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Classroom Segregation Without Tracking: Chance, Legitimacy, and Myth in "Racial Paradise"

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ABSTRACT

Though schools do not track in Brazil, I find that racial classroom segregation in Brazil is on par with recent estimates from North Carolina high schools (Clotfelter et al., 2020). How does racial classroom segregation occur without tracking, and in a supposed "racial paradise," no less? Using national, student-level data spanning from 2011 to 2017, I describe racial classroom segregation among Brazilian 5th and 9th graders and assess potential mechanisms identified in the literature. The findings are consistent with segregation by chance in which (1) schools typically assign students to classrooms pseudo-randomly, producing initial assignments that can be substantially segregated and (2) schools choose to move forward with these assignments, even when they are highly segregated, rather than make race-conscious adjustments. This is consistent with racial democracy, a prominent colorblind ideology in Brazil.

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Chance, Legitimacy, and Myth in "Racial Paradise"

Abstract

Though schools do not track in Brazil, I find that racial classroom segregation in Brazil is on par with recent estimates from North Carolina high schools (Clotfelter et al., 2020). How does racial classroom segregation occur without tracking, and in a supposed "racial paradise," no less? Using national, student-level data spanning from 2011 to 2017, I describe racial classroom segregation among Brazilian 5th and 9th graders and assess potential mechanisms identified in the literature. The findings are consistent with segregation by chance in which (1) schools typically assign students to classrooms pseudo-randomly, producing initial assignments that can be substantially segregated and (2) schools choose to move forward with these assignments, even when they are highly segregated, rather than make race-conscious adjustments. This is consistent with racial democracy, a prominent colorblind ideology in Brazil.

Keywords: Random segregation; Index bias; Racial democracy; Colorblind racial ideology

Introduction

Classroom segregation – how the grouping of students for whole-class instruction maps onto student characteristics – has long concerned education and inequality scholars who argue that it enables differential treatment within schools, particularly along racial and economic lines (Bowles & Gintis, 1976; Mickelson, 2001). To date, researchers have focused primarily on classroom segregation that occurs as a direct or downstream consequence of tracking, a practice in which students are segregated by perceived ability for differentiated instruction, typically involving explicit status markers denoting "high ability" versus "low ability" classrooms. This may entail assigning students to a suite of classrooms across many subjects or tracking may be differentiated across subjects to – at least ostensibly – allow a student to be assigned to high-track classrooms in some subjects and low-track classrooms in others (Lucas & Berends, 2002).

US high schools are particularly known for producing racial classroom segregation via tracking, and for the charged debate surrounding this practice. A recent study by Clotfelter et al. (2020) measured racial and ethnic classroom segregation in North Carolina. They report far more white/black segregation among 10th graders, which have a Dissimilarity Index score (D) of .20, than among 4th graders ($D = .06$), a difference they attribute to tracking in high schools.

Brazil prides itself on higher cross-race interaction and the absence of *de jure* segregation in its history, with political leaders often evoking a favorable comparison to US segregationism and racial conflict (Telles, 2004). Furthermore, Brazil does not use classroom-level tracking. Yet repeating the Clotfelter et al. analysis in Brazil's public schools reveals that racial classroom segregation in both 5th ($D = .18$ to $.29$) and 9th ($D = .16$ to $.25$) grade is on par not with North Carolina's non-tracking elementary schools, but with its tracked high schools. This highlights the possibility that non-tracking school systems are not exempt from becoming highly classroom segregated.

How does racial classroom segregation occur without tracking, and in a supposed "racial paradise," no less? The composition of Brazilian schools is such that even truly random assignment would systematically produce substantial racial segregation. I contend that in Brazil there is (1) widespread use of arbitrary, or pseudo-random, classroom assignment and (2) acceptance of the racial segregation that results, two avoidable practices that together segregate classrooms en masse. This explanation is compatible with historical conditions and Brazil's colorblind ideology of racial democracy, which facilitate the legitimacy of *de facto* racial segregation and undermine the legitimacy of race-conscious desegregation. It is also consistent with a long line of measurement research demonstrating the potential for substantial segregation under random assignment.

Drawing from the insights of Critical Race Theory, this study builds on the segregation measurement literature concept of random segregation – a hypothetical value benchmarking segregation to what would occur in an idealized colorblind world – to propose the concept of "segregation by chance." Segregation by chance is the practice of producing segregation by making the assignment process pseudo-random. The analysis proceeds by describing the extent of racial classroom segregation in Brazil then analyzing whether the pattern of segregation is compatible with segregation by chance or with more traditional mechanisms. I compare the observed data to simulated datasets in which students are assigned to classrooms by random assignment, age sorting, or achievement sorting; measure the explanatory power of the random assignment baseline; and consider the explanatory power of alternative explanations that have been identified in the literature (i.e., age sorting, achievement sorting, teacher steering, and parent lobbying) for comparison. The findings are consistent with racial segregation that is primarily, though not solely, segregation by chance.

Classroom Segregation without Tracking? "It's Unimaginable."

The absence of tracking in Brazil appears to promote the assumption that there is no classroom segregation. When I interviewed a former state secretary of education in 2017 to better understand my initial findings, he explained to me that students are not segregated within Brazilian schools. He recounted a story about prejudice causing between-school racial and economic segregation and then continued, "But [segregation] in between [classrooms]? One school – difference between classes, classrooms, and so on – it's almost – it's unimaginable at the moment for me" (June 7, 2017). A current state secretary of education I interviewed noted that her state has no classroom assignment guidelines, yet was adamant that classrooms are not segregated by race in her state. When asked if she had heard of classroom segregation elsewhere in Brazil, she quipped, "*Aqui nos Estados Unidos*" ("Here in the United States") (June 7, 2017). She later explained, unprompted, that there is no tracking in Brazil. These interviews comport with dozens of informal interactions I had with state and municipal education administrators while triangulating my findings. The common belief appears to be that Brazil does not track, therefore there is no classroom segregation.

Tracking is ever-present in the international literature on classroom-level segregation. Yet Gamoran's (2010) international review lists only six countries that track within schools. Many nations sort between schools rather than within them (Hanushek & Woessmann, 2006) and tracking countries like the US only track in some schools and at some grade levels. However, tracking is a crucial feature of US educational discourse, having come into fashion as a response to the racial integration of schools (Mickelson, 2001) and remained the topic of a bitter debate sometimes called the “tracking wars” (e.g., Loveless, 2011). That discourse has so dominated the classroom segregation literature that tracking is now the primary framework available for understanding classroom segregation. It is unclear whether classroom segregation does not occur without tracking, as my interviewees seem to have concluded, or if classroom segregation only appears to be an epiphenomenon of tracking because of narrow case selection in the literature.

One non-tracking context that has received attention is US elementary schools. Though few classroom segregation analyses include US elementary schools, those that do consistently find low racial segregation (Clotfelter et al., 2003, 2008, 2020; Conger, 2005; Kalogrides & Loeb, 2013; Morgan & McPartland, 1981). In fact, two of these studies offer evidence that at least some US elementary schools proactively balance their classrooms on racial lines. As I discuss below, random classroom assignment can produce meaningful racial segregation. Clotfelter et al. (2003, 2008) find that some North Carolina districts have less average racial classroom segregation than would have occurred under random assignment, indicating that there may be intentional balancing efforts. This is strikingly exceptional given the persistence of racial segregation throughout US society, and supports the presumption that widespread classroom segregation does not occur in non-tracking contexts.

Pseudo-Tracking

One possibility is that Brazilian schools are only nominally non-tracking. What little is known about racial classroom segregation in Brazil comes from a small literature focused on the possibility of pseudo-tracking (academically sorting students into classrooms without formally differentiated instruction) by test scores or age/grade distortion. Soares (2005) reports that 32%

of the total achievement variation in Minas Gerais occurs at the classroom level, which is three times the amount of the school-level variation. In a national study of 5th graders in 2009, de Oliveira et al. (2013) identify 10% of schools in which at least 33.4% of the variation within the school is between classrooms. In a study reported by Instituto Unibanco (2017), Mariana Leite identifies 426 elementary schools across the country with substantial classroom segregation by test scores and reports that higher-performing classrooms are assigned more experienced teachers than lower-performing classrooms in the same school and grade. While only about five percent of 5th grade students and four percent of 9th grade students in my sample have principals who report assigning students to classrooms based on achievement, more may do so informally (Table B1).

Other scholars consider sorting by age/grade distortion (the discrepancy between a student's age and that expected at his/her grade level due to delayed entry, stop-out, and/or retention). Bartholo and de Costa (2014) find evidence of age sorting in Rio de Janeiro's public school system, although it is not within schools as they are defined in the present study. In Brazil, students are often divided into separate shifts that attend classes in the same institution at different times of day. In the present study, I define a school as an institution-specific shift, as this is the population among which classroom assignments are made. Bartholo and de Costa (2014) find substantial shift segregation – segregation between schools that share a location and administration – by race and class that results from selecting students into shifts according to age/grade distortions. An earlier study by de Costa and Koslinski (2006) suggests this process also occurs at the classroom level; they found Rio de Janeiro schools dividing their classrooms by age and making exceptions for high-income and high-achieving students. Principals frequently indicate that they age sort classrooms; about 35% of 5th graders and 37% of 9th graders in my sample have principals who report age sorting (Table B1).

Altogether, these studies indicate that Brazilian schools may be sorting students on academic criteria as a pseudo-tracking assignment practice. However, it remains unclear whether either practice promotes substantial racial segregation at a national scale.

Teacher Steering and Parent Lobbying

Another possibility is that segregating practices that are typically secondary to tracking independently promote segregation in non-tracking contexts. Tracking is approached as both a primary mechanism of classroom segregation and a context that promotes secondary, segregation-exacerbating mechanisms. The latter are the focus of a subarea of the tracking literature that considers whether and why schools are more racially and economically segregated than academic differences predict. Though some studies do not find exacerbated segregation (Garet & DeLany, 1988; Haller, 1985; Haller & Davis, 1981), a substantial scholarship does. These scholars explain this “knock-on” segregation with consideration of how status influences a dynamic classroom assignment process, showing that classroom segregation is influenced by biased assessments of ability, parent lobbying for classroom assignments, teacher steering during the assignment process, and schools competing for the enrollment of advantaged students (Delany, 1991; Grissom et al., 2015; Lewis & Diamond, 2015; Oakes & Guiton, 1995; Watanabe, 2008). Altogether, this scholarship argues that, as Oakes and Guiton (1995) put it, “irregularities favor the advantaged” (p.26) when it comes to classroom assignment.

Of these secondary segregation mechanisms, tracking is not inherently prerequisite for teacher steering and parent lobbying, so they could occur in non-tracking contexts. Grissom et al.

(2015) describe the micropolitics of classroom assignment in which teachers compete for particular types of students, steering lower-status students into classrooms with newer and less effective teachers. Additionally, parent lobbying for teachers and peers can also increase segregation, whether because racially privileged parents are more likely to lobby for classroom assignments (Delany, 1991; Oakes & Guiton, 1995) or because they lobby more successfully (Lewis & Diamond, 2015).

Segregation by Chance

Another possible mechanism of classroom segregation in non-tracking contexts is segregation by chance. It has long been understood in the segregation measurement literature that segregation occurs under random assignment (Cortese et al., 1976). This random segregation (also called small-unit bias, index bias, expected segregation, and random unevenness) can be substantial when assignment is highly stochastic and groups (i.e., racial groups) or units (i.e., classrooms) are small. This is akin to the problem of random sampling with a small N in which it is likely that important characteristics (e.g., race) will be unbalanced across treatment conditions (e.g., classroom) because the assignment variable, despite being random and uncorrelated with race on average, happens to be correlated with race in a given iteration. On average, there is some imbalance, and this expected value of segregation under random assignment is a function of classroom and racial group sizes (Cortese et al., 1976).

Thus, when schools group students into classrooms according to criteria that are uncorrelated with race, they can produce substantial segregation because classrooms are small samples of the school-grade population. While I spoke to one former principal who described using random number generators, in practice schools may approach assignment haphazardly or use arbitrary – rather than random – criteria like the alphabetical order of names.

How Much Segregation Can Occur by Chance?

Random baselines are commonly used throughout the sciences as either bias corrections or non-zero null hypotheses when the expected value of a measure under random assignment is non-zero. The literature on segregation between organizational units tends to differentiate segregation that must have been socially produced from that which could be due to chance (i.e., segregation net of the random baseline) through bias-correction or statistical testing (F. D. Blau, 1977; Bygren, 2013; Carrington & Troske, 1997; Cortese et al., 1976; Fossett, 2017; Winship, 1977). A similar scholarship on segregation in networks differentiates between a baseline model of homophily under random assortment and homophily which occurs net of the baseline (P. M. Blau, 1977; Fararo & Skvoretz, 1987; McPherson et al., 2001).

The expected value of segregation under random assignment is often substantial when units are small (e.g., Bygren, 2013; Carrington & Troske, 1997). This is true in the present case; random assignment would produce as much racial classroom segregation in Brazil as would pseudo-tracking sorting practices. Figure 1 shows the distribution of racial classroom segregation in Brazilian public schools in four simulated assignment processes: random assignment, age sorting, strict sorting by test scores as though they are directly observed, and sorting based on a noisy proxy of test scores ($r = .75$). Each distribution includes the simulated classroom segregation level of each school-shift-grade-year in the analytic sample under the given condition (simulations and sample are explained in more detail below). The distribution of racial segregation is similar in each condition, with random assignment producing only slightly less segregation than age and achievement sorting. In Brazil, chance is potentially as potent a source of classroom segregation as the usual suspects.

[Figure 1 about here]

Does Segregation Occur by Chance? Toward an Alternative Approach

However, Figure 1 – like the random baselines used in prior studies – does not tell us *whether* substantial classroom segregation occurs by chance, just that it *may*. The literature consistently considers segregation net of random baselines to enable researchers to focus on the remaining segregation. The logic is that this compares the observed world to a colorblind ideal in which "race had no effect" on assignments (Cortese et al., 1976, p. 633).

