Institutional Variation in Child Maltreatment Reporting*

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Abstract

Involvement with the child welfare system is a common feature of childhood in the United States and varies widely across places and demographic groups. A central question is whether this variation reflects differences in children's underlying risk of maltreatment or differences in how institutions respond to similar circumstances. We study this question in the context of public schools in Michigan, using student records linked to more than six million reports to Child Protective Services between 2017 and 2024. Across schools, reporting rates vary widely—from virtually none to over half of students being reported at some point between kindergarten and fifth grade. Adapting value-added methods and leveraging student moves across schools, we find that school-specific factors (as opposed to student risk) account for roughly 40 percent of reporting variation across schools. Higher-reporting schools exhibit distinct institutional characteristics, including lower academic value added. Students attending higher-reporting schools are more likely to have reports that are subsequently screened in and substantiated by Child Protective Services. After accounting for differences in overall school quality, we find little evidence that exposure to higher-reporting schools affects students' academic outcomes or attendance. Taken together, our findings underscore the central role of institutional practices in shaping children's exposure to the child welfare system.

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1 Introduction

Involvement with the child welfare system is a common experience for children in the United States, with potentially significant consequences for children and families. By age 18, more than one third of children are subject to a formal child welfare investigation for alleged maltreatment and roughly five percent enter foster care (Doyle, Emanuel and Eyzaguirre, 2025; Kim et al., 2017; Yi, Edwards and Wildeman, 2020). Prior work has extensively documented that rates of child welfare system involvement vary substantially across children who live in different places, as well those from different racial and socioeconomic backgrounds. More than half of Black children are investigated at some point during childhood, and up to ten percent enter foster care. Across states, cumulative childhood investigation risk ranges from 14 percent to 63 percent, while cumulative risk of confirmed maltreatment ranges from 3 percent to 27 percent (Yi et al., 2023).

This variation is also stark at much finer geographic levels. Figure 1 shows the distribution of Child Protective Services (CPS) reporting rates across census tracts in Michigan—the context of this study—for children observed from kindergarten through fifth grade. In some tracts, virtually no children are reported to CPS during elementary school, while in others nearly 70 percent experience at least one report. The magnitude of this local variation highlights that children's experience of child welfare reporting—and the institutions that initiate contact with the system—differs profoundly depending on where they live.

This striking variation also raises a central question: to what extent are differences in children's exposure to the child welfare system due to differences in underlying risk versus differences in how institutions and professionals respond to similar circumstances? In the context of child maltreatment reporting, observed disparities across places and groups may arise because children face different levels of maltreatment risk or because reporters differ in their propensity to contact CPS when concerns arise. Distinguishing between these explanations is essential for interpreting disparities in child welfare involvement and for understanding the role of institutional practices in shaping children's exposure to CPS.

In this study, we focus on identifying the sources of variation in child maltreatment reporting, the initial step of the child welfare process. We conceptualize reporting as the outcome of both demand-side factors—driven by children's underlying risk of maltreatment—and supply-side factors—driven by reporting tendencies, beliefs, discretion, and organizational culture. Our primary contribution is to isolate and quantify the role of supply-side variation in reporting—holding underlying risk constant—and to assess how much differences in reporting across children and places reflect institutional practices rather

than differences in risk.

Disentangling the relative roles of demand- and supply-side factors in child maltreatment reporting has been difficult for two closely related reasons. First, doing so requires comprehensive data that track not only children who are reported to CPS, but also the much larger population of children who could have been reported but were not. Most administrative data in this area are drawn from CPS records and therefore condition on reporting itself, making it difficult to distinguish differences in underlying risk from differences in reporting behavior. Second, children are not randomly assigned to the environments in which reporting decisions occur. As a result, observed differences in reporting rates may reflect selection on underlying maltreatment risk rather than differences in reporters' tendencies, discretion, or institutional practices.[1]

We attempt to overcome both issues by focusing on child maltreatment reporting in public schools. We use administrative student-level data from Michigan public schools—both traditional and charter—linked to child welfare reports made to the Department of Health and Human Services' CPS. The data include all calls made to Michigan CPS between the 2017–18 and 2023–24 school years, including reports that are not screened in for investigation. Our sample includes over six million reports involving students in Michigan public schools.

Public schools are a useful setting for studying this question for at least two reasons. First, a large share of maltreatment reports originate with school staff (17% in our data), and reports from educators are the most common among mandated reporters, making understanding school-based reporting tendencies important in its own right. Second, the school setting allows us to use the rich administrative data from both education and child welfare agencies to control for student risk.

To address the challenge of disentangling student risk from institutional tendencies, we adapt teacher-value-added models (Chetty, Friedman and Rockoff, 2014; Kane and Staiger, 2008) to create an arguably unbiased estimate of the role of school staff in child maltreatment reporting separately from a child's risk of maltreatment. We create residualized measures of reports to CPS from models that control for a rich set of student, school, and neighborhood characteristics, and apply empirical Bayes shrinkage to account for estimation error. Our

[1] A small number of studies have attempted to separate reporting behavior from underlying maltreatment risk in highly controlled settings. For example, Lane et al. (2002) study children hospitalized for serious fractures and use blinded expert assessments to classify the likelihood of abuse based solely on clinical information. Comparing reporting decisions across children with similar injuries and expert-assessed risk, they find that minority children are substantially more likely to be evaluated and reported to CPS. While this approach provides compelling evidence that reporting decisions can vary independently of underlying risk, it is necessarily limited to a narrow medical context and a highly selected sample, making it difficult to assess how institutional reporting practices contribute to population-level variation in child welfare exposure.

setting is well-suited to this approach because the detailed administrative data allow us to control flexibly for students' prior and contemporaneous exposure to the child welfare system, as well as detailed educational trajectories.

Our identification strategy relies on a selection-on-observables assumption followed by a validation exercise using students who move across schools. To begin, we argue that the institutional context bolsters the credibility of the selection-on-observables assumption. Child maltreatment reporting behavior at a school is much less visible to parents than publicly available measures, like average test scores, and parents are more likely to choose schools based on other factors, like student demographic characteristics, distance to home, or academic quality (e.g., Hastings, Kane and Staiger (2006), Bayer, Ferreira and McMillan (2007), Campos and Kearns (2024)). In addition, we conduct falsification tests analogous to those used in mover-based designs that separate supply- and demand-side factors in other institutional contexts (e.g., Finkelstein, Gentzkow and Williams (2016), Chetty, Friedman and Rockoff (2014)). Specifically, we show that when students switch to schools with higher reporting tendencies, reports made by school staff increase sharply, with no evidence of differential pre-trends. In contrast, reports made outside of school (e.g., reports that occurred during the summer) do not change. Together, these patterns indicate that our school reporting tendency measure is forecast-unbiased and that our estimates may capture causal differences in school reporting practices rather than unobserved differences in student risk, allowing us to quantify the relative contributions of supply- and demand-side factors to variation in school reporting.

In what follows, we begin by establishing that there is significant variation across schools in the prevalence of child maltreatment reporting, both overall and by specific demographic and socioeconomic characteristics. We show that 21 percent of students are reported to CPS at least once by the end of fifth grade. Students who are ever reported are disproportionately economically disadvantaged: 86 percent of reported students are economically disadvantaged, compared with 50 percent of students who are never reported. Black students experience particularly high exposure to reporting, with 31 percent reported by fifth grade, compared with 19 percent of White students.

Reporting is not only common but highly uneven across educational settings. In the median elementary school, approximately 28% of students are reported to CPS between kindergarten and fifth grade (Figure A1). At the 10th percentile of the distribution, fewer than 11% of students are reported, while at the 90th percentile, more than 47% of students are reported. Reporting rates also differ by student race: the 10th percentile

of the elementary school distribution for Black students is greater than the median of the distribution for White students. These patterns underscore both the scale of school involvement in child maltreatment reporting and substantial disparities in student exposure, consistent with prior literature documenting disparities at subsequent points of contact with the system (e.g., screened-in reports and substantiated investigations) in other contexts (e.g., Doyle, Emanuel and Eyzaguirre (2025); Kim et al. (2017); Yi, Edwards and Wildeman (2020)).

Next, using our value-added methods validated with student-mover designs, we demonstrate that 42% of this reporting is due to differences in supply-side factors (i.e., reporting tendencies across schools) rather than differences in children's underlying maltreatment risk. The distribution of school reporting tendencies has a standard deviation of 0.0119, implying that attending a school with one standard deviation higher "reports-added" increases a student's probability of being reported by school staff by approximately 1.2 percentage points in a given year, which represents a 47% increase relative to the baseline likelihood of being reported in a given year. Thus, institutional factors meaningfully shape students' exposure to child welfare reporting independent of underlying risk.

Importantly, we find that various institutional features are associated with school reporting tendency measures. We find that higher reporting tendencies are systematically related to observable features of school staffing, resources, and student composition. For example, high-reporting schools are significantly more likely to employ a school nurse, consistent with a greater institutional capacity to identify and respond to concerns about student well-being. At the same time, higher reports-added is negatively correlated with traditional measures of school quality. Schools with stronger reporting tendencies tend to have lower attendance and test score value added, suggesting that reporting tendencies capture dimensions of school practice that are distinct from academic effectiveness.

Finally, we examine the implications of attending higher-reporting schools for students' subsequent outcomes. We do so in two ways. First, we study the downstream outcomes of reports initiated by schools, focusing on whether these reports are screened in for investigation by Michigan's central intake office and whether, if screened in, investigations are substantiated, indicating that investigators determined there was sufficient evidence that maltreatment occurred. While these outcomes are clearly imperfect proxies for the accuracy of the reports, they likely provide some signal about whether higher-reporting schools are identifying cases that CPS ultimately views as sufficiently credible to warrant

investigation or substantiation.[2]

We find that schools with higher reporting value added have modestly lower rates at which their reports are screened in for investigation and subsequently substantiated, consistent with the idea that higher reporting volume may include a greater share of marginal cases near agency thresholds. Importantly, however, these differences in rates are small. As a result, the numbers of screened-in and substantiated cases increase by similar relative magnitudes as overall reporting increases. In other words, higher reports-added does not simply reflect the production of reports that fail to meet CPS thresholds; higher-reporting schools also identify a meaningful number of cases that proceed to investigation and substantiation.

Second, we examine academic outcomes for students in kindergarten through fifth grade who attend schools with higher reporting value added. These estimates compare students facing different institutional reporting tendencies, holding underlying risk constant. In specifications that control only for student and neighborhood characteristics, higher reporting tendencies are associated with lower attendance and test scores. However, as discussed above, higher-reporting schools differ systematically along other supply-side dimensions, including academic effectiveness. Once we account for these differences—most notably schools' test score value added—the associations between reporting tendencies and academic outcomes attenuate substantially and, in the case of attendance, flip direction. In the fully controlled specifications, which condition on school academic effectiveness and students' prior outcomes, the estimated effects on attendance and test scores are small in magnitude and generally statistically indistinguishable from zero. These results indicate little evidence that the additional reporting associated with higher-reporting schools has adverse effects on students' academic performance or attendance, above and beyond differences in overall school quality. This pattern holds broadly across students with different baseline risks of child welfare involvement.

Conceptually, our approach parallels a broader literature on institutional variation in the provision of services. Our framework is closely related to research, originating in health economics (e.g., Finkelstein, Gentzkow and Williams (2016), Badinski et al. (2023), Doyle and Staiger (2025)), that decomposes variation in service utilization into demand-side factors driven by individual characteristics and supply-side factors driven by individual and institutional practices and organizational culture. By bringing these ideas to the study of child maltreatment reporting, we provide new evidence on how institutional environments shape children's exposure to the child welfare system.

[2]There is well-documented variation in caseworker discretion and thresholds for screening, substantiation, and removal decisions (e.g., Doyle (2007), Bald et al. (2022), Baron and Gross (2024), Baron et al. (2024)).

Our findings also contribute to the literature on whether disparities in child welfare system engagement are driven by differences in supply-side factors, like reporting tendencies, biases, beliefs, or organization culture, or demand-side factors (e.g., Baron et al. (2024); Drake et al. (2023); Rittenhouse, Putnam-Hornstein and Vaithianathan (2022)). Our estimates suggest that supply-side factors play a significant role in these disparities at the reporting stage.

This study is also related to the growing literature demonstrating how schools affect children's lives in many ways beyond academic instruction. Prior research shows that schools differ substantially in their disciplinary practices, their effects on arrest and teen pregnancy, and other non-academic outcomes, with these impacts often weakly correlated with schools' effects on test scores (Beuermann et al., 2022; Chetty, Friedman and Rockoff, 2014; Cullen, Jacob and Levitt, 2006; Jackson, 2018; Jackson et al., 2020; Rose, Schellenberg and Shem-Tov, 2025). We show that schools may also differ systematically in how they engage with child welfare systems—a common and high-stakes point of institutional contact for children and families—and that this variation is driven in large part by supply-side institutional practices that are not well captured by standard measures of school quality.

