

*Technical Report*

# DISPARITIES IN STUDENT DISCIPLINE BY RACE AND FAMILY INCOME

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## Disparities in Student Discipline by Race and Family Income

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### **Abstract**

This study explores student discipline disparities by race (black/white) and family income. First, we decompose gaps across districts, across schools in the same district, and within schools. Second, we assess disparities using regression models. Third, we examine punishments for fights between black and white or poor and non-poor students. We find that black and poor students are disciplined more often and harshly than their peers, with disparities arising across districts, across schools, and within schools. Moreover, black students receive slightly longer suspensions after interracial fights (controlling for discipline histories and background characteristics), suggesting at least some degree of intentional discrimination.

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## **I. Introduction**

In the United States, students of color are suspended and expelled from school at higher rates than white students. The U.S. Department of Education's Office for Civil Rights (2016) reports that, compared to white children, black children are 3.6 times more likely to receive an out-of-school suspension in preschool, 3.8 times more likely to receive an out-of-school suspension in grades K-12, and 2.2 times more likely to be referred to law enforcement or subject to a school-related arrest. Among K-12 students, 18% of black boys and 10% of black girls received an out-of-school suspension in 2013-14, compared to only 5% of white boys and 2% of white girls. Although the Office for Civil Rights does not release similar comparisons for poor and non-poor students nationwide, researchers have observed higher suspension rates for Arkansas students from low-income families than their peers (Anderson & Ritter, 2017) and found that black students who attend high-poverty schools are suspended at higher rates than black students who attend other schools (Loveless, 2017).

These gaps are among the most discussed and disputed topics in education policy today. The Obama administration issued a "Dear Colleague" letter in January 2014 that outlines the federal laws prohibiting discrimination in school discipline, provides examples of both intentional discrimination (e.g., a school issuing unequal punishments to "similarly situated" students of different races who get into a fight) and disparate impact (e.g., a facially neutral school uniform policy that adversely impact students of a particular race), and describes remedies for violations (U.S. Department of Justice & U.S. Department of Education, 2014). The letter has become a topic of fierce debate. Much of that debate focuses on the evidence, or lack thereof, that differences in suspension rates reflect discriminatory practices. Researchers have found evidence of discrimination in many aspects of American life, including employee hiring

(Lavergne & Mullainathan, 2004), criminal justice (Park, 2017), mortgage lending (Munnell et al., 1996), peer-to-peer lending (Pope & Sydnor, 2011), and medical care (Schulman et al., 1999). However, evidence of intentional discrimination in student punishment by race or family income remains elusive. This is true despite documentation of markedly different punishment patterns by race, family income, and gender (e.g., Skiba et al., 2012) and evidence that school leaders' incentives affect how severely they punish high-performing and low-performing students (Figlio, 2006).

Fundamental questions about discipline gaps remain, partly because identifying the causes of these disparities is empirically challenging. Gaps could arise from actual differences in student behaviors across groups, differences in how schools respond to the same behaviors from different groups, or a combination of the two. Ideally, researchers would be able to observe the true behaviors of many students, observe whether and how schools punish those behaviors, and then assess whether schools treat students of different backgrounds similarly. Unfortunately, this type of analysis is not feasible at a large scale using conventional data, which tend to include information about infractions but not behaviors.

What is possible, however, is a careful examination of where disparities arise, along with a targeted look at whether disparities appear where the likeliest explanation is intentional discrimination. For example, do gaps arise within schools, across schools in the same district, or across districts? To what extent do observably similar student behaviors give rise to disparate punishments? Answers to these questions would help to diagnose the causes of discipline gaps and identify their solutions. If the gaps arise from true differences in students' behaviors, then eliminating gaps might require addressing the root causes of student misbehavior, whether inside or outside of schools. If gaps arise within schools that punish black and poor students more

harshly than other students for similar behaviors, then eliminating gaps might require oversight and training of school staff. If, however, gaps arise across schools or districts (due to harsher discipline practices in schools with higher proportions of black or poor students), policymakers might need to compel certain schools to adopt less punitive practices.

This study uses statewide, student-level data from Louisiana from the 2000-01 through 2013-14 school years to examine gaps in exclusionary discipline between black and white students and between poor and non-poor students.<sup>1</sup> We define students' poverty status by their eligibility for free or reduced-price lunch (FRPL).<sup>2</sup> These data enable us to explore, within and across schools, whether schools punish black and poor students more often and more harshly than they punish white and non-poor students. Furthermore, the data distinguish between potentially violent or harmful infractions (such as possessing a weapon) and seemingly nonviolent infractions (such as disrespecting authority), which provides an opportunity to assess what types of offenses lead to students missing school and developing records of misbehavior.

This paper focuses on three sets of analyses. First, we decompose raw, black/white and poor/non-poor gaps in discipline into across-district, across-school-within-district, and within-school components. Second, to examine possible explanations for these gaps, we estimate the conditional probabilities of various discipline outcomes by race and poverty status, controlling for observable student characteristics, details about the infraction, and various combinations of school, grade, and year fixed effects. Third, to assess whether schools treat students of different backgrounds similarly, we test for gaps in a context in which gaps seemingly should not arise if not for discriminatory school practices: fights between black and white or poor and non-poor students with similar prior discipline records and background characteristics.

Our findings reveal clear patterns in exclusionary discipline by race and poverty status. First, large discipline gaps exist. Black students account for 46% of the student population during this period but 64% of the student suspensions, while poor students account for 62% of the population but 74% of the suspensions. These disparities are the product of substantial within-school and across-school gaps in discipline. Within a regression framework, being black and poor are consistently significant predictors of several discipline outcomes—being suspended, being suspended multiple times in the same year, and length of suspension in days—even in models with rich sets of covariates and fixed effects. Among those with no prior suspensions, black and poor students are more likely than their peers to be suspended for both a nonviolent infraction and a violent infraction, although suspensions for nonviolent infractions are more common. Finally, when black and white students with similar discipline records fight each other, black students tend to receive slightly longer suspensions. This finding, which is robust to numerous model and sample specifications, suggests at least some degree of intentional discrimination towards black students.

The paper proceeds with a description of the existing research on student discipline gaps. We then describe our data, methods, and results, before concluding with discussion of the study's implications for diagnosing and addressing these gaps.

## **II. Background on Discipline Gaps**

### ***A. The Incidence and Effects of Exclusionary Discipline***

Concerns about the overuse of exclusionary disciplinary practices have persisted for decades, along with concerns that the practices disproportionately affect certain populations. The Children's Defense Fund (1974, 1975) released reports in the mid-1970s with profiles of suspended and expelled students, noting that the suspension rate for black children was twice as

high as the rate for any other group and that many of those suspensions were imposed for non-dangerous, nonviolent infractions. More recently, in the summer of 2011, the U.S. Departments of Education and Justice announced a collaborative project, the Supportive School Discipline Initiative, that would “support the use of school discipline practices that foster safe, supportive, and productive learning environments while keeping students in school” (U.S. Department of Education, 2016). They reported that of the 49 million students enrolled in public schools in 2011-12, 3.5 million received an in-school suspension, 3.45 million received an out-of-school suspension, and 130,000 were expelled. Suspension and expulsion rates, according to their analysis, were three times higher for black students than white students.<sup>3</sup>

Many states and districts have responded to the heightened attention to discipline gaps—and clarification of the related Federal laws—by limiting, postponing, or outright banning exclusionary discipline practices (Anderson & Ritter, 2017). Steinberg and Lacoë (2017) reported that 22 states and the District of Columbia had revised their laws as of May 2015 to deemphasize exclusionary discipline, while 23 of the largest 100 school districts have required non-punitive discipline strategies or limits to the use of suspension. They noted evidence in U.S. Department of Education data of a recent decline in the rates of exclusionary discipline from 2006 to 2011. Such a decline is evident in California, which has emphasized reducing suspensions for relatively minor “disruption” or “willful defiance” incidents that account for a large portion of the gap in black and white suspension rates (Losen, Martinez, & Okelola, 2014; Loveless, 2017). A recent study of student discipline in Maryland also shows exclusionary discipline rates declining across student racial categories, although large black/white discipline gaps remain (Porowski, O’Conner, & Passa, 2014).

Experts have seen these rates, and the associated gaps between student subgroups, as problematic in part because of the associations between exclusionary discipline and negative outcomes for students. Being suspended from school is correlated with lower academic achievement (Arcia, 2006; Beck & Muschkin, 2012; Raffaele Mendez, Knoff, & Ferron, 2002; Raffaele Mendez, 2003; Skiba & Rausch, 2004), lower probability of on-time graduation (Ekstrom, Goertz, Pollack, & Rock, 1986; Raffaele Mendez, 2003; Suh, Suh, & Houston, 2007; Wehlage & Rutter, 1986), and greater contact with the juvenile justice system via the “school-to-prison pipeline” (Morgan, Salomon, Plotkin, & Cohen, 2014; Fabelo et al., 2011; Nicholson-Crotty, Birchmeier, & Valentine, 2009; Skiba, Arredondo, & Williams, 2014). These relationships are largely correlational, leaving unanswered questions about whether suspended students’ outcomes would have been better had they not been suspended or expelled (Steinberg & Lacoé, 2017). Yet the lack of causal evidence does not mean that causal relationships do not exist, and there is an intuitive argument that students are more likely to learn when they are present in school.<sup>4</sup>

### ***B. Possible Explanations for Gaps***

While certain groups of students are suspended and expelled at higher rates than their peers, basic questions about the causes of those gaps remain unanswered. Understanding gaps in student discipline requires attention to student behaviors, infractions, and punishments, and the relationships between them. We refer to *behaviors* as what students do in school, *infractions* as behaviors that schools document as misconduct, and *punishments* as the penalties associated with those infractions. In this context, discipline gaps could arise through some combination of three basic pathways. First, there could be actual differences in the behaviors of black or poor students and their peers. Second, there could be differences in the ways that schools translate behaviors to



infractions for black and poor students relative to their peers (e.g., if schools more commonly write up black students for the same behaviors). Third, there could be differences by race and income in the ways that schools translate infractions to punishments (e.g., if schools suspend black students for more days than white students for similar infractions).

The first possible explanation for discipline gaps—not exclusive of the others—is that students of different races and family incomes simply behave differently from one another. These differences, if they exist, could arise within schools (if poor and minority students behave differently from their schoolmates) or across schools (if students in schools with higher proportions of black or poor students behave differently from students in other schools).

Discipline gaps that arise from true differences in behaviors may not be attributable to discriminatory practices by schools. If, for example, black and white students are suspended at different rates or lengths of time but those differences are proportionate to these groups' actual rates and severity of misbehavior, then schools might be responding in nondiscriminatory ways to the behaviors they observe. Of course, this exonerates neither schools nor broader societal forces from contributing to varying levels of misbehavior. School culture can affect student behavior, perhaps especially for poor and minority students (Haynes, Emmons, & Ben-Avie, 1997), and black and poor students might behave differently from their peers due to their disproportionate experiences with poverty and the challenges that accompany it. This includes challenges related to physical health (Chen, 2004), stress and anxiety (Bradley & Corwyn, 2002), exposure to violence (Gorman-Smith & Tolan, 1998), and varying norms and home environments (Leventhal & Brooks-Gunn, 2000). A review of the related literature by Gregory, Skiba, and Noguera (2010) concluded that poverty-related factors likely account for part of the discipline gaps observed but that black/white gaps often persist after controlling for

socioeconomic status (SES). These black/white differences among students of similar SES might reflect actual differences in behaviors, with a variety of possible explanations that include cultural mismatches between students and teachers (Monroe, 2005), perceived pressures to “act white” (Fordham & Ogbu, 1986; Fryer, 2006), or students’ reactions to other forms of bias in schools (Gershenson, Holt, & Papageorge, 2016). True behavior differences are hard to identify empirically, since researchers seldom have data on students’ actual behaviors beyond whether schools recorded those behaviors as infractions.

A second explanation for discipline gaps is that schools might translate behaviors to infractions differently for students of different races or family income. Whether a student is referred to the office for punishment depends on both the student’s behavior and the educator’s perception of, or tolerance for, that behavior (Skiba & Williams, 2014). If a black student and white student engage in the same behaviors, the black student could be more likely to receive an infraction if teachers or administrators disregard, forgive, or never notice the white student’s misbehavior. This would be more plainly reflective of discrimination. It could arise across schools—without any particular school or person treating different groups differently—if schools with predominantly poor or minority students respond to misbehavior differently than other schools. It also could arise within schools as a direct result of discriminatory behavior.

