The Effects of Immigration Enforcement on Student Outcomes in a New Era of Immigration Policy in the United States[†]

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Abstract: This study presents the first evidence, to our knowledge, of the effects of the surge in interior immigration apprehensions in 2025 in the United States on student academic performance using detailed student-level administrative records from Florida and an identification strategy that reflects localized community differences in increased enforcement. We find evidence that immigration enforcement reduced test scores for both U.S.-born and foreign-born Spanish-speaking students while also reducing the likelihood that these students are involved in disciplinary incidents in schools. Both of these effects are more pronounced for students in middle and high schools.

Keywords: immigration enforcement; recent immigrants; immigrant students; human capital

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1. Introduction

In the first part of 2025 there has been a dramatic increase in interior immigration enforcement in the United States, a pattern that has continued unabated since that point. Monthly interior immigration apprehensions by Immigration and Customs Enforcement (ICE) between February and August 2025 were over twice the monthly apprehension rate during the first three months of fiscal year (FY) 2025, twice the monthly apprehension rate during FY2024, and at least fifty percent higher than the monthly apprehension rate in any year since FY2014.[4]

We present the first evidence, to our knowledge, of the effects of this marked increase in interior immigration enforcement on student academic performance. We employ detailed student-level data from a large urban school district in Florida (LUSD) – one of the ten largest in the United States – and introduce a novel identification strategy in which we measure a school's exposure to increased immigration enforcement based upon the mix of the countries of origin of the school's student body, which we interpret as a proxy for differentially increased community-level enforcement. Using this measure, we estimate that, even among schools in the school district's top tercile of percent foreign-born students, some schools are as much as ten times as exposed to immigration enforcement (using our community-based measure of exposure) than are other schools with very large foreign-born populations. We estimate the effects of recent immigration enforcement exposure on attendance, disciplinary incidents, and test scores of a range of students, including both Spanish-speaking and non-Spanish-speaking students and

[4] Data downloaded from Transactional Records Access Clearinghouse and reports from the Office of Homeland Security Statistics, September 17, 2025.

foreign-born and U.S.-born students.[5] We find evidence that the recent surge in interior immigration enforcement reduced test scores for both U.S.-born and foreign-born Spanish-speaking students, especially in higher-poverty middle and high schools. Meanwhile, these same students have reduced rates of disciplinary incidents. Importantly, because we are comparing groups of differentially-affected communities, our identification strategy captures the modest, though statistically significant, *differential* consequences of the surge in interior immigration enforcement; the *overall* effects of this increase are likely larger.

Florida is an especially good place to study this question. For one, Florida has a very large and diverse population of foreign-born students, and LUSD is no exception. Sizeable numbers of foreign-born students hail from 27 different birth countries – providing considerable variation for our causal identification strategy – and LUSD educates a large number of U.S.-born Spanish-speaking students as well. Over 30 percent of LUSD's student body is Spanish-speaking and over 15 percent are foreign-born. In addition, Florida tests students three times per year, so we can compare test score changes between December 2024, just before the surge in interior immigration enforcement, and May 2025, when enforcement had dramatically ramped up, to the same time period in the immediately prior years.

While this is the first known study of the academic performance effects of the very recent surge in interior immigration enforcement, this study substantively builds upon the prior literature on the consequences of immigration enforcement on U.S. children. In the only other known studies regarding recent immigration enforcement, Dee (2025) finds a marked reduction in aggregate daily attendance rates in five school districts in California's Central Valley following a set of nearby immigration raids that began on January 7, 2025, but does not study

[5] We have no way of knowing whether a foreign-born student is a documented or undocumented immigrant. By definition, all U.S.-born students are citizens, but some of their parents are undocumented immigrants.

test scores or student behavior, make use of individual-level data, or identify treatment effects based on local differences in enforcement exposure.[6] Mumma (2025) similarly finds evidence from aggregate data in Connecticut and Rhode Island that increases in interior immigration enforcement increased absenteeism among English learner students. Studies of prior rounds of enhanced interior immigration enforcement have found evidence that these episodes harmed student educational outcomes and mental health, and had other effects on affected families.[7]

We contribute to the literature in several principal ways. For one, we are the first, to our knowledge, to estimate the effects of the sharp increase in interior immigration enforcement in 2025 on any educational outcome other than aggregate school or district-level attendance rates. The 2025 change in immigration policy is different in nature, conduct, scale, and the degree of interior enforcement than recent prior increases in immigration enforcement, making the study of the 2025 immigration policy changes important *per se*. We use student-level data to investigate effects on test scores, disciplinary outcomes, and attendance.

[6]Dee does find that the increases in absences after January 7, 2025 are more pronounced in districts closer to the location of the raids.

[7] Studies of prior rounds of enhanced interior immigration enforcement have found evidence that this activity led Hispanic students to leave a school (Dee and Murphy, 2020) and children of likely undocumented immigrants to repeat a grade or drop out of school (Amuedo-Dorantes and Lopez, 2015; Amuedo-Dorantes and Lopez, 2017). There is evidence from aggregate school-district level data as well: White-Hispanic gaps in aggregate test scores increased in school districts with larger incidences of deportations within 25 miles (Kirksey et al., 2020), Hispanic students' test scores declined with increased rates of deportation proceedings in their county (Kirksey, 2023), and Hispanic and non-Hispanic Black students' aggregate scores declined in communities following the rollout of the Secure Communities program (Bellows, 2019). County-level apprehension rates reduced Hispanic students' test scores and sense of safety in a student-level study of seven California school districts (Kirksey and Sattin-Bajaj, 2021). Reduced likelihood of maternal deportation (either through Deferred Action for Childhood Admissions or "Safe Zone" policies) improved child mental health and educational outcomes (Amuedo-Dorantes, Bucheli, and Martines-Donate, 2022; Hainmueller et al., 2017). Case studies of specific raids in Tennessee and Texas demonstrated reductions in academic and mental health outcomes (Heinrich, Hernández, and Shero, 2023; Zuniga, 2018). Relatedly, studies have also documented considerable family trauma associated with prior enforcement rounds (Capps et al., 2015; Dreby, 2012), and have found that prior enforcement rounds reduced infant health (Amuedo-Dorantes, Churchill, and Jong, 2022; Tome et al., 2021; Vu, 2024) and increased Hispanic mortgage foreclosure rates (Rugh and Hall, 2016) though had no effect on adult crime (Miles and Cox, 2014).

Second, we introduce a much more granular measure of exposure to interior immigration enforcement intensity. The idea here is that there are many other factors occurring simultaneously with overall increases in immigration enforcement, but that some communities are much more affected by enforcement increases than others. Our insight comes from the fact that interior immigration enforcement intensity (measured by annualized apprehension rates per thousand non-citizens from a given country of origin) has increased much more dramatically for people born in some countries than in others. Matching data on the timing and country of origin of apprehensions in Florida to estimates from the American Community Survey of numbers of Florida resident non-citizens born in each country, we calculate that, while enforcement intensity has increased across the board, there are dramatic differences across countries of origin in implied annualized apprehension rates in Florida in 2025 versus winter 2024 or prior periods. For instance, implied annualized apprehension rates increased from 1 to 4 per thousand Florida resident non-citizens born in the Dominican Republic, from 1 to 8 per thousand Venezuelans, from 2 to 13 per thousand Ecuadoreans, from 9 to 25 per thousand Nicaraguans, from 11 to 29 per thousand Mexicans, from 12 to 38 per thousand Hondurans, and from 21 to 68 per thousand Guatemalans. We can therefore compare two schools in Florida with identical fractions of foreign-born students – but one with a disproportionate number of, say, Guatemalan students and one with a disproportionate number of Venezuelan students – and suspect that the former school has been much more exposed to recent increases in interior immigration enforcement intensity than the latter school. As LUSD has over 200 schools with vastly different compositions of foreign-born students, there is considerable variation in implied changes in enforcement intensity.

