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ABSTRACT

Reducing Racial Inequality in Access to the Ballot Reduces Racial Inequality in Children's Later-Life Outcomes

The Voting Rights Act (VRA) of 1965 removed barriers to voting for Black Americans in the South; existing work documents that this in turn led to shifts in the distribution of public funding towards areas with a higher share of Black residents and also reduced Black-White earnings disparities. We consider how expanded access to the ballot improved the well-being of children, and in doing so document that the immediate effects of expanded voting access last well into the next generation. Specifically, within a cohort-based differences-in-differences design, we test how early-life exposure to the VRA differentially impacted later-life outcomes of Black Americans. We find that increased exposure to the VRA before the age 18 leads to higher educational attainment and earnings in adulthood for Black Americans, with little or no impact on whites.

JEL Classification: J15, N12, D72

Keywords: Voting Rights Act, racial inequality, voting, childhood exposure

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1. Introduction

Amongst the monumental policy shifts that occurred as part of the Civil Rights Movement, the Voting Rights Act (VRA) of 1965 stands as one of the most important. The Voting Rights Act forced Southern states to eliminate discriminatory aspects of the voting and registration process (e.g., the literacy test) and also introduced Federal oversight of jurisdictions with a history of racial discrimination in elections. These "covered" jurisdictions, largely in the South, were required to receive preclearance from the Federal Department of Justice before altering their local election practices in any subsequent elections.

Existing empirical evidence documents a clear impact of the VRA on African Americans' ability to participate in the political process (Husted and Kenny 1997; Cascio and Washington 2014). But, in expanding the franchise to an otherwise underrepresented group, the Act may plausibly have had impacts on racial disparities in a wide array of areas beyond voting rights. Martin Luther King, Jr., for instance, described voting as "Civil Right No. 1", because with the vote, African Americans could "vote out of office public officials who bar the doorway to decent housing, public safety, jobs, and decent integrated education."

With that in mind, some research has considered the impacts of the VRA on the broader welfare of impacted citizens. Existing evidence documents that, in addition to providing access to the ballot, the VRA altered state spending patterns. States impacted by the VRA started to direct a larger share of transfers to areas with a higher share of Black residents, and – perhaps as a result – education spending increased (Cascio and Washington 2014); likewise, states' spending on redistribution increased (Husted and Kenny 1997). Two very recent papers document more directly that the expansion of the franchise differentially and immediately impacted the welfare of Black Americans. Aneja and Avenancio-Leon (2019) find that areas covered by the VRA experienced declines in Black-White earnings disparities, perhaps largely owing to increased public employment; Facchini, Knight, and Testa (2020) document that Black-White disparities in arrests decline in southern states with the passage of the VRA.

Our paper builds on the recent research documenting direct effects of the VRA on closing disparities in areas of life outside of voting. We assess the impacts of *childhood* (ages 0-18) exposure to the Voting Rights Act on later-life outcomes, and in doing so consider the possibility that the VRA not only had immediate effects on newly enfranchised voters, but also generated a host of benefits for their children that lasted well into the next generation. Motivating our focus on children's exposure to the VRA is a growing body of evidence on the importance of early-life neighborhood and schooling environments, as well as family socioeconomic status, in determining later-life outcomes (e.g., Jackson, Johnson, and Persico 2016; Chetty et al. 2020; Chetty, Hendren, and Katz 2016; Currie et al. 2014). To the extent that the VRA created changes on any of these fronts, we may expect large impacts of the VRA on individuals who were children when it was passed. Our paper therefore aims to speak to the political forces that may drive improvements in childhood environment and in turn close disparities in later-life outcomes.

More specifically, we compare Black-White differences in educational attainment, labor market outcomes, and other outcomes in adulthood for children who grew up in a location and time period where the VRA was active to those who did not. We draw on restricted-use data from the Panel Study of Income Dynamics (PSID) as well as data from the 1990 and 2000 5% public-use microdata samples of the Decennial Census and estimate cohort-based difference-in-differences models. We find that Black children who were more exposed to the VRA went on to obtain more education and experience higher earnings relative to those who were less exposed and also relative to white children with equal exposure. An additional year of VRA coverage yields 0.04-0.05 more years of education for Black relative to White children who are similarly exposed. As such, a consequence of any VRA exposure is to close existing Black-White disparities in educational attainment. A parallel finding exists in the domain of labor market outcomes, with Black children's differential future hourly wages increasing by 1.4% for every year of VRA exposure.

How might these results occur? Two broad (non-mutually exclusive) mechanisms seem particularly plausible. The first is a *distribution in funding* channel; the increased political power of Black voters induced by the Voting Rights Act led elected officials to transfer more funds to areas with a larger share of Black residents, especially for schooling (Cascio & Washington, 2014). Thus, one explanation is that increased localized investment in public goods in areas with a large share of Black residents may have improved the environment for children growing up in those areas. A second possible channel stems from *improvements within the household*. As noted, recent work documents that the VRA causally reduced Black-White earnings disparities (Aneja & Avenancio-Leon, 2019) and disparities in arrests of adults (Facchini et al., 2020). Thus, rather than government investment in the area that a large share Black youths reside, improvements in early-life environment may have been driven by improvements in socioeconomic conditions of a child's family, which in turn translates into better later-life outcomes (as in the literature on intergenerational transmission of education and earnings (Chetty et al. 2014)). Our data do not offer the opportunity to definitively identify a mechanism, but rather we take these potential mechanisms as motivation to guide some of our analysis reported below.

Our paper makes several contributions to the literature. First, its focus on *individual*-level rather than commonly used aggregate outcomes, for instance at the county level, provides estimates of the consequences of Black enfranchisement for those directly affected. Research documenting increased funding allocation for counties with more Black residents informs our work, but the aggregated outcomes data disguises the distribution of spending across schools and more importantly, across Black and white individuals. This is a relevant concern given work on other reforms during the same era; Cascio, Gordon, and Reber (2013) document that Federal Title I grants to school districts in the 1960's did not always generate increased spending, and – when they did – only led to academic gains for white students, with districts potentially targeting any increased spending towards those students. We shift the focal unit towards individuals and relate exposure to voting rights legislation directly to individuals and examine whether Black-white inequality decreased in later-life outcomes.

Second, while recent studies have begun to document the VRA's impact on individuals, they focus on the effect of exposure during *adulthood* on *short-term* outcomes. For example, Aneja & Avenancio-Leon (2019) find that the VRA reduced the Black-White earnings gap in counties and states covered by the preclearance provision. Facchini et al. (2020) focus on the consequences for policing and show that Black arrest rates declined after the passage of the VRA in covered counties. Our research build on these prior studies by examining the long-run consequences as measured by educational, labor market, and health outcomes experienced by children exposed to the VRA.

Third, we contribute to a broader literature on differential access to the political process and the well-being of children. For instance, Kose, Kuka, and Shenhav (2020) find that the women's suffrage in the United States led to increased spending on education (especially in the South, where education spending was lower). Later in life, children who grew up under women's suffrage (vs. before) ultimately accumulated more years of education, and some children (notably, not Southern Black youths) received higher later-life earnings. Carruthers and Wanamaker (2015) focus specifically on the South and find that women's suffrage in the South – which implied white women's suffrage, as Black voters were disenfranchised at the time – increased school spending, but only in White schools. Naidu (2012) shows that disenfranchisement of Black voters in the post-Reconstruction period through the introduction of poll taxes and literacy tests (the latter of which was ultimately only repealed in Southern states through the Voting Rights Act) significantly reduced investment in Black, but not white, schools, reducing the teacher-pupil ratio by 10-23%. Focusing on political representation in elected office rather than at the ballot box, Logan (2020) studies the Reconstruction-Era South and documents, among other effects, that the presence of Black local elected officials is associated with significantly higher Black literacy rates of children aged 10 and 15. Collectively, these papers support the notion that shifts in political representation and/or access to the political process that can have large impacts on children's immediate and longer-run outcomes.

