Long-run Impacts of School Desegregation and School Quality on Adult Health

Rucker C. Johnson*
Goldman School of Public Policy
University of California, Berkeley

December 2009

* Please direct correspondence to Rucker Johnson, University of California, Berkeley, Goldman School of Public Policy, 2607 Hearst Ave, Berkeley, CA 94547, or email to ruckerj@berkeley.edu. I wish to thank John Logan (Brown University, American Communities Project) for sharing data on school desegregation court cases, Sarah Reber for sharing the Office of Civil Rights school data, and the PSID staff for access to the confidential restricted-use PSID geocode data.

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

ABSTRACT

This paper investigates the extent and ways in which childhood school quality factors causally influence later-life health outcomes. The study analyzes the health trajectories of children born between 1950 and 1975, and followed through 2007, using the Panel Study of Income Dynamics (PSID), spanning four decades linked with multiple data sources containing detailed neighborhood attributes and school quality resources that prevailed at the time these children were growing up.

I estimate the long-run impacts of court-ordered school desegregation plans on later-life health by exploiting quasi-random variation in the timing and scope of desegregation implementation during the 1960s, 70s, and 80s. I find school desegregation significantly narrowed black-white adult health disparities for the cohorts exposed to integrated schools during childhood. The analysis disentangles the effects of neighborhood and school quality. Difference-in-differences estimates and sibling-difference estimates indicate that school desegregation and accompanied increases in school quality resulted in significant improvements in adult health for blacks. The results suggest the mechanism through which school desegregation led to beneficial health outcomes in adulthood for blacks include the significant improvement in access to school resources reflected in reductions in class size and increases in per-pupil spending. The results highlight the significant impacts of educational attainment on future health status, and point to the importance of school quality in influencing socioeconomic mobility prospects, which in turn have far-reaching impacts on health. Taken together, the study finds that racial differences in adult health can be accounted for by childhood family, neighborhood, and school quality factors.

I. INTRODUCTION

Racial segregation that results in race differences in access to school quality has often been cited as a culprit in perpetuating inequality in attainment outcomes. Since the landmark 1954 Supreme Court *Brown v. Board of Education* decision and subsequent court-ordered implementation of school desegregation plans during the 1960s, 70s and 80s, scholars have investigated the consequences of school desegregation on socioeconomic attainment outcomes of black children (Clotfelter, 2004). Studies since the Coleman Report (Coleman, 1966) have focused primarily on black-white differences in academic outcomes, such as test scores and educational attainment, and attempted to assess roles of schools and family background in contributing to these racial disparities (see, e.g., Ferguson, 1998, and the review by Schofield, 1995). Despite the unprecedented changes that accompanied desegregation, no large-scale data collection effort was undertaken to investigate school desegregation program effects, particularly on longer-run outcomes. There are not any desegregation experiments that have followed students over a long time horizon beyond one's early 20s.

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

At the same time, education has been shown to be one of the strongest correlates of health status, and this is true across generations (e.g., Cutler and Lleras-Muney, 2006). Large gaps in morbidity and mortality between more- and less-educated individuals have been documented in numerous countries and have held true across time. Relatedly, recent evidence has highlighted the central role that school quality plays in shaping subsequent socioeconomic mobility prospects (Johnson, 2009), which may in turn affect adult health.

This paper investigates the extent and ways in which childhood school quality factors causally influence later-life health outcomes. The primary difficulty in disentangling the relative importance of childhood family, neighborhood, and school quality factors is isolating variation in neighborhood and school quality characteristics that are unrelated to family factors.

This paper addresses these issues and investigates the long-run consequences of dimensions of childhood school quality and neighborhood on adult health status. The paper uses the longest-running US nationally-representative longitudinal data spanning four decades linked with multiple data sources containing detailed neighborhood attributes and school quality resources that prevailed at the time these children were growing up. The study analyzes the health trajectories of children born between 1950 and 1975 followed through 2007.

The data and historic policy shift with the enactment of court-ordered school desegregation during the childhood period of these birth cohorts, provide a unique opportunity to evaluate long-run impacts of ground-breaking legislation designed to improve school resources for minority children. To this end, I have obtained a comprehensive desegregation case inventory spanning the period from 1954-1990 that contains detailed information on each school district of whether there was a court-ordered desegregation plan, the year of the initial court order, and the type of desegregation court order. This desegregation case data was compiled by The American Communities Project at Brown University.

The analysis proceeds in two stages. I first present new evidence of how court-ordered school desegregation influenced the quantity and quality of educational inputs received by minority children. I find strong evidence that desegregation plans were effective in narrowing black-white school resource gaps of per-pupil school spending and class size and decreasing school segregation (though white flight thwarted some of the integration and leveling up of school resources over time). I then assess the effects of the court-ordered desegregation plans of public schools on adult health outcomes. I exploit the wide variation in the timing and scope of implementation of desegregation plans to identify their effects.

As an alternative empirical strategy, I use sibling comparisons to identify the effects of school quality and school desegregation on adult health. This use of sibling models follows the research design previously utilized by Altonji (1996) to analyze the effects of school quality on wages. I estimate within-family effects of school quality inputs on later-life health. Sibling fixed effect models have the advantage of explicitly accounting for observed and unobserved between-family endowment and resource heterogeneity that often plague OLS estimates. I exploit policy-induced changes in per-pupil spending and school resources that are unrelated to child family- and neighborhood-level determinants of adult health status. This identification strategy compares the adult health of individuals who were exposed to integrated schools during childhood with the corresponding adult health of their siblings (evaluated at the same age) who grew up in the same communities but who had already reached age 18 prior to the desegregation plan implementation or who were exposed to integrated schools for only a limited period of their childhood, conditional on year of birth effects.

The empirical analysis makes three unique contributions by investigating: (1) non-racial integration aspects of court-ordered desegregation through its impacts on per-pupil spending; (2) the effects of court-ordered desegregation plans of public schools on adult health outcomes and attempts to separately identify the effects of neighborhood and school quality; and (3) the role of childhood school and neighborhood quality in contributing to socioeconomic and racial health disparities in adulthood.

It is hypothesized that school desegregation may have long-run impacts on the adult health status of African Americans through several potential mechanisms: (1) school quality resource effects (e.g., the distribution and level of per-pupil spending, class size, teacher quality); (2) peer exposure effects (e.g., children in classrooms with highly motivated and high-achieving students are likely to perform better due to positive spillover effects on other students in the classroom); and (3) effects on parental, teacher, and community-level expectations of child achievement. The long-run effects of each of these hypothesized mechanisms operate in part on how they influence the quality and quantity of educational attainment and adult economic status. For example, attending schools with a high concentration of children from poor families may reduce the school's capacity to provide quality instruction and may expose students to negative peer pressure that lowers their academic performance. Integration may also influence long-term health outcomes in ways that are unrelated to academic achievement and educational outcomes.

Because I am able to observe individuals in their 30s, 40s, and into their 50s, I am able to examine the effects of child school quality resources on adult health status through mid life, and also see if the effects are stronger at later ages than earlier ages. If effects of school quality on adult health status operate through their effects on the socioeconomic mobility process (e.g., via effects on educational attainment and adult economic status), then we would expect the effects to become more pronounced over the course of adulthood. The data and methods improve upon prior research, which lacked access to long-term individual panel data and relied on aggregate state-level analyses.

The remainder of the paper is organized in the following way. I begin with a brief history of school desegregation litigation. The next section provides an overview of related studies of the effects of segregation and school quality and discusses methodological challenges in estimating school effects. The data and measures are described in section III. Section IV discusses the empirical strategy, econometric model, and estimation methods. The results are presented in section V, with concluding statements provided in the final section.

II. BRIEF HISTORY OF SCHOOL DESEGREGATION & RELATED STUDIES

Background. Residential segregation may affect access to quality schools and subsequent mobility prospects through their effects on 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…), which was generated by 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).

Racial disparities in school resources were compounded by racial school segregation within districts prior to the enactment of desegregation plans (especially in the South). Before school desegregation plans were enacted, school district spending, particularly in the South, was directed disproportionately to the majority-white schools within districts, which will not be reflected in the district-level spending data. This may result in substantial measurement error in actual per-pupil spending resources available to blacks prior to the enforcement of these desegregation plans. This is the likely reason that, for blacks, I find school district spending has no appreciable relationship with adult health or socioeconomic attainments until birth cohorts who reached school-age after school desegregation plans were in effect (especially in the South).

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. School desegregation did not begin in earnest in the South until after 1964, and a significant share occurred over the five-year period between 1968 and 1972. The passage of the 1964 Civil Rights Act prohibited federal aid to segregated schools and allowed the Justice Department to join suits against school districts that were in violation of the Brown vs. Board order to integrate. This resulted in a significant drop in the extent of racial school segregation thereafter reinforced by the actions of local Federal courts. Thus, there is a sharp post-1964 discontinuity in school desegregation.

Small school districts in the South began to desegregate in increasing number in the 1960s after the Federal government threatened to withhold Title I funds (Cascio *et al*., 2007). Larger school districts in the South began desegregating in significant volume after the Supreme Court 1968 ruling in *Green vs. New Kent County, Virginia* (391 U.S. 430) (Weiner *et al*., 2008). By the end of the seven-year period, 1965-1972 when court action was at its peak, southern schools became the least segregated in the country. School districts in other regions of the country 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.[1] Desegregation cases began to expand explicit goals beyond racial integration to include goals of promoting adequacy of school funding for minority

[1] 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.

student achievement. The 1977 Milliken II decision allowed courts to mandate spending on compensatory educational programs for minority students (Orfield and Eaton, 1996). This occurred in Los Angeles and Detroit, for example. No other important court decisions occurred between 1975 and 1990.

In sum, there exists substantial variation in the timing and intensity of school desegregation efforts. A substantial portion of school districts adopted desegregation plans only after court order (or the threat of them) due to individual cases filed in local Federal court. 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. The strategic process of seeking to establish legal precedence pursued by the NAACP and the diverse set of agents who initiated the litigation process make it unlikely that the precise timing of court-orders are a function of school district and community characteristics and preferences. There was an idiosyncratic nature of the timing of mandated desegregation plan implementation. There was also an element of randomness in the length of time it took for initial litigation to result in court-ordered desegregation plan implementation (e.g., decisions may be appealed, adding further variableness to the date of actual implementation).

Related Studies. There is a large body of literature that examines the effects of school spending on academic performance and educational attainment (see e.g., Hanushek (1997), Hedges, Greenwald, and Laine (1994) for reviews). Evidence is mixed on the extent school resources matter. Fewer studies have evaluated the effectiveness of schools by analyzing effects on labor market outcomes. An important limitation of most recent studies that find insignificant results focusing on the effects of school quality on labor market outcomes using longitudinal individual-level data is that earnings are observed at relatively young ages (the average age is around 23 years old). Based on these factors Card and Krueger (1996) in their summary of the research evidence conclude, "Our review of the literature reveals a high degree of consistency across studies regarding the effects of school quality on student's subsequent earnings. The literature suggests that a 10 percent increase in school spending is associated with a 1 to 2 percent increase in annual earnings for students later in their lives" (p. 133).

In recent years, economists have expanded this more traditional focus to consider whether and how school segregation influence adult labor market outcomes (Vigdor, 2006; Ashenfelter, Collins, and Yoon, 2005; Boozer, Krueger, and Wolkon, 1992; Grogger, 1996) and criminal involvement (Weiner, Lutz, and Ludwig, 2008). An early forerunner of this literature is Coleman (1966), who found a negative correlation between black students' test scores and the black enrollment share at their schools; these results were often interpreted as evidence of the role of segregation. This original work has been criticized for not being able to address the selection bias associated with the systematic sorting of students to different types of schools.

Among the studies that have tried to address endogeneity and self-selection using non-experimental methods, one approach focused on uncovering effects of school peers has used variation in minority exposure of different cohorts at the same school (Hoxby, 2000; Hanushek et al., 2002). This empirical strategy compares the outcomes of successive cohorts at the same school to identify peer effects and rests on the assumption that sorting is based on permanent school characteristics, and is independent of cohort-specific racial composition differences. These studies find significant negative effects of exposure to black classmates. It remains unclear whether race is merely correlated with other characteristics such as educational quality or other peer attributes. It is difficult to disentangle separate roles of peer race, peer income (parental SES), and peer achievement in desegregation effects. Studies tend to show that benefits of advantaged peers for whites are smaller than the benefits for minorities, particularly high-performing minorities (Hanushek, Rivkin, & Kain, 2002; Hoxby, 2000; Cooley, 2006). Minorities also tend to be influenced more by the achievement of same-race peers. Blacks benefit from having more blacks, after controlling for peer achievement (Vigdor and Nechyba (forthcoming)). However, other evidence shows that the apparently positive effects of advantaged peers disappear once unobserved differences in teacher quality are taken into account (teachers generally more willing to teach advantaged students). Prior studies suggest apparent positive effects of integration on achievement are due to the achievement and parental SES of peers rather than peer race. Card and Rothstein (2005) find no evidence that relative exposure to black students impacts black student performance.

Methodological Challenges in Estimating Effects of Schools and Segregation. The primary methodological challenge in estimating the causal effects of school quality and segregation during childhood on adult health status is that unobserved factors that affect health may also be correlated with school quality factors, leading to biased estimates of school and/or segregation effects. This can arise from the endogeneity of residential location. That is, individuals and families choose where they live based on the characteristics they value (Tiebout, 1956). In this context, parents who care more about the health or well-being of their children will be less likely to choose to live in an area with poor quality schools, high crime, pollution, or a poor health care system. It is important to bear in mind, however, that African Americans, particularly in the South prior to 1964, are less subject to bias arising from the endogenous selection of families into neighborhoods due to the substantial residential location constraints they faced because of school segregation, racial discrimination and exclusionary zoning. The point remains, however, that the set of complex and nuanced characteristics that influence neighborhood choices are not likely to be well measured or accounted for appropriately in econometric models.

Furthermore, inadequate controls for childhood family and neighborhood characteristics can lead to omitted variable bias of estimated school effects. In their summary of the school literature, Card and Krueger echo this general concern, “In our view, the most important omitted variables [in previous studies] are likely to be measures of family background and characteristics of the areas in which individuals attended school” (p. 113).

A stream of this literature that utilizes an identification strategy that addresses the non-random sorting of children to schools takes advantage of quasi-experimental variation induced by policy changes. The study most directly related to the approach taken in this paper is Guryan (2004), who uses variation in the scope and timing of major court-ordered desegregation plans in the 1970s and 1980s to identify the effect of school segregation on black high school dropout rates. Using data from the 1970 and 1980 censuses, he uses difference-in-difference and fixed effect methods and finds that desegregation explains ½ of the decline in the black high school dropout rate during the 1970s among the 125 large school districts he analyzed that were subject to such orders over that time period.

This paper also builds on the findings of a recent paper by Johnson (2009) that investigates the extent and ways in which childhood family and neighborhood quality (including effects emanating from school quality) causally influence later-life health outcomes. Following birth cohorts over their life using the same PSID data as the present paper, estimates of sibling and child neighbor correlations in health were used to bound the proportion of inequality in health status in childhood through mid life that are attributable to childhood family, neighborhood and school quality. The results consistently showed a significant scope for both childhood family and neighborhood background (including school quality). The results imply substantial persistence in health status across generations that are linked in part to low intergenerational economic mobility. The results suggest that three-fifths of adult health disparities may be attributable to family and neighborhood background. While the within-family resemblance in adult health was significantly stronger than the within-child-neighbor resemblance, the child neighbor resemblance was quite substantial. The upper bound estimate on the potential scope of child neighborhood/school influences for health trajectories was substantial (net of the similarity arising from similar family characteristics). The results suggest that disparities in neighborhood background account for between one-third and 40 percent of the variation in health status in mid life. Taken together, the results contained in Johnson (2009) indicate that the composite neighborhood and school quality effects reflected in the significant child neighbor correlations in adult health appear to emanate from the direct effects of neighborhood quality during childhood on child health that may carry over into adulthood, as well as indirect school/neighborhood effects via their influence on the socioeconomic mobility process.[2]

III. DATA AND MEASURES

The primary data utilized is the restricted, confidential geocoded version of the PSID (1968-2007) with identifiers at the neighborhood block level in which children grew up. I then merge on an array of neighborhood and school information from multiple data sources that prevailed in the 1960s and 1970s when these children were growing up. This includes an extensive set of measures from 1960 and 1970 Census data, 1962 Census of Governments, and

[2] The estimated health effects from a one standard deviation change in the index of neighborhood/school environment (captured by the neighborhood random effects from the four-level hierarchical models estimated in that paper) provide a useful comparison to discuss effect sizes of the school attributes in the present paper (discussed further in the results section).

Common Core data (CCD) compiled by the National Center for Education Statistics, as well as a comprehensive case inventory of court litigation regarding school desegregation over the entire 1955-1990 period obtained from The American Community Project at Brown University.

The PSID began interviewing a national probability sample of families in 1968. These families were re-interviewed each year through 1997, when interviewing became biennial. All persons in PSID families in 1968 have the PSID "gene," which means that they are followed in subsequent waves. When children with the "gene" become adults and leave their parents' homes, they become their own PSID "family unit" and are interviewed in each wave. This sample of "split offs" has been found to be representative (Fitzgerald, Gottschalk and Moffitt, 1998). Moreover, the genealogical design implies that the PSID sample today includes numerous adult sibling groupings who have been members of PSID-interviewed families for nearly four decades.

Measurement of Health. The key adulthood health outcome examined is general health status (GHS). The general health status question is: "Would you say your health in general is excellent, very good, good, fair, or poor?" This question was asked of household heads and wives (if present) in each survey between 1984-2007, and was asked of all family members in 1986.[3] GHS is highly predictive of morbidity measured in clinical surveys, and it is one of the most powerful predictors of mortality, even when controlling for physician-assessed health status and health-related behaviors. (For reviews of this extensive literature, see Idler and Benyamini (1997) and Benyamini and Idler (1999).) GHS is also frequently used as a global measure of health status.

In order to scale the GHS categories, I use the health utility-based scale that was developed in the construction of the Health and Activity Limitation index (HALex). (A

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

discussion of the various options for treatment of the GHS variable is described in Appendix A.) The HALex scores associated with GHS categories are based on the U.S. National Health Interview Survey, which contains a fuller health instrument than utilized in the PSID. A multiplicative, multi-attribute health utility model was used to assign scores and quantify the distance between the different GHS categories. The technical details of the scaling procedures are discussed at length elsewhere (Erickson, Wilson, Shannon, 1995; Erickson, 1998). Thus, using a 100-point scale where 100 equals perfect health, the interval health values associated with GHS used in this paper are: [95, 100] for excellent, [85, 95) for very good, [70,85) for good, [30,70) for fair, and [1,30) for poor health. Consistent with previous research, the skewness and nonlinearity of this scaling is reflected in the fact that the "distances" between excellent health, very good health, and good health are smaller than between fair and poor health. This scaling is currently used by the National Center for Health Statistics to estimate health-related quality of life measures and years of healthy life (*Healthy People* 2000). I then estimate all of the regression models of health status using the interval regression method. While the HALex approach with interval regressions is superior to alternatives, as described in the appendix, I have also estimated identical models to those reported in the tables but using poor/fair health as the dependent variable in a logit model.[4] The substantive conclusions are unchanged.

