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LONG-RUN IMPACTS OF SCHOOL DESEGREGATION & SCHOOL QUALITY
ON ADULT ATTAINMENTS

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ABSTRACT

This paper investigates the long-run impacts of court-ordered school desegregation on an array of adult socioeconomic and health outcomes. The study analyzes the life trajectories of children born between 1945 and 1968, and followed through 2013, using the Panel Study of Income Dynamics (PSID). The PSID data are linked with multiple data sources that describe the neighborhood attributes, school quality resources, and coincident policies that prevailed at the time these children were growing up. I exploit quasi-random variation in the timing of initial court orders, which generated differences in the timing and scope of the implementation of desegregation plans during the 1960s, 70s, and 80s. Event study analyses as well as 2SLS and sibling-difference estimates indicate that school desegregation and the accompanied increases in school quality resulted in significant improvements in adult attainments for blacks. I find that, for blacks, school desegregation significantly increased both educational and occupational attainments, college quality and adult earnings, reduced the probability of incarceration, and improved adult health status; desegregation had no effects on whites across each of these outcomes. The results suggest that the mechanisms through which school desegregation led to beneficial adult attainment outcomes for blacks include improvement in access to school resources reflected in reductions in class size and increases in per-pupil spending.

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I. INTRODUCTION

Racial segregation that results in race differences in access to school quality has often been cited as perpetuating inequality in attainment outcomes. Since the landmark 1954 Supreme Court *Brown v. Board of Education* decision and subsequent court-ordered implementation of school desegregation plans during the 1960s, 70s and 80s, scholars have investigated the consequences of school desegregation on socioeconomic attainment outcomes of black children (Clotfelter, 2004; Rivkin & Welch, 2006). However, few large-scale data collection efforts were undertaken to investigate school desegregation program effects, particularly on longer-run outcomes. A recent, but growing body of evidence indicates that school desegregation improved black students' educational attainment (Guryan, 2004; Reber, 2010; Hanushek et al., 2009), increased blacks' subsequent adult incomes (Ashenfelter et al., 2005), and decreased rates of criminal offending by black youth (Weiner, Lutz, Ludwig, 2009).

This paper contributes to the literature a unified evaluation of the long-run impacts of school desegregation on adult outcomes across several domains using a more compelling research design and more comprehensive data. I investigate the extent and mechanisms by which school desegregation and resultant changes in school inputs causally influence subsequent adult socioeconomic and health outcomes. The primary difficulty in disentangling the relative importance of childhood family, neighborhood, and school quality factors is isolating variation in school quality characteristics that are unrelated to family and neighborhood factors.

This study analyzes the life trajectories of children who were born between 1945 and 1968 and have been followed through 2013, using the longest-running US nationally-representative longitudinal data spanning more than four decades. To this data, I link information from multiple data sources that contain detailed neighborhood attributes, school quality resources, and coincident policies that prevailed at the time these children were growing up. I also obtained and linked a comprehensive desegregation case inventory for the years between 1954 and 1990 that contains detailed information for every US school district that implemented a court-ordered desegregation plan, the year of the initial court order, and

the type of desegregation court order.[1] The implementation of court-ordered school desegregation during the childhoods of these birth cohorts provides a unique opportunity to evaluate their long-run impacts.

The analysis is presented in two stages. First, I present new evidence of how court-ordered school desegregation influenced the quantity and quality of educational inputs received by minority children. Utilizing an event-study research design with both district-level and school-level data, the primary empirical strategy exploits quasi-random variation in the timing of initial court orders to identify effects. I find that desegregation plans were effective in narrowing black-white gaps in per-pupil school spending and class size and decreasing school segregation. Second, I investigate the long-run impacts of court-ordered desegregation on subsequent attainment outcomes, including whether graduated from high school, years of completed education, college quality, adult earnings and occupational attainment, income and poverty status, probability of incarceration, and adult health status. I estimate fully non-parametric event study models and use the wide variation in the timing of initial court orders and scope of desegregation to identify their effects.

School desegregation and the accompanied increases in school quality resulted in significant improvements in adult attainments for blacks. I find that, for blacks, school desegregation significantly increased both educational and occupational attainments, college quality and adult earnings, reduced the probability of incarceration, and improved adult health status; desegregation had no effects on whites across each of these outcomes. In order to attempt to identify the potential mechanisms, I analyze the role of desegregation-induced changes in per-pupil spending and racial school integration, respectively, independent of district-specific trends and other coincident policies. Changes in per-pupil spending and racial integration resultant from court-ordered desegregation are interpreted as markers for the intensity of treatment. I find that blacks' adult attainments increased significantly with both the amount of induced increase in school spending and the duration of desegregation exposure, with no apparent dose-response in the amount of racial integration resultant from court orders. Desegregation had no effects on whites' adult outcomes, in neither the duration of exposure nor the intensity of treatment. The results suggest that the mechanisms through which school desegregation led to beneficial adult attainment outcomes for

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blacks include improvement in access to school resources reflected in reductions in class size and increases in per-pupil spending.

As an alternative empirical strategy, I use sibling comparisons to identify the effects of school desegregation on adult socioeconomic and health outcomes. This identification strategy compares the adult outcomes of individuals who were exposed to integrated schools during childhood with the corresponding adult outcomes of their siblings (evaluated at the same age) who grew up in the same communities but had already reached age 18 prior to desegregation or were exposed to integrated schools for only a limited period of their childhood, conditional on year of birth effects. The pattern of results is similar across all of the empirical approaches (event study models, 2SLS and sibling fixed effect models), and reveals significant long-run impacts of school desegregation and school quality on a broad range of adult outcomes. The results are robust to a battery of specification tests, which provides supportive evidence that the estimates reflect the causal impacts of school desegregation and school quality. As evidenced herein, the black-white adult socioeconomic and health disparities gap narrowed for the cohorts exposed to integrated schools during childhood.

The empirical analysis builds on and extends the literature by investigating (1) non-racial integration aspects of court-ordered desegregation through its impacts on per-pupil spending; (2) the effects of court-ordered school desegregation on adult SES and health outcomes while simultaneously accounting for other important coincident policy changes; and (3) the role of childhood school quality in contributing to socioeconomic and racial health disparities in adulthood. By examining life-course effects of school desegregation across a broad range of subsequent outcomes, I attempt to shed light on the mechanisms through which differences in school quality translate into differences in adult outcomes.

The remainder of the paper is organized as follows. Section II briefly describes the timing of court-ordered school desegregation. Section III presents the data used. Section IV outlines the empirical strategy. Section Va presents results of the effects of school desegregation on school quality inputs (per-pupil spending; class size; school segregation). This informs what the typical "treatment" represented for the average black child. Section Vb presents results of the long-run impacts on adult outcomes.Section VI

presents conclusions and summary discussion to put the magnitudes in perspective in relation to previous studies. All appendix material is in the online appendix.

II. USING THE TIMING OF COURT-ORDERED DESEGREGATION AS A QUASI-EXPERIMENT

It is hypothesized that school desegregation may have long-run impacts on the adult economic and health status of African Americans through several potential mechanisms: (1) school quality resource effects (e.g., the distribution and level of per-pupil spending, class size, teacher quality); (2) peer exposure effects (e.g., children in classrooms with highly motivated and high-achieving students are likely to perform better due to positive spillover effects on other students in the classroom); and (3) effects on parental, teacher, and community-level expectations of child achievement. The long-run effects of each hypothesized mechanism operate via their influence on the quality and quantity of educational attainment. I examine the hypothesized primary mechanism: changes in school quality resulting from abrupt shifts in racial school segregation.[2]

An understanding of the causes of the timing of desegregation is critical to the identification strategy. Accordingly, Appendix B provides a brief history of school desegregation litigation and implementation with an eye towards identification issues and demonstrating the validity of the research design—namely, the quasi-random timing of initial court orders. To document the substantial variation in the timing and intensity of school desegregation efforts, I use a comprehensive desegregation case inventory compiled by legal scholars for the years between 1954 and 1990 that contains detailed information for every US school district that implemented a court-ordered desegregation plan, in conjunction with additional data from Welch and Light (1987) on the dates of major desegregation plan implementation for large urban districts. Figure A1 presents the dates of initial court orders across the country among the 868 school districts ever subject to court-mandated desegregation between 1954 and 1980. Districts exhibit a great deal of variation in the year in which the initial court order was issued and the subsequent timing when major desegregation plan implementation actually took place; this variation is evidenced both within and across regions of the country (see Appendix Figures A0-A2).

Most school districts did not adopt major school desegregation plans until forced to do so by court order (or threat of litigation) due to individual cases filed in local Federal court. The importance of legal precedent caused the NAACP to strategically bring suits first, and foremost, when and where there was the greatest likelihood of winning, not where the largest potential gains from desegregation could be achieved for a particular local community at a point in time. Enforcement of desegregation did not begin in earnest until the mid-1960s. State and federal dollars proved to be the most effective incentives to desegregate the schools. A critical turning point was the enactment of Title VI of the 1964 Civil Rights Act (CRA) and Title I funds of the 1965 Elementary & Secondary Education Act (ESEA), which prohibited federal aid to segregated schools and allowed the Justice Department to join suits against school districts that were in violation of the Brown vs. Board order to integrate. This Act dramatically raised the amount of federal aid to education from a few million to more than one billion dollars a year; and, for the first time, the threat of withholding federal funds became a powerful inducement to comply with federal desegregation orders (Cascio et al., 2010; Holland, 2004). This resulted in a significant drop in the extent of racial school segregation thereafter reinforced by local Federal courts. Thus, there is a sharp post-1965 discontinuity in school desegregation.

This pattern and discontinuity after 1965 is also evident in the time lag between initial court order and major desegregation plan implementation, which occurs in the South and non-South (Appendix Figure B6). For initial court orders meted out after 1965, there is immediate implementation (on average, major plan implemented within 1-2 years of initial court order). On the other hand, for initial court orders meted out before 1965, there is more than a 10-year delay in implementation of a major plan (i.e., there is a systematic long delay that decreases in years leading up to 1965).

Litigation and desegregation plan implementation accelerated substantially between 1964 and 1972. For example, only 6 percent of the districts that would eventually undergo court-ordered desegregation had implemented major plans by 1968 (when the PSID began); by 1972 this rose to over 56 percent. It is this period of substantial growth in litigation activity, spurred by landmark court cases like the 1968 Green decision (which required immediate actions to effectively implement desegregation

plans), that forms the basis of the research design. The process became highly decentralized with a diverse set of agents that initiated court litigation following the Brown decision, which contributed to the idiosyncratic nature of the timing and location where legal challenges arose that resulted in initial court orders. Differences across districts in when desegregation court cases were first filed and the length of time it took these cases to proceed through the judicial system represents a plausibly exogenous source of identifying variation in the timing of school desegregation.

III. DATA

I compiled data on school spending and school segregation, linked them to a comprehensive database of the timing of court-ordered school desegregation, and linked these data to a nationally-representative longitudinal dataset that follows individuals from childhood into adulthood. Education funding data come from several sources that are combined to form a panel of per-pupil spending for US school districts in 1967 and annually from 1970 through 2000.[3] School segregation data come from the Office of Civil Rights (OCR), and are combined to form a panel used to construct school segregation indices that span the period 1968 through 1988. The school segregation and spending data are then linked to a database of desegregation litigation between 1954 and 2000.

The data on longer-run outcomes come from the Panel Study of Income Dynamics (PSID) that links individuals to their census blocks during childhood.[4] The sample consists of PSID sample members born between 1945 and 1968 who have been followed into adulthood through 2013; these individuals were between the ages of 45 and 67 in 2013. I include all information on them for each wave, 1968 to 2013.[5] Due to the oversampling of black and low-income families, 45 percent of the sample is black.

I match the earliest available childhood residential address to the school district boundaries that prevailed in 1969 to avoid complications arising from endogenously changing district boundaries over time. The algorithm is outlined in Appendix A. Each record is merged with data on the timing of court-ordered desegregation, data on racial school segregation, student-to-teacher ratios, school spending at the school district level that correspond with the prevailing levels during their school-age years. Finally, I

merge in county characteristics and information on other key policy changes during childhood (e.g., the timing of hospital desegregation, rollout of "War on Poverty" initiatives and expansion of safety net programs—described in Section IV) from multiple data sources.[6] This allows for a rich set of controls.

The comprehensive desegregation court case data I use contains an entire case inventory of every school district ever subject to court desegregation orders over the 1955-1990 period (American Communities Project), and major plan implementation dates in large districts (compiled by Welch/Light). Every court case is coded according to whether it involved segregation of students across schools, whether the court required a desegregation remedy, and the main component of the desegregation plan. The combined data from the American Communities Project (Brown University) and Welch/Light provide the best available data that have ever been utilized to study this topic for several reasons. First, the year of the initial court order (available for all districts) is plausibly more exogenous than the exact year in which a major desegregation plan was implemented because opposition groups to integration can delay major desegregation plan implementation by lengthening the court proceedings or by implementing inadequate desegregation plans (supportive evidence on this point is presented in Appendix B). And, court-ordered desegregation by legal mandate is plausibly more exogenous than other more voluntary forms of desegregation. Second, the date of the initial court order is precisely measured for all districts.

Sixty-nine percent of the PSID individuals born between 1945-1968 followed into adulthood grew up in a school district that was subject to a desegregation court order sometime between 1954 and 1990 (i.e., 9,156 out of 13,246 individuals), with the timing of the court order not necessarily occurring during their school-age years. Eighty-eight percent of the PSID black individuals born between 1945-1968 followed into adulthood grew up in a school district that was subject to a desegregation court order sometime between 1954 and 1990 (i.e., 4,618 out of 5,245 black individuals). The share of individuals exposed to school desegregation orders during childhood increases significantly with birth year over the 1945-1970 birth cohorts analyzed in the PSID sample (Appendix Figure B5).

After combining information from the aforementioned 5 data sources, the main sample used to analyze adult attainment outcomes consists of PSID individuals born between 1945-1968 originally from

school districts that were subject to desegregation court orders sometime between 1954 and 1990; this includes 9,156 individuals from 3,702 childhood families, 645 school districts, 448 counties, representing 39 different states. I restrict the estimation sample to individuals who grew up in school districts that were ever subject to court-ordered desegregation, since school districts of upbringing that were never under court order are arguably too different to provide a credible comparison group. Appendix A lists the sources and years of all data elements. Appendix Table C0 contains sample descriptive statistics for various childhood measures by race.

