

New Evidence about *Brown v. Board of Education*: The Complex Effects of School Racial Composition on Achievement

by Eric A. Hanushek, John F. Kain, and Steven G. Rivkin*

June 2007
(revised January 2008)

Abstract

Although the goals of school integration legally mandated by *Brown v. Board of Education* are very broad, here we focus more narrowly on how school racial composition affects scholastic achievement. Uncovering this effect is difficult, because racial mixing in the schools is not an accident but rather an outcome of both government and family choices. Our evaluation, made possible by rich panel data on the achievement of Texas students, disentangles racial composition effects from other aspects of school quality and from differences in abilities and family background. The pattern of estimates provides strong support for the notion that a higher percentage of black schoolmates reduces achievement for blacks, particularly those with higher initial achievement, while the coefficients for whites are typically much smaller and not significant at conventional levels. Given the existing level of segregation in Texas, racial composition appears to explain a meaningful portion of the racial achievement gap. The underlying causes of the link between achievement and peer racial composition remain uncertain, but the evidence indicates that racial composition does not serve as a proxy either for peer academic achievement or for unmeasured school quality.

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I. Introduction

Five decades after the landmark 1954 school desegregation case of *Brown v. Board of Education*, a surprising amount of uncertainty still exists about the ultimate effects of school desegregation on academic, social, and labor market outcomes for both minority and white students.¹ The ruling in *Brown* held that separate but equal was unconstitutional in the case of education and led to dramatic changes in schools throughout the country. This paper investigates one fundamental underlying presumption of that historic legal decision – that school racial composition directly affects student outcomes and thus the black-white achievement gap.

Legal forces and the residential location decisions of households have combined to shape the racial composition of schools. The seminal work of Welch and Light (1987) documented both the desegregation of many school districts following *Brown* and subsequent Supreme Court decisions and the countervailing white exodus from many cities and towns that dampened the impact of school desegregation on interracial contact. There was considerable variation across the US in the intensity of desegregation efforts and the extent of white flight, both of which contribute to the substantial differences across jurisdictions in school attendance patterns today.²

Over the last decade demographic changes have led to a decline in the average share of blacks' schoolmates who are white despite declining school and residential segregation in most parts of the country.³ The recent retreat of the U.S. Supreme Court from issues related to affirmative action and racial assignment policies may amplify this trend. Through all of this,

¹ *Brown v. Board of Education*, 347 U.S. 483 (1954).

² See also the analyses of Coleman, Kelley, and Moore (1975), Clotfelter (1976, (2004), and Reber (2005).

³ Rivkin and Welch (2006) describe changes in school segregation, and Iceland and Weinberg (2002) describe changes in residential segregation.

however, we have had very little understanding of how racial composition of schools – the focal point of *Brown* – impacts on the learning of African Americans or the racial achievement gap.⁴

In Texas public schools, the focus of our analysis, the black enrollment share remained at approximately 15 percent over the period 1968 to 1998, while the white enrollment share fell precipitously (from 64 to 45 percent), largely offset by growth in the Hispanic enrollment share.⁵ Even so, the average percentage of blacks' schoolmates who were white increased from 24 to 35 percent between 1968 and 1980 before slipping back to 31 percent in 1998. As with the nation, the unequal distribution of blacks across schools today results primarily from residential separation across districts rather than from unequal school distributions within districts.⁶

Again similar to the U.S. as a whole, the average achievement for blacks is substantially below that of whites in Texas. For example, the average mathematics score for black seventh graders falls 0.7 standard deviations below that of whites, or at the 24th percentile of the white distribution.⁷ Further, only 29 percent of blacks score in the top half of the state distribution (see Appendix Table A1).

In this paper we investigate the impact of racial composition on test scores and the racial test score gap. The purposeful government programs to reallocate students among schools in combination with efforts of both blacks and whites to procure particular types of neighborhoods and schools clearly complicates the identification of racial composition effects. We use stacked

⁴ As evidence of the uncertainty, when the U.S. Supreme Court during its 2006-2007 term considered two cases about the race based school assignment policies employed in Louisville and Seattle, a large proportion of the 64 *amicus curiae* briefs discussed potential effects on student outcomes, but these show little consensus about research evidence on either the direction or magnitude of any impacts; see the evaluations in Linn and Welner (2007). *Parents Involved in Community Schools v. Seattle School District No. 1* and *Meredith v. Jefferson County Board of Education* were decided by the U.S. Supreme Court on June 28, 2007.

⁵ The description of the changing racial and ethnic composition of Texas schools along with the data sources and computational details is found in Hanushek, Kain, and Rivkin (2002b).

⁶ Rivkin (1994) shows that in 1988, even if all U.S. school districts had been perfectly integrated such that each school had the district share of all racial groups, housing patterns would still have led to large numbers of blacks having few white schoolmates. Dissimilarity indices from Texas show the same.

⁷ The comparable black-white mathematics score gap for students age 13 in 1996 for the nation is 0.9 standard deviations (U.S. Department of Education (2000)). The gap in Texas state NAEP scores is, however, less than that for the nation (U.S. Department of Education (1997)).

panel data on performance and racial composition for multiple cohorts of Texas public school students to isolate arguably random variation in the racial composition of schools that results from both persistent cohort demographic differences within schools and student mobility. Because of continued debate over the appropriate structure of empirical models of achievement, we compare results for alternative methods of controlling for unobserved student heterogeneity that potentially confound estimates of racial composition effects.

Our empirical analysis shows that the black enrollment share adversely affects achievement, and the effects are roughly twice as large for blacks as for whites. The pattern of results strongly suggests that racial composition is not serving as a proxy for peer achievement, leaving the precise causal linkages that underlie the relationship between achievement and racial composition uncertain. Finally, the key component of racial composition is the black enrollment share, with concentrations of other minority groups, notably Hispanics, exerting a much smaller effect that is not significantly different from zero in most specifications.

The magnitude of our estimates suggests that the elimination of all differences in the black enrollment share in Texas public schools for grades 5-7 (corresponding to our observation period) would close over 10 percent of the seventh grade black-white test score gap (i.e., moving from 0.7 to 0.61).⁸ However, the reduction of a 30 percentage point difference in school proportion black is a sizeable change that would likely involve involuntary student movements and might well alter the relationship between achievement and proportion black estimated from the existing distributions of blacks and whites. Moreover, as noted, a majority of the uneven distributions of blacks and whites in the schools comes from racial differences in the pattern of residencies among districts and not from attendance patterns within districts – thus limiting the scope of policy actions.

⁸ If the impacts of racial composition held for all earlier grades, the comparable closing of the gap for an even distribution of blacks from grade 1 through 7 would be even larger.

II. Prior Research on Racial Peer Effects

The only social science evidence of harm from school segregation cited by the U.S. Supreme Court in *Brown* involved psychological studies of black children that related low self-esteem to segregated schooling.⁹ Most early (post-*Brown*) analyses focused on short run effects of purposefully moving students, including the effects of desegregation on achievement, self-esteem, and racial attitudes (Crain and Mahard (1978), Cook (1984), Armor (1995)). More recently, Guryan (2004) examined the impact of school desegregation on the probability of dropping out of high school.

The research most directly related to our work focuses on whether peer racial composition, as opposed to desegregation actions per se, affects achievement of blacks as well as other demographic groups. The landmark legislatively mandated civil rights report *Equality of Educational Opportunity* (Coleman et al. (1966)) and its offshoot (U.S. Commission on Civil Rights (1967)) provided early empirical evidence that racial isolation harms academic achievement, although (Armor (1972)) raises questions about the findings. Subsequent work by Crain (1970), Hanushek (1972), Boozer, Krueger, and Wolkon (1992), Grogger (1996), Hoxby (2000), and Hanushek and Raymond (2005) also finds that school racial composition affects academic, social, and economic outcomes. On the other side, Rivkin (2000) finds no evidence that exposure to whites increases academic attainment or earnings for black men or women in the high school class of 1982, and Cook and Evans (2000) indicate that little of the black-white difference in National Assessment of Educational Progress scores can be attributed to racial concentration. A comprehensive review finds the evidence on achievement and psychological differences to be very mixed (Schofield (1995)). As highlighted in the next section, the difficulty of isolating exogenous variation in racial composition likely contributes to the disparate findings.

⁹ Footnote 11 of *Brown* refers to the doll studies of Kenneth and Mamie Clark (Clark and Clark (1939)) that found that blacks in the segregated South tended to identify with white dolls and not black dolls.