On one hand, this is strange: how does one position colorblind assignment as a neutral practice if it entails institutions foregoing less-segregating alternatives and (predictably) producing high segregation en masse? On the other hand, it is consistent with the pervasive common sense that colorblind policies and practices are ideal for promoting racial equality (Bobo et al., 1997; Bonilla-Silva, 2006). This common sense is challenged by Critical Race Theory scholars who have demonstrated myriad ways the colorblind ideal works against the pursuit of racial equality, including in science, where the colorblind ideal orients research methodologies and constrains the purview of race scholarship (Crenshaw, 2019). In this case, the field's reliance on the random baseline approach naturalizes colorblind assignment by using it as a benchmark and precludes investigation of the segregation it produces by considering it only as a hypothetical value. This leaves little opening for calling the practice into question.

Hence, I propose the concept of "segregation by chance." Segregation by chance is the practice of producing segregation by making the assignment process pseudo-random. I also use it to refer the segregation produced by this practice. That is, I contend that schools can practice and produce more or less segregation by chance and whether they do this and the resulting consequences are worthy objects of study.

Consider a school deciding whether to use race-stratified random classroom assignment (minimizing racial segregation) or to use simple random assignment. Given the former choice, racial segregation is predetermined and kept low. The latter choice, on the other hand, is an oft-segregating random draw from a set of possibilities determined by the school's racial composition and classroom sizes. Or consider that when random assignment is used, schools can choose to have less segregation than would occur by chance. When schools "draw" highly racially segregated assignments prior to starting the school year, they can rearrange students to provide a more balanced set of assignments. They can simply re-randomize assignments until they get a low segregation draw. The key point here is that there is no neutral condition; the choices, actions, and inactions of a school – whether to use stratified or fully random assignment, whether to desegregate classrooms initially assigned at random – have ramifications for whether and how much segregation occurs by chance. Moreover, whereas random baselines are invariable to these choices, the perspective I propose sees them and their consequences as objects of study: Does segregation occur by chance? How? How much?

Legitimacy and Segregation in Brazil

I turn now to considering how the Brazilian ideological context may shape how classroom segregation occurs by facilitating and constraining the legitimacy of racial segregation and desegregation. I follow Weber's (1978) descriptive account of legitimacy as the condition of being "approximately or on the average, oriented toward determinable 'maxims'" such that a legitimate condition is understood to be accordant with broadly accepted norms and values, inducing an obligation to at least tolerate it (31).

When Brazil entered the 20th century, slavery had only recently been abolished, in 1888. Compared to the US, Brazil had a far greater population with both European and non-European ancestry, owing to the male-dominant demographics of Portuguese colonizers who more often had children with non-white partners and victims compared to the colonizers of the US who primarily migrated as families (Telles, 2004). Brazil was also in the midst of *branqueamento*, a national eugenics policy promoting European migration and cross-racial marriage as a grand project to design a white nation through the dilution of black blood (Loveman, 2009).

By mid-century, the government was actively promoting the ideology of racial democracy, a patriotic, racism-denying ideology that reframes Brazil as a “racial paradise” with a single, mixed Brazilian race and presents multiraciality as a consequence of racial harmony (Bailey, 2009; Freyre, 1946; Telles, 2004). The 1964-1985 military dictatorship embraced the myth of racial democracy and brutally crushed dissidents, hampering racial justice movements. Today, racial democracy lives on; in response to the murder of João Alberto Silveira Freitas, Vice President Mourão declared “there is no racism” in Brazil (Camazano, 2020). However, this ideology is increasingly contested by the growing Black Movement, which promotes positive black identity among Afro-Brazilians and challenges racism and inequality (Bailey, 2009; Telles, 2004). Some consider racial democracy to now serve primarily as an aspiration: the promise of a raceless society once racism is extinguished (Bailey, 2009).

Importantly, racial democracy grew in explicit recognition that Brazil did not implement *de jure* segregation and anti-miscegenation like the US, and frames Brazil as non-segregationist (Bailey, 2009; Telles, 2004). *De facto* racial segregation is commonly assumed to be epiphenomenal, typically to class. This is the case with respect to housing, though there is sizable racial residential segregation net of class (Telles, 2004). This myth of a race-neutral and racially harmonious Brazil is a pre-existing narrative that lends legitimacy to *de facto* racial segregation not otherwise readily explained.

Meanwhile, race-based integration may face greater barriers to legitimacy than does racial segregation. Another important component of racial democracy, antiracialism, construes the discussion of race and racism as a racist, foreign intervention (Guimarães, 2001; Schwartzman, 2009). Though Brazilians see one another as raced, reliably categorizing photographs into racial groups (Bailey, 2009), it is considered improper to make racial ascriptions explicit. Ascriptions to darker racial groups are particularly improper; when ascribing the race of someone one sees as black, it is polite to instead use a lighter category like *moreno* (Schwartzman, 2009). This system of manners upholds the pretense of a single Brazilian race even as it implies the superiority of whiteness. Thus, racial democracy is a colorblind ideology that goes beyond US colorblind or laissez-faire racism (Bobo et al., 1997; Bonilla-Silva, 2006), denying the existence of race not only as an axis of oppression but as a socially meaningful category. This can work against race-based classroom integration by calling into question the appropriateness of school administrators who might otherwise acknowledge color differences among students and consider those differences to organize less segregated classrooms.

To be clear, race-based integration is not without its proponents. Most notably, public colleges began adopting racial affirmative action policies in 2001, a major win for the Black Movement. Yet Telles and Paixão (2013) note that by 2010, “class quotas ha[d] become more common than race quotas, even though the debate ha[d] been almost entirely about race quotas” (p. 10). They argue that the strong opposition to race quotas specifically reflects denial of racism's role in creating racial inequality in higher education. The rationale of equalizing opportunity failed to legitimate race-based college integration despite awareness of stark racial inequities in college-going. Classroom integration may be considered illegitimate as well, particularly when classroom segregation has not been established as a social problem.

Given the US history of *de jure* segregation, classroom racial segregation might be conspicuous absent a policy like tracking to make it ostensibly race-neutral. In Brazil, racial segregation without a clear source is likely to receive the benefit of the doubt due to a common belief that the nation is inherently race-neutral. While there is some evidence of non-tracking US schools integrating their classrooms (Clotfelter et al., 2003, 2008), race-based integration in Brazil has questionable legitimacy owing to antiracialism. These factors make Brazil particularly susceptible to classroom segregation by chance, which can only be a substantial driver of racial segregation if school administrators choose not to intervene when random segregation happens to be high. Otherwise, even a school using random assignment could keep segregation by chance low by monitoring drafted classroom assignments for substantial racial imbalance and reassigning some students to better balance the classrooms.

Data

I investigate classroom segregation in Brazil using *Prova Brasil* 2011-2017, a publicly-available dataset based on a biennial, nationwide student achievement test that includes a student survey with self-reported demographic information as well as identifiers linking students to their classrooms (which are stable across subjects), shifts, and school administrations (Instituto Nacional de Estudose Pesquisas Educacionais Anisio Teixeira, 2017). I use these identifiers to link *Prova Brasil* to *Censo Escolar* 2011-2017, a biennial national survey of teachers and principals (Instituto Nacional de Estudose Pesquisas Educacionais Anisio Teixeira, 2017). Collected at the end of the school year, this survey aims to include all Brazilian public-school 5^{th}- and 9^{th}-graders except those attending very small schools.

I focus on public schools in which classroom segregation is possible, restricting the data to multi-classroom schools where there is one set of students eligible for assignment to one set of classrooms per school-grade-year (e.g., each shift within a school administration is considered a school). I also include schools only if all of their classrooms have race item response rates of at least 75%. The full sample includes 53,452 school-year observations in 5th grade and 32,068 in 9th grade. (See Appendix B for descriptive statistics.) Overall, the samples include over 5.3 million students. Though they are not representative of all Brazilian 5th and 9th grade students, these samples cover a broad swath of the country and include thousands of distinct school systems. This breadth ensures that the present study identifies general patterns rather than local idiosyncrasies.

Measuring Racial Segregation

With the exception of Figure 2 in which I replicate another study's method, I measure racial segregation across classrooms in the same school-grade-year using the multigroup Information Theory Index, denoted H. This measure enables me to use more than two racial groups and to decompose segregation without bias (Reardon et al., 2000; Reardon & Firebaugh, 2002). Tracking analyses often consider how classroom segregation becomes curriculum-wide segregation; here, I focus on the production of classroom segregation itself, as students in Brazil's public schools are typically grouped into classrooms that remain together for each subject.

H operationalizes segregation as the degree to which students are unevenly distributed across classrooms given a school's population in a given grade and year. H is based on entropy (E), a heterogeneity measure:

$$E = \sum_{m=1}^{M} p_m ln\left(\frac{1}{p_m}\right), \tag{1}$$

where p_m is the proportion in group m (e.g., proportion white). H compares the heterogeneity of classrooms to that of their school, weighting the contribution of each group and classroom according to relative size:

$$H = \frac{1}{E}\sum_{m=1}^{M} p_m \sum_{j=1}^{J} \frac{n_j p_{jm}}{N p_m} \ln\left(\frac{p_{jm}}{p_m}\right), \tag{2}$$

where n_j is the number of students in classroom j, N is the number of students in the school, p_{jm} is the proportion of students in classroom j who are in group m, and E is the entropy of the school.

$H = 0$ when every classroom is proportional to the school, and $H = 1$ when classrooms are completely segregated, meaning no racial group shares a classroom with any other. Values of H can be deceptively small; on average, $H = .065$ for white/black segregation *between* schools in the average US district that is at least 5 percent black and 5 percent white (supplemental analysis using Reardon et al., 2021).

I use multigroup H to simultaneously consider the segregation of all racial groups (Reardon et al., 2000; Reardon & Firebaugh, 2002). To stray as little as possible from students' emic racial categories and capture the experiences of as many students as I can, I do not combine or drop categories. Instead, I measure segregation among all six racial categories offered in the

Prova Brasil survey: white, *parda/o* (roughly, brown), *preta/o* (roughly, black), indigenous, *amarela/o* (roughly, Asian), and "I don't know."

How Racially Segregated Are Classrooms?

US high schools are particularly known for classroom segregation by race due to tracking. A recent study in North Carolina measured white/black classroom segregation among 4th grade elementary school children and 10th grade high schoolers in 2017 (Clotfelter et al., 2020). The latter are the best large-scale estimates of how much racial segregation currently occurs under tracking in the US. The authors find disconcertingly high average levels of classroom segregation in North Carolina's high schools, which they attribute to tracking.

Figure 2 presents a comparison of their findings for white/black segregation among 4th and 10th graders in 2017 to my findings for racial segregation among Brazilian 5th and 9th graders in 2017. I follow their procedure, using the population-weighted average of the Dissimilarity Index (D) in municipalities in which at least 4 percent of students are in each racial group. D is a binary segregation measure with a random baseline that is particularly sensitive to low group sizes, making the findings sensitive to how race is operationalized. I estimate racial segregation in Brazil several ways, comparing whites to all non-whites, to *negros* (a composite of *pardos* and *pretos*), to *pardos*, and to *pretos*, the latter of which is likely to be higher due to the small portion of *preta/o* students.

[Figure 2 about here]

Regardless of how race is operationalized, Brazilian classroom segregation levels in both primary and secondary schools are more comparable to North Carolina's tracking high schools than they are to its non-tracking elementary schools. Whereas North Carolina's 4th graders experience little classroom segregation ($D = .06$) compared to its 10th graders ($D = .20$), the most conservative estimates of 5th and 9th grade segregation in Brazil are $.18$ and $.16$, respectively. The highest estimates from Brazil are even greater than North Carolina's high schoolers, putting 5th and 9th grade segregation at $.29$ and $.25$, respectively.

Hereafter, all estimates use H rather than D. Appendices C-E enrich this description of the extent of classroom segregation. Appendix C depicts classroom segregation in schools that fall at different levels of multigroup segregation to provide a visual representation of schools at different values of H. Appendix D describes the scale of racial segregation in the Brazilian public school system by decomposing the multigroup racial segregation between classrooms throughout the nation into units long-understood as segregated: regions, municipalities, and schools. In each year and grade, the plurality of racial segregation (38-42% in 5th grade, 30-35% in 9th grade) in Brazil's multi-classroom public schools occurs between classrooms in the same school, not traditional suspects like regional, municipal, or school differences. Between-shift segregation is also considered, but it is minimal in each year and grade. Appendix E describes how each racial group contributes to multigroup classroom segregation. Whereas each binary segregation in 9th-grade contributes to multigroup segregation in similar amounts as would occur under random assignment, multigroup segregation in 5th grade is more driven by segregation of *pardos* and students who responded "I don't know" than it would be under random assignment.

What Causes Brazil's Classroom Segregation?

I begin investigating mechanisms by considering which assignment process is most consistent with the observed pattern: random assignment, age sorting, strict achievement sorting, or noisy achievement sorting? In Figure 3, I compare observed values of racial segregation to simulated estimates of how much racial segregation would be expected under random assignment and the various sorting conditions. Each simulation of student assignment assigns the students in the observed data to hypothetical, equal-sized classrooms in their school-grade-year to model what would occur under a particular assignment regime. I use random assignment to proxy for arbitrary assignment, giving each student an equal probability of being assigned to each classroom. To proxy for age sorting and strict achievement sorting, I assign students by the rank of their age and the average of their Portuguese and Math scores, respectively. To account for two shortcomings in the achievement measure – that it uses end of year scores and that schools may sort by perceived ability rather than achievement – the noisy achievement sorting simulating sorts students by a noisy proxy for their test score average in which classical error is added such that the reliability is $.75$. Random assignment and noisy achievement sorting include random variation, so I simulate 50 assignments in each school-grade-year and take the mean to estimate each observation's baseline.

The panels in Figure 3 displays the grade 5 and 9 trends in racial segregation with respect to the simulated baselines of the four assignment conditions. These trends are compared to the trend in simulated random segregation in each panel. In the three sorting panels, the trends are also compared to the trend from simulations of the given type of sorting (e.g., the trend from a simulation of noisy achievement sorting is plotted against the noisy achievement sorting baseline). Across the four panels, the observed segregation and random segregation lines run nearly parallel, with observed segregation – especially in 5th grade – being consistently greater than random segregation. In the age sorting and strict achievement sorting panels, observed segregation has a substantially flatter trend than the $y = x$ pattern we would see if segregation occurred in the same way as the relevant sorting simulation. Instead, it tracks better with the random segregation trend. The observed segregation lines do not contrast as markedly with the noisy achievement sorting simulations, but the trends are somewhat flatter and again trend more similarly with the random segregation line.