Two recent studies provide suggestive evidence. Gilliam, Maupin, Reyes, Accavitti, and Shic (2016) asked 135 early childhood teachers to observe videos of four children—a black boy, black girl, white boy, and white girl—to watch for “challenging behavior in the classroom,” noting to the teachers that “sometimes this involves seeing behavior *before* it becomes problematic.” The researchers selected videos with, in their view, no signs of challenging behavior, and then tracked participants’ eye movements to see whether teachers tended to

monitor some students more than others when looking for misbehavior. They found that teachers focused a disproportionate share of their time gazing at black children, and particularly black boys. They accompanied this task with a vignette experiment to test whether teachers' attitudes differed toward students with stereotypically black and white names. They found that teachers rated white students' misbehavior as more severe, which they interpreted as consistent with a "shifting standards" hypothesis (Biernat & Manis, 1994). They found no relationship between the children's race and whether teachers thought they should be suspended or expelled.

Okonofua and Eberhardt (2015) experimented with names in a similar context. With a group of K-12 teachers as study participants, they described students who had misbehaved twice, randomly assigning participants to see either stereotypically black or white student names. They found that teachers felt more troubled by black students' misbehaviors and were more inclined to regard the black students as troublemakers. A second experiment, designed similarly to the first, showed that teachers were more likely to see themselves suspending the black students in the future.

If, as these studies suggest, teachers monitor and assess similar-behaving black and white students in their classrooms differently, we might expect within-school racial gaps in how schools translate behaviors to infractions. At the same time, across-school gaps could arise if schools with higher proportions of black students tend to write up students for behaviors that would not yield write-ups in other schools. An assortment of theories and findings support this possibility. Welch and Payne (2010) looked to schools to test the "racial threat" theory (Blalock, 1967) that authorities use more aggressive forms of control in settings with higher proportions of blacks relative to whites. Using survey responses from 294 public middle and high schools, they found that leaders of schools with higher proportions of black students reported being more

likely to use punitive and extremely punitive discipline, with those differences persisting even after controlling for an assortment of school characteristics.<sup>5</sup> This stricter enforcement in schools with higher proportions of black students seems consistent with a trend toward “no excuses” philosophies in urban schools (Thernstrom & Thernstrom, 2004). It is also consistent with findings that black/white gaps in office referrals often arise not from severe or objectively clear misbehaviors but rather from behaviors like loitering and disrespectful conduct that are more subject to the discretion of school officials (Shaw & Braden, 1990; Skiba et al., 2002).

Anderson and Ritter (2017) used longitudinal, student-level infraction data from Arkansas to study exclusionary discipline gaps within and across schools. The authors conditioned their analysis on a sample of students who were referred to the principal’s office for a behavioral infraction. Discipline outcomes were coded either exclusionary (i.e., expulsion or out-of-school suspension) or inclusionary/no discipline (i.e., in-school suspension or no discipline). Using logistic regression models that controlled for student and infraction characteristics (but omitted school fixed effects), they found that black students are approximately 2.4 times as likely as white students—and FRPL-eligible students are 1.2 to 1.5 times as likely as non-FRPL students—to receive exclusionary discipline. While the FRPL results persisted in models with school fixed effects, the race results did not, leading them to conclude that the key driver of black/white discipline gaps was likely related to differences in the types of schools that black and white students attend. There are two notable differences between the authors’ work and the current analysis. First, we focus on in-school and out-of-school suspensions as forms of discipline that remove students from their typical educational settings (disregarding distinctions in the location of that suspension). Second, we use a decomposition approach to explore the origins of discipline disparities. As this paper shows, comparisons of

regression models with and without school fixed effects can yield misleading impressions of the relative balance of within-school and across-school sources of the gaps. The addition of school fixed effects leads to students in relatively (or fully) segregated schools receiving little (or no) weight in the resulting analyses. This makes comparisons to models without school fixed effects difficult to interpret, as highly segregated schools play an important part in understanding across-school gaps.<sup>6</sup>

A third possible explanation for discipline gaps is that schools translate the same infractions to more severe punishments for black and poor students than their peers. For example, black and white students might both receive write-ups for chronic tardiness, but gaps would arise if schools punish black students more severely for that infraction. This type of gap also would suggest intentional discrimination, whether as a product of different treatment within schools or across them.

Similar to Anderson and Ritter (2017), Kinsler (2011) estimated differential referral rates for behavioral infractions, as well as the occurrence and length of North Carolina students' suspensions by race, using school fixed effects and indicators for infraction types. He found that black students were more likely to be referred to administrators for behavioral infractions, but once students were referred to the administrators, schools treated black and white students similarly for similar infractions, with most variation in discipline arising across schools. However, as noted previously, comparing within-school and between-school sources of discipline gaps in this context relies on potentially misleading comparisons of models with and without school fixed effects. Skiba et al. (2014) used hierarchical linear models to test the relationships between exclusionary discipline and the characteristics of infractions, student demographics, and schools in a Midwestern state. They found that variables at each level were

independently predictive of students' likelihood of being suspended. However, school-level variables explained a large part of the racial gaps in exclusionary discipline, rendering the contributions of infraction and student characteristics statistically insignificant.

Although recent attention to discipline gaps has generated more and better evidence about the origins of race- and income-based gaps in exclusionary discipline, there remains much to learn. This study contributes to this literature in several ways. First, perhaps the most elusive question in researching student discipline gaps is whether schools discriminate by punishing black and white or poor and non-poor students differently for the same behaviors. Using rich student-level discipline data from Louisiana, we can isolate punishments from infractions that seemingly should not generate gaps: fights between black and white, and poor and non-poor, students with similar prior discipline records. Second, we decompose discipline gaps into their within-school, across-school (within-district), and across-district components. In doing so, we show why comparing regression models with and without school fixed effects to infer about the relative within-school and between-school components of these gaps can yield misleading interpretations. Third, we use regression models to add nuance to our understanding of these gaps and to explore questions of policy interest (e.g., disaggregating suspensions for violent and nonviolent suspensions and considering students who develop records of repeated suspensions).<sup>7</sup> Fourth, by focusing on gaps in the southern state of Louisiana, we illuminate the race and class dynamics of student discipline in a state with large populations of black and poor students and a long history of problems where race, class, and schools collide (Egalite, Mills, & Wolf, 2017).

### **III. Data**

This study uses data provided by the Louisiana Department of Education (LDOE) for the 2000-01 through 2013-14 school years. LDOE provided records for students in grades K-12 in

Louisiana public schools, which includes both traditional public schools and charter schools.

Taken together, this provides nearly 10 million student-year observations, with approximately 1.8 million unique observations spread over 14 school years.

The LDOE data contain variables commonly found in state administrative data, including students' basic demographic characteristics (e.g., race, free or reduced-price lunch eligibility, gender, and special education status), grade levels, and test scores from Louisiana's state assessments, which we standardized by test type, subject, year, and grade. Our free/reduced-price lunch (FRPL) variable contains a non-missing value for each student observation, and we generally combine students' eligibility for free or reduced-price lunch into a single "FRPL" category to facilitate interpretation. As shown in Table 1, approximately 55% of the sample is eligible for free lunch and 7% is eligible for reduced-price lunch. A much higher percentage of black students (83%) than white students (42%) in the sample is eligible for FRPL. Our race variable contains a non-missing value for nearly all student observations. We coded that variable into three categories: black (46% of the sample), white (48%), and other race (5%), with the latter encompassing several racial and ethnic groups. The sample is split almost evenly between male (51%) and female (49%) students, and approximately 12% of students have a disability and the corresponding special education (SPED) status. SPED status is an important covariate for studying discipline gaps by race and poverty status, since black and poor students comprise a disproportionate share of the population of disabled students and students with disabilities are suspended at higher rates than their peers (Losen et al., 2015). This is true despite federal rules protecting students from punishments for behaviors caused by their disabilities.

LDOE also provided rich data on student infractions and their corresponding punishments. The infraction variable distinguishes between 49 types of offenses that could yield

a suspension. We coded these infractions as “violent” if they had a relatively high probability of inflicting serious physical or emotional harm on a classmate or staff member and “nonviolent” if they had a relatively low probability of inflicting serious harm.<sup>8</sup>

Table 1 shows large raw differences in suspension rates by race and poverty (FRPL) status. With respect to race, 25% of black students were suspended in a given year, with 13% suspended for a violent infraction and 19% for a nonviolent infraction (some students were suspended for both a violent and nonviolent infraction in the same year).<sup>9</sup> This compares to 12% of white students suspended in a given year, with 5% suspended for a violent infraction and 9% for a nonviolent infraction. With respect to FRPL status, 21% of poor students were suspended in a given year (11% for a violent infraction and 16% for a nonviolent infraction), which compares to 12% of non-poor students suspended (5% for a violent infraction and 10% for a nonviolent infraction).

Table 2 displays the counts and percentages of these infractions statewide—and disaggregated for black, white, poor (FRPL), and non-poor (non-FRPL) students. The nine most common infraction types account for 92% of the recorded infractions. We grouped all other infraction codes in an “Other” category. The most common infractions, in order, are willful disobedience (23% of all infractions), fights in school (14%), habitually violates a rule (13%), and disrespects authority (13%). In general, the distribution of infractions that yield suspensions for black, white, poor, and non-poor students is similar, although relatively large proportions of black students’ suspensions and poor students’ suspensions resulted from fights in school (16% and 15%, respectively, compared to 10% for white and non-poor students). Approximately 29% of black students’ suspensions and 30% of poor students’ suspensions resulted from violent



infractions. This compares to 28% of white students' suspensions and 26% of non-poor students' suspensions.

Our data on punishments also include a variable showing the length, in days, of each suspension, which we use as a measure of the punishment's severity.<sup>10</sup> The data show where the suspension was administered (i.e., an in-school suspension, out-of-school suspension, or off-site suspension), although our analyses do not distinguish based on this location. Critically for analyzing fights involving black and white students, or poor and non-poor students, we observe the date when each infraction occurred. To identify these interracial or inter-FRPL fights, we flagged infractions coded as fights that occurred on the same day in the same school. Our data do not explicitly link the students who fought one another, so we limited our sample to cases in which only two students were disciplined for fighting on the same day: a black student and white student (for the race analyses) or a FRPL and non-FRPL student (for the poverty analyses).<sup>11</sup>

#### **IV. Methods**

We examine discipline disparities in three ways. First, we decompose raw discipline gaps across districts, across schools within the same district, and within schools. Second, we use an assortment of regression models to examine the size and predictors of various types of disparities. Third, we examine the possibility of discriminatory school practices by comparing the punishments that result when black and white, or poor and non-poor students, get into fights with each other.

##### ***A. Decomposing Gaps into Across-District, Across-School, and Within-School Components***

We begin by breaking the average raw black/white and poor/non-poor discipline gaps into across-district, across-school-within-district, and within-school components. We do so for two discipline outcomes: whether students were suspended and for how many days they were

suspended. Decomposing the raw gaps into these three components is useful for identifying where gaps arise and which interventions might reduce those gaps.

Specifically, we start by defining the raw average discipline rate ( $\bar{R}_{isd}$ ) for a given group of students in a given grade weighted across students, schools, and districts. Specifically, we define  $\bar{R}_{isd}$  as:

$$(1) \quad \bar{R}_{isd} = \frac{\sum_i \sum_s \sum_d \text{Group}_{isd} Y_{isd}}{\sum_i \sum_s \sum_d \text{Group}_{isd}},$$

where  $\text{Group}_{isd}$  takes a “1” for a black (or FRPL) student, and a “0” for a white (or non-FRPL) student, and  $Y_{isd}$  indicates a suspension outcome for student  $i$  in school  $s$  and district  $d$ . We define  $\bar{\bar{R}}_{isd}$  as the discipline rate for the historically non-disadvantaged group (white or non-poor). The overall gap in suspension rates between two groups of students is simply  $\bar{R}_{isd} - \bar{\bar{R}}_{isd}$ .

