School Desegregation and Long-Run Health

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Abstract

I investigate the impact of court-ordered school desegregation that followed *Brown v. Board of Education (1954)* on long-run health outcomes and behaviors among Black Americans. Using detailed survey data on adults older than 50 years old from the Health and Retirement Study and a difference-in-differences design, I find that desegregation improved Black self-reported health, preventive care use, and mortality decades later. I find no detectable changes in chronic conditions or unhealthy behaviors such as smoking and drinking. Across demographic subgroups, Black self-reported health improvements are related to desegregation's positive effects on racial integration and high school completion, but changes in preventive care and mortality are not. Therefore, while desegregation's consequences for educational attainment may have facilitated improvements in some health outcomes, desegregation itself was also important for health, independent of its educational impacts.

1 Introduction

Despite persistent gaps in Black-white health outcomes such as life expectancy (Schwandt et al., 2021) and chronic conditions (Williams & Jackson, 2005) in the United States, many of these outcomes began to partially converge during the 20th century (Boustan & Margo, 2016). This occurred contemporaneously with educational and wage improvements for Black Americans, which may have contributed to their positive health trends. One of the most important milestones for educational equity was *Brown v. Board of Education (1954)*, which ruled that racial segregation in public schools was unconstitutional and resulted in greater levels of educational resources for

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Black students. Pre-desegregation increases in school quality for Black students have been linked to improved relative earnings (Card & Krueger, 1992a,b; Welch, 1973) and health (Frisvold & Golberstein, 2011), but the full health implications of court ordered desegregations have so far not been studied. Understanding these effects is important for explaining the factors contributing to improvements in Black health, as well as revealing the far-reaching consequences of school desegregation.

In this paper, I examine the reduced form effects of court-ordered school desegregation on the long-run health outcomes and behaviors of impacted Black students, and then explore the role of changes in educational attainment and peer composition in mediating these effects. *Brown v. Board of Education (1954)* did not specify a clear timeline for desegregation, so a gradual rollout of integration plans by individual districts occurred over the following three decades. As documented by R. C. Johnson (2019) and Guryan (2004), most school districts did not adopt desegregation plans until a court ordered them to do so, typically after the NAACP Legal Defense and Educational Fund filed a lawsuit. As a result, court orders for school districts to desegregate stretched from the 1950s through the 1980s, with earlier cases concentrated in Southern states, and later cases focusing on overcoming the effects of residential segregation in Northern districts. Guryan (2004) emphasizes that legal precedent, rather than a sense of where desegregation would be the most impactful for Black students, was the primary factor in how districts were targeted, creating variation in desegregation timing. Students therefore had differential exposure to desegregated schools based on their year of birth and the location where they attended school. I exploit these differences in exposure through a difference-in-differences empirical strategy to determine the effects of a desegregation on health, which could occur through either the direct effects of desegregation or its downstream consequences on other outcomes. Using similar empirical strategies, previous research has documented that for Black students, desegregation court orders successfully resulted in more racial integration (Reber, 2005), increased funding per pupil (Reber, 2010), led to increased educational attainment (Guryan, 2004; R. C. Johnson, 2011; Anstreicher et al., 2022), and raised future earnings (R. C. Johnson, 2011; Anstreicher et al., 2022).

While there are many channels through which desegregation could affect Black health, these channels can be grouped into those that had direct effects on health through childhood experiences and those that had indirect effects on health mediated through educational attainment. Most channels would be expected to improve health, although it is also possible that factors such as exposure to violent responses to desegregation could worsen health. Potential direct effects of desegregation on health include peer effects in positive or negative health behaviors due to racial integration, improvements in trust toward the health care system, or health consequences of changes in exposure to discrimination. In particular, public health research has posited the theory of "weathering effects," in which physiological responses to racial discrimination may cause early health deterioriation, such as through high blood pressure or cholesterol levels (Geronimus, 1992; Geronimus et al., 2006). Indirect effects of desegregation on health could also come through its previously documented educational attainment effects; a large literature has documented robust correlations between education levels and health outcomes, and some evidence suggests a causal relationship (Lleras-Muney, 2022). While these effects may occur for many reasons, the same educational effects on health may apply to the case of desegregation court orders.

This study is the first, to my knowledge, to comprehensively examine the impact of school desegregation on a wide range of health outcomes and to investigate the role of educational effects. R. C. Johnson (2011) applies the same reduced form empirical strategy to PSID data to show that desegregation improves self-reported overall health among Black respondents among a younger age group (ages 20-50 at the time of the survey). Kim et al. (2022) study effects of segregation after some districts were released from court-ordered desegregation plans in 1991, and find that Black students exposed to more segregation reported worse health and smoked more. Other studies have examined the impact of desegregation on birth outcomes, with Shen (2018) estimating that desegregation decreased pre-term births among Southern Black mothers by 1.7 percentage points and Liu et al. (2012) finding that desegregation lowered the risk of Black teen pregnancy. These studies, however, may not capture the long-run health effects of desegregation if health returns vary over the life cycle (Gehrsitz & Williams Jr, 2022), and they consider a limited range of health measures. Improvements in school quality for Black students prior to desegregation have also been linked to better health, with Aaronson et al. (2021) finding mortality reductions as a result of the Rosenwald schools and Frisvold & Golberstein (2011) finding positive impacts of greater pre-desegregation school funding for Black students in the South on self-reported health, smoking, obesity, and mortality.

My sample consists of over 5,000 non-Hispanic Black respondents and 18,000 non-Hispanic white respondents of the Health and Retirement Study (HRS) who attended school in one of the 36 states with at least one desegregation order. In the restricted HRS data, I observe many health outcomes and health behaviors across multiple years for most of the sample; to improve power and to avoid testing many individual outcomes, I follow Kling et al. (2007) and Hoynes et al. (2016) by combining many related outcomes into standardized indices, based on the average z-scores for each outcome relative to race-specific averages during the years immediately prior to the beginning of desegregation. I construct indices for self-reported health status (self-reported overall health, self-reported hearing, and self-reported vision); chronic conditions (cancer, lung disease, strokes, arthritis, psychological conditions, high blood pressure, diabetes, and heart conditions); preventive care (cholesterol tests, flu shots, mammograms, pap smears, and prostate exams); and unhealthy behaviors (currently smoking, consuming three or more drinks per sitting, and not vigorously exercising). I additionally consider individual health outcomes of interest, such as mortality and reported health care experiences. To construct desegregation probabilities, I combine desegregation court order dates from the American Communities Project (ACP) with data on district-level school enrollment by race to estimate the probability that each individual was subject to a desegregation court order based on their year of birth and state of schooling.

I utilize a difference-in-differences design, exploiting the staggered rollout of desegregation court orders by regressing each outcome on the probability that an individual was affected by a desegregation court order for at least a year, along with individual-level covariates and fixed effects for cohort, state, and survey year.[1] The identifying assumption is that health outcomes would have evolved in parallel for segregated and desegregated cohorts in the absence of a court order. I assess the plausibility of this condition in numerous ways. First, I show that my main results are robust to controlling for other programs, such as hospital desegregations studied by Almond et al. (2006), Food Stamp Program rollouts studied by Hoynes et al. (2016), and community health centers studied by Bailey & Goodman-Bacon (2015), which may have been correlated with desegregation court orders and improved Black health; results are also robust to including linear time trends in area characteristics that may have been associated with desegregation court orders. Second, I follow R. C. Johnson (2011) by using the dates of initial court orders, rather than the implementation

[1] This is conceptually similar to a two-sample instrumental variables design, as outcomes and exposure probabilities come from different data sources, but I opt for a reduced form approach since exposure probabilities come from population-level data where sampling error is likely to be small.

dates of desegregation plans, in order to avoid potentially endogenous resistance of court orders.I therefore estimate an intent-to-treat effect of court orders. Finally, event study estimates do not show any significant pre-trends during years leading up to a court order.

For Black respondents, desegregation is associated with statistically significant increases of 0.14 standard deviations in the self-reported health index and 0.13 standard deviations in the preventive care index, and a decrease in annual mortality of 0.8 percentage points per year. There are no statistically significant effects on the unhealthy behaviors index or the chronic conditions index. Results are robust to modifying the set of controls and sample restrictions, and are similar when utilizing estimators that are robust to heterogeneous treatment effects. I find less evidence of effects on white health, consistent with previous literature finding little education or income effects on white students from desegregation. To determine whether changes in peer composition or high school completion (and subsequent outcomes mediated through this channel) explain my results, I follow Angrist et al. (2022) by comparing effects of desegregation on Black health to effects of desegregation on these intermediate outcomes separately among various demographic subgroups. Effects on self-reported health are strongly related to effects on desegregation's integration and high school completion effects, but there are improvements in self-reported health even among groups with no increase in high school completion; in contrast, effects on preventive care and mortality are essentially unrelated to effects on high school completion and peer composition across demographics.

My results illustrate that desegregation had long-run benefits on a range of life outcomes, beyond immediate educational and economic effects. These benefits may partially explain patterns such as the convergence in Black-white health outcomes during the 20th century. The case of desegregation also speaks to the complex ways in which educational interventions more broadly can impact future health. Only effects on self-reported health appear to be mediated by effects on high school completion, suggesting that factors such as the direct psychological effects of reduced discrimination are independently important when considering the health effects of educational policies.

The rest of the paper is organized as follows. Section 2 provides background on school desegregation and a conceptual framework for how desegregation may impact health. Section 3 describes the data and sample. Section 4 describes the empirical strategy and identification conditions. Section 5 presents reduced-form results and Section 6 explores mechanisms. Section 7 concludes.

2 Background

2.1 Desegregation Court Orders

Despite the ruling in *Brown v. Board of Education* that racial segregation in public schools was unconstitutional, there was almost no immediate desegregation in the wake of the decision. The *Brown II* decision in the following year instead ordered states to desegregate "with all deliberate speed." As a result, it took several decades for school desegregation to become widespread, and often required the use of desegregation court orders against individual school districts. Guryan (2004) and R. C. Johnson (2011) documented that these generally occurred after lawsuits from the NAACP Legal Defense and Educational Fund.

The first desegregation court orders generally focused on Southern school districts with de jure racial segregation. Anstreicher et al. (2022) note that court orders accelerated following the 1964 Civil Rights Act, after which the US Attorney General could join lawsuits against segregated school districts. During the 1970s, desegregation court orders also became more common outside of the South. *Swann v. Charlotte-Mecklenburg Board of Education (1971)* allowed busing to be used as a desegregation tool and *Keyes v. School District No. 1, Denver (1973)* ruled that de facto school segregation, such as that resulting from residential segregation, was sufficient grounds for desegregation court orders. While not every school district was subject to desegregation court orders, those near districts subject to orders were indirectly affected and often voluntarily desegregated shortly afterwards. For example, Logan et al. (2008) note that "Within a metropolitan area where even one segregation case was successfully pursued by the plaintiffs, other districts were effectively put on notice that they were at risk of court action. In addition, such cases may have raised attention to the racial composition of schools throughout the area." Desegregation court orders used a variety of strategies, including busing, rezoning of attendance zones, and freedom of choice plans; in this study, I consider the average effects of these plans in aggregate.

Previous research has documented that desegregation court orders were effective in increasing racial integration (Reber, 2005), even when accounting for white flight. Beyond just changing peer composition, the court orders also substantially increased educational resources for Black students

(Reber, 2010).[2] Desegregation had profound effects on educational attainment and economic well-being, resulting in approximately one additional year of education for Black students on average, greater high school graduation rates, and higher lifetime incomes (Guryan, 2004; R. C. Johnson, 2011; Bergman, 2018; Tuttle, 2019; Anstreicher et al., 2022).[3]

2.2 Conceptual Framework

Desegregation may impact health through several distinct channels; most, but not all, of these channels would be expected to improve health. These channels can be divided into two broad categories: *direct effects* of desegregation itself and *indirect effects* that operate through increased educational attainment. This framework is not intended to be exhaustive – and some channels cannot neatly be sorted into either category – but it nevertheless clarifies the mechanisms that could explain desegregation's health effects.

Direct Health Effects

First, desegregation itself could directly impact Black health through changes in peer composition, due to previous findings that desegregation successfully facilitated racial integration. This could lead to peer effects in the adoption of positive or negative health behaviors (Sacerdote, 2011). In addition, there could be improved trust in the medical system and more future interactions with white medical providers as a result of integration. Alsan & Wanamaker (2018), and Alsan et al. (2019) have documented empirical evidence of medical mistrust among Black Americans based both on negative experiences with the health care system and the presence of historical instances of medical mistreatment of Black Americans. It is possible that this mistrust could be affected by racially integrated environments earlier in life, given the relatively low levels of racial diversity among physicians (Castillo-Page, 2016). In addition, education and income levels are positively correlated with medical trust among Black men in some settings (Idan et al., 2020). These effects

[2] For example, Tuttle (2019) documents that prior to desegregation court orders in Jefferson County, Kentucky, the county's predominantly white schools had much higher levels of spending per pupil, spending on facilities, and teacher education levels than the predominantly Black schools. Case studies also illustrate overcrowding and shortages of school supplies in predominantly Black schools in both the North and South prior to desegregation court orders (see Lukas (1986) for examples in Boston and Patterson et al. (2001) for examples in Atlanta and Clarendon County).

[3] Desegregation has been associated with increased educational attainment more broadly, beyond the case of Black-white integration, with Antman & Cortes (2021) finding that Mexican-American school desegregation in California improved the educational outcomes of Mexican students.

could have large effects on health, as research on medical mistrust has found that greater Black trust in physicians is associated with greater preventive care use (LaVeist et al., 2009; Musa et al., 2009). Even if mistrust is not changed, it is also possible that early experiences with integration could help Black patients respond to racially biased medical systems (Hoffman et al., 2016).[4]

In addition, desegregation court orders may have changed exposure to societal discrimination or discrimination from peers. The "weathering effects" hypothesis in public health argues that physiological responses to stress occur from exposure to racism (Geronimus, 1992; Geronimus et al., 2006). A related theory known as "John Henryism" argues that there also may be physiological effects such as higher blood pressure from a need to work harder as a response to societal disadvantage (James et al., 1983; McEwen, 1998). Court orders could therefore improve health by decreasing everyday discrimination resulting from having to attend a segregated school. These psychological effects were highlighted in Chief Justice Warren's opinion in *Brown v. Board of Education*, where he wrote: "To separate them from others of similar age and qualifications solely because of their race generates a feeling of inferiority as to their status in the community that may affect their hearts and minds in a way unlikely ever to be undone."[5] For some Black students, however, direct discrimination may have increased, either through micro-aggressions from white teachers or peers or from exposure to violent protests, such as the response to Boston's busing court order.

Finally, it is possible that improved school resources and facilities for Black students after desegregation directly affected health. Though historical evidence on these particular inputs is limited, it is possible that school-based public health interventions may have scaled with school funding, which increased after desegregation, and unsafe school facilities may have health effects.[6]

Indirect Health Effects through Educational Attainment

Second, desegregation could affect Black health by increasing the amount of education that Black students received. Previous work has shown that desegregation improved the education of Black

[4]Reporting on desegregation court orders suggests that for Black advocates of desegregation plans, a concern about interacting with predominantly white social systems was an important motivation. Lukas (1986) says of a Black mother in Boston, "For the foreseeable future, she knew, Boston would be a 'white world'... If her children were going to make their living there, they would have to know how to get along with such people."

