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

Rucker C. Johnson

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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 1970, and followed through 2011, 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 quasirandom 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.

Rucker C. Johnson
Goldman School of Public Policy
University of California, Berkeley
2607 Hearst Avenue
Berkeley, CA 94720-7320
and NBER
ruckerj@berkeley.edu

An online appendix is available at:
http://www.nber.org/data-appendix/w16664

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 *Brown v. Board of Education* Supreme Court 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 quality 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 1970 and have been followed through 2011, 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 was conducted 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 both non-parametric and 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. To attempt to identify the potential mechanisms, I isolate for every district the desegregation-induced change in per-pupil spending and racial school integration, respectively, independent of district-specific trends and other coincident policies. The district-specific 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 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 robustness of the results to a battery of specification tests provide supportive evidence that the estimates reflect the causal impacts of school desegregation and school quality. As here evidenced, 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. The focus of the next section is the analysis 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. The data and measures used to evaluate the long-run impacts on adult outcomes are described in section III. Section IV discusses the empirical strategy, econometric model, and estimation methods. The long-run results are presented in section V. This includes subsections that a) attempt to rule out competing explanations and violations of the identifying assumptions; b) evaluate the robustness of the results and explore their sensitivity to alternative functional form, specification tests, and alternative empirical strategies (with different underlying identification assumptions); and c) involve specifications that attempt to explore potential mechanisms. Summary discussion to put the magnitudes in perspective in relation to previous studies and concluding statements are provided in the final section.

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 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 (Figure A4). 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.

Estimating the Effects of Court-Ordered School Desegregation on School Resources. The first stage of the analysis investigates how court-ordered school desegregation influenced the quantity and quality of educational inputs received by minority children. 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 data includes measures from 1968-1988 Office of Civil Rights (OCR) data; 1962-1992 Census of Governments (COG) annual school finance data; Common Core data (CCD) compiled by the National Center for Education Statistics; along with the comprehensive case inventory of court litigation regarding school desegregation over the entire 1955-1990 period (American Communities Project), and major plan implementation dates in large districts (compiled by Welch/Light). The desegregation court case data contains an entire case inventory of every school district ever subject to court desegregation orders. 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.

The primary empirical strategy uses this 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 A1 also shows that collectively the bulk of pre-treatment school quality, SES, demographic, and labor market related characteristics do 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.

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 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. School- and district-level data used to analyze racial school segregation among students span the period 1968-1988 and include 815 districts; school district-level data used to analyze per-pupil spending span the period 1962-1992 and include 669 districts; and the school- and district-level data used to analyze class size and racial school segregation among teachers is available for the period 1968-1972 and include 759 districts and 33,952 schools. The first analysis with district-level panel data exploits the plausibly exogenous timing of initial court orders to estimate the following event study equation (1):

$$Y_{c,t} = \sum_{y=-5}^{-1} \pi_y \cdot 1\left(t - T_c^* = y\right) + \sum_{y=1}^{6} \tau_y \cdot 1\left(t - T_c^* = y\right) + X'_{ct}\beta + Z'_{ct}\gamma + (W_{1960c} * t)'\phi + \eta_c + \lambda_t + \varphi_g * t + \varepsilon_{ct}$$

where $Y_{c,t}$ is per-pupil spending, student-to-teacher ratio, segregation dissimilarity index or black-white exposure index among students in school district *c* in year *t* = (1962,…,1992); *g* indexes region (defined by 9 census division categories); and the indicator function, $1(\ )$, is equal to one when the year of observation is *y* = (…,-5, -4, -3,…, 1, 2,…,6,…) years removed from the date, T_c^*, when school district *c* was first issued the court order (*y*=0 is omitted).[3] The models include school district fixed effects (η_c), year fixed effects (λ_t), and census division-specific linear time trends ($\varphi_g * t$).

School desegregation efforts occurred against the backdrop of the broader civil rights movement and overlapped the same period as federal "War on Poverty" initiatives were implemented.[4] To control for possible coincident policies and the expansion of other programs, I include measures at the county-level for the timing of hospital desegregation, roll-out of "War on Poverty" policy initiatives (Z_{ct})—community health centers, Head Start and Project Follow-Through—and real per capita transfer programs (X_{ct} : per capita cash income support, medical care, and retirement and disability programs[5] (REIS)). Also included are measures of 1960 county characteristics ($W_{1960c} * t$: poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election (proxy for segregationist preferences)) each interacted with linear time trends to control for differential time trends in district outcomes that might be correlated with the timing of initial court orders.

The point estimates of interest, π_y and τ_y , are identified using variation in the timing of initial court orders. Because the indicator for $y = 0$ is omitted, π_y is interpreted as the average difference in outcomes *y* years *before* the court order was issued, and τ_y is the average difference in outcomes *y* years *after* the desegregation court order. Estimates of π_y allow a visual and statistical evaluation of the potential importance of pre-treatment, time-varying school district-level, unobservables; estimates of τ_y allow the post-treatment dynamics to be explored. The π_y and τ_y vectors traces out the (equilibrium) adjustment path for school resource 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).[6]

A key asset of this identification strategy is that estimates of π_y and τ_y will be unbiased even if there are pre-existing and permanent differences between school districts. The school district fixed effects control for time-invariant community characteristics such as preferences for racial integration and education. With the inclusion of year fixed effects and census division-specific time trends, the estimates will provide unbiased estimates of the impact of court-ordered school desegregation even if regions varied in their K-12 education policies or their average levels of funding support from year to year. Additionally, time-varying, community-level characteristics and measures of government transfers adjust the estimates for observed differences in characteristics and changes in federal programs.

The regression models are weighted by 1968 district student enrollment to yield estimates that are representative of the impacts for the average child.[7] I make sure the results are robust to the use of a balanced panel to avoid confusing the time path of how communities respond to desegregation with changes in the composition of school districts in the analytic sample. The standard errors are clustered at the school district level to account for serial correlation (Bertrand et al., 2004).

Finally, I use school-level data to estimate event-study models that examine impacts of court-ordered desegregation on average class size, separately by race. These regression models include school-level fixed effects, year fixed effects, and are weighted by the school's pre-treatment race-specific student enrollment, to yield estimates that are representative of the impacts for the average black child and white child, respectively; standard errors are once again clustered at the school district level.

The Effectiveness of School Desegregation. I build on the findings of Welch and Light (1987), Guryan (2004), Reber (2005), and Weiner et al. (2008) by first analyzing the effectiveness of desegregation court-orders in reducing the extent of racial school segregation (but using a larger sample of 815 districts, instead of the 125 that prior studies had). I then extend these findings to show that in the years immediately following court orders, desegregation had notable impacts on two key school quality resource indicators among blacks—1) increases in per-pupil spending and 2) reductions in the student-to-teacher ratio. These results are presented in Figures 1-4. The figures plot the regression coefficients on indicator variables for years before and after desegregation orders are enacted (year before initial court-order is the reference category) on school district racial segregation among both students and teachers, per-pupil spending, and the student-to-teacher ratio, respectively. The changes are all statistically significant.

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 1A (top left graph), 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 1B, we also witness a significant increase in the black-white exposure index among students (an alternative measure of school segregation).[8]

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 Figures 2A & 2B, 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. Figures 3A-3C show court-ordered desegregation effects on school district per-pupil spending, separately by revenue source (local; state; federal). The results indicate that, on average, school district per-pupil spending increased by nearly $1,000 by the end of the fourth year after court-ordered desegregation relative to the year immediately preceding the initial court order, which differed markedly from the trend leading up to the year these rulings went into effect. This is a substantial increase given that the average level of per-pupil school spending in 1967 among districts that had not yet implemented a plan was $2,738 (in 2000 dollars). 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 3B). I do not find a similar pattern in districts that were not under court-order, nor is there a significant pre-existing time trend among the districts under court order prior to the year in which the order was issued. 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 the level whites were previously receiving (i.e., without affecting prevailing resource levels for white students).

I test for this relationship empirically by estimating identical models of the level of school district per-pupil spending from state revenue sources on the timing of court-ordered desegregation (with the inclusion of school district fixed effects and region-specific year effects), separately for school districts with a small proportion of black students (<0.2) versus districts with a large proportion of black students (>0.4).[9] As shown in Figure 3C, I find precisely this pattern: no significant changes in per-pupil school spending among districts that had a small proportion of black students; in contrast, we see substantial and statistically significant increases in per-pupil spending from state revenue sources among districts that had a large proportion of black students. These results complement the findings of Reber (2010) for Lousiana, and Cascio et al. (2010), and employ larger samples and geographic coverage.

Reductions in Class Size. With the use of school-level data (N=33,952 schools from 33 different states), Figure 4 provides supportive evidence of reduced average class size for blacks following desegregation court orders. 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. 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. 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. 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.

III. DATA AND MEASURES

The primary micro dataset utilized to analyze long-run outcomes is the restricted, confidential geocoded version of the PSID (1968-2011) with identifiers at the neighborhood block level in which children grew up.[10] I then merge neighborhood and school characteristics, and information on other key policy changes (e.g., the timing of hospital desegregation, rollout of "War on Poverty" initiatives and expansion of safety net programs), from multiple data sources on the conditions that prevailed in the 1960s, 70s, and 80s when these children were growing up.[11]

The sample consists of PSID sample members born between 1945 and 1968 who have been followed into adulthood; these individuals were between the ages of 43 and 65 in 2011. I include all information on them for each wave, 1968 to 2011.[12] I include both the Survey Research Center (SRC) component and the Survey of Economic Opportunity (SEO) component, commonly known as the "poverty sample," of the PSID sample. Due to the oversampling of black and low-income families, 45 percent of the sample is black.

School Measures. I use the census block as the definition of neighborhood, which comprises a smaller geographic area than previous studies utilize; and I match childhood residential location address histories to blocks and school district boundaries that prevailed in 1969 (the algorithm is outlined in Appendix A)[13]. Each record is merged with a set of school quality resource indicators for 1960-1990 (including per-pupil spending, class size) and measures of the extent of racial school segregation and school desegregation efforts at the school level.

Sixty-six 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., 8,777 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 (Figure 5A).

I merged the school district expenditures data, information on student-teacher ratios, and the constructed school segregation indices, to the PSID data using the census block/tract contained in the Geocode file based on the earliest available address in childhood (or county of birth when census block information is unavailable). After combining information from the 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 8,258 individuals from 3,495 childhood families, 627 school districts, 433 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. The appendices list the sources and years of all data elements along with details of the PSID survey questions used to construct key measures. Appendix Table A0 contains sample descriptive statistics for various childhood measures by race.

Outcomes of interest. 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 25-45); 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 25-45). 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

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 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 delay in implementation of major desegregation plan following (exogenous) initial court order 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 in detail below).

I begin by estimating equations of the form:

$$\begin{aligned}(2)\quad Y_{icb} = & \sum_{t-T=-20}^{-2} \alpha_{t-T}^{r} \cdot 1\left(t_{icb} - T_c^{*} = t-T\right) + \sum_{t-T=0}^{12} \theta_{t-T}^{r} \cdot 1\left(t_{icb} - T_c^{*} = t-T\right) + \sum_{t-T=13}^{20} \delta_{t-T}^{r} \cdot 1\left(t_{icb} - T_c^{*} = t-T\right) \\ & + X_{icb}\beta + Z_{cb}\gamma + (W_{1960c} * b)\phi^{r} + \eta_c^{r} + \lambda_b^{r} + \varphi_g^{r} * b + \varepsilon_{icb}\end{aligned}$$

where *i* indexes the individual, *c* the school district, *b* the year of birth, *g* the region of birth (defined by 9 census division categories), *r* the racial group, and the indicator variables, $1\left(t_{icb} - T_c^* = t - T\right)$, equal one if the year the individual from school district *c* turned age 17 (t_{icb}) minus the year of the initial court order in school district *c* (T_c^*) equals a value between -20 and 20, which is the full support of years individuals were age 17 relative to initial court order years in the sample. (Accordingly, as defined in the event study model above)/For example, values for $\left(t_{icb} - T_c^*\right)$ between -20 and -2 represent pre-treatment years; a value of -1 represents an individual who was 18 when court-ordered desegregation was first enacted and thus was not exposed, which is used as the reference group category; values between 0 and 12 represent school-age years of desegregation exposure; and values greater than 12 represent years beyond school-age exposure years. The event study year (*t* - *T*) is zero when the year in which an individual is age 17 (typically, senior year in high school) equals the initial year of the desegregation court order for the school district in which the person grew up.

The validity of the research design relies upon the exogeneity of the timing of initial court orders, which is addressed and supported by the model specification in several ways. First, the model includes race-specific school district fixed effects (η_c^r), race-specific birth year fixed effects (λ_b^r), race-by-region of birth cohort trends ($\varphi_g^r * b$), and 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 ($W_{1960c} * b$: 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.

As aforementioned, 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-level measures that capture the geographic timing of hospital desegregation (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_{cb}). The data sources used to compile these measures are detailed in the Data Appendix. While this work draws heavily from prior research that has 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 25-45) with controls for age, age squared and age cubed to avoid confounding life cycle and birth cohort effects.

Estimation of equation (2) provides an unrestricted description of the subsequent adult attainment outcomes in relation to the cohort- and district-specific timing of court-ordered school desegregation, separately by race, θ^{r}_{t-T} . The estimates of θ^{r}_{t-T} provide precise pictures of the exact timing of any changes in attainment outcomes in relation to the number of school-age years of exposure to court-ordered desegregation, separately for blacks and whites; while the estimates of α^{r}_{t-T} provide precise pictures/visual portrait 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 in the timing of the initial court orders.

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). 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 Table A1 & Appendix B). In contrast, as will be shown in Section III, 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 (2) estimates that form the response function of school desegregation effects to test for any dose-response with years of exposure. Furthermore, estimated effects beyond the maximum 12 school-age years of exposure (δ^r_{t-T} , for event study years (t-T)>12) provide an additional specification test, as these should not exhibit significant trends in outcomes since these additional years do not represent any change in school-age exposure[14]. This motivates fitting a parametric version of the unrestricted event study model (above) that provides simple summaries of the magnitudes and statistical significance of desegregation's impacts. A spline specification is used to place some structure on the relationship between court-ordered desegregation exposure and adult outcomes to improve precision, but the structure imposed is flexible enough to allow several important specification tests to examine whether the detected impacts support a causal interpretation of school desegregation. The chosen spline specification, informed by the non-parametric event study models, is:

$$
\begin{aligned}
&Y_{icb} = \theta_0^r\left(t_{icb} - T_c^*\right)\cdot D_{cb}1\left(t_{icb} - T_c^* < 0\right) \\
&+ \theta_1^r \cdot D_{cb}1\left(t_{icb} - T_c^* \geq 0\right) + \theta_2^r\left(t_{icb} - T_c^*\right)\cdot D_{cb}1\left(0 \leq t_{icb} - T_c^* \leq 12\right) + \theta_3^r \cdot D_{cb}1\left(t_{icb} - T_c^* \geq 12\right) \\
(3)\quad &+ \theta_4^r\left(t_{icb} - T_c^*\right)\cdot D_{cb}1\left(t_{icb} - T_c^* > 12\right) \\
&+ X_{icb}\beta + Z_{cb}\gamma + (W_{1960c} * b)\phi^r + \eta_c^r + \lambda_b^r + \varphi_g^r * b + \varepsilon_{icb}
\end{aligned}
$$

The specification allows a partial test of the identifying assumption through its test of pre-existing time trends in outcomes prior to court orders and a break in this trend once desegregation orders go into effect. θ_0^r captures the pre-period linear trend in outcomes prior to desegregation; θ_1^r represents any discrete change in outcome associated with having any desegregation exposure; θ_2^r represents estimated impact of each additional year of desegregation exposure, ranging from 0 to 12 years of exposure; θ_3^r represents any discrete change in outcome associated with exposure throughout one's school-age years; θ_4^r captures the post-desegregation linear trend for years beyond school-age (i.e., this represents years of exposure during pre-school years or prior to birth, and thus, no difference in actual exposure during school-age years). Figure 5B illustrates a graphical representation that motivates the spline specification, and the patterns we may expect to see if consistent with a causal impact of desegregation.

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 two factors: 1) elementary students may have fewer social adjustments than older students; and 2) 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), as well as a discrete change associated with any exposure and potential additional impact over and above that for exposure throughout one's school-age years.

Let k denote the number of years before or after the initial court order that an individual turned 17, which is constructed from variables for year aged 17 (t) relative to the year of the initial desegregation order (T_c^*) in district c. Thus, if we take the example for k<12, [$\theta_1 + \theta_2 k$] gives the expected difference in adult outcomes between individuals who became age 17 k years after initial court orders are enacted, relative to individuals who had reached age 17 the year prior to it (age 18 the year the court order took effect). I use the year before court orders are enacted as the reference point. The specification allows for the effects to manifest immediately following the first year of child exposure to desegregation court orders and to be a function of the duration of exposure, which is important both because it often took several years for a major desegregation plan to be fully implemented following a court order and the effects of integrated schools may increase with a child's exposure to the "treatment".[15] These terms are all interacted with race to test for differential desegregation impacts by race. The standard errors are clustered at the childhood county level.

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 identify the neighborhood and school of upbringing based on the earliest childhood address (in most cases, 1968).[16] The resultant potential measurement error of school quality will tend to lead to attenuation bias of coefficients toward zero. Appendix Table A14 shows similar results when the sample is restricted to individuals who lived in their childhood residence prior to the initial court order. The analysis does capture school district characteristics that changed significantly from year to year. The latter part of Section V provides more discussion of various falsification and specification tests performed.

V. RESULTS

Educational Attainment. Figures 6A and 6B present the non-parametric and parametric event study model results for blacks and whites on the same graph for the effects of court-ordered school desegregation on the probability of graduating from high school and years of completed schooling, respectively. The average high school graduation rates for blacks and whites for these birth cohorts is 0.73 and 0.88, respectively. As detailed in Section IV, all models include race-specific school district fixed effects, race-specific region and year of birth effects; controls for linear cohort trends in 1960 county characteristics; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs; and childhood family characteristics.

The results indicate that, for blacks, the onset of desegregation exposure produces an immediate jump in the likelihood of graduating from high school. Each additional year of exposure leads to a 2 percentage-point increase in the likelihood of high school graduation (coefficient on 1 to 12 years of exposure spline) with an additional jump for those exposed throughout their school-age years (Figure 6A; Table A1). Similarly, there are large, statistically significant effects on completed years of education for blacks. Each additional year of exposure to court-ordered desegregation leads to between a 0.11 and 0.15 increase in years of education for blacks (Figure 6B; Table A1). 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 13.8 percentage-point increase in the likelihood of graduating from high school and roughly a 0.5 increase in years of education for blacks (Table A1). 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; but after enactment, we see a structural break in the trend for blacks. Furthermore, I find no significant effects for blacks for years of exposure beyond one's school-age years (as evidenced by the insignificant coefficients on "14" & "15").

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 blacks' improvements in educational attainment were driven by secular trends in desegregated districts.

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.

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 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 2009 and match it with the Integrated Post-secondary Education Data System (IPEDS) 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 collegiate freshman class as markers of college quality.

Figure 7 presents the non-parametric event study model estimates for blacks 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, are 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 (if anything, it appears downward) and negligible effects beyond school-age years. Estimated effects for whites are consistently small with point estimates near zero.[17]

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, using the same model specifications and sample of ever-treated districts. Figures 8-10 and Tables 2-4 present desegregation effects by race on adult economic outcomes (ages 25-45), including occupational attainment (Figure 8), wages (Figure 9A), annual work hours (Figure 9B), annual family income (Figure 10A), and the annual incidence of poverty (Figure 10B). In light of the parallel set of findings across all these long-run economic outcomes, the results are discussed in succession.

The results indicate that, for blacks, court-ordered desegregation significantly increased occupational attainment, as reflected in the 5.2 point increase in the occupational prestige index associated with a 5-year increase in exposure (Figure 8; Table A3). The average occupational prestige index for blacks and whites prior to desegregation was 30 and 60, respectively. Furthermore, among blacks, there is an immediate jump in wages and work hours with exposure to court-ordered desegregation, and each additional year of exposure leads to a 1.2 percent increase in wages (coefficient on 1 to 12 years of exposure spline) with an additional jump for those exposed throughout their school-age years (Figures 9A & 9B; Table A2). 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.

I find this translated into substantial gains in adult family economic status among blacks. As shown in Figures 10A and 10B, 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. We see improvements accompanied longer durations of exposure and an additional jump for those exposed throughout their school-age years (Figure 10A); and in similar fashion, we see a reduction in the annual incidence of poverty with duration of desegregation exposure and an additional decline in poverty risk for those exposed throughout their school-age years (Figure 10B). The average effects of a 5-year exposure to court-ordered school desegregation lead to about a $5,900 increase in annual family income (Table A4) and an 11 percentage-point decline in the annual incidence of poverty in adulthood (Table A2, column 3). The estimated magnitudes of desegregation impacts are on par with the coefficients on parental education.[18]

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 (shown by the insignificant pre-desegregation coefficients on the “-7 to -1” spline term), nor is there any evidence of effects on blacks for years of exposure beyond one’s school-age years across the range of adult economic outcomes (shown by the insignificant coefficient on the “>13” spline term). 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, using the same model specifications. The proportion of blacks (whites) ever incarcerated is 0.08 (0.04) for this sample of birth cohorts.

Among blacks, Figures 11A and 11B 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 elementary school leads to nearly a 2 percentage-point reduction in the annual incidence of incarceration (Figure 11B) and a 22 percentage-point decline in the probability of adult incarceration (Figure 11A). The results do not indicate any pre-existing trends in these outcomes prior to court-ordered desegregation, nor are there significant effects on blacks for years when desegregation court orders correspond with one’s pre-school years (Table A5)—two important specification tests that support the validity of the research design. 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 parallels 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, using the same model specifications. 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.

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 parametric event study results (Figure 12), based on the interval regression model estimates, indicate that, for blacks, there is a discontinuous jump in adult health status with exposure to court-ordered desegregation, and each additional year of exposure leads to a 0.3 point increase in the health status index (coefficient on 0 to 12 years of exposure spline) with an additional jump for those exposed throughout their school-age years. The average effects of a 5-year exposure to court-ordered school desegregation yields about a 3 point increase in the adult health status index, which represents substantial improvements in adult health status. Linear probability models of the annual incidence of problematic health yielded similar patterns reflecting reductions in the probability of fair/poor health (but with less precision, Table A6).

A useful way to interpret the estimate is in relationship to the size of the effect of age on health. For blacks, the impact of each additional year of desegregation exposure is equivalent (on average) to reaching a level of health deterioration about 1 year later. 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. There is little evidence of pre-existing time trends in adult health in the years leading up to the court order, nor are there significant effects of court orders that correspond with non-school ages for blacks. 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.