Outcomes of interest. The set of school inputs examined include per-pupil spending, student-to-teacher ratios, and racial school segregation among students and teachers. The measure of school spending during childhood is the average school spending (in real 2000 dollars) during expected school-age years (ages 5 through 17) in an individual's childhood school district.[7] Similarly, I measure the average student-to-teacher ratio during ages 5 through 17. The measures of racial school segregation include the average school-age black-white dissimilarity index and black-white exposure index among both students and teachers, respectively. The set of adult attainments examined chronologically over the life cycle include 1) educational outcomes—whether graduated from high school, years of completed education, college quality (proxied by 25th and 75th percentiles of SAT scores of the freshman class of college attended); 2) labor market and economic status outcomes (all expressed in real 2000 dollars)—occupational attainment (Duncan occupational prestige index), log wages, annual work hours, family income, annual incidence of poverty in adulthood (ages 20-50); 3) criminal involvement and incarceration outcomes—whether ever incarcerated (jail or prison) and the annual incidence of incarceration in adulthood; and 4) health outcomes—self-assessed general health status and the annual incidence of problematic health (ages 20-50). All analyses include men and women with controls for gender, given well-known gender differences in labor market, incarceration, and health outcomes. This data is combined to provide new evidence on the long-run impacts of school desegregation.

IV. EMPIRICAL APPROACH

The primary empirical strategy uses quasi-random variation in the timing of initial court orders to identify the effects of desegregation. Systematic variation in the timing of desegregation court orders could lead to spurious estimates of desegregation impacts if those same school district characteristics are associated with differential trends in the outcomes of interest. As shown below, the main way I test for this possibility is to use an event study model, which reveals no significant pre-existing time trends in the outcomes of interest. The exogeneity of this timing is supported theoretically by the documented legal history of school desegregation and by my own empirical examination of the issue. Table B1 also shows that collectively the bulk of pre-treatment school quality, SES, demographic, and labor market related characteristics does not significantly (jointly) predict the year of the initial court order (Appendix B). On the other hand, I find that districts with a larger minority population, greater per-capita school spending, and smaller proportion of residents with low income are each strongly associated with longer delays in major desegregation implementation following the initial court order. These results suggest that the timing of initial court litigation is more plausibly exogenous than the timing of major desegregation plan implementation. These findings inform the empirical approach used to identify school desegregation impacts.

Point-in-time comparisons of integrated and segregated school systems confound the effect of desegregation plans with the effect of factors that influenced their implementation. I match changes in school inputs and adult attainment outcomes of blacks and whites to the exact timing of court-ordered school desegregation. Average outcome trends in the years leading up to desegregation are compared to rule out competing explanations. As will be shown, the evidence is consistent with the identifying assumption that the timing of the initial court order is otherwise unrelated to trends in subsequent outcomes. Evidence of endogenous delays in implementation of major desegregation plans following (exogenous) initial court orders supports the research design's reliance on the timing of initial court

orders for identification, instead of directly using the timing of major desegregation plan implementation as prior studies have (discussed below).

The first stage of the analysis investigates how court-ordered school desegregation influenced the quantity and quality of educational inputs received by minority children. Following Card and Krueger (1992), I measure school quality as the purchased inputs to a school—per-pupil spending and the student-teacher ratio. Newly compiled school district-level and school-level panel datasets allow this analysis to use the staggered timing of court-ordered school desegregation within an event study analysis (cf. Jacobson, LaLonde and Sullivan, 1993; McCrary, 2007) to quantify desegregation effects on school resources. The event study framework compares school district per-pupil spending, student-to-teacher ratios, and school segregation levels among both students and teachers in the years immediately after court-ordered desegregation to the levels that prevailed in the years immediately before court orders for all districts that were ever subject to court orders. The second part of the analysis uses the same fully non-parametric event study models to quantify desegregation effects on educational attainment, incarceration, and adult economic and health status outcomes, separately by race. The analysis sample is restricted to districts that were ever subject to desegregation court orders, since districts that were never subject to court orders differ (e.g., on average, have a small number of minority students) and do not provide a valid counterfactual for the time-path of what would have occurred in the absence of desegregation.

To motivate the empirical strategy, I describe the policy experiment below. Individuals who turned 17 years old during the initial year of the desegregation court order in their school district should have completed secondary school by the time reforms were enacted. Such cohorts should be unaffected by desegregation so I classify them as "*unexposed*". In contrast, individuals who turned 16 years old or younger during the year of the passage of a court-ordered desegregation would likely have been attending primary or secondary school when desegregation plans were implemented. I refer to these cohorts as "*exposed*". One can estimate the exposure effect on school inputs and adult outcomes for blacks and whites from a particular district by comparing the change in outcomes between exposed and unexposed birth cohorts from that district, separately for blacks and whites. To account for any underlying

differences across birth cohorts, one can use the (race-specific) difference in outcomes across the same birth cohorts in districts that had not desegregated *during that time* as a comparison. The difference in outcomes between exposed and unexposed black cohorts in a treated district minus the difference in outcomes across the same black birth cohorts in comparison districts yields a Difference-in-Difference (DiD) estimate of the exposure effect on outcomes for blacks from that district. Similarly, one can obtain a DiD estimate of desegregation exposure effects for whites based on differences between exposed and unexposed white cohorts. The key identifying assumption is that the timing of initial court orders is otherwise unrelated to within-district changes in outcomes across birth cohorts. Under this assumption, an additional test of whether there is a causal effect of school desegregation is whether we witness larger improvements in school inputs and adult outcomes for blacks that experienced desegregation exposure for more of their school-age years (i.e., a dose-response effect); and likewise, we can examine exposure-duration effects for whites.

Theoretically, it is hypothesized that for African Americans, attending integrated schools during one's elementary school years may result in greater benefits than exposure to integrated schools only later in the school careers due to three factors: 1) elementary students may have fewer social adjustments than older students; 2) early learning begets later learning; and 3) secondary schools are more likely to track students by academic ability (and race), which could reduce benefits of desegregation for minorities. For these reasons, we may expect a dose-response effect of school desegregation exposure (and accompanied improvements in school quality).

I estimate fully non-parametric event study models of the form:

$$
\begin{aligned}
(1) \quad Y_{idb} = & \sum_{T=-20}^{-1} \alpha_T^r \cdot I_{T_{idb}=T} + \sum_{T=1}^{12} \theta_T^r \cdot I_{T_{idb}=T} + \sum_{T=13}^{20} \delta_T^r \cdot I_{T_{idb}=T} \\
& + X_{idb}\beta + Z_{db}\gamma + (W_{1960d} * b)\phi^r + \eta_d^r + \lambda_b^r + \varphi_g^r * b + \varepsilon_{idb}
\end{aligned}
$$

where i indexes the individual, d the school district, b the year of birth, g the region of birth (defined by 9 census division categories), and r the racial group. The variable T_{idb} is the year individual i from school district d turned age 17 minus the year of the initial desegregation court order in school district d.

Accordingly, the timing indicators, $I_{T_{idb}=T}$, are equal 1 if the year individual *i* from school district *d* turned age 17 minus the year of the initial desegregation court order in school district *d* equals *T* and zero otherwise. I include indicators for values of *T* between -20 and 20, which is the full support of years individuals were age 17 relative to initial court order years in the sample. Values of *T* between -20 and -1 represent unexposed cohorts who turned between the ages of 18 and 37 in the year of the initial court order; a value of 0 is our reference category and represents individuals who turned 17 in the year of the initial court order and were thus not exposed; values between 1 and 11 represent exposed cohorts who were "partially treated" because they were of school-going age (6 through 16) at the time of the initial court order but had less than 12 years of expected exposure; and values of 12 and greater represent fully treated exposed cohorts who turned 5 or younger during the year court-ordered desegregation was enacted and were therefore expected to attend desegregated schools for all 12 years of public schooling.

This DiD event study model compares the difference in outcomes between birth cohorts within the same district exposed to desegregation for different amounts of time (variation in exposure), separately by race. To only rely on variation across birth cohorts within districts I include race-specific school district fixed effects, η_d^r. To account for general underlying differences across birth cohorts (irrespective of exposure), I include race-specific birth year fixed effects (λ_b^r) and race-by-region of birth cohort trends ($\varphi_g^r * b$). With the birth-cohort fixed effects, the estimated changes across birth cohorts in desegregated districts are all relative to the changes across the same birth cohorts in districts that did not implement desegregation plans during that time.

The coefficients on the full set of event study year indicators ($\alpha_T^r; \theta_T^r; \delta_T^r$) map out the dynamic treatment effects (across birth cohorts from the same school district) of court-ordered desegregation on school inputs and adult attainment outcomes, separately by race.[8] I plot the estimated dynamic treatment effects for both blacks and whites to illustrate how school inputs and adult outcomes evolve for cohorts in school before, during, and after desegregation (relative to changes for the same birth cohorts in similar

districts that had not enacted desegregation plans at that time). The estimates of θ_T^r illustrate the exact timing of changes in outcomes in relation to the number of school-age years of exposure to court-ordered desegregation, separately by race; while the estimates of α_T^r provide a precise visual depiction of whether there are systematic time trends preceding enactment of court-ordered desegregation. The former uses the specific timing of changes to test for causal impacts of desegregation by race; the latter provides a test of endogeneity of the timing of initial court orders.

As long as the timing of court-ordered desegregation is exogenous to changes in outcomes across birth cohorts within districts, the coefficients θ_T^r should uncover the causal effects of school desegregation on adult outcomes, separately by race. It is important to note that, because the childhood school district prior to reforms may not always be the same school district an individual actually attends (due to residential mobility after reforms), θ_T^r are intent-to-treat estimates that quantify the policy effects of desegregation in an individual's childhood school district.[9] The model includes controls for an extensive set of child and childhood family characteristics (X_{icb}: parental education and occupational status, parental income, mother's marital status at birth, birth weight, child health insurance coverage, gender). The set of controls also involve interactions between 1960 characteristics of the county of birth and linear trends in the year of birth ($W_{1960d} * b$: 1960 county poverty rate, percent black, average education level, percent urban, population size), which include the percent of the county that voted for Strom Thurmond in the 1948 Presidential election (as a proxy for white segregationist preferences) as further controls for trends in factors hypothesized to influence the timing of desegregation.

The period in which school desegregation occurred overlaps other important coincident policy changes, including hospital desegregation in the South (Chay *et al*, 2009), the roll out and significant expansion of the safety net via War on Poverty and Great Society programs and initiatives, and is against the backdrop of the broader Civil Rights era. To account for these policy changes, I directly include county-by-cohort-level measures that capture the timing of hospital desegregation $\times$ race (exposure based

on place and year of birth), roll out of community health centers, state-funded initiatives for kindergarten introduction, Head Start per-capita expenditures at age 4, per-capita expenditures from Title-I school funding, and per-capita expenditures on food stamps, AFDC, Medicaid, unemployment insurance, each averaged over the individual's childhood years (Z_{db}). The data sources used to compile these measures are detailed in the Data Appendix. While this work draws heavily from prior research that have examined these other policy impacts, few studies have attempted to simultaneously account for such a comprehensive set of policies, in this case to isolate the causal impact of school desegregation. The models that analyze the economic and health status outcomes of interest use all available person-year observations in adulthood (for ages 20-50) with controls for age, age squared, and age cubed to avoid confounding life cycle and birth cohort effects. ε_{idb} is a random error term and the standard errors are clustered at the school district level.[10]

The identification strategy herein that exploits the quasi-random timing of initial court orders using an event study framework differs from influential design-based studies in the desegregation literature that have mostly relied upon the timing of major desegregation plan implementation using data from Welch/Light in 120 large districts (e.g., Guryan, 2004; Reber, 2005; Baum-Snow & Lutz, 2010). The most important among these is that of Guryan (2004), who did not estimate an event study model. Endogenous delays of major desegregation plan implementation following initial court orders threaten the validity of this previously used strategy. The existence of systematic, pre-desegregation trends could indicate that the timing of major desegregation plan implementation was endogenous to factors affecting post-plan outcomes, as evidence herein suggests (see Appendix B & Table B1). In contrast, as will be shown in Section V, I find no evidence of systematic pre-existing time trends in outcomes preceding initial court orders, which supports the validity of the research design.

I present graphical plots, separately by race, based on equation (1) estimates that form the response function of school desegregation effects to test for any dose-response with years of exposure.[11] The delays in desegregation implementation following initial court orders and well-documented riots and

protests surrounding the initial years of desegregation implementation in many communities suggest that desegregation efforts may have become more effective with time, as racial animosity and tensions lessened. Thus, this could generate a pattern in which there is a significant relationship between outcomes and event study years beyond 12 (δ_T^r) (since those who were pre-school ages at the time of initial court orders benefited from the experiences of the first cohort of black children who were pioneers of integration even if they each had the maximum 12 school-age years of exposure), which I explore.

Va. EFFECTS ON SCHOOL INPUTS

I present the effect of court-ordered desegregation on average school-age racial segregation, spending, and student-to-teacher ratio for the sample of court-ordered districts linked to individuals in the PSID. Similar event study model results of desegregation effects on school inputs using all districts that were ever under desegregation court orders are presented in Appendix B. The similarity of the results among all districts ever under court order and the subset of those districts that overlap the PSID affirm the representativeness and generalizability of the findings reported from the PSID. The event study figures trace out the (equilibrium) adjustment path for school inputs from the pre-desegregation plan period to the implementation of plans—allowing for possibility that efficacy of desegregation plans may erode over the long-run due to "white flight" (private school attendance or movement out of the district).

