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ABSTRACT

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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 black/white classroom segregation in Brazil is greater than recent estimates from North Carolina high schools (Clotfelter et al., 2020). How does race-based 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 a segregation by chance regime in which (1) schools typically assign students to classrooms arbitrarily, producing initial assignments that are sometimes segregated by chance, and (2) schools choose to move forward with the racially segregated "draws" rather than make race-conscious adjustments.

Keywords: Index bias; Random segregation; 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 classroom segregation by race due to the use of tracking and the charged debate surrounding it. A recent study by Clotfelter et al. (2020) measured racial and ethnic segregation within schools and between classrooms (i.e. classroom segregation) and segregation within counties and between schools (i.e. school segregation) for North Carolina's 10th graders in 2017. They report the total white/black segregation, summing classroom and school segregation, to have a Dissimilarity Index score of .52 in math, of which nearly 40% is due to classroom segregation ($D = .20$).

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). Yet repeating the Clotfelter et al. analysis in Brazil's public schools reveals that the total white/black segregation of Brazil's 5th and 9th graders is roughly on par with that of US 4th and 10th graders. Even more surprising is that *classroom* segregation in both 5th ($D = .29$) and 9th ($D = .25$) grade in Brazil is greater than in US high schools ($D = .20$), despite Brazil not using classroom-level tracking. This highlights the possibility that non-tracking school systems are not exempt from becoming highly classroom segregated.

How does race-based classroom segregation occur without tracking, and in a supposed "racial paradise," no less? I contend that this phenomenon is rooted in (1) the ideological and historical differences between the US and Brazil that cause racial segregation to face different barriers to legitimacy in each, and (2) the potential potency of chance as a segregating force when a society is in denial about race's social reality.

The analysis proceeds by describing the extent of racial classroom segregation in Brazil; comparing the observed data to simulated datasets in which students are assigned to classrooms by random assignment, age sorting, or achievement sorting; and estimating associations between classroom segregation and indicators of classroom sorting mechanisms. The findings are consistent with segregation that occurs due to arbitrary assignment rather than the age sorting, achievement sorting, teacher steering, and parent lobbying mechanisms that have been identified in the literature. Racial segregation by chance is congruent with the hypothesis that racial classroom segregation without tracking is made possible in Brazil due to antiracialism and racism denial rooted in the myth of "racial democracy."

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, 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). Another 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 that some call 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 classrooms in North Carolina's elementary schools are often less racially segregated 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 conclusion 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 at the school level. 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 1).

Other scholars consider sorting by age/grade distortion (the discrepancy between a student's age and that expected at his/her grade level). 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 within school administrations – 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 1). 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 secondary mechanisms of segregation under tracking 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, teacher steering and parent lobbying are most likely to occur in non-tracking schools. Grissom et al. (2015) describe the micropolitics of classroom assignment in which teachers compete for particular students, resulting in lower-status students tending to be in classrooms with newer and less effective teachers. Additionally, parent lobbying can also increase segregation, whether because racially privileged parents are more likely to lobby for classrooms (Delany, 1991; Oakes & Guiton, 1995) or because they lobby more successfully due to deference from school administrators (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 segregation by chance (also called small-unit bias, index bias, random segregation, 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 Occurs 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 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 ($r = .75$). The distribution of racial segregation is similar in each condition, with random assignment producing only slightly less segregation than age and achievement sorting. In the average school, the mean racial segregation after 50 random assignment draws is 70% of the observed 5th grade average and 86% of the observed 9th grade average (Table 1). Segregation by chance is potentially a potent source of classroom segregation.

However, this analysis – like the random baselines used in prior studies – does not tell us *whether* substantial classroom segregation occurs by chance. The literature consistently considers segregation net of random baselines to enable researchers to focus on the remaining segregation, positioning segregation by chance as both asocial and inevitable (otherwise removing the random baseline overcorrects in cases with less stochastic assignment). This approach to segregation by chance is useful for certain questions, but leaves gaps in our understanding; I was unable to find any studies that investigate whether arbitrary assignment does – not just *may* – produce substantial segregation in schools or otherwise.

This study departs from tradition and conceptualizes classroom segregation by chance as a social outcome that is impacted by schools' decisions just as segregation from tracking is. Consider a school deciding whether to use race-stratified random classroom assignment (minimizing racial segregation) or to use simple random assignment. In the former case, racial segregation is predetermined and kept low. In the latter, it is an oft-segregating random draw from a set of possibilities based on the school's racial composition and classroom sizes. Even when random assignment is used, schools can choose to have less segregation than would occur by chance; when they "draw" highly racially segregated assignments prior to starting the school year, they can rearrange students to provide a more balanced set of assignments or simply try another draw. Schools choose not to integrate classrooms, so segregation by chance must be understood as a practice to understand classroom segregation.

This perspective is also useful for practitioners and policy makers. I have shown that similarly high levels of racial segregation would occur under random assignment as under age and achievement sorting. Those looking to reduce racial segregation in Brazil's schools will be better equipped knowing not just how much more segregated classrooms are than they would be under random assignment, but also which assignment process is more commonly the culprit.

Legitimacy and Segregation in the US and Brazil

I turn now to considering how the US and Brazilian contexts may shape how classroom segregation occurs. 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). I define a logic as a narrative, drawn from extant cultural norms and myths, that renders a practice recognizable. A "legitimating logic" renders the practice recognizable as a right and proper way of doing things.

In the United States

A hallmark of the 20th century US is the expansion of and subsequent partial disbanding of a nationwide tapestry of policies promoting and enforcing *de jure* racial segregation. Starting with *Brown vs The Board of Education of Topeka Kansas* in 1954, school integration was a crucial site in the decades-long delegitimization of segregationism, and explicit racism more broadly, in the US. Due to school segregation's special place in the nation's relationship to racism, segregationism is a ready explanation for racial segregation along institutional boundaries in education. This makes legitimating logics crucial to sustaining racial segregation in schools; that is, broad tolerance of segregation is conditional upon participants and onlookers recognizing it as occurring due to practices consistent with cultural narratives of acceptable segregation.

This is hardly a substantial barrier to segregation along most institutional boundaries in education because placements in most institutional units are either commodified or ostensibly subject to student/parent agency, fitting dominant narratives in which segregation results from markets, cultural clash, and free choices. The residential segregation that produces substantial segregation across districts or neighborhood schools is construed as the result of “natural antagonism between ‘cultures’” (Nash, 2003) and fair, market forces rather than an intended consequence of government policies (Rothstein, 2017). Segregated friendship networks and cafeteria seating are chalked up to natural cultural differences expressed through student choices, ignoring institutional roles (Thomas, 2005). The primacy of individual choices renders most racial segregation in education as either an acceptable, if undesirable, consequence of respecting fundamental rights or a self-evidently optimal organization of collective preferences.

Classroom segregation is particularly resistant to market, cultural clash, and free choice logics because classroom assignments are explicitly determined by schools, even if student and parent input is sought. Tracking provides a legitimating logic for the racial segregation it produces by framing segregation as an unfortunate byproduct of meritocracy, and this may explain why it is the dominant source of racial classroom segregation in the US.

In parallel, concomitant with the delegitimization of segregationism and overt racism was the transformation from widespread explicit racism to a racism-denying ideology that positions undoing harm as unnecessary intervention (Bobo et al., 1997; Bonilla-Silva, 2006). This has constrained efforts at school integration; for example, in Charlotte-Mecklenburg, North Carolina, one of the nation’s most successful court-ordered desegregation programs was ordered stopped –

against the school district's wishes – on the basis that "achieving diversity [was] not a proper grounds for race-conscious action" (*Capacchione v. Charlotte-Mecklenburg Schools*, 1999, p. 291). This ruling is indicative of a contested space in which race-based educational integration is often pursued as self-evidently legitimate (i.e., the purpose is to "achiev[e] diversity") and this legitimacy is challenged by "reverse discrimination" activists arguing that the US is a post-racial society and framing the consideration of race to redress racism as the real racism. Classroom integration efforts have not caught the attention of "reverse discrimination" activists, presumably because they are not prominent, which may explain why in some cases classrooms tend to be less segregated than would be expected by chance (Clotfelter et al., 2003, 2008).

In Brazil

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, at best, dubious consent) with non-whites than the US colonizers 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 now consider racial democracy an aspiration: the promise of a raceless society (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). Consequently, *de facto* racial segregation is commonly assumed to be epiphenomenal, typically to class. This is the case with respect to housing, though racial residential segregation net of class remains sizable (Telles, 2004). This myth of a race-neutral and racially harmonious Brazil lends legitimacy to *de facto* racial segregation otherwise not readily explained. One might think of this as a legitimating logic-in-waiting, a pre-existing narrative that for many renders racial segregation tolerable regardless of its character.