[5]The role of the government in supporting or combating discrimination was also highlighted as important in a previous Kansas course decision cited in the *Brown* ruling: "The impact is greater when it has the sanction of law."

[6]For example, reports on conditions in Black schools in Clarendon County, North Carolina prior to court orders stated that these schools lacked running water and that "both white schools had flush toilets, but the three Black schools had none – only outhouses" (Patterson et al., 2001).

students as measured by per-pupil funding (Reber, 2010) and facility quality (Tuttle, 2019).[7] These changes have been associated with greater educational attainment and higher future incomes for Black students.

Educational attainment and health outcomes exhibit a robust correlation, and there is some evidence that this relationship is causal, based on other historical education policies and their effects on mortality, self-reported health, and birth outcomes (Lleras-Muney, 2005; Deming, 2009; Currie & Moretti, 2003).[8] The effects of desegregation court orders on educational outcomes suggest that to the extent that the health-education gradient is causal, Black health could have improved in part through increased educational attainment.

Education could improve health through numerous channels (Grossman, 1972), such as through its effects on personality traits like risk aversion, knowledge about complex health decisions (Cutler & Lleras-Muney, 2010), and downstream effects on earnings and occupations. Effects on economic trajectories may be particularly important in the context of desegregation, as desegregation increased future Black earnings and occupational prestige (Ashenfelter et al., 2006; R. C. Johnson, 2011; Anstreicher et al., 2022). Higher incomes and occupational prestige could affect health through the causal effects of occupation-specific differences in mortality (N. J. Johnson et al., 1999), a greater ability to afford medical care, or increased access to health insurance. Subsequent changes in earnings could also impact mobility (Baum-Snow & Lutz, 2011; Tuttle, 2019), with potential migration to areas with lower pollution or better health care quality (Deryugina & Molitor, 2020; Finkelstein et al., 2021).[9]

One set of health outcomes that could potentially worsen due to the education channel, however, is health behaviors. Even though education may increase knowledge about the harms of negative health behaviors, if products such as alcohol and cigarettes are normal goods (Cawley & Ruhm, 2011), higher incomes may encourage their consumption.

[7] In settings beyond desegregation, school facility quality has been linked to educational outcomes (Cellini et al., 2010; Lafortune & Schönholzer, 2022).

[8] Lleras-Muney (2022) highlights that there are exceptions to these causal findings, with other studies suggesting that health effects of educational interventions may vary based on the setting. For example Clark & Royer (2013) and Meghir et al. (2018) notably did not find any health effects from schooling reforms in the UK or Sweden.

[9] Tuttle (2019) finds that Black students affected by desegregation plans in Jefferson County, KY later lived in neighborhoods with tract-level incomes that were higher by 3.4 percent. However, this is not guaranteed to improve health, as migration to urban areas with more disease or a higher prevalence of smoking or drinking could increase mortality (Black et al., 2015).

3 Data and Descriptive Statistics

3.1 Sample Construction and Outcomes

The sample is based on restricted data from the Health and Retirement Study (HRS) between 1992 and 2018, inclusive. The HRS is a panel survey from the University of Michigan that surveys approximately 10,000 individuals every two years, focusing on those close to retirement age or older. I limit the sample to non-Hispanic Black or white respondents.[10] I also limit the sample to respondents who turned 17 between 1940 and 1986, inclusive.[11] I limit the sample to individuals for whom I observe either the state where they attended school or the state where they were born; the state where they attended school is used as their geographic location in my analysis unless it is missing, in which case I use the state where they were born and assume that they also attended school in that state. Finally, I only keep responses for individuals who were between 51 and 100 years old during the year of the survey. The final sample consists of approximately 5,800 non-Hispanic Black respondents with over 32,000 distinct survey responses, and approximately 18,000 non-Hispanic white respondents with over 124,000 distinct survey responses.

For each individual, I observe a range of contemporaneous and childhood demographic information, including years of education, their father's employment status when growing up, and self-reported measures about their childhood such as whether they grew up in a rural area, their health, and their financial situation.[12] I also observe health measures for each individual; for chronic health conditions, respondents are typically asked whether a doctor has ever diagnosed them with a given condition. The data also include sample weights, which are applied throughout.

Since I consider numerous individual outcomes, and many are relatively rare, most of my main estimation results combine outcomes into groups of related variables, following Kling et al. (2007) and Hoynes et al. (2016), in order to create standardized indices for each outcome of interest;

[10] I drop Hispanic respondents from the sample because treatment of Hispanic students varied substantially by district. For example, after desegregation court orders, Austin schools initially classified Hispanic students as "white" as a means of nominally complying with desegregation orders, grouping Hispanic students and non-Hispanic Black students in the same schools (Wells, 2009).

[11] The start year of 1940 is chosen to have a large pre-desegregation sample prior to the first court order; I show robustness of my results to having a more narrow age window. The end year of 1986 is used since it is the last date of a desegregation court order in the data.

[12] I record that individuals completed high school if they report 12 or more years of education, and record that they completed college if they report 16 or more years of education.

this avoids issues of finding significant results simply by testing numerous outcomes, and improves power by combining multiple rare outcomes. For each index, I transform each response into the race-specific z-score, relative to the distribution of race-specific responses for individuals who turned 17 between 1942 and 1951; this corresponds to the 10-year period immediately prior to the first court order in the data.[13] For each outcome within the index, I then average the z-scores for the respondent during the survey year, and include in my estimation only the individuals for whom every outcome within the index is observed during that year. As a result, the average of each index is, by construction, zero among individuals who turned 17 between 1942 and 1951.

I analyze four indices, with the signs of the outcomes standardized within each index so that an increase in the outcomes always corresponds to either better or worse health: self-reported health status (self-rated overall health, self-rated vision, and self-rated hearing);[14] chronic conditions (cancers other than skin cancer, lung diseases, strokes, arthritis, psychological conditions, high blood pressure, diabetes, or heart conditions);[15] preventive care (cholesterol tests, flu shots, mammograms, pap smears, and prostate exams);[16] and unhealthy behaviors (currently smoking, having three or more drinks per sitting, and vigorously exercising less than once per week). In addition, I separately consider some individual outcomes of interest, such as annual mortality and experiences with the health care system. For many outcomes, results are subject to potential telescoping effects, as they are self-reported; nevertheless, this may not be a concern when looking across all results, as some questions ask about positive health outcomes while others ask about negative outcomes.

Table 1 shows respondent-level summary statistics and Table 2 shows response-level summary statistics, separately for the non-Hispanic Black and white samples. Table 1 shows that compared to white respondents, Black respondents have less education on average and were more likely to attend

[13]This is analogous to estimating the distribution within a control group in randomized controlled trials.

[14]For each outcome, respondents are asked to rate their health on a 5 point scale. I convert each into a binary measure of whether respondents rate their health as "good" (3 out of 5) or better. While self-reported health is, by construction, a subjective measure, there is some evidence that self-reported health is predictive of future mortality, even beyond information about health conditions (McGee et al., 1999; Benjamins et al., 2004).

[15]During most years, the "lung diseases" question of the HRS asks if a doctor has ever told the respondent that they have a chronic lung disease such as chronic bronchitis or emphysema. The "heart conditions" question typically asks if a doctor ever told them that they have had a heart attack, coronary heart disease, angina, congestive heart failure, or other heart problems.

[16]Although recommendations have changed over time (Einav et al., 2020), the CDC recommends that women should begin mammogram screenings when they are 50 and pap smears when they are 21; recommendations for prostate exams are less strong, but the CDC states that men 55 and older can consider prostate exams. The CDC recommends that all individuals should begin screenings for high blood cholesterol when they are 20. Any systematic differences by age groups in screening recommendations should be captured by age controls in my analyses.

school in the South. Table 2 shows that at the response level, Black respondents are younger than white respondents on average, but have higher levels of annual mortality. Despite being younger, there are many health measures for which Black respondents have worse health, although this is not uniformly true across all health outcomes. For example, Black respondents are more likely than white respondents to say that their overall health is not at least "good," and they are more likely to have been diagnosed with strokes, arthritis, high blood pressure, and diabetes. While utilization of most preventive care is similar across races, Black respondents are substantially less likely to report receiving a flu shot in the last year. They are also more likely to report currently smoking or not vigorously exercising. Appendix Figure A.1 shows the full age distribution by race.

3.2 Desegregation Exposure

To determine the effects of desegregation court orders, I first need to estimate the probability that each student was impacted by a court order. The challenge is that court orders typically occurred at the district level, while I only observe the state where respondents attended school.

I start with the list of desegregation court orders from the American Communities Project (ACP) at Brown University. This is the same set of cases used by R. C. Johnson (2011) and Logan et al. (2008), and includes the case date and implementation date for all court cases that resulted in desegregation plans between 1952 and 1986. I then merge this with the white and Black student populations of large school districts in 1968-1970 (the earliest year available) from the Office for Civil Rights School Desegregation Database, collected by the U.S. Department of Education. In total, 411 out of the 865 school districts for which we observe a desegregation court order in the ACP data are matched to enrollment counts; most districts that are not matched are very small, but this nevertheless results in dropping some court orders.

I define my baseline probability of desegregation exposure as the share of students of a given race in a state affected by a court order up until and including that year, out of all students of that race in the same state *ever* affected by a court order. Formally, for an individual of race r born in year b in state s, this probability is computed as:

$$P_{bs}^{r} = \frac{\sum_{d \in D_s} N_{rd} \mathbb{1}(YearDeseg_d \leq b + 17)}{\sum_{d \in D_s} N_{rd}} \tag{1}$$

where D_s is the set of districts d in state s observed in both the desegregation and school population data sets, N_{rd} is the number of students of race r in district d in 1968-1970, and $\mathbb{1}(YearDeseg_d \leq b + 17)$ is an indicator variable for district d desegregating by year $b + 17$. Following R. C. Johnson (2011), I record desegregation dates relative to when a student turns 17, since a student turning 17 during the year of the court order likely would have been affected by the court order for their final year of high school.

This measure of P^r_{bs} assumes that desegregation patterns in a state are consistent with the patterns among districts subject to court orders; this is reasonable if surrounding school districts desegregated around the same time to avoid the threat of litigation, as suggested by historical accounts of desegregation (Logan et al., 2008; R. C. Johnson, 2019). In addition, while I am not able to identify and exclude individuals who grew up in school districts not subject to desegregation court orders, R. C. Johnson (2011) reports that 88% of the Black respondents in his PSID sample grew up in a school district subject to a court order; some of the remaining individuals would have grown up in states that I drop from the sample due to never having a court order. Therefore, it is unlikely that the presence of individuals in school districts never subject to a court order introduces substantial measurement error. Results should be interpreted as capturing not only the consequences of court orders in the districts where they took effect, but also their spillover effects for other school districts.

Appendix Figure A.2 shows the geographic distribution of the baseline desegregation probability measure in multiple years. The patterns in these maps are consistent with the historical background presented in Section 2. In 1960, there were still very few states with desegregation court orders, but these became more widespread after the 1964 Civil Rights Act. By 1975, many Northern states also had desegregation court orders, and these were largely completed by 1980. Appendix Figure A.3 illustrates an example of court order timing in one particular state – South Carolina – which had several districts with court orders.

Appendix Figure A.4 shows the distribution of estimated desegregation probabilities in the sample, as well as desegregation court order timing in the sample after the first court order in a state. Panel (a) shows that about half of the Black sample has a positive probability of being exposed to a desegregation court order; this fraction is lower for the white sample (panel (b)), since white respondents are less likely to live in the Southern states that were subject to earlier desegregation court orders. Panels (c) and (d) show the evolution of exposure to desegregation orders following a state's first order; for both white and Black respondents there is an immediate jump in exposure probability, followed by a gradual increase over the following 20 years.[17]

In addition to the baseline desegregation probability measure, I construct an alternate "lower-bound" desegregation probability measure. This measure is identical to the baseline measure, but replaces the denominator in Equation 1 with the *total* number of students of a given race in the same state (including those never affected by a desegregation court order). Appendix Figure A.5 constructs the maximum value of the "lower-bound" measure in each state, and then plots the population-weighted distribution. Panel (a) confirms that most Black respondents were in states where most Black students in large districts were affected directly by court orders. Panel (b) suggests that the share of white respondents likely to be directly affected is smaller.

3.3 Additional Data Sources

I bring in several additional data sets to ensure that results are attributable to school desegregation, rather than other contemporaneous social transfer programs, changes in healthcare access for Black Americans, or demographic trends, From the data in Baum-Snow & Lutz (2011), I merge in county-level information on the population share that is Black, has a high school diploma, and works in manufacturing, as well as the population density. From the data in Hoynes et al. (2016), I merge in the dates of community health center (CHC) openings studied by Bailey & Goodman-Bacon (2015) and annual food stamp program (FSP) expenditures per capita; both of these programs have been shown to improve long-run health.[18] From the data in Finkelstein & McKnight (2008), I merge in annual information from the American Hospital Association (AHA) on the number of beds per capita and the number of hospitals per capita by county.[19] I create a state-year level measure of each variable, as well as the share of the population that had a CHC or food stamp program in

[17]California is an example of the long time frame over which desegregation court orders could take place in a state. The first case with a desegregation court order took place in 1955 against the relatively small El Centro School District. Court orders against large school districts were not widespread until the 1970s, with the first court order against San Francisco Unified School District in 1971 and a court order against the Board of Education of the City of Los Angeles in 1976. The last California school district subject to a court order was Bakersfield in 1984, almost 30 years after the state's first court order.

[18]FSP data are available for 1965-1978; I use these endpoint values as the values for all years outside of this window. Based on the findings in Hoynes et al. (2016) that food stamps are particularly impactful during the first five years of life, I estimate the expected FSP expenditures per capita during the first five years for each respondent.

[19]AHA data are available for all states other than Alaska and Virginia, so these data are not used in my baseline analyses. The data for all other states are available beginning in 1950, so for earlier years, I use 1950 values.

their county, by taking a weighted average based on county populations in 1960. Finally, I estimate the share of individuals in a state living in a county with a desegregated hospital by year, using desegregation dates for counties in the Deep South from Anderson et al. (2020).[20]

3.4 Motivating Time Series

Time series of key outcomes motivate the observation that partial convergences in Black-white health occurred contemporaneously with school desegregation court orders. Panels (a) and (b) of Figure 1 plot the time series of racial differences in two health outcomes – reporting that self-rated health is at least "good" and receiving a flu shot in the last year – among white and Black respondents (between the ages of 55 and 64). While estimates are noisy, point estimates suggest that gaps in these outcomes began to narrow after desegregation court orders. For respondents who turned 17 prior to 1960, there was almost a 20 percentage point difference in the probability of rating their health as at least good; this gap dropped to approximately 12 percentage points among those who turned 17 between 1975 and 1979. Flu shots show a similar pattern, with the cohorts turning 17 prior to 1960 having about a 10 percentage point White-Black difference; by 1975-1979, this drops to only about 2 percentage points, and the difference is no longer statistically distinguishable from zero.

