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Racial Segregation and the Black-White Test Score Gap

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ABSTRACT

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Racial segregation is often blamed for some of the achievement gap between blacks and whites. We study the effects of school and neighborhood segregation on the relative SAT scores of black students across different metropolitan areas, using large microdata samples for the 1998-2001 test cohorts. Our models include detailed controls for the family background of individual test-takers, school-level controls for selective participation in the test, and city-level controls for racial composition, income, and region. We find robust evidence that the black-white test score gap is higher in more segregated cities. Holding constant family background and other factors, a shift from a fully segregated to a completely integrated city closes about one-quarter of the raw black-white gap in SAT scores. Specifications that distinguish between school and neighborhood segregation suggest that neighborhood segregation has a consistently negative impact but that school segregation has no independent effect (though we cannot reject equality of the two effects). We find similar results using Census-based data on schooling outcomes for youth in different cities. Data on enrollment in honors courses suggest that within-school segregation increases when schools are more highly integrated, potentially offsetting the benefits of school desegregation and accounting for our findings.

The racial gap in student achievement is a pervasive and divisive feature of American life. Black-white differences in standardized test scores lie at the core of the debate over affirmative action in college admissions (Bowen and Bok, 1998; Kane, 1998) and public sector hiring (McCrary, 2004), and figure prominently in the recent No Child Left Behind Act. Many years before the Supreme Court's *Brown v. Board* decision, segregation was identified as a possible factor in the academic achievement of black children.[1] Studies since the Coleman Report (Coleman, 1966) have found that test scores are lower at schools with higher black enrollment shares (see, e.g., Ferguson 1998, and the review by Schofield 1995). Likewise, there is a strong negative correlation between education outcomes and the fraction of black residents in a neighborhood (e.g., Massey and Denton, 1993).

Establishing whether segregation actually *causes* lower achievement is difficult, however, because individuals are not randomly assigned to neighborhoods or schools.[2] A credible research design has to address the possibility that students who attend schools with larger black enrollment shares – or live in predominantly black neighborhoods – have other characteristics that contribute to their lower achievement. In this paper, we circumvent the endogeneity of school and neighborhood choice by aggregating to the metropolitan level and relating the black-white achievement gap in a metropolitan area to black-white differences in relative exposure to minority neighbors and schoolmates.[3] Differencing eliminates the effects of city-wide variables that may be correlated with racial segregation (such as the level

[1] Crowley (1932) presents an early study of the effect of racially segregated schools on academic achievement, based on comparisons of test scores for black students in two all-black and four mixed-race schools in Cincinnati. She constructed matched samples from the two groups of schools, matching on age, grade, and IQ, and found no difference in achievement test scores between the schools.
[2] On the general problem of inferring peer group effects from observational data, see Manski (1993) and Brock and Durlauf (2001).
[3] Although cities with segregated neighborhoods tend to have segregated schools, school segregation also depends on institutional features like the number of school districts (Urquiola, 2005) and the presence of desegregation programs (Reber, 2005). We show below that the two have substantial independent variation.

of school spending or the efficiency of local schools).[4] We also control for a rich set of measured differences in the family backgrounds of test-takers, and for other variables, like city size and income inequality, that may affect black student's relative achievement. We apply this approach to a large sample of SAT-takers from the 1998-2001 cohorts of high school graduates.

We reach two main conclusions. First, there is a robust and quantitatively important relationship between black relative test scores and the degree of segregation in different metropolitan areas. Our estimates suggest that the move from a highly segregated city to an integrated city is associated with a 45 point narrowing of the black-white SAT gap – about one-quarter of the raw differential. Second, neighborhood segregation seems to matter more than school segregation: In models that include both measures we consistently find that neighborhood segregation exerts a strong negative effect on relative test scores, whereas the effects of school segregation are small and statistically insignificant. We cannot reject, however, that the two have equal effects.

These findings are robust to a variety of specifications and estimation strategies. We focus on two main problems that might lead us to overstate the effects of segregation: differential sorting of white and black families across different cities, and the possibility that segregation is affected by the relative abilities of black and white students in a city. We show that our estimated segregation effects are unaffected by including a control for the residual wage gap between black and white workers in a city, which should proxy for unobserved family background differences that would result from either source of endogeneity. We also examine the changing inter-city distributions of higher- and lower-educated whites and blacks and find no evidence of selective migration, once we control for

[4] Throughout this paper we use "cities" to refer to metropolitan areas – specifically, Metropolitan Statistical Areas (MSA's) or, in the largest urbanized areas, Primary Metropolitan Statistical Areas (PMSAs).

observed city characteristics. Finally, we show that measures of residential and school segregation in a city are highly persistent, and therefore unlikely to be responsive to recent shocks to unobserved student ability. Even controlling for neighborhood segregation the remaining component of school segregation is extremely stable, suggesting that the small school segregation coefficients cannot be attributed to measurement error in this variable.

A third potential concern is selective participation in the SAT. Our analyses all point to the conclusion that, if anything, sample selection leads our results to understate the effects of segregation on black relative achievement. As a robustness check, however, we estimate a series of models using 2000 Census data on schooling outcomes of 16-24 year olds. Estimates from these models are similar to our SAT results.

The finding that black relative achievement is unaffected by differences in school segregation, once we control for neighborhood segregation, leads us to consider the role of *within-school* segregation. Holding constant the level of neighborhood segregation, white students are more likely to enroll in honors classes in cities with more integrated schools, whereas blacks are not. This behavior is consistent with the presence of ability tracking programs that offset the integrative effects of between-school desegregation efforts (Clotfelter, Ladd, and Vigdor, 2003; Clotfelter, 2004), and may help to explain why differences in school segregation have little effect on black relative achievement.

We conclude with an analysis that attempts to distinguish between the direct effects of racial segregation and "indirect" effects operating through school quality and the relative exposure of black and white students to lower income peers. Measures of relative school quality are uncorrelated with the relative exposure of black students to minority neighbors or schools, so the potential for these variables to contribute to observed segregation effects is very small. On the other hand, segregation is highly correlated with exposure to low-income peers at both the school and neighborhood levels. Consistent with our finding that minority exposure at the school level has little or no effect on black relative achievement (holding constant neighborhood exposure), exposure to high-poverty schoolmates also has no effect. But relative neighborhood income does seem to be an important determinant of test score gaps, and accounts for as much as half of our estimated residential segregation effects.

II. The Effects of Segregated Schools and Neighborhoods on Student Achievement

a. Theoretical Channels

The literature has identified four mechanisms through which racial or ethnic segregation might affect the educational achievement of black students. First, what might be called "direct" exposure effects arise in peer group models where minorities have lower expectations or aspirations than non-minorities (all else held constant). For example, models of race-based cultural norms (e.g., Ogbu and Forham, 1986; Ogbu, 2003) assert that black children have lower norms of achievement than otherwise similar whites, and that exposure to peers with lower aspirations reduces achievement. Since segregation by definition raises the relative exposure of black students to black peers, pure exposure effects create a link from segregation to the black-white achievement gap.[5]

A second set of "indirect" exposure effects arise from the correlation between minority status and other characteristics that may negatively effect achievement. Black children, for example, are more likely to have a single parent than white children. If single-parent families have lower educational aspirations, and if student performance is affected by

[5] A similar prediction arises from Austen-Smith and Fryer's (2005) model of endogenous peer group interactions, although in this model the negative effect of black peers reaches a peak when the fraction black is ½, and disappears in an all-black setting. This model also predicts that the negative effects of exposure to black peers are concentrated among black children with low potential achievement, whereas Ogbu's (2003) study focuses on black children with high potential achievement.

peer aspirations (Coleman, 1966; Sewell and Hauser, 1975) then a rise in segregation will lower the relative achievement of black children.

While theoretically important, the distinction between direct and indirect exposure effects is empirically inaccessible. An important example is test score performance (Hoxby, 2000). Suppose that a given student's achievement is affected by the average academic ability of his or her classmates. Since black students have lower test scores than whites, and much of this gap is unexplained by observed family background factors, one could argue that a higher fraction of black peers exerts a direct exposure effect via the academic ability of the peer group. Alternatively, one could argue that the black-white test score gap reflects unmeasured background factors, and is properly interpreted as an indirect exposure effect. In our main analysis we make no distinction between direct and indirect exposure effects, although in later specifications we estimate minority exposure effects holding constant exposure to low-income schoolmates and neighbors.

Another channel through which racial or ethnic exposure could indirectly affect relative achievement is through differences in school quality that are correlated with the racial composition of schools. In the pre-*Brown v. Board* era, separate school systems made it possible for white voters to divert resources from black to white schools (Boozer, Krueger, and Wolkon, 1992). Even today, concerns over differential resources are central to litigation over school finance rules (Schrag, 2003) and to accountability rules that hold schools responsible for race-specific achievement levels.

Finally, there may be "macro" influences that create a direct link from segregation to black relative achievement. If, for example, the nature of the local broadcast media varies across cities—with, perhaps, more racially targeted radio stations in cities with more segregation—exposure to the media culture could produce effects of city-level segregation even on children who live in racially mixed neighborhoods. Models of statistical discrimination (e.g. Coate and Loury, 1993) could yield similar results. Students' education choices might be influenced by local (white) employers' attitudes toward blacks, which might in turn depend on the extent of employers' exposure to black neighbors and schoolmates. These models imply impacts of city-level segregation that extend beyond any direct effects of neighborhood- and school-level exposure.

b. Previous Evidence on Peer Group and Segregation Effects

Much of the existing literature has equated the effect of segregation with that of school-level exposure to black or minority peers, and has documented that student achievement is lower in schools with a higher fraction of black or minority students. Coleman (1966), for example, found that black students earned lower test scores at schools with a higher black enrollment share. As subsequent critics have emphasized, the interpretation of these findings is clouded by the lack of controls for the non-random sorting of students to different types of schools (see e.g., Jencks and Mayer, 1990).

Recent researchers have adopted three main approaches to address the sorting issue: (1) using *within-school* variation in minority exposure; (2) using experimental or quasi-experimental variation in exposure; and (3) aggregating to a level at which sorting is arguably reduced or eliminated.

Hoxby (2000) and Hanushek, Kain, and Rivkin (2002) follow the first approach. Both studies relate the achievement (or achievement growth) of students in different cohorts *at the same school* with the racial and ethnic composition of their particular cohort. This research design assumes that, while students may sort across schools on the basis of long-run factors like average racial composition, choices are unaffected by cohort-specific variation in these factors.[6] Both papers find very large exposure effects. The estimates reported by Hanushek, Kain, and Rivkin (2002), for example, imply that excess exposure of black students to black grademates causes the black-white test score gap to grow by 0.07 standard deviations with each year in school, enough to account for most of the black-white test score gap by 12th grade.[7]

Experimental evidence on the effects of neighborhood peers comes from the recent Moving to Opportunity (MTO) project, which offered incentives for public housing residents to move to lower poverty neighborhoods (Sanbonmatsu et al., 2006). MTO had a modest effect on the quality of subjects' neighborhoods (lowering the poverty rate by about 13 percentage points), but no significant effect on children's academic achievement. The experiment has very limited power to measure the effect of exposure to *minority* neighbors, however, since it only lowered the fraction of minority neighbors of the treatment group by about 7 percentage points.[8]

Guryan (2004) conducts a quasi-experimental analysis of the effect of school segregation on black dropout rates, using variation in the scope and timing of major court-ordered desegregation plans in the 1970s and 1980s. He finds a modest but statistically significant effect, with black dropout rates falling 3 percentage points relative to whites as a result of policies that on average reduced relative black exposure to black schoolmates by

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[6] As in other contexts – for example between-twin estimates of returns to schooling (Bound and Solon, 1999) – it is unclear whether this strategy reduces the covariance between racial exposure and unobserved ability relative to the remaining variation in exposure. An additional concern in the Hanushek, Kain and Rivkin (2002) study is that there may be systematic trends in the ethnic composition of schools that covary with trends in average student characteristics at the school. Hoxby (2000) pays special attention to such trend factors.

