

Racial Segregation and the Black-White Test Score Gap

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January 2007

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We are grateful to the Andrew Mellon Foundation and the College Board for assistance in obtaining the SAT data used in this study, and to Jacob Vigdor, Jon Guryan, and Sarah Reber for supplying other data. We thank Florence Neymotin and Ashley Miller for outstanding research assistance, and Ken Chay, Nicole Fortin, Jeff Kling, Thomas Lemieux, Justin McCrary, anonymous referees, and seminar participants at Berkeley, Columbia, Syracuse, Yale, NBER, and the Universities of Connecticut, Illinois, and Maryland for helpful comments and suggestions. Card's research was supported by a grant from the NICHD and Rothstein's by Princeton's Center for Economic Policy Studies.

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ABSTRACT

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

The racial gap in student achievement is a pervasive and divisive feature of American life. Black-white differences in standardized test scores lie at the core of the debate over affirmative action in college admissions (Bowen and Bok, 1998; Kane, 1998; Krueger, Rothstein, and Turner, 2006) and public sector hiring (McCrary, forthcoming), and figure prominently in the recent No Child Left Behind Act. Many years before the Supreme Court's *Brown v. Board* decision, segregation was identified as a possible factor in the academic achievement of black children.¹ Studies since the Coleman Report (Coleman, 1966) have found that test scores are lower at schools with higher black enrollment shares (see, e.g., Ferguson 1998, and the review by Schofield 1995). Likewise, there is a strong negative correlation between education outcomes and the fraction of black residents in a neighborhood (e.g., Massey, Condran, and Denton, 1987).

Establishing whether exposure to a higher fraction of black peers actually *causes* lower achievement is difficult, however, because individuals are not randomly assigned to neighborhoods or schools.² A credible research design has to deal with the possibility that students who attend schools with larger black enrollment shares – or live in predominantly black neighborhoods – have other characteristics that contribute to their lower achievement. In this paper, we address the endogeneity of school and neighborhood choice by aggregating to the metropolitan level and relating the black-white achievement gap in different cities to the degree of racial segregation in the area, as measured by the black-white difference in relative exposure to minority neighbors and schoolmates.³ Aggregation abstracts from

¹ Crowley (1932) presents an early study of the effect of racially segregated schools on academic achievement, based on comparisons of test scores for black students in two all-black and four mixed-race schools in Cincinnati. She constructed matched samples from the two groups of schools, matching on age, grade, and IQ, and found no difference in achievement test scores between the schools.

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

³ Although cities with segregated neighborhoods tend to have segregated schools, school segregation also depends on institutional features like the number of school districts (Urquiola, 2005) and the presence of

differences among families in tastes for mixed-race neighborhoods, while differencing eliminates the effect of city-wide variables that may be correlated with racial segregation (such as the level of school spending or the efficiency of local schools).⁴ We also control for a rich set of measured differences in the family backgrounds of black and white test-takers, and for other variables, like region, city size and income inequality, that may affect black students' relative achievement. We apply this approach to a large sample of SAT-takers from the 1998-2001 cohorts of high school graduates.

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

We also conduct a parallel analysis of schooling outcomes of 16-24 year olds measured in the 2000 Census. In sparse specifications like those used in earlier work (e.g., Cutler and Glaeser, 1997), the Census results parallel our findings for SAT scores. Further investigation, however, suggests that effects of segregation on educational attainment fall in size and significance as richer family background characteristics are added. Effects on our achievement measure, by contrast, are robust to quite detailed controls.

desegregation programs (Reber, 2005). We show below that the two have substantial independent variation.
⁴ Throughout this paper we use "cities" to refer to metropolitan areas – specifically, Metropolitan Statistical Areas (MSA's) or, in the largest urbanized areas, Primary Metropolitan Statistical Areas (PMSAs).

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

Our primary analysis does not distinguish between the direct effects of exposure to minority students and “indirect” effects operating through school quality or the characteristics of peer groups. We conclude with an investigation of these channels. Observed indicators of relative school quality are only weakly correlated with the relative exposure of black students to minority neighbors or schools. On the other hand, segregation is highly correlated with exposure to low-income peers at both the school and neighborhood levels: differences in neighborhood incomes account for as much as half of our estimated residential segregation effects.

II. Peer Group and Segregation Effects: Sources and Evidence

Racial or ethnic segregation can affect the relative educational achievement of black students through several mechanisms. One of the most widely discussed channels is a peer exposure effect, arising from the fact that students’ outcomes depend on the expectations and achievement of their peers, and from a presumed correlation between these characteristics and the racial composition of the peer group. A second is that the black enrollment share at a school may be correlated with school quality (Boozer, Krueger, and

Wolkon, 1992; Card and Krueger, 1992; Shrag, 2003; *Brown v. Board of Education*). A third possibility is that residents of more segregated cities have different cultural norms or beliefs about the relative abilities of blacks and whites, leading to lower levels of school achievement by black children (as in, for example, the statistical discrimination model of Coate and Loury, 1993).

Much of the existing literature has focused on peer exposure effects. An early and important example is Coleman (1966), who found a negative correlation between black students' test scores and the black enrollment share at their schools. As subsequent critics have emphasized, Coleman could not address the biases caused by non-random sorting of students to different types of schools (see e.g., Jencks and Mayer, 1990). Nevertheless, their results are often interpreted as evidence that segregation reduces black students' test scores.

Recent studies have used a variety of strategies to circumvent the sorting issue. One approach is to use the variation in minority exposure of different cohorts *at the same school* (Hoxby, 2000; Hanushek, Kain, and Rivkin, 2002). Provided that sorting is based on permanent school characteristics and is independent of cohort-specific racial composition differentials, contrasts between successive cohorts at the same school should identify exposure effects. These studies find large negative effects of exposure to black classmates.⁵

A second strand uses experimental or quasi-experimental variation in peer characteristics.⁶ Guryan (2004) is particularly relevant to our investigation: He uses variation in the scope and timing of major court-ordered desegregation plans in the 1970s and 1980s to identify the effect of school segregation on black students' high school dropout

⁵ See Card and Rothstein (2006) for further discussion.

⁶ The most prominent experiment in this area is the Moving to Opportunity (MTO) project, which offered incentives for public housing residents to move to lower poverty neighborhoods (Sanbonmatsu et al., 2006). This experiment focused on the effect of neighborhood poverty rates, and offers very limited power to measure the effect of exposure to minority neighbors. See also Jacob's (2004) study of the effect of housing project demolitions, which has a similar limitation but like MTO yields little evidence of poverty rate effects on academic achievement.

rates. He finds a modest but statistically significant effect, with black dropout rates falling 3 percentage points relative to whites as a result of policies that on average reduced relative black exposure to black schoolmates by about 20 percentage points.⁷

A final strategy is to eliminate sorting bias via aggregation. Although students of differing abilities may sort to different schools or neighborhoods *within* a city, it is plausible that the distribution of potential abilities *across* metropolitan areas is closer to random (conditional on observed control variables). Evans, Oates, and Schwab (1992) use variation across metropolitan areas in average student characteristics to identify peer effects. Cutler and Glaeser (1997) extend this approach by distinguishing between the outcomes of blacks and whites in the same city, under the weaker assumption that the black-white *difference* in potential ability in a city is unrelated to the degree of residential segregation. They focus on educational attainment, and find larger black-white gaps in high school completion in segregated than in integrated cities.

Our approach is very similar. We extend Cutler and Glaeser's (1997) analysis by examining educational achievement rather than attainment; by incorporating a richer set of family background and metropolitan-level controls; and by attempting to separately identify the effects of school and neighborhood segregation.

III. Empirical Framework

To illustrate our empirical approach and its relationship to a standard peer effect model, we begin with a simplified specification that focuses on school-level influences. We then extend this model to allow for neighborhood effects as well.

⁷ As we discuss below, it is possible that in the longer run some of the integrative effect of desegregation programs is offset by a rise in within-school segregation. Guryan's (2004) estimates, which identify segregation effects on the earliest affected cohorts, would not incorporate such offsetting effects. We are unable to take advantage of variation in the timing of desegregation, as our data describe only a single cohort, but we present analyses that exploit cross-sectional variation in scope.

Assume that a student’s test score depends on his or her own characteristics, the racial composition and other characteristics of his or her schoolmates, school resources and quality, and an unobserved error with a school-level component that may vary by race.

