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Exposure to Same-Race Teachers and Student Disciplinary Outcomes for Black Students in North Carolina

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Using student-level administrative data from North Carolina, we explore whether exposure to same-race teachers affects the rate at which Black students receive exclusionary discipline, such as out-of-school suspensions, in-school suspensions, and expulsion. We find consistent evidence that exposure to same-race teachers is associated with reduced rates of exclusionary discipline for Black students. This relationship holds for elementary, middle, and high school grade ranges for male and female students, and for students who do and do not use free and reduced-price lunch. Although we find reductions in referrals for a number of different types of offenses, we find particularly consistent evidence that exposure to same-race teachers lowers office referrals for willful defiance across all grade levels, suggesting that teacher discretion plays a role in driving our results.

Keywords: *teachers, discipline, race, disproportionality*

Observers of the American educational system have long been concerned over unequal educational opportunity for students from different demographic backgrounds. Achievement gaps that see Black and Latino students trailing their White and Asian peers in achievement (Reardon & Robinson, 2008) and graduation rates (Murnane, 2013) have received the bulk of attention from researchers. While these gaps are viewed as a source of persistent racial disparities, a smaller body of literature has revealed that Black and Latino students are also disproportionately subject to harsh discipline actions (Skiba et al., 2011; Skiba, Michael, Nardo, & Peterson, 2002). Indeed, this concern over disparate disciplinary outcomes has come to fuel concern over what is commonly called the "school-to-prison" pipeline, in which students who experience harsh discipline see higher likelihoods of eventually becoming engaged with the justice system (Curtis, 2014). One disciplinary technique that causes particular concern is exclusionary discipline, whereby students are removed from the classroom as punishment. Exclusionary discipline is especially pernicious because removing students from the classroom decreases instructional time, feeding into achievement gaps as well as discipline gaps (Morris & Perry, 2016). The overreliance on exclusionary disciplines for students of color, for Black students in particular (U.S. Department of Justice, Civil Rights Division, & U.S. Department of Education, Office for Civil Rights, 2014), is a cause for concern.

One common policy prescription to address these disparities in outcomes for students of color is to increase the share of Black and Latino teachers (National Education Association, 2014).

While past research offers support for the argument that exposure to same-race teachers may improve minority students' academic achievement (Dee, 2004; Egalite, Kisida, & Winters, 2015), far less attention has been paid to whether these policy prescriptions could also affect other outcomes, such as disciplinary outcomes.

We seek to fill this gap in the literature by using quasi-experimental methods on a rich set of administrative data from North Carolina to determine whether exposure to Black teachers is associated with student discipline outcomes for Black children. Specifically, we explore whether exposure to same-race teachers affects the likelihood that students are subject to exclusionary discipline actions (i.e., expulsion, out-of-school suspension, in-school suspension), both overall and for students in different grade ranges.

North Carolina presents an interesting context in which to explore these questions. The state is large and diverse with regard to student demographics as well as types of schools. Schools are required by law to report disciplinary incidents to the state. North Carolina follows national trends with regard to racial and gender disparities in expulsions and suspensions (State Board of Education and Department of Public Instruction, 2016). In 2012–2013, Black male students were the group most likely to be suspended across the state. Our data cover the whole state for multiple years, which allows us to identify patterns over and above idiosyncratic trends unique to individual schools.

We find that exposure to Black teachers is associated with lower discipline rates on average for Black students. This relationship is consistent across elementary, middle, and high school grades, although estimates are most precisely estimated and most consistent at the elementary school level. We find that the pattern of results holds using multiple analytic strategies, including student fixed effects and instrumental variables strategies. The results hold across student gender and subsidized lunch use categories. Overall, our results suggest that Black students see modest, but consistent, reductions in exclusionary discipline exposure when they are matched with larger shares of Black teachers. We find that Black teachers are associated in particular with reductions in office referrals for defiance-related offenses.

Relevant Literature

A growing number of commentators have noted that the race composition of the teaching workforce is vastly mismatched to the current public school student population (National Education Association, 2014). In fall 2014, public school students in the United States became majority "minority" for the first time (Maxwell, 2014). Yet, teacher diversity has decreased even as the share of the student population from Black and Latino backgrounds has grown (Boser, 2014). North Carolina's experience reflects these national trends. From 2001 to 2013, the percentage of Black or Latino teachers in the state declined slightly from 15.61% to 14.95%, while the share of Black or Latino students rose from 33.63% to 39.35% (Figure 1).

The representation gap in North Carolina between teachers and students varied in extent from district to district, but the patterns were similar statewide. Figure 2 represents this graphically, showing the relationship between the demographic makeup of students and teachers among the local educational agencies in North Carolina in 2012–2013. The graph on the left shows the share of teachers and students in each local education agency (LEA) who are Black, whereas the graph on the right shows the same relationship for the share White. Note that if a district had identical representation among its student body and its teaching workforce, it would lie on the 45° line. Instead, we see that in every LEA across the state, Whites are more heavily concentrated in the teaching workforce than in the student body (i.e., all points lie above the 45° line). By contrast, every LEA has a higher concentration of Black students than Black teachers.

There are serious implications of this underrepresentation of Black and Latino teachers for students. Scholars argue that this demographic mismatch can drive inequality in student outcomes (Grissom, Kern, & Rodriguez, 2015). For instance, in a study that exploited random assignment of teachers to students, Dee (2004) finds that Black and White children alike had significantly higher achievement when assigned to a same-race teacher; as White students are far more likely to have same-race teachers, they have disproportionate access to this benefit. Numerous studies at the K–12 level find that compared with

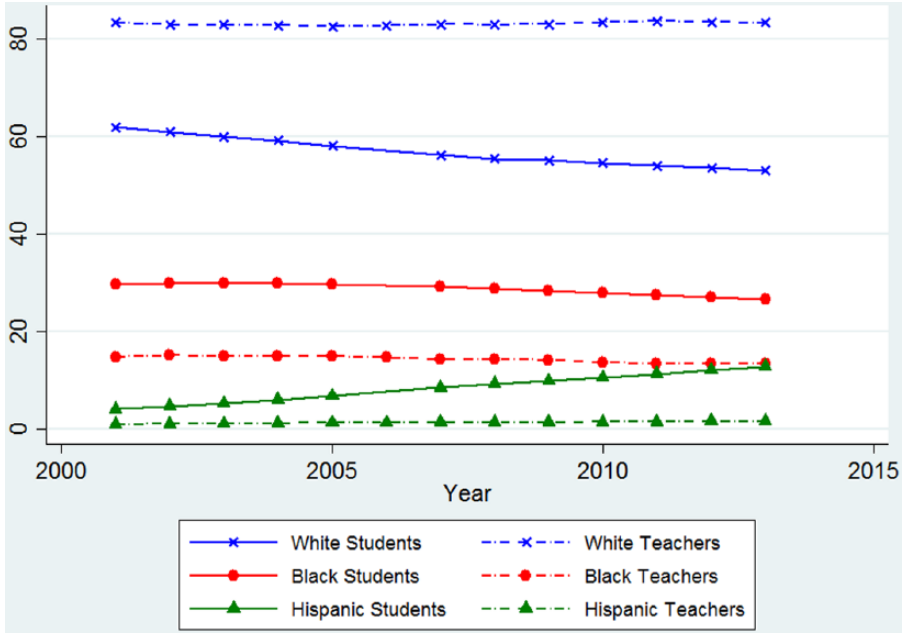

FIGURE 1. *Demographic composition in North Carolina over time: Teachers and students.*
Source. Author calculations from North Carolina Education Research Data Center (NCERDC) data.

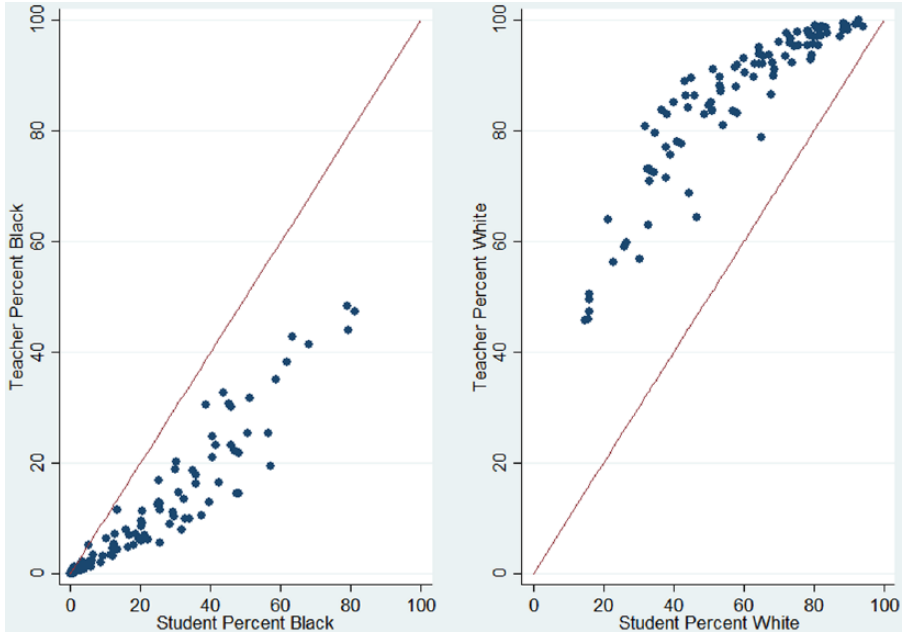

FIGURE 2. *Demographic representation of teachers and students in North Carolina by LEA, 2012–2013.*
Source. Authors' calculations from North Carolina Education Research Data Center (NCERDC) data.
Note. Charter schools included in LEAs where they are geographically located. LEA = local education agency.

their White colleagues, teachers of color produce more favorable academic results for students of color, on average, on standardized test scores (Dee, 2005; Egalite et al., 2015; Hanushek, Kain,

O'Brien, & Rivkin, 2005 but see Ehrenberg, Goldhaber, & Brewer, 1995), attendance (Farkas, Grobe, Sheehan, & Shuan, 1990), advanced-level course enrollment (Klopfenstein, 2005), and college-going rates (Hess & Leal, 1997). Studies at the postsecondary level have likewise documented benefits of match to race-congruent instructors, in terms of both contemporaneous course performance and downstream outcomes (Fairlie, Hoffman, & Oreopoulos, 2014).

