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Abstract

Uncovering the effects of school integration is difficult, because racial mixing in the schools is not an accident but instead represents a complex mixture of governmental and individual choices. Much of the current integration of schools traces its origin to the political and legal history that followed *Brown v. Board of Education*. The goals and implications of the implemented policies are very broad, and it is virtually impossible to think of a comprehensive evaluation of this collection of actions. Here we focus on one piece: what is the effect of school integration on scholastic achievement? Our evaluation of this policy is made possible by rich panel data on the achievement of Texas students. These data, part of the UTD Texas Schools Project, allow us to disentangle integration effects from differences in individual student abilities and from other aspects of school quality. The simple conclusion is that *ceteris paribus* schools with higher concentrations of minority students lead to lower achievement for Black students but minimal effects on whites or Hispanics.

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How Much Does School Integration Affect Student Achievement?

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One of the most explosive policy issues of the twentieth century was school integration. The political debate and conflict touched most areas of the country. Now, close to fifty years after the landmark school desegregation case of *Brown v. Board of Education*, a surprising amount of uncertainty still exists about the ultimate effects of the policies that have been put into place. Much of the public debate was over the proper role of racial desegregation and the best methods for accomplishing its purposes. The scholarly debate has, however, been more focused on the impact. A large part of this has been essentially an accounting exercise, identifying the changes in the degree of integration. But the motivation behind school desegregation has been improving the attitudes and performance of students historically affected by segregative practices. Here we concentration on one such outcome: the scholastic achievement of students in settings with varying degrees of racial concentration. By exploiting rich data on individual and school experiences for students throughout the State of Texas, we can directly estimate the impact of integration on student achievement.

The ruling in *Brown v. Board of Education* (1954) led to dramatic changes in schools throughout the country. The history of changes in enrollment patterns both for the nation and for Texas provides an important backdrop for this study. These changes did not take place overnight, and even 15 years after the ruling the schools remained largely segregated. The decade of the 1970s witnessed further reduction in segregation brought about largely through legal pressure on local school districts. But the countervailing trend of the large-scale exodus of Whites from many cities and towns undoubtedly dampened the impact of school desegregation on inter-racial contact.

Texas schools are interesting as an example of the changes that have occurred in previously segregated systems. It goes through court ordered desegregation within the context of decentralization of the population and rapid overall growth. Combining these various forces leaves today's Black public school students in Texas far more to have White schoolmates than did their parents or grandparents in the late 1960s.

A key question that is the focus of this work is whether inter-racial contact raises academic achievement and other academic outcomes for Blacks as well as for Hispanics and other minorities. The decision in Brown v. Board of education certainly assumed this to be the case, ruling that separate but equal, while not inherently unconstitutional in all areas, was unconstitutional in the case of education because of the important role of peers in the education process. The landmark legislatively mandated civil rights report on the <u>Equality of Educational Opportunity</u> (Coleman 1966) and its offshoots (U. S. Commission on Civil Rights 1967) provided empirical evidence that racial isolation harms academic achievement. Subsequent work by Crain (1970), Boozer, Krueger, and Wolkon (1992), and Grogger (1996) also found that school racial composition affected academic, social, and economic outcomes. Kain and O'Brien (199?), upon which this paper builds, found that Blacks benefit a great deal from moving to the suburbs. In contrast, Cook (1984) concluded that the available evidence found that desegregation had little if any effect on mathematics and reading achievement in elementary school, and Rivkin (2000) found no evidence that exposure to Whites increased academic attainment or earnings for Black men or women in the high school class of 1982. Overall, there remains considerable disagreement about the nature and magnitude of benefits of desegregation efforts, let alone about the costs of these (e.g., Crane and Mahard 1978; Armor 1995).

The contrasting findings and lack of consensus concerning the importance of school racial composition emanate in large part from the difficulty of isolating the causal impact of peer characteristics. For example, if families with greater resources or a greater commitment to schooling tend to choose more racially integrated schools, the racial composition effects are easily confounded with other factors.[1] As Manski (1992) and Moffitt (1998) point out, the empirical analysis of peer influences has been inhibited by both conceptual and data problems -- problems that raise serious questions about interpretation of the existing studies, even those that use more sophisticated econometric techniques including instrumental variables. In the studies of school racial composition effects, neither Crain nor Boozer et al. provide many statistical controls for differences in socio-economic background or prior academic preparation.[2] Unlike the other papers, Grogger (1996) uses a longitudinal data set that contains information on family background and achievement measures, though it is unlikely that this small number of variables would account for all factors that are related to both outcomes and the choice of schools. The inclusion of private school students in the analysis further increases the likelihood that the school racial composition coefficients are biased upward. Rivkin (2000) does use school district aggregate measures of exposure to Whites in order to overcome the nonrandomness of both neighborhood location within districts and attendance in non-neighborhood schools; nevertheless, unobserved differences among districts may contaminate the estimates.

A second important issue is the identification of the causal linkage that underlies any observed relationship between achievement and racial composition. As Boozer et al (1992) point out, a positive relationship between outcomes and the percentage of students who are White might be driven by peer effects, school quality, or some combination of the two. These explanations carry different policy implications, and it is important to identify the underlying causes of the link between outcomes and racial composition. While Rivkin (2000) finds little effect of racial composition, he does find that high school quality is an important determinant of

[1]See Tiebout (1956) for a discussion of the link between family preferences and neighborhood location.

[2]Boozer, Krueger, and Wolkon (1992) use two stage least squares in an attempt to control for nonrandom selection into integrated schools. The 2sls estimates are much less precise than the ols estimates; moreover the instrumenting strategy uses variation across time and state in school racial composition, and such variation may be correlated with other determinants of earnings.

achievement, though in this case it exerts an effect that is largely uncorrelated with racial composition.

This paper makes use of a unique matched panel data set on students and schools to identify the impacts of racial composition on academic achievement. While controls for observable characteristics are used, it is the ability to control for fixed individual, school, and school by grade effects on test score gains that permits the clearest identification of racial composition effects. Ultimately, we identify these effects by considering differences in the changes in racial composition for successive cohorts of students in a given school as they age.3 This panel data approach is robust to most of the commonly cited estimation dangers. Moreover, comparisons of models with and without school fixed effects provide information regarding the contribution of school quality to any achievement/racial composition relationship.

Our basic estimation of elementary school achievement growth indicates that racial composition affects achievement: both Blacks and Hispanics benefit from attending schools with students of other race and ethnic groups, with the effect being noticeably more pronounced for Blacks. In contrast, percent minority has little influence on White achievement. Because the estimates are largely invariant to the inclusion of observable school characteristics and school fixed effects, the findings support the notion that peer influences rather than school quality differences drive the link between outcomes and school racial composition.

Prior to the presentation of the empirical model and results, we document changes in racial composition and school enrollment patterns for the thirty year period from 1968 to 1998 for the state of Texas as a whole and the 65 school districts that are included in all surveys used in the analysis. This section contrasts changes in school enrollment patterns within districts with changes in the concentration of Blacks, Hispanics, and Whites among districts, providing a clear

[3] This methodology is similar to that used by Hoxby (1998) in the estimation of class size and racial composition effects for students in Connecticut. Hoxby (2000) extends the general approach to Texas, although the parameterization differs from the one we employ.

demonstration of the contrasting forces of white flight from the central cities and expanded integration efforts.

Racial separation in public schools today is primarily attributable to residential segregation. Rivkin (1994) shows that even if all U.S. school districts had been perfectly integrated (each school having the district share of all demographic groups) in 1988, housing patterns would lead to a schooling system in which large numbers of Blacks would have few White schoolmates. This section applies the approach used in Rivkin (1994) to document changes over time in student enrollment patterns in the state of Texas.

The Texas data on students (discussed below) along with data from the Office of Civil Rights (OCR) Bi-Annual Survey of Public Schools for 1968, 1980, and 1992 are used in the description of school and district enrollment patterns. Adding the OCR data permits us to document enrollment patterns over the thirty year period from 1968 to 1998. The OCR data provide school enrollment counts for American Indians, Asians, Blacks, Hispanics, and Whites for a sample of 65 Texas school districts.[4]

The experience of Texas public schools is quite similar to those of all southern states grouped together as well as the U.S. as a whole. Table 1 shows the demographic composition of Texas public schools. Between 1968 and 1998 the decline in White enrollment as a percentage of the total was roughly offset by increases in Hispanic enrollment, while the Black enrollment declined only slightly. White enrollment fell from 64 percent to 45 percent of the

[4] The OCR data contain a sample of districts for each state. Our analysis eliminates one sampled district that was reconstituted over the time period. Importantly, because the OCR surveys only a portion of the public schools in Texas, the data must be weighted by the inverse probability of selection into the sample to generate statewide projections. Not surprisingly the different samples produce slightly different segregation and enrollment statistics for 1992, the year the two data sets overlap. However, the aggregate differences are minor (as shown below), and the statistics for the individual school districts are virtually identical.

total during the thirty year period, while Hispanic enrollment increased from 19 percent in 1968 to 38 percent in 1998. In sum, Texas public schools experienced substantial changes in demographic composition.[5] Note also that the rate of attendance at private schools in Texas is below that for the nation – 6 versus 11 percent in 1997 – and is virtually unchanged between 1980 and 1997.