We then take an approach similar to others who have decomposed gaps related to students’ exposure to teachers of varying experience (Clotfelter, Ladd, & Vigdor, 2005) and performance (Goldhaber, Lavery, & Theobald, 2015) into across-district, across-school, and within-school components. To do this, we define additional sets of suspensions rates. The first,  $\bar{R}_d$  and  $\bar{\bar{R}}_d$ , replaces  $Y_{isd}$  with the average suspension outcome in a given district,  $\bar{Y}_d$ . Assigning students the average suspension rate in a given district, gaps between  $\bar{R}_d$  and  $\bar{\bar{R}}_d$  arise from black and white (or poor and non-poor) students attending different districts. The second,  $\bar{R}_{sd}$  and  $\bar{\bar{R}}_{sd}$ , replaces  $Y_{isd}$  with the average suspension outcome in a given school,  $\bar{Y}_{sd}$ . With these two rates in hand, we decompose the overall gap into the following components:

$$(2) \quad \bar{R}_{isd} - \bar{\bar{R}}_{isd} = \bar{R}_d - \bar{\bar{R}}_d + \left( (\bar{R}_{sd} - \bar{\bar{R}}_{sd}) - (\bar{R}_d - \bar{\bar{R}}_d) \right) + \left( (\bar{R}_{isd} - \bar{\bar{R}}_{isd}) - (\bar{R}_{sd} - \bar{\bar{R}}_{sd}) \right).$$

Given that  $\bar{R}_d$  and  $\bar{\bar{R}}_d$  are a function of the same variable ( $\bar{Y}_d$ ), any difference between these two rates arises from black/white or poor/non-poor students attending different districts. Similarly, a

non-zero value of  $\left(\left(\bar{R}_{sd} - \bar{\bar{R}}_{sd}\right) - \left(\bar{R}_d - \bar{\bar{R}}_d\right)\right)$  is due to black/white or poor/non-poor students attending different schools within the same district. Finally, a non-zero value of  $\left(\left(\bar{R}_{isd} - \bar{\bar{R}}_{isd}\right) - \left(\bar{R}_{sd} - \bar{\bar{R}}_{sd}\right)\right)$  is due to within-school differences in suspensions for black/white or poor/non-poor students. We present kernel density plots of these gaps by grade, as well as the raw numerical gaps and the share of gaps explained at each level.<sup>12</sup>

### ***B. Regression Analyses Examining Gaps***

Second, we estimate linear probability and OLS regression models to more closely examine gaps and explore questions of substantive interest. These models assess several outcomes: whether students were suspended in a given school year; whether they were suspended multiple times in the same school year (which might yield a record or reputation for being a troubled student); whether a student's first suspension of the school year was for a violent or nonviolent infraction; length of suspension (in days) for a particular infraction; and length of suspension for a particular infraction for a student's first suspension of the year.

An important goal of this study is to assess whether gaps are driven by discriminatory behaviors by adults working in schools. Our models include covariates and fixed effects to account for the nonrandom sorting of students to schools, as well as spurious correlations between student characteristics and the propensity to get punished. Formally, we use models of the following form:

$$(3) \quad Y_{igst} = \alpha_0 + \beta_1 \text{Race}_{igst} + \beta_2 \text{FRPL}_{igst} + \beta_3 X_{igst} + \alpha_{gst} + \varepsilon_{igst},$$

where discipline outcomes for student  $i$  in grade  $g$  in school  $s$  in time  $t$  are modeled as a linear function of race,  $\text{Race}_{igst}$ , with binary indicators for black and other-race students (with white students as the reference group); FRPL status,  $\text{FRPL}_{igst}$ , with a binary indicator for qualifying

for either free or reduced-price lunch students (with non-FRPL students as the reference group); a vector of observable student characteristics related to race, FRPL, special education, gender, math and English language arts (ELA) scores from the prior school year, and a set of indicator variables for the type of infraction (in certain specifications with suspension length as an outcome); school-grade-year (SGY) fixed effects,  $\alpha_{gst}$  (or grade and year fixed effects in some models); and an idiosyncratic error term,  $\varepsilon_{igst}$ . We cluster our standard errors to the school-grade-year level.

The primary coefficients of interest across our statistical models estimate the black/white and poor/non-poor (FRPL/non-FRPL) differences in discipline outcomes. Models with and without additional student covariates yield notably different results. Models with these covariates are useful for comparing discipline outcomes for students who are similar but for their race or poverty status. However, these models might control for characteristics that are fundamental to how a group is perceived (e.g., preventing raw comparisons of how relatively low-scoring black students and relatively high-scoring white students are punished). Since each type of comparison is substantively important, we include models with and without student covariates.

Students in Louisiana are not randomly assigned to schools, and prior research suggests that a large share of the variation in differential patterns of student punishment is explained across, rather than within, schools (Anderson & Ritter, 2017; Kinsler, 2011; Skiba et al., 2014). In order to ensure that our analysis is not conflated by across-school differences and to build on the extant literature, our preferred specification for estimating the presence of discipline gaps within students' local environments uses SGY fixed effects. Students within these cells have more similar in-school and out-of-school experiences, and focusing on gaps within SGYs helps to identify gaps local to a cohort of peers within a particular grade and school.

Models with infraction fixed effects, with days of suspension as their dependent variable, are well suited for identifying gaps in suspension length that arise from similar infractions. These models are not necessarily well suited for identifying evidence of discriminatory school practices. For example, if black students are suspended for more days than white students for getting into fights, it could reflect administrators discriminating against black students. Alternatively (or additionally), it could reflect fights involving black students being systematically more severe than—or otherwise different from—fights involving white students.

### ***C. Gaps from Fights between Black and White, and Poor and Non-Poor, Students***

Our third methodological approach explores a very particular setting in which disparities seem most likely to reflect discriminatory school discipline practices. We test for differences in the length of suspensions that black and white, and poor and non-poor, students receive when they fight each other in school. We do so while controlling for these students' prior discipline histories and other background characteristics. These covariates account for the possibility that administrators might punish students differently based on their prior discipline records—and that certain groups of students (e.g., those who have fought previously or have lower test scores) might be more likely to instigate or escalate these fights.

The key identifying assumption is that when black and white (or poor and non-poor) students with similar discipline records and background characteristics get into a fight at school that results in both students receiving a fighting infraction, the black and white students' behaviors should warrant equal punishments. The assumption would be violated if, in these very particular circumstances, black and white students exhibit systematically different behaviors from each other. If this assumption holds, then gaps in suspension duration arising from these

fights are likely attributable to discriminatory behavior from schools that treat similar-behaving black and white (or poor and non-poor) students differently.

We examine two related outcomes for these fight analyses: the number of days for which students are suspended and whether a student receives a longer suspension than the student with whom he or she fought. Formally, our base model for these analyses is:

$$(4) \quad Y_{ist} = \alpha_0 + \beta_1 \text{Group}_{ist} + \beta_3 \text{PriorFight}_{ist} + \alpha_{st} + \varepsilon_{ist}$$

where discipline outcomes for student  $i$  in school  $s$  in time  $t$  are modeled as a linear function of race or FRPL,  $\text{Group}_{ist}$ , with binary indicators for black or FRPL students (with white or non-FRPL students, respectively, as the reference group); the number of prior fight infractions for student  $i$  in school  $s$  in time  $t$ ,  $\text{PriorFight}_{ist}$ ; school-year fixed effects,  $\alpha_{st}$ ; and an idiosyncratic error term,  $\varepsilon_{ist}$ . We control for the number of prior fights in which a student was involved in the current year to address the possibility that students are disciplined differently for their first fight and subsequent fights. We cluster our standard errors to the school-year level. For these analyses, we restrict our sample to fighting infractions involving different-race or different-FRPL status students on the same day in the same school.

In addition to the base model, we include specifications that control for a vector of observable student characteristics related to race (for the poor/non-poor analyses), FRPL (for the race analyses), special education, gender, and math and ELA test scores from the prior school year. We also include specifications with fight occurrence fixed effects, which replace school-year fixed effects with a separate dummy variable representing each individual fight between a black and white student or between a poor and non-poor student. Furthermore, we restrict the sample of our fight analysis in four ways: 1) limited to students without a prior fight in the current school year; 2) limited to students without a prior fight at any point in our data; 3) limited

to students whose first suspension in the current school year is a fight; and 4) limited to students whose first suspension ever in our data is a fight. For all analyses with the number of suspension days as their outcome, we censor the number of days to 20 to limit the influence of outlier (very long) suspensions.

## **V. Results**

### ***A. Decomposing Gaps into Across-District, Across-School, and Within-School Components***

First, we decompose gaps in two outcomes—the likelihood of getting suspended and the number of days suspended—into across-district, across-schools-within-the-same-district, and within-school components. We present this analysis visually in Figures 1 through 6, with the underlying raw numbers presented in Appendix Tables A1 and A2. Figures 3 and 6 show male/female gaps for purposes of comparison and illustration.

A few important patterns emerge from the data. The first relates to the changing size and nature of discipline gaps across the age spectrum from kindergarten through grade 12. The overall gap between black and white students in whether they were suspended (shaded gray density in Figure 1) starts around 3 percentage points in kindergarten, grows to a peak of 21 percentage points in grades 6 and 7, and shrinks to 9 percentage points in grade 12. A similar pattern appears in the poor/non-poor gaps in whether students were suspended (Figure 2) and for the days suspended outcome (Figures 4 and 5). The larger gaps in middle school could reflect higher rates of exclusionary discipline after students leave elementary school, with those rates declining in high school as many struggling students drop out.

The second interesting pattern—different from patterns observed in other studies (e.g., Anderson & Ritter, 2017; Kinsler, 2011)—is that within-school differences account for a large portion of the overall black/white and poor/non-poor gaps, especially in middle and high

school.<sup>13</sup> This is particularly the case for outcomes showing whether students were suspended. For both the black/white and poor/non-poor comparisons, within-school differences account for at least 50 percent of the gap in kindergarten and grades 5 through 12. Still, differences across schools constitute an important share of the black/white and poor/non-poor gaps as well. This stands in stark contrast to the male/female discipline gaps (Figure 3), which arise almost entirely within schools (likely due largely to the more even balance of male and female students across schools). With respect to the suspension length outcome (Figures 4 through 6), black/white and poor/non-poor discipline gaps are spread more evenly across districts, across schools, and within schools. For this outcome, too, within-school differences account for virtually all of the male/female discipline gaps.

The relatively large within-school differences in suspension rates for black and white, and poor and non-poor, students have important implications. They indicate that many Louisiana students attend schools in which black and poor students are suspended at much higher rates than white and non-poor students—and that many Louisiana administrators are suspending their black and poor students at much higher rates than they suspend their white and non-poor students. This, in itself, is not necessarily evidence of discrimination, as these differences in punishments could reflect differences in behaviors. Our subsequent analyses examine this question more closely. However, this does mean that discipline gaps are potentially evident to many students, teachers, and administrators, and not simply a pattern that arises from differences across schools that escape the view of those working within a single school.

### ***B. Regression Analyses Examining Gaps***

We next examine black/white and poor/non-poor discipline gaps within a regression framework. The first three tables in this section use linear probability models (LPMs) to estimate



likelihood of suspension. Table 3 examines whether students are suspended in a given year, Table 4 examines whether they are suspended multiple times in a given year, and Table 5 examines whether these suspensions arise from violent or nonviolent infractions. The other two tables in this section use ordinary least squares (OLS) regression to estimate the number of days a student is suspended. Table 6 examines predictors of the length of students' suspensions, and Table 7 does the same for students' first suspensions of the school year.

