

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

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ABSTRACT

This paper investigates the extent and ways in which childhood school quality factors causally influence subsequent adult socioeconomic and health outcomes. The study analyzes the life trajectories of children born between 1950 and 1970, and followed through 2007, using the Panel Study of Income Dynamics (PSID). The PSID data are linked with multiple data sources that describe the neighborhood attributes and school quality resources that prevailed at the time these children were growing up.

I estimate the long-run impacts of court-ordered school desegregation plans on adult attainments by exploiting quasi-random variation in the timing of initial court orders, which generated differences in the timing and scope of the implementation of these plans during the 1960s, 70s, and 80s. Difference-in-differences estimates, sibling-difference estimates, and 2SLS/IV 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 educational attainment 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. This narrowed black-white adult socioeconomic and health disparities for the cohorts exposed to integrated schools during childhood. The results highlight the significant impacts of educational attainment on future health status and risk of incarceration, and point to the importance of school quality in influencing socioeconomic mobility prospects, which in turn have far-reaching impacts on health.

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I. INTRODUCTION

Racial segregation that results in race differences in access to school quality has often been cited as perpetuating inequality in attainment outcomes. Since the landmark 1954 Supreme Court *Brown v. Board of Education* decision and subsequent court-ordered implementation of school desegregation plans during the 1960s, 70s and 80s, scholars have investigated the consequences of school desegregation on socioeconomic attainment outcomes of black children (Clotfelter, 2004). Many studies since the Coleman Report (Coleman, 1966) have focused primarily on black-white differences in academic outcomes, and attempted to assess the roles of schools and family background in contributing to racial disparities (see, e.g., Ferguson, 1998). However, no large-scale data collection effort was undertaken to investigate school desegregation program effects, particularly on longer-run outcomes.

While many prior studies have examined the effects of school resources on test scores and more proximate student achievement outcomes, less evidence is available on how school quality influences socioeconomic attainments at mid-adulthood ages using longitudinal data. Still fewer studies have documented how school resources might influence adult health status via their impacts on educational attainment and adult economic status.

This paper investigates the extent and mechanisms by which childhood school quality factors 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 1950 and 1975 and have been followed through 2007, using the longest-running US nationally-representative longitudinal data spanning four decades.¹ To this PSID data, I link information from multiple data sources that contain detailed neighborhood attributes and school quality resources that prevailed at the time these children were growing up. The implementation of court-ordered school desegregation plans during the childhoods of these birth cohorts provides a unique opportunity to evaluate their long-run impacts. I obtained 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. 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.

The analysis proceeds in three stages. First, I present models of the predictors of the timing of initial desegregation court orders, which serves to demonstrate the exogeneity of the “treatment” (borrowing from the parlance of the medical literature). I show that collectively the pre-treatment school quality, SES, demographic, and labor market related characteristics do not significantly (jointly) predict the year of the initial court order. Second, 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, the primary identification strategy uses variation in the timing and scope of desegregation plan implementation that was induced by the quasi-random variation in the timing of initial court orders. I find strong evidence that desegregation plans were effective in narrowing black-white gaps in per-pupil school spending and class size and decreasing school segregation (though white flight thwarted some of the integration and leveling up of school resources over time). Third, I investigate the long-run impacts of the court-ordered desegregation plans on subsequent attainment outcomes, including educational attainment, adult earnings, income and poverty status, probability of incarceration, and adult health status. I exploit the wide variation in the timing and scope of implementation of desegregation plans 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 educational attainment 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.

As an alternative empirical strategy, I use sibling comparisons to identify the effects of school quality and school desegregation on adult socioeconomic and health outcomes. 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. I estimate within-family effects of school quality inputs on later-life health. Sibling fixed effect models have the advantage of explicitly accounting for observed and unobserved between-family endowment and resource heterogeneity that often plague OLS estimates. I exploit policy-induced changes in per-pupil spending and school resources that are unrelated to child family- and neighborhood-level determinants of adult economic and health status. 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 who had already reached age 18 prior to the desegregation plan implementation or who 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 (difference-in-difference, sibling fixed effect, and 2SLS/IV models), and reveal significant long-run impacts of school desegregation and school quality on a broad range of adult outcomes. This narrowed black-white adult socioeconomic and health disparities for the cohorts exposed to integrated schools during childhood. The results are robust to a variety of specification tests.

The empirical analysis makes three unique contributions by investigating: (1) non-racial integration aspects of court-ordered desegregation through its impacts on per-pupil spending; (2) the effects of court-ordered desegregation plans of public schools on adult SES and health outcomes and attempts to separately identify the effects of neighborhood and school quality; and (3) the role of childhood school and neighborhood quality in contributing to socioeconomic and racial health disparities in adulthood. This work provides a broader view of the mechanisms by which (access to) dimensions of school quality inputs influence long-run outcomes. 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 results

highlight the significant impacts of educational attainment on future health status and risk of incarceration, and point to the importance of school quality in influencing socioeconomic mobility prospects, which in turn have far-reaching impacts on health.

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 operates via their influence on the quality and quantity of educational attainment and adult economic status. For example, attending schools with a high concentration of poor children may reduce the school's capacity to provide quality instruction and may expose students to negative peer pressure that lowers their academic performance. I examine the hypothesized primary mechanism: changes in school quality resulting from abrupt shifts in racial school segregation. Integration may also influence long-term outcomes in ways that are unrelated to academic achievement and educational outcomes.

Because I observe individuals in their 30s, 40s, and into their 50s, I can analyze the effects of child school quality resources on adult economic and health status through mid life, and also see if the effects are stronger at later ages than earlier ages. If the long-run health consequences manifest/operate through (intermediate) effects on socioeconomic mobility (e.g., via effects on educational attainment and adult economic status), then we would expect the effects to become more pronounced over the course of adulthood. The data and methods improve upon prior research, which lacked access to panel data which follow children from birth to adulthood, relied on aggregate state-level analyses, and/or failed to address the endogeneity of residential location.

The remainder of the paper is organized as follows. I begin with 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. The next section analyzes 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/hypotheses 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 related studies and concluding statements are provided in the final section.

II. 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...)²

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.

An understanding of the causes of the timing of desegregation is critical to the identification strategy. 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 important role of precedent in the US legal process caused the NAACP to pursue the strategy to first, and foremost, bring suits 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. 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. 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. 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).³ 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 1). The Supreme Court did not set a timetable 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. 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 reason congressional enactment of the Elementary and Secondary Education Act of 1965 was among the most important events in effecting compliance was 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). This resulted in a significant drop in the extent of racial school segregation thereafter reinforced by the actions of local Federal courts. Thus, there is a sharp post-1965 discontinuity in school desegregation.

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*

In order 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.⁴ Multiple sources were used to compile the comprehensive desegregation case inventory assembled by the team of scholars for The American Community Project at Brown University. 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 1,057 school districts. 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 2 presents the dates of initial court orders and resultant major school desegregation plan implementation across the country among the 1,057 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 2, 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 (Section III).

In Figure 3, 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 3). 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. These findings inform the empirical approach used to identify school desegregation impacts (Section III).

State and federal dollars proved to be the most effective incentives to desegregate the schools. 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 2, the ensuing years of 1968-1972 bracket the period of maximum desegregation activity. Figure 4 presents a map that summarizes the overall geographic pattern and timing of initial court orders overlayed with the childhood residential locations of the (nationally-representative) PSID sample of black and white children in 1968 (Figure 4a); and, analogously, Figure 5 shows this for the resultant subsequent major desegregation plan implementation in US school districts/counties⁵ (among the subset of districts for which this information is available). 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 857 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.

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.⁶ This legal history of school desegregation is important because it illustrates that there was significant variation in both the timing, nature and scope of desegregation efforts; and most importantly for my research design purposes, a vast majority of this heterogeneity, particularly its timing, was driven by an assortment of idiosyncratic, exogenous factors. The key to the identification strategy pursued in this paper is thus to capitalize on this source of identifying variation. 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 1, similar patterns emerge). These data are linked with the desegregation court case and plan implementation data.

Columns (1)-(6) of Table 1 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.⁷

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.

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

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. Using the staggered timing of court-ordered school desegregation (and plan implementation) within an event study analysis (cf. Jacobson, LaLonde and Sullivan, 1993; McCrary, 2007), I quantify desegregation effects on school resources. I exploit the variation in the timing of court orders in one set of models to analyze desegregation effects and exploit the variation in the timing of major desegregation plan implementation in the other set. Because of the aforementioned structural break in the lag between initial court order and desegregation plan implementation (see Figure 3), the models that use the timing of initial court orders include an interaction term for pre-1965 court orders. The discussion of the models below applies similarly for the court order and plan implementation specifications.

A newly compiled school district panel dataset allows this analysis to exploit variation in the timing of initial court orders and subsequent desegregation plan implementation. The data includes measures from 1968-1982 Office of Civil Rights (OCR) data; 1962-1982 Census of Governments 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 event study framework compares school district per-pupil spending, student-to-teacher ratios (class size), and school segregation levels in the years immediately after court-ordered desegregation to the levels that prevailed in the years immediately before court orders (plan

implementation) for districts that underwent court-orders at some point during the 1960s or 70s. The analysis exploits plausibly exogenous determinants in the timing of initial court orders (and desegregation plan implementation in a subset of analyses) to estimate the following event study equation,

$$Y_{c,t} = \theta_c + \gamma_{r(c),t} + \sum_{y=-5}^{-1} \pi_y D_c 1(t - T_c^* = y) + \sum_{y=1}^6 \tau_y D_c 1(t - T_c^* = y) + X'_{ct} \beta + \varepsilon_{ct} \quad , \quad (1)$$

where $Y_{c,t}$ is per-pupil spending, student-to-teacher ratio, segregation dissimilarity index or black-white exposure index in school district c in year $t=1962, \dots, 1982$; θ_c is a set of school district fixed effects; $\gamma_{r(c),t}$ is a set of year fixed effects or region-by-year fixed effects; and X_{ct} is a column vector including a constant and school district demographic characteristics. D_c is a dummy variable equal to one if the school district ever implemented a desegregation plan, and the indicator function, $1(\cdot)$, is equal to one when the year of observation is $y = -5, -4, \dots, 1, \dots, 6$, years removed from the date, T_c^* , when school district c was first issued the court order (or implemented a desegregation plan for a subset of analyses) ($y=0$ is omitted).⁸

The point estimates of interest, π_y and τ_y , are identified using variation in the timing of desegregation plan implementation. Because the indicator for $y = 0$ is omitted, π_y is interpreted as the average difference in outcomes y years *before* the plan was implemented, and τ_y is the average difference in outcomes y years *after* the desegregation plan was implemented. 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).⁹

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 that implemented desegregation plans and those that did not. The school district fixed effects control for time-invariant community characteristics such as preferences for racial integration and education. With the inclusion of region-by-year fixed effects, the estimates will provide unbiased estimates of the impact of court-ordered school desegregation plans even if regions varied in their K-12 education policies or their average level of funding support from year to year. Additionally, time-varying, community-level (i.e., county, school district, or neighborhood) 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 black student enrollment to yield estimates that are representative of the impacts for the average black child. If I instead treat the 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 also interesting, I am more interested in documenting the impacts of school desegregation for the average black student. 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).¹⁰

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.¹¹ To control for the possible coincident expansion of other programs, I also include measures of childhood county per capita transfer payments for cash income support, medical care, and retirement and disability programs (that prevailed during their school-age years). Both the models that examine impacts on school quality inputs and the models that examine long-run impacts on adult outcomes (Section V) include these controls for childhood county per capita transfer payments from income-support programs.¹²

The Effectiveness of School Desegregation Plans. 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. I then extend these findings to show that in the years leading up to and immediately following implementation, desegregation court-orders (plan implementation) 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 6, 8, and 9. The figures plot the regression coefficients on indicator variables for years before and after desegregation orders are enacted (year before initial court-order (implementation) is the reference category) on school district racial segregation, per-pupil spending, and the student-to-teacher ratio, respectively. The changes are all statistically significant. These models include school district fixed effects and region-specific year effects. The figures show effects induced by desegregation court-orders that represent post-1964 court orders (the interaction terms for pre-1965 court orders reveal that due to the significant lag between initial court orders and major plan implementation during the pre-'65 legal/enforcement regime, effects during the early desegregation era were much smaller).

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. Those districts that desegregated primarily under pressure from the courts had enrollments of 7.5 million students in 1972. As shown in Figure 6 (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. With regard to school segregation, 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 two years after implementation, 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 the lower left graph of Figure 6, we witness a parallel significant pattern for the black-white exposure index (an alternative measure of school segregation), as it increases by about 0.10-0.15 within 2-4 years following initial court orders, again with no pre-existing time trend leading up to the court order. This represents a significant decline in school segregation, as the average black-white exposure index in 1968 among districts that had not yet implemented a plan was 0.16. A more immediate and even sharper decline in school segregation (for both the dissimilarity index and the black-white exposure index) emerges when years before and after major desegregation plan implementation is analyzed; e.g., the dissimilarity index declines by nearly 0.25 points and the black-white exposure index increases by 0.15 within 1-2 years following major plan implementation (shown in the upper- and lower-right graphs of Figure 6). Levels of racial integration in schools peaked around 1988.

Figure 7 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. I extend these results to examine the court-ordered desegregation effects on school district per-pupil spending, separately by revenue source (local; state; federal). The results are shown in Figure 8. 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, we see that 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 (top graph in Figure 8). 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 (>0.35).¹³ As shown in the bottom graph of Figure 8, 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.

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.

Figure 9 provides supportive evidence of reduced average class size for blacks following desegregation court orders. The results for the student-teacher ratio do not exhibit any pre-existing time trend but fall sharply following implementation, with reductions in class size of about 3 to 4 students five years later. As a robustness check for the estimated court-order induced effects on school quality inputs, I alternatively used a balanced panel of school districts that includes districts only if they contributed 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). The 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.¹⁴

Models are weighted by baseline black student enrollment so that results can be interpreted as desegregation effect experienced by the average black child. Similarly, the results presented in the lower-left graph for whites is weighted by baseline white student enrollment, so that the results can be interpreted as desegregation effect experienced by the average white child. As shown, 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 lower right graph uses school-level data for the subset of years in which this information is available and models are weighted by black student enrollment at the school-level (the analytic sample includes 14,869 schools from 667 districts from 33 different states; standard errors clustered at school-level); the three other graphs use all years of data aggregated up to the school district level. These results are reinforced with the use of school-level data, which demonstrate identical patterns. More immediate and sharper reductions in average class size for blacks are found by analyzing the years immediately before and after major desegregation plan implementation (as shown in the upper-right graph of Figure 9). The sharp trend break in school resource inputs (per-pupil spending, class size, school segregation) immediately following implementation of

school desegregation plans strongly suggests the estimates reflect the causal impact of desegregation plans.¹⁵

IV. DATA AND MEASURES

The primary micro dataset utilized is the restricted, confidential geocoded version of the PSID (1968-2007) with identifiers at the neighborhood block level in which children grew up. I then merge neighborhood and school information from multiple data sources on the conditions that prevailed in the 1960s, 70s, and 80s when these children were growing up. This includes measures from 1968-1982 Office of Civil Rights (OCR) data; 1960, 1970, 1980 Census data; 1962-1982 Census of Governments data; Common Core data (CCD) compiled by the National Center for Education Statistics; Regional Economic Information System (REIS) data; as well as 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 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. This sample of “split offs” has been found to be representative (Fitzgerald, Gottschalk and Moffitt, 1998). Moreover, the genealogical design implies that the PSID sample today includes numerous adult sibling groupings who have been members of PSID-interviewed families for nearly four decades.

The selected sample consists of PSID sample members born between 1950 and 1975; these individuals were between 0 and 18 years old in one of the first six waves of interviewing and were between the ages of 37 and 57 in 2007. I include all information on them for each wave, 1968 to 2007.¹⁶ The primary analyses use the sample of original sample children born between 1951 and 1970. The sample includes males and females; all analyses control for gender, given well-known differences in labor

market and health outcomes for men and women. 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. I apply sample weights in all the analyses to produce nationally-representative estimates.

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 (the algorithm used for matching individuals to schools is outlined in the Data Appendix). 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-five percent of the original sample PSID children followed into adulthood that are analyzed in this paper (i.e., 4,683 out of 7,212 children) grew up in a school district that underwent a desegregation litigation case sometime between 1950 and 1990. These children lived in 1,073 different neighborhoods from 186 different school districts, representing 33 different states (based on childhood residence in 1968). 82 percent of original sample black children followed into adulthood grew up in school districts that underwent a desegregation litigation case sometime between 1950 and 1990 (i.e., 2,914 out of 3,558 black children). Figure 4 presents the spatial distribution of PSID original sample black and white children and demonstrates the strong overlap with districts that underwent court-ordered desegregation. Figure 10 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 1950-1975 birth cohorts analyzed in the PSID sample.

I merged the school district expenditures data, information on student-teacher ratios, teacher salaries, and the constructed school segregation indices, to the PSID data using the census block/tract contained in the Geocode file at the 1968 survey interview. After combining data from the 5 data sources, the main sample (born between 1951 and 1970) contains 130,402 person-year observations from 7,212

individuals from 2,383 childhood families, 1,658 childhood neighborhoods, 349 school districts, representing 40 different states. The mean age is about 35 for most outcome measures considered, with age ranging from 20 to 57, and an average of 18 observations per person (of valid adult income observations). Appendix A and Appendix Table A0 lists the sources and years of all data elements along with details of the PSID survey questions used to construct key measures. Appendix Table A1 contains descriptive statistics for childhood family- and neighborhood-level measures for the sample by race.

Outcomes of interest. A broad range of adult outcomes are analyzed including educational attainment (completed years of education; high school dropout; high school diploma; college attendance; 4-year college degree), adult earnings, wages, annual work hours, family income and poverty status (all expressed in real 2000 dollars), whether ever incarcerated, and adult health status. Given well-known gender differences in labor force participation rates and criminal involvement, the labor market and incarceration outcomes are presented for men. The regression models estimated for other economic, education, and health outcomes include men and women, with controls for gender.

The key adulthood health outcome examined is the general health status (GHS) question: “Would you say your health in general is excellent, very good, good, fair, or poor?” This question was asked of household heads and wives (if present) in each survey between 1984 and 2007, and was asked of all family members in 1986.¹⁷ GHS is highly predictive of morbidity measured in clinical surveys, and it is a powerful predictor of mortality, even when controlling for physician-assessed health status and health-related behaviors (Benyamini and Idler, 1999). GHS is also frequently used as a global measure of health status. Due to the complexity of the health status changes for women during the childbearing years, I exclude self-assessed health status measures of women in the years they were pregnant.

Spells of incarceration are recovered from information on PSID respondents’ collected in each survey that includes whether a respondent was incarcerated at the time of the interview. The 1995 wave added a 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.¹⁸

Desegregation Data. 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. I augment this data with the dataset compiled by Welch and Light (1987) for the US Commission on Civil Rights, which covers all districts that in 1968 were 20 to 90 percent minority with enrollments of 50,000 or more, and a random sample of districts that were 10-90 percent minority with enrollments of between 15,000-50,000. In 1968 these districts accounted for 45 percent of minority enrollment in the US.

While data is available on the precise timing (exact year(s)) of major desegregation plan implementation following the initial court order on only this subset of districts, the combined data from the American Communities Project (Brown University) and Welch/Light provide the best available data that have been utilized to study this topic for three reasons. First, as shown, 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. 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, as is the year of major desegregation plan implementation for the 125 large school districts from Welch/Light. Third, in the large districts for which information is available on both the year of the initial court order along with the year of major desegregation plan implementation, the initial implementation year of major desegregation plans resulted in the single largest decline in racial school segregation that the district experienced. This data is combined to provide new evidence on the long-run impacts of school desegregation.

V. EMPIRICAL APPROACH

Point-in-time comparisons of integrated and segregated school systems confound the effect of the desegregation plans with the effect of factors that influenced the implementation of the plan. I match changes in adult attainment outcomes of blacks and whites to the exact timing of school desegregation. Comparisons of average outcome trends in the years leading up to major 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 use of IV/2SLS approach, where the initial year of the court order serves as an instrument for the year of major desegregation plan implementation (discussed in detail below).

Analytic data sample selection choices and estimation strategy are guided by the insights and considerations discussed in Section II. In particular, the aforementioned pattern of results led me to 1) restrict analysis to quasi-random timing of court orders that occurred b/w 1965-1990('85) to identify desegregation effects among children who grew up in districts in which I lack precise desegregation plan implementation information; and 2) for children from subset of large districts in which I have precise desegregation plan implementation information, I use 2SLS/IV approach to identify effects to address endogenous delays in implementation of major desegregation plans (prior to 1965).

In choosing the preferred sample for this analysis, there is a trade-off between sample size (using the entire sample of PSID original sample children but not all of whom grew up in districts that ever experienced court-ordered desegregation) and targeting (using the smaller sub-sample of children who grew up in districts that underwent court-ordered desegregation and for which I have precise information of the major desegregation plan implementation year(s)). To reduce potential bias, in the main model specifications I limit the estimation sample to individuals who grew up school districts that were subject to court-ordered desegregation at some point during the 1960s, 70s, or 80s, since individuals from school

districts of upbringing that never implemented desegregation plans are arguably too different to provide a credible comparison group. If the sample included children from school districts that were never subject to desegregation court-orders, the identifying assumption would be more stringent and require that both when and if a district was ever under court-order to be uncorrelated with trends in the outcome variable. Thus, in analyses that include all PSID original sample children, models include school district fixed effects and along with birth cohort fixed effects interacted with an indicator for whether the school district of upbringing ever experienced court-ordered desegregation (1950-1990).

I utilize three different, but complementary, empirical approaches to estimate the long-run effects of school desegregation and school quality on adult attainment outcomes: (1) difference-in-difference and fixed effect models; (2) 2SLS/IV models; and (3) sibling fixed effect models. I discuss each in turn, and as will be shown, each method uncovers a parallel set of findings of significant, lasting impacts for blacks, with no effects for whites.

Difference-in-Difference Approach. I estimate the impacts of court-ordered school desegregation, and the improvements in school quality for African Americans that accompanied their enactment, on subsequent adult attainments. The difference-in-difference regression analysis attempts to isolate the component of school quality that is attributable to court-ordered desegregation plans that were enacted in many cities in the 1960s, 1970s, and 1980s, when many of these children were growing up. I take advantage of the wide variation in the timing and scope of implementation of desegregation plans to identify their effects. The identification strategy exploits differences in childhood exposure during school-age years to racially-integrated schools based on variation across school districts and across birth cohorts (1950-1975) in the timing of implementation of court-ordered desegregation plans. I measure the proportion of an individual's school-age childhood years (i.e., ages 5-17) in which they resided in a school district that had implemented school desegregation plans. I utilize the birth cohort variation in exposure to school desegregation among the broad range of birth cohorts (1950-1975) to identify effects on adult socioeconomic and health outcomes (see Figure 10).

Specifically, I employ a difference-in-difference framework and use variation across school districts and across birth cohorts to estimate equation (2):

$$(2) \quad Y_{icb} = \theta_0(b - T_c^*) \cdot D_{cb} 1(b - T_c^* < 0) + \theta_1(b - T_c^*) \cdot D_{cb} 1(0 \leq b - T_c^* \leq 12) \\ + \theta_2(b - T_c^*) \cdot D_{cb} 1(b - T_c^* > 12) + X_{icb}\beta + \eta_c + \lambda_b + \varepsilon_{icb}$$

where Y_{icb} represents an age-adjusted adult outcome of interest for individual i who turned 17 in year b and was raised/grew up in school district c ; and $(b - T_c^*)$ represents “Year Aged 17 – Year of Desegregation Plan Implementation” (i.e., year aged 17 relative to the year of court-ordered desegregation plan implementation). The adult outcomes of interest include: educational attainment, earnings, wages, work hours, family income and poverty status, whether ever incarcerated, and health status. The actual models estimated use all available person-year observations in adulthood (for ages 20-45) of the outcomes of interest with controls for age, age squared and age cubed to avoid confounding life cycle and birth cohort effects (equation (1) above abstracts from this feature to ease illustration). A spline specification is used to place some structure on the relationship between desegregation plan 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 key parameter of interest is θ_1 (relative to θ_0), where θ_1 captures the impact of each additional year of exposure to integrated schools, ranging from 0 to 12 years of exposure. Let k denote the number of years before or after the initial desegregation plan implementation that an individual turned 17, which is constructed from variables for year aged 17 (b) relative to the year of the initial desegregation plan implementation (T_c^*) in district c . Thus, $\theta_1 k$ gives the expected difference in adult outcomes between individuals who reached/became age 17 k years after initial desegregation plan implementation, relative to individuals who had reached age 17 the year prior to it (age 18 the year the plan was implemented). I use the year before the plans are implemented (court orders are enacted) as the reference point. The specification allows for the effects to manifest immediately following the first year

of implementation or child exposure 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".¹⁹

The identification comes from variation across school districts across birth cohorts in the adoption of school desegregation plans as distinct from trends due to other factors. The identifying assumption of the model is that, absent court-ordered school desegregation exposure during childhood, the black children would have experienced outcomes similar to those who grew up in those same communities but who had already reached age 18 prior to the desegregation plan implementation, conditional on (race-specific; region-specific) year of birth effects. Or, alternatively, their outcomes would have been similar to those who were born in same year and grew up in same region of the country but for whom desegregation plan implementation in their school district of upbringing occurred after they had reached age 18. The specification allows a partial test of this identifying assumption through its test of pre-existing time trends in outcomes prior to plan implementation and a break in this trend once desegregation plans go into effect. θ_0 captures the pre-period linear trend in outcomes prior to desegregation. θ_2 captures the post-plan linear trend for years beyond school-age (i.e., this represents years of exposure during pre-school years or prior to birth; for example, this enables a comparison of outcomes for children who grew up in districts that experienced desegregation throughout their school-age years, but was implemented at kindergarten/1st grade with those for whom it was implemented when they were 3 years old, and thus no difference in actual exposure during school-age years. This provides an important specification test in that the coefficient on θ_2 should be insignificant, if the results are consistent with (reflect) a causal impact of desegregation.