Finally, a recent investigation of racial peer influences by Angrist and Lang (2004) exploits the potential impacts of the Massachusetts voluntary interdistrict integration program (Metco) on students in the receiving districts. They find little evidence that white students in the receiving district are affected by added blacks entering through the Metco program, although blacks in the receiving district appear more sensitive to the influx of lower achieving black students.

III. Methodology

Uncovering the effect of school racial composition on achievement is difficult primarily because racial mixing in the schools is not an accident but rather an outcome of both government and family choices. Some families have the opportunities and desire to live in racially mixed neighborhoods and others do not; some participate in voluntary or involuntary desegregation programs and others do not; some districts and communities pursue aggressive desegregation efforts and others actively resist such programs. These and other factors entering into residential location decisions impede efforts to isolate exogenous variation in racial composition that can be used to identify its causal effect on student outcomes.

Extensive prior work into the effects of class size, teacher characteristics, peer turnover, and other school and peer variables indicates that typically available variables provide inadequate controls for confounding influences related to both the outcome and causal factor of interest. Since numerous actors and institutions combine to determine the allocation of students among schools by race, the deficiency of single equation models – while large for peer effects in general – is likely to be even larger in the study of racial composition. Consequently, alternative methods are required to isolate exogenous variation in racial composition.

Our approach takes advantage of the stacked panel data from the Texas Schools Project to account for systematic factors related to choices by schools and parents that affect both the racial composition of schools and achievement. Because the quality of education may vary systematically by race within schools, we estimate separate regressions for blacks and whites and

compare the results to those produced by regressions over pooled samples. In the separate regression case the fixed effects for schools and grades vary by race, while in the latter case these fixed effects are constrained to be equal for blacks and whites.

A. Empirical Model of the Impact of Racial Composition

Equation (1) highlights the key identification issues that must be addressed in the absence of random assignment. Here achievement A for a black (white) student i in grade G and school s in year y is modeled as a function of student, family, school, and peer factors:

$$(1) \quad A_{iGsy} = \alpha_{iGy} + \beta X_{iGsy} + \delta S_{iGsy} + \lambda b_{iGsy} + e_{iGsy}$$

where b is racial composition in grade G , X and S are vectors of flows of contemporaneous family background and school inputs during grade G , α is an individual intercept specific to grade G in year y which captures the cumulative effects on each student of family, neighborhood, and school experiences, and e is a stochastic term capturing other unmeasured influences.¹⁰ If b were uncorrelated with e and α , OLS would yield an unbiased estimate of λ . But as noted above, the complications inherent in the determination of the distribution of peer racial composition in combination with existing evidence on peer, teacher, and school effects on achievement strongly suggest that typically available variables contained in X and S will not account adequately for potentially confounding factors.

Our basic approach for the estimation of λ is to use the panel data methods to control for race specific student, family, school, and community factors that could potentially bias the estimated racial composition effects, leaving only exogenous variation in racial composition to identify the parameter estimates. We begin by expanding the error term e from equation (1) into a series of components in order to highlight both the types of school and neighborhood factors accounted for directly by the panel data methods and those factors that remain unaccounted for:

$$(2) \quad e_{iGsy} = \omega_s + \xi_G + \psi_y + \rho_{Gy} + \pi_{sG} + \varphi_{sy} + \tau_{sGy} + \varepsilon_{iGsy}$$

where the first three terms are fixed school (ω), grade (ξ), and year (ψ) effects, the next three terms (π, φ, ρ) are second level interactions among these three components, the seventh term (τ) is the third level interaction, and the final term (ε) is a random error.

The school fixed effect (ω) captures time invariant differences in neighborhoods and schools, many of which are likely related to both achievement and school racial composition. These include school facilities, public services, community type, and working conditions that influence teacher supply. The grade, year, and year-by-grade fixed effects (ξ, ψ, ρ) account for statewide trends in racial composition and achievement by grade and year and other factors including changes in test difficulty.

Because school quality may vary over time and by grade for each school, Equation (2) also includes interactions between school and both grade and year. The school-by-grade component captures any systematic differences across grades in a school that are common to all years, and the school-by-year term accounts for systematic year-to-year differences that are common to all grades in a school. The school-by-grade fixed effects (π) account not only for school specific influences such as the curricular structure of instruction but also such possibilities as achievement and racial composition varying systematically with age, as would be the case if white exit from schools rises at the same age as achievement of blacks declines (say, because of peer or community influences). The school-by-year fixed effects (φ) remove in a very general way not only school specific performance trends but also idiosyncratic variation over time in school administration and in neighborhood and local economic conditions that likely affect mobility patterns including such things as the introduction of new race-related school policies or the myriad changes documented to occur in “transitional neighborhoods.” For example, an

¹⁰ We exclude the small number of students retained in grade in order to avoid problems introduced by the non-comparability of test results across grades and years.

economic shock that reduces neighborhood employment and income would not bias the estimates; nor would a shock to local school finances or the quality of the local school board, because each of these would affect all grades in a school.

The seventh term, τ , is the full three-way interaction between school, grade, and year; it cannot be included in our estimation, because there would be no variation left in racial composition across time or grades.¹¹ Ignoring this three-way interaction means that grade specific variation over time in school average teacher quality or other achievement determinants could potentially bias the estimates. Yet, because teacher assignments and other relevant aspects of school decisions are frequently not known until immediately prior to the beginning of school year, we do not expect changes over time in school and teacher quality for specific grades to be systematically linked with yearly changes in racial composition through parental behavioral responses.¹² We do include information on teacher experience and class size, because these variables (which have been shown to be significant determinants of achievement) might be incidentally linked to racial composition. The sensitivity of the racial composition estimates to these controls provides information about the likely effects of both observed and unobserved changes in school and teacher quality not accounted for by the included fixed effects.

The variation used to identify the parameter estimates for racial composition can be illustrated by considering a single school. (In a more general case with multiple schools, the

¹¹ We restrict attention to variations in racial composition at the grade rather than classroom level. We believe there are conceptual reasons for doing this, but we also have no alternative because our data do not support classroom specific analysis. The complication of any classroom analysis comes from selective placement of students into classrooms, which responds to the choices of school administrators and the preferences of parents and which is likely to reflect some influence of racial composition and parental bargaining skills. The estimator employing grade level aggregation is closely related to the use of grade average percent black as an instrumental variable, although the IV estimator would capture black-white differences in grade average classroom composition. Clotfelter, Ladd, and Vigdor (2003) find significant variations in the racial composition of classrooms by district, school, classroom, and academic track in middle school but much less so in primary school. Their descriptive analysis does not address implications for student performance, but, given our inclusion of school-by-year and school-by-grade fixed effects in regressions estimated separately by race, any such variation likely has a negligible effect on the estimates in this paper.

coefficients would reflect the average of these within school relationships across the sample). With multiple years of data for one grade, we could use cohort differences in achievement and racial composition to identify the racial composition effect entirely within a grade of the school. However, unobserved changes over time could bias the estimates produced by this “school-by-grade fixed effects” model that removes any variation across schools and grades within the school. Alternatively, with multiple grades of data for a single year, we could use grade differences in achievement and racial composition within the school to identify the racial composition effect. However, systematic differences by cohort or grade could bias the estimates produced by this “school-by-year fixed effects” model.

Fortunately, the availability of data for multiple years and grades permits the simultaneous inclusion of school-by-grade and school-by-year fixed effects. In this case the racial composition effect is identified by deviations from a school’s average racial composition for each grade and year. Although this eliminates primary sources of bias, the possibility that unobserved differences by grade and year including test difficulty and grade specific policy changes at the district or state level could still contaminate the estimates. But, the availability of data for a number of schools enables us to control for average grade-by-year effects across all schools.

In this framework, the remaining variation in racial composition comes both from students switching schools and from persistent cohort-to-cohort differences reflecting natural demographic variations in cohort composition within schools. Because either of these sources of variation may be systematically related to racial composition and achievement, we must further account for mobility and other components of student heterogeneity directly.

¹² Among other things, transactions costs and the presence of multiple children in the majority of families would tend to limit family mobility in response to concerns about school quality for a single grade even if relevant teacher and classroom assignments were known in a timely manner.

Mobility induced changes, although frequently ignored in achievement research based on cohort comparisons, present particularly serious problems in studying racial composition.¹³ Hanushek, Kain, and Rivkin (2004a) show that blacks are much more likely to move than whites and thus are likely to contribute disproportionately to year-to-year changes in school racial composition. Moving to a new school tends to affect adversely both the movers and non-movers, particularly if the move occurs during the academic year.¹⁴ Moreover, the evidence shows that movers tend to have lower prior achievement, indicating that determinants of learning in prior periods were less conducive to achievement and potentially altering any effects of peer achievement. Consequently, the effects of mobility and unobserved student differences would tend to lower average achievement for blacks in schools that experience increases in the black enrollment share. Finally, high in-migration to a school may raise class size, introducing another avenue for spurious correlation.

