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Do School Learning Opportunities Compound or Compensate for Background Inequalities? Evidence from the Case of Assignment to Effective Teachers

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Abstract

Are equal educational opportunities sufficient to narrow long-standing economic and racial inequalities in achievement? In this article, I test the hypothesis that poor and minority students benefit less from effective elementary school teachers than do their nonpoor and white peers, thus exacerbating inequalities. I use administrative data from public elementary schools in North Carolina to calculate value-added measures of teachers' success in promoting learning, and I assess benefits for different students. Results suggest that differential benefits of effective teachers uniquely exacerbate black–white inequalities but do not contribute to economic achievement gaps. Racial differences are small, on average, relative to the benefits for all groups; are not explained by differences in prior achievement; and are largest for low-achieving students. Teacher-related learning opportunities are crucial for all students, but these results point to a disconnect between typical school learning opportunities and low-achieving minority students.

Keywords

achievement gaps, opportunities to learn, racial and economic inequality, teacher effectiveness, value-added models, heterogeneous effects

Research and policy addressing educational equity often focus on ensuring equal access to learning opportunities in school, and social background disparities remain a particular concern (e.g., Coleman et al. 1966; Condron 2009; Reardon 2011). For instance, attention has long focused on the important, if complex, opportunities related to classroom teachers (e.g., Coleman et al. 1966; Covay Minor et al. 2015; Goldhaber, Lavery, and Theobald 2015; Lauen and Henry 2015). But differential *access* to valuable learning opportunities is just one potential source of inequality; differential *benefits* of these opportunities may also contribute to disparities. Because students come to school with different resources and dispositions, they may experience the same opportunities differently—and often to the detriment of poor and minority students. As a result, schooling may contribute to social background disparities even if learning opportunities are distributed evenly among all students.

Theories of social advantage and learning provide diverging expectations about how differential

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benefits of school learning opportunities influence inequality. On the one hand, opportunities may *compound* existing inequalities by being more beneficial for students from advantaged backgrounds. This pattern is predicted by a model of learning in which opportunities are most valuable for students with greater academic preparation (Sorensen and Hallinan 1977; Stanovich 1986). It is also consistent with the cultural capital perspective that school opportunities align best with the resources enjoyed by advantaged groups, leading to differential profits for school instruction (Calarco 2014a). On the other hand, opportunities may *compensate* for existing inequalities by being most beneficial for students from disadvantaged backgrounds. This view is consistent with the notion that school opportunities are, in part, substitutes for other resources that would lead, in the absence of schooling, to even larger achievement gaps (Downey, von Hippel, and Broh 2004; Raudenbush and Eschmann 2015).

Compounding or compensating effects of school highlight potential interactions between home and school resources for learning, which may explain distinct patterns of inequality along different dimensions of social background. For instance, seasonal learning comparisons suggest that schooling exacerbates early black–white achievement gaps more than economic ones (Downey et al. 2004), but differences in access to observed resources explain little of these differential trajectories (Condron 2009; Fryer and Levitt 2004). A plausible explanation for these results is that racial-minority students benefit the least from school learning opportunities, reflecting fundamental disparities in how students and families interact with school resources (Lareau and Horvat 1999; Roscigno and Ainsworth 1999). Similarly, benefits may be most different for students facing other challenges, such as low-achieving students.

In this article, I test for compounding or compensatory influences of schooling related to economic background and race-ethnicity for one of the most important educational resources that students experience in elementary school: assignment to a teacher who effectively promotes learning. Drawing on a large set of administrative records from North Carolina and value-added measures of teacher effectiveness, I test for differential benefits of assignment to a more effective teacher by economic disadvantage and among white, black, and Hispanic students. To better understand differential benefits, I then consider whether initial patterns persist net of prior achievement and whether effects are most different for struggling or successful students.

BACKGROUND

School Opportunities to Learn and Inequality

As students (and families) with different resources engage with the institution of formal schooling, the results can exacerbate or mitigate social inequalities. Many stratification accounts emphasize that formal education is a domain in which privileged social actors secure unique advantages, with the result of reproducing persistent social inequalities (e.g., Gamoran 1987; Lareau 1989; Lucas 2001). Conversely, others argue that common and compensatory influences of schooling equalize outcomes, especially relative to the stark developmental inequalities observed outside of school (e.g., Downey et al. 2004).

To isolate how schooling contributes to students' development and educational inequality, stratification and education research focuses on the concept of *opportunity to learn* in school. Opportunity to learn encompasses the "the presentation of a certain amount of material" (Sorensen 1989:6), which reflects the quantity of curricular material presented but also the quality of presentation and supportiveness of the social environment (Carroll 1963; Pianta et al. 2007; Wang 1998). Richer learning opportunities—more material, presented in a clear and engaging manner, and in a supportive classroom environment—enable greater student learning. However, they do not guarantee it; learning occurs as individual students interact with particular material, and therefore it also depends on the resources, such as academic preparation and effort, that students bring to the classroom (Carroll 1963; Sorensen and Hallinan 1977). Learning opportunities are difficult to isolate in practice, given the complexity of instruction in school. Yet researchers have emphasized that meaningful clusters of learning opportunities are structured by the organization of schooling (Sorensen 1970, 1989), such as differences between curricular tracks (e.g., Gamoran, Nystrand, and Berends 1995; Kilgore and Pendleton 1993).

In this article, I consider *teacher-related* learning opportunities, which reflect the constellation of relative learning opportunities that result from being assigned to a particular teacher for the school year. As the adult who fundamentally shapes the content, presentation, and social environment of instruction at school, a teacher has a formative influence on the quality of students' learning experiences. For instance, recent research on opportunities to learn highlights aspects related to specific teacher characteristics and styles of instruction (Covay Minor et al. 2015). In addition, a growing body of research points to more subtle differences among individual teachers, as reflected by substantial effects on students' development that are poorly captured by traditional proxies for teacher quality (Hanushek and Rivkin 2010; Jennings and DiPrete 2010; Nye, Konstantopoulos, and Hedges 2004; Rivkin, Hanushek, and Kain 2005). This suggests that assignment to an effective teacher is an important source of relatively rich, if complex, school learning opportunities.

It is important to note that teacher-related learning opportunities are not necessarily teacher caused; they also reflect the classroom and school resources at the teacher's disposal and other aspects of local context. Analogous to school effects (Jennings et al. 2015; Raudenbush and Willms 1995), the effects of assignment to a particular teacher likely reflect educational practices and conditions. These distinct influences may not be distinguishable in teacher effectiveness measures, and it is inappropriate to hold teachers accountable for factors outside their control (Raudenbush 2004). However, from a student's or parent's perspective, both practices and environment shape learning opportunities. Therefore, the total impact of assignment to a particular teacher reflects a substantial set of teacher-related learning opportunities. These opportunities have the potential to compound or compensate for social background inequalities.

We can distinguish two ways that school learning opportunities, including teacher-related ones, may contribute to inequality. First, some students may have less *access* to valuable learning opportunities at school. A long research tradition highlights how privileged students enjoy greater access to resources that support learning opportunities, such as school financial resources (Condron and Roscigno 2003), teachers with more experience (Kalogrides, Loeb, and Béteille 2013), and higher track placements (Lucas and Berends 2007). In this vein, recent research assesses economic and racial disparities in access to more effective teachers, showing some evidence of small inequalities; other studies, however, fail to find evidence of group differences, suggesting that teacher disparities are less pronounced than is often assumed (Goldhaber et al. 2015; Isenberg et al. 2016; Mansfield 2015). The data for this study also show no evidence of economic and racial disparities in access to more effective teachers (see Appendix Table A2).

Second, some students may experience fewer *benefits* from exposure to the opportunities that schools provide, as students with different resources and dispositions interact with learning opportunities in the classroom. If socially advantaged students experience greater benefits of school learning opportunities at school, for instance, then even equitable access would exacerbate existing inequalities, and richer school opportunities would merely magnify these differences (Sorensen and Hallinan 1977). In this article, I focus on how differential benefits may contribute to or mitigate inequality.

Teachers, Differential Benefits, and Inequality

In general, how might opportunities at school, especially those related to teacher assignments, benefit students from different social backgrounds? One hypothesis, often assumed in models of educational inequality, is that school opportunities have similar effects for different groups of students. Given the interactive nature of learning and differential resources students bring to the classroom, it is implausible that benefits are precisely the same for all students. Still, group differences may be inconsequential if variability is small or if the differential benefits occur primarily among individual students within social background groups rather than between groups.

A second hypothesis concerns compounding effects: students from privileged groups reap the greatest rewards from opportunities in school, compounding preexisting inequalities. Compounding effects are predicted by two distinct, although not mutually exclusive, mechanisms. One mechanism is differences in students' academic preparation, which may enable greater learning from similar learning opportunities. For instance, a student with a stronger vocabulary may understand instructional materials better and therefore learn more from a particular lesson.

This type of cumulative advantage process contributes to “rich-get-richer” dynamics in literacy development (Stanovich 1986) and human capital accumulation (Cunha and Heckman 2007), and it suggests that greater school opportunities fundamentally exacerbate social background inequalities (Sorensen and Hallinan 1977). However, this widening of social background differences is not due to social characteristics per se but merely the legacy of prior inequalities.

There is some empirical support for the notion that high-achieving students especially benefit from assignment to teachers who can provide rich learning opportunities. One example is experienced teachers, who likely provide richer instruction than novice teachers. Clotfelter, Ladd, and Vigdor (2006) present suggestive evidence that the benefits of teacher experience are higher in mathematics for fifth-grade students with above-average achievement; however, these estimates are not statistically significant and the pattern does not hold in reading. Another example is teachers placed by the Teach for America program, who may provide unique learning experiences compared with their peers in hard-to-staff schools. Applying distributional methods to experimental evaluation data in elementary grades, Penner (2016) finds these teachers have positive effects in mathematics throughout the entire achievement distribution. However, consistent with the compounding hypothesis, Teach for America teachers are especially beneficial in reading for students at the top of the distribution but detrimental for students at the bottom.