Fewer studies have empirically assessed the role of teacher characteristics in determining student disciplinary outcomes. This is a substantial oversight, because many studies have detailed both the overrepresentation of Black and Latino students among students exposed to exclusionary discipline (Skiba et al., 2002; U.S. Department of Justice, Civil Rights Division, & U.S. Department of Education, Office for Civil Rights, 2014), and the negative relationship between student disciplinary actions and downstream student outcomes. Exclusionary discipline is associated with poorer academic outcomes for students (Gregory, Skiba, & Noguera, 2010), as well as noncognitive outcomes such as arrests and antisocial behaviors, contributing to what is known in popular conversation as the "school-to-prison pipeline" (Hemphill, Toumbourou, Herrenkohl, McMorris, & Catalano, 2006).

There is a good reason to think that teacher characteristics could have important impacts on whether students are exposed to exclusionary discipline. Demographic match may affect student (mis)behavior. For instance, if, on average, students have better rapport with same-race teachers (Villegas & Irvine, 2010), they may be less likely to act up in class. In addition, teachers serve as gatekeepers who determine whether the severity and frequency of student misbehavior merit an office referral (Skiba et al., 2002). If teachers are subconsciously inclined to be more (or less) lenient toward same-race students (Gregory & Mosely, 2004; Gregory et al., 2010), demographic match could matter for student disciplinary outcomes even if demographic match does not affect student behavior. Research on younger children provides evidence that implicit bias of White teachers may drive more negative interpretations of behavior (Downey & Pribesh, 2004). Recent evidence using data from the Educational Longitudinal Survey (Gershenson, Holt, & Papageorge, 2016) indicates that Black teachers tend to have systematically more favorable expectations of Black students than do White teachers; such differences in subjective evaluations could lead to different applications of discipline.

Consistent with theory, a handful of studies have found that Black students are less likely to receive exclusionary discipline in schools with higher concentrations of Black teachers (Grissom, Nicholson-Crotty, & Nicholson-Crotty, 2009; Meier, 1984; Meier & Stewart, 1992), with similar results for Latino students (Meier, 1993). However, these studies look at the relationship of aggregate discipline rates for minority students with aggregate teacher demographic composition. These studies speak directly to the racial composition of the staff at the school level. Because these studies lack the data to examine differential discipline rates within schools using individual-level data, they risk confounding the effects of teacher demographics with other aspects of the school that may be correlated with teacher demographics. For instance, teachers of color are disproportionately employed in hard-to-staff schools, which also enroll a disproportionate share of students of color (Achinstein, Ogawa, Sexton, & Freitas, 2010). If those schools also have discipline policies that are more (or less) harsh than average, estimates of the relationship of demographic match on disciplinary outcomes will be biased if school characteristics are excluded (Clotfelter, Ladd, & Vigdor, 2006). Avoiding this problem requires data that look at the differential discipline rates of students with and without same-race teachers *within the same school.* Our study explores the direct impact of teacher–student match.

Currently, we know of only one study that has used such data (Kinsler, 2011). Like ours, that study used North Carolina data, but in only 1 year, and for a very small subset of elementary school students (~50,000 third- to fifth-grade students out of a state population of more than 315,000 in those grades in that year). One of the key data limitations the study faced was that certain districts did not reliably record student identifiers that could be linked to other North Carolina data files. Kinsler's analysis therefore excludes districts in which fewer than 80% of discipline records could be matched to individual students.

Our examination of 2001 data suggests that the districts that were nonusable under Kinsler's criteria include Charlotte-Mecklenburg and Wake; these are the two largest districts in North Carolina, and both serve diverse student populations.[1] The study was also limited to only 1 year of data, limiting the analytical techniques available. Kinsler finds that while Black students who are matched to Black teachers are less likely to be suspended than are those who are matched to White teachers, this difference is not significant.

Our study expands upon this sample considerably. We are able to observe a broader range of students across more years and more grade levels. Importantly, we are able to look at middle and high school grades as well as students in elementary school grades. Given our multiple years of data, we are also able to use stronger and more varied quasi-experimental techniques detailed below. Given this more powerful set of tools, we find consistent significant, negative effects of exposure to same-race teachers on the likelihood of receiving exclusionary discipline for Black students.

Method

Data and Sample

We use administrative data from North Carolina from 2007–2008 school year through the 2012–2013 school year to determine whether student disciplinary outcomes for Black students are related to exposure to same-race teachers. Since 2001, the state has collected disciplinary records for each student, including offense type and whether expulsion/out-of-school suspension consequences are attached to each disciplinary incident. Since 2007–2008, the state has also included incidents that resulted in a wider variety of outcomes, including in-school suspension and detentions. We therefore focus on the 2008 to 2013 period. All years given will refer to the spring of the academic year, so 2007–2008 will be denoted as 2008. North Carolina data also allow us to identify which teachers are matched to each student for each class during this time period, as well as the race of teachers serving as instructors for those classes. A handful of school covariates (urbanicity, enrollment, pupil-to-teacher ratios, demographic composition of the school, and charter/magnet status) are also drawn from the Common Core of Data surveys maintained by the National Center for Education Statistics.

Our sample includes elementary, middle, and high school (1st grade–12th grade) students attending North Carolina public schools from 2007–2008 to 2012–2013. Because Black students experience exceptionally high exclusionary discipline rates, our main analyses focus on this subgroup, although some follow-on analyses estimate effects of teacher demographic composition for non-Black students as well. Student race is captured through variables that assign students to mutually exclusive race/ethnicity categories in the administrative data.[2]

We also drop a small share of observations (<5% of total observations) that could not be linked to teachers with observed demographic information. We observe 2,236,678 student-school-year observations across 6 years for Black students.

Models

A naïve analysis examining the effects of teacher race composition on student disciplinary outcomes might use simple ordinary least squares (OLS) techniques:

$$\begin{aligned} Discipline_{igst} = {} & \beta TchPctBlack_{igst} + \\ & \delta TchPctOtherRace_{igst} + \\ & \lambda StudentChar_{igst} + \\ & \delta SchoolChar_{st} + \theta_t + \varepsilon_{igst}. \end{aligned} \tag{0.1}$$

The outcome of interest (*Discipline*) for student *i* in grade *g* in school *s* in year *t* is modeled as a function of the share of the student's own teachers who are Black (*TchPctBlack*) or other race (non-White, non-Black; *TchPctOtherRace*), time-varying and time-invariant student characteristics (e.g., race, sex, family income; *StudentChar*); time-varying and time-invariant school characteristics (such as demographic makeup; *SchoolChar*); and year fixed effects θ_t. Finally, ε is an independently and identically distributed error term.

However, Equation 0.1 may not account for unobservable school factors associated with both exposure to Black (or other race) teachers and student discipline. For instance, if Black teachers are more likely to serve in hard-to-staff schools and these schools are also more likely to use

exclusionary discipline, then our estimates of teacher demographics would be subject to bias. The inclusion of school-grade fixed effects θ_{gs} controls for time-invariant observable and unobservable school-grade characteristics (which may include a culture of discipline use):

$$\begin{aligned} Discipline_{igst} = {} & \beta TchPctBlack_{igst} + \\ & \delta TchPctOtherRace_{igst} + \\ & \lambda StudentChar_{igst} + \\ & \delta SchoolChar_{st} \\ & + \theta_t + \theta_{gs} + \varepsilon_{igst}. \end{aligned} \tag{0.2}$$

However, the school-grade fixed effects estimates may still be subject to bias if schools are differentially likely to match students to Black or other race (i.e., non-White) teachers based on their expectation that students will be subject to exclusionary discipline. For instance, past studies using North Carolina data suggest that Black teachers are disproportionately likely to teach lower achieving students compared with their White colleagues (Clotfelter et al., 2006); if students in lower track courses are disproportionately likely to be subject to discipline, this would bias our school-grade fixed effects estimates by making it appear as though being matched to Black teachers was associated with a greater degree of discipline. Past papers that look at the effects of teacher race composition on student achievement outcomes (e.g., Egalite et al., 2015) have addressed this concern by using student fixed effects (θ_i) to compare student outcomes in years when they have exposure to teachers of different races. We use such student fixed effects to estimate our main set of results:

$$\begin{aligned} Discipline_{igst} = {} & \beta TchPctBlack_{igst} + \\ & \delta TchPctOtherRace_{igst} + \\ & \lambda StudentChar_{igst} + \\ & \delta SchoolChar_{st} + \theta_{gs} + \\ & \theta_t + \theta_i + \varepsilon_{igst}. \end{aligned} \tag{0.3}$$

These models allow us to compare students' discipline records in years where they are exposed to a larger fraction of Black (or other race) teachers compared with their own discipline records in years where Black teachers make up a smaller share of their teachers.