In order to purge these contaminating influences, we control directly for the effects of moving on school changers with a vector of mobility variables that allow for different effects by timing, number, and type of move (elements of X),¹⁵ for the external effects of peer turnover with measures of both the proportion of students new to the school at the start of the year and the proportion who enter during the school year; and for teacher/organizational factors with measures of teacher experience and class size (elements of S).

¹³ An identifying assumption in a number of studies that make use of cohort differences is that either raw cohort differences or differences remaining following the removal of school specific trends over time are not correlated with confounding factors. This approach, which builds on the intuition that students close in age in the same school have many similar experiences, has been used in a variety of circumstances (e.g., Ehrenberg and Brewer (1995), Ferguson and Ladd (1996), and more recently generalized by Hoxby (2000)).

¹⁴ Hanushek et al. (2004a) show that moving students tend to suffer academically in the year of a move and that higher aggregate turnover in a school has a negative impact on all students. Moreover, black students have higher mobility rates and attend schools with significantly higher student turnover than whites. Hanushek, Kain, and Rivkin (2004b) also find that year-to-year differences in student racial composition and student demographic variables affect teacher transitions as well.

¹⁵ Indicator variables differentiate both among those moving during the summer, school year, or at least twice in the same year and among within district changes, district changes within geographic region, and moves across regions.

The key remaining issue is the appropriate method for controlling for student-specific heterogeneity, α in Equation (1). Equation (3) specifies α as a function of prior school and family variables, racial composition in previous grades, and unobserved “ability” γ .¹⁶

$$(3) \quad \alpha_{iGy} = \beta \sum_{g=1}^{G-1} \theta^{G-g} X_{igy} + \delta \sum_{g=1}^{G-1} \theta^{G-g} S_{igy} + \lambda \sum_{g=1}^{G-1} \theta^{G-g} b_{igy} + (\gamma_i + \sum_{g=1}^{G-1} \theta^{G-g} \gamma_i)$$

This formulation captures the notion that the families, communities, and schools exert cumulative effects that establish the knowledge base at the start of grade G and therefore affect achievement at the end of grade G.¹⁷ The effects of prior period variables are assumed to decline exponentially as a function of time from the present at a constant rate $(1-\theta)$, where $0 \leq \theta \leq 1$. This formulation subsumes most commonly estimated specifications of achievement models. At the extreme of $\theta = 0$, past inputs are not relevant for current achievement, i.e., having a good fourth grade teacher does not have any implications for math achievement at the end of the fifth grade.¹⁸ On the other hand, $\theta = 1$ implies no depreciation of the influence of past inputs, i.e., that the impact of a good fourth grade teacher on 4th grade achievement equals her impact on 5th grade achievement and achievement in all future grades.

The term γ is a student fixed effect that is added to reflect the panoply of early childhood influences, prenatal care, heredity, and other factors – factors often jointly referred to simply as innate ability – that have a continuing influence on learning. Notice that our formulation is

¹⁶ Boardman and Murnane (1979) and Todd and Wolpin (2003) also highlight the importance of unobserved ability and the cumulative nature of learning.

¹⁷ This representation makes clear the interpretation of the various inputs (X , S , and b). These represent the flow of these inputs in each grade, while the cumulative inputs in equation 3, appropriately weighted, provide the stock of each input prior to grade G. At times the flows are measured by the level of specific inputs that do not change frequently, such as the educational attainment of parents, but the conceptual idea is that parents with different educational attainment provide differing flows of inputs to their child’s learning. Moreover, with separation and new family relationships, these inputs can themselves vary over time.

¹⁸ This formulation does constrain the dynamics of the educational process by assuming that the impact of past inputs comes entirely through the effects on educational outcomes. Thus, for example, the inspirational fourth grade teacher has her effects on sixth grade performance come entirely through her impact on (discounted) fourth grade achievement and not through changing future learning patterns and implicitly the impact of future inputs. The alternative model would have varying dynamic impacts of

learning-based in that the value of γ affects the quantity of skills and knowledge acquired at each grade, and these increments to achievement are subject to depreciation. This explicitly permits the affects of ability on achievement to increase with age. The exact formulation and interpretation depends, however, on the measurement of achievement. If measured with vertically integrated tests, differences in γ would contribute to a widening of the skill distribution over time as long as θ were not equal to zero.¹⁹ On the other hand, if skills were measured by location in the distribution (as we do here with standardized scores), the complicated final term in parentheses could be replaced with γ_i , because ability induced differences in relative achievement would remain constant over time.²⁰

Equation (3) includes a mixture of time invariant and time varying differences that could potentially bias estimates of racial composition effects if not incorporated directly into the estimation. But, as can be readily seen by writing equations 1 and 3 for grade G-1, including the student's prior test score on the right hand side of equation 1 captures much if not all of the student heterogeneity that might be systematically related to racial composition without imposing an assumption about the value of θ . Only the effect of contemporaneous ability γ is not directly accounted for by lagged achievement. Thus, a key identifying assumption is that any variation in γ not correlated with the prior test score is orthogonal to the variation in teacher and school characteristics that remains following the inclusion of the multiple levels of school fixed effects. We have little reason to believe that, conditional on prior score, schools or parents act to alter grade average teacher or peer characteristics in ways that are related

different past inputs, but such a model would be extraordinarily difficult to estimate given our strategy of dealing with unmeasured inputs through various fixed effects.

¹⁹ In testing terms this implies having vertically scaled scores that indicate skills and knowledge over time and not just measurement relative to a grade-specific norm for learning.

²⁰ Note that, more generally, this holds for all time invariant factors. Consequently, if the distributions of school quality and family and community environments were fixed through grade G, current characteristics would fully describe schooling, family, and community histories. Of course this would rule out the use of panel estimators and make it virtually impossible to identify the causal effects of specific factors. Moreover, the notion of constant school and teacher quality contradicts evidence of substantial student mobility and within school variation over time in the quality of education.

systematically to this unobserved ability. Nonetheless, we also provide estimates of lagged achievement models with student fixed effects that account for any such individual heterogeneity as a sensitivity check.

There does remain some ambiguity and disagreement over the most appropriate method for accounting for unobserved heterogeneity, and therefore we estimate a series of common specifications including models that use levels of achievement scores with student fixed effects to account for unobserved heterogeneity and those that use test score gains (current minus prior year score) as a dependent variable rather than including lagged achievement as a regressor.

Importantly, these alternatives impose strong assumptions on the value of θ that would introduce specification error if violated.²¹ The levels model with inclusion of the student fixed effect imposes the assumption that the prior influences identified in equation 3 do not persist at all ($\theta=0$), while the gains model imposes the assumption that they do not depreciate at all ($\theta=1$). A comparison of results for the various models permits an assessment of the assumptions underlying the respective specifications.²²

B. Additional Estimation Issues

Under some circumstances, fixed effects estimators tend to exacerbate measurement error and introduce attenuation bias (see Griliches and Hausman (1986), Wooldridge (2002)). The introduction of fixed effects can reduce the bias from unobserved heterogeneity, but it can also reduce reduce the signal to noise ratio in the presence of measurement error. Because racial

²¹ Rivkin (2005) describes potential specification biases related to assumptions regarding knowledge depreciation, and this discussion draws from that work.

²² A final consideration is the possibility that the linear model in lagged achievement may fail to capture the full complexity of the relationship between prior inputs and achievement. This could happen if, for example, the rate at which current learning builds upon past knowledge varies across the skill distribution or the structure of the available tests leads to systematically higher gains in classrooms that focus on material emphasized on tests (see Bacolod and Tobias (2006), Hanushek et al. (2005), and Tobias (2004)). To examine the effects of any such nonlinearities, our preliminary analyses divided prior grade test scores into twenty equally sized intervals and included an indicator variable for each group but one in the specification. But this more flexible functional form left the estimated racial composition effects virtually unchanged, leading us to maintain the linearity assumption for prior achievement throughout the empirical analysis presented below.

composition is measured at a single point in the school year and there may be some uncertainty or error in student classification, there is almost certainly some noise in the racial composition variable. Moreover, the structure of the data potentially amplifies the problem, because many schools contribute only a single grade (either 5th or 7th) in some or all years of the sample. In such cases, the inclusion of school-by-year fixed effects essentially drops these observations from the regressions, substantially reducing the effective sample size used to identify the racial composition parameter.