D represents a set of dummy indicators for the three spline intervals: years before plan went into effect ($b - T_c^* < 0$); school-age years of exposure ($0 \leq b - T_c^* \leq 12$); and years beyond school-age ($b - T_c^* > 12$). The model includes school district fixed effects (η_c) and birth cohort fixed effects (λ_b), and an

extensive set of controls for childhood family and neighborhood characteristics (X_{icb}). In a subset of specifications, I include a vector of birth cohort-by region of birth fixed effects to account for different trends in outcomes among individuals raised in treated districts in the South relative to the rest of the country. The models are estimated separately by race. (The county/school district fixed effects control for time-invariant community characteristics such as preferences for racial integration. The childhood race-region-year fixed effects control for race-specific time trends common to children at the region-year of birth level). The standard errors are clustered by school district.

I also estimate a variant of this model specification motivated by the hypothesis 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 compared with older students who have spent more time in segregated environments; and 2) secondary schools are more likely to track students by academic ability (and race), which could reduce benefits of desegregation for minorities. Specifically, the second model specification involves the estimation of equation (3):

$$Y_{icb} = \theta_0(18 - Age_{cb}^*) \cdot D_{cb}1(Age_{cb}^* \geq 18) + \theta_1 D_{cb}1(15 \leq Age_{cb}^* \leq 17) + \theta_2 D_{cb}1(11 \leq Age_{cb}^* \leq 14) + \theta_3 D_{cb}1(Age_{cb}^* \leq 10) + \theta_4(t - T_c^*) \cdot D_{cb}1(Age_{cb}^* \leq 5) + X_{icb}\beta + \eta_c + \lambda_b + \varepsilon_{icb}$$

where Age_{cb}^* represents the individual's age when the desegregation plan was first implemented in their school district of upbringing. The key parameters of interest include first exposure during high school (θ_1), junior-high/middle school (θ_2), or elementary school yrs (θ_3), relative to those who turned age 18 when the plan went into effect (i.e., no exposure). As in equation (2), childhood school-district specific trends in subsequent attainment outcomes (correlated with the timing of court orders) are a potential violation of the identification assumption. To assess this threat to the causal interpretation of the empirical estimates, this model includes an important specification test in that there should not exist a significant post-plan linear trend for years beyond school-age, if consistent with a causal impact of desegregation (i.e., θ_4 should be insignificant). Furthermore, θ_0 provides another test of pre-existing

time trends in outcomes prior to plan implementation. The similarity of trends in attainment outcomes in treatment and control groups in the period before initial court orders provides supportive evidence in favor of the identifying assumption.

In order to address endogenous delay in implementation of major desegregation plan following (exogenous) initial court order, I employ an IV/2SLS approach, where the initial year of the court order serves as an instrument for the year of major desegregation plan implementation. I use a simplified (more parsimonious) specification for the second-stage of the 2SLS/IV model:

$$Y_{icb} = \alpha + \delta SDP_{cb} + X_{icb}\beta + \eta_c + \lambda_{b(r)} + \varepsilon_{icb}$$

where SDP_{cb} represents the number of school-age years a child was exposed to integrated schools brought about through the implementation of a court-ordered major desegregation plan, and i , c , and b indexes individuals, school districts of upbringing, and the year in which an individual turned 17, respectively. The identification comes from variation across school districts across birth cohorts in adoption of major desegregation plans induced by quasi-random timing of initial court orders. These models include the same set of baseline controls for child-specific and childhood family factors as contained in the main difference-in-difference models. The latter part of Section VI provides more discussion of a variety of falsification exercises and specification tests performed.

Because I did not want to include endogeneous residential moves (e.g., residential moves induced by school quality changes that accompanied desegregation plan implementation), this analysis does not attempt to 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).²⁰ The resultant potential measurement error of school quality will tend to lead to attenuation bias of coefficients toward zero. The analysis does capture school district characteristics that were changing significantly from year to year. I control for childhood neighborhood characteristics in the models, including neighborhood poverty rates, and neighborhood and housing quality indices (more details of measures provided in Data Appendix).

Using Sibling Differences to Estimate School 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 because of the rapid changes that occurred over this period of their childhoods. Within sibling pairs that attended schools with different resources, the younger sibling experienced integrated schools for a longer period of childhood and typically had access to greater school resources as reflected in greater per-pupil spending and lower class sizes during adolescent years. The sibling comparisons evaluate adult health outcomes at the same age and controls for birth order, year of birth, birth weight, whether mother was married at birth, are included in all specifications.

The sibling difference approach is a complement to the primary difference-in-difference strategy. In particular, to the extent that one is concerned that the timing of court-ordered school desegregation implementation is not purely exogenous across cities, school district changes not driven by endogenous residential mobility will clearly be exogenous within families. One potential parental response to the presence of city differences in the timing and scope of implementation of school desegregation is to move to a different city. I restrict the sample to siblings who grew up in the same city to eliminate this source of bias.

That is, the sibling differences in school desegregation exposure during school-age years and school resources during adolescence are the result of policy-induced school regime shifts unlikely to be endogenous, especially within families. The sibling approach assumes parents treat their children similarly and do not reallocate resources within the family as a result of school desegregation.

In a subset of models across these empirical approaches, I add educational attainment to the model to examine how much of the effects of school desegregation and school quality on adult economic and health outcomes operate through effects on educational attainment.

VI. RESULTS

Educational Attainment. Table 2 contains estimates of the expanded difference-in-difference (DiD) model specifications of the effects of court-ordered school desegregation on the probability of graduating from high school (columns 1-4) and years of completed schooling (columns 5-7), respectively. The expanded DiD specifications permit partial tests of the identifying assumption. For high school graduation, the baseline model presented in column (1) includes race-specific year of birth and region of birth fixed effects with controls for gender, birth weight, and childhood family/neighborhood factors; the subsequent columns sequentially add childhood county fixed effects and school district fixed effects along with controls for changes in county per-capita government transfer programs. The average high school graduation rates for blacks and whites for these birth cohorts is 0.73 and 0.88, respectively (here those who earn GEDs are classified as dropouts following Heckman & LaFontaine (2007)).

The results indicate that each additional year of exposure to court-ordered desegregation leads to a 1.3 percentage-point increase in the likelihood of graduating from high school for blacks (coefficient on 0 to 12 years of exposure spline). These effects are large, statistically significant, and robust across the various model specifications. 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 (i.e., a standard deviation change) translates into a 6.5 percentage point increase in the likelihood of graduating from high school for blacks. The main effects pertain to exposure to desegregation court orders enacted after 1964 and the discussion of results will focus on them (the interaction term for pre-'65 court orders suggest smaller effects for early desegregation litigation that most often was not accompanied by major plan implementation within a few years of the court order). The results across the range of adult outcomes analyzed are insensitive to whether the sample is restricted to those who grew up in school districts that were ever subject to court orders at some point between 1950-90 (not necessarily during an individual's school-age years); when the full sample is used, models include a dummy indicator for whether the child's school district was ever subject to court order interacted with year of birth fixed effects.

Similarly, large, statistically significant effects of childhood exposure to court-ordered desegregation on completed years of education are found for blacks. The models shown in columns (5)-

(7) account for regional differences in secular trends and the regional pattern of the timing of initial court orders by including race-specific year of birth and region of birth fixed effects. The results indicate that each additional year of exposure to court-ordered desegregation leads to a 0.08 increase in years of education for blacks. As shown in Figure 11, the implied effects translate into roughly a full additional year of completed education when evaluating a change from no exposure to exposure to court-ordered desegregation throughout one's school-age years. Once again the results are robust, as the point estimates and their significance remain essentially unchanged with the inclusion of an extensive set of childhood controls, childhood county fixed effects, race-specific year of birth and region fixed effects (column 6 of Table 2), as well as school district fixed effects along with controls for changes in county per-capita government transfer programs (column 7). It is unsurprising that some of the estimated significant desegregation effects on blacks have wide confidence intervals in these expanded models given the sample size and how saturated these models are with layers of fixed effects. The various fixed effects included still permit sufficient identifying variation to detect effects.

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 desegregation plans implemented at different times. Credibility of the research design is supported by the fact that there is very little evidence of pre-existing trends in years of education before desegregation orders are enacted, but once court-orders are enacted, 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 across the various model specifications (as evidenced by the insignificant coefficient on the ">12" spline term).

In stark contrast, for whites there are consistently no significant effects found across the model specifications and the point estimates are negligible. The small, insignificant effects found for whites provides further evidence to rule out the competing hypothesis that the black improvements in educational attainment were driven by secular trends in desegregated districts. These results are highlighted in Figure

11 that displays the estimated effects of desegregation exposure for whites and blacks on the same graph for the probability of high school graduation and completed years of education, respectively.

I also estimate multinomial logit models of educational attainment, where the four categories are: High School Dropout/GED (reference category (0)); (1) High School Graduate, no college; (2) Attend College, no 4-year degree; and (3) 4-year College Graduate or more. The proportion of blacks (whites) across these educational attainment categories for the analytic sample are: 0.267 (0.125) for high school dropout; 0.353 (0.334) for high school graduate, no college; 0.313 (0.350) for some college, no 4-year degree; and 0.067 (0.191) for 4-year college graduate. This model enables one to examine whether the effect of school desegregation and the associated effects that duration of exposure had on blacks were limited to those on the margin of dropping out of high school, or whether such effects also led to increased college attendance and completion rates. We may expect that those whose school districts became integrated during their elementary school years may have experienced an increased likelihood of both high school graduation and college attendance due to longer exposure to higher quality schools. The models include the same set of baseline controls along with race-specific region of birth and year of birth fixed effects. The results presented in Figure 12 show the average effects of a 5-year exposure to court-ordered school desegregation (i.e., a standard deviation change), due to post-'64 orders, separately by race.

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. In particular, the results show that for blacks, on average, a 5-year exposure to court-ordered desegregation led to a 7.7 percentage-point reduction in the likelihood of dropping out of high school (equivalent to a 29% decline), and a corresponding increase of roughly a 4 percentage-point increase in the likelihood of graduating from high school with no college attendance, and nearly a 3 percentage-point increase in the likelihood of becoming a 4-year college graduate (equivalent to a 40% increase). In contrast, once again no significant effects are found for whites across any of the educational attainment categories.

Table 3 presents the 2SLS/IV estimates of the effects of major desegregation plan implementation on the probability high school graduation and years of education, respectively, by race. This set of analyses use the year of the initial court decision, intersected with a child's school-age years of exposure, as an instrument for the initial year of major desegregation implementation and resultant childhood exposure to major desegregation plans. The first-stage results are highly significant and displayed in column (1) of Table 3. The sample includes original sample PSID children born between 1951-70 who grew up in large school districts that implemented major desegregation plans sometime during the 1960s or 70s (Welch/Light desegregation data). The models include race-specific controls for year of birth fixed effects, gender, age at most recent survey interview, and childhood family/neighborhood factors.

As shown, the results strongly reinforce the previous findings and indicate a parallel set of significant effects of comparable magnitudes for both high school graduation and years of completed education among blacks. For example, the results imply that a year of exposure to major desegregation plans led to a 2.9 percentage-point increase in the likelihood of graduating from high school and a 0.08 of a year increase in education attainment (the identical point estimate found for blacks in the models presented in Table 2 (columns 5-7)). No significant effects are found for whites.

Men's Labor Market Outcomes & Adult Family Income and Poverty Status.

The next series of regression results presented reveal large, significant effects of court-ordered desegregation on black's adult economic status and labor market outcomes, using the same sequence of model specifications. Table 4 presents desegregation effects by race on adult economic outcomes, including men's annual earnings (column 1-3), wages (column 4), annual work hours (column 5), and family income-to-needs ratio (column 6) and poverty status among men and women (columns 7-9). In light of the parallel set of findings across all these long-run economic outcomes, these results are discussed in succession and are highlighted in Figure 13. All models control for the following set of child family/neighborhood background factors: parental income, parental education, mother's marital status at birth, birth weight, parental smoking and alcohol use, neighborhood poverty rate, and neighborhood and housing quality indices, and columns (6)-(9) control for gender; all of the economic outcome measures

have been converted to 2000 dollars. The models include flexible controls for age (quadratic) and analyze adult economic outcomes for ages up to 45 to avoid conflating birth cohort and life cycle effects.²¹

The results indicate that an additional year of exposure to court-ordered desegregation significantly increases black men's annual earnings by roughly 5 percent (column 3), which is the combination of a 2.9 percent significant increase in wages (column 4) and an increase in annual work hours of 39 hours (column 5). Furthermore, among black men and women, an additional year of exposure to court-ordered desegregation significantly increases the family income-to-needs ratio by about 0.1 (column 6) and leads to a reduction in the annual incidence of poverty in adulthood of between 1.6-1.9 percentage points (depending on specification, columns 7-9). As shown in columns (3) and (9), these results are robust to the inclusion of childhood county fixed effects, race-specific year of birth and region of birth fixed effects, along with controls for childhood family and neighborhood factors, and changes in county per-capita government transfer programs. These effects witnessed for blacks represent substantial improvements in adult economic status, as evidenced by the fact that the average effects of a 5-year exposure to court-ordered school desegregation (i.e., a standard deviation change), due to post-'64 orders translates into about 25 percent increase in annual earnings, reflecting the combination of a 15 percent increase in wages and an increase in annual work hours of 195 hours. Furthermore, the results indicate that the average effects of a 5-year exposure to court-ordered school desegregation lead to about a 0.5 increase in the family income-to-needs ratio and about a 9 percentage-point decline in the annual incidence of poverty in adulthood for blacks.

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 (as shown by the insignificant pre-desegregation coefficients on the "<0" 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 and various model specifications (as shown by the insignificant coefficient on the ">12" 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, as highlighted in Figure 13 and Table 3. The point estimates are negligible for whites. Once again, these represent important specification tests that are affirming of the credibility of the research design and simultaneously rule out several competing explanations for the pattern of results.

Effects of Court-Ordered Desegregation by Induced-Change in School Quality for Blacks.

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 different ways across school districts and was applied in many different initial school environments accordingly based on the different forms that racial segregation took—*de jure* in the South and *de facto* in other regions of the country. I augment the primary model specifications to investigate whether impacts appear to differ by the scope of desegregation (as proxied by the estimated (residual) change in per-pupil spending (school segregation) implied by the models estimated in Section III that are net of region-specific trends and time-invariant school district characteristics). For each district, I compute the change in school district per-pupil spending induced by the court-order from the year preceding enactment to the first several years following implementation. 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 educational and adult economic attainment outcomes among blacks.

The results are presented in Table 5. The sample for this subset of analyses is restricted to PSID original sample black children who grew up in school districts that were initially subject to court order sometime after 1963 for which I have school district per-pupil spending (school segregation) information 1 year before and 3 years after initial court order, obtained from school district finance data (1962-82) and OCR school data (1968-82). The estimated district-specific induced-change in per-pupil spending (school segregation) are net of school district fixed effects and region-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 Figures 7 and 9). These models include the same set of control variables as in Tables 2 and 4.

For both black's educational and adult economic attainments, the results presented in Table 5 suggest that changes in school quality resulting from the integration of schools 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 resulted in larger improvements in school quality (reflected at least in part by larger increases in per-pupil spending) are shown to result in more beneficial educational and adult economic outcomes for blacks who grew up in those court-ordered desegregation districts. To facilitate interpretation of marginal effects, the units of the per-pupil spending are in thousands of dollars, so that a 1-unit change represents a \$1,000 change in spending (2000 dollars). Thus, the results indicate that each additional year of exposure to school desegregation that resulted in an additional \$1,000 increase in per-pupil spending led to educational attainment among blacks that was about 0.08 of a year higher than the average improvement in years of education among blacks induced by school desegregation. This effect translates into roughly a 0.9 of a year increase in educational attainment when evaluating a change from no exposure to exposure to court-ordered desegregation throughout one's school-age years. As shown in column (3), these effects persist after the inclusion of corresponding increases in the black-white exposure index that accompanied desegregation. On the other hand, there is suggestive evidence that reductions in school segregation levels that were not accompanied by significant changes in school resources (as reflected in per-pupil spending) did not have appreciable long-run impacts on black's education and economic attainments.

Conversely, the results indicate that exposure to school desegregation throughout one's school-age years that resulted in an additional \$1,000 increase in per-pupil spending led to a family income-to-needs ratio that was about 0.6 higher and an annual poverty incidence in adulthood among blacks that was 6.8 percentage points lower than the average effect induced by school desegregation. This evidence is

consistent with a dose-response relationship between the resultant change in school quality that accompanied desegregation for blacks and the duration of exposure to school desegregation.

Table 6 presents the 2SLS/IV estimates of the effects of major desegregation plan implementation on men's annual earnings (columns 1-2), wages (columns 3-4), annual work hours (columns 5-6), and family income-to-needs ratio (columns 7-8) and poverty status among men and women (columns 9-10), respectively, by race. The models include the same set of controls as in Table 3. As shown, the results strongly reinforce the previous findings and indicate a parallel set of significant effects of comparable magnitudes for each of these adult labor market and economic status outcomes among blacks.

For example, the results imply that a year of exposure to major desegregation plans significantly increases black men's annual earnings by 5.8 percent (column 1), which is the combination of a 2.1 percent significant increase in wages (column 3) and an increase in annual work hours of 39 hours (column 5). Furthermore, among black men and women, an additional year of exposure to major desegregation plans significantly increases the family income-to-needs ratio by 0.04 (column 7) and leads to a significant reduction in the annual incidence of poverty in adulthood of 2.2 percentage points (column 9). In contrast, I find small, insignificant effects on whites across each of these economic outcomes in adulthood. The pattern of results and magnitudes of effects are very similar to those reported in the models presented in Table 4.

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 regression results presented reveal large, significant effects of court-ordered desegregation on black men's annual incidence of incarceration, probability of ever being incarcerated by age 30, and probability of any deviant behavior (defined as ever being expelled/suspended from school, charged with a crime, or incarcerated), using the same sequence of model specifications. Among men, the proportion of blacks (whites) that were ever

incarcerated by age 30 is 0.212 (0.080), and the corresponding proportion for any deviant behavior is 0.376 (0.258), for this sample of birth cohorts. Table 7 presents effects by race on these outcomes for men, where columns (1)-(4) display the linear probability model (OLS) estimates of the effects of court-ordered desegregation and columns (5)-(6) display the 2SLS/IV estimates of the effects of major desegregation plan implementation. The model specification used is a variant of the ones utilized in the prior models, which serve to highlight the larger reduction in the likelihood of incarceration among blacks who experienced desegregation during their elementary school years, and thus were exposed to integrated schools throughout their childhood years (relative to those with more limited exposure). The models include the same set of baseline controls as in the previous models presented.

As shown, the results indicate that relative to growing up in segregated schools throughout one's school years, for blacks, exposure to desegregation beginning in one's elementary school years leads to a 22.5 percentage-point reduction in the probability of deviant behavior (column 1), a 14.7 percentage-point reduction in the probability of incarceration by age 30 (column 2), and a 3.8 percentage-point decline in the annual incidence of incarceration during ages 20-34 (i.e., during the peak ages of criminal involvement) (column 3). 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 of desegregation court orders that correspond with one's pre-school years, which represent two important specification tests that support the validity of the research design. These differences are less dramatic when comparisons are made for smaller increments of desegregation exposure. Importantly, I find no desegregation effects on the probability of incarceration for white men (column 4), which follows the pattern of results for educational attainment by race.

Similarly, the 2SLS/IV estimates of the effects of exposure to major desegregation plans throughout one's school-age years (relative to no exposure) imply about an 8 percentage-point reduction in the annual incidence of incarceration and the probability of ever being incarcerated by age 30 for black men (column 6), with small insignificant effects for white men.

Adult Health Status

Scholars have long hypothesized that education has a causal effect on subsequent health, though the precise channels through which education influences adult health have not been well established in empirical research to date (Cutler and Lleras-Muney, 2006). Education has been shown to be a very strong correlate of health status in cross-sectional work, and this is true across generations. 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 regression results presented reveal large, significant improvements in adult health status among blacks resulting from exposure to court-ordered school desegregation, using the same sequence of model specifications. The main health outcome analyzed is self-assessed general health status (GHS). GHS is highly predictive of morbidity measured in clinical surveys, and it is a powerful predictor of mortality, even when controlling for physician-assessed health status and health-related behaviors (Benyamini and Idler, 1999). In order 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) (further details provided in the Data Appendix). 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 in this paper 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 the 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 regression results are presented in Table 8. The results indicate that an additional year of exposure to court-ordered desegregation (due to post-‘64 court orders) significantly increases the adult health status index for blacks by between 0.3-0.6 points (columns 1-3, depending on specification). As shown in column (3), these results are robust to the inclusion of childhood county fixed effects, race-specific year of birth and region of birth fixed effects, along with controls for gender, birth weight, child health insurance coverage, childhood family and neighborhood factors, and changes in county per-capita government transfer programs. These effects witnessed for blacks represent substantial improvements in adult health status, as evidenced by the fact that the average effects of a 5-year exposure to court-ordered school desegregation (i.e., a standard deviation change) translates into about a 3 point increase in the adult health status index (based on column 2).

A useful way to interpret the estimate is in relationship to the size of the effect of age on health, with the impact of each additional year of desegregation exposure for blacks equivalent (on average) to blacks reaching a level of health deterioration about 1 year later than they would have if that year was instead spent in segregated school regimes. 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. Additionally, there is little evidence of a pre-existing time trends for these health outcomes observed for blacks 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, I once again find negligible desegregation effects on the adult health status of whites.

Furthermore, the results presented in column (4) of Table 8 suggest that changes in school quality resulting from the integration of schools played an important role. I find that court-ordered desegregation that resulted in larger improvements in school quality (reflected at least in part by larger increases in per-pupil spending) are shown to result in more beneficial health outcomes for blacks who grew up in those court-ordered desegregation districts. The results suggest that exposure to school desegregation

throughout one's school-age years that resulted in an additional \$1,000 increase in per-pupil spending led to an adult health status index that was about 4.5 points higher than the average effect induced by school desegregation.

Similarly, the 2SLS/IV estimates imply that the effects of 5 years of exposure to major desegregation plans (relative to no exposure) result in about a 2.6 point increase in the adult health index for blacks (column 5), with insignificant desegregation effects found for whites (column 6).

The results presented in the first column of Table 9 are sibling fixed effect models designed to assess the long-run effects of school desegregation on adult health. I find that black children who were exposed to implemented, court-ordered school desegregation for the majority of their school-age years experienced significantly improved health outcomes in adulthood as compared with their older siblings who grew up in segregated school environments with weaker school resources (controlling for age and birth cohort effects). I find that 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. The results, as a whole, suggest that benefits for minority children do not come at the expense of white students.

As shown in column (2) of Table 9, the sibling fixed effect results reveal that individuals who attended schools during their adolescent years with higher per-pupil spending as compared with levels that prevailed when their siblings were adolescents experienced better subsequent health outcomes in adulthood (evaluated at the same age). The identification of these effects is driven largely by significant per-pupil spending increases in a relatively short period of the 1970s in many areas. I find little evidence that observable differences among siblings are related to differences in the quality of the 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 health status. I find similar patterns using the sibling fixed effect models for the educational and socioeconomic attainment outcomes (these additional results are suppressed to conserve space; available upon request).