2. Data

Our analysis sample is based on restricted individual-level data from a panel survey supplemented with county-level characteristics from a wide range of sources. The main data comes from the nationally representative Panel Study of Income Dynamics (PSID), which follows a set of households and associated individuals from 1968 until 2015. Our analysis sample tracks 25 birth cohorts of individuals, born between 1945 and 1969, from childhood to adulthood. These birth cohorts span various degrees of childhood exposure to the implementation of the VRA.

We examine the effect of childhood exposure to the VRA, so it is necessary to match individuals' early residential locations to local VRA implementation. Restricted PSID data enables the linking of individuals to their residential counties during birth and childhood.[1] Recall that a subset of jurisdictions (counties and states) in the South were "covered" by the VRA in the sense that any changes in election practices required Federal approval. We use the 1965 VRA coverage status of these residential counties for the remainder of the analyses. Note that we consider the impact of this legislation to include both the concurrent interventions of preclearance, a system that prohibits jurisdictions from implementing any new electoral procedure without first obtaining federal approval, and the elimination of literacy tests. We rely on the U.S. Department of Justice's Civil Rights Division to identify jurisdictions brought under this additional federal oversight, which include the entire states of Alabama, Georgia, Louisiana, Mississippi, South Carolina and Virginia, as well as select counties in North Carolina. All seven states had literacy tests in place until the passage of the VRA.[2]

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[1] We rely on restricted data on the counties where individuals were born or grew up, as well as exact location of children's parents at birth and early childhood to pinpoint the residential locations of individuals. We code childhood county as the county of birth for individuals with restricted geospatial data and the county of parents' residence at birth for those missing the county of birth variable. For remaining individuals, we code childhood counties as counties of residence at age 1, 5, and 10, as well as the county the child grew up in as reported in restricted geospatial files. We also include a more restrictive childhood county variable that removes observations where there is only data for children's residential counties at age 10. Note that doing so removes no more than 3% of the sample.

[2] Preclearance coverage also extended to select counties in Arizona and the state of Hawaii.

We limit the analysis to white and black individuals to examine the differential effects among white and Black children of early-life exposure to the VRA. Covariates in the data span individual demographics (gender, marital status) and family background characteristics (respondent's father's level of education). County-level characteristics include population and share of black residents in 1960 from the County and City Data Book and the share of county-level votes cast for Strom Thurmond in the 1948 presidential election as a proxy for segregationist preferences. We furthermore account for early-life county-level characteristics in acknowledgement of the various civil rights and social policies being implemented during the study period (often interacted with race, as we also include county fixed effects). These include individual exposure during the ages of 5-17 to the timing of school desegregation, an indicator for whether the individual was born after hospital desegregation in the county, and county-level Title I grant entitlement.

Since exposure to the VRA during childhood may not materialize in changes in educational investments, productivity, and well-being until later in life, the availability of long-run outcomes is key for drawing conclusions on lasting effects. Our main outcomes include educational attainment, labor market measures, migration, and health. Specifically, we include measures for years of completed education (the highest observed for an individual in the panel), whether an individual attained a certain number of years of education, hourly wages, annual earnings, employment status, whether the individual continues to live in their childhood county or the South, and self-reported general health status as an adult. The labor market outcomes are averaged across ages 25-35. Monetary outcomes are measured in 2011 dollars. These means of these outcomes for Black and white respondents are reported in Appendix Table 1. We furthermore examine the outcome of parents' economic well-being during childhood as a potential channel of influence, under the assumption that increased enfranchisement through VRA exposure can increase parental income that in turn affects children's long-term outcomes.

The final sample includes 10,972 individuals. An advantage of the PSID is the granularity of respondents' childhood geospatial location and the possibility of tracking them through to outcomes during adulthood. A limitation is the sample size of the longitudinal survey. We supplement the PSID with the 1990 and 2000 5% public-use microdata samples of the Decennial Census to provide a significantly larger sample and test the robustness of our results in a different dataset. OF course, a limitation of Census data is that we cannot link individuals to the county that they grew up; instead, we observe the state of birth and code individuals as VRA-exposed if they were born in a test that had a literacy test prior to 1965.

3. Empirical Approach

We study the impacts of the VRA on long-run individual outcomes using variation in the timing and intensity of exposure to the legislation among children born into different birth cohorts and across counties. We estimate cohort-based difference-in-difference models of the following form:

$$Y_{ict} = \alpha + \beta_1 VRA\ Cov._c \times Years\ Exposure_t \times Black_i + \beta_2 VRA\ Cov._c \times Years\ Exposure_t + \mathbf{x}'_{ict}\Pi + \gamma_c + \delta_{r(i)t} + \varepsilon_{ict}$$

where Y_{ict} represents an adult outcome for an individual *i* who grew up in county *c* and was born in year *t* (drawing on outcomes described in the previous section). $VRA\ Cov._c$ signifies that the individual grew up in a county that was covered by the preclearance provision of the VRA. Given the overlap in geographic coverage of preclearance oversight and bans of literacy tests in covered counties, we interpret this variable as the combined treatment across VRA provisions, though we include a robustness check replacing the "VRA Cov." dummy with an indicator for having had a literacy test in the state prior to 1965 and observe similar results. $Years\ Exposure_t$ quantifies the dosage of treatment and is expressed on a scale of 0 to 18, with 18 denoting full exposure from birth through the end of secondary schooling. Specifically, we calculate this variable as the number of years of the individual's childhood from ages 0-18 that occurred after VRA's passage. For example, a child born in 1960 would have received 13 years of VRA exposure if he or she grew up in a VRA covered county. We furthermore include all remaining two-way interactions, a vector of individual and county-level covariates, and separate county and race-birth year fixed effects. Individual covariates embedded in $\mathbf{x}_{ict}$ include race by gender fixed effects and educational attainment for the father of the respondent, while county-level characteristics include population and share of population who were black in 1960 alongside the timing of rollout for policy programs, all interacted with race. County fixed effects absorb time-invariant county-specific characteristics that matter for educational and employment access and other facets of individual well-being. Race-indexed year-of-birth fixed effects ($\delta_{r(i)t}$) account for shocks over time that may differentially impact Black and white individuals.

Our main coefficient of interest is β_1 on the triple interaction term. It identifies the *differential* effect on Black children relative to White children of having spent one additional year of one's childhood in a county that was covered by the VRA during a time period when the VRA was active;

or, put differently, it captures the impact of VRA coverage relative to equally exposed white respondents. β_2 captures the impact of one additional year of VRA exposure on later-life outcomes of white children. Finally, $\beta_1 + \beta_2$ captures the overall (rather than differential) effect of one additional year of coverage for black children. Thus, β_1 is useful for assessing whether the VRA specifically generated gains for Black children and considering the impacts of the VRA on closing later-life disparities in education and earnings, while $\beta_1 + \beta_2$ is useful for assessing differences in outcomes across Black children covered vs. not covered by the VRA.

To interpret our empirical evidence as causal, we rest on the following identifying assumption: in the absence of treatment, outcomes in covered and non-covered counties would have evolved the same way over time for individuals who were and were not eventually exposed to the VRA. We undertake additional analyses to examine whether trends in the years leading up to and following the VRA are similar in counties that are or are not affected by voting rights legislation. The first check creates a placebo VRA treatment taking place in 1943, thereby shifting our sample period to over two decades *prior* to the actual year of VRA passage. In the absence of differential pre-trends across these counties, we would expect to find null results of this placebo treatment on the long-term education and labor market outcomes of both Black and white children. In a similar vein, we examine whether there are persistent differences in post-VRA trends among cohorts that are fully exposed from birth through secondary schooling by constructing a placebo VRA treatment in 1985.