[Figure 3 about here]

One challenge to distinguishing which simulated assignment processes fit the observed data better than others is that the simulated segregation levels are correlated, particularly for random assignment and noisy achievement sorting. To parse this, I estimate 5th- and 9th- grade two-level hierarchical multiple regression models in which level-1 is the school-year-grade and level-2 is the year-grade. Given the set of baselines $\boldsymbol{X_{it}}$ describing the expected segregation in school i in year t under each assignment process, I model segregation, H_{it}, in each grade as

$$H_{it} = \gamma_{00} + u_{0t} + (\boldsymbol{\gamma_{.0}} + \boldsymbol{u_{.t}})\boldsymbol{X_{it}} + r_{it} \tag{3}$$

$$r_{it} \sim N(0, \sigma^2); \begin{bmatrix} u_{0t} \\ u_{.t} \end{bmatrix} \sim MVN\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \tau_{00} & \tau_{0.} \\ \tau_{.0} & \tau_{..} \end{bmatrix}\right),$$

where γ_{00} is the year-average intercept, u_{0t} is a year-specific intercept, $\boldsymbol{\gamma_{.0}}$ is the set of year-average slopes on each baseline, $\boldsymbol{u_{.t}}$ are year-specific slopes, and r_{it} is the total within-year error. The estimates of interest are $\boldsymbol{\gamma_{.0}}$, which are year-average estimates, meaning that they are each an average of four year-specific slopes. This is preferable to an OLS estimate, which would implicitly give more weight to the slopes of years with more observations when incorporating the four years of data into a single model. In addition to estimating these models in the observed data, I estimate them for each of the 50 simulations of random assignment and the 50 simulations of noisy achievement sorting to compare reality to what would be observed if classrooms were assigned either randomly or by an achievement proxy, respectively.

[Table 1 about here]

Table 1 presents the regression results. The 1st and 4th columns present the findings from the 5th and 9th grade models, respectively. The 2nd and 5th columns present the average estimates and their 10-90% ranges over the 50 draws in the random assignment condition. This depicts what one would observe if all schools used random assignment. The 3rd and 6th columns present similar estimates for the noisy achievement sorting condition. Net of the sorting baselines, the random baseline continues to have a strong association with observed segregation ($\gamma = 1.105$ in 5th grade, $\gamma = .917$ in 9th grade). That is, an increase in the random baseline is associated with a similarly-sized increase in observed segregation. This comports with the near-one coefficient that would occur under truly random assignment. The pseudo-tracking baselines have weak relationships with observed segregation; the largest statistically significant estimate is $.053$ for the age sorting baseline in 9th grade, whereas the coefficient would be 1 if schools used the same sorting process as the age sorting simulation. Instead, this estimate is more similar to, if somewhat greater than, what would be observed under random assignment. There are also other small but noteworthy differences from what would occur under true random assignment: the coefficient on strict achievement sorting is significantly different from 0 in 5th grade; the intercepts, particularly in 5th grade ($\gamma = .013$), are greater than would occur under random assignment; and there is less within-year variance explained and more slope variation over time.

How Much Segregation is Explained by the Random Assignment Baseline?

It appears that the random assignment baseline is a strong but imperfect predictor of observed segregation. To quantify how much segregation it explains, I consider both how much variance it explains and its predicted contribution to nationwide segregation using bivariate models of schools within years similar to the model described in Equation 3. If segregation by chance is the primary source of classroom segregation in Brazil, it should explain substantial variance and plausibly contribute most of the segregation.

I measure the variance explained by a predictor, X_{it}, as the percentage of total within-year variance explained when adding X_{it} to a null model,

$$\%V = 100 * \frac{\sigma_{null}^2 - \sigma^2}{\sigma_{null}^2}, \tag{4}$$

where σ^2 is the variance of the level-1 residual (r_{it} in Equation 3) in the bivariate model and σ_{null}^2 is the variance in a null model that excludes X_{it}.

Another way to consider how much segregation the random baseline explains is to consider the proportion of nationwide segregation that one would attribute to the random baseline if the bivariate model described a causal relationship. The estimates are not causal, so the predicted contribution should not be confused with the actual contribution, which is unknown. Given a predictor X_{it} and observed multigroup segregation H_{it}, I compute the predicted contribution of the predictor in a given grade as

$$\%S = 100 * \frac{\sum_t \sum_i \frac{N_{it} E_{it}}{N_t E_t} \gamma X_{it}}{\sum_t \sum_i \frac{N_{it} E_{it}}{N_t E_t} H_{it}}, \tag{5}$$

where γ is the estimated association between X_{it} and H_{it} in the bivariate model; E_{it} are E_t are entropy measures of multigroup racial diversity used in the computation of H_{it}; and N_{it} are N_t are the number of students in the school-year it and in the year t, respectively. The numerator is the predicted contribution of X_{it} over all years t and the denominator is the total classroom segregation over all years t. $\%S$ contextualizes the coefficient by taking into account the size of the predictor; for example, an association of $\gamma = 1$ implies different contributions to segregation depending on the size and distribution of the predictor in the sample.

The strong association observed in the previous section between classroom segregation and the random baseline is also apparent in these bivariate models. Under random assignment, the coefficient γ would be one, but in both grades it is statistically significantly greater than one. (See Appendix F for detailed tables of estimates). The random baseline explains 15.9% of the total variation in racial segregation in the 5th grade sample and 23.6% in the 9th grade sample. In both cases, this is high, but lower than what occurs when the outcome variable is changed to segregation under simulated random assignment ($\%V = 28.7$ in 5^{th} grade; $\%V = 31.0$ in 9^{th} grade). Under universal, truly random assignment, the predicted contribution metric for the random baseline would be 100%. The metric for the observed data is not far off: 82.3% in 5th grade and 90.5% in 9th grade. The random baseline explains much of the variation in classroom segregation and plausibly contributes the vast majority of it, nearly as much as it would under truly random assignment.

Alternative Explanations

While this analysis has pointed to segregation by chance as the primary driver of racial classroom segregation, the findings are not dispositive. A causal account is difficult for a number of reasons, including challenges to measuring stochasticity of classroom assignments in observed data and accounting for the mechanical associations between the random baseline and classroom size and racial composition that cannot be fully controlled for without removing all variation in the random baseline. The extant literature provides no guidance for this task because it approaches segregation by chance as a hypothetical matter (i.e., how much segregation *could be* by chance?) rather than as a social matter (i.e., how much segregation *is* by chance?).

In lieu of causal analysis, one can assess how alternative explanations fare relative to the segregation by chance hypothesis. If segregation is occurring primarily due to arbitrary classroom assignments, the regression coefficient γ, $\%V$, and $\%S$ from bivariate models regressing observed racial segregation on proxies for other processes should not be much greater than they would be under random assignment. This section uses similar bivariate models as the previous section, replacing the random baseline with various measures that are likely to be correlated with segregation produced by sorting and other processes. I compare this to similar estimates using segregation under simulated random assignment as the outcome. Figures 4 and 5 compile the results, comparing for each variable considered the 95% confidence interval of $\%V$ and $\%S$ using the true, observed data to the 90-10% range of estimates in the 50 simulations using segregation under simulated random assignment as the outcome. Variable construction details are provided in Appendix A and additional modeling and result details are provided in Appendix F.

[Figure 4 about here]

[Figure 5 about here]

One possibility is that segregation is driven by achievement or age sorting, that flaws in my simulations of these processes downwardly biased their regression coefficients in the above analysis. Achievement sorting is particularly difficult to simulate due to my reliance on end-of-year test scores to proxy for beginning-of-year perceived ability. Simulated assignments also create classrooms that are as equal-sized within a school as possible, but schools may vary classroom size in ways that affect age or achievement segregation.

If achievement sorting was a substantial source of racial segregation, one would expect segregation to be correlated with principals' reported use of achievement sorting, levels of test score segregation, and/or – because test score segregation does not produce racial segregation without racial differences in test scores – the racial stratification by test scores in a school, in addition to the simulated achievement sorting baselines discussed above. Likewise, if age sorting was a substantial source of racial segregation, one would expect segregation to be correlated with principal reports, age segregation, and/or racial stratification by age, in addition to the simulated age sorting baseline. Yet in both 5th and 9th grade, of these variables, the only ones that explain more variation than they would under random assignment nonetheless only explain about 1% of the variance. Those with greater than expected predicted contributions have $\%S$ scores no more than two percentage points greater than they would under universal random assignment.

It could be that neither arbitrary assignment nor pseudo-tracking practices are driving racial classroom segregation in Brazil. It could be due to parents lobbying for particular classroom assignments or teachers steering students so as to teach their preferred pupils. One might think these processes are contingent on parent and teacher status, respectively. For example, higher SES parents – which I operationalize using parental education measures – should be more effective lobbyists. Teachers with greater experience, pay, and tenure status should be more effective at getting themselves assigned whiter students due to their greater micropolitical power. Thus, racial segregation should be correlated with SES segregation and racial stratification by SES, or with white-nonwhite student disparities in their teachers' experience, salary, and tenure status.

In both grades, SES stratification and the three teacher disparities measures have precise, near-zero regression coefficients. SES segregation has a stronger coefficient and a greater predicted contribution than the other proxies; though $\%V$ is small in both grades, $\%S$ is 6.1% in 5th grade and 4.1% in 9th grade. However, this is only 2.2 and 1.6 percentage points more than would have occurred under random assignment, respectively. One shortcoming of this sensitivity analysis is that SES non-response is high, so the subsample could be distinct from the population of multi-classroom public schools in Brazil. This is less of a concern for the teacher disparity estimates, which have high missingness primarily because either the same teachers teach their respective subjects to both classrooms or the teachers in the grade do not vary with respect to the characteristic, rendering teacher steering irrelevant.

Finally, I consider the possibility that segregation occurs due to particularities specific to places or school administrations. There may be more willingness to segregate or desegregate by race in some places and among some school administrators. Though this would be inconsistent with universal, fully-random assignment as modeled in the random baseline simulation, it is not entirely inconsistent with the notion of segregation by chance as the influence of racial democracy and antiracialism may vary geographically.

I account for place differences by adding random intercepts at one of three geographic scales to my model: municipalities, states, and regions. The percentage of variance explained indicates how much the mean racial segregation varies across places at a given scale. To account for administrative differences, I consider the correlation between segregation in one shift within a school administrative body and segregation in the other shift administered by the same body in the same grade and year (i.e. the “peer shift”) and I also consider the correlation between segregation in one year and segregation in other years in the same school-grade.

[Table 2 about here]

Of these five factors, municipality random effects and the correlation in segregation levels across peer shifts stand out while the others have similar $\%V$ and $\%S$ scores as they would under universal random assignment. These two factors and the random baseline are all likely to be correlated with one another; to disentangle their relationships to racial segregation, Table 2 presents two-level and three-level bivariate and multiple regression models including the six possible combinations of the three predictors. These models use the subsample of schools with peer shifts. The full model uses a three-level hierarchical regression model, stratified by grade, in which level-1 is the school-year-grade and includes peer shift segregation and the random baseline as predictors, level-2 is the municipality-year-grade, and level-3 is the year-grade. (Appendix F describes the modeling approach in more detail).

In both grades, the random baseline consistently has a near-1 association with classroom segregation across the models whereas the coefficient on peer shift segregation is not robust, becoming null or changing sign once municipality-year random intercepts are included. While adding municipality random intercepts explains only as much as 2.4 percentage points more variance than is explained by the random baseline alone, this lower explanatory power may be due to restricting the sample to schools with peer shifts.

Discussion

Though the literature on racial classroom segregation has focused primarily on tracking in US high schools, Brazil's non-tracking 5th- and 9th-grade classrooms are roughly as racially segregated as North Carolina's 10th grade classrooms. Classroom-level segregation is a primary source of overall racial segregation in Brazil's school system, accounting for more segregation than regional-level and school-level segregation. How does this happen?

To answer this question, it was necessary to add to the traditional random segregation benchmark the concept of segregation by chance. Drawing from insights in Critical Race Theory, this study builds on the segregation measurement literature concept of random segregation – a hypothetical value benchmarking segregation to what would occur in an idealized colorblind world – to consider segregation by chance, the practice of producing segregation by making the assignment process pseudo-random.

Both simulation analyses and regression analyses point to segregation by chance as the primary source of racial classroom segregation in Brazil. In simulations, random assignment produces levels of racial segregation similar to pseudo-tracking practices like age and achievement sorting. The bivariate association between observed segregation and the random baseline is also strong enough that the baseline would account for over 80% of 5th grade segregation and over 90% of 9th grade segregation were the estimated relationship causally-identified.

I assess the possibility that this relationship is an artifact of other processes in two ways: simulating alternative approaches to assignment and analyzing the relationships between observed segregation and indicators of non-chance assignment practices. The pattern of observed segregation is more consistent with simulations using random assignment than with pseudo-tracking simulations. Whereas the random baseline is highly predictive of observed racial segregation levels, proxies for the pseudo-tracking, teacher steering, and parent lobbying practices observed in the literature are little more predictive than they would be under random assignment.

Segregation also does not appear to be driven by school features that are stable over short periods (e.g, specific faculty, student composition, organizational culture, community practices, etc.). Schools' racial segregation levels vary as much over time as they would under random assignment while the segregation levels of peer shifts are unrelated after accounting for municipal tendencies. Classroom segregation is also geographically diffuse; state differences explain similar amounts of variation as they would under universal random assignment. That is, while classroom segregation is consistent over space and time at a national scale, underlying this uniformity is a remarkably noisy local process, much as it would be under random assignment.

Nonetheless, segregation by chance is not the sole source of classroom segregation. The random baseline explains less variation and implies a somewhat smaller contribution to segregation than it would if all schools used fully random assignment. Graphical analyses show that there is consistently more segregation in 5th grade than predicted in simulations of random assignment. Multiple regression analyses also show that simulated achievement sorting in 5th grade and age sorting in 9th grade remain associated with observed segregation after accounting for the random baseline; though the coefficients are much smaller than they would be in a sorting regime, they would not occur under universal, truly-random assignment. Municipality random intercepts also explain more variation than they would under random assignment. Additionally, in some regression estimates the coefficient on the random baseline is significantly greater than one, indicating that some of the non-chance segregation is correlated with the random baseline (e.g., a feedback effect). Finally, the patterns of classroom segregation in 9th grade are more consistent with random assignment than those in 5th grade, across all analyses.

