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The 'Good' Schools: Academic Performance Data, School Choice, and Segregation

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The 'Good' Schools:
Academic Performance Data, School Choice, and Segregation

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Abstract

We examine the effects of disseminating academic performance data—either status, growth, or both—on parents' school choices and their implications for racial, ethnic, and economic segregation. We conduct an online survey experiment featuring a nationally representative sample of parents and caretakers of children age 0-12. Participants choose between three randomly sampled elementary schools drawn from the same school district. Only growth information—alone and not in concert with status information—has clear and consistent desegregating consequences. Because states that include growth in their school accountability systems have generally done so as a supplement to and not a replacement for status, there is little reason to expect that this development will influence choice behavior in a manner that meaningfully reduces school segregation.

Key Words

growth, status, school choice, segregation, desegregation

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Pre-Registration

This experiment has been pre-registered on the American Economic Association's registry for randomized controlled trials (Trial # 7014).

Replication

Data and code for replication purposes are available upon request.

Introduction

Framed in the abstract, most Americans express support for diversity in society and schools. In 2019, Pew Research Center found that more than three-quarters of Americans said that the fact that the U.S. population is made up of people of many different races and ethnicities was either "good" or "somewhat good," while five percent felt it was "somewhat bad" and only one percent considered it "very bad" (Horowitz, 2019). In 2021, The Century Foundation found that 84 percent of respondents considered it at least somewhat important to them that public schools in their community "have a mix of students from different racial/ethnic backgrounds," with just under half saying it was "extremely" (26%) or "very" (23%) important to them (Potter et al., 2021). But there is reason to question the sincerity of this sentiment—especially when it comes to translating intention into behavior. Americans' support for diverse schools may have limits when it runs counter to their freedom to choose where to live or where to send their own child to school.

Expressed intentions aside, schools in the U.S. remain racially defined. White students, on average, attend schools in which roughly two-thirds of their fellow students are also White. Meanwhile, Black and Hispanic students, on average, go to schools where roughly two-thirds of their fellow students are also Black or Hispanic (de Brey et al., 2019). The concentration of students by race and ethnicity often occurs even within putatively diverse districts: "Even white families that choose to live in racially diverse school districts…often make neighborhood and school selections that enroll their children in the district's "whitest" schools rather than schools that fully reflect the larger community's diversity…For example, only 29 percent of first-grade students are white in relatively diverse metropolitan areas, but the average white student in these areas attends an elementary school that is 53 percent white" (Turner et al., 2021).

Despite the persistence of segregated schools, the empirical evidence demonstrating the positive effects of integration is robust. Multiple studies find that students of color perform better academically when they move from racially isolated educational environments to racially integrated ones (Billings et al., 2014; Bergman, 2018; Guryan, 2004; Johnson, 2019). Moreover, concerns that the introduction of less-advantaged students into Whiter and wealthier institutions may negatively affect their new classmates appear to be unfounded. Researchers generally do not observe negative effects of integration-oriented school reassignment programs on the academic outcomes of students in the receiving schools (Angrist & Lang, 2004; Johnson, 2019).

The resilience of school segregation is grounded in powerful systemic forces that flow from a long history of legal and extra-legal discrimination in housing and educational opportunity as well as parental fears, suspicions, beliefs, and misbeliefs (Rothstein, 2017). Achieving diverse schools at scale will depend on addressing these issues, but it is not clear that either the political will or the government capacity to do so exist at present. However, two developments potentially alter the landscape of American education in ways that could nudge families in the direction of selecting more diverse schools. On their own, such behavioral shifts are unlikely to undo centuries of segregation in one fell swoop, but they could improve meaningfully on the status quo.

The first development involves the increase of public school choice options that partially uncouple school assignment from a family's place of residence. While communities across the U.S. are rapidly diversifying, residential segregation remains a persistent feature of American life (Lichter et al., 2015). To the extent that these demographic divisions endure, short-term progress on school desegregation may rely on loosening the relationship between families' home addresses and their school assignments. However, the integrative promise of school choice

reforms has thus far been met mostly with disappointment, as many families choose schools in a manner that exacerbates rather than diminishes existing segregation (Weiher & Tedin, 2002; Garcia, 2008; Frankenberg & Lee, 2011, Kotok et al., 2015).

The second development is the growing availability of academic performance data that more accurately captures schools' contributions to student learning, rather than merely reflecting the social and economic advantages borne by the students they serve. The conventional wisdom often holds that the "best" schools are those attended primarily by the children of the most socially and economically privileged families. Many of these schools are indeed highly effective institutions, but so are many other schools that have large positive effects on their students' educational outcomes—even if those students have fewer initial advantages. The collection and dissemination of better measures of school effectiveness could weaken the relationship between Americans' perceptions of school quality and the racial, ethnic, and economic composition of the student body. If so, then the conditions may be more favorable moving forward for the posited desegregating consequences of school choice.

We distinguish between two ways of measuring students' academic performance: achievement *status* and achievement *growth*. Achievement status (sometimes referred to as "achievement levels" or simply "achievement") measures students' academic performance at a single point in time. Examples of school-level status include average standardized test scores or the percentage of students scoring above a pre-designated proficiency threshold. Following the passage of the No Child Left Behind Act of 2002 (NCLB), all states were required to calculate and disseminate achievement status information for each school (Hess & Petrilli, 2007). Scholars criticized the use of achievement status as an indicator of school performance, arguing that school-level status largely reflected the demographic composition of the student body rather than

school effectiveness (Chingos & West, 2015; Rothstein et al., 2008; Schneider, 2017).

Achievement growth, on the other hand, measures the rate of improvement in students' academic performance over time. There are a variety of ways to measure growth, ranging from a simple year-to-year difference in an individual student's test scores to more complex statistical models that compare a student's performance to peers with similar prior test scores and, in some cases, similar demographic backgrounds (when aggregated to the teacher or school level, the latter approach is often referred to as a value-added model) (Castellano & Ho, 2013; Harris & Weingarten, 2011). Growth has two chief advantages over status as a measure of school quality. First, because school-level growth measures changes in academic performance during students' time in school, it more accurately reflects the effects of educators on student learning (Stiefel et al., 2011). Second, school-level growth bears a much weaker underlying relationship to the racial, ethnic, and economic composition of the student body, making it easier to identify highly effective schools, regardless of the kinds of the students they serve (Reardon, 2019).

Beginning in 2005, states could apply to the U.S. Department of Education to supplement their NCLB-mandated status-oriented accountability systems with an additional growth-based indicator (U.S. Department of Education, 2005). Following the passage of the Every Student Succeeds Act of 2015 (ESSA), all states are required to use multiple measures—often including a measure of growth—to assess school performance (Barone, 2017). As of January 2020, 43 states and the District of Columbia calculated school-level indicators of growth and included this information in addition to school-level status in their annual school report cards (Data Quality Campaign, 2020).

In this article, we examine the effects of disseminating academic performance data—either status, growth, or both—on parents' school choices and their implications for racial,

ethnic, and economic segregation. We conduct an online survey experiment featuring a nationally representative sample of parents and caretakers of children age 0-12. In this experiment, participants make a series of choices between three randomly sampled elementary schools drawn from the same school district (the districts themselves are also randomly sampled from all districts serving grades 3-8 nationwide, with sampling weighted by district size). To guide this choice, participants receive a range of demographic information about each school. In addition, some participants are randomly assigned to receive information about each school's average achievement status and/or average achievement growth.

One commonly expressed reservation about survey experiments is that they sacrifice external validity (the capacity to generalize the results beyond the experimental context) in the pursuit of internal validity (the capacity to generate an unbiased estimate of the average effect of the treatment). By their nature, they are artificial and contrived. We share that reservation, but we have also taken some unusual steps to reduce the artificiality. We ask participants to choose between real schools in real school districts using actual academic performance and demographic data drawn from the Stanford Education Data Archive, or SEDA. Participants complete this exercise six times. For one of these exercises, they choose between three randomly selected schools drawn from their own local school district, allowing us to probe whether possible exposure to information outside of that provided by the experiment leads to different results.

We find that, in the absence of either status or growth information (i.e., the control group), participants tend to choose schools that are relatively White, affluent, and close to home. The provision of status information steers participants towards higher status schools, which also tend to be less Black, less Hispanic, and more affluent than the schools chosen by the control group. By contrast, the provision of growth information steers participants towards higher

growth schools, which are, on average, less White and less affluent than the schools chosen by the control group. The provision of both status and growth information steers participants towards schools with both higher status and higher growth—but not towards schools that are any more diverse than those chosen by the control group. These patterns generally hold even when participants choose among schools from their own district. In the context of our experiment, only the provision of growth information—alone and not in concert with status information—has clear and consistent desegregating consequences. Because states that include growth in their school accountability systems have generally done so as a supplement to and not a replacement for status, there is little reason to expect that this development in its current form will influence parental choice behavior in a manner that meaningfully reduces school segregation.