Another set of mechanisms for compounding influences of schooling concerns mismatches between students’ backgrounds and experiences and the school environment. Critical scholarship highlights ways in which schools, still overwhelmingly staffed by white educators, fail to meet the needs of children of color (e.g., Irvine 1990; Ladson-Billings 2006; Valenzuela 1999). Racial differences in judgments of students’ ability (Ready and Wright 2011), perceptions of behavior (Irizarry 2015; Morris 2005), and expectations for success (McKown and Weinstein 2008) all reflect a climate that potentially mutes black and Hispanic students’ ability to take advantage of ostensibly similar school learning opportunities (Lewis and Diamond 2015). These problems are likely exacerbated by the demographic distance between students of color and the typical white teacher (Dee 2004; Downey and Pribesh 2004; Egalite, Kisida, and Winters 2015), but they are more fundamental and complex than the composition of the teaching force alone. As social actors, all teachers are influenced by social stereotypes, and even unconscious biases influence expectations and students’ experiences of classroom learning opportunities (Bergh et al. 2010). In turn, the quality of relationships between students and teachers moderates how much students gain from instruction (Crosnoe et al. 2010).

The cultural capital literature provides a complementary perspective on how the resources that students and families bring to school may lead to differential success in interacting with educators and school institutions (Lareau and Horvat 1999; Lareau and Weininger 2003). For instance, middle-class parents are better able to customize their children’s school experiences (Horvat, Weininger, and Lareau 2003), and parents’ interventions in school may complement learning opportunities, making them especially effective for advantaged students. Cultural capital also informs how students respond to opportunities in school. Gaddis (2013) traces measures of cultural capital to habitus, that is, dispositions toward schooling, that explain benefits for school performance. Calarco’s (2014a, 2014b) work documents how middle-class students’ help-seeking allows them to take greater advantage of similar lessons and instruction; differences in students’ taken-for-granted “logics of action” in the classroom lead to differential classroom experiences, especially as students navigate ambiguous interactions with teachers. Middle-class students may thus enjoy different profits of school opportunities that compound inequality.

A third, competing hypothesis about differential effects is that opportunities are most beneficial to disadvantaged students, therefore *compensating* for inequality. An example is greater benefits of a high-reading group in early elementary school for black and Hispanic students (Tach and Farkas 2006). Due to differences in students’ experiences outside of school, less advantaged students may be more sensitive to the quality of school opportunities. This could occur in two ways. First, many learning opportunities in schools, especially those relating to basic skills, may overlap with the skills advantaged children are likely to develop in the absence of school (Engel, Claessens, and Finch 2013). If these opportunities substitute for experiences among advantaged families, then school opportunities may matter the most for disadvantaged students. A second potential source of compensating influences of schooling is parental responses to school opportunities. For instance, middle-class parents are more likely than working-class families to intervene in school and provide supplemental educational services, such as tutoring (Horvat et al. 2003; Robinson and Harris 2014). To the extent that this effort takes the form of responding to challenging situations at school (Calarco 2014a), then relatively poor school opportunities may be less detrimental to more advantaged students. If so, greater opportunities would be more beneficial to disadvantaged students, and therefore compensatory.

In short, theories provide competing and potentially crosscutting hypotheses about how differential benefits of school learning opportunities may compound or compensate social background achievement inequalities. The patterns may also be different for distinct dimensions of social background. Seasonal learning comparisons, for instance, suggest that compounding effects of school opportunities may be strongest for racial disparities and less relevant for class disparities (Downey and Condron 2016). These possibilities highlight the importance of testing how specific school opportunities compound or compensate for specific social disparities.

Despite the potential for differential teacher benefits, there is relatively little evidence about whether more effective teachers are most beneficial for students from different social backgrounds. In one of the most direct assessments to date, Konstantopoulos (2009) tests for differential teacher effects in the Tennessee STAR class size experiment, where randomized classroom assignments enable unbiased estimates of teacher effectiveness. The results suggest compounding effects (greater benefits for white and nonpoor students), but estimates are imprecise and few are statistically distinguishable from zero. Clotfelter and colleagues (2006) find a similar pattern—larger benefits for nonpoor students and students whose parents have more education—for assignment to an experienced teacher among fifth-grade students in North Carolina. Studies also report evidence that teacher effects are highly correlated across different types of students (e.g., Condie, Lefgren, and Sims 2014; Fox 2015); this work suggests that teachers' relative ranking is similar across groups, but these studies do not directly assess the magnitude of differential benefits that students experience. Existing evidence suggests that differences in teacher effects are likely to be small relative to teachers' overall effects, but prior work is ambiguous about the direction and precise size of consequential compounding or compensatory influences.

RESEARCH QUESTIONS

The primary research question I address in this article is, do the benefits of assignment to a more effective teacher differ for students from different racial and economic backgrounds? The compounding hypothesis predicts that assignment to a more effective teacher is most beneficial for white and nonpoor students. Conversely, the compensatory hypothesis predicts that the benefits are greatest for black, Hispanic, and poor students.

I also ask two supplemental questions related to the influence of students' prior academic preparation and individual teacher differences. First, do teacher benefits differ by students' economic and racial background, controlling for differences in prior achievement? Theories that locate differential effects of learning opportunities in students' prior preparation suggest there are no differences independent of prior achievement, whereas theories that view school practices as fundamentally shaped by race and class hierarchies predict differential benefits, even for students with similar scores. Second, are racial and economic differences in teacher benefits larger among low- or high-achieving students? An intersectional hypothesis suggests that social background differences are most pronounced among students facing the greatest academic challenges. The benefits of school opportunities for this underserved group (low-achieving poor and minority students) have important implications for how schooling contributes to educational stratification and how improving opportunities is likely to shape inequality.

Throughout, I consider whether the answers to these questions differ for economic background and race-ethnicity, as these different dimensions of social background may be associated with distinct processes of stratification as students interact with teachers and other school personnel.

METHOD

Data

To calculate estimates of teacher effectiveness and assess differential benefits, I use longitudinal student information covering 1.5 million student-year observations from administrative records of third-, fourth-, and fifth-grade students in North Carolina public schools between 2006 and 2013, collected from the Department of Public Instruction and prepared for research use by the North Carolina Education Research Data Center (NCERDC). These data include yearly student achievement scores on standardized tests, which allow me to characterize teachers' effectiveness in promoting learning for their students over time. This indicator of teacher-related learning opportunities, available for a large number of students, allows new, precise tests of hypotheses of compounding or compensatory school influences.

I focus on this time period because course enrollment information, not available in previous years, allows for linking students and teachers to individual classrooms. Research using earlier data from North Carolina used the teacher who administered yearly assessments as the basis for student–teacher links, an imperfect proxy that, at best, precluded considering a substantial number of cases (in recent data, the proctor and classroom teacher do not match for approximately a quarter of cases). Classroom linkages based on the new enrollment records match closely with independent records of class sizes from school activity reports (correlation above .9 in each year) and with the subset of cases that NCERDC could link unambiguously to teacher records via recorded names (match rate of 98 percent).

Sample

The analytic sample consists of students linked to a single teacher in a focal academic course who is observed with at least 10 students in other years. The latter requirement ensures an indication of a teacher's effectiveness is available independent of the focal year. This eliminates students who cannot be linked to a teacher, students in a course taught by multiple teachers, and the small number of teachers observed in only one year. The resulting samples for mathematics and reading comprise over three quarters of the public school students in the state (for descriptive statistics, see Appendix Table A1).

My analyses focus on the two socially important background dimensions theorized to be related to benefits of school learning opportunities: economically disadvantaged students (relative to not economically disadvantaged students) and black and Hispanic students (relative to white students).

Measures

Achievement.

The dependent variable for all analyses is mathematics or reading achievement, as measured by students' performance on North Carolina's End-of-Grade tests.[1] These standardized tests assess learning in content standards specified by the state, reflecting an important domain of the academic learning opportunities that schools and teachers are expected to provide. Scores are approximately normally distributed and show no evidence of ceiling or floor effects. To facilitate interpretation and comparison, I standardize achievement scores to have a mean of 0 and a standard deviation of 1 in the full population in each year and each grade.

Social background.

Measures of social background are based on administrative records. The economic background variable is based on participation in the National School Lunch Program, a standard proxy for economic disadvantage determined by family income less than 185 percent of the federal poverty line, or receipt of means-tested transfers, such as food stamps. For expositional simplicity, I use the labels *poor* and *nonpoor* for eligible and noneligible students, respectively. About half of all students are economically disadvantaged by this criteria. Racial-ethnic group membership is collected by schools in accordance with state and federal reporting requirements. I focus on non-Hispanic white (55 percent of the sample), black/African American (25 percent), and Hispanic (11 percent) students.

Teacher effectiveness.

To measure differences among teachers in the relative opportunities to learn that students experience, I calculate value-added estimates of teacher effectiveness for teachers in grades 3, 4, and 5.[2] This approach (for overviews, see Koedel, Mihaly, and Rockoff 2015; McCaffrey et al. 2003) uses longitudinal information to draw conclusions about the effect of being assigned to a specific teacher as opposed to an average teacher, a counterfactual difference that summarizes a wide range of potential differences between classrooms.[3] These inferences would be straightforward if all teachers were assigned comparable students, for example, if students were randomly assigned to schools and classrooms. Because this is not the case, value-added models control for observed student and school characteristics to isolate teacher-related influences from differences among students. I use standard value-added models, drawing on evidence that this approach identifies substantive differences in teacher effectiveness (described below) and research demonstrating the utility of value-added methods for understanding schooling inequalities (Jennings et al. 2015). I then conduct sensitivity analyses to assess whether possible biases in these measures alter the results.