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 who 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 lower class sizes during adolescent 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 attainments.

Robustness & Falsification Tests. The main model specifications were chosen to minimize potential bias. We have already witnessed the results to be robust to alternative functional form, specification tests, and alternative empirical strategies (with different underlying identification assumptions). As a robustness check to address potential endogenous migration, I also find similar results when the sample is restricted to individuals who lived in their childhood residence prior to the initial court order (e.g., see Appendix Table A14).

Table A7 probes the robustness of these estimates further. As a falsification exercise, I re-estimated equation (2) replacing the timing of initial court-ordered desegregation variables with unsuccessful litigation cases and the 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 A7). 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 is 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, whether pre-school ages or beyond age 17); d) had the largest impacts on blacks in communities where desegregation resulted in the largest changes in school quality inputs (Table A8); 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 actually 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 one potential pathway that merits careful consideration is through impacts of school quality improvements (i.e., greater school resources for blacks in integrated schools) on the socioeconomic mobility process. 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.

Building on the results presented in Section II, I isolate for every district the desegregation-induced change in per-pupil spending and racial school integration, respectively, which is net of time-invariant school district characteristics, district-specific trends and a host of other coincident policy changes (see Figure A3). 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.

In order to assess the relative roles of school resources and peer effects as potential mechanisms underlying the desegregation effects, I estimate parametric event study models of the form:

$$
\begin{aligned}
(4) \quad Y_{icb} &= \theta_0^r \left(t_{icb} - T_c^*\right) \cdot D_{cb} 1\left(t_{icb} - T_c^* < 0\right) \cdot SPEND_c + \theta_1^r \left(t_{icb} - T_c^*\right) \cdot D_{cb} 1\left(t_{icb} - T_c^* < 0\right) \cdot SEG_c \\
&+ \theta_2^r \left(t_{icb} - T_c^*\right) \cdot D_{cb} 1\left(0 \le t_{icb} - T_c^* \le 12\right) \cdot SPEND_c + \theta_3^r \left(t_{icb} - T_c^*\right) \cdot D_{cb} 1\left(0 \le t_{icb} - T_c^* \le 12\right) \cdot SEG_c \\
&+ \theta_4^r \left(t_{icb} - T_c^*\right) \cdot D_{cb} 1\left(t_{icb} - T_c^* > 12\right) \cdot SPEND_c + \theta_5^r \left(t_{icb} - T_c^*\right) \cdot D_{cb} 1\left(t_{icb} - T_c^* > 12\right) \cdot SEG_c \\
&+ X_{icb}\beta + Z_{cb}\gamma + (W_{1960c} * b)\phi^r + \eta_c^r + \lambda_b^r + \varphi_g^r * b + \varepsilon_{icb}
\end{aligned}
$$

where $SPEND_c$ is the desegregation-induced change in per-pupil spending in district *c*; SEG_c is the desegregation-induced change in racial school segregation among students in district *c* (as measured by the black-white exposure index); with the inclusion of the same set of controls as previously discussed in Section IV.[21] The terms used in the specification to capture the duration of desegregation exposure is simplified to improve precision in this expanded model (which is supported by the earlier desegregation results reported which were roughly linear in school-age exposure years to a first approximation). 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 event study framework allows one to inspect whether districts that underwent larger changes in school spending (segregation) resultant from desegregation exhibited differential trends in outcomes preceding the enactment of court orders, which I use as an additional specification test.

The results are presented in Table A8. For blacks' educational, economic and health attainments, the results suggest that changes in school quality resulting from integration played an important role. 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. These significant effects persist after the inclusion of corresponding increases in the black-white exposure index that accompanied desegregation. 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.

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. Interestingly, once again 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 infusion of school funding that accompanied desegregation (as reported in Section II for districts with significant black enrollment) was used to level up resources for blacks to the level whites were receiving prior to desegregation.

In order to summarize the results on the mechanisms, I estimate 2SLS models in which the key explanatory variables of interest—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 the individual's duration of desegregation exposure interacted with the respective school district's desegregation-induced change in school spending (segregation). The 2SLS models are presented in Table 2 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 intensity of treatment that may capture the combined effects of improvements in school resources and teacher quality.

To facilitate interpretation of marginal effects, the units of the average per-pupil spending during an individual's school-age years are in thousands of dollars—thus, a 1-unit change represents a $1,000 change in spending (2000 dollars) in each of one's K-12 years. In similar fashion, the key school segregation variable is defined such that a one-unit increase in "Black-White Exposure Index $_{(age\ 5\text{-}17)}$" represents a 0.15 increase in the black-white exposure index or a standard deviation increase in racial school integration experienced in each of one's K-12 years.[22]

The 2SLS results highlight significant positive effects of desegregation-induced increases in school spending on blacks' adult attainments. In contrast, these 2SLS models reveal small, insignificant effects for increases in racial integration (holding spending changes constant). As a placebo falsification test using the 2SLS models, it is shown in Table A9 that school spending increases have no significant impacts on adult outcomes when they occur during non-school ages (whether during preschool ages or after age 19), 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 results for blacks indicate that a $1,000 increase in school spending (which corresponds to roughly a 25-30 percent increase) experienced throughout one's school-age years is associated with an additional 1.4 years of completed education, a 58 percent increase in wages, an increase of $18,635 in annual family income, a 34 percentage-point reduction in the annual incidence of adult poverty, and a 2.1 percentage-point reduction in the annual incidence of adult incarceration. These magnitudes are similar to the previously discussed event study results (Figures 6-11) in comparisons of individuals exposed to desegregation beginning in one's elementary school years relative to growing up in segregated schools throughout one's school years. There are no corresponding significant effects for whites on either of these markers of the intensity of treatment across the adult outcomes. Moreover, that I find no school spending effects for poor whites, and that Title I federal spending is already explicitly controlled for in these models, provides further support that the school spending effects capture desegregation-induced impacts, not Title I funding.

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 A15). Finally, it is noteworthy that other concurrent policy advents were explicitly considered (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), but 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. His IV estimates 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 A10, 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 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 A10 expands 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 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 A13). 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 A11. The similar pattern of the PSID and OCR-CCD results further demonstrates that the findings are generalizable and representative for these birth cohorts, and allays 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). Evidence is mixed on how much school resources matter. 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 (around 23 years old). Based on these factors, Card and Krueger (1996) conclude, "Our review of the literature reveals a high degree of consistency across studies regarding the effects of school quality on student's subsequent earnings. The literature suggests that a 10 percent increase in school spending is associated with a 1 to 2 percent increase in annual earnings for students later in their lives" (p. 133). Inadequate controls for childhood family and neighborhood characteristics can lead to omitted variable bias of estimated school effects. Card and Krueger echo this concern. A strength of the present analyses, in addition to its credible research design, is 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 45.

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 driven mostly 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 herein show that labor market outcomes, and adult income and health status rose in line with blacks' educational improvements (in quantity and quality in absolute and relative terms), as did declines in incarceration. Table A12 presents a summary of the implied Wald estimates of the returns to education (reflecting a combination of increased quantity 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 produced using modern schooling interventions and data sources from more recent 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 most impacted by school desegregation were just extremely large. Thirdly, 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.

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, it 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 and 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 across a broad set of socioeconomic and health outcomes, 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 precise 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. 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 models estimated upon which Figures 1-3 are based also include dummy indicators for each of the corresponding years in excess of 6 before and after court-ordered desegregation, respectively; these are not displayed in the figures because of the lack of precision due to limited observations that far away from the year of initial court order.
[4] For example, this period included the desegregation of hospitals (and workplaces), and the introduction of Medicaid, Medicare, Head Start, and the Supplemental Nutrition Program for Women, Infants and Children (WIC). Further, AFDC, Social Security, and disability income programs expanded.
[5] I am grateful to Doug Almond, Hilary Hoynes, and Diane Schanzenbach for sharing the Regional Economic Information System (REIS) data for the 1959 to 1978 period.
[6] Note, however, that the point estimates corresponding to $y < -3$ and $y>3$ are estimated from a smaller sample of school districts than estimates for the intervening years. This is because school district-level data on per-pupil spending and teacher-to-student ratios is not available annually for many districts before 1968. As a robustness check for court-order induced effects on dimensions of school quality, I used a balanced panel of school districts that includes districts only if they contribute to the identification of the entire vector of leads and lags of implementation impacts (i.e., districts that have school quality information in at least three years before and three years after implementation). Evidence shows that the increase in the treatment effect in the first 4 years after the court order is not a spurious result of the differing set of districts identifying the parameters.
[7] If I instead treat individual school districts as the observational unit and estimate unweighted regressions, then the estimates will represent the impact experienced for the average school district. While this parameter is intriguing, I am most interested in documenting the impacts of school desegregation for the average black student.

[8] Levels of racial integration in schools peaked around 1988.
[9] Among the set of school districts that underwent court-ordered school desegregation at some time between 1954 and 1980, the 25th and 75th percentile of the school district proportion of students who were black was 0.2 and 0.4, respectively, in 1970.
[10] 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. 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.
[11] This includes measures from 1968-1988 Office of Civil Rights (OCR) data; 1960, 1970, 1980 Census data; 1962-1992 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 entire 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).
[12] 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).
[13] A substantial share of school districts were counties during this period, including more than one-half of Southern school districts.
[14] Except in the case in which desegregation plans became more effective with time would we expect a significant relationship between outcomes and event study years beyond 12, which I explore.
[15] It is also possible that outcomes may have been influenced by the announcement of impending desegregation (e.g., "white flight" in response to the announcement by the Federal court that desegregation would begin at the start of the next school year).
[16] Among original sample children in the PSID, the average proportion of childhood spent growing up in the 1968 neighborhood was roughly two-thirds.
[17] 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.
[18] The sum of coefficients of mother's and father's education on adult wages is roughly 0.04.
[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] The estimated equation also includes the main effects without the interaction terms in school spending and segregation; equation (4) abstracts from this to ease the number of terms shown. The school spending and segregation terms are centered around the average desegregation-induced changes ($1,000 for per-pupil spending; 0.15 for black-white exposure index), so that the coefficient on the main desegregation exposure term represent the desegregation impact for the average change in these key school inputs. These models use the same sample as the aforementioned ones but include dummy indicators if district-specific desegregation induced-changes in per-pupil spending (school segregation) cannot be computed because of missing data; the occurrence of missing data occurs most often in small, rural areas.
[22] The excluded instrument for this school spending (segregation) variable is the number of school-age-years of desegregation exposure interacted with the respective school district's desegregation-induced change in school spending (segregation).
[23] The first-stage models include as predictors the years of desegregation exposure (for relevant ages 5-17; 20-24; 0-4) 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).

FIGURE 1A.

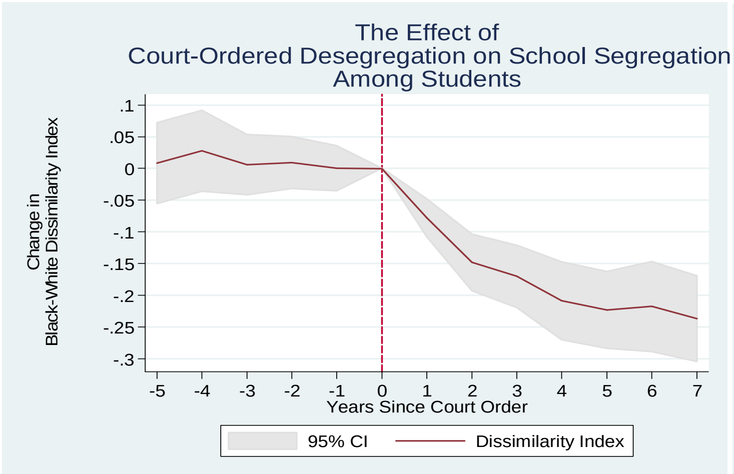
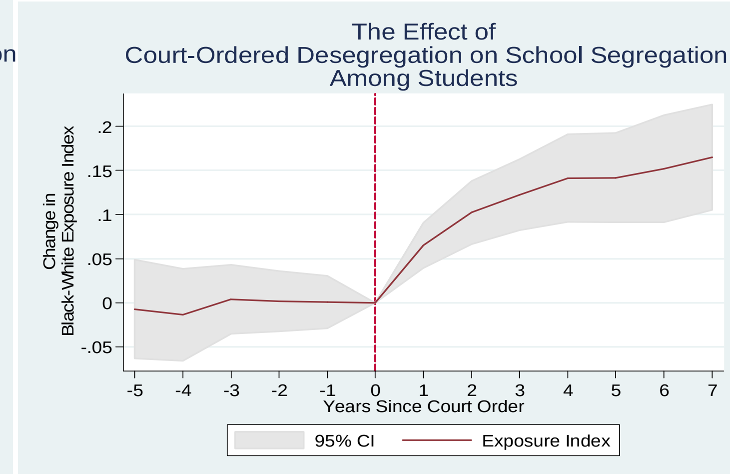


FIGURE 1B.



Data: Office of Civil Rights (OCR) School-level & School district-level Data, 1968-1988; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all school districts from OCR data that were ever subject to court-ordered desegregation (N=815 school districts; 7,527 school district-year observations).

Models: Results are based on event-study models that include school district fixed effects, year fixed effects, census division-specific linear time trends, and controls at the county-level for the timing of hospital desegregation, roll-out of "War on Poverty" policy initiatives--community health centers, Head Start and Project Follow-Through--and controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election (proxy for segregationist preferences)) each interacted with linear time trends. Models are weighted by 1968 district student enrollment, so that estimates are representative of the impacts for the average child; standard errors are clustered at the school district level.

FIGURE 2A.

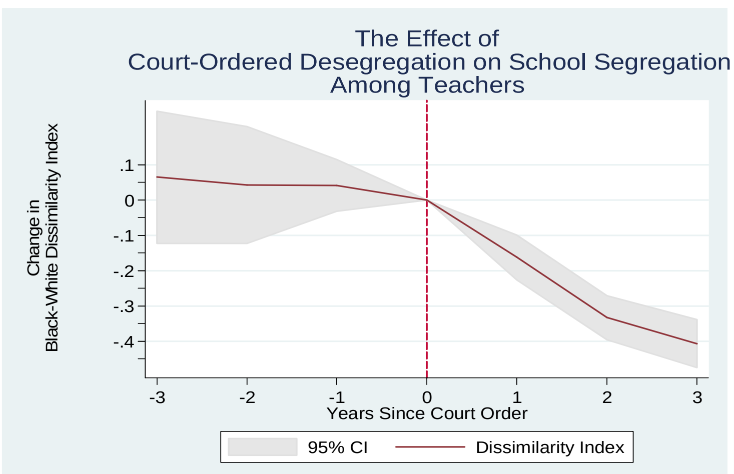
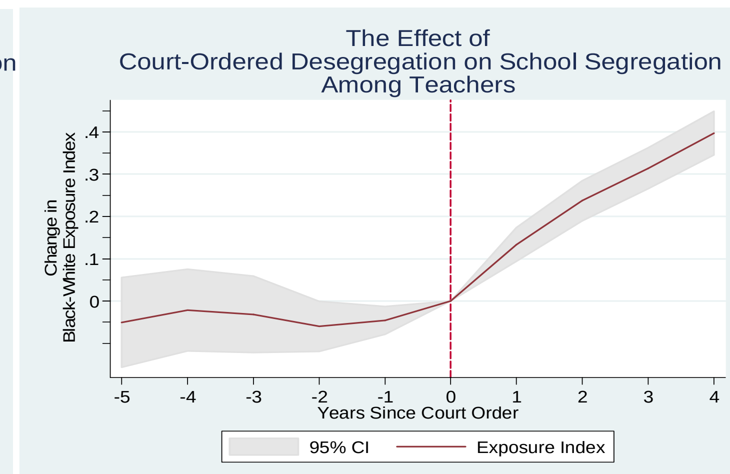


FIGURE 2B.



Data: Office of Civil Rights (OCR) School-level & School district-level Data, 1968-1972; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all school districts from OCR data that were ever subject to court-ordered desegregation (N=759 school districts; 3,324 school district-year observations).

Models: Results are based on event-study models that include school district fixed effects, year fixed effects, and controls at the county-level for the timing of hospital desegregation, roll-out of "War on Poverty" policy initiatives--community health centers, Head Start and Project Follow-Through. Models are weighted by 1968 district black student enrollment, so that estimates are representative of the impacts for the average black child; standard errors are clustered at the school district level.

FIGURE 3A.

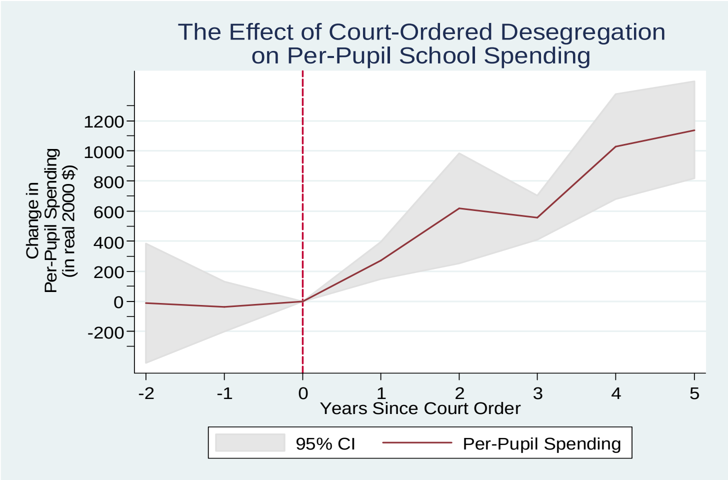


FIGURE 3B.

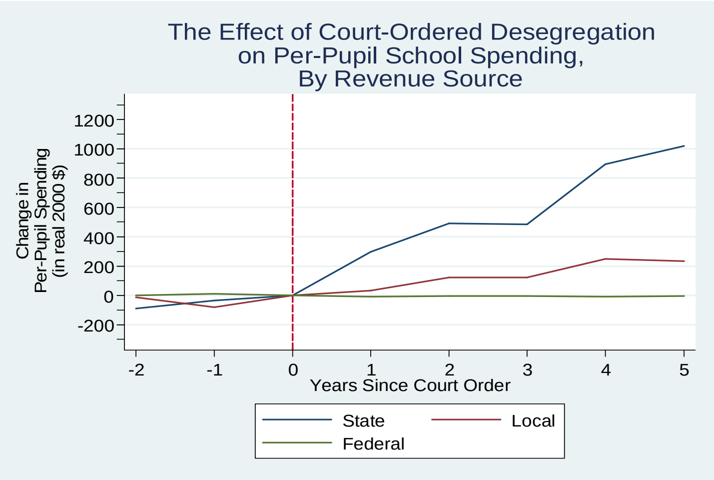


FIGURE 3C.

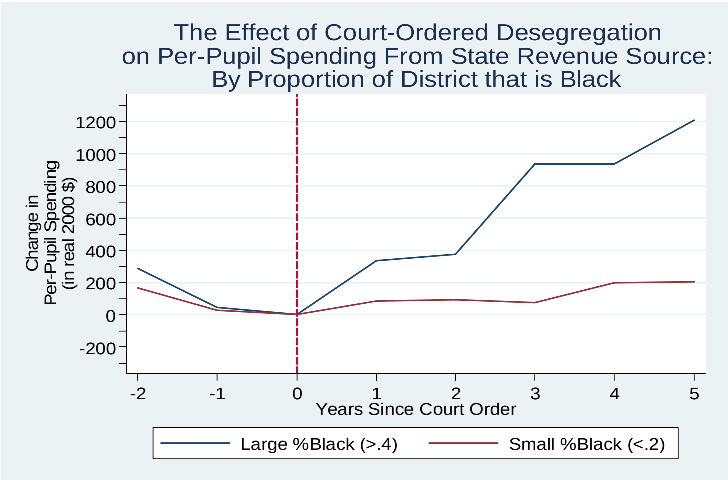
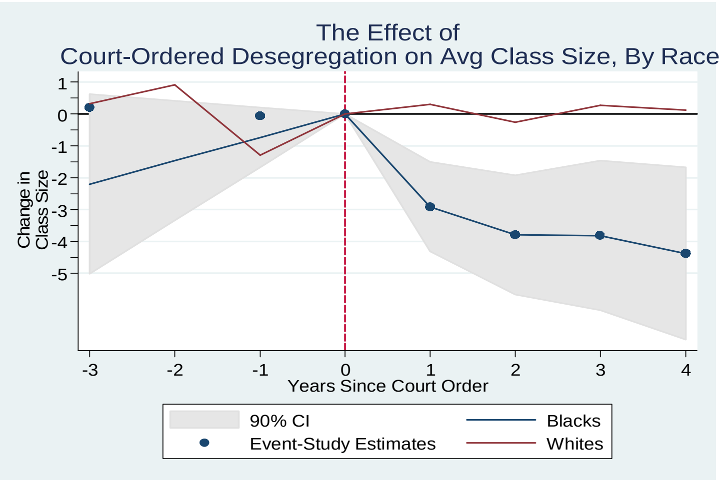


FIGURE 4.



Data-Figs3A-3C: Census of Governments (COG) School District Finance Data, 1962-1992; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all school districts from COG data that were ever subject to court-ordered desegregation (N=669 school districts; 13,933 school district-year observations).

Models-Figs3A-3C: Results are based on event-study models that include school district fixed effects, year fixed effects, census division-specific linear time trends, and controls at the county-level for the timing of hospital desegregation, roll-out of "War on Poverty" policy initiatives--community health centers, Head Start and Project Follow-Through--and controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election (proxy for segregationist preferences)) each interacted with linear time trends. Models are weighted by 1968 district student enrollment, so that estimates are representative of the impacts for the average child; standard errors are clustered at the school district level.

Data-Fig4: Office of Civil Rights (OCR) School-level Data, 1968-1972; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all schools from OCR data that were ever subject to court-ordered desegregation (N= 33,952 schools).

Models-Fig4: Results are based on non-parametric event-study models that include school fixed effects and year fixed effects. Models are weighted by 1968 school's race-specific student enrollment, so that estimates are representative of the impacts for the average black child and white child, respectively; standard errors are clustered at the school district level. Also shown are results of representative impacts for black children that use a parametric event-study model specification with pre-treatment linear time trend (with confidence interval), which include school FE and year FE.

FIGURE 5A.