The shifts in demographic composition were nonetheless not the primary determinant of changes in school enrollment patterns and interracial contact during this period. Rather it was the expansion of school desegregation that led to profound changes in the racial make-up of Texas public schools. Table 2 shows that the exposure index (average percentage of Blacks' schoolmates who were White) increased by roughly 50 percent between 1968 and 1980, rising from 24 to 35 percent.[6] Since 1980, however, the index has declined somewhat, reflecting the decline in the White share of enrollment and the passing of the period in which most desegregation cases took effect.

The dramatic increase in the average percentage of Blacks' schoolmates who were White at a time when the White enrollment share was declining implies that schools were becoming less segregated, i.e. that more and more Blacks and Whites were mixed together in schools. To see the enrollment patterns, we employ analogues of Lorenz curves (cf. Taeuber and James 1982).[7] These descriptions possess the desirable property of scale invariance, meaning that if White enrollment declines by 10 percent, the curves will not shift if the decline is 10 percent at each school attended by Whites. Scale invariance allows for meaningful comparisons across time despite changes in demographic composition.

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[5] Differences between the PEIMs and OCR data for 1992 suggest that the OCR either undercounted or undersampled Hispanics, which would lead to an overstatement of the decline in the White share of enrollment. This discrepancy, however, cannot overturn the significant changes that appear in the data.

[6] The exposure index equals $\Sigma^{n}_{i=1} B_i * PW_i / B$, where B_i equals the number of Blacks in school i, PW_i equals the proportion of White students in school i and B equals the number of Black students in the region.

[7] See Duncan and Duncan (1955) and Atkinson (1975) for comprehensive discussions of Lorenz curves.

The top panel of Figure 1 presents overall segregation curves calculated from OCR data for 1968 and 1980 and from PEIMS data for 1992 and 1998. The curves are derived from information on Black and White student enrollments. All schools in the sample are ordered from lowest to highest according to the White enrollment percentage in the school. The cumulative percentage of Black students is plotted against the cumulative percentage of White students. The diagonal line represents perfect integration, attainable only if each school has the population shares of Blacks and Whites. Any deviations from perfect integration cause the curve to fall below the 45 degree line, and curves further from the line indicate greater segregation.[8]

The figure documents both the substantial reduction in segregation that occurred during the 1970s and fairly constant degree of segregation from 1980 onward. While there were changes at various points in the distribution between 1980 and 1998, these shifts paled in comparison to those occurring prior to 1980.

The dissimilarity indexes reported in the second row of Table 2 not surprisingly show a similar pattern.[9] These indexes summarize the entire distribution in a single index number. They show that the percentage of Whites (or Blacks) who would have to change schools in order to achieve perfect integration. The Texas index declines from 74 percent in 1968 to 61 percent in 1980 and only slightly more in the subsequent period.

Changes in the overall segregation curves and dissimilarity indexes reflect shifts in both school enrollment patterns within districts and the distributions of Blacks and Whites into districts. In order to gain a better understanding of the constraints imposed by residential housing patterns, we follow Rivkin (1994) by aggregating the data to the school district level and reconstructing the segregation curves and dissimilarity indexes. These measures essentially ignore the patterns of school attendance within districts, focusing instead on the distribution of

[8]When curves cross there is no simple segregation ranking because crossing implies that different parts of the distribution are more or less unequal in different years. See Allison (1978) for a discussion of this issue.

[9]DEFINE DISSIMILARITY INDEX

students among districts. One interpretation of these district curves and indexes is that they bound the potential for integration given residential choice and supreme court decisions making it virtually impossible to impose cross-district remedies. Importantly, however, they provide a lower bound on the contribution of residential housing patterns, because they ignore any housing segregation that occurs within districts.

The bottom panel of Figure 1 reveals a substantial increase in residential segregation between 1968 and 1980 and rough stability in the years that follow. It is not possible to quantify the extent to which this increase in residential segregation was a direct response to district desegregation efforts. Welch and Light (1987) provide overwhelming evidence of White flight in response to the implementation of desegregation plans, though Rivkin (1994) shows that between 1968 and 1988 the trend toward White exiting of central cities occurred regardless of whether segregation plans had been adopted.[10] Moreover, Massey and Denton (1993) document that the pattern of suburbanization of Blacks and Whites carried many of the prior segregated housing arrangements to the suburbs.

The residential segregation curves represent upper bounds on the ability of districts to integrate the schools. Regardless of the integration efforts within the individual districts, the overall segregation curve could not lie inside the residential curve, and it is identical to the residential curve only when all districts are perfectly integrated. The fact that the residential dissimilarity index was 52.3 in 1998 implies that even if all Texas districts became fully integrated and there was no additional White flight, the overall dissimilarity index could drop only from 59.1 to 52.3. The convergence over time of the overall and residential segregation

[10] In their research on U.S. school districts, Welch and Light (1987) find that the most far-reaching desegregation plans on average increased White enrollment losses by approximately 4 percentage points in the single years prior to and following plan implementation and by 9 percentage points in the implementation year. Such losses far exceeded the districts' average White enrollment decline of 3.25 percentage points per year in the years outside of the implementation period between 1968 and 1985.

curves and dissimilarity indexes demonstrates both the expanded district desegregation efforts and the increased geographic separation of Black and White school age children.

Appendex Tables A1 to A3 complement the aggregate state statistics with information on the 65 school districts sampled in each of the OCR surveys used in this work. The dramatic declines in White enrollment and increases in Hispanic enrollment in Houston and Dallas along with the concurrent reduction in Black/White segregation fit exactly into the overall pattern observed for Texas. The increases in Black exposure to Whites following 1968 and decline in later years experienced by most of these districts fits into the general pattern observed for Texas. Interestingly, it is the smaller urban districts that experienced the most pronounced and long lasting gains in interracial contact, perhaps because the community structure was not as conducive to White flight. Finally, some suburban districts – Richardson being a prime example – experienced both a significant influx of Blacks and a dramatic reduction in school segregation that combined to generate a substantial increase in inter-racial contact at the school level.[11]

The cornerstone of the analysis of racial composition effects on achievement is a unique matched panel data set of school operations constructed by the UTD Texas Schools Project, a project conceived of and directed by John Kain. The data track the universe of three successive cohorts of Texas public elementary students as they progress through school, beginning with students who attended third grade in 1992. 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 peer group characteristics. We

[11] While these 65 districts share a similar pattern of reduced segregation, expanded exposure of Blacks to Whites, and reduced White enrollment in the urban districts, there are pronounced differences in the timing and magnitude of changes. In future work we intend to exploit these differences to gain a better understanding of the role that the desegregation history of districts plays in the link between achievement

use data for grades three through seven for the two younger cohorts and grades three through six for the oldest cohort. The youngest cohort attended 5th grade in 1996, while the oldest cohort attended 5th grade in 1994. Only Black, Hispanic, and White students are included; the relatively small number of Asian students and even smaller numbers of Native Americans are excluded in order to simplify the models.

The student (PEIMS) 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) and Title I services, but the panel feature can be exploited to account implicitly for time invariant individual effects on achievement gains. Importantly, students who switch schools can be followed as long as they remain in a Texas public school.

Beginning in 1993, the Texas Assessment of Academic Skills (TAAS) was administered each spring to eligible students enrolled in grades three through eight. The criteria referenced tests evaluate student mastery of grade-specific subject matter. Unique IDs link the student records with the test data. This paper presents results for mathematics, although the results are qualitatively quite similar for reading achievement. Consistent with the findings of our previous work on Texas, schools appear to exert a much larger impact on math than reading in grades 4 through 7 (see Hanushek, Kain, and Rivkin (1998) and Rivkin, Hanushek, and Kain (2000)). 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. The regression results are robust to a number of transformations including the raw percentage correct. In order to avoid complications associated with classification as limited English proficient (LEP) or disabled, all LEP and special education students are dropped from the analysis.

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and school racial composition. It may well be that the route taken to produce a given racial composition is an important determinant of the benefits of interracial contact for minority students.

Importantly, the student database can be linked to information on teachers and schools through the school IDs. The school data contain detailed information on individual teachers including grade and subject taught, class size, years of experience, highest degree earned, and student population served. While 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 school average class size and the distribution of teacher experience for teachers in regular classrooms for the appropriate grade and school year.

The inability to assign students to classrooms also means that the racial composition variables are computed by grade rather than by classroom. Such aggregation, as described below, has a beneficial effect because it reduces problems introduced by the nonrandom division of students into classes. For example, if higher achieving minority students tended to have more white classmates, estimates derived from variation at the classroom level would confound actual peer effects with unobserved student characteristics. On the other hand, if integration effects are localized to the individual classroom, imprecision in the estimation will result.