The selected sample consists of PSID sample members who were children when the study began and who have been followed into adulthood. Specifically, I choose PSID sample members born between 1950 and 1975, which consists of individuals who were children 0-18

[4] The key shortcoming of an ordered logit or ordered probit regression 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). They conclude "…the ordered probit regression does not allow for any sensible approximation of the true degree of inequality."

years old in one of the first five waves of interviewing. I then obtain all available information on these individuals for each wave, 1968 to 2007. Therefore, by 2007 the oldest person in the sample is 57 and the youngest is 37.[5] (A summary discussion of sample attrition issues is presented in Appendix B.)

The sample includes males and females and all analyses control for gender, given well-known differences in health status, health behaviors, and labor market outcomes for men and women. Due to the complexity of the health status changes for women during the childbearing years, I exclude self-assessed health status measures of women in the years they were pregnant. I include both the Survey Research Center (SRC) component and the Survey of Economic Opportunity (SEO) component, commonly known as the "poverty sample," of the PSID sample. I apply sample weights in all the analyses to produce nationally-representative estimates.

School Measures. I have merged to the PSID geocode data a set of school quality resource indicators for 1960-1980 (including per-pupil spending, class size) and measures of the extent of racial school segregation and school desegregation efforts. The school quality, teacher salary, and school segregation data covering the period of the 1960s and 1970s come from three sources:

(1) Office of Civil Rights (OCR) of the US Department of Health and Human Services, data for 1968-1976. 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.

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

(2) The CCD 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.

(3) This compiled history of desegregation court-orders involved the use of case dockets and bibliographies for desegregation orders from the Department of Justice, NAACP Legal Defense Fund, and the US Department of Education. This data was provided to me by John Logan of Brown University as part of his work on the American Communities Project.

The multiple sources used to compile the comprehensive desegregation case inventory assembled by the team of scholars for The American Community Project at Brown University included case dockets and bibliographies for desegregation orders from the Department of Justice, NAACP Legal Defense Fund, and the US Department of Education (Logan et al., 2008). 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, and the year of the initial court order. Following Logan et al (2008), in addition to school districts covered by formal court orders, I also define as "under court order" those districts that implemented desegregation plans in response to pressure from the US Department of Health, Education, and Welfare (HEW). The resultant case inventory is significantly more comprehensive than the one used by Welch and Light (1987). The total case inventory includes 358 court cases, which resulted in desegregation plans involving 1,057 school districts.

Sixty-three percent of original sample PSID children analyzed in this paper (i.e., 3,559 out of 5,607 children) grew up in a school district that underwent a desegregation litigation case sometime between 1950 and 1990. This subset of children represents 942 different child neighborhoods and 135 different counties in childhood (based on their childhood residence in 1968).

I merged the school district expenditures data, information on student-teacher ratios, teacher salaries, and the constructed school segregation indices, to the PSID sample by the census tract contained in the Geocode file at the 1968 survey interview. After combining data from the 5 data sources, the resulting sample used to analyze adult health contains 60,280 person-year observations from 5,607 individuals from 2,072 families, 1,468 neighborhoods, and 272 counties. The mean age is 34, with age ranging from 20 to 57, and an average of 11 observations per person. Appendix C lists the sources and years of all data elements along with details of the PSID survey questions used to construct these measures. Appendix Table C1 contains descriptive statistics for all neighborhood- and school-level measures for the sample by race.

IV. EMPIRICAL APPROACH

Estimating Effects of Court-Ordered School Desegregation on School Resources. The first stage of the analysis investigates how court-ordered school desegregation influenced the quantity and quality of educational inputs received by minority children. I measure school quality as the purchased inputs to a school. Using the staggered timing of court-ordered school desegregation plan implementation within an event study analysis (cf. Jacobson, LaLonde and Sullivan, 1993; McCrary, 2007), I quantify desegregation plan effects on school resources. In particular, I provide new evidence on non-racial integration aspects of court-ordered desegregation through its impacts on per-pupil spending and the student-teacher ratio (class size). The analysis utilizes the aforementioned school district finance data released annually by the Census Bureau.

Although cross-sectional comparisons of school districts that underwent court-ordered desegregation with districts that did not may be misleading, a newly compiled school district panel dataset allows the analysis to exploit variation in the timing of desegregation plan implementation. The event study framework in this analysis compares school district per-pupil spending, student-to-teacher ratios (class size), and school segregation levels in the years immediately after desegregation plan implementation to their respective levels that prevailed in the years immediately before plan implementation among school districts that underwent court-orders at some point during the 1960s or 70s. The analysis exploits plausibly exogenous determinants in the timing of desegregation plan implementation to estimate the impacts on school resources in the following event study framework,

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

where $Y_{c,t}$ is per-pupil spending, student-to-teacher ratio, or the segregation dissimilarity index in school district c in year t=1962,…,1982; θ_c is a set of school district fixed effects; $\gamma_{r(c),t}$ is a set of year fixed effects or region-by-year fixed effects (alternatively, allow state-specific linear time trends); and X_{ct} is a column vector including a constant and school district demographic characteristics (….). D_c is a dummy variable equal to one if the school district ever implemented a desegregation plan, and the indicator function, $1(\ )$, is equal to one when the year of observation is y = -5, -4,…, 1,…,6, years removed from the date, T_c^*, when school district c first implemented a desegregation plan (y=0 is omitted).

The point estimates of interest, π_y and τ_y, are identified using variation in when desegregation plans were implemented. Because the indicator for $y = 0$ is omitted, π_y is interpreted as the average difference in outcomes (school spending; class size; school segregation) y years *before* the plan was implemented, and τ_y is the average difference in outcomes y years *after* the desegregation plan was implemented. Estimates of π_y allow a visual and statistical evaluation of the potential importance of pre-treatment, time-varying school district-level, unobservables; estimates of τ_y allow the post-treatment dynamics to be explored. The π_y and τ_y vectors traces out the (equilibrium) adjustment path for school resource inputs from the pre-desegregation plan period to the implementation of desegregation plans—allowing for possibility that efficacy of desegregation plans may erode over the long-run due to "white flight" (private school attendance).[6]

A key asset of this identification strategy is that estimates of π_y and τ_y will be unbiased even if there are pre-existing and permanent differences between school districts that implemented desegregation plans and those that did not. The county/school district fixed effects control for time-invariant community characteristics such as preferences for racial integration and education. With the inclusion of region-by-year fixed effects, the estimates will provide unbiased estimates of the impact of court-ordered school desegregation plans even if regions of the country varied in their K-12 education policies or their average level of funding support from

[6] Note, however, that the point estimates corresponding to $y < -3$ and $y>3$ are estimated from a smaller sample of school districts than estimates for the intervening years. This is because school district-level data on per-pupil spending and teacher-to-student ratios is not available annually for many districts before 1968. As a robustness check for court-order induced effects on dimensions of school quality, I used a balanced panel of school districts that includes districts only if they contribute to the identification of the entire vector of leads and lags of implementation impacts (i.e., districts that have school quality information in at least three years before and three years after implementation). Evidence shows that the increase in the treatment effect in the first 4 years after the court order is not a spurious result of the differing set of districts identifying the parameters.

year to year. Additionally, time-varying, community-level (i.e., county, school district, or neighborhood) characteristics and measures of government transfers adjust the estimates for observed differences in characteristics and changes in federal programs. The standard errors are clustered by school district.[7]

Evaluating Health Impacts of Court-Ordered School Desegregation and School Quality. Sparse direct evidence is available on how school quality affects adult health status via its impacts on educational attainment and adult economic status. I examine the long-run effects of school district per-pupil spending, teacher-to-student ratio (class size), average teacher salary, and the extent of school segregation on health through mid-life, where it is hypothesized that the effects of school quality operate through their influence on the socioeconomic mobility process (e.g., via effects on educational attainment and adult economic status). The analysis herein examines effects over a much longer time horizon than prior studies using micro data. This is particularly important for health outcomes, as there is likely a longer lag between poor school quality and the manifestation of health effects. I use the census block as the definition of neighborhood, which comprises a much smaller geographic area than previous studies utilize; and I match childhood residential location address histories to blocks and school district boundaries (Appendix C details the algorithm used for matching individuals to schools).

I utilize three different, but complementary, empirical approaches to estimate long-run effects of school desegregation and school quality on adult health: (1) models that include an unusually extensive set of childhood family and neighborhood controls; (2) difference-in-difference and fixed effect models; (3) and sibling fixed effect models. I discuss each in turn.

---

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

The rich set of family background controls and unique measures merged on from multiple data sources collected on aspects of schools and the physical, service and social environments of childhood neighborhoods—including school quality and school segregation, mandated school desegregation plans, parental expectations for child achievement, neighborhood poverty and crime, parental income and education, child health insurance, race and residential segregation, health behaviors, housing quality, connectedness to informal sources of support, rate of time preference—help isolate impacts of school quality on adult health.

Parental income and school district per-pupil spending (average levels that prevailed during adolescence (ages 12-17)) are dimensions of childhood families and schools that I give particular emphasis to in the regression analysis. I also analyze residential segregation and make use of a unique set of measures of parental and neighborhood expectations of children's educational attainment. The effects of childhood neighborhood factors are analyzed but presented in detail in a companion paper by Johnson (2009). The aim here is to isolate the role of childhood school quality, independent of family background and neighborhood quality.

I utilize a broad array of available measures in the PSID of family and neighborhood background. In addition to detailed measures of family economic resources and socioeconomic status during childhood, additional factors include residential segregation, parental and neighborhood-level measures of expectations of child achievement, child health insurance coverage, birth weight, unintended fertility timing preferences (unintended pregnancy), parental health behaviors (alcohol and smoking), parental connectedness to informal sources of support, and parental self-reports of neighborhood and housing conditions. The self-reports of housing/neighborhood conditions include: whether live in Public Subsidized Housing; poor neighborhood for children, whether there exist plumbing problems, housing structural problems, security problems, cockroach or rat problems, insulation problems, neighborhood cleanliness problems, overcrowding, noise, or traffic problems, burglary, robbery, assault, drug use, or problems related to having too few police. This survey information is used along with 1970-2000 census tract based measures—particularly, neighborhood poverty rate. The effects of childhood neighborhood factors are presented in detail in a companion paper by Johnson (2009). I control for parental education, parental health status, birth order, whether born into a two-parent family, year of birth, and region of birth. I also make use of a unique set of measures of parental aspirations/motivation and long-term planning, parental personality, habits and skills that were collected in the early years of the PSID. (Appendix B contains details of the PSID survey questions used to construct these measures as well as the descriptive statistics by race). Because of the detailed measures of childhood family and neighborhood characteristics included in the model of adult health status, I am able to minimize the problem of omitted variables bias of estimated childhood school quality effects that has been suggested for prior studies that have examined labor market outcomes.

Difference-in-Difference Approach. I estimate the impacts of court-ordered school desegregation, and the improvements in school quality for African Americans that accompanied their enactment, on subsequent health attainments in adulthood. The difference-in-difference regression analysis attempts to isolate the component of school quality that is attributable to court-ordered desegregation plans that were enacted in many cities in the 1960s, 1970s, and 1980s, when many of these children were growing up. I take advantage of the wide variation in the timing and scope of implementation of desegregation plans to identify their effects. The identification strategy exploits differences in childhood exposure during school-age years to racially-integrated schools based on variation across school districts and across birth cohorts

(1950-1975) in the timing of implementation of court-ordered desegregation plans. I measure the proportion of an individual's school-age childhood years (i.e., ages 5-17) in which they resided in a school district that had implemented school desegregation plans. I utilize the birth cohort variation in exposure to school desegregation among the broad range of birth cohorts (1950-1975) to identify effects on adult health outcomes.

Specifically, I employ a difference-in-difference framework and use variation across school districts and across birth cohorts to estimate the following model:

$$H^*_{ticb} = \alpha + \delta SDP_{cb} * black + X_{icb}\beta + \varphi Age_{ticb} + \mu_c + \lambda_b + \theta_s * t + \varepsilon_{ticb}\,, \qquad (2)$$

where *SDP* represents the proportion of school-age years an individual was exposed to school desegregation, *i* indexes individuals, *c* indexes school districts, *b* indexes birth cohorts, *t* indexes age of individual at which adult health outcome is measured, and *s* indexes state of birth. The identification comes from variation across school districts across birth cohorts in the adoption of school desegregation plans as distinct from trends due to other factors. The model includes school district fixed effects, birth cohort fixed effects and state-specific linear time trends. The county/school district fixed effects control for time-invariant community characteristics such as preferences for racial integration. The childhood race-region-year fixed effects control for race-specific time trends common to children at the region-year of birth (birth cohort) level. The standard errors are clustered by county.[8]

The identifying assumption of the model is that, absent court-ordered school desegregation exposure during childhood, the black children would have experienced outcomes similar to those who grew up in those same communities but who had already reached age 18 prior to the desegregation plan implementation, conditional on (race-specific; region-specific)

[8] This part of the research design is similar in many ways to recent studies by Guryan (2004) on the impacts of school desegregation on black high school dropout rates and Weiner, Lutz, and Ludwig (2008) to investigate impacts on crime.

year of birth effects; or, alternatively, similar to those who were born in same year and grew up in same region of the country but for whom desegregation plan implementation in their school district of upbringing occurred after they had reached age 18.

Because I did not want to include endogeneous residential moves (e.g., residential moves induced by school quality changes that accompanied desegregation plan implementation), this analysis does not attempt to incorporate information of family moves across school districts during the child's school-age years. Instead, I identify the neighborhood and school of upbringing based on the earliest childhood address (in most cases, 1968). The resultant potential measure error of school quality will tend to lead to attenuation bias of coefficients toward zero. The analysis does capture school district characteristics that were changing significantly from year to year.

Threats to Identification for Difference-in-Difference Approach. Childhood school-district specific trends in subsequent attainment outcomes (correlated with the timing of court orders) are a potential violation of the identification assumption. To assess this threat to the causal interpretation of the empirical estimates, I examined trends in attainment outcomes in treatment and control groups in the period before court order implementation. The similarity in the pre-trends provides supportive evidence in favor of the identifying assumption. The latter part of Section V provides more discussion of a variety of falsification exercises and specification tests performed.

Using Sibling Differences to Estimate School Effects.

The sibling fixed effect approach enables one to control for time-invariant aspects of all family and neighborhood background shared by siblings. The effect of school desegregation and school quality is identified by capitalizing on the fact that siblings of different ages may have matriculated through different school systems because of the rapid changes that occurred over this period of their childhoods. Within sibling pairs that attended schools with different resources, the younger sibling experienced integrated schools for a longer period of childhood and typically had access to greater school resources as reflected in greater per-pupil spending and lower class sizes during adolescent years. The sibling comparisons evaluate adult health outcomes at the same age and controls for birth order, year of birth, birth weight, whether mother was married at birth, are included in all specifications.

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

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

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

V. Results

Before discussing the estimates from the difference-in-difference models, I begin by presenting nationally-representative estimates of the bivariate relationship between adult health status and childhood school quality (i.e., school district per-pupil spending, class size, racial school segregation and desegregation plans), socioeconomic status in childhood (i.e., parental education, income), parental expectations for child achievement, and race by birth cohort and residential segregation.

Descriptive Results. The results presented first document descriptively the extent of racial health disparities by birth cohort and how they evolve over the course of adulthood. These figures display the proportion of years in poor health as an adult as well as the age pattern of the health index (which was described earlier). The age patterns of the conditional expectations are calculated using a Jianqing Fan (1992) locally weighted regression smoother, which allows the data to determine the shape of the function, rather than imposing, for example, a linear or quadratic form. The differences presented are all statistically significant.

These figures reveal several patterns. First, the relationships between the parental income-to-needs ratio and adult health exhibit nonlinearities. Furthermore, the socioeconomic gradient in health appears to widen over the life course, as the health deterioration rate is more rapid in adulthood among those who grew up in more disadvantaged child neighborhood/school and family environments. For example, twenty-three percent of adulthood years between ages 35 and 55 is spent in fair or poor health among those who grew up in poverty, while those rates are thirteen percent, eight percent, and six percent among the near-poor, those whose parental-income-to-needs ratio is 2 to 3, and those growing up in affluent families, respectively (Figure 1). As shown in Figure 1, the health status of a twenty-five year old who grew up in poverty is roughly at the same level of health as a fifty-year old who grew up in an affluent family (i.e., parental-income-to-needs ratio greater than three).

This pattern is particularly striking when we examine health status by race. As shown in Figure 2, black-white differences in health status widen significantly over the life course (both in levels and in proportionate terms). Indeed, by age 55 the health status of the average African-American is problematic, while the average health status of whites at 55 is good or very good (65 versus 85 on the health status index). A quarter of whites report themselves in excellent health well into their 50s; among blacks, the same points are reached before age 40. I conduct a systematic analysis of the evolution of racial health disparities in the final section. Figure 2 also shows that blacks who grew up in extremely segregated environments in childhood experienced worse health in adulthood, relative to blacks in less segregated areas.

Residential segregation may affect access to quality schools and subsequent mobility prospects through their effects on child school district per-pupil spending. Before school desegregation plans were enacted, school district spending, particularly in the South, was directed disproportionately to the majority-white schools within districts, which will not be reflected in the district-level spending data. This may result in substantial measurement error in actual per-pupil spending resources available to blacks prior to the enforcement of these desegregation plans. This is the likely reason that, for blacks, I find school district spending has no appreciable relationship with adult health and socioeconomic attainments until birth cohorts who reached school-age after school desegregation plans were in effect (especially in the South).

The Effectiveness of School Desegregation Plans. I next present results of the impacts of court-ordered desegregation plans of public schools. Figure 3 presents the dates of school desegregation plan implementation across the country among the 1,057 school districts that introduced such plans over the 1954-1980 period. 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.

