Hookworm Eradication as a Natural Experiment for Schooling and Voting in the American South[*]

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Abstract

Educational attainment is robustly associated with greater political participation, yet the causal nature of this finding remains contested. To assess this relationship, I leverage a natural experiment in the Rockefeller Sanitary Commission's (RSC) anti-hookworm campaign, which exogenously expanded primary and secondary education in the early-20th century American South. I evaluate two RSC hookworm interventions: exposure to the campaign and proportion treated. I use genetic matching to control for observable factors that influenced the haphazard dispensing of treatment, and implement new matching methods for continuous campaign interventions. I also use a variety of methods to assess the robustness of the results to a number of alternative accounts. Throughout, I find a consistent positive effect of education on participation, suggesting additional evidence for a causal interpretation of the `education effect'.

Key words: education, participation, Rockefeller Sanitary Commission, hookworm, American political development, natural experiments, matching.

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1   Introduction

Identifying the *causal effect* of education on political engagement is a difficult inferential problem. Yet, it is one that has garnered some of the most consistent attention in political science. The standard view is that obtaining more schooling drives people to be more engaged, through the influence education has on an individual's cognitive ability, civic values, resources and social networks (Brady, Verba, and Schlozman 1995, Campbell 2006, Wolfinger and Rosenstone 1980). However, recent work has questioned whether the robust association between schooling and voting is causal (Berinsky and Lenz 2011, Kam and Palmer 2008). The decision to obtain more schooling is strongly influenced by many early-life factors that may "accumulate" over a lifetime, increasingly sorting people into different education trajectories. Many of these factors also drive political participation. Without adequate controls for all these relevant forces, there is no guarantee that analyses that uncover positive effects do more than repeatedly measure the same biases that lead people to seek out more schooling in the first place.

In spite of this renewed attention, progress seems to be at a standstill. The existing experimental research is limited since randomizing educational attainment is difficult, expensive and controversial. Further, one of the main conclusions from recent debate is that it is challenging, using non-experimental methods, to adequately control for selection forces when estimating the contemporary education effect (e.g., Henderson and Chatfield 2011, Kam and Palmer 2008, 2011, Mayer 2011). By the time people reach college age, observable and unobservable factors may so strongly influence education choices, that it is exceedingly difficult to find comparable adults with different levels of educational attainment to measure the influence education has on participation. Moreover, selection pressures driving education may grow more influential over a lifetime, compounding on prior choices that increasingly sort students onto

different paths. Studying early-life education can provide better inferences, but such studies may not be possible due to the lack of variation in early schooling due to modern mandatory schooling laws.

In this study, I leverage an historical exogenous intervention in pre-adult schooling to study the 'education effect' on participation. I exploit an early-20th century public health campaign to eradicate hookworm infection in the American South as a natural experiment in schooling expansion. Between 1909 and 1915, the Rockefeller Sanitary Commission (RSC) treated over 2.6 million children for hookworm though county dispensaries across 10 Southern states and Kentucky.[1] Due to the campaign, long-term hookworm rates declined significantly, providing an exogenous boost in education attainment for school-age children (Bleakley 2007, Miguel and Kremer 2004).

The RSC targeted counties based on imperfectly measured rates of hookworm, collected in a haphazard and error-prone fashion. Not every county was targeted, and counties with higher measured rates of hookworm were more likely to receive treatment. This haphazard assignment of the RSC campaign allows researchers to use its impact on education as a valid instrument to study the effect of exogenous shifts in schooling on changes in political participation. The instrument may be especially informative since, in this period, the remarkably low levels of primary and secondary schooling allow for gains in education at an earlier age, when selection pressures are less influential. Further, in being exogenously assigned *to counties*, any aggregate increase in participation as a consequence of the campaign may be interpreted as the causal effect of schooling on voting, *independent* of any individual-level factors that differentiate people in their participation- and education-seeking behaviors.

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[1] These are: Alabama, Arkansas, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

In the analysis, I evaluate two county-level interventions: exposure to the campaign and number of treatments dispensed.[2] Though haphazardly targeted, counties receiving the RSC campaign are different from those not receiving it due to having higher pre-campaign hookworm incidence. Thus, I match on a number of covariates that predict hookworm incidence prior to estimation. Since the number of RSC treatments is a continuous measure, I utilize a novel genetic matching technique to obtain covariate balance for non-binary instrumental variables (Diamond and Sekhon 2014, Henderson 2015b). I then estimate *difference-in-differences* to subtract off any baseline imbalances in prior turnout and schooling that remain after genetic matching. Finally, I implement a number of sensitivity analyses to assess the robustness of the IV estimates to concerns about instrument strength, exogeneity and exclusivity.

Across multiple tests, I find a consistent and positive effect of education on participation in the early-20th century American South. In doing so, this study provides additional evidence that educational attainment can have a meaningful *causal* impact on political engagement, at least in this historical setting. More broadly, the finding suggests, when data are available, that future scholars may gain leverage by looking backwards in time to study the social and political effects of American education. Beyond the substantive results, this study also makes novel methodological contributions to matching methods for non-binary instruments, and innovates sensitivity analyses to investigate possible violations of IV assumptions in aggregate data.

2   Participation and the Education Effect

The canonical account of participation expects people to vote when the benefits of doing so outweigh the costs (Downs 1957, Wolfinger and Rosenstone 1980). Accordingly, education is

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[2] Following Bleakley (2007), I use pre-RSC hookworm incidence as an instrument and recover similar, though weaker results. See Section IV.1, p. 8 in the Online Appendix.

thought to increase voting rates by expanding politically-relevant resources that help subsidize the costs (Brady, Verba, and Schlozman 1995, Wolfinger and Rosenstone 1980) and enhance the (often immaterial) benefits obtained from participating (Schlozman 2002). On the costs side, education can improve a person's intelligence or reasoning ability, reducing the difficulty with collecting electoral information or navigating elections institutions, like voter registration (Rosenstone and Hansen 1993). Schooling may also affect the benefits people receive from voting, by heightening its civic importance or by networking politically engaged citizens (Campbell 2006, Jackson 1996, Schlozman 2002, Rosenstone and Hansen 1993).

In the context of Jim Crow, Southern educational instruction may also have led people to see voting as a valued part of their civic (or sectional) duty, perhaps in protecting the established racial order (Woodward 1951). Yet, crosscurrent to this, much of the expansion in schooling across the region during the period was led by reform-minded individuals who may have transmitted to students progressive values like self-improvement, citizenship and social engagement (Dewey 1916, Woodward 1951). A final reason to expect a turnout effect from schooling in the South is that more education could help poor whites overcome disenfranchising ballot and literacy tests under Jim Crow (Keyssar 2009).

The main alternative to such an account is that education merely "proxies" other factors that cause political participation (Berinsky and Lenz 2011, Kam and Palmer 2008, 2011, Tenn 2007). This "education-as-proxy" view is most clearly articulated by Kam and Palmer (2008), who outline an expansive theoretical account of the various socialization and selection forces that may drive both greater educational attainment and participation. Accordingly, people differentiate in their resources, abilities, and values early on in life, which is reinforced over time as new choices and experiences further narrows a person's trajectory (Grusky 2001, Luster and McAdoo 1996, Saunders 1990). By adulthood, this differentiation may sort people into

predominantly *participant* and *non-participant* types, with a core difference across types being the propensity to obtain more education. As an empirical matter, any association between schooling and voting would be spurious, induced by not adequately accounting for these important pre-educational factors.

Recent empirical work on education has focused on improving research design in an effort to take this criticism seriously. However, the results from this effort have been largely inconclusive (Berinsky and Lenz 2011, Henderson and Chatfield 2011, Kam and Palmer 2008, Mayer 2011). A prominent work in this vein, Kam and Palmer (2008), uses panel data to estimate the effects of college attendance on an index of participatory activities. The authors recover a null finding after matching on a propensity score estimated from a long battery of pre-education parental and student controls, a result they replicate for college completion in a subsequent rejoinder (Kam and Palmer 2011).[3] In another notable study, Tenn (2007) estimates the marginal impact of one additional year of school on participation, controlling for prior education and important covariates like income, gender, and sociodemographics. Tenn (2007) also recovers a null effect.

One of the major challenges in this research is the difficulty in accounting for all the possible unobservable factors (e.g., work ethic or labor market value) that may influence educational attainment and political engagement. Yet, even beyond these unobservables, education may be so highly sorted that it becomes difficult to find comparable people *just on the observables* in any particular sample of individuals. Henderson and Chatfield (2011) show in the Youth-Parent Socialization Panel Study that college attenders in the late-1960s remain substantially different from non-attenders even after maximizing 'comparability' through genetic

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[3] The result for college attendance in the Youth-Parent Socialization Panel Study was shown in follow-up studies by Henderson and Chatfield (2011) and Mayer (2011) to be sensitive to choices about which covariates to control for, and how to conduct matching.

matching on 155 relevant covariates in the data (Diamond and Sekhon 2014). Such dissimilarity (i.e., lack of 'common support') means that *any* observational analysis of the education effect must make a strong extrapolation assumption about the influence of even *observed* covariation driving education and turnout.