In order to mitigate the reduction in effective sample size caused by the inclusion of school-by-year fixed effects, we construct an alternative control for time varying factors. Rather than treating each school as a separate entity, we associate each middle school with its elementary school feeders to create an attendance zone that spans the grades in the sample. A far smaller number of attendance zones have only a single grade-school combination in a given sample year. The attendance zone-by-year fixed effects, denoted by ϕ_{ay} , replace φ_{sy} and account for time varying neighborhood shocks and year-to-year changes experienced by school attendance zones along with any changes at the district level. Although they miss idiosyncratic school specific shocks, these are less likely to be systematically related to school racial composition.

The introduction of student fixed effects also exacerbates the potential for attenuation bias. In models that include school-by-grade and school or attendance zone-by-year fixed effects, within school differences between and within cohorts identify racial composition effects. However, in models with student fixed effects only within cohort variation is used to identify the effects of racial composition. We document changes in the proportion black residual variance following the introduction of the respective fixed effects to provide information on the likely importance of measurement error induced bias.

IV. UTD Texas Schools Data

The cornerstone of the analysis of racial composition effects on achievement is a unique stacked panel data set of school operations constructed by the UTD Texas Schools Project, a project conceived of and directed by John Kain. The data we employ track the universe of three successive cohorts of Texas public elementary students as they progress through school. For each cohort there are over 200,000 students in over 3,000 public schools. Unlike many data sets that sample only small numbers from each school, these data enable us to create quite accurate measures of racial composition and peer group characteristics. We use data for grades four through six for the last cohort and grades four through seven for two earlier cohorts. The most recent cohort attended fifth grade in 1996, while the earliest cohort attended fifth grade in 1994. Only black and white students are included in the achievement analysis, although all students are used in the calculations of peer characteristics. In addition as noted above, we exclude the small number of students retained in grade from the analysis, because tests are not vertically integrated across grades.²³

The student data contain a limited number of student, family, and program characteristics including race, ethnicity, gender, and eligibility for a free or reduced price lunch (the measure of economic disadvantage). However, the panel feature of the data is exploited to account implicitly for a more extensive set of background characteristics.

Importantly, students who switch schools can be followed as long as they remain in a Texas public school. This ability to follow students permits accurate assessment of mobility effects and detailed investigation of the sensitivity of racial composition to the cause of its change (mobility, structural moves from elementary to middle school, or changed peers in the same school).

²³ The highest rate of grade retention occurs in seventh grade for black boys where it reaches 3.1 percent in Texas (1.6 percent for white boys). In addition to being quantitatively small, the estimation removes school-by-grade fixed effects so that only time varying retention rates could have any effect on the estimation.

Students who leave Texas public schools – for private schools, for home schooling, or for schools in a different state – cannot be followed. The losses to private schools could be problematic if the choice depended in part on variations in the racial composition of the public school, though the various fixed effects and controls for student heterogeneity make it unlikely that even purposeful selection would introduce bias. The possibility that the racial composition effects differ for such students certainly exists and would not be captured in our estimation. However, the small share of private school enrollment in Texas (less than 6 percent overall and far lower for lower income, minority students) and fact that the proportion exiting our sample declines with age indicates that our estimates are based on the overwhelming majority of students in the state.

Beginning in 1993, the Texas Assessment of Academic Skills (TAAS) was administered each spring to eligible students enrolled in grades three through eight. The tests, labeled criteria referenced tests, evaluate student mastery of grade-specific subject matter. This paper presents results for mathematics, although the results are qualitatively quite similar for reading. Consistent with the findings of our previous work on Texas, schools appear to exert a larger impact on math than reading in grades 4 through 7 (see Hanushek, Kain, and Rivkin (2002a) and Hanushek et al. (2004a)).²⁴

Each math test contains approximately 50 questions. Because the number of questions and average percent right varies across time and grades, we transform all test results into standardized scores with a mean of zero and variance equal to one. In the empirical analysis, an additional grade-by-year fixed effect (ρ_{Gy}) is introduced to capture grade-by-year differences in the statewide testing regime. The regression results are robust to a number of transformations including the raw percentage correct. In order to avoid complications associated with

²⁴ Part of the difference between math and reading might relate specifically to the TAAS instruments, which appear to involve some truncation at the top end. For math, the outcomes are less bunched around the passing scores than they are for reading.

classification as limited English proficient (LEP) or disabled, all LEP and special education students are dropped from the direct achievement analysis, although again these students are included in the peer racial composition calculations.²⁵

Importantly, the student database can be merged with detailed information on teachers and classrooms including grade and subject taught, class size, years of experience, highest degree earned, and population served. Although individual student-teacher matches are not possible, students and teachers can be uniquely related to a grade on each campus. Each student is assigned the average class size and the distribution of teacher experience for teachers in regular classrooms for the appropriate grade, school, and year.

V. Empirical Results

This section reports the results of a series of regressions on the effects of school racial composition on mathematics achievement. Specifications differ by included fixed effects, by included school and teacher characteristics, and by the approach for controlling for unobserved heterogeneity.²⁶ Peer achievement included in some specifications provides information about the potential source of any racial composition effects.²⁷ We report both separate regressions for

²⁵ The peer achievement calculations will under-represent LEP and special education students because they take the TAAS tests less frequently than regular education students (Hanushek et al. (2002a)). It is unclear how this might affect our peer achievement estimates, although they remain a relatively small proportion of the student population.

²⁶ A number of included variables, reported in the tables, are based on prior findings about specific factors affecting achievement growth (Hanushek et al. (2004a), Rivkin, Hanushek, and Kain (2005), and Hanushek et al. (2005)). All specifications include indicators for different types of school-to-school moves (defined by during or end of year moves, by multiple moves in a year, and by moves to same district, other district, or out of sample) and an indicator for free lunch eligibility for each student and year. Specifications that do not remove student fixed effects contain dummy variables for the race, gender and ethnicity of each student, and a full set of grade-by-year indicators. Measured teacher and school characteristics include the proportion of students who are new to the school each year; the proportion of teachers with zero years of experience; and class size (all calculated by grade). Preliminary specifications also included a measure of teacher turnover (proportion of teachers new in grade G), but it was found to have no significant effect, and its exclusion had virtually no impact on the other coefficients. Because some prior work suggests that class size and experience effects are larger for lower income students, these effects are permitted to differ for blacks and Hispanics.

²⁷ We use the average achievement of current schoolmates two years prior to measure differences in cognitive achievement. This captures stable cognitive ability differences but does not include any contemporaneous innovations in achievement that might reflect interactive behavior. Inclusion of current achievement raises the essentially insoluble reflection problem described by Manski (1993). Our approach

blacks and whites and regressions over combined samples that fully interact race with all covariates other than the fixed effects. All specifications include indicators for subsidized lunch eligibility, and various types of student moves, transitions to middle school, and proportion of students Hispanic. Robust standard errors clustered by school account for both the level at which peer composition is measured and any serial correlation in the errors within schools.

In addition to the estimates of the average effects of racial composition on achievement, we investigate differences by initial achievement level and by student mobility. There is speculation that racial isolation is more harmful for initially high achieving blacks, which we examine by allowing the effect of racial composition to vary by initial achievement. The estimates by mobility provide an additional robustness check. A finding that movers were driving the racial concentration results would raise concerns that the results were simply reflecting further unobserved heterogeneity imbedded in selection effects, and we compare the proportion black coefficients observed for movers with those observed for nonmoving students.

For computational considerations, all but the student fixed effect regressions use aggregate data weighted by the number of students in the cell. Data for the basic regressions are aggregated by race, school, grade, and year; in the regressions that permit effects to vary by mobility status, the data are aggregated by mobility status, race, school, grade, and year; in those that permit estimates to vary by initial achievement, the data are aggregated by initial achievement category, race, school, grade, and year. Not surprisingly given the linear structure of the model, preliminary results show that these student weighted aggregate regressions produce virtually identical estimates for the coefficients on proportion black and for robust standard errors as estimates from student level regressions of comparable specifications.

A. Baseline Results and Model Comparisons

takes the “characteristics” view of ability as opposed to the “behavioral” view, as described in and extended by Brock and Durlauf (2001). See also Moffitt (2001) and Hanushek et al. (2003). Nonetheless, empirically we find the pattern of changes in the racial composition coefficients is virtually identical regardless of whether lagged or current average achievement is used to capture peer achievement.

