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IMMIGRATION ENFORCEMENT POLICY AND STUDENT ATTENDANCE: PERSISTENT AND ACUTE EFFECTS

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ABSTRACT

How do changes in immigration policy affect student attendance? We use student-by-day administrative records from a mid-sized 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 heightened enforcement and immigration enforcement actions (IEAs) cause a substantial and persistent increase in absences among foreign-born students, with the daily probability of absence rising by 2.2-2.4 percentage points (33-36%) relative to a pre-treatment mean of 6.7 percent. These effects reflect both a persistent elevation in absences spanning the full post-treatment period and acute, short-lived spikes on dates proximate to specific enforcement events. The persistent elevation accounts for most of the overall effect and shows little 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 find no evidence of spillover effects onto U.S.-born students, including those in mixed-nativity households, though limited statistical power cannot rule out spillovers up to roughly half the magnitude of the main effect. 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

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1 INTRODUCTION

In 1982, the U.S. Supreme Court issued the *Plyler v. Doe* decision and held that undocumented children could not be denied access to a public education in the United States. The ruling argued 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 policies that 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 was accompanied by a sharp change in federal immigration policy, leading to widespread disruption in immigrant communities across the United States (Rodrigues, 2025). A growing body of research has begun to document the effects of these changes on K-12 schooling. Using school-level data, Dee (2025) finds attendance declines in California’s agricultural Central Valley, while 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 actions (IEAs) reduced student achievement.

Each of these studies has provided timely evidence on an important policy issue and documented the negative effects of recent changes in federal immigration enforcement on students. However, questions remain about the nature of these effects. First, administrative data available for these analyses are potentially noisy proxies for students’ own actual vulnerability to immigration enforcement. For example, approximately 70% of students classified as MLLs are U.S.-born (Sugarman & Lee, 2017). Estimates using this treatment definition therefore average over students with distinct risk and response profiles, attenuating measured effects for the students experiencing the most harm. Second, most existing designs cannot separate a sustained shift in attendance behavior from acute responses to specific enforcement events. Each

of these channels has different implications for whether schools invest in sustained reengagement or event-triggered responses (Lenhoff & Singer, 2025).

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 detailed timeline of immigration enforcement activities from community-sourced reports and administrative records obtained through a Freedom of Information Act (FOIA) request. Our data include student birthplace—a necessary condition for most categories of deportability—alongside the conventional proxies as well as a household linkage identifying U.S.-born students who live with a foreign-born student. These data allow us to estimate effects under a set of candidate definitions for the affected population within a single setting and address control group contamination concerns, thereby allowing us to provide direct evidence on the measurement problem common in this literature while also allowing us to document effects in a setting that is particularly supportive of immigrant students.

Our results reveal that the dominant effect of the post-inauguration enforcement climate is not a transient spike in absences around specific events but a sustained deterioration, with daily absence rates among foreign-born students increasing by approximately 2 percentage points (33 percent) across the post-inauguration period. We additionally find evidence of short-duration acute effects that are more modest in magnitude, but educationally meaningful and concentrated on the day of and after high-profile enforcement events. When comparing across potential definitions of student vulnerability, estimates fall nearly monotonically as the treatment definition carries less information about enforcement risk and are largest when vulnerability is measured by student birthplace. We also show that effects increase with grade level, inverting the age pattern documented following large-scale workplace raids and concentrating the attendance burden among students nearest the transition to postsecondary education and the labor market.

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.

Finally, we examine the potential for spillover effects on vulnerable U.S.-born students (e.g., multilingual learners or students with a non-English home language). Across specifications, our estimates of spillover effects are small in magnitude and imprecise, but allow us to rule out differential spillovers larger than a 1.5-1.7 percentage point increase in the probability of a student being absent.

Our paper proceeds as follows. Section 2 describes prior research on immigration enforcement and student attendance and provides 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 potential spillover effects on U.S.-born students who live with a foreign-born household member or belong to specific subgroups. We conclude our paper with a discussion of implications, limitations, and areas for future research in Section 7.

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 fear within affected communities, and may radiate outward to individuals who are not directly targeted. These parallels suggest 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

As a “wicked problem” (Childs & Lofton, 2021; Weber & Khademian, 2008), chronic absenteeism is a complex issue that is difficult to definitively solve without coordinated responses across multiple social

sectors (Dee, 2024; Kearney, 2008). Beyond immigration enforcement activity, chronic absenteeism is also associated with poverty (Singer et al., 2021; Gee, 2018); disability and chronic illness (Kim et al., 2020; Gottfried et al., 2019); hostile school climate, including peer harassment and violence (Hughes et al., 2015; Grinshteyn & Yang, 2017); unsafe and/or complex commutes (Burdick-Will et al., 2019; Stein & Grigg, 2019; Burdick-Will & Stein, 2026); and unengaged or overwhelmed parents (Gottfried & Gee, 2017). Disaggregating absence by type in Indiana, Malkus and Hollon (2026) found that students from disadvantaged groups—poor, Black, and Latino students—have higher rates of unexcused absences compared to white and high-SES students. These students may experience greater difficulty in having their absences marked as excused even when they are for legitimate reasons (e.g. being unable to access medical care that would result in obtaining a doctor’s note).

The consequences of chronic absenteeism are wide-reaching, impacting students’ academic achievement and non-academic well-being. Students who are chronically absent from school perform worse on math and reading assessments (Malkus & Hollon, 2026; Smerillo et al., 2018; Gottfried, 2009), with spillover effects on the classmates of absent students (Gottfried, 2019). Chronic absenteeism is also associated with lower likelihood of high school graduation (Smerillo et al., 2018) and weaker social-emotional skills such as self-efficacy (Santibañez & Guarino, 2021). Additionally, chronic absenteeism in early elementary school is associated with continued chronic absenteeism in high school (Ansari & Planta, 2019).

The relationship between immigration enforcement and school attendance has been documented across multiple enforcement regimes, geographic contexts, and time periods. Early 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 multilingual learners (MLLs), Hispanic non-MLLs, and non-Hispanic students in counties approved versus rejected for 287(g) participation. They found that local participation in 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 multilingual 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, the author disentangles 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 20, 2025, has generated a new wave of research connecting the effects of immigration policy and student attendance. Dee (2025) studied the post-inauguration period 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 study period, suggesting potential enrollment losses with longer-term developmental consequences for students and fiscal consequences for school districts. Slungaard Mumma (2025) used data from public-facing attendance dashboards in Connecticut and Rhode Island and found 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 on student attendance. However, several questions remain unanswered. Most existing studies rely on aggregate attendance measures, at the school or district level, which limits researchers' ability to consider multiple

treatment definitions and spillover effects on U.S.-born students. The literature has also struggled to distinguish between acute responses to specific enforcement events and persistent shifts in attendance behavior, a distinction of substantial relevance to policymakers and practitioners seeking to mitigate these negative effects (Lenhoff & Singer, 2025). 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 experienced achievement declines, suggesting that enforcement disrupts broader school and community environments. Weber (2022) replicated and extended 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) linked 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. Avila (2024) studied a raid in Allen, Texas that occurred just 40 days before the state accountability testing using a triple-differences design and found that 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) used 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 similar 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 operate 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 Assistance 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 and 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 percent 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 especially 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 (Lowenhaupt et al 2021; 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 actions.

In Liberty City, 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. While just 5 percent of teachers statewide identify as Hispanic/Latino, almost a quarter of all teachers in Liberty City are Hispanic/Latino. More directly relevant to the outcomes we examine, the Liberty City School District (LCSD) revised its policies in 2024 to create an Immigration

Enforcement Response Team and to prohibit staff from sharing any student data with federal agencies.² 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, student demographics, and student address information with records of immigration enforcement actions in both the Liberty City community and broader state/region. Community-sourced records of IEAs were collected as part of a partnership between researchers, school district staff and a local non-profit immigrant advocacy organization. The school district maintained these records while the advocacy organization trained volunteers on properly identifying ICE agents. Compared to available administrative data, these records have the advantage of allowing us to identify IEAs within the municipal boundaries rather than at more aggregate levels. This community-sourced list of immigration enforcement actions contains 16 enforcement events reported between January 20, 2025, and the end of the 2024-25 school year; typically sightings of ICE agents in the community. As high-profile IEAs outside of Liberty City (e.g., in a nearby community or elsewhere in the state/region) may still impact Liberty City students, we additionally incorporate administrative records provided by Immigration and Customs Enforcement (ICE) in response to a FOIA request.³ Because community reports reflect residents' perceived enforcement presence, and reporting intensity may itself rise with community

² The policy was a blanket prohibition on data sharing rather than a rule targeted at specific data elements.

³ These records are maintained by the Deportation Data Project (<https://deportationdata.org>).

anxiety, the administrative records provide a complementary event measure independent of local reporting behavior.

Event-level data are useful for unpacking the mechanisms by which changes in immigration policy might affect student attendance. Vulnerable students may respond to policy changes through several distinct channels. For instance, students may respond via a low-intensity, persistent shift in their baseline propensity to be absent while responses to specific events manifest as acute response concentrated in the days surrounding an event. Different combinations of these channels may 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 percent of vulnerable students were absent on each post-treatment day. Conversely, if effects were only through the high-intensity, acute channel, we may obtain approximately the same point estimate if every vulnerable student were absent on just five specific days (or half of vulnerable students absent on ten specific days, etc.).

To be clear, analyses relying on aggregate attendance data recover this combined effect; the contribution of daily records is not a different average effect but the ability to separate its components. That separation proves meaningful in our setting as the post-inauguration increase in absence probability is roughly 1.5 times as large on the day after a high-profile enforcement event as on other post-treatment days, an educationally meaningful distinction that data aggregated to the week or month would smooth over.

We merge these records of IEAs with attendance data at the student-by-day level, which capture each student's status on that particular day (i.e., absent, present, tardy, or dismissed early).⁴ Students are considered absent if they are marked absent for more than half of the school day. These records contain

⁴ 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.

additional information about 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 from nine other reasons.

Student demographic data provided by LCSD include students' race/ethnicity, date of birth, enrollment entry/exit date, home language, multilingual learner status, and recent immigrant status.⁵ We merge these data with birth location data, which LCSD collects at the time students enroll in the district, and student mailing addresses. We group students into households via shared mailing address and identify mixed-nativity households where a U.S.-born student shares an address with a student born outside the United States. While mailing addresses are routinely collected by school districts, collecting birthplace is somewhat less common. Liberty City School District requests that students provide birthplace documentation, such as a birth certificate, when they enroll in the district. As these records were hand-entered by district staff, two members of the research team coded 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 U.S. and foreign born. We exclude a small number of students from our analytic sample (N = 200; 4% of student-day observations) because birth location data are unavailable.⁶

The combined student attendance, address, demographic, and birth location data 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, a change in policy required that

⁵ 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.

⁶ 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. Given the small number of students affected, this is unlikely to meaningfully change our findings. We first note that our results using an alternative treatment definition are not subject to this missing data issue. Next, to assess sensitivity of our estimates to excluding these observations, we conduct a bounding exercise by re-estimating our primary specification under the two extreme assumptions that all students with unknown birth location were born outside the U.S. and that none were. Results, available upon request, are qualitatively similar to those we report here in all cases which is unsurprising given the small number of affected observations.

the district affirmatively mark students as present starting in the 2022-23 school year. Restricting our analysis to use attendance records for the 535 school days covering the 2022-23 through 2024-25 school years 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 1,355,000 student-by-school day records.

We define the treatment period, during which we anticipate vulnerable students responding to IEAs, as beginning on January 20, 2025 (i.e., the date of the presidential inauguration). While IEAs occurred prior to this date as part of routine enforcement, 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. 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 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).

3.1 Disenrollment during the study period

A potential concern about our data is that shifts in federal immigration policy may cause families to leave the district entirely. If disenrollment is correlated with treatment status, then our panel would become compositionally different over time in ways that could bias our estimates. For example, if the families most sensitive to enforcement leave the district, the remaining treated students would be less responsive on average, thereby attenuating our estimates. Conversely, if the families who stay are those most constrained

in their ability to relocate and correspondingly most sensitive to immigration policy, the opposite could occur. In Appendix A, we show that there were no notable changes in patterns of 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 pressures. Our results are therefore informative about the effects of immigration policy on student attendance but not enrollment, an issue that may be particularly relevant given the enrollment-based funding systems present in nearly all states.

