Fertility Transitions Along the Extensive and Intensive Margins

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Abstract

By augmenting the standard quantity-quality model with an extensive margin, we generate sharp testable predictions of causes of fertility transitions. We test the model on two generations of Southern black women affected by a large-scale school construction program. Consistent with our model, women facing improved schooling opportunities for their children became more likely to have at least one child but chose to have smaller families overall. By contrast, women who themselves obtained more schooling due to the program delayed childbearing along both the extensive and intensive margins and entered higher quality occupations, consistent with education raising opportunity costs of child rearing.

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1 Introduction

All societies that embark on a sustained path of economic development experience a decline in fertility concurrent to other important societal changes, such as increases in schooling and declines in mortality. Many different forces are plausible explanations for fertility transitions, including skill-biased technical change, a decline in the cost of contraception, an increase in the relative wages of women, an increase in life expectancy, and a decline in the value of child labor.[1] But the importance of these different factors remains unsettled.

One reason is that it is impossible to separately identify the different proposed causes of the transition using the standard implementation of the workhouse model used to study fertility patterns, the quantity-quality model of Becker and Lewis (1973). In order to obtain tractable results, researchers impose auxiliary assumptions on the quantity-quality model to ensure that fertility is always positive. This simplifies the analysis because it focuses exclusively on the intensive margin. But along the intensive margin, every explanatory cause of the fertility transition has the same prediction—that quantity and quality of children are substitutes.

We argue that there is some scope for identification along the extensive margin—the option to remain childless. Take, for example, a change that causes a decrease in the price of investing in child quality, say because of expanded access to high-quality schools or increased rates of return to education. An augmented quantity-quality model that allows for an extensive margin predicts an increase in the probability that a woman will have a first child. Intuitively, it is necessary to have at least one child in order to invest in the quality of children. Consequently, fertility along the extensive margin increases as the opportunity to invest in child quality expands. We refer to this complementarity at the extensive margin as "essential complementarity." Note that this prediction stands in contrast to the well-known response along the intensive margin, where a positive educational shock causes fertility to decline.

[1] For a recent critical survey on the evidence, see Galor (2012).

Next, consider an increase in the cost of raising children, say through improved labor market conditions and consequently an increase in the opportunity cost of a woman's time. In this scenario, our augmented quantity-quality model predicts that women will reduce fertility along both the extensive and intensive margin. Therefore, our refinement of the standard quantity-quality model generates sharp testable predictions of how variation in different variables affect fertility along the extensive margin that we can use to test the model and improve our understanding of the forces that shape demographic transitions.

Our empirical application examines fertility along the extensive and intensive margins for two generations of women in the American South in response to a large-scale school-building program.[2] The Rosenwald Rural Schools Initiative (Aaronson and Mazumder, 2011) prompted the construction of almost 5,000 new schools, potentially easing significant constraints on the cost of educating children. Schools were targeted to one particular demographic group, rural blacks, allowing the formation of control groups such as urban blacks and rural whites within the same county. Moreover, the building occurred over two decades—between 1913 and 1932—providing variation in access to schooling across cohorts.

From the decennial Censuses, we construct two distinct samples of women. The first sample includes women who were of childbearing age when the schools were built but too old to have attended themselves. For rural black women in this cohort, the schooling opportunities for their prospective children expanded and therefore the price of child quality declined. Consistent with the idea that parents substitute quality for quantity, we show that these women's fertility declined along the intensive margin. We also show that the share of rural black women who had any children increased (fertility increased along the extensive

[2] Other studies that test the quantity-quality model include Schultz (1985), Bleakley and Lange (2009), Becker, Cinnirella, and Woessmann (2010), and Qian (2009). However, none explicitly distinguishes between the extensive and the intensive fertility margin. There have also been many empirical studies examining the effects of women's education on fertility more generally including Strauss and Thomas (1995), Black, Devereux and Salvanes (2008) and McCrary and Royer (2011).

margin), consistent with our extension of the standard quantity-quality model. Overall, we find that the effects along the two margins roughly cancel each other out. We therefore conclude that the evidence from the introduction of the Rosenwald schools supports the idea of essential complementarity in response to a decline in the price of child quality. Further, models that abstract from the extensive margin will fail to capture the full effect of the change in opportunities on fertility decisions and may lead to incorrect inferences.

The Rosenwald Initiative also provides insight into the role of the opportunity cost of women's use of time as a factor in the fertility transition. To investigate this hypothesis, we use a second sample of women who were of school-going age at the time the schools were built. Aaronson and Mazumder (2011) document the substantial impact the schools had on the human capital accumulation of these women. Our model predicts that as the value of female time increases, fertility will decline along *both* the extensive and the intensive margin. We show that by the ages of 18-22, rural black women who were exposed to the Rosenwald schools during childhood were more likely to work in a higher paying occupation, less likely to have children, and less likely to have larger families if they did have children. These effects are quantitatively meaningful, statistically significant, and consistent with the hypothesis that the per-child time cost of childrearing increases with the education and work opportunities of mothers. Therefore, the evidence from the Rosenwald-educated women suggests a strong direct effect of increasing schooling opportunities on fertility.

Although it is common to see fertility declines along both the extensive and the intensive margin during 20th century demographic transitions, most studies have focused solely on the intensive margin. We believe that emphasizing the extensive margin is a novel and useful contribution to the literature and, importantly, plays a relevant practical role in shaping fertility trends. That said, although the preponderance of our empirical evidence supports such an extension of the standard quantity-quality model, we acknowledge that in some cases our results are mixed or not as precisely estimated as one might like. Therefore, we think it would be particularly useful if future work explores our findings in other contexts.[3]

Section 2 describes a simple model of the fertility transition based on Galor (2012). The discussion centers on how essential complementarity and the extensive margin provide additional explanatory evidence on the fertility transition. Sections 3 and 4 introduce the Rosenwald Rural Schools Initiative and the data that provide the empirical evidence reported in section 5. Section 6 concludes with thoughts on the relevance of the extensive fertility margin in developing economies.

2 The Extensive and the Intensive Margin in Fertility Choices

Our framework relies on Galor (2012). Households maximize preferences $U(c, n, e)$ subject to the budget constraint:

$$n\left(\tau^q + \tau^e e\right) + c \leq I. \quad (1)$$

Household income I is spent on consuming goods and services c, raising n children, and investing e in the quality of those n children.[4] The cost of rearing and investing into children depends on the parameters τ^q and τ^e. The parameter τ^q represents a fixed cost of rearing children that is independent of the investments made into these children. The parameter τ^e affects the costs of investing in the quality of children. Both costs depend on the quantity n of children.

At an interior solution (n^*, e^*), the shadow prices of quantity and quality

[3] One potential example is Lucas (2011), who finds that women of childbearing age around the time that malaria was eradicated in Sri Lanka experienced an increase in fertility, whereas those who were born post-eradication experienced an improvement in education and a reduction in fertility.

[4] We denote the quality of children using the letter e because quality investment is typically associated with education. However, e can also represent investment into the health and general well-being of children.

are:

$$p_n = \tau^q + \tau^e e^* \tag{2}$$

$$p_e = n^* \tau^e. \tag{3}$$

Because the shadow price of quantity p_n increases in the quality of children e^*, increased investments in quality will tend to reduce the quantity of children. Likewise, the shadow price of quality p_e increases in the number of children n^*, so additional children reduce investment in child quality. It is this substitution between quality and quantity at the interior solution that generates a fertility transition (Becker, Murphy, and Tamura 1990; Galor and Weil 1996, 2000).

It is common in the literature to impose an Inada condition

$$\lim_{n \to 0^+} \frac{\partial U(c, n, e)}{\partial n} = \infty \tag{4}$$

of preferences over fertility, ensuring that fertility levels are always positive.[5] However, this assumption removes important behavioral distinctions operating during the transition from high to low fertility levels. At high fertility levels, the interaction of quality and quantity in the budget constraint (1) leads to the familiar quality-quantity tradeoff. But the quantity and quality of children are necessarily complements around the extensive margin, or at low fertility levels, simply because it is essential to have children in order to consume the complementary good child quality, an idea that we label "essential complementarity."

