Supplemental Information

SUPPLEMENTAL METHODS

Sample Restriction and Sample Attrition

Among the 3263 Black children who participated in at least 1 CDS wave, we included 2750 who were school-aged (5–17 years) between 1991 and 2014. Of these, 2654 had nonmissing values of the exposure and covariates. We thus performed a complete case analysis because such low levels of missingness are unlikely to result in substantial bias.[79,80] To avoid unstable estimates, we excluded children if <10 in the CDS sample were born in a given year or attended a given district, yielding a sample size of 2643. Finally, we excluded children who never lived in districts under desegregation orders in 1991, as districts never under a court order may not serve as appropriate controls.[13] The final analytic sample included 1248 Black children living in 439 school districts at baseline (defined as their first observed schooling year). We used all available person-year observations in the CDS such that some children provided multiple outcome measurements (mean, 1.5 observations). The sample size for each outcome also varied because some outcomes were collected in limited years or for limited age ranges.

We evaluated whether results may be biased due to sample attrition. In the CDS sample, children dropping out between waves before they reached 17 years of age may be problematic. To be more specific, children exposed to higher school segregation may have worse health outcomes, which may make them more likely to drop out of the sample. In this case, we would not be able to observe their later health outcomes. We found that 130 children in our sample dropped out before age 17 years. We compared the differences between children who dropped out and children who did not drop out out of the sample by regressing a binary indicator of dropping out on the exposure to school segregation and individual and school district characteristics. We did not find significant differences for most of the characteristics, except that children in districts with a smaller percentage of students eligible for free or reduced-price lunch and children in earlier cohorts were more likely to drop out of the sample.

Coding of Outcomes

Supplemental Table 4 shows details of each of the outcomes we used in this study. General health status was asked of both primary caregivers and of children aged ≥8 years using a 5-point Likert scale from poor to excellent. This variable was dichotomized as poor, fair, or good versus very good or excellent. In cases where caregivers and children both answered this question and gave different answers, we chose the answer indicating poorer health. We also captured whether the caregiver reported that a physician had diagnosed the child with asthma, obesity, or a mental/emotional health problem (coded as binary variables). For outcomes elicited in multiple survey waves, the child was considered to have a diagnosis if they ever reported having it. The BPI score (range, 0–27) was coded as a continuous variable; a higher score indicates a greater level of behavior problems.[34]

We created 2 outcomes measuring children's physical activity. One question asked how many days a week children got at least 30 minutes of vigorous physical activity outside school. We created a binary indicator representing whether children reported participation in >30 minutes of physical activity at least 3 days a week outside school. There were also 2 questions asking about the frequency and duration of physical activity inside school (i.e., PE classes) per week. We created a binary indicator representing whether children reported participation in >30 minutes of physical activity at least 3 days a week in school PE classes.

Three alcohol consumption–related outcomes (ever drank, drank at least monthly in the past year, and had ≥5 drinks at a time at least monthly in the past year) were coded as binary variables on the basis of the original questions. Two smoking outcomes (ever smoked and ever smoked regularly) were coded as binary variables, whereas the number of days smoked in the past month was coded as a continuous variable.

Black-White Dissimilarity Index

The dissimilarity index, a standard measure of segregation used in previous research,[13,42] represents how evenly 2 groups are distributed across units of a larger whole. The Black-White dissimilarity index measures how evenly Black and White students in particular are distributed across schools (or neighborhoods, in the case of residential segregation) within a school district. Ranging from 0 to 1, it can be interpreted as the proportion of students in these groups who would have to change schools (or neighborhoods) to achieve an even distribution of Black and White students within a district.

For measuring Black-White school segregation, the dissimilarity index is calculated as

$$D = \frac{1}{2}\sum_{i=1}^{N}\left|\frac{b_i}{B} - \frac{w_i}{W}\right|,$$

where i indexes schools (out of N total schools in a district), b_i is the number of Black students in school i, B is the total population of Black students in the district, w_i is the number of White students in school i, and W is the total population of White students in the district.