Literature Review

School Choice and School Segregation

The nation's history of school choice is tightly entwined with its history of school segregation, but the relationship is neither simple nor straightforward. Families' choices among schools, school sectors, school districts—their exercise of what economists characterize as "the exit option" (Hirschman, 1970) or "voting with their feet" (Tiebout, 1956)—have often frustrated efforts to create schools that are internally diverse. Advocates have long maintained that policies that increase the number of educational options for families can also increase school diversity by allowing students to attend schools outside of their own neighborhood (e.g., Osborne & Langhorne, 2017). However, the evidence supporting this argument is decidedly mixed. In many—but not all—circumstances, the expansion of school choice appears to exacerbate rather than ameliorate existing patterns of segregation (Weiher & Tedin, 2002; Garcia, 2008; Bifulco et al., 2009; Frankenberg & Lee, 2011; Kotok et al., 2015). Both the details of the school choice

policy and the circumstances of the place where it is implemented matter immensely.

In one of its early iterations, school choice was a tool with the expressed intent of preserving racial segregation—one element of the "massive resistance" southern states mounted in response to *Brown v. Board of Education* (Orfield, 1969). By closing or defunding public schools while providing White families tuition grants to attend private segregated academies, districts deliberately cultivated school choice as a strategy to sidestep the Supreme Court's order to desegregate public schools. The Supreme Court subsequently outlawed such use of public funds when pursued with the specific intent to support segregated private schools, but the choice of many White and affluent families to opt for private over public schools remains a barrier to more diverse traditional public school systems (Orfield, 1969).

As federal courts shifted their focus to segregated schools in the north and west, additional forms of school choice emerged as vehicles available to White families reluctant to have their children attend integrated schools. Most visible and important was the choice to engage in "White flight" to suburban districts that were overwhelmingly White and armed with a variety of policy tools to help keep them that way (Holme & Finnegan, 2018). Between 1950 and 1980, the percentage of White Americans living in suburbs nearly doubled (increasing from 24.5% to 47.4%); during the same period, the proportion of Black Americans living inside central cities rose from 41.2% to 59.7% (National Research Council, 2001). Demographic-induced changes in school enrollment and judicially-mandated desegregation orders were not the only factors driving White suburbanization, and scholars at the time clashed over the question of how much responsibility to attribute to school-oriented causes versus other factors (see, for example, Coleman et al., 1976; Farley, 1976; Orfield, 1976; Pettigrew, 1976). Subsequent analyses support the view that while White flight from school desegregation was not the only

culprit, it was an important contributing factor, especially in those metropolitan areas where the degree of change crossed a "tipping point" (Baum-Snow & Lutz, 2011; Boustan, 2010; Clotfelter, 2004; Card et al., 2008).

Although it received less attention at the time, another form of choice—one that also hinged on residential choice—was sorting into "enclave schools," which were racially and socioeconomically distinct neighborhood schools within central cities (Henig et al., 1999; McDermott et al., 2015). Shifting among neighborhoods within cities is less costly than opting to move to a different school district, making it attractive especially to those who valued urban life and proximity to their places of employment. Preserving enclave schools, however, required that White and affluent residents make an ongoing investment of political energy and resources in order to defend favorable attendance zones and transfer practices (Henig et al., 1999; McDermott et al., 2015; Trounstine, 2018).

School choice is not always anathema to diverse schools. Some have touted it as a less politically contentious and potentially more sustainable policy instrument for integration (Osborne & Langhorne, 2017; Kahlenberg, 2001; Kahlenberg, 2012). Magnet schools—schools with special themes and for which enrollment is not strictly limited by neighborhood attendance zones—emerged in the late 1960s primarily as a tool for encouraging White families to send their children to more integrated schools in more integrated neighborhoods (Steel & Eaton, 1996; Goldring & Smrekar, 2000). While most magnet programs used choice primarily as a way to retain families that might otherwise have opted to exit, some took on the bigger challenge of inducing flows of students across city-suburb boundaries, inviting families of color to send their children to predominantly White suburban schools or enticing White suburban families to send their children to city schools with special funding and academic themes (Eaton, 2001; Bifulco et

al., 2009; Holme & Finnegan, 2018; Mantil, 2021).

Charter schools—publicly funded but privately run schools that also allow families to choose them regardless of their neighborhood of residence—emerged in the 1990s. While charter schools were promoted primarily based on their potential to improve student performance, early advocates also argued that they could result in school communities that were more demographically diverse (Hassell, 1999). Like magnet schools, charters weakened the previously tight bond between the school demographics and the demographics of the surrounding neighborhood. If parents exercise choice based on race-neutral criteria such as academic performance, it was argued, charters could provide more demographically integrated schooling options than those available in traditional districts with assigned zones that reified segregated housing patterns. The empirical research suggests that, overall, the proliferation of charter schools has been more likely to increase rather than decrease racial, ethnic, and economic segregation within school districts (Finnegan et al., 2004; Frankenberg et al., 2011; Garcia, 2007; Kotok, 2017; Marcotte & Dalane, 2019; Monarrez et al., 2021). On the other hand, growth in the charter school sector also corresponds with declining segregation between districts (Monarrez et al., 2021). Moreover, there is a small core of advocates, schools, and funders committed to creating charters that are "diverse-by-design." In 2018, researchers at the Century Foundation found that 20% of charters showed some consideration of diversity in their school model, and they identified 125 intentionally diverse charter schools with an institutional commitment to racially and economically integrated enrollment (Potter & Quick, 2018).

That the literature fails to provide a consistent link between choice and school diversity reflects in part its tendency to refer to school choice in a broad and undifferentiated manner that fails to draw potentially important distinctions among types of choice. One distinction has to do

with the difference between *place-based* school choice (actuated via residential relocation) and *non-place-based* school choice that eliminates the link between residential location and eligibility to attend a school. A second distinction is between *inter-district* and *intra-district* choice. A third involves the difference between *laissez faire* choice (actuated by unregulated individual family decisions) and *managed* choice, wherein government and other institutions attempt to channel choices in socially desirable directions, via regulation, incentive, or information provision. These various types of choice differ in ways likely to affect the extent and direction of change as well as the likelihood of engendering enthusiastic uptake versus resistance and backlash. Our analysis in this article focuses on non-placed-based, intra-district choice, with specific attention to the question of whether government can steer choices toward more diverse schooling outcomes through intentional policies regarding information dissemination.

The Expansion of Intra-District Choice

Among both academics and policy-makers, the most noteworthy forms of school choice have been those involving suburban flight, charter schools, and private schools. There are good reasons for this. Even when prompted by schooling options, residential relocation to the suburbs has had broader and sometimes devasting impacts on central cities' political economy and social well-being (Kang, 2021). Charters and private school choice warrant attention because of their role in broader debates about the proper role of government versus privatization (Henig, 2018). Over the past several decades, however, an array of new options has expanded, providing opportunities for households to exercise school choice without having to abandon cities or traditional public schools. Charters are part of this new landscape of intra-district public school choice, but so are an array of new options that have taken root more quietly.

The standard portrayal of public education in the U.S. is of a system comprising place-

based neighborhood schools with enrollment limited to those within defined attendance zones. This was—and still is—accurate overall, but it was never as monolithic as portrayed, and its dominance has been waning. With limited publicity and often without a broad policy or plan, school districts have quietly incorporated elements of the magnet and charter school models into their standard operating procedures. One manifestation of this is "open enrollment": a form of public school choice that allows students to transfer to any school within a district (intra-district open enrollment) or between districts in the same state (inter-district open enrollment) as long as space is available. As of 2018, 33 states and the District of Columbia had intra-district open enrollment policies, while 43 states and the District of Columbia had inter-district open enrollment policies (Wixom, 2019).

Relatedly, a number of districts have adopted a "portfolio management model" in which the central district oversees an array of different types of schools with a variety of student assignment mechanisms (Hill et al., 2013; Bulkley et al., 2020). In school districts such as Denver, New Orleans, and Washington, D.C., students are able to apply to any school in the district via a single common application system (Hesla, 2018). In other places, the shift has been more incremental and sometimes based on a reorientation of administrative routines rather than a formally announced policy. Some districts have gradually and quietly loosened the criteria for requesting a transfer to a school outside the family's assigned attendance zone. Whereas once this may have required parents to document that their zoned school was unable to meet their child's specific needs, some districts now default to accepting transfer requests as long as space is available. Furthermore, whereas once this may have required parents to schedule meetings with the principal of the receiving school, some districts now provide simple online options for submitting transfer requests.

Academic Performance Information and School Choice

When asked about what they value most when choosing schools, parents generally emphasize academic performance (Haderlein, 2021). Yet internet search data indicate that parents inquire for information about other school characteristics—such as student demographics—more often than they inquire about student achievement (Schneider & Buckley, 2002; Dougherty et al., 2013). The most persuasive studies of parents' revealed school preferences rely on parents' rank-ordered choices in districts that employ a centralized application system in concert with various intra-district school choice policies. In these contexts, parents appear to prioritize student test scores and other measures of achievement status, student demographics (specifically, schools in which their child would be a part of the racial/ethnic majority), and proximity to home when ranking schools (Denice & Gross, 2016; Glazerman & Dotter, 2017; Harris & Larsen, 2019). Parents do not appear to value schools with higher rates of achievement growth (Abdulkadiroglu et al., 2020; Glazerman & Dotter, 2017). The absence of interest in higher growth schools, however, could be explained by the fact that most Americans are largely unaware of how their local schools perform in this regard (Houston et al., 2021).