Another necessary condition for identification is to show that there are no policies or programs whose rollout coincided with the implementation of the VRA in select jurisdictions. We also consider the set of "War on Poverty" policy programs and how they may have differentially affected Black vs. white individuals in a given county. Controls include individual exposure to the school desegregation as defined by the timing of court-ordered desegregation litigation cases, presence of whether the individual was born after hospitals desegregated in the county, and county-level Title I grant entitlement, all interacted with race.

To ensure that our findings using the above specification are generalizable beyond the PSID context, we supplement our analyses using 5% microdata from the 1990 and 2000 Decennial Census. We estimate a difference-in-differences specification that replaces the geographic treatment variation from county-level preclearance coverage under the VRA to a state-level indicator for ever imposing a literacy test. The model takes the following form:

$$Y_{ist} = \sigma + \rho_1 LitTest_s \times Years\ Exposure_t \times Black_i + \rho_2 LitTest_s \times Years\ Exposure_t + \mathbf{x}'_{ist}\Omega + \theta_s + \pi_{r(i)t} + \epsilon_{ist}$$

Y_{ist} represents the adult outcome for an individual *i* born in year *t* who grew up in state *s*. We include a more parsimonious set of individual control variables due to data limitations in the Census, alongside state fixed effects and race-birth year fixed effects as before. A causal interpretation on the coefficient of interest is based on the assumption of outcomes evolving similarly over time for individuals who were and were not eventually exposed to the VRA in states with and without literacy tests if the VRA were not in fact implemented.

4. Results

We now proceed to the results of estimating the equation described in the previous section using the PSID data, taking on a variety of outcomes, but focusing in particular on education and labor market outcomes.

Table 1 reports our main results for education-related outcomes. Panel A reports results with a very limited set of controls; Panel B is our preferred set of specifications, with a more complete set of controls. Panel A includes year-of-birth-by-race fixed effects, early-life county fixed effects, race-by-gender fixed effects, in addition to the full interaction of race, early-life VRA exposure, and a dummy indicating early-life residence in a VRA covered county. Panel B includes all of those controls but adds the additional covariates described in the previous section (race-by-father's education fixed effects, school desegregation exposure-by-race, etc.). Recall that our primary coefficient of interest is "VRA Cov. X Years Exp. X Black", which identifies how an additional year of early life exposure to the Voting Rights Act differentially impacted Black youths' later life outcomes.

In Column 1, we take as an outcome the highest number of years of education an individual has received by adulthood. Across both panels, we find a clear positive differential impact of the VRA on total years of education attained by adulthood for Black children. One additional year of VRA exposure is associated with an additional 0.04-0.05 years of education. Because white children are essentially unaffected, this reflects both the differential magnitude and the overall effect on Black children. In other words, Black children with full exposure to the VRA (all 18 years of childhood) completed an additional 0.79-0.88 years of education relative to those with no exposure.[3] It is worth noting that in our sample, the average years of education for Black respondents is 12.84 and the

[3] The 0.79 and 0.88 figures come from simply multiplying the coefficients by 18. This of course takes the assumption of linearity in the impact of years of exposure quite seriously. In analyses discussed below, we adopt a more non-parametric approach to estimating how effects differ across cohorts more or less exposed to the VRA.

average for White respondents is 13.58. Thus, the magnitude of the effect on fully vs. non-exposed youths is large enough to have closed the gap.

Columns 2-5 take dummies for distinct levels of education to assess where in the respondents' educational career the observed increase occurs. For example, the outcome in Column 2 is an indicator equal to one if respondents report at least 12 years of education (i.e.., high school completion). Remaining columns capture strictly greater than 12 years of education (some college), at least 14 years (at least 2 years of college), and at least 16 years (at least 4 years of college). Across both panels, we observe significant increases at every level with the exception of "at least 16 years" in Column 5. The magnitudes of the estimated effects are worth noting. Panel B reports an 0.8 percentage point differential increase in the likelihood of at least completing high school per year of exposure (Column 2); Columns 3 and 4 report a 1 percentage point differential increase in the likelihood of going on to any or at least two years of college. But note that the baseline averages are quite different across these columns, with 89% of respondents reporting at least 12 years of education, 49% reporting more than 12 years, and 41% reporting at least 14 years. Thus, in both raw magnitudes but also in percentage terms, our estimated effects our are larger for the shift to completing at least some college; one additional year of exposure increases the likelihood of high school completion by roughly 1%, but increases the likelihood of completing some college by 2 to 2.4%. Finally, note that across all columns, there is no effect on white youths (from the coefficient "VRA Cov. X Years Exp."); in this case, improved later-life outcomes for Black youths do not come at the expense of other children.

Next, we consider the effects of early-life VRA exposure on labor market outcomes later in life, with results reported in Table 2. Again, Panel A includes minimal controls, Panel B includes a more complete set of controls. All outcomes are averaged across an individual's observations between the ages of 25 and 35. Columns 1 and 2 take as an outcome the respondent's wage, conditional on reporting a positive wage (logged in Column 2), Columns 3 and 4 take the respondent's reported annual earnings (logged in Column 4), and Column 5 measures the fraction of years between ages 25 an 35 that an individual reports being employed, conditional on reporting being in the labor force.

Results are relatively similar across both panels, so we focus on our preferred specifications in Panel B. We find that one additional year of early-life VRA exposure differentially increases Black respondents' wages by \$0.24 (or 1.4%) and increases annual earnings by \$352.13 relative to White youths with similar exposure to the VRA; that is, early-life VRA exposure served to reduce later-life Black-White earnings disparities. We observe no effect on employment status. Again, to reframe these magnitudes and focus on overall effects $(\beta_1 + \beta_2)$ for Black youths rather than differential effects

(β_1), the estimates imply that a fully covered (18 years of exposure) Black youth's wage is $2.30 higher than a non-covered (0 years) Black youth later in life (relative to a $16.56 sample average)[4]; likewise, annual earnings are $2,041 higher (relative to a $29,819 sample average)[5]. Unlike the education results, we do observe some evidence of a negative effect on white respondents' wage outcome (significant at the 10% level in Panel B, Column 1) and income (not significant but negative in Panel B, Column 3). We note that this is not inconsistent with findings from Aneja & Avenancio-Leon (2020) who find that the VRA had an immediate positive effect on Black adults' earnings and a negative effect on White adults' earnings; however, we hesitate to put too much stock into the effect on White earnings given the marginal significance, paired with the lack of corroborating evidence from analysis in Census data which we report and discuss below.

Finally, we note that a plausible channel through which earnings may have increased is through the effect of VRA exposure on education, documented in Table 1, as we know that more years of education is associated with higher earnings. We can make an attempt to speak to how much of our earnings effect is explained by our education effect in two ways. First, we have estimated a simpler specification taking "wage" as the outcome, but dropping all VRA-related variables and instead inserting "total years of education" on the righthand side. In doing so, we can report that, in our sample, one additional year of education is associated with an increase in wage of $1.31. Since we found that one additional year of childhood VRA exposure leads to 0.05 additional years of education for Black respondents, a simple calculation suggests that – if increased education were the *only* channel through which the VRA increased later life earnings – earnings would increase by $0.065 ($1.31*0.05) for each year of VRA exposure. That accounts for roughly 50% of our estimated overall effect of the VRA on Black respondent earnings. Second, we can of course estimate our wage model and include total years of education as a control (interacted with race to account for potential differences across race groups in labor market returns to education (Card and Krueger 1992)); this is problematic because we have shown that years of education is impacted by VRA exposure, so we emphasize that this finding is suggestive at best, but we note that in doing so, we continue to observe a positive significant impact on earnings of roughly $0.08 per year of exposure. Under that approach, controlling for

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[4] Calculated as the linear combination of $\beta_1 + \beta_2$, which, here is -0.108+0.236=0.127 (the combination of which is significant with a p-value of 0.011), multiplied by 18.