We focus on school-level measures of potential exposure, rather than individual-level measures of potential exposure, both because many students from a given country of heritage are born in the United States (indeed, half of all Spanish-speaking students in Florida, and in LUSD, are U.S.-born), and would therefore be missed in an analysis that links country-specific enforcement intensity changes to affected students, as well as because we conceptualize the change in enforcement as a *community-level* phenomenon. We present circumstantial evidence that the communities that we argue are more likely to be heavily enforced are also the communities most apparently "chilled" by increased enforcement, as measured by declines in cellphone activity in non-school locations in the neighborhood. The high frequency at which Florida tests students also contributes to internal validity of our findings.

Third, our use of individual microdata at the scale that we are able to observe allows us to study pre-post changes in student outcomes for a variety of subgroups, including sample splits by Spanish-speaking and non-Spanish-speaking students, foreign-born and non-foreign-born students, and the interaction between the two. This allows us, for example, to observe that the estimated effects of enforcement intensity are similar for foreign-born Spanish-speakers and US-born Spanish-speakers, leading us to conclude that the effects of enforcement appear to be working not through a specific student's likelihood of being deported but rather through the likelihood that a student's undocumented parents might be at risk of deportation or through broader school/neighborhood-level climate factors. These microdata also allow us to investigate differential incidence of enforcement by measures of prior performance; we observe that the effects of increased enforcement intensity have larger effects on lower-performing Spanish-speakers than on higher-performing Spanish-speakers.

Taken together, this paper therefore yields several new insights about the effects of interior immigration enforcement in the United States, and provides an early picture of the academic implications of the recent surge in immigration enforcement in 2025.

2. Data

We rely on two primary sources of data in our analysis. The first is student-level administrative data from LUSD covering the school years between 2022-23 and 2024-25. There are several reasons why we focus on this timeframe. First, as described above, we leverage the rise in immigration apprehensions following the change in presidential administration in January 2025. Second, we drop years before 2022-23 because (1) Florida started administering the new statewide standardized test (Florida Assessment of Student Thinking [FAST]) in the 2022-23 school year and (2) 2022-23 school year (and later years) are less susceptible to the data availability and data quality issues related to the Covid-19 pandemic.

Administrative school records contain information about student test scores (in grades 3 through 10 in English language arts [ELA] and in grades 3 through 8 in math); student demographics, English learner, home language, and special education status, student absences, school enrollment (including the entry and withdrawal dates to and from each school in which the student was enrolled), and disciplinary incidents.[8] More importantly for our research questions, the data also include information about student country of birth and language spoken at home. In the analysis, our primary outcomes of interest are student test scores (in nationally-normed percentile points) averaged across subjects and disciplinary incidents. The unique aspect of these data is that we can observe these outcomes at a higher frequency than typical (i.e.,

[8] In Florida, middle school students either take the regular FAST test in math or the end of course test if they are enrolled in advanced math courses such as Algebra I, Geometry, or Algebra II. As such, in the analysis, we restrict the math scores to students in grades 3 through 6 – grades in which all students take the regular FAST test in math.

annual) because (1) Florida administers FAST three times a year (once at the beginning of the school year in August or early September, once at the end of December, and once at the beginning of May); and (2) disciplinary data include the day of the incident. As such, we are able to make comparisons within the same school year, between post-January (post-inauguration) and pre-January (pre-inauguration) as we describe in the Empirical Strategy section below.

We link these student-level administrative records with school-level immigration apprehension intensity measures that we calculated using the number of immigration apprehensions by country of birth in the state of Florida obtained using Freedom of Information Act requests from the United States Immigration and Customs Enforcement (ICE)[9] and the distribution of country of birth in each school in LUSD. In particular, we first calculate the apprehension intensity by country of origin as follows:

$$ArrInt_{ct} = \frac{ObsArr_{ct}}{NonCitizen_c} x \frac{365}{N_t} \quad (1)$$

where $ArrInt_{ct}$ represents apprehension intensity for country *c* in period *t*, $ObsArr_{ct}$ is the number of observed immigration apprehensions for country *c* in period *t* in Florida, $NonCitizen_c$ is the estimated number of non-citizens from country *c* in the state from the 2023 American Community Survey, and N_t is the number of days in period *t*.[10] We designate three periods during the year to align with the testing schedule in Florida: (1) the fall period between the third test of the prior school year and the first test of the current school year (May to September); (2) the winter period between the first and second tests of the current school year (September to December); and (3) the spring period between the second and third tests of the

[9] Immigration apprehension data can be found at deportationdata.org, accessed on August 29, 2025.
[10] We are unable to use number of apprehensions at a geographical level smaller than the state as apprehension location is not available at the county level.

current school year (January to May).[11] We then calculate school-level intensity by weighting $ArrInt_{ct}$ with the share of students from each country:

$$ArrInt_{st} = \sum_{c=1}^{C} ArrInt_{ct} * w_{sc} \quad (2)$$

where $ArrInt_{st}$ is the apprehension intensity in school *s* and period *t* and w_{sc} is the share of students in school *s* from country *c*.

Figure 1 presents the spring (post-inauguration) apprehension intensity versus winter (pre-inauguration) apprehension intensity by country of birth in Florida in the 2024-25 school year for the 27 countries with at least 50 children in LUSD in 2024-25 born in that country, with circle size proportional to the size of the non-citizen population from that country[12] and the dashed red line representing the 45-degree line. The results suggest that apprehension intensity increased significantly for all countries of origin after the inauguration. At the same time, as mentioned in the introduction, for some countries of origin the initial level and change in the apprehension rate per thousand non-citizens is dramatically different than for others in absolute terms. This variation in change in enforcement intensity is what generates our identifying variation.

Figure 2 applies this logic to the school level, weighting the country-specific apprehension intensities in Florida by the countries of birth of all students in the school.[13] The figure plots the change in apprehension intensities in LUSD schools from winter to spring in the 2024-25 school year. It is immediately apparent that, while all schools experience increases in

[11] We measure spring period apprehension intensities between January 21 and the timing of the spring test in each year, because the change of presidential administrations took place on January 20, 2025.
[12] We group countries of origin into three size categories to preserve the anonymity of the county-level school district.
[13] Students born in the United States proper and in Puerto Rico are assigned enforcement intensities of zero in all periods for this calculation. All other students' countries of origin have positive enforcement intensities in all periods.

implied apprehension intensities (a function of the fact that apprehension intensities increased for all countries of origin), some schools have much greater changes in implied apprehension intensities than others. This is not due to some schools having more foreign-born students than others: The different markers divide the schools by foreign-born percentage, with blue circles representing schools that fall into the lowest tercile based on foreign-born student share, red triangles representing the middle tercile, and green squares presenting the top tercile. Once again, we find significant increases in immigration apprehension intensity and that increase is highly correlated (positive) with school-level pre-inauguration apprehension intensity, which implies that high pre-inauguration intensity schools experienced a much larger rise in apprehension intensity compared to lower pre-inauguration intensity schools.