First, we assess whether black and poor students are more likely than white and non-poor students to be suspended after controlling for other student characteristics and various sets of fixed effects. The first specification in Table 3 shows, in raw terms, that black students are 13 percentage points more likely to be suspended in a given year than white students, with the constant indicating a suspension rate of 12% for white students. That 13-percentage-point difference persists when we add school-grade-year fixed effects that focus comparisons within students' grade-level cohorts. Poor students are 9 percentage points more likely to be suspended in a given year than non-poor students (with a baseline suspension rate of 12%), and that difference also persists in models with school-grade-year fixed effects (Column 4). Both the black/white and poor/non-poor gaps remain significant when we estimate both gaps simultaneously (Columns 5 and 6) and when we additionally control for special education status, gender, and prior test scores (Columns 7 and 8). Even in these saturated models, we find a black/white gap of 11 percentage points and a poor/non-poor gap of 6 to 7 percentage points. These models also show associations between the likelihood of suspension and having a disability, being male, and having lower test scores.<sup>14</sup>

A comparison of Table 3 and Appendix Table A1 (or Figure 1) reveals an important point about assessing the relative contributions of within-school and between-school factors. As shown

in Table 3, introducing school-grade-year (SGY) fixed effects had virtually no impact on our gap estimates. It might be tempting to compare coefficients across those models and conclude that the gaps arose within schools, since the magnitude and significance of the estimates persisted with SGY fixed effects. However, those comparisons can mislead. A model that regresses suspension outcomes on race and includes SGY fixed effects will assign the greatest weight to students in SGYs with a relatively even balance of white and black students. It will give no weight at all to students in fully segregated SGYs, since there is no within-SGY variation in student race. Yet these settings are particularly important for studying the source of discipline gaps, since a potentially important driver of these gaps is the difference between predominantly black (or poor) and predominantly white (or non-poor) schools. As a result, we do not compare, for example, the first two columns of Table 3 for this purpose. We prefer the decomposition method described above for assessing the relative impact of within-school, between-school (within district), and between-district factors.<sup>15</sup>

Table 4 shows gaps in whether students receive multiple suspensions (and therefore accrue lengthy discipline records, miss school repeatedly, and potentially acquire reputations as troubled students). The model specifications used for this table are parallel to the specifications from Table 3, and the results are largely parallel as well. Both with and without school-grade-year fixed effects, we observe black/white gaps of 8 percentage points and poor/non-poor gaps of 6 percentage points in whether students receive multiple suspensions in the same year. These compare to baseline multiple suspension rates of 6 percent for both white and non-poor students, as indicated by the constants in Columns 1 and 4, respectively. These gaps also persist in the presence of covariates, with similar associations evident in students' disability status, gender, and prior test scores.

We next consider whether suspensions for violent or nonviolent infractions—or some combination of the two—produce these gaps. We focus on students' first suspensions in a school year in order to examine the hypothesis that black and poor students start to accrue discipline records for relatively minor offenses that may not warrant exclusionary discipline. Table 5 shows the results from models parallel to those from Columns 1 through 8 from Table 3. Table 5 is divided into two panels: one comparing students suspended for violent suspensions to those not suspended in order to estimate the likelihood of being suspended for a violent infraction (Panel A) and one comparing students suspended for nonviolent infractions relative to those not suspended in order to estimate the likelihood of being suspended for a nonviolent infraction (Panel B). We find that black and poor students are more likely than their peers to be suspended for a nonviolent infraction, but they are also more likely than their peers to be suspended for a violent infraction. Black/white differences amount to 5 to 7 percentage points for violent infractions, depending on which covariates are included, and 8 to 9 percentage points for nonviolent infractions. Poor/non-poor differences amount to 3 to 5 percentage points for violent infractions and 3 to 7 percentage points for nonviolent infractions.

Our final two tables in this section examine suspension length (in days) as the outcome. Since suspension length varies across infractions of different severity—and since Table 2 reveals differences in the distribution of infractions by race and poverty status—we introduce models with and without infraction type fixed effects (i.e., which of the LDOE infraction types yielded the suspension). Models with infraction fixed effects can test for gaps in the severity of punishments issued to students whose behaviors were coded as the same type of infraction. Of course, these models cannot address the possibility that schools translate behaviors to infractions differently for black and poor students relative to their peers.

Table 6 shows that black students consistently receive longer suspensions than white students—and poor students consistently receive longer suspensions than non-poor students—for the same recorded infractions. This is first evident in raw comparisons of suspension length. Column 1 shows that suspensions for black students last, on average, 0.4 days longer than suspensions for white students (whose suspensions last an average of 2.2 days). These differences persist, with similar magnitude, when we introduce infraction fixed effects to restrict comparisons to suspensions for the same type of incident (Column 2). When we introduce school-grade-year fixed effects—our preferred model for exploring within-school gaps in suspension length (Column 3)—we see that black students’ suspensions are approximately 0.1 days longer than white students’ suspensions. Racial gaps are similar in magnitude and significance in models that control for FRPL and other covariates (Columns 7 through 10). Similar to previous tables, gaps are evident based on poverty status but smaller in magnitude than the racial gaps. Column 5 shows that poor students are suspended for approximately 0.2 days longer than non-poor students—relative to a baseline of 2.3 days—and this gap remains significant in models with assorted sets of fixed effects and covariates. A model with infraction and school-grade-year fixed effects (Column 6) shows that poor students’ suspensions last 0.05 days longer than the suspensions of their non-poor peers.

These results, more than those that precede them, suggest the possibility of discriminatory practices within schools that lead black and poor students to receive more severe punishments for similar infractions. However, this evidence is not conclusive. Schools might punish students differently depending on those students’ existing discipline records. Perhaps schools are more lenient in punishing students’ first offenses, and since black and poor students are more likely to have multiple infractions in the same year (see Table 4), the gaps observed in

Table 6 could result from schools' handling of students with different existing records. The models in Table 7 mitigate that concern by restricting the sample to students' first recorded infraction (suspension) of the year.<sup>16</sup> Even with this restriction, black/white and poor/non-poor gaps appear. For example, models with infraction type fixed effects reveal a black/white gap of 0.4 days (Column 2) and poor/non-poor gap of 0.1 days (Column 5). In models with infraction and school-grade-year fixed effects, the black/white gap is 0.05 days (Column 3) and the poor/non-poor gap is 0.02 days (Column 6). These differences are statistically significant in these models and all other models tested for this table.

Table 7 provides stronger evidence that school leaders punish black and poor students more severely than white and non-poor students for similar infractions. However, another potential source of bias remains. It is possible (although unobservable) that behaviors of black and poor students systematically differ from behaviors of white and non-poor students even when they yield the same infraction code. Perhaps, for example, black students' "willful disobedience," as it is recorded, is generally more severe than white students' willful disobedience. While we have no reason to believe this is the case, our next set of analyses—involving fights between black and white students or poor and non-poor students—help us focus even more narrowly on punishments for the same infraction types that arise from very similar circumstances.

### ***C. Gaps from Fights between Black and White, and Poor and Non-Poor, Students***

Our final tables examine punishment gaps arising from fights between black and white students and between poor and non-poor students. Tables 8 and 9 present results for the same four specifications, looking separately at two outcomes: the length of the suspension in days and, in cases in which the students received different punishments from one other, whether the

disadvantaged (black or poor) student received the longer suspension. The models differ in which covariates they include and whether they include school-year or fight occurrence fixed effects. Tables 10 and 11 show the robustness of our findings to analyses that restrict our samples to fighting students' first suspensions or fights in either the current school year or ever in our data.

Table 8 shows consistent evidence that black students receive longer suspensions than white students for these interracial fights, with the differences modest in magnitude but statistically significant. In models that only control for a student's prior number of fights—which is positively associated with suspension length—black students receive suspensions that are approximately 0.05 days longer, on average, than the white students with whom they fight. This compares to a baseline suspension length of 2.9 days for white students. These gaps persist when we control for students' FRPL status, special education status, gender, and prior test scores. In these models, the gaps range from 0.04 days (with fight occurrence fixed effects) to 0.05 days (with school-year fixed effects).

Results from LPMs that instead test whether the black student received a longer suspension than the white student appear in Columns 5 through 8. These results largely mirror the others. In models that only control for a student's prior number of fights, the probability that the black student receives a longer suspension is 1.6 to 1.7 percentage points higher than the probability that the white student receives a longer suspension. For example, Column 5 indicates that white students received a longer suspension after 4.6 percent of interracial fights, while black students received a longer suspension after 6.3 percent of interracial fights (controlling for the number of prior fights). In models with additional covariates, the estimated differences range from 1.1 to 1.2 percentage points and are also statistically significant.

Table 9 contains some evidence of systematic differences in the punishments of poor and non-poor students who fight each other, but the evidence is somewhat less consistent than for the black/white comparisons. Columns 1 through 4 show no significant differences in the average number of days of poor and non-poor students' suspensions (although this could be a product of larger standard errors). Columns 5 through 8, however, indicate that poor students are 1.1 to 1.4 percentage points more likely to receive a longer suspension than their non-poor fighting partners. These differences, although modest, are statistically significant in all models.

We observe similar results when we restrict the samples to fights between two students who had not been previously suspended for fighting (in some models) or suspended at all (in other models). Tables 10 and 11 show black/white and poor/non-poor disparities after restricting the sample two students who had previously: not been suspended for fighting in that school year, never been suspended for fighting in our data, not been suspended at all in that school year, or never been suspended in our data. In each case, we show results with and without student covariates (race, FRPL, special education status, gender, and prior test scores). Each table provides estimates for black/white gaps in Panel A and poor/non-poor gaps in Panel B.

Table 10 shows nearly identical estimates for black/white gaps across all samples and models. The gaps range from 0.04 days to 0.05 days per fight, in each case significant ( $p < .05$ ), which is consistent with the estimates in Table 8. The poor/non-poor gaps are consistent with the estimates in Table 9 in that they have positive but not statistically significant coefficients (with one exception), perhaps due to relatively large standard errors. Table 11 mirrors Table 10 but replaces the outcome variable with whether the black or poor student received the longer suspension. Here, too, the results are very similar to those from our preferred models. Table 11 shows that black students are 1.1 to 2.1 percentage points more likely to receive a longer

suspension than white students (comparable to results in Columns 5 and 7 of Table 8) and poor students are 1.1 to 1.5 percentage points more likely to receive a longer suspension than white students (comparable to results in Columns 5 and 7 of Table 9). These differences are all significant with  $p < .01$ .

The stability of our estimates with these various sample restrictions provides confidence that our preferred models from Tables 8 and 9 are not biased from black or poor students being systematically more disruptive or antagonistic than the students with whom they fight.

## **VI. Discussion**

Questions about why poor and minority students are suspended at higher rates than their peers and what to do about it have emerged among the most pressing and controversial issues facing education policymakers. At this point there is little dispute that black and poor students are suspended and expelled at higher rates than their peers. However, addressing inequities in exclusionary discipline requires not only establishing that gaps exist but also explaining their origins. Gaps in exclusionary discipline could arise from true differences in students' behaviors, differences in how schools translate those behaviors to infractions, and differences in how schools punish students for the same infractions. The reality that gaps could arise within schools, across schools within districts, or across districts adds complexity, while the lack of available data on the true behaviors of large numbers of students imposes constraints on how researchers can assess these gaps.

This study uses rich administrative data from the state of Louisiana to explore the causes of black/white and poor/non-poor gaps in exclusionary discipline. Louisiana is an appropriate setting for this study due to its large (and relatively even) populations of black and white students and its historical challenges related to race, class, and schools. We observe large black/white and



poor/non-poor differences in student discipline, with these gaps evident in a variety of contexts. For example, we see that substantial portions of discipline gaps arise within schools, meaning that these gaps are potentially observable to many students and staff—and not simply patterns that arise from between-school differences that escape the view of individuals working within a single school. Black and poor students tend to receive longer suspensions than white and non-poor students for their first infractions, and while Louisiana’s black and poor students are more likely than their peers to be suspended for nonviolent infractions, they are also more likely to be suspended for violent infractions.

A fundamental—and much debated—question about discipline gaps is whether they arise from intentional discrimination towards minority or poor students. Discrimination of this type is extremely difficult to identify in large-scale administrative data, as these data typically do not provide information about students’ true behaviors. This study tests for gaps arising from situations so narrowly defined that explanations other than discrimination seem unlikely. In particular, we examine what happens when a black student and a white student (or a poor student and a non-poor student) fight each other, controlling for other characteristics related to students’ backgrounds and prior fight histories. Even in this very particular context, we find that black students are punished more severely than white students. The difference averages about 0.05 days across black-white fights—with black students (and poor students) one to two percentage points more likely to receive a longer suspension. These models cannot provide conclusive evidence of racial bias, since we must rely on some unverifiable assumptions, including that black students do not systematically behave differently than white students in these interracial fights (after accounting for students’ background characteristics). Still, with our findings robust to numerous alternate specifications, this study provides perhaps the strongest evidence to date of

systematic discrimination in student discipline. Moreover, although these particular differences are small in magnitude, there is reason to believe that disparities could be larger in circumstances less amenable to this type of analysis. We examine black/white and poor/non-poor fights because we believe they provide the most credible glimpse in our data at whether schools punish students differently for similar behaviors. With these fights, however, differences in how students are punished are likely known to the administrators who determine the punishments as well as many other staff members, students, and parents. This awareness could temper the resulting disparities. If so, one might expect larger disparities if black and white or poor and non-poor students are punished at different times for different incidents.