I build on the findings of Welch and Light (1987), Guryan (2004), Reber (2005), and Weiner et al. (2008) by first analyzing the effectiveness of desegregation court-orders in reducing the extent of racial school segregation. I then extend these findings to show that in the years leading up to and immediately following implementation, desegregation plans had notable impacts on two key school quality resource indicators among blacks—1) increases in per-pupil spending and 2) reductions in the student-to–teacher ratio. These results are presented in Figures 4-7. The figures plot the regression coefficients on indicator variables for years before and after desegregation orders are implemented (year before implementation is the reference category) on school district racial segregation, per-pupil spending, and student-to-teacher ratio, respectively. The changes are all statistically significant. As shown in Figure 4, following court desegregation orders, there is a sharp decline in the school district racial dissimilarity index, which ranges from zero to one and represents the proportion of black students who would need to be reassigned to a different school for perfect integration to be achieved given the district's overall racial composition. With regard to school segregation, there is no evidence of pre-existing segregation trends in the school districts prior to the court orders. Within two years after implementation, the dissimilarity index dropped by roughly 0.2 which is a substantial and rapid decrease given the average dissimilarity index in 1968 across the school districts was about 0.7. The change in the dissimilarity index 4 years after the court order is equal to X percent of the average index in 1970 and to X percent of the 1970 cross-sectional standard deviation of the index.

In Figure 5, the results indicate that, on average, school district per-pupil spending increased by nearly $1,000 by the end of the fourth year after desegregation implementation relative to the year immediately preceding enactment, which differed markedly from the trend leading up to the year these plans went into effect. This is a substantial increase given that the average level of per-pupil school spending in 1968 was roughly $3,500 (in 2000 dollars). Figure 6 provides supportive evidence of resultant increases in school resources for blacks following desegregation court orders in the form of reduced average class sizes.

As a robustness check for the estimated court-order induced effects on school quality, I alternatively used a balanced panel of school districts that includes districts only if they contributed to the identification of the entire vector of leads and lags of implementation impacts (i.e., districts that have school quality info in at least three years before and three years after implementation). The evidence shows that the increase in the treatment effect in the first 4 years after the court order is not a spurious result of the differing set of districts identifying the parameters.

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

The sharp trend break in school resource inputs (per-pupil spending, class size, school segregation) immediately following implementation of school desegregation plans—with similar magnitudes found among the early-adopter districts (1960s) and late-adopter districts (1970s)—strongly suggests the estimates reflect the causal impact of desegregation plans.

With the timing of court-ordered school desegregation in mind, Figure 8 presents adult health status by race, birth cohort, and school desegregation plan status. I find substantial birth cohort differences in adult health status among African Americans. In particular, blacks born in the early 1950s (in the pre-Brown vs. Board of Education era) have significantly worse health when compared with birth cohorts born between 1955-1963 and 1964-1968, evaluated at similar ages. Furthermore, blacks born between 1964-1968, who grew up in the post-Civil Rights Act era and reached school-age years after the school desegregation efforts began to accelerate, had significantly better health in adulthood evaluated at similar ages, relative to birth cohorts born prior to 1964. For example, by age 40, blacks born between 1964 and 1968 had a roughly 7-point higher health utility index score relative to blacks born between 1950-1954; this magnitude is comparable to the raw black-white difference in health at age 40 observed among individuals born between 1964 and 1968. In contrast, as shown in Figure 8, there are no significant birth cohort differences in adult health among whites; thus, we see the raw age-adjusted black-white gap in adult health narrowed significantly in successive cohorts born between 1950 and 1968.

The second panel of Figure 8 highlights the significant birth cohort variation in childhood exposure to school desegregation plans, where we see less than twenty percent of school-age years among PSID original sample children born in the early 1950s were spent exposed to school desegregation plans, while those born in the late 1960s were exposed to school desegregation plans (integrated schools) for about seventy percent of their school-age years. In the third panel of Figure 8, I present differences in adult health status among blacks whose childhood schools were under court-order to desegregate as compared with blacks whose schools did not implement desegregation plans during their childhood years; importantly, we see that the differences in adult health status do not emerge until ages 35 and beyond, which is the pattern we would expect if these differences are driven by how school quality influences socioeconomic mobility. The difference in adult health status by age 40 among blacks who attended schools with a court-ordered desegregation plan versus those who were not exposed to school desegregation plans in childhood is about five points on the health utility index.

Figure 9 presents adult health status by child school district per-pupil spending and class size. About seven percent of adulthood is spent in fair or poor health among those who grew up in school districts in which spending per-pupil was in the top quartile, compared with twice that proportion (0.15) among those who resided in districts in which school spending was in the bottom quartile of per-pupil school spending (Figure 10); and these differences appear to widen after age 35 when the labor market returns to schooling become more pronounced (Figure 9). The difference in adult health status by age 40 between individuals who attended schools in the bottom versus top quartile of class size (i.e., ≤23 vs. ≥27) is about five points on the health utility index, while significant health differences were not present at age 25 (Figure 9). The association between school quality resources and adult health status among blacks is particularly strong. The difference in adult health status by age 40 among blacks born after 1964 who attended schools in the bottom versus top quartile of per-pupil school spending (i.e., <\$3,650 vs. >\$5,750) is about seven points on the health utility index, while only minor health differences were present at age 25.

Residential segregation may also influence subsequent mobility prospects through their effects on expectations for child achievement. Accordingly, Figure 10 presents adult health status by self-reported parental expectations for child achievement (measured during childhood).

The bivariate relationship show that nearly one-quarter of adulthood years between ages 35 and 55 are spent in fair or poor health among children whose parents had low expectations for child achievement, relative to eight percent among those whose parents had college-bound expectations for their child. These parental expectations are likely influenced in part by neighborhood and school resources, as evidenced by the strong neighborhood component in the similarity of parental expectations.

Regression Results for Adult Health. I next estimate a series of models building toward a full model specification that includes a rich array of observable child family-level, neighborhood-level and school-level characteristics to attempt to identify determinants of adult health status. I conduct a systematic analysis of the evolution of socioeconomic and racial health disparities in adulthood. Table 1 contain these regression results, where the series of models reported include the raw age-adjusted race gap for cohorts born between 1950-1954, 1955-1963, and 1964-1968 (column(1)), a model that includes controls for childhood family characteristics (column(2)), a model that controls for childhood neighborhood, school, and family background characteristics (column (3)). Table 2 presents the results from the full model specification separately for young adult ages (i.e., ages 20-34) and mid-adulthood ages (i.e., ages 35-57), in order to examine the age-profile of the estimated effects of child school quality on adult health.

The childhood neighborhood quality factors are included as controls but suppressed in the tables, since the focus of this paper is on childhood school quality and family background. The aim here is to isolate the role of childhood school quality and family background, independent of neighborhood quality. The child neighborhood quality results are presented in detail in a companion paper by Johnson (2009). The estimated effects of a one standard deviation change in neighborhood or family environment index reported in Johnson (2009) provide a useful comparison to discuss effect sizes. One must use some caution, however, with drawing causal inferences from these coefficient estimates. The estimates are intended instead to summarize the relationships between the health trajectory over the life course with various dimensions of schools and family background. The robustness of the results for causal inference is examined in detail in the final section of the paper.

From the hierarchical random effects models and the estimated adjusted neighbor correlation estimates reported in Johnson (2009), I calculate how one would expect an individual's adult health status to change given a one standard deviation change in the index of child family environment, and the corresponding predicted change in adult health for a one standard deviation in the index of neighborhood/school environment (this quantity is captured by the estimates of the standard deviation of the child family and neighborhood random effects, respectively). Those results suggest that a one standard deviation change in the index of neighborhood/school environment is equivalent to roughly a 8-point change in the health utility index in mid-adulthood; thus, the upper bound estimate on the potential scope of child neighborhood/school influences for health trajectories is substantial.

Gaps in health between blacks and whites are large and exist at all stages in life. The general health status (GHS) index in adulthood is 6.5 points lower for blacks, on average, but I find substantial birth cohort differences in the magnitude of black-white health disparities in adulthood (evaluated at the same ages) (column (1) of Table 1). In particular, while the age-adjusted average black-white difference in adult health status for cohorts born in the early 1950s is 9.3 points, this difference is reduced to 4.7 and 3.3 points, among the cohorts born between 1955-1963 and 1964-1968, respectively. These cohort differences are completely driven by health improvements experienced by African Americans over this period; I do not find any significant birth cohort differences for whites. Furthermore, the black-white gap in health status increases in levels and in proportionate terms over the course of adulthood, independent of year of birth. A useful way to interpret the estimate is in relationship to the size of the effect of age on health, with the race gap by middle-age, among those born in the early 1950s, equivalent (on average) to blacks reaching a level of health deterioration about 20 years prior to their white counterparts. That is, GHS is 9.3 points lower for black adults (column(1) of Table 1), which is equal to roughly 20 years evaluated at an effect of age during one's mid-30s and 40s of -0.41.

The raw black-white gap in health status during mid-adulthood ages for individuals born in the early 1950s is equivalent to about a one standard deviation combined change in both the index of child neighborhood and family environments; and the raw black-white gap in health status during mid-adulthood ages for individuals born in the mid-to-late 1960s is equivalent to about a one standard deviation change in the index of child neighborhood. For these birth cohorts, it is plausible that the average childhood family and neighborhood environments between blacks and whites differ by as much as one standard deviation of the family/neighborhood environment index.

The specification that includes the childhood family, neighborhood, and school-related factors is shown in column (3) of Table 1. Comparing the estimates in this column with the estimates in column (2) and the descriptive results shows the bias that occurs when estimating either the direct effects of child school resources on adult health without controlling for family/neighborhood background characteristics or the direct effects of child family characteristics that omit neighborhood and school resource measures. Controlling for neighborhood and school characteristics reduces the estimated health effects in adulthood of parental income among those who grew up in near-poor and middle-class families by about 40 percent (as shown in column (2)-(3) of Table 1, spline specification coefficient estimates on income-to-needs ratio change from 1.46 to 0.86 when the income-to-needs ratio is in the range of 1-3). Similarly, all the school resource coefficients decline significantly when the extensive set of family and neighborhood background controls are included (the models that include school variables without family/neighborhood variables are not shown). However, the estimated effects of various dimensions of schools remain large and significant with the inclusion of the extensive set of family and neighborhood background factors. Similarly, the effects of various dimensions of family background remain significant with the inclusion of the extensive set of child neighborhood and school characteristics.

The joint hypothesis that the school-related factors are empirically unimportant is clearly rejected by the data; the *F*-statistic is XX with a p-value less than 0.0001. Most of the effect of school quality is due to two school resource measures: school district per-pupil spending and class size. For example, the results indicate that a 10 percent increase in school district-per-pupil spending is associated with a subsequent 1.4 point improvement in the adult health utility index and attending schools with large average class sizes ($\geq$27) is associated with a 1.4 point reduction (column (3) of Table 1); the magnitudes of the estimated impacts of these school resource measures are equivalent to nearly one-fifth of a standard deviation change in the neighborhood environment index. As aforementioned, I find school district spending has no appreciable relationship with adult health among blacks until birth cohorts who reached school-age after school desegregation plans were in effect, which is likely because of substantial measurement error in actual per-pupil spending resources available to blacks prior to the enforcement of these desegregation plans. In addition, I find the adult health outcomes of African Americans who grew up in the South are particularly sensitive to the level of racial residential segregation that prevailed during their childhood years. The estimated impacts of school segregation became insignificant after controlling for both residential segregation and school quality measures (per-pupil spending and the average student-to-teacher ratio). Blacks who grew up in more segregated neighborhoods in the South had significantly worse health in adulthood, both compared with whites and compared with blacks who grew up in other regions of the country or blacks who grew up in the South in areas (time periods) where (when) racial neighborhood and school segregation was less extreme.

The estimates in column (3) of Table 1 imply that, among individuals born between 1955 and 1968, black-white disparities in adult health would not exist if it were not for differences in childhood family, neighborhood and school quality factors between the racial groups (e.g., after controlling for both childhood family, and neighborhood and school quality factors, the black-white health gap is eliminated during adulthood). While the initial raw black-white differences in health among individuals born in the early-to-mid 1950s were significantly larger (as compared with more recent birth cohorts), race differences in childhood family, and neighborhood and school quality factors combined account for about one-half of the black-white health gap among these older cohorts.

The school quality measures and racial residential segregation and school segregation indices appear to have stronger relationships with health over time, with stronger links to adulthood health than childhood health (not shown) and stronger links to health in middle-age relative to young adulthood (as shown in Table 2). The age-profile of these estimated effects suggest that the linkages may be the result of how they influence the socioeconomic mobility process. For example, school district per-pupil spending was not significantly related to child health (not shown); in contrast, as shown in Table 2, a ten percent increase in per-pupil school spending is associated with 1.2 and 2.4 improvement in GHS score at ages 20-34 and ages 35-57, respectively. Similarly, while attending child schools with large class sizes is only weakly associated with adult health at ages 20-34, this relationship becomes large and significant (coefficient equals -2.1) when individuals reach ages 35 and beyond (which coincides with the period in the life cycle when labor market returns to schooling become more pronounced). As well, the age-profile of the school quality estimated effects on health is more stark in models that do not simultaneously include both class size and per-pupil spending in the same model, but rather include one or the other (results not shown). It is important to distinguish these life cycle effects from the birth cohort effects.

As shown in column (2) of Table 2 for health status at ages 35-57, among blacks who grew up in the South a one standard deviation increase in childhood residential segregation (dissimilarity index) is related to a 2.1 point reduction in GHS, holding the level of school segregation constant. I also find that for health status at ages 35-57 among whites, a one standard deviation increase in child school segregation (dissimilarity index) is related to a 0.8 point significant improvement in GHS, holding the level of childhood residential segregation constant. The estimated effects of child racial residential and school segregation indices on health at young adult ages are generally insignificant and much smaller in magnitude.

Parental and neighborhood-level average expectations for child achievement had substantive, independent influences on the health trajectory over the course of adulthood. These factors again appear to have stronger relationships with health over time, with stronger links to adulthood health than childhood health (not shown) and stronger links to health in middle-age relative to young adulthood; evidence suggestive that the linkages may be the result of how they influence the socioeconomic mobility process. For example, as shown in column (2) of Table 2 for health status at ages 35-57, low parental expectations and neighborhood-level low expectations for child achievement are independently associated with a 4.1 point and 1.1 point lower GHS, respectively (relative to college-bound expectations). Johnson (2008) demonstrates these factors also significantly influence mobility prospects, and explain part of black-white differences in rates of upward mobility from poor families.

For purposes of comparison, consider the estimated effects of parental income on adult health, where I find substantially larger impacts of income in the lower tail of the distribution highlighting the negative effects of child poverty. For example, the results in column (3) of Table 1 indicate that a one-unit increase in the family income-to-needs ratio from half of the poverty line to 1.5 times the poverty line translates into a 2.4 point increase in adult GHS (0.5*3.9930+0.5*0.8579), which is equivalent to roughly 8 years younger. The estimated effects of a one-unit standard deviation change in school quality on adult health compare favorably.

Effects of Court-ordered School Desegregation

For each district, I compute the change in school segregation and class size induced by the court-order from the year preceding enactment to the first several years following implementation.[9] I then exploit variation in the scope of desegregation court orders in addition to quasi-random variation in the timing to assess whether there is evidence of a dose-response effect of school quality improvements on subsequent health status attainments in adulthood.

As previously shown in the second panel of Figure 8, the share of children exposed to school desegregation orders increases significantly with year of birth over the 1950-1970 birth cohorts analyzed in the PSID sample. The identification strategy used to evaluate the long-run health effects of school desegregation court orders effectively compares the adult health

[9] A similar estimate of the change in per-pupil spending induced by desegregation plans could not be conducted because of the lack of accurate information using school district level data of the pre-desegregation spending resources available to black students.

outcomes of blacks who attended integrated schools (i.e., court-ordered desegregation occurred prior to/during their childhood school-ages) with the adult health outcomes of blacks who were already adults when their childhood school district's court order was first implemented, where the two groups' health outcomes are evaluated at the same ages in adulthood. I estimate the extent to which the black-white gap in adult health status narrowed as a result of childhood exposure to school desegregation (i.e., I compare the black-white gap in the child cohorts that experienced school desegregation plans relative to the black-white gap in cohorts just prior to school desegregation).

The results are presented in Table 3. The first column of Table 3 contains estimates of the basic difference-in-difference specification. The estimates control for region-specific time trends to account for the regional pattern in the timing of court-ordered desegregation plans (as shown in Figure 3). In columns (2) through (4) of Table 3, I add childhood county fixed effects to the baseline difference-in-difference model specification. The results presented in the first column of Table 3 indicate that the black-white gap in adult health status narrowed significantly for the cohorts exposed to desegregation plans in childhood relative to the black-white gap in adult health observed among cohorts just prior to school desegregation. The results suggest this impact is attributable to the relative improvements in school quality that blacks who were exposed to desegregation court orders experienced during childhood. As shown in column (2) of Table 3, these findings are robust to the inclusion of child county fixed effects. The results demonstrate that there is a significant difference in adult health outcomes among blacks between cohorts that were born less than 10 years apart but who differed in whether they attended integrated schools. The results indicate that school desegregation resulted in a statistically significant 2.6 point narrowing of the black-white adult health status gap, or roughly a 50 percent reduction from the raw unadjusted black-white adult health status gap observed among cohorts born between 1955 and 1963. Furthermore, the estimated effect of school desegregation plans for blacks improved adult health status, on average, by between 0.8 to 2.6 points; these magnitudes are similar to the effects of between 0.11 to 0.35 of a standard deviation increase in the neighborhood quality index.

The results presented in columns (3)-(4) of Table 3 suggest that changes in school quality and peers resulting from the integration of schools played an important role. The results shown in columns (3) and (4) are restricted to individuals who grew up in school districts that implemented desegregation plans between 1954-1990 for which data is available on school segregation and student-to-teacher ratio information one year before and four years after initial implementation. I find that desegregation plans that resulted in larger improvements in school quality (reflected at least in part by larger reductions in class size and larger declines in racial school segregation) are shown to result in more beneficial outcomes for blacks who grew up in those court-ordered desegregation districts. In particular, school desegregation plans that resulted in a 10-point larger than average decline in the school segregation dissimilarity index, or that were associated with an additional reduction in class size (over and above the average decline), each led to an adult GHS score among blacks that was about one point higher than the average improvement in adult heath induced by school desegregation.

The sibling fixed effect results reveal that individuals who attended schools during their adolescent years with higher per-pupil spending as compared with levels that prevailed when their siblings were adolescents experienced better subsequent health outcomes in adulthood (evaluated at the same age). The identification of these effects is driven largely by significant per-pupil spending increases in a relatively short period of the 1970s in many areas. I find little evidence that observable differences among siblings are related to differences in the quality of the high schools they attend. There is no evidence that the results are biased by a positive correlation between sibling differences in school inputs and sibling differences in other factors that are favorable to adult health status (robustness checks not shown).