3.2 Operationalizing treatment

Any credibly causal estimation of effects requires defining treated and control groups. A challenge in our setting—and in similar studies discussed in Section 2—is that no available administrative record directly measures the construct of interest (citizenship or legal resident status). Furthermore, the population plausibly affected by changes in immigration policy is broader than the population at direct risk of removal. Students who themselves are potentially removable face the most direct exposure, but U.S.-born students with an undocumented parent or sibling, students in immigrant-origin communities, and students who simply fear encounters with enforcement agencies may all reasonably respond to the policy changes we study. Any feasible treatment definition therefore classifies students with error relative to an ideal definition.

Our data support several candidate definitions, each capturing a different observable correlate of exposure. Birthplace, collected at enrollment with documentation requirements for U.S.-born students, identifies whether a student is a *jus soli* citizen. Of course, students classified as treated by this definition may not actually be at risk of deportation if they are naturalized citizens or hold secure legal status. Our household linkage identifies U.S.-born students who share a mailing address with a foreign-born student and is the closest feasible measure of mixed-nativity households in data containing no parental information. Multilingual learner status identifies students who received language services, a group that mixes non-citizen students with U.S.-born children of non-English-speaking households. Home language and student

race/ethnicity share this ambiguity. The recent immigrant indicator conditions on foreign birth but excludes long-tenured foreign-born students who may still lack secure status. Additionally, we construct an ensemble definition which considers a broad definition of treated: the set of students born outside of the U.S., U.S.-born students with a foreign-born peer living in the household, students identified as multilingual learners, students with a non-English home language, and students classified as being a recent immigrant.

In our preferred specification, we define students born outside the U.S. as treated, with U.S.-born students forming the control group. Misclassification can enter this definition in two directions. Some foreign-born students are citizens or hold secure status and face little enforcement risk, while some U.S.-born control students plausibly respond to enforcement themselves, perhaps because they live in a mixed status household. Both forms of contamination compress the treatment-control contrast and bias estimates toward zero, so our estimates are most naturally read as lower bounds on effects for students genuinely at risk from immigration enforcement. Section 4.1 develops the logic by which we select among candidate definitions, including the question of whether the appropriate estimand concerns the directly exposed or the broader affected population, and Section 4.2 formalizes our approach to spillovers onto U.S.-born students, with estimates presented in Section 6.

<< Table 1 – Student characteristics by birthplace and school year. >>

Table 1 reports student characteristics and attendance patterns by nativity status 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 2.3 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%).

This pattern changes 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 by both an increase in absences among foreign-born students, whose absence rate rises from 8.8 to 9.3 percent, and decrease in absences among U.S.-born students whose absence rate falls from 11.1 to 9.7 percent. 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 – Monthly 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.⁷ 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 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 these descriptive observations in Section 4.

4 METHODS

We estimate the causal effect of the post-inauguration change in immigration policy on student attendance using a difference-in-differences identification strategy. We first examine the assumptions required to obtain

⁷ This pattern is also visible during the 2021-22 school year, though COVID-19 related disruptions appear to also influence attendance patterns for students in both groups.

credible causal estimates using this approach in a simplified setting before discussing the definition of treatment, potential spillover effects, and short-term responses to specific high-profile events.

The intuition behind our identification strategy is apparent in Figure 1, which shows that absence rates for foreign-born and U.S.-born students followed closely parallel seasonal patterns through the 2023-24 school year and fall 2024. The deviation in these parallel trends, coinciding with the January 2025 inauguration, potentially reflects the effects that we are interested in identifying. Within the difference-in-differences framework, we leverage this pattern to construct a plausible counterfactual for treated students. Specifically, we estimate a standard two-way fixed effects (TWFE)⁸ specification following Equation (1).

$$Y_{it} = \gamma_i + \lambda_t + (D_i \cdot \mathbf{1}[t \geq t^*])\tau^{DiD} + \epsilon_{it} \quad (1)$$

Here, Y_{it} takes a value of 1 if student i is absent on day t and 0 otherwise. The terms γ_i and λ_t represent individual student and calendar day (e.g., February 12, 2025) fixed effects, respectively, and capture both individual students' propensity to be absent and common shocks that affect all students equally on a given day (e.g., inclement weather). D_i is an indicator for treated students and is interacted with an indicator that takes a value of 1 following the January 2025 inauguration and 0 otherwise. The coefficient on this interacted term, τ^{DiD} , is an estimate of the effect of the post-inauguration enforcement climate on student absences. We deliberately characterize this estimand as an effect of the enforcement climate rather than of enforcement actions themselves. Because treatment is defined by the inauguration date rather than by the timing of specific enforcement events, this coefficient captures the combined effect of heightened enforcement activity, changes in several federal programs, and the fear, anxiety, and awareness that accompany them. Attendance is binary, meaning that this estimate has a straightforward interpretation as a

⁸ 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.

change in the probability of treated students being absent on post-inauguration school days and is credibly causal under a set of standard assumptions.

Before stating these assumptions, it is worth being explicit about the variation that identifies this coefficient. Because the model includes a separate fixed effect for every calendar day, all shocks common to students on a given day, including the seasonal evolution of absences across the school year, are absorbed. Identification therefore does not require that the fall semester (or any other period) serve as a valid control period for the spring. Our estimates are driven by changes in the difference between foreign-born and U.S.-born absence rates within the same school days.⁹

Our first, and strongest, identifying assumption is that trends in student absences for treated and untreated students would have evolved in parallel had there not been changes in federal immigration policy. This parallel trend assumption is supported by Figure 1, and we provide more formal evidence via an event-study analogue of Equation (1) in Section 5. Next, we must assume no anticipation behavior—i.e., that there were no effects on student attendance prior to the January 2025 inauguration. While this assumption may not be fully satisfied in our setting, we show in Appendix B that our findings are robust to a wide range of potential start dates ranging from the November 2024 election through February 1, 2025. To the extent that anticipatory effects bias our results, this bias is small in magnitude and would attenuate estimation of true effects. Lastly, we must assume that members of the control group are unaffected by changes in immigration policy. We return to this assumption in Sections 4.2 and 6.

4.1 Defining the treated group

As established in Section 3.2, any feasible treatment definition classifies students with error relative to the unobservable construct of interest. Contamination from either unexposed students in the treated group or

⁹ As a direct check, we re-estimate Equation (1) restricting the sample to spring-semester school days in all three years so that the counterfactual foreign-born/U.S.-born gap is identified exclusively from prior springs. The resulting estimate ($\beta = 0.025$, $SE = 0.004$) is statistically indistinguishable from, and slightly larger than, our preferred estimate, indicating that including fall observations does not drive our findings.

exposed students in the control group compresses the treatment-control contrast and biases estimates toward zero. Two questions naturally follow. First, which population should the estimand target: students at direct risk of enforcement or the broader set of students plausibly affected? Second, given a target population, which observable definition measures it with the least error?

On the first question, one view holds that the treated group should be broad because U.S.-citizen children in mixed-status families may outnumber undocumented students and immigration policy plausibly affects them as well. We do not dispute the premise, but broadening the treated group would replace a well-defined contrast (because foreign birth is a necessary condition of direct enforcement risk) with an average over heterogeneous and largely unobservable exposure without solving the underlying measurement problem. As we show in Section 5, an ensemble definition classifying a student as treated if any candidate criterion applies yields an estimate attenuated relative to birthplace alone, indicating that casting a wider net dilutes rather than sharpens the measured effect. We therefore target effects on more directly exposed students who were born outside of the U.S. and address the broader population separately with specifications, described in Section 4.2, that both remove plausibly affected U.S.-born students from the control group and estimate their response directly.

With the estimand fixed, the choice among candidate definitions becomes a measurement question which can be evaluated based upon empirical criteria. If the available proxies were equally noisy measures of the same underlying vulnerability, they would produce similar point estimates in expectation. Systematic differences in the magnitude are informative about differences in misclassification because definitions that more tightly bound the truly exposed population should produce larger estimates while noisier definitions mechanically attenuate toward zero. In Section 5, we re-estimate Equation (1) under each candidate definition and find that estimates attenuate nearly monotonically as the definition becomes a less direct measure of enforcement risk. Birthplace—the only definition conditioning on a necessary condition for deportability—yields the largest estimate. We therefore treat the cross-definition pattern as consistent with the view that our preferred definition contains the least measurement error for the construct of interest rather

than merely as a robustness exercise. We report estimates using all candidate definitions to facilitate comparisons with other studies in this literature and to indicate the scope of these dilution effects.

The logical endpoint of this reasoning is a falsification test. A definition that carries no information about enforcement vulnerability should produce an estimate of zero. We implement this test by defining “treatment” as special education status among U.S.-born students, a characteristic with no plausible connection to enforcement risk. We restrict this test to U.S.-born students because disability status is strongly correlated with nativity as shown in Table 1. In the full sample, students without disabilities would be disproportionately foreign-born, and a disability-based “placebo” would partially reconstruct the contrast of our main specification. Restricting to U.S.-born students purges this composition problem and leaves a comparison in which neither group contains students expected to be affected.

Allowing the treatment definition to vary across model specifications allows the composition of the control group to vary in ways that may reflect undesirable contrasts. As a concrete example, consider a specification of Equation (1) where D_i takes a value of 1 for students identified as immigrants under the Title III definition (i.e., have spent less than three years in U.S. schools) and 0 otherwise. The control group here would then include all foreign-born students who have spent multiple years in the U.S., compressing the treatment-control contrast on which effects are identified. This compression would be unavoidable using aggregated school-by-group-by-time attendance data. However, we can force comparisons with a “clean” control group with using our student-level data. Across each candidate definition, we estimate Equation (1) using an unrestricted control group, mirroring what would be found with aggregate attendance rates, and using a control group constrained to students not identified as treated by the ensemble definition (i.e., U.S.-born students who are not multi-lingual learners and do not have a non-English home language). Presenting our results in this way is informative in two regards. First, within control group definition, differences in estimates are informative about the extent to which heterogeneity attenuates estimates of effects. Second, within treatment definition, differences in estimates across either specification of the control group are informative about the extent to which control group contamination biases estimates of each.

4.2 Accounting for potential spillovers

As previously mentioned, our identification relies on the assumption that members of the control group are not affected by changes in immigration policy. This requirement is an instance of the stable unit treatment value assumption (SUTVA) in that each student's potential outcomes must depend only on their own treatment status, not on the treatment status of others. When treatment status is defined by student birthplace, as in our preferred specification, this assumption may be violated through spillover effects, which may be general (affecting all members of the control group) or specific (affecting identifiable subsets of the control group). We consider each form in turn.

General spillovers, which would occur if changes in U.S. immigration policy affected the propensity of all students to attend school, are not a phenomenon that we can empirically address, as there would be no unaffected control units in our data. If these spillovers were positive and increased the propensity of control group students to attend school, we would overestimate the true effect. We view the risk of this occurring as minimal because there is no clear mechanism by which the policy changes we study could increase student attendance for U.S.-born students. Instead, we assume that general spillovers, if present, would decrease the propensity of all students to be present, resulting in estimates that would represent the differential effect for treated students. Under this form of SUTVA violation, our design does not recover the effect of enforcement relative to a no-enforcement counterfactual and instead estimates the differential effect on treated students relative to a control group that may itself be modestly affected. We argue that this estimand is still policy relevant, especially for communities like Liberty City with large shares of vulnerable students.

The second, and arguably more concerning, form of spillover occurs when changes in immigration policy affect members of the control group. For example, as a result of defining our treated group as students born outside the U.S., the control group may include U.S.-born siblings of those students. Those U.S.-born siblings would plausibly experience a chilling effect of immigration policy on attendance alongside the treated group, resulting in our estimates experiencing a downward bias. In our setting, we can directly

address this risk by augmenting Equation (1) to force a clean comparison of students born outside the U.S. with uncontaminated control group students (Section 4.1) as shown in Equation (2).

$$Y_{it} = \gamma_i + \lambda_t + (D_i \cdot \mathbf{1}[t \geq t^*])\delta_1 + (S_i \cdot \mathbf{1}[t \geq t^*])\delta_2 + v_{it} \quad (2)$$

Equation (2) follows the structure of Equation (1) by including individual student (γ_i) and day (λ_t) fixed effects as well as the interaction of treated group membership (D_i) with a post-inauguration treatment indicator but differs in that we include an indicator for membership in a group potentially subject to specific spillovers S_i (e.g., U.S.-born students in mixed-nativity households), again interacted with the post-inauguration treatment indicator. Mechanically, this constrains treatment effects for students born outside the U.S. (δ_1) to be estimated by a “clean” comparison with student-day observations that are not members of the spillover group S . Additionally, this approach estimates potential spillover effects on U.S.-born students from spillover group S as δ_2 , also estimated from the set of clean control student-day observations. We examine four potential U.S.-born spillover groups: students who live in a household with another student born outside of the U.S. (i.e., mixed-nativity households, identified by shared mailing address as described in Section 3), multilingual learners, students with a non-English home language, and Hispanic students.