In our analysis we view integration as a special case of peer influences. . We consider how the composition of the school and the characteristics of others affect the learning of students. In that context, an important consideration is how racial and ethnic compositions of schools interact with other attributes of the student body

The identification of specific integration effects is a daunting task. Not only must the analysis address the issue of the endogenous choice of neighborhoods and schools, but it must also separate peer influences from the effects of other school characteristics. In this section we outline an empirical framework for examining peer influences. Subsequently we estimate a series of specifications using the matched panel data in an attempt to identify integration effects on math achievement. An important component of this is learning more about how specification errors, which pervade prior estimates, contaminate the results.

Attempts to directly estimate general peer effects on educational achievement have been relatively limited. Hanushek (1972, 1992) finds no peer achievement effects, while Henderson, Mieszkowski, and Sauvageau (1976), Kain and O'Brien (1999), and Summers and Wolfe (1977) report positive influences of higher achieving peers at least for some students. Consideration of ability tracking in schools likewise has yielded mixed results, even though policy has presumed that tracking is generally bad for achievement (e.g., see Argys, Rees, and Brewer 1996; Oakes 1985). The evidence on achievement effects of racial composition has been much more voluminous, although the results are no easier to summarize or interpret (cf. Armor 1995; Crane and Mahard 1978). One theme, that follows the interpretation of Moffitt (1998) and motivates our work here, is that existing peer results appear very sensitive to the measurement and specification of various influences on achievement. Jencks and Mayer (1990) show that additional family background controls tend to reduce estimated peer group effects. Moreover, Evans, Oates, and Schwab (1992) use instrumental variables to demonstrate the possibility that unmeasured influences may bias upward estimates of peer group effects, though the validity of their instruments is questionable and the results are also consistent with sizeable peer effects.[12] We attempt to replicate alternative specifications within a consistent database so that elements of the previous inconsistency of findings can be disentangled.

Empirical Model

The key issue in the identification of integration effects is the separation of the causal effects of peers from other possible influences on performance. The issues are most easily seen if we begin with a naïve educational production function model where the current achievement (A_{igs}) of student i in grade g in school s is a function of current family background (X_{igs}) and school factors (S_{gs}) along with characteristics of peers (P_{gs}).

$$A_{igs} = X_{igs}\beta + S_{gs}\delta + P_{gs}\lambda + e_{igs} \qquad (1)$$

The parameter of interest is the causal impact of peer groups (λ), and the estimate will be consistent only if the measured family background, school, and peer inputs (X, S, and P) are orthogonal to the error term. It is highly unlikely that single equation estimates of equation (1) will be consistent, particularly because current achievement is a function of the cumulative history of individual, family, school, and neighborhood inputs, many of which will not be observable. For example, if family background and income are measured with error, aggregate measures of race or family characteristics of peers may capture family influences in addition to those of peers. Fiscal externalities such as those considered by Fernandez and Rogerson (1996) could induce spending patterns that lead to correlations between race and unmeasured or poorly measured school characteristics included in e. Finally, the achievement of classmates and of the

[12] See Rivkin (2000) for a discussion of Evans, Oates, and Schwab.

individual may be simultaneously determined, with high achievement by one student directly improving the achievement of classmates and vice versa.

The solution to these myriad problems is the identification of variation in peer group characteristics that is orthogonal to the error. Such variation could be generated by natural experiments, policy actions, or the identification of valid instrumental variables. While researchers have pursued each of these courses, Moffitt (1998) raises serious doubts about the validity of existing methodologies to identify social interaction effects. He points out that there is little theoretical justification for most choices of instruments, and that many policy actions are contaminated by behavioral changes in response to the policies.

We pursue a different strategy that makes use of the repeated panels to isolate the causal effects of specific peer characteristics on mathematics achievement. In simplest terms, we use individual, school, and school by grade fixed effects to purge the error of the components that are leading to the inconsistency in the estimation of λ. Essentially the estimates are identified by within school and grade differences in race and other peer group characteristics between cohorts. We argue that such changes are unlikely to confound actual peer group effects with other influences.

Basic Value Added Specification. We begin by considering gains in achievement instead of the level of achievement identified in Eq. 1. This value added specification reduces the data requirements to the inputs relevant for grade g, since all of the historical influences on the current achievement level drop out. In level form, without quite detailed information on prior peer group, school, and family characteristics and detailed knowledge of the appropriate specification, there is no reason to believe that current peer group effects can be separated from prior peer and family influences. This approach both removes the influence of any time invariant unobserved family or individual ability influences on achievement level and isolates the impact of peer group that is specific to grade g. Nonetheless, potential problems remain which lead us to take the value added formulation further.

Consider a basic value added model (which provides the starting point for the empirical analysis):

$$\Delta A_{igs}^{c} = X_{igs}^{c}\boldsymbol{b} + S_{gs}^{c}\boldsymbol{d} + \overline{A}_{(-i)}^{c}\,\boldsymbol{l}_{A} + \overline{SD}_{(-i)}^{c}\,\boldsymbol{l}_{SD} + \overline{FL}_{(-i)}^{c}\,\boldsymbol{l}_{FL} + \overline{B}_{(-1)}^{c}\,\boldsymbol{l}_{B} + \overline{H}_{(-i)}^{c}\,\boldsymbol{l}_{H} + \boldsymbol{u}_{igs}^{c} \qquad (2)$$

where ΔA_{igs}^{c} is the achievement gain (difference between current year and previous year test scores) for student i in grade g in school s in cohort c; X is a vector of time-varying individual characteristics that includes indicator variables identifying eligibility for a free or reduced price lunch, school transfer, and participation in a Title 1 compensatory program; and S is a vector of teacher characteristics that includes average class size, percent of teachers with zero years of experience, and percent of teachers with one year of experience.

The heart of the paper is investigation of how racial and ethnic compositions of schools enter into achievement. Outside of the direct investigations of desegregation and integration, however, a variety of other attributes of peers have entered into the educational process. In this work we attempt to link the various strands of peer group effects. Specifically, we separate peer characteristics into several distinct measures. The "main effects" of race and ethnic composition are measured by $\overline{B}_{(-1)}^{c}$, the proportion of schoolmates in the same grade who are Black, and $\overline{H}_{(-i)}^{c}$, the proportion of schoolmates who are Hispanic. Additionally, indirect compositional effects include: $\overline{A}_{(-i)}^{c}$, the average prior math achievement for a student's schoolmates in the same grade; $\overline{SD}_{(-i)}^{c}$, the standard deviation of their prior math achievement; and, $\overline{FL}_{(-i)}^{c}$, the proportion of schoolmates in the same grade who are eligible for a free or reduced price lunch;

The included family and school variables control for factors that may contaminate estimated integration effects. Increases or decreases in average peer income or achievement may result from similar changes in own family income that precipitate a school transfer and exert a direct effect on outcomes. Alternatively, shifts in local labor market conditions may cause changes in both own family and peer group average income, making it difficult to disentangle the influences of peers and family. Changes in school characteristics may affect both own achievement and that of peers, and may even affect the socio-economic composition of the school. Omitting information on school characteristics may confound peer and school influences in the peer group coefficient. Finally, the availability of Title 1 programs is linked to school average income, and the absence of information on Title 1 eligibility may lead the peer average income coefficient to confound programmatic and social interaction effects. We also control for the impact of school transfers on individual achievement. Large changes in peer group characteristics carry substantial weight in the identification of peer group coefficients, and they are likely to result from school switches. Evidence suggests that the act of switching schools may reduce academic achievement in the period following a move,[13] consequently it is imperative to account for such transfers.

A prime reason for concern about each of these factors is that race and ethnic background is likely to be correlated with each. If these factors are not adequately dealt with, the influence of racial composition could appear significant when in fact it is merely a proxy for one or more of them.

Error Structure and Fixed Effect Estimation. The explanatory variables provide important controls for changes in student, family, and school circumstances, but the bulk of the confounding variation introduced by differences in students and schools is accounted for by making use of the

[13] See Hanushek, Kain, and Rivkin (1998) and Kain and O'Brien (1998, 1999) for evidence on mobility effects.

matched panel structure of the data. The error term in equation 2 (υ) has the following components:

$$\upsilon_{igs}^{c} = \omega_{i} + \omega_{g} + \omega_{c} + \omega_{s} + \omega_{gs} + \varepsilon_{igs}^{c} \tag{3}$$

The first five terms are individual, grade, cohort, school, and school by grade error components, and the final term is a random error. It is highly likely that most if not all of these error components are correlated with the peer group variables notwithstanding the inclusion of the measured explanatory variables. Specifically, unmeasured or mismeasured components of achievement growth, whether related to individual, grade, or school, can easily be correlated across individuals in the same school and grade and are likely to be correlated with measured peer attributes. In such a case peer measures will partially proxy other influences, leading to a misstatement of the causal impact of peers. The standard instrumental variables approach looks for variables that are correlated with the true peer measures but uncorrelated with the error components in (3), but finding such instruments is clearly difficult.