3. Empirical Framework

To identify the causal effect of immigration apprehensions on student outcomes, we leverage the rise in apprehensions after the presidential inauguration in January 2025 in difference-in-differences (DiD) and difference-in-difference-in-differences (DDD) frameworks. Specifically, we compare schools with relatively high versus relatively low levels of implied immigration enforcement intensity in the period after the inauguration in January 2025 to the winter period in 2024 (our difference-in-differences). Because there may be other reasons why outcomes might vary between winter and spring that have nothing to do with changes in immigration enforcement intensity, we carry out the same difference-in-differences analysis using data from the 2022-23 and 2023-24 academic years. We then compare these two difference-in-differences approaches to estimate our preferred DDD analysis. We carry out this analysis for outcomes that are observed more than annually (test scores, disciplinary incidents, measures of attrition). Formally, we estimate the following equation using OLS:

$$Y_{isty} = \alpha + \beta_1 ArrInt_{s,w2425} * Spring_y * Y2425 + \beta_2 ArrInt_{s,w2425} * Spring_y + \quad (3)$$

$$\beta_3 ArrInt_{s,w2425} * Y2425 + \beta_4 Spring_y * Y2425 + \delta_i + \gamma_t + \theta_y + \varepsilon_{isty}$$

where Y_{isty} is the outcome of student *i* in school *s*, period *t*, year *y*, $ArrInt_{s,w2425}$ is school-level apprehension intensity during winter period of 2024-25 (the period right before the presidential inauguration), $Spring_y$ is an indicator for the spring period in each year, $Y2425$ is an indicator for 2024-25 school year, δ_i is student fixed effects, γ_t is period fixed effects, and θ_y is year fixed effects. In this setting, β_1, our parameter of interest, can be interpreted as the differential change in the relationship between pre-inauguration immigration apprehension intensity from winter to spring periods in 2024-25 compared to the prior two years.

In the preferred specification, we use the school-level apprehension intensity in the period right before inauguration, but we also check the robustness of our findings to using (1) the change in school-level apprehension intensity between spring and winter periods in 2024-25 (post-inauguration versus the period right before inauguration) and (2) pre-inauguration school-level intensity averaged over all periods between the winter period in 2023-24 school year and the winter period in 2024-25. We cluster our standard errors at the school level.

As a "gut check" for determining whether our measures of increased implied immigration apprehension intensity are actually reflective of the types of community-level factors that we believe them to be, we first carry out estimates of equation (3) in which the dependent variable is a measure of non-school activity in the neighboring community. Specifically, we make use of Advan cellphone ping data linked to specific locations in the neighborhood – the average number of unique visitors per week to "restaurants and other eating places" within two miles of the

school.[14] We interpret declines in visits to these locations as evidence of community-level stress and avoidance of being out in public to the extent possible.

Online Appendix Tables 1A and 1B present evidence that indicates that our measures of differential levels of implied enforcement intensity at the school level are related to broader community-level patterns of reduced foot traffic. While in the years 2022-23 and 2023-24 neighborhoods with higher levels of implied immigration enforcement tended to have significantly greater rates of foot traffic in local restaurants and other eating places in the period between January and April ("spring") versus September through December ("winter"), that robust seasonal increase disappeared in the 2024-25 academic year when "winter" was before the inauguration and "spring" was after the inauguration, suggesting a relative chilling in localized foot traffic following the inauguration. The triple-difference – comparing 2024-25 to preceding years – suggests that a 1-point increase in school-level immigration apprehension intensity, which roughly corresponds to (1) going from the school in the 5th percentile of the intensity distribution to the school in the 95th percentile or to (2) 1/1000 difference in pre-2025 apprehension rates for noncitizens (or about a 3/1000 difference in 2025 apprehension rates), is associated with about a 2 percent decline in foot traffic around elementary schools and a 5 percent decline in foot traffic around middle and high schools, a difference that – while modest in magnitude – is strongly statistically distinct from zero and evidence that what we are considering to be neighborhood measures of increased immigration enforcement led to meaningful reductions in community foot traffic.

4. Results

[14] "Restaurants and other eating places" comprise about 70 percent of non-school points of interest measured in the Advan data.

Table 1 presents our main results and reveals that an increase in relative exposure to immigration apprehensions reduces test scores among Hispanic and Spanish-speaking students while also reducing the likelihood that they are involved in disciplinary incidents in school. In this analysis, we restrict the sample to students who took all three tests in the subjects they are tested for the test score analysis (roughly 93 percent of the students fit this description) and to students who did not leave the district during the school year (about 97 percent of the students) for the disciplinary incident analysis. We discuss the implications of attrition for our results below.

In particular, the first three columns present DiD results (the coefficient on $ArrInt_{s,w2425} * Spring_y$) using the 2024-25 school year for all students, Hispanic students, and students who reported Spanish as their home language; columns 4-6 present the same coefficients using the prior two years; and the last three columns present the DDD coefficient (β_1 in equation (3)) for test scores (nationally-normed percentile rankings averaged across subjects) in the top panel and for disciplinary incidents in the bottom panel. (Online Appendix Table 2 further breaks down the DiD analysis, showing the relationships between differential implied immigration enforcement rates and outcomes in winter versus spring in each year that undergird the DiD analysis.) There are two results worth highlighting.

First, the DiD results reveal no differential change in the relationship between school-level relative apprehension intensity and student outcomes from winter to spring periods in the prior two years. This is expected given that there was no significant change in apprehension intensity in those two years from winter to spring periods. In contrast, we find a negative and statistically significant change in this relationship for test scores in 2024-25 when high-intensity

schools experienced a significant rise in immigration apprehensions from winter to spring for Hispanic students and Spanish-speakers.

Second, the DDD results reported in the last three columns also reveal significant negative effects on both test scores and disciplinary incidents for these student groups. The estimated coefficients suggest that a 1-point increase in school-level immigration apprehension intensity reduces test scores by 0.53 percentiles for Hispanic students and by 0.7 percentiles for Spanish-speakers. These are relatively modest effects: For example, the White-Hispanic test score gap in LUSD is 17 percentiles (so the effect on Hispanic students represents only about 3 percent of this gap) while the gap between English-speakers and Spanish-speakers is 8 percentiles (so the effect on Spanish-speakers corresponds roughly to 10 percent of this gap).

We also find that a 1-point increase in school-level relative immigration enforcement intensity reduces disciplinary incidents by 0.8 percentage points for both Hispanic students and Spanish-speakers, corresponding to 13-14 percent of the dependent variable mean. This negative effect could arise for two reasons. First, it could be due to change in student behavior: Students who are more likely to be exposed to immigration apprehensions (Hispanic and Spanish-speaking students) might try to avoid getting in trouble at school. Second, it could be due to change in educators' attitudes towards these students: Teachers and principals might be more lenient towards similar behaviors from students who experience hardships due to increased immigration enforcement. Unfortunately, due to data limitations, we are unable to disentangle these two channels.

To what extent can these effects be explained by the possibility that the relative apprehension intensity measure is capturing differences in student composition across schools (foreign-born or recent immigrant share, in particular)? Online Appendix Table 3 repeats the

DDD analysis presented in Table 1, this time controlling for the share of foreign-born students in schools and its interactions with $Spring_y$ and $Y2425$ in columns labeled as (II). The results remain virtually unchanged, providing evidence against this potential concern. As an alternative, we investigate how our Table 1 results would look if, instead of our measure of exposure to immigration enforcement, we considered the differential post-inauguration effects of having more Hispanic-identifying students (panel A of Online Appendix Table 4) or of having more Spanish-speaking students (panel B of Online Appendix Table 4). Counter to the statistically significant relationships observed in Table 1, we find no such patterns when we replace our measure of immigration enforcement exposure with measures of student Hispanic ethnicity or Spanish language status. Taken together, these results suggest that our estimated effects of differential levels of implied immigration enforcement are not merely proxies for more Hispanic or Spanish-speaking students in the school.