Given these inferential concerns, other research has leveraged natural experiments in education, focusing on interventions that may randomly 'nudge' some people onto a different schooling path than they otherwise would have taken (Berinsky and Lenz 2011, Dee 2004, Milligan, Moretti, and Oreopoulos 2004, Sondheimer and Green 2010). As a result of this nudge, at least some variation in educational attainment may be independent of those observed and unobserved forces that drive education and participation outcomes, allowing for a causal estimate of their relationship. Some of this work has uncovered positive education effects (Dee 2004, Milligan, Moretti, and Oreopoulos 2004). Most notably of these, Sondheimer and Green (2010) provide the strongest (and only) experimental evidence of a positive effect, showing that three randomized interventions in early education increased both high school completion and later participation. However, other work using natural experiments has recovered null findings, leaving the debate far from resolved (Berinsky and Lenz 2011).

An under-appreciated concern in this work is that universal education may compound the challenges in studying the education effect. Compulsory schooling laws ensure there is little variation in education prior to adulthood, clearly of significant benefit to society. Yet, this limited variation prevents scholars from studying early-life educational outcomes where sorting pressures might be less problematic. A central motivation in this study is to look backwards in time to study a natural experiment in education prior to the period of universal schooling and its attendant selection forces. By studying an exogenous expansion in early-education due to a

public health campaign at the turn of the century, progress can be made on the contemporary question of whether schooling in fact causes more participation in elections.

3   Hookworm Eradication as a Natural Experiment

Between 1910 and 1915 the Rockefeller Sanitary Commission (RSC) spent 1 million dollars to diagnose and treat over 2.6 million school-age children for hookworm infection in 10 Southern states and Kentucky. As a result of the campaign's success in reducing hookworm infection, hundreds of thousands of children were able to receive additional years of schooling they otherwise would not have obtained due to the debilitating effects of parasitic infection (Bleakley 2007, Miguel and Kremer 2004). Notably, the places that received hookworm treatment were selected in a haphazard way on the basis of imperfectly measured pre-campaign rates of hookworm infection. Due to error in this process, the delivery of hookworm treatments is plausibly exogenous to pre-campaign education levels. This haphazard feature of the campaign allows for its use as an instrumental variable (IV) to study the effects of expanded education on turnout in the American South.

At the turn of the 20th-century, most U.S. Southern states were major breeding grounds for the hookworm parasite, *Necator Americanus*. Figure 1 shows the density of hookworm incidence across the 11 states included in the study. As can be seen, many counties, and especially those along the Southeastern coast, exhibited startlingly high infection rates.[4] Due to serious developmental deficiencies in childhood stemming from hookworm, widespread infection (in some counties topping 90% amongst children) became a national concern. In

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[4] Hookworm rates come from RSC archival reports (Rockefeller Sanitary Commission 1909, 1915), and data collected independently by Thoman (2009). Hookworm incidence data are collected for 740 counties, but are missing (and imputed) for 555 counties. See below and Section IV.2, p. 11 of the Online Appendix for more details.

particular, hookworm's impediment to primary (and secondary) education spurred the RSC to found in 1909 with the express purpose of eliminating the disease so more Southerners could go to school (Bleakley 2007, Miguel and Kremer 2004, Rockefeller Sanitary Commission 1909). Over a five-year period (1910 to 1915), the RSC collected and tested samples from children across much of the South. The RSC also provided hundreds of thousands of treatments to cure those already infected, as well as publicized ways to prevent future infections through better hygiene and sanitation.

4   Estimating the Effect of Education

In this study, I estimate the effect that changes in county-level education rates had on changes in turnout, using two instruments drawn from the RSC campaign: county exposure to any RSC dispensaries and the proportion of infected receiving treatment. Prior to estimation for each instrument, I adjust for a number of covariates related to hookworm incidence, education and turnout outcomes in the counties. Covariate adjustment is done through genetic matching to minimize differences on observed covariates across matched-pairs for both dichotomous and continuous interventions (Diamond and Sekhon 2014).[5] While matching is generally successful in reducing bias, some differences remain after matching. Hence *difference-in-differences* are estimated to ensure that any remaining imbalances in baseline education or turnout are not influencing IV results.

Standard genetic matching is used for dichotomous exposure to the RSC campaign (Diamond and Sekhon 2014, Sekhon 2011). However, continuous interventions pose some

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[5] Genetic matching uses an evolutionary algorithm to maximize similarities across treatment and control groups on covariates over multiple matching iterations. The method has been used widely to non-parametrically condition on variables prior to estimation (Diamond and Sekhon 2014).

challenge for matching analyses, since these must be made discrete to facilitate matching. For the proportion treated instrument, I implement a new genetic matching approach for continuous interventions. [6] Developed more fully in Henderson (2015b), the method is based on an optimal non-bipartite matching algorithm for continuous interventions innovated by Lu et al. (2011, 2001) and extended by Keele and Morgan (2017). Optimal non-bipartite matching (NBP) finds paired-units with very different dose levels, but similar values on X covariates. The matching is optimal, in that, for some continuous z instrument and a covariate distance function $d\{X\}$, NPB recovers *the* vector of matched pairs that minimizes the global sum of distances in $d\{X_i, X_j\}$ (Henderson 2015b, Keele and Morgan 2017, Lu et al. 2011, 2001).

Standard NBP matching weights covariates equally in $d\{X_i, X_j\}$ when minimizing global distances. However, some covariates may be particularly imbalanced or influential in driving variation in voting or education outcomes. Thus, the approach used here adopts an evolutionary optimization step to NBP to find covariate weights, w, that minimize differences on each covariate (rather than optimally over $d\{X_i, X_j\}$). For a given set of weights, a weighted distance function $d\{w, X_i, X_j\}$ is minimized at each step using NBP matching, simultaneously maximizing distances on the continuous treatments across matched units. Over multiple evolutionary steps, the w weights are retained that minimize differences on the most imbalanced (standardized) covariates (Diamond and Sekhon 2014, Henderson 2015b, Sekhon and Mebane 1998).

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[6] Standard (bipartite) genetic matching is used for the dichotomous RSC CAMPAIGN instrument. Here matching is 1-to-1, without calipers, and with replacement to estimate the (local) average treatment effect for the treated (LATT) counties. I allow ties in the matches, so multiple equally 'good' controls can be matched to each treated unit, with weights to partition these uniformly. Ultimately, 385 unique control counties are matched to 633 treated counties, for an effective sample size of 1,018. Given this weighting, I use weighted two-stage-least-squares (2SLS) to estimate IV effects for the RSC CAMPAIGN instrument. Continuous NBP genetic matching is used for the RSC % TREATED instrument. NBP matching is optimal and 1-to-1, with only one odd unit discarded. No calipers are used on $d\{X\}$. The estimand derived from optimal matching on a discretized continuous intervention is proportional to the (local) average treatment effect (LATE). See Henderson (2015a, 2015b) for more details.

Each level of a continuous instrument z can be considered an (approximately) discrete 'dose' across matched units. Each matched-pair has one "high" and one "low" dose unit based on the value of z. After matching, the average difference in a matched-pair on an outcome between "high" and "low" doses is an estimate of the average treatment effect conditional on X.[7] In a series of simulations and applications, Henderson (2015b) finds that genetic dose matching maximizes covariate balance, and outperforms a number of alternative approaches, especially under model misspecification.[8] After genetic matching on both binary and continuous instruments, estimation proceeds in a typical fashion through two-stage-least-squares (2SLS).[9]

4.1   Data Sources

The data for this study are collected from decennial censuses, presidential elections (Clubb, Flanigan, and Zingale 2006), and the Rockefeller archival reports on the RSC campaign (Rockefeller Sanitary Commission 1909, 1915, Thoman 2009).[10] Since this analysis focuses on estimating difference-in-differences, data cover the period before and after the RSC campaign, specifically between the 1900s and 1930s.

The hookworm instruments are collected from the annual reports of the Rockefeller Sanitary Commission and from additional archival research (Rockefeller Sanitary Commission 1909, 1915, Thoman 2009). The annual reports include an exhaustive list of all the counties receiving any dispensaries to administer treatments, which is used to construct the main RSC

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[7] Henderson (2015a) finds this estimate is proportional to an average discretizing shift $E\{z_1 - z_0\}$.

[8] See Henderson (2015b), and Section III, p. 2 in the Online Appendix for details on this genetic matching approach and its implementation. Also, see Henderson (2015a) for a discussion of the *error-in-variables* structure of matching with continuous interventions.

[9] As additional robustness checks, I estimate bootstrap standard errors, and produce Hodges-Lehmann (HL) point estimates, which are robust to possibly weak instruments (Imbens and Rosenbaum 2005). See Section V.3, p. 21 in the Online Appendix for more details.

[10] Many of the measures of hookworm incidence and the RSC treatments were collected by Eric Thoman, who graciously agreed to share his data.

CAMPAIGN instrument. These reports also document the proportion of infected children receiving treatments (RSC % TREATED), and include the county rate of hookworm incidence collected during the campaign.

County-level *schooling* measures come from the decennial census in 1910 for the pre-campaign period, and 1920 for the post-campaign period. Schooling rates in the census report the proportion of children currently enrolled between ages 6 and 17. I also examine schooling rates separately for children in primary (aged 6 to 13) or secondary school (aged 14 to 17). These data can assess whether the RSC instruments had similar effects on schooling expansion for different age groups.[11] Census data also include measures of educational attainment proportional to county school-age and total populations.