Finally, our findings contribute to active policy debates in child welfare, including those surrounding mandated reporting laws. School staff constitute one of the largest groups of mandated reporters and are legally required to report suspected child maltreatment to CPS, making schools a central entry point into the child welfare system. Recent policy discussions have raised concerns that existing reporting regimes may generate unintended consequences, including over-reporting, racial and socioeconomic disparities in CPS involvement, and intrusive investigations that ultimately do not substantiate maltreatment (Ho et al., 2017; McTavish et al., 2017). Reflecting these concerns, several states have recently pursued or enacted reforms aimed at modifying mandated reporting policies, including clarifying statutory definitions of abuse and neglect, expanding and updating training requirements for educators, and piloting alternative reporting pathways—such as "warmlines"—that connect families to supportive services without automatically triggering formal CPS investigations (Child Welfare Information Gateway, 2023; New York State Legislature, 2024).

Understanding the welfare implications of the disparities that we document is an important question for future research. Nonetheless, our findings provide several pieces of evidence that are relevant for evaluating these debates. First, attending a school with higher reporting value-added increases not only the number of reports, but also the number of screened-in and substantiated cases, suggesting that higher-reporting schools are not merely generating noise. Second, after accounting for differences in overall school quality,

we find little evidence that exposure to higher-reporting schools adversely affects students' academic outcomes or attendance, including among students at elevated baseline risk of child welfare involvement.

More broadly, our analysis establishes that school reporting tendencies constitute an important institutional margin shaping children's exposure to the child welfare system and provides a framework for measuring these tendencies in administrative data. This creates a foundation for future work to assess welfare more directly—for example, by distinguishing whether variation in reports-added reflects differences in reporting thresholds versus differences in diagnostic skill (Chan, Gentzkow and Yu, 2022).

2 Background

A family's involvement with the child welfare system typically begins when an individual files a report of suspected child maltreatment with CPS. Reports may be made by mandated reporters—such as educators, physicians, and law enforcement officers—as well as by family members and other community members. Once a report is filed, CPS determines whether to take further action, which may include an investigation, the provision of services, or, in some cases, temporary or permanent placement of a child outside the home. While the core structure of this process is broadly similar across the United States, specific procedures and organizational features vary across states.

Figure 2 summarizes the CPS process in Michigan, the context of this study. When individuals suspect child abuse or neglect, they contact a statewide CPS hotline. Calls are routed to one of two centralized intake centers, located in Detroit and Grand Rapids. Calls are assigned quasi-randomly to screeners who are on queue at the time, with no discretionary reassignment. Intake screeners collect information about the child and the alleged maltreatment and determine whether the report meets the statutory threshold for investigation. Calls last approximately 15 minutes on average; roughly 40 percent of calls are screened in. Reports that are screened out receive no further CPS action. Reports that are screened in are referred to a local county child welfare office for investigation, typically within 24 hours, and investigations must be completed within 30 days. Each county operates its own local office, and investigations are assigned to caseworkers on a rotational basis.

During the investigation, the caseworker assesses whether there is sufficient evidence to substantiate the alleged maltreatment. Approximately one quarter of investigations result in substantiated findings. Substantiation decisions are based on the evidence

available in the case rather than on assessed risk, meaning that serious allegations may remain unsubstantiated while less severe allegations may be substantiated. In addition, substantiation practices vary substantially across investigators and counties in Michigan (e.g., Baron and Gross (2024)), making substantiation an imperfect proxy for both report accuracy and underlying risk. Nevertheless, unsubstantiated investigations typically conclude CPS involvement. Conditional on substantiation, the caseworker further evaluates the child's ongoing risk in the home. A subset of cases are classified as high risk and proceed to court involvement. Approximately three percent of all investigations result in foster care placement, generally reflecting cases in which children face imminent risk of maltreatment.

2.A Reporting by School Staff

School staff are mandated reporters, meaning they are legally required to report suspected child maltreatment to CPS.[3] Our data, described below, identify the role of the reporter, distinguishing reports filed by educational personnel from those filed by law enforcement, medical professionals, family members, and others. Schools may also initiate reports indirectly—for example, by advising a family member or a police officer to contact CPS—but such indirect involvement cannot be observed in the administrative data.

School-based staff constitute the largest group of mandated reporters, accounting for 17% of all reports (Figure 3); only family members submit reports more frequently. Within schools, teachers and school counselors each account for approximately one-third of school-based reports.

Figure 4 shows that reports filed by school staff are screened out at higher rates than reports filed by law enforcement, though only modestly more often than reports filed by medical professionals. Sixty-one percent of education-based reports are screened out at intake, compared with 47% of reports filed by law enforcement and 58% of reports filed by medical professionals. Among reports that proceed to investigation, a smaller share of school-based reports are ultimately substantiated: 7% are substantiated, compared with 13% of reports filed by medical professionals and 28% of those filed by law enforcement.

[3]In Michigan, other mandated reporters include medical professionals, law enforcement officers, family court and welfare employees, clergy, and social workers.

3 Data

3.A Data Sources

We use administrative student-level data from Michigan public schools—both traditional and charter—linked to records of child welfare reports made to the Michigan Department of Health and Human Services (DHHS) Child Protective Services (CPS). The CPS data include all calls made between the 2017–18 and 2023–24 school years, including reports that are not screened in for investigation. Importantly, these data identify the role of the reporter, allowing us to distinguish reports filed by school-based staff from those filed by other reporters. The linked sample includes over 2.2 million CPS reports involving students enrolled in Michigan public schools. A substantial share of CPS reports occur when children are below school age. In our analyses of school reporting tendencies, we focus on reports involving elementary school students and use reports made at younger ages to control for children's underlying risk of maltreatment.

We use CPS data from years prior to 2017 to measure students' prior exposure to the child welfare system. For these earlier years, the data include only reports that were screened in for investigation and do not identify the reporter's role. Nonetheless, they allow us to control for prior CPS investigations, including those occurring before kindergarten entry, when estimating school reporting tendencies. Also, we exclude reports made during the COVID-19 pandemic (March 2020 through June 2021), when many Michigan schools operated fully or partially remotely.

3.B Matching Across Data Systems

The Michigan Education Data Center (MEDC) linked CPS records to Michigan public school administrative data using a probabilistic record linkage algorithm. Because the CPS and education datasets do not share a common unique identifier, MEDC staff conducted the linkage using students' first and last names and dates of birth, relying on the most recent personally identifiable information available in statewide K–12 enrollment records, which serve as the base population for the match.

The matching procedure assigns a posterior probability to each potential match, reflecting the model's confidence that two records correspond to the same individual. MEDC applied post-processing rules and manual review to remove likely false positives, particularly in cases where name similarity was high but dates of birth differed meaningfully. Overall, 75.6 percent of CPS records that passed basic validation were successfully matched to a Michigan public

school student record.

This match rate is consistent with expectations, as CPS records include children who never enroll in Michigan public schools (e.g., attend private schools or are homeschooled) over the course of the time period we observe. Match rates are substantially lower for students reported at young ages in recent years because many will not have entered the public school system (typically in kindergarten) by the end of our sample period. The resulting linked dataset provides comprehensive coverage of CPS involvement for children observed in Michigan public schools and forms the basis for our analysis.

3.C Analysis Sample

To construct our analysis sample, we start with all students observed in kindergarten through fifth grade in a Michigan public school between the 2017–18 and 2023–24 school years. Because the COVID-pandemic substantially altered schooling environments and child welfare reporting, we exclude reports made between March 2020 and June 2021 when estimating school reporting tendencies. This completely eliminates data from the 2020-21 school year and means that our estimates for the 2019-20 school year are based on six rather than nine months of data.

The resulting analysis sample consists of 3,642,261 student-year observations covering 1,232,220 unique children. These children are involved in 872,071 CPS reports while enrolled in grades K–5 and an additional 619,748 reports occurring outside the elementary school years, which we use to characterize prior individual child welfare system exposure.

3.D Descriptive Statistics

Table 1 describes the sample of students used to to estimate the value-added models which will identify school reporting tendencies. Eleven percent of students experienced at least one CPS investigation prior to entering kindergarten, and 21% experienced an investigation by the end of fifth grade.[4]

By the end of fifth grade, 9% of students in the analysis sample had experienced at least one substantiated investigation. There are substantial disparities in these rates: 31% of Black students were the subject of a CPS investigation and 13% experienced a substantiated investigation by fifth grade, compared with 19% and 8%, respectively,

[4]These rates are lower than those shown in Figure A1 because the figure follows students observed continuously from kindergarten through fifth grade, whereas the table includes students observed for fewer grades or years.

among White students. Students who were investigated by the end of fifth grade are also disproportionately economically disadvantaged, more likely to receive special education services, and more likely to be chronically absent from school.

Table 2 shows the school and neighborhood characteristics for the same sample of students. Students who are ever reported to CPS attend schools with below-average performance on state standardized reading and math exams, and 74% attend Title I schools, which receive additional federal funding due to high concentrations of students from low-income families. Students who are investigated by CPS also tend to live in lower-income neighborhoods with lower marriage rates.

3.E Variation Across Schools in Reporting

The share of students ever reported to CPS during elementary school varies widely across Michigan schools. In Figures A1, 5, and 6, we assign each student to the school they attended in kindergarten and calculate, for each school, the fraction of students who received a CPS report between kindergarten and fifth grade.[5] At the median school, 9% of students receive at least one CPS report *filed by school staff* prior to entering middle school (Figure 5). At the 90th percentile of the distribution, this share rises to 20%, while at the 10th percentile, it falls to 3%.

These patterns vary across demographic subgroups. At the 90th percentile of the distribution, 21%, 25%, and 27% of White, Black, and economically disadvantaged students, respectively, are reported to CPS by school staff between kindergarten and fifth grade. Similar disparities are evident for reports filed by non–school-based reporters (Figure 6): at schools at the 90th percentile, 38% of White students receive a non-school-based CPS report during elementary school, compared with 50% of Black students and 48% of economically disadvantaged students.

Our goal in this study is to determine how much of this variation reflects differences in school reporting practices rather than differences in the underlying risk of maltreatment among students enrolled at a school. Figure 7 provides suggestive evidence that schools with higher rates of school-based reporting also serve students at higher underlying risk. Specifically, rates of reporting by school staff are positively correlated with rates of reporting by non–school-based reporters, with a correlation coefficient of 0.72. This imperfect relationship indicates that some of the observed variation in school-based reporting

[5]We restrict the sample to students who entered kindergarten in 2017 or 2018, allowing us to observe them through the end of fifth grade.

may reflect differences in school behaviors that operate independently from student risk. We therefore turn to isolating this variation in school reporting tendencies.

4 Methods for Identifying School Reporting Tendencies

Our goal is to obtain unbiased estimates of each school's reporting "tendency," defined as the school's propensity to report students to CPS independently of students' underlying risk of maltreatment. The ideal experiment we seek to approximate is one in which students are randomly assigned to schools, allowing us to ask whether a given child would be more likely to be reported to CPS if she attended school A rather than school B.

A naive approach would measure a school's reporting tendency using its average number of reports per student. However, as documented above, schools serve populations of students who differ systematically in their underlying risk of maltreatment. As a result, simple school-level reporting rates conflate student-level risk factors with school-level reporting behavior and cannot separately identify differences in reporting practices across schools.

We adapt methods from the teacher value-added literature to isolate school-level reporting behavior (Chetty, Friedman and Rockoff, 2014; Kane and Staiger, 2008). In their standard application, these approaches estimate teacher effects by taking average teacher-level residuals from regressions of student outcomes—such as test scores—on a rich set of student and classroom characteristics, and then shrinking these estimates toward the mean based on their statistical precision. These estimators fall within the class of empirical Bayes methods and, under random assignment of students to teachers, yield the best linear unbiased predictors of teacher effects on student outcomes (Morris, 1983).

Although this approach relies on stronger assumptions than standard quasi-experimental designs, we believe a selection-on-observables strategy is plausible in this context. First, the scope for student sorting based on schools' reporting behavior is likely smaller than sorting based on academic performance. In almost all cases, child maltreatment reports are anonymous and confidential, meaning that families generally cannot observe whether school staff have reported other children to CPS, let alone distinguish reports filed by school personnel from those filed by other professionals in the community.

By contrast, families frequently consider observable measures of school quality—such as school quality and student body composition—when choosing schools (Abdulkadiroğlu et al., 2020; Bayer, Ferreira and McMillan, 2007; Campos and Kearns, 2024). Measures that are less visible, such as differences in CPS reporting practices, may play a more limited role

in school choice decisions. As a result, it is unlikely that families systematically select into schools based on unobserved differences in reporting practices.

Second, and perhaps more importantly, our data include a rich set of student-, school-, and neighborhood-level characteristics that help mitigate concerns about selection on unobservables. We control for children's prior involvement with the child welfare system and for neighborhood-level reporting from non–school-based reporters, which serve as proxies for baseline maltreatment risk and for contemporaneous, location-specific shocks to risk, respectively. Because there may be factors unobservable in administrative education data that may drive variation in reporting rates across schools, we also make use of the comprehensive data to provide validity tests below that support the argument that the main identifying assumptions of our approach may hold in this context.

We estimate static measures of school-level child welfare "reports-added" following the approach and notation in Kane and Staiger (2008). For student i attending school j in year t, we model the propensity to receive a school-based child welfare report, R^*_{ijt}, as a function of a vector of student-, school-, and neighborhood-level covariates X_{ijt}; a school-specific reporting tendency, ν_j; a school-year shock, θ_{jt}; and an idiosyncratic student-year shock, ε_{ijt}:

$$R^*_{ijt} = X'_{ijt}\beta + \nu_j + \theta_{jt} + \varepsilon_{ijt}. \tag{1}$$

Our object of interest is ν_j: the effect of school j's reporting tendencies on child welfare reports received by students at that school.