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, making it improper to make racial ascriptions explicit (Guimarães, 2001; Schwartzman, 2009). Ascriptions to darker racial groups are particularly improper; when ascribing someone in your presence who you see as black, it is polite to instead use a lighter category like *moreno* (Schwartzman, 2009). Brazilians see one another as raced, reliably categorizing photographs into racial groups (Bailey, 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); it denies the existence of race not only as an axis of oppression but as a socially meaningful category. This works against race-based classroom integration by calling into question the appropriateness of school administrators acknowledging color differences among students and explicitly considering those differences when organizing classrooms.

However, 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. 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 logic of equalizing opportunity failed to legitimate race-based college integration despite awareness of stark racial inequities in college-going. Thus, while there are likely teachers, principals, and other school administrators who support proactive racial integration of classrooms as they do of universities, this position presumably faces an even tougher battle because classroom segregation has not been established as a social problem that would legitimate race-based classroom integration.

Given their different ideological contexts, the US and Brazil are likely to have mechanisms of classroom segregation. Whereas racial segregation in US schools is liable to raise suspicion unless it adheres to a legitimating logic like tracking, unexplained segregation in Brazil is likely to be given the benefit of the doubt. While there is some evidence of non-tracking US schools integrating their classrooms, 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 unexplained and unintended racial segregation is accepted by school administrators. 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.

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 5th- and 9th-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 a school is defined as the set of students eligible for assignment to the same set of classrooms (e.g., each shift within a school administration is 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 Table 1 for more detail.) 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.

Measures

Racial Segregation

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 grouped into classrooms that remain together for each subject.

Unless otherwise stated, I measure racial segregation across classrooms using the Information Theory Index. This enables measuring segregation among more than two racial groups and decomposing segregation without bias (Reardon et al., 2000; Reardon & Firebaugh, 2002). The Information Theory Index, denoted H, operationalizes segregation as the degree to which students are unevenly distributed across classrooms given a school's population. Unless otherwise stated, the segregation measures reported here are multigroup segregation measures which simultaneously consider the segregation of all racial groups. 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.

Measuring racial segregation requires measuring race, an inherently fraught task. So as 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* (yellow, similar to Asian), and "I don't know." It is not obvious that this is the ideal approach nor what alternatives would be preferable, so I err toward operationalizing race in a more emic and data-retentive way.

Simulating Classroom Assignments

I simulate classroom assignment under four conditions: random assignment, age sorting, strict achievement sorting, and noisy achievement sorting. Each simulation 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 estimate a baseline level of segregation for each school-grade-year so as to capture the segregation expected under each assignment condition. Random assignment and noisy achievement sorting include random variation. In these cases, I simulate 50 assignments in each school and take the mean to estimate the baseline.

I use random assignment to proxy for the arbitrary segregation condition that would produce substantial segregation by chance. I model it by randomly assigning each student to a classroom in their school with an equal probability of being assigned to each classroom.

Age sorting is a proxy for the process of sorting students based on age/grade distortion. Following the *Prova Brasil*'s wording, I operationalize age in 5th grade as age on the day of the survey and age in 9th grade as age at the end of the year. I rank students by age and sort them into equal-sized classrooms by rank. I randomly assign students whose ages are not observed.

I use both strict and noisy achievement sorting to proxy for the process of assigning students to classrooms based on achievement or perceived ability. I operationalize achievement as the average of *Prova Brasil* Portuguese and math scores. For strict sorting, I rank students on achievement and sort them into equal-sized classrooms by rank. One shortcoming is that scores are taken at the end of the school year. Further, schools may sort by perceived ability rather than achievement. Noisy achievement sorting models aim to address this. In these models, I add classical error to achievement such that the "noisy achievement" has a reliability of $.75$ as a measure of achievement. I then rank students on this measure and sort them into equal-sized classrooms by rank. I randomly assign students when achievement is not observed.

Segregation Predictors

School characteristics indicative of different classroom segregation processes include classroom segregation by age, Portuguese achievement, math achievement, and SES; stratification across racial groups by age, Portuguese achievement, math achievement, and SES; racial disparities in teachers' experience, tenure status, and salary; and principal-reported sorting on age and achievement. For example, if age sorting is driving racial segregation, racial segregation should be positively associated with age segregation, racial stratification by age, and principal-reported age sorting. Racial segregation may also be shaped by tendencies of school administrations or particular places, so I also consider segregation levels in other shifts under the same school administration, segregation of the same school in adjacent years, and municipality, state, and region random-intercepts. (See Appendix A.) Some variables necessitate choices about how to measure differences across races. I report racial stratification findings using stratification among all groups because supplementary analyses show that findings do not differ for stratification of specific groups. I report racial disparities as white-nonwhite disparities because supplementary analyses show that the findings do not differ when focusing on other groups (e.g., *pardo-nonpardo*). These supplementary analyses are available upon request.

Methods

The analysis occurs in three stages: describing the extent of classroom segregation; comparing how the observed data fit random assignment to how they fit other simulated classroom assignments; and comparing the association between classroom segregation and the random baseline to associations with indicators of other classroom sorting mechanisms.

Describing Classroom Segregation

To describe the extent of segregation, I compare Brazil to North Carolina, replicating the procedure Clotfelter et al. (2020) use to describe racial segregation in the US state. I follow Clotfelter et al. by estimating black/white (or *preto*/white) segregation as a population-weighted average of the Dissimilarity Index in places (counties or municipalities) that are at least four percent white and at least four percent black. Segregation is estimated between classrooms within schools and between schools within places, where “total segregation” is the sum of average within-school segregation and average between-school segregation. Whereas Clotfelter et al. look at between-school segregation within counties, I look at segregation within municipalities because there is no county-like unit available. Because Brazilian municipalities are smaller than North Carolina counties, between-school segregation and total segregation in Brazil are lower than they would be if comparable units were used. One drawback to using the Dissimilarity Index for these purposes is that it is not additively decomposable, biasing “total segregation” (Reardon & Firebaugh, 2002). It is unclear if this bias differs between places.

Simulation Analyses

The second stage of the analysis compares classroom segregation by race in the observed data to that in data simulating the four hypothetical assignment processes outlined above, so as to assess whether the data is more consistent with random assignment or with pseudo-tracking assignment. I begin with a graphical analysis, comparing the observed LOWESS associations of racial segregation and each simulated baseline with the associations in the four types of simulations. Patterns differed little among random assignment draws and noisy achievement sorting draws, so the first draw was used.

The graphical analysis is limited because the four baselines are correlated. To disentangle their associations with observed classroom segregation, I estimate 5th- and 9th- grade two-level hierarchical multiple regression models of schools within years, in which the set of classroom assignments specific to a school in a given grade and year is nested within years. Given the set of baselines $\boldsymbol{X_{it}}$ describing the expected segregation of classroom assignment i in year t under each assignment process, I model the racial segregation of the classroom assignment H_{it} as

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

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

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 associations, meaning that they are the means of the 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. Note also that the baselines, $\boldsymbol{X_{it}}$, are not centered such that γ_{00} indicates the predicted amount of segregation when each baseline predicts no segregation. I estimate these models in the observed data as well as in the 50 simulations of random assignment and the 50 of noisy achievement sorting. These estimates offer a picture of what would be observed if classrooms were assigned randomly or by a correlate of achievement.

Regression Analyses

The third stage of the analysis compares racial segregation's association with the random baseline to its associations with a host of predictors, first by estimating year-average bivariate associations and then by estimating year-average multiple regression associations among a set of potential predictors identified in the bivariate analysis.

I assess the estimated associations using metrics which are influenced by both the effects and the prevalence of practices with the goal of describing the pattern of segregation and assessing which potential mechanisms the patterns are most consistent with. This will provide insights into which mechanisms are least and most likely to be major sources of classroom segregation nationwide, helping clarify the big picture. Having little information on schools' practices, I tackle this problem by making use of correlates that are hypothesized causes (e.g., sorting policies), mediators (e.g., achievement segregation), moderators (e.g., achievement stratification), and even effects (e.g., teacher disparities as an effect of lobbying for teachers) of the practices identified in the literature. As in the model described in Equation 3, the estimates 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.