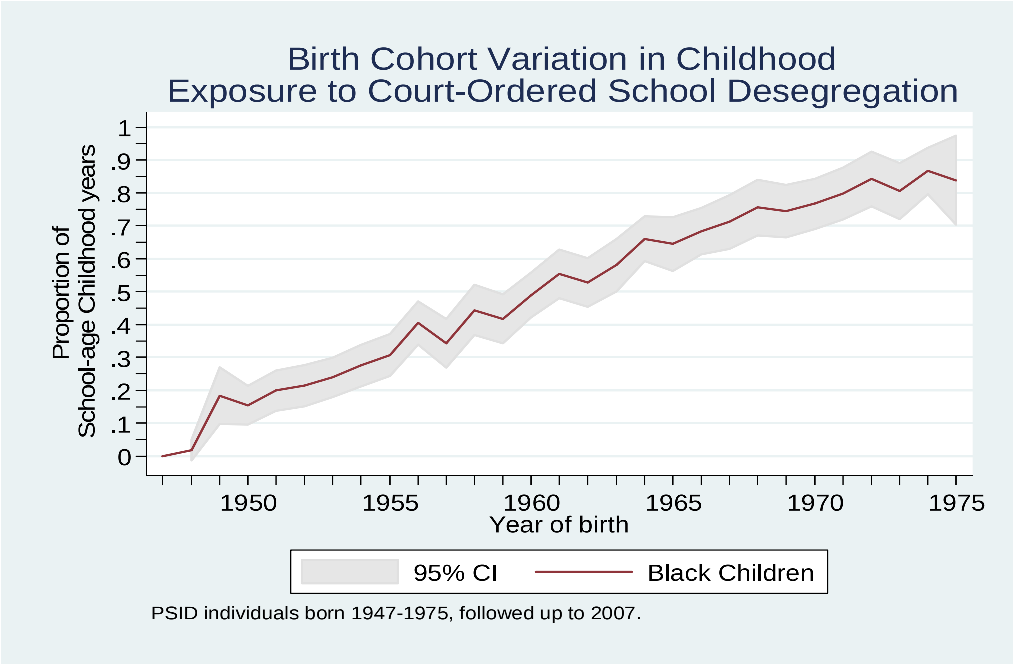


FIGURE 5B. CHILDHOOD EXPOSURE TO DESEGREGATION

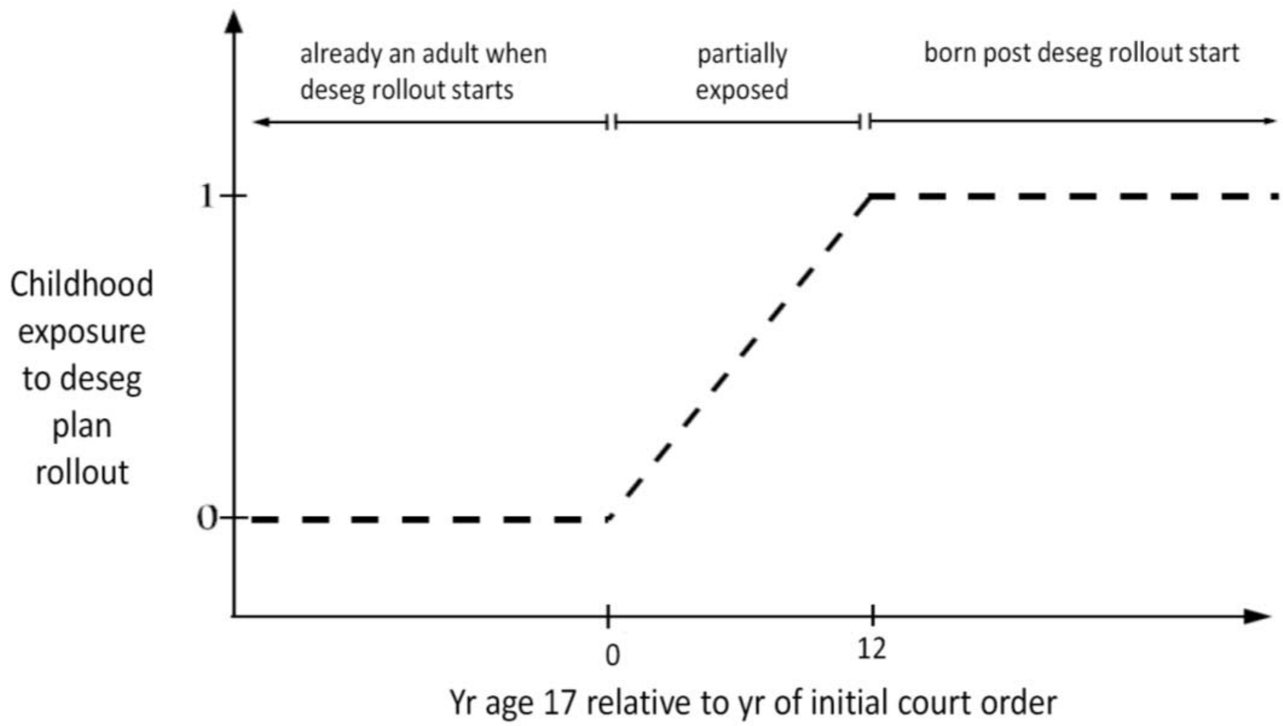


FIGURE 6A

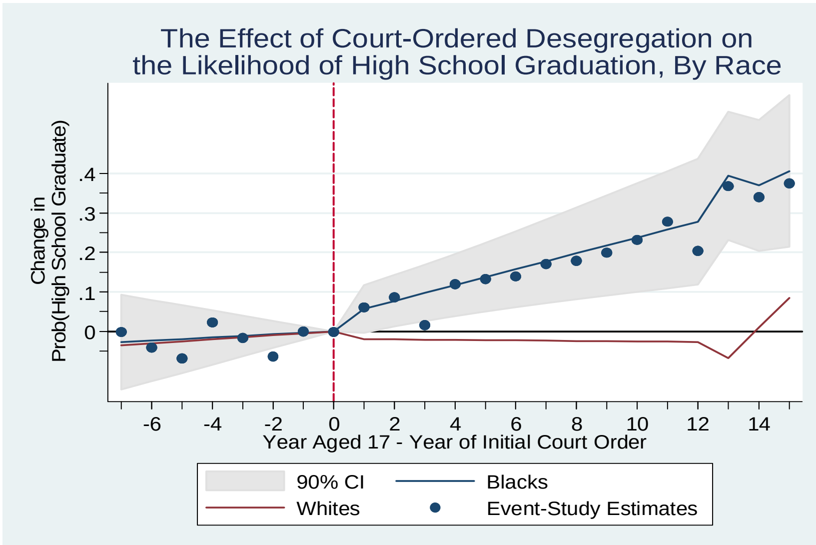
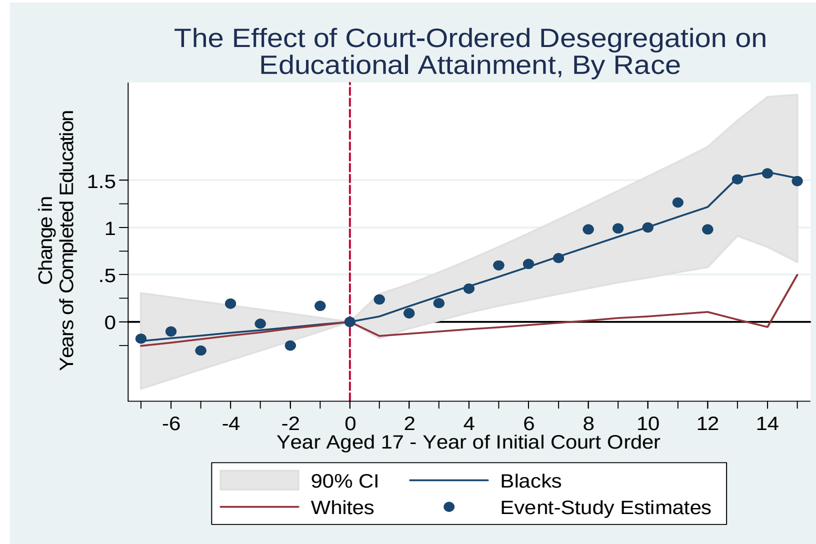


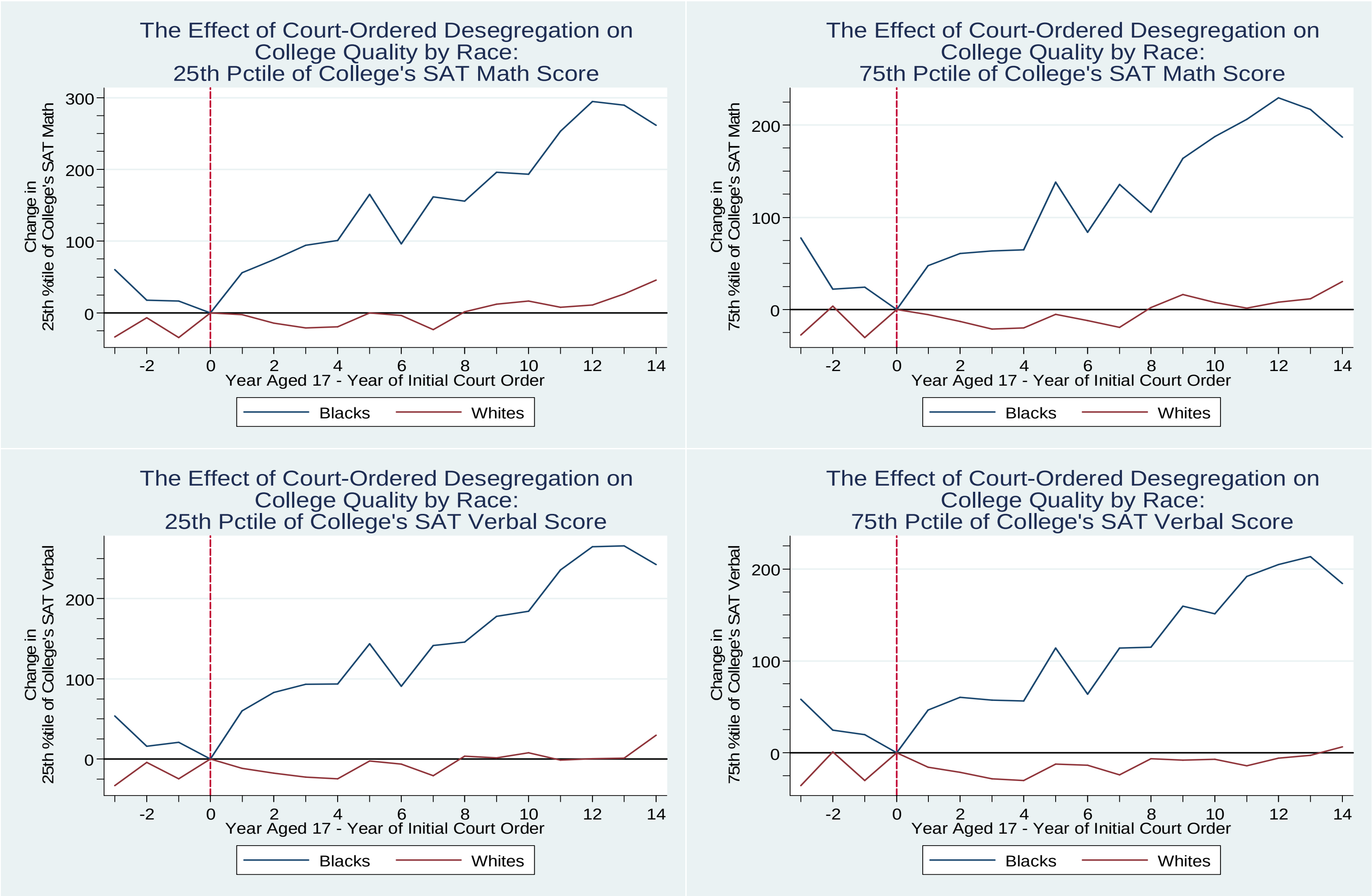
FIGURE 6B



<u>Data</u>: PSID geocode Data (1968-2011), matched with childhood school and neighborhood 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 2011, who grew up in school districts that were ever subject to court-ordered desegregation. (N=8,258 individuals from 3,495 childhood families, 627 school districts, 433 counties).

<u>Models</u>: Results are based on event-study models--both non-parametric and parametric (w/CI) estimates--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 stat 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 childhood county level. Results for whites not statistically significant from 0.

FIGURE 7.



Data: PSID geocode Data (1968-2011), 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 2011, 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, 254 counties).

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.

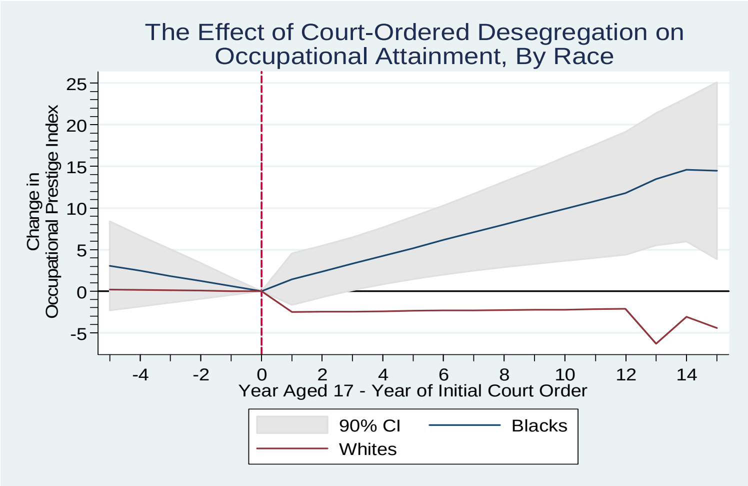


FIGURE 9A.

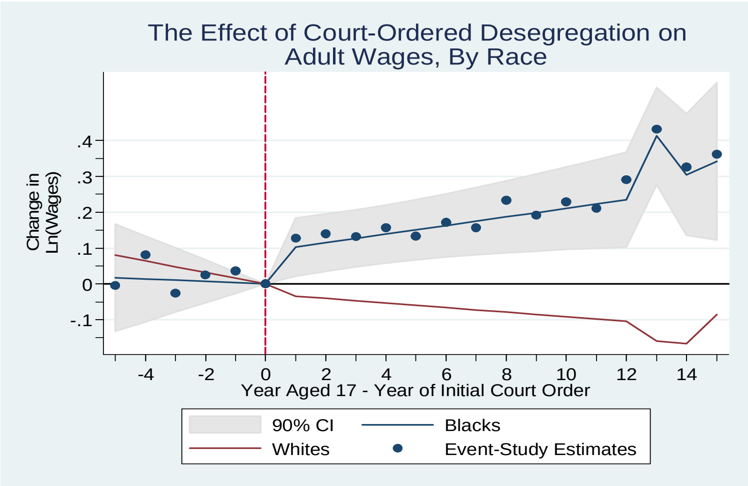
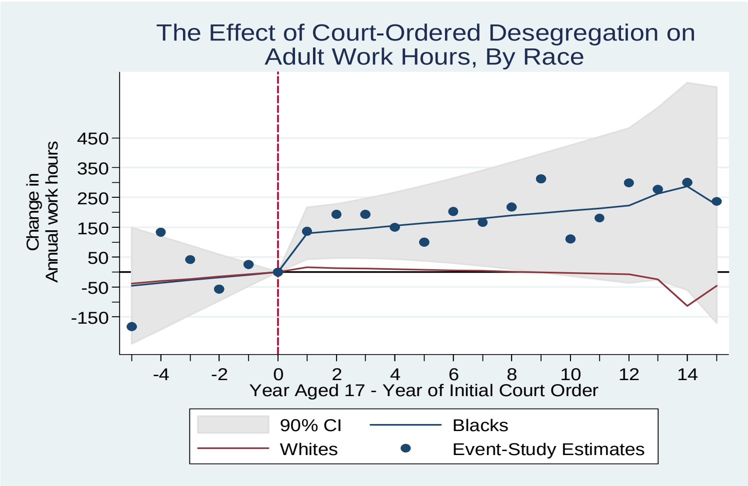


FIGURE 9B.



Data: Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All person-year observations (ages 25-45) are included except those in which individual was in school or pregnant/yrs immediately following childbirth. (N=55,670 person-year wage observations, 6,213 individuals from 2,723 childhood families, 583 school districts, 411 counties). (N=60,633 person-year work hours' observations, 6,472 individuals from 2,775 childhood families, 583 school districts, 412 counties). (N=6,905 individuals w/occupation info from 3,229 childhood families, 606 school districts, 419 counties).

Models: Results are based on event-study models--both non-parametric and parametric (w/CI) estimates--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 childhood county level. Results for whites not statistically significant from 0.

FIGURE 10A.

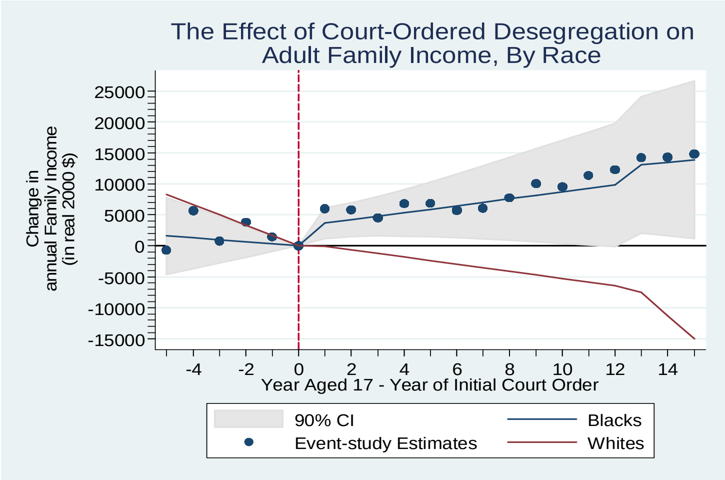
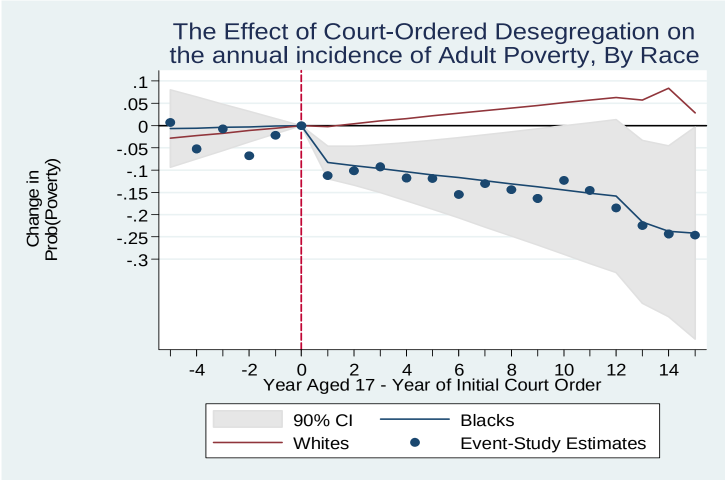


FIGURE 10B.



Data: Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All person-year observations (ages 25-45) are included except those in which individual was in school or pregnant/yrs immediately following childbirth. (N=75,233 person-year observations, 6,806 individuals from 2,847 childhood families, 599 school districts, 423 counties).

FIGURE 11A.

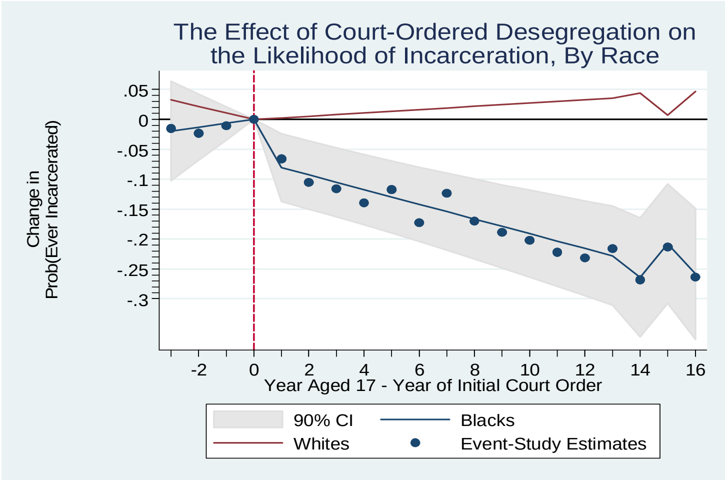
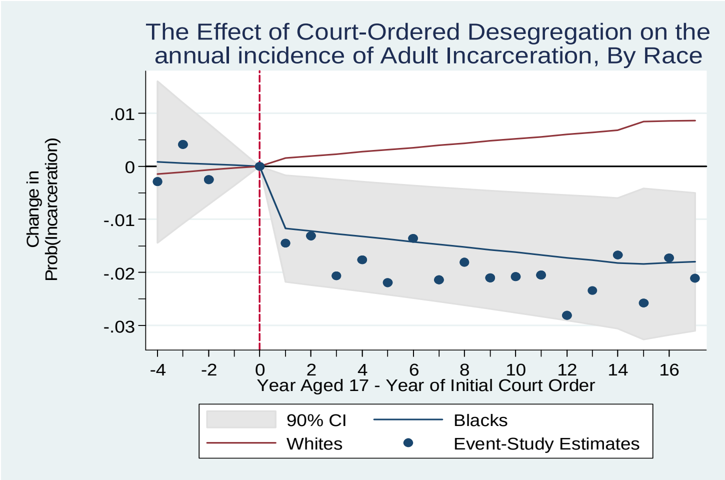
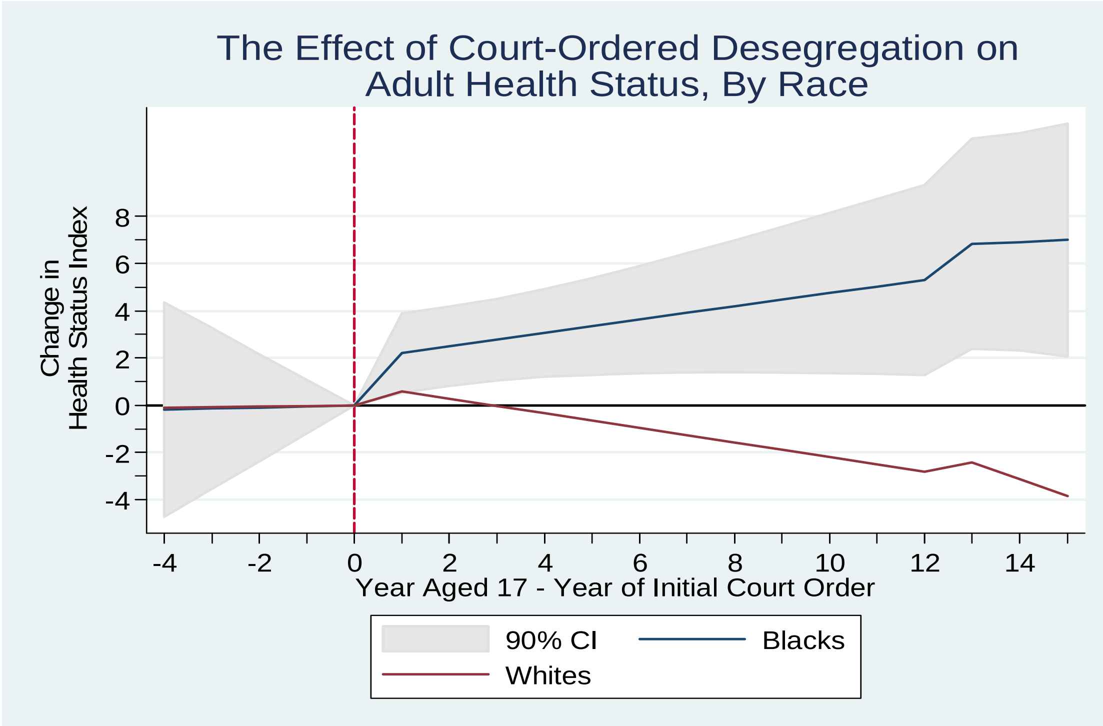


FIGURE 11B.