4.3 Decomposition of treatment effects

The estimate from Equation (1) captures the average effect of the post-inauguration enforcement climate across all post-inauguration days ($N = 93$)¹⁰ for students born outside the U.S. This is informative for

¹⁰ This count excludes February 3, 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.

assessing the *overall* impact of immigration policy 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 estimate a flexible specification of Equation (1) that allows treatment effects to vary on school days near a high-profile immigration enforcement arrest, as shown by Equation (3):

$$Y_{it} = \gamma_i + \lambda_t + (D_i \cdot \mathbf{1}[t \geq t^*])\phi^{DiD} + \sum_{k=\underline{k}}^{\bar{k}} (D_i \cdot \mathbf{1}[k_{it} = k])\phi_k + \zeta_{it} \quad (3)$$

Equation (3) retains the post-inauguration treatment indicator from (1) but adds interactions between the treatment group indicator D_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, centered at the date of the event and bounded by $\{\underline{k}, \bar{k}\}$. The coefficient ϕ^{DiD} 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 ϕ_k trace out the dynamic response to enforcement events in relative time: $\phi_{k=0}$ captures the additional effect of an IEA on the day that it occurs, $\phi_{k=-1}$ the day before, $\phi_{k=1}$ the day after, and so on.

Intuitively, this specification might be understood as a localized event study that allows for dynamic treatment effects in the proximity of a particular event and pools across all enforcement events, thereby estimating a common relative-time profile rather than separate effects for each event. Note that the interpretation of ϕ^{DiD} differs from τ^{DiD} in that the former is the effect on days far removed from an event (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, τ^{DiD} 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.

This specification also nests coarser approaches to separating event-driven from ambient responses, such as splitting the post-period into weeks with and without enforcement activity or interacting the treatment indicator with an enforcement-period dummy in a triple-difference framework. Equation (3) makes the same comparison at daily resolution within a single model: the relative-time indicators play the role of the enforcement-period interaction while days outside the event window identify the persistent component. Alternatives, such as a week-level triple-difference model, would additionally require designating matched control weeks in the pre-period—variation that the calendar-day fixed effects already absorb nonparametrically—and would average over day-of and day-after responses that prove empirically distinct in Section 5.

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 (3) 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 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 $k \in \{-2, \dots, +2\}$; days falling outside this window are not assigned relative-time indicators and are instead captured by ϕ^{DiD} . We select this five-day window in light of the frequency of enforcement episodes during our study period. Because episodes occur in close succession, materially wider windows would overlap in some cases, requiring individual school days to be attributed to multiple episodes and leaving few post-inauguration days outside any window with which to identify the persistent component.

We estimate these dynamic additive effects across three geographies: the community-sourced list of events described previously (12 episodes), records of arrests statewide (11 episodes), and records of arrests from the ICE administrative region (16 episodes). For the two administrative geographies, we define an event as a day on which non-custodial arrests exceeded the 75th percentile¹¹ of daily arrest levels during the equivalent January–June period of 2024. This decision rule is designed to capture high-profile incidents, which should reasonably be accompanied by unusually large numbers of non-custodial arrests. Because baseline arrest activity is mechanically higher at broader geographies, the threshold is set separately for each. Applying the state-level threshold to the administrative region would classify many more days as events without identifying incidents that are salient relative to that geography’s norm. These definitions also explain why the geographically narrower community list contains more episodes than the state measure: the community log includes sightings that need not culminate in an arrest, while the administrative measures count only days of unusually elevated arrest activity.

We note that this decomposition allows us to assess both the magnitude and duration of acute responses to discrete enforcement events in two informative ways. First, by comparing the frequency-weighted contributions of ϕ^{DiD} and the ϕ_k estimates to the overall effect from Equation (1), 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. Second, a pattern in which, for example, $\phi_{k=0}$ is large but subsequent ϕ_k estimates attenuate quickly toward zero would indicate sharp, short-lived disruptions that do not persist beyond the day of the event. Throughout all analyses, we cluster standard errors at the household level, permitting arbitrary serial correlation in attendance within students from the same household across days. Because students sharing a school and grade may be exposed to common shocks (e.g., a rumor of enforcement activity near a particular building), we confirm that all results are robust to clustering standard errors at the school-by-grade level.

¹¹ Our findings are not sensitive to this choice. Defining events using stricter thresholds (the 90th or 95th percentile) isolates fewer episodes but yields qualitatively similar results.

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 because counterfactual outcomes are unobserved, 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 the post-inauguration enforcement climate 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 (1).

Figure 2 – Event study estimates of effects post-inauguration enforcement climate on student absences.

Because our data are at the student-by-day level, daily estimates of the treatment-control differential are noisy and imprecise. In our event study, we bin estimates by month in this figure for interpretability while maintaining the calendar day fixed effects of our primary estimating equation. We display estimates showing nine school months fully before treatment onset (January 2024 through December 2024), the month that treatment occurs (January 2025), and four months fully after treatment occurs (February through May 2025). Importantly, the fixed effects structure of these event studies is unchanged, meaning that Figure 2 then plots the difference in propensity to be absent for students born outside the U.S. (compared with students born inside the U.S.) net of individual propensity and shocks common to all students, averaged at the month level. As is common in difference-in-differences event studies, the omitted category is set as the period immediately before treatment occurs (December 2024). If the parallel trends assumption holds, the pre-inauguration coefficients should be statistically indistinguishable from zero and exhibit no systematic trend.

The event study is consistent with this expectation. Across the eight monthly estimates preceding inauguration, all are statistically indistinguishable from zero and no clear pre-trends are evident. The average point estimate during the pre-inauguration period is small compared with the statistically significant estimates in the post-treatment period. To the extent that anticipation following the November 2024 election elevated absences among treated students in our December 2024 reference month, both the pre-period estimates and our main results are conservative; Appendix B shows the estimates are insensitive to treating earlier dates as the onset of treatment. The estimate for January 2025 is near zero and not statistically significant. Beginning in February 2025, the monthly estimates shift upward and remain elevated above December 2024 levels through the end of the school year. The May 2025 estimate is smaller than those of the preceding three months, though it remains positive. Because this is a single, imprecisely estimated month that coincides with end-of-year attendance patterns, we cannot distinguish genuine attenuation from noise and our study window closes before any return to pre-inauguration patterns could be observed, a limitation we return to in Section 7. We next turn to estimates of the overall effect under our preferred definition of treatment, several alternatives, and a placebo definition for which we expect no effect, shown in Table 2.

<< Table 2 – Effects of the post-inauguration enforcement climate on student attendance, by treatment and control definition. >>

Panel A, column 1 of Table 2 reports the overall effect from Equation (1), 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, representing a 33 percent increase relative to the pre-treatment absence rate of 6.7 percent. In concrete terms, this implies that a student born outside the U.S. would have been absent on average two additional days during the 93 school days following the inauguration on January 20, 2025. Benchmarked against the absenteeism literature, which finds that each additional absence reduces achievement by roughly 0.005-0.01 standard deviations (Gershenson et al., 2017; Liu et al., 2021), the

average effect implies modest but non-trivial learning costs per student, which are concentrated, as we show below, among students for whom absences are most immediately consequential.

The remaining columns of Panel A implement the cross-definition comparison developed in Section 4.1. Recent studies of enforcement effects on attendance have relied on proxies for the affected population, such as MLL status or home language; if measurement error in student vulnerability drives differences in estimated magnitudes, estimates should be largest under the definition that most tightly bounds the population at direct risk and attenuate as definitions grow noisier. That is precisely the pattern the table shows. The birthplace definition yields the largest estimate (+2.2 percentage points); defining treatment by residence in a mixed-nativity household yields +2.0 percentage points; current MLL status, +1.6; non-English home language, +1.5; and the recent-immigrant indicator, +1.1. The ensemble definition—classifying a student as treated if any of these criteria apply—yields +1.6 percentage points, indicating that casting a wider net dilutes rather than sharpens the measured effect. All are statistically significant at the 99 percent confidence level. The final column of Panel A implements the falsification test described in Section 4.1 by restricting the sample to U.S.-born students and defining “treatment” by special education status (i.e., a characteristic that should carry no information about vulnerability to immigration policy).¹² This test produces a precisely estimated null of −0.3 percentage points.

Panel B reports estimates across treatment definitions with the control group restricted to the set of “clean” comparison students (i.e., those not identified by our ensemble definition). Estimates in Panel B tend to be larger if somewhat less precise than those in Panel A. For foreign-born students, we find an increased probability of being absent of 2.4 percentage points (36%) with similar increases for students in mixed-nativity households and current MLL students. The increase in magnitude of estimates for recent immigrants is substantially larger for recent immigrant students (0.9 percentage points), reflecting the

¹² Absence reason codes might appear to offer an outcome-based placebo (e.g., absences attributed to illness). These codes are reported by families, however, and are plausibly endogenous to treatment in that a household keeping a child home out of fear may record the absence as illness. Because a placebo outcome requires that the outcome be unaffected by treatment through any channel, we rely on the group-based falsification test instead.

downward bias caused by including foreign-born students in the control group for this definition in Panel A. Estimates under the home language and ensemble definitions are essentially unchanged, while the point estimate for our placebo treatment definition is now a precisely estimate effect of 0.

We note that the gradual attenuation of point estimates across both panels is the pattern our measurement-error interpretation predicts. Estimates shrink as definitions carry less information about enforcement risk and reach zero when they carry none. These results are difficult to reconcile with a secular post-inauguration change in attendance affecting broad student subgroups. Two practical implications follow. First, differences in estimated magnitudes across studies plausibly reflect how each study measures its treated population rather than genuine contextual differences. Second, researchers relying on the proxy definitions standard in administrative data should interpret their estimates as lower bounds on the effects experienced by the most directly affected students.

We next decompose our preferred estimate (i.e., treatment defined by birthplace and comparison group restricted to clean controls) following Equation (3), which separates the persistent post-inauguration shifts 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, events occurring anywhere in the state, and events within the broader ICE region of responsibility, yielding 12, 11, and 16 episodes, respectively. We feature the state-level decomposition, shown in Figure 3, for two reasons. First, state-level episodes are constructed from administrative arrest records and are therefore unaffected by the reporting concerns noted in Section 3. Second, the state is plausibly the broadest geography at which enforcement remains salient to Liberty City families, as such events receive statewide news coverage and circulate through local social networks. The community and regional decompositions appear in Appendix C.

**<< Figure 3 – Persistent and acute effects on absences
relative to high-profile events, state level >>**

The persistent effect closely mirrors the overall estimate in the first column of Table 2 and is remarkably stable across all three geographic definitions, ranging from +2.1 to +2.3 percentage points and statistically significant at the 99 percent confidence level in each estimation. This stability is expected because the persistent component captures the baseline elevation in absences on post-inauguration days outside any event window, and its magnitude should be largely invariant to which set of events defines that window unless absences were entirely concentrated around high-profile enforcement actions. The acute effects are modest by comparison, but instructive for understanding the persistent and acute channels. State-level episodes produce no same-day effect but an additional 1.4 percentage point increase in absence probability on the day following an event, implying a total day-after effect roughly 1.6 times the persistent elevation and potentially reflecting the time required for news of enforcement activity to reach families. This day-after response is not an artifact of our event definition and remains similar in magnitude when episodes are defined using stricter criteria that isolate fewer, more salient days.

Appendix C reports these results using the community and region-level geographies. Community-level episodes produce a moderate positive point estimate on the day of an event (+0.9 percentage points) that is significant at the 95 percent confidence level, and regional episodes show no detectable response at any lag, consistent with events at that geographic remove being too diffuse to generate a concentrated attendance response. Importantly, the pre-event placebo estimates (one and two days before an episode) in all three figures are small and insignificant, providing reassurance that the day-after response is not driven by pre-existing trends around event dates. Taken together, these results indicate that the dominant channel is the persistent shift associated with the post-inauguration enforcement climate; high-profile enforcement events layer a detectable but short-lived disruption that is concentrated on the day after news plausibly reaches families and sits atop the sustained, persistent effect.