Conclusion

This case study demonstrates that racial classroom segregation is not specific to tracking contexts. Despite their abundance, the classroom segregation literature has rarely looked at non-tracking contexts. The findings presented here illustrate the need to cast a wider net: racial classroom segregation in Brazil is on par with that in the US high schools that have captured researchers' attention, and it appears to be primarily segregation by chance, a mechanism that has received little attention.

Though classroom segregation has garnered little interest in Brazil, it is clear that classroom assignments matter. Alves and Soares (2007, 2008) have demonstrated that learning gains vary greatly between same-school classrooms in Brazil. Botelho et al. (2015) identified widespread racial discrimination in grading in Brazil; if classrooms are racially segregated, this could amplify racial inequity. Moreover, classroom segregation by race reduces interracial contact (Moody, 2001). These concerns persist even when segregation occurs by chance.

Segregation by chance lends itself to interpretations that strip schools of agency and, with it, responsibility: *if it happened by chance, how could it be helped?* In the case of classroom segregation, the answer is: *quite easily*. Segregation by chance can only be a substantial driver of racial classroom segregation if schools choose to accept the racial segregation that results from arbitrary assignment. Otherwise, even a school using random assignment could keep segregation by chance low by monitoring proposed classroom assignments for substantial racial imbalance and reassigning some students to rebalance classrooms before the schoolyear begins.

The more interesting question might be: *if it is only by chance, why don't schools just fix it?* It is not due to racial ambiguity, as Brazilians reliably racially categorize one another (Bailey, 2009). I offer an explanation rooted in racial ideology, arguing that racial classroom segregation without a clear source may be more tolerated, and race-based integration less tolerated, in Brazil than in the US. Brazil's relationship to racial segregation is shaped by the absence of *de jure* segregation in the 20th century. This is a long-standing, government-promoted *cause célèbre* used to promote the narrative that Brazil is a "racial paradise." This ideology, called racial democracy, imagines Brazilians as a single mixed race and Brazilian society as free from racial difference. As a national myth, this ideology helps legitimate *de facto* racial segregation as not racial *per se*. Another consequence of racial democracy is antiracialism, a system of manners that hampers race-based integration efforts by discouraging explicit racial ascription.

If Brazil's racial classroom segregation by chance is due to denying the social reality of race, racial segregation by chance may be a feature of other Brazilian institutions as well; prior work has shown the potential for substantial occupational segregation by chance in other contexts (Bygren, 2013; Carrington & Troske, 1997). Additionally, racial segregation by chance may also be relevant to other societies, such as France (Beaman & Petts, 2020), where denial about the social reality of race and taboos around discussing race are widespread. Though there are colorblind and post-racial tendencies in US discourse, the prominent role of schools in US conceptions of illegitimate racial segregation may render segregation that lacks a clear source too suspect for widespread racial classroom segregation by chance to occur. Instead, economic segregation by chance seems particularly likely in the US. Class is often understood in terms of race (McDermott, 2006); norms minimize economic differences (e.g., the notion that nearly everyone is middle class); data on students' economic characteristics are very coarse (i.e., free or reduced-priced lunch); and economic segregation is rarely problematized in everyday discourse. Thus, much as a colorblind racial ideology facilitates racial segregation by chance within Brazilian schools, the class-blind ideology and data framework in US schools may facilitate economic segregation by chance.

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Tables

Table 1. *Hierarchical Multiple Regression Model of Classroom Racial Segregation (H) on Simulated Baselines in Observed Data and in Simulations of Random Classroom Assignment and Noisy Achievement Sorting.*

	Grade 5			Grade 9		
	Observed	Random Assignment	Noisy Ach. Sorting	Observed	Random Assignment	Noisy Ach. Sorting
Random Assignment Baseline	1.105	0.998	-0.000	0.917	1.002	0.000
	(1.033,1.177)	(0.985,1.013)	(-0.021,0.018)	(0.875,0.960)	(0.980,1.024)	(-0.023,0.023)
Noisy Ach. Sorting Baseline	0.007	-0.000	1.000	0.066	-0.001	1.000
	(-0.061,0.075)	(-0.018,0.015)	(0.982,1.021)	(-0.002,0.133)	(-0.019,0.018)	(0.975,1.028)
Strict Ach. Sorting Baseline	0.052	-0.001	0.000	0.014	-0.000	0.000
	(0.029,0.076)	(-0.009,0.010)	(-0.011,0.010)	(-0.017,0.045)	(-0.012,0.012)	(-0.014,0.016)
Age Sorting Baseline	0.023	-0.000	0.000	0.053	-0.001	0.000
	(-0.015,0.061)	(-0.007,0.006)	(-0.008,0.006)	(0.037,0.068)	(-0.009,0.010)	(-0.010,0.008)
Intercept	0.013	-0.000	-0.000	0.005	-0.000	-0.000
	(0.011,0.015)	(-0.001,0.000)	(-0.001,0.001)	(0.004,0.006)	(-0.001,0.001)	(-0.001,0.001)
Variance Explained (%)	0.161	0.287	0.444	0.239	0.310	0.457
		(0.282,0.293)	(0.441,0.449)		(0.304,0.317)	(0.452,0.463)
# of Observations	53452	53452	53452	32068	32068	32068
Year Variation	SD (p-value)	SD (90-10 range)		SD (p-value)	SD (90-10 range)	
Random Assignment Baseline	0.063	0.017	0.021	0.026	0.024	0.020
	(0.013)	(0.004,0.033)	(0.007,0.039)	(>.500)	(0.008,0.045)	(0.008,0.031)
Noisy Ach. Sorting Baseline	0.057	0.019	0.023	0.057	0.023	0.027
	(0.032)	(0.006,0.035)	(0.009,0.039)	(0.033)	(0.007,0.042)	(0.009,0.051)
Strict Ach. Sorting Baseline	0.013	0.010	0.011	0.025	0.011	0.015
	(>.500)	(0.002,0.016)	(0.003,0.019)	(0.078)	(0.004,0.020)	(0.005,0.026)
Age Sorting Baseline	0.036	0.006	0.008	0.009	0.008	0.009
	(0.000)	(0.002,0.013)	(0.002,0.015)	(>.500)	(0.003,0.017)	(0.004,0.015)
Intercept	0.002	0.001	0.001	0.001	0.001	0.001
	(0.158)	(0.000,0.001)	(0.000,0.001)	(>.500)	(0.000,0.001)	(0.000,0.001)

Note: Each column presents the results of a 2-level HLM model with years at level 2 such that each coefficient is the tendency in the average year over 2011, 2013, 2015, and 2017. Segregation is estimated using multigroup *H* index.

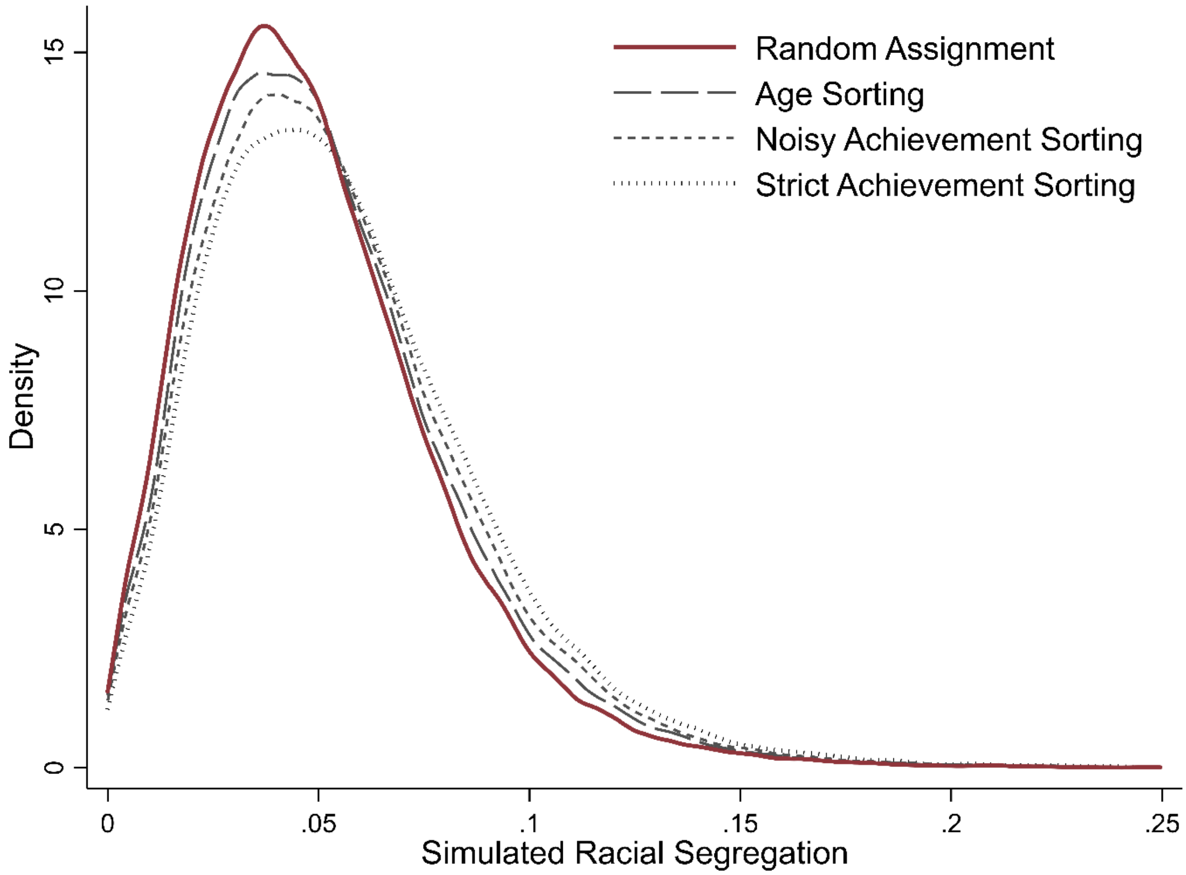
Table 2. *Hierarchical Multiple Regression Models of Classroom Racial Segregation (H), by Grade.*

	(1)	(2)	(3)	(4)	(5)	(6)
Grade 5						
Intercept	0.065	0.065	0.065	0.065	0.065	0.065
	(0.061,0.068)	(0.061,0.068)	(0.063,0.067)	(0.062,0.067)	(0.063,0.067)	(0.062,0.067)
Random Baseline	1.173	--	--	1.129	--	1.081
	(1.108,1.238)	--	--	(0.998,1.261)	--	(0.962,1.200)
Segregation in Peer Shift	--	0.218	--	--	0.026	-0.020
	--	(0.194,0.242)	--	--	(-0.021,0.073)	(-0.061,0.021)
Muni-Year Random Intercepts			X	X	X	X
Variance Explained (%)	16.6	4.7	6.3	19.0	13.4	24.3
# of Observations	5778	5778	5778	5778	5778	5778
# of Municipality-Years	--	--	260	260	260	260
Grade 9						
Intercept	0.048	0.048	0.050	0.050	0.050	0.050
	(0.046,0.050)	(0.046,0.050)	(0.048,0.052)	(0.047,0.052)	(0.048,0.052)	(0.048,0.052)
Random Baseline	1.085	--	--	1.006	--	0.936
	(0.933,1.237)	--	--	(0.851,1.161)	--	(0.776,1.097)
Segregation in Peer Shift	--	0.146	--	--	-0.204	-0.200
	--	(0.124,0.167)	--	--	(-0.279,-0.129)	(-0.263,-0.136)
Muni-Year Random Intercepts			X	X	X	X
Variance Explained (%)	26.7	2.0	8.7	26.1	21.1	34.6
# of Observations	1082	1082	1082	1082	1082	1082
# of Municipality-Years	--	--	160	160	160	160

Note: Each column presents the results of a 3-level HLM model with municipality-years at level 2 and years at level 3 such that each coefficient is the tendency in the average municipality in the average year over 2011, 2013, 2015, and 2017. Each sample is restricted to observations for which segregation in peer shift is observed and municipalities with at least 10 such observations. Variance explained is the percentage reduction in level-1 variance as compared to an empty 2-level model of observations within years. Coefficient variation is in standard deviation units with p-values in parentheses. Segregation is estimated using multigroup H index.

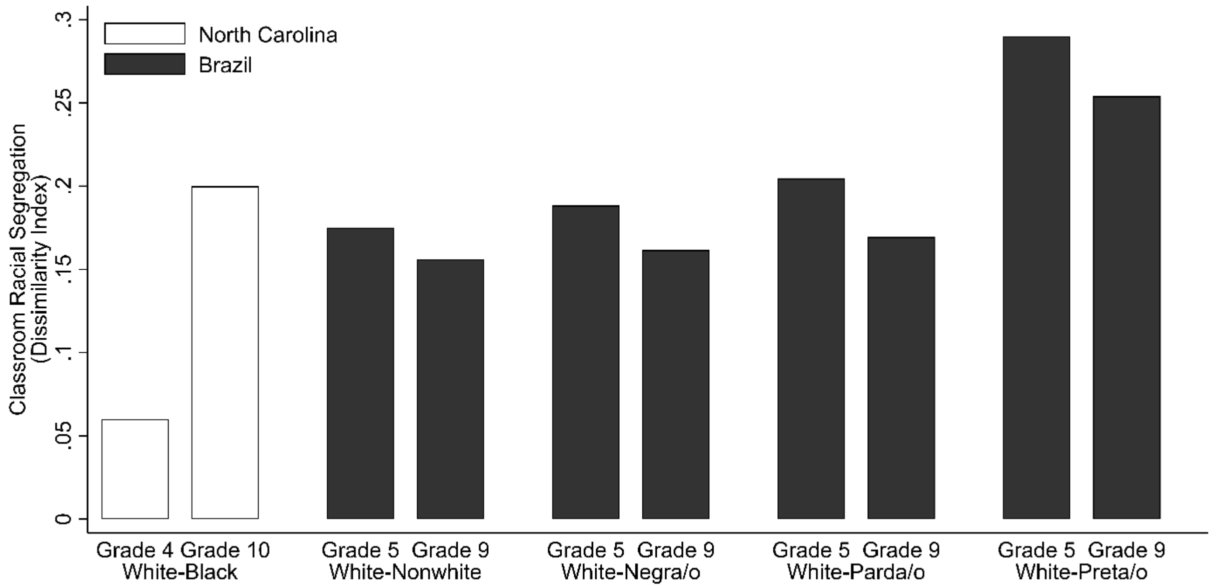
Figures

Figure 1. *Distribution of Classroom-Level Racial Segregation (H) by Simulated Classroom Assignment Processes, Over All Years and Grades.*