Of course, discriminatory practices might also exist even where we observe gaps across schools rather than within them. If schools that enroll high percentages of poor and minority students employ harsher discipline practices than other schools, then poor and minority students could accrue discipline records that non-poor and white students would not accrue for similar behaviors. Moreover, broader economic and societal patterns of discrimination could yield varying behaviors from students of different races and socioeconomic classes. These represent different types of problems than within-school gaps—and would require solutions tailored to those problems—but still can reflect discrimination in student discipline. As this study shows, discipline gaps arise from multiple sources and likely require more than one type of response.

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<sup>1</sup> Some studies refer to in-school suspensions, in which students are removed from their classrooms but remain in the school building, as a form of exclusionary discipline. Others do not. This study regards both in-school and out-of-school suspensions as forms of exclusionary discipline, since they exclude students from their routine instruction and activities.

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<sup>2</sup> Through the National School Lunch Program, students whose household income is at or below 130 percent of the poverty line are eligible for free lunch, while students whose household income is at or below 180 percent of the poverty line are eligible for reduced-price lunch.

<sup>3</sup> See Losen, Hodson, Keith II, Morrison, and Belway (2015) for a description of the changes in racial discipline gaps from 1972-73 through 2011-12.

<sup>4</sup> The decision of whether to remove a student from school could also have implications for that student's classmates. These externalities have not received as much attention from researchers as the direct effects on the suspended students (see Kinsler, 2013, for analysis that considers the externalities of suspending students along with the deterrent and direct effects on suspended students).

<sup>5</sup> Note that this also could affect how schools translate infractions to punishments. More generally, this distinction is conflated in studies that compare the severity of punishments for black and white students without separately comparing (or controlling for) the infractions that yielded those punishments.

<sup>6</sup> It is important to note that a finding that discipline disparities arise across schools rather than within them does not rule out the possibility of discriminatory or inequitable causes of the disparities. For example, various forms of discrimination could lead black and white students to behave differently or attend different types of schools. This type of research is analogous in many ways to the expansive research on wage gaps by race and gender (e.g., Cotton, 1988; Groshen, 1991; Reimers, 1983; Weichselbaumer & Winter-Ebmer, 2005). This research tends to show that controlling for variables such as occupation, education, and experience yield smaller estimates of wage gaps than simple raw comparisons, although race and gender differences on these covariates could themselves result from various forms and sources of discrimination.

<sup>7</sup> Questions about how to punish infractions of different severities have entered policy discussions about zero-tolerance laws, among other issues (e.g., see Curran, 2016).

<sup>8</sup> The following infractions were coded as violent (as labeled in LDOE data): immoral or vicious practices; habits injurious to his/her associates; weapon (Sec 921 of Title 18 of the U.S. Code); weapon (not prohibited by federal law); throws missiles liable to injure others; fights while under school supervision; commits any other serious offense; murder; assault and/or battery; rape and/or sexual battery; kidnapping; arson; misappropriate with violence; use weapon prohibited by federal law; possess blade with length less than 2.5 in.; serious bodily injury; bullying; cyber bullying; and sexual harassment.

<sup>9</sup> Some students were suspended for both a violent and nonviolent infraction in the same year.

<sup>10</sup> In the decomposition, we measure punishment length at the student-year level. In the regression analyses, we measure it at the student-infraction-year level.

<sup>11</sup> We also have a variable showing the race of each school administrator. We explored using this variable to test whether the punishments assigned for interracial fights vary by the race of the person administering the punishment (e.g., for evidence on the relationship between teacher-student race match and student discipline, see Lindsay & Hart, 2017; for both teacher-student and principal-student match, see Kinsler, 2011). However, we do not observe which administrator actually determined punishments (e.g., a principal or assistant principal), and our conversations with school leaders indicate that this varies considerably across schools. We tested whether punishments for interracial fights vary by the overall racial composition of the administrative staff (see Price & Wolfers, 2010, for an analogous approach), and we found no statistically significant relationships between administrators' race and the punishment gaps

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between black and white students. Because of the data ambiguities, we omit those analyses from the paper, but they are available upon request.

<sup>12</sup> Note that a negative gap would imply that white/non-poor students are more likely to be suspended or have longer suspensions than black/poor students.

<sup>13</sup> Kinsler (2011) did find evidence of within-school differences in the likelihood of being referred to the principals' office for a behavioral offense. However, conditional on being referred to the principal's office and controlling for infraction, the within-school differences in the likelihood or length of suspensions were not statistically significant.

<sup>14</sup> We also estimated models that allowed the black/white gap to vary by income and/or gender. In both cases we saw small but statistically significant negative effects for the interactions between black and low-income or black and male variables, on the order of 1 percentage point.

<sup>15</sup> Appendix Tables A3 and A4 present results from regression models with assorted weights, although these weights cannot address the issue of empty cells for segregated SGYs. These tables include models like those presented in Tables 3 and 4, respectively.

<sup>16</sup> We focus on first infractions—rather than controlling for prior discipline records—because students' prior discipline records could make for problematic controls if black or poor students previously received suspensions for behaviors that would not have yielded suspensions for white or non-poor students.

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**Table 1**  
*Descriptive Statistics*

	All students		Race comparison		Poverty comparison	
			Black	White	FRPL	Non-FRPL
	N	%	%	%	%	%
Total	9,999,240					
Suspended	1,802,382	18%	25%	12%	21%	12%
Suspended, violent infraction	870,791	9%	13%	5%	11%	5%
Suspended, nonviolent infraction	1,370,761	14%	19%	9%	16%	10%
Race/Ethnicity						
Black	4,630,883	46%	100%	0%	62%	21%
White	4,810,988	48%	0%	100%	32%	74%
Other	549,214	5%	0%	0%	6%	5%
Poverty status						
Free lunch	5,456,642	55%	77%	33%	88%	0%
Reduced-price lunch	738,126	7%	6%	9%	12%	0%
Full-price lunch	3,804,472	38%	17%	58%	0%	100%
Special education status						
SPED	1,057,802	12%	13%	11%	14%	8%
Non-SPED	7,936,088	88%	87%	89%	86%	92%
Gender						
Male	5,126,563	51%	51%	52%	51%	52%
Female	4,872,677	49%	49%	48%	49%	48%
Standardized state test score ( <i>t</i> -1)						
English language arts	3,622,000	0.12	-0.17	0.37	-0.09	0.47
Math	3,625,553	0.11	-0.25	0.42	-0.11	0.49
Science	3,512,728	0.10	-0.30	0.45	-0.13	0.50
Social studies	3,511,937	0.10	-0.24	0.39	-0.11	0.47

*Notes.* The unit of observation is the student-year, meaning that students observed in multiple years account for multiple observations. In total, the data contain 9,999,240 student-year observations from 1,778,128 students. The columns with test scores show standardized scores, not percentages.

**Table 2**  
*Number of Infractions by Infraction Type and Student Subgroup*

	All students		Race Comparison				Poverty Comparison			
			Black		White		FRPL		Non-FRPL	
	N	%	N	%	N	%	N	%	N	%
Total number of suspensions	4,258,559	100%	2,915,863	100%	1,223,363	100%	3,272,024	100%	986,535	100%
Willful disobedience	999,339	23%	699,943	24%	273,162	22%	774,576	24%	224,763	23%
Fights in school	604,719	14%	467,074	16%	125,606	10%	504,000	15%	100,719	10%
Habitually violates a rule	559,983	13%	393,453	13%	151,917	12%	436,237	13%	123,746	13%
Disrespects authority	536,668	13%	393,442	13%	131,529	11%	426,962	13%	109,706	11%
Any other serious offense	315,827	7%	186,856	6%	118,142	10%	221,356	7%	94,471	10%
Profane	255,728	6%	164,830	6%	83,912	7%	191,955	6%	63,773	6%
Leaves school	256,553	6%	157,183	5%	88,563	7%	172,977	5%	83,576	8%
Habitually tardy	203,312	5%	133,782	5%	61,372	5%	137,799	4%	65,513	7%
Injurious habits	183,594	4%	118,794	4%	58,913	5%	149,304	5%	34,290	3%
Other	342,836	8%	200,506	7%	130,247	11%	256,858	8%	85,978	9%
Violent infractions	1,232,478	29%	856,312	29%	343,206	28%	977,094	30%	255,384	26%
Nonviolent infractions	3,026,081	71%	2,059,551	71%	880,157	72%	2,294,930	70%	731,151	74%

*Notes.* The unit of observation is the infraction, so some students have multiple observations within the same year while students who did not commit an infraction are not represented. The table lists the nine most common infractions and aggregates all other infractions as “Other.” Columns with percentages show the percentage of infractions recorded for that group of students that were of the infraction type listed. The following infractions were coded as violent (as labeled in LDOE data): immoral or vicious practices; habits injurious to his/her associates; weapon (Sec 921 of Title 18 of the U.S. Code); weapon (not prohibited by federal law); throws missiles liable to injure others; fights while under school supervision; commits any other serious offense; murder; assault and/or battery; rape and/or sexual battery; kidnapping; arson; misappropriate with violence; use weapon prohibited by federal law; possess blade with length less than 2.5 in.; serious bodily injury; bullying; cyber bullying; and sexual harassment.

**Table 3**  
*Predictors of Whether Students were Suspended in Given School Year*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black	0.128*** (0.001)	0.126*** (0.001)			0.109*** (0.001)	0.105*** (0.001)	0.106*** (0.001)	0.114*** (0.001)
Other race	-0.017*** (0.001)	-0.014*** (0.001)			-0.028*** (0.001)	-0.025*** (0.001)	-0.031*** (0.001)	-0.036*** (0.001)
FRPL			0.091*** (0.001)	0.088*** (0.000)	0.047*** (0.001)	0.062*** (0.000)	0.067*** (0.001)	0.064*** (0.001)
SPED							0.024*** (0.001)	0.020*** (0.001)
Male							0.106*** (0.001)	0.103*** (0.001)
Math score ( <i>t</i> -1)							-0.022*** (0.000)	-0.019*** (0.000)
ELA score ( <i>t</i> -1)							-0.050*** (0.000)	-0.045*** (0.000)
Constant	0.122*** (0.001)	0.123*** (0.000)	0.124*** (0.001)	0.126*** (0.000)	0.102*** (0.001)	0.095*** (0.000)	0.056 (0.145)	0.088*** (0.001)
Observations	9,981,117	9,981,117	9,989,263	9,989,263	9,981,117	9,981,117	3,615,828	3,615,828
R-squared	0.029	0.190	0.013	0.184	0.031	0.195	0.119	0.211
Year FEs	No	No	No	No	No	No	Yes	No
Grade FEs	No	No	No	No	No	No	Yes	No
SGY FEs	No	Yes	No	Yes	No	Yes	No	Yes

*Notes.* The unit of observation is the student-year. Standard errors appear in parentheses and account for the clustering of students within school-grade-year. “SGY FEs” refers to school-grade-year fixed effects.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