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 are able to exercise considerably more individual discretion. If the enforcement climate operates 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 (1) with indicators for each grade, estimating separate effects for each grade level. Figure 4 presents these results.

**<< Figure 4 – Effects of post-inauguration enforcement
climate on student absences, by grade >>**

The pattern shown in Figure 4 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 0 and 2 percentage points, 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.

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. Several explanations could fit this pattern. First, older students may be more aware of enforcement activity, more likely to consume news and social media, and more able to act on their own attendance decisions. This would suggest 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 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 (1) 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.

This finding carries three important implications. First, it stands in contrast with prior research. Dee (2025) found absence effects concentrated among the youngest students, with pre-K absences rising most sharply; we find small and statistically insignificant estimates in the earliest grades, including Pre-K, and effects that rise nearly monotonically thereafter. Appendix D shows that this gradient is not an artifact of our treatment definition; the same pattern appears under the MLL, home-language, recent-immigrant, and ensemble definitions, thereby ruling out the possibility that the divergence arises because our birthplace measure excludes U.S.-born children of immigrants whom proxy-based definitions capture. The remaining candidate explanations are contextual. The enforcement activity we study consists of individual arrests and sightings rather than the large-scale workplace raids in other research, which may bear differently on the parental decisions that govern young children’s attendance. Furthermore, Liberty City’s institutional posture may have been reassuring to families on the margin of attendance decisions. We do not adjudicate among these explanations here but note that our results indicate that the grade-level incidence of enforcement effects may vary with the nature of enforcement and local context rather than being a stable feature of these events.

Second, some portion of the response among older students may reflect neither fear nor risk aversion because high school students may miss school to work if enforcement disrupts their parents’ employment or if they anticipate eventual deportation, or to participate in protest activity responding to enforcement, a salient possibility in this setting given the widespread absences among both groups during the February 3 “Day Without Immigrants” protest. We cannot distinguish these channels with our data, but each reinforces the conclusion that attendance responses among older students are not purely parental decisions. Lastly, we note that the students most affected tend to be those closest to the transition into postsecondary education and the labor market. For these students, effects of 3 to 6 percentage points translate roughly to three to six additional days absent over the post-inauguration period alone. Absence-driven losses in learning and engagement at this stage may carry meaningful consequences for their

postsecondary enrollment, their subsequent economic productivity, and the society-wide returns that follow from both. Given that these students already are approaching working age, these are costs that begin accruing in the short term rather than only over a generational horizon.

6 SPILLOVER EFFECTS ON U.S.-BORN STUDENTS

The estimates in Section 5 measure effects on students who are themselves foreign-born. The population plausibly affected by the post-inauguration enforcement climate, however, is broader. Nationally, U.S.-born children in immigrant families substantially outnumber foreign-born children, and the large majority of children living with an undocumented parent are themselves U.S. citizens (Capps et al., 2016). Spillovers onto these students matter for multiple reasons. Substantively, they determine the total attendance cost of the enforcement climate, which our main estimates alone may understate. Methodologically, as shown in Table 2, when affected U.S.-born students are included in the control group estimates understate effects on the directly exposed.

To estimate these spillovers, we augment our primary specification, as formalized in Equation (2), with a parallel interaction term identifying U.S.-born students who belong to plausibly vulnerable subgroups (e.g., students in mixed-nativity households and multilingual learners). We examine each group in a separate regression because the subgroups overlap substantially with one another and joint inclusion would allocate a common response across heavily overlapping indicators rather than provide an interpretable test of each.

Before turning to results, we note the scope of each measure. The household-based indicator is the most direct available measure of mixed-status exposure, but it captures only one household configuration: U.S.-born students who share an address with an enrolled foreign-born student (e.g., a sibling). Mixed-status families with immigrant parents but exclusively U.S.-born children are not identified using this approach, as no students would appear in our data as being born outside the U.S. The gap between the approximately 150 students this measure identifies and the nearly half of U.S.-born students with a non-English home language (see Table 1) indicates that the undercount is substantial. The demographic

indicators are correspondingly noisier proxies for household-level exposure, but they are the only measures in our data that reach the parental channel that the household linkage cannot.

**<< Table 3 – Effects of the post-inauguration enforcement
climate on student attendance net of spillovers. >>**

Results of this spillover analysis are reported in Table 3. Across all four specifications, the spillover coefficients are small in magnitude and statistically insignificant, suggesting no detectable differential effect of the post-inauguration enforcement climate on the attendance of U.S.-born students in these subgroups. The main treatment effect for foreign-born students remains stable at a 2.3-2.4 percentage point increase in the probability of being absent. These estimates also reconcile an earlier result in that the attenuated estimate under the mixed-nativity household treatment definition in Panel A of Table 2 (+2.0 percentage points) is consistent with the enrollment-weighted average for the foreign-born effect (+2.4 among roughly 1,100 students) and the near-zero response of U.S.-born students in mixed-nativity households (+0.3 among roughly 150 students). The attenuation in Table 2 is thus partially accounted for by dilution with unaffected (or nearly unaffected) U.S.-born students.

While the stability of estimated effects for foreign-born students is reassuring from a methodological perspective, two limitations bound what this analysis can establish for the potential spillover subgroups. First, the confidence intervals cannot rule out differential spillovers of up to roughly half the magnitude of our main estimate, or a 1.5-1.7 percentage point increase in the probability of being absent. Second, these tests speak only to differential spillovers onto identifiable subgroups of U.S.-born students. As discussed in Section 4.2, spillovers common to all U.S.-born students are untestable in our design and, under that form of SUTVA violation, our estimates recover the differential effect on foreign-born students relative to a control group that may itself be modestly affected rather than the effect relative to a non-enforcement counterfactual.

Our null spillover estimates stand in contrast with Figlio and Özek (2025), who found achievement declines for U.S.-born Spanish-speaking students comparable to those for foreign-born students. Several factors may reconcile these findings. Achievement operates on a different margin than attendance because psychological distress can depress performance among students who continue attending school, a pattern consistent with Avila’s (2024) findings that achievement effects were not primarily driven by absenteeism. Context may also matter. The buffering effects of Liberty City’s welcoming institutional posture may accrue disproportionately to indirectly exposed students, for whom reassurance from trusted local institutions is plausibly most effective. Additionally, our limited power leaves room for modest effects that we cannot detect. Finally, we note that the composition of Liberty City tempers the aggregate implication of our nulls. Because the district’s foreign-born share is unusually high, its spillover-eligible population is comparatively small. In more typical districts, where U.S.-born children in immigrant families far outnumber foreign-born students, per-student spillovers even below what we are powered to detect could aggregate to district-level attendance losses rivaling the direct effect we document.

7 CONCLUSION

We estimate the causal effect of the post-inauguration enforcement climate 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 to 2.4 percentage points—a 33 to 36 percent increase relative to the pre-treatment absence rate of 6.7 percent—and that absences remained elevated through the end of the school year. Liberty City’s institutional context makes these estimates particularly informative. Municipal and school leaders took deliberate steps to protect immigrant families, including a blanket prohibition on sharing student data with federal agencies and barring law enforcement from assisting federal immigration enforcement officials.

Three features of our setting and design imply that our estimates are conservative. First, any feasible treatment definition misclassifies students relative to the unobservable construct of enforcement

vulnerability, and both directions of misclassification compress the treatment-control contrast toward zero. Second, spillovers common to all U.S.-born students are untestable in our design and, if present, would only depress the control-group attendance, meaning our estimates would recover a differential effect relative to a control group that may itself be modestly affected. Third, to the extent that Liberty City's welcoming context buffered the enforcement climate's effects, effects in less supportive environments are plausibly larger. Future research is needed in more settings to explore this possibility.

Our analysis offers several contributions to the emerging literature on immigration enforcement and education. First, we show that treatment definitions produce nearly monotonically attenuation in estimates as they carry less information about enforcement risk and are largest under nativity status, decrease across a set of more conventional proxies, and are zero under a placebo definition. This finding is consistent with classical measurement error from misclassification of the treated population. Existing proxy-based estimates should therefore be read as lower bounds on effects for the most directly affected students. The complementary spillover analysis finds no detectable differential response among U.S.-born students in vulnerable subgroups, indicating that our main estimates are not an artifact of a contaminated control group, though our statistical power is limited and we cannot rule out spillovers up to roughly half the magnitude of our main estimate. Quantifying such effects in settings with richer household data or larger populations of spillover subgroups is a priority for future work in this area.

Second, we decompose the overall effect into a persistent shift in baseline absence rates and an acute response to specific enforcement events. The persistent component dominates this decomposition, with foreign-born students remaining approximately 2.3 percentage points more likely to be absent on any post-inauguration school day, regardless of proximity to a specific enforcement event. Acute responses are detectable but concentrated near a high-profile enforcement event, with no detectable response to regional activity. This decomposition matters because the two channels carry different implications for which interventions districts devote resources towards (Lenhoff & Singer, 2025).

Third, we document a steep grade-level gradient in treatment effects, with estimates increasing from statistically insignificant in the early elementary grades to effects of +3 to +6 percentage points among

high school students. This pattern is difficult to reconcile with models in which the attendance response operates entirely through parental decisions. Because this gradient inverts the age pattern documented following large-scale workplace raids (Dee, 2025), it also suggests that the grade-level incidence of enforcement effects is contingent on the nature of enforcement activity and local context rather than a stable feature of these events. The concentration among older students is particularly concerning given that absences in the upper grades carry higher marginal costs for course completion, credit accumulation, and on-time graduation.

Several limitations bound our conclusions. Our estimates come from a single district whose demographic composition and institutional posture are distinctive, and the external validity of both the magnitude and the grade-level incidence of effects is an empirical question we do not address here. Our results speak to attendance conditional on enrollment, not to enrollment itself, which may respond over longer horizons with consequences for district finances and staffing. Our spillover tests, as noted, are informative about differential rather than general effects and cannot rule out modest responses among U.S.-born students. Finally, our study window closes at the end of the 2024-25 school year. Studies of discrete enforcement events under prior administrations document eventual returns to baseline attendance (Heinrich et al., 2023; Kirksey, 2025). Whether recovery occurs under a sustained policy regime, such as the current federal posture, is a question our data cannot answer.

Nonetheless, our findings carry practical implications for schools operating in enforcement-active environments. The persistence of attendance effects suggests that effective district responses require sustained engagement rather than mobilization only in the immediate aftermath of specific enforcement events. Because the burden falls most heavily on secondary students, reengagement efforts (e.g., outreach and credit recovery) may be best targeted there rather than distributed uniformly across grades. The breadth of estimated effects underscores that awareness of enforcement impacts should extend beyond multilingual and immigrant-origin staff to all educators and school personnel. Proactive communication with immigrant families about 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 disengagement. Partnerships

with community-based organizations can ensure families are informed of their rights and maintain access to essential services. More broadly, the magnitude of our estimate in a district with a comparatively welcoming context of reception suggests that effects elsewhere may be substantially larger.

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TABLES

Table 1 – Student characteristics by birthplace and school year.

	2023-24 School Year			2024-25 School Year		
	U.S.-Born (N=1,800)	Foreign Born (N=1,200)	Diff.	U.S.-Born (N=1,700)	Foreign Born (N=1,200)	Diff.
<i>Demographics</i>						
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
IEP or 504 Plan	0.388	0.089	-0.299**	0.388	0.089	-0.299**
Ever MLL	0.312	0.941	0.628**	0.299	0.937	0.638**
Non-English Home Language	0.468	0.990	0.521**	0.478	0.992	0.515**
Recent Immigrant	0.001	0.638	0.637**	0.000	0.585	0.585**
Foreign-Born Student in Household	0.075	1.000	0.925**	0.075	1.000	0.925**
Title 1 Student	0.845	0.963	0.118**	0.836	0.963	0.127**
Unhoused Student	0.061	0.019	-0.042**	0.075	0.028	-0.047**
<i>Attendance</i>						
Present	0.793	0.829	0.036**	0.795	0.810	0.015+
Tardy	0.092	0.080	-0.012*	0.105	0.095	-0.010
Early Dismissal	0.002	0.002	-0.000	0.003	0.002	-0.001**
Absent	0.111	0.088	-0.022**	0.097	0.093	-0.004

Notes: Significance of difference-in-means determined using Welch's t-test. + - $p < 0.10$; * - $p < 0.05$; ** - $p < 0.01$. Student counts rounded to the nearest multiple of 100 to facilitate district anonymity.