[5] See previous footnote for calculation. Note that while β_2 in column 3 is significant at the 10% level, $\beta_1 + \beta_2$ is not statistically significant.

education explains only about 37% of our original estimate on earnings. Taken together, we think that VRA-driven increases in education play an important, but partial, role in increases in earnings.

The prior results, for the sake of tractability and ease of discussion, make the strong assumption of linearity in the impact of years of exposure on our outcomes. To relax that assumption and also to begin to provide a sense of the mechanisms driving our results, we also estimate a model which replaces the "years of exposure" variable with a set of dummies capturing relevant cohort groupings. Specifically, we create four dummies: one for cohorts born in 1965 or later, who (if in a covered county) were exposed to the VRA from birth; one for cohorts born from 1960-1964, for whom all of their *school-age* years took place under the VRA; one for cohorts born 1952-1959, for whom the VRA became active while they were in relatively early grades (K-8); and finally one for cohorts born from 1948-1951, for whom the VRA became active while they were in high school. These are constructed to be mutually exclusive categories, though note that students in 1960-1964 birth cohorts were not only covered prior to entering school, but were of course also covered through K-8 and high school.

One broad channel through which the VRA can have lasting impacts is by improving the early-life environments of children. In pointing to this broad category, we have in mind research documenting the importance of wellbeing and environment in very early childhood (e.g., Heckman 2006; Hoynes, Schanzenbach, and Almond 2016). In this case, we might expect the largest results for youths who were covered by the VRA in the earliest years of their lives (the 1960-1964 cohorts and the post-1965 cohorts); effects for cohorts covered during school-age, but not in pre-school years, may be smaller.

With the above in mind, we report results from two regressions in Figure 1. We take as outcomes "total years of education" (as in Table 1, Column 1) and "average wage from age 25-35" (as in Table 2, Column 1). The models we estimate include the same sets of controls as in Panel B of Tables 1 and 2, respectively. The only difference between the models estimated to produce Figure 1 and the models reported in Tables 1 and 2 is, again, that we replace the "years of exposure" variable with the set of cohort-grouping dummies outlined above. The figure depicts the coefficients "VRA cov. X [cohort group] X Black" for education (in Panel A) and earnings (in Panel B). Both panels reveal essentially no effect on education or earnings for the oldest cohort, those who – if in a VRA county – were only exposed during high school. This makes sense with respect to both mechanisms discussed above, as they would not have benefitted from any very early-life exposure and, at most, would have benefitted from four years of school-age exposure at a late stage in their K-12 career.

The pattern of the remaining coefficients differs across the two outcomes. For education (Panel A), we observe significant positive effects for all three remaining cohort groups. Notably, that includes the cohorts born between 1952-1959 with no pre-school age exposure to the VRA, but who would have experienced any benefits resulting from within-school effects for the majority of their K-12 careers. Specifically, we observe a differential effect of 0.86 additional years of education for those cohorts, 1.11 additional years of education for the '60-'64 cohorts, and .2 additional years of education for the post-'65 cohorts. None of these estimates are statistically different from each other.

In the case of earnings, the pattern of coefficients suggests that it is pre-school-age exposure (or the cumulative effects of pre-school-age and school-age exposure) that matters most. We estimate that there is essentially no effect of VRA exposure on earnings for cohorts that are only exposed by the time they have already entered school. On the other hand, there are strong positive differential effects for the cohorts with at least some pre-school-age exposure. The '60-'64 cohorts and post-'65 cohorts experience wages positive differential effects of \$3.77 and \$3.82. The shift that occurs wherein pre-school-age exposure generates effects but (exclusively) during-school-age exposure does not is statistically significant; the coefficient on the "K-8" group and the coefficient on the "prior to school" group are statistically different with a p-value < 0.01. The fact that there is a discrete shift in the part of a child's life under which they should be covered to be impacted by the VRA when wage is the outcome could explain why some of our earnings findings from the linear-in-exposure years was less statistically precise than for the education outcome. Finally, we also note that in these specifications, we do not observe a negative effect of VRA exposure on White respondents for any of the cohort groups (coefficients not reported). That may have been an artifact of imposing linearity in Table 2, Column 1.

In summary, we find that VRA exposure has a positive impact on total education both for Black children exposed very early in life and those exposed once they have entered school. On the other hand, we only observe positive results on wages for children exposed from prior to entering school. While suggestive, we think that this points to within-school impacts of the VRA (driven by, for instance, more equitable school funding) on education outcomes, and very early-life impacts of the VRA on later-life earnings (e.g., improved health and/or socioeconomic environment in first five years of life). This conclusion is further supported by the fact that our estimate magnitudes suggest that the increase in education cannot fully explain the increase earnings, suggesting a non-education channel through which earnings are impacted.

4.1 Robustness and Validity of Estimates

Next, we turn to several tests to probe the robustness and validity of our findings. Some such tests are reported in Table 3, Panels A and B, which vary our main specification in several ways for our two main outcomes: total years of education (Panel A) and wages (Panel B). In Column 1 (related to some of the immediately preceding discussion), we replace the "years of exposure" variable with a variable capturing years of *school-age* exposure, defined in roughly the same way as our main "years of exposure" variable but capturing exposure between the ages of 5 and 18. The variable therefore runs from 0 to 13 (rather than 18). The basic direction of estimates is consistent with our main findings, but it is worth commenting on the magnitudes. We find that full school-age exposure is associated with one additional year of education (0.077*13) and $3.39 additional dollars in average wage from age 25-35 ($0.261*13). Notably, the school-age education estimate is larger than our full-exposure estimate from the previous section, while the earnings estimate is smaller. This aligns with the findings from the previous section suggesting that school-age exposure matters more for later-life accumulated education and full-childhood exposure matters more for later-life earnings.

In Column 2, we show that results are robust to replacing the county-level VRA coverage dummy with a dummy indicating that a child's early-life state had a literacy test prior to 1965; the literacy test is the geographic identifier for treatment used in Cascio & Washington (2014), and also in some additional analysis we conduct, discussed below. Column 3 uses a more restrictive early-life county identifier than our main analysis. Whereas the county identifier used in the main analysis classifies early-life counties as primarily the county of birth or parents' residence at birth, or the county that the individual grew up in during the ages of 1, 5, or 10, the more restrictive identifier excludes the very small share of individuals for whom childhood counties are only observed at age 10. Results are essentially unchanged.

Finally, in Column 4, we restrict the sample to children who spent their early life in states in the South, Border states[6], or DC, rather than including children who spent their early life in any county throughout the country. This is important in accounting for differential trends in outcomes across the broad Southern region of the country which coincide with the timing of the Voting Rights Act, especially those induced by other aspects of Civil Rights legislation and the Civil Right movement. Once restricted to children who grew up in these states (or DC), our identification hinges on whether outcomes differ across children who grew up specifically in a county covered by the VRA

[6] Southern states include states in the former Confederacy (AL, AR, FL, GA, MS, NC, SC, TN, TX). Border states include bordering non-former Confederacy states (DE, KY, MD, MO, WV).

relative to children who grew up in other parts of the (broadly defined) South. Column 4 shows that, with this restriction, results are relatively similar to our main findings, albeit with some loss of precision due to a large loss in the number of observations.

