Annenberg Institute
BROWN UNIVERSITY

EdWorkingPaper No. 26-1453

Immigration Enforcement Actions and Empty Desks: Persistent and Acute Attendance Effects

Andrew M. Camp
Brown University

Jonathon Acosta
Brown University

Janelle Haire
Brown University

Edom Tesfa
Brown University

How do immigration enforcement actions (IEAs) affect student attendance, and through what channels? We use student-by-day administrative records from a mid-size school district to estimate the causal effect of heightened federal immigration enforcement following the January 2025 presidential inauguration on student attendance using a difference-in-differences design. We find that IEAs cause a substantial and persistent increase in absences among foreign-born students, with the daily probability of absence rising by 2.2 percentage points (37%) relative to a pre-treatment mean of 5.9%. We decompose these effects into two distinct channels: 1. a sustained elevation in absences spanning the full post-treatment period and 2. acute, short-lived spikes on dates proximate to specific enforcement events. The sustained elevation in absences dominates and show no signs of attenuation during our study period. Effects increase nearly monotonically with grade level, consistent with older students exercising greater autonomy over their own attendance decisions. We also show that estimates using more common proxies for student vulnerability, such as MLL status, likely understate the effects experienced by the most directly affected students.

VERSION: April 2026

Suggested citation: Camp, Andrew, Jonathon Acosta, Janelle Haire, and Edom Tesfa. (2026). Immigration Enforcement Actions and Empty Desks: Persistent and Acute Attendance Effects. (EdWorkingPaper: 26-1453). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/bz9z-w627

IMMIGRATION ENFORCEMENT ACTIONS AND EMPTY DESKS: PERSISTENT AND ACUTE ATTENDANCE EFFECTS

Andrew M. Camp[1†], Jonathon Acosta[1], Janelle Haire, and Edom Tesfa[1]

[1]Brown University

This version: April 2026
(Click here for most recent version)

ABSTRACT

How do immigration enforcement actions (IEAs) affect student attendance, and through what channels? We use student-by-day administrative records from a mid-size school district to estimate the causal effect of heightened federal immigration enforcement following the January 2025 presidential inauguration on student attendance using a difference-in-differences design. We find that IEAs cause a substantial and persistent increase in absences among foreign-born students, with the daily probability of absence rising by 2.2 percentage points (37%) relative to a pre-treatment mean of 5.9%. We decompose these effects into two distinct channels: 1. a sustained elevation in absences spanning the full post-treatment period and 2. acute, short-lived spikes on dates proximate to specific enforcement events. The sustained elevation in absences dominates and show no signs of attenuation during our study period. Effects increase nearly monotonically with grade level, consistent with older students exercising greater autonomy over their own attendance decisions. We also show that estimates using more common proxies for student vulnerability, such as MLL status, likely understate the effects experienced by the most directly affected students.

Keywords: Student attendance, School absences, Immigrant students, Immigration enforcement

Acknowledgements: We thank Jesse Bruhn, participants in the Annenberg Institute's Half-Baked seminar series, and attendees of the Association for Education Finance and Policy's 51st Annual Conference for helpful comments on an earlier version of this paper. We are especially grateful to Liberty City School District for their continued partnership and collaboration.

---

† Corresponding author: andrew_camp@brown.edu

1 INTRODUCTION

In 1982, the U.S. Supreme Court issued the Plyler v. Doe decision which established that undocumented children could not be denied access to a public education in the United States. The ruling found that "the long-range costs of excluding any children from public schools may well outweigh the costs of educating them" (*Plyler v. Doe*, 1982). Despite this legal precedent, federal actions in adjacent policy domains (i.e., immigration enforcement) can undermine the exercise of that right by discouraging families from sending their children to school. Schooling is a keystone of both human capital development and civic participation (Card, 1999; Dee, 2004). Student attendance is a necessary condition for schooling to occur, meaning that policies which adversely affect student attendance may have long-term negative impacts on both the students themselves and the economic and civic societies in which those students live.

The inauguration of Donald J. Trump to his second term as President, accompanied by a sharp escalation in immigration enforcement activities, has caused widespread disruption in immigrant communities across the United States (Rodrigues, 2025). A growing body of research has begun to document the effects of this escalation on K-12 schooling. Using school-level data, Dee (2025) finds attendance declines in California's agricultural Central Valley; Slungaard Mumma (2025) documents similar effects in Connecticut and Rhode Island. Effects on students extend beyond attendance, with Figlio and Özek (2025) finding that increased immigration enforcement activities reduced student achievement. This emerging literature leaves several questions unanswered. First, existing estimates of effects on student attendance do not distinguish between persistent level-shifts in attendance behavior and acute responses to specific enforcement events, a distinction that matters for how schools design re-engagement interventions. Moreover, most research relies on relatively crude proxies for the population at risk (e.g., multi-lingual learner status) and none examine whether effects vary across grade levels in ways that could inform how districts allocate support and response resources.

We address these gaps using the case of Liberty City,[1] a mid-sized city with a strong institutional orientation toward immigrant inclusion located in the northeastern United States. We assemble a novel data set combining daily student-level administrative records from the Liberty City school district with a comprehensive timeline of immigration enforcement actions (IEAs) drawn from community-sourced reports and records obtained through a Freedom of Information Act (FOIA) request. Using student birthplace as a direct measure of vulnerability to immigration enforcement, we employ a difference-in-differences design to estimate the causal effect of the post-inauguration enforcement regime on attendance among foreign-born students. Our episode-level variation allows us to further decompose these effects into persistent shifts in attendance behavior and acute responses to specific enforcement events.

Our results reveal that the dominant effect of immigration enforcement on attendance is not a transient spike in absences around specific events but rather a sustained deterioration with daily absence rates among foreign-born students increasing by approximately two percentage points (34%) across the post-inauguration period. The acute effects of specific IEA episodes are comparatively modest and are concentrated in the day of and day after an event. We further document substantial heterogeneity across grade levels, with younger students substantially less affected than their older peers. These findings are robust to alternative definitions of the treated populations, tests for selective attrition, and when accounting for potential spillover effects on U.S.-born students.

Our paper proceeds as follows. Section 2 describes prior research on immigration enforcement and student attendance as well as providing context on Liberty City. Section 3 details our data sources and provides descriptive evidence motivating our identification strategy. Section 4 formalizes our identification strategy while Section 5 presents our primary results. In Section 6, we discuss several extensions of our analysis that are informative for both policymakers and researchers in this area. We conclude our paper with a discussion of implications, limitations, and areas for future research in Section 7.

---

[1] Liberty City is a pseudonym for the community. In later sections, we describe the community in more detail while preserving its anonymity for the safety and protection of its residents and school district staff.

2 Prior Research and Study Context

Immigration enforcement actions share key features with other traumatic community events whose educational consequences are well documented. Research on adolescent exposure to violent crime (Burdick-Will, 2016) and gun violence (James et al., 2021) finds that students living in proximity to such events are more likely to miss school than otherwise similar peers living farther away, with downstream effects on academic performance and student wellbeing. Like these events, IEAs are difficult to predict, generate acute fear within affected communities, and may radiate outward to individuals who are not directly targeted. This parallel suggests that we should expect IEAs to disrupt schooling along similar dimensions. The extant literature on immigration enforcement and student outcomes which we review here confirms this expectation.

2.1 Immigration Policy and Student Attendance

The relationship between immigration enforcement and school attendance has been documented across multiple enforcement regimes, geographic context, and time periods. The earliest evidence comes from studies of federal cooperation programs that expanded the reach of immigration enforcement into local communities. Bellows (2021) examined the rollout of enforcement agreements between local law enforcement and federal immigration authorities (i.e., 287(g) agreements) in North Carolina using individual-level student records and a triple-difference design comparing Hispanic MLL, Hispanic non-MLL, and non-Hispanic students in counties approved versus rejected for 287(g) participation. They found that the program increased absences by approximately one day per year for Hispanic students, with the effect driven by chronic absenteeism among Hispanic students who had ever been classified as multi-lingual learners. Kirksey et al. (2020) linked county-level deportation data to California school district records and found that increases in local deportations widened White-Latino chronic absenteeism gaps, with effects moderated by geographic proximity to enforcement activity.

Workplace raids and targeted enforcement operations produce the sharpest short-run attendance disruptions documented in the literature. Heinrich et al. (2023) studied a 2018 raid in Morristown, Tennessee using linked health and education administrative data and documented substantial spikes in student absences in the twelve months following the raid. Kirksey (2025) used weekly attendance data from a California Central Valley district spanning 2014-2018 and found that large immigration arrest events produced immediate attendance drops of 10-11 percentage points among migrant and Latino students, alongside a sustained decline of approximately two percentage points over the study period. Findings from this study are noteworthy in that, as with our analysis, they disentangle short-term and sustained effects thereby highlighting the need to consider these channels separately.

The escalation in enforcement activity beginning with the second inauguration of Donald J. Trump on January 20th, 2025 has generated a new wave of research connecting the effects of immigration policy and student attendance. Dee (2025) studied the January-February 2025 U.S. Customs and Border Protection (CBP) raids in California's Central Valley using school-level data and an event-study design. He found that daily absence counts increased by roughly 22 percent following the raids, with pre-K absences rising by approximately 35 percent. These effects were not transitory in that elevated absences persisted through the end of the observation window, suggesting potential enrollment losses with longer-term developmental consequences for students and fiscal consequences for school districts. Slungaard Mumma (2025) documented similar patterns at a broader geographic scale using data from public-facing attendance dashboards in Connecticut and Rhode Island. They find evidence of a widening absence gap between MLL and non-MLL students following the enforcement escalation.

