ONLINE APPENDIX

LONG-RUN IMPACTS OF SCHOOL DESEGREGATION ON ADULT ATTAINMENTS[*]

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Appendix A: Data Sources

A. *Desegregation Court Case Data*

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

Structure of Data & Information Compiled for each Court Case:

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

B. *Desegregation Plan Implementation Data*

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

C. *School Data*

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

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

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

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

Per-Pupil Spending Data

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

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

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

annually with data available until 2010. I combine these data sources to construct a long panel of annual per-pupil spending for each school district in the United States between 1967 and 2010.

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

D. *Sources of Data on Segregation*

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

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

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

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

Hospital Desegregation Data

Hospital Desegregation. The desegregation of hospitals in the South can be initially dated from 1964 when federally-mandated policies began to be enforced. In particular, developments in all three branches of government—judicial, executive, legislative—were influential. First, Hill-Burton Act's 'separate but equal' clause was ruled unconstitutional in 1963. Second, Title VI of the Civil Rights Act of 1964 put teeth in enforcement. Third, with the introduction of Medicare in 1965, a hospital had to be racially desegregated in order to be eligible to receive Medicare funding. The staggered timing of

hospital desegregation in the South led to differences in the timing of improved access to hospital care for minorities, and resulted in timing differences in the implementation of Medicare in parts of the South that had not desegregated their hospitals prior to 1965.

Using the American Hospital Association's Annual Survey of Hospitals (spanning the period 1946-1980) along with the Centers for Medicare Provider of Service data files dating back to the early 1960s to identify the precise date in which a Medicare-certified hospital was established in each county of the US (an accurate marker for hospital desegregation compliance), I find that ¼ of counties in the South—and 75 percent of counties in the Mississippi Delta—lacked a Medicare-certified hospital by the end of 1966. Almond, Chay, & Greenstone (2008) and Finkelstein and McKnight (2008) have independently used this type of data previously to measure the timing of hospital desegregation. I also construct measures of the individual's age at which hospital desegregation occurred and a race-specific distance to the nearest hospital as an index of segregation and access during childhood (created using GIS mapping technologies and historical hospital address and childhood residential location information).

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

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

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

F. *Pre-Existing County Characteristics*

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

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

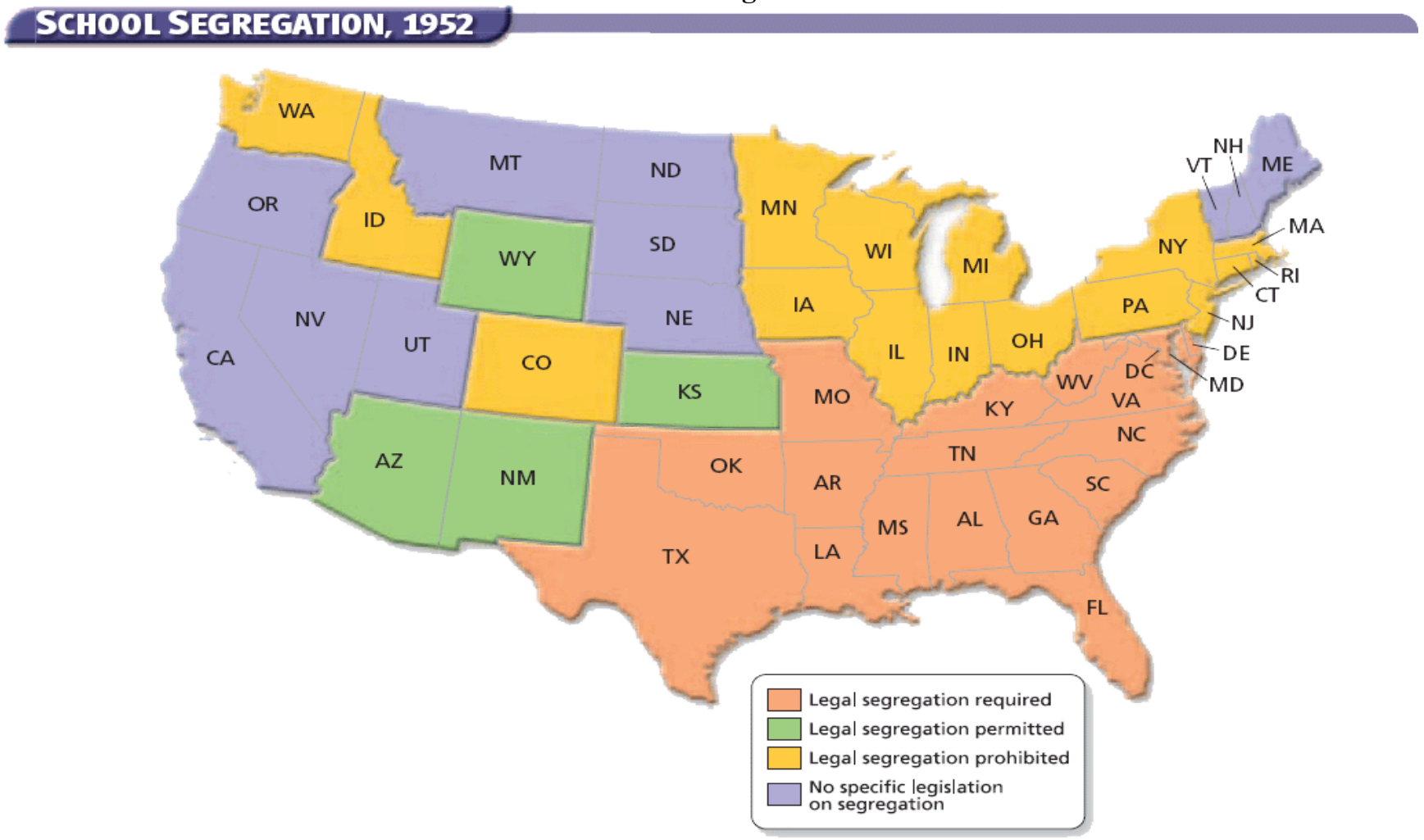
In order to limit the possibility that school district boundaries were drawn in response to school desegregation, I utilize 1969 school district geographies. The "69-70 School District Geographic Reference File" (Bureau of Census, 1970) relates census tract and school district geographies. For each census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

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

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

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

Figure A0.


SCHOOL SEGREGATION, 1952
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AL
OH
WV
GA
FL
SC
NC
VA
PA
NY
DC
MD
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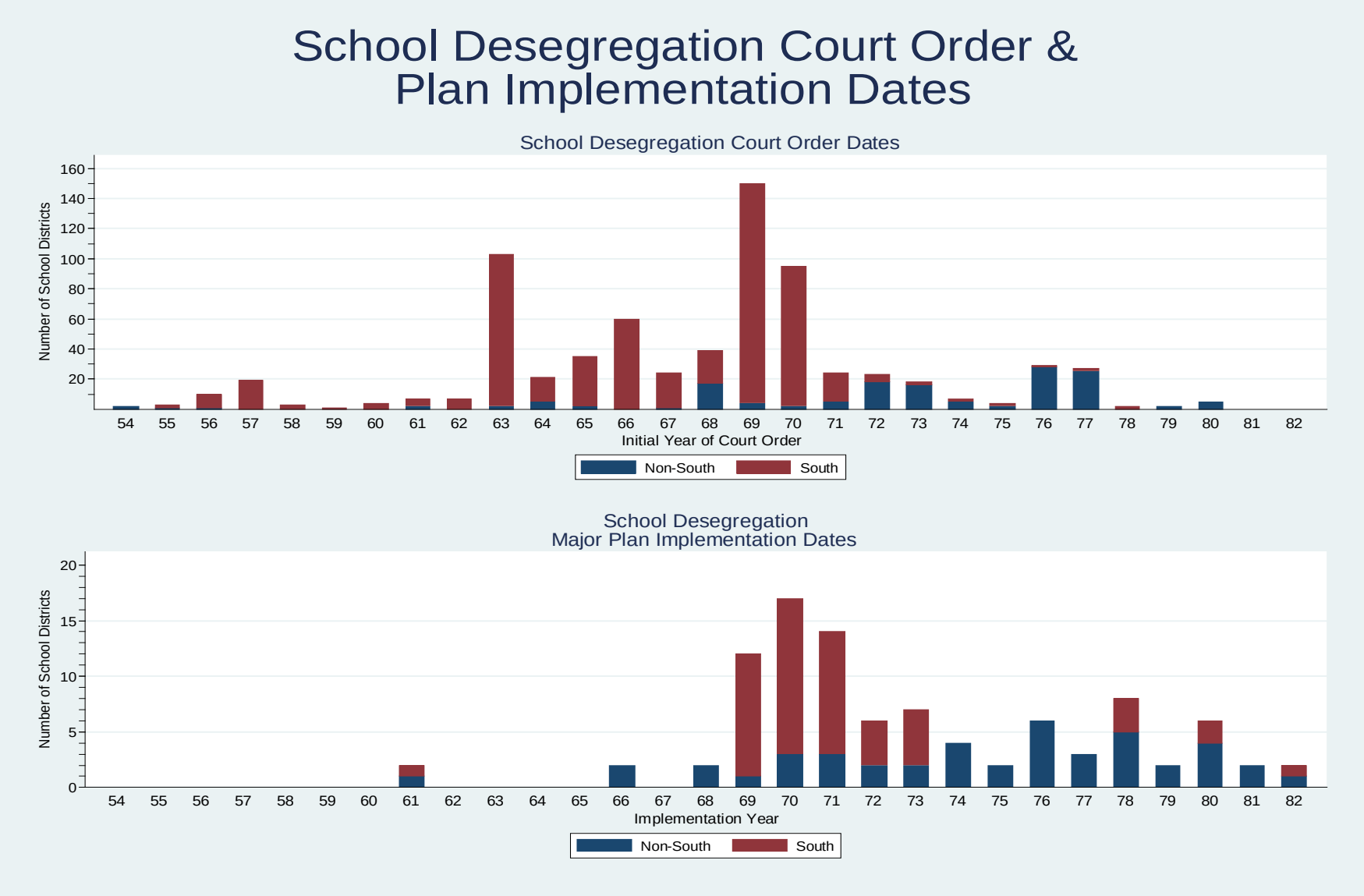
NJ
CT
RI
MA
VT
NH
ME
Legal segregation required
Legal segregation permitted
Legal segregation prohibited
No specific legislation on segregation

Figure A1.



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

FIGURE A2. GEOGRAPHIC TIMING OF COURT-ORDERED SCHOOL DESEGREGATION

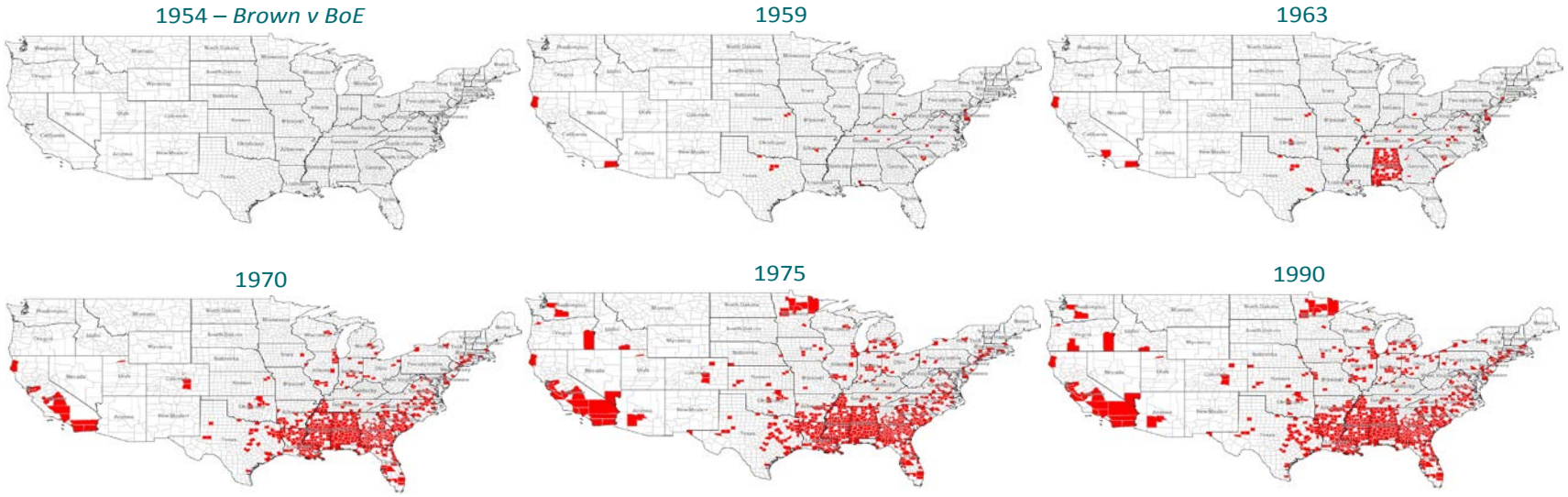


Figure A2b.

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

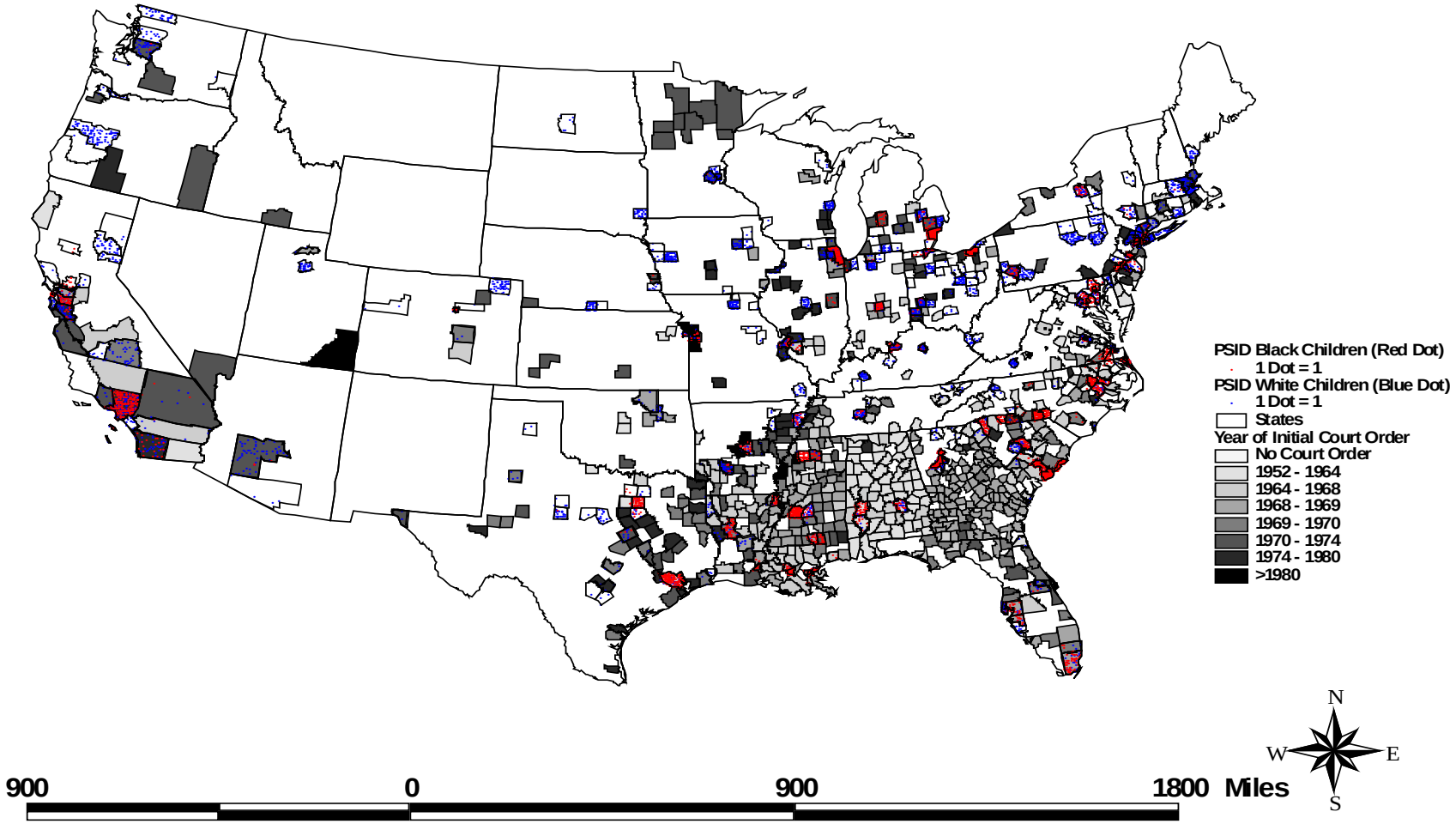


Figure A2c.