We first construct residualized measures of child welfare reporting, $\hat{R}_{ijt}$, for each student-year by partialling out observed covariates:

$$\hat{R}_{ijt} \equiv \nu_j + \theta_{jt} + \varepsilon_{ijt} = R^*_{ijt} - X'_{ijt}\hat{\beta}. \tag{2}$$

Table A1 lists the full set of covariates included in X_{ijt}. Briefly, we control for student demographic characteristics—including race/ethnicity, gender, economic disadvantage, and English Language Learner and special education status—as well as demographic characteristics of the student's home Census tract. We additionally control for leave-out school-level averages of these student and neighborhood characteristics, along with school urbanicity. Lastly, we include county-by-year fixed effects to account for reporting and investigation tendencies that vary across CPS offices.

Importantly, we control for students' prior exposure to the child welfare system by including lagged counts of CPS reports and substantiated investigations received by student i

in previous years, including years prior to kindergarten entry. These lagged measures include reports filed not only by school personnel, but also by family and community members, law enforcement, and medical professionals.

We further include a control for the leave-out average number of *non–school-based* reports among students attending the same school in the prior year, defined as the average number of reports per student filed by non-school-based reporters during the preceding 12 months (including the summer). This variable captures underlying aspects of student risk and community reporting behavior that operate independently of school reporting tendencies. Using lagged non-school-based reports also mitigates concerns about contemporaneous shocks that may jointly affect school- and non-school-based reporting.

We do *not* include school-level covariates that might be correlated with any "supply-side" determinants of reporting behavior. For example, we do not include measures of support staff (e.g., nurses, social workers) who work in a particular school or characteristics of the educators in the school (e.g., race or gender of teachers or administrators). We later explore whether any of these factors are correlated with our measures of school reporting tendency.

The regressions include school fixed effects to account for correlations between unobserved, time-invariant school characteristics and reporting outcomes.[6] However, we exclude these fixed effects when constructing the residuals in equation (2). As a result, the residualized outcome $\hat{R}_{ijt}$ decomposes into a persistent school-level reporting component ν_j, a transitory school-year shock θ_{jt}, and an idiosyncratic student-year shock ε_{ijt}.

If a student has been reported to CPS previously by staff in the same school, educators may respond differently to subsequent concerns involving that same student. Including later reports for the same student in the same school could therefore complicate the interpretation of school reporting tendencies. Accordingly, we estimate "hazard" models in which we exclude all student-year observations following a student's first CPS report at a given school.[7]

We assume that the unobserved components of reporting, $\{\nu_j, \theta_{jt}, \varepsilon_{ijt}\}$, are uncorrelated. Following Kane and Staiger (2008), we construct a school's reports-added as

$$VA_j = \bar{\nu}_j \left(\frac{\hat{\sigma}^2_\nu}{\text{Var}(\bar{\nu}_j)} \right),$$

where $\bar{\nu}_j$ denotes the average residualized reporting outcome for school j. This estimate is "shrunken" based on its reliability as a measure of the true school-level reporting tendency,

[6] We include school fixed effects in order to estimate the coefficients on the other predictors in the model, conditional on unobservable time-invariant school factors that might otherwise bias these estimates.

[7] If the student switches schools, we include observations from the new school.

ν_j.

Reliability is defined as the share of the total across-school variance in average residual reports, $\text{Var}(\bar{\nu}_j)$, that is attributable to variance in true school reporting tendencies, $\hat{\sigma}^2_\nu$. In practice, we estimate $\hat{\sigma}^2_\nu$ using the covariance of $\bar{\nu}_j$ across adjacent years. The year-to-year correlation in residual reports per school is approximately 0.56, indicating substantial persistence in school reporting behavior over time.

5 Results

5.A Estimates of Reporting Value-Added

Figure 8 presents the distribution of estimated school-level reporting value added (reports-added) under three specifications. The first panel reports estimates based solely on school staff reports with no additional controls. The second panel adds student, school, and neighborhood controls, while the third panel further incorporates controls for students' prior exposure to the child welfare system through lagged reports. Across all three specifications, the distribution of school reporting value added shows substantial and persistent heterogeneity in reporting tendencies across schools that is not explained by observable differences in student populations. Including controls also meaningfully changes the school's estimated reports-added: the correlation between reports-added with no controls and all controls is 0.56.

In our preferred specification, which includes the full set of controls, the distribution has a standard deviation of 0.0119. Attending a school with reporting value added one standard deviation above the mean increases a student's probability of receiving a CPS report from school staff by approximately 1.2 percentage points in a given year, corresponding to a 57 percent increase relative to the sample mean reporting probability of 2.1 percent.

5.B Validation of Reports-Added Estimates

We validate our reports-added estimates by examining whether students' reporting outcomes change when they move between schools with different reporting tendencies. We begin by estimating event-study models that compare the evolution of school-based reports for students who switch schools during elementary school:

$$\widehat{R}_{i,g,t} = \sum_{k \in \{-3,\ldots,3\}} \beta_k \, \text{event}^k_{i,t} \times \Delta\widehat{VA}_{j(1),j(0)} + \Delta\widehat{VA}_{j(1),j(0)} + \gamma_g + \gamma_t + \varepsilon_{i,g,t}. \tag{3}$$

The dependent variable, $\widehat{R}_{i,g,t}$, indicates whether student i received a school-based CPS report in year t, residualized for observable characteristics as in Equation 1. We estimate this regression on a sample of approximately 106,000 students who switch schools exactly once during elementary school, in either second or third grade.[8]

The variable $\Delta\widehat{VA}_{j(1),j(0)}$ denotes the change in reporting value added between the student's post-switch school and pre-switch school. To avoid any mechanical correlation between the left- and right-hand-side variables, we re-estimate reporting value added using only students who never switch schools. The resulting measure is highly correlated with our baseline estimate (correlation = 0.913).

By interacting changes in reporting value added with event-time indicators, this specification allows us to assess whether students' likelihood of being reported changes upon moving to schools with stronger reporting cultures, and whether students who move to higher-reporting schools exhibit differential pre-move trends.[9] We include grade and year fixed effects, as well as the main effect of $\Delta\widehat{VA}_{j(1),j(0)}$, to absorb time-invariant differences in reporting outcomes correlated with moves to higher-reporting schools.

The left panel of Figure 9 presents the event-study estimates based on residualized reports. In the year of the move, the estimated coefficient is 1.03 and statistically indistinguishable from one, indicating that students who move to schools with higher reporting value added experience a proportional increase in their own likelihood of being reported. The coefficients on the pre-move indicators are small and statistically insignificant, providing no evidence of differential pre-trends prior to switching schools.

For comparison, the right panel of Figure 9 presents event-study estimates where the dependent variable is an indicator for receiving any school-based report that year, without any risk adjustment. The independent variable is the school's average reporting rate, shrunken using the same empirical Bayes procedure. In this case, the estimated coefficient in the first post-move year is 0.42. The difference between this estimate and the risk-adjusted estimate reflects the contribution of underlying student risk to reporting outcomes. Interpreted jointly, these results suggest that approximately 42 percent of the variation in school-based reporting reflects school-level reporting practices, while the remaining 58 percent reflects differences in student risk.

[8]We restrict to second and third grade switchers in order to follow all students for at least three years post-switch, but show results are similar when we include fourth-grade switchers (Figure A2). Results are also similar if we include student fixed effects (Figure A2) or include students who never switch schools, where we set event-time interactions to zero for never-switchers (Figure A3).

[9]We do not restrict to the hazard sample in these models, since that sample mechanically contains no reports prior to event time $t-1$.

We further assess this relationship using binscatter plots that relate changes in students' reporting outcomes to the magnitude of changes in reports-added induced by school moves (Figure 10). Panel A of the figure shows that, for residualized reports, the relationship is approximately linear, with a slope of 1.008, supporting the assumption that reporting outcomes can be modeled using additively separable school and time effects (Finkelstein, Gentzkow and Williams, 2016). Panel B shows the analogous relationship for unadjusted reports, which is also approximately linear, with a slope of 0.458—closely matching the corresponding event-study estimate.

Finally, we test whether reports-added is picking up reporting behaviors present in the broader community, for example among local law enforcement or medical professionals, rather than at the school itself. Specifically, we examine changes in reports received during the summer preceding the school year from reporters not affiliated with schools. As shown in Panel A of Figure 10, the relationship between changes in reports-added and summer reporting is flat: the estimated slope is 0.018 with a standard error of 0.055. Figure A4 shows that the same pattern holds in the event-study specification. This suggests that our reports-added estimates capture school-specific factors that are uncorrelated with community reporting norms.

To summarize, we show that students who move to schools with higher reporting cultures receive more reports from school staff immediately after switching, and that this relationship is linear. Because the change in reports is statistically indistinguishable from one, this means that our estimates of reports-added are "forecast-unbiased" (Chetty, Friedman and Rockoff, 2014). These reporting behaviors are also specific to the school and do not reflect broader community reporting norms. Overall, reporting behaviors are responsible for 42 percent of the variation in student reports across schools.

5.C Characteristics of High-Reporting Schools

Table 3 describes bivariate associations between school characteristics and school-level reporting behavior. Specifically, we regress average school-year residual reports on individual school "supply-side" characteristics—such as staff demographics, resources, and measures of school performance—one at a time. We use residual reports rather than reports-added estimates as the outcome because value-added estimates are unbiased predictors but not unbiased estimates of true school effects (Jacob and Rothstein, 2016). Column 1 reports sample means, while Column 2 presents coefficients from bivariate regressions that include county-by-year fixed effects.

Several broad patterns emerge from the bivariate associations in Table 3. First, reporting is strongly related to schools' institutional capacity to observe and respond to student needs. Schools that employ a school nurse—though relatively uncommon—exhibit substantially higher reporting rates. Second, reporting tendencies are closely linked to traditional indicators of academic effectiveness. Reporting is higher at schools with lower test score and attendance value-added. These patterns suggest that reporting tendencies capture dimensions of school practice that are not well summarized by standard measures of school quality.

Third, reporting varies systematically with staff composition. Schools with a higher share of female educators and a higher share of non-white teachers and administrators tend to report more frequently. Finally, reporting is negatively associated with teacher experience and formal credentials. While these associations should be interpreted descriptively, they point to the possibility that reporting practices may be shaped by organizational norms, professional experiences, or approaches to student well-being that differ across schools.

5.D The Impacts of Attending a High-Reporting School

We next examine the implications for students' outcomes of attending an elementary school with a higher reporting tendency. We focus on two sets of outcomes measured during kindergarten through fifth grade: downstream child welfare system outcomes and students' academic outcomes. In these analyses, we control for not only student and neighborhood risk factors (i.e., the same ones included in the value-added models), but also for the supply-side factors shown in Table 3. In addition, we estimate the reports-added measure for this analysis using a ten-fold jackknife to avoid a mechanical correlation between the reporting tendency predictor and student outcomes.[10]

Table 4 examines how school reporting tendencies affect the likelihood that students experience different stages of the child welfare process. As Panel A shows, attending a school with a higher reports-added significantly increases the probability that a student receives a report from school staff, is screened in for investigation, and has a substantiated case, even after conditioning on a rich set of school characteristics. Relative to the dependent variable mean, a one standard deviation increase in reports-added raises the annual probability of a school-based report by approximately 20 percent (0.42 percentage points on a base of 2.1 percent), increases the probability of a screened-in investigation by about 18 percent (0.20

[10]Specifically, the residual investigations for students in each fold are excluded when calculating their reports-added.

percentage points on a base of 1.1 percent), and increases the probability of substantiation by roughly 17 percent (0.03 percentage points on a base of 0.18 percent).

At the same time, conditional on a school-based report occurring, reports initiated by higher-reporting schools are modestly less likely to be screened in or substantiated (Panel B). A one standard deviation increase in reports-added is associated with a 1.0 percentage point decline in the probability that a school-based report is screened in (2% from a mean of 53 percent) and a 0.3 percentage point decline in the probability of substantiation (4% from a mean of 8.5 percent). This pattern is consistent with higher-reporting schools identifying some more marginal cases that lie closer to CPS screening thresholds. Importantly, however, because higher reporting tendencies substantially increase the overall number of reports, the unconditional likelihood that students experience screened-in investigations and substantiated cases still rises significantly with school reporting tendencies. Higher-reporting schools therefore do not merely generate additional reports that fail to meet agency thresholds.

We also examine effects on overall child welfare involvement, regardless of reporter. This distinction is important because reports are classified as school-based only when the reporter is directly identified as school staff; in practice, school personnel may also trigger reports indirectly—for example, by contacting law enforcement or other mandated reporters who then file a report. In addition, higher school reporting could partially crowd out reports that would otherwise be initiated by other reporters. Consistent with incomplete crowd-out, higher school reporting tendencies increase the overall likelihood that a student experiences any report, investigation, or substantiated case from all reporters combined, shown in Panel C. A one standard deviation increase in reports-added raises the annual probability of any report by approximately 6 percent relative to the mean (0.41 percentage points on a base of 6.9 percent), with similarly sized increases in screened-in investigations (about 6 percent) and substantiated cases (about 4 percent). While these effects are smaller than those for school-based reports alone, they indicate that higher school reporting contributes to a net increase in children's contact with the child welfare system rather than simply shifting the source of reports across reporters.

