Fertility Transitions Along the Extensive and Intensive Margins

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Abstract

By allowing for an extensive margin in the standard quantity-quality model, we generate new insights into fertility transitions. We test the model on Southern black women affected by a large-scale school construction program. Consistent with our model, women facing improved schooling opportunities for their children were 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 an increase in schooling and decline 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 the standard implementation of the workhorse model, the quantity-quality model of Becker and Lewis (1973), does not generate distinct predictions for the different proposed causes of the fertility transition. 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 at least one 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 decline in the costs of education causes fertility to decline.

Next, consider an increase in the cost of raising children, say because labor

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

market conditions for women improve and women's opportunity cost of time increase. 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 fertility along the extensive margin varies with different variables. We can use these predictions 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 in response to a large-scale school-building program in the American South.[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 us to form 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 Rosenwald schools were built but were 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 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

[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 distinguish between the extensive and intensive fertility margins. 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).

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.

We use a second sample of women who were of school-going age at the time the schools were built to provide insights into how changing opportunity costs of women's time factored into the fertility transition. Aaronson and Mazumder (2011) document the substantial impact the schools had on the human capital accumulated by 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 (i) less likely to have children, (ii) less likely to have larger families if they had children, and (iii) more likely to work in a higher paying occupation. These effects are quantitatively meaningful, statistically significant, and consistent with the hypothesis that the per-child time cost of child rearing increases with the education and work opportunities of mothers. Therefore, the evidence from the Rosenwald-educated women suggests that providing greater schooling opportunities to girls will reduce their fertility along both the intensive and extensive margin.

As we argue below, the literature's focus on the intensive margin has been in part for analytical convenience. However, childlessness is also often viewed as rare and therefore not economically relevant, especially for societies undergoing demographic transitions. At least for the demographic transition in the US during the late 19th century and early 20th century, this belief is mistaken.

Figure 1 presents estimates, by birth cohort, of the fraction of women aged 45 to 59 who never gave birth.[3] Around 10 percent of Southern black women

[3]The number of children ever born was asked in the 1900, 1910, and 1940 to 1990 Censuses. We select a lower bound age of 45 under the assumption that fertility is essentially complete by 45. We set an upper age limit because of concern that differential mortality by completed fertility might bias our estimates if women older than 59 are included. Prior to 1970, the fertility question was only asked of women who were ever married. We impute

born during the mid-19 century remained childless throughout their lives. But among women born toward the end of the 19th and early 20th century, childlessness was a wide-spread phenomenon; roughly 25% of these women never had children. After World War I, childlessness began to recede, reaching mid-19th century levels within three decades.[4] Boyd (1989) and Morgan (1991) explore reasons for the surge in childlessness around the turn of the century. 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. Indeed, in many Western countries, widespread childlessness was a common phenomenon during the latter half of the 19th and beginning of the 20th century (Rowland 2007). Today, according to data from the Demographic Health Surveys, childlessness rates stand between 5 and 10% in many developing countries. Moreover, those countries with a higher share of childless women also have lower fertility along the intensive margin, consistent with our results from the younger Rosenwald-educated cohort of women.

That said, although the preponderance of our empirical evidence supports an extension of the standard quantity-quality model to allow for essential complementarity along the extensive margin, we acknowledge that, in some cases, our results are mixed or not as precisely estimated as one might like. Further, there are some potential confounding factors such as migration that we may not be able to fully address. Therefore, we think it would be particularly useful if future work explores our findings in other environments. The most useful such work will rely on exogenous variation or interventions that are predicted to generate differential responses in fertility along both the intensive and exten-

non-marital fertility for earlier years using non-marital fertility rates in 1970. For estimates based on a similar approach but using only the 1900 and 1910 Census, see Morgan (1991).

[4]Rural status was not asked in all censuses, and we can therefore not construct the same consistent time-series for rural Southern black women, the target population of the Rosenwald intervention. However, for Southern blacks overall, the level of childlessness is a bit lower than for the North throughout but the temporal patterns look very similar. For the subset of Censuses for which it is possible to identify rural status, rural Southern black women also exhibited similar trends in childlessness as the rest of the population, although the level is 5 to 10 percentage points lower. Results for these subgroups are available on request.

sive margins.[5] Typically, interventions that lead to differential responses along both margins will increase the value of having children, holding child quality investments constant, and increase the returns to these investments. Examples include health campaigns that address specific child health issues such as the campaign to eradicate Hookworm (Bleakley and Lange 2009) and Malaria (Lucas 2013). Similarly, conditional transfer programs, which are increasingly used to raise school attendance,[6] pay parents for their children's attendance at school and/or participation in health initiatives. They should therefore raise fertility along the extensive margin and lower it along the intensive margin. Moreover, since many of these programs include local randomization, they might yield almost ideal settings for testing the theory.

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.

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:

[5] Some interventions, such as increasing the compulsory schooling age, raise the cost of having children, implying fertility declines along both the extensive and intensive margins. Similarly, regulations against child labor or a decline in the wage of child labor also imply fertility declines along both margins.

[6] Akresh, De Walque, and Kazianga (2013) report that, by 2011, 22 countries in Latin America and the Caribbean implemented or are in the process of initiating programs that impact approximately 135 million beneficiaries. In addition, they report that a "growing number of countries in Asia" implemented conditional cash transfer programs and that "several CCT pilot programs have begun in Kenya, South Africa, Malawi, and Morocco. Schultz (2004), Attanasio, Meghir, and Santiago (2012), and Todd and Wolpin (2006) are among a large number of papers evaluating these programs.