Online Appendix Table 5 checks the robustness of the findings in Table 1 to alternative measures of school-level relative apprehension intensity. In the first three columns we use the change in apprehension intensity from the winter period to the spring period in 2024-25 while in the last three columns we present findings using the average apprehension intensity between the winter period of 2023-24 and the winter period of 2024-25. The findings reveal similar patterns as in Table 1.

Can these effects on students be explained by student attrition (i.e., some students leaving the district or not taking the test as a result of increased immigration enforcement) or increased student absences, which could explain the decline in both test scores and disciplinary incidents (Dee, 2025; Mumma, 2025; Kirksey and Sattin-Bajaj, 2021)? Table 2 examines this question and shows no effect of increased immigration enforcement on the likelihood that the student takes the

test, leaves the district, or does not attend school. In particular, we replace the outcomes of interest used in Table 1 with an indicator that equals one if the student did not take the test in the top panel; with an indicator if the student left the district in the winter or spring periods in the middle panel; and with percent absent days that we observe annually in the bottom panel. To examine the extent of differential attrition (i.e., certain student groups are more likely to leave the sample) or a differential change in absences, in the last six columns, we break down the sample based on student performance on the first and second tests of the year (below median versus above median of the average test score on these two tests). The top two panels present the DDD estimates (the coefficient on the $ArrInt_{s,w2425} * Spring_y * Y2425$) while the last panel presents the DD estimate (the coefficient on the $ArrInt_{s,w2425} * Y2425$). We do not find any economically or statistically significant effect of relative immigration enforcement intensity on these outcomes.[15]

Table 3 presents the results in Table 1 for different student groups (by grade level, gender, average test scores on the first and second tests of the year, and nativity) among Spanish-speakers in the top panel and non-Spanish speakers in the bottom panel. The results in the top panel suggest that the adverse test score effects are primarily concentrated among middle and high school students, and are slightly larger for female students and lower-performing students. For example, a 1 point increase in the school-level apprehension intensity measure reduces test scores among students in middle and high school by 1.3 percentiles (about 10 percent of the gap between Spanish and English speakers in those grades). The larger estimated effects for middle and high school students might reflect the effects of increased immigration enforcement being

[15] That said, we acknowledge that an increase in immigration enforcement could lead to student attrition at the end of the school year (rather than during the school year). We also note that absences could have increased in the spring period of 2024-25 with the rise in immigration enforcement, but not at a scale that influences the annual absence rate.

more salient for older children, or there being more of a relationship with measured community stress near middle and high schools (as noted in Online Appendix Tables 1A and 1B). Importantly, the findings suggest that this is not just a foreign-born issue: We find similar effects for foreign-born and U.S. born Spanish-speakers. In fact, while we do not find any significant adverse effects on the test scores of non-Spanish speaking students, we find a statistically significant and negative effect when we look at U.S.-born students overall (coefficient of -0.42, p-value: 0.072) and U.S.-born middle and high school students (coefficient of -0.92, p-value: 0.045).

For disciplinary incidents, we find that slightly larger negative effects for middle and high school students, male students, lower-performers, and U.S.-born students although it is important to note that these student groups have larger baseline values of incidents, so the percent change from these baselines are similar across student groups. We do not find any statistically significant effect on disciplinary incidents for non-Spanish speakers.

Finally, in Online Appendix Tables 6A and 6B, we break down the analysis presented in Table 3 by school poverty. In particular, we use a school neighborhood poverty measure created by the National Center for Education Sciences (NCES) for 2021-22 school year (neighborhood income-to-poverty ratio for the neighborhood in which the school is located) and examine the effects separately for above-median poverty schools (i.e., those with lower than median income-to-poverty ratio) and for below-median poverty schools. The results suggest that the adverse effects of immigration enforcement on student test scores are more pronounced in higher-poverty school settings. For example, a 1-point increase in the school-level apprehension intensity measure reduces test scores among students in middle and high school by 1.7 percentiles in

higher-poverty school settings while the same number for lower-poverty middle and high schools is 0.4 percentiles for Spanish-speaking students.

5. Conclusion

We find evidence that the 2025 surge in interior immigration enforcement in the United States led to modestly-sized negative effects on the academic performance of Spanish-speaking students – both foreign-born and US-born. The effects are stronger for lower-performing students than for higher-performing students, are somewhat stronger for girls than for boys, and are concentrated in middle and high schools rather than elementary schools. This is the first evidence on student test scores and disciplinary incidents, and the first student-level evidence on school attendance, in the wake of the recent enforcement surge.

Our identification strategy makes clear that not all immigrant communities have been affected equally from the recent enforcement surge. The surge in 2025 roughly proportionately increased enforcement for all countries of origin, but this means that populations who had low rates of apprehensions pre-surge continued to experience relatively low rates of apprehensions post-surge, while populations with larger rates of pre-surge apprehensions experienced sometimes massively larger rates of apprehensions post-surge. Meanwhile, we find very similar estimated effects for Spanish-speaking US citizens as we do for Spanish-speaking foreign-born children (some of whom may be citizens, but many of whom are not.) This set of findings – and our identification strategy more broadly – signals that we can likely identify the communities of US citizens and non-citizens alike who are experiencing and may continue to experience especially pronounced incidence of increased interior immigration enforcement intensity.

We recognize that the estimated effects that we find are modest in magnitude. That said, our estimated magnitudes of increased immigration enforcement effects are not out of line with

other recently-published papers studying the effects of out-of-school externalities on student test scores, such as Gershenson and Tekin's (2018) result that proximity to the Beltway sniper attacks reduced student outcomes, Laurito's (2022) finding that natural disasters in a student's home country lowered foreign-born students' scores, and Özek's (2023) finding that an influx of Hurricane Maria migrants affected incumbent students' scores. Moreover, we are focusing solely on the *relative* degree to which communities (and schools) are affected in an environment in which all Hispanic, Spanish-speaking, and immigrant families are likely influenced by very public and well-known changes in interior immigration enforcement practices; in the interest of obtaining causal inference, we are only able to capture a partial effect of the overall effects of increased interior immigration enforcement. Finally, of course, we study only the early days of the recent surge and tactical and strategic changes in interior immigration enforcement in the United States; to the degree to which we are identifying the early effects of a sustained practice, our results could be seen as an early bellwether of effects of the country's new immigration policy.

