NBER WORKING PAPER SERIES

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

Rucker C. Johnson

Working Paper 16664
http://www.nber.org/papers/w16664

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
January 2011

I wish to thank John Logan (Brown University, American Communities Project) for sharing data on school desegregation court cases, Sarah Reber for sharing the Office of Civil Rights school data, and the PSID staff for access to the confidential restricted-use PSID geocode data. I am grateful for detailed comments received from 4 anonymous referees, David Card, Sheldon Danziger, and seminar participants at the NBER labor studies meetings, IRP Summer Workshop (University of Wisconsin-Madison), UC-Berkeley, University of Chicago, University of Michigan, Duke, University of North Carolina, Wellesley College, Chicago Federal Reserve Bank, ASSA/AEA annual conference, Midwest Economics Association meetings, and APPAM annual conference. The views expressed herein are those of the author and do not necessarily reflect the views of the National Bureau of Economic Research.

Long-run Impacts of School Desegregation & School Quality on Adult Attainments
Rucker C. Johnson
NBER Working Paper No. 16664
January 2011
JEL No. I00,I21,I28,J15

## 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.

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

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

## I. INTRODUCTION

Racial segregation that results in race differences in access to school quality has often been cited as perpetuating inequality in attainment outcomes. Since the landmark 1954 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). 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 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. I also 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.[2] 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.

The analysis was conducted in three stages. First, I estimated models of the predictors of the timing of initial desegregation court orders, which serves to demonstrate the exogeneity of the "treatment". 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 (Appendix B). 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 that desegregation plans were effective in narrowing black-white gaps in per-pupil school spending and class size and decreasing school segregation. 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. I estimate within-family effects of school quality inputs on later-life health. 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 reveals 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 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. By examining life course effects of school desegregation across a broad range of subsequent outcomes, I attempt to shed light on the mechanisms through which differences in school quality translate into differences in adult outcomes.

The remainder of the paper is organized as follows. The focus of the next section is the analysis of the effects of school desegregation on school quality inputs (per-pupil spending; class size; school segregation). This informs what the typical "treatment" represented for the average black child. The data and measures used to evaluate the long-run impacts on adult outcomes are described in section III. Section IV discusses the empirical strategy, econometric model, and estimation methods. The long-run results are presented in section V. This includes subsections that a) attempt to rule out competing explanations and violations of the identifying assumptions; b) evaluate the robustness of the results and explore their sensitivity to alternative functional form, specification tests, and alternative empirical strategies (with different underlying identification assumptions); and c) involve specifications that attempt to explore potential mechanisms. Summary discussion to put the magnitudes in perspective in relation to previous studies and concluding statements are provided in the final section.

## II. Using the Timing of Court-ordered Desegregation as a Quasi-experiment

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

An understanding of the causes of the timing of desegregation is critical to the identification strategy. Accordingly, Appendix B provides a brief history of school desegregation litigation and implementation with an eye towards identification issues and demonstrating the validity of the research design—namely, the quasi-random timing of initial court orders. This appendix includes analysis of school desegregation policy to describe aspects of the nature and timing of steps taken to desegregate the schools. I briefly summarize the key insights that emerged from this analysis.

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] Figure A2 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. Districts exhibit a great deal of variation in the year in which the initial court order was issued and the subsequent timing when major desegregation plan implementation actually took place; this variation is evidenced both within and across regions of the country (see Figures A1-A5).

Most school districts did not adopt major school desegregation plans until forced to do so by court order (or threat of litigation) due to individual cases filed in local Federal court. The importance of legal precedent caused the NAACP to strategically bring suits first, and foremost, when and where there was the greatest likelihood of winning, not where the largest potential gains from desegregation could be achieved for a particular local community at a point in time.

Enforcement of desegregation did not begin in earnest until the mid-1960s. State and federal dollars proved to be the most effective incentives to desegregate the schools. A critical turning point was the enactment of Title VI of the 1964 Civil Rights Act (CRA) and Title I funds of the 1965 Elementary & Secondary Education Act (ESEA), which prohibited federal aid to segregated schools and allowed the Justice Department to join suits against school districts that were in violation of the Brown vs. Board order to integrate. This Act dramatically raised the amount of federal aid to education from a few million to more than one billion dollars a year; and, for the first time, the threat of withholding federal funds became a powerful inducement to comply with federal desegregation orders (Cascio et al., 2010; Holland, 2004). This resulted in a significant drop in the extent of racial school segregation thereafter reinforced by local Federal courts. Thus, there is a sharp post-1965 discontinuity in school desegregation.

This pattern and discontinuity after 1965 is also evident in the time lag between initial court order and major desegregation plan implementation, which occurs in the South and non-South (Figure A3). For initial court orders meted out after 1965, there is 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 (i.e., there is a systematic long delay that decreases in years leading up to 1965).

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

The process became highly decentralized with a diverse set of agents that initiated court litigation following the Brown decision, which contributed to the idiosyncratic nature of the timing and location where legal challenges arose that resulted in initial court orders. This legal history of school desegregation is important because it illustrates the 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 (see Appendix B for details).

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. I find little evidence that pre-treatment characteristics significantly predict the timing of court orders (Table A1).

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 makes 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. These findings inform the empirical approach used to identify school desegregation impacts.

*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, 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 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.[5]

While data is available on the 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, 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.

The event study framework compares school district per-pupil spending, student-to-teacher ratios, 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,

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

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,…,1982; $\theta_c$ 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, $1(\ )$, is equal to one when the year of observation is $y$ = -5, -4,…, 1,…,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).[6]

The point estimates of interest, $\pi_y$ and $\tau_y$, are identified using variation in the timing of desegregation plan implementation. Because the indicator for $y$ = 0 is omitted, $\pi_y$ is interpreted as the average difference in outcomes $y$ years *before* the plan was implemented, and $\tau_y$ is the average difference in outcomes $y$ years *after* the desegregation plan was implemented. Estimates of $\pi_y$ allow a visual and statistical evaluation of the potential importance of pre-treatment, time-varying school district-level, unobservables; estimates of $\tau_y$ allow the post-treatment dynamics to be explored. The $\pi_y$ and $\tau_y$ 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).[7]

A key asset of this identification strategy is that estimates of $\pi_y$ and $\tau_y$ 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 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 individual school districts as the observational unit and estimate unweighted regressions, then the estimates will represent the impact experienced for the average school district. While this parameter is intriguing, I am most interested in documenting the impacts of school desegregation for the average black student. 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).[8]

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.[9] To control for the possible coincident expansion of other programs, I 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.[10]

*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 1-3. 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. As shown in Figure 1 (top left graph), following court desegregation orders, there is a sharp decline in the school district racial dissimilarity index, which ranges from zero to one, and represents the proportion of black students who would need to be reassigned to a different school for perfect integration to be achieved given the district's overall racial composition. There is no evidence of pre-existing segregation trends in the school districts prior to the court orders. Such a trend, had it existed, would have raised concern about the validity of the approach. Within 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 1, we witness a parallel significant pattern for the black-white exposure index (an alternative measure of school segregation). 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.[11]

*Increased Per-Pupil Spending*. Figure 2 shows court-ordered desegregation effects on school district per-pupil spending, separately by revenue source (local; state; federal). The results indicate that, on average, school district per-pupil spending increased by nearly $1,000 by the end of the fourth year after court-ordered desegregation relative to the year immediately preceding the initial court order, which differed markedly from the trend leading up to the year these rulings went into effect. This is a substantial increase given that the average level of per-pupil school spending in 1967 among districts that had not yet implemented a plan was $2,738 (in 2000 dollars). Importantly, the large increase in school district per-pupil spending is driven solely by the infusion of state funds following the timing of court-ordered school desegregation (top graph in Figure 2). 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).[12] As shown in the bottom graph of Figure 2, 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.

*Reductions in Class Size*. Figure 3 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.[13]

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. 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[14]; 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 (upper-right graph of Figure 3). 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]

## III. 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.[16] 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 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.[17] The primary analyses use the original sample children born between 1951 and 1970, which 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 is outlined in Appendix A). 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). 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 (Figure A7).

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 information 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). The appendices and Appendix Table D0 lists the sources and years of all data elements along with details of the PSID survey questions used to construct key measures. Appendix Table D1 contains sample descriptive statistics for childhood family- and neighborhood-level measures by race.

*Outcomes of interest*. A broad range of adult outcomes are analyzed including educational attainment, 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 only 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.[18] 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).

This data is combined to provide new evidence on the long-run impacts of school desegregation.

## IV. Empirical Approach

Point-in-time comparisons of integrated and segregated school systems confound the effect of desegregation plans with the effect of factors that influenced their implementation. I match changes in adult attainment outcomes of blacks and whites to the exact timing of school desegregation. Average outcome trends in the years leading up to desegregation are compared to rule out competing explanations. As will be shown, the evidence is consistent with the identifying assumption that the timing of the initial court order is otherwise unrelated to trends in subsequent outcomes. Evidence of endogenous delay in implementation of major desegregation plan following (exogenous) initial court order supports use of 2SLS/IV 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 to identify desegregation effects among children who grew up in districts for which I lack precise desegregation plan implementation information; and 2) for children from the subset of large districts for 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, though not all 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 in 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, 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 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 enacted in many cities in the 1960s, 1970s, and 1980s, when many of these children were growing up. 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 in the timing of initial court orders and subsequent implementation of desegregation plans. I measure the proportion of an individual's school-age years (i.e., ages 5-17) in which they resided in a school district that was under court order to desegregate. I utilize the birth cohort variation in exposure to court-ordered school desegregation among the broad range of birth cohorts to identify effects on adult socioeconomic and health outcomes (Figure A7).

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

$$(2) \qquad \begin{aligned} Y_{icb} &= \theta_0 \left(b - T_c^*\right) \cdot D_{cb} 1\left(b - T_c^* < 0\right) + \theta_1 \left(b - T_c^*\right) \cdot D_{cb} 1\left(0 \le b - T_c^* \le 12\right) \\ &+ \theta_2 \left(b - T_c^*\right) \cdot D_{cb} 1\left(b - T_c^* > 12\right) + X_{icb}\beta + \eta_c + \lambda_b + \varepsilon_{icb} \end{aligned}$$

where $Y_{icb}$ represents an age-adjusted adult outcome of interest for individual *i* who turned 17 in year *b* and grew up in school district *c*; and $\left(b - T_c^*\right)$ represents "Year Aged 17 – Year of Initial Court Order". Because of the structural break in the lag between initial court order and desegregation plan implementation, the models include an interaction term for pre-1965 court orders (equation (2) above abstracts from this feature to ease illustration). 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. A spline specification is used to place some structure on the relationship between court-ordered desegregation exposure and adult outcomes to improve precision, but the structure imposed is flexible enough to allow several important specification tests to examine whether the detected impacts support a causal interpretation of school desegregation.

*D* represents a set of dummy indicators for the three spline intervals: years before court orders went into effect ($b - T_c^* < 0$); school-age years of exposure $\left(0 \leq b - T_c^* \leq 12\right)$; and years beyond school-age ($b - T_c^* > 12$). The key parameter of interest is $\theta_1$ (relative to $\theta_0$), where $\theta_1$ captures the impact of each additional year of exposure to court-ordered desegregation, ranging from 0 to 12 years of exposure. Let *k* denote the number of years before or after the initial court order that an individual turned 17, which is constructed from variables for year aged 17 (*b*) relative to the year of the initial desegregation order ($T_c^*$) in district *c*. Thus, $\theta_1 k$ gives the expected difference in adult outcomes between individuals who became age 17 *k* years after initial court orders are enacted, relative to individuals who had reached age 17 the year prior to it (age 18 the year the court order took effect). I use the year before court orders are enacted as the reference point. The specification allows for the effects to manifest immediately following the first year of child exposure to desegregation court orders and to be a function of the duration of exposure, which is important both because it often took several years for a major desegregation plan to be fully implemented following a court order and the effects of integrated schools may increase with a child's exposure to the "treatment".[19]

The identification comes from variation across school districts across birth cohorts in court-ordered desegregation 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 court order, conditional on (race-specific; region-specific) year of birth effects. Or, alternatively, their outcomes would have been similar to those who were born in the same year and grew up in the same region of the country but court-ordered desegregation 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 court orders and a break in this trend once desegregation orders go into effect. $\theta_0$ captures the pre-period linear trend in outcomes prior to desegregation. $\theta_2$ 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 experienced desegregation throughout their school-age years: for one it occurred in kindergarten and for another court-ordered desegregation occurred at 3 years old, and thus there is no difference in actual exposure during school-age years. This provides an important specification test in that the coefficient on $\theta_2$ should be insignificant, if the results are consistent with a causal impact of desegregation.

The model includes school district fixed effects ( $\eta_c$ ) and birth cohort fixed effects ( $\lambda_b$ ), 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):

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

where $Age^{*}_{cb}$ represents the individual's age when court-ordered desegregation first occurred in their school district of upbringing.

The key parameters of interest include first exposure during high school ($\theta_1$), junior-high/middle school ($\theta_2$), or elementary school yrs ($\theta_3$), relative to those who turned age 18 when desegregation court orders 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., $\theta_4$ should be insignificant). Furthermore, $\theta_0$ provides another test of pre-existing time trends in outcomes prior to court-ordered desegregation. 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.