Reduction of Segregation within School Districts. The extent of segregation within districts diminished sharply during the period 1968-72. The changes were greatest in the Southeast, which had a smaller proportion of highly segregated districts in 1972 than any region of the country. As shown in Figure 1 (and Appendix Figure B1a), following court desegregation orders, there is a sharp decline in the school district racial dissimilarity index, which ranges from zero to one, and represents the proportion of black students who would need to be reassigned to a different school for perfect integration to be achieved given the district's overall racial composition. There is no evidence of pre-existing segregation trends in the school districts prior to the court orders. Such a trend, had it existed, would have raised concern about the validity of the approach. Within three years after court order, the dissimilarity index

dropped by roughly 0.2 which is a substantial and rapid decrease given the average black-white

dissimilarity index in 1968 among school districts that had not yet implemented a desegregation plan was

0.83. The change in the dissimilarity index 4 years after the court order is equal to 36 percent of the

average index in 1970 and to a full standard deviation change in the level of school segregation (based on

the 1970 cross-sectional standard deviation of the index). Similarly, as shown in Figure B1b, we also

witness a significant increase in the black-white exposure index among students (an alternative measure

of school segregation).[12]

Desegregation involved not only reassignments of student to schools, but also a merging of

teachers and staff in the district, so that there would no longer be identifiably all-black and all-white

schools within the district. As shown in Figure 2 and Appendix Figures B2a-B2b, we see a parallel

pattern of sharp declines in racial school segregation among teachers (for both the dissimilarity index and

the black student-to-white teacher exposure index) emerge after desegregation court orders were enacted

(not documented in prior studies).

Increased Per-Pupil Spending. Figure 3 depicts how school-age per-pupil spending evolved for

cohorts that were expected to graduate seven years prior to the initial desegregation court order through

those that were expected to graduate 17 years post reform. The series of event-study estimates is relative

to the effect for event study year 0 (those that turned 17 years old in the initial year of court-ordered

desegregation in their school district). Because the outcome is in logs, the values represent percent

changes in average school-age spending relative to the cohort from the same district that was 17 the initial

year of court-ordered desegregation. As one can see, unexposed cohorts -7 through -1 (turned ages 18

through 24 the year of the first court-order) experienced no apparent significant changes in school-age

per-pupil spending in the years immediately leading up to the initial court. The *p*-value for the joint

hypothesis that all these pre-reform event-study years is equal to zero is above 0.1. This lends credibility

to the exogeneity of the timing of court orders. Consistent with court-ordered desegregation reducing

racial inequality in spending, exposed black cohorts, on average, see large spending increases that

increase with years of exposure, while whites, on average, did not experience any significant spending

changes. The results indicate that among blacks with 12 years of exposure (age 5 during the year of the initial court order), average school-age per-pupil spending was 22.5 percent greater (or about $1,300 more (in 2000 dollars)) than that experienced among unexposed black cohorts from those districts who were 17 or older when the court rulings went into effect. Appendix Figures B3a-B3d show court-ordered desegregation effects on school district per-pupil spending, separately by revenue source (local; state; federal). Importantly, the large increase in school district per-pupil spending is driven solely by the infusion of state funds following the timing of court-ordered school desegregation (Figure B3c). I do not find a similar pattern in districts that were not under court order, nor do I find significant spending changes in districts with a small minority proportional enrollment following court orders (Figure B3d). I find insignificant and negligible effects on per-pupil spending from local or federal sources.

Recall that before school desegregation plans were enacted, school district spending, particularly in the South, was directed disproportionately to the majority-white schools within districts, which will not be reflected in the district-level spending data. A political economy explanation for these results is that state legislatures were under pressure to ensure that the level of school resources available to whites would not be negatively affected by integration. The larger the proportion of the school district's students who were non-white, the larger was the share of school resources that may need to be redistributed toward minority students following school desegregation in the absence of an increase in state funding. As a result, states infused greater funds into districts undergoing desegregation to ensure the level that black students received could be leveled-up to what whites were previously receiving (i.e., without affecting prevailing resource levels for white students).

Reductions in Class Size. Figure 4 and Figure B4 provide supportive evidence of reduced average class size for blacks following desegregation court orders.[13] With the use of school-level data, the results for class size do not exhibit any pre-existing time trend but fall sharply following court orders, with reductions in class size for blacks of about 3 to 4 students two years later (Figure B4). The results indicate no significant effects on the average class size among white students, while significant reductions were experienced in class size for the average black student (using district-level data, Figure 4 shows average

school-age student-to-teacher ratios decline by one for fully exposed black cohorts relative to unexposed cohorts). The sharp trend break in school resource inputs (per-pupil spending, class size, school segregation) immediately following court orders strongly suggests the estimates reflect the causal impact of desegregation.

Vb. EFFECTS ON ADULT OUTCOMES

Educational Attainment. Figure 5 presents non-parametric event study model results for blacks and whites on the same graph for the effects of court-ordered school desegregation on years of completed schooling. Black cohorts with more school-age years of desegregation exposure have higher completed years of education than unexposed cohorts and cohorts with fewer years of exposure. For black children, exposure to court-ordered desegregation in all 12 school-age years increases educational attainment by roughly one year (p-value<.01). Even though each event-study year is estimated with noise, among black cohorts with more than 5 years of exposure (i.e., those age 12 or younger at the time of the initial court order) the 90 percent confidence interval for all individual event-study years lies above zero. Note that testing the difference between individual years of exposure is low powered, and is not a test of the broader hypothesis that court-ordered desegregation has a causal impact on adult outcomes. To test this broader hypothesis, I find for blacks the post-desegregation event-study years are jointly significant at less than the 1% level. Furthermore, a test of equality of the post-desegregation event-study year indicators across the two racial groups yields a *p*-value below 0.01. Each additional year of exposure to court-ordered desegregation leads to a 0.1 increase in years of education for blacks. To put these estimates in perspective, the gap in completed years of education between black and white children is one full year. Thus, the estimated effect of desegregation exposure throughout all 12 school-age years for black children is large enough to eliminate the black-white educational attainment gap.

To examine the margin of educational attainment affected, I find similar event study results on the probability of graduating from high school with large, statistically significant effects for blacks (Figure 6). The average high school graduation rates for blacks and whites for these birth cohorts is 0.73 and 0.88, respectively. The results indicate that, for blacks, there is an immediate jump in the likelihood

of graduating from high school with exposure to court-ordered desegregation, and each additional year of exposure leads to a 1.8 percentage-point increase in the likelihood of high school graduation with an additional jump for those exposed throughout their school-age years. The mean and standard deviation change in exposure to court-ordered desegregation for the sample is roughly 5 years; thus, a 5-year increase in exposure translates into a 14.5 percentage point increase in the likelihood of graduating from high school and roughly a 0.6 increase in years of education for blacks. The desegregation effect sizes for blacks are comparable to the influence of having college-educated parents.

The pre-desegregation coefficients permit a partial test of the identifying assumption that, in the absence of court-ordered desegregation, educational attainment would have trended similarly in districts which had initial desegregation court orders enacted at different times. Credibility of the research design is supported by the fact that there is very little evidence of pre-existing trends in either high school graduation or completed education before desegregation orders are enacted (the *p*-value for the joint hypothesis that all the pre-desegregation event-study years is equal to zero is above 0.1 for both blacks and whites); but after enactment, we see a structural break in the trend for blacks. As aforementioned, the significant effects observed for blacks beyond one's school-age years may reflect that desegregation efforts may have become more effective with time and/or that black children who were pre-school ages at the time of initial court orders benefited from the experiences of the first cohort of black children who were pioneers of integration.

In stark contrast, for whites there are consistently no significant effects on either the likelihood of high school graduation nor years of completed education, and the point estimates are negligible. The small, insignificant effects for whites provide further evidence to rule out the competing hypothesis that the black improvements in educational attainment were driven by secular trends in desegregated districts.[14]

College Quality. Equally important impacts of court-ordered desegregation may extend beyond blacks' improvements in the quantity of years of completed education to the quality of education received (in both absolute and relative terms). Accordingly, I next examine desegregation effects on college

quality. A growing body of evidence demonstrates significant labor market returns to college quality (Andrews et al., 2011; Hoekstra, 2009). I use information collected on college name reported by respondents between 1975 and 2013 and match it with the Integrated Post-secondary Education Data System (IPEDS) in order to link respondents with college quality indicators for the college attended. I use the 25th and 75th percentiles of the SAT math and verbal scores of the freshman class to which the individual attended college as markers of college quality.

Figure 7 presents the non-parametric event study model estimates for black and whites on the same graph for the effects of desegregation on these measures of college quality. Across each of the SAT math/verbal 25th and 75th percentile score outcomes, we see parallel patterns that mirror the effects found for years of education and high school graduation. Namely, I find large estimated effects for blacks that increase with school-age years of desegregation exposure, with no pre-existing time trend and negligible effects beyond school-age years. Estimated effects for whites are consistently small with point estimates near zero.[15]

Labor Market Outcomes, *Adult Family Income and Poverty Status*. The next series of results reveal large, significant effects of court-ordered desegregation on blacks' adult economic status and labor market outcomes. Figures 8-11 present desegregation effects by race on adult economic outcomes (ages 20-50), including wages, occupational attainment, annual family income, and the annual incidence of poverty. In light of the parallel set of findings across all these long-run economic outcomes, the results are discussed in succession.

Adult outcomes for blacks generally improved monotonically with the number of school-age years of exposure to desegregation. The results indicate that, for blacks, court-ordered desegregation significantly increased adult wages and annual earnings, as there is an immediate jump in wages and earnings with exposure to court-ordered desegregation, and each additional year of exposure leads to a 2 percent increase in wages with an additional jump for those exposed throughout their school-age years (Figures 8 & 9). Among black cohorts with more than 5 years of exposure the 90 percent confidence interval for all individual event-study years lies above zero. I find for blacks the post-desegregation event-

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study years are jointly significant at less than the 1% level. Furthermore, a test of equality of the post-desegregation event-study year indicators across the two racial groups yields a *p*-value below 0.01. These effects for blacks represent substantial improvements in adult labor market outcomes, as the average effects of a 5-year exposure to court-ordered school desegregation lead to about a 15 percent increase in wages and an increase in annual work hours of roughly 165, which combined to result in a 30 percent increase in annual earnings. Furthermore, among blacks, desegregation exposure led to significant improvements in occupational attainment, as reflected in the 5.2 point increase in the occupational prestige index associated with a 5-year increase in exposure (Figure 12). The average occupational prestige index for blacks and whites prior to desegregation was 30 and 60, respectively.

I find this translated into substantial gains in adult family economic status among blacks. The effects on family income reflect (a) increases in own income, (b) increases in other income due to increases in the likelihood of being married (i.e., there are more potential earners), and (c) increase in the income of one's family members (which is likely if persons marry individuals who were also affected by desegregation). As shown in Figures 10 and 11, for blacks a similar pattern emerges of an immediate jump in family income and corresponding decline in the likelihood of adult poverty with exposure to court-ordered desegregation; each additional year of exposure leads to a roughly $1,000 increase in family income with an additional jump for those exposed throughout their school-age years (Figure 10); and in similar fashion, each additional year of exposure leads to a 1.3 percentage-point reduction in the annual incidence of poverty with an additional decline in poverty risk for those exposed throughout their school-age years (Figure 11). The average effects of a 5-year exposure to court-ordered school desegregation lead to an 11 percentage-point decline in the annual incidence of poverty in adulthood and about a 25 percent increase in annual family income. The estimated magnitudes of desegregation impacts are on par with the coefficients on parental education.[16]

It is equally noteworthy that there is no evidence of pre-existing time trends for any of these outcomes leading up to the year in which court-orders are enacted (the *p*-value for the joint hypothesis that all the pre-desegregation event-study years is equal to zero is above 0.1 for both blacks and whites);

whereas the post-desegregation event-study years for blacks are jointly significant at less than the 1%

level. Furthermore, a test of equality of the post-desegregation event-study year indicators across the two

racial groups yields a *p*-value below 0.01. Equally striking as the substantial magnitudes of the effects on

blacks, is the consistent absence of any significant impacts on whites across all of these outcomes. These

important specification tests affirm the credibility of the research design and rule out several competing

explanations for the pattern of results.

Probability of Incarceration. The substantial racial disparities in incarceration, most pronounced

among high school dropouts, have been well-documented (see e.g., Raphael (2005); Western (2007)).

Increased investments in school quality may reduce the frequency of negative social outcomes such as

crime (see, e.g., evidence from the Perry Pre-School Project (Schweinhart et al., 2005)). The next series

of results reveal large, significant effects of court-ordered desegregation on blacks' annual incidence of

incarceration and probability of ever being incarcerated in adulthood. The proportion of blacks (whites)

ever incarcerated is 0.08 (0.04) for this sample of birth cohorts.

Among blacks, Figures 13a and 13b reveal a substantial discontinuous drop in both the likelihood

of ever being incarcerated and the annual incidence of incarceration with exposure to court-ordered

desegregation, respectively. The results also highlight the larger reduction in the likelihood of

incarceration among blacks exposed to integrated schools throughout their childhood years (vs those with

more limited exposure). For blacks the results indicate that, relative to growing up in segregated schools

throughout one's school years, exposure to desegregation beginning in one's elementary school years

leads to a 3 percentage-point reduction in the annual incidence of incarceration (Figure 13b) and a 22

percentage-point decline in the probability of adult incarceration (Figure 13a). The results do not indicate

any pre-existing trends in these outcomes prior to court-ordered desegregation. These differences are

somewhat less dramatic when comparisons are made for smaller increments of desegregation exposure

(e.g., about a 10 and 15 percentage-point reduction in the probability of adult incarceration if the court

order first occurred during high school and middle school, respectively, relative to no exposure).

Furthermore, the incarceration effects explain a significant amount of the work hours' effects of

desegregation for blacks. Importantly, I find no desegregation effects on the probability of incarceration for whites, which follows the pattern of results for educational attainment by race.

Adult Health Status. Education has been shown to be a very strong correlate of health status in cross-sectional work and across generations. Scholars have long hypothesized that education has a causal effect on subsequent health, though the precise ways education influences adult health have not been well established (Cutler and Lleras-Muney, 2006). Large gaps in morbidity and mortality between more- and less-educated individuals have been well documented. Furthermore, gaps in health between blacks and whites are large and appear to widen over the life cycle, suggestive of an important role of childhood conditions.