Taken together, this body of work establishes that immigration enforcement has negative effects student attendance. However, several questions remain unanswered. Most existing studies rely on aggregate attendance measures, such as at the school- or district-level, that cannot identify which students are most affected or trace individual attendance trajectories over time. The literature has also struggled to distinguish between acute responses to specific enforcement events and persistent shifts in attendance behavior, in part because of data limitations that prevent researchers from linking daily attendance records to a precise timeline of enforcement activity. Finally, the proxies used to identify the population at risk vary widely across studies, from Hispanic ethnicity to MLL status, and the sensitivity of estimates to these definitional choices has received limited attention. Our study addresses each of these gaps.

2.2 Immigration Policy and Other Student Outcomes

The effects of immigration enforcement on students extend well beyond attendance. A substantial body of evidence documents harms to academic achievement, enrollment, disciplinary outcomes, health, and family economic stability; outcomes that both contextualize the attendance effects we study and illuminate the broader mechanisms through which enforcement disrupts children's schooling.

As with the attendance effects literature, research on federal cooperation programs provides the earliest evidence on achievement effects. Bellows (2019) exploited the staggered rollout of Secure Communities across U.S. counties and found that the program reduced ELA achievement for Hispanic students. Notably, non-Hispanic Black students also experience achievement declines, suggesting that enforcement disrupts broader school and community environments. Weber (2022) replicates and extends this analysis, finding that Hispanic students in "sanctuary" jurisdictions experienced no achievement decline, suggesting that local non-cooperation policies can insulate students from enforcement harms. Kirksey and Sattin-Bajaj (2021) link county-level ICE arrest data to California student records and found that arrests corresponded to academic achievement declines of 0.09-0.17 standard deviations for Latino students and MLLs, with effects larger under the first Trump administration than under Obama's second term.

Raid-specific studies document some of the largest achievement effects in the literature. Using state assessment data and a triple differences design, Avila (2024) studied a 2019 raid in Allen, Texas finding test that on assessments administered just 40 days after the operation both test scores and passing rates declined for Hispanic students. Importantly, these effects were not primarily driven by absenteeism implying instead that psychological distress was an operative mechanism. Bennett et al. (2025) use individual ICE arrest records from the New Orleans field office and student achievement data to estimate declines of 0.27-0.52

standard deviations in student achievement. A study of the 2025 enforcement wave has yielded similarly large results. Figlio and Özek (2025) used student-level administrative data from a large Florida school district and found test score declines for both U.S.-born and foreign-born Spanish-speaking students, with effects concentrated in high-poverty schools and among lower-performing students. The fact that U.S.-born students were affected comparably suggests that the negative effects of enforcement operates through school, family, and community channels rather than solely through individual immigration status.

Beyond academic outcomes, enforcement has documented effects on student health and family economic resources which may be upstream determinants of school readiness and performance. Heinrich et al. (2023) found elevated diagnoses of depression, substance use disorder, and self-harm among children in communities affected by the Tennessee raid alongside increases in exclusionary disciplinary actions. Figlio and Özek (2025) documented declines in disciplinary incidents following the 2025 enforcement surge, a pattern consistent with fear-driven withdrawal from school life rather than behavioral disruption. Alsan and Yang (2024) documented significant declines in welfare program (e.g., Supplemental Nutrition Assistant Program or SNAP) participation among Hispanic citizen-headed households following Secure Communities activation, with effects muted in sanctuary cities. These safety-net withdrawal effects represent indirect but consequential channels through which enforcement undermines the conditions necessary for children to succeed in school.

2.3 Immigration and Liberty City

Liberty City is a historically immigrant-receiving community in the northeastern United States. The city was a manufacturing hub during the peak of the American Industrial Revolution, home to a dense population of mill workers and their families. Since the decline of northeastern manufacturing, the city has faced economic and social challenges characteristic of the postindustrial era such as population decline, an eroding tax base, and recurring municipal fiscal distress. Following national immigration reform in 1965, the city welcomed migrants from across Latin America and Africa, reversing its population trajectory from decline to modest steady growth by 1980. Fueled by continued immigration and the U.S.-born children of earlier arrivals, Liberty City became a majority-minority community in the 2000s. At present, approximately 40% of city residents are foreign born and more than two-thirds identify as Hispanic or Latino. These demographic realities make the city and its residents acutely exposed to federal immigration enforcement.

It is precisely this exposure that has motivated local institutional responses, positioning Liberty City as what Golash-Boza and Valdez (2018) term a welcoming "nested" context of reception. The nested contexts of reception framework emphasizes that immigrant incorporation is shaped not only by federal policy but by layered local, state, and institutional conditions that immigrants encounter in daily life. Public schools are central to these nested contexts; operating as state institutions with which immigrant-origin children and their families regularly interact, and mediate the relationship between families and the broader policy environment (Murillo et al., 2021; Thompson et al., 2020). The practices, policies, and posture of school and district leaders can either reinforce or buffer against the effects of hostile federal policy (Rodriguez et al., 2022). Critically, these welcoming nested contexts are not fixed and can be disrupted by external shocks such as global pandemics (Lowenhaupt et al., 2025) or, as we examine here, local immigration enforcement activity.

In Liberty City, both municipal and school leaders have taken deliberate steps to construct a welcoming local context. Following the first Trump election, the city convened a community meeting with residents, elected officials, school leaders, and city directors to address anxieties about federal enforcement. At that meeting, the police chief publicly stated that local law enforcement serves residents, not federal immigration authorities. More directly relevant to the outcomes we examine, the Liberty City School District (LCSD) revised its policies in 2024 to explicitly prohibit sharing undocumented students' information with federal agencies. These actions reflect a deliberate effort by school and municipal leaders to signal institutional protection and maintain trust with immigrant families.

This welcoming context was disrupted in late January 2025 when the first known arrest by Immigration and Customs Enforcement under the second Trump administration was carried out in Liberty City. Within hours, city officials contacted local leaders and district staff to confirm that a resident with a criminal warrant had been arrested. The panic generated by this event and the enforcement actions that followed form the basis of the disruptions we study.

3 Data

Our analyses combine student-by-day attendance records and student's demographic information with records of immigration enforcement actions in the Liberty City community. Our primary source of IEA records is community sourced and were collected in partnership with school district staff and a local non-profit immigrant advocacy organization. Through our partnership with these organizations, we were able to maintain a log of all reported IEAs in the community. The school district maintained the log while the advocacy organization trained volunteers on properly identifying ICE agents. The log offers the advantage of allowing us to identify IEAs within the municipal boundaries rather than at the state level where most publicly available data is reported.

While our community-sourced records of IEAs have the advantage of capturing community members' perceptions of IEAs, there are two potential flaws with these data. First, these data cover only the Liberty City community. IEAs occurring nearby, but outside of Liberty City, may still have impacts on student attendance because students and families may be aware of and respond to these events. Second, community members reporting IEAs may not be aware of all incidents in Liberty City. Neither of these limitations introduce bias into our estimates of overall effects as our identification strategy relies on comparing groups across the full post-treatment period; not from event timing relative to specific IEAs. However, as discussed in Section 4, we also decompose effects on student attendance into persistent and acute (time-limited) effects. To explore the robustness of these analyses using the community-sourced data, we also incorporate government data provided by Immigration and Customs Enforcement (ICE) in response to a Freedom of Information Act Request from the Deportation Data Project.[2]

---

[2] https://deportationdata.org/

Event-level data is useful for unpacking the mechanisms by which IEAs might affect student attendance. Vulnerable students may respond to enforcement through two distinct channels: a low-intensity, persistent shift in their baseline propensity to be absent and a high-intensity, acute response concentrated in the days surrounding salient IEAs. Critically, different combinations of these channels can produce observationally equivalent estimates in a standard difference-in-differences framework, making it difficult to distinguish between them without leveraging variation in the timing of specific enforcement events. As an illustration, consider a scenario in which there are 100 school days following the onset of treatment. If effects were only through the low-intensity, persistent channel, we might obtain a point estimate of 0.05 if an additional 5% of vulnerable students were absent on each post-treatment day. Conversely, if effects were only through the high-intensity, acute channel, we may obtain that same point estimate if every vulnerable student were absent on just five specific days (or half of vulnerable students absent on ten specific days).

Our attendance data are at the student-by-school day level and contain information about each student's status on that particular day (i.e., absent, present, tardy, or early dismissals).[3] Students are considered absent if they are marked absent for more than half of the school day. These records contain additional information for each student's attendance status on a given day. For example, absences due to suspensions are differentiated from absences due to illness or family emergencies. Similarly, early dismissals due to disciplinary incidents are differentiated for nine other reasons.

Student demographic data provided by LCSD include student's race/ethnicity, date of birth, enrollment entry/exit date, home language, multilingual learner status, and recent immigrant status.[4] We augment these data with birth location data which LCSD collects at the time students enroll in the district. Students are required to provide documentation, such as a birth certificate for U.S.-born students, before they can be enrolled in the district. As these records were hand-entered by district staff, two members of

---

[3] Students may be marked as both present and tardy or present and dismissed early on a particular day, reflecting scenarios in which a student is present for most of the day but tardy to a single class period or dismissed early. Here, we retain the tardy/early dismissal observations as they are more informative about attendance patterns.

[4] Recent immigrant status is defined by Title III § 3301(6) as school-aged students born in a foreign country or U.S. territory, except Puerto Rico, who have not attended schools in the U.S. for more than three full academic years.

the research team classified birth locations using keyword matching and manual review to classify students born in any state or U.S. territory (i.e., *jus soli* citizens) as born inside the U.S. and students born in another country as born outside the U.S. For a small number of students in our analytic sample (N = 200; 4%), birth location data is unavailable.[5]

The combined student attendance, demographics, and birth locations comprise approximately 1,950,000 student-by-school day observations from the 2021-22 through 2024-25 school years. We omit student attendance records for the 2021-22 school year for two principled reasons. First, these records are less informative about each student's propensity to be absent (or dismissed early) due to several outbreaks of COVID-19 variants during this school year. Second, change in policy required that the district affirmatively mark students as present starting in the 2022-23 school year. Restricting our analysis to use attendance records for the 536 school days covering the 2022-23 through 2024-25 school year then reduces potential measurement error that would be introduced if district staff forgot to mark a student as absent, tardy, or dismissed early. In total, our analytic data frame contains approximately 1,350,000 student-by-school day records.