Panel (c) shows the corresponding average desegregation probabilities among Black respondents in the sample; court orders increased substantially around 1964, which is approximately when these observed health gaps began to narrow. These time series do not account for other factors that may have also played a role in narrowing gaps around the same time, so these should not be interpreted as causal. The following section describes the research strategy to obtain causal estimates.

[20] Due to data limitations, these estimates are necessarily imprecise. Prior to the implementation of Medicare, there are little specific data on hospital desegregation dates; once Medicare was implemented, researchers have determined whether a hospital was still desegregated based on whether or not they were certified by Medicare. I make the simplifying assumption that all Southern hospitals were segregated until Medicare was implemented and required hospitals to be desegregated in order to receive funding, and that all Northern hospitals were desegregated. This approximation is known to not be quite right, however; for example, Smith (2016) notes that until 1947, Black patients and students were excluded from the University of Chicago Hospital. This is likely not a large concern, however, since Northern hospitals did desegregate earlier than Southern hospitals, and because the majority of Black respondents grew up in the South. In addition, recent work (Anderson et al., 2020) has suggested that hospital desegregations may have had relatively minor effects on Black health outcomes.

4 Empirical Strategy

My empirical strategy is a difference-in-differences approach that leverages the staggered nature of desegregation court orders. I estimate the following regression equation separately for Black and white respondents, where each observation is a survey response in year t for individual i of race r born in year b in state s, and individuals are included for each of their HRS responses:

$$Y_{it} = \beta P^r_{bs(i)} + \lambda_{b(i)} + \theta_{s(i)} + \gamma_t + \eta X_{it} + \epsilon_{it} \tag{2}$$

where Y_{it} is the health outcome of interest; β is the intent-to-treat parameter of interest for the effect of being exposed to a desegregation court order for at least one year; $P^r_{bs(i)}$ is the probability that the individual attended a school impacted by a desegregation order by the time that they turned 17 (as defined in Equation 1); λ, θ, and γ are vectors of fixed effects for birth year, state of school attendance, and survey year, respectively; X_{it} is a vector of covariates, and ϵ_{it} is a heteroskedasticity-robust error term clustered at the state level.

In my baseline specification, X_{it} includes a cubic in age at the time of the survey, a gender indicator, indicators for the respondent's father being unemployed or absent while growing up, and an indicator for growing up in a rural area. The baseline specification also includes controls for contemporaneous programs: community health centers, hospital desegregation, and food stamp program rollouts. In robustness analyses, I show results for specifications that omit these controls, as well as results that control for additional measures of health care access (hospital characteristics and hospital desegregation), and time trends that vary by state demographics.

For outcomes measured once per respondent, such as education, I modify Equation 2 by removing survey year fixed effects, estimating (where X_{it} now omits functions of age):

$$Y_{it} = \beta P^r_{bs(i)} + \lambda_{b(i)} + \theta_{s(i)} + \eta X_{it} + \epsilon_{it} \tag{3}$$

The validity of this estimation strategy requires that in the absence of desegregation, cohorts that were exposed to a desegregation court order would have had health outcomes evolve in parallel to those of cohorts who were subject to a court order. Previous research compellingly argues that the assumption of parallel trends likely holds for desegregation court orders with respect to contemporaneous outcomes that were more easily observable, such as academic outcomes, suggesting that this assumption is also likely to hold for health outcomes that do not manifest until decades later. Guryan (2004) emphasizes historical evidence that legal precedence, rather than a sense of where desegregation would benefit Black students, was the primary factor in the NAACP Legal Defense and Education Fund's prioritization of certain school districts. R. C. Johnson (2011) presents additional empirical evidence that desegregation orders were unrelated to existing trends that would bias my results. First, in his event study specifications for outcomes such as educational inputs, educational attainment of Black students, and Black adult wages, there is little evidence of pre-trends prior to the dates of desegregation court orders. Second, he regresses desegregation dates on a range of school district covariates, such as labor market characteristics, school quality, and demographics, and finds that these characteristics cannot jointly predict court order timing.[21]

I further address potential identification concerns by including several controls in the X_{it} vector of Equation 2 that could plausibly be correlated with both the timing of desegregation orders and health outcomes. While it is difficult to enumerate every contemporaneous program that could potentially impact long-run health, and even harder to get reliable data on each of these programs, I check the robustness of my results to some of the most important programs studied in recent literature. If results change very little when adding these controls, this bolsters confidence that any effects are attributable to school desegregation.

Finally, I check for problematic pre-trends by estimating an event study version of Equation 2, following Sun & Abraham (2021) to construct an interaction-weighted estimator. This approach is also robust to potential heterogeneity in treatment effects. Goodman-Bacon (2021) notes that heterogeneity in the presence of dynamic treatment results in inconsistent estimates from Equation 2. I first estimate cohort-specific average treatment effects on the treated as:

$$Y_{it} = \lambda_{b(i)} + \theta_{s(i)} + \gamma_t + \eta X_{it} + \sum_{e \neq C} \sum_{\tau} \delta_{e,\tau} \mathbb{1}_{\{E_i = e\}} \mathbb{1}_{\{RelYear_i \in \tau\}} + \epsilon_{it} \tag{4}$$

where each variable is defined analogously to Equation 2, τ is a 3-year grouping of years relative to

[21] Characteristics do, however, predict the amount of time between the court order and implementation, motivating the use of court order dates rather than eventual implementation dates. I follow this approach and compute an intent-to-treat effect of court orders, due to potentially endogenous delays in implementation.

a reference year for each state, e indexes cohorts, and C denotes the control cohort, corresponding to the last treated group. The interaction-weighted estimator β_τ is a weighted average of $\hat{\delta}_{e,\tau}$.

This approach requires defining a "first" treatment year for each cohort. Given the continuous nature of the desegregation probability measure, there is not a single natural choice of the "first" treatment year. Therefore, I show results both relative to the first desegregation court order in a state and relative to the first year in a state during which 20% of Black students were affected based on the baseline measure (in order to address cases where only small districts were affected first within a state).[22] For each choice of threshold, the coefficient β_τ for "1-3 years before" is normalized to zero. Since this approach uses the "last-treated" states as a control, this also results in a different control depending on the binary treatment constructed.[23] If the coefficients prior to treatment are not statistically distinguishable from zero, this provides further evidence for the identification assumption.

The estimates from Equation 2 are used for the main results throughout, as the need for a binary treatment variable in the event study approach ignores much of the useful variation in probability of exposure to a court order. Nevertheless, the specification serves as a useful check for pre-trends, as well as a test for whether the magnitudes of effects found in the main specification are reasonable even in the presence of potential treatment effect heterogeneity.

5 Reduced-Form Results

5.1 Peer Composition and Education Effects

Before estimating the effects of desegregation on health outcomes, I first test for effects on peer composition and educational attainment, to confirm that the baseline desegregation measure replicates changes in these outcomes for Black students from previous studies with district-level geographic information. Tables 3 and 4 show results for peer composition and educational outcomes, respectively, estimated following Equation 3.

Table 3 uses the 2015-2017 Life History Mail Survey's question about the majority race at each school that the respondent attended. Column (1) shows effects of desegregation court orders on

[22] I check (not shown) that results are similar when using other thresholds for the share of students affected.

[23] When using the "first court order" measure, Arizona and Wisconsin are used as controls; when using the "20 percent affected" measure, Illinois is used as the control.

ever attending a school in which the majority race was not Black; Column (2) shows effects on ever attending a school in which the majority race was non-Hispanic White. Both columns suggest that the baseline measure of desegregation probability increased integration, with court orders being associated with a 16.8 percentage point increase in the probability of ever attending a school in which the majority race was not Black. These effects are particularly pronounced in the South, with positive but statistically insignificant effects outside of the South. Effects are comparable among rural and non-rural areas.

Table 4 shows – for both Black and white respondents – the effects of desegregation court orders on educational attainment, measured by high school completion and years of education. For Black respondents, desegregation court orders increase high school completion by 4.5 percentage points and educational attainment by 0.4 years on average. These aggregate results are not statistically significant, but results are significant at the 1% level for respondents who grew up in the South or in rural areas. This is consistent with findings from Anstreicher et al. (2022) that desegregation's education effects were concentrated in the South, and his results are of a similar magnitude. Point estimates for overall effects on high school completion, while not statistically significant, are slightly larger than the findings in Guryan (2004) that desegregation plans decreased Black dropout rates by 1-3 percentage points Reassuringly, there are very few significant effects among white respondents, which bolsters the case for treating white respondents as a placebo group. Effects for white respondents are only statistically significant for high school completion – but not years of education – among those who grew up in rural areas, and the magnitude (4 percentage points) is much smaller than effects for Black respondents.

5.2 Overall Health Effects

Figure 2 shows the results of estimating Equation 2 for each summary index, separately among the Black and white samples; exact values are shown in Appendix Tables A.1 through A.4. For the Black sample, the estimates suggest that desegregation improved the self-reported health index by 0.14 standard deviations and improved the preventive care index by 0.13 standard deviations (both significant at the 1% level). For these outcomes, desegregation had a substantial positive impact on long-run Black health. Results for the chronic conditions and unhealthy behaviors indices are not statistically significant in the Black sample. Improvements in self-reported health are consistent with R. C. Johnson (2011), who found that desegregation was associated with greater self-reported health among a younger sample of individuals (ages 20-50), as well as findings that pre-desegregation school investments improved the self-reported health of Black students (Frisvold & Golberstein, 2011).

In contrast, the only white health index with a statistically significant effect is self-reported health, which is only significant at the 10% level and of a much smaller magnitude than the effects on Black self-reported health. This suggests that desegregation improved Black health with little impact on white health. This is consistent with previous studies on the effects of desegregation (Guryan, 2004; R. C. Johnson, 2011), which have consistently found no effects of desegregation on white educational or career outcomes.

Table 5 shows results for additional health outcomes that are not components of the indices. Results in Column (1) suggest that desegregation court orders lowered annual Black mortality by 0.8 percentage points (relative to a mean of 2.2 percentage points per year), which is significant at the 5% level. Most other health outcomes considered – frequent pain, obesity, losing permanent teeth, and wearing a hearing aid – are not statistically significant. One notable exception is the results for seeing a dentist within the last two years – a health behavior that is potentially related to comfort interacting with the medical system – for which a desegregation court order is associated with a 9 percentage point increase among Black respondents. Appendix Table A.5 also shows results – among the smaller sample of respondents of the HRS's Psychosocial and Lifestyle Questionnaire – on interactions with the health care system. Although standard errors are large, point estimates are consistent with desegregation resulting in a lower probability of reporting ever being unfairly denied health care or treatment, and ever receiving poorer medical treatment because of race. Desegregation is also associated with a large increase in self-reported control over health (approximately 0.9 points on a 10 point scale).

Figure 3 shows the Sun & Abraham (2021) event study results from estimating Equation 4 for each index among the Black sample relative to two event indicators: the first desegregation court order in a state and the year in which 20% of Black students were exposed to a desegregation court order. While estimates from this specification are noisy and discard useful variation in desegregation probabilities, results are consistent with the baseline difference-in-difference estimates. For the three outcomes that were statistically significant (self-reported health, preventive care, and annual mortality), post-treatment magnitudes are in line with the overall estimates. In addition, there are no notable pre-trends for any of these outcomes.

5.3 Effects on Index Components and Discussion

To understand the drivers of the summary index results and to interpret magnitudes, Appendix Tables A.1 through A.4 show the full results of estimating Equation 2 on each index as well as components of the indices. Results are shown both among all respondents and respondents and the Index Sample (those who responded to all index component questions).

For self-reported health, Appendix Table A.1 shows that coefficients on self-rated overall health, hearing, and vision are all large and positive, with the overall health and vision coefficients being statistically significant. Nevertheless, effect sizes are large, with each estimate representing a 5-12% increase relative to the mean. It is perhaps not surprising that some of the largest effects of desegregation are on self-reported health, as previous large healthcare interventions have often detected effects for overall self-reported health (Finkelstein et al., 2012) or vision improvements (Manning et al., 1987). To benchmark effects relative to other interventions, Finkelstein et al. (2012) reports that the Oregon insurance expansion had an intent-to-treat effect of 0.039 and a local average treatment effect of 0.133 on self-reported health being good or better; this suggests that the effects of desegregation court orders were roughly comparably to those of insurance expansion.

For preventive care, Appendix Table A.3 shows that only the coefficient on cholesterol tests is statistically significant, but point estimates for all outcomes suggest an increase in Black preventive care. Since some of these types of preventive care – such as flu shots – typically are available at little cost to patients, these effects are consistent with an explanation in which desegregation bolsters trust in the medical system. This is also consistent effects on seeing a dentist in Table 5, which may imply greater interactions with medical providers more broadly.

Appendix Tables A.2 and A.4 are consistent with little effects of desegregation court orders on either chronic conditions or health behaviors. Point estimates for effects on individual chronic conditions are evenly split between positive and negative effects, and the only effect that is statistically significant is on the likelihood of strokes. While only significant at the 10% level, this finding may warrant further investigation, given the large attention given to Black-white gaps in the incidence of strokes and hypotheses that these may be related to “mistrust of the healthcare system" (Cruz-Flores et al., 2011). In addition, it is possible that these estimates understate the true benefits of desegregation on chronic conditions, since recorded chronic conditions are a convolution of true underlying chronic conditions and interactions with the medical system that result in a diagnosis; if desegregation increased interactions with the medical system, this may result in more chronic conditions that were previously undetected. Similarly, Appendix Table A.4 suggests mixed impacts of desegregation in unhealthy behaviors. This is consistent with the mixed evidence from previous research on the impacts of educational interventions on health behaviors (Galama et al., 2018), but differs from results on pre-desegregation educational improvements for Black students, which found declines in smoking (Frisvold & Golberstein, 2011). Nevertheless, these results could be consistent with some negative health behaviors being normal goods if desegregation increased income. It is also possible that early desegregation court orders may have had little informational benefit on health behaviors such as smoking, for which education gaps did not emerge until the mid-1960s (De Walque, 2010).

Reassuringly, for the individual outcomes used to construct the indices, most coefficients for white respondents are statistically insignificant. In addition, signs of the coefficients are split between positive and negative health effects. This provides additional support that desegregation likely had no impact on white health.

5.4 Heterogeneity

Table 6 shows heterogeneity results from estimating Equation 2 for each summary index, with an additional interaction term between demographic characteristics and $P^r_{bs(i)}$. To compare groups, I use a reduced version of the preventive care index that excludes gender-specific preventive care.

In some cases there are substantial differences across demographic groups. Panel A shows that, by gender, effects on preventive care were concentrated among females. This contrasts with some previous studies of the health impacts of other educational interventions, which have often found larger impacts on males (Lleras-Muney, 2022).