I estimate teacher effectiveness with a value-added model where i indexes students, j indexes teachers, and t indexes year:

$$Y_{ijt} = \beta_0 + f\left(Y_{i,t-1}\right) + \boldsymbol{\beta} X_{it} + \mu_j + \varepsilon_{it}. \quad (1)$$

This equation models mathematics or reading achievement at the end of the school year (Y_{ijt}) as a function of several observable characteristics: prior mathematics achievement ($f[Y_{i,t-1}]$, which I specify as a third-order polynomial); a vector of observable student, classroom, and school characteristics (X_{it}: prior achievement in alternative subject, gender, eligibility for free/reduced lunch, race-ethnicity, limited-English-proficiency designation, disability designation, migrant status, gifted designation in mathematics or reading, an indicator for grade retention, cubic polynomials of classroom, school-level mean prior achievement in reading and mathematics, and grade and year fixed effects); and a teacher effect (μ_j).

I estimate the parameters of the model treating teacher effects as fixed, based on evidence of the appropriateness of this "dynamic ordinary least squares" approach under several plausible underlying data-generating processes (Guarino, Reckase, and Wooldridge 2015). The estimated fixed effects ($\hat{\mu}_j$) provide measures of teacher effectiveness for subsequent analyses. To mitigate the unreliability of estimates based on a single year, all teacher effectiveness estimates are calculated separately for each year, based on a "leave-out" sample of students from all other years. Mean estimated reliability is .91 for the sample of math teachers and .78 for the sample of reading teachers (reflecting smaller teacher effects on reading outcomes).[4]

In the metric of student achievement for these samples, estimated teacher effectiveness has a standard deviation of .20 in mathematics and .14 in reading, which are consistent with estimates of teacher effects in other contexts (Hanushek and Rivkin 2010). For interpretability, I standardize all effectiveness measures to have a mean of 0 and a standard deviation of 1 among all teachers. Regression estimates thus correspond to the estimated effects of a standard-deviation change in observed teacher effectiveness.

Validity and alternative effectiveness specifications. Are value-added teacher effectiveness measures valid reflections of relative differences in the quality of learning opportunities students experience in school? The systematic sorting of students to classrooms and schools might lead some types of teachers to appear more effective than they are. Advantaged families tend to secure access to schools and classrooms with teachers with more experience and better credentials (Clotfelter, Ladd, and Vigdor 2005; Lankford, Loeb, and Wyckoff 2002), and teacher labor-market and organizational mechanisms also contribute to these disparities (Boyd et al. 2010; Kalogrides et al. 2013). Thus, due to teaching particular students, some teachers may seem more effective, which could distort comparisons across groups.

The teacher effectiveness measures I use here draw on a large and growing literature on these methods; this work provides evidence on validity and identifies potential weaknesses (for a recent review, see Everson 2017). Evidence for validity comes from several complementary approaches. First, the magnitude of variability of observational teacher effectiveness corresponds with experimental and quasiexperimental estimates (Hanushek and Rivkin 2010; Nye et al. 2004; Rivkin et al. 2005). Second, these measures are predictive of effects in studies when teachers have been randomly assigned to classrooms (Kane et al. 2013; Kane and Staiger 2008).[5] Third, quasiexperimental evidence from teacher transfers in large-scale administrative data suggests minimal bias (Bacher-Hicks, Kane, and Staiger 2014; Chetty, Friedman, and Rockoff 2014). Finally, value-added estimates are robust to alternative covariate specifications, including adding detailed family income information not typically available, which suggests these potentially important characteristics do not confound estimates (Chetty et al. 2014). Although prior research does not definitively rule out teacher value-added biases, a recent review concluded that "most of the reviewed literature appears to suggest that in many cases differences in classrooms due to sorting do not appear to introduce significant bias in effect estimates, especially when several years of data are included in the analyses"; less work, however, directly assesses between-school sorting (Everson 2017:51).[6]

These measures reveal some meaningful information about teacher influences, but it is important to consider how possible biases may influence the current research design. To assess the potential influences of measurement on the substantive conclusions, I investigate how results change when different covariates are included in the teacher effectiveness model (for an example, see Isenberg et al. 2013). By leaving out likely important control variables (the individual and aggregate characteristics in X_{it} in Equation 1), these results provide an indication of how omitted characteristics may influence conclusions about differential effects.

A more specific concern about measurement is that individual teachers might provide different kinds of learning opportunities to different types of students, leading overall effectiveness to be an imperfect proxy for students' experiences.[7] Evidence on this possibility is mixed: student–teacher match effects (based on race or gender) suggest some differences exist (Dee 2004; Gershenson, Holt, and Papageorge 2016), but teacher effectiveness measures show little heterogeneity by student characteristics (Condie et al. 2014; Fox 2015). Even small differences, however, could influence conclusions about the current research questions. To consider how such heterogeneity (including but not limited to teacher characteristics) might affect the results, I relax the assumption of homogeneous teacher effects by calculating subgroup-specific effectiveness estimates by economic disadvantage and for black and white students.

Analyses

To test hypotheses about differential benefits of school opportunities, I estimate the effects of being assigned to a more effective teacher and interactions with social background. The basic model for the achievement of student i assigned to teacher j at the end of year t is the following:[8]

$$Y_{ijt} = \beta_1(TE_{ijt}) + \beta_2(BG_{it}) + \beta_3(TE_{ijt})(BG_{it}) + \beta_4(Y_{i,t-1}) + \beta \boldsymbol{X}_{it-1} + \varepsilon_{it} \quad (2)$$

In Equation 2, TE_{ijt} represents the estimated effectiveness of a student's teacher ($\hat{\mu}_j$ from Equation 1, calculated from data in years other than t); BG_{it} represents an indicator for the student's social background group; $Y_{i,t-1}$ is the lagged dependent variable; $\boldsymbol{X}_{it-1}$ reflects a vector of student covariates measured in the prior year (prior achievement in the other subject, the nonfocal social background [economic disadvantage or race-ethnicity], gender, limited English proficiency, academically gifted designation, grade retention, classroom aggregate demographics and prior achievement, and grade-by-year fixed effects); and ε_{it} is a normally distributed disturbance term. In this model, β_1 reflects the benefits of a more effective teacher for the reference group (nonpoor or white students), and β_2 reflects the expected conditional achievement gap between groups when teacher effectiveness is average. The key parameter is β_3, which provides a test of differential benefits. The compensatory hypothesis predicts $\beta_3 > 0$, whereas the compounding hypothesis predicts $\beta_3 < 0$.

To assess differences net of academic preparation, I allow for an interaction between prior achievement and teacher effectiveness in the model:

$$Y_{ijt} = \beta_1^*(TE_{ijt}) + \beta_2^*(BG_{it}) + \beta_3^*(TE_{ijt})(BG_{it}) + \beta_4^*(Y_{i,t-1}) + \beta_5^*(TE_{ijt})(Y_{i,t-1}) + \beta \boldsymbol{X}_{it-1} + \varepsilon_{it} \quad (3)$$

In Equation 3, β_5^* represents how the benefits of assignment to an effective teacher differ for students with different academic preparation: a positive value signifies that teacher effectiveness is most beneficial to students who arrive with the greatest knowledge. My theoretical interest is whether this inclusion changes the interaction between effectiveness and social background (β_3^*) as compared to β_3 in Equation 2. If prior preparation explains teacher benefit differences, then β_3^* should be smaller (in absolute value) than β_3 and statistically different; if prior preparation is the sole explanation, then β_3^* should be statistically indistinguishable from 0.

Finally, to test for differential social background differences by academic preparation, I include a three-way interaction term:

$$Y_{ijt} = \beta_1(TE_{ijt}) + \beta_2(BG_{it}) + \beta_3(TE_{ijt})(BG_{it}) + \beta_4(Y_{i,t-1}) + \beta_5(TE_{ijt})(Y_{i,t-1}) + \beta_6(BG_{it})(Y_{i,t-1}) + \beta_7(TE_{ijt})(BG_{it})(Y_{i,t-1}) + \beta \boldsymbol{X_{it}}_{-1} + \varepsilon_{it} \quad (4)$$

Here β_7 reflects how differences in teacher benefits by social background vary by initial achievement level. Positive values of β_7 support the intersectional hypothesis that differential teacher effects are most responsible for compounding inequality among low-achieving students.

Across all models, I adjust standard errors for the clustering of observations within schools. In addition to reporting differences in the metric of standardized achievement, I present interaction estimates as percentages of the main effect for the reference group (non–economically disadvantaged students or white students), based on the ratio $\hat{\beta}_3/\hat{\beta}_2$, with standard errors accounting for uncertainty in both estimates.

Alternative model specifications. Because students and teachers are not assigned to classrooms at random, these observational models identify the parameters of interest only if they successfully account for confounding influences on both teacher assignments and student achievement. Although this cannot be tested directly, I implement several alternative analyses to probe the plausibility of this assumption and the robustness of the substantive results.

One critical issue is the sufficiency of observed characteristics, and I assess sensitivity to alternative sets of covariates in Equation 2. As described earlier, specifications that omit demographic or aggregate classroom characteristics help gauge how these controls change conclusions relative to simpler models with only the primary prior achievement covariates. One particular weakness of the standard covariates is that they do not directly measure educators' assessments of students, which likely inform classroom placement. Therefore, in some specifications, I add teacher-reported judgments from the prior year about students' academic proficiency and expected grade in mathematics and reading (this information was collected to calibrate cut points in the standardized tests). I also include interactions between teacher effectiveness and classroom characteristics to assess whether compositional differences account for differential teacher effects.