A potential concern about our analysis is that IEAs may cause families to leave the district entirely. If disenrollment is correlated with treatment status (e.g., if families of students born outside the U.S. are more likely to relocate in response to enforcement) then our panel would become compositionally different over time in ways that could bias our estimates. If the families most sensitive to enforcement leave, the remaining treated students would be less responsive on average, attenuating our estimates. Conversely, if the families who stay are those most constrained in their ability to relocate and therefore most anxious, the opposite could occur. In Appendix A, we show that there were no notable changes in patterns of

[5] Missing records may reflect families who refused to provide birthplace information at the time of enrollment, potentially because they were reluctant to disclose an international birthplace. If this is the case, the excluded students may be disproportionately drawn from the population most vulnerable to immigration enforcement, and their exclusion would modestly attenuate our estimates by removing some of the most-affected students from the treated group. Given the small number of students affected, this is unlikely to meaningfully change our findings. We confirm that our results using alternative treatment definitions (which do not rely on birthplace and therefore include these students) are not sensitive to their inclusion or exclusion.

disenrollments during our study period, suggesting that compositional change in our panel is not a meaningful concern over this time horizon. We note, however, that disenrollment effects may emerge over longer periods. Families may, for example, relocate between school years rather than mid-year to avoid interrupting students' educational experiences or in response to continued immigration enforcement pressures.

Identifying which students may be affected by an IEA is challenging for several reasons. First, student characteristics used in prior studies, such as MLL status or home language, are imperfect proxies for vulnerability to immigration enforcement as none directly measure citizenship or documentation status. A student classified as a MLL may either be a U.S. citizen born into a household where English is not the primary language or may be an undocumented non-citizen—two students with different exposure to enforcement risk. Home language suffers from the same ambiguity as speaking a language other than English at home tells us little about a family's immigration status. While our data include an indicator for being a recent immigrant, this variable is defined as having spent less than three years in U.S. schools and therefore may fail to identify students who have spent substantial time in U.S. schools but are still undocumented. Each of these proxies introduces measurement error of unknown magnitude to any identification of IEA effects. Although the direction of this error is conservative in the sense that it would bias point estimates towards zero, this provides little comfort when the goal is to accurately characterize the magnitude of effects on students.

Instead of relying on these proxies, we leverage the previously discussed student birthplace records to identify students born outside of the United States as the treated group, with U.S.-born students forming the comparison group. This definition is not without its own limitations. Some foreign-born students may be citizens (e.g., if one or both parents are U.S. citizens) while others may face little risk of deportation and be unresponsive to IEAs. Conversely, some U.S.-born students may live in mixed-status households where a parent or sibling may be undocumented. These students would plausibly be affected by IEAs yet would be considered members of our "comparison" group. As with the previously discussed proxies, both sources of misclassification would bias our estimates towards zero, suggesting that the effects we report likely understate the true impact on affected students. In section 6.3 we discuss our approach to estimating potential spillover effects on these vulnerable U.S.-born students.

Despite these shared limitations, we argue that birthplace is a stronger proxy than alternatives for two reasons. First, being born outside the United States is a necessary condition for many categories of deportability, creating a more direct link between our treatment indicator and actual enforcement risk than proxies based on language. Second, and more concretely, we show in Section 6.2 that our main specification, which defines treatment based on birth location, produces larger point estimates when using birthplace than when using MLL status, home language, or the recent immigrant indicator—a pattern consistent with less measurement error in treatment defined by birthplace and one that would not arise if all proxies were equally noisy measures of the same underlying construct.

We define the treatment period, during which we anticipate vulnerable students responding to IEAs, as beginning on January 20th, 2025 (i.e., the date of the presidential inauguration). While IEAs occurred prior to this date, the change in administration marked a clear shift in federal enforcement posture, with executive orders signed on the first day in office directing a substantial expansion of interior enforcement operations (Trump, 2025). We acknowledge, however, that the choice of start date is to some degree an arbitrary decision. Media reporting documents increases in the intensity of IEAs in the weeks leading up to the inauguration (e.g., Hiltzik, 2025). An earlier date would capture any anticipatory effects in the weeks between the November 2024 election and inauguration while a later date would more precisely align treatment with the first IEAs in the Liberty City community in late January. Ultimately, the choice of treatment start date reflects a tradeoff between two forms of attenuation: an earlier start date risks classifying untreated days as treated, diluting the estimated effect, while a later start date risks excluding genuinely treated days from the treatment period, similarly compressing estimates toward zero. We show in Appendix B that our choice of the inauguration has little practical consequence for our findings through a specification curve analysis (Simonsohn et al., 2020).

<< Table 1 – Student Characteristics by Birthplace and School Year >>

Table 1 reports student characteristics and attendance patterns by birthplace and school year. During the 2023-24 school year, foreign-born students attended school at higher rates than their U.S.-born peers and were 3.6 percentage points more likely to be marked present and 2.4 percentage points less likely to be absent, with both differences statistically significant. Foreign-born students were also less likely to be tardy. Early dismissal rates were low for both groups (0.2%) and showed no meaningful difference by birthplace in either year.

This pattern attenuates markedly in 2024-25. The gap in absence rates narrows from 2.4 percentage points to 0.4 percentage points and is no longer statistically distinguishable from zero. The difference in presence rates shrinks by more than half, to 1.5 percentage points, and is only marginally significant. The convergence is driven almost entirely by changes among foreign-born students, whose absence rate rises from 8.9 to 9.3 percent while U.S.-born students' absence rate falls from 11.3 to 9.7 percent. Early dismissal rates, by contrast, remain nearly identical across groups and years — a pattern we return to in Section 6.1. These descriptive patterns are consistent with a post-inauguration attendance decline concentrated in absences among foreign-born students, though we defer causal interpretation to Section 5.

<< Figure 1– Average Absence Rate by Student Birthplace and School Year >>

Figure 1 plots monthly absence rates by birthplace across two school years. In Panel A (2023-24), the two groups follow similar seasonal patterns — rising through the fall, peaking in December or January, and declining through the spring — with foreign-born students consistently absent at lower rates than their U.S.-born peers.[6] The gap between groups is roughly stable across months. Panel B (2024-25) shows a striking departure from this pattern. Both groups track each other closely through the fall, but beginning in

[6] This pattern is also visible during the 2021-22 school year, though COVID-19 related disruptions appear to also influence attendance patterns.

January, coinciding with the presidential inauguration marked by the dashed vertical line, the gap between the two groups narrows sharply and remains compressed through the end of the school year. While U.S.-born absence rates follow their typical post-winter decline, foreign-born absence rates do not fall comparably, eliminating much of the pre-inauguration gap. We formalize our identification strategy motivated by this visual evidence in Section 4.

4 METHODS

Our objective is to estimate the causal effect of immigration enforcement actions on student attendance. To formalize, consider the following setup. Let Y_{it} denote the binary attendance status of student i on day t where $Y_{it} = 1$ indicates an absence. Let G_i be a binary indicator equal to one if a student i is a member of the treated group (born outside the U.S.) and zero otherwise. Let t^* denote the date at which the treated group is first exposed to treatment (January 20th, 2025). Finally, let $Y_{it}(1)$ and $Y_{it}(0)$ denote the potential attendance outcomes for student i on day t under treatment and in the absence of treatment, respectively. Our target estimand is the average treatment effect on the treated (ATT):

$$ATT = \mathbb{E}[Y_{it}(1) - Y_{it}(0) | G_i = 1] \quad \text{for all} \quad t \geq t^* \tag{1}$$

The quantity represented by (1) captures the average change in the probability of absence among foreign-born students attributable to the post-inauguration enforcement environment. Because attendance is binary, the ATT has a straightforward interpretation as a change in the probability of being absent. The fundamental challenge in recovering this estimand is that the counterfactual $Y_{it}(0)$ for treated students is unobserved. We can construct an estimator of the ATT if we assume that, absent treatment, attendance trends would have evolved similarly for treated and comparison students. This assumption is inherently untestable, but several pieces of evidence support its plausibility. First, Figure 1 shows that absence rates for foreign-born and U.S.-born students followed closely parallel seasonal patterns across the 2022-23 and 2023-24 school years, with both groups exhibiting similar month-to-month fluctuations around a stable level difference. The pre-inauguration months of the 2024-25 school year reproduce this pattern: through the fall, the two groups track each other with the same seasonal shape observed in prior years. The departure from this pattern begins precisely in January 2025, coinciding with the inauguration, and persists through the remainder of the school year.

A potential concern is that absence rates for the two groups appear to converge slightly during the fall of 2024-25, which could suggest a pre-existing trend toward convergence that would continue independent of treatment. However, a similar narrowing of the gap is visible in the fall months of both prior school years before the groups diverge again through the winter — this appears to be a recurring seasonal feature rather than a trend specific to the treatment year. We assess this assumption more formally in Section 5, where we present event study estimates that allow us to examine whether pre-inauguration trends in absences differed systematically between the two groups (Figure 2). Under this identifying assumption of continued parallel trends absent treatment, we can estimate treatment effects using the familiar difference-in-differences specification:

$$Y_{it} = \beta_0 + G_i\beta_1 + \mathbf{1}[t \geq t^*]\beta_2 + (G_i \cdot \mathbf{1}[t \geq t^*])\delta_{DiD}^{2\times2} + \epsilon_{it} \tag{2}$$

Here, β_1 captures baseline differences between groups (i.e., differences in pre-treatment levels) while β_2 captures common time effects. Under the parallel trend assumption, OLS estimation of $\delta_{DiD}^{2\times2}$ yields an unbiased and consistent estimate of the ATT. However, the panel structure of our data—with individual students observed repeatedly over several years—allows us to make considerable gains in efficiency through a two-way fixed effects (TWFE) estimator:[7]

$$Y_{it} = \gamma_i + \lambda_t + (G_i \cdot \mathbf{1}[t \geq t^*])\delta_{DiD}^{TWFE} + \nu_{it} \tag{3}$$

[7] An extensive recent literature has documented significant problems with the TWFE estimator in settings with staggered treatment (see Roth et al., 2023 for an overview of these issues). This is not an issue in our setting as all treatment occurs at the same time.

