MICROSEGREGATION BY CHANCE: A NEW EXPLANATION FOR RACIAL SEGREGATION WITHIN SCHOOLS

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Acknowledgements
I owe a special thanks to Sebastian Munoz-Najar, David Grusky, sean reardon, Florencia Torche, Filipe Recch, Leonardo Rosa, Martin Carnoy, Aliya Saperstein, and Jeremy Freese. I am also grateful for useful feedback from workshops supported by Stanford’s Lemann Center for Educational Entrepreneurship and Innovation in Brazil, Center on Poverty and Inequality, Sociology and Education Network, and Center for Education Policy Analysis.

Funding
The research reported here was supported by the Institute of Education Sciences, U.S. Department of Education, through Grant R305B140009 to the Board of Trustees of the Leland Stanford Junior University. The opinions expressed are those of the authors and do not represent views of the Institute or the U.S. Department of Education or the Board of Trustees of the Leland Stanford Junior University.

Key Words
Microsegregation – Classroom Segregation – Student Sorting – Racial Segregation – Dynamic Segregation Models
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At small scales, substantial segregation can occur simply by chance. Classroom segregation is a classic case of microsegregation believed to be produced by sorting policies like within-school ability tracking and manipulation by school actors. This paper introduces another mechanism: segregation by chance. I draw on the case of racial segregation between classrooms in Brazil, where sorting is rare, to demonstrate the importance of segregation by chance. Using biennial surveys of the full 5th grade public school population of Brazil in 2011-2015, I show that classroom segregation within schools is, unexpectedly, a greater driver of racial composition differences than segregation between schools, municipalities, and regions. I take a novel methodological approach to measuring how much segregation occurs by chance in the context of dynamic group assignment, a conceptual advancement better suited to organizational settings. I find that segregation by chance accounts for 75% of classroom segregation, or about 30% of all racial segregation in the Brazilian public-school system. In addition to introducing a potent mechanism into the classroom segregation literature, this analysis demonstrates both the importance of microsegregation and the sizable role chance can play in producing it.
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It has long been understood that segregation – the uneven distribution of groups (e.g., racial groups) across units (e.g., classrooms) – occurs under random assignment (Cortese et al. 1976). This segregation by chance (also called random segregation, index bias, expected segregation, small-unit bias, and random unevenness) can be substantial when assignment is highly stochastic and groups or units are small.

Classroom segregation – how the grouping of students for whole-class instruction maps onto status groupings – is a classic case of microsegregation (i.e., segregation at small scales). A robust literature describes the harmful consequences of classroom segregation by race, including disparities in teacher perceptions, teacher qualifications, teaching quality, learning materials, social stigma, and peer influence that exacerbate inequality (Mickelson 2001; Oakes 2005; Watanabe 2008; Oakes and Guiton 1995; Grissom et al. 2015; Thiemann 2018). This literature details mechanisms that are top-down, like tracking and other sorting policies, as well as others that are dynamic, agentic, and responsive, like parent competition over classroom assignments and teacher competition over students (Oakes and Guiton 1995; Grissom et al. 2015; Lewis and Diamond 2015). The latter play out over the course of negotiating beginning-of-the-year classroom allocations and classroom tinkering and switching throughout the schoolyear (Delany 1991).

Though Grissom et al. (2015) and Thiemann (2018) recently found evidence of classroom segregation in US elementary schools, which are typically non-sorting, school systems that rarely track within schools have rarely been considered in research on classroom segregation. This is a substantial blind-spot because within-school tracking is not the dominant manner of organizing classroom assignments. For example, when reviewing international research on
tracking and achievement, Gamoran (2010) identifies only six countries that track within schools. This is partly because many nations sort between schools rather than within them (Hanushek & Woessmann 2006). It is also common for sorting to occur in only some grades, as in US schools. Is there classroom segregation in these non-tracking contexts?

Drawing on the case of classroom racial segregation in Brazilian public schools – where tracking is rare – I find substantial microsegregation. What causes it? Grissom et al. (2015) provides evidence that teachers in a large US school district steer students into classrooms, but there may be other mechanisms as well. Given that classrooms are typically small, it is trivial to show that substantial racial segregation would occur by chance under random assignment, but this is unsatisfying. Schools could secretly sort by characteristics like achievement, retention history, or perceived behavior that are associated with race, in which case the role of chance is merely a theoretical exercise. On the other hand, demonstrating that chance is an important source of classroom segregation – as I believe I show below – can save schools and researchers interested in reducing racial segregation substantial time and effort that would otherwise be spent on intensive reforms aimed at reducing minor sources of segregation. That is, policy, practice, and future research will be best informed by empirically measuring how much microsegregation occurs by chance as opposed to active processes.

In answering this question, it is vital that we account for the dynamism of classroom assignment, allowing for agency and reactivity in our concepts of chance and active segregation. As I noted above, classroom assignment is not a singular, top-down process but rather a continuous process influenced by a range of school actors. Indeed, analyzing segregation in any organizational setting would seem to demand concepts and methods that account for dynamic segregation; to do otherwise would presume no agency. Once we allow for dynamic, agentic, and responsive assignment, a new conceptual problem arises: how
ought we categorize reactions to chance? I present a reconceptualization of segregation by chance that treats reactions to chance as conceptually distinct from the clearly active and clearly chance mechanisms of segregation. As I note below, I do not measure all three distinctly, but this reconceptualization is useful in that it draws out how my measurement choices treat reactions to chance.

To ask how much classroom racial segregation is caused by chance (and, conversely, action), I must set a benchmark expectation for schools. In other words, when asking how segregation would change in the absence of a set of segregation mechanisms, what should be the counterfactual condition? Policy-makers, practitioners, and scholars care about classroom racial segregation out of concern that racial segregation in a racially discriminatory society is likely to have damaging consequences. For this reason, I argue that the benchmark should be no segregation. At bottom, this is a normative claim that schools in racially stratified contexts should be accountable for preventing/undoing not only the racial segregation they intend or that their agents actively create, but also the racial segregation that they allow.

Thus, decomposing classroom racial segregation into chance and active mechanisms demands a methodological approach that empirically demonstrates mechanisms, explicitly engages with dynamic assignment and the possibility of reactions to chance segregation, and sets no segregation as the benchmark outcome of school practices. These considerations demand a new approach to measuring the role of chance in microsegregation.

To date, segregation by chance has not been measured empirically. Instead, scholars have measured how much segregation would hypothetically occur under random assignment either as a falsification exercise or to isolate how much segregation was actively produced (Cortese et al. 1976; Blau 1977; Winship 1977; Carrington and Troske 1997; Allen et al. 2009; Rathelot 2012; Bygren 2013).
Nor have they engaged with dynamic assignment and the complexities raised by reactions to chance. Decomposing the causes of segregation into all three types is data intensive and perhaps imprudent without first presenting evidence that some segregation occurs by chance, whether purely by chance or due to reactions to chance. Prior approaches implicitly muddle reactions to chance with clearly active mechanisms, an approach that is not particularly informative for policy makers, practitioners, and scholars interested in reducing racial segregation. I also make a compromise, but I come down on the other side, muddling reactions to chance with chance-only segregation. This is preferable because segregation due to reactions to chance is a downstream consequence of chance-only segregation. This approach is well-suited to the question of what change in segregation would result from replacing stochastic (or, conversely, race-correlated) assignments with a practice that ensures no segregation.

In treating no segregation as the counterfactual, I set the benchmark expectation for schools at no racial segregation between classrooms. Available methods set colorblind assignment as the benchmark; using segregation under random assignment as the counterfactual compares observed segregation to what would occur if race had no social meaning or correlation with socially meaningful characteristics (Cortese et al. 1976). Using no segregation as the baseline expectation for schools complements this intention-focused approach with a consequence-oriented approach. Perhaps more importantly, it avoids taking a side on whether anti-segregation efforts count as negative segregation. Using available methods to measure how much active classroom segregation occurs in a school system, there are plausible circumstances in which some schools’ efforts to reduce segregation would negate the active segregating that occurs in other schools. In my approach, no school can have negative active segregation.
Applying this new approach yields unexpected empirical findings that have substantial policy implications. In the specific case of racial segregation among public-school 5th graders in Brazil, I show that the public-school system is segregated at a smaller scale than previously believed. In fact, about 40 percent of all segregation occurs within schools between classrooms. This is a surprise and, given the lack of tracking in Brazil, poses a bit of a mystery: how does all this classroom segregation occur? The evidence suggests that most of it occurs due to initial chance segregation and reactions thereto; that is, it is segregation that would not occur without chance assignments. My model estimates that 75% of classroom segregation is attributable to chance, with the most classroom-segregated schools being the most segregated by chance. This implies that 30% of all racial segregation among 5th graders in Brazil’s public-school system – or at least 20% in the whole system, public and private – is microsegregation by chance.

The general implications of these findings are threefold. First, they introduce a currently unappreciated mechanism of classroom segregation – chance – and demonstrate its substantial explanatory capacity in a large nation with over 2,500 distinct school systems. More broadly, the findings demonstrate the importance of thinking anew about segregation at small scales, segregation in dynamic and agentic contexts, and how chance processes structure social interaction. Methodologically, this paper starts forging a new path for assessing the role of chance in microsegregation, using three somewhat idiosyncratic analyses that demonstrate how one might triangulate evidence that segregation occurs by chance and empirically measure how much segregation occurs by chance. By empirically measuring segregation by chance, explicitly accounting for dynamism and agency, and centering considerations of the consequences of segregation rather than the intentions behind it, my approach better informs policy, practice, and future research.
The paper is organized as follows. I begin by reviewing the literature on mechanisms of classroom segregation and explaining how classroom segregation can occur by chance. Drawing on this discussion, I make the case that classroom segregation is dynamic lay out a simple model of dynamic classroom assignment that grounds how I conceptually and methodologically parse the mechanisms of segregation into chance and active sources. I offer a critique of prior ways of conceptualizing segregation by chance and formalize the challenge of empirically measuring segregation by chance when assignment is dynamic. I proceed to describing my data and measures, after which the analysis section describes the scale of racial segregation in Brazilian schools, offers three pieces of evidence that classroom segregation occurs by chance, and estimates how much of the observed racial segregation is microsegregation by chance. I conclude by considering implications for policy and practice in Brazil and beyond, as well as remaining methodological limitations.

What Causes Classroom Segregation?

Established Classroom Segregation Mechanisms

Education scholars are concerned about classroom segregation – how the grouping of students for whole-class instruction maps onto status groupings – because it enables differential treatment within schools, particularly along racial and economic lines (Bowles and Gintis 1976; Mickelson 2001). Researchers have described classroom-level racial and economic inequalities with respect to teacher perceptions, teacher qualifications, teaching quality, learning materials, social stigma, and peer influence (Mickelson 2001; Oakes 2005; Watanabe 2008; Oakes and Guiton 1995; Grissom et al. 2015; Thiemann 2018).