To confirm these results, we also use a novel instrumental variables analytic strategy. Specifically, we adapt techniques used previously by Bettinger and Long (2005, 2010) to explore whether exposure to tenured versus adjunct instructors affects postsecondary student outcomes. This strategy uses (plausibly exogenous) deviations from long-term trends in the race composition of faculty at a given school-grade level to determine whether, for instance, Black 10th graders have lower likelihood of suspension in years where Black teachers make up a larger share of the 10th-grade teaching staff of a given school compared with the historical school-grade average.

We fit a first-stage equation to determine the predicted share of teachers who will be from each racial group. Below, we illustrate the technique, using as an example the share of a student's teachers who are Black (*TchPctBlack*) as the first-stage outcome:

$$\begin{aligned} TchPctBlack_{igst} = {} & \pi_1 TchPctBlack_{gst} + \\ & \pi_7 TchPctOtherRace_{gst} + \\ & \delta SchoolChar_{st} + \\ & \lambda StudentChar_i + \\ & \theta_s + \theta_t + \theta_{gs} + \varepsilon_{igst}. \end{aligned} \tag{0.4}$$

Our main instruments are the terms $TchPctBlack_{gst}$ and $TchPctOtherRace_{gst}$. These terms capture the share of teachers from different race/ethnic categories in a particular school-grade in a particular year. Because we include a school-grade fixed effect (θ_{gs}), these specifically capture the deviation in the racial composition for each school grade in a given year, relative to the historical average racial composition for that school grade.

The predicted values generated by the first-stage equation are then entered into the second-stage equation:

$$\begin{aligned} Discipline_{igst} = {} & \beta TchPctBlack_{igst} + \\ & \delta TchPctOtherRace_{igst} + \\ & \lambda StudentChar_{igst} + \\ & \delta SchoolChar_{st} + \theta_{gs} + \\ & \theta_t + \varepsilon_{igst}. \end{aligned} \tag{0.5}$$

TABLE 1

Yearly Number of Suspensions Reported to North Carolina Department of Public Instruction

	Elementary grades		Middle school grades		High school grades	
	OSS	ISS	OSS	ISS	OSS	ISS
2007–2008	44,827	14,864	109,825	89,036	145,301	85,575
2008–2009	42,291	19,478	103,176	101,445	145,853	108,304
2009–2010	40,670	23,772	100,437	124,797	133,668	144,451
2010–2011	40,152	24,993	92,497	133,512	131,637	150,416
2011–2012	38,100	23,661	88,272	132,866	122,304	153,577
2012–2013	42,503	26,838	88,476	125,746	112,056	143,994

Source. Authors' calculations from North Carolina Education Research Data Center (NCERDC) data.
Note. Expulsions are too rare to list separately. Elementary school grades = Grades 1–5; middle school grades = 6–8; high school grades = 9–12. OSS = out-of-school suspension; ISS = in-school suspension.

The outcome of interest (*Discipline*) in our second-stage equations is therefore a function of the predicted values of *TchPctBlack and TchPctOtherRace*, time-varying and time-invariant student characteristics (e.g., race, sex, family income, special education status); time-varying school characteristics (such as demographic makeup); and school-grade and year fixed effects. Note that while we do estimate coefficients for other race teachers so that exposure to White teachers—by far the majority of the teaching force—is the comparison condition, because the theoretical relationship of interest is exposure to same-race teachers, we often interpret only the "Fraction Teachers Black" coefficient.

We use missing dummy variables in both equations to maintain the greatest possible degree of information from observations with missing data. All models are estimated using robust standard errors clustered at the school-grade level.

Measures

Our main dependent variable captures the extent to which middle and high school students are exposed to exclusionary disciplinary consequences: in-school suspensions, out-of-school suspensions, or expulsion. Table 1 catalogs the frequency with which suspensions were reported from 2007–2008 through 2012–2013; expulsions occurred too infrequently to be separated out. Our main outcome is a variable that captures whether students have received *any* exclusionary disciplinary consequence—in-school suspensions, out-of-school suspensions, or expulsion—during a given school year.

Our main predictor variable, as described above, is the proportion of a student's teachers who are Black or other race (non-Black, non-White) in a given school year. Note that while this measure is relatively intuitive for middle and high school students, it may be slightly less accurate for elementary school students. That is, while most elementary school students are assigned to a primary teacher in a self-contained class, many also see separate teachers for select subjects (often physical education, art or music, and specialized reading instruction). Although students may see their subject specialist teachers for, say, only 1 hour of 7 in the day, those teachers receive equal weight to the self-contained teacher in our main measure. We explore the sensitivity of our estimates to specifications that use simple indicators for whether the self-contained teacher is Black/other race versus White for elementary school students as well.

Our instrumental variables models use a predicted version of our race composition variables (fraction teachers Black and fraction teachers other race); these predictions are derived from first-stage equations using the instrument (i.e., the share of teachers of different races matched to students in a particular school grade in a given year) described briefly above. We generate the year-specific demographic composition of

teachers by calculating the share Black/other race among teachers who teach at least five students in a given grade–school-year combination. The five-student minimum is meant to address cases where teachers who, say, primarily teach eighth-grade math students have an aberrant (advanced) sixth grader in the class. As our fictional eighth-grade teacher is not the one who most sixth graders would reasonably expect to encounter, we exclude her from our instrument for the sixth-grade class.[3]

We use a host of student covariates to control for student-level factors that may be correlated both with disciplinary outcomes and with the likelihood of being matched to a same-race teacher. Time-invariant student demographic controls include dummies that capture student sex (male = 1). Time-variant standardized student controls include indicators for whether students are using subsidized (free or reduced-price) lunch, and whether students are classified as limited English proficient. In our multivariate, school-grade fixed effect, and instrumental variables models, we include a control for a 1-year-lagged discipline record. This control is excluded from our student fixed effects models as any time-invariant personal propensity to be subject to exclusionary discipline is captured in the student fixed effect, but robustness checks, as presented in Table 6, show that our results are very similar if this is included as a control.

We likewise control a number of school-level covariates that may be associated with both students' exposure to same-race teachers and disciplinary outcomes. Specifically, we include controls for school size (logged total enrollment); urbanicity of the school (urban, rural, or town vs. suburban); racial composition of the school (%Black, %Hispanic, %other race; %White is omitted); and share of students on free or reduced-price lunch.

We also include an indicator for whether a school is "persistently exclusionary"; the primary intent of this measure was to create a dimension along which to stratify schools for analyses of heterogeneity, so we constructed it to reflect a time-invariant assessment of whether a school consistently excluded high shares of students. To determine which schools are exclusionary, we first determined the share of students of all races who were excluded at least once in a given year, and coded middle and high (elementary) schools that excluded at least 25% (5%) of their students (of all races) during a given year as "highly exclusionary." To maintain a time-invariant measure of which schools were more punitive, we then characterized as "persistently exclusionary" those middle and high (elementary) schools that had excluded more than 25% (5%) of their students in at least 3 of the 6 years covered in our analytic sample. The thresholds of 25% exclusion for middle and high schools and 5% for elementary schools were at roughly the median of the distribution for Black students in 2012.

Table 2 shows descriptive statistics for Black students, broken down by grade levels. Although our grade-level divisions follow traditional grade configurations (1–5, 6–8, 9–12), roughly 25% of students in our sample attend schools with other grade configurations as well (e.g., K–8, 7–9); our results look similar if we restrict the sample to only students in schools with those traditional configurations. Black students have somewhat greater exposure to Black teachers in middle school grades than at elementary grades or high school grades; roughly 28% of middle school students' teachers were Black, compared with 19% and 23% at the elementary and high school levels, respectively.