In particular, note that the value of remaining childless $V_0(I)$ is independent of the cost of rearing children or investing into child quality. By contrast, the value function capturing optimal fertility conditional on having children $V(I, \tau^q, \tau^e)$ depends negatively on the child cost parameters (τ^q, τ^e). A woman

[5] Such an assumption is imposed by Barro and Becker (1989), Becker, Murphy, and Tamura (1990), Galor and Weil (2000), Doepke (2004), Galor (2012), and many others. The exceptions that we are aware of are Gobbi (2011), who analyzes the dynamics of voluntary childlessness during the demographic transition, and Baudin, de la Croix and Gobbi (2012), who consider the relationship between childlessness and education in the U.S. in a modern setting.

will choose to have children if $V(I, \tau^q, \tau^e)$ exceeds $V_0(I)$.

Now suppose there is a decline in the price of child quality τ^e. The value of having children $V(I, \tau^q, \tau^e)$ rises without impacting the value of remaining childless V_0, implying that more women will choose to have a child. But as fertility increases along the extensive margin, it will decline along the intensive margin as women substitute out of quantity into quality. Thus, a decline in τ^e will compress the distribution of family size from both sides. The impact on total fertility depends on the magnitude of these offsetting effects. By contrast, an increase in the direct cost of rearing children τ^q results in fertility declines along both margins, leading to an unambiguous decline in total fertility. Thus, observed declines in fertility along the extensive margin cannot be attributed exclusively to factors that lower τ^e.

This simple model illustrates the value of examining fertility along both the extensive and intensive margins. However, to make the example more concrete, consider how some of the hypotheses advanced as explanations for the fertility transition roughly map into our stylized model. Some argue that improved access to schooling, increased returns to education because of skill-biased technical change, or increased life expectancy lead to observed declines in fertility. We can think of these factors as reductions in τ^e because they imply the cost of acquiring additional lifetime earnings through increased investments into child quality decline. As we argued above, declines in τ^e would not just lower fertility along the intensive margin but would also raise fertility along the extensive margin. Alternatively, improved access to labor markets for women raises the opportunity cost of rearing children, represented in our model as an increase in τ^q.[6] An increase in τ^q should lower fertility along both the extensive and the intensive margin. Observing fertility along both margins allows us to empirically distinguish explanations of the fertility transition that map into reductions in τ^e and explanations that map into increases in τ^q. Examining only the intensive margin precludes this distinction.

[6]Improvements in contraceptive technology, which reduce the costs of averting births, could also be viewed as an increase in τ^q.

3 The Rosenwald Schools

Our empirical test draws on the Rosenwald Rural Schools Initiative, a matching grant program that partly funded the construction of almost 5,000 schoolhouses for rural blacks in 14 southern states between 1913 and 1932.[7] The Rosenwald School movement originated from a 1912 donation by Julius Rosenwald, a Chicago area businessman, to the Tuskegee Institute in Alabama. Booker T. Washington, Tuskegee's principal, was long troubled by the inadequate resources provided to schools for rural blacks in the South.[8] Washington convinced Rosenwald to use part of his initial donation to fund six experimental schools near Tuskegee and, shortly thereafter, to partly fund another 100 schools primarily in Alabama over the next few years. The program spread rapidly from there. By the close of the Rural Schools Initiative in 1932, capacity had expanded to accommodate roughly 36 percent of the Southern rural black school-age population.

By far, the key component of the program was the construction of modern school facilities that were conducive to learning, including building designs that provided for adequate lighting, ventilation, and sanitation. The construction of new schools also made access easier for those who lived far from existing school buildings. This was particularly true for high school instruction, which was virtually nonexistent prior to the Fund's involvement starting in 1926. Other potentially helpful actions included provisions for adequate school equipment (e.g. desks, blackboards, and books), efforts to improve teacher quality, and increases in the length of the school term.

Figure 1 displays the fraction of school-age black children in a county who could have been seated in a Rosenwald school when the program closed in 1932. The substantial across-county variation in access to Rosenwald schools, in concert with variation in the timing of construction over two decades, provides the basis of our main identification strategy.

[7] This section draws heavily from Aaronson and Mazumder (2011). See also McCormick (1934), Ascoli (2006), and Hoffschwelle (2006), for more details.

[8] See, for example, Bond (1934), Mydral (1944), and Margo (1990) for accounts of black schooling at the turn of the 20th century.

However, the timing and location of schools was likely not random. Indeed, the schools were funded through matching grants, with the Rosenwald Fund ultimately contributing, on average, around 15 percent of construction costs.[9] The remaining money came primarily from local blacks and state and county governments. This funding mechanism suggests that individuals from communities that were particularly open to improving black schools, and thus were able to convince the Fund to invest in their community, might have experienced better outcomes even in the absence of the Rosenwald program. Aaronson and Mazumder (2012) provide several pieces of evidence regarding the possibility of selective school location. We follow their empirical strategy, described in section 4.3 to deal with selective school location.

We consider the effect of the Rosenwald schools on the fertility decisions of two distinct samples of women. The first group ("older cohorts") includes women who were of childbearing age when the schools were built but were too old to attend the schools themselves. For these older cohorts, the introduction of the program provided lower cost access to high-quality schools for their children; in the model, we interpret this development as a decline in the price of child quality τ^e, which is expected to increase fertility along the extensive margin and decrease fertility along the intensive margin.

Our second group ("younger cohorts") is women who were of school-going age when Rosenwald schools were open. For these younger cohorts, Aaronson and Mazumder (2011) demonstrate that exposure to Rosenwald schools improved school attendance, increased years of completed education, and raised cognitive ability as measured by military exams. As adults, these women faced

[9] The Rosenwald fund contributed roughly 25 percent of construction costs for the earliest schools. During the last five years of the program, the Rosenwald share fell to 10 to 15 percent. Although the Rosenwald Fund ultimately only covered a small share of the building expenses, it played a crucial role in providing the prestige and credibility to garner the necessary financial and nonfinancial support of local white and black communities. For example, the Fund hired canvassers to explain available opportunities and guide local black leaders through the fundraising process (Hoffschwelle 2006). The Fund consulted with and, to varying degrees, gained the support of White government officials who acted as the state agents for black schools. Rosenwald money also likely helped procure local white acquiescence, including county education board approval for maintaining schools post-construction (Donohue, Heckman, and Todd 2002).

an increase in the opportunity cost of rearing children τ^q, which we predict to cause their fertility to fall along both the extensive and intensive margin. In addition, the introduction of the program lowered the cost of access to high-quality schools for the children of the younger cohort. That is, the younger cohort also face a decline in τ^e. Observing declines in fertility along the extensive margin therefore underestimate the strength of the opportunity cost effect and declines along the intensive margin overstate the same.

4 Data and Empirical Specification

4.1 Fertility

Our primary sample of southern women is drawn from the 1910, 1920, and 1930 decennial Censuses using the Integrated Public Use Microdata Series (IPUMS, see Ruggles et al. 2010). We use the 1.4% sample for 1910, the 1% sample for 1920, and a 6% sample for 1930. The 1930 data combines the publicly available 1% IPUMS with an early version of the 5% sample, with duplicate observations discarded.

For the older cohorts, women from all three IPUMS who were between the ages of 25 and 49 at the time of the Census are included. As we discuss below, we track their fertility over the 10 years before the Census interview. For example, we measure the fertility experience of the 1930 sample as they move from the ages of 15—39 in 1920 to 25—49 in 1930. The 1920 sample allows us to include women who were of childbearing age when the earliest schools were built. It also provides us with a large sample of women from a "control" group who were living in non-Rosenwald counties. We ensure that no women in our sample of older cohorts could have attended a Rosenwald school themselves. Recall that this allows us to generate a clear prediction that the fertility effects should differ between the extensive and intensive margins.

For our sample of younger cohorts, only women who were between the ages of 18 and 22 in the 1930 IPUMS are included.[10] A clear limitation of this part

[10]Few women above the age of 22 in 1930 were exposed to the schools and few women

of the analysis is that the timing of the intervention and the availability of detailed geographic IPUMS data restricts us to only cross-sectional evidence from one Census. Moreover, the 1930 Census comes at the beginning of their childbearing years; we cannot observe completed fertility. This latter concern is addressed in more detail in section 5.2.

Fertility measures are constructed using counts of surviving children under the age of 10 who can be linked to their biological mothers in the Census years.[11] We limit the analysis to children under 10 because we wish to avoid problems associated with children leaving their parent's household. We construct three measures of fertility—total fertility, total fertility conditional on at least one child, and an indicator of whether a woman had at least one child under the age of 10—to correspond to decisions on the intensive and extensive margins.