Specifically, we assume:

$$(1) \quad y_{ijsc} = X_{ijsc}\alpha + B_{sc}\beta + Z_{sc}\gamma + M_{sc}\theta + \epsilon_{ijsc},$$

where y_{ijsc} represents the test score (or some alternative measure of achievement) of student i of race group j who attends school s in city c , X_{ijsc} is a vector of student characteristics, B_{sc} represents the fraction of minority students in school s ,⁸ Z_{sc} is a vector of other average characteristics of the students in the school, and M_{sc} measures the quality and resources of the school. The coefficient β captures the direct effects of exposure to minority schoolmates and neighbors, while indirect effects would arise from omission of components of Z and M that are correlated with B .⁹

We can decompose the error in equation (1) into three components:

$$\epsilon_{ijsc} = \mu_{jc} + u_{jsc} + e_{ijsc},$$

where μ_{jc} represents the average unobserved ability of students in race group j in city c , u_{jsc} represents a common error component for students of group j in school s (assumed to average to 0 across all schools in the city), and e_{ijsc} is a student-specific error with mean 0 for each race group in each school. Any non-randomness in the sorting of students to schools or neighborhoods produces a non-trivial school-by-race component, u_{jsc} , which may be

⁸ In our empirical specification “minorities” are blacks and Hispanics. We have tested for differential effects of exposure to the two groups and fail to reject equality in a wide range of alternative specifications. Tables that report separate effects are available upon request.

⁹ Student i ’s achievement might also depend on the average achievement of other students at the school. (1) can be seen as the reduced form for a model that includes these so-called “endogenous” peer effects (Manski, 1993): If a student’s race affects her achievement and her achievement affects that of her classmates, it will appear that the racial composition of the class affects the average achievement. It is not relevant to our investigation whether β (and, similarly, γ) reflects the direct effects of student characteristics or indirect effects operating through student achievement; in either case, the racial composition (and other exogenous characteristics) of the class affects achievement.

correlated with the observed variables in (1). This will bias OLS estimates of β , γ and θ from student-level data.

The effects of non-random sorting within a city can be eliminated by averaging the achievement outcomes of each race group to the city level. Specifically, equation (1) implies that the mean outcome of group j in city c is:

$$(1') \quad y_{jc} = X_{jc}\alpha + B_{jc}\beta + Z_{jc}\gamma + M_{jc}\theta + \mu_{jc},$$

where X_{jc} represents the mean characteristics of students of group j in city c , B_{jc} is the average fraction of minority students at schools attended by race group j in city c , Z_{jc} represents the mean other characteristics of race- j students' peer groups, and M_{jc} is the average quality of the schools attended by race- j students.

Averaging eliminates the effects of within-city sorting (i.e. the impacts of u_{jsc}), but does not eliminate any across-city differences in the average unobserved "abilities" of students, μ_{jc} , which would lead to biases in the estimation of equation (1') across cities if they are correlated with the exposure variables B_{jc} , Z_{jc} , and M_{jc} . Any differences in unobserved ability that are common across race groups in a city can be "differenced out" by comparing blacks and whites within the same city. Specifically, (1') implies:

$$(2) \quad y_{1c} - y_{2c} = (X_{1c} - X_{2c})\alpha + (B_{1c} - B_{2c})\beta + (Z_{1c} - Z_{2c})\gamma + (M_{1c} - M_{2c})\theta + \mu_{1c} - \mu_{2c},$$

$$(2') \quad \Delta y_c = \Delta X_c\alpha + \Delta B_c\beta + \Delta Z_c\gamma + \Delta M_c\theta + \Delta\mu_c,$$

where $j=1$ represents blacks and $j=2$ represents whites, Δy_c denotes the difference in mean test scores between black and white students in the same city, and ΔX_c denotes differences in student characteristics.¹⁰

The difference ΔB_c in equation (2') is an index of the degree of segregation of the

¹⁰ If the coefficients in equation (1) differ between black and white students, (2') will have additional terms reflecting the difference between black and white coefficients evaluated at either the black or the white mean, as in a standard Blinder-Oaxaca decomposition. In our empirical analysis, we allow α to vary across groups in a specific fashion described below. We have also tested for differences in the effects of exposure (i.e. in β), but have found no evidence of this.

city’s schools, and is closely related to standard concepts of “exposure” and “isolation.”¹¹

When schools are fully segregated, $B_{1c} = 1$ and $B_{2c} = 0$, so $\Delta B_c = 1$. When they are completely integrated, $B_{1c} = B_{2c}$, so $\Delta B_c = 0$.¹² ΔZ_c measures other differences in average student characteristics (e.g., family incomes) at schools attended by black and white children, while ΔM_c measures differences in the average quality of black and white students’ schools.

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

$$(3) \quad \Delta \mu_c = F_c \psi + \Delta B_c \varrho + \eta_c,$$

where F_c is a vector of city characteristics (including geographic controls and measures of the city racial composition), ϱ reflects any *causal* “macro” effect of school segregation on city-wide black achievement (beyond the effects operating through exposure introduced earlier), and η_c represents all remaining unobserved differences. This leads to a model of the form:

$$(4) \quad \Delta y_c = \Delta X_c \alpha + \Delta B_c (\beta + \varrho) + \Delta Z_c \gamma + \Delta M_c \theta + F_c \psi + \eta_c$$

OLS estimation of this equation will yield consistent estimates of the combined segregation effect ($\beta + \varrho$) provided that η_c is uncorrelated with ΔB_c , conditional on the control variables ΔX_c (describing the average black-white gaps in student characteristics) and F_c (describing other city characteristics).

A key threat to the identification of the segregation effects in equation (4) is *differential* sorting of black and white families to different metropolitan areas. For example, if

¹¹ In the segregation literature (e.g. Massey and Denton, 1988; Iceland, Weinberg, and Steinmetz, 2002), B_{jc} is known as an index of exposure of race- j students to minorities, and ΔB_c is an isolation index (though this is sometimes scaled by the city-level black share, as in Cutler, Glaeser and Vigdor, 1999).

¹² Equation (1) could be generalized to include nonlinearities in the peer effect (as in Austin-Smith and Fryer, 2005). This would add to (2’) the black-white difference in the mean of higher-order powers of the school-level minority share, which could be identified from variation across cities in the within-race heterogeneity of exposure. There is very little such variation, and estimates of such specifications are quite imprecise. Point estimates indicate that the marginal effect of the minority share is declining (in absolute value) with B_{sc} , but that it is negative throughout the $[0, 1]$ range.

achievement-oriented black families migrate to cities where schools are less racially segregated, and if their characteristics are not fully captured in the measured student background variables, then η_c may be negatively correlated with ΔB_c . We adopt a two-stage approach to controlling flexibly for student characteristics, first using individual-level data to create an index of these characteristics, then controlling at the city level for the black-white difference in this index and in other characteristics like parental education, income, and family structure. Our richest specifications also control for the black-white gap in the wages earned by parents of high school students, which proxy for unobserved ability differences that might otherwise bias the segregation effect.

Our main analyses differ from equation (4) in two ways. First, as we noted in Section II, it is unrealistic to assume that all the relevant characteristics of schoolmates and neighbors can be measured. In our main analysis we focus on a “reduced form” specification that excludes the Z and M variables:

$$(5) \quad \Delta y_c = \Delta X_c \alpha' + \Delta B_c \beta' + F_c \psi' + \eta_c,$$

where

$$\beta' = \beta + \varrho + \{\gamma \text{cov}[\Delta Z_c, \Delta B_c | \Delta X_c, F_c] + \theta \text{cov}[\Delta M_c, \Delta B_c | \Delta X_c, F_c]\} / \text{var}[\Delta B_c | \Delta X_c, F_c]$$

reflects the usual omitted variables formula.¹³ In this reduced form, β' incorporates the direct effects of exposure to minority schoolmates (β), any “macro” effects of city-wide school segregation (ϱ), indirect peer effects arising from black-white differences in other peer characteristics ΔZ_c that are correlated with segregation, and black-white relative school quality effects that are correlated with the degree of school segregation.