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

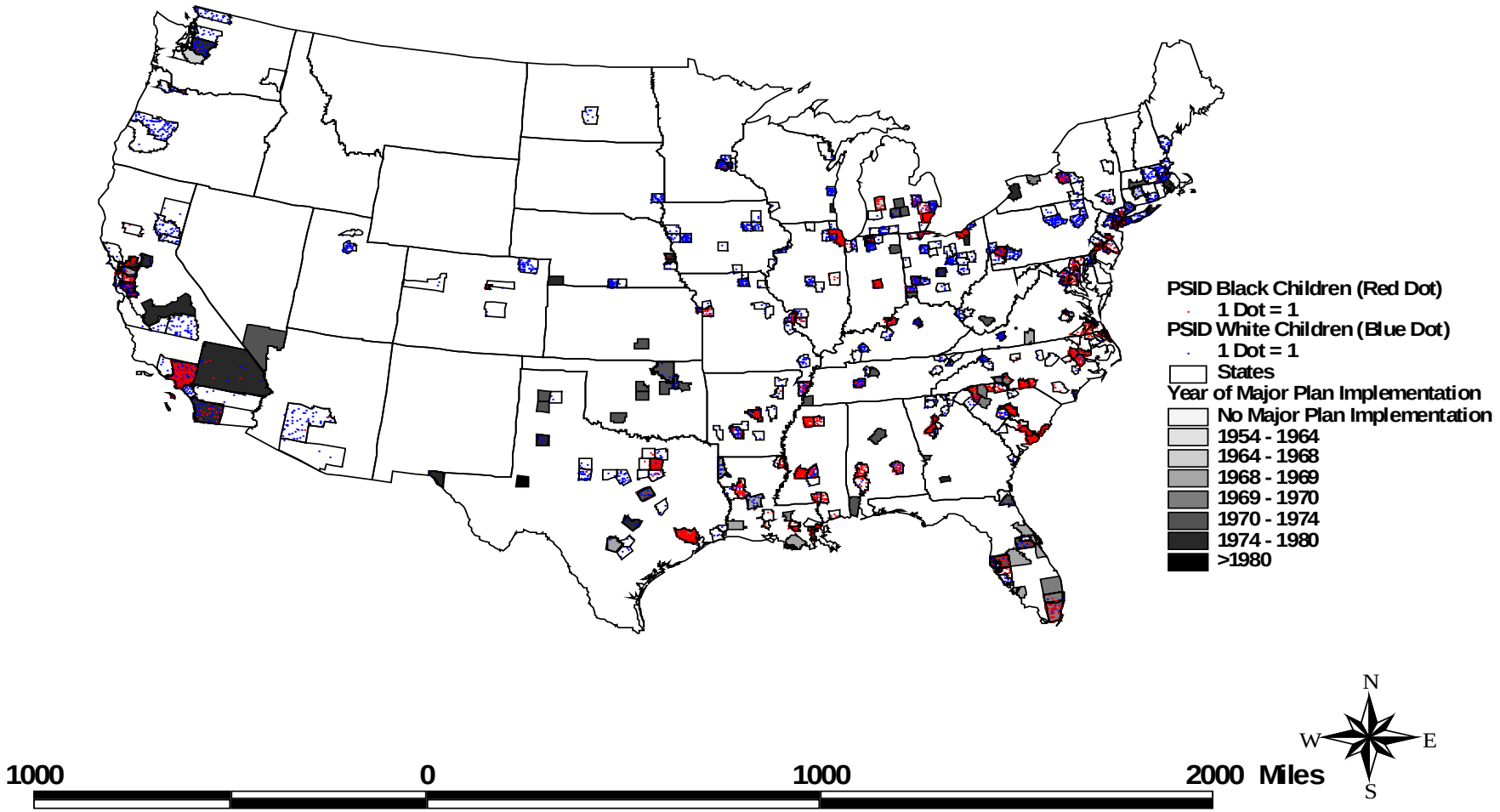


Figure A3.

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

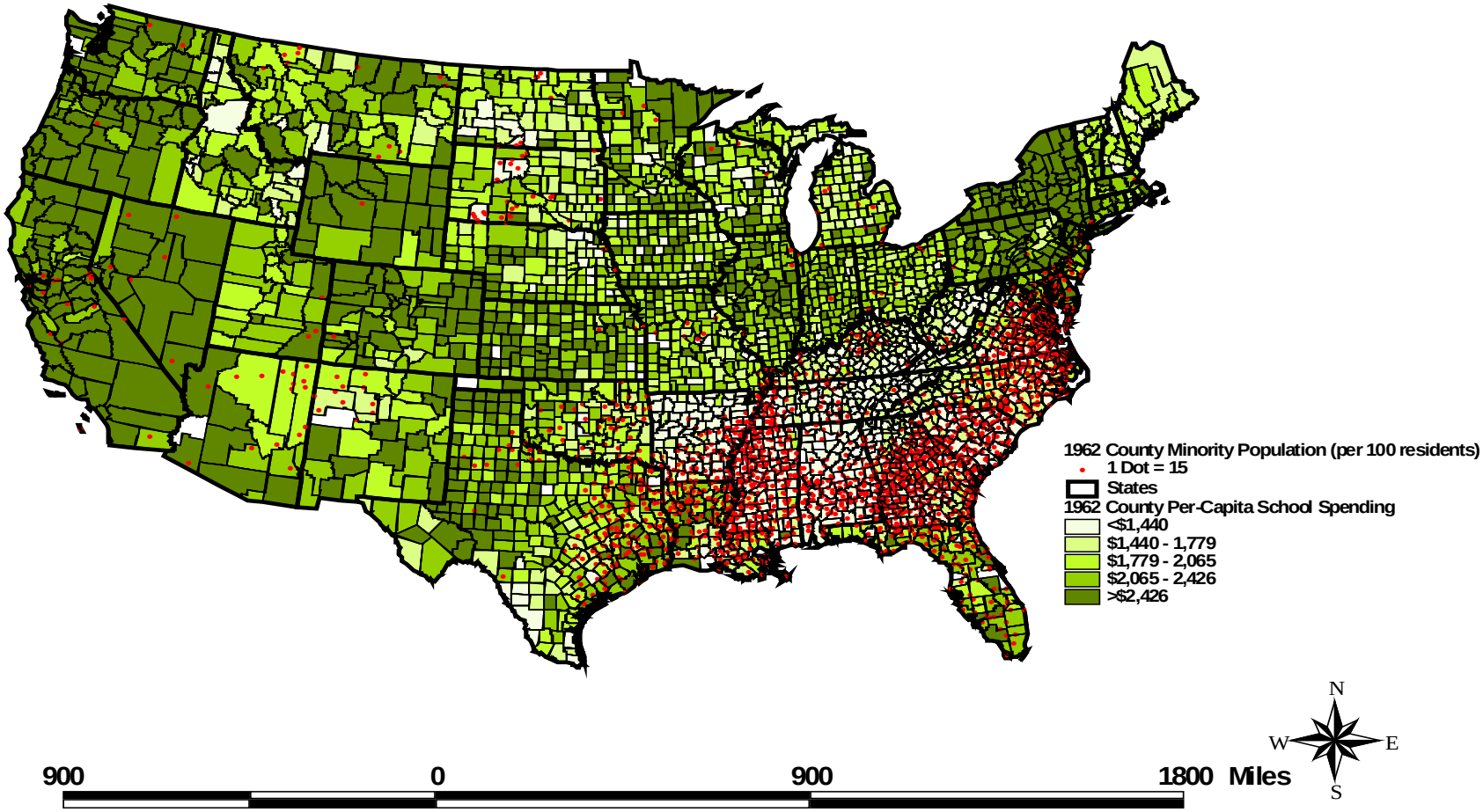


FIGURE A4. US COUNTY POVERTY RATES in 1960

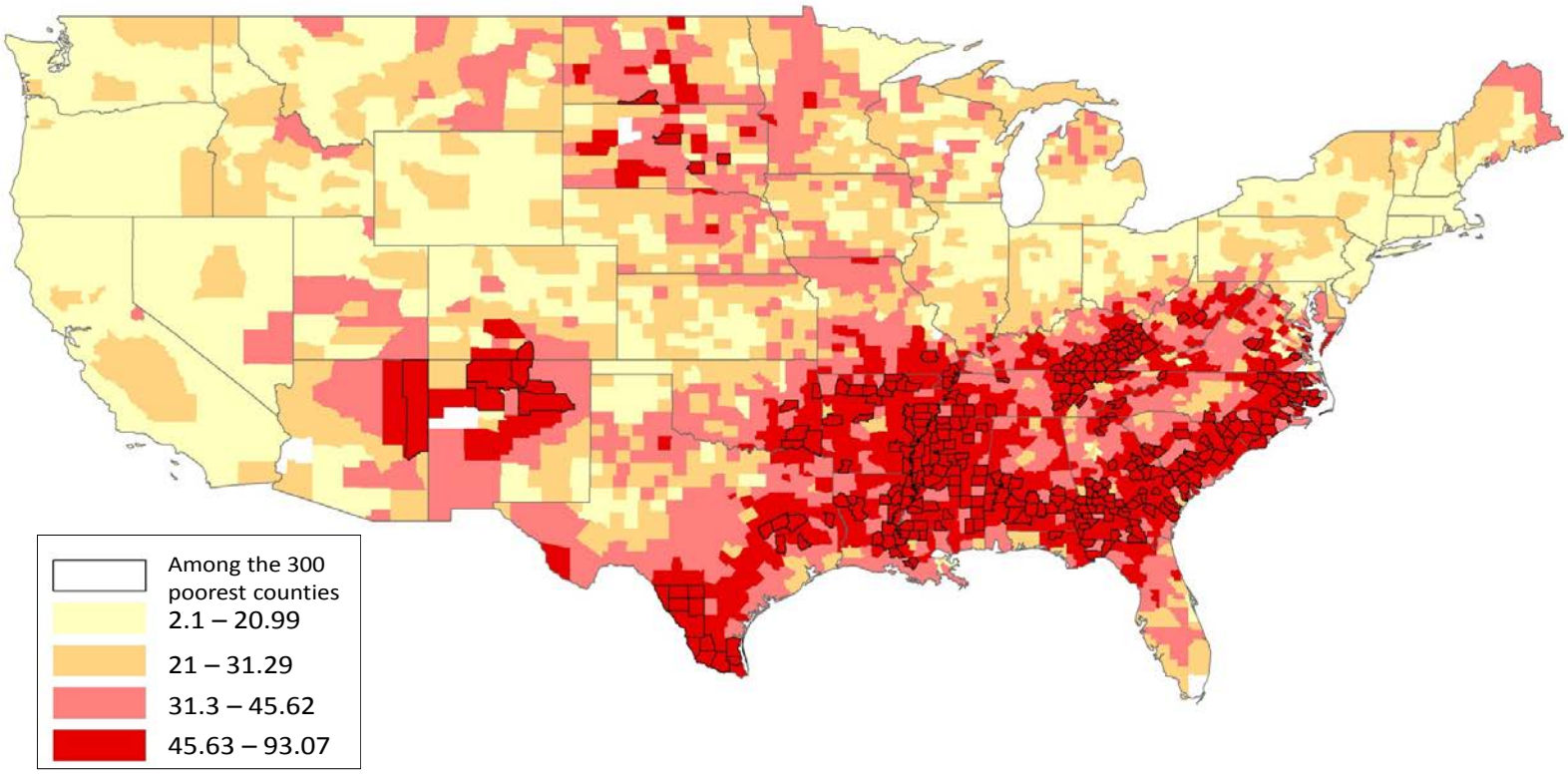
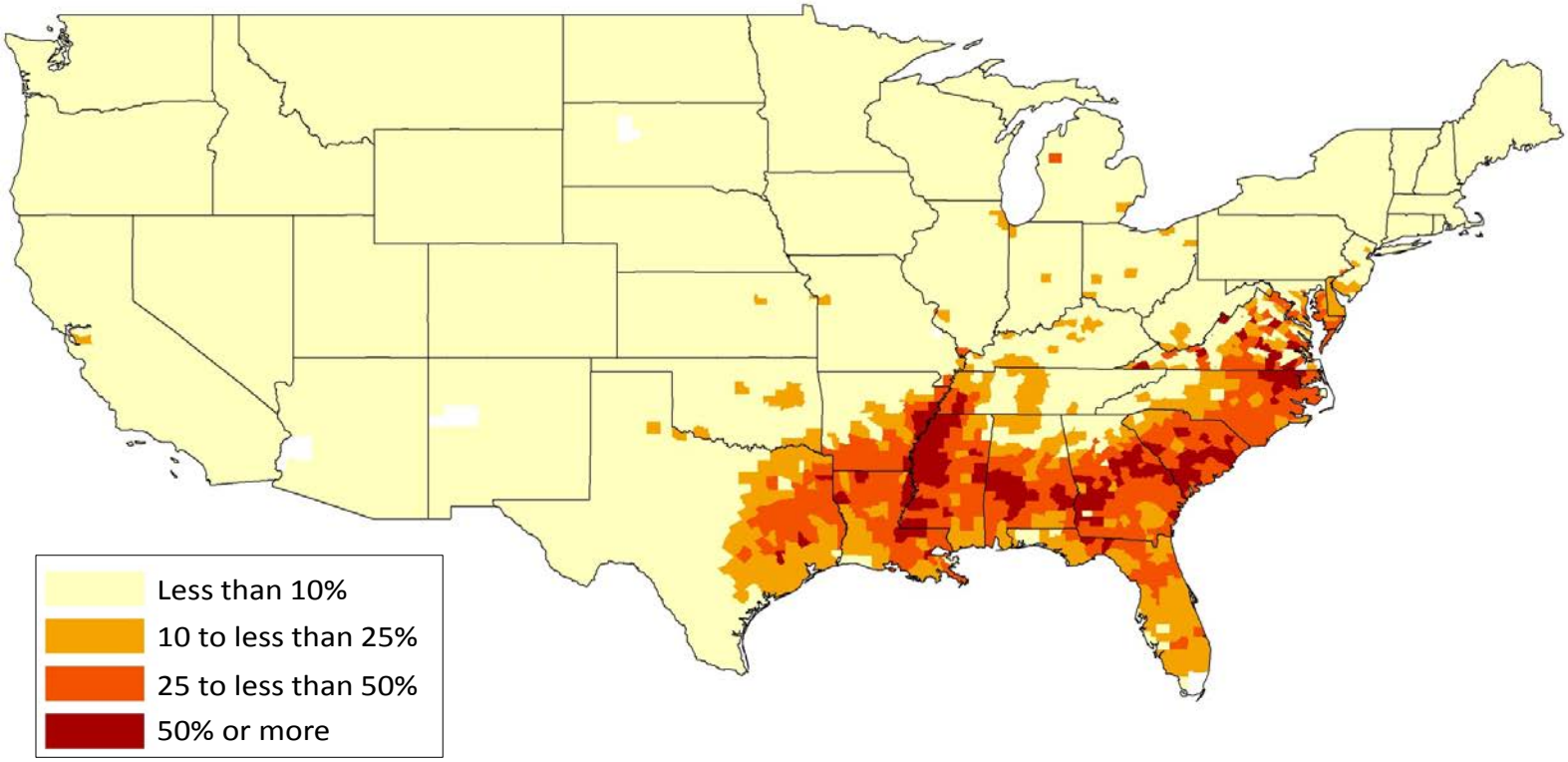


FIGURE A5. COUNTY POPULATION: PERCENT AFRICAN AMERICAN - 1960



APPENDIX B: A BRIEF HISTORY OF US SCHOOL DESEGREGATION

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

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

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

Timeline of School Integration in the US

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

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

Enforcement of desegregation did not begin in earnest until the mid-1960s. State and federal dollars proved to be the most effective incentives to desegregate the schools. A critical turning point was

the enactment of Title VI of the 1964 Civil Rights Act (CRA) and Title I funds of the 1965 Elementary & Secondary Education Act (ESEA), which prohibited federal aid to segregated schools and allowed the Justice Department to join suits against school districts that were in violation of the Brown vs. Board order to integrate. The congressional enactment of ESEA was among the most important events in effecting compliance because it dramatically raised the amount of federal aid to education; from a few million to more than one billion dollars a year; and, for the first time, the threat of withholding federal funds became a powerful inducement to comply with federal desegregation orders (Cascio et al., 2010; Holland, 2004).

Figure A3 presents a map of the geographic variation in school spending in the US in 1962 overlaid with the residential locations of minorities in that year. The map illustrates the concentration of minorities in the South where school district per-pupil spending levels were lowest. Another example of how financial incentives played a role in facilitating compliance is evident in President Nixon's proposal to provide financial incentives to school districts to comply with desegregation orders, which led to congressional enactment of the Emergency School Aid Act of 1972 to assist the federal courts in achieving desegregation (Ehrlander, 2002, p. 23). Federal dollars soon constituted 30 percent of the budget of many Southern school systems. The availability of federal money continued to influence desegregation into the 1980s. I find a significant correlation in the amount of federal funds received by school districts in the years 1966-1970 with the percentage of black students enrolled in previously all-white schools.