[7] This is based on Hanushek, Kane, and Rivkin's (2002) computation that integration would reduce the black share in the average black student's classroom by 25 percentage points and raise the black share in the average white student's classroom by 5 percentage points. Using their estimated exposure effects (which vary by race), this would narrow the gap in gain scores by 0.07 (in standard deviations of level scores) per year, for a cumulative effect over 12 years of 0.83 standard deviations. A key implication of their model is that the black-white test score gap rises with grade. This is not evident in aggregate data (Perie et al., 2005).

[8] See Sanbonmatsu et. al (2006), Table 2. See also Jacob's (2004) study of the effect of housing project demolitions, which yields similar results but has similar limitations.

about 20 percentage points.[9]

A final strand of recent research uses an aggregate research design similar to our own. Although students of differing abilities may sort to different schools or neighborhoods within a given city, these studies assume that the distribution of potential abilities across metropolitan areas is as good as random (conditional on observed control variables). Evans, Oates, and Schwab (1992) use the average characteristics of the metropolitan area as instruments for peer group characteristics. Cutler and Glaeser (1997) extend this idea by distinguishing between the outcomes of blacks and whites in the same city, under the weaker assumption that the black-white *difference* in potential ability in a city is unrelated to the degree of residential segregation. Our basic framework is very similar. We extend Cutler and Glaeser's (1997) analysis by including a much richer set of family background and metropolitan-level control variables that may be correlated with segregation, by distinguishing between the effects of school and neighborhood exposure, and by focusing on test scores as a measure of achievement.

c. Empirical Specification

We begin by assuming that the test score of a given student depends on his or her own characteristics, the racial composition of his or her school and neighborhood, other characteristics of schoolmates and neighbors, and an unobserved error with school- and neighborhood-level components that may vary by race. Specifically, we assume:

$$(1) \qquad y_{ijsnc} = X_{ijsnc}\alpha_j + Z_{sc}\beta_j + W_{nc}\nu_j + B_{sc}\gamma_j + R_{nc}\delta_j + u_{jsc} + v_{jnc} + \varepsilon_{ijsc},$$

where y_{ijsnc} represents the test score (or some alternative measure of achievement) of student

[9] As we discuss below, it is possible that in the longer run some of the integrative effect of desegregation programs is offset by a rise in within-school segregation. Guryan's (2004) estimates, which identify segregation effects on the earliest affected cohorts, would not incorporate such offsetting effects.

i of race group j who attends school s and lives in neighborhood n in city c, X_{ijsnc} is a vector of characteristics of the student, Z_{sc} is a vector representing the average characteristics of the students in school s and other features of the school, W_{nc} is a vector of the average characteristics of the neighborhood, B_{sc} represents the fraction of minority students in school s, R_{nc} is the fraction of minority residents in the neighborhood, u_{jsc} is a shared error component for students of group j in school s and city c, v_{jnc} is a similar error component for residents of group j in neighborhood n, and ε_{ijnsc} is an individual-level error (with mean 0 for each race group in each school and neighborhood).[10] The coefficients γ_j and δ_j capture the direct effects of exposure to minority schoolmates and neighbors, while indirect effects would arise from omission of components of Z and W that are correlated with B and R.

Any non-randomness in the sorting of students to schools or neighborhoods produces a correlation between the unobserved error components in equation (1) and the measures of school- and neighborhood-level exposure, potentially biasing OLS estimates of γ_j and δ_j from student-level data. The effects of non-random sorting within a city can be eliminated by averaging the achievement outcomes of each race group to the city level. Specifically, equation (1) implies that the mean outcome of group j in city c is:

$$(1') \qquad y_{jc} = X_{jc}\alpha_j + Z_{jc}\beta_j + W_{jc}\nu_j + B_{jc}\gamma_j + R_{jc}\delta_j + \mu_{jc},$$

where X_{jc} represents the mean characteristics of students of group j in city c, Z_{jc} and W_{jc} represent the mean characteristics of the school-level and neighborhood-level peer groups of race-j students, B_{jc} is the average fraction of minority students at schools attended by race group j in city c, R_{jc} is the average fraction of minority neighbors of students in group j in city c, and μ_{jc} is the average of $u_{jsc}+ v_{jnc}$ across all students of race j in city c.

[10] We do not include error components that vary by city or by race and city, since these will be absorbed by the school × race effects. In our empirical specification, "minorities" are blacks and Hispanics; we have tested for differential effects of exposure to the two groups and fail to reject equality in a wide range of alternative specifications. Tables that report separate effects are available upon request.

Although averaging eliminates the effects of within-city sorting, there still may be differences in the average unobserved "abilities" of students -- or in the average quality of the local schools -- that would lead to biases in the estimation of equation (1') across cities. Any differences that are common across race groups in a city can be "differenced out" by comparing blacks and whites within the same city. Specifically, (1') implies:

$$y_{1c} - y_{2c} = X_{1c}\alpha_1 - X_{2c}\alpha_2 + Z_{1c}\beta_1 - Z_{2c}\beta_2 + W_{1c}\nu_1 - W_{2c}\nu_2 + B_{1c}\gamma_1 - B_{2c}\gamma_2 + R_{1c}\delta_1 - R_{2c}\delta_2 + \mu_{1c} - \mu_{2c}, \tag{2}$$

where j=1 represents blacks and j=2 represents whites. If the coefficients in equation (1) are the same for whites and blacks, equation (2) takes a particularly simple form:

$$\Delta y_c = \Delta X_c\alpha + \Delta Z_c\beta + \Delta W_c\nu + \Delta B_c\gamma + \Delta R_c\delta + \Delta\mu_c , \tag{2'}$$

where Δy_c, for example, denotes the difference in mean test scores between blacks and whites in the same city.

The differences ΔB_c and ΔR_c in equation (2') are closely related to standard segregation indexes of "exposure" and "isolation."[11] When schools and neighborhoods are fully segregated, $B_{1c} = R_{1c} = 1$ and $B_{2c} = R_{2c} = 0$, so $\Delta B_c = \Delta R_c = 1$. When they are completely integrated, $B_{1c} = B_{2c}$ and $R_{1c} = R_{2c}$, so $\Delta B_c = \Delta R_c = 0$. ΔZ_c and ΔW_c measure other differences in the schools and neighborhoods of black and white children, such as the gap in average school quality between the schools attended by blacks and whites, or the gap in average incomes in the neighborhoods of black and white children.

Although differencing eliminates any city-wide factors that affect blacks and whites equally, there may be remaining differences in unobserved determinants of achievement between the two groups. We posit that this remaining gap can be decomposed as:

[11] In the segregation literature (e.g. Massey and Denton, 1988; Iceland, Weinberg, and Stienmetz, 2002), B_{jc} and R_{jc} are known as indices of exposure of race-j students to minorities, and ΔB_c and ΔR_c are similar to isolation indices (Cutler, Glaeser and Vigdor 1999).

$$(3) \qquad \mu_{1c} - \mu_{2c} = F_c\psi + \eta_c \,,$$

where F_c is a vector of city characteristics—including measures of the city racial composition—and η_c represents all remaining unobserved differences. This leads to a model of the form:

$$(4) \qquad \Delta y_c = \Delta X_c \alpha \; + \; \Delta Z_c \beta \; + \; \Delta W_c \nu + \Delta B_c \gamma + \Delta R_c \delta \; + \; F_c \psi \; + \; \eta_c \,.$$

OLS estimation of this equation will yield consistent estimates of γ and δ provided that η_c is uncorrelated with ΔB_c and ΔR_c, conditional on the control variables included in (4).

A key threat to the identification of the segregation effects in equation (4) is *differential* sorting of black and white families to different metropolitan areas. For example, if achievement-oriented black families migrate to cities where schools or neighborhoods are less racially segregated, and if their characteristics are not fully captured in the measured student background variables, then η_c may be negatively correlated with ΔB_c and/or ΔR_c. Our main specifications include a rich set of controls for the observed characteristics of black and white students in different cities, including parental education and income, as well as various city-level variables. In Section V, however, we present some robustness checks and additional analyses to evaluate the likely biases in these models.

Our main analyses simplify equation (4) in two ways. First, as we noted in Section II, it is unrealistic to assume that all the relevant characteristics of schoolmates and neighbors can be measured. We therefore focus on a "reduced form" specification that excludes the W and Z variables:

$$(5) \qquad \Delta y_c = \Delta X_c \alpha' \; + \; \Delta B_c \gamma' + \Delta R_c \delta' \; + \; F_c \psi' \; + \; \eta_c \,,$$

where γ' and δ' are related to γ and δ by the usual omitted variables formulas. Thus, γ' will incorporate the direct effects of exposure to minority schoolmates, indirect effects associated with the characteristics of the schools and neighborhoods that can be predicted from knowledge of ΔB_{c}, conditional on ΔR_c, and any "macro" effects of city-wide school segregation. In section V, below, we attempt to estimate the indirect exposure effects that derive from school resources and the incomes of schoolmates and neighbors.

Second, we present estimates both of equation (5) and of even simpler specifications that include only one of the segregation measures at a time. School and neighborhood segregation are highly correlated across cities, making it difficult to distinguish their separate effects even when the combined effect is precisely estimated. When only residential segregation is included, for example, the resulting coefficient provides an estimate of $\delta' + \pi\gamma'$, where π is the coefficient on ΔR from an auxiliary regression of ΔB on ΔR, ΔX, and F. Empirically, π is close to one, so the coefficient is approximately the sum of δ' and γ'.

d. Taking Advantage of Student-Level Covariates

The aggregated model (5) has only as many degrees of freedom as the number of metropolitan areas in the sample, limiting the flexibility of our controls for family background factors. To fully exploit our rich microdata, we partial out the student-level covariates observed in the SAT files (mother's education, father's education, and family income), using a highly flexible specification that is fully interacted by race. We then aggregate "residual" SAT scores to the city level, and include sparser parameterizations of the ΔX vector in our city-level analysis. The procedure is described in greater detail in the Appendix. Although the first stage adjustment may not fully eliminate the effect of observable student characteristics, we anticipate that the inclusion of ΔX_c in the second stage model absorbs most of their remaining variation.

e. Adjusting For Selective Participation in the SAT

A concern with the use of SAT test scores to measure achievement is selective test participation. As discussed below, we restrict our sample to cities in states where a majority of college-bound students write the SAT (rather than the alternative ACT test). Even within "SAT states", however, test participation rates vary. Presumably, students at "low performing" schools are under-represented in the test-taking population, with greater under-representation in cities with lower overall participation. Positive selection into participation will tend to *attenuate* any negative effects of segregation on black relative test scores (Gronau, 1974; Heckman, 1979).[12] We attempt to reduce such biases by re-weighting the average scores from different high schools in a city to reflect their relative enrollments, and by including a control function in our empirical model based on relative SAT participation rates across high schools in a city.

These adjustments are derived from a conventional bivariate normal model of test participation and test score outcomes (Heckman, 1979). As shown in the Appendix, such a model leads to a specification for the black-white difference in the adjusted, reweighted test scores in city c that differs from equation (5) by the addition of two terms:

$$(6) \qquad \Delta r_c = \Delta X'_c \alpha' + \Delta B_c \gamma' + \Delta R_c \delta' + F_c \psi' + \zeta \Delta\lambda_c + \zeta \Delta\theta_c + \eta_c + \Delta e_c .$$

In this equation, ζ is a coefficient that reflects the correlation between the unobserved component of the individual test participation equation and the unobserved component of the test outcome equation, $\Delta\lambda_c$ is the black-white difference in the enrollment-weighted average of the school-specific inverse Mills ratio function (evaluated at the test participation rate of black or white students at each high school in the city), and $\Delta\theta_c$ is an unobserved error component that reflects the black-white difference in the degree of *within-school*

[12] The correlation of SAT-taking rates and average scores across schools is positive in our data, which would be consistent with negative selection into test-taking. We strongly suspect that the individual level selection is positive, but that large differences in the unobserved determinants of participation rates and mean scores dominate the across-school correlation.

selectivity of test-writers.

