Racial Segregation and the Black-White Test Score Gap

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ABSTRACT

Racial segregation is often blamed for part of the achievement gap between blacks and whites. In this paper 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. Without controlling for neighborhood segregation, we find that school segregation is negatively associated with black relative test scores, and also with relative education and employment outcomes measured in the 2000 Census. In models that include both school and neighborhood segregation, however, the effect of relative exposure to black schoolmates is uniformly small and statistically insignificant, while neighborhood segregation has a strong negative effect. Instrumental variables estimates that isolate the components of school segregation associated with court-ordered desegregation plans or the geographic features of a city are consistent with this result but imprecise. Models that include school segregation, neighborhood segregation, and measures of the relative exposure of blacks to other characteristics of their neighbors (e.g. education and income) show weaker effects of neighborhood segregation, suggesting that the socio-economic status of neighbors, rather than their race, may be the primary source of these effects.

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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). The racial gap in test scores also figures 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 explanation for lower black achievement.[1] Consistent with this idea, studies since the Coleman Report (Coleman, 1965) have found that test scores are lower at schools with higher black enrollment shares (see, e.g., Ferguson, 1998, and Hanushek, Kain, and Rivkin, 2002). 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 black achievement is difficult, however, because individuals are not randomly assigned to neighborhoods or schools. A credible research design has to address the possibility that students who attend schools with a larger black enrollment share – or live in predominantly black neighborhoods – are different from those in other schools and neighborhoods, and that these differences contribute to their lower achievement.[2] One approach is to focus on city-wide averages. Assuming that average student ability (conditional on race) is the same in different cities, cross-city comparisons can identify the effects of school or neighborhood composition on student outcomes. Evans, Oates, and Schwab (1992), for example, studied the effects of economically disadvantaged peer groups on teenage pregnancy and dropout behavior, using city-wide averages of income, education, and unemployment as instrumental variables for

[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 an all-black and a mixed-race high school in Cincinnati. She finds no difference between the schools.

[2] On the general problem of inferring peer group effects from observational data, see Manski (1993) and Brock and Durlauf (2001).

the fraction of disadvantaged peers at a student's school.[3]

An important limitation of the cross-city research design is that the demographic composition of a city may be correlated with unobserved characteristics that influence mean outcomes. In this paper we build on an idea of Cutler and Glaeser (1997) and relate the achievement *gap* between black and white students in a city to *differences* in their exposure to black peers in neighborhoods and schools.[4] Within-city comparisons eliminate the effects of city-wide variables that may be correlated with racial segregation, such as the level of school funding or the efficiency of local schools. We also control directly for observed differences in the family background characteristics of white and black students in different cities, and for many other characteristics, including region and racial composition, that may affect black relative performance across cities. We apply this approach to an unusually rich micro data set containing SAT scores for black and white high school students who wrote the test between 1998 and 2001.

Unlike previous studies we attempt to separately identify the effects of school and neighborhood segregation on black relative achievement, using data on school racial composition from the Common Core of Data and the Private School Survey and data on Census tract composition from the 2000 Census. In light of the extremely high correlation across MSAs between segregation at the elementary and secondary levels (above 0.98), we treat secondary-school segregation as a summary statistic for the school conditions that a student experienced over his or her lifetime. Although school segregation is related to the degree of residential segregation in a city, it is also affected by a variety of structural and

[3] Other recent studies that use geographically aggregated outcomes include Hoxby (2000), who examines the effects of variation in the number of school districts on city-wide test scores, and Hsieh and Urquiola (2003), who use area-wide scores to examine the impact of private school competition.

[4] Cutler and Glaeser (1997) focus on the effects of residential segregation on educational attainment and labor market outcomes. We extend their work by distinguishing between neighborhood and school segregation; by controlling for a much larger set of individual and city characteristics that may influence the within-city black-white gap; and by examining test scores as an outcome measure.

institutional features, including the extent of inter-district "choice" in a city and the strength of local desegregation programs. Building on this fact, we test for possible biases in the estimated effect of school segregation (arising from omitted variables or measurement error), using as instrumental variables measures of the structure of local educational governance and of the strength of court-ordered desegregation programs in the 1970s and early 1980s.[5] Finally, we test whether the estimated neighborhood segregation effects reflect race *per se*, or whether racial segregation also proxies for differential exposure to other peer characteristics (e.g. family income) that are correlated with race.

Our analysis leads to two main conclusions. First, although black relative test scores are lower in cities with more highly segregated schools, the effect of school segregation becomes uniformly small and statistically insignificant once we control for neighborhood segregation. Our instrumental variables specifications confirm this pattern, though they are imprecise. Second, neighborhood segregation has robust negative effects on black relative test scores, though these also fall in size and significance once controls are added for the relative exposure of black and white children to neighboring families with differing income, education, and marital status. Thus, as suggested by Wilson (1987), race may not be the primary source of neighborhood segregation effects: rather, racial segregation may proxy for relative exposure to economically successful neighbors.

While SAT scores play a critical role in regulating entry to higher education, not all students write the test. We eliminate the most extreme selection biases by excluding data from states where a majority of college-bound students write the ACT test (Clark, 2003). We also re-weight the observed test scores to adjust for differences in SAT participation

[5] Guryan (2004) shows that the implementation of a major school desegregation program during the 1970s was associated in the short term with a modest but statistically significant 3 percentage point reduction in black dropout rates relative to whites.

rates across schools, and include a selection correction based on average school-level participation rates. To confirm the robustness of our findings, we estimate a series of models using 2000 Census data on employment and schooling outcomes of 16-24 year olds. Estimates from these models show a striking similarity to our SAT results, suggesting that selective test participation is not driving our main conclusions.

The finding that school segregation has little effect on black relative achievement once controls are added for neighborhood segregation leads us to consider three potential mechanisms that might confound the true effect of school desegregation: unobserved differences in school quality, unobserved differences in schoolmate characteristics, and *within-school* segregation (Clotfelter, Ladd, and Vigdor, 2003; Clotfelter, 2004). Using data from the CCD and the Schools and Staffing Survey, we conclude that there is no systematic relationship between school segregation and observable indicators of the relative quality of the schools attended by black students. To evaluate the potential influence of unobserved schoolmate characteristics we use school-level data on the fraction of students who participate in free or reduced price lunch programs. We find that school segregation is highly correlated with black-white relative exposure to low-income schoolmates. To the extent that low-income schoolmates lower achievement, however, this pattern would tend to *reinforce* any causal effect of school segregation, and thus cannot explain our finding of no effect.

Finally, we study within-school segregation patterns using data on the relative participation of black and white students in honors and advanced placement (AP) classes. Holding constant the level of neighborhood segregation, we find that the black-white gap in honors and AP participation is wider in cities with more racially integrated schools. This pattern is consistent with claims that ability tracking and related programs offset the integrative effects of between-school desegregation efforts, and may help to explain why differences in school segregation do not appear to influence black relative achievement.

II. Empirical Framework

a. Basic Model

Our starting point is a model that expresses the test score of a given student as a function of his or her own characteristics, the characteristics of his or her schoolmates and school, the characteristics of his or her neighborhood, the racial composition of the school and neighborhood, and an unobserved error with a school-level component that may vary by race. Specifically, we assume:

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

where y_{ijsc} represents the test score (or some alternative measure of achievement) of student i of race group j in school s and city c, X_{ijsc} is a vector of observed 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_{ijsc} is a vector of the average characteristics of i's neighbors, B_{sc} represents the fraction of black students in school s, R_{ijsc} is the fraction of black residents in student i's neighborhood, u_{jsc} is a shared error component for students of group j in school s and city c, and ε_{ijsc} is an individual-level error (with mean 0 for each race group in each school). Note that the u_{jsc} terms incorporate any unobserved race-specific citywide influences on test scores. Racial segregation effects arise in model (1) through the γ_j and δ_j coefficients, which measure the effects of exposure to black schoolmates or black neighbors on student achievement *holding constant the other characteristics of schoolmates and neighbors*.[6] As we show momentarily, equation (1) implies that the test score *gap* between

[6] If student test scores are affected by the average scores of their peers, a rise in the fraction of black

blacks and whites in a city depends on the degree of racial segregation of schools and neighborhoods and on other contextual factors, such as the gap in average incomes between the neighbors of a typical black student and a typical white student.

In principle equation (1) can be estimated by ordinary least squares (OLS) using student-level data. As noted by Evans, Oates, and Schwab (1992), and Cutler and Glaeser, (1997), however, any non-randomness in the sorting of students to schools or neighborhoods is likely to bias the resulting estimates of γ_j and δ_j. To eliminate this bias we aggregate achievement outcomes by race group to the city level and then difference between blacks and whites. Specifically, equation (1) implies that the mean outcome of race group j in city c is:

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

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 black students at schools attended by race group j in city c, R_{jc} is the average fraction of black neighbors of students in group j in city c, and u_{jc} is the mean "unobserved ability" of students of race j in city c. The difference in mean outcomes between black and white students in city c is then given by:

$$(2) \qquad y_{1c} - y_{2c} = X_{1c}\alpha_1 - X_{2c}\alpha_2 + Z_{1c}\beta_1 - Z_{2c}\beta_2 + W_{1c}\varphi_1 - W_{2c}\varphi_2 + B_{1c}\gamma_1 - B_{2c}\gamma_2 + R_{1c}\delta_1 - R_{2c}\delta_2 + u_{1c} - u_{2c},$$

where j=1 represents blacks and j=2 represents whites.

If the coefficients in equation (1) are the same for whites and blacks then equation

schoolmates or neighbors will tend to lower test scores, since black students have lower average test scores than whites. Alternatively, if black students value academic achievement less than whites (Ogbu and Forham, 1986; Ogbu, 2003), and if individual student performance is affected by peer norms, a rise in the fraction of black classmates or black neighbors will tend to lower achievement. See also Austen-Smith and Fryer (2003), who present a model of "acting white" that implies a nonlinear effect of racial composition on black outcomes.