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

Household income I is spent on consuming goods and services c, raising n children, and investing e in the quality of those n children.[7] 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 are:

$$\begin{aligned} p_n &= \tau^q+\tau^e e^* \qquad (2)\\ p_e &= n^*\tau^e. \qquad (3)\end{aligned}$$

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^*; that is, 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

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

on preferences over fertility, ensuring that fertility levels are always positive.[8]

[7] 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.

[8] 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 (2013), who analyzes the dynamics of voluntary childlessness during the demographic transition, and Baudin, de la Croix and Gobbi (2012),

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 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

who consider the relationship between childlessness and education in the U.S. in a modern setting.

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.[9] 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.

3 The Rosenwald Schools

Our empirical work 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.[10] 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.[11] Washington convinced Rosenwald 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, eventually encompassing 14 Southern states.[12] The Rural Schools Initiative closed in 1932, within a year of Julius Rosenwald's death and following a significant decline in the Rosenwald Fund's endowment due to the stock market crash. At that

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

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

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

[12] We exclude Missouri where only four Rosenwald schools were built.

point, 76% of counties with rural black children had a Rosenwald school and 92% of rural black children in the 14 states lived in a county with at least one Rosenwald school. While in most counties the number of schools was insufficient to serve all potential students, capacity had expanded to accommodate roughly 36% of the Southern rural black school-age population.

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. Constructing physical facilities was a deliberate strategy meant to counteract potential expropriation given the fungibility of money and the institutional environment of the time. 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 2 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 interquartile range of student capacity ranged from just under 20 to over 45% by 1932. This substantial across-county variation in access to Rosenwald schools, in concert with variation in the timing of construction between 1913 and 1932, provides the basis of our identification strategy.

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% of construction costs.[13]

[13] The Rosenwald fund contributed roughly 25% 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 non-financial support of local white and black communities, especially in the early years. 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 may have helped procure local white acquiescence, including county education board approval for maintaining

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 (2011) 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 endogenous school location.

We consider the effect of the Rosenwald schools on women who were of childbearing age when the schools were built but were too old to attend the schools themselves ("older cohort"). For this cohort, 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.

We also separately analyze the group of women, born between 1908 and 1912, who were of school-going age when Rosenwald schools were open and of child-bearing age by the 1930 Census ("younger cohort"). For these cohorts, Aaronson and Mazumder (2011) demonstrate that exposure to Rosenwald schools ultimately improved school attendance, increased years of completed education, and raised cognitive ability as measured by military exams. After completing their additional education, these women faced an increase in the opportunity cost of rearing children τ^q, which we predict causes their fertility to fall along both the extensive and intensive margin. In addition, like the older cohort of women, the introduction of the program lowered the cost of access to high-quality schools for their children. 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.

schools post-construction (Donohue, Heckman, and Todd 2002).

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 an early version of the 5% sample for 1930.

For the older cohorts, women from all three IPUMS who were between 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 to 39 in 1920 to 25 to 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, we include women who were between the ages of 18 and 22 in the 1900, 1910 and 1930 IPUMS.[14] A limitation of this part of the analysis is that the 1930 Census comes at the beginning of the childbearing years of the younger cohorts; 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.[15] We limit the

[14] An earlier version of the paper used only the 1930 sample and estimated a cross-sectional regression. We use the age cutoff because few women above the age of 22 in 1930 were exposed to the schools and few women below the age of 18 have children. The results are qualitatively similar if we expand our age range by a year or two in either direction. Note that we exclude 18- to 22-year-old women in the 1920 Census. Virtually none of these women attended Rosenwald schools. However, they would be part of our older cohort sample since they face a reduction in τ^e as a result of Rosenwald school availability in 1920.

[15] 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

analysis to children under 10 because we wish to avoid problems associated with children leaving their parent's household.[16] 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 4.0) and rural whites (5.8 to 4.7). The urban TFR is much lower but also trends downward by a comparable 25% during these two decades.

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

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 one 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 non-biological relationships; and (3) we can drop ambiguous matches.

Using synthetic cohorts grouped by birth cohort, race, and state of birth, we calculated the correlation between our under age 10 fertility measure and completed fertility as reported in the 1940 and 1950 Censuses. For blacks and whites, the (cell-size weighted) correlation is roughly 0.8 and 0.9 respectively.

[16]We found that this situation is rare. In particular, we compared the number of children aged 1-3 living with their mothers in the 1920 Census to the number of children aged 11-13 living with their mothers in the 1930 Census. Among whites, 6.8% of children "slipped" from their mother's household. Among blacks, the slippage rate was roughly 0%. Note that any mortality, of mothers or children, will appear as if children left their parents' household. Moreover, we expect this slippage issue to be less of a concern among our under 10 population.

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. 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[17] relative to the estimated number of rural blacks between the ages of 7 and 13 in the county in each year.[18] 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 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 19% 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

[17] 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.

[18] 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.

in Figure 2.