The availability of information pertaining to these preferences can influence parents' school choices. Large-scale field experiments in Charlotte-Mecklenberg, New Orleans, and New York City demonstrate that the distribution of academic performance information can increase enrollment in higher performing schools according to the metrics provided (Corcoran et al., 2018; Hastings & Weinstein, 2008; Valant & Weixler, 2020). The experiment in New Orleans is particularly relevant to our study, as it indicates that the distribution of information about student growth can steer parents towards higher growth schools (Valant & Weixler, 2020). Similarly, in the context of an online survey experiment, the distribution of district-level student growth

information can steer individuals towards higher-growth districts within a metropolitan area (Houston & Henig, 2021). Importantly, the shift towards higher-growth districts coincides with a shift towards less White and less affluent districts.

In this study, we extend the external validity of previous work along several important dimensions. First, our experiment features a nationally representative sample of parents and/or caretakers of children age 0-12. Prior studies focused on school choices in specific and idiosyncratic districts, or they featured samples that were broader but not representative of the primary group that makes school choices: parents of young children. Second, the participants in our experiment choose between randomly sampled schools in a representative sample of school districts from across the country. Again, this expands upon previous district-specific studies. It also provides us the opportunity to learn about the kinds of districts where the provision of status and/or growth information may facilitate desegregation, where it would likely have no effect, and where it could potentially exacerbate pre-existing segregation.

The multi-dimensional representativeness of our research design comes with an important trade-off. Our experiment takes place in the context of an online survey in which participants have no personal stake in the school choices that they indicate. This loss of realism is a threat to external validity along a different dimension. We take two additional steps to address this concern. First, participants are making choices between real schools in real districts based on real demographic and educational data. An alternative strategy could have constructed idealized alternatives that more sharply delineate the academic performance and demographic attributes of the schools. Constraining choices to genuine options forces respondents to make tougher decisions between schools that differ in degree but not sharply in kind, possibly muting the patterns of selection we can isolate, but enhancing the verisimilitude of our experimental context.

Second, we also ask participants to choose a school from a set of options located in their own local school district. We expect some of the participants to recognize some of these schools, allowing them to supplement the information that we provide with their own personal knowledge of the schools and their reputations. Asking participants to choose between schools in their home district ought to increase the extent to which our stylized survey experiment reflects the actual experience of weighing the strengths and weaknesses of local educational options.

Methods

School Data

In our experiment, we use the Stanford Education Data Archive v4.0 (SEDA) for measures of school-level average achievement status, average achievement growth, and student demographics (Fahle et al., 2021). The test score data in SEDA are drawn from the ED*Facts* data system maintained by the U.S. Department of Education, which collects standardized test score data from each state. The test score data in ED*Facts* are coarsened (the percentage of students meeting various levels of performance) rather than raw scale scores, and they are aggregated at the school-subgroup-subject-grade-year level (subgroups include race/ethnicity, gender, and socioeconomic disadvantage). Based on these values, the research team that maintains SEDA estimates school-level average scores and converts them to a common scale to allow the comparison of student achievement in states that employ different standardized tests (Reardon et al., 2017; Reardon et al., 2019).

SEDA contains school-level test score and demographic data for nearly every U.S. public school that serves students in grades 3-8 from 2009 to 2018. We focus on the subset of elementary schools (defined as containing grade three) that are in districts that have at least three such schools from which participants in our experiment can choose. This subset consists of

31,391 schools in 2,894 districts. For each school, we use the empirical Bayes grade cohort scale estimates of average status and average growth, pooled across all grades, years, and subjects.

The SEDA website features a graphical user interface that allows general users to explore school-level and district-level academic performance data without specialized software. This platform features simplified language when reporting values like status and growth. When presenting academic performance information to the participants of our experiment, we adopt the same language used on the website as of March 2021. For example, when presenting status information for a school, we use the phrase "Average Test Scores: Students score ___ grade levels above/below the U.S. average." When presenting growth information for a school, we use the phrase "Learning Rates: Students learn ___% more/less each grade than the U.S. average."

Survey Experiment

We partnered with the survey research firm YouGov to recruit a nationally representative sample of 2,800 parents/caretakers of children age 0-12 for an online survey. YouGov interviewed 3,209 participants who were then matched down to a sample of 2,800 using a sampling frame on gender, age, race, and education based on the American Community Survey 1-year sample. YouGov also collected other basic demographic information for each participant. The survey took place from March 16-31, 2021.

The survey begins by randomly assigning participants to one of four groups:

1. Control group: participants receive neither status nor growth data when choosing schools
2. Status group: participants receive status data when choosing schools
3. Growth group: participants receive growth data when choosing schools
4. Both group: participants receive both status and growth data when choosing schools

Next, the survey asks participants to identify their local public school district. We provide each participant with a list of one or more school districts associated with their zip code based on the 2019 school district geographic relationship files maintained by the U.S. Department of Education (Geverdt, 2021). Six percent of participants did not recognize any of the school districts on their list. Furthermore, 17 percent of participants live in zip codes in which the local district does not contain three eligible elementary schools from which to choose; they are not asked to identify their local district.

All participants are then given the following prompt:

> Please imagine that you are looking for a new school for an elementary school-age child in your family. We will provide basic information about three schools from one school district. This information comes from the Stanford Education Data Archive, which provides accurate data about real schools across the United States. You will be asked to choose the school that best meets the needs of your family.

The next six survey items feature a series of choices between three randomly sampled elementary schools drawn from a randomly sampled district (with district sampling weighted by total enrollment to produce a proportional number of large and small districts). In one instance, participants choose between three randomly sampled elementary schools drawn from their own local school district. If participants are unable to identify their home district or if they live in a district without three elementary schools, all six of the districts they encounter are random. To avoid ordering effects, the position of the home-district item in the survey sequence is random.

For each school option, all participants receive a series of demographic and geographic data points: total enrollment; the percentage of White, Black, and Hispanic students; the percentage of students of another race/ethnicity; the percentage of economically disadvantaged students (defined as eligible for free and reduced-price lunch, or FRPL); the percentage of students with limited English proficiency (LEP); and the distance from home (which is set to

vary randomly between 10, 20, and 30 minutes away). In addition, participants in the status group receive information about the school's average status, participants in the growth group receive information about the school's average growth, and participants in the both group receive both of these types of academic performance information (with the order of the information randomized to avoid ordering effects). See Figure 1 for an example of a school choice survey item as seen by a participant in the both group.

[Figure 1 about here]

Analytic Approach

To check for balance between experimental groups, we compare the demographic composition of the control group with the demographic compositions of each of the other randomly assigned groups. To accomplish this, we use a series of ordinary least squares (OLS) regression equations:

$$X_i = a + bS_i + cG_i + dB_i + u_i, \qquad (1)$$

where X_i is one of the available demographic characteristics collected by YouGov; S_i, G_i, and B_i are indicators of experimental group status (the status group, the growth group, and the both group); and u_i is the error term for participant i.

To estimate the average effects of status and/or growth information on the characteristics of the schools chosen by participants, we use the following OLS regression equation:

$$Y_i = a + bS_i + cG_i + dB_i + u_i, \qquad (2)$$

where Y_i is one of the school characteristics featured in the survey (e.g., average status, average growth, total enrollment, percent White, etc.), averaged across the participants' multiple school choices. Because the variation in school-level demographics and academic performance differs from district to district, we standardize these outcomes within each choice set. We take each

characteristic of the chosen school (e.g., percent White), subtract the average value of that characteristic in the choice set (e.g., the average percent White across all three school options), and then divide by the standard deviation of that characteristic in the choice set (e.g., the standard deviation of percent White across all three school options). For the primary analyses, we average these standardized values across all non-home-district choices. We also conduct a separate analysis focused specifically on choices in participants' home districts.

Equation (2) does not control for the demographic variables that we use when checking for balance between experimental groups. We do not observe any precision gains when including demographic covariates in the model, nor do we observe any important differences between the unadjusted and adjusted results. This is likely because there are so few observable differences in baseline characteristics between experimental groups (see Table 1 for more information). Moreover, when the covariates are excluded from the model, the value of the intercept becomes substantively meaningful: the average outcome of the control group. For these reasons, we focus on the unadjusted values in the text. For reference, please see Table B1 in the online appendix for the results of the primary analyses that also control for all available demographic covariates.

We also consider how the average effects of status and/or growth information vary by participants' racial identity and household income using the following equation:

$$Y_i = a + bS_i + cG_i + dB_i + e(S_iZ_i) + f(G_iZ_i) + g(B_iZ_i) + u_i, \quad (3)$$

where Y_i represents the average racial composition (percent White) or the average economic composition (percent FRPL) of participants' chosen schools, and Z_i represents an indicator of participants' race (White or person of color) or household income (greater or less than $100,000). We conduct these analyses in order to consider the potential segregating or desegregating consequences of providing status and/or growth information (i.e., Does the

provision of growth data tend to steer White participants towards schools with fewer White students?). We intentionally limit ourselves to these specific individual-level treatment effect heterogeneity analyses for two reasons. First, examining how the effects of status and/or growth information vary by participants' race and household income has important theoretical and practical implications for our understanding of the intersection of parental school preferences and racial and economic segregation. Second, by restricting the individual-level heterogeneity analyses to a small subset of possible combinations, we reduce our exposure to the multiple comparisons problem that arises with each additional statistical test.