*2SLS/IV Approach*. In order to address endogenous delay in implementation of major desegregation plan following (exogenous) initial court order, I employ a 2SLS/IV 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 V provides more discussion of a variety of falsification exercises and specification tests performed.

Because I did not want to include endogenous residential moves (e.g., those induced by school quality changes that accompanied desegregation), 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 (Appendix C).

*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.[21] 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 outcomes at the same age and controls for birth order, year of birth, birth weight, and whether mother was married at birth.

The sibling difference approach complements 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 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, 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.

## V. RESULTS

*Educational Attainment*. Table 1 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 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.[22]

Similarly, large, statistically significant effects of childhood exposure to court-ordered desegregation on completed years of education are found for blacks. The models 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. Each additional year of exposure to court-ordered desegregation leads to a 0.08 increase in years of education for blacks. Figure 4 (top left panel) shows 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. 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 1), 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 completed education before desegregation orders are enacted; but after enactment, we see a structural break in the trend for blacks. Furthermore, I find no significant effects for blacks for years of exposure beyond one's school-age years 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 across the model specifications, and the point estimates are negligible. The small, insignificant effects for whites provide further evidence to rule out the competing hypothesis that the black improvements in educational attainment were driven by secular trends in desegregated districts. These results are highlighted in Figure A8 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.[23]

Table 2 presents the 2SLS/IV estimates of the effects of major desegregation plan implementation on the probability of high school graduation and years of education, respectively, by race. This set of analyses uses the year of 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 2.[24] The models include race-specific controls for year of birth fixed effects, gender, age at most recent survey interview, and childhood family/neighborhood factors.

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 (identical point estimate found for blacks in Table 1). The desegregation effect sizes for blacks are comparable to the influence of having college-educated parents. No significant effects are found for whites.

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

The next series of regression results reveal large, significant effects of court-ordered desegregation on blacks' adult economic status and labor market outcomes, using the same sequence of model specifications. Table 3 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, the results are discussed in succession and are highlighted in Figures 4 & A10. 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 are in 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.[25]

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 significantly increases the family income-to-needs ratio by about 0.1

(column 6) and reduces the annual incidence of poverty in adulthood by 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 for blacks represent substantial improvements in adult economic status. The average effects of a 5-year exposure to court-ordered school desegregation (due to post-'64 orders) yields about a 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 (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 (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 (Figure 4 and Table 3). The point estimates are negligible for whites. These important specification tests affirm the credibility of the research design and rule out several competing explanations for the pattern of results.

Table 4 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 Table 2. 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. 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 significantly reduces the annual incidence of poverty in adulthood by 2.2 percentage points (column 9). 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 in Table 3. The estimated magnitudes of desegregation impacts are on par with the coefficients on parental education.[26]

*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 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) 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 5 presents effects by race on these outcomes for men. 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 those in prior models, which serve to highlight the larger reduction in the likelihood of incarceration among blacks who were exposed to integrated schools throughout their childhood years (versus those with more limited exposure). The models include the same set of baseline controls as previous models.

For blacks the results indicate that, relative to growing up in segregated schools throughout one's school years, exposure to desegregation beginning in one's elementary school years leads to a 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 (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—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. Furthermore, the incarceration effects explain a significant amount of the work hours' effects of desegregation for black males.

*Adult Health Status*

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

The next series of regression results use the same sequence of model specifications, and reveal large, significant improvements in blacks' adult health status resulting from exposure to court-ordered school desegregation. The main health outcome analyzed is self-assessed general health status (GHS). To scale the GHS categories, I use the health utility-based scale that was developed in the construction of the Health and Activity Limitation index (HALex) (details in Appendix C). The results are based on interval regression models using a 100-point scale where 100 equals perfect health—the interval health values associated with GHS used are: [95, 100] for excellent, [85, 95) for very good, [70,85) for good, [30,70) for fair, and [1,30) for poor health.

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

The regression results (Table 6) 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). 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. The effects for blacks represent substantial improvements in adult health status, as the average effects of a 5-year exposure to court-ordered school desegregation yields about a 3 point increase in the adult health status index (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 if that year were 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. This magnitude is also comparable to the impacts of parental education. There is little evidence of pre-existing time trends in adult health in the years leading up to the court order, nor are there significant effects of court orders that correspond with non-school ages for blacks. Following the pattern of results for the education and adult socioeconomic attainment outcomes, I again find negligible desegregation effects on the adult health status of whites. Similarly, the 2SLS/IV estimates imply that the effects of 5 years of exposure to major desegregation plans result in about a 2.6 point increase in the adult health index for blacks (column 4), with insignificant desegregation effects found for whites (column 5).

The results across the main set of adult attainment outcomes analyzed using the expanded difference-in-difference model specifications are summarized in Figure 4. 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 strongly support the exogeneity of the initial court order. First, the pre-period trend is flat, showing no systematic differences in school district trends prior to 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 implementation) to result in a pattern like this. The small, insignificant effects for whites provides further evidence to rule out the competing hypothesis that blacks' improvements in outcomes were driven by secular trends in outcomes in desegregated districts.[27]

Table 7 presents sibling fixed effect models designed to assess the long-run effects of school desegregation on education and adult health. I find that black children who were exposed to court-ordered school desegregation for the majority of their school-age years experienced significantly improved education and health outcomes in adulthood, compared with their older siblings who grew up in segregated school environments with weaker school resources (controlling for age and birth cohort effects). Negligible effects are found for whites. I find that education and health outcomes among blacks were particularly affected by changes in access to school resources associated with desegregation, not simply changes in exposure to white students.

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 education and health outcomes in adulthood (evaluated at the same age) (columns (3),(6) of Table 7). 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 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 sibling fixed effect models for socioeconomic attainment outcomes.[28]

***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 (pre-school ages or beyond age 17) 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 expected if endogeneity issues do not drive 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 holds, then we might expect the estimates to change very little with the addition of these characteristics. 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.

Table A2 probes the robustness of these estimates further. As an additional falsification exercise, I re-estimated equation (2) replacing the timing of initial court ordered desegregation variables with litigation cases that were not successful and the corresponding year of their court ruling to identify effects; in essence estimating the effects of a series of "placebo" initiatives. If my baseline estimates capture the effects of school desegregation – not an earlier or later unobserved shock or intervention – the largest estimates of desegregation effects should arise from estimation of the model as originally specified. Indeed, this is the case (Table A2). In particular, a placebo treatment variable is included in the model which captures the years of childhood exposure to unsuccessful court litigation. The coefficient on the placebo variable should be small and insignificant. Indeed, when I used the placebo and the corresponding year of their court ruling to identify effects, they are not associated with any measurable impact on any outcome of interest. These results demonstrate that timing of *unsuccessful* court litigation is unrelated to adult attainment outcomes; only the timing of initial year of successful litigation that led to court-ordered school desegregation is significantly associated with blacks' adult socioeconomic & health attainments. This provides additional evidence that the main results are not spurious, and helps rule out confounding influences from changing local demographic characteristics or social policies. If such omitted variables spuriously inflate the estimated effect of desegregation, the placebo coefficient should be significant. It is not.

These falsification tests provide additional evidence that unobserved factors do not contaminate the estimates. The results are robust to many other sensitivity tests including adding more fixed effects, examining subgroups of the sample, and placebo tests on groups not likely to be affected (e.g., contemporaneous black adult employment rates (in occupations outside of K-12 education), providing further evidence of the exogeneity of the treatment. The results, as expected, show no significant impact of desegregation 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); d) had the largest impacts on blacks in communities where desegregation resulted in the largest changes in school quality inputs (Table 8); and finally e) had no effects on whites. The results support a causal interpretation of the effects of school desegregation by uncovering sharp differences in the estimated long-run effects on cohorts born within a fairly narrow window of each other that differ in whether and how long they actually attended desegregated schools.

*Exploring the 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 amount of desegregation achieved by the courts varied from district to district, as did the resultant change in access to school quality inputs received by minority children. This was in part because desegregation was achieved in a variety of ways across school districts and was applied in many different initial school environments based on the form of racial segregation—*de jure* in the South and *de facto* in other regions of the country. 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 II 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 education, economic, and health attainment outcomes among blacks.

The results are presented in Table 8. 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 after 1963, for which I have school district per-pupil spending (school segregation) information 1 year before and 3 years after initial court order. The estimated district-specific induced-change in per-pupil spending (school segregation) is 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.

For blacks' educational, economic and health attainments, the results suggest that changes in school quality resulting from integration played an important role. The results indicate significant interactive effects of school desegregation exposure with the resultant change in access to school quality, as proxied by changes in per-pupil spending. I find that court-ordered desegregation that led to larger improvements in school quality resulted in more beneficial educational, economic, and health outcomes in adulthood for blacks who grew up in those court-ordered desegregation districts. To facilitate interpretation of marginal effects, the units of the per-pupil spending are in thousands of dollars—a 1-unit change represents a $1,000 change in spending (2000 dollars). Thus, 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. 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 did not have appreciable long-run impacts on blacks' adult 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, an annual poverty incidence in adulthood that was 6.8 percentage points lower, and an adult health status index that was about 4.5 points higher among blacks than the average effect induced by school desegregation.

The difference-in-difference, 2SLS/IV, 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. 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 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 their school careers. 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. Both patterns are consistent with a treatment dose-response relationship.

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 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. The lack of heterogeneity in effects between southern and non-southern school districts is particularly noteworthy.

## VI. Summary discussion and Conclusion

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

I control for possible confounders in a number of ways. First, I examine the determinants of the timing of the occurrence of the initial court order and major desegregation plan adoption, and find that collectively the pre-treatment school quality, SES, demographic, and labor market related characteristics do not significantly predict the year of the initial court order. 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, 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. 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. 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 (Welch/Light data). Using data from the 1970 and 1980 censuses, he uses difference-in-difference and fixed effect methods and finds that desegregation explains ½ of the decline in the black high school dropout rate during the 1970s among the 125 large school districts he analyzed. 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 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. There were longer delays in implementation of major desegregation plans following initial court orders for districts that had significant minority proportion, larger per-capita school spending, teacher salary, smaller average student-to-teacher ratios, and/or greater income (Table A1). These factors likely lead OLS estimates of the effects of desegregation plans to be understated.

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.

The findings of the present study show that labor market outcomes, and adult income and health status rose in line with blacks' 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). Table 9 presents a summary of the implied Wald estimates of the returns to education (reflecting a combination of both increased quantity and quality) across the adult outcomes. A Wald estimate of the returns to education on wages is the ratio of the estimates of the desegregation effects on wages and completed years of education, yielding a return of 27 percent (0.0219/0.0800). 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. The incarceration effects of desegregation are consistent with Lochner and Moretti (2004), who report that a 10 percentage-point increase in high school graduation rates would reduce overall violent crime arrest rates for blacks by 25 percent and reduce murder arrests by two-thirds.

There are several plausible explanations for the much larger estimates obtained in these analyses. First, improved school environments could have facilitated a higher quality teacher workforce 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.[29]

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

A limitation of the court-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. 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.

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. 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.

*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.[30] 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] 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.

[3] Integration may also influence long-term outcomes in ways that are unrelated to educational outcomes.

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

[5] 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.

[6] The models estimated upon which Figures 1-3 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).

[7] 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.

[8] 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.

[9] 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.

[10] 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.

[11] 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 1). Levels of racial integration in schools peaked around 1988.

[12] 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.

[13] 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.

[14] The analytic sample includes 14,869 schools from 667 districts from 33 different states; standard errors clustered at school-level.

[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 began interviewing a national probability sample of families in 1968. These families were re-interviewed each year through 1997, when interviewing became biennial. All persons in PSID families in 1968 have the PSID "gene," which means that they are followed in subsequent waves. When children with the "gene" become adults and leave their parents' homes, they become their own PSID "family unit" and are interviewed in each wave. Moreover, the genealogical design implies that the PSID sample today includes numerous adult sibling groupings who have been members of PSID-interviewed families for nearly four decades.

[17] The PSID maintains extremely high wave-to-wave response rates of 95-98%. Appendix C 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).

[18] 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. 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.

[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] 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.

[22] 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.

[23] In Appendix C, I also present results from multinomial models of educational attainment, which 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.

[24] 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).

[25] 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.

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

[27] 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.

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

[29] 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.

[30] 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).

**BIBLIOGRAPHY**


Almond, Douglas, and Kenneth Y. Chay. 2003. "The Long-Run and Intergenerational Impact of Poor Infant Health: Evidence from Cohorts Born During the Civil Rights Era," mimeo.

Almond, Douglas, Hilary W. Hoynes, and Diane Whitmore Schanzenbach. (2008). "Inside the War on Poverty: The Impact of Food Stamps on Birth Outcomes." *National Bureau of Economic Research Working Paper 14306*, September.

Altonji, J. and T. Dunn. 1996. Using Sibling Models to Estimate Effects of School Quality on Wages. *The Review of Economics & Statistics,* MIT Press, vol. 78(4): 665-71, November.

Ashenfelter, O., Collins, W., Yoon, A. 2006. "Evaluating the Role of Brown v. Board of Education in School Equalization, Desegregation, and the Income of African Americans." *American Law and Economics Review* 8(2):213-248.