Instrument Construction

Our instrument was constructed as follows. First, we calculated the number of years elapsed since each district's release date for each district in which a child attended in each schooling year. We then calculated the instrument by averaging these years-elapsed values between the first observed schooling year and the year that health outcomes were measured, providing a moving summary of children's segregation experiences throughout their schooling. The number of years elapsed for districts that were never released from court order equals 0. For children who were observed multiple times, the value of the instrument varies across waves.

Validating IV Assumptions

The validity of IV analysis rests on several assumptions: (1) The instrument (average years elapsed from a district's release date) must affect the exposure (school racial segregation), (2) the instrument can only affect the outcomes of interest through exposure to school segregation, and (3) the instrument does not share unmeasured common causes with the outcome. We tested the first assumption by regressing the exposure on the instrument (known as the first stage of IV analysis), to examine whether the exposure (average school segregation) was associated with the instrument. We show the first-stage coefficient and F statistic in Supplemental Table 5 for the sample with self-rated health. Each year elapsed since a district's release was associated with a 0.062-SD increase in the average Black-White dissimilarity index. The F statistic for the first stage is well above the standard cutoff of 10, indicating that our instrument for school segregation is strong. We also visualized the instrument-exposure relationship for the self-rated health outcome in Supplemental Fig 3. The average coefficient of exposure to school segregation increased from 0.40 to 0.63 among children who always lived in districts that were under desegregation orders versus among children who lived in districts that, on average, had been released from court orders for 20 years.

Although we cannot empirically test the second and third assumptions, previous work revealed that the timing of releases appears conditionally random (ie, quasi-random or arbitrary).[13,15,16,27] In particular, the timing of these releases was independent of changes in neighborhood characteristics or other health and social policies because many factors could affect release procedures, including unequal court caseloads across districts, the varying duration of the release process due to complications of judicial proceedings and appeals on decisions, and so forth. Hence, our instrument (the timing of these releases) is unlikely to be systematically related to child health through other channels and only captures exogenous variation in exposure to school segregation. To bolster the case that our IV analysis satisfies these assumptions, we provide additional supportive evidence from 3 falsification checks.

First, to provide evidence that our instrument is quasi-randomly distributed, we examined whether the observed characteristics at baseline were different between those with different levels of exposure. To be more specific, we regressed each observed individual and district characteristic at baseline on our instrument, controlling for other covariates. As shown in Supplemental Table 6, our instrument was not associated with the majority of the observed individual and district characteristics at baseline, the exceptions being birth cohort and districts' residential segregation levels (the association is significant but of a small magnitude). We controlled for these characteristics in our IV models to account for possible confounding.

Second, to support the assumption that the instrument only affected the outcomes of interest through school segregation, rather than through other mediators that affect overall population health (ie, racially discriminatory sentiment within the district related to court order releases), we ran similar analyses on an alternative population that ought not to have been affected by the recent resegregation within schools. If court order decisions only affected the outcomes of interest through school segregation, we should expect null IV estimates for such an untreated group. To do so, we chose a sample of Black people from the main PSID sample who were aged ≥18 years (ie, were likely to have finished high school) in 1991, when the Supreme Court's ruling made it easier for districts to be released from desegregation orders. For each individual, we calculated the average Black-White dissimilarity index between 1991 and the year in which outcomes were measured (exposure) and the average years elapsed from a district's release date between 1991 and the year in which outcomes were measured (instrument). We then conducted IV analysis for several relevant outcomes, including self-rated health, body mass index, stress level (measured by Kessler 6 Psychological Distress Scale), alcohol consumption, binge drinking (≥3 drinks per day), current smoking status, and the number of cigarettes per day. Continuous variables included body mass index, stress, and number of cigarettes per day (coded as 0 for nonsmokers). Binary variables included self-rated health (good, very good, or excellent vs poor or fair health), current smoking status, and binge drinking. Results of this falsification check (Supplemental Table 7) revealed that IV estimates were statistically insignificant for all outcomes of interest, indicating that the instrument is unlikely to affect overall population health through other channels.