If test takers were randomly selected at each high school, but different fractions of students wrote the test at different schools, the control function $\Delta\lambda_c$ would fully correct for selectivity biases in the observed test scores and $\Delta\theta_c$ would equal 0. In general, however, test writers are not randomly selected within schools and so the error component $\Delta\theta_c$ will not vanish. If a rise in school or neighborhood segregation causes black relative test scores to fall but also causes a rise in the *relative* within-school selectivity of black test takers, the presence of this term will lead to attenuation in the estimated negative effect of segregation on relative test scores.

III. Data Sources and Sample Overview

Our primary source of student achievement data is a sample of SAT records for roughly one third of test takers in the 1998-2001 high school graduation classes.[13] These data include self-reported family background characteristics as well as high school identifiers, which we use to match enrollment from the appropriate editions of the Common Core of Data (CCD, for public school students) and the 1997-8 Private School Survey (PSS). To minimize the impact of measurement errors we estimate the number of students, the number of test takers, and the racial composition of each school using averages over the four years in our data.[14] We assign students to Metropolitan Statistical Areas (MSAs) based on year-2000 definitions, using school location information in the CCD and PSS files.[15] We

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[13] The sampling rate was 100% for black and Hispanic test-takers and for those from California and Texas, and 25% for others. We use sampling weights in all computations of city-level averages. We exclude observations for students who reported ethnicity other than white or black (primarily Hispanics and Asians) and those who did not report their race/ethnicity.

[14] This strategy cannot be employed with the PSS, as only one year of data is available.

[15] Where a larger metropolitan area is designated a Consolidated Metropolitan Statistical Area (CMSA) with several sub-areas (Primary Metropolitan Statistical Areas, or PMSAs), we treat the PMSA as the relevant city definition. In every specification, however, we estimate standard errors that are "clustered" by CMSA.

restrict our analysis of SAT outcomes to MSAs in states with overall test participation rates of 25% or higher, which we refer to as "SAT states."

As described above, we use the SAT microdata to estimate race-specific, within-school models relating test scores to three key family background variables -- mother's education, father's education, and income.[16] We then form an enrollment-weighted average of the residual scores for black and white students from the high schools in each city. Our primary dependent variable is the black-white difference in this weighted average.

Recognizing that SAT scores are influenced not just by the racial composition of a student's 12th-grade school but also by the composition of her schools in earlier grades, we attempt to measure the average exposure of white and black students to minority schoolmates throughout their educational careers. We compute exposure rates for high schools in the MSA in 1998-2001 and for elementary schools in 1988-1991, and form an average of these that puts two-thirds weight on the latter and one-third on the former. Our school segregation measure is the black-white difference in this lifetime exposure measure.[17]

We use data on the racial composition and population of Census tracts in 2000 (from the full population counts, Census 2000 Summary File 1) to construct measures of neighborhood-level exposure to black and Hispanic neighbors, and thereby of city-level residential segregation.[18] We also use Summary Files computed from 2000 Census long-form data to estimate the average family background characteristics of black and white

[16] These regressions are fit by race, and include unrestricted high school dummies and 114 background dummies, formed from the 14 income categories reported in the SAT and the full interaction of the 10 categories for each parent's education. The income and education categories include "missing" as one possibility.
[17] When we analyze outcomes that are only available for public schools or for which we cannot readily distinguish different grades (e.g. teacher-student ratios), we use point-in-time school segregation measures computed over the relevant schools and grade levels.
[18] Census tracts are initially defined to encompass demographically homogenous neighborhoods of about 4,000 residents, but once drawn generally have stable boundaries. Exposure measures based on Census Block Groups (typically about 1000 residents) are nearly perfectly correlated across cities with the tract-based measures and lead to virtually identical estimates.

students in each city, supplementing this with information from the public use samples (PUMS) for characteristics (e.g. parental education and residual parental wages) that are not tabulated elsewhere. We also use the PUMS data to construct a measure of the black-white gap in degree attainment that that is free from any test participation biases. Further details on our data sources and merging methods are presented in a Data Appendix, available on request.

Table 1 gives an overview of the patterns of segregation and test scores for a selection of cities with different patterns of residential and school segregation. The first two columns show the fraction black and Hispanic in the metropolitan area.[19] Columns C-E show the mean exposure of black and white students in each city to minority (black and Hispanic) schoolmates, while the final columns show parallel measures of tract-level exposure to minority neighbors.

The first two panels of the table present data for cities with the lowest and highest levels of school segregation, among the subset of all MSA's in SAT states with at least 5% black population shares. The five lowest segregation cities are all in the South: in these cities, the typical black-white gap in exposure to minority schoolmates (ΔB_c) is about 6%. In three of the cities the gap in exposure to minority neighbors (ΔR_c) is comparable, but in two cities (Wilmington, North Carolina and Gainesville, Florida) neighborhoods are substantially more segregated. Among the 5 most-segregated cities, 4 are in the mid-Atlantic region: all have highly segregated neighborhoods as well as schools.[20]

We can only identify separate effects of school and neighborhood segregation to the extent that the two vary independently. The two bottom panels of Table 1 present data for

[19] We treat Hispanics as a distinct racial category, excluding them from both the white and black groups. In 2000 Census data, where possible we include multi-race non-Hispanics as blacks if they report black as one of their races; we never count multi-race individuals as white.

[20] Some of the most highly segregated cities in the U.S., like Detroit and Chicago, are in states where a majority of students write the ACT. These cities are excluded from Table 1 and from all of our SAT analyses.

the cities with the biggest divergence between the two measures, first for cities with relatively integrated schools and then for cities with relatively segregated schools.[21] The degree of neighborhood segregation is similar in the two groups of cities but the extent of school segregation is much smaller in the first group (mean exposure gap=13%) than in the second (mean exposure gap=49%). Although residential and school segregation are highly correlated, there is clearly substantial independent variation in the two factors.

Table 2 presents some comparisons between the students in all 331 MSA's in the country (columns A-B), those in the 189 cities from SAT states that are included in our analysis sample (columns C-D), and those in the 142 cities that are excluded from our test score samples (columns E-F). On average 43 percent of white high school students and 31 percent of black high school students from cities in the SAT states write the SAT. Blacks are slightly under-represented in the SAT state cities whereas Hispanics are over-represented.[22] Cities from SAT states also have slightly less segregated neighborhoods and schools than cities in other states.

The bottom two rows in Table 2 show average SAT scores for the different city groups and the mean test gap between whites and blacks. Average SAT scores are lower in high-participation states (Dynarski, 1987; Rothstein, forthcoming), but the black-white difference is very similar for cities in SAT and non-SAT states, suggesting that use of within-city differences reduces problems associated with selective test participation.

As a final descriptive exercise, Figures 1-3 show the correlations across cities between the black-white adjusted test score gap and the relative segregation of neighborhoods (Figure 1), the relative segregation of schools (Figure 2), and the part of the

[21] We define the degree of divergence as the residual from a regression of school segregation (ΔBc) on neighborhood segregation (ΔRc).

[22] California, Texas, and Florida are all SAT states. In Table 2 (and in the remainder of our analysis), cities are weighted by $(1/N_{bc} + 1/N_{wc})^{-1}$ where N_{bc} and N_{wc} are the numbers of blacks and whites in the city population. Cities with very few blacks thus receive very low weights.

relative segregation of schools that is orthogonal to the relative segregation of neighborhoods (Figure 3). There is a strong negative relationship in the first two graphs between each racial segregation measure and the relative test scores of black students.[23] The relationship is weaker when we focus on the component of school segregation that is orthogonal to neighborhood segregation, and seems to be driven more heavily by a few outliers. As we document below, this relationship disappears entirely as we add control variables, though the relationship between residential segregation and black relative test scores remains strong.

IV. Regression Models for Black-White Gaps in Participation and Scores

a. Basic Models

Table 3 presents an initial set of estimates of the model given by equation (6). The upper panel summarizes models for the black-white gap in adjusted SAT scores, while the lower panel shows a parallel set of models that have the gap in SAT participation as the dependent variable. All the models include main effects for the overall fraction black and Hispanic in the city's schools, dummies for 5 census divisions, and (in the upper panel only) the black-white gap in a Mill's ratio formed from the race-specific SAT participation rates in the city.[24] We present three sets of specifications: models with only school segregation in columns A-C; models with only neighborhood segregation in columns D-F; and models with both segregation variables in columns G-I.

The most parsimonious models, in columns A and D, show strong negative effects

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[23] The SAT-state MSA with the most segregated schools is Gary, Indiana. Newark, New Jersey is second. Graphs using the black-white gaps in *unadjusted* scores look very similar to Figures 1-3.

[24] Although there are nine Census divisions, only six are represented among SAT states. In Table 3 and the remainder of the paper, we exclude cities (4 of 189 in SAT states) for which we cannot construct black-white differences in family background characteristics, introduced in Column C, using the 2000 Census microdata sample.

of racial segregation (measured across schools or neighborhoods) on average SAT scores and on SAT participation. The -125 coefficient in the model for SAT scores in column A, for example, implies that moving from complete segregation to complete integration would raise black relative SAT scores by 125 points, or about 60 percent of the overall black-white gap.[25] The -0.13 coefficient in the corresponding model for SAT participation suggests that a shift from complete segregation to full integration would raise the city-wide relative black participation rate by 13 percentage points.

The models in columns B and E add controls for a vector of MSA characteristics (the log of population, the log of land area, the fractions of residents with 13-15 and 16+ years of education, log mean household income, and the Gini coefficient of household income) and for black-white gaps in observed characteristics (parental education and family income) among SAT-takers in the MSA.[26] These additions reduce the size of the estimated segregation effect on test scores, but raise the size of the effect on participation. Finally, the most general specifications in columns C and F add controls for the black-white differences in several additional family characteristics (parental education, family income, child poverty, single-parenthood, and maternal employment), measured from 2000 Census data. These models also include controls for the mean difference in residual wages between black and white parents, computed separately for men and women. The motivation for including these wage gap measures is discussed in Section IV. In these specifications the effects of segregation on SAT scores are reduced somewhat, but remain statistically significant. The effects on SAT participation are also reduced and are no longer significant.

[25] The standard deviation of combined SAT scores is about 200, so the black-white gap is approximately one standard deviation, similar (in effect size) to the gap measured in the NAEP at ages 9, 13, or 17 (Perie, Morand, and Lutkus, 2005).

[26] For the analysis of SAT participation we do not control for the relative characteristics of SAT takers, since the population at risk includes all students in a city.