Data: 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 1968-2011 &, where available, 1995 svy reports of whether/when ever incarcerated. Models of annual incidence of adult incarceration include all person-year observations (ages 18-39). (N=100,143 person-year observations, 6,590 individuals from 2,383 child families, 465 school districts, 245 counties).
Models: Results are based on event-study models--both non-parametric and parametric (w/CI) estimates--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 childhood county level. Results for whites not statistically significant from 0.

FIGURE 12.



<u>Data</u>: PSID geocode Data (1968-2011), matched with childhood school and neighborhood 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 2011, who grew up in school districts that were ever subject to court-ordered desegregation. Health Status index (1-100(perfect health)) based on self-assessed health (E/VG/G/F/P), 1985-2011; interval regression model estimated, where E=[95,100]; VG=[85,95); G=[70,85); F=[30,70); P=[1,30). All person-year observations (ages 25-45) are included except those in which individual was pregnant/yrs immediately following childbirth. (N=45,284 person-year observations, 5,693 individuals from 2,555 childhood families, 565 school districts, 396 counties).

<u>Models</u>: Results are based on event-study models--both non-parametric and parametric (w/CI) estimates--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 childhood county level. Results for whites not statistically significant from 0.

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),** ages 25-45		**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$_{(age\ 5\text{-}17)}$	**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 on childhood county)

*** 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.

Table 2. Exploring the Mechanisms: School Spending vs Racial School Integration. 2SLS Estimates of Desegregation-Induced Effects of Per-Pupil Spending on Adult Socioeconomic Attainments by Race.

	Second Stage, Dependent variable:									
	Years of Education		**Ln(Wage),** age 25-45		**Annual Family Income,** age 25-45		**Annual Incidence of Adult Poverty: Prob(Poverty),** age 25-45		**Annual Incidence of Incarceration: Prob(Incarceration),** age 18-39	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Blacks	**Whites**	**Blacks**	**Whites**	**Blacks**	**White**	**Blacks**	**Whites**	**Blacks**	**Whites**
School District Per-pupil Spending$_{(age\ 5\text{-}17)}$	**1.4475***	0.1619	**0.5807****	-0.1851	**18,634.65***	17,045.85	**-0.3399***	-0.0758	**-0.0212***	-0.0102
	(0.8963)	(0.8841)	(0.2574)	(0.2301)	(9,183.16)	(21,066.61)	(0.1984)	(0.0594)	(0.0118)	(0.0166)
Black-White Exposure Index$_{(age\ 5\text{-}17)}$	-0.2810	-0.4774	-0.2952	0.1889	-8,077.57	6,608.08	0.2033	0.0713	-0.0093	0.0136
	(0.7693)	(1.6172)	(0.4580)	(0.3910)	(11,412.88)	(42,839.11)	(0.2395)	(0.0821)	(0.0119)	(0.0095)
Number of person-year observations	--	--	18,435	16,063	26,863	31,100	26,863	31,100	39,032	31,016
Number of individuals	4,291	2,611	2,289	1,651	2,630	2,611	2,630	2,611	2,581	1,920
Number of childhood families	1,458	1,328	904	878	966	1,328	966	1,328	792	896
Number of school districts	274	326	192	265	198	326	198	326	132	290
Number of childhood counties	213	186	148	175	153	186	153	186	81	137

Robust standard errors in parentheses (clustered at childhood county level)

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

Data: Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. Estimated district-specific desegregation induced-change in per-pupil spending (school segregation) are net of school district fixed effects, district-specific time trends, & coincident policy changes (see also Figures 1B, 3A). Key (instrumented) variables are defined such that a one-unit increase in "School District Per-pupil Spending$_{(age\ 5\text{-}17)}$" represents a $1,000 spending increase in each of one's K-12 years (roughly a standard deviation increase); and a one-unit increase in "Black-White Exposure Index $_{(age\ 5\text{-}17)}$" represents a 0.15 increase in the black-white exposure index or a standard deviation increase in racial school integration experienced in each of one's K-12 years.

Models: First-stage models, which are highly significant, include as predictors the # of school-age yrs of exposure to desegregation interacted with the respective district's desegregation-induced changes in school spending and racial school segregation, respectively; these are the excluded instruments for the school spending and segregation variables. Results are based on 2SLS models that include: school district fixed effects, race-specific yr of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for 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, % 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).

Appendix Table A0. Descriptive Statistics by Race

	Blacks (N=4,265)	Whites (N=3,993)
Adult Outcomes:		
High School Graduate	0.73	0.88
Years of Education	12.60	13.51
Ln(Wages), at age 30	2.26	2.63
Annual Work Hours, at age 30	1540.06	1895.99
Adult Family Income, at age 30	$31,020	$52,937
In Poverty, at age 30	0.24	0.05
Occupational Prestige Index	34.42	48.57
Ever Incarcerated, by age 30	0.08	0.04
Annual Incidence of Incarceration, at age 25	0.0063	0.0014
Fair/Poor Health, at age 30	0.11	0.05
Adult Health Status Index, at age 30	84.16	88.78
Age (range: 20-65)	32.7	34.3
Year born (range: 1945-1968)	1958	1957
Female	0.45	0.43
Childhood school variables:		
Per-pupil spending (avg, ages 5-17)	$3,508	$3,865
Black-White Dissimilarity Index (avg, ages 5-17)	0.58	0.49
Any court-ordered desegregation, age 5-17	0.68	0.57
# of exposure yrs to desegregation, age 5-17	5.58	4.22
1960 District Percent Black	26.18	12.13
1960 District Poverty Rate (%)	28.29	18.32
Childhood family variables:		
Income-to-needs ratio (avg, ages 12-17):	1.54	3.48
In poverty (%)	0.41	0.07
Mother's years of education	10.15	11.81
Father's years of education	9.21	11.74
Born into two-parent family	0.40	0.70
Childhood neighborhood variables:		
Neighborhood poverty rate	0.20	0.07
Residential segregation dissimilarity index$_{county}$	0.72	0.71

Note: All descriptive statistics are sample weighted to produce nationally-representative estimates of means. Dollars are CPI-U deflated in real 2000 $.

Table A1. Effects of Court-Ordered School Desegregation on Educational Attainment, by Race

	Dependent variable:			
	Probability(High School Graduate)		**Years of Education**	
	(1)	(2)	(3)	(4)
Exposure to Court-Ordered Desegregation	**Blacks**	**Whites**	**Blacks**	**Whites**
(Year aged 17 - Year of Initial Court Order), spline:				
(-7 to -1): (no exposure, linear trend prior to court order)	0.0039	0.0050	0.0291	0.0367
	(0.0104)	(0.0070)	(0.0440)	(0.0561)
>0: any exposure (dummy indicator)[1]	**0.1375*** **	**-0.0221**	**0.4800** **	**-0.0561**
	(0.0531)	(0.0364)	(0.1905)	(0.3095)
(1 to 12): # of school-age exposure years	**0.0201*** **	**-0.0007**	**0.1051*** **	**0.0232**
	(0.0072)	(0.0070)	(0.0332)	(0.0489)
≥13: exposed for all K-12 years (dummy indicator)	**0.0961** **	**-0.0404**	**0.2018**	**-0.1055**
	(0.0482)	(0.0659)	(0.1903)	(0.3004)
14: (beyond school-age years of exposure)	-0.0201	0.0781	0.0632	-0.0766
	(0.0429)	(0.0855)	(0.2609)	(0.3471)
15: (beyond school-age years of exposure)	0.0119	0.0577	-0.0043	0.4705
	(0.0544)	(0.0897)	(0.2643)	(0.3800)
Number of individuals	4,265	3,993	4,265	3,993
Number of childhood families	1,489	2,066	1,489	2,066
Number of school districts	332	498	332	498
Number of childhood counties	273	337	273	337

Robust standard errors in parentheses (clustered at childhood county level)

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

Footnote 1: The variable "*# of school-age exposure years* " is centered around 5 (i.e., *any exposure* *(*# of exposure yrs* - 5)), so that the coefficient on the "*any exposure* " dummy indicator can be interpreted as the average effect of 5 years of desegregation exposure.

Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All models 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). The models include dummy indicators for each event study year < -7 and each event study year > 15 -- the coefficients on these vars are suppressed to conserve space. See corresponding non-parametric & parametric event study model results presented in Figures 6A-6B.

Table A2. Effects of Court-Ordered School Desegregation on Adult Earnings, Work Hours, & Poverty by Race

	Dependent variable:		
	Ln(Wage), age 25-45	**Annual Work Hours** (include 0s), age 25-45	**Annual Incidence of Adult Poverty: Prob(Poverty),** age 25-45
Exposure to Court-Ordered Desegregation	(1)	(2)	(3)
(Main Effects apply to non-Hispanic Blacks)			
(Year aged 17 - Year of Initial Court Order), spline:			
(-7 to -1): (no exposure, linear trend prior to court order)	-0.0034	9.5284	0.0013
	(0.0181)	(23.5339)	(0.0105)
>0: any exposure (dummy indicator)[1]	**0.1516*****	**164.5327****	**-0.1101****
	(0.0506)	(76.4113)	(0.0470)
(1 to 12): # of school-age exposure years	**0.0123***	**8.6377**	**-0.0069**
	(0.0068)	(13.1312)	(0.0085)
≥13: exposed for all K-12 years (dummy indicator)	**0.1320*****	**35.2418**	**-0.0562*****
	(0.0388)	(67.2178)	(0.0207)
>13: (linear trend beyond school-age years of exposure)	-0.0157	7.1414	-0.0125
	(0.0215)	(28.8625)	(0.0111)
(-7 to -1)*White	-0.0130	-1.8093	0.0045
	(0.0231)	(27.1431)	(0.0108)
>0*White	**-0.2120*****	**-156.2084+**	**0.1325*****
	(0.0736)	(101.1304)	(0.0489)
(1 to 12)*White	**-0.0190+**	**-10.8483**	**0.0129+**
	(0.0134)	(16.3104)	(0.0087)
≥13*White	**-0.1452***	**-63.6052**	**0.0394**
	(0.0805)	(89.5761)	(0.0315)
>13*White	-0.0307	-39.5970	0.0209
	(0.0357)	(40.5326)	(0.0148)
Total Effect for Whites, spline:			
(-7 to -1)	-0.0163	7.719	0.0058
>0	**-0.0604**	**8.3243**	**0.0224**
(1 to 12)	**-0.0068**	**-2.2105**	**0.0061**
≥13	**-0.0132**	**-28.3635**	**-0.0168**
>13	-0.0465	-32.4555	0.0084
Number of person-year observations	55,670	60,633	75,233
Number of individuals	6,213	6,472	6,806
Number of childhood families	2,723	2,775	2,847
Number of school districts	583	583	599
Number of childhood counties	411	412	423

Robust standard errors in parentheses (clustered at childhood county level)

*** p<0.01, ** p<0.05, * p<0.10 (2-tailed test), +p<0.10 (one-tailed test)

Footnote 1: The variable "*# of school-age exposure years* " is centered around 5 (i.e., *any exposure* *(*# of exposure yrs* - 5)), so that the coefficient on the "*any exposure* " dummy indicator can be interpreted as the average effect of 5 years of desegregation exposure.

Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All models 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). Models include flexible controls for age (quadratic), survey year, and analyze adult economic outcomes for ages≤45 to avoid conflating birth cohort and life cycle effects. The models include dummy indicators for each event study year < -7 -- the coefficients on these vars are suppressed to conserve space. See corresponding non-parametric & parametric event study model results presented in Figures 9A-9B, 10B.

Table A3. Effects of Court-Ordered School Desegregation on Occupational Attainment, by Race

	Dependent variable: **Duncan Occupational Prestige Index**	
	(1)	(2)
Exposure to Court-Ordered Desegregation	**Blacks**	**Whites**
(Year aged 17 - Year of Initial Court Order), spline:		
(-7 to -1): (no exposure, linear trend prior to court order)	-0.6067	-0.0380
	(0.6483)	(0.6667)
>0: any exposure (dummy indicator)[1]	**5.1932****	**-2.3794**
	(2.2841)	(2.4883)
(1 to 12): # of school-age exposure years	**0.9396****	**0.0359**
	(0.3960)	(0.4913)
≥13: exposed for all K-12 years (dummy indicator)	**0.7646**	**-4.2289**
	(2.2752)	(3.6546)
14: (beyond school-age years of exposure)	1.1220	3.2320
	(2.7375)	(5.2054)
15: (beyond school-age years of exposure)	0.9936	1.9006
	(3.5974)	(3.8105)
Number of individuals	3,454	3,451
Number of childhood families	1,369	1,906
Number of school districts	304	483
Number of childhood counties	247	325

Robust standard errors in parentheses (clustered at childhood county level)
*** p<0.01, ** p<0.05, * p<0.10

Table A4. Effects of Court-Ordered School Desegregation on Adult Family Income, by Race

	Dependent variable: **Annual Family Income,** ages 25-45	
	(1)	(2)
Exposure to Court-Ordered Desegregation	**Blacks**	**Whites**
(Year aged 17 - Year of Initial Court Order), spline:		
(-7 to -1): (no exposure, linear trend prior to court order)	-316.1357	-1,664.411
	(754.0868)	(979.1625)
>0: any exposure (dummy indicator)[1]	**5,893.032****	**-2,364.477**
	(2,695.461)	(4,139.272)
(1 to 12): # of school-age exposure years	**561.3269**	**-576.8335**
	(514.7213)	(768.3236)
≥13: exposed for all K-12 years (dummy indicator)	**2,665.305+**	**-580.7366**
	(2,025.056)	(5,243.696)
>13: (linear trend beyond school-age years of exposure)	401.0598	-3,726.573
	(790.7464)	(2,345.883)
Number of person-year observations	38,429	37,179
Number of individuals	3,700	3,136
Number of childhood families	1,333	1,563
Number of school districts	256	480
Number of childhood counties	214	329

Robust standard errors in parentheses (clustered at childhood county level)
*** p<0.01, ** p<0.05, * p<0.10 (two-tailed test), +p<0.10 (one-tailed test)

Footnote 1: The variable "*# of school-age exposure years*" is centered around 5 (i.e., *any exposure**(*# of exposure yrs* - 5)), so that the coefficient on the "*any exposure*" dummy indicator can be interpreted as the average effect of 5 years of desegregation exposure.

Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All models include same set of controls as main specifications (as detailed in notes of Table 2). See corresponding non-parametric & parametric event study model results presented in Figures 8 & 10B.

Table A5. Effects of Court-Ordered School Desegregation on the Likelihood of Adult Incarceration, by Race

	Dependent variable:			
	Probability(Ever Incarcerated)		**Annual Incidence of Adult Incarceration: Prob(Incarcerated),** ages 18-39	
	(1)	(2)	(3)	(4)
Exposure to Court-Ordered Desegregation	**Blacks**	**Whites**	**Blacks**	**Whites**
(Year aged 17 - Year of Initial Court Order), spline:				
(-7 to -1): (no exposure, linear trend prior to court order)	0.0066	-0.0109	-0.0001	0.0004
	(0.0168)	(0.0200)	(0.0023)	(0.0008)
>0: any exposure (dummy indicator)[1]	**-0.1420*****	**0.0163**	**-0.0147****	**0.0039**
	(0.0378)	(0.0629)	(0.0065)	(0.0052)
(1 to 12): # of school-age exposure years	**-0.0123*****	**0.0028**	**-0.0005***	**0.0004**
	(0.0031)	(0.0093)	(0.0002)	(0.0006)
≥13: exposed for all K-12 years (dummy indicator)	**-0.0242**	**0.0056**	**0.0003**	**0.0011**
	(0.0361)	(0.0325)	(0.0026)	(0.0054)
>13: (linear trend beyond school-age years of exposure)			0.0001	0.0001
			(0.0010)	(0.0023)
14: (beyond school-age years of exposure)	0.0564	-0.0372		
	(0.0516)	(0.0394)		
15: (beyond school-age years of exposure)	0.0058	0.0027		
	(0.0475)	(0.0567)		
Number of person-year adult observations	--	--	59,390	40,753
Number of individuals	4,011	2,579	4,011	2,579
Number of childhood families	1,251	1,153	1,251	1,153
Number of school districts	208	373	208	373
Number of childhood counties	147	197	147	197

Robust standard errors in parentheses (clustered at childhood county level)

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

Footnote 1: The variable "*# of school-age exposure years* " is centered around 5 (i.e., *any exposure* *(*# of exposure yrs* - 5)), so that the coefficient on the "*any exposure* " dummy indicator can be interpreted as the average effect of 5 years of desegregation exposure.

Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All models 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). Models in columns (3) & (4) include flexible controls for age (quadratic). All models include dummy indicators for each event study year < -7 and each event study year > 15 -- the coefficients on these vars are suppressed to conserve space. See corresponding non-parametric & parametric event study model results presented in Figures 11A-11B.

Table A6. Effects of Court-Ordered School Desegregation on Adult Health Status, by Race

	Dependent variable:			
	Annual Incidence of Problematic Health: Prob(Fair/Poor Health), age 25-45		**Adult Health Status Index,** age 25-45 (Based on Interval Regression Model: 100pt-scale, 100=perfect health)	
	(1)	(2)	(3)	(4)
Exposure to Court-Ordered Desegregation	**Blacks**	**Whites**	**Blacks**	**Whites**
(Year aged 17 - Year of Initial Court Order), spline:				
(-7 to -1): (no exposure, linear trend prior to court order)	0.0033	0.0018	0.0481	0.0154
	(0.0111)	(0.0050)	(0.6896)	(0.2761)
>0: any exposure (dummy indicator)[1]	**-0.0471***	**0.0015**	**3.3401*****	**0.0108**
	(0.0279)	(0.0175)	(1.2434)	(1.1091)
(1 to 12): # of school-age exposure years	**0.0113**	**0.0028**	**0.2823+**	**-0.2360**
	(0.0040)	(0.0027)	(0.2141)	(0.1748)
≥13: exposed for all K-12 years (dummy indicator)	**-0.0190**	**-0.0102**	**1.2390**	**0.6428**
	(0.0201)	(0.0266)	(1.2612)	(1.3724)
>13: (linear trend beyond school-age years of exposure)	-0.0006	0.0077	0.0782	-0.6573
	(0.0095)	(0.0117)	(0.5276)	(0.5453)
Number of person-year adult observations	22,328	22,956	22,328	22,956
Number of individuals	2,958	2,735	2,958	2,735
Number of childhood families	1,154	1,432	1,154	1,432
Number of school districts	225	460	225	460
Number of childhood counties	189	313	189	313

Robust standard errors in parentheses (clustered at childhood county level)

*** p<0.01, ** p<0.05, * p<0.10 (2-tailed test); +p<.10 (one-tailed test)

Footnote 1: The variable "*# of school-age exposure years* " is centered around 5 (i.e., *any exposure* *(*# of exposure yrs* - 5)), so that the coefficient on the "*any exposure* " dummy indicator can be interpreted as the average effect of 5 years of desegregation exposure.

Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All models 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, gender), and flexible controls for age (quadratic). All models include dummy indicators for each event study year < -7 -- the coefficients on these vars are suppressed to conserve space. See corresponding event study model results presented in Figure 12.

Appendix Table A7. Falsification Tests Using Unsuccessful Desegregation Court Litigation: Placebo Effects on Adult Outcomes, by Race

	Dependent variable:							
	Probability (High School Graduate)	**Years of Education**	**Occupational Prestige Index**	**Probability (Ever Incarcerated)**	**Ln(Wage), ages 25-45**	**Ln(Family Income), ages 25-45**	**Probability (Poverty), ages 25-45**	**Adult Health Status Index, ages 25-45**
Years of Exposure to Unsuccessful Desegregation Court Litigation$_{(age\ 5\text{-}17)}$	0.0031	-0.0137	-0.2906	-0.0001	-0.0076	-0.0177	0.0046	0.0240
	(0.0044)	(0.0247)	(0.2561)	(0.0029)	(0.0056)	(0.0112)	(0.0039)	(0.1267)
Years of Exposure to Unsuccessful Desegregation Court Litigation*White	-0.0008	0.0182	0.2951	0.0009	0.0061	0.0144	-0.0059	-0.0086
	(0.0036)	(0.0267)	(0.3261)	(0.0012)	(0.0076)	(0.0132)	(0.0040)	(0.1472)
Number of person-year adult observations	--	--	--	--	54,139	72,191	72,191	54,139
Number of individuals	6,921	6,921	6,341	6,341	6,014	6,570	6,570	6,014
Number of childhood families	2,816	2,816	2,938	2,938	2,607	2,723	2,723	2,607
Number of school districts	613	613	602	602	591	613	613	591
Number of childhood counties	437	437	428	428	427	437	437	427

Robust standard errors in parentheses (clustered at childhood county level)
*** p<0.01, ** p<0.05, * p<0.10

Sample includes all PSID individuals born between 1945-1968, followed into adulthood through 2011, who grew up in school districts that had desegregation court litigation at some point b/w 1954-90 (desegregation court case data, American Communities Project). All models 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). Results in this table 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 black's adult socioeconomic & health attainments (see Tables 1-6).