In Table 5 we explore the relationship between higher reports-added and academic outcomes, including annual attendance rates and standardized test scores measured during kindergarten through fifth grade. The regression models include one observation per student x year, and standard errors are clustered by student and school. As shown in earlier sections, schools with higher reporting tendencies differ systematically along several supply-side

dimensions and tend to have lower attendance and test score value-added. In specifications that control only for student and neighborhood characteristics, attending a school with a one standard deviation higher reports-added is therefore associated with lower attendance and substantially lower test scores in both English language arts and math. For example, a one standard deviation increase in reports-added is associated with declines of roughly 0.02–0.03 standard deviations in test scores. We then ask whether higher reporting tendencies are associated with worse academic outcomes conditional on these other observable differences across schools. As additional school characteristics are added to the model—and in particular once we account for schools' academic value-added—these negative associations attenuate sharply. For attendance, the estimated association reverses sign and becomes positive once school value-added is included, while for test scores the estimates move closer to zero. In the fully controlled specifications that additionally condition on prior student outcomes, the estimated effects of reporting tendencies on attendance and test scores are small in magnitude and generally statistically indistinguishable from zero. For example, we can rule out effects larger than 0.5 percent of a standard deviation for test scores given a one standard deviation increase in reports-added.

Table A2 further examines whether the effects of school reporting tendencies vary across students with different baseline risks of child welfare involvement. We find that higher reporting tendencies increase the likelihood of school-based reports, screened-in investigations, and substantiated cases throughout the risk distribution. In relative terms, the proportional increase in reporting is largest for students with lower baseline risk, which is mechanically unsurprising given their low baseline probabilities of being reported. In absolute terms, however, reporting increases at all points in the risk distribution, indicating that higher-reporting schools shift children's exposure to the child welfare system broadly rather than concentrating effects on a narrow subset of students.

The heterogeneity analysis is particularly informative for academic outcomes. Across all risk groups—including students with prior child welfare involvement and those in the highest predicted risk quartile—we find little evidence that attending a higher-reporting school is associated with worse attendance or test scores once differences in observable school characteristics are taken into account. There is no clear gradient in academic effects across the risk distribution, suggesting that the academic consequences of reporting above and beyond overall school quality are small. Even among students at elevated baseline risk, the estimated associations between reporting tendencies and academic outcomes are generally statistically indistinguishable from zero, and we find no evidence of substantial academic

disruptions for any particular subgroup.

At the same time, it is important to note the limits of examining heterogeneity by risk in this context. Even among students in the highest predicted risk quartile, the probability of experiencing a child welfare report in a given year remains relatively low (17%), reflecting the fact that most children are not reported in any given year. As a result, while these analyses allow us to examine heterogeneity across observable and predicted risk groups, they may not fully capture effects for the very highest-risk children. Identifying such students ex ante is inherently difficult, and it remains possible that more pronounced effects, either positive or negative, could emerge in settings where risk is more sharply concentrated.

Taken together, these results suggest that while higher-reporting schools increase students' exposure to the child welfare system, they do not appear to generate large or systematic academic effects once differences in overall school quality are taken into account. These patterns hold broadly across students with different baseline risks of child welfare involvement. At the same time, the presence of additional screened-in and substantiated cases indicates that higher reporting reflects more than noise or purely discretionary reporting at intake. Without explicit welfare weights on false positives and false negatives, it is difficult to take a stand on what these patterns imply for overall child welfare. Rather, our estimates highlight the meaningful role of institutional reporting practices in shaping children's interactions with the child welfare system and the extent to which those interactions depend on the schools children attend.

6 Conclusion

This paper studies the sources and consequences of variation in child maltreatment reporting, focusing on the role of different institutional practices across public schools. Using statewide administrative data from Michigan that link public school records to all CPS reports over a seven-year period, we document substantial heterogeneity in reporting behavior across schools. Children with similar observable characteristics face markedly different probabilities of being reported to CPS depending on the schools they attend.

To disentangle differences in children's underlying risk of maltreatment from differences in reporting behavior, we adapt value-added methods from the education literature to estimate school-level reporting tendencies that are independent of student risk. Leveraging rich controls for students' prior child welfare involvement, neighborhood conditions, and educational trajectories—and validating our estimates using student movers—we show that a

sizable share of variation in reporting reflects supply-side differences in institutional practices rather than differences in underlying risk. Our estimates suggest that roughly 40 percent of the variation in school-based reporting is attributable to these institutional factors.

We also document that schools with higher reporting tendencies differ systematically along observable dimensions. Higher-reporting schools are more likely to employ a greater share of non-White teachers and principals and to have greater access to certain student support resources, such as school nurses. Moreover, higher reporting tendencies are negatively correlated with traditional measures of school quality, such as test score and attendance value-added, underscoring that reporting cultures capture dimensions of school practice that are distinct from academic effectiveness.

We find that attending a higher-reporting school increases students' exposure to the child welfare system, including the likelihood of screened-in investigations and substantiated cases. While reports generated by higher-reporting schools are modestly less likely to be screened in or substantiated conditional on a report occurring, higher reporting nonetheless leads to a net increase in cases that progress through investigation and substantiation. This pattern is consistent with higher-reporting schools identifying additional cases near agency thresholds, rather than simply generating noise at intake. Without explicit welfare weights on false positives and false negatives, it is difficult to assess the welfare implications of these patterns, but they underscore the meaningful role of institutional reporting practices in shaping children's interactions with the child welfare system and the extent to which those interactions depend on the schools children attend.

We also examine the academic implications of attending higher-reporting schools. While higher reporting tendencies are negatively correlated with attendance and test scores in raw comparisons, these relationships attenuate substantially—and, in the case of attendance, reverse sign—once differences in overall school quality are taken into account. In fully controlled specifications that condition on schools' academic effectiveness and prior student outcomes, we find little evidence of large or systematic academic effects associated with higher reporting.These patterns hold across students with different baseline risks of child welfare involvement.

More broadly, our results show that children's exposure to the child welfare system depends in meaningful ways on the schools they attend, reflecting differences in institutional reporting practices rather than differences in underlying risk alone. Whether—and under which contexts—this supply-side variation is welfare improving or welfare reducing is an important question for future research.

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Table 1: Characteristics of Michigan K-5 Students

	All	Never Reported	Ever Reported	White	Black	Hispanic
N	1,232,220	970,304	261,916	778,211	279,302	106,302
Prop. Students		0.787	0.213	0.632	0.227	0.086
Ever Reported Before KG	0.110		0.516	0.096	0.165	0.100
	(0.313)		(0.500)	(0.295)	(0.371)	(0.300)
Ever Reported, KG-5th Grade	0.197		0.927	0.174	0.285	0.195
	(0.398)		(0.260)	(0.379)	(0.451)	(0.396)
Ever Reported Through 5th Grade	0.213		1.000	0.188	0.306	0.209
	(0.409)		(0.000)	(0.391)	(0.461)	(0.407)
Any Substantiated Report Before KG	0.047		0.223	0.041	0.073	0.042
	(0.213)		(0.416)	(0.199)	(0.261)	(0.201)
Any Substantiated Report, KG-5th Grade	0.054		0.255	0.047	0.082	0.052
	(0.226)		(0.436)	(0.211)	(0.274)	(0.223)
Any Substantiated Report Through 5th Grade	0.086		0.406	0.075	0.132	0.081
	(0.281)		(0.491)	(0.263)	(0.339)	(0.273)
Demographics						
White	0.632	0.651	0.559			
	(0.482)	(0.477)	(0.496)			
Black	0.227	0.200	0.326			
	(0.419)	(0.400)	(0.469)			
Hispanic	0.086	0.087	0.085			
	(0.281)	(0.281)	(0.279)			
Other Race	0.056	0.062	0.030			
	(0.229)	(0.242)	(0.170)			
Female	0.487	0.489	0.478	0.484	0.491	0.490
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Age at End of KG	5.179	5.178	5.186	5.188	5.158	5.180
	(0.395)	(0.392)	(0.406)	(0.401)	(0.378)	(0.396)
Economically Disadvantaged	0.578	0.502	0.859	0.469	0.851	0.766
	(0.494)	(0.500)	(0.348)	(0.499)	(0.356)	(0.424)
IEP	0.142	0.122	0.216	0.145	0.139	0.146
	(0.349)	(0.327)	(0.411)	(0.352)	(0.346)	(0.353)
ELL	0.086	0.099	0.036	0.048	0.015	0.376
	(0.280)	(0.299)	(0.186)	(0.213)	(0.120)	(0.484)
Repeated KG	0.163	0.153	0.201	0.188	0.102	0.165
	(0.369)	(0.360)	(0.401)	(0.391)	(0.303)	(0.371)
Chronically Absent (Attendance <90%)	0.244	0.215	0.353	0.173	0.431	0.286
	(0.429)	(0.411)	(0.478)	(0.378)	(0.495)	(0.452)
Attendance Rate	0.920	0.926	0.897	0.935	0.879	0.915
	(0.095)	(0.089)	(0.111)	(0.077)	(0.124)	(0.093)
Great Starts Readiness Program	0.308	0.288	0.383	0.262	0.433	0.391
	(0.462)	(0.453)	(0.486)	(0.440)	(0.495)	(0.488)
Head Start	0.086	0.069	0.151	0.062	0.157	0.101
	(0.281)	(0.253)	(0.358)	(0.241)	(0.364)	(0.302)

Note: Students in Michigan public or charter schools from 2017-18 through 2023-24 school years. We keep students' first year observed in this period. Economically Disadvantaged = received free or reduced price lunch, SNAP or TANF, were homeless or in foster care, or were a migrant. IEP = special education services, ELL=English Language Learner.

Table 2: School and Neighborhood Characteristics of Michigan K-5 Students

	All	Never Reported	Ever Reported	White	Black	Hispanic
N	1,232,220	970,304	261,916	778,211	279,302	106,302
Prop. Students		0.787	0.213	0.632	0.227	0.086
School Characteristics						
Charter School	0.121	0.119	0.129	0.054	0.304	0.130
	(0.326)	(0.324)	(0.336)	(0.226)	(0.460)	(0.337)
Total School Enrollment	454.5	458.9	438.2	444.1	480.3	452.9
	(229.0)	(226.8)	(236.4)	(208.7)	(281.1)	(226.6)
Number Teachers	26.258	26.518	25.293	25.860	26.835	26.650
	(11.547)	(11.497)	(11.683)	(10.714)	(13.680)	(11.515)
Title I School	0.486	0.438	0.667	0.413	0.690	0.594
	(0.500)	(0.496)	(0.471)	(0.492)	(0.462)	(0.491)
Mean Std. Test Score (Reading)	-0.001	0.040	-0.153	0.127	-0.368	-0.109
	(0.442)	(0.433)	(0.443)	(0.347)	(0.471)	(0.421)
Mean Std. Test Score (Math)	-0.005	0.039	-0.171	0.137	-0.421	-0.123
	(0.484)	(0.475)	(0.480)	(0.375)	(0.507)	(0.451)
Urban District	0.280	0.269	0.320	0.156	0.568	0.396
	(0.449)	(0.444)	(0.466)	(0.363)	(0.495)	(0.489)
Suburban District	0.501	0.525	0.415	0.550	0.386	0.445
	(0.500)	(0.499)	(0.493)	(0.497)	(0.487)	(0.497)
Rural District	0.219	0.206	0.266	0.294	0.046	0.158
	(0.413)	(0.404)	(0.442)	(0.456)	(0.209)	(0.365)
Census Tract Characteristics						
Prop. BA+	0.282	0.299	0.218	0.304	0.204	0.234
	(0.196)	(0.204)	(0.149)	(0.192)	(0.164)	(0.178)
Prop. Married	0.281	0.299	0.218	0.304	0.204	0.234
	(0.196)	(0.204)	(0.149)	(0.192)	(0.164)	(0.178)
Unemployment Rate	0.073	0.068	0.091	0.056	0.124	0.076
	(0.063)	(0.059)	(0.070)	(0.039)	(0.088)	(0.058)
Median HH Income	58,888	61,755	48,265	64,752	41,660	51,851
	(29,290)	(30,391)	(21,710)	(28,186)	(22,968)	(24,996)
Prop. Public Assistance	0.029	0.027	0.036	0.024	0.042	0.036
	(0.030)	(0.029)	(0.032)	(0.023)	(0.038)	(0.035)
Family Poverty Rate	0.125	0.115	0.160	0.089	0.222	0.156
	(0.125)	(0.121)	(0.133)	(0.089)	(0.154)	(0.131)

Note: Students in Michigan public or charter schools from 2017-18 through 2023-24 school years. We keep students' first year observed in this period. IEP = special education services. Standardized test scores are scaled to have mean zero and standard deviation one. Dollars are in 2015 amounts.

