The School to Prison Pipeline:

Long-Run Impacts of School Suspensions on Adult Crime

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Abstract

Schools face important policy tradeoffs in monitoring and managing student behavior. Schools with strict disciplinary policies may stigmatize suspended students and expose them to the criminal justice system at a young age. On the other hand, school discipline is also designed to address the negative spillover impacts of misbehavior on the learning of other students. In this paper we estimate the impact of school discipline practices on student achievement, educational attainment and adult criminal activity. We show that there is wide and persistent variation in suspension rates across schools. Using exogenous variation in school assignment caused by a large and sudden school zone boundary change and a supplementary design based on principal switches, we find that schools with stricter discipline practices have substantial negative long-run impacts. Students who attend a school with a 10 percent higher number of suspensions are 10 percent more likely to be arrested and 12 percent more likely to be incarcerated as adults. We also find negative impacts of school suspension on high school graduation and four-year college attendance. The impacts are largest for males and minorities. Our findings highlight the large social cost and limited incapacitation impact of harsh school suspension policies.

I. Introduction

Early school experiences are important predictors of future criminal behavior. Attending school for an additional year reduces students' likelihood of engaging in subsequent criminal activity (Anderson, 2014; Cook & Kang, 2016; Lochner & Moretti, 2004), as does enrolling in a higher quality school and being exposed to more advantaged peers (Billings, Deming & Rockoff, 2014; Cullen, Jacob, & Levitt, 2006; Deming, 2011). Yet there is little evidence of the exact mechanisms by which schools can have a long-run influence on criminal activity. One possibility is school discipline. Schools with strict disciplinary policies (e.g., zero-tolerance) may expose students to the criminal justice system at a young age, and there is a positive correlation between being suspended in school and later-life engagement with the criminal justice system (Fabelo et al., 2011). Beyond the use of sworn officers directly arresting or reporting young offenders to school administrators (Owens, 2017; Weisburst, 2019), disciplinary practices of suspensions and expulsions may impact educational achievement, educational attainment and adult criminal justice activity. These long-term effects may be a result of suspended or expelled students associating with at-risk peers or being labeled as troublemakers. Because of these concerns, in 2014, the Obama administration urged schools to limit exclusionary discipline practices (U.S. Department of Education [ED], 2014).

In this paper, we study the impact of school discipline on the educational achievement, educational attainment, and subsequent criminal activity of students. We first show that schools vary widely in average suspension rates. Drawing on the teacher value-added literature, we estimate school effects on suspensions by conditioning on student characteristics and student achievement. Like the average suspension rates, school effect estimates vary widely across schools, but – unlike raw suspension rates – they are not related to observable student characteristics. Exploiting a large and sudden boundary change in Charlotte-Mecklenburg schools (CMS) in the fall of 2002, when about half of CMS students attended a new school, we find that schools' predicted suspension rates stay largely the same across school years even when student composition changes dramatically.

We first estimate school effects on suspensions using data prior to 2002. We then compare middle school students who lived in the same neighborhood and were previously assigned the same schools, but who lived on opposite sides of a newly drawn boundary. We find that students who are assigned to the "stricter" school are significantly more likely to be suspended in the 2002-03 school year, even though we define our school suspension effect based on data *prior* to the redrawing of school boundaries.[1]

Our findings imply that a one standard deviation increase in the school suspension effect increases the actual number of days suspended by slightly more than a third of a day, or about 16 percent. We then estimate the relationship between schools' suspension effects and measures of educational achievement, attainment, and subsequent criminal activity. We find that schools with greater suspension effects have negative impacts on student outcomes. Students assigned to schools that are one standard deviation "stricter" are 1.7 percentage points more likely to drop out of school (a 15 percent increase) and 2.4 percentage points less likely to attend a 4-year college (an 11 percent decrease). We also find large impacts on adult crime outcomes. Students assigned to schools that are one standard deviation "stricter" are: 3.2 percentage points more likely to have ever been arrested (a 17 percent increase), 2.5 percentage points more likely to

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[1] This identification strategy was employed in Billings, Deming & Rockoff (2014), but they focus on the impacts of a change in student racial composition on student outcomes. Here, we implement this same identification strategy to control for student sorting, but create a measure of school disciplinary effects that is independent of changes in school racial composition.

have ever been incarcerated (a 20 percent increase), have 0.14 more adult arrests (a 25 percent increase) and 0.11 more distinct incarceration spells (a 28 percent increase).

When schools suspend students, they either place them in a classroom for the day with other suspended students or send them home. We find larger impacts for in-school suspensions, which is consistent with a large literature on the long-term negative influence of school peers (Black, Devereux, & Salvanes, 2013; Carrell, Hoekstra & Kuka, 2018; Gould, Lavy, & Paserman, 2009). We find that the impacts of attending a stricter school are largest among minorities and males, suggesting that strict suspension policies may serve to expand pre-existing gaps educational attainment and incarcerations.

A key concern in this analysis is whether variation in "strictness" across schools is due to policy choices made by administrators or underlying variation in school context. To understand the role of administrators, we collect archival data on principals from school websites and estimate the principal effects on school suspension. Using a switching design in the spirit of Chetty, Friedman and Rockoff (2014), we show that principals' estimated effects on school suspensions predict changes in actual school suspensions after they change schools, and that principals explain roughly one-third of the variation in school discipline practices.

Our results have important implications for school disciplinary and criminal justice policies. School suspensions have large negative impacts on longer-term outcomes that mirror the negative impact of early exposure to the criminal justice system (Aizer & Doyle, 2015; Mueller-Smith, 2015; Dobbie, Goldin & Yang, 2018). Importantly, our research design captures the *total* impact of school discipline on the school population, including any positive spillover from incapacitating disruptive peers. We find no statistically significant impact on achievement and can rule out positive impacts of school discipline above 0.04 standard deviations. Thus, while a comprehensive benefit-cost analysis is beyond the scope of this paper, it seems unlikely that the gains from removing disruptive peers would outweigh the substantial long-term costs to students who are suspended because of harsher disciplinary policy.

II. Institutional Context

II.A. The Redrawing of School Boundaries

Prior to the summer of 2002, CMS operated under a court-ordered desegregation plan that used busing to achieve racial integration in schools. This use of race-based busing was a continuation of past court cases and policies enacted to follow the landmark Supreme Court decision Brown vs. Board of Education in 1954. In 1997, a CMS parent sued the district because their child was denied entrance to a magnet program based on race (*Capacchione v. Charlotte-Mecklenburg Schools*). This case led to a series of court battles that ended in April of 2002 and forced CMS to end race-based busing. Over the summer of 2002, school attendance boundaries were redrawn under a court order that prohibited the use of race in student assignment. Decisions about where to draw the boundaries were based only on school capacity and the geographical concentration of students around a school (Smith, 2004; Mickelson et al., 2009; Billings et al., 2014). This mechanical redistricting process rarely took advantage of environmental features such as streams and major roads, and was controversial because it often bisected existing neighborhoods.

Figure 1 provides an illustration of redistricting for two middle schools in our sample. The top panel shows school zone boundaries in the school year prior to the change and the bottom panel shows boundaries in the school year following the change. In the center of both panels, we outline one example census block group, where students in the same census block group, who previously were assigned to the same school, are subsequently assigned to different middle schools with substantially different suspension rates (i.e., share of students suspended per year). Approximately 50 percent of students were reassigned to a new school over the summer of 2002.

II.B. Suspension Policies in CMS

Most public schools allow for considerable principal discretion in policies around school suspension, with formal procedures reserved only for more serious long-term suspensions and expulsions. The main guidelines regarding student suspensions for our study are based on NC Department of Education and the Charlotte-Mecklenburg student conduct handbook, which outlines the procedures for student suspensions.[2] Suspension policies involve a range of discretionary practices such as parental meetings, after-school interventions, and in-school suspensions. Even the process for short-term out-of-school suspension is almost completely at the discretion of the principal and only long-term suspensions of 11 days or more require superintendent approval.[3] Concern regarding the potential negative long-term effects of school suspensions made recent headlines and resulted in a moratorium on K-2nd grade suspensions.[4]

Figures 2 and 3 provide an illustration of the distribution of school suspension rates across our sample of middle schools. Figure 2 shows a wide range in the share of students ever

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[2] The CMS student code of conduct is available online. (3/4/2019) http://schools.cms.k12.nc.us/croftES/Pages/StudentCodeofConduct.aspx

[3] N.C. Gen. Stat. '115C-391(c). Principals have the authority to suspend, for up to ten days, any student who violates the code of student conduct. Districts need not allow appeals from these short-term suspensions. Stewart v. Johnston County Bd. of Educ. 129 N.C. App. 108, 498 S.E.2d 382 (1998). Suspensions of eleven days up through the end of the school year must be approved by the superintendent.

[4] Recent press coverage (7/20/2016). *CMS reviews school suspension policy.* *https://www.wcnc.com/article/news/education/cms-reviews-school-suspension-policy/278241914*

suspended (in- or out-of-school) in 2003, with a mean of 22 percent and a standard deviation of 10 percent. Much of this variation could simply reflect differences in student characteristics. Because of this concern, our preferred measure of school suspension effects conditions on student demographics and baseline test scores. Figure 3 shows that even after including these controls, there is considerable variation in suspension rates across schools with a standard deviation of about 5.7 percent. Appendix Figures A1 and A2 show that using total days suspended generates similar variation.

III. Data

We use administrative records that track all CMS school students longitudinally from 1998-99 through 2010-11. The data include information on student demographics (e.g., gender, race), state test scores for grades 3 through 8 in math and reading, and annual counts of days suspended. These data also include students' home addresses in every year, which we use to determine students' school assignments under the busing and post-busing regimes.

In order to match longer-term criminal justice outcomes to our school records, we incorporate administrative records for all adult (defined in North Carolina as age 16 and above) arrests and incarcerations in Mecklenburg County from 1998 through 2013.[5] The arrest data include information on the number and nature of charges, and the incarceration data include a time and date of entry and exit, with stints in county jail and state prison both included in length of incarceration for individuals who serve concurrently. These data allow us to observe future

[5] We match students uniquely based on full name and date-of-birth using the same procedure that has been incorporated and verified in Deming (2011), Billings et al. (2014), and Billings, Deming and Ross (2019). Our match rate between the criminal records and student records is 94%.

criminal behavior of CMS students within Mecklenburg County, regardless of whether they transfer or drop out of CMS schools.

We also incorporate data on college attendance records from the National Student Clearinghouse (NSC), a nonprofit organization that provides degree and enrollment verification for more than 3,300 colleges and 93% of students nationwide. NSC information is available through the summer of 2009 for every student of college age who had ever attended a CMS school, including students who transfer to other districts or private schools or who drop out of school altogether.