Altonji, J. and T. Dunn. 1996. "Using Siblings to Estimate the Effect of School Quality on Wages." *The Review of Economics and Statistics*. MIT Press, November, 78 (4): 665-71.

Bond, Horace Mann. 1934. *The Education of the Negro in the American Social Order*, New York, NY: Octagon Press.

Bond, Horace Mann, 1969. *Negro Education in Alabama: A Study in Cotton and Steel*, New York, NY: Octagon Press.

Boozer, M., Krueger, A., Wolkon, S. 1992. "Race and School Quality Since Brown v. Board of Education." *Brookings Papers on Economic Activity, Microeconomics* 1992, 269-326.

Bound, J. and G. Solon. 1999. "Double Trouble: On the Value of Twins-Based Estimation of the Return to Schooling". *Economics of Education Review*. 18:169-82.

Cain, G. and H. Watts. 1972. "Problems in Making Policy Inferences from the Coleman Report". *American Sociological Review*. 35(2): 228-252.

Cascio, E., Gordon, N., Lewis, E., and S. Reber. 2008. "From Brown to Busing." *Journal of Urban Economics* 64(2008):296-325.

Cascio, E., Gordon, N., Lewis, E., and S. Reber. 2010. "Paying for Progress: Conditional Grants and the Desegregation of Southern Schools". *Quarterly Journal of Economics*.

Card, D. 1999. "The Causal Effect of Education on Earnings," in *Handbook of Labor Economics: Volume 3A*, edited by O. Ashenfelter and D. Card, New York: North-Holland, 1801-63.

Card, D. and A. Krueger. 1992. "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States." *Journal of Political Economy* 100: 1-40.

_______. 1996. "School Resources and Student Outcomes: An Overview of the Literature and New Evidence from North and South Carolina." *Journal of Economic Perspectives* 10:31-50.

Card, D. and J. Rothstein. 2007. "Racial Segregation and the Black-white Test Score Gap." *Journal of Public Economics* 91(11-12):2158-2184.

Case Anne and L. Katz. 1991. "The Company you Keep: the Effects of Family and Neighborhood on Disadvantaged Families". National Bureau of Economic Research Working Paper no. 3705.

Case, Anne, Darren Lubotsky and Christina Paxson. 2002. "Economic Status and Health in Childhood: The Origins of the Gradient". *American Economic Review*. December, 92(5): 1308-1334.

Chay, K., Guryan, J., and B. Mazaumder. 2009. *Birth Cohort and the Black-Whie Achievement Gap: The Roles of Access and Health Soon After Birth*. NBER Working Paper #15078.

Clotfelter, C.T. 2004. *After Brown: The Rise and Retreat of School Desegregation*. Princeton University Press, Princeton, N.J.

Clotfelter, C.T., H.F. Ladd and J. Vigdor (2006). "Federal Oversight, Local Control and the Specter of "Resegregation" in Southern Schools." *American Law and Economics Review*.

Coleman, J., Campbell E., Hobson C., McPartland J., Mood, Al, Weinfeld, F., and R. York. 1966. *Equality and Educational Opportunity*. U.S. Department of Health, Education, and Welfare: Washington, D.C.

Collins, William and Robert Margo, 2006, "Historical Perspectives on Racial Differences in Schooling in the United States," In *Handbook of the Economics of Education: Volume 1*, edited by E. Hanushek and F. Welch. New York: North-Holland, 107-154.

Currie, J. and E. Moretti. 2003. Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings. *Quarterly Journal of Economics* 118(4): 1495-1532.

Cutler, D. and A. Lleras-Muney. 2006. Education and Health: Evaluating Theories and Evidence. National Bureau of Economic Research Working Paper #12352.

Cutler, D., E. Richardson, and T. Keeler. 1997. "Measuring the Health of the U.S. Population". *Brookings Papers on Economic Activity. Microeconomics*. 1997:217-282.

Deaton, A. and C. Paxson. 1998. "Health, Income, and Inequality over the Life Cycle". In *Frontiers in the Economics of Aging*, David Wise, Ed. Chicago: University of Chicago Press. 431-462.

Donohue, John and James Heckman. 1991. "Continuous vs. Episodic Change: The Impact of Civil Rights Policy on the Economic Status of Blacks," *Journal of Economic Literature*, 29(4), 1603-1664.

Donohue, John, James Heckman, and Petra Todd. 2002. "The Schooling of Southern Blacks: The Roles of Legal Activism and Private Philanthropy, 1910-1960", *Quarterly Journal of Economics*, 117(1), 225-268.

Duncan, Greg, J. Boisjoly, and K. M. Harris. 2001. "Sibling, Peer, Neighbor, and Schoolmate Correlations as Indiciators of Importance of Context for Adolescent Development". *Demography*. August 38 (3):437-447.

Erickson, Pennifer. 1998. "Evaluation of a Population-based Measure of Quality of Life: the Health and Activity Limitation Index (HALex)". *Quality of Life Research*. 7:101-114.

Erickson, Pennifer, R. Wilson and I. Shannon. 1995. "Years of Healthy Life". *Healthy People 2000: Statistical Notes*.7:1-14.

Evans W.N., W. Oates and R.M. Schwab. 1992. "Measuring peer group effects: a study of teenage behavior". *Journal of Political Economy*. 100:966-91.

Ferguson, R. F. 1998. "Can Schools Narrow the Black-white Test Score Gap?" In Jencks, C., Phillips, M. (Eds.), *Inequality in America: What Role for Human Capital Policies?* MIT Press, Cambridge, MA.

Fitzgerald, J., P. Gottschalk and R. Moffitt. 1998. "An analysis of sample attrition in panel data: The Michigan Panel Study of Income Dynamics". *Journal of Human Resources*. 33(2):251-99.

_______. 1998b. "The impact of attrition in the Panel Study of Income Dynamics on intergenerational analysis". *Journal of Human Resources*. 33(2):300-44.

Greenberg, Jack. 2004. *Crusaders in the Courts: How a Dedicated Band of Lawyers Fought for the Civil Rights Revolution*. NY: Basic Books.

Griliches, Z. 1979. "Sibling models and data in economics: beginnings of a survey". *Journal of Political Economy*. 87:S37-64.

Grogger, Jeff. 1996. "Does School Quality Explain the Recent Black/White Wage Trend?" *Journal of Labor Economics*, 14(2): 231-253.

Guryan, J. 2004. "Desegregation and Black Dropout Rates." *American Economic Review* 94(4): 919-943.

_______. 2001. "Does money matter? Regression-discontinuity estimates from education finance reform in Massachusetts. Cambridge, MA: NBER Working Paper 8269.

Hanushek, R., Kain, J., and S. Rivkin. 2004. "New Evidence about Brown v. Board of Education: The Complex Effects of School Racial Composition on Achievement." Working Paper, Hoover Institution, Stanford University.

Heckman, J.J. and P.A.LaFontaine. 2007. "The American High School Graduation Rate: Trends & Levels". NBER Working Paper.

Hoxby, Caroline M. 2000. "Peer Effects in the Classroom: Learning from Gender and Race Variation". National Bureau of Economic Research: Working Paper #7867.

_______. 2001. "All School Finance Equalizations are Not Created Equal," *Quarterly Journal of Economic*, 1231 - 1189.

Hoynes, Hilary and Diane Schanzenbach. (2009). "Consumption Reponses to In-Kind Transfers: Evidence from the Introduction of the Food Stamp Program." *American Economic Journal: Applied Economics* 1(4): 109-39.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. (1993). "Earnings Losses of Displaced Workers." *American Economic Review* 83 (4): 685-709.

Johnson, Rucker C. 2010. "The Health Returns of Education Policies: From Preschool to High School & Beyond." *American Economic Review Papers and Proceedings* (May), 100(2): 188-94.

Johnson, Rucker C. 2009. "Health Dynamics and the Evolution of Health Inequality over the Life Course: The Importance of Neighborhood and Family Background". Unpublished manuscript, UC-Berkeley.

Johnson, Rucker C. 2009. "Who's on the Bus? The Role of Schools as a Vehicle to Intergenerational Mobility". Unpublished manuscript, UC-Berkeley.

Katz, L., J. Kling and J. Liebman. 2001. "Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment". *Quarterly Journal of Economics*. 116(2):607-654.

Kremer, Michael. 1997. "How Much Does Sorting Increase Inequality?" *Quarterly Journal of Economics*. Feb., 112(1): 115-139.

Lankford, H. and J. Wyckoff. 2000. "The Effect of School Choice and Residential Location on the Racial Segregation of Students." Unpublished Manuscript (October).

Leventhal, T and J. Brooks-Gunn. 2001. "Moving to Opportunity: What About the Kids?" forthcoming in *Choosing a Better Life: Evaluating the Moving to Opportunity Social Experiment*, eds. J. Goering and J. Ferris. Washington, DC: Urban Institute Press.

Lochner, Lance and Enrico Moretti. 2004. "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports." *American Economic Review*. 94(1):155-89.

Logan, J., Oakley, D., and J. Stowell. 2008. "School Segregation in Metropolitan Regions, 1970-2000: The Impacts of Policy Choices on Public Education." *American Journal of Sociology* 113(6) (May 2008): 1611-1644.

Lutz, Byron F. 2005. "Post Brown vs. the Board of Education: The Effects of the End of Court-Ordered Desegregation." Federal Reserve Board Finance & Economics Discussion Series Working Paper.

Manski, Charles F. 1993. "Identification of Endogenous Social Effects: The Reflection Problem". *The Review of Economic Studies*. 60(3): 531-542.

McCrary, Justin. 2007. "The Effect of Court-ordered Hiring Quotas on the Composition and Quality of Police." *American Economic Review* 97(1).

Murray, Sheila, William Evans, and Robert Schwab. 1998. "Education-Finance Reform and the Distribution of Education Resources" *American Economic Review* 88(4).

NAACP. 2004. *Remembering Brown 50 Years Later.* Available at: http://www.naacpldf.org/content/pdf/publs/Remembering_Brown/pdf.

Orfield, G. 1983. *Public School Desegregation in the United States: 1968-1980*. Washington, DC: Joint Center for Political Studies.

_______. 2000. “The 1964 Civil Rights Act and American Education.” In: Groffman, B. (Ed.), *Legacies of the 1964 Civil Rights.* University of Virginia Press, Charlottesville and London, pp. 89-128.

Reber, Sarah. 2007. “School Desegregation and Educational Attainment for Blacks.” Cambridge, MA” NBER Working Paper 13193.

_______. 2005. “Court-ordered Desegregation: Successes and Failures in Integration since Brown.” *Journal of Human Resources* 40(3): 559-590.

Regional Economic Information System (REIS), 1969-1989. Bureau of Economic Analysis, U.S. Department of Commerce, CIESIN (http://www.ciesin.org/datasets/reis/reis-home.html, accessed February 2009).

Rivkin, Steven. 1994. “Residential Segregation and School Integration.” *Sociology of Education* 67:279-292.

_______. 2000. “School Desegregation, Academic Attainment, and Earnings.” *Journal of Human Resources*, Spring 2000, 35(2):333-346.

Rivkin, Steven G. and Finis Welch. 2006. “Has school desegregation improved academic and economic outcomes for blacks?” In *Handbook of the Economics of Education, Volume 2*, Edited by Eric A. Hanushek and Finis Welch. Amsterdam: Elsevier. pp. 1020-1049.