The models in columns G-I of Table 3 include both segregation measures simultaneously. In the sparsest specification (G), school segregation appears to have the larger effect. When we add controls for metropolitan and SAT-taker characteristics, however, the school segregation coefficient falls to near zero and the residential segregation coefficient becomes large and negative. The sum of the two coefficients is quite close to the residential segregation effect in the corresponding models in Columns D-F. We can reject that the neighborhood segregation effect is zero, but not (in column I) that the two forms of segregation have equal effects. Similar patterns are seen in the participation models: Residential segregation appears to reduce relative black participation while school segregation tends to increase it, and here we can reject equal effects. Overall, it seems that residential segregation matters, but controlling for this, differences in relative exposure to minority schoolmates have little effect on black relative achievement. Taking the coefficient on residential segregation in column F of Table 3 as a benchmark, the implied effect of moving from a highly segregated city (Gary Indiana, ΔR_c=0.70) to a nearly unsegregated city (Fort Walton Beach Florida, ΔR_c=0.06) is a 45 point closing in the black-white SAT gap (or roughly a 0.22 "effect size").[27]

We have estimated many alternative specifications to probe the robustness of this conclusion. Some of these alternative models are presented in Appendix Table 1. In one check, we include a dummy variable for cities from the three states with high fractions of Hispanic immigrants – California, Florida, and Texas. This has no effect on the pattern of results seen in Table 3. In a second check, we compared the effects of alternative school segregation measures. When only elementary school segregation for our cohort of test

[27] The -70 coefficient implies a 70/200 =0.35 standard deviation effect of a one-unit decrease in minority share in the neighborhood. This implies that the -7 percentage point treatment effect on minority exposure in the MTO experiment should have yielded a 0.025 standard deviation effect on test scores. The estimated treatment effect on math scores (Sanbonmatsu et et., 2006, Table 4, row 1, column 5) was 0.018 (s.e. 0.03).

takers is included, it has a coefficient of -19 (standard error 20) – very similar to the coefficient estimate for the "lifetime" segregation average in column I of Table 3. When we try to include separate effects for elementary and school segregation their coefficients are of opposite signs (elementary segregation negative, high school segregation positive, individually and jointly significant) but the sum is small and positive, while the effect of residential segregation remains large and negative.

Finally, we estimated models that allow the effects of minority exposure to differ for black and white students. Specifically, we allowed black students' exposure to minority neighbors to have separate effects on black and white test scores, and similarly allowed white students' exposure rates to affect both groups. Consistent with the "differenced" functional form used in Table 3, these models (reported in columns E-G of Appendix Table 1) indicate that black exposure to minority neighbors reduces black students' test scores but has little effect on whites, while the reverse is true for white exposure to minorities.[28] We cannot reject the assumption that exposure to minority neighbors has a similar negative effect on both blacks and whites, and that neighborhood segregation therefore widens the black-white test score gap.

d. Selection into SAT-taking

A potential concern with the results so far is that we may not have fully controlled for selective SAT participation. To probe the robustness of our results, Columns H and I of Appendix Table 1 present estimates of our basic specifications that omit the Mill's ratio control function and ignore the re-weighting adjustment for differential high school level participation within a given city. These simpler unadjusted models show a significant but

[28] These models dispense with the unrestricted MSA fixed effect that is implicit in our differenced models. The absence of cross-race effects suggests that any MSA effects are uncorrelated with racial composition.

slightly smaller effect of residential segregation on relative test scores, but no effect of school segregation once the residential measure is included.[29] The fact that the unadjusted models show smaller effects is consistent with the predicted pattern of selectivity biases, under the assumption that higher scoring students are more likely to write the test in all cities. Arguably, any remaining selection problems in our adjusted models have a similar effect, implying that the results in Table 3 understate the true effects of segregation.

A second and perhaps more persuasive way to evaluate the impact of selective test participation is to examine models for black-white relative attainment based on outcomes for a random sample of youths. We used the 2000 Census 5-percent micro samples to estimate the fraction of 16-24 year olds in each city who either are currently enrolled in school or have completed high school.[30] We then constructed the black-white gap in this outcome and related it to our city control variables and the segregation measures.

The resulting estimates are presented in Table 4, using a sample of 234 MSA's with at least 50 students of each race in the 5 percent Census samples. The specifications in columns A-E include only neighborhood segregation, while the models in columns F-J include both segregation measures. The specifications are similar to those in Table 3, with a few exceptions: the Mills ratio term is excluded; the SAT-taker background characteristics (introduced in columns B, E, and H of Table 3) are omitted; and the Census-based measures of black-white gaps in observable characteristics (introduced in columns C, F, and I of Table 3) are introduced in three stages, with just the parental education measures included in

[29] We have also explored other types of selection corrections, including artificially trimming the data to retain the same fraction of the high school population in each city. Our basic results of large negative effects of residential segregation and essentially zero effects of non-residential segregation have held up in every specification.

[30] To insulate against bias from endogenous mobility of young people who have left their parents' homes, we assign individuals to the MSA where they lived in 1995, when they were aged 11-19. A limitation of the Census data is that there is no family background information for children who are no longer living with their parents. Consequently, we make no individual-level adjustments for family background.

Columns C and H of Table 4, the remaining measures except for wage differences added in Columns D and I, and the parental wage measures added in E and J.

The simplest models in columns A-C suggest that there is a significant negative effect of neighborhood segregation on black youths' relative education outcomes. These findings are similar in spirit, though smaller in magnitude, to results reported by Cutler and Glaeser (1997), whose models include fewer controls.[31] The corresponding models in columns F-H suggest that once neighborhood segregation is taken into account, there is little or no additional effect of school segregation. Although imprecise, these estimates show the same pattern as our findings for test scores, suggesting that selective SAT participation is not the driving our main results.

Nevertheless, examination of the richest specifications in Table 4 (columns D-E and I-J suggests that inferences about the effects of segregation on educational attainment are sensitive to the set of background control variables. In particular, once the full set of relative background variables we use in Table 3 are added, the estimated impacts of school segregation on its own, or of school and neighborhood segregation taken together, fall in magnitude and become insignificant. By contrast, the models in Table 3 show robust negative effects of relative exposure to minority neighbors on black-white relative test scores. One potential explanation for the difference is that neighborhood segregation has smaller effects on basic achievement outcomes (like completing high school) than on higher-level achievement outcomes (like college entry test scores). Unfortunately, however, the Census outcome models have limited power against reasonable effect sizes, so it is difficult

[31] Cutler and Glaeser (1997) use a 1% sample of the 1990 Census, and relate black relative high school graduation rates (and other outcomes) to a residential segregation measure. Their models only control for 4 city-wide variables: log population, the fraction of blacks in the city, log median income, and manufacturing share of employment. Their estimates imply that moving from complete segregation to complete integration would raise relative high school graduation rates of black 20-24 year olds by 30 percentage points. This is enormous: Even in highly segregated cities in their sample the black graduation rate is 74%.

to reach definitive conclusions.

V. Confounding Influences

Our reading of the results in Tables 3 and 4 is that there is a relatively strong relationship between segregation and the black-white achievement gap, and that this relationship cannot be attributed to selective test participation. More tentatively, the link appears to run through neighborhoods rather than schools. In this section we address two key questions: (1) Is the effect of neighborhood segregation on relative achievement overstated because of omitted variables biases? (2) Is the effect of school segregation on relative achievement understated?

a. Is the Effect of Neighborhood Segregation Overstated?

The most obvious source of concern with the results in Table 3 is that there are unobserved differences in the latent abilities of black and white students in different cities that are correlated with the degree of segregation in the city. Although the test score gap is computed from "residual" test scores that control very flexibly for the observed parental education of SAT test takers, and our models control for black-white differences in parental characteristics of the SAT takers in each city as well as differences in family characteristics observed in the Census, it is still possible that unobserved ability gaps remain.

Recent work (e.g., Heckman and Carneiro, 2003; Cunha et. al, 2005) has shown that the academic achievement of children is strongly correlated with the cognitive ability of their parents, and that cognitive ability is a key determinant of earnings. This research suggests that a useful proxy for the unobserved ability of a child (conditional on parental education) is the unexplained component of his or her parent's earnings. Building on this idea, we fit a standard wage determination model separately by race and gender, and constructed estimates of the mean residual wage gaps between black and white mothers and fathers in each city.[32] The models reported in the final columns of Tables 3 and 4 include these residual wage gaps as additional controls. Their inclusion has essentially no impact on the estimated segregation effects, suggesting that differences in the unobserved cognitive abilities of black and white parents in different cities are not biasing our main results.

Despite this, the potential for endogeneity bias merits further consideration. There are two reasons why unobserved differences in the family backgrounds of black and white children could be correlated with the degree of segregation in a city. One is that the degree of residential segregation varies endogenously with the characteristics of the local population. The other is that people selectively move to a city (or move out) in response to the patterns of segregation, yielding an endogenously selected population. We suspect that the first mechanism is less important than the second, as residential segregation patterns are highly stable over time. Table 5A, for example, presents cross-city correlations between various measures of residential segregation in 2000, 1990, 1980, 1970, and 1960. For 2000 we use two indexes of segregation: our own relative exposure index (ΔR_c) and the so-called isolation index constructed by Glaeser and Vigdor (2001). For earlier years we show the correlations with isolation indexes constructed from Census tabulations by Cutler, Glaeser, and Vigdor (1999). Over any 20 year interval the correlation of residential segregation measures across cities is 0.8 or higher.[33]

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[32] Specifically, we used the 2000 Census 5% sample to identify adults with resident children age 18 or under. For each person we constructed an hourly wage (based on earnings and hours last year), and then regressed wages on MSA fixed effects, years of education, indicators for high school dropout and college graduation, and a cubic in potential experience, separately by race and gender. The MSA fixed effect is the mean residual wage for the race/gender group in that city.

[33] It is possible, of course, that segregation depends on the unobserved ability gap in the city at some point far in the past. We expect that any correlation between current segregation and historical ability gaps is absorbed by our controls for observed characteristics of current students' parents.

A more serious concern is the selective mobility of higher and lower ability black and white families across cities with differing levels of segregation. To quantify the potential impact of selective mobility, we examined changes in the distribution of high- and low-skill black and white families across cities over the 30 year period for which we could assemble appropriate data. We divided black and white adults in each census year, 1970 through 2000, into "high", "medium" and "low" education groups, choosing the cutoffs to keep the shares of each group approximately constant over time. We then calculated the mean segregation index experienced by each race/education group in each year, using the 2000 value of residential segregation for their city of residence as the measure of segregation. (The choice of a fixed base year for the segregation measure means that changes over time are driven solely by changes in the distribution of the group across cities).

The left-hand panels in Figure 4 show the mean values of the segregation index for each education group in 1980, 1990, and 2000, with blacks in the top row, whites in the middle row, and the black-white difference in the bottom row. These figures show that both blacks and whites shifted steadily toward less segregated cities between 1970 and 2000. The shift was more prominent for high-education blacks, consistent with a pattern of endogenous mobility that could lead to bias in our sparsest achievement models.

The second and third columns in Figure 4 show similar graphs for *residual* residential segregation, after controlling for census division and racial composition effects (second column) and the full set of control variables used in our main specifications (third column). Simply controlling for geographic region eliminates about half of the differential mobility of low- and high-skill blacks, and it is almost entirely eliminated by the addition of the remaining control variables. To the extent that mobility differences by observed education are informative about the relationship with unobserved ability, these graphs suggest that endogenous mobility might introduce bias in extremely sparse models but should not be a problem for our main estimates.

In sum, based on the observed mobility patterns of different education groups of whites versus blacks, the stability of residential segregation within cities over time, and the results from our investigation of residual wage gaps, we believe it is unlikely that the residential segregation effects in Table 3 are significantly biased by omitted ability factors.

b. Is the Effect of School Segregation Understated?

We turn now to the second question raised by the results in Table 3: Are the effects of school segregation understated? An obvious concern is measurement error. If school segregation is poorly measured, or highly variable over time, then the measures we have constructed may be unreliable, leading to attenuation biases. Attenuation would be particularly severe in models that include residential segregation, as the two are so highly correlated. To assess the reliability of school segregation, we constructed a number of independent measures for U.S. cities over a ten year period, and correlated them with our primary index. One limitation is that we have data on private schools only for 1997-98. The segregation measures for earlier years thus include only public schools.