The results across the main set of adult attainment outcomes analyzed using the expanded difference-in-difference model specifications are summarized in Figure 14. The primary identification strategy hinges on the assumption that there are no underlying trends in the average school quality inputs and subsequent attainment outcomes of school districts that are correlated with the timing of the initial court order. This assumption was evaluated directly in event study analyses. These results show strong evidence supporting the exogeneity of the initial court order. First, the pre-period trend is flat, showing no systematic differences in school district trends prior to the initial court order. Second, school quality inputs increased sharply during the first several years after court-ordered desegregation was first enacted (relative to the levels of segregation, per-pupil spending, class size, respectively, that prevailed one year prior to the court order). The long-run impacts exhibit a similar pattern: 1) no systematic evidence of pre-existing time trends in the years *before* these court orders are enacted; and 2) subsequent adult attainment outcomes improve significantly with duration of exposure to desegregation up to school-age years and not thereafter for blacks, with no effects found for whites. This provides strong evidence for the validity of the identification strategy, as any confounding factor would have to very closely mimic the timing of the initial court order (and subsequent plan implementation) to result in a pattern like this. The small, insignificant effects found for whites provides further evidence to rule out the competing hypothesis that the black improvements in outcomes were driven by secular trends in these outcomes in desegregated districts. Furthermore, black children's subsequent adult outcomes improved most among those who were from districts that experienced the largest changes in school quality inputs following desegregation. Additionally, Weiner et al. (2010) report no systematic relationship between the timing of these court orders and either the level or change in political composition of local federal courts.

I estimate the extent to which the black-white gap in completed education, and adult economic and health status narrowed as a result of childhood exposure to school desegregation (i.e., I compare the black-white gap in the child cohorts that experienced school desegregation plans relative to the black-white gap in cohorts just prior to school desegregation), and the results imply a leading contributing role

of school desegregation in narrowing the gaps in socioeconomic and adult health outcomes witnessed for these birth cohorts.

The difference-in-difference estimates 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. The increase in subsequent adult economic and health status among African Americans for successive cohorts born between 1950 and 1975 mirrored the improvements in access to school quality that accompanied school desegregation during their school-age years. African-Americans who attended integrated schools during their elementary school years appear to benefit more than those exposed to integrated schools only later in the school careers, which is consistent with a treatment dose-response relationship.

Robustness & Falsification Tests

The baseline specification was 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). For example, adding controls for dimensions of school quality in a school district of upbringing in years the individual was *not* in school (not of primary or secondary school age) (i.e. when the individual is not between the ages of 6 and 18) does not significantly alter the results. The estimated effects on adult outcomes of per-pupil spending in years in which the individual was not in K-12 schooling are very close to zero, and the effects of experienced per-pupil spending remains significant and essentially unchanged. This is what we would expect if endogeneity issues are not driving the results. This finding confirms that the results do not simply reflect community-level differences in attitudes about the importance of education that are correlated with determinants of adult attainments. The lack of significant effects of court-ordered school desegregation in periods that correspond with years beyond school-ages, also eliminates the concern that there is a monotonic relationship between subsequent (age-adjusted) attainment outcomes and the timing of the

initial court order that (instead) reflect secular trends in outcomes that would have prevailed in the absence of school desegregation.

As an additional way of evaluating the validity of the identifying assumption of the model, I tested whether exposure to court-ordered desegregation is uncorrelated with changes in child county per-capita transfer payments from income-support programs that might influence outcomes under consideration, conditional on the controls already in specification (1) above. If the identifying assumption of the model (namely, that the timing of the initial court order is otherwise unrelated to trends in subsequent outcomes) holds, then we might expect the estimates to change very little with the addition of these characteristics (correlates of outcomes). As witnessed in the results presented, these additional controls have very little impact on the coefficients of interest, which is not surprising given that significant relationships between these government transfer programs and desegregation exposure arise about as often as would be expected through pure chance. The broad set of childhood family/neighborhood controls have the expected signs and significantly improve the precision of the coefficients of interest (and overall fit of the model).

Table 10 probes the robustness of these estimates further. As an additional falsification exercise, I re-estimated equation (3) replacing the timing of initial court ordered desegregation variables with litigation cases that were not successful and the corresponding year of their court ruling to identify effects; in essence estimating the effects of a series of “placebo” initiatives. If my baseline estimates are capturing the effects of school desegregation – not some 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, as shown in Table 10. 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. 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. This provides additional evidence that the main results are not spurious findings and helps rule out confounding influences from changing local demographic characteristics or social policies. If such omitted variables are spuriously inflating 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 plan 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 results (i.e., other factors that happen to be changing at the same time these desegregation orders are implemented). Based on the robustness of the results, such an alternative explanation would have to be a cause that meets the following very strict criteria: a) it closely follow the timing of desegregation (given the evidence showing no pre-existing time trends); b) yet it be geographically confined to the specific school districts that were undergoing desegregation implementation (given the robustness of the results to the inclusion of race-specific year of birth and 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); had the largest impacts on blacks in communities where desegregation resulted in the largest changes in school quality inputs; and finally e) had no effects on whites. The results support a causal interpretation of the effects of school desegregation by uncovering a sharp difference 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 Mechanisms. The analysis cannot cleanly identify the mechanism 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 most obvious channel through which these child school-related impacts manifest is through their effects on educational attainment and adult earnings, which in turn influence adult health. To provide some suggestive evidence of the importance of this pathway, I examine to what extent the estimated effects of school desegregation on subsequent adult outcomes (probability of incarceration, and adult economic and health status) are reduced once measures of educational attainment are included. The results strongly suggest that the increases in the quantity and quality of educational attainment among blacks that resulted from desegregation played a central role in the subsequent improvements in adult economic status and health status experienced for these cohorts. I find that a significant part of the impacts was the result of a combination of increases in the levels of educational attainment and in the returns to education. There is also some evidence that measures of school quality inputs steepen the education slope (not shown).

I hypothesize that the effects likely depend on desegregation program type and student characteristics. Various unreported specifications assessed whether the reduced-form effect of court-ordered desegregation plans on subsequent attainment outcomes differ by region, size of total enrollment, proportion minority, segregation levels prior to litigation, desegregation plan type, and several other school district characteristics. There is no evidence that the effects vary by these characteristics. I find that the estimated effects of desegregation court orders on adult economic and health status are similar for the subset of black children who grew up in the South and those who grew up in other regions of the country (with the inclusion of the set of controls). The lack of heterogeneity in effects between southern and non-southern school districts is particularly noteworthy.

In supplementary analyses, I also investigated whether school desegregation had any measurable impacts on parental and neighborhood-level average expectations for child achievement among minority families and neighborhoods. While far from providing definitive evidence on this, the results show that

school desegregation exposure was associated with increases in parental and neighborhood-level average expectations for child achievement for these cohorts, independent of other childhood family socioeconomic factors and time trends.

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 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 (jointly) predict the year of the initial court order. Second, I estimate event study models that further support the validity of the research design. Third, I perform a variety of robustness checks to test the validity of the identifying assumptions.

The findings of this study contribute to the literature in several important ways. First, this study is the most comprehensive to date on the topic, especially in terms of the range of empirical approaches utilized, broad set of outcomes analyzed, and the long time horizon considered. Second, this paper provides important and 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 implementation of desegregation plans during the 1960s, 70s and 80s to identify these effects. I find that school desegregation significantly increased educational attainment among blacks exposed to major desegregation plans during their school-age years, with impacts found on completed years of schooling, the likelihood of graduating from high school, attending college, and graduating with a 4-year college degree. The analysis disentangles the effects of neighborhood attributes and school quality.

Difference-in-differences 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 outcomes and reductions in adult poverty incidence for blacks. This research highlights the important role that school quality plays in influencing the risk of dropping out of high school, incarceration, the likelihood of graduating from college, and adult earnings, which in turn affect later-life health. The significant long-run impacts of school desegregation found for blacks with parallel findings across a broad set of socioeconomic outcomes and health status indicators of well-being, with no corresponding impacts found for whites, is striking.

The results suggest that the mechanisms through which school desegregation led to beneficial socioeconomic outcomes in adulthood for blacks include improvement in access to school resources, which is reflected in reductions in class size and increases in per-pupil spending. Furthermore, the evidence is consistent with a dose-response effect of school quality improvements and the duration of exposure to them on subsequent attainments in adulthood. African-Americans who attended integrated schools during their elementary school years appear to benefit more than those exposed to integrated schools only later in the school careers. 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.

Putting the magnitudes in perspective in relation to previous studies.

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 the extent to which 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 (averaging 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. In their summary of the school literature, Card and Krueger echo this concern, “In our view, the most important omitted variables [in previous studies] are likely to be measures of family background and characteristics of the areas in which individuals attended school” (p. 113). A strength of the analyses contained in this paper, in addition to its credible research design, is both the extensive set of controls for childhood family and neighborhood characteristics and the ability to follow adult attainment outcomes into one’s peak earnings years through age 45.

The study most directly related to the approach taken in this paper is Guryan (2004), who uses variation in the timing of desegregation plan implementation in the 1970s and 1980s to identify the effects of school segregation on black high school dropout rates for a subset of large school districts (using 125 districts from Welch/Light desegregation data). Using data from the 1970 and 1980 censuses, he uses difference-in-difference and fixed effect methods and finds that desegregation explains $\frac{1}{2}$ of the decline in the black high school dropout rate during the 1970s among the 125 large school districts he analyzed that implemented plans over that time period. Guryan (2004) reports IV estimates that are two to four times larger in magnitude than OLS estimates. This pattern is consistent with the findings of this study. One explanation for the larger estimated effects in this paper (as was the case with Guryan’s IV estimates) 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. We witnessed this in the results from Table 1, in which there was systematic evidence of 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. These factors likely lead OLS estimates of the effects of desegregation plans to be understated.

The findings of the present study show that labor market outcomes, and adult income and health status rose in line with black's educational improvements, as did declines in the incidence of incarceration, with private rates of return as high as 30 percent for those who experienced integrated schools throughout childhood (relative to those who grew up in segregated schools). A Wald estimate of the returns to education (reflecting a combination of both increased quantity and quality) on wages is the ratio of the estimates of the desegregation effects on wages (Table 6) and completed years of education (Table 3), yielding a return of 25 percent (0.02/0.08). These estimates are notably larger than the 8 to 14 percent returns typically estimated using modern era schooling interventions and data sources from more recent (younger) birth cohorts (e.g., Card, 1999). 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.

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 and thus boosted the return to a year of school. A second possibility is that the returns to schooling for those who were most impacted by school desegregation plans 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. This could arise if marginal returns are higher for those with low levels of schooling and the instrument (e.g., school reforms, school accessibility) mainly affects this segment of the population by lowering the costs of schooling. It seems plausible that desegregation disproportionately benefited those students with high costs of schooling and with especially high marginal rates of return.

Finally, the data and methods improve upon prior research, which lacked access to panel data which follow children from birth to adulthood, relied on aggregate state-level analyses, and/or failed to address the endogeneity of residential location.

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 (0.24 vs. 0.12 standard deviations on math and reading student test scores for each grade). Krueger and Whitmore (2002) find that this result is largely driven by a larger treatment effect for all students regardless of race in predominantly black schools, suggesting that benefits from additional resources are higher in such schools. There was also a small, positive within-school interaction between small class and an indicator variable for black students, which means that black students gain a little more from small classes than their white classmates do (Krueger and Whitmore, 2002). They also report black males assigned to small classes are 1.2 percentage points less likely to be convicted of a crime and committed crimes that carried about 24 percent less time, on average, than their peers in regular-sized classes back in elementary school, but these impacts were imprecisely measured. The Project Star experimental results estimate the impact of a seven-student reduction in a given year. Relatedly, Lochner and Moretti (2004) 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 for blacks by roughly two-thirds.

Consider the estimated “first stage” effect of court-ordered desegregation on average class size (per-pupil spending) among blacks using a similar specification as the long-run models (Figures 8 & 9), along with the fact that teachers comprise the largest component of school spending. If we use average class size during one’s school-age years as a marker of school quality inputs and make the restrictive assumption that the level of school resources was the only channel through which desegregation influenced outcomes, then the implied effect on blacks of a one-student reduction in the average class size experienced beginning in elementary school through high school on the likelihood of graduating from high school is 3.7 percentage points, increases completed years of education by 0.1 of a year, increases adult earnings by 7.5 percent, translates into a 4 percentage-point reduction in the incidence of adult

poverty, decreases the annual incidence of incarceration among males by 1.2 percentage points, decreases the probability of deviant behavior among males by 3.1 percentage points, and increases the adult health index by 2.3 points (on par with the impacts of parental education) (as shown in Table 11). These results are intended to be interpreted broadly as capturing the composite effects of school quality changes experienced by blacks that were induced by school desegregation, which may include an amalgam of peer effects, school resource effects, and teacher quality effects; class size serves here as a marker for these school quality changes.²² Of course, school desegregation likely affected outcomes through other avenues as well, so this interpretation should not be taken literally but is meant as an exercise to help gauge the magnitudes. Thus, these results are only suggestive since the potential pathways that may have resulted in these long-run impacts may (or may not) be fully captured by the estimates.

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

Finally, this paper is among the first to provide evidence to assess the extent and ways in which childhood school quality factors causally influence later-life health outcomes. The results suggest that both childhood school quality factors play important roles in the intergenerational transmission of economic status and influence adult health outcomes (through their influence on the socioeconomic mobility process). The results indicate that both family background and school quality during childhood serve as primary gatekeepers of the intergenerational transmission of adult health status and play a large role in producing racial health disparities. The results indicate that school desegregation significantly

narrowed black-white adult health disparities for the cohorts exposed to integrated schools during childhood. 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. Small and statistically insignificant results are found across each of these adult outcomes for whites, and thus, suggest that benefits for minority children do not come at the expense of white students. The results are robust to a battery of falsification tests and model specification checks.

The evidence collectively paints a consistent picture of significant later-life health returns of school quality. The analysis documents significant black-white differences in adult health that narrowed for successive cohorts born between 1950 and 1970. Racial inequality in school quality varied significantly across school districts, differed by school characteristics, and narrowed over this period. The quality of black children's education improved in quantity and quality in both absolute and relative terms. 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 in this paper. 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. It is important to bear in mind that these gains may have occurred against the backdrop of countervailing influences, such as the rise in single-parent families, concentrated poverty, deterioration of neighborhood conditions for low-income families with the exodus of the middle class to the suburbs, and sentencing policy reforms during the mid-1980s and 90s that sky-rocketed incarceration rates among African-Americans. This may account for the increasing heterogeneity in outcomes witnessed among blacks in successive cohorts since this period.

The results may have implications for policy in the context of the current economic and legal environment. Racial segregation in public schools fell sharply from 1968 until the early 1970s, remained constant throughout the remainder of the 1970s, and has increased slightly since then (Orfield, 1983; Boozer, Krueger, and Wolkon, 1992). Yet today public schools are somewhat more segregated than they were in the early 1980s (Clotfelter, 2004; Rivkin, 1994). Prior to the 1970s segregation in schools was largely attributable to segregation patterns within districts, while today it is increasingly attributable to residential location patterns between districts (Lankford and Wyckoff, 2000) and the tracking of students within schools. School districts under a court-ordered desegregation plan are monitored by the courts. However, the Supreme Court issued three rulings in the early 1990s that significantly altered the legal basis for court-mandated desegregation (see for example, Lutz, 2005). It became easier to terminate court-ordered desegregation plans and return school control to local authority without external monitoring of minority student performance, which may result in reduced school resources targeted for minority students. Two recent studies by Clotfelter, Ladd, and Vigdor (2005) and Lutz (2005) find that dismissal of court-ordered desegregation plans led to increases in racial school segregation and increased black high school dropout rates. This removal of court oversight has resulted in an increased likelihood of a return to neighborhood schooling and re-segregation of public schools. At the federal level, this represents a movement away from court-ordered desegregation as a central tool to improve school quality. There has been an erosion of public attitudes and support for the perspective that schools must be integrated in order for blacks to receive a high quality education. Only limited research evidence has considered the question of the potential harm from the increasing trend in dismissal of desegregation orders. (That is, will court's dismissal of desegregation plans reverse gains achieved by their implementation?)

The results herein demonstrate that education policies can have substantial effects on future health. The lessons that can be gleaned from the particular case of court-ordered school desegregation and its long-run consequences are relevant for contemporary debates about school reforms and equity of school finance. Given the importance of local finance in K-12 public education, the impacts that residential segregation has on the distribution of educational resources across public school districts may

continue to be significant. There remains considerable variation across states in spending per public school student, with per student spending in the top five states being roughly a third to more than two-thirds greater than the national average, and close to twice the expenditures for the bottom five states (National Education Association data for 2004–2005). Within states, local funding, primarily from property taxes, represents more than 40 percent of revenues for primary and secondary education, contributing to inequities in educational resources across school districts and neighborhoods.

Additionally, teachers' salaries have declined in real terms and also display wide variation across states, and states and school districts face challenges in recruiting and retaining well-qualified teachers in areas such as science and math (Dillon, 2007).

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 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.²³ 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.

¹ The PSID oversampled low-income families and blacks, which enables sufficient sample sizes to analyze race differences in adult attainments. Probability sample weights are used to produce nationally-representative estimates.

² 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).

³ 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.

⁴ A more complete explanation of sources for the desegregation case data and its construction is contained in the Data Appendix.

⁵ 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.

⁶ 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.

⁷ 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.

⁸ The models estimated upon which Figures 6, 8, and 9 are based also include dummy indicators for 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 (plan implementation).

⁹ 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.

¹⁰ This part of the research design is similar in setup to a recent study by Reber (2007) on the impacts of court-ordered school desegregation on indices of racial school segregation.

¹¹ 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.

¹² 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.

¹³ 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.

¹⁴ Taken together, the results presented for all school districts that implemented school desegregation plans over this period are consistent with evidence Reber (2007) found for Louisiana. Namely, she found that in Louisiana, between 1965 and 1970, when court orders were enacted, they were accompanied by large increases in school funding resources for black students, where the infusion of state funds was used to “level-up” school spending in integrated schools to the level previously experienced only in the white schools.

¹⁵ Among districts that took major steps to desegregate, the implementation of desegregation was followed by substantial positive changes in reported community-wide attitudes toward school desegregation in a majority of school districts. Serious disruptions to education process were reported in less than 20 percent of districts that underwent desegregation implementation between 1966-75 (Office of Civil Rights Report, 1977).

¹⁶ The PSID maintains extremely high wave-to-wave response rates of 95-98%. Appendix A discusses the extent to which sample selection, including mortality, may bias the reported estimates. Studies have concluded that the PSID sample of heads and wives remains representative of the national sample of adults (Gottschalk et al, 1999; Beckett et al, 1997).

¹⁷ For a significant share of the individuals in our sample who were children in 1968, 1984 represents roughly the year in which they became heads of households as adults.

¹⁸ This annual data alone on incarceration has limitations. Among the most important is that this will only identify incarceration in a given year if it was 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 1995 wave added a crime history module to the PSID including information on whether respondents had 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.

¹⁹ 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).

²⁰ Among original sample children in the PSID, the average proportion of childhood spent growing up in the 1968 neighborhood was roughly two-thirds.

²¹ Columns (1)-(4) simplify the exposure specification by not including pre-'65 court order interaction terms because of the smaller male-only sample for labor market outcomes; similar patterns of results when interactions are included. The interaction terms of pre-'65 court orders with the other spline segments (columns (5)-(9)) are suppressed to conserve space.

²² The typical reduction in the average student-to-teacher ratio induced by desegregation for these black birth cohorts was about 3-4 students. The typical variation used to identify the effects in the model is smaller, since the student-to-teacher ratio is averaged across one's school-age years; a standard deviation change in the avg student-to-teacher ratio is 2.7.

²³ Recent examples include Chay, Guryan, and Mazumder (2009) (desegregation of hospitals and academic achievement), Almond, Chay and Greenstone (Civil rights and infant mortality), Finkelstein & McKnight (Medicare introduction), Cascio, Gordon, Lewis and Reber (Title I), Ludwig and Miller (Head Start), Almond, Hoynes and Schanzenbach (food stamps and birth outcomes), and McCrary (court-ordered police hiring quotas).

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Figure 1.

SCHOOL SEGREGATION, 1952

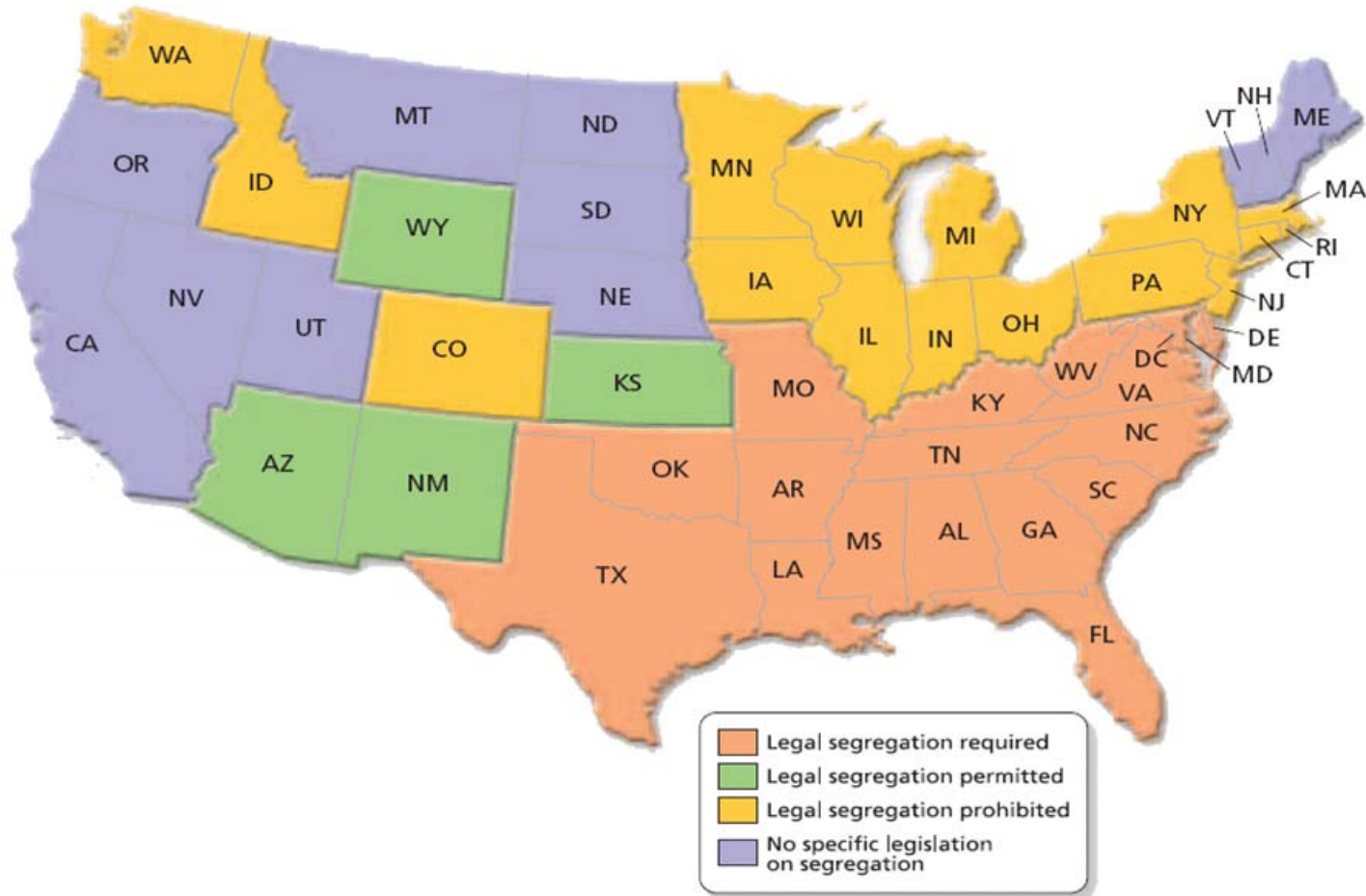
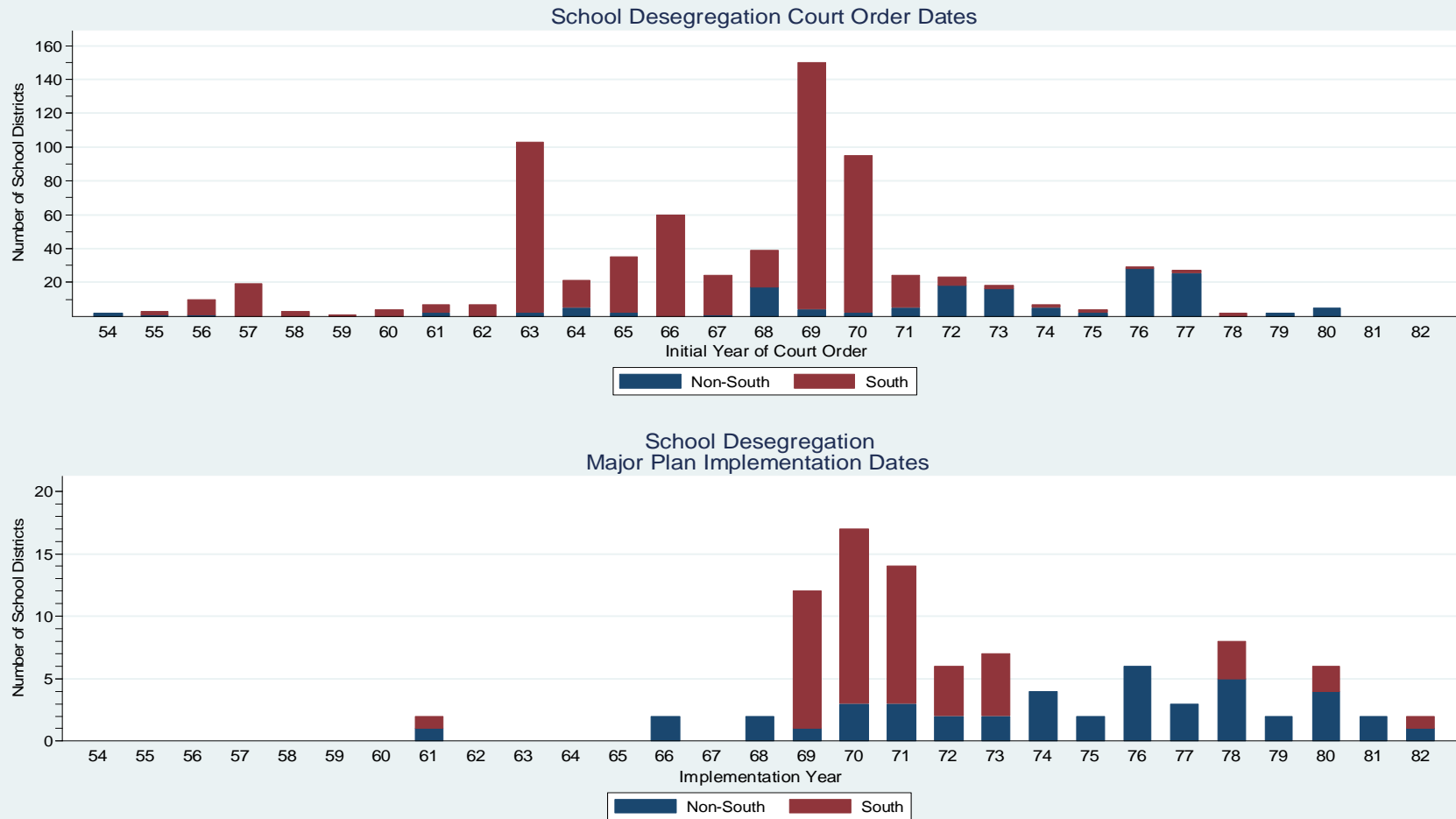


Figure 2.