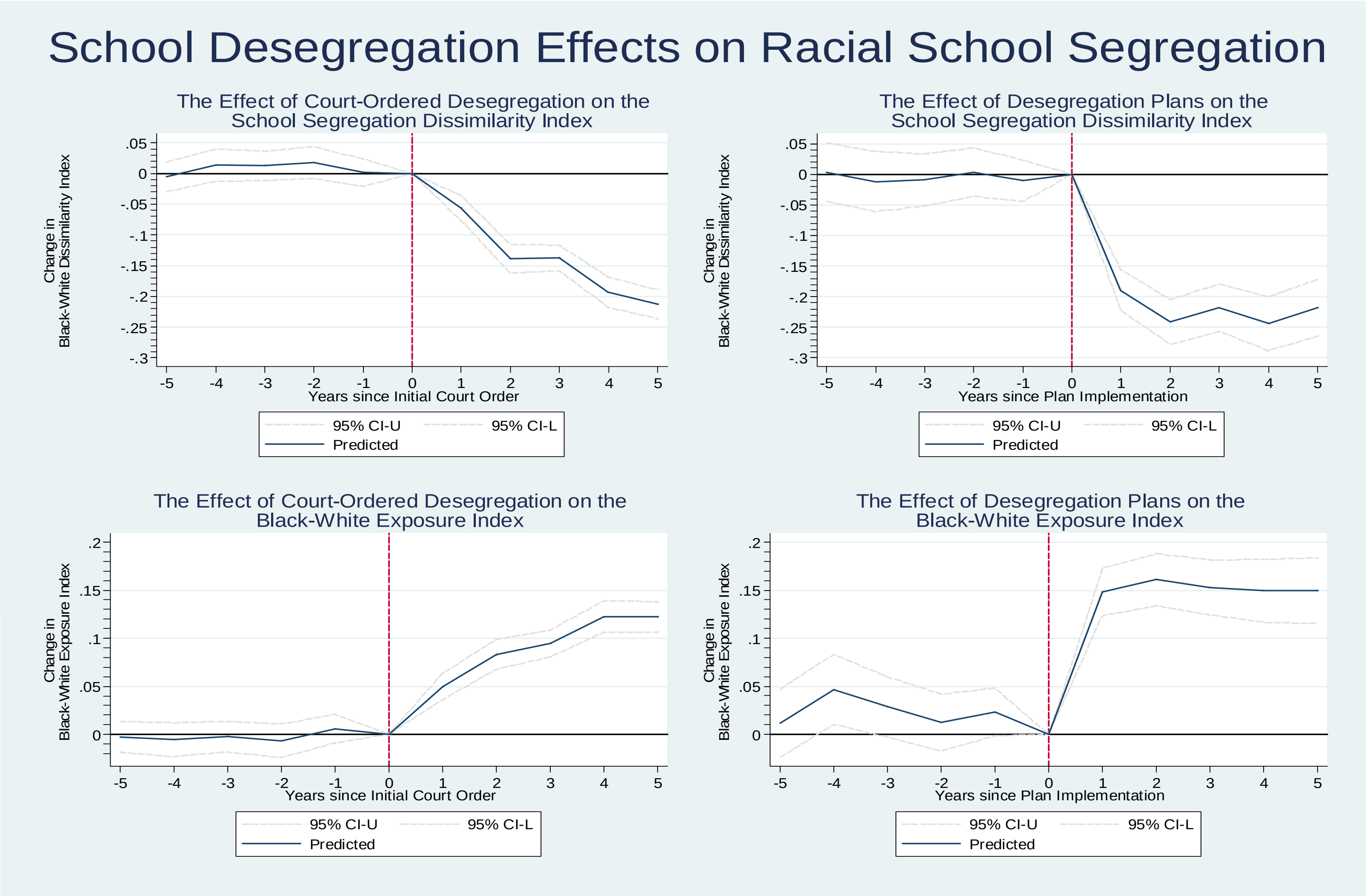
Weiner, D., Lutz B., Ludwig, J. 2010. The Effects of School Desgregation on Crime. Manuscript (June).

Welch, F., Light, A. 1987. New Evidence on School Desegregation. US Commission on Civil Rights, Washington, DC.

Wilson, Franklin. 1985. “The Impact of School Desegregation Programs on White Public-School Enrollment, 1968-1976.” *Sociology of Education* 58(3):13-153.

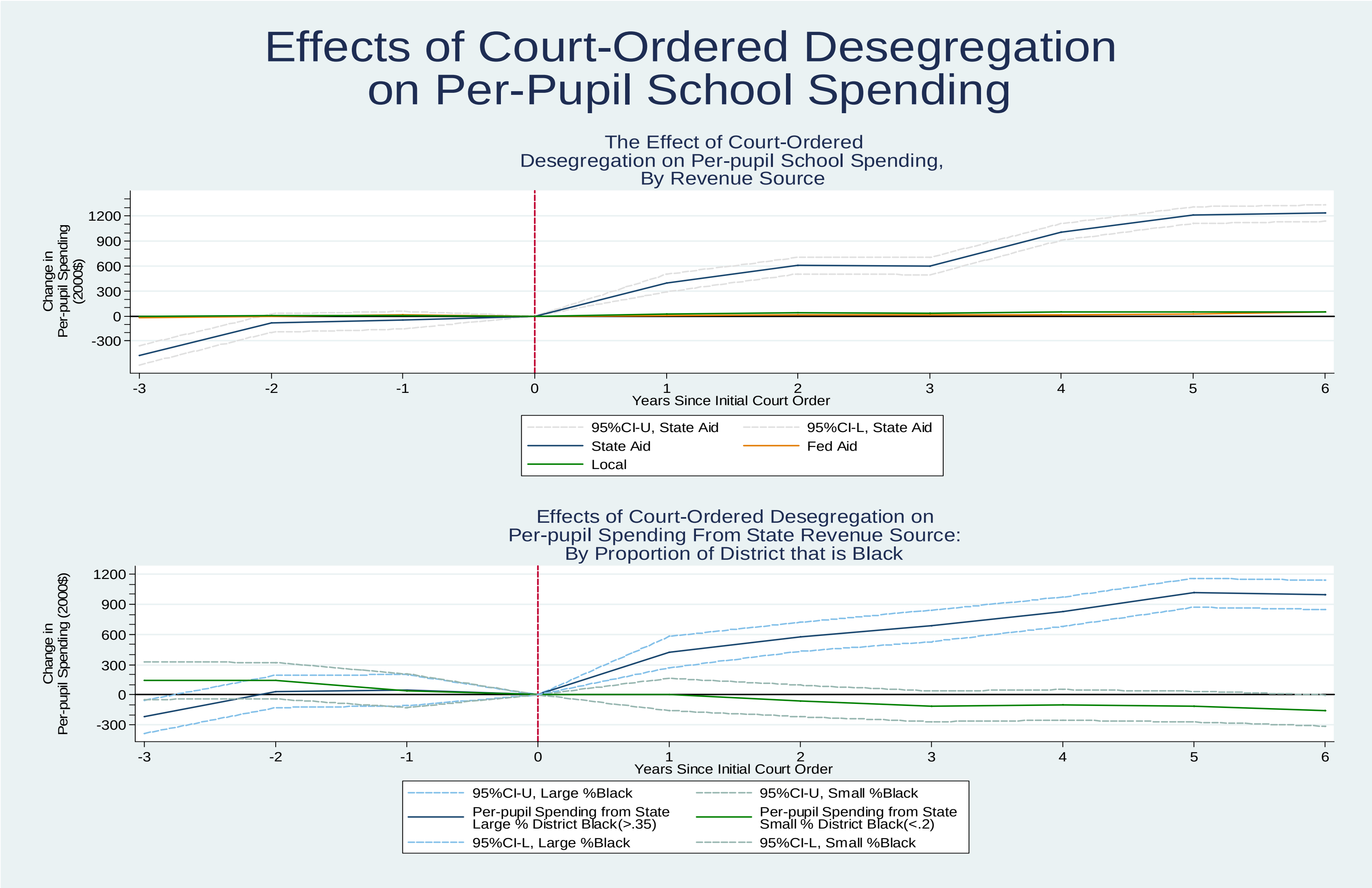
Van Doorslaer, Eddy and Andrew Jones. 2003. “Inequalities in Self-Reported Health: Validation of a New Approach to Measurement”. *Journal of Health Economics.* 22:61-87.

**Figure 1.**



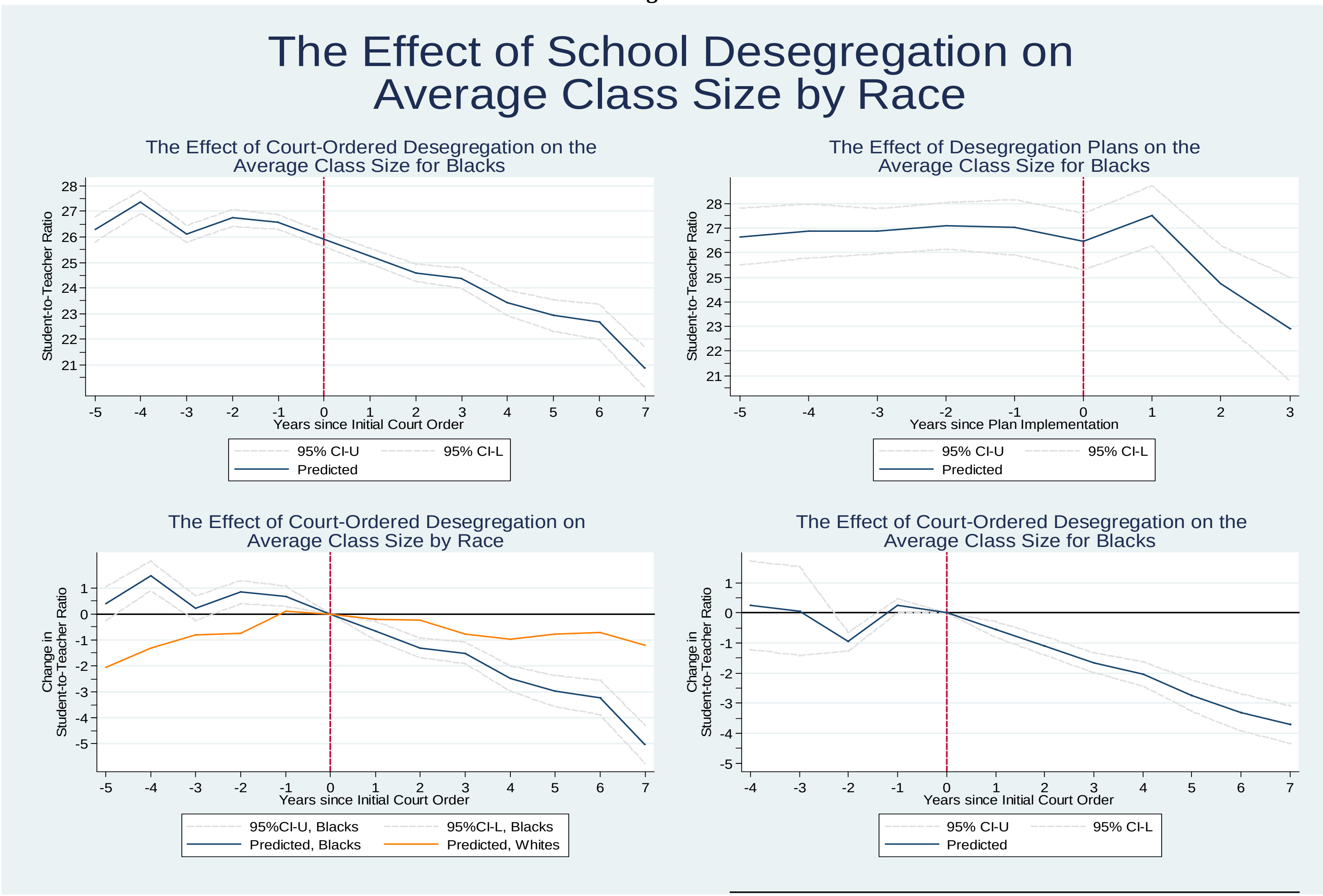

School Data: Office of Civil Rights,1968-82. Includes all districts under court order sometime b/w 1954-80 (N=655, American Communities Project data; N=99, Welch/Light data). Results based on regression w/school district FE, region*year FE, and controls for changes in gov't transfer programs. Avg black-white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

**Figure 2.**




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 3.**




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.

**Table 1. Effects of Court-Ordered School Desegregation on Educational Attainment, by Race**

| | Dependent variable: | | | | | | |
|---|---|---|---|---|---|---|---|
| | **Probability(Graduating from High School)** | | | | **Years of Education** | | |
| *Exposure to Court-Ordered Desegregation* (Main Effects apply to non-Hispanic Blacks for post-'64 court orders) | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **(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) |
| *Total Effect for Whites, spline:* | | | | | | | |
| <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.

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

| | Dependent variable: | | | | |
|---|---|---|---|---|---|
| | **Yrs of Exposure to Major Desegregation Plan$_{(age\ 5\text{-}17)}$** | **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$_{(age\ 5\text{-}17)}$ | | 0.0292*** | -0.0122 | 0.0800*** | -0.0402 |
| | | (0.0092) | (0.0100) | (0.0214) | (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 3. 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)** | | |
| *Exposure to Court-Ordered Desegregation* | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| (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 | 0.0025 | 0.0261 | -0.0043 | -2.3299 | 0.0326 | -0.0055 | -0.0026 | -0.0013 |
| | (0.0352) | (0.0351) | (0.0349) | (0.0237) | (15.9044) | (0.0604) | (0.0055) | (0.0056) | (0.0056) |
| **(0 to 12)** | **0.0575*** | **0.0598**** | **0.0509*** | **0.0285*** | **38.6930*** | **0.0953*** | **-0.0187***** | **-0.0174***** | **-0.0156***** |
| | **(0.0316)** | **(0.0260)** | **(0.0267)** | **(0.0148)** | **(20.2173)** | **(0.0561)** | **(0.0047)** | **(0.0048)** | **(0.0047)** |
| (0 to 12)*#of yrs before '65 for pre-'65 court orders | -- | -- | -- | -- | -17.0799*** | -0.0204* | 0.0046** | 0.0044** | 0.0041** |
| | | | | | (6.1410) | (0.0124) | (0.0019) | (0.0019) | (0.0019) |
| >12 (beyond school-age years of exposure) | -0.0241 | -0.0270 | -0.0263 | -0.0012 | -25.0001 | 0.0146 | -0.0112 | -0.0088 | -0.0101 |
| | (0.0455) | (0.0414) | (0.0404) | (0.0220) | (38.6976) | (0.0838) | (0.0102) | (0.0097) | (0.0093) |
| <0*White | -0.0053 | 0.0100 | 0.0063 | -0.0097 | 10.6585 | 0.0146 | -0.0038 | -0.0072 | -0.0069 |
| | (0.0295) | (0.0270) | (0.0283) | (0.0278) | (12.6100) | (0.0431) | (0.0048) | (0.0054) | (0.0054) |
| (0 to 12)*White | -0.0509** | -0.0454* | -0.0395+ | -0.0302* | -37.0326** | -0.0975** | 0.0100** | 0.0087* | 0.0073+ |
| | (0.0227) | (0.0273) | (0.0290) | (0.0175) | (17.9770) | (0.0423) | (0.0040) | (0.0047) | (0.0047) |
| (0 to 12)*(#of yrs before '65 for pre-'65 court orders)*White | -- | -- | -- | -- | 0.9360 | 0.0166*** | -0.0015** | -0.0013* | -0.0011+ |
| | | | | | (2.8892) | (0.0064) | (0.0007) | (0.0007) | (0.0007) |
| >12*White | 0.0790 | 0.0873 | 0.0579 | -0.0144 | 64.2180 | -0.0117 | 0.0014 | -0.0003 | 0.0003 |
| | (0.0619) | (0.0616) | (0.0618) | (0.0267) | (41.4063) | (0.1030) | (0.0101) | (0.0101) | (0.0096) |
| *Total Effect for Whites, spline:* | | | | | | | | | |
| <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.