Lastly, we also explore how the average effects of status and/or growth information vary by the demographic compositions of the school districts in which the choices take place. To conduct this analysis, we construct a long-form dataset in which each participant appears five times: once for each non-home-district choice. We then use the following equation:

$$Y_{icd} = a + bS_i + cG_i + dB_i + e(S_i Z_d) + f(G_i Z_d) + g(B_i Z_d) + u_{icd}, \qquad (4)$$

where Y_{icd} represents the racial composition (percent White) or the economic composition (percent FRPL) of the chosen school for participant i in choice set c, which features three schools from district d. Z_d represents one of four district-level demographic variables: 1) the percentage of White students in the district, 2) the percentage of FRPL-eligible students in the district, 3) the district's White/Black relative diversity index (a measure of racial segregation ranging from 0-1 which captures White-Black differences in exposure to White students; see Reardon & Firebaugh, 2002), and 4) the district's FRPL/non-FRPL relative diversity index (a measure of economic segregation ranging from 0-1 which captures FRPL/non-FRPL differences in exposure to non-FRPL students; see Reardon & Firebaugh, 2002). All district-level demographic data are pulled from SEDA. Once again, we restrict ourselves to these particular

district-level heterogeneity analyses in order to prioritize the most theoretically and policy relevant comparisons while limiting our exposure to the multiple comparisons problem.

For equations (1), (2), and (3), we calculate HC2 robust standard errors (MacKinnon & White, 1985). For equation (4), we calculate CR2 robust standard errors, clustered at the participant level (Bell & McCaffrey, 2002).

Findings

Balance and Missing Data

Table 1 displays the frequencies of participants' demographic characteristics by experimental condition. Our use of random assignment establishes groups with similar demographic compositions. There is only one instance in which the demographic profile of an experimental group is statistically distinguishable from the control group. Participants in the growth group are about one year older on average.

[Table 1 about here]

Our original sample consists of 2,800 participants. However, we are missing outcome data for 16 participants, reducing our analytic sample to 2,784. Depending on the experimental group, about 7-8 percent of participants are missing data on at least one of the demographic variables. For analyses in which we adjust for demographic differences between experimental groups (see Table B1 in the online appendix), we impute an arbitrary value for the missing data and control for an indicator of missingness.

Average Effects of Status and/or Growth Information

Figure 2 displays the primary results of our study: the average characteristics of the schools chosen by participants in each of the four experimental groups. These results are derived from the values in Table A1 in the appendix, which contains our estimates of the average

differences between groups.

[Figure 2 about here]

Relative to the choice set mean (represented by zero on the y-axis), participants in the control group choose schools with higher status (+0.17 standard deviations, or SD), lower enrollment (-0.05 SD), a larger White population (+0.21 SD), a smaller Black population (-0.07 SD), a smaller Hispanic population (-0.09 SD), a larger population of students of other races/ethnicities (+0.09 SD), a smaller FRPL-eligible population (-0.21 SD), a smaller LEP population (-0.13 SD), and schools that are closer to home (-0.28 SD).

Compared to their peers in the control group, participants in the status group choose schools with higher status (+0.14 SD), higher growth (+0.10 SD; likely because status and growth are modestly correlated at the school level), higher enrollment (+0.06 SD), a smaller Black population (-0.06 SD), a smaller Hispanic population (-0.07 SD), a larger population of students of other races/ethnicities (+0.05 SD), a smaller population of FRPL-eligible students (-0.07 SD), and schools that are further from home (+0.11 SD).

Compared to their peers in the control group, participants in the growth group choose schools with higher growth (+0.35 SD), a smaller White population (-0.07 SD), a larger population of FRPL-eligible students (+0.05 SD), a larger population of LEP students (+0.05 SD), and schools that are further from home (+0.15 SD).

Compared to their peers in the control group, participants in the both group choose schools with higher status (+0.07 SD), higher growth (+0.25 SD), higher enrollment (+0.05 SD), and schools that are further from home (+0.14 SD).

To summarize, in the absence of either status or growth information, participants tend to choose schools that are relatively White, affluent, and close to home. The provision of status

information alone steers participants towards higher status, higher growth, less Black, less Hispanic, more affluent, and more distant schools. The provision of growth information alone steers participants towards higher growth, less White, less affluent, and more distant schools. The provision of both types of academic performance information steers participants towards slightly higher status, higher growth, and more distant schools—but only insofar as those choices do not lead to schools that are less White or less affluent than those chosen by the control group.

In the context of our experiment, the provision of status information has only modest educational benefits for the individual while also exacting large social costs. Status information guides participants towards slightly more effective (i.e., higher growth) schools, but it also influences school choices in ways that can actively exacerbate racial and economic segregation. By contrast, the provision of growth information guides participants to more effective schools in ways that run counter to the conventional wisdom that the "good" schools almost always serve students who are White and affluent. The provision of both types of academic performance information also guides participants towards more effective schools, but it does not appear to have the same desegregating consequences as the provision of growth information alone.

Home District Analysis

A common critique of online survey experiments like ours is that the experimental context is too abstract and too removed from participants' actual experiences as parents looking for appropriate educational options for their children. We share this concern and seek to reduce its potency by asking participants to choose between three randomly selected schools in their own local school district. We view this as a test of what happens when the experimental context is not wholly constructed by the researcher but is instead occurring in a somewhat more realistic setting in which many participants may bring personalized, local knowledge about the schools'

reputations to bear when making their choices. If respondents draw on local knowledge, the result likely would diminish the impact of the information we provide. If that local knowledge consists of racially-tinged stereotypes and biases, it might diminish the impact of growth information when diverse schools are performing well. If the local knowledge consists of accurate insider information on genuine school performance, on the other hand, it could diminish the impact of status information that simply reflected demographic factors.

Based on their zip codes, we were able to match 2,148 participants (77 percent of the sample) to the school district where they live. Figure 3 displays the results of the analyses that focus on participants' choices between three schools in their home district (see also Table A2). The magnitudes and directions of the average effects in participants' home districts are largely consistent with the effects that we observe in randomly sampled districts. This suggests that the additional localized knowledge that participants bring to bear when considering schools in their own communities does not seem to alter the general pattern of results described above.

[Figure 3 about here]

However, the results in Figure 3 are less precise than their counterparts in Figure 2. This loss of precision occurs for two reasons. First, the sample size is smaller. Second, there is greater variation in the outcomes. In the non-home-district analyses, we average the characteristics of participants' chosen schools over multiple different choices, reducing the overall variation in these characteristics. In the home-district analyses, we are estimating the effects of status and/or growth information on participants' choices in the context of a single choice set, resulting in a wider distribution of school characteristics. Due to this loss of precision, the average effects we observe in the home-district analyses are not always statistically significant.

Individual-Level Treatment Effect Heterogeneity

For a better understanding of the effects of status and/or growth information on school segregation, we consider how the results vary by participants' race and household income. Specifically, the tendency of growth information to steer participants towards less White and less affluent schools would only have desegregating consequences if growth information steers *White* participants towards less White schools and *affluent* participants towards less affluent schools. If the effects of providing growth information are concentrated among participants of color and/or low-income participants, then the resulting social outcome could be greater rather than less segregation. Such an outcome may still be equity-inducing along other dimensions if growth information guides participants of color and low-income participants towards more academically effective schools—even if they are less racially or economically integrated.

Figure 4 displays the average racial composition (top panel) and average economic composition (bottom panel) of the schools chosen by participants in each experimental group, disaggregated by participants' race (White or person of color) and income (greater or less than $100,000). This information is also available in Table A3, which contains our estimates of the differences in the average treatment effects between racial and income-based groups.

[Figure 4 about here]

With two notable exceptions, the differences in the average effects of status and/or growth information between White participants and participants of color as well as between participants with income above and below $100,000 are statistically indistinguishable from zero. The first exception applies to the effects of growth information on the racial composition of schools chosen by participants in each experimental group. Compared to their counterparts in the control group, White participants in the growth group choose schools with a 0.11 SD smaller proportion of White students. By contrast, compared to their counterparts in the control group,

participants of color in the growth group choose schools with only a 0.01 SD smaller proportion of White students. A similar difference appears with respect to provision of both status and growth information. Compared to their counterparts in the control group, White participants in the both group choose schools with a 0.05 SD smaller proportion of White students. By contrast, compared to their counterparts in the control group, participants of color in the both group choose schools with a 0.06 SD larger proportion of White students.

The results of these individual-level treatment effect heterogeneity analyses provide a clearer case for the desegregating effects of disseminating growth information. In general, the primary results regarding the racial and economic composition of the chosen schools hold for both White participants and participants of color as well as for both more affluent and less affluent participants. Moreover, with respect to the specific case of growth information steering participants towards less White schools, the effects are actually larger among White participants.