(2) takes a particularly simple form:

(2') $\Delta y_c = \Delta X_c \alpha + \Delta Z_c \beta + \Delta W_c \varphi + \Delta B_c \gamma + \Delta R_c \delta + \Delta u_c$,

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 summarize the racial segregation of schools and neighborhoods in a city, and are closely related to standard segregation indexes.[7] In particular, full racial segregation implies that $B_{1c} = R_{1c} = 1$ and $B_{2c} = R_{2c} = 0$, leading to values for ΔB_c and ΔR_c of 1. At the other extreme, complete racial integration implies that $B_{1c} = B_{2c}$, $R_{1c} = R_{2c}$, and $\Delta B_c = \Delta R_c = 0$. The differences ΔZ_c and ΔW_c measure other potentially important differences in the characteristics of the schoolmates and neighbors of black and white children. For example, if W includes the average family income in a neighborhood, then ΔW_c includes the difference between the average incomes in the neighborhoods of black and white children.

The strategy of aggregating and differencing between races eliminates any bias caused by the endogenous sorting of students to schools within a given city, and also eliminates any city-wide variables that affect the two race groups equally. Nevertheless, there may be remaining differences between the black and white students in a city. We posit that the unobserved ability gap can be decomposed as:

(3) $u_{1c} - u_{2c} = F_c \psi + v_c$,

where F_c is a vector of city characteristics (including region dummies, the mean and dispersion in family income in a city, and the overall fraction of black students in the city) and v_c represents all remaining unobserved differences between black and white students in city c. Assuming that the differenced specification (2') is valid, this leads to a model of the

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[7] In the segregation literature (e.g. Massey and Denton 1988; Iceland, Weinberg et al. 2002), B_{jc} and R_{jc} are known as indices of exposure of race-j students to blacks, and ΔB_c and ΔR_c (or versions of them that scale by the city fraction black—see Cutler, Glaeser and Vigdor 1999) are sometimes known as isolation indices. We do not rescale by the city fraction black (B_c), but instead control for it separately.

form:

$$(4) \qquad \Delta y_c = \Delta X_c \alpha + \Delta Z_c \beta + \Delta W_c \varphi + \Delta B_c \gamma + \Delta R_c \delta + F_c \psi + v_c .$$

OLS estimation of this equation will yield consistent estimates of γ and δ if v_c, the unexplained difference in black-white test outcomes, is uncorrelated with ΔB_c and ΔR_c conditional on the other control variables included in (4).

Consideration of equation (4) suggests a number of possible biases that could affect the estimates of γ and δ. One is non-random sorting of black and white families to different metropolitan areas. For examples, 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 v_c may be negatively correlated with ΔB_c and/or ΔR_c. As a partial control for this problem, we include in F_c an estimate of the difference in the mean residual wage gap between black and white parents in city c. To the extent that children's academic achievement is related to the same unmeasured factors that determine their parents' labor market success, the mean residual wage gap controls for non-random sorting of black and white families to different cities.[8]

Another potential source of bias arises if the degree of racial segregation in a city's schools is endogenous with respect to the ability gap between black and white students in the city. To assess this possibility we consider two instruments for school segregation: a measure of the concentration of students in larger school districts in an MSA (which influences segregation because districts often try to equalize the fraction of minorities across schools, but there are almost no interdistrict desegregation programs), and an estimate of the "bite" of Court-ordered school desegregation programs in the city in the 1970s and early

[8] We have also used data on migration flows for blacks and whites in the National Longitudinal Survey of Youth (NLSY) to examine the impacts of racial segregation on relative migration patterns of blacks and whites with differing AFQT scores. These data show no indication of selective migration flows.

1980s.

A third specification issue is the presence of unobserved differences in the quality of the schools attended by black and white students. Specifically, if school quality has a positive effect on achievement, and if black schools are of lower relative quality in more segregated cities, specifications that ignore school quality will lead to negatively biased estimates of the school segregation effect. The omission of school quality may also lead to some bias in the neighborhood segregation effect. In specifications that include both segregation measures, however, we would expect most of the bias to be concentrated on the school segregation effect. In section V, below, we confirm this conjecture by examining data on observed indicators of school quality.

A similar argument applies to the effect of omitted neighborhood characteristics. It seems plausible—and we confirm empirically below—that the gap in measures of neighborhood quality between blacks and whites in a city is highly correlated with the degree of racial segregation of its neighborhoods. Specifications that omit key neighborhood characteristics, then, will lead to measured neighborhood segregation effects that incorporate both the direct effect of exposure to black neighborhoods as well as the effect of any gap in the quality of neighborhoods where black and white families reside.

b. Controlling for Student-Level Covariates

The aggregated model (4) has only as many degrees of freedom as the number of metropolitan areas in the sample, limiting the flexibility with which we can control for family background factors. To fully exploit our rich underlying 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 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:

$$\text{(5)} \qquad r_{1c} - r_{2c} = \Delta X'_c \alpha + \Delta Z_c \beta + \Delta W_c \varphi + \Delta B_c \gamma + \Delta R_c \delta + F_c \psi + v_c + e_{1c} - e_{2c},$$

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 $\hat{f}$, linear measures from the SAT data, and several background variables that are not observed in the SAT files but can be constructed from census data). Although the first stage adjustment may not fully eliminate the effect of the X^1 variables, we anticipate that the inclusion of $\Delta X'_c$ in the second stage model absorbs most of the remaining variation in Δe_c.

c. Adjusting For Selective Participation in the SAT

Although we only use data for cites in states where a majority of college-bound students write the SAT (rather than the alternative ACT), there is still substantial variation in citywide test participation rates. Presumably, students at "low performing" schools are under-represented in the test-taking population, with greater under-representation in cities with lower overall participation.[9] Such a tendency will lead to attenuation in the measured

[9] 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 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.

effects of variables that influence scores, like school or neighborhood segregation (Gronau, 1974; Heckman, 1978). 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 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, 1978). 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:

$$\Delta r_c = \Delta X'_c \alpha + \Delta Z_c \beta + \Delta W_c \varphi + \Delta B_c \gamma + \Delta R_c \delta + F_c \psi + \zeta \Delta\lambda_c + \zeta \Delta\theta_c + v_c + \Delta e_c . \tag{6}$$

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 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 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, then the control function $\Delta\lambda_c$ would fully correct for selectivity biases in the observed test scores. More generally, however, the set of test takers at each high school is non-random, and the control function only adjusts for the "between school" component of selectivity bias, while the within-school component remains in the error term of equation (6). 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.

A second problem caused by selective test participation is that we cannot use the SAT data to estimate the average characteristics of the students at each school (i.e., Z_{1c} and Z_{2c}). To the extent that the relevant peer group for the SAT-takers in a school is the set of SAT-takers of the same race in that school, Z_{jc} will be well-measured by X^1_{jc}, and we can reinterpret the measured effect of the observed student-level characteristics as representing a combination of "own" and peer group effects (i.e., as representing $\alpha + \beta$). If the relevant peer groups include non-test-takers, however, ΔX^1_c may not fully control for differences in schoolmate characteristics of black and white test takers, leading to biases in the estimated segregation effects. In section III, below, we examine one available measure of school-wide student characteristics – the fraction receiving free or reduced price lunches – and find that relative exposure to schoolmates receiving subsidized lunches is highly correlated with relative exposure to black schoolmates. Assuming that low-income peers depress performance, omission of this measure from our primary equation will thus lead to a negatively biased estimate of the direct effect of segregation on black relative outcomes.

III. Data Sources and Sample Overview

a. Data Sources

Our primary source of student achievement data is a sample of SAT records for 25% of white test takers and 100% of black test-takers in the 1998-2001 SAT test cohorts.[10] These files include self-reported demographic and family background information as well as

[10] We also have and use observations on 100% of white test takers in California and Texas. We use sampling weights in all computations of test score or student characteristic 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.

high school identifiers, which we use to match school-level information 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 in enrollment counts in the CCD 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. We assign students to Metropolitan Statistical Areas (MSAs) based on year-2000 definitions, using school location information in the CCD and PSS files.[11] As noted earlier, we 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."

Using the SAT microdata, we first estimate race-specific models relating test scores to three key family background variables -- mother's education, father's education, and income.[12] We then form enrollment-weighted average of the residual scores for black and white students from the high schools in each city. Finally, we construct the difference in the black-white residual test score for each city.

We construct estimates of the average fraction of black schoolmates for black and white high school students in each MSA using school-level data from the CCD and PSS.[13] We also compute a similar measure of the relative exposure of black and white students to Hispanic schoolmates.[14]

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[11] 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.

[12] 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.

[13] 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 school segregation measures computed over the relevant schools and grade levels.

[14] 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.

We use data from the 2000 Census for several purposes. First, we construct parallel measures of neighborhood-level exposure to black and Hispanic neighbors, using Census tracts as the unit of exposure and drawing tract-level population counts by race and ethnicity from the 2000 Census full population counts (Summary File 1).[15] Second, we construct estimates of the average family background characteristics of black and white students in each city, using the Summary Files (based on the full 1-in-6 set of respondents who fill out the Census long form) where possible and in other cases (e.g. parental education) by matching youth to their parents in the 5-percent public use samples (PUMS). Finally, for some supplementary analyses, we use the PUMS data to construct alternative measures of black-white gaps in academic outcomes (such as high school completion) that are not subject to biases that selectivity into SAT-taking may introduce.

Further details on our data sources and merging methods are presented in a Data Appendix, available on request.

b. Overview of Sample

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 three columns of the table show the absolute and relative levels of exposure of black and white residents in each city to black neighbors, while the next three columns show parallel measures of within-school exposure to black schoolmates. Finally, the three right-hand columns show average SAT scores for black and white test-takers in the city, and the racial gap in scores. Test scores are only reported for MSA's in "SAT states."

[15] Census tracts are initially defined to encompass demographically homogenous neighborhoods of about 4,000 residents, but once drawn generally have stable boundaries. We also construct exposure measures based on Census Block Groups (which have typical populations of about 1000 residents). These are nearly perfectly correlated across cities with the tract-based measures and lead to virtually identical estimates.

The upper panel of the table presents data for the five cities that have the lowest level of residential segregation, and the five cities with the highest level of residential segregation.[16] The low-segregation cities are all in the South. Even in these relatively integrated cities blacks are unevenly distributed across Census tracts, with at least an eight percentage point gap between the fraction of blacks in the tract of a typical black resident and that of a typical white resident, and a similar gap between the fractions of black schoolmates at schools attended by black and white students. The most highly segregated cities are all in the Midwest, and also have similar degrees of residential and school segregation.