Our alternative is to remove the fixed components of the combined error in (3). Not only do the Texas data provide multiple observations for each student in a cohort as they progress through school, but they also report information on multiple student cohorts. This enables us to remove all of the fixed error components contained in equation (3), including fixed school by grade effects.

Notice how each of the fixed effects accounts for unobserved differences in students and schools. If only student fixed effects in gains were included, all fixed student and family factors that affected the rate of learning would be accounted for. However, any differences in schools that were not perfectly correlated with the student fixed effect or the including covariates but were correlated with peer group composition could contaminate the estimates. Controlling for school fixed effects addresses this issue, but allows for the possibility that systematic differences across grades within schools could contaminate the results. If achievement of students and their schoolmate changes over time in a systematic way, the fixed student and school effects could fail to account for all of the confounding variation in students and schools. This problem is eliminated by making use of the multiple cohorts and including school by grade fixed effects.

The importance of the multiple cohorts should not be underestimated. Consider the possibility that achievement for students in some schools tends to decline as the students age, particularly as they become adolescents. If only fixed individual and school effects are removed, the peer effect estimates would be identified by changes in achievement gains of both students and peers. The results would suggest that peer average achievement had a large impact on students when in fact the decline was brought about by other factors. On the other hand, if fixed student and school by grade effects are removed, such systematic changes in specific schools would not drive the results. A much stronger case can be made that the remaining differences in peer group characteristics result from two uncontaminated sources: random differences between cohorts in the number and characteristics of students who transfer in or out of the school as students age; and random changes in family income and achievement across cohorts for those who remain in the same school.

Measurement of Peer Influences. The central issue in this analysis is the construction of the peer group characteristics. Primary attention goes to the racial composition of schools – the general focus of most desegregation policy. In this analysis proportion Black and proportion Hispanic are calculated from information on schoolmates in the same grade.

Socio-economic status of the family is measured by the proportion eligible for a free or reduced price lunch (based on current information). The construction of this is straightforward, though proportion eligible for a reduced price lunch is likely to be a noisy measure of peer economic circumstances.[14]

The construction of the average achievement of relevant peers – a quality measure – is much more problematic. As Moffitt (1998) points out, the outcomes for all students are determined simultaneously, i.e. each student affects all others. The identification of the effect of current peer achievement would require some type of exclusion or functional form restriction, which, if violated, would lead to biased estimates. The likely existence of omitted variables bias further compounds this problem. Evidence strongly suggests that teacher quality varies substantially within schools and is an important determinant of achievement.[15] Because very little of the variation in teacher quality is explained by observable characteristics, omitted variables bias would almost certainly contaminate estimates that use current peer group achievement. Without a systematic method to control for variations in teacher quality, it is not possible to disentangle the effects of teachers from the influence of peer group academic achievement. Even partial reassignment of students on the basis of student unobservables of the type proposed by Moffitt would not identify peer group effects in the absence of measures of teacher quality.

Importantly, as long as the concern is the ability of classmates (as opposed to their contemporaneous behavior), the question of whether high achieving peers raise achievement can be addressed with a peer achievement measure from a previous period. Because teachers rarely teach successive grades and because mobility implies that average prior scores are determined by a number of teachers and schools, any link between teacher quality and peer group average achievement is considerably weakened. The use of a predetermined outcome variable also

[14] The division of students into two family income categories misses substantial within category variation. In addition, student cooperation is required to be classified and students may become more reluctant as they age, though the school by grade fixed effects should address this problem. Unfortunately, there is no additional information on family income, so that this widely-used variable is the sole indicator of economic circumstances.

eliminates the direct causal relationship between current achievement and the achievement of peers.[16]

We use the mathematics achievement score in grade g-2 for current peers to construct the average achievement and standard deviation of achievement variables.[17] The problem with achievement in the previous grade (g-1) is that the dependent variable is the test score gain. A particularly good teacher who substantially increases achievement in grade g-1 might reduce the expected gains in grade g, given that the grade g-1 test score provides the baseline with which to measure grade g achievement gains. School specific nonrandom measurement error in the grade g-1 score may also be negatively correlated with grade g gains.

As an informal specification test in preliminary work, we compared student fixed effect estimates of peer group effects for a sample of school switchers with estimates for a sample of nonswitchers. Because school switchers enter the school in grade g, downward bias should be a much bigger problem for non-switchers if grade g-1 scores are used to measure peer achievement as opposed to grade g-2 scores. The estimates confirm this belief; the coefficient on average peer achievement for g-1 scores was much more negative for the sample of nonswitchers, while the coefficients for the switcher and non-switcher samples were virtually identical if grade g-2 scores were used to measure peer achievement.

One remaining concern is the identification of the correct subset of students who are the relevant peers. Without information on the division of students into classrooms or friends, we are forced to use aggregate grade level data. We do explore the possibility that peer influences are stronger for same race/ethnicity students by calculating the proportion reduced price lunch and peer achievement variables from data for same race/ethnicity schoolmates. A comparison

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[15] See Rivkin, Hanushek, and Kain (1998) and Sanders and Horn (1994) for evidence on teacher quality.
[16] If, however, the relevant aspect of peers is how their current behavior (say, their classroom interactions with the teachers) interacts with each student's behavior and outcomes, there is little hope for separating peers from the achievement determination of each student.
[17] Own test score is also excluded in the calculation of the standard deviation of achievement.

between specifications that use data for all students with those that use data for same race students provides information on the racial/ethnic character of social interaction effects.[18]

The estimates of peer group effects are presented in Tables 3-5. Table 3 reports results from levels and value added specifications that do not remove either student or school fixed effects. These preliminary specifications are similar to the bulk of existing work, and they provide a baseline from which to compare the fixed effect estimates. Table 4 reports results from student, school, and school by grade fixed effects specifications. Using the same models, Table 5 reports results from specifications that permit peer group effects to vary by a student's ranking in her school's test score distribution. Specifically, separate peer group effects are estimated for each of the four quartiles of the test score distribution (based on scores in grade g-2 in order to avoid problems introduced by using information contained in the dependent variable as a regressor).

All specifications include interaction terms between race and the percent minority variables to permit the effects to differ based on student race and ethnicity. In addition, each specification is estimated twice, once with peer group characteristics constructed from information on the entire student body and once with peer group characteristics constructed from information restricted to own race/ethnicity schoolmates. Though not reported, all specifications contain dummy variables for the race and ethnicity of each child along with dummy variables for reduce price lunch eligibility, school transfer, Title 1 program eligibility, and cohort by grade indicators (the exception are the specifications that include cohort by grade fixed effects). Table 3 also reports estimates in which only subsets of the peer characteristics are included. Because the

[18] The interpretation could be complicated if there were systematic racial placements in specific classrooms. In the absence of nonlinearities in the effects, however, the grade averages (which are equivalent to using campus and grade as instruments, yields consistent estimates of the effects. While this dataset does not permit looking at within grade placements, we are investigating the possibility of using a related dataset to consider whether within grade segregation is an important problem.

estimates are quite insensitive to the inclusion of the other peer group variables, the remaining tables report results only from specifications that include all five characteristics.

Baseline Models. Table 3 presents basic models of the importance of racial composition for explaining the level and growth of achievement. The top panel considers just racial composition as the complete measure of student body peers.[19] These models, explaining the level of student achievement. Such models are commonplace in prior analysis estimated from three separate regressions in a manner comparable to most past estimation.

The bottom panel reports estimates from specifications that simultaneously include not only racial composition but also other measures of family and of achievement. We focus our discussion on this latter set of estimates, because the results for the value added models are quite similar in the two panels. Because the estimates are quite insensitive to the inclusion of measurable aspects of school variables, all tables report only specifications that include the school variables.

Not surprisingly, there is a very strong positive relationship between math achievement level and the average achievement of peers in the levels specifications (col. 1 and 3). However, this relationship disappears or is reversed for the value-added specifications that examine growth in achievement (col. 2 and 4). Coefficients from the levels specifications almost certainly confound peer effects with omitted family characteristics and achievement growth models substantially reduce the problem of omitted variables. We would nevertheless expect the value-added estimates to confound unobserved influences on the rate of learning with peer group effects and thus overstate true peer group effects.[20]

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[19] The top panel was replicated to obtain effects of economic composition (% free lunch), and the mean and standard deviation of schoolmate's achievement.

[20] One possible explanation for the negative value added estimates is related to the test score instrument used in Texas. The test does a poor job of capturing gains in knowledge at the upper end of the distribution. To the extent that lower achieving students are catching up to others in Texas in terms of basic skills and

In contrast to the levels specifications, the value-added estimates conform to prior expectations. A higher proportion of Black or Hispanic schoolmates significantly reduces each student's achievement gains. The effects of racial composition appear to be larger for own race students. In other words, these estimates provide an achievement argument for racial integration.