District-Level Treatment Effect Heterogeneity

We continue our study of the potential implications of disseminating different types of academic performance information for school segregation by examining how the average effects of status and/or growth information vary by the demographic compositions of the school districts in which the choices take place. Figure 5 (see also Table A4) displays the average differences in the racial and economic compositions of the schools chosen by participants in the control group and the three other experimental groups, disaggregated by four different measures of district-level demographics: 1) the percentage of White students in the district, 2) the percentage of FRPL-eligible students in the district, 3) the district's White/Black relative diversity index (W/B RDI), and 4) the district's FRPL/non-FRPL relative diversity index (F/NF RDI). In each plot, the dark gray line represents the linearized average racial or economic compositions of the schools

chosen by participants in the control group. The red, green, and blue lines represent the analogous values for the participants in the status, growth, and both groups, respectively.

[Figure 5 about here]

Plots A-F display the average racial and economic compositions of the chosen schools at every point along the district racial spectrum (0-100% White). We focus here on participants' choices in very non-White districts (less than 25% White). In these contexts, participants in the control group tend to choose Whiter and more affluent schools than the choice set mean. The choices of participants in the status group are generally indistinguishable from their peers in the control group. By contrast, participants in the growth group choose less White and less affluent schools than their peers in the control group. Participants in the both group fall somewhere in between. Their choices with respect to school racial composition are similar to the control group, but they choose slightly less affluent schools.

Plots G-L display the average racial and economic compositions of the chosen schools at every point along the district economic spectrum (0-100% FRPL). We focus here on participants' choices in very low-income districts (greater than 75% FRPL). In these contexts, participants in the control group tend to choose Whiter and more affluent schools than the choice set mean. The choices of participants in the status group are generally indistinguishable from their peers in the control group. Participants in the growth group choose less White and less affluent schools than their control group counterparts. Participants in the both group again fall somewhere in between. Their choices with respect to school racial composition are similar to the control group, but they choose slightly less affluent schools.

Plots M-R display the average racial and economic compositions of the chosen schools at every point along the White/Black racial segregation spectrum (0-1 W/B RDI, where 0 is

completely desegregated and 1 is completely segregated). We focus here on participants' choices in very racially segregated districts (around 0.75 W/B RDI, which represents the upper bound of racial segregation among the districts in our study). In these contexts, participants in the control group tend to choose much Whiter and much more affluent schools than the choice set mean. The choices of participants in the status group and the both group are generally indistinguishable from their peers in the control group. However, participants in the growth group again choose less White and less affluent schools than their control group counterparts.

Plots S-X display the average racial and economic compositions of the chosen schools at every point along the FRPL/non-FRPL economic segregation spectrum (like its racial equivalent, F/NF RDI theoretically ranges from 0-1, but the highest value among districts in our study is 0.48). We do not observe any statistically meaningful variation in average treatment effects along this dimension.

In short, the district-level treatment effect heterogeneity analysis largely reinforces our conclusions from the analysis of average treatment effects and their implications for school segregation. Only the provision of growth information alone produces a clear and consistent pattern of participant choices that would have desegregating consequences. When participants receive both types of academic performance information, their school choices with respect to racial and economic composition are generally consistent with those of their control group peers.

Conclusion

Public school choice has expanded dramatically over the last few decades. Parents of more than four-in-ten school-age children indicate that they had multiple public options when choosing a school for their child (de Brey et al., 2021). In a country with entrenched residential segregation, advocates of school choice often tout the potential desegregating consequences of

uncoupling students' home addresses and school assignments. However, the relationship between school choice and school segregation is far from straightforward. Indeed, the preponderance of evidence suggests that many varieties of school choice tend to exacerbate rather than ameliorate pre-existing patterns of racial, ethnic, and economic segregation.

The way in which states and districts traditionally measured and reported school quality may have abetted this dynamic. Prior to the passage of ESSA in 2015, most states' school accountability systems focused almost exclusively on measures of student achievement status. While such measures may offer a useful—if limited—window on the condition of *students'* knowledge and skills in the tested domains, they provide little if any insight into *schools'* contributions to student learning. Because students enter school with varying levels of academic preparation and because students vary in the extent to which they encounter out-of-school obstacles to academic performance, school-level achievement status largely reflects the racial, ethnic, and economic composition of the student body. If guided by the official measures, families seeking the highest performing schools for their children would almost invariably be directed towards the Whitest and most affluent schools. Unless school choice policies deliberately prioritize low-income students and students of color when assigning seats in these schools, the end result is likely to be greater rather than less segregation.

ESSA now requires states to use multiple measures when evaluating school quality. One of the biggest shifts has been the widespread adoption of growth as one of those indicators. Growth is not a perfect measure of school effectiveness. In most of its current formulations, it does not capture changes in achievement outside of grades 3-8, it is limited to student performance on standardized math and reading tests, it can suffer from year-to-year volatility (growth tends to be a more reliable measure when it incorporates multiple years of data), and it

does not account for ongoing out-of-school factors that may inhibit student learning but that are outside of educators' control. But, despite its shortcomings, growth is a meaningful improvement over status. Moreover, growth bears a much weaker underlying relationship to the racial, ethnic, and economic composition of the student body. Therefore, as states begin to disseminate information on school-level growth, we might expect many families to consider schools that they would have otherwise written off under the previous status-based accountability regime.

To test this hypothesis, we conducted an online survey experiment with a nationally representative sample of parents and caretakers of children aged 0-12. We asked participants to choose their preferred school from a set of three public schools randomly sampled from the same school district (districts were also randomly sampled from all districts nationwide, weighted by district size). All participants received demographic information for each school. In addition, participants were randomly assigned to receive status data, growth data, both, or neither. They completed this task six times, each with three different schools drawn from a different district (including one round in which they chose between three schools in their local district).

We find evidence that giving participants status information steers participants towards schools that are less Black, less Hispanic, and less affluent than those chosen by participants who only receive demographic information. This finding is consistent with the argument that a status-oriented school accountability system could exacerbate school segregation. Alternatively, giving participants growth information results in a pattern of school choices that could reduce racial, ethnic, and economic divisions between schools. The provision of both status and growth information steers parents towards higher performing schools on both dimensions but not towards schools that serve a less advantaged population.

The latter finding—that the provision of both status and growth information generally

does not have desegregating consequences in the context of our experiment—is likely to be disappointing to those who argue that the adoption of growth as a measure of school quality will better align the structure of school choice reforms towards the goal of school desegregation. Measuring and reporting school-level growth is a positive development in other respects, given its superiority to status as a measure of school effectiveness. However, because the general trend among states is to modify their school accountability systems by including growth as a supplement to status rather than as a replacement for status, such changes may do little to alter families' school choice behavior in a way that meaningfully reduces existing school segregation.

To the extent that our experimental findings generalize to families' actual school enrollment decisions, only a wholesale switch from a status-oriented system to a growth-oriented system of school accountability would result in a choice-driven reduction in segregation. Not only is such a shift politically implausible—it would be tremendously difficult to justify or sustain a policy of withholding school performance information from families—it could also generate its own distortions with respect to school quality (see previous comments about some of the shortcomings of current measures of growth). School quality is a multi-dimensional concept, and we do not advocate for any system of school evaluation that relies on a single metric.

Governmental efforts to use power and authority to promote school diversity have all too often sparked public backlash, attenuating or reversing any progress. That history helps to explain the appeal of less intrusive efforts that might further the goal of inducing diversity with less political trauma and disruption. We undertook this study with the hope and some expectation that informational nudges might be a promising complement—although in no way a sufficient alternative—to more direct challenges to the systemic factors that promote and sustain racial, ethnic, and economic separation. We conclude with less optimism. Adding growth information

to the array of data publicly available to parents and citizens is a good thing to do, but, on its own, it is unlikely to resolve the fraught tensions between perceived individual self-interest and the collective good that have bedeviled past efforts to use school choice to achieve more diverse educational environments for coming generations.

It is possible that combining growth with other types of information omitted from our experiment could have salutary effects. Valant and Newark (2020), for example, find that parents place considerable weight on other parents' narrative comments when evaluating schools—even when those comments appear to contradict conventional measures of academic performance. If higher-growth schools are indeed more effective, presumably parental comments would paint such schools in a positive light more often than not, easing the way for some to consider more diverse educational options. Making more dramatic progress, however, may mean working through the political process to build sufficient support for deliberate, informed, and authoritative action that goes beyond relying on parental choices alone.

Over the long term, such efforts may even be facilitated by the widespread distribution of information about student growth. One of the chief virtues of measuring and reporting growth is that it upends the conventional wisdom that the most effective schools are almost always the Whitest and most affluent. Many of those schools are indeed excellent educational institutions—but so are many schools that disproportionately serve disadvantaged students. Our experiment provides participants with student growth data, but it does not supplement that data with serious efforts to educate participants about the ways in which that information might improve their ability to find the best school for their child. A better understanding of the distribution of school quality, promulgated intentionally and over time, may reduce some of the resistance to integration among White and affluent families.