Table 1 presents coefficients for the proportion black (λ) for a series of different specifications estimated separately by race. By comparing the rows of this table, these estimates provide direct comparisons of the three basic models that differ by maintained hypotheses about the depreciation parameter, θ , in equation 3. Estimates in the first two rows do not control for prior achievement (i.e., maintain that $\theta=0$); the middle two rows control for prior achievement by using test score gain as the dependent variable (i.e., maintain that $\theta=1$); and the final two rows include prior achievement as a regressor to control more flexibly for student heterogeneity (i.e., do not constrain θ). The columns display how the estimated impact of racial concentration is affected by different sets of fixed effects that deal with the underlying school and individual heterogeneity. Column 1 includes no school fixed effects and only grade-by-year fixed effects to account for test differences over time; column 2 includes school-by-grade fixed effects (which subsume school and grade fixed effects); column 3 introduces fixed effects for school-by-year, while the final column substitutes attendance zone-by-year fixed effects for the school-by-year fixed effects. (As noted earlier, because of the grade structure of schools, the inclusion of school-by-year fixed effects removes a substantial proportion of students from the identification of the racial composition effects, while inclusion of the attendance zone-by-year fixed effects removes far fewer).

The overall pattern of estimates is consistent with expectations regarding the impact of specification error arising from incorrect assumptions about the value of θ , the rate at which knowledge depreciates over time (Rivkin (2005)). Specifically, while the differences are unlikely to be statistically significant, in virtually all specifications for both blacks and whites the magnitude of the effect of racial concentration appears largest in the model estimated in level form, followed by the value added model with lagged achievement, and then by the simple gains model where θ is assumed to be 1. Student heterogeneity appears to bias the levels estimates upwards, even in specifications with school-by-grade and school-by-year fixed effects.

Moreover, the estimates of racial concentration effects in the simple levels model, which does not explicitly take into account heterogeneity from past student experiences, proves to be very sensitive to the specification of other factors entering into achievement – reflecting the fact that historical heterogeneity is only accounted for through correlations with included contemporaneous factors.

Interestingly, going across the columns, the inclusion of school-by-grade fixed effects increases the estimated impact of proportion black students regardless of the basic specification of the relationship.²⁸ Holding constant the combination of stable school, neighborhood, and peer factors influencing achievement across schools (through school-by-grade fixed effects), the impact of black concentration is estimated to be quantitatively larger for both blacks and whites than in the simple model of column 1.

Both value added specifications (gains and lagged achievement models) produce quite stable estimates of the effects of proportion black on achievement of blacks and whites, though the estimates for whites are much smaller and never significant. For blacks, the coefficient in the lagged achievement specification reported in the bottom panel ranges between -0.21 and -0.31. (We return to the interpretation of this magnitude below). Part of the apparent impact of racial concentration seen in column 2, however, reflects time varying neighborhood, school, and district effects, measured by either school-by-year (column 3) or attendance district-by-year (column 4) fixed effects.

Although the estimates of the impact of racial concentration are highly significant in the models without school-by-year or attendance zone-by-year fixed effects, the addition of these terms reduces the precision of the estimates. In the case of school-by-year fixed effects (col. 3), the estimates are not significant at any conventional level while in the case of attendance zone-by-year fixed effects the estimates are significant at the 10 percent but not 5 percent level.

²⁸ This finding also holds if just school fixed effects, as opposed to school-by-grade fixed effects, are used.

A number of factors might contribute to the decline in the magnitude and significance of the estimates for blacks following the inclusion of school-by-year or attendance zone-by-year fixed effects in addition to any mitigation of bias caused by unobserved, time varying school and neighborhood factors. As noted earlier, the inclusion of school-by-year fixed effects substantially lowers the sample size and number of school-grade-year cells used to identify the estimates. Thus, in addition to reducing the effective variance of proportion black used in the estimation, the estimates are essentially generated from a subset of schools in the sample. And while the attendance zone-by-year fixed effects exert a much smaller impact on the effective sample size, they do absorb a portion of the remaining variance in proportion black. As the first column of Appendix Table A1 shows, the residual variance in proportion black for blacks that remains following the inclusion of the various sets of variables and fixed effects is 1.0 percent of the overall variance in models with just school fixed effects, 0.8 percent in models with school-by-grade fixed effects, 0.3 percent in models with school-by-grade and attendance zone-by-year fixed effects, and 0.1 percent in models with school-by-grade and school-by-year fixed effects. The much smaller variances for the second level models would reduce the precision of the estimates even in the absence of measurement error. We return to these issues below.

A significant threat to the identification of the racial composition effect is that systematic, time-varying school factors might be correlated with the varying racial compositions. Indeed, as noted previously, other research has suggested that systematic teacher mobility is important because it leaves minority and disadvantaged students with less experienced teachers. Further, the higher mobility rates of black students puts them in schools subject to more disruption by student turnover and threatens to move class sizes away from chosen levels. To deal with these problems, we add direct measures of teacher and school characteristics to the prior specifications.

As seen in Table 2 (which begins with columns 3 and 4 of the previous table), the estimates of λ are barely affected by the inclusion of measured teacher and school characteristics (class size, proportion on teachers in their first year of experience, and school mobility rates)

despite the fact that both class size and teacher experience are significant achievement determinants (coefficients not reported). In addition, the proportion Hispanic does not have a significant impact on either black or white student achievement (not shown) and does not affect the magnitude of the proportion black coefficient, confirming that it is the black concentration and not the minority concentration in a school that matters.

Importantly, there is also little or no evidence that peer achievement drives the observed relationship between achievement and proportion black despite the fact black students disproportionately fall in the lower portions of the achievement distribution. As Appendix Table A2 shows, over 40 percent of black students fall in the bottom quartile of the state distribution, while over 35 percent of white students fall in the top quartile. Nevertheless, the inclusion of average peer achievement, shown in columns 3 and 6, does not affect the proportion black coefficients in any of the achievement models for either whites or blacks. This also holds if peer achievement is calculated over only same race peers (results not shown). Of course there remains a possibility of a more complicated interrelationship among peer achievement and race, a possibility that we investigate below.

Separate estimation by race as done in Tables 1 and 2 might contribute to the lowered precision by producing noisy estimates of the various school fixed effects in schools in which black or white enrollment is quite small. Such estimates could move the proportion black coefficients far from the true parameter value and increase the estimated standard errors. Combining the samples of blacks and whites mitigates this problem at the cost of imposing the restriction of equality across races in the various school fixed effects.

Table 3 reports proportion black coefficients for a subset of specifications reported in Tables 1 and 2 but estimated over a combined sample of blacks and whites.²⁹ Because these specifications fully interact prior achievement and all covariates with race, it is only the fixed

effects for which the assumption of equality across race is imposed.³⁰ The results are qualitatively quite similar to those reported in Table 1 across models, though the point estimates generally tend to be somewhat smaller in Table 2. Our preferred value added model with lagged achievement (i.e., unconstrained θ) produces a stable proportion black coefficient equal to -0.18 which is unaffected by the inclusion of the school and teacher variables. This coefficient, only slightly smaller than that from separate samples, is much more precisely estimated than coefficients from the comparable specifications in Table 1 and Table 2. Note, however, that the estimates from the levels model with school-by-grade, attendance zone-by-grade, and all of the specific time varying measures are much larger for blacks and smaller for whites than those found in Table 2 and from those estimated from the value-added specifications in Table 3. Again, the pattern of results for the constrained models in Table 3 (i.e., those where θ is set to 0 or to 1) follows the predicted bias found previously.

Sensitivity Checks

Although the stability of the proportion black coefficient in the lagged achievement model provides support for the belief that it captures a causal effect, there remains the possibility that prior achievement does not fully account for unobserved heterogeneity that would contaminate the results. Two sensitivity checks on the estimates, however, support this modeling. The first interacts mobility with proportion black to examine the extent to which movers, the group most likely to experience time varying shocks that could bias the estimates, drive the results. Specifically, we divide students into three categories: 1) students that remain in the same school; 2) students that move from elementary to middle school within the same attendance zone (structural move); and 3) students that switch attendance zones (family move). We do not have strong priors about which source of difference in racial composition would lead to a larger effect,

²⁹ The columns of Table 3 correspond to Table 1 (column 2); Table 1 (column 2) with the addition of teacher and school characteristics and peer achievement; Table 2 (column 4); and Table 2 (column 6), respectively.

though we would expect that those who remain in the same school might be less sensitive to changes in the student body given the relative stability of their existing network of friends. The second sensitivity check adds student fixed effects to the specifications in order to provide additional controls for unobserved heterogeneity.