In (3), student fixed effects (γ_i) absorb each student's time-invariant propensity to be absent, generalizing β_1 from a single group-level intercept to student-specific baselines. This yields two benefits: it removes between-student variation that would otherwise contribute to residual variance and it controls nonparametrically for any stable student characteristics (observed and unobserved) that might differ across groups. Calendar day fixed effects (λ_t) replace the single post-period indicator with day-specific intercepts, flexibly absorbing common shocks (e.g., weather, holidays, school events) that affect attendance for all students on a given day. Treatment is indicated by the term $G_i \cdot \mathbf{1}[t \geq t^*]$ and equals one for students born outside the U.S. on and after January 20th, 2025 while the coefficient of interest (δ_{DiD}^{TWFE}) estimates the change in probability of absences attributable to post-inauguration immigration enforcement actions.

The estimates from (3) capture the average effect of IEAs across all post-inauguration days (N = 93[8]) for students born outside the U.S. This is informative for assessing the *overall* impact of immigration enforcement on attendance but does not distinguish between a sustained elevation in absences attributable to the broader enforcement climate and acute responses to specific enforcement events. To separate these channels, we extend (3) as:

$$Y_{it} = \gamma_i + \lambda_t + (G_i \cdot \mathbf{1}[t \geq t^*])\delta_{DiD}^{Persist} + \sum_{k=\underline{k}}^{\overline{k}} (G_i \cdot \mathbf{1}[k_{it} = k])\tau_k + \epsilon_{it} \tag{4}$$

Equation (4) retains the post-inauguration treatment indicator from (3) but adds interactions between the treatment group indicator G_i and a vector of relative-time indicators $\mathbf{1}[k_{it} = k]$, where k denotes the number of days between observation t and a documented IEA, bounded by $\{\underline{k}, \overline{k}\}$. The

[8] This count excludes February 3rd, 2025, on which a nationally organized "Day Without Immigrants" protest led to widespread absences among both foreign-born and U.S.-born students. Because this event affected attendance for both groups, its inclusion would attenuate our estimates by introducing a day on which the comparison group's attendance is not informative about counterfactual trends. In results available upon request, we confirm that our findings are not sensitive to this exclusion.

coefficient $\delta_{DiD}^{Persist}$ captures any persistent change in absence probability for treated students on post-inauguration days that are sufficiently distant from any enforcement event. The estimates of τ_k trace out the dynamic response to enforcement events in relative time: τ_0 captures the additional effect of an IEA on the day that it occurs, τ_1 the day after, and so on. This specification pools across all enforcement events, estimating a common relative-time profile rather than separate effects for each event. Note that the interpretation of $\delta_{DiD}^{Persist}$ differs from δ_{DiD}^{TWFE} in that the former is the effect on days far removed (i.e., outside the event window) while the latter averages across all post-treatment days. If specific enforcement events have acute impacts on student attendance, δ_{DiD}^{TWFE} will reflect a mixture of the persistent and acute channels, with the relative weight depending on the frequency and spacing of events in the post-treatment period.

Because multiple enforcement actions can occur in close temporal proximity, we group individual IEAs into episodes, defined as any sequence of enforcement activity in which consecutive events occur within two calendar days of one another. This clustering is necessary for the decomposition in Equation (4) because overlapping event windows from actions occurring in rapid succession would confound the acute response estimates. We select a two-day clustering window because it reflects the most common empirical pattern of enforcement activity in our data, where episodes typically span two consecutive days; results are substantively similar under a three-day window. Under this definition, a series of actions occurring on Monday, Tuesday, and Thursday of the same week would constitute a single episode. The relative-time variable k takes a value of zero on all days during an episode, negative values on the days preceding it, and positive values on the days following it. We define the event window as $k \in \{-2, +3\}$, with $k = -1$ as the omitted category; days falling outside this window are not assigned relative-time indicators and are instead captured by $\delta_{DiD}^{Persist}$. Across the three geographic definitions we consider (community, state, and ICE regions) this procedure yields 12, 11, and 16 episodes, respectively.

This decomposition allows us to assess both the magnitude and duration of acute responses to enforcement events. A pattern in which τ_0 is large but subsequent τ_k estimates attenuate quickly toward zero would indicate sharp, short-lived disruptions that do not persist beyond the day of the event. By comparing the frequency-weighted contributions of $\delta_{DiD}^{Persist}$ and the τ_k estimates to the overall effect from Equation (3), we can assess the degree to which the attendance impact is driven by acute responses to specific enforcement events versus a sustained shift in behavior across the post-inauguration period.

5 RESULTS

Before presenting our main estimates, we assess the credibility of the parallel trend assumption underlying our identification strategy. While this assumption is fundamentally untestable (i.e., as we do not observe counterfactual behavior of treated students in the absence of treatment) we can examine whether the two groups followed similar attendance trajectories prior to the onset of heightened enforcement. Systematic divergence in the pre-treatment period would cast doubt on the assumption that post-treatment divergence reflects a causal effect of IEAs rather than a continuation of pre-existing trends. Figure 1 provides strong evidence that this assumption is likely justified. However, in Figure 2 we formally examine empirical support for our assumption using an event-study analogue of Equation (3).

<< Figure 2 – Event Study Estimates of IEA Effects >>

Because our data are at the student-by-day level, daily estimates of the treatment-comparison differential are noisy. We therefore bin estimates by week to improve precision, plotting the average differential absence rate for foreign-born students in each week relative to inauguration, with the omitted category normalized to zero at the week immediately prior. The solid horizontal lines display the average of the weekly estimates in the pre- and post-inauguration periods, respectively; the vertical distance between them provides a visual analog to our TWFE estimate from Equation (3). If the parallel trends assumption holds, the pre-inauguration coefficients should be statistically indistinguishable from zero and exhibit no systematic trend.

The pre-period estimates are consistent with this expectation. Across the twenty weeks preceding inauguration, only four are statistically distinguishable from zero. We note that two of these four are in the weeks immediately following the 2024 election, suggesting potential anticipatory effects which we explore in Appendix B. The average point estimate during the pre-inauguration period approaches zero, though is slightly elevated. The bias introduced by these positive and statistically insignificant point estimates in pre-treatment periods, if any, would push our estimates towards zero. Beginning at inauguration, the weekly estimates shift upward and remain persistently elevated through the end of the school year, with no indication of attenuation over time. The gap between the pre- and post-inauguration averages—visible in the distance between the two horizontal lines—foreshadows the formal estimates we present in Table 2.

<< Table 2 – Effect of Immigration Enforcement on Student Absences >>

Column 1 of Table 2 reports the overall effect from Equation (3), which captures the average change in absence probability for foreign-born students across all post-inauguration days. The estimated effect is 2.2 percentage points (SE = 0.003, $p < 0.01$), representing a 37% increase relative to the pre-treatment absence rate of 5.9%. In concrete terms, this implies that a students born outside the U.S. would have been absent two additional days during the 93 school days following the inauguration on January 20th, 2025.

Columns 2 through 4 of Table 2 decompose this overall effect using Equation (4), which separates the persistent post-inauguration shift from acute responses on days proximate to specific enforcement events. Because the relevant geography over which families perceive and respond to enforcement activity is unknown, we estimate this decomposition under three definitions of IEA exposure: events documented within the Liberty City community (Column 2), events occurring anywhere in the state (Column 3), and events within the broader ICE region of responsibility (Column 4). These definitions yield 12, 11, and 16 IEA episodes, respectively, where episodes are defined as enforcement activity occurring within two calendar days of each other.

The persistent effect first reported in column 1 is remarkably stable across all three geographic definitions, ranging from 2.1 to 2.3 percentage points and statistically significant at the 99% confidence level in each estimation. This stability is expected because the persistent component captures the baseline elevation in absences on post-inauguration days that fall outside any event window and its magnitude should be largely invariant to which set of events defines that window. The acute effects are more variable across definitions and generally modest in magnitude. Community-level events generate a marginally significant 0.6 percentage point spike on the day of the event but no significant effects the day before or after. State-level events produce no same-day effect but a significant 1.1 percentage point increase the day after, potentially reflecting the time required for news of more distant events to reach families.

Regional events show no detectable acute effects at any lag, consistent with events at this geographic remove being too diffuse to generate a concentrated attendance response. We note, however, that with only 6 to 11 episodes per geographic definition, we have limited statistical power to detect acute effects, and the variation in point estimates across columns may partly reflect imprecision rather than meaningful differences in how families respond to events at different distances. Importantly, the pre-event placebo estimates (two days before an IEA) are small and insignificant across all three definitions, providing reassurance that the acute effects are not driven by pre-existing trends around event dates. Taken together, these results indicate that the dominant channel is the persistent shift in the post-inauguration enforcement climate rather than acute responses to specific events — though local enforcement actions do generate detectable, short-lived disruptions.

We next turn to investigate heterogeneity in the treatment effect by grade level, which speaks to the mechanism through which enforcement affects attendance. For younger students, the decision to miss school is almost entirely a parental one; older students exercise considerably more individual discretion. If IEAs operate partly through students' own perceptions of enforcement risk — rather than solely through parental decisions — we would expect effects to increase with grade level. We test this by interacting the treatment indicator in Equation (3) with indicators for each grade, estimating separate effects for each grade level. Figure 3 presents these results.