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

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

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

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

Extent of Desegregation Actions (post-1965 period). Substantial steps to desegregate schools during the period 1966-75 are reported in an estimated 1,400 school districts. While these districts

represent a small proportion of the 19,000 school districts in the country, they encompass about half of the minority public school children in the country. Although the actions to desegregate were most heavily concentrated in the Southern and Border States, such actions were found in a moderate number of districts in other regions of the country as well.

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

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

In Figure B6, 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 B6). Namely, the results suggests that for initial court orders meted out after 1965, there is roughly immediate implementation (on average, major plan implemented within 1-2 yrs of initial court order); and the lag does not differ over time for court orders after 1965. On the other hand, for initial court orders meted out before 1965, there is more than a 10-year delay in implementation of a major plan (following initial court order, major plan is not implemented, on average, for 10 years; there is a systematic long delay that decreases in years leading up to 1965. During the 1955-64 period (after Brown but prior to the passage of the Civil Rights Act), the earlier the initial court order, the longer the delay in implementation of a major plan. This pattern and discontinuity after 1965 in the time lag between initial court order and major desegregation plan implementation occurs in the South and non-South.

In 1964, 1 percent of African American students in the South attended school with whites; by 1968, this had risen to 32 percent. As shown in Figure A1, the ensuing years of 1968-1972 bracket the period of maximum desegregation activity. Figure A2 presents a map that summarizes the overall geographic pattern and timing of initial court orders overlaid with the childhood residential locations of the (nationally-representative) PSID sample of black and white children in 1968 (Figure A2b); and, analogously, Figure A2c shows this for the resultant subsequent major desegregation plan implementation in US school districts/counties[ii] (among the subset of districts for which this information is available). The figures demonstrate the strong overlap of residential locations of original sample PSID children with districts that underwent court-ordered desegregation.

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

As shown, districts exhibit a great deal of variation in the year in which the initial court order was issued and the subsequent timing when major desegregation plan implementation actually took place; this variation is evidenced both within and across regions of the country. In most regions, the initial court order took place in a narrower period than the 30-year period observed in the country as a whole; similarly, the span in timing of major desegregation plan implementation is narrower within regions than across the country as a whole. The regional pattern and clustering reflects the evolution of legal precedent. Figure B5 highlights the significant birth cohort variation in childhood exposure to court-ordered school desegregation for the PSID sample. The share of children exposed to school desegregation orders increases significantly with year of birth over the 1945-1970 birth cohorts analyzed in the PSID sample.

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

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

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

The 1962 county measures used as independent variables in the model include: the log(county population), percent of the population that is minority, per-capita school spending, the percent of school spending that comes from intergovernmental grants (state/federal), median income, percent of households with income <$3,000 (in 1961 dollars), percent of households with income >$10,000, percent with 12 or more years of education, population change between 1950-60, percent of residents in an urban area,

percent of residents in rural or farm area, percent of residents living in group quarters, median age, percent of residents that are school-age, percent of residents 65 or older, percent of residents that voted for the incumbent President, and the county mortality rate (all constructed from the 1962 Census of Governments, City & County Data Book). I include the size of the population to capture the fact that large districts/counties may face differential costs and opposition to the desegregation process. I also estimate an alternative model specification that includes the 1962 average student-to-teacher ratio and average teacher salary, instead of the per-capita school spending level (as shown in Table B1, similar patterns emerge). These data are linked with the desegregation court case and plan implementation data.

Columns (1)-(6) of Table B1 presents estimates from least-squares regressions of the year each school district had an initial court order (among those that first became subject to court order after 1962) on 1962 characteristics and region fixed effects, while the final two columns ((7)-(8)) use the same set of independent variables to examine determinants of the delay between the initial court order and major desegregation plan implementation (in years). Column (1) shows estimates for the full sample, column (3)-(8) show results for the subset of counties in which original sample PSID children grew up, and columns (5)-(8) display results for the subsample of counties for which information is available on the dates of major desegregation plan implementation.

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

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

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

[ii] While the data is available at the school district level, the maps are presented at the county level for convenience, so I use counties and school districts interchangeably here in reference to the maps.
[iii] School desegregation litigation cases have been initiated by school districts, plaintiffs, federal district court judges, parents of students in affected districts, and non-school governmental organizations.
[iv] I find similar results when I also define as "under court order" those districts that implemented desegregation plans in response to pressure from HEW in addition to school districts covered by formal court orders.

Appendix B1: Desegregation effects on school inputs using all districts ever under court order

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

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

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

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

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

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

levels of funding support from year to year. Additionally, time-varying, community-level characteristics and measures of government transfers adjust the estimates for observed differences in characteristics and changes in federal programs.

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

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

The Effectiveness of School Desegregation. I build on the findings of Welch and Light (1987), Guryan (2004), Reber (2005), and Weiner et al. (2008) by first analyzing the effectiveness of desegregation court-orders in reducing the extent of racial school segregation (but using a larger sample of 815 districts, instead of the 125 that prior studies had). I then extend these findings to show that in the years immediately following court orders, desegregation had notable impacts on two key school quality resource indicators among blacks—1) increases in per-pupil spending and 2) reductions in the student-to-teacher ratio. 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). These results are presented in Figures B1a-B4. The figures plot the regression coefficients on indicator variables for years before and after desegregation orders are enacted (year before initial court-order is the reference category) on school district racial segregation among both students and teachers, per-pupil spending, and the student-to-teacher ratio, respectively. The changes are all statistically significant. The similarity of the results among all districts ever under court order and the subset of those districts that overlap the PSID affirm the representativeness and generalizability of the findings reported from the PSID.

I also estimate identical models of the level of school district per-pupil spending from state revenue sources on the timing of court-ordered desegregation (with the inclusion of school district fixed effects and region-specific year effects), separately for school districts with a small proportion of black students (<0.2) versus districts with a large proportion of black students (>0.4). 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. As shown in Figure B3c, I find precisely this pattern: no significant changes in per-pupil school spending among districts that had a small proportion of black students; in contrast, we see substantial and statistically significant increases in per-pupil spending from state revenue sources among districts that had a large proportion of black students. These results complement the findings of Reber (2010) for Lousiana, and Cascio et al. (2010), and employ larger samples and geographic coverage.

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[i] The models estimated upon which Figures B1a-B4 are based also include dummy indicators for each of the corresponding years in excess of 6 before and after court-ordered desegregation, respectively; these are not displayed in the figures because of the lack of precision due to limited observations that far away from the year of initial court order.

[ii] 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.

[iii] 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.

[iv] 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.

FIGURE B1a.

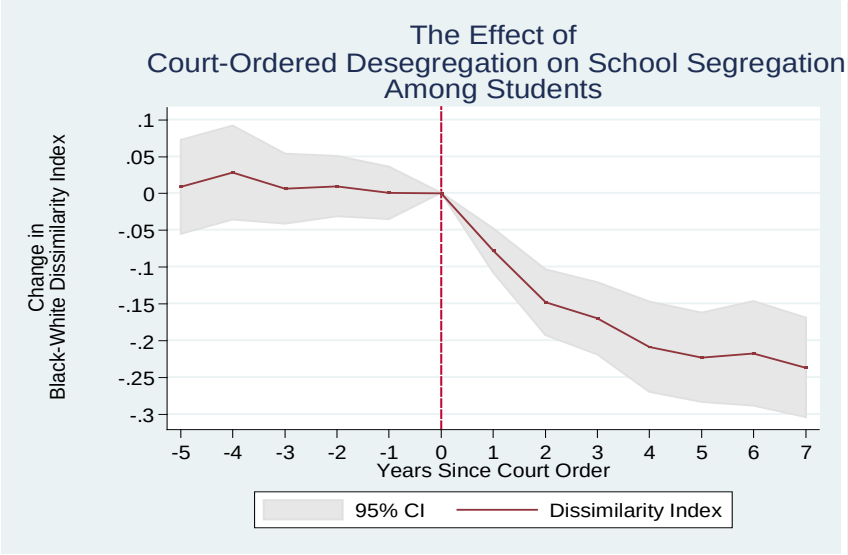
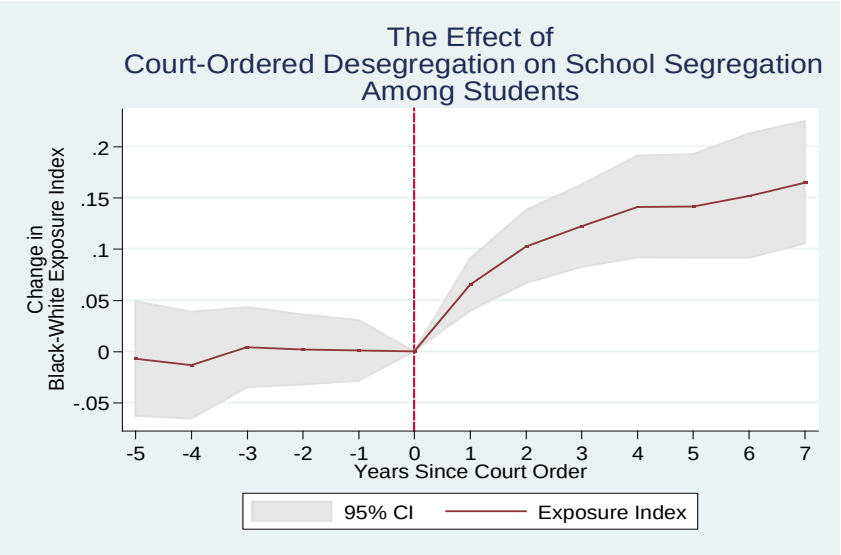


FIGURE B1b.



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

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

FIGURE B2a.

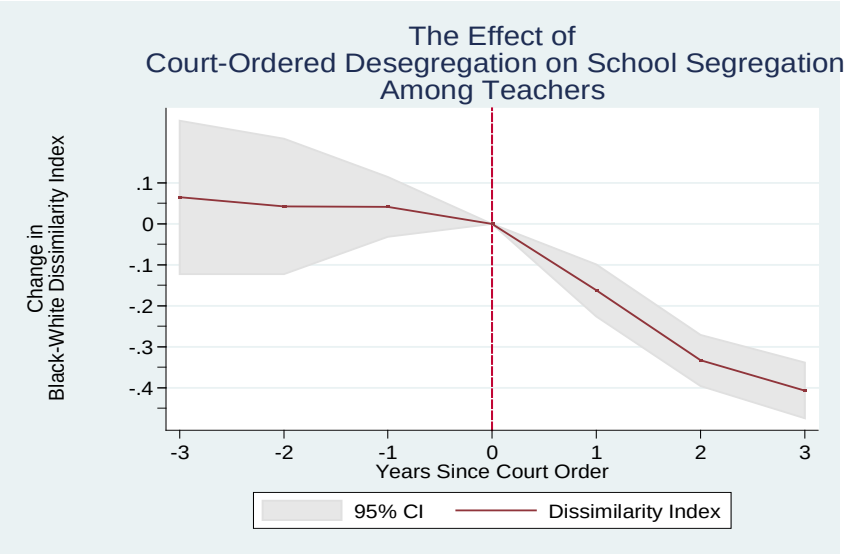
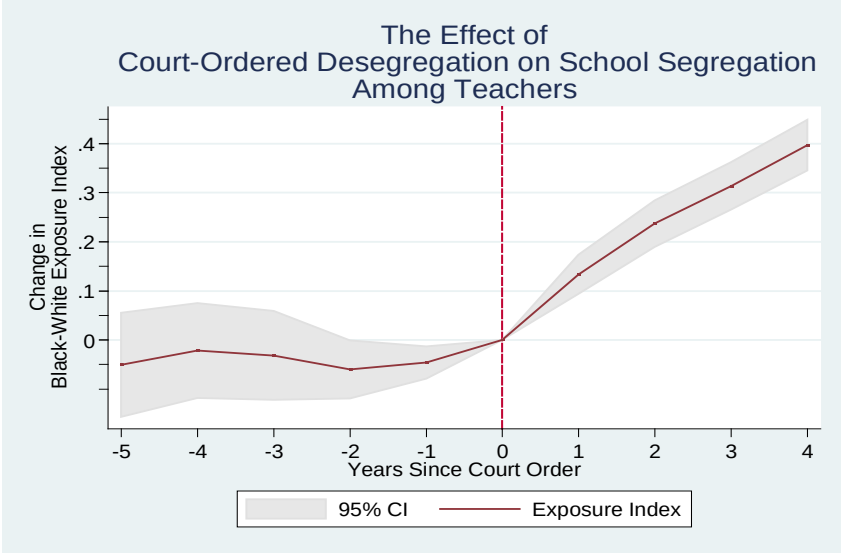


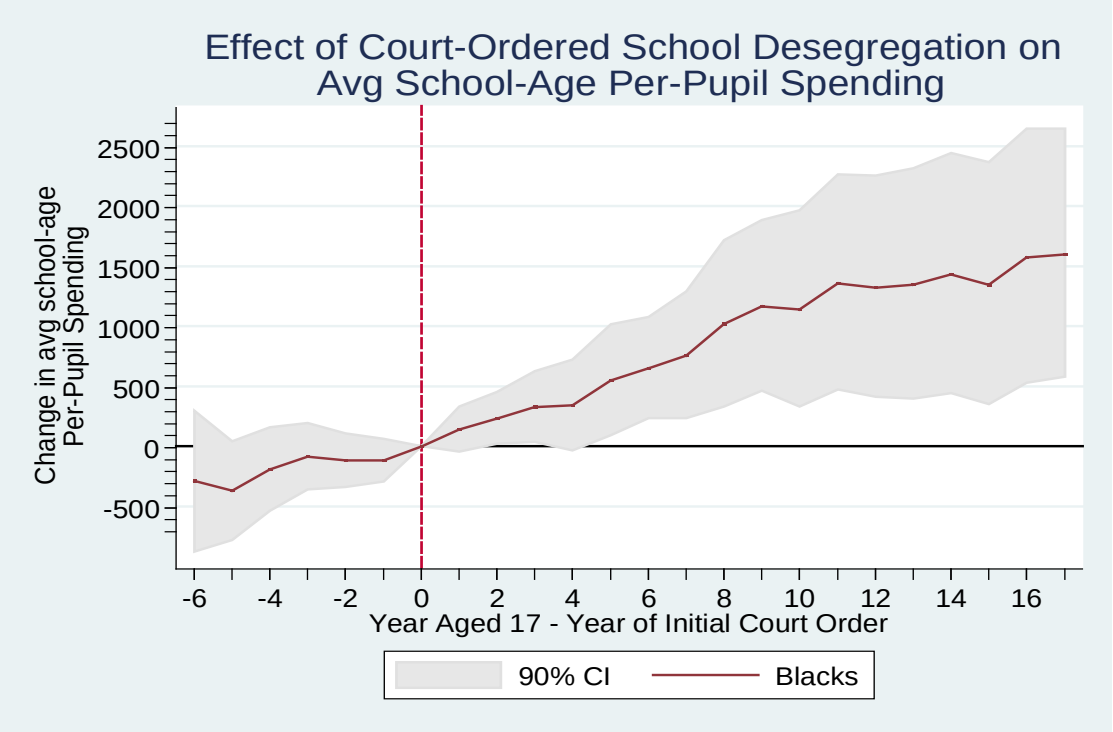
FIGURE B2b.



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

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

FIGURE B3a



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

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

FIGURE B3b.