Panel B shows that for most measures, health improvements are larger for Black respondents who grew up in rural areas than in non-rural areas. One interesting exception to this is health behaviors; the statistically significant point estimate suggests that desegregation worsened unhealthy behaviors in rural areas. This could occur if income effects in these regions were larger than the effects of better health information, or if the health behaviors of white students in desegregated schools varied across regions.

Finally, Panel C shows results separately for states in the South and non-South. For both self-reported health and chronic conditions indices, health benefits are significantly larger in Southern states; in fact, while preventive care effects are similar across regions, self-reported health effects are only statistically significant in the South. This is consistent with the results from Shen (2018), who finds that reductions in pre-term births were largest in the South after desegregation. She hypothesizes that this may be due to worse pre-desegregation health outcomes for Black individuals in the South relative to the non-South. In addition, school districts were larger in area in the South, which made white flight from the district more difficult than in other regions (Clotfelter, 2001).

5.5 Robustness

Table 7 presents a series of robustness checks on Black health outcomes to test the sensitivity of results to alternate covariates and sample restrictions; Appendix Table A.6 shows robustness results among white respondents. For comparison, baseline results are duplicated in Panel A.

Panel B excludes controls for contemporaneous programs (food stamp programs, hospital desegregations, and community health centers). Panel C includes, in addition to the baseline controls, further individual controls (self-reported childhood health and family finances), controls for hospital characteristics (hospitals per capita and hospital beds per capita), and linear time trends by cohort interacted with state characteristics in 1960 (population density, unemployment rate, the share of the population in manufacturing, the share of the population that is Black, and the share of the population with a high school degree); this allows for the possibility that there were pre-existing health trends in areas that were more likely to have an earlier desegregation court order. Panel D modifies the sample restrictions to begin the sample with individuals who turned 17 in 1950, closer to the first desegregation date; this excludes many older individuals among which there is more variation in health outcomes, but addresses concerns of earlier cohorts differing substantially from later cohorts in ways other than exposure to desegregation court orders. Finally, Panel E uses the lower-bound desegregation probability measure instead of the baseline measure. In all cases, results remain qualitatively similar, and estimates for the self-reported health index and preventive care index are still significant.

6 Peer Composition and Educational Attainment as Mechanisms

Black health improvements could come either from direct effects of desegregation, or from the resulting increases in educational attainment found in previous research (Guryan, 2004; R. C. Johnson, 2011; Reber, 2010; Anstreicher et al., 2022). Direct effects include both the immediate changes in peer composition due to integration, as well as other changes that are harder to quantify, such as the broader psychological effects of discrimination. In this section, I examine the contributions of one immediate desegregation effect – changes in peer composition – and increases in educational attainment in explaining Black health improvements from desegregation; Tables 3 and 4 suggest that both of these effects are present in the HRS sample, so these are ex ante plausible mechanisms.

To evaluate the extent to which health effects can be mediated by each of these outcomes, I modify an approach from Angrist et al. (2022) that leverages variation in effects across demographics. The intuition for this approach is that the effects of desegregation on both health and potential mechanisms (peer composition and educational attainment) vary across subgroups; if a given mechanism mediates health effects, we would expect larger health effects within subgroups that experienced larger changes in the mechanism due to desegregation.

Formally, consider the following potential relationship between desegregation and a mechanism S_i (omitting race subscripts since only Black respondents are included in this analysis):

$$S_i = \pi_1 P_{bs(i)} + \tilde{\lambda}_{b(i)} + \tilde{\theta}_{s(i)} + \pi_2 X_i + \pi_3 X_i P_{bs(i)} + \nu_i \tag{5}$$

where S_i is the mechanism (such as schooling) for individual i born in state s in year b; all other variables are defined analogously to in Equation 2. Define the effect of desegregation on the mechanism for individuals with the set of characteristics X as $\pi(X) = \pi_1 + \pi_3 X_i$.

If desegregation has health effects that are partially mediated through the mechanism S_i, health outcome Y_{it} can be written as:

$$Y_{it} = \alpha_1 S_i + \alpha_2 P_{bs(i)} + \lambda^*_{b(i)} + \theta^*_{s(i)} + \gamma^*_t + \eta^* X_{it} + \epsilon^*_{it} \tag{6}$$

Combining Equations 5 and 6 gives the following reduced form equation for the potentially heterogeneous impacts of school desegregation $P_{bs(i)}$ on health outcome Y_{it}:

$$\begin{aligned} Y_{it} &= P_{bs}(\alpha_1\pi_1 + \alpha_1\pi_2 X_{it} + \alpha_2) + (\alpha_1\tilde{\lambda}_b + \lambda_b^*) + (\alpha_1\tilde{\theta}_s + \theta_s^*) + \gamma_t^* + X_{it}(\alpha_1\pi_1 + \eta^*) + (\nu_i + \epsilon_{it}^*) \\ &= (\pi(X)\alpha_1 + \alpha_2)P_{bs} + \lambda_b + \theta_s + \gamma_t + \eta X_{it} + \epsilon_{it} \\ &= \beta(X)P_{bs(i)} + \lambda_{b(i)} + \theta_{s(i)} + \gamma_t + \eta X_{it} + \epsilon_{it} \end{aligned} \tag{7}$$

where $\beta(X) = \pi(X)\alpha_1 + \alpha_2$ is the reduced form effect of desegregation on the health outcome Y for individuals with the set of characteristics X.

This approach is related to an instrumental variables (IV) design, since if $\alpha_2 = 0$, then Equation 5 represents the first stage of a model in which P_{bs} is an instrument for S_i, and α_1 in Equation 6 is the causal effect of S_i on future health outcomes. Angrist et al. (2022) show that in this case, $\beta(X) = \pi(X)\alpha_1$, so across groups, regressing the reduced form $\beta(X)$ on the first stage $\pi(X)$ yields a slope that is equivalent to the IV estimate of the effect of S_i, and the y-intercept will equal 0. If desegregation affects health through channels other than the mechanism, however, then $\alpha_2 \neq 0$, so the y-intercept from regressing the reduced form on the first stage will no longer equal 0 (since $\beta(X) = \pi(X)\alpha_1 + \alpha_2$).

Figures 4 and 5 visually relate desegregation's health effects to effects on educational attainment and peer composition, respectively. These graphs plot, for each outcome with statistically significant effects in the previous section, the reduced form effects of desegregation on health ($\beta(X)$ from Equation 7) against the "first stage" effects of desegregation on either high school completion or ever attending a majority non-Black school ($\pi(X)$ from Equation 5), separately by demographic groups defined prior to desegregation, following Angrist et al. (2022)[24]

Panel (a) of each figure shows a positive and statistically significant relationship between desegregation's effects on self-reported health and both high school completion and racial integration. For example, both peer composition and educational attainment changed substantially in the South following desegregation, and the South also exhibited some of the self-reported health effects. This suggests that both channels are important mediators for self-reported health. Nevertheless, even

[24] Since groups are not mutually exclusive and the choice of omitted groups in the regression equation is arbitrary, I compute each effect based on the expectation of all other covariates, conditional on the X of interest. For comparability, the preventive care index includes only sources of preventive care that are relevant to both men and women. In the regression equation for the line of best fit, each observation given weights based on the inverse of the standard error of the reduced form estimates. Each regression includes individual-level controls. Estimates do not correspond to estimates in Table 6 and 4 because groups are included in these regressions that were not previously controls.

groups with no change in high school completion (such as the non-South and non-rural areas) had better self-reported health, as indicated by the statistically significant positive y-intercept.

In contrast, results for preventive care in panel (b) and mortality in panel (c) suggest that effects on these outcomes are unrelated to changes in either peer composition or high school completion. By process of elimination, this suggests that other direct effects of desegregation, such its psychological effects, may have been important for these outcomes.

Another way of testing for the role of education in health improvements is by treating desegregation as an instrument for education levels, and assessing the overidentification statistic from the resulting two-stage least squares estimates, with health indices as the outcomes. Appendix Table A.7 shows the results of this analysis. For self-reported health, the coefficient on high school completion is large and statistically significant, but the overidentification tests for both self-reported health and preventive care reject desegregation court orders as a valid instrument for education. This provides additional support for the presence of direct effects of desegregation in both long-term Black self-reported health and preventive care.

7 Conclusion

In this paper, I present the most comprehensive evidence to date on the impact of school desegregation orders on long-run Black health. Preventive care and self-reported health status are particularly responsive to school desegregation, with large improvements in these outcomes Black students. These results contribute to the growing body of evidence on the beneficial impacts of school desegregation, which have extended far beyond the immediate educational consequences.

This may understate some of the beneficial health consequences of school desegregation. Since I find significant reductions in mortality as a result of desegregation, effects on other health outcomes may be attenuated due to selection: individuals who counterfactually survived as a result of desegregation court order are likely to be the less healthy respondents. In addition, the variation that I use does not capture potential spillover effects of desegregation to regions that had not yet been affected by court orders, such as increases in the national supply of Black physicians and improved racial attitudes from white physicians in national healthcare markets (Chin, 2022).

Mechanisms suggest both the importance and limitations in understanding the health effects as a consequence of changes in peer composition or educational attainment. Indeed, desegregation court orders increased Black educational attainment, and these effects are related to improvements in self-reported health, but mortality and preventive care effects appear to be unrelated to either educational attainment effects or peer composition effects. Some of desegregation's benefits may then have come from the broader psychological effects of desegregation itself; this is consistent with patterns in which Black-white gaps in health outcomes remain even within comparable education levels (Williams & Jackson, 2005). This finding offers lessons for studies of the health effects of educational policies more broadly; beyond the educational effects of such policies, the manner in which the policies are implemented may themselves have important health consequences.

Future research on the impact of desegregation court orders can explore additional mechanisms that may mediate health effects, and examine a broader range of outcomes. To investigate mechanisms beyond education attainment, other intermediate outcomes can be examined (such as characteristics of residential locations, occupations, income, and family structure). Some additional health outcomes that may be particularly promising to study include trust in the medical system and insurance claims. In addition, mortality can be examined in Census data sets with larger sample sizes and better geographic information on schooling location.

Another important direction in this research agenda is to understand the effects of present-day school segregation. Despite desegregation court orders, contemporary levels of school segregation remain high (Reardon et al., 2012; Caetano & Maheshri, 2023), particularly after cases such as *Parents Involved in Community Schools v. Seattle School District No. 1 (2007)* limited the ability of districts to consider race in school assignments. My results suggest that if current school segregation reduces educational attainment, there may be future consequences on self-reported health, and the experience of attending a school with high levels of segregation itself may reduce interactions with the medical system. Understanding whether these relationships are likely to hold in the present is critical in determining the consequences of contemporary segregation.

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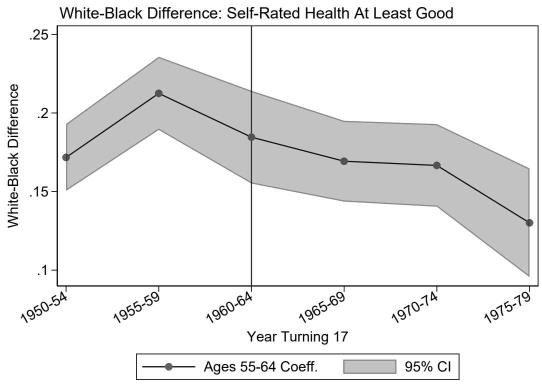
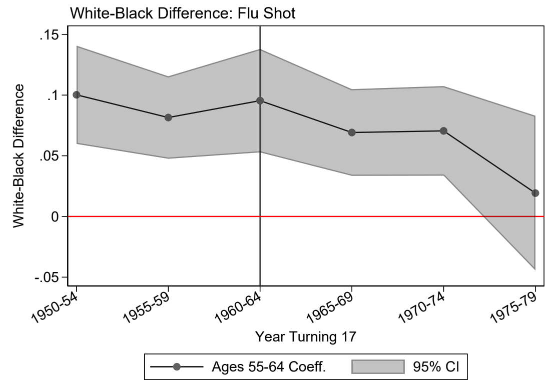
Figures

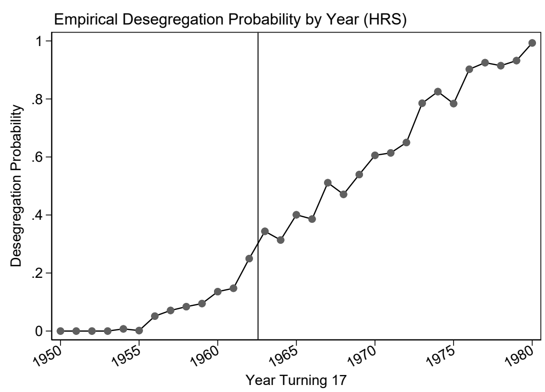
Figure 1: Time Series of Health Outcomes and Desegregation Probabilities



(a) White-Black Differences: Self-Rated Overall Health (Ages 55-64)



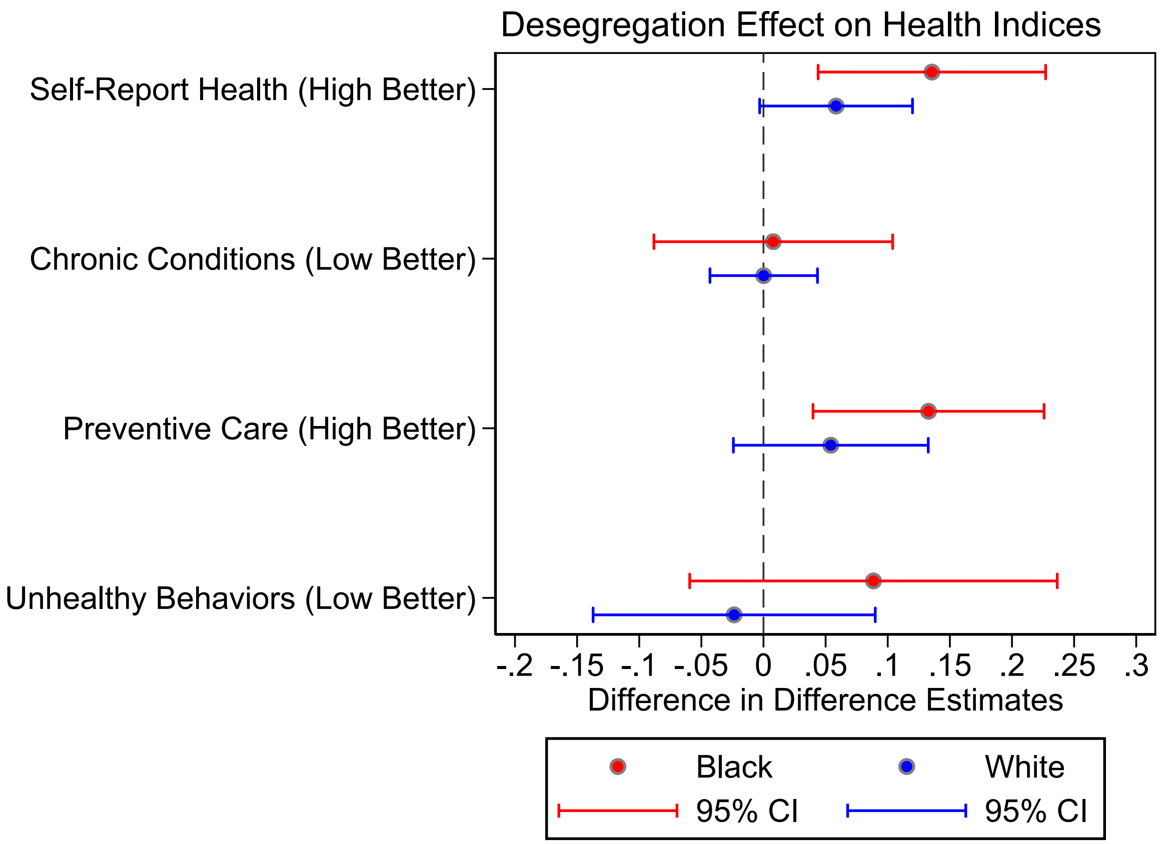
(b) White-Black Differences: Flu Shots (Ages 55-64)



(c) Overall Desegregation Probabilities

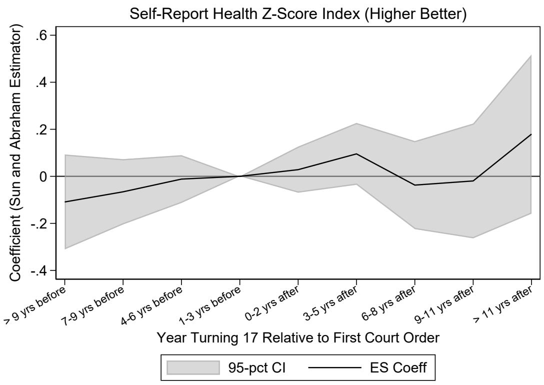
Notes: Panels (a) and (b) show the time series of White-Black differences in the indicated health outcome based on birth years, among individuals who were between ages 55 and 64 when responding to the HRS. 95% confidence intervals are shown in gray. Panel (c) shows the corresponding time series of non-Hispanic Black desegregation probabilities P_{bs}^{black} by year; the time series does not monotonically increase only due to differences in distribution of states in the HRS by birth year. In each graph, the vertical line corresponds to 1964, when desegregation court orders began to accelerate due to the Civil Rights Act.