Table 2 – Effects of the post-inauguration enforcement climate on student attendance, by treatment and control definition.

<i>Panel A: Unrestricted Control Group</i>							
	Born Outside U.S.	Mixed Nativity Household	Recent Immigrant	Current MLL	NEHL	Ensemble Definition	SWDs (Placebo)
Treated x Post	0.022** (0.003)	0.020** (0.003)	0.011** (0.003)	0.016** (0.003)	0.015** (0.004)	0.016** (0.004)	-0.003 (0.005)
Obs. (Student-Days)	1,362,200	1,362,200	1,362,200	1,362,200	1,362,200	1,362,200	1,362,200
N Students Treated	1,100	1,200	600	1,400	1,800	1,800	600
N Students Control	1,600	1,400	2,000	1,200	800	800	900
Treated Pre-Period Mean	0.067	0.068	0.067	0.071	0.075	0.075	0.106
Control Pre-Period Mean	0.098	0.101	0.092	0.104	0.111	0.113	0.094
<i>Panel B: Clean Control Group</i>							
	Born Outside U.S.	Mixed Nativity Household	Recent Immigrant	Current MLL	NEHL	Ensemble Definition	SWDs (Placebo)
Treated x Post	0.024** (0.005)	0.022** (0.005)	0.020** (0.005)	0.019** (0.004)	0.015** (0.004)	0.016** (0.004)	0.000 (0.006)
Obs. (Student-Days)	938,900	1,006,100	704,600	1,143,400	1,350,400	1,362,200	595,400
N Students Treated	1,100	1,200	600	1,400	1,800	1,800	600
N Students Control	800	800	800	800	800	800	800
Treated Pre-Period Mean	0.067	0.068	0.067	0.071	0.075	0.075	0.106
Control Pre-Period Mean	0.113	0.113	0.113	0.113	0.113	0.113	0.113

*Notes: Each column of each panel displays the coefficient on Treated x Post from Equation (1) under a different treatment definition. Panel A reports estimate with an unrestricted control group while Panel B restricts the sample to treated students and the clean control group described in Section 4.2. The ensemble definition classifies a student as treated if any of the birthplace, household nativity, multilingual learner, home language, or recent immigrant criteria apply. The final column is a falsification test and is estimated on a sample restricted to U.S.-born students and treatment is defined based on student disability status. Student counts rounded to the nearest multiple of 100 to facilitate district anonymity. Standard errors clustered at the household-level. + - $p < 0.10$; * - $p < 0.05$; ** - $p < 0.01$.*

Table 3 – Effects of the post-inauguration enforcement climate on student attendance net of spillovers.

	Born Outside U.S.	Mixed Nativity Household	Current MLL	Non-English Home Lang.	Hispanic
Born Outside U.S. x Post	0.024** (0.005)	0.024** (0.005)	0.024** (0.005)	0.024** (0.005)	0.023** (0.006)
Mixed Nativity x U.S.-Born x Post		0.005 (0.006)			
MLL x U.S.-Born x Post			0.006 (0.005)		
NEHL x U.S.-Born x Post				0.004 (0.006)	
Hispanic x U.S.-Born x Post					0.003 (0.006)
Obs. (Student-Days)	938,900	1,006,100	1,185,600	1,355,300	1,216,100
N Students Treated	1,100	1,100	1,100	1,100	1,100
N Students Spillover	-	100	400	700	900
N Students Control	800	800	800	800	800
Treated Pre-Inauguration Mean	0.067	0.067	0.067	0.067	0.067
Spillover Pre-Inauguration Mean	-	0.072	0.079	0.084	0.099
Control Pre-Inauguration Mean	0.113	0.113	0.113	0.113	0.113

*Notes: The first column replicates our preferred estimates from Table 2. Row 1 of columns 2-5 demonstrates the robustness of our estimates to potential spillover effects on U.S.-born students. Subsequent rows estimate spillover effects, using Equation (2), for U.S.-born students in the indicated subgroup (relative to the clean control group described in Section 4.2). Student counts rounded to the nearest multiple of 100 to facilitate district anonymity. Standard errors clustered at the household-level. + - $p < 0.10$; * - $p < 0.05$; ** - $p < 0.01$.*

FIGURES

Figure 1 – Monthly average absence rate by student birthplace and school year.

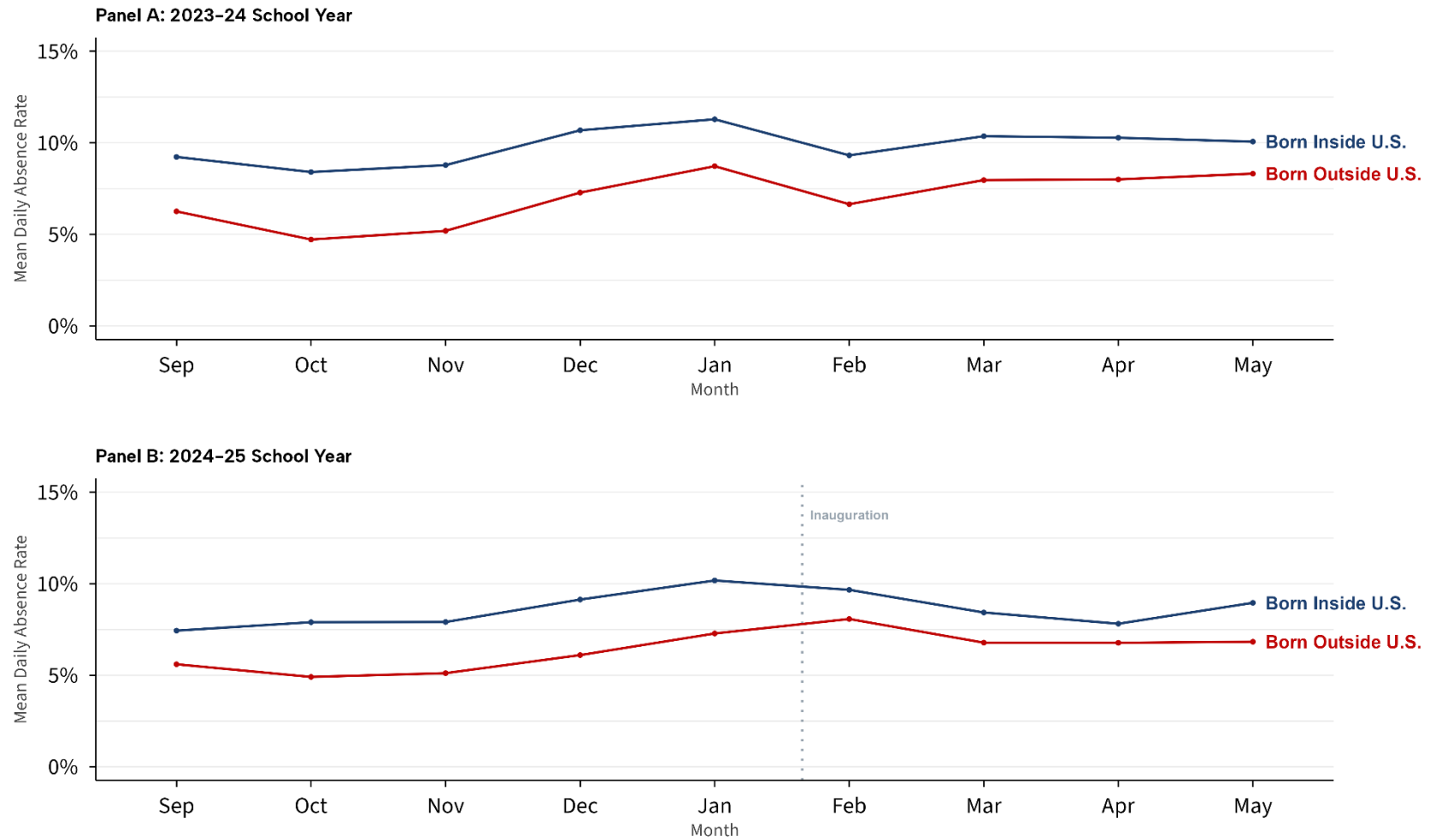
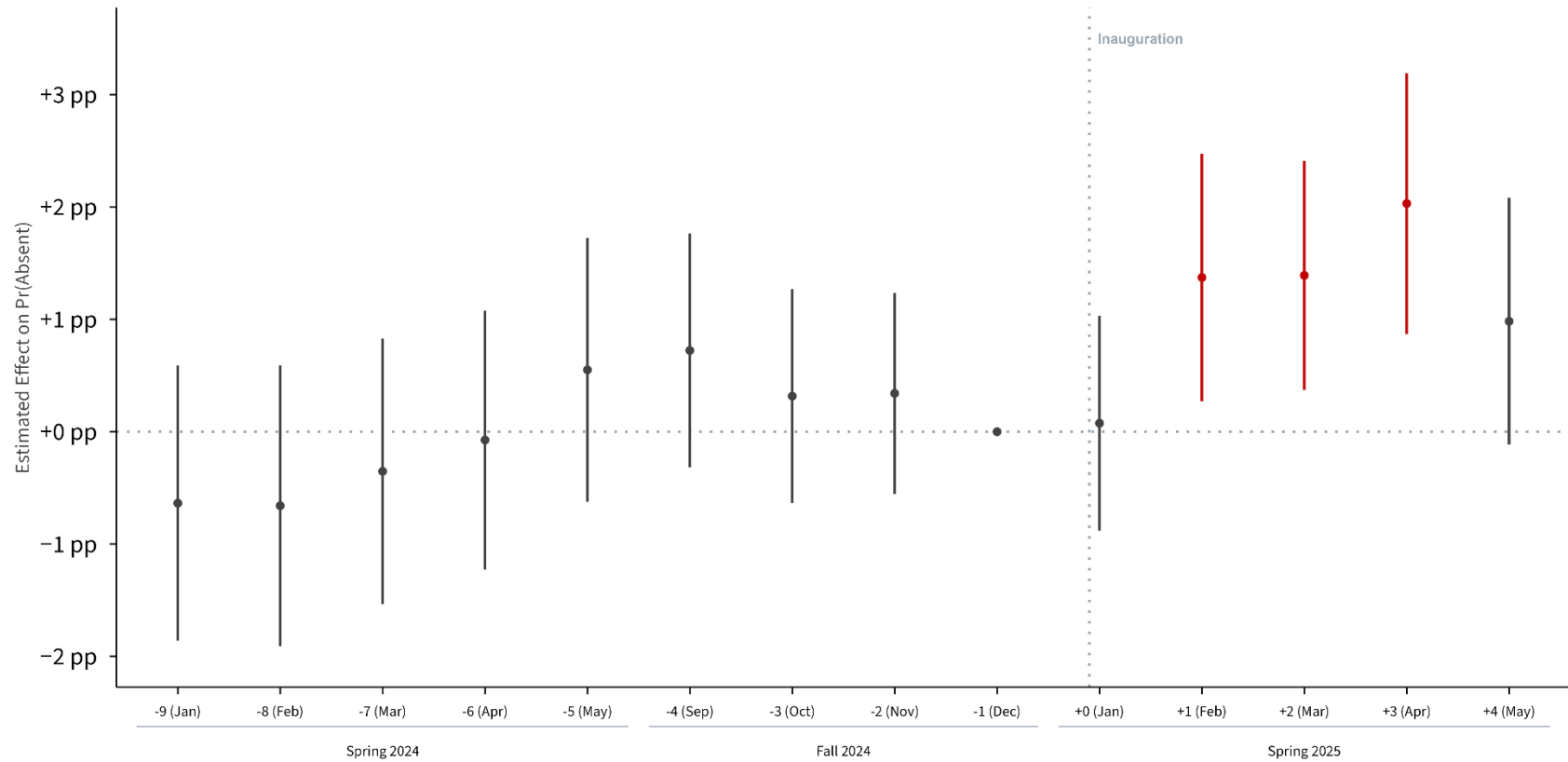
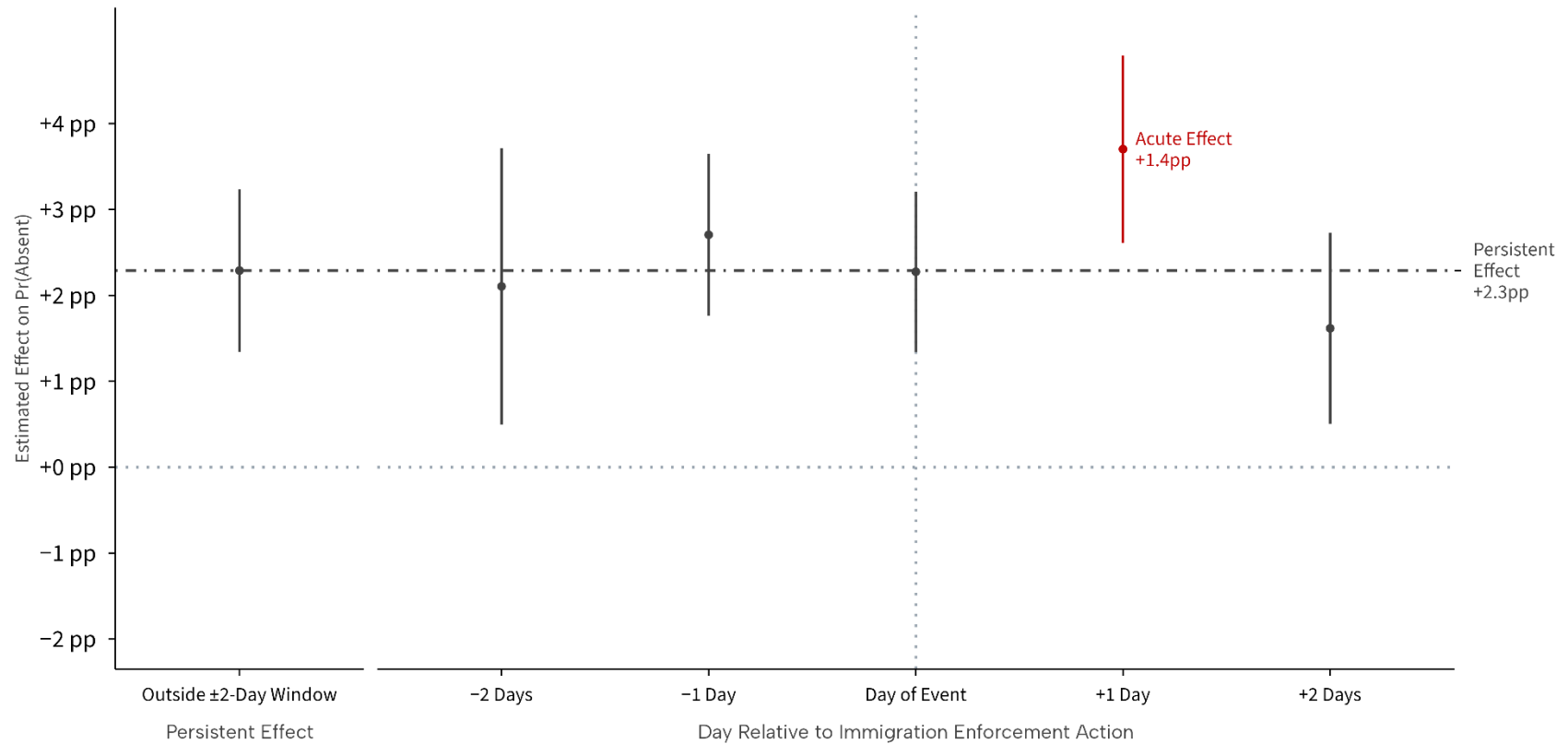