We define residential neighborhoods within Mecklenburg County using the 371 block groups from the 2000 Census with at least one CMS student. We use address records from each school year to assign students to 2000 census geographies and middle school zones for both the pre- and post-2002 boundaries. Because families may sort in response to the policy change, it would be problematic to use their contemporaneous addresses to assign students to neighborhoods and school zones. Instead, we assign every student to pre- and post-2002 school zones based on their earliest listed address, which is observed in spring 1999 in most cases.

Our main analysis sample is restricted to the 2002-03 school year, as these are the students who experienced the effect of the school boundary change, but we use student data prior to 2002-03 in the estimation of school effects. We focus our attention on middle school students. We exclude elementary students because few students are suspended during these years, making it difficult to estimate schools' causal effects on suspension. Moreover, because our data on college attendance and crime end in 2009 and 2013 respectively, we are not able to observe these outcomes for many of the younger cohorts at the time of the boundary change. We exclude high school students because they are legally able to drop out of school at age 16 and thus we are concerned that they may leave the sample in ways that are correlated with the suspension effects of their assigned high school.

Sixth graders in 2002-03 who progress through school at the normal rate of one grade a year would enter 12th grade in the 2008-09 school year. Because our data on crime extends through 2013, we use two main measures of criminal activity: whether the individual was arrested between the ages of 16 and 21 and whether the individual was incarcerated between the ages of 16 and 21. This allows us to observe crime outcomes for all students who were in grades 6 through 8 in 2002-03. We also measure the number of arrests and number of incarceration stints between the ages of 16 and 21. Our data on college attendance end in the summer of 2009, which limits our ability to examine longer run measures of educational attainment such as college degree completion. Thus we focus on seventh and eighth grade students and measure whether they attended college within 12 months of the fall after their expected high school graduation date.

We restrict our sample to the approximately 98% of middle school students in 2002-03 with valid address information. Following Billings et al. (2014), we chose to further limit our sample to the 88% of students who were enrolled in CMS in 2001-02 in order to control for school and neighborhood prior to the boundary re-zone, leaving us with 26,246 students.

Table 1 lists descriptive statistics for this sample. Overall, 48% of students are black, 39% are white, and 8% are Hispanic. On average, 23% of students are suspended at least once, and the average suspension duration is 2.3 days. Of this sample, approximately 12% eventually drop out of CMS, while approximately 23% attend a 4-year college within 12 months of their expected high school graduation. Between the ages of 16 and 21, 19% are arrested at least once and 13% are incarcerated at least once. 51 percent of students were reassigned to a new school in 2002-03.

IV. Empirical Analysis

There are two key parts to our empirical strategy. In the first part, we generate predictions of school effects on suspensions. The goal of this step is to separate school effects on suspensions from the characteristics of students they serve. We draw on methodology from the teacher value-added literature, which estimates teacher effects on student achievement (e.g., Chetty et al., 2014; Kane & Staiger, 2008).

While this literature on teacher value-added finds that teachers' estimated effects largely capture their true causal effects, this method may not necessarily yield unbiased estimates of a schools' causal effects on suspensions. Therefore, in the second part of our analysis we leverage the re-zoning of CMS schools in 2002-03, when students who lived in the same neighborhoods and previously attended the same school were assigned to different schools. Using variation in school assignment from this natural experiment, we estimate the extent to which school effects predicted prior to 2002-03 impact subsequent suspensions and long-run educational and criminal outcomes.

IV.A. Estimating School Effects

To estimate school effects on suspensions, we generate suspension residuals after controlling for observable student baseline characteristics. We use only data from school years prior to the boundary re-zoning (i.e., 1999-00 and 2000-01[6]) to fit the following OLS model:

$$S_{i,g,t} = \beta_0 A_{i,g-1,t-1} + \beta_1 X_{i,g,t} + \Gamma_t + \mathrm{H}_g + \nu_{i,g,t},$$

$$\text{where } \nu_{i,g,t} = \mu_s + \theta_{s,t} + \varepsilon_{i,g,t}. \tag{1}$$

$S_{i,g,t}$ represents the z-scores of the total number of days student *i* is suspended in year *t*, which includes in-school and out-of-school suspensions. We also estimate several alternative specifications using different outcome variables, including ISS z-scores, OSS z-scores, test score z-scores, and indicator variable of whether a student was ever suspended in a given school year. Similar to the value-added literature (e.g., Chetty et al., 2014; Kane & Staiger, 2008), we control for a vector of student-level observable baseline test scores ($A_{i,g-1,t-1}$), comprising a cubic polynomial of prior-year test scores on state English and mathematics tests. For students with no baseline test information, we set test scores equal to zero and include an indicator variable for having a missing test score. We also control for race, gender, special education status, and limited-English proficiency ($X_{i,g,t}$), year fixed effects (Γ_t) and grade fixed effects (H_g).[7]

Following Kane and Staiger (2008), we decompose the student-year level residuals from Equation 1 ($\nu_{i,g,t}$) into the component that is attributable to schools (μ_s), the component that is attributable to year-to-year school-level variation ($\theta_{s,t}$), and the component that attributable to

[6] Although we could also include 2001-02 (the year directly before the boundary re-zoning) to estimate school effects, we exclude this year because student test scores from this year will be used as control variables in our reduced form models.

[7] In our preferred specification, we do not include peer controls or school controls (i.e., peer- or school-level aggregates of these student-level controls), since Kane et al. (2013) find that including these controls *adds* bias to teacher value-added models, but test for the importance of peers in our discussion of mechanisms.

student-level idiosyncratic error ($\varepsilon_{i,g,t}$).[8] Using these variance components, we generate an empirical Bayes shrunken estimate of school effects by multiplying school-by-year level average residuals from Equation 1 ($\bar{v}_{s,t}$) by an estimate of their reliability, accounting for the different number of students in each school per year ($n_{s,t}$):

$$\hat{\mu}_s = \bar{v}_{s,t} * \left(\frac{\hat{\sigma}_\mu^2}{\hat{\sigma}_\mu^2 + \left(\sigma_\theta^2 + \left(\hat{\sigma}_\varepsilon^2 / n_{s,t}\right)\right)/2} \right). \quad (2)$$

We present the distribution of our shrunken estimates of school effects ($\hat{\mu}_s$) in Figure 4, both for suspensions and test scores. The standard deviation of $\hat{\mu}_s$ is 0.091 for suspensions and 0.038 for test scores.[9] These results for test scores are consistent with prior work (Deming, 2014), and suggest substantially larger school effects on suspensions, relative to test scores. One explanation for this difference is that school leaders—and their policies—likely have greater direct control over suspensions than students' test score outcomes.

IV.B. Impacts of Schools on Suspensions, Education, and Crime

The re-zoning of CMS schools in the 2002-03 meant that students who live in the same neighborhoods and previously attended the same school could be assigned to attend very different schools in 2002-03. Following Billings et al. (2014), we leverage the variation caused by this natural experiment, to estimate the effects of students who live in the same

[8] We estimate the school-level variance (σ_μ^2) as the year-to-year covariance in school-by-year average residuals: $\hat{\sigma}_\mu^2 = Cov(\bar{v}_{s,t}, \bar{v}_{s,t-1})$. We estimate the student-level idiosyncratic variance (σ_ε^2) as the variance in within-school deviations in student outcomes: $\hat{\sigma}_\varepsilon^2 = Var(\bar{v}_{s,t} - v_{i,g,t})$. Finally, we estimate the year-to-year school-level variation (σ_θ^2) as the remainder of the total variation: $\hat{\sigma}_\theta^2 = Var(\varepsilon_{i,g,t}) - \hat{\sigma}_\mu^2 - \hat{\sigma}_\varepsilon^2$.

[9] For sensitivity analyses, we generate additional school effects on ISS, OSS, and an indicator of ever suspended. The SD for school effects on ISS z-scores is 0.134, school effects on OSS z-scores is 0.060, and school effects on an indicator of annual suspensions is 0.037.

neighborhoods and attended the same school in 2001-02, but were re-zoned into two different schools in 2002-03. To do so, we estimate the following OLS model:

$$Y_i = \beta_0 \hat{\mu}_s + \beta_1 A_i + \beta_2 X_i + \eta_{z,j} + \gamma_g + \epsilon_i. \tag{3}$$

Where Y_i represents a range of student-level outcomes on student behavior (e.g., suspensions), education (e.g., achievement, attainment) and criminal outcomes (e.g., arrests, incarcerations). Similar to Equation 1, we condition on a third-order polynomial of baseline test scores (A_i) and demographic characteristics (X_i), though these controls are only included for precision. We also include fixed effects for the 2001-02 school zone-by-neighborhood ($\eta_{z,j}$) and cohort (γ_g).

Our main parameter of interest is β_0, which represents the relationship between predicted school effects ($\hat{\mu}_s$) and outcome (Y_i). With old school zone by neighborhood fixed effects, β_0 is identified by students who live in the same neighborhood, were assigned the same school in 2001-02, and assigned to different schools in 2002-03 as a result of the newly drawn boundary. In neighborhoods where there is no new boundary, $\hat{\mu}_s$will have the same value for all students and thus will not contribute to the estimation of β_0. We define neighborhoods using census-block-groups (CBGs). CBGs contained a median number of 177 middle school students in 2002-03. Despite this relatively small definition of neighborhood, 50% of students in our sample had a new boundary drawn through their neighborhood.

We focus on the reduced-form effect of being assigned to a new school. An obvious alternative is to use the assigned school effect as an instrument for attended school effect as part of a two-stage least squares (2SLS) procedure. However, we choose to follow the approach described by Billings et al. (2014) for the same reasons they identify. Of primary concern is that, to use a 2SLS procedure, we would need to account for differential exposure to the new school zone boundaries (e.g., 6th graders will have more exposure than 8th graders) and the choice of choice of scaling requires strong assumptions about the cumulative effects of exposure to the treatment. We also do not know the effects of schools outside of CMS, which presents problems for students who leave the district. Even if we knew the appropriate scaling factor, it would be impossible to apply it to students who leave CMS. Therefore, we choose to focus our main results on the reduced form effects of school assignment.

IV.C. Balance Checks

The primary threat to our identification strategy is that students are systematically sorted across opposite sides of a newly drawn school boundary in a way that is correlated with school suspension effects. This non-random sorting would confound our estimates of the effect of being re-zoned to a school with a different suspension effect. We cannot measure the relationship between unobserved student characteristics and predicted school effects, but we can test whether students' observed baseline characteristics (e.g., race, gender, baseline test scores test scores) are systematically correlated with being re-zoned to a school with higher or lower suspension effects.