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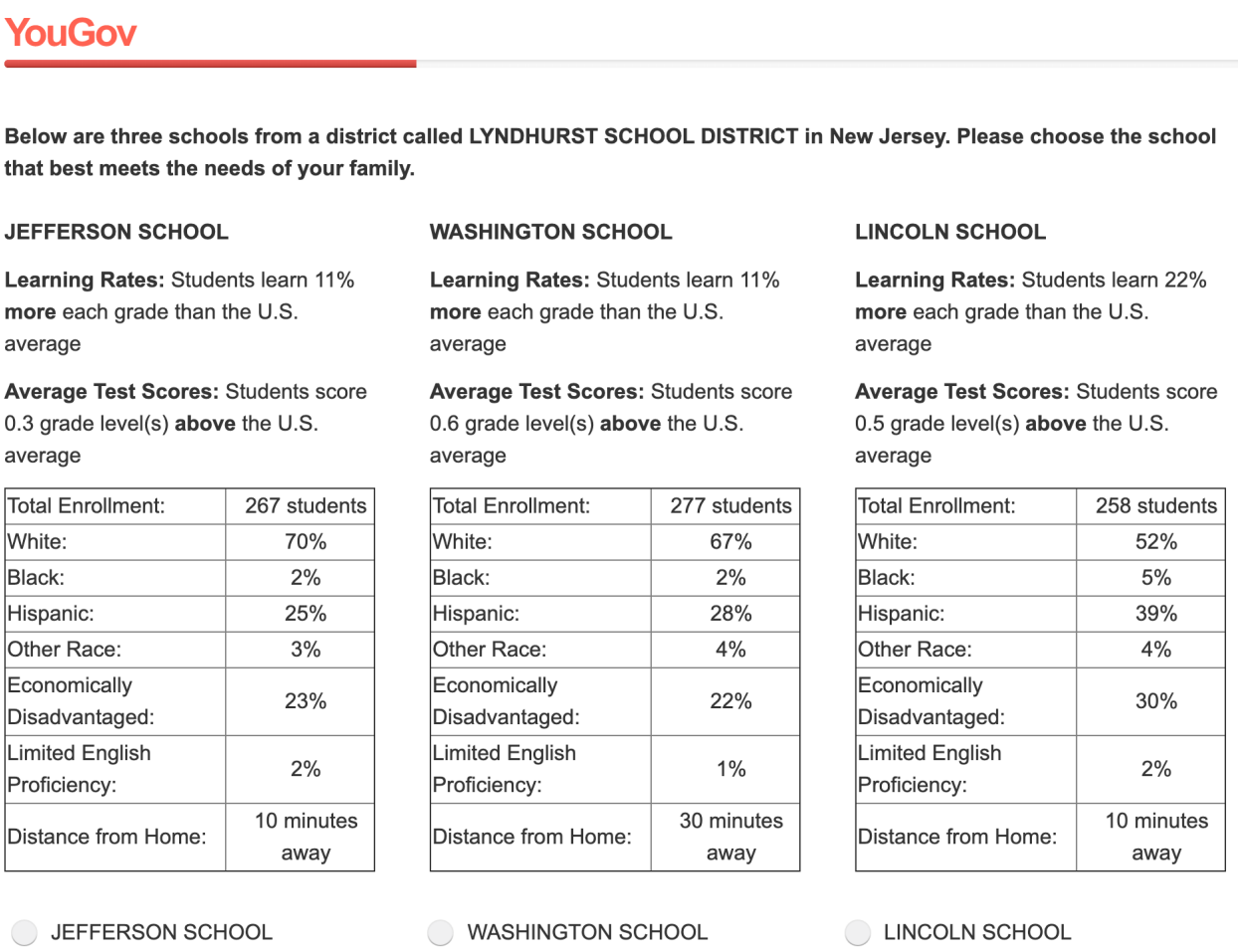
Figures

Figure 1. Excerpt from survey experiment



YouGov

Below are three schools from a district called LYNDHURST SCHOOL DISTRICT in New Jersey. Please choose the school that best meets the needs of your family.

JEFFERSON SCHOOL

Learning Rates: Students learn 11% **more** each grade than the U.S. average

Average Test Scores: Students score 0.3 grade level(s) **above** the U.S. average

Total Enrollment:	267 students
White:	70%
Black:	2%
Hispanic:	25%
Other Race:	3%
Economically Disadvantaged:	23%
Limited English Proficiency:	2%
Distance from Home:	10 minutes away

JEFFERSON SCHOOL

WASHINGTON SCHOOL

Learning Rates: Students learn 11% **more** each grade than the U.S. average

Average Test Scores: Students score 0.6 grade level(s) **above** the U.S. average

Total Enrollment:	277 students
White:	67%
Black:	2%
Hispanic:	28%
Other Race:	4%
Economically Disadvantaged:	22%
Limited English Proficiency:	1%
Distance from Home:	30 minutes away

WASHINGTON SCHOOL

LINCOLN SCHOOL

Learning Rates: Students learn 22% **more** each grade than the U.S. average

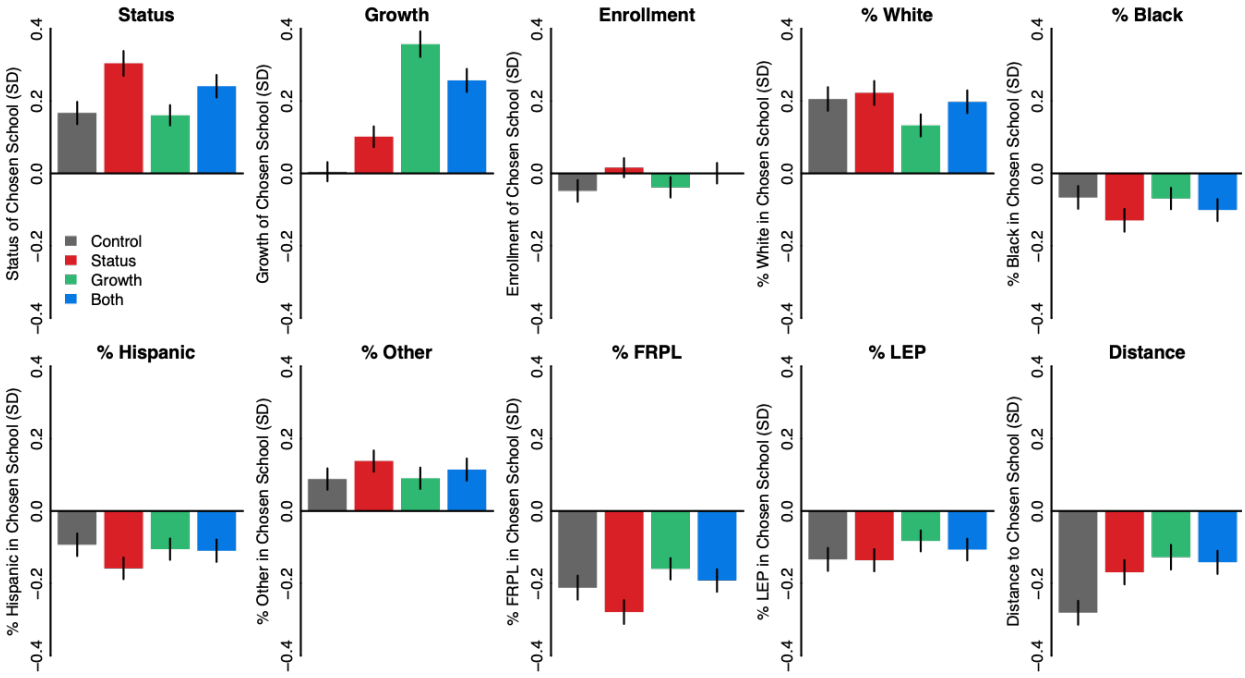
Average Test Scores: Students score 0.5 grade level(s) **above** the U.S. average

Total Enrollment:	258 students
White:	52%
Black:	5%
Hispanic:	39%
Other Race:	4%
Economically Disadvantaged:	30%
Limited English Proficiency:	2%
Distance from Home:	10 minutes away

LINCOLN SCHOOL

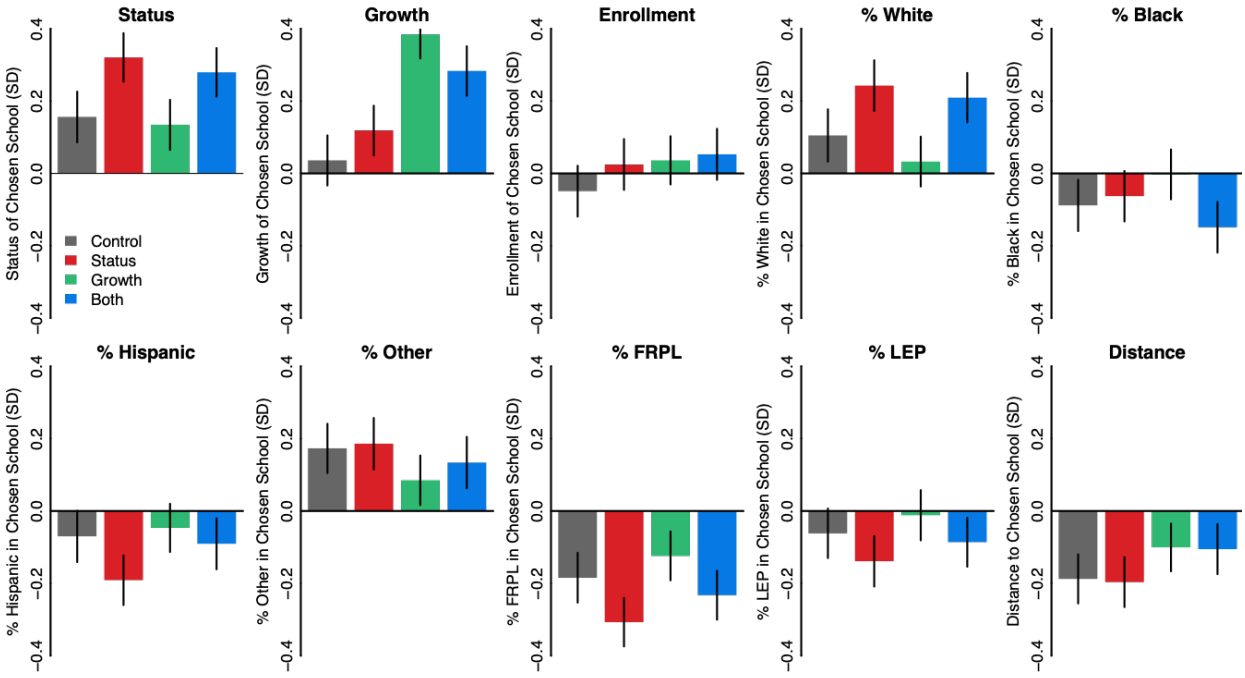
Note: The information above focuses on students in grades 3-8. Students in other grades are not included.