The next series of results reveal large, significant improvements in blacks' adult health status resulting from exposure to court-ordered school desegregation. The main health outcome analyzed is self-assessed general health status (GHS). To scale the GHS categories, I use the health utility-based scale that was developed in the construction of the Health and Activity Limitation index (HALex) (details in Appendix C). The results are based on interval regression models using a 100-point scale where 100 equals perfect health—the interval health values associated with GHS used are: [95, 100] for excellent, [85, 95) for very good, [70,85) for good, [30,70) for fair, and [1,30) for poor health. Alternatively, I define problematic health as an indicator of whether the individual self-reported not being in excellent or very good health (ages 20-50). Linear probability models of the annual incidence of problematic health yielded similar patterns reflecting increases in the probability of excellent/very good health.[17]

The general health status (GHS) index in adulthood is 6.5 points lower for blacks, on average, but I find substantial birth cohort differences in the magnitude of black-white health disparities in adulthood (evaluated at the same ages) (Johnson, 2009). In particular, while the age-adjusted average black-white difference in adult health status for cohorts born in the early 1950s is 9.3 points, this difference is reduced to 4.7 and 3.3 points, among cohorts born between 1955-1963 and 1964-1968, respectively. These cohort differences are completely driven by health improvements experienced by African Americans over this period; I do not find any significant birth cohort differences for whites.

The non-parametric event study results (Figures 14a-14b), based on both the interval regression model and linear probability model of the annual incidence of problematic health, indicate that, for blacks, adult health status improves monotonically with duration of exposure to court-ordered desegregation. The average effect of a 5-year exposure to court-ordered school desegregation yields a 11 percentage-point increase in the annual incidence of being in excellent/very good health. There is no evidence of pre-existing time trends in adult health in the years leading up to the court order (the *p*-value for the joint hypothesis that all the pre-desegregation event-study years is equal to zero is above 0.1 for both blacks and whites); whereas the post-desegregation event-study years for blacks are jointly significant at the 0.05 level. The magnitudes and precision of the event study estimates increase when focusing on ages 35-50 (Figure 14b), likely due to the fact that most health problems do not manifest in one's 20s.

A useful way to interpret the estimate is in relationship to the size of the effect of age on health, with the impact of each additional year of desegregation exposure for blacks equivalent (on average) to blacks reaching a level of health deterioration about 1 year later than if that year were spent in segregated schools. For example, GHS is roughly 3 points higher for black adults who experienced 5 years of exposure to court-ordered school desegregation (relative to blacks who did not), which is equal to roughly 7 years evaluated at an effect of age during one's mid-30s and 40s of -0.41. This magnitude is also comparable to the impacts of parental education. Following the pattern of results for the education and adult socioeconomic attainment outcomes, the desegregation effects on the adult health status of whites are statistically insignificant.

Addressing Endogenous Residential Mobility: One potential parental response to the presence of city differences in the timing and scope of school desegregation is to move to a different city (Baum-Snow & Lutz, 2011). Because I did not want to include endogenous residential moves, this analysis does not incorporate information of family moves across school districts during the child's school-age years. Instead, I identified the school district of upbringing based on the earliest childhood address (in most cases, 1968).[18] One may still worry that the results are biased by endogenous residential mobility. To

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address potential bias, I re-estimated all models limiting the analysis sample to those who lived at their (earliest) childhood residence prior to the enactment of initial court orders in their respective district. The results are presented in Appendix D. I find nearly identical results as those in the full sample. This indicates that endogenous residential mobility is not an important source of bias in the analysis.

Using Sibling Differences to Estimate Desegregation Effects. The sibling fixed effect approach enables one to control for time-invariant aspects of all family and neighborhood background shared by siblings. The effect of school desegregation and school quality is identified by capitalizing on the fact that siblings of different ages may have matriculated through different school systems, as there were rapid changes during that time.[19] Within sibling pairs that attended schools with different resources, the younger sibling experienced integrated schools for a longer period of childhood and typically had access to greater school resources as reflected in greater per-pupil spending and smaller class sizes during school-age years. The sibling comparisons evaluate adult outcomes at the same age and control for birth order, year of birth, birth weight, and whether mother was married at birth. The sibling difference approach complements the primary event study difference-in-difference strategy. I restrict the sample to siblings who grew up in the same city to eliminate endogenous migration as a potential source of bias.

Table 1 presents sibling fixed effect models designed to assess the long-run effects of school desegregation on education, socioeconomic attainment, and adult health status. I find that black children who were exposed to court-ordered school desegregation for the majority of their school-age years experienced significantly improved education, economic, and health outcomes in adulthood, compared with their older siblings who grew up in segregated school environments with weaker school resources (controlling for age and birth cohort effects). Negligible effects are found for whites. I find that education, economic and health outcomes among blacks were particularly affected by changes in access to school resources associated with desegregation, not simply changes in exposure to white students.[20] I find little evidence that observable differences among siblings are related to differences in the quality of high schools they attend. There is no evidence that the results are biased by a positive correlation between sibling differences in school inputs and sibling differences in other factors that are favorable to adult

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attainments.

Robustness & Falsification Tests. Table E1 probes the robustness of these estimates further. As a falsification exercise, I re-estimated equation (1) replacing the timing of initial court ordered desegregation variables with litigation cases that were not successful and the corresponding year of their court ruling to identify effects; in essence estimating the effects of a series of "placebo" initiatives. If my baseline estimates capture the effects of school desegregation – not an earlier or later unobserved shock or intervention – the largest estimates of desegregation effects should arise from estimation of the model as originally specified. Indeed, this is the case (Table E1). In particular, a placebo treatment variable is included in the model which captures the years of childhood exposure to unsuccessful court litigation. The coefficient on the placebo variable should be small and insignificant. Indeed, when I used the placebo and the corresponding year of their court ruling to identify effects, they are not associated with any measurable impact on any outcome of interest. These results demonstrate that timing of *unsuccessful* court litigation is unrelated to adult attainment outcomes; only the timing of initial year of successful litigation that led to court-ordered school desegregation is significantly associated with blacks' adult socioeconomic & health attainments. This provides additional evidence that the main results are not spurious, and helps rule out confounding influences from changing local demographic characteristics or social policies. If such omitted variables spuriously inflate the estimated effect of desegregation, the placebo coefficient should be significant. It is not.

These falsification tests provide additional evidence that unobserved factors do not contaminate the estimates. The results are robust to many other sensitivity tests including adding more fixed effects, examining subgroups of the sample, and placebo tests on groups not likely to be affected (e.g., contemporaneous black adult employment rates (in occupations outside of K-12 education), providing further evidence of the exogeneity of the treatment. The results, as expected, show no significant impact of desegregation exposure for any of these groups—the point estimates are small, mostly statistically insignificant, and negative compared to the consistently positive and significant estimates for blacks.

The evidence collectively is not consistent with alternative omitted-variables counter-explanations of the results (i.e., other factors that happened to change at the same time these desegregation orders were enacted). Based on the robustness of the results, such an alternative explanation must be a cause that meets the following very strict criteria: a) it closely follows the timing of initial court orders (given the evidence showing no pre-existing time trends); b) yet it be geographically confined to the specific school districts in which desegregation court orders were being enacted (given the robustness of the results to the inclusion of cohort-by-race-by-region of birth fixed effects); c) its impacts are constrained only to school-age years of exposure (given the evidence showing no effects for non-school age years beyond age 17); d) had the largest impacts on blacks in communities where desegregation resulted in the largest changes in school quality inputs (Tables F1-F2); and finally e) had no effects on whites. The results support a causal interpretation of the effects of school desegregation by uncovering sharp differences in the estimated long-run effects on cohorts born within a fairly narrow window of each other that differ in whether and how long they attended desegregated schools.

Exploring the Potential Mechanisms. The analysis cannot cleanly identify the precise mechanisms through which school desegregation influenced long-run adult outcomes, but two potential pathways that merit careful consideration is through impacts of peer effects and school quality improvements (i.e., greater school resources for blacks in integrated schools) on the socioeconomic mobility process. In order to assess the relative roles of school resources and peer effects as potential mechanisms underlying the desegregation effects, I estimate 2SLS models in which the key explanatory variables of interest—log of average per-pupil spending experienced during one's school-age years and the average level of racial school integration (i.e., the average black-white exposure index during ages 5-17)—are predicted in a first-stage model using only the individual's duration of desegregation exposure (fully non-parametric specification for school-age exposure years), with the same full set of controls as in equation (1). Identification is based on the timing of court-ordered desegregation and the strong first-stage results of desegregation-induced changes in average school-age per-pupil spending and school segregation, respectively, were presented in Figures 1, 2b and B1b.[21] The 2SLS models are presented in

Tables 2-4 for the main adult attainment outcomes, and include the same set of controls as the prior models, estimated separately by race. These estimates are not intended to be interpreted as the causal impacts of school spending per se, but rather as markers of the treatment dosage that may capture the combined effects of improvements in school resources and teacher quality. I present three sets of 2SLS estimates: (1) include only instrumented average black-white exposure index (without instrumented spending); (2) include only instrumented average school spending (without instrumented racial segregation); (3) include both instrumented average black-white exposure index and instrumented average school spending in the same model.[22] The results highlight the importance of examining effects of both changes in access to school quality, as proxied by changes in per-pupil spending, and changes in peer exposure as measured by changes in racial school segregation.

The 2SLS estimates shown in Table 2 indicate significant positive effects of desegregation-induced increases in school spending on blacks' educational attainment and adult wages, which are an order of magnitude larger than the corresponding naïve OLS estimates. The beneficial effects of desegregation-induced increases in school spending for blacks are particularly pronounced when simultaneously accounting for changes in racial school segregation (columns (3), column (6)). In contrast, these 2SLS models reveal small, insignificant effects for increases in racial integration for both blacks and whites (both in models without spending (column (1)) and in those holding spending changes constant (column (3)). I also find desegregation-induced increases in school spending for blacks are associated with significant reductions in the annual incidences of poverty and problematic health in adulthood (Table 3), and reduced both the likelihood and incidence of adult incarceration (Table 4). These significant spending effects for blacks persist after the inclusion of corresponding increases in the black-white exposure index that accompanied desegregation, and, if anything, appear stronger once changes in school segregation are accounted for. The results for blacks indicate that a 10 percent increase in school spending experienced throughout one's school-age years is associated with 0.49 additional years of completed education, a 21 percent increase in wages, a 7.1 percentage-point reduction in the annual incidence of

adult poverty, a 6.9 percentage-point reduction in the annual incidence of problematic health in adulthood, and an 11.9 percentage-point reduction in the likelihood of ever being incarcerated.

On the other hand, there is suggestive evidence that reductions in school segregation levels that were not accompanied by significant changes in school resources did not have equally large impacts on blacks' adult attainments. In general, the magnitudes of the desegregation impacts across the various adult outcomes for blacks were insensitive to how much reduction in racial school segregation resulted from court orders. In particular, for blacks I find no significant effects of increases in racial integration on adult health and economic status. Furthermore, for blacks the only significant effects of increased racial integration uncovered are for the likelihood of incarceration, but they run in the opposite direction once school spending changes are accounted for (i.e., increases in integration appear associated with increased likelihood of incarceration for blacks, holding spending changes constant). For whites, there are no corresponding significant effects of either of these markers of treatment dosage on educational attainment and likelihood of incarceration. There is some pattern of evidence that desegregation-induced increases in school spending were protective for whites in increasing wages and reducing the annual incidences of poverty and problematic health in adulthood, but these results are less consistent and contingent upon the inclusion of changes in racial school segregation.

The amount of desegregation achieved by the courts varied from district to district, as did the resultant change in access to school quality inputs received by minority children. This was in part because desegregation was achieved in a variety of ways across school districts and was applied in many different initial school environments based on the form of racial segregation—*de jure* in the South and *de facto* in other regions of the country. To further explore potential mechanisms, in additional analyses presented in Appendix F, I isolate for every district the desegregation-induced change in per-pupil spending and racial school integration, respectively, which are net of time-invariant school district characteristics, district-specific trends and a host of other coincident policy changes (see Figure F1). I augment the primary model specifications for adult outcomes to investigate whether impacts appear to differ by the scope of desegregation (as proxied by the estimated desegregation-induced change in per-pupil spending (school

segregation)). For each district, I compute the change in school district per-pupil spending (school segregation) induced by the court order from the year preceding enactment to the first several years following it. I then exploit variation in the scope of desegregation court orders in addition to quasi-random variation in the timing to assess whether there is evidence of a dose-response effect of school quality improvements on subsequent education, economic, and health attainment outcomes among blacks. This can be viewed as a triple-difference strategy that compares the difference in outcomes between treated and untreated cohorts within districts (variation in exposure) and across districts with larger or smaller changes in school spending due to desegregation (variation in intensity). The full details of the estimation methods and results are presented in Appendix F. Importantly, I find no evidence that districts that underwent larger changes in school spending resultant from desegregation exhibited differential trends in outcomes preceding the enactment of court orders, which provides additional support for the identification strategy.

The results of these models once again suggest that changes in school quality resulting from integration played an important role in improving blacks' educational, economic and health attainments. The results indicate significant interactive effects of school desegregation exposure with the resultant change in access to school quality, as proxied by changes in per-pupil spending. I find that court-ordered desegregation that led to larger improvements in school quality resulted in more beneficial educational, economic, and health outcomes in adulthood for blacks who grew up in those court-ordered desegregation districts. Interestingly, in these additional models, I find no effects on whites in either the duration of desegregation exposure nor the resultant change in school resources, which is precisely the pattern one might expect if the state infusement of school funding that accompanied desegregation (as reported in Figure B3c for districts with significant black enrollment) was used to intentionally direct school resources to minority children and level up resources for them to the level whites were receiving prior to desegregation. Moreover, the fact that I find no school spending effects for poor whites, coupled with the fact that Title I federal spending is already explicitly controlled for in these models, provides further support that the school spending effects are capturing desegregation-induced impacts, not Title I funding.

As an additional placebo falsification test using the 2SLS models, it is shown in Appendix Table F3 that school spending increases have no significant impacts on blacks' adult outcomes when they occur during non-school ages after individuals should have left school (between the ages of 20 and 24), but rather all the estimated long-run effects of per-pupil spending are confined to school-age years of exposure, as we would expect.[23] The preponderance of results indicates small, insignificant effects for increases in racial integration (holding spending changes constant).