Because the random baseline is mechanically correlated with classroom size and school racial diversity, racial segregation that occurs entirely by chance could also be spuriously associated with other segregation predictors. To assess whether observed associations 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 association. 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. To assess the explanatory power of X_{it}, I report the percentage of total within-year variance explained when adding X_{it} to the model,

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

where σ^2 is taken from the bivariate model and σ_{null}^2 is the variance of r_{it} in a null model that excludes X_{it}.

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 classroom assignment i within place-year p within year t with place-year random intercepts u_{0p} and year specific intercepts v_{00t}:

$$H_{ipt} = \gamma_{000} + u_{0p} + v_{00t} + r_{ipt} \tag{5}$$

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

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 5 while σ_{null}^2 in Equation 4 continues to be the variance of r_{it} in a null two-level model of assignments within years.

To assess the potential impact implied by γ_{10}, I also report what I call the predicted contribution to segregation. This 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. Of course, the estimates are not causal, so the predicted contribution should not be confused with the actual contribution, which is unknown. Instead, the predicted contribution measure contextualizes the estimated associations by weighing both association strength and the prevalence/size of the predictor. Given a school characteristic X_{it}, I compute the predicted contribution as

$$\%S = 100 * \frac{\sum_t \sum_i \frac{N_{it}E_{it}}{N_t E_t} \gamma_{10} X_{it}}{\sum_t \sum_i \frac{N_{it}E_{it}}{N_t E_t} H_{it}}, \quad (6)$$

where 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.

The multiple regression model uses three-level HLM, stratified by grade, in which the set of classroom assignments specific to a school in a given grade and year is nested within municipality-years, which are nested within years. I model the racial segregation of the classroom assignment H_{ipt} as

$$H_{ipt} = \gamma_{000} + u_{0p} + v_{00t} + (\boldsymbol{\gamma}_{.00} + \boldsymbol{u}_{.p} + \boldsymbol{v}_{.0t})\boldsymbol{X}_{ipt} + r_{ipt} \quad (7)$$

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

where $\boldsymbol{X}_{ipt}$ is a predictor describing the classroom assignment *i* within municipality-year *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, $\boldsymbol{\gamma}_{.00}$ is a set of year-average slopes on the variables in $\boldsymbol{X}_{ipt}$, $\boldsymbol{u}_{.p}$ is a set of municipality-year-specific slopes, $\boldsymbol{v}_{.0t}$ is a set of year-specific slopes, and r_{ipt} is the total within-year error.

How Racially Segregated Are Classrooms?

US high schools are particularly known for classroom segregation by race due to tracking. A recent study by Clotfelter et al. (2020) measured racial and ethnic segregation using the Dissimilarity Index, D, within schools and between classrooms (i.e. classroom segregation) and segregation within counties and between schools (i.e. school segregation) in North Carolina. Figure 2 presents a comparison of their findings for white/black segregation among 4th and 10th graders in 2017 to my findings for white/*preto* segregation among Brazilian 5th and 9th graders in 2017 following their procedure (see also table A1 in Appendix B). In Figure 2, the gray portion of the bars is between-school segregation and the black portion is classroom segregation, where the sum is what Clotfelter et al. refer to as "total segregation." Between-school segregation and total segregation in Brazil are likely underestimated here because the Brazilian analysis uses municipalities as the population of interest whereas the North Carolina analysis uses counties.

Overall, Brazil's 5th graders experienced more white/black segregation ($D = .52$) than North Carolina's 4th graders ($.49$) while Brazil's 9th graders experienced less ($.44$) than North Carolina's 10th graders ($.53$). In each case, the number of students who would need to be reassigned in order to balance classrooms and schools is roughly half of the maximum possible. This is despite substantially lower between-school segregation in Brazil; in both grade levels, Brazilian between-school segregation is just over half that of North Carolina (Brazil 5th grade, $D = .23$; North Carolina 4th grade, $D = .43$; Brazil 9th grade, $D = .18$, North Carolina 10th grade, $D = .33$).

Whereas North Carolina's 4th graders are primarily segregated between schools with little classroom segregation ($D = .06$), half of the segregation among Brazil's 5th graders is due to classroom segregation ($D = .29$). Brazil's 9th graders are nearly as segregated as its 5th graders ($D = .25$). In each grade analyzed, Brazil's students are more segregated than North Carolina's high-schoolers ($D = .20$). Classroom segregation also contributes over half of the total segregation in both grades, whereas in North Carolina, it contributes at most 37.7%.

Appendices C and D provide a richer description of the extent of classroom segregation. Appendix C describes the scale of racial segregation in the Brazilian public school system by decomposing the multi-group 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. Appendix D describes how each racial group contributes to multigroup classroom segregation. Each 9th-grade group and dyad of groups contributes similarly to segregation. Multigroup segregation in 5th grade is more driven by segregation of *pardos* and students who responded "I don't know" – particularly segregation between those groups and whites and each of them – and less driven by segregation of Asian and indigenous students. After subtracting random baselines, all 9th-grade estimates are very low while segregation of 5th-grade *pardos* and "I don't know" students – and especially segregation between those groups – contribute more to multigroup segregation.

Random Assignment or Pseudo-Tracking

Is the observed pattern of classroom segregation more consistent with random assignment or pseudo-tracking? Each panel in Figure 3 compares racial segregation under five conditions – the observed value and simulated values using random assignment, age sorting, strict achievement sorting, and noisy achievement sorting – to the simulated baseline for one of the four assignment processes. Thus, in each panel, one line is the observed pattern, one is the pattern for the condition corresponding to the X-axis, and three lines are non-corresponding conditions.

In the random baseline panel, all five lines track similarly. In the age sorting and strict achievement sorting panels, observed segregation has a smaller slope than the corresponding conditions, tracking better with the non-corresponding conditions. In the 5th grade noisy achievement sorting panel, the observed segregation line is not particularly more similar to any one condition, whereas, in the 9th grade panel, it tracks better with the random assignment and age sorting lines. Over the eight panels, the observed lines deviate most from the age sorting and strict achievement lines, tracking more similarly with the noisy achievement lines and, in particular, the random assignment lines. Observed segregation also tends to track less closely with all of the simulation lines in 5th grade due to having a higher intercept.

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. Table 2 attempts to parse this by regressing observed segregation on the four simulated baselines. The 1st and 4th columns present the findings for 5th and 9th grade, 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 other 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 similar increase (110.5% and 91.7%, respectively) in observed segregation. This comports with the near-one associations that would occur under random assignment.

The other baselines have weak associations. Only the strict achievement sorting baseline is significant in 5th grade and only the age sorting baseline is significant in 9th grade. In both cases, the estimated association is about .05, or five percent of what it would be if all schools used the same sorting process as the simulations. These estimates are more similar to what would be observed under random assignment than under pseudo-tracking assignment.

However, the pseudo-tracking baselines are typically more associated with observed segregation than would occur under random assignment. Likewise, the intercepts – particularly in 5th grade ($\gamma = .013$) – are greater than would occur under random assignment. It is also noteworthy that, compared to random assignment, the within-year variance explained by the simulated baselines is less and some slopes vary more over time.

Correlates of Non-Chance Segregation

Simulated assignments imperfectly proxy for actual assignments. There are also classroom segregating mechanisms that are not pseudo-tracking, namely teacher steering and parent lobbying. Schools and their localities may also have different tendencies toward segregating net of demographic context and assignment policies due to preferences for racial segregation or integration. To further assess whether segregation by chance drives the classroom segregation in Brazil, I consider several correlates of non-chance segregation processes.

Bivariate Analysis

I begin by estimating bivariate associations between racial segregation and the set of correlates in the observed data. These associations might occur under random assignment, in which case the relationship would be incidental to the characteristics of students in the school rather than a signal of how segregation occurred. To assess this possibility, I also estimate the associations in simulations using random assignment (n=50).

I contextualize these regression results in two ways: one, explanatory power as measured by the amount of within-year variance explained by a predictor and, two, impact as measured by the percentage of segregation that would be attributable to the predictor if the model described a causal relationship. Figure 4 presents the variance explained by each variable along with the 10th-90th percentile range of the variance explained when simulating random assignment. Figure 5 presents the 95% confidence interval for the predicted contribution of each variable along with the 10th-90th percentile range under random assignment. Importantly, this metric does not capture causality or describe the predictor's true contribution; rather, it provides a sense of how big the estimated association is. Further details are provided in Appendix E.

The strong association observed in the previous section between classroom segregation and the random baseline is also apparent in the bivariate analysis. Under random assignment, this association would be one; yet in both grades the association is statistically significantly greater than one. 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 lower than would happen if all schools used random assignment. Under universal truly-random assignment, the predicted contribution metric for the random baseline is 100%. The metric for the observed data is not far off: 82.3% in 5th grade and 90.5% in 9th grade.