The results presented in the final column of Table X are sibling fixed effect models designed to assess the long-run effects of school desegregation on adult health. I find that black children who were exposed to implemented, court-ordered school desegregation for the majority of their school-age years experienced significantly improved health outcomes in adulthood as compared with their older siblings who grew up in segregated school environments with weaker school resources (controlling for age and birth cohort effects). I find that health outcomes among blacks were particularly affected by changes in access to school resources associated with desegregation, not simply changes in exposure to white students. The results also suggest that whites experienced worse subsequent adult health outcomes (other things equal) among cohorts exposed to school desegregation plans during childhood, except in districts where state funds were used to "level-up" school spending in integrated schools to the level previously experienced only in the white schools. The results, as a whole, suggest that benefits for minority children do not come at the expense of white students.

The difference-in-differences estimates and sibling-difference estimates indicate that school desegregation and accompanied increases in school quality resulted in significant improvements in adult health for African-Americans. The pattern of results is remarkably similar across all of the empirical approaches. The increase in subsequent adult health among African Americans for successive cohorts born between 1950 and 1975 mirrored the improvements in access to school quality that accompanied school desegregation during their school-age years. African-Americans who attended integrated schools during their elementary school years appear to benefit more than those exposed to integrated schools only later in the school careers, which is consistent with a treatment dose-response relationship. This may be due to two factors: 1) elementary students may have fewer social adjustments compared with older students who have spent more time in segregated environments; and 2) secondary schools are more likely to track students by academic ability (and race), which could reduce benefits of desegregation for minorities.

The analysis cannot cleanly identify the mechanism through which school desegregation influenced long-run health outcomes, but one potential pathway that merits careful consideration is through impacts of school quality improvements (i.e., greater school resources for blacks in integrated schools) on the socioeconomic mobility process. The most obvious channel through which these child school-related impacts manifest is through their effects on educational attainment and adult earnings, which in turn influence adult health. To provide some suggestive evidence of the importance of this pathway, I examine to what extent the estimated effects of school desegregation plans on subsequent adult health status are reduced once measures of educational attainment and adult economic status are included. I find that a significant part of the impacts were the result of a combination of increases in the levels of educational attainment and in the returns to education. There is also some evidence that measures of school quality inputs steepen the education slope.

A myriad/variety of robustness checks were performed along with tests for potential endogeneity of timing of school desegregation across cities; none of which altered the main findings. Falsification tests provide additional evidence that unobserved factors do not contaminate the estimates. For example, adding controls for dimensions of school quality in a child school district of upbringing in years the individual was *not* in school (not of primary or secondary school age) (i.e. when the individual is not between the ages of 6 and 18) does not significantly alter the results. The estimated effects on adult health of per-pupil spending in years in which the individual was not in K-12 schooling are very close to zero, and the effects of experienced per-pupil spending remains significant and essentially unchanged. This is what we would expect if endogeneity issues are not driving the results. This finding confirms that the results do not simply reflect community-level differences in attitudes about the importance of education that are correlated with determinants of health.

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

In supplementary analyses, I also investigated whether school desegregation had any measurable impacts on parental and neighborhood-level average expectations for child achievement among minority families and neighborhoods. While far from providing definitive evidence on this, the results show that school desegregation exposure was associated with increases in parental and neighborhood-level average expectations for child achievement for these cohorts, independent of other childhood family socioeconomic factors and time trends.

DISCUSSION AND CONCLUSION

This paper provides among the first evidence to assess the extent and ways in which childhood school quality factors causally influence later-life health outcomes. The results suggest that both childhood school and neighborhood and quality factors play important roles in the intergenerational transmission of health status and influence adult health outcomes (through their influence on the socioeconomic mobility process).

I estimated the effects of court-ordered school desegregation (and the resultant effects they had on school resources—e.g., increases in per-pupil spending and reductions in class size) on subsequent health outcomes in adulthood by exploiting the large variation in the scope and timing of implementation of the court orders that occurred in the 1960s, 70s and 80s across the set of school districts subject to such orders. I find strong evidence that desegregation plans were effective in narrowing black-white school resource gaps of per-pupil spending, class size, and decreasing school segregation (though white flight thwarted some of the integration and leveling up of school resources over time). The analysis attempts to disentangle the effects of neighborhood and school quality on subsequent health outcomes. In the process, the study results highlight the significant impacts of educational attainment on future health status, and point to the importance of school quality in influencing socioeconomic mobility prospects, which in turn have far-reaching impacts on health.

Moreover, I find that health outcomes for blacks were better among blacks who experienced the largest improvements in school resources and largest declines in school segregation levels. The results also indicate the black-white health disparity in adulthood was smallest in areas where school resources improved the most in response to desegregation orders—which is consistent with dose-response impacts. The results suggest the mechanism through which school desegregation led to beneficial health outcomes in adulthood for blacks include the significant improvement in access to school resources reflected in reductions in class size and increases in per-pupil spending. The magnitude of the estimated effects of some dimensions of school quality are larger than estimates reported in previous research and, taken together, are larger than the impact of increasing parents' income by a comparable amount.

The evidence collectively paints a consistent picture of significant later-life health returns of school quality. The analysis documented significant black-white differences in adult health that narrowed for successive cohorts born between 1950 and 1975. Racial inequality in school quality varied significantly across school districts, differed by school characteristics, and narrowed over this period. The quality of black children's education improved in quantity and quality in both absolute and relative terms. The results demonstrate that racial convergence in school quality and educational attainment following court-ordered school desegregation played a significant role in accounting for the reduction in the black-white adult health gap.

The study finds that racial differences in adult health can be accounted for by childhood family, neighborhood, and school quality factors. The evidence presented in this paper challenges future research to further our understanding of the underlying processes that produce health disparities between different racial, ethnic, and socioeconomic groups. The results indicate that both family background and neighborhood/school quality during childhood serve as primary gatekeepers of the intergenerational transmission of adult health status and play a large role in producing racial health disparities.

This work contributes to a growing literature that evaluates the longer-run effects of the Civil Rights Act, Great Society, and War on Poverty policy initiatives.[10] The present research findings are the first estimates of the effects of school desegregation (and school quality) on adult health outcomes using a plausibly exogenous source of identifying variation.

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

Racial segregation in public schools fell sharply from 1968 until the early 1970s, remained constant throughout the remainder of the 1970s, and has increased slightly since then (Orfield, 1983; Boozer, Krueger, and Wolkon, 1992). Overall, public schools are somewhat more segregated today than they were in the early 1980s (Clotfelter, 1998; Rivkin, 1994). We have witnessed a changing pattern of racial segregation in schools over the past four decades. Prior to the 1970s segregation in schools was largely attributable to segregation patterns within districts, while today it is increasingly attributable to residential location patterns between districts (Lankford and Wyckoff, 2000) and the tracking of students within schools.

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

The results may have implications for policy in the context of the current economic and legal environment. The Supreme Court issued three rulings in the early 1990s that significantly altered the legal basis for court-mandated desegregation (see for example, Lutz, 2005). It became easier to terminate court-ordered desegregation plans and return school control to local authority without external monitoring of minority student performance, which may result in reduced school resources targeted for minority students. School districts under a court-ordered desegregation plan are monitored by the courts. This removal of court oversight has resulted in an increased likelihood of a return to neighborhood schooling and re-segregation of public schools. At the federal level, this represents a movement away from court-ordered desegregation as a central tool to improve school quality. There has been an erosion of public attitudes and support for the perspective that schools must be integrated in order for blacks to receive a high quality education. There is only limited research evidence that has considered the question of the potential harm from the increasing trend in dismissal of desegregation orders. That is, will court's dismissal of desegregation plans reverse gains achieved by their implementation? Two recent studies by Clotfelter, Ladd, and Vigdor (2005) and Lutz (2005) find that dismissal of court-ordered desegregation plans led to increases in racial school segregation and increased black high school dropout rates.

The results of the present paper demonstrate that education policies can have substantial effects on future health. The lessons that can be gleaned from the particular case of court-ordered school desegregation and its long-run consequences are relevant for contemporary debates about school reforms and equity of school finance. Given the importance of local finance in K-12 public education, the impacts that residential segregation has on the distribution of educational resources across public school districts may continue to be significant. There remains considerable variation across states in spending per public school student, with per student spending in the top five states roughly a third to more than two-thirds greater than the national average, and close to twice the expenditures for the bottom five states (National Education Association data for 2004–2005). Within states, local funding, primarily from property taxes, represents more than 40 percent of revenues for primary and secondary education (Cohen and Johnson, 2004), contributing to inequities in educational resources across school districts and neighborhoods. Additionally, teachers' salaries have declined in real terms and also display wide variation across states, and states and school districts face challenges in recruiting and retaining well-qualified teachers in areas such as science and math (Dillon, 2007).

This study highlights the importance of analyses on the returns to education policies beyond labor market outcomes. The findings of this paper strongly suggest that estimates of the returns to education that focus on increases in wages substantially understate the total returns. The results suggest that perhaps the most effective policies to promote long-term health lie outside of traditional health care policy, and instead may take the form of education and housing policy. Education and housing policy programs targeted toward childhood conditions may provide vitally important means to improve population health and reduce health disparities. In this way, education and housing policy is health policy.

Appendix A

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 increase with lowering GHS categories. 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 previously been employed, 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).[11] 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

[11] 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).

obtained using truly continuous generic measures like the SF36 (Gerdtham et al. 1999) or the Health Utility Index Mark III (HUI) (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.

Interval Regression Model. Our method makes the assumption that underlying the categorical empirical distribution of the responses to the GHS question is a latent, continuous health variable. We 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.

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

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

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

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

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

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

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 we 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 our 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 B

Table A2 reports descriptive statistics for the samples used in the models of adult health status for the full sample. Income is the total for the family in which the child lives, and it is measured at various points in the childhood. Earnings are total labor market earnings during the previous calendar year. Drinking and smoking of parents are indicated by whether the family spent any money on these goods. Income is the total for the family in which the child lives, and it is measured from the five-year average for the years 1967-1972. All dollar values are expressed in 1997 dollars using the CPI-U. The parental income measure is specified as the income-to-needs ratio and I explore nonlinearities in effects at the bottom of the income distribution (child poverty).

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

Set of Questions Related to Parental Expectations, Aspirations and Other Factors

REFERENCES

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

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

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

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

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

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

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

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

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

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

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

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

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

Case, Anne, Angela Fertig and Christina Paxson. 2003. "The Development of the Gradient: Earnings and Health Dynamics". Unpublished manuscript, Center for Health and Wellbeing, Princeton University.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Johnson, Rucker C. and Robert F. Schoeni. 2007. "The Influence of Early-Life Events on Human Capital, Health Status, and Labor Market Outcomes Over the Life Course". National Poverty Center working paper.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

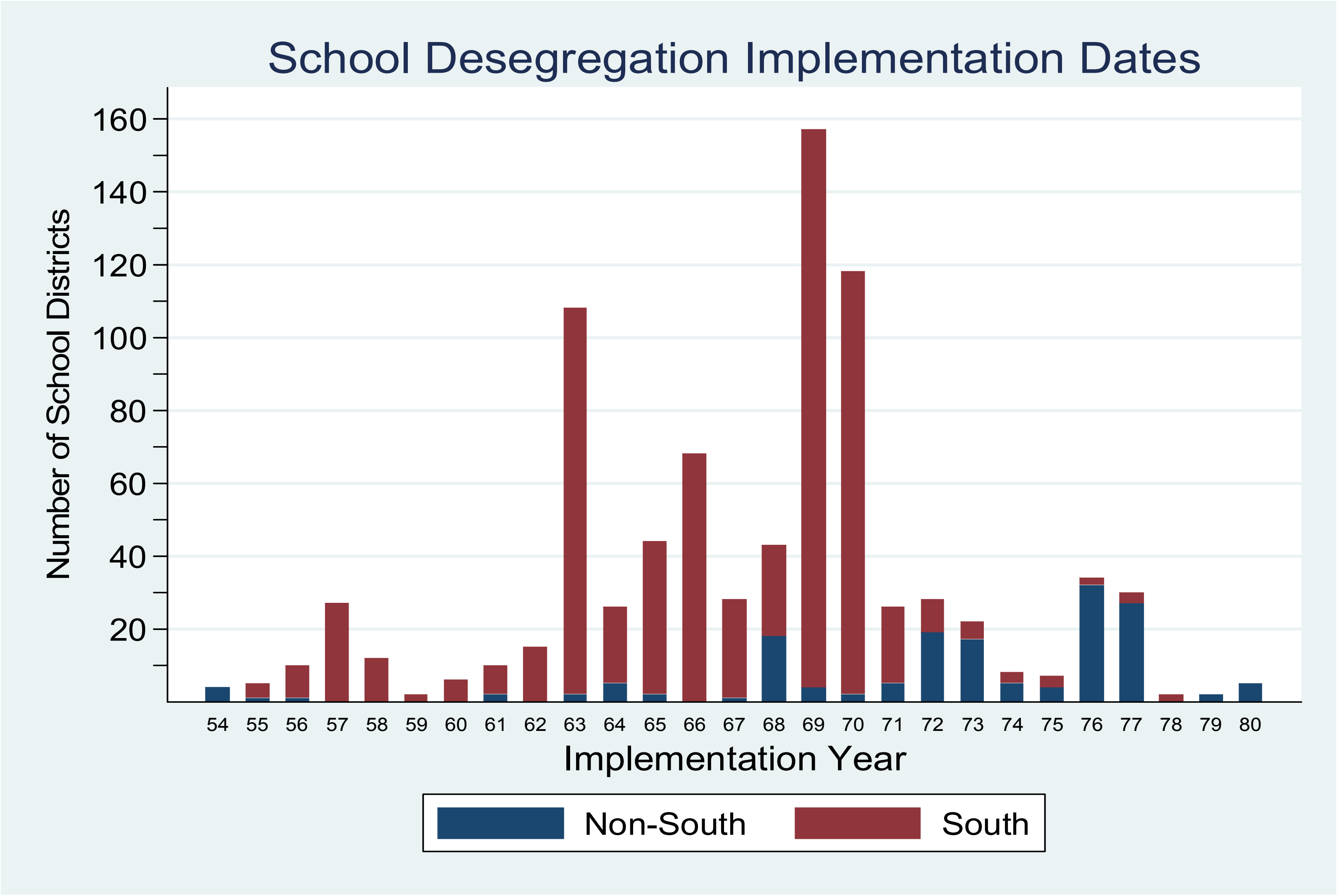
Tiebout, Charles M. 1956. "A Pure Theory of Local Expenditures". *Journal of Political Economy*. 64: 416-424.

Weiner, D., Lutz B., Ludwig, J. 2008. The Effects of School Desgregation on Crime. Manuscript (November).

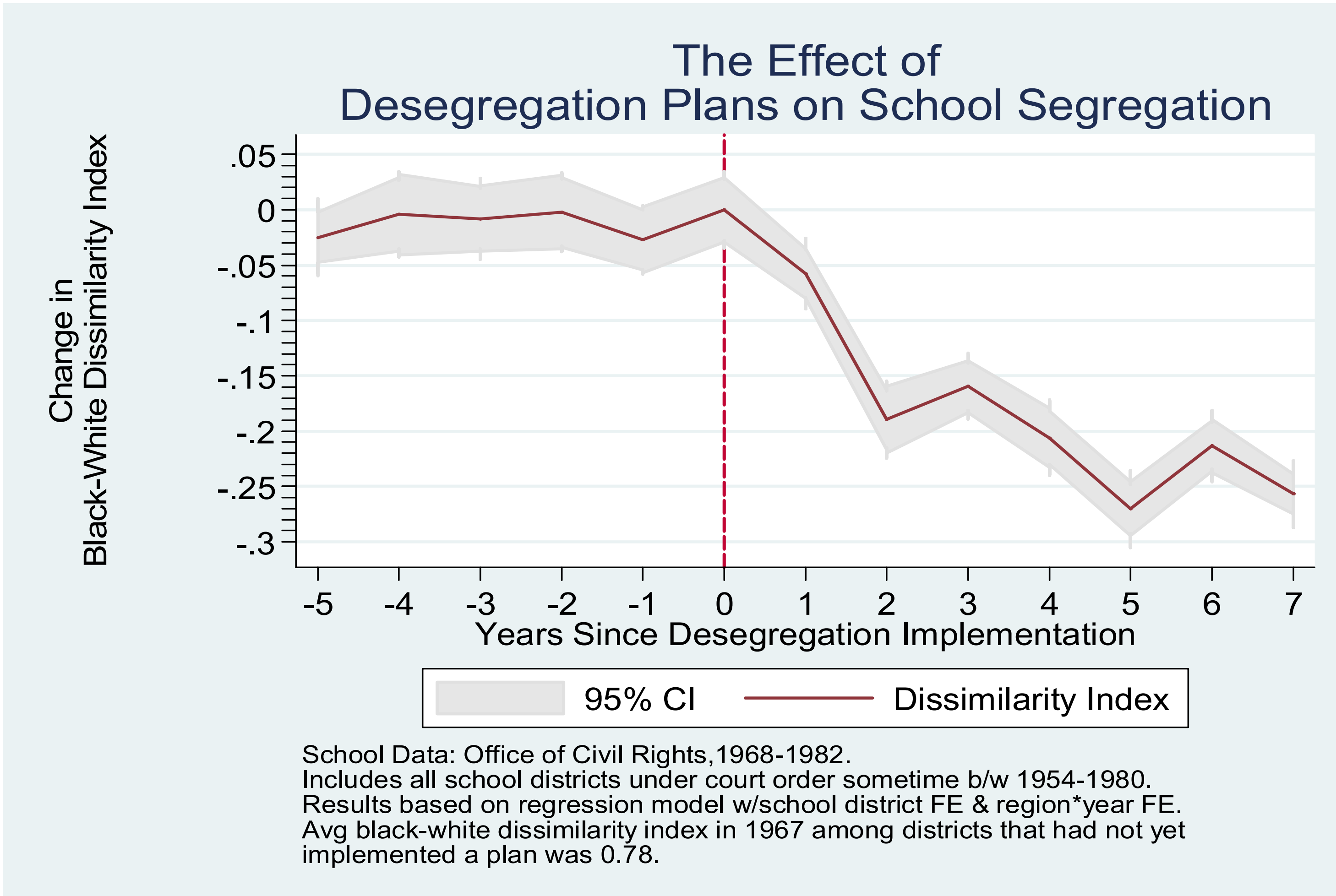
Welch, F., Light, A. 1987. New Evidence on School Desegregation. US Commission on Civil Rights, Washington, DC.