**Table 4. 2SLS/IV Estimates of Effects of Desegregation Plans on Adult Economic Attainment Outcomes, by Race**

Second stage, Dependent variable:

| | **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** |
| Years of Exposure to Major Desegregation Plan$_{(age\ 5\text{-}17)}$ | 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 2.

**Table 5. 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$_{(age\ 5\text{-}17)}$ | | | | | -0.0071+ | |
| (main effect applies to non-Hispanic Blacks) | | | | | (0.0051) | |
| Years of Exposure to Desegregation Plan*White | | | | | 0.0097+ | |
| | | | | | (0.0076) | |
| *Effect for Whites,* Exposure Yrs to Desegregation Plan | | | | | 0.0026 | |
| ***Age when Desegregation Plan 1 st 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** | -0.0763** | -0.0071 | 0.0099 | | |
| | (0.0754) | (0.0299) | (0.0074) | (0.0066) | | |
| Middle School (dummy 0\|1, age 11-14) | -0.1174 | -0.0599* | -0.0073 | 0.0077 | | |
| | (0.0911) | (0.0352) | (0.0131) | (0.0091) | | |
| Elementary School (dummy 0\|1, age ≤10) | -0.2254** | -0.1465*** | -0.0378* | -0.0003 | | |
| | (0.1010) | (0.0410) | (0.0212) | (0.0103) | | |
| Linear trend prior to court order, | | | | | | |
| (18 - Age when 1st court order)*no exposure | 0.0015 | 0.0012 | -0.0007 | -0.0033** | | |
| | (0.0074) | (0.0051) | (0.0008) | (0.0017) | | |
| Linear trend for pre-school years, | | | | | | |
| Age ≤5 when initial court order occurred | -0.0106 | 0.0145 | -0.0060 | 0.0019 | | |
| | (0.0182) | (0.0161) | (0.0070) | (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 2. 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 6. Effects of Court-Ordered School Desegregation on Adult Health Status, by Race**

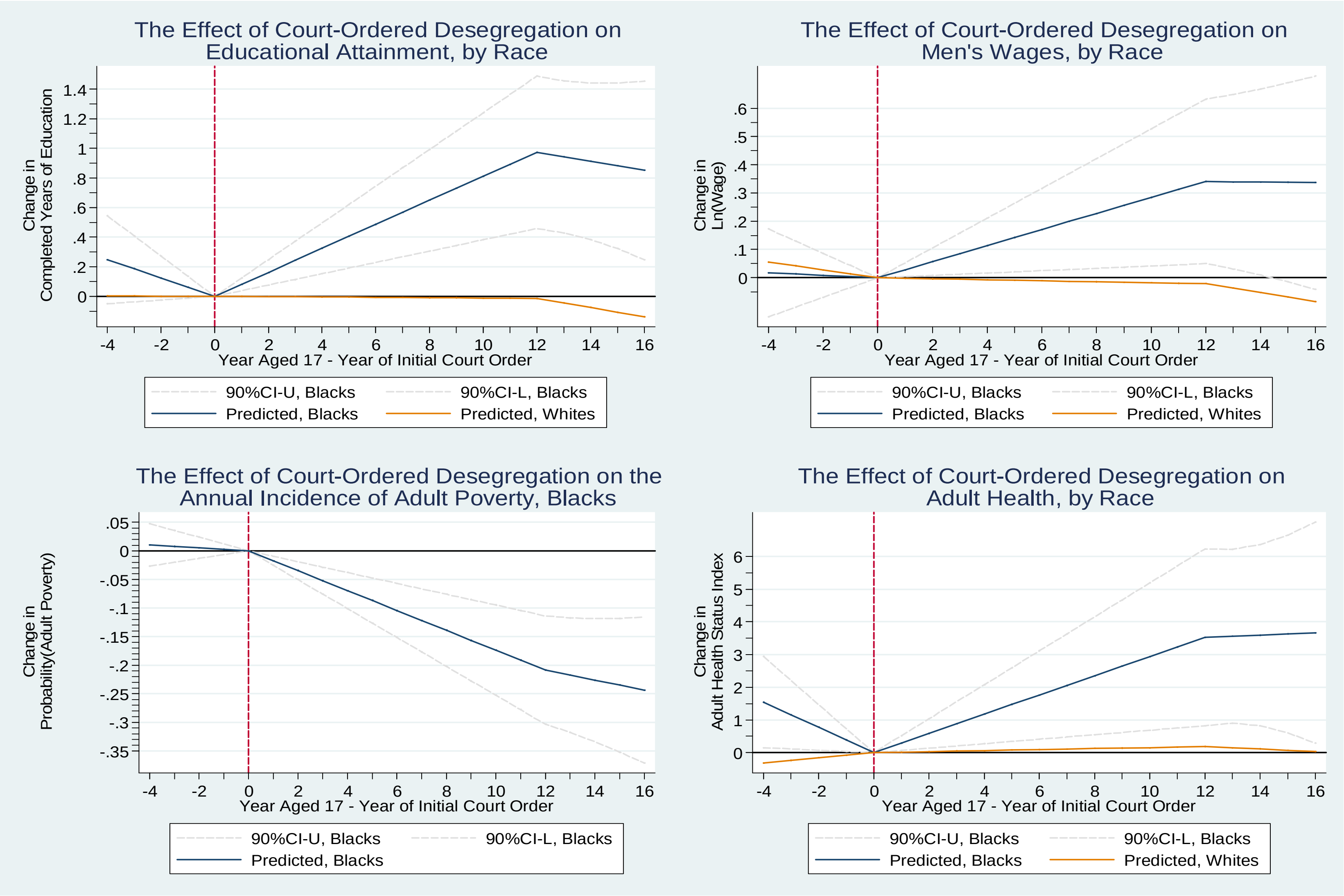
| | Dependent variable: Adult Health Status Index, ages 25-45 (Based on Interval Regression Model: 100pt-scale, 100=perfect health) | | | | |
|---|---|---|---|---|---|
| | Interval Regression Estimates | | | 2SLS/IV Estimates | |
| | All | | | Blacks | Whites |
| | (1) | (2) | (3) | (4) | (5) |
| Years of Exposure to Major Desegregation Plan$_{(age\ 5\text{-}17)}$ | | | | 0.5222*** | -0.1787 |
| | | | | (0.1944) | (0.2697) |
| (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* | 0.1022 | 0.2039 | | |
| | (0.2120) | (0.3813) | (0.3975) | | |
| **(0 to 12)** | **0.2937**** | **0.5978*** | **0.5121+** | | |
| | **(0.1369)** | **(0.3633)** | **(0.3775)** | | |
| (0 to 12)*#of yrs before '65 for pre-'65 court orders | 0.0205 | -0.1676* | -0.1301+ | | |
| | (0.0361) | (0.0968) | (0.0990) | | |
| >12 (beyond school-age years of exposure) | 0.0348 | 0.2032 | 0.0734 | | |
| | (0.3949) | (0.4643) | (0.4821) | | |
| <0*White | 0.4663* | 0.4567* | 0.3933+ | | |
| | (0.2428) | (0.2604) | (0.2624) | | |
| (0 to 12)*White | -0.2780* | -0.4734** | -0.4613** | | |
| | (0.1510) | (0.1911) | (0.1933) | | |
| (0 to 12)*(#of yrs before '65 for pre-'65 court orders)*White | 0.0112 | 0.0073 | 0.0040 | | |
| | (0.0433) | (0.0351) | (0.0354) | | |
| >12*White | -0.0726 | -0.0640 | -0.0357 | | |
| | (0.4874) | (0.3613) | (0.3718) | | |
| *Total Effect for Whites, spline:* | | | | | |
| <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 |
| Childhood county fixed effects? | no | yes | yes | yes | yes |
| Controls for Δchild county per-capita govt transfer programs? | no | no | yes | no | no |
| Number of person-year adult observations | 52,737 | 52,737 | 52,737 | 9,802 | 6,274 |
| Number of individuals | 5,494 | 5,494 | 5,494 | 1,104 | 655 |
| Number of childhood families | 2,069 | 2,069 | 2,069 | 366 | 267 |
| Number of childhood neighborhoods | 1,472 | 1,472 | 1,472 | 292 | 227 |
| Number of school districts | 330 | 330 | 330 | 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 2.

**Figure 4. 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 1-4, 6).

**Table 7.**
**Long-run Effects of School Desegregation & School Quality on Educational Attainment & Adult Health: Sibling Fixed Effect Estimates**

| | Dependent variable: | | | | | |
|---|---|---|---|---|---|---|
| | **Years of Education** | | | **general health status in adulthood** Interval Regression Model: 100pt-scale, 100=perfect health | | |
| | **Black** | **White** | **All** | **Black** | **White** | **All** |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Years of Exposure to Court-Ordered Desegregation$_{(\text{age } 5\text{-}17)}$ | 0.1294* | 0.0061 | | 0.6417** | -0.1506 | |
| | (0.0729) | (0.0950) | | (0.2941) | (0.4022) | |
| Ln(School district per-pupil spending)$_{(\text{age } 12\text{-}17)}$ | | | 0.9635** | | | 4.5134* |
| | | | (0.3913) | | | (2.7367) |
| Age - 30 | | | | -0.2850*** | -0.2952*** | -0.2704*** |
| | | | | (0.0648) | (0.0414) | (0.0299) |
| Constant | 12.5932*** | 13.5622*** | 13.4177*** | 83.4535*** | 88.8694*** | 85.9419*** |
| | (1.1435) | (1.1904) | (0.7728) | (3.7065) | (6.0959) | (7.5074) |
| Sibling Fixed Effects? | yes | yes | yes | yes | yes | yes |
| Race-specific year of birth effects? | yes | yes | yes | yes | yes | yes |

Robust Standard errors in parentheses (clustered on school)

*** 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 supressed to conserve space).

**Table 8. Interactive Effects of Court-Ordered School Desegregation & Induced-Change in Per-Pupil Spending on Black's Adult Socioeconomic & Health Attainments**

| | Dependent variable: | | | | | |
|---|---|---|---|---|---|---|
| | **Years of Education** | | | **Adult Family Income-to-Needs Ratio** | **Probability (Adult Poverty)** | **Adult Health Status Index** |
| *Exposure to Court-Ordered Desegregation* | (1) | (2) | (3) | (4) | (5) | (6) |
| Yrs of Exposure to Court-Ordered Desegregation$_{(age\ 5\text{-}17)}$ | 0.2510*** | 0.1949** | 0.2596*** | 0.0761** | -0.0173*** | 0.5322+ |
| | (0.0587) | (0.0812) | (0.0803) | (0.0329) | (0.0059) | (0.3272) |
| Yrs of Exposure to Court-Ordered Desegregation* ↑ΔPer-Pupil Spending$_{(t-1,t+3)}$ | 0.0892*** | | 0.0764** | 0.0515** | -0.0057+ | 0.3763* |
| | (0.0311) | | (0.0358) | (0.0215) | (0.0038) | (0.2034) |
| Yrs of Exposure to Court-Ordered Desegregation* ↑ΔBlack-White Exposure Index$_{(t-1,t+3)}$ | | 0.0106 | 0.0093 | | | |
| | | (0.0110) | (0.0116) | | | |
| Sample from districts initially subject to court orders ≥1964? | yes | yes | yes | yes | yes | yes |
| Year of birth fixed effects? | yes | yes | yes | yes | yes | yes |
| Childhood county fixed effects? | yes | yes | yes | yes | yes | yes |
| Number of adult person-year observations | -- | -- | -- | 23,770 | 23,770 | 9,847 |
| Number of Individuals | 1,193 | 915 | 915 | 1,493 | 1,493 | 991 |
| Number of Childhood Families | 342 | 270 | 270 | 402 | 402 | 293 |
| Number of Childhood Neighborhoods | 256 | 217 | 217 | 300 | 300 | 218 |
| Number of School Districts | 51 | 43 | 43 | 51 | 51 | 50 |

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 1-3). Models include same set of control variables as in Tables 1&3.