Seven predictors relate to achievement sorting: the simulated strict and noisy achievement sorting baselines, an indicator of whether principals report achievement sorting, classroom segregation by Portuguese test scores, racial stratification by Portuguese test scores, classroom segregation by math test scores, and racial stratification by math test scores. Among them, the baselines and stratification predictors have stronger associations than they would under random assignment. Nonetheless, the stratification predictors explain little of the variation in racial segregation in either grade while the baselines explain meaningful variation in classroom segregation but no more than they would explain under random assignment. The estimated associations for segregation and stratification variables each imply small but potentially meaningful impacts on segregation – as much as eight percent on the predicted contribution metric – but in no cases is the contribution more than two percentage points greater than under random assignment. Likewise, the associations with the sorting baselines imply large contributions to segregation but no more than would occur under random assignment.

Four predictors relate to age sorting: the simulated age sorting baseline, whether principals report using age sorting, age segregation, and the age stratification of racial groups. None explain more variation than they would under random assignment. Additionally, none of the small predicted contributions implied by the estimated associations are more than two percentage points greater than the random baseline.

I also consider the degree to which the classrooms and racial groups in schools are differentiated by SES using SES segregation and stratification predictors and racial disparities in teacher status as measured by teachers' experience, salary, and tenure status. These variables are intended to indicate teacher steering and parent lobbying, though other sorting processes could produce associations between them and classroom segregation. In both grades, SES stratification and teacher disparities have precise, near-zero estimated association with classroom segregation. SES segregation has a stronger association; though it explains little variation in either grade, the predicted contribution is 6.1% in 5th grade and 4.1% in 9th grade. However, this is only 2.2% and 1.6% more than would have occurred under random assignment, respectively.

To assess the role of place, I alternately included random intercepts at three geographic scales: municipalities, states, and regions. The percentage of variance explained indicates how much the mean racial segregation varies across places at a given scale. In both grades, little variation occurs at the state or regional levels, similarly to under random assignment. However, there is substantial variation at the municipal level – about 10.6% of the total variation in 5th grade and 9.7% in 9th grade. This is 4.2 percentage points more than would occur under random assignment in 5th grade, and about 2.5 percentage points more in 9th grade.

Finally, I included two measures to capture whether racial segregation is local to school administrations, by looking at segregation in peer shifts, and/or to the school itself, by looking at segregation in the preceding and following survey years. Segregation in adjacent years is minimally associated with segregation in a given year, as expected under random assignment. Segregation in peer shifts, though, is more associated with racial segregation than it would be under random assignment. The predicted contribution metrics are 18.1% (5th grade) and 15.5% (9th grade), or 8.6 and 6.2 percentage points more than would occur under random assignment. The explanatory power is smaller, though, at 2.3 and 1.4 percentage points more than simulated.

Multiple Regression Analysis

The municipality random intercepts are the only variable that explains substantially more variation than would occur under random assignment while peer shift segregation is the only variable that implies a substantially greater contribution to segregation than would occur under random assignment. Both capture differences in local tendencies and are likely to be correlated both with one another and with the random baseline. To assess whether they account for the random baseline's association with classroom segregation, I consider a multiple regression analysis focused on the schools for which I observe peer shifts.

In Table 3, models 1-3 present bivariate associations using each of the three variables. Model 4 loads the random baseline and municipality-year random intercepts while model 5 loads peer shift segregation along with municipality-year random intercepts. Model 6 is the full model with the random baseline, peer shift segregation, and municipality-year random intercepts. In both grades, the random baseline is consistently associated with classroom segregation across the models, with an association near one. Peer shift segregation has a less robust association; when municipality-year random intercepts are included, the association becomes null in 5th grade and is flipped in 9th. Additionally, while accounting for municipality differences in means explains 6.3% (5th) and 8.7% (9th) of the within-year variation in classroom segregation, adding them to the random-baseline-only model explains little additional variation in either grade.

Results

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 more racially segregated than 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?

Both simulation analyses and regression analyses using observed school features point to segregation by chance as a major contributor. In simulations, random assignment produces levels of racial segregation similar to pseudo-tracking practices like age and achievement sorting. The association between observed segregation and the random baseline is also strong enough that it would account for over 80% of 5th grade segregation and over 90% of 9th grade segregation were it a causally-identified estimate.

I assess the possibility that this association is an artifact of other processes in two ways: simulating alternative approaches to assignment and analyzing the associations between observed segregation and indicators of non-chance assignments. The pattern of observed segregation is more consistent with simulations using random assignment than with pseudo-tracking simulations. Whereas racial segregation is strongly and robustly associated with the expected value of segregation under random assignment, its associations with indicators of non-chance assignment practices are similar to random assignment. For example, the academic segregation that would be expected to accompany racial segregation if driven by pseudo-tracking practices typically has no more association with racial segregation than it would under random assignment. The exception is age segregation in 5^{th} grade, which has a predicted contribution score of 5.2%, compared to 4.1% in simulations using random assignment. However, the score is much greater – 18.8% – in the age sorting simulation (analysis available upon request).

Additionally, racial segregation is chaotic over time; after accounting for their random baselines, two schools with high and low segregation respectively in one year have similar segregation levels two years later. Likewise, segregation levels in peer shifts are not positively associated after accounting for municipal tendencies. This indicates that segregation is not driven by school features that are stable over short periods (.e.g, specific faculty, student composition, organizational culture, community practices, etc.). Classroom segregation is also geographically diffuse; state differences explain only 0.7 percentage points more variation in 5^{th} grade and no more in 9^{th} grade than they would under random assignment.

Yet the evidence is clear that segregation by chance is not the sole source of classroom segregation. The random baseline explains less variation and implies a smaller contribution to segregation than it would if all schools used fully random assignment. Graphical analyses show that there is consistently more segregation in 5^{th} grade than predicted in simulations of random assignment. Multiple regression analysis also shows that simulated achievement sorting in 5^{th} grade and age sorting in 9^{th} grade remain associated with observed segregation after accounting for the random baseline. Though these associations are weak, 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 estimates the association between random baselines and observed segregation levels is significantly greater than one, indicating that some of the non-chance segregation is associated 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.

Limitations

It is possible that flaws in simulations of assignment practices downwardly bias their estimated associations with observed segregation. Random assignment proxies for arbitrary assignment and end-of-year test scores proxy for achievement or perceived ability at the beginning of the year. Simulated assignments 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. However, if these flaws were distorting the overall picture, I would expect different findings in the analysis using correlates of non-chance segregation. For example, if achievement sorting were driving racial segregation, I would expect schools' levels of achievement segregation and racial stratification of achievement to be stronger predictors of racial segregation.

Some correlates of non-chance segregation have high missingness, meaning that the bivariate associations of different predictors are estimated with distinct subsamples of the population of schools. This study looks to identify coarse patterns rather than exact associations, which mitigates against the risk of non-random missingness, but it is still possible that the observed sample is substantially different from the population, which could lead to large disagreements between sample estimates and true associations in the multi-classroom public school population, particularly when samples are small due to non-response. This is primarily an issue for the SES predictors, as students who do not provide parental education information could be in substantially different schools than those who do. It is also a concern when considering segregation in peer shifts, which is primarily missing due to schools without shift systems. In this case, the assumption is that the importance of school administrations implied by the correlation of segregation levels across school shifts is generalizable to schools without shift systems. It is less of a concern for the teacher disparity measures, which have high missingness primarily because the same teachers teach their respective subjects to both classrooms or the teachers in the grade do not vary with respect to the characteristic.

Further, it must be stressed that the analyses do not describe causal relationships. The conclusions I draw about mechanisms of segregation are based on consistency and inconsistency with the patterns expected under known classroom segregation mechanisms. For example, sorting students by achievement may well contribute substantially to racial segregation when implemented; my finding that achievement segregation and racial stratification by achievement have small associations with racial segregation merely indicates that, even if the causal effect is high, achievement sorting is unlikely to be a major contributor to racial segregation nationwide.

Finally, while the data implicate segregation by chance as the primary driver of racial classroom segregation, the findings are not dispositive. Proving segregation by chance would require identifying the causal role of stochasticity in classroom assignments. This is difficult for a number of reasons, including challenges to measuring stochasticity of classroom assignments in observed data and 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?).

