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Federal Reserve Bank of Chicago

**Fertility Transitions Along the
Extensive and Intensive Margins**

*Daniel Aaronson, Fabian Lange, and
Bhashkar Mazumder*

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1 Introduction

During the development process, societies eventually experience sustained declines in fertility from the high rates associated with the Malthusian equilibrium to those close to or below replacement typically associated with highly educated, wealthy populations. Central to the large literature that models this fertility transition¹ is Becker and Lewis' (1973) insight that parents consider the number of offspring jointly with investments into their children's human capital. While this quantity-quality trade-off plays a critical role in generating a switch in fertility behavior, there is no consensus on the underlying economic forces that drive fertility transitions. Prominent candidates include longer longevity and lower child mortality, skill-biased technical change, child labor laws, and opportunity costs of mothers' time. However, distinguishing between these explanations has proven challenging.

This paper examines the fertility transition through a new lens: the decision to not have children. Researchers examining candidate explanations typically introduce assumptions on preferences that ensure fertility is positive, allowing the application of marginal arguments. This approach, while convenient, ignores the option to not have children. We argue that this ex-

¹The more general "demographic transition" also encompasses the simultaneous change in mortality and age-structure of societies. Examples of models of the demographic and economic transition include Becker, Murphy, and Tamura (1990), Doepke (2004), Soares (2005) and many others. Particularly important are the contributions unifying growth and demographic change by Galor and Weil (1996, 1999, 2000). An incomplete list of empirical examinations of the quantity-quality trade-off includes Rosenzweig and Wolpin (1980), Bleakley and Lange (2009), Becker, Cinnirella, and Woessmann (2010), and Angrist, Lavy, and Schlosser (2010).

tensive fertility margin provides additional identifying information on the forces and mechanisms that shape the fertility transition.

The value of incorporating an extensive margin into standard models of fertility arises because the quantity-quality trade-off does not apply to a first child. Indeed, it is necessary to have at least one child in order to invest in the quality of children. When the price of quality declines, one child becomes more attractive relative to the option of remaining childless. Consequently, fertility along the extensive margin increases as the opportunities to invest into child quality expand. We refer to this complementarity around the extensive margin as "essential complementarity."

Essential complementarity can coexist with substitution between quantity and quality at higher levels of fertility. However, it implies that the response to changes in the constraints that agents face will differ at low levels of fertility. These distinctive behavioral responses allow us to narrow the candidate explanations for the fertility transition to those that are consistent with the observed empirical variation along both the extensive and the intensive margin.²

Empirically, fertility declines along both the extensive and the intensive margin during the transition. Figure 1 uses all developing countries in the Demographic and Health Surveys (DHS) database to plot the levels (Panel

²To be clear, the point of our paper is not that the extensive margin is necessarily quantitatively as important as the intensive margin. Indeed, in most cases, it is not. Rather, we argue that fertility choices along the extensive margin provide further information to narrow the focus of the search for the fundamental causes of fertility transitions.

A) and the changes (Panel B) in the number of children among 40-44 year old women with offspring against the change in the share of childless 40-44 year old women.³ In all of these countries, the norm is to have at least one child by age 40-44. Nevertheless, the share of childless women generally rises as fertility declines.

Figure 1 here

This pattern also arises in the American South during the early twentieth century, the provenance of our main empirical application. Figure 2a and 2b plot the change in completed fertility for black and white southern women aged 40 to 44 between 1900-1910 and 1940-1950.

Figures 2 a and b here

As in Figure 1, the decline in large family sizes over the transition is accompanied by an increase in the share of women who choose to remain childless.