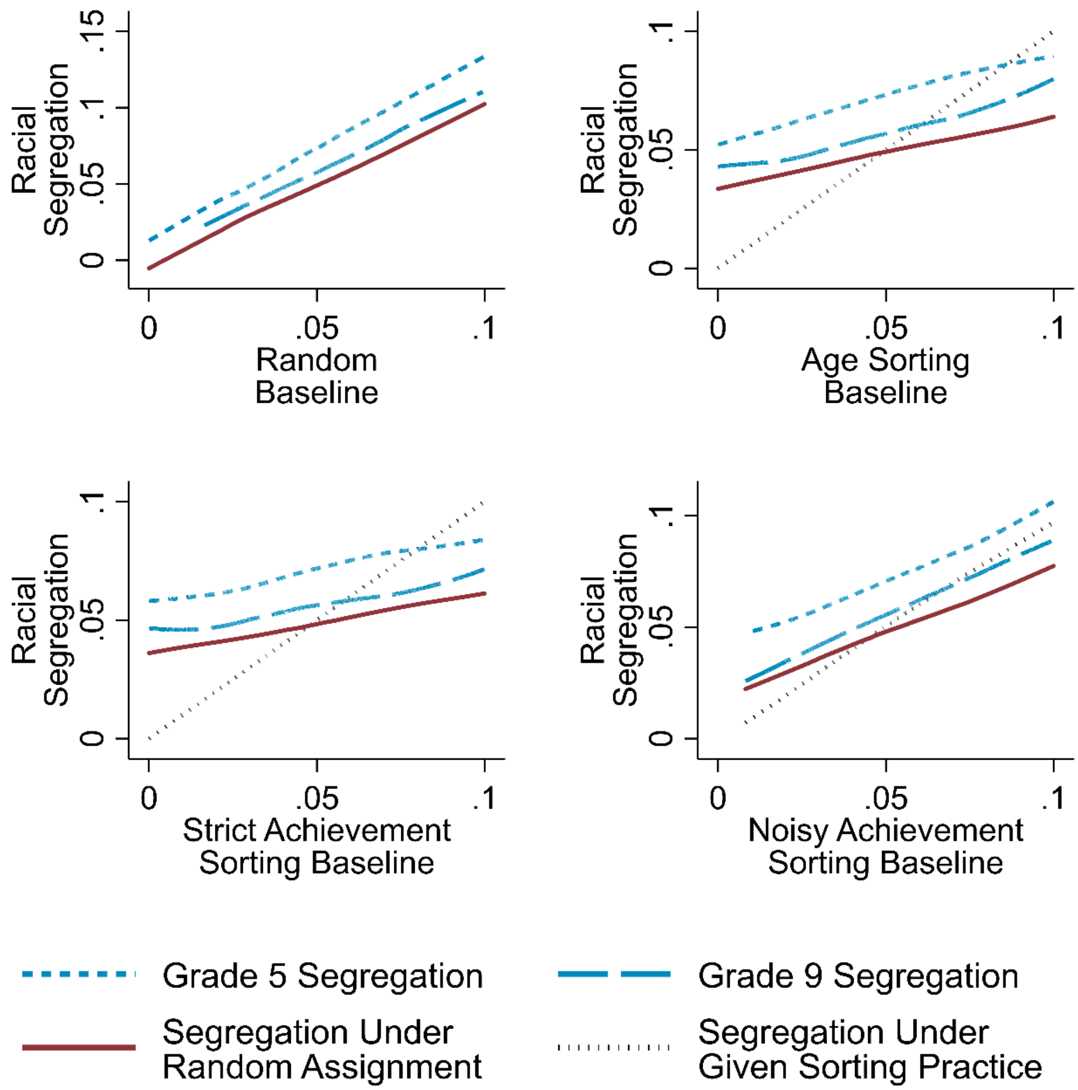
Note: Segregation is estimated using multigroup *H* index. Kernel density plot using the Epanechnikov kernel. Random assignment and noisy achievement sorting lines are each for the distribution of one draw per school-year-grade.

Figure 2. *Binary Classroom Segregation (D) by Race in Brazil and North Carolina in 2017.*



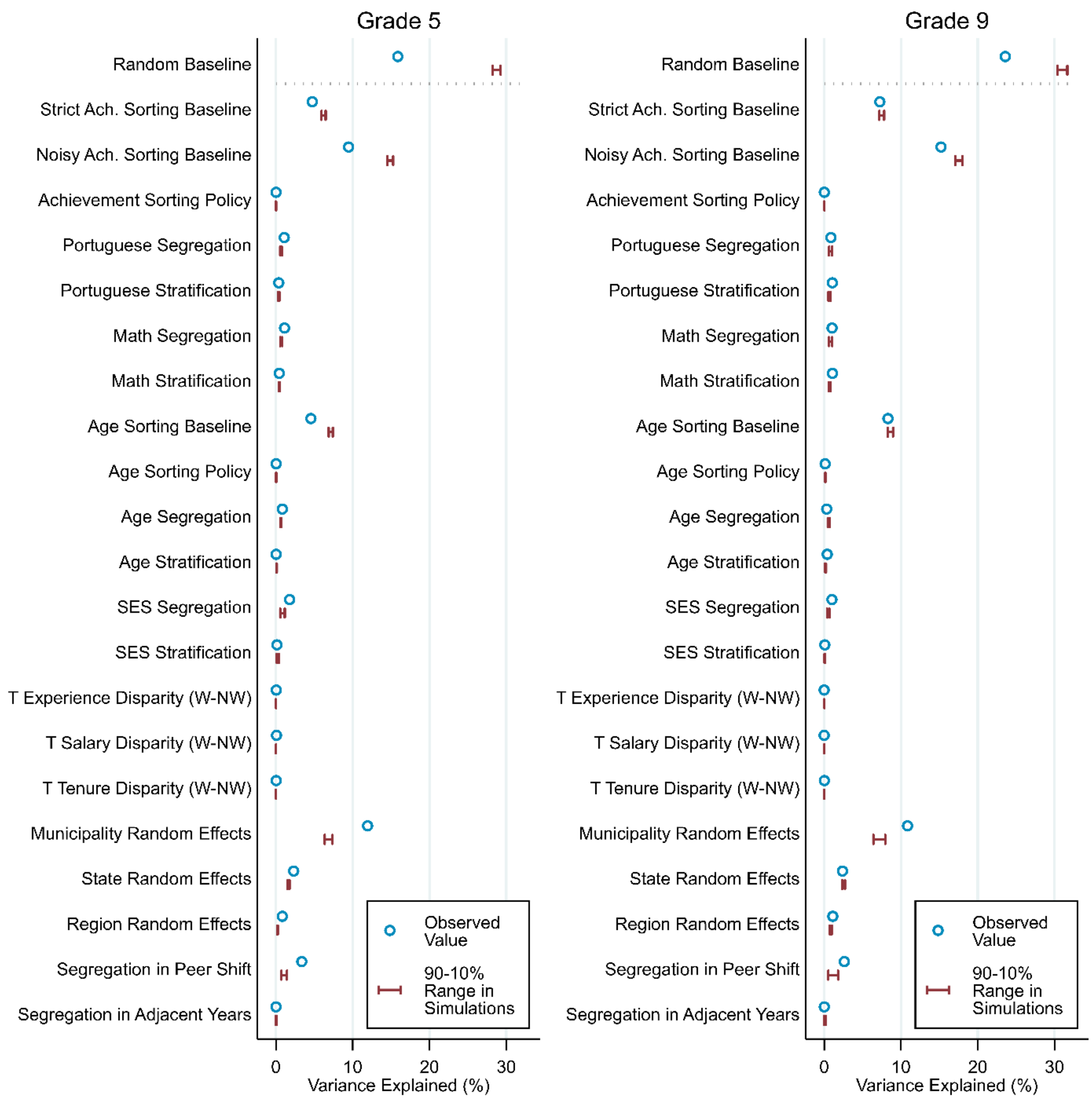
Note: North Carolina estimates from Clotfelter et al. (2020). Segregation estimates use the two-group Dissimilarity Index (D) to replicate Clotfelter et al. (2020). Nonwhite is defined as all students who do not select white. *Negra/o* (roughly, Afro-Brazilian) is a constructed category that combines *Parda/o* (roughly, brown) and *Preta/o* (roughly, black).

Figure 3. *Relationships between Observed Racial Segregation (H), Simulated Random Segregation, and Simulated Sorting Segregation, by Simulated Baseline.*



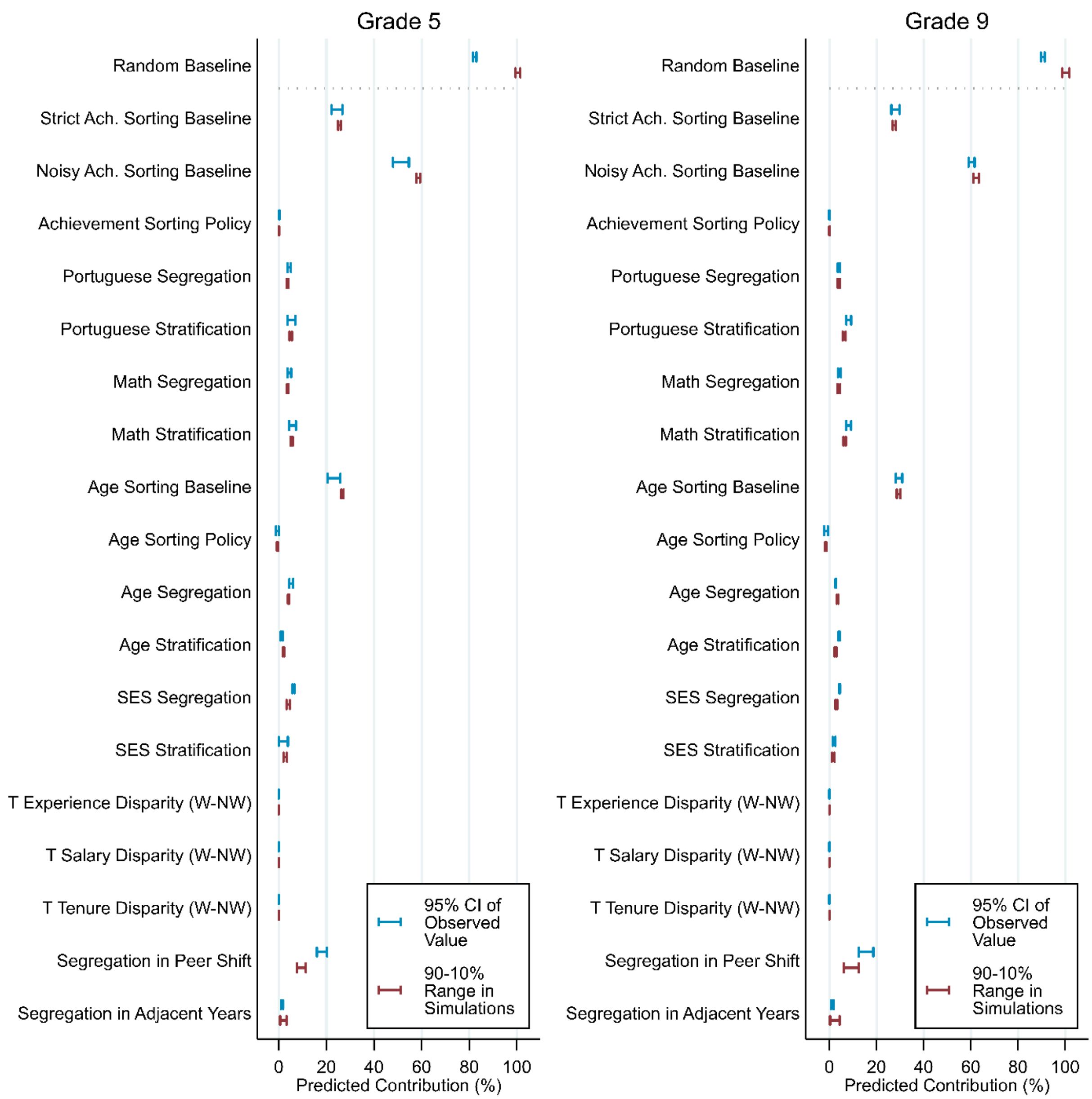
Note: "Segregation under given sorting practice" refers to the sorting practice for which the school-year-grade average estimate, or baseline, is the x-axis in the given plot. Lines are LOWESS lines fit to a 10% random sample of all observations over 2011, 2013, 2015, and 2017. LOWESS lines for simulated random and sorting segregation use both grades. Segregation is estimated using multigroup *H* index. Lines for segregation under random assignment and segregation under noisy achievement sorting are each the set of a single draw per school-year in the grade (LOWESS lines vary little across draws such that plots including lines for all 50 draws per school-year-grade are similar to those using one draw).

Figure 4. *Within-Year Variance Explained (%V) by Predictor, in the Observed Data and When Simulating Random Assignment, by Grade.*



Note: Variance explained is the percentage of within-year variance explained by the predictor. Segregation is estimated using multigroup *H* index.

Figure 5. *Predicted Contribution (%S) by Predictor, in the Observed Data and When Simulating Random Assignment, by Grade.*



Note: Predicted contribution is the amount of segregation that would be attributed to the predictor (as a percentage of the total classroom-level racial segregation in the model sample) if the model results described a causal relationship, contextualizing the size of the bivariate regression coefficient. This is not the actual contribution to segregation as the model does not identify the causal effect of the predictor. Segregation is estimated using multigroup *H* index.

Appendix A. Constructing Segregation Correlates

I measure segregation by achievement, age, and SES as I do segregation by race, operationalizing SES as the student-reported educational attainment of their mothers and fathers using whichever one is greater.

I also use H to measure the racial stratification by each of these characteristics within schools. Racial stratification by a characteristic is the degree to which that characteristic is unevenly distributed across racial groups, indicating the extent to which the distributions within the different racial groups do not overlap. I capture this by measuring racial stratification as the "segregation" of the characteristic across racial groups, as opposed to classrooms. One concern with the stratification measures is that using each racial group could dampen the signal when one group is stratified from the rest. In supplemental analyses, I included stratification measures that used binary race schemes comparing one racial group to all others (e.g., whites vs nonwhites), for each racial group. These analyses, which are available upon request, did not substantively alter the findings.