The lower panel of Table 1 presents data for the cities with the biggest divergence between the level of residential segregation and the level of school segregation. It is this divergence that enables us to identify the separate effects of residential and school segregation. We define the degree of divergence as the residual from a regression of our measure of school segregation (ΔB_c) on our measure of neighborhood segregation (ΔR_c). We list first the five cities with the least segregated schools relative to neighborhoods (i.e. with the largest negative residuals) and then the five with the most segregated schools relative to neighborhoods (i.e. with the largest positive residuals). The former are all in the Southeast, while the latter are scattered more widely.[17] Note, however, the substantial overlap between the residential segregation measures in the two groups. It is also interesting to compare the black-white test score gaps in the various groups of cities: the average gap is

[16] We restrict the sample for this table to cities with at least a 10% black population share, as our segregation measures are bounded above by the black share in the city.
[17] The median MSA has nine school districts serving secondary grades, but four of the five MSAs that have the least segregated schools relative to neighborhoods contain two or fewer districts each. By contrast, only one of the five MSAs with the most segregated schools has this few districts. Below, we use the systematic relationship between the number of districts and the extent of across-school segregation, which is almost certainly related to school desegregation jurisprudence, as a source of systematic, exogenous variation in school segregation.

smallest in the least residentially segregated cities (mean gap = -166 for 4 cities) and roughly comparable in the other groups (mean gap = -189 for the cities with the least segregated schools relative to neighborhoods, and -198 for the cities with the most segregated schools relative to neighborhoods).

Table 2 presents some comparisons between the students in all 331 MSAs (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). Blacks are slightly under-represented in the SAT state cities (11% of the student population versus 12% overall) whereas Hispanics are over-represented (25% of students versus 21% overall).[18] Cities from SAT states also have slightly lower rates of racial segregation at both the neighborhood and high school levels. 43 percent of white high school students and 31 percent of black high school students from cities in the SAT states write the SAT.

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 2004), but the black-white difference is very similar for cities in SAT and non-SAT states, suggesting that use of within-city differences may moderate problems associated with selective test participation.

As a final descriptive exercise, Figures 1, 2 and 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 relative segregation of schools that is orthogonal to the relative segregation of

[18] 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.

neighborhoods (Figure 3). The scatter of points in the first two graphs suggest a strong negative relationship between either measure of racial segregation and the relative test scores of black students. [19] Interestingly, however, there is no correlation between the test score gap and the component of school segregation that is orthogonal to neighborhood segregation.

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

a. Basic Models

We turn to the task of estimating equation (10') using city-level data for MSAs in high SAT participation states. Table 3 presents an initial set of estimates that exclude neighborhood-level segregation effects. All the models include the relative exposure of blacks and whites to black schoolmates and a parallel term representing relative exposure to Hispanic schoolmates, as well as main effects for the overall fraction black and Hispanic in the city's schools. All models—here and throughout the paper—also include 11 basic city-level control variables (log of city population, log of city land area, the fractions of city residents with 13-15 and 16+ years of education, log mean household income, the Gini coefficient of household income in the city, and dummies for 5 Census divisions[20]).

The models in columns A and B explore the effects of school segregation on SAT participation rates. Column A presents a baseline specification that includes only the basic city-level controls, with no additional student background characteristics. This specification shows a significant negative effect of relative exposure to black schoolmates on the black-white gap in test participation (row 1). The effect of relative exposure to Hispanic

[19] The 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.

[20] Although there are nine Census divisions, only six are represented among SAT states. In Table 3 and the remainder of the paper, we restrict the sample to 185 cities (out of 189 in SAT states) for which we can construct black-white differences in family background characteristics, introduced in Column B, using the 2000 Census microdata sample.

schoolmates appears to be of similar magnitude, but is less precisely estimated and insignificant (row 2). The effect of the overall fraction of black students in a city (row 3) is small and insignificantly different from zero, indicating that black-white participation gaps do not vary substantially with the average racial composition of schools, once *relative* exposure to black schoolmates is held constant. The fraction Hispanic main effect (row 4) does appear to be significant, however, indicating that blacks are relatively more likely to write the SAT in cities with more Hispanics.

The model in column B adds a set of additional controls for the black-white gaps in several family background variables, computed from 2000 Census data.[21] These background variables are jointly significant, and their addition substantially reduces the size of the estimated school segregation effects. Evidently, most of the apparent correlation between school segregation and relative SAT participation is attributable to differences in the relative family background characteristics of black and white students in different cities. Once these are taken into account, there is little evidence that relative participation depends on relative school segregation, suggesting that selection biases in the relation between the test score gap and school segregation may be relatively minor.

Columns C-G present models for the gaps in adjusted test scores. Column C repeats the specification from Column A but adds a control for the black-white gap in the mean inverse Mills ratio (averaged across schools in the city) to absorb the between-school component of selection bias. The estimated selection coefficient is negative and significant, consistent with the hypothesis of positive selection into test-taking. The coefficient on relative exposure to black schoolmates is also negative and is reasonably precisely estimated: Consistent with the simple scatterplot in Figure 2, higher relative exposure of black students

[21] 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.

to black schoolmates (i.e. more segregation) is associated with lower black relative scores. The effect of relative exposure to Hispanic students is somewhat smaller, though still significant (and not significantly different from the black exposure effect), while the "main effects" of the overall fractions of black and Hispanic students in a city are small and insignificantly different from zero.

Columns D, E, and F add additional controls for black-white gaps in observable background characteristics, estimated from the SAT samples and 2000 Census data. The model in column D adds $\Delta\hat{f}_c$, our simple one-dimensional summary of the relative parental education and income differences between black and white test takers in different cities. This variable has a significant positive effect, and leads to a 20% reduction in the size of the estimated black exposure effect. Its addition also reduces the size of the coefficient on the inverse Mills ratio term. The coefficient on the differenced background index is surprisingly large in magnitude, considering that that the dependent variable is already adjusted to remove the effects of individual test-takers' background characteristics. The index is measured in SAT points, so the 1.35 coefficient in column D implies that a 10 point widening in family background characteristics between the black and white high school students in a city increases the gap in *adjusted* test scores between black and white test takers by 13.5 points, and the gap in *actual* test scores by 23.5 points. Taken literally, this indicates that peer characteristics have a larger effect on a student's test scores than does his or her own family background. More plausibly, the coefficients used to form $\hat{f}$ may be attenuated, with the city average capturing some of the individual-level variation.

Column E shows that augmenting the model with estimates of the black-white differences in family background characteristics from the Census reduces the size of the estimated school segregation effects by nearly 50 percent (and also reduces the size of the coefficient of the relative family background index). Interestingly, once the Census controls are added the effect of our selection correction term switches sign, though it is not significantly different from zero. Finally, in column F we add some additional controls for the differences in the relative background characteristics of SAT takers, loosening the restriction implicit in the use of the estimated background index that these variables have the same relative effects across MSAs as they have within schools. Not surprisingly, this addition substantially reduces the precision of the background index's coefficient. It also reduces the magnitude of the estimated school segregation effects, so that relative exposure to black and Hispanic schoolmates is no longer statistically significant. We suspect that the model in column F is over-fit, since it includes 31 highly collinear explanatory variables in a model with only 185 observations. Moreover, the effects of some of the background variables are large and seemingly "wrong signed." For example, the large negative effects of the gaps in the fraction of fathers with a BA and mothers with some college measured from the SAT data seem implausible.

b. Estimating the Separate Effects of School and Residential Segregation

The next step is to augment the models in Table 3 with measures of neighborhood segregation. Column A of Table 4 presents a model for relative SAT participation that includes the same controls as the model in column B of Table 3, along with two additional variables representing the relative exposure of black residents to black and Hispanic neighbors. (For simplicity we do not report the effects of the student background variables) The neighborhood segregation effects are both negative, while their addition causes the estimated effects of exposure to black or Hispanic schoolmates to become positive. Interestingly, the Hispanic exposure effects are much larger than the black effects.

Column B extends the model in column A by including two additional city-level control variables: the black-white gaps in the mean wage residuals of mothers and fathers in the city.[22] The residual wage of fathers has a positive effect on SAT participation (consistent with the hypothesis that residual wages reflect unobserved ability, and that children of higher ability parents are more likely to write the SAT), though the effect of mothers' residual wages is essentially zero. More importantly, the addition of these extra controls has no noticeable effect on the size or precision of the estimated segregation effects.

Column C presents a final model for SAT participation in which we impose the assumption that it is relative exposure to *minorities* (blacks or Hispanics) at the school or neighborhood level that affects the relative test participation rate of black students, constraining the effects of the two types of peers to be identical. This specification leads to minority exposure effects that are roughly a weighted average of the black and Hispanic relative exposure effects in the more general model, with more of the weight on the black exposure effects. Like the more general model, this specification suggests that relative exposure to minority schoolmates has a weak positive effect on relative participation, whereas relative exposure to minority neighbors has a stronger (marginally significant) negative effect on participation rates.

Columns D and E of Table 4 present models for adjusted SAT scores that include the same control variables as the models in Columns E and F of Table 3. In both cases, the addition of the residential segregation measures causes the estimated effect of exposure to black schoolmates to fall to nearly zero, and causes the estimated effect of Hispanic

[22] The residual wage is computed as the MSA fixed effect in a regression of wages on years of education, indicators for high school dropout and college graduation, and a cubic in potential experience. The regression is computed separately for each race and for each gender, on samples of adults with resident children aged 0-18.

schoolmates to become positive (but insignificant). In contrast, the neighborhood segregation effects are large, statistically significant, and relatively stable whether SAT-based background controls are excluded (Column D) or included (Column E). Column F extends the specification in column E by adding the residual wage gaps between black and white mothers and fathers in each MSA. The father's wage gap variable is statistically significant but has the "wrong sign,", perhaps reflecting a relationship with selection into SAT-taking, as indicated in column B. In any case, the inclusion of the wage gap variables has essentially no effect on the estimated segregation effects. Finally, in column G we report a model similar to the one in column F but restricting the relative exposure effects for blacks and Hispanics to be equal. Again, the estimated minority exposure effects lie between the estimated effects for exposure to blacks or Hispanics, but closer to the black effects. The restricted model suggests that exposure to minority schoolmates has no effect on the relative test scores of blacks, while exposure to minority neighbors has a strong negative effect.