Figure 2: Effect of Desegregation on Overall Health Indices

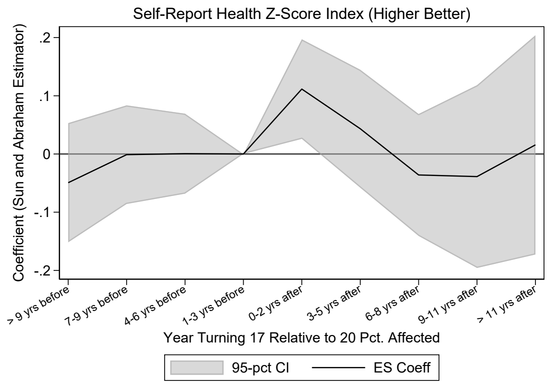


Notes: This graph shows, separately by race, the difference-in-difference coefficient from Equation 2 for each summary index. 95-percent confidence intervals, based on heteroskedasticity-robust standard errors, are shown for each estimate. The sample consists of $N = 29266$ non-Hispanic Black responses and $N = 113765$ non-Hispanic white responses for self-reported health; $N = 27987$ non-Hispanic Black responses and $N = 108339$ non-Hispanic white responses for chronic conditions; $N = 14787$ non-Hispanic Black responses and $N = 55421$ non-Hispanic white responses for preventive care; and $N = 16800$ non-Hispanic Black responses and $N = 61658$ non-Hispanic white responses for unhealthy behaviors.

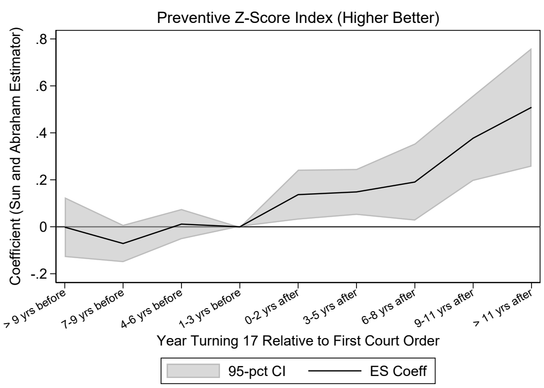
Figure 3: Event Studies for Black Health Outcomes (continued on next page)



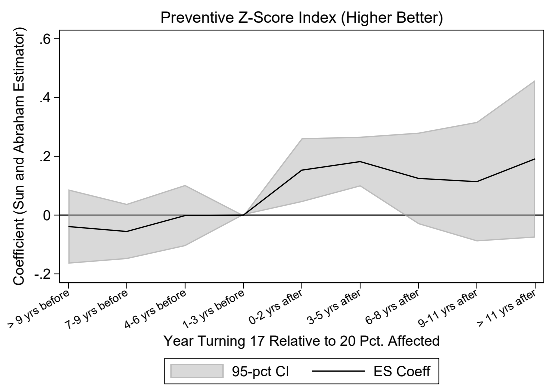
(a) Self-Reported Health: First Court Order



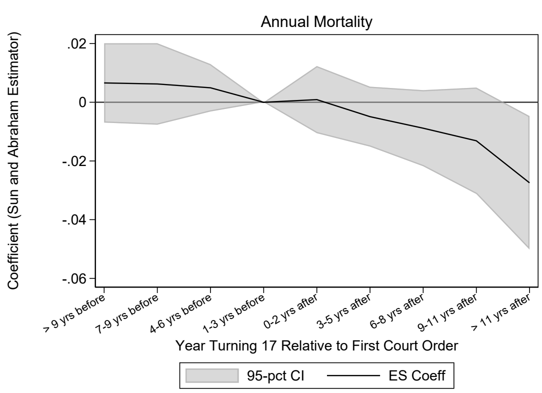
(b) Self-Reported Health: 20% Affected



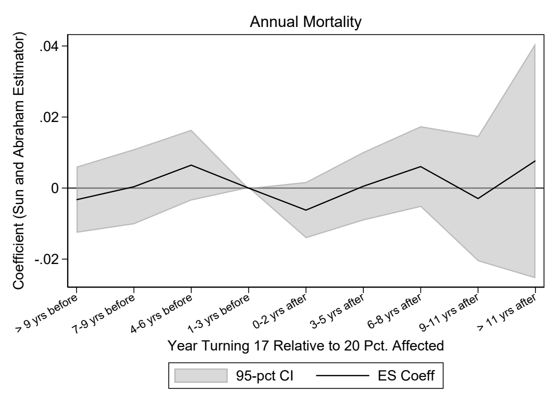
(c) Preventive Care: First Court Order



(d) Preventive Care: 20% Affected

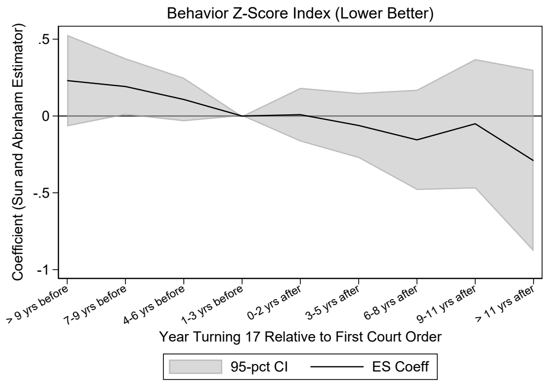


(e) Annual Mortality: First Court Order

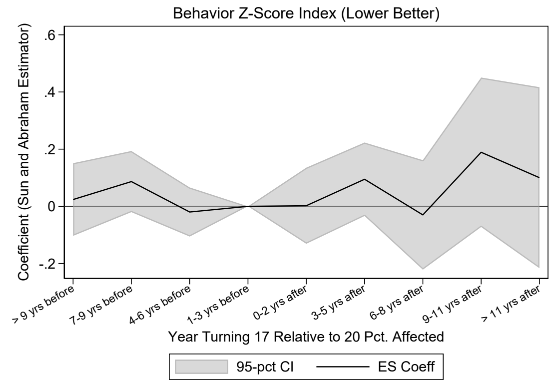


(f) Annual Mortality: 20% Affected

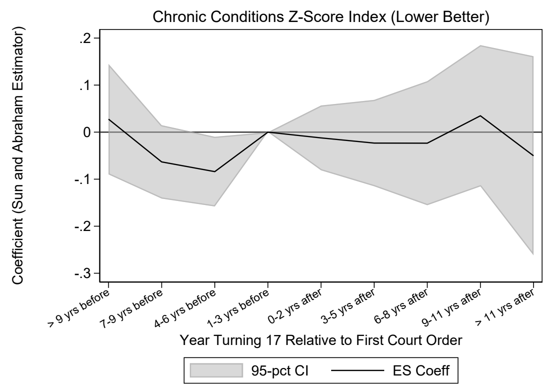
Figure 3: Event Studies for Black Health Outcomes (continued)



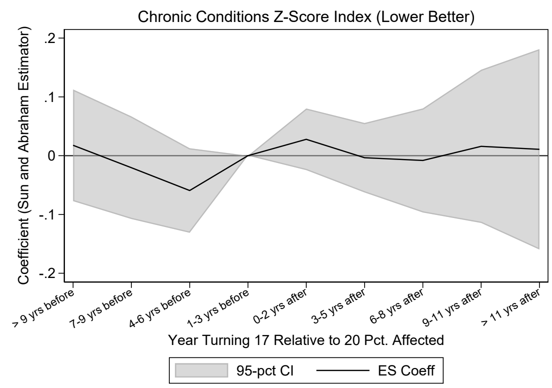
(g) Unhealthy Behaviors: First Court Order



(h) Unhealthy Behaviors: 20% Affected



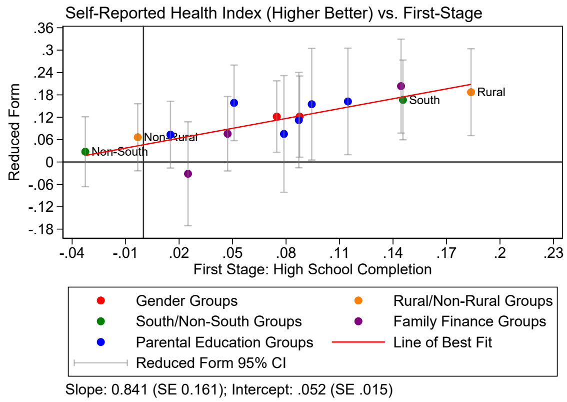
(i) Chronic Conditions: First Court Order



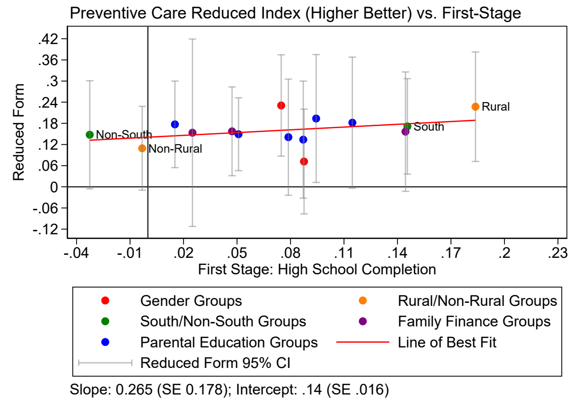
(j) Chronic Conditions: 20% Affected

Notes: These graphs show, among non-Hispanic Black respondents, the Sun and Abraham (2021) event study estimates, relative either to the first year in each state with any court order (in panels (a), (c), (e), (g), and (i)) or the first year in each state in which 20% of Black students were affected by a court order (in panels (b), (d), (f), (h), and (j)). Each estimate uses the last-treated state as a control; when using the first court order, Arizona and Wisconsin are the controls and when using the 20% threshold, Illinois is the control. Relative years are grouped into bins, and the coefficients for "1-3 years before" are normalized to zero. 95-percent confidence intervals, based on heteroskedasticity-robust standard errors clustered at the state level, are shown in the shaded area. All regressions control for individual covariates (gender, dummies for the respondent's father being unemployed or absent, and dummies for growing up in a rural area); survey characteristics (survey dummies and a cubic in age at the time of survey response); and area characteristics (community health center exposure, desegregated hospital exposure, and food stamp program exposure).

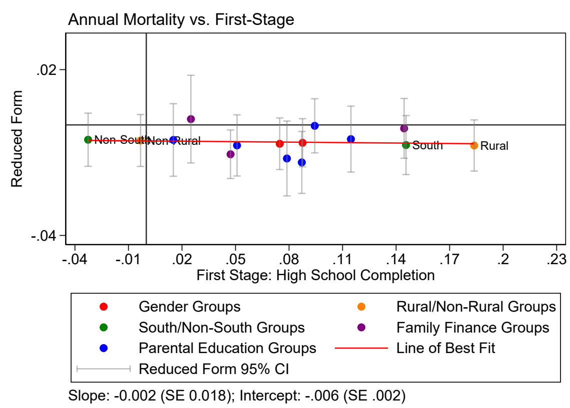
Figure 4: Health Effects vs. Education Effects of Desegregation



(a) Self-Reported Health Index



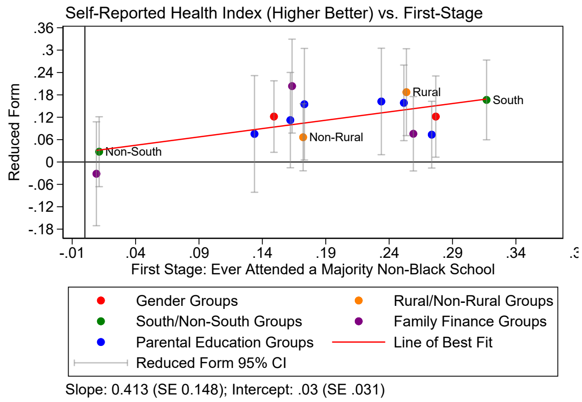
(b) Preventive Care Index



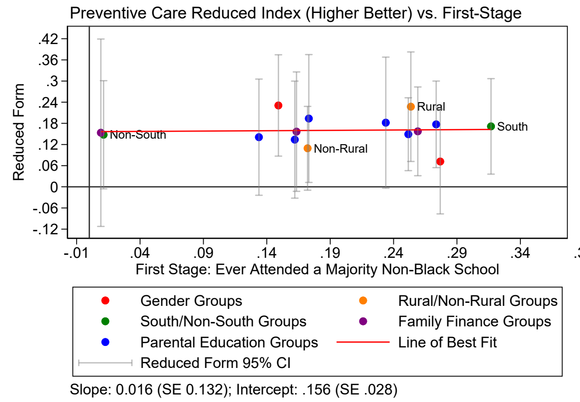
(c) Annual Mortality

Notes: These graphs plot, separately by demographic subgroups, the reduced form effects of desegregation probabilities on the indicated health outcome (from Equation 7) against the first stage effects of desegregation probabilities on high school completion (from Equation 5). Each point is a separate demographic subgroup; for an individual outcome, all reduced form estimates are computed within one regression and all first stage estimates are computed within one regression. Weights for the regression lines are based on the inverse of the standard error on the reduced form estimate.