To date, researchers have focused primarily on segregation that occurs when policies like ability tracking sort students by design. This may entail assigning students to a suite of classrooms across many subjects or tracking may be differentiated across subjects to – at least
One subarea of the tracking literature considers whether and why schools are more racially and economically segregated than academic differences predict. Though some studies do not find exacerbated segregation (e.g., Haller and Davis 1981; Haller 1985; Garet and Delany 1988), a substantial scholarship attempts to explain it with consideration to how status influences a dynamic classroom assignment process. These studies show that classroom segregation is influenced by biased assessments of ability, parent competition over classroom assignments, teacher competition over students, and schools competing for the enrollment of advantaged students (Delany 1991; Oakes and Guiton 1995; Watanabe 2008; Grissom et al. 2015; Lewis and Diamond 2015). Altogether, this scholarship argues that, as Oakes and Guiton (1995) put it, “irregularities favor the advantaged” (p.26) when it comes to classroom assignment.

The literature on parent competition is particularly relevant to this study because it demonstrates how responses to a segregation regime can further segregate classrooms by status characteristics like race. Delany (1991) observes a dynamic classroom assignment process with frantic reorganization early in the school year, during which time particularly savvy parents increase their children’s access to high-track classrooms as schools make exceptions to the formal criteria to cope with the realpolitik of allocating resources. Oakes and Guiton (1995) call attention to the segregation-exacerbating effects of parent influence when schools do not make it explicit that they allow parents to lobby for higher track placements, allowing only the most entitled families to have a say. These studies suggest the racial segregation of classrooms may result from racially privileged parents lobbying more for high-track classrooms, while Lewis and Diamond’s (2015) findings show that it may also
result from more successful lobbying. While white parents in the district they analyzed were able to get their children into high-track classrooms with little difficulty, black parents who similarly lobbied for their children were resisted by school personnel.

Parents lobbying and teacher steering may segregate classrooms in non-tracking contexts as well. Recently, scholars have also found classroom segregation in non-sorting US schools (Grissom et al. 2015; Thiemann 2018), raising the specter of classroom segregation after schools have de-tracked. This literature suggests that, even in the absence of strict, formal criteria for classroom assignments, we might nonetheless expect racial segregation.

Classroom Segregation in Brazil

This paper focuses on racial segregation within Brazilian public schools, one of the many school systems in which within-school tracking is rare. This is a fruitful case for three reasons. First, there appears to be little

The literature on classroom segregation in Brazil is limited. Though the common perception is that schools rarely sort students – and that therefore there is no segregation – there are two recent studies suggesting informal tracking by academic performance that enables substantial disparities in access to experienced, full-time teachers. Bartholo and da Costa (2014) find evidence of informal tracking 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 da Costa (2014) find substantial shift segregation – segregation between schools within school buildings – by race and class that results from selecting students into shifts according to age/grade distortion, which results
from delayed school entry, stopout, and retention. This study raises the possibility that informal tracking occurs at more granular scales, like classroom segregation.

A study by Mariana Leite reported by Instituto Unibanco (2017) identified 426 Brazilian elementary schools with substantial classroom segregation by test scores, finding that higher-performing classrooms are assigned more experienced, full-time teachers than lower-performing classrooms in the same school and grade. This raises the question of whether there is a system of informal tracking within Brazilian schools. Although fewer than five percent of 5th grade principals reported sorting classrooms by achievement in 2015, 33 percent reported sorting on age/grade distortion, echoing Bartholo and da Costa’s (2014) findings (INEP 2011-2015a,b). However, in the grade (5) and years (2011-2015) I analyze, these reported practices have little to no relationship with classroom differences in student achievement, age, or race within schools (see Appendix TBD). Thus, it is unclear whether informal tracking causes classroom segregation by test scores in Brazil, but it is potentially one way in which racial segregation is actively produced within schools.

Chance as a Mechanism of Classroom Segregation

Classroom segregation by race can also occur by chance in non-tracking schools. When schools do not have policies determining which students are grouped together to form classrooms, arbitrariness in how students get grouped – for example, when assignment is based on the alphabetical order of surnames – can produce substantial segregation due to the small size of classrooms. 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).

Fig. 1 uses simulations to demonstrate how classroom and racial group sizes moderate how much racial segregation tends to occur under random assignment. For a sense of magnitude,
consider that white and black students in US schools are generally thought of as highly segregated. The median within-district between-school segregation of US 5th graders was .065 in 2015 (Stanford Education Data Archive (SEDA) 2017). For common classroom sizes (e.g., 20-30 students), substantial segregation is expected by chance.

Nonetheless, small unit size does not ensure racial segregation by chance. Schools could prevent segregation by stratifying assignment by race. Or classroom assignment might occur more dynamically; for example, schools could reassign students when classrooms happen to be racially segregated beyond some threshold of acceptability. This could occur within moments of initial assignment – the first set of classroom allocations to be considered by any school actor – as when an administrator divides students by surname, immediately notices this happened to unevenly divide students by race, then reshuffles a few students to better balance the classrooms. Or it could occur over long stretches, as when initial assignments are made prior to the school year but it isn’t until the schoolyear has already begun that teachers notice, call attention to, and successfully problematize the racial differences such that the school takes steps to balance the classrooms.

Preventing and undoing classroom segregation would require a school leadership that observes race, prefers its even distribution, and believes it is legitimate to act on that preference. In Brazil, the ideology of racial democracy works against this by presenting Brazil as a society that is not organized along racial lines (Freyre 1946; Fry 2000; Bailey 2009; Guimaraes 2001; Telles 2004). The antiracialism component of racial democracy construes it as improper to make racial ascriptions explicit, especially ascriptions to darker racial groups (Guimaraes 2001; Schwartzman 2009). Consequently, school leaders may question the appropriateness of acknowledging the extent of color differences among students and explicitly considering those differences when organizing classrooms, resulting in
colorblind classroom assignment practices that do not address segregation by chance.

Appendix A discusses the Brazilian context in detail.

Active segregation may also occur in addition to segregation by chance. Some schools may sort by design while others use arbitrary assignment, but even within the same school active and chance segregation can cooccur. For example, lighter-skinned students may be more likely to seek out or be sought out by influential teachers, partially shaping classroom assignments that are otherwise arbitrary. When it comes to initial assignments, these actions could complement chance in causing segregation, but they would also reduce the role of segregation by chance by lowering the stochasticity in assignments. That is, the combination of chance and active segregation at initial assignment is zero-sum but not all-or-nothing.

I noted above how dynamic classroom assignments allow for responses to segregation such that segregation by chance may elicit desegregating actions that partly negate it. I also described how a subarea of the tracking literature describes how tracking elicits responses that exacerbate racial segregation. Perhaps active segregation can also be a response to segregation by chance. For example, we might think that white students (and their parents) prefer whiter classrooms and at some threshold of segregation those in less white classrooms are sufficiently incentivized to whiter classrooms. It may be that, when segregation by chance is large enough to induce some students to change classrooms, this increases segregation beyond other students' thresholds such that even more switch classrooms, and so on, in a several-stage dynamic process (i.e. “tipping”) (Schelling 1971).

**Dynamic Segregation**

The Classic Literature on Dynamic Residential Segregation
This section seeks to draw out the above analogy to mechanisms residential racial segregation. Explanations of residential segregation in the US can be split into two strains. On the one hand, scholars focus on government policies to show how segregation is the result of intentional, top-down actions like segregated public housing, racial and economic zoning, and discriminatory government mortgages (e.g., see Rothstein 2017). This approach is vital to efforts to show legal culpability and call upon the government to redress residential segregation.

A complementary literature details the many mechanisms of residential segregation that are agentic, dynamic, and responsive (Charles 2003). Racial segregation has been exacerbated by actors, such as real estate agents steering their clients (Massey & Denton 1993; Yinger 1998; Hanson & Hawley 2011), homebuyer preferences that can be directly or indirectly racial (Massey & Denton 1993; Bader & Krysan 2015; Krysan et al. 2009; Bruch & Mare 2006; Xie & Zhou 2012), and racially-restrictive covenants limiting who could purchase homes in a neighborhood (Jones-Correa 2000). It also occurs because residents’ means – social as well as financial (Krysan et al. 2014) – of acquiring neighborhood membership are racially stratified. This research is complemented by a classic literature modeling segregation as a dynamic process, like Schelling’s (1971) tipping model and Sampson’s (2012) concepts of neighborhood social reproduction and the “looking-glass neighborhood”, which explain how neighborhoods can get locked into feedback loops contingent upon initial conditions and the preferences of agents such that segregation is an emergent consequence of prior segregation. Though it might be tempting to misread this classic literature as negating government culpability, instead it ought to be read as enriching our understanding of top-down mechanisms by considering how people respond to spatial inequality. Segregated public housing, racial and economic zoning, and discriminatory government mortgages segregated
US neighborhoods both directly and through downstream consequences in which they shaped and accelerated these more agentic processes.

Dynamic Classroom Segregation

Though I’ve found that people often picture classroom segregation as the consequence of a single, formal, intentional, top-down assignment process, the literature suggests that classroom assignment is also dynamic, agentic, and responsive – even in the context of formal tracking. Grissom et al. (2015) describe the micropolitics of classroom assignment in which teachers compete for particular students when deciding who will teach whom in the coming school year, resulting in lower status students being steered into classrooms with teachers who are more novice, newer to the school, or believed to be less effective. In light of this, the initial classroom assignment can be thought of as a proposal that merely begins a negotiation process that presumably ends with a decision on how classrooms will be divided at the beginning of the school year.

Even after these decisions are made, classroom assignments remain somewhat unstable; Delany’s (1991) rich description of school scheduling and the allocation of students, teachers, and curricula highlights the tinkering that alters classroom assignments after the school year begins, reaching its peak in the first two weeks but continuing throughout the year. As I noted above, this tinkering can be shaped or even initiated by parents competing for classroom allocations they believe will favor their children (Delany 1991; Oakes & Guiton 1995; Lewis & Diamond 2015). Their preferences and perceptions guide how they respond to classroom segregation, potentially enabling dynamic feedback loops like “classroom tipping,” in which classroom racial segregation motivates white flight which motivates more white flight, or “looking-glass classrooms,” in which classroom racial segregation leads people with racial
biases to perceive a difference in competence that motivates attempts to get reassigned to
whiter classrooms.