In addition to the general sample restrictions, I further restrict the samples for analyses using these measures to only include schools in which there are multiple classes with at least 25 percent response rates to the relevant item. Additionally, stratification predictors are only included if the school has students from multiple racial groups.

I measure racial disparities in teacher status by considering teachers' experience, tenure status, and salary, as reported by teachers in *Censo Escolar*. Tenure status is a binary indicator of whether a teacher has tenure at the school. Teacher salary and experience are originally reported in bins. I interpolate a continuous measure by using interval regression to fit a normal distribution y' to the original measure y, giving observations within a bin the mean value of y' when it falls within the same bin. This is the expected value for a randomly chosen teacher given that y is normally distributed.

Given a characteristic, C_{tj}, of teacher t of classroom j, I measure teacher disparities by averaging each classroom's teachers' characteristics then taking the difference in means between whites (W) and nonwhites (NW) in these classroom values:

$$D^C = \frac{1}{W}\sum_j \frac{W_j}{T_j}\sum_t C_{tj} - \frac{1}{NW}\sum_j \frac{NW_j}{T_j}\sum_t C_{tj}. \tag{A1}$$

I further restrict the samples for analyses using teacher disparities to schools in which there are survey responses from math and Portuguese teachers (which may be the same teacher), the relevant characteristic is reported for each teacher surveyed, mean values vary across classrooms, and there are at least five white and five nonwhite students in the school.

One concern with focusing on white-nonwhite disparities is that other disparities could be more important, particularly in schools with few white students. In supplemental analyses, I included teacher disparities measures focused on *pardos*, *pretos*, and students who responded "I don't know". These analyses, which are available upon request, did not substantively alter the findings.

The two predictors capturing school assignment policy are drawn from the same item in the *Censo Escolar* principal surveys, which asks principals how they determine classroom assignments. Possible replies include achievement homogeneity, achievement heterogeneity, age homogeneity, age heterogeneity, other, and none. The measures of achievement sorting policy and age sorting policy are indicators of whether the principals reported achievement homogeneity and age homogeneity, respectively.

Appendix B. Descriptive Statistics

Table B1. *Descriptive Statistics of Schools in the Analytic Sample, Over All Years.*

	Grade 5			Grade 9		
	N	Mean	SD	N	Mean	SD
Racial Segregation	53,452	0.073	0.048	32,068	0.057	0.035
School Characteristics						
# Students	53,452	58.61	25.09	32,068	68.19	31.99
# Classes	53,452	2.42	0.82	32,068	2.49	0.90
Average Classroom Size	53,452	24.01	4.78	32,068	27.02	5.78
% White	53,452	31.70	15.40	32,068	32.96	18.81
% *Parda/o*	53,452	44.03	15.10	32,068	45.39	15.68
% *Preta/o*	53,452	8.66	6.46	32,068	10.18	7.30
% Indigenous	53,452	2.41	3.18	32,068	2.11	2.99
% *Amarela/o*	53,452	2.18	2.45	32,068	3.50	3.10
% Don't Know	53,452	11.01	7.84	32,068	5.85	4.71
Segregation Correlates						
Random Baseline	53,452	0.051	0.016	32,068	0.049	0.016
Strict Ach Sorting Baseline	53,452	0.058	0.034	32,068	0.055	0.032
Noisy Ach Sorting Baseline	53,452	0.055	0.022	32,068	0.053	0.021
Test Score Sorting Policy	52,866	0.051	0.221	31,725	0.036	0.187
Portuguese Segregation	53,435	0.039	0.062	32,044	0.034	0.050
Portuguese Stratification	53,424	0.080	0.054	32,042	0.072	0.049
Math Segregation	53,435	0.040	0.064	32,044	0.032	0.048
Math Stratification	53,424	0.079	0.053	32,042	0.070	0.048
Age Sorting Baseline	53,452	0.052	0.031	32,068	0.051	0.030
Age Sorting Policy	52,866	0.347	0.476	31,725	0.366	0.482
Age Segregation	49,773	0.082	0.098	31,190	0.084	0.114
Age Stratification	49,764	0.146	0.126	31,188	0.115	0.100
SES Segregation	6,684	0.037	0.050	25,210	0.033	0.045
SES Stratification	6,679	0.079	0.062	25,209	0.085	0.066
T Exp. Disparity	16,415	0.055	2.279	5,743	0.034	1.247
T Salary Disparity	13,620	0.003	0.270	4,136	0.003	0.192
T Tenure Disparity	11,444	0.003	0.160	6,482	0.001	0.119
Segregation in Peer Shift	12,228	0.069	0.045	4,030	0.055	0.035
Segregation in Adjacent Years	18,256	0.072	0.045	8,858	0.056	0.033

Note: Students are included in the analytic sample if they responded to the race question. Schools are included in the analytic sample if they are public schools within which all classes in the given grade have at least 75% of students responding to the race item and there are at least two classes. Correlates are missing due to non-response or inapplicability (e.g., if there is only one shift in the school building). Segregation, stratification, and teacher disparity variables are further restricted for comparability (see Appendix A).

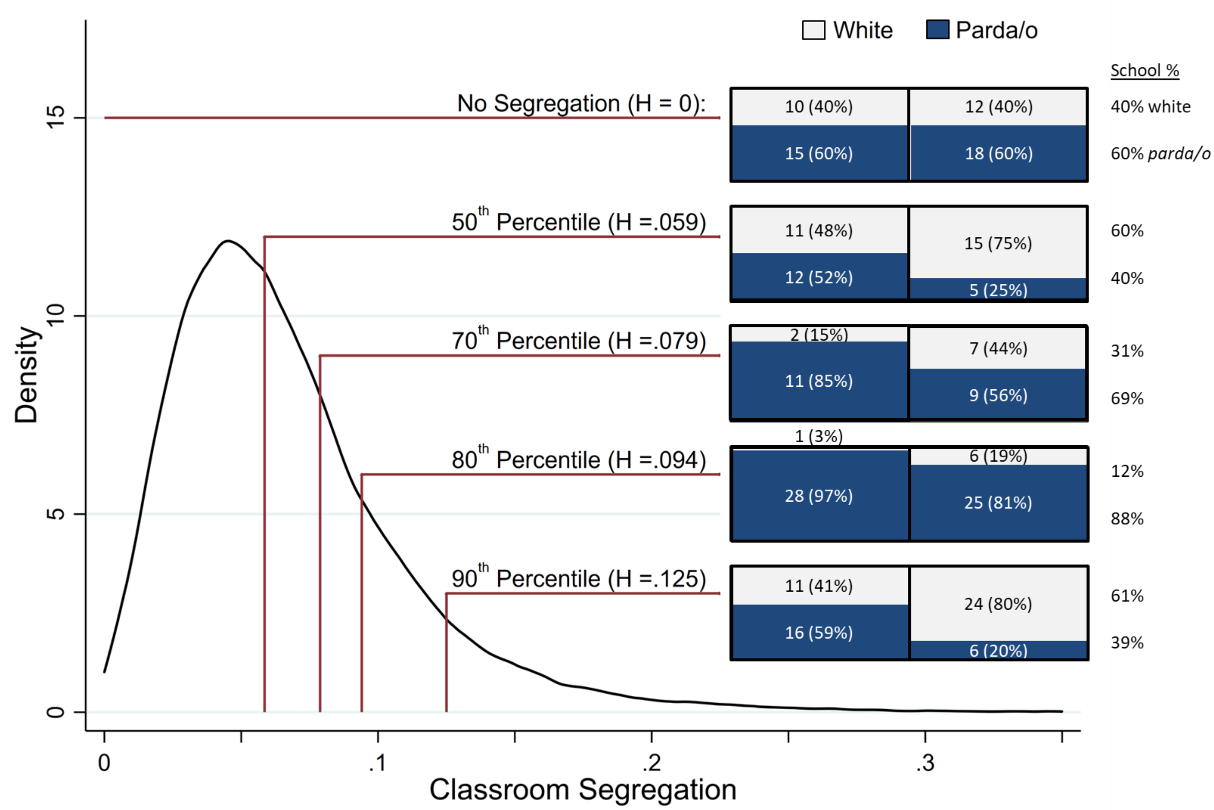
Appendix C. Visualizing Classroom Segregation in Brazil

Figure C1 presents the distribution of segregation over all years and both grades and presents example schools, drawn from the data, to visualize the values of H. Looking at the racial distributions in actual schools offers a sense of how segregated they are. To ease interpretability, I chose only cases with two classrooms, in which all students were white or *parda/o*. First, consider what such a school would look like if there was no segregation, the benchmark case of complete evenness. The top example depicts a hypothetical completely unsegregated school, with two black-outlines squares representing the classrooms within the school in a given grade and with the each classroom filled in blue such that the proportion of the box that is blue equals the proportion of the classroom that is *parda/o* while the remaining gray portion of the box represents the white portion of the classroom. In the class with no segregation, white students make up 40 percent of each classroom as well as 40 percent of the school-grade as a whole while *parda/o* students make up 60 percent of each classroom and of the school. That is, each classroom has proportional representation relative to the school. Conversely, one can think of schools as segregated to the extent that the classroom percentages for either racial group differ from the school percentage for that group.

The median school is noticeably different from the school with no segregation. This school, which actually falls at the 49.9 percentile has an H index roughly equal to the average racial segregation in 9th grade sample. Whereas the first classroom (the one on the left-hand side) is roughly half white and half *parda/o*, the second classroom is only 25 percent *parda/o*. If students had been evenly distributed, each classroom would be nearly 40 percent *parda/o*, so this is a noticeable difference. I leave it to the reader to determine whether this difference is substantial enough to suspect that students, teachers, and others were aware of it.

In the more segregated example schools, it is harder to imagine the racial distribution went unnoticed. In the 70th percentile school, only two of nine white students were in one classroom while the other seven were in the other. In the 80th percentile school, six of seven white students were in one of the two classrooms. The 90th percentile school had more similar total numbers of whites and *pardos*, with roughly 40 percent of the school being *parda/o* and 60 percent being white. Nevertheless, the first classroom had the opposite breakdown: about 60 percent *parda/o* and 40 percent white. The second classroom, on the other hand, has roughly twice the proportion white and roughly one-third the proportion *parda/o*.

Figure C1. Distribution of classroom-level racial segregation, over all years and grades (N=85,520), with example schools.



Note: Segregation in this context can be thought of as the degree to which the percentage white (or *parda/o*) in the classrooms differ from the percentage white (or *parda/o*) in the school, where school percentages indicate the percentage in the school in the grade and year depicted. Example schools are the nearest to the target percentiles given specified parameters to ease interpretability. The "no segregation" school is hypothetical and exists for reference. The actual percentiles are 49.9, 69.4, 79.4, and 91.0. Each example is a two-classroom school with only white and parda/o students, with each black box depicting a classroom with shading proportional to the count of white and parda/o students. Only school observations with segregation less than .35 are depicted in the kernel density plot. The kernel density plot uses the Epanechnikov kernel.

Appendix D. Scale Decomposition

Unlike most segregation measures, the index H is additively decomposable, allowing for the unambiguous attribution of segregation to its within-unit and between-unit components (Reardon et al., 2000; Reardon & Firebaugh, 2002). Given K schools in municipality L, the segregation across all classrooms J in L, $H_{j \subset L}$, is the sum of a between-school within-municipality component, $H_{K \subset L}$, and a within-school between-classrooms component that is the weighted average of the k within-school segregation values $H_{j \subset k}$:

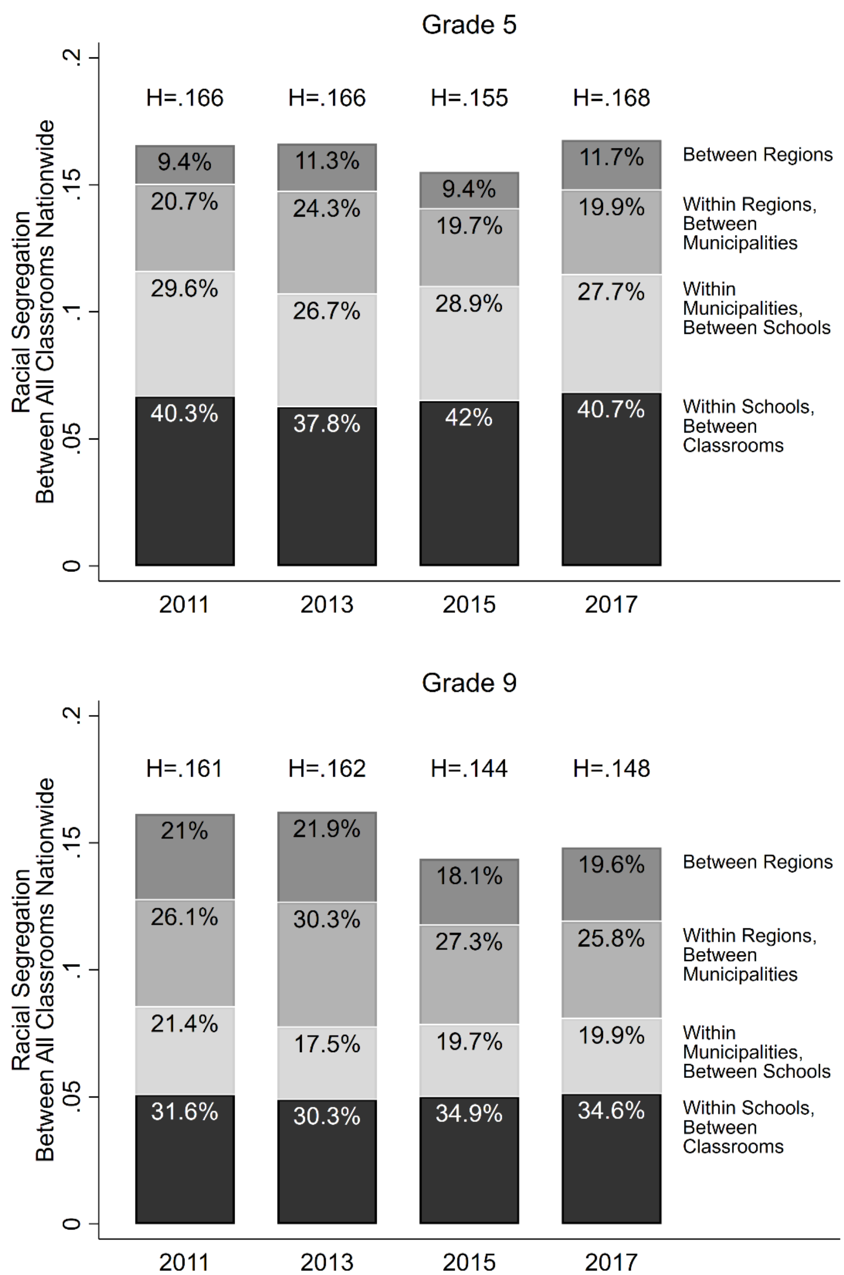
$$H_{j \subset L} = H_{K \subset L} + \sum_{k} \frac{N_{kL} E_{kL}}{N_L E_L} H_{j \subset k}, \tag{D1}$$

where E_{kL} and E_L are the entropy of school k in municipality L and the entropy of the municipality L, respectively, and similarly N_{kL} and N_L are respectively the total student populations of school k in municipality L and of municipality L. Likewise, segregation between classrooms within a state can be decomposed into its between-municipality and within-municipality components, and so on.