4.3 The Empirical Specification

The key empirical challenge 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 or the exposure rates[19] 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. We supplemented the analysis in Aaronson and Mazumder (2011) by adding our measures of fertility in the pre-Rosenwald era as additional explanatory variables and also found that they did not appear to be systematically related to Rosenwald exposure rates.[20]

[19]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.

[20]Specifically, to predict school location, we regressed Rosenwald exposure on our measures of pre-Rosenwald external and internal margins of fertility for both blacks and whites and the covariates previously used by Aaronson and Mazumder (2011). The fertility measures are computed as of either 1900, 1910, or the change from 1900 to 1910. We measured Rosenwald exposure in either 1919, 1925, 1931, or the average exposure for 1920 through 1929. For our analysis of the older cohorts, the average exposure over 1920-29 is the source of most of the variation in Rosenwald exposure. For the younger cohorts, the Rosenwald exposure in 1925 and in 1931 is a good proxy for the variation in school exposure. The results suggest that pre-Rosenwald fertility measures do not predict Rosenwald school location. In particular, among the black extensive margin measures, the only positive effect arises when we use the 1900 black extensive margin and the 1925 Rosenwald exposure rate. However, the points estimate flips to negative when the exposure rate is measured in 1919 or 1931 and is always statistically and economically insignificant using the 1910 or the change between

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 statistical model for the older cohort is:

$$
\begin{aligned}
y_{ibct} &= \beta_0 black_i + \beta_1 rural_i + \beta_2 X_i + \beta_3 age_{it} + t + c + && (5)\\
& (\gamma_0 + \gamma_1 black_i + \gamma_2 rural_i + \gamma_3 (black_i * rural_i)) \times E_{tc} + \varepsilon_{ibct} && (6)
\end{aligned}
$$

which relates a fertility outcome y_{ibct} for individual i born in year b living in county c in Census year t to black and rural indicators, controls X_{it}, age, calendar-year dummies (short-hand$t$), county-fixed effects (c), 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 both urban and rural blacks in a county that were unrelated to the introduction of the schools, we can difference out a common "black" effect by using $\widehat{\gamma}_2 + \widehat{\gamma}_3$.[21] A third estimator uses the difference between rural blacks and rural whites in order to

1900 and 1910 black extensive margin measures. Likewise, we find one positive, statistically significant effect among the black intensive margin coefficients. But that result also does not hold up to using alternative measures of the Rosenwald exposure or the black intensive margin measures. Moreover, its worth emphasizing that the effects are never significant when we look at the change in these fertility variables between 1900 and 1910. In contrast to these cross-sectional regressions, our main models include county fixed effects and state by year controls. As in Aaronson and Mazumder (2011), we continue to find that white literacy is a consistent predictor of Rosenwald exposure across various specifications with several significant coefficients that are always of a similar sign. Results are available upon request.

[21] 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.

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 specification for the younger cohort of women. Here we use women who were between the ages of 18 and 22 in 1900, 1910 and 1930. In this case, we modify equation (5) by replacing E_{tc} with E_{bc} (see section 4.2). 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.

The most crucial part of our identification strategy is that we assume that there are no within county group specific trends that could confound our results. Therefore, 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[22] 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 and run a simple cross-sectional model.[23] 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.

[22]The 1890 IPUMS is not available.

[23]In an earlier draft of the paper, our main estimates used a cross-sectional regression of 18-22 year olds in 1930 and we found somewhat stronger results than what are reported here. In response to the referees and editor, we added the 1900 and 1910 data to allow for county fixed effects and state-specific trends. To be conservative for the placebo, we use just a cross-sectional regression with the 1900 data to mimic the stronger results we originally obtained when using the 1930 data.

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. Our preferred triple difference estimator ($\widehat{\gamma}_3$) shows that going from no exposure to complete exposure results in an *increase* of 0.055 children with a standard error of 0.078. The three alternative estimators (black rural - black urban, black rural - white rural, black rural) range from 0.036 to 0.091, though in no case are they statistically significant. On their own, these positively signed point estimates 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 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 5.0 (standard error of 2.6) percentage points which is statistically significant at the 10% level.[24] The effects are very similar (4.5 to 4.9 percentage points) and also statistically significant for the three other estimators. 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 0.100 (0.108) fewer children though the results is not statistically significant. The alternative estimators also are of the same sign and are of a similar magnitude. The black rural-white rural estimator is statistically significant at the 10% level.

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

[24]The baseline 10-year probability of having children among rural blacks is 46.2 percent. See table 1.

stronger for married women, especially along the extensive margin. Complete exposure to Rosenwald raised the probability of having a child by 7.2 to 9.5 percentage points, depending on the estimator. Along the intensive margin, our preferred ($\widehat{\gamma}_3$) estimator suggests a decline of 0.118 (0.118) children. Once again, the other estimators are of a similar magnitude and are not statistically different from zero with one exception. We see the evidence broadly suggesting that fertility for all women aged 25-49 rose by about 10 to 15% along the extensive margin and fell by around 3% along the intensive margin in response to the availability of higher quality schooling for all rural black children in a county.[25] On balance, the response along the extensive margin 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 distribution 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 all 25-49 year old women.[26] On the extensive margin, our preferred estimate of $\widehat{\gamma}_3$ is very close to zero at 0.006. The other three estimates range from 0.033 to 0.058 and none are statistically significant. That said, we cannot reject that the placebo estimates differ from our main estimates. On the intensive margin, two of the estimators are signed in the wrong direction with one near zero. The other two estimators are negative but the point estimates are one-quarter to one-half as large as the main estimates.