We have estimated a wide variety of alternative specifications to probe the robustness of the conclusion that school segregation has little or no effect on relative test scores controlling for neighborhood segregation. 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 4. In a second check, we add an additional measure of relative school segregation in the elementary schools in each city, with the idea that this may measure school-level peers during students' formative years better than does the high school segregation measure. When the highly collinear (correlation 0.98) measures of relative exposure to minority classmates in both high schools and elementary schools are included together, both have very small but quite imprecise coefficient estimates.

When only the elementary school segregation measure is included, it has a coefficient of 8.0 (standard error 29.1) – very similar to the coefficient estimate on relative exposure to minority schoolmates at the high school level in the model in column G of Table 4.

Finally, we estimated models that allow the effects of minority exposure to differ for black and white students. Specifically, we estimated a model with separate coefficients on the fractions of minority students in the average white and black students' schools, and on the fractions of minority neighbors in the average white and black residents' census tracts. This model (reported in column I of Appendix Table 1) yields estimated effects of exposure to minority schoolmates for blacks and whites that are both very close to 0 (6.7 and -6.6, respectively), an estimated effect of exposure to minority neighbors for black students that is negative and significant (-97.7 with a standard error of 30.4), and an estimated effect of exposure to minority neighbors for white students that is also negative but somewhat imprecise (-45.2 with a standard error of 91.5).[23] We obviously 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.

c. Are the Segregation Effects Causal?

Table 4 runs a "horse race" between school and residential segregation, and the residential measure wins handily. One interpretation of this finding is that school segregation has no causal effect, once the level of neighborhood segregation is taken into account. An alternative is that the estimated school or neighborhood segregation effects are biased relative to the true causal relationship by mismeasurement of school segregation or by omitted factors that influence both school segregation and black-white test score gaps. To

[23] The dependent variable is the black-white adjusted test score gap, so in the models with separate effects of minority exposure on black and white students the sign of the coefficient on white exposure is reversed.

assess this possibility, this section presents two instrumental variables (IV) estimates that isolate arguably exogenous components of school segregation.[24] For simplicity, we collapse the separate black and Hispanic exposure indices into measures of exposure to minority schoolmates or neighbors, as in Column G of Table 4.

The first source of exogenous variation that we consider is the structure of local governance. MSAs vary widely in the typical size of school districts. Although many school districts operate programs to reduce the variation in racial and ethnic composition across high schools, there are few such inter-district programs. As a result, one would expect greater school segregation in metropolitan areas with more, smaller school districts, controlling for the degree of neighborhood segregation. We construct an index of the fragmentation of district governance over secondary schools (Hoxby 2005; Rothstein 2004; Urquiola 1999). This index, which takes on its minimum value of zero in an MSA with just a single district and approaches its theoretical maximum of one as the size of an MSAs largest district approaches zero, is our first instrument for school segregation.[25]

Our second source of variation comes from differences across cities in the "bite" of court-ordered school desegregation programs implemented in the 1970s and early 1980s in many U.S. cities. We use Welch and Light's (1987) estimate of the change in the "dissimilarity index"—an alternative index of racial segregation—for a city's schools between the year prior to the city's major desegregation plan and the last year of

[24] Each of our IV models treats residential segregation as exogenous and instruments only for the school measure. We have also examined the residential segregation measure. It is extremely stable over time, and models in which we instrument for residential segregation with a similar measure computed from 1980 data are nearly identical to those obtained by OLS.

[25] Hoxby (2000) has argued that fragmentation is correlated with school productivity. To the extent that any such productivity differentials raise black and white test scores equally, they are eliminated by our differencing strategy. In any case, Rothstein's (2005) re-analysis of Hoxby's data suggests that the evidence for such a correlation is weak.

implementation.[26] This variable is available for only 60 MSAs in our sample, and we use a more parsimonious specification for this analysis.

IV models using these instruments for school segregation (while controlling for neighborhood segregation) are reported in Panel B of Table 5, with first stage estimates in Panel A. As a point of departure the model in column A shows a baseline OLS specification. Column B presents an IV model using the choice index as an instrument. The first stage is highly significant, though somewhat weaker than ideal. The IV estimate, in the second panel, is extremely imprecise, but the point estimate is even larger (more positive) than the OLS estimate, providing no evidence of an upward bias in the baseline OLS estimate.

Columns C and D repeat the exercise using the desegregation instrument. Column C presents the OLS estimate for the subsample of cities with court order data. Given the small sample size, we adopt a relatively parsimonious model similar to the one in column E of Table 3 (though we omit a few of the Census background controls, as detailed in the table notes). The OLS estimates for this specification in the subsample are quite similar to the full-specification estimates from our full sample, but relatively imprecise. The first stage estimate (upper panel, column D) shows that the court orders continue to have notable effects on observed measures of school segregation, even after two decades or more. The IV estimates in the lower panel are again imprecise, but give no indication that bias in the OLS estimates masks an underlying negative effect of school segregation.

d. Selection into SAT-taking

[26] This variable is set to zero for cities without a major desegregation plan. The Welch and Light measure pertains to districts, rather than to MSAs; we multiply their measure by the share of metropolitan enrollment that is in the relevant districts. MSAs with no districts in the Welch and Light sample are excluded.

A potential concern with the results so far is that despite our efforts to adjust for selection biases—restricting the sample to cities in high-SAT-participation states, re-weighting the data from different high schools to offset differences in school-level participation, and controlling for the average inverse Mill's ratio terms associated with each high school—the models may be biased by selective participation, leading to a faulty conclusion about the relative effects of school and neighborhood segregation. To probe the robustness of our results, Table 6 presents estimates of our basic (OLS) specifications that omit the Mill's ratio control function and that do not use our re-weighting procedure in the computation of city-level adjusted test score gaps. Beyond these differences, specifications A and B of Table 6 are identical to the ones in column E of Tables 3 and 4, respectively, while the model in column C of Table 6 restricts the black and Hispanic exposure effects to be equal. As in the previous models, the simpler unadjusted models show a significant effect of relative exposure to black schoolmates that falls in size and becomes statistically insignificant once we control for residential segregation. The residential segregation coefficients from the unadjusted models are negative, as in the adjusted models, but somewhat smaller in absolute value, perhaps reflecting the impact of attenuation biases arising from selective participation.[27]

A second and arguably more persuasive way to evaluate the impact of selective test participation is to examine models for black-white relative achievement based on outcomes for a random sample of youths. We use the 2000 Census 5-percent micro samples to construct two outcome measures for 16-24 year olds in each city: the fraction either employed or in school (an indicator of gainful activity), and the fraction who either are

[27] We have also explored other forms of correction, 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.

currently enrolled or have completed high school (an indicator of education attainment). A limitation of the Census data is that there is no family background information for children who are no longer living with their parents.[28] Consequently, we make no individual-level adjustments for family background. Instead, we regress the black-white difference for each outcome measure on our school and residential segregation measures and the same Census-based family background measures used in Tables 3-6.

Table 7 presents a series of models for each of the two outcomes, fit to a sample of 234 MSAs with at least 50 students of each race in the PUMS samples. The models in the upper panel look at the black-white gap in the fraction of youths who are employed or in school, while the lower panel presents models for the gap in the fraction who are enrolled or have obtained a high school degree. The models in columns A-C include only our school segregation measures, while the models in columns D-F include school and neighborhood segregation measures. Within each group, we begin with specifications that include no other controls (columns A and D), then add the basic city controls included in all our previous models (columns B and E), and finally add Census-based controls for family background differences between blacks and whites in each city (columns C and F).

Beginning with the models in columns A and B, note that with no controls, or only a limited set of city controls, there appears to be a negative relationship between black youths' relative outcomes in a city and their relative exposure to black (and perhaps Hispanic) schoolmates. As shown in the parallel models in columns E and F, however, adding controls for residential segregation essentially eliminates the effect of relative exposure to black schoolmates, and suggests instead that relative exposure to black neighbors is a key determinant of black youth's relative outcomes. These results are remarkably similar to our

[28] 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.

findings for black relative test scores, and suggest that the test score findings cannot be attributed to statistical problems arising from selective SAT participation.

Examination of the models in columns C and F suggests that inferences about the effects of relative segregation on employment or educational attainment are sensitive to the set of background control variables added to the model.[29] In particular, once the relative background variables we use in Tables 3-6 are added, the estimated impacts of school segregation on its own, or school and neighborhood segregation taken together, fall in magnitude and become insignificant. By contrast, the models in Table 4 show robust negative effects of relative exposure to minority neighbors on black-white relative test scores. One plausible explanation for the difference is that neighborhood segregation has smaller effects on basic achievement outcomes (like being in gainful activity or 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 to say anything conclusive about this.

V. Possible Confounders of School-level Segregation Effects

The results in Tables 4-7 suggest that relative exposure to black neighbors has a negative impact on black relative achievement, whereas relative exposure to black schoolmates has little or no effect. In this section, we explore three explanations for the somewhat puzzling lack of any school-level exposure effect. The first is that the relative quality of schools attended by black students is correlated with neighborhood and school segregation in such a way as to offset a true negative effect of school racial composition. A second is that unobserved differences in school-level peer characteristics bias the effects of

[29] The controls in Column E are similar to those used by Cutler and Glaeser (1997), who find large and significant effects of residential segregation on black relative outcomes in a somewhat different specification.

schoolmates' racial composition. Third, classroom peer groups may be what matter for student performance, and cities with more aggressive school integration programs may also have more segregation within schools.[30] We investigate each of these hypotheses in turn.

a. Relative School Quality

Our first candidate explanation for the contrasting effects of neighborhood and school segregation is that either or both types of segregation may be related to the gap in school quality between the schools attended by the black and white students in a city. Unfortunately, there are few sources of information on school quality that can be used to assess this hypothesis. The only school-level measure that is universally available (from the Common Core of Data) is the number of full-time-equivalent (FTE) teachers. We use this source to compute the number of teachers per student at public schools attended by white and black students in each MSA. Measures of spending are available only at the district level. Using the CCD Local Education Agency Finance Survey (also known as the F-33 portion of the Census of Governments) we compute expenditures per pupil in districts attended by white and black students in each MSA.[31] For information on other dimensions of school quality we turn to the Schools and Staffing Survey (SASS), which has information on the qualifications, experience, and characteristics of a national sample of teachers that can be matched to the racial composition of the schools in which they teach.