**Parsing Dynamic Segregation into Chance and Active Mechanisms**

This paper asks how much racial segregation in Brazilian schools is due to chance versus
active mechanisms, whereas prior analyses of segregation by chance which have asked what
minimum amount of active segregation would have to occur to produce the observed amount
of segregation in a given context. This is an important distinction in part because the latter
implicitly discounts the importance of chance segregation, perhaps reflecting a narrow focus
on intention. But segregation is consequential regardless of its cause, not least because
interracial contact is believed to enable positive cross-race interactions (Allport et al. 1954,
Dovidio et al. 2003), including interracial contact due to classroom heterogeneity (Moody
2001). One might also consider the long list of disparities associated with ability tracking:
teacher perceptions, teacher qualifications, teaching quality, learning materials, social stigma,
and peer influence (Mickelson 2001; Oakes 2005; Watanabe 2008; Oakes and Guiton 1995;
Grissom et al. 2015; Thiemann 2018). While academic sorting and curricula differentiation
likely exacerbate these segregation consequences, at the root of each is classroom-level status
hierarchy, which may well occur when classrooms are segregated by race or another status-
laden characteristic. Thus, while intention should not be discounted, making it the sole focus
of segregation studies dismisses socially consequential segregation. This is particularly
important in non-tracking contexts like Brazil where my findings suggest doing away with
classroom sorting policies would do little to alter patterns of segregation.

Rather than asking whether or by how much some number of candidate segregation
mechanisms effect segregation when they are present, I take a holistic approach that asks how
much segregation is due to broad categories of mechanisms: actions and chance. This is a
useful first cut that centers policy impact over average causal effects, though the two are admittedly interlinked. By parsing classroom segregation in Brazil into chance and active mechanisms, this study guides future research and policy toward the questions and changes in practice that promise to be most fruitful. By showing that substantial segregation occurs by chance, this study provides suggestive evidence that small policy changes can yield large reductions to racial segregation in the Brazilian public school system while redirecting attention to questions currently outside the purview of classroom segregation research (e.g., Why don’t schools undo unintended segregation? How are chance, sorting, and agent-driven segregation experienced differently?).

A Simple Model of Dynamic Classroom Assignment

How should we think about parsing active and chance segregation when segregation is dynamic? Fig. 2 diagrams potential segregation mechanisms when classroom assignment is dynamic. This model lacks in detail due to the dearth of research on classroom assignment and segregation within Brazilian schools, but it can still be useful as an abstract guide for thinking about parsing dynamic segregation into chance and active components.

Segregation processes are divided into three stages. The first stage concerns the bases for initial assignments. These formal and informal criteria may be systematically linked to race (e.g., achievement, age-grade distortion, assessed behavior, parent demands, etc.) or they may be as arbitrary as the alphabetical order of surnames or however a backroom administrator happens to haphazardly bunch a set of names into smaller groups. Note that some uses race-correlated criteria can limit segregation, as when schools stratify assignments by race or race-correlated characteristics to proactively balance classrooms, but because the counterfactual condition is no segregation the use of these criteria adds less active segregation into the system rather than negating segregation. Also, these approaches to assignment are zero-sum,
as indicated by the arrow showing a negative association. The initial assignment is then the set of classroom allocations based on these criteria at the first moment assignments are considered by a school actor. At this timepoint, segregation is clearly due to some combination of active and chance mechanisms.

The second stage of segregating is when the period of negotiating in which the initial set of assignments is taken under consideration. During this period, agents with privileged access to school information – school leaders, administrators, teachers, and particularly privy parents – can try to influence classroom assignments in a continual, dynamic process that lasts until beginning-of-the-year assignments are settled. This is where we would expect to find teachers steering students into or out of their classrooms (Grissom et al. 2015). Some of these actions would occur regardless of initial segregation, but others may be reactions to initial segregation. Some reactions may desegregate classrooms, as when school actors are concerned to find racial segregation and switch students around to desegregate or when they compromise over which teacher gets the most desired and undesired pupils. They may also exacerbate segregation, as when classroom disparities in age raise concerns about exposing young students to their stigmatized older peers. I divide the reactions during assignment negotiations into those responding to active and chance segregation. In some cases, this may be a clean division; for example, the intentions and stakeholders (or lack thereof) producing initial segregation may importantly modify whether assignment changes are attempted and whether they are successful. We might also suppose that some reactions are to segregation itself, without concern for the cause; such reactions are due to both active and chance segregation in proportion to their contributions to initial segregation.

When this stage completes, the school year begins with a set of allocations, but it remains unsettled throughout the year. The third stage of classroom segregating involves the tinkering
and classroom switching that takes place continually throughout the school year but particularly in the first few weeks after school begins (Delany 1991). Here, also, segregation is actively produced or undone, both independent of segregation levels and in response to them, some of which reacts to active segregation and some of which reacts to chance segregation. However, the specific mechanisms are likely different. Last minute scheduling changes and conflicts necessitate tinkering with classroom assignments, which savvy parents can shield themselves from or use to their advantage. Racial segregation results when, for instance, a desired teacher’s classroom size is being reduced and whiter students are better able to retain their spots. This is also the period when parents can lobby schools to switch their children’s classrooms (Oakes & Guiton 1995; Lewis & Diamond 2015).

Barriers to Parsing the Sources of Segregation when Assignment is Dynamic

As Fig. 2 demonstrates, the school year ends with classrooms that are racially segregated due to an amalgam of mechanisms. Some of these mechanisms are clearly active; action-only mechanisms include the use of race-correlated assignment criteria and reactions to the segregation produced in this way. Chance-only segregation is caused by the use of arbitrary criteria. And then there are reactions to chance-only segregation, a set of mechanisms that occur when assignment is dynamic. This interaction of chance and action is not clearly classifiable as either.

One contribution of this paper is to bring attention to reactions to chance and begin conceptualizing it. Ideally, one would parse the sources of segregation into the three categories depicted in Fig. 2, with reactions to chance belonging to a distinct category because it is conceptually distinct. However, this is a costly task. Capturing initial segregation would require close observation so as not to mistake immediate responses to initial assignment as among the sources of initial segregation. Validating the coding of as stochastic
assignments would necessitate doing this for a large sample of schools to see a near-random pattern. Causally estimating the role of reactions to chance requires both exogenous variation and strong control over initial assignment; schools induced to move away from arbitrary assignment are likely to also change their use of race-correlated criteria, making it difficult to distinguish the effects of either. This underlines the importance of a first cut that empirically assesses the extent to which segregation is produced by chance or reactions to chance, as that will inform whether such intensive analyses could be worthwhile.

The task of parsing the sources of segregation is more tractable when reactions to chance are classified within active or chance segregation for the sake of measurement. As I discuss below, other treatments of segregation by chance implicitly classify reactions to chance as active segregation. I take the opposite approach. The questions most relevant to reducing classroom segregation are “how much less racial segregation would occur if arbitrary initial assignments were instead stratified by race?” and “how much less racial segregation would occur if race-correlated initial assignments were instead stratified by race?” In other words, if we are interested in the effects of chance-only or active-only segregation on end-of-year racial segregation, we should compare to counterfactuals in which chance-only or active-only segregation does not occur. Because reactions to chance are a downstream consequence of chance-only segregation and would not occur in its absence, my measurements treat reactions to chance as part of segregation by chance.

Prior Scholarship Parsing the Sources the Segregation

There is a long tradition of scholars comparing observed segregation to the amount of segregation that would hypothetically occur under random assignment (Cortese et al. 1976; Blau 1977; Winship 1977; Carrington and Troske 1997; Allen et al. 2009; Rathelot 2012; Bygren 2013). This comparison serves as a sort of “sanity check” that enables scholars to
assess whether the patterns they find could plausibly be due to chance, but it falls short of measuring how much segregation was produced by chance processes. For example, segregation could be entirely produced by actions (e.g., following stratified random assignment) yet be less than or equal to the amount of segregation that would have occurred under randomization.

Some scholars have sought to parse the sources of segregation into chance and active processes by subtracting from observed segregation the amount of segregation that would occur under randomization (Cortese et al. 1976; Carrington and Troske 1997; Allen et al. 2009; Bygren 2013; Fossett 2017). However, the hypothetical segregation under randomization is an upper bound of chance-only segregation; in a single-step assignment process, segregation greater than the amount that would occur under randomization indicates that the assignment process was not entirely random, in which case the method overcorrects for chance by an unknown amount.

Furthermore, consider a world with two schools, each with two classrooms and four students, half one race and half another. One school divides students strictly by race (segregation of value 1) and one that uses racially-stratified assignment such that it is completely unsegregated (a value of 0). All of the segregation is actively produced but the average amount of segregation is the same as what would be expected under random assignment (.5) such that these measures would find no active segregation. While useful as a conservative test of whether there is any active segregation, this approach is too prone to underestimating active segregation to be useful for parsing the mechanisms of segregation.

The issue is further complicated when assignment occurs dynamically, in which case these methods muddle actions and reactions to chance. Observed segregation less hypothetical segregation under randomization is the combination of active-only segregation in the initial
stage, agents’ later-stage actions that occur independently of the initial stage, agents’ reactions to active-only segregation, and – crucially – agents’ reactions to chance-only segregation. The justification for this, as with the above assumptions, is that the estimand captures how much more or less segregation occurs than would occur in a world in which race had no social meaning or correlation with socially meaningful characteristics (Cortese et al. 1976). In other words, colorblindness is the benchmark expectation.

Given the consequences of racial segregation, it would also make sense to instead hold institutions like schools responsible for ensuring classroom segregation is nominal. If some schools do have these values and actively desegregate their classrooms, an odd thing happens using the traditional methods: anti-segregation efforts are construed as negative segregation that lowers the apparent active segregation in other schools. Suppose there are two sets of schools that both use random assignment, but one set of schools undoes all segregation while the others react to chance segregation by further segregating. By traditional methods, the former schools would have negative active segregation while the latter have positive active segregation. Thus, when averaging over all schools, the former schools’ anti-segregation efforts are a sort of credit that discounts some of the active segregation of other schools.

Measuring Segregation by Chance as Chance-Only Mechanisms Plus Reactions to Chance

I take a different approach, empirically measuring segregation by chance as the combination of chance-only mechanisms and reactions to chance. This approach asks how much segregation is caused, both immediately and in the interim between initial assignment and the end of the schoolyear, by using arbitrary assignment criteria where the remainder is active segregation caused by using race-correlated criteria. It explicitly considers the dynamism of segregation within organizational settings with agentic actors, accounting for reactions to chance. Here the benchmark is complete desegregation. Though a nominal amount of
segregation may be desirable and certainly a tiny amount of segregation is unavoidable given odd numbers, this benchmark complements the colorblindness benchmark approach of prior studies by centering the consequences of segregation rather than the intentions behind it. This is also preferable because I avoid treating anti-segregation efforts as though they negate active segregation.