Third, to provide evidence that the instrument does not share unmeasured common causes with the outcome, we performed an IV analysis on several placebo outcomes that ought not to have been affected by the court order but would be affected by other confounders (eg, unobserved family characteristics) that might be correlated with the instrument. We chose height and diabetes, as these 2 child outcomes are more likely affected by family-level risk factors (eg, malnutrition in infancy, congenital predisposition to type 1 diabetes) rather than school segregation. The results (Supplemental Table 8) reveal that IV estimates were small in magnitude and were not statistically significantly different from 0 for these 2 outcomes, indicating that it is unlikely that unmeasured causes, such as family characteristics, are simultaneously correlated with the instruments and the outcomes.

Adjustments for Specific Outcomes to Account for Possible Survivorship Bias

The outcomes of ever diagnosed with asthma, obesity, and mental problems and ever drank alcohol, smoked, or smoked regularly are not reversible, meaning that once children get them, these outcomes would not change, even if children experienced different levels of school segregation in later waves (ie, these are survival outcomes). To account for this, we made additional adjustments when estimating the effects of these outcomes.

First, we dropped children who were already diagnosed with the outcome before their school-age years because these children would no longer be at risk (34 children were diagnosed with asthma, and 3 children were diagnosed with mental problems before age 5 years). Second, for children who were diagnosed with the outcome in a given wave, we dropped later observations after the outcome occurred (83 children for asthma, 1 for obesity, 38 for mental problems, 7 for ever drank alcohol, and 4 for ever smoked).

As a robustness check, we used an alternative method. Instead of dropping later observations after the outcome occurred, we collapsed each child's observations into 1 row. Hence, the main dependent variable represented an unconditional probability of being diagnosed with the outcome at some point during one's observed schooling years, and the exposure and instrument represented the average exposure and the average years since the school district was released from the desegregation order across all schooling-age childhood observations. This method had lower statistical power because of the smaller sample size. Nevertheless, the results are consistent with our main results (Supplemental Table 9).

Secondary Analyses Using an Alternative Racial Segregation Measure

We performed sensitivity analyses using an alternative racial segregation measure, the isolation index. The isolation index has been commonly used in previous studies on residential racial segregation and health.[67,68] In this case, the Black isolation index measures the extent to which Black students are enrolled in schools with high proportions of other Black students. It ranges from 0 to 1, with higher values representing higher segregation. Although the Black isolation index and Black-White dissimilarity index are highly correlated, they are conceptually distinct: The Black isolation index is highly dependent on the relative size of Black and White student bodies within districts, whereas the Black-White dissimilarity index is not. For example, if Black students make up a large proportion of a school district, they will have high levels of isolation from White students, regardless of how evenly they are distributed across schools. In contrast, the Black-White dissimilarity index captures changes in racial distribution (i.e., evenness) among schools, rather than changes in district composition.[26]

We found that average years elapsed from the district's release date was also a valid instrument for school racial segregation as measured by the Black isolation index. The F statistics for the first stage of IV models for all outcomes of interest were >10 (results not shown). IV results were similar when using the Black isolation index, although estimates were larger than in the primary models (Supplemental Table 10). A 1-SD increase in school racial segregation increased the probability of ever drinking alcohol (0.80; 95% CI, 0.12% to 1.49%) and drinking at least once a month (0.65; 95% CI, 0.093% to 1.2%), whereas the effect on the BPI was marginally significant (8.14; 95% CI, −0.13% to 16.4%). As in the main results, we found no significant effects on other outcomes.