Summary statistics of the fertility measures are available in Table 1. The 10-year fertility rates vary substantially by race and rural status and over time. For the older cohort, we can roughly approximate the better known total fertility rate (TFR) by multiplying these 10-year fertility rates by 3.5. This approximate TFR declined rapidly between 1910 and 1930 for rural blacks (5.3 to 3.9) and rural whites (5.8 to 4.6). The urban TFR is much lower but also trends downward by a comparable 25% during these two decades.

below the age of 18 have children. The results are qualitatively similar to expanding our age range by a year or two in either direction. Note that we cannot include 18- to 22-year-old women in the 1920 Census because these women would have been part of our older cohort sample (they would have faced a reduction in τ^e as a result of Rosenwald schools available at the time).

[11] The 1910 to 1930 Censuses do not ask women about the total numbers of children that were ever born. We merge our sample of women with children under 10 via their household ID (serial) and the mother's ID within the household (pernum for the mother, momloc for the child). The links are summarized in the IPUMS variable momrule, which is equal to 1 when there is a clear and convincing mother-child link (a son/daughter linked to a wife/spouse) and greater than one when there are various ambiguities in the relationship. Using this procedure, we can perfectly replicate the IPUMS reported count of children (nchild). However, we use our procedure for three reasons: (1) we can construct fertility for the 5% 1930 sample that does not include nchild; (2) we can drop nonbiological relationships; and (3) we can drop ambiguous matches.

4.2 Rosenwald Exposure

Women are linked to the Rosenwald schools through county of residence, rural status, and birth year. We obtained information on the schools from files that the Rosenwald Fund used to track their construction projects. Each file includes, among other information, the location (state and county), year of construction, and number of teachers. Our analysis uses 4,932 schools with the capacity to hold 13,746 teachers in 888 counties. See Aaronson and Mazumder (2011) for more details.

We use different measures of exposure to the Rosenwald schools for each of our two samples. In each case, we start by measuring the coverage of the schools for each cohort in each county. Specifically, we calculate the ratio of the Rosenwald Fund's count of Rosenwald teachers in county c in year t multiplied by an assumed class size of 45[12] relative to the estimated number of rural blacks between the ages of 7 and 13 in the county in each year.[13] Denote this ratio by $T_{t,c}$. For the older cohorts, exposure is defined as $E_{tc} = \frac{1}{10}\sum_{k=1}^{10} T_{t-k,c}$, the 10-year average of T_{tc} between Census year $t-1$ and $t-10$ in county c. This measure reflects the expanded schooling opportunities that women of childbearing age might expect for their children based on the Rosenwald schools they observe in their community. For the younger cohorts, we use $E_{bc} = \frac{1}{7}\sum_{k=1}^{7} T_{b+6+k,c}$, the average coverage during the years when these birth cohorts b were aged 7—13. This measure reflects how the Rosenwald program affected educational opportunities when these women were of school age.

Table 1 presents summary statistics of the Rosenwald exposure measures

[12] An average class size of 45 is consistent with surveys of rural black southern schools in state and county education board reports at the time. It was also the standard assumption in internal Rosenwald Fund documents.

[13] We confine our analysis to the effects of exposure during the ages of 7—13 because we cannot distinguish which schools (among those built after 1926) included high school instruction. However, our results are robust to defining exposure over the ages of 7—17. The rural black population counts are computed from the digitized 100% 1920 and 1930 Census manuscript files available through ancestry.com and interpolated for 1919 and 1921 through 1929. In a small minority of cases, our exposure measure exceeds 1. In such cases, we topcode values at 1.

for both cohorts. Over this period of declining fertility, there was a rapid increase in exposure to Rosenwald schools among the older cohorts, rising from 0 in 1910 to roughly 19.2% among rural black women in 1930. Almost all of this increase occurred after 1920. The exposure measure averages 7.4% for our younger cohorts of rural black women who were between 18 and 22 years old in 1930. Again, both measures exhibit significant cross-county dispersion, as in Figure 1.

4.3 The Empirical Specification

The key empirical challenge we face is that the Rosenwald schools were not randomly located. Indeed, the Rosenwald Fund's refrain is clear on this point: "Help only where help was wanted, when an equal or greater amount of help was forthcoming locally, and where local political organizations co-operated" (McCormick, 1934). The matching grant aspect of the program further assured nonrandom placement of schools. Aaronson and Mazumder (2011) discuss a number of tests to quantify the extent of the selection bias and find that it is small. In particular, they show that black socioeconomic characteristics do not predict the location of the Rosenwald schools[14] and, further, levels and trends in black schooling before the program were similar in counties that never had a Rosenwald school to those that did. They also show that the effects on human capital are similar when they only use variation arising from the first schools that were built in Alabama for plausibly idiosyncratic reasons.

To deal with endogenous selection, we follow Aaronson and Mazumder's main empirical strategy of controlling for a rich set of covariates, including county-fixed effects and time trends, and applying differencing estimators that exploit that the program was targeted at one demographic group. The basic

[14] They do find that white literacy levels predict the location of the schools, consistent with the Rosenwald Fund's approach to locating in areas where white backlash could be minimized.

statistical model for the older cohort is:

$$\begin{aligned} y_{ibct} &= f(black_i, rural_i, X_{it}, age_{it}, t, c) + \\ &\quad (\gamma_0 + \gamma_1 black_i + \gamma_2 rural_i + \gamma_3(black_i * rural_i)) \times E_{tc} + \varepsilon_{ibct} \end{aligned} \tag{5}$$

which relates a fertility outcome y_{ibct} for individual i born in year b living in county c in Census year t to a flexible function in black and rural indicators, controls X_{it}, age, calendar-year dummies, county-fixed effects, and E_{tc}, the exposure to Rosenwald schools in county c at time t. We interact our Rosenwald exposure measure with race and rural status to take advantage of the explicit targeting of the treatment to rural blacks while allowing other groups, particularly rural whites and urban blacks, to serve as controls.

This approach provides four different estimators of the effect of school exposure on fertility. The sum of the OLS estimators $\widehat{\gamma}_{0,}$ $\widehat{\gamma}_{1,}$, $\widehat{\gamma}_2$, and $\widehat{\gamma}_3$ provides an "undifferenced" estimate of the effect on rural blacks. To the extent that there were other factors that may have affected the fertility of all blacks in a county, including urban blacks, that were unrelated to the introduction of the schools, we can difference out such effects by using $\widehat{\gamma}_2 + \widehat{\gamma}_3$. There may be actual effects on blacks living in areas classified as urban according to the Census to the extent that the Rosenwald Fund and the Census Bureau had different definitions of rural counties. A third estimator uses the difference between rural blacks and rural whites in order to remove any common "rural" effect that both blacks and whites shared. This is represented by $\widehat{\gamma}_1 + \widehat{\gamma}_3$. Finally, the "triple difference estimator" $\widehat{\gamma}_3$ differences out both rural and race effects and is therefore our preferred estimator. Any alternative explanation for the result estimated by $\widehat{\gamma}_3$ must reflect confounding factors that affected only rural blacks and not rural whites or urban blacks in the same county.

We construct an analogous cross-sectional specification for the younger cohort of women who were between the ages of 18 and 22 in 1930. In this case, we modify equation (5) as follows: we (1) drop the time dummies, (2) replace E_{tc} with E_{bc}, and (3) replace the county-fixed effects with state-fixed effects. Because both E_{tc} and E_{bc} can take on values between 0 and 1, we interpret the coefficients in equation 5 as the effect of going from no Rosenwald exposure in one's county to complete exposure.

As an additional robustness check, we report placebo regressions that test for Rosenwald effects prior to the actual intervention. In particular, using the same sample selection criterion, we merge older cohorts of women from the 1880, 1900, and 1910 IPUMS[15] with Rosenwald school data backdated 20 years (that is, a school built in 1922 would be coded as built in 1902). For the younger cohorts, we take advantage of the larger 5% sample from the 1900 IPUMS and suppose that schools were opened 30 years prior to their actual date. In both cases, the timing of fertility decisions predates the construction of actual schools so there should be no association between the Rosenwald exposure measure and female fertility decisions. If that is not the case, it would suggest the Rosenwald schools are confounding long-run trends in fertility that are consistent with additional schooling resources.