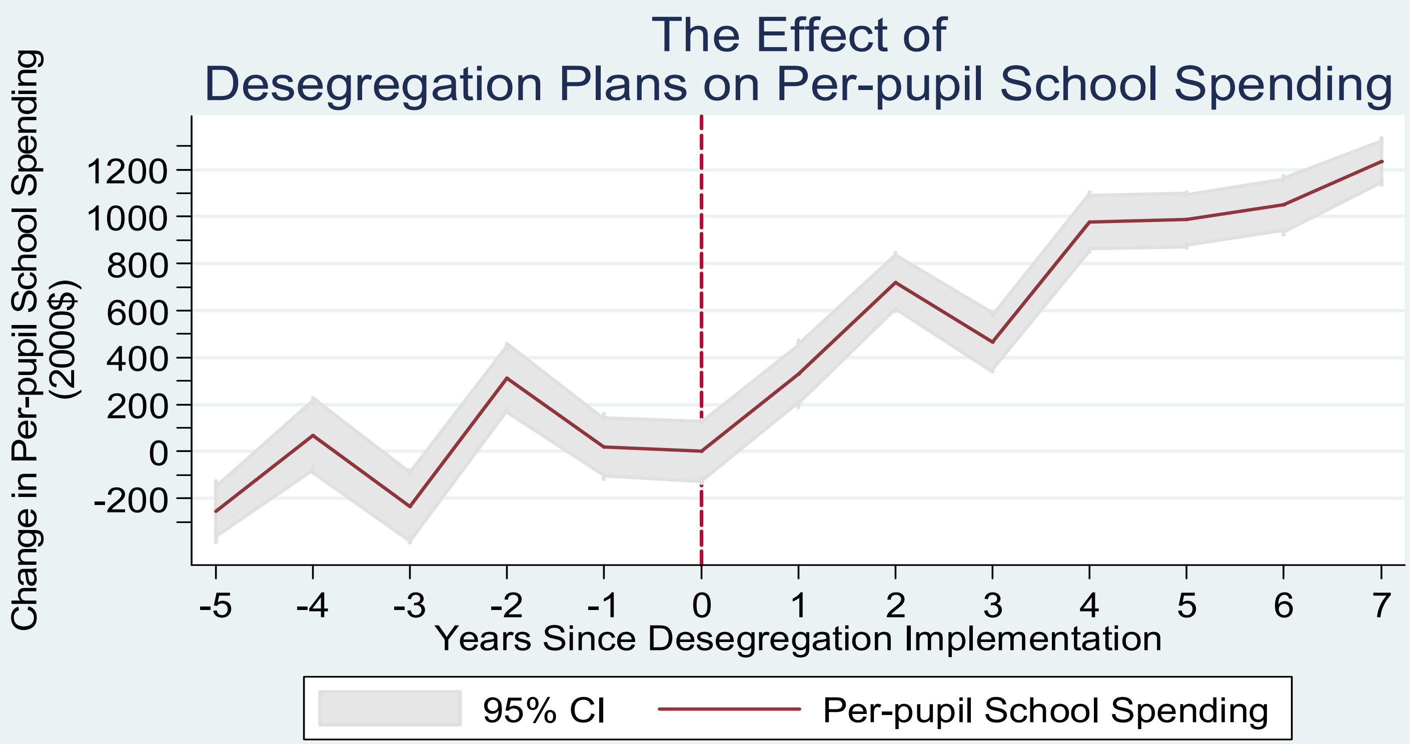
Wilson, Franklin. 1985. "The Impact of School Desegregation Programs on White Public-School Enrollment, 1968-1976." *Sociology of Education* 58(3):13-153.

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

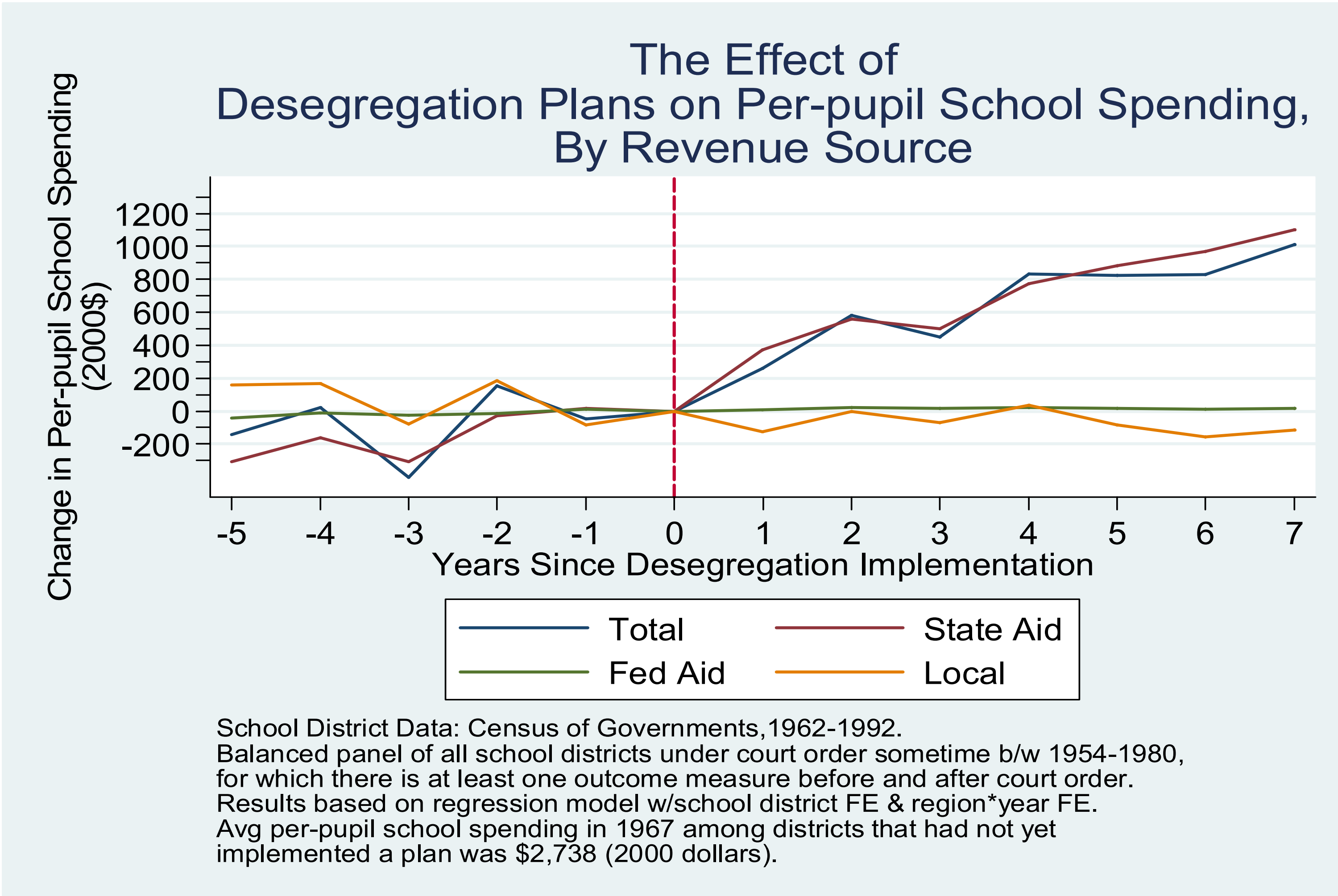


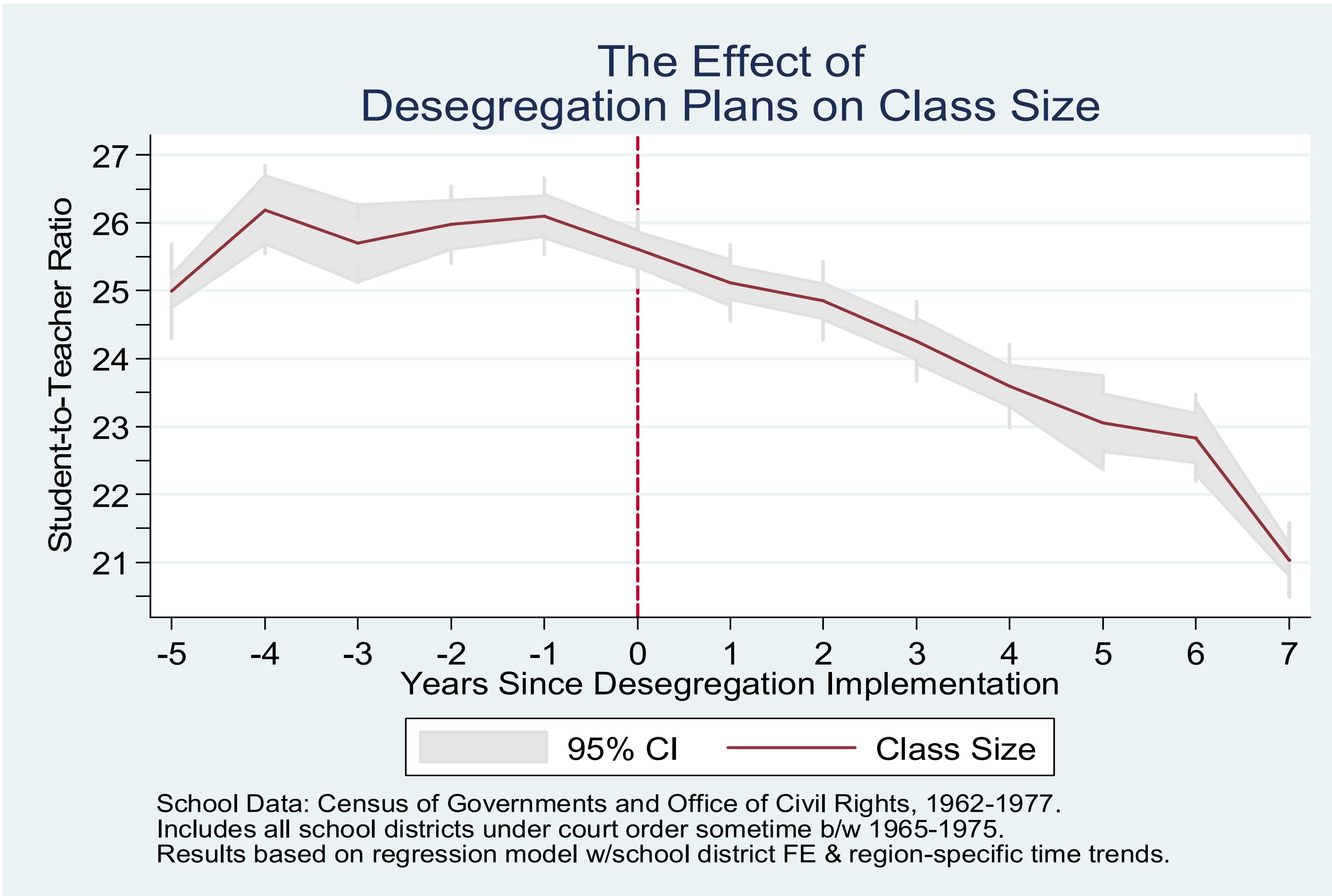
School Desegregation Implementation Dates
Number of School Districts
160
140
120
100
80
60
40
20
54 55 56 57 58 59 60 61 62 63 64 65 66 67 68 69 70 71 72 73 74 75 76 77 78 79 80
Implementation Year
Non-South
South


The Effect of
Desegregation Plans on School Segregation
Change in
Black-White Dissimilarity Index
.05
0
-.05
-.1
-.15
-.2
-.25
-.3
-5
-4
-3
-2
-1
0
1
2
3
4
5
6
7
Years Since Desegregation Implementation
95% CI
Dissimilarity Index
School Data: Office of Civil Rights,1968-1982.
Includes all school districts under court order sometime b/w 1954-1980.
Results based on regression model w/school district FE & region*year FE.
Avg black-white dissimilarity index in 1967 among districts that had not yet
implemented a plan was 0.78.

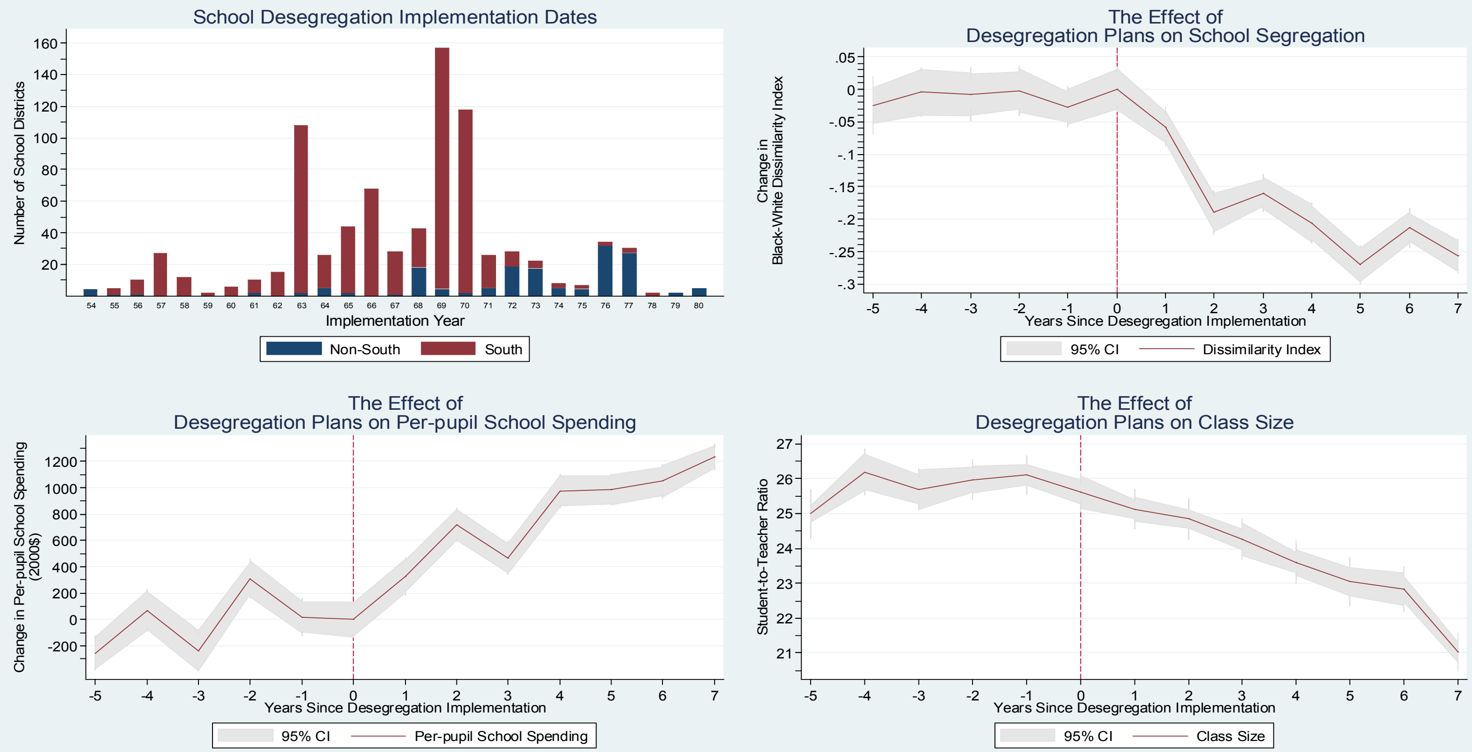


School District Data: Census of Governments,1962-1992.
Balanced panel of all school districts under court order sometime b/w 1954-1980, for which there is at least one outcome measure before and after court order.
Results based on regression model w/school district FE & year FE.
Avg per-pupil school spending in 1967 among districts that had not yet implemented a plan was $2,738 (2000 dollars).

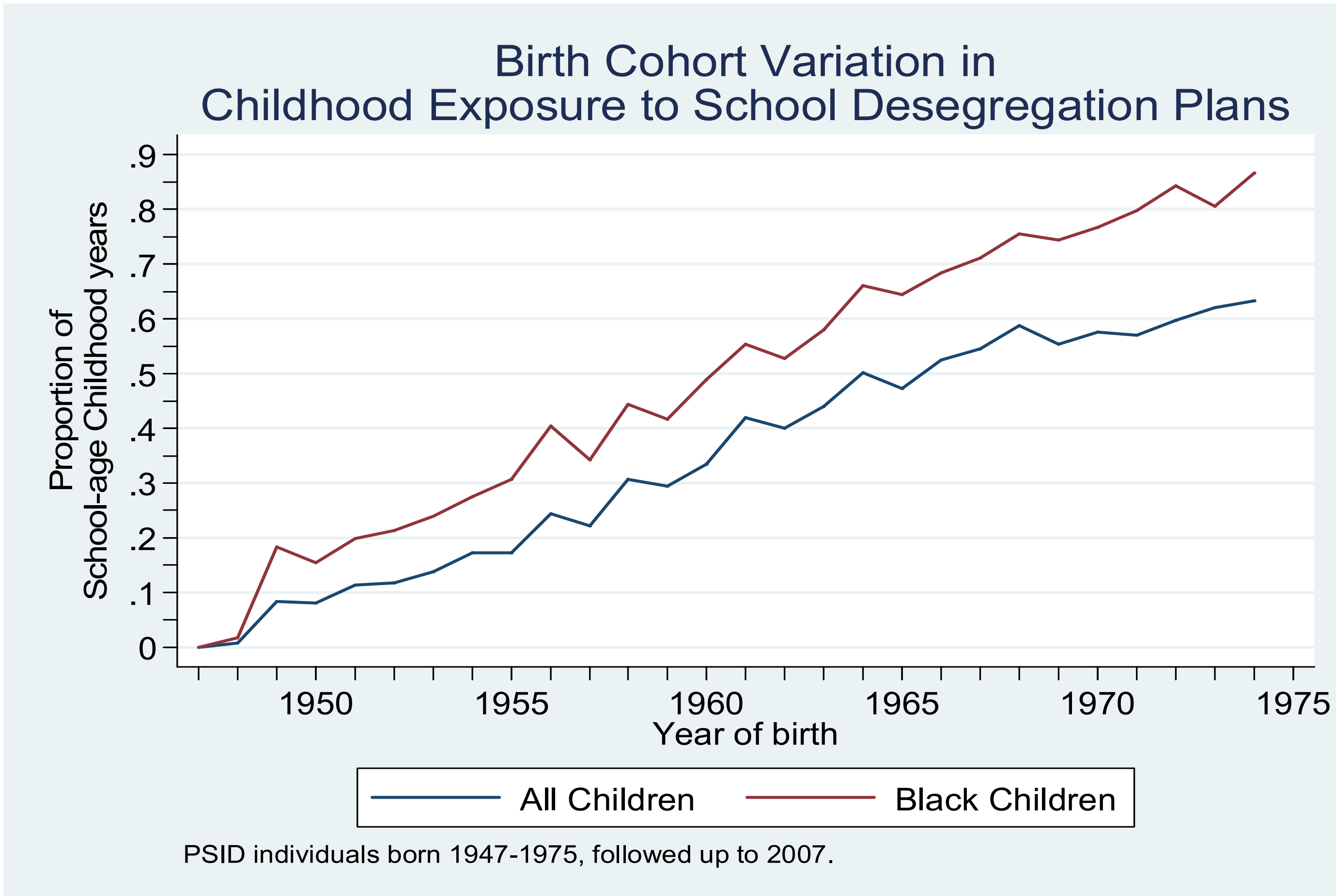

The Effect of
Desegregation Plans on Per-pupil School Spending,
By Revenue Source
Change in Per-pupil School Spending (2000$)
1200
1000
800
600
400
200
0
-200
-5
-4
-3
-2
-1
0
1
2
3
4
5
6
7
Years Since Desegregation Implementation
Total
State Aid
Fed Aid
Local
School District Data: Census of Governments,1962-1992.
Balanced panel of all school districts under court order sometime b/w 1954-1980,
for which there is at least one outcome measure before and after court order.
Results based on regression model w/school district FE & region*year FE.
Avg per-pupil school spending in 1967 among districts that had not yet
implemented a plan was $2,738 (2000 dollars).