The results are summarized in Table 5B. The first column and row pertain to our primary index, which includes public and private school students and is constructed to approximate the school career of our test cohorts. The second variable included is an analogous index computed only over public school students, which is nearly perfectly correlated across cities with our primary index. The remaining variables separate out public elementary and secondary-level segregation, for both 1988-1991 and 1998-2001. The raw correlations among variables are extremely high, never below 0.94.

The lower panel of Table 5B reports correlations for residuals from regressions of school segregation measures on our full vector of control variables (including residential segregation). The correlations remain quite high even for these residuals, at least 0.64 and mostly above 0.75. This compares favorably with the reliability of other commonly used constructs, and leads us to conclude that measurement error attenuation is not a serious concern with our school segregation measures.

Nevertheless, as a final check, we present an instrumental variables analysis that isolates the component of school segregation that is attributable to court-ordered school desegregation programs implemented in the 1970s and early 1980s in many U.S. cities. The instrument is based on Welch and Light's (1987) estimate of the change in the "dissimilarity index"—an alternative index of racial segregation—for the schools in the main school district in an MSA, from the year prior to the city's major desegregation plan to the last year of implementation of the order.[34] Welch and Light (1987) only collected data for larger school districts, which typically serve the central city of the MSA. We multiply the change in dissimilarity in this district by its share of metropolitan enrollment. Thus, the instrument reflects both the "bite" of the main desegregation plan and the size of the desegregated district relative to the overall MSA.

Table 6 presents the IV analysis. Given the small sample size for which the instrument is available, we adopt a parsimonious model similar to the one in column H of Table 3 (though with slightly less flexible controls for SAT-takers' background characteristics). OLS estimates in Column A are quite similar to those from our full sample, though less precise. Column B shows the first stage estimate: Even after two decades or

[34] This variable is set to zero for MSAs containing districts in the Welch and Light sample without a major desegregation plan. Our sample for the IV analyses thus consists of only 60 MSAs that are both in SAT states and in the Welch and Light sample.

more the court orders continue to have sizable effects on observed measures of school segregation. Finally, column C shows the IV estimate. This is relatively imprecise, but gives no indication that the OLS estimate is biased in such a way as to mask an underlying negative effect of school segregation.

V. Within School Segregation?

One potential explanation for our finding that school segregation has little or no effect on relative achievement is that in cities with highly segregated neighborhoods, school integration efforts are offset by programs and behaviors that lead to *within school* segregation (Clotfelter, Ladd, and Vigdor, 2003; Clotfelter, 2004; Eyler, Cook, and Ward, 1983).[35] As a proxy for within-school exposure, we use data on course enrollment patterns from the SAT data set. SAT-takers are asked whether they have taken honors courses and whether they intend to claim advanced placement (AP) credit or course exemptions in college on the basis of high school work. Column A of Table 7 presents models for the fraction of students in a city who intend to claim college-level credit in any subject, while Columns B through D present models for the fraction of students who indicated that they had taken honors courses in math, English, or any subject, respectively.

In Panels A and B we present estimates of the relationships between the school and neighborhood segregation measures and the black and white means of the course-taking variables. The estimates in Panel A show no significant relationship between either school

[35] Anecdotal evidence suggests that districts often create special programs to attract white students to high-minority schools or, alternatively, to avoid truly desegregating their school systems in the face of judicial oversight. As an example of the latter, the federal district court judge's opinion in *People Who Care v. Rockford Board of Education*, 851 F. Supp. 905 (1993) states: "The court finds that the ability grouping and tracking practices of the Rockford School District (hereinafter 'RSD') did not represent a trustworthy enactment of any academically acceptable theory or practice. The RSD tracking practices skewed enrollment in favor of whites and to the disadvantage of minority students. The court finds that it was the policy of the RSD to use tracking to intentionally segregate white students from minority students…." (p. 940)

or neighborhood segregation and black course-taking. The estimates in Panel B, by comparison, show relatively strong negative impacts of school segregation on honors and AP participation by whites, many of which are at the margin of significance. To interpret these impacts, note that a rise in our segregation index implies that whites are relatively less exposed to minorities. Thus, a negative coefficient means that white students are more likely to take honors and AP classes in cities with more integrated schools and neighborhoods. Finally, Panel C reports estimates for the black-white difference in honors participation at the city level. Increased school segregation is associated with large positive effects on the black-white gap in honors course taking and in AP participation. Increases in neighborhood segregation have negative effects, although the coefficients are mostly smaller and uniformly insignificant.

Though participation rates in honors and AP courses are limited measures of within-school exposure, the results in Table 7 support the hypothesis that across-school integration is associated with within-school segregation Holding constant neighborhood segregation, white students are more likely to participate in "high track" courses when schools are more integrated, presumably limiting the classroom-level exposure of blacks to whites.[36] To the extent that school peer effects operate through classroom-level exposure, then, our school segregation measure may have relatively little signal for the relevant peer group.

VI. Indirect Effects of School Quality and Peer Characteristics

All of our specifications so far have excluded any characteristics of the schools and neighborhoods of black and white students other than their minority composition. As noted

[36] We have also estimated models for the tracking measures that separate out the components of school segregation attributable to court-ordered desegregation. Standard errors are large, but the results indicate that, if anything, court-ordered desegregation has larger effects on tracking than does the residual component.

in the discussion of equation (5), our coefficient estimates capture direct minority exposure effects as well as any indirect effects associated with relative resources, peer characteristics, or macro segregation effects that can be predicted by the relative exposure of black and white students to minority schoolmates and neighbors. As a final step in our analysis we explore the potential contributions of two types of indirect effects: those arising from differences in school quality, and those arising from the relative incomes of schoolmates and neighbors.

a. Relative School Quality

Unfortunately, there are few measures of school quality available at the national level for broad samples of schools. We focus on a limited set of school resource measures (from the Common Core of Data, or CCD) and teacher quality measures (from the Schools and Staffing Survey, or SASS). Since the magnitude of any indirect effect of school resources depends on the extent to which differences in the resources at schools attended by black and white students in a city are correlated with ΔB_c and ΔR_c, we begin by presenting a series of models for the city-wide "quality gaps" between black and white students' schools, with the same explanatory variables included in our models for the black-white test score gap in Table 3. These models are presented in Table 8.

In Column A, we examine the number of full-time-equivalent teachers per student at public schools attended by white and black students in each MSA. Column B reports estimates for expenditures per pupil in districts enrolling white and black students.[37] Black-white gaps in these two measures are essentially unrelated to the degree of school or

[37] District-level spending per capita data are available from the CCD Local Education Agency Finance Survey (also known as the F-33 portion of the Census of Governments). Unfortunately, this is an imperfect measure of the actual spending at black and white students' schools if spending varies across schools in each district.

neighborhood segregation in a city.

Columns C-F report similar models for the black-white gap in average teacher characteristics, estimated from the SASS. Models for the gaps in average salaries and experience between the teachers at black and white students' schools (columns C and D) are noisy but show no significant segregation effects. The model in column E shows that neighborhood segregation is associated with fewer teachers who have undergraduate degrees in education at black students' schools relative to those attended by white students. Assuming that the fraction of teachers with an education major is a negative quality indicator, this could mean that part of our estimated segregation effects on test scores reflect indirect effects of teacher credentials, though we suspect any such effect is small. Finally, column F shows that black students have a substantially lower relative fraction of white teachers in cities with greater school segregation. Interestingly, there is no corresponding effect of neighborhood segregation.

We have also estimated variants of the models in Table 3 that included the school quality measures directly. As is well known from the omitted variables formula, there is a direct connection between the coefficient estimates in Table 8 and the difference in the estimated effects of segregation with and without controls for school quality gaps.[38] Consistent with the findings in Table 8, the estimated segregation effects on test scores are invariant to inclusion of any of the available measures, either alone or in combination.

b. Schoolmate and Neighborhood Income

Differences in the average characteristics of schoolmates and neighbors of black and

[38] Unfortunately, as the SASS samples relatively few teachers in each MSA, the teacher quality measures are quite noisy. This attenuates their estimated effects on test scores and reduces the impact of their inclusion on the segregation coefficients. By contrast, measurement error in the resource variables would not bias the estimates in Table 8.

white students may also contribute to indirect exposure effects. To provide some evidence on the potential magnitudes, we used data from the CCD to estimate the black-white gap in average exposure to schoolmates receiving free school lunches (a common though imperfect proxy for low income) and data from the Census to compute the black-white gap in average neighborhood income.

Columns A and B of Table 9 present models in which we regress these measures on our racial segregation measures. As shown in column A, the black-white gap in exposure to schoolmates receiving free lunches is positively related to the relative segregation of the schools in a city, but negatively related to the degree of neighborhood segregation. Thus, any negative effect of schoolmate poverty on test scores should contribute negatively to the estimated effect of school segregation but positively to the estimated effect of residential segregation. The model in column B shows that the black-white gap in mean neighborhood income is negatively related to neighborhood segregation (but uncorrelated with school segregation). Any positive effect of neighborhood income on student achievement should therefore contribute negatively to the estimated residential segregation effect.

Columns C, D, and E present models that assess these conjectures directly, by adding the school lunch and neighborhood income measures to the specification shown in column I of Table 3. Consistent with the pattern of results in Table 3, differential exposure to low-income *schoolmates*—at least using an admittedly limited proxy based on school lunch participation—has little effect on relative black test scores, while differential exposure to low-income *neighbors* seems to reduce black performance. After controlling for the indirect effect associated with neighborhood income, the estimated effect of exposure to minority neighbors remains negative but is reduced by about one-half and is no longer statistically insignificant. These estimates suggest that an important share of the neighborhood segregation effect measured in our main specifications can be explained as an indirect effect of exposure to low income neighbors (Wilson, 1987), rather than as a direct minority exposure effect.

VII. Summary and Conclusions

In this paper we present new evidence on the effects of racial segregation on the relative achievement of black students. Building from a model in which the racial composition of school and neighborhood peer groups exerts both direct and indirect causal effects on student achievement, we show that the black-white achievement *gap* in a city will vary with the relative segregation of schools and neighborhoods in the city.

Our main empirical evidence is based on SAT outcomes for one third of test takers in the 1998-2001 test cohorts. We match test-takers to information on the racial composition of their high schools and to an extensive set of family background characteristics of black and white students in their cities. To address concerns about potential selectivity biases in the SAT outcomes, we also use 2000 Census data to construct measures of the relative achievement of black and white youth in different metropolitan areas.

When we focus on one type of segregation at a time, both school and neighborhood segregation appear to have negative effects on black relative test scores and educational attainment. In models that include both school and neighborhood segregation, however, the effects of relative exposure to black and Hispanic schoolmates are uniformly small and statistically insignificant, whereas the effects of relative exposure to black and Hispanic neighbors are negative. Probes into possible explanations for the absence of school segregation effects, including instrumental variables estimates based on court ordered desegregation programs, give no indication that our estimates are biased in a way that would obscure negative effects of school segregation.

Taken as a whole, our results indicate that segregation matters for black relative achievement. The precise channels for these effects remains open, although our tentative conclusion is that the neighborhood composition matters more than school composition. Moreover, an important share of the neighborhood segregation effect may be an indirect effect deriving from the strong correlation between a neighborhood's minority composition and its mean income.