SUPPLEMENTAL REFERENCES

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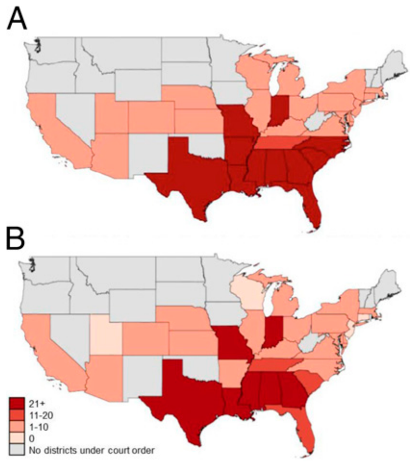
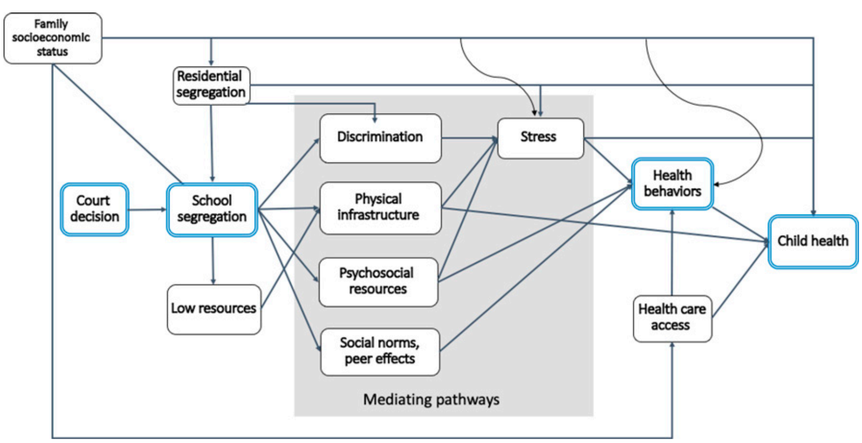


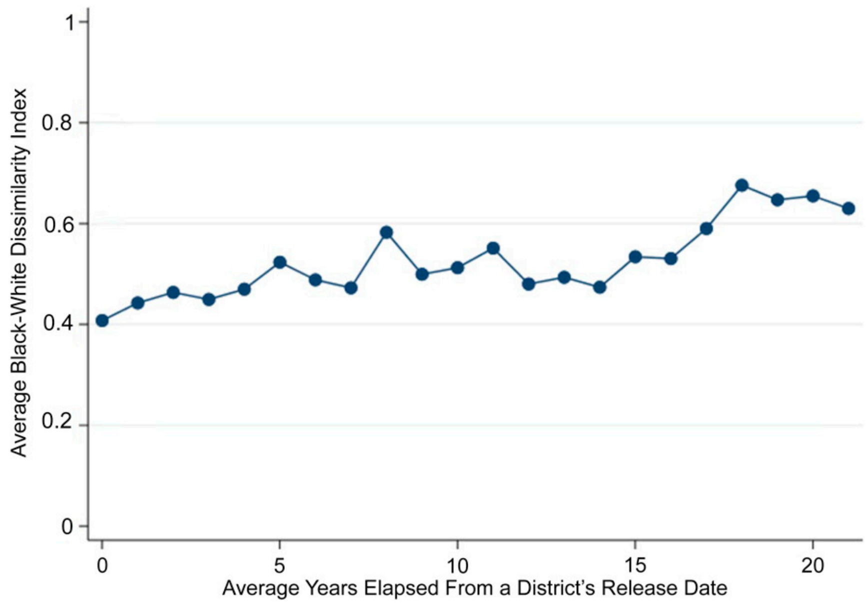
SUPPLEMENTAL FIGURE 1

A, Number of districts under court-ordered desegregation in 1991, by state. B, Number of districts released from court-ordered desegregation in 1992–2013, by state. Map created using data from Qui and Hannah-Jones[24] and Reardon et al.[13]



SUPPLEMENTAL FIGURE 2

Conceptual framework linking school racial segregation and child health and behaviors. Boxes with double borders represent the IV and key exposures and outcomes of interest. Court decisions are not affected by the same confounders as school segregation itself, which is the rationale for the IV approach. In other words, increased school segregation caused by release from desegregation orders provides exogenous variation in school segregation, enabling us to leverage a quasi-experiment.