Political *engagement* is measured as county-level turnout rates in presidential elections taken over the period of the RSC campaign. The numerator for this measure is reported turnout counts from official sources aggregated at the county-level (Clubb, Flanigan, and Zingale 2006). The denominator is an estimate of the vote-eligible (or in the South 'effectively-enfranchised') population in the counties using various population and demographic data from the census. Prior to 1919, women were denied the franchise in federal elections. And in the Jim Crow South, blacks were unable to vote due to a variety of disenfranchising poll tests. Thus, the denominator for this measure primarily consists of counts of all adult (21 years or older) white men, and after 1919, adult white men and women. Hence these turnout figures measure the rate of voter turnout amongst the eligible voter (rather than total) population.[12]

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[11] In additional robustness checks, I analyze schooling measures from the 5% census samples for the same years housed by the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al. 2010). Age groups for these data nearly-perfectly correspond with the census data. Age groups in the IPUMS are 6 to 17, 6 to 14, and 15 to 17.

[12] See Clubb, Flanigan, and Zingale (2006) for how to measure eligible voters in the South.

As the turnout data is collected at the county, it is difficult to measure the participation of just those populations that could have been exposed to the RSC campaign. Four particular election years are chosen to provide the best possible inferences given the county-level data. The baseline election measure is average turnout from years 1916 and 1920, and the post-campaign measure is average turnout over 1928 and 1932. These years are chosen due to the timing of the RSC campaign. Elections in 1916 and 1920 serve as the baseline since both occur *after* the intervention, but *before* most of the school-age population potentially exposed to the campaign could have voted. The 1928 and 1932 elections are chosen since these are the earliest possible years when most (or all) school-age children at the time of the RSC campaign would be 21 and eligible to vote.[13]

Including baseline election years after RSC implementation may help account for any turnout differences that could have emerged directly from the campaign, and not from expanding schooling rates. If there is a potential direct effect or exclusion violation due to counties receiving the treatment, using these years as baselines should help account for these potentially confounding effects on turnout. Further, these post-campaign years are chosen to capture the earliest possible impacts of education on voting, rather than any cumulative effects (e.g., voting becoming habit-forming). Thus, the results here are potentially underestimating the education effect, since the baseline election years contain some population of voters treated by the RSC

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[13] Only those Southerners born between 1894 and 1909 (i.e., 6 to 17 in 1911 to 1915) could have been in school during the RSC campaign, and thus able to receive hookworm treatment. Assuming these populations are equally distributed across age groups, this would mean that about 0/8th of this population would have been eligible in 1912, 1/8th eligible in 1916, 3/8th eligible in 1920, 7/8th eligible in 1928, and 8/8th eligible in 1932. Including years 1912 and 1936 does not change the results, nor does breaking this into individual-year comparisons, e.g., 1928 and 1912. I present additional results in Section VI.2, p. 47 of the Online Appendix to assess the robustness of the findings to choosing earlier elections as baselines. I find that after matching, the results are robust to choosing any baseline elections years before 1928. I also assess how long the treatment effects last, finding that the education effect appears to persist until the mid-to-late 1940s. These are presented in Section VI.3, p. 48 of the Online Appendix.

campaign (i.e., partially post-treatment), while the follow-up elections do not contain all the RSC-eligible voters (i.e., partially pre-treatment). Finally, these years are also chosen to balance a number of political features.[14] These contain two Democratic-winning (1916 and 1932) and two Republican-winning (1920 and 1928) elections. They also have high inter-correlation, minimizing idiosyncratic variation across counties.[15]

Lastly, a number of pre-campaign census covariates are used during matching. These include prior turnout and vote choice in 1908 (Clubb, Flanigan, and Zingale 2006), and previous schooling, literacy, population, demographic and economic factors collected from the 1900 census enumeration.[16] These covariates contain a small number of missing items (between 13% and 16%). Additionally, 172 counties (13%) are missing either schooling or turnout outcome measures. Finally, of the 633 counties receiving county dispensaries (i.e., RSC CAMPAIGN measure is 1), 590 of these have recorded rates of hookworm incidence, and 549 have counts of the number of treatments administered. An additional 150 counties not receiving dispensaries have hookworm incidence rates measured from the preliminary survey. This leaves 84 treated counties with missing data on the RSC % TREATED instrument. Also, 43 treated counties, and 512 control counties are missing data on prior hookworm rates. To handle missing values, I impute all missing data through Bayesian multiple chained equations (MICE) using the 'mice' package in R (van Buuren and Groothuis-Oudshoorn n.d.). I validate this imputation approach through a number of simulations, and assess whether the IV results are sensitive to imputation

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[14] Women are enfranchised by 1920. Also, 1932 was a big Democratic swing year. Results are robust to using just elections in 1920 and 1928.

[15] A benefit of studying turnout in the Democratic South is that the lack of two-party competition helps alleviate confounding issues related to partisan voting. For instance, national election swings largely have an uniform effect across the South, and do not depend very much on local factors that might bias county comparisons over time.

[16] Table I, p. 7 in the Online Appendix includes the list of covariates, with descriptive statistics.

choices. The simulations indicate that the imputations have high predictive accuracy. Further, the

results are robust to various approaches to dealing with missingness.[17]
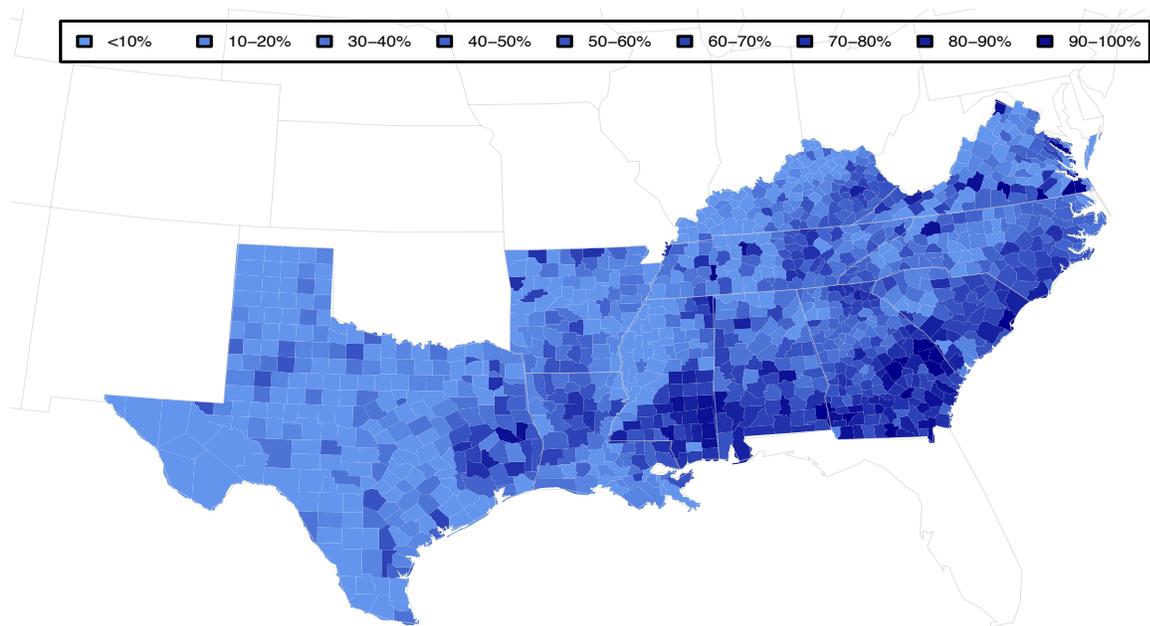

Figure 1: **RSC County Measures of Hookworm Infection in the South, 1910s**: The figure
displays county-level hookworm rates as measured by the RSC in the pre-campaign
period for 10 Southern states and Kentucky.

In the campaign's first two years, the RSC sought to identify areas with the highest rates

of hookworm infection. This information helped determine which counties would receive the

educational and medical treatments over the last three years of the campaign. Importantly, the

method of measurement used was quite haphazard. Each state was assigned an inspector, who

consulted with local physicians, and investigated schools, clinics, and marketplaces, to gain an

impressionistic view of county infection prevalence. During this investigation, lab specimens

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[17] See Section IV.2, p. 11 of the Online Appendix for details and robustness checks on the
imputation approach. I validate the imputations by randomly imputing a subset of units with non-
missing data, and measure the mean square prediction error using real data.

were collected and tested for *some*, but not all counties. For places without any lab testing, the qualitative judgment of the state inspector substituted for scientific measurement. Additionally, laboratory testing involved a great deal of idiosyncratic variation. Samples were mostly taken from children in schools, orphanages, and other public institutions (Rockefeller Sanitary Commission 1915). Yet, the number of schools, farms and persons examined varied widely. For example, some counties had as few as 200 people sampled, while others had well over 2,700 samples taken. Individuals were also sampled in an unsystematic way, contributing additional noise to the resulting hookworm measures.

Between 1912 and 1915, the RSC ramped up treatment by sending dispensaries to counties measured in the preliminary survey as the most heavily infected. According to archival records, not every county received a dispensary to deliver treatments during any part of the campaign. These counties (42.8%) largely were excluded from the scientific survey during the initial state inspection. While most counties having the initial scientific survey received a dispensary later on, as many as 22.7% of these did not receive any follow up after 1910. Though there is no sharp determination for which counties did and did not receive dispensaries, evidence and archival reports show that areas with higher observed rates of childhood infection were prioritized to enhance the impact on children's health.

3.1   Using the RSC Campaign to Study Education and Turnout

The RSC campaign was successful in reducing hookworm incidence amongst children. Follow-up reports show that the campaign immediately lowered infection by more than 7 percentage points, likely doubling that over the subsequent decade (Bleakley 2007, Rockefeller Sanitary Commission 1915). Children by far were the most widely infected, and thus the campaign had the greatest impact on their health. By removing a significant impediment to

schooling, hookworm eradication greatly expanded children's educational attainment following the intervention (Bleakley 2007). Due to the haphazard nature of the selection of counties to receive treatment, the RSC campaign provides a natural experiment to estimate the causal impact of more schooling on political participation.