A second approach to assessing potential bias focuses on subsamples of schools where classroom assignments are balanced in terms of observable characteristics; in such schools, student sorting may be less systematic and identifying assumptions more plausible (see Clotfelter et al. 2006). I define "balanced-assignment" schools as those in which prior achievement (and in separate tests, economic background and race-ethnicity) is not a jointly significant predictor of classroom assignments across all year–grade combinations in the data. Because student sorting is less pronounced in these schools, different results from these schools may indicate potential problems related to sorting overall.

Finally, for all specifications, I consider robustness to the inclusion of school fixed effects, which focuses on variation within schools. This may exclude some meaningful between-school variation in teacher effectiveness, but it likely removes spurious differences related to student sorting.

RESULTS

Differences in Access to Effective Teachers

Before addressing the primary research questions, I document disparities in access to effective teachers in this sample by calculating differences in the mean teacher effectiveness of teachers assigned to different groups (full estimates are in Appendix Table A2). Consistent with recent research (Goldhaber et al. 2015; Isenberg et al. 2016; Mansfield 2015), the mean differences tend to be small or not detectable. In mathematics, poor students experience teachers with lower effectiveness by .04 standard deviations; in reading, the disparity is .03.[9] However, I find no evidence of disparities for black or Hispanic students relative to white students: the differences are not statistically significant in mathematics and show small advantages for both minority groups in reading (of .04 and .06 standard deviations). As in previous research in other settings, these small differences counter common assumptions about access to high-quality teachers, at least with respect to effectiveness in promoting student learning.

Conclusions about teacher effectiveness disparities are sensitive to the covariates included in the value-added model, however (see also Isenberg et al. 2013). In specifications that do not

Table 1. Estimates of Interactions between Teacher Effectiveness, Economic Background, and Prior Achievement as Predictors of Mathematics and Reading Achievement.

	Mathematics			Reading		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Teacher effectiveness (TE)	.130*	.132*	.135*	.057*	.059*	.061*
	(.002)	(.002)	(.002)	(.001)	(.001)	(.002)
EDS	−.064*	−.064*	−.065*	−.078*	−.078*	−.079*
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
EDS × TE	.001	−.002	−.002	−.001	−.005*	−.006*
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
TE × Prior achievement[a]		−.005*	−.015*		−.005*	−.011*
		(.001)	(.001)		(.001)	(.001)
EDS × TE × Prior achievement			.019*			.010*
			(.002)			(.002)
Prior achievement	.663*	.663*	.666*	.608*	.608*	.602*
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
EDS × Prior achievement			−.004*			.012*
			(.002)			(.002)

Note: EDS = economically disadvantaged student. The outcome variable is achievement, standardized to have a mean of 0 and a standard deviation of 1 within grade and year. Standard errors, displayed in parentheses, are adjusted for clustering within schools. All models also control for student characteristics measured in the prior year (gender, race-ethnicity, economic disadvantage, limited English proficiency, gifted designation, retained, achievement in the alternative subject), classroom aggregate characteristics (proportion nonwhite, proportion economically disadvantaged, and mean prior achievement in mathematics and reading), and grade-by-year fixed effects.
[a]Prior achievement reflects same-subject standardized test score in the previous year.
*$p < .05$.

account for differences in the composition of classroom and school characteristics, I find clear disparities for poor, black, and Hispanic students. These gaps are larger still when individual covariates (other than prior achievement) are omitted, ranging from .1 to .3 standard deviations. In other words, disparities appear to be larger for effectiveness estimates that are less likely to isolate teachers' influence on learning. This may reflect selection effects that work in favor of teachers of more advantaged students, highlighting the importance of assessing how selection may influence our understanding of differential benefits.

Do Teacher Benefits Differ by Social Background?

Turning to tests of the interactional hypotheses, key estimates from basic models of teacher effects on student achievement are presented in Models 1 (mathematics) and 4 (reading) of Table 1 (for economic background) and Table 2 (for race-ethnicity) and are summarized in Figure 1. Not surprisingly, given previous evidence of the validity of these measures, assignment to a more effective teacher predicts a substantial increase in learning, corresponding to .13 of a standard deviation in mathematics achievement at the end of the year and .06 in reading.

Turning to differential benefits of a more effective teacher, patterns are strikingly different between economic and racial-ethnic background. I find no evidence that poor students see a larger or smaller benefit of assignment to a more effective teacher. Interactions in both subjects are statistically indistinguishable from zero and precisely estimated. The results are thus not consistent with either the compounding or compensatory hypothesis, at least for this measure of economic background. The story is different by race-ethnicity, however. For black students, teacher benefits are significantly smaller than for white students in both mathematics and reading, providing consistent support for the compounding hypothesis. The same is true for Hispanic students in reading but not in mathematics, where there are no discernable differences. The reading interaction may reflect language

Table 2. Estimates of Interactions between Teacher Effectiveness, Racial Background, and Prior Achievement as Predictors of Mathematics and Reading Achievement.

	Mathematics			Reading		
Variable	(1)	(2)	(3)	(4)	(5)	(6)
Teacher effectiveness (TE)	.134*	.136*	.137*	.059*	.061*	.062*
	(.002)	(.002)	(.002)	(.001)	(.001)	(.002)
Black	−.068*	−.068*	−.071*	−.068*	−.068*	−.068*
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Black × TE	−.008*	−.013*	−.009*	−.005*	−.010*	−.009*
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Black × Prior achievement[a]			−.007*			.002
			(.002)			(.002)
Black × TE × Prior achievement			.018*			.009*
			(.002)			(.002)
Hispanic	.053*	.053*	.053*	.003	.003	.004
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
Hispanic × TE	−.002	−.005*	−.006*	−.008*	−.013*	−.011*
	(.002)	(.003)	(.002)	(.003)	(.003)	(.003)
Hispanic × Prior achievement			−.000			.007*
			(.002)			(.003)
Hispanic × TE × Prior achievement			.010*			.010*
			(.002)			(.002)
Mathematics prior achievement	.663*	.663*	.665*	.608*	.608*	.607*
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Teacher effectiveness × Prior achievement		−.006*	−.012*		−.006*	−.010*
		(.001)	(.001)		(.001)	(.001)

Note: The outcome variable is achievement, standardized to have a mean of 0 and a standard deviation of 1 within grade and year. Standard errors, displayed in parentheses, are adjusted for clustering within schools. All models also control for student characteristics measured in the prior year (gender, race-ethnicity, economic disadvantage, limited English proficiency, gifted designation, retained, achievement in the alternative subject), classroom aggregate characteristics (proportion nonwhite, proportion economically disadvantaged, and mean prior achievement in mathematics and reading), and grade-by-year fixed effects.
[a]Prior achievement reflects same-subject standardized test score in the previous year.
*$p < .05$.

barriers at school that mute the benefits of relative learning opportunities (despite controlling for both limited English proficiency and prior reading achievement), but it may also be spurious given the sensitivity checks presented below.[10]

In short, social background differences in the apparent benefits of assignment to an effective teacher are small relative to the benefits for all types of students. But, these interactions point to diverging effects of this source of school learning opportunities. I find little evidence of differential benefits by economic background and mixed results for Hispanic students (little difference in mathematics; smaller benefits in reading), and the benefits of more effective teachers tend to compound black–white inequalities.

What Is the Substantive Magnitude of These Effects?

Are these differences substantively meaningful? I offer three ways to gauge the magnitude of the estimated interactions. Perhaps the most natural metric is the size of interactions relative to the estimated benefits of teacher effectiveness for the reference group; estimates of these proportions are presented in Figure 1. For instance, the estimates suggest that the effects of assignment to a more effective teacher are 6.3 percent lower for black students than for white students in mathematics (the teacher effectiveness slope is .937 as steep) and 8.5 percent lower in reading. These disparities reflect modest but meaningful differences.

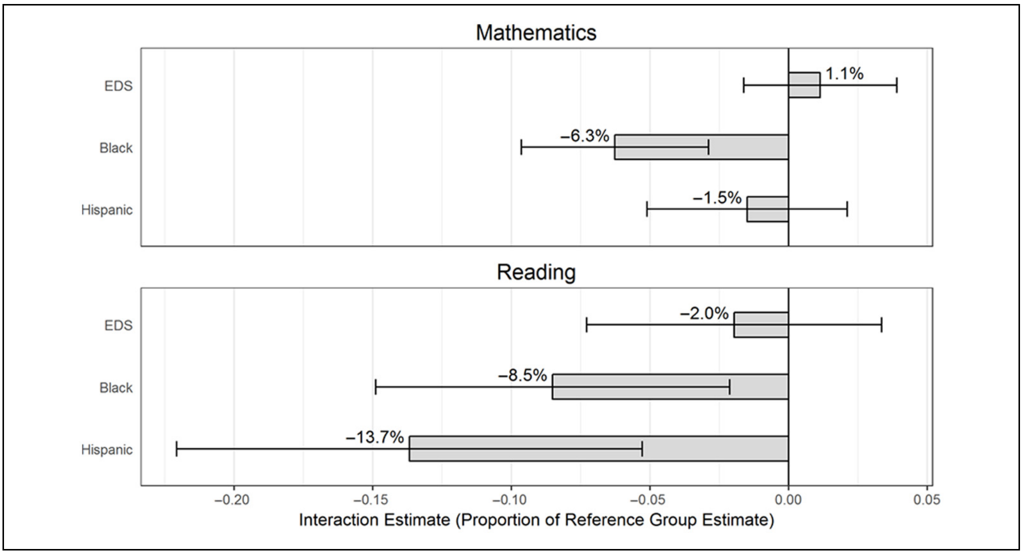

Figure 1. Estimated interactions between social background and assignment to a more effective teacher on achievement.