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Appendix

A. Taking Advantage of Student-Level Covariates

To fully exploit our rich microdata, we partial out the student-level covariates observed in the SAT files (mother's education, father's education, and family income) before aggregating to the city level. We estimate separate student-level models for white and black test takers that each include unrestricted school effects and a highly flexible specification for these covariates:

$$Y_{ijsc} = \zeta_{jsc} + f_j(X_{ijsc}) + \varepsilon_{ijsc}.$$

We then form an adjusted test score for each student:

$$r_{ijsc} = Y_{ijsc} - \hat{f}_j(X_{ijsc}),$$

and consider a city-level model for the difference in mean adjusted test scores:

$$\text{(A1)} \quad r_{1c} - r_{2c} = \Delta X'_c \alpha' + \Delta B_c \gamma' + \Delta R_c \delta' + F_c \psi' + v_c + e_{1c} - e_{2c},$$

where $e_{jc} = f_{jc} - \hat{f}_{jc}$, f_{jc} represents the mean of $f_j(X_{ijsc})$ for students of race j in city c, $\hat{f}_{jc}$ represents its estimated counterpart; and $\Delta X'_c$ includes black-white differences in a limited selection of background variables (including $\hat{f}$, several of its individual arguments, and additional measures that are available from Census data). Although the first stage adjustment may not fully eliminate the effect of observable student characteristics, we anticipate that the inclusion of $\Delta X'_c$ in the second stage model absorbs most of the remaining variation in Δe_c.

B. Derivation of Selection-Corrected Estimation Model

Assume that the probability that student i in race group j in school s in city c writes the SAT is given by a latent index model of the form:

$$\text{(B1)} \quad P(\text{i writes test} \mid X_{ijsc};\ s,j,c) = p_{ijsc} = P(X_{ijsc}\pi_j + \mu_{ijsc} \geq k_{jsc}),$$

where μ_{ijsc} is an error component and k_{jsc} is a school and group-specific threshold. Assuming that μ_{ijsc} and the error ε_{ijsc} in the test score outcome model (equation 1) are jointly normally distributed, with a distribution that is constant across schools (but may vary by race) the expected test score for student i in group j in school s, conditional on writing the test, is

$$\text{(B2)}\quad E[\, y_{ijsc} \mid \text{i writes test}, X_{ijsc}; s,j,c \,] = X_{ijsc}\alpha_j + Z_{sc}\beta_j + W_{ijsc}\nu_j + B_{sc}\gamma_j + R_{ijsc}\delta_j + u_{jsc} + \zeta_j\lambda(p_{ijsc}) ,$$

where $\lambda(p)$ is the inverse Mills ratio function evaluated at $\Phi^{-1}(p)$ and ζ_j is a race-specific coefficient that depends on the correlation of μ_{ijsc} and ε_{ijsc}. The adjusted observed test score for individual i is therefore:

$$\text{(B3)}\quad r_{ijsc} = X'_{ijsc}\alpha_j + Z_{sc}\beta_j + W_{ijsc}\nu_j + B_{sc}\gamma_j + R_{ijsc}\delta_j + u_{jsc} + \zeta_j\lambda(p_{ijsc}) + e_{ijsc},$$

where e_{ijsc} combines the estimation error in $\hat{f}_j$ and the deviation of y_{ijsc} from its conditional expectation.

A simple average of the observed test scores in a city will contain a participation-weighted average of the school effects u_{jsc}'s that differs from the unconditional mean u_{jc}. The first step in our adjustment procedure is therefore to reweight the data to obtain an enrollment-weighted average of the observed residual test scores for black and white students.

$$r_{jc} = 1/N_{jc}\ \Sigma_s\ N_{isc}\, r_{jsc} = 1/N_{jc}\, \Sigma_s\, N_{jsc}/M_{jsc}\, \Sigma_i\, r_{ijsc} = 1/N_{jc}\ \Sigma_s\, \Sigma_i\, p_{jsc}^{-1}\ r_{ijsc} ,$$

where N_{jc} is the total number of 12th graders of group j in city c, N_{jsc} is the number of 12th graders in school s, M_{jsc} is the number of test-takers in group j in school s, and $p_{jsc} = M_{jsc}/N_{jsc}$ is the test participation rate of group j in school s. Equation (B3) implies that:

$$\text{(B4)}\quad r_{jc} = X'_{jc}\alpha_j + Z_{jc}\beta_j + W_{jc}\nu_j + B_{jc}\gamma_j + R_{jc}\delta_j + u_{jc} + \zeta_j\,(1/N_{jc})\,\Sigma_s\,\Sigma_i\, p_{jsc}^{-1}\,\lambda(p_{ijsc}) + e_{jc} ,$$

where Z_{jc}, W_{jc}, R_{jc}, B_{jc} and u_{jc} are the same as in equation (2) of the main text.

Next, consider a first order expansion of the selection-correction function for individual i around p_{jsc}, the test participation rate for students of group j in school s:

$$\lambda(p_{ijsc}) = \lambda(p_{jsc}) + (p_{ijsc} - p_{jsc})\, \lambda'(p_{jsc}) + \xi_{ijsc} .$$

For a range of probabilities between 0.2 and 0.8 the function $\lambda(p)$ is approximately linear and the error ξ_{ijsc} is small. Using this expansion:

$$(1/N_{jc}) \Sigma_s \Sigma_i p_{jsc}^{-1} \lambda(p_{ijsc}) = (1/N_{jc}) \Sigma_s \Sigma_i p_{jsc}^{-1} \{ \lambda(p_{jsc}) + (p_{ijsc} - p_{jsc})\, \lambda'(p_{jsc}) + \xi_{ijsc} \}$$

$$= \lambda_{jc} + \theta_{jc} + \xi_{jc} ,$$

where

$$\lambda_{jc} = (1/N_{jc}) \Sigma_s \Sigma_i p_{jsc}^{-1} \lambda(p_{jsc}) ,$$

$$\xi_{jc} = (1/N_{jc}) \Sigma_s \Sigma_i p_{jsc}^{-1} \xi_{ijsc} ,$$

$$\theta_{jc} = (1/N_{jc}) \Sigma_s N_{jsc} \lambda'(p_{jsc}) (1/N_{jsc}) \Sigma_i (p_{ijsc} - p_{jsc})$$

$$= (1/N_{jc}) \Sigma_s N_{jsc} \lambda'(p_{jsc}) \{ p^T_{jsc} - p_{jsc} \},$$

and p^T_{jsc} is the average test participation probability *among the test writers of group j in school s*. Note that the first term, λ_{jc}, is just an enrollment-weighted average of the inverse Mills ratio functions evaluated at the (race-specific) test participation rates at each school. The second term, ξ_{jc}, is an average approximation error, which we expect to be small. The third term, θ_{jc}, is more problematic. This term measures the degree of "within-school" selectivity of test-takers. It disappears if test participation is random within a school, but is strictly positive otherwise.

Combining these results with equation (B4), an approximate expression for the average adjusted test score for group j in city c is:

(B5) $$r_{jc} = X'_{jc} \alpha_j + Z_{jc} \beta_j + W_{jc} \nu_j + B_{jc} \gamma_j + R_{jc} \delta_j + u_{jc} + \zeta_j \lambda_{jc} + \zeta_j \theta_{jc} + e_{jc} .$$

Differencing between blacks and whites in the same city and substituting equation (3) from the main text for the difference in the unobserved ability components leads to:

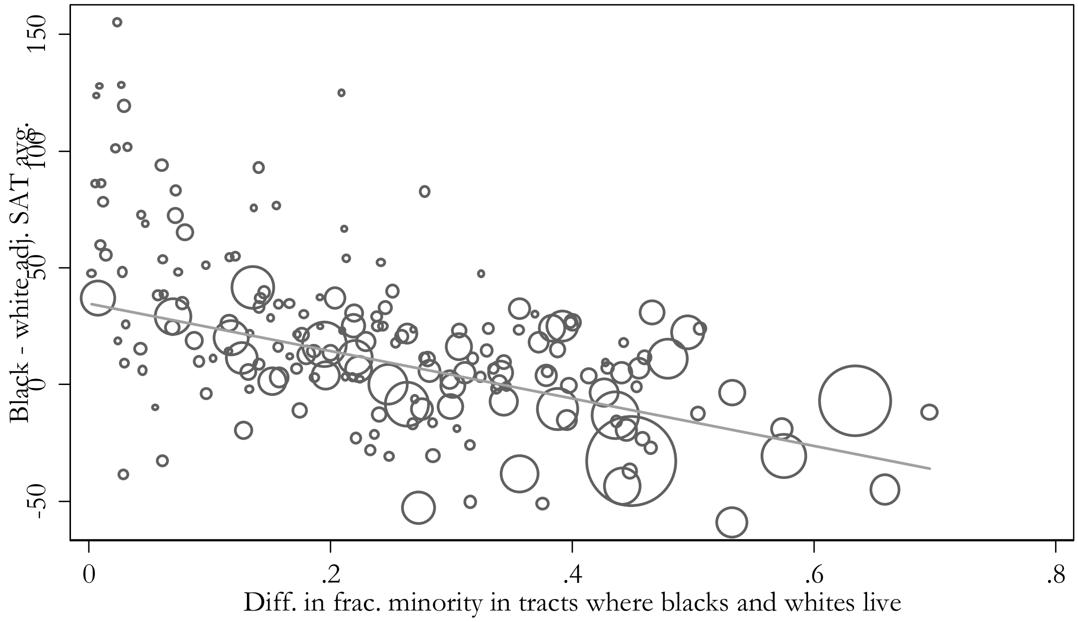
$$\text{(B6)} \quad \Delta r_c = r_{1c} - r_{2c} = X'_{1c}\alpha_1 - X'_{2c}\alpha_2 + Z_{1c}\beta_1 - Z_{2c}\beta_2 + W_{1c}\nu_1 - W_{2c}\nu_2$$

$$+ B_{1c}\gamma_1 - B_{2c}\gamma_2 + R_{1c}\delta_1 - R_{2c}\delta_2 + F_c\psi + \zeta_1\lambda_{1c} - \zeta_2\lambda_{2c}$$

$$+ \zeta_1\theta_{1c} - \zeta_2\theta_{2c} + \eta_c + e_{1c} - e_{2c}.$$

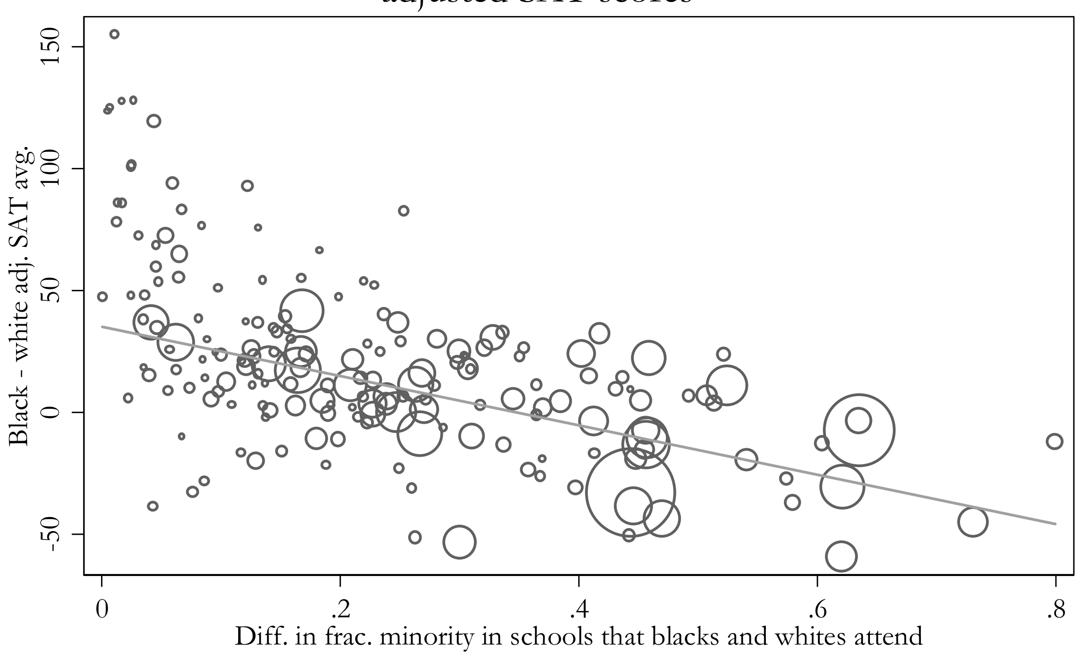
or, if the coefficients β, δ, γ, and ζ are the same for whites and blacks, equation (6) in the text.