To understand the implications of these patterns in the data, we add an extensive margin to the model Galor (2010) uses to organize the empirical evidence on the demographic transition. Based on the comparative statics of this model, we rule out uncausal explanations of the fertility transition relying exclusively on improvements in life-expectancy, as well as declines in

³Figure 1 uses the first and the last surveys available for each country in the DHS. Panel B limits itself to countries for which these surveys are at least 10 years apart.

the cost of education, increases in the preferences for education, and skill-biased technical change explanations that imply that wages for all off-spring increase with time.⁴ Each of these fail to predict the empirically observed declines in fertility along both the intensive and extensive margins. For instance, consider an exogenous increase in longevity that raises the returns to investing into child human capital. The quantity-quality model suggests that exogenous increases in longevity lead parents to substitute away from having many children towards having fewer, better educated kin. Following this reasoning, increases in longevity have been repeatedly proposed as driving (Meltzer 1992) or contributing (Galor 1999) to the fertility transition. However, all else equal, the life-opportunities of children expand as the mortality rate declines, implying that the option of having children becomes more attractive compared to the alternative of remaining childless. Consequently, we expect that fewer women will remain childless as longevity increases. Similar reasoning implies that fewer women should remain childless as the costs of education decline or preferences for education increase.

We present new empirical evidence from a large-scale school building campaign targeted towards rural blacks in the American South between 1913 and 1932, the Rosenwald Rural Schools Initiative (Aaronson and Mazumder 2011). From the decennial Censuses, we build a sample of women that went

⁴Interestingly, Galor and Weil (2000) and Galor (2010) present a model whereby accelerating technical change reduces the overall life-chances of individuals holding education constant while at the same time raising the returns to education. This mechanism is consistent with declining fertility both along the intensive and the extensive margin.

through their childbearing years during this period, allowing us to compare changes in fertility along both the intensive and extensive margins as schooling opportunities expanded. Consistent with the assumption that parents substitute quality for quantity, we show that fertility declined along the intensive margin in response to the Rosenwald Initiative. Crucially for our argument, we also show that the share of rural black women that did not have children over the period we study also declined as schooling opportunities expanded. During this particular episode, the effects along the extensive and the intensive margin roughly cancel each other out. Overall, the evidence from the Rosenwald era support the idea of essential complementarity and models that abstract from the extensive margin will fail to capture the full effect of the change in opportunities on fertility decisions.

An alternative explanation of the transition stresses that the opportunity costs of women's time changes rapidly during the transition. As economies develop, wages rise, and female education improves (Schultz 1985). As the value of female time increases, our model suggests that fertility will decline along both the extensive and the intensive margin. We show that rural black women that were of an age to have gone through Rosenwald schools themselves, were significantly less likely, both statistically and economically, to have children by age 22, when we can last observe them with currently available data. Moreover, the decline in fertility arises along both the intensive and extensive margins. These results are consistent with the hypothesis that the per-child time cost of rearing children increases with the education of

mothers. Therefore, the evidence from the Rosenwald-educated women suggests a strong direct effect of increasing schooling opportunities on fertility.

The paper is organized as follows. Section 2 describes a simple model of the fertility transition based on Galor (2010). The discussion in this section centers around how essential complementarity and the extensive margin provide additional evidence on models of the fertility transition. In section 3, we introduce the Rosenwald School Initiative that provides the empirical evidence presented in section 4. Section 5 concludes.

2 Quantity and Quality as Essential Complements

In this section, we modify a simple model of fertility proposed by Galor (2010) to account for essential complementarity.

Agents trade-off consumption c , the number of children n , and the education e that they endow to their children. Child education is valued since it raises human capital h according to the function $h(e)$. Like Galor, we make the standard assumptions that $h(0) \geq 0$, $\lim_{e \rightarrow \infty} h'(e) = 0$, $h'(e) > 0$, and $h''(e) < 0$.

Choices between consumption, fertility, and education are ranked according to the following preferences:

$$u(c, n, e) = \ln c + \gamma \text{Max} \{ \ln n + \beta \ln h(e), \kappa_i \}. \quad (1)$$

Equation (1) is similar to that proposed in Section 3 of Galor (2010), with the notable difference that we allow agents to set n (and e) to zero. If agents choose not to have children, they obtain the outside option κ_i , which is allowed to differ across individuals.