Let’s begin by considering how scholars have measured segregation by chance to date. In their estimations of segregation by chance, these analyses focus on the upper bound of chance-only segregation, leaving out the ways that segregation by chance is prevented or reacted to. In this formulation, the classroom segregation $H_i$ in school-observation $i$ is the sum of average active segregation ($\alpha$), expected segregation under random assignment (denoted $h(X_i)$, where $X_i$ is the matrix describing classroom and racial group sizes), and a residual that varies due to both random draws and variation in active segregation:

$$H_i = \alpha + h(X_i) + e_i.$$  \hfill (1)

Empirically measuring segregation by chance poses new challenges. I model classroom segregation as a simple linear function of $h(X_i)$. This raises the possibility of confounding. Let $g(X_i)$ denote a function included in $\alpha$ that describes the association between active segregation and the school characteristics $X_i$ on which $h(X_i)$ depends. Though $X_i$ is observed, it cannot be controlled for because it is the source of all variation in $h(X_i)$.

Add to this the possibility that school actors react to initial chance-only segregation by desegregating or further segregating. Then the total effect of segregation by chance – the combination of chance-only segregation and segregation due to reactions to chance – increases slower or faster than expected segregation under randomization. Let $z(h(X_i))$ describe active segregation as a function of expected segregation under random assignment:
\[ H_i(h(X_i)) = \alpha + h(X_i) + z(h(X_i)) + e_i \]
\[ \tilde{H}_i(h(X_i)) = \beta_0 + h(X_i) + z(h(X_i)) + g(X_i) + v_i, \]

where \( \beta_0 \) is active segregation unassociated with classroom and racial group sizes. Thus, unlike the traditional formulation (Eq. 1), \( H_i(h(X_i)) \) can deviate from a line with slope 1. Because it is unclear whether these deviations are due to confounding, \( g(X_i) \), or reactions to chance, \( z(h(X_i)) \), identifying how classroom segregation depends on expected segregation under randomization, \( h(X_i) + z(h(X_i)) \), is not straightforward.

There is no general solution to this challenge, we can plausibly rule out substantial confounding under certain conditions. If there is a confounding problem, \( \tilde{H}_i(h(X_i)) \) is likely to be nonlinear, school segregation levels are likely to be at least somewhat consistent over the short term, and the conditional distributions of segregation given expected segregation under randomization are unlikely to exhibit patterns consistent with random assignment. I discuss these considerations at greater length in the analysis section.

Note that reactions to chance can also create these patterns, so the three patterns above are not evidence of confounding. However, their absence can be taken as evidence that classroom and group sizes are not driving the relationship between observed segregation and expected segregation under randomization. In other words, this approach is only useful for confirming that confounding is small in cases in which there is little confounding and reactions to chance do not cause these aforementioned patterns.

With respect to each pattern, the naïve estimated effect of expected segregation under randomization appears to be driven by initial chance segregation and modest, linear reactions to it rather than confounding. For the patterns I observe to occur without segregation by chance, active segregation would have to be a function of classroom and racial group sizes that (a) tends to be a roughly linear transformation of their mechanical effect on expected
segregation under randomization, (b) is unassociated with the many school characteristics that are stable over the short term; and (c) varies conditional on expected segregation under randomization in a similar manner to random draws.

This is not dispositive evidence of causality, nor is it useful for ruling out small biases. As discussed above, this would require intensive data collection and experimentation. Instead, with publicly available nationwide data, this approach provides strong evidence that there is no more than modest bias in the naively estimated proportion of segregation that occurs due to chance and reactions to chance.

Data

Brazil’s public-school system offers a particularly interesting case for analyzing classroom segregation by chance because classroom sorting is rare and, perhaps consequently, classroom segregation has been ignored in the literature (see Appendix A for details). It appears it is assumed not to occur.

I investigate classroom segregation in Brazil using Prova Brasil 2011-2015, a biennial student achievement test that includes identifiers linking students to their classrooms and schools as well as a student survey with demographic information (Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira (INEP) 2011-2015a). These publicly available data were collected at the end of the school year and attempt to include all Brazilian public-school students in 5th and 9th grade with the exception of very small schools. I focus on 5th grade, when classrooms are stable across subjects and there are still many white students in public schools.

Students are considered to have responded to the questionnaire if they did not skip the race item. I restrict the data to multi-classroom schools with at least two classrooms all of which
have response rates of at least 75%. The full sample includes 40,237 school-year observations in which the average school is composed of 56-62 students in 2.4-2.5 classrooms depending on the year. This sample is not representative of all Brazilian schools, but it includes 38-55% of the 5th grade students in Brazil’s multi-classroom schools in 2011-2015.

The data shows how this case provides leverage for identifying the total effect of segregation by chance. Many schools have small classrooms, but there is also substantial variation in classroom sizes. In each year, 24-27% of schools have fewer than 20 students per classroom while 10-14% have more than 30. This is important because observing a wider range of $h(X_i)$ means capturing more of the curve shown in Fig. 1 such that fewer functions of $X_i$ are roughly linear over the observed range of $h(X_i)$. Yet due to the large number of cases, there are nonetheless many observations with similar values $h(X_i)$, enabling comparison of the observed and simulated distributions of segregation. Additionally, by including over 2,500 distinct school systems in each year, the sample ensures that the study is identifying general patterns rather than local idiosyncrasies.

Measures

I measure racial segregation across classrooms, $H_i$, as multigroup segregation using the Information Theory Index ($H$), a weighted ratio of the heterogeneity of classrooms compared to the heterogeneity of the schools they are in (Reardon and Firebaugh 2002). This operationalizes segregation as the degree to which students are unevenly distributed across classrooms given the school population. A value of 0 indicates that every classroom is proportional to the school and a value of 1 indicates complete segregation where classrooms are entirely dissimilar.
This requires measuring race, an inherently fraught task. 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 student survey: white, pardal,\(^2\) black, indigenous, yellow,\(^3\) and “I don’t know.” It is not obvious that this is the ideal approach nor what alternatives might be preferable. The Brazilian case brings issues with measuring race to the fore, so I discuss them at length in Appendix B. Additionally, Appendix B shows that my findings are robust to dichotomous (white/nonwhite) and trichotomous (white/parda/black) operationalizations of race.

I measure each school’s expected racial segregation across classrooms under randomization, \(h(X_i)\), by simulating \((n = 500)\) random classroom assignment in each school and taking the mean hypothetical segregation under this scenario. I randomly assign each student to a classroom in their school with an equal probability of being assigned to each classroom.

**Analysis**

This section proceeds in three parts: a description of the extent of racial segregation across classrooms in Brazilian public schools, evidence that segregation by chance is a key mechanism, and estimating the proportion of segregation that is due to the combination of initial chance segregation and reactions to it.

Hidden Racial Segregation between Classrooms

Fig. 3 describes the scale of racial segregation in the Brazilian school system by decomposing (Reardon and Firebaugh 2002) the racial segregation between classrooms throughout the nation into higher-scale units long-understood as segregated: regions, municipalities, and schools. In each year, the plurality (38-42%) of racial segregation in Brazil’s multi-classroom
public schools occurs between classrooms in the same school, not traditional suspects like between regions (9-11%), between municipalities within regions (20-24%), or between schools within municipalities (27-29%).

Because there is no comparable data for schools that are not in Prova, it is unclear how much of the overall segregation between classrooms among all 5th graders is within schools. One might wonder 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 Fig. 3 overstates the role of microsegregation.

One solution is to provide a lower bound on the proportion of segregation that occurs within schools. Suppose the private sector was all-white. Given 13-16% private school enrollment according to Sinopse Estatistica da Educacao(203,560),(778,618)Basica (2011, 2013, 2015), I simulate the proportion of segregation within schools under this extreme hypothetical. This would diminish the role of microsegregation substantially, but it would remain large. In 2011, 2013, and 2015, the percentage of segregation within schools would reduce to 28%, 27%, and 25%, respectively. Even under the most extreme assumptions, much of the racial segregation among all Brazilian 5th graders in multi-classroom schools is microsegregation within public schools.

Looking at a specific school allows us to see just how segregated classrooms are.

Fig. 4 illustrates the segregation in a school near the 80th percentile of segregation (percentile = 80.4; $H_i = .104$). This two-classroom school has 50 students, 27 of whom identified themselves as white and 23 who identified themselves as pardal. While one classroom is 64% white, the other is only 35%. The difference between this school’s classrooms is stark, but it is no outlier: 1 out of 5 school-observations in this sample are even more segregated.
Fig. 3-4 show that microsegregation is the primary driver of racial segregation among 5th graders in Brazil’s multi-classroom schools. This is surprising; to date, the scholarship has assumed no classroom segregation by race within Brazil’s public schools.

Evidence that Classroom Segregation Occurs by Chance

I provide three pieces of evidence that confounding does not drive $\hat{H}_1(h(X_i))$. I begin by arguing that the linearity of $\hat{H}_1(h(X_i))$ suggests the relationship is due to initial chance segregation and reactions to it, not confounding. That is,

$$\hat{H}_1(h(X_i)) = \beta_0 + \beta_1 h(X_i) + \nu_i,$$

such that $\beta_1 h(X_i)$ identifies how much classroom segregation at the end of the school year is due to initial segregation by chance and its downstream effects (i.e. reactions to chance).

Fig. 5 and Table 1 use data stacked over all years to describe observed segregation as a function of expected segregation under randomization (findings are consistent across years; see Appendix C). The observations fall squarely along a linear fitted line with a slope slightly and significantly greater than 1 ($\hat{\beta}_1 = 1.08, p < .001$ for $H_0: \beta_1 = 1$). A quadratic fitted line closely tracks the linear prediction over the region where most observations fall, deviating slightly only at the tails, where local means are noisier due to the inclusion of fewer schools. This suggests overfitting in the quadratic model. Table 2 corroborates this interpretation; while the quadratic term is significant, the Bayesian Information Criteria (BIC) of the two models show that adding the quadratic term does not make the model more informative.
How does this occur? Though $g(X_i)$ could be an approximately linear transformation of $h(X_i)$ by coincidence, this is not expected; whereas the latter is a mechanical relationship, the former is an amalgam of any social processes by which classroom and racial group sizes are associated with segregation.

If $g(X_i)$ is not a linear transformation of $h(X_i)$, the sum of $g(X_i)$ and $z(h(X_i))$ must be a linear transformation of $h(X_i)$. This could happen one of two ways:

$$g(X_i) \equiv ah(X_i) - z(h(X_i)) + r_i$$

or

$$g(X_i) \equiv r_i$$

$$z(h(X_i)) \equiv (\hat{\beta}_1 - 1)h(X_i) + u_i$$

There is no reason to expect the former whereas a linear function $z(h(X_i))$ occurs if actors react to initial chance segregation by (de)segregating in proportion to it. This is the more plausible explanation, suggesting $H_i(h(X_i))$ could identify $h(X_i) + z(h(X_i))$ as in Eq. 3.