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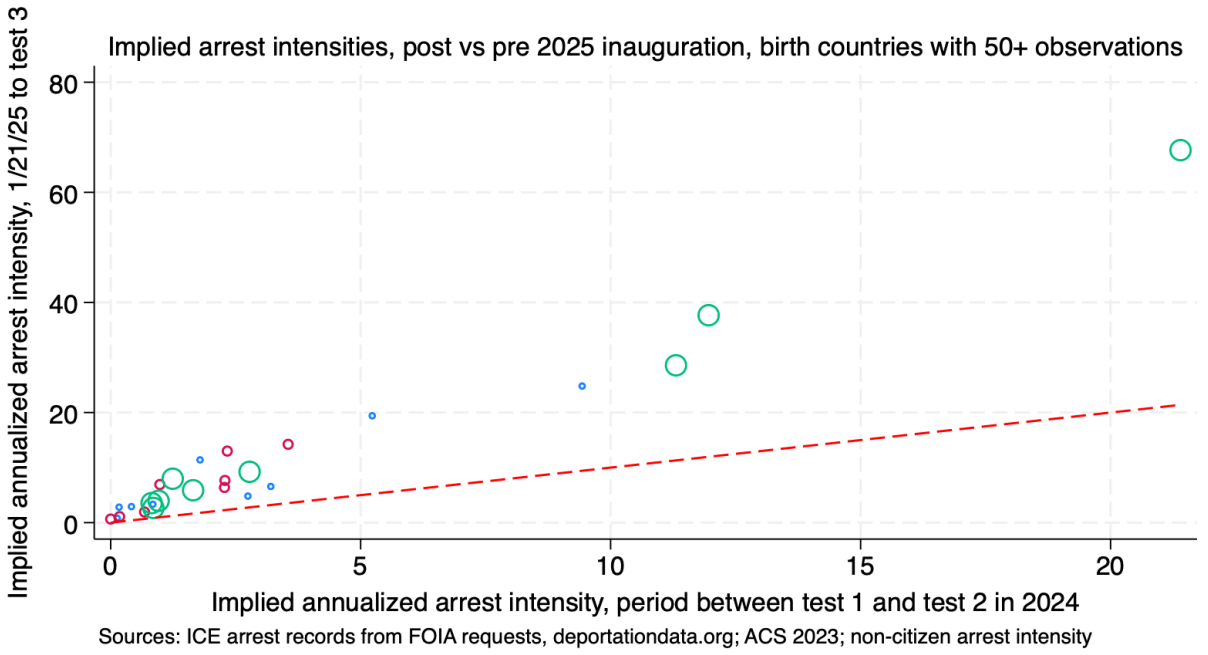
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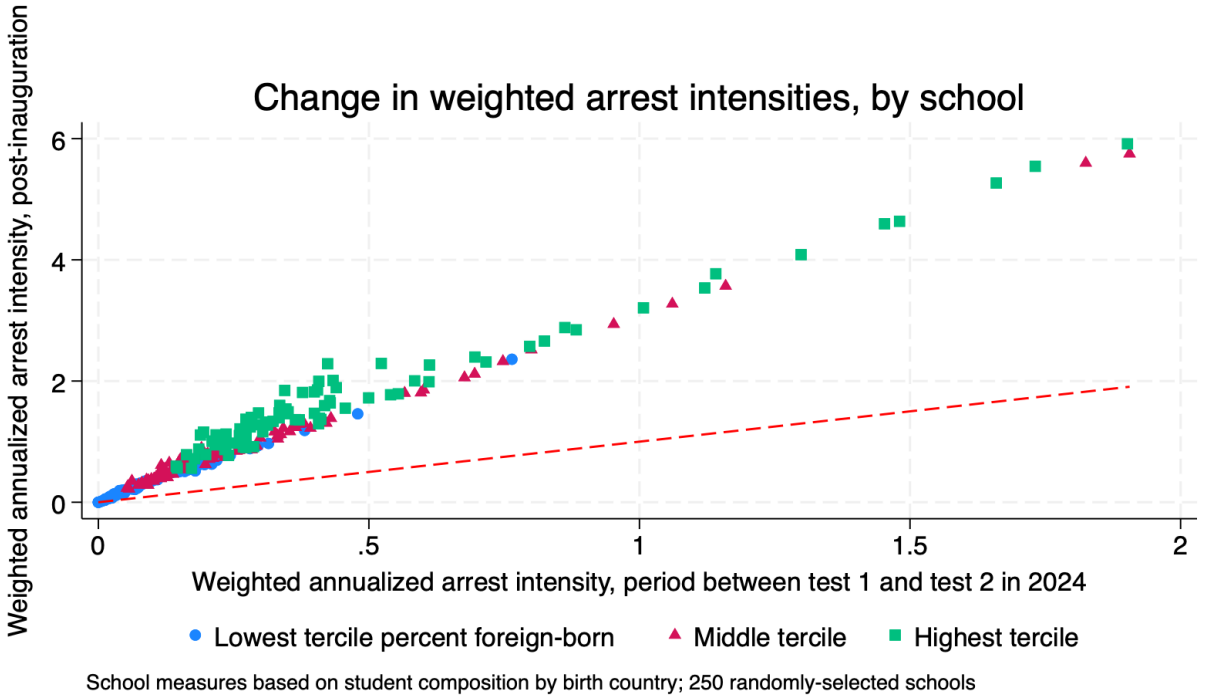
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Figure 1: Country-Level Arrest Intensities, Post- versus Pre-Inauguration, Birth Countries with 50 or More Observations



Notes: The figure presents the spring (post-inauguration) apprehension intensity versus winter (pre-inauguration) apprehension intensity by country of birth in Florida in the 2024-25 school year for the 27 countries with at least 50 children in LUSD in 2024-25 born in that country. Circle sizes are proportional to the size of the non-citizen population from that country and the dashed red line representing the 45-degree line.

Figure 2: Change in School-Level Arrest Intensities from Pre- to Post-Inauguration by Pre-Inauguration Arrest Intensity, LUSD Schools



Notes: The figure plots the change in school-level apprehension intensities in LUSD schools from winter to spring in the 2024-25 school year against the pre-inauguration apprehension intensity, by the share of foreign-born students in the school (blue circles represent schools in the lowest tercile, red triangles represent middle tercile, and the green squares represent the highest tercile schools).

Table 1: Effects of Immigration Enforcement on Student Test Scores and Disciplinary Incidents - Overall, Hispanic Students, and Spanish-Speakers

	Difference-in-differences using 2024-25			Difference-in-differences using 2022-23 and 2023-24			Difference-in-difference-in-differences		
	Overall	Hispanic	Spanish-speaking	Overall	Hispanic	Spanish-speaking	Overall	Hispanic	Spanish-speaking
Average test score	-0.352	-0.659*	-0.779**	-0.064	-0.130	-0.074	-0.288	-0.529**	-0.705***
	(0.312)	(0.340)	(0.390)	(0.288)	(0.318)	(0.297)	(0.219)	(0.205)	(0.209)
SD of dependent variable	28.70	28.39	28.41	28.46	27.91	27.72	28.56	28.09	27.97
N	338,574	151,548	99,987	684,096	304,653	198,408	1,022,670	456,201	298,395
Disciplinary incidents	0.006	-0.002	-0.003	0.007	0.006	0.006	-0.001	-0.008**	-0.008***
	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)
Mean of dependent variable	0.067	0.058	0.059	0.067	0.060	0.060	0.067	0.059	0.060
N	362,304	160,718	105,778	732,150	321,374	209,004	1,094,454	482,092	314,782

Notes: Robust standard errors clustered at the school level are given in parentheses. Difference-in-differences estimates represent the coefficient on the interaction between the school-level immigration enforcement measure in winter 2024-25 and the spring test (semester) indicator in the test score (disciplinary incident) analysis and the interaction between the school-level immigration enforcement measure in winter 2024-25 and the spring semester indicator in the disciplinary incident analysis. Difference-in-difference-in-differences estimates present the estimated coefficient on the interaction between the school-level immigration enforcement measure in winter 2024-25, the spring test (semester) indicator, and an indicator for 2024-25 school year in the test score (disciplinary incident) analysis. All specifications include student fixed effects. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.