School Desegregation Court Order & Plan Implementation Dates



(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 3.

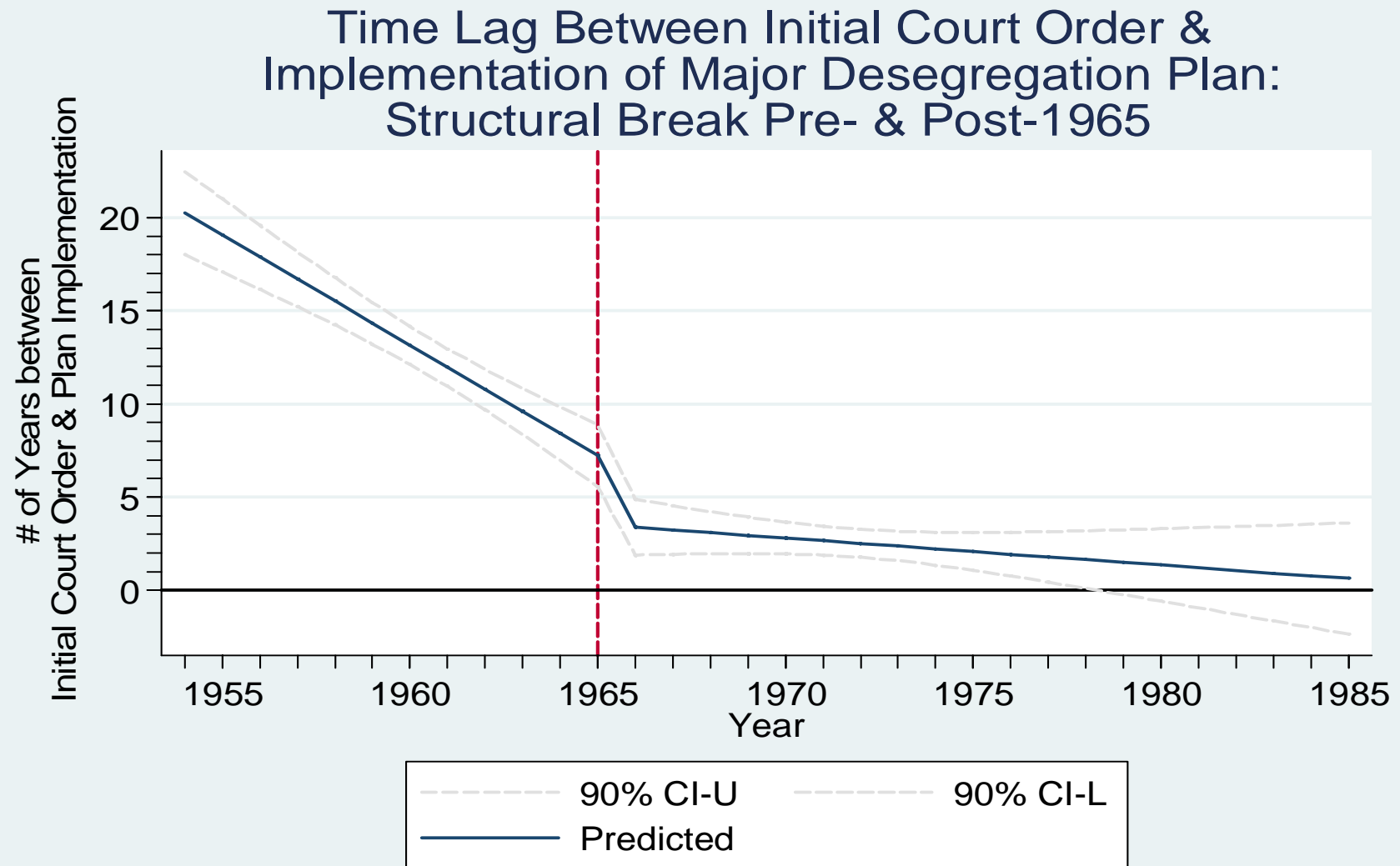


Figure 4

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

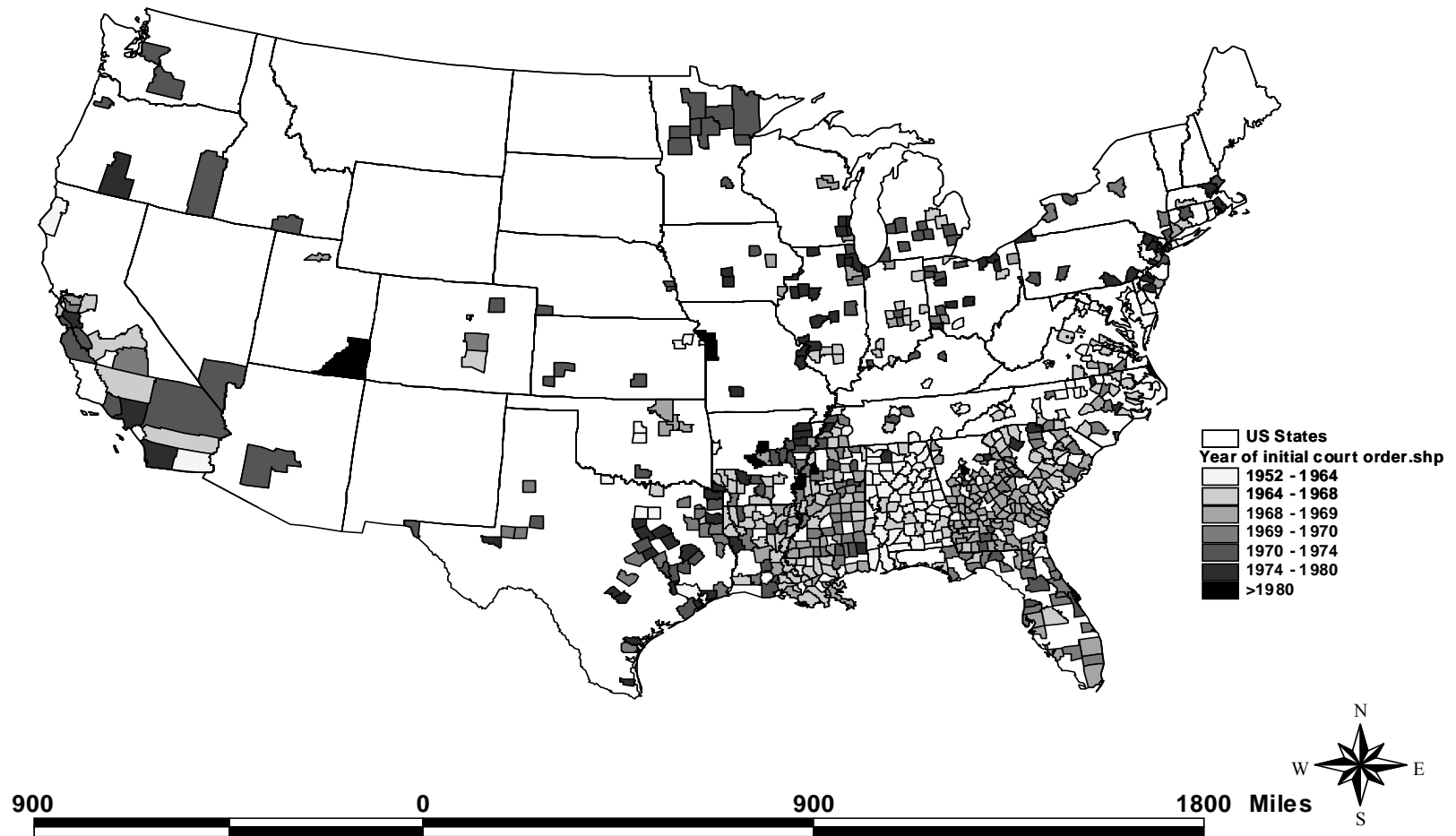


Figure 4a.

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

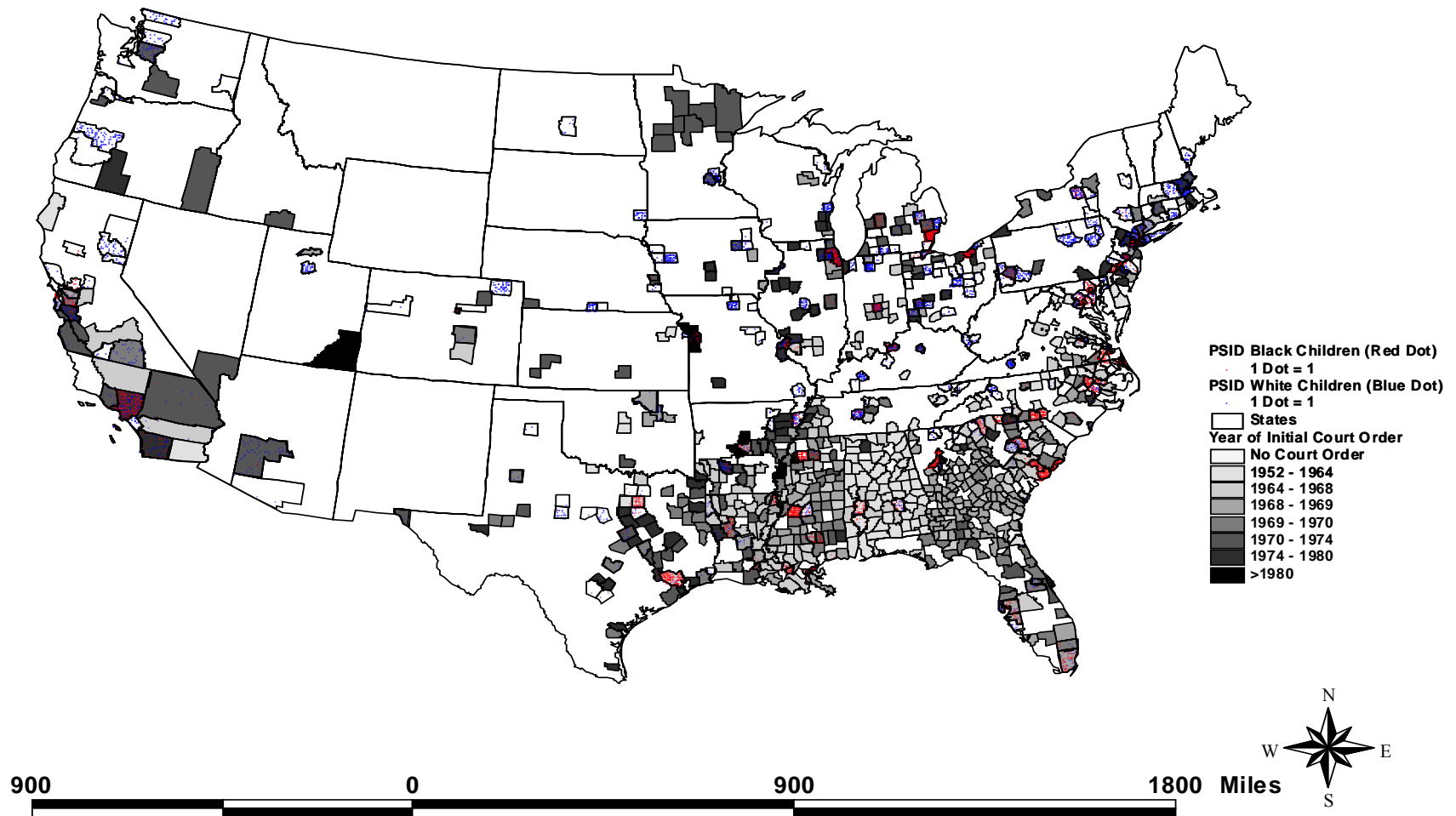


Figure 5.

The Geographic Timing of Implementation of Court-Ordered School Desegregation Plans in Large Districts

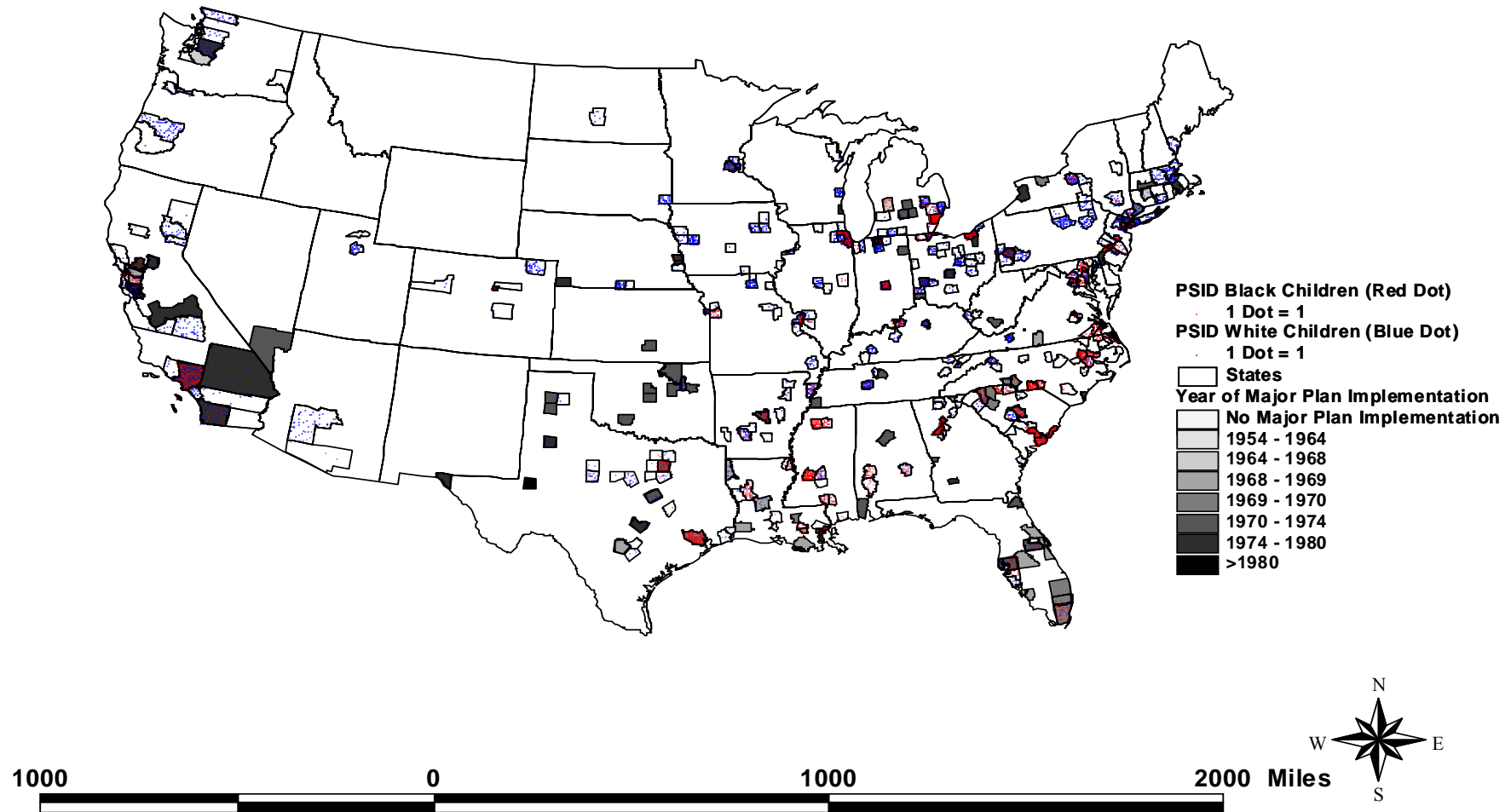


Table 1: Determinants of the Timing of Court-Ordered School Desegregation Using 1962 County Characteristics

	Dependent variable:						Delay b/w Initial Court Order & Major Desegregation Plan Implementation (years)	
	Initial Year of Court Order							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1962 County variables:								
Log population	-0.8040*** (0.2768)	-0.8541*** (0.2847)	-0.1439 (0.8200)	0.4198 (0.8907)	-1.3639 (1.0195)	-1.9489* (1.0794)	1.1884 (0.9768)	1.3207 (1.1221)
Percent minority, spline (< 20)	0.0877* (0.0449)	0.0858* (0.0450)	-0.1660 (0.1486)	-0.1629 (0.1489)	-0.1791 (0.2081)	-0.0635 (0.2123)	0.2001 (0.1943)	0.1527 (0.2085)
Percent minority, spline (≥ 20)	-0.0159 (0.0253)	-0.0182 (0.0252)	-0.0322 (0.1125)	0.0026 (0.1136)	-0.1762 (0.2520)	-0.1913 (0.2547)	0.5389** (0.2359)	0.5381** (0.2568)
Per-capita school spending (\$000s)	0.0082 (0.0162)		0.5960 (1.3015)		-2.3282 (2.1433)		5.4804** (2.2330)	
% of school spending revenue from state/fed govt	-0.0899*** (0.0186)	-0.0940*** (0.0191)	-0.1298** (0.0655)	-0.1043 (0.0666)	-0.0833 (0.0879)	-0.0805 (0.0877)	0.0684 (0.0825)	0.0758 (0.0877)
Student-to-teacher ratio		-0.0039 (0.0311)		-0.2896 (0.1787)		0.1965 (0.1867)		-0.3806 (0.2894)
Average teacher salary		0.0005 (0.0006)		-0.0020 (0.0015)		0.0021 (0.0019)		0.0014 (0.0019)
Median income	-0.0002 (0.0015)	-0.0002 (0.0014)	-0.0034 (0.0043)	-0.0033 (0.0044)	0.0086 (0.0067)	0.0062 (0.0069)	-0.0207*** (0.0065)	-0.0210*** (0.0070)
% of households with income <\$3,000	0.0713 (0.1005)	0.0761 (0.0996)	0.1065 (0.3589)	0.1170 (0.3594)	0.8007 (0.6187)	0.4575 (0.6321)	-2.5174*** (0.5757)	-2.4205*** (0.6244)
% of households with income > \$10,000	0.1178 (0.1377)	0.1065 (0.1380)	-0.0208 (0.3786)	0.0416 (0.3807)	-0.0672 (0.7080)	-0.0378 (0.7071)	0.8514+ (0.6280)	0.9291 (0.6656)
% of adults with 12 or more years of education	0.0877** (0.0393)	0.0903** (0.0396)	0.2574** (0.1070)	0.1992* (0.1116)	-0.2369 (0.1660)	-0.1699 (0.1732)	-0.0071 (0.1606)	0.0009 (0.1788)
1950-60 population change	0.0050 (0.0088)	0.0051 (0.0088)	-0.0232 (0.0177)	-0.0191 (0.0175)	-0.0016 (0.0216)	-0.0041 (0.0215)	-0.0184 (0.0220)	-0.0159 (0.0232)
% of residents in urban areas	0.0060 (0.0137)	0.0058 (0.0137)	-0.0437 (0.0595)	-0.0402 (0.0591)	0.0339 (0.1150)	0.0282 (0.1145)	-0.0199 (0.1147)	-0.0150 (0.1214)
% of residents in rural or farm area	0.0352 (0.0248)	0.0361 (0.0256)	0.1822 (0.1279)	0.1970 (0.1281)	0.2554 (0.4184)	0.3849 (0.4209)	0.5533 (0.4473)	0.4997 (0.4840)
% living in group quarters	0.0617 (0.0534)	0.0568 (0.0586)	0.1397 (0.2185)	0.1957 (0.2196)	0.3980 (0.2847)	0.3673 (0.2860)	-0.1526 (0.2866)	-0.2322 (0.3074)
Median age	-0.4279** (0.1754)	-0.4281** (0.1747)	-1.3912*** (0.5256)	-1.4594*** (0.5283)	-0.4847 (1.0443)	-0.2984 (1.0532)	-0.3123 (1.0220)	-0.1917 (1.0951)
% of residents who are school-age (5-20)	-0.2907 (0.1894)	-0.2933 (0.1911)	-2.2507*** (0.6443)	-2.4145*** (0.6489)	-0.9571 (1.1669)	-0.5218 (1.2006)	0.1894 (1.1408)	0.1512 (1.2355)
% of residents who are elderly (65+)	0.2258 (0.2039)	0.2209 (0.2046)	0.1049 (0.6581)	-0.0283 (0.6616)	0.7359 (0.8173)	0.6766 (0.8171)	0.0935 (0.8227)	0.0097 (0.8788)
% who voted for incumbent President	0.0615 (0.0444)	0.0508 (0.0468)	0.2834** (0.1237)	0.3241** (0.1252)	0.0059 (0.1801)	-0.0241 (0.1830)	0.0204 (0.1636)	0.0579 (0.1818)
Mortality rate (annual deaths per 10,000 residents)	-0.6088 (1.8752)	-0.6125 (1.8842)	-16.0529* (9.0305)	-13.7160 (9.0891)	-14.4197 (14.2740)	-11.1113 (14.1562)	5.1065 (14.5443)	2.7650 (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

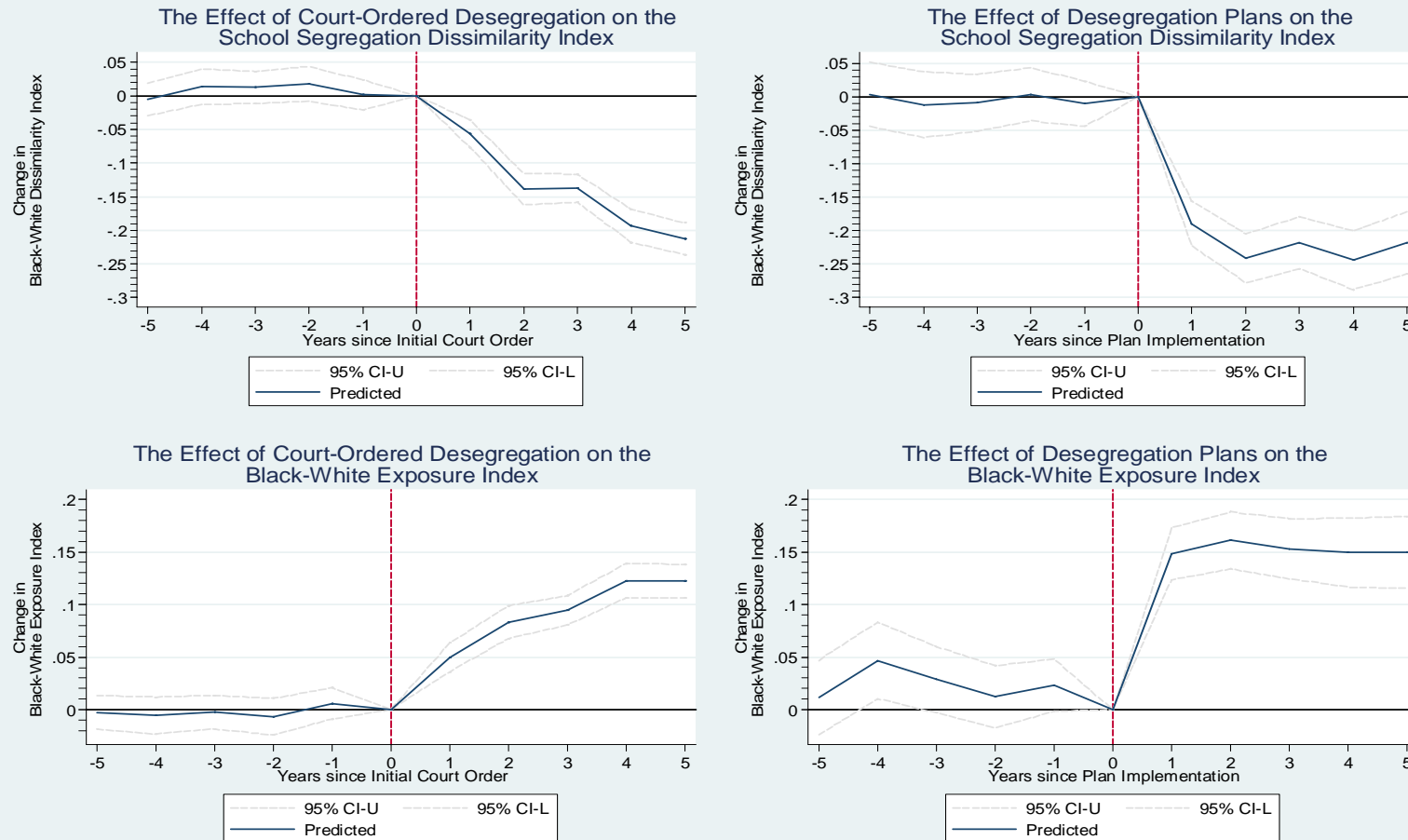
Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10

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.