We also explore richer specifications that add measures of the segregation of *neighborhoods* in the city to equation (5). Students may be influenced by neighborhood peer

¹³ Of course, η_c in (5) incorporates the portions of $\Delta Z_c \gamma$ and $\Delta M_c \theta$ that are orthogonal to ΔB_c .

groups as well as schoolmates. Augmenting equation (1) with terms representing the neighborhood's racial composition (R_{nc} , where n indexes neighborhoods), and other neighborhood characteristics, then aggregating to the city level and differencing between races, leads to a reduced form specification that includes a measure of the racial segregation of neighborhoods in a city, ΔR_c :

$$(6) \quad \Delta y_c = \Delta X_c \alpha'' + \Delta B_c \beta'' + \Delta R_c \delta'' + F_c \psi'' + \eta_c.$$

Just as ΔB_c is computed as the difference in fraction minority between the schools attended by the average black and white students, ΔR_c is the difference in fraction minority between the average students' neighborhoods. The coefficients β'' and δ'' represent the reduced-form effects of school- and neighborhood-level segregation, respectively. Interpretation of these coefficients is complicated because omitted school quality, omitted schoolmate characteristics, and omitted neighborhood characteristics will all load jointly onto the measured segregation indexes ΔB_c and ΔR_c . Evidence presented below, however, suggests that black-white differences in schoolmate characteristics load primarily onto the school-level segregation measure while differences in neighborhood characteristics are mainly captured by the residential segregation measure. Roughly speaking, then, β'' can be interpreted as a summary of the various school-related channels (including direct peer exposure effects at the school level, the effect of other schoolmate characteristics, and school quality effects), while δ'' is a summary of neighborhood-related channels (direct peer exposure effects at the neighborhood level and the effect of other neighbor characteristics).¹⁴

Measures of school and neighborhood segregation are highly correlated across cities. This limits our ability to separately identify the effects of ΔB_c and ΔR_c . It also influences the interpretation of specifications that include only one at a time. When only school

¹⁴ The coefficients will also reflect any causal "macro" effects of school and residential segregation, respectively.

segregation is used, for example, its coefficient captures the direct and indirect effects of both school and residential segregation, with the residential segregation effects multiplied by a factor characterizing the relationship between the two: $\beta' = \beta'' + \pi \delta''$, where π is the coefficient on ΔB from an auxiliary regression of ΔR on ΔB , ΔX , and F . Empirically, π is close to one—meaning that a one unit increase in school segregation is associated with a nearly commensurate increase in residential segregation—so the sparse specification estimates approximately the sum of the reduced form residential and school segregation effects.

A concern with the use of SAT test scores to measure achievement is selective test participation. As discussed below, we restrict our sample to cities in states where a majority of college-bound students write the SAT (rather than the alternative ACT test). Even within “SAT states”, however, test participation rates vary. Presumably, students at low performing schools are under-represented in the test-taking population, with greater under-representation in cities with lower overall participation. Positive selection into participation will tend to *attenuate* any negative effects of segregation on black relative test scores (Gronau, 1974; Heckman, 1979).¹⁵ We reduce these 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 school-by-race SAT participation rates.

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

¹⁵ 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. Clark, Rothstein, and Schanzenbach (2007) conclude 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. Our aggregation strategy abstracts from the sorting that drives this result.

$$(7) \quad \Delta y_c = \Delta X_c \alpha'' + \Delta B_c \beta'' + \Delta R_c \delta'' + F_c \psi'' + \Delta \lambda_c \zeta + \Delta \tau_c \zeta + \eta_c.$$

In this equation, $\Delta \lambda_c$ is the black-white difference in the inverse Mills ratio function, evaluated at the race-specific test participation rates at each school and averaged to the city level using enrollment weights. $\Delta \tau_c$ is an unobserved error component that reflects the black-white difference in the degree of *within-school* selectivity of test-writers. The coefficient ζ reflects the correlation between the unobserved component of the individual test participation equation and the unobserved component of the test outcome equation, and equals zero if selection into test participation is completely random.

There are large differences in test participation rates between schools. If all students at a school have the same propensity to write the test, the control function $\Delta \lambda_c$ will fully correct for selectivity biases in the observed test scores and $\Delta \tau_c$ will equal 0. More likely, however, test writers will be selectively drawn from the population of students at each school and the error component $\Delta \tau_c$ will not vanish. If a rise in school or neighborhood segregation causes black relative test scores to fall but also causes a rise in the *relative* within-school selectivity of black test takers, our inability to control for this term will lead to attenuation in the estimated negative effect of segregation on relative test scores.

Although we cannot examine within-school selectivity in our data set, we have used the specification developed above to examine between-city differences in the black-white test participation gap.¹⁶ Results (presented in Card and Rothstein, 2006) generally parallel those that we present below for test scores: Black students in more residentially segregated cities have lower relative participation rates, while school segregation has a weak positive effect. Assuming positive selection into participation, this suggests that an analysis that fails to control for participation rates will *understate* the negative effect of residential segregation

¹⁶ Clark, Rothstein, and Schanzenbach (2007) present a more general analysis of selective test participation using SAT data in combination with data on test scores on the ACT test.

on black students' scores and *overstate* that of school segregation. Although we absorb the first-order effect of this via our controls for participation rates, it seems likely that within-school selectivity differences move in the same direction. Selective test participation is therefore unlikely to account for the results presented below; rather, the patterns that we identify would likely be stronger if we could use a representative sample for estimation.

III. Data Sources and Sample Overview

Our primary source of student achievement data is a sample of SAT records for roughly one third of test takers in the 1998-2001 high school graduation classes.¹⁷ These data include self-reported family background characteristics as well as high school identifiers, which we use to match enrollment counts from the appropriate editions of the Common Core of Data (CCD, for public school students) and the 1997-8 Private School Survey (PSS). To minimize the impact of measurement errors we estimate the number of students, the number of test takers, and the racial composition of each school using averages over the four years in our data.¹⁸ We assign students to Metropolitan Statistical Areas (MSAs) based on year-2000 definitions, using school location information in the CCD and PSS files.¹⁹ 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."

Family characteristics are strong predictors of student test scores. This suggests that ΔX_c is a key control in our aggregate specifications, and that the effects of family

¹⁷ The sampling rate was 100% for black and Hispanic test-takers and for those from California and Texas, and 25% for others. We use sampling weights in all computations of city-level averages. We exclude observations for students who reported ethnicity other than white or black (primarily Hispanics and Asians) and those who did not report their race/ethnicity.

¹⁸ This strategy cannot be employed with the PSS, as only one year of data is available.

¹⁹ 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.

background variables should be modeled as flexibly as possible. Our aggregate models have only as many degrees of freedom as the number of metropolitan areas in the sample, limiting the flexibility of our controls. It is thus essential that we select a small number of controls that can absorb any effects of background characteristics on student outcomes. While it is straightforward to include black-white differences in mean parental education and family income in our city-level specifications, these cannot capture nonlinear effects nor coefficients that vary with race. To capture such effects, we use our individual-level microdata to develop a “background index,” a nonlinear function of the available family characteristics that best predicts student test scores within schools.

Note that equation (1) can be written as

$$(8) \quad y_{ijsc} = \mathbf{X}_{ijsc}\alpha + \varphi_{ijsc} + e_{ijsc},$$

where the race-group and school-specific component $\varphi_{ijsc} = \mathbf{B}_{sc}\beta + \mathbf{Z}_{sc}\gamma + \mathbf{M}_{sc}\theta + \mu_{jc} + u_{jsc}$ captures all school-, race-, and city-level effects. We estimate race-specific versions of equation (8), including school fixed effects and a highly flexible parameterization of our student-level covariates.²⁰ We use the coefficients from these regressions, α_j^{FE} , to form a unidimensional individual-level index, $W_{ijsc} = \mathbf{X}_{ijsc}\alpha_j^{\text{FE}}$.²¹ The city-level ΔX_c controls in our richest models include this index, five additional background measures constructed directly from the SAT microdata (family income and four education indicators, for whether each parent has some college and a college degree), and nine measures computed over all young people in the city from Census data.²²

²⁰ Specifically, we include dummies for 10 maternal education categories, 10 paternal education categories, 14 family income categories, and gender, and we allow the coefficients to vary freely with race. Specifications that interact the various measures give similar results.

²¹ In principle, standard errors in our city-level regressions should be adjusted to account for sampling error in α_j^{FE} . In practice, our samples are quite large within cities, and the α_j^{FE} vector is extremely precise. Standard errors presented below do not adjust for the two-step estimation; when we have computed these adjustments they were negligible.