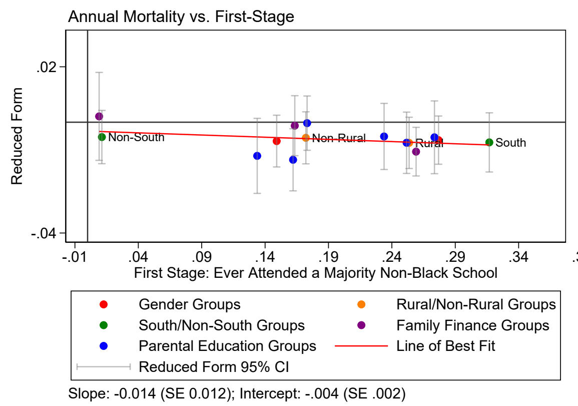
Figure 5: Health Effects vs. Peer Composition Effects of Desegregation



(a) Self-Reported Health Index



(b) Preventive Care Index



(c) Annual Mortality

Notes: These graphs plot, separately by demographic subgroups, the reduced form effects of desegregation probabilities on the indicated health outcome (from Equation 7) against the first stage effects of desegregation probabilities on whether the respondent ever reported attending a school in which the largest race/ethnicity was not non-Hispanic Black (from Equation 5). Each point is a separate demographic subgroup; for an individual outcome, all reduced form estimates are computed within one regression and all first stage estimates are computed within one regression. Weights for the regression lines are based on the inverse of the standard error on the reduced form estimate.

Tables

Table 1: Respondent-Level Summary Statistics

	(1)	(2)	(3)	(4)	(5)
	Black		**White**		
Variable	Average	Observations	Average	Observations	P-Value on Difference
		A: Basic Demographics			
Male	0.44	5829	0.48	18196	0.000***
Year of Birth	1948.47	5829	1947.12	18196	0.000***
		B: Educational Attainment			
High School Completion	0.73	5808	0.88	18146	0.000***
College Completion	0.17	5808	0.31	18146	0.000***
Years of Education	12.36	5808	13.51	18146	0.000***
		C: School Region			
Northeast	0.12	5829	0.21	18196	0.000***
South	0.64	5829	0.28	18196	0.000***
Midwest	0.19	5829	0.39	18196	0.000***
West	0.06	5829	0.13	18196	0.000***
		D: Childhood Characteristics			
Rural School Location	0.45	5263	0.43	16409	0.207
Self-Rate Childhood Health	4.00	5829	4.16	18196	0.000***

Notes: The sample for this table is drawn from the 1992-2018 HRS responses; see the text for details on sample selection. Respondent-level variables are measured once over the lifetime of an individual. All estimates are weighted using survey weights, based on the last year that the respondent was sampled with a non-zero weight. Column (5) shows the p-value for the difference in means across Black and White respondents for the given outcome. *: p-value less than 0.10; **: p-value less than 0.05; ***: p-value less than 0.01.

Table 2: Response-Level Summary Statistics

	(1)	(2)	(3)	(4)	(5)
	Black		**White**		
Variable	Average	Observations	Average	Observations	P-Value on Difference
		A: Basic Demographics			
Male	0.42	32642	0.47	124172	0.000***
Age in Survey Year	63.64	32640	64.52	124169	0.000***
Annual Mortality (per 10,000)	247.35	29956	185.76	114423	0.000***
		B: Self-Rated Health			
Overall Health At Least Good	0.63	32616	0.79	124099	0.000***
Hearing At Least Good	0.84	32619	0.82	124080	0.000***
Vision At Least Good	0.67	32583	0.85	124032	0.000***
		C: Chronic Conditions			
Cancer (Non-Skin)	0.11	32565	0.13	123781	0.000***
Lung Disease	0.09	32535	0.10	123632	0.000***
Stroke	0.09	32471	0.05	123363	0.000***
Arthritis	0.57	32027	0.54	121982	0.000***
Psychological Condition	0.16	32461	0.18	123502	0.000***
High Blood Pressure	0.71	31267	0.51	118736	0.000***
Diabetes	0.30	31252	0.17	118688	0.000***
Heart Conditions	0.21	32406	0.21	123318	0.411
		D: Preventive Care			
Cholesterol Test	0.78	16273	0.82	60026	0.000***
Flu Shot	0.50	16352	0.61	60385	0.000***
Mammogram (Among Women)	0.76	9920	0.74	33066	0.019**
Pap Smear (Among Women)	0.62	9899	0.57	33004	0.000***
Prostate Exam (Among Men)	0.67	6355	0.70	26960	0.003***
		E: Unhealthy Behaviors			
Currently Smokes	0.30	24274	0.22	90992	0.000***
Three or More Drinks per Sitting	0.10	26723	0.12	97356	0.000***
No Vigorous Exercise	0.69	31189	0.59	118537	0.000***

Notes: The sample for this table is drawn from the 1992-2018 HRS responses; see the text for details on sample selection. Response-level variables are measured once per individual survey response, so may be repeated for a given individual. All estimates are weighted using survey weights. Column (5) shows the p-value for the difference in means across Black and White respondents for the given outcome. *: p-value less than 0.10; **: p-value less than 0.05; ***: p-value less than 0.01.

Table 3: Effect of Desegregation on Black School Peer Composition

	(1)	(2)
	Majority Race Not Black	Majority Race White
	A: Overall	
Overall Coefficient	0.168**	0.155*
(SE)	(0.074)	(0.081)
N	1427	1427
Mean	0.331	0.297
	B: Rural and Non-Rural	
Non-Rural	0.163**	0.140*
(SE)	(0.077)	(0.084)
Rural	0.176**	0.183**
(SE)	(0.082)	(0.091)
Rural - Non-Rural Difference	0.012	0.043
(SE)	(0.057)	(0.061)
N	1427	1427
	C: Region	
Non-South	0.031	0.008
(SE)	(0.095)	(0.113)
South	0.277**	0.273***
(SE)	(0.111)	(0.102)
South - Non-South Difference	0.247**	0.265**
(SE)	(0.101)	(0.104)
N	1427	1427

Notes: This table shows the coefficients from regressing the indicated individual-level school peer composition variables on the probability of being exposed to a desegregation court order for at least one year of school, as well as covariates. Outcomes come from the 2015 and 2017 Life History Mail Surveys. In Column (1), the outcome is reporting ever attending a school up to 12th grade where the majority race was White, Hispanic, or Other (not Black). In Column (2), the outcome is reporting ever attending a school up to 12th grade where the majority race was White. The sample is Non-Hispanic Black respondents who turned 17 between 1940 and 1986. Each panel represents a different regression, with panels B and C using regressions that include interactions between desegregation probabilities and the indicated characteristic. Heteroskedasticity-robust standard errors are shown in parentheses. Controls are included for gender, indicators for a father being unemployed or absent, and indicators for growing up in a rural area. Additional program controls include CHCs and FSP rollout. All estimates use sample weights, based on the weight for the last survey year that the respondent is observed. *: p-value less than 0.1; **: p-value less than 0.05; ***: p-value less than 0.01.

Table 4: Effect of Desegregation on Education Outcomes

	(1)	(2)	(3)	(4)
	Black		**White**	
	High School Completion	Years of Education	High School Completion	Years of Education
		A: Overall		
Overall Coefficient	0.045	0.425	0.017	0.104
(SE)	(0.034)	(0.291)	(0.015)	(0.153)
N	5013	5013	15818	15818
Mean	0.746	12.498	0.893	13.618
		B: Rural and Non-Rural		
Non-Rural	–0.016	–0.071	0.003	0.052
(SE)	(0.041)	(0.335)	(0.014)	(0.144)
Rural	0.145***	1.230***	0.040**	0.186
(SE)	(0.040)	(0.301)	(0.019)	(0.201)
Rural - Non-Rural Difference	0.162***	1.301***	0.038***	0.134
(SE)	(0.048)	(0.341)	(0.013)	(0.143)
N	5013	5013	15818	15818
		C: Region		
Non-South	–0.021	–0.028	0.008	0.095
(SE)	(0.040)	(0.309)	(0.016)	(0.162)
South	0.124***	0.967***	0.039	0.126
(SE)	(0.042)	(0.337)	(0.027)	(0.256)
South - Non-South Difference	0.145***	0.995***	0.031	0.031
(SE)	(0.046)	(0.311)	(0.030)	(0.263)
N	5013	5013	15818	15818

Notes: This table shows the coefficients from regressing the indicated individual-level educational attainment variables on the probability of being exposed to a desegregation court order for at least one year of school, as well as covariates. The sample is Non-Hispanic Black and Non-Hispanic White respondents who turned 17 between 1940 and 1986. Each panel represents a different regression, with panels B and C using regressions that include interactions between desegregation probabilities and the indicated characteristic. Heteroskedasticity-robust standard errors are shown in parentheses. Controls are included for gender, indicators for a father being unemployed or absent, and indicators for growing up in a rural area. Additional program controls include CHCs and FSP rollout. High school completion is defined as reporting at least 12 years of education. All estimates use sample weights, based on the weight for the last survey year that the respondent is observed. *: p-value less than 0.1; **: p-value less than 0.05; ***: p-value less than 0.01.

Table 5: Effect of Desegregation on Additional Helath Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Annual Mortality	Frequent Pain	Obese	Lost All Permanent Teeth	Saw Dentist in Past 2 Years	Wears Hearing Aid
			A: Non-Hispanic Black			
Coeff.	–0.008**	–0.001	0.049	–0.047	0.091***	–0.002
(SE)	(0.004)	(0.049)	(0.041)	(0.038)	(0.030)	(0.005)
Obs.	26936	29272	12323	9917	26881	28750
Mean	0.022	0.353	0.455	0.188	0.491	0.010
			B: Non-Hispanic White			
Coeff.	–0.004*	–0.007	–0.035*	–0.018	0.033	0.003
(SE)	(0.002)	(0.021)	(0.021)	(0.018)	(0.025)	(0.003)
Obs.	105099	113840	42946	30238	104156	107175
Mean	0.017	0.336	0.325	0.115	0.719	0.024

Notes: This table shows the coefficients on the probability of being exposed to a desegregation court order for at least one year of school, based on the difference-in-differences specification, where each outcome is indicated in the column title, and each outcome is in a separate regression. Results for 1-year mortality exclude individuals who could not be verified as either dead or alive during the following year. The sample is students who turned 17 between 1940 and 1986, inclusive, and were between the ages of 51 and 90 during the survey year. Heteroskedasticity-robust standard errors clustered at the state level are shown in parentheses. All estimates use sample weights. *: p-value less than 0.1, **: p-value less than 0.05; ***: p-value less than 0.01.

Table 6: Heterogeneity in Black Health Outcomes

	(1)	(2)	(3)	(4)
	Self-Report Health Z-Score Index (Higher Better)	Chronic Conditions Z-Score Index (Lower Better)	Preventive Z-Score Index (Higher Better)	Behavior Z-Score Index (Lower Better)
		A: Gender		
Female	0.132***	0.005	0.207***	0.086
(SE)	(0.045)	(0.054)	(0.076)	(0.076)
Male	0.140**	0.012	0.051	0.091
(SE)	(0.058)	(0.049)	(0.069)	(0.088)
Male - Female Difference	0.008	0.007	–0.156**	0.005
(SE)	(0.042)	(0.037)	(0.067)	(0.063)
N	29266	27987	14886	16800
		B: Rural and Non-Rural		
Non-Rural	0.091**	0.010	0.101*	0.030
(SE)	(0.042)	(0.045)	(0.060)	(0.077)
Rural	0.209***	0.004	0.211**	0.184*
(SE)	(0.067)	(0.060)	(0.083)	(0.101)
Rural - Non-Rural Difference	0.118**	–0.006	0.110**	0.154*
(SE)	(0.055)	(0.032)	(0.051)	(0.083)
N	29266	27987	14886	16800
		C: Region		
Non-South	0.028	0.064	0.144*	0.167**
(SE)	(0.053)	(0.047)	(0.079)	(0.078)
South	0.216***	–0.034	0.141*	0.036
(SE)	(0.051)	(0.055)	(0.075)	(0.086)
South - Non-South Difference	0.188***	–0.098***	–0.003	–0.131*
(SE)	(0.050)	(0.035)	(0.082)	(0.070)
N	29266	27987	14886	16800

Notes: This table shows the results of a heterogeneity analysis for each outcome indicated in the column titles, with a separate regression for each panel that includes interactions with the indicated characteristic. The reduced preventive care index only includes cholesterol tests and flu shots for comparability. Heteroskedasticity-robust standard errors are shown in parentheses. All estimates use sample weights. *: p-value less than 0.1; **: p-value less than 0.05; ***: p-value less than 0.01.

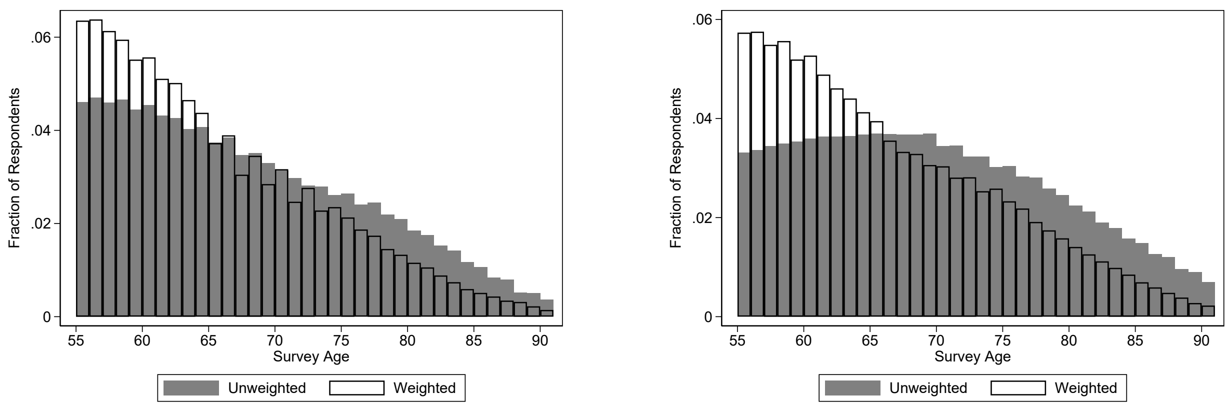
Table 7: Robustness - Effect of Desegregation on Black Health Outcomes