Figure 6.

School Desegregation Effects on Racial School Segregation



School Data: Office of Civil Rights, 1968-82. Includes all districts under court order sometime b/w 1954-80 (N=655, American Communities Project data; N=99, Welch/Light data). Results based on regression w/school district FE, region*year FE, and controls for changes in gov't transfer programs. Avg black-white dissimilarity (exposure) index in '68 among districts that had not yet implemented a plan was 0.83 (0.16).

Figure 7.
Geographic Variation in School Spending in the U.S. in 1962

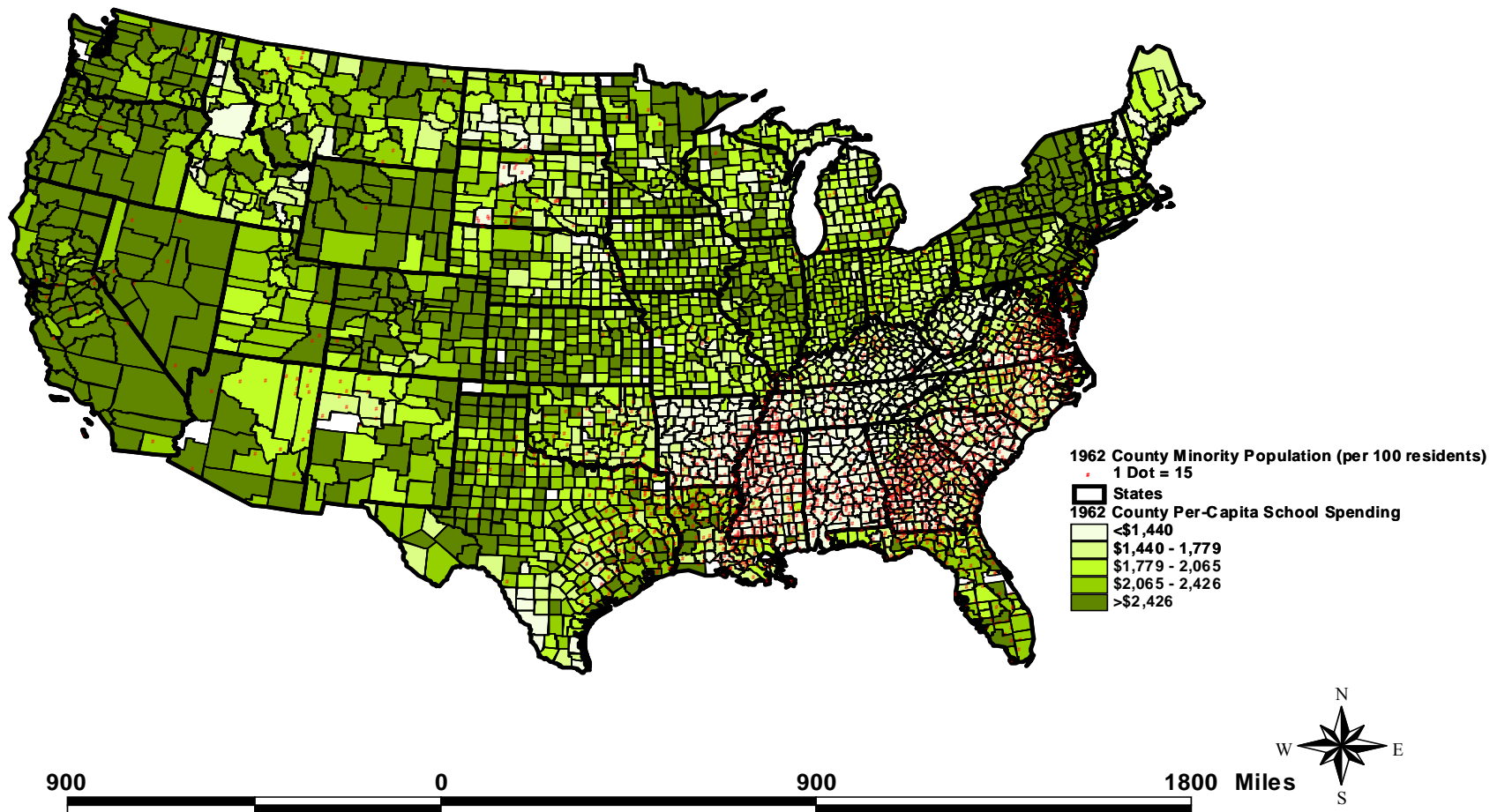
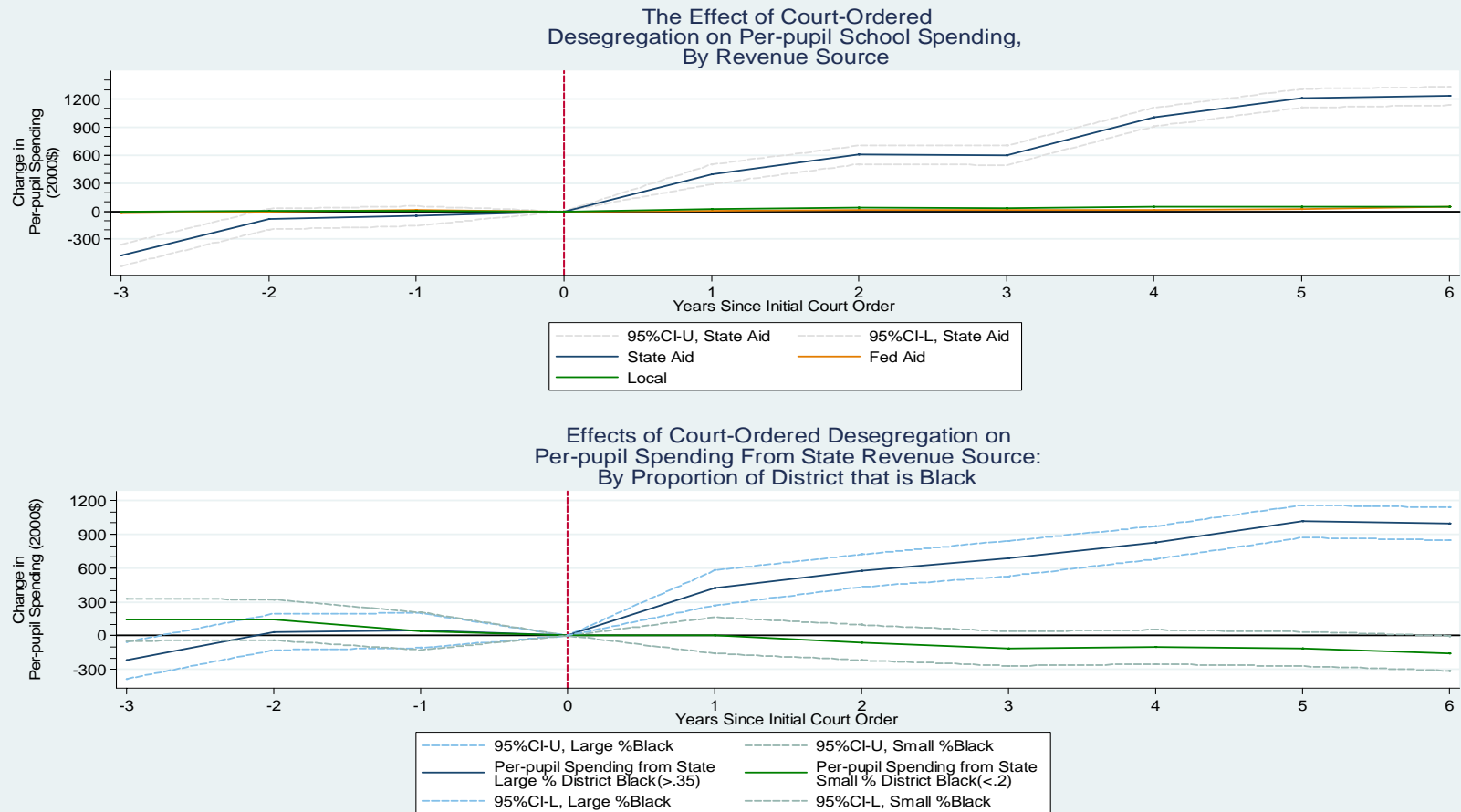


Figure 8.

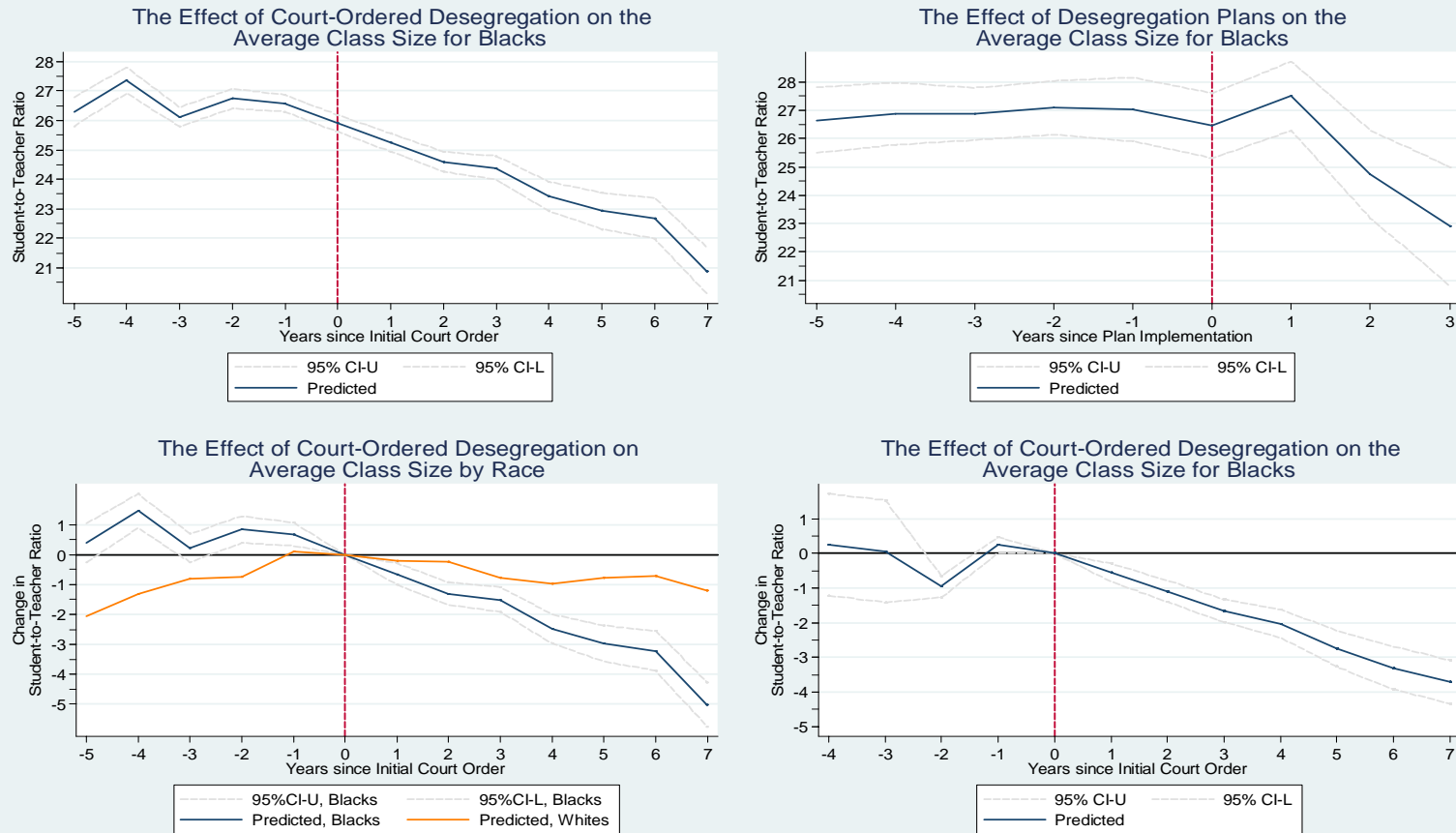
Effects of Court-Ordered Desegregation on Per-Pupil School Spending



School District Data: Census of Governments, 1962-92. Balanced panel of all school districts under court order sometime b/w 1954-80, for which there is at least one measure before and after court order (N=564). Results based on regression model with school district FE, year FE, & controls for changes in gov't transfer programs; results robust to inclusion of region-specific year FE. Avg per-pupil school spending in '67 among districts that had not yet implemented a plan was \$2,738 (2000\$).

Figure 9.

The Effect of School Desegregation on Average Class Size by Race



School Data: Census of Governments & Office of Civil Rights, 1962-77. Includes all districts under court order sometime b/w 1954-80 (N=667, American Communities Project data; N=99, Welch/Light data). Results based on regression w/school district FE, region-specific linear time trends, and controls for changes in gov't transfer programs. Models are weighted by baseline black student enrollment so that results can be interpreted as desegregation effect experienced by the average black child. Similarly, result in lower left graph for whites is weighted by baseline white student enrollment so results can be interpreted as desegregation effect experienced by the average white child; no significant effects are found for whites. The lower right graph uses school-level data for subset of years in which this information is available and models are weighted by black student enrollment at the school-level (14,869 schools from 667 districts from 33 different states; standard errors clustered at school-level); the three other graphs use all years of data aggregated up to the school district level.

Figure 10.

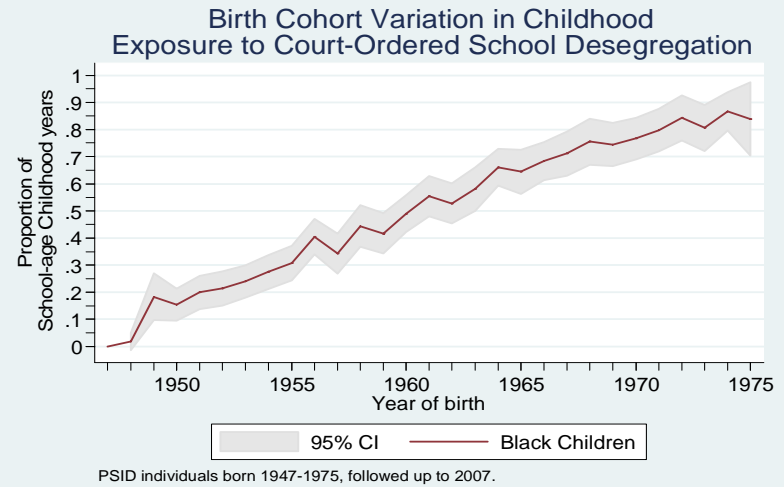
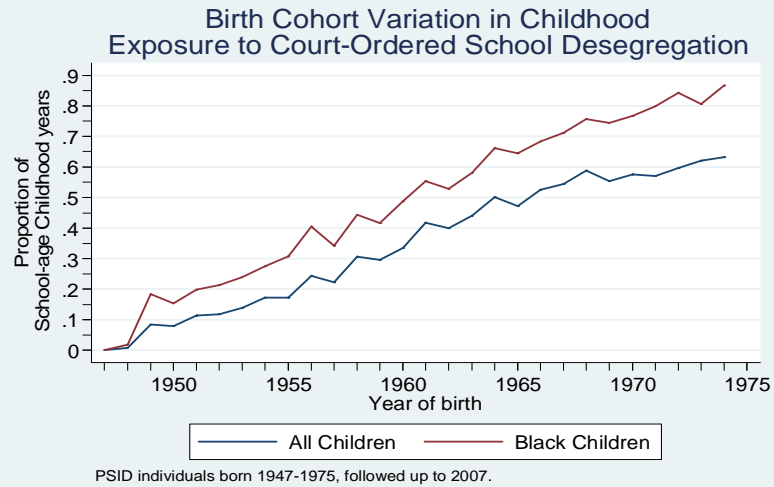


Table 2. Effects of Court-Ordered School Desegregation on Educational Attainment, by Race

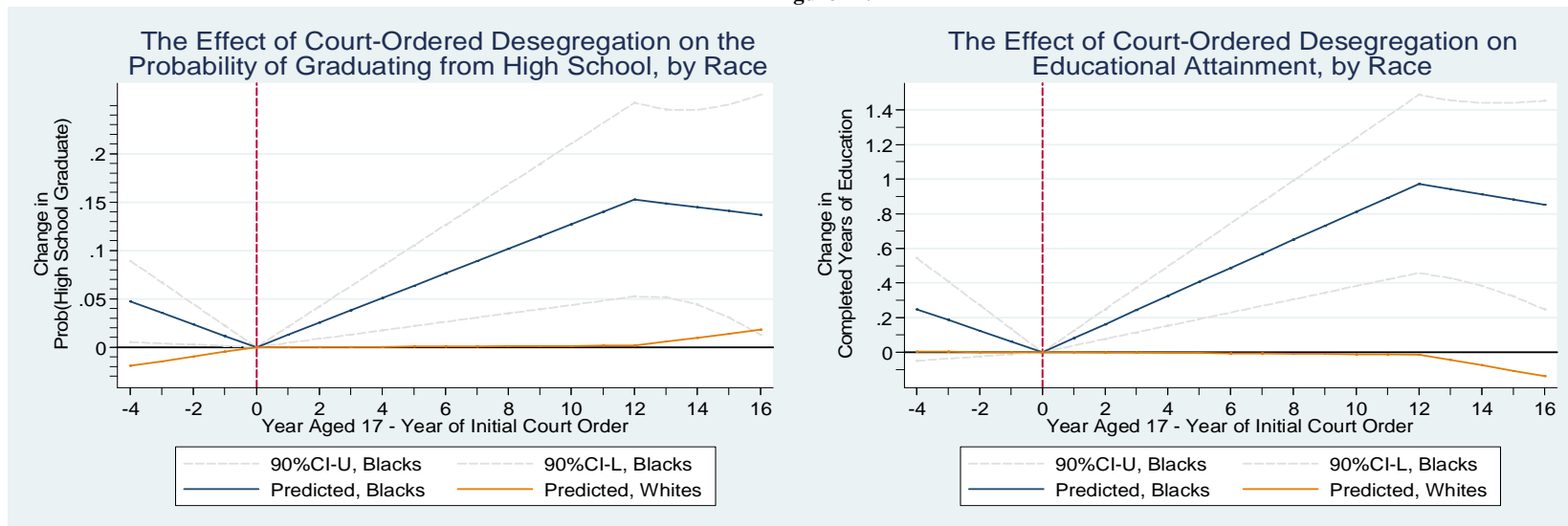
	Dependent variable:						
	Probability(Graduating from High School)				Years of Education		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Exposure to Court-Ordered Desegregation</i>							
(Main Effects apply to non-Hispanic Blacks for post-'64 court orders)							
(Year aged 17 - Year of Initial Court Order), spline:							
<0 (no exposure, linear trend prior to court order)	-0.0115*	0.0011	-0.0040	-0.0026	-0.0371	-0.0618	-0.0351
	(0.0064)	(0.0071)	(0.0087)	(0.0092)	(0.0286)	(0.0452)	(0.0483)
(0 to 12)	0.0137**	0.0141**	0.0114*	0.0104+	0.0800***	0.0811***	0.0788***
	(0.0056)	(0.0063)	(0.0068)	(0.0067)	(0.0282)	(0.0261)	(0.0272)
(0 to 12)*#of yrs before '65 for pre-'65 court orders	-0.0022**	-0.0024**	-0.0033	0.0024	-0.0061+	-0.0066	0.0251
	(0.0009)	(0.0010)	(0.0034)	(0.0043)	(0.0043)	(0.0139)	(0.0265)
>12 (beyond school-age years of exposure)	-0.0024	0.0014	0.0014	-0.0006	-0.0428	-0.0306	-0.1206*
	(0.0155)	(0.0154)	(0.0151)	(0.0166)	(0.0705)	(0.0570)	(0.0690)
<0*White	0.0161**	-0.0008	0.0110	0.0103	0.0001	0.0612	0.0512
	(0.0073)	(0.0085)	(0.0091)	(0.0097)	(0.0451)	(0.0515)	(0.0507)
(0 to 12)*White	-0.0122**	-0.0102+	-0.0106+	-0.0102+	-0.0449	-0.0823***	-0.0924***
	(0.0059)	(0.0076)	(0.0068)	(0.0067)	(0.0426)	(0.0293)	(0.0301)
(0 to 12)*(#of yrs before '65 for pre-'65 court orders)*White	0.0012	0.0011	0.0010	0.0008	-0.0032	0.0020	0.0069
	(0.0014)	(0.0016)	(0.0019)	(0.0023)	(0.0084)	(0.0081)	(0.0073)
>12*White	0.0050	0.0063	-0.0014	-0.0050	0.0415	-0.0007	0.0362
	(0.0191)	(0.0196)	(0.0186)	(0.0196)	(0.1040)	(0.0832)	(0.0885)
<i>Total Effect for Whites, spline:</i>							
<0	0.0046	0.0003	0.0070	0.0077	-0.0370	-0.0006	0.0161
(0 to 12)	0.0015	0.0039	0.0008	0.0002	0.0351	-0.0012	-0.0136
(0 to 12)*#of yrs before '65 for pre-'65 court orders	-0.0010	-0.0013	-0.0023	0.0032	-0.0093	-0.0046	0.0320
>12	0.0026	0.0077	0.0000	-0.0056	-0.0013	-0.0313	-0.0844
Full sample w/ever court order indicator*year of birth FE?	yes	--	yes	yes	--	yes	yes
Subsample who grew up in districts ever under court order?	no	yes	no	no	yes	no	no
Region of birth & Race-specific year of birth fixed effects?	yes	yes	yes	yes	yes	yes	yes
Race-specific region of birth fixed effects?	yes	no	yes	yes	no	yes	yes
Childhood county fixed effects?	no	yes	yes	--	no	yes	--
Childhood school district fixed effects?	no	no	no	yes	no	no	yes
Controls for Δchild county per-capita govt transfer programs?	no	no	no	no	no	no	yes
Number of individuals	5,436	2,958	5,436	5,436	3,582	6,307	6,307
Number of childhood families	2,068	1,083	2,068	2,068	1,182	2,216	2,216
Number of childhood neighborhoods	1,477	824	1,477	1,477	891	1,562	1,562
Number of school districts	332	142	332	332	143	337	337

Robust standard errors in parentheses (clustered at school district level; clustered at neighborhood level if school district FE are included)

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

Sample includes original sample PSID children born between 1951-70. All models control for gender, age at most recent survey interview, and the following set of child family/neighborhood background factors: parental income, parental education, mother's marital status at birth, birth weight, parental smoking and alcohol use, neighborhood poverty rate, and neighborhood and housing quality indices. The interaction terms of pre-'65 court orders with the other spline segments are suppressed to conserve space. PSID sample weights are used in all specifications to produce nationally-representative estimates.

Figure 11.



Based on regression that includes childhood county fixed effects, race-specific year of birth fixed effects, race-specific region of birth fixed effects, controls for gender, & child family/neighborhood. Effects shown represent post-64 court-orders. The point estimates for blacks remain significant and of roughly the same magnitude for subsample of individuals who grew up in districts that were subject to court orders at some point b/w '54-90 & with controls for changes in gov't transfer programs; no significant effects on whites (Table 2).

Figure 12. Multinomial Model of Effects of School Desegregation on Educational Attainment, by Race

Multinomial model categories: HS Dropout (0=reference); (1)HS Grad, no college; (2)Attend College, no 4-yr degree; (3) 4-yr College Grad.