5 Results

5.1 Fertility Among the Older Cohorts

Table 2 shows the results for our older cohort of women. Recall that these women were too old to have gone through the Rosenwald schools themselves but their children were potentially exposed to the schools. Column (1) shows the effect of Rosenwald exposure on overall fertility in the last 10 years. Using the triple difference estimator ($\widehat{\gamma}_3$), we find that going from no exposure to complete exposure results in an *increase* of 0.011 children with a standard error of 0.078. The three alternative estimators (black rural - black urban, black rural - white rural, black rural) reveal larger, though generally statistically insignificant, positive point estimates. On their own, these results appear to contradict the prediction of the standard quantity-quality model that relaxing the constraints to invest into education leads to *lower* fertility rates.

The results on overall fertility, however, conflate opposing effects along the

[15] The 1890 IPUMS is not available.

extensive and intensive margins. Along the extensive margin (column 2), our preferred estimator indicates that complete exposure to the schools increases the probability that a woman had a child in the preceding 10 years by 3.0 (2.6) percentage points.[16] The effects are similar for the black rural minus black urban estimator and the undifferenced estimator and slightly larger and statistically significant if we use the black rural minus white rural difference. Column (3) reports results along the intensive margin. Among women who had at least one child in the preceding 10 years, full exposure leads to 0.159 (0.098) fewer children, a result that is marginally statistically significant. The alternative estimators show the same signed effect but are smaller in absolute value and broadly statistically insignificant.

Columns (4) to (6) repeat this exercise for a subsample of married women. Childbearing was relatively less common among unmarried women in the early 20th century compared to today. Therefore, the results are unsurprisingly stronger for married women, especially along the extensive margin. Complete exposure to Rosenwald raised the probability of having a child by 4.4 (2.2) to 6.6 (2.3) percentage points, depending on the estimator. Along the intensive margin, our preferred ($\widehat{\gamma}_3$) estimator suggests a decline of 0.192 (0.105) children, although other estimators tend to be smaller and not statistically different from zero.[17] We see the evidence broadly suggesting that fertility for all women aged 25—49 rose by a little more than 5% along the extensive margin and fell by slightly less along the intensive margin in response to the availability of higher quality schooling for all rural black children in a county.[18] On balance, the response along the extensive margin slightly dominates the response along the intensive margin and thus average fertility increases somewhat with exposure, particularly for married women. Our results imply that the number of black children growing up in small families increased as the dis-

[16]The baseline 10-year probability of having children among rural blacks is 45.6%. See table 1.

[17]There is no statistically significant effect on either the intensive or extensive margin for unmarried women and no effect on the probability of marriage.

[18]Typical Rosenwald exposure in 1930 was roughly one-third, suggesting actual average effects along the extensive and intensive margins on the order of 2%.

tribution of the number of children was "compressed" from both sides. Indeed, the (unreported) probability that a woman had exactly one child under 10 increased by 3.8 (1.5) percentage points in counties with complete Rosenwald coverage.

Columns (7) to (9) report results from the 1880-1910 placebo regressions for married 25-49 year old women.[19] On the extensive margin, two estimates are above 0, one estimate is below 0, and one is virtually 0. None are statistically significant. That said, we cannot reject a difference between the real and placebo estimate of $\widehat{\gamma}_3$, our preferred estimator. Indeed, the point estimates are essentially the same. On the intensive margin, three of the estimators are signed in the wrong direction and the fourth is near zero.

Overall, these findings generally match the predictions of essential complementarity. For our older cohorts, the Rosenwald initiative represented a decline in the price of the quality of education τ^e, as the program led to improvements in both school access and school quality. This, in turn, led parents to invest more heavily into the quality of children. Indeed, Aaronson and Mazumder (2011) find that exposure to the schools led to large improvements in the human capital of students. Our model predicts that this decline in τ^e raised fertility along the extensive margin because of essential complementarity. The model also predicts that fertility will decline along the intensive margin because quantity and quality substitute for each other at higher levels of fertility. Both predictions are broadly consistent with the data. Importantly, the results based on total fertility, combining fertility across both margins, might have led one to mistakenly conclude that the schools had no, or even a paradoxical positive, effect on the fertility of the older cohorts. However, enhancing the model to distinguish between the separate effects of essential complementarity and the quantity-quality tradeoff enables us to reconcile the empirical patterns in fertility among this cohort of women.

[19]Results are similar for all 25-49 year old women.

5.2 Fertility Among the Younger Cohorts

In Table 3, we present the results for the younger cohorts. Aaronson and Mazumder (2011) show that exposure to the Rosenwald schools during childhood had a significant positive effect on the average level of human capital of girls. As adults, increased access to higher quality schooling as children likely raised the opportunity cost of procreation τ^q. In response, fertility should decline along both the intensive and extensive margins.

We start by showing the overall effect of Rosenwald exposure at ages 7—13 on fertility at ages 18—22 in column (1). Using our preferred specification, full exposure to the Rosenwald schools leads to a 0.251 (0.092) decline in the number of children per woman. For a county that goes from 0 to complete Rosenwald exposure, the magnitude of the effect is roughly 60% of the mean fertility rate of 0.39 for rural black women in this age group. The point estimates range from -0.13 to -0.39 across alternative estimators but are all statistically significant at conventional levels.

In columns (2) and (3), we demonstrate that this overall decline is due to a reduction in fertility along both the extensive and intensive margins. The evidence is especially strong along the intensive margin as all four estimators show large and statistically significant negative effects of school exposure on fertility. The triple difference estimator suggests that full exposure leads to a 0.850 (0.408) decline in the number of children among women who have at least one child. The evidence along the extensive margin is more mixed: the point estimate for our preferred estimator is negative and economically meaningful but not statistically significant. The estimator when differencing across rural and urban blacks delivers the economically and statistically strongest evidence for a decline in fertility along the extensive margin. It is worth reemphasizing that the negative effect along the extensive margin may be attenuated by the potentially offsetting effect of a decline in τ^e, as experienced by the older cohorts. Moreover, the negative effect along the intensive margin is enhanced by the same decline in τ^e.

In columns (4) to (6) of Table 3, we focus on women between the ages of 20 and 22, among whom fertility rates are much higher and potential effects of exposure on fertility are therefore easier to detect. Indeed, we find notably larger negative effects for total fertility and along both the extensive and intensive margins. For example, the triple difference estimator suggests that complete exposure to Rosenwald schools on average leads to more than one-half fewer children for women in this age group (or 0.22 fewer children at the average Rosenwald exposure rate). Overall, the response in fertility behavior among the younger cohorts is larger than that among the older cohorts, suggesting that changes in the opportunities for women due to increased education can have an important impact on the onset of fertility.

Finally, columns (7) to (9) report placebo regressions for the 20 to 22 year olds using 1900 data. Along the extensive margin, the results are all over the board and statistically insignificant. Along the intensive margin, the point estimates tend to be negative but small and statistically insignificant. We see little evidence in these exercises that longer-running trends in fertility are confounding our Rosenwald school findings.

To further understand fertility choices in light of additional schooling, table 4 breaks out marriage and fertility outcomes among the 18 to 22 year olds. Complete exposure to Rosenwald appears to delay marriage and childbearing among married women but not childbearing among unmarried women. By age 22, full exposure to Rosenwald schools as school children led to a reduction in the probability of marriage by age 22 of -6.9 (4.4) to -13.3 (4.7) percentage points (column 1). These are economically large, albeit sometimes statistically insignificant, effects that have a direct impact on overall fertility because of the close connection between marriage and childbearing. We also observe a decline in fertility within marriage: average fertility among married women by age 22 declined by about 0.42 (0.23) children. Again, the (unreported) effects are larger among the more fecund 20—22 population.

Consistent with the opportunity cost view, Table 5 reports evidence that the occupational standing of women educated in Rosenwald schools rose compared to those that did not go through the schools. Because of data limitations on education and earnings in Censuses before 1940, we use the Census-derived occscore measure, which assigns an occupation to the median income of all individuals working in that occupation in 1950. We find that in most specifications, exposure to Rosenwald schools at ages 7—13 significantly raises the occscore of the younger cohort (columns 1 and 2), consistent with the view that Rosenwald-educated women had better opportunities in the labor force than those who did not go through the schools themselves. We also find (unreported) that edscore, which is based on a measure of occupational educational attainment in 1950, rose for the younger cohort. No such effect on occscore or edscore is found among the older cohorts who were too old to have obtained Rosenwald educations themselves (column 3).