In this study, I utilize two instruments from the RSC campaign to estimate the effect of education on turnout: county campaign exposure and proportion of treatments administered. Counties receiving any dispensaries would likely experience an expansion in schooling through the reduction of hookworm. This would have exposed some children to more of those educational factors expected to elicit greater political engagement in adulthood. County exposure, however, may not capture the additional educational boost that accrues from treating greater numbers of children. Thus, I also examine the proportion of infected that receive treatment as an instrument for education.[18]

For the RSC interventions to be valid instruments, they must be exogenously assigned to counties. According to archival reports, the RSC based its targeting decisions substantially on the hookworm measurements it collected, intervening in places with greater apparent prevalence Rockefeller Sanitary Commission 1909, 1915). To the degree the hookworm measures contained random variation due to their haphazard collection, some of the RSC targeting would also be randomized. In spite of this error, places with higher infection rates differ on average from those with lower rates, most notably in being poorer, undereducated and politically disengaged. Yet,

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[18] In a related study, Bleakley (2007) looks at hookworm incidence rates to assess the RSC's impact on education. His motivation is that places with greater infection should experience larger reductions in hookworm, and thus also greater expansions in educational attainment after the campaign. This is an imperfect instrument since other factors (like infrastructural investments or better economic growth) occurring over the period could also reduce hookworm rates, but have little to do with the campaign. Nevertheless, given this logic, I also include hookworm incidence as an instrument as a robustness check, and recover similar results. These are presented in Section IV.1, p. 8 in the Online Appendix.

since error in measuring hookworm is likely unrelated to any underlying factors that drive education and participation, so would much of the variation in the RSC county interventions. This haphazard targeting ensures that some counties receiving the campaign are comparable to others not receiving it after taking into account the process used to target them.

The above assignment process is denoted as being conditional *on the basis of a covariate*, in this case measured hookworm incidence. While the RSC relied on impressionistic and biological evidence of hookworm prevalence, conditioning directly on this information is not possible without discarding the many counties that did not receive any lab testing. The approach taken here is to condition on economic, political and social characteristics that strongly correlate with both pre-campaign hookworm incidence and the assignment of counties to the RSC campaign. Counties with similar values on observable factors are assumed to have the same probability of receiving the RSC campaign. While not directly testable, two implications of this assumption are testable. First, counties receiving and not receiving treatment with similar values in observable predictors should not differ in *prior* education and turnout rates, providing a placebo test to validate the design. Secondly, after conditioning on covariates, counties should have similar hookworm prevalence rates. I confirm both of these in empirical tests below, providing evidence that the RSC instruments indeed satisfy the exogeneity assumption.[19]

5   Evidence of a Positive Education Effect

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[19] Two additional findings support instrument exogeneity. Predominantly black counties (>60%) receiving the intervention experienced remarkably similar rates of educational expansion, but no appreciable increase in turnout, as expected under Jim Crow disenfranchisement. Rosenbaum (2002) sensitivity tests also affirm that estimates are robust to possible unobserved confounders that triple the odds of treatment for the treated counties. See Sections V.4 (p. 27) and V.2 (p. 19) in the Online Appendix for more details.

By all accounts, the RSC campaign was successful in reducing hookworm incidence rates. Treated counties experienced a 7 percentage point drop over a ten-year period following the intervention (Bleakley 2007). Given this decrease in hookworm rates, children receiving treatments appear to have obtained additional years of schooling they otherwise would have not been able to due to the disease (Bleakley 2007, Miguel and Kremer 2004).[20] As evidence of this, in Table 1, I report the results of a series of *difference-in-differences* OLS regressions estimating the effect of each RSC campaign measure on the change in schooling rates before and after matching. These results are broken down by three age groups (6 to 17, 6 to 13, and 14 to 17) based on the fact that hookworm may have had more influence on secondary rather than primary schooling. Results in Table 1 show that the RSC campaign instruments all positively increased schooling trends over the period. Before matching, exposure to any campaign treatment resulted in a 1.6% increase in county-level schooling. The estimates (not shown) are just larger for children aged 14 to 17 (2.1%), however, than for those aged 6 to 13 (1.4%). Counties receiving additional numbers of treatments also saw greater improvements in schooling after the campaign.

Similar results are found after matching on important factors. Again as shown in Table 1, both RSC campaign interventions are positively influencing changes in schooling attainment following the campaign period. County exposure to the campaign itself (RSC CAMPAIGN) is associated with a 1.7% increase for all school-age children, and a 1.6% improvement for students age 6 to 13 and 14 to 17, taken separately. Counties receiving additional hookworm treatments also experienced expansions in schooling, for all children (2.2%), as well as those at primary (2.3%) and secondary (2.0%) school-age. As shown in the table, each of these estimates is

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[20] The vast majority of infections (about 85%) in 1910 involved children 18 years or younger, with most of these aged 12 to 16. Education was also far from universal in this period. Both foster conditions for hookworm eradication to have big impacts on Southern educational attainment. See Section II, p. 2 in the Online Appendix for evidence on the effectiveness of the RSC in reducing hookworm infection.

statistically distinct from zero. Both receiving the campaign and the number of treatments

dispensed are associated with positive and significant shifts in schooling trends over the period.

Overall, these results provide strong evidence that reducing hookworm infection expanded the

educational opportunities for children receiving the campaign in the South.

Table 1: **Main Effect of RSC Campaign Instruments on Schooling Outcomes: *Difference-in-Differences*, Before and After Matching by Age Group**

	BEFORE MATCHING				AFTER MATCHING			
	AGES 6 - 17				AGES 6 - 13		AGES 14 - 17	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RSC Campaign	0.016 (0.005)**		0.017 (0.003)***		0.016 (0.003)***		0.016 (0.003)***	
RSC %Treated		0.014 (0.006)*		0.022 (0.006)***		0.023 (0.006)***		0.020 (0.007)**
Turnout$_{(t-2)}$	0.073 (0.011)***	0.080 (0.011)***	0.740 (0.007)***	0.079 (0.011)***	0.076 (0.006)***	0.090 (0.011)***	0.050 (0.006)***	0.044 (0.014)***
Schooling$_{(t-2)}$	0.047 (0.011)***	0.048 (0.011)***	-0.004 (0.008)	0.048 (0.011)***	0.060 (0.007)***	0.046 (0.011)***	0.060 (0.008)***	0.060 (0.014)***
%Black$_{(t-2)}$	-0.185 (0.013)***	-0.172 (0.013)***	-0.032 (0.008)***	-0.173 (0.012)***	-0.168 (0.008)***	-0.171 (0.013)***	-0.133 (0.008)***	-0.173 (0.016)***
Observations	1295	1294	1266	1294	1295	1294	1266	1294
R^2	0.532	0.527	0.831	0.531	0.588	0.583	0.464	0.381

Estimates and standard errors are *difference-in-differences* OLS, with additional controls. Matching on the RSC CAMPAIGN retains 633 unique treated and 385 unique control units, for an effective *N* of 1018. Since matching is with replacement for RSC CAMPAIGN, weighted OLS is used to estimate effects.
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{+}$ $p < 0.1$.

Importantly, the RSC campaign had a large enough impact on schooling, after matching

on covariates, to use the interventions as instruments for education. A common way of testing

whether this impact is sufficiently large is by inspecting the *F*-statistic associated with the

variance in schooling explained by the instruments. Instruments with an *F*-statistic greater than

10 in the first-stage regression are sufficiently strong for valid IV inference (Staiger and Stock

1997). A possible issue in using the RSC instruments before matching, is that only RSC county

exposure surpasses the *F*>10 threshold. This could add some weak instruments bias in the IV

estimates drawn *before matching*. Matching in an IV analysis can help strengthen instruments in

the first stage by accounting for the additional variation in schooling explained by covariates, prior to IV estimation (Lu et al. 2001). As both Table 2 and Table 3 show, *after* matching, virtually all of the instruments are sufficiently strong across all age groups.[21] Significantly, after conditioning on covariates through genetic matching, the strong instruments assumption holds in this analysis, allowing for consistent IV estimates of the effect of education on turnout.[22]

5.1   Instrumental Effects of Education on Turnout

The RSC campaign had a substantial effect in expanding primary and secondary education in the South. The central question in this study is whether this exogenous expansion in schooling resulted in a subsequent increase in participation in presidential elections. Turning to the IV results presented in Table 2, we see indeed that greater educational attainment stemming from hookworm eradication is associated with a significant increase in county-level turnout. Notably, a positive education effect generally is recovered both before *and* after matching on relevant covariates, and using each RSC instrument for education.

Table 2 reports the 2SLS estimates for all school-age (6 to 17) children using both schooling instruments. All results are *difference-in-differences* including additional baseline covariates during estimation. The first two columns present the estimates before matching. As can be seen, these are consistently positive and statistically different from zero at the $p<0.05$

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[21] *F*-statistics for the campaign are all greater than 20 after matching, and *F*-statistics for proportion treated are all greater than 14 except for high school aged children.