**Table 4**  
*Predictors of Whether Students were Suspended Multiple Times in Given School Year*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black	0.082*** (0.001)	0.083*** (0.000)			0.070*** (0.001)	0.071*** (0.000)	0.070*** (0.001)	0.078*** (0.001)
Other race	-0.008*** (0.001)	-0.006*** (0.000)			-0.015*** (0.001)	-0.013*** (0.000)	-0.018*** (0.001)	-0.020*** (0.001)
FRPL			0.059*** (0.000)	0.055*** (0.000)	0.030*** (0.000)	0.037*** (0.000)	0.037*** (0.000)	0.037*** (0.000)
SPED							0.029*** (0.001)	0.024*** (0.001)
Male							0.067*** (0.000)	0.064*** (0.000)
Math score ( <i>t</i> -1)							-0.013*** (0.000)	-0.012*** (0.000)
ELA score ( <i>t</i> -1)							-0.034*** (0.000)	-0.030*** (0.000)
Constant	0.055*** (0.000)	0.055*** (0.000)	0.056*** (0.000)	0.059*** (0.000)	0.043*** (0.000)	0.038*** (0.000)	-0.040 (0.090)	0.029*** (0.001)
Observations	9,981,117	9,981,117	9,989,263	9,989,263	9,981,117	9,981,117	3,615,828	3,615,828
R-squared	0.020	0.160	0.010	0.154	0.023	0.163	0.083	0.183
Year FEs	No	No	No	No	No	No	Yes	No
Grade FEs	No	No	No	No	No	No	Yes	No
SGY FEs	No	Yes	No	Yes	No	Yes	No	Yes

*Notes.* The unit of observation is the student-year. Standard errors appear in parentheses and account for the clustering of students within school-grade-year. “SGY FEs” refers to school-grade-year fixed effects.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

**Table 5**  
*Predictors of Whether First Suspension was for Violent or Nonviolent Infraction*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>Panel A: Violent infractions</u>								
Black	0.063*** (0.000)	0.064*** (0.000)			0.051*** (0.000)	0.054*** (0.000)	0.060*** (0.001)	0.068*** (0.001)
Other race	-0.007*** (0.000)	-0.008*** (0.000)			-0.014*** (0.000)	-0.013*** (0.000)	-0.018*** (0.001)	-0.019*** (0.001)
FRPL			0.049*** (0.000)	0.042*** (0.000)	0.029*** (0.000)	0.029*** (0.000)	0.034*** (0.000)	0.032*** (0.000)
Constant	0.044*** (0.000)	0.044*** (0.000)	0.042*** (0.000)	0.047*** (0.000)	0.032*** (0.000)	0.031*** (0.000)	-0.009 (0.107)	0.028*** (0.000)
Observations	8,812,528	8,812,528	8,819,806	8,819,806	8,812,528	8,812,528	3,127,917	3,127,917
R-squared	0.015	0.118	0.009	0.113	0.018	0.120	0.060	0.137
<u>Panel B: Nonviolent infractions</u>								
Black	0.091*** (0.001)	0.094*** (0.001)			0.080*** (0.001)	0.078*** (0.000)	0.077*** (0.001)	0.084*** (0.001)
Other race	-0.012*** (0.001)	-0.008*** (0.000)			-0.018*** (0.001)	-0.016*** (0.000)	-0.020*** (0.001)	-0.026*** (0.001)
FRPL			0.059*** (0.001)	0.065*** (0.000)	0.027*** (0.000)	0.046*** (0.000)	0.047*** (0.001)	0.047*** (0.001)
Constant	0.085*** (0.000)	0.083*** (0.000)	0.089*** (0.001)	0.085*** (0.000)	0.074*** (0.001)	0.063*** (0.000)	0.044 (0.126)	0.059*** (0.001)
Observations	9,348,601	9,348,601	9,356,338	9,356,338	9,348,601	9,348,601	3,301,142	3,301,142
R-squared	0.019	0.182	0.008	0.177	0.021	0.185	0.098	0.201
Student controls	No	No	No	No	No	No	Yes	Yes
Year FEs	No	No	No	No	No	No	Yes	No
Grade FEs	No	No	No	No	No	No	Yes	No
SGY FEs	No	Yes	No	Yes	No	Yes	No	Yes

*Notes.* The unit of observation is the student-year. Table examines first suspension in school year. Standard errors appear in parentheses and account for the clustering of students within school-grade-year. Student controls consist of special education status, gender, and math and ELA scores from the prior year. “SGY FEs” refers to school-grade-year fixed effects. See the Data section for a description of how violent and nonviolent infractions were defined.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

**Table 6**  
*Predictors of Length of Suspension (in Days)*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Black	0.429*** (0.007)	0.395*** (0.006)	0.099*** (0.003)				0.382*** (0.006)	0.091*** (0.003)	0.302*** (0.008)	0.065*** (0.005)
Other race	0.018* (0.010)	0.040*** (0.010)	-0.018** (0.007)				0.034*** (0.010)	-0.022*** (0.007)	0.047*** (0.014)	-0.030*** (0.011)
FRPL				0.248*** (0.006)	0.179*** (0.006)	0.050*** (0.003)	0.039*** (0.005)	0.026*** (0.003)	0.122*** (0.006)	0.047*** (0.005)
SPED									-0.102*** (0.008)	-0.063*** (0.006)
Male									0.030*** (0.005)	0.035*** (0.004)
Math ( <i>t-1</i> )									-0.057*** (0.004)	-0.017*** (0.003)
ELA ( <i>t-1</i> )									-0.064*** (0.004)	-0.034*** (0.003)
Constant	2.219*** (0.006)	1.877*** (0.007)	2.074*** (0.004)	2.322*** (0.007)	2.016*** (0.008)	2.103*** (0.004)	1.856*** (0.007)	2.059*** (0.005)	2.011*** (0.298)	1.999*** (0.007)
Observations	4,253,426	4,253,426	4,253,426	4,256,324	4,256,324	4,256,324	4,253,426	4,253,426	1,922,514	1,922,514
R-squared	0.007	0.042	0.183	0.002	0.037	0.183	0.042	0.183	0.046	0.180
Infraction FEs	No	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	No	No	No	No	No	No	No	No	Yes	No
Grade FEs	No	No	No	No	No	No	No	No	Yes	No
SGY FEs	No	No	Yes	No	No	Yes	No	Yes	No	Yes

*Notes.* The unit of observation is the infraction, and the sample is restricted to students who were suspended. The number of days suspended is censored to 20 for suspensions that exceeded 20 days. Standard errors appear in parentheses and account for the clustering of students within school-grade-year. “SGY FEs” refers to school-grade-year fixed effects.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

**Table 7**  
*Predictors of Length of Suspension for First Offense (in Days)*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Black	0.411*** (0.006)	0.361*** (0.006)	0.047*** (0.004)				0.365*** (0.006)	0.044*** (0.004)	0.292*** (0.008)	0.024*** (0.006)
Other race	0.013 (0.011)	0.040*** (0.010)	-0.023** (0.009)				0.043*** (0.010)	-0.025*** (0.009)	0.058*** (0.015)	-0.013 (0.014)
FRPL				0.210*** (0.006)	0.124*** (0.006)	0.022*** (0.004)	-0.013** (0.006)	0.010** (0.004)	0.107*** (0.007)	0.028*** (0.006)
SPED									-0.064*** (0.008)	-0.027*** (0.008)
Male									-0.019*** (0.005)	-0.025*** (0.005)
Math ( <i>t-1</i> )									-0.041*** (0.004)	-0.005 (0.004)
ELA ( <i>t-1</i> )									-0.045*** (0.004)	-0.014*** (0.004)
Constant	2.106*** (0.005)	1.728*** (0.006)	1.904*** (0.005)	2.216*** (0.007)	1.875*** (0.007)	1.918*** (0.005)	1.735*** (0.007)	1.898*** (0.006)	1.745*** (0.471)	1.890*** (0.009)
Observations	1,801,105	1,801,105	1,801,105	1,802,382	1,802,382	1,802,382	1,801,105	1,801,105	802,597	802,597
R-squared	0.008	0.056	0.247	0.002	0.051	0.247	0.056	0.247	0.064	0.245
Infraction FEs	No	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	No	No	No	No	No	No	No	No	Yes	No
Grade FEs	No	No	No	No	No	No	No	No	Yes	No
SGY FEs	No	No	Yes	No	No	Yes	No	Yes	No	Yes

*Notes.* The unit of observation is the infraction, and the sample is restricted to students who were suspended. The number of days suspended is censored to 20 for suspensions that exceeded 20 days. Standard errors appear in parentheses and account for the clustering of students within school-grade-year. “SGY FEs” refers to school-grade-year fixed effects.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

**Table 8**  
*Discipline Gaps in Fights Between Black and White Students*

Outcome variable	Number of days suspended				Whether received longer suspension than peer			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black student	0.054*** (0.010)	0.052*** (0.011)	0.054*** (0.013)	0.036*** (0.012)	0.017*** (0.003)	0.016*** (0.003)	0.012*** (0.003)	0.011** (0.004)
Student characteristics								
Number of prior fights	0.100*** (0.020)	0.129*** (0.013)	0.106*** (0.020)	0.129*** (0.013)	0.034*** (0.003)	0.040*** (0.004)	0.034*** (0.003)	0.040*** (0.004)
FRPL			-0.010 (0.029)	0.059*** (0.019)			0.015*** (0.004)	0.020*** (0.006)
SPED			-0.121*** (0.034)	-0.068*** (0.023)			-0.009** (0.004)	-0.025** (0.007)
Male			-0.106*** (0.039)	-0.021 (0.039)			-0.012*** (0.004)	0.002 (0.012)
Math score ( <i>t</i> -1)			0.025 (0.024)	-0.007 (0.015)			-0.003 (0.003)	-0.007 (0.005)
ELA score ( <i>t</i> -1)			-0.064*** (0.024)	-0.017 (0.015)			-0.003 (0.003)	0.000 (0.005)
Constant	2.892*** (0.027)	2.853*** (0.018)	2.964*** (0.045)	2.817*** (0.039)	0.046*** (0.004)	0.038*** (0.005)	0.041*** (0.006)	0.023** (0.012)
Observations	40,280	40,280	40,280	40,280	40,280	40,280	40,280	40,280
R-squared	0.008	0.009	0.014	0.016	0.018	0.019	0.021	0.021
School-Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Fight occurrence FEs	No	Yes	No	Yes	No	Yes	No	Yes

*Notes.* The unit of observation is the infraction, and the sample is restricted to students who were suspended for fighting (with a student of a different race). The number of prior fights refers to the number of fights for which the student had been suspended earlier in the same school year. The number of days suspended is censored to 20 for suspensions that exceeded 20 days. Standard errors appear in parentheses and account for the clustering of students within schools.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$



**Table 9**  
*Discipline Gaps in Fights Between Poor and Non-Poor Students*

Outcome variable	Number of days suspended				Whether received longer suspension than peer			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
FRPL student	0.187 (0.132)	0.185 (0.130)	0.192 (0.151)	0.274 (0.221)	0.014*** (0.003)	0.013*** (0.003)	0.012*** (0.003)	0.011*** (0.003)
Student characteristics								
Number of prior fights	0.021 (0.050)	0.100*** (0.020)	0.029 (0.047)	0.109*** (0.019)	0.033*** (0.002)	0.037*** (0.003)	0.032*** (0.002)	0.037*** (0.003)
Black			-0.033 (0.147)	-0.629 (0.686)			0.012*** (0.003)	0.021*** (0.005)
Other race			-0.074 (0.111)	-0.337 (0.302)			0.002 (0.007)	-0.005 (0.011)
SPED			-0.182*** (0.057)	-0.276* (0.153)			-0.009** (0.004)	-0.032*** (0.006)
Male			-0.190*** (0.057)	0.001 (0.061)			-0.001 (0.003)	-0.007 (0.010)
Math ( <i>t</i> -1)			-0.023 (0.031)	-0.050 (0.059)			-0.005* (0.003)	-0.010** (0.004)
ELA ( <i>t</i> -1)			-0.040 (0.030)	-0.003 (0.021)			-0.006** (0.003)	-0.004 (0.004)
Constant	3.413*** (0.040)	3.307*** (0.059)	3.508*** (0.080)	3.642*** (0.334)	0.052*** (0.003)	0.047*** (0.005)	0.040*** (0.004)	0.032*** (0.009)
Observations	61,502	61,502	61,502	61,502	61,502	61,502	61,502	61,502
R-squared	0.007	0.007	0.011	0.011	0.009	0.009	0.011	0.011
School-Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Fight occurrence FEs	No	Yes	No	Yes	No	Yes	No	Yes

*Notes.* The unit of observation is the infraction, and the sample is restricted to students who were suspended for fighting (with a student of a different FRPL status). The number of prior fights refers to the number of fights for which the student had been suspended earlier in the same school year. The number of days suspended is censored to 20 for suspensions that exceeded 20 days. Standard errors appear in parentheses and account for the clustering of students within schools.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