Because we cannot extend the analysis beyond the 1930 Census with current data, we cannot determine how much our results on the Rosenwald-educated women reflect changes to timing of fertility or completed fertility.[20] That said, we find a strong association between fertility at young and old ages in general. In particular, we constructed a data set of the average number of children under 10 by state of birth, race, and birth cohort from the 1900—1950 Censuses. The correlation between the fertility of 18- to 22-year-old black women and 38- to 42-year-old black women from the same state of birth and birth cohort is 0.54. Adjusted for sampling error, this correlation rises to 0.87.[21] For Rosenwald-only states, the adjusted correlation is 0.81. Therefore, we view our measure of fertility as a useful proxy for completed

[20] In due course, as the 1940 Census geographic data becomes available, we will be able to consider fertility for these women up to age 32 and thus learn whether the Rosenwald intervention primarily delayed the onset of fertility or whether the intervention reduced fertility up through the early 30s as well.

[21] To compute the sampling error-adjusted correlation between the fertility of the young, ϕ_g^y, and old, ϕ_g^o, among group g, let N_g^y and N_g^o be the number of individuals of group g for which we observe f_i^y and f_i^o, the fertility of individual i at a young or at an old age. Note that the Censuses do not allow us to observe the same individual at both young and old ages. It can be shown that $corr(\phi_g^y, \phi_g^o) = \frac{\widehat{cov}\left(f_g^y, f_g^0\right)}{\left(\widehat{var}(f_g^y) - \frac{1}{G}\sum\limits_g\left(\frac{1}{N_g^y}\widehat{s}^2_{y,i\in g}\right)\right)^{1/2}\left(\widehat{var}(f_p^y) - \frac{1}{G}\sum\limits_g\left(\frac{1}{N_g^y}\widehat{s}^2_{o,i\in g}\right)\right)^{1/2}}$

where $\widehat{s}^2_{y,i\in g} = \frac{1}{N_g^y - 1}\sum\limits_g\left(\left(e^y_{i\in g}\right)^2\right)$ is the sampling variance for the young, derived from

the sample residuals within group. An analogous formula applies to the sampling variance of the old, $\widehat{s}^2_{o,i\in g}$. A derivation is available from the authors on request.

Note that we remove group cells with fewer than five observations.

fertility.

Finally, one potentially confounding explanation for our results is the passage of the 1921 Sheppard-Towner Act (Moehling and Thomasson 2012). Sheppard-Towner provided federal funding for maternal and infant health care, particularly in rural areas, between 1922 and 1929. Moehling and Thomasson find that infant mortality fell in areas with more intense treatment. To test whether this channel potentially impacts our fertility results, we collected all available race- and county-specific infant mortality rates from the 1922-1931 Censuses of Births, Stillbirths, and Infant Mortality. We find no association between infant mortality and Rosenwald exposure and therefore conclude that the Sheppard-Towner Act is not driving our findings.

6 Discussion

This paper explores the implications of using an augmented quantity-quality model to explain fertility choices along the extensive and intensive margin after a wholesale change in the availability of higher quality schools. We show that the predictions of essential complementarity are largely consistent with how women of childbearing age adapted their fertility behavior when faced with an increase in schooling opportunities for their children. In particular, among our older cohorts, the probability of having a child rose and the number of children, conditional on having children, fell in response to the introduction of Rosenwald schools. These two competing effects roughly offset each other. We also find that the expansion of Rosenwald schools caused those women who were educated in the Rosenwald schools, our younger cohorts, to change their fertility behavior substantially. The increase in education among these women was accompanied by a substantial decline in early fertility (along both the extensive and the intensive margin), a delay in marriage, and an increase in the quality of their chosen occupations. This behavior is consistent with the notion that education raised the opportunity costs of fertility.

It is common to see fertility declines along both the extensive and the intensive margin during demographic transitions. Over the first half of the twentieth century, childlessness became more prevalent among southern black and white women, at the same time that large families became less common (see figure 2 and the appendix). Developing countries today display a similar pattern. According to data from the Demographic Health Surveys over the last 30 years, modern-day developing countries with high fertility along the intensive margin are simultaneously those with high fertility along the extensive margin.

Introducing an extensive margin within a standard quantity-quality model generates additional tests regarding the channels driving demographic transitions. For example, skill-biased technical change or improvements in longevity will act analogous to a decline in the price of investing in the quality of children. Therefore, these explanations fail to generate the simultaneous decline in fertility along both the intensive and the extensive margin that is typical during demographic transitions. These explanations are therefore unlikely to be the sole driving forces behind the transition. Instead, we tentatively propose that increases in the opportunity cost of childbearing induced by increased schooling attainment among young women play an important role in the demographic transition. One plausible interpretation would be that improved schooling opportunities induce greater schooling investments, which subsequently raise the opportunity cost of childbearing and lower fertility along all margins.

7 Appendix: Childlessness in the United States, 1840-1945 (preliminary)

This brief appendix provides background on childlessness in the U.S. during the 19th and early 20th centuries. We report the population-weighted fraction of ever-married women aged 45 to 59 who are childless using the 1900, 1910, and 1940-1990 IPUMS.[22] These age ranges and Censuses provide a time-series for the cohorts born 1841 to 1865 and 1881 to 1945. The sample is restricted to ever-married women because completed fertility was not asked

[22] Completed fertility was not asked in the 1920 and 1930 Censuses.

among never-married women prior to 1970.[23] The 45-59 year old range is chosen to correspond to ages when fertility is complete but risk of mortality is still low. The magnitude of any mortality bias will change with improvements in life-expectancy. Therefore, we also compute (unreported) rates for younger woman; those trends are comparable to the figures presented here.

Figure 3 (labeled figure 2) displays the results for all Southern women. Around 5 to 10 percent of cohorts born during the mid-19th century are childless. That rate spikes to over 20 percent for cohorts born between the 1880s and 1910s. Starting in the 1910s, childlessness begins to recede, reaching mid-19th century levels by the late 1920s. Figure 4 shows a similar but higher pattern for Southern blacks, peaking at over one-third around the turn of the 20th century. A significant demographic literature studies black-white differences in childlessness and a shorter one explores reasons for the surge in childlessness in the decades around 1900 (see e.g. Boyd 1989). For our purposes, a key insight is that there is significant temporal variation in the extensive fertility margin during the time period that we study.

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Table 1: Summary Statistics

	Older Cohort				Younger Cohort			
	Black, Rural	Black, Urban	White, Rural	White, Urban	Black, Rural	Black, Urban	White, Rural	White, Urban
Fertility Measures								
Total Fertility	1.215	0.485	1.397	0.761				
	[1.645]	[1.061]	[1.482]	[1.128]				
Total Fertility, 1910	1.509	0.607	1.666	0.959				
	[1.729]	[1.125]	[1.580]	[1.303]				
Total Fertility, 1920	1.344	0.509	1.519	0.801				
	[1.662]	[1.052]	[1.503]	[1.159]				
Total Fertility, 1930	1.127	0.464	1.321	0.720	0.388	0.207	0.391	0.213
	[1.613]	[1.051]	[1.449]	[1.086]	[0.839]	[0.616]	[0.762]	[0.558]
Extensive Margin	0.456	0.234	0.599	0.406				
	[0.498]	[0.423]	[0.490]	[0.491]				
Extensive Margin, 1910	0.551	0.299	0.656	0.453				
	[0.497]	[0.458]	[0.475]	[0.498]				
Extensive Margin, 1920	0.512	0.258	0.634	0.418				
	[0.50]	[0.438]	[0.482]	[0.493]				
Extensive Margin, 1930	0.425	0.222	0.582	0.396	0.226	0.131	0.258	0.154
	[0.494]	[0.415]	[0.493]	[0.489]	[0.418]	[0.337]	[0.438]	[0.361]
Intensive Margin	2.664	2.073	2.331	1.874				
	[1.440]	[1.232]	[1.220]	[1.025]				
Intensive Margin, 1910	2.739	2.028	2.541	2.116				
	[1.433]	[1.161]	[1.260]	[1.139]				
Intensive Margin, 1920	2.624	1.972	2.395	1.917				
	[1.426]	[1.184]	[1.211]	[1.037]				
Intensive Margin, 1930	2.648	2.095	2.270	1.818	1.716	1.581	1.514	1.383
	[1.444]	[1.251]	[1.206]	[0.990]	[0.914]	[0.850]	[0.739]	[0.640]
Rosenwald Measures								
Own Exposure to Rosenwald	0.000	0.000	0.000	0.000	0.074	0.080	0.073	0.068
	[0.000]	[0.000]	[0.000]	[0.000]	[0.106]	[0.123]	[0.133]	[0.120]
Rosenwald Exposure in Last 10 Years	0.139	0.198	0.146	0.166				
	[0.177]	[0.246]	[0.213]	[0.234]				
Rosenwald Exposure Last 10, 1910	0.000	0.000	0.000	0.000				
	[0.000]	[0.000]	[0.000]	[0.000]				
Rosenwald Exposure Last 10, 1920	0.006	0.005	0.005	0.003				
	[0.015]	[0.015]	[0.016]	[0.012]				
Rosenwald Exposure Last 10, 1930	0.192	0.247	0.198	0.218				
	[0.182]	[0.253]	[0.227]	[0.246]				
Other Measures								
Married	0.788	0.652	0.850	0.763	0.490	0.442	0.476	0.402
	[0.409]	[0.476]	[0.357]	[0.425]	[0.50]	[0.497]	[0.499]	[0.490]
Labor Force Status	0.465	0.636	0.127	0.230	0.376	0.531	0.189	0.422
	[0.499]	[0.481]	[0.333]	[0.421]	[0.484]	[0.499]	[0.391]	[0.494]
Literate	0.728	0.845	0.941	0.972	0.877	0.945	0.978	0.993
	[0.445]	[0.362]	[0.236]	[0.164]	[0.328]	[0.227]	[0.148]	[0.082]
Occscore (hundreds of 1950$)	8.088	8.805	16.714	21.378	7.154	9.781	17.408	20.869
	[5.281]	[6.880]	[9.810]	[8.269]	[5.093]	[6.795]	[9.244]	[6.153]
N	71,273	39,475	191,669	107,689	20,623	9,659	46,661	24,119