SUPPLEMENTAL FIGURE 3

Average exposure to school racial segregation by average years elapsed since school district release from court-ordered desegregation. Sample was drawn from the 1997, 2002, 2007, and 2014 waves of the Panel Study of Income Dynamics Child Development Supplement, and includes 1248 Black children who ever resided in school districts that had been under court-ordered desegregation in 1991. Average Black-White dissimilarity index represents average level of school segregation of the school districts children attended between their first observed schooling year (baseline) and the year in which a given health outcome was measured. Average years elapsed from a district's release date was calculated by averaging years elapsed since a district's release year for all districts in which children lived between each child's baseline schooling year and the year in which outcomes were measured.

SUPPLEMENTAL TABLE 4 Details of Child Outcomes in the CDS

	Range	Available Years	Ages Included, y	Original Question
Caregiver-rated health	1–5	1997, 2002, 2007, 2014	0–17	In general, would you say your child's health is excellent, very good, good, fair, or poor?
Self-rated health	1–5	1997, 2002, 2007, 2014	8–17	In general, how is your health? Would you say excellent, very good, good, fair, or poor?
Asthma	Yes/no	1997, 2002, 2007, 2014	0–17	Has your doctor or health professional ever said that your child had asthma?
Obesity	Yes/no	2002, 2007, 2014	0–17	Has your doctor or health professional ever said that your child had obesity?
Mental/emotional problem	Yes/no	1997, 2002, 2007, 2014	0–17	Has your child ever seen a psychiatrist, psychologist, doctor, or counselor about an emotional, mental, or behavioral problem?
BPI	0–27	1997, 2002, 2007, 2014	0–17	The BPI includes 27 questions on antisocial behavior, hyperactivity, and other indications of behavioral problems.
No. of days of PE class at school	0–7; categorical	2002, 2007	10–17	In an average week, on how many days do you go to PE classes at school? Please enter in a number between 0 and 7.
No. of minutes exercising at school	0–4; categorical	2002, 2007	10–17	During an average PE class at school, how many minutes do you spend actually exercising or playing sports?
No. of days of exercise outside school	0–7; categorical	2002, 2007	10–17	Including everything you do outside a PE class, how many days a week do you get at least 30 min of vigorous physical activity?
Alcohol consumption				
Ever	Yes/no	2002, 2007, 2014	12–17	Have you had a drink of beer, wine, or liquor (not just a sip or a taste of someone else's drink) >2 or 3 times in your life?
No. of days	0–7; categorical	2002, 2007, 2014	12–17	During the past 12 mo, on how many days did you drink alcohol?
No. of days at a time	0–7; categorical	2002, 2007, 2014	12–17	Over the past 12 mo, on how many days did you drink ≥5 drinks in a row?

SUPPLEMENTAL TABLE 4 Continued

	Range	Available Years	Ages Included, y	Original Question
Smoking				
Ever	Yes/no	2002, 2007, 2014	12–17	Have you ever tried cigarette smoking, even just 1 or 2 puffs?
Regularly	Yes/no	2002, 2007, 2014	12–17	Have you ever smoked cigarettes regularly, that is, at least 1 cigarette every day for 30 d?
No. of days in past month	0–30	2002, 2007, 2014	12–17	During the past 30 d, on how many days did you smoke cigarettes?

Children from the first 3 CDS waves might have been interviewed in multiple waves. For children from CDS 2014, data were only available for 1 survey wave when this analysis was conducted. In addition, drinking and smoking variables in CDS 2014 were suppressed in the public release file to protect the anonymity of respondents. We gained restricted access to these variables; however, these variables were excluded from the final analyses because of high levels of missingness.