To test this, we estimate a regression like Equation 3, except with $\hat{\mu}_s$ as the outcome variable and demographics and prior test scores as the key independent variables, along with old school zone by neighborhood fixed effects. We then conduct an F-test for the joint hypothesis that all the covariates are equal to zero. The results in Table 2 show that none of the coefficients are individually statistically significant, and we fail to reject this hypothesis that all coefficients are jointly equal to zero.

V. Main Results

Table 3 contains our main results. We estimate the reduced form relationship between assigned school suspension effects and a range of student outcomes.[10] Because the outcomes have different scales, we transform school effects into school-level standard deviation units for ease of interpretation. The outcome in column (1) is the average number of days students are suspended in middle school, beginning in the 2002-03 school year. A one standard deviation increase in the estimated school effect increases the average annual number of days suspended by 0.38, a 16 percent increase. Columns (2) and (3) show this result is split across an increase of 0.08 days for in-school suspensions and 0.30 days for out-of-school suspensions, which correspond to increases of 18 and 16 percent respectively. We also observe an imprecisely estimated increase in an indicator variable of whether a student was ever suspended in a given school year.

Columns (5), (6), and (7) present results for educational achievement and attainment. The outcome in column (5) is the average of standardized scores on math and reading state tests.[11] We find no evidence that school suspension effects have an impact on students' academic achievement. Because we measure the total effect on students, this may be due to a balancing of two opposing forces: negative effects of lost instructional time for those students who were suspended and positive effects of reduced number of disruptive peers in the classroom for students who were not (Kinsler, 2013).

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[10] As described above, we prefer a reduced form interpretation to these results, as it makes no assumptions about the cumulative effects of attending a school for multiple years. An alternative approach is to scale the reduced form estimates using a first stage estimate of the assigned school effect on an attended school effect. Our estimate of this first stage parameter is 0.34, and it is significant at the less than 1 percent level.

[11] To increase precision, we average across math and reading outcomes on state standardized tests in middle school, beginning in the 2002-03 school year.

While we find no evidence that suspensions impact achievement on state tests, the results in columns (6) and (7) suggest that suspensions negatively affect educational attainment. Column (6) shows that a 1 SD increase in assigned school suspension effect increases the likelihood that a student subsequently drops out of school by 1.7 percentage points, a 15 percent increase. Column (7) shows that a 1 SD increase in assigned school suspension effect decreases the likelihood of attending a 4-year college by 2.4 percentage points, an 11 percent decrease.

Columns (8) through (11) present results for adult crime. In column (8) the outcome is an indicator for whether a student has ever been arrested in Mecklenburg County between the ages of 16 and 21; in column (9), the outcome is an indicator for whether a student has ever been incarcerated in county jail or state prison between the ages of 16 and 21. We find that students assigned a school with a 1 SD higher school effect on suspensions are about 3.2 percentage points more likely to have ever been arrested and 2.5 percentage points more likely to have ever been incarcerated, which correspond to an increase of 17 percent and 20 percent of their respective sample means. In addition to indicators of ever being arrested and incarcerated, we examine number of distinct arrests and incarceration spells in columns 10 and 11. Students assigned a school with a 1 SD higher school effect on suspensions have an average of 0.14 more arrests and 0.11 more incarcerations, which correspond to an increase of 25 percent and 28 percent over their sample means. In Appendix Table A1, we disaggregate our main crime outcomes by type of crime. We find no effects on serious violent crime (i.e., murder, manslaughter, rape, robbery, and aggravated assault), but positive effects on serious property crime (e.g., arson, burglary, larceny, and motor vehicle theft) and positive effects for all other crime (e.g. drugs, fraud/forgery, simple assault, trespassing, vandalism etc.).

The bottom of Table 3 disentangles the effect of in-school suspensions (ISS) and out-of-school suspensions (OSS). To do so, we estimate school effects on each outcome separately and then fit a version of Equation 3 that includes both school suspension effects. For almost all outcomes, the effects load exclusively on ISS. The only exception is for college outcomes which are primarily driven by OSS. The ISS results may be driven by pooling at-risk youth together in lieu of a normal classroom environment, which is consistent with prior work showing that school peers have relatively larger effects on antisocial behavior than neighborhood peers (Billings & Hoekstra, 2019). The role of OSS in college outcomes may be a result of the more serious stigma of OSS on student records and later teacher/principal perceptions which may influence formal and informal recommendations to attend college.

V.A. School Effects on the Extensive Margin

Thus far, our results have focused on the intensive margin: school effects on the number of days students are suspended. An increase in the average number of days students are suspended could be accomplished by suspending the same students for longer periods, or by suspending more students. We also estimate school effects on the extensive margin by re-estimating Equation 1 using an indicator of whether a student was suspended as the outcome (as opposed to the number of days suspended in previous analyses).

In Table 4, we present an analogous set of results using this measure of the school effect. The results of this analysis are consistent with our main results. Students assigned a school with a 1 SD higher suspension effect are suspended 0.32 more days per year, an increase of 14 percent from the mean. Like our main results, there are also increases in the likelihood of being suspended, but this result is imprecisely estimated. We do not find any statistically significant effects on test scores or likelihood of dropping out of school, but we estimate a decrease in likelihood of attending a 4-year college of 2.1 percentage points, a decrease of 9 percent. For adult arrest outcomes, we again find strong evidence that being assigned to a school that is more likely to students increases the probability that students experience negative crime outcomes later in live. We find that students assigned a school with a 1 SD higher school effect on suspensions are about 2.5 percentage points more likely to have ever been arrested and 2 percentage points more likely to have ever been incarcerated, increases of 13 percent and 16 percent, respectively. Number of arrests increase by 0.12 and number of incarceration spells increase by 0.09, increases of 21 percent and 23 percent, respectively.

V.B. Variation in Effects by Race and Gender

Table 5 shows results by race and gender. We define minority students as black and Hispanic and all other students as non-minority. Panel A shows that being assigned to a strict school has larger effects for minority students across nearly every outcome. Assignment to a 1 SD higher suspension school increases the average number of suspensions by roughly half a day for minority students, which is nearly three times as large as the effect for non-minorities. Effects on adult crime are also substantially larger for minority students: assignment to a 1 SD higher suspension school increases arrests by 4 percentage points and incarceration by 3.1 percentage points, compared to 2.5 and 1.8 percentage points for non-minority students. Differences are even larger for number of arrests and incarcerations. Panel B presents results by gender. We find substantially larger effects for male students across nearly every outcome.

Panel C presents results by race and gender together. The negative effects of suspensions are heavily concentrated among minority male students. Minority males assigned a 1 SD higher suspension school are suspended for 0.87 more days, roughly six times the effect for non-minority males. The negative long run effects of suspensions are also largest for minority males. The most pronounced differences are the number of incarcerations and arrests, where the effects are roughly three to four times as large for minority males as other groups.[12]

While the overall effects of suspensions on long run outcomes are overwhelmingly negative – especially for minority men – we do find positive effects on the academic achievement of non-minority male students assigned to strict schools. Consistent with prior studies (e.g., Carrell & Hoekstra, 2010), this suggests that removing disruptive peers from the classroom may have positive spillover impacts on educational achievement. However, unlike recent work on the long-run effects of disruptive peers (e.g., Carrell et al., 2018), we find no evidence of long-run benefits.

VI. Mechanisms

VI.A. Student Characteristics

One potential explanation for our main results is that school suspension effects are driven by variation in exposure to peers. For example, using the same boundary change, Deming et al., (2014) find that students have lower test scores when assigned to schools with higher concentrations of minority students. If school effects on suspensions were correlated with characteristics of peers, our results could be driven by peer influence. Our identification strategy

[12] In Appendix Table A2, we also present the results by gender and risk quartile, which is defined as the estimated number of suspended days based on student demographics and prior achievement. We find a similar pattern described above, where negative effects are concentrated among the males with the greatest risk of suspension.

– which relies on variation in *assigned* school – is robust to any individual sorting after this assignment, but could be affected by if peers move non-randomly into schools after rezoning.

To test for the influence of peers, we re-estimate our main specification replacing the outcome variable with characteristics of the actual peers in the assigned school. We present the results of this test in Appendix Table A3. Our preferred estimates of school effects are unrelated to peer characteristics at the 5% level; of the nine tests, one is significant at the 10 percent level and the magnitude is small. As a point of comparison, we also test the relationship between peer characteristics and a "naïve" school effect, which is generated using the same methodology described in Equations 1 and 2, but does not control for student achievement or demographics. The naïve estimate is significantly related (at the 1% level) to peer baseline test scores, proportion black, proportion Hispanic, and proportion of students with limited English proficiency. These results highlight the importance of controlling for student baseline characteristics when estimating school effects on suspensions, and provide reassurance that peer characteristics do not drive our results.

VI.B. School Quality

A second potential explanation for our results is that the estimated school effects on suspensions are capturing an overall measure of school quality, rather than the distinct effect of suspensions. For example, Deming (2011) shows that students who attend higher quality schools are less likely to be arrested and incarcerated. The pattern of results described above could, therefore, be explained by an overall school quality effect that is negatively correlated with the suspension effect. To test for this, we examine the correlation between school suspension effects and school test score effects.[13] The correlation is very small (0.026) and is not statistically significantly different from zero. Moreover, suspension effects explain less than 1% of the variation in test scores effects. Figure 5 shows a scatterplot of school effects on the two outcomes.

We also test for school quality effects by re-estimating Equation 3, including suspension effects and test score effects together in a "horse race" specification. The results are in Table 6. We find that suspension effects are nearly identical to our main results (Table 3) and that the school effects on test scores are not statistically significant predictors of any of our outcomes. Overall, we find that school suspension effects are distinct from effects on academic achievement.

A related concern is that we may be capturing variation in teacher quality, rather than variation in school-level suspension policies. The literature on teacher value-added, for example, consistently finds more variation in teacher effects within schools than school-level variation (e.g., Bacher-Hicks, Kane, & Staiger, 2014; Chetty et al., 2017). To compare the variation at the teacher and school levels, we re-estimate Equation 1 with teacher random effects nested within school random effects.[14] The results of this analysis, presented in Table 7, show substantially more variation at the school level for suspensions than test scores. In addition, we find substantially less variation at the teacher level for suspensions compared to test scores. These

[13] The estimation of school effects for test scores is identical to our main school effect for suspensions with the only difference being the substitution of end-of-grade test scores for days suspended in Equation 1. To increase precision, we average across math and reading outcomes.
[14] Similar to Equation 1, we fit an OLS model of student suspensions ($S_{i,g,t}$) controlling for students' baseline achievement ($A_{i,g-1,t-1}$) students' demographic characteristics ($X_{i,g,t}$), year fixed effects (Γ_{t}) and grade-level fixed effects (H_g): $S_{i,g,t} = \beta_0 A_{i,g-1,t-1} + \beta_1 X_{i,g,t} + \Gamma_{\mathrm{t}} + \mathrm{H}_g + \nu_{i,g,t}$, where $\nu_{i,g,t} = \mu_s + \theta_{s,t} + \vartheta_j + \varepsilon_{i,g,t}$. Like Equation 1, we decompose the error term ($\nu_{i,g,t}$) into i.i.d. components for the school (μ_s), school-by-year ($\theta_{s,t}$), and student ($\varepsilon_{i,g,t}$). The only difference is that we now include an additional component for teacher (ϑ_j).

findings suggest that schools—rather than teachers—are the substantial driver of suspension effects.