Figure 2. Average characteristics of chosen schools



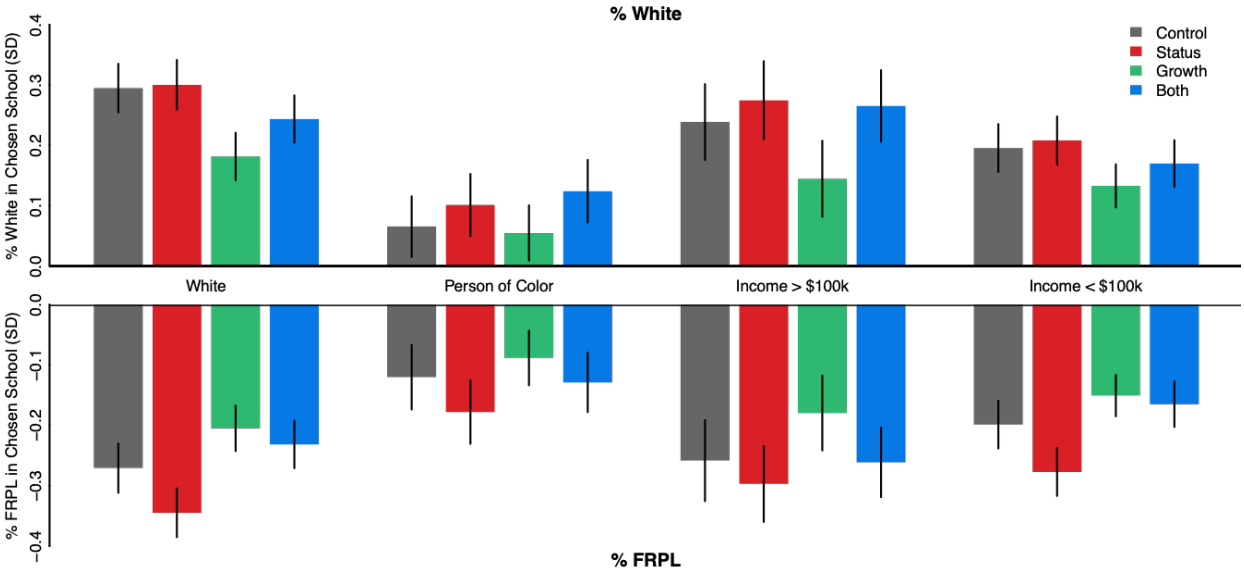
Notes. Bars represent the average characteristics of the schools chosen by participants in each experimental group; vertical lines represent 95% confidence intervals; outcomes standardized within each choice set and averaged over all non-home-district choices; n = 2,784.

Figure 3. Average characteristics of chosen schools in home district



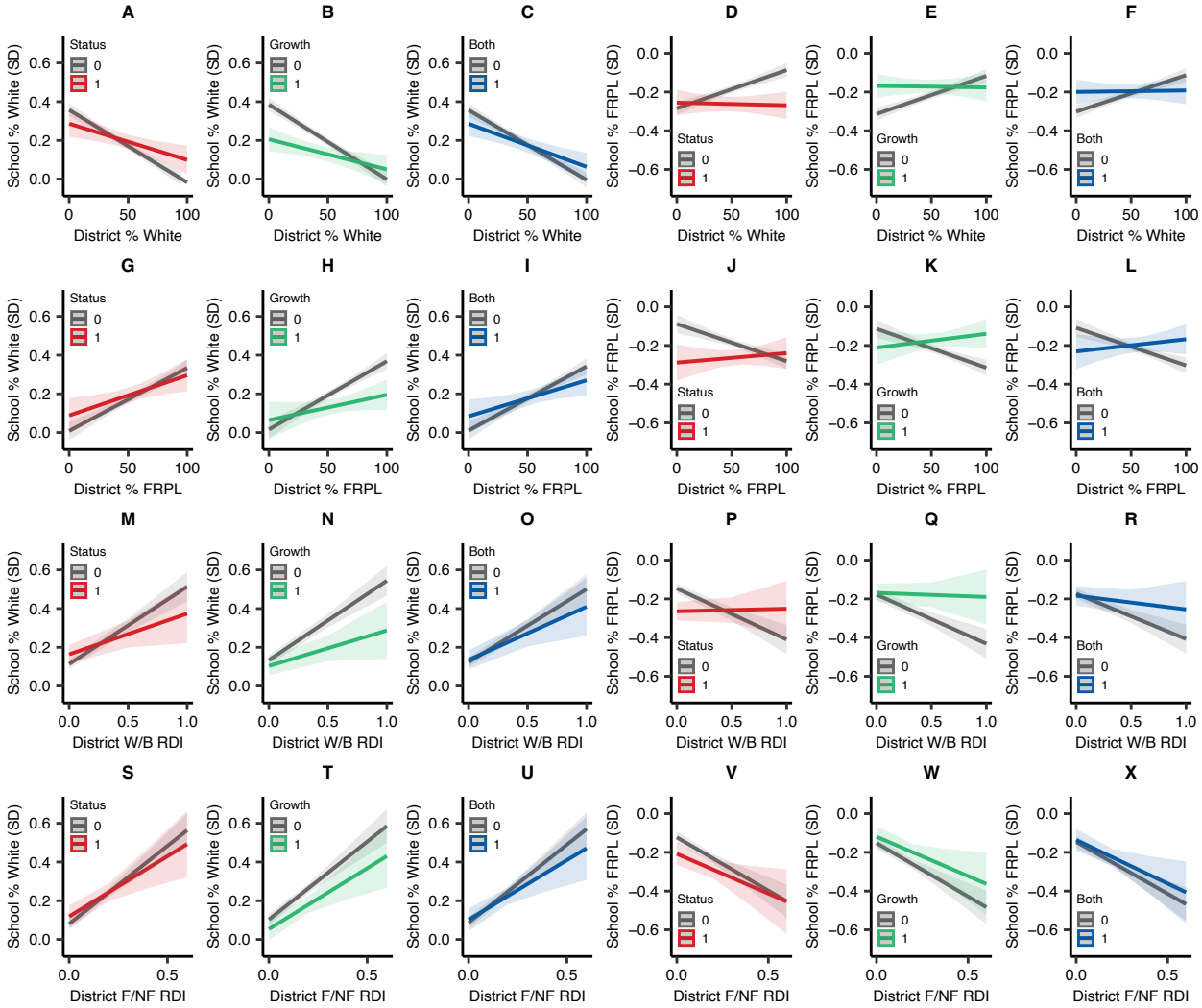
Notes. Bars represent the average characteristics of the schools chosen by participants in each experimental group; vertical lines represent 95% confidence intervals; outcomes standardized within each participant's home-district choice set; sample size varies due to cases in which there was no variation in the outcome characteristic within the participant's home-district choice set.

Figure 4. Individual-level heterogeneity analysis



Notes. Bars represent the average characteristics of the schools chosen by participants in each experimental group, disaggregated by participants race and household income; vertical lines represent 95% confidence intervals; outcomes standardized within each choice set and averaged over all non-home-district choices; n = 2,784.

Figure 5. District-level heterogeneity analysis



Notes. Lines represent the linearized average characteristics of the schools chosen by participants in each experimental group; shaded areas represent 95% confidence intervals; outcomes standardized within each choice set, excluding the home-district choice; participants = 2,784; observations = 13,920; standard errors clustered at the participant level.