²² If spillover effects of background characteristics are proportional to their direct effects and α_j^{FE} is unbiased,

Recognizing that SAT scores are influenced not just by the racial composition of a student's 12th-grade school but also by the composition of her schools in earlier grades, we attempt to construct a school segregation variable that measures the average exposure of white and black students to minority schoolmates throughout their educational careers. We compute exposure rates for high schools in the MSA in 1998-2001 and for elementary schools in 1988-1991, and form an average of these that puts two-thirds weight on the latter and one-third on the former. Our school segregation measure is the black-white difference in this lifetime exposure measure.²³

We use data on the racial composition and population of Census tracts in 2000 (from the full population counts, Census 2000 Summary File 1) to construct measures of neighborhood-level exposure to black and Hispanic neighbors, and a corresponding city-level residential segregation index.²⁴ We also use Summary Files computed from 2000 Census long-form data to estimate the average family background characteristics of black and white children in each city, supplementing this with information from the public use samples (PUMS) for characteristics (e.g. parental education and residual parental wages) that are not tabulated elsewhere. We also use the PUMS data to construct a measure of the black-white gap in degree attainment that is free from any test participation biases. Further details of our data sources and merging methods are presented in a Data Appendix, available on request.

the background index ΔW_c should be a sufficient control. We include the other SAT-based measures to guard against possible violations, and the Census measures to capture background differences that are unavailable in the SAT data.

²³ 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), however, we use point-in-time school segregation measures computed over the relevant schools and grade levels. Results are not sensitive to the particular measures used.

²⁴ Census tracts are initially defined to encompass demographically homogenous neighborhoods of about 4,000 residents, but once drawn generally have stable boundaries. Exposure measures based on Census Block Groups (typically about 1000 residents) are nearly perfectly correlated across cities with the tract-based measures and lead to virtually identical estimates.

Table 1 gives an overview of the patterns of segregation and test scores for a selection of cities with different patterns of residential and school segregation. The first two columns show the fraction black and Hispanic in the metropolitan area.²⁵ Columns C-E show the mean exposure of black and white students in each city to minority (black and Hispanic) schoolmates, while the final columns show parallel measures of tract-level exposure to minority neighbors.

The first two panels of the table present data for cities with the lowest and highest levels of school segregation in our data set.²⁶ The five least segregated cities are all in the South: in these cities, the typical black-white gap in exposure to minority schoolmates (ΔB_c) is about 6%. In three of the cities the gap in exposure to minority neighbors (ΔR_c) is comparable, but in two (Wilmington, North Carolina and Gainesville, Florida) neighborhoods are substantially more segregated. Among the 5 most segregated cities, 4 are in the mid-Atlantic region; all have highly segregated neighborhoods as well as schools.²⁷ All of our specifications include fixed effects for census divisions—the South, e.g., is divided into the South Atlantic, East South Central, and West South Central divisions—so we identify segregation effects only from variation among metropolitan areas within the same division. Approximately two thirds of the variation in both residential and school segregation is within divisions.

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

²⁵ 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.

²⁶ Cities with black shares below 5% are excluded from Table 1 but included in our regression samples. All of our regressions 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.

²⁷ Some of the most segregated cities in the U.S., like Detroit and Chicago, are in states where a majority of students write the ACT. These cities are excluded from Table 1 and from all of our SAT analyses.

the cities with the biggest divergence between the two measures, first for cities with relatively integrated schools and then for cities with relatively segregated schools.²⁸ The degree of neighborhood segregation is similar in the two groups of cities but the extent of school segregation is much smaller in the first group (mean exposure gap=13%) than in the second (mean exposure gap=49%). Although residential and school segregation are highly correlated, there is clearly substantial independent variation in the two factors.

Table 2 presents some comparisons between the students in all 331 MSA's in the country (columns A-B) and those in the 189 cities from SAT states that are included in our analysis sample (columns C-D). On average 43 percent of white high school students and 31 percent of black high school students from cities in the SAT states write the SAT. Blacks are slightly under-represented in the SAT state cities whereas Hispanics are over-represented.²⁹ Cities from SAT states also have slightly less segregated neighborhoods and schools than cities in other states.

The bottom two rows in Table 2 show average SAT scores for the different city groups and the mean test gap between whites and blacks. Average SAT scores are lower in high-participation states (Dynarski, 1987; Clark, Rothstein, and Schanzenbach, 2007), but the black-white difference is very similar for cities in SAT and non-SAT states, suggesting that use of within-city differences reduces problems associated with selective test participation.

As a final descriptive exercise, Figures 1-3 show the correlations across cities between the black-white 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

²⁸ We define the degree of divergence as the residual from a regression of school segregation (ΔB_c) on neighborhood segregation (ΔR_c).

²⁹ California, Texas, and Florida are all SAT states. Exclusion of cities in these states from our sample has approximately zero effect on the estimates presented below.

schools that is orthogonal to the relative segregation of neighborhoods (Figure 3).³⁰ There is a strong negative relationship in the first two graphs between each racial segregation measure and the relative test scores of black students. The relationship of relative test scores to school segregation is substantially weakened when we focus on the component of school segregation that is orthogonal to neighborhood segregation, and seems to be driven more heavily by a few outliers. As we document below, this relationship disappears entirely as we add control variables, though the relationship between residential segregation and black relative test scores remains strong.

IV. Regression Models for Black-White Gaps in Scores

a. Basic Models

Table 3 presents our estimates of the model given by equation (7). The dependent variable is the black-white SAT score gap in the city. All the models include main effects for the overall fraction black and Hispanic in the city's schools, dummies for 5 census divisions, and the black-white gap in an inverse Mill's ratio formed from the race-specific SAT participation rates at each school.³¹ We present three sets of specifications: models with only school segregation in columns A-C; models with only neighborhood segregation in columns D-F; and models with both variables in columns G-I.

The most parsimonious models, in columns A and D, show strong negative effects of racial segregation on average SAT scores. The -125 coefficient in column A, for example, implies that moving from complete segregation to complete integration of a city's schools would raise black relative SAT scores by 125 points, or about 60 percent of the overall black-

³⁰ Graphs using the black-white gaps in *unadjusted* scores look very similar to Figures 1-3.

³¹ Although there are nine Census divisions, only six are represented among SAT states. In Table 3 and the remainder of the paper, we exclude cities (4 of 189 in SAT states) for which we cannot construct black-white differences in family background characteristics, introduced in Column C, using the 2000 Census microdata sample.

white gap.³² The models in columns B and E add controls for a vector of MSA characteristics (the log of population, the log of land area, the fractions of residents with 13-15 and 16+ years of education, log mean household income, and the Gini coefficient of household income) and for black-white gaps in SAT takers' observed characteristics (including the background index discussed above). These additions reduce the size of the estimated segregation effect – especially for school segregation.

The most general specifications in columns C and F add controls for the black-white differences in several additional family characteristics, measured from 2000 Census data.³³ These models also include controls for the mean difference in residual wages between black and white parents, computed separately for men and women.³⁴ Neal and Johnson (1996) find that nearly all of the black-white gap in wages can be attributed to ability differences, as measured by test scores, so this control should absorb much of the otherwise-unobserved variation in black-white ability gaps. In these specifications the segregation effects are reduced somewhat but remain statistically significant.

The models in columns G-I of Table 3 include both segregation measures simultaneously. In the sparsest specification (G), school segregation appears to have the larger effect. When we add controls for metropolitan and SAT-taker characteristics, however, the school segregation coefficient falls to near zero and the residential segregation

³² The standard deviation of combined SAT scores is about 200, so the black-white gap is approximately one standard deviation, similar (in effect size) to the gap measured in the NAEP at ages 9, 13, or 17 (Perie, Morand, and Lutkus, 2005).

³³ Specifically, we control for the black-white difference in four parental education measures (the fraction of mothers and of fathers with some college and with college degrees), two family structure measures (the fraction of children living with one parent and with neither parent), the employment rate of children's mothers, the median income of families with children, and the fraction of children living in poverty. All are measured over children aged 0-17.

³⁴ We identified working adults with resident children age 18 or under in the 2000 Census PUMS. For each, we constructed an hourly wage (based on earnings and hours in 1999), then regressed this on MSA fixed effects, years of education, indicators for high school dropout and college graduation, and a cubic in potential experience, separately by race and gender. The residual wage measures are the MSA fixed effects. We have also estimated specifications that use raw wages, without adjustment for covariates. These yield similar results.

coefficient becomes large and negative. The sum of the two coefficients is quite close to the residential segregation effect in the corresponding models in Columns D-F. We can reject that the neighborhood segregation effect is zero, but not (in column I) that the two forms of segregation have equal effects.