The older cohort includes 25-49 year old women from the 1910, 1920 and 1930 IPUMS. The younger cohort includes women 18-22 years old from the 1930 IPUMS. The extensive margin is the probability that a woman has at least one child. The intensive margin is the number of children a woman has, conditional on having at least one child. Refer to the text for details on how the variables are constructed.

Table 2: The Effect of Rosenwald Exposure on the Fertility of the Older Cohorts

	1910-1930						Placebo, 1880-1910		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All, 25-49			Married, 25-49			Married, 25-49		
	Total fertility	Extensive margin	Intensive margin	Total fertility	Extensive margin	Intensive margin	Total fertility	Extensive margin	Intensive margin
$\gamma 0$	-0.025	-0.012	0.019	-0.043	-0.023	0.028	-0.161**	-0.044**	-0.144**
	[0.043]	[0.016]	[0.047]	[0.048]	[0.015]	[0.052]	[0.069]	[0.021]	[0.073]
$\gamma 1$	0.035	0.009	0.110	0.041	0.004	0.144	-0.052	-0.046	0.054
	[0.043]	[0.018]	[0.075]	[0.052]	[0.020]	[0.087]	[0.103]	[0.039]	[0.126]
$\gamma 2$	0.041	-0.005	0.044	0.059	0.001	0.045	-0.061	0.002	-0.030
	[0.044]	[0.016]	[0.045]	[0.051]	[0.016]	[0.049]	[0.080]	[0.024]	[0.081]
Preferred Estimator									
	Triple Difference								
B-W Rural - B-W Urban ($\gamma 3$)	0.011	0.030	-0.159	0.077	0.062**	-0.192*	0.164	0.049	0.107
	[0.078]	[0.026]	[0.098]	[0.090]	[0.029]	[0.105]	[0.145]	[0.048]	[0.153]
Alternative Estimators									
	Difference in Difference								
Black, Rural-Urban ($\gamma 2 + \gamma 3$)	0.052	0.025	-0.115	0.136	0.063**	-0.147	0.103	0.051	0.078
	[0.085]	[0.029]	[0.089]	[0.097]	[0.031]	[0.094]	[0.127]	[0.047]	[0.129]
B-W Rural ($\gamma 1 + \gamma 3$)	0.047	0.039*	-0.049	0.119	0.066***	-0.048	0.112	0.003	0.161*
	[0.067]	[0.021]	[0.074]	[0.075]	[0.023]	[0.074]	[0.101]	[0.029]	[0.084]
	Undifferenced Effect of Exposure								
Rural black ($\gamma 0 + \gamma 1 + \gamma 2 + \gamma 3$)	0.062	0.023	0.013	0.135*	0.044**	0.025	-0.110	-0.039	-0.013
	[0.067]	[0.021]	[0.075]	[0.074]	[0.022]	[0.076]	[0.091]	[0.025]	[0.077]
N	410106	410106	200351	326914	326914	190732	368436	368436	266424
R2	0.133	0.115	0.111	0.157	0.141	0.113	0.136	0.100	0.097

Columns (1)-(6): Sample includes 25-49 year old women from the 1910, 1920 and 1930 IPUMS. Columns (7)-(9): Sample includes 25-49 year old women from the 1880, 1900, and 1910 IPUMS. The dependent variables are as follows. Columns 1, 4, and 7: the number of 0-9 year olds at the time of the Census; columns 2, 5, and 8: an indicator of having at least one child between the age of 0 and 9; columns 3, 6, and 9: the number of children conditional on at least one child. . All specifications contain county fixed effects, state-specific time trends, race and rural specific trends, a full sets of age and year dummies and literacy. Robust standard errors, clustered at the county level, are in brackets. Stars indicate probability values: *** =p<0.01, ** = p<0.05, * = p<0.10.

Table 3: The Effects of Rosenwald Exposure on the Fertility of Younger Cohorts

	1930						Placebo, 1900		
	(1)	(2)	(3)	(4)	(5)	(6)	(4)	(5)	(6)
Sample	18-22 year olds			20-22 year olds			20-22 year olds		
	Overall Fertility	Extensive Margin	Intensive Margin	Overall Fertility	Extensive Margin	Intensive Margin	Overall Fertility	Extensive Margin	Intensive Margin
$\gamma 0$	0.133***	0.044*	0.088	0.015	0.004	-0.092	0.371**	0.136	0.302
	[0.036]	[0.026]	[0.102]	[0.084]	[0.052]	[0.157]	[0.151]	[0.089]	[0.314]
$\gamma 1$	0.122	0.075*	0.351	0.396*	0.138	1.015	-0.278	-0.141	-0.166
	[0.075]	[0.041]	[0.388]	[0.225]	[0.091]	[0.649]	[0.198]	[0.115]	[0.605]
$\gamma 2$	-0.136***	-0.039	-0.070	0.043	0.041	0.092	-0.383**	-0.162*	-0.222
	[0.046]	[0.031]	[0.109]	[0.106]	[0.062]	[0.180]	[0.164]	[0.098]	[0.323]
Preferred Estimator									
	Triple Difference								
B-W Rural - B-W Urban	-0.251***	-0.067	-0.850**	-0.740***	-0.205*	-1.793***	0.157	0.143	-0.082
($\gamma 3$)	[0.092]	[0.052]	[0.408]	[0.265]	[0.117]	[0.689]	[0.257]	[0.150]	[0.612]
Alternative Estimators									
	Difference in Difference								
Black, Rural-Urban	-0.387***	-0.106**	-0.920**	-0.697***	-0.164	-1.701**	-0.226	-0.019	-0.305
($\gamma 2 + \gamma 3$)	[0.086]	[0.044]	[0.399]	[0.247]	[0.100]	[0.689]	[0.202]	[0.120]	[0.462]
B-W Rural	-0.129**	0.008	-0.500***	-0.344**	-0.067	-0.778***	-0.121	0.003	-0.248
($\gamma 1 + \gamma 3$)	[0.059]	[0.034]	[0.146]	[0.147]	[0.073]	[0.293]	[0.159]	[0.082]	[0.240]
	Undifferenced Effect of Exposure								
Rural black	-0.132**	0.013	-0.482***	-0.286**	-0.022	-0.778***	-0.133	-0.023	-0.168
($\gamma 0 + \gamma 1 + \gamma 2 + \gamma 3$)	[0.056]	[0.033]	[0.136]	[0.144]	[0.072]	[0.276]	[0.152]	[0.076]	[0.225]
N	101062	101062	21669	59231	59231	16450	36449	36449	11401
R2	0.069	0.062	0.075	0.038	0.036	0.050	0.051	0.043	0.043

Columns (1)-(6): The full sample includes women 18-22 years old from the 1930 1 and 5% IPUMS. Columns (7)-(9): The sample includes women 20-22 years old from the 1900 5% IPUMS. The table displays coefficient estimates from a regression of the indicated fertility measure on the own age 7 to 13 exposure variable described in the text. All specifications include race and rural dummies and their interaction, age dummies, and state fixed effects. Robust standard errors, clustered by county, are in brackets. Stars indicate probability values: *** = p<0.01, ** = p<0.05, * = p<0.10.