[22] In an additional step, I further assess the robustness of IV estimates to this strong instruments assumption through both parametric (2SLS) and non-parametric (HL) estimation approaches. HL estimates are used in IV analyses to provide an additional test of the weak instruments assumption, since these provide correct coverage in hypothesis testing, regardless of the strength of the first-stage association (Rosenbaum2002). HL confidence intervals contain as much or as little information as is available in the instrument. HL results are more conservative than 2SLS in incorporating the information about first stage effects in the resulting IV standard errors. See Table VII in Section V.3, p. 23 of the Online Appendix for the results of this analysis.

level for the RSC binary campaign (1.649, $p=0.007$) and proportion children treated (1.748, $p=0.015$) instruments. While each of these IV estimates indicate a positive causal effect, these rely on the assumption that the difference-in-differences design is adequately subtracting off the bias in previous education and turnout that persists across counties targeted by the RSC prior to matching. Examining the difference-in-difference estimates *after* matching then can provide a stronger test of a causal effect, since genetic matching reduces or eliminates these differences in previous outcomes.

Table 2: **Effect of Schooling on Turnout Using RSC Campaign Instruments: *Difference-in-Differences*, Before and After Matching**

	BEFORE MATCHING		AFTER MATCHING	
	RSC Campaign	RSC % Treated	RSC Campaign	RSC % Treated
Δ Schooling$_{(t-1,t)}$	1.649	1.748	0.819	1.145
	(0.752)*	(0.887)*	(0.266)**	(0.441)**
F-statistic	7.563	5.823	27.441	15.125
Observations	1295	1294	1266	1294
R^2	0.532	0.527	0.831	0.531

Estimates and standard errors are *difference-in-differences* 2SLS, with additional controls. *F*-statistics are Cragg-Donald. Matching on the RSC CAMPAIGN retains 633 unique treated and 385 unique control units, for an effective *N* of 1018. Since matching is with replacement for RSC CAMPAIGN, weighted 2SLS is used to estimate effects.
$^{***}p < 0.001,\ ^{**}p < 0.01,\ ^{*}p < 0.05,\ ^{+}p < 0.1$.

The last two columns in Table 2 display the IV results after genetic matching to reduce imbalances across covariates. Again, both the campaign exposure (0.819, $p=0.000$) and proportion treated (1.145, $p=0.001$) instruments recover significant positive education effects at the $p<0.05$ level.[23] Significantly, after conditioning on covariates and subtracting off any remaining imbalances through a difference-in-differences design, *both* instruments recover a

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[23] The HL results are similar to the 2SLS estimates, though standard errors are typically larger due to the conservative feature of the HL rank estimator. See Table VII (p. 23) in the Online Appendix for the HL results for children aged 6 to 17.

positive effect of education on turnout. These findings provide additional evidence that greater exposure to schooling positively influences subsequent political engagement in elections, at least amongst primary and secondary school-age children in the early-20th century American South.[24]

To provide additional clarity to the findings, I present two binned-scatterplots in Figure 2. The plots illustrate the association between the percent of children receiving treatment (RSC % TREATED), and *differences* in county trends in education and turnout rates, before and after the campaign. Counties are grouped into 101 bins based on 1% breaks, so that points represent county binned-averages, after continuous genetic matching on the RSC % TREATED instrument. As can be seen in Figure 2, there is a roughly linear relationship between the (binned) percent of children being treated across counties, and trends in education and turnout rates following the intervention.[25] As interpretation of the above result, treating a greater proportion of children for hookworm infection in counties yields an approximately linear increase in both educational attainment and political participation.

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[24] To assess whether these effects generalize to other similar interventions, I replicate the estimation strategy exploiting a series of New Deal anti-malaria efforts in the 1930s and 1940s, including the Agricultural Adjustment Act (1933) and the National Malaria Eradication Program (1947). These programs had the effect of reducing malaria infection in the South, targeting areas with the greatest rates of infection. The interventions appear to have expanded both childhood education (0.023; $p=0.031$) and adult political participation (0.018; $p=0.038$) the most, where pre-New Deal malaria infection was the heaviest. These results should be interpreted with much caution since both adults and children benefited directly from these public health policies, each surely had spillover economic or sociodemographic effects, and anyway mandatory public education was expanding dramatically during this period. Nevertheless these are suggestive that the educational and participatory effects of the hookworm campaign are not idiosyncratic to other similar public health investments. See Section VI.1, p. 45 in the Online Appendix.

[25] There is heteroskedasticity in these bivariate associations, even after conditioning on covariates, due in part to many counties having few or no children treated. A nice property of discretizing this instrument during genetic matching is that such heteroskedastic variance is absorbed appropriately into the standard errors of the (now-binary) treatment estimates.
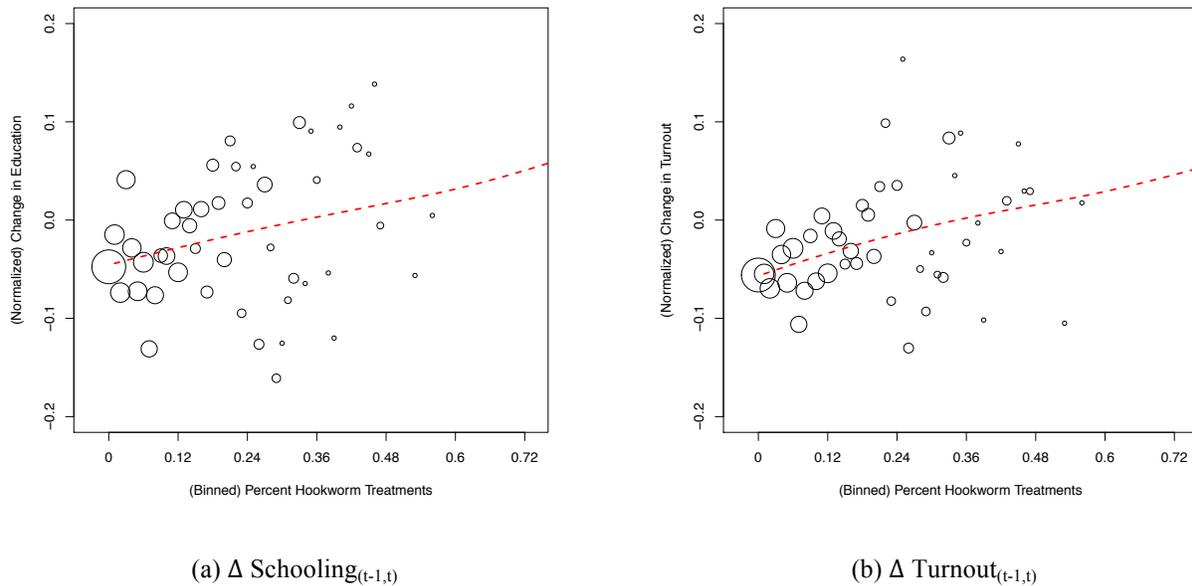
(a) Δ Schooling$_{(t-1,t)}$

(b) Δ Turnout$_{(t-1,t)}$

Figure 2: **Scatterplots Comparing *Difference-in-Differences* in Schooling and Turnout with the RSC % Treated Instrument, After Matching**: The figure presents scatterplots of the change in (a) Schooling and (b) Turnout as these vary with the RSC % Treated instrument. Education and voting means are binned into 101 groups by 0.01 intervals of RSC % Treated. Point sizes are proportional to the log number of counties in each bin. Dashed red lines are bin-weighted lowess estimates.

An important issue in interpreting these results substantively is that the schooling and turnout outcomes use different denominator populations. Education rates capture the proportion of school-age children in school, while turnout rates are the proportion of *eligible* voters participating in four Presidential elections. According to the census, school-age children make up 30% of the total population over the period. Normalizing effects to the total population scale, I find that 115 additional students on average went to school as a result of the county RSC campaign.[26] The vote-eligible population is tougher to precisely measure. According to Clubb, Flanigan, and Zingale (2006), the vote-eligible population in 1916 is 26% of the total population for Southern counties (though this jumps to 44% following women's enfranchisement).

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[26] The average county population in 1920 is 22,563, with an effect size of 0.017 impacting 30% of the population yields: 22,563 x 0.017 x 0.3 = 115.

Assuming a vote-eligible proportion of 0.26, the IV matching estimate (β=0.819) implies the RSC campaign yielded 82 additional voters, for an effective estimate of 0.713.[27]

The above estimates focus on the influence of education on turnout given the average impact the RSC campaign had on overall childhood educational attainment. Yet, the effect of the RSC on schooling may vary across levels of education, since hookworm infection is more prevalent for certain school-age groups. Children aged 12 to 16 were the most likely to contract hookworm. Thus, eradicating the disease could have had a larger impact for this age-group, producing greater gains in secondary rather than primary school achievement. (Though Table 1 above shows roughly similar gains for both age groups.) Consequently, stratifying on county-level measures of schooling at older-ages may provide more precise estimates of the (local) average effect of schooling on turnout.[28]

In stratifying by school age, we see virtually identical turnout effects for primary- and secondary-age children. Table 3 presents 2SLS for each instrument before and after matching, for two age-groups: 6 to 13 and 14 to 17. As seen in the first two columns, the IV estimates, stratifying on ages 6 to 13, are positive and statistically different from zero. Positive estimates are recovered for all matching specifications and instruments. Similarly, as displayed in the last two columns of Table 3, the IV estimates for the subsample of children aged 14 to 17 are again

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[27] For turnout this is 22,563 x 0.0139 x 0.26 = 82. Setting the vote-eligible proportion at 0.44 inflates the effect size to 1.2, which could imply a potential violation of the exclusion restriction. However, census schooling rates are likely to be measured with some error (Ruggles et al. 2010). Such error could attenuate estimates of the RSC effect on schooling, implying an overly large turnout effect. Further, while turnout counts are likely well-measured, the vote-eligible population is not. This could also yield an attenuation effect in turnout differences across treated counties that *inflates* the apparent effect of the RSC on overall voting. See sensitivity analysis in Section 6.2 for more details.