Table 2: Effects of Immigration Enforcement on Student Attrition and Absences - Overall, Hispanic Students, and Spanish-Speakers by Prior Test Scores

	Overall			Above median on fall and winter tests			Below median on fall and winter tests		
	Overall	Hispanic	Spanish-speaking	Overall	Hispanic	Spanish-speaking	Overall	Hispanic	Spanish-speaking
Did not take the test	0.011	0.010	-0.001	-0.001	0.004	0.000	-0.004	-0.002	-0.006
	(0.009)	(0.009)	(0.007)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
Mean of dependent variable	0.072	0.080	0.083	0.024	0.027	0.026	0.065	0.068	0.071
N	1,193,661	541,842	356,961	569,148	255,900	168,057	571,962	257,889	168,951
Left the district	0.000	-0.000	-0.001	0.002	0.005	0.001	-0.001	0.001	0.001
	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.004)	(0.004)	(0.005)
Mean of dependent variable	0.030	0.031	0.031	0.013	0.015	0.014	0.031	0.032	0.032
N	1,307,934	584,096	383,534	379,432	170,600	112,038	381,308	171,926	112,634
Percent absent days	-0.001	-0.001	-0.001	-0.001	-0.002	-0.003	-0.001	-0.000	-0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)
Mean of dependent variable	0.093	0.093	0.093	0.065	0.069	0.071	0.095	0.092	0.092
N	547,227	241,046	157,391	168,968	75,459	49,251	170,080	75.898	49,530

Notes: Robust standard errors clustered at the school level are given in parentheses. The top and second panels present difference-in-difference-in-differences estimates - the estimated coefficients on the interaction between the school-level immigration enforcement measure in winter 2024-25, the spring test (semester) indicator, and an indicator for 2024-25 school year in the top (second) panel. The third panel presents the difference-in-differences estimates - the estimated coefficients on the interaction between the school-level immigration enforcement measure in winter 2024-25 and an indicator for 2024-25 school year. All specifications include student fixed effects. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.

Table 3: Effects of Immigration Enforcement on Student Test Scores and Disciplinary Incidents, Subgroup Analysis, Difference-in-Difference-in-Differences Estimates

	Spanish-Speaking Students							
	Elementary	Middle and high school	Male	Female	Lower performing	Higher performing	Foreign born	U.S. born
Average test score	-0.123	-1.290***	-0.596**	-0.831***	-1.043***	-0.590*	-0.697**	-0.745***
	(0.147)	(0.488)	(0.274)	(0.256)	(0.327)	(0.324)	(0.275)	(0.234)
SD of dependent variable	26.90	28.65	28.08	27.81	13.93	18.73	28.83	27.08
N	113,926	184,469	153,309	145,086	144,048	154,347	112,686	185,709
Disciplinary incidents	-0.007***	-0.012	-0.011**	-0.006	-0.012**	-0.006*	-0.006*	-0.010**
	(0.002)	(0.016)	(0.004)	(0.004)	(0.006)	(0.003)	(0.003)	(0.004)
Mean of dependent variable	0.021	0.091	0.078	0.040	0.110	0.036	0.055	0.062
N	139,778	175,004	161,926	152,856	99,280	215,502	109,972	204,810
	Non-Spanish-Speaking Students							
	Elementary	Middle and high school	Male	Female	Lower performing	Higher performing	Foreign born	U.S. born
Average test score	0.055	-0.287	-0.045	-0.052	-0.538	-0.256	0.752	-0.301
	(0.282)	(0.509)	(0.337)	(0.346)	(0.485)	(0.377)	(0.901)	(0.319)
SD of dependent variable	27.31	28.65	28.71	27.49	18.11	14.71	29.79	27.84
N	278,495	445,780	368,157	356,118	282,672	297,210	91,464	632,811
Disciplinary incidents	0.007	0.007	0.009	0.004	0.010	0.008	0.000	0.009
	(0.006)	(0.015)	(0.009)	(0.006)	(0.013)	(0.006)	(0.011)	(0.008)
Mean of dependent variable	0.034	0.100	0.091	0.049	0.153	0.039	0.054	0.072
N	348,540	431,132	397,564	382,108	193,238	431,334	93,224	686,448

Notes: Robust standard errors clustered at the school level are given in parentheses. Difference-in-difference-in-differences estimates present the estimated coefficient on the interaction between the school-level immigration enforcement measure in winter 2024-25, the spring test (semester) indicator, and an indicator for 2024-25 school year in the test score (disciplinary incident) analysis. All specifications include student fixed effects. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.

Online Appendix Table 1A: Relationship between Immigration Enforcement Intensity and Neighborhood Foot Traffic, by School Year, Period, and Grade Level

		Overall			Elementary schools			Middle and high schools		
		Fall/Winter	Spring	Difference-in-differences	Fall/Winter	Spring	Difference-in-differences	Fall/Winter	Spring	Difference-in-differences
2024-25		63.398**	61.198**	-2.447	71.055**	67.861***	-3.474	63.398**	61.198**	-2.447
		(27.144)	(24.361)	(7.358)	(28.139)	(25.070)	(8.489)	(27.144)	(24.361)	(7.358)
	N	4,097	4,776	8,873	2,659	3,100	5,759	4,097	4,776	8,873
2022-23 and 2023-24		48.423	76.492***	27.809***	64.345**	86.509***	21.910*	48.423	76.492***	27.809***
		(31.049)	(23.927)	(10.181)	(31.335)	(24.086)	(11.356)	(31.049)	(23.927)	(10.181)
	N	7,678	9,941	17,619	4,993	6,465	11,458	7,678	9,941	17,619

Notes: Robust standard errors clustered at the school-by-year level are given in parentheses. The first column presents the estimated relationship between the school-level immigration enforcement measure in winter 2024-25 and neighborhood foot traffic in fall/winter of 2024-24 school year (first row) and 2022-23 and 2023-24 school year (second row); the second column repeats the same analysis using spring foot traffic, and the third row presents the difference-in-difference estimate (the coefficient on the interaction between the school-level immigration enforcement measure in winter 2024-25 and the spring indicator). To calculate the dependent variable, we first restrict the cell phone activity data to school months (September-May) and focus on points of interests (POIs) that are labeled as "Restaurants and Other Eating Places", which constitute roughly 70 percent of the non-school POIs in the data. For each school, we then calculated the average unique visitors per week for these non-school POIs for 2022-23, 2023-24, and 2024-25 school year. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.

Online Appendix Table 1B: Effects of Immigration Enforcement on Neighborhood Foot Traffic – Overall and by Grade Level

	Difference-in-differences using 2024-25			Difference-in-differences using 2022-23 and 2023-24			Difference-in-difference-in-differences		
	All schools	Elementary schools	Middle and high schools	All schools	Elementary schools	Middle and high schools	All schools	Elementary schools	Middle and high schools
Average unique visitors per week	-2.447	-3.474	1.283	27.809***	21.910*	51.415**	-30.304***	-25.440***	-50.148**
	(7.358)	(8.489)	(14.181)	(10.181)	(11.356)	(20.984)	(8.651)	(9.111)	(20.360)
Mean of dependent variable	874.3	860.3	900.3	919.1	903.7	947.9	904.1	889.2	931.9
N	8,873	5,759	3,114	17,619	11,458	6,161	26,492	17,217	9,275

Notes: Robust standard errors clustered at the school-year level are given in parentheses. Difference-in-differences estimates represent the coefficient on the interaction between the school-level immigration enforcement measure in winter 2024-25 and the spring semester indicator. Difference-in-difference-in-differences estimates present the estimated coefficient on the interaction between the school-level immigration enforcement measure in winter 2024-25, the spring semester indicator, and an indicator for 2024-25 school year. To calculate the dependent variable, we first restrict the cell phone activity data to school months (September-May) and focus on points of interests (POIs) that are labeled as "Restaurants and Other Eating Places", which constitute roughly 70 percent of the non-school POIs in the data. For each school, we then calculated the average unique visitors per week for these non-school POIs for 2022-23, 2023-24, and 2024-25 school year. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.