**Table 9. Effects of Desegregation Exposure on Blacks' Adult Outcomes & the Returns to Education**

| | Dependent variable: | | | | | | |
|---|---|---|---|---|---|---|---|
| | **Years of Education** | **Ln(Wage), Men ages 25-45** | **Men's Annual Work Hours** | **Adult Family Income-to-Needs Ratio** | **Probability (Adult Poverty)** | **Probability (Incarceration), Men age 20-34** | **Adult Health Status Index, ages 25-45** |
| 5-Year Exposure to Desegregation | 0.4000*** | 0.1095*** | 195.1860*** | 0.1995*** | -0.1100*** | -0.0355** | 2.6110*** |
| Implied Wald Estimate of Returns to Education (quantity/quality) | -- | 0.2738 | 487.9650 | 0.4988 | 0.2750 | 0.0888 | 6.5275 |
| Mean for Blacks (at age 30) | 12.8 | 2.2478 | 1,645.8 | 2.2294 | 0.2977 | 0.0137<br>0.212 (ever) | 83.8612 |

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

This sumary table contains the main results for blacks based on estimates shown in Tables 1-6. 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; Welch/Light).

## 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 where then calculated based on the school level enrollment by race. The OCR surveys were not comprehensive in all years, but the large size of school districts and the heavy representation of districts that had involvement of the courts in desegregating its schools ensured that most districts with significant minority student enrollment were included in the data in most years. Before the 1967 school year, no school-level data on enrollment by race are available.

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 Schazenbach for sharing the Regional Economic Information System (REIS) data for the 1959 to 1978 period. Per capita county transfer payments include measures for public assistance (AFDC, General Assistance), 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: A BRIEF HISTORY OF US SCHOOL DESEGREGATION

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

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

Most school districts did not adopt major school desegregation plans until forced to do so by court order (or threat of litigation) due to individual cases filed in local Federal court. Civil rights organizations avoided taking on legal cases early on that had a high risk of failure, even if the potential local benefits were large. The cascading impacts that would accompany legal victory due to the role of precedent juxtaposed with the potential risks of losing outweighed considerations of where targeted efforts would have the greatest impacts or where impacts would be felt for the largest number of blacks in the short-run. As the recorded legal history of desegregation documents, the legal arm of the NAACP (Legal Defense & Educational Fund)…"followed a strategic approach that rejected simple accumulation of big cases, in favor of incremental victories that built a favorable legal climate…" (*Council for Public Interest Law*, 1976, p.37).[i] Guryan (2004) presents this intuition formally in a model that demonstrates that in an environment in which precedent has a strong effect on the subsequent probability of success, an agent with the objective of desegregating the nation's schools should optimally choose to prioritize the likelihood of success almost to the exclusion of any local benefits of desegregation when choosing where to bring litigation.

*Timeline of School Integration in the US*

At the time of the Brown decision in 1954, seventeen southern states and the nation's capitol required that all public schools be racially segregated (Figure A1). The Supreme Court did not set a time table for dismantling school segregation and turned the implementation of desegregation over to US district courts. The aftermath of Brown and process to see desegregation established in public schools can be characterized as consisting of several developmental periods—from neonatal and infancy (1954-65) to adolescence (1966-75) and young adulthood (1976-1989). The post-*Brown* era up through the mid-to late 1980s can be codified by two distinct periods: pre- and post-1965. The 1954-65 period was characterized by Southern states' intent to thwart implementation of Brown and resist compliance with the desegregation orders. The South's massive resistance to the Court's rulings ensued for the next 10 years and the delay tactics were initially very successful. The case-by-case litigation approach largely failed during the first decade following Brown. Legal scholar Walter Gellhorn described the pace of desegregation during these years as that "of an extraordinarily arthritic snail" (cited in Wilkinson, From Brown to Bakke, p. 102). By 1965, only 2 percent of African American children in the Deep South attended integrated schools and more than 75 percent of the schools in the South remained segregated.

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

Enforcement of desegregation did not begin in earnest until the mid-1960s. State and federal dollars proved to be the most effective incentives to desegregate the schools. A critical turning point was the enactment of Title VI of the 1964 Civil Rights Act (CRA) and Title I funds of the 1965 Elementary & Secondary Education Act (ESEA), which prohibited federal aid to segregated schools and allowed the Justice Department to join suits against school districts that were in violation of the Brown vs. Board order to integrate. The congressional enactment of ESEA was among the most important events in effecting compliance because it dramatically raised the amount of federal aid to education; from a few million to more than one billion dollars a year; and, for the first time, the threat of withholding federal funds became a powerful inducement to comply with federal desegregation orders (Cascio et al., 2010; Holland, 2004).

Figure A6 presents a map of the geographic variation in school spending in the US in 1962 overlaid with the residential locations of minorities in that year. The map illustrates the concentration of minorities in the South where school district per-pupil spending levels were lowest. Another example of how financial incentives played a role in facilitating compliance is evident in President Nixon’s proposal to provide financial incentives to school districts to comply with desegregation orders, which led to congressional enactment of the Emergency School Aid Act of 1972 to assist the federal courts in achieving desegregation (Ehrlander, 2002, p. 23). Federal dollars soon constituted 30 percent of the budget of many Southern school systems. The availability of federal money continued to influence desegregation into the 1980s. I find a significant correlation in the amount of federal funds received by school districts in the years 1966-1970 with the percentage of black students enrolled in previously all-white schools.

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

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

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

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

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

*Nature of Pressure to Desegregate (pre- vs. post-1965 period).* In many districts, desegregation was a process that came as a result of pressures from many sources. As the major impetus, court orders were most often reported in districts with high initial levels of segregation and with moderate-to-high proportions of minority students. Districts which desegregated under local pressures generally had low initial levels of segregation and low proportions of minority students. Figure A2 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 A2, the change in the pace of desegregation litigation activity and plan implementation after 1965 is striking. Many districts took steps overnight that changed the school systems from being predominantly segregated to predominantly desegregated. These steps were often taken subsequent to a specific court order or following direct threat from the US Department of Health, Education, and Welfare (HEW) to cut off Federal funds. The nature of timing of initial court litigation was highly idiosyncratic. Court-ordered desegregation by legal mandate is plausibly more exogenous than other more voluntary forms of desegregation. The extent of voluntary desegregation prior to court intervention varied across districts, but voluntary action of districts was more endogenous. As well, anti-integration groups can delay major desegregation plan implementation by lengthening the court proceedings or by implementing inadequate desegregation plans; thus, the timing of initial court orders is likely more plausibly exogenous than the actual implementation date of major desegregation plans (additional evidence provided near the end of this Appendix).

In Figure A3, 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 A3). Namely, the results suggests that for initial court orders meted out after 1965, there is roughly immediate implementation (on average, major plan implemented within 1-2 yrs of initial court order); and the lag does not differ over time for court orders after 1965. On the other hand, for initial court orders meted out before 1965, there is more than a 10-year delay in implementation of a major plan (following initial court order, major plan is not implemented, on average, for 10 years; there is a systematic long delay that decreases in years leading up to 1965. During the 1955-64 period (after Brown but prior to the passage of the Civil Rights Act), the earlier the initial court order, the longer the delay in implementation of a major plan. This pattern and discontinuity after 1965 in the time lag between initial court order and major desegregation plan implementation occurs in the South and non-South.

In 1964, 1 percent of African American students in the South attended school with whites; by 1968, this had risen to 32 percent. As shown in Figure A2, the ensuing years of 1968-1972 bracket the period of maximum desegregation activity. Figure A4 presents a map that summarizes the overall geographic pattern and timing of initial court orders overlaid with the childhood residential locations of the (nationally-representative) PSID sample of black and white children in 1968; and, analogously, Figure A5 shows this for the resultant subsequent major desegregation plan implementation in US school districts/counties[ii] (among the subset of districts for which this information is available). The figures demonstrate the strong overlap of residential locations of original sample PSID children with districts that underwent court-ordered desegregation.

In the figure, districts that were subject to court orders are shaded (no shading indicates no court-ordered desegregation); the shading of the districts/counties is assigned by its initial court order date, with darker shading denoting a later initial court ruling. The lightest gray represents communities in which the initial court order occurred between 1954 and 1963—the early desegregation period; and the next darkest gray shades denotes communities in which the initial court order occurred between 1964-1968 during the expansion of federal enforcement as a "national emphasis program" and under Title VI of the 1964 CRA and Title I of the 1965 ESEA; the next darkest grays indicate communities in which the initial court order occurred between 1968 and 1972 during the expansion following the 1968 Green Supreme Court ruling; the darkest gray and black represent the corresponding smaller number of communities in which the initial court order occurred between 1974 to 1980 and after 1980, respectively. Not surprisingly, the concentration of activity occurred in places with at least a 20 percent black population. A substantial portion of the US population of minority children in 1960 lived in the shaded 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. Figure A7 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.

Only token desegregation efforts occurred prior to the passage of the 1964 Civil Rights Act. The figure shows that litigation and desegregation plan implementation accelerated substantially between 1964 and 1972. For example, only 6 percent of the districts that would eventually undergo court-ordered desegregation had implemented major plans by 1968 (when the PSID began); by 1972 this rose to over 56 percent. It is this period of substantial growth in litigation activity, spurred by landmark court cases like the 1968 Green decision, that forms the basis of the research design. By 1976, 45 percent of the South's African American students were attending majority-white schools, compared with just 28 percent in the Northeast and 30 percent in the Midwest.

The process became highly decentralized with a diverse set of agents that initiated court litigation following the Brown decision, which also contributed to the idiosyncratic nature of the timing and location where legal challenges arose that resulted in initial court orders.[iii] Differences across districts in when desegregation court cases were first filed and the length of time it took these cases to proceed through the judicial system represents a plausibly exogenous source of identifying variation in the timing of school desegregation. The exogeneity of this timing is supported theoretically by the documented legal history of school desegregation and by my own empirical examination of the issue below.

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

The 1962 county measures used as independent variables in the model include: the log(county population), percent of the population that is minority, per-capita school spending, the percent of school spending that comes from intergovernmental grants (state/federal), median income, percent of households with income <$3,000 (in 1961 dollars), percent of households with income >$10,000, percent with 12 or more years of education, population change between 1950-60, percent of residents in an urban area, percent of residents in rural or farm area, percent of residents living in group quarters, median age, percent of residents that are school-age, percent of residents 65 or older, percent of residents that voted for the incumbent President, and the county mortality rate (all constructed from the 1962 Census of Governments, City & County Data Book). I include the size of the population to capture the fact that large districts/counties may face differential costs and opposition to the desegregation process. I also estimate an alternative model specification that includes the 1962 average student-to-teacher ratio and average teacher salary, instead of the per-capita school spending level (as shown in Table A1, similar patterns emerge). These data are linked with the desegregation court case and plan implementation data.

Columns (1)-(6) of Table A1 presents estimates from least-squares regressions of the year each school district had an initial court order (among those that first became subject to court order after 1962) on 1962 characteristics and region fixed effects, while the final two columns ((7)-(8)) use the same set of independent variables to examine determinants of the delay between the initial court order and major desegregation plan implementation (in years). Column (1) shows estimates for the full sample, column (3)-(8) show results for the subset of counties in which original sample PSID children grew up, and columns (5)-(8) display results for the subsample of counties for which information is available on the dates of major desegregation plan implementation.

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

On the other hand, I find that districts with a larger minority population, greater per-capita school spending, and smaller proportion of residents with low income are each strongly associated with longer delays in major desegregation implementation following the initial court order. These results are consistent with the legal history of school desegregation, and suggest that the timing of initial court litigation is more plausibly exogenous than the timing of major desegregation plan implementation. In sum, the idiosyncratic nature of court litigation timing documented in the legal history of school desegregation make a prima facie case for treating initial court orders as exogenous shocks, which influenced the timing of major desegregation plan implementation and generated changes in school quality from abrupt shifts in racial school segregation. This case is bolstered by the empirical evidence that the bulk of 1962 district/county characteristics fail to predict the timing of initial court orders.