Related to the previously noted concern around trends occurring in the South in the years leading up to 1965 and the years following, we also conduct two placebo exercises. The first shifts the 25-year birth cohort window to focus only on individuals born early enough that they have *no* exposure to the VRA, specifically focusing analysis on cohorts born between 1923 and 1947. Individuals born in 1947 turn 18 in 1965. This therefore shifts our sample window 22 years earlier than the 1945-1969 birth cohorts used in the main analysis. We therefore define 1943 as the placebo year of VRA introduction (22 years prior to 1965) and define a placebo "years of exposure" variable around 1943 in the same way that we defined the true years of exposure variable around 1965 in the main analysis. That helps assess whether differential trends in outcomes in soon-to-be VRA-covered counties for black respondents leading up to the passage of the VRA can explain our main results. Results are reported in Table 4, Panels A and B, with Panel A replicating our results on education from Table 1 and Panel B replicating our results on labor market outcomes from Table 2. All specifications include the full set of controls included in those initial analyses. None of the coefficients on "VRA Cov. X (placebo) Years of Exp. X Black" are significant, and many are much closer to zero than our main estimates or even negative. Our second placebo test follows the same idea, but for cohorts born entirely after 1965, all of whom were exposed to the VRA (if in a VRA-covered county). Barring a gradually growing impact of the VRA on children or more general trends impacting children in those counties unrelated to the VRA, we should therefore not observe a difference in outcomes across treatment-county cohorts born in earlier years vs. later years relative to differences in non-treated county children. Thus, we adopt as our sample children born between 1965 and 1989 (again, taking 25 birth cohorts as in the main analysis); that window is 20 years later than our main analysis, so we correspondingly define the placebo VRA treatment year as 1985. Results are reported in Panels C and D, and again none of the "VRA Cov. X (placebo) Years of Exp. X Black" are significant and positive (one is significant and negative). These results help support our claim that our main analysis is identifying a causal effect of exposure to the VRA and not simply broader trends occurring in the region.

Next, in Appendix Table 2, we conduct parallel analysis to our PSID analysis in the public-use 5% samples of Decennial Census data from 1990 and 2000. The public-use Decennial Census data has some advantages and some disadvantages relative to the PSID data. The key disadvantage is the inability to identify with precision where an individual spent his/her childhood, which is necessary for assigning treatment based on the VRA coverage status of an individual's childhood county of residence. We also take advantage of childhood county to incorporate a rich set of fixed effects and controls for childhood environment in the PSID analysis, which is not possible in Census data. Instead, in Census data, we can observe only an individual's *state* of birth. While not as rich as childhood county, that allows us to at least identify whether a child grew up in a state that had a literacy test prior to 1965, under the (imperfect) assumption that state of birth reflects the state where the individual spent his/her childhood. Thus, cohort-based difference-in-differences models taking "Lit. Test" as the geographic treatment identifier, similar to Column 2 of Table 3 are possible. The key advantage of the Census is a much larger sample than the PSID and, more specific to our purposes, the ability to test that our results are robust to examining similar models in a different dataset with different methods of sampling respondents.

In short, in estimating models regressing education and earnings outcomes on the full interaction of "born in lit. test state", Black, and years of exposure (based on birth year as in our main analysis) and the minimal set of controls available[7], we find results that are very similar to our estimates from the PSID, albeit with smaller magnitudes. For example, in the Census data, we estimate that a fully exposed (vs. non-exposed) Black individual is 5 percentage points more likely to report completing high school (significant at the 1% level), compared to 10 percentage points in the PSID. Likewise, average total annual earnings between ages 25-35 are 10% higher for fully exposed Black Census respondents (significant at the 1% level), compared to 18% higher in the PSID. As alluded to previously, in the Census data, we observe no negative effects of VRA exposure on White respondents' earnings and small (but statistically significant) positive effects on their education outcomes.

4.2 Probing the mechanism & additional results

Finally, we present two additional sets of results primarily aimed at attempting to better understand the mechanism driving our results. We had outlined two broad and non-mutually exclusive categories of mechanisms explaining the later-life improvements documented in the paper. The first is within-household improvements in environment and well-being, realized chiefly through higher parental earnings. This mechanism is motivated by recent research documenting that the VRA led to

[7] Controls include: the full interaction of race, gender, and marital status; the interaction of Census year, race, and birth year; and state of birth-by-race FE's.

an immediate earnings increase among Black adults due in large part to greater enforcement of labor discrimination law (Aneja & Avenancio-Leon 2019). The second mechanism is a more equitable distribution of public financing (as in Cascio & Washington, 2014) that generates improvements in early-life localized access to opportunity and public goods.[8] Since our focus is on youths, one can imagine that a particularly important channel is more funding for and spending in schools. It is plausible to imagine state and local government such as school boards directing more funding to schools with a larger share of Black students. Thus, one mechanism is that the VRA expanded the political power of Black voters, which attracted more investment into their local schools, with accompanying school-exposure resulting in better long-run outcomes. A testable implication is that we may expect our results to be stronger in counties with a higher share of Black residents, where political power increased the most.

Table 5 takes a variety of different outcomes from the PSID not otherwise explored in our paper, some of which help speak to the mechanisms above. Columns 1 and 2 take respondents' response to the question "Were your parents poor when you were growing up, pretty well off, or what?" as outcomes. Respondents' were offered three options "poor," "average / it varied," or "pretty well off." In column 1, we estimate a model taking a dummy equal to 1 if the respondent responded "poor" to that particular question; Column 2 takes a dummy equal to 1 if the respondent reported the family was "pretty well off." Otherwise, the model is estimated on a sample, and with controls, that matches the one used in the education specifications of Table 1. We find, from Column 2, that Black respondents more exposed to the VRA are more likely to report that their parents were "pretty well off" than those less exposed. There is no change in the likelihood of reporting having been "poor," so the movement towards "pretty well off" comes from individuals who would otherwise have reported "average / it varied." This result is consistent with Aneja & Avenancio-Leon's (2020) finding that adults' labor market outcomes improved as a result of the PSID; we show here that that is true in our sample, and therefore is a highly plausible channel through which we observe the later-life improvements that we do.

[8] One way in which to achieve a greater distribution of public goods towards Black individuals and communities is through greater Black political representation. Yet the scope for this theoretical channel is somewhat limited in our study context. There were few Black state-level politicians including governors and legislators during our sample period and the increase among Black representatives by 1980 were greater on a per capita basis in covered states relative to non-covered states (Cascio and Washington, 2014). Rates of Black officeholding in the South lagged far behind the shares of African Americans in the population into the late 1980s, and efforts to dilute minority voting strength through electoral procedures such as at-large elections were not challenged successfully at scale until after the 1982 amendments to the VRA (Davidson and Grofman, 1994).

Columns 3 and 4 examine how migration patterns change as a result of exposure to the VRA. Column 3 tests the likelihood that an individual is found living in a VRA-covered county in adulthood (defined as counties covered by the VRA with its passage in 1965, not counties covered in later amendments) as a function of early-life VRA exposure; Column 4 does the same, testing likelihood of living in the South more generally (defined as above). We find that full exposure to the VRA in childhood increases the likelihood of a Black respondent living in a VRA-covered county in adulthood by 12.6 percentage points relative to unexposed respondents. We observe a smaller effect, and imprecisely estimated, for the likelihood of living in the South more generally. (We note, however, that we have estimated similar models in the Decennial Census data, and find clear significant evidence of increased likelihood of living in a state that had a literacy test prior to 1965, but also increased likelihood of living in the broader South. Results are in Appendix Table 2. Also, in the Census data, unlike in the PSID, we find no changes in the likelihood of White individuals living in the South as a function of VRA exposure.) In short, VRA exposure alters the likelihood of moving to the North. The role that may play in explaining our results is complex; however, some recent evidence documents negative outcomes for Black migrants to the North.[9] If the VRA slowed migration towards those conditions (critically, paired with improved conditions for Black Southerners, perhaps driven by the two prior mechanisms), that shift could provide a partial explanation for our findings.

Lastly, less directly connected to the proposed potential mechanisms, we assess the impact of early-life VRA exposure on later-life self-reported health status. The result, taking a dummy indicating that health is "very good" as an outcome, is reported in the final column. This specification roughly matches the earnings specifications, in that we average the respondents' responses across ages 25-35. We find suggestive, but imprecisely estimated (p-value = 0.199), evidence of a positive differential impact on later-life health. As the earnings results above seemed to be driven by very early-life exposure to the VRA, one possibility was that the VRA improved early-life health, improving later-life health, which may have partially explained the earnings result (as in Kose, Kuka, and Shenhav (2020)). However, we do not have evidence to support this channel.