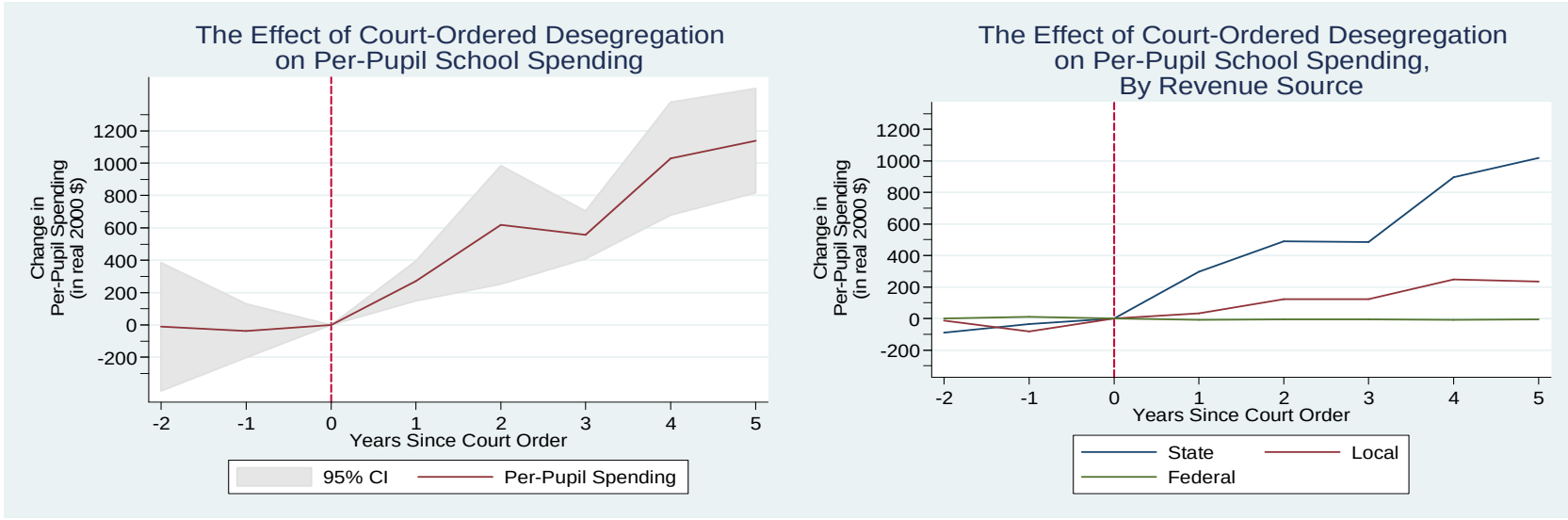


FIGURE B3c.

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

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

FIGURE B3d.

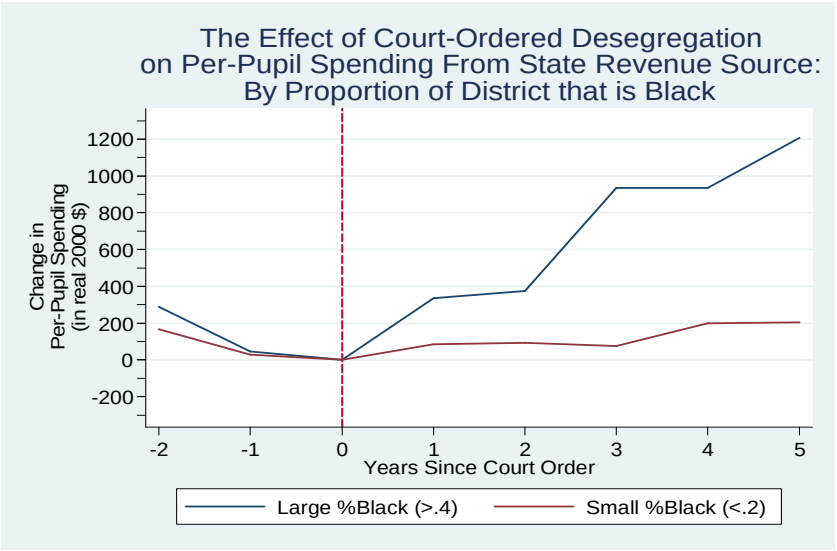
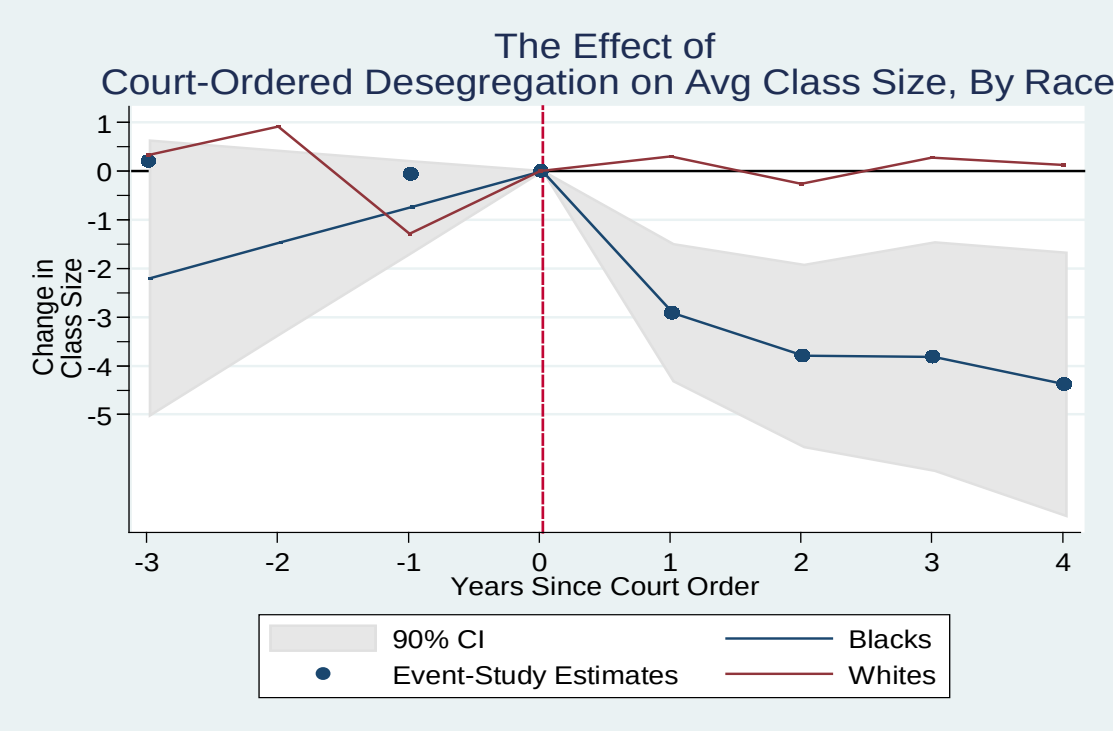


FIGURE B4.



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

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

FIGURE B5.

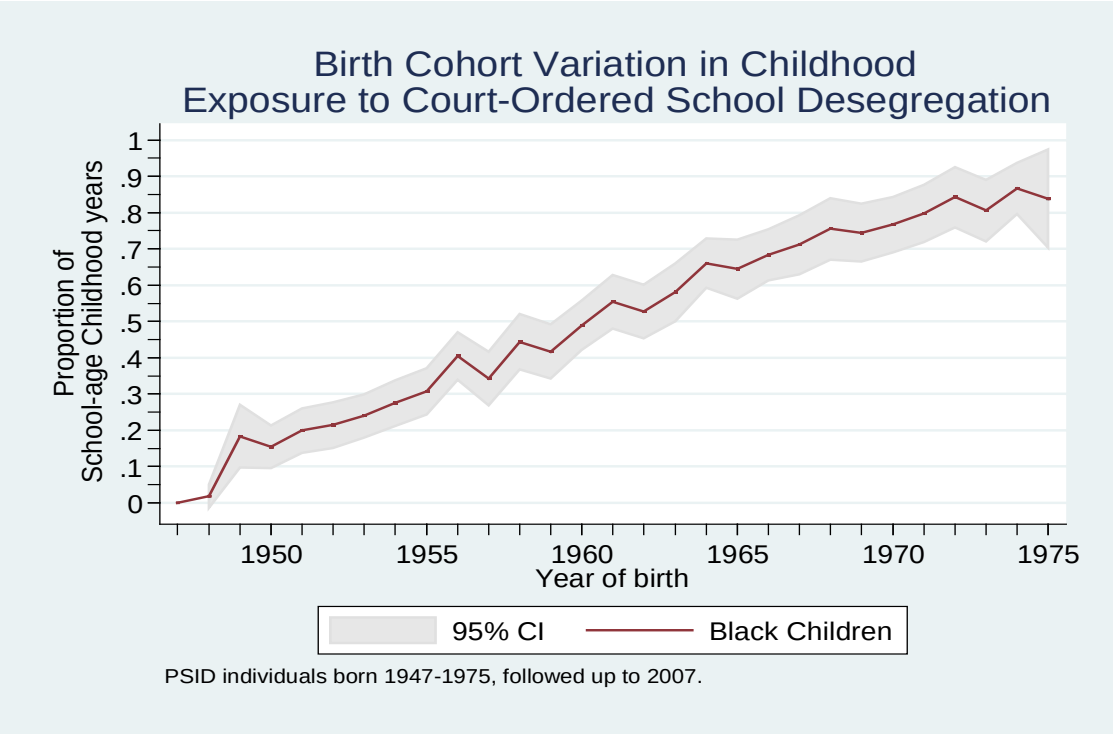
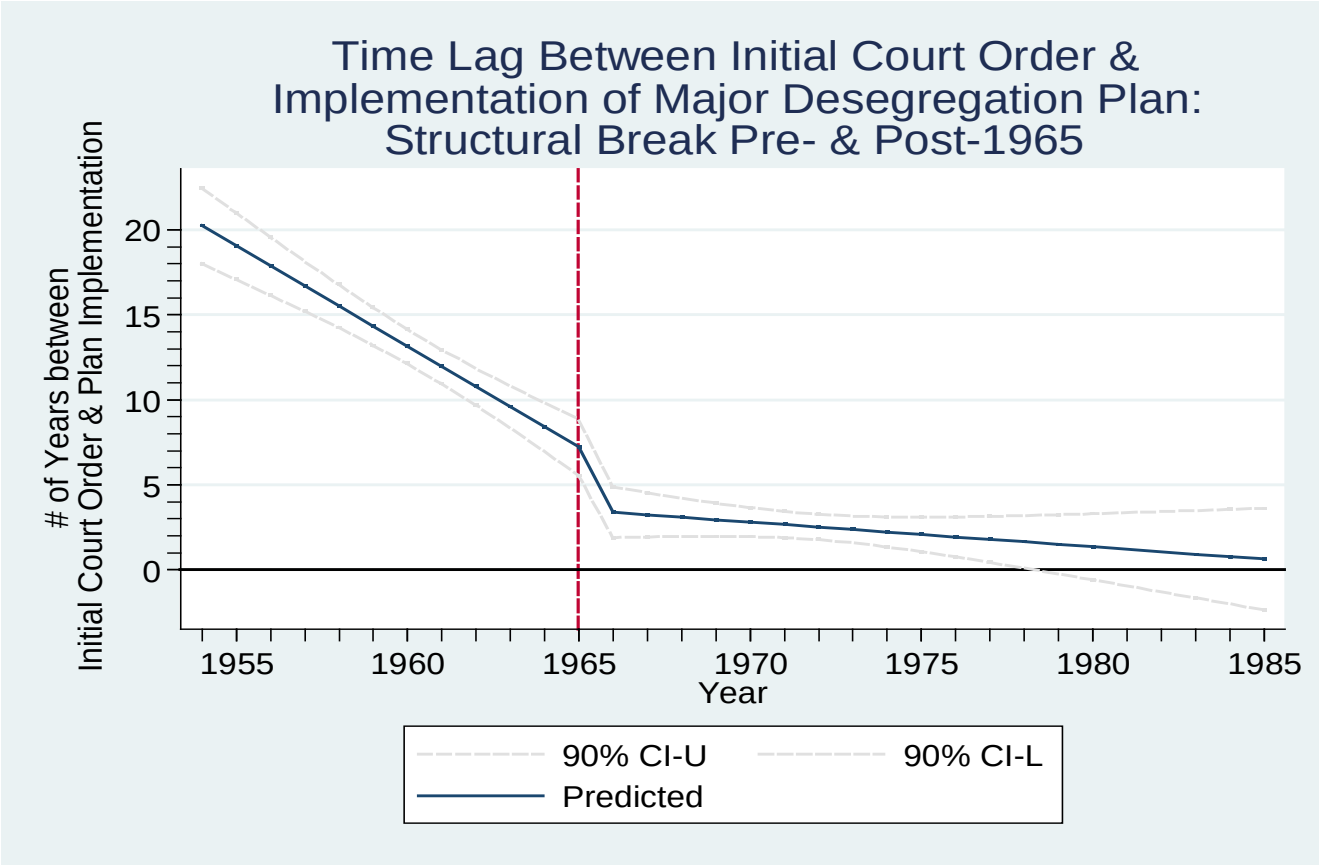

Birth Cohort Variation in Childhood
Exposure to Court-Ordered School Desegregation
Proportion of
School-age Childhood years
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
1950
1955
1960
1965
1970
1975
Year of birth
95% CI
Black Children
PSID individuals born 1947-1975, followed up to 2007.

Figure B6.


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

Appendix Table B1: Determinants of the Timing of Court-Ordered School Desegregation Using 1962 County Characteristics

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

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

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

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

PSID sample

Studies have concluded that the PSID sample of heads and wives remains representative of the national sample of adults (Fitzgerald, Gottschalk, and Moffitt, 1998a; Becketti et al, 1988), and that the sample of "split offs" is representative (Fitzgerald, Gottschalk and Moffitt, 1998b). The 95-98% wave-to-wave response rate of the PSID makes this possible. Appendix Table C0 reports descriptive statistics for the sample used in the models of adult outcomes, separately by race. The substantial race differences in childhood family characteristics are highlighted in this table.

Multinomial Models of Education Attainment

In addition to the main education models reported, I also estimate multinomial logit models of educational attainment, where the four categories are: High School Dropout/GED (reference category (0)); (1) High School Graduate, no college; (2) Attend College, no 4-year degree; and (3) 4-year College Graduate or more. I find that the effects of school desegregation for blacks were not limited to those on the margin of dropping out of high school, but also had significant effects that led to increased college attendance and completion rates. The results demonstrate that there is a significant difference in both high school dropout rates and college attendance and completion rates among blacks between cohorts that were born less than 7 years apart but differed in whether and how long they attended integrated schools; with no significant effects for whites across any of the educational attainment categories.

Incarceration Measures

Spells of incarceration are recovered from information on PSID respondents' collected in each survey (1968-2013) that includes whether a respondent was incarcerated at the time of the interview. The 1995 wave added a supplemental crime history module to the PSID including several key questions that I use to augment and obtain more precise information about the timing and duration of incarceration and minimize measurement error.

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

Health Index

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Appendix Table C0. Descriptive Statistics by Race

	Blacks (N=4,473)	Whites (N=3,993)
Adult Outcomes:		
High School Graduate	0.79	0.86
Years of Education	12.60	13.51
Ln(Wages), at age 30	2.26	2.63
Annual Work Hours, at age 30	1540.06	1895.99
Adult Family Income, at age 30	$31,020	$52,937
In Poverty, at age 30	0.24	0.05
Occupational Prestige Index	34.42	48.57
Ever Incarcerated, by age 30	0.08	0.04
Annual Incidence of Incarceration, at age 25	0.0063	0.0014
Adult Health Status Index, at age 30	84.16	88.78
Age (range: 20-50)	32.7	34.3
Year born (range: 1945-1968)	1958	1957
Female	0.45	0.43
Childhood school variables:		
Per-pupil spending (avg, ages 5-17)	$3,508	$3,865
Black-White Dissimilarity Index (avg, ages 5-17)	0.58	0.49
Any court-ordered desegregation, age 5-17	0.68	0.57
# of exposure yrs to desegregation, age 5-17	5.58	4.22
1960 District Percent Black	26.18	12.13
1960 District Poverty Rate (%)	28.29	18.32
Childhood family variables:		
Income-to-needs ratio (avg, ages 12-17):	1.54	3.48
In poverty (%)	0.41	0.07
Mother's years of education	10.15	11.81
Father's years of education	9.21	11.74
Born into two-parent family	0.40	0.70
Childhood neighborhood variables:		
Neighborhood poverty rate	0.20	0.07
Residential segregation dissimilarity index$_{county}$	0.72	0.71