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

[25] Typical Rosenwald exposure in 1930 was roughly one-third, suggesting actual average effects along the extensive and intensive margins should be scaled down by one-third.

[26] Results are similar for married 25-49 year old women.

to invest more heavily into the quality of children. 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, even if our estimates are sometimes insufficiently precise to warrant strong conclusions. 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.

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 0.130 (0.091) 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 33% of the mean fertility rate of 0.40 for rural black women in this age group. The point estimates range from 0.01 to -0.42 across alternative estimators but only the black rural - black urban difference is statistically significant.

In columns (2) and (3), we find some evidence that is consistent with the notion 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 relatively large negative effects of school exposure on fertility and two are statistically significant at the 5% level and an additional one is statistically significant at the 10% level. The triple difference estimator which is not statistically significant suggests that full exposure leads to a 0.451 (0.337) 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 but small and 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. But 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 the intensive margins and somewhat larger effects on total fertility. For example, the triple difference estimator suggests that complete exposure to Rosenwald schools on average leads to about one-third fewer children for women in this age group (or 0.11 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 generally small and statistically insignificant and of mixed sign. Along the intensive margin, the point estimates tend to be negative but are much smaller than the column 6 estimates. We see little evidence in these exercises that longer-running trends in fertility are confounding our Rosenwald school findings.

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.[27] 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.[28] For Rosenwald-only states, the adjusted correlation is 0.81. Therefore, we view our measure of fertility as a useful proxy for completed fertility.

5.3 Marriage

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 lowered the marriage

[27] While in principle it may be possible to use the 1940 Census geographic data to consider fertility for these women up to age 32, Aaronson and Mazumder (2011) showed that former students aged 17 to 22 who were themselves exposed to Rosenwald schools had dramatically higher migration rates between 1935 and 1940, creating more potential for selection problems.

[28] 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 $corr(\phi_g^y, \phi_g^o) = \frac{\widehat{cov}\left(f_g^y, f_g^0\right)}{\left(\widehat{var}(f_g^y) - \frac{1}{G}\sum_g\left(\frac{1}{N_g^y}\widehat{s}^2_{y,i\in g}\right)\right)^{1/2}\left(\widehat{var}(f_p^y) - \frac{1}{G}\sum_g\left(\frac{1}{N_g^y}\widehat{s}^2_{o,i\in g}\right)\right)^{1/2}}$

where $\widehat{s}^2_{y,i\in g} = \frac{1}{N_g^y - 1}\sum_g\left(\left(e^y_{i\in g}\right)^2\right)$ 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.

rate by age 22 by between 6.4 and 12.7 percentage points (column 1). Although with one exception they are not statistically significant, these are economically large effects that have a direct impact on overall fertility because of the close connection between marriage and childbearing. Indeed, we observe a decline in fertility exclusively within marriage. Average fertility among married women by age 22 declined by about 0.26 (0.21) children. Again, the (unreported) effects are larger among the more fecund 20—22 population.

5.4 Occupation

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).

5.5 Other Identification Threats

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 evidence that the change in black infant mortality over this time is associated with Rosenwald exposure especially relative to white infant mortality.[29] We also do not find that controlling for infant mortality, when possible, has a discernible effect on our main estimates. We therefore conclude that spurious effects operating through infant mortality, possibly because of the Sheppard-Towner Act, are not driving our findings.

Finally, the "Great Migration" of blacks from the South to the North which began around World War I presents a threat to our identification strategy if there was selective migration based on fertility status that was also correlated with Rosenwald exposure. For example, perhaps women who were living in a Rosenwald county but not planning on having children were more likely to migrate out of the South than women who were planning on having children.[30]

We use two approaches to address this issue. First, we estimate a model of out-migration using women born between 1895 and 1905 who were therefore between 25 and 35 in 1930 and part of our older cohort. Using the 1940 census we estimate the likelihood of moving across states between 1935 and 1940 as a function of Rosenwald exposure over the 1920 to 1935 period.[31] The coefficient on Rosenwald exposure is 0.014 (0.014) for white women and 0.015 (0.017) for black women. So the black-white difference is 0.01 (0.022) or 1 percentage

[29] Although we find a negative association between black infant mortality rates and Rosenwald exposure of -1.07 (0.80), this estimate is statistically insignificant and indistinguishable from that on white infant mortality [-1.63 (0.98)]. We also find no effect of Rosenwald on white or black stillbirth rates. These results are robust to a number of specification and sample selection choices, including using median regression to minimize the effects of outliers, restricting the sample to counties that are heavily or entirely rural, and looking at long time differences.

[30] Aaronson and Mazumder (2011) were able to address endogenous migration by using a sample of men in World War II enlistment records who were matched to their county of birth through the Social Security Administration NUMIDENT file. They found similar effects on human capital outcomes when Rosenwald exposure was measured by county of birth as they did when exposure was based on county of residence. Unfortunately, we have no comparable sample linking women to their county of birth.

[31] Exposure is calculated using State Economic Area (SEA) rather than county since county is unavailable in the 1940 IPUMS.

point with a standard error twice the size. Clearly there is not a statistically or quantitatively meaningful effect of Rosenwald exposure on the likelihood of migration among women of this age range.[32] Unfortunately we cannot conduct an analogous analysis of the 1920-1930 period.