Note: EDS = economically disadvantaged student. Estimates based on Models 1 and 4 reported in Tables 1 and 2. Percentages report the size of the estimated interaction between teacher effectiveness and background as a share of the reference group (non–economically disadvantaged students or white) teacher effectiveness effect. Error bars show the 95% confidence intervals for each ratio accounting for uncertainty in the numerator and denominator.

If patterns for teacher-related learning opportunities generalize to all school learning opportunities, these proportional differences should extrapolate to overall school influences. A relevant comparison is thus differential school year learning, adjusting for summer trajectories. For instance, Downey and colleagues (2004: Table 3, “Contrasts” panel) report lower school learning by approximately 5 percent for socioeconomic status (.12/2.33 in mathematics; .06/1.03 in reading) and 15 percent lower for black students (.34/2.33 and .18/1.03), with inconsistent differences for Hispanic students. In comparison, the teacher effectiveness estimates here are less detrimental to black students and poor students.[11]

Second, we can ask how differential benefits contribute to existing achievement gaps and how this compares to differences in exposure to effective teachers. Based on the model parameters in Tables 1 and 2 and the mean teacher value added for each group, we can calculate groups’ expected achievement if teachers were allocated equally or if the benefits of a more effective teacher were the same across groups (see online Supplemental Materials for details of the implementation). These calculations (Table 3) show that differential teacher effects explain 5 to 10 percent of black–white achievement gaps conditional on prior performance, meaning up to a 10th of the group disparity in learning experienced in a single year. Not surprisingly, these differences represent a much smaller portion of the larger raw achievement gaps that exist between groups (.5 to 2.5 percent). However, these calculations also demonstrate that racial differences in exposure to more effective teachers play an even smaller role than inequitable access, typically explaining less than 2 percent of the conditional achievement gap.[12]

A third way to gauge the size of the estimates is to examine how achievement gaps would be expected to change with plausible changes in teacher effectiveness in a classroom or school. Consider proposals to replace teachers via targeted recruitment and retention policies. If it were possible to replace the bottom 5 percent of teachers in terms of effectiveness with average teachers, we would expect an increase in mean effectiveness of roughly .1 standard deviations. Model estimates suggest this would increase the conditional black–white achievement gap by approximately .5 percent (for mathematics, $.1 \times .008/.147$), with similar magnitude for the Hispanic–white reading

Table 3. Implied Contributions to Achievement Gaps.

	Mathematics			Reading		
Variable	EDS	Black	Hispanic	EDS	Black	Hispanic
Raw achievement difference	0.731	0.792	0.536	−0.763	−0.760	−0.717
Conditional achievement difference	0.150	0.147	0.067	−0.183	−0.179	−0.132
Contribution to gaps						
Differential exposure to TE	0.006	−0.001	0.003	0.002	−0.003	−0.002
% Raw gap	3.8	0.1	0.6	0.3	−0.4	−0.3
% Conditional gap	0.8	0.7	4.8	1.2	−1.7	−1.6
Differential TE slope	−0.003	0.0170	0.0040	0.002	0.0102	0.016
% Raw gap	−0.4	2.1	0.7	0.3	1.3	2.3
% Conditional gap	−2.0	11.5	5.9	1.2	5.7	12.4
Differential TE exposure and slope	−0.003	0.0159	0.0073	0.0037	0.0069	0.0139
% Raw gap	−0.4	2.1	0.7	0.3	1.3	1.6
% Conditional gap	−2.0	11.5	5.9	1.2	5.7	12.4

Note: EDS = economically disadvantaged student; TE = teacher effectiveness. For achievement differences, the reference group for economically disadvantaged students is non–economically disadvantaged students, and the reference group for Black and Hispanic students is white students. Achievement differences are in standardized units (standard deviation of 1 within grades and years). The conditional achievement difference is the gap controlling for achievement in the prior year. See online Supplementary Materials for details of the calculations of the contribution to differences.

gap. As an upper bound, consider the local consequences of a more intensive policy, such as targeted transfer incentives to fill positions with top-quintile teachers as opposed to average ones (e.g., Glazerman et al. 2013). If such a policy resulted in a standard-deviation increase in effectiveness for students in affected classrooms, the current results predict the black–white conditional achievement gap would increase by 5 percent among these students (for mathematics, 1 × .008/.147).

Are Background Interaction Estimates Robust to Alternative Specifications?

A fundamental potential threat to the conclusions of these observational analyses is that they do not fully account for students' selection to different teachers. To assess whether such selection effects drive results via either the measurement of effectiveness or model specification, I consider several alternative specifications for the main interaction results.

With respect to alternative covariate specifications of the value-added measures, results are uniformly robust (see Appendix Table A3 and Figure A1). The same is not true for the disparities in access to effective teachers reported earlier, which suggest larger disparities in the models that are less likely to isolate teachers' influence on student learning. The relative robustness of the differential-benefits estimates suggest that systematic student sorting is less problematic for these conclusions than for those about differential access.

To assess whether results depend on the homogeneous measurement assumption in Equation 1 (and most value-added models), I implement subgroup-specific value-added measures among the subsample of teachers observed with at least 10 poor and 10 nonpoor students or 10 black and 10 white students (see characteristics in Table 1).[13] The pattern of results is similar within this unique subsample and with subgroup-specific effectiveness measures (see Appendix Table A2 and Figure A1). The main difference is that the tendency of teacher effectiveness to compound black–white inequalities is more pronounced (15 to 25 percent of the white estimate), suggesting that the main results may be conservative. These subgroup-specific differences bolster the main conclusion that differential effects of school learning opportunities tend to compound racial inequalities but do not alter economic inequalities. At the same time, they highlight the need to better understand how race shapes teaching and local classroom experiences.

I also consider several alternative specifications of the outcome model (results presented in Appendix Table A4 and Figure A2). The pattern of results is not substantially or systematically different when focusing on within-school variation by including school fixed effects. Similarly, results are robust to excluding covariates from the model (classroom aggregates or nonachievement individual variables) and to including additional controls for prior teachers' judgments of students' academic proficiency level. Results are also similar when classroom characteristics (mean achievement or demographics) are allowed to interact with teacher effectiveness. Estimates for black–white differences are 15 percent smaller, which suggests that the types of students assigned to teachers explain a portion of, but do not account for, differential benefits. Although it is impossible to gauge the influence of unobserved variables in the observational model, the similarity of the key results across each specification provides some evidence of their robustness.

In a complementary test of the influences of systematic student sorting, I focus on a subsample of schools in which classroom assignments are consistently balanced with respect to student characteristics, and I calculate teacher effectiveness based only on these schools. Balanced schools are defined as those for which mean prior achievement, student race-ethnicity, and economic disadvantage do not differ significantly between classrooms across all grade–year cells. Within each school, I regress prior achievement (or social background) on grade–year indicators and individual classroom indicators; I then test the joint significance of the classroom indicators.[14] Approximately one eighth of students in the analytic sample are in schools that meet the criteria for prior achievement and demographic characteristics (see Appendix Table A1).

Estimates are less precise in these subsamples, but for most groups and subjects, the results within the balanced schools do not alter the overall conclusions. Within the balanced-assignment subsample, I found no statistically significant differences related to economic background, but the point estimate in mathematics is positive and large (7.0 percent larger than the nonpoor effect). This provides some suggestive evidence of compensatory teacher effects for poor students. Black student interaction effects remain negative in mathematics (−6.4 and −4.7 percent, depending on whether teacher effectiveness is estimated only within the subsample, versus −6.3 percent) and are even more pronounced in reading (−12.8 and −13.2 percent, versus −8.5 percent). It is not clear if these differences in magnitude are the result of less biased estimates, a meaningful difference in this particular subsample, or chance alone. Nonetheless, all alternative estimates support the basic original conclusion of compounding effects of black–white inequality. In contrast, the patterns for Hispanic students are quite different in the balanced-assignment sample. The point estimates are greater, suggesting Hispanic students in these schools experience advantages in mathematics and no differences in reading. The reasons for the differences in this subsample are again unclear, but these specification tests suggest the need for caution in interpreting the suggestive compounding effects in reading for Hispanic students.[15]

In summary, the sensitivity of results across alternative specifications supports the following conclusions: I do not find strong evidence for differential benefits related to economic background, but there is a possible compensatory effect in mathematics; the lower benefits of teacher effectiveness for black students are robust to alternative specifications, providing no evidence that conclusions are the result of systematic sorting; and results for Hispanic students are inconsistent across specifications, failing to support the initial suggestive evidence for compounding effects in reading.

Does Academic Preparation Explain Background Differences?

To test whether differential effects by academic preparation explain group differences, Models 2 and 5 in Tables 1 and 2 allow for interactions between a student's prior achievement and the effectiveness of the teacher to whom the student was assigned. These interactions suggest that students with lower prior preparation benefit most from assignment to a more effective teacher. These differences are small compared with the main teacher effects—a more effective teacher is more beneficial for all students—but they suggest overall compensatory benefits of teacher-related learning opportunities. This could reflect the importance of higher-quality school resources for students who arrive at school with relatively poor academic preparation.

Accounting for prior achievement leads to lower estimated interactions in all cases, suggesting that achievement differences mask some of the tendency of teacher-related learning opportunities to compound background disparities. For economic background, the mathematics interaction switches sign but remains negligible.[16] In reading, the conditional interaction is significant and negative, suggesting benefits are 8 percent (−.005/.059) lower for poor students than for nonpoor students.

Racial differences in teacher benefits are even more pronounced net of prior achievement interactions. In mathematics, the expected benefit of a more effective teacher is 10 percent (−.013/.136) lower for black students than for white students, and it is 16 percent (−.010/.061) lower in reading. Hispanic benefits are also 4 percent (−.005/.136) lower in mathematics and 21 percent (−.013/.061) lower in reading. Differential preparation therefore seems to mask some of the unique advantages that white students experience from teacher-related learning opportunities.