Discussion

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: black/white classroom segregation in Brazil is on par with that in the US high schools that have captured researchers' attention, and it appears to occur 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: *only too easily*. Segregation by chance can only be a substantial driver of racial classroom segregation if schools choose to accept unexplained and unintended racial segregation. 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 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 unexplained racial segregation in schools 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. In the US, the strong association between racial segregation and malicious intent works against the possibility of racial classroom segregation by chance, but this may change if colorblind, post-racial discourse becomes further entrenched. At present, economic segregation by chance seems more likely in the US. Economic inequality is often understood in racial terms (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. *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
% Yellow	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).

Table 2. *Hierarchical Multiple Regression Model of Classroom Racial Segregation 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
<u>Year Variation</u>	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 in 2011, 2013, 2015, and 2017.

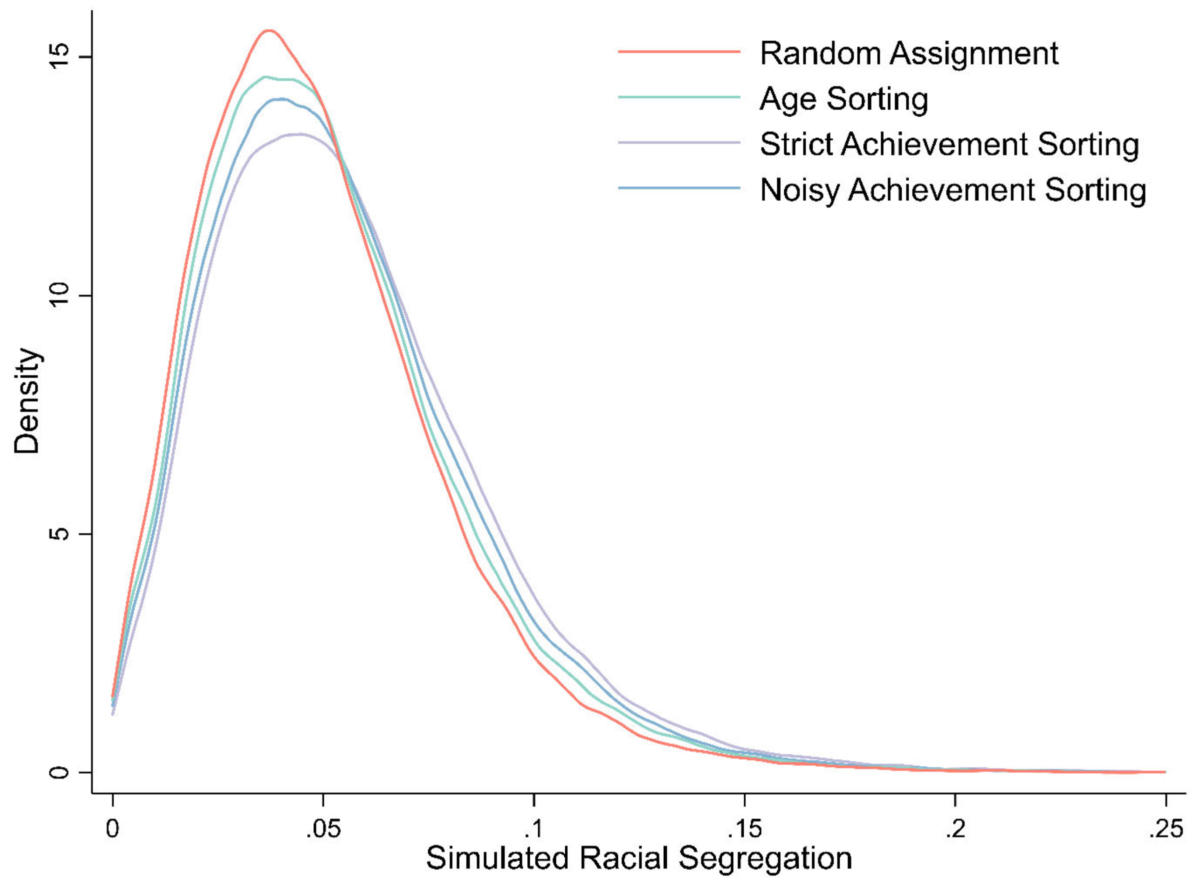
Table 3. *Hierarchical Multiple Regression Models of Classroom Racial Segregation, by Grade.*

	(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 in 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.