Figure 2 – Event study estimates of effects post-inauguration enforcement climate on student absences.



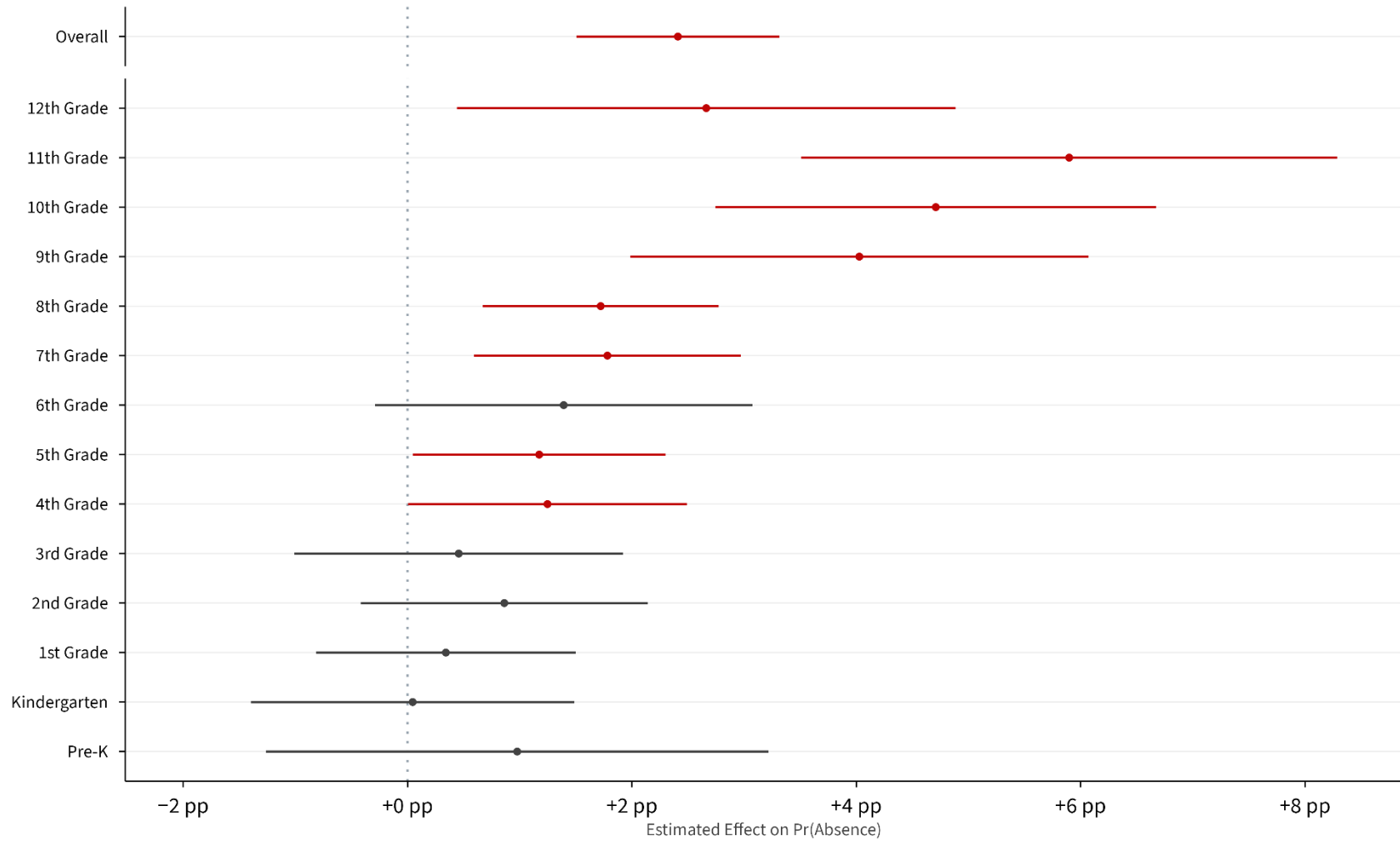
Notes: Estimates are from an event study specification estimated over the full sample (2022-23 through 2024-25 school years). For visual clarity, only coefficients from spring 2024 through spring 2025 are displayed.

Figure 3 – Persistent and acute effects on absences relative to high-profile events, state level.



Notes: Estimates are from Equation (3) using state-level enforcement episodes (11 episodes; see Section 4.3). Points show acute effects on days $k \in \{-2, \dots, +2\}$ relative to an episode; the horizontal reference line shows the persistent effect estimated from post-inauguration days outside event windows. Whiskers denote 95% confidence intervals; standard errors clustered at the student level.

Figure 4 – Effects of post-inauguration enforcement climate on student absences, by grade.



APPENDIX A: SAMPLE COMPOSITION & STUDENT DISENROLLMENTS

One potential concern with the main estimates we present is that immigration enforcement actions 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.

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

To evaluate this possibility, we estimate a simple difference-in-differences model, as specified by Equation A.1, with student disenrollment (e.g., dropping out, transferring to another district, etc.) as our primary outcome. Data for this analysis are at the student-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 then 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.	0.019+ (0.009)
Born Outside U.S. x Post	-0.006 (0.016)
Observations	8,200

*Notes: Estimates are from the disenrollment linear probability model described in Appendix A. Standard errors clustered at the student level. + - $p < 0.10$; * - $p < 0.05$; ** - $p < 0.01$*

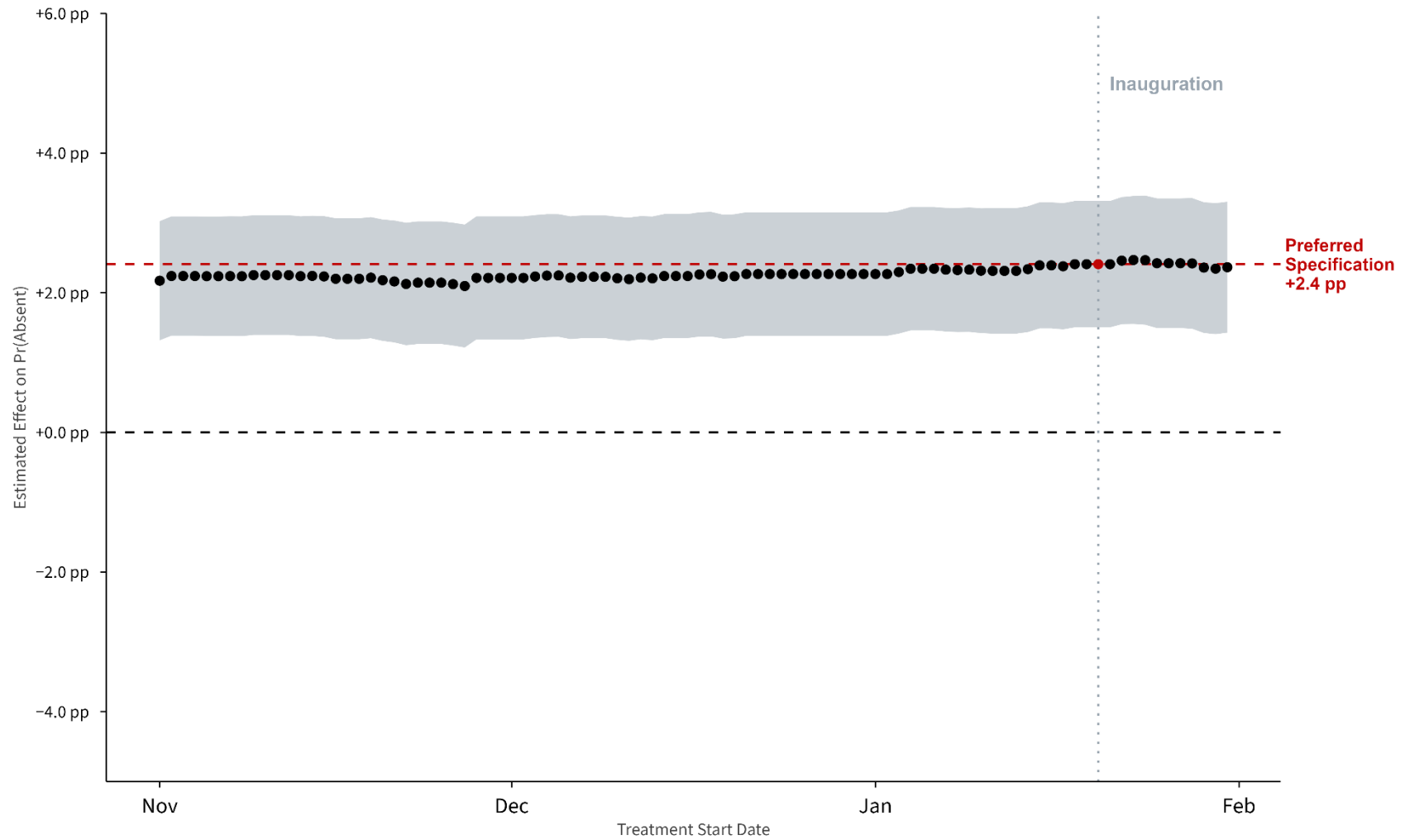
Estimates reported in Table A.1 suggest that differential disenrollment does not explain the attendance effects documented in our main analysis. The coefficient on foreign-born status indicates that, prior to the 2024-25 school year, students born outside the U.S. were slightly more likely to disenroll before the end of the school year than their U.S.-born peers. The coefficient of interest in this analysis, α_2 , is small in magnitude and not statistically significant, indicating that students born outside the U.S. were not more likely to disenroll during the 2024-25 school year as compared to previous years. 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.