Table 3: Characteristics of Higher-Reporting Schools (Bivariate Associations)

	Sample Mean (sd)	Bivariate Association
Prop. Teachers Female	0.837	0.026**
	(0.128)	(0.012)
Prop. Teachers Black	0.071	0.070***
	(0.174)	(0.010)
Prop. Teachers Hispanic	0.016	0.047***
	(0.040)	(0.008)
Prop. Teachers Other Race	0.024	0.045***
	(0.050)	(0.010)
Prop. Principals Black	0.123	0.141***
	(0.322)	(0.032)
Prop. Principals Hispanic	0.013	0.371***
	(0.108)	(0.067)
Prop. Principals Other Race	0.016	0.168**
	(0.121)	(0.076)
Average Teacher Age	43.577	0.000
	(4.919)	(0.011)
Teacher Years at School	10.762	-0.036***
	(4.469)	(0.012)
Prop. Teacher Master's Degree	0.513	-0.022*
	(0.216)	(0.012)
Student-Teacher Ratio	17.633	-0.014
	(35.308)	(0.023)
Charter	0.131	0.044
	(0.338)	(0.036)
Title I	0.385	0.257***
	(0.487)	(0.023)
Math Test Score VA	0.001	-0.093***
	(0.934)	(0.014)
ELA Test Score VA	0.001	-0.133***
	(0.934)	(0.015)
Attendance VA	0.000	-0.202***
	(0.994)	(0.015)
School Has Counselor	0.141	-0.063**
	(0.348)	(0.027)
School Has Nurse	0.023	0.375***
	(0.150)	(0.074)
N	11,530	11,530

Note: Each coefficient reports the bivariate association between a school characteristic and the average residual likelihood of a CPS report at school j in year t. All regressions include county-by-year fixed effects and are estimated separately for each characteristic. Continuous variables are standardized. The sample includes one observation per school-year. Standard errors are clustered at the school level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Reports-Added and Reporting Outcomes in K-5

	Any Report	Screened In	Substantiated
Panel A. School Reports			
Reports-Added	0.0042***	0.0020***	0.0003***
	(0.0002)	(0.0001)	(0.0000)
Coef. / Mean	0.1964	0.1750	0.1651
Dep. Var. Mean	0.0214	0.0114	0.0018
N Student-Years	3,448,867	3,448,867	3,448,867
N Students	1,232,185	1,232,185	1,232,185
Panel B. School Reports, if Reported			
Reports-Added		-0.0099***	-0.0030**
		(0.0026)	(0.0012)
Coef. / Mean		-0.0185	-0.0353
Dep. Var. Mean		0.5343	0.0849
N Student-Years		73,757	73,757
N Students		67,871	67,871
Panel C. Any Reports			
Reports-Added	0.0041***	0.0024***	0.0005***
	(0.0004)	(0.0003)	(0.0001)
Coef. / Mean	0.0595	0.0555	0.0421
Dep. Var. Mean	0.0689	0.0432	0.0119
N Student-Years	3,448,867	3,448,867	3,448,867
N Students	1,232,185	1,232,185	1,232,185

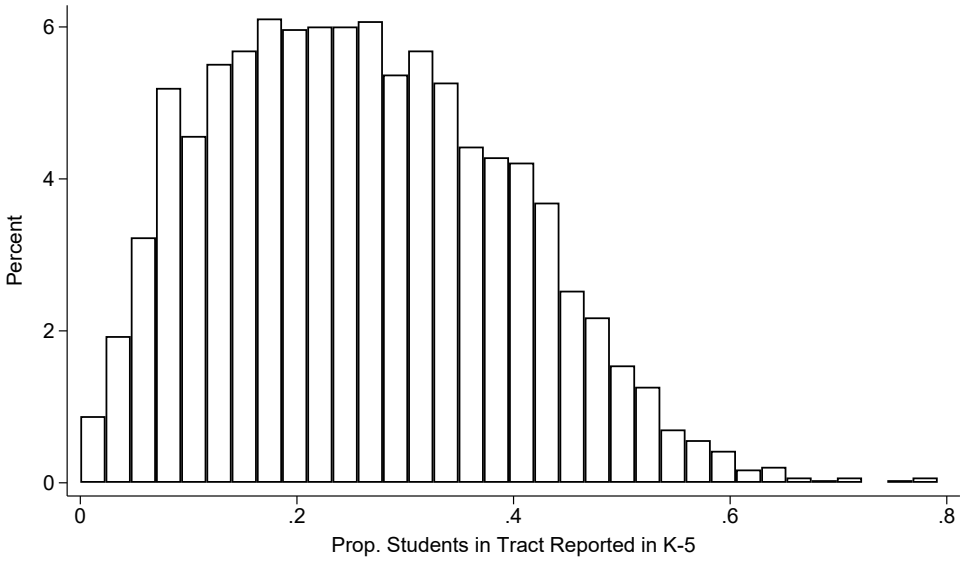
Note: * p<0.1, ** p<.05, *** p<0.01 (clustered at student and school level). There is one observation per student-year in K-5. Regressions control for student demographics and neighborhood characteristics, as well as school characteristics that do not enter the value-added estimation, including charter status, teacher characteristics, and test score and attendance value-added. Reports-added is estimated as a ten-fold jackknife, where the residual investigations for students in each fold are excluded when calculating their reports-added. Reports-added is estimated using the static approach in Kane & Staiger (2008), where the dependent variable is a binary outcome for any reports from school staff in year t.

Table 5: Reports-Added and Academic Outcomes in K-5

	Student Controls	Adds Lagged Reports	Add School Chars.	Add School VA	Add Lagged Y
Panel A. Attendance					
Reports-Added	-0.0014***	-0.0013***	-0.0010**	0.0020***	0.0011***
	(0.0005)	(0.0005)	(0.0004)	(0.0004)	(0.0003)
Coef. / Mean	-0.0015	-0.0014	-0.0011	0.0022	0.0012
Dep. Var. Mean	0.9240	0.9240	0.9240	0.9240	0.9251
N Student-Years	3,418,420	3,418,420	3,418,420	3,418,420	2,282,066
N Students	1,225,609	1,225,609	1,225,609	1,225,609	963,906
Panel B. ELA Test Scores					
Reports-Added	-0.0214***	-0.0196***	-0.0174***	-0.0087*	-0.0023
	(0.0051)	(0.0050)	(0.0049)	(0.0046)	(0.0023)
Dep. Var. Mean	0.0232	0.0232	0.0232	0.0232	0.0586
N Student-Years	1,288,936	1,288,936	1,288,936	1,288,936	611,322
N Students	802,757	802,757	802,757	802,757	480,018
Panel C. Math Test Scores					
Reports-Added	-0.0252***	-0.0235***	-0.0214***	-0.0084*	-0.0004
	(0.0058)	(0.0057)	(0.0055)	(0.0048)	(0.0026)
Dep. Var. Mean	0.0232	0.0232	0.0232	0.0232	0.0677
N Student-Years	1,288,635	1,288,635	1,288,635	1,288,635	610,449
N Students	802,604	802,604	802,604	802,604	479,521

Note: * p<0.1, ** p<.05, *** p<0.01 (clustered at student and school level). There is one observation per student-year in K-5. Regressions progressively control for student demographics and neighborhood characteristics; lagged reporting outcomes; school characteristics such as charter status and teacher race/gender; test score and attendance value-added; and the lagged dependent variable. Reports-added is estimated as a ten-fold jackknife, where the residual investigations for students in each fold are excluded when calculating their reports-added. Reports-added is estimated using the static approach in Kane & Staiger (2008), where the dependent variable is a binary outcome for any reports from school staff in year t.

Figure 1: Distribution of Reporting Rates Across Census Tracts



Note: Children who started in a Michigan public or charter school in Fall 2017 or Fall 2018. We omit tracts with fewer than 10 students living there in Kindergarten and exclude reports from March 2020 through June 2021.

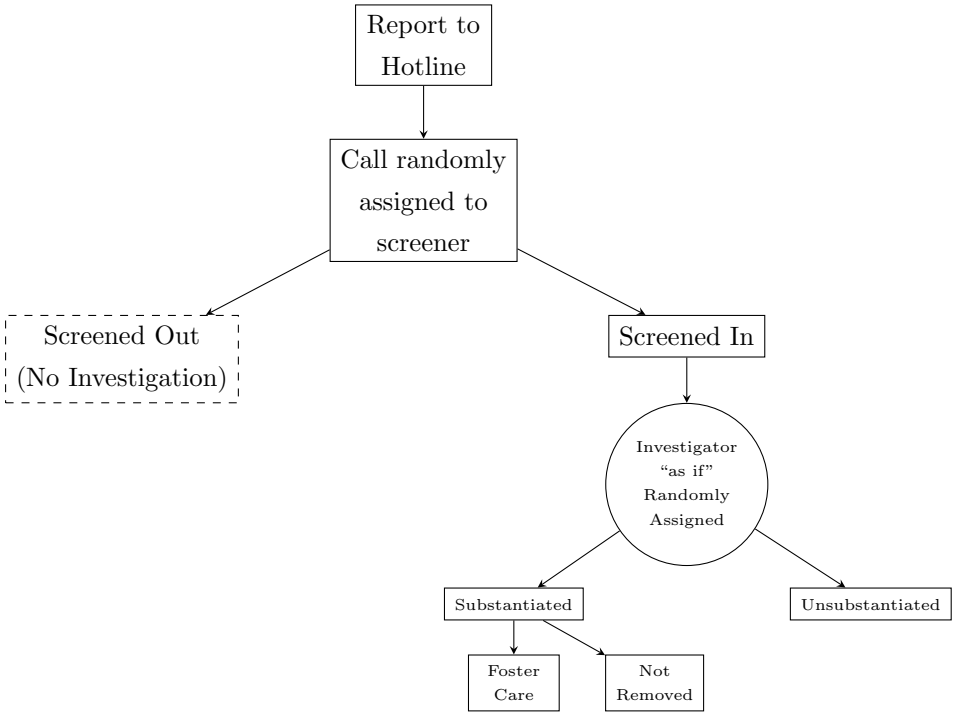
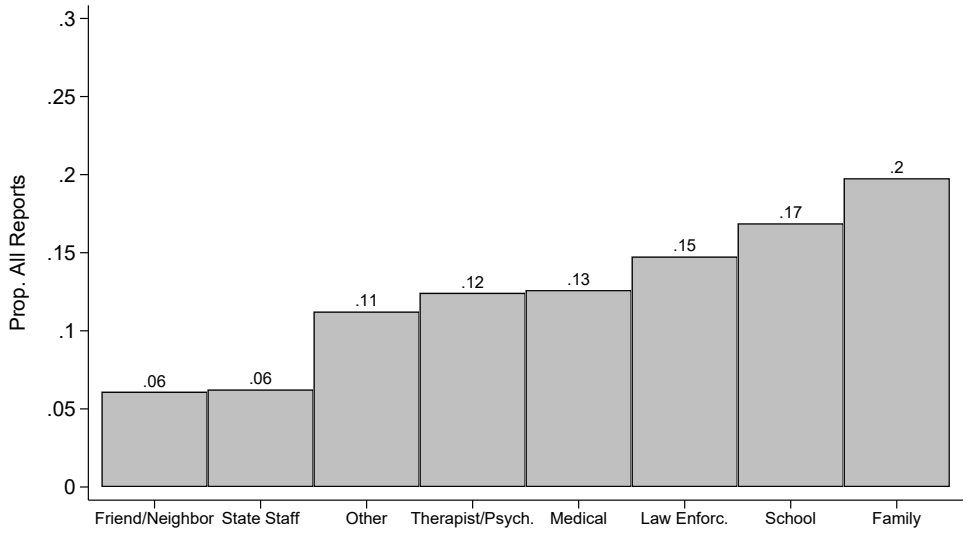


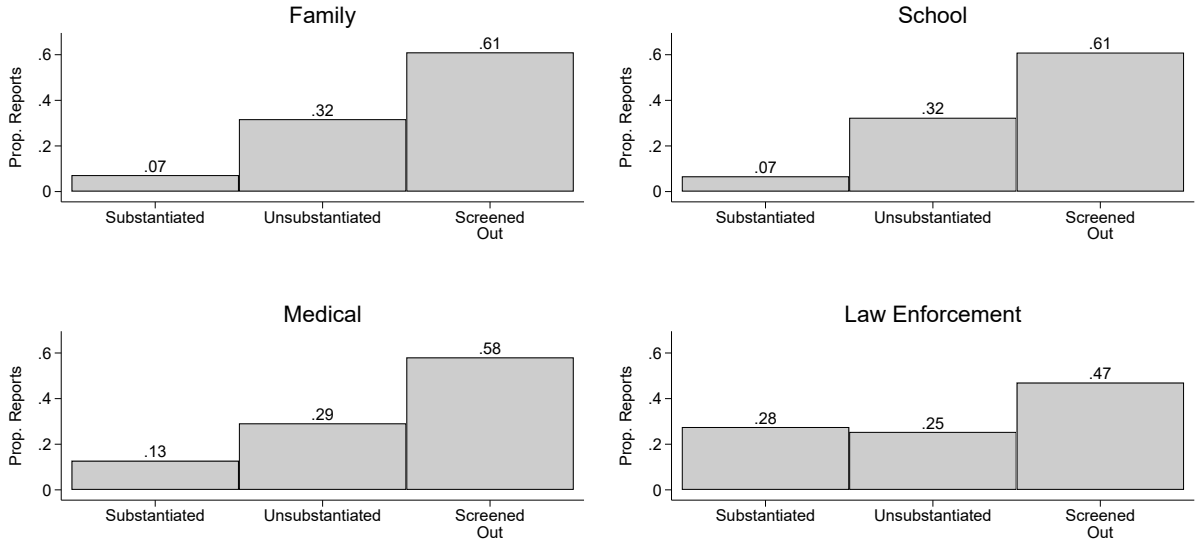
Figure 2: Child Protective Services Reporting and Investigation Process in Michigan

