5.5%	-6.25%
Number of Observations	1,204,250	888,014

Notes: Above shows the DiD estimates from the specification using just the immigrant flag to proxy for undocumented status, compared with the specification using the immigrant and missing SSN flags as a proxy. All results are expressed as percentages of the treatment group's (the undocumented group of students via each proxy) mean. Both columns are the results from the most saturated models, which include student fixed effects, school fixed effects, cohort by year fixed effects, and controls for gifted and talented program participation, being an unschooled asylum seeker, being an unaccompanied minor, having Limited English Proficiency (LEP), being disabled, and transferring schools within the school year. Standard errors are clustered by student, and star significance levels are: *** is significant at the $\alpha = 0.01$ level, ** is significant at the $\alpha = 0.05$ level, and * is significant at the $\alpha = 0.10$ level.

Appendix C Disciplinary Codes Included in Each Outcome Measure

The Texas Education Agency (TEA), who owns the student-level data I use for this project, has multiple different disciplinary codes that I combined into my five outcome measures. Table 10 explicitly lists which disciplinary codes are included in each outcome measure.

Table 10: Disciplinary Codes Included in Each Outcome Measure

Outcome	**Disciplinary Codes Included**
In-School Suspensions	• Partial day in-school suspension • Full day in-school suspensions
Out-of-School Suspensions	• Partial day out-of-school suspension • Full day out-of-school suspension
Expulsions	• Expulsions without any alternative educational services offered • Expulsions to on or off campus DAEPs • Expulsions to JJAEPs • Expulsions to JJAEPs by special education officers
Transfers to DAEPs	• Expulsions to on or off campus DAEPs • Placements in DAEPs without expulsions • Court-mandated transfers to DAEPs with or without expulsions • Special education transfers to DAEPs without expulsions
Transfers to JJAEPs	• Special Education expulsions to JJAEPs • Special Education placements in JJAEPs without expulsions • Non-Special Education expulsions to JJAEPs • Court-mandated placements in JJAEPs without expulsions

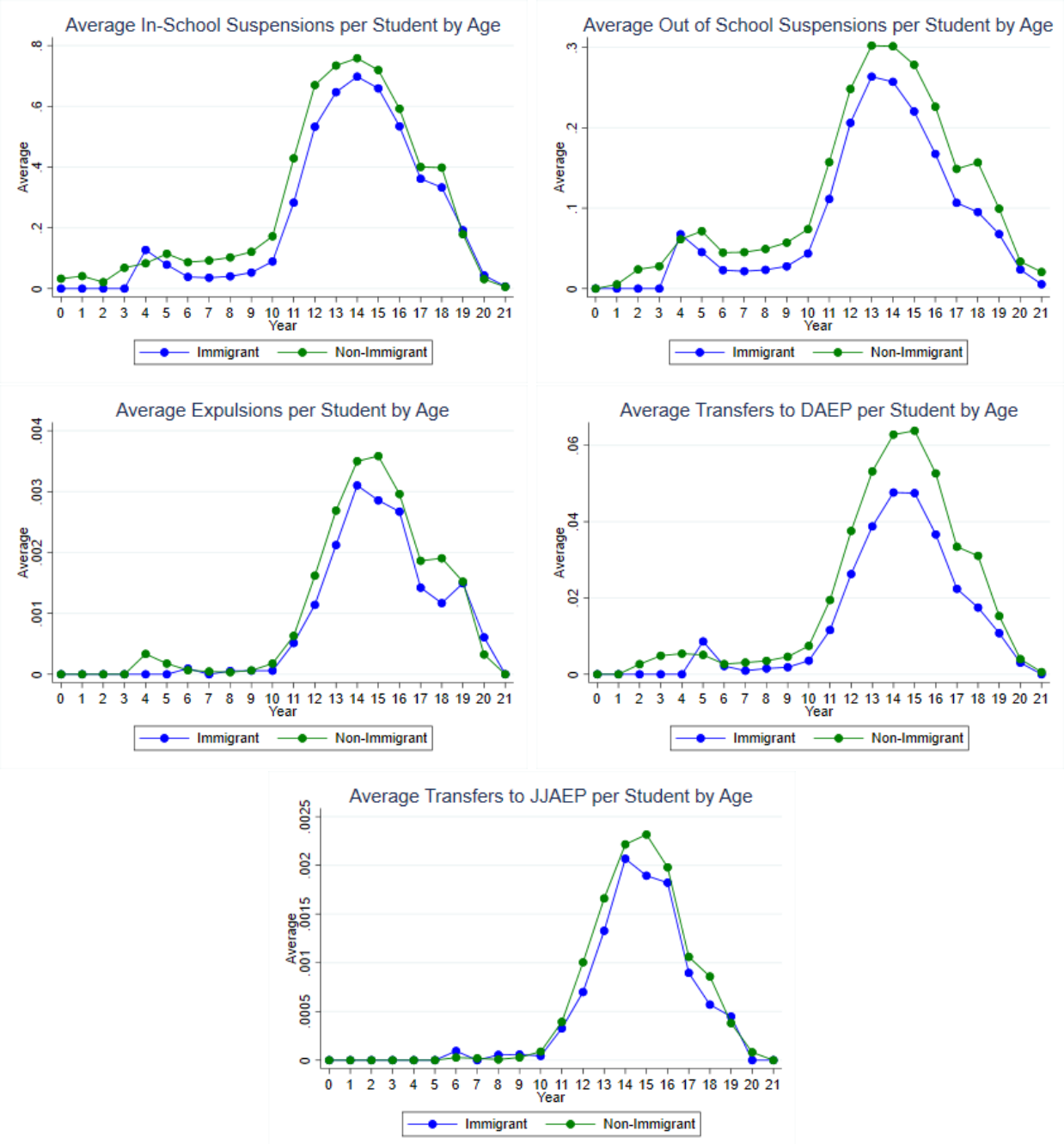
Notes: Above shows the TEA disciplinary codes I included in each of my outcome variables of interest.