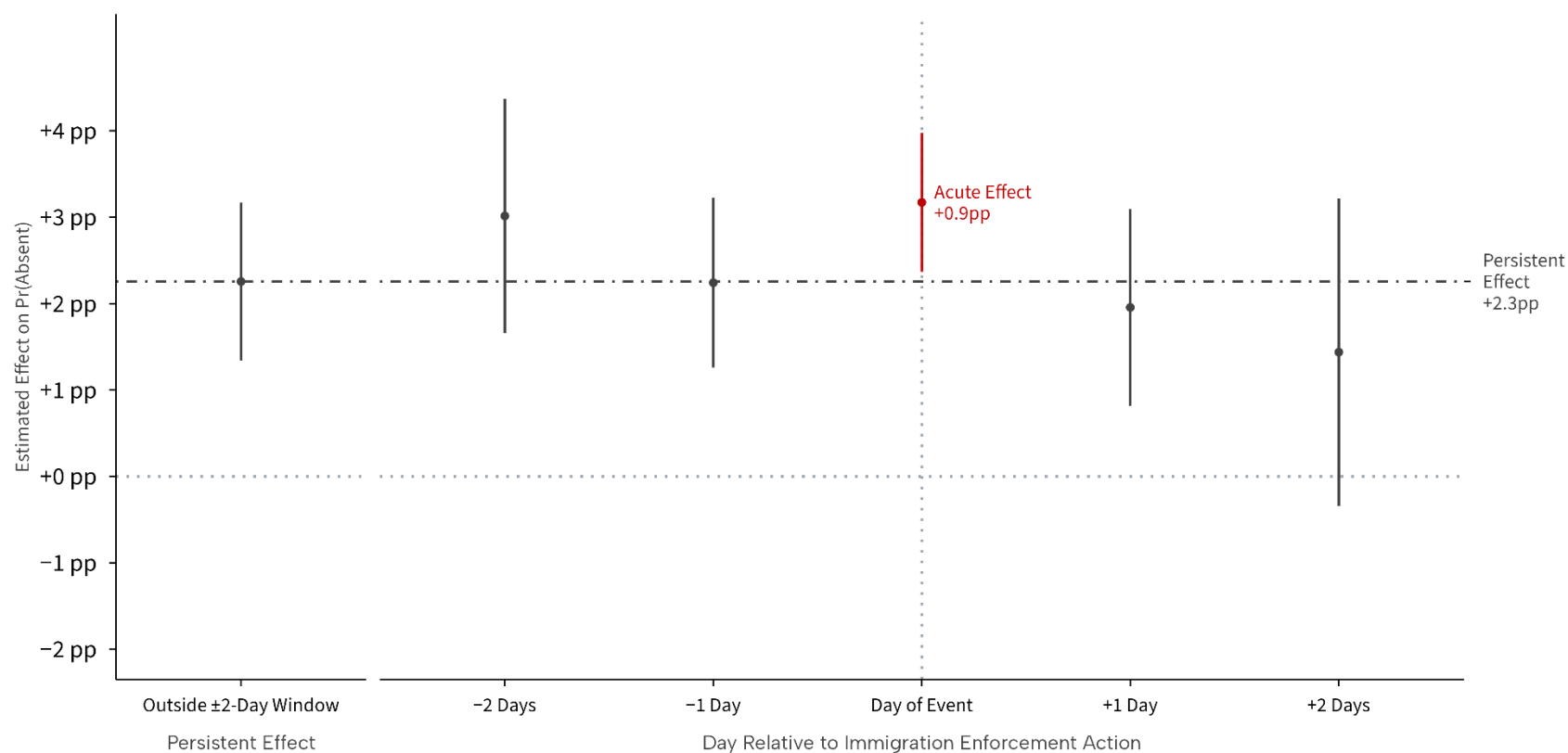
Figure B.1 – Specification curve analysis for sensitivity to treatment start date.



Notes: Each point is the estimate of Equation (1) under an alternative treatment start date between November 1, 2024 and January 31, 2025 (86 specifications). The vertical reference line marks our preferred start date of January 20, 2025.

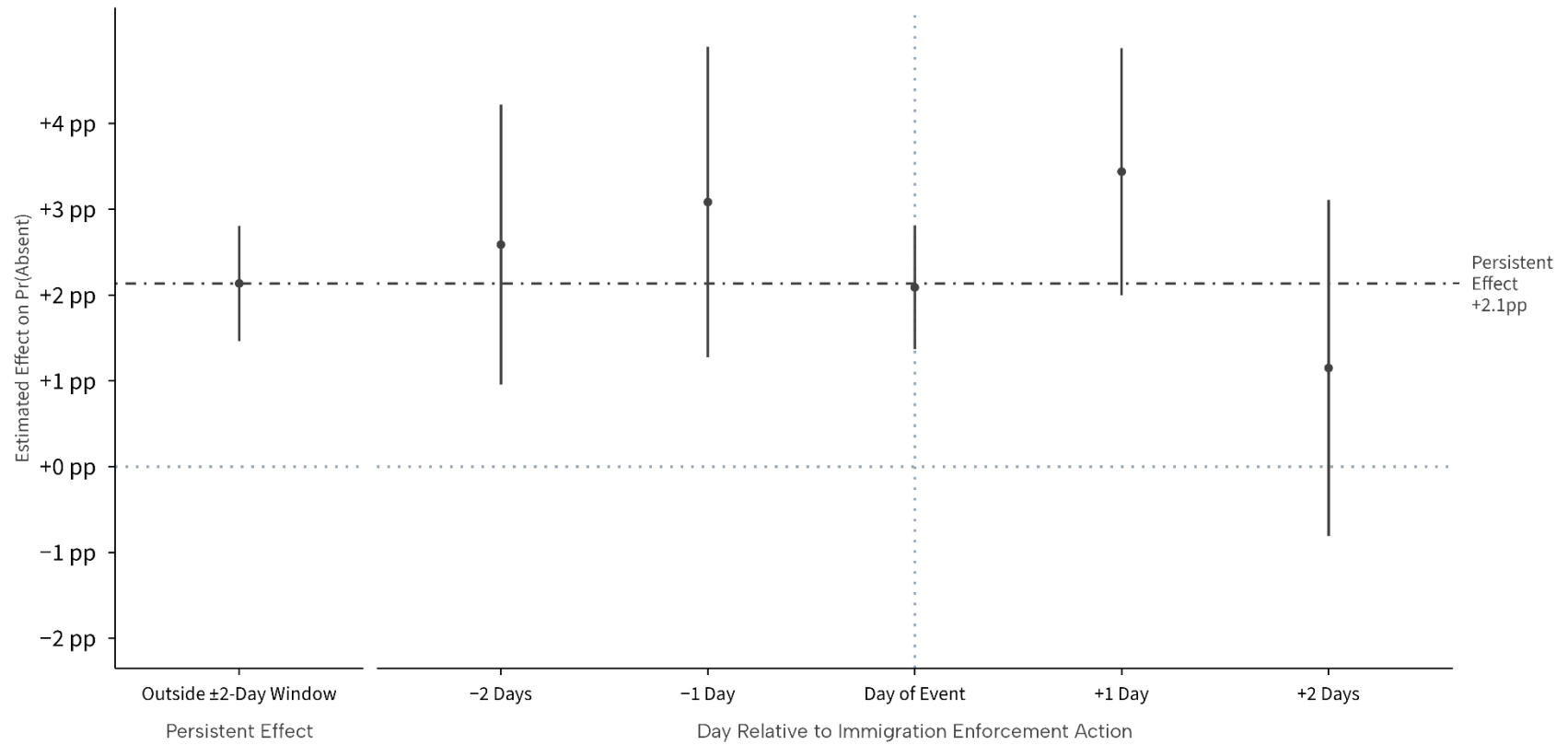
APPENDIX C: PERSISTENT AND ACUTE EFFECTS ON ABSENCES RELATIVE TO HIGH-PROFILE EVENTS, COMMUNITY AND REGION LEVELS

Figure C.1 – Persistent and acute effects on absences relative to high-profile events, community level.



Notes: Replicates Figure 3 using the community-sourced list of enforcement episodes (12 episodes; see Section 4.3).

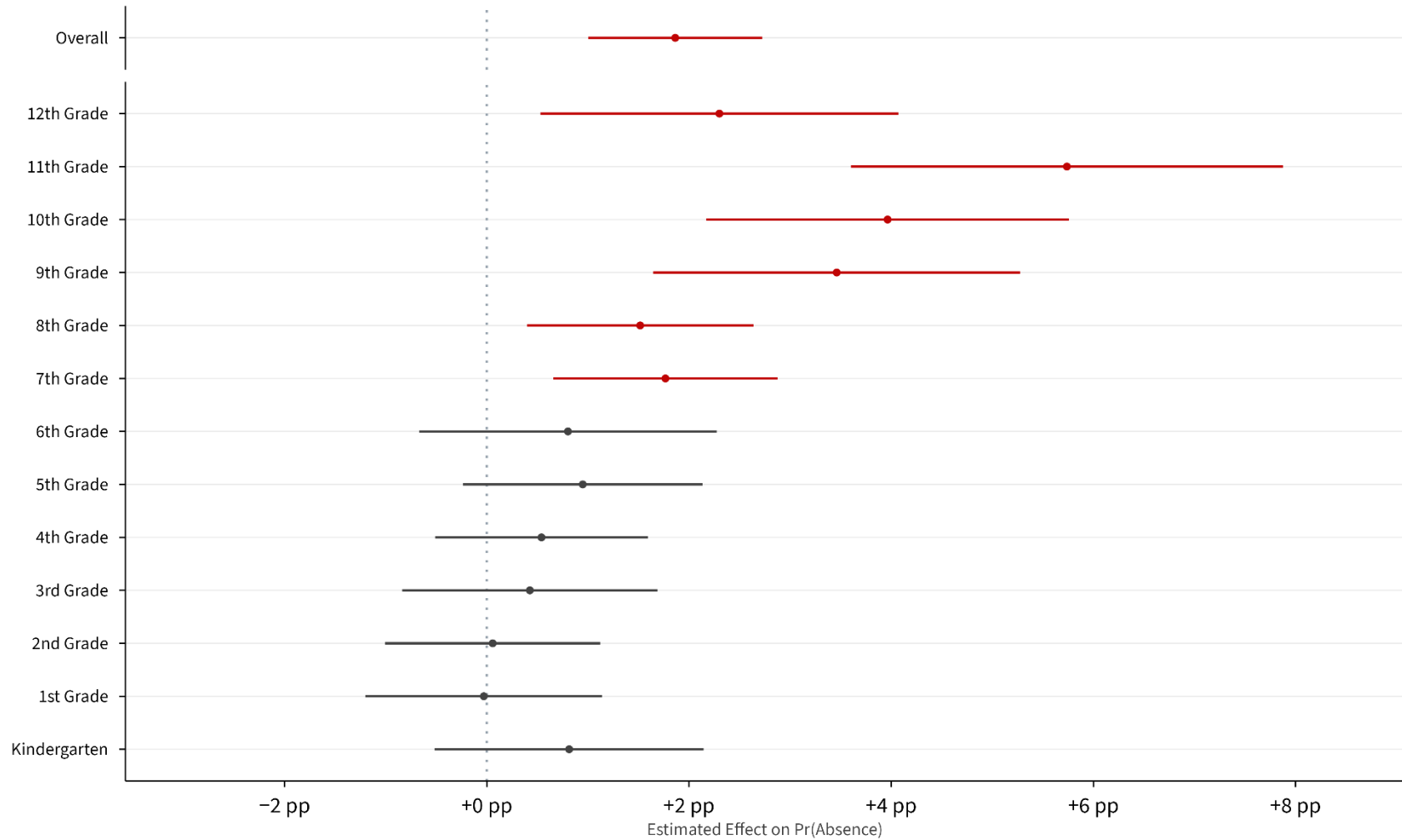
**Figure C.2 – Persistent and acute effects on absences
relative to high-profile events, administrative region level.**



Notes: Replicates Figure 3 using ICE administrative-region enforcement episodes (16 episodes; see Section 4.3).