The budget constraint is given by

$$yn(\tau^q + \tau^e e) + c \leq y \quad (2)$$

where y , c , n , and e are restricted to be non-negative. Full income y is derived from selling a unit of time in the labor market at a rate y . Income is expended on consumption c and on child rearing n . Raising children entails a fixed cost per child τ^q and an additional cost τ^e for every unit of education. The quantity-quality trade-off is evident in the fact that the shadow price of children, $p_n = y(\tau^q + \tau^e e)$, depends on the education choice and the shadow price of education, $p_e = yn\tau^e$, depends on the number of children.

2.1 The Intensive Margin

Galor (2010) shows the following comparative statics hold for the interior solution:

$$\frac{\partial e}{\partial \beta} > 0, \frac{\partial n}{\partial \beta} < 0, \frac{\partial e}{\partial \tau^q} > 0, \frac{\partial n}{\partial \tau^q} < 0, \frac{\partial e}{\partial \tau^e} < 0, \frac{\partial n}{\partial \tau^e} > 0 \text{ if } \partial \ln e / \partial \ln \tau^e > -1.$$

Increased orientation towards child education (β) raises education and lowers fertility because additional education raises the shadow cost of children, p_n . The interaction between quantity and quality also leads parents to increase human capital investments and to lower fertility as the direct fixed cost of children, τ_q , increases. Increases in the time-cost of educating children, τ_e , unambiguously reduces educational attainment but only increases fertility if p_n decreases. If the substitution out of education is not sufficiently strong, fertility declines with τ_e .

Lastly, consider changes in the returns to education, as captured by $m(e; \theta) = \frac{h'(e)}{h(e)}$. We let the parameter θ capture an increase in the return to education in the sense that $\frac{\partial m(e; \theta)}{\partial \theta} > 0$. A straightforward application of the implicit function theorem establishes that education increases and fertility falls in response to an increase in θ .

These results suggest that the increase in education and decline in fertility along the intensive margin observed during the demographic transition are consistent with variation in a number of different parameter, including increases in β , τ^q , and θ and a decrease in τ_e if $\partial \ln e / \partial \ln \tau^e > -1$. Without additional evidence, it is difficult to discern which of these forces are generating the fertility transition.⁵

⁵See Galor (2010) for an insightful discussion of the empirical evidence.

2.2 The Extensive Margin

This additional evidence can come from the extensive fertility margin. To see this argument, note that the value function of an individual without children reduces to:

$$V_0(y, \gamma, \kappa_i) = \ln y + \gamma \kappa_i$$

and she chooses to remain childless if $V_0(y, \gamma, \kappa_i)$ exceeds the maximum value attainable at the interior solution, $V_{n>0}(y, \tau_q, \tau_e, \theta, \beta, \gamma)$. Using the envelope theorem, it is straightforward to show that:

- 1) fewer women remain childless if β increases or the cost parameters τ_q and τ_e decline.⁶
- 2) a change in future orientation γ does not affect the extensive margin.⁷
- 3) there is an ambiguous effect from changes in θ . This is because the sign of $\frac{\partial V_{n>0}}{\partial \theta} = \gamma \beta \frac{1}{h(e, \theta)} \frac{\partial h(e; \theta)}{\partial \theta}$ is determined by whether changes in the return to human capital ($\frac{\partial h(e; \theta)}{\partial \theta}$) raise or lower the value parents place on their offspring. It is only when increases in θ lower the value of children that fertility declines along the extensive margin.

Our key observation is that introducing the extensive margin provides additional information that can inform how one should model the fertility

⁶In particular, $\frac{\partial V_{n>0}}{\partial \beta} > 0$, $\frac{\partial V_{n>0}}{\partial \tau_e} < 0$, and $\frac{\partial V_{n>0}}{\partial \tau_q} < 0$. These three parameters do not affect the value of remaining childless.