The event study, difference-in-difference, 2SLS, and sibling-difference estimates indicate that school desegregation and accompanied increases in school quality resulted in significant improvements in adult socioeconomic and health outcomes for African-Americans. The pattern of results is remarkably similar across all of the empirical approaches. It is particularly noteworthy that that the estimated effects of desegregation court orders on adult attainments are similar for the subset of black children who grew up in the South and those who grew up in other regions (e.g., see Appendix Table G3). Finally, it is noteworthy that other concurrent policy changes were explicitly controlled for (including hospital desegregation in the South, the roll-out and/or expansions of AFDC, Medicaid, Food Stamps, Community Health Centers, Title I funding, Head Start, and kindergarten introduction), and do not account for the pattern of results presented here.

Contextualizing the magnitudes with previous studies. The study most directly related to the approach taken in this paper is Guryan (2004), who uses variation in the timing of major desegregation plan implementation in the 1970s and 1980s to identify the effects of school segregation on black high school dropout rates for 125 large school districts (Welch/Light data and 1970-80 censuses). He applies a difference-in-difference (DiD) strategy and finds that desegregation led to 3 percentage-point decline in the black high school dropout rate during the 1970s. Guryan (2004) reports IV estimates that are two to four times larger in magnitude than his main DiD estimates. This pattern is consistent with the findings of this study.

In Appendix Table G1, I replicate the main results of Guryan (2004) using similar model specifications for the likelihood of high school graduation as he employed, but using my PSID data

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among the subsample that grew up in the districts that overlap the Welch/Light data (representing 75 different counties; column 2). Column (3) of Appendix Table G1 appends his basic specification with a parametric event study model, which reveal a pre-existing negative time trend in the likelihood of high school graduation for blacks in the years leading up to major desegregation plan implementation; this result puts into question the exogeneity of major plan implementation timing due to endogenous delays following initial court orders. In most other respects, I am able to replicate the main results of Guryan using the PSID. Namely, when one uses the arguably endogenous timing of major plan implementation for identification, 1) for blacks, it is shown that there does not appear to be a dose-response with duration of desegregation exposure (column 4); 2) the estimated effect of any desegregation exposure increases the likelihood of high school graduation by roughly 4 percentage points for blacks (not significantly different from Guryan's estimate with his specification); 3) no significant effects for whites. In stark contrast, when one uses the preferred parametric event study specification on this same PSID subsample that overlaps Welch/Light districts, but instead using the timing of initial court order for identification, I find large impacts for blacks similar in magnitude as those reported in the main results presented in this paper (columns 5-6).

One explanation for the larger estimated effects in this paper than ones based directly on models of the effects of desegregation plans is that the timing of initial court orders is more plausibly exogenous than the year of first implementation of major desegregation plans, due to endogenous delays in effective implementation.[24] There were longer delays in implementation of major desegregation plans following initial court orders for districts that had significant minority proportion, larger per-capita school spending, teacher salary, smaller average student-to-teacher ratios, and/or greater income (Table B1). These factors likely lead OLS estimates of the effects of desegregation plans to be understated.

I also find a similar pattern of results for the effects of court-ordered school desegregation on district-level high school dropout rates using the Office of Civil Rights (OCR) Data and Common Core Data (CCD)—Local Education Agency Universe Survey and Non-Fiscal Survey Database—for all school districts in the US for available years 1972-1999 with the preferred research design, as reported in

Appendix Table G2. The similar pattern of the PSID and OCR-CCD results serves to further demonstrate that the findings are generalizable and representative for these birth cohorts, and allay concerns that the results are specific to the PSID.

A large body of literature examines the effects of school spending on academic performance and educational attainment (Hanushek, 1997; Hedges, Greenwald, and Laine, 1994). While evidence is mixed on the extent to which school resources matter, the results of this paper are in line with Card and Krueger (1992) and other recent studies that use randomized and quasi-random variation in school inputs (e.g., Jackson, Johnson, Persico, 2015; Chetty et al, 2013; Fredrikkson et al, 2012). Jackson, Johnson, Persico (2015), using evidence from court-ordered school finance reforms, find that, for children from low-income families, a 10 percent increase in per-pupil spending throughout one's K-12 years leads to 0.46 additional years of completed education, 9.6 percent higher earnings, and a 6.1 percentage-point reduction in the annual incidence of adult poverty. An important limitation of most recent studies that find insignificant results focusing on the effects of school quality on labor market outcomes using longitudinal individual-level data is that earnings are observed at young ages (averaging around 23 years old). A strength of the analyses contained in this paper, in addition to its credible research design, is both the extensive set of controls for childhood family and neighborhood characteristics and the ability to follow adult attainment outcomes into one's peak earnings years through age 50.

Experimental evidence from the Tennessee Project Star class size intervention demonstrates that black students benefited about twice as much as whites from being assigned to a small class. Krueger and Whitmore (2002) find that this result is mostly driven by a larger treatment effect for all students regardless of race in predominantly black schools, suggesting that benefits from additional resources are higher in such schools; and may lead to better adult socioeconomic attainments (Chetty et al., 2011).

The findings of the present study show that labor market outcomes, and adult income and health status rose in line with blacks' educational improvements (in quantity and quality in both absolute and relative terms), as did declines in the incidence of incarceration. Table G4 presents a summary of the implied Wald estimates of the returns to education (reflecting a combination of both increased quantity

33

and quality) across the adult outcomes. A Wald estimate of the returns to education on wages is the ratio of the estimates of the desegregation effects of 5 years of exposure on wages and completed years of education, yielding a return of 31 percent (0.15/0.48). These estimates are notably larger than the 8 to 14 percent returns typically estimated using modern era schooling interventions and data sources from more recent (younger) birth cohorts (e.g., Card, 1999), but these do not typically account for improvements in the quality of education. If a Wald estimate is constructed based on effects on the incidence of adult poverty, probability of incarceration, and adult health status, the implied returns to education are even larger. The incarceration effects of desegregation are consistent with Lochner and Moretti (2004), who report that a 10 percentage-point increase in high school graduation rates would reduce overall violent crime arrest rates for blacks by 25 percent and reduce murder arrests by two-thirds.

There are several plausible explanations for the much larger estimates obtained in these analyses. First, improved school environments could have facilitated a higher quality teacher workforce (Jackson, 2009) and thus boosted the return to a year of school. A second possibility is that the returns to schooling for those who were most impacted by school desegregation were just extremely large. Third, the marginal returns to education for the groups affected by school desegregation may be larger than the average return. Card (1999) shows that heterogeneous rates of return to education may arise due to differing costs of education, preferences, or marginal returns to the production function relating schooling to earnings. Card suggests that one possible explanation for the tendency for many IV estimates of the returns to schooling to exceed OLS estimates is that in the presence of heterogeneous returns, the marginal returns to education for the groups affected by the instrument may be larger than the average return.[25]

VI. SUMMARY DISCUSSION AND CONCLUSION

Differences across districts in when desegregation court cases were first filed and the length of time it took these cases to proceed through the judicial system are used as a plausibly exogenous source of identifying variation to analyze the long-run impacts of school desegregation. The exogeneity of the timing of initial court orders is supported theoretically by the documented legal history of school

desegregation and by my own empirical examination of the issue. The analysis capitalizes on this source of identifying variation.

I control for possible confounders in a number of ways. First, I estimate event study models that support the validity of the research design. Second, I examine the determinants of the timing of the occurrence of the initial court order and major desegregation plan adoption, and find that collectively the pre-treatment school quality, SES, demographic, and labor market related characteristics do not significantly predict the year of the initial court order. Third, I perform a variety of robustness checks to test the validity of the identifying assumptions.

The findings of this study contribute to the literature in several important ways. First, this study is the most comprehensive to date on the topic, especially in terms of the range of empirical approaches utilized, broad set of outcomes analyzed, and the long time horizon considered. Second, this paper provides important, new estimates of the impact of court-ordered school desegregation. I use an event-study framework and exploit the wide quasi-random variation in the timing and scope of court-ordered desegregation during the 1960s, 70s and 80s to identify these effects. I find that school desegregation significantly increased educational attainment among blacks exposed to desegregation during their school-age years, with impacts found on the likelihood of graduating from high school, completed years of schooling, attending college, graduating with a 4-year college degree, and college quality. Non-parametric event-study estimates and sibling-difference estimates indicate that school desegregation and the accompanied increases in school quality also resulted in significant improvements in adult labor market and health status outcomes, and reductions in both the annual incidence of adult poverty and incarceration for blacks. The significant long-run impacts of school desegregation found for blacks with parallel findings across a broad set of socioeconomic outcomes and health status indicators of well-being, with no corresponding impacts found for whites, is striking.

The results suggest that the mechanisms through which school desegregation led to beneficial socioeconomic outcomes in adulthood for blacks include improvement in access to school resources, which is reflected in reductions in class size and increases in per-pupil spending. Furthermore, the

evidence is consistent with a dose-response effect of school quality improvements and the duration of exposure to them on subsequent attainments in adulthood. The magnitude of the estimated effects of dimensions of school quality are larger than estimates reported in previous research and, taken together, are larger than the impact of increasing parents' income by a comparable amount.

Finally, the present data and methods improve upon prior research, which lacked access to panel data that follow children from birth to adulthood, relied on aggregate state-level analyses, and/or failed to address the endogeneity of residential location. This paper is among the first to assess and provide evidence on the extent and ways in which childhood school quality factors causally influence later-life health outcomes. The evidence collectively paints a consistent picture of significant later-life health returns of school quality. The results highlight the significant impacts of educational attainment on future health status, and point to the importance of school quality in influencing socioeconomic mobility prospects, which in turn have far-reaching impacts on health. The results demonstrate that racial convergence in school quality and educational attainment following court-ordered school desegregation played a significant role in accounting for the reduction in the black-white adult health gap. While no single explanation likely accounts for this rapid convergence, this work shows that school desegregation was a primary contributor, explaining a sizable share of the narrowing of the racial education, and economic and health status gaps among the cohorts examined. Small, statistically insignificant results across each of these adult outcomes for whites suggest that benefits for minority children do not come at the expense of white students.

A limitation of the court-ordered desegregation results is their reduced-form nature. I cannot separately identify the pathways through which desegregation impacts subsequent adult attainments. It may not be the school desegregation so much as the nature and type of school desegregation implementation (e.g., how much it changed access to school resources for minority children) that matter most for long-run economic well-being and thereby adult health. Future research should further uncover the precise structure of the underlying causal linkages between school desegregation and subsequent

attainment. Separately identifying and disentangling the mechanisms underlying the overall causal impact of desegregation is very difficult with available data and is left for future work.

This study illustrates the gains in human capital acquisition among blacks that occurred due to greater accessibility of dimensions of school quality. The findings highlight the large productivity gains that can arise when substantial improvement to school inputs are introduced to equalize differences in access to school quality. *Brown* offered the hope and promise of better educational opportunities for minority children in the US, and was intended not only to promote equitable access to school quality but also to alter the attitudes and socialization of children -- beginning at the youngest ages. A motivation of this study was to attempt to quantify the extent to which progress was made in fulfillment of policy expectations and to evaluate the enduring impact of what is arguably the most important subcomponent of legal actions during the Civil Rights era. This work contributes to a growing literature that evaluates the longer-run effects of the Civil Rights Act, Great Society, and War on Poverty policy initiatives.[26] The present research is the first to contribute estimates of the effects of school desegregation (and school quality) on adult economic and health outcomes using a plausibly exogenous source of identifying variation. This study highlights the importance of analyses on the returns to education policies beyond labor market outcomes. The findings of this paper strongly suggest that estimates of the returns to education that focus on increases in wages substantially understate the total returns. Given the scarcity of large-scale educational experiments that had such dramatic changes in access to school quality, it is important to learn as much as possible about the long-run consequences of one of the great social experiments of inclusion.

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[1] This desegregation case data was compiled by legal scholars for The American Communities Project at Brown University, and I combine it with additional information from Welch and Light (1987) on the dates of major desegregation plan implementation for large urban districts. See Appendix A for more details.

[2] Integration may also influence long-term outcomes in ways that are unrelated to academic achievement and educational outcomes.

[3] The Census of Governments has been conducted every five years since 1972 and records school spending for every school district in the US. The Historical Database on Individual Government Finances (INDFIN) contains school district finance data annually for a sub-sample of districts from 1967, and 1970 through 1991. After 1991, the CCD School District Finance Survey (F-33) includes data on school spending for every school district in the United States. Additional details on how these databases were compiled and the coverage of districts in these data are in Appendix B.

[4] The PSID began interviewing a national probability sample of families in 1968. These families were re-interviewed each year through 1997, when interviewing became biennial. All persons in PSID families in 1968 have the PSID "gene," which means that they are followed in subsequent waves. When children with the "gene" become adults and leave their parents' homes, they become their own PSID "family unit" and are interviewed in each wave. The original geographic cluster design of the PSID enables comparisons in adulthood of childhood neighbors who have been followed over the life course. Moreover, the genealogical design implies that the PSID sample today includes numerous adult sibling groupings who have been members of PSID-interviewed families for more than four decades. I include both the Survey Research Center component and the Survey of Economic Opportunity component, commonly known as the "poverty sample," of the PSID sample.

[5] The PSID maintains extremely high wave-to-wave response rates of 95-98%. Studies have concluded that the PSID sample of heads and wives remains representative of the national sample of adults (Gottschalk et al, 1999; Becketti et al, 1997).

[6] The data I use include measures from 1968-1988 Office of Civil Rights (OCR) data; 1960, 1970, 1980, and 1990 Census data; 1962-1999 Census of Governments (COG) data; Common Core data (CCD) compiled by the National Center for Education Statistics; Regional Economic Information System (REIS) data; the comprehensive case inventory of court litigation regarding school desegregation over the 1955-1990 period (American Communities Project), and major plan implementation dates in large districts (compiled by Welch/Light); and American Hospital Association's Annual Survey of Hospitals (1946-1990) and the Centers for Medicare Provider of Service data files (dating back to 1960s) to identify the precise date in which a Medicare-certified hospital was established in each county of the US (an accurate marker for hospital desegregation compliance). Many school districts were counties during this period, including more than one-half of Southern school districts.

[7] The average level of district per-pupil spending across all school-age years provides a summary measure of the level of financial resources available in the individual's childhood school district during all their school-going years (ages 5 through 17 corresponding to expected grades K-12). I use the natural log of this average measure to capture the fact that school spending likely exhibits diminishing marginal product (all results are robust to using the level of average school-age spending).