Figure 1. Residential segregation and black-white gaps in adjusted SAT scores



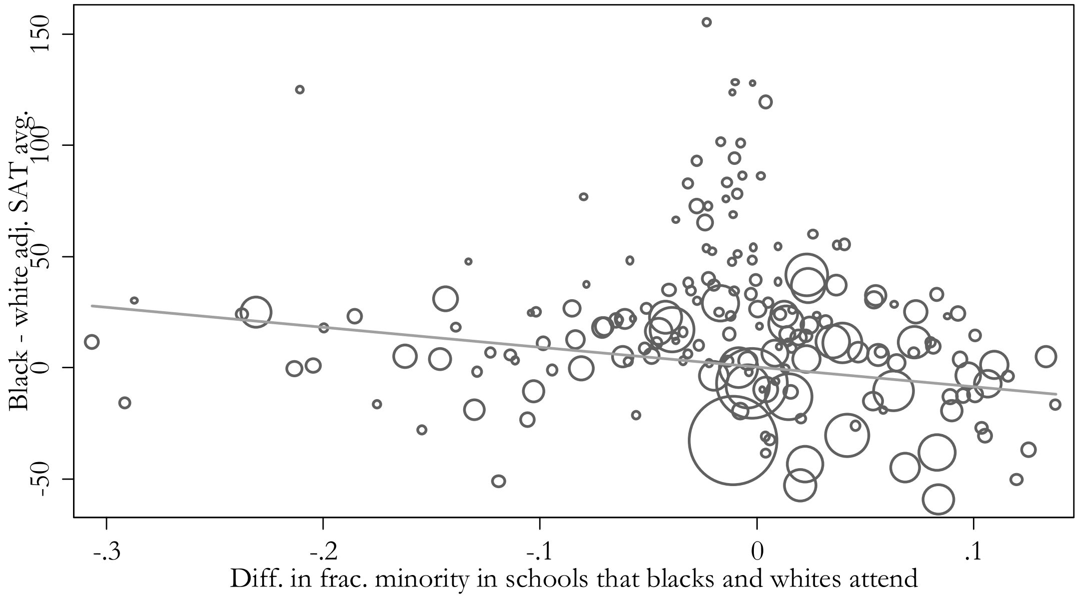
Notes: Sample is metropolitan areas in SAT states. Circle sizes are proportional to the sampling error variance in MSA black-white gaps (see notes to Table 1 for details). Line is the weighted least squares regression line (slope -102, s.e. 10).

Figure 2. School segregation and black-white gaps in adjusted SAT scores



Notes: See notes to Figure 1. Line is the weighted least squares regression line (slope -101, s.e. 9).

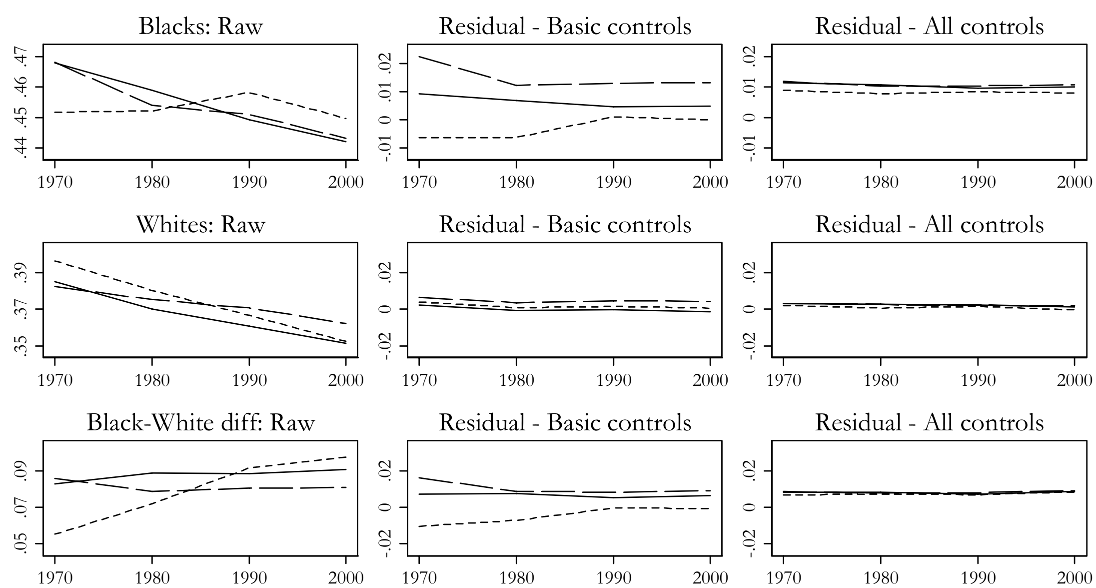
Figure 3. School segregation unexplained by residential segregation and black-white gaps in adjusted SAT scores



Notes: See notes to Figure 1. "Residual school segregation" is the residual from a weighted bivariate regression of school segregation on residential segregation (coeff. 0.99, s.e. 0.01). Line is the weighted least squares regression line (slope -89, s.e. 31).

Figure 4. Mobility-induced changes in segregation, 1970-2000

Mean residential segregation (or residual), by race, year, and education level



Notes: Top row depicts blacks, middle whites, and bottom the black-white difference. Solid line is the mean for everyone; short dash for low education individuals; long dash for high education. Segregation is measured in 2000; changes over time reflect changes in MSA weighting attributable to changes in the geographic distribution of the population. Details available from the authors.

Table 1: Residential and school segregation in most- and least-segregated metropolitan areas

	City fraction black	City fraction Hispanic	School fr. minority			Census tract fr. minority		
			Avg. for black students	Avg. for white students	Diff.	Avg. for black residents	Avg. for white residents	Diff.
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)
Integrated schools								
Fort Walton Beach, FL	9%	4%	19%	14%	5%	19%	13%	6%
Wilmington, NC	16%	2%	31%	25%	6%	40%	14%	25%
Brazoria, TX	8%	23%	36%	29%	7%	38%	29%	9%
Anchorage, AK	6%	6%	18%	10%	8%	17%	11%	6%
Gainesville, FL	19%	6%	40%	32%	9%	44%	21%	23%
Segregated schools								
Gary, IN	19%	10%	90%	10%	80%	83%	13%	70%
Newark, NJ	22%	13%	85%	12%	73%	80%	14%	66%
New York, NY	23%	25%	85%	22%	64%	84%	20%	63%
Bergen-Passaic, NJ	8%	17%	74%	11%	63%	67%	14%	53%
Philadelphia, PA-NJ	20%	5%	73%	11%	62%	69%	11%	58%
Integrated schools, given residential segregation								
Fort Myers-Cape Coral, FL	6%	10%	35%	19%	16%	57%	11%	46%
Fort Pierce-Port St. Lucie, FL	11%	8%	39%	24%	15%	57%	13%	44%
Hagerstown, MD	8%	1%	14%	5%	9%	43%	6%	37%
Odessa-Midland, TX	6%	36%	47%	37%	10%	64%	31%	33%
Tampa-St. Petersburg-Clearwater, FL	10%	10%	36%	20%	17%	54%	14%	39%
Segregated schools, given residential segregation								
Tallahassee, FL	33%	4%	68%	27%	41%	55%	28%	27%
Jersey City, NJ	12%	40%	83%	38%	45%	69%	38%	31%
New Haven-Meriden, CT	13%	10%	70%	12%	58%	58%	13%	45%
Fort Worth-Arlington, TX	11%	18%	62%	17%	46%	54%	20%	34%
Trenton, NJ	19%	10%	72%	14%	57%	64%	18%	47%
Average	14%	21%	63%	23%	40%	62%	22%	40%

Notes: Segregation rankings in first two panels are by difference in fraction minority (black and Hispanic) in black and white students' schools, as in column E. In second two panels, rankings are by the residual from a regression of this measure on an analogous measure computed over census tracts (column H). In each case, the 5 most-segregated and least-segregated cities in SAT states with at least 5% black population are shown. Average listed in bottom row is average over all cities meeting these criteria, weighted by $(N_w^{-1} + N_b^{-1})^{-1}$, where N_w and N_b are the number of white and black residents of the MSA, respectively.

Table 2. Summary statistics, metropolitan areas

	All Cities		In SAT states		Not in SAT states	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
	(A)	(B)	(C)	(D)	(E)	(F)
N	331		189		142	
Population (millions)	2.856	3.010	3.042	3.168	2.412	2.552
Fraction black	0.12	0.20	0.11	0.08	0.14	0.10
Fraction Hispanic	0.21	0.07	0.25	0.23	0.09	0.10
log(Mean HH income)	10.98	----------	10.99	0.20	10.96	0.16
Segregation (Black fraction minority - white fraction minority)						
Residential (Tract), 2000	0.34	0.19	0.32	0.20	0.38	0.16
Elementary schools, 1998-2001 cohorts	0.36	0.21	0.35	0.20	0.40	0.23
High schools, 1998-2001	0.31	0.19	0.29	0.18	0.37	0.22
School career avg., 1998-2001 cohorts	0.35	0.20	0.33	0.19	0.39	0.22
SAT-taking rate						
All students	0.28	0.14	0.34	0.10	0.14	0.11
White students	0.32	0.14	0.39	0.09	0.16	0.12
Black students	0.22	0.12	0.27	0.08	0.09	0.09
SAT-takers						
Avg. SAT	1033.4	71.3	999.3	46.0	1114.8	53.0
Black-white avg. SAT	-193.2	36.5	-194.0	34.3	-191.4	41.4
Black-white avg. SAT (reweighted)	-203.0	42.1	-197.7	36.0	-215.5	52.1

Notes: All summary statistics are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$, where N_w and N_b are the number of white and black residents of the MSA, respectively. Average SATs and black-white SAT differences use SAT sampling weights within cities.

Table 3. Basic estimates of school segregation's effect on black-white differences in SAT participation and residual scores

	School Segregation			Residential Segregation			School & Nbhd. Segregation		
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)
Dependent variable is B-W adjusted test score gap									
Black-white difference: Fr. minority in students' schools	-125	-78	-43				-88	-10	-7
	(19)	(24)	(19)				(34)	(27)	(25)
Black-white difference: Fr. minority in residents' nbhds.				-121	-111	-70	-43	-103	-63
				(18)	(25)	(20)	(31)	(27)	(24)
N	185	185	185	185	185	185	185	185	185
R-squared	0.56	0.71	0.78	0.54	0.73	0.79	0.57	0.73	0.79
p-value, residential=school=0							0.00	0.00	0.00
Dependent variable is B-W participation gap									
Black-white difference: Fr. minority in students' schools	-0.13	-0.20	0.03				0.09	0.07	0.14
	(0.06)	(0.08)	(0.05)				(0.08)	(0.10)	(0.08)
Black-white difference: Fr. minority in residents' nbhds.				-0.19	-0.31	-0.08	-0.27	-0.38	-0.20
				(0.06)	(0.09)	(0.05)	(0.08)	(0.11)	(0.08)
N	185	185	185	185	185	185	185	185	185
R-squared	0.60	0.64	0.81	0.63	0.68	0.81	0.63	0.68	0.82
p-value, residential=school=0							0.00	0.00	0.05
Controls									
MSA demographic characteristics	n	y	y	n	y	y	n	y	y
B-W background controls, SAT takers (upper panel only)	n	y	y	n	y	y	n	y	y
B-W background controls, 15-19 year olds in Census data	n	n	y	n	n	y	n	n	y
B-W difference in residual parental wages	n	n	y	n	n	y	n	n	y

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. City-level black-white differences in residual SATs (top panel) are computed over SAT-taker data that are re-weighted using school-by-race participation rates; see text for details. All specifications include controls for census division fixed effects and main effects for the fraction black and fraction Hispanic in the city's schools; those in top panel also include a control for the black-white difference in an inverse Mills ratio computed from city-by-race-level SAT participation rates . All standard errors are clustered on the CMSA.