If this inference is true, then Table 1 indicates that arbitrary classroom assignments induce active segregation downstream such that the total effect is 8% more than would occur under randomization. Additionally, the intercept is small but significantly greater than 0 ($\hat{\beta}_0 = .02, p < .001$), indicating that a small amount of classroom segregation is unrelated to classroom and racial group sizes. If the naïve regression is not substantially biased, segregation by chance is the primary, but not sole, driver of classroom segregation by race.

Should we believe $H_i(h(X_i))$ is minimally biased by confounding? I test two predictions that adjudicate between a hypothesized model in which segregation is primarily total segregation by chance and an alternative model in which active segregation associated with classroom...
and racial group sizes spuriously produces the observed relationship between segregation and expected segregation under randomization.

**Prediction 1:** Deviations from $h(X_i)$ are *not* nested within schools over time.

If racial segregation is truly driven by chance, a school’s classroom segregation levels should be uncorrelated over time after netting out $h(X_i)$. That is, segregation should deviate from the expected segregation under randomization in a way that looks like random draws. On the other hand, the socially produced association between segregation and classroom and racial group sizes is expected to be nested within schools. It depends on school characteristics that are typically stable over the short term, like demographic composition, community values, organizational culture, and staff. If the apparent total segregation by chance is an artifact of $g(X_i)$, a given school should be consistently more (or less) segregated than expected under random assignment.\(^4\)

Fig. 6 leverages repeated observations within schools to show that there is no correlation across years in schools’ classroom segregation levels after netting out $h(X_i)$. They appear to be unrelated to school features. Recall that the relationship between observed segregation and $h(X_i)$ depicted in Fig. 5 is consistent over years (see Fig. C1). Fig. 6 shows that this stable pattern emerges from an unstable process where the observed segregation in individual schools bounces up and down around $\hat{H}_i(h(X_i))$ with no apparent pattern. Consistent with Eq. 3 and Prediction 1, it is as though the observations within schools are random draws.

**Prediction 2:** The distribution of segregation conditional on $h(X_i)$ exhibits decreasing right-skew and increasing variance as $h(X_i)$ increases, just as it would under random assignment.

Fig. 7 displays how randomly assigned segregation is distributed conditional on expected segregation under randomization. Each CDF is drawn over a small bin of $h(X_i)$. Random
segregation is simulated \((n = 50)\) in each of the schools in the analytic sample. The 20 bins are equally wide and drawn from the highest density region of \(h(X_i)\), altogether containing 79% of school-observations. Bin 1 is at the low range of expected segregation while Bin 20 is at the high range.

Fig. 7 shows that, when classroom assignment is random, segregation has a specific pattern of heteroscedasticity with respect to expected segregation under random assignment. At a given expected segregation level, the distribution of random draws is right-skewed, as evidenced by the rapid initial incline and long tail at the top. The CDFs steadily shift and tilt to the right as expected segregation increases, the skewness decreasing and the variance increasing.

If the classroom segregation in Brazilian schools is primarily initial chance-only segregation, this pattern should be evident in the observed data. The distributions will not align exactly, given that \(\hat{H}_i(h(X_i))\) suggests some active segregation and reaction to chance \((\hat{\beta}_0 \neq 0, \hat{\beta}_1 \neq 1)\). These other sources of segregation shift and stretch the conditional distributions. Nonetheless, if Eq. 3 is accurate, the right-shift apparent in Fig. 7 should occur, barring extenuating circumstances.\(^5\)

Fig. 8 shows the observed conditional distributions. Consistent with Prediction 2, they closely follow the right-shift pattern expected under random assignment. In Appendix D, I affirm that this pattern is plausibly due to the random segregation in Fig. 7 by demonstrating that a parsimonious function of random segregation based on \(\hat{H}_i(h(X_i))\) reproduces the observed segregation distribution and nearly reproduces the conditional distributions.

These findings uphold both predictions that follow from Eq. 3, which states that there is no confounding in the relationship between observed segregation and expected segregation.
under randomization. Though I cannot rule out bias in $\hat{R}_1(h(X_i))$, the findings cast doubt that any such bias is substantial; only under extraordinary circumstances could active segregation processes be driving classroom segregation in Brazil.

For these findings to occur without segregation by chance, active segregation would have to be a function of classroom and racial group sizes that (a) tends to be a linear transformation of their mechanical effect on expected segregation under randomization, (b) is unassociated with the many school characteristics that are stable over the short term; and (c) varies conditional on expected segregation under randomization in a similar manner to random draws.

The more plausible explanation is that segregation by chance plays a strong causal role in the racial segregation between classrooms in Brazil such that $\hat{\beta}_1 h(X_i)$ is a close estimate of the causal effects – both immediate and downstream – of segregation by chance, $h(X_i) + z(h(X_i))$.

The Amount of Segregation due to Chance

Granted this interpretation, how much of the classroom segregation occurs by chance? Fig. 9 depicts the implied proportion of segregation that results from either chance-only segregation or reactions to it as it varies by classroom segregation level (see Appendix E for computation details). Two estimates are presented: my best estimate, which grants the assumption in Eq. 3, and a conservative estimate that attributes the greater-than-1 slope to confounding instead of reactions to chance segregation. This is also the estimate that prior approaches to segregation by chance would produce – the amount that would occur if initial assignment was random and there was no reaction to chance. For the reader unconvinced by the evidence that
Segregation by chance drives classroom segregation, Fig. 9 at the very least depicts how much segregation *could* be segregation by chance.

Segregation by chance accounts for the majority of classroom segregation in most schools. In the average school, 63% of the racial segregation between classrooms is attributable to segregation by chance. This number drops to 58% when using the conservative estimate. These numbers are greater among schools in the top quintile of observed segregation – schools at least as segregated as the one in Fig. 4. Among these highly segregated schools, segregation by chance contributes 86% (or 80%) of racial segregation across classrooms. These findings are consistent across years (see Fig. C2).

All told, 75% (69%) of classroom segregation is due to chance. This equates to 28-31% (26-29%) of all racial segregation among multi-classroom public schools in 2011, 2013, and 2015. When considering all schools, public and private, 19-21% of all racial segregation would be classroom segregation by chance even if the private sector were only composed of white students. Microsegregation by chance is one of the main sources of racial segregation in Brazil’s school system.

**Conclusion**

This paper began by challenging how researchers have conceptualized segregation by chance for the past 40 years, calling for empirical measurement, consideration of dynamic assignment and possible reactions to chance, and setting a benchmark that centers the consequences rather than intentions of racial segregation. By appreciating the social meaning of chance segregation, this conceptual approach is better suited to informing policy, practice, and future segregation by chance research in settings in which people have substantial agency over how people are grouped, as is often the case within organizations. This conceptual shift creates a methodological problem: how do you empirically decompose segregation into
chance and active mechanisms? The analysis offers one answer to this challenge, primarily by patching together evidence that the observed pattern of segregation is driven by chance rather than spuriously resulting from the school factors that mechanically moderate segregation under randomization.

The descriptive findings show that classroom segregation is a far greater source of racial segregation in Brazil than previously believed. How does this occur? As it turns out, mostly by chance, particularly in the most classroom-segregated schools. This is a new, potentially powerful, and quite different explanation for classroom segregation, which is typically explained by sorting policies like tracking and competitive practices among schools, parents, students, and teachers. I hope this will call attention to classroom segregation in non-tracking contexts, which is currently a blindspot in the classroom segregation literature. More broadly, these findings demonstrate the importance of thinking anew about segregation at small scales and how chance processes structure social interaction.

Implications for Policy and Practice

It was not inevitable that there would be so much segregation by chance. Indeed, segregation by chance with respect to any given student characteristic is both preventable and correctable if schools are empowered to proactively diversify classrooms. Schools could even continue their current approach to initial classroom assignments; Brazilian schools could avoid most racial segregation by acknowledging substantial classroom segregation prior to the start of the schoolyear or even at the beginning of the schoolyear, then reshuffling students to reduce segregation to nominal levels. This places a minimal burden on schools, yet my findings demonstrate that Brazilian schools neither prevented nor undid classroom racial segregation by chance in 2011-2015. This may be due to the ideology of racial democracy, which masks
racial inequalities and discourages the explicit identification of perceived racial differences. Therefore, proactive diversification practices may require official promotion through policy.

Might this happen in other contexts? In the US, racial segregation by chance between classrooms seems unlikely. Given that racial segregation is strongly associated with malicious intent in the US, even principals not committed to racial equality may correct for racial segregation by chance absent a segregation-legitimating ideology like tracking. At the very least, they likely want to avoid looking like segregationists.

However, economic segregation by chance may well occur. 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 color-blinding racial ideology enables racial segregation by chance within Brazilian schools, the class-blinding ideology and data framework in US schools may enable economic segregation by chance.

Case-Specificity of Methods

Though this case specifically identifies segregation by chance as a classroom segregation mechanism, it also highlights inadequacies in the traditional approach to segregation by chance. Prior approaches have eschewed empirically decomposing segregation into chance and active components, instead focusing on the segregation that would hypothetically occur under random assignment. Indeed, they have typically attended to segregation by chance out of concerns about measurement bias, rather than as a substantive feature of social contexts. Consequently, these methods and the conceptualizations of chance segregation that accompany them are ill-fitted to understanding the actual sources of segregation, particularly when assignment is dynamic and subject to actor influence. The traditional approach also sets
colorblind assignment as the benchmark expectation of schools, centering segregation intentions rather than its consequences and producing odd scenarios if there are schools that seek to have nominal segregation as opposed to colorblind segregation.

My findings demonstrate the important role chance can play in microsegregation, both immediately and by inducing reactions. In recognition that segregation by chance shapes social contexts, this analysis departs from the tradition by acknowledging that initial, chance-only segregation may elicit reaction. It improves upon prior analyses by accounting for dynamic assignment and agency, centering the consequences of segregation rather than the intentions behind it, and empirically measuring segregation by chance. However, there are many cases where substantial segregation by chance may occur but cannot be measured empirically using this method. This is particularly true if there is substantial active segregation, confounding, or large, non-linear downstream effects. Here, simulation-only approaches remain useful demonstrations of how much segregation could occur under random assignment, but this is hardly informative about what to change or what mechanisms to study next. Accurately describing and understanding micro-level contexts will require building on this article’s reconceptualization of segregation by chance and further developing identification methods.
NOTES

1 In Brazil’s basic education system, students are grouped into classrooms that remain together (if not necessarily in the same room) for each subject, so I focus on the production of classroom segregation instead of discussing how classroom segregation becomes curriculum-wide segregation.

2 *Parda* (literally, brown) is a Brazilian racial category that refers to individuals who are between white and black on the racial continuum. It is sometimes translated as brown, multiracial, or *mullatto*, but I leave it untranslated because it is more complex than skin tone or ancestry.