	(1)	(2)	(3)	(4)	(5)
	Self-Report Health Index	Chronic Conditions Index	Preventive Care Index	Behavior Index	Annual Mortality
			A: Baseline		
Coefficient	0.136***	0.008	0.133***	0.089	–0.008**
(SE)	(0.047)	(0.049)	(0.047)	(0.075)	(0.004)
Observations	29266	27987	14787	16800	26936
Outcome Mean	0.158	–0.053	–0.015	0.186	0.022
			B: Person-Level Controls Only		
Coefficient	0.148***	0.004	0.163***	0.067	–0.006*
(SE)	(0.046)	(0.051)	(0.049)	(0.071)	(0.004)
Observations	29266	27987	14787	16800	26936
Outcome Mean	0.158	–0.053	–0.015	0.186	0.022
			C: Additional Controls and Area Trends		
Coefficient	0.110***	0.012	0.126***	0.104*	–0.007**
(SE)	(0.042)	(0.044)	(0.045)	(0.061)	(0.004)
Observations	27823	26615	14058	15979	25613
Outcome Mean	0.153	–0.050	–0.017	0.181	0.022
			D: Later Start Year (1950)		
Coefficient	0.074*	–0.002	0.097***	0.079	–0.005
(SE)	(0.041)	(0.037)	(0.034)	(0.055)	(0.004)
Observations	26223	25105	13284	14899	26221
Outcome Mean	0.180	–0.063	–0.018	0.217	0.018
			E: Lower-Bound Desegregation Probability		
Coefficient	0.163***	0.015	0.135**	0.091	–0.010**
(SE)	(0.049)	(0.051)	(0.056)	(0.090)	(0.004)
Observations	29266	27987	14787	16800	26936
Outcome Mean	0.158	–0.053	–0.015	0.186	0.022

Notes: This table shows the coefficients on the probability of being exposed to a desegregation court order for at least one year of school, based on the difference-in-differences specification, where each outcome is indicated in the title of the panel. Each column and panel represents a separate regression. The baseline sample is non-Hispanic Black students who turned 17 between 1940 and 1986, inclusive, and were between the ages of 51 and 90 during the survey year. The baseline specification includes individual-level covariates in addition to controls for contemporaneous programs. The regressions for person-level controls only drop the covariates for contemporaneous programs. The regressions with additional controls and area trends include all covariates from the baseline specification, in addition to hospital beds per capita, the number of hospitals per capita, self-reported childhood health status, self-reported family finances as a child, and linear time trends in state averages of county characteristics in 1960 (the share of the population that is Black, the share with a high school degree, the percent working in manufacturing, the percent unemployed, and the population density). The Later Start Year regressions are identical to the baseline, but begin with Black students who turned 17 in 1950. Non-parametric age controls include indicators for five-year age bins, rather than a cubic in age. Heteroskedasticity-robust standard errors clustered at the state level are shown in parentheses. The Lower-Bound Desegregation Probability regressions additionally include students who were never directly affected by a desegregation court order in the denominator for constructing desegregation probabilities. All estimates use sample weights. *: p-value less than 0.1; **: p-value less than 0.05; ***: p-value less than 0.01.

Appendix A   Appendix Exhibits

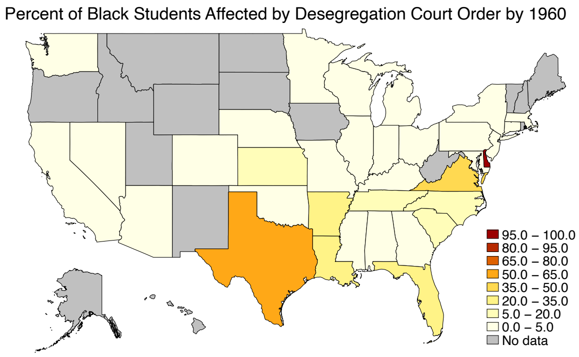
Figure A.1: Sample Age Distribution



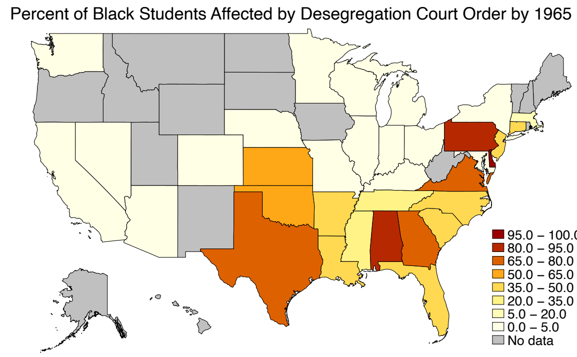
(a) Black, Non-Hispanic (b) White, Non-Hispanic

Notes: These histograms show the distributions of ages in the sample at the time of responding to the HRS, separately for the Black Non-Hispanic respondents and for the White Non-Hispanic respondents. Respondents are included for each time that they respond to the HRS. Weighted histograms apply sample weights, while unweighted histograms do not.

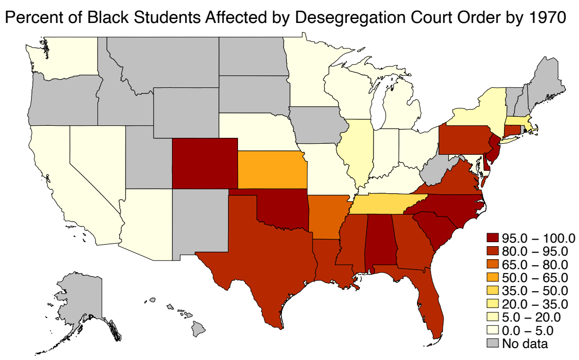
Figure A.2: Timing of Desegregation Court Orders by State



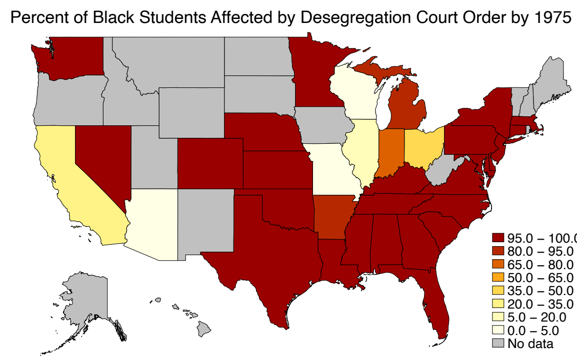
(a) 1960



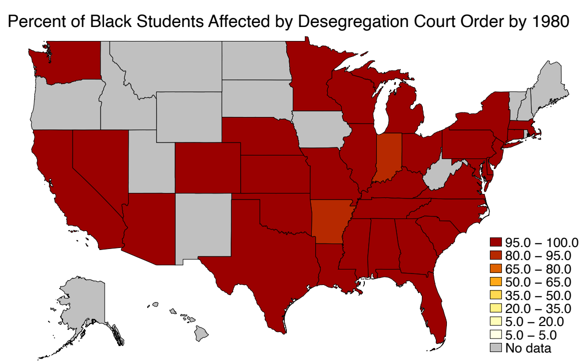
(b) 1965



(c) 1970



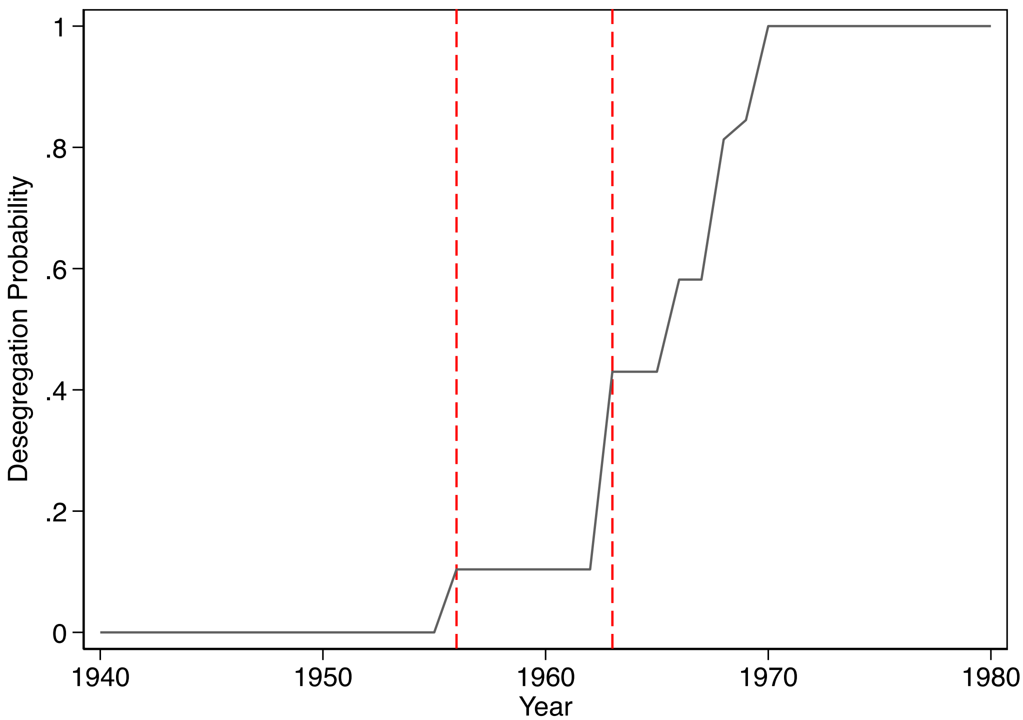
(d) 1975



(e) 1980

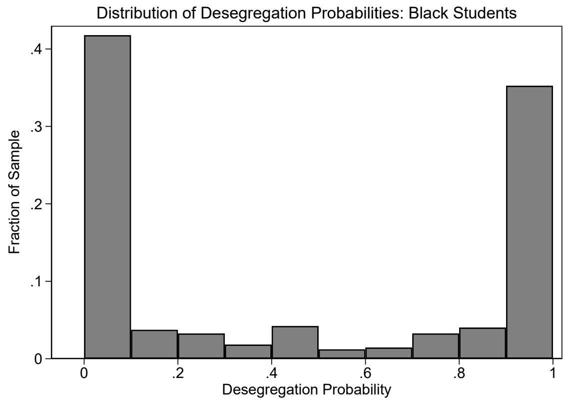
Notes: These graphs show the geographic distribution of the baseline desegregation probability measure P_{bs}^{black} over time for each state based on Equation 1. States in gray did not have a desegregation court order, and therefore are not included in the sample.

Figure A.3: South Carolina Desegregation Probabilities

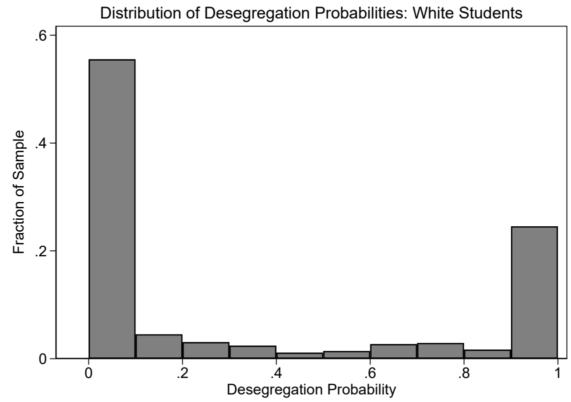


Notes: This graph shows the desegregation probability P_{bs}^{black} over time for South Carolina – as an illustrative example – based on Equation 1. The year corresponds to the year of desegregation court orders, and for respondents the year corresponds to the year in which they turned 17. The first court order in 1956 corresponds to the Sumter court order and the court order. The second court order in 1963 corresponds to the Charleston court order.

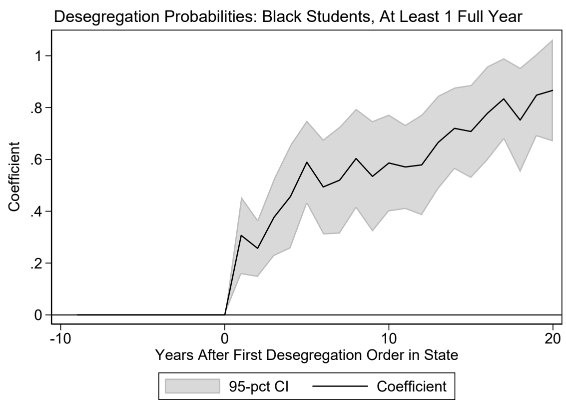
Figure A.4: Desegregation Probabilities by Race



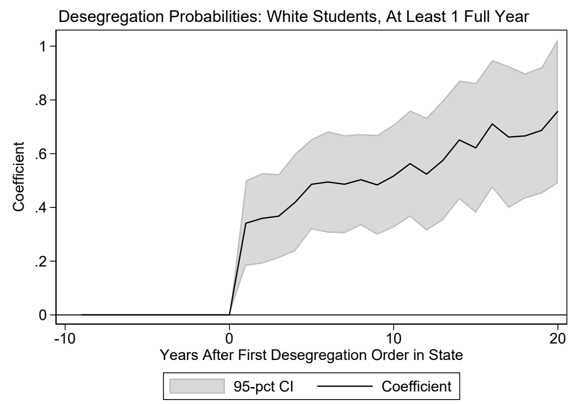
(a) Black Desegregation Probabilities



(b) White Desegregation Probabilities



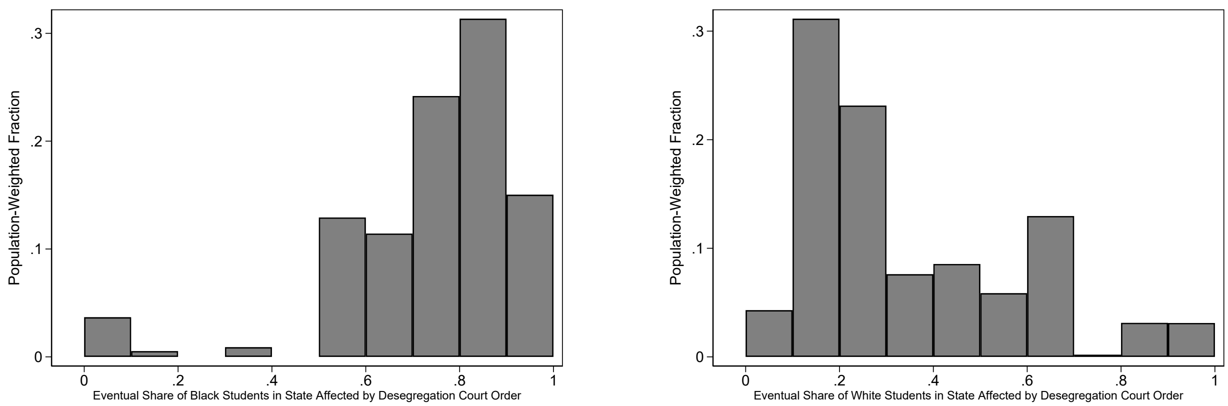
(c) Black Desegregation Time Path



(d) White Desegregation Time Path

Notes: Panels (a) and (b) show the distribution of respondent-level probabilities of being impacted by a desegregation court order for at least one year, using the baseline measure from Equation 1; full sample sizes are shown in Table 1. Panels (c) and (d) show the coefficients and confidence intervals from regressing the probability of being impacted by a desegregation court order for at least one year on years relative to the first desegregation court order in a state; this corresponds to a first-stage for the event study plots shown in Figure 3. The final period (year 20) corresponds to the average across all periods 20 or more years after the first desegregation order.

Figure A.5: State-Level Maximum of Lower-Bound Desegregation Probabilities



(a) Black, Non-Hispanic

(b) White, Non-Hispanic

Notes: These histograms show the distribution of the maximum value of the "lower-bound desegregation probability" across states, out of states that ever had a desegregation court order. Graphs are weighted by the geographic distribution of HRS respondents, after applying relevant sample weights.