Tables

Table 1. Balance and missing data

		Experimental Group		
(%)	**Control**	**Status**	**Growth**	**Both**
Parent/caretaker	100.00	100.00	100.00	100.00
Female	59.92	62.17	63.55	58.23
White	61.03	60.87	61.42	61.68
Black	9.85	10.43	11.21	12.13
Hispanic	20.25	16.52	16.45	16.92
Asian	3.88	5.36	3.55	3.29
Other race/ethnicity	4.99	6.81	7.38	5.99
Less than high school	5.27	5.65	6.24	5.54
High school	24.55	25.51	25.82	23.50
Some college	17.34	15.94	15.32	16.02
College (2-year)	11.79	12.32	10.07	11.83
College (4-year)	25.80	25.07	24.68	26.20
Post-graduate	15.26	15.51	17.87	16.92
Married	69.90	68.41	70.21	72.16
Employed full-time	50.35	50.43	52.62	50.45
Less than $50k	39.23	40.99	41.93	41.18
Between $50-100k	33.58	32.97	32.88	28.62
Greater than $100k	27.19	26.04	25.19	30.21
Democrat	45.21	48.12	48.65	48.95
Republican	30.65	30.00	29.93	29.19
Liberal	27.88	30.72	27.80	32.19
Conservative	24.69	27.54	27.38	24.85
Born-again Christian	32.59	33.48	35.74	34.73
Age (years)	38.10	38.51	39.18*	38.45
Number of children	1.69	1.69	1.68	1.71
Youngest child age (years)	5.42	5.58	5.57	5.59
Oldest child age (years)	7.49	7.74	7.61	7.79
Missing any covariate	8.32	6.96	7.09	7.04
Number of observations	721	690	705	668

Notes. Status, Growth, and Both compared to Control; * $p < 0.05$.

Appendix (for inclusion in printed materials)

Table A1. Average effects of information on the characteristics of chosen schools

		Experimental Group		
Outcome	**Intercept (Control)**	**Status**	**Growth**	**Both**
Status	0.17*	0.14*	-0.01	0.07*
	(0.02)	(0.02)	(0.02)	(0.02)
Growth	0.00	0.10*	0.35*	0.25*
	(0.01)	(0.02)	(0.02)	(0.02)
Enrollment	-0.05*	0.06*	0.01	0.05*
	(0.02)	(0.02)	(0.02)	(0.02)
% White	0.21*	0.02	-0.07*	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)
% Black	-0.07*	-0.06*	0.00	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)
% Hispanic	-0.09*	-0.07*	-0.01	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)
% Other Race	0.09*	0.05*	0.00	0.03
	(0.02)	(0.02)	(0.02)	(0.02)
% FRPL	-0.21*	-0.07*	0.05*	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
% LEP	-0.13*	0.00	0.05*	0.03
	(0.02)	(0.02)	(0.02)	(0.02)
Distance	-0.28*	0.11*	0.15*	0.14*
	(0.02)	(0.02)	(0.02)	(0.02)

Notes. Each row represents a separate OLS regression; outcomes standardized within each choice set and averaged over all non-home-district choices; values are regression coefficients (robust standard errors in parentheses); $n = 2{,}784$; * $p < 0.05$.

Table A2. Average effects of status and/or growth on the characteristics of chosen schools in home district

		Experimental Group		
Outcome	**Intercept (Control)**	**Status**	**Growth**	**Both**
Status (*n* = 2,141)	0.16*	0.16*	-0.02	0.12*
	(0.04)	(0.05)	(0.05)	(0.05)
Growth (*n* = 2,145)	0.04	0.08	0.35*	0.25*
	(0.04)	(0.05)	(0.05)	(0.05)
Enrollment (*n* = 2,148)	-0.05	-0.05	0.09	0.10*
	(0.04)	(0.05)	(0.05)	(0.05)
% White (*n* = 2,117)	0.10*	0.14*	-0.07	0.10*
	(0.04)	(0.05)	(0.05)	(0.05)
% Black (*n* = 2,047)	-0.09*	0.03	0.09	-0.06
	(0.04)	(0.05)	(0.05)	(0.05)
% Hispanic (*n* = 2,109)	-0.07	-0.12*	0.02	-0.02
	(0.04)	(0.05)	(0.05)	(0.05)
% Other (*n* = 1,975)	0.17*	0.01	-0.09	-0.04
	(0.04)	(0.05)	(0.05)	(0.05)
% FRPL (*n* = 2,144)	-0.18*	-0.12*	0.06	-0.05
	(0.04)	(0.05)	(0.05)	(0.05)
% LEP (*n* = 2,063)	-0.06	-0.08	0.05	-0.02
	(0.04)	(0.05)	(0.05)	(0.05)
% Distance (*n* = 1,910)	-0.19*	-0.01	0.09	0.08
	(0.04)	(0.05)	(0.05)	(0.05)

Notes. Each row represents a separate OLS regression; outcomes standardized within each participant's home-district choice set; values are regression coefficients (robust standard errors in parentheses); sample size varies due to cases in which there was no variation in the outcome characteristic within the participant's home-district choice set; * $p < 0.05$.

Table A3. Individual-level heterogeneity analysis

	Outcome			
	% White		**% FRPL**	
Intercept	0.07*	0.20*	-0.12*	-0.21*
	(0.03)	(0.02)	(0.03)	(0.02)
Status	0.04	0.01	-0.06	-0.08*
	(0.04)	(0.03)	(0.04)	(0.03)
Growth	-0.01	-0.06*	0.03	0.05*
	(0.03)	(0.03)	(0.04)	(0.03)
Both	0.06	-0.02	-0.01	0.03
	(0.04)	(0.03)	(0.04)	(0.03)
Z	0.23*	0.01	-0.15*	-0.02
	(0.03)	(0.03)	(0.03)	(0.03)
Z × Status	-0.03	0.01	-0.02	0.02
	(0.05)	(0.04)	(0.05)	(0.04)
Z × Growth	-0.10*	-0.02	0.03	-0.01
	(0.05)	(0.04)	(0.05)	(0.04)
Z × Both	-0.11*	0.03	0.05	-0.03
	(0.05)	(0.04)	(0.05)	(0.04)
Z =	White	> $100k	White	> $100k

Notes. Each column represents a separate OLS regression; outcomes standardized within each choice set and averaged over all non-home-district choices; values are regression coefficients (robust standard errors in parentheses); racial and income categories compared to all other participants; n = 2,784; * $p < 0.05$.

Table A4. District-level heterogeneity analysis

	Outcome							
	% White				**% FRPL**			
Intercept	0.42*	-0.02	0.12*	0.09*	-0.35*	-0.04	-0.15*	-0.13*
	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	(0.04)	(0.02)	(0.02)
Status	-0.07	0.08	0.05	0.04	0.03	-0.20*	-0.12*	-0.09*
	(0.04)	(0.06)	(0.03)	(0.04)	(0.04)	(0.06)	(0.03)	(0.04)
Growth	-0.18*	0.05	-0.03	-0.05	0.15*	-0.10	0.01	0.03
	(0.04)	(0.06)	(0.03)	(0.04)	(0.04)	(0.06)	(0.03)	(0.03)
Both	-0.07	0.07	0.01	0.02	0.10*	-0.12*	-0.01	0.01
	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.06)	(0.03)	(0.04)
Z	-0.05*	0.04*	0.05*	0.09*	0.03*	-0.03*	-0.04*	-0.06*
	(0.00)	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)	(0.01)	(0.01)
Z × Status	0.02*	-0.01	-0.02	-0.02	-0.02*	0.02*	0.03*	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Z × Growth	0.02*	-0.02*	-0.02*	-0.02	-0.02*	0.03*	0.02*	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Z × Both	0.01*	-0.01	-0.01	-0.02	-0.02*	0.03*	0.02	0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Z =	% White (10's)	% FRPL (10's)	White/ Black RDI (0.1's)	FRPL/ non-FRPL RDI (0.1's)	% White (10's)	% FRPL (10's)	White/ Black RDI (0.1's)	FRPL/ non-FRPL RDI (0.1's)

Notes. Each column represents a separate OLS regression; outcomes standardized within each choice set, excluding the home-district choice; values are regression coefficients (with cluster robust standard errors in parentheses); standard errors clustered at the participant level; participants = 2,784; observations = 13,920; * $p < 0.05$.

Online Appendix

Table B1. Adjusted average effects of status and/or growth on the characteristics of chosen schools

		Experimental Group		
Outcome	**Intercept**	**Status**	**Growth**	**Both**
Status	0.12*	0.14*	0.00	0.08*
	(0.05)	(0.02)	(0.02)	(0.02)
Growth	-0.12*	0.10*	0.35*	0.26*
	(0.05)	(0.02)	(0.02)	(0.02)
Enrollment	-0.03	0.07*	0.02	0.05*
	(0.05)	(0.02)	(0.02)	(0.02)
% White	0.18*	0.02	-0.07*	-0.01
	(0.05)	(0.02)	(0.02)	(0.02)
% Black	-0.06	-0.06*	-0.01	-0.04
	(0.05)	(0.02)	(0.02)	(0.02)
% Hispanic	-0.12*	-0.06*	-0.01	-0.01
	(0.05)	(0.02)	(0.02)	(0.02)
% Other Race	0.03	0.05*	0.00	0.03
	(0.05)	(0.02)	(0.02)	(0.02)
% FRPL	-0.18*	-0.07*	0.05*	0.01
	(0.05)	(0.02)	(0.02)	(0.02)
% LEP	-0.12*	0.00	0.05*	0.03
	(0.05)	(0.02)	(0.02)	(0.02)
Distance	-0.15*	0.11	0.15	0.14
	(0.05)	(0.02)	(0.02)	(0.02)

Notes. Each row represents a separate OLS regression; outcomes standardized within each choice set and averaged over all non-home-district choices; values are regression coefficients (robust standard errors in parentheses); analyses control for all covariates listed in Table 1; $n = 2{,}784$; * $p < 0.05$.