[28] Stratification can also be motivated since hookworm does not uniformly afflict all school age children. Stratifying on ages likely to be similarly affected can reduce additional variance in schooling not expected to be influenced by the RSC campaign. Stratifying in this way also clarifies whether or not the RSC is impacting schooling only for some subset of the population, especially older school-age children in certain counties.

positive and statistically distinct from zero at standard levels.[29] These stratification results indicate that the education effects on turnout uncovered here are not just about expanding schooling at high-school age, *but also* at earlier educational stages in life.

Table 3: **Effect of Schooling on Turnout Using RSC Campaign Instruments: *Difference-in-Differences*, After Matching by Age Group**

| | AFTER MATCHING | | | |
| | AGES 6 - 13 | | AGES 14 - 17 | |
	RSC Campaign	RSC % Treated	RSC Campaign	RSC % Treated
Δ Schooling$_{(t-1,t)}$	0.861	1.103	0.859	1.277
	$(0.290)^{**}$	$(0.429)^{*}$	$(0.291)^{**}$	$(0.590)^{*}$
F-statistic	21.947	14.547	21.844	7.512
Observations	1295	1294	1266	1294
R^2	0.727	0.602	0.724	0.440

Estimates and standard errors are *difference-in-differences* 2SLS, with additional controls. *F*-statistics are Cragg-Donald. Matching on the RSC CAMPAIGN retains 633 unique treated and 385 unique control units, for an effective *N* of 1018. Since matching is with replacement for RSC CAMPAIGN, weighted 2SLS is used to estimate effects.
$^{***}\, p < 0.001,\, ^{**}\, p < 0.01,\, ^{*}\, p < 0.05,\, ^{+}\, p < 0.1.$

6   Assessing Exogeneity and Instrument Exclusion

Clearly the RSC hookworm campaign is associated with an increase in educational attainment and voter participation in the South. Yet, a possible issue in interpreting these relationships as causal is that the RSC targeted counties in a way that *negatively* correlates with a number of factors that likely influence schooling and political participation in the South.[30] Prior to covariate matching, counties exposed to the RSC campaign, that received more hookworm treatments, or that had more infected are much less participatory and educated than counties with lower hookworm incidence. Targeted counties are also less literate, more rural, poorer, and more

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[29] Similar results are recovered for the HL estimates stratifying by age groups. See Table VIII (p. 24) of the Online Appendix for details.
[30] See Figures II – IV in Section V.1, p. 14 in the Online Appendix for balance plots showing differences on 19 covariates across the RSC instruments before and after matching.

agricultural, and thus significantly underdeveloped along factors that correlate positively with
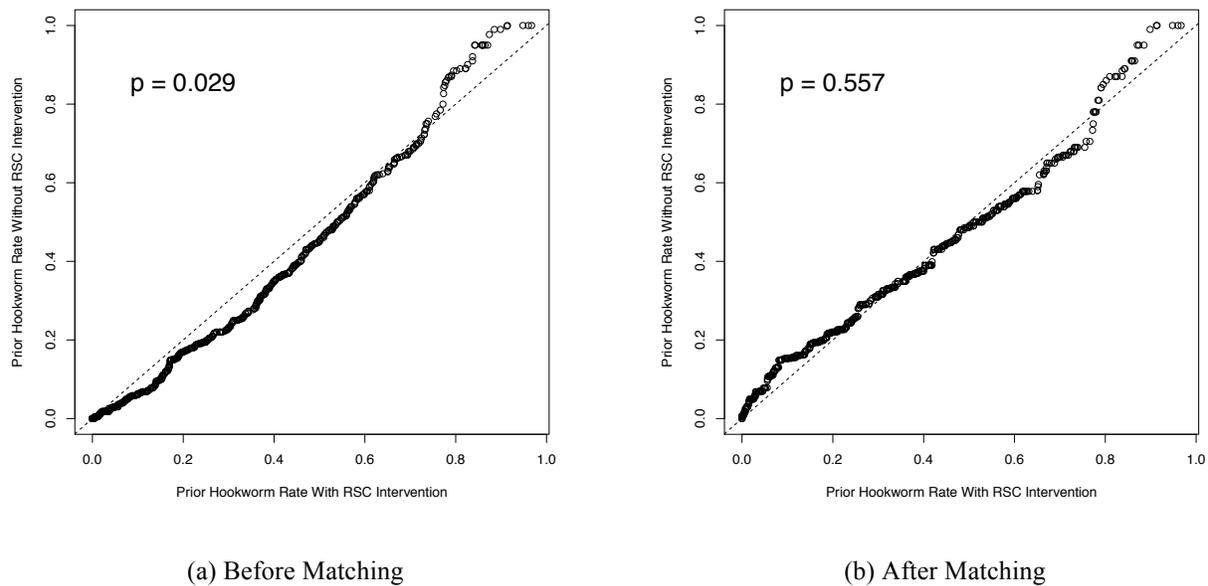
education and participation.[31]

(a) Before Matching   (b) After Matching

Figure 3: **QQPlots of Hookworm Incidence In Treated and Not Treated Counties Before and After Matching**: The figure presents quantile-quantile (QQ) plots comparing the distribution moments of pre-campaign county hookworm rates (a) Before and (b) After Matching counties on 19 covariates.

This study holds that the RSC campaign was *as-if* randomly assigned after controlling for

prior schooling, turnout, and a battery of social and economic variables, all of which strongly

predict hookworm incidence. While not directly testable, a number of implications of this

assumption can be evaluated. First, the covariates used in the matching analysis must be

sufficiently balanced (or uncorrelated with the instruments) after matching.[32] Indeed, after

matching, counties receiving the (binary) RSC campaign are much more similar to counties that

[31] Differences emerge from the RSC decision to target counties with greater rates of hookworm. Given these differences, simple comparisons of education and turnout across targeted counties are unlikely to recover unbiased estimates of the causal effect of education on turnout, without adjusting for factors driving the RSC's targeting decisions.

[32] Figures II(b) - IV(b), p. 16 in the Online Appendix show balance plots for the instruments after matching.

do not. Mean prior education and turnout are very similar, with *t*-test *p*-values greater than 0.5. Further, mean differences are greatly reduced and balance is improved on nearly every covariate, except proportion black, farm acreage, and number of tenant farmers. Similar improvements in balance are made through dose matching, though this was somewhat less successful in balancing on covariates for the proportion treated instrument. Thus, matching is successfully removing much of the bias from these observable factors, especially for the binary RSC campaign instrument, but some bias due to these imbalances may remain.

Table 4: **Placebo Test of Hookworm Campaign Instruments on Prior Turnout and Schooling Outcomes**

	PRIOR SCHOOLING		PRIOR TURNOUT	
	β_{OLS}	*p*-value	β_{OLS}	*p*-value
BEFORE MATCHING				
RSC Campaign	-0.026	0.003	-0.021	0.061
RSC % Treated	-0.022	0.016	-0.040	0.000
AFTER MATCHING				
RSC Campaign	-0.000	0.970	-0.004	0.552
RSC % Treated	-0.013	0.151	-0.014	0.215

Estimates and *p*-values are standard OLS. Matching on the RSC CAMPAIGN retains 633 unique treated and 385 unique control units, for an effective *N* of 1018. Since matching is with replacement for RSC CAMPAIGN, weighted OLS is used to estimate effects.

A second implication is that the RSC targeted based on the distribution of hookworm infection. Thus conditioning on relevant covariates (excluding hookworm incidence) should balance hookworm rates across targeted and non-targeted counties. Indeed, this is precisely the case. Figure 2(a) presents a QQplot comparing the quantiles of prior-hookworm rates for

counties receiving the RSC campaign to the quantiles for counties not receiving the campaign.[33] The plot shows that targeted counties had significantly greater hookworm infections than those not targeted, as confirmed in archival reports. Figure 2(b) reports the quantiles of hookworm incidence across counties after matching. Importantly, matched counties look much more similar in their prior hookworm rates, with means that are statistically indistinguishable with a *t*-test *p*-value of 0.557. This finding strongly suggests that matching on the 19 covariates successfully balanced the main covariate driving the RSC's targeting decisions. Notably, this result was obtained by *only matching on sociodemographic variables* related to hookworm, education and turnout. Hookworm incidence was never used as a covariate during matching.

The above tests provide information that conditioning on covariates is removing most of the bias from observed factors on the resulting IV estimates. Yet, these do not test whether any unobserved or unreported factors are a concern. Remaining imbalances on covariates after matching above may also be an issue in estimation. To assess these possibilities, I conduct a series of placebo tests. These estimate the effect of each RSC campaign instrument on *previous* schooling and turnout, before the RSC campaign occurred. These tests should recover a zero effect by construction since the RSC campaign could not have influenced educational attainment or political engagement before any hookworm treatments were disseminated. Any differences on these tests imply that observed or unobserved factors may be a concern in interpreting the IV estimates as causal.