Table 4. Residential and school segregation effects on black-white difference in school persistence, measured from Census data

	Neighborhood Segregation					School & Neighborhood Segregation				
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)	(J)
B-W fr. minority in students' schools						-1.8	-2.0	-3.2	-1.1	-1.3
						(4.0)	(3.5)	(3.0)	(2.9)	(2.9)
B-W fr. minority in residents'	-6.0	-11.8	-5.6	-3.2	-2.8	-4.4	-10.0	-2.7	-2.3	-1.6
neighborhoods	(1.9)	(3.0)	(2.5)	(2.6)	(2.6)	(3.8)	(4.1)	(3.8)	(3.7)	(3.7)
Control variables										
MSA demographic characteristics	n	y	y	y	y	n	y	y	y	y
B-W gaps in parental educ.	n	n	y	y	y	n	n	y	y	y
B-W gaps in other observables	n	n	n	y	y	n	n	n	y	y
B-W gap in residual parental wages	n	n	n	n	y	n	n	n	n	y
N	234	234	234	234	234	234	234	234	234	234
R-squared	0.30	0.37	0.50	0.55	0.55	0.30	0.37	0.50	0.55	0.55
p-value, residential=school=0						0.01	0.00	0.03	0.42	0.49

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Dependent variable is the difference between blacks and whites in the percentage of youth who have finished HS or who are enrolled in school, measured over 16-24 year olds in the 2000 census who lived in the metropolitan area in 1995. Sample excludes MSAs with fewer than 50 black or 50 white observations. The persistence gap ranges in principle from -100 to 100, and has sample mean -6.9 and S.D. 3.8. All specifications include controls for census division fixed effects and main effects for the fraction black and fraction Hispanic in the city's schools. Columns C-E (and H-J) introduce control variables brought in in column F (and I) of Table 3 in three stages. All standard errors are clustered on the CMSA.

Table 5A. Correlations of residential segregation over time

	B-W fr. black in residents' neighborhoods, 2000	Isolation index				
		2000	1990	1980	1970	1960
	(A)	(B)	(C)	(D)	(E)	(F)
N	331	314	291	264	200	149
Raw (weighted) correlations						
Residential seg. index	1	1.00	0.93	0.90	0.75	0.73
2000 Isolation	1.00	1	1	0.89	0.75	0.72
1990	0.93	0.92	1	0.90	0.79	0.77
1980	0.90	0.89	0.90	1	0.94	0.91
1970	0.75	0.75	0.79	0.94	1	0.97
1960	0.73	0.72	0.77	0.91	0.97	1
1950	0.62	0.64	0.54	0.67	0.71	0.78
Residual correlations (between variables pre-residualized against control variables plus 2000 school segregation)						
Residential seg. index	1	0.99	0.66	0.59	0.42	0.42
2000 Isolation	0.99	1	0.65	0.59	0.41	0.41
1990	0.66	0.65	1	0.65	0.54	0.49
1980	0.59	0.59	0.65	1	0.83	0.73
1970	0.42	0.41	0.54	0.83	1	0.88
1960	0.42	0.41	0.49	0.73	0.88	1

Notes: Isolation measures are from Cutler, Glaeser, and Vigdor (1999), and are computed over areas that were tracted within the contemporaneous MSA boundaries in each year.

Table 5B. Correlations across various measures of school segregation

	Lifetime, 1998-2001 cohort		1998-2001		1988-1991	
	Public and private schools	**Public schools only**	**HS**	**Elem.**	**HS**	**Elem.**
	(A)	**(B)**	**(C)**	**(D)**	**(E)**	**(F)**
N	331	331	331	331	298	298
Raw (weighted) correlations						
Lifetime, public and private	1	0.99	0.96	0.97	0.98	0.99
Lifetime, public only	0.99	1	0.97	0.98	0.98	0.99
Public HS, 1998-2001	0.96	0.97	1	0.97	0.99	0.94
Public elem., 1998-2001	0.97	0.98	0.97	1	0.98	0.96
Public HS, 1988-91	0.98	0.98	0.99	0.98	1	0.95
Public elem., 1988-91	0.99	0.99	0.94	0.96	0.95	1
Residual correlations (between variables pre-residualized against control variables plus 2000 residential segregation)						
Lifetime, public and private	1	0.97	0.79	0.78	0.79	0.95
Lifetime, public only	0.97	1	0.83	0.79	0.78	0.97
Public HS, 1998-2001	0.79	0.83	1	0.79	0.95	0.66
Public elem., 1998-2001	0.78	0.79	0.79	1	0.77	0.71
Public HS, 1988-91	0.79	0.78	0.95	0.77	1	0.64
Public elem., 1988-91	0.95	0.97	0.66	0.71	0.64	1

Notes: 1988-91 school segregation measures are unavailable for MSAs in states that did not report the racial composition of schools in those years. 1998-2001 lifetime segregation average places 2/3 weight on the 1988-91 elementary segregation measure and 1/3 on the 1998-2001 high school segregation measure; when the former is unavailable, a segregation measure is computed using the relevant cohort in the first available data. For example, for Maine the 1988-91 elementary segregation measure is replaced by a measure for grades 5-8 in 1993-4; 6-9 in 1994-5; 7-10 in 1995-6; and 8-11 in 1996-7.

Table 6. Instrumental variables estimates of school segregation effect

	Residential & school	
	OLS	1st stage
	(A)	(B)
B-W fr. minority in students' schools	-18	
	(36)	
B-W fr. minority in residents' neighborhoods	-113	0.93
	(30)	(0.09)
Change in dissimilarity index induced by major desegregation plans (/100)		0.23
		(0.06)
N	60	60

Notes: Models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$ and standard errors are clustered CMSA. Control variables are those in Column H of Table 3, but with just a singl control for the black-white difference in SAT-takers' background index in place c full set of SAT-taker black-white differences in observables.

Table 7. Residential and school segregation effects on honors course-taking among SAT-takers

	Plan to claim adv. / exempt status in any subject	Took honors courses		
		Math	English	Any subject
	(A)	(B)	(C)	(D)
Panel A: Percentage among black SAT-takers				
B-W fr. minority in students' schools	-3.3	2.0	-2.8	2.6
	(7.4)	(7.4)	(11.3)	(12.1)
B-W fr. minority in residents' census tracts	9.3	-2.5	-7.3	-11.6
	(9.7)	(7.8)	(12.0)	(12.9)
Panel B: Percentage among white SAT-takers				
B-W fr. minority in students' schools	-15.4	-10.5	-21.9	-14.0
	(6.7)	(8.7)	(10.3)	(9.5)
B-W fr. minority in residents' census tracts	21.9	7.0	6.4	8.4
	(6.9)	(8.4)	(11.0)	(10.5)
Panel C: Difference between black and white percentages				
B-W fr. minority in students' schools	12.3	10.2	14.8	12.4
	(6.4)	(7.5)	(7.3)	(7.8)
B-W fr. minority in residents' census tracts	-4.4	-5.3	-11.4	-14.9
	(8.9)	(7.6)	(8.8)	(8.6)

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. All columns include controls from column D of Table 3, except that controls measuring B-W gaps in Table 3 are included here as black averages, white averages, and black-white gaps in Panels A, B, and C, respectively. All standard errors are clustered on the CMSA.

Table 8. Estimates of residential and school segregation's effects on black-white differences in school resources and teacher characteristics

	Resources (CCD)		Teacher characteristics (SASS)			
	PP Expenditures ($1,000s)	Teacher / pupil ratio * 100	Avg. salary ($1,000s)	Avg. experience	Fr. teachers with education majors	Fr. teachers white
	(A)	(B)	(C)	(D)	(E)	(F)
B-W fr. minority in students' schools	1.22	0.45	-3.08	4.44	0.12	-0.59
	(0.99)	(0.36)	(5.30)	(3.23)	(0.12)	(0.14)
B-W fr. minority in residents' neighborhoods	-0.68	-0.67	-0.90	-1.45	-0.34	-0.04
	(0.99)	(0.42)	(7.36)	(3.92)	(0.14)	(0.18)
R-squared	0.33	0.37	0.12	0.15	0.16	0.62
p-value, residential=school=0	0.35	0.28	0.75	0.34	0.05	0.00

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Dependent variable in each column is the estimated difference between the average of the indicated variable in black students' schools (districts in col. A) and that in white students' schools. School segregation measures are computed over current (1998-2001) enrollment in all grades, and restricted to public schools in Columns A and B. All columns include controls from column D of Table 4. All standard errors are clustered on the CMSA.

Table 9. Relationship between racial segregation and income differences between black & white students' schools and residents' tracts

	Dependent variable: B-W Gap in				
	Fr. free lunch in school	ln(per capita income) in tract	Adjusted SAT score		
	(A)	(B)	(C)	(D)	(E)
Black-white difference: Fr. minority in students' schools	0.64	0.03	-7	-16	-10
	(0.05)	(0.05)	(25)	(25)	(25)
Black-white difference: Fr. minority in residents' nbhds.	-0.25	-0.34	-63	-59	-34
	(0.06)	(0.08)	(24)	(26)	(27)
Black-white difference: Fr. free lunch in students' schools				9	
				(41)	
Black-white difference: ln(per capita income) in residents' nbhds.					61
					(28)
N	292	323	185	176	185
R-squared	0.94	0.91	0.79	0.79	0.80
p-value, residential=school=0	0.00	0.00	0.00	0.00	0.15

Notes: Models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$ and standard errors are clustered on the CMSA. Control variables in columns C-E are those in Column I of Table 3. Columns A & B omit control variables measured only over SAT-takers; in these columns, racial composition main effects and segregation measures are not the cohort averages used in SAT analyses, but are measured over all grades in 1998-2001 (using public schools only in column A).

Appendix Table 1. Alternative specifications

	Base model	CA/TX/FL indicator	Separate elementary and HS seg. effects		Separate exposure effects			Un-reweighted data, no selection controls	
					B-W Gap	Black scores	White scores		
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)
Black-white difference: Fr. minority in students' schools	-7	-7						-11	
	(25)	(24)						(22)	
Black-white difference: Fr. minority in residents' nbhds.	-63	-63	-53	-91				-30	-40
	(24)	(24)	(22)	(26)				(25)	(18)
CA/TX/FL		5							
		(6)							
Black-white difference: Fr. minority in students' elementary schools			-19	-63					
			(20)	(25)					
Black-white difference: Fr. minority in students' high schools				87					
				(34)					
Fr. minority in black residents' nbhds.					-70	-60	10		
					(26)	(22)	(20)		
Fr. minority in white residents' nbhds.					69	-19	-88		
					(71)	(71)	(44)		
N	185	185	185	185	185	185	185	185	185
R-squared	0.79	0.79	0.79	0.80	0.79	0.79	0.83	0.80	0.80
p-value, residential=school=0	0.00	0.00	0.00	0.00	0.00	0.02	0.06	0.07	0.00
p-value, school=0				0.02					

Notes: Model in Column A is that from Table 3, Column I. Remaining columns modify the specification slightly, with the same set of control variables (except that in Columns H and I, the inverse Mill's ratio control is omitted and black-white gaps in SAT-takers' background characteristics are computed from un-reweighted data). Dependent variable in Column F is the adjusted mean score among blacks; in Column G among whites.