<< Figure 3 – Effects on IEAs on Absences by Grade Level >>

The pattern shown in Figure 3 is striking. Point estimates increase nearly monotonically with grade level, from small and statistically insignificant effects in Pre-K through 3rd grade to large and statistically significant effects in the upper grades. Among elementary-age students (Pre-K through 5th grade), the estimates cluster between a 0 and 2 percentage points increase in students' probability of being absent, with only 5th grade reaching statistical significance at conventional confidence levels. A visible jump occurs at the transition to middle school—effects for 7th and 8th graders are roughly 2 percentage points and statistically significant. The largest effects appear among high school students, where point estimates range from approximately 3 percentage points (12th grade) to nearly 6 percentage points (11th grade), though confidence intervals widen considerably due to smaller grade-level sample sizes. The 6th grade estimate is a notable exception to the monotonic pattern — small and imprecisely estimated — which may reflect the transitional nature of that grade in this district's school configuration.

This gradient is difficult to reconcile with a story in which the attendance effects are driven entirely by parental decisions, since parents of younger children presumably face the same enforcement environment as parents of older students. Two complementary mechanisms could generate this pattern. First, older students are more aware of enforcement activity, more likely to consume news and social media, and more able to act on their own attendance decisions — suggesting that student agency plays a role beyond household-level responses. Second, older foreign-born students in our sample have potentially spent a larger share of their lives outside the United States and may be more likely to lack secure immigration status themselves, meaning the grade-level gradient could partly reflect increasing direct enforcement risk rather than (or in addition to) increasing autonomy. We cannot cleanly separate these channels with our data, but both point to the same substantive conclusion: the aggregate estimates from Equation (3) mask substantial variation in how different age groups experience the enforcement environment, with the burden concentrated among older students for whom enforcement is most salient — whether through personal risk, individual awareness, or both.

6 EXTENSIONS

The results presented in Section 5 establish that immigration enforcement actions cause a persistent increase in absences among foreign-born students, with additional acute effects concentrated on the day of local enforcement events. In this section, we probe these findings along two dimensions. First, we test whether IEAs also disrupt instructional time through early dismissals — a margin not captured by our primary analysis of full-day absences. Next, we assess the sensitivity of our estimates to alternative definitions of the treated group, which allows us to benchmark our findings against prior work and provides additional evidence on the role of measurement error in treatment classification.

6.1 Effects on Early Dismissals

Our primary analysis, reported in Table 2, captures whether a student is absent for the full school day. However, enforcement actions may also affect students who are already at the school when an IEA occurs or when news of the event circulates. In these cases, the relevant margin is not whether a student attends school but whether they leave early (e.g., if parents withdraw their student mid-day after an IEA occurs nearby). If IEAs generate this type of response, our estimates focused on absences would understate the total disruption to instructional time.

We test for these effects on early dismissals by replacing the binary absence indicator in Equations (3) and (4) with an indicator for whether a student was dismissed early for any reason other than disciplinary incidents. Because early dismissals are relatively rare events, we have less statistical power for this analysis, and null results should be interpreted with caution. Nevertheless, the timing of early dismissals is informative about the mechanism by which IEAs impact students—if enforcement events generate acute effects on early dismissals but not on next-day absences, this would suggest that families respond in real time to information about enforcement activity but that the disruption does not carry over into subsequent attendance decisions. We report the results of this analysis in Table 3.

<< Table 3 – Effect of Immigration Enforcement on Student Early Dismissals >>

Table 3 presents a pattern that, at first glance, appears counterintuitive. The overall effect (Column 1) indicates that foreign-born students are 0.1 percentage points less likely to be dismissed early in the post-inauguration period, an estimate that is statistically significant but negative—the opposite of what we would expect if families were pulling students out of school in response to the enforcement environment. Relative to the pre-treatment mean of 0.2%, this represents a roughly 50% decline in early dismissals, though the absolute magnitude is small. One possible interpretation is that families who keep their children home on high-anxiety days are precisely those who would otherwise have dismissed them early — in effect, the absence margin substitutes for the early dismissal margin. Students who do attend school in the post-inauguration period may be drawn from families less inclined to disrupt the school day, mechanically reducing the early dismissal rate among those who show up. We note that because early dismissals are observed only for students who attend school, these estimates are conditional on a post-treatment outcome and should be interpreted descriptively rather than as causal effects comparable to those in Table 2.

The decomposition in Columns 2 through 4 reinforces this interpretation. None of the acute event-day estimates are statistically significant at conventional levels, and the point estimates are uniformly small. There is no evidence that specific enforcement events trigger a wave of mid-day pickups. Combined with the negative persistent effect, these results suggest that the attendance disruption from IEAs operates almost entirely on the extensive margin (i.e., whether a student attends at all) rather than the intensive margin (i.e., whether a student who attends completes the full day). The absence estimates in Table 2 thus likely capture the primary channel through which enforcement affects instructional time.

6.2 Alternative Treatment Definitions

Our preferred treatment definition relies on student birthplace, which we argue in Section 3 is the most direct available proxy for vulnerability to immigration enforcement actions. However, recent studies of IEA effects on attendance have relied on alternative proxies including MLL status, home language, and composite immigrant indicators. Differences in estimated effect sizes across studies could reflect either genuine differences in contexts or differences in how well each proxy captures the affected population.

If measurement error in the treatment group drives differences in estimated magnitudes, we would expect estimates to be largest under the birthplace definition—which conditions most directly on a necessary condition for deportation—and attenuate under definitions that introduce additional noise. We speak directly to this question by re-estimating Equation (3) using each of these alternative proxies as the treatment indicator and report our results in Table 4.

<< Table 4 – Estimates of Overall Effects Under Alternative Treatment Definitions >>

The pattern we observe from these analyses is consistent with attenuation due to measurement error. Our preferred birthplace definition yields the largest estimate at 2.2 percentage points (a 37% increase relative to the pre-treatment mean of 5.9%). Estimates attenuate monotonically as the treatment definition becomes a noisier proxy for enforcement vulnerability: MLL status produces an estimate of 1.7 percentage points (27%), non-English home language 1.5 percentage points (23%), current MLL status 1.4 percentage points (22%), and the recent immigrant indicator 1.1 percentage points (20%). All estimates are statistically significant at the 99% confidence level.

This pattern is intuitive; the MLL and non-English home language treatment definitions cast the widest nets, classifying many U.S.-born students with no direct exposure to enforcement risk as treated — diluting the estimated effect toward zero. Current MLL status is somewhat more restrictive but still includes native-born children of English-speaking households who entered language services for other reasons. The recent immigrant indicator is the most restrictive proxy but captures only students enrolled in U.S. schools for fewer than three years, missing long-tenured foreign-born students who may still face enforcement risk. Birthplace, by contrast, conditions directly on a necessary condition for deportability without imposing arbitrary duration restrictions. The fact that estimates are largest under this definition strongly suggests that differences in estimated magnitudes across studies reflect treatment group measurement rather than contextual variation. As a result, researchers relying on proxy definitions that are standard in administrative data should interpret their estimates as lower bounds on the effects experienced by the most directly affected students.

6.3 Spillover effects on U.S.-born students

A potential limitation with results from our primary specification is that U.S.-born students who share demographic characteristics with foreign-born peers (e.g., students in mixed-status households) may themselves be affected by immigration enforcement. If so, these students are not truly "untreated," and their inclusion in the reference group would attenuate our main estimates. To investigate this possibility, we augment our primary specification represented by Equation (3) with a parallel interaction term identifying U.S.-born students who belong to plausibly vulnerable subgroups: MLL students, students with a non-English home language (NEHL), Hispanic students, and any non-white student. Each group is examined in a separate regression to avoid collinearity between the spillover indicator and the foreign-born treatment variable.

<< Table 5 – Spillover Effects on U.S.-Born Students >>

Results are reported in Table 5. Across all four specifications, the spillover coefficients are small in magnitude and statistically insignificant, suggesting no detectable effect of immigration enforcement on the attendance of U.S.-born students in these subgroups. The main treatment effect for foreign-born students remains stable at a 0.023-0.025 percentage point reduction in the probably of being absent, consistent with the removal of marginally affected students from the comparison group. These results suggest that our primary estimates are not meaningfully attenuated by contamination of the control group. We note, however, that the null spillover estimates should be interpreted with caution as demographic characteristics may be imprecise proxies for actual household-level immigration exposure. Our analysis may therefore lack sufficient power to detect modest spillover effects among U.S.-born students. As such, these results do not rule out the possibility of community-level effects in our setting but rather suggest that any such effects are not large enough to meaningfully bias our main estimates.

7 CONCLUSION

In some ways, Liberty City is an ideal place to be a migrant. The community has a welcoming context of reception, district officials have adopted policies to protect migrant students, and city and school bureaucrats increasingly reflect the cultures and origins of the migrants. As such, the effects that we have identified are likely conservative estimates of the negative impacts on vulnerable students. We estimate the causal effect of immigration enforcement actions on student attendance using daily administrative records from a single school district. We find that the onset of heightened enforcement following the January 2025 presidential inauguration increased absences among foreign-born students by 2.2 percentage points — a 37% increase relative to pre-treatment levels — with effects persisting throughout the remainder of the school year.

Our analysis offers three contributions to the emerging literature on immigration enforcement and education. First, we leverage student birthplace records to identify students born outside the United States as the treated group, rather than relying on proxy measures such as multilingual learner status or home language that are standard in administrative data. We show that these proxies produce monotonically attenuating estimates, consistent with classical measurement error from misclassification of the treated population. This finding suggests that existing estimates based on proxy definitions likely understate the effects experienced by the most directly affected students.