Results

As a first cut at analysis, Table 3 shows the relationship between teacher demographic composition and student discipline using OLS models (Equation 0.1). We see heterogeneity in the coefficients across grade levels. While the coefficient for the "all-grade" sample—the sample including all students in Grades 1 through 12 (column 1)—is effectively null, suggesting that there is no relationship between teacher race composition and exposure to exclusionary discipline, this reflects the aggregation of two contrary effects. For elementary grade and middle school grade Black students (columns 3 and 5), exposure to Black teachers is negatively associated with discipline; that is, students exposed to a greater share of Black teachers are less likely to be suspended or expelled. However, for high school grade students, greater exposure to Black

TABLE 2

Descriptive Statistics for Black Students in Grades 1–12, 2012–2013

	1	2	3	4
	All grades	Elementary grades	Middle grades	High grades
	M (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)
Consequences: Student received				
Any exclusionary consequence	25.50	12.77	36.47	32.56
	(43.59)	(33.37)	(48.14)	(46.86)
Student's own teacher composition				
Fraction teachers White	0.74	0.78	0.68	0.73
	(0.27)	(0.28)	(0.27)	(0.26)
Fraction teachers Black	0.22	0.19	0.28	0.23
	(0.26)	(0.27)	(0.25)	(0.24)
Fraction teachers other race	0.04	0.03	0.04	0.04
	(0.10)	(0.11)	(0.08)	(0.09)
School-grade teacher composition				
Fraction White	0.74	0.79	0.69	0.73
	(0.22)	(0.21)	(0.23)	(0.20)
Fraction Black	0.22	0.18	0.27	0.22
	(0.20)	(0.20)	(0.21)	(0.18)
Fraction other race	0.04	0.03	0.04	0.05
	(0.06)	(0.08)	(0.06)	(0.05)
Student characteristics				
Male	50.73	50.85	50.87	50.46
	(49.99)	(49.99)	(49.99)	(50.00)
Free/reduced-price lunch	75.21	79.98	77.12	70.23
	(43.18)	(40.02)	(42.01)	(45.73)
Limited English proficient	0.63	0.74	0.59	0.59
	(7.93)	(8.55)	(7.69)	(7.64)
Grade	6.46	2.99	7.00	10.35
	(3.38)	(1.44)	(0.82)	(1.12)
School characteristics				
Charter school	3.07	3.89	3.72	1.53
	(17.25)	(19.33)	(18.93)	(12.28)
Magnet school	8.33	7.90	9.55	7.88
	(27.63)	(26.98)	(29.39)	(26.94)
School %FRL	63.44	69.63	63.97	55.27
	(21.77)	(22.68)	(20.67)	(18.61)
School %Black	43.85	44.21	43.67	43.54
	(22.62)	(23.58)	(21.91)	(21.96)
School %Hispanic	14.79	17.48	14.26	11.86
	(11.18)	(13.35)	(10.03)	(7.77)
School %other race	7.48	7.52	7.46	7.46
	(6.18)	(6.28)	(6.04)	(6.18)
School mean standardized achievement	−0.20	−0.21	−0.19	−0.19
	(0.42)	(0.39)	(0.40)	(1.04)

(continued)

TABLE 2 (CONTINUED)

	1	2	3	4
	All grades	Elementary grades	Middle grades	High grades
	M (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)
School size	854.09	579.95	762.78	1,270.86
	(504.34)	(200.65)	(299.48)	(616.91)
Pupil teacher ratio	15.56	15.09	15.82	15.94
	(2.54)	(2.10)	(2.52)	(2.93)
Persistently exclusionary school	48.53	56.07	46.01	41.16
	(49.98)	(49.63)	(49.84)	(49.21)
Urban school	39.86	40.77	39.60	38.95
	(48.96)	(49.14)	(48.91)	(48.76)
Rural school	36.18	33.96	37.70	37.73
	(48.05)	(47.36)	(48.46)	(48.47)
Town school	13.47	14.22	13.27	12.71
	(34.14)	(34.93)	(33.93)	(33.30)
Suburban school	10.48	11.05	9.43	10.61
	(30.63)	(31.35)	(29.22)	(30.80)
Unique students	361,160	147,509	94,982	118,669

Source. Authors' calculations from North Carolina Education Research Data Center (NCERDC) data.
Note. Any exclusionary consequence indicator includes suspensions or expulsions. Persistently exclusionary schools have high rates of exclusion for 3 or more years from 2008 to 2013. High exclusion rates are >25% for middle and high schools, and 5% for elementary schools. FRL = free/reduced-price lunch.

teachers is associated with significantly greater exposure to exclusionary discipline in the multivariate case (column 7).

To account for the possibility that unobserved school characteristics might be associated with both exposure to Black teachers and student discipline, we next estimate our models using school-grade fixed effects (Equation 0.2). When we compare students within the same school-grade cell, we see that students with greater exposure to Black teachers are more likely to receive exclusionary discipline than are their peers who take fewer classes with Black teachers, in both the middle school (Table 3, column 6) and the high school (Table 3, column 8) estimates, as well as in the all-grade estimates presented in column 2. While the elementary school estimates remain significant and negative, they attenuate toward 0 when school-grade fixed effects are introduced (column 4).

These estimates may be subject to bias, however, if exposure to Black teachers is endogenous within school-grade combinations. That is, if students are strategically matched to teachers so that students with a higher likelihood of receiving exclusionary discipline are more (or less) likely to be matched to Black instructors than are their peers with lower likelihood of disciplinary problems, our school-grade fixed effects estimates would be biased. To test for this, we run a set of regressions where next year's exposure to Black and other race teachers predicts current-year discipline rates (Table 3, Panel B); these coefficients should only be significant if we see nonrandom matching of students to teachers (Rothstein, 2010).

In fact, we do see evidence of sorting within school grade across all grade levels: Students who go on to have a higher share of Black teachers next year have greater rates of exclusionary discipline in the current year in our all-grade (column 2), elementary grade (column 4), middle school grade (column 6), and high school grade (column 8) samples. Indeed, the magnitude of

TABLE 3

Ordinary Least Squares and School-Grade Fixed Effects Estimates of Effects of Teacher Race Composition on Disciplinary Outcomes for Black Students: Any Exclusionary Discipline

	All (1–12) grades		Elementary (1–5) grades		Middle (6–8) grades		High (9–12) grades	
	1	2	3	4	5	6	7	8
	b (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)
Panel A. Coefficient on student's own teacher composition variables								
Fraction teachers Black	−0.000	0.015***	−0.024***	−0.014***	−0.019**	0.027***	0.031***	0.044***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.008)	(0.007)	(0.007)	(0.005)
Fraction teachers other race	−0.020***	−0.019***	−0.012**	−0.012**	−0.029	−0.014	−0.010	−0.023***
	(0.006)	(0.005)	(0.006)	(0.005)	(0.018)	(0.014)	(0.012)	(0.009)
Full controls	Y	Y	Y	Y	Y	Y	Y	Y
School-grade FE		Y		Y		Y		Y
Unique students	592,919	592,919	323,817	323,817	265,694	265,694	299,679	299,679
Unique schools	2,633	2,633	1,560	1,560	878	878	687	687
Student school years	2,236,678	2,236,388	904,290	904,126	567,814	567,733	764,574	764,529
Panel B. Selection on school-year fixed effects								
Next-year fraction teachers Black		0.056***		0.019***		0.071***		0.113***
		(0.002)		(0.002)		(0.006)		(0.006)

(continued)

TABLE 3 (CONTINUED)

	All (1–12) grades		Elementary (1–5) grades		Middle (6–8) grades		High (9–12) grades	
	1	2	3	4	5	6	7	8
	b (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)
Next-year fraction teachers other race		−0.025***		0.005		−0.042***		−0.055***
		(0.005)		(0.005)		(0.013)		(0.011)
Full controls		Y		Y		Y		Y
School-grade FE		Y		Y		Y		Y
Unique students		470,847		266,183		212,812		208,766
Unique schools		2,595		1,538		833		667
Student school years		1,521,322		684,574		424,014		412,734

Source. Authors' calculations from North Carolina Education Research Data Center (NCERDC) data. Coefficient (robust standard error clustered at school-grade level).
Note. Sample includes Black students enrolled in North Carolina public schools from 2007–2008 to 2012–2013. All models include year fixed effects, student controls, and school controls. Student controls include sex, subsidized lunch use, limited English proficiency indicators, and prior-year discipline indicators. School controls include logged enrollment; pupil–teacher ratio; urbanicity; share of students using subsidized lunch; share of students Black, Hispanic, and other (non-White) race; charter and magnet status; school average standardized achievement scores; indicators for whether the school is persistently exclusionary. Persistently exclusionary elementary (middle, high) schools exclude more than 5% (25%) of students for at least 3 years from 2008 to 2013. Missing dummy variables included for all variables, except teacher race composition. Teacher race composition variables include fraction of student's own teachers Black and fraction of student's own teachers other race (non-Black, non-White). Any exclusion indicates whether student received suspension or expulsion consequences at least once in school year. FE = fixed effects.
*$p < .10$. **$p < .05$. ***$p < .01$.

coefficients in this test for selection is larger than the magnitude of the coefficients estimating current-year relationships; in middle and high school grades, the coefficients are over twice as large in the test for selection (Panel B) as they are in the estimates for the present year (Panel A). This suggests that principals may strategically match teachers and students, such that Black teachers are disproportionately likely to teach students with established records of discipline problems; these selection effects may cause substantial bias in the school-grade fixed effects estimates.

This analysis provides strong evidence that more rigorous techniques are required to purge our estimates of bias. Following past researchers (Dee, 2005; Egalite et al., 2015), we next estimate the effect of higher exposure to Black teachers using student fixed effects, as in Equation 0.3. Using these more rigorous methods, we find that Black students have lower discipline rates when higher shares of their teachers are Black (Table 4). These results hold for the all-grade sample (column 1), and for the elementary (column 2), middle school (column 3), and high school grade (column 4) samples as well. The effects are relatively small in magnitude but precisely estimated. Point estimates for the different grade levels range from −0.016 for high school grade students to −0.028 for elementary grade students. This suggests that if Black students were to be exposed to all-Black teaching forces (i.e., *Fraction Teachers Black* = 1) compared with entirely non-Black teaching forces (*Fraction Teachers Black* = 0), they would be roughly 2 to 3 percentage points less likely to receive exclusionary discipline in a given year. However, this would entail a drastic change in the teaching force most students encounter; if instead Black students encountered a half-Black teaching force compared with the all-grade student average of 22% (i.e., an increase of 28 percentage points), Black students would see roughly a ½-percentage-point to 1-percentage-point decline in the likelihood of exclusionary discipline on average. For instance, using the coefficient for all students from Table 3, and assuming that the share of teachers Black fraction increased from 22% to 50%, we would expect a decrease in the likelihood of exclusionary discipline of −0.006: (−0.020 × [0.5 − 0.22] = −0.006). A 28-percentage-point increase in the share of teachers who are Black represents a roughly 1 standard deviation change in the rate of exposure to Black teachers for Black students (Table 2).