A second approach uses the observation from Black et al (2013) that railroads significantly increased the probability of migration out of the South. We split our samples by whether a county was either below or above the median level of railroad coverage for blacks (0.43).[33] The idea is that this stratifies the sample into two groups, one of which might be more likely to have experienced migration than the other for reasons that are plausibly exogenous to Rosenwald exposure.[34] In principle, if the results are fairly similar between the two samples, we might be more comfortable that our results are not driven by selective migration based on fertility since railroad access would have presumably facilitated selective outmigration. We find that, if anything, the extensive margin effects among the older cohorts are stronger in the counties with lower railroad coverage.[35] The comparison of the intensive margin effects are mixed with no clear differences between the samples. Overall, we think this provides some indirect evidence that our results are robust to out-migration but we acknowledge that this is still an open question that future research might better able to resolve.

6 Discussion

This paper explores what a quantity-quality model augmented by an extensive margin implies for how fertility choices responded to a wholesale change in the

[32]In Aaronson and Mazumder (2011), we also find no migration effect for men and women aged 22 to 29. We do find black migration to the North, but not within the South, increased for 17 to 22 year olds.

[33]We thank Seth Sanders for sharing this data

[34]There is a very low correlation between black railroad coverage and Rosenwald exposure (~0.04) suggesting that the two are largely orthogonal.

[35]For example for our preferred triple difference estimator on the sample of married women, the coefficient is twice as big in the low railroad coverage counties (0.157 with a standard error of 0.066) compared to the high railroad coverage counties (0.077 with a standard error of 0.033).

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. 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.

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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	1.233	0.494	1.420	0.762				
	[1.652]	[1.077]	[1.489]	[1.133]				
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	1.135	0.474	1.339	0.719	0.398	0.209	0.399	0.209
	[1.617]	[1.071]	[1.454]	[1.091]	[0.847]	[0.615]	[0.765]	[0.551]
Fraction with more than one child in last 10 years	0.462	0.234	0.606	0.404				
	[0.499]	[0.424]	[0.489]	[0.491]				
Fraction >=1 child, 1910	0.551	0.299	0.656	0.453				
	[0.497]	[0.458]	[0.475]	[0.498]				
Fraction >=1 child, 1920	0.512	0.258	0.634	0.418				
	[0.50]	[0.438]	[0.482]	[0.493]				
Fraction >=1 child, 1930	0.428	0.221	0.588	0.394	0.232	0.133	0.264	0.152
	[0.495]	[0.415]	[0.492]	[0.489]	[0.422]	[0.339]	[0.441]	[0.359]
Number of Children if >= 1 Child	2.668	2.107	2.344	1.883				
	[1.440]	[1.247]	[1.223]	[1.032]				
#Children if >= 1 child, 1910	2.739	2.028	2.541	2.116				
	[1.433]	[1.161]	[1.260]	[1.139]				
#Children if >= 1 child, 1910	2.624	1.972	2.395	1.917				
	[1.426]	[1.184]	[1.211]	[1.037]				
#Children if >= 1 child, 1930	2.652	2.141	2.278	1.828	1.717	1.580	1.512	1.376
	[1.444]	[1.271]	[1.208]	[0.999]	[0.910]	[0.827]	[0.733]	[0.627]
Rosenwald Measures								
Own Exposure to Rosenwald	0.000	0.000	0.000	0.000	0.074	0.076	0.073	0.063
	[0.000]	[0.000]	[0.000]	[0.000]	[0.103]	[0.121]	[0.132]	[0.118]
Rosenwald Exposure in Last 10 Years	0.131	0.185	0.137	0.151				
	[0.171]	[0.244]	[0.208]	[0.229]				
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	0.190	0.235	0.197	0.200				
	[0.178]	[0.253]	[0.226]	[0.245]				
Other Measures								
Married	0.790	0.655	0.853	0.761	0.495	0.442	0.480	0.394
	[0.407]	[0.475]	[0.354]	[0.426]	[0.50]	[0.497]	[0.50]	[0.489]
Labor Force Status	0.407	0.625	0.121	0.251	0.377	0.541	0.183	0.430
	[0.491]	[0.484]	[0.327]	[0.434]	[0.485]	[0.498]	[0.387]	[0.495]
Literate	0.719	0.846	0.938	0.971	0.875	0.947	0.978	0.994
	[0.450]	[0.361]	[0.242]	[0.167]	[0.331]	[0.224]	[0.147]	[0.079]
Occscore (hundreds of 1950$)	8.014	8.985	16.693	21.680	6.845	9.976	17.322	21.239
	[5.396]	[7.031]	[9.933]	[8.318]	[5.20]	[6.978]	[9.449]	[6.060]
N	63,040	37,287	168,304	104,413	17,220	8,781	38,836	22,612