Second, we decompose the overall treatment effect into a persistent shift in baseline absence rates and acute responses to specific enforcement events. The persistent component dominates: foreign-born students are approximately 2 percentage points more likely to be absent on any post-inauguration school day, regardless of proximity to a specific enforcement event. Acute responses to local enforcement actions are detectable but modest and short-lived. This decomposition matters because the two channels carry different policy implications — persistent effects reflect a sustained disruption to human capital accumulation that compounds over time, while acute effects represent episodic disruptions that, in isolation, would be less consequential for long-run outcomes.

Third, we document a steep grade-level gradient in treatment effects, with estimates increasing nearly monotonically from negligible effects in the early elementary grades to effects of 3 to 6 percentage points among high school students. This pattern is consistent with older students exercising greater individual agency over their attendance decisions and potentially facing more direct personal enforcement risk, and it is difficult to reconcile with models in which the attendance response is driven entirely by parental decisions at the household level. The concentration of effects among older students is particularly concerning given that absences in the upper grades carry higher marginal costs for academic outcomes, including course completion, credit accumulation, and on-time graduation.

Our findings carry practical implications for schools operating in enforcement-active environments. The persistence of attendance effects, and their concentration among older students, suggest that effective district responses require sustained engagement rather than mobilization only in the immediate aftermath of specific enforcement events. The breadth of our estimated effects underscores that awareness of immigration enforcement impacts should extend beyond multilingual and immigrant-origin staff to all educators and school personnel. The persistent nature of the attendance decline suggests that proactive communication with immigrant families (e.g., district policies on immigration policing, emergency care planning, and safe travel to and from school) may help mitigate the uncertainty that appears to drive sustained disengagements. Partnerships with community-based organizations can further ensure that families are informed of their rights and that students maintain access to essential services during periods of heightened enforcement. More broadly, the magnitude of our estimates in a district with a comparatively welcoming context of reception suggests that effects in less supportive environments may be substantially larger, a possibility that merits attention from both researchers and policymakers.

References

Alsan, M., & Yang, C. S. (2024). Fear and the safety net: Evidence from Secure Communities. *Review of Economics and Statistics*, *106*(6), 1427–1441. https://doi.org/10.1162/rest_a_01250

Avila, S. (2024). The effect of workplace raids on academic performance: Evidence from Texas. *Sociological Science*, *11*, 258–296. https://doi.org/10.15195/v11.a11

Bellows, L. (2019). Immigration enforcement and student achievement in the wake of Secure Communities. *AERA Open*, *5*(4), 2332858419884891. https://doi.org/10.1177/2332858419884891

Bellows, L. (2021). The effect of immigration enforcement on school engagement: Evidence from 287(g) programs in North Carolina. *AERA Open*, *7*, 23328584211039467. https://doi.org/10.1177/23328584211039467

Bennett, C., Graves, V., & Meadows, B. (2025). ICE at the door, tests on the floor: Student achievement and local immigration enforcement. *RSF: The Russell Sage Foundation Journal of the Social Sciences*, *11*(4), 104–122. https://doi.org/10.7758/RSF.2025.11.4.05

Burdick-Will, J. (2016). Neighborhood violent crime and academic growth in Chicago: Lasting effects of early exposure. *Social Forces*, *95*(1), 133–158. https://doi.org/10.1093/sf/sow041

Card, D. (1999). The causal effect of education on earnings. In *Handbook of Labor Economics* (Vol. 3, pp. 1801–1863). Elsevier. https://doi.org/10.1016/S1573-4463(99)03011-4

Dee, T. S. (2004). Are there civic returns to education? *Journal of Public Economics*, *88*(9–10), 1697–1720. https://doi.org/10.1016/j.jpubeco.2003.11.002

Dee, T. S. (2025). *Recent immigration raids increased student absences*. https://doi.org/10.26300/A62E-H526

Figlio, D., & Özek, U. (2025). *The effects of immigration enforcement on student outcomes in a new era of immigration policy in the United States* (No. W34452; p. w34452). National Bureau of Economic Research. https://doi.org/10.3386/w34452

Golash-Boza, T., & Valdez, Z. (2018). Nested contexts of reception: Undocumented students at the University of California, Central. *Sociological Perspectives*, *61*(4), 535–552. https://doi.org/10.1177/0731121417743728

Heinrich, C., Hernández, M., & Shero, M. (2023). Repercussions of a raid: Health and education outcomes of children entangled in immigration enforcement. *Journal of Policy Analysis and Management*, *42*(2), 350–392. https://doi.org/10.1002/pam.22443

Hiltzik, M. (2025, January 22). Inside the Bakersfield raids that showed how Trump's immigration policies will sow chaos. *Los Angeles Times*. https://www.latimes.com/business/story/2025-01-22/column-inside-the-bakersfield-raids-that-showed-how-trumps-immigration-policies-will-sow-chaos

James, S., Gold, S., Rouhani, S., McLanahan, S., & Brooks-Gunn, J. (2021). Adolescent exposure to deadly gun violence within 500 meters of home or school: Ethnoracial and income disparities: study examines adolescent exposure to deadly gun violence near home or school. *Health Affairs*, *40*(6), 961–969. https://doi.org/10.1377/hlthaff.2020.02295

Kirksey, J. J. (2025). Weeks after the raid: The immediate and sustained changes in student attendance rates following immigration arrests. *Educational Evaluation and Policy Analysis*, *47*(4), 1219–1244. https://doi.org/10.3102/01623737241288838

Kirksey, J. J., & Sattin-Bajaj, C. (2021). Immigration arrests and educational impacts: Linking ICE arrests to declines in achievement, attendance, and school climate and safety in California. *AERA Open*, *7*, 23328584211039787. https://doi.org/10.1177/23328584211039787

Kirksey, J. J., Sattin-Bajaj, C., Gottfried, M. A., Freeman, J., & Ozuna, C. S. (2020). Deportations near the schoolyard: Examining immigration enforcement and racial/ethnic gaps in educational outcomes. *AERA Open*, *6*(1), 2332858419899074. https://doi.org/10.1177/2332858419899074

Lowenhaupt, R., González, P. A., Queenan, J. (Jenna), Tesfa, E., & Dabach, D. B. (2025). District leadership practices in disrupted contexts of reception: Supporting immigrant communities in times of crisis. *American Journal of Education*, *131*(4), 499–535. https://doi.org/10.1086/736180

Murillo, M. A., Quartz, K. H., Garcia, L. W., & Liboon, C. A. (2021). Nested contexts of reception and K–12 schools: Addressing immigration status. *AERA Open*, *7*, 23328584211056344. https://doi.org/10.1177/23328584211056344

Plyler v. Doe, 457 U.S. 202 (Supreme Court of the United States 1982). https://tile.loc.gov/storage-services/service/ll/usrep/usrep457/usrep457202/usrep457202.pdf

Rodrigues, M. (2025, December 10). *Kids missed school across the country because of fears of ICE activity, school principals say in survey*. https://www.bostonglobe.com/2025/12/10/metro/immigrants-miss-school-fear-ice-poll/

Rodriguez, S., Roth, B. J., & Villarreal Sosa, L. (2022). “Immigration enforcement is a daily part of our students’ lives”: School social workers’ perceptions of racialized nested contexts of reception for immigrant students. *AERA Open*, *8*, 23328584211073170. https://doi.org/10.1177/23328584211073170

Roth, J., Sant’Anna, P. H. C., Bilinski, A., & Poe, J. (2023). What’s trending in difference-in-differences? A synthesis of the recent econometrics literature. *Journal of Econometrics*, *235*(2), 2218–2244. https://doi.org/10.1016/j.jeconom.2023.03.008

Simonsohn, U., Simmons, J. P., & Nelson, L. D. (2020). Specification curve analysis. *Nature Human Behaviour*, *4*(11), 1208–1214. https://doi.org/10.1038/s41562-020-0912-z

Slungaard Mumma, K. (2025). *The effect of the second Trump administration and the attendance of immigrant-origin students*. https://doi.org/10.26300/VXTN-R577

Thompson, K. D., Umansky, I. M., & Porter, L. (2020). Examining contexts of reception for newcomer students. *Leadership and Policy in Schools*, *19*(1), 10–35. https://doi.org/10.1080/15700763.2020.1712732

Trump, D. J. (2025, January 20). *Executive order 14159—Protecting the American people against invasion*. https://www.govinfo.gov/app/details/DCPD-202500126

Weber, R. (2022). Apprehension and educational outcomes among hispanic students in the United States: The impact of Secure Communities. *PLOS ONE*, *17*(10), e0276636. https://doi.org/10.1371/journal.pone.0276636

Tables

Table 1 – Student Characteristics by Birthplace and School Year

	2023-24 School Year			*2024-25 School Year*		
	Born Inside U.S.	**Born Outside U.S.**	**Difference**	**Born Inside U.S.**	**Born Outside U.S.**	**Difference**
Demographics						
Black	0.174	0.134	-0.039**	0.178	0.14	-0.038**
Hispanic	0.533	0.482	-0.051**	0.550	0.556	0.006
White	0.191	0.230	0.039*	0.172	0.189	0.016
Other Race/Ethnicity	0.102	0.153	0.051**	0.100	0.116	0.015
Ever MLL	0.312	0.941	0.628**	0.299	0.937	0.638**
Non-English Home Language	0.476	0.991	0.514**	0.478	0.992	0.515**
Recent Immigrant	0.001	0.636	0.635**	0.000	0.585	0.585**
Title 1 Student	0.882	0.979	0.097**	0.836	0.963	0.127**
Unhoused Student	0.092	0.034	-0.058**	0.075	0.028	-0.047**
Attendance						
Present	0.793	0.829	0.036**	0.795	0.810	0.015+
Tardy	0.092	0.08	-0.012*	0.105	0.095	-0.010
Early Dismissal	0.002	0.002	0.000	0.003	0.002	-0.001**
Absent	0.113	0.089	-0.024**	0.097	0.093	-0.004

*Notes: Significance of difference-in-means determined using Welch's t-test. + - $p < 0.1$; * - $p < 0.05$, ** - $p < 0.01$*