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[30] This mechanism is consistent with anecdotal evidence suggesting that districts with stronger desegregation programs often create special programs to attract white students to high-minority schools (Clotfelter, Ladd et al. 2003; Clotfelter 2004; Eyler, Cook et al. 1983), and that these programs may reduce exposure. For example, 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)

[31] Unfortunately, if resource allocations are not equal across schools in each district, this is an imperfect measure of the actual spending at black and white students' schools.

Table 8 presents models with the same explanatory variables as were used to explain black-white test score gaps in Table 4, including city-level controls and black-white differences in family background characteristics. In the first two columns, the dependent variables are the black-white gap in per pupil expenditures and in teachers per pupil in the MSA. It appears from these columns that there is little relationship between segregation and spending, but that school and neighborhood segregation have some impact on class size. Specifically, residential segregation is associated with larger classes at black students' schools (though this is only significant for the Hispanic exposure measure), while school segregation is associated with smaller classes for blacks. To the extent that smaller classes (i.e., a higher ratio of teachers to students) raise achievement, these findings may help explain our estimated school and neighborhood segregation effects, since they suggest that black students have lower quality schooling in cities with more segregated neighborhoods but higher quality schools in cities with more segregated schools. It should be noted, however, that the size of the effect is modest: the coefficient of 1.15 in the first row of column B implies that moving from fully integrated to fully segregated schools would raise the teacher-pupil ratio by 0.01, implying a class size reduction of about 15% at the mean. Most recent studies suggest the effect of such a change would be modest.[32] Moreover, we suspect some of the "effect" of relative segregation on class size may reflect black students' disproportionate likelihood of being assigned to special education programs (Donovan and Cross, 2002), which have smaller classes but would not be expected to raise SAT scores.

Columns C-F of Table 8 report similar models for gaps in average teacher characteristics, estimated from the SASS, between schools attended by black and white

[32] For example, Krueger (1999) finds that the STAR experiment, which raised teachers per pupil by about 40-50 percent, had an effect on third grade test scores of about 0.2 standard deviations. Using the effect size in the text, Krueger's estimate would imply an effect of school segregation on SAT scores of under 20 points. This might be understated somewhat, given Krueger and Whitmore's (2002) conclusion that black students are more sensitive to class size than are whites, but 40 points seems like a reasonable upper bound.

students. The model in column C 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. The models for the gap in average salaries and experience between the teachers of black and white students (columns D and E) are relatively noisy but show no significant segregation effects. Finally, the model in column F shows that both school and neighborhood segregation lead to increases in the relative fraction of black students' teachers who have undergraduate degrees in education, with a larger effect for neighborhood segregation. Assuming that the fraction of teachers with an education major is a negative quality indicator, this could result in some overstatement of the relative causal effect of neighborhood segregation, though we suspect any such effect is small. Overall, we interpret the results in columns A-F of Table 8 as suggesting that unmeasured school quality effects are an unlikely explanation for our finding that neighborhood segregation affects black relative achievement while school segregation does not.

b. Unmeasured Schoolmate Characteristics

As we noted in Section II, our data sources do not allow us to estimate the difference between the average characteristics—other than racial composition—of school-level peer groups of black and white students in a city. Our suspicion is that omission of other characteristics will tend to lead to an *overstatement* of the negative effect of exposure to minority schoolmates, since schools with more black students tend to have more students with disadvantaged family backgrounds. To provide some evidence on this conjecture, we used data from the CCD to estimate the black-white gap in the average fraction of schoolmates receiving free or reduced price lunches. Column G of Table 8 presents coefficient estimates from a model relating this measure of relative peer group economic status to our measures of school and neighborhood segregation.[33] As we expected, there is a strong positive correlation with exposure to both black and Hispanic schoolmates. On the other hand, there is essentially no relationship with residential segregation. To the extent that the presence of lower-income peers depresses academic achievement, these findings suggest that the absence of data on the characteristics of school-based peers would, if anything, lead us to find an effect of school segregation, but would not lead to any magnification of the effect of neighborhood segregation. Thus, missing data on school-level peers seems like an unlikely explanation for our main findings.

c. Across-school segregation and within-school exposure

Our final candidate explanation is that student achievement is primarily affected by classroom-level rather than school-level peers, and that variation across cities in the relative exposure of black and white students to black *schoolmates* (conditional on residential segregation) is only weakly correlated with the relative exposure to black *classmates*. This would lead us to find little effect of non-residential school segregation, as it would have little signal for the classroom-level segregation that would be the relevant measure. This hypothesis is difficult to test directly, as there are no national data on the racial composition of high school classrooms. We therefore focus on an indirect test, based on the covariance between the prevalence of ability tracking and the residential and non-residential components of school segregation.

We use data on course enrollment patterns from the SAT data set to measure ability

[33] We do not include the free lunch measure as a control in our SAT score models because we suspect that it is a less reliable proxy for school poverty at the secondary than at the elementary level, as take-up rates are lower among older students. In Table 8, we measure free lunch rates over all grades.

tracking. Specifically, 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. Columns A and B of Table 9 present models for the fraction of students who indicated that they had taken honors courses in math or English, respectively, while Columns C and D present models for the fraction of students who intended to claim college-level credit in any subject (column C) or in math or English (column D). As in earlier tables, we combine our measures of exposure to blacks and Hispanics into indexes of exposure to minority schoolmates or neighbors, though estimates from models that separate the two groups are very similar.

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 or neighborhood segregation and black course-taking. The estimates in Panel B, by comparison, show relatively strong negative impacts of 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 the relative exposure of blacks to minority schoolmates 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 also tend to have positive effects, although the coefficients are smaller and uniformly insignificant.

Though relative participation in honors and AP courses is a limited measure of within-school segregation, the results in Table 9 seem to offer fairly strong support for the within-school segregation hypothesis. In particular, 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.[34]

VI. Do Neighborhood Segregation Effects Reflect Race or Other Factors?

As a final interpretative exercise, we examine the source of the neighborhood "peer effects" that are implied by our estimates: Are black neighbors inherently bad for student performance, or do our results reflect other neighborhood characteristics that are correlated with race? To explore this, we add to our basic specification controls for the relative exposure of white and black residents to alternative neighborhood characteristics, such as the poverty rate. Without these controls, the measured effect of relative exposure to black or Hispanic neighbors presumably combines the direct effect of racial composition with the effects of other relative neighborhood quality variables that are correlated with racial composition.

Table 10 reports the results. Again, for simplicity we have combined relative exposure to black and Hispanic schoolmates or neighbors into measures of exposure to minority schoolmates and neighbors. Column A presents our baseline specification, without additional neighborhood controls. Column B adds a control for the difference in the log of per capita income between neighborhoods in which black and white families reside. This

[34] 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. Also, though we focus in Table 9 on tracking in secondary grades, an analysis of data on kindergarten classroom composition from the Early Childhood Longitudinal Survey suggests that cities with more non-residential school segregation have schools that are, at the kindergarten level, significantly *more* internally segregated than are schools in cities with less school segregation.

variable has a significant positive effect, indicating that in cities where blacks' neighborhoods have higher relative incomes, black relative SAT scores are higher. Moreover, the addition of this variable has a notable effect on the estimated residential segregation coefficient, reducing the effect of exposure to minority neighbors by about 40%. The next four columns explore alternative controls for differences in the economic status of black and white neighborhoods, while the last includes all of the neighborhood measures together. None of the other neighborhood variables is a significant predictor of the black-white test score gap. However, when all are included (in column G), the effect of relative exposure to black neighbors is about 25% smaller than we obtained when the neighborhood measures are excluded, and is only marginally significant.

We interpret the pattern of coefficients in Table 10 as suggesting that the measured neighborhood segregation effects in models that exclude other relative neighborhood characteristics overstate the effects of minority exposure *per se*. Indeed, looking at the specifications in columns B and G, it appears that relative exposure to low income neighbors has as important an effect as does relative exposure to minority neighbors.

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 a causal effect 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. The model also suggests that in measuring the effects of racial segregation it is important to control for relative exposure of black and white students to other characteristics of school and neighborhood peer groups, such as family income. Otherwise, these differences will tend to lead to an overstatement of the effects of race *per se*.

Our main empirical evidence is based on SAT outcomes for all the black test takers and one-quarter of the white 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 different cities. We use data from the summary files of the 2000 Census to construct estimates of the relative exposure of white and black students in a city to a variety of neighborhood characteristics, including racial/ethnic composition, income, and family structure. To address concerns about potential selectivity biases in the SAT outcomes, we also use 2000 Census micro data to construct measures of the relative achievement of black and white youth in different metropolitan areas.

Without controlling for neighborhood segregation, we observe that school segregation has a negative effect on black relative test scores and on achievement measures from the Census. 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 and assessments of correlated differences in unobserved school or peer quality, give no indication that our estimates are biased in a way that would obscure negative effects of school segregation. Finally, in models that include school segregation, neighborhood segregation, and measures of the relative exposure of blacks to other characteristics of their neighbors, even the neighborhood segregation effects are diminished.

Taken as a whole, our results suggest that concerns over the racial isolation of black youth may be overstated. Consistent with the findings of Cutler and Glaeser (1997), neighborhoods appear to matter for student achievement. As suggested by Wilson (1987), however, race *per se* may not be the primary source of these effects: rather, exposure to more economically successful neighbors may contribute to the apparent effect of race. Moreover, holding constant neighborhood characteristics, the racial composition of schools seems to have little effect on black relative achievement. Given recent trends toward ending formal desegregation programs in many cities, this may be good news.