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All, 25-49			Married, 25-49			Placebo, All, 25-29		
	Total fertility	Extensive margin	Intensive margin	Total fertility	Extensive margin	Intensive margin	Total fertility	Extensive margin	Intensive margin
Rosenwald Exposure ($\gamma 0$)	0.011	0.001	0.044	-0.015	-0.014	0.052	-0.132*	-0.044**	-0.132
	[0.041]	[0.014]	[0.046]	[0.045]	[0.014]	[0.049]	[0.072]	[0.022]	[0.098]
R-Exposure * Black ($\gamma 1$)	-0.019	-0.005	-0.024	-0.027	-0.009	-0.014	0.083	0.027	0.095
	[0.046]	[0.019]	[0.086]	[0.052]	[0.019]	[0.097]	[0.101]	[0.043]	[0.141]
R-Exposure * Rural ($\gamma 2$)	0.035	-0.001	0.009	0.056	0.008	0.008	0.092	0.052*	0.029
	[0.042]	[0.015]	[0.044]	[0.047]	[0.014]	[0.048]	[0.085]	[0.027]	[0.103]
<u>*Preferred Estimator*</u>									
	Triple Difference								
B-W Rural - B-W Urban	0.055	0.050*	-0.100	0.139	0.087***	-0.118	0.045	0.006	-0.026
($\gamma 3$)	[0.078]	[0.026]	[0.108]	[0.087]	[0.027]	[0.118]	[0.143]	[0.050]	[0.169]
<u>*Alternative Estimators*</u>									
	Difference in Difference								
Black, Rural-Urban	0.091	0.049*	-0.091	0.195**	0.095***	-0.110	0.137	0.058	0.003
($\gamma 2 + \gamma 3$)	[0.077]	[0.026]	[0.101]	[0.088]	[0.027]	[0.110]	[0.124]	[0.044]	[0.137]
B-W Rural	0.036	0.045**	-0.124*	0.112	0.078***	-0.132*	0.128	0.033	0.069
($\gamma 1 + \gamma 3$)	[0.071]	[0.023]	[0.074]	[0.078]	[0.024]	[0.074]	[0.113]	[0.035]	[0.101]
	Undifferenced Effect of Exposure								
Rural black	0.083	0.045*	-0.070	0.153*	0.072***	-0.073	0.088	0.041	-0.034
($\gamma 0 + \gamma 1 + \gamma 2 + \gamma 3$)	[0.072]	[0.023]	[0.076]	[0.078]	[0.024]	[0.077]	[0.105]	[0.032]	[0.094]
N	373,044	373,044	182,067	297,252	297,252	173,677	487,285	487,285	291,457
R2	0.137	0.119	0.112	0.160	0.144	0.113	0.114	0.079	0.095

Sample in columns 1 through 6 includes 25-49 year old women from the 1910, 1920 and 1930 IPUMS. Sample in columns 7 through 9 are for a placebo test that uses the 1880, 1900 and 1910 IPUMS. The dependent variables are: 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 by year fixed effects, race by year fixed effects, rural by year fixed effects, race by rural by year fixed effects, age fixed effects 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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample	18-22 year olds			20-22 year olds			Placebo, 20-22 year olds		
	Overall Fertility	Extensive Margin	Intensive Margin	Overall Fertility	Extensive Margin	Intensive Margin	Overall Fertility	Extensive Margin	Intensive Margin
Rosenwald Exposure (γ0)	0.321***	0.125***	0.260**	0.246**	0.104	0.099	0.371**	0.136	0.302
	[0.057]	[0.032]	[0.110]	[0.111]	[0.063]	[0.173]	[0.151]	[0.089]	[0.314]
R-Exposure * Black (γ1)	0.107	0.076*	0.087	0.243	0.100	0.417	-0.278	-0.141	-0.166
	[0.069]	[0.040]	[0.306]	[0.190]	[0.089]	[0.529]	[0.198]	[0.115]	[0.605]
R-Exposure * Rural (γ2)	-0.292***	-0.120***	-0.172	-0.182	-0.069	-0.032	-0.383**	-0.162*	-0.222
	[0.058]	[0.033]	[0.115]	[0.118]	[0.068]	[0.188]	[0.164]	[0.098]	[0.323]
Preferred Estimator									
	Triple Difference								
B-W Rural - B-W Urban	-0.130	-0.021	-0.451	-0.320	-0.072	-0.894	0.157	0.143	-0.082
(γ3)	[0.091]	[0.052]	[0.337]	[0.240]	[0.114]	[0.588]	[0.257]	[0.150]	[0.612]
Alternative Estimators									
	Difference in Difference								
Black, Rural-Urban	-0.422***	-0.141***	-0.623**	-0.502**	-0.141	-0.926	-0.226	-0.019	-0.305
(γ2 + γ3)	[0.087]	[0.046]	[0.317]	[0.222]	[0.096]	[0.565]	[0.202]	[0.120]	[0.462]
B-W Rural	-0.023	0.055	-0.364**	-0.078	0.028	-0.476	-0.121	0.003	-0.248
(γ1 + γ3)	[0.061]	[0.035]	[0.151]	[0.153]	[0.076]	[0.307]	[0.159]	[0.082]	[0.240]
	Undifferenced Effect of Exposure								
Rural black	0.006	0.060*	-0.276*	-0.014	0.063	-0.410	-0.133	-0.023	-0.168
(γ0 + γ1+γ2 + γ3)	[0.059]	[0.032]	[0.147]	[0.152]	[0.077]	[0.290]	[0.152]	[0.076]	[0.225]
N	173,172	173,172	39,724	102,014	102,014	30,485	36,449	36,449	11,401
R2	0.098	0.089	0.112	0.072	0.066	0.094	0.051	0.043	0.043