I first decomposed the nationwide racial segregation between classrooms into several nested institutional units: regions, states, municipalities, municipalities X administrations (i.e., state schools vs municipal schools within a municipality), school administrations, schools, and classrooms. For simplicity, the analysis collapses units to focus on the institutional boundaries that were found to be most consequential; for example, because shift-level segregation – segregation between schools within school administrations – was minimal in each year and grade, it is excluded below. In each year and grade, the plurality of racial segregation in Brazil's multi-classroom public schools occurs between classrooms in the same school, not traditional suspects like regional differences, municipality differences within regions, or school differences within municipalities. Classroom-level segregation accounts for roughly 40 percent of the segregation in grade 5 and roughly 30-35 percent in grade 9.

However, the data set is limited to public schools. It is unclear how segregated private sector classrooms are or how much segregation occurs between sectors. Brazil is known for its relatively large and disproportionately white private sector, so it is possible Figure D1 overstates the role of classroom-level segregation. One solution is to provide a lower bound on the proportion of segregation that occurs within schools. Suppose the private sector was all-white and every school-grade had multiple classrooms. Given 13-16% private school enrollment in both grades according to *Sinopse Estatistica da Educacao Basica* (Instituto Nacional de Estudose Pesquisas Educacionais Anisio Teixeira, 2011, 2013, 2015, 2017), I simulate the proportion of segregation at the classroom level within each grade and year under this extreme hypothetical. This provides a lower bound estimate of the contribution of classroom-level segregation for all multi-classroom schools. The role of classroom segregation is diminished substantially, but it remains large; in 2011, 2013, 2015, and 2017, the percentage of segregation at the classroom-level in 5th grade would reduce to 28%, 27%, 25%, and 26%, respectively. In 9th grade, the lower bounds are 22%, 21%, 19%, and 19%, respectively. Even under the most extreme assumptions, classroom-level segregation is an important component of the segregation among all multi-classroom schools in both 5th and 9th grade.

Figure D1. *Racial Segregation Decomposed by Segregation Scale, by Year and Grade.*



Note: Total segregation between classrooms across the nation is reported at top.

Appendix E. Which Racial Groups Are Segregated?

Using a multigroup segregation measure captures the racial segregation experienced by more students at the expense of flattening the segregation of particular groups and of particular dyads of groups into a single measure. To better understand how each racial group and racial group dyad contributes to multigroup segregation, I follow Reardon et al.'s (2000) between-group decomposition of H. Given six racial groups A, B, C, D, E, and F, the proportion of multigroup classroom segregation of the six groups, $H^M = H^{A\backslash B\backslash C\backslash D\backslash E\backslash F}$, that is due to the segregation of group A from group B is

$$P^{A\backslash B} = \pi^{A\backslash B} \frac{E^{A\backslash B} H^{A\backslash B}}{E^M H^M}, \tag{E1}$$

where π_{AB} is the proportion of the school population that is in either group A or group B. Similarly, one can compute the proportion of segregation that is due to segregation between group A and all non-A students, in which case $\pi = 1$.

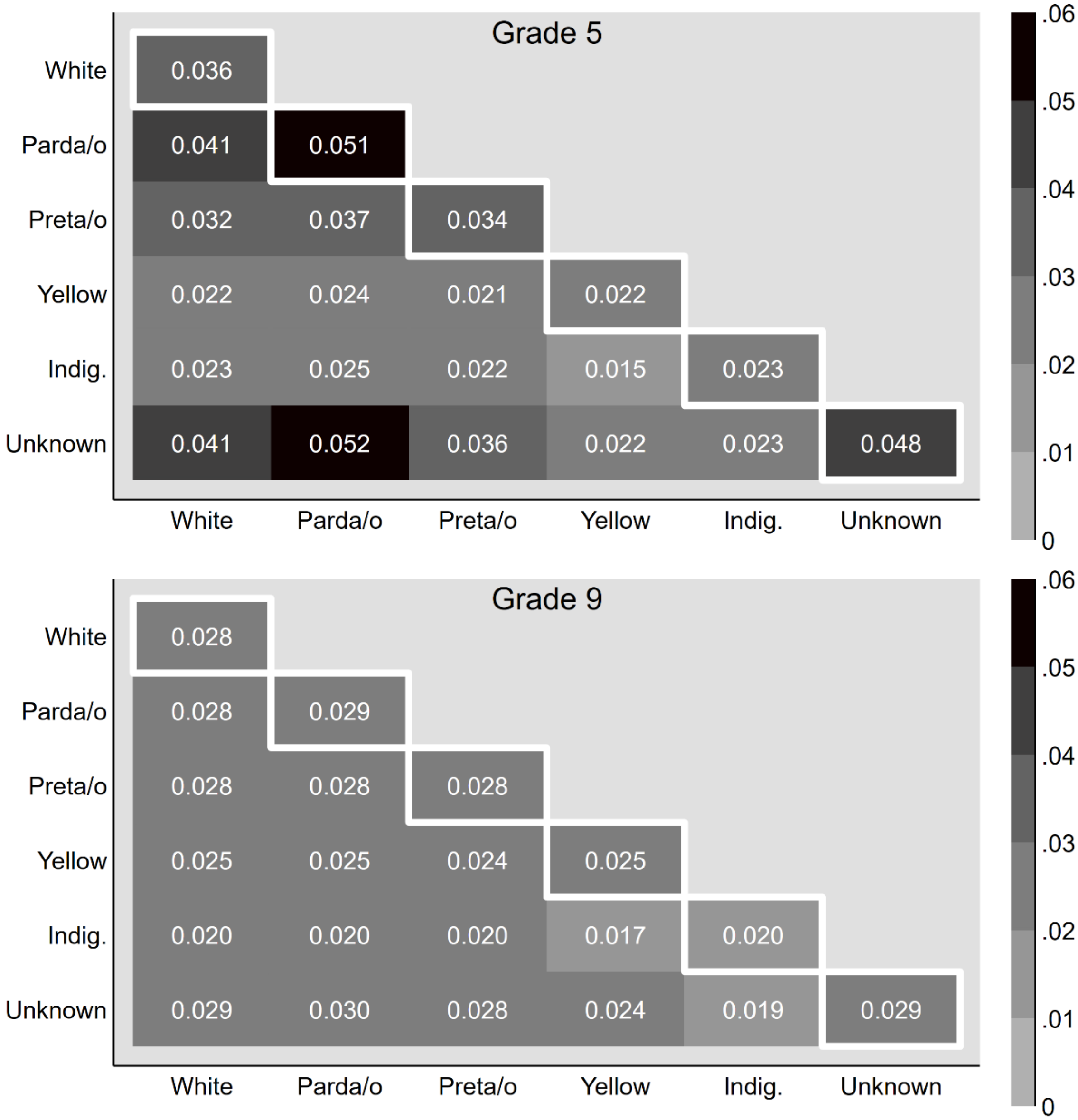
Drawing from Eq. D1, the amount of all classroom segregation in the nation that is due to the classroom-level segregation of groups A and B is

$$H^{A\backslash B} = \sum_{k}^{K} \frac{N_{kL} E_{kL}}{N_L E_L} P_k^{A\backslash B} H_{j\subset k}^{M}, \tag{E2}$$

where $H_{j\subset k}^{M}$ is the multigroup segregation among classrooms j in school k, $P_k^{A\backslash B}$ is the proportion of multigroup segregation due to segregation among groups A and B in school k, $\frac{N_{kL} E_{kL}}{N_L E_L}$ weights segregation by diversity and population, and the sum is taken over all K schools in the nation.

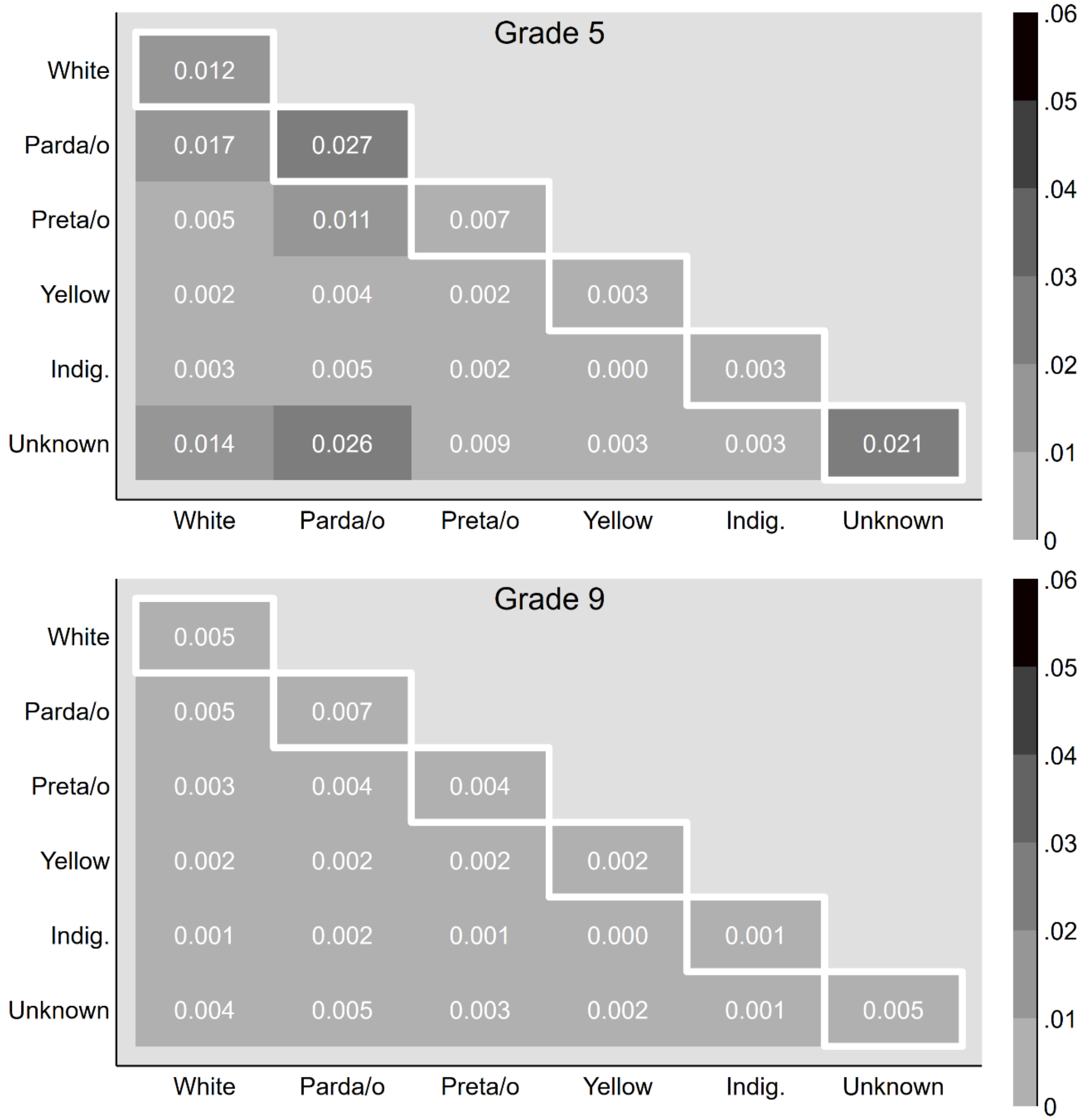
I compute these values in each grade and year for each dyad as well as for each racial group using all other students as the comparison, then average over years within each grade. Note that these values do not sum to the total segregation value (e.g., .166 in grade 5 in 2011) because the segregation the segregations of different groups from one another are not discrete phenomena. Additionally, some of the pattern would occur under random assignment. To isolate the pattern that would not occur under random assignment, I repeat this analysis in simulations using random assignment (N=50), subtracting the average result in the simulations from the observed results. Figure E1 presents the group decomposition without accounting for random assignment while Figure E2 presents them after removing the values under random assignment.

Figure E1. *Dyad-Specific Classroom Segregation Contribution to Total Segregation between Classrooms across the Nation, by Grade.*



Note: White-highlighted boxes on the diagonal refer to segregation between the given group and all others (i.e. the white X white box reports the contribution from white-nonwhite segregation). “Unknown” is used as shorthand for students who responded “I don’t know”.

Figure E2. *Dyad-Specific Classroom Segregation Contribution to Total Segregation between Classrooms across the Nation Net of the Average Value under Random Assignment, by Grade.*



Note: White-highlighted boxes on the diagonal refer to segregation between the given group and all others (i.e. the white X white box reports the contribution from white-nonwhite segregation). “Unknown” is used as shorthand for students who responded “I don’t know”.

Appendix F. Alternative Explanation Analysis

As in the model described in Equation 3, the estimates assessing possible alternative explanations use hierarchical linear models, stratified by grade, in which the set of classroom assignments specific to a school in a given grade and year is nested within years. Each model uses a group-mean-centered predictor X_{it}, describing the classroom assignment i in year t. To assess whether observed coefficients could occur under random assignment, I repeat each model 50 times, each with the values of H_{it} and X_{it} in a simulation of random classroom assignment. I then average the γ_{10} estimates to get a single counterfactual coefficient. If racial classroom segregation is primarily due to chance, these simulated estimates should be similar to the observed data. Note, however, that I do not do this for the teacher disparities predictors because, *a priori*, they have no association with racial segregation given random classroom assignment.