[8] For example, α_{-5}^{r} is the effect of the passage of a court-ordered desegregation on outcomes of *untreated* cohorts that turned age 17 five years prior to reforms for racial group *r*. Also, θ_{5}^{r} is the effect of court-ordered desegregation on the outcomes of *treated* cohorts that turned age 17 five years after the passage of desegregation court orders.

[9] Because some individuals may have moved away from their pre-reform school district or may have dropped out of school before the age of 17, my measure of school-age desegregation exposure is a noisy measure of the amount of desegregation individuals were actually exposed to. Using the actual desegregation an individual is exposed to would introduce selection bias, because school-age years of exposure would be determined in part by the decisions of individual parents. By using the individual's childhood residential location prior to the court order, one removes

any potential bias due to endogenous residential sorting. As shown in Appendix D, the results are nearly identical when the analysis sample is restricted to those who lived at their (earliest) childhood residence prior to the enactment of initial court orders in their respective district.

[10] The statistical significance of all the main results are very similar when I cluster the standard errors at the childhood county level (instead of district level). The estimates are very similar for models that include age fixed effects (instead of age cubic) for adult economic and health status outcomes.

[11] Wherever possible, I present event study figures showing the 10 years leading up to court order and 17 years following court orders; for some outcomes in which data during pre-treatment years is more limited, results are presented for the 5-8 years leading up to court orders (the estimated models are the same throughout).

[12] Levels of racial integration in schools peaked around 1988.

[13] With the use of school-level data, models are weighted by baseline black student enrollment in the school, so that results can be interpreted as the desegregation effect experienced by the average black child. Similarly, the results presented on the same graph for whites is weighted by baseline white student enrollment, so that the results can be interpreted as the desegregation effect experienced by the average white child (N=33,952 schools from 33 different states).

[14] In additional specifications not shown to conserve space, I find similar results using a parsimonious set of controls (results available upon request). The inclusion of the extensive set of childhood family/neighborhood factors, coincident policies and government transfer programs largely do not influence the point estimates of desegregation impacts (but tend to improve their precision). This further supports the exogeneity of the timing of initial court orders, as this array of childhood factors and coincident policies (while independently related to adult attainment outcomes) does not appear systematically related to the timing of initial court orders.

[15] Because of the smaller sample size for the models on college quality, the point estimates are much less precise with such a saturated model. The pattern that emerges is clear, however, and an F-test rejects the null hypothesis and affirms the joint significance of the coefficients on the exposure years for blacks.

[16] The sum of coefficients of mother's and father's education on adult wages is roughly 0.04.

[17] Linear probability models of the annual incidence of problematic health, where problematic health is defined as being in fair or poor health yielded similar patterns, but with less precision due to the relatively younger ages (under 50) to study the onset of major health problems and use of fully non-parametric event study models.

[18] Among original sample children in the PSID, the average proportion of childhood spent growing up in the 1968 neighborhood was roughly two-thirds.

[19] This use of sibling models follows the research design previously utilized by Altonji and Dunn (1996) to analyze the effects of school quality on wages. The sibling approach assumes parents treat their children similarly and do not reallocate resources within the family as a result of school desegregation.

[20] These additional results are suppressed to conserve space; available upon request.

[21] Qualitatively similar patterns of results are found for the effects of desegregation-induced changes in racial school segregation among students whether segregation is measured by black-white dissimilarity index or exposure index.

[22] I also use naïve OLS estimates of effects of school spending and school segregation as a baseline comparison with the 2SLS estimates.
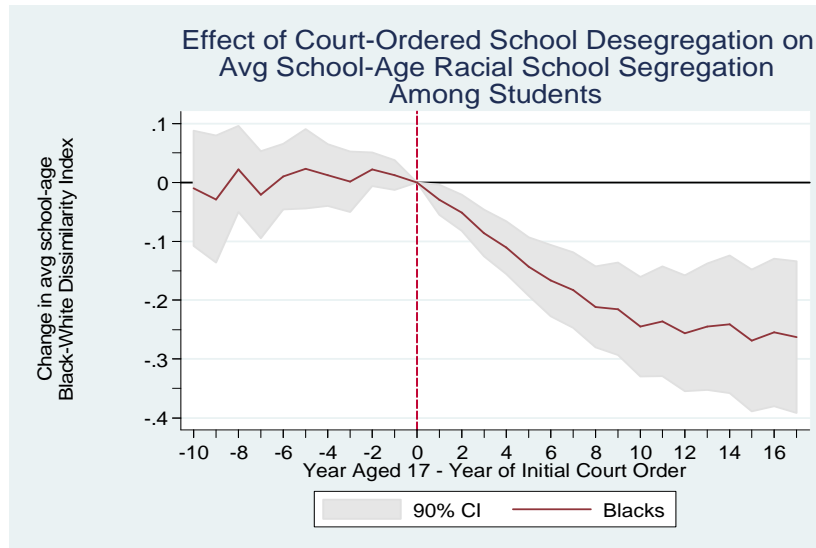
[23] The first-stage models include as predictors the years of desegregation exposure (for relevant ages 5-17; 20-24) interacted with the respective school district's desegregation-induced change in school spending.

[24] In other specifications (not shown to conserve space), I compared IV and non-IV estimated effects of major school desegregation plans on educational attainment, separately by race; the IV estimates for blacks are 2.4 times greater for high school graduation and 1.6 times greater for completed years of schooling (relative to the corresponding non-IV estimates using the same sample and model specification).
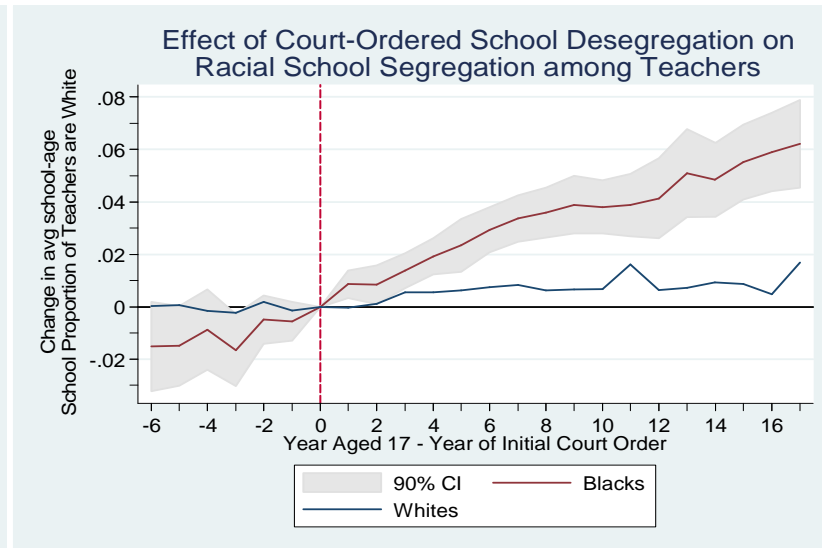
[25] This could arise if marginal returns are higher for those with low levels of schooling and the instrument (e.g., school reforms, school accessibility) mainly affects this segment of the population by lowering the costs of schooling. It seems plausible that desegregation disproportionately benefited those students with high costs of schooling and with especially high marginal rates of return.

[26] Recent examples include Chay, Guryan, and Mazumder (2009) (desegregation of hospitals and academic achievement), Almond, Chay and Greenstone (Civil rights and infant mortality), Finkelstein & McKnight (Medicare introduction), Cascio, Gordon, Lewis and Reber (Title I), Ludwig and Miller (Head Start), Almond, Hoynes and Schanzenbach (food stamps and birth outcomes), and McCrary (court-ordered police hiring quotas).
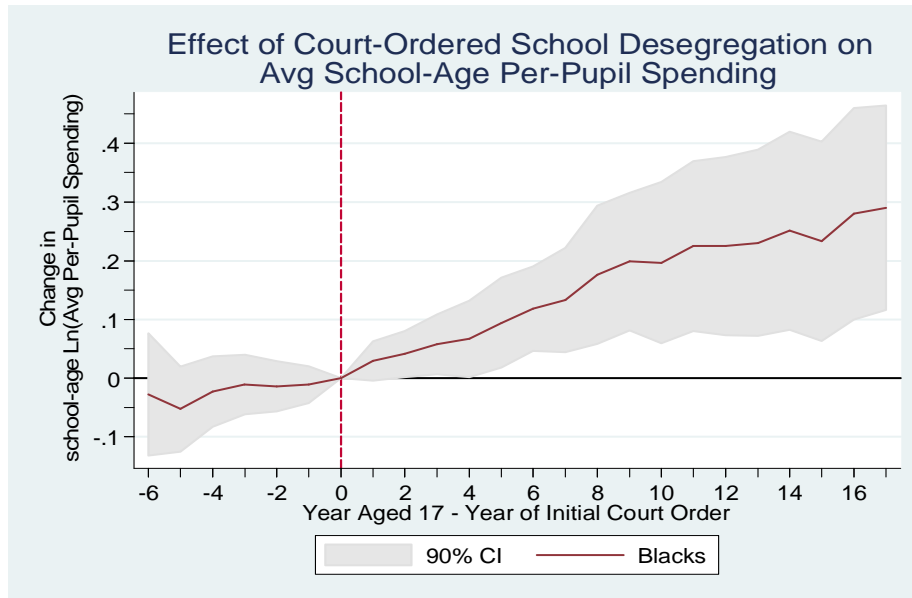
Effect of Court-Ordered School Desegregation on Avg School-Age Racial School Segregation Among Students

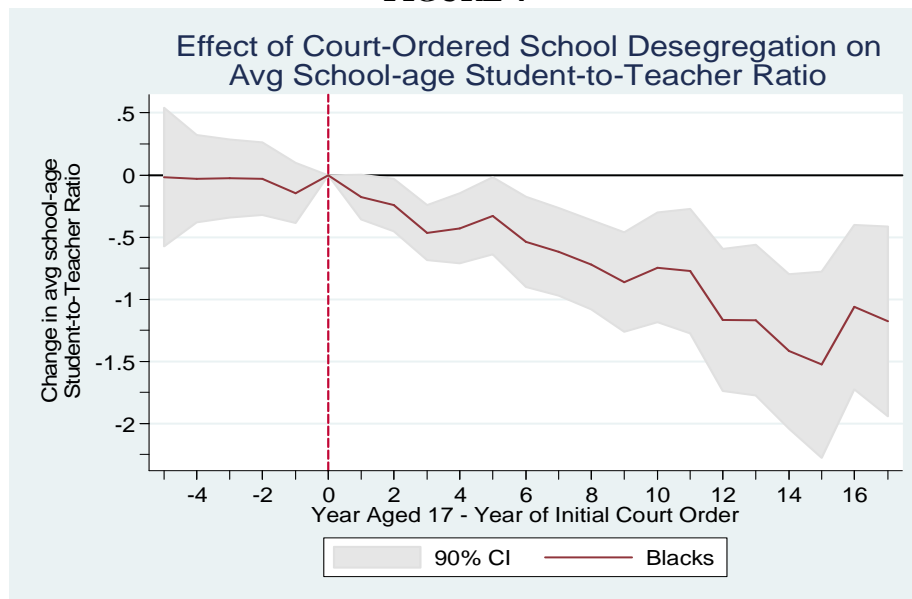Effect of Court-Ordered School Desegregation on Racial School Segregation among Teachers

Data: PSID geocode Data (1968-2013), matched with childhood school characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation.

Models: Results are based on non-parametric event-study models that include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Standard errors are clustered at the school district level.

FIGURE 3

Effect of Court-Ordered School Desegregation on
Avg School-Age Per-Pupil Spending

FIGURE 4

Effect of Court-Ordered School Desegregation on
Avg School-age Student-to-Teacher Ratio

Data: PSID geocode Data (1968-2013), matched with childhood school characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation.