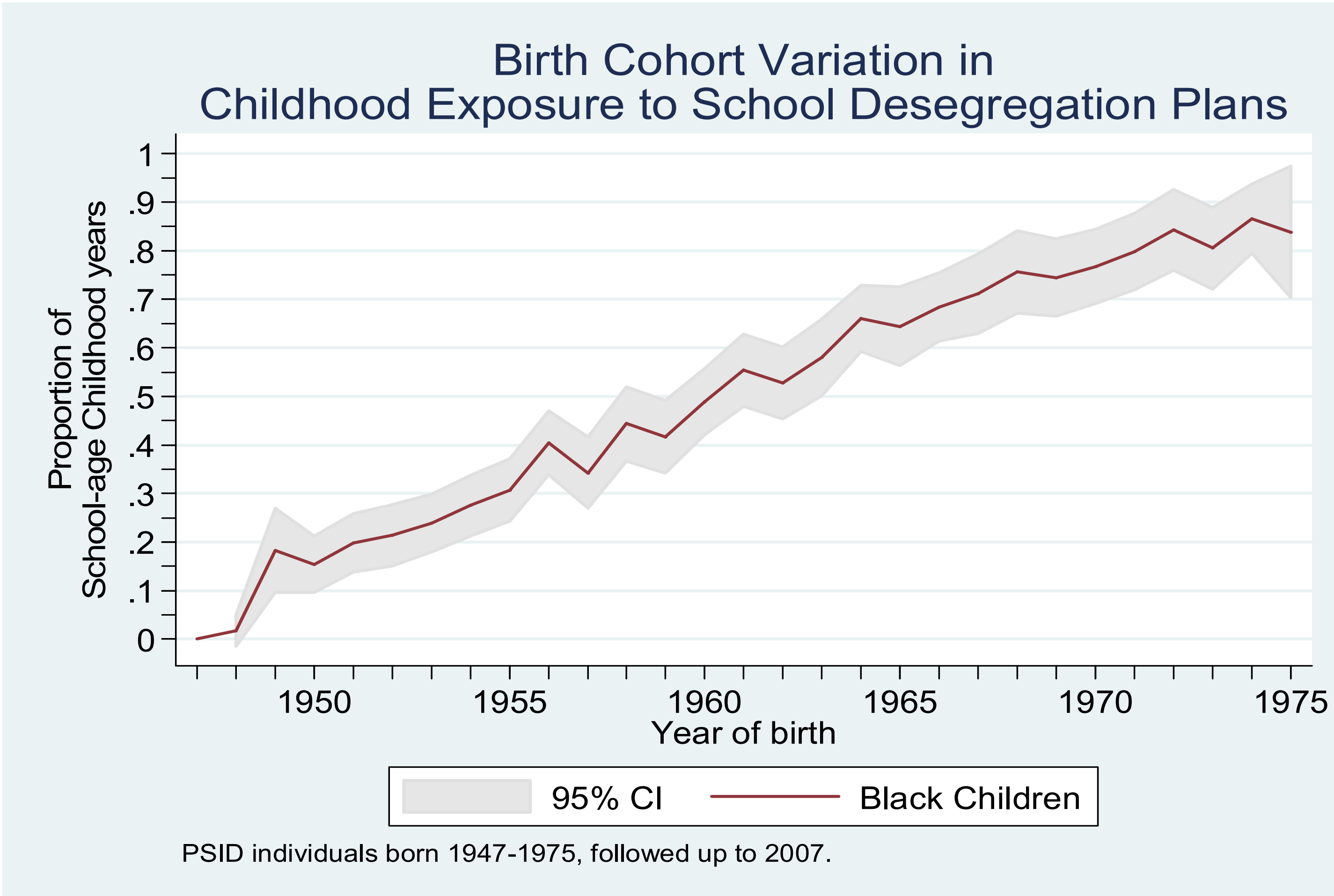

The Effect of
Desegregation Plans on Class Size
Student-to-Teacher Ratio
27
26
25
24
23
22
21
-5
-4
-3
-2
-1
0
1
2
3
4
5
6
7
Years Since Desegregation Implementation
95% CI
Class Size
School Data: Census of Governments and Office of Civil Rights, 1962-1977.
Includes all school districts under court order sometime b/w 1965-1975.
Results based on regression model w/school district FE & region-specific time trends.

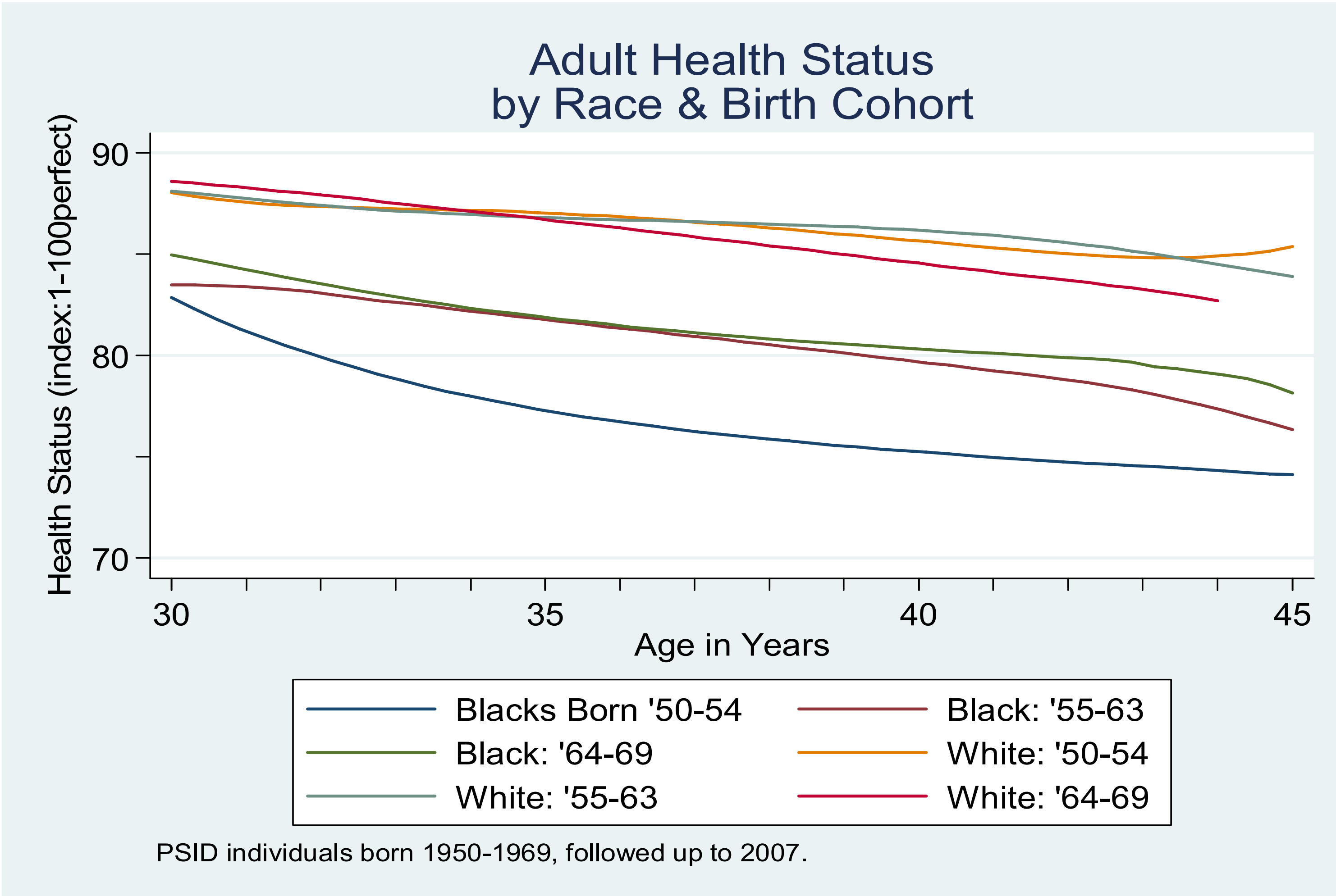
School Desegregation Plan Implementation Dates & The Effects on School Quality

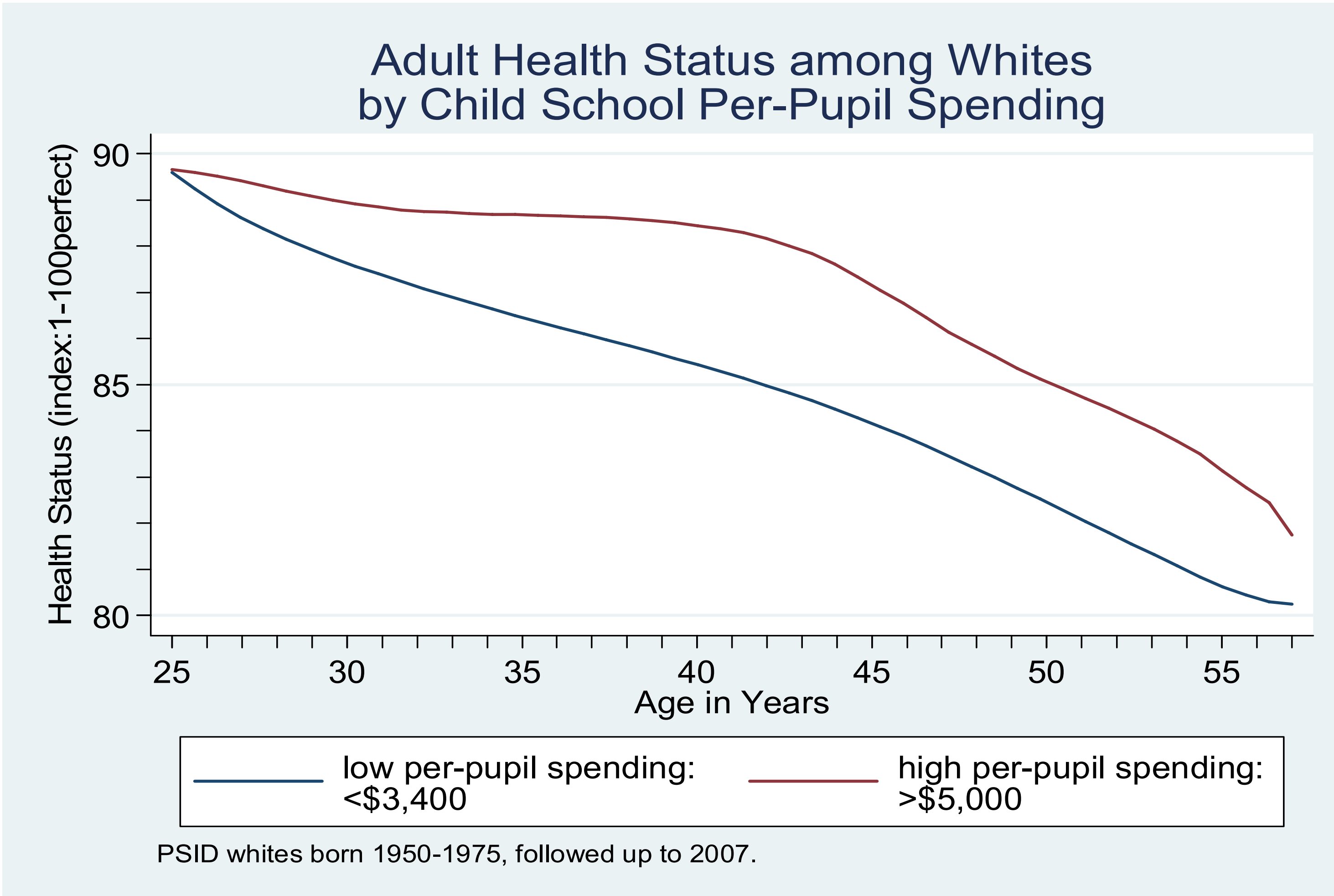


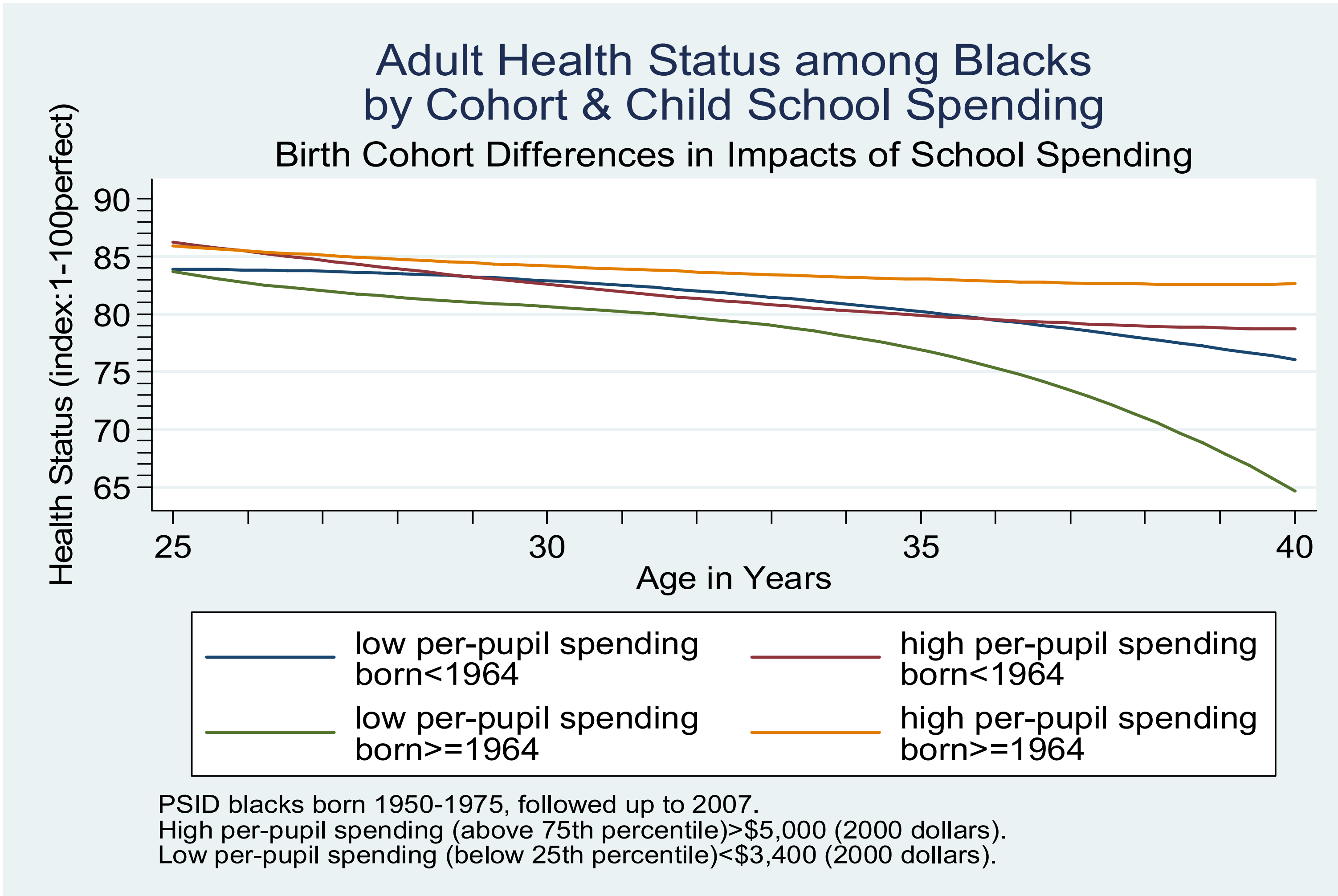
School Data: Census of Governments,1962-1992; Office of Civil Rights,1968-1982.
Note: includes all school districts that were under court order sometime b/w 1954-1980.
Results based on regression models w/school district FE & region*year FE.

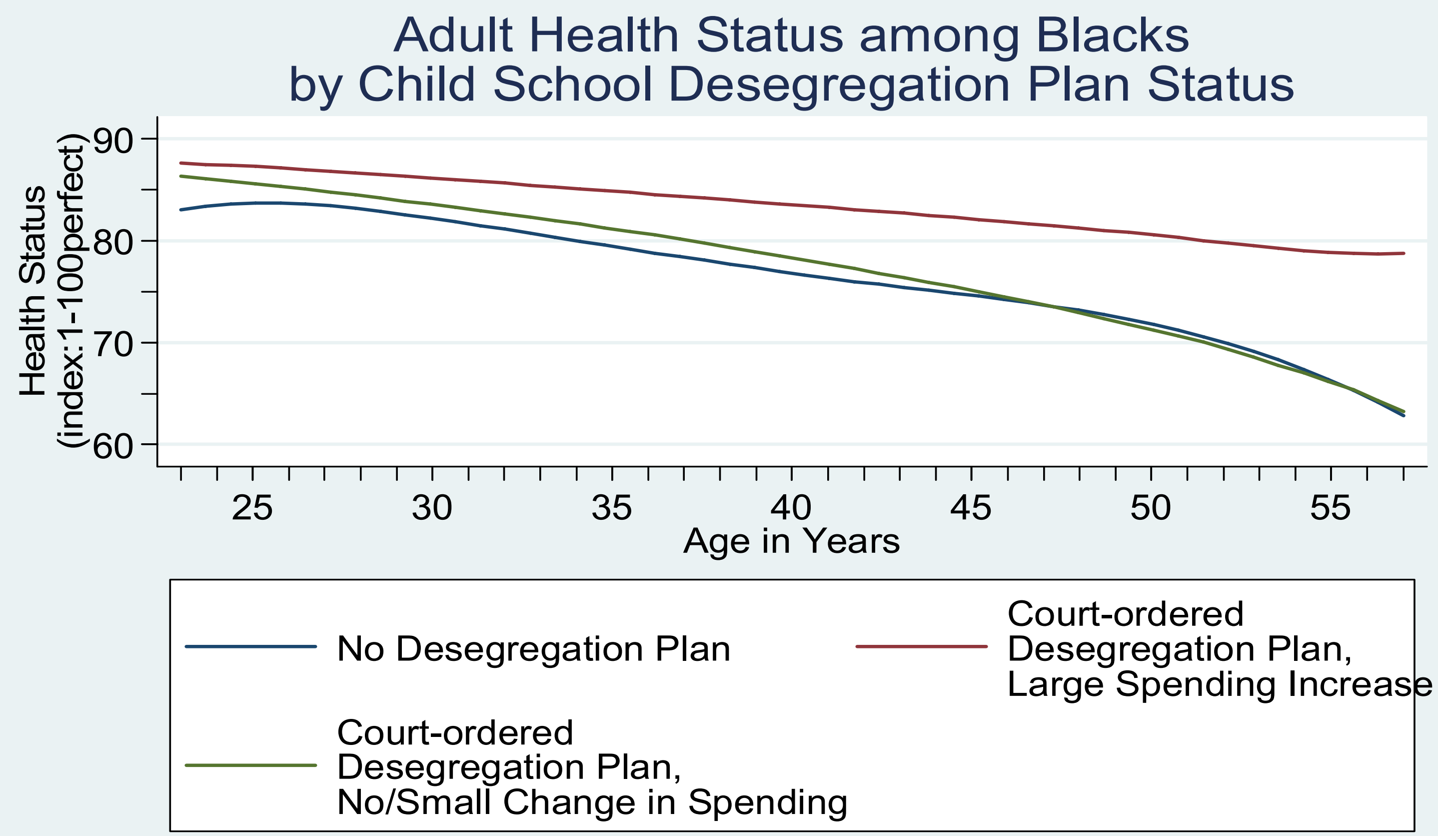

Birth Cohort Variation in
Childhood Exposure to School Desegregation Plans
Proportion of
School-age Childhood years
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1950
1955
1960
1965
1970
1975
Year of birth
All Children
Black Children
PSID individuals born 1947-1975, followed up to 2007.