VI.C. School Leadership

A third explanation is that school effects are driven by policies and practices of school leadership. To test for this, we estimate school effects separately for each year from 2001 through 2011, using an equation similar to Equation 1.[15] In Table 8 we present estimates of the autoregression of the estimate for each year on the estimate from prior year. In column (1), we show that—across all schools and years—the coefficient on the prior year is 0.937, with a standard error of 0.068. This indicates that the estimated effect from the prior year is a near perfect predictor of the school effect in the current year.

In column (2), we include a term interacting the prior-year school effect with an indicator variable for a new principal in the current year and find that having a new principal attenuates the year-to-year autocorrelation by approximately one-third, to 0.670.[16] While school principals are only one component of the leadership team within a school, this result indicates that a change in leadership substantially attenuates the relationship of school effects across years. Column (3) shows that changes in student composition in the summer of 2002 did not affect the strong autocorrelation in our school suspension effect, which suggests that school effects persist across large changes in student composition.

[15] We exclude the first year in our panel (1999-00) because we do not observe if a school has a new principal in that year.

[16] In order to determine if a school has a new principal, we compare annual historical snapshots of CMS school websites, which provide the name of each school's principal. We obtain this information through the waybackmachine.org, which archives most internet pages.

As a second test of the effect of school leaders on suspensions, we estimate principal effects on school suspensions and then measure the extent to which suspensions change as high- or low- value-added principals switch schools. This strategy is inspired by the teacher switcher quasi-experiments used in the value-added literature (Bacher-Hicks et al., 2014; Chetty et al., 2014). The main limitation of this analysis is the small number of principal switches in our sample. As is common in the teacher effects literature, we fit a leave-out value-added model in which we estimate principals' effects on suspensions using only data from other schools.[17] We then measure the extent to which principals' estimated effects correspond to changes in suspensions when principals switch schools.[18] The results are in Table 9. Column (1) shows the relationship between estimated principal effects and actual suspensions, based on nine principals entering a new school.[19] Though the results are based on a small sample, we find that when principals who are one standard deviation above average enter a school, suspensions increase by 0.41 standard deviations. Column (2) presents consistent evidence on the effects on suspensions after principals exit schools, based on nine principal exits. Finally, in the column (3), we stack

[17] The regression is similar to Equation 1, but includes principal-by-school level random effects instead of school-level random effects. We therefore capture the effect of each principal on suspensions in a given school. To avoid a mechanical relationship between principal effects and suspensions, we only use data from a principal's tenure in other schools when predicting their impact in a given school.

[18] We estimate an OLS regression on the full sample of 480 school-by-year observations from 2001 through 2011: $Susp_{j,s,t} = \beta VA_{j,s}^{-s} + \delta Switch_{j,s,t} + \Gamma_s + \Theta_t + \varepsilon_{j,s,t}$. $Susp_{j,s,t}$ is average suspension z-score of school s in year t with principal j. $VA_{j,s}^{-s}$ is the estimated leave-school-out suspension effect of principal j. $VA_{j,s}^{-s}$ is set to 0 for principals who do not switch (since they do not have a leave-school-out VA estimate) and $Switch_{j,s,t}$ is an indicator variable that is equal to 1 if a principal switches schools (i.e., has non-zero $VA_{j,s}^{-s}$). Γ_s and Θ_t are school and year fixed effects, respectively. Therefore, β captures the relationship between the leave-out principal effects and mean suspensions, based on all years that the principal j is in school s (relative to all other years with other principals for school s).

[19] Though principal turnover is common, there are only nine principal switches in consecutive years across the schools in our sample.

results across the nine principal entrances and nine principal exits, finding that a one-standard deviation change in principal effect corresponds to a 0.54 standard deviation change in suspensions. These findings align with the above analysis in Table 8 and provide additional evidence that suspensions are affected by school leadership.[20]

VII. Conclusion

Misbehaving peers can have strong negative impacts on other students in the classroom, and thus disciplinary policy is an important lever for schools and principals seeking to improve learning outcomes. In this paper we use a large and sudden change in school assignment to estimate the impact of suspensions on aggregate student outcomes. We find that schools with higher suspension rates have negative long-run impacts on students. Students who attend a school with a 10 percent higher suspension rate are 10 percent more likely to be arrested and 12 percent more likely to be incarcerated as adults. This shows that early censure of school misbehavior causes increases in adult crime – that there is, in fact, a "school to prison pipeline". We also find negative impacts on educational attainment and can rule out all but very small increases in student achievement due to incapacitation of disruptive peers.

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[20] Because we find evidence that principals influence school suspension rates, a concern is that they may switch schools endogenously in response to the redrawn school zone boundaries between 2002 and 2003. Principal movement is not uncommon in CMS. Between 2001 and 2011, approximately 25% of schools have a new principal in any given year. However, the number of schools with new principals in the year following the boundary change was quite similar, at 28%, suggesting that there was not an atypical level of principal movement as a result of the boundary change. As a test of the possible influence of endogenous principal movement, in Appendix Table A4 we present our main results restricted to only schools without any principal movement between 2002 and 2003. Though the smaller sample reduces precision, the results are consistent with our main findings.

A key concern in this study is whether variation in school "strictness" is due to policy choices made by administrators or underlying variation in school context. We find no evidence that our results are driven by increases in overall school quality or peer characteristics. Moreover, we show that school effects change when principals change schools. This provides suggestive evidence that school leadership – and possibly other policy choices that are correlated with the timing principal switches – drive differences in school suspension rates.

Our findings have important implications for school disciplinary and criminal justice policies. In 2014, the Obama administration issued the first national guidance on school discipline, urging schools to limit suspensions and other practices that remove students from the classroom (ED, 2014). However, with a changing political climate and little causal evidence—in support of or against—of the impact of exclusionary discipline on students, the U.S. Department of Justice and Department of Education issued a joint statement in 2018 rescinding the Obama-era guidance (ED, 2018). Our results contribute to this debate by demonstrating that exclusionary discipline practices have large negative impacts on adult crime and educational attainment.

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Table 1. Summary of Sample Descriptive Statistics

	Mean	SD
Male	0.508	0.500
Black	0.475	0.499
Hispanic	0.078	0.267
White	0.394	0.489
Limited English Proficiency	0.063	0.243
Special Education	0.097	0.296
Days Suspended	2.334	6.041
Days ISS	0.438	1.343
Days OSS	1.896	5.431
Ever Suspended Indicator	0.228	0.419
Ever ISS Indicator	0.163	0.369
Ever OSS Indicator	0.216	0.411
Test Scores (SD Units)	-0.036	0.996
Dropout Indicator	0.117	0.321
Attended 4-Year College Indicator	0.229	0.420
Arrested Indicator (16-21)	0.187	0.390
Incarcerated Indicator (16-21)	0.125	0.331
Number of Arrests (16-21)	0.569	1.779
Number of Incarceration Spells (16-21)	0.391	1.487
Assigned New School in 2002-03	0.505	0.500
N	26246	

Notes: These descriptive statistics are for school students in grades 6 through 8 in CMS the 2002-03 school year. Suspension outcomes are presented both in units of raw days and indicators of ever suspended in 2002-03. Test scores are the average of students' scores on the math and reading state tests and are standardized across the full sample by year and grade. Due to data limitations on college attendance, college outcomes are only presented for the 17,275 seventh and eighth grade students in our sample, and are measured as any attendance within the 12-month period after the student would have graduated on time from high school. Crime outcomes are measured beginning at age 16 through age 21.

Table 2. Tests of Covariate Balance

	Predicted School Effect on Suspensions	Predicted School Effect on Test Scores
Prior-Year Days Suspended	0.001	-0.000
	(0.001)	(0.001)
Prior-Year Test Scores	0.001	0.002
	(0.002)	(0.003)
Black	0.001	0.007
	(0.008)	(0.014)
Hispanic	-0.010	-0.001
	(0.008)	(0.011)
Male	0.001	-0.003
	(0.003)	(0.003)
Special Education	-0.001	0.001
	(0.004)	(0.005)
Limited English Proficiency	0.006	-0.001
	(0.009)	(0.009)
Demographics Missing	0.004	0.003
	(0.006)	(0.006)
Prior-Year Test Scores Missing	0.005	0.001
	(0.005)	(0.005)
P-value for joint hypothesis F-test (all coefficients = 0)	0.942	0.734
N	26246	26246

Notes: In this table, we present the results of regressions of school effects on a set of baseline variables. School effects are in school-level standard deviation units. Each regression includes neighborhood by old school zone fixed effects and grade fixed effects. We present the results for school effects on suspensions in column (1) and school effects on test scores in column (2). In the second to last row, we present the p-value on an F-test for the joint hypothesis that all the coefficients in each column are equal to zero. Standard errors are clustered at the neighborhood by old school zone level. *** p<0.01, ** p<0.05, * p<0.10

Table 3. Impacts of Days Suspended on Suspensions, Achievement, Attainment and Crime

	Days Susp.	Days ISS	Days OSS	Susp. Indicator	Test Scores	Dropout	4-Year College	Arrested (16-21)	Incarc. (16-21)	Number Arrests (16-21)	Number Incarc. (16-21)
Panel A: Main Results											
Sch. Effect on Suspensions	0.382***	0.078***	0.304***	0.013	0.004	0.017*	-0.024**	0.032***	0.025***	0.137***	0.110***
	(0.128)	(0.030)	(0.115)	(0.013)	(0.019)	(0.010)	(0.011)	(0.009)	(0.007)	(0.039)	(0.033)
N	26246	26246	26246	26246	21153	26246	17275	26246	26246	26246	26246
Panel B: Variation in Main Effects by ISS and OSS											
Sch. Effect on ISS	0.356***	0.089***	0.268**	0.011	-0.008	0.006	-0.009	0.025***	0.019***	0.145***	0.118***
	(0.118)	(0.031)	(0.113)	(0.012)	(0.021)	(0.015)	(0.009)	(0.009)	(0.006)	(0.045)	(0.035)
Sch. Effect on OSS	0.035	-0.024	0.058	0.003	0.019	0.022	-0.028**	0.013	0.011	-0.018	-0.020
	(0.150)	(0.026)	(0.149)	(0.010)	(0.025)	(0.019)	(0.013)	(0.016)	(0.012)	(0.056)	(0.052)
N	26246	26246	26246	26246	21153	26246	17275	26246	26246	26246	26246