Table 4 presents the estimates for a lagged achievement specification with school-by-grade and attendance zone-by-year fixed effects and teacher and school characteristics (the specification reported in Table 3, Column 4, but excluding peer achievement). The results make clear that school switchers do not drive the estimates reported in Tables 1-3, as the coefficients on proportion black interacted with school mover are insignificant for both blacks and whites. By comparison to family move estimates, the interactions with nonmover and the structural move indicator are roughly twice as large in magnitude for both blacks and whites and significant at the 1 percent level for blacks. The estimated impact of racial composition on nonmovers and on students undergoing a structural move match closely the previous estimates in Tables 1-3.

Inclusion of student fixed effects goes one step further in adjusting for potential unobserved heterogeneity, but it does so again at the cost of potentially exacerbating the impact of measurement error.³¹ Table 5 reports OLS and IV estimates from lagged achievement specifications with teacher and school characteristics and school-by-grade fixed effects that account for student fixed effects by first differencing. Because the inclusion of a lagged dependent variable in a student fixed effects model produces inconsistent estimates, we use twice lagged scores as an instrument for the difference in lagged achievement in the second specification.³²

³⁰ The fact that the coefficient on lagged achievement is flexible and permitted to vary by race also may mitigate any bias introduced by the imposition of the restriction of equal school fixed effects across races.

³¹ In earlier versions of this analysis, we had a computational error in the fixed effects models, and we thank David Armor for pointing this problem out to us.

³² Because the estimates are not sensitive to the inclusion of an interaction term between race and lagged achievement, the first difference models do not include such a term.

The instrumental variable estimate of the racial composition effect for blacks is significant but slightly smaller than the estimates in Table 3 (0.14 v. 0.18), while the coefficient for whites remains smaller and not significant at conventional levels. Whether the small decline in the coefficient for blacks reflects sampling error, the amplification of errors in variables induced bias, or the mitigation of omitted variables bias is uncertain, but in any case the addition of the student fixed effects does not markedly alter the estimated effects of proportion black.

Taken together, the findings provide strong support for the belief that higher black concentrations reduce black achievement. The effects appear to be substantially larger for blacks than whites, eliminating general concerns that these are simply the impact of differential school quality. Further, from direct estimation, there is little or no evidence that proportion black serves as a proxy for unaccounted for peer achievement, minority status, student turnover, teacher quality, or income.³³ Rather the effects appear to be related specifically to the black enrollment share, as they are conditioned on proportion Hispanic and insensitive to the inclusion of peer average achievement.

C. Differential Effects

To this point the specifications permit variation in the proportion black effect by race but no other dimension. The possibility remains that racial composition and peer achievement effects vary along a number of dimensions including academic preparation, percent black, and gender.

One oft-discussed question is whether the effect of peers is constant across the achievement distribution or whether peers exert differential effects according to location in the skill distribution. To investigate this question, we divide students on the basis of their third grade mathematics score relative to others in the state. Specifically, we calculate 25th, 50th, and 75th

³³ The inclusion of proportion eligible for a subsidized lunch has virtually no effect on the proportion black coefficients. Further, we find no evidence that any systematic teacher movements across grades and cohorts within schools could be affecting the basic results. Specifically, in specifications that include school-by-grade fixed effects, student proportion black is not significantly different for teachers who switch grades than for teachers who remain in the same grade.

percentile scores for blacks in 3rd grade, divide all students into quartile groupings on the basis of those scores, and estimate separate proportion black effects for blacks and whites in each group.³⁴

The results reported in the left panel of Table 6 reveal that the adverse effects of higher proportion black increase as initial achievement increases for blacks but not for whites. The top row shows a monotonic pattern for blacks in which the negative effect of a ten percentage point increase in proportion black on annual mathematics achievement rises from -0.013 to -0.020 standard deviations. In contrast, the coefficients for whites exhibit the opposite pattern and are not significant at any conventional level.

A related concern is the possibility that peer achievement effects differ according to initial skill, raising doubts about the assumption of constant peer effects for blacks and whites imposed in Tables 2 and 3. We examine this possibility by permitting separate peer achievement and racial composition effects for each group. The results in the right panel of Table 6 show that the inclusion of peer achievement interactions has little effect on the proportion black coefficients, again providing little or no support for the notion that racial composition serves as a proxy for peer achievement.

Racial composition effects could also vary nonlinearly with school proportion black, in that a given increase in the black enrollment share in a school with a low black enrollment share might produce a very different effect from an identical increase in a school with higher black enrollment. As shown in Figure 1, there are a number of districts in which black enrolment is quite low and a smaller set of districts with far higher black enrolment shares. However, polynomial specifications up to quartics in proportion black produce little or no evidence in support of effect variation by racial composition (not shown). None of the higher order polynomial terms are significant at conventional levels.

³⁴ The 3rd grade test score distribution for blacks is to the left of that for whites and exhibits much higher variance, meaning that classification based on blacks' scores leads to much larger differences among categories in average scores. Specifications are comparable to those estimated in Table 3 (column 4) with the addition of indicators for test score group interacted with race.

Finally, some authors have suggested that the peer influences on black boys differ from those on black girls (Hoxby (2000), Ferguson (2001)). We find no significant differences in the effects of compositional differences by gender on achievement (not shown).

VI. Conclusions, Interpretations, and Policy Implications

The difficulties of isolating school and peer group effects have been well documented. The interrelated decisions of families, teachers, and school officials that determine the distribution of students among schools certainly complicate the identification of any effects of racial composition. However, by using a fixed effects framework that accounts for the cumulative effects of observed and unobserved determinants of learning, we overcome many of the methodological problems that impede the estimation of these effects.

Four primary aspects of the analysis give us confidence that we have isolated the causal effect of school racial composition. First, we incorporate general measures of systematic differences in schools, grades and years through fixed effects that absorb both stable and time varying effects of neighborhood, curriculum, school leadership, peers, teachers, and school specific patterns of achievement change across grades, regardless of whether we can identify and measure the specific factors. Second, we control for time varying factors that may be related to changes in racial composition: teacher experience, class size, school mobility rates, and school switches brought about by family economic changes or other shocks. Third, we find very stable estimates in the value added models that are insensitive to the inclusion of important covariates. Fourth, the results of our preferred models are consistent with general models of knowledge acquisition and achievement.

The pattern of estimates provides strong evidence that school proportion black negatively affects mathematics achievement growth, and the effects are roughly twice as large for blacks as for whites. Our finding of the particular importance of racial composition for blacks reinforces the findings from other, quite different approaches to investigating these effects (Angrist and

Lang (2004), Guryan (2004), and Hanushek and Raymond (2005)) where differential effects by race are also found. Importantly, this effect does not appear to be driven by school quality or peer achievement, and there is little evidence that effects vary by gender or school proportion black. There is, however, evidence that the negative effects of black concentration may increase as the achievement level of black students increase. Finally, Hispanic enrollment share appears to exert a far smaller effect, indicating that it is proportion black rather than proportion minority that is the key aspect of peer race/ethnic composition in terms of achievement for blacks and whites.

Our data do not enable the identification of the mechanisms underlying the racial composition effects, and the pattern of results is generally consistent with a variety of existing behavioral hypotheses. In particular, a number of researchers, commentators, and community leaders emphasize that some blacks discourage others from excelling academically, but this view remains controversial. The early discussions, drawing on numerous perspectives and reaching different conclusions, can be found in Fordham and Ogbu (1986), Cook and Ludwig (1997), Steele and Aronson (1998), Ainsworth-Darnell and Downey (1998), Ferguson (1998a, (2001), McWhorter (2000), and Bishop et al. (2001). More recently, a series of analyses have focused on cultural issues, including economic models that determine cultural behavior (Austen-Smith and Fryer (2003), Fryer and Levitt (2003), Ogbu (2003), and Thernstrom and Thernstrom (2003)). Others have suggested that teachers lower expectations for black students or that schools might adjust placement in academic tracks as the black concentration increases (see Ferguson (1998b)). The difficulty with the latter explanations, as opposed to the direct social interaction effect, is that time invariant components of expectations and tracking for each school have been removed, leaving only the cohort-to-cohort innovations in racial composition. Nonetheless, each of these explanations would tend to produce a stronger relationship between achievement and proportion black for blacks at the higher end of the initial achievement distribution – something that is suggested in our data. Unfortunately, our administrative data do not enable us to isolate the underlying behavioral mechanism.