⁷This is because $\frac{\partial V_0}{\partial \gamma} = \kappa_i > \ln n + \beta \ln h(e) = \frac{\partial V_{n>0}}{\partial \gamma}$ for women who choose to remain childless and $\frac{\partial V_0}{\partial \gamma} = \kappa_i < \ln n + \beta \ln h(e) = \frac{\partial V_{n>0}}{\partial \gamma}$ for women who choose to have children. In words, the value of remaining childless increases by more for those women who choose not to have children and the opposite is true for those who choose to have children.

transition. For instance, the empirical evidence rules out uncausal explanations based on declining costs of education τ_e , increasing preferences for education β , or increases in returns to education θ that improve life-chances. All of these channels predict declining fertility along the intensive margin but none are consistent with the observed decline along the extensive margin. Viable candidates that are consistent with the evidence along both margins include increases in direct fertility costs τ_q and increases in the returns to education that simultaneously degrade an individuals' life chances. We emphasize that we do not need to obtain causal estimates to obtain these additional restrictions. Instead, the additional restrictions derive from the auxiliary assumption of essential complementarity and the observed time-series and cross-country evidence (e.g. Figures 1 and 2).

2.3 Specific Examples

To highlight the implications of using the extensive margin and the assumption of essential complementarity, we briefly consider three popular explanations of the fertility transition.

First, a number of papers (e.g. Meltzer (1992), Soares (2005), Cervelatti and Sunde (2005), Hazan (2009), among others) examine the role of increases in longevity that are a product of improvements in health conditions. On the intensive margin, a quantity-quality trade-off arises because parents invest more into each child as the horizon in which their investment can be realized is lengthened. But, by the same logic, longer life-expectancy for one's off-

spring also increases the value of having children in the first place, holding educational choices fixed. Thus, if θ_H represents the health environment, $\frac{\partial h(e, \theta_H)}{\partial \theta_H} > 0$ and more women will choose to have children in response to an improvement in health conditions. Consequently, declining mortality does not generate a decline in fertility along the extensive margin, as observed in the data.

A second explanation of the fertility transition relies on how the pace of technological change, denoted by θ_P , affects the returns to education. Galor (2010), for instance, argues that faster technological change raises the returns from education ($\frac{\partial m(e; \theta_P)}{\partial \theta_P} > 0$).⁸ But as the pace of technological change accelerates, educational investments rapidly depreciate ($\frac{\partial h(e; \theta_P)}{\partial \theta_P} < 0$), reducing the value of children to their parents, holding education investments fixed. This explanation is consistent with the decline in fertility along the intensive and extensive margins and thus has the potential to explain both aspects of the fertility transition. It, however, hinges on the assumption about depreciating human capital which stands in contrast to many other contributions, notably Becker et al (1990),⁹ who assume increases in the returns to human capital investment raise the value of children.

A third explanation emphasizes that fertility declines as opportunity costs for women's time increases.¹⁰ It is plausible that either due to custom or biol-

⁸Recall, $m(e, \theta) = \frac{h'(e, \theta)}{h(e, \theta)}$.

⁹The model notation in Becker, Murphy, and Tamura (1990) differs from the Galor (2010) notation used here. We associate the Becker et al parameter A, which governs the productivity of human capital investments, with θ .

¹⁰Indeed, Schultz (1985) finds Swedish fertility declined substantially during the sec-

ogy, the time-burden for raising children falls disproportionately on women. We associate variation in female time costs with τ_q , the parameter governing the fixed costs of rearing a child. As discussed above, increases in τ_q lower fertility along both the intensive and the extensive margin. Consequently, improved female opportunities over time appear to be a viable explanation of declining fertility along both margins. In the empirical work below, we will provide direct evidence from the American South supporting a role for female opportunity costs in the fertility transition.