Table A.1: Effect of Desegregation on Self-Rated Health Measures

	(1)	(2)	(3)	(4)
	Z-Score Index (Higher Better)	Self-Rated Health At Least Good	Self-Rated Hearing At Least Good	Self-Rated Vision At Least Good
		A: Non-Hispanic Black		
Coeff.	0.136***	0.075*	0.040	0.077***
(SE)	(0.047)	(0.039)	(0.030)	(0.029)
Obs.	29266	29310	29320	29290
Mean (Full)	0.158	0.633	0.842	0.676
		B: Non-Hispanic White		
Coeff.	0.058*	0.029*	0.024	0.021
(SE)	(0.031)	(0.015)	(0.019)	(0.015)
Obs.	113765	113944	113930	113884
Mean (Full)	0.138	0.794	0.827	0.849
		C: Non-Hispanic Black, Index Sample		
Coeff.	0.136***	0.076*	0.041	0.077***
(SE)	(0.047)	(0.039)	(0.030)	(0.029)
Obs.	29266	29266	29266	29266
Mean (Full)	0.158	0.633	0.842	0.676
		D: Non-Hispanic White, Index Sample		
Coeff.	0.058*	0.029*	0.024	0.021
(SE)	(0.031)	(0.015)	(0.019)	(0.015)
Obs.	113765	113765	113765	113765
Mean (Full)	0.138	0.794	0.827	0.849

Notes: This table shows the coefficients on the probability of being exposed to a desegregation court order for at least one year of school, based on the difference-in-differences specification, where each outcome is indicated in the column title, and each outcome is in a separate regression. The sample is students who turned 17 between 1940 and 1986, inclusive, and were between the ages of 51 and 90 during the survey year. The index is constructed to have a mean of 0 for each race among those who turned 17 between 1942 and 1951. Heteroskedasticity-robust standard errors clustered at the state level are shown in parentheses. All estimates use sample weights. *: p-value less than 0.1, **: p-value less than 0.05; ***: p-value less than 0.01.

Table A.2: Effect of Desegregation on Chronic Conditions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Z-Score Index (Lower Better)	Cancer (Non-Skin)	Lung Disease	Stroke	Arthritis	Psychological Condition	High Blood Pressure	Diabetes	Heart Conditions
				A: Non-Hispanic Black					
Coeff.	0.008	–0.013	0.027	–0.048*	0.012	0.031	–0.007	–0.007	0.022
(SE)	(0.049)	(0.020)	(0.026)	(0.025)	(0.045)	(0.034)	(0.031)	(0.038)	(0.031)
Obs.	27987	29279	29246	29189	28845	29189	28227	28215	29155
Mean (Full)	–0.053	0.105	0.090	0.092	0.569	0.157	0.707	0.301	0.208
				B: Non-Hispanic White					
Coeff.	0.000	0.006	–0.005	–0.007	0.020	0.001	–0.040	–0.009	0.005
(SE)	(0.022)	(0.015)	(0.013)	(0.007)	(0.024)	(0.016)	(0.027)	(0.018)	(0.020)
Obs.	108339	113671	113567	113322	112125	113437	109307	109271	113291
Mean (Full)	–0.096	0.131	0.097	0.047	0.534	0.181	0.501	0.165	0.211
				C: Non-Hispanic Black, Index Sample					
Coeff.	0.008	–0.012	0.027	–0.048*	0.014	0.029	–0.004	–0.006	0.026
(SE)	(0.049)	(0.020)	(0.026)	(0.026)	(0.046)	(0.034)	(0.032)	(0.038)	(0.031)
Obs.	27987	27987	27987	27987	27987	27987	27938	27987	27987
Mean (Full)	–0.053	0.107	0.091	0.093	0.573	0.158	0.707	0.300	0.211
				D: Non-Hispanic White, Index Sample					
Coeff.	0.000	0.007	–0.004	–0.006	0.019	0.003	–0.038	–0.007	0.006
(SE)	(0.022)	(0.015)	(0.014)	(0.006)	(0.024)	(0.016)	(0.027)	(0.018)	(0.020)
Obs.	108339	108339	108339	108339	108339	108339	108189	108339	108339
Mean (Full)	–0.096	0.134	0.098	0.048	0.539	0.184	0.500	0.165	0.213

Notes: This table shows the coefficients on the probability of being exposed to a desegregation court order for at least one year of school, based on the difference-in-differences specification, where each outcome is indicated in the column title, and each outcome is in a separate regression. The sample is students who turned 17 between 1940 and 1986, inclusive, and were between the ages of 51 and 90 during the survey year. The index is constructed to have a mean of 0 for each race among those who turned 17 between 1942 and 1951. Heteroskedasticity-robust standard errors clustered at the state level are shown in parentheses. All estimates use sample weights. *: p-value less than 0.1, **: p-value less than 0.05; ***: p-value less than 0.01.

Table A.3: Effect of Desegregation on Preventive Care

	(1)	(2)	(3)	(4)	(5)	(6)
	Z-Score Index (Higher Better)	Cholesterol Test	Flu Shot	Mammogram	Pap Smear	Prostate Exam
			A: Non-Hispanic Black			
Coeff.	0.133***	0.072**	0.055	0.019	0.063	0.042
(SE)	(0.047)	(0.031)	(0.044)	(0.034)	(0.047)	(0.049)
Obs.	14787	14905	14972	9022	9006	5876
Mean (Full)	–0.015	0.782	0.499	0.761	0.628	0.674
			B: Non-Hispanic White			
Coeff.	0.054	0.012	0.021	0.035	0.008	0.038
(SE)	(0.040)	(0.017)	(0.023)	(0.026)	(0.038)	(0.030)
Obs.	55421	55926	56236	30359	30311	25543
Mean (Full)	–0.088	0.824	0.604	0.750	0.581	0.699
			C: Non-Hispanic Black, Index Sample			
Coeff.	0.133***	0.070**	0.055	0.019	0.061	0.043
(SE)	(0.047)	(0.031)	(0.042)	(0.035)	(0.046)	(0.050)
Obs.	14787	14787	14787	8942	8942	5845
Mean (Full)	–0.015	0.782	0.499	0.762	0.630	0.675
			D: Non-Hispanic White, Index Sample			
Coeff.	0.054	0.014	0.021	0.033	0.009	0.035
(SE)	(0.040)	(0.016)	(0.021)	(0.026)	(0.038)	(0.029)
Obs.	55421	55421	55421	30025	30025	25396
Mean (Full)	–0.088	0.823	0.603	0.751	0.584	0.701

Notes: This table shows the coefficients on the probability of being exposed to a desegregation court order for at least one year of school, based on the difference-in-differences specification, where each outcome is indicated in the column title, and each outcome is in a separate regression. The sample is students who turned 17 between 1940 and 1986, inclusive, and were between the ages of 51 and 90 during the survey year. The index is constructed to have a mean of 0 for each race among those who turned 17 between 1942 and 1951. Heteroskedasticity-robust standard errors clustered at the state level are shown in parentheses. All estimates use sample weights. *: p-value less than 0.1, **: p-value less than 0.05; ***: p-value less than 0.01.

Table A.4: Effect of Desegregation on Unhealthy Behaviors

	(1)	(2)	(3)	(4)
	Z-Score Index (Lower Better)	Currently Smokes	Three or More Drinks per Sitting	Not Vigorously Exercising
	A: Non-Hispanic Black			
Coeff.	0.089	0.042	–0.012	0.029
(SE)	(0.075)	(0.040)	(0.029)	(0.030)
Obs.	16800	21696	24121	28165
Mean (Full)	0.185	0.303	0.098	0.686
	B: Non-Hispanic White			
Coeff.	–0.024	–0.012	0.012	–0.039*
(SE)	(0.058)	(0.036)	(0.017)	(0.024)
Obs.	61658	82900	89599	109141
Mean (Full)	0.165	0.225	0.124	0.588
	C: Non-Hispanic Black, Index Sample			
Coeff.	0.089	0.025	0.015	0.054*
(SE)	(0.075)	(0.045)	(0.033)	(0.031)
Obs.	16800	16800	16800	16800
Mean (Full)	0.185	0.304	0.121	0.692
	D: Non-Hispanic White, Index Sample			
Coeff.	–0.024	–0.016	0.001	–0.014
(SE)	(0.058)	(0.037)	(0.027)	(0.026)
Obs.	61658	61658	61658	61658
Mean (Full)	0.165	0.227	0.151	0.587

Notes: This table shows the coefficients on the probability of being exposed to a desegregation court order for at least one year of school, based on the difference-in-differences specification, where each outcome is indicated in the column title, and each outcome is in a separate regression. The sample is students who turned 17 between 1940 and 1986, inclusive, and were between the ages of 51 and 90 during the survey year. The index is constructed to have a mean of 0 for each race among those who turned 17 between 1942 and 1951. Heteroskedasticity-robust standard errors clustered at the state level are shown in parentheses. All estimates use sample weights. *: p-value less than 0.1, **: p-value less than 0.05; ***: p-value less than 0.01.

Table A.5: Effect of Desegregation on Black Health Interactions and Attitudes

	(1)	(2)	(3)
	Ever unfairly denied health care or treatment	Ever received poorer medical service because of race	Control over health (1-10)
Coeff.	–0.025	–0.036	0.931***
(SE)	(0.019)	(0.050)	(0.251)
N	2489	5066	5706
Mean	0.055	0.164	7.239

Notes: This table shows the coefficients on the probability of being exposed to a desegregation court order for at least one year of school, based on the difference-in-differences specification, where each outcome is indicated in the column title, and each outcome is in a separate regression. The sample is non-Hispanic Black students who turned 17 between 1940 and 1986, inclusive, and were between the ages of 51 and 90 during the survey year. Heteroskedasticity-robust standard errors clustered at the state level are shown in parentheses. Values are taken from the Psychosocial and Lifestyle Questionnaire, and all estimates use corresponding sample weights. *: p-value less than 0.1, **: p-value less than 0.05; ***: p-value less than 0.01.

Table A.6: Robustness - Effect of Desegregation on White Health Outcomes

	(1)	(2)	(3)	(4)	(5)
Specification	Self-Report Health Index	Chronic Conditions Index	Preventive Care Index	Behavior Index	Annual Mortality
			A: Baseline		
Coefficient	0.058*	0.000	0.054	–0.024	–0.004*
(SE)	(0.031)	(0.022)	(0.040)	(0.058)	(0.002)
Observations	113765	108339	55421	61658	105099
Outcome Mean	0.138	–0.096	–0.088	0.165	0.017
			B: Person-Level Controls Only		
Coefficient	0.065**	–0.008	0.052	–0.005	–0.004**
(SE)	(0.029)	(0.021)	(0.040)	(0.061)	(0.002)
Observations	113765	108339	55421	61658	105099
Outcome Mean	0.138	–0.096	–0.088	0.165	0.017
			C: Additional Controls and Area Trends		
Coefficient	0.052*	–0.001	0.056	–0.025	–0.004
(SE)	(0.029)	(0.018)	(0.045)	(0.058)	(0.002)
Observations	110525	105248	53806	59776	102119
Outcome Mean	0.141	–0.097	–0.087	0.167	0.017
			D: Later Start Year (1950)		
Coefficient	0.026	0.003	0.025	–0.053	–0.007***
(SE)	(0.030)	(0.021)	(0.033)	(0.050)	(0.002)
Observations	93064	88349	44859	49388	93594
Outcome Mean	0.163	–0.113	–0.104	0.199	0.012
			E: Lower-Bound Desegregation Probability		
Coefficient	0.055	0.042	0.046	0.013	–0.009***
(SE)	(0.039)	(0.042)	(0.058)	(0.129)	(0.004)
Observations	113765	108339	55421	61658	105099
Outcome Mean	0.138	–0.096	–0.088	0.165	0.017

Notes: This table shows the coefficients on the probability of being exposed to a desegregation court order for at least one year of school, based on the difference-in-differences specification, where each outcome is indicated in the title of the panel. Each column and panel represents a separate regression. The baseline sample is Black students who turned 17 between 1940 and 1986, inclusive, and were between the ages of 51 and 90 during the survey year. The baseline specification includes individual-level covariates in addition to controls for contemporaneous programs. The regressions for person-level controls only drop the covariates for contemporaneous programs. The regressions with additional controls and area trends include all covariates from the baseline specification, in addition to hospital beds per capita, the number of hospitals per capita, self-reported childhood health status, self-reported family finances as a child, and linear time trends in state averages of county characteristics in 1960 (the share of the population that is Black, the share with a high school degree, the percent working in manufacturing, the percent unemployed, and the population density). The Later Start Year regressions are identical to the baseline, but begin with White students who turned 17 in 1950. Non-parametric age controls include indicators for five-year age bins, rather than a cubic in age. Heteroskedasticity-robust standard errors clustered at the state level are shown in parentheses. The Lower-Bound Desegregation Probability regressions additionally include students who were never directly affected by a desegregation court order in the denominator for constructing desegregation probabilities. All estimates use sample weights. *: p-value less than 0.1; **: p-value less than 0.05; ***: p-value less than 0.01.

Table A.7: 2SLS Estimates Instrumenting Education with Desegregation

	(1)	(2)
	HS Completion	Years of Education
	A: Self-Reported Health Index (High Better)	
High School Completion	0.918***	.
(SE)	(0.183)	.
Years of Education	.	0.108***
(SE)	.	(0.023)
First Stage F-Stat	26.004	26.445
Overid test p-val	0.32	0.06
Overid test df	3	3
N	76938	76938
	B: Chronic Conditions Index (Lower Better)	
High School Completion	–0.162	.
(SE)	(0.166)	.
Years of Education	.	–0.015
(SE)	.	(0.020)
First Stage F-Stat	9.408	9.722
Overid test p-val	0.23	0.13
Overid test df	3	3
N	74766	74766
	C: Preventive Care Index, Reduced (Higher Better)	
High School Completion	0.503*	.
(SE)	(0.258)	.
Years of Education	.	0.048
(SE)	.	(0.031)
First Stage F-Stat	6.334	6.112
Overid test p-val	0.09	0.08
Overid test df	3	3
N	40454	40454
	D: Unhealthy Behaviors Index (Lower Better)	
High School Completion	0.165	.
(SE)	(0.257)	.
Years of Education	.	0.029
(SE)	.	(0.030)
First Stage F-Stat	22.369	19.221
Overid test p-val	0.12	0.15
Overid test df	3	3
N	45513	45513

Notes: This table shows the results of estimating the 2SLS IV regression for the impact of educational attainment on each outcome, instrumenting educational attainment with desegregation probabilities, plus interactions with covariates (gender, rural area, Southern states, and indicators for self-reported childhood financial status). The reduced preventive care index includes only the types of preventive care that are relevant to the full sample (cholesterol tests and flu shots). The sample is restricted to observations with non-missing covariates. Heteroskedasticity-robust standard errors clustered at the state level are shown in parentheses. All estimates use sample weights. *: p-value less than 0.1; **: p-value less than 0.05; ***: p-value less than 0.01.