Models: Results are based on non-parametric event-study models that include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Standard errors are clustered at the school district level.
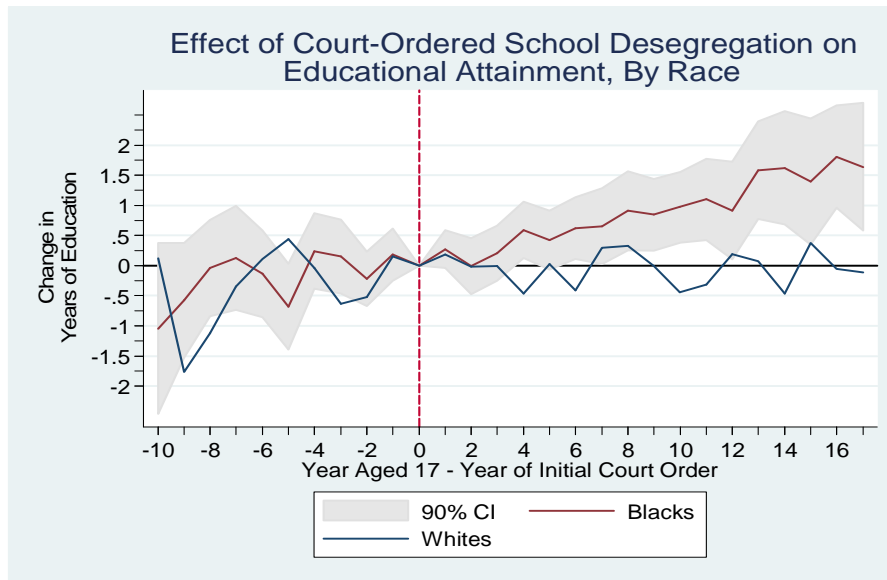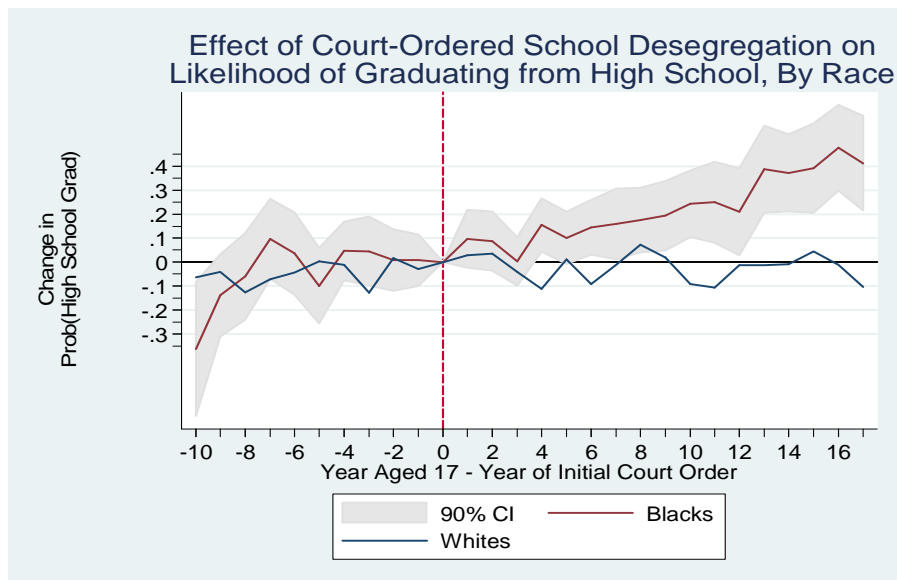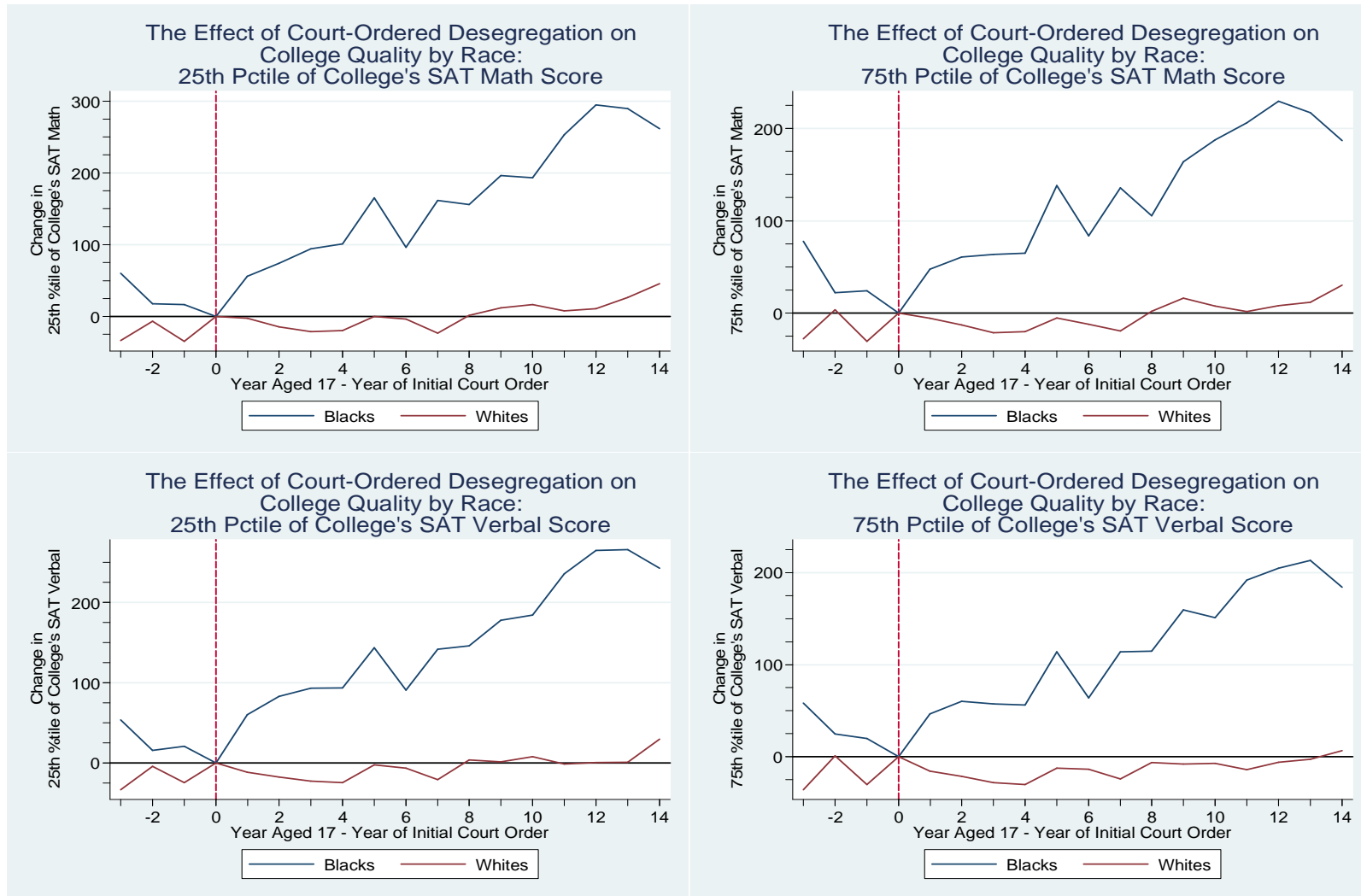
FIGURE 5

Effect of Court-Ordered School Desegregation on Educational Attainment, By Race

FIGURE 6

Effect of Court-Ordered School Desegregation on Likelihood of Graduating from High School, By Race

<u>Data</u>: PSID geocode Data (1968-2013), matched with childhood school characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. (N=8,548 individuals from 3,562 childhood families, 631 school districts).

<u>Models</u>: Results are based on non-parametric event-study models that include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Standard errors are clustered at the school district level. Results for whites not statistically significant from 0 (see Appendix Figures C1b-C2b).
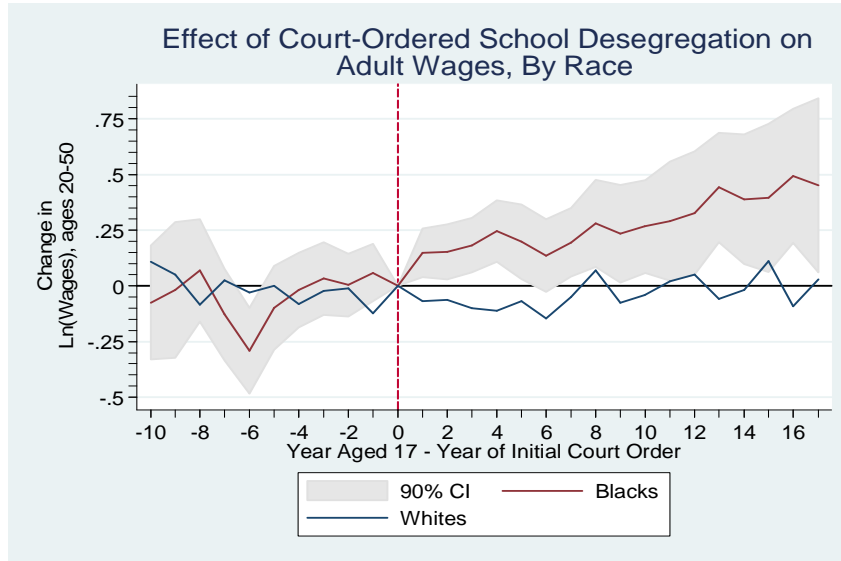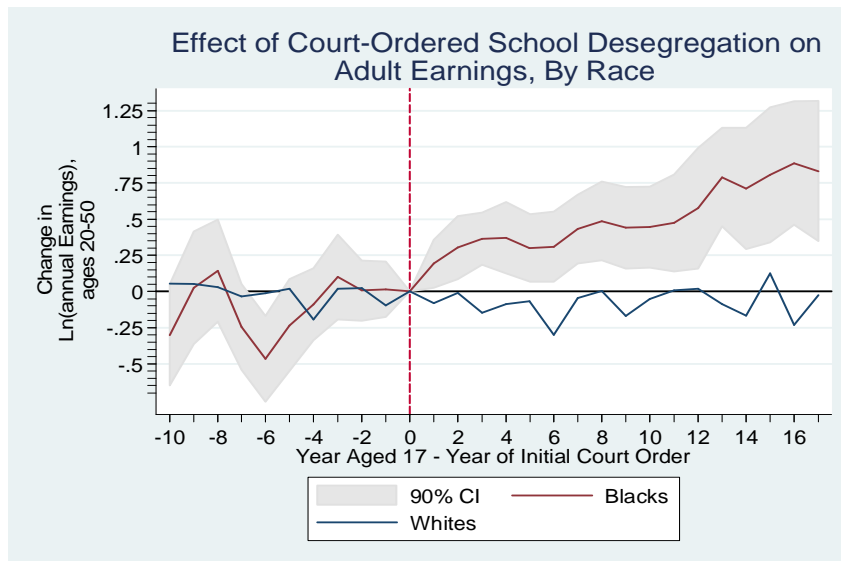
FIGURE 7.

The Effect of Court-Ordered Desegregation on College Quality by Race: 25th Pctile of College's SAT Math Score

The Effect of Court-Ordered Desegregation on College Quality by Race: 75th Pctile of College's SAT Math Score

The Effect of Court-Ordered Desegregation on College Quality by Race: 25th Pctile of College's SAT Verbal Score

The Effect of Court-Ordered Desegregation on College Quality by Race: 75th Pctile of College's SAT Verbal Score

Data: PSID geocode Data (1968-2013), matched with childhood school and College characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation for whom college information available. (N=1,570 individuals from 1,116 childhood families, 360 school districts).

Models: Results are based on non-parametric event-study models that include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Statisitically significant results for blacks, none for whites.
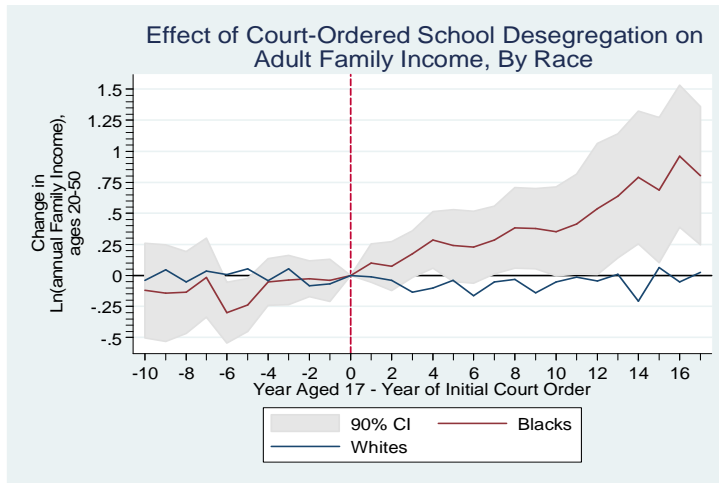
FIGURE 8

Effect of Court-Ordered School Desegregation on Adult Wages, By Race

FIGURE 9

Effect of Court-Ordered School Desegregation on Adult Earnings, By Race
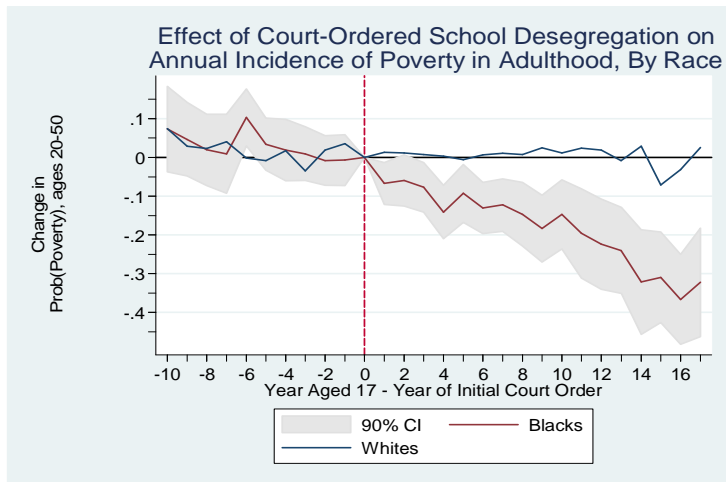
Data: PSID geocode Data (1968-2013), matched with childhood school characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. All person-year positive earnings observations (ages 20-50) are included except those in which individual was in school (N=97,568 person-year wage observations, 8,597 individuals from 3,584 childhood families, 636 school districts).