Online Appendix Table 2: Relationship between Immigration Enforcement Intensity and Student Outcomes, by School Year and Period, Overall, Hispanic Students, and Spanish-Speaking Students

		Average test score								
		Overall			Hispanic students			Spanish-speaking students		
		Fall/Winter	Spring	Difference-in-differences	Fall/Winter	Spring	Difference-in-differences	Fall/Winter	Spring	Difference-in-differences
2024-25		-11.901***	-12.253***	-0.352	-9.098***	-9.757***	-0.659**	-7.108***	-7.887***	-0.779**
		(2.331)	(2.423)	(0.255)	(2.124)	(2.280)	(0.277)	(1.813)	(2.039)	(0.319)
	N	225,716	112,858	338,574	101,032	50,516	151,548	66,658	33,329	99,987
2022-23 and 2023-24		-11.110***	-11.173***	-0.064	-8.236***	-8.367***	-0.130	-6.234***	-6.308***	-0.074
		(2.141)	(2.293)	(0.257)	(1.843)	(2.042)	(0.283)	(1.546)	(1.748)	(0.264)
	N	456,064	228,032	684,096	203,102	101,551	304,653	132,272	66,136	198,408
		Disciplinary incidents								
		Fall/Winter	Spring	Difference-in-differences	Fall/Winter	Spring	Difference-in-differences	Fall/Winter	Spring	Difference-in-differences
2024-25		0.018	0.025	0.006	0.009	0.008	-0.002	0.004	0.002	-0.003
		(0.014)	(0.019)	(0.006)	(0.011)	(0.013)	(0.004)	(0.010)	(0.012)	(0.003)
	N	181,152	181,152	362,304	80,359	80,359	160,718	52,889	52,889	105,778
2022-23 and 2023-24		0.013	0.020	0.007*	0.002	0.008	0.006	-0.004	0.001	0.006
		(0.012)	(0.015)	(0.004)	(0.008)	(0.012)	(0.004)	(0.007)	(0.009)	(0.003)
	N	366,075	366,075	732,150	160,687	160,687	321,374	104,502	104,502	209,004

Notes: Robust standard errors clustered at the school level are given in parentheses. The first column in the top panel presents the estimated relationship between the school-level immigration enforcement measure in winter 2024-25 and student test scores in fall/winter of 2024-24 school year (first row) and 2022-23 and 2023-24 school year (second row); the second column repeats the same analysis using spring test scores, and the third row presents the difference-in-difference estimate (the coefficient on the interaction between the school-level immigration enforcement measure in winter 2024-25 and the spring test indicator). Columns (4)-(6) in the top panel repeats the same analysis using school Hispanic share, and columns (7)-(9) uses school Spanish-speaker share. The bottom panel presents the results for disciplinary incidents. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.

Online Appendix Table 3: Effects of Immigration Enforcement on Student Test Scores and Disciplinary Incidents - Overall, Hispanic Students, and Spanish-Speakers, Robustness Check Controlling for School Foreign-Born Share and Interactions

	Overall		Hispanic		Spanish-speaker	
	(I)	(II)	(I)	(II)	(I)	(II)
Average test score	-0.288	-0.330	-0.529**	-0.501**	-0.705***	-0.659***
	(0.219)	(0.234)	(0.205)	(0.226)	(0.209)	(0.248)
SD of dependent variable	28.56		28.09		27.97	
N	1,022,670		456,201		298,395	
Disciplinary incidents	-0.001	-0.002	-0.008**	-0.009**	-0.008***	-0.010***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Mean of dependent variable	0.067		0.059		0.060	
N	1,022,670		482,092		314,782	
Controlling for foreign-born share	No	Yes	No	Yes	No	Yes

Notes: Robust standard errors clustered at the school level are given in parentheses. Difference-in-difference-in-differences estimates present the estimated coefficient on the interaction between the school-level immigration enforcement measure in winter 2024-25, the spring test (semester) indicator, and an indicator for 2024-25 school year in the test score (disciplinary incident) analysis. Columns labeled as (I) repeat the results presented in Table 1. Columns labeled as (II) introduce (1) school foreign-born share interacted with the spring test (semester) indicator, and an indicator for 2024-25 school year; (2) school foreign-born share interacted with the spring test (semester) indicator; and (3) school foreign-born share interacted with an indicator for 2024-25 school year in the test score (disciplinary incident) analysis. All specifications include student fixed effects. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.

Online Appendix Table 4: Effects of School Hispanic and Spanish-Speaker Share on Student Test Scores and Disciplinary Incidents - Overall, Hispanic Students, and Spanish-Speakers

	Panel A: School Hispanic Share								
	Difference-in-differences using 2024-25			Difference-in-differences using 2022-23 and 2023-24			Difference-in-difference-in-differences		
	Overall	Hispanic	Spanish-speaking	Overall	Hispanic	Spanish-speaking	Overall	Hispanic	Spanish-speaking
Average test score	0.085	-0.366	-0.500	-0.317	-0.311	0.194	0.402	-0.055	-0.694
	(0.801)	(1.021)	(1.118)	(0.698)	(0.816)	(0.800)	(0.900)	(1.061)	(1.197)
SD of dependent variable	28.69	28.40	28.42	28.47	27.92	27.72	28.56	28.10	27.98
N	346,023	155,484	102,303	684,879	305,019	198,639	1,030,902	460,503	300,942
Disciplinary incidents	0.002	0.008	0.009	-0.009	0.004	0.001	0.011	0.004	0.008
	(0.012)	(0.015)	(0.015)	(0.011)	(0.014)	(0.015)	(0.011)	(0.013)	(0.014)
Mean of dependent variable	0.0668	0.0579	0.0587	0.0671	0.0599	0.0602	0.0670	0.0593	0.0597
N	369,478	164,556	108,062	732,630	321,566	209,114	1,102,108	486,122	317,176
	Panel B: School Spanish-Speaker Share								
	Difference-in-differences using 2024-25			Difference-in-differences using 2022-23 and 2023-24			Difference-in-difference-in-differences		
	Overall	Hispanic	Spanish-speaking	Overall	Hispanic	Spanish-speaking	Overall	Hispanic	Spanish-speaking
Average test score	-0.317	-1.307	-1.484	-0.552	-0.560	0.195	0.236	-0.748	-1.679
	(0.997)	(1.216)	(1.290)	(0.829)	(0.931)	(0.891)	(1.074)	(1.226)	(1.346)
SD of dependent variable	28.69	28.40	28.42	28.47	27.92	27.72	28.56	28.10	27.98
N	346,023	155,484	102,303	684,879	305,019	198,639	1,030,902	460,503	300,942
Disciplinary incidents	0.012	0.011	0.008	0.000	0.010	0.004	0.012	0.001	0.004
	(0.015)	(0.017)	(0.018)	(0.013)	(0.015)	(0.016)	(0.013)	(0.014)	(0.015)
Mean of dependent variable	0.0668	0.0579	0.0587	0.0671	0.0599	0.0602	0.0670	0.0593	0.0597
N	369,478	164,556	108,062	732,630	321,566	209,114	1,102,108	486,122	317,176

Notes: Robust standard errors clustered at the school level are given in parentheses. Difference-in-differences estimates in the top panel represent the coefficient on the interaction between the school Hispanic share in winter 2024-25 (school Spanish-speaker share in bottom panel) and the spring test indicator in the test score analysis and the interaction between the school Hispanic share in winter 2024-25 (school Spanish-speaker share in bottom panel) and the spring semester indicator in the disciplinary incident analysis. Difference-in-difference-in-differences estimates in the top panel present the estimated coefficient on the interaction between the school Hispanic share in winter 2024-25 (school Spanish-speaker share in bottom panel), the spring test (semester) indicator, and an indicator for 2024-25 school year in the test score (disciplinary incident) analysis. All specifications include student fixed effects. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.