Finally, in Table 6, we re-estimate our main specifications, taking total years of education and average wage from age 25-35 as outcomes, but splitting the sample by county-level percent Black in

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[9] Derenoncourt (2019) finds negative effect on adult outcomes of *children* of migrants; pointing to potential mechanisms, she notes that by the early 1960's (when the earliest cohorts we consider start to enter adulthood) she observes "decreases in white public school enrollment and urban residence within the commuting zone; higher local government expenditures on police and higher murder rates; and increased rates of incarceration." Other work documents higher infant mortality (Eriksson and Niemesh 2016) and higher incarceration rates (Eriksson 2019) of migrants relative to non-migrants.

1960. Cascio & Washington (2014) document that counties with a higher Black share saw an increase in state transfers with the passage of the VRA. Thus, to the extent that distribution of funds drives our results, we may expect larger results in counties with a higher share of Black residents. Columns 1 and 3 report results for counties with less than 25% Black share (the 75th percentile of the sample overall, but the median amongst Black respondents); Columns 2 and 4 report results for counties with greater than 25% Black share. We in fact find that our results are more pronounced in counties with a *lower* share of Black residents, contrary to what might have been expected based on a mechanism implied by Cascio & Washington (2014).[10] It of course remains possible that there is a public redistribution of funds occurring at the sub-county level, which we would not pick up here.

5. Conclusion

This paper studies the impact of childhood exposure to the Voting Rights Act on later-life outcomes. We view later-life outcomes as interesting on their own, but also useful as a window into improvements into childhood environment that are otherwise hard to quantify. To make causal claims, we rely on spatial variation in VRA coverage, with only a subset of Southern states and counties directly impacted by the preclearance provision and the elimination of the literacy tests, and across-birth cohort timing variation. With that variation, we estimate a cohort-based difference-in-differences model, estimating differences in later-life outcomes for birth cohorts whose childhoods overlapped with 1965 versus those whose childhoods largely took place prior to 1965, for individuals who grew up in areas covered by the VRA versus not covered. Based on estimates from the restricted-use Panel Study of Income Dynamics, we find that Black children fully exposed to the VRA during their childhood (ages 0-18) accumulate an additional 0.882 years of education, are 25 percentage points more likely to enroll in higher education, and earn wages between the ages of 25-35 that are 14% higher than non-exposed Black children.

The importance of the impacts of the Voting Rights Acts continues to be highly relevant today, especially in light of the 2013 *Shelby County v. Holder* Supreme Court decision that invalidated key provisions in the original 1965 VRA, essentially releasing "covered" jurisdictions from requiring pre-clearance to modify election practices. Research documents that voter suppression tactics have arisen in previously covered areas that may disproportionately target minorities[11] and that some of the

[10] We reach a similar conclusion if we instead interact our treatment indicators with a linear measure of Pct. Black in 1960.
[11] For example, a set of voting reforms introduced by the North Carolina state legislature in 2013 were deemed to be discriminatory in the 4th U.S. Circuit Court of Appeals, as they would "target African-Americans with almost surgical precision."

inequality-reducing labor market effects that occurred with the passage of the VRA are now happening in reverse with its dismantling (Aneja and Avenancio-León 2019). Likewise, in documenting the positive long-run effects for children who were exposed to the passage of the Voting Rights Act, our paper suggests an important source of inequality across race groups that will grow larger over the coming decades, as children of the post-Shelby-era grow into adulthood, if equal access to the ballot is not protected.

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TABLES and FIGURES

Table 1: Impacts of Early-life VRA Exposure on Total Education Accumulation

	(1) Total Years of Educ.	(2) At least 12 years of educ. (HS grad)	(3) >12 years of educ. (Some coll.)	(4) At least 14 years of educ. (Some coll.)	(5) At least 16 years of educ. (4-yr. coll.)
PANEL A: Minimal Controls					
VRA Cov. x Years Exp.	0.001	-0.000	0.003	0.001	0.002
	(0.013)	(0.002)	(0.003)	(0.003)	(0.003)
VRA Cov. x Years Exp. X Black	0.045***	0.006**	0.010**	0.010**	0.001
	(0.017)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	12,469	12,469	12,469	12,469	12,469
R-squared	0.167	0.104	0.136	0.134	0.158
PANEL B: Full Controls					
VRA Cov. x Years Exp.	-0.003	-0.001	0.003	0.000	0.002
	(0.014)	(0.002)	(0.004)	(0.003)	(0.003)
VRA Cov. x Years Exp. X Black	0.052***	0.008***	0.011**	0.010**	0.002
	(0.018)	(0.003)	(0.005)	(0.004)	(0.004)
Observations	10,972	10,972	10,972	10,972	10,972
R-squared	0.266	0.136	0.201	0.207	0.235

Notes: Sample includes all individuals in the PSID born between 1945 and 1969 and followed into adulthood. Panel A specifications regress educational outcomes on the full interaction of race, early-life VRA exposure, and a dummy indicating early-life residence in a VRA covered county. They use a parsimonious set of controls including year-of-birth-by-race fixed effects, childhood county fixed effects, race-by-gender fixed effects. Panel B specifications furthermore include father's educational attainment level, 1960 county population and share of black population, share of county-level votes cast for Strom Thurmond in the 1948 presidential election, individual exposure during the ages of 5-17 to the timing of school desegregation, an indicator for whether the individual was born after hospital desegregation in the county, and county-level Title I grant entitlement, all interacted with race. Standard errors are clustered at the childhood county level. *** p<0.01, ** p<0.05, * p<0.1

Table 2: Impacts of Early-life VRA Exposure on Labor Market Outcomes (Averages across ages 25-35)

	(1) Hourly wage	(2) Ln(Hourly wage)	(3) Annual earnings	(4) Ln(Annual Earnings)	(5) Employment
PANEL A: Minimal Controls					
VRA Cov. x Years Exp.	-0.088	-0.004	-175.750	-0.004	0.000
	(0.061)	(0.004)	(141.054)	(0.006)	(0.002)
VRA Cov. x Years Exp. X Black	0.197***	0.011**	245.480	0.009	-0.001
	(0.068)	(0.005)	(168.636)	(0.008)	(0.002)
Observations	9,178	9,178	9,531	9,193	9,445
R-squared	0.287	0.269	0.364	0.304	0.206
PANEL B: Full Controls					
VRA Cov. x Years Exp.	-0.108*	-0.006	-238.718	-0.005	0.001
	(0.062)	(0.004)	(155.188)	(0.006)	(0.002)
VRA Cov. x Years Exp. X Black	0.236***	0.014***	352.130*	0.010	0.000
	(0.071)	(0.005)	(197.264)	(0.009)	(0.003)
Observations	8,289	8,289	8,588	8,302	8,529
R-squared	0.315	0.295	0.389	0.325	0.218

Notes: Sample includes all individuals in the PSID born between 1945 and 1969 and followed into adulthood. Panel A specifications regress labor market outcomes on the full interaction of race, early-life VRA exposure, and a dummy indicating early-life residence in a VRA covered county. They use a parsimonious set of controls including year-of-birth-by-race fixed effects, childhood county fixed effects, race-by-gender fixed effects. Panel B specifications furthermore include father's educational attainment level, 1960 county population and share of black population, share of county-level votes cast for Strom Thurmond in the 1948 presidential election, individual exposure during the ages of 5-17 to the timing of school desegregation, an indicator for whether the individual was born after hospital desegregation in the county, and county-level Title I grant entitlement, all interacted with race. Standard errors are clustered at the childhood county level. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Robustness Tests