SUPPLEMENTAL TABLE 5 Coefficients and *F* Statistics From First Stage of IV Analysis

	First-Stage Coefficient (95% CI)	*F* Statistic
Instrument (average years elapsed)	0.062** (0.045, 0.079)	50.8

Sample was drawn from the 1997, 2002, 2007, and 2014 waves of the Panel Study of Income Dynamics Child Development Supplement, and includes 1248 Black children who ever resided in school districts that had been under court-ordered desegregation in 1991. Models regressed the exposure (average Black-White dissimilarity index) on the instrument (average years elapsed since the school district was released from court-ordered desegregation). For simplicity, we only show the results for the sample with self-rated health outcome. First-stage coefficients were similar for other outcomes, and all *F* statistics were >10. All models adjusted for baseline school district characteristics, individual characteristics, and fixed effects for birth year and state. SEs were clustered at the individual and district levels.
***P* < .01.

SUPPLEMENTAL TABLE 6 Association of Baseline Characteristics With Instrument

	Coefficient (95% CI)
Individual characteristics	
Age at interview (years)	−0.007 (−0038 to 0.025)
Birth year	0.987** (0.897 to 1.076)
Female sex	−0.004 (−0.012 to 0.005)
Family income at baseline	37.71 (−460.11 to 535.54)
Parents ever married at baseline	−0.005 (−0.011 to 0.001)
School district characteristics (baseline)	
Total students enrolled	−2531 (−5187 to 126)
Black students (%)	0.0003 (−0.001 to 0.0005)
White students (%)	−0.0004 (−0.001 to 0.0005)
Hispanic students (%)	−0.0002 (−0.0009 to 0.0005)
Free or reduced-price lunch (%)	0.001 (−0.001 to 0.004)
Residential segregation	0.01** (0.004 to 0.016)

Sample was drawn from the 1997, 2002, 2007, and 2014 waves of the Panel Study of Income Dynamics Child Development Supplement, and includes 1248 Black children who ever resided in school districts that had been under court-ordered desegregation in 1991. Models regressed each observed individual and district characteristics at baseline on our instrument (average years elapsed since the school district was released from court-ordered desegregation), controlling for all other covariates in the main model. SEs were clustered at the individual and district levels.
***P* < .01.

SUPPLEMENTAL TABLE 7 Falsification Test: Association of School Racial Segregation With Health in Unexposed Sample

	Coefficient (95% CI)
Body mass index	0.23 (−0.99 to 1.45)
Stress	−0.13 (−0.83 to 0.56)
Good health	−0.032 (−0.097 to 0.033)
Smoking	0.024 (−0.054 to 0.10)
No. of cigarettes	0.46 (−0.37 to 1.30)
Alcohol consumption	0.021 (−0.084 to 0.13)
No. of drinks per week	0.04 (−0.12 to 0.20)
Binge drinking	0.006 (−0.049 to 0.061)

Sample was drawn from the 1997, 2002, 2007, and 2014 waves of the Panel Study of Income Dynamics Child Development Supplement, and includes 2511 Black people (15 356 person-year observations) from the main PSID sample who had finished high school (aged ≥18 y) in 1991 and ever lived in districts that had been under court-ordered desegregation in 1991. All models were adjusted for baseline (first-observed) school district characteristics, individual characteristics, and fixed effects for birth year and state. Exposure to school segregation was measured by the Black-White dissimilarity index. The IV was the average years elapsed since the school district was released from court-ordered desegregation. SEs were clustered at the individual and district levels.

SUPPLEMENTAL TABLE 8 Falsification Test: Association of School Racial Segregation With Placebo Outcomes

	Coefficient (95% CI)
Height	−1.64 (−6.07 to 2.78)
Diabetes	0.0050 (−0.011 to 0.021)

Sample was drawn from the 1997, 2002, 2007, and 2014 waves of the Panel Study of Income Dynamics Child Development Supplement, and includes 1248 Black children who ever resided in school districts that had been under court-ordered desegregation in 1991. Models regressed the exposure (average Black-White dissimilarity index) on the instrument (average years elapsed since the school district was released from court-ordered desegregation). All models were adjusted for baseline school district characteristics, individual characteristics, and fixed effects for birth year and state. SEs were clustered at the individual and district levels.