---

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

**Figure A1.**

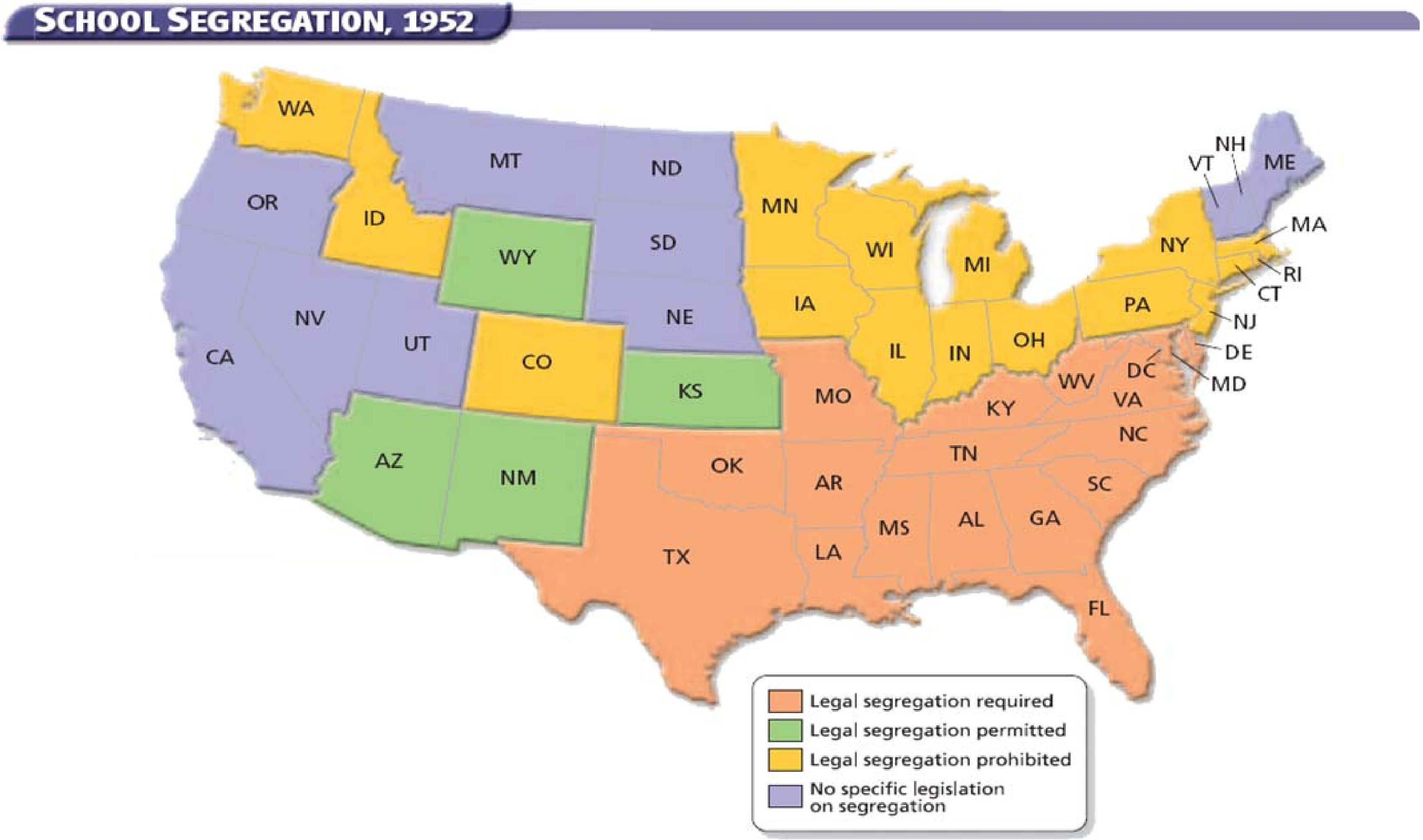
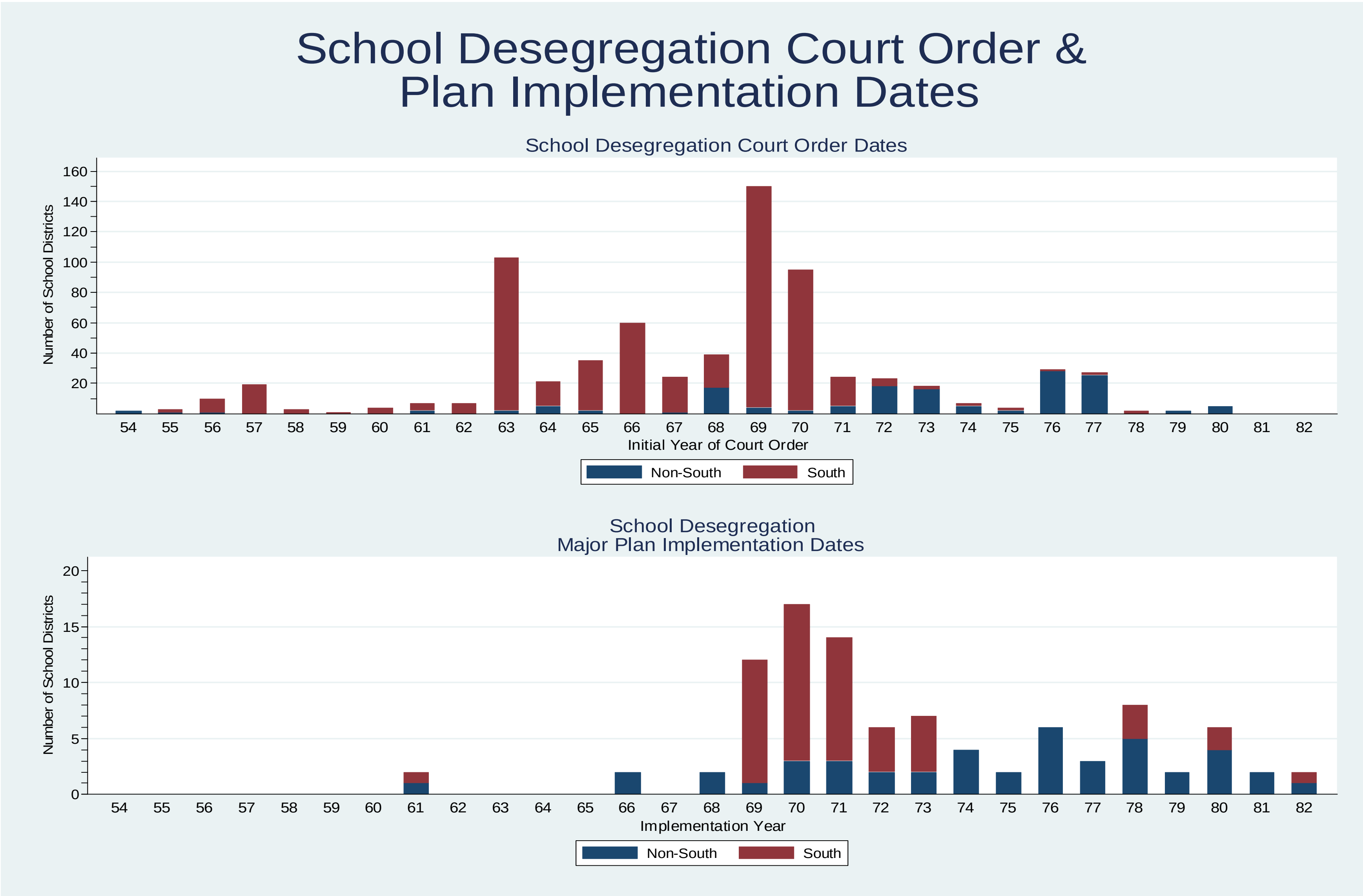

SCHOOL SEGREGATION, 1952
WA
MT
ND
MN
OR
ID
WY
SD
WI
MI
NV
UT
CO
NE
IA
IL
IN
OH
PA
NY
VT
NH
ME
MA
RI
CT
NJ
DE
MD
DC
CA
KS
MO
KY
WV
VA
NC
TN
AZ
NM
OK
AR
SC
MS
AL
GA
TX
LA
FL
Legal segregation required
Legal segregation permitted
Legal segregation prohibited
No specific legislation on segregation

**Figure A2.**




(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 A3.**

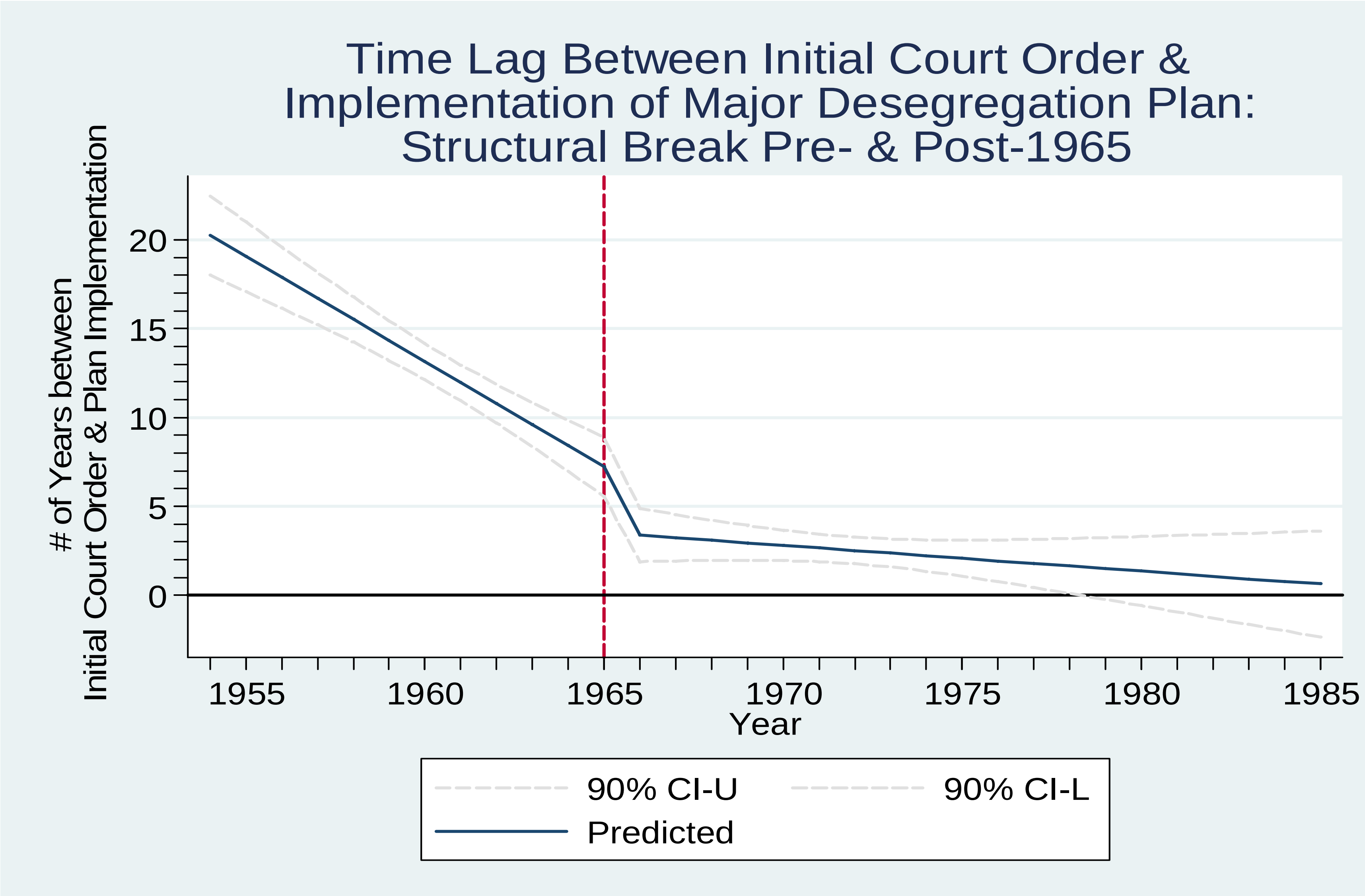

Time Lag Between Initial Court Order &
Implementation of Major Desegregation Plan:
Structural Break Pre- & Post-1965
# of Years between
Initial Court Order & Plan Implementation
0
5
10
15
20
1955
1960
1965
1970
1975
1980
1985
Year
90% CI-U
90% CI-L
Predicted