Notes: Within each column and panel, we present the coefficient, standard error, and sample size from a separate estimate of Equation 3. The results are interpreted as the effect of being assigned to a school with a 1 SD increase in estimated school effect on days suspended. Each regression includes neighborhood by old school zone fixed effects. In this sense, we are comparing students who attended the same school in 2001-02 and lived in the same neighborhood but were assigned different schools in 2002-03. In addition to these fixed effects, all regressions control for lagged achievement on state tests, LEP status, SPED status, gender, race, and grade level. Standard errors are clustered at the neighborhood by old school zone level. * $p < 0.1$ ** $p<0.05$ *** $p < 0.01$

Table 4. Impacts of Suspension Likelihood on Suspensions, Achievement, Attainment and Crime

	Days Susp.	Days ISS	Days OSS	Susp. Indicator	Test Scores	Dropout	4-Year College	Arrested (16-21)	Incarc. (16-21)	Number Arrests (16-21)	Number Incarc. (16-21)
Panel A: Main Results											
Sch. Effect on Pr(Suspend)	0.317***	0.060***	0.256**	0.013	0.005	0.013	-0.021**	0.025***	0.020***	0.117***	0.089***
	(0.120)	(0.023)	(0.112)	(0.011)	(0.017)	(0.011)	(0.010)	(0.009)	(0.006)	(0.035)	(0.030)
N	26246	26246	26246	26246	21153	26246	17275	26246	26246	26246	26246
Panel B: Variation in Main Effects by ISS and OSS											
Sch. Effect on Pr(ISS)	0.297**	0.064**	0.233**	0.007	-0.011	0.004	-0.006	0.017*	0.011*	0.123***	0.099***
	(0.118)	(0.027)	(0.113)	(0.009)	(0.020)	(0.016)	(0.009)	(0.009)	(0.006)	(0.045)	(0.036)
Sch. Effect on Pr(OSS)	0.086	-0.024	0.111	0.011	0.019	0.021	-0.023*	0.015	0.015	0.000	-0.016
	(0.156)	(0.038)	(0.146)	(0.013)	(0.024)	(0.019)	(0.013)	(0.015)	(0.011)	(0.058)	(0.055)
N	26246	26246	26246	26246	21153	26246	17275	26246	26246	26246	26246

Notes: Within each column and panel, we present the coefficient, standard error, and sample size from a separate estimate of Equation 3. The results are interpreted as the effect of being assigned to a school with a 1 SD increase in estimated school effect on suspension likelihood. Each regression includes neighborhood by old school zone fixed effects. In this sense, we are comparing students who attended the same school in 2001-02 and lived in the same neighborhood but were assigned different schools in 2002-03. In addition to these fixed effects, all regressions control for lagged achievement on state tests, LEP status, SPED status, gender, race, and grade level. Standard errors are clustered at the neighborhood by old school zone level. * $p < 0.1$ ** $p<0.05$ *** $p < 0.01$

Table 5. Variation in School Suspension Effects by Race and Gender

	Days Susp.	Days ISS	Days OSS	Susp. Indicator	Test Scores	Dropout	4-Year College	Arrested (16-21)	Incarc. (16-21)	Number Arrests (16-21)	Number Incarc. (16-21)
Panel A: Effects by Race											
Minority student (N=14493)	0.468**	0.073*	0.395**	0.002	-0.000	0.015	-0.016	0.040***	0.031***	0.198***	0.156***
	(0.208)	(0.042)	(0.191)	(0.017)	(0.023)	(0.013)	(0.012)	(0.013)	(0.011)	(0.055)	(0.046)
	[3.755]	[0.611]	[3.144]	[0.443]	[-0.504]	[0.157]	[0.172]	[0.259]	[0.183]	[0.865]	[0.613]
Nonminority student (N=11753)	0.172	0.119***	0.053	0.035*	0.031	0.016	-0.019	0.025**	0.018**	0.053*	0.039*
	(0.156)	(0.040)	(0.140)	(0.018)	(0.021)	(0.014)	(0.025)	(0.012)	(0.008)	(0.030)	(0.023)
	[0.831]	[0.185]	[0.646]	[0.158]	[0.521]	[0.066]	[0.297]	[0.098]	[0.054]	[0.205]	[0.118]
Panel B: Effects by Gender											
Male student (N=13345)	0.582***	0.140***	0.442**	0.023	-0.006	0.030*	-0.020	0.042***	0.036***	0.216***	0.166***
	(0.218)	(0.050)	(0.203)	(0.019)	(0.021)	(0.016)	(0.014)	(0.014)	(0.011)	(0.062)	(0.054)
	[3.204]	[0.533]	[2.670]	[0.389]	[-0.128]	[0.137]	[0.199]	[0.257]	[0.185]	[0.896]	[0.645]
Female student (N=12901)	0.157	-0.003	0.160*	0.001	0.016	0.003	-0.029	0.021*	0.013	0.058*	0.049**
	(0.111)	(0.039)	(0.094)	(0.014)	(0.020)	(0.011)	(0.021)	(0.011)	(0.009)	(0.034)	(0.023)
	[1.662]	[0.303]	[1.359]	[0.240]	[0.033]	[0.096]	[0.258]	[0.114]	[0.063]	[0.231]	[0.129]
Panel C: Effects by Race and Gender											
Minority male (N=7320)	0.873**	0.186**	0.687**	0.015	-0.022	0.037**	-0.024	0.055***	0.045***	0.323***	0.241***
	(0.367)	(0.077)	(0.348)	(0.024)	(0.029)	(0.019)	(0.018)	(0.017)	(0.014)	(0.091)	(0.077)
	[4.822]	[0.750]	[4.073]	[0.525]	[-0.618]	[0.185]	[0.139]	[0.354]	[0.272]	[1.373]	[1.020]
Minority female (N=7173)	0.077	-0.047	0.124	-0.013	0.019	-0.004	-0.020	0.028*	0.017	0.078	0.064**
	(0.160)	(0.044)	(0.139)	(0.017)	(0.020)	(0.015)	(0.025)	(0.017)	(0.016)	(0.047)	(0.032)
	[2.666]	[0.469]	[2.196]	[0.360]	[-0.392]	[0.129]	[0.206]	[0.162]	[0.092]	[0.346]	[0.196]
Nonminority male (N=6025)	0.142	0.106**	0.036	0.050	0.061**	0.017	0.008	0.042**	0.032**	0.088	0.067
	(0.269)	(0.051)	(0.234)	(0.036)	(0.027)	(0.017)	(0.030)	(0.020)	(0.014)	(0.061)	(0.047)
	[1.236]	[0.271]	[0.966]	[0.223]	[0.463]	[0.078]	[0.273]	[0.139]	[0.078]	[0.316]	[0.188]
Nonminority female (N=5728)	0.260*	0.145*	0.115	0.030	-0.004	0.011	-0.032	0.017	0.011	0.024	0.021
	(0.141)	(0.078)	(0.092)	(0.021)	(0.033)	(0.017)	(0.041)	(0.016)	(0.011)	(0.029)	(0.024)
	[0.405]	[0.095]	[0.310]	[0.090]	[0.581]	[0.054]	[0.322]	[0.054]	[0.028]	[0.088]	[0.045]
N	26246	26246	26246	26246	21153	26246	17275	26246	26246	26246	26246

Notes: Within each column and for each subsample, we estimate a separate regression of Equation 3. We present the coefficient, standard error in parentheses, and the sample means of the outcome in brackets. Panel A presents the results by race. Panel B presents the results by gender. Panel C presents the results by race and gender. The results are interpreted as the effect of being assigned to a school with a 1 SD increase in estimated school effect on days suspended. Each regression includes neighborhood by old school zone fixed effects. In addition to these fixed effects, all regressions control for lagged achievement on state tests, LEP status, SPED status, gender, race, and grade level. We define ‘‘minority’’ as black and Hispanic students, and ‘‘nonminority’’ as all other ethnicities (including whites). Standard errors are clustered at the neighborhood by old school zone level. * $p < 0.1$ ** $p<0.05$ *** $p < 0.01$

Table 6. Comparison of School Effects on Suspensions and School Effects on Test Scores

	Days Susp.	Days ISS	Days OSS	Susp. Indicator	Test Scores	Dropout	4-Year College	Arrested (16-21)	Incarc. (16-21)	Number Arrests (16-21)	Number Incarc. (16-21)
Sch. Effect on Suspensions	0.371***	0.077**	0.295**	0.013	0.004	0.016*	-0.024**	0.031***	0.024***	0.133***	0.104***
	(0.127)	(0.030)	(0.115)	(0.013)	(0.019)	(0.010)	(0.011)	(0.010)	(0.007)	(0.039)	(0.034)
Sch. Effect on Test Scores	0.127	0.015	0.113	0.000	0.002	0.010	-0.002	0.005	0.012	0.056	0.063*
	(0.080)	(0.022)	(0.075)	(0.008)	(0.013)	(0.009)	(0.008)	(0.011)	(0.009)	(0.040)	(0.038)
N	26246	26246	26246	26246	21153	26246	17275	26246	26246	26246	26246

Notes: Each column presents the coefficient, standard error, and sample size from a separate estimate of Equation 3, which includes both school effects on suspensions and school effects on test scores as predictor variables. The results are interpreted as the effect of being assigned to a school with a 1 SD increase in estimated effect. Each regression includes neighborhood by old school zone fixed effects. In addition to these fixed effects, all regressions control for lagged achievement on state tests, LEP status, SPED status, gender, and race. Standard errors are clustered at the neighborhood by old zone and new school zone level. * $p < 0.1$ ** $p<0.05$ *** $p < 0.01$

Table 7. Decomposition of Variance at School-, Teacher-, Year- and Student-Level

	Days Suspended	Test Scores
School-level standard deviation	0.160	0.090
Within-school teacher-level standard deviation	0.059	0.228
Within-teacher year-level standard deviation	0.312	0.179
Idiosyncratic (student-level) standard deviation	0.815	0.445
Total SD	0.889	0.538
N (student-year-course)	115967	115967

Notes: This table uses student-year-course level data from grades 6 through 8 math and reading classrooms in 2000 and 2001 to estimate the variance at the school, teacher, year, and student-level idiosyncratic error. Each column presents a separate regression. The outcome in the first column is the number of days suspended z-score. The outcome in the second column is the average math and reading z-score. In each column, we report the raw standard deviation of suspension and test score residuals and decompose this variation into components driven by idiosyncratic within-year student-level variation, within-teacher year shocks, and within-school teacher variation, and persistent school-level variation across years. The corresponding variances to the standard deviations in rows 1 – 4 sum to total variance in row 5.