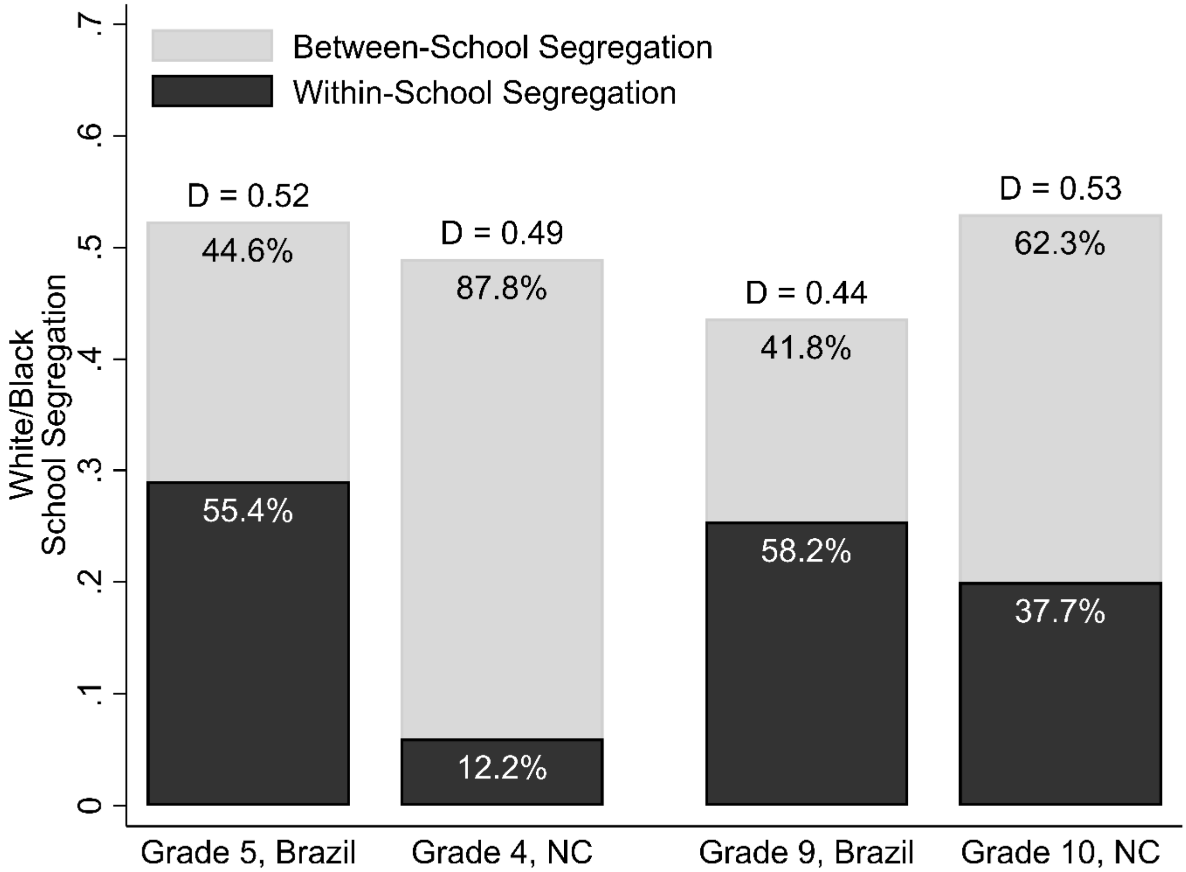
Figures

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



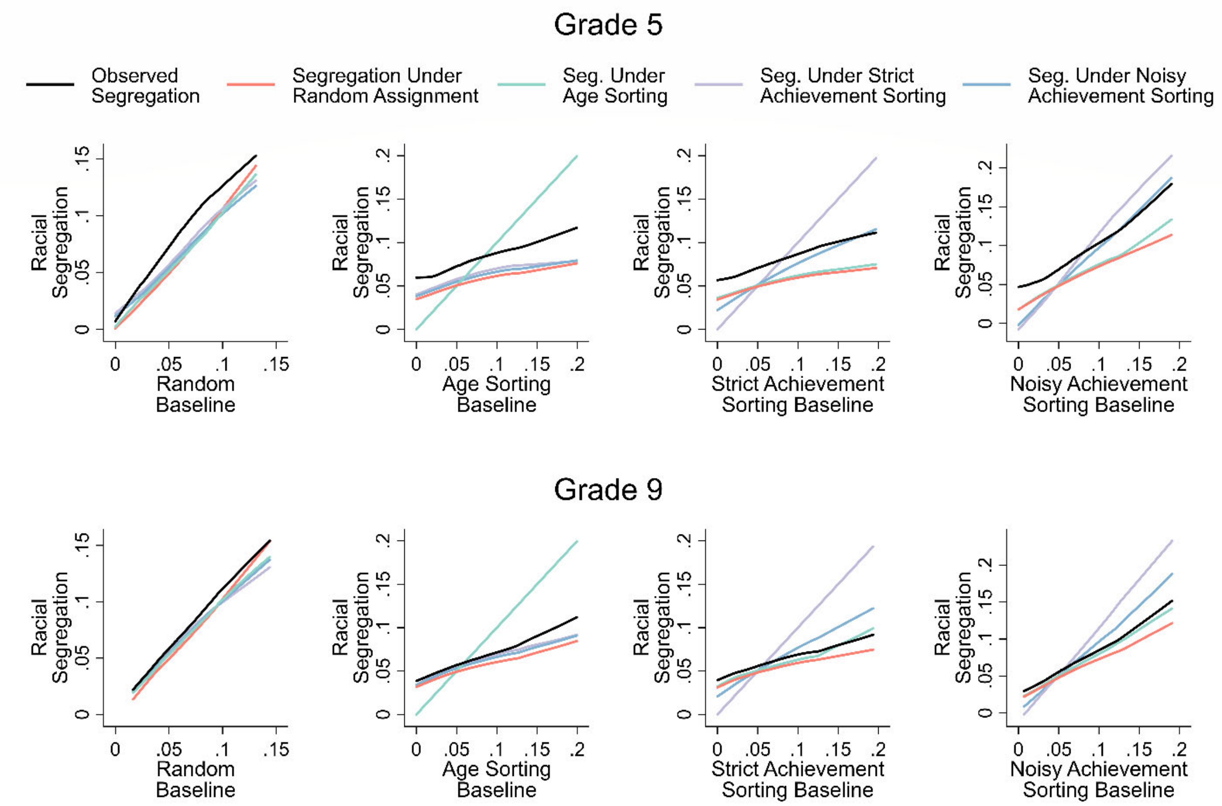
Note: Kernel density plot using the Epanechnikov kernel. Random assignment and noisy achievement sorting lines are each for the distribution of one draw per school-year-grade.

Figure 2. *White/Black Within- and Between-School Segregation in Brazil and North Carolina in 2017.*



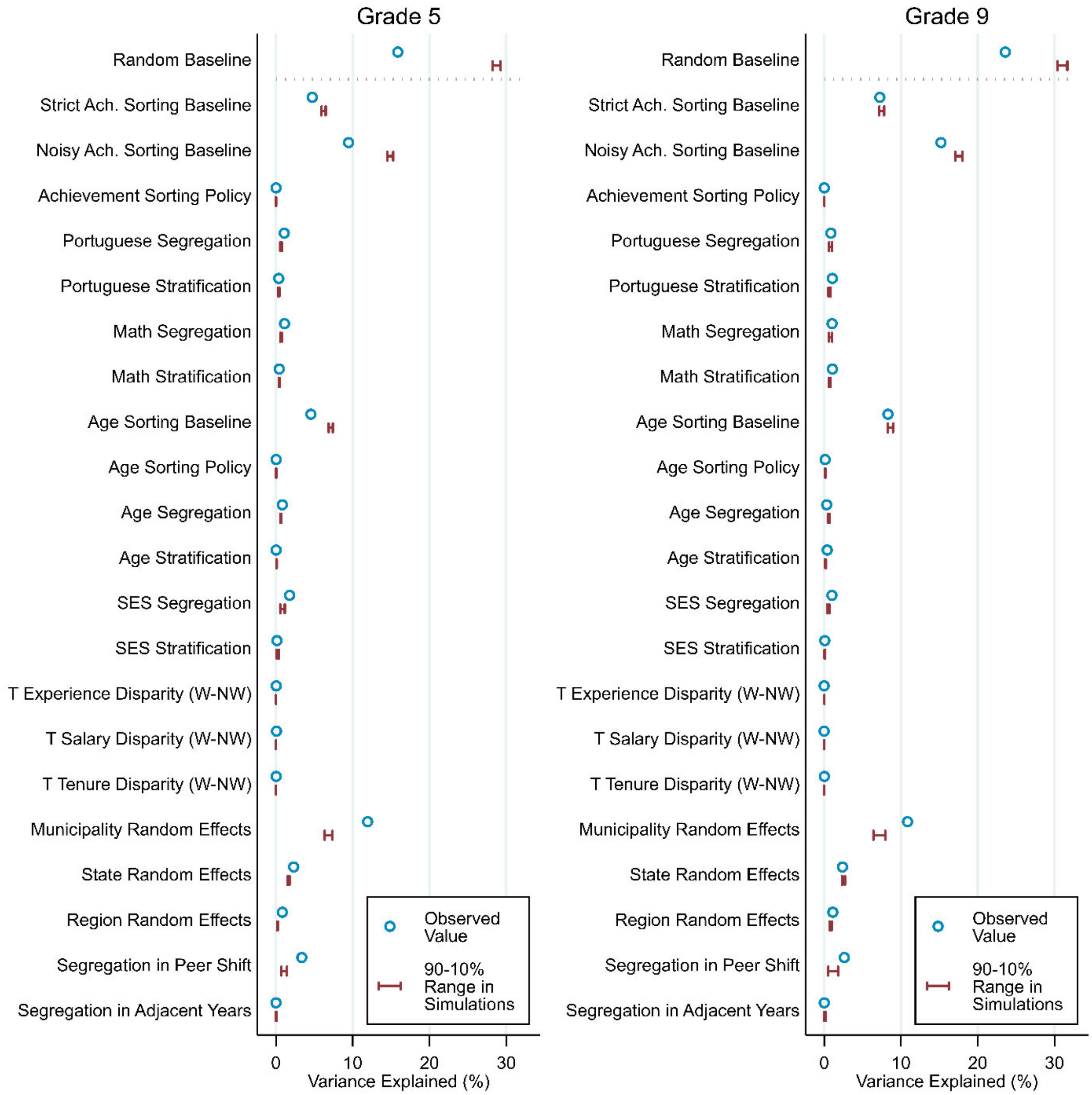
Note: North Carolina estimates from Clotfelter et al. (2020). Segregation estimates use the Dissimilarity Index.

Figure 3. *Relationships between Observed and Simulated Racial Segregation, by Grade and Simulated Baseline, Over All Years.*