Models: Results are based on non-parametric event-study models that include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight); and controls for gender, age (cubic), svy year FE. Standard errors are clustered at the school district level. Results for whites not statistically significant from 0 (see Appendix Figures C3b-C4b).
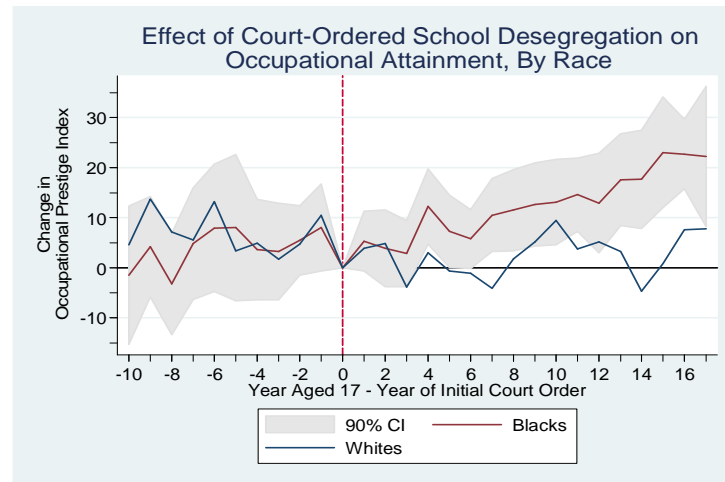
FIGURE 10

Effect of Court-Ordered School Desegregation on
Adult Family Income, By Race

Change in
Ln(annual Family Income),
ages 20-50

90% CI — Blacks
Whites

FIGURE 11

Effect of Court-Ordered School Desegregation on
Annual Incidence of Poverty in Adulthood, By Race

Change in
Prob(Poverty), ages 20-50

90% CI — Blacks
Whites

FIGURE 12

Effect of Court-Ordered School Desegregation on
Occupational Attainment, By Race

Change in
Occupational Prestige Index

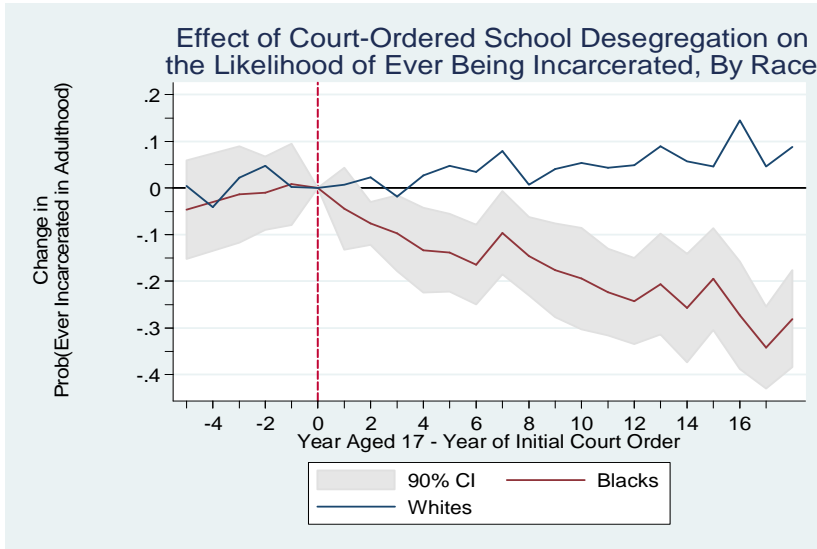90% CI — Blacks
Whites

Year Aged 17 - Year of Initial Court Order

Data: PSID geocode Data (1968-2013), matched with childhood school characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. All person-year observations (ages 20-50) are included except those in which individual was in school (N=142,499 person-year family income observations, 9,156 individuals from 3,702 childhood families, 645 school districts).
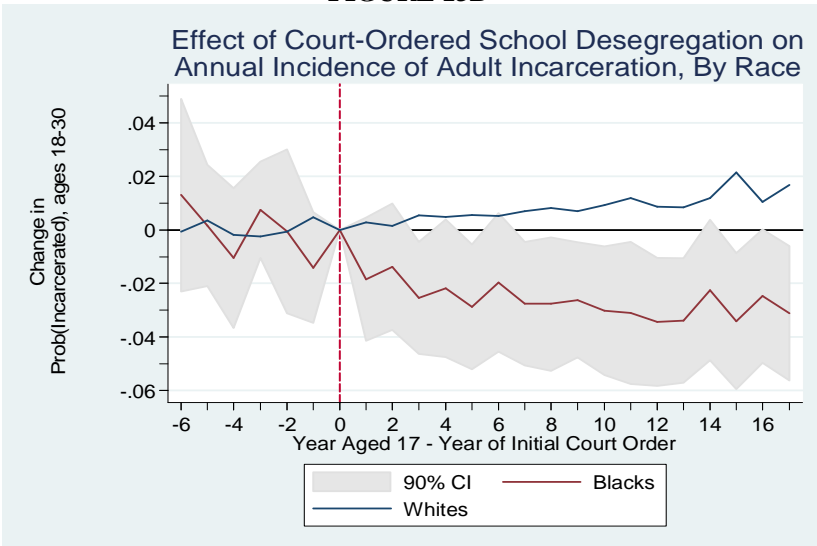
Models: Results are based on non-parametric event-study models that include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight); and controls for gender, age (cubic), svy year FE. Standard errors are clustered at the school district level. Results for whites not statistically significant from 0 (see Appendix Figures C5b-C6b).
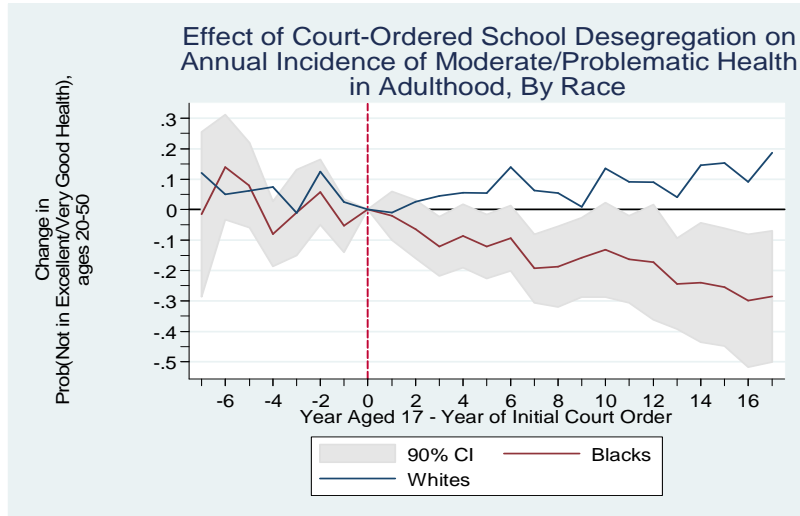
FIGURE 13A

Effect of Court-Ordered School Desegregation on the Likelihood of Ever Being Incarcerated, By Race

FIGURE 13B

Effect of Court-Ordered School Desegregation on Annual Incidence of Adult Incarceration, By Race
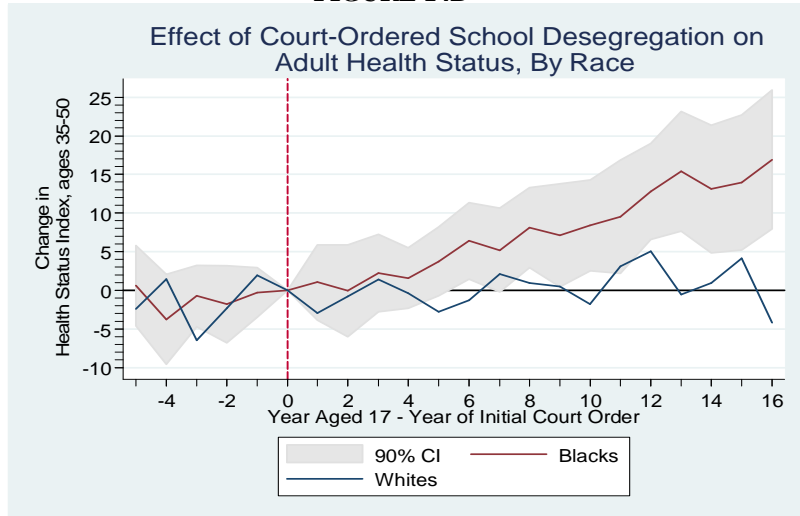
Data: PSID geocode Data (1968-2013), matched with childhood school characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, first observed before age 21 and followed until at least age 25, who grew up in school districts that were ever subject to court-ordered desegregation. Incarceration info based on reason for non-response for each survey 1968-2013 &, where available, 1995 svy reports of whether/when ever incarcerated. Models of annual incidence of adult incarceration include all person-year observations (ages 18-30). (N=96,584 person-year observations, 8,539 individuals from 3,411 childhood families, 524 school districts).

Models: Results are based on non-parametric event-study models that include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight); and controls for gender, age FE, svy year FE. Standard errors are clustered at the school district level. Results for whites not statistically significant from 0 (see Appendix Tables C7b-C8b).

FIGURE 14A

Effect of Court-Ordered School Desegregation on Annual Incidence of Moderate/Problematic Health in Adulthood, By Race

FIGURE 14B

Effect of Court-Ordered School Desegregation on Adult Health Status, By Race

Data: PSID geocode Data (1968-2013), matched with childhood school characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. All person-year self-assessed health status observations (ages 20-50) are included except those in which individual was pregnant/yrs immediately following childbirth (Figure 14A: N=75,729 person-year health status observations, 7,527 individuals from 3,330 childhood families, 613 school districts). Health Status index (1-100(perfect

health)) based on self-assessed health (E/VG/G/F/P), 1985-2013; interval regression model estimated, where E=[95,100]; VG=[85,95); G=[70,85); F=[30,70); P=[1,30). (Figure 14B: N=42,011 person-year observations at ages 35-50 for 5,598 individuals from 2,797 childhood families, 570 school districts).

Models: Results are based on non-parametric event-study models that include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight); and controls for gender, age (cubic), svy year FE. Standard errors are clustered at the school district level. Results for whites not statistically significant from 0 (see Appendix Figures C10b-C11b).

Table 1.
Long-run Effects of School Desegregation on Educational, Economic, & Health Attainment:
Sibling Fixed Effect Estimates

	Dependent variable:					
	Years of Education		Ln(Family Income)		general Health Status in adulthood, Interval Regression Model: 100pt-scale, 100=perfect health	
	Black	White	Black	White	Black	White
	(1)	(2)	(3)	(4)	(5)	(6)
Years of Exposure to Court-Ordered Desegregation$_{(age 5-17)}$	**0.1294***	0.0356	**0.0358***	-0.0067	**0.6417****	-0.1506
	(0.0729)	(0.0962)	(0.0189)	(0.0179)	(0.2941)	(0.4022)
Sibling Fixed Effects?	yes	yes	yes	yes	yes	yes

Robust Standard errors in parentheses (clustered at school district level)

*** p<0.01, ** p<0.05, * p<0.10

Note: All models include flexible controls for age (quadratic), gender, year of birth, birth order, birth weight, whether born into a two-parent family, and parental income (coefficients supressed to conserve space).

Table 2. 2SLS Estimates of Effects of Court-Ordered School Desegregation's Induced-Change in Racial School Integration & Induced-Change in Per-Pupil Spending on Educational Attainment & Adult Wages, By Race

	Dependent variable:					
	Years of Education			Ln(Wage), ages 20-50		
2SLS Estimates (school segregation & per-pupil spending instrumented using timing of court-ordered desegregation):	(1)	(2)	(3)	(4)	(5)	(6)
School Segregation:						
Avg Black-White Exposure Index$_{(age 5-17)}$ *Black	0.8200		-3.3974	0.3226		-1.3538
	(2.7650)		(3.1813)	(0.9070)		(1.0358)
Avg Black-White Exposure Index$_{(age 5-17)}$ *White	-1.3306		-2.7275	-0.1260		-1.8259
	(3.3394)		(5.1829)	(0.8734)		(1.2109)
School Spending:						
Ln(Avg Per-Pupil Spending)$_{(age 5-17)}$ *Black		**3.0670***	**4.8706****		**1.4367***	**2.1425****
		(1.8084)	(1.8955)		(0.8280)	(0.9644)
Ln(Avg Per-Pupil Spending)$_{(age 5-17)}$ *White		0.1281	1.5577		1.0692	**2.0143***
		(2.6961)	(4.2764)		(0.7185)	(-1.0481)
Number of adult person-year observations	--	--	--	97,568	97,568	97,568
Number of Individuals	8,548	8,548	8,548	8,597	8,597	8,597
Number of Childhood Families	3,562	3,562	3,562	3,584	3,584	3,584
Number of School Districts	631	631	631	636	636	636

Robust standard errors in parentheses (clustered at school district level). Includes same full set of controls as main models.

*** p<0.01, ** p<0.05, * p<0.10

Table 3. 2SLS Estimates of Effects of Court-Ordered School Desegregation's Induced-Change in Racial School Integration & Induced-Change in Per-Pupil Spending on the Annual Incidences of Poverty & Problematic Health in Adulthood, By Race

Dependent variable:

	Probability(Poverty), ages 20-50			Probability(Moderate/Problematic Health), ages 20-50		
2SLS Estimates (school segregation & per-pupil spending instrumented using timing of court-ordered desegregation) :	(1)	(2)	(3)	(4)	(5)	(6)
School Segregation:						
Avg Black-White Exposure Index$_{(age 5-17)}$ *Black	-0.2640		0.3316	-0.3524		0.3000
	(0.4559)		(0.5421)	(0.6547)		(0.7067)
Avg Black-White Exposure Index$_{(age 5-17)}$ *White	0.2455		**0.5254****	0.5339		**1.9164****
	(0.1648)		(0.2535)	(0.5049)		(0.7829)
School Spending:						
Ln(Avg Per-Pupil Spending)$_{(age 5-17)}$ *Black		**-0.5350+**	**-0.7065+**		**-0.5554+**	**-0.6910***
		(0.3918)	(0.4643)		(0.4275)	(0.4134)
Ln(Avg Per-Pupil Spending)$_{(age 5-17)}$ *White		-0.0508	**-0.3203+**		**-0.4935+**	**-1.4163****
		(0.1463)	(0.2213)		(0.3591)	(0.6317)
Number of adult person-year observations	142,781	142,781	142,781	75,729	75,729	75,729
Number of Individuals	9,156	9,156	9,156	7,527	7,527	7,527
Number of Childhood Families	3,702	3,702	3,702	3,330	3,330	3,330
Number of School Districts	645	645	645	613	613	613

Robust standard errors in parentheses (clustered at school district level). Includes same full set of controls as main models.
*** p<0.01, ** p<0.05, * p<0.10

Table 4. 2SLS Estimates of Effects of Court-Ordered School Desegregation's Induced-Change in Racial School Integration & Induced-Change in Per-Pupil Spending on the Annual Incidence of Incarceration in Adulthood, By Race

	Probability(Ever Incarcerated in Adulthood)			Prob(Incarceration), ages 18-30
2SLS Estimates (school segregation & per-pupil spending instrumented using timing of court-ordered desegregation) :	(1)	(2)	(3)	(4)
School Segregation:				
Avg Black-White Exposure Index$_{(age 5-17)}$ *Black	0.1625		**1.2750****	**0.1078***
	(0.4310)		(0.5212)	(0.0572)
Avg Black-White Exposure Index$_{(age 5-17)}$ *White	0.2584		0.4976	-0.0226
	(0.4742)		(0.6684)	(0.0282)
School Spending:				
Ln(Avg Per-Pupil Spending)$_{(age 5-17)}$ *Black		**-0.5916***	**-1.1945*****	**-0.1047***
		(0.3583)	(0.3958)	(0.0563)
Ln(Avg Per-Pupil Spending)$_{(age 5-17)}$ *White		0.0191	-0.2412	0.0277
		(0.2723)	(0.3854)	(0.0304)
Number of adult person-year observations	--	--	--	96,584
Number of Individuals	4,885	4,885	4,885	8,539
Number of Childhood Families	2,015	2,015	2,015	3,411
Number of School Districts	434	434	434	524

Robust standard errors in parentheses (clustered at school district level). Includes same full set of controls as main models.
*** p<0.01, ** p<0.05, * p<0.10