Appendix D Controlling for Age

Given that I use student fixed effects within my main specification (Equation 1), the variation I am identifying my treatment effect on is within student changes year over year. However, students also get older year over year, and when students get older, their misbehavior increases. For example, a 10 year old will not be arrested, but a 16 year old might. With student fixed effects, I am comparing a student when they are 16 to themselves when they are 10 - which means that in order to recover the treatment effect, I need to include some form of age control.

A linear control for age isn't sufficient because, as seen in Figure 4, the relationship between delinquency outcomes and age is not linear. The averages of each variable by age show relatively similar trends across all outcomes and between both the immigrant and non-immigrant groups. There is an exponential increase beginning in ages 8-10, there is a peak between ages 14-16, and then there is an exponential decrease in ages beyond 16. A linear control variable for age assumes a single direction, and would thus miss-control for the other half of the picture (i.e., the peak and then decrease).

Figure 4: Averages of Each Outcome by Student by Age

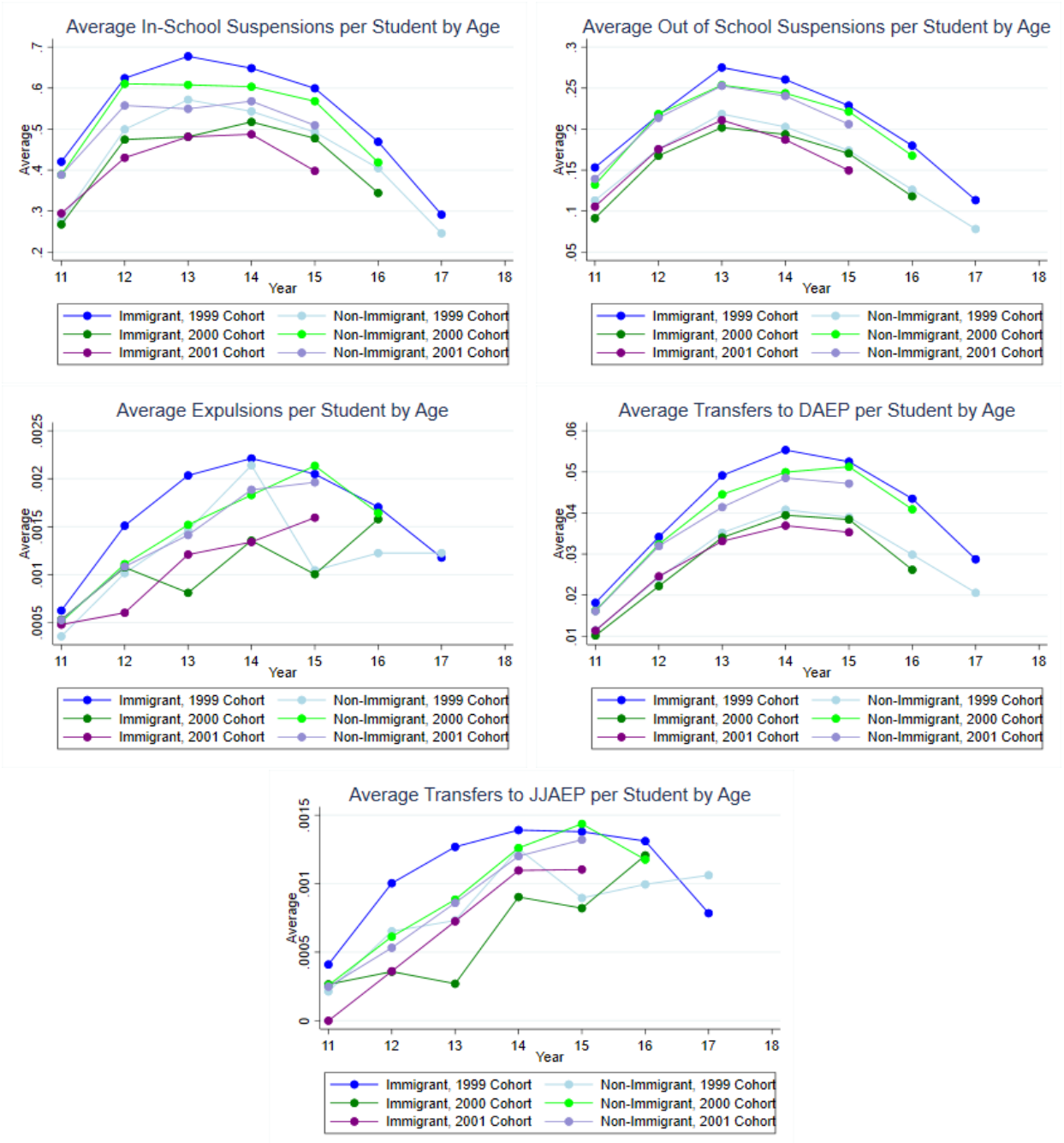


Notes: Above shows the plotted averages of the immigrant (blue) and non-immigrant (green) groups at each age in the raw data. The trend lines appear to be similar regardless of the outcome variable, which speaks to the strong correlation between delinquency and student age. The trends begin to exponentially increase in ages 8-10, they peak in ages 14 - 16, and then exponentially decrease beyond age 16.