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Appendix: 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:

(A1) $P(\text{i writes test} \mid X_{ijsc};\ s,j,c) = p_{ijsc} = P(X_{ijsc}\,\pi_j + \mu_{ijsc} \geq k_{jsc})$,

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

(A2) $E[\, y_{ijsc} \mid \text{i writes test}, X_{ijsc}; s,j,c\,] = X_{ijsc}\alpha_j + Z_{sc}\,\beta_j + W_{ijsc}\,\varphi_j$

$$+ \ B_{sc}\,\gamma_j + R_{ijsc}\,\delta_j + u_{jsc} + \zeta_j\,\lambda(p_{ijsc}) ,$$

where $\lambda(p)$ is the inverse Mills ratio function evaluated at $\Phi^{\sim 1}(p)$ 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:

(A3) $r_{ijsc} = X'_{ijsc}\,\alpha_j + Z_{sc}\,\beta_j + W_{ijsc}\,\varphi_j + B_{sc}\,\gamma_j + R_{ijsc}\,\delta_j + u_{jsc} + \zeta_j\,\lambda(p_{ijsc}) + e_{ijsc}$,

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 12[th] graders of group j in city c, N_{jsc} is the number of 12[th] 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 (A3) implies that:

$$\text{(A4)} \quad r_{jc} = X'_{jc}\alpha_j + Z_{jc}\beta_j + W_{jc}\varphi_j + B_{jc}\gamma_j + R_{jc}\delta_j + u_{jc} + \zeta_j (1/N_{jc}) \Sigma_s \Sigma_i p_{jsc}^{-1} \lambda(p_{ijsc}) + e_{jc},$$

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 (A4), an approximate expression for the average adjusted test score for group j in city c is:

$$\text{(A5)} \quad r_{jc} = X'_{jc}\alpha_j + Z_{jc}\beta_j + W_{jc}\varphi_j + B_{jc}\gamma_j + R_{jc}\delta_j + u_{jc} + \zeta_j\lambda_{jc} + \zeta_j\theta_{jc} + e_{jc}.$$

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:

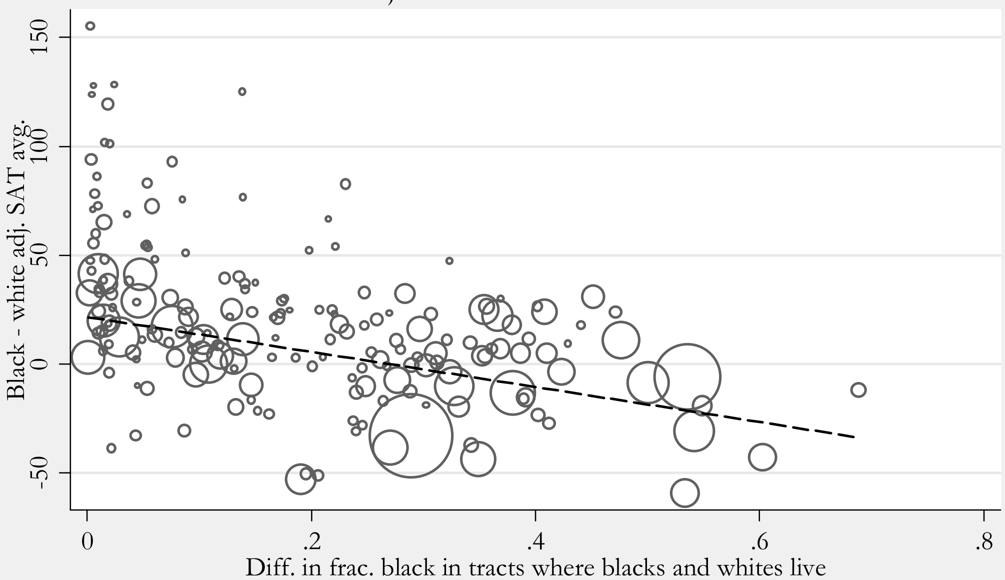
$$\text{(A6)} \quad \Delta r_c = r_{1c} - r_{2c} = X'_{1c}\alpha_1 - X'_{2c}\alpha_2 + Z_{1c}\beta_1 - Z_{2c}\beta_2 + W_{1c}\varphi_1 - W_{2c}\varphi_2$$

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

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

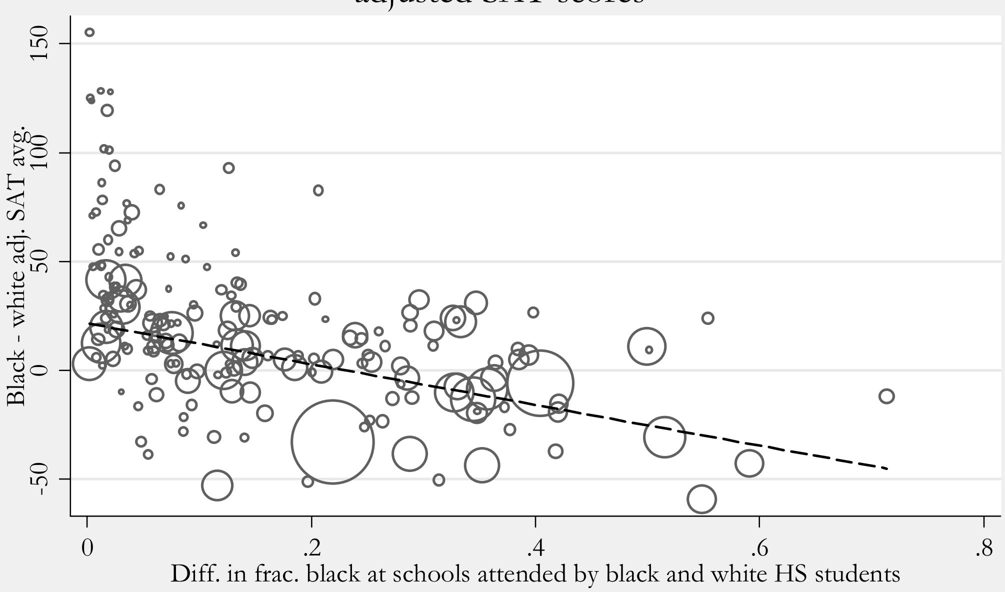
or, if the coefficients β, δ, γ, and ζ are the same for whites and blacks, equation (6) in the text.

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



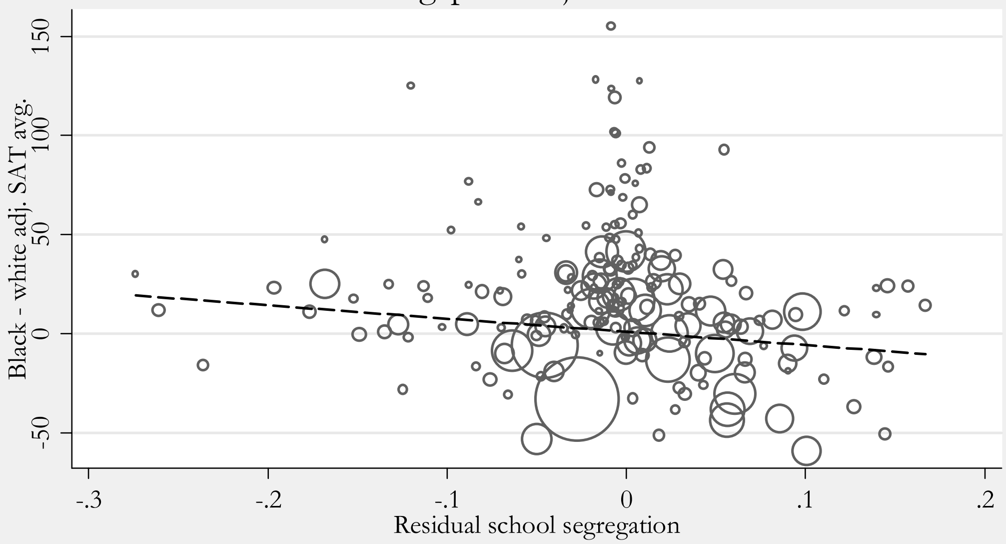
Notes: Sample is metropolitan areas in SAT states. Circle sizes are proportional to the sampling error variance in MSA black-white gaps (see text for details). Line is the weighted least squares regression line.

Figure 2. School 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 text for details). Line is the weighted least squares regression line.

Figure 3. School segregation unexplained by 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 text for details). "Residual school segregation" is the residual from a weighted bivariate regression of school segregation on residential segregation (coeff. 0.89, s.e. 0.03). Line is the weighted least squares regression line, which has a slope insignificantly different from zero.

Table 1: Residential and school segregation in representative metropolitan areas

Name	Residential Fraction black in: Blacks' tracts (A)	Whites' tracts (B)	Diff. (C)	High school students Fraction black in: Blacks' schools (D)	Whites' schools (E)	Diff. (F)	SAT State? (G)	Avg. SAT score Blacks (H)	Whites (I)	Diff. (J)
Least residentially segregated cities										
Jacksonville, NC MSA	25.9%	17.1%	8.8%	32.5%	23.9%	8.6%	Y	886	1014	-128
Lawton, OK MSA	27.5%	17.6%	9.9%	29.5%	22.0%	7.5%	N			
Dover, DE MSA	29.4%	18.7%	10.7%	29.0%	22.3%	6.6%	Y	843	1012	-169
Killeen-Temple, TX MSA	29.7%	17.5%	12.3%	32.5%	18.2%	14.2%	Y	869	1021	-152
Charlottesville, VA MSA	25.7%	12.5%	13.1%	27.6%	16.1%	11.5%	Y	858	1073	-215
Most residentially segregated cities										
Flint, MI PMSA	70.1%	7.5%	62.6%	69.8%	7.6%	62.2%	N			
Cleveland-Lorain-Elyria, OH PMSA	70.4%	6.3%	64.0%	69.3%	7.7%	61.7%	N			
Chicago, IL PMSA	72.3%	5.6%	66.7%	65.7%	7.2%	58.5%	N			
Gary, IN PMSA	73.6%	4.8%	68.8%	76.3%	4.8%	71.4%	Y	798	993	-195
Detroit, MI PMSA	79.0%	5.7%	73.3%	80.9%	5.2%	75.7%	N			
Cities with the least segregated schools relative to neighborhoods										
Fort Pierce-Port St. Lucie, FL MSA	45.2%	6.3%	39.0%	29.4%	20.1%	9.3%	Y	848	1053	-205
Louisville, KY-IN MSA	53.1%	7.6%	45.5%	33.7%	15.3%	18.5%	N			
Lakeland-Winter Haven, FL MSA	37.2%	9.7%	27.6%	27.4%	21.4%	5.9%	Y	835	1017	-182
Jackson, TN MSA	54.9%	18.4%	36.6%	47.3%	33.2%	14.1%	N			
Wilmington, NC MSA	36.8%	12.1%	24.7%	29.8%	23.8%	6.0%	Y	843	1022	-179
Cities with the most segregated schools relative to neighborhoods										
New Haven-Meriden, CT PMSA	41.2%	6.9%	34.3%	49.4%	7.6%	41.8%	Y	779	1018	-238
New Orleans, LA MSA	70.1%	16.5%	53.6%	76.8%	17.8%	59.1%	N			
Saginaw-Bay City-Midland, MI MSA	55.6%	4.2%	51.3%	62.4%	5.0%	57.4%	N			
Albany, GA MSA	71.8%	28.8%	42.9%	78.7%	28.5%	50.2%	Y	837	1021	-184
Goldsboro, NC MSA	47.9%	25.9%	22.1%	61.3%	28.3%	33.0%	Y	844	1018	-173

Notes: Residential segregation rankings are by difference in fraction black between blacks' and whites' census tracts, as in Column C, among 119 MSAs and PMSAs with at least 10% blacks. Segregation of schools relative to neighborhoods is the residual from a bivariate regression of a similar segregation measure computed over high schools (Column F) on the residential segregation measure (coefficient 0.89, s.e. 0.03).