Model includes controls for gender, childhood family/neighborhood factors, race-specific region of birth and year of birth fixed effects.

Results show avg effects of 5-year exposure to court-ordered school desegregation (i.e., standard deviation change), due to post-'64 orders.

Lines connecting the category #s denote insignificance ($p < .10$). Statistically significant effects on blacks; no significant effects on whites.

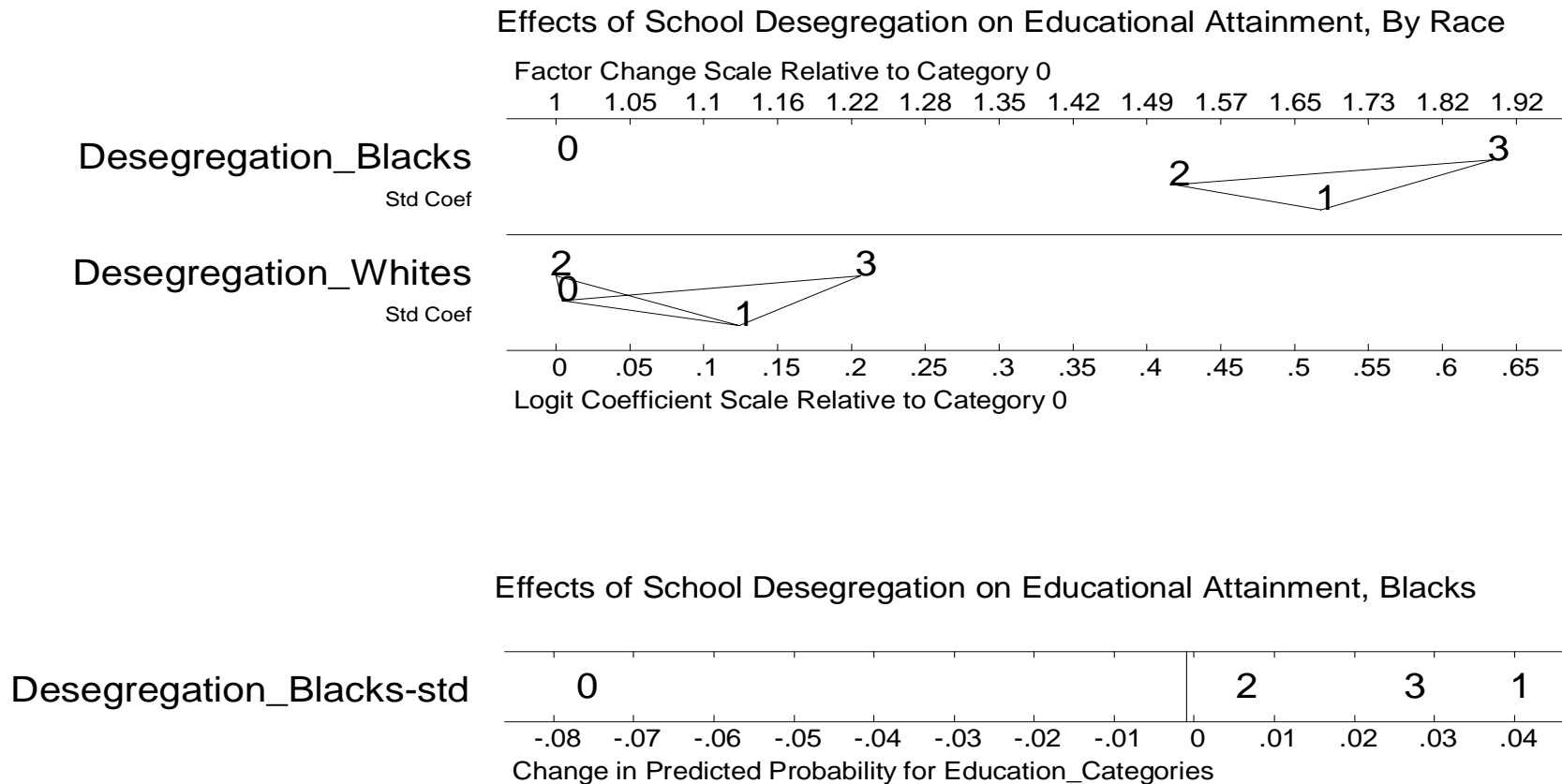


Table 3. 2SLS/IV Estimates of Effects of Desegregation Plans on Educational Attainment, by Race

Dependent variable:					
	Yrs of Exposure to Major Desegregation Plan _(age 5-17)	Probability(Graduate from High School)		Years of Education	
	First-Stage	Second Stage			
	(1)	(2)	(3)	(4)	(5)
		Blacks	Whites	Blacks	Whites
Years of Exposure to Major Desegregation Plan _(age 5-17)		0.0292*** (0.0092)	-0.0122 (0.0100)	0.0800*** (0.0214)	-0.0402 (0.0473)
(Initial year of court order - 1965), spline: ≤0	0.1155 (0.3136)				
Post-64 court order	4.8995*** (1.5397)				
>0	-0.0927** (0.0366)				
Individual > age 17 in year of initial court order	0.5708 (0.4063)				
(Age in year of initial court order - 17)* not beyond school-age in litigation yr	-0.6403*** (0.0749)				
Number of Individuals	2,154	1,057	572	1,378	633
Number of Childhood Families	690	362	241	394	254
Number of Childhood Neighborhoods	556	291	205	314	213
Number of School Districts	68	42	54	46	55
R-squared	0.8189				

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

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

Sample includes original sample PSID children born between 1951-70 who grew up in large school districts that implemented major desegregation plans sometime during the 1960s or 70s (Welch/Light desegregation data). Models include race-specific controls for year of birth fixed effects, gender, age at most recent survey interview, and childhood family/neighborhood factors.

Table 4. Effects of Court-Ordered School Desegregation on Adult Economic Outcomes, by Race

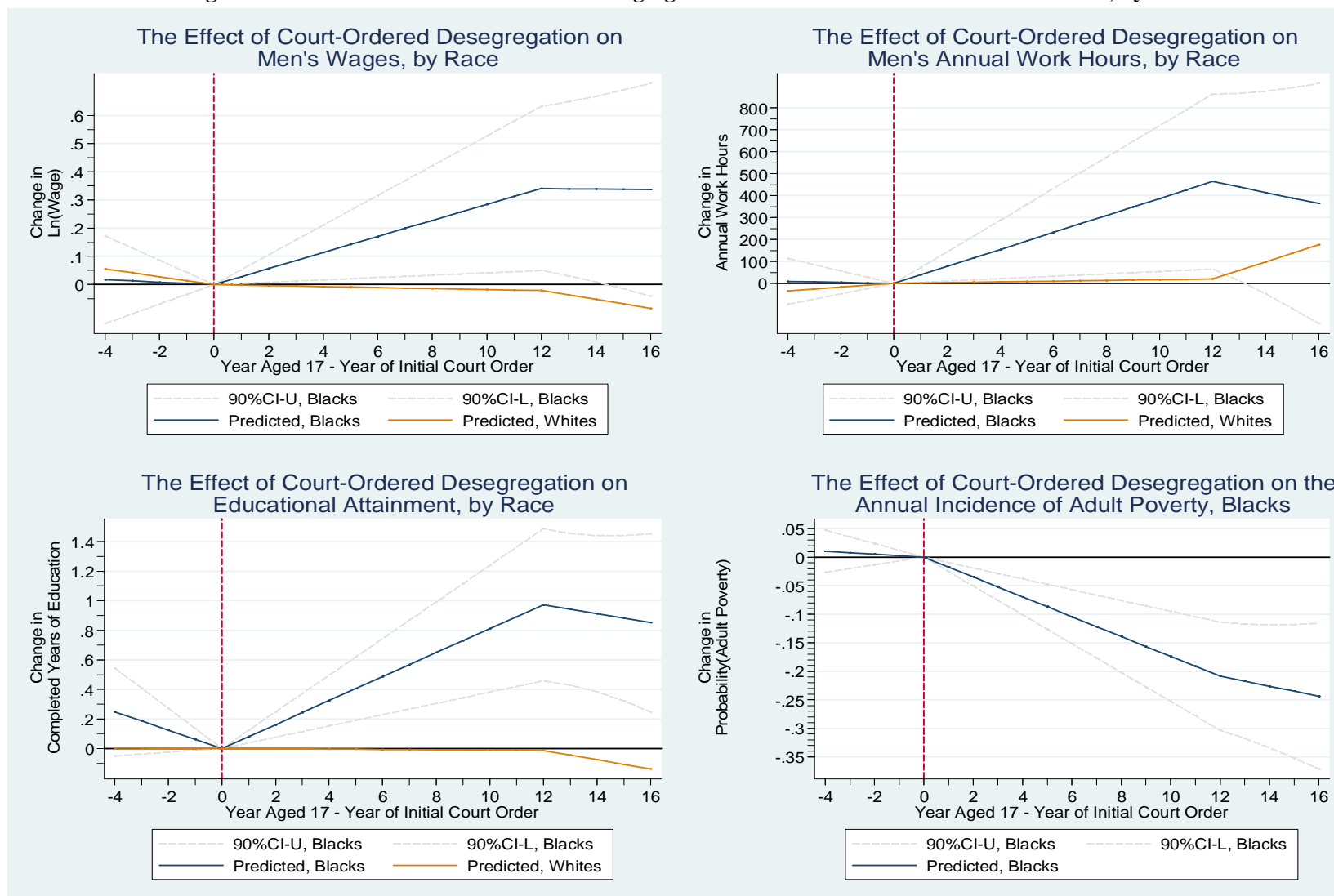
	Dependent variable:								
	Ln(Annual Earnings), Men ages 30-45			Ln(Wage), Men 30-45	Men's Annual Work Hours	Adult Family Income-to- Needs Ratio	Probability(Adult Poverty)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Exposure to Court-Ordered Desegregation</i> (Main Effects apply to non-Hispanic Blacks) (Year aged 17 - Year of Initial Court Order), spline:									
<0 (no exposure, linear trend prior to court order)	0.0121 (0.0352)	0.0025 (0.0351)	0.0261 (0.0349)	-0.0043 (0.0237)	-2.3299 (15.9044)	0.0326 (0.0604)	-0.0055 (0.0055)	-0.0026 (0.0056)	-0.0013 (0.0056)
(0 to 12)	0.0575* (0.0316)	0.0598** (0.0260)	0.0509* (0.0267)	0.0285* (0.0148)	38.6930* (20.2173)	0.0953* (0.0561)	-0.0187*** (0.0047)	-0.0174*** (0.0048)	-0.0156*** (0.0047)
(0 to 12)*#of yrs before '65 for pre-'65 court orders	--	--	--	--	-17.0799*** (6.1410)	-0.0204* (0.0124)	0.0046** (0.0019)	0.0044** (0.0019)	0.0041** (0.0019)
>12 (beyond school-age years of exposure)	-0.0241 (0.0455)	-0.0270 (0.0414)	-0.0263 (0.0404)	-0.0012 (0.0220)	-25.0001 (38.6976)	0.0146 (0.0838)	-0.0112 (0.0102)	-0.0088 (0.0097)	-0.0101 (0.0093)
<0*White	-0.0053 (0.0295)	0.0100 (0.0270)	0.0063 (0.0283)	-0.0097 (0.0278)	10.6585 (12.6100)	0.0146 (0.0431)	-0.0038 (0.0048)	-0.0072 (0.0054)	-0.0069 (0.0054)
(0 to 12)*White	-0.0509** (0.0227)	-0.0454* (0.0273)	-0.0395+ (0.0290)	-0.0302* (0.0175)	-37.0326** (17.9770)	-0.0975** (0.0423)	0.0100** (0.0040)	0.0087* (0.0047)	0.0073+ (0.0047)
(0 to 12)*(#of yrs before '65 for pre-'65 court orders)*White	--	--	--	--	0.9360 (2.8892)	0.0166*** (0.0064)	-0.0015** (0.0007)	-0.0013* (0.0007)	-0.0011+ (0.0007)
>12*White	0.0790 (0.0619)	0.0873 (0.0616)	0.0579 (0.0618)	-0.0144 (0.0267)	64.2180 (41.4063)	-0.0117 (0.1030)	0.0014 (0.0101)	-0.0003 (0.0101)	0.0003 (0.0096)
<i>Total Effect for Whites, spline:</i>									
<0	0.0068	0.0125	0.0324	-0.0140	8.3286	0.0472	-0.0093	-0.0098	-0.0082
(0 to 12)	0.0066	0.0144	0.0114	-0.0017	1.6604	-0.0022	-0.0087	-0.0087	-0.0083
(0 to 12)*#of yrs before '65 for pre-'65 court orders	--	--	--	--	-16.1439	-0.0038	0.0031	0.0031	0.0030
>12	0.0549	0.0603	0.0316	-0.0156	39.2179	0.0029	-0.0098	-0.0091	-0.0098
Child county fixed effects & Race-specific year of birth FE?	yes	yes	yes	yes	yes	yes	yes	yes	yes
Race-specific region of birth fixed effects?	no	yes	yes	no	no	no	no	yes	yes
Controls for Δchild county per-capita govt transfer programs?	no	no	yes	no	no	no	no	no	yes
Number of person-year adult observations	6,808	6,808	6,808	6,808	16,002	64,863	64,863	64,863	64,863
Number of individuals	1,055	1,055	1,055	1,055	1,592	4,423	4,423	4,423	4,423
Number of childhood families	641	641	641	641	846	1,366	1,366	1,366	1,366
Number of childhood neighborhoods	516	516	516	516	663	1,013	1,013	1,013	1,013
Number of school districts	116	116	116	116	127	147	147	147	147

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

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

Sample includes PSID original sample children born b/w 1951-70 who grew up in school districts that were subject to court orders at some point b/w 1954-90. All models control for the following set of child family/neighborhood background factors: parental income, parental education, mother's marital status at birth, birth weight, parental smoking and alcohol use, neighborhood poverty rate, and neighborhood and housing quality indices, and columns (6)-(9) control for gender. Models include flexible controls for age (quadratic) and analyze adult economic outcomes for ages ≤45 to avoid conflating birth cohort and life cycle effects. Columns (1)-(4) simplify exposure specification by not including pre-'65 court order interaction terms because of smaller male-only sample for labor market outcomes; similar patterns of results when interactions are included. The interaction terms of pre-'65 court orders with the other spline segments (columns (5)-(9)) are suppressed to conserve space. PSID sample weights are used in all specifications to produce nationally-representative estimates.

Figure 13. The Effects of Court-Ordered Desegregation on Education & Adult Economic Status, by Race



Sample includes PSID original sample children born b/w 1951-70 who grew up in school districts that were subject to court orders at some point b/w 1954-90. Results based on regressions that include childhood county fixed effects, race-specific year of birth fixed effects, controls for gender, & child family/neighborhood. Models include flexible controls for age (quadratic) and analyze adult economic outcomes for ages ≤ 45 to avoid conflating birth cohort and life cycle effects. Effects shown represent post-'64 court-orders. The point estimates for blacks remain significant and of roughly the same magnitude with race-specific region of birth fixed effects and controls for changes in gov't transfer programs. No significant effects on whites (Table 4).

Table 5. Interactive Effects of Court-Ordered School Desegregation & Induced-Change in Per-Pupil Spending on Black's Educational & Adult Economic Attainments

Dependent variable:

	Years of Education			Adult Family Income-to-Needs Ratio	Probability (Adult Poverty)
<i>Exposure to Court-Ordered Desegregation</i>	(1)	(2)	(3)	(4)	(5)
Yrs of Exposure to Court-Ordered Desegregation _(age 5-17)	0.2510*** (0.0587)	0.1949** (0.0812)	0.2596*** (0.0803)	0.0761** (0.0329)	-0.0173*** (0.0059)
Yrs of Exposure to Court-Ordered Desegregation* ↑ΔPer-Pupil Spending _(t-1,t+3)	0.0892*** (0.0311)		0.0764** (0.0358)	0.0515** (0.0215)	-0.0057+ (0.0038)
Yrs of Exposure to Court-Ordered Desegregation* ↑ΔBlack-White Exposure Index _(t-1,t+3)		0.0106 (0.0110)	0.0093 (0.0116)		
Sample from districts initially subject to court orders ≥ 1964?	yes	yes	yes	yes	yes
Year of birth fixed effects?	yes	yes	yes	yes	yes
Childhood county fixed effects?	yes	yes	yes	yes	yes
Number of adult person-year observations	--	--	--	23,770	23,770
Number of Individuals	1,193	915	915	1,493	1,493
Number of Childhood Families	342	270	270	402	402
Number of Childhood Neighborhoods	256	217	217	300	300
Number of School Districts	51	43	43	51	51

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

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

Sample restricted to PSID original sample black children who grew up in school districts that were initially subject to court order sometime after 1963 for which I have school district per-pupil spending (school segregation) information 1 year before and 3 years after initial court order, obtained from school district finance data (1962-82) and OCR school data (1968-82). The estimated district-specific induced-change in per-pupil spending (school segregation) are net of school district fixed effects and region-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 7&9). Models include same set of control variables as in Tables 2&4.

Table 6. 2SLS/IV Estimates of Effects of Desegregation Plans on Adult Economic Attainment Outcomes, by Race

Second stage, Dependent variable:										
Years of Exposure to Major Desegregation Plan _(age 5-17)	Ln(Annual Earnings), Men ages 25-45		Ln(Wage), Men 25-45		Men's Annual Work Hours		Adult Family Income-to- Needs Ratio		Probability(Adult Poverty)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Blacks	Whites	Blacks	Whites	Blacks	Whites	Blacks	Whites	Blacks	Whites
	0.0580**	0.0058	0.0219*	-0.0032	39.0372*	12.1726	0.0399**	-0.0677	-0.0220***	-0.0014
	(0.0234)	(0.0218)	(0.0128)	(0.0140)	(22.2243)	(20.6214)	(0.0200)	(0.0596)	(0.0075)	(0.0039)
Number of person-year observations	4,806	3,515	4,806	3,515	6,021	3,710	27,489	13,514	27,489	13,514
Number of Individuals	561	312	561	312	630	313	1,746	719	1,746	719
Number of Childhood Families	283	188	283	188	308	188	487	283	487	283
Number of Childhood Neighborhoods	232	166	232	166	250	166	381	233	381	233
Number of School Districts	37	51	37	51	39	51	47	55	47	55

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

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

Sample includes original sample PSID children born between 1951-70 who grew up in large school districts that implemented major desegregation plans sometime during the 1960s or 70s (Welch/Light desegregation data). Models include race-specific controls for year of birth fixed effects, gender, age, and childhood family/neighborhood factors. The first-stage results are displayed in Table 3.

Table 7. Effects of Court-Ordered School Desegregation on the Likelihood of Incarceration among Men, by Race

	Dependent variable:							
	Prob(Deviant Behavior)		Prob(Ever Incarcerated), by age 30		Probability(Incarceration), ages 20-34		Prob(Ever Incarcerated), by age 30	
	OLS Estimates				2SLS/IV Estimates			
	Blacks		Blacks	Whites	All Men	Blacks		
	(1)	(2)	(3)	(4)	(5)	(6)		
Years of Exposure to Desegregation Plan _(age 5-17) (main effect applies to non-Hispanic Blacks)					-0.0071+ (0.0051)			
Years of Exposure to Desegregation Plan*White					0.0097+ (0.0076)			
<i>Effect for Whites, Exposure Yrs to Desegregation Plan</i>					0.0026			
Age when Desegregation Plan 1st implemented:								
≥18, no exposure (reference category)								
High School (dummy 0 1, age 15-17)						-0.0362 (0.0580)		
Middle School (dummy 0 1, age 11-14)						-0.0487 (0.0956)		
Elementary School (dummy 0 1, age ≤10)						-0.0766** (0.0375)		
Age when Initial Court Order occurred:								
≥18, no exposure (reference category)								
High School (dummy 0 1, age 15-17)	-0.1528** (0.0754)	-0.0763** (0.0299)	-0.0071 (0.0074)	0.0099 (0.0066)				
Middle School (dummy 0 1, age 11-14)	-0.1174 (0.0911)	-0.0599* (0.0352)	-0.0073 (0.0131)	0.0077 (0.0091)				
Elementary School (dummy 0 1, age ≤10)	-0.2254** (0.1010)	-0.1465*** (0.0410)	-0.0378* (0.0212)	-0.0003 (0.0103)				
Linear trend prior to court order, Age when 1 st court order)*no exposure	0.0015 (0.0074)	0.0012 (0.0051)	-0.0007 (0.0008)	-0.0033** (0.0017)				
Linear trend for pre-school years, ≤5 when initial court order occurred	-0.0106 (0.0182)	0.0145 (0.0161)	-0.0060 (0.0070)	0.0019 (0.0033)				
Number of person-year adult observations	--	--	11,292	7,362	12,562	--		
Number of individuals	452	904	830	464	1,335	624		
Number of childhood families	239	385	357	279	662	316		
Number of childhood neighborhoods	188	295	273	237	541	264		
Number of school districts	54	67	64	81	64	38		

Robust standard errors in parentheses (clustered at school district level); *** p<0.01, ** p<0.05, * p<0.10 (2-tailed test), + p<0.10 (one-tailed test)

All models include race-specific year of birth fixed effects, controls for age (quadratic), and childhood family/neighborhood factors. Sample for 2SLS/IV estimates include original sample PSID children born between 1951-70 who grew up in large school districts that implemented major desegregation plans sometime during the 1960s or 70s (Welch/Light desegregation data). The first-stage results are displayed in Table 3. Column (1) includes those with information from the 1995 survey IW crime module, who grew up in districts ever subject to court orders after 1964. Deviant behavior is defined as ever expelled/suspended from school, charged with a crime, or ever incarcerated (column(1)). Columns (3)-(5) are models of the annual incidence of incarceration.