Table 8. Persistence of School Effects Across Years and Leadership Changes

	M1	M2	M3
Lagged School Effect	0.937***	0.947***	0.948***
	(0.068)	(0.062)	(0.062)
(Lagged School Effect) X (Indicator for New Principal)		-0.277**	-0.276**
		(0.124)	(0.123)
(Lagged School Effect) X (Indicator for 2003)			-0.012
			(0.211)
Indicator for New Principal	0.014	0.013	0.013
	(0.014)	(0.015)	(0.015)
N (school-by-year)	480	480	480

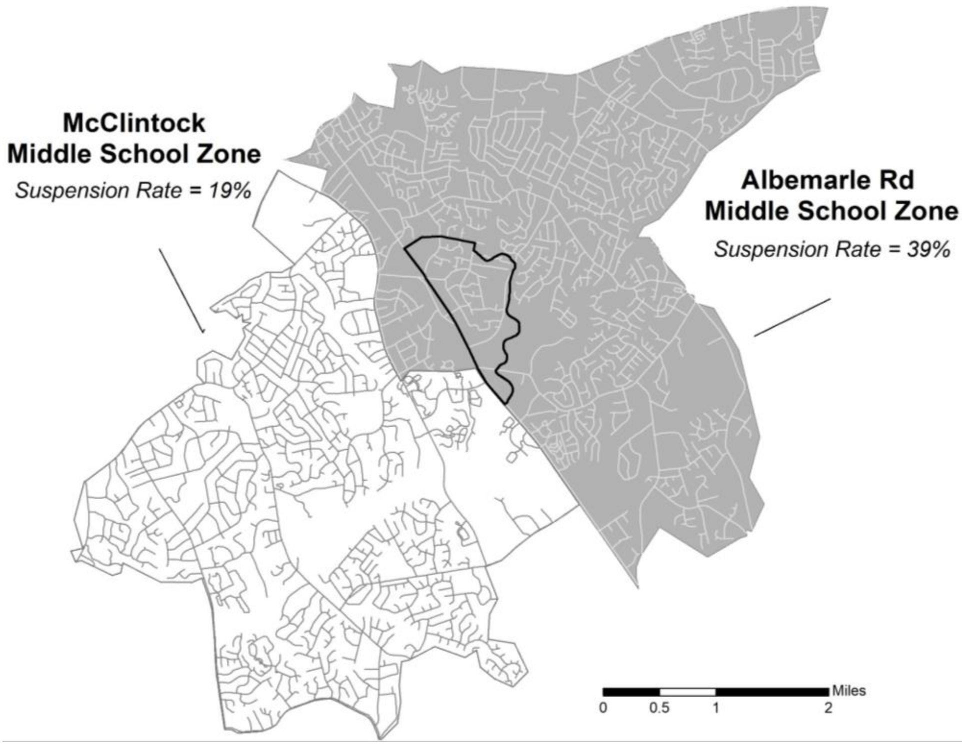
Notes: This table presents results of autoregressions of school-by-year effects on suspensions, using data from 2001 through 2011. We estimate each school-by-year effect using only data from each year and condition on baseline student test scores, student demographics, and grade fixed effects. All autoregressions regressions in this table include year fixed effects. Standard errors are clustered at the school level. * $p < 0.1$ ** $p<0.05$ *** $p < 0.01$

Table 9. Principal Effects on Suspensions

	Entry	**Exit**	**Entry and Exit**
Estimated Principal Effect	0.410**	0.523**	0.538***
	(0.183)	(0.262)	(0.204)
Principal Entrance Indicator	0.035		0.032
	(0.021)		(0.033)
Principal Exit Indicator		-0.044	-0.040
		(0.042)	(0.042)
N (school-by-year)	480	480	480

Notes: This table presents results of regressions of principals' estimated effects on school-by-year mean suspensions, using data from 2001 through 2011. We estimate principal effects using only data from other schools. All regressions in this table include school and year fixed effects. Standard errors are clustered at the school level. * p < 0.1 ** p<0.05 *** p < 0.01

Panel A. Before redistricting (2001-02 school year):



Panel B. After redistricting (2002-03 school year):

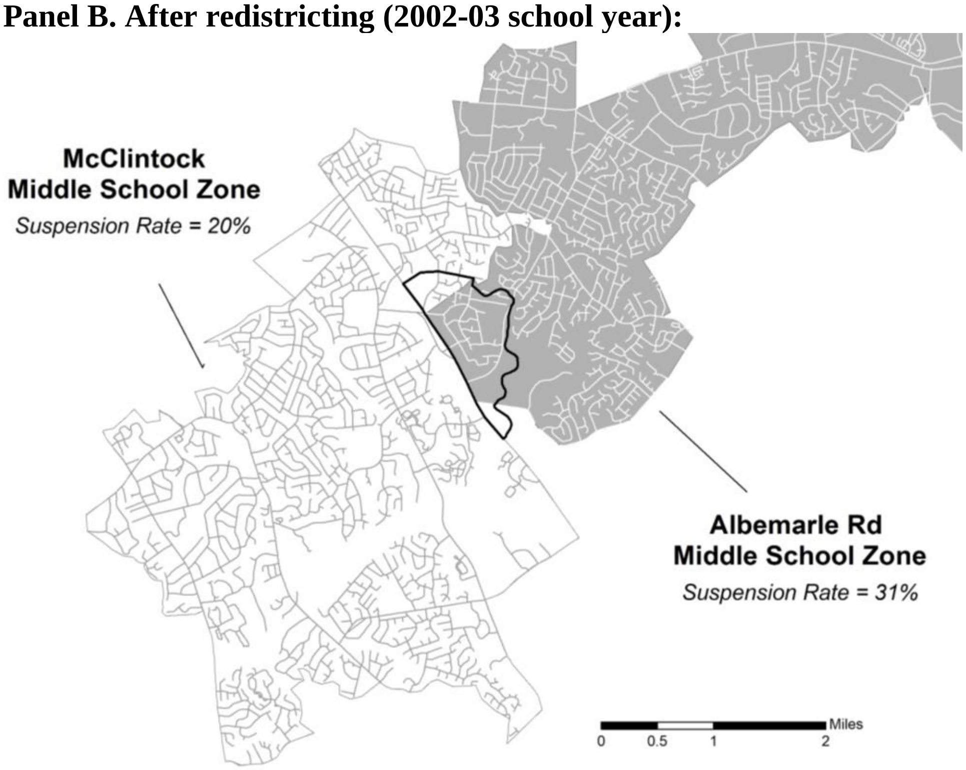


Figure 1. Redistricting for Two Middle Schools

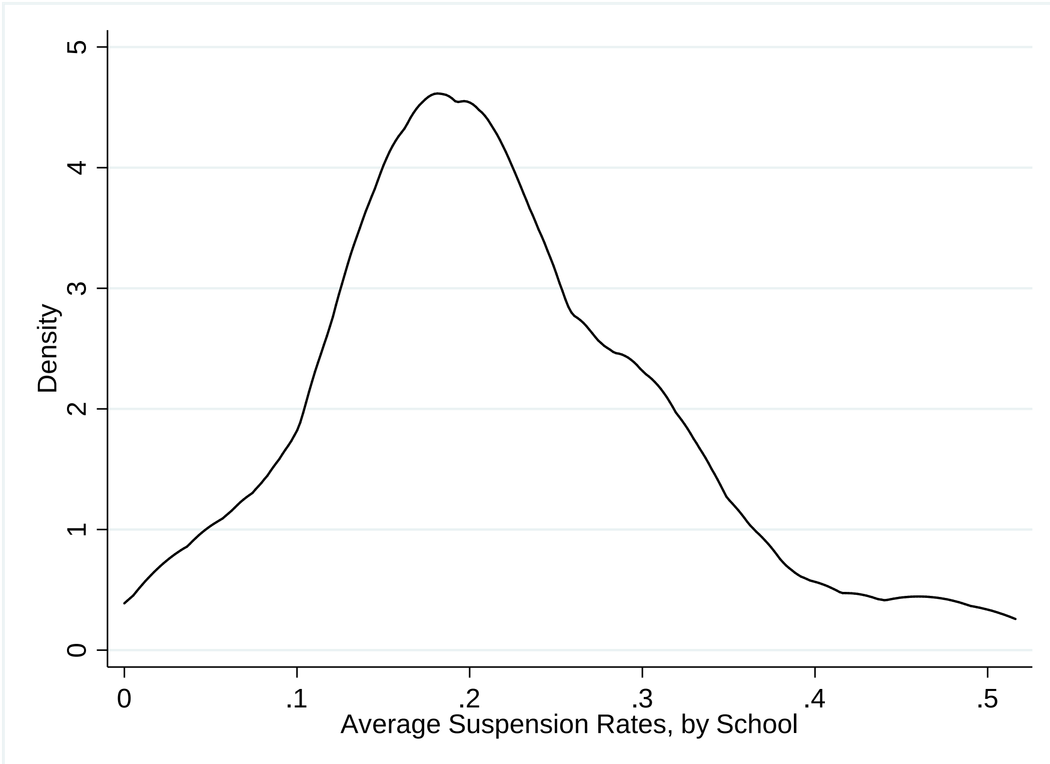


Figure 2. Distribution of Average Suspension Rates, by School

Notes: This figure plots the distribution of school average suspension rates, weighted by the number of students in each school. Sample includes all schools serving students in grades 6 through 8 in 2003. The distribution has a mean of 0.216 and standard deviation of 0.097.