Figure 3: Distribution of Reporter Types



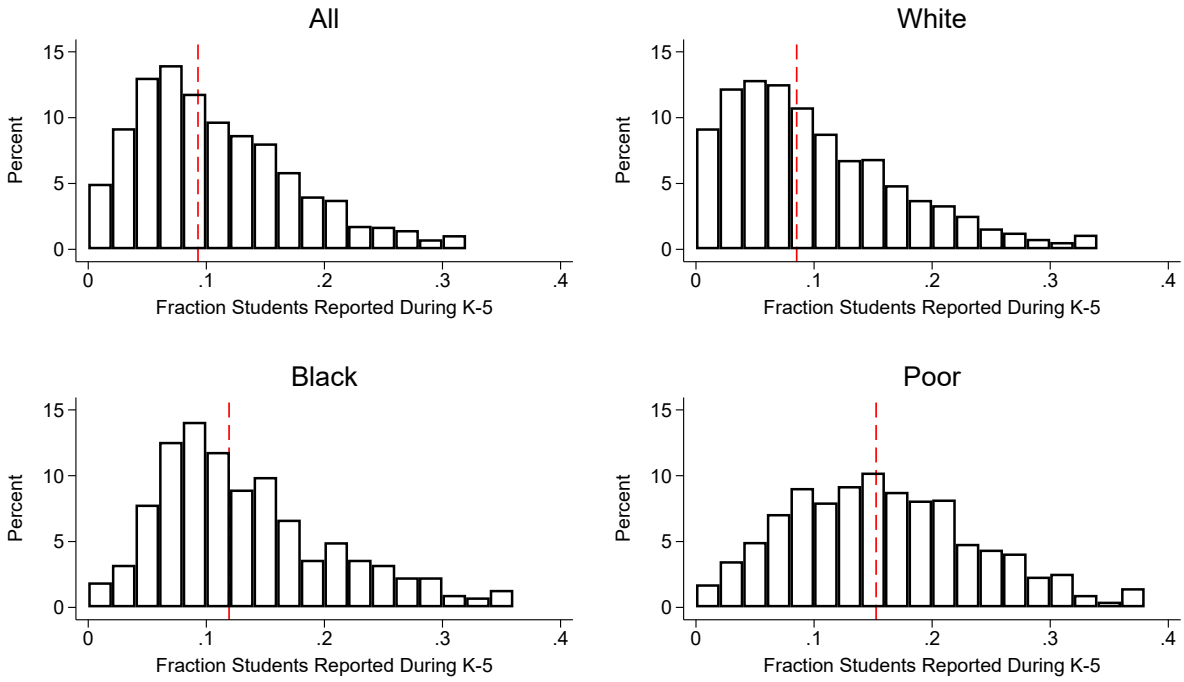
Note: All child welfare reports for Michigan public/charter school students filed between 2017 and 2024, by reporter type. We exclude reports from March 2020 through June 2021.

Figure 4: Report Outcomes by Reporter Type



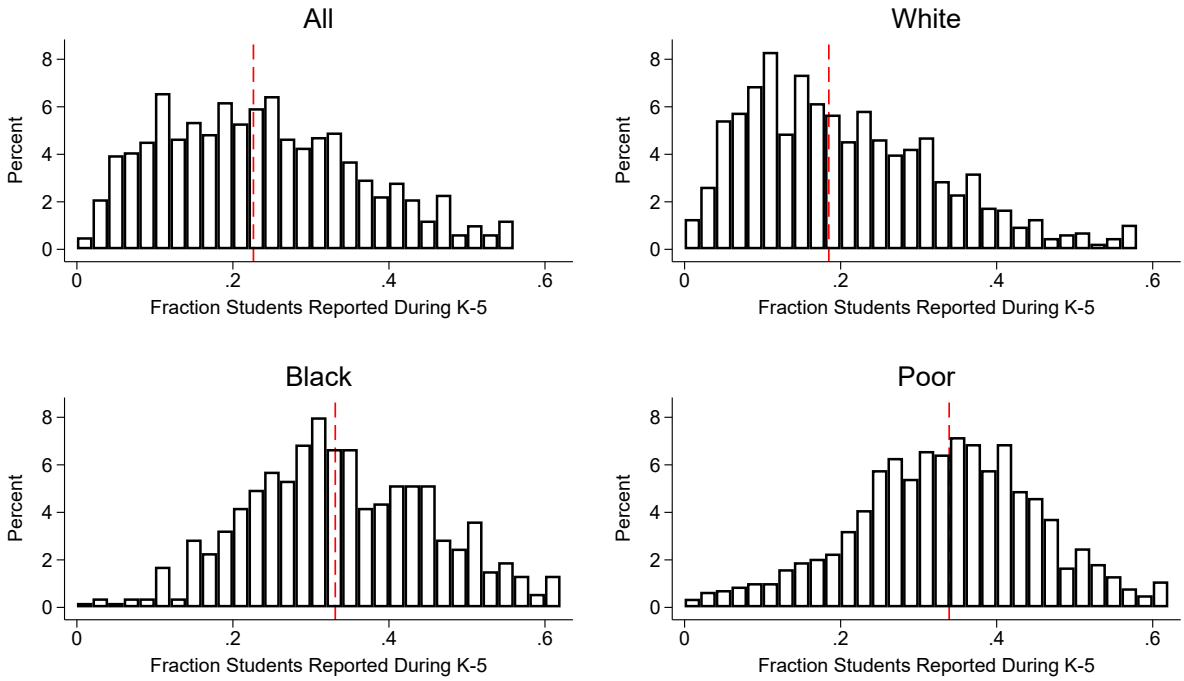
Note: All child welfare reports for Michigan public/charter school students filed between 2017 and 2024, by reporter type. We exclude reports from March 2020 through June 2021.

Figure 5: Proportion Students Ever Reported by a School Staff Member in K-5



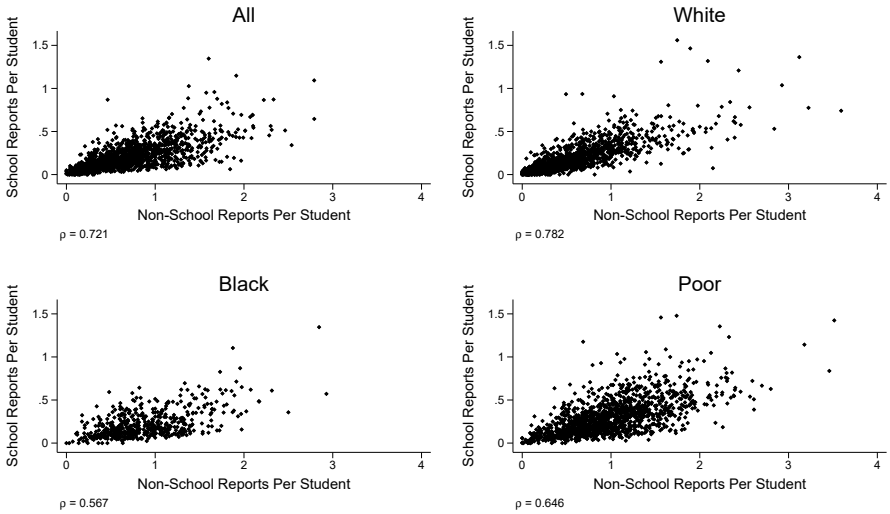
Note: Children who started in a Michigan public or charter school in Fall 2017 or Fall 2018. The vertical red line represents the median school. We omit schools with fewer than 25 students fitting the sample criteria and exclude reports from March 2020 through June 2021.

Figure 6: Proportion Students Ever Reported by a Non-School Staff Member in K-5



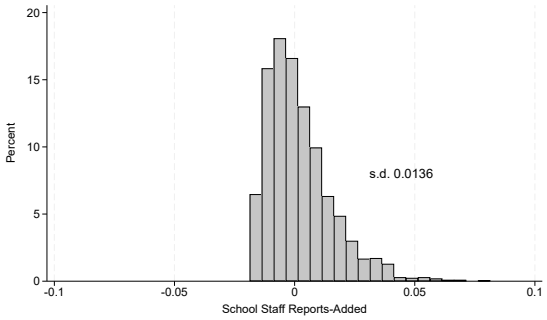
Note: Children who started in a Michigan public or charter school in Fall 2017 or Fall 2018. The vertical red line represents the median school. We omit schools with fewer than 25 students fitting the sample criteria and exclude reports from March 2020 through June 2021.

Figure 7: School and Non-School Reports Per Student

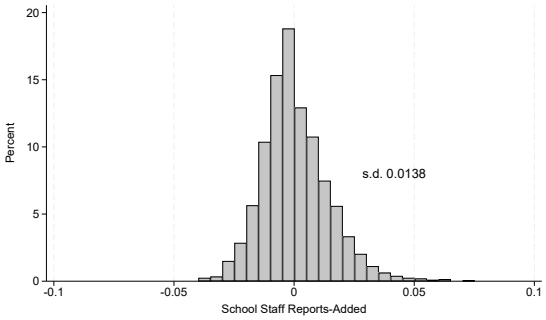


Note: Children who started in a Michigan public or charter school in Fall 2017 or Fall 2018. We omit schools with fewer than 25 students fitting the sample criteria and, for visual purposes, omit schools above the 99th percentile of either variable. We exclude reports from March 2020 through June 2021.

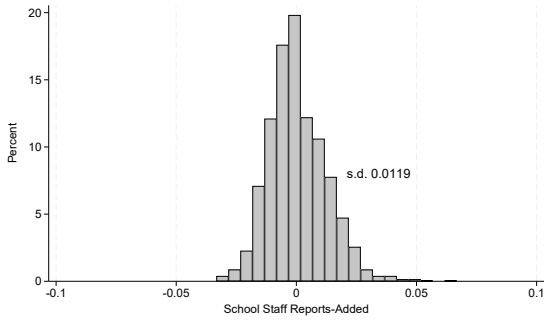
Figure 8: Distribution of Static Reports-Added Across Schools



(a) No Controls



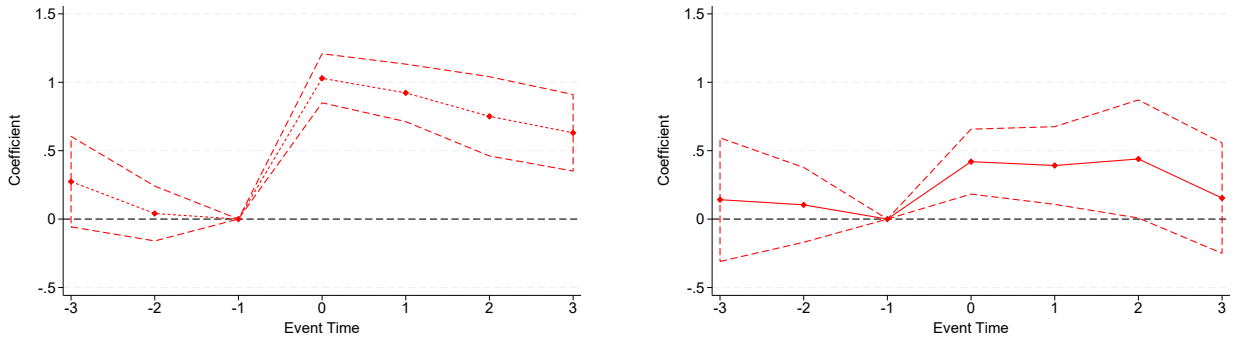
(b) Add Student and School Controls



(c) Add Controls for Lagged Reports

Note: Static (Kane & Staiger) school-level reports-added estimates. The dependent variable is a binary indicator for whether the student received any reports where they were the alleged victim. In addition to other student, school, and neighborhood controls (including lagged reports), we include a sparse set of student and school mobility controls. We omit all observations for a student at a given school after their first report while attending that school. A student who is assigned to a school with a one standard deviation higher reports-added experiences a 1.2 percentage point increase in the likelihood of being reported by school staff, a 57% increase relative to the sample mean (2.1 percent).