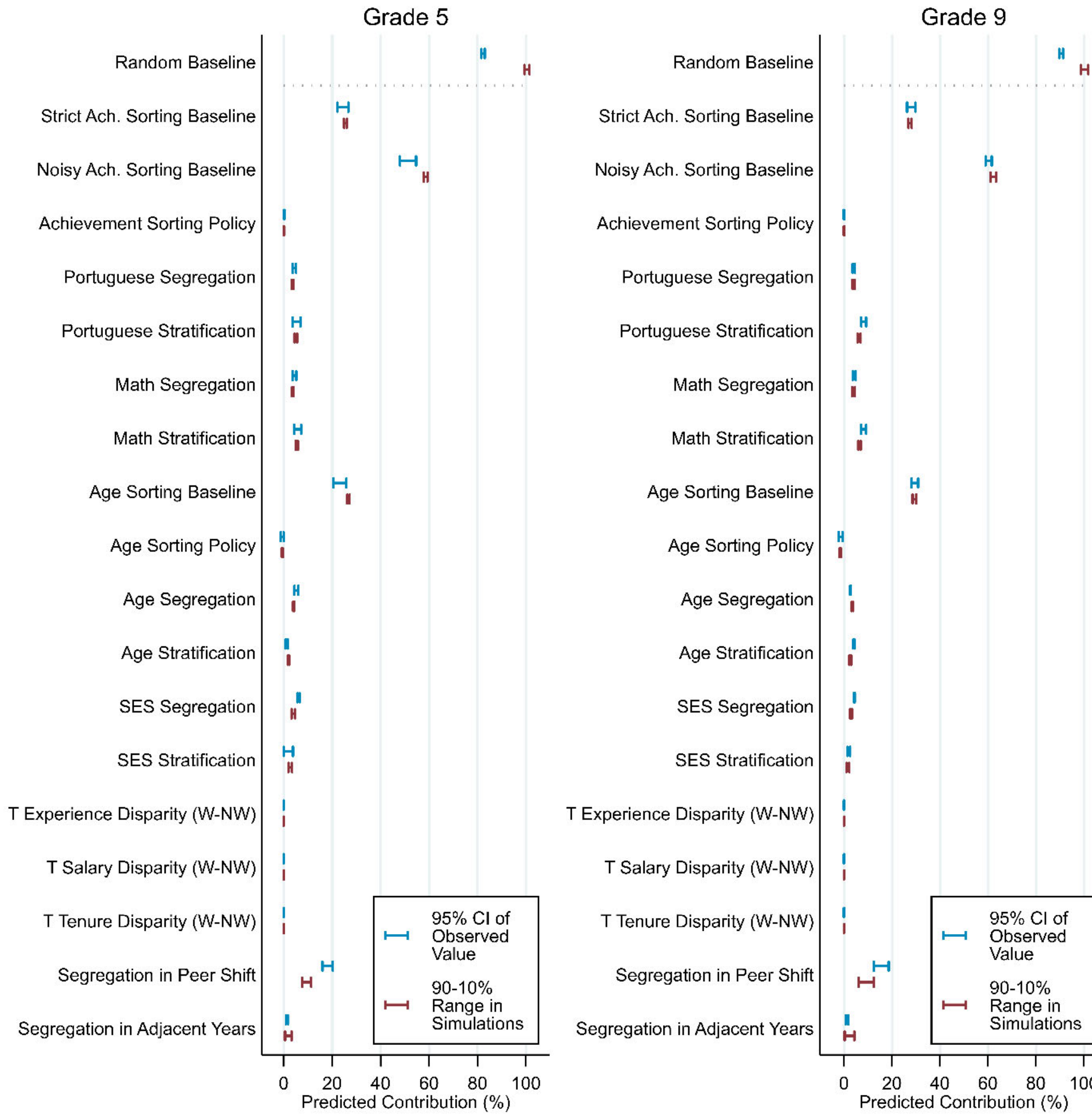
Note: Lines are LOWESS lines. Lines for segregation under random assignment and segregation under noisy achievement sorting are each the set of a single draw per school-year in the grade. LOWESS lines vary little across draws such that plots including lines for all 50 draws per school-year-grade are similar to those using one draw.

Figure 4. *Within-Year Variance Explained 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.

Figure 5. *Predicted Contribution 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, giving a sense of the size of the estimated association. This is not the actual contribution to segregation as the model does not identify the causal effect of the predictor.

Appendix A

Constructing Segregation Correlates

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

We 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. We 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, we 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 our findings.

In addition to the general sample restrictions, we 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.

We 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. We 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, we 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:

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

We 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, we 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 our 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

Brazil-North Carolina Comparison Table

Table A1. White/Black within- and between-school segregation in Brazil and North Carolina in 2017.

	Brazil Grade 5		North Carolina Grade 4		Brazil Grade 9		North Carolina Grade 10	
	D	%	D	%	D	%	D	%
Between-School Segregation	0.23	44.6%	0.43	87.8%	0.18	41.8%	0.33	62.3%
Within-School Segregation	0.29	55.4%	0.06	12.2%	0.25	58.2%	0.20	37.7%
Total	0.52		0.49		0.44		0.53	

Note: North Carolina estimates from Clotfelter et al. (2020). Segregation estimates use the Dissimilarity Index.

Appendix C

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 $H_{j\subset k}$:

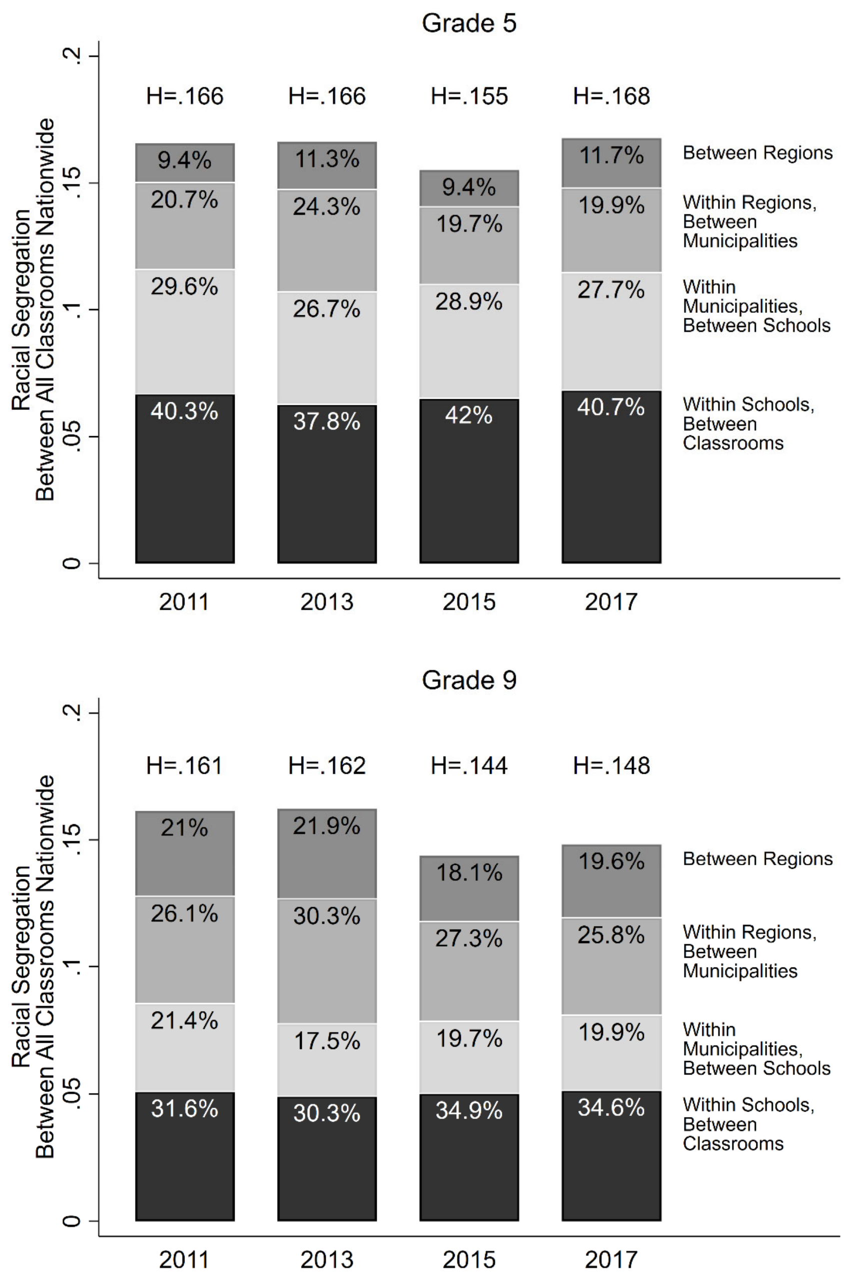
$$H_{j\subset L} = H_{K\subset L} + \sum_{k} \frac{N_{kL}E_{kL}}{N_L E_L} H_{j\subset k}, \tag{A2}$$

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.

We 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, our analysis collapses units to focus on the institutional boundaries that were found to be most consequential. 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, our 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 A1 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), we 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 A1. *Racial Segregation Decomposed by Segregation Scale, by Year and Grade.*



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

Appendix D

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, we 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

$$P^{A\backslash B} = \pi^{A\backslash B} \frac{E^{A\backslash B} H^{A\backslash B}}{E^M H^M}, \tag{A3}$$

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 $\pi = 1$.