Online Appendix Table 5: Effects of Immigration Enforcement on Student Test Scores and Disciplinary Incidents, Robustness to Alternative Immigration Enforcement Intensity Measures

	Difference in intensity between spring and winter 2024-25 school year			Average intensity during the year before spring 2024-25 school year		
	Overall	Hispanic	Spanish-speaking	Overall	Hispanic	Spanish-speaking
Average test score	-0.133	-0.259**	-0.352***	-0.316	-0.621**	-0.842***
	(0.128)	(0.123)	(0.116)	(0.267)	(0.249)	(0.249)
SD of dependent variable	28.56	28.09	27.97	28.56	28.09	27.97
N	1,022,670	456,201	298,395	1,022,670	456,201	298,395
Disciplinary incidents	-0.000	-0.003*	-0.003**	-0.001	-0.009**	-0.009**
	(0.002)	(0.002)	(0.001)	(0.005)	(0.004)	(0.004)
Mean of dependent variable	0.067	0.059	0.060	0.067	0.059	0.060
N	1,094,454	482,092	314,782	1,094,454	482,092	314,782

Notes: Robust standard errors clustered at the school level are given in parentheses. Difference-in-difference-in-differences estimates present the estimated coefficient on the interaction between the school-level immigration enforcement measure provided, the spring test (semester) indicator, and an indicator for 2024-25 school year in the test score (disciplinary incident) analysis. All specifications include student fixed effects. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.

Online Appendix Table 6A: Effects of Immigration Enforcement on Student Test Scores and Disciplinary Incidents, Subgroup Analysis, Difference-in-Difference-in-Differences Estimates, Above-Median Poverty Schools

	Spanish-Speaking Students							
	Elementary	Middle and high school	Male	Female	Lower performing	Higher performing	Foreign born	U.S. born
Average test score	-0.188	-1.723***	-0.711*	-0.908***	-0.873**	-0.895*	-0.772**	-0.808***
	(0.170)	(0.642)	(0.389)	(0.296)	(0.410)	(0.464)	(0.364)	(0.300)
SD of dependent variable	26.28	28	27.52	27.14	12.85	19.41	27.80	26.62
N	60,759	90,792	78,372	73,179	73,254	78,297	53,766	97,785
Disciplinary incidents	-0.007***	-0.011	-0.013***	-0.004	-0.009	-0.008**	-0.009***	-0.009*
	(0.002)	(0.019)	(0.004)	(0.003)	(0.006)	(0.004)	(0.003)	(0.005)
Mean of dependent variable	0.022	0.102	0.084	0.046	0.119	0.042	0.062	0.067
N	74,928	89,174	84,532	79,570	50,578	113,524	53,982	110,120
	Non-Spanish-Speaking Students							
	Elementary	Middle and high school	Male	Female	Lower performing	Higher performing	Foreign born	U.S. born
Average test score	-0.011	-0.711	-0.035	-0.656	-0.318	-0.548	-0.136	-0.466
	(0.325)	(0.544)	(0.366)	(0.447)	(0.547)	(0.419)	(0.944)	(0.374)
SD of dependent variable	26.95	28.65	28.48	27.40	15.73	17.28	29.87	27.65
N	113,276	168,373	143,892	137,757	107,142	112,056	35,235	246,414
Disciplinary incidents	0.008	0.016	0.011	0.009	0.007	0.013	0.004	0.012
	(0.009)	(0.018)	(0.012)	(0.009)	(0.018)	(0.009)	(0.013)	(0.011)
Mean of dependent variable	0.050	0.131	0.119	0.068	0.196	0.059	0.075	0.097
N	142,592	170,164	159,702	153,054	74,222	169,526	37,702	275,054

Notes: Robust standard errors clustered at the school level are given in parentheses. Difference-in-difference-in-differences estimates present the estimated coefficient on the interaction between the school-level immigration enforcement measure in winter 2024-25, the spring test (semester) indicator, and an indicator for 2024-25 school year in the test score (disciplinary incident) analysis. Above median poverty schools are defined as those in LUSD that fall into the bottom half of the neighborhood income-to-poverty ratio measure provided by the National Center for Education Statistics. All specifications include student fixed effects. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.

Online Appendix Table 6B: Effects of Immigration Enforcement on Student Test Scores and Disciplinary Incidents, Subgroup Analysis, Difference-in-Difference-in-Differences Estimates, Below-Median Poverty Schools

	Spanish-Speaking Students							
	Elementary	Middle and high school	Male	Female	Lower performing	Higher performing	Foreign born	U.S. born
Average test score	0.115	-0.405	-0.094	-0.685	-1.129	-0.023	-0.094	-0.649
	(0.350)	(1.905)	(0.774)	(0.609)	(0.791)	(1.057)	(0.940)	(0.476)
SD of dependent variable	27.16	28.96	28.43	28.18	15.19	17.71	29.33	27.35
N	53,167	93,677	74,937	71,907	71,340	75,504	58,920	87,924
Disciplinary incidents	-0.006	-0.020	-0.001	-0.015	-0.027**	0.003	-0.004	-0.011*
	(0.005)	(0.017)	(0.010)	(0.012)	(0.011)	(0.009)	(0.012)	(0.006)
Mean of dependent variable	0.019	0.079	0.071	0.035	0.099	0.032	0.048	0.056
N	64,850	85,830	77,394	73,286	48,588	102,092	55,990	94,690
	Non-Spanish-Speaking Students							
	Elementary	Middle and high school	Male	Female	Lower performing	Higher performing	Foreign born	U.S. born
Average test score	0.061	-0.175	-0.044	0.863	-0.509	-0.210	3.878*	-0.045
	(0.699)	(1.670)	(0.994)	(0.929)	(1.110)	(1.005)	(2.044)	(0.797)
SD of dependent variable	26.37	28.09	28.11	26.75	19.46	12.97	29.08	27.18
N	165,219	277,407	224,265	218,361	176,979	183,705	56,229	386,397
Disciplinary incidents	0.002	-0.044*	-0.001	-0.030***	-0.026	-0.006	-0.022	-0.015
	(0.007)	(0.024)	(0.014)	(0.010)	(0.023)	(0.009)	(0.027)	(0.012)
Mean of dependent variable	0.0222	0.0795	0.0721	0.0356	0.120	0.0287	0.0399	0.0561
N	205,948	260,968	237,862	229,054	119,740	261,084	55,522	411,394

Notes: Robust standard errors clustered at the school level are given in parentheses. Difference-in-difference-in-differences estimates present the estimated coefficient on the interaction between the school-level immigration enforcement measure in winter 2024-25, the spring test (semester) indicator, and an indicator for 2024-25 school year in the test score (disciplinary incident) analysis. Above median poverty schools are defined as those in LUSD that fall into the top half of the neighborhood income-to-poverty ratio measure provided by the National Center for Education Statistics. All specifications include student fixed effects. *, **, *** imply that the estimated coefficient is statistically different from zero at 10, 5, and 1 percent level respectively.