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

FIGURE C1a

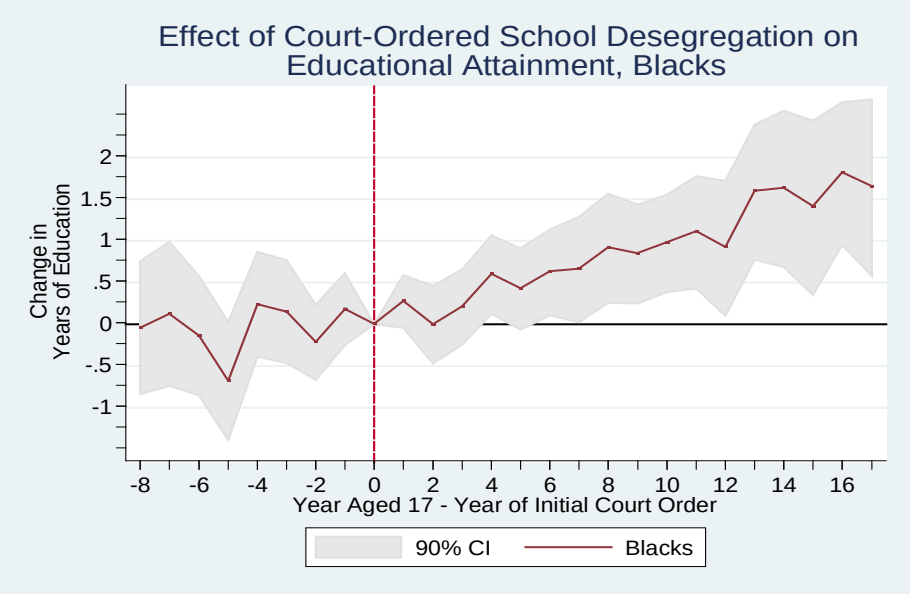
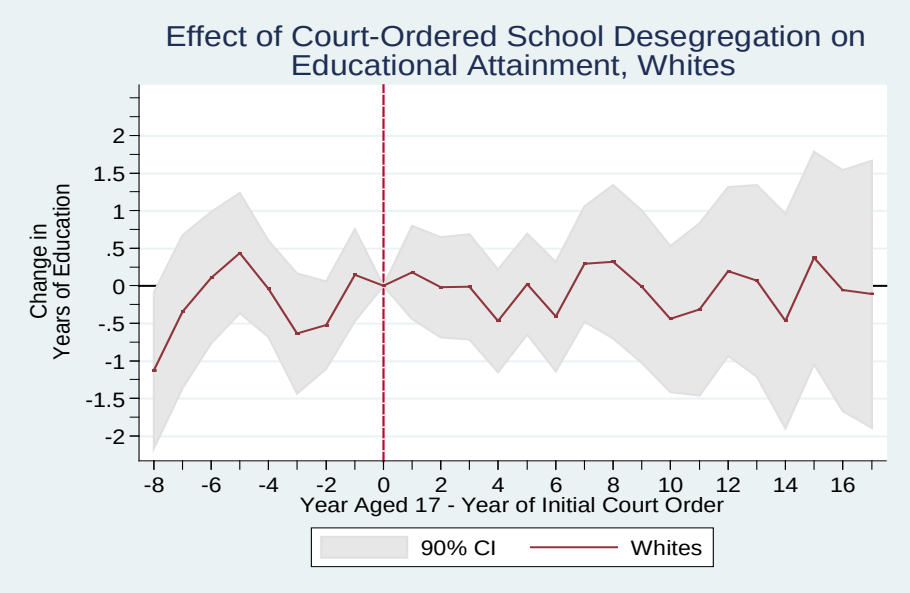


FIGURE C1b



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

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

FIGURE C2a

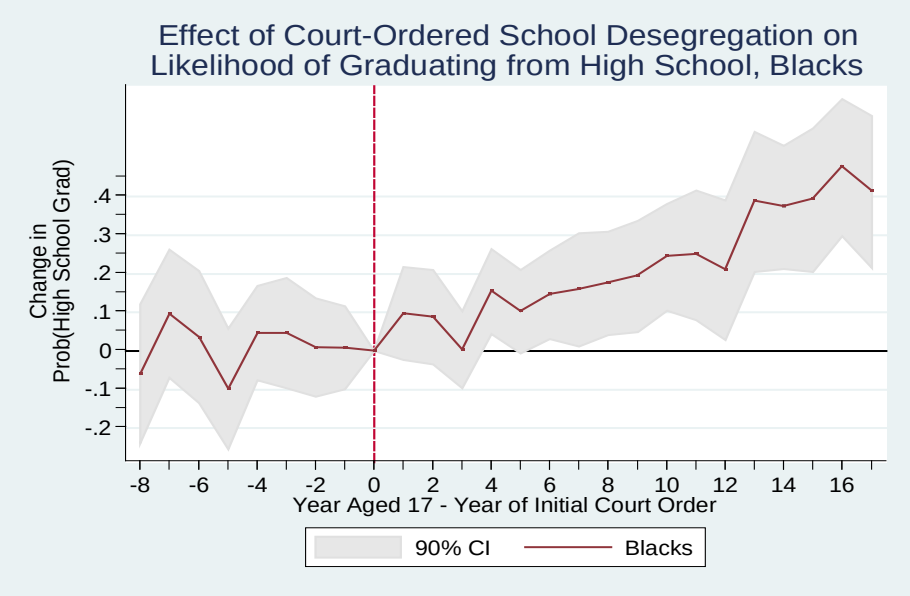
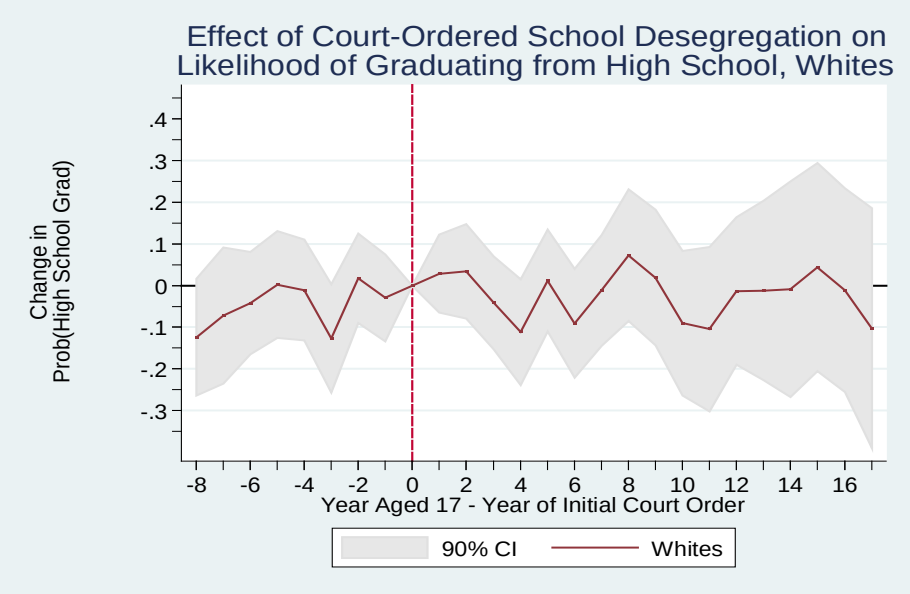


FIGURE C2b



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

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

FIGURE C3a

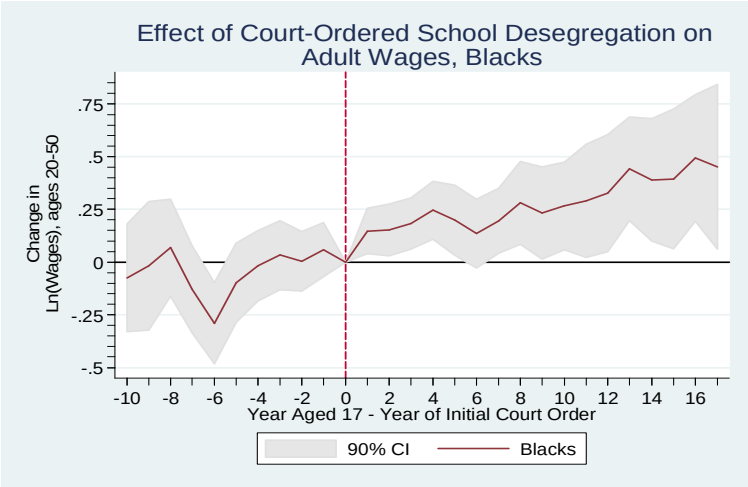


FIGURE C3b

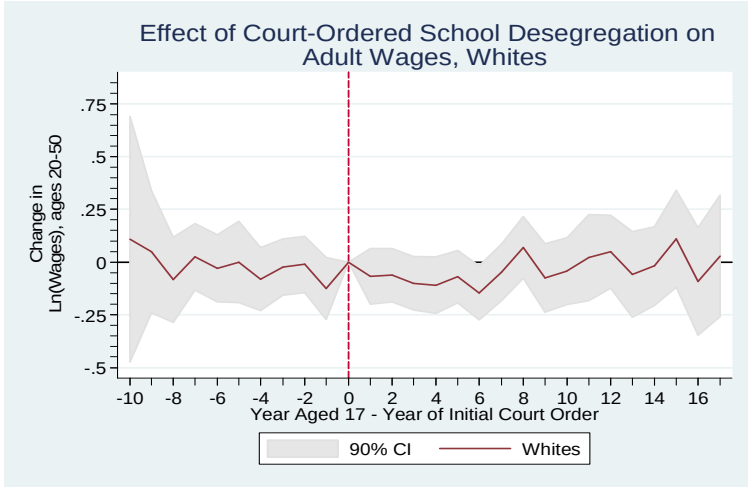


FIGURE C4a

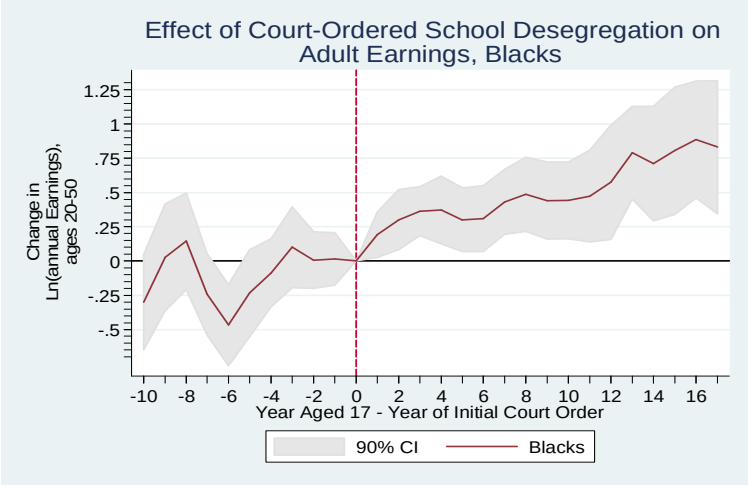
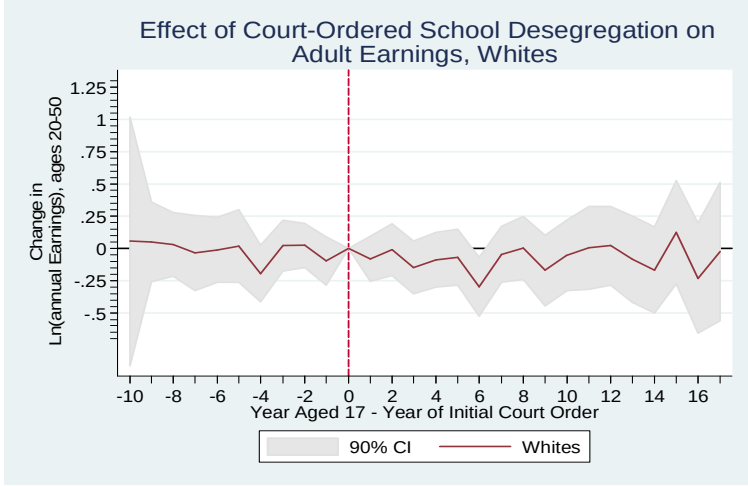


FIGURE C4b



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

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

FIGURE C5a

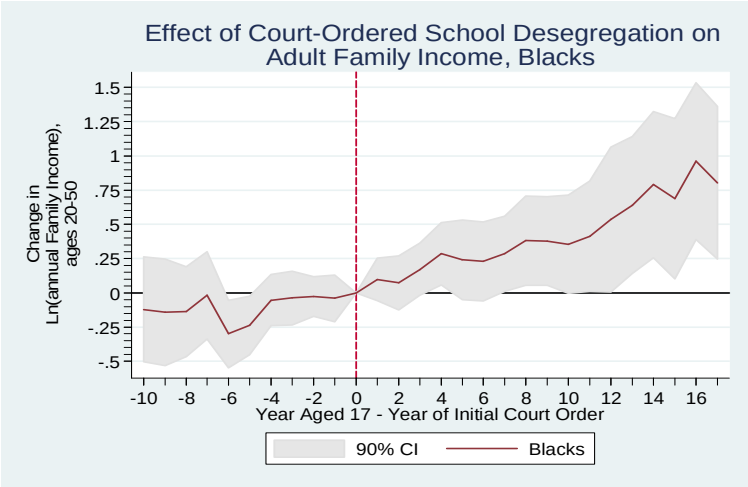


FIGURE C5b

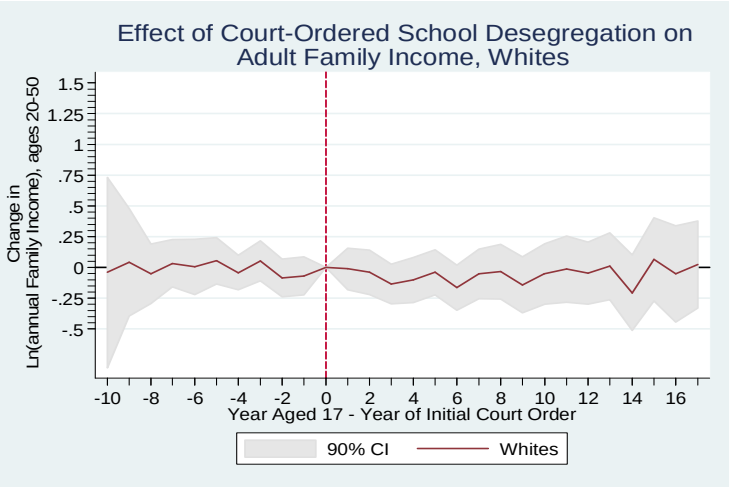


FIGURE C6a

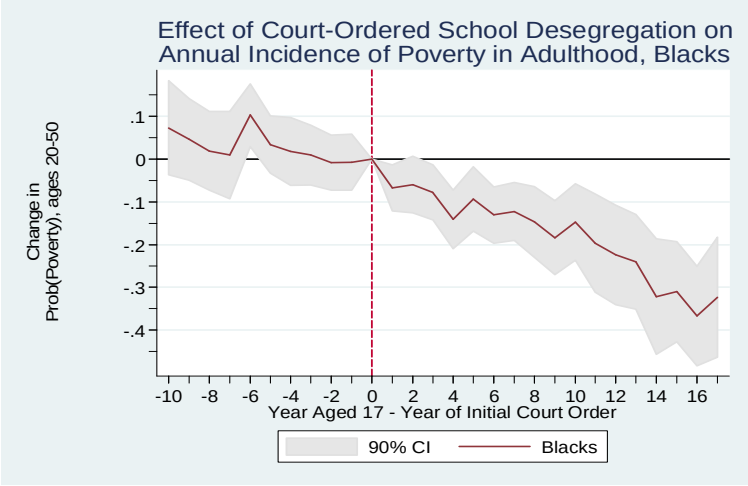
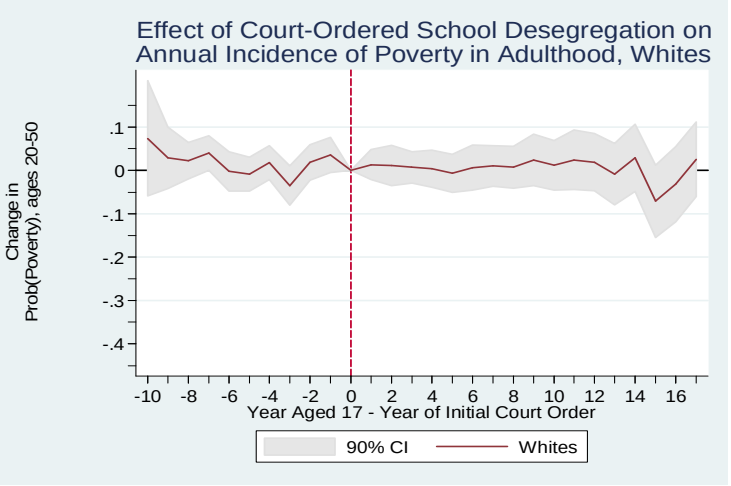


FIGURE C6b



Data: PSID geocode Data (1968-2013), matched with childhood school characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. All person-year observations (ages 20-50) are included except those in which individual was in school (N=142,499 person-year family income observations, 9,156 individuals from 3,702 childhood families, 645 school districts).