An alternative way to contextualize these results is with respect to the overall likelihood of the outcome. That is, while a 1-percentage-point decline in the likelihood of exclusionary discipline is fairly modest, only about 11% (35%, 32%) of elementary (middle, high school) aged students receive exclusionary discipline. A 1-percentage-point decline on a base of 11% to 35% of students receiving exclusionary discipline would therefore represent a decline of between 3% and 9% relative to the current prevalence of exposure to exclusionary discipline.

Alternative Specification: Instrumental Variables

Our student fixed effects estimates may be biased if students are dynamically matched to teachers, such that they are more (or less) likely to encounter Black teachers in years where they are, for reasons unrelated to teacher race, more (or less) likely to be exposed to discipline. This could happen if, for instance, a teacher who observes that a child is becoming somewhat more unruly in a fourth-grade class recommends that a principal assign the child to a same-race teacher in fifth grade. If such cases happen frequently, that would threaten the validity of our estimates. To test the sensitivity of our results to different econometric specifications, we turn to a second estimation technique not subject to the same threats to validity. Instrumental variables analysis allows us to carve out the variation in exposure to Black and other race teachers that is drawn from changes in the composition of the teacher workforce within a school grade over time, which should not be related to the discipline record or trajectory of any individual student.

First-stage results show that our instruments—the deviation of the fraction of teachers who are Black/other race versus White, compared with the historical average within grade-year cell—are strongly related to students' own exposure to Black and other race teachers. Students have significantly higher exposure to Black teachers in years where Black teachers

TABLE 4

Student Fixed Effects Estimates of Effects of Teacher Race Composition on Disciplinary Outcomes for Black Students: Any Exclusionary Discipline

	1	2	3	4
	All grades (1–12)	Elementary grades (1–5)	Middle grades (6–8)	High grades (9–12)
	b (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)
Fraction teachers Black	−0.020***	−0.028***	−0.017**	−0.016***
	(0.002)	(0.002)	(0.007)	(0.005)
Fraction teachers other race	−0.004	−0.010*	0.011	0.002
	(0.005)	(0.005)	(0.016)	(0.009)
Outcome mean	*0.241*	*0.108*	*0.352*	*0.315*
Unique students	480,829	237,419	179,998	213,920
Unique schools	11,641	6,911	21,46	2,474
Student school years	2,124,278	817,721	482,023	678,754
School-grade FE	Y	Y	Y	Y
Student FE	Y	Y	Y	Y

Source. Authors' calculations from North Carolina Education Research Data Center (NCERDC) data. Coefficient (robust standard errors clustered at school-grade levels).

Note. Sample includes Black students enrolled in North Carolina public schools from 2007–2008 to 2012–2013. All models include year- and school-grade fixed effects, student controls, and school controls. Student controls include sex, subsidized lunch use, and limited English proficiency indicators. School controls include logged enrollment; pupil–teacher ratio; urbanicity; share of students using subsidized lunch; share of students Black, Hispanic, and other (non-White) race; charter and magnet status; school average standardized achievement scores; indicators for whether the school is persistently exclusionary. Persistently exclusionary elementary (middle, high) schools exclude more than 5% (25%) of students for at least 3 years from 2008 to 2013. Missing dummy variables included for all variables, except teacher race composition. Teacher race composition variables include fraction of student's own teachers Black and fraction of student's own teachers other race (non-Black, non-White). Teacher race composition reflects all teachers matched to student. FE = fixed effects.

*$p < .10$. **$p < .05$. ***$p < .01$.

make up a greater share of the teaching force compared with school-grade historical averages (Table 5, Panel A, columns 1–4). The first-stage results for students' exposure to other race (non-Black, non-White) teachers are likewise highly related to the share of other race teachers available at the school-grade level for students in all grade ranges (Table 4, Panel B). Results of *F* tests suggest that both instruments are strongly relevant ($p < .001$) at all grade levels.

The reduced-form instrumental variable results reveal a strikingly similar story to the student fixed effects results. At all grade levels, coefficients are larger in magnitude but less precisely estimated than are the student fixed effects estimates. Point estimates for the different grade levels range from −0.031 for elementary grade students to −0.048 for high school grade students. This suggests that if Black students were to be exposed to all-Black teaching force compared with entirely non-Black teaching forces, they would be roughly 3 to 5 percentage points less likely to receive exclusionary discipline in a given year; if instead Black students encountered a half-Black teaching force compared with the all-grade student average of 22%, we would expect students to have just less than a 1-percentage-point decline in the likelihood of exclusionary discipline.[4]

Taken together, we see a strikingly similar pattern of results between the two estimation strategies: While the magnitude of the coefficients differs slightly, both estimation strategies suggest that exposure to a higher fraction of teachers who are Black reduces the likelihood of receiving exclusionary discipline for Black students at all grade levels. In the rest of the article, we present results from the student fixed effects

TABLE 5

Instrumental Variables Estimates of Effects of Teacher Race Composition on Disciplinary Outcomes for Black Students: Any Exclusionary Discipline

	1	2	3	4
	All grades (1–12)	Elementary grades (1–5)	Middle grades (6–8)	High grades (9–12)
	b (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)
Panel A. First-stage results: Predict student's own teachers-fraction Black				
Instrument: School-grade teacher composition				
Fraction: Black teachers	0.945***	0.952***	0.962***	0.880***
	(0.006)	(0.007)	(0.013)	(0.016)
Fraction: Other race teachers	0.034***	0.039***	0.060**	−0.010
	(0.010)	(0.011)	(0.025)	(0.031)
Panel B. First-stage results: Predict student's own teachers-fraction other race				
Instrument: School-grade teacher composition				
Fraction: Black teachers	−0.000	−0.004	0.003	0.011
	(0.002)	(0.003)	(0.005)	(0.007)
Fraction: Other race teachers	0.839***	0.845***	0.805***	0.838***
	(0.010)	(0.013)	(0.021)	(0.030)
Panel C. IV results				
Student: Fraction teachers Black	−0.033***	−0.031***	−0.037*	−0.048*
	(0.008)	(0.008)	(0.022)	(0.026)
Student: Fraction teachers other race	−0.010	−0.031**	0.036	0.053
	(0.016)	(0.014)	(0.060)	(0.048)
Outcome mean	*0.241*	*0.108*	*0.352*	*0.315*
F test on excluded instruments	12,059.113	772.688	1,905.683	3,644.989
School-grade FE	Y	Y	Y	Y
Unique students	592,919	323,817	265,694	299,679
Unique school grades	11,985	7,131	2,300	2,554
Unique schools	2,633	1,560	878	687
Student school years	2,236,388	904,126	567,733	764,529

Source. Authors' calculations from North Carolina Education Research Data Center (NCERDC) data. Coefficient (robust standard errors clustered at school-grade levels).

Note. Sample includes Black students enrolled in North Carolina public schools from 2007–2008 to 2012–2013. All models include year- and school-grade fixed effects, student controls, and school controls. Student controls include sex, subsidized lunch use, limited English Proficiency indicators, and prior-year discipline receipt indicators. School controls include logged enrollment; pupil–teacher ratio; urbanicity; share of students using subsidized lunch; share of students Black, Hispanic, and other (non-White) race; charter and magnet status; school average standardized achievement scores; indicators for whether the school is persistently exclusionary. Persistently exclusionary elementary (middle, high) schools exclude more than 5% (25%) of students for at least 3 years from 2008 to 2013. Missing dummy variables included for all variables, except teacher race composition. Teacher race composition variables include fraction of student's own teachers Black and fraction of student's own teachers other race (non-Black, non-White). Any exclusion indicates receipt of suspension or expulsion at least once in school year. FE = fixed effects.

*$p < .10$. **$p < .05$. ***$p < .01$.

strategy, which tend to be more conservative in magnitude but more precisely estimated, as our preferred set of estimates. However, we also include alternate versions of the same tables in Appendix A (available in the online version of the journal) that use instrumental variables

techniques to emphasize the consistency of the patterns regardless of estimation methods and to provide full transparency for the cases where our estimates are sensitive to estimation methods. In general, our results for the remainder of the article are similar regardless of which method is used, although the IV estimates are less often significant due to lower precision.

Robustness Checks

To test the robustness of the results to different model specifications, we re-estimate our models using a number of different techniques. Results are presented in Table 6; each cell represents the coefficient on the "Fraction Teachers Black" variable for the sample indicated in the column label, as the estimation techniques are changed according to the description in the row label. The first row gives baseline estimates from Table 4 to ease comparisons. A similar version of the table for the instrumental variables specification is given in Appendix Table A1 (available in the online version of the journal).