Table 8. Effects of Court-Ordered School Desegregation on Adult Health Status, by Race

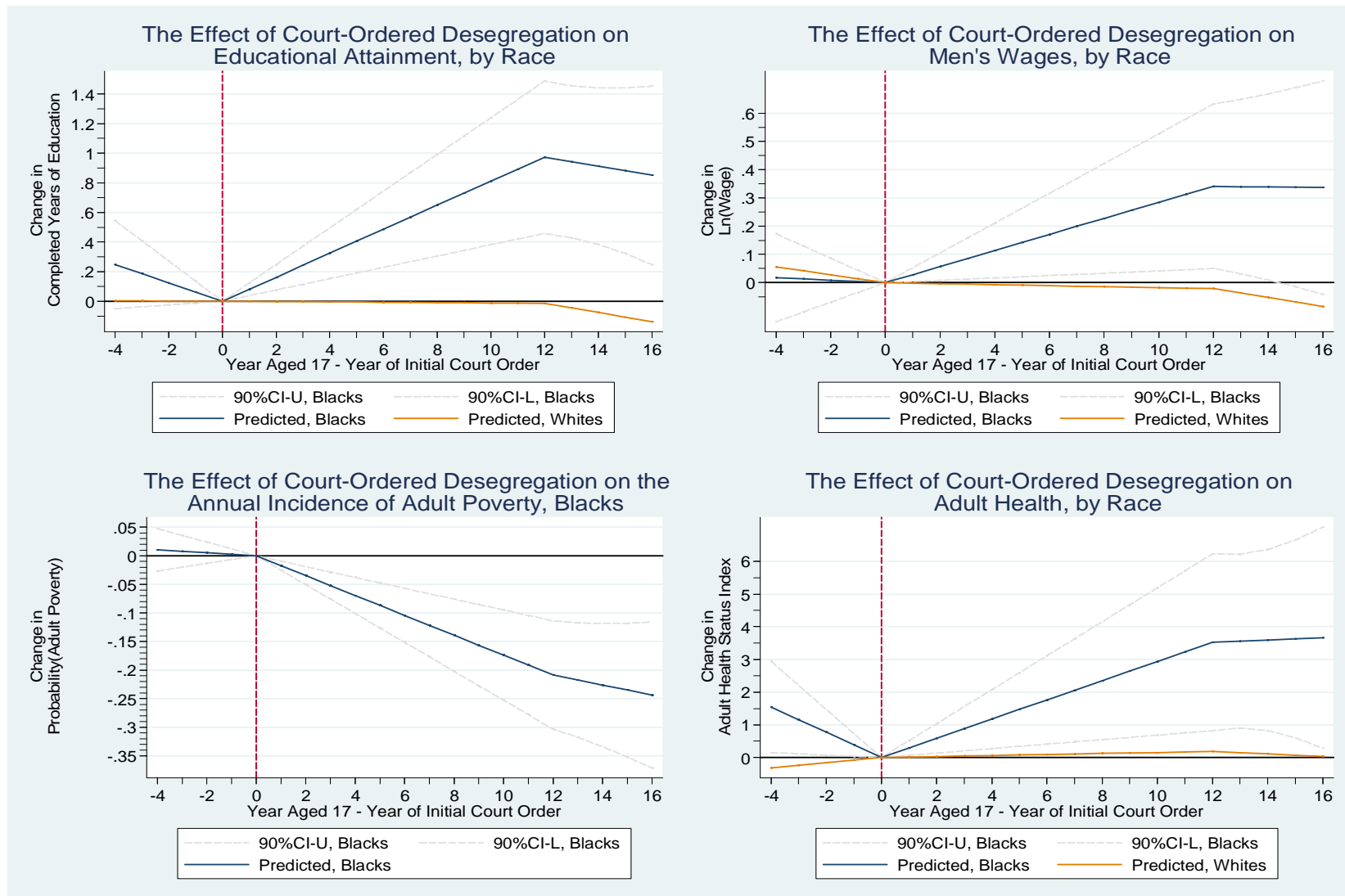
	Dependent variable:					
	Adult Health Status Index, ages 25-45 (Based on Interval Regression Model: 100pt-scale, 100=perfect health)					
	Interval Regression Estimates				2SLS/IV Estimates	
	All			Blacks	Blacks	Whites
	(1)	(2)	(3)	(4)	(5)	(6)
Years of Exposure to Major Desegregation Plan _(age 5-17)					0.5222*** (0.1944)	-0.1787 (0.2697)
Yrs of Exposure to Court-Ordered Desegregation _(age 5-17)				0.5322+ (0.3272)		
Yrs of Exposure to Court-Ordered Desegregation* ↑ΔPer-Pupil Spending _(t-1,t+3)				0.3763* (0.2034)		
(Main Effects apply to non-Hispanic Blacks) (Year aged 17 - Year of Initial Court Order), spline: <0 (no exposure, linear trend prior to court order)	-0.3867* (0.2120)	0.1022 (0.3813)	0.2039 (0.3975)			
(0 to 12)	0.2937** (0.1369)	0.5978* (0.3633)	0.5121+ (0.3775)			
(0 to 12)*#of yrs before '65 for pre-'65 court orders	0.0205 (0.0361)	-0.1676* (0.0968)	-0.1301+ (0.0990)			
>12 (beyond school-age years of exposure)	0.0348 (0.3949)	0.2032 (0.4643)	0.0734 (0.4821)			
<0*White	0.4663* (0.2428)	0.4567* (0.2604)	0.3933+ (0.2624)			
(0 to 12)*White	-0.2780* (0.1510)	-0.4734** (0.1911)	-0.4613** (0.1933)			
(0 to 12)*(#of yrs before '65 for pre-'65 court orders)*White	0.0112 (0.0433)	0.0073 (0.0351)	0.0040 (0.0354)			
>12*White	-0.0726 (0.4874)	-0.0640 (0.3613)	-0.0357 (0.3718)			
<i>Total Effect for Whites, spline:</i> <0	0.0796	0.5589	0.5972			
(0 to 12)	0.0157	0.1244	0.0508			
(0 to 12)*#of yrs before '65 for pre-'65 court orders	0.0317	-0.1603	-0.1261			
>12	-0.0378	0.1392	0.0377			
Race-specific year of birth and region of birth fixed effects?	yes	yes	yes	yes	yes	yes
Childhood county fixed effects?	no	yes	yes	yes	yes	yes
Controls for Δchild county per-capita govt transfer programs?	no	no	yes	no	no	no
Number of person-year adult observations	52,737	52,737	52,737	9,847	9,802	6,274
Number of individuals	5,494	5,494	5,494	991	1,104	655
Number of childhood families	2,069	2,069	2,069	293	366	267
Number of childhood neighborhoods	1,472	1,472	1,472	218	292	227
Number of school districts	330	330	330	50	42	55

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

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

All models include controls for age (quadratic), gender, childhood family/neighborhood factors, and an indicator dummy for districts ever subject to court orders b/w 1954-90 interacted with year of birth FE. Sample for 2SLS/IV estimates include those who grew up in large school districts that implemented major desegregation plans sometime during the 1960s or 70s (Welch/Light desegregation data). The first-stage results are displayed in Table 3. Column (4) is restricted to PSID original sample black children who grew up in school districts that were initially subject to court order sometime after 1963 for which I have school district per-pupil spending information 1 year before and 3 years after initial court order, obtained from school district finance data (1962-82). The estimated district-specific induced-change in per-pupil spending are net of school district fixed effects and region-specific time trends; centered around the average change (\$1,000 for per-pupil spending), so that the main effects capture the average desegregation impact (see also Figures 7&9).

Figure 14. The Effects of Court-Ordered Desegregation on Adult Socioeconomic & Health Attainments, by Race



Sample includes PSID original sample children born b/w 1951-70 who grew up in school districts that were subject to court orders at some point b/w 1954-90. Results based on regressions that include race-specific year of birth and region of birth fixed effects, controls for gender, & child family/neighborhood. Models include flexible controls for age (quadratic) and analyze adult outcomes for ages ≤ 45 to avoid conflating birth cohort and life cycle effects. Effects shown represent post-'64 court-orders. The point estimates for blacks remain significant and of roughly the same magnitude with childhood county fixed effects and controls for changes in gov't transfer programs. No significant effects on whites (Tables 2-6, 8).

Table 9.
Long-run Effects of School Desegregation & School Quality on Adult Health:
Sibling Fixed Effect Estimates

(Dependent variable: general health status in adulthood), (ages 20-57)		
Interval Regression Model: 100pt-scale, 100=perfect health		
	(1)	(2)
School Desegregation Plan Exposure _(age 5-17)	-1.6738 (1.7653)	
School Desegregation Plan Exposure _(age 5-17) *Black	3.6910* (2.2732)	
Ln(School district per-pupil spending) _(age 12-17)		3.1433** (1.5034)
Age - 30	-0.2631*** (0.0192)	-0.2561*** (0.0239)
Constant	88.0108*** (1.0713)	83.2183*** (2.6310)
Sibling Fixed Effect?	yes	yes
Person-year observations	61,373	42,455
Number of Individuals	6,075	4,280
Number of Families	1,756	1,262

Robust Standard errors in parentheses (clustered on child family)

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

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

**Table 10. Falsification Tests Using Unsuccessful Desegregation Court Litigation:
Placebo Effects on Adult Outcomes, by Race**

	Dependent variable:				
	Years of Education	Ln(Annual Earnings), Men ages 25-45	Adult Family Income-to-Needs Ratio	Probability (Adult Poverty)	Adult Health Status Index, ages 25-45
Years of Exposure to Unsuccessful Desegregation Court Litigation _(age 5-17)	0.0131 (0.0273)	-0.0035 (0.0114)	-0.0068 (0.0180)	0.0046 (0.0039)	0.0240 (0.1267)
Years of Exposure to Unsuccessful Desegregation Court Litigation*White	0.0107 (0.0408)	0.0051 (0.0126)	0.0315 (0.0335)	-0.0059 (0.0040)	-0.0086 (0.1472)
Number of person-year adult observations	--	28,858	72,191	72,191	52,737
Number of individuals	6,307	2,808	6,134	6,134	5,494
Number of childhood families	2,216	1,564	2,185	2,185	2,069
Number of childhood neighborhoods	1,562	1,181	1,546	1,546	1,472
Number of school districts	337	295	335	335	330

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

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

All models include race-specific year of birth fixed effects, and controls for region of birth, age (quadratic), gender, and childhood family/neighborhood factors. Sample includes original sample PSID children born between 1951-70 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). 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 2-8).

**Table 11. 2SLS/IV Estimates of Effects of School Quality on Adult Attainment Outcomes among Blacks:
Evidence from Court-Ordered School Desegregation,
Using Average Class Size during School-age years as a Marker of School Quality Inputs**

	Dependent variable:							
	Prob(High School Graduate)	Yrs of Education	Ln(Annual Earnings), Men ages 25-45	Family Income-to- Needs Ratio	Probability (Adult Poverty)	Probability (Incarceration), Men ages 20-34	Prob (Deviant Behavior), Males	Health Status, ages 25-45
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Avg Class Size _(age 5-17)	-0.0368*** (0.0140)	-0.0959* (0.0564)	-0.0745*** (0.0282)	-0.0576* (0.0335)	0.0417*** (0.0133)	0.0127* (0.0066)	0.0312+ (0.0236)	-2.2932** (0.9715)
Person-year observations	--	--	4,802	27,472	27,472	7,584	--	9,798
# of individuals	1,057	1,179	560	1,745	1,745	882	383	1,103
# of child families	362	372	283	487	487	403	224	366
# of child neighborhoods	291	295	232	381	381	322	187	292
# of school districts	42	43	37	47	47	45	32	42

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

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

The timing of initial school desegregation court-orders is used here as an instrument for the average class size experienced by blacks during their school-age years. The sample includes PSID original sample children who grew up in large school districts that implemented major desegregation plans sometime during the 1960s or 70s (Welch/Light desegregation data). The first-stage effects are similar to those presented in Figure 9. The results are intended to be interpreted broadly as capturing the composite effects of school quality changes experienced by blacks that were induced by school desegregation, which may include an amalgam of peer effects, school resource effects, and teacher quality effects; class size serves here as a marker for these school quality changes. The typical reduction in the average student-to-teacher ratio induced by desegregation for these black birth cohorts was about 3-4 students. The typical variation used to identify the effects in the model is smaller, since the student-to-teacher ratio is averaged across one's school-age years; a standard deviation change in the avg student-to-teacher ratio is 2.7. Models include year of birth fixed effects, and controls for region of birth, age (quadratic), gender, and childhood family/neighborhood factors. The magnitude and significance of the estimated effects on adult health status persist with the inclusion of school district fixed effects (se clustered at neighborhood level).

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 1,057 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 1968-1982. 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, 1962-1982.
- (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).

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.

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 1968-1976. 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 were 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.

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.

E. 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.

I am grateful to Doug Almond, Hilary Hoynes, and Diane Schazebach 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), medical care (Medicare, Medicaid, military), and retirement and disability benefits.

F. Matching PSID Individuals to their Childhood School Districts

In order to limit the possibility that school district boundaries were drawn in response to pressure for desegregation, I utilize 1970 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, 1980 and 1990 to school districts using this resulting digital map based on their centroid locations.

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 1970 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.

Algorithm for Matching Individuals to Schools

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.

The criterion for a match is outlined below. The earliest criteria the individual meets is how the merge is accomplished. If there is one high school in the individual’s census block/tract, then the individual is assigned the characteristics of that school. If there are multiple high schools in the individual’s census block/tract and all high schools are in the same district, then the individual is assigned the mean characteristics of the high schools in the block/tract. If there are multiple high schools in the individual’s census block/tract that do not belong to the same district, attempts were made to identify which high school is correct based on census place (city), and the individual is assigned the characteristics of this school; if this is not possible, the individual is assigned the mean characteristics of the high schools in the zip code. If there is one elementary or middle school in the individual’s census block/tract, and the school is a member of a district that contains at least one high school, then the individual is assigned the mean characteristics of the high school or high schools in the district associated with the elementary school. If there are multiple elementary or middle schools in the individual’s block/tract, and these schools belong to different school districts, attempts were made to identify which school district is correct based on census place (city), and the individual is assigned the mean characteristics of the high schools in the district associated with the school. If this is not possible, the individual is assigned the mean of the high schools associated with the school districts. The individual is matched to the mean of the school districts in the childhood county of residence.

Appendix B: PSID Data & Measures

PSID sample

The selected sample consists of PSID sample members born between 1950 and 1975; these individuals were between 0 and 18 years old in one of the first six waves of interviewing and have been followed into adulthood. I obtain all available information on them for each wave, 1968 to 2007. In 2007, the oldest respondent is 57 and the youngest is 37.

The first wave of PSID interviewing in 1968 included 2,856 families containing 8,710 children 0-18 years old. 167 of these children died by 2007. These individuals are included in the analyses for the years they are observed alive. Any selective attrition with respect to mortality is likely to lead to an understatement of the impact of adverse childhood conditions, if those who suffer premature death disproportionately grow up in the more disadvantaged childhood family and neighborhood environments. I estimated mortality models, but there were too few deaths to precisely estimate any relationships. Of these 8,710 children, 5,628 had at least one valid report of health status in adulthood. Adult GHS is based on reports for PSID heads and wives/"wives" (1984-2007) as well as all family members in 1986. A small minority of respondents lacked valid addresses and were not able to be matched to neighborhoods in the geocode file—these cases were disproportionately located in rural areas.

The main sample (born between 1951 and 1970) contains 7,212 individuals from 2,383 childhood families, 1,658 childhood neighborhoods, 349 school districts, representing 40 different states. Data are combined across all waves for each person, and in total there are 130,402 person-year observations, or an average of 18 observations per person, for the analyses of adult income.

Studies have concluded that the PSID sample of heads and wives remains representative of the national sample of adults (Fitzgerald, Gottschalk, and Moffitt, 1998a; Beckett 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.

Child Family & Neighborhood Measures

I utilize a broad array of available measures in the PSID of family and neighborhood background. In addition to detailed measures of family economic resources and socioeconomic status during childhood, additional factors include residential segregation, parental and neighborhood-level measures of expectations of child achievement, child health insurance coverage, birth weight, unintended fertility timing preferences (unintended pregnancy), parental health behaviors (alcohol and smoking), parental connectedness to informal sources of support, and parental self-reports of neighborhood and housing conditions. The self-reports of housing/neighborhood conditions include: whether live in Public Subsidized Housing; poor neighborhood for children, whether there exist plumbing problems, housing structural problems, security problems, cockroach or rat problems, insulation problems, neighborhood

cleanliness problems, overcrowding, noise, or traffic problems, burglary, robbery, assault, drug use, or problems related to having too few police. This survey information is used along with 1970-2000 census tract based measures—particularly, neighborhood poverty rate. The effects of childhood neighborhood factors are presented in detail in Johnson (2009).

I control for parental education, parental health status, birth order, whether born into a two-parent family, year of birth, and region of birth. I also make use of a unique set of measures of parental aspirations/motivation and long-term planning, parental personality, habits and skills that were collected in the early years of the PSID. Because of the detailed measures of childhood family and neighborhood characteristics included in the model of adult health status, I am able to minimize the problem of omitted variables bias of estimated childhood school quality effects that has been suggested for prior studies that have examined labor market outcomes.

Table A0 contains a summary of the variable definitions and data sources of all key measures used in the analyses, the year(s) of data collection, and the relevant survey questions used to construct these measures. Table A1 reports descriptive statistics for the samples used in the models of adult health status both for the full sample and separately by race. The substantial race differences in childhood family and neighborhood characteristics are highlighted in this table.

Income is the total for the family in which the child lives, and it is measured from the five-year average for the years 1967-1972. All dollar values are expressed in 1997 dollars using the CPI-U. The parental income measure is specified as the income-to-needs ratio and I explore nonlinearities in effects at the bottom of the income distribution (child poverty).

Child health insurance coverage is measured through information collected in the first five waves of the PSID (1968-1972) on whether the parent (head of household) had access to private health insurance coverage and if so, whether the entire family was covered. I include an indicator variable defined as lack of private health insurance coverage in childhood years during 1968-1972. Lack of private health insurance may discourage preventive medical care use. For those who lacked private coverage for their children, the data suggest that public health insurance coverage was utilized to some extent, but there were not enough individuals in the sample who persistently lacked public and private insurance during these childhood years to define “no public or private insurance during childhood” as an additional category.

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

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).

to be biased and induce (health) state dependence because within-category variation increases when going down from excellent to poor health.

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

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

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

$$P_{ij} = P(H_i = j) = \Phi\left(\frac{\mu_U - x_i\beta}{\sigma_v}\right) - \Phi\left(\frac{\mu_L - x_i\beta}{\sigma_v}\right),$$

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:

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

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 .

Appendix C: Descriptive Results

Figure 10 highlights the significant birth cohort variation in childhood exposure to court-ordered school desegregation, where we see roughly 20 percent of school-age years among PSID original sample black children born in the early 1950s were spent exposed to school desegregation, while those born in the late 1960s were exposed to court-ordered school desegregation (integrated schools) for about 75 percent of their school-age years.

I present nationally-representative estimates of the bivariate relationship between adult health status and childhood school quality (i.e., school district per-pupil spending and class size), race by birth cohort and school desegregation plan status, socioeconomic status in childhood (i.e., parental education, income), and parental expectations for child achievement. These figures display the age pattern of the health index (which was described earlier) over the course of adulthood. The age patterns of the conditional expectations are calculated using a Jianqing Fan (1992) locally weighted regression smoother, which allows the data to determine the shape of the function, rather than imposing, for example, a linear or quadratic form. Some additional figures also display the proportion of years in poor health as an adult. The differences presented are all statistically significant.

With the timing of court-ordered school desegregation in mind, Figures A2 and A3 present adult health status by race, birth cohort, and school desegregation plan status. I find substantial birth cohort differences in adult health status among African Americans. In particular, blacks born in the early 1950s (in the pre-Brown vs. Board of Education era) have significantly worse health when compared with birth cohorts born between 1955-1963 and 1964-1968, evaluated at similar ages. Furthermore, blacks born between 1964-1968, who grew up in the post-Civil Rights Act era and reached school-age years after the school desegregation efforts began to accelerate, had significantly better health in adulthood evaluated at similar ages, relative to birth cohorts born prior to 1964. For example, by age 40, blacks born between 1964 and 1968 had a roughly 7-point higher health utility index score relative to blacks born between 1950-1954; this magnitude is comparable to the raw black-white difference in health at age 40 observed among individuals born between 1964 and 1968. In contrast, as shown in Figure A2, there are no significant birth cohort differences in adult health among whites; thus, I find that the raw age-adjusted black-white gap in adult health narrowed significantly for successive birth cohorts of the 1950s and 1960s.

Figure A3 presents differences in adult health status among blacks whose childhood schools were under court-order to desegregate as compared with blacks whose schools did not implement desegregation plans during their childhood years. I distinguish between blacks whose childhood school desegregation plan implementation was accompanied by significant increases in per-pupil spending with those whose desegregation plans were not. Importantly, we see significantly better health in adulthood

among blacks who grew up in desegregated schools that underwent significant increases in per-pupil spending ($> \$1,000$), but no significant adult health differences between those who grew up in segregated schools and those who grew up in school districts whose desegregation plans were not accompanied by increases in per-pupil spending ($< \$300$). We also see that these differences by desegregation plan status become more pronounced over the course of adulthood (particularly, ages 35 and beyond), which is the pattern we would expect if these differences were driven by how school quality influences socioeconomic mobility. The difference in adult health status by age 40 among blacks who attended schools with a court-ordered desegregation plan versus those who were not exposed to school desegregation plans in childhood is about five points on the health utility index.

Figures A4-A6 present adult health status by child school district per-pupil spending and class size. About seven percent of adulthood is spent in fair or poor health among those who grew up in school districts in which spending per-pupil was in the top quartile, compared with twice that proportion (0.15) among those who resided in districts in which school spending was in the bottom quartile of per-pupil school spending; and these differences appear to widen after age 35 when the labor market returns to schooling become larger. The difference in adult health status by age 40 between individuals who attended schools in the bottom versus top quartile of class size (i.e., ≤ 23 vs. ≥ 27) is about five points on the health utility index, while significant health differences were not present at age 25 (Figure A6).

The association between school quality resources and adult health status among blacks is particularly strong. The difference in adult health status by age 40 among blacks born after 1964 who attended schools in the bottom versus top quartile of per-pupil school spending (i.e., $< \$3,650$ vs. $> \$5,750$) is about seven points on the health utility index, while only minor health differences were present at age 25 (Figure A5). There is likely substantial measurement error in actual per-pupil spending resources available to blacks prior to the enforcement of these desegregation plans, because school district spending, particularly in the South, was directed disproportionately to the majority-white schools within districts (which will not be reflected in district-level spending data). This is the likely reason that, for blacks, I find school district spending has no appreciable relationship with adult health and socioeconomic attainments until birth cohorts who reached school-age after school desegregation plans were in effect (especially in the South).

Figure A7 presents significant bivariate relationships between adult health and parental income, parental education, and self-reported parental expectations for child achievement (measured during childhood). The relationships between the parental income-to-needs ratio and adult health exhibit nonlinearities. Furthermore, the socioeconomic gradient in health appears to widen over the life course, as the health deterioration rate is more rapid in adulthood among those who grew up in more disadvantaged child neighborhood, school and family environments. For example, twenty-three percent

of adulthood years between ages 35 and 55 is spent in fair or poor health among those who grew up in poverty, while those rates are thirteen percent, eight percent, and six percent among the near-poor, those whose parental-income-to-needs ratio is 2 to 3, and those growing up in affluent families, respectively (Figure 9). As shown in Figure A7, the health status of a twenty-five year old who grew up in poverty is roughly at the same level of health as a fifty-year old who grew up in an affluent family (i.e., parental-income-to-needs ratio greater than three).

Segregation may influence subsequent mobility prospects through their effects on expectations for child achievement. As shown in the bottom panel of Figure A7, the bivariate relationship shows that nearly one-quarter of adulthood years between ages 35 and 55 are spent in fair or poor health among children whose parents had low expectations for child achievement, relative to eight percent among those whose parents had college-bound expectations for their child. These parental expectations are likely influenced in part by neighborhood and school resources, as evidenced by the strong neighborhood component in the similarity of parental expectations. Additionally, self-reported parental expectations for child achievement were higher among black parents who were able to raise their children in integrated schools, independent of parental SES.

Data Appendix Table A0.

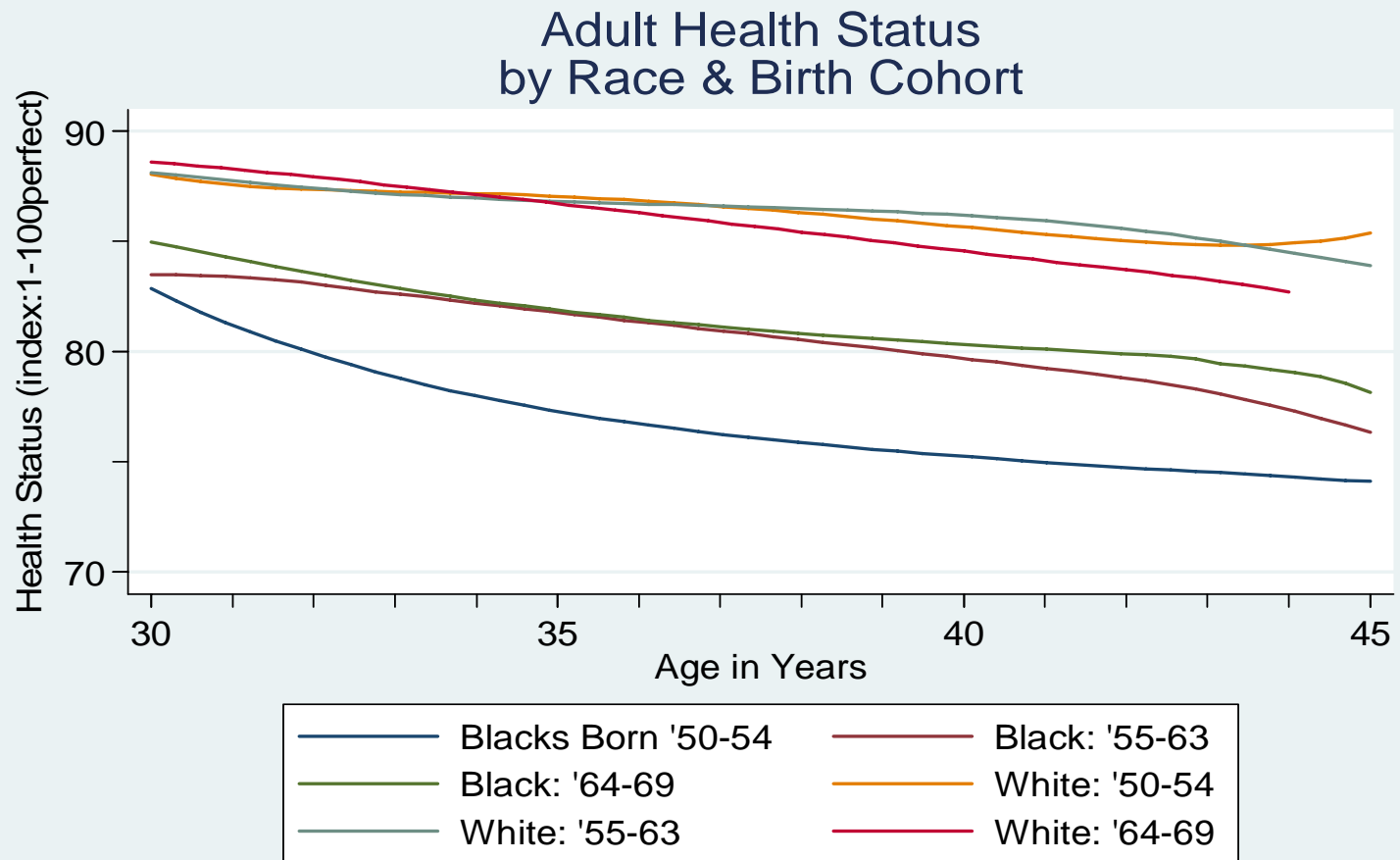
Measures	Data Source	Year(s) collected	Survey Question	Definition
General Health Status	PSID	Adulthood:1984-2007; Childhood (retrospective): 1999/2001	"Would you say your health in general is excellent, very good, good, fair, or poor?"	--
Parental Health Status	PSID	Measured during parent's ages 50s and 60s (1984-2007).	"Would you say your health in general is excellent, very good, good, fair, or poor?"	Proportion of years when parent was in 50s and 60s in which they were in fair/poor health
Child School quality	Office of Civil Rights (OCR) School data; Common Core data of NCES; Census of Governments	1962-1982	PSID respondent's residential location during school-age years matched to school resource data	School district per-pupil spending; avg class size; school segregation
Neighborhood Poverty Rate	1970-2000 Census	Child neighborhood: 1970 Census; Adult neighborhood: 1980-2000 (linearly interpolate for non census years)	PSID respondent's residential location (1968-2007) matched to decennial census tract info	low poverty neighborhood (<10% poor); medium poverty neighborhood (10-30%); high poverty neighborhood (>30%)
Childhood Racial Residential Segregation	1970 Census	1970 Census	Black-white dissimilarity index _{county} : b_{it} & w_{it} = # of black & white individuals in neighborhood i at time t ; B_t & W_t = total # black & white individuals in county.	$\frac{1}{2} * \sum_{i=1}^n \left \frac{b_{it}}{B_t} - \frac{w_{it}}{W_t} \right $
Childhood Economic Residential Segregation	1970 Census	1970 Census	Poverty status dissimilarity index _{MSA} : p_{it} & r_{it} = # of poor & non-poor families in neighborhood i at time t ; P_t & R_t = total # poor & non-poor families in MSA.	$\frac{1}{2} * \sum_{i=1}^n \left \frac{p_{it}}{P_t} - \frac{r_{it}}{R_t} \right $
Childhood Neighborhood/Housing Quality	PSID	1975	Parental self-reports: whether there exist plumbing or insulation problems, or burglary, robbery, assault, drug use problems, or too few police in neighborhood in which they live.	High crime neighborhood=avg response among all PSID households who live in same neighborhood report major crime-related problems; housing insulation/plumbing problems=avg response among all PSID households who live in same neighborhood report insulation/p
Parental/neighborhood Expectations for Child Achievement	PSID	1968-1972	Parental self-reports: "How much education do you think your children will have when they stop going to school? What do you really think will happen?"	low expectations=may not finish high school; college-bound expectations (ref. cat). Neighborhood-level measures obtained by computing avg response among all PSID HHs who live in same neighborhood.
Parental/neighborhood Connectedness to informal sources of support	PSID	1968-1972	Index (0-9) of Connectedness to Potential Sources of Help (constructed from survey responses): Attends church once a month or more; # of neighbors known by name; Has relatives within walking distance; Goes to organizations once a month or more (PTA mtg).	Neighborhood-level measures obtained by computing avg index score based on responses among all PSID HHs who live in same neighborhood.