Table 4: Marriage Rates, Marital and Extramarital Fertility Among the Younger Cohorts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	18-22 years old						
	All	Married			Unmarried		
	Prob of Marriage	Overall Fertility	Extensive Margin	Intensive Margin	Overall Fertility	Extensive Margin	Intensive Margin
γ_0	0.065	0.007	-0.069	0.088	0.004	0.000	-0.145
	[0.043]	[0.083]	[0.061]	[0.102]	[0.005]	[0.003]	[2.222]
γ_1	-0.052	0.391**	0.202*	0.323	-0.025	-0.015*	0.850
	[0.055]	[0.199]	[0.107]	[0.408]	[0.017]	[0.009]	[2.494]
γ_2	-0.001	-0.038	0.059	-0.068	0.003	0.004	-0.057
	[0.045]	[0.093]	[0.066]	[0.109]	[0.006]	[0.003]	[2.673]
Preferred Estimator							
	Triple Difference						
γ_3	-0.081	-0.416*	-0.069	-0.824*	-0.010	0.002	-0.902
(B-W Rur - B-W Urb)	[0.069]	[0.231]	[0.129]	[0.431]	[0.025]	[0.013]	[2.872]
Alternative Estimators							
	Difference in Difference						
Black, Rural-Urban	-0.082	-0.454**	-0.010	-0.893**	-0.008	0.005	-0.959
$(\gamma_2 + \gamma_3)$	[0.062]	[0.214]	[0.103]	[0.417]	[0.025]	[0.013]	[1.218]
B-W Rural	-0.133***	-0.024	0.133*	-0.502***	-0.035**	-0.014	-0.051
$(\gamma_1 + \gamma_3)$	[0.047]	[0.135]	[0.076]	[0.149]	[0.018]	[0.009]	[1.317]
	Undifferenced Effect of Exposure						
Effect on Rural Blacks	-0.069	-0.056	0.124*	-0.482***	-0.028	-0.010	-0.254
$(\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3)$	[0.044]	[0.128]	[0.074]	[0.136]	[0.017]	[0.009]	[0.653]
N	100992	46255	46255	21370	54737	54737	299
R^2	0.066	0.066	0.054	0.075	0.007	0.008	0.141

The estimates are based on the same specification as that used in table 3. For details, refer to the notes in that table. Robust standard errors, clustered by county, are in brackets. Stars indicate probability values: *** = p<0.01, ** = p<0.05, * = p<0.10.

Table 5: The Effect of the Rosenwald Schools Initiative on Occupational Score, by Cohort

	(1)	(2)	(3)
	Ages 18 to 22	Ages 20 to 22	Ages 25 to 49
γ_0	0.393***	0.387***	-0.016
	[0.095]	[0.136]	[0.042]
γ_1	-0.071	-0.198	0.092
	[0.132]	[0.193]	[0.056]
γ_2	-0.640***	-0.765***	0.048
	[0.156]	[0.222]	[0.057]
<u>Preferred Estimator</u>			
		Triple Difference	
γ_3	0.460**	0.826***	-0.107
(B-W Rur - B-W Urb)	[0.213]	[0.293]	[0.076]
<u>Alternative Estimators</u>			
		Difference in Difference	
Black, Rural-Urban	-0.180	0.061	-0.060
$(\gamma_2 + \gamma_3)$	[0.170]	[0.235]	[0.052]
B-W Rural	0.389**	0.628***	-0.016
$(\gamma_1 + \gamma_3)$	[0.164]	[0.241]	[0.065]
		Undifferenced Effect of Exposure	
Effect on Rural Blacks	0.142	0.249	0.016
$(\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3)$	[0.116]	[0.184]	[0.051]
N	29603	18491	98549
R^2	0.412	0.407	0.419

The table displays coefficient estimates from a regression of log(occupational score) on Rosenwald exposure. The first two columns use age 7 to 13 Rosenwald exposure, the third column uses average exposure over the previous decade. The specification for column 1 and 2 mirror that used in table 3. The column 3 specification is the same as that used in table 2. For details refer to the notes in those table. Robust standard errors, clustered by county where appropriate, are in brackets. Stars indicate probability values: *** = p<0.01, ** = p<0.05, * = p<0.10.

Figure 1

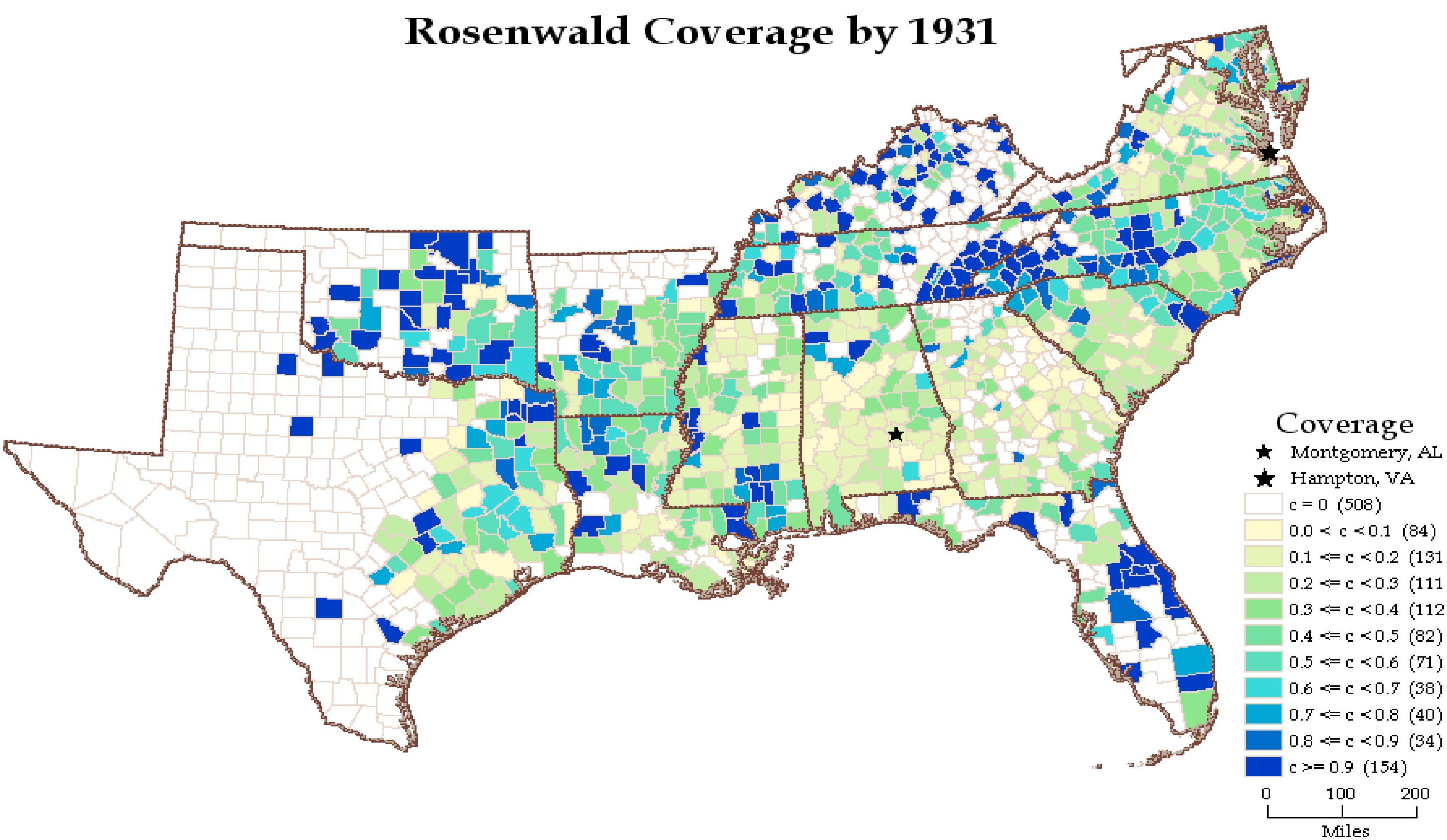

Rosenwald Coverage by 1931
Coverage
Montgomery, AL
Hampton, VA
c = 0 (508)
0.0 < c < 0.1 (84)
0.1 <= c < 0.2 (131)
0.2 <= c < 0.3 (111)
0.3 <= c < 0.4 (112)
0.4 <= c < 0.5 (82)
0.5 <= c < 0.6 (71)
0.6 <= c < 0.7 (38)
0.7 <= c < 0.8 (40)
0.8 <= c < 0.9 (34)
c >= 0.9 (154)
0
100
200
Miles