Table A8. Interactive Effects of Court-Ordered School Desegregation & Induced-Change in Per-Pupil Spending on Educational Attainment, by Race

	Dependent variable:			
	Years of Education			
	(1)	(2)	(3)	(4)
Exposure to Court-Ordered Desegregation	**Blacks**		**Whites**	
(Year aged 17 - Year of Initial Court Order), spline:				
(-7 to -1): (no exposure, linear trend prior to court order)	0.0185	0.0226	0.0382	0.0458
	(0.0629)	(0.0648)	(0.0589)	(0.0559)
(-7 to -1)*↑ΔPer-Pupil Spending$_{(t-1,t+4)}$	-0.0433	-0.0423	0.0244	0.0236
	(0.0486)	(0.0498)	(0.0440)	(0.0442)
(-7 to -1)*↑ΔBlack-White Exposure Index$_{(t-1,t+4)}$		-0.0198		-0.0070
		(0.0249)		(0.0134)
>0: any exposure (dummy indicator)[1]	**0.4990****	**0.4362***	-0.1636	-0.1600
	(0.2414)	(0.2369)	(0.3462)	(0.3308)
(any exposure)*↑ΔPer-Pupil Spending$_{(t-1,t+4)}$	**0.3443***	**0.3587****	0.1274	0.1333
	(0.1827)	(0.1812)	(0.1759)	(0.1765)
(any exposure)*↑ΔBlack-White Exposure Index$_{(t-1,t+4)}$		-0.0203		-0.0315
		(0.0943)		(0.0719)
(1 to 12): # of school-age exposure years	**0.1021****	**0.1043****	0.0161	0.0162
	(0.0442)	(0.0419)	(0.0551)	(0.0558)
(# of exposure years)*↑ΔPer-Pupil Spending$_{(t-1,t+4)}$	-0.0282	-0.0222	-0.0265	-0.0259
	(0.0270)	(0.0288)	(0.0378)	(0.0380)
(# of exposure years)*↑ΔBlack-White Exposure Index$_{(t-1,t+4)}$		-0.0595***		-0.0029
		(0.0181)		(0.0174)
≥13: exposed for all K-12 years (dummy indicator)	**0.3984+**	**0.3821+**	-0.2249	-0.2278
	(0.2723)	(0.2898)	(0.3546)	(0.3577)
(exposed all K-12)*↑ΔPer-Pupil Spending$_{(t-1,t+4)}$	**0.3202+**	0.2512	0.1501	0.1421
	(0.02233)	(0.2361)	(0.3771)	(0.3797)
(exposed all K-12)*↑ΔBlack-White Exposure Index$_{(t-1,t+4)}$		0.0874		-0.1589
		(0.1694)		(0.2805)
Number of individuals	3,962	3,962	2,878	2,878
Number of childhood families	1,404	1,404	1,398	1,398
Number of school districts	312	312	457	457
Number of childhood counties	256	256	308	308

Robust standard errors in parentheses (clustered at childhood county level)

*** p<0.01, ** p<0.05, * p<0.10 (2-tailed test); +p<.10 (one-tailed test)

Footnote 1: The variable "*# of school-age exposure years* " is centered around 5
(i.e., *any exposure* *(*# of exposure yrs* - 5)), so that the coefficient on the "*any exposure* " dummy indicator can be interpreted as the average effect of 5 years of desegregation exposure. The estimated district-specific induced-change in per-pupil spending (school segregation) are net of school district fixed effects and district-specific time trends; these changes are centered around the respective average change ($1,000 for per-pupil spending; 0.15 for black-white exposure index) in the model, so that the main effects capture the average desegregation impact (see also Figures 1-3).

Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All models 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). The models include dummy indicators for each event study year < -7 and each event study year > 13 -- the coefficients on these vars are suppressed to conserve space.

Table A9. Exploring the Mechanisms. 2SLS Estimates of Desegregation-Induced Effects of Per-Pupil Spending on Black's Adult Socioeconomic Attainments: Placebo Tests for non-school ages

	Blacks Second Stage, Dependent variable:				
	Years of Education	**Ln(Wage),** age 25-45	**Annual Family Income,** age 25-45	**Annual Incidence of Adult Poverty: Prob(Poverty),** age 25-45	**Annual Incidence of Incarceration: Prob(Incarceration)** age 18-39
	(1)	(2)	(3)	(4)	(5)
School District Per-pupil Spending$_{(age\ 5\text{-}17)}$	**1.1841******	**0.5176*****	**13,732.27+**	**-0.2796***	**-0.0170+**
	(0.4522)	(0.2611)	(9065.39)	(0.1529)	(0.0126)
School District Per-pupil Spending$_{(age\ 20\text{-}24)}$	-0.4702	-0.0255	-16,010.87***	0.1740***	-0.0056
	(0.3285)	(0.1538)	(4,457.67)	(0.0538)	(0.0072)
School District Per-pupil Spending$_{(age\ 0\text{-}4)}$	0.5769	-0.1759	-23,013.99**	0.4341*	-0.0014
	(0.6352)	(0.4297)	(10,151.39)	(0.2259)	(0.0075)
Number of person-year observations	--	17,654	24,839	24,839	38,701
Number of individuals	3,951	2,204	2,457	2,457	2,565
Number of childhood families	1,341	875	916	916	781
Number of school districts	202	147	147	147	118
Number of childhood counties	138	102	102	102	63

Robust standard errors in parentheses (clustered at childhood county level)

*** p<0.01, ** p<0.05, * p<0.10 (2-tailed test); +p<.10 (one-tailed test)

Data: Sample includes all PSID black individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. The estimated district-specific desegregation induced-change in per-pupil spending is net of school district fixed effects, district-specific time trends, and coincident policy changes (see also Figure 3A). The key (instrumented) variables are defined such that a one-unit increase in "School District Per-pupil Spending$_{(age\ 5\text{-}17)}$" represents a $1,000 spending increase in each of one's K-12 years (roughly a standard deviation increase).

Models: The first-stage models, which are highly significant, include as predictors the # of years of exposure to desegregation (for relevant ages 5-17; 20-24; 0-4) interacted with the respective district's desegregation-induced change in school spending; these are the excluded instruments for the school spending variables. Results are based on 2SLS models that include: 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).

Appendix Table A10. Replicating Guryan (*AER* , 2004) **using PSID.**
Identification from Timing of Initial Court Orders (exogenous) **vs Timing of Major Desegregation Plan Implementation:**
Effects of Court-Ordered School Desegregation on Educational Attainment, by Race

	Dependent variable:					
	Probability(High School Graduate)					
	Replicating Guryan, Use Timing of Major Desegregation Plan Implementation				**Use Timing of Initial Court Orders**	
	(1)	(2)	(3)	(4)	(5)	(6)
Timing of Major Desegregation Plan Implementation:	**Whites**	**Blacks**			**Blacks**	
(Year aged 17 - Year of Major Plan Implementation)						
(-7 to -1): (no exposure, linear trend prior to major plan implementation)			-0.0151*	-0.0152+		
			(0.0082)	(0.0105)		
>0: any exposure (dummy indicator)[1]	**-0.0071**	**0.0419***	**0.0468***	**0.0460***		
	(0.0510)	(0.0244)	(0.0273)	(0.0267)		
(1 to 12): # of school-age exposure years				-0.0002		
				(0.0051)		
Exposure to Court-Ordered Desegregation:						
(Year aged 17 - Year of Initial Court Order), spline:						
(-7 to -1): (no exposure, linear trend prior to court order)					0.0039	0.0072
					(0.0104)	(0.0149)
>0: any exposure (dummy indicator)[1]					**0.1375***	**0.0667***
					(0.0531)	(0.0390)
(1 to 12): # of school-age exposure years					**0.0201***	**0.0195***
					(0.0072)	(0.0060)
≥13: exposed for all K-12 years (dummy indicator)					**0.0961****	**0.1001***
					(0.0482)	(0.0587)
14: (beyond school-age years of exposure)					-0.0244	-0.0261
					(0.0429)	(0.0616)
15: (beyond school-age years of exposure)					0.0119	0.0462
					(0.0544)	(0.0724)
Sample restricted to districts that overlap Welch/Light Deseg Data?	yes	yes	yes	yes	no	yes
Number of individuals	2,293	2,901	2,901	2,901	4,116	2,901
Number of childhood families	1,194	894	894	894	1,465	894
Number of school districts	194	120	120	120	326	120
Number of childhood counties	98	75	75	75	269	75

Robust standard errors in parentheses (clustered at childhood county level)

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

Footnote 1: In columns (4)-(6), the variable "*# of school-age exposure years* " is centered around 5 (i.e., *any exposure* *(*# of exposure yrs* - 5)), so that the coefficient on the "*any exposure* " dummy indicator can be interpreted as the average effect of 5 years of desegregation exposure.

Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All models 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). Models include dummy indicators for each event study year <-7 (columns 3-6) and each event study year > 15 (columns 4-6) -- the coefficients on these vars are suppressed to conserve space. See corresponding non-parametric & parametric event study model results presented in Figure 1.

Appendix Table A11. Using OCR-CCD District-level Data to Explore the Mechanisms: School Spending vs Racial School Integration.
2SLS Estimates of Desegregation-Induced Effects of Per-Pupil Spending on High School Dropout Rates.

	Second Stage, Dependent variable:	
	High School Dropout Rate (%)	
Pre-Desegregation:	(1)	(2)
(-7 to -1): (no exposure, linear trend prior to court order)	0.9629*	0.7778
	(0.5223)	(0.7073)
(-7 to -1)*↑ΔPer-Pupil Spending$_{(t-1,t+4)}$	3.2876***	3.2716***
	(0.9095)	(0.9037)
(-7 to -1)*↑ΔBlack-White Exposure Index$_{(t-1,t+4)}$		0.0313
		(0.2375)
Exposure to Court-Ordered Desegregation:		
(exposed)*↑ΔPer-Pupil Spending$_{(t-1,t+4)}$	**-5.3806***	**-5.5910***
	(3.0060)	(2.9516)
(exposed)*↑ΔBlack-White Exposure Index$_{(t-1,t+4)}$		3.6711
		(5.1750)
Number of district-year observations	3,066	3,066
Number of school districts	587	587

Robust standard errors in parentheses (clustered at school district level)
*** p<0.01, ** p<0.05, * p<0.10

<u>Data</u>: Analysis uses school district-level data from OCR-CCD spanning 1972-1992 linked with desegregation timing, among districts that were ever subject to court-ordered desegregation. These models are weighted by (pre-desegregation) district % of enrollment that is black to attempt to capture average effects for black children. The estimated district-specific desegregation induced-change in per-pupil spending (school segregation) are net of school district fixed effects and district-specific time trends. The key variables are defined such that a one-unit increase in "School District Per-pupil Spending" represents a $1,000 spending increase (roughly a standard deviation increase); and a one-unit increase in "Black-White Exposure Index" represents a 0.15 increase in the black-white exposure index or a standard deviation increase in racial school integration; these changes are centered around the respective average change ($1,000 for per-pupil spending; 0.15 for black-white exposure index) in the model, so that the coefficient on the main pre-desegregation trend term captures the average linear trend in high school dropout rates leading up to the court order.

<u>Models</u>: Results are based on 2SLS models that include: school district fixed effects, year fixed effects, census division-specific linear trends; controls at the county-level for the timing of hospital desegregation, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start, food stamps, medicaid, AFDC, UI, Title-I, 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 trends. The first-stage models, which are significant, include as predictors desegregation exposure interacted with the respective district's desegregation-induced changes in school spending and racial school segregation, respectively.

Appendix Table A12. Effects of Desegregation Exposure on Blacks' Adult Outcomes & the Returns to Education

	Years of Education	**Ln(Wage),** age 25-45	**Annual Work Hours,** age 25-45	**Probability (Poverty),** age 25-45	**Annual Family Income,** age 25-45	**Occupational Prestige Index**	**Probability (Ever Incarcerated)**	**Probability (Incarcerated),** age 18-39	**Probability (Fair/Poor Health),** age 25-45	**Adult Health Status Index,** age 25-45
5-Year Exposure to Desegregation	**0.4800****	**0.1516*****	**164.5327****	**-0.1101****	**5,893.032****	**5.1932****	**-0.1420*****	**-0.0147****	**-0.0471***	**3.3401*****
	(0.1905)	(0.0506)	(76.4113)	(0.0470)	(2,695.461)	(2.2841)	(0.0378)	(0.0065)	(0.0279)	(1.2434)
Implied Wald Estimate of Returns to Education (quantity/quality)	--	0.3158	342.7765	0.2294	$12,277	10.8192	0.2958	0.0306	0.0981	6.9585
Mean for Blacks (at age 30)	12.60	2.26	1,540.06	0.24	$31,020	34.42	0.08	0.0063	0.11	84.16
Mean for Whites (at age 30)	13.51	2.63	1,895.99	0.05	$52,937	48.57	0.04	0.0014	0.05	88.78

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

This sumary table contains the main results for blacks based on estimates shown in Tables 1-6. Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All models 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).

Appendix Table A13: Determinants of the Timing of Court-Ordered School Desegregation Using 1962 County Characteristics

	Dependent variable:							
	Initial Year of Court Order						**Delay b/w Initial Court Order & Major Desegregation Plan Implementation** (years)	
1962 County variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log population	-0.8040***	-0.8541***	-0.1439	0.4198	-1.3639	-1.9489*	1.1884	1.3207
	(0.2768)	(0.2847)	(0.8200)	(0.8907)	(1.0195)	(1.0794)	(0.9768)	(1.1221)
Percent minority, spline (< 20)	0.0877*	0.0858*	-0.1660	-0.1629	-0.1791	-0.0635	0.2001	0.1527
	(0.0449)	(0.0450)	(0.1486)	(0.1489)	(0.2081)	(0.2123)	(0.1943)	(0.2085)
Percent minority, spline (≥ 20)	-0.0159	-0.0182	-0.0322	0.0026	-0.1762	-0.1913	0.5389**	0.5381**
	(0.0253)	(0.0252)	(0.1125)	(0.1136)	(0.2520)	(0.2547)	(0.2359)	(0.2568)
Per-capita school spending ($000s)	0.0082		0.5960		-2.3282		5.4804**	
	(0.0162)		(1.3015)		(2.1433)		(2.2330)	
% of school spending revenue from state/fed govt	-0.0899***	-0.0940***	-0.1298**	-0.1043	-0.0833	-0.0805	0.0684	0.0758
	(0.0186)	(0.0191)	(0.0655)	(0.0666)	(0.0879)	(0.0877)	(0.0825)	(0.0877)
Student-to-teacher ratio		-0.0039		-0.2896		0.1965		-0.3806
		(0.0311)		(0.1787)		(0.1867)		(0.2894)
Average teacher salary		0.0005		-0.0020		0.0021		0.0014
		(0.0006)		(0.0015)		(0.0019)		(0.0019)
Median income	-0.0002	-0.0002	-0.0034	-0.0033	0.0086	0.0062	-0.0207***	-0.0210***
	(0.0015)	(0.0014)	(0.0043)	(0.0044)	(0.0067)	(0.0069)	(0.0065)	(0.0070)
% of households with income <$3,000	0.0713	0.0761	0.1065	0.1170	0.8007	0.4575	-2.5174***	-2.4205***
	(0.1005)	(0.0996)	(0.3589)	(0.3594)	(0.6187)	(0.6321)	(0.5757)	(0.6244)
% of households with income > $10,000	0.1178	0.1065	-0.0208	0.0416	-0.0672	-0.0378	0.8514+	0.9291
	(0.1377)	(0.1380)	(0.3786)	(0.3807)	(0.7080)	(0.7071)	(0.6280)	(0.6656)
% of adults with 12 or more years of education	0.0877**	0.0903**	0.2574**	0.1992*	-0.2369	-0.1699	-0.0071	0.0009
	(0.0393)	(0.0396)	(0.1070)	(0.1116)	(0.1660)	(0.1732)	(0.1606)	(0.1788)
1950-60 population change	0.0050	0.0051	-0.0232	-0.0191	-0.0016	-0.0041	-0.0184	-0.0159
	(0.0088)	(0.0088)	(0.0177)	(0.0175)	(0.0216)	(0.0215)	(0.0220)	(0.0232)
% of residents in urban areas	0.0060	0.0058	-0.0437	-0.0402	0.0339	0.0282	-0.0199	-0.0150
	(0.0137)	(0.0137)	(0.0595)	(0.0591)	(0.1150)	(0.1145)	(0.1147)	(0.1214)
% of residents in rural or farm area	0.0352	0.0361	0.1822	0.1970	0.2554	0.3849	0.5533	0.4997
	(0.0248)	(0.0256)	(0.1279)	(0.1281)	(0.4184)	(0.4209)	(0.4473)	(0.4840)
% living in group quarters	0.0617	0.0568	0.1397	0.1957	0.3980	0.3673	-0.1526	-0.2322
	(0.0534)	(0.0586)	(0.2185)	(0.2196)	(0.2847)	(0.2860)	(0.2866)	(0.3074)
Median age	-0.4279**	-0.4281**	-1.3912***	-1.4594***	-0.4847	-0.2984	-0.3123	-0.1917
	(0.1754)	(0.1747)	(0.5256)	(0.5283)	(1.0443)	(1.0532)	(1.0220)	(1.0951)
% of residents who are school-age (5-20)	-0.2907	-0.2933	-2.2507***	-2.4145***	-0.9571	-0.5218	0.1894	0.1512
	(0.1894)	(0.1911)	(0.6443)	(0.6489)	(1.1669)	(1.2006)	(1.1408)	(1.2355)
% of residents who are elderly (65+)	0.2258	0.2209	0.1049	-0.0283	0.7359	0.6766	0.0935	0.0097
	(0.2039)	(0.2046)	(0.6581)	(0.6616)	(0.8173)	(0.8171)	(0.8227)	(0.8788)
% who voted for incumbent President	0.0615	0.0508	0.2834**	0.3241**	0.0059	-0.0241	0.0204	0.0579
	(0.0444)	(0.0468)	(0.1237)	(0.1252)	(0.1801)	(0.1830)	(0.1636)	(0.1818)
Mortality rate (annual deaths per 10,000 residents)	-0.6088	-0.6125	-16.0529*	-13.7160	-14.4197	-11.1113	5.1065	2.7650
	(1.8752)	(1.8842)	(9.0305)	(9.0891)	(14.2740)	(14.1562)	(14.5443)	(15.3410)
Region controls?	yes	yes	yes	yes	yes	yes	yes	yes
Full sample?	yes	yes	no	no	no	no	no	no
Subsample that overlaps PSID original sample kids?	no	no	yes	yes	yes	yes	yes	yes
Subsample with desegregation implementation dates?	no	no	no	no	yes	yes	yes	yes
Observations	616	616	161	161	62	62	62	62

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10

Data: 1962 Census of Governments, City & County Data Book; Desegregation court case data compiled by legal scholars for American Communities Project/Brown University; Major desegregation plan implementation dates obtained from Welch/Light data.

Appendix Table A14. Robustness check to address concern of potential endogenous migration: similar results when sample restricted to individuals who lived in childhood residence prior to initial court order.
School Desegregation Effects on Educational Attainment, by Race

	Dependent variable:	
	Probability (High School Graduate)	**Years of Education**
Exposure to Court-Ordered Desegregation	(1)	(2)
(Main Effects apply to non-Hispanic Blacks for post-'64 court orders)		
(Year aged 17 - Year of Initial Court Order), spline:		
<0 (no exposure, linear trend prior to court order)	0.0031	-0.0078
	(0.0145)	(0.0876)
(0 to 12)	**0.0192+**	**0.1508****
	(0.0141)	(0.0694)
>12 (beyond school-age years of exposure)	-0.0320	-0.0011
	(0.0236)	(0.1414)
<0*White	0.0209**	-0.0655
	(0.0081)	(0.0648)
(0 to 12)*White	-0.0129*	-0.1328*
	(0.0078)	(0.0683)
>12*White	0.0396	0.0690
	(0.0261)	(0.1846)
Total Effect for Whites, spline:		
<0	0.0240	-0.0733
(0 to 12)	**0.0063**	**0.0180**
>12	0.0076	0.0679
District ever court order indicator*year of birth FE?	yes	--
Subsample who grew up in districts ever under court order?	no	yes
Number of individuals	5,280	2,594
Number of childhood families	1,946	925
Number of childhood neighborhoods	1,369	704
Number of school districts	516	205

Robust standard errors in parentheses (clustered at childhood county level)
*** p<0.01, ** p<0.05, * p<0.10 (2-tailed test); + p<0.10 (1-tailed test)

Sample includes original sample PSID children born between 1945-1968 who lived in their childhood residence prior to the initial desegregation court order. All models include same set of controls as main specifications.

Appendix Table A15. Additional Specifications:
Similar Estimated Effects of Desegregation in South and Non-South

	Dependent variable: **Years of Education**
(Main Effects apply to Blacks)	
Years of Exposure to Court-Ordered Desegregation$_{(age\ 5\text{-}17)}$	**0.1049****
	(0.0424)
Years of Exposure to Court-Ordered Desegregation$_{(age\ 5\text{-}17)}$*South	-0.0108
	(0.0528)
Years of Exposure to Court-Ordered Desegregation$_{(age\ 5\text{-}17)}$*White	-0.0618
	(0.0501)
Years of Exposure to Court-Ordered Desegregation$_{(age\ 5\text{-}17)}$*South*White	-0.0391
	(0.0494)
Number of individuals	8,258
Number of childhood families	3,495
Number of school districts	627
Number of childhood counties	433

***p<0.01, **p<0.05, *p<0.10; Robust standard errors in parentheses (clustered at childhood county level)

Model includes same sample and set of control variables as in Table 1.

Figure A0.