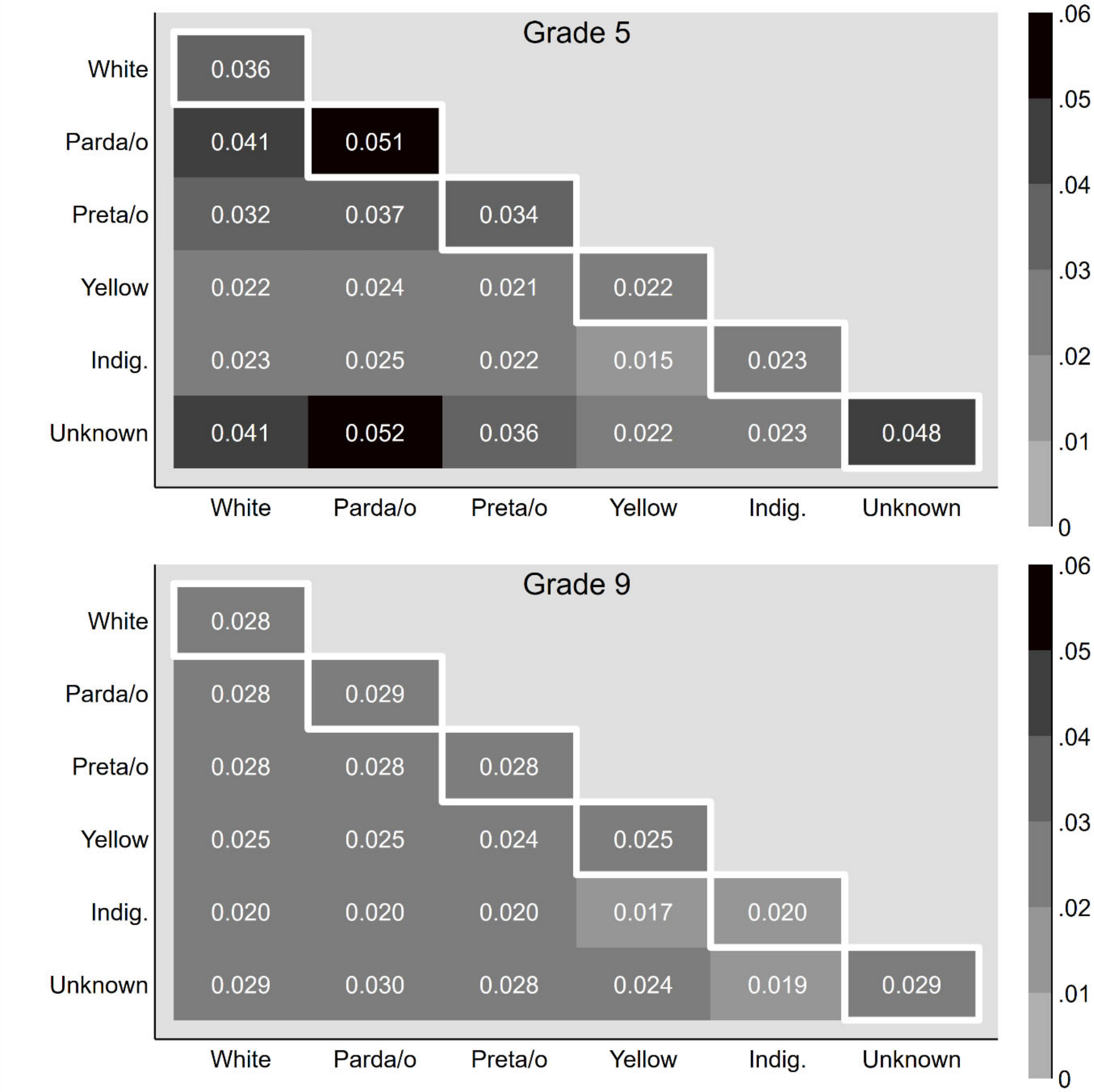
Drawing from Eq. A2, the amount of all classroom segregation in the nation that is due to the classroom-level segregation of groups A and B is

$$H^{A\backslash B} = \sum_{k}^{K} \frac{N_{kL} E_{kL}}{N_L E_L} P_k^{A\backslash B} H_{j\subset k}^{M}, \tag{A4}$$

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.

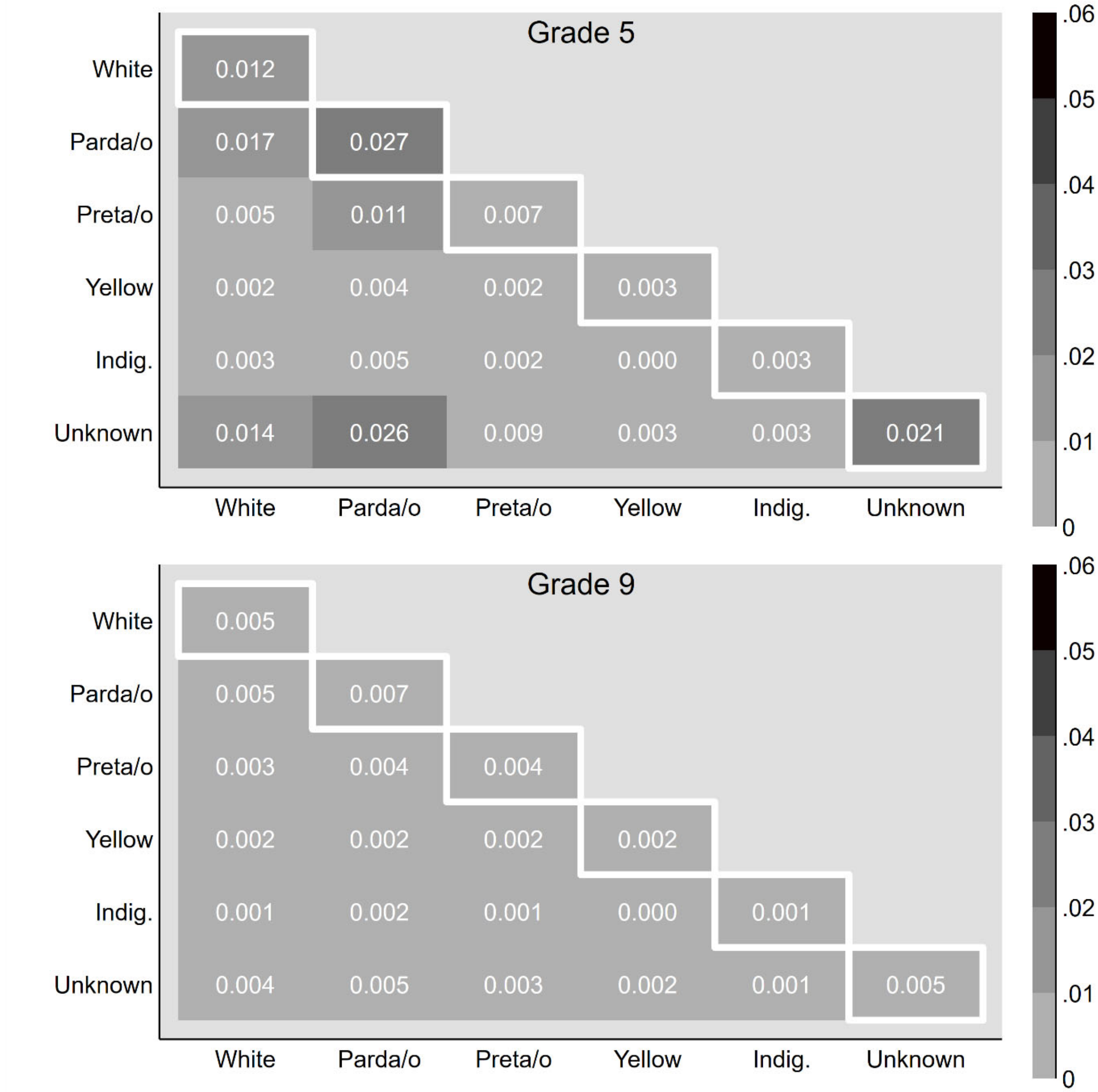
We compute these values in each grade and year for each dyad as well as for each racial group using all other students as the comparison, then average over years within each grade. Note that these values do not sum to the total segregation value (e.g., .166 in grade 5 in 2011) because the segregation 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, we repeat this analysis in simulations using random assignment (N=50), subtracting the average result in the simulations from the observed results. Figure A2 presents the group decomposition without accounting for random assignment while Figure A3 presents them after removing the values under random assignment.

Figure A2. *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 A3. *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 E

Bivariate Association Tables

Table A2. *Fifth Grade Bivariate Relationships between Each Predictor and Racial Segregation in Observed Data and in Simulations of Random Classroom Assignment.*

	Bivariate Association		Variance Explained (%)		Pred. Contribution to Seg. (%)	
	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)

	Bivariate Association		Variance Explained (%)		Pred. Contribution to Seg. (%)	
	Observed	Simulations	Observed	Simulations	Observed	Simulations
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)
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 associations. 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 association. This is not the actual contribution to segregation as the model does not identify the causal effect of the predictor.

Table A3. *Ninth Grade Bivariate Relationships between Each Predictor and Racial Segregation in Observed Data and in Simulations of Random Classroom Assignment.*

	Bivariate Association		Variance Explained (%)		Pred. Contribution to Seg. (%)	
	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)

	Bivariate Association		Variance Explained (%)		Pred. Contribution to Seg. (%)	
	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 associations. 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 association. This is not the actual contribution to segregation as the model does not identify the causal effect of the predictor.