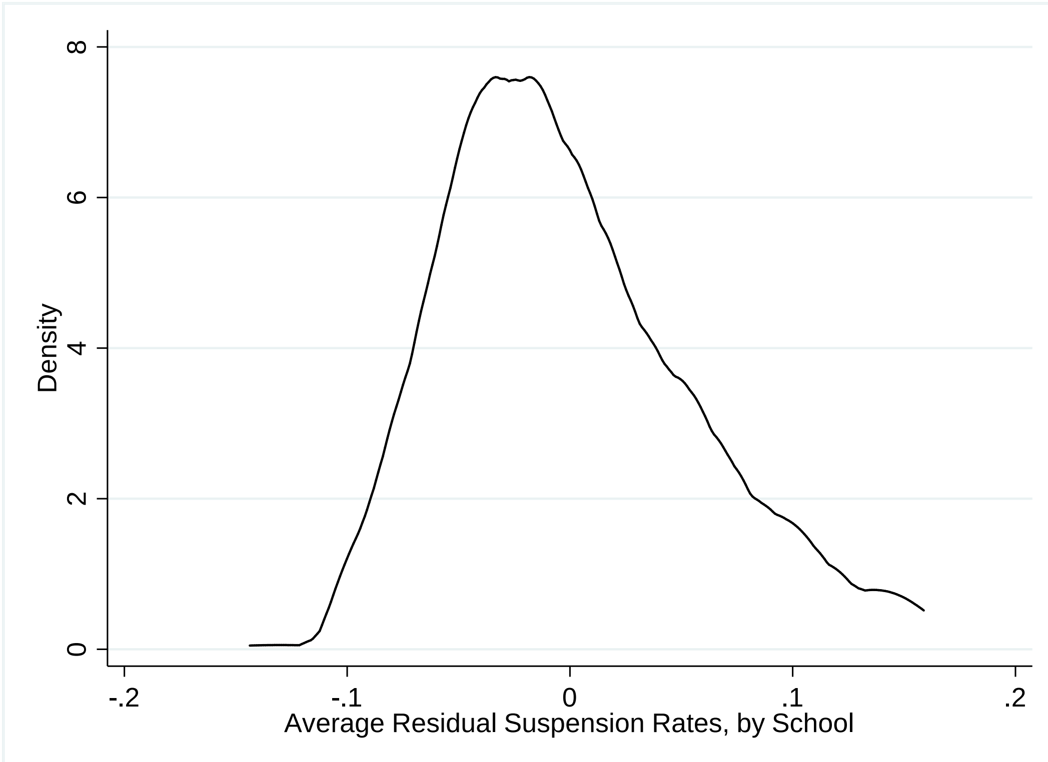


Figure 3. Distribution of Average Residual Suspension Rates, by School

Notes: This figure plots the distribution of school average residual suspension rates, weighted by the number of students in each school. Residuals are calculated at the student level, by conditioning on student demographics, baseline test scores, grade, and year. Sample includes all schools serving students in grades 6 through 8 in 2003. The distribution has a mean of 0.000 and standard deviation of 0.057.

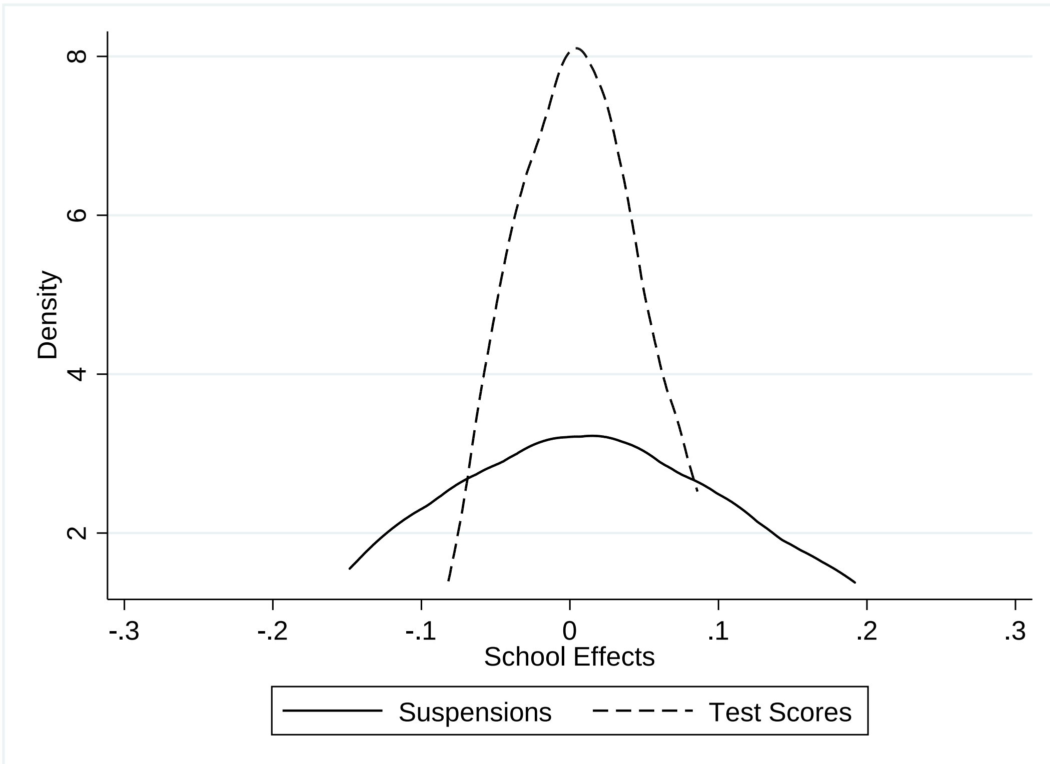


Figure 4. Empirical Distribution of School Effects

Notes: This figure plots kernel densities of the empirical distribution of school effects on suspensions and test scores, weighted by the number of students in each school. The standard deviations of the suspension effect and test score effect are 0.091 and 0.038, respectively.

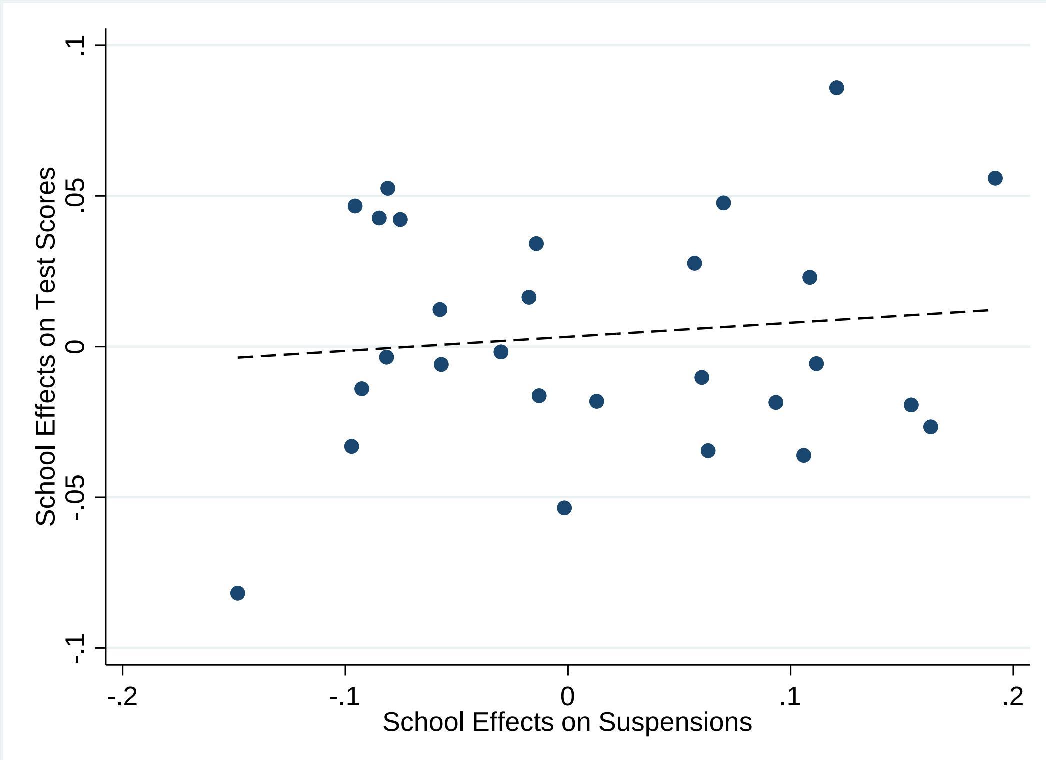


Figure 5. Comparison of School Effects on Suspensions and School Effects on Test Scores

Notes: This figure plots schools' estimated effects on test scores vs. their estimated effects on suspensions. Schools' estimated suspension effects explain less than 1% of the variance in their estimated effects on test scores and the relationship is not statistically significant at the 5% level.

Appendix

Appendix Table A1. Impacts of Days Suspended on Type of Arrest

	Serious Violent Crime Arrest (16-21)	Serious Property Crime Arrest (16-21)	Other Arrest (16-21)	Number of Serious Violent Crime Arrests (16-21)	Number of Serious Property Crime Arrests (16-21)	Number of Other (Non-Serious) Arrests (16-21)
Sch. Effect on Suspensions	0.001	0.018**	0.013**	0.000	0.040**	0.097***
	(0.004)	(0.006)	(0.005)	(0.005)	(0.016)	(0.025)
N	26246	26246	26246	26246	26246	26246

Notes: In this table we present the relationship between school suspension effects and subsequent type of arrest. Serious violent crimes are murder, manslaughter, rape, robbery, and aggravated assault. Serious property crimes are arson, burglary, larceny, and motor vehicle theft. Other arrest captures all other arrests, including drugs, fraud/forgery, simple assault, trespassing, and vandalism. The results are interpreted as the effect of being assigned to a school with a 1 SD increase in estimated school effect on days suspended. Each regression includes neighborhood by old school zone fixed effects. In addition to these fixed effects, all regressions control for lagged achievement on state tests, LEP status, SPED status, gender, race, and grade level. Standard errors are clustered at the neighborhood by old school zone level. * $p < 0.1$ ** $p<0.05$ *** $p < 0.01$

Table A2. Variation in School Suspension Effects by Gender and Risk Group Quartile

	Days Susp.	Days ISS	Days OSS	Susp. Indicator	Test Scores	Dropout	4-Year College	Arrested (16-21)	Incarc. (16-21)	Number Arrests (16-21)	Number Incarc. (16-21)
Panel A: Effects for Male Students by Risk Quartile											
Risk Quartile 1	0.004	0.129**	-0.126	0.093***	0.048**	-0.019	-0.038	0.067***	0.036**	0.086	0.034
	(0.258)	(0.057)	(0.265)	(0.033)	(0.023)	(0.017)	(0.052)	(0.022)	(0.014)	(0.076)	(0.058)
Risk Quartile 2	-0.197	-0.015	-0.181	-0.021	0.041	0.023	0.015	0.019	0.017	0.014	0.041
	(0.362)	(0.082)	(0.311)	(0.037)	(0.052)	(0.027)	(0.032)	(0.030)	(0.029)	(0.089)	(0.079)
Risk Quartile 3	0.213	0.152	0.061	0.034	-0.029	0.007	-0.000	0.041	0.070**	0.340**	0.310*
	(0.844)	(0.150)	(0.756)	(0.045)	(0.045)	(0.033)	(0.028)	(0.034)	(0.034)	(0.159)	(0.162)
Risk Quartile 4	1.981***	0.295***	1.686***	0.041	-0.047**	0.065**	-0.005	0.074**	0.039**	0.430***	0.258*
	(0.627)	(0.104)	(0.588)	(0.035)	(0.022)	(0.031)	(0.013)	(0.031)	(0.019)	(0.155)	(0.134)
Panel B: Effects for Female Students by Risk Quartile											
Risk Quartile 1	0.085	0.002	0.083	-0.003	-0.021	0.019	-0.128*	0.035	0.020	0.062	0.063
	(0.093)	(0.018)	(0.084)	(0.034)	(0.031)	(0.015)	(0.074)	(0.031)	(0.014)	(0.062)	(0.054)
Risk Quartile 2	0.120	0.114	0.006	0.028	-0.058	-0.014	0.006	0.006	-0.006	0.001	-0.009
	(0.114)	(0.081)	(0.118)	(0.021)	(0.070)	(0.023)	(0.040)	(0.025)	(0.011)	(0.030)	(0.020)
Risk Quartile 3	-0.064	-0.073	0.009	-0.005	0.022	-0.007	-0.026	0.034*	0.016	0.088	0.064
	(0.209)	(0.058)	(0.182)	(0.029)	(0.021)	(0.026)	(0.031)	(0.020)	(0.019)	(0.061)	(0.044)
Risk Quartile 4	0.455	0.061	0.394	0.004	0.031	-0.002	0.018	0.015	0.025	0.076	0.071
	(0.316)	(0.107)	(0.276)	(0.030)	(0.036)	(0.027)	(0.021)	(0.041)	(0.027)	(0.084)	(0.056)
N	26246	26246	26246	26246	21153	26246	17275	26246	26246	26246	26246