**Figure A4.**

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

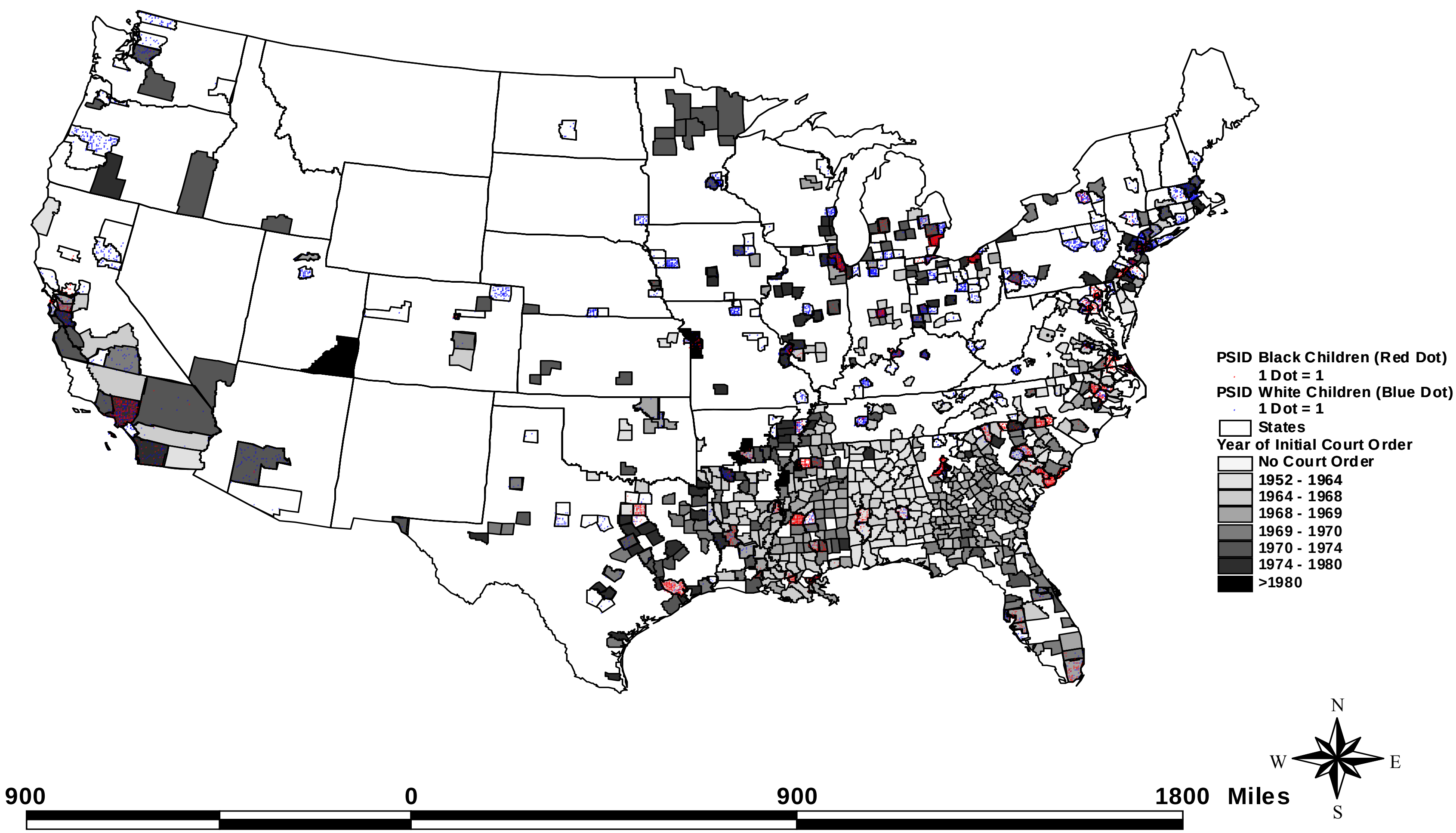


**Figure A5.**

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

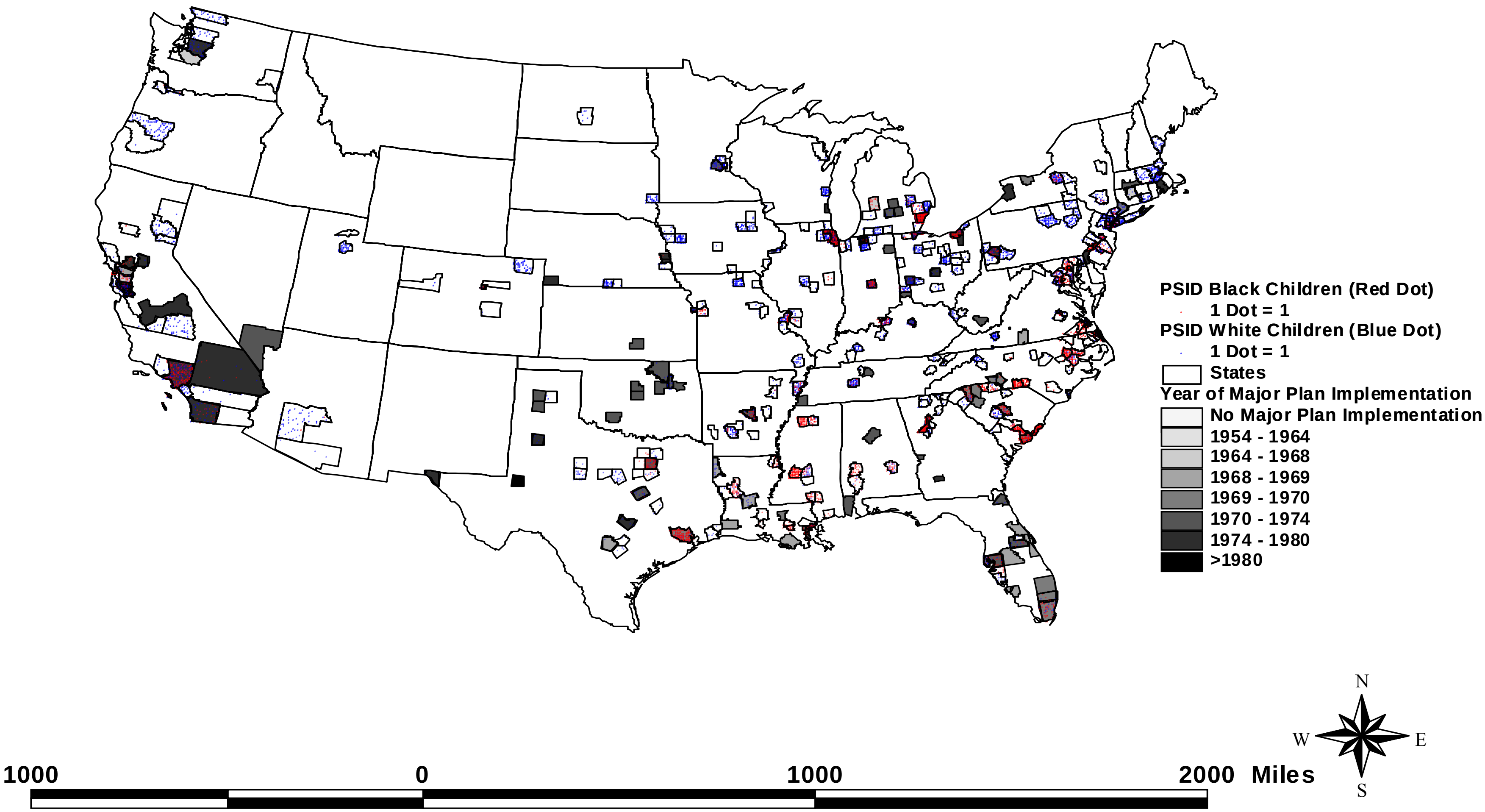


**Table A1: Determinants of the Timing of Court-Ordered School Desegregation Using 1962 County Characteristics**

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

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

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

**Figure A6.**

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

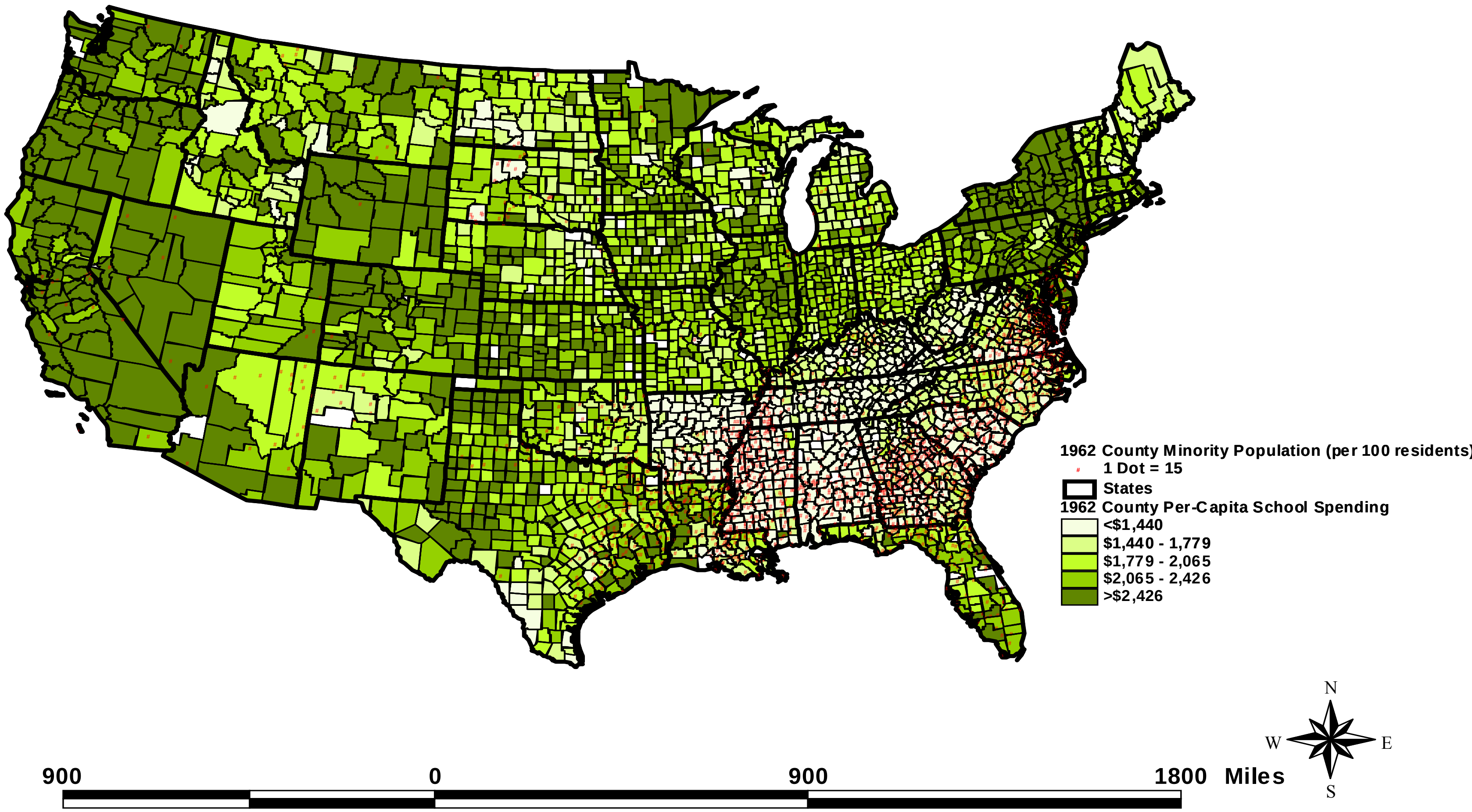


**Figure A7.**

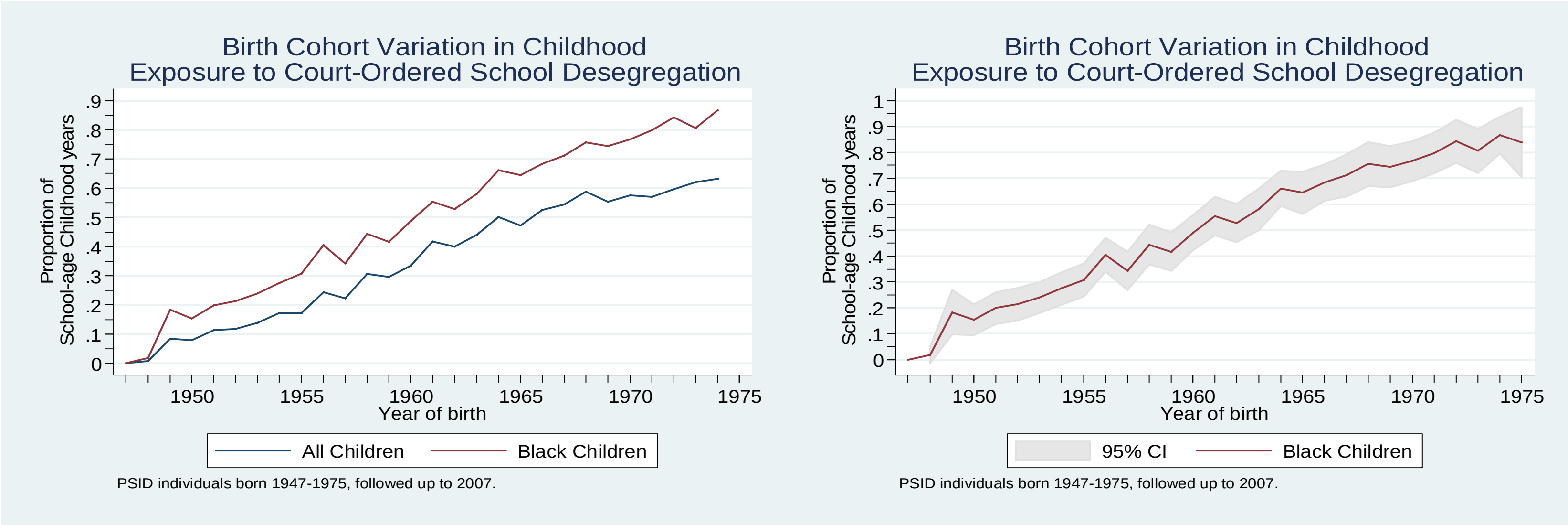

Birth Cohort Variation in Childhood
Exposure to Court-Ordered School Desegregation
Proportion of
School-age Childhood years
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1950
1955
1960
1965
1970
1975
Year of birth
All Children
Black Children
PSID individuals born 1947-1975, followed up to 2007.
Birth Cohort Variation in Childhood
Exposure to Court-Ordered School Desegregation
Proportion of
School-age Childhood years
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
1950
1955
1960
1965
1970
1975
Year of birth
95% CI
Black Children
PSID individuals born 1947-1975, followed up to 2007.

**Table A2. 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$_{(age\ 5\text{-}17)}$ | 0.0131 | -0.0035 | -0.0068 | 0.0046 | 0.0240 |
| | (0.0273) | (0.0114) | (0.0180) | (0.0039) | (0.1267) |
| Years of Exposure to Unsuccessful Desegregation Court Litigation*White | 0.0107 | 0.0051 | 0.0315 | -0.0059 | -0.0086 |
| | (0.0408) | (0.0126) | (0.0335) | (0.0040) | (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 1-6).