3 Yellow is typically the way Asians are categorized in Brazil.

4 This isn’t strictly true because $H_i(h(X_i))$ suggests some active segregation. Though it is small on average, if it is highly variable and varies primarily between-schools, segregation levels could be correlated across years within schools for reasons other than confounding.

5 For example, if each school has its own intercept and slope, these could be heteroscedastic in $h(X_i)$ in such a way as to cancel out the heteroscedasticity in random segregation.
REFERENCES


Menezes-Filho, Naércio Aquino. 2007. *Os determinantes do desempenho escolar do Brasil.* IFB.


Table 1. OLS regression estimates of the relationship between observed classroom segregation and expected segregation under random assignment.

<table>
<thead>
<tr>
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<th>(2)</th>
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<tr>
<td>$h(X_i)$: Expected Segregation under Random Assignment$^a$</td>
<td>1.085***</td>
<td>1.234***</td>
</tr>
<tr>
<td></td>
<td>(.015)</td>
<td>(.064)</td>
</tr>
<tr>
<td>$h(X_i)^2$</td>
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<td>-1.286*</td>
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<tr>
<td></td>
<td>(.546)</td>
<td>(.546)</td>
</tr>
<tr>
<td>Intercept</td>
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<td>.014***</td>
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* $p < .05$, ** $p < .01$, *** $p < .001$

$^a$ $H_0: \beta_1 = 1$

Note: Models include all years, stacked, with Huber-White standard errors to account for heteroscedasticity. See Table C1 for year-specific models.

Fig. 1. Mean classroom segregation under random assignment, by classroom size and the proportions of 2 groups in the school.
Note: Each point estimate is drawn from 500 simulations randomly dividing students into 2 classrooms of equal size.
Fig. 2. A diagram of active and chance mechanisms of classroom segregation when classroom assignment is dynamic. Loop symbols indicate dynamic processes wherein reactions to segregation that affect segregation levels can induce additional reactions to segregation during the same time period.
Fig. 3. Racial segregation decomposed by segregation scale, by year.

Note: Total segregation between classrooms across the entire nation ($H_t$, reported at top) is divided into its constituent parts (Reardon and Firebaugh 2002).

Fig. 4. The distribution of 5th graders in a 2-classroom school near the 80th percentile of racial segregation between classrooms within schools.

Note: Each white dot represents one white student while black dots represent parda students. This school was blindly chosen (given specified parameters to ease interpretability) from the distribution of all multi-classroom schools over all years. The actual percentile is 80.4 ($H_i = .104$).

Fig. 5. Mean classroom segregation by expected segregation under random assignment (all years).

* $p < .05$, ** $p < .01$, *** $p < .001$ where $H_0: \beta_1 = 1$.

Note: Estimates are from Table 1. Bin size is weighted by number of schools. School-observations in the top 0.1% of expected segregation are not shown. See Fig. C1 for year-specific figures.

Fig. 6. Across-year within-school pairwise correlations of schools’ classroom segregation levels net of expected segregation under random assignment.  
*Note:* Scatterplots suppress outliers, but correlations are computed using the full sample.  
Fig. 7. Cumulative density functions under random assignment conditional on expected segregation under randomization.

Note: Random segregation was simulated ($n = 50$) within each school-observation given observed classroom sizes and race group proportions. Bins are equally wide and drawn from the highest density region of expected segregation under randomization, altogether containing 79% of school-observations. School-observations in the top 1% of expected segregation are not shown.

Fig. 8. Observed cumulative density functions conditional on expected segregation under randomization.

Note: Bins are equally wide and drawn from the highest density region of expected segregation under randomization, altogether containing 79% of school-observations. School-observations in the top 1% of expected segregation are not shown.

Fig. 9. Proportion of classroom segregation that is segregation by chance, by observed classroom segregation level (all years).

Note: Estimates are based on Table 1, where the best estimate is $\pi_i = \frac{\hat{\beta}_1 h(X_i)}{\hat{H}_i} = \frac{\hat{H}_i - \hat{\beta}_0}{\hat{H}_i}$ and the conservative estimate is $\pi^*_i = \frac{h(X_i)}{\hat{H}_i} = \frac{1}{\hat{\beta}_i} \pi_i$. The kernel density plot uses the Epanechnikov kernel. The 0.1% most segregated schools are omitted. See Fig. C2 for year-specific figures. Source: Prova Brasil 2011, 2013, 2015.
APPENDIX A. The Brazilian Context in Detail

The Organization of Brazil’s Public-School System

In Brazil’s basic education system – the equivalent of primary school in the US – classrooms are composed of students who take all courses together. The centered courses, math and Portuguese, may be taught by the same or by separate teachers. About 1 in 4 school buildings hold school in shifts (i.e. morning, afternoon, evening) with each shift composed of a different and stable set of students attending school together. In essence, there are multiple schools nested within one location and under the same school leadership. I refer to these building-specific shifts as schools. The school buildings are nested in municipalities within the nation’s 7 official regions.

Racial Inequality and Segregation in Brazilian Schools

Racial stratification in Brazil is substantial, with large inequalities favoring whites in income, employment, health, incarceration, and seemingly every other metric of well-being and resource access (Cortese et al. 1976; Andrews 2014; Haller and Eder 2016). Education is no exception (Marteleto et al. 2016). The most visible source of educational inequality is school segregation across the public/private sectors and between the northern and the more-developed southern regions (Telles 2004; Telles and Paixao 2013), but school resources are also unevenly distributed among schools with the same local administrative body (Soares 2002; Franco et al. 2007; da Cunha et al. 2009). These factors coincide such that schools that are disproportionately attended by whiter students tend to have preferable infrastructure, administrative organization, learning materials, pedagogy, and teacher qualifications (Soares 2002; Louzano 2007; da Cunha et al. 2009).
There are also substantial educational differences within schools; 70 to 90% of the differences in public-school students’ test scores are differences between students attending the same school (Menezes-Filho 2007). And in Brazil’s second-largest state, Minas Gerais, classroom differences explain 32% of the total achievement variation, 3 times as much as differences between schools (Soares 2005). One possible explanation for achievement variation within schools is that, in Brazil like the US (Thiemann 2018), more effective teachers tend to teach higher-achieving students even in the absence of tracking, but this would require widespread segregation within schools. Leite (2017) is the only example I have found of a researcher attending to any sort of segregation between classrooms within schools in Brazil, but the focus of her study is the quite rare case of schools that report sorting on student achievement (Leite 19 June, 2017).

The inattention to classroom segregation appears to be due to the presumption that it simply does not occur because most schools do not segregate by design. In interviews with two state secretaries of education and two secretaries of education in major cities, as well as conversations with numerous scholars of Brazilian education at the Stanford Lemann Center for Entrepreneurship and Educational Innovation in Brazil, I have been consistently told – prior to sharing my findings – that racial segregation does not occur in Brazil’s schools because, unlike the US, there is no tracking (personal correspondence 6/7/2017).

They are right that segregation by design is rare. Despite principal reports of sorting on age and achievement, in the grade (5) and years (2011-2015) I analyze, these reported practices have little to no relationship with classroom differences in student achievement, age, SES, or race within schools (analysis available upon request) (Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira (INEP) 2011-2015a,b). Brazilian schools are non-sorting; what
government officials and scholars have missed is that this does not necessarily prevent classroom segregation.

Racial Ideology and Colorblind Classroom Segregation

Recall that mitigating racial segregation by chance in schools with non-rigid classroom assignment practices requires that schools observe race, strongly prefer low racial segregation, and be empowered to prevent or undo it.

Some barriers to preventing and undoing classroom segregation by chance are not necessarily specific to racial segregation in Brazil. The common perception that classroom segregation is rare to nonexistent is one likely barrier to schools proactively ensuring classrooms are representative of the student body. Widespread complacency about classroom segregation in non-sorting schools may also enable segregation by chance by reducing school leaders’ external motivation to desegregate; they need not fear backlash over a problem that has not been problematized. Segregation by chance may also be inherently harder to notice and problematize because it is both unpredicted and unintended.

In addition to these general barriers to preventing and undoing segregation by chance, the racial ideology known as racial democracy makes Brazil particularly susceptible to racial segregation by chance.

Racial democracy is a contested ideology that acts as a center of gravity for the social science and politics of race in Brazil. Its origins are commonly traced to sociologist Gilberto Freyre’s (1946) book Casa-Grande Senzala (In English, The Masters and the Slaves), originally published in 1933 (Freyre 1946). His study and the many others that followed described race relations in
Brazil – where intermarriage has long been common and large-scale segregation was never legally instituted – as benign in comparison to the Jim Crow US (Freyre 1946; Fry 2000; Telles 2004; Bailey 2009). Though racial democracy scholars acknowledged that “in Brazil, everybody believes it is better to be white” (Harris 1964[59] in Bailey 2009[26]), these texts emphasized how Brazilian society was less racist than the US. They noted greater racial ambiguity and interaction across the color continuum, treated race as a class epiphenomenon, and projected progress toward a single meta-race. This interpretation was adopted by the state and elites who deployed it to characterize Brazil as a racial paradise, denying both racial inequality and the notion that there are races (or social actions that make race “real”) (Guimaraes 2001; Telles 2004).

Since the waning of the military dictatorship in the 1970s and 1980s, scholars and black movement activists have vigorously contested the ideology of racial democracy. They document racial discrimination and severe inequality to counter the overly rosy picture of race relations (Fernandes 1971; Hasenbalg 1985; Silva 1985). They also promote brighter racial boundaries and stronger racial identification to counter the common position that acknowledging race is prejudice – what Guimaraes (2001) terms antiracialism and what US readers might understand as the ideal of not seeing race (Guimaraes 2001).

This scholarship highlights two ways racial democracy operates as a color-blinding ideology that makes Brazilian schools particularly susceptible to racial segregation by chance: masking racial inequalities and discouraging the explicit identification of perceived racial differences.

As noted above, one element of racial democracy has been the denial of racial inequality, either outright or by treating it as a minor or epiphenomenal dimension of inequality (Fernandes 1971).
Echoes of this myth are apparent in recent surveys on discrimination where, despite low reports of racial discrimination relative to class, it is race not class indicators that are most strongly correlated with reports of class discrimination (Layton and Smith 2017). This denial of racial inequality likely keeps schools from prioritizing the prevention of racial segregation.

Scholars have questioned whether this denialism extends to the masses of Brazil; regardless of racial classification, most Brazilians endorse structural racism narratives of income gaps and support anti-discrimination efforts (Bailey 2009; Telles and Bailey 2013; Bailey et al. 2015). For example, Bailey (2009) notes racial democracy also acts as a creed, a vision that Brazil should be free of racial divisions and inequality (Bailey 2009). However, de facto segregation does not clearly violate this creed because it is ostensibly non-prejudicial. This is doubly so for segregation by chance because it lacks a malicious actor.