The full sample includes women 18-22 years old from the 1900, 1910 and 1930 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 county fixed effects, race by year fixed effects, rural by year fixed effects, race by rural by year fixed effects, age fixed effects and state by year 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		
	Probability of Marriage	Overall Fertility	Extensive Margin	Intensive Margin	Overall Fertility	Extensive Margin	Intensive Margin
Rosenwald Exposure (γ0)	0.052	0.041	-0.046	0.112	0.008	0.002	3.136
	[0.043]	[0.078]	[0.054]	[0.095]	[0.005]	[0.003]	[2.646]
R-Exposure * Black (γ1)	-0.042	0.300*	0.189**	0.050	-0.024	-0.016*	-2.002
	[0.059]	[0.163]	[0.094]	[0.319]	[0.017]	[0.009]	[2.699]
R-Exposure * Rural (γ2)	0.011	-0.104	0.015	-0.090	-0.001	0.002	-4.979
	[0.047]	[0.090]	[0.060]	[0.104]	[0.005]	[0.003]	[3.053]
Preferred Estimator							
	Triple Difference						
γ_3	-0.085	-0.259	-0.034	-0.463	-0.008	0.005	3.380
(B-W Rur - B-W Urb)	[0.069]	[0.210]	[0.120]	[0.350]	[0.026]	[0.013]	[3.226]
Alternative Estimators							
	Difference in Difference						
Black, Rural-Urban	-0.073	-0.364*	-0.019	-0.554*	-0.009	0.007	-1.599
$(\gamma_2 + \gamma_3)$	[0.062]	[0.191]	[0.099]	[0.322]	[0.026]	[0.013]	[1.035]
B-W Rural	-0.127***	0.040	0.155**	-0.413***	-0.031	-0.012	1.379
$(\gamma_1 + \gamma_3)$	[0.044]	[0.132]	[0.074]	[0.142]	[0.020]	[0.010]	[1.612]
	Undifferenced Effect of Exposure						
Effect on Rural Blacks	-0.064	-0.023	0.125*	-0.391***	-0.025	-0.008	-0.464
$(\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3)$	[0.043]	[0.126]	[0.071]	[0.131]	[0.020]	[0.010]	[0.652]
N	87,449	39,952	39,952	18,625	47,497	47,497	206
R^2	0.068	0.069	0.056	0.075	0.008	0.009	0.167

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
Rosenwald Exposure ($\gamma 0$)	0.324***	0.323***	0.002
	[0.084]	[0.119]	[0.035]
R-Exposure * Black ($\gamma 1$)	-0.050	-0.200	0.081*
	[0.119]	[0.178]	[0.045]
R-Exposure * Rural ($\gamma 2$)	-0.606***	-0.679***	-0.007
	[0.153]	[0.212]	[0.055]
Preferred Estimator			
		Triple Difference	
γ_3	0.356*	0.598**	-0.086
(B-W Rur - B-W Urb)	[0.197]	[0.295]	[0.078]
Alternative Estimators			
		Difference in Difference	
Black, Rural-Urban	-0.250	-0.081	-0.093
$(\gamma_2 + \gamma_3)$	[0.157]	[0.236]	[0.060]
B-W Rural	0.306*	0.398	-0.005
$(\gamma_1 + \gamma_3)$	[0.161]	[0.249]	[0.067]
		Undifferenced Effect of Exposure	
Effect on Rural Blacks	0.024	0.041	-0.010
$(\gamma_0 + \gamma_1 + \gamma_2 + \gamma_3)$	[0.107]	[0.185]	[0.052]
N	28,155	17,636	96,020
R^2	0.435	0.422	0.428

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.

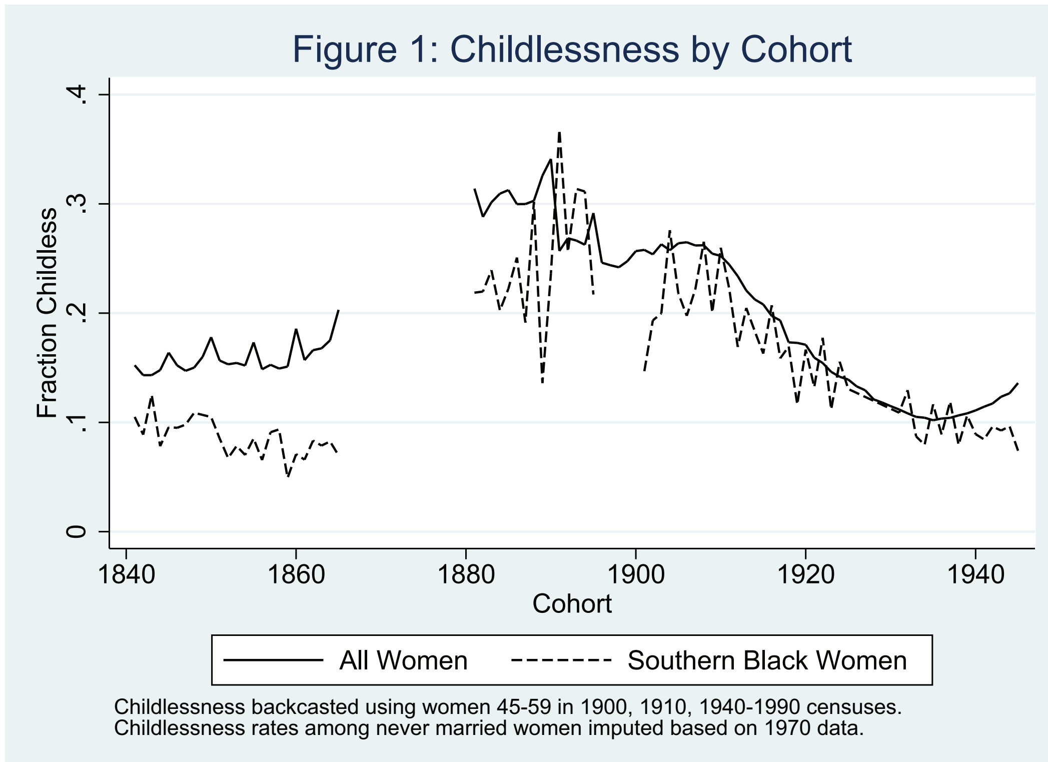
Figure 1: Childlessness by Cohort
Fraction Childless
0
.1
.2
.3
.4
1840
1860
1880
1900
1920
1940
Cohort
All Women
Southern Black Women
Childlessness backcasted using women 45-59 in 1900, 1910, 1940-1990 censuses.
Childlessness rates among never married women imputed based on 1970 data.