The second row uses a different version of the main predictor variable for the elementary grade students. Specifically, because most elementary school students spend most hours in the classroom of a single teacher heading a self-contained classroom, we substitute a set of main predictor variables that indicate whether each student's self-contained teacher is Black or other race (non-Black, non-White) versus White. The coefficient on this alternate predictor remains statistically significant. While the magnitude of the coefficient declines, it is important to contextualize this decline appropriately. That is, our main predictor, fraction teachers Black, is a continuous variable, and a one-unit increase in that predictor represents a drastic change in the teaching force encountered (from 0% Black to 100% Black); most students are unlikely to see such large changes in teaching forces from year to year as the variable reflects the races of multiple teachers (e.g., a self-contained teacher, a physical education teacher, an art teacher, and a reading specialist). However, our indicator for race of self-contained teacher is binary, and it is perfectly realistic to assume that a student who has a Black self-contained teacher in 1 year may have a White self-contained teacher the next. Our estimate of −0.018, then, suggests that students matched to Black self-contained teachers are nearly 2 percentage points less likely to receive exclusionary discipline than when they are matched to White self-contained teachers; this represents roughly a 17% decline compared with the base rate of exclusionary discipline receipt of 10.8% for our elementary grade sample.

The third row utilizes a new version of the dependent variable that includes detention as well as suspensions and expulsions in the "exclusionary discipline" category. In high school, in particular, detention is a commonly used discipline technique that regulates the presence of students in schools, although detention compels their presence rather than forbidding it. To the extent that schools substitute detention for suspensions, ignoring detentions may miss a major source of restrictive punishments. Results are very similar to the baseline specification.

The fourth row adds a control to our fixed effects models for whether students were exposed to exclusionary discipline in the prior year. Results are very similar to our main results, although the middle school estimates decline somewhat in magnitude and become only marginally significant ($p < .010$).

The fifth row includes controls for student exceptionalities. Specifically, it includes indicators for whether a student has a documented exceptionality or is classified as gifted for a given school year. These variables were excluded from our main specifications because these classifications may be endogenous to exposure to same-race teachers (Nicholson-Crotty, Grissom, Nicholson-Crotty, & Redding, 2016), but their inclusion results in virtually identical estimates.

The sixth row uses student-school fixed effects in place of the student fixed effects in our main specifications. This addresses the possibility that some portion of our fixed effects results may be driven by mobility by the same student across schools. These specifications therefore identify only off of changes in the racial composition of teachers that students are assigned to across years within the same schools, rather than by variation in the racial composition of teaching staff that students encounter due to school changes. The estimates are nearly identical to our main specifications.

TABLE 6

Robustness Checks for Student Fixed Effects Estimates of Fraction Teachers Black for Black Students: Any Exclusionary Discipline

	1	2	3	4
	All grades (1–12)	Elementary grades (1–5)	Middle grades (6–8)	High grades (9–12)
	b (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)
Baseline estimates	−0.020***	−0.028***	−0.017**	−0.016***
	(0.002)	(0.002)	(0.007)	(0.005)
Self-cont. teacher Black		−0.018***		
		(0.001)		
DV: Include detention	−0.021***	−0.028***	−0.017**	−0.019***
	(0.002)	(0.003)	(0.007)	(0.005)
Add controls: Lagged discipline	−0.019***	−0.025***	−0.011*	−0.014***
	(0.002)	(0.002)	(0.007)	(0.005)
Add controls: Exceptionality	−0.020***	−0.028***	−0.017**	−0.016***
	(0.002)	(0.002)	(0.007)	(0.005)
FE: Student-school fixed effects	−0.021***	−0.028***	−0.018**	−0.013***
	(0.002)	(0.003)	(0.008)	(0.005)
FE: School fixed effects	−0.021***	−0.029***	−0.022**	−0.016***
	(0.003)	(0.003)	(0.009)	(0.006)
Sample: Exclude 2008, 2009	−0.022***	−0.035***	−0.018**	−0.011*
	(0.003)	(0.003)	(0.008)	(0.005)
Sample: Non-Black students	−0.006***	−0.006***	−0.003	−0.004
	(0.001)	(0.001)	(0.005)	(0.004)
School-grade FE	X	X	X	X
Student FE	X	X	X	X

Source. Authors' calculations from North Carolina Education Research Data Center (NCERDC) data. Coefficient (robust standard errors clustered at school-grade levels).

Note. Sample includes Black students enrolled in North Carolina public schools from 2007–2008 to 2012–2013. All models include year- and school-grade fixed effects, student controls, and school controls. Student controls include sex, subsidized lunch use, and limited English proficiency indicators. School controls include logged enrollment; pupil–teacher ratio; urbanicity; share of students using subsidized lunch; share of students Black, Hispanic, and other (non-White) race; charter and magnet status; school average standardized achievement scores; indicators for whether the school is persistently exclusionary. Persistently exclusionary elementary (middle, high) schools exclude more than 5% (25%) of students for at least 3 years from 2008 to 2013. Missing dummy variables included for all variables, except teacher race composition. Teacher race composition variables include fraction of student's own teachers Black and fraction of student's own teachers other race (non-Black, non-White). FE = fixed effects.

*$p < .10$. **$p < .05$. ***$p < .01$.

The seventh row uses school fixed effects in place of the school-grade fixed effects in our main specifications.[5] To the extent that persistent differences in teacher race composition across grades within schools drive between-grade differences in exclusion, the use of school-grade fixed effects may purge our estimates of some meaningful sources of variation. Results are broadly similar to the baseline results.

A final set of robustness estimates checks whether the main results hold when different samples are used. The estimates in the eighth row exclude results from 2008 and 2009, the first year of the new reporting system that mandated reporting of all in-school suspensions (and other disciplinary consequences). Note that in Table 1, we see particularly low rates of in-school suspensions in those first 2

years, which may be due to incomplete compliance with reporting under the new rules. The results become slightly larger in magnitude for most of the grade levels, except at the high school level, but the basic pattern of results remains the same.

Note that all of the models in the first seven rows of Table 6 yield results that are substantively similar to the main results, although the magnitude of the coefficients fluctuates somewhat as model specifications change. In all specifications, we see that greater exposure to Black teachers is associated with lower rates of exclusionary discipline at all grade levels.

Finally, in the last row of Table 6, we estimate the effects of exposure to Black teachers for non-Black students. To the extent that Black students benefit from Black teachers through role model effects or cultural congruence, we might expect that non-Black students would not see the same benefits of exposure to Black teachers as would Black students. Indeed, the pattern of results here is markedly different from all other columns. While the point estimates are negative for non-Black students across all grade levels—significantly so for the all-grade and elementary grade samples—the estimates are much smaller in magnitude than the estimates for Black students. For instance, the all-grade student fixed effects estimates are less than a third as large ($b = -0.006$) for non-Black students as for Black students ($b = -0.020$). Thus, although there is modest evidence that exposure to Black teachers results in lower likelihood of discipline among non-Black students—as it does for Black students—these results suggest that the effect is weaker than for Black students and markedly less consistent across different grade ranges.

Effects by Incident Types

We next turned to determining which types of incidents were reduced by exposure to Black teachers. This question is of interest because it may help us determine the margins along which the reduction in exclusionary punishment occurs. Critics of school discipline systems have decried the race gaps in exclusion due to offenses such as "willful defiance," which may depend heavily on teacher interpretation of student behavior (Adams, 2015). Examining whether exposure to same-race teachers decreases such referrals speaks to that debate.

Because students may be flagged for multiple violations in a single incident and we assume that the most serious violation is the one that triggers punishment, we established a priority scheme to determine which category a particular incident would fall under. We assumed that certain types of offenses (e.g., violent offenses and drug offenses) would offer teachers little latitude around reporting, and therefore we prioritized those offense types. Thus, a student involved in an incident who was charged with both missing a class and fighting would be flagged for a "violent" incident in our coding scheme as fighting is categorized as a violent disruption. The priority order was (a) violent offenses, (b) drug offenses, (c) interpersonal offenses, (d) defiance-related offenses, (e) missed class offenses, and (f) other offenses. Violent offenses included charges such as fighting, assault, possession of weapons, or aggressive behavior. Drug offenses included charges such as possession of tobacco or alcohol. Interpersonal offenses included charges such as verbal harassment, communicating threats, or bullying. Defiance-related offenses included charges such as insubordination or disorderly conduct. Missed class offenses included charges such as truancy or being late to class. Other offenses included charges such as theft or dress code violations. A complete list of the types of offenses included in each category is given in Appendix B (available in the online version of the journal).

We then counted, for every student, the number of incidents they were involved in for a given year in which violent offense was the most serious offense, the number in which drug offenses were the most serious offenses, and so on. We also included a count of the total number of incidents a student was involved in. Results are reported in Table 7; the mean number of incidents per student is reported in square brackets below standard errors for each group.