Figure 9: Event-Study Estimates of School Reports Surrounding Moves

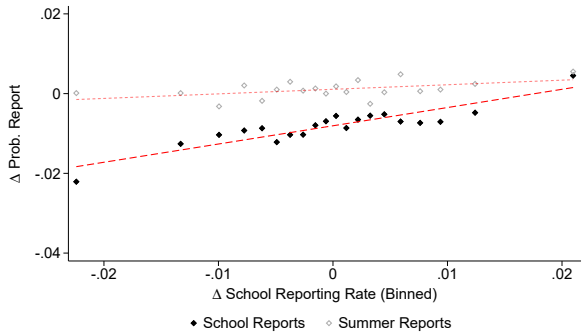


(a) Risk-Adjusted Reports

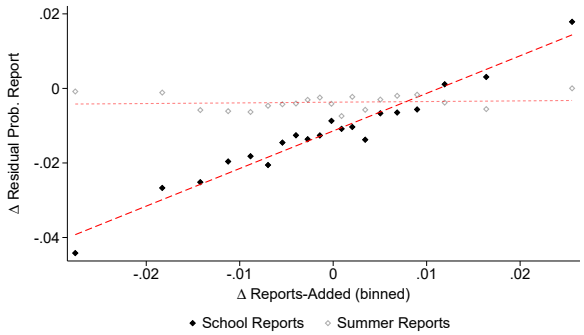
(b) Unadjusted Reports

Note: Event study estimates of the change in student reports from school staff when they switch schools. The left panel shows changes in risk-adjusted reporting probabilities when students switch to schools with reports-added higher by one (coefficient in year 1 = 1.029). The right panel shows changes in the probability of a school-based report when students switch to schools with higher unadjusted reporting rates (coefficient in year 1 = 0.420). For comparability, we shrink unadjusted school reports using the same empirical Bayes approach as we use for reports-added. Both independent variables are estimated on a sample of students who never switch schools. Coefficients are estimated from a model containing only students who switch schools in 2nd or 3rd grade. We include grade and year fixed effects as well as the main effect for the change in reports-added. We present the interaction coefficient between event-time surrounding the switch and the difference in the old and new school's reports-added. We restrict to students who only switched schools once. Standard errors are clustered at the student and post-switch school level.

Figure 10: Binscatter of Changes in School Reports Surrounding Moves



(a) Unadjusted Reports



(b) Risk-Adjusted Reports

Note: Binscatter of the change in student reports from school staff when they switch schools. The left panel shows changes in risk-adjusted reporting probabilities when students switch to schools with reports-added higher by one. The right panel shows changes in the probability of a report when students switch to schools with higher unadjusted reporting rates. For comparability, we shrink unadjusted school reports using the same empirical Bayes approach as we use for reports-added. Both independent variables are estimated on a sample of students who never switch schools. We show both school-based reports (black dots) and show reports over the summer preceding the year (grey dots) as a placebo test. Slope coefficients (robust standard errors) in the left panel are 0.458 (0.075) for school reports and 0.114 (0.045) for summer reports. In the right panel, they are 1.008 (0.069) and 0.018 (0.055).

Appendix A

Table A1: Regressions Predicting Reports for Elementary-Age Children, 2018-24

	School Staff	Summer, Non-School Staff
N (Student x Year)	3,448,920	3,448,920
School Fixed Effects	X	X
County x Year Fixed Effects	X	X
Adjusted R-Squared	0.061	0.052
Total Number Reports Pre-KG		
All Reports	0.0157***	0.0128***
	(0.0005)	(0.0004)
Substantiated Reports	-0.0074***	-0.0132***
	(0.0014)	(0.0013)
Lagged # School-Based Reports		
1 Year Ago	0.0587***	0.0470***
	(0.0016)	(0.0013)
2 Years Ago	0.0318***	0.0191***
	(0.0013)	(0.0010)
3+ Years Ago	0.0049***	0.0004
	(0.0006)	(0.0005)
Lagged # Substantiated School-Based Reports		
1 Year Ago	-0.0175***	0.0137***
	(0.0031)	(0.0028)
2 Years Ago	-0.0060**	0.0088***
	(0.0025)	(0.0022)
3+ Years Ago	0.0044***	0.0078***
	(0.0014)	(0.0013)
Lagged # Non-School Reports		
1 Year Ago	0.0243***	0.0465***
	(0.0024)	(0.0023)
2 Years Ago	0.0220***	0.0189***
	(0.0020)	(0.0019)
3+ Years Ago	-0.0017*	0.0001
	(0.0010)	(0.0008)
Lagged # Substantiated Non-School Reports		
1 Year Ago	0.0058	0.0035
	(0.0045)	(0.0043)
2 Years Ago	0.0086**	0.0092***
	(0.0037)	(0.0034)
3+ Years Ago	0.0077***	0.0095***
	(0.0020)	(0.0017)
Grade (omitted: KG)		
1st Grade	0.0051***	0.0038***
	(0.0004)	(0.0003)
2nd Grade	0.0064***	0.0042***
	(0.0004)	(0.0003)
3rd Grade	0.0068***	0.0036***
	(0.0005)	(0.0003)
4th Grade	0.0060***	0.0036***
	(0.0005)	(0.0003)
5th Grade	0.0057***	0.0035***
	(0.0005)	(0.0004)
Race (omitted: white)		
Black	0.0026***	-0.0007**
	(0.0004)	(0.0003)
Hispanic	-0.0007	-0.0009***
	(0.0004)	(0.0003)
Other Race	0.0000	-0.0004
	(0.0004)	(0.0003)
Female	-0.0015***	0.0005***
	(0.0002)	(0.0001)
Age Start of KG	0.0001	-0.0004**
	(0.0002)	(0.0002)
Poor	0.0157***	0.0075***
	(0.0003)	(0.0002)
IEP	0.0155***	0.0015***
	(0.0004)	(0.0002)
ELL	-0.0052***	-0.0050***
	(0.0004)	(0.0002)
Ever in MI Pre-K Program	-0.0030***	-0.0019***
	(0.0003)	(0.0002)

Ever in Head Start	0.0044***	0.0019***
	(0.0005)	(0.0003)
Ever in Great Starts Readiness Pgm	-0.0014***	-0.0006***
	(0.0002)	(0.0002)
Same School Last Year	-0.0067***	-0.0053***
	(0.0004)	(0.0003)
Same School Two Years Ago	-0.0023***	-0.0011***
	(0.0003)	(0.0002)
Same School 3+ Years Ago	-0.0020***	-0.0006**
	(0.0003)	(0.0003)
Student Neighborhood Characteristics		
Prop. Adults BA+	0.0134	-0.0047
	(0.0123)	(0.0101)
Prop. Married	-0.0198	0.0037
	(0.0123)	(0.0101)
Unemployment Rate	0.0016	-0.0002
	(0.0026)	(0.0023)
Log Median Household Income	0.0001	0.0000
	(0.0001)	(0.0001)
Prop. Receiving Public Assistance	0.0045	0.0060*
	(0.0044)	(0.0035)
Family Poverty Rate	0.0066***	-0.0006
	(0.0015)	(0.0013)
School Leave-Out Average Characteristics		
Non-School Reports Per Student Last Year	-0.0004	-0.0186
	(0.0154)	(0.0113)
Prop. Students Black	-0.0024	-0.0034
	(0.0074)	(0.0044)
Prop. Students Hispanic	-0.0021	0.0058
	(0.0084)	(0.0055)
Prop. Students Other Race	0.0057	0.0010
	(0.0071)	(0.0047)
Prop. Students Female	-0.0005	0.0003
	(0.0049)	(0.0035)
Average Age Start of KG	-0.0025	-0.0025
	(0.0048)	(0.0034)
Prop. Students Poor	-0.0069*	0.0030
	(0.0035)	(0.0023)
Prop. Students IEP	-0.0015	0.0035
	(0.0059)	(0.0041)
Prop. Students ELL	0.0055	-0.0092**
	(0.0056)	(0.0039)
Prop. Students Ever in MI Pre-K Program	-0.0006	-0.0028
	(0.0036)	(0.0024)
Prop. Students Ever in Head Start	-0.0089	-0.0040
	(0.0055)	(0.0039)
Prop. Students Ever in Great Starts Readiness Pgm	-0.0061*	-0.0038*
	(0.0036)	(0.0023)
Prop. Students Same School Last Year	0.0028	0.0038**
	(0.0023)	(0.0016)
Prop. Students Same School Two Years Ago	0.0060*	-0.0006
	(0.0032)	(0.0019)
Prop. Students Same School 3+ Years Ago	-0.0046	-0.0012
	(0.0037)	(0.0023)
Average Prop. Adults BA+	-0.0659	0.0597
	(0.0587)	(0.0467)
Average Prop. Married	0.0593	-0.0566
	(0.0580)	(0.0465)
Average Unemployment Rate	-0.0221*	-0.0010
	(0.0124)	(0.0082)
Average Median Household Income	0.0006	0.0001
	(0.0007)	(0.0005)
Average Prop. Receiving Public Assistance	0.0091	-0.0080
	(0.0225)	(0.0125)
Average Family Poverty Rate	-0.0041	0.0056
	(0.0092)	(0.0059)
Urbanicity (vs. Suburban)		
Urban	-0.0219***	0.0043***
	(0.0015)	(0.0010)
Rural	0.0127***	-0.0046***
	(0.0025)	(0.0015)

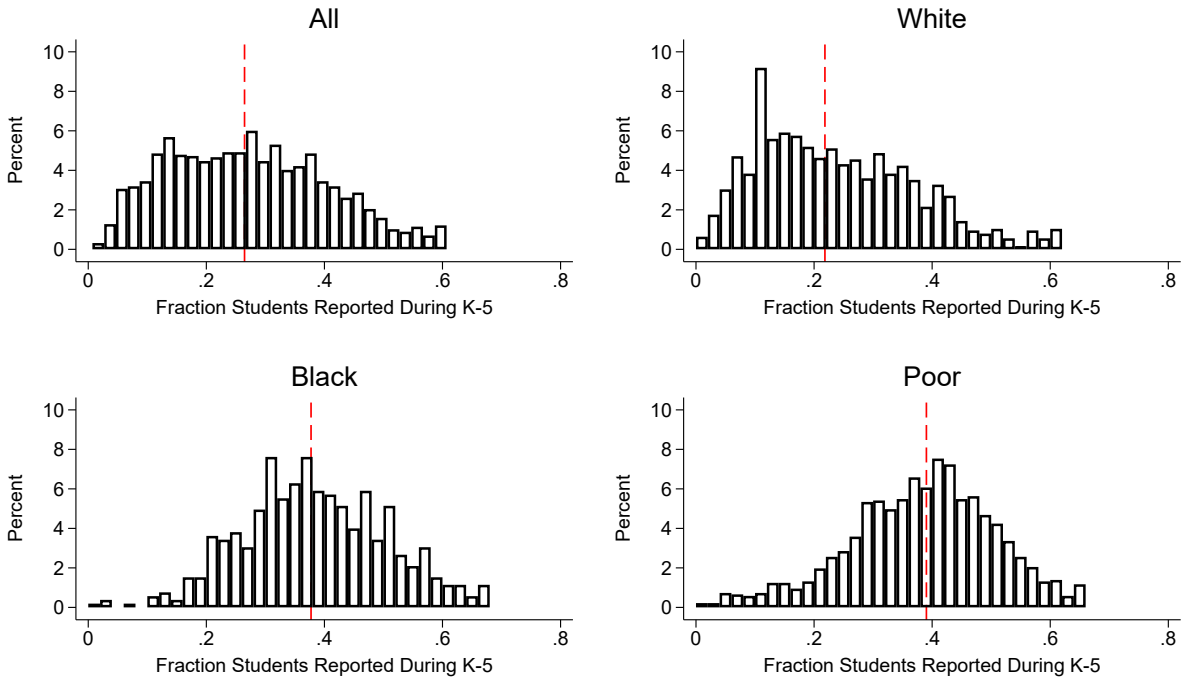
Note: * p<0.1, ** p<.05, *** p<0.01 (clustered by school). K-5 students enrolled in a Michigan public school in the 2018-2024 school years (excluding 2020-21). Dependent variable is a binary indicator for any CPS report where the child was listed as an alleged victim. We drop student-year observations after their first report from any source while attending a given school. Standard errors are clustered at the school level. Regressions include indicators for missing or imputed control variables.