Figure 2
Coverage of Rosenwald Schools, by County, 1919

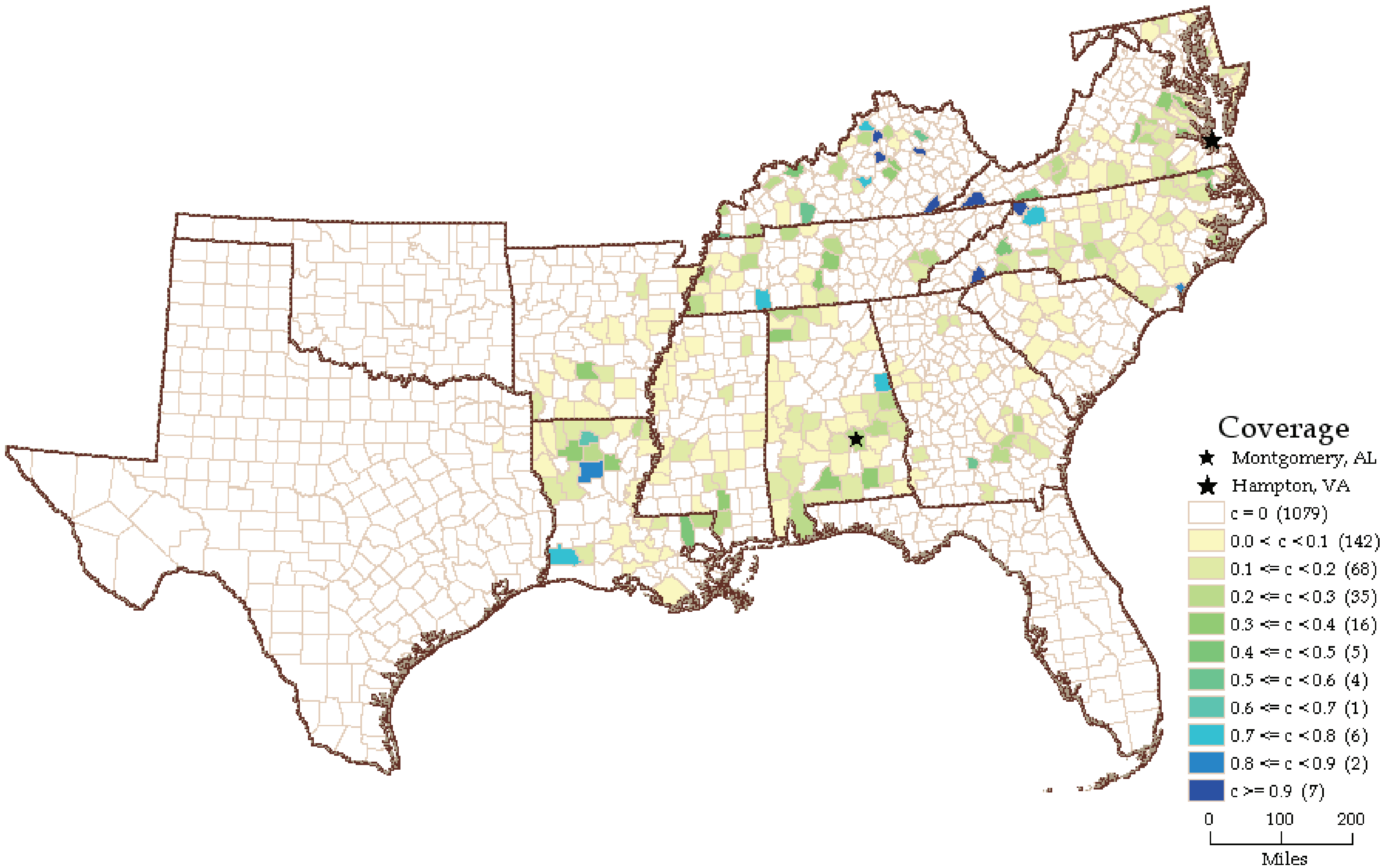

Figure 2
Coverage of Rosenwald Schools, by County, 1931