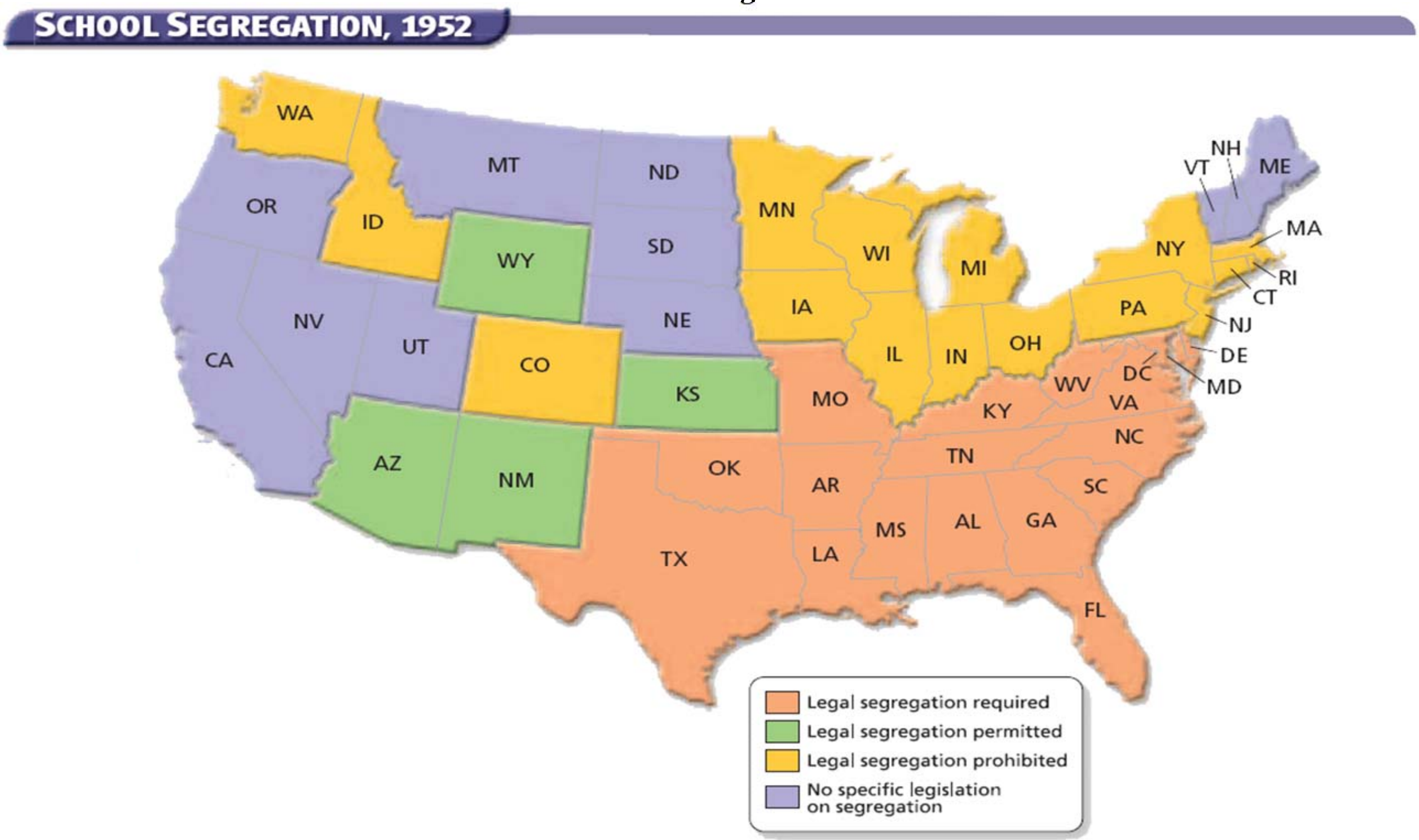
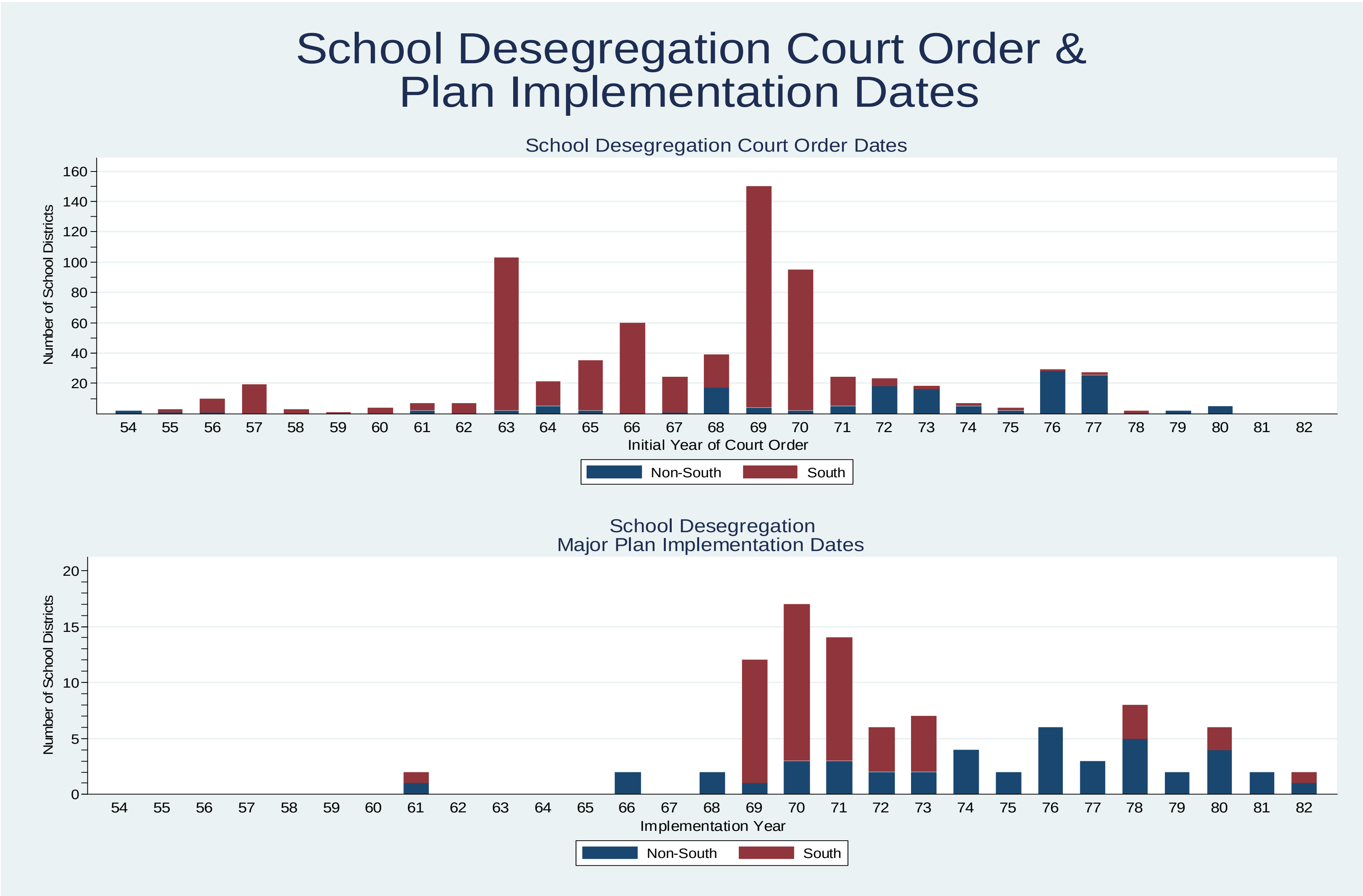

SCHOOL SEGREGATION, 1952
WA
OR
CA
ID
NV
MT
WY
UT
AZ
CO
NM
ND
SD
NE
KS
OK
TX
MN
IA
MO
AR
LA
WI
IL
MI
IN
OH
KY
TN
MS
AL
GA
FL
SC
NC
VA
WV
PA
NY
VT
NH
ME
MA
RI
CT
NJ
DE
DC
MD
Legal segregation required
Legal segregation permitted
Legal segregation prohibited
No specific legislation on segregation

Figure A1.



(1)Desegregation Court Case Data: universe of districts ever subject to court orders (N=868), Brown Univ/American Communities Project. (2)Major Plan Implementation Dates: Welch/Light data from 125 large school districts.

FIGURE A2. GEOGRAPHIC TIMING OF COURT-ORDERED SCHOOL DESEGREGATION

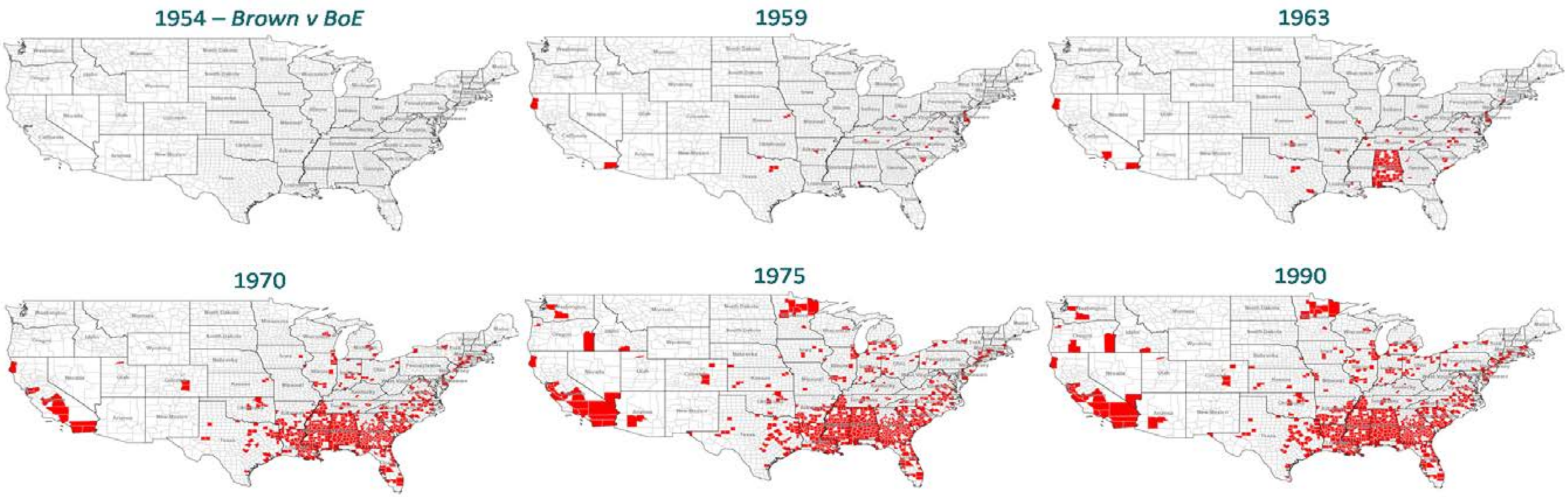
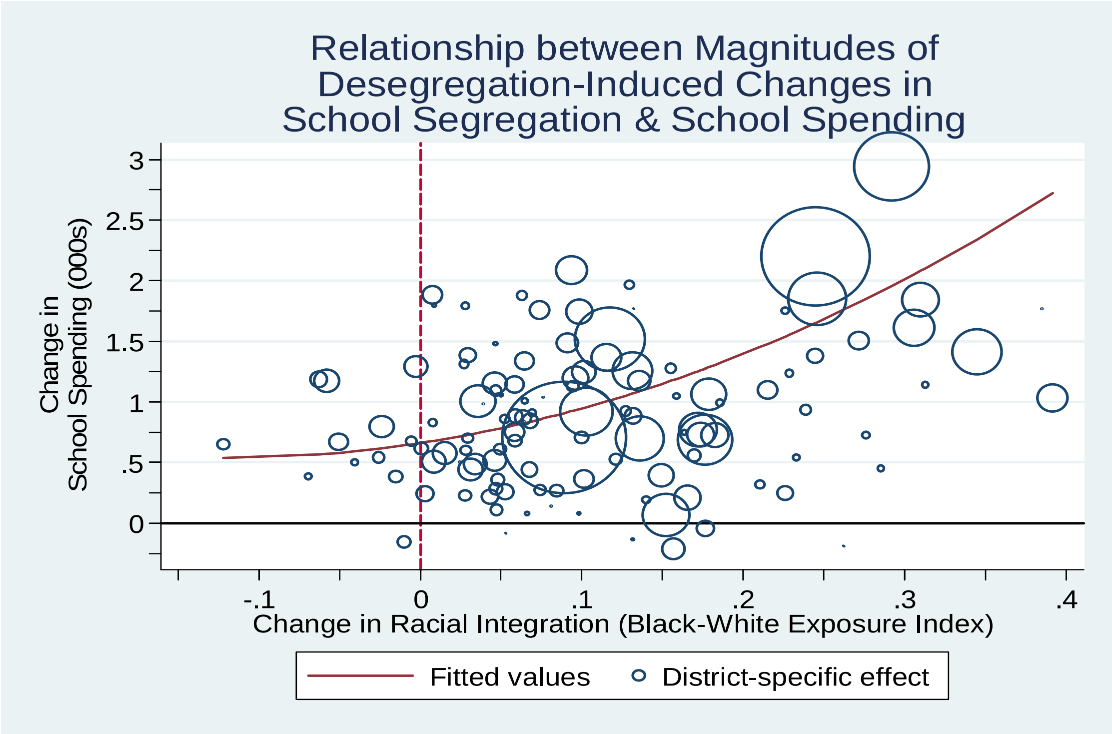


FIGURE A3.



<u>Note</u>: I find that the main predictor of desegregation-induced changes in school spending is pre-treatment (1960) District % black, not 1960 county poverty rates & other factors.
Furthermore, I find that the desegregation-induced changes in per-pupil spending & racial school integration are similar in districts that overlap the PSID sample vs the full universe of court-ordered districts. This lends further support to the representativeness of the PSID & generalizability of results for these birth cohorts.

Figure A4.

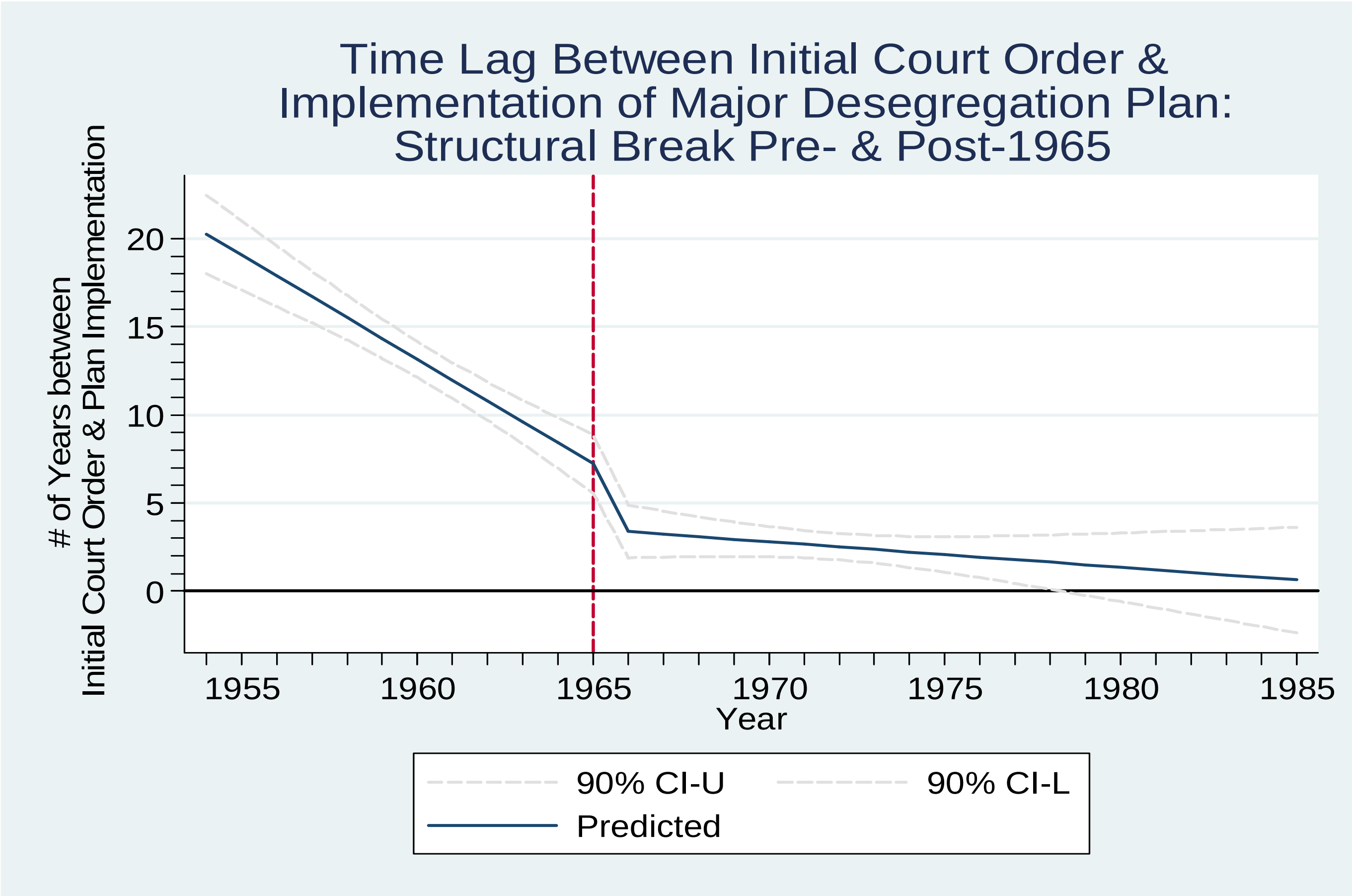

Time Lag Between Initial Court Order &
Implementation of Major Desegregation Plan:
Structural Break Pre- & Post-1965
# of Years between
Initial Court Order & Plan Implementation
0
5
10
15
20
1955
1960
1965
1970
1975
1980
1985
Year
90% CI-U
90% CI-L
Predicted

Figure A5.

Geographic Variation in School Spending in the U.S. in 1962

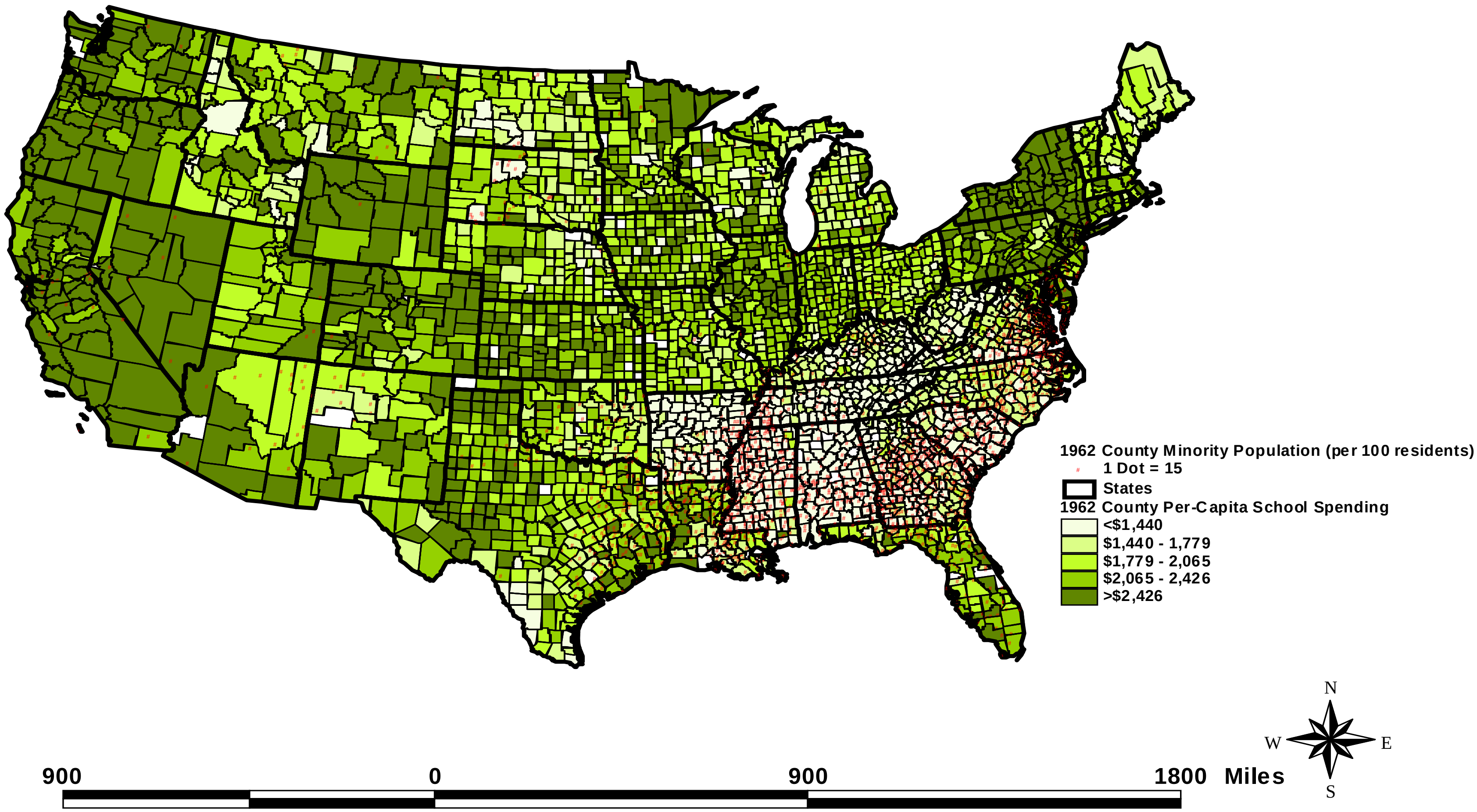


Figure A6.

The Geographic Timing of Court-Ordered School Desegregation in the U.S.