**Table 10***Gaps in Days Suspended for Black/White and Poor/Non-Poor Fights That Were Students' First Fights or Suspensions*

	First fight of year		First fight ever		First suspension of year		First suspension ever	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>Panel A: Race comparison</u>								
Black student	0.047*** (0.010)	0.046*** (0.014)	0.049*** (0.013)	0.043** (0.017)	0.041*** (0.011)	0.045*** (0.015)	0.054*** (0.018)	0.048* (0.025)
Constant	2.998*** (0.005)	3.085*** (0.039)	2.984*** (0.049)	3.012*** (0.071)	2.887*** (0.005)	2.961*** (0.045)	2.944*** (0.061)	2.895*** (0.094)
Observations	29,824	29,824	17,232	17,232	21,488	21,488	10,506	10,506
R-squared	0.008	0.008	0.007	0.007	0.009	0.008	0.007	0.007
<u>Panel B: FRPL comparison</u>								
FRPL student	0.232 (0.168)	0.247 (0.195)	0.354 (0.289)	0.389 (0.338)	0.023 (0.016)	0.003 (0.018)	0.054** (0.023)	0.034 (0.028)
Constant	3.388*** (0.084)	3.478*** (0.044)	3.465*** (0.153)	3.697*** (0.127)	3.244*** (0.008)	3.309*** (0.048)	3.383*** (0.073)	3.333*** (0.102)
Observations	47,216	47,216	27,490	27,490	34,338	34,338	18,128	18,128
R-squared	0.007	0.007	0.006	0.006	0.008	0.012	0.008	0.011
Student controls	No	Yes	No	Yes	No	Yes	No	Yes
# of years in data control	No	No	Yes	Yes	No	No	Yes	Yes
School-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fight occurrence FEs	No	No	No	No	No	No	No	No

*Notes.* The unit of observation is the infraction, and the sample is restricted to students who were suspended for fighting with a student of a different race (Panel A) or poverty status (Panel B). The “first fight” and “first suspension” sample restrictions apply to both students involved in the fight. For example, the “first fight of year” columns restrict the sample to fights between two students who had not been suspended for a fight earlier in that school year. Student controls consist of FRPL status (Panel A only), black and other race (Panel B only), special education status, gender, and math and ELA scores from the prior year. The reference group for “Black student” is white students. The reference group for “FRPL student” is non-FRPL students. The number of days suspended is censored to 20 for suspensions that exceeded 20 days. Standard errors appear in parentheses and account for the clustering of students within schools.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

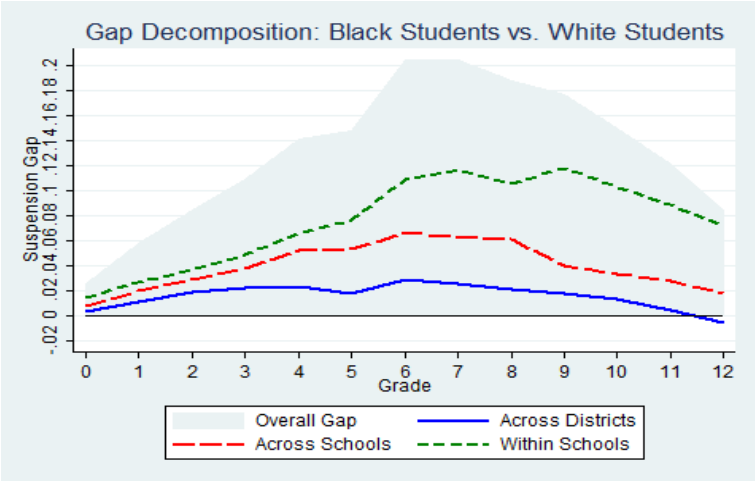
**Table 11***Who Received Longer Suspensions for Black/White and Poor/Non-Poor Fights That Were Students' First Fights or Suspensions*

	First fight of year		First fight ever		First suspension of year		First suspension ever	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>Panel A: Race comparison</u>								
Black student	0.014*** (0.004)	0.011*** (0.004)	0.017*** (0.005)	0.013*** (0.005)	0.016*** (0.004)	0.012*** (0.004)	0.021*** (0.005)	0.018*** (0.006)
Constant	0.079*** (0.002)	0.074*** (0.005)	0.074*** (0.004)	0.067*** (0.007)	0.075*** (0.002)	0.074*** (0.006)	0.061*** (0.005)	0.059*** (0.010)
Observations	29,824	29,824	17,232	17,232	21,488	21,488	10,506	10,506
R-squared	0.007	0.010	0.007	0.010	0.008	0.011	0.010	0.011
<u>Panel B: FRPL comparison</u>								
FRPL student	0.013*** (0.003)	0.011*** (0.003)	0.014*** (0.004)	0.012*** (0.004)	0.015*** (0.003)	0.013*** (0.003)	0.015*** (0.004)	0.012*** (0.005)
Constant	0.082*** (0.001)	0.069*** (0.003)	0.073*** (0.003)	0.058*** (0.006)	0.080*** (0.002)	0.070*** (0.004)	0.071*** (0.004)	0.052*** (0.007)
Observations	47,216	47,216	27,490	27,490	34,338	34,338	18,128	18,128
R-squared	0.008	0.008	0.007	0.007	0.008	0.010	0.008	0.010
Student controls	No	Yes	No	Yes	No	Yes	No	Yes
# of years in data control	No	No	Yes	Yes	No	No	Yes	Yes
School-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fight occurrence FEs	No	No	No	No	No	No	No	No

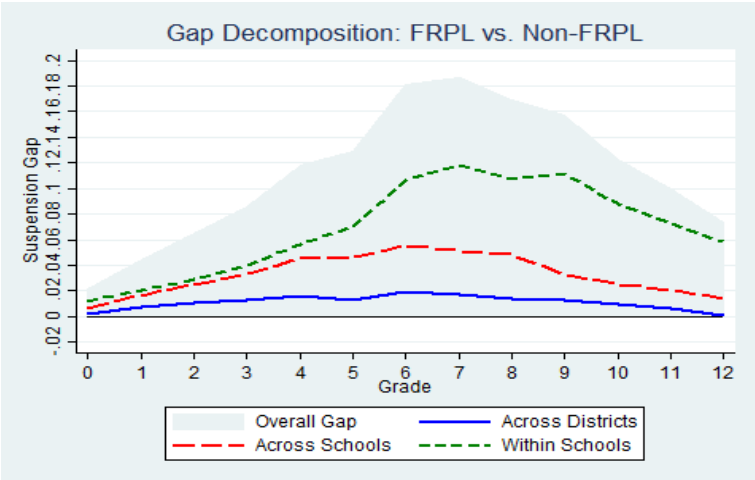
*Notes.* The unit of observation is the infraction, and the sample is restricted to students who were suspended for fighting with a student of a different race (Panel A) or poverty status (Panel B). The “first fight” and “first suspension” sample restrictions apply to both students involved in the fight. For example, the “first fight of year” columns restrict the sample to fights between two students who had not been suspended for a fight earlier in that school year. Student controls consist of FRPL status (Panel A only), black and other race (Panel B only), special education status, gender, and math and ELA scores from the prior year. The reference group for “Black student” is white students. The reference group for “FRPL student” is non-FRPL students. The number of days suspended is censored to 20 for suspensions that exceeded 20 days. Standard errors appear in parentheses and account for the clustering of students within schools.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

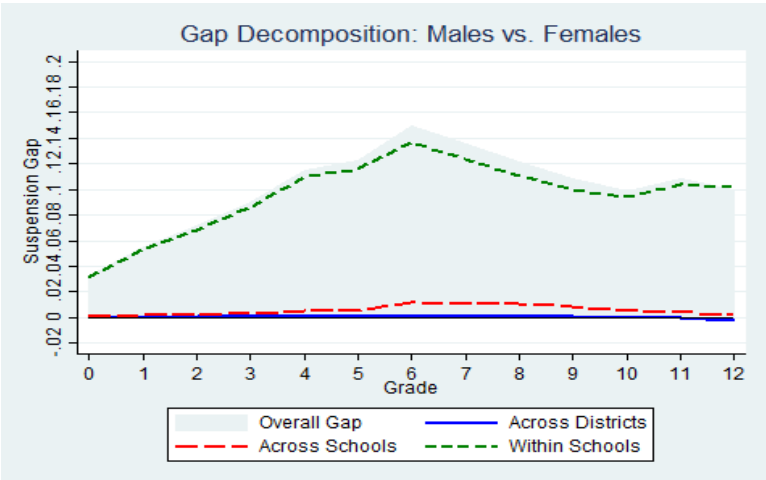
**Figure 1**  
*Decomposition of Black/White Gaps—Whether Suspended*



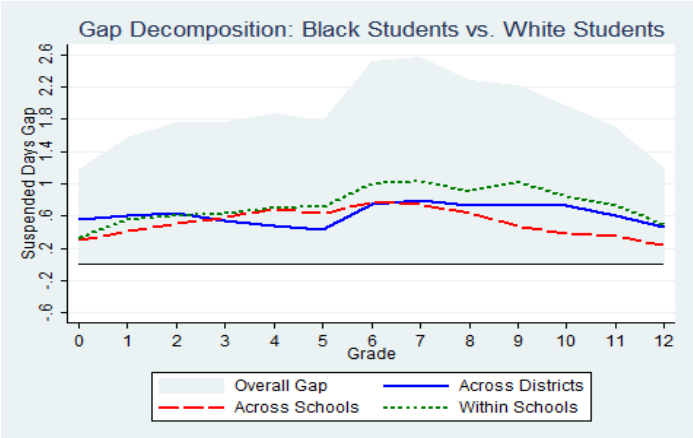
**Figure 2**  
*Decomposition of Poor/Non-Poor Gaps—Whether Suspended*



**Figure 3**  
*Decomposition of Male/Female Gaps—Whether Suspended*

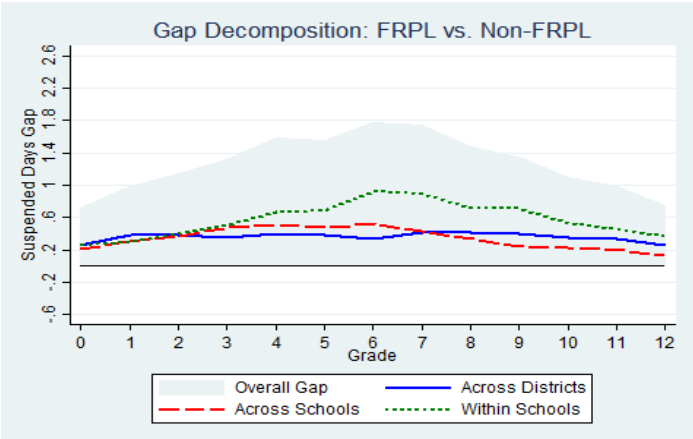


**Figure 4**  
*Decomposition of Black/White Gaps—Number of Days Suspended*



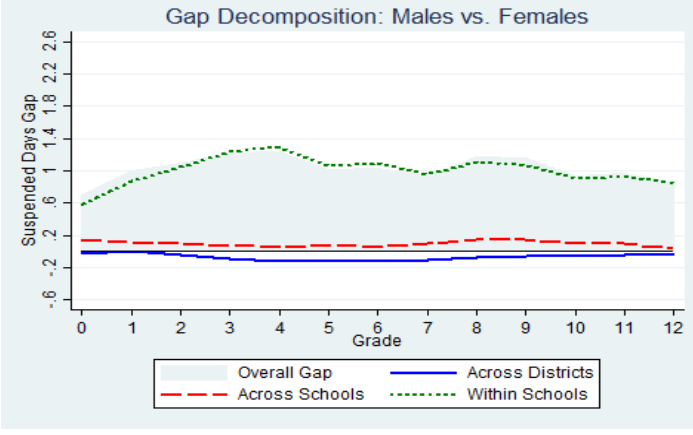
Note: Sample is restricted to students with at least one suspension in given school year.

**Figure 5**  
*Decomposition of Poor/Non-Poor Gaps—Number of Days Suspended*



Note: Sample is restricted to students with at least one suspension in given school year.