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

FIGURE C7a

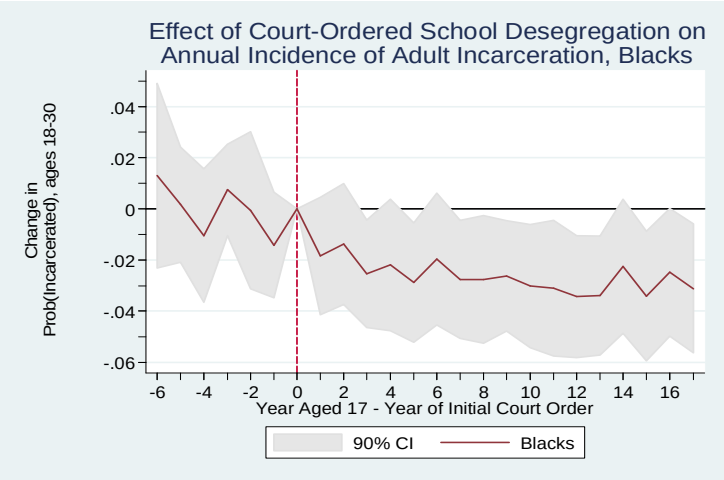


FIGURE C7b

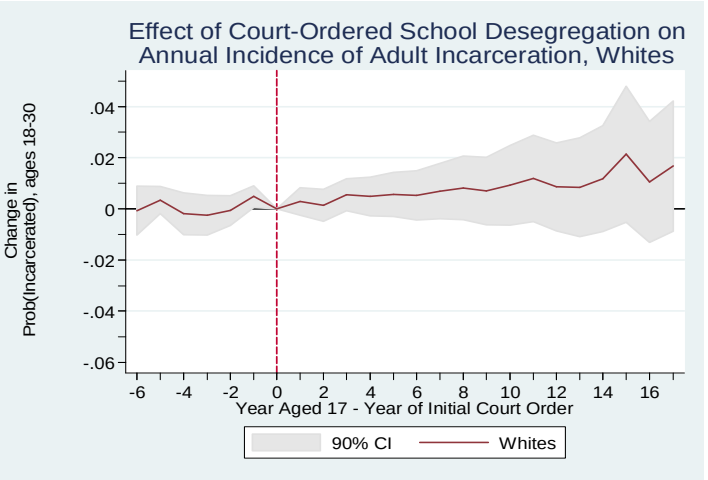


FIGURE C8a

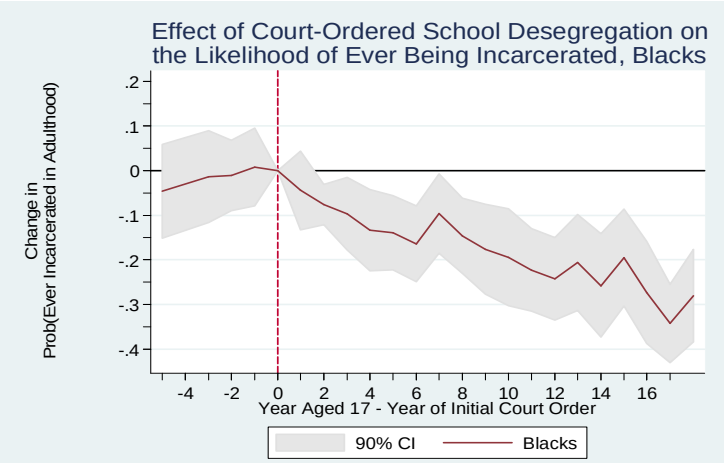
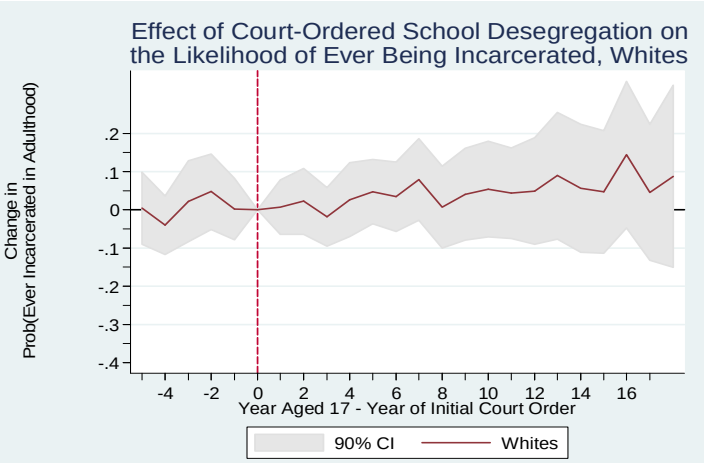


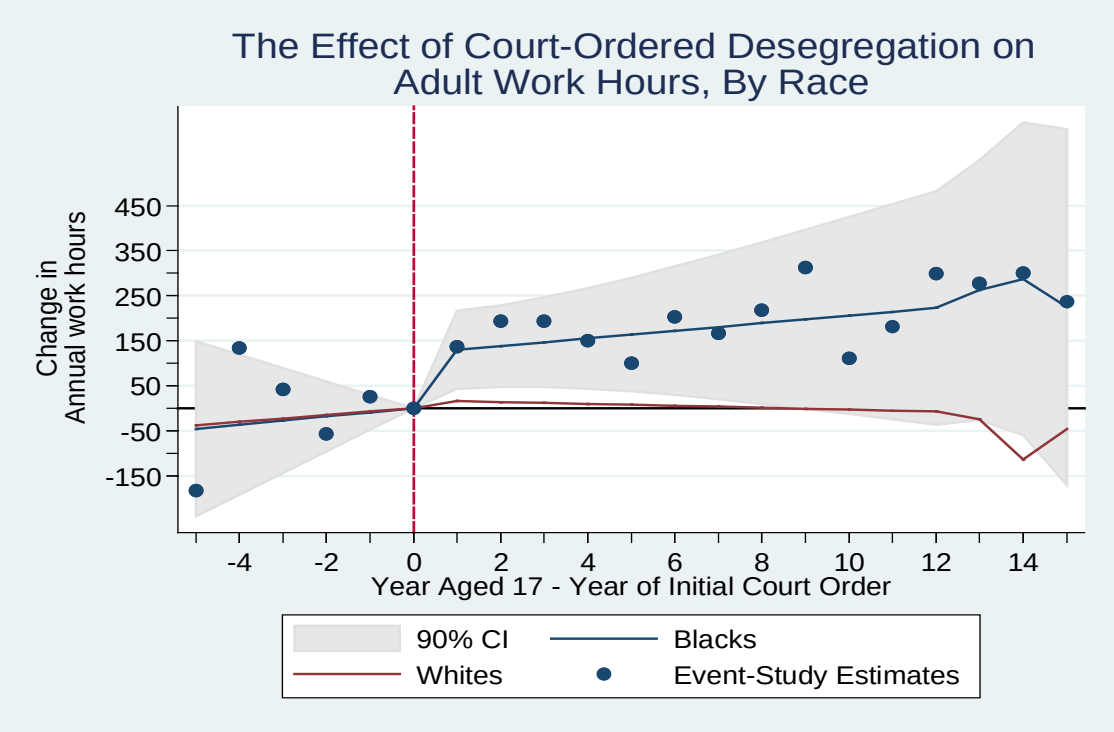
FIGURE C8b



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

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

FIGURE C9.



Data: PSID geocode Data (1968-2013), matched with childhood school and neighborhood characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. All person-year observations (ages 20-50) are included except those in which individual was in school or pregnant/yrs immediately following childbirth.(N=85,497 person-year work hours' observations, 8,396 individuals from 3,557 childhood families, 633 school districts).

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

FIGURE C10a

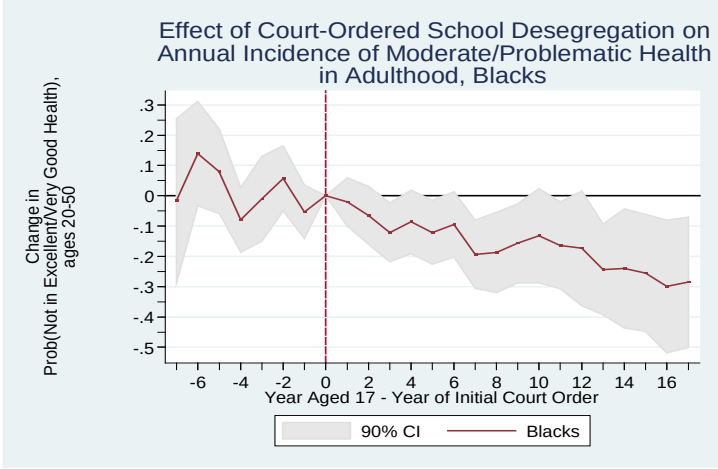


FIGURE C10b

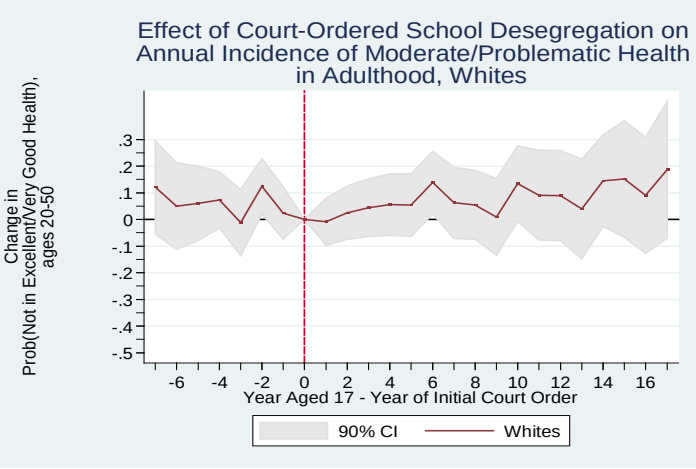


FIGURE C11a

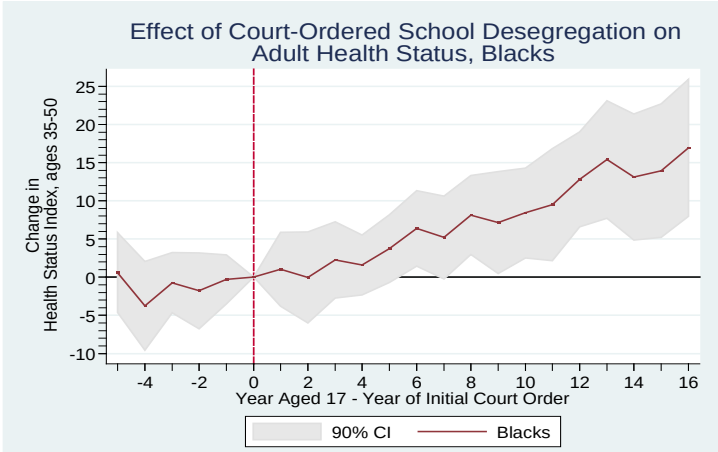
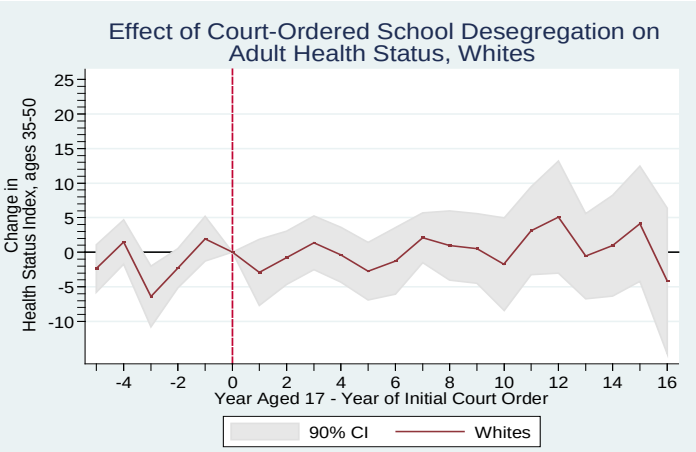


FIGURE C11b



Data: PSID geocode Data (1968-2013), matched with childhood school characteristics; court-ordered desegregation case litigation data (1954-2000; Brown Univ/American Communities Project). Analysis sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. All person-year self-assessed health status observations (ages 20-50) are included except those in which individual was pregnant/yrs immediately following childbirth (Figure C10a-C10b: N=75,729 person-year health status observations, 7,527 individuals from 3,330 childhood families, 613 school districts). Health Status index (1-100(perfect health)) based on self-assessed health (E/VG/G/F/P), 1985-2013; interval regression model estimated, where E=[95,100]; VG=[85,95); G=[70,85); F=[30,70); P=[1,30). (Figure C11a-C11b: N=42,011 person-year observations at ages 35-50 for 5,598 individuals from 2,797 childhood families, 570 school districts).