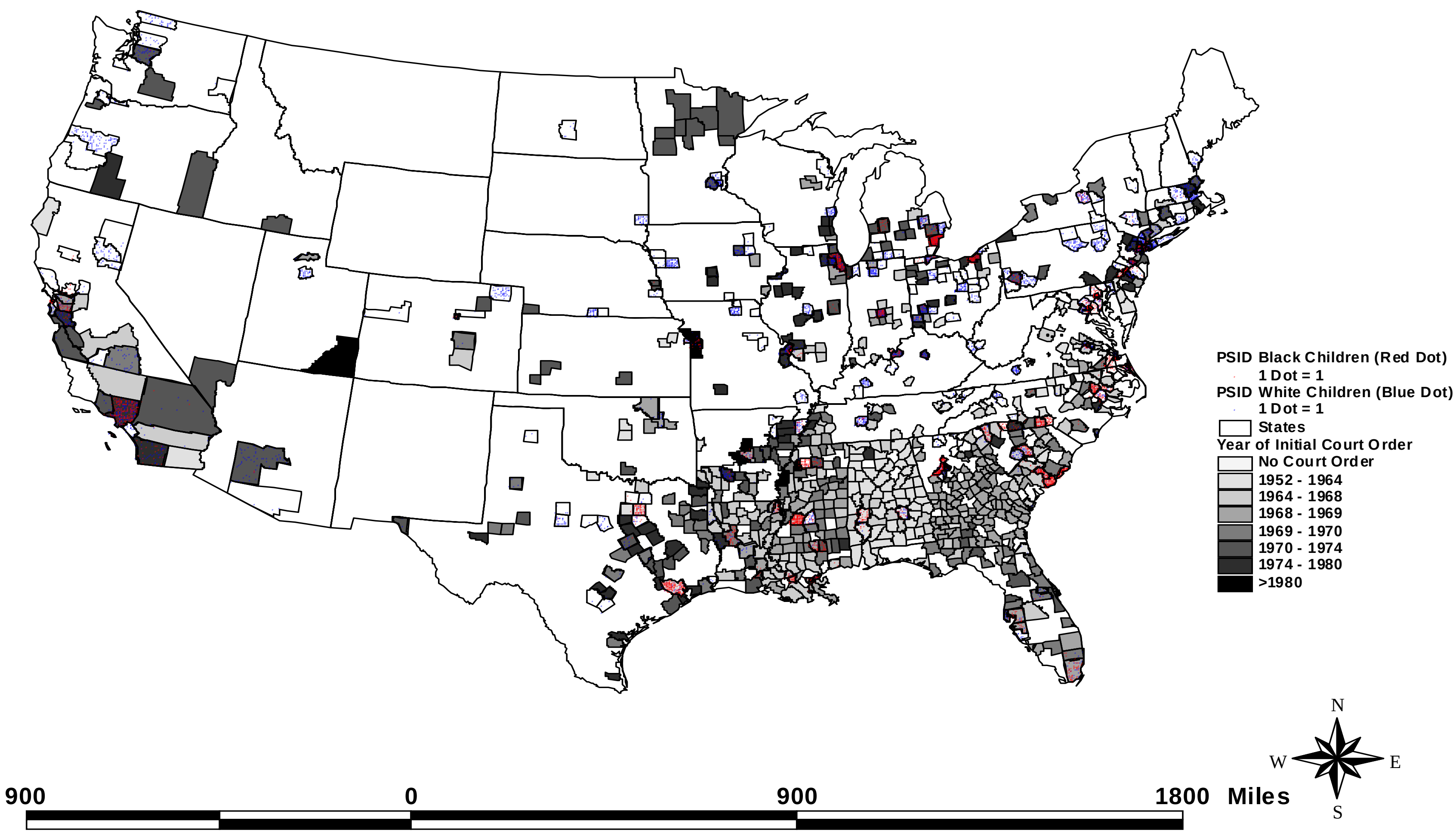
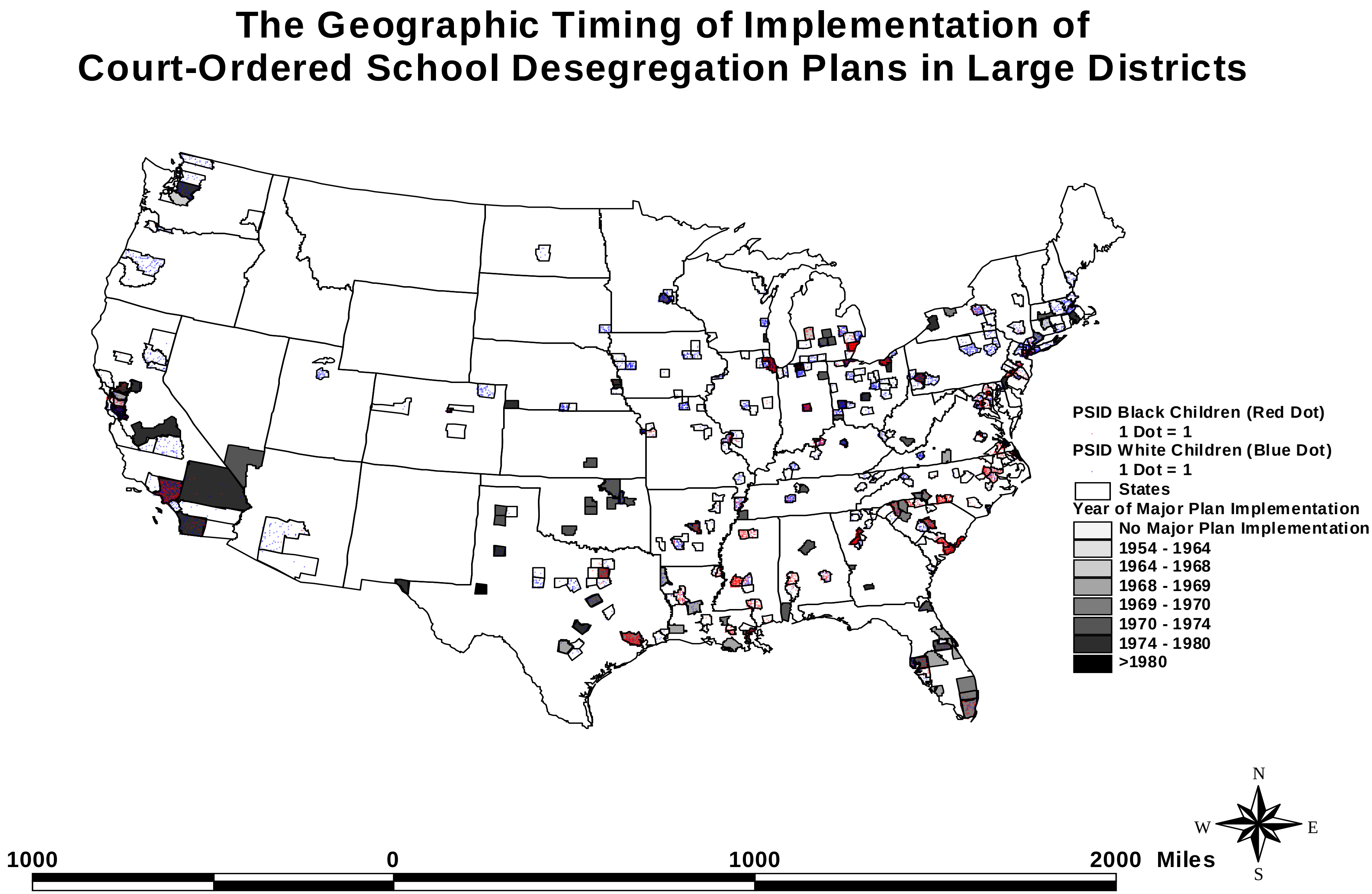


Figure A7.


The Geographic Timing of Implementation of
Court-Ordered School Desegregation Plans in Large Districts
PSID Black Children (Red Dot)
1 Dot = 1
PSID White Children (Blue Dot)
1 Dot = 1
States
Year of Major Plan Implementation
No Major Plan Implementation
1954 - 1964
1964 - 1968
1968 - 1969
1969 - 1970
1970 - 1974
1974 - 1980
>1980
N
W
E
S
1000
0
1000
2000 Miles

Appendix A: Data Sources

A. *Desegregation Court Case Data*

The desegregation court case data contains the universe of desegregation court cases in the US from 1954-90 assembled by the team of legal scholars for The American Community Project in association with Brown University (directed by John Logan). Every court case is coded according to whether it involved segregation of students across schools, whether the court required a desegregation remedy, and what was the main component of the desegregation plan. Multiple sources were used to compile the comprehensive desegregation case inventory. Every case was checked against legal databases, including Westlaw, to confirm the name of the case, the school districts involved, whether the case actually covered the issue of school segregation, whether there was a court-ordered plan, the type of desegregation plan, and the year of the initial court order. The resultant case inventory is significantly more comprehensive than the one obtained by use of data in Welch and Light (1987) alone. The total case inventory includes 358 court cases, which resulted in desegregation plans involving 868 school districts.

Structure of Data & Information Compiled for each Court Case:

- **Case Name:**
- **Year of Initial Decision:**
- **Did the case relate to school segregation?**
- **Did the court require a desegregation plan, affirm an existing plan, or refer to a previous case requiring a plan?**
- **If so, what did the plan require?**
- **Description of Court Case:**
- **Current status of this court case, or if there was a plan, the status of the plan (if known):**
- **Year of Current status:**
- **Was there a U.S. Department of Health, Education and Welfare (HEW) action?**
- **Year of HEW Action:**
- **Description of HEW Action:**

B. *Desegregation Plan Implementation Data*

I augment this data with major desegregation plan implementation information in large school districts originally compiled by Welch and Light (1987). Welch/Light investigated desegregation histories of 125 mostly large school districts. Welch and Light (1987) report the year in which school desegregation was implemented for each school district. The Welch/Light data cover all districts that in 1968 were 20 to 90 percent minority with enrollments of 50,000+, and a random sample of districts that were 10-90 percent minority with enrollments of between 15,000-50,000.

C. *School Data*

The school quality, teacher salary, and school segregation data covering the period of the 1960s, 70s, and 80s come from four sources:

(1) Office of Civil Rights (OCR) of the US Department of Health and Human Services, data for 1968-1988. OCR produced data containing school enrollment statistics broken down by race and school segregation indices for a large sample of the nation's school districts.
(2) Census of Governments, School District Finance Data, 1962-1999.
(3) The Common Core data (CCD) compiled by the National Center for Education Statistics is an annual, national statistical database that contains detailed revenue and expenditure data for all public elementary and secondary schools and school agencies and school districts in the US.
(4) The multiple sources used to compile the comprehensive desegregation case inventory (1954-1990) assembled by the team of scholars for The American Community Project at Brown University

included case dockets and bibliographies for all desegregation court orders from the Department of Justice, NAACP Legal Defense Fund, and the US Department of Education (Logan et al., 2008).

I have merged this desegregation court case data and information on major plan implementation year with district-level enrollment data from the Office of Civil Rights (OCR) Data and Common Core of Data and as collected by Welch and Light for the Office of Civil Rights. The enrollment data is used to calculate school segregation dissimilarity and exposure indices. I am grateful to Sarah Reber for sharing the OCR school data with me (as described further below).

Per-Pupil Spending Data

The data from the Historical Database on Individual Government Finances (INDFIN) represents the Census Bureau's first effort to provide a time series of historically consistent data on the finances of individual governments. This database combines data from the *Census of Governments Survey of Government Finances (F-33)*, the National Archives, and the *Individual Government Finances Survey*. The School District Finance Data FY 1967-91 is available annually from 1967 through 1991. It contains over one million individual local government records, including counties, cities, townships, special districts, and independent school districts. The INDFIN database frees the researcher from the arduous task of reconciling the many technical, classification, and other data-related changes that have occurred over the last 30 years. For example, this database includes corrected statistical weights that have been standardized across years, which had not been done previously. Furthermore, although most governments retain the ID number they are assigned originally, there are circumstances that result in a government's ID being changed. Since a major purpose of the INDFIN database is tracking government finances over time, it is critical that a government possess the same ID for all years (unless the ID change had a major structural cause). For example, All Alaska IDs were changed in the 1982 Census of Governments. In addition, new county incorporations, where governments in the new county area are re-assigned an ID based on the new county code (e.g., La Paz County, AZ), cause ID changes. Thus, if a government ID number was changed, the ID used in the database is its current GID number, including those preceding the cause of the change, so that the ID is standardized across years.

In addition to standardizing the data, the Census Bureau has corrected a number of errors in the INDFIN database that were previously in other sources of data. For example, for fiscal years 1974, 1975, 1976 and 1978 the school district enrollment data that had previously been released were useless (either missing or in error for many records). Thus, in August 2000, these missing enrollment data were replaced with those from the employment survey individual unit files. This enables us to more accurately compute per pupil expenditures for those years. In addition, source files before fiscal 1977 were in whole dollars rather than thousands. This set a limit on the largest value any field could hold. If a figure exceeded that amount, then the field contained a special "overflow" flag (999999999). Few governments exceeded the limit (Port Authority of NY and NJ and Los Angeles County, CA are two that did). For the INDFIN database, actual data were substituted for the overflow flag. Finally, in some cases the Census revised the original data in source files for the INDFIN database. In some cases, official revisions were never applied to the data files. Others resulted from the different environment and operating practices under which source files were created. Finally, some extreme outliers were identified and corrected (e.g., a keying error for a small government that ballooned its data).

The Common Core of Data (CCD) School District Finance Survey (F-33) consists of data submitted annually to the National Center for Education Statistics (NCES) by state education agencies (SEAs) in the 50 states and the District of Columbia. The purpose of the survey is to provide finance data for all local education agencies (LEAs) that provide free public elementary and secondary education in the United States. Both NCES and the Governments Division of the U.S. Census Bureau collect public school system finance data, and they collaborate in their efforts to gather these data. The Census of Governments, which was recorded every five years until 1992, records administrative data on school spending for every district in the United States. After 1992, the Public Elementary-Secondary Education Finances data were recorded annually with data available until 2010. I combine these data sources to construct a long panel of annual per-pupil spending for each school district in the United States between 1967 and 2010.

Per-pupil spending data from before 1992 is missing for Alaska, Hawaii, Maryland, North Carolina, Virginia, and Washington, D.C. Per-pupil spending data from 1968 and 1969 is missing for all states. Spending data in Florida was also missing for 1975, 1983, 1985-1987, and 1991. Spending data in Kansas was also missing for 1977 and 1986. Spending data in Mississippi was also missing for 1985 and 1988. Spending data in Wyoming was also missing for 1979 and 1984. Spending data for Montana is missing in 1976, data for Nebraska is missing in 1977, and data for Texas is missing in 1991. Where there was only a year or two of missing per pupil expenditure data, we filled in this data using linear interpolation.

D. *Sources of Data on Segregation*

I use data from the surveys conducted by the Office of Civil Rights (OCR) of the Office of Education to estimate the measures of segregation for school districts from 1968-1988. The exposure of blacks to whites is the percent white in schools, weighted by black enrollment and vice-versa for exposure of whites to blacks; data on racial composition at the *school level* are required to calculate these indexes. I obtained from Sarah Reber the original binary EBCDIC data files for the OCR surveys for 1968-1974 and 1976 (the survey was not conducted in 1975), who converted the files to ASCII for analysis. Similar school-level data on students and teachers by race were published for 1967 by the Office of Education; these data were entered for analysis. The exposure indexes where then calculated based on the school level enrollment by race. The OCR surveys were not comprehensive in all years, but the large size of school districts and the heavy representation of districts that had involvement of the courts in desegregating its schools ensured that most districts with significant minority student enrollment were included in the data in most years. Before the 1967 school year, no school-level data on enrollment by race are available.

As aforementioned, the data on school district spending, student enrollments, and numbers of teachers are obtained from the *Census of Government* (COG) for the available years from 1962-92. I use the version of the COG contained in the Historical Database on Individual Government Finance -- a longitudinally consistent version of the COG produced by the Census Bureau. The COG data are organized at the level of the school district. These figures are converted to 2000 dollars using the CPI-deflator. Per-pupil school expenditures is total expenditures by the district divided by total student enrollment.

Data on student-teacher ratios at the school level are not available before 1968. Student-teacher ratios by race are calculated from Office of Civil Rights (OCR) data. The OCR data (described below) contain information on the number of teachers in every school, as well as the number of black students and the total number of students. To calculate the black student-teacher ratio for 1970-1972, I calculated the student-teacher ratio (total students, any race, divided by total teachers, any race) in every school; I then calculated the weighted average student-teacher ratio for schools in each district, with black enrollment in the school as weights. For example, the analyses that analyze desegregation effects on average class size by race using school-level data, include 14,869 schools from 667 districts from 33 different states.

The demographic data on districts/counties are obtained from the 1960, 1970, 1980 and 1990 decennial censuses. I use versions of the census data summarized at the geographic level of the census tract.

Hospital Desegregation Data

Hospital Desegregation. The desegregation of hospitals in the South can be initially dated from 1964 when federally-mandated policies began to be enforced. In particular, developments in all three branches of government—judicial, executive, legislative—were influential. First, Hill-Burton Act's 'separate but equal' clause was ruled unconstitutional in 1963. Second, Title VI of the Civil Rights Act of 1964 put teeth in enforcement. Third, with the introduction of Medicare in 1965, a hospital had to be racially desegregated in order to be eligible to receive Medicare funding. The staggered timing of hospital desegregation in the South led to differences in the timing of improved access to hospital care for minorities, and resulted in timing differences in the implementation of Medicare in parts of the South that had not desegregated their hospitals prior to 1965.

Using the American Hospital Association's Annual Survey of Hospitals (spanning the period 1946-1980) along with the Centers for Medicare Provider of Service data files dating back to the early 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), I find that ¼ of counties in the South—and 75 percent of counties in the Mississippi Delta—lacked a Medicare-certified hospital by the end of 1966. Almond, Chay, & Greenstone (2008) and Finkelstein and McKnight (2008) have independently used this type of data previously to measure the timing of hospital desegregation. I also construct measures of the individual's age at which hospital desegregation occurred and a race-specific distance to the nearest hospital as an index of segregation and access during childhood (created using GIS mapping technologies and historical hospital address and childhood residential location information).

E. *County Head Start Spending & Public Transfer Program Data*

I use administrative data about county-level Head Start expenditures (1965-80) with single-age county-level population counts (SEER Population Data, 1969-1999). In particular, PSID data are linked to county Head Start spending during the first 15 years of the program, when these individuals were 3-5 years old, acquired from the National Archives and Records Administration (NARA). This historical county-level data enables me to compile an estimate of Head Start program expenditures per poor 4-year old in the county for each year between 1965 and 1980. Special thanks to Doug Miller and Martha Bailey, who helped me compile this information and confirm the accuracy of it, and the rollout of community health centers.

I am grateful to Doug Almond, Hilary Hoynes, and Diane Schazenbach for sharing the Regional Economic Information System (REIS) data for the 1959 to 1978 period. Per capita county transfer payments include measures for public assistance (AFDC, General Assistance, Food Stamps), medical care (Medicare, Medicaid, military), and retirement and disability benefits.

F. *Pre-Existing County Characteristics*

The pre-existing demographic, socioeconomic, and school-related characteristics at the county level were obtained originally from the county tabulations of the 1960/2 Census, were taken from the City and County Databook.

G. *Matching PSID Individuals to their Childhood School Districts*

In order to limit the possibility that school district boundaries were drawn in response to school desegregation, I utilize 1969 school district geographies. The "69-70 School District Geographic Reference File" (Bureau of Census, 1970) relates census tract and school district geographies. For each census tract in the country, it provides the fraction of the population that is in each school district. Using this information, I aggregate census tracts to 1970 district geographies with Geographic Information Systems (GIS) software. I assign census tracts from 1960, 1970, 1980 and 1990 to school districts using this resulting digital map based on their centroid locations. I also use the full universe of school addresses (1970 Elementary & Secondary General Information System (ELSEGIS) Public School Universe Data) and map them to PSID childhood addresses (census blocks) to identify the closest neighborhood school in the district using GIS mapping technologies.

To construct demographic information on 1970-definition school districts, I compile census data from the tract, place, school district and county levels of aggregation for 1960, 1970, 1980 and 1990. I construct digital (GIS) maps of 1969 geography school districts using the 1969-1970 School District Geographic

Reference File from the Census. This file indicates the fraction by population of each census tract that fell in each school district in the country. Those tracts split across school districts I allocated to the school district comprising the largest fraction of the tract's population. Using the resulting 1970 central school district digital maps, I allocate tracts in 1960, 1980 and 1990 to central school districts or suburbs based on the locations of their centroids. The 1970 definition central districts located in regions not tracted in 1970 all coincide with county geography which I use instead.

The school data from the OCR, Census of Governments, and Common Core of Data are merged to the individual-level geocoded version of the Panel Study of Income Dynamics for original sample children based on the census block where they grew up. Based on the school district of upbringing, I compute for each individual the average per-pupil school spending, student-to-teacher ratio, and school segregation levels experienced during their school-age years (as well as averaged over their adolescent years (ages 12-17)); similarly I compute for each individual the county per-capita transfer payments from income-support programs averaged over their school-age and adolescent years.

APPENDIX B: A BRIEF HISTORY OF US SCHOOL DESEGREGATION

Background. Residential segregation may affect access to quality schools and subsequent mobility by reducing school resources (e.g., school district per-pupil spending, class size, teacher quality). During the 1950s, 60s, and 70s when the individuals in the PSID sample were school-age, there was substantial variation across districts in school quality inputs (e.g., per-pupil spending, pupil/teacher ratio…). During this time period, there was limited state support for K-12 education (in the vast majority of states) and a heavy reliance on local property taxes. During the 1960s and 70s, states, on average, contributed roughly 40 percent of the cost of K-12 education, and much of this aid was a flat per pupil payment that was not related to local property wealth of the district (National Center for Education Statistics).

Before school desegregation plans were enacted, school district spending, particularly in the South, was directed disproportionately to the majority-white schools within districts, something which is not evident from district-level spending data. While the premise of the 1954 Brown decision was "separate is inherently unequal", the Brown decision alone was not sufficient to compel school districts to integrate. Minimal school desegregation occurred in the 1950s and early 1960s following the *Brown I* and *II* rulings issued in 1954 and 1955.

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. Civil rights organizations avoided taking on legal cases early on that had a high risk of failure, even if the potential local benefits were large. The cascading impacts that would accompany legal victory due to the role of precedent juxtaposed with the potential risks of losing outweighed considerations of where targeted efforts would have the greatest impacts or where impacts would be felt for the largest number of blacks in the short-run. As the recorded legal history of desegregation documents, the legal arm of the NAACP (Legal Defense & Educational Fund)…"followed a strategic approach that rejected simple accumulation of big cases, in favor of incremental victories that built a favorable legal climate…" (*Council for Public Interest Law*, 1976, p.37).[i] Guryan (2004) presents this intuition formally in a model that demonstrates that in an environment in which precedent has a strong effect on the subsequent probability of success, an agent with the objective of desegregating the nation's schools should optimally choose to prioritize the likelihood of success almost to the exclusion of any local benefits of desegregation when choosing where to bring litigation.

Timeline of School Integration in the US

At the time of the Brown decision in 1954, seventeen southern states and the nation's capitol required that all public schools be racially segregated (Figure A0). The Supreme Court did not set a time table for dismantling school segregation and turned the implementation of desegregation over to US district courts. The aftermath of Brown and process to see desegregation established in public schools can be characterized as consisting of several developmental periods—from neonatal and infancy (1954-65) to adolescence (1966-75) and young adulthood (1976-1989). The post-*Brown* era up through the mid-to late 1980s can be codified by two distinct periods: pre- and post-1965. The 1954-65 period was characterized by Southern states' intent to thwart implementation of Brown and resist compliance with the desegregation orders. The South's massive resistance to the Court's rulings ensued for the next 10 years and the delay tactics were initially very successful. The case-by-case litigation approach largely failed during the first decade following Brown. Legal scholar Walter Gellhorn described the pace of desegregation during these years as that "of an extraordinarily arthritic snail" (cited in Wilkinson, From Brown to Bakke, p. 102). By 1965, only 2 percent of African American children in the Deep South attended integrated schools and more than 75 percent of the schools in the South remained segregated.

Landmark Court Decisions on the Road from Segregation to Desegregation & Integration

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. The congressional enactment of ESEA was among the most important events in effecting compliance because it 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).

Figure A5 presents a map of the geographic variation in school spending in the US in 1962 overlaid with the residential locations of minorities in that year. The map illustrates the concentration of minorities in the South where school district per-pupil spending levels were lowest. Another example of how financial incentives played a role in facilitating compliance is evident in President Nixon's proposal to provide financial incentives to school districts to comply with desegregation orders, which led to congressional enactment of the Emergency School Aid Act of 1972 to assist the federal courts in achieving desegregation (Ehrlander, 2002, p. 23). Federal dollars soon constituted 30 percent of the budget of many Southern school systems. The availability of federal money continued to influence desegregation into the 1980s. I find a significant correlation in the amount of federal funds received by school districts in the years 1966-1970 with the percentage of black students enrolled in previously all-white schools.