Overall, it seems that residential segregation matters, but controlling for this, differences in relative exposure to minority schoolmates have little effect on black relative achievement. Taking the coefficient on residential segregation in column F of Table 3 as a benchmark, the implied effect of moving from a highly segregated city (Gary Indiana, $\Delta R_c=0.70$) to a nearly unsegregated city (Fort Walton Beach Florida, $\Delta R_c=0.06$) is a 45 point closing in the black-white SAT gap (or roughly a 0.22 “effect size”).³⁵

We have estimated many alternative specifications to probe the robustness of this conclusion. When we estimate separate models for black and white students’ test scores, we find approximately equal effects of the fraction black in black students’ schools on black test scores and the fraction black in white students’ schools on white test scores and zero “cross” effects, suggesting that neither the city-level fraction black nor the degree of segregation is correlated with unobserved determinants of city mean test scores. The basic results are invariant to different measures of school segregation (i.e., just for elementary or for high schools) and to several alternative strategies for dealing with selection into SAT participation. Card and Rothstein (2006) describe these tests in greater detail.

b. Effects on Educational Attainment

We can also examine black-white differences in educational attainment, which can be

³⁵ The -70 coefficient implies a $70/200 = 0.35$ standard deviation effect of a one-unit decrease in minority share in the neighborhood. For comparison, this implies that the -7.5 percentage point treatment effect on minority exposure in the MTO experiment should have yielded a 0.026 standard deviation effect on test scores. The estimated treatment effect on math scores (Sanbonmatsu et al., 2006, Table 5, row 1, column 5) was 0.018 (s.e. 0.030).

measured from Census data without the potential biases arising from selective SAT participation. We used the 2000 Census 5-percent micro samples to estimate the fraction of 16-24 year olds in each city who either are currently enrolled in school or have completed high school.³⁶ We then constructed the black-white gap in this outcome and related it to our city control variables and the segregation measures.

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

The simplest models in columns A-C suggest that there is a significant negative effect of neighborhood segregation on black youths' relative education outcomes. These findings are similar in spirit, though smaller in magnitude, to results reported by Cutler and Glaeser (1997), whose models include fewer controls.³⁷ The corresponding models in columns F-H suggest that controlling for neighborhood segregation, there is little or no additional effect of school segregation. Although imprecise, these estimates show the same

³⁶ To insulate against bias from endogenous mobility of young people who have left their parents' homes, we assign individuals to the MSA where they lived in 1995, when they were aged 11-19. A limitation of the Census data is that there is no family background information for children who are no longer living with their parents. Consequently, we make no individual-level adjustments for family background.

³⁷ Cutler and Glaeser (1997) use a 1% sample of the 1990 Census. They relate individual attainment of a high school diploma to race and its interaction with city-level residential segregation. This specification is equivalent to our city-level specification for graduation rate gaps, but Cutler and Glaeser allow only four other city-wide variables—log population, the fraction of blacks in the city, log median income, and manufacturing share of employment—to affect black relative attainment. Their estimates are much larger than even those in the sparsest specification in our Table 4.

pattern as our findings for test scores.

Nevertheless, examination of the richest specifications in Table 4 (columns D-E and I-J) suggests that inferences about the effects of segregation on educational attainment are sensitive to the set of background control variables. In particular, once the full set of relative background variables are added, the estimated impacts of neighborhood segregation on its own and of school and neighborhood segregation taken together fall in magnitude and become insignificant. By contrast, the models in Table 3 show robust negative effects of relative exposure to minority neighbors on black-white relative test scores. One potential explanation for the difference is that neighborhood segregation has smaller effects on basic achievement outcomes (like completing high school) than on higher-level achievement outcomes (like college entry test scores).³⁸ The Census outcome models have limited power against reasonable effect sizes, however, so it is difficult to reach definitive conclusions.

V. Confounding Influences

Tables 3 and 4 indicate that there is a relatively strong relationship between segregation and the black-white achievement gap, and that this relationship cannot be attributed to selective test participation. More tentatively, the link appears to run through neighborhoods rather than schools. In this section we investigate several potential biases that might lead us either to overstate the effect of neighborhood segregation or to understate the independent effect of school segregation.

a. Is the Effect of Neighborhood Segregation Overstated?

³⁸ An alternative explanation is that segregation is too stable over time for these specifications: By controlling for educational attainment of parents (i.e. of the previous generation, who were exposed to a level of segregation that is extremely highly correlated with the current measure), we may be absorbing the segregation effect on attainment.

The most obvious source of concern with the results in Table 3 is that there are unobserved differences in the latent abilities of black and white students in different cities that are correlated with the degree of segregation in the city. This sort of endogeneity could arise in two ways: Segregation in a city could arise endogenously as the consequence of the unobserved characteristics of a city's population, or across-city mobility into and out of segregated cities could depend on unobserved characteristics, differently for white and black families. Either would create a correlation between segregation and the black-white gap in student ability. Although we control flexibly for the observed education and income of SAT test takers' parents, and we also include controls for differences in family characteristics observed in the Census, it is still possible that unobserved ability gaps remain.

Recent work (e.g., Heckman and Carneiro, 2003; Cunha et. al, 2006; Neal and Johnson, 1996) has shown that the academic achievement of children is strongly correlated with the cognitive ability of their parents, and that cognitive ability is a key determinant of earnings. This research suggests that a useful proxy for the unobserved ability of a child (conditional on parental education) is the unexplained component of his or her parent's earnings. The models reported in the final columns of Tables 3 and 4 include black-white residual wage gaps as additional controls. Their inclusion has essentially no impact on the estimated segregation effects, suggesting that differences in the unobserved cognitive abilities of black and white parents in different cities are not biasing our main results.

The working paper version of this article (Card and Rothstein, 2006) presents further investigations of the possibility of endogeneity. Neither reverse causation nor selective mobility appears capable of accounting for our results: Segregation in a city is extremely stable over time, leaving little scope for responsiveness to current population characteristics, and there is no indication of the differential mobility of high-ability black families from

highly segregated cities that would be needed to create bias.

b. Is the Effect of School Segregation Understated?

Specifications that include both residential and school segregation suggest another source of potential bias. The two are highly correlated— $\rho=0.95$ —so substantial measurement error in either could severely bias their relative coefficients. Given the patterns seen in Table 3, measurement error in school segregation is a particular concern. To investigate this, we computed a (public) school segregation index using data 10 years prior to that used in our main analysis. The correlation between that and a similar measure computed over public schools for our cohort is above 0.95, suggesting that school segregation is extremely stable and reliable. Even when we residualize school segregation using the other variables in our models (including residential segregation), the correlation remains above 0.7. This suggests that school segregation effects are attenuated by at most 30% even in our richest models, and that measurement error is unlikely to account for our results.

As a second check, we present an instrumental variables analysis that isolates the component of school segregation that is attributable to court-ordered school desegregation programs implemented in the 1970s and early 1980s in many U.S. cities. There was a great deal of variation in the aggressiveness of the Court-ordered remedies that is plausibly exogenous to gaps in population characteristics. Moreover, the effects of these desegregation programs appear to persist even today (Reber, 2005). Our instrument is based on Welch and Light's (1987) estimate of the change in the “dissimilarity index”—an alternative index of racial segregation—for the schools in a city from the year prior to the

city’s major desegregation plan to the last year of implementation of the order.³⁹

Unfortunately, Welch and Light (1987) only collected data for large school districts, which typically serve the central city of larger MSAs. We multiply the change in dissimilarity in the covered district by its share of metropolitan enrollment. Thus, the instrument reflects both the “bite” of the main desegregation plan and the size of the desegregated district relative to the overall MSA.

Table 6 presents the segregation analysis using the 60 SAT-state MSAs with at least one district in the Welch and Light (1987) sample. Given the small sample size, we adopt a parsimonious specification. OLS estimates in Column A are quite similar to (but less precise than) those from our full sample and richer specifications. Column B shows the first stage estimate: After more than two decades the court orders—many of which are still in effect—continue to have sizable effects on observed measures of school segregation. Finally, column C shows the IV estimate. This is quite imprecise, but gives no indication that the OLS estimate is biased in such a way as to mask an underlying negative effect of school segregation.

V. Within School Segregation?

One potential explanation for our finding that school segregation has little or no effect on relative achievement is that in cities with highly segregated neighborhoods, school integration efforts are offset by programs and behaviors that lead to *within school* segregation (Clotfelter, Ladd, and Vigdor, 2003; Clotfelter, 2004; Eyler, Cook, and Ward, 1983).⁴⁰ As a

³⁹ This variable is set to zero for MSAs containing districts in the Welch and Light sample without a major desegregation plan; MSAs with no districts in the Welch and Light sample are excluded.