	(1)	(2)	(3)	(4)
Variation relative to main model:	**School-age exposure as "Years Exp."**	**Lit. Test as "VRA cov."**	**Restrictive early-life county ID.**	**Early-life south/border states only**
PANEL A: Outcome: Total years educ.				
VRA Cov. x Years Exp.	-0.002	0.002	-0.003	-0.005
	(0.020)	(0.013)	(0.015)	(0.018)
VRA Cov. x Years Exp. X Black	0.077***	0.044**	0.052***	0.047**
	(0.027)	(0.018)	(0.020)	(0.022)
Observations	10,972	10,972	10,545	4,795
R-squared	0.266	0.266	0.271	0.261
PANEL B: Outcome: Avg. Wage (25-35)				
VRA Cov. x Years Exp.	-0.092	-0.086	-0.074	-0.077
	(0.088)	(0.058)	(0.062)	(0.073)
VRA Cov. x Years Exp. X Black	0.261**	0.232***	0.192***	0.152*
	(0.103)	(0.069)	(0.071)	(0.086)
Observations	8,289	8,289	8,029	3,565
R-squared	0.315	0.315	0.321	0.300

Notes: Sample includes all individuals in the PSID born between 1945 and 1969 and followed into adulthood. The outcomes in Panels A and B are total years of educational attainment and average wage during age 25-35, respectively. All specifications regress outcomes on the full interaction of race, early-life VRA exposure, and a dummy indicating early-life residence in a VRA county. Column 1 defines early-life VRA exposure as years of exposure during the schooling ages of 5-18, while remaining columns define VRA exposure as ages 0-18. Column 2 defines VRA county as those in states mandating literacy tests as opposed to counties covered under preclearance in the other columns. Column 3 matches county characteristics to a more restrictive early-life county identifier, while Column 4 restricts the sample to southern and border states (plus DC) only. Covariates in all models include race-by-gender fixed effects, father's educational attainment level, 1960 county population and share of black population, share of county-level votes cast for Strom Thurmond in the 1948 presidential election, individual exposure during the ages of 5-17 to the timing of school desegregation, an indicator for whether the individual was born after hospital desegregation in the county, and county-level Title I grant entitlement, all interacted with race. Specifications furthermore include year-of-birth-by-race fixed effects, and childhood county fixed effects. Standard errors are clustered at the childhood county level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: Pre- and Post-1965 Cohort Placebo Tests

	(1)	(2)	(3)	(4)	
PANEL A: Pre-period placebo (YOB 1923-1947), Education	Total Years of Educ.	At least 12 years of educ.	>12 years of educ.	At least 14 years of educ.	At least 16 years of educ.
VRA Cov. x Years Exp.	0.016	0.006	-0.003	-0.000	-0.003
	(0.026)	(0.005)	(0.004)	(0.004)	(0.004)
VRA Cov. x Years Exp. X Black	0.045	0.003	0.001	0.000	0.006
	(0.038)	(0.008)	(0.006)	(0.006)	(0.004)
Observations	3,583	3,583	3,583	3,583	3,583
R-squared	0.446	0.368	0.370	0.365	0.334
PANEL B: Pre-period placebo (YOB 1923-1947), Labor outcomes	Hourly wage	Ln(Hourly wage)	Annual earnings	Ln(Annual Earnings)	Employment
VRA Cov. x Years Exp.	0.071	0.009	-390.710	0.016**	0.001
	(0.110)	(0.007)	(306.859)	(0.008)	(0.002)
VRA Cov. x Years Exp. X Black	-0.078	-0.004	466.360	-0.021	0.002
	(0.152)	(0.009)	(340.228)	(0.014)	(0.004)
Observations	2,854	2,854	3,089	2,859	2,751
R-squared	0.485	0.505	0.386	0.498	0.254
PANEL C: Post-period placebo (YOB 1965-1989), Education	Total Years of Educ.	At least 12 years of educ.	>12 years of educ.	At least 14 years of educ.	At least 16 years of educ.
VRA Cov. x Years Exp.	0.025*	-0.001	0.002	0.003	0.010***
	(0.013)	(0.002)	(0.003)	(0.003)	(0.003)
VRA Cov. x Years Exp. X Black	-0.026	-0.003	-0.003	-0.002	-0.009**
	(0.017)	(0.003)	(0.005)	(0.004)	(0.004)
Observations	8,854	8,854	8,854	8,854	8,854
R-squared	0.290	0.134	0.217	0.235	0.263
PANEL D: Post-period placebo (YOB 1965-1989), Labor outcomes	Hourly wage	Ln(Hourly wage)	Annual earnings	Ln(Annual Earnings)	Employment
VRA Cov. x Years Exp.	0.071	0.000	42.781	0.003	0.000
	(0.057)	(0.008)	(161.713)	(0.010)	(0.002)
VRA Cov. x Years Exp. X Black	0.011	-0.000	1.267	-0.008	-0.002
	(0.075)	(0.011)	(191.062)	(0.015)	(0.002)
Observations	7,217	7,217	7,581	7,235	7,674
R-squared	0.521	0.708	0.356	0.648	0.174

Notes: Panels A and B include all individuals in the PSID born between 1923 and 1947 and followed into adulthood, while Panels C and D use birth cohorts between 1965-1989. Specifications regress educational and labor market outcomes on the full interaction of race, early-life VRA exposure, and a dummy indicating early-life residence in a VRA covered county. Panels A and B define a placebo VRA exposure variable using 1943 as the year of VRA implementation instead of 1965. Similarly, Panels C and D define another placebo VRA exposure variable using 1985 as the year of implementation. Covariates include race-by-gender fixed effects, father's educational attainment level, 1960 county population and share of black population, share of county-level votes cast for Strom Thurmond in the 1948 presidential election, individual exposure during the ages of 5-17 to the timing of school desegregation, an indicator for whether the individual was born after hospital desegregation in the county, and county-level Title I grant entitlement, all interacted with race. Specifications furthermore include year-of-birth-by-race fixed effects, and childhood county fixed effects. Standard errors are clustered at the childhood county level. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Other outcomes

VARIABLES	(1) Self-report: "Parents were poor" in respondent's childhood	(2) Self-report: "Parents were well-off" in respondent's childhood	(3) Lives in VRA-covered county as adult	(4) Lives in South as adult	(5) Self-report: "Very good health" as adult
VRA Cov. x Years Exp.	-0.001	-0.010**	-0.005**	-0.003*	-0.001
	(0.003)	(0.004)	(0.002)	(0.002)	(0.004)
VRA Cov. x Years Exp. X Black	-0.006	0.012***	0.007**	0.005	0.006
	(0.004)	(0.005)	(0.003)	(0.003)	(0.005)
Observations	8,362	8,362	9,697	9,728	8,038
R-squared	0.241	0.127	0.753	0.753	0.184