The landmark court decision of 1968 in Green v. School Board of New Kent County required immediate actions to effectively implement desegregation plans that promised to work right away. The 1968 Green decision led to an acceleration of desegregation activity and set the pattern for a number of court-orders and desegregation plans that followed in many other districts across the country. Following the Supreme Court ruling in Green, the various Courts of Appeals held that desegregation plans based on "freedom of choice", or zoning which followed traditional residential patterns, were inadequate and deemed no longer acceptable. School desegregation encompassed not only the abolition of dual attendance systems for students, but also the merging into one system of faculty, staff, and services, so that no school could be marked as either a "black" or a "white" school.

In 1970, the Court approved busing, magnet schools, and compensatory education as permissible tools of school desegregation policy (*Swann v. Charlotte-Mecklenberg Board of Education*), and the ruling was among the first attempts to implement a large-scale urban desegregation plan. Schools in other regions of the country remained segregated until the mid-1970s and these districts began accelerating school desegregation efforts after the 1973 *Keyes vs. Denver School District* decision (413 U.S. 189), which ruled that court-ordered litigation applied to areas which had not practiced *de jure* segregation. This case was the first involving school desegregation from a major non-Southern city, and it marked the beginning of large-scale desegregation plans in regions outside the South. The case also ushered in a period of equal desegregation efforts in both the North and the South, regardless of whether the school segregation resulted from state action (legal mandate) or residential segregation patterns. Desegregation cases began to expand explicit goals beyond racial integration to include goals of promoting adequacy of school funding for minority student achievement. The 1977 Milliken II decision allowed courts to mandate spending on compensatory educational programs for minority students. This occurred in Los Angeles and Detroit, for example. No other important court decisions occurred between 1975 and 1990.

School Desegregation Data: The Nature, Pattern, and Timing of Initial Court Orders & Implementation

Most previous studies have not had access to data on the nature and timing of desegregation policy and action, and have been limited primarily to an examination of "white flight" and/or have been geographically limited. I provide analysis of school desegregation policy to describe aspects of the nature and timing of steps taken to desegregate the schools, which is instructive for the empirical approach pursued to identify its impacts.

Extent of Desegregation Actions (post-1965 period). Substantial steps to desegregate schools during the period 1966-75 are reported in an estimated 1,400 school districts. While these districts represent a small proportion of the 19,000 school districts in the country, they encompass about half of the minority public school children in the country. Although the actions to desegregate were most heavily concentrated in the Southern and Border States, such actions were found in a moderate number of districts in other regions of the country as well.

Nature of Pressure to Desegregate (pre- vs. post-1965 period). In many districts, desegregation was a process that came as a result of pressures from many sources. As the major impetus, court orders were most often reported in districts with high initial levels of segregation and with moderate-to-high proportions of minority students. Districts which desegregated under local pressures generally had low initial levels of segregation and low proportions of minority students. Figure A1 presents the dates of initial court orders and resultant major school desegregation plan implementation across the country among the 868 school districts that introduced such plans between 1954 and 1980. In the South, the largest share of school districts desegregated over the five-year period between 1968 and 1972, and school segregation declined to a far larger extent in the South relative to the rest of the country over this period.

Most desegregation plans implemented prior to 1965 were minor (referred to as "freedom of choice" plans), were not strictly enforced, and achieved only token levels of integration. My focus will be on the impacts of major desegregation plans whose implementation accelerated after 1965 coupled with actions spurred by the 1968 Green decision. The desegregation activity that took place after 1965 was in stark contrast with that of earlier years. As shown in Figure A1, the change in the pace of desegregation litigation activity and plan implementation after 1965 is striking. Many districts took steps overnight that changed the school systems from being predominantly segregated to predominantly desegregated. These steps were often taken subsequent to a specific court order or following direct threat from the US Department of Health, Education, and Welfare (HEW) to cut off Federal funds. The nature of timing of initial court litigation was highly idiosyncratic. Court-ordered desegregation by legal mandate is plausibly more exogenous than other more voluntary forms of desegregation. The extent of voluntary desegregation prior to court intervention varied across districts, but voluntary action of districts was more endogenous. As well, anti-integration groups can delay major desegregation plan implementation by lengthening the court proceedings or by implementing inadequate desegregation plans; thus, the timing of initial court orders is likely more plausibly exogenous than the actual implementation date of major desegregation plans (additional evidence provided near the end of this Appendix).

In Figure A4, I present evidence on the length of time between initial court order and major desegregation plan implementation. We see this lag exhibits a clear structural break in 1965 (Figure A4). Namely, the results suggests that for initial court orders meted out after 1965, there is roughly immediate implementation (on average, major plan implemented within 1-2 yrs of initial court order); and the lag does not differ over time for court orders after 1965. 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 (following initial court order, major plan is not implemented, on average, for 10 years; there is a systematic long delay that decreases in years leading up to 1965. During the 1955-64 period (after Brown but prior to the passage of the Civil Rights Act), the earlier the initial court order, the longer the delay in implementation of a major plan. This pattern and discontinuity after 1965 in the time lag between initial court order and major desegregation plan implementation occurs in the South and non-South.

In 1964, 1 percent of African American students in the South attended school with whites; by 1968, this had risen to 32 percent. As shown in Figure A1, the ensuing years of 1968-1972 bracket the period of maximum desegregation activity. Figure A2 presents a map that summarizes the overall geographic pattern and timing of initial court orders overlaid with the childhood residential locations of the (nationally-representative) PSID sample of black and white children in 1968 (Figure A6); and, analogously, Figure A7 shows this for the resultant subsequent major desegregation plan implementation in US school districts/counties[ii] (among the subset of districts for which this information is available). The figures demonstrate the strong overlap of residential locations of original sample PSID children with districts that underwent court-ordered desegregation.

In the figure, districts that were subject to court orders are shaded (no shading indicates no court-ordered desegregation); the shading of the districts/counties is assigned by its initial court order date, with darker shading denoting a later initial court ruling. The lightest gray represents communities in which the initial court order occurred between 1954 and 1963—the early desegregation period; and the next darkest gray shades denotes communities in which the initial court order occurred between 1964-1968 during the expansion of federal enforcement as a "national emphasis program" and under Title VI of the 1964 CRA and Title I of the 1965 ESEA; the next darkest grays indicate communities in which the initial court order occurred between 1968 and 1972 during the expansion following the 1968 Green Supreme Court ruling; the darkest gray and black represent the corresponding smaller number of communities in which the initial court order occurred between 1974 to 1980 and after 1980, respectively. Not surprisingly, the concentration of activity occurred in places with at least a 20 percent black population. A substantial portion of the US population of minority children in 1960 lived in the shaded 868 districts/counties that eventually were subject to court-ordered desegregation.

As shown, 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. In most regions, the initial court order took place in a narrower period than the 30-year period observed in the country as a whole; similarly, the span in timing of major desegregation plan implementation is narrower within regions than across the country as a whole. The regional pattern and clustering reflects the evolution of legal precedent. Figure A3 highlights the significant birth cohort variation in childhood exposure to court-ordered school desegregation for the PSID sample. The share of children exposed to school desegregation orders increases significantly with year of birth over the 1945-1970 birth cohorts analyzed in the PSID sample.

Only token desegregation efforts occurred prior to the passage of the 1964 Civil Rights Act. The figure shows that 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, that forms the basis of the research design. By 1976, 45 percent of the South's African American students were attending majority-white schools, compared with just 28 percent in the Northeast and 30 percent in the Midwest.

The process became highly decentralized with a diverse set of agents that initiated court litigation following the Brown decision, which also contributed to the idiosyncratic nature of the timing and location where legal challenges arose that resulted in initial court orders.[iii] 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. 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 below.

The primary identification strategy uses this variation in the timing of major desegregation plan implementation that was induced by differences in the year of the initial court order. Systematic variation in desegregation plan adoption could lead to spurious estimates of the plans' impact if those same school district characteristics are associated with differential trends in the outcomes of interest. To explore this, I compiled characteristics of school districts in 1962, prior to the surge of court-ordered desegregation cases and significant integration efforts that ensued in subsequent years (of the same decade). I use these "pre" characteristics to predict the year in which the initial court order took place and the year in which the school district actually implemented a major desegregation plan, respectively.

The 1962 county measures used as independent variables in the model include: the log(county population), percent of the population that is minority, per-capita school spending, the percent of school spending that comes from intergovernmental grants (state/federal), median income, percent of households with income <$3,000 (in 1961 dollars), percent of households with income >$10,000, percent with 12 or more years of education, population change between 1950-60, percent of residents in an urban area, percent of residents in rural or farm area, percent of residents living in group quarters, median age, percent of residents that are school-age, percent of residents 65 or older, percent of residents that voted for the incumbent President, and the county mortality rate (all constructed from the 1962 Census of Governments, City & County Data Book). I include the size of the population to capture the fact that large districts/counties may face differential costs and opposition to the desegregation process. I also estimate an alternative model specification that includes the 1962 average student-to-teacher ratio and average teacher salary, instead of the per-capita school spending level (as shown in Table A13, similar patterns emerge). These data are linked with the desegregation court case and plan implementation data.

Columns (1)-(6) of Table A13 presents estimates from least-squares regressions of the year each school district had an initial court order (among those that first became subject to court order after 1962) on 1962 characteristics and region fixed effects, while the final two columns ((7)-(8)) use the same set of independent variables to examine determinants of the delay between the initial court order and major desegregation plan implementation (in years). Column (1) shows estimates for the full sample, column (3)-(8) show results for the subset of counties in which original sample PSID children grew up, and columns (5)-(8) display results for the subsample of counties for which information is available on the dates of major desegregation plan implementation.

The magnitude of the association between the school district characteristics and the year of the initial court order is weak. I find that districts that had either significant minority proportion, larger per-capita school spending, teacher salary, smaller average student-to-teacher ratios, or greater income, generally did not experience an initial court order earlier or later than other districts (columns 1-6); however, these characteristics are significant predictors of the delay between the initial court order and major desegregation plan implementation (columns 7-8). Aside from differences in population concentration, only the proportion of the population with 12 or more years of education significantly predict coming under court order later; while the proportion of the population that is school-age is predictive of coming under court order sooner. Because parental education, neighborhood SES characteristics, and region of birth will be included in regression specifications, this correlation need not be a threat to the internal validity of the analysis. Interestingly, holding spending levels constant, districts that received a greater proportion of 1962 school spending from state and federal sources were more likely to have initial court orders sooner. This pattern may be expected if intergovernmental grants result in the financial ramifications of desegregation to not be borne solely by local residents, which may lessen opposition to desegregation implementation. Furthermore, I find that neither urbanicity, the proportion of the population in rural areas, nor the county mortality rate is generally predictive of the timing of initial court orders. While these regression results show a few statistically significant impacts of district characteristics on the timing of the initial court order, the quantitative importance of these predictors is small and most of the variation remains unexplained. I find little evidence that pre-treatment characteristics significantly predict the timing of court orders.[iv]

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 are consistent with the legal history of school desegregation, and suggest that the timing of initial court litigation is more plausibly exogenous than the timing of major desegregation plan implementation. In sum, the idiosyncratic nature of court litigation timing documented in the legal history of school desegregation make a prima facie case for treating initial court orders as exogenous shocks, which influenced the timing of major desegregation plan implementation and generated changes in school quality from abrupt shifts in racial school segregation. This case is bolstered by the empirical evidence that the bulk of 1962 district/county characteristics fail to predict the timing of initial court orders.

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[i] An elaborate discussion of the legal history of the school desegregation court decisions and the strategy used by the NAACP is contained in NAACP (2004) and www.naacp.org/legal/history/index.htm.

[ii] While the data is available at the school district level, the maps are presented at the county level for convenience, so I use counties and school districts interchangeably here in reference to the maps.

[iii] School desegregation litigation cases have been initiated by school districts, plaintiffs, federal district court judges, parents of students in affected districts, and non-school governmental organizations.

[iv] I find similar results when I also define as “under court order” those districts that implemented desegregation plans in response to pressure from HEW in addition to school districts covered by formal court orders.

Appendix C: Supplementary Regression Results, PSID Data, & Measures

Validating the PSID with OCR-CCD data

To address concerns that my findings are driven by the sampling design of the PSID, I replicate the analysis for high school completion using the combined OCR-CCD district-level data. I focus on dropout rates (grades 9-12) because this is the most reliable data that can be compared across time. I combine the district-level data on dropout rates from the Office of Civil Rights (OCR) with the Common Core Data Local Education Agency Universe Survey for all school districts in the United States, which together span the period 1972 to 1999. I focus on districts ever under court order.

To validate the PSID analysis, I compute district-specific desegregation-induced increases in school spending and racial integration using the same method as that employed for the PSID data. I link the timing of school desegregation and the district-specific induced changes in per-pupil spending and racial segregation (black-white exposure index) to the high school dropout data from the OCR-CCD by year. I then estimate the effects of desegregation exposure and resultant increases in school spending (due to desegregation) on the district dropout rate. Because high school dropout information at the district level is not disaggregated by race, I weight these models by the district's (pre-desegregation) percent of enrollment that is black to attempt to capture average effects for black children. I include the set of controls as the district-level regression models that the main results in Figures 1 & 3 are based upon.

It is important to note that while one might expect the patterns in the OCR-CCD district-level data to be similar to those in the PSID, there are numerous reasons to expect some differences between the results presented in the PSID and the OCR-CCD samples. First, because these data are at the district level rather than the individual level and because the OCR-CCD data are based on the school district attended (rather than the school district of birth) any effects might reflect changes in school composition that occur as a result of school quality changes associated with desegregation. Finally, while I analyze the effect of desegregation exposure and induced effects of changes in school spending for an individual over their entre 12 years of public schooling in the PSID, in the OCR-CCD I analyze the effect of contemporaneous spending in a given year. In sum, there are numerous reasons to expect differences between the results presented in the PSID and the OCR-CCD samples. However, should the results be similar between the OCR-CCD data and the PSID sample, this robustness check would indicate that my findings are robust and generalizable.

I estimate a parametric event study model with event study years interacted with the desegregation-induced changes in school spending and racial segregation, respectively (results presented in Appendix Table A11). First, I find that districts that underwent larger changes in school spending resultant from desegregation exhibited increasing high school dropout rates in the years *preceding* the enactment of court orders. The results show that this pre-existing trend was subsequently reversed in districts in which desegregation led to significant increases in per-pupil spending. In particular, the results indicate that a $1,000 increase in per-pupil spending is associated with a 5-percentage point reduction in high school dropout rates in the first five years following desegregation. Note that this estimate is not directly comparable to that from the PSID sample because this estimate is based on annual spending at the district level, not the cumulative effect of a sustained spending increase (experienced at the student level) for all 12 years of a student's life. Because we expect the latter to be much larger, the results from the OCR-CCD data are consistent with those from the PSID. The results suggest that high school dropout rates were insensitive to how much reduction in racial school segregation resulted from court orders. In this respect as well, the findings reveal similar patterns with my main PSID results.

PSID Sample

Studies have concluded that the PSID sample of heads and wives remains representative of the national sample of adults (Fitzgerald, Gottschalk, and Moffitt, 1998a; Becketti et al, 1988), and that the sample of "split offs" is representative (Fitzgerald, Gottschalk and Moffitt, 1998b). The 95-98% wave-to-wave response rate of the PSID makes this possible.

Multinomial Models of Educational Attainment

In addition to the main education models reported, I also estimate multinomial logit models of educational attainment, where the four categories are: (1) High School Dropout/GED; (2) High School Graduate, no college; (3) Attend College, no 4-year degree; and (4) 4-year College Graduate or more. I find that the effects of school desegregation for blacks were not limited to those on the margin of dropping out of high school, but also had significant effects that led to increased college attendance and completion rates. The results demonstrate that there is a significant difference in both high school dropout rates and college attendance and completion rates among blacks between cohorts that were born less than 7 years apart but differed in whether and how long they attended integrated schools; with no effects for whites across any of the educational attainment categories.

Incarceration Measures

Spells of incarceration are recovered from information on PSID respondents' collected in each survey (1968-2011) that includes whether a respondent was incarcerated at the time of the interview. The 1995 wave added a supplemental crime history module to the PSID including several key questions that I use to augment and obtain more precise information about the timing and duration of incarceration and minimize measurement error.

The annual data alone on incarceration has limitations. Among the most important is that this will only identify incarceration in a given year if it were on-going at the time of the survey interview. As a result, we are likely to miss individuals serving shorter sentences that did not coincide with the time of the interview. The supplemental crime history module that was added to the 1995 wave of the PSID aims to address this limitation and includes information on whether respondents had ever been suspended/expelled from school; ever been booked or charged with a crime; whether ever placed in a juvenile correctional facility; whether ever served time in jail or prison, the number of times and the month and year of release. This information is used together to analyze the annual incidence of incarceration and whether ever incarcerated by age 30.

Health Index

A number of previous studies using surveys have demonstrated that a change in GHS from fair to poor represents a much larger degree of health deterioration than a change from excellent to very good or very good to good (e.g., Van Doorslaer and Jones, 2003; Humphries and Van Doorslaer, 2000). More generally, this research has shown that health differences between GHS categories are larger at lower levels of GHS. Thus, assuming a linear scaling would not be appropriate.

To analyze health disparities in the presence of a multiple-category health indicator, three alternative approaches have been used, each with its own set of advantages and disadvantages. The most common and simplest approach is to dichotomize GHS by setting a cut-off point above which individuals are said to be in good health (e.g., excellent/very good/good vs. fair/poor). The disadvantage of this approach is that it does not utilize all of the information on health. Additionally, it uses a somewhat arbitrary cut-off for the determination of healthy/not-healthy, and the measurement of inequality over time can be sensitive to the choice of cut-off (Wagstaff and Van Doorslaer, 1994).

A second approach is to estimate an ordered logit or ordered probit regression using the GHS categories as the dependent variable, and rescale the predicted underlying latent variable of this model to compute "quality weights" for health between 0 and 1 (Cutler and Richardson, 1997; Groot, 2000). The key shortcoming of this approach is the probit and logit link functions are inadequate to model health due to the significant degree of skewness in the health distribution (i.e., the majority of a general population sample report themselves to be in good to excellent health). Van Doorslaer and Jones (2003) assess the validity of using ordered probit regressions to impose cardinality on the ordinal responses comparing it with a gold standard of using the McMaster 'Health Utility Index Mark III' (HUI).[1] They conclude "…the ordered probit regression does not allow for any sensible approximation of the true degree of inequality."

The third approach, adopted first by Wagstaff and Van Doorslaer (1994), assumes that underlying the categorical empirical distribution of the responses to the GHS question is a latent, continuous but unobservable health variable with a standard lognormal distribution. This assumption allows "scoring" of the GHS categories using the mid-points of the intervals corresponding to the standard lognormal distribution. The lognormal distribution allows for skewness in the underlying distribution of health. The health inequality results obtained using this scaling procedure have been shown to be comparable to those obtained using truly continuous generic measures like the SF36 (Gerdtham et al., 1999) or the Health Utility Index Mark III (Humphries and van Doorslaer, 2000) in Canada, but has not been validated as an appropriate scaling procedure using U.S. data. The disadvantage of this approach is it inappropriately uses OLS on what remains essentially a categorical variable and does not exploit the within-category variation in health. This is particularly problematic for the analysis of health dynamics over a relatively short time horizon. Ignoring within-category variation in health will cause health deterioration estimates to be biased and induce (health) state dependence because within-category variation increases when going down from excellent to poor health.

[1] The McMaster Health Utility Index can be considered a more objective health measure because the respondents are only asked to classify themselves into eight health dimensions: vision, hearing, speech, ambulation, dexterity, emotion, cognition, and pain. The Health Utility Index Mark III is capable of describing 972,000 unique health states (Humphries and van Doorslaer, 2000).

Several surveys have been undertaken that contain both the GHS question and questions underlying a health utility index. In this paper, we adopt a latent variable approach that combines the advantages of approaches two and three above, but avoids their respective pitfalls. Specifically, utilizing external U.S. data that contain both GHS and health utility index measures, we use the distribution of health utility-based scores across the GHS categories to scale the categorical responses and subject our indicators to the transformation that best predicts quality of life. This scaling thus translates our measures into the metric that reflects the underlying level of health. Specifically, using a 100-point scale where 100 equals perfect health and zero is equivalent to death, the interval health values associated with GHS are: [95, 100] for excellent, [85, 95) for very good, [70,85) for good, [30,70) for fair, and [1,30) for poor health.

Interval Regression Model. The method assumes that underlying the categorical empirical distribution of the responses to the GHS question is a latent, continuous health variable. I estimate interval regression models using the aforementioned values to scale the thresholds for GHS, where interval regression models are equivalent to probit models with known thresholds.

The measure of health status has categorical outcomes excellent (E), very good (VG), good (G), fair (F), and poor (P). The model can be expressed as

$$H_i = \begin{cases} 1 \ (\text{E}) & \text{if } 95 \le H_i^* \le 100 = \text{perfect health} \\ 2 \ (\text{VG}) & \text{if } 85 \le H_i^* < 95 \\ 3 \ (\text{G}) & \text{if } 70 \le H_i^* < 85 \\ 4 \ (\text{F}) & \text{if } 30 \le H_i^* < 70 \\ 5 \ (\text{P}) & \text{if } 1 \le H_i^* < 30 \ , \end{cases}$$

where H^* is the continuous latent health variable and is assumed to be a function of socio-economic variables x:

$$H_i^* = x_i\beta + v_i \ , \quad v_i \sim N(0, \sigma_v^2).$$

Given the assumption that the error term is normally distributed, the probability of observing a particular value of y is

$$P_{ij} = P(H_i = j) = \Phi\left(\frac{\mu_{\mathrm{U}} - x_i\beta}{\sigma_v}\right) - \Phi\left(\frac{\mu_{\mathrm{L}} - x_i\beta}{\sigma_v}\right) \ ,$$

where j indexes the categories, $\Phi(\bullet)$ is the standard normal distribution function, and μ represent the threshold values previously discussed. Because the threshold values are known, it is possible to identify the variance of the error term σ_v^2. Because I use the health utility-based values to score the thresholds for GHS, the linear index for the interval regression model is measured on the same scale. This scaling thus translates the measures into the metric that reflects the underlying level of health. With independent observations, the log-likelihood for the interval regression model takes the form:

$$\log L = \sum_i \sum_j H_{ij} \log P_{ij} \ ,$$

where the H_{ij} are binary variables that are equal to 1 if $H_{ij} = j$. This can be maximized to give estimates of β.