Table 2 – Effect of Immigration Enforcement on Student Absences

		Event Decomposition		
	Overall	**Community**	**State**	**Region**
Born Outside US x Post	0.022**	0.020**	0.021**	0.023**
	(0.003)	(0.003)	(0.003)	(0.003)
IEA -2 Days		0.001	0.001	-0.012
		(0.005)	(0.006)	(0.009)
Day of IEA		0.006*	-0.003	-0.003
		(0.003)	(0.003)	(0.003)
IEA +1 Days		0.006	0.011**	-0.006
		(0.004)	(0.004)	(0.007)
N Student-Days	1,362,221	1,362,221	1,362,221	1,362,221
N Students	4,296	4,296	4,296	4,296
N IEA Episodes	-	12	11	16
Pre-Treatment Mean	0.059	0.059	0.059	0.059
Student FEs	Y	Y	Y	Y
Calendar Day FEs	Y	Y	Y	Y

*Notes: Standard errors clustered at the student-level. IEA episodes are defined as occurring within two calendar days of each other. + - p < 0.1; * - p < 0.05, ** - p < 0.01*

Table 3 - Effect of Immigration Enforcement on Student Early Dismissals

		Event Decomposition		
	Overall	**Community**	**State**	**Region**
Born Outside US x Post	-0.001*	-0.001+	-0.001+	-0.001*
	(0.000)	(0.000)	(0.000)	(0.000)
IEA -2 Days		0.002	-0.002	-0.002
		(0.001)	(0.001)	(0.002)
Day of IEA		-0.001	0.000	0.000
		(0.001)	(0.001)	(0.001)
IEA +1 Days		0.000	0.000	0.000
		(0.001)	(0.001)	(0.001)
N Student-Days	1,362,221	1,362,221	1,362,221	1,362,221
N Students	4,296	4,296	4,296	4,296
N IEA Episodes	-	12	11	16
Pre-Treatment Mean	0.002	0.002	0.002	0.002
Student FEs	Y	Y	Y	Y
Calendar Day FEs	Y	Y	Y	Y

*Notes: Standard errors clustered at the student-level. IEA episodes are defined as occurring within two calendar days of each other. + - $p < 0.1$; * - $p < 0.05$, ** - $p < 0.01$*

Table 4 – Estimates of Overall Effects Under Alternative Treatment Definitions

	Born Outside U.S.	Current MLL	Non-English Home Language	Recent Immigrant
Treated x Post	0.022**	0.016**	0.015**	0.011**
	(0.003)	(0.003)	(0.004)	(0.003)
N Student-Days	1,362,221	1,362,221	1,362,221	1,362,221
N Students	4,296	4,296	4,296	4,296
Pre-Treatment Mean	0.059	0.064	0.064	0.055
Student FEs	Y	Y	Y	Y
Calendar Day FEs	Y	Y	Y	Y

*Notes: Standard errors clustered at the student-level. + - $p < 0.1$; * - $p < 0.05$, ** - $p < 0.01$*

Table 5 – Spillover Effects on U.S.-Born Students

	(A)	(B)	(C)	(D)
Born Outside US x Post	0.023**	0.023**	0.023**	0.025**
	(0.004)	(0.004)	(0.004)	(0.006)
U.S.-Born x MLL x Post	0.003			
	(0.004)			
U.S.-Born x NEHL x Post		0.004		
		(0.004)		
U.S.-Born x Hispanic x Post			0.002	
			(0.004)	
U.S. Born x non-white x Post				0.004
				(0.006)
N Student-Days	1,362,221	1,362,221	1,362,221	1,362,221
N Students	4,296	4,296	4,296	4,296
Pre-Treatment Mean (Treated)	0.059	0.059	0.059	0.059
Pre-Treatment Mean (Spillover)	0.071	0.085	0.081	0.081
Student FEs	Y	Y	Y	Y
Calendar Day FEs	Y	Y	Y	Y

*Notes: Standard errors clustered at the student-level. + - p < 0.1; * - p < 0.05, ** - p < 0.01*