Table A1. Descriptive Statistics by Race

	All (N=7,212)	Black (N=3,198)	White (N=3,801)
Adult Health Status:			
Excellent	0.26	0.20	0.30
Very Good	0.35	0.29	0.39
Good	0.29	0.36	0.24
Fair	0.09	0.13	0.05
Poor	0.02	0.03	0.01
Age (range: 20-57)	37.8	37.8	37.8
Year born (range: 1950-1970)	1960	1960	1960
Female	0.50	0.55	0.50
<u>Childhood family variables:</u>			
Income-to-needs ratio (5-yr avg, 1968-1972):			
<1 (child poverty)	0.12	0.43	0.06
1-3	0.55	0.48	0.56
>3	0.34	0.09	0.38
Parent's (head's) education:			
High school dropout	0.41	0.74	0.35
High school graduate	0.31	0.20	0.33
College-educated	0.28	0.05	0.32
Born into two-parent family	0.80	0.49	0.85
Low birth weight (<5.5 pounds)	0.07	0.09	0.06
No private child health insurance, 1968-1972	0.10	0.24	0.08
Parental health behaviors (1997 \$):			
Smoked cigarettes at some point, 1968-1972	0.73	0.80	0.72
Alcohol consumption (5-yr avg, 1968-1972)	\$421	\$299	\$437
Parental health status:			
Proportion of 60s mother in fair/poor health	0.32	0.64	0.27
Proportion of 60s father in fair/poor health	0.33	0.66	0.31
<u>Childhood neighborhood variables:</u>			
Neighborhood poverty:			
High poverty neighborhood (>30%)	0.05	0.24	0.01
Medium poverty neighborhood (10-30%)	0.18	0.40	0.14
Low poverty neighborhood (<10%)	0.78	0.36	0.85
Residential segregation dissimilarity index _{county}	0.70	0.71	0.70
High crime neighborhood	0.16	0.26	0.15
N'hood low expectations for child achievement	0.17	0.29	0.15
N'hood college-bound expectations	0.72	0.58	0.74
N'hood connectedness to informal sources of help	6.09	5.82	6.14
Neighborhood plumbing problems	0.14	0.24	0.12
Neighborhood housing insulation problems	0.14	0.18	0.14

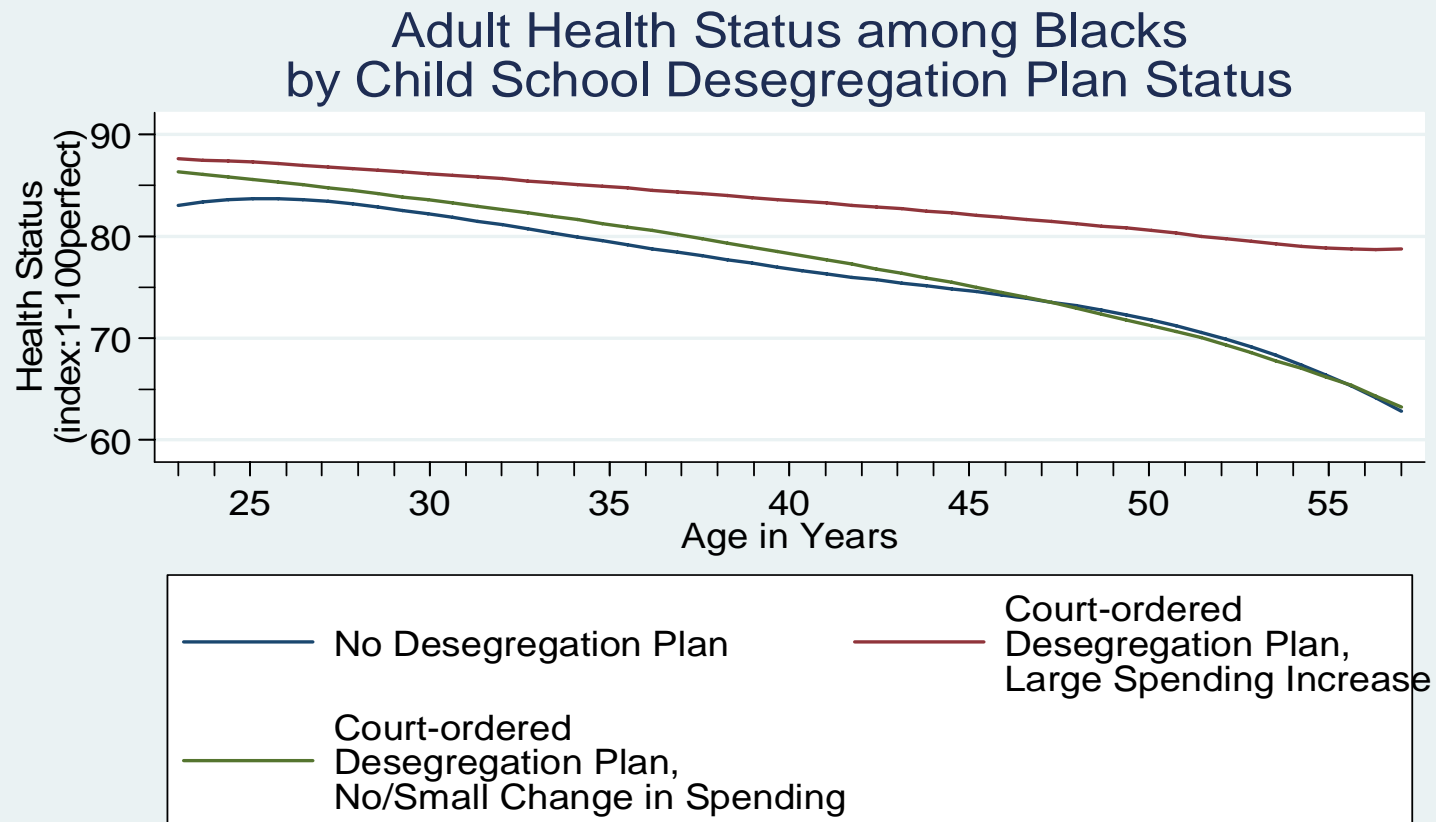
Note: All descriptive statistics are sample weighted to produce nationally-representative estimates of means. Black-white differences in all childhood family and neighborhood factors are statistically significant.

FIGURE A2.



PSID individuals born 1950-1969, followed up to 2007.

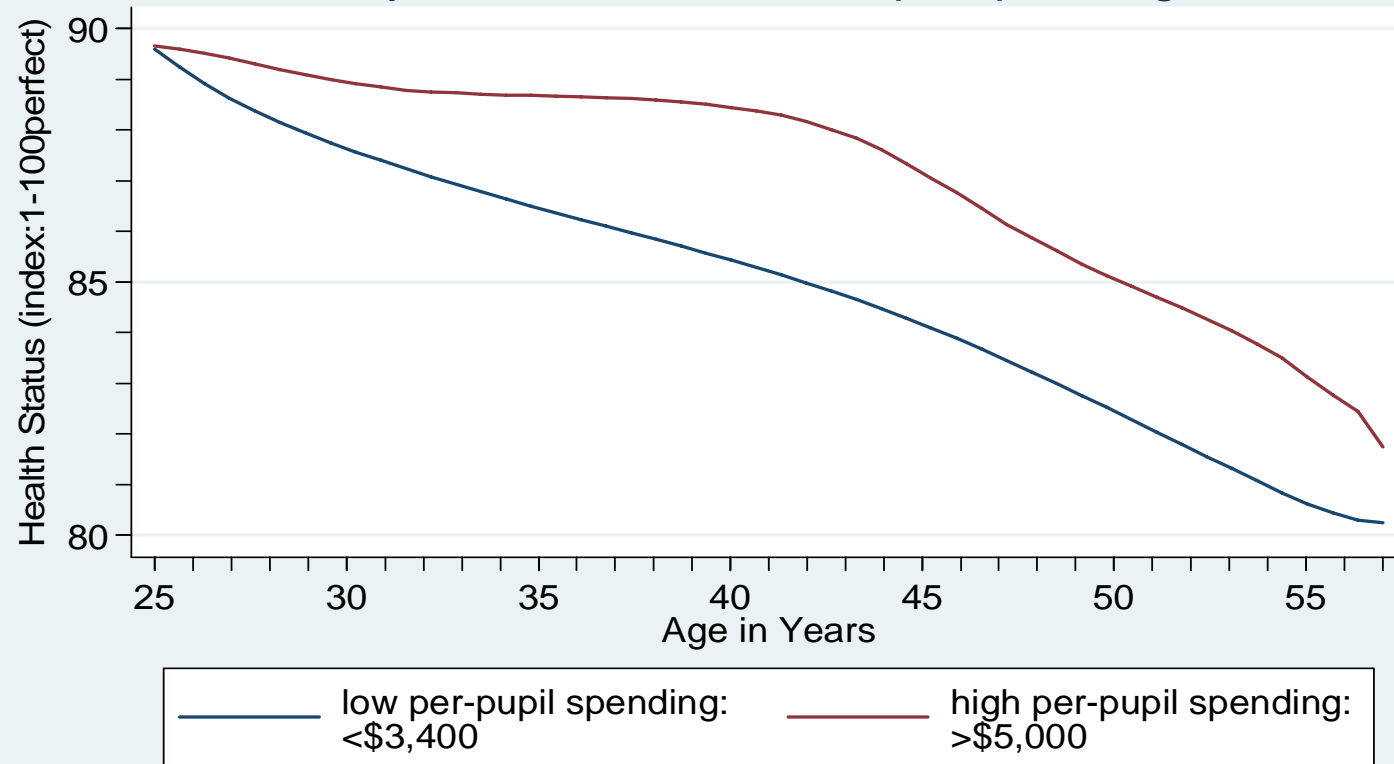
FIGURE A3.



PSID blacks born 1950-1975, followed up to 2007.
Court-ordered desegregation w/large increase in per-pupil spending ($\geq \$1,000$);
Court-ordered desegregation w/ no/small change in per-pupil spending ($< \$300$).

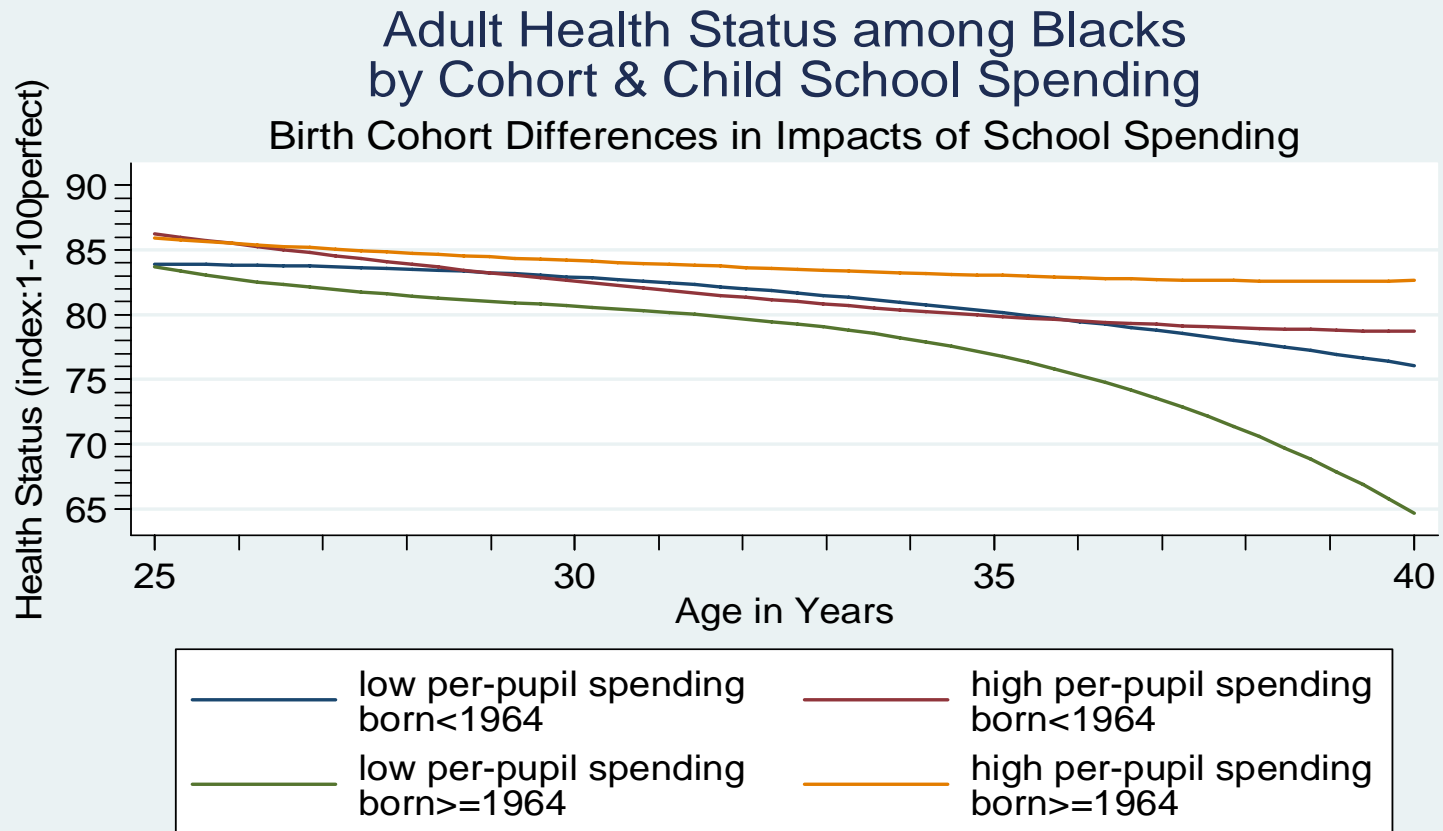
FIGURE A4.

Adult Health Status among Whites by Child School Per-Pupil Spending



PSID whites born 1950-1975, followed up to 2007.

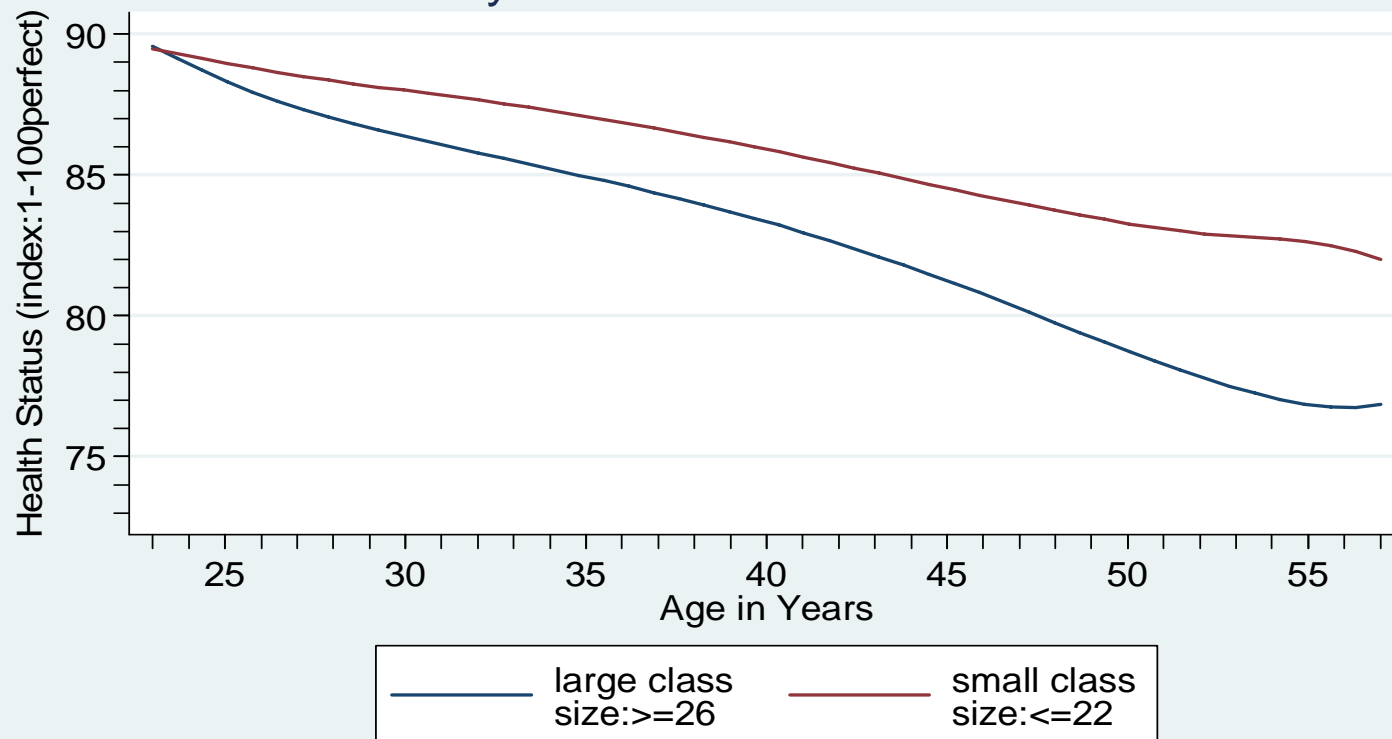
FIGURE A5.



PSID blacks born 1950-1975, followed up to 2007.
High per-pupil spending (above 75th percentile) > \$5,000 (2000 dollars).
Low per-pupil spending (below 25th percentile) < \$3,400 (2000 dollars).

FIGURE A6.

Adult Health Status, All Races by Child School Class Size

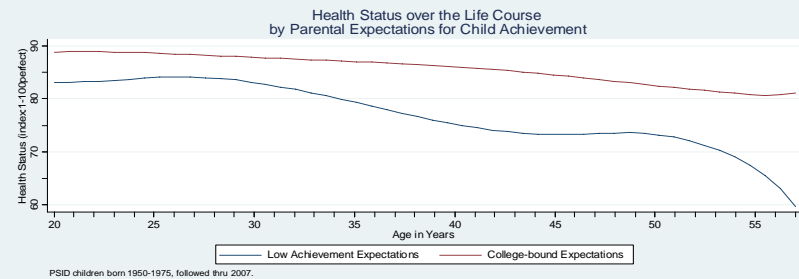
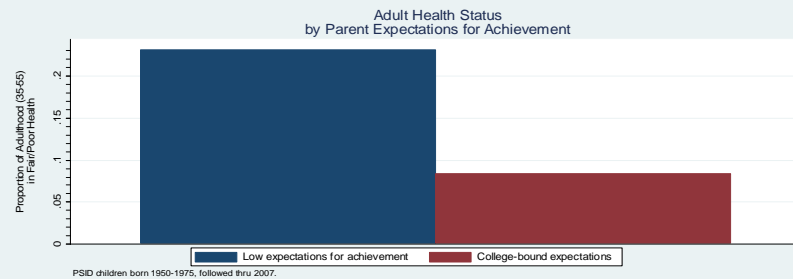
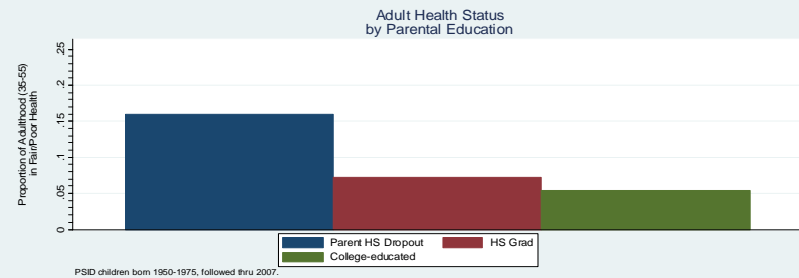
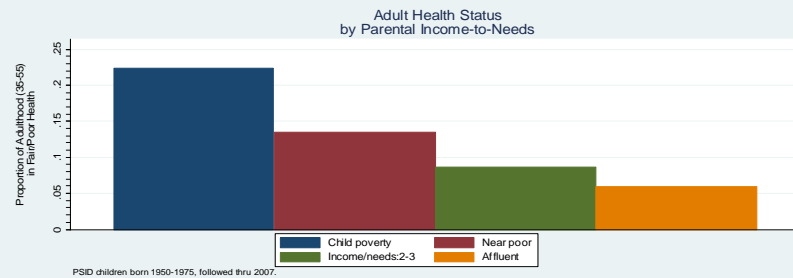
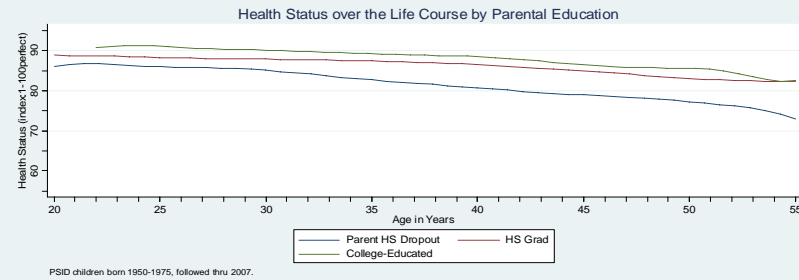
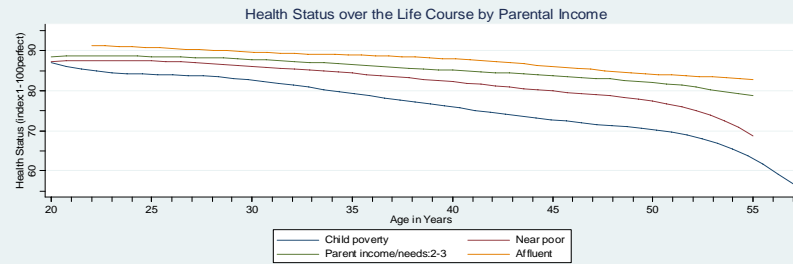


PSID individuals born 1950-1975, followed up to 2007.

Large class (above 75th percentile) ≥ 26 ; Small class (below 25th percentile) ≤ 22 .

FIGURE A7.

Adult Health Status by Parental SES & Expectations for Child Achievement



Data: PSID, 1968-2007
(Individuals born b/w 1950-1975)