⁴⁰ Anecdotal evidence suggests that districts often create special programs to attract white students to high-minority schools or, alternatively, to avoid truly desegregating their school systems in the face of judicial oversight. As an example of the latter, the federal district court judge’s opinion in *People Who Care v. Rockford Board of Education*, 851 F. Supp. 905 (1993) states: “The court finds that the ability grouping and tracking

proxy for within-school exposure, we use data on course enrollment patterns from our SAT data set. SAT-takers are asked whether they have taken honors courses and whether they intend to claim advanced placement (AP) credit or course exemptions in college on the basis of high school work. Column A of Table 7 presents models for the black-white difference in the fraction of students in a city who intend to claim college-level credit in any subject, while Columns B through D present models for the difference in the fraction of students who indicated that they had taken honors courses in math, English, or any subject, respectively.

Increased school segregation has large, positive, marginally significant effects on the black-white gap in honors course taking and in AP participation. Increases in neighborhood segregation have negative effects, although the coefficients are mostly smaller and uniformly insignificant. To interpret these impacts, note that a rise in our segregation index implies that whites are relatively less exposed to minorities. Holding constant neighborhood segregation, white students are more likely to participate in “high track” courses (so the black-white gap in participation is more negative) when schools are more integrated, presumably limiting the classroom-level exposure of blacks to whites.⁴¹ Though participation rates in honors and AP courses are limited measures of within-school exposure, the results support the hypothesis that across-school integration is associated with within-school segregation. To the extent that school peer effects operate through classroom-level exposure, then, our school segregation measure may fail to capture the racial composition of the relevant peer groups.

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)

⁴¹ Unfortunately, our court-ordered desegregation instrument does not provide enough power to identify these effects with any precision in the IV specification used in Table 6.

VI. Indirect Effects of School Quality and Peer Characteristics

As noted in the discussion of equation (5), our coefficient estimates capture direct minority exposure effects as well as any indirect effects associated with relative resources and peer characteristics that can be predicted by the relative exposure of black and white students to minority peers. As a final step in our analysis we explore the potential contributions of one type of indirect effects: those arising from differences in the relative incomes of schoolmates and neighbors (Wilson, 1987).⁴² We use Census and CCD data to estimate the black-white gap in average neighborhood income and in exposure to schoolmates receiving free school lunches (a common though imperfect proxy for low income).

Columns A and B of Table 7 present models in which we regress these measures on our racial segregation indices. Column A shows that the black-white gap in exposure to schoolmates receiving free lunches is positively related to the relative segregation of the schools in a city but negatively related to the degree of neighborhood segregation. Thus, any negative effect of schoolmate poverty on test scores should contribute negatively to the estimated effect of school segregation but positively to the estimated effect of residential segregation. The model in column B shows that the black-white gap in mean neighborhood income is negatively related to neighborhood segregation but uncorrelated with school segregation. Any positive effect of neighborhood income on student achievement should therefore contribute negatively to the estimated residential segregation effect.

Columns D and E present models that assess these conjectures directly, by adding

⁴² We have also explored the relationship between segregation and school resources. Neither school nor residential segregation is associated with differences in funding for black versus white students' schools. Black students are more likely to have non-white teachers in cities with segregated schools, and less likely to have teachers with education majors (a negative quality indicator) in cities with segregated neighborhoods. Both effects are small, and in any case both appear to go the wrong way to account for our results.

the school lunch and neighborhood income measures to the specification shown in column I of Table 3 (reproduced here in Column C). Consistent with the pattern of results in Table 3, differential exposure to low-income *schoolmates*—at least using an admittedly limited proxy based on school lunch participation—has little effect on relative black test scores, while differential exposure to low-income *neighbors* seems to reduce black performance. After controlling for the indirect effect associated with neighborhood income, the estimated effect of residential segregation remains negative but is reduced by about one-half and is no longer statistically significant. These estimates thus suggest that an important share of the neighborhood segregation effect measured in our main specifications can be explained as an indirect effect of exposure to low income neighbors, rather than as a direct effect of exposure to minorities *per se*.

VII. Summary and Conclusions

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

Our main empirical evidence is based on SAT outcomes for one third of test takers in the 1998-2001 test cohorts. We match test-takers to information on the racial composition of their high schools and to an extensive set of family background characteristics of black and white students in their cities.

Considered separately, both school and neighborhood segregation have negative

effects on black relative achievement. 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 exposure to minority neighbors remain significantly negative. Probes into possible explanations for the absence of school segregation effects, including instrumental variables estimates based on court ordered desegregation programs, give no indication that our estimates are biased in a way that would obscure negative effects of school segregation.

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

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Appendix: Derivation of Selection-Corrected Estimation Model

The main problem that we confront in implementing a selection correction is that we do not have observations on students who do not take the exam. Thus, while we can estimate the fraction of students at each school who are observed, we have no information about variation in test participation rates within each school. We show here that the traditional selection correction aggregates to the city level in a straightforward way if all students at a school have the same propensity to take the exam, and that any within-school variation in the test-taking propensity appears as an omitted variable in our city-level analysis.

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:

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

where ω_{ijsc} is a normally distributed error component and k_{jsc} is a school and group-specific threshold. The error ϵ_{ijsc} in the test score outcome model may be correlated with ω_{ijsc} , but the two have the same joint normal distribution across schools. The expected test score for student i in group j in school s , conditional on writing the test, is

$$(A2) \quad E[y_{ijsc} \mid i \text{ writes test}, X_{ijsc}; s, j, c] = X_{ijsc} \alpha + B_{sc} \beta + Z_{sc} \gamma + M_{sc} \theta + \mu_{jc} + u_{jsc} + \zeta \lambda(p_{ijsc}),$$

where $\lambda(p)$ is the inverse Mills ratio function evaluated at $\Phi^{-1}(p)$ and ζ_j is a coefficient that depends on the correlation between ω_{ijsc} and ϵ_{ijsc} .

A simple average of the observed test scores in a city will contain a participation-weighted average of the school effects u_{jsc} 's. Although u_{jsc} has mean zero across all students in each city (by assumption), if the school-by-race participation rate is correlated with u_{jsc} the participation-weighted mean may be non-zero. To eliminate this, we reweight the data to

obtain enrollment-weighted averages of observed test scores, separately for black and white students:

$$(A3) \quad y_{jc} = 1/N_{jc} \sum_s N_{isc} y_{jsc} = 1/N_{jc} \sum_s N_{jsc}/M_{jsc} \sum_i y_{ijsc} = 1/N_{jc} \sum_s \sum_i p_{jsc}^{-1} y_{ijsc},$$

where N_{jc} is the total number of 12th graders of group j in city c , N_{jsc} is the number of 12th graders in school s , M_{jsc} is the number of test-takers in group j in school s , and $p_{jsc} = M_{jsc}/N_{jsc}$ is the test participation rate of group j in school s . Equation (A2) implies that:

$$(A4) \quad y_{jc} = X_{jc} \alpha + B_{jc} \beta + Z_{jc} \gamma + M_{jc} \theta + \zeta (1/N_{jc}) \sum_s \sum_i p_{jsc}^{-1} \lambda(p_{ijsc}) + \mu_{jc} + v_{jc},$$

where X_{jc} , B_{jc} , Z_{jc} , M_{jc} and μ_{jc} are the same as in Section 3 of the main text and v_{jc} is the city-race average of the deviation of e_{ijsc} from its conditional mean.

We do not observe the individual-level selection probability, p_{ijsc} , but must use its average for race- j students in school- s , p_{jsc} . Consider a first order expansion of the selection-correction function for individual i around p_{jsc} :

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

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:

$$\begin{aligned} (1/N_{jc}) \sum_s \sum_i p_{jsc}^{-1} \lambda(p_{ijsc}) &= (1/N_{jc}) \sum_s \sum_i p_{jsc}^{-1} \{ \lambda(p_{jsc}) + (p_{ijsc} - p_{jsc}) \lambda'(p_{jsc}) + \xi_{ijsc} \} \\ &= \lambda_{jc} + \tau_{jc} + \xi_{jc}, \end{aligned}$$

where

$$\begin{aligned} \lambda_{jc} &= (1/N_{jc}) \sum_s \sum_i p_{jsc}^{-1} \lambda(p_{jsc}), \\ \xi_{jc} &= (1/N_{jc}) \sum_s \sum_i p_{jsc}^{-1} \xi_{ijsc}, \\ \tau_{jc} &= (1/N_{jc}) \sum_s N_{jsc} \lambda'(p_{jsc}) (1/N_{jsc}) \sum_i (p_{ijsc} - p_{jsc}) \\ &= (1/N_{jc}) \sum_s N_{jsc} \lambda'(p_{jsc}) \{ \mathbf{p}_{jsc}^T - p_{jsc} \}, \end{aligned}$$

and \mathbf{p}_{jsc}^T is the average test participation probability *among the test writers of group j in school s* .