Table 4 reports results from these placebo tests before and after matching. The first two columns show the estimates of the instruments' effects on prior schooling, and the second two

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[33] QQplots report the quantiles of two distributions, showing differences in the distributions when these fall off the 45-degree line. This test is not available for the proportion treated instrument since its denominator is a measure of hookworm incidence.

columns on prior turnout.[34] As can be seen, significant and negative estimates emerge before matching, reflecting the targeting differences discussed above. Matching largely eliminates this bias. After matching on covariates, the mean differences in prior schooling across both instruments are statistically indistinguishable from zero and very small in magnitude. Prior turnout differences across matched counties receiving and not receiving the RSC campaign are also indistinguishable from zero (-0.004, p=0.552), as are placebo estimates for the proportion treated (-0.014, p=0.216) instrument. These tests show that matching is greatly reducing bias from both observed and potentially unobserved factors that may correlate with pre-campaign schooling and participation outcomes. Yet, some small differences in prior turnout remain after matching. Consequently, I estimate the effects of education on turnout using a *difference-in-difference* design, subtracting off any influence these small prior imbalances may have on outcomes after the RSC campaign. More generally, however, these results provide additional evidence in support of instrument exogeneity.

6.1   Evaluating Alternative Causal Accounts

The above analysis shows that the RSC campaign had a positive impact on increasing both schooling and turnout rates in the early-20th century South. The placebo results suggest that after matching on controls, these effects are independent of any prior factors that may correlate with education, participation and RSC county targeting. Further, the RSC had a sufficiently large impact on education to preclude the possibility that weak instruments are driving the positive turnout effects recovered under 2SLS estimation. In combination, this evidence suggests that the RSC had an important causal effect on improving education and political participation by

[34] In this placebo, prior schooling is from 1910 and prior turnout is from the 1912 presidential election. Neither of these is used during matching, though counties *are* matched on schooling in 1900 and turnout in 1908.

reducing hookworm infection across treated Southern counties. One general way to interpret this result is that there are civic and participatory returns to improving the physical and developmental health of citizens. Yet, this study also interprets these improved rates of political participation as the exclusive result of expanding education, and not due to any direct or alternative influence that the RSC could have on turnout. This is the IV *exclusion restriction*, and under this assumption, education is seen as expanding the resources, networks or civic values of citizens in ways that cascade into participatory returns in adulthood.[35]

This exclusion assumption is not directly testable in general, as it involves many complex counterfactuals. However, some potential violations can be tested indirectly. A way to do this is to identify any (observed) factors besides education that may impact turnout, and assess whether the RSC campaign had an apparent effect on these alternative forces. For example, greater wealth appears to increase county turnout *before* the RSC sent dispensaries. If the RSC increased such wealth, this could account for some of the turnout effects attributed to education. Thus, it is important to show whether the RSC had an effect on any relevant alternative forces, and if so, whether this impact is likely to bias the interpretation that greater education is a cause of expanded turnout.

Notably, the RSC instruments appear to have *no effect* on subsequent county population growth or mobility, crop or farm values and production, unemployment, or wages and income. The RSC campaign does correlate with increased manufacturing production and factory employment, though the magnitude of these effects are small relative to gains made in education. Also, virtually all of these factors appear to *decrease*, rather than increase turnout in the pre-RSC county data. This suggests these alternative effects are most likely *attenuating* the education

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[35] IV analyses also assume that the assignment of the instrument for county i does not influence assignment for j, usually called the Stable Unit Value Treatment Assumption, as well as complier monotonicity, that there are no counties assigned to Z that always defy assignment by taking *1-Z*.

effect reported above. The main theory for why growing manufacturing might increase turnout has to do with empowered labor organizing, something largely suppressed in this period in the American South (Mayhew 1986, Wolfinger and Rosenstone 1980). Further, to the degree the RSC campaign influenced other economic or health factors driving turnout, the Great Depression in 1929 may have wiped out these effects by flattening wages, productivity, health and nutrition.

Another possible exclusion violation stems from the ecological interpretation of county-level treatment effects. The main ecological inference drawn here is that *only* treated children would have experienced greater educational attainment due to the anti-hookworm intervention.[36] Since the RSC narrowly targeted medical treatments to only some school-age children, it is unlikely that the campaign would have had broader education effects, for example, by encouraging children (or their parents) not receiving any hookworm treatments to seek more schooling in general.[37] The second ecological inference essentially restates the IV exclusion restriction: only those children exogenously encouraged to receive more schooling are the ones increasingly turning out to vote. This would be violated if the RSC campaign influenced some

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[36] Both the intention-to-treat, $E[Y|Z]$, and first stage, $E[D|Z]$, effects are well-estimated using either aggregate or individual-level data, assuming RSC treatment does *not* have an effect on the composition of the counties, or movement between them. Otherwise, aggregating data prior to estimation could be conditioning on a post-treatment variable that induces a 'reversal' in the sign of individual-level treatment estimates (Pearl 2014, Spenkuch 2017). I find empirically that the RSC campaign had no discernible effect on changes in county population or mobility. Thus, any concerns about interpreting aggregate effects here mainly involve IV excludability, and not more classical ecological inference problems. See Section V.4, p. 33 in the Online Appendix.

[37] Relatedly, the relevant SUTVA assumption here operates at the county and not individual-level. In other words, if the dispensaries had "spillover" effects amongst individuals but within counties, this would be included in the total county-level effect of the RSC campaign. This would not constitute a SUTVA violation, nor would it confound estimation, though could alter how we interpret the (individual-level) effective impact size of treatment. On the other hand, the RSC campaign was *targeted* at the level of counties and not individuals, though of course was administered to individual children. This is analogous to studying interventions randomized at the school- or classroom-level, where estimating student-level effects generally could be biased. Hence the advantage of estimating aggregate county-level effects is that this follows directly from the particular assignment process used by the RSC campaign.

factor (e.g., labor or agricultural markets) that also increased the participation rates of those not directly receiving treatment. A particular violation of this sort would be if parents had more time to get politically engaged since they could afford to spend less time caring for a sick child. Again, this concern is difficult to directly assess. The indirect evidence above suggests such a concern in general may be remote, at least for the socioeconomic indicators collected by the census. A corollary of parents having more free time due to improvements in children's health, is that they would be more economically productive. Yet, the RSC appears to have had little average impact on adult agricultural or manufacturing productivity and wages.[38]

Perhaps the biggest concern is that reducing hookworm infection may directly improve people's health or intellectual development, independent of schooling, in ways that facilitate greater participation. It is impossible to test this for the RSC in the South. Contemporary evidence in Kenya, however, has shown that hookworm eradication is *not* associated with better scholastic performance or cognitive ability amongst children (Miguel and Kremer 2004). While not dispositive, this suggests the RSC may have put children back to school, but not directly improved their intellectual faculties absent education, either through improved cognition or general health. Moreover, in this period in the South, hookworm is almost entirely a childhood disease. This greatly diminishes the likelihood that adults would have experienced direct health benefits from hookworm eradication that influenced their own political behavior.

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[38] Such concerns rest on difficult-to-assess counterfactuals. Some indirect effects of the RSC campaign could spillover into increased turnout, though many such paths seem unlikely. For instance, women predominantly provided health care for children in the early 20th century American South, though voted at much lower rates (typically 16 - 20 percentage points less) than men. In contrast, fathers provided relatively little childcare investment (Bleakley 2007). Thus, freeing up parents from caring for no-longer-sick children would likely have minor impact on increasing adult voter turnout. To the degree this concern persists, sensitivity analysis on the exclusion restriction can assess how much impact this mechanism would need to have to preclude interpreting education's effect on turnout as causal.

6.2   Sensitivity Tests of Alternative Accounts

To empirically assess concerns about these alternative accounts in general, I implement a sensitivity analysis of the exclusion restriction assumption (Conley, Hansen, and Rossi 2012, Rosenbaum 2002). This analysis identifies lower bounds on estimates of the education effect as alternative paths are allowed to be more influential on (rather than irrelevant to) turnout. For the test, consider the model of turnout $Y = \beta D + \gamma Z$, where D is schooling, and Z is some exogenous instrument.[39] Under the exclusion restriction, γ is zero. However, we can relax this assumption, and evaluate the impact that increasing values of γ have on estimates of the education effect β. To do this, for each γ to be evaluated, replace Y with the adjusted outcome $\mathbf{Y} = Y - \gamma Z$, and estimate $\beta(\gamma)$ through standard 2SLS. The main quantity of interest here is the lower 95% confidence interval of $\beta(\gamma)$ when modeling the adjusted $\mathbf{Y}$. In other words, the test allows us to evaluate how much of the treatment effect of Z on Y must be explained by alternative paths, for the estimate of the education effect to be indistinguishable from zero.