Another barrier to problematizing racial segregation by chance is the way de jure segregation in the US has historically served as a foil vindicating de facto segregation in Brazil. Since the earliest racial democracy texts, Brazil has been specifically celebrated for its lack of segregation relative to the apartheid US (Fry 2000) (on the non-Jim Crow US as an apartheid nation, see Massey and Denton 1993; Rothstein 2017). Segregation is precisely the sort of race relation that Brazil is supposed to be better about. This has been apparent in my interviews with Brazilian education leaders where, in schools as in neighborhoods, de jure segregation (tracking) in the US provides an extreme foil that (mis)construes de facto segregation in Brazil as benign if not non-existent (personal correspondence 6/7/2017).

Additionally, racial democracy operates as a color-blinding ideology by discouraging the explicit identification of perceived racial differences. Antiracialism construes it as improper to view
people as belonging to different racial groups, manifesting in a system of manners that simultaneously minimize phenotype differences and affirm white desirability by, for instance, lightening a person’s color when describing them (Schwartzman, 2009). Without state backing, school leaders likely question the appropriateness of acknowledging the extent of color differences among students or explicitly considering those differences when organizing classrooms.

Altogether, this makes Brazil particularly susceptible to racial segregation by chance.
APPENDIX B. On Measuring Race

Measuring race is an inherently fraught task. The Brazilian case brings these issues to the fore, so I discuss them here at length. I discuss the validity of my chosen approach with respect to three concerns: whether “race” is an appropriate description of what I measure, using a survey with a small number of discrete categories to choose among, and measurement bias due to racial fluidity. I then assess whether my inferences would change if I took an alternative approach.

Race vs Color

As is commonly noted, race in Brazil differs substantially from the US and is typically called color. Though the black movement and state policies promote discrete categorizing, most Brazilians describe themselves along a color continuum that can include very specific gradations. Open-ended survey items generate upwards of 100 responses (though 95 percent use one of six terms) (Silva 1987; Telles 2004; Bailey 2009). This categorization scheme differs from the US racial scheme in that it is primarily determined by phenotype (i.e. how a person looks), rather than by ancestry and rules of hypo- and hyper-descent (Snipp 2003; Telles 2004). Consequently, referring to this phenomenon as race is criticized as an etic description that persists in research despite race offering no more analytic utility than color or other terms for what Loveman (1999) instructively calls groupness (Loveman 1999; Harris et al. 1993; Bourdieu and Wacquant 1999; Bailey 2008, 2009; Banton 2012; Monk Jr 2016).

Nonetheless, I choose to call this phenomenon race. I use the term race instead of color because I think US readers are aware Brazil does race differently but may misinterpret color to mean a single trait: skin pigment. Yet what Brazilian’s call color is a general organizing principal that takes into account additional features like hair color, hair texture, eye color, nose shape, and lip
shape and is shaped by other characteristics like class and context (Harris and Kottak 1963; Nogueira 1985; Bailey 2009; Harris et al. 1993; Telles 2004). Additionally, Brazilian readers are familiar with the use of race in reference to census categories, so my chosen description conveys the appropriate meaning.

Discretizing a Continuum

The color continuum also differs from the century-old census scheme typically used by researchers, which lists the categories white, *parda*, black and – more recently – yellow and indigenous. Consequently, responses to race self-identification items depend upon whether the item is categorical or more open-ended (Silva 1987; Telles 2004; Bailey 2009). Though Brazilians asked to categorize photographs according to this scheme do so consistently (Bailey 2009), interviewer-identified census categorizations imperfectly match self-identification categories (Telles and Lim 1998; Bailey 2009; Bailey et al. 2013). Accordingly, Bailey (2009) questions whether the people grouped together by these categories ought to be thought of as groups and whether self-identification on the census format ought to be thought of as capturing identity (Bailey 2009).

Though I would prefer a variety of items to capture more dimensions of race (Bailey et al. 2013, 2014), I believe the census-style item used in my analysis is informative. Indeed, Bailey, Saperstein, and Penner (2014) find that in Brazil (unlike the US) using only individuals’ self-identifications on a census-like item is preferable to coupling it with interviewer-measured skin color when modeling income inequality (but see Monk Jr 2016). Additionally, since the introduction of affirmative action policies Brazilians have increasingly used census-like racial identifications on open-ended questions (Bailey et al. 2018).
Given that most terms used for open-ended items are color gradations (in the pigment sense) (de Geografia e Estadística (IBGE) 1999), they can be thought of as nested within the colors used in the census categories. Thus, the reader intent on using emic classifications might think of the census categories as a discretized continuum that bluntly measures where individuals fall on the color scale. In this case, segregation by the census categories is a suboptimal stand-in for measuring color segregation, much as a discretized income measure can be used to imperfectly measure income segregation. Such measurement error would lead me to underestimate segregation levels (Owens et al. 2016).

Fluidity and Error

Given variable self-identification, differences between subjective and ascribed identification, and (smaller) differences among ascriptions, the reader may also find a one-time measure of self-identification unsatisfying. For example, one might be concerned with race as identified by a specific person (e.g., a teacher or principal); yet, the research described above suggests this would be quite noisily captured by one-time self-identification. In this sense, I am using a noisy – potentially very noisy – measure, depending on what aspect(s) of race the reader prioritizes.

I leave it to the reader to decide what the target characteristic is and, therefore, how noisy my measure is. Throughout the paper, however, I present estimates as though race-as-measured is race-as-it-is. In doing so, I provide the most conservative estimates of racial segregation in Brazil because noise in the measure of a characteristic causes underestimation of segregation levels (Owens et al. 2016). In other words, the more error-prone the survey is, the more my findings underestimate segregation.

Robustness to Alternative Measures
Table B1 and Fig. B1-B10 repeat the analyses from the main text with alternative operationalizations of race: a trichotomous measure (black vs *parda* vs white) and a dichotomous measure (white vs non-white). Two differences arise deploying these measures, none of which substantively alter the inference that classroom segregation by chance is a primary mechanism of racial segregation in Brazil’s multi-classroom public schools.

First, Fig. B1-B2 show that classroom segregation is less concentrated within schools using these measures. Using trichotomous race, the plurality of racial composition differences still occurs at this micro scale in each year. However, using dichotomous race, each scale accounts for similar amounts of racial segregation.

Second, Table B1 shows that the BIC test does not consistently favor linear over quadratic models. Specifically, the BIC test favors the quadratic model when using dichotomous race. Nonetheless, Fig. B4 shows that the quadratic and linear predictions are nearly identical over the region with nearly all the schools.
Table B1. Using alternative operationalizations of race: OLS regression estimates of the relationship between observed classroom segregation and expected segregation under random assignment.

|                     | Black – *Parda* – White |  | White – Non-White |  |
|---------------------|--------------------------|  |-------------------|  |
|                     | (1)                      | (2) | (1)               | (2) |
| $h(X_i)$: Expected Segregation under Random Assignment$^a$ | 1.183*** (.037) | 1.202 (.141) | 1.094*** (.026) | 1.390*** (.051) |
| $h(X_i)^2$          | -0.146 (.1298)           |     | -2.765*** (.382) |
| Intercept           | .012*** (.001)            | .011*** (.003) | .008*** (.001) | .003*** (.001) |
| $R^2$               | .11                      | .11 | .08               | .08 |
| BIC                 | -120,492                 | -120,483 | -129,724          | -129,774 |
| N                   | 40,233                   | 40,233 | 40,237            | 40,237 |

* $p < .05$, ** $p < .01$, *** $p < .001$

$^aH_0: \beta_1 = 1$

Note: Models include all years, stacked, with Huber-White standard errors to account for heteroscedasticity. Four school-observations are missing when using trichotomous race because schools with no students who selected one of the three categories.

Fig. B1. Using trichotomous race (black vs *parda* vs white): Racial segregation decomposed by segregation scale, by year.

*Note:* Total segregation between classrooms across the entire nation ($H_t$, reported at top) is divided into its constituent parts (Reardon and Firebaugh 2002).

**B2.** Using dichotomous race (white vs non-white): Racial segregation decomposed by segregation scale, by year.

*Note:* Total segregation between classrooms across the entire nation ($H_t$, reported at top) is divided into its constituent parts (Reardon and Firebaugh 2002).

Fig. B3. Using trichotomous race (black vs *pardo* vs white): Mean classroom segregation by expected segregation under random assignment (all years).

* $p < .05$, ** $p < .01$, *** $p < .001$ where $H_0: \beta_1 = 1$.

*Note:* Estimates are from Table C1. Bin size is weighted by number of schools. School-observations in the top 1% of expected segregation are not shown.

Fig. B4. Using dichotomous race (white vs non-white): Mean classroom segregation by expected segregation under random assignment (all years).

* $p < .05$, ** $p < .01$, *** $p < .001$ where $H_0: \beta_1 = 1.$

*Note:* Estimates are from Table C1. Bin size is weighted by number of schools. School-observations in the top 1% of expected segregation are not shown.

**Fig. B5.** Using trichotomous race (black vs *parda* vs white): Across-year within-school pairwise correlations of schools’ classroom segregation levels net of expected segregation under random assignment.

*Note:* Scatterplots suppress outliers, but correlations are computed using the full sample.

Fig. B6. Using dichotomous race (white vs non-white): Across-year within-school pairwise correlations of schools’ classroom segregation levels net of expected segregation under random assignment.

Note: Scatterplots suppress outliers, but correlations are computed using the full sample.

Fig. B7. Using trichotomous race (black vs *parda* vs white): Cumulative density functions conditional on expected segregation under randomization.

*Note:* Random segregation was simulated \( n = 20 \) within each school-observation given observed classroom sizes and race group proportions. Bins are equally wide and drawn from the highest density region of expected segregation under randomization, altogether containing 81% of school-observations. School-observations in the top 1% of expected segregation are not shown.

Fig. B8. Using dichotomous race (white vs non-white): Cumulative density functions conditional on expected segregation under randomization. 

*Note:* Random segregation was simulated \( (n = 20) \) within each school-observation given observed classroom sizes and race group proportions. Bins are equally wide and drawn from the highest density region of expected segregation under randomization, altogether containing 86% of school-observations. School-observations in the top 1% of expected segregation are not shown.

Fig. B9. Using trichotomous race (black vs *parda* vs white): Proportion of classroom segregation that is segregation by chance, by observed classroom segregation level (all years).

*Note:* Estimates are based on Table B1, where the best estimate is \( \pi_i = \frac{\hat{\beta}_1 h(X_i)}{\hat{H}_i} = \frac{\hat{H}_i - \hat{\beta}_0}{\hat{H}_i} \) and the conservative estimate is \( \pi_i^* = \frac{h(X_i)}{\hat{H}_i} = \frac{1}{\hat{\beta}_1} \pi_i \). The kernel density plot uses the Epanechnikov kernel. The 0.1% most segregated schools are omitted.