Note that λ_{jc} is an enrollment-weighted average of the inverse Mills ratio functions evaluated

at the (race-specific) test participation rates at each school. ξ_{jc} is an average approximation error, which we expect to be small. τ_{jc} is more problematic. This term measures the degree of “within-school” selectivity of test-takers. It disappears if test participation rates are uniform within a school, but is strictly positive otherwise.⁴³

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

$$(A5) \quad y_{jc} = X_{jc} \alpha + B_{jc} \beta + Z_{jc} \gamma + M_{jc} \theta + \zeta \lambda_{jc} + \zeta \tau_{jc} + \mu_{jc} + \nu_{jc}.$$

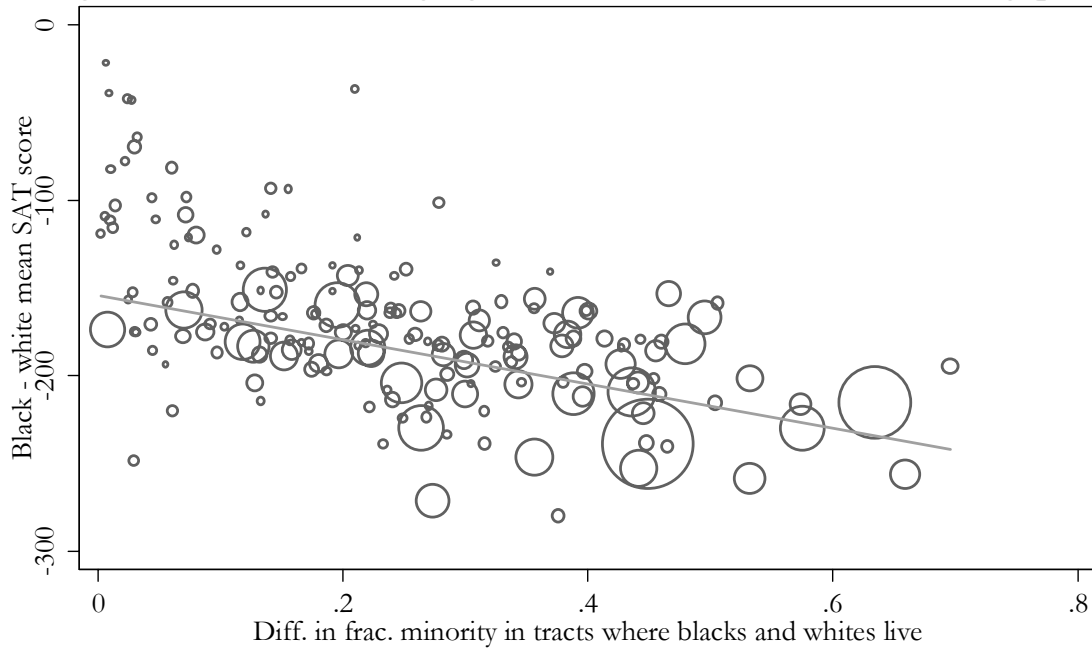
Differencing between blacks and whites in the same city and substituting equation (3) from the main text for the difference in the unobserved ability components leads to:

$$(A6) \quad \Delta y_c = y_{1c} - y_{2c} = \Delta X_c \alpha + \Delta B_c \beta + \Delta Z_c \gamma + \Delta M_c \theta + F_c \psi + \zeta \Delta \lambda_c + \zeta \Delta \tau_c + \Delta \nu_c + \eta_c.$$

Omission of the ΔZ_c and ΔM_c variables and inclusion of a neighborhood segregation measure, as discussed in Section III, produces equation (7) in the text.

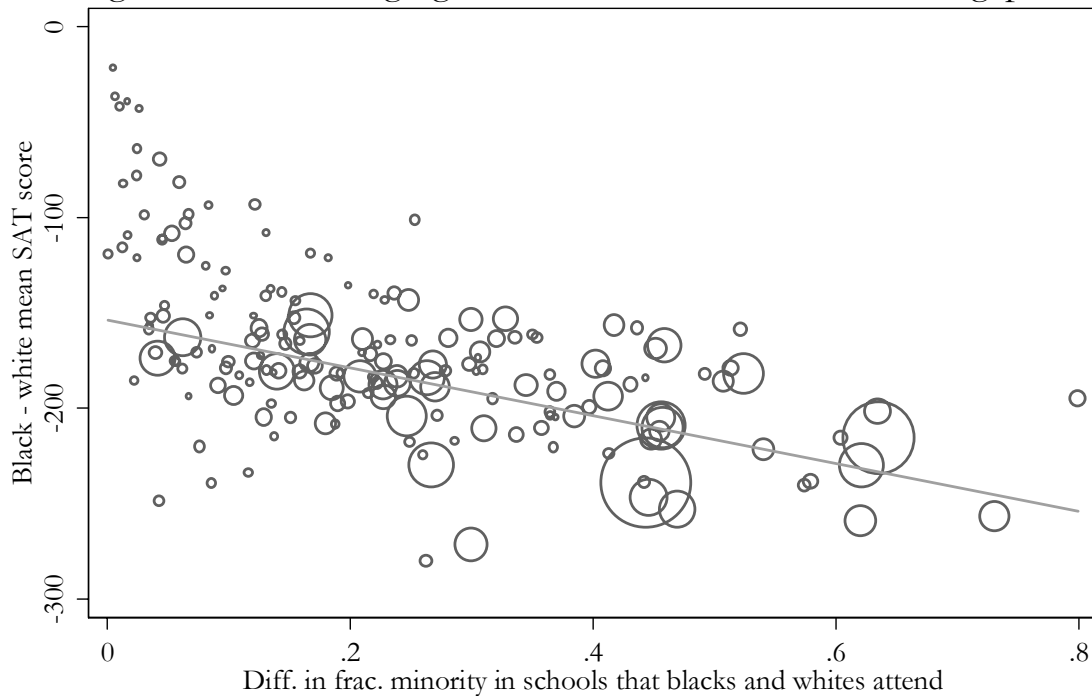
⁴³ As developed here, differences between p_{jisc}^T and p_{jisc} reflect differences in the X characteristics of test-takers and non-test-takers at the school. We cannot estimate the within-school selection probability because we lack data on the X characteristics of non-takers. Our main specifications include controls for the mean characteristics of both SAT-takers and all students in each city. The difference between these likely helps to absorb the τ term.

Figure 1. Residential segregation and black-white SAT score gap



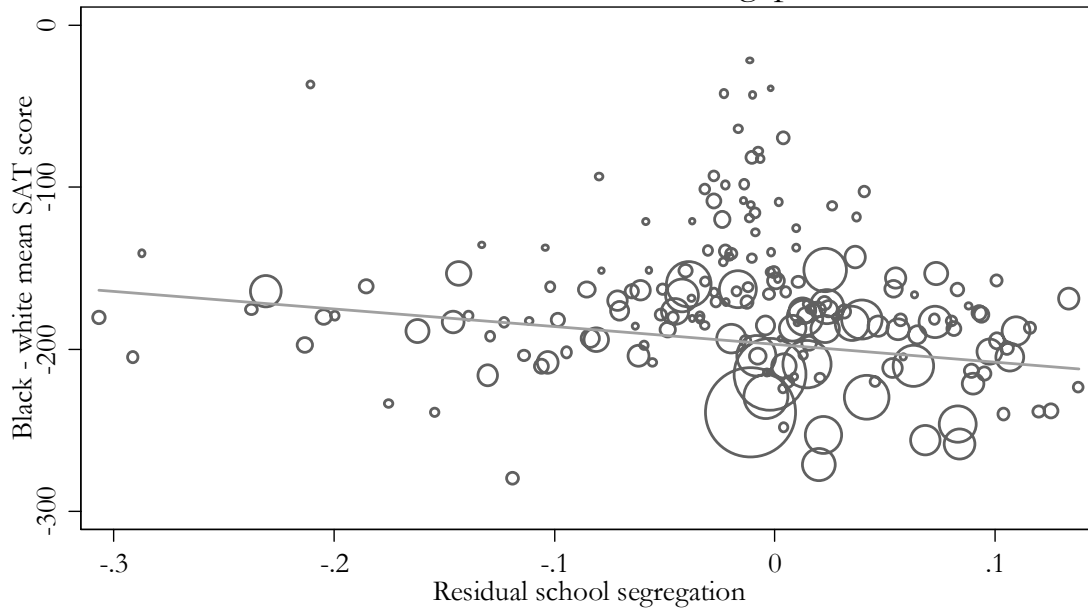
Notes: Sample is metropolitan areas in SAT states. Circle areas are inversely proportional to the sampling error variance in MSA black-white gaps (see notes to Table 1 for details). Line is the weighted least squares regression line (slope -127, s.e. 13).