Exposure to Black teachers was associated with a lesser number of total incidents for students in all grade combinations; results are significant across all grade-level samples. A 25-percentage-point increase in a student's share of teachers who are Black is associated with a

TABLE 7

Estimates of Fraction Teachers Black on Numbers of Disciplinary Incidents by Incident Type, Student Fixed Effects Estimates

	1	2	3	4
	All grades (1–12)	Elementary grades (1–5)	Middle grades (6–8)	High grades (9–12)
	b (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)
Dependent variable				
All incidents	−0.131***	−0.107***	−0.135***	−0.191***
	(0.015)	(0.012)	(0.047)	(0.032)
	[0.984]	*[0.416]*	*[1.411]*	*[1.338]*
Violent incidents	−0.017***	−0.034***	−0.013	−0.006
	(0.003)	(0.004)	(0.010)	(0.004)
	[0.146]	*[0.113]*	*[0.248]*	*[0.109]*
Drug incidents	−0.000	−0.000	0.000	−0.002
	(0.000)	(0.000)	(0.001)	(0.001)
	[0.007]	*[0.000]*	*[0.006]*	*[0.015]*
Interpersonal Incidents	−0.005***	−0.005***	−0.009***	−0.003**
	(0.001)	(0.001)	(0.003)	(0.001)
	[0.019]	*[0.013]*	*[0.035]*	*[0.014]*
Defiance incidents	−0.074***	−0.053***	−0.103***	−0.108***
	(0.009)	(0.007)	(0.032)	(0.020)
	[0.460]	*[0.157]*	*[0.719]*	*[0.625]*
Absence/tardy incidents	−0.008***	−0.000	0.000	−0.045***
	(0.003)	(0.000)	(0.006)	(0.009)
	[0.073]	*[0.001]*	*[0.045]*	*[0.178]*
Other incidents	−0.027***	−0.017***	−0.016	−0.028**
	(0.005)	(0.003)	(0.013)	(0.013)
	[0.200]	*[0.061]*	*[0.247]*	*[0.331]*
School-grade FE	Y	Y	Y	Y
Student FE	Y	Y	Y	Y
Unique students	592,919	323,817	265,694	299,679
Unique schools	2,633	1,560	878	687
Student school years	2,124,278	817,721	482,023	678,754

Note. Coefficient (robust standard errors clustered at school-grade levels; *Group Mean of Outcome*. All models include year- and school-grade fixed effects, student controls, and school controls. Student controls include sex, subsidized lunch use, and limited English proficiency indicators. School controls include logged enrollment; pupil–teacher ratio; urbanicity; share of students using subsidized lunch; share of students Black, Hispanic, and other (non-White) race; charter and magnet status; school average standardized achievement scores; indicators for whether the school is persistently exclusionary. Persistently exclusionary elementary (middle, high) schools exclude more than 5% (25%) of students for at least 3 years from 2008 to 2013. Missing dummy variables included for all variables, except teacher race composition variables. Teacher race composition variables include fraction of student's own teachers Black and fraction of student's own teachers other race (non-Black, non-White). Number of incidents refers to any incidents that resulted in any disciplinary consequences, including nonexclusionary consequences. Incident-type classification is given in Appendix B (available in the online version of the journal). FE = fixed effects.
*$p < .10$. **$p < .05$. ***$p < .01$.

decrease in disciplinary referrals ranging from 0.027 (for elementary grade students) to 0.048 (for high school students). While these reductions are relatively modest in magnitude, declines of this magnitude would represent a 4% decline in the number of referrals at the high school level

and a decline of 6% in the number of referrals at the elementary level.

Table 7 also shows that exposure to Black teachers resulted in a reduction in the number of violent incidents for which elementary aged students received referrals; reductions in involvement in violent incidents at the other grade levels are nonsignificant. This suggests that at the elementary school level, exposure to Black teachers likely reduces exposure to exclusionary discipline partially through inducing improvements in student behavior, as teachers likely have relatively little latitude in deciding not to report aggressive behavior. However, at the upper grades, we see less evidence of effects of exposure to same-race teachers on violent behavior. Likewise, across all grade levels, we see no effects on drug incidents, which presumably disallow any teacher discretion.

However, across all grade levels, there is more widespread evidence of a decline in incident types that involve more teacher discretion, including referrals for defiance-related incidents and interpersonal incidents. Exposure to a higher share of Black teachers was predictive of a decline in referrals for which the most serious type of offense was defiance or interpersonal across all grade levels. A 25-percentage-point increase in a student's share of teachers Black would be associated with a decrease in defiance-related referrals ranging from 0.013 incidents at the elementary school level to 0.027 at the high school level. Again, this represents a decline of 4% at the high school level and 8% at the elementary school level.[6] While the magnitude of the coefficients for interpersonal incidents is smaller than that for defiance-related incidents, the base rate of referrals for interpersonal incidents is also lower, and hence the proportion reduction in interpersonal referrals is roughly comparable with the reduction in defiance-related incidents. These types of incidents are all subject to teacher discretion.

Heterogeneity of Effects by Student and School Characteristics

We next tested for stability of our estimates across students and schools with different characteristics, returning to using the binary indicator for whether a student ever received exclusionary discipline as the dependent variable. With respect to student characteristics, we tested whether effects persisted for students of both sexes, and for more and less economically advantaged students. With respect to socioeconomic advantage, we split students according to whether they ever used free and reduced-price lunch. We used interaction models to constrain all coefficients except those related to teacher demographics to be identical across subgroups. Coefficients represent the estimates for fraction teachers Black for the group named in the row label, in the grade levels designated in the column header.

The results (Table 8) suggest that the benefits of exposure to same-race teachers accrue to Black students regardless of individual characteristics. That is, point estimates are negative across all grade spans for males and females alike, and for students who both used and who never used subsidized lunch. Results remained significant for most subgroups but were not statistically significant in the middle school sample for females.

We next sought to determine whether the pattern of our results held across different school types. Table 9 shows the results using the same analytic strategies as the previous table, with the subgroups now defined by whether schools are persistently exclusionary, by whether the student body was 45% or more Black, whether the school was a charter/magnet or a traditional public school, and school urbanicity. Estimates for the combined-grade and elementary samples are significant and negative across all types of schools. Results are slightly less consistent for middle and high school students when broken down by school characteristics. Point estimates for middle and high school grade students are nearly always negative; the one exception is that the estimates for charter/magnet middle school grade students are (nonsignificantly) positive.

There were some inconsistencies in terms of which groups saw the largest effects. For instance, among elementary aged students, effects were larger for subsidized-lunch-using students than for students who never used subsidized lunch, and the interaction term was statistically significant at $p < .05$. However, when all grades were combined, the never-subsidized lunch-using groups showed larger effects

TABLE 8

Heterogeneity in Estimates of Fraction Teachers Black by Student Characteristics, Student Fixed Effects Estimates

	1	2	3	4
	All grades (1–12)	Elementary grades (1–5)	Middle grades (6–8)	High grades (9–12)
	b (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)
Baseline estimates	−0.020***	−0.028***	−0.017**	−0.016***
	(0.002)	(0.002)	(0.007)	(0.005)
Student type				
Ever-FRL students	−0.018***	−0.030***	−0.016**	−0.016***
	(0.002)	(0.003)	(0.007)	(0.005)
	[0.273]	*[0.124]*	*[0.385]*	*[0.352]*
Never-FRL students	−0.029***	−0.012***	−0.025**	−0.017**
	(0.004)	(0.004)	(0.011)	(0.007)
	[0.138]	*[0.039]*	*[0.177]*	*[0.187]*
Male students	−0.019***	−0.031***	−0.022***	−0.021***
	(0.003)	(0.003)	(0.008)	(0.006)
	[0.300]	*[0.156]*	*[0.432]*	*[0.373]*
Female students	−0.020***	−0.024***	−0.012	−0.011**
	(0.003)	(0.003)	(0.008)	(0.005)
	[0.180]	*[0.058]*	*[0.268]*	*[0.256]*
School-grade FE	Y	Y	Y	Y
Student FE	Y	Y	Y	Y

Source. Authors' calculations from North Carolina Education Research Data Center (NCERDC) data. Coefficient (robust standard errors clustered at school-grade levels).

Note. Sample includes Black students enrolled in North Carolina public schools from 2007–2008 to 2012–2013. All models include year- and school-grade fixed effects, student controls, and school controls. Student controls include sex, subsidized lunch use, and limited English proficiency indicators. School controls include logged enrollment; pupil–teacher ratio; urbanicity; share of students using subsidized lunch; share of students Black, Hispanic, and other (non-White) race; charter and magnet status; school average standardized achievement scores; indicators for whether the school is persistently exclusionary. Persistently exclusionary elementary (middle, high) schools exclude more than 5% (25%) of students for at least 3 years from 2008 to 2013. Missing dummy variables included for all variables, except teacher race composition. Race composition variables include fraction of student's own teachers Black and fraction of student's own teachers other race (non-Black, non-White). Subgroups significantly differ at $p < .05$. FRL = free/reduced-price lunch; FE = fixed effects.

*$p < .10$. **$p < .05$. ***$p < .01$.

(p value for the interaction term < .05); this was largely driven by patterns at the middle school level. Likewise, while elementary school students in persistently exclusionary schools saw significantly larger effects of exposure to same-race teachers than did their peers in less-exclusionary schools (p value for the interaction term < .01), the opposite pattern held for high school aged students (albeit with only a marginally significant interaction term; p value < .10). Notwithstanding some differences in terms of which subgroups of Black students benefitted the most from exposure to same-race teachers, however, the main pattern of our results was markedly consistent across subtypes of students and schools.