**Figure 6**  
*Decomposition of Male/Female Gaps—Number of Days Suspended*



Note: Sample is restricted to students with at least one suspension in given school year.

**Table A1***Raw Gaps in Whether Suspended Across Districts, Across Schools, and Within Schools*

Grade	Source of Gap	Black/White		FRPL/Non-FRPL		Male/Female	
K	Across Districts	0.004	13.5%	0.002	11.0%	0.000	0.6%
	Across Schools	0.008	29.9%	0.007	32.0%	0.001	2.1%
	Within School	0.015	56.7%	0.012	57.0%	0.031	97.3%
	<i>Total</i>	0.026		0.021		0.032	
Grade 1	Across Districts	0.011	19.4%	0.007	16.3%	0.000	0.4%
	Across Schools	0.020	34.1%	0.017	37.3%	0.002	3.0%
	Within School	0.027	46.5%	0.021	46.4%	0.053	96.6%
	<i>Total</i>	0.058		0.044		0.055	
Grade 2	Across Districts	0.019	22.1%	0.011	16.4%	0.000	0.6%
	Across Schools	0.029	34.6%	0.025	39.0%	0.002	3.1%
	Within School	0.037	43.3%	0.029	44.6%	0.069	96.3%
	<i>Total</i>	0.085		0.064		0.071	
Grade 3	Across Districts	0.022	20.2%	0.013	15.5%	0.000	0.4%
	Across Schools	0.038	35.0%	0.033	38.8%	0.003	3.6%
	Within School	0.049	44.8%	0.039	45.7%	0.086	96.0%
	<i>Total</i>	0.109		0.086		0.090	
Grade 4	Across Districts	0.023	16.4%	0.016	13.3%	0.001	0.5%
	Across Schools	0.052	37.0%	0.045	38.5%	0.005	4.4%
	Within School	0.066	46.6%	0.057	48.2%	0.110	95.2%
	<i>Total</i>	0.142		0.118		0.115	
Grade 5	Across Districts	0.018	12.2%	0.013	10.3%	0.001	0.5%
	Across Schools	0.053	36.1%	0.046	35.6%	0.006	4.6%
	Within School	0.076	51.7%	0.070	54.1%	0.116	94.9%
	<i>Total</i>	0.148		0.129		0.122	
Grade 6	Across Districts	0.029	14.1%	0.019	10.6%	0.002	1.1%
	Across Schools	0.067	32.6%	0.056	30.6%	0.012	7.8%
	Within School	0.109	53.2%	0.107	58.8%	0.137	91.1%
	<i>Total</i>	0.205		0.181		0.150	
Grade 7	Across Districts	0.026	12.5%	0.017	9.3%	0.001	0.6%
	Across Schools	0.063	30.8%	0.052	27.5%	0.011	8.1%
	Within School	0.117	56.7%	0.118	63.2%	0.124	91.3%
	<i>Total</i>	0.206		0.187		0.136	
Grade 8	Across Districts	0.021	11.2%	0.014	8.1%	0.000	0.3%
	Across Schools	0.062	32.8%	0.048	28.5%	0.011	8.7%
	Within School	0.106	56.0%	0.108	63.4%	0.111	91.0%
	<i>Total</i>	0.189		0.170		0.122	
Grade 9	Across Districts	0.018	10.3%	0.013	8.1%	0.000	0.4%
	Across Schools	0.040	22.8%	0.033	20.9%	0.009	8.0%
	Within School	0.119	66.9%	0.112	71.0%	0.100	91.7%
	<i>Total</i>	0.177		0.157		0.109	
Grade 10	Across Districts	0.014	9.3%	0.010	7.9%	0.000	0.1%
	Across Schools	0.034	22.3%	0.026	20.8%	0.005	5.3%
	Within School	0.103	68.4%	0.088	71.4%	0.094	94.6%
	<i>Total</i>	0.151		0.123		0.099	
Grade 11	Across Districts	0.005	3.7%	0.007	6.7%	0.000	0.3%
	Across Schools	0.028	23.3%	0.021	20.8%	0.005	4.2%
	Within School	0.089	73.0%	0.072	72.5%	0.104	95.5%
	<i>Total</i>	0.122		0.100		0.109	
Grade 12	Across Districts	-0.005	-6.1%	0.001	1.7%	-0.003	-2.6%
	Across Schools	0.018	21.5%	0.014	18.9%	0.001	1.5%
	Within School	0.072	84.6%	0.058	79.4%	0.102	101.2%
	<i>Total</i>	0.085		0.073		0.100	

**Table A2***Raw Gaps in Number of Days Suspended Across Districts, Across Schools, and Within Schools*

Grade	Source of Gap	Black/White		FRPL/Non-FRPL		Male/Female	
K	Across Districts	0.554	47.0%	0.254	35.8%	-0.020	-2.9%
	Across Schools	0.294	25.0%	0.204	28.8%	0.136	19.6%
	Within School	0.329	28.0%	0.252	35.5%	0.579	83.2%
	<i>Total</i>	1.177		0.710		0.695	
Grade 1	Across Districts	0.605	38.5%	0.377	38.7%	0.008	0.9%
	Across Schools	0.403	25.7%	0.303	31.1%	0.121	12.2%
	Within School	0.562	35.8%	0.295	30.2%	0.864	86.9%
	<i>Total</i>	1.569		0.975		0.994	
Grade 2	Across Districts	0.644	36.8%	0.377	32.9%	-0.047	-4.3%
	Across Schools	0.503	28.8%	0.365	31.8%	0.089	8.2%
	Within School	0.603	34.4%	0.404	35.3%	1.045	96.2%
	<i>Total</i>	1.750		1.146		1.086	
Grade 3	Across Districts	0.544	30.9%	0.348	26.4%	-0.087	-7.2%
	Across Schools	0.583	33.1%	0.470	35.7%	0.074	6.0%
	Within School	0.632	35.9%	0.499	37.9%	1.233	101.1%
	<i>Total</i>	1.758		1.317		1.219	
Grade 4	Across Districts	0.471	25.3%	0.402	25.5%	-0.117	-9.5%
	Across Schools	0.690	37.0%	0.509	32.3%	0.055	4.4%
	Within School	0.702	37.7%	0.665	42.2%	1.291	105.0%
	<i>Total</i>	1.862		1.577		1.229	
Grade 5	Across Districts	0.428	24.2%	0.387	25.0%	-0.108	-10.7%
	Across Schools	0.631	35.6%	0.476	30.8%	0.067	6.6%
	Within School	0.713	40.3%	0.682	44.1%	1.054	104.1%
	<i>Total</i>	1.772		1.545		1.012	
Grade 6	Across Districts	0.739	29.4%	0.337	19.0%	-0.120	-11.7%
	Across Schools	0.768	30.6%	0.516	29.1%	0.056	5.4%
	Within School	1.005	40.0%	0.923	52.0%	1.091	106.3%
	<i>Total</i>	2.512		1.777		1.026	
Grade 7	Across Districts	0.786	30.7%	0.422	24.3%	-0.109	-11.6%
	Across Schools	0.749	29.2%	0.427	24.5%	0.093	9.9%
	Within School	1.029	40.1%	0.889	51.2%	0.955	101.7%
	<i>Total</i>	2.564		1.738		0.938	
Grade 8	Across Districts	0.734	32.3%	0.408	27.9%	-0.076	-6.4%
	Across Schools	0.631	27.8%	0.337	23.0%	0.151	12.8%
	Within School	0.905	39.9%	0.719	49.1%	1.104	93.6%
	<i>Total</i>	2.271		1.465		1.179	
Grade 9	Across Districts	0.728	32.9%	0.392	29.2%	-0.052	-4.5%
	Across Schools	0.469	21.2%	0.241	18.0%	0.149	12.8%
	Within School	1.017	45.9%	0.709	52.8%	1.066	91.7%
	<i>Total</i>	2.214		1.341		1.163	
Grade 10	Across Districts	0.733	37.4%	0.351	31.9%	-0.056	-6.0%
	Across Schools	0.385	19.7%	0.218	19.8%	0.092	9.8%
	Within School	0.842	43.0%	0.530	48.2%	0.903	96.2%
	<i>Total</i>	1.961		1.099		0.939	
Grade 11	Across Districts	0.601	35.6%	0.329	33.6%	-0.046	-4.7%
	Across Schools	0.354	21.0%	0.196	20.1%	0.095	9.8%
	Within School	0.734	43.5%	0.452	46.3%	0.918	94.9%
	<i>Total</i>	1.689		0.977		0.967	
Grade 12	Across Districts	0.468	39.3%	0.251	33.8%	-0.035	-4.2%
	Across Schools	0.234	19.6%	0.128	17.3%	0.038	4.4%
	Within School	0.491	41.1%	0.363	48.9%	0.848	99.7%
	<i>Total</i>	1.193		0.742		0.851	

*Notes.* The negative gaps indicate that female students had longer suspensions than male students.

**Table A3***Predictors of Whether Students were Suspended, Weighted Models*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black	0.128*** (0.001)	0.125*** (0.001)	0.115*** (0.001)	0.128*** (0.001)	0.126*** (0.001)	0.125*** (0.001)			
Other race				-0.017*** (0.001)	-0.014*** (0.001)	-0.011*** (0.003)			
FRPL							0.091*** (0.001)	0.088*** (0.000)	0.085*** (0.000)
Constant	0.122*** (0.001)	0.123*** (0.000)	0.129*** (0.000)	0.122*** (0.001)	0.123*** (0.000)	0.149*** (0.001)	0.124*** (0.001)	0.126*** (0.000)	0.119*** (0.000)
Observations	9,432,233	9,432,233	8,464,345	9,981,117	9,981,117	7,674,447	9,989,263	9,989,263	9,829,624
R-squared	0.027	0.192	0.198	0.029	0.190	0.192	0.013	0.184	0.192
SGY FEs	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Weights	No	No	Yes	No	No	Yes	No	No	Yes

*Notes.* The unit of observation is the student-year. Standard errors appear in parentheses and account for the clustering of students within school-grade-year. “SGY FEs” refers to school-grade-year fixed effects. Weighted regressions account for the extent to which race and income proportions vary across SGY cells. The regression in Column 3 is weighted by  $1/p(\text{Black})p(\text{White})$  for each SGY cell. The regression in Column 6 is weighted by  $1/p(\text{Black})p(\text{White})p(\text{Other race})$  in each SGY cell. The regression in Column 9 is weighted by  $1/p(\text{FRPL})p(\text{not FRPL})$  in each SGY cell.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$



**Table A4**  
*Predictors of Whether Students were Suspended Multiple Times in Same Year, Weighted Models*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Black	0.082*** (0.001)	0.083*** (0.001)	0.075*** (0.001)	0.082*** (0.001)	0.083*** (0.000)	0.082*** (0.001)			
Other race				-0.008*** (0.001)	-0.006*** (0.000)	0.000 (0.002)			
FRPL							0.059*** (0.000)	0.055*** (0.000)	0.052*** (0.000)
Constant	0.055*** (0.000)	0.055*** (0.000)	0.055*** (0.000)	0.055*** (0.000)	0.055*** (0.000)	0.065*** (0.001)	0.056*** (0.000)	0.059*** (0.000)	0.055*** (0.000)
Observations	9,432,233	9,432,233	8,464,345	9,981,117	9,981,117	7,674,447	9,989,263	9,989,263	9,829,624
R-squared	0.020	0.162	0.163	0.020	0.160	0.161	0.010	0.154	0.164
SGY FEs	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Weights	No	No	Yes	No	No	Yes	No	No	Yes

*Notes.* The unit of observation is the student-year. Standard errors appear in parentheses and account for the clustering of students within school-grade-year. “SGY FEs” refers to school-grade-year fixed effects. Weighted regressions account for the extent to which race and income proportions vary across SGY cells. The regression in Column 3 is weighted by  $1/p(\text{Black})p(\text{White})$  for each SGY cell. The regression in Column 6 is weighted by  $1/p(\text{Black})p(\text{White})p(\text{other Race})$  in each SGY cell. The regression in Column 9 is weighted by  $1/p(\text{FRPL})p(\text{not FRPL})$  in each SGY cell.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$