Results from this analysis are presented in Figure 3. On the y-axis are estimates of the education effect $\beta(\gamma)$ at various values of γ. And the x-axis displays values of γ evaluated as a proportion of the total observed effect of Z on Y. In Figure 3(a) for the RSC CAMPAIGN instrument, alternative factors must account for at least 0.59 of the instrumented education effect, for the education coefficient β to reach statistical insignificance. In Figure 3(b) for the RSC % TREATED instrument, this lower bound reaches zero when non-excluded paths account for 0.54 of the instrumented education effect. Thus, the direct effect of the instrument on Y (or the sum of all non-excluded paths), must be at least 1.2 to 1.4 times as influential as the education path, for the IV estimates of the education effect to be interpreted as statistically zero. Substantively, this

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[39] Notably, γZ can represent the indirect influence of all possible (linear) alternatives Q through which Z can impact Y. See Section V.4, p. 27 in the Online Appendix for more details.

implies a lower bound at between 33 and 57 additional voters mobilized (assuming a vote-eligible proportion of 0.26 or 0.44 respectively) in each county by the RSC CAMPAIGN. Consequently, an effective estimate size of between 0.287 and 0.496 is consistent with a positive education effect under this sensitivity analysis.
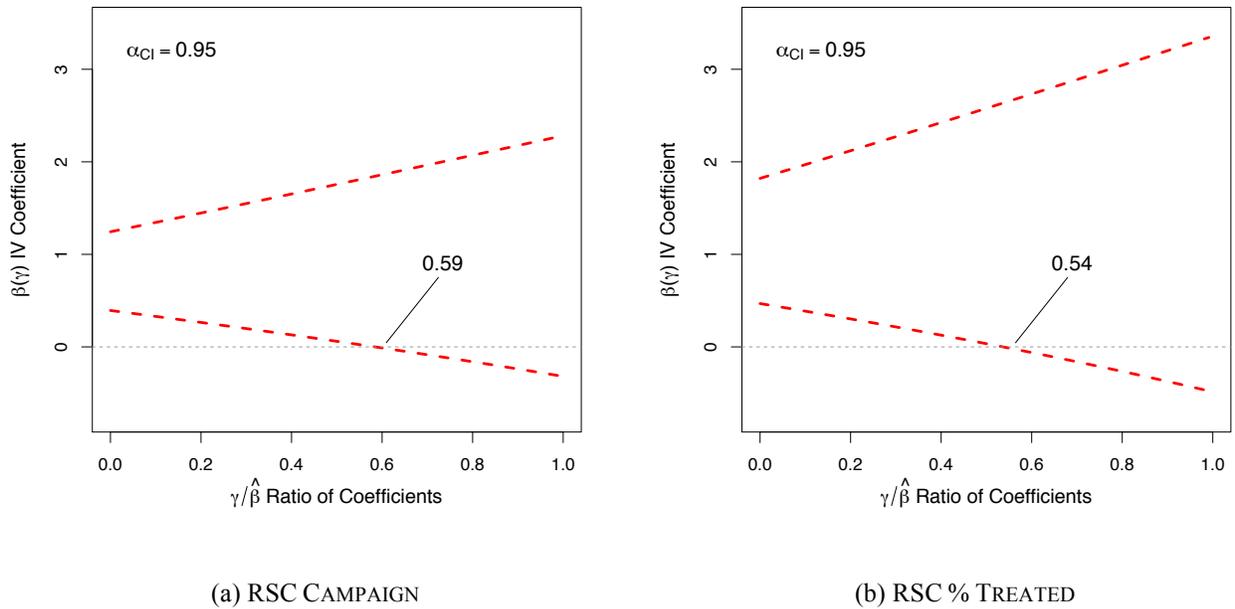

(a) RSC CAMPAIGN   (b) RSC % TREATED

Figure 4: **Bounds on 95% Confidence Intervals for Relaxing the Exclusion Restriction**: The figure presents a sensitivity analysis to assess how robust estimates are to possible exclusion restriction violations. The y-axis arrays the resulting IV estimate of the education effect as the direct effect of the RSC on turnout, γ, varies from 0 to the total RSC intention-to-treat effect (ITT) on turnout. The x-axis arrays the hypothetical direct treatment effect, as a ratio of the estimated IV education effect, $\gamma/\hat{\beta}$

To provide a benchmark for this result, I replicate the analysis using all other possible pathways with available covariates in the census. Here I find the next most robust factor (WHITE LITERACY) is sensitive to alternatives at 0.26 of its influence. Importantly, no other alternative is as influential as the education path. Further, these sensitivity analyses show that the education pathway is much more robust to possible exclusion restriction violations than any other competing explanation linking the RSC intervention to increased turnout. Finally, this sensitivity

test has a direct interpretation as providing bounds on the kinds of ecological (excludability) inferences being made from the data. At the lowest bound consistent with a causal interpretation of an education effect, the RSC campaign yielded 33 new voters among the 115 newly educated students for each county on average, or roughly 20,889 of 72,795 new students across the 633 treated counties in the data. At this bound, the remaining 51,906 newly educated are inferred to have *not* participated in the subsequent election in 1928 and 1932, while the RSC induced 31,017 additional voters through some alternative or direct (non-excluded) mechanism.

Exclusion violations can pose challenges to IV analysis. Yet, in the case of the RSC, many of the most obvious potential violations appear to be of minor consequence. Moreover, to the degree such violations do emerge, using baseline election years occurring *after* the RSC campaign, but before treated recipients could vote, is likely to reduce the impact of possible violations on estimation. This is so since at least some of the influence these other forces may have had on turnout will be factored in during estimation.[40] Nevertheless, the core assumption necessary in this analysis is that the RSC is only influencing the participation and education decisions of Southerners treated for hookworm. However, sensitivity analysis here shows that a positive causal effect of education is evident so long as non-excluded alternatives account for no more than 0.59 of the total effect of the RSC campaign on changes in turnout.

7   Conclusion

In this study, I exploit a novel natural experiment in eradicating hookworm infection in the American South to estimate the education effect. The RSC campaign significantly reduced hookworm infection amongst a cohort of school-age children, thereby expanding their

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[40] See Section V.4, p. 27 in the Online Appendix for details, including additional exploratory analysis of interpreting the excludability assumption generally, and using aggregate data.

educational attainment. The campaign was targeted in a largely haphazard way on the basis of imperfectly measured hookworm incidence rates. As consequence, counties with different treatment assignment, but similar observable characteristics, may be comparable to draw unbiased inferences about the education effect. In studying two instruments from the RSC campaign in a difference-in-differences design, I consistently recover positive effects of education on turnout. These results provide evidence that additional schooling does impact later vote participation, and importantly that this relationship appears to be causal in nature. Further, this education effect emerges not only for high school students, but also for children earlier on in their educational trajectory.

This positive education effect appears unlikely to be the result of other alternative effects the RSC may have had on political engagement in the South. The findings are robust to possibly weak instruments, and after matching on important covariates. A number of validity checks and sensitivity tests also discount the possibility that exclusion restriction or exogeneity violations could entirely account for the positive IV estimates recovered. In addition to these substantive findings, this study makes advances in research design and estimation more broadly. This is principally in extending a new genetic algorithm to conduct matching on continuous rather than binary treatments. Further, I adapt and apply non-parametric estimation approaches to the study of instrumental variables, and also utilize novel extensions of sensitivity analyses to assess the robustness of results to possible violations of as-if randomization or excludability.

Following an exogenous expansion in early-20$^{\text{th}}$ century education, more Southerners subsequently voted as adults. Importantly, the intervention occurred in a period prior to compulsory schooling, allowing for greater leverage into the effects of pre-adult schooling on turnout. Early education interventions are likely subject to less selection pressure than educational outcomes of focus in most recent scholarly work, for example college-attainment or

completion (Sondheimer and Green 2010, Tenn 2007). Additionally, the RSC campaign took place primarily in the low-participation, Democratic South. The relatively low levels of competition in these elections may indicate that additional confounding factors like partisan tides may be less of an issue than in other settings.

However, a concern in generalizing the findings to broader research on participation in the U.S. is that these are drawn from a unique natural experiment in a particular historical place and time. If a positive education effect is idiosyncratic to these particular historical or geographical conditions, then we would not expect a similar expansion in schooling today or in other parts of the country, to yield comparable vote returns. Nevertheless, the political effects of the RSC public health intervention are interesting in their own right, as indeed are the findings that educational attainment increased rates of political participation in the 20th century American South. For instance, modestly expanding the size of the Southern electorate could have swung some state and primary elections, including three-way presidential contests in Louisiana, Mississippi and South Carolina in 1948. In relation to prior theoretical and empirical work, studying such historical interventions can speak to the conditions under which, or the populations for whom, we could expect a causal education effect to arise.[41] More broadly, the findings reported here from the analysis of the RSC campaign join a larger scholarly effort of

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[41] Indeed, explicit to IV estimation is that inferences are valid only for those who would have complied with treatment, that is, took up more education as a result of being assigned a specific form of encouragement (e.g., proximity to a college, born before a school-grade cutoff, assigned a Vietnam draft lottery number). Some of the strongest empirical evidence about the education effect has been drawn from the use of plausibly exogenous instruments (e.g., Berinsky and Lenz 2011, Dee 2004, Sondheimer and Green 2010). Each finding individually speaks to a local (and possibly idiosyncratic) effect, but in sum joins a wider mapping of measurements taken in different samples, at multiple times and places.

accumulating greater evidence to assess whether (and when) the 'education effect' has any causal interpretation.[42]

In spite of the recent turn to natural experiments to study education, most prior work still relies on elaborating appropriate models for how people chose to obtain more schooling. This effort has proven challenging (e.g., Henderson and Chatfield 2011).[43] In exploiting an exogenous intervention in pre-adult schooling, this study aims to measure the association between education and turnout, free of the forces that typically plague observational research lacking plausibly-exogenous natural variation. The findings extend positive results recovered from other contemporary instrumental variables approaches of the education effect. Yet, in investigating educational outcomes in a period with less powerful selection forces, this study breaks new ground by looking backwards in time to provide new evidence of a causal effect of education on participation.

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[42] From a comparative perspective, for example, conditions in the early 20th century American South rather closely resemble some parts of the contemporary developing world, combining relative poverty in income, health, and education, with a semi-authoritarian political order.
[43] The 'local compliers' problem is magnified when people receive all sorts of non-random encouragements to obtain more schooling. For instance, regression analyses of the education effect using a high-quality representative survey, are only able to make inferences about the particular subsample of people who, in receiving education, happened to comply with a highly complex array of non-random interventions.

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