In short, in elementary school, more effective teachers seem to be compensatory with respect to prior achievement. These differences do not explain differential teacher benefits by social background, but this dynamic tends to mask differences in the benefits of assignment to a more effective teacher, especially to the detriment of students of color. These school opportunities seem to uniquely complement white students' nonacademic learning resources, which is consistent with a cultural-mismatch perspective on schooling.

Are Social Background Differences More Pronounced for Struggling Students?

The above analyses suggest that black (and possibly Hispanic) students tend to experience smaller benefits of assignment to more effective teachers (compared with white students), whereas lower-achieving students experience somewhat stronger benefits (relative to high-achievers). But these analyses ignore the potential for these two dimensions of educational inequality to amplify one another. An intersectional perspective highlights the possibility that learning opportunities may most compound inequality for struggling minority students.

Three-way interaction models (Models 3 and 6 in Tables 1 and 2, summarized in Figure 2) show differences in estimated teacher benefits related to both social background dimensions and prior achievement. Negative estimates suggest that differential teacher benefits compound both economic and racial differences among low-achieving students, with little difference among high-achieving students. To interpret these estimates, Figure 2 represents predicted effects of assignment to a more effective teacher for students one standard deviation above and below the mean. In mathematics, poor, black, and Hispanic students' benefits are 10 to 18 percent lower (relative to nonpoor or white students) among low achievers and 3 to 14 percent higher among high achievers. In reading, poor, black, and Hispanic low-achieving students have disadvantages of 20 to 30 percent; for their high-achieving peers, the disadvantage is not statistically distinguishable from zero. These differences are driven by low-achieving white and nonpoor students receiving especially large benefits from effective teachers, which compounds social inequalities among struggling students.

Regardless of social background, high-achieving students experience similar benefits from more effective teachers. Teacher-related learning opportunities seem to help struggling nonpoor and white students catch up, but struggling black, Hispanic, and poor students are left behind. These results suggest that struggling nonpoor and white students have a variety of home advantages that complement school opportunities, whereas struggling poor and minority students face unique barriers in trying to connect with these opportunities.

In short, the compounding hypothesis appears most applicable for students facing dual dimensions—social background and academic preparation—of disadvantage in school. These students' smaller benefits from learning opportunities suggests a form of intersectional inequality in how students experience school.

DISCUSSION

A long-standing hypothesis in research on schooling and inequality holds that greater opportunities to learn necessarily compound existing inequalities (Sorensen and Hallinan 1977). I find mixed support for this hypothesis for the important case of learning opportunities associated with

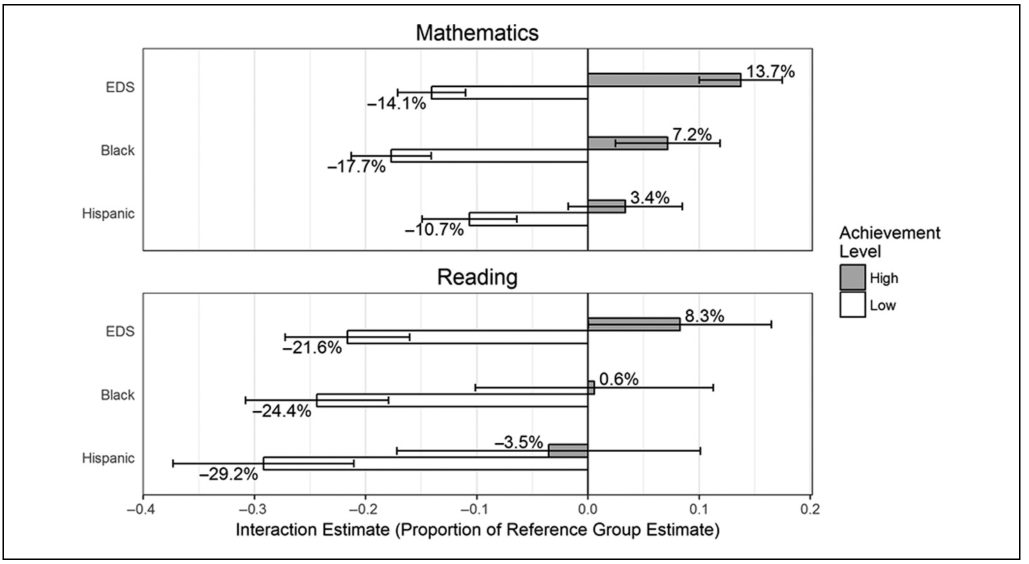

Figure 2. Estimated interactions between social background and assignment to a more effective teacher on achievement of high- and low-achieving students.

Note: EDS = economically disadvantaged student. High (low) achievement = 1 standard deviation above (below) the mean. Estimates based on Models 3 and 6 in Tables 1 and 2. Percentages report the size of the estimated interaction between teacher effectiveness and background as a share of the reference group (non–economically disadvantaged students or white) teacher effectiveness effect. Error bars show the 95% confidence intervals for each ratio accounting for uncertainty in the numerator and denominator.

assignment to a more effective elementary school teacher. I did not find differences related to economic background, at least as indicated by eligibility for free or reduced-price lunch. Black students, however, tend to experience smaller benefits from assignment to a more effective teacher, meaning that teacher-related learning opportunities compound racial disparities, especially among academically struggling students. Because these racial differences are not explained by differences in prior achievement, they seem to reflect unique barriers faced by minority students in accessing the full advantages of school opportunities. School inequalities cannot be viewed merely in terms of access to valuable resources.

These results have the most direct implications for proposals to promote school equity by improving teacher quality, which is increasingly defined in terms of improving student achievement. The current findings certainly justify focusing policy attention on teachers; like previous research, I find that assignment to more effective teachers is associated with greater learning for all student groups, on average. However, the findings also show that equalizing access to high-quality teachers may do less than previously imagined to narrow racial achievement gaps. Teachers who provide high-quality learning opportunities do not necessarily meet the needs of all students equally, and this does not seem to be explained by differences in prior academic preparation. Policy makers must also consider how to promote culturally responsive pedagogy that enables success for socially marginalized groups (Ladson-Billings 1995). More generally, the compounding influences of effective teachers suggest that merely providing equal school opportunities, without addressing potential differential benefits, will exacerbate some social background inequalities.

It is important to note several limitations of the current approach. Although teachers are an instructive case as an important source of learning opportunities that students experience in school, they are only one specific source. These patterns echo some broader patterns, but additional work is required to assess whether these findings generalize to other aspects of learning opportunities. In particular, the focus on teachers cannot speak to the influence of resources common to all classrooms, such as curricular standards, or differences in opportunities that students experience within the classroom. Furthermore, these analyses consider only opportunities related to achievement as measured on standardized tests, yet many important schooling opportunities are in other domains. For instance, it remains to be seen whether similar interactions hold for teachers' effects on social and behavioral skills. Moreover, this large-scale research design does not reveal any of the nuanced mechanisms by which students experience assignment to a more effective teacher differently, beyond demonstrating that black–white differences are not a function of preexisting achievement. Rather, these results should be seen as a complement to qualitative work showing how home advantages affect school learning. In that context, these results direct our attention toward the unique racial dimensions of these differences.

Despite these limitations, the findings demonstrate the importance of considering the potentially compounding or compensating effects of learning opportunities at school. Two patterns, in particular, provide intriguing contributions to our understanding of how schools contribute to educational inequality. First, the findings highlight differences in school influences on economic and racial inequalities. Downey and Condron (2016) recently proposed the metaphor of "refraction," emphasizing that different social disparities may be altered in distinct ways as students pass through school. Racial differences in the benefits of learning opportunities, differences not reflected between poor and nonpoor students, offer a specific example. Because these differences persist when controlling for prior academic preparation, they seem to reflect disconnects between minority students' needs and the opportunities schools typically provide. Such compounding effects help explain why school experiences may do more to lessen class disparities than racial gaps (Downey et al. 2004).

A second notable pattern in these results is the tendency for teacher benefits to most compound racial and economic inequalities among low-achieving students. The troubling implication is that low-achieving students from socially disadvantaged groups are not only at risk of exposure to lower-quality learning opportunities, but they also derive fewer benefits from school opportunities. Viewed through an intersectionality lens, academically struggling poor and minority students find themselves in a particularly precarious position. These students not only struggle, but compared with other students, they benefit the least from teachers with a record of classroom success. Unfortunately, these analyses do not reveal exactly why this occurs or conversely, what enables low-achieving nonpoor and White students to see relatively large benefits of richer school opportunities. But the descriptive results highlight the importance of improving school experiences for students facing overlapping challenges.

Overall, the case of school learning opportunities related to teachers suggests differential effects by social background are small relative to main effects. Access to high-quality learning opportunities remains the first-order concern for educational equity. But the differential benefits observed highlight that equal opportunities in school can exacerbate some background inequalities, and this may contribute to gaps in learning throughout the year. Therefore, in addition to monitoring access to learning opportunities, we must also devote attention to how these opportunities shape inequality.

APPENDIX

Table A1. Characteristics for Main Analytic and Alternative Samples.