FIGURES

Figure 1 – Average Absence Rate by Student Birthplace and School Year

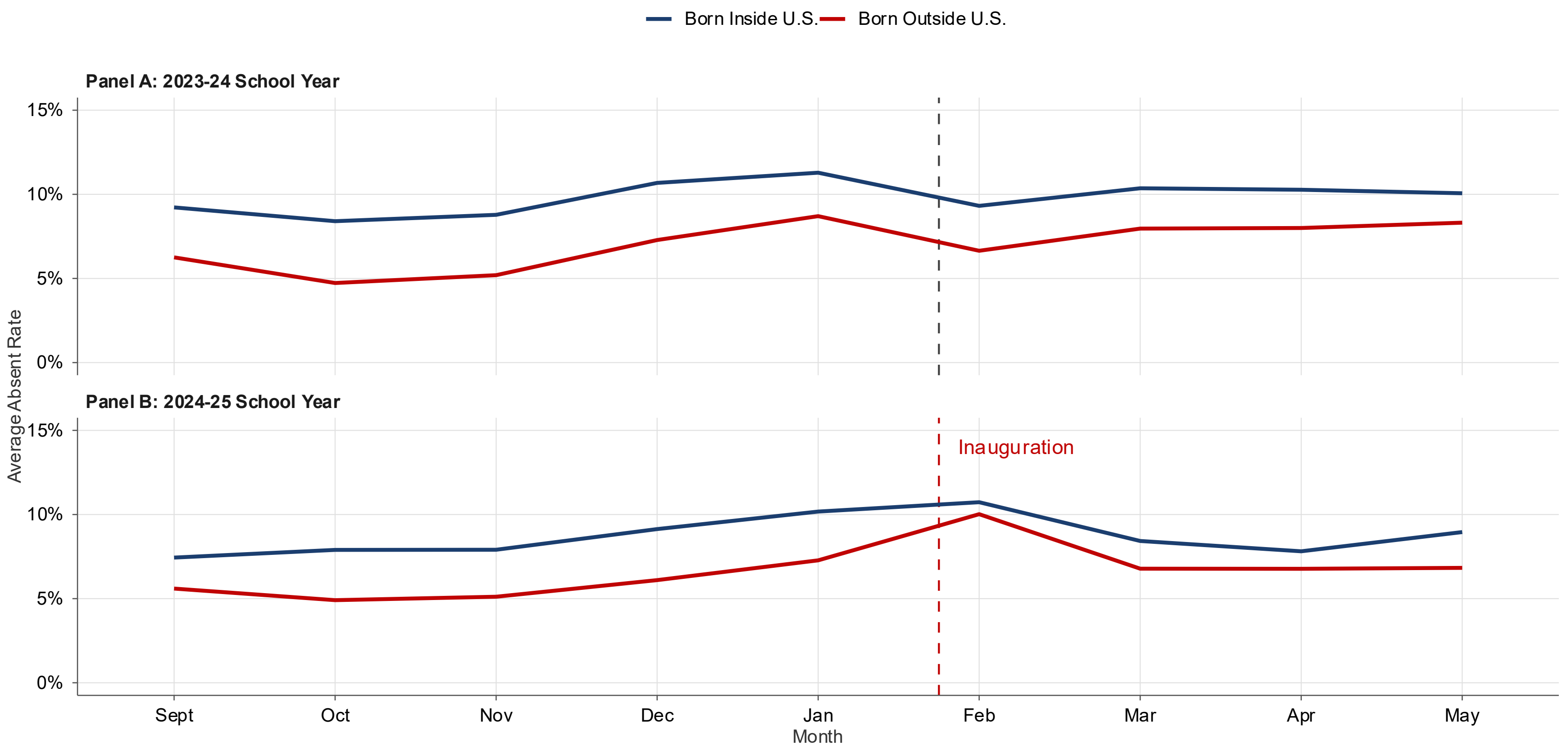
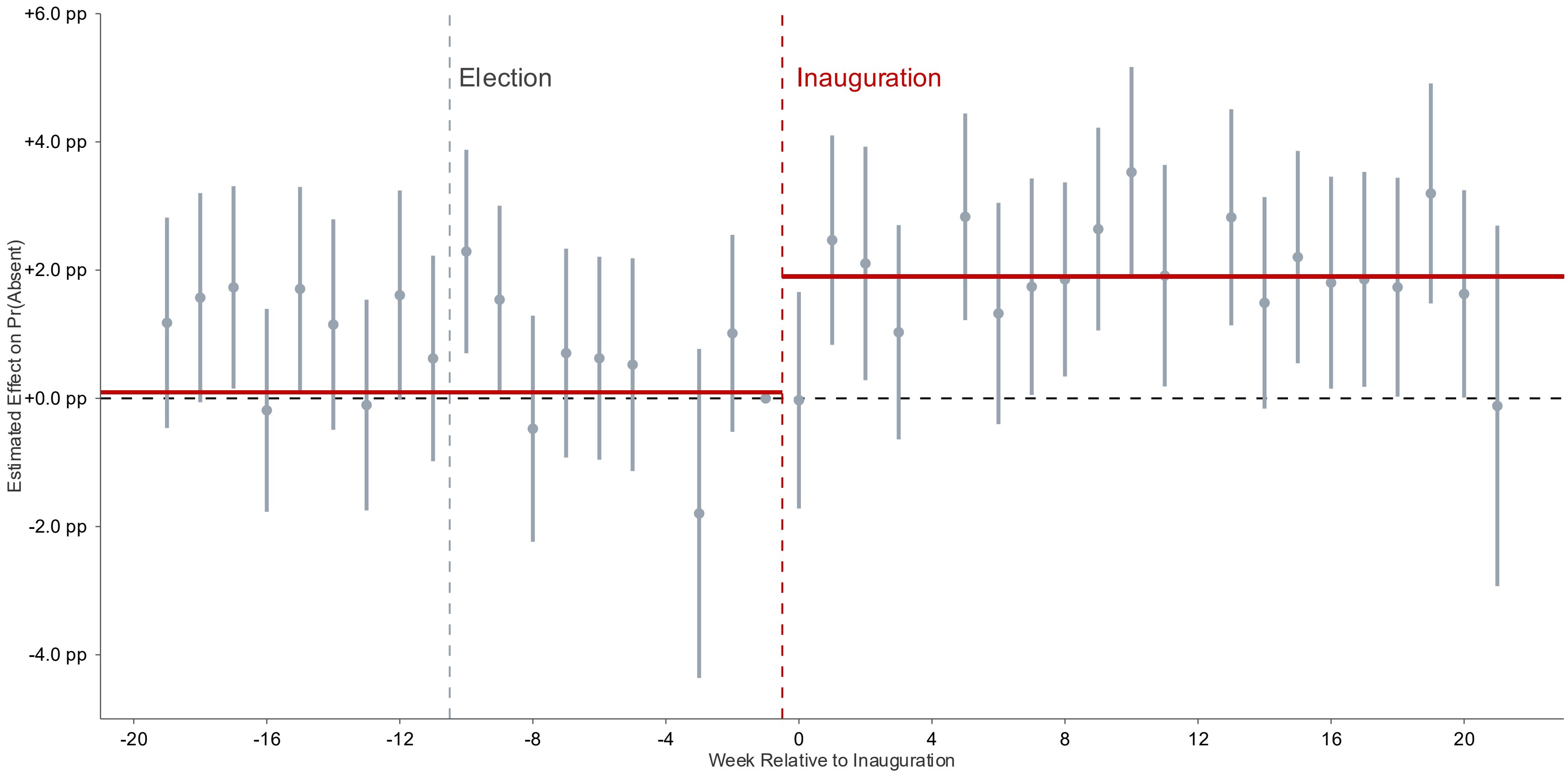
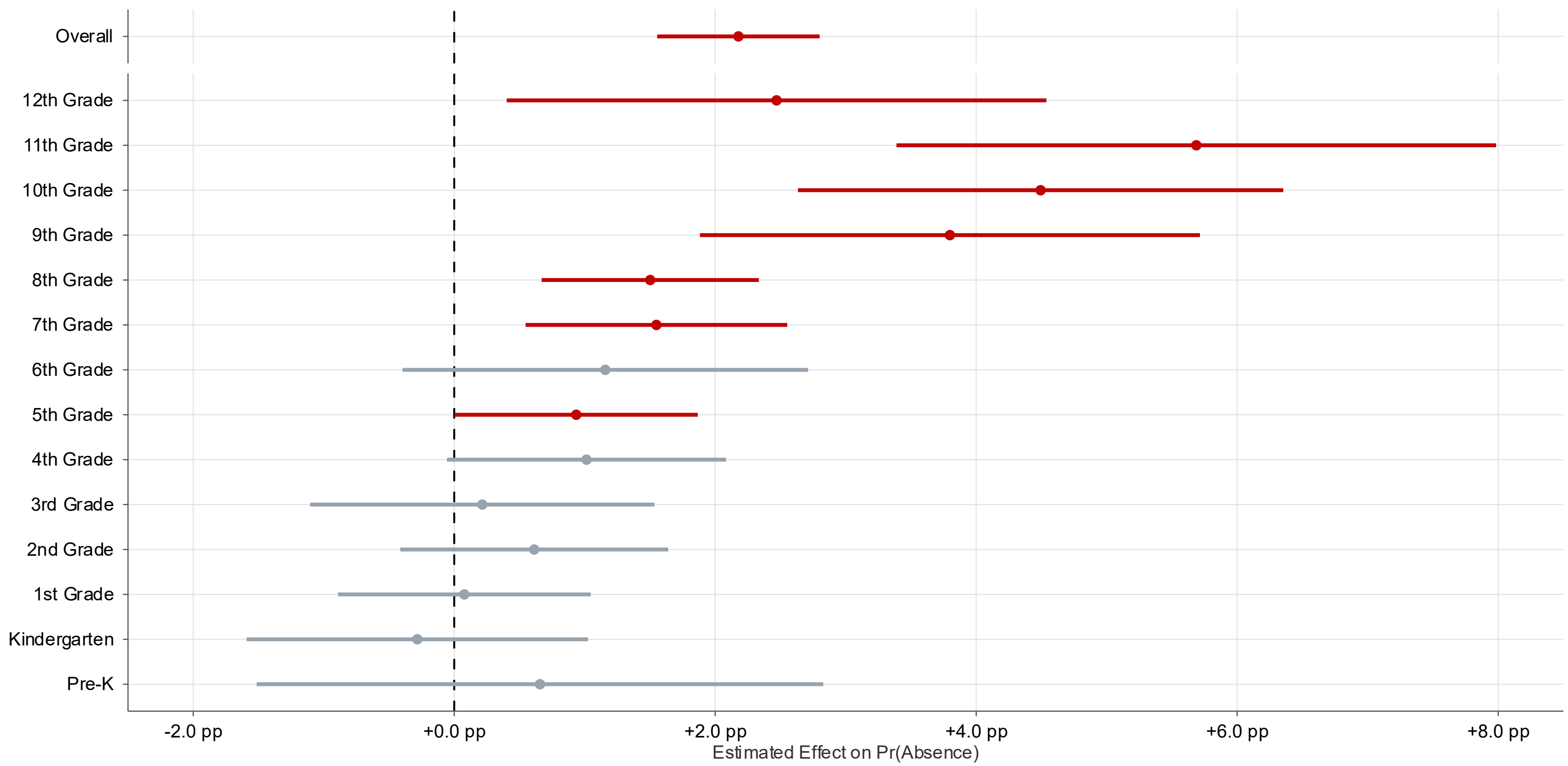


Figure 2 – Event Study Estimates of IEA Effects



Note: Estimates are from an event study specification estimated over the full sample (2022-23 through 2024-25 school years). For visual clarity, only coefficients from the 2024-25 school year are displayed. Horizontal lines indicate the average of pre- and post-inauguration coefficients, respectively, where the pre-inauguration average is calculated over the full pre-treatment period.

Figure 3 – Effects on IEAs on Absences by Grade Level



APPENDIX A: SAMPLE COMPOSITION & STUDENT DISENROLLMENTS

One potential concern with the main estimates we present is that immigration enforcement activity may cause some vulnerable students to disenroll from the district entirely rather than simply be absent. If this is the case, the attendance effects reported in Table 2 would represent a lower bound on the true impact of enforcement, as the most severely affected students would exit the analytic sample and no longer contribute to the estimation of treatment effects. Additionally, differential attrition by students born outside the U.S. would change the composition of the remaining sample in ways that may bias our primary estimates.

$$D_{it} = \alpha_0 + G_i\alpha_1 + (G_i \cdot \mathbf{1}[t \geq 2024])\alpha_2 + \epsilon_{it} \qquad (1)$$

To evaluate this possibility, we estimate a simple linear probability model of student disenrollment at the student-by-school-year level. The outcome, D_{it}, is a binary indicator equal to one if a student exited enrollment before the end of the school year. G_i is a binary indicator for students born outside the United States, meaning that α_1 represents the probability of students born outside the U.S. exiting before the end of the school year (as compared to students born inside the U.S.) in the 2022-23 and 2023-24 school years. Estimates of α_2 represent a change in the probability of vulnerable student disenrollment during the 2024-25 school year. We report the results of this regression in Table A.1.

Table A.1 – Disenrollment Analysis

	Disenroll
Born Outside U.S.	-0.032**
	(0.009)
Born Outside U.S. x Post	-0.019+
	(0.011)
Constant	0.129**
	(0.004)
Observations	8,205

*Note: + - p < 0.1; * - p < 0.05, ** - p < 0.01*

Estimates reported in Table A.1 suggest that differential disenrollment does not explain the attendance effects documented in our main analysis. The coefficient on foreign-born status indicates that, prior to the 2024-25 school year, students born outside the U.S. were significantly less likely to disenroll before the end of the school year than their U.S.-born peers. The interaction term is negative and marginally significant, indicating that, if anything, this gap widened during the period of heightened enforcement activity. This pattern is inconsistent with the concern that enforcement drove vulnerable students out of the district and suggests instead that the students experiencing attendance disruptions remain enrolled throughout the school year. Our main estimates are therefore unlikely to be downward-biased by selective attrition from the analytic sample.

APPENDIX B: SENSITIVITY TO CHOICE OF TREATMENT PERIOD

Our main specification defines the treatment period as beginning January 20, 2025. As discussed in Section 3, this choice reflects a tradeoff between capturing the full scope of the post-inauguration enforcement regime and avoiding contamination from untreated days. To assess whether our findings are sensitive to this decision, we conduct a specification curve analysis in which we vary the treatment start date across a range of plausible alternatives, from November 1st, 2024 through January 31st, 2025. Each date yields a separate estimate of δ_{DiD}^{TWFE} as defined in Equation (3), and we plot the resulting distribution of point estimates and confidence intervals in Figure B.1.

The results confirm that our main findings are not driven by the choice of treatment onset. Across 86 specifications, the point estimates range from a 0.018 to 0.023 increase in the probability of student born outside the U.S. being absent, with all estimates statistically significant at conventional levels. Estimates are largest when the start date is set to January 25th, 2025. consistent with earlier dates diluting the estimate with untreated days. This result is intuitive in that federal immigration policy did not formally change until after the inauguration. The narrow range of estimates across plausible start dates indicates that the attendance effects we document reflect a broad shift in behavior following the inauguration rather than a response concentrated on a single date that could be captured or missed depending on coding decision. Additionally, these results indicate that anticipatory results, which we cannot conclusively rule out, do not systematically bias our findings.

Figure B.1 – Specification Curve Analysis for Sensitivity to Treatment Start Date