For the place predictors, I assess their role solely by their explanatory power because this captures the extent to which place-specific means vary across places relative to the total variance within years. I use a null model of the classroom segregation of school i within place p within year t with place-year random intercepts u_{0p} and year specific intercepts v_{00t}:

$$H_{ipt} = \gamma_{000} + u_{0p} + v_{00t} + r_{ipt} \tag{F1}$$

$$r_{ipt} \sim N(0, \sigma^2);\ u_{0p} \sim MVN(0, \tau_{00});\ v_{00t} \sim MVN(0, \tau_{000}).$$

To assess the explanatory power of the place-year random intercepts, I report the percentage of total within-year variance explained by adding the place-year level into the model. In other words, σ^2 in Equation 4 is drawn from the model in Equation F1 while σ^2_{null} in Equation 4 continues to be the variance of r_{it} in a null two-level model of assignments within years (i.e. the Equation 3 model).

Table F1. *Fifth Grade Bivariate Relationships between Each Predictor and Racial Segregation in Observed Data and in Simulations of Random Classroom Assignment.*

	Regression Coefficient (γ)		Variance Explained (%V)		Pred. Contribution to Seg. (%S)	
	Observed	Simulations	Observed	Simulations	Observed	Simulations
Random Baseline	1.184	0.996	15.874	28.721	82.298	100.502
(N = 53452)	(1.174,1.195)	(0.985,1.006)		(28.223,29.258)	(81.572,83.024)	(99.422,101.463)
Strict Ach. Sorting Baseline	0.307	0.220	4.734	6.193	24.407	25.454
(N = 53452)	(0.278,0.335)	(0.216,0.226)		(5.927,6.475)	(22.145,26.668)	(24.909,26.083)
Noisy Ach. Sorting Baseline	0.677	0.532	9.463	14.887	51.311	58.582
(N = 53452)	(0.633,0.720)	(0.525,0.539)		(14.529,15.269)	(47.975,54.646)	(57.792,59.344)
Achievement Sorting Policy	0.003	0.001	0.017	0.011	0.209	0.108
(N = 52866)	(0.002,0.004)	(0.000,0.002)		(0.000,0.022)	(0.117,0.302)	(0.050,0.159)
Portuguese Segregation	0.079	0.102	1.085	0.668	4.352	3.664
(N = 53435)	(0.067,0.092)	(0.092,0.111)		(0.550,0.773)	(3.668,5.036)	(3.312,3.973)
Portuguese Stratification	0.052	0.034	0.370	0.371	5.277	4.959
(N = 53424)	(0.036,0.068)	(0.030,0.037)		(0.304,0.446)	(3.647,6.907)	(4.502,5.494)
Math Segregation	0.078	0.102	1.119	0.678	4.381	3.675
(N = 53435)	(0.064,0.092)	(0.095,0.112)		(0.562,0.812)	(3.603,5.158)	(3.413,3.999)
Math Stratification	0.057	0.037	0.431	0.444	5.729	5.433
(N = 53424)	(0.043,0.072)	(0.034,0.040)		(0.373,0.515)	(4.281,7.178)	(4.987,5.837)
Age Sorting Baseline	0.322	0.256	4.560	7.168	23.163	26.745
(N = 53452)	(0.284,0.360)	(0.251,0.260)		(6.877,7.407)	(20.449,25.877)	(26.231,27.167)
Age Sorting Policy	-0.001	-0.001	0.026	0.022	-0.646	-0.574
(N = 52866)	(-0.003,-0.000)	(-0.001,-0.001)		(0.011,0.034)	(-1.222,-0.069)	(-0.781,-0.368)
Age Segregation	0.045	0.035	0.836	0.641	5.200	4.104
(N = 49773)	(0.038,0.052)	(0.032,0.037)		(0.567,0.726)	(4.402,5.998)	(3.836,4.380)
Age Stratification	0.006	0.007	0.026	0.090	1.196	1.946
(N = 49764)	(0.004,0.009)	(0.006,0.008)		(0.067,0.118)	(0.732,1.660)	(1.638,2.242)
SES Segregation	0.132	0.082	1.786	0.844	6.145	3.930
(N = 6684)	(0.122,0.142)	(0.067,0.098)		(0.556,1.150)	(5.678,6.612)	(3.211,4.691)
SES Stratification	0.022	0.021	0.132	0.216	1.901	2.630
(N = 6679)	(-0.000,0.043)	(0.015,0.027)		(0.109,0.367)	(-0.006,3.808)	(1.952,3.384)

	Regression Coefficient (γ)		Variance Explained (%V)		Pred. Contribution to Seg. (%S)	
	Observed	Simulations	Observed	Simulations	Observed	Simulations
T Experience Disp. (W-NW)	-0.000	0	0.043	0	-0.005	0
(N = 16415)	(-0.001,0.001)				(-0.053,0.044)	
T Salary Disp. (W-NW)	0.002	0	0.074	0	0.007	0
(N = 13620)	(-0.003,0.008)				(-0.010,0.024)	
T Tenure Disp. (W-NW)	-0.002	0	0.034	0	-0.007	0
(N = 11444)	(-0.009,0.005)				(-0.028,0.015)	
Municipality Intercepts	--	--	11.941	6.872	--	--
(N = 53452)				(6.359,7.360)		
State Intercepts	--	--	2.308	1.619	--	--
(N = 53452)				(1.528,1.765)		
Region Intercepts	--	--	0.832	0.200	--	--
(N = 53452)				(0.155,0.241)		
Segregation in Peer Shift	0.184	0.097	3.366	1.049	18.119	9.568
(N = 12228)	(0.163,0.206)	(0.078,0.115)		(0.699,1.439)	(16.004,20.234)	(7.672,11.341)
Seg. in Adjacent Years	0.014	0.021	0.009	0.050	1.360	2.054
(N = 18256)	(0.009,0.018)	(0.005,0.032)		(0.000,0.104)	(0.923,1.798)	(0.522,3.207)

Note: Each cell presents estimates from either a single model or several models. All estimates are from HLM models reporting year-average bivariate regression coefficients. Output for observed data show estimates with 95% confidence intervals using robust standard errors. Output for simulated random classroom assignment (n=50) show mean estimates with the 90-10% range of estimates. In the case of teacher disparities, we know *a priori* that there is no association given random assignment. Variance explained is the percentage of within-year variance explained by the predictor. Predicted contribution to segregation is the amount of segregation that would be attributed to the predictor (as a percentage of the total classroom-level racial segregation in the model sample) if the model results described a causal relationship, giving a sense of the size of the estimated coefficient. This is not the actual contribution to segregation as the model does not identify the causal effect of the predictor.

Table F2. *Ninth Grade Bivariate Relationships between Each Predictor and Racial Segregation in Observed Data and in Simulations of Random Classroom Assignment.*

	Regression Coefficient (γ)		Variance Explained ($\%V$)		Pred. Contribution to Seg. ($\%S$)	
	Observed	Simulations	Observed	Simulations	Observed	Simulations
Random Baseline	1.046	0.999	23.592	30.990	90.528	100.197
(N = 32068)	(1.037,1.055)	(0.985,1.015)		(30.393,31.677)	(89.750,91.306)	(98.766,101.806)
Strict Ach. Sorting Baseline	0.288	0.244	7.243	7.509	28.038	27.554
(N = 32068)	(0.270,0.306)	(0.238,0.250)		(7.164,7.811)	(26.287,29.789)	(26.818,28.232)
Noisy Ach. Sorting Baseline	0.645	0.575	15.216	17.525	60.336	62.341
(N = 32068)	(0.632,0.659)	(0.564,0.585)		(17.058,17.991)	(59.073,61.599)	(61.102,63.424)
Achievement Sorting Policy	0.001	0.000	0.008	0.008	0.053	0.028
(N = 31725)	(-0.002,0.003)	(-0.001,0.002)		(0.000,0.012)	(-0.112,0.217)	(-0.041,0.110)
Portuguese Segregation	0.065	0.120	0.880	0.804	4.061	3.998
(N = 32044)	(0.058,0.072)	(0.106,0.133)		(0.632,1.018)	(3.622,4.500)	(3.550,4.454)
Portuguese Stratification	0.072	0.048	1.056	0.671	8.246	6.317
(N = 32042)	(0.063,0.081)	(0.043,0.052)		(0.542,0.803)	(7.232,9.261)	(5.662,6.903)
Math Segregation	0.073	0.120	1.022	0.806	4.275	3.996
(N = 32044)	(0.062,0.083)	(0.104,0.136)		(0.600,1.033)	(3.664,4.887)	(3.478,4.548)
Math Stratification	0.074	0.050	1.073	0.700	8.218	6.405
(N = 32042)	(0.065,0.083)	(0.046,0.054)		(0.607,0.817)	(7.205,9.231)	(5.984,6.961)
Age Sorting Baseline	0.329	0.282	8.300	8.635	29.561	29.382
(N = 32068)	(0.314,0.344)	(0.275,0.289)		(8.278,8.966)	(28.200,30.922)	(28.608,30.150)
Age Sorting Policy	-0.002	-0.002	0.118	0.136	-1.404	-1.550
(N = 31725)	(-0.003,-0.001)	(-0.002,-0.002)		(0.097,0.175)	(-2.208,-0.601)	(-1.807,-1.280)
Age Segregation	0.017	0.039	0.319	0.582	2.714	3.480
(N = 31190)	(0.016,0.018)	(0.035,0.043)		(0.464,0.696)	(2.592,2.835)	(3.146,3.809)
Age Stratification	0.022	0.012	0.395	0.172	4.141	2.578
(N = 31188)	(0.020,0.024)	(0.010,0.014)		(0.121,0.230)	(3.848,4.435)	(2.215,2.977)
SES Segregation	0.075	0.056	1.004	0.525	4.326	2.943
(N = 25210)	(0.071,0.080)	(0.049,0.064)		(0.389,0.672)	(4.073,4.580)	(2.594,3.344)
SES Stratification	0.015	0.010	0.079	0.064	1.999	1.594
(N = 25209)	(0.011,0.019)	(0.007,0.014)		(0.031,0.111)	(1.426,2.573)	(1.078,2.215)

	Regression Coefficient (γ)		Variance Explained (%V)		Pred. Contribution to Seg. (%S)	
	Observed	Simulations	Observed	Simulations	Observed	Simulations
T Experience Disp. (W-NW)	0.000	0	-0.010	0	0.004	0
(N = 5743)	(-0.001,0.001)				(-0.030,0.038)	
T Salary Disp. (W-NW)	0.000	0	-0.009	0	0.001	0
(N = 4136)	(-0.005,0.005)				(-0.029,0.031)	
T Tenure Disp. (W-NW)	-0.006	0	0.018	0	-0.023	0
(N = 6482)	(-0.012,-0.000)				(-0.045,-0.002)	
Municipality Intercepts	--	--	10.857	7.116	--	--
(N = 32068)				(6.404,7.942)		
State Intercepts	--	--	2.381	2.520	--	--
(N = 32068)				(2.342,2.698)		
Region Intercepts	--	--	1.115	0.857	--	--
(N = 32068)				(0.722,0.971)		
Segregation in Peer Shift	0.157	0.095	2.621	1.178	15.545	9.374
(N = 4030)	(0.126,0.188)	(0.062,0.125)		(0.520,1.801)	(12.459,18.630)	(6.146,12.426)
Seg. in Adjacent Years	0.013	0.020	0.009	0.063	1.271	2.029
(N = 8858)	(0.007,0.018)	(0.003,0.044)		(0.000,0.181)	(0.707,1.836)	(0.268,4.328)

Note: Each cell presents estimates from either a single model or several models. All estimates are from HLM models reporting year-average bivariate regression coefficients. Output for observed data show estimates with 95% confidence intervals using robust standard errors. Output for simulated random classroom assignment (n=50) show mean estimates with the 90-10% range of estimates. In the case of teacher disparities, we know *a priori* that there is no association given random assignment. Variance explained is the percentage of within-year variance explained by the predictor. Predicted contribution to segregation is the amount of segregation that would be attributed to the predictor (as a percentage of the total classroom-level racial segregation in the model sample) if the model results described a causal relationship, giving a sense of the size of the estimated coefficient. This is not the actual contribution to segregation as the model does not identify the causal effect of the predictor.

Based on these findings, I perform a multiple regression analysis reported in Table 2. The models without municipality-year random intercepts follow the model described in Equation 3. The models including municipality-year random intercepts use three-level HLM, stratified by grade, in which level-1 is the school-year-grade, level-2 is the municipality-year-grade, and level-3 is the year-grade. I model the racial segregation in school-grade-year i, H_{ipt}, as

$$H_{ipt} = \gamma_{000} + u_{0p} + v_{00t} + (\boldsymbol{\gamma}_{.\mathbf{00}} + \boldsymbol{u}_{.\boldsymbol{p}} + \boldsymbol{v}_{.\mathbf{0}\boldsymbol{t}})\boldsymbol{X}_{\boldsymbol{ipt}} + r_{ipt} \tag{F2}$$

$$r_{it} \sim N(0, \sigma^2); \begin{bmatrix} u_{0p} \\ u_{.p} \end{bmatrix} \sim MVN\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \tau_{00} & \tau_{0.} \\ \tau_{.0} & \tau_{..} \end{bmatrix}\right); \begin{bmatrix} v_{00t} \\ v_{.0t} \end{bmatrix} \sim MVN\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \tau_{000} & \tau_{0.0} \\ \tau_{.00} & \tau_{..0} \end{bmatrix}\right),$$

where $\boldsymbol{X}_{\boldsymbol{ipt}}$ is a set of predictors potentially associated with segregation in school *i* within municipality *p* in year *t*, γ_{000} is the year-average intercept, u_{0p} is a municipality-year-specific intercept, v_{00t} is a year-specific intercept, $\boldsymbol{\gamma}_{.\mathbf{00}}$ is a set of year-average slopes on the variables in $\boldsymbol{X}_{\boldsymbol{ipt}}$, $\boldsymbol{u}_{.\boldsymbol{p}}$ is a set of municipality-year-specific slopes, $\boldsymbol{v}_{.\mathbf{0}\boldsymbol{t}}$ is a set of year-specific slopes, and r_{ipt} is the total within-year error.