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


Adult Health Status
by Race & Birth Cohort
Health Status (index:1-100perfect)
90
80
70
30
35
40
45
Age in Years
Blacks Born '50-54
Black: '55-63
Black: '64-69
White: '50-54
White: '55-63
White: '64-69
PSID individuals born 1950-1969, followed up to 2007.

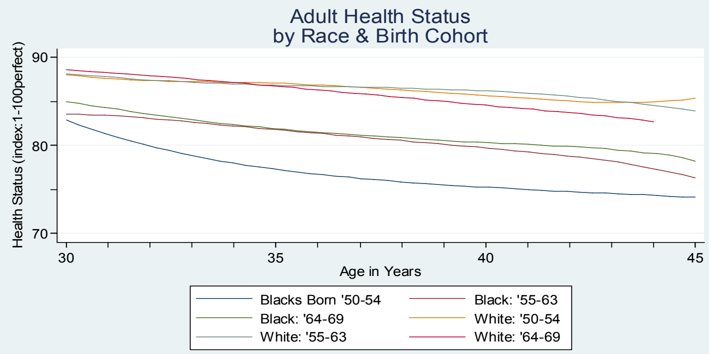

Adult Health Status among Whites
by Child School Per-Pupil Spending
Health Status (index:1-100perfect)
90
85
80
25
30
35
40
45
50
55
Age in Years
low per-pupil spending: <$3,400
high per-pupil spending: >$5,000
PSID whites born 1950-1975, followed up to 2007.


Adult Health Status among Blacks
by Cohort & Child School Spending
Birth Cohort Differences in Impacts of School Spending
Health Status (index:1-100perfect)
90
85
80
75
70
65
25
30
35
40
Age in Years
low per-pupil spending born<1964
high per-pupil spending born<1964
low per-pupil spending born>=1964
high per-pupil spending born>=1964
PSID blacks born 1950-1975, followed up to 2007.
High per-pupil spending (above 75th percentile)>$5,000 (2000 dollars).
Low per-pupil spending (below 25th percentile)<$3,400 (2000 dollars).

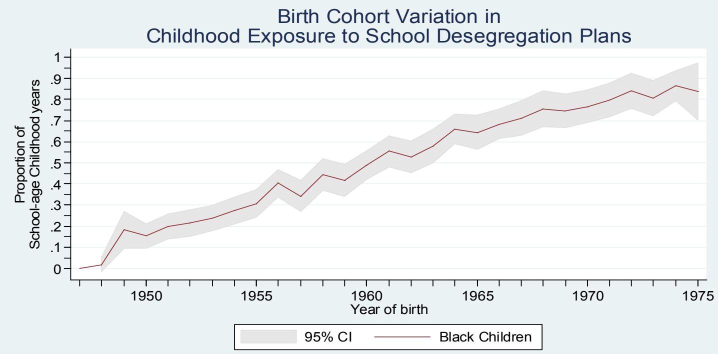


PSID blacks born 1950-1975, followed up to 2007.
Court-ordered desegregation w/large increase in per-pupil spending (>=$1,000);
Court-ordered desegregation w/ no/small change in per-pupil spending (<$300).

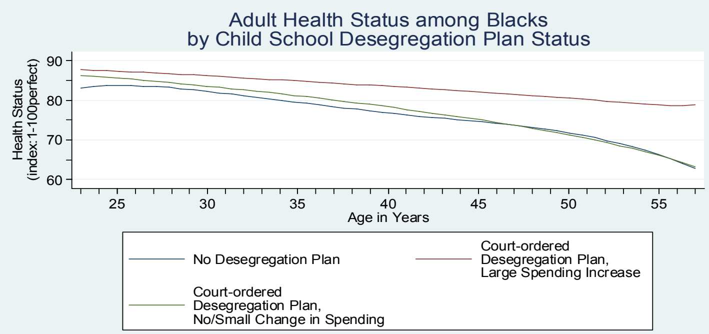
Adult Health Status
by Race, Birth Cohort, & School Desegregation Plan Status



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

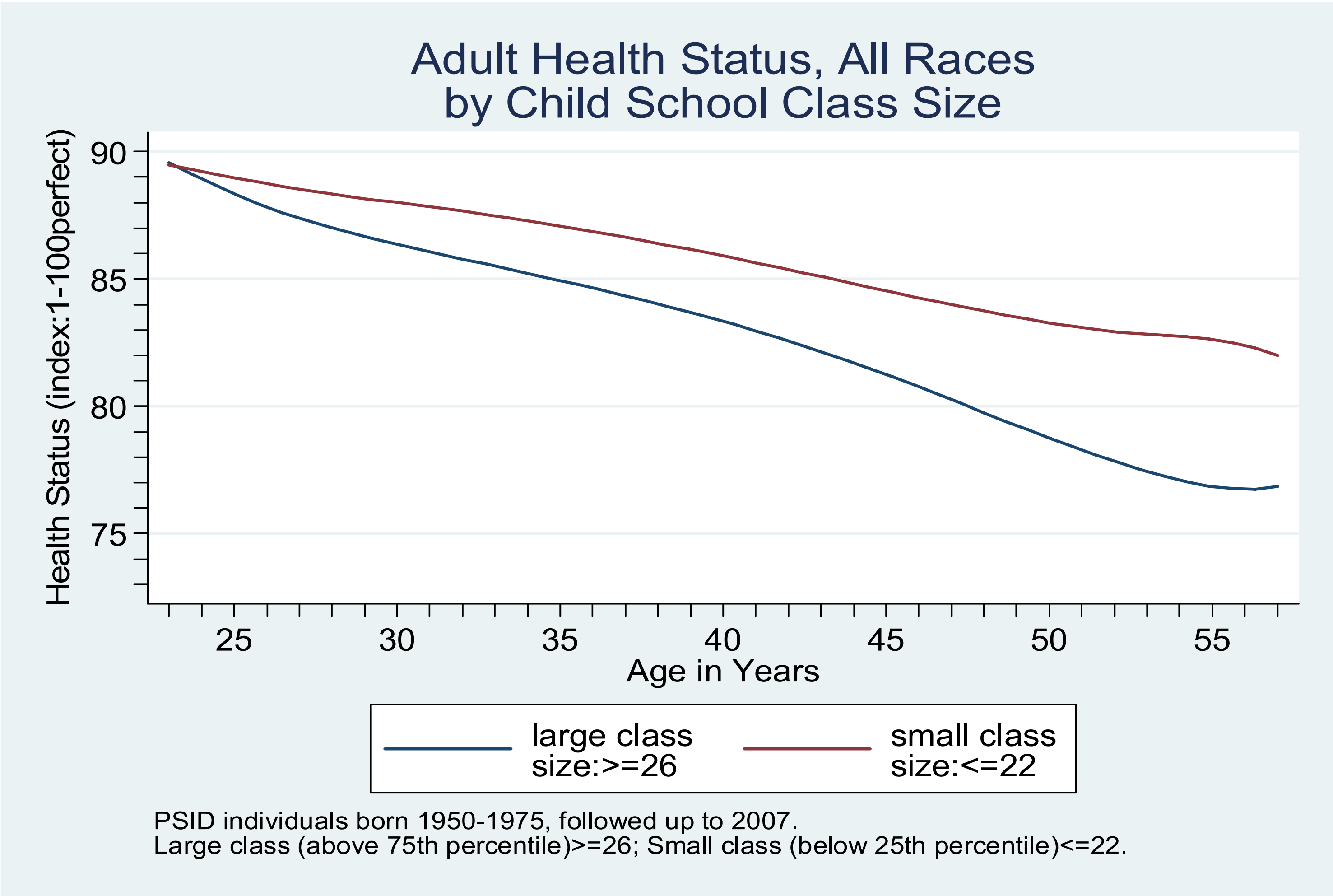


PSID individuals born 1947-1975, followed up to 2007.

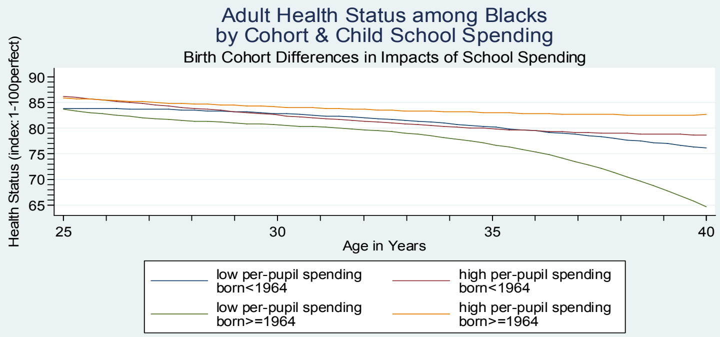


PSID blacks born 1950-1975, followed up to 2007.
Court-ordered desegregation w/large increase in per-pupil spending (>=$1,000);
Court-ordered desegregation w/ no/small change in per-pupil spending (<$300).

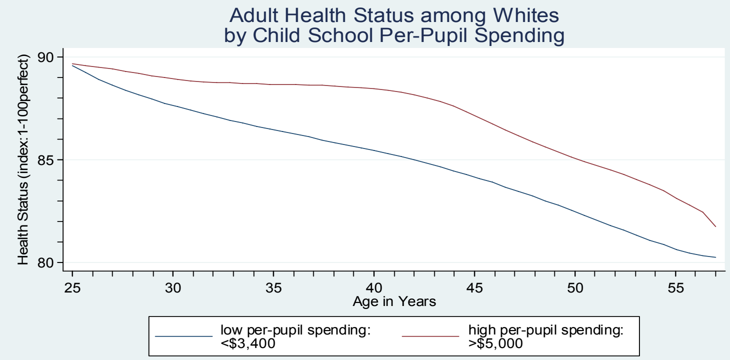
Data: PSID, 1968-2007
(Individuals born b/w 1950-1975)


Adult Health Status, All Races
by Child School Class Size
Health Status (index:1-100perfect)
90
85
80
75
25
30
35
40
45
50
55
Age in Years
large class size:>=26
small class size:<=22
PSID individuals born 1950-1975, followed up to 2007.
Large class (above 75th percentile)>=26; Small class (below 25th percentile)<=22.

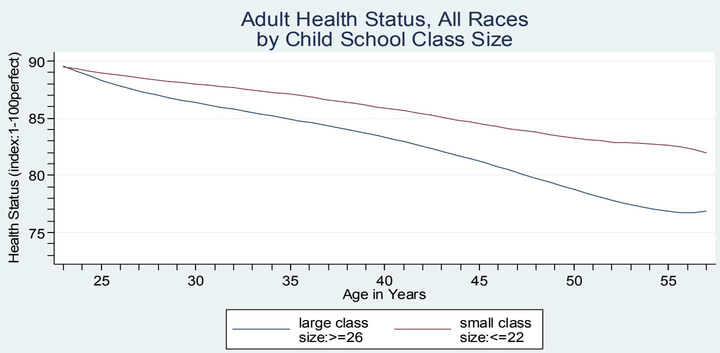
Adult Health Status
by Child School Quality



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



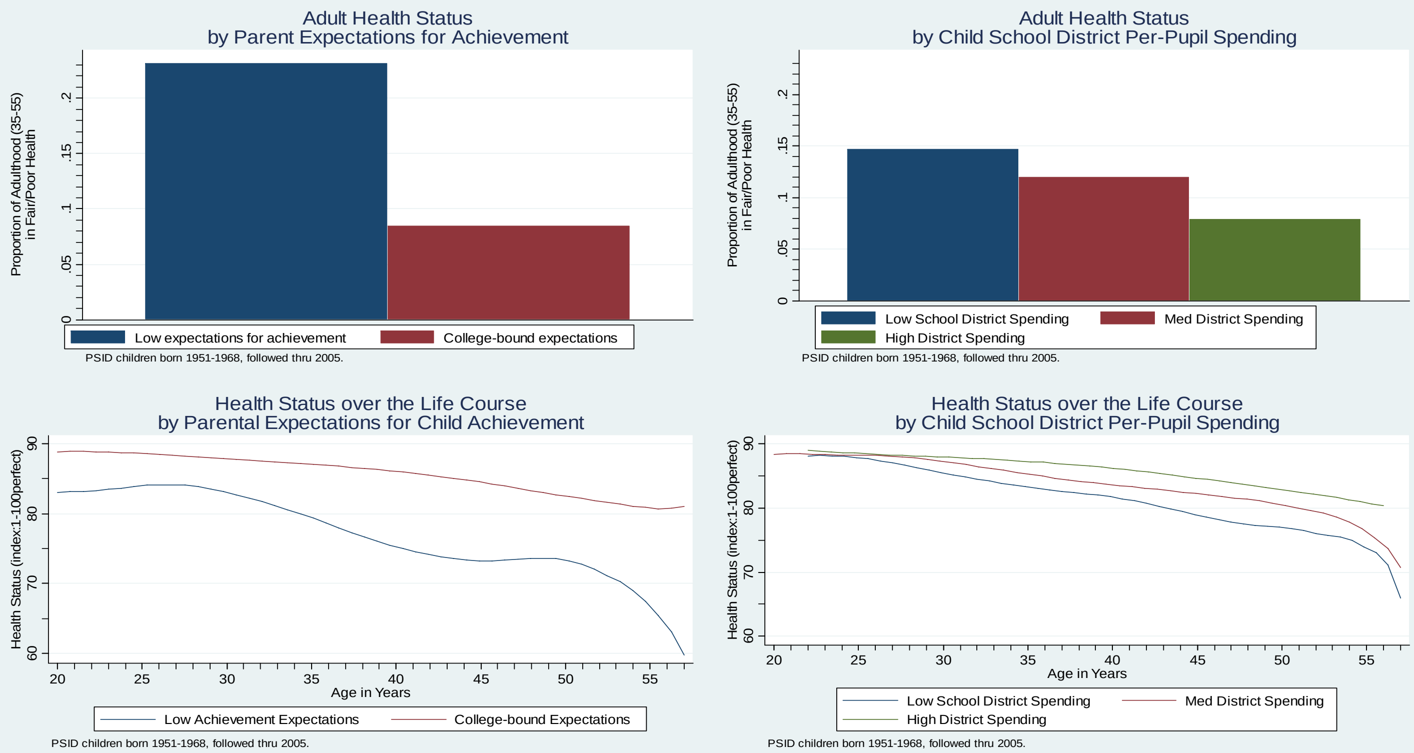
PSID whites born 1950-1975, followed up to 2007.



PSID individuals born 1950-1975, followed up to 2007.
Large class (above 75th percentile)>=26; Small class (below 25th percentile)<=22.

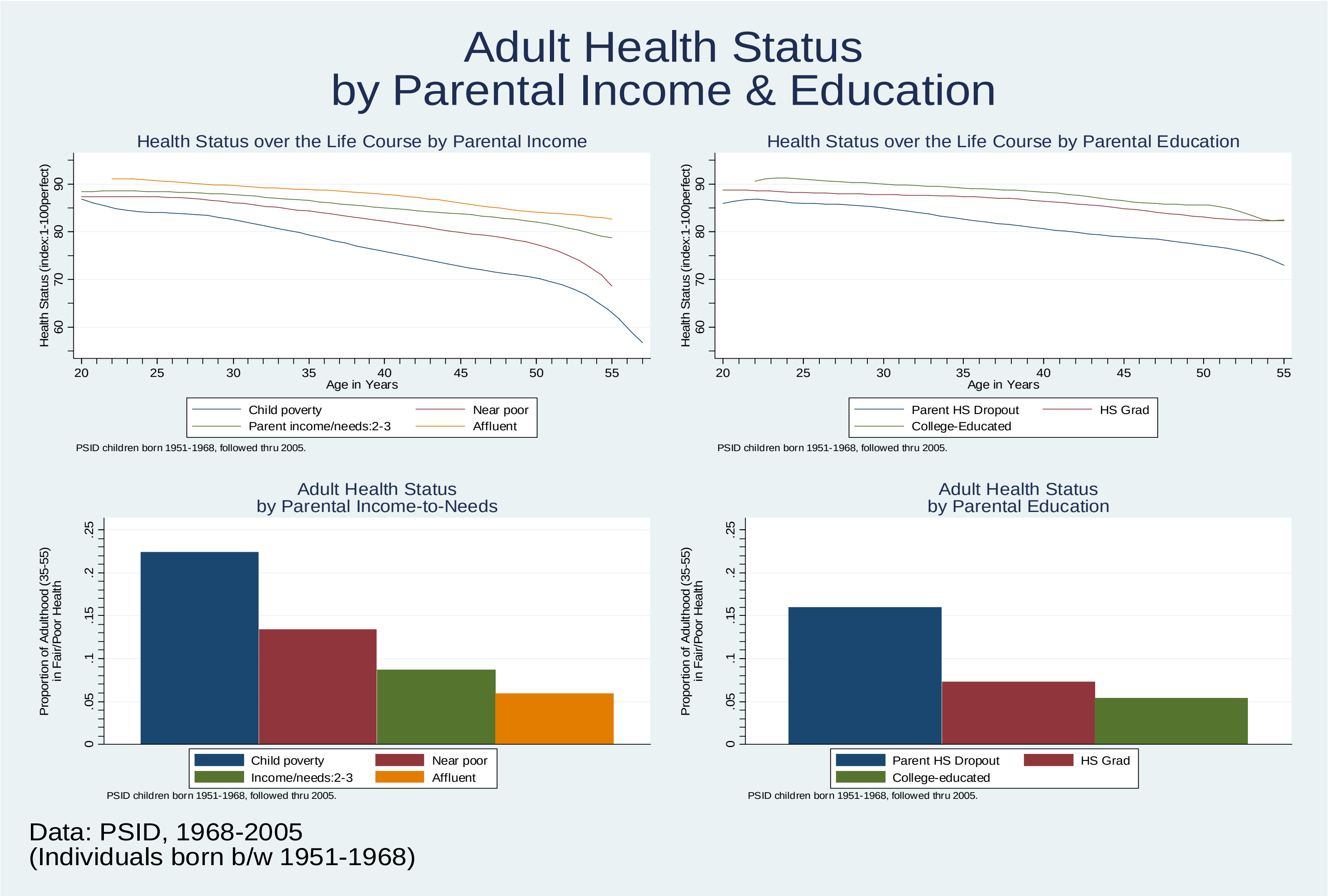
Data: PSID, 1968-2007
(Individuals born b/w 1950-1975)

Adult Health Status
by School Spending & Parent Expectations for Achievement



Data: PSID, 1968-2005
(Individuals born b/w 1951-1968)

Figure 1.