In completing the picture, a higher proportion of classmates eligible for free or reduced lunch significantly lowers achievement gains. On the other hand, achievement gains are not negatively related to the standard deviation of student achievement as might be expected if heterogeneity reduces the effectiveness of classroom instruction. The negative relationship observed in the levels specifications disappears once value added models are introduced. (Note, however, that we do not have heterogeneity at the classroom level, but instead at the grade level).

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the third controls for student and school by grade fixed effects. In our opinion, it is the variation in peer group variables that remains after controlling for both student and school by grade fixed effects that offers the most convincing identification of the true effects of peers on mathematics achievement.

The full estimates of integration effects, shown in Table 4, reveal interesting results. Though the coefficient magnitudes differ slightly, whether peer characteristics are constructed from all students in the grade or just own-race students in the grade appears to matter little. Comparing estimates from the right and left panel shows that they are not very sensitive to the construction of the peer group variables except in the case of proportion eligible for reduced price lunch.

In sharp contrast, within both columns 1-3 and 4-6 the estimates are quite sensitive to the error specification and estimation. Student fixed effects produce quite different estimates than the simple value added models in Table 3, and the addition of school fixed effects further leads to sizeable changes for most variables. Perhaps most important, specifications that include school by grade fixed effects produce quite different estimates than those that control for only school fixed effects. This sensitivity to specification is visible evidence that problems of omitted variables bias are quite severe and difficult to account for even with panel data. The availability of the multiple cohorts permits the inclusion of a much more comprehensive set of controls that significantly alters the estimated coefficients and conclusions about peer effects.

The specific estimates point to the importance of peer achievement and racial composition and the lack of effect of peer income. The estimates support the belief that peer average achievement has a positive, albeit small, effect on mathematics achievement gains. The estimates in columns 3 and 6 suggest that a one standard deviation increase in peer average achievement leads to less than a 0.05 standard deviation increase in math gains. Notice that the coefficient magnitude is almost twice as large if school rather than school by grade fixed effects are included.

The racial and ethnic composition variables suggest that Blacks benefit from attending school with higher proportions of nonBlacks, and the results are quite strong (t-statistics>6). A ten percentage point reduction in school percentage Black is associated with a .03 standard deviation increase in math gains. The estimated effects of minority concentrations on either white or Hispanic students are essentially zero (the negative interaction term for Hispanics is opposite in sign and roughly equal in magnitude to the main effect).

Despite small differences on the basis of the all or own-race distinction, neither the estimates on column 3 nor those in column 6 support the view that lower income peers harm achievement. Finally, there is no evidence in any of the specifications that changes in the heterogeneity of students affects the rate of achievement growth. This finding suggests that ability grouping per se may have minimal effects on average achievement even though it likely alters the distribution of achievement.

Differences by Quartile. The results in Table 4 reveal significant but small effects of peer average achievement, but the possibility remains that peer influences affect some students more than others depending on their achievement levels relative to schoolmates. To examine this possibility, we interacted all five peer group variables with indicators for the student's position in the school achievement distribution. (All specifications also include indicators for the main effect of achievement quartile).

The results indicate that most peer group effects exhibit little variation by quartile.[21] There is virtually no difference among quartiles in the magnitude of the effect of school proportion Black (main plus interaction effect). Similarly, peer average achievement effects are slightly higher for students in the center quartiles of the distribution, but the differences are small, particularly when the characteristics of own race peers are used.

21 Preliminary work also showed that the addition of quadratic terms added little if any explanatory power.

The difficulties of isolating school and peer group effects have been well documented. The role of peers, particularly in the context of racial integration, can be complex. By using a very large, matched panel data set from the state of Texas, we overcome many of the myriad methodological problems that impede the estimation of these effects. The results certainly support the view that standard specifications are subject to biases, as the sequential introduction of student, school, and school by grade fixed effects leads to substantial changes in the magnitude and often the direction of peer effect estimates. We believe that the variation in peer group characteristics that remains after controlling for student and school by grade fixed effects in the rate of achievement growth and a number of time varying student, family, and school characteristics provides the most valid source of identification for the estimation of peer group effects.

In addition, it appears that proportion Black has a strong and significant effect on the mathematics achievement growth for Blacks but not for nonBlack students. A ten percent change in Black classmates yields a 3-3.5 percent change in mathematics performance for Black students. What is particularly important is that this effect cannot be interpreted as school quality effects or as the effect of achievement differences of classmates. These effects are eliminated by the modeling structure. Moreover, these effects do not appear for White or Hispanic students.

These estimates warrant further investigation. Specifically, the racial composition of schools, as discussed earlier, has changed dramatically and has in many instances been under court supervision. It would be important to investigate whether the path to any specific racial composition affects performance. Additionally, the changing face of schools, documented and analyzed by Kain and O'Brien (1998, 1999), has led to widely different school situations for Blacks in Texas. Again, if there are different outcomes based on the path of individual students, it is important to separate these. While these analyses are beyond the scope of this paper, they are feasibly studied within the Texas data set.

The results indicate that average peer achievement affects learning, though the magnitude of the effect is quite small. Many previous studies have not found such consistent and statistically significant effects of either peer average achievement or proportion Black on measured achievement, but a quite plausible explanation is that the sample size of the Texas data permit precise estimation of quite small effects. In fact these results parallel the finding of small but precise estimates of class size effects.

The results themselves provide little evidence that average income or the heterogeneity of peers in terms of variation in achievement levels affects growth in mathematics achievement. These results should be qualified by the fact that proportion eligible for a reduced price lunch is a noisy measure of income and by the fact we use grade rather than classroom level data. While it

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School Year	1968	1980	1992	1992	1998
Data Source	OCR	OCR	OCR	PEIMS	PEIMS
Percentage Black	16.1	14.4	15.2	14.3	14.4
Percentage Hispanic	19.3	30.4	29.7	34.5	37.9
Percentage White	64.3	54.1	52.7	48.8	45.0
Enrollment	2,662,720	2,846,106	3,504,860	3,464,371	3,897,641

Table 2. Black/White Exposure and Dissimilarity Indexes, 1968 to 1998

School Year	1968	1980	1992	1992	1998
Data Source	OCR	OCR	OCR	PEIMS	PEIMS
	24.4	35.2	33.0	34.6	30.9
Overall	74.2	61.1	59.6	57.5	59.1
Residential	44.3	54.7	53.3	51.7	52.3

(absolute value of huber adjusted t statistics in parentheses)

	All Peer Characteristics		Own Race Peer Characteristics	
	Level	Gain	Level	Gain
	1	2	3	4
proportion black	-0.10	-0.05	-0.10	-0.05
	(3.35)	(2.92)	(3.19)	(2.91)
black*proportion black	0.08	-0.09	0.08	-0.09
	(1.71)	(3.64)	(1.76)	(3.54)
proportion hispanic	0.12	-0.06	0.12	-0.06
	(5.47)	(6.00)	(5.54)	(5.97)
hispanic*proportion hispanic	0.13	-0.05	0.14	-0.05
	(5.21)	(3.61)	(5.18)	(3.51)
sample size	1,385,816		1,380,140	

Table 3. Estimated Effects of Peer Group Characteristics on Mathematics Achievement Level and Achievement Gains, Part II

B. Combined Peer Effect Models

	All Peer Characteristics		Own Race Peer Characteristics	
	Level	Value added	Level	Value added
	1	2	3	4
Proportion eligible for reduced price lunch	0.14	-0.09	0.03	-0.06
	(4.68)	(4.57)	(1.48)	(4.11)
average math score in grade g-2	0.37	-0.07	0.30	-0.06
	(22.92)	(5.98)	(25.67)	(7.33)
Standard deviation of scores in grade g-2	-0.16	0.06	-0.17	0.02
	(5.23)	(2.47)	(10.69)	(1.30)
proportion black	0.16	-0.07	-0.06	-0.04
	(4.81)	(3.57)	(2.31)	(2.58)
black*proportion black	0.03	-0.08	0.05	-0.09
	(0.67)	(3.41)	(1.38)	(3.56)
proportion hispanic	0.28	-0.06	0.17	-0.06
	(10.73)	(3.90)	(8.73)	(5.45)
hispanic*proportion hispanic	0.08	-0.04	0.10	-0.04
	(3.61)	(2.76)	(4.56)	(2.96)
sample size	1,385,816		1,380,140	

Note: all equations include school characterstics.

(absolute value of huber adjusted t statistics in parentheses)

	All Peer Characteristics			Own Race Peer Characteristics		
	1	2	3	4	5	6
school fixed effects	No	yes	no	no	yes	no
school by grade fixed effects	No	no	yes	no	no	yes
proportion eligible for reduced price lunch	0.10	0.09	0.09	0.06	0.02	0.00
	(2.36)	(1.22)	(1.28)	(2.20)	(0.60)	(0.09)
average math score in grade g-2	0.06	0.12	0.05	0.04	0.07	0.03
	(2.69)	(4.14)	(1.92)	(2.75)	(4.49)	(2.56)
standard deviation of scores in grade g-2	0.04	0.06	0.00	0.01	0.00	-0.02
	(1.01)	(1.25)	(0.03)	(0.43)	(0.23)	(0.93)
proportion black	-0.02	0.02	-0.08	-0.01	0.00	-0.07
	(0.38)	(0.13)	(0.64)	(0.26)	(0.04)	(0.55)
black*proportion black	0.15	-0.25	-0.26	0.16	-0.20	-0.23
	(2.79)	(7.64)	(8.23)	(2.90)	(5.54)	(6.82)
proportion hispanic	-0.06	0.27	0.07	-0.02	0.27	0.09
	(1.24)	(2.29)	(0.73)	(0.65)	(2.38)	(1.01)
hispanic*proportion hispanic	-0.06	-0.06	-0.07	-0.07	-0.05	-0.06
	(1.59)	(2.14)	(2.75)	(1.75)	(1.48)	(2.18)
sample size		1,386,386			1,380,583	

Note: all equations include school characteristics and individual fixed effects.

Table 5. Estimated Effects of Schoolwide and Own Race Peer Group Characteristics on Mathematics Test Score Gains by Quantile of School Test Score Distribution, Controling for Student and School by Grade Fixed Effects
(absolute value of huber adjusted t statistics in parentheses)

	Quartile of School Test Score Distribution			
	Bottom Quartile	Second Quartile	Third Quartile	Top Quartile
A. Peer Group Characteristics for all Students				
proportion eligible for reduced price lunch	0.10	0.10	0.10	0.05
	(1.30)	(1.51)	(1.45)	(0.66)
average math score in grade g-2	0.09	0.10	0.11	0.05
	(2.68)	(3.60)	(4.24)	(1.88)
standard deviation of scores in grade g-2	-0.03	0.01	0.09	0.04
	(0.49)	(0.22)	(1.92)	(0.96)
proportion black	-0.17	-0.15	-0.12	-0.07
	(1.28)	(1.15)	(0.94)	(0.58)
black*proportion black	-0.13	-0.18	-0.16	-0.25
	(2.69)	(4.44)	(4.42)	(6.76)
proportion hispanic	-0.01	0.01	0.05	0.12
	(0.13)	(0.10)	(0.47)	(1.20)
hispanic*proportion hispanic	0.02	-0.03	-0.04	-0.08
	(0.50)	(0.92)	(1.33)	(2.97)
sample size	1,386,386			
B. Peer Group Characteristics for own race				
proportion eligible for reduced price lunch	0.04	0.05	0.05	-0.02
	(1.23)	(1.72)	(1.57)	(0.74)
average math score in grade g-2	0.03	0.06	0.06	0.04
	(1.66)	(3.76)	(3.96)	(2.42)
standard deviation of scores in grade g-2	-0.08	-0.02	0.03	0.05
	(3.33)	(0.68)	(1.43)	(2.15)
proportion black	-0.19	-0.17	-0.13	-0.06
	(1.47)	(1.36)	(1.00)	(0.46)
black*proportion black	-0.10	-0.14	-0.14	-0.22
	(1.93)	(3.22)	(3.49)	(5.64)
proportion hispanic	0.00	0.01	0.06	0.14
	(0.06)	(0.12)	(0.62)	(1.47)
hispanic*proportion hispanic	0.02	-0.02	-0.04	-0.07
	(0.45)	(0.68)	(1.36)	(2.14)
sample size	1,380,583			

Appendix Table A1. Racial Composition of Texas Districts, 1968-1998

	Percentage Black (%)				Percentage Hispanic (%)				Percentage White (%)			
	1968	1980	1992	1998	1968	1980	1992	1998	1968	1980	1992	1998
ABILENE	7.0	9.1	10.5	11.4	11.1	20.0	24.7	27.2	81.8	69.6	63.2	59.7
ALDINE	21.0	17.1	32.3	35.9	7.9	18.1	32.7	45.0	70.6	62.5	30.3	15.4
AMARILLO	7.1	8.5	9.0	10.0	5.4	13.6	22.6	29.7	87.5	75.3	65.2	57.2
ARLINGTON	1.9	3.9	12.7	18.2	1.6	4.0	10.8	18.1	96.3	90.1	70.6	56.5
AUSTIN	15.0	18.6	19.1	17.8	19.2	27.2	35.9	42.9	65.6	52.8	42.8	36.7
BARBERS HILL	10.8	5.4	3.4	2.8	0.8	2.7	5.6	10.4	88.4	91.7	90.8	86.5
BECKVILLE	22.5	13.0	13.1	16.5	0.0	1.7	1.4	3.2	77.5	85.3	85.0	78.6
BIRDVILLE	0.4	0.5	2.0	3.2	0.7	4.5	8.1	11.9	98.7	93.1	85.8	79.6
BRENHAM	40.7	32.3	30.1	29.5	1.1	2.7	6.3	10.4	58.2	64.2	62.3	58.0
BRYAN	26.6	23.7	23.0	24.4	12.0	18.1	25.8	31.2	61.3	57.6	50.7	43.7
CARRIZO SPRINGS	1.6	0.9	1.0	0.9	74.5	81.0	85.1	87.5	23.9	18.0	13.4	11.1
CHANNELVIEW	0.0	2.2	9.2	13.1	1.3	10.7	24.8	36.4	98.6	84.7	63.6	48.7
CLEAR CREEK	0.3	2.2	5.9	6.6	2.5	4.3	8.7	11.3	95.5	90.8	77.9	73.6
COMMERCE	17.7	20.5	21.2	22.7	0.7	2.1	3.2	5.7	81.6	75.8	74.2	69.2
CONROE	14.1	5.1	6.1	5.6	1.0	2.8	10.7	14.3	84.7	91.5	81.9	78.4
CORPUS CHRISTI	5.4	5.9	5.7	5.9	46.6	65.6	67.7	68.4	47.9	28.0	25.7	24.4
CYPRESS-FAIRBANKS	17.4	4.3	8.0	9.1	3.2	6.4	13.0	18.8	79.3	85.9	71.9	64.4
DALLAS	30.8	49.5	45.5	40.7	7.6	19.0	36.5	47.1	61.2	30.1	15.7	10.2
DEL VALLE	11.3	16.4	14.4	13.5	18.6	35.2	43.0	56.9	69.3	46.6	40.6	27.8
EAGLE PASS	0.0	0.0	0.0	0.1	86.7	95.4	96.5	96.8	13.3	4.4	2.7	1.8
ECTOR COUNTY	6.5	5.7	5.5	4.9	14.7	30.2	44.0	50.9	78.7	63.7	49.7	43.1
EL PASO	2.9	3.9	4.9	4.7	54.2	67.0	72.7	76.5	42.3	28.1	21.2	17.5
ENNIS	26.2	22.7	19.1	16.8	11.1	18.6	27.4	34.4	62.6	58.5	53.2	48.5
FERRIS	46.2	29.5	19.6	12.9	11.5	21.8	28.6	33.4	42.3	48.5	51.8	53.6
FORT BEND	16.5	18.1	28.5	27.5	32.1	17.1	14.3	16.3	51.4	60.4	46.6	42.3
GALVESTON	38.6	42.9	40.3	36.7	19.0	23.3	26.0	31.5	42.2	32.8	31.1	29.1
GARLAND	4.6	7.4	12.6	15.4	1.7	7.5	15.1	22.6	93.5	83.1	67.3	55.6
HARDIN-	27.7	23.5	13.7	13.1	0.3	1.1	1.6	2.4	72.0	75.3	84.6	84.4

JEFFERSON												
HOUSTON	33.3	44.9	37.0	34.0	12.9	27.8	46.5	52.4	53.3	25.2	13.6	10.8
IRVING	2.1	2.5	10.7	13.8	2.8	8.6	24.9	38.3	94.9	87.1	58.0	41.1
KATY	13.3	2.9	4.6	4.8	3.4	5.8	10.2	13.3	83.3	88.9	81.4	77.6
KLEIN	24.9	4.3	9.5	12.4	1.6	2.3	10.8	14.6	73.3	91.3	72.9	65.8
LEWISVILLE	4.9	2.6	4.6	5.7	0.8	4.2	6.9	9.5	94.0	91.6	86.1	81.5
LUBBOCK	11.4	13.1	13.5	14.3	19.1	30.5	37.0	40.8	69.4	55.6	48.3	43.5
MART	48.9	31.4	30.6	30.2	3.3	4.1	3.6	5.9	47.8	64.4	65.7	63.9
MIDLAND	12.0	11.9	10.0	10.2	10.9	23.0	32.0	37.5	77.1	64.4	57.1	51.0
MINEOLA	22.8	14.2	14.1	11.4	0.2	1.6	5.3	12.6	77.1	83.8	80.4	75.1
MONTGOMERY	47.5	22.1	11.9	8.4	0.0	2.0	4.0	4.5	52.5	76.0	83.8	86.8
NORTH EAST	0.1	3.5	7.5	9.1	7.4	19.3	30.1	35.6	92.0	75.7	60.4	52.8
NORTH FOREST	38.1	86.1	88.0	83.8	3.3	6.6	9.7	14.9	58.4	7.2	2.2	1.2
NORTHSIDE	1.8	4.4	6.3	6.7	16.1	37.0	48.6	50.7	81.8	57.4	43.3	40.2
PALESTINE	38.6	30.8	31.7	32.3	1.3	6.8	12.5	18.6	60.1	61.8	55.4	48.6
PASADENA	0.0	2.0	4.7	5.3	5.9	23.3	42.6	56.3	93.8	71.3	48.1	34.6
PLAINVIEW	8.0	7.8	7.3	6.9	27.5	46.1	56.6	62.1	64.5	46.0	35.8	30.3
PLANO	6.6	3.0	5.1	6.1	2.4	2.5	5.3	7.6	91.0	93.5	83.2	75.5
PLEM-STIN-PHIL	0.0	0.2	0.6	0.3	1.5	8.0	11.0	14.4	98.5	91.8	86.2	83.7
PORT ARTHUR	41.0	54.3	56.2	58.9	4.0	5.9	10.4	16.3	54.9	34.7	23.7	16.1
RICHARDSON	4.1	5.3	14.8	20.4	0.6	1.7	8.2	15.3	95.1	90.1	68.7	55.2
ROBSTOWN	2.0	1.7	0.8	0.9	86.7	96.0	98.0	97.5	11.3	2.2	1.1	1.5
ROCKWALL	16.0	6.5	3.5	3.8	1.2	2.5	5.7	9.3	82.8	90.4	90.2	85.6
SAN ANGELO	5.3	5.1	5.8	5.9	21.5	32.7	39.2	43.1	73.1	61.6	53.8	49.8
SAN ANTONIO	14.7	14.6	11.3	10.4	58.2	74.0	82.1	84.1	26.9	11.0	6.2	5.1
SAN AUGUSTINE	49.8	51.9	55.5	57.2	0.1	0.5	1.5	4.4	50.1	47.5	42.8	38.4
SEYMOUR	6.0	7.1	6.1	6.7	7.1	9.2	15.9	13.9	86.9	83.1	77.6	78.8
SHERMAN	12.0	15.1	18.1	19.2	0.0	1.9	5.5	10.5	88.0	82.4	74.8	68.6
SNYDER	5.2	4.8	4.5	4.0	16.8	28.6	33.8	39.9	77.8	66.4	61.4	55.7
SPRING BRANCH	0.0	4.4	7.6	6.3	0.9	8.8	36.8	45.7	98.9	81.5	47.1	40.2
TEMPLE	22.4	20.8	24.7	27.4	11.1	13.8	18.6	21.0	66.2	64.7	55.3	50.0

TEXARKANA	27.0	34.9	44.3	49.4	0.1	0.3	1.2	2.8	72.8	64.3	53.4	46.8
TEXAS CITY	9.1	14.3	16.5	18.6	9.2	17.0	22.5	25.1	81.6	67.4	59.7	55.2
TYLER	29.3	34.1	34.3	36.5	0.6	4.3	14.0	21.7	70.0	61.3	51.0	40.9
VIDOR	0.0	0.0	0.0	0.3	1.1	1.9	2.4	2.4	98.8	98.1	97.4	97.1
WACO	19.9	36.8	41.5	40.7	12.0	17.3	29.5	36.2	68.0	45.4	28.3	22.5
WEST OSO	22.4	17.6	15.8	14.5	77.1	81.2	82.3	82.3	0.5	1.1	1.7	3.1
WICHITA FALLS	12.7	15.5	15.5	16.1	6.9	10.7	15.0	17.5	80.1	71.7	67.0	63.4
YSLETA (EL PASO)	2.9	2.4	2.6	2.5	57.8	73.9	81.6	86.1	38.9	22.9	14.9	10.6

Table A2. Enrollment in Selected Texas School Districts, 1968 to 1998

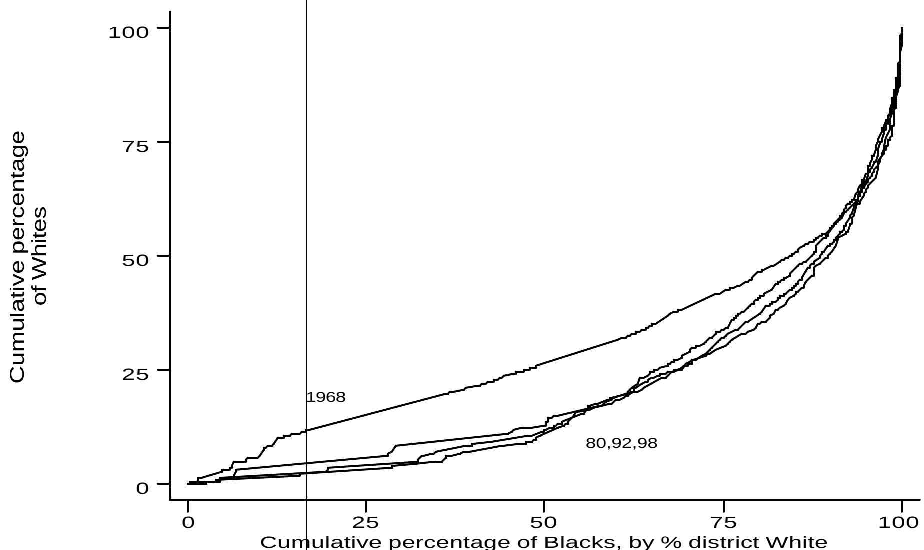
	Enrollment			
	1968	1980	1992	1998
ABILENE	19,465	17,901	18,940	19,578
ALDINE	20,344	34,186	42,389	48,583
AMARILLO	29,821	26,407	27,943	29,408
ARLINGTON	20,295	33,200	44,505	54,603
AUSTIN	51,760	55,369	66,683	76,524
BARBERS HILL	612	1,404	1,756	2,366
BECKVILLE	427	538	420	473
BIRDVILLE	13,862	15,944	18,905	20,739
BRENHAM	3,380	3,556	4,543	4,953
BRYAN	8,703	9,699	11,872	13,584
CARRIZO SPRINGS	1,752	2,864	2,374	2,327
CHANNELVIEW	3,336	4,716	5,268	6,337
CLEAR CREEK	9,897	18,607	21,892	28,281
COMMERCE	1,336	1,474	1,587	1,676
CONROE	7,155	19,231	23,371	30,933
CORPUS CHRISTI	46,110	37,383	41,330	40,940
CYPRESS-FAIRBANKS	5,785	22,228	43,776	55,760
DALLAS	159,924	129,305	137,628	157,360
DEL VALLE	2,947	4,099	5,096	5,238
EAGLE PASS	4,622	8,123	10,622	11,842
ECTOR COUNTY	24,855	23,502	27,527	28,622
EL PASO	62,105	61,285	64,176	63,985
ENNIS	3,102	3,335	4,165	4,663
FERRIS	1,041	921	1,476	1,841
FORT BEND	4,369	19,794	38,664	49,260
GALVESTON	13,030	9,765	8,372	10,023
GARLAND	19,135	30,383	39,192	46,655
HARDIN-JEFFERSON	1,832	1,767	2,086	2,351
HOUSTON	246,098	194,060	196,198	210,983
IRVING	22,721	21,325	23,889	27,131

KATY	1,482	8,424	20,407	28,335
KLEIN	1,766	17,975	27,020	31,130
LEWISVILLE	3,041	10,972	20,372	32,610
LUBBOCK	33,143	29,928	29,268	30,105
MART	910	703	647	699
MIDLAND	18,154	15,559	21,654	23,338
MINEOLA	1,216	1,203	1,609	1,550
MONTGOMERY	596	1,210	2,044	3,042
NORTH EAST	25,772	33,930	41,093	46,685
NORTH FOREST	14,556	17,375	12,306	13,400
NORTHSIDE	16,837	33,517	51,884	60,331
PALESTINE	3,924	3,342	3,686	3,818
PASADENA	33,756	36,577	38,402	40,882
PLAINVIEW	6,568	6,051	6,127	6,310
PLANO	4,139	23,027	31,967	43,316
PORT ARTHUR	17,055	11,615	11,959	11,575
RICHARDSON	26,318	37,128	32,706	34,081
ROBSTOWN	5,243	4,534	4,377	4,315
ROCKWALL	1,181	2,982	4,449	6,940
SAN ANGELO	14,885	14,035	16,959	17,242
SAN ANTONIO	79,353	60,994	59,662	61,131
SAN AUGUSTINE	1,542	1,359	1,132	1,074
SEYMOUR	1,137	868	742	777
SHERMAN	7,042	6,076	5,926	6,070
SNYDER	3,616	3,444	3,356	3,225
SPRING BRANCH	35,704	33,354	27,095	30,896
TEMPLE	7,508	7,924	8,073	8,719
TEXARKANA	7,127	6,026	5,612	5,301
TEXAS CITY	7,312	5,882	5,940	5,993
TYLER	16,239	15,985	16,515	16,653
VIDOR	5,506	5,979	5,614	5,658
WACO	18,873	13,887	14,686	16,137
WEST OSO	2,533	2,084	1,916	1,959
WICHITA FALLS	18,815	14,502	15,117	15,564
YSLETA (EL PASO)	30,208	44,820	49,932	47,641

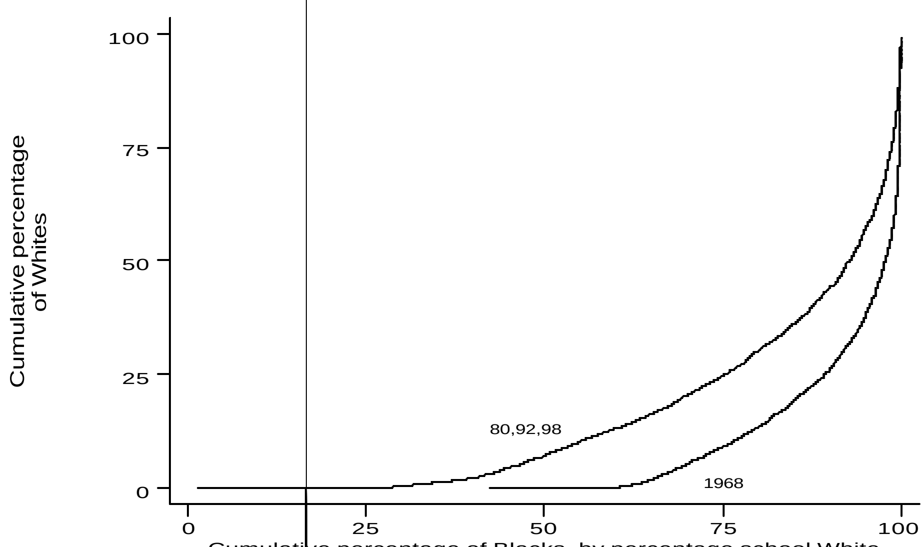
Table A3. Black/White Exposure and Dissimilarity Indexes for Selected Texas School Districts, 1968 to 1998

	Dissimilarity Index (%)				Exposure Index (%)			
	1968	1980	1992	1998	1968	1980	1992	1998
ABILENE	48.5	36.4	30.2	24.8	52.8	60.7	58.1	56.1
ALDINE	88.8	18.6	23.8	30.5	9.1	61.5	29.6	14.8
AMARILLO	76.1	38.5	47.3	52.1	39.2	67.8	54.1	44.8
ARLINGTON	69.7	42.6	33.3	36.2	86.3	82.4	61.9	47.3
AUSTIN	85.2	41.7	53.6	58.5	12.0	45.1	29.1	22.1
BARBERS HILL	3.4	6.9	7.9	12.0	88.4	91.5	90.8	86.2
BECKVILLE	13.2	6.8	1.3	16.0	76.5	85.1	85.0	77.4
BIRDVILLE	78.7	36.6	20.0	16.4	95.9	90.3	85.1	78.6
BRENHAM	58.1	4.6	9.6	12.1	29.2	64.0	61.4	56.8
BRYAN	82.3	10.7	21.3	25.5	13.6	57.0	46.4	39.4
CARRIZO SPRINGS	45.1	21.7	14.5	15.7	18.1	19.1	14.0	11.3
CHANNELVIEW	n.a.	36.0	31.6	25.5	n.a.	82.5	62.1	46.2
CLEAR CREEK	50.3	29.1	19.7	26.3	93.2	87.8	75.9	70.3
COMMERCE	7.6	9.7	5.9	6.0	81.3	75.2	74.0	68.8
CONROE	58.7	36.4	38.3	42.4	37.4	84.5	73.1	65.8
CORPUS CHRISTI	90.7	50.2	51.1	42.3	8.8	21.5	20.1	22.0
CYPRESS-FAIRBANKS	74.9	31.7	26.2	28.4	22.2	81.2	66.9	59.2
DALLAS	93.8	62.9	64.7	66.7	5.6	16.6	9.1	6.2
DEL VALLE	19.0	9.5	11.9	12.4	67.1	46.2	40.0	27.3
EAGLE PASS	n.a.	n.a.	76.8	88.9	n.a.	n.a.	6.6	1.4
ECTOR COUNTY	97.1	76.6	28.1	25.0	6.8	18.4	49.1	44.1
EL PASO	49.0	39.1	40.6	44.9	48.0	39.1	28.4	23.6
ENNIS	37.6	6.4	8.4	7.1	41.5	58.6	52.9	48.5
FERRIS	71.7	5.6	6.2	9.8	18.0	48.6	51.6	53.7
FORT BEND	13.1	57.0	67.8	58.2	51.3	41.8	20.4	22.2
GALVESTON	63.3	21.7	23.5	41.9	17.7	29.1	27.7	21.8
GARLAND	66.3	44.9	17.1	19.3	56.4	59.4	65.6	53.9
HARDIN-JEFFERSON	32.6	19.9	15.7	13.2	63.1	72.3	82.9	82.6
HOUSTON	91.7	70.8	64.9	67.7	5.7	12.2	9.0	7.0

IRVING	55.6	29.8	21.7	25.0	88.3	84.5	57.5	41.3
KATY	57.4	29.2	24.7	21.2	36.9	86.9	79.0	74.4
KLEIN	39.3	42.1	48.2	57.1	47.3	81.5	57.3	43.2
LEWISVILLE	33.7	32.6	24.5	29.0	89.8	89.5	83.1	76.8
LUBBOCK	92.2	62.9	53.9	58.9	13.5	30.4	31.9	24.4
MART	57.5	2.6	3.4	8.0	27.8	64.5	65.6	63.5
MIDLAND	58.4	30.2	30.5	27.7	36.6	58.2	50.7	46.2
MINEOLA	7.6	8.2	8.3	8.8	76.7	83.5	80.0	75.7
MONTGOMERY	88.5	4.5	7.0	1.9	9.7	75.8	83.4	86.7
NORTH EAST	64.8	40.9	39.9	41.5	91.2	71.0	53.7	43.3
NORTH FOREST	69.4	59.9	45.1	46.3	24.8	4.9	1.8	1.1
NORTHSIDE	39.4	44.4	35.5	34.8	75.7	49.1	39.1	36.8
PALESTINE	71.5	6.9	7.9	4.8	20.5	61.6	54.8	48.4
PASADENA	78.1	46.4	42.2	43.6	94.5	69.2	47.2	35.3
PLAINVIEW	47.8	11.1	12.4	19.7	40.7	46.8	35.8	29.4
PLANO	28.4	38.4	28.7	25.4	87.2	88.4	79.1	72.4
PLEM-STIN-PHIL	n.a.	32.0	24.0	66.9	n.a.	92.6	87.3	84.0
PORT ARTHUR	83.2	66.1	42.2	47.7	12.5	17.8	19.3	12.7
RICHARDSON	96.4	44.4	42.5	40.4	3.5	77.2	56.2	45.0
ROBSTOWN	15.4	33.5	23.4	24.0	16.7	2.7	1.3	1.6
ROCKWALL	1.4	9.3	24.0	18.5	82.8	90.0	87.4	82.7
SAN ANGELO	54.0	46.7	31.8	28.4	41.6	45.6	46.5	46.1
SAN ANTONIO	83.9	66.6	67.1	62.4	9.2	10.0	5.6	5.1
SAN AUGUSTINE	91.4	6.2	2.8	3.8	7.9	47.3	42.8	38.3
SEYMOUR	19.0	17.1	9.5	13.5	84.9	81.7	77.2	78.2
SHERMAN	41.5	9.2	14.6	13.5	74.3	81.3	72.2	65.4
SNYDER	57.1	40.2	36.5	13.0	59.1	49.6	47.9	55.7
SPRING BRANCH	85.3	47.7	45.1	51.7	96.9	66.6	38.0	31.1
TEMPLE	67.7	43.0	23.7	21.2	25.0	47.6	50.1	46.2
TEXARKANA	65.4	35.1	30.3	28.7	28.1	52.4	42.6	39.8
TEXAS CITY	32.3	12.9	9.7	15.8	69.2	63.6	58.9	53.2
TYLER	81.2	49.4	57.0	56.7	16.2	43.9	32.7	27.0
VIDOR	n.a.	n.a.	n.a.	51.9	n.a.	n.a.	n.a.	96.5
WACO	86.6	13.1	21.8	20.7	10.8	45.3	27.9	22.6
WEST OSO	63.7	18.9	19.9	9.2	0.5	1.1	1.7	3.2
WICHITA FALLS	82.2	40.2	45.9	39.1	18.9	58.9	52.7	53.0
YSLETA (EL PASO)	73.4	28.8	21.3	22.7	23.3	36.9	26.7	18.6



Residential Segregation Curves, 1968 to 1998



Overall Segregation Curves, 1968 to 1998