Table 2. Summary statistics for cities in the SAT sample

	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	0.09	0.11	0.08	0.14	0.10
Fraction Hispanic	0.21	0.21	0.25	0.23	0.09	0.10
log(Mean HH income)	10.98	0.19	10.99	0.20	10.96	0.16
Segregation (Black fraction black - white fraction black)						
Residential (Tract)	0.29	0.20	0.26	0.18	0.36	0.24
High schools	0.26	0.19	0.22	0.16	0.34	0.23
SAT-takers' schools (reweighted)			0.21	0.15		
High schools, residualized from residential	0.00	0.06	-0.01	0.06	0.02	0.07
SAT-taking rate						
All students	0.28	0.13	0.34	0.10	0.14	0.11
White students	0.32	0.14	0.38	0.09	0.16	0.11
Black students	0.21	0.11	0.27	0.07	0.09	0.09
SAT-takers						
Avg. SAT	1033.5	71.2	999.5	45.7	1114.8	53.0
Black-white avg. SAT	-193.3	36.5	-194.0	34.3	-191.6	41.3

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

	B-W participation rate		B-W avg. residual SAT score			
	(A)	(B)	(C)	(D)	(E)	(F)
Black-white difference: Fr. black in HS students' schools	-0.21	-0.02	-120.0	-98.4	-55.1	-49.0
	(0.07)	(0.06)	(29.7)	(28.6)	(22.2)	(25.1)
Black-white difference: Fr. Hispanic in HS students' schools	-0.19	-0.05	-68.5	-82.7	-28.0	-14.5
	(0.12)	(0.06)	(23.7)	(31.9)	(28.6)	(27.6)
Fr. black "main effect:" Fr. black in white & black HS students' schools	0.03	-0.08	-7.3	5.6	8.5	10.2
	(0.07)	(0.08)	(32.2)	(28.6)	(28.2)	(29.1)
Fr. Hispanic "main effect:" Fr. Hispanic in white & black HS students' schools	0.37	0.22	-5.9	27.0	1.1	1.6
	(0.12)	(0.07)	(12.8)	(21.6)	(25.1)	(20.7)
B-W background index (SAT-takers)				1.35	1.05	2.14
				(0.31)	(0.23)	(0.92)
B-W fraction of kids living with one parent (census)		-0.43			-69.4	-105.5
		(0.18)			(65.6)	(60.1)
B-W fraction of kids living with neither parent (census)		-0.36			-96.3	-118.0
		(0.22)			(73.8)	(72.9)
B-W: Mothers some college (census)		0.20			23.1	40.8
		(0.11)			(47.9)	(43.5)
B-W: Mothers BA+ (census)		0.04			66.6	62.8
		(0.16)			(56.3)	(56.8)
B-W: Fathers some college (census)		0.18			-40.4	-45.2
		(0.09)			(55.6)	(44.0)
B-W: Fathers BA+ (census)		0.22			-13.7	-23.9
		(0.17)			(46.0)	(49.3)
B-W employment rate of mothers (census)		-0.05			-14.0	-17.5
		(0.12)			(46.3)	(51.5)
B-W median family income (census; $10,000s)		0.004			1.5	0.9
		(0.007)			(2.2)	(2.4)
B-W child poverty rate (census)		-0.33			-100.9	-52.8
		(0.13)			(57.4)	(57.0)
B-W inverse Mills ratio			-46.2	-26.2	24.3	22.4
			(14.3)	(14.2)	(14.6)	(14.8)
B-W: Fathers some college (SAT-takers)						57.8
						(57.5)
B-W: Fathers BA+ (SAT-takers)						-159.3
						(70.0)
B-W: Mothers some college (SAT-takers)						-156.8
						(55.3)
B-W: Mothers BA+ (SAT-takers)						108.4
						(70.2)
B-W family income (SAT-takers; in SAT points)						0.36
						(2.13)
N	185	185	185	185	185	185
R-squared	0.73	0.84	0.59	0.65	0.74	0.77
p-value, B-W fr. Black=B-W fr. Hispanic	0.86	0.64	0.13	0.72	0.42	0.29
p-value, B-W fr. Black=B-W fr. Hispanic=0	0.01	0.70	0.00	0.00	0.04	0.15

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. All columns include controls for the log of the city population and for the city land area, fraction with some college and BAs, log mean HH income, gini coefficient, and census division effects. City-level black-white differences in residual SATs (columns C-F) are computed over SAT-taker data that are re-weighted using school-by-race participation rates; see text for details. All standard errors are clustered on the CMSA.

Table 4. Residential and school segregation effects on black-white differences in SAT participation and residual scores

	B-W partic. rate			B-W avg. residual SAT score			
	(A)	(B)	(C)	(D)	(E)	(F)	(G)
Black-white difference: Fr. black in HS students' schools	0.03	0.05		-3.1	0.4	-8.6	
	(0.08)	(0.08)		(29.4)	(32.9)	(31.5)	
Black-white difference: Fr. Hispanic in HS students' schools	0.30	0.29		80.9	79.2	94.6	
	(0.14)	(0.13)		(63.4)	(64.3)	(58.5)	
Black-white difference: Fr. minority in HS students' schools			0.10				10.4
			(0.08)				(29.2)
Black-white difference: Fr. Black in residents' census tracts	-0.07	-0.08		-88.5	-81.4	-73.3	
	(0.09)	(0.09)		(27.7)	(30.5)	(28.7)	
Black-white difference: Fr. Hispanic in residents' census tracts	-0.36	-0.37		-137.7	-120.7	-126.3	
	(0.13)	(0.13)		(56.1)	(61.0)	(55.8)	
Black-white difference: Fr. minority in residents' census tracts			-0.17				-78.9
			(0.08)				(28.0)
B-W inverse Mills ratio				30.3	30.7	28.3	25.4
				(15.1)	(16.1)	(15.6)	(14.8)
B-W residual wage gap in MSA: Mothers		0.00	0.00			-5.9	-14.7
		(0.07)	(0.06)			(26.9)	(25.7)
B-W residual wage gap in MSA: Fathers		0.08	0.08			-47.7	-41.2
		(0.04)	(0.04)			(17.3)	(16.2)
Census B-W bkgd. controls	y	y	y	y	y	y	y
B-W background index (SAT-takers)	n	n	n	y	y	y	y
Additional SAT-taker B-W bkgd. controls	n	n	n	n	y	y	y
N	185	185	185	185	185	185	185
R-squared	0.81	0.81	0.81	0.76	0.78	0.79	0.79
p-value, school segregation effect=0	0.10	0.08	0.19	0.44	0.46	0.24	0.72
p-value, residential segregation effect=0	0.02	0.02	0.05	0.00	0.01	0.01	0.01

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. All columns include controls for the log of the city population and for the city land area, fraction with some college and BAs, log mean HH income, gini coefficient, and census division effects. Census background controls are those in Column B of Table 3; additional SAT-taker controls are those in Column F of that table. All standard errors are clustered on the CMSA.

Table 5. Instrumental variables estimates of school segregation effect

	Full sample		Welch & Light subsample	
	(A)	(B)	(C)	(D)
Panel A: First stage; dependent variable is school segregation index				
Black-white difference: Fr. minority in residents' census tracts	0.89	0.90	0.96	0.94
	(0.06)	(0.06)	(0.12)	(0.11)
Choice index in MSA (=1 - Herfindahl for district enrollment of HS students)		0.074		
		(0.025)		
Change in dissimilarity index induced by major desegregation plans (/100)				0.16
				(0.05)
N	185	185	60	60
F statistic, exclusion of instruments		8.7		11.0
Panel B: OLS/IV; dependent variable is black-white gap in residual test scores				
	OLS	**IV**	**OLS**	**IV**
Black-white difference: Fr. minority in residents' census tracts	-78.9	-193.2	-78.3	-125.2
	(28.0)	(118.4)	(43.3)	(98.4)
Black-white difference: Fr. minority in SAT-takers' schools	10.4	138.0	-13.3	35.4
	(29.2)	(133.8)	(44.1)	(111.5)

Notes: Models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$ and standard errors are clustered on the CMSA. Columns A and B include controls from column G of Table 4; Columns C and D include all controls from Column E of Table 3 except the family structure, employment, income, and poverty variables.

Table 6: Estimates for SAT averages unadjusted for participation rates

	(A)	(B)	(C)
Black-white difference: Fr. black in SAT takers' schools	-42.3	-21.3	
	(21.3)	(25.9)	
Black-white difference: Fr. Hispanic in SAT-takers' schools	-17.5	72.6	
	(25.9)	(48.9)	
Black-white difference: Fr. minority in SAT-takers' schools			-3.5
			(20.9)
Black-white difference: Fr. black in residents' census tracts		-38.1	
		(26.0)	
Black-white difference: Fr. Hispanic in residents' census tracts		-105.9	
		(39.9)	
Black-white difference: Fr. minority in residents' census tracts			-58.5
			(25.4)
N	185	185	185
R-squared	0.78	0.79	0.78
p-value, school segregation effect=0	0.095	0.276	0.867
p-value, residential segregation effect=0		0.029	0.023

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Dependent variable is the difference between black and white means of un-reweighted adjusted SAT scores. Control variables are as in Column F of Table 4, except that the inverse Mill's ratio term is excluded and the un-reweighted SAT-taker data are used to compute the black-white difference in family background among SAT-takers. All standard errors are clustered on the CMSA.