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

FIGURE D1a

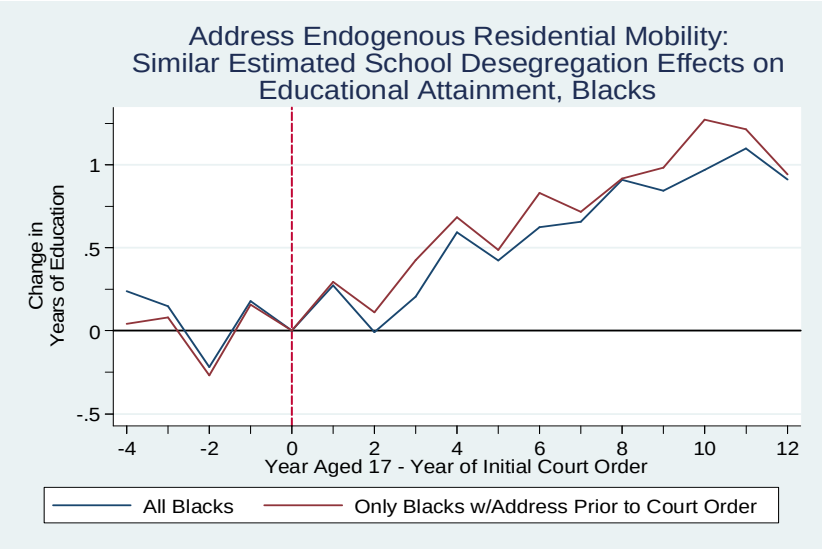


FIGURE D1b

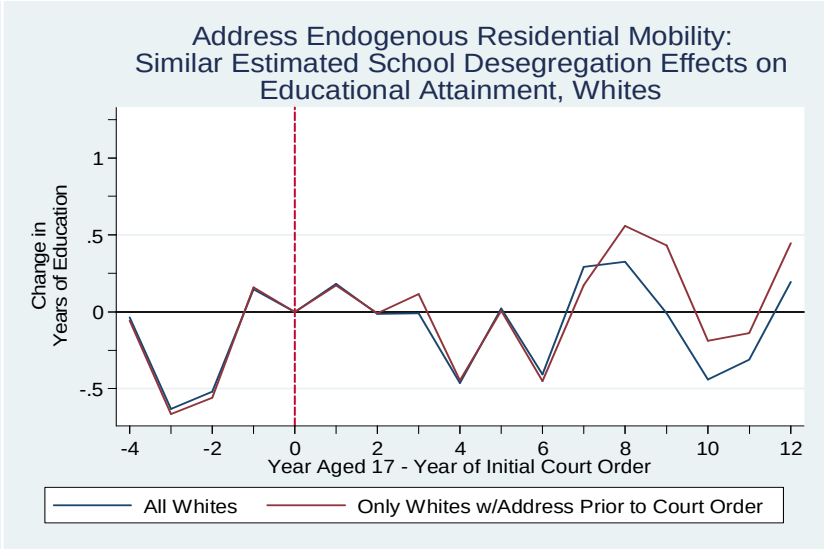


FIGURE D2a

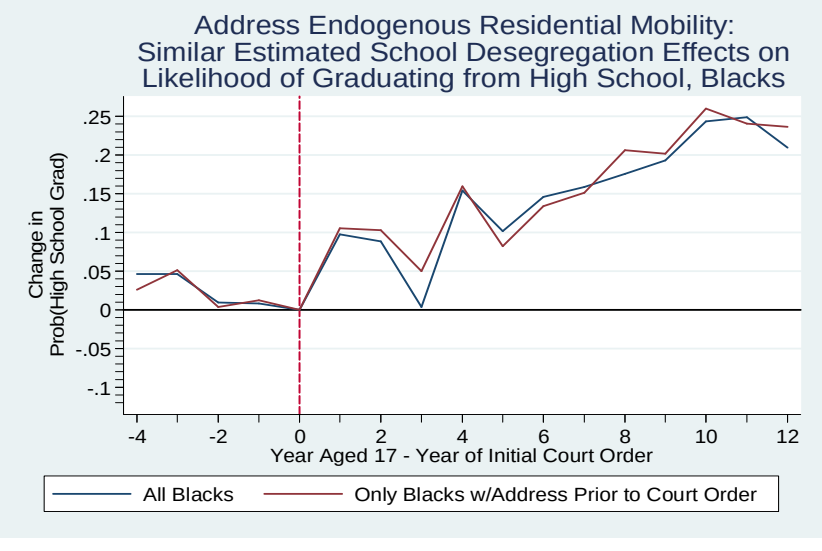


FIGURE D2b

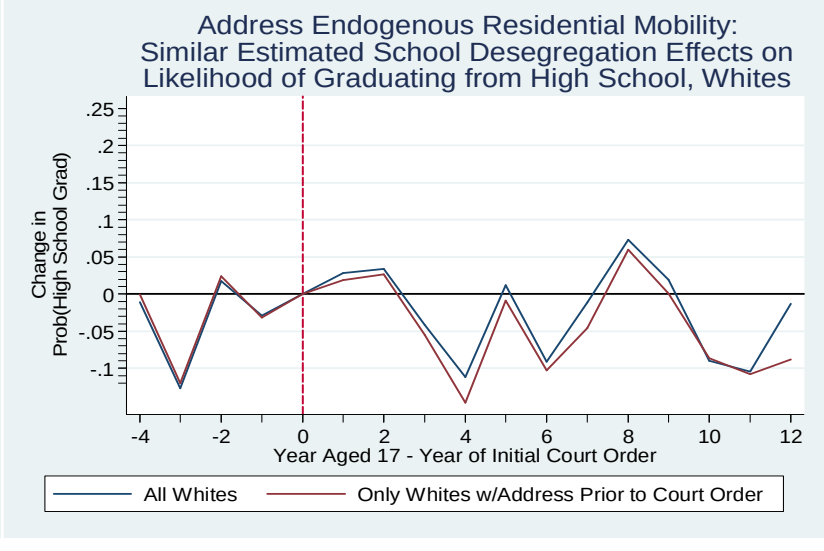


FIGURE D3a

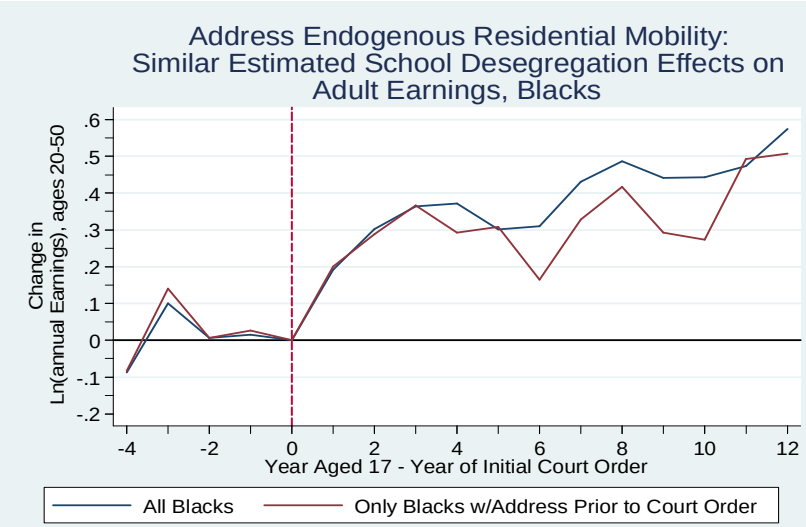


FIGURE D3b

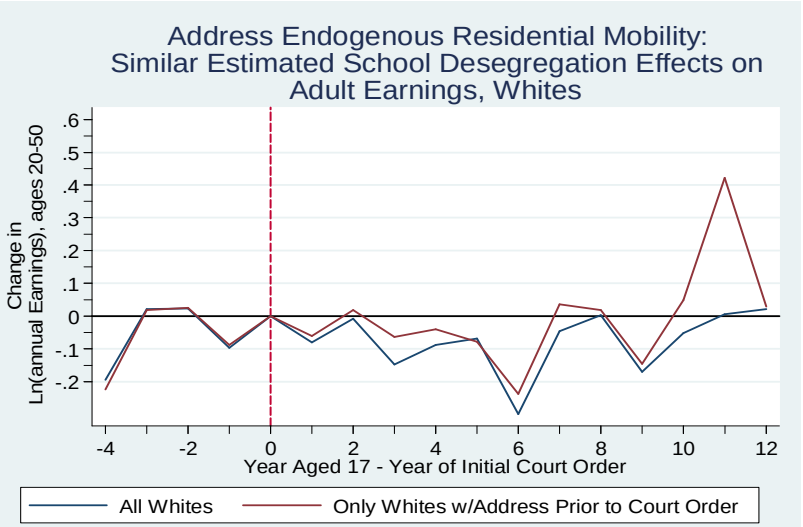


FIGURE D4a

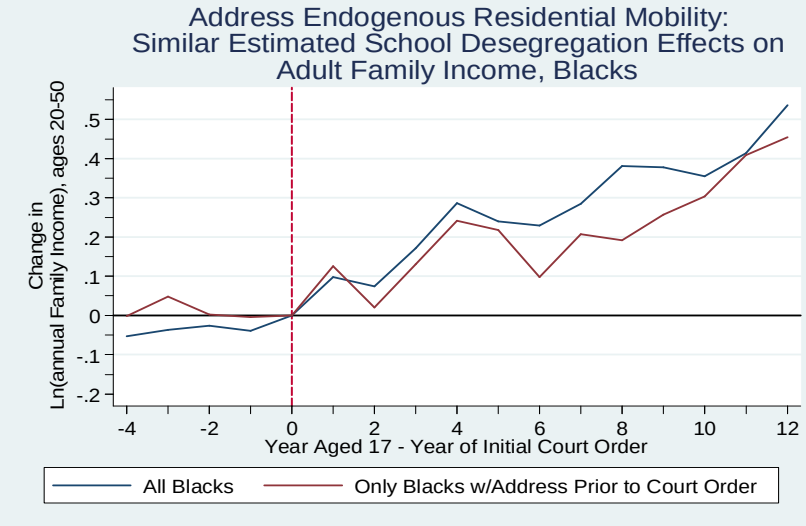


FIGURE D4b

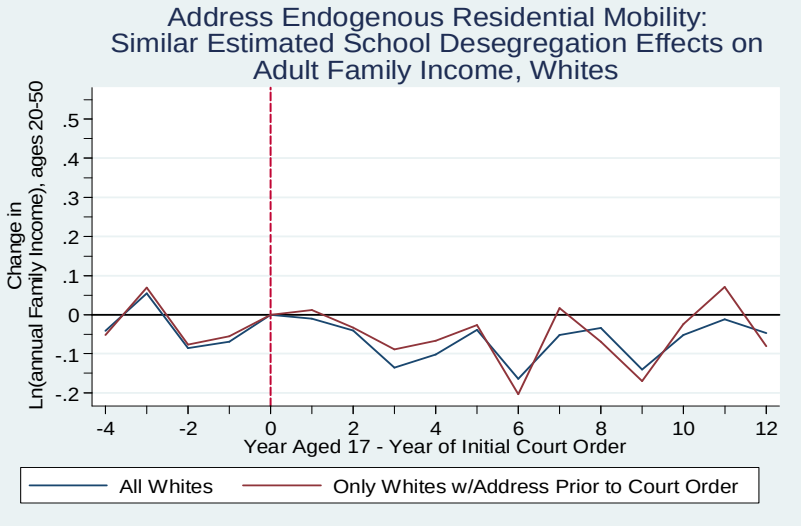


FIGURE D5a

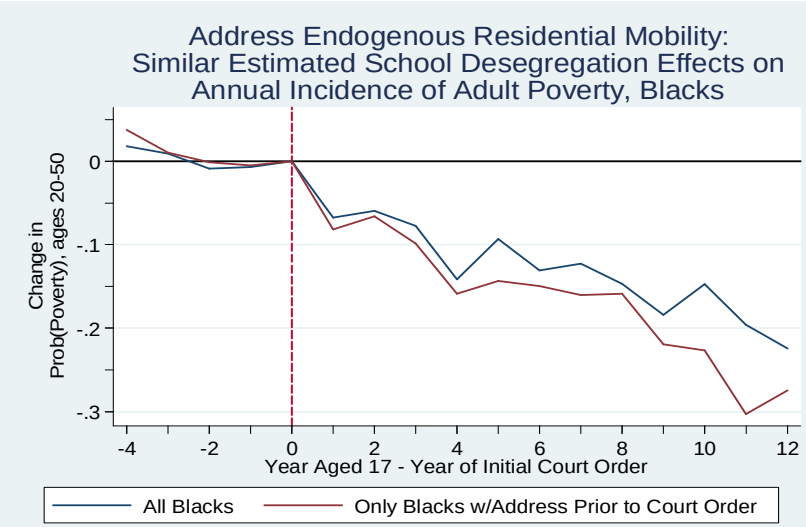


FIGURE D5b

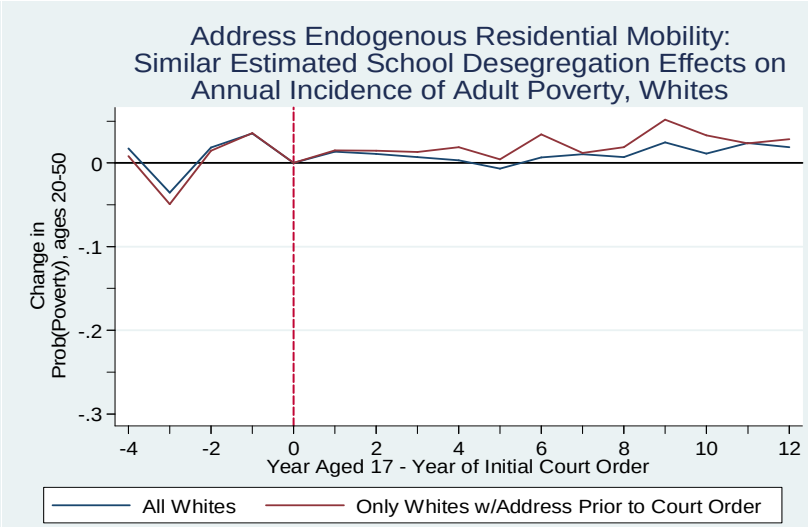


FIGURE D6a

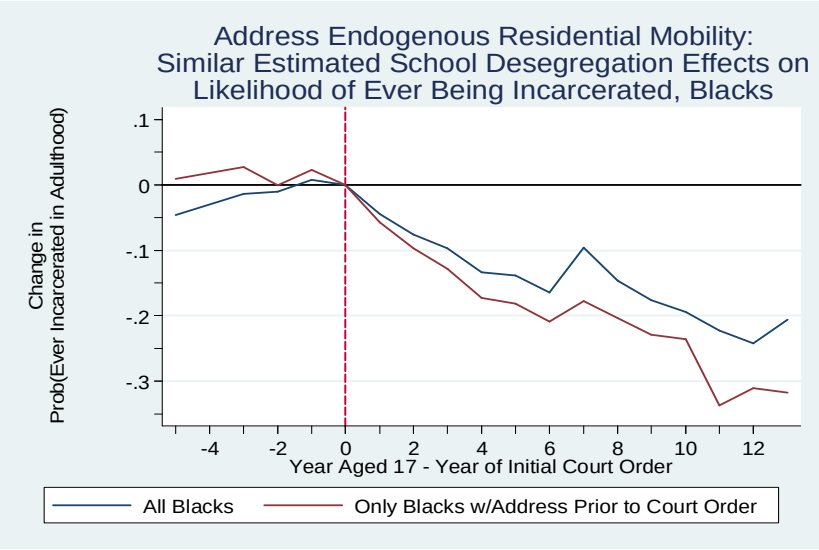
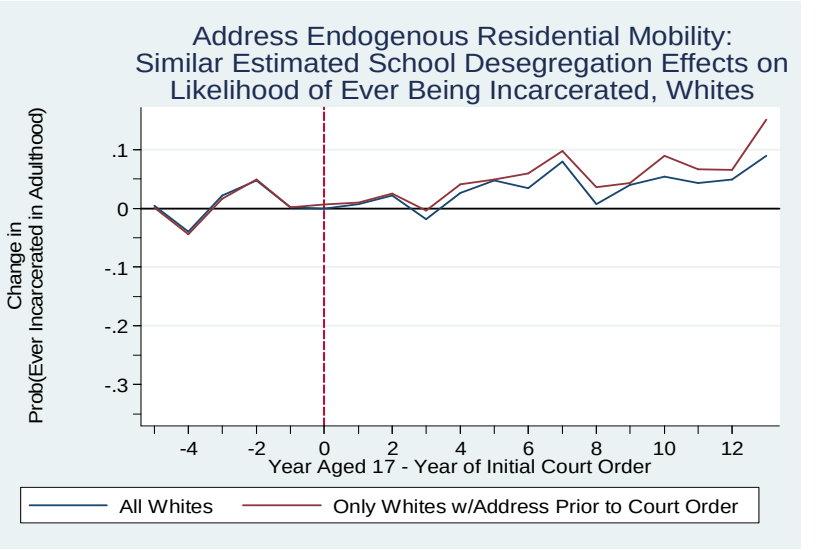


FIGURE D6b



Appendix Table E1. Falsification Tests Using Unsuccessful Desegregation Court Litigation: Placebo Effects on Adult Outcomes, by Race

	Dependent variable:							
	Probability (High School Graduate)	**Years of Education**	**Occupational Prestige Index**	**Probability (Ever Incarcerated)**	**Ln(Wage),** ages 20-50	**Ln(Family Income),** ages 20-50	**Probability (Poverty),** ages 20-50	**Adult Health Status Index,** ages 20-50
Years of Exposure to Unsuccessful Desegregation Court Litigation$_{(age\ 5\text{-}17)}$	0.0031	-0.0137	-0.2906	-0.0001	-0.0076	-0.0177	0.0046	0.0240
	(0.0044)	(0.0247)	(0.2561)	(0.0029)	(0.0056)	(0.0112)	(0.0039)	(0.1267)
Years of Exposure to Unsuccessful Desegregation Court Litigation*White	-0.0008	0.0182	0.2951	0.0009	0.0061	0.0144	-0.0059	-0.0086
	(0.0036)	(0.0267)	(0.3261)	(0.0012)	(0.0076)	(0.0132)	(0.0040)	(0.1472)
Number of person-year adult observations	--	--	--	--	54,139	72,191	72,191	54,139
Number of individuals	6,921	6,921	6,341	6,341	6,014	6,570	6,570	6,014
Number of childhood families	2,816	2,816	2,938	2,938	2,607	2,723	2,723	2,607
Number of school districts	613	613	602	602	591	613	613	591
Number of childhood counties	437	437	428	428	427	437	437	427

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

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

Sample includes all PSID individuals born between 1945-1968, followed into adulthood through 2013, who grew up in school districts that had desegregation court litigation at some point b/w 1954-90 (desegregation court case data, American Communities Project). All models include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Results in this table demonstrate that timing of UNSUCCESSFUL court litigation is unrelated to adult attainment outcomes; only the timing of initial year of successful litigation that led to court-ordered school desegregation is significantly associated with black's adult socioeconomic & health attainments (see Figures 5-14).

Appendix F: Exploring Potential Mechanisms

To attempt to identify the potential mechanisms, I isolate for every district the desegregation-induced change in per-pupil spending and racial school integration, respectively, independent of district-specific trends and other coincident policies. For each district, I compute the change in school district per-pupil spending (school segregation) induced by the court order from the year preceding enactment to the first several years following it.. The district-specific changes in per-pupil spending and racial integration resultant from court-ordered desegregation are interpreted as markers for the intensity of treatment. In order to further assess the relative roles of school resources and peer effects as potential mechanisms underlying the desegregation effects, I estimate parametric event study models of the form:

(F1)

$$Y_{idb} = \theta_0^r\left(t_{idb} - T_d^*\right)\cdot D_{db}1\left(t_{idb} - T_d^* < 0\right)\cdot SPEND_d + \theta_1^r\left(t_{idb} - T_d^*\right)\cdot D_{db}1\left(t_{idb} - T_d^* < 0\right)\cdot SEG_d$$
$$+ \theta_2^r\left(t_{idb} - T_d^*\right)\cdot D_{db}1\left(0 \le t_{idb} - T_d^* \le 12\right)\cdot SPEND_d + \theta_3^r\left(t_{idb} - T_d^*\right)\cdot D_{db}1\left(0 \le t_{idb} - T_d^* \le 12\right)\cdot SEG_d$$
$$+ \theta_4^r\left(t_{idb} - T_d^*\right)\cdot D_{db}1\left(t_{idb} - T_d^* > 12\right)\cdot SPEND_d + \theta_5^r\left(t_{idb} - T_d^*\right)\cdot D_{db}1\left(t_{idb} - T_d^* > 12\right)\cdot SEG_d$$
$$+ X_{idb}\beta + Z_{db}\gamma + (W_{1960d} * b)\phi^r + \eta_d^r + \lambda_b^r + \varphi_g^r * b + \varepsilon_{idb}$$

where t_{idb} is the year the individual from school district *d* turned age 17; T_d^* is the year of the initial court order in school district *d*; $SPEND_d$ is the desegregation-induced change in per-pupil spending in district *d*; SEG_d is the desegregation-induced change in racial school segregation among students in district *d* (as measured by the black-white exposure index); with the inclusion of the same set of controls as previously discussed in Section IV.[i] The terms used in the specification to capture the duration of desegregation exposure is simplified to improve precision in this expanded model (which is supported by the earlier desegregation results reported which were roughly linear in school-age exposure years to a first approximation). This can be viewed as a triple-difference strategy that compares the difference in outcomes between treated and untreated cohorts within districts (variation in exposure) and across districts with larger or smaller changes in school spending due to desegregation (variation in intensity). The event study framework allows one to inspect whether districts that underwent larger changes in school spending (segregation) resultant from desegregation exhibited differential trends in outcomes preceding the enactment of court orders, which I use as an additional specification test.