Table A2: Reports-Added and Outcomes in K-5 by Student Reporting Risk

	Any School Report	Screened In School Report	Substantiated School Report	Attendance	ELA Test Score	Math Test Score
Panel A. No Pre-KG Reports						
Reports-Added	0.0023***	0.0010***	0.0001***	0.0020***	-0.0078*	-0.0079
	(0.0001)	(0.0001)	(0.0000)	(0.0004)	(0.0048)	(0.0049)
Coef. / Mean	0.2064	0.1703	0.1179	0.0001		
Dep. Var. Mean	0.0111	0.0059	0.0008	0.9280	0.0829	0.0861
N Student-Years	3,121,674	3,121,674	3,121,674	3,093,837	1,170,647	1,170,436
N Students	1,096,920	1,096,920	1,096,920	1,090,815	722,755	722,632
Panel B. Any Pre-KG Reports						
Reports-Added	0.0167***	0.0080***	0.0014***	0.0020***	-0.0126***	-0.0093*
	(0.0009)	(0.0006)	(0.0002)	(0.0006)	(0.0047)	(0.0052)
Coef. / Mean	0.1402	0.1242	0.1266	0.0016		
Dep. Var. Mean	0.1191	0.0644	0.0111	0.8865	-0.5676	-0.5925
N Student-Years	327,193	327,193	327,193	324,583	118,289	118,199
N Students	135,265	135,265	135,265	134,794	80,002	79,972
Panel C. Risk Quartile 1						
Reports-Added	0.0016***	0.0007***	0.0001	0.0011***	-0.0032	-0.0084
	(0.0002)	(0.0001)	(0.0000)	(0.0003)	(0.0083)	(0.0085)
Coef. / Mean	0.2354	0.2078	0.2527	0.0001		
Dep. Var. Mean	0.0068	0.0034	0.0004	0.9542	0.5083	0.5516
N Student-Years	520,632	520,632	520,632	519,274	267,636	267,664
N Students	166,338	166,338	166,338	166,160	159,861	159,882
Panel D. Risk Quartile 2						
Reports-Added	0.0025***	0.0010***	0.0002**	0.0016***	-0.0222***	-0.0180**
	(0.0003)	(0.0002)	(0.0001)	(0.0005)	(0.0066)	(0.0071)
Coef. / Mean	0.2033	0.1556	0.2120	0.0002		
Dep. Var. Mean	0.0123	0.0064	0.0009	0.9472	0.1911	0.1917
N Student-Years	504,606	504,606	504,606	503,250	260,715	260,704
N Students	166,657	166,657	166,657	166,459	158,595	158,593
Panel E. Risk Quartile 3						
Reports-Added	0.0032***	0.0015***	0.0002***	0.0026***	-0.0037	-0.0027
	(0.0003)	(0.0002)	(0.0001)	(0.0005)	(0.0054)	(0.0059)
Coef. / Mean	0.1995	0.1796	0.1531	0.0002		
Dep. Var. Mean	0.0160	0.0084	0.0013	0.9328	-0.1483	-0.1609
N Student-Years	516,633	516,633	516,633	515,022	259,265	259,207
N Students	166,610	166,610	166,610	166,402	156,559	156,523
Panel F. Risk Quartile 4						
Reports-Added	0.0092***	0.0046***	0.0007***	0.0028***	-0.0003	0.0010
	(0.0006)	(0.0004)	(0.0001)	(0.0005)	(0.0049)	(0.0051)
Coef. / Mean	0.1720	0.1538	0.1334	0.0008		
Dep. Var. Mean	0.0535	0.0299	0.0052	0.9039	-0.5388	-0.5766
N Student-Years	494,045	494,045	494,045	491,826	233,417	233,282
N Students	166,616	166,616	166,616	166,375	145,111	145,085

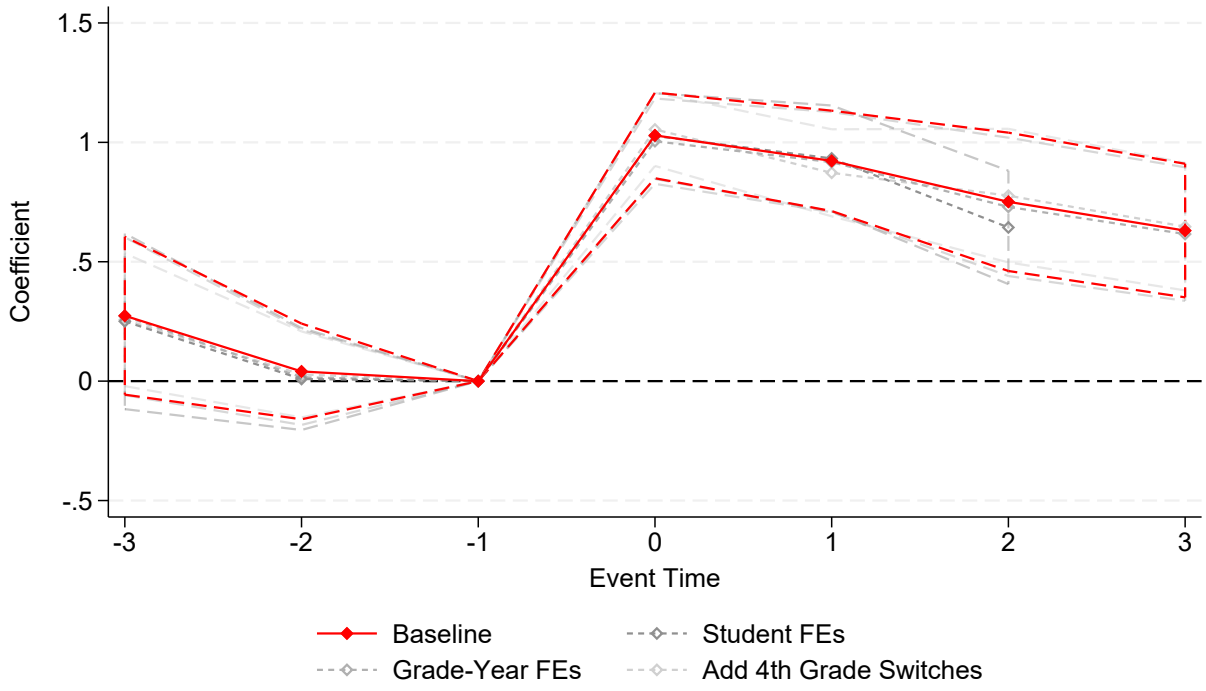
Note: * p<0.1, ** p<.05, *** p<0.01 (clustered at student and school level). There is one observation per student-year in K-5. Predicted probabilities of a student having any report during K-5 come from a logistic regression with the following characteristics: age in KG; number of investigations and substantiated investigations prior to KG; participation in public pre-K, Head Start, or the Great Starts Readiness Program; race/ethnicity; gender; and KG neighborhood characteristics. Regressions control for student demographics and neighborhood characteristics, as well as school characteristics that do not enter the value-added estimation, including charter status, teacher characteristics, and test score value-added. Reports-added is estimated as a ten-fold jackknife, where the residual investigations for students in each fold are excluded when calculating their reports-added. Reports-added is estimated using the static approach in Kane & Staiger (2008), where the dependent variable is a binary outcome for any reports from school staff in year t.

Figure A1: Proportion Students Ever Reported in K-5



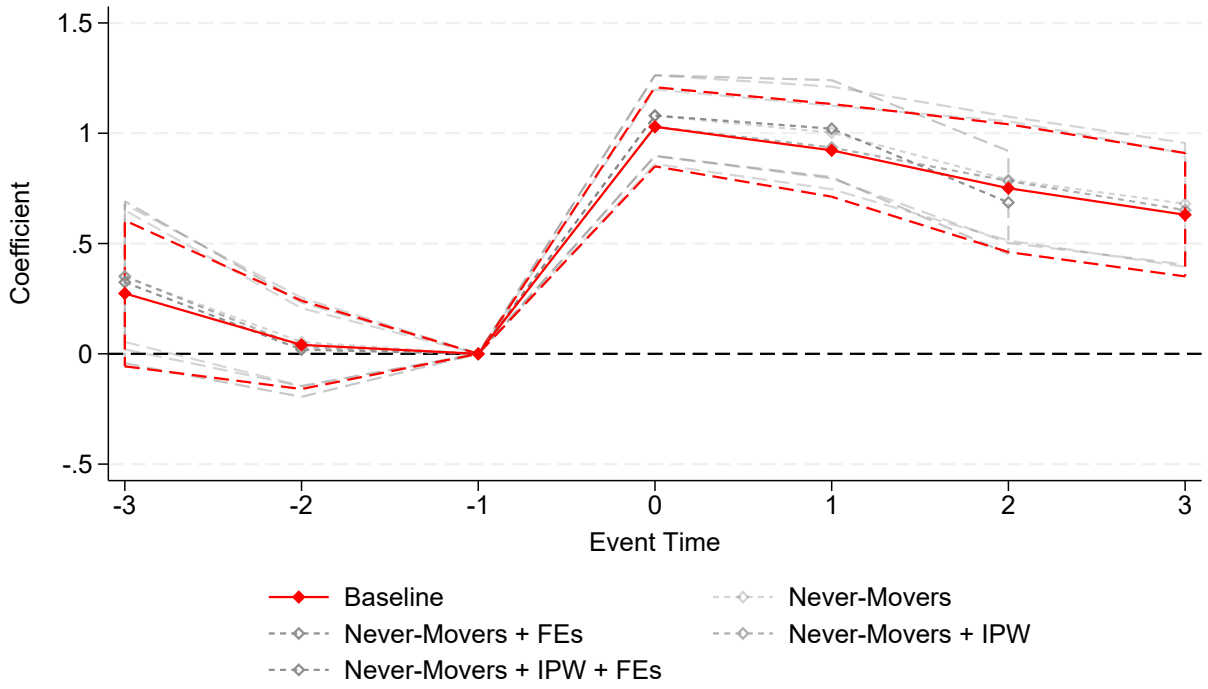
Note: Children who started in a Michigan public or charter school in Fall 2017 or Fall 2018. We omit schools with fewer than 25 students fitting the sample criteria and exclude reports from March 2020 through June 2021.

Figure A2: Robustness of Event-Study Estimates to Alternative Specifications



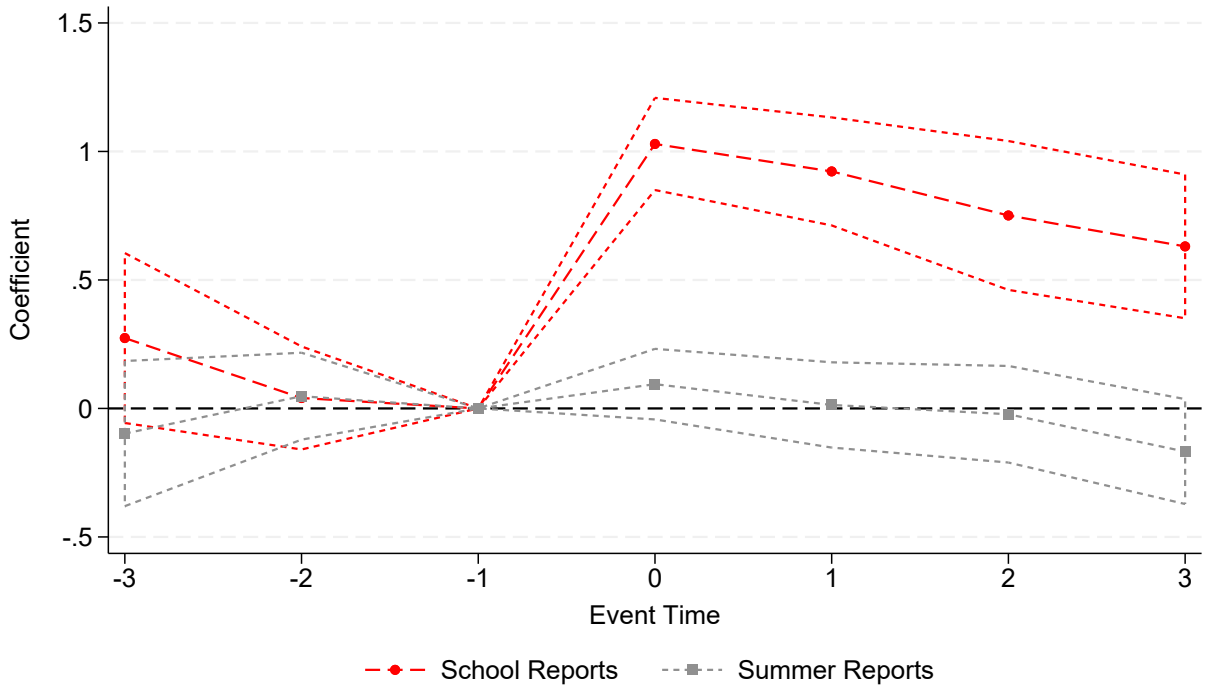
Note: Event study estimates of the change in student reports from school staff when they switch to schools with higher reports-added. Coefficients are estimated from a model containing only students who switch schools in 2nd or 3rd grade, except for the 4th grade model. We include grade and year fixed effects as well as the main effect for the change in reports-added. We present the interaction coefficient between event-time surrounding the switch and the difference in the old and new school's reports-added. FE = student fixed effects; in these models we pool all post-switch periods starting with $t + 2$ to provide the necessary degrees of freedom. We restrict to students who only switched schools once. Standard errors are clustered at the student and post-switch school level.

Figure A3: Robustness of Event-Study Estimates to Including Never-Movers



Note: Event study estimates of the change in student reports from school staff when they switch to schools with higher reports-added. Coefficients are estimated from a model containing only students who switch schools in 2nd or 3rd grade as well as students who never move schools. We include grade and year fixed effects as well as the main effect for the change in reports-added. We present the interaction coefficient between event-time surrounding the switch and the difference in the old and new school's reports-added; the event-time indicators are all set to zero for never-movers. IPW = inverse propensity weighting, where we estimate propensity scores using a logistic regression with student demographics, reporting histories, and neighborhood characteristics from their first year in Michigan public schools. FE = student fixed effects; in these models we pool all post-switch periods starting with $t+2$ to provide the necessary degrees of freedom. Standard errors are clustered at the student and post-switch school level.

Figure A4: Event-Study Estimates of School and Summer Reports Surrounding Moves



Note: Event study estimates of the change in student reports from school staff when they switch schools. Event studies show the changes in risk-adjusted reporting probabilities when students switch to schools with reports-added higher by one. Reports-added is estimated on a sample of students who never switch schools. Coefficients are estimated from a model containing only students who switch schools in 2nd or 3rd grade. We include grade and year fixed effects as well as the main effect for the change in reports-added. We present the interaction coefficient between event-time surrounding the switch and the difference in the old and new school's reports-added. We restrict to students who only switched schools once. Standard errors are clustered at the student and post-switch school level.