An alternative to the linear age control would be either a cubic control for age or age fixed effects. Both of these options, however, assume a stagnant relationship between age that does not change based on when the student was that age. For example, a 12 year old in 2010 will be assumed to react the same way as a 12 year old in 2015 if I include either age fixed effects or a single cubic age control. Figure 5 shows that this is not the case, as the age trends for different birth cohorts[27] vary considerably both between immigrants and non-immigrants and between cohorts. Thus, a control that assumes a single trend for each age group regardless of birth year isn't sufficient.

[27]Birth cohort is defined as the year of observation minus the student's age.

Figure 5: Averages of Each Outcome by Age and Birth Cohort



Notes: Above shows the plotted averages of the immigrant and non-immigrant outcome trends by age for three different birth cohorts. Birth cohorts are calculated by taking the year of the observation and subtracting the age of the student. I show three of the largest birth cohorts - those born in 1999, 2000, and 2001. I include these trends delineated between immigrants and non-immigrants to show that even when looking at immigrant only or non-immigrant only cohorts, there is still considerable variation in the average trends between cohorts.

Hence, I take the most aggressive route to control for the variation in outcomes due to age: including birth cohort by year fixed effects. This creates a control for the interaction between every age group and every birth year, which soaks up much more variation than any of the previous controls. Further, this is a dynamic control that allows for the relationship between age and delinquency outcomes to vary depending on when the student was that age. This addresses the issue identified prior, as a 12 year old in 2010 would be compared to only other 12 year olds in 2010, not to 12 year olds in 2015. Thus, the way that I chose to control for age is via birth cohort by year fixed effects, as it is the most aggressive and dynamic option.

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