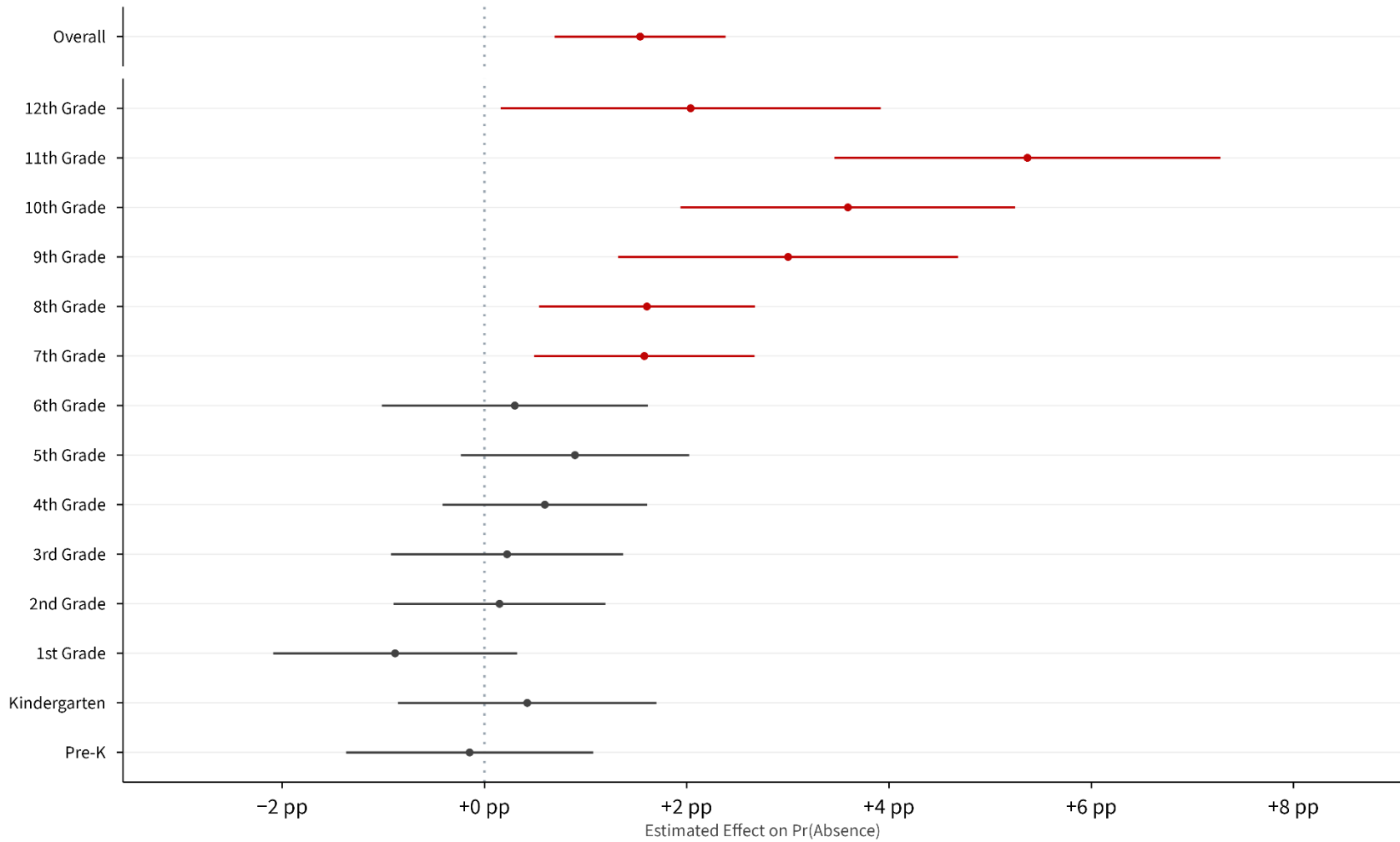
APPENDIX D: EFFECTS BY GRADE LEVEL, ALTERNATIVE TREATMENT DEFINITIONS

Figure D.1 – Effects of the post-inauguration enforcement climate on student absences, by grade; treatment defined by MLL status.



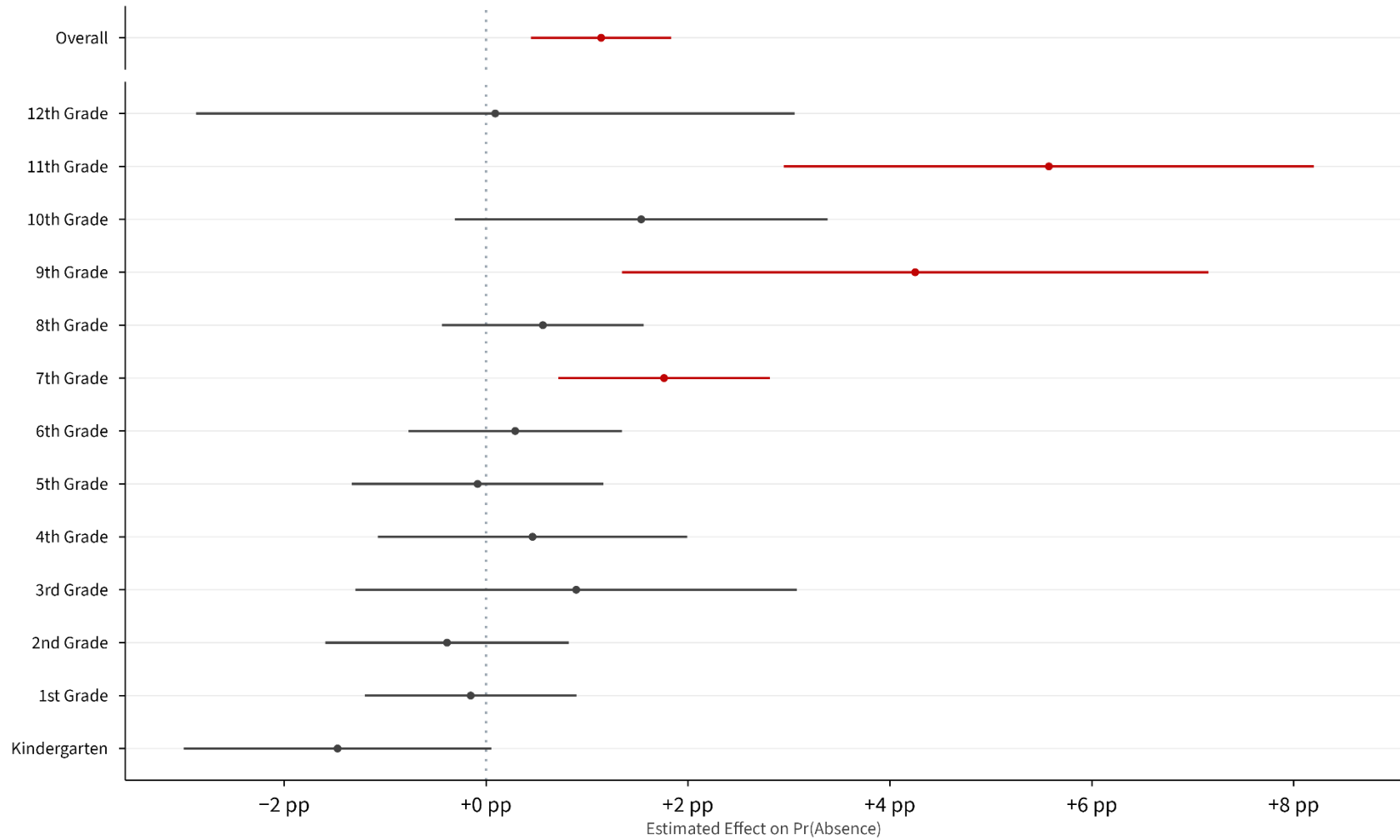
Notes: Replicates Figure 4 with treatment defined by multilingual learner status.

Figure D.2 – Effects of the post-inauguration enforcement climate on student absences, by grade; treatment defined by home language.



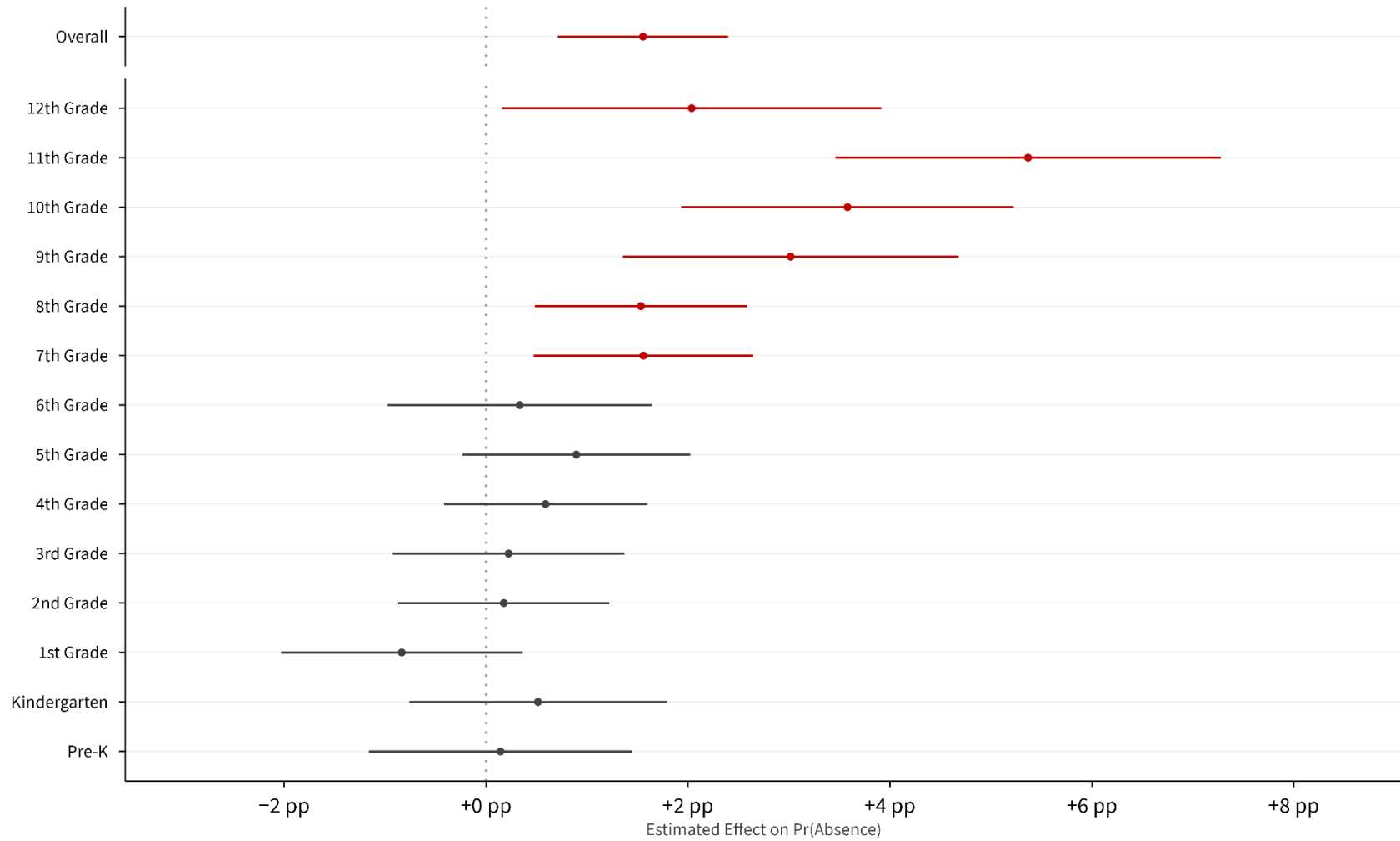
Notes: Replicates Figure 4 with treatment defined by non-English home language.

Figure D.3 – Effects of the post-inauguration enforcement climate on student absences, by grade; treatment defined as recent immigrant.



Notes: Replicates Figure 4 with treatment defined by recent immigrant status.

Figure D.4 – Effects of the post-inauguration enforcement climate on student absences, by grade; ensemble treatment definition.



Notes: Replicates Figure 4 with treatment defined by the ensemble definition (see Table 2 notes).