The results are presented in Table F1. For blacks' educational, economic and health attainments, the results suggest that changes in school quality resulting from integration played an important role. The results indicate significant interactive effects of school desegregation exposure with the resultant change in access to school quality, as proxied by changes in per-pupil spending. I find that court-ordered desegregation that led to larger improvements in school quality resulted in more beneficial educational, economic, and health outcomes in adulthood for blacks who grew up in those court-ordered desegregation districts. These significant effects persist after the inclusion of corresponding increases in the black-white exposure index that accompanied desegregation. Importantly, I find no evidence that districts that underwent larger changes in school spending resultant from desegregation exhibited differential trends in outcomes preceding the enactment of court orders, which provides additional support for the identification strategy. On the other hand, there is suggestive evidence that reductions in school segregation levels that were not accompanied by significant changes in school resources did not have equally large impacts on blacks' adult attainments. In general, the magnitudes of the desegregation impacts across the various adult outcomes for blacks were insensitive to how

much reduction in racial school segregation resulted from court orders. Interestingly, once again I find no effects on whites in either the duration of desegregation exposure nor the resultant change in school resources.

In order to summarize the results on the mechanisms, I estimate 2SLS models in which the key explanatory variables of interest—average per-pupil spending experienced during one's school-age years and the average level of racial school integration (i.e., the average black-white exposure index during ages 5-17)—are predicted in a first-stage model using the individual's duration of desegregation exposure interacted with the respective school district's desegregation-induced change in school spending (segregation). The 2SLS models are presented in Table F2 for the main adult attainment outcomes, and include the same set of controls as the prior models, estimated separately by race. These estimates are not intended to be interpreted as the causal impacts of school spending per se, but rather as markers of the intensity of treatment that may capture the combined effects of improvements in school resources and teacher quality.

To facilitate interpretation of marginal effects, the units of the average per-pupil spending during an individual's school-age years are in thousands of dollars—thus, a 1-unit change represents a $1,000 change in spending (2000 dollars) in each of one's K-12 years. In similar fashion, the key school segregation variable is defined such that a one-unit increase in "Black-White Exposure Index *(age 5-17)*" represents a 0.15 increase in the black-white exposure index or a standard deviation increase in racial school integration experienced in each of one's K-12 years.[ii] The 2SLS results highlight significant positive effects of desegregation-induced increases in school spending on blacks' adult attainments. In contrast, these 2SLS models reveal small, insignificant effects for increases in racial integration (holding spending changes constant). As a placebo falsification test using the 2SLS models, it is shown in Table F3 that school spending increases have no significant impacts on adult outcomes when they occur during non-school ages (after age 19), but rather all the estimated long-run effects of per-pupil spending are confined to school-age years of exposure, as we would expect. The results for blacks indicate that a $1,000 increase in school spending (which corresponds to roughly a 25-30 percent increase) experienced throughout one's school-age years is associated with an additional 1.4 years of completed education, a 58 percent increase in wages, an increase of $18,635 in annual family income, a 34 percentage-point reduction in the annual incidence of adult poverty, and a 2.1 percentage-point reduction in the annual incidence of adult incarceration. These magnitudes are similar to the previously discussed event study results (Figures 5-13) in comparisons of individuals exposed to desegregation beginning in one's elementary school years relative to growing up in segregated schools throughout one's school years. There are no corresponding significant effects for whites on either of these markers of the intensity of treatment across the adult outcomes.

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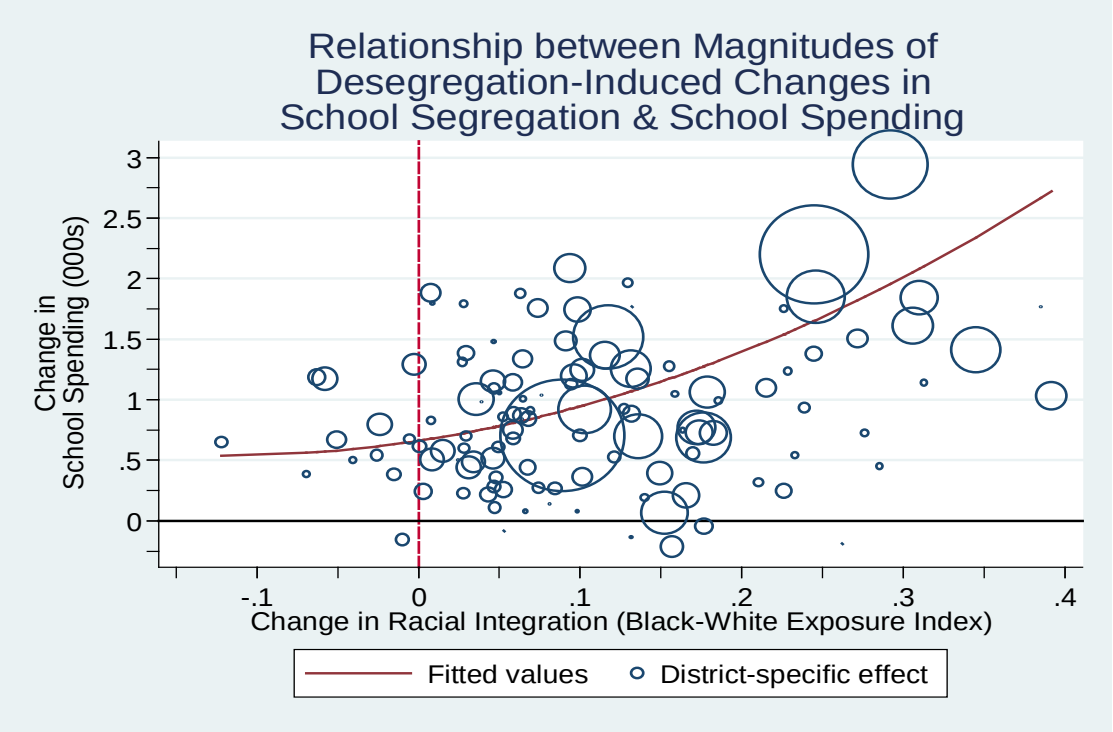
[i] The estimated equation also includes the main effects without the interaction terms in school spending and segregation; equation (F1) abstracts from this to ease the number of terms shown. The school spending and segregation terms are centered around the average desegregation-induced changes ($1,000 for per-pupil spending; 0.15 for black-white exposure index), so that the coefficient on the main desegregation exposure term represent the desegregation impact for the average change in these key school inputs. These models use the same sample as the aforementioned ones but include dummy indicators if district-specific desegregation induced-changes in per-pupil spending (school segregation) cannot be computed because of missing data; the occurrence of missing data occurs most often in small, rural areas.

[ii] The excluded instrument for this school spending (segregation) variable is the number of school-age-years of desegregation exposure interacted with the respective school district's desegregation-induced change in school spending (segregation).

FIGURE F1.



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

Table F1. Interactive Effects of Court-Ordered School Desegregation & Induced-Change in Per-Pupil Spending on Educational Attainment, by Race

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

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

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

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

Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. All models include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). The models include dummy indicators for each event study year < -7 and each event study year > 13 -- the coefficients on these vars are suppressed to conserve space.

Table F2. Exploring the Mechanisms: School Spending vs Racial School Integration.
2SLS Estimates of Desegregation-Induced Effects of Per-Pupil Spending on Adult Socioeconomic Attainments by Race.

	Second Stage, Dependent variable:									
	Years of Education		**Ln(Wage),** age 20-50		**Annual Family Income,** age 20-50		**Annual Incidence of Adult Poverty: Prob(Poverty),** age 20-50		**Annual Incidence of Incarceration: Prob(Incarceration),** age 18-30	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Blacks	**Whites**	**Blacks**	**Whites**	**Blacks**	**White**	**Blacks**	**Whites**	**Blacks**	**Whites**
School District Per-pupil Spending$_{(age\ 5\text{-}17)}$	**1.4475***	0.1619	**0.6602***	-0.1851	**18,634,65***	17,045.85	**-0.3399***	-0.0758	**-0.0212***	-0.0102
	(0.8963)	(0.8841)	(0.3786)	(0.2301)	(9,183.16)	(21,066.61)	(0.1984)	(0.0594)	(0.0118)	(0.0166)
Black-White Exposure Index$_{(age\ 5\text{-}17)}$	-0.2810	-0.4774	-0.2952	0.1889	-8,077.57	6,608.08	0.2033	0.0713	-0.0093	0.0136
	(0.7693)	(1.6172)	(0.4580)	(0.3910)	(11,412.88)	(42,839.11)	(0.2395)	(0.0821)	(0.0119)	(0.0095)
Number of person-year observations	--	--	18,435	16,063	26,863	31,100	26,863	31,100	39,032	31,016
Number of individuals	4,291	2,611	2,289	1,651	2,630	2,611	2,630	2,611	2,581	1,920
Number of childhood families	1,458	1,328	904	878	966	1,328	966	1,328	792	896
Number of school districts	274	326	192	265	198	326	198	326	132	290

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

Data: Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. The estimated district-specific desegregation induced-change in per-pupil spending (school segregation) are net of school district fixed effects, district-specific time trends, & coincident policy changes (see also Figures 1B, 3A). The key (instrumented) variables are defined such that a one-unit increase in "School District Per-pupil Spending$_{(age\ 5\text{-}17)}$" represents a $1,000 spending increase in each of one's K-12 years (roughly a standard deviation increase); and a one-unit increase in "Black-White Exposure Index $_{(age\ 5\text{-}17)}$" represents a 0.15 increase in the black-white exposure index or a standard deviation increase in racial school integration experienced in each of one's K-12 years.

Models: The first-stage models, which are highly significant, include as predictors the # of school-age years of exposure to desegregation interacted with the respective district's desegregation-induced changes in school spending and racial school segregation, respectively; these are the excluded instruments for the school spending and segregation variables. Results are based on 2SLS models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight) and controls for gender, age (cubic).

Table F3. Exploring the Mechanisms. 2SLS Estimates of Desegregation-Induced Effects of Per-Pupil Spending on Black's Adult Socioeconomic Attainments: Placebo Tests for non-school ages

	Blacks Second Stage, Dependent variable:				
	Years of Education	**Ln(Wage),** age 20-50	**Annual Family Income,** age 20-50	**Annual Incidence of Adult Poverty: Prob(Poverty),** age 20-50	**Annual Incidence of Incarceration: Prob(Incarceration)** age 18-30
	(1)	(2)	(3)	(4)	(5)
School District Per-pupil Spending$_{(age\ 5\text{-}17)}$	**1.1841*** **	**0.5176** **	**13,732.27+**	**-0.2796***	**-0.0170+**
	(0.4522)	(0.2611)	(9065.39)	(0.1529)	(0.0126)
School District Per-pupil Spending$_{(age\ 20\text{-}24)}$	-0.4702	-0.0255	-16,010.87***	0.1740***	-0.0056
	(0.3285)	(0.1538)	(4,457.67)	(0.0538)	(0.0072)
Number of person-year observations	--	17,654	24,839	24,839	38,701
Number of individuals	3,951	2,204	2,457	2,457	2,565
Number of childhood families	1,341	875	916	916	781
Number of school districts	202	147	147	147	118
Number of childhood counties	138	102	102	102	63

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

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

Data: Sample includes all PSID black individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. The estimated district-specific desegregation induced-change in per-pupil spending is net of school district fixed effects, district-specific time trends, and coincident policy changes (see also Figure 3). The key (instrumented) variables are defined such that a one-unit increase in "School District Per-pupil Spending$_{(age\ 5\text{-}17)}$" represents a $1,000 spending increase in each of one's K-12 years (roughly a standard deviation increase).

Models: The first-stage models, which are highly significant, include as predictors the # of years of exposure to desegregation (for relevant ages 5-17; 20-24) interacted with the respective district's desegregation-induced change in school spending; these are the excluded instruments for the school spending variables. Results are based on 2SLS models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight), and controls for gender, age (cubic)).

Appendix G: *Supplementary Regression Results & Validating the PSID Results Using Other Data*:

While the tests thus far show that the estimates are internally valid, readers might wonder how these patterns generalize to districts that are not included in the PSID. To address this, I replicated the analyses for high school graduation using the combined Office of Civil Rights (OCR) data and Common Core Data (CCD)—Local Education Agency Universe Survey and Non-Fiscal Survey Database—for all school districts in the US, which together span the period 1972 to 1999. I combine the district-level data and focus on dropout rates (grades 9-12) because this is the most reliable data that can be compared across time. I focus on districts ever under court order with the preferred research design.

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

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

I estimate a parametric event study model with event study years interacted with the desegregation-induced changes in school spending and racial segregation, respectively (results presented in Appendix Table G2). First, I find that districts that underwent larger changes in school spending resultant from desegregation exhibited increasing high school dropout rates in the years *preceding* the enactment of court orders. The results show that this pre-existing trend was subsequently reversed in districts in which desegregation led to significant increases in per-pupil spending. In particular, the results indicate that a $1,000 increase in per-pupil spending is associated with a 5-percentage point reduction in high school dropout rates in the first five years following desegregation. Note that this estimate is not directly comparable to that from the PSID sample because this estimate is based on annual spending at the district level, not the cumulative effect of a sustained spending increase (experienced at the student level) for all 12 years of a student's life. Because we expect the latter to be much larger, the results from the OCR- CCD data are consistent with those from the PSID. The results suggest that high school dropout rates were insensitive to how much reduction in racial school segregation results from court orders. In this respect as well, the findings reveal similar patterns with my main PSID results.

Appendix Table G1. Replicating Guryan (*AER*, 2004) using PSID.
Identification from Timing of Initial Court Orders (exogenous) vs Timing of Major Desegregation Plan Implementation:
Effects of Court-Ordered School Desegregation on Educational Attainment, by Race

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

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

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

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

Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2011, who grew up in school districts that were ever subject to court-ordered desegregation. All models include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Models include dummy indicators for each event study year <-7 (columns 3-6) and each event study year > 15 (columns 4-6) -- the coefficients on these vars are suppressed to conserve space. See corresponding non-parametric & parametric event study model results presented in Figure 1.

Appendix Table G2. Using OCR-CCD District-level Data to Explore the Mechanisms: School Spending vs Racial School Integration. 2SLS Estimates of Desegregation-Induced Effects of Per-Pupil Spending on High School Dropout Rates.

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

Appendix Table G3. Additional Specifications: Similar Estimated Effects of Desegregation in South and Non-South

	Dependent variable:
	Years of Education
(Main Effects apply to Blacks)	
Years of Exposure to Court-Ordered Desegregation$_{(age\ 5\text{-}17)}$	**0.1049****
	(0.0424)
Years of Exposure to Court-Ordered Desegregation$_{(age\ 5\text{-}17)}$*South	-0.0108
	(0.0528)
Years of Exposure to Court-Ordered Desegregation$_{(age\ 5\text{-}17)}$*White	-0.0618
	(0.0501)
Years of Exposure to Court-Ordered Desegregation$_{(age\ 5\text{-}17)}$*South*White	-0.0391
	(0.0494)
Number of individuals	8,548
Number of childhood families	3,562
Number of school districts	631

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

Model includes same sample and set of control variables as main results.

Appendix Table G4. Effects of Desegregation Exposure on Blacks' Adult Outcomes & the Returns to Education

	Years of Education	**Ln(Wage),** age 20-50	**Annual Work Hours,** age 20-50	**Probability (Poverty),** age 20-50	**Annual Family Income,** age 20-50	**Occupational Prestige Index**	**Probability (Ever Incarcerated)**	**Probability (Incarcerated),** age 18-30	**Adult Health Status Index,** age 20-50
5-Year Exposure to Desegregation	**0.4800****	**0.1516*****	**164.5327****	**-0.1101****	**5,893.032****	**5.1932****	**-0.1420*****	**-0.0147****	**3.3401*****
	(0.1905)	(0.0506)	(76.4113)	(0.0470)	(2,695.461)	(2.2841)	(0.0378)	(0.0065)	(1.2434)
Implied Wald Estimate of Returns to Education (quantity/quality)	--	0.3158	342.7765	0.2294	$12,277	10.8192	0.2958	0.0306	6.9585
Mean for Blacks (at age 30)	12.60	2.26	1,540.06	0.24	$31,020	34.42	0.08	0.0063	84.16
Mean for Whites (at age 30)	13.51	2.63	1,895.99	0.05	$52,937	48.57	0.04	0.0014	88.78

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

This sumary table contains the main results for blacks based on event study estimates shown in Figures 5-14. Sample includes all PSID individuals born 1945-1968, followed into adulthood through 2013, who grew up in school districts that were ever subject to court-ordered desegregation. All models include: race-specific school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender) and age (cubic).