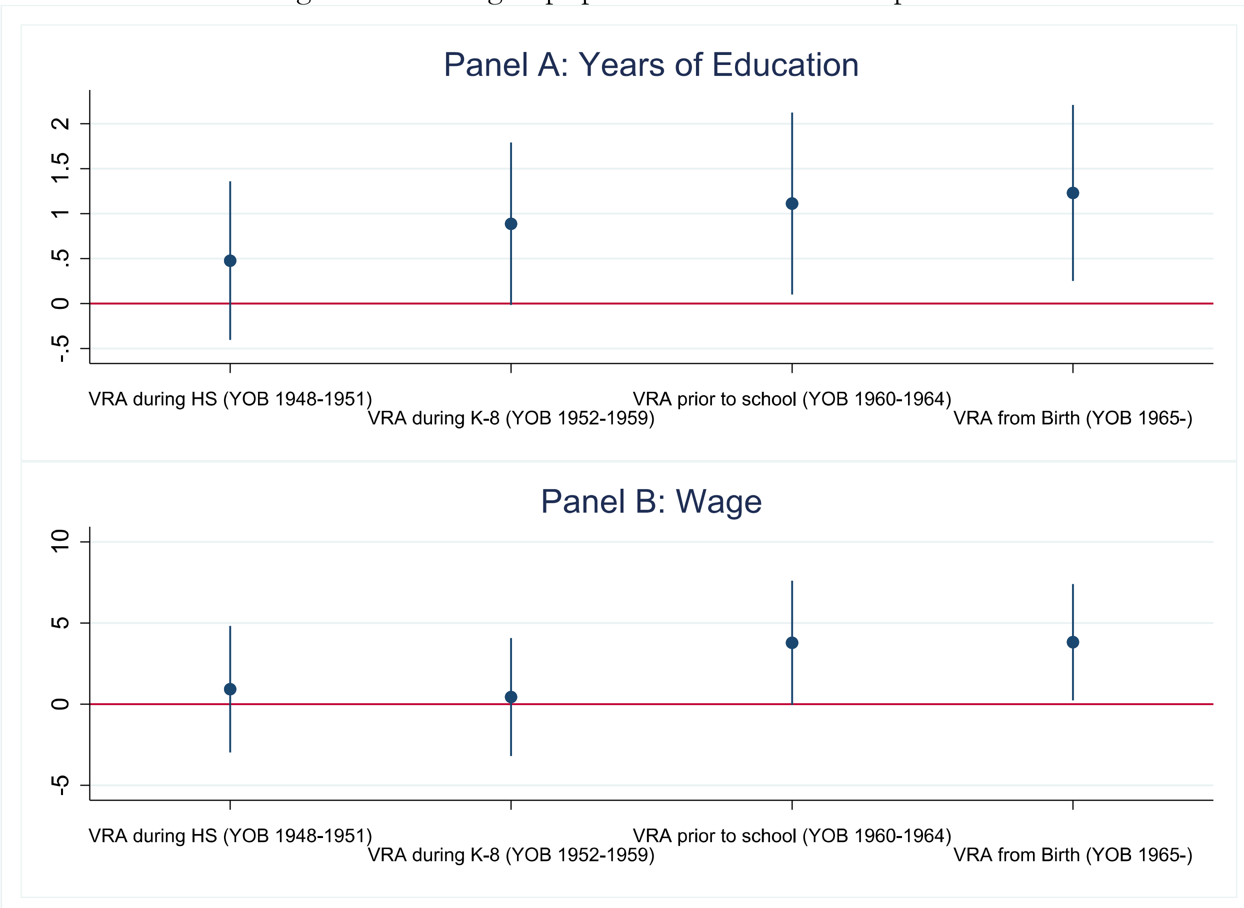
Notes: Sample includes all individuals in the PSID born between 1945 and 1969 and followed into adulthood. Specifications regress outcomes on the full interaction of race, early-life VRA exposure, and a dummy indicating early-life residence in a VRA covered county. Covariates include race-by-gender fixed effects, father's educational attainment level, 1960 county population and share of black population, share of county-level votes cast for Strom Thurmond in the 1948 presidential election, individual exposure during the ages of 5-17 to the timing of school desegregation, an indicator for whether the individual was born after hospital desegregation in the county, and county-level Title I grant entitlement, all interacted with race. Specifications furthermore include year-of-birth-by-race fixed effects, and childhood county fixed effects. Standard errors are clustered at the childhood county level. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Heterogeneity in treatment effects by early-life county race composition

	(1)	(2)	(3)	(4)
	Total Years of Educ.	Total Years of Educ.	Avg. wage (ages 25-35)	Avg. wage (ages 25-35)
VRA Cov. x Years Exp.	0.011	0.011	-0.070	-0.014
	(0.023)	(0.024)	(0.097)	(0.105)
VRA Cov. x Years Exp. X Black	0.065**	0.016	0.435***	0.055
	(0.032)	(0.026)	(0.120)	(0.117)
County Black Share	<25%	>25%	<25%	>25%
Observations	8,255	2,717	6,303	1,986
R-squared	0.274	0.209	0.303	0.274

Notes: Sample includes all individuals in the PSID born between 1945 and 1969 and followed into adulthood. Odd columns limit the sample to childhood counties with the black share of the population at less than 25%. Even columns limit the sample to childhood counties with the black share of the population at more than 25%. All specifications regress educational or wage outcomes on the full interaction of race, early-life VRA exposure, and a dummy indicating early-life residence in a VRA covered county. Covariates include race-by-gender fixed effects, father's educational attainment level, 1960 county population and share of black population, share of county-level votes cast for Strom Thurmond in the 1948 presidential election, individual exposure during the ages of 5-17 to the timing of school desegregation, an indicator for whether the individual was born after hospital desegregation in the county, and county-level Title I grant entitlement, all interacted with race. Specifications furthermore include year-of-birth-by-race fixed effects, and childhood county fixed effects. Standard errors are clustered at the childhood county level. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Cohort-group specific effects of VRA exposure



Notes: Sample includes all individuals in the PSID born between 1945 and 1969 and followed into adulthood. Specifications regress outcomes on the full interaction of race, a dummy indicating early-life residence in a VRA covered county, and dummies for cohort-groupings indicating exposure at various points of childhood to the VRA. Cohorts born between 1948-1951 were exposed to the VRA during part or all of high school only. Those born between 1952-1959 were exposed partially or fully through K-8 and beyond. Cohorts born between 1960-1964 were exposed partially or fully during pre-school years and fully from kindergarten onwards. Cohorts born in 1965 or later were fully exposed from birth through the end of high school. Covariates include race-by-gender fixed effects, father's educational attainment level, 1960 county population and share of black population, share of county-level votes cast for Strom Thurmond in the 1948 presidential election, individual exposure during the ages of 5-17 to the timing of school desegregation, an indicator for whether the individual was born after hospital desegregation in the county, and county-level Title I grant entitlement, all interacted with race. Specifications furthermore include year-of-birth-by-race fixed effects, and childhood county fixed effects. Standard errors are clustered at the childhood county level. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX: Additional Tables

Appendix Table 1: Means of Outcome Measures in PSID, by Race

	Black	White	(Black – White)
Educ. Outcomes			
Max Years Educ	12.9	13.6	-0.7
Educ. >= 12	0.87	0.92	-0.05
Educ. > 12	0.45	0.56	-0.11
Educ. >= 14	0.35	0.48	-0.13
Educ. >= 16	0.13	0.29	-0.16
(obs.)	*3931*	*7041*	
Labor Outcomes			
Wage*	13.73	18.04	-4.31
Annual Earnings*	21,875	32,673	-10798
Employment	0.79	0.93	-0.14
(obs.)	*5514*	*3015*	

The table reports average outcomes within our estimation sample separately for Black and White respondents.

Appendix Table 2: Replicating Main Analysis in Decennial Census data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		EDUCATION		EARNINGS		MOBILITY	
VARIABLES	At least 12 years of educ. (HS grad)	>12 years of educ. (Some coll.)	At least 16 years of educ. (4-yr. coll.)	Log Annual Earnings (Ages 25-35)	Log Annual Earnings (Ages 35-45)	Lives in former Lit. Test state as adult	Lives in South as adult
Lit. Test X Years Exp.	0.002***	0.003***	0.002***	-0.001	0.001	0.000	-0.000
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
Lit. Test X Years Exp. X Black	0.003***	0.001**	-0.000	0.006***	0.002*	0.008***	0.005***
	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)
Observations	7,480,441	7,480,441	7,480,441	2,187,283	2,950,617	7,480,441	7,480,441
R-squared	0.029	0.036	0.028	0.174	0.183	0.505	0.492

Analysis reported in this table is drawn from 1990 and 2000 5% samples of Decennial Census microdata. The sample includes individuals born between 1945-1969, as in PSID analysis. Columns 1-3 and 6-7 include individuals who are at least 25 at the time of the Census. Columns 4 and 5 include age ranges noted in column headings. In addition to those reported, controls include: the full interaction of race, gender, and marital status; the interaction of Census year, race, and birth year; and state of birth-by-race FE's.

Standard errors are clustered at the childhood state level. *** p<0.01, ** p<0.05, * p<0.1