SUPPLEMENTAL TABLE 9 Association of School Segregation With Health Among Black Children, Using an Alternative Method to Account for Possible Survivorship Bias

	Coefficient (95% CI)		
Outcome	OLS	Reduced Form	IV
Asthma	−0.0040 (−0.052 to 0.044)	0.0046 (−0.0032 to 0.013)	0.087 (−0.0059 to 0.23)
Obesity	0.0020 (−0.032 to 0.036)	0.0026 (−0.0033 to 0.0085)	0.044 (−0.062 to 0.15)
Mental/emotional problem	−0.013 (−0.060 to 0.033)	0.0030 (−0.0042 to 0.010)	0.056 (−0.084 to 0.20)
Ever drank alcohol	0.033 (−0.054 to 0.12)	0.034** (0.0022 to 0.066)	0.26** (0.013 to 0.52)
Ever smoked	0.017 (−0.054 to 0.088)	0.0017 (−0.026 to 0.029)	0.013 (−0.20 to 0.23)

Sample was drawn from the 1997, 2002, 2007, and 2014 waves of the Panel Study of Income Dynamics Child Development Supplement, and includes Black children who ever resided in school districts that had been under court-ordered desegregation in 1991. The association was estimated with regressions of health on cumulative average segregation (OLS), regressions of health on the average years elapsed since release from desegregation order (reduced form), or IV estimates of the effects of cumulative average segregation on health with the average years elapsed since the school district was released from the desegregation order as an instrument. All models adjusted for baseline school district characteristics, individual characteristics, and fixed effects for birth year and state. Exposure to school segregation was measured by the Black-White dissimilarity index. SEs were clustered at the individual and district levels.

**$P < .01$.

SUPPLEMENTAL TABLE 10 Association of School Segregation With Health Among Black Children (Sensitivity Analysis Using Black Isolation Index)

Outcome	Coefficient (95% CI) OLS	Coefficient (95% CI) IV
Poor, fair, good health	−0.0089 (−0.083 to 0.065)	0.22 (−0.16 to 0.59)
Asthma	−0.023 (−0.063 to 0.017)	0.19 (−0.033 to 0.41)
Obesity	0.0063 (−0.021 to 0.034)	0.11 (−0.074 to 0.29)
Mental/emotional problem	−0.0096 (−0.048 to 0.029)	0.16 (−0.10 to 0.43)
BPI	−0.40 (−1.50 to 0.70)	8.14 (−0.13 to 16.4)
PE class >3 d/wk	−0.09 (−0.22 to 0.038)	−0.023 (−0.39 to 0.34)
Vigorous activities outside PE class >3 d/wk	0.02 (−0.10 to 0.14)	0.31 (−0.23 to 0.85)
Alcohol consumption		
Ever	−0.0067 (−0.090 to 0.077)	0.49 (−0.012 to 1.00)
At least monthly	−0.0016 (−0.12 to 0.11)	0.65* (0.093 to 1.20)
Had ≥5 drinks in a row at least monthly	−0.013 (−0.098 to 0.072)	0.21 (−0.092 to 0.52)
Smoking		
Ever	0.024 (−0.048 to 0.096)	0.077 (−0.27 to 0.43)
Regularly	0.015 (−0.048 to 0.079)	0.11 (−0.17 to 0.39)
No. of days	−0.96 (−1.99 to 0.068)	0.57 (−3.18 to 4.32)

Sample was drawn from the 1997, 2002, 2007, and 2014 waves of the Panel Study of Income Dynamics Child Development Supplement, and includes Black children who ever resided in districts that had been under the desegregation order in 1991. All models were adjusted for baseline school district characteristics, individual characteristics, and fixed effects for birth year and state. Exposure to school segregation was measured by the Black isolation index. The IV was the average years elapsed since the school district was released from court-ordered desegregation. SEs were clustered at the individual and district levels.

* $P < .05$.