Discussion

Our results highlight some interesting and consistent patterns with regard to teacher–student race congruence. In particular, we found that for Black students, exposure to a larger proportion of same-race teachers decreases the

TABLE 9

Heterogeneity in Estimates of Fraction Teachers Black by School Characteristics, Student Fixed Effects Estimates

	Student FE estimates			
	1	2	3	4
	All grades (1–12)	Elementary grades (1–5)	Middle grades (6–8)	High grades (9–12)
	b (*SE*)	*b* (*SE*)	*b* (*SE*)	*b* (*SE*)
Baseline estimates	−0.020***	−0.028***	−0.017**	−0.016***
	(0.002)	(0.002)	(0.007)	(0.005)
School type				
<45% Black	−0.015***	−0.015***	−0.024**	−0.023***
	(0.003)	(0.004)	(0.010)	(0.007)
	[0.233]	*[0.094]*	*[0.342]*	*[0.306]*
≥45% Black	−0.021***	−0.033***	−0.012	−0.012*
	(0.003)	(0.003)	(0.009)	(0.006)
	[0.249]	*[0.122]*	*[0.363]*	*[0.325]*
More exclusionary	−0.023***	−0.035***	−0.016	−0.008
	(0.003)	(0.003)	(0.010)	(0.007)
	[0.287]	*[0.144]*	*[0.424]*	*[0.398]*
Less exclusionary	−0.014***	−0.011***	−0.018**	−0.024***
	(0.003)	(0.003)	(0.009)	(0.006)
	[0.193]	*[0.057]*	*[0.284]*	*[0.250]*
Traditional public school	−0.020***	−0.028***	−0.023***	−0.016***
	(0.002)	(0.003)	(0.008)	(0.005)
	[0.245]	*[0.111]*	*[0.364]*	*[0.319]*
Charter/magnet	−0.017***	−0.029***	0.009	−0.017
	(0.005)	(0.006)	(0.016)	(0.011)
	[0.213]	*[0.090]*	*[0.296]*	*[0.285]*
Urban	−0.018***	−0.035***	−0.003	−0.011*
	(0.003)	(0.004)	(0.010)	(0.006)
	[0.231]	*[0.103]*	*[0.348]*	*[0.302]*
Nonurban	−0.021***	−0.021***	−0.027***	−0.022***
	(0.003)	(0.003)	(0.009)	(0.007)
	[0.247]	*[0.111]*	*[0.355]*	*[0.323]*
School-grade FE	Y	Y	Y	Y
Student FE	Y	Y	Y	Y

Source. Authors' calculations from North Carolina Education Research Data Center (NCERDC) data. Coefficient (robust standard errors clustered at school-grade levels).

Note. Sample includes Black students enrolled in North Carolina public schools from 2007–2008 to 2012–2013. All models include year- and school-grade fixed effects, student controls, and school controls. Student controls include sex, subsidized lunch use, and limited English proficiency indicators. School controls include logged enrollment; pupil–teacher ratio; urbanicity; share of students using subsidized lunch; share of students Black, Hispanic, and other (non-White) race; charter and magnet status; school average standardized achievement scores; indicators for whether the school is persistently exclusionary. Persistently exclusionary elementary (middle, high) schools exclude more than 5% (25%) of students for at least 3 years from 2008 to 2013. Missing dummy variables included for all variables, except teacher race composition. FE = fixed effects.

*$p < .10$. **$p < .05$. ***$p < .01$.

likelihood of receiving exclusionary discipline. Our results are relatively small in magnitude but are consistent across a variety of grade spans and are robust to various specifications of the models, including the use of student fixed effects and instrumental variables analyses. Results are consistent regardless of subsidized lunch status and for students of both genders. Our broad pattern of results holds across multiple school types, although estimates for middle and high school grade samples are not always significant.

Exposure to a large share of same-race teachers significantly reduces the number of reported incidents for Black students, particularly for types of offenses that required more subjective evaluation, suggesting that teacher discretion may play a role in these outcomes. Unlike the decision to suspend students, which requires action on the part of both a teacher (to refer the student to the office) and an administrator (to make the decision to suspend), office referrals depend solely on teacher discretion. Therefore, our results suggest that a teacher "gatekeeper effect" may be one mechanism through which we see these associations.

Our results should be interpreted with some limitations in mind. While our results present a clear picture that exposure to same-race teachers has particular benefits for Black children, the mechanisms for that result are not fully clear. We cannot fully distinguish whether Black teachers have better average classroom control (i.e., the results operate through Black teachers inducing the marginally misbehaving child to improve his or her behavior); whether Black students simply respond differently to Black teachers (i.e., children are inclined to behave better for Black teachers, even without any special effort on the part of the teacher); or whether Black teachers are more tolerant of mild misbehavior (i.e., Black teachers are marginally less inclined to discipline students harshly for the same level of misbehavior, or are less likely to misread behavior that the student did not intend to be defiant as misbehavior). In any of these cases, the student who avoids exclusionary discipline might benefit by avoiding the loss of learning time associated with exclusion and by avoiding the stigma of suspension or expulsion. However, the implications for spillover effects would differ. The first two mechanisms would suggest that, all else equal, exposure to Black teachers would produce a less disruptive classroom environment. By contrast, the latter mechanism would suggest either a comparable level of disruption (if student behavior is the same regardless of the teacher tolerance for misbehavior) or possibly greater disruption (if students misbehave more, given greater tolerance for misbehavior). As classroom disruptions are tied to lower levels of student achievement (Carrell & Hoekstra, 2010; Figlio, 2007), which mechanism predominates deserves future study to contextualize our results.

Our analyses could be read to suggest that multiple mechanisms may be at work. For instance, we see that exposure to same-race teachers significantly reduces the number of referrals for defiance-related incidents, a pattern that is consistent across all grade levels. Because the decision about which behaviors to classify as "defiance" is particularly subject to teacher interpretation, this suggests that same-race teachers may be less likely to make discretionary referrals for Black students compared with non-Black teachers. However, the reduction in referrals for violent incidents (which teachers should, by state school policy, have little discretion in reporting) at the elementary grade level may also suggest that Black students may have improved behavior as well when exposed to a greater share of Black teachers, at least for younger children. Evidence on this point is suggestive rather than conclusive, however.

Although the results are subject to some limitations, there are some important policy implications. Given the increasing diversity of the public school student population, teacher diversity may be considered important as a political goal. However, our results also show particular relationships for exposure to Black teachers that indicate that recruiting a diverse teaching force may modestly improve discipline outcomes for Black students. This conclusion is still more compelling because non-Black students saw null effects to small advantages from being matched to Black teachers, in terms of discipline outcomes, which suggests that improvements in the disciplinary climate for Black students will not result in off-setting negative effects for non-Black students. Note that while our results do raise the possibility of benefits from a more

diverse teacher workforce in terms of student discipline, gaining the full range of expected benefits from a more diverse workforce would depend on the classroom-management skills of newly recruited teachers being similar to those observed in our sample.

One read of our results might suggest that principals and superintendents should strategically group Black students (within or across schools) so as to maximize the exposure of Black students to Black teachers. We do not think that our results support this interpretation. Speaking specifically to cross-school sorting, for instance, our results show that at the middle and high school levels, the benefits of exposure to same-race teachers are significant at conventional levels only in schools with more modest concentrations of Black students (<45%), suggesting that benefits of exposure to same-race teachers for older children may be somewhat smaller in more segregated environments. While elementary students see greater benefits of exposure to same-race teachers in more segregated schools, we would also note that across all grade levels, the mean level of exposure to exclusionary discipline (given in brackets in Table 9) is higher in schools with higher concentrations of Black students than in less racially concentrated schools. This leads us to caution against interpretations that would lead to more racially segregated groupings of students, even if this did facilitate greater exposure to same-race teachers for Black students.

Finally, our results suggest that it may be useful to explore pedagogical and classroom-management strategies that Black teachers use. If they systematically employ distinct strategies that appear effective in managing classroom behavior, it may be useful to teach those strategies to other teachers struggling with classroom management. Future qualitative work may be especially useful to shed light on these mechanisms. Given the connection between exclusionary school discipline and later outcomes—both criminal justice and labor market—these relationships are worth further exploration.

Authors' Note

All errors are our own.

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Notes

1. However, the next three largest districts—Winston Salem/Forsyth, Cumberland, and Guilford—would all have been included in this analysis.

2. Our sample includes only students classified as Black in the North Carolina data; students classified as multiracial are excluded from main analyses, although some of these students may be Black. Only about 3.2% of students fall into the multiple race category.

3. Note that in our models, we do not observe exposure to other types of school personnel who may be responsible for student disciplinary referrals. For instance, students may receive disciplinary referrals from bus drivers, hall monitors, school administrators, and so on. Our estimation strategies identify solely off of the racial composition of teachers rather than these other staff members.

4. Using the coefficient for all students from Table 4, we would expect a decrease in the likelihood of exclusionary discipline of −0.009: (−0.033 × [0.5 −0.22] = −0.009).

5. These specifications cluster standard errors at the school level.

6. The proportion decline in referrals associated with a 25-percentage-point increase in share of teachers Black is actually smaller for middle school students than for high school students in terms of both the overall number of incidents (2.4% decline) and the number of defiance-related incidents (3.5% decline). Although the point estimates are larger in magnitude for middle grade students than for high school students, the base rate of disciplinary incidents is slightly lower.

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