The magnitudes of the black composition effects are educationally significant. On average the black share of school enrollment is almost 30 percentage points higher for black students than for white students. Elimination of this gap would reduce the proportion black from roughly 0.39 to 0.16 for black students and raise the proportion black from 0.09 to 0.16 for whites.³⁵ Using the coefficient for blacks of -0.18 and the coefficient for whites of -0.07, such a redistribution of students would reduce the racial achievement gap by 0.046 standard deviations in a single year. The cumulative effect of such a reduction for grades 5-7 (the sample period) depends upon the rate at which knowledge depreciates over time. If the rate of depreciation were equal to one minus the coefficient on lagged achievement (roughly 0.4 for blacks and whites), the three year cumulative effect of racial composition equalization would reduce the race achievement gap by roughly 13 percent, moving it from 0.70 to 0.61 standard deviations.³⁶

These estimates represent extremes in the possible changes in racial compositions. More modest, and perhaps more achievable, changes still imply substantial closing in the test score gap. For example, a reduction in the percentage of black classmates for black students of ten percentage points coupled with no change for whites would still close the black-white gap by slightly more than 5 percent over grades 1-7 if our estimated effects (and depreciation rates) hold for early grades.

The policy implications of these findings are, nonetheless, unclear, largely because of the imbalance in the distribution of students across jurisdictions. The *Brown* decision and refinements through subsequent cases sharply restrict the circumstances in which inter-district remedies are permissible (Rivkin and Welch (2006)), a key limitation given housing patterns in Texas.³⁷

Increases in the number of charter schools provides one possible approach, because they can draw

³⁵ The compositions do vary some across grades, and the calculations below allow for these grade-specific differences in composition.

³⁶ If the racial composition factors were similar for earlier grades, this change in racial composition throughout grades 1 through 7 would imply closing the seventh grade achievement gap by 21 percent.

students from across school districts. Nonetheless, the experience to date in Texas suggests that charters do not expand interracial contact but instead on average lead to increased racial segregation.

A related issue concerns possible variation in the racial composition effect to the intensity of desegregation efforts. Our sample covers a period without much in the way of new, far-reaching desegregation activity, and the relationship between achievement and racial composition might depend upon both programmatic and historical factors that determine school attendance patterns in a given district. Consequently, active initiatives designed to increase substantially black exposure to whites might produce a somewhat different relationship between achievement and racial composition.

An alternative supported by a range of prior investigations would emphasize a change in focus to housing policy. Over three decades ago, Kain and Persky (1969) suggested that: “*De facto* school segregation is another widely recognized limitation of Negro opportunities resulting from housing market segregation. A large body of evidence indicates that students in ghetto schools receive an education that is much inferior to that offered elsewhere.” This led them to argue for more aggressive policies promoting housing desegregation as opposed to expensive compensatory strategies that left ghettos unaffected. More recently, the outcomes of the Gautreaux Program (Rosenbaum (1995)) have reinforced the possibility of favorable outcomes from housing dispersal programs. Nonetheless, other recent analyses of the Moving to Opportunity experiment suggest caution about what can be expected (Kling, Liebman, and Katz (2007)).³⁸ Nonetheless, policies that support the continued suburbanization of black Americans

³⁷ The recent decisions on intradistrict assignment of students in the Seattle and Louisville cases are also relevant. In a 5-4 decision in June 2007, the court held that race-based assignment of pupils within districts was not permissible under the 14th amendment of the U.S. Constitution.

³⁸ Early results by Ludwig, Ladd, and Duncan (2001) indicated that moves to low poverty areas lead to significant increases in student achievement, but these effects were subsequently found to be small or nonexistent. Kling et al. (2007) show that the changes in school characteristics were small, even when the neighborhood differences from the MTO experiment were large, thus opening questions about how to

and the slow but steady decline in black-white segregation that has marked the last two decades (Cutler, Glaeser, and Vigdor (1999), Iceland and Weinberg (2002)) would, by the results of this paper, lead to improved schooling outcomes and a slight decline in the racial achievement gap.

interpret the results from a policy perspective. Other analyses of neighborhood effects also suggest some caution on the results that might accrue, e.g., see Oreopoulos (2003).

Appendix Table A1. Residual Variance as a Percentage of Variance of Peer Proportion Black, by Race and Specification

Residuals are obtained from regressions of proportion black on the same variables as Table 2 (column 2) and the specified fixed effects

| Specification | Blacks | Whites |
|--|--------|--------|
| school fixed effects regression | 1.0% | 3.3% |
| school-by-grade fixed effects regression | 0.8% | 2.8% |
| school-by-grade and school-by-year fixed effects regression | 0.1% | 0.6% |
| school-by-grade and attendance zone-by-year fixed effects regression | 0.3% | 1.3% |
| school-by-grade and student fixed effects regression | 0.5% | 1.6% |
| actual variance | 0.0807 | 0.0123 |

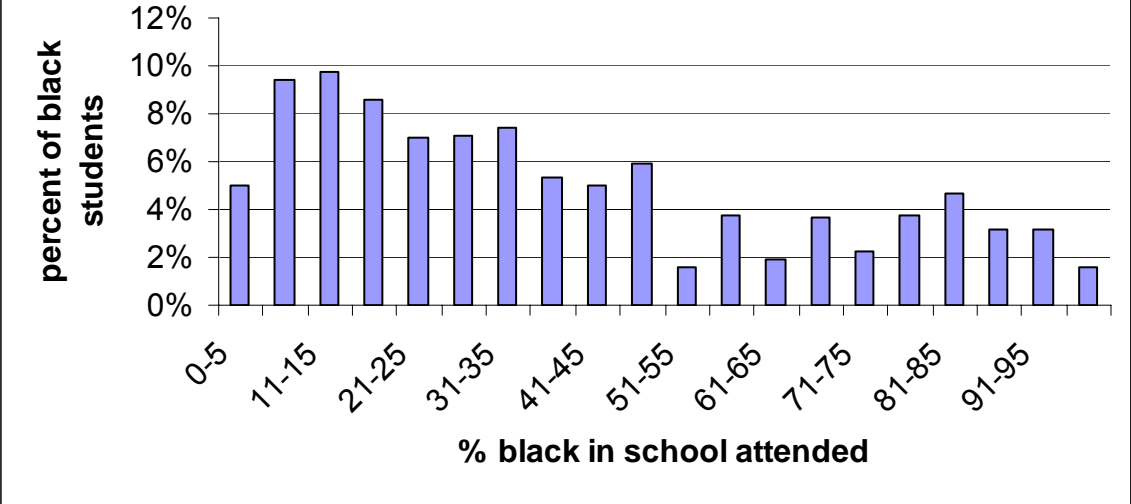
Appendix Table A2. Distributions of Blacks and Whites by Quartile of State Math Test Score Distribution

| Quartile of Distribution of Third Grade State Scores: | | | | All |
|---|-----------------|----------------|--------------|-----|
| Bottom Quartile | Second Quartile | Third Quartile | Top Quartile | |

Placement in achievement distribution

| | | | | | |
|----------------|------|------|------|------|-----|
| Black students | 41.4 | 30.0 | 19.1 | 9.5 | 100 |
| White students | 14.7 | 21.6 | 28.5 | 35.2 | 100 |

Figure 1: Distribution of Black Students by Percent Black in School



**Table 1. Estimated Effects of Proportion Black on Mathematics Achievement (λ)
Based on Separate Regressions by Race**
(Samples: 142,106 Blacks and 661,352 Whites)

| Year-by-grade (ρ_{Gy}) fixed effects | | | | |
|---|-----------------|--|--|-----------------|
| School-by-grade (π_{sG}) fixed effects | | | | |
| | | School-by-year (ϕ_{sy}) fixed effects | Attendance zone-by-year (ϕ_{ay}) fixed effects | |
| | (1) | (2) | (3) | (4) |
| <i>1. Levels Model ($\theta = 0$)</i> | | | | |
| Blacks | -0.15 (3.93) | -0.39 (3.04) | -0.36 (1.11) | -0.21 (1.24) |
| Whites | 0.00 (0.14) | -0.14 (1.63) | -0.17 (1.19) | -0.15 (1.72) |
| <i>2. Gains Model ($\theta = 1$)</i> | | | | |
| Blacks | -0.09 (3.74) | -0.26 (2.43) | -0.21 (0.96) | -0.23 (1.69) |
| Whites | 0.00 (0.09) | -0.02 (0.32) | -0.04 (0.31) | 0.07 (1.02) |
| <i>3. Gains Model (θ not set = 1)</i> | | | | |
| Blacks | -0.11 (4.32) | -0.31 (3.07) | -0.24 (1.14) | -0.23 (1.78) |
| Whites | 0.00 (0.00) | -0.08 (1.27) | -0.09 (0.83) | -0.11 (1.68) |

Note: Absolute value of Huber-White adjusted t-statistics clustered by school in parentheses All specifications include indicators for within district, between district, and between region school changes other than transitions to middle school, indicators for school changes during the school year and multiple changes in one year, an indicator for a middle school transition, and indicators for subsidized lunch eligibility, and female.