		Mathematics				Reading			
Variable	All	Main	EN	BW	BA	Main	EN	BW	BA
Students	1,981,463	1,572,936	1,290,820	820,743	254,460	1,537,673	1,256,127	794,971	216,375
Proportion of all students	1	0.794	0.651	0.414	0.128	0.776	0.634	0.401	0.109
Schools	1,976	1,580	1,543	1,315	299	1,559	1,522	1,295	274
Districts	219	210	192	164	99	209	192	167	107
Female	0.490	0.494	0.494	0.494	0.493	0.494	0.494	0.495	0.494
American Indian	0.015	0.014	0.014	0.011	0.013	0.014	0.014	0.011	0.013
Asian	0.025	0.024	0.023	0.025	0.028	0.025	0.023	0.025	0.028
Black	0.266	0.256	0.237	0.302	0.281	0.256	0.237	0.300	0.273
Hispanic	0.122	0.122	0.116	0.123	0.122	0.122	0.116	0.123	0.119
White	0.532	0.545	0.570	0.496	0.518	0.544	0.571	0.496	0.529
Multiracial	0.040	0.039	0.040	0.043	0.038	0.039	0.040	0.044	0.038
Economically disadvantaged	0.516	0.518	0.510	0.525	0.549	0.516	0.507	0.522	0.545
Mathematics achievement	0.000	0.025	0.037	0.004	0.019	0.027	0.038	0.005	0.006
Standard deviation	1.000	0.991	0.978	0.991	0.998	0.991	0.977	0.991	0.990
Reading achievement	0.000	0.018	0.031	0.000	−0.004	0.023	0.037	0.007	−0.005
Standard deviation	1.000	0.993	0.979	0.989	0.998	0.992	0.977	0.987	0.991
Teachers		17,868	12,259	7,034	3,435	17,958	12,274	7,005	3,042
Years of experience		11.3	11.6	11.6	11.4	11.3	11.6	11.7	11.2
Standard deviation		8.6	8.5	8.5	8.8	8.7	8.6	8.7	8.7
At least two years experience		0.857	0.876	0.879	0.853	0.857	0.875	0.879	0.845
Mean effectiveness		0.011	0.026	0.025	0.064	0.002	0.003	0.019	0.038
Standard deviation		0.981	0.953	0.938	0.980	0.962	0.911	0.880	0.983
Mean reliability		0.910	0.925	0.934	0.908	0.813	0.837	0.856	0.806
Standard Deviation		0.070	0.057	0.049	0.070	0.123	0.107	0.094	0.125

Note: All = all students; Main = analytic sample reported in text; EN = subsample of teachers with at least 10 poor and nonpoor students; BW = subsample of teachers with at least 10 black and white students; BA = "balanced-assignment" schools in which prior achievement, economic background, and race/ethnicity are not predictive of classroom placement within grade–year cells.

Table A2. Teacher Effectiveness Gaps by Economic and Racial Background for Alternative Measures of Teacher Effectiveness.

	Mathematics			Reading		
Teacher effectiveness specification	EDS	Black	Hispanic	EDS	Black	Hispanic
All controls	−.042*	.007	−.025	−.025*	.056*	.042*
	(.010)	(.014)	(.015)	(.009)	(.013)	(.012)
Individual controls only	−.100*	−.058*	−.051*	−.174*	−.100*	−.113*
	(.011)	(.015)	(.015)	(.011)	(.015)	(.014)
Prior achievement only	−.180*	−.167*	−.104*	−.308*	−.282*	−.276*
	(.012)	(.015)	(.015)	(.013)	(.016)	(.017)

Note: EDS = economically disadvantaged students. Estimates reflect difference in teacher effectiveness for students from different groups. The reference group for economically disadvantaged students is non–economically disadvantaged students; the reference group for black and Hispanic students is white students. As in the main text, teacher effectiveness estimates are standardized to have a mean of 0 and a standard deviation of 1 in the population of teachers. Standard errors, displayed in parentheses, are adjusted for clustering within schools.
*$p < .05$.

Table A3. Teacher Effectiveness–Social Background Interaction Estimates for Alternative Effectiveness Measure Specifications.

	EDS		Black		Hispanic	
Variable	Est	*SE*	Est	*SE*	Est	*SE*
Mathematics						
Main[a]	.011	.014	−.063	.017	−.015	.018
Alternative effectiveness covariates						
Individual controls only	.005	.013	−.063	.017	−.018	.018
Prior achievement only	−.003	.013	−.053	.018	−.024	.017
Subgroup-specific effectiveness						
Teachers of poor and nonpoor students	.033	.014				
Background-specific TE	.014	.016				
Teachers of black and white students			−.045	.018		
Race-specific TE			−.143	.020		
Reading						
Main[b]	−.020	.027	−.085	.033	−.137	.043
Alternative effectiveness covariates						
Individual controls only	−.034	.025	−.095	.031	−.141	.040
Prior achievement only	−.039	.025	−.100	.030	−.143	.039
Subgroup-specific effectiveness						
Teachers of poor and nonpoor students	.008	.028				
Background-specific TE	.039	.043				
Teachers of black and white students			−.080	.036		
Race-specific TE			−.272	.043		

Note: EDS = economically disadvantaged students; TE = teacher effectiveness; Est = estimated interaction with teacher effectiveness, expressed as proportion of estimated benefit for advantaged group; *SE* = standard error, accounting for uncertainty in numerator and denominator (adjusted for clustering of observations within schools). All interaction estimates are reported as a proportion of the estimated benefit for the reference group (non–economically disadvantaged or white).
[a]Estimates correspond to Model 1 in Tables 1 and 2 (and Figure 1) of the main text.
[b]Estimates correspond to Model 4 in Tables 1 and 2 (and Figure 1) in the main text.

Table A4. Teacher Effectiveness–Social Background Interaction Estimates for Alternative Outcome Model Specifications.

Variable	EDS		Black		Hispanic	
	Est	*SE*	Est	*SE*	Est	*SE*
Mathematics						
Main[a]	.011	.014	−.063	.017	−.015	.018
School fixed effects	.024	.016	−.049	.021	−.002	.021
Alternative covariates						
Achievement only[c]	.024	.013	−.058	.015	−.020	.016
Individual characteristics only[c]	.010	.014	−.065	.017	−.016	.018
With teacher judgments[d]	.010	.014	−.063	.018	−.009	.018
With classroom–TE interactions[d]	.018	.014	−.053	.017	−.002	.018
Schools with balanced classroom assignments						
Balanced-assignments subsample	.066	.039	−.064	.045	.046	.045
Effectiveness based only on subsample	.070	.037	−.047	.047	.069	.045
Reading						
Main[b]	−.020	.027	−.085	.033	−.137	.042
School fixed effects	−.017	.035	−.091	.042	−.156	.057
Alternative covariates						
Achievement only[c]	−.017	.026	−.084	.027	−.152	.038
Individual characteristics only[c]	−.031	.026	−.097	.032	−.144	.041
With teacher judgments[d]	−.052	.026	−.111	.031	−.141	.04
With classroom–TE interactions[d]	−.015	.027	−.078	.033	−.126	.043
Schools with balanced classroom assignments						
Balanced on achievement and demographics	−.024	.065	−.128	.083	−.005	.096
Measures based only on subsample	−.032	.064	−.132	.086	−.043	.097

Note: EDS = economically disadvantaged students; TE = teacher effectiveness; Est = estimated interaction with teacher effectiveness, expressed as proportion of estimated benefit for advantaged group; *SE* = standard error, accounting for uncertainty in numerator and denominator (adjusted for clustering of observations within schools). All interaction estimates reported as a proportion of the estimated benefit for the reference group (non–economically disadvantaged or white).

[a]Estimates correspond to Model 1 in Tables 1 and 2 (and Figure 1) of the main text.

[b]Estimates correspond to Model 4 in Tables 1 and 2 (and Figure 1) in the main text.

[c]Covariates removed from the primary specification.

[d]Covariates added to the primary specification.

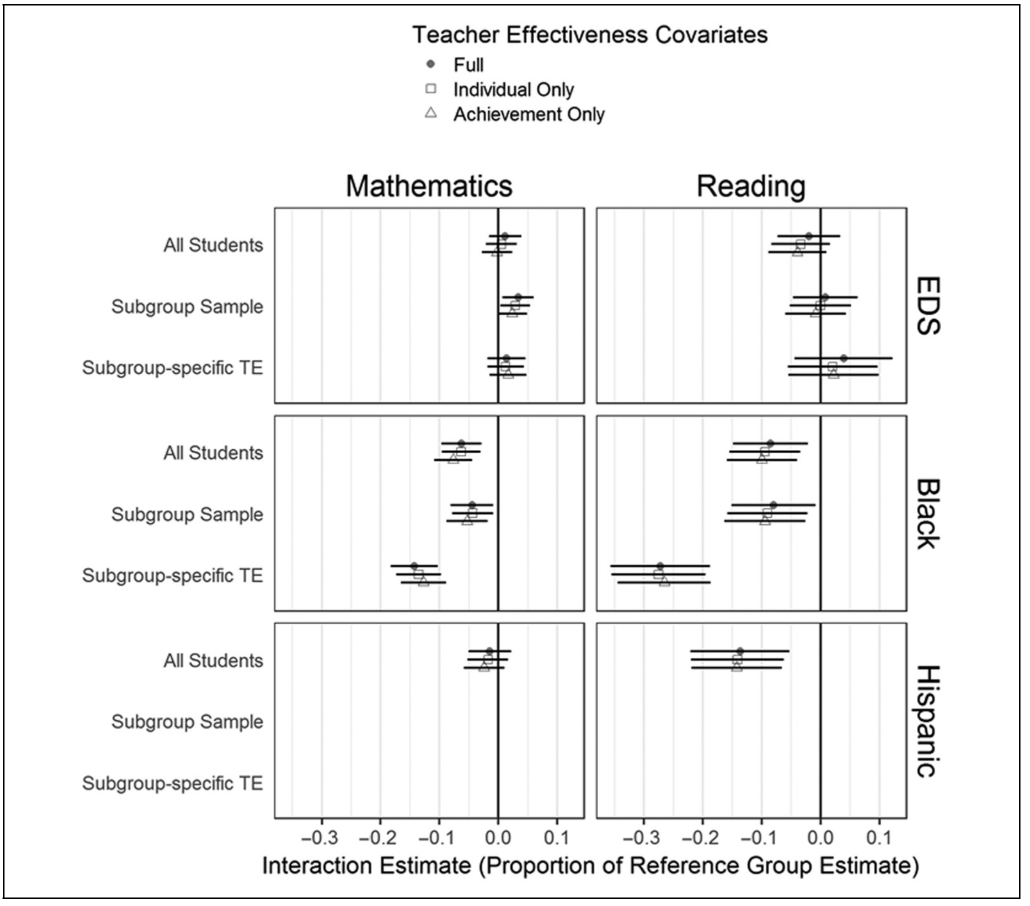

Figure A1. Interaction estimates and 95% confidence intervals for alternative teacher effectiveness measure specifications.