Figure 2
Distribution of Number of Children Ever Born, Black Married Southern Women, aged 40-44

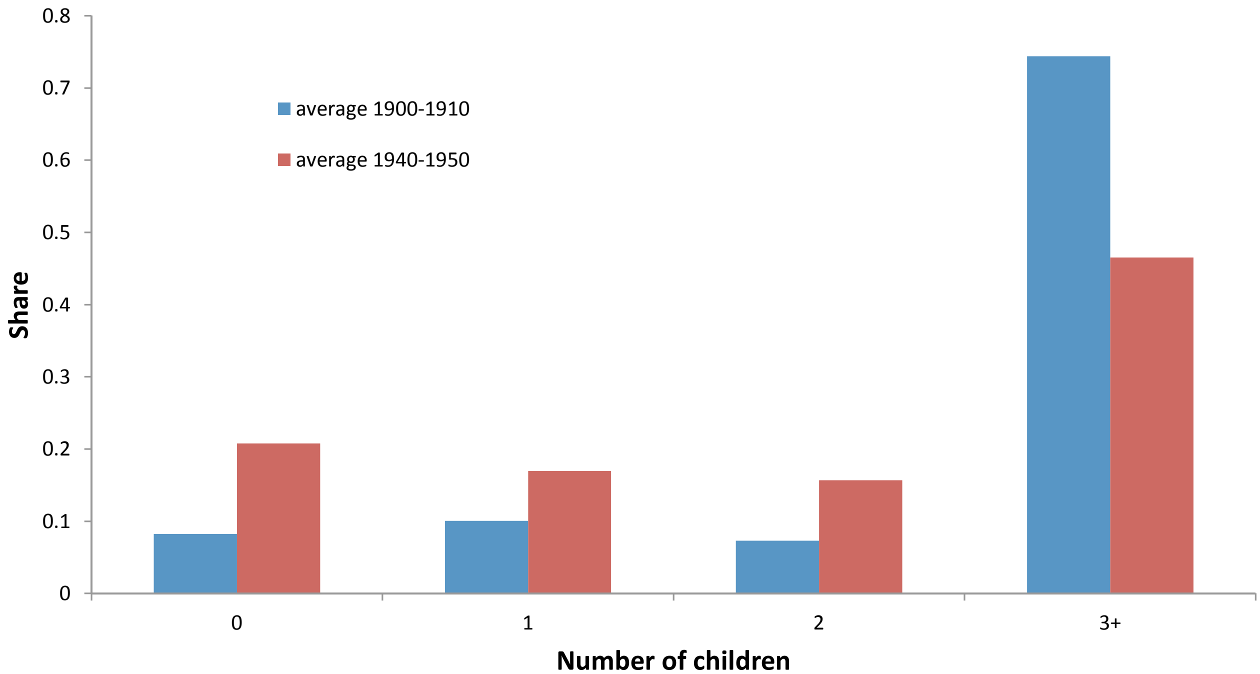
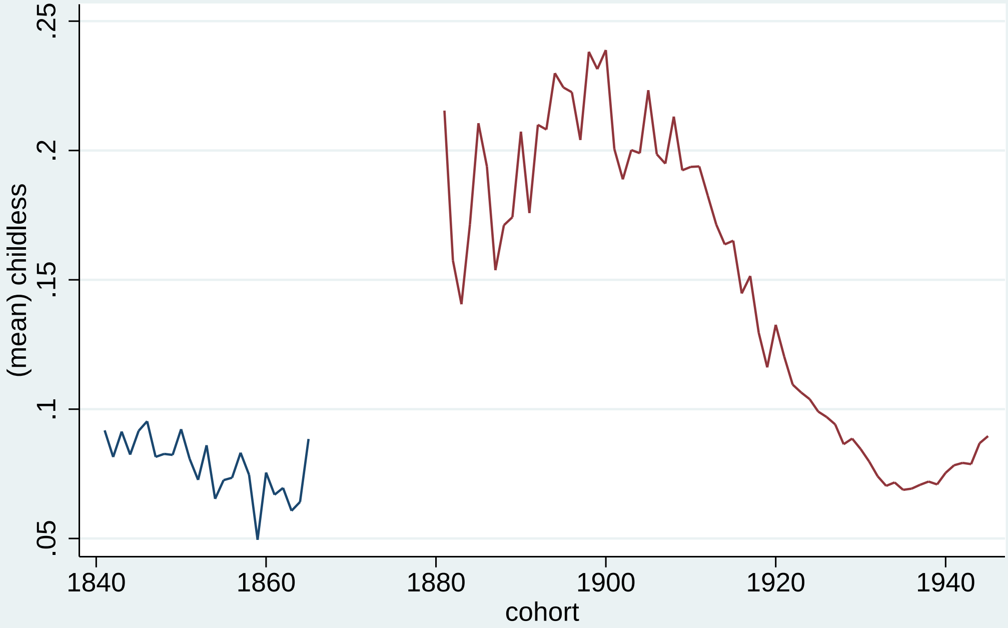


Fig. 2: Childlessness by Cohort

American South 1840-1945, Ever Married



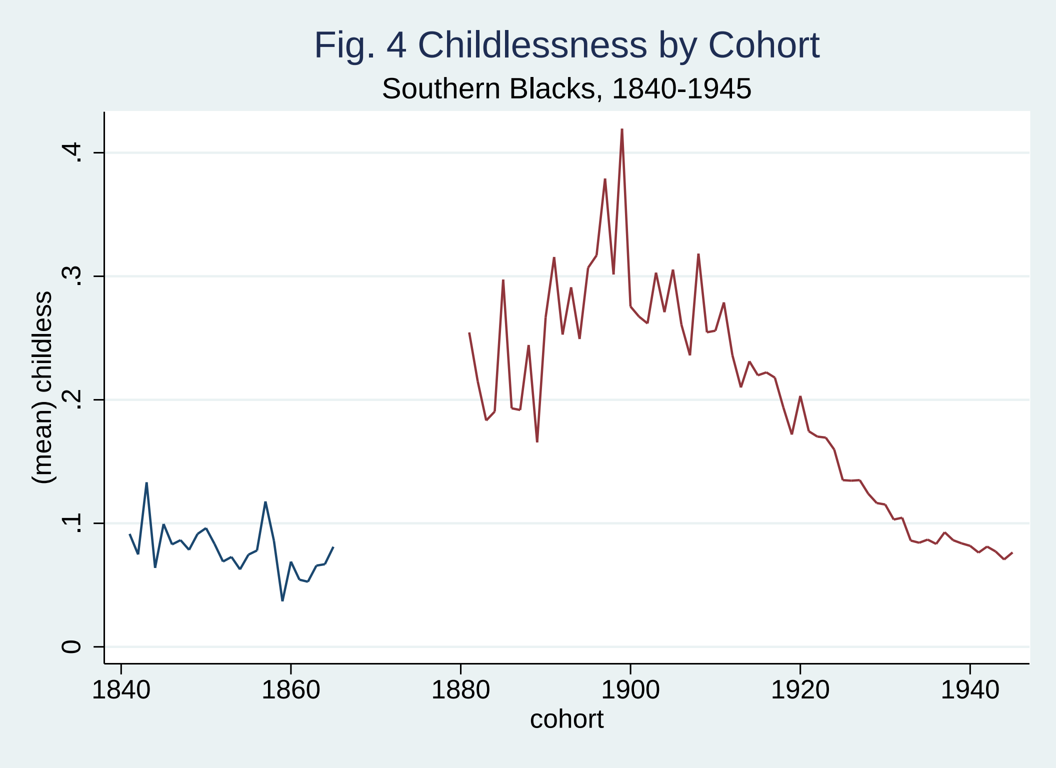


Fig. 4 Childlessness by Cohort
Southern Blacks, 1840-1945
(mean) childless
0
.1
.2
.3
.4
1840
1860
1880
1900
1920
1940
cohort