3 The Rosenwald Rural Schools Initiative

The Rosenwald Rural Schools Initiative was a school construction program targeted at rural blacks in the American South in the early 20th century.¹¹ Six schools built in Alabama between 1912 and 1914 were the genesis of a matching grant program that supported the construction of almost 5,000 schoolhouses across 14 Southern states over the next two decades.

Figure 3 displays the location of Rosenwald schools, as the Initiative ended in 1932. In particular, the map’s colors signify our estimate, explained below, of the fraction of school-age black children in a county that could have been seated in a Rosenwald school. By 1932, coverage was quite significant; over 90

and half of the 19th century as female wages rose in reaction to world agricultural price fluctuations.

¹¹See Aaronson and Mazumder (2011) for more details about the program’s history and activities. Other important sources include McCormick (1934), Donohue, Heckman, and Todd (2002), Ascoli (2006), and Hoffschwelle (2006).

percent of rural black children lived in a county with at least one Rosenwald school and capacity existed to hold roughly 36 percent of the Southern rural black school-age population and 25 percent of all Southern black school-age children. But the extent and timing to which local communities could accommodate children varied noticeably. For example, although Oklahoma was among the last states to be funded by Rosenwald, by 1930 it had the second highest share of rural black coverage. In contrast, although Alabama was the site of dozens of the first Rosenwald schools, by 1930 its Rosenwald coverage was among the lowest. This variation is what is used to pin down the effects of the schools.

However, school location is by no means random. Indeed, the Rosenwald Fund was unambiguous in its requirement that funding be conditional on local support (e.g. McCormick 1934; Hoffschwelle 2006). Matching grants were a mechanism for ensuring this self-reliance. Local blacks and state and county governments provided the majority of the funding, particularly after construction was complete. This arrangement 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. Moreover, families with the highest demand for educational resources may have migrated to the counties where the Rosenwald schools were built. Aaronson and Mazumder (2011) provide a variety of evidence that selection of these sorts is not the basis of the additional human capital acquisition that

was caused by the program (see appendix). In this paper, we follow their primary strategy of introducing county fixed effects and location-specific time trends to combat potential school and family location selection issues.

We surmise that the Rosenwald school's main contribution was twofold: a) to substantially reduce the cost of education, τ^e , and b) to raise the return to education, θ , through higher quality schooling. These conjectures are consistent with the economically significant effects of Rosenwald on school attendance, literacy, completed years of education, and cognitive ability estimated in Aaronson and Mazumder (2011). Supplementary evidence on a potential increase to θ may be the higher rate of return to human capital typically found in developing societies like the rural South in the early 20th century (Psacharopoulos and Patrinos 2004).

These results lead to several predictions. The first set is relevant for women whose children are eligible for Rosenwald schooling. If we associate Rosenwald with a decline in τ^e , the introduction and expansion of Rosenwald schools is expected to increase fertility along the extensive margin and decrease fertility along the intensive margin, as long as the demand for education is sufficiently elastic. Thus the impact on total fertility is dependent on the magnitudes of these offsetting effects. If we associate the initiative with an increase in θ , while making the highly plausible assumption that $\frac{\partial h(e;\theta)}{\partial \theta} > 0$, we arrive at the same conclusion: exposure to Rosenwald should raise fertility along the extensive margin and lower fertility along the intensive margin.

The second set of predictions is relevant for women who attended Rosenwald schools and are old enough to raise children that could attend the schools as well. If we associate the human capital that these women acquired with an increase in τ_q , we expect their fertility to fall along both the intensive and extensive margins. Consequently, there will be an unambiguously negative effect of Rosenwald schools on total fertility among these women.

As we show below, these predictions are broadly consistent with the data.

3.1 Data

Rosenwald Schools

Through an agreement with the caretaker of the Rosenwald Fund’s archives – Fisk University in Nashville, Tennessee – we received digital versions of the index cards used to keep track of the Fund’s 4,972 construction projects. These cards are the only complete database of the individual Rosenwald schools. Each card contains a description of a school, teacher home, or industrial shop, or some combination thereