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The Effects of School Desegregation on Mixed-Race Births

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Abstract

We find a strong positive correlation between black exposure to whites in their school district and the prevalence of later mixed-race (black-white) births, consistent with the literature on residential segregation and endogamy. However, that relationship is significantly attenuated by the addition of a few control variables, suggesting that individuals with higher propensities to have mixed-race births are more likely to live in desegregated school districts. We exploit quasi-random variation to estimate causal effects of school desegregation on mixed-race childbearing, finding small to moderate statistically insignificant effects. Because the upward trend across cohorts in mixed-race childbearing was substantial, separating the effects of desegregation plans from secular cohort trends is difficult; results are sensitive to how we specify the cohort trends and to the inclusion of Chicago/Cook County in the sample. Taken together, the analyses suggest that while lower levels of school segregation are associated with higher rates of mixed-race childbearing, a substantial portion of that relationship is likely due to who chooses to live in places with desegregated schools. This suggests that researchers should be cautious about interpreting the relationship between segregation—whether residential or school—and other outcomes as causal.

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Introduction

Scholars have long viewed exogamy—marrying outside of one's group—as the ultimate marker of social assimilation (Gordon 1964). The extensive literature on interracial and interethnic marriage in the United States often cites residential segregation as a potential determinant of exogamy.[1] What causes this correlation between residential segregation and interracial marriage is unclear: it could be that residential segregation directly reduces interracial marriage, that the same factors that cause people to prefer a same-race partner also prompt them (or their parents) to choose a more segregated neighborhood, or a combination of the two.[2] Qian (1997) notes that that residential segregation is correlated with school segregation, and that school segregation could promote endogamy.

This paper is the first to empirically examine the relationship between school segregation and mixed-race childbearing. We estimate *causal* effects of school desegregation by exploiting quasi-random, policy-related variation. Court-ordered school desegregation, implemented in the 1960s, 70s and 80s, created a natural experiment: otherwise similar populations experienced differing levels of school segregation. This strategy has been applied to studies of how school segregation affects outcomes such as education, wages and employment, criminal activity, and teen childbearing.[3] We analyze the effects of desegregation on births with one white and one black parent, though we use the phrase "mixed-race" for expositional simplicity.

Our question bridges several literatures. Research on family formation and mixing of social groups has focused primarily on marriage—defining groups by nativity, religion, and,

[1] See Kalmijn 1998 for a review. Also see: Kalmijn 1993; Qian 1997; Fu 2001; Harris and Ono 2005; Qian and Lichter 2007; Lichter et al. 2007; Kalmijn 2010; Qian and Lichter 2011; Christensen 2011; Furtado and Theodoropoulos 2011; Furtado and Trejo 2013.
[2] Kalmijn (1998) provides a clear exposition of this issue as relates to the demography literature.
[3] Following Guryan (2004), Reber (2005), Weiner, Lutz, and Ludwig (2009), Baum-Snow and Lutz (2011), Johnson (2015), and Bifulco, Lopoo, and Oh (2015), we rely on variation in the timing of implementation of court-ordered school desegregation plans to estimate the causal effects of school desegregation.

most commonly in the contemporary literature cited above, race and ethnicity. Yet family formation in the United States is only partially defined by marriage.[4] This has prompted research on a broader range of outcomes, including interracial and interethnic cohabitation and dating.[5] Meanwhile, the literature on trends in mixed-race *births* is dominated by descriptive questions relating to health outcomes at birth, and the measurement and reporting of race.[6] Another literature studies how exposure to diverse peers—whether influenced by explicit school desegregation policies or more generally—affects intergroup relations. Though advocates and some researchers have characterized this literature as conclusively identifying positive effects of desegregation on intergroup relations (see, for example, Wells, Duran, and White 2008), these studies are unable to surmount the considerable empirical challenges they face, analyzing self-selected peer groups and/or small or unrepresentative samples. This paper, in contrast, uses a large and representative dataset and quasi-random variation in desegregation to estimate causal effects of school desegregation on a revealed preference measure of social interactions.[7]

While our measure of revealed preference—two parents of different races having a child together—is a good indicator of major changes in social norms and behavior, it will not capture subtler shifts. Desegregation could have improved intergroup relations on other important margins without affecting mixed-race births. We therefore emphasize the interpretation the

[4] Batson, Qian and Lichter (2006) find this to be particularly important for studying endogamy, with interracial couples constituting a higher share of all cohabiting couples than of all married couples.
[5] For a sampling of this literature, see Fujino 1997; Yancey 2002; Blackwell and Lichter 2004; Joyner and Kao 2005; Yancey 2007; Feliciano, Robnett and Komaie 2009; Herman and Campbell 2012; Qian, Glick and Batson 2012.
[6] Atkinson, MacDorman and Parker (2001) describe trends in mixed-race births in the US from 1971-1995.
[7] We recently became aware of a working paper by Shen (2016), which also examines the effects of court-ordered school desegregation on biracial births in addition to other birth outcomes. Our results are consistent with her findings, but we explore more specifications, which reveal the results are more sensitive than that paper suggests, so we characterize the findings differently.

analysis allows: the extent to which desegregation affected black-white *mixed-race birth rates*, rather than intergroup relations more generally.

Mixed-race birth rates, like rates of interracial marriage and cohabitation, have increased substantially since the Civil Rights Era. Throughout the paper, we focus on black-white pairings for two reasons: first, most of the desegregation plans we study were focused primarily on reducing black-white segregation, and second, these two groups were measured consistently over time in the natality data we analyze.[8] Figure 1 shows the percent of births to black parents, ages 18 to 35, where the other parent is white, separately for black mothers and fathers.[9] The dots indicate the comparable figures for marriages—that is, the percent of married black women (men), ages 18 to 35, who have a white husband (wife). Black father-white mother pairings are much more common than the reverse and increased dramatically, from 1.8 to 16.8 percent of births to black fathers between 1968 and 2003. White father-black mother births were nearly zero in 1968 and accounted for 2.5 percent of births to black mothers in 2003.[10] These patterns are broadly similar for births and marriages, though black father-white mother births increased faster than black husband-white wife marriages after 1990.

In this paper, we ask whether school desegregation contributed to these trends. These years were marked not only by major changes in the level of racial segregation in public schools but also by enormous shifts in social norms; while we view changes in *local* norms brought about by school desegregation as a key mechanism for any impact on mixed-race births, we do not want to conflate *secular* cohort trends common to all counties with the impact of school

[8] Due to data limitations, we treat blacks as a monolithic group despite the within-race diversity detailed by Batson, Qian and Lichter (2006).
[9] The trends in births to white mothers (and fathers) where the other parent is black are quite similar but the levels are about one-tenth of those shown in Figure 1, reflecting blacks' smaller share of the population.
[10] Father's race is not always reported on birth records. For this figure and the main analysis below, we include only observations where the father's race is reported.

desegregation. We find evidence of a strong positive *correlation* between school desegregation and mixed-race childbearing for both black men and women. However, including a small number of covariates—the racial composition of the school district, county fixed effects, and cohort fixed effects—substantially moderates this relationship. The estimated causal effects of desegregation plan implementation are moderate and not statistically significantly different from zero. We caution that the estimates are sensitive to specification, and are likely biased downwards due to measurement error, as discussed below. Taken together, the descriptive and causal analyses suggest that while lower levels of school segregation are associated with higher rates of mixed-race childbearing, a substantial portion of that relationship is likely due to who chooses to live in places with desegregated schools.

Background and Literature

Legal Background

We briefly summarize key legal and policy issues to make two points related to our approach to estimating causal effects: (1) the variation we use in the timing of court-ordered desegregation plans is likely uncorrelated with school-district-specific trends that could also affect interracial birth rates; and (2) all state-level laws forbidding interracial marriage were repealed prior to our period of study, so are not relevant omitted variables.

History and Timing of School Desegregation

Our identification strategy relies on variation in the timing of implementation of major court-ordered school desegregation plans. Despite the Supreme Court's declaration in *Brown* that separate schools were inherently unequal and that schools must desegregate "with all deliberate speed," the lower courts ultimately enforced the mandate of *Brown* district by district. Over time, the courts required districts to do more to desegregate, and districts outside the South were increasingly subject to court order.[11] It was not random *which* districts were forced to desegregate under court order but the timing of the legal process created randomness in *when they actually implemented a major desegregation plan*.[12] Our strategy for identifying causal effects requires the timing of court-ordered plan implementation to be uncorrelated with *trends* in other determinants of the outcome—in this case, mixed-race births. For more institutional detail on this period, we refer the reader to Reber (2005), which uses a similar empirical strategy and sample and finds little evidence of systematic trends in desegregation or white enrollment *before* plan implementation.

Laws on Interracial Marriage

While intimate interracial relationships and marriage were certainly stigmatized and discouraged during much of our period of study, interracial marriages were legal in all states by 1968, when we begin our study. In 1967, the U.S. Supreme Court ruled in *Loving v. Virginia* (388 U.S. 1) that laws forbidding interracial marriage were unconstitutional. The bulk of the states either never had such laws, or had repealed them before *Loving.*[13] The twelve states (which, aside from Oklahoma, were all Southern or Border regions) that still had such laws in 1967 repealed them as a direct effect of *Loving*. In some specifications, we control for trends in interracial births separately by region (South versus non-South), which will account for any trends in interracial births common to the Southern states, including any lagged effects of the repeal of these laws. These controls do not affect the results.

[11] See Welch and Light (1987), Guryan (2004), and Reber (2005) for a discussion of the legal history of desegregation and the timing of plan implementation. See Guryan (2004) for a discussion and model of the decision of where to bring desegregation cases first.

[12] Not all districts required court supervision to desegregate—only about half of districts in the former Confederacy were ever supervised by a court by 1976. Some districts desegregated voluntarily or in response to the threat of withdrawal of federal funds. Court-ordered school desegregation plans were particularly important for larger districts, districts with high black enrollment shares, and districts with stronger historical preferences for segregation (Cascio et al., 2008).

[13] We refer the reader to Fryer (2007) for the history of state-level anti-miscegenation laws in the U.S.

Theory and Potential Mechanisms

The school desegregation we study increased exposure of black to white students and of white to black students in schools (Reber, 2005; we replicate this finding below in Figure 3). The increase in interracial contact resulting from desegregation plans was at least partially mitigated by within-school segregation of classrooms and segregation of extracurricular activities and friendship circles.[14] It also was often accompanied by heightened tensions surrounding race relations. The interracial contact induced by school desegregation—whatever its intensity—might have affected mixed-race births through a number of channels.

While our empirical strategy will not allow us to distinguish among them, we briefly discuss some of these potential channels, which are not mutually exclusive. First, desegregation might increase mixed-race births by providing students with more opportunities to meet people of other races. In this case, there could be no change in racial attitudes or receptiveness to other-race partners, simply more structurally-induced interactions (see, e.g., Harris and Ono 2005).[15] Second, the mechanism which has attracted the most interest in relation to school desegregation and affirmative action policies, is that the increase in exposure generated through legally-induced structural change might change culture and attitudes towards those of other races. The production of a consensual mixed-race birth requires potential parents of both races to be receptive to an other-race partner, so changes in mixed-race births could be driven by changes in blacks' attitudes towards whites, whites' attitudes towards blacks or a combination of the two.[16] This hypothesis dates back to Allport's (1954) contact theory, which posits that the timing, duration,

[14] See Clotfelter (2004) and Echenique and Fryer (2007).
[15] Kalmijn and Van Tubergen (2010) find that culture and preferences are more important than structural factors (such as exposure *per se*) in determining intermarriage rates across national origin groups in the US.
[16] For the mixed-race birth do be observed in the natality data, the mother also report information about the father. We discuss the implications of missing data for fathers for the analysis below.

and nature of the contact is likely important in determining whether increased contact affects attitudes, and in which direction.[17]

Yet a third possible mechanism is suggested by status exchange models (see Kalmijn 2010), which posit that white race carries greater social status than black, and that a marital partner with greater status along one dimension is willing to enter the union in exchange for an increase in social status on another dimension. This model is relevant here as school desegregation has been shown to affect socioeconomic outcomes for black students. For example, school desegregation appears to have improved educational attainment for blacks (Guryan 2004; Reber 2010; Johnson 2015) so could have increased mixed-race pairings by narrowing the status gap between blacks and whites.

Empirical Literature on Social Effects of Exposure to Diversity

The most closely related existing studies of the social effects of exposure to diversity fall into two categories: observational studies of the effects of school desegregation, and quasi-experimental studies of exposure to other-race college roommates or assigned peer group members.[18]

Literature on Social Effects of Desegregated Schooling

Wells et al. (2009) ask a similar question to ours using different methods. They interview about 100 adults who graduated from a diverse set of recently desegregated high schools in 1980. Twenty to 25 years after high school graduation, subjects of both races report positive memories of attending racially mixed schools and frequently state that those experiences positively affected their attitudes about other races. The authors argue that desegregation had important effects on

[17] Similarly, if court-ordered desegregation plans increase contact but also sufficiently increase racial tensions, intergroup relations could deteriorate in response.
[18] See Pettigrew and Tropp (2006) for a review and meta-analysis of the experimental and observational literature on impact of intergroup contact.

racial tolerance. In most cases, however, the adults appear to have gone on to live more segregated existences than those they experienced as students.[19] This highlights the importance of differentiating between one's *attitude* towards members of other races and one's *relations* with members of other races. This could reflect "social desirability" bias if respondents felt social pressure to report positive or tolerant attitudes towards other races, or could be an unbiased reflection of the fact that racial attitudes and intergroup social interactions are not the same. As Herman and Campbell (2012) find, respondents may have positive and tolerant attitudes towards members of other races without having strong social connections to them.

Quasi-Experimental Studies of Exposure to Diverse Peers in College

A wave of studies take advantage of the random assignment of roommates in college to identify causal peer effects, including the effects of exposure to different-race peers on both racial attitudes and interactions. The results consistently find positive effects of having an other-race roommate on reported attitudes towards members of other racial and ethnic groups, but effects on *social interactions*, such as mixed-race friendships, are mixed.[20] The characteristics of the other group matter in determining the impact of exposure: Carrell, Hoekstra and West (2015) find when whites are randomly exposed to higher aptitude blacks in their first-year squadrons at the US Air Force Academy, white-black rooming choices are more common the next year. While these results may be of limited relevance in non-college settings—due to both the select sample of students attending the colleges studied and the unusual ability of college administrators to place students into peer groups with potentially intensive social interactions—they do point to the potential for exposure to diverse peers to improve intergroup relations.

[19] The subjects generally describe interracial dating in these recently integrated public high schools as present, but infrequent and clandestine.

[20] See Boisjoly et al. (2006), Marmaros and Sacerdote (2006), and Camargo, Stinebrickner and Stinebrickner (2010).

Data

Data on School Desegregation Plans and Levels of Segregation

We use data on the timing of implementation of major court-ordered desegregation plans and measures of segregation from Welch and Light (1987) and the Common Core of Data from the National Center for Education Statistics. Welch and Light (WL) identified major school desegregation plans for 108 of the 125 large school districts they sampled.[21] Of these districts, seven never implemented their court-ordered desegregation plans: because these districts likely differed on unobserved characteristics, and consistent with the literature, we exclude these seven never-treated districts from the analysis. Thus, we identify the effects of desegregation plans using only variation in the *timing* of implementation among districts that ever implemented a court ordered plan. Figure 2 shows the considerable variation in this timing.

Panel A of Table 1 reports summary statistics on three standard measures of segregation for the WL districts.[22] All three measures show considerable reductions in segregation between 1968 and 1984;[23] for example, the average exposure of blacks to whites was 0.26 in 1968, indicating that the average black student in WL districts attended a school that was 26 percent white; by 1984, the exposure index had risen to 0.41. Prior research shows that these

[21] Welch and Light (1987) took a stratified random sample of large districts with significant minority and non-minority populations. See Welch and Light (1987) for more detail. Implementation of a "major desegregation plan," as defined by Welch and Light, caused large reductions in school segregation on average (Guryan 2004 and Reber 2005; replicated in Figure 3). Despite its non-universal coverage, the WL major plans have been used in most of the literature that exploits the variation in timing of desegregation plans on outcomes because it is the best-validated source with data on the timing of plan *implementation*. The American Communities Project at Brown University collected data on desegregation court cases in a larger number of school districts. Because it was designed for examining correlates of longer-term trends in segregation over decades (Logan, Oakley, and Stowell 2008), it has less detail on when desegregation plans were actually implemented and their content.

[22] Black exposure to whites can be interpreted as the white share of enrollment in the average black's school and vice-versa for white exposure to blacks. The dissimilarity index can be interpreted as the share of blacks (or whites) that would have to be reassigned to another school so that every school in the district has the same racial composition. See Reber (2005) and references therein for more detail.

[23] We report segregation for these years because they span most of the desegregation activity and have relatively little missing data.

desegregation plans reduced segregation substantially (Guryan 2004, Reber 2005). Following Reber (2005), we estimate regressions of three measures of segregation on a series of indicators for the number of years relative to plan implementation (where 0 is the last year prior to implementation and the excluded category) and year fixed effects. The coefficients on the time-relative-to-implementation indicators, with 95 percent confidence intervals, are plotted in Figure 3. Implementation of a desegregation plan was associated with sharp reductions in segregation according to all three measures: black exposure to whites, white exposure to blacks, and the dissimilarity index. Panel A, for example, indicates that, controlling for general trends over time (year fixed effects), plan implementation increases black exposure to whites by almost 15 percentage points in the first two years, and those gains were largely sustained for the following decade.[24]

The births data we use to construct the dependent variable are reported at the county rather than the school district level, so we assign plan implementation years to counties based on the implementation year of the WL district they contain and conduct the analysis at the county level.[25] We refer to these as the "WL counties." The WL districts typically account for a large share of their counties' enrollment: for 40 (of 100) counties, the WL district and the county are

[24] See Reber (2005) for more details on these analyses.

[25] We exclude three Virginia counties (Norfolk, Roanoke, and Pittsylvania) that include WL districts because their geography is not consistently coded over time, and Richland County, South Carolina because comparison of counts of births in the natality file to the population of the county and counts of births in the 1960 County Data book suggest it is miscoded in the natality data. Two counties contain more than one treated district; we code the desegregation years for these counties as follows: Los Angeles County contains three treated districts (Los Angeles Unified School District (1978), Long Beach Unified School District (1980), and Pasadena Unified School District (1970); because Los Angeles Unified is by far the largest district in the county, we assign 1978 as the treatment year for Los Angeles County. Jefferson County, Alabama contains two treated districts (Birmingham City School District (1970) and Jefferson County School District (1971)); both have considerable enrollment, so we assign Jefferson County the earlier treatment year (1970).

coterminous; for half of the remaining districts, the WL district accounts for more than 90 percent of black enrollment in the county in 1970.[26]

Data on Mixed-Race Births

To construct our dependent variable, we use National Center for Health Statistics data on the near-universe[27] of births in the United States from 1968 to 2003 (these are the same data represented graphically in Figure 1). In principle, we are interested in all kinds of mixed-race partnerships, including dating, marriage, and cohabitation in addition to mixed-race childbearing. However, data on those other partnerships are not available at fine enough geography for this analysis, so we focus our attention on mixed-race births. We focus on two samples: births to black mothers and births to black fathers. [28]

Panel B of Table 1 reports summary statistics for 1968 and 2003, separately for the WL counties and the rest of the United States. In 1968, 0.4 percent of births to black mothers in the WL counties report a white father, compared to 0.3 percent in the rest of the United States. Two percent of births to black fathers in the WL counties and 1.6 percent in the rest of the United States had a white mother in 1968. Consistent with the literature on endogamy, rates of mixed-race births increased dramatically over this period, and black male-white female pairings are much more common than black female-white male pairings. These trends are evident in both WL counties and the rest of the US.

Table 1 also shows the high and rising prevalence of missing paternal information. For black (white) mothers in the WL counties, the percent of births with missing data on father's race

[26] We use the 1970 School District Data Book (SDDB) to calculate this; this calculation excludes Rochester and Buffalo due to difficulties processing the raw files for New York State.
[27] In some early years and states, the available births data are a 50 percent sample of all births. We use the appropriate weights to account for this sampling, and in all cases, the samples are quite large.
[28] Neither parental education nor Hispanic ethnicity are consistently reported for all states over the relevant time period so we do not use these in our analyses.

increased from 20.6 (3.8) to 37.3 (11.1) between 1968 and 2003; these trends were roughly similar in the rest of the U.S. Missing data on fathers may introduce measurement error and/or bias in our estimates of the causal effects of desegregation plans on mixed-race births. Indeed, this is a potentially important limitation of the study, and we discuss it in detail after presenting the main estimates of the effects of desegregation on mixed-race births. Finally, although there are only 100 WL counties, Table 1 shows that they account for nearly half of births to black mothers and close to one-third of those to white mothers.

County Characteristics

Panel C of Table 1 shows how the WL counties compare to the rest of the country on key socio-demographic characteristics (measured prior to desegregation plan implementation). The WL counties have larger populations and higher population density, higher black shares in the population, and are slightly more likely to be located in the South, compared to the rest of the United States. While the analysis may not apply to the typical school district or county in the United States, the sampled counties do account for a large share of both school enrollment and births, particularly for blacks. Most relevant, the sampled counties are the only ones that for which we have reliable data on plan implementation dates necessary for the quasi-experimental analysis.

Unit of Analysis

The desegregation plans we study treated *cohorts* of (prospective) parents at different times in different school districts, which we match to their *counties* (the smallest geographic unit at which births are recorded). We therefore conduct the analysis at the county-cohort level, separately for black mothers and fathers. We assign each mother (father) to her *cohort* (year of expected high school graduation) based on the year and month of the child's birth and the age of the mother (father) and to a *county* based on the county where the mother resides at the time of the child's birth. To assign cohort, we assume "on-time" high school graduation at age 18, with no retention in grade.[29] Because court-ordered desegregation, the treatment of interest, varies only at the cohort-county level in our data, we collapse the micro data to the cohort-county level separately for black mothers and fathers. To assign parents to school desegregation plan implementation, we assume that the mothers' county of residence at the time of birth is the same county in which the parents attended school from kindergarten through high school; we discuss limitations of this assumption following the results. In the analysis of births to black mothers, for example, an example observation would be black mothers who turned 18 in 1975 and gave birth in El Paso County, TX; this observation would include births taking place over a range of years. The outcome variable is the percent of those births where the father was white. In most specifications we weight by the number of births. The point estimates are thus equivalent to estimating the regressions on the micro data, but the standard errors are likely more accurate (Bertrand, Duflo, and Mullainathan 2004).

Figure 4 shows age-adjusted trends in mixed-race births across parent's cohort (labeled by the year of expected high school graduation), rather than calendar year (as in Figure 1), separately by parent and region for the WL counties we analyze. Again, we see that black-white mixed-race childbearing is consistently higher for black fathers, compared to black mothers, and increased across cohorts. Not surprisingly, Southern children of black mothers and fathers are less likely to have another parent who is white. Although the cohort trend is similar in both

[29] While this will not hold for all cases, we do not observe information on year of high school graduation in the birth records. We ultimately examine effects of the timing of desegregation over multi-year ranges, reducing the measurement error we expect to be associated with this assumption.

regions (the lines are roughly parallel), the levels are quite different, so we include South-by-cohort controls in some specifications.

School Segregation and Mixed-Race Births: Descriptive Evidence

Although school segregation has been put forth as a possible explanation for low rates of exdogamy (and presumably, mixed-race childbearing), little research examines the relationship between these two variables. We begin in Table 2 by reporting the results of simple regressions relating school segregation and subsequent mixed-race childbearing for the WL sample then turn to the causal analysis. By design, these first specifications do not utilize the quasi-random variation in timing of desegregation plans, so should not be interpreted as causal effects.

We estimate regressions using data aggregated to the county-cohort level, separately for black mothers and fathers, weighted by births. The key independent variable of interest is a measure of the level of school segregation. We assign each county-cohort the black to white index of exposure for the county's WL district during the cohort's senior year ($SchoolSeg_{ct}$), ranging from 0 to 1.[30] We begin with a simple regression to see the unconditional correlation between school segregation and mixed-race childbearing. We then add a series of control variables; the specification with the most controls is:[31]

$$(1)\quad PerMixed_{ct} = \beta_0 + \beta_1 SchoolSeg_{ct} + \beta_2 BlackShare_{ct} + \lambda_c + \gamma_t + e_{ct},$$

where $PerMixed_{ct}$ is the percent of births in county c and cohort t that are mixed-race, ranging from 0 to 100. $BlackShare_{ct}$ is the black share of public school enrollment in the relevant WL

[30] Although the level of segregation experienced throughout an individual's schooling may influence subsequent mixed-race partnering, we assign the segregation index from the cohort's senior year because the segregation data start in 1968, so we do not have information about the levels of segregation early in the careers of the early cohorts. We could impute or estimate these values, but we prefer the transparency and consistency of the senior-year measure. Following the endogamy literature, we use an exposure/isolation measure, rather than racial balance index such as the dissimilarity index; the results are broadly similar if we use the dissimilarity index.
[31] The literature on endogamy suggests that residential segregation is a strong correlate of endogamy; unfortunately, we do not have good measures of residential segregation for this sample in 1960 to include in this analysis.

district during the cohort's senior year. Note that racial composition mechanically affects exposure (and isolation, as used elsewhere in the literature) indices; for example, black students in districts with low white enrollment shares cannot experience high exposure to whites regardless of how evenly the races are spread across schools within the district. In some specifications, we include county fixed effects (λ_c), and cohort fixed effects (γ_t).[32]

Results

The results for black mothers and fathers are presented in Table 2. The findings are generally consistent with empirical observations in the endogamy literature.[33] Column 1 shows the results for a regression of mixed-race childbearing on senior-year school segregation without any controls; column 7 shows the results with all the controls included as in Equation (1). The mean of the dependent variable for the entire sample, 1.9 for mothers and 5.7 for fathers, is reported in the first row of each panel. The simple correlation between exposure of blacks to whites during the senior year of high school and the probability of later having a mixed-race child is positive and statistically significant at the one-percent level (column 1). The coefficient of 3.427 indicates that a ten percentage point increase in a black mother's exposure to whites in school during her senior year is associated with a 0.34 percentage point increase in the probability of her having a mixed-race child. For a black father, a ten percentage point increase in black exposure to whites during the senior year is associated with a 1.6 percentage point increase in the probability that his child's mother is white. In subsequent columns, we explore

[32] For consistency with the quasi-experimental analysis, the sample restrictions are the same as for Table 3 and described in the next section.

[33] This exercise is methodologically most similar to the work of Lichter et al. (2007) on Hispanic-non-Hispanic exogamy. Christensen (2011) reveals a negative correlation between MSA-level black-white residential segregation and black-white intermarriage.

how this relationship is affected by the inclusion of control variables, individually and in combination.

Consistent with literature on the importance of group size in determining social interactions (Blau 1977), we control for the black share of the relevant population, which we measure as the black share of enrollment in the county's WL school district during the senior year. The endogamy literature finds that black out-marriage rates are higher when the black share of the population is lower (Kalmijn 1993). Column 2 shows that a similar relationship holds for mixed-race births: the black share of enrollment in high school is negatively correlated with mixed-race childbearing.[34] Black share is also an important predictor of school segregation levels (Cascio et al. 2008). In column 3, we include both black share and school desegregation measures; the effect of racial composition remains strong, while the coefficient on school segregation is eliminated for mothers and reduced by half for fathers.

The remaining relationship between school segregation and mixed-race childbearing may also be due to unobserved differences in other determinants of mixed-race childbearing that are correlated with segregation. For example, families who have more favorable attitudes towards other races may systematically locate in counties with less segregated schools. Similarly, Table 1 shows that school segregation declined over time, so later cohorts attended less segregated schools and may also have been more likely to have an other-race partner for other reasons. We therefore introduce county and cohort fixed effects to the regression, separately (columns 4 and 5) and together (column 6). Each set of fixed effects reduces the coefficient on school segregation, and when both are included, the coefficient on school segregation is negative

[34] Kalmijn (1993) finds a nonlinear relationship between the black share of the population and rates of exogamy. If we include the black share of enrollment squared in the regressions in Table 2, the coefficient is statistically significant, but the overall conclusion is unchanged.

(though small and statistically insignificant) for mothers and is substantially reduced compared to column 1 for fathers. Finally, we include all the controls in column 7; the coefficient on school segregation becomes more negative (but still statistically indistinguishable from 0) for mothers and closer to 0 for fathers.

Overall, while we see a strong positive unconditional correlation between mixed-race childbearing and segregation in schools, as measured by black exposure to whites in column 1, this relationship is unlikely to be wholly causal. The fact that simply controlling for county and cohort fixed effects substantially moderates the apparent relationship between segregation and mixed-race childbearing strongly suggests that there are omitted determinants of mixed-race childbearing correlated with school segregation, such as changes in attitudes across cohorts and sorting of more tolerant families into counties with less segregated schools. The same likely holds for the correlation between residential segregation and endogamy, but an in-depth analysis of this issue is beyond the scope of this paper.

Causal Effects of Court-Ordered Desegregation Plans on Mixed-Race Births

Empirical Strategy

To estimate causal effects of school desegregation on mixed-race childbearing, we exploit quasi-random variation in the timing of the implementation of major court-ordered desegregation plans for a sample of large school districts that implemented a plan sometime between 1961 and 1986—the Welch and Light sample described above. This sample of school districts and empirical approach have been used to estimate the effects of school desegregation on levels of segregation and white flight (Reber 2005), crime and victimization (Weiner, Lutz, and Ludwig 2009), residential and schooling choices (Baum-Snow and Lutz 2011), and teen childbearing (Bifulco, Lopoo, and Oh 2015).

Because the "treatment" of interest—exposure to a desegregation plan—varies at the county-cohort level, we aggregate the data to the county-cohort level, as described above. Consistent with the literature, we estimate regressions of the following form, separately for black mothers and fathers:

$$(2)\ PerMixed_{ct} = \beta_0 + \beta_1 Exposed1\_4_{ct} + \beta_2 Exposed5\_8_{ct} + \beta_3 Exposed9\_12_{ct} + \beta_4 ExposedFully_{ct} + \lambda_c + \gamma_t + e_{ct}$$

where $PerMixed_{ct}$ is defined as in the previous section, the percent of births in county *c* and cohort *t* that are mixed-race, ranging from 0 to 100.

The key independent variables of interest—$Exposed1\_4_{ct}$, $Exposed5\_8_{ct}$, $Exposed9\_12_{ct}$, $ExposedFully_{ct}$ —indicate for *how many years* the parent (the mother in the mothers' samples and the father in the fathers' samples) in cohort *t* schooled in county *c* were exposed to a court-ordered desegregation plan. $Exposed1\_4_{ct}$ is equal to 1 if individuals in that county-cohort were exposed to a desegregation plan for one to four years; that is, the plan was implemented when they were in ninth grade (four years of exposure) through twelfth grade (one year of exposure). $Exposed5\_8_{ct}$ is equal to 1 for county cohorts with 5 to 8 years of exposure to a desegregation plan (plan implemented in grades 5 through 8); $Exposed9\_12_{ct}$ is equal to 1 for county-cohorts with 9 to 12 years of exposure (implemented in grades 1 through 4); finally, $ExposedFully_{ct}$ is equal to 1 if the plan was implemented when the county-cohort was in kindergarten or earlier and was exposed to desegregation throughout their schooling. At most, one of these indicator variables is equal to 1, and the omitted category is "never exposed to a desegregation plan"; all of the indicators will be 0 for cohorts that had completed high school before a desegregation plan was implemented in their county.[35]

[35] We do not have adequate power to break these into more detailed years of exposure treatment indicators.

County fixed effects (λ_c) control for determinants of mixed-race childbearing that are constant within counties across cohorts, and cohort fixed effects (γ_t) control for changes in the determinants of mixed-race childbearing across cohorts that are common across counties. There are strong underlying trends in mixed-race births across cohorts (Figure 4), which vary depending on county characteristics and by region. We therefore consider a range of alternative specifications of the cohort effects, as discussed further below.[36] We do not control for *contemporaneous* racial composition of the county since it could be changing in direct response to desegregation plan implementation; the county fixed effects will account for persistent differences in racial composition across counties.[37]

The β's in Equation (2) are the key parameters of interest, tracing out the dynamic treatment effect of exposure to a desegregation plan. They indicate the extent to which cohorts of mothers (fathers) who were exposed to desegregation plans for a given amount of time produced mixed-race children at a different rate than mothers (fathers) in the same county but different cohorts, who were not exposed to desegregation, or compared to parents in the same cohort, but educated in different counties that desegregated at different times. This strategy may underestimate the effects of desegregation plan on mixed-race births for a number of reasons. First, there will be measurement error in the treatment indicators for years of exposure to a desegregation plan. We impute years of exposure assuming that the mother's county of residence at the time of the child's birth is the county where the parents completed all of their schooling

[36] In theory, we could also control for potential determinants of mixed-race childbearing at the individual level by estimating equation (2) with micro data. For example, age and education are both predictors of exogamy and mixed-race childbearing. However, if school desegregation affects these outcomes, they are endogenous and including them would constitute over-controlling. In practice, education is not consistently reported in the natality data for the cohorts we study. In results not reported, we have controlled for parental age (even though it is potentially endogenous), and the results are unaffected.

[37] We do not use the log-linear specification common in the demography literature to decompose trends in endogamy because we seek to isolate the effect of a specific policy—court-ordered school desegregation—that independently affected population characteristics.

through age 18. This imputation will not always be accurate due to cross-county mobility of parents and the fact that not all parents finish their schooling at age 18. Second, there could be spillovers across cohorts if potential partners in nearby cohorts are treated; that is, an "untreated" father might still be affected if, for example, younger potential other-race partners are more receptive to an other-race partner because of their own exposure to desegregated schools.[38]

We include multiple treatment indicators to trace out the dynamic effects of plan implementation. We might, for example, expect a larger effect for cohorts exposed to desegregation at younger ages (and therefore, longer), in which case $\beta_4 > \beta_3 > \beta_2 > \beta_1$. We cannot, however, distinguish among three, non-mutually-exclusive possible explanations for any such effect: (1) exposure to desegregation at earlier ages might matter more, as emphasized in perpetuation theory (see Wells and Crain 1994 for a review of the literature), (2) there could be a dose-response relationship such that longer exposure to desegregation affects outcomes more, regardless of the age at which it happens, or (3) the nature of the desegregation plan itself could change the longer the plan is in place (although Figure 3 shows that the reduction in segregation was fairly immediate and sustained, on average).[39]

Sample Restrictions

Race

[38] The likelihood that a black mother, for example, has a child with a white father depends on the attitudes and availability of potential white partners and may therefore depend on *potential partners'* exposure to desegregation plans. We do not have the statistical power to explicitly consider or control for the desegregation treatment status of potential partners, though it is likely highly correlated with own treatment status since potential partners are typically similarly aged. That is, in the mother sample, we consider exposure based on the mother's birth year, and similarly for the father sample. In some sense, individuals are partially treated if potential partners are treated, so this may bias the results towards zero by creating measurement error in the assignment of treatment status.

[39] Reber (2005) and our Figure 3 show that desegregation plans reduced within-district segregation quickly, phasing in over only a couple of years. White flight played out over time, however, and did offset some of the initial increases in black exposure to whites. This implies that while blacks attending high school in a newly desegregated school had slightly more exposure to whites compared to later cohorts in the same district, the whites who remained might have been more (or less) tolerant of blacks. In any case, we cannot separate the effects of age of exposure, length of exposure, and phase-in of the plan itself.

We focus on mixed-race births to black mothers (e.g., the probability that a black mother has a child with a white father) and fathers. We do not conduct the analogous exercises for samples of white mothers and fathers, because white flight likely changed the composition of the sample of whites in desegregating counties over time, making it difficult to separate treatment effects of desegregation from compositional changes.

We exclude births for which the race of either parent is unknown—in practice, this affects only the fathers. The prevalence of missing paternal data is high and increasing over time (Table 1). Births with missing data on father's race are necessarily excluded from the analysis of fathers; we also exclude these births when analyzing the sample of mothers. We discuss this limitation of the analysis in more detail below.

Cohorts

For ease of comparison to plan implementation dates, we define cohorts by the year in which they are expected to graduate from high school, assuming on-time entry and progression. We restrict our analysis to the 1965-1985 cohorts. These cohorts span the years during which most desegregation plans were implemented, and we can capture most of their lifetime fertility in the available natality data, which start in 1968. We observe fertility at least through age 35 for all cohorts but miss early fertility for the early cohorts. For example, the 1965 cohort was 20 in 1968, the first year of our data; thus, the parental age composition of observed births is skewed slightly older than for later cohorts. Nevertheless, we include these early cohorts so that we have at least a few never-treated cohorts for almost all counties (most plans were implemented after 1967). The cohort fixed effects control for differences in age composition that are common to cohorts across counties.

Age

We restrict the sample to parents who are between 18 and 35 when their child was born—an age range that captures 82 to 87 percent of lifetime fertility.[40] We choose age 18 rather than a younger minimum age so that we are observing parents who have had the potential to be treated by at least a year of school desegregation. On the one hand, younger parents are more likely living in the same county where they went to high school. On the other hand, desegregation could have changed the age profile of fertility (via changes in educational attainment, for example), so we wish to capture as much of lifetime fertility as possible. We can observe births through age 35 for all cohorts. We also check the sensitivity of the results to alternative age ranges (Table 4).

Results

Table 3 presents the results from the estimation of alternative specifications of equation (2), ranging from the most parsimonious to our preferred, most flexible treatment of cohort control variables, adding in controls one at a time for the reader's benefit. The top panel shows the effect of exposure to a desegregation plan for the sample of black mothers on the likelihood that her child has a white father. Analogous results for black fathers are shown in Panel B.

Column 1 of Table 3 begins by estimating a version of equation (2) controlling only for county fixed effects and weighting each county-cohort observation by the number of births in the county-cohort cell. Given the strong upward trend in mixed-race births across parental birth cohorts during this period (Figure 4), it is unsurprising that this specification with no cohort controls yields positive and statistically significant effects of exposure to a desegregation plan on

[40] For the 1971, 1972, and 1973 cohorts, we could observe all births between ages 15 and 48; this should capture the overwhelming majority of lifetime fertility, especially for women. For these cohorts, 82, 83, 87, and 82 percent of all births were to parents ages 18 to 35 for black mothers, black fathers, white mothers, and white fathers, respectively.

the percent of births that are mixed-race—both for black mothers (Panel A) and black fathers (Panel B). In column 2, we introduce cohort fixed effects to avoid confounding the effects of exposure to desegregation plans with national cohort trends, yielding much smaller and in most cases less precise and only marginally statistically significant estimates. For ease of exposition we focus on Panel B going forward in our discussion, examining black father-white mother pairings, which are more common than white father-black mother pairings.

Before proceeding with specifications that allow cohort effects to vary by county characteristics, we first check for potential outlier counties. Column 3 presents results for an *unweighted* version of the same specification in column 2 (column 2 weights by the number of births in the county-cohort). This approach estimates how much the typical desegregation *plan*—regardless of the size of the county in which it was implemented—affected mixed-race birth rates in its county. The unweighted estimates are much less precise, but the coefficients on the treatment variables are opposite in sign, suggesting that one or more large counties may be driving the positive finding in column 2.

To further explore this possibility, we checked the influence of the five counties with the most births by estimating results dropping each, one at a time, from the sample (results in Appendix Table 1), finding that Cook County (Chicago) is particularly influential.[41] In column 4, we exclude Chicago; the precision of estimates is essentially unaffected (compared to column 2, the analogous specification including Chicago births) but the coefficients on the treatment variables become small and statistically insignificant. It is possible that the treatment effect of desegregation was particularly strong in Chicago, but results from column 5, which keeps

[41] The top five counties by sample size are Chicago/Cook County, Los Angeles County, Detroit/Wayne County, Philadelphia County, and Houston/Harris County. Appendix Table 1 repeats the specification reported in Table 3, column 2 and then reports estimates of the same specification, excluding one county at a time.

Chicago births in the sample but allows for a Chicago-specific linear cohort trend, suggest that Chicago experienced a different underlying cohort trend not captured by the cohort fixed effects. As in column 4, the coefficients on the treatment indicators are small and statistically insignificant. In other words, the positive estimates for black fathers in column 2 are sensitive to allowing a differential cohort trend for Chicago.[42]

Aside from Chicago's unique influence, cohort trends in mixed-race births may have differed across counties with different characteristics. For example, the black share of the population and whether a county is in the South are both significant predictors of county-level changes in mixed-race births across cohorts (results not shown).[43] We allow cohort trends to vary by Southern region (column 6), by 1960 county black share (column 7), and both at the same time (column 8). We include these interactions of county characteristics with cohort fixed effects in case these cohort trends are correlated with desegregation plan implementation and to improve power (by explaining some of the residual variation in the outcome).

These specifications (columns 6, 7 and 8) include Chicago observations and exclude the Chicago-specific linear cohort trend. Allowing either South-by-cohort (column 6) or 1960 black share-by-cohort (column 7) specific linear cohort trends yields results somewhat larger than the specification with only cohort and county fixed effects in column 2. However, when we include

[42] Chicago has a large influence on the results because (a) it contributes many observations to the analysis, (b) it had an unusually flat trend in mixed race birth through the early cohorts, and (c) it had a relatively late plan implementation date (1982). Intuitively, this means that Chicago mostly acts as a control, "pulling down" the estimated trend in cohort effects and making the trends (and treatment effects) in other districts look more positive. While we prefer not to drop observations on an *ad hoc* basis, we would also not want to draw too strong a conclusion from analysis that is so sensitive to a single county. We therefore present a range of specifications demonstrating this sensitivity.

[43] To understand how cohort trends varied with county characteristics, we estimated county-level regressions of the "long change" in *PerMixed* between the early and late cohorts on the county characteristics reported in Table 1 (log population, population density, percent black in the population, Southern region, and percent of the vote cast for Lyndon Johnson in 1964). Percent black in the population was by far the most important (negative) predictor of changes; trends also appear to vary by region. We therefore allow cohort trends to vary with black share of the population and Southern region. We also experimented with including interactions of the cohort effects with the other county-level characteristics, and they did not affect the results.

both sets of interactions, South-by-cohort and black-share-by-cohort, the coefficients are smaller and insignificant; the standard errors are smaller than those in column 2, so the loss of significance is not due to lost precision.

Comparing column 8 to column 5 reveals that the Chicago-specific linear trend in column 5's specification was not simply driven by its location outside the South and its high black share. In column 9, we add county-specific linear cohort trends for all counties. Including these controls may more fully control for differences in trends in other determinants of mixed-race childbearing across counties. However, this may constitute "over-controlling" since county-specific trends are, by definition, correlated with the desegregation treatments, which "turn on" at some point and then increase smoothly for subsequent cohorts. If implementation of a desegregation plan generates gradual changes in mixed-race childbearing across cohorts, some of that effect may "load onto" the county specific trends. The estimates are small, negative, and statistically insignificant (despite having smaller standard errors compared to column 2). Column 10 shows the results are similar when *only* the Chicago-specific trend is included, suggesting that the Chicago-specific trend is doing most of the "work" of the county-specific trends in column 9. That is, while in theory including county-specific cohort trends may "over-control," in practice, these trends are mainly accounting for the differential cohort trend in Chicago.

Although results are sensitive across specifications, all of the specifications that account for the differential trend in Chicago (columns 4, 5, 9 and 10) find small and statistically insignificant effects of school desegregation plan on mixed-race childbearing.[44] Because Chicago

[44] In results not shown, we explored heterogeneity of treatment effects along several key county characteristics including the share of the county that was black in 1960, the black/white median family income gap in 1970, region, and county population. Unfortunately, we lack the statistical power for these analyses to be informative, so we do not report the results.

experienced such different trends and has such influence on the estimates, we prefer those specifications that account for Chicago's influence in some way.

To give the reader a sense of the precision of the estimates, we discuss the magnitude and confidence intervals for the estimates in column 10. The treatment indicators are mutually exclusive, not additive, so the coefficient of 0.234 for the "fully exposed" indicator means that the child of a black father exposed to a desegregation plan during his entire school career is 0.2 percentage points more likely to have a white mother, compared to children of black fathers who were never exposed. In the WL sample, the probability that a child born to a black father had a white mother increased 6.7 percentage points between the 1968 and 1985 cohorts, and the probability that a child born to a black mother had a white father increased 1.3 percentage points. The 95% confidence intervals suggest that full exposure (from kindergarten through high school graduation) to a desegregation plan changed the probability that a black father had a mixed-race child by between -1.6 and 2.1 percentage points, and the probability that a black mother had a mixed-race child by between -0.6 and 0.9 percentage points. These confidence intervals include small negative and small positive impacts of school desegregation; at the upper bound of the confidence interval, the estimates suggest that school desegregation explained about a quarter of the trend for black fathers and about 15 percent of the trend for black mothers. However, we expect these estimates are biased down due to measurement error, as discussed further below.

Limitations

The analysis above faces two important potential limitations. First, mobility of parents means that treatment indicators are measured with error. Second, some of the birth records are missing data on the father's race. We discuss the implications of each of these and present sensitivity analyses in turn.

Mobility

We assigned treatment status to births based on the child's county at birth, assuming the parent attended school in this county. Thus, treatment status is measured with error due to parental mobility prior to the child's birth. If mobility is unrelated to exposure to the treatment, this will create classical measurement error in the treatment variables and the estimate will be biased towards zero. Further, research shows positive relationships between educational attainment and exogamy (e.g., Qian 1997), between educational attainment and geographic mobility (e.g., Molloy, Smith, and Wozniak 2011), and between mobility and exogamy (e.g., Rosenfeld and Kim 2005). To the extent that school desegregation increases mobility of those parents for whom it induces mixed-race births, the analysis may "miss" some of the desegregation-induced mixed-race births if the parents no longer reside in the county where they were schooled, leading to downward biased estimates of the effects of school desegregation.

If desegregation affects mobility differentially for people with different propensities to form a mixed-race partnership, we could mistake compositional changes in the population for treatment effects of desegregation plans. This is why we do not study the samples of white parents: if desegregation plans cause the least tolerant whites to leave the county, we might observe increases in the share of births to white mothers with black fathers, even if no *individual's* propensity to have an other-race partner is changed.

We address concerns about mobility in two sets of sensitivity analyses (Table 4). First, we restrict the sample to younger mothers (fathers); mobility should be less important the closer the parent is to school age. Second, we restrict the sample of mothers to those born in the same state as they are giving birth, though of course they could have changed counties.[45] In both cases,

[45] We do not repeat this exercise for fathers because their state of birth is not reported in the natality data.

the results are similar. Nevertheless, we expect the estimates in Table 3 are biased towards zero due to classical measurement error.

Missing Paternal Information

The share of births where the father's race is missing has been increasing over time, largely due to increases in non-marital childbearing.[46] Desegregation plans might affect the willingness to report the father's race for a given *mixed-race* birth, in which case part of any effect we estimate would be due to reporting.[47] This would represent a "real" effect of desegregation plans; an increased willingness to report an other-race father reflects some change in attitudes or environment, albeit on a different margin. We expect this reporting effect to be small, though we cannot address it directly with available data.

In theory, we could do a bounding exercise in which we assume all births to black mothers with unreported father's race to be to white fathers. In practice, however, the exercise would not be informative because births with unreported fathers' race swamp mixed-race births in the data.[48] Ultimately, the results can be interpreted as the effect of desegregation plans on mixed-race childbearing *for the types of births where the father's race is reported.*

In Appendix Table 2, we report results from analyses exploring the potential impact of missing paternal race data. In Table 3, we limited our data to observations where the race of the father is reported (so we are estimating the effect of desegregation on the probability the father is white, among births where the father's race is reported). Appendix Table 2 (Panel A) shows the

[46] Information about the father, including race, is more likely to be missing when parents were not married. Though reporting of unmarried fathers' characteristics increased over time, the simultaneous increase in the prevalence of non-marital births swamped this effect so that the overall share of births missing paternal race data increased nonetheless.
[47] Desegregation-induced changes in reporting would reflect "real" changes in racial attitudes, albeit on a less intensive margin than changes in mixed-race births themselves.
[48] In our sample from 1968 to 2003, the share of births to black mothers in which the father's race was not reported grew from 23 to 36 percent, while the share of births to black mothers in which the father's race was reported as white grew from just 0.4 to 3.2 percent.

same specifications as in Table 3 including all births in the sample, showing the effect of desegregation on *reporting a white father* among all births.[49] The results are consistent with the (non-) findings reported in Table 3.

Giving birth to a child without reporting the father on the birth certificate could be considered an adverse outcome, and desegregation could have affected this outcome directly.[50] In Appendix Table 2, Panel B, we estimate the same specifications as in Table 3 but with missing paternal race as an outcome. We observe a consistently positive relationship between the implementation of desegregation plans and the prevalence of not reporting father's race (or other characteristics) for black mothers. This finding is broadly consistent with Bifulco, Lopoo, and Oh (2015), who conclude that court-ordered school desegregation plans increased fertility for black teenagers. As in the main results for mixed-race childbearing, the estimates are sensitive to how we specify cohort controls. Results from the specifications controlling for South- and/or 1960 black share-by-cohort (columns 6-8) are not statistically significant.

Because the dependent variables in Panels A and B of Appendix Table 2 use the same denominator, we can add the coefficients in panels A and B to get the estimated effect of desegregation plans on having a child where the father is *either* not reported (from Panel B) or white (from Panel A). Any exploration of how much missing paternal data biases estimates requires some assumption of how likely the desegregation-induced missing fathers were to be white. For example, if the desegregation-induced "missing fathers" were white 3.1 percent of the time—the same percent as for black women who reported the father's race in 2003 in the sampled counties—then the estimated effect of full exposure to a desegregation plan on mixed-

[49] We cannot do the same sensitivity analysis for missing black fathers, as we do not know who they are.
[50] Tan et al. (2004) point to missing father's information on the birth record as "a novel indicator for identifying high risk population of adverse pregnancy outcomes."

race births would be 0.292 percentage points (9.418 × 0.031) higher than the (negative) estimates in Panel A (based on column 10). This would not change the substantive conclusion that the estimated effects are small and sensitive to how the cohort controls are specified.

VII. Conclusion

We find that while attending less-segregated schools is strongly positively *correlated* with subsequent mixed-race childbearing, the relationship is greatly attenuated by the addition of just a few covariates, suggesting that preference-based sorting into school is an important determinant of this relationship. When we exploit quasi-random variation in the timing of implementation for court-ordered desegregation plans to estimate causal effects of school desegregation, we find no significant impact on mixed-race births in our preferred estimates, accounting for the differential trend in cohort effects for Chicago.

We interpret these results by revisiting three key caveats. First, the bar of mixed-race births is a high one. Desegregation could have affected many relevant attitudes and behaviors related to racial tolerance and intergroup relations without having an impact on the prevalence of mixed-race births. Second, our treatment measures the timing of court-ordered desegregation plans. These plans were often accompanied by significant social tension, and may not have been accompanied by the types of quality social interactions emphasized in discussions of contact theory, due to institutional segregation across classrooms and self-selection into social peer groups within them. Finally, the causal estimates may be biased down due to measurement error in the key treatment indicator variables. School desegregation may explain none of the increase in mixed-race births in recent decades or as much as a quarter of the increase in mixed-race births with black fathers (the 95th percentile of the confidence interval). Given the downward bias of measurement error, the true effect could be larger.

While the nature of this natural experiment and available data limit our ability to precisely estimate the causal effects of school desegregation on mixed-race childbearing, our examination reveals the critical role sorting on preferences plays. Future work attempting to determine effects of—rather than correlates with—desegregation must identify variation in exposure that is independent of preferences to estimate the impact of the environment on social relations and other outcome.

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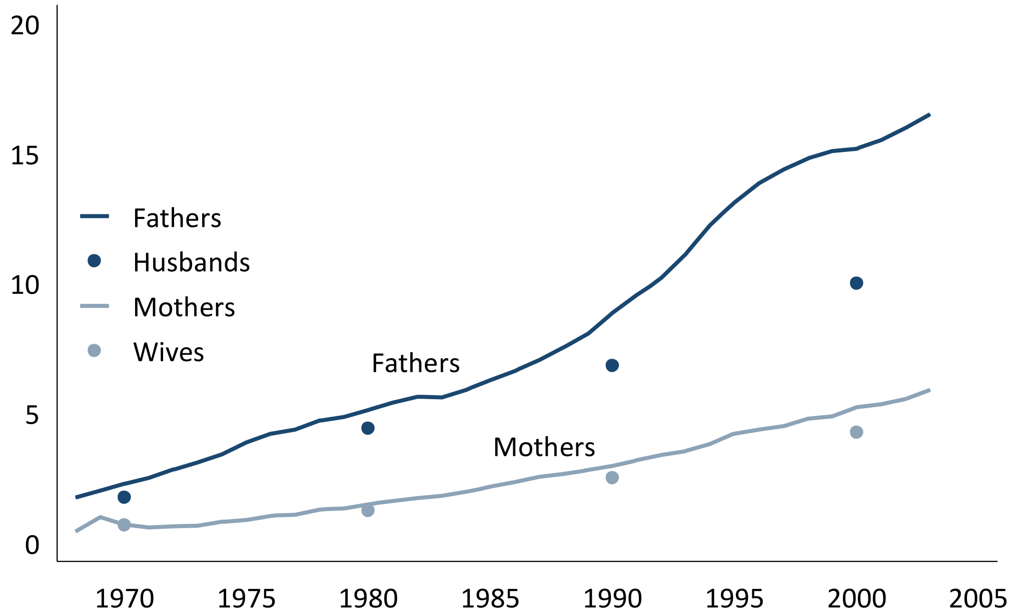
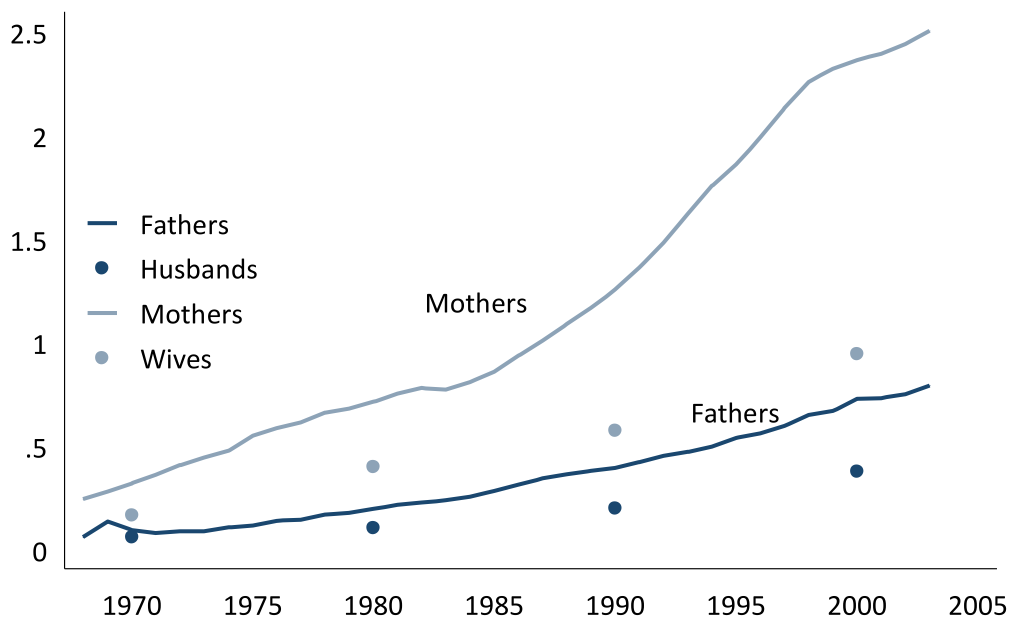
Figure 1. Mixed-race Births and Marriages, United States

A. Mixed-race Births and Marriages, Percent of Blacks with White Partner

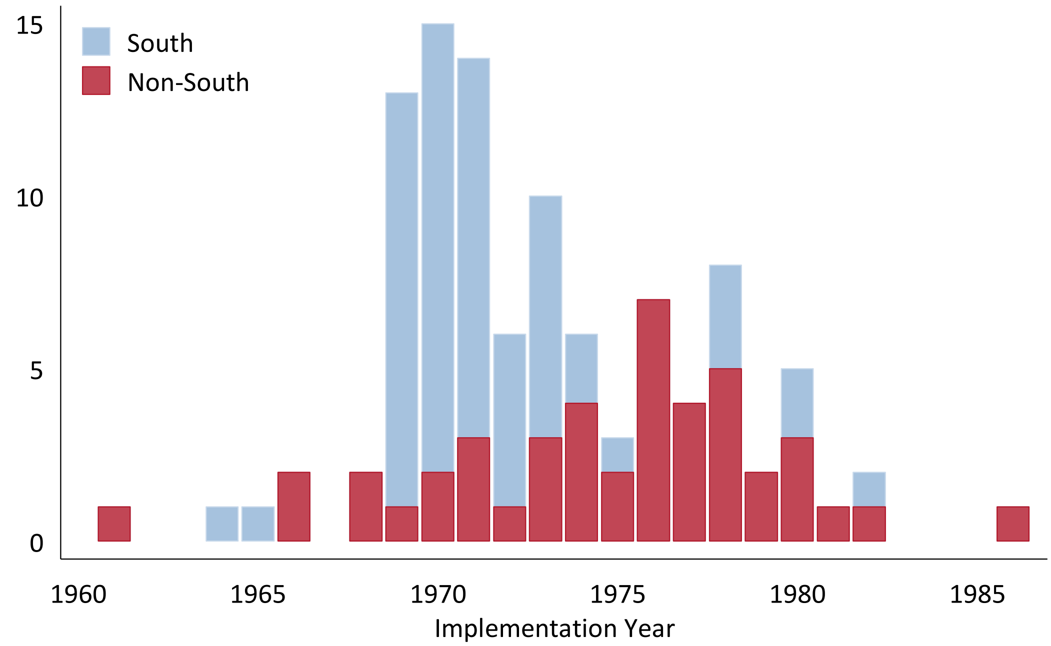


B. Mixed-race Births and Marriages, Percent of Whites with Black Partner



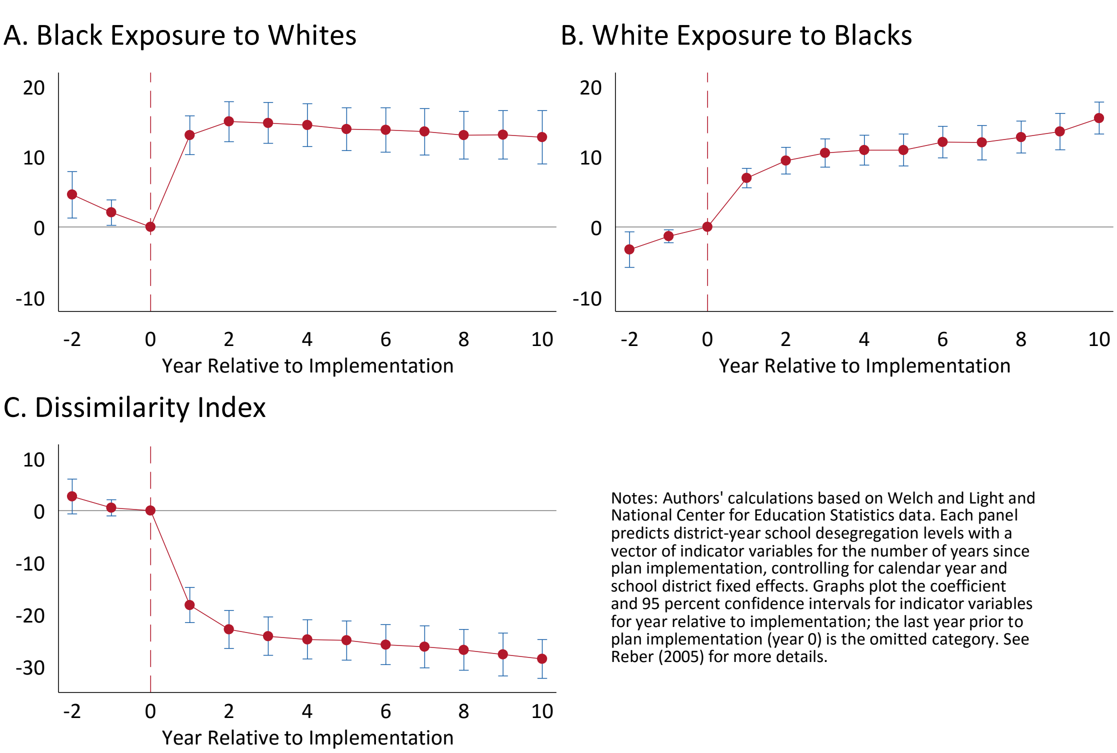
Note: Authors' calculations based on National Center for Health Statistics (NCHS) data (1968-2004) obtained from the National Bureau of Economic Research (NBER) and Census Data obtained from IPUMS. Data are restricted to births where both parents' race is reported and where the focal parent/spouse is 18-35 years old. Observations with missing father race are excluded.

Figure 2. School Desegregation Plan Implementation Year, by Region



Authors' calculations based on Welch and Light (1987). See text for sample details.

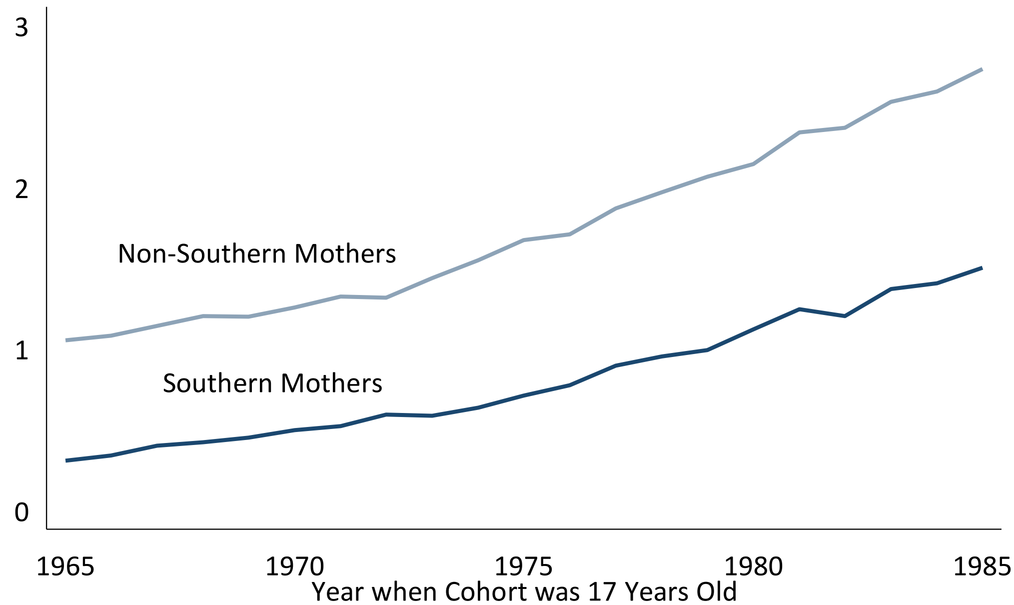
Figure 3. Effects of Plan Implementation on School Segregation



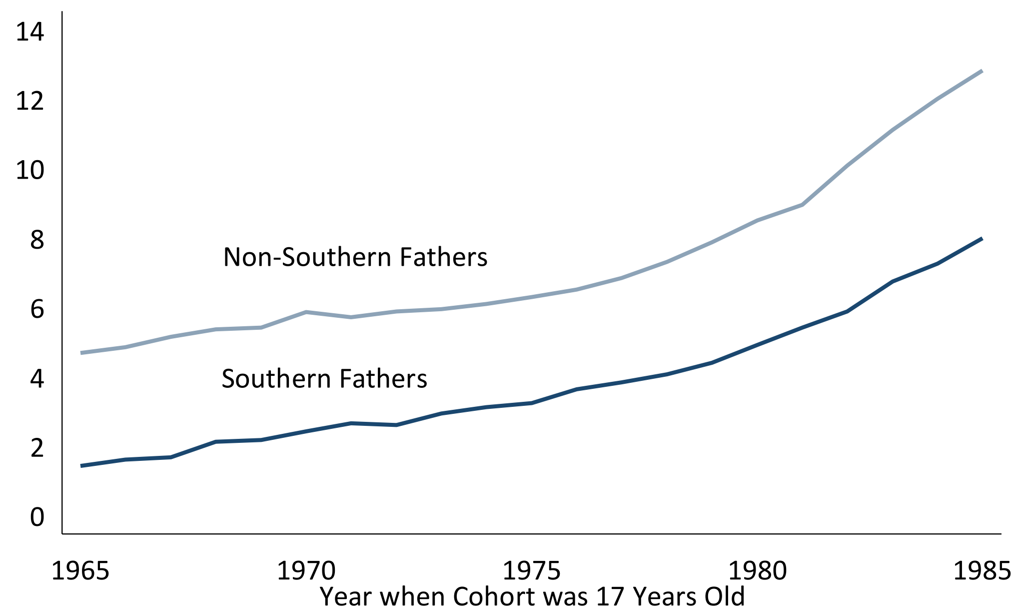
Notes: Authors' calculations based on Welch and Light and National Center for Education Statistics data. Each panel predicts district-year school desegregation levels with a vector of indicator variables for the number of years since plan implementation, controlling for calendar year and school district fixed effects. Graphs plot the coefficient and 95 percent confidence intervals for indicator variables for year relative to implementation; the last year prior to plan implementation (year 0) is the omitted category. See Reber (2005) for more details.

Figure 4. Percent of Births to Blacks with Other Parent White

A. Mothers



B. Fathers



Notes: Authors' calculations based on National Center for Health Statistics data (1968-2004) obtained from the National Bureau of Economic Research. Graph plots the coefficients on cohort indicators from a regression controlling for age fixed effects; for comparability with the analysis in Table 3, the sample is limited to mothers (fathers) aged 18 to 35 where the race of the other parent is observed. The estimates are rescaled so that the level for the 1965 cohort (the omitted category) is equal to its observed mean.

Table 1. Summary Statistics

A. School Segregation in Treated Districts	1968		1984	
Black exposure to whites	0.260		0.405	
White exposure to blacks	0.083		0.267	
Dissimilarity index	0.744		0.414	
Number of Districts	89		89	
	Sampled counties		Rest of United States	
B. Births	1968	2003	1968	2003
Mixed-race births*				
% of births to black mothers with white father	0.4	3.1	0.3	4.4
% of births to black fathers with white mother	2.0	13.2	1.6	19.4
% of births to white mothers with black father	0.4	2.7	0.2	2.0
% of births to white fathers with black mother	0.1	1.0	0.1	0.7
Percent of births with race of father missing				
Black mothers	20.6	37.3	28.4	35.1
White mothers	3.8	11.1	3.7	11.0
Percent of births				
All mothers	34.5	33.6	65.5	66.4
Black mothers	47.7	48.1	52.3	51.9
White mothers	32.3	30.4	67.7	69.6
Number of birth records	1,201,618	1,368,468	2,278,588	2,703,777
Number of counties**	100	98	3,001	-
C. County-level Characteristics	Sampled counties		Rest of United States	
Mean population (thousands), 1960	591.2		39.9	
Black percent of population, 1960	13.8		9.7	
Percent of counties in the South	52.0		45.5	
Mean population density, 1960	1768		121	
Percent votes for LBJ, 1964	62.3		62.3	
Number of counties	100		3,005	

Sources: Panel A: Welch and Light (1987) and National Center for Education Statistics Common Core of Data. Panel B: National Center for Health Statistics (1968, 2003) and Welch and Light (1987). Panel B: County and City Data Book, 1947-1977 (ICPSR Study 7736).

Notes: Summary statistics in Panel A are limited to districts present in both years of data. Alaska and Washington, DC are excluded in Panel B due to inconsistent reporting of race in the natality data. The summary statistics in Panels B and C are calculated using the same sample restrictions as the main analysis. See text.

* Calculations exclude births where the race of either parent is missing (in practice, this only affects fathers).

** After 1988, the natality data do not provide specific county codes for smaller counties, so we do not report the number of counties in the Rest of the United States; two Welch and Light counties are not identified in 2003.

Table 2. School Segregation and Black-white Mixed-race Births: Correlations

	A. Percent of Children Born to Black Mothers that Have a White Father						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mean of the dependent variable				1.871			
Black exposure to whites in schools (senior year)	3.427***		1.098	2.475***	2.813***	0.026	-0.204
	(0.751)		(0.776)	(0.550)	(0.805)	(0.503)	(0.494)
Black share in schools (senior year)		-4.158***	-3.566***				-3.467***
		(0.599)	(0.806)				(1.046)
R-squared	0.12	0.22	0.22	0.73	0.27	0.87	0.87
County FE				X		X	X
Cohort FE					X	X	X

	B. Percent of Children Born to Black Fathers that Have a White Mother						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mean of the dependent variable				5.665			
Black exposure to whites in schools (senior year)	15.81***		8.170***	7.931***	14.70***	3.938***	3.576***
	(2.622)		(2.193)	(1.373)	(2.671)	(1.113)	(1.134)
Black share in schools (senior year)		-16.32***	-11.82***				-5.496**
		(2.278)	(1.970)				(2.548)
R-squared	0.25	0.32	0.36	0.85	0.34	0.93	0.93
County FE				X		X	X
Cohort FE					X	X	X
Observations	2100	2100	2100	2100	2100	2100	2100
Counties	100	100	100	100	100	100	100

Sources: Authors' analysis of National Center for Health Statistics (1968-2003); Welch and Light (1987).
Notes: All regressions are weighted by the number of births in the county-cohort cell. Standard errors clustered on county in parentheses. Sample is limited to mothers (fathers) aged 18 to 35 where the race of the other parent is known. Outcome variable ranges from 0 to 100 and is the percent of births where the other parent is white in the county-cohort cell. County-cohorts are assigned black exposure to whites (the exposure index, ranging from 0 to 1) and black share for the relevant Welch and Light district from their senior year of high school. Some missing segregation data are imputed; see text.
* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 3. Effects of Court-ordered Desegregation on Black-white Mixed-race Births

	A. Percent of Children Born to Black Mothers that Have a White Father									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
# years of potential exposure	County FE Only	+Cohort FE	Unweighted	Cohort FE, Excl Chicago	Cohort FE, Chicago Trend	CohortXSouth	CohortXPerBl	CohortXSouth CohortXPerBl	Add Cnty Trends	Add Chicago Trend Only
1 to 4	0.738***	0.061	-0.223	-0.062	-0.067	0.168	0.083	0.059	-0.009	-0.055
	(0.110)	(0.106)	(0.168)	(0.110)	(0.098)	(0.140)	(0.096)	(0.115)	(0.037)	(0.098)
5 to 8	1.302***	0.258	-0.282	-0.079	-0.073	0.554*	0.323	0.275	0.005	-0.022
	(0.155)	(0.251)	(0.314)	(0.204)	(0.194)	(0.307)	(0.216)	(0.248)	(0.077)	(0.186)
9 to 12	1.738***	0.305	-0.606	-0.167	-0.155	0.831**	0.534*	0.395	-0.020	0.005
	(0.179)	(0.376)	(0.495)	(0.341)	(0.327)	(0.417)	(0.316)	(0.335)	(0.122)	(0.268)
fully exposed	2.509***	0.562	-1.075	-0.093	-0.077	1.302**	0.912**	0.705	0.080	0.183
	(0.178)	(0.505)	(0.695)	(0.446)	(0.436)	(0.560)	(0.426)	(0.464)	(0.155)	(0.374)
R-squared	0.85	0.87	0.82	0.87	0.87	0.87	0.90	0.90	0.95	0.91
	B. Percent of Children Born to Black Fathers that Have a White Mother									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
# years of potential exposure										
1 to 4	1.512***	0.354**	-0.073	0.219	0.060	0.325	0.374***	0.132	-0.123	-0.107
	(0.177)	(0.161)	(0.282)	(0.213)	(0.229)	(0.218)	(0.135)	(0.157)	(0.127)	(0.178)
5 to 8	3.172***	1.185**	-0.059	0.586	0.461	1.242**	1.196***	0.718	0.093	0.119
	(0.376)	(0.467)	(0.645)	(0.371)	(0.376)	(0.619)	(0.388)	(0.446)	(0.202)	(0.328)
9 to 12	4.192***	1.363*	-0.822	0.515	0.365	1.703*	1.792***	0.874	-0.056	0.089
	(0.436)	(0.727)	(1.094)	(0.677)	(0.677)	(0.882)	(0.643)	(0.690)	(0.295)	(0.629)
fully exposed	6.382***	1.952*	-1.863	0.728	0.583	2.621**	2.635***	1.282	-0.122	0.234
	(0.512)	(0.984)	(1.534)	(0.870)	(0.889)	(1.239)	(0.915)	(1.019)	(0.433)	(0.949)
R-squared	0.92	0.93	0.90	0.93	0.93	0.93	0.95	0.95	0.98	0.95
County FE	X	X	X	X	X	X	X	X	X	X
Cohort FE		X	X	X	X	X	X	X	X	X
Cohort FE X South						X		X	X	X
Cohort FE X 1960 fraction black							X	X	X	X
County-specific linear trend									X	
Chicago-specific linear trend					X				X	X
Excluding Chicago				X						
Weight	births	births	county	births	births	births	births	births	births	births
Observations	2100	2100	2100	2,079	2100	2100	2100	2100	2100	2100
Counties	100	100	100	99	100	100	100	100	100	100

Sources: Authors' analysis of National Center for Health Statistics (1968-2003); 1960 City and County Data Book (ICPSR, 2008); Welch and Light (1987).
Notes: Standard errors clustered on county in parentheses. Sample is limited to mothers (fathers) aged 18 to 35 where the race of the other parent is known. Outcome variable ranges from 0 to 100 and is the percent of births where the other parent is white in the county-cohort cell.
* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 4. Sensitivity to Alternative Sample Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Percent of Children Born to Black Mother that Have a White Father, by Mother's Age							
	18-35	18-30	18-25	16-35	16-30	16-25	18-35	18-35
# years of potential exposure								
1 to 4	-0.055	-0.034	-0.047	-0.059	-0.040	-0.057	-0.081	-0.019
	(0.098)	(0.092)	(0.098)	(0.095)	(0.090)	(0.094)	(0.112)	(0.136)
5 to 8	-0.022	-0.004	0.032	-0.016	0.000	0.030	-0.093	0.029
	(0.186)	(0.179)	(0.187)	(0.183)	(0.177)	(0.183)	(0.215)	(0.241)
9 to 12	0.005	0.009	-0.030	0.029	0.032	-0.003	-0.103	0.106
	(0.268)	(0.257)	(0.260)	(0.266)	(0.254)	(0.255)	(0.301)	(0.345)
fully exposed	0.183	0.168	0.075	0.204	0.186	0.096	-0.211	0.443
	(0.374)	(0.350)	(0.365)	(0.371)	(0.346)	(0.356)	(0.400)	(0.486)
R-squared	0.91	0.88	0.84	0.91	0.89	0.84	0.85	0.86
Limited to first births							X	
Mother born in same state as child								X
	B. Percent of Children Born to Black Father that Have a White Mother, by Father's Age							
	18-35	18-30	18-25	16-35	16-30	16-25		
# years of potential exposure								
1 to 4	-0.107	-0.112	-0.192	-0.107	-0.112	-0.190		
	(0.178)	(0.193)	(0.203)	(0.178)	(0.194)	(0.204)		
5 to 8	0.119	0.113	-0.040	0.126	0.120	-0.033		
	(0.328)	(0.334)	(0.343)	(0.326)	(0.333)	(0.343)		
9 to 12	0.089	0.100	-0.056	0.129	0.140	-0.007		
	(0.629)	(0.631)	(0.630)	(0.631)	(0.634)	(0.633)		
fully exposed	0.234	0.118	-0.105	0.294	0.183	-0.019		
	(0.949)	(0.970)	(0.977)	(0.954)	(0.973)	(0.979)		
R-squared	0.95	0.94	0.91	0.95	0.94	0.92		
Observations	2100	2100	2100	2100	2100	2100	2100	2100
Number of Counties	100	100	100	100	100	100	100	100

Sources: Authors' analysis of National Center for Health Statistics (1968-2003); 1960 City and County Data Book (ICPSR, 2008); Welch and Light (1987).

Notes: All regressions include the same controls as Table 3, column 10. Standard errors clustered on county in parentheses.

* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Appendix Table 1. Sensitivity of Estimates to Excluding Large Counties

	A. Percent of Children Born to Black Mothers that Have a White Father						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Excluded county	None	Chicago	Los Angeles	Philadelphia	Baltimore	Detroit	Houston
Share of sample	0.000	0.135	0.093	0.062	0.040	0.039	0.033
# years of potential exposure							
1 to 4	0.061	-0.062	0.043	0.063	0.077	0.071	0.050
	(0.106)	(0.110)	(0.102)	(0.113)	(0.106)	(0.108)	(0.110)
5 to 8	0.258	-0.079	0.283	0.268	0.307	0.283	0.259
	(0.251)	(0.204)	(0.228)	(0.277)	(0.256)	(0.256)	(0.259)
9 to 12	0.305	-0.167	0.395	0.263	0.401	0.341	0.300
	(0.376)	(0.341)	(0.338)	(0.413)	(0.374)	(0.382)	(0.386)
fully exposed	0.562	-0.093	0.686	0.494	0.582	0.574	0.559
	(0.505)	(0.446)	(0.453)	(0.557)	(0.518)	(0.513)	(0.516)
R-squared	0.867	0.867	0.858	0.867	0.868	0.866	0.866
	B. Percent of Children Born to Black Fathers that Have a White Mother						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Excluded county	None	Chicago	Los Angeles	Philadelphia	Baltimore	Detroit	Houston
Share of sample	0.000	0.133	0.096	0.062	0.043	0.035	0.032
# years of potential exposure							
1 to 4	0.354**	0.219	0.300*	0.370**	0.380**	0.363**	0.346**
	(0.161)	(0.213)	(0.170)	(0.164)	(0.162)	(0.164)	(0.166)
5 to 8	1.185**	0.586	1.116**	1.303**	1.283***	1.230**	1.153**
	(0.467)	(0.371)	(0.506)	(0.498)	(0.478)	(0.473)	(0.481)
9 to 12	1.363*	0.515	1.293	1.273	1.621**	1.452*	1.284*
	(0.727)	(0.677)	(0.784)	(0.795)	(0.710)	(0.737)	(0.744)
fully exposed	1.952*	0.728	1.876*	1.756	1.973*	1.955*	1.879*
	(0.984)	(0.870)	(1.077)	(1.082)	(1.018)	(0.996)	(1.007)
R-squared	0.930	0.929	0.930	0.931	0.931	0.930	0.930
Weight	births	births	births	births	births	births	births
Observations	2,100	2,079	2,079	2,079	2,079	2,079	2,079
Counties	100	99	99	99	99	99	99

Sources: See Table 3.

Notes: Standard errors clustered on county in parentheses. See Table 3 notes. Column 1 repeats the results in Table 3 column 2, including county and cohort fixed effects; columns 2-7 use the same specification, excluding one county at a time (indicated in each column header according to the WL school district in the county).

Appendix Table 2. Role of Missing Father's Race

	A. Percent of Children Born to Black Mothers that have a White Father (denominator includes missing father race)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
# years of potential exposure										
1 to 4	0.447***	-0.028	-0.306*	-0.086	-0.100	0.008	-0.034	-0.073	-0.036	-0.140*
	(0.105)	(0.075)	(0.165)	(0.089)	(0.079)	(0.105)	(0.070)	(0.088)	(0.027)	(0.079)
5 to 8	0.779***	0.032	-0.432	-0.157	-0.162	0.172	0.047	-0.026	-0.030	-0.212
	(0.143)	(0.168)	(0.291)	(0.165)	(0.157)	(0.229)	(0.154)	(0.191)	(0.047)	(0.153)
9 to 12	0.958***	-0.087	-0.815*	-0.360	-0.361	0.185	0.024	-0.119	-0.046	-0.370
	(0.129)	(0.263)	(0.439)	(0.278)	(0.270)	(0.325)	(0.229)	(0.269)	(0.075)	(0.234)
fully exposed	1.380***	-0.037	-1.213**	-0.419	-0.417	0.391	0.193	0.006	-0.020	-0.328
	(0.139)	(0.356)	(0.608)	(0.370)	(0.364)	(0.391)	(0.306)	(0.343)	(0.089)	(0.292)
R-squared	0.86	0.88	0.84	0.88	0.88	0.88	0.91	0.91	0.95	0.91
	B. Percent of Children Born to Black Mothers where Father's Race is Missing									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
# years of potential exposure										
1 to 4	8.048***	2.544***	2.834***	2.168**	3.191***	2.514*	2.607***	2.990**	2.543***	3.616***
	(1.763)	(0.863)	(0.661)	(0.961)	(1.100)	(1.400)	(0.846)	(1.448)	(0.757)	(1.295)
5 to 8	10.77***	2.922**	4.420***	4.039**	4.670***	2.304	2.779**	3.389	2.334*	5.119**
	(1.891)	(1.356)	(1.245)	(1.790)	(1.687)	(2.844)	(1.329)	(2.894)	(1.328)	(2.215)
9 to 12	14.70***	4.666*	6.282***	6.599**	7.143**	3.472	4.044*	5.108	1.171	7.440**
	(1.744)	(2.399)	(2.016)	(3.083)	(3.004)	(3.798)	(2.413)	(3.691)	(2.522)	(3.109)
fully exposed	18.31***	5.679	8.410***	8.771**	9.117**	4.346	4.512	6.312	1.591	9.418**
	(2.120)	(3.519)	(2.845)	(4.364)	(4.361)	(4.360)	(3.559)	(4.204)	(3.571)	(3.614)
R-squared	0.93	0.94	0.90	0.93	0.94	0.94	0.94	0.94	0.98	0.94
County FE	X	X	X	X	X	X	X	X	X	X
Cohort FE		X	X	X	X	X	X	X	X	X
Cohort FE X South						X		X	X	X
Cohort FE X 1960 fraction black							X	X	X	X
County-specific linear trend									X	
Chicago-specific linear trend					X				X	X
Excluding Chicago				X						
Weight	births	births	county	births	births	births	births	births	births	births
Observations	2100	2100	2100	2,079	2100	2100	2100	2100	2100	2100
Counties	100	100	100	99	100	100	100	100	100	100

Sources: Authors' analysis of National Center for Health Statistics (1968-2003); 1960 City and County Data Book (ICPSR, 2008); Welch and Light (1987).

Notes: Standard errors clustered on county in parentheses. Results in Panel A are the same as in Table 3, except that the sample includes births where the father's race is missing (the denominator is different). Panel B reports the results of the same specification with missing father's race as the outcome. Outcome variables range from 0 to 100 and are the percent of births where the father is white (Panel A) or missing (Panel B).

* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.