Note: EDS = economically disadvantaged students; TE = teacher effectiveness. "Full" covariates with the "all students" sample correspond to the main specification reported in Models 1 and 4 of Tables 1 and 2. "Subgroup sample" includes all teachers observed with at least 10 students in the focal and reference group in other years. "Subgroup-specific TE" uses teacher effectiveness measures calculated separately within group (among the subgroup sample of teachers). "Individual only" covariates omit classroom and school aggregate characteristics. "Achievement only" covariates additionally omit all individual characteristics except for prior achievement.

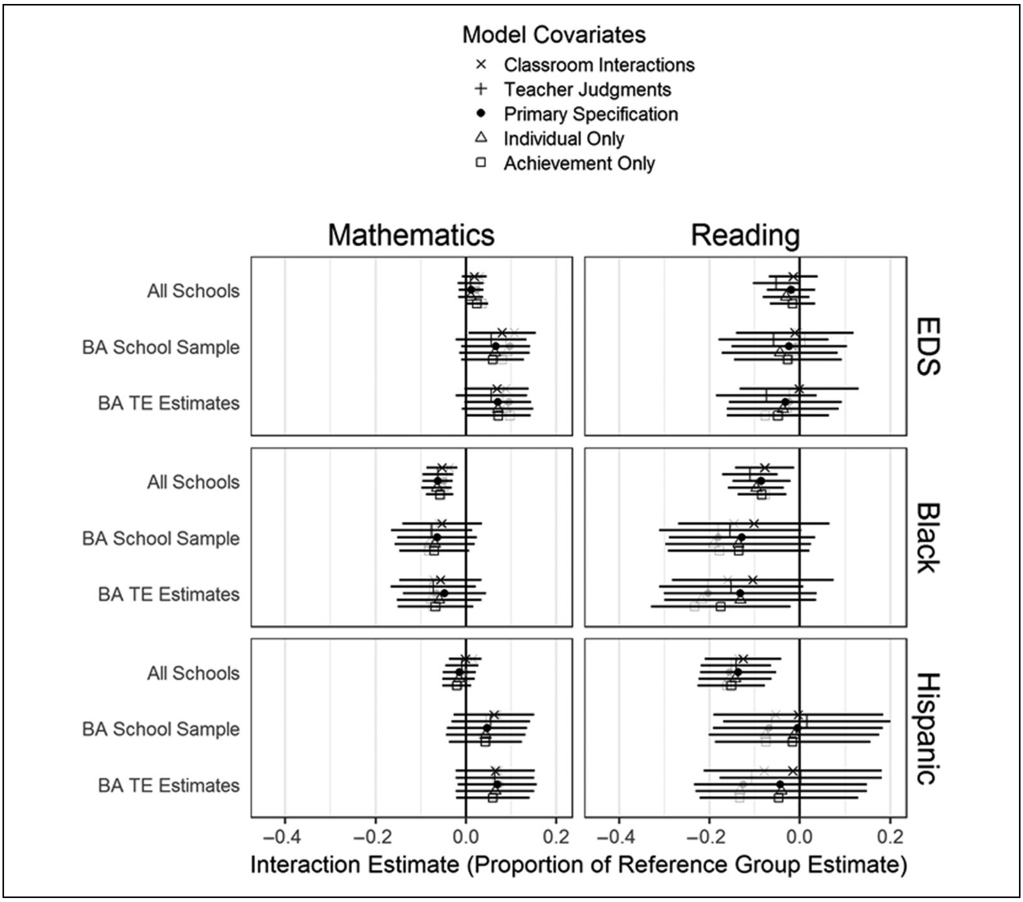

Figure A2. Interaction estimates and 95% confidence intervals for alternative model specifications.
Note: BA = balanced assignment; TE = teacher effectiveness. Black (gray) shapes represent estimates without (with) school fixed effects. The "primary specification" covariates with the "all schools" sample correspond to the main specification reported in Models 1 and 4 of Tables 1 and 2. The "BA school sample" limits the analysis to schools with balanced classroom assignments in terms of prior achievement, race-ethnicity, and economic disadvantage. The "BA TE estimates" calculates teacher effectiveness solely within this subsample. Covariate specifications alter the control variables of the model in Equation 2 of the text: "individual only" omits classroom characteristics, "achievement only" additionally omits all covariates except prior achievement, "teacher judgments" add to the primary specification indicators of previous teachers' judgment of students' academic proficiency, and "classroom interactions" add to the primary specification interactions between teacher effectiveness and classroom aggregate characteristics.

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NOTES

1. For more information, see http://www.ncpublicschools.org/accountability/testing/eog/.
2. Grade 3 teachers are considered only prior to 2011, when pretests at the beginning of the year allow value-added estimates during grade 3.
3. As noted earlier, not all differences in "teacher effectiveness" are the responsibility of teachers. For instance, assignment to one teacher may include assignment to an inferior classroom space. However, such differences still reflect the relative opportunities related to teacher assignments. I thus refer to value-added measures as indicators of teacher-related learning opportunities rather than teacher-caused opportunities.
4. Reliability estimates are calculated as $\frac{\hat{\sigma}_{\mu}^2}{\hat{\sigma}_{\mu}^2 + \frac{\hat{\sigma}_{\varepsilon}^2}{N_j}}$, where $\hat{\sigma}_{\mu}^2$ reflects the estimated variance in teacher effects from the model in Equation 1, $\hat{\sigma}_{\varepsilon}^2$ reflects estimated variance within teachers, and N_j represents the number of student observations for teacher j.
5. An experimental evaluation also found benefits of providing an opportunity to recruit a teacher previously identified as highly effective (Glazerman et al. 2013). Although this does not assess the precise accuracy of value-added estimates, it supports a degree of predictive validity across different school contexts (at least for teachers willing to transfer).
6. Skeptical perspectives highlight general agreement about some value in value-added measures, despite debate about the size of possible biases. For example, an influential paper on the limits of these measures concluded that they "have substantial signal but nevertheless introduce important misclassification into any assessment of teacher quality" (Rothstein 2010:206). For more on this paper and subsequent methodological debates, see Everson's (2017) review. The imprecision of estimates remains a concern for conclusions about individual teachers, especially estimates based on a single year, but this limitation does not threaten their use for the aggregate conclusions presented here.
7. For instance, imagine a population of just two teachers: A, a more effective teacher who provides extra enrichment geared toward mainly nonpoor students, and B, a teacher who provides lesser, but equivalent, opportunities to all students. The average effectiveness of teacher A would understate the typical opportunities he or she provides to nonpoor students and overstate those opportunities for poor students. The benefits of average teacher effectiveness would seem largest for nonpoor students (the benefits of teacher A would be highest), even if there were no interaction between background and opportunities. By contrast, a subgroup-specific measure of effectiveness would more accurately characterize opportunity differences between classrooms.
8. Subscripting by teacher j in Equations 2, 3, and 4 is redundant, because individual students have a single teacher in each year. I include it to highlight that the outcome is the same as in Equation 1.
9. These disparities can be thought of as standardized differences in exposure to teacher-related learning opportunities, because the effectiveness measure has been scaled to have a mean of 0 and a standard deviation of 1 for the teachers in the sample. The differences are smaller in the metric of standardized student achievement, because a standard deviation in teacher quality corresponds to .20 student standard deviations in mathematics and .14 in reading.
10. The Hispanic coefficient in mathematics is notably positive, reflecting an advantage relative to white peers after controlling for prior achievement and other variables, despite a large raw gap and a substantial gap conditional on prior achievement. This result is surprising, but it echoes previous research noting relatively favorable *trends* for Hispanic students, which in some cases are more favorable than for white students (Reardon and Galindo 2009). In supplemental analyses (not shown), I find that controls for three variables are most responsible for the positive Hispanic estimate (beyond prior mathematics achievement): prior achievement in reading, economic disadvantage, and limited English proficiency.
11. This comparison is meant to be instructive rather than definitive. For instance, note that Fitzpatrick, Grissmer, and Hastedt (2011) find no statistically significant difference in the effects of additional days of schooling by social background using different methods but the same data, although they do not report estimates or precision of these tests.
12. Although instructive, these implied contributions to achievement gaps do not necessarily present a practical way to address inequalities, as it is not clear how schools might equalize the benefits of learning opportunities. This limitation motivates the next calculation.
13. Sample sizes preclude doing the same for Hispanic students.
14. In supplementary testing, I found that classrooms' gender composition tends to be much more balanced than expected by chance.
15. As shown in Appendix Figure A2, the inclusion of school fixed effects makes the largest difference for Hispanic interactions in the balanced-classroom-assignment subsample, providing more reason to interpret these results with caution.
16. For the estimates suggesting a compensatory effect in the balanced-assignment subsample, controlling for prior achievement interactions reduces the positive point estimate by approximately half (not shown).

SUPPLEMENTAL MATERIAL

The supplemental material is available in the online version of the article.

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