Fig. B10. Using dichotomous race (white vs non-white): Proportion of classroom segregation that is segregation by chance, by observed classroom segregation level (all years).

Note: Estimates are based on Table B1, where the best estimate is \( \pi_i = \frac{\beta_1 h(X_i)}{\bar{H}_i} = \frac{\bar{H}_i - \bar{\beta}_0}{\bar{H}_i} \) and the conservative estimate is \( \pi_i^* = \frac{h(X_i)}{\bar{H}_i} = \frac{1}{\bar{\beta}_1} \pi_i \). The kernel density plot uses the Epanechnikov kernel. The 0.1% most segregated schools are omitted.

APPENDIX C. Year-Specific Versions of Stacked Analyses

Table C1. OLS regression estimates of the relationship between observed classroom segregation and expected segregation under random assignment, by year.

<table>
<thead>
<tr>
<th></th>
<th>2011</th>
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<th>2013</th>
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<th>2015</th>
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<td></td>
<td>(1)</td>
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<tr>
<td>$h(X_i)$: Expected Segregation(^a)</td>
<td>1.108***</td>
<td>1.309***</td>
<td>1.081**</td>
<td>1.037</td>
<td>1.054*</td>
<td>1.329**</td>
</tr>
<tr>
<td></td>
<td>(.024)</td>
<td>(.093)</td>
<td>(.028)</td>
<td>(.118)</td>
<td>(.026)</td>
<td>(.119)</td>
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<tr>
<td>$h(X_i)^2$</td>
<td>-1.656*</td>
<td>.386</td>
<td>-2.432*</td>
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<tr>
<td></td>
<td>(.780)</td>
<td>(1.005)</td>
<td>(1.041)</td>
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<tr>
<td>Intercept</td>
<td>.018***</td>
<td>.012***</td>
<td>.017***</td>
<td>.018***</td>
<td>.020***</td>
<td>.013***</td>
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<td>(.001)</td>
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<tr>
<td>$R^2$</td>
<td>.14</td>
<td>.14</td>
<td>.13</td>
<td>.13</td>
<td>.11</td>
<td>.11</td>
</tr>
<tr>
<td>BIC</td>
<td>-53115</td>
<td>-53113</td>
<td>-36201</td>
<td>-36192</td>
<td>-47262</td>
<td>-47260</td>
</tr>
<tr>
<td>N</td>
<td>15,762</td>
<td>10,642</td>
<td>13,833</td>
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</tr>
</tbody>
</table>

\* $p < .05$, ** $p < .01$, *** $p < .001$

\(^a\) $H_0: \beta_1 = 1$

Note: Using Huber-White standard errors to account for heteroscedasticity.

Fig. C1. Mean classroom segregation by expected segregation under random assignment, by year.

* $p < .05$, ** $p < .01$, *** $p < .001$ where $H_0: \beta_1 = 1$.

Note: Estimates are from Table C1. Bin size is weighted by number of schools. School-observations in the top 0.1% of expected segregation are not shown.

**Fig. C2.** Proportion of classroom segregation that is segregation by chance, by observed classroom segregation level, by year.

*Note:* Estimates are based on Table 1, where the best estimate is $\pi_i = \frac{\hat{\beta}_1 h(X_i)}{\hat{H}_i} = \frac{\hat{H}_i - \hat{H}_0}{\hat{H}_i}$ and the conservative estimate is $\pi_i^* = \frac{h(X_i)}{\hat{H}_i} = \frac{1}{\hat{\beta}_1} \pi_i$. The kernel density plot uses the Epanechnikov kernel. The 0.1% most segregated schools are omitted.

APPENDIX D. Further Tests of Prediction 2

Fig. 7 and 8 affirm Prediction 2, that the conditional distribution of segregation has a similar pattern of change as $h(X_t)$ increases as it would under random assignment. However, as noted in the text, the observed and random conditional distributions do not align because there is some active segregation and reaction to chance. This appendix builds on the Prediction 2 analysis by considering whether there is a data-generating process in which (1) random segregation is the only source of change in the conditional distribution as $h(X_t)$ increases and (2) the overall and conditional distributions align with the observed data? In other words, is it plausible that the observed pattern in the conditional distributions is indeed due to the pattern under randomization shown in Fig. 7?

Why don’t the observed and randomly-generated conditional distributions align? First, $\hat{H}_i(h(X_t))$ suggests small but non-zero average active segregation and a modest average reaction to chance segregation. These forces will shift and stretch the conditional distribution, respectively. Furthermore, schools presumably have their own latent response functions – that is, they vary in amount of active segregation and reactions to chance segregation – so the true data-generating process is more complex than $\hat{H}_i(s_i)$, where $s_i$ is segregation under randomization in school-iteration $i$. This variation in active segregation and reaction to chance segregation, as well as their covariance, also alters the shapes of the conditional distributions.

Given the estimate average effect coefficients $\hat{\beta}_0$ and $\hat{\beta}_1$ from $\hat{H}_i(h(X_t))$, I consider plausible functions $H_i(s_i)$ where

$$H_i(s_i) = \beta_0 + \beta_1 s_i,$$

(??)
\[
\beta_{0i} = \hat{\beta}_0 + u_i, \quad \Phi^{-1}(u_i) \sim N(0, \tau_u)
\]

\[
\beta_{1i} = \begin{cases} 
\frac{-\beta_{0i}}{s_i}, & H_i^*(s_i) < 0 \\
\frac{1 - \beta_{0i}}{s_i}, & H_i^*(s_i) > 1 \\
\beta_{1i}^*, & 0 \leq H_i^*(s_i) \leq 1
\end{cases}
\]

\[
\beta_{1i}^* = \hat{\beta}_1 + v_i, \quad v_i \sim N(0, \tau_v)
\]

\[
\begin{pmatrix} u_i \\ v_i \end{pmatrix} \sim N \left( \begin{pmatrix} \tau_u \\ \tau_{uv} \end{pmatrix} \right).
\]

Note that \(\beta_{1i}\), the school-specific average reaction to chance segregation, is a latent response \(\beta_{1i}^*\) truncated when it would produce segregation above 0 or below 1, the limits of segregation. Note also that the variation and covariation of \(\beta_{0i}\) and \(\beta_{1i}\) are the same over \(h(X_i)\), by design. Though it is plausible that the true data-generating process is more complex, this ensures that I am not simply using the school-specific response function to reproduce the pattern of conditional distribution change over \(h(X_i)\). Instead, the conditional distribution pattern in \(H_i(s_i)\) is entirely due to changes in the conditional distribution of \(s_i\).

I consider different values for \(\tau_u, \tau_v\), and \(\tau_{uv}\) with the aim of minimizing the difference between the overall distributions of \(H_i(s_i)\) and observed segregation, measured as the two-sample Kolmogorov-Smirnov statistic. Fig. D1 compares the observed distribution to the distribution simulated using my preferred function \(H_i(s_i)\) where

\[
\begin{pmatrix} u_i \\ v_i \end{pmatrix} \sim N \left( \begin{pmatrix} .122 \\ .060 \end{pmatrix} \right).
\]

The distributions are plotted with 83% Dvoretzky–Kiefer–Wolfowitz bands such that the null hypothesis that both samples are drawn from the same distribution is rejected when the bands do not overlap. Fig. D1 shows that the overall distributions are closely aligned such that they do not appear to describe samples from different distributions.
Fig. D2 makes similar comparisons among the conditional distributions in bins 1, 5, 10, 15, and 20. The simulated and observed conditional distributions are similar, but not exactly aligned. For the most part, they differ significantly when segregation at low percentiles of segregation. At these values, it appears that the school-specific response function smooths out differences between the conditional CDFs. This can be seen by comparison to Fig. 7, where the right-ward shift of the conditional CDFs of random segregation is far greater at low percentiles. Specifying $H_i(s_i)$ to allow for more complex variation or assume different distributions of the school-specific response parameters might improve upon these results.

In the main text, the hypothesized model accurately predicted that the conditional distribution of segregation would have decreasing right-skew and increasing variance in line with what we see under random assignment. This toy exercise nearly reproduces the observed conditional distributions using a parsimonious function of random segregation, affirming that the observed pattern is plausibly due to the heteroscedasticity in random segregation.
Fig. D1. Cumulative density functions, observed vs approximate segregation.

Note: Approximate segregation is the simulated function of random segregation given by Eq. D1. The 83% confidence bands are given by the Dvoretzky–Kiefer–Wolfowitz (DKW) bounds such that overlapping bands indicate we cannot reject the null ($p < .05$) that the two distributions are drawn from the same underlying distribution. The 1% most segregated schools are not shown.

Fig. D2. Conditional cumulative density functions, observed vs approximate segregation.

Note: Each band is the 83% Dvoretzky–Kiefer–Wolfowitz (DKW) bounds of a conditional CDF where overlapping bands indicate we cannot reject the null ($p < .05$) that the two distributions are drawn from the same underlying distribution. Approximate segregation is the simulated function of random segregation given by Eq. D1. The CDFs are drawn within equally wide small bins of expected segregation under random assignment. Bins 1, 5, 10, 15, 20 are presented. The 5% most segregated schools are omitted. Source: Prova Brasil 2011, 2013, 2015.
APPENDIX E. Computing the Proportion of Segregation by Chance

Following Reardon and Firebaugh (Reardon and Firebaugh 2002), the proportion \( \pi_t \) of total segregation in the Brazilian school system \( (H) \) that is classroom segregation in year \( t \) is given by

\[
\pi_t = \sum_{j=1}^{j} \frac{N_{jt}E_{jt}H_{jt}}{N_tE_H} 
\]

where \( N_{jt} \) is number of students and \( E_{jt} \) is the entropy measure of racial diversity in school \( j \) and year \( t \). The latter is given by

\[
E_{jt} = \sum_{r=1}^{r} p_{rjt} \ln \left( \frac{1}{p_{rjt}} \right) 
\]

where \( p_{rjt} \) is the proportion of students who chose racial category \( r \) in school \( j \) and year \( t \).

Using the estimates in Table 1, I compute the total chance segregation between classrooms in school-observation \( i \) as \( H^C_i = H_i - \bar{\beta}_0 \) for the primary estimate and \( H^C_i = \frac{H_i - \bar{\beta}_0}{\bar{\beta}_i} \) for the conservative estimate. I compute the proportion in year \( t \) \( (\pi^C_t) \) similarly, replacing \( H_{jt} \) with \( H^C_i \) in Eq. 6. Over all years, the proportion of classroom segregation that is attributable to either chance-only segregation or reactions to it is then

\[
\pi^C = \frac{1}{N} \sum_{t=1}^{t} N_t \frac{\pi^C_t}{\pi_t} 
\]