Notes: Within each column and for each subsample, we estimate a separate regression of Equation 3. We present the coefficient, and standard error in parentheses. Risk quartiles are defined by generating four equal sized groups of students for each gender, based on the predicted number of days suspended. We predict days suspended using student demographics and prior achievement. Quartile 1 indicates students least at risk of suspension; quartile 4 indicates those most at risk. The results are interpreted as the effect of being assigned to a school with a 1 SD increase in estimated school effect on suspension likelihood. Each regression includes neighborhood by old school zone fixed effects. In addition to these fixed effects, all regressions control for lagged achievement on state tests, LEP status, SPED status, gender, race, and grade level. Standard errors are clustered at the neighborhood by old school zone level. * $p < 0.1$ ** $p<0.05$ *** $p < 0.01$

Appendix Table A3. Relationship Between School Effects and Peer Characteristics

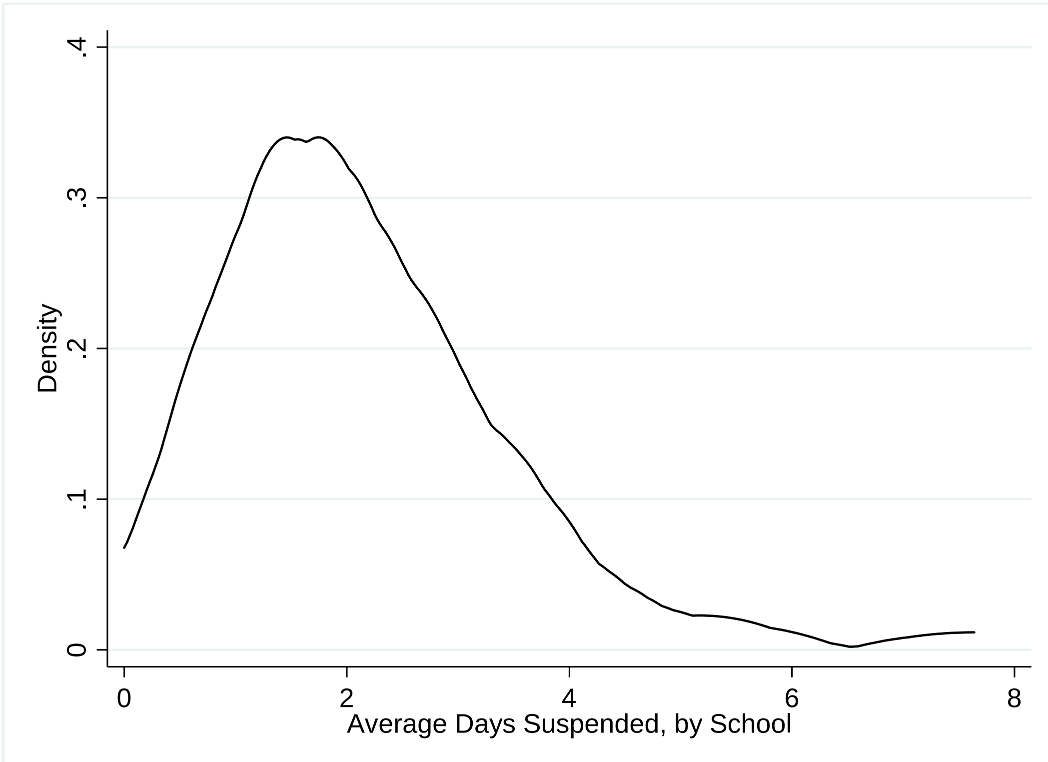
	Mean Baseline Test Scores	Proportion Missing Baseline Test Scores	Proportion Black	Proportion Hispanic	Proportion White	Proportion Male	Proportion SPED In Prior Year	Proportion LEP In Prior Year	Proportion Missing SPED or LEP
Preferred Sch. Effect	-0.010	-0.002	-0.006	0.007	0.004	0.003*	-0.002	0.008	0.001
	(0.031)	(0.005)	(0.031)	(0.007)	(0.032)	(0.002)	(0.004)	(0.009)	(0.004)
Naïve Sch. Effect	-0.128***	0.011	0.095***	0.025***	-0.120***	0.002	-0.000	0.030***	-0.003
	(0.018)	(0.007)	(0.025)	(0.007)	(0.024)	(0.002)	(0.004)	(0.007)	(0.005)
N	26246	26246	26246	26246	26246	26246	26246	26246	26246

Notes: Within each column, we present the coefficient, standard error, and sample size from a separate estimate of Equation 3. The results are interpreted as the effect of being assigned to a school with a 1 SD increase in estimated school effect. Each column contains a different outcome, identified by all other students in the school and year. Each regression includes neighborhood by old school zone fixed effects and grade level indicators. Standard errors are clustered at the neighborhood by old school zone level. * $p < 0.1$ ** $p<0.05$ *** $p < 0.01$

Appendix Table A4. Sensitivity of Main Results to Principal Switches

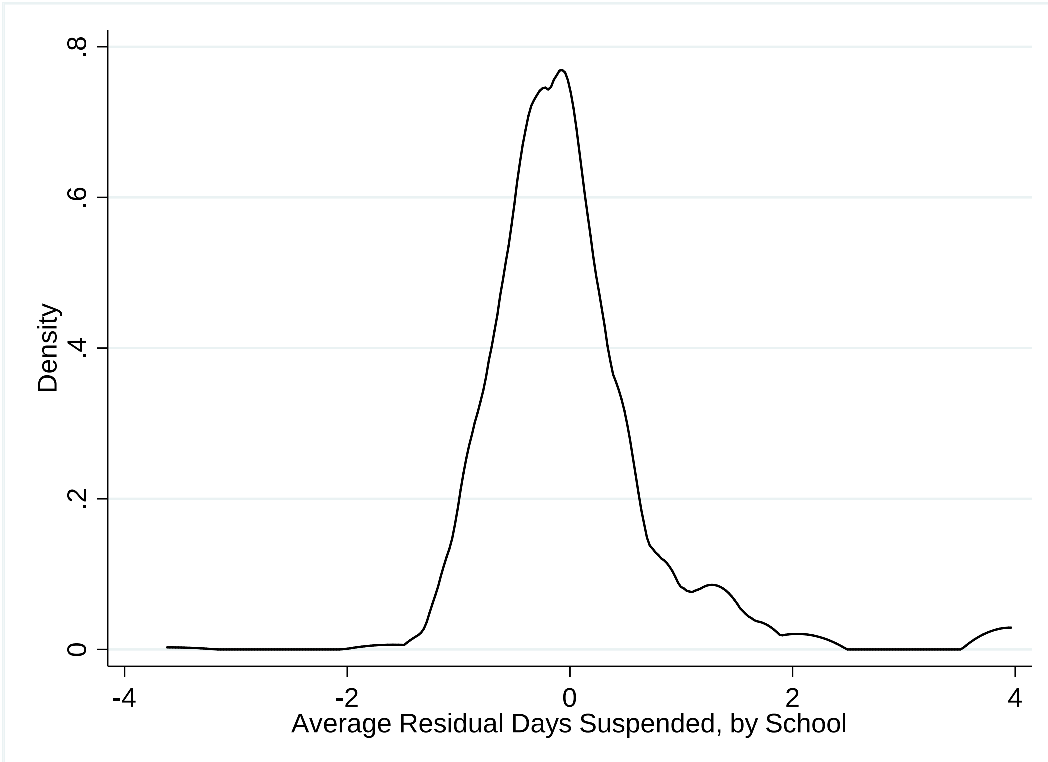
	Days Susp.	Days ISS	Days OSS	Susp. Indicator	Test Scores	Dropout	4-Year College	Arrested (16-21)	Incarc. (16-21)	Number Arrests (16-21)	Number Incarc. (16-21)
Sch. Effect on Suspensions	0.390**	0.076*	0.314*	0.016	-0.004	0.016	-0.016	0.023*	0.021***	0.128***	0.109***
	(0.178)	(0.040)	(0.166)	(0.017)	(0.023)	(0.010)	(0.011)	(0.013)	(0.008)	(0.049)	(0.037)
N	18833	18833	18833	18833	15180	18833	12397	18833	18833	18833	18833

Notes: Sample includes students in grades 6 through 8 in 2003 who were assigned schools that did not have a new principal. Within each column, we present the coefficient, standard error, and sample size from a separate estimate of Equation 3. The results are interpreted as the effect of being assigned to a school with a 1 SD increase in estimated school effect on days suspended. Each regression includes neighborhood by old school zone fixed effects. In addition to these fixed effects, all regressions control for lagged achievement on state tests, LEP status, SPED status, gender, race, and grade level. Standard errors are clustered at the neighborhood by old school zone level. * $p < 0.1$ ** $p<0.05$ *** $p < 0.01$



Appendix Figure A1. Distribution of Average Days Suspended, by School

Notes: This figure plots the distribution of average number of days suspended, weighted by the number of students in each school. Sample includes all schools serving students in grades 6 through 8 in 2003. The distribution has a mean of 2.158 and standard deviation of 1.332.



Appendix Figure A2. Distribution of Average Residual Days Suspended, by School

Notes: This figure plots the distribution of average residual number of days suspended, weighted by the number of students in each school. Residuals are calculated at the student-level, by conditioning on student demographics, baseline test scores, grade, and year. Sample includes all schools serving students in grades 6 through 8 in 2003. The distribution has a mean of 0.000 and standard deviation of 0.796.