Table 7. Residential and school segregation effects on black-white differences in alternative outcome measures, measured from Census data

Dependent variable: B-W gap in percentage who are employed or in school	(A)	(B)	(C)	(D)	(E)	(F)
B-W fr. Black in HS students'	-10.4	-6.0	0.8	4.7	5.1	2.6
schools	(2.4)	(3.4)	(2.1)	(3.9)	(4.2)	(3.2)
B-W fr. Hispanic in HS students'	-0.3	-2.9	2.3	-4.2	-12.3	-8.4
schools	(3.2)	(4.3)	(4.3)	(9.9)	(13.3)	(8.2)
B-W fr. Black in residents' census				-17.6	-18.0	-3.0
tracts				(4.5)	(4.5)	(3.8)
B-W fr. Hispanic in residents' census				4.8	7.6	11.0
tracts				(11.1)	(13.7)	(8.5)
Basic city controls	n	y	y	n	y	y
B-W gaps in observables	n	n	y	n	n	y
N	234	234	234	234	234	234
R-squared	0.21	0.37	0.58	0.30	0.41	0.59
p-value, school segregation effect=0	0.00	0.21	0.84	0.40	0.28	0.40
p-value, resid. segregation effect=0				0.00	0.00	0.30

Dependent variable: B-W gap in percentage who have finished HS or are in school	(A)	(B)	(C)	(D)	(E)	(F)
B-W fr. Black in HS students'	-10.2	-8.1	-1.5	1.6	0.9	-1.1
schools	(1.8)	(3.0)	(1.9)	(3.8)	(3.9)	(3.1)
B-W fr. Hispanic in HS students'	-2.7	-8.8	-4.5	-13.3	-13.9	-12.1
schools	(5.1)	(5.1)	(4.8)	(8.8)	(12.7)	(8.7)
B-W fr. Black in residents' census				-13.2	-14.5	-0.6
tracts				(3.7)	(5.4)	(4.2)
B-W fr. Hispanic in residents' census				11.9	3.6	8.2
tracts				(9.5)	(11.8)	(9.3)
Basic city controls	n	y	y	n	y	y
B-W gaps in observables	n	n	y	n	n	y
N	234	234	234	234	234	234
R-squared	0.18	0.34	0.55	0.23	0.36	0.55
p-value, school segregation effect=0	0.00	0.02	0.54	0.21	0.51	0.38
p-value, resid. segregation effect=0				0.00	0.03	0.67

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Dependent variables range in principle from -100 to 100, and have means (S.D.s) of -11.7 (3.8) and -6.9 (3.8), respectively. Each is measured over 16-24 year olds in the 2000 census, assigned to the metropolitan area where they lived in 1995. Sample excludes MSAs with fewer than 50 blacks or 50 whites in this sample. Control variables are those in column B of Table 3. 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)				School demographics (CCD)
	PP Expenditures ($1,000s)	Teacher / pupil ratio * 100	Fraction white	Avg. salary ($1,000s)	Avg. exper.	BA: Educ. Major	Fraction free lunch in school
	(A)	(B)	(C)	(D)	(E)	(F)	(G)
B-W fr. Black in students' schools	1.03	1.15	-0.54	-3.56	5.09	0.21	0.44
	(1.01)	(0.44)	(0.15)	(5.43)	(3.94)	(0.11)	(0.06)
B-W fr. Hispanic in students' schools	-0.19	0.66	-0.67	-3.26	2.89	-0.07	0.61
	(1.93)	(0.99)	(0.29)	(13.59)	(10.72)	(0.34)	(0.11)
B-W fr. Black in residents' census tracts	0.18	-0.82	0.10	0.01	2.38	0.54	-0.11
	(1.17)	(0.51)	(0.20)	(7.44)	(5.20)	(0.17)	(0.08)
B-W fr. Hispanic in residents' census tracts	-0.33	-1.89	0.11	5.23	1.82	-0.12	-0.15
	(1.95)	(0.87)	(0.34)	(12.55)	(9.15)	(0.38)	(0.11)
N	323	305	320	320	320	320	300
R-squared	0.36	0.38	0.62	0.12	0.15	0.18	0.87
p-value, school segreg. effect=0	0.60	0.03	0.00	0.79	0.41	0.16	0.00
p-value, resid. segreg. effect=0	0.97	0.06	0.86	0.91	0.87	0.00	0.07

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 enrollment in all grades, and over public schools in Columns A, B, and G. All columns include controls from column F of Table 4, minus the SAT-taker background index and inverse Mill's ratio terms. All standard errors are clustered on the CMSA.

Table 9. Residential and school segregation effects on measures of honors course-taking among SAT-takers

	=100 if honors courses in		=100 if plan to claim adv. / exempt status in	
	Math	English	Any subject	Math or English
	(A)	(B)	(C)	(D)
Panel A: Avg. among black SAT-takers				
B-W fr. minority in HS students' schools	3.9	-7.0	0.3	1.1
	(8.0)	(13.3)	(6.0)	(4.9)
B-W fr. minority in residents' census tracts	3.9	3.3	-8.4	-4.4
	(8.0)	(13.5)	(7.6)	(6.4)
Panel B: Avg. among white SAT-takers				
B-W fr. minority in HS students' schools	-18.2	-28.1	-11.1	-9.5
	(9.6)	(12.1)	(6.6)	(5.1)
B-W fr. minority in residents' census tracts	-12.9	-12.2	-17.7	-13.2
	(8.1)	(12.1)	(7.5)	(6.1)
Panel C: Difference between black and white averages				
B-W fr. minority in HS students' schools	17.5	13.6	17.9	15.1
	(8.5)	(7.7)	(5.7)	(5.3)
B-W fr. minority in residents' census tracts	10.8	7.7	10.1	5.2
	(7.8)	(9.4)	(6.5)	(5.4)

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. All columns include controls from column G of Table 4, though background measures in panels A and B are averaged over black and whites separately, rather than differenced as in Table 4 and panel C. All standard errors are clustered on the CMSA.

Table 10. Distinguishing effects of exposure to blacks from exposure to high-poverty neighborhoods on adjusted SAT scores

	(A)	(B)	(C)	(D)	(E)	(F)	(G)
Black-white difference: Fr. minority in	7.9	0.2	7.0	4.3	9.7	10.7	4.1
HS students' schools	(26.2)	(25.2)	(25.8)	(26.9)	(26.1)	(26.2)	(26.9)
Black-white difference: Fr. minority in	-92.0	-56.4	-94.8	-83.6	-76.6	-80.5	-67.7
residents' census tracts	(26.1)	(27.0)	(27.1)	(29.7)	(26.3)	(29.2)	(36.4)
B-W tracts: log(per capita income)		66.3					100.2
		(27.6)					(50.5)
B-W tracts: Male employment rate			-28.1				-30.6
			(63.6)				(64.2)
B-W tracts: Fr. of adults with less than				79.9			138.3
a high school education				(93.2)			(104.7)
B-W tracts: Fr. of adults with a BA or				118.8			-7.4
more education				(118.1)			(99.7)
B-W tracts: Child poverty rate					-102.5		7.8
					(77.0)		(150.3)
B-W tracts: Fr. of kids with two parents						44.7	11.6
						(49.0)	(93.2)
N	185	185	185	185	185	185	185
R-squared	0.76	0.77	0.76	0.77	0.77	0.76	0.78
p-value, school segregation effect=0	0.76	1.00	0.79	0.87	0.71	0.68	0.88
p-value, residential segregation effect=0	0.00	0.04	0.00	0.01	0.00	0.01	0.06
p-value, non-race tract exposure=0	0.00	0.02	0.66	0.60	0.19	0.36	0.06

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Control variables are those in column G of Table 4. All standard errors are clustered on the CMSA.

Appendix Table 1. Alternative estimates

	Base		Minority exposure							
	Base	CA/TX/FL indic.	Base	CA/TX/FL indic.	Elem. Schl. Seg.		College grads seg.		Separate B, W effects	
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)	(J)
School segregation measures										
Black-white difference: Fr. black in HS students' schools	-9.9	-5.2								
	(30.0)	(28.9)								
Black-white difference: Fr. Hispanic in HS students' schools	90.8	100.4								
	(59.5)	(60.0)								
Black-white difference: Fr. minority in HS students' schools			7.9	13.4	3.9		8.2	-11.0		
			(26.2)	(25.1)	(47.7)		(26.3)	(22.6)		
Black-white difference: Fr. minority in elementary students' schools					4.9	8.0				
					(52.2)	(29.1)				
Fr. minority in white HS students' schools									6.6	
									(77.2)	
Fr. minority in black HS students' schools									6.7	
									(27.9)	
Residential segregation measures										
Black-white difference: Fr. Black in residents' census tracts	-85.8	-88.3								
	(28.0)	(28.4)								
Black-white difference: Fr. Hispanic in residents' census tracts	-139.9	-149.1								
	(53.3)	(53.8)								
Black-white difference: Fr. minority in residents' census tracts			-92.0	-96.1	-93.1	-92.5	-72.3			
			(26.1)	(26.1)	(27.5)	(27.5)	(45.5)			
Black-white difference: Fr. minority in college degreed residents' census tracts							-25.1	-75.8		
							(45.8)	(27.2)		
Fr. minority in white residents' census tracts									45.2	56.2
									(91.5)	(22.1)
Fr. minority in black residents' census tracts									-97.7	-91.8
									(30.4)	(20.8)
CA/TX/FL		6.7		6.7						
		(5.3)		(5.4)						
N	185	185	185	185	185	185	185	185	185	185
R-squared	0.77	0.77	0.76	0.77	0.76	0.76	0.76	0.76	0.76	0.76
p-value, school segregation effect=0	0.30	0.25	0.76	0.59	0.93		0.75	0.63	0.97	
p-value, residential segregation effect=0	0.00	0.00	0.00	0.00	0.00	0.00	0.11		0.00	0.00
p-value, alternative segregation effect=0					0.93	0.78	0.58	0.01		

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Control variables are those in column F of Table 4. All standard errors are clustered on the CMSA.