Table 2. Effect of Teacher and School Characteristics and of Peer Achievement on Estimated Effects of Proportion Black on Mathematics Achievement (λ) Based on Separate Regressions by Race
(Samples: 142,106 Blacks and 661,352 Whites)

| | Year-by-grade (ρ_{Gy}) and School-by-grade (π_{sG}) fixed effects | | | | | |
|---|--|------------------------------------|------------------|---|------------------------------------|------------------|
| | School-by-year (ϕ_{sy}) fixed effects | | | Attendance zone-by-year (ϕ_{ay}) fixed effects | | |
| | (1) | teacher and school characteristics | | (4) | teacher and school characteristics | |
| | | (2) | peer achievement | | (5) | peer achievement |
| <i>1. Levels Model ($\theta = 0$)</i> | | | | | | |
| Blacks | -0.36 (1.11) | -0.39 (1.23) | -0.42 (1.36) | -0.21 (1.24) | -0.25 (1.43) | -0.23 (1.37) |
| Whites | -0.17 (1.19) | -0.16 (1.17) | -0.16 (1.15) | -0.15 (1.72) | -0.15 (1.71) | -0.14 (1.63) |
| <i>2. Gains Model ($\theta = 1$)</i> | | | | | | |
| Blacks | -0.21 (0.96) | -0.16 (0.75) | -0.17 (0.80) | -0.23 (1.69) | -0.22 (1.60) | -0.22 (1.57) |
| Whites | -0.04 (0.31) | -0.04 (0.31) | -0.04 (0.33) | 0.07 (1.02) | 0.07 (0.99) | 0.07 (1.02) |
| <i>3. Gains Model (θ not set = 1)</i> | | | | | | |
| Blacks | -0.24 (1.14) | -0.21 (1.02) | -0.23 (1.11) | -0.23 (1.78) | -0.23 (1.79) | -0.22 (1.74) |
| Whites | -0.09 (0.83) | -0.08 (0.83) | -0.09 (0.83) | -0.11 (1.68) | -0.11 (1.66) | -0.10 (1.63) |

All specifications include indicators for within district, between district, and between region school changes other than transitions to middle school, indicators for school changes during the school year and multiple changes in one year, an indicator for a middle school transition, and indicators for subsidized lunch eligibility, and female. Specifications with school and teacher characteristics also include the proportion of teachers in their first year, class size interacted with grade, the proportion of students who move to the school prior to the start of the school year, and the proportion who move to the school during the year. Peer achievement as measured by twice lagged math score averaged over current students in the school and grade.

Table 3. Estimated Effects of Proportion Black on Mathematics Achievement (λ) for Combined Black and White Samples
 (absolute value of Huber –White adjusted t-statistics clustered by school in parentheses; 803,457 observations)

| | Year-by-grade (ρ_{Gy}) and School-by-grade (π_{sG}) fixed effects | | Year-by-grade (ρ_{Gy}), School-by-grade (π_{sG}), and attendance zone-by-year (ϕ_{ay}) fixed effects | |
|--|---|---|--|---|
| | | teacher and school characteristics and peer achievement | | teacher and school characteristics and peer achievement |
| | (1) | (2) | (3) | (4) |
| <i>1. Levels Model ($\theta = 0$)</i> | | | | |
| Blacks | -0.38 (4.67) | -0.35 (4.41) | -0.32 (3.75) | -0.33 (3.90) |
| Whites | -0.15 (1.89) | -0.10 (1.27) | -0.08 (0.99) | -0.08 (1.02) |
| <i>2. Gains Model ($\theta = 1$)</i> | | | | |
| Blacks | -0.14 (2.18) | -0.13 (1.97) | -0.11 (1.56) | -0.11 (1.54) |
| Whites | -0.09 (1.35) | -0.09 (1.30) | -0.07 (0.96) | -0.07 (1.00) |
| <i>3. Lagged Achievement Model (θ not set = 1)</i> | | | | |
| Blacks | -0.23 (3.66) | -0.22 (3.51) | -0.18 (2.71) | -0.18 (2.84) |
| Whites | -0.12 (1.90) | -0.09 (1.53) | -0.07 (1.16) | -0.07 (1.18) |

All specifications include indicators for within district, between district, and between region school changes other than transitions to middle school, indicators for school changes during the school year and multiple changes in one year, an indicator for a middle school transition, and indicators for subsidized lunch eligibility, female, and black. Specifications with school and teacher characteristics also include the proportion of teachers in their first year, class size interacted with grade, the proportion of students who move to the school prior to the start of the school year, and the proportion who move to the school during the year. Peer achievement as measured by twice lagged math score averaged over current students in the school and grade. All variables are fully interacted with race.

Table 4. Value Added Estimated Effects of Proportion Black on Mathematics Achievement (λ) by Source of Variation for Combined Black and White Samples
 (absolute value of Huber-White adjusted t-statistics clustered by school in parentheses; lagged achievement models with θ not set = 1)

| | No Move | Structural Move | Family Move |
|------------------------|-----------------|-----------------|-----------------|
| proportion black*black | -0.18 (2.81) | -0.22 (3.21) | -0.10 (1.54) |
| proportion black*white | -0.08 (1.27) | -0.11 (1.70) | -0.04 (0.62) |

Notes: All specifications incorporate a lagged achievement variable and include school-by-grade and attendance zone-by-year fixed effects, indicators for within district, between district, and between region school changes other than transitions to middle school, indicators for school changes during the school year and multiple changes in one year, an indicator for a middle school transition, indicators for subsidized lunch eligibility and female along with the proportion of teachers in their first year, class size interacted with grade, the proportion of students who move to the school prior to the start of the school year, and the proportion who move to the school during the year.

Table 5. OLS and IV Student and School-by-Grade Fixed Effect Estimates of the Effects of Proportion Black on Mathematics Achievement (λ)

(Combined black and whites samples with unconstrained lagged Achievement Model; absolute value of t-statistics in parentheses; 486,515 observations)

| | OLS | IV |
|--|-----------------|-----------------|
| Blacks | -0.10 (1.66) | -0.14 (2.10) |
| Whites | -0.06 (1.00) | -0.10 (1.58) |
| t statistic on twice lagged test score in 1st stage for IV model | | 147.8 |

Notes: The lagged achievement specifications are estimated in first differences by student to remove student fixed effects. All models include a full set of grade-by-year dummies; indicators for within district, between district, and between region school changes other than transitions to middle school; indicators for school changes during the school year and multiple changes in one year; an indicator for a middle school transition; indicators for subsidized lunch eligibility; the proportion of teachers in their first year, class size interacted with grade, the proportion of students who move to the school prior to the start of the school year, the proportion who move to the school during the year, and school-by-grade fixed effects. All variables other than lagged achievement are fully interacted by race.

Table 6. Estimated Effects of Proportion Black on Mathematics Achievement (λ) by Quartile of 3rd Grade Test Score Distribution

(absolute value of Huber –White adjusted t-statistics in parentheses; 749,563 observations)

| | No Peer Achievement Variables | | | | With Peer Achievement Variables | | | |
|------------------------|-------------------------------|-----------------|-----------------|-----------------|---------------------------------|-----------------|-----------------|-----------------|
| | Bottom Quartile | Second Quartile | Third Quartile | Top Quartile | Bottom Quartile | Second Quartile | Third Quartile | Top Quartile |
| proportion black*black | -0.13 (1.92) | -0.16 (2.43) | -0.18 (2.77) | -0.20 (3.07) | -0.13 (1.95) | -0.16 (2.42) | -0.18 (2.82) | -0.21 (3.17) |
| proportion black*white | -0.09 (1.37) | -0.11 (1.71) | -0.08 (1.21) | -0.06 (0.92) | -0.09 (1.31) | -0.12 (1.83) | -0.08 (1.23) | -0.05 (0.80) |

Notes: lagged achievement specification with same controls as Column 4 of Table 3 plus indicators for 3rd grade test quartile; peer achievement is fully interacted with race/ethnicity and 3rd grade test quartile.

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