Adult Health Status
by Parental Income & Education
Health Status over the Life Course by Parental Income
Health Status (index:1-100perfect)
Age in Years
Child poverty
Near poor
Parent income/needs:2-3
Affluent
PSID children born 1951-1968, followed thru 2005.
Health Status over the Life Course by Parental Education
Health Status (index:1-100perfect)
Age in Years
Parent HS Dropout
HS Grad
College-Educated
PSID children born 1951-1968, followed thru 2005.
Adult Health Status
by Parental Income-to-Needs
Proportion of Adulthood (35-55)
in Fair/Poor Health
Child poverty
Near poor
Income/needs:2-3
Affluent
PSID children born 1951-1968, followed thru 2005.
Adult Health Status
by Parental Education
Proportion of Adulthood (35-55)
in Fair/Poor Health
Parent HS Dropout
HS Grad
College-educated
PSID children born 1951-1968, followed thru 2005.
Data: PSID, 1968-2005
(Individuals born b/w 1951-1968)

Table 1. Race & SES Differences in Adult Health (Age 20-57):
Importance of Child School Quality & Family Background
(Dependent variable: general health status in adulthood)
Interval Regression Model: 100pt-scale, 100=perfect health

	Raw race gap	Controls for Fam bckgrd	Controls for Child School + Nhood + Fam
Childhood factors	(1)	(2)	(3)
Black born 1964-68	-3.3108***	0.8150	1.1368
Non-Hispanic white (ref category), no cohort diffs for whites			
Black born 1955-63	-4.6944***	-0.4388	-0.1200
	(0.5769)	(0.6751)	(0.7876)
Black born 1950-54	-9.2700***	-4.6846***	-4.8451***
Family income-to needs ratio (avg during 1967-1972), spline:			
Income-to-needs ratio*ratio is <1		3.0771*	3.9330*
		(2.0347)	(2.0714)
Income-to-needs ratio* ratio is 1 to 3		1.4639***	0.8579***
		(0.3140)	(0.3115)
Income-to-needs ratio* ratio is >3		0.2464*	0.2386**
		(0.1263)	(0.1207)
Parent head's education:			
High school dropout		-2.0383***	-1.6643***
High school graduate (reference category)		(0.4139)	(0.4034)
College-educated		0.8006**	0.7914**
		(0.3993)	(0.3927)
Residential segregation dissimilarity index, 1970 (MSA)			-0.2571
			(0.3494)
Residential segregation dissimilarity index*Black			1.8592**
			(0.9393)
Residential segregation dissimilarity index*Blacks in South			-3.1875***
			(1.1278)
Child School factors			
School segregation dissimilarity index			0.3921*
			(0.2249)
School segregation dissimilarity index*Black			0.5285
			(0.8768)
School segregation dissimilarity index*Blacks in South			0.0009
			(1.0170)
Ln(School district per-pupil spending)			1.4325*
			(0.8211)
Ln(School district per-pupil spending)*Blacks born before 1964			-1.8365
			(1.8288)
Large class size (≥27)			-1.4214***
			(0.4728)
Parental low expectations for child achievement			-2.7375***
College-bound expectations (reference categeory)			(0.7568)
Neighborhood low expectations for child achievement			-0.7615*
			(0.5462)
Age - 30	-0.1718***	-0.2002***	-0.2037***
	(0.0210)	(0.0206)	(0.0205)
Constant	88.4202***	91.2839***	93.1779***
	(0.3032)	(2.2171)	(2.5714)
Log-likelihood	-1505567.4	-1483745.1	-1474852.1
Number of counties	272	272	272
Number of neighborhoods	1,468	1,468	1,468
Number of families	2,072	2,072	2,072
Number of individuals	5,607	5,607	5,607
Number of person-year observations	60,280	60,280	60,280

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

Note: All models include controls for age squared, age cubed, gender, year of birth; columns (2)-(3) include controls for region of birth, birth order, birth weight, whether born to a two-parent family, child helth insurance coverage, parental smoking and alcohol use, and indices intended to capture parental aspirations/motivation and long-term planning horizon (rate of time preference proxy); and column (3) also includes the following controls for neighborhood/housing quality: neighborhood poverty rate, whether high crime, insulation problems, plumbing problems, and connectedness to informal sources of support (coefficients supressed to conserve space). Robust standard errors in parentheses and all standard errors are Huber-corrected, clustered on neighborhood.

Table 2. Age-Profile of Effects of Child School Quality on Adult Health: Young Adulthood vs. Mid-Adulthood Ages

(Dependent variable: general health status in adulthood)

Interval Regression Model: 100pt-scale, 100=perfect health

	Young Adulthood (Ages 20-34)	Mid-Adulthood (Ages 35-57)
Childhood factors	(1)	(2)
Black born 1964-68	0.6929	1.6845
Non-Hispanic white (ref category), no cohort diffs for whites		
Black born 1955-63	-0.1983	-0.0398
	(0.7450)	(1.1698)
Black born 1950-54	-3.3444***	-4.8194***
Residential segregation dissimilarity index, 1970 (MSA)	-0.0550	-0.4547
	(0.3056)	(0.5042)
Residential segregation dissimilarity index*Black	1.2405	2.5991**
	(1.0138)	(1.1954)
Residential segregation dissimilarity index*Blacks in South	-2.1688*	-4.2281***
	(1.1505)	(1.5114)
Child School factors		
School segregation dissimilarity index	0.0368	0.7700**
	(0.2081)	(0.3032)
School segregation dissimilarity index*Black	0.4395	0.8560
	(0.9156)	(1.1599)
School segregation dissimilarity index*Blacks in South	-0.1947	0.0619
	(1.0305)	(1.3826)
Ln(School district per-pupil spending)	1.1372+	2.3680*
	(0.6958)	(1.2615)
Ln(School district per-pupil spending)*Blacks born before 1964	-2.3262+	-1.7634
	(1.5603)	(2.8133)
Large class size (≥27)	-0.5622+	-2.1092***
	(0.4158)	(0.6543)
Parental low expectations for child achievement	-1.4410**	-4.1177***
College-bound expectations (reference categeory)	(0.6404)	(1.1166)
Neighborhood low expectations for child achievement	-0.5214	-1.0217*
	(0.5125)	(0.7838)
Family income-to needs ratio (avg during 1967-1972), spline:		
Income-to-needs ratio*ratio is <1	4.9956***	3.0118
	(1.8413)	(3.1673)
Income-to-needs ratio* ratio is 1 to 3	0.7197***	1.0337**
	(0.2785)	(0.4726)
Income-to-needs ratio* ratio is >3	0.1975*	0.2424+
	(0.1088)	(0.1594)
Parent head's education:		
High school dropout	-1.1414***	-2.2468***
High school graduate (reference category)	(0.4006)	(0.5612)
College-educated	1.2675***	0.3263
	(0.3714)	(0.5469)
Age - 30	-0.2201***	-0.2475**
	(0.0345)	(0.0988)
Constant	93.3903***	93.6366***
	(2.2728)	(4.0094)
Log-likelihood	-719883.94	-742211.9
Number of counties	271	250
Number of neighborhoods	1,434	1,277
Number of families	2,005	1,790
Number of individuals	5,245	4,144
Number of person-year observations	32,079	28,201

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

Note: All models include controls for age squared, age cubed, gender, year of birth, region of birth, birth order, birth weight, whether born into a two-parent family, child health insurance, parental smoking and alcohol use, and indices intended to capture parental aspirations/motivation and long-term planning horizon (rate of time preference proxy), and also include the following controls for neighborhood/housing quality: neighborhood poverty rate, whether high crime, insulation problems, plumbing problems, and connectedness to informal sources of support (coefficients supressed to conserve space). Robust standard errors in parentheses and all standard errors are Huber-corrected, clustered on neighborhood.

Childhood Factors and Racial Health Disparities in Adulthood (Age 20-57)

	(Dependent variable: general health status in adulthood)					
	Raw race gap	Controls for Fam bckgrd	Controls for Child Nhood, School + Fam bckgrd	Child backgrd + Parental health	Only Adult Nhood + SES	Child bckgrd + Adult SES
Non-Hispanic white (ref category), no cohort diffs for whites	(2)	(3)	(4)	(5)	(6)	(7)
Black born 1964-68	-4.8589***	-0.9246***	0.3890	0.7828	-3.5293***	1.9482***
Black born 1955-63	-5.0405***	-1.2908***	-0.1578	0.3989	-4.1945***	1.0903***
	(0.2112)	(0.2163)	(0.2419)	(0.2458)	(0.1944)	(0.2603)
Black born 1950-54	-10.1586***	-6.3713***	-6.1087***	-5.5713***	-9.0681***	-4.8914***
# of counties	270	270	270	270	270	270
# of neighborhoods	1,428	1,428	1,428	1,428	1,428	1,428
# of families	1,935	1,935	1,935	1,935	1,935	1,935
# of individuals	4,705	4,705	4,705	4,705	4,705	4,705
# of person-yr obs	51,082	51,082	51,082	51,082	51,082	51,082

Note: Results based on 4-Level Hierarchical Random Effects Interval Regression Model Estimates: 100pt-scale, 100=perfect health.

Table 5. Long-run Effects of Childhood School Desegregation Plans on Adult Health: Blacks born in the 1950s and 1960s (Dependent variable: general health status in adulthood), ages 25-45

Interval Regression Model: 100pt-scale, 100=perfect health

	Blacks	
	Born in 1950s	**Born in 1960s**
	(1)	(2)
School Desegregation Plan Exposure $_{(age\ 5\text{-}17)}$	4.1332***	4.4070***
	(1.5640)	(1.7185)
Specification	Difference-in-Difference	
Childhood County Fixed Effect?	yes	yes
Family background controls?	yes	yes
Person-year observations	9,955	6,690
Number of Individuals	1,008	842
Number of Families	467	404
Number of Counties	70	68

Robust Standard errors in parentheses (clustered on individual)

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

Notes: The comparison group for the estimated desegregation plan effects are blacks who grew up in school districts that later implemented desegregation plans but which occurred after these individuals were 18 or older. Sample includes individuals born between 1951-1959 (1960-1969) who grew up in school districts that implemented desegregation plans at some point between 1954-1990. All models control for age (in quadratic form) and the following set of child family background factors: parental income, parental education, mother's marital status at birth, birth weight, and parental smoking and alcohol use. PSID sample weights are used in all specifications.

Table 6. Long-run Effects of Childhood School Desegregation Plans on Adult Health
(Dependent variable: general health status in adulthood)

Interval Regression Model: 100pt-scale, 100=perfect health

	(1)	(2)	(3)
School Desegregation Plan during Childhood	-0.2151	-0.8434	-0.4205
	(0.6533)	(0.8920)	(1.4426)
School Desegregation Plan during Childhood*Black	2.7494***	3.4560**	4.5864*
	(1.0594)	(1.5207)	(2.3710)
School Desegregation Plan during Childhood* ↑ΔPer-Pupil Spending$_{(t-1,t+3)}$			-1.5545
			(1.4966)
School Desegregation Plan during Childhood* ↑ΔPer-Pupil Spending$_{(t-1,t+3)}$ *Black			3.2650**
			(1.6303)
Specification		Difference-in-Difference	
Childhood County Fixed Effect?	no	yes	yes
Person-year observations	71,714	71,714	24,767
Number of Individuals	7,111	7,111	2,603
Number of Families	2,275	2,275	789
Number of Counties	299	299	84

Robust Standard errors in parentheses (clustered on individual)

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

Notes: The comparison group for the estimated desegregation plan effects are individuals who grew up in school districts that later implemented desegregation plans but which occurred after these individuals were 18 or older. Regressions include controls for year of birth and an indicator for whether the individual's child school district ever implemented desegregation plans between 1954-1990 interacted with race. All models control for age (in quadratic form) and the following set of child family background factors: parental income, parental education, mother's marital status at birth, birth weight, and parental smoking and alcohol use. PSID sample weights are used in all specifications. Column (3) is restricted to individuals who grew up in school districts that implemented desegregation plans between 1954-1990 for which I have school district per-pupil spending information 1 year before and 3 years after initial implementation, obtained from school district finance data (1962-1982).

↑Δ represents the increase in per-pupil spending from one year prior to the implementation of desegregation to the third year after desegregation implementation--this value has been centered around the average 5-year induced increase across all districts under court-order ($1,000), so that the main effect captures the impact of desegregation plans associated with the average change in per-pupil spending; to facilitate interpretation of marginal effects, the units of the per-pupil spending are in thousands of dollars, so that a 1-unit change represents a $1,000 change in spending (2000 dollars).

Table 7. Long-run Effects of Childhood School Desegregation Plans on Adult Health: The Role of Educational Attainment (Dependent variable: general health status in adulthood), ages 25-45

Interval Regression Model: 100pt-scale, 100=perfect health

	Whites born in 1950s	Blacks born in 1950s	
	(1)	(2)	(3)
School Desegregation Plan Exposure (age 5-17)	-3.5121	4.1332***	2.1868
	(3.2101)	(1.5640)	(2.2903)
Years of education			1.4047***
Specification	Difference-in-Difference		
Childhood County Fixed Effect?	yes	yes	yes
Family background controls?	yes	yes	yes
Person-year observations	5,368	9,955	9,955
Number of Individuals	479	1,008	1,008
Number of Families	260	467	467
Number of Counties	75	70	70

Robust Standard errors in parentheses (clustered on individual)

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

Notes: The comparison group for the estimated desegregation plan effects are individuals who grew up in school districts that later implemented desegregation plans but which occurred after these individuals were 18 or older. Sample includes individuals born between 1951-1959 who grew up in school districts that implemented desegregation plans at some point between 1954-1990. All models control for age (in quadratic form) and the following set of child family background factors: parental income, parental education, mother's marital status at birth, birth weight, and parental smoking and alcohol use. PSID sample weights are used in all specifications.

Long-run Effects of Child School Quality on Adult Health: Sibling Fixed Effect Estimates

(Dependent variable: general health status in adulthood), (ages 20-57) Interval Regression Model: 100pt-scale, 100=perfect health				
Childhood school-related factors	Controls for School spending + Child Nhood + Fam	Add Controls: Pre-school +School quality	Sibling FE: Pre-school +School quality	Sibling FE: School desegregation
Pre-school years:	(1)	(2)	(3)	(4)
Head Start program participation		-0.1968	1.1763***	
Did not attend pre-school (reference category)		(0.7683)	(0.2213)	
Other Preschool program		0.8337**	0.5566***	
		(0.4231)	(0.2146)	
School-age years:				
Ln(School district per-pupil spending)$_{(age\ 12\text{-}17)}$	2.9752*	2.2589**	1.3032**	
	(1.7805)	(0.8828)	(0.5448)	
School Desegregation Plan Exposure $_{(age\ 5\text{-}17)}$				-2.0441***
				(0.5363)
School Desegregation Plan Exposure$_{(age5\text{-}17)}$*Black				3.2798***
				(0.6845)
Ever attended private school		0.2264	-1.2586***	
		(0.4359)	(0.3136)	
Proportion of childhood attended private school		2.8768**	-1.4269*	
		(1.1245)	(0.8080)	
Placement in gifted/advanced curriculum		0.7548*	1.1944***	
		(0.4350)	(0.1995)	
Grade repetition		-1.4196**	-0.7856***	
		(0.6908)	(0.2030)	
Placement in special education		-4.6093***	-4.6394***	
		(1.1523)	(0.3274)	
Ever suspended/expelled from school		-1.6575***	-1.0427***	
		(0.4915)	(0.1622)	
Parental low expectations for child achievement	-2.6270*	-1.9203**		
College-bound expectations (reference category)	(1.5272)	(0.9004)		
Neighborhood low expectations for child achievement	-1.3868	-1.0733+		
	(1.2782)	(0.6675)		
Expenditures per student at college attended (2000$):				
<$4,000	3.4376***			
Did not attend college (reference category)	(1.1376)			
$4,000-10,000	3.2344***			
	(1.0405)			
>$10,000	3.7513**			
	(1.5527)			
Age - 30	-0.2364*	-0.2378***	-0.2532***	-0.2675***
	(0.1520)	(0.0235)	(0.0115)	(0.0104)
Constant	73.9289***	83.3994***	83.2785***	87.7899***
	(6.9871)	(2.7643)	(1.2179)	(0.6034)
Sibling Fixed Effect?	no	no	yes	yes
Person-year observations	14,603	45,758	43,393	64,000
Number of Individuals	1,085	4,224	3,984	6,304
Number of Families	779	1,794	1,554	1,762

Robust Standard errors in parentheses (clustered on individual)

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

Note: All models include controls for age squared, age cubed, gender, year of birth, region of birth, birth order, birth weight, whether born into a two-parent family, parental income and education, child health insurance, parental smoking and alcohol use, and indices intended to capture parental aspirations/motivation and long-term planning horizon (rate of time preference proxy), and also include the following controls for neighborhood/housing quality: neighborhood poverty rate, whether high crime, insulation problems, plumbing problems, and connectedness to informal sources of support (coefficients supressed to conserve space). The non-fixed effect models in columns (1) and (2) are sample-weighted to account for the oversampling of blacks and low-income families.