Figure 2. School segregation and black-white SAT score gap



Notes: See notes to Figure 1. Line is the weighted least squares regression line (slope -125, s.e. 12).

Figure 3. School segregation unexplained by residential segregation and black-white SAT score gap



Notes: See notes to Figure 1. "Residual school segregation" is the residual from a weighted bivariate regression of school segregation on residential segregation (coeff. 0.99, s.e. 0.03). Line is the weighted least squares regression line (slope -109, s.e. 39).

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

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

Notes: Segregation rankings in first two panels are by difference in fraction minority (black and Hispanic) in black and white students' schools, as in column E. In second two panels, rankings are by the residual from a regression of this measure on an analogous measure computed over census tracts (column H). In each case, the 5 most-segregated and least-segregated cities in SAT states with at least 5% black population are shown. Average listed in bottom row is over all 119 cities meeting these criteria and is weighted by $(N_w^{-1} + N_b^{-1})^{-1}$, where N_w and N_b are the number of white and black residents of the MSA, respectively.

Table 2. Summary statistics, metropolitan areas

	All Cities		In SAT states	
	Mean	S.D.	Mean	S.D.
	(A)	(B)	(C)	(D)
N	331		189	
Population (millions)	2.856	3.010	3.042	3.168
Fraction black	0.12	0.09	0.11	0.08
Fraction Hispanic	0.21	0.21	0.25	0.23
log(Mean HH income)	10.98	0.19	10.99	0.20
Segregation (Black fraction minority - white fraction minority)				
Residential (Tract), 2000	0.34	0.19	0.32	0.20
Elementary schools, 1998-2001 cohorts	0.36	0.21	0.35	0.20
High schools, 1998-2001	0.31	0.19	0.29	0.18
School career avg., 1998-2001 cohorts	0.35	0.20	0.33	0.19
SAT-taking rate				
All students	0.28	0.14	0.34	0.10
White students	0.32	0.14	0.39	0.09
Black students	0.22	0.12	0.27	0.08
SAT-takers				
Avg. SAT	1033.4	71.3	999.3	46.0
Black-white avg. SAT	-193.2	36.5	-194.0	34.3
Black-white avg. SAT (reweighted)	-203.0	42.1	-197.7	36.0

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 SAT score difference

	School Segregation			Residential Segregation			School & Nbhd. Segregation		
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)
Black-white difference: Fr. minority in students' schools	-142	-78	-43				-102	-10	-7
	(25)	(24)	(19)				(37)	(27)	(25)
Black-white difference: Fr. minority in residents' nbhds.				-136	-111	-70	-46	-103	-63
				(23)	(25)	(20)	(31)	(27)	(24)
MSA demographic characteristics	n	y	y	n	y	y	n	y	y
B-W background controls, SAT takers	n	y	y	n	y	y	n	y	y
B-W background controls, 0-17 year olds in Census data	n	n	y	n	n	y	n	n	y
B-W difference in residual parental wages	n	n	y	n	n	y	n	n	y
N	185	185	185	185	185	185	185	185	185
R-squared	0.60	0.82	0.86	0.58	0.83	0.87	0.60	0.83	0.87
p-value, residential=school							0.38	0.05	0.21
p-value, residential=school=0							0.00	0.00	0.00

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

MSA demographic characteristics: Log population, land area, fraction of adults with BAs and with some college, log mean household income, and (household-level) Gini coefficient.

Background controls, SAT-takers: Background index estimated from within-school regressions (see Appendix for details), fraction of fathers and mothers with some college and with college degrees, and family income (scaled in SAT points).

Background controls, 0-17 year olds in Census: Fraction of youths' (resident) mothers and fathers with some college and college degrees, fraction of children in one-parent families and living without either parent, maternal employment rate, median family income, and child poverty rate.

Residual parental wages: MSA fixed effects from regressions of wages on education, dummies for HS graduation and college completion, and a cubic in potential experience, estimated separately for each race/gender using parents with resident children under 18.

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

	Neighborhood Segregation					School & Neighborhood Segregation				
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)	(J)
B-W fr. minority in students' schools						-1.8 (4.0)	-2.0 (3.5)	-3.2 (3.0)	-1.1 (2.9)	-1.3 (2.9)
B-W fr. minority in residents' neighborhoods	-6.0 (1.9)	-11.8 (3.0)	-5.6 (2.5)	-3.2 (2.6)	-2.8 (2.6)	-4.4 (3.8)	-10.0 (4.1)	-2.7 (3.8)	-2.3 (3.7)	-1.6 (3.7)
<i>Control variables</i>										
MSA demographic characteristics	n	y	y	y	y	n	y	y	y	y
B-W gaps in parental educ.	n	n	y	y	y	n	n	y	y	y
B-W gaps in other observables	n	n	n	y	y	n	n	n	y	y
B-W gap in residual parental wages	n	n	n	n	y	n	n	n	n	y
N	234	234	234	234	234	234	234	234	234	234
R-squared	0.30	0.37	0.50	0.55	0.55	0.30	0.37	0.50	0.55	0.55
p-value, residential=school						0.73	0.26	0.94	0.85	0.96
p-value, residential=school=0						0.01	0.00	0.03	0.42	0.49

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Dependent variable is the difference between blacks and whites in the percentage of youth who have finished HS or who are enrolled in school, measured over 16-24 year olds in the 2000 census who lived in the metropolitan area in 1995. Sample excludes MSAs with fewer than 50 black or 50 white observations. The persistence gap ranges in principle from -100 to 100, and has sample mean -6.9 and S.D. 3.8. Specifications are as in Table 3, except that SAT-taker background controls (and the inverse Mills ratio) are excluded and Census background measures from Table 3 are here introduced in two stages. All standard errors are clustered on the CMSA.

Table 5. Instrumental variables estimates of school segregation effect on black-white SAT score gap

	Residential & school		
	OLS	1st stage	IV
	(A)	(B)	(C)
B-W fr. minority in students' schools	-18 (36)		37 (98)
B-W fr. minority in residents' neighborhoods	-113 (30)	0.93 (0.09)	-164 (80)
Change in dissimilarity index induced by major desegregation plans (/100)		0.23 (0.06)	
N	60	60	60

Notes: Models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$ and standard errors are clustered on the CMSA. Control variables are those in Column G of Table 3, plus the black-white difference in a background index computed over SAT-takers using within-school variation (see appendix for details).

Table 6. Residential and school segregation effects on black-white gaps in honors course-taking among SAT-takers

	Plan to claim adv. / exempt status in any subject	Took honors courses		
		Math	English	Any subject
	(A)	(B)	(C)	(D)
B-W fr. minority in students' schools	12.3 (6.4)	10.2 (7.5)	14.8 (7.3)	12.4 (7.8)
B-W fr. minority in residents' census tracts	-4.4 (8.9)	-5.3 (7.6)	-11.4 (8.8)	-14.9 (8.6)

Notes: Dependent variables are the black-white gap in the fraction of SAT-takers with the specified characteristic. All columns include the controls used in Column I of Table 3, less those for gaps in residual wages. Models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$, and standard errors are clustered on the CMSA.

Table 7. Relationship between racial segregation and income differences between black & white students' schools and residents' tracts

	Dependent variable: B-W Gap in				
	Fr. free lunch in school	ln(per capita income) in tract	Mean SAT score		
	(A)	(B)	(C)	(D)	(E)
Black-white difference: Fr. minority in students' schools	0.64 (0.05)	0.03 (0.05)	-7 (25)	-16 (25)	-10 (25)
Black-white difference: Fr. minority in residents' nbhds.	-0.25 (0.06)	-0.34 (0.08)	-63 (24)	-59 (26)	-34 (27)
Black-white difference: Fr. free lunch in students' schools				9 (41)	
Black-white difference: ln(per capita income) in residents' nbhds.					61 (28)
N	292	323	185	176	185
R-squared	0.94	0.91	0.87	0.87	0.87
p-value, residential=school	0.00	0.00	0.21	0.36	0.62
p-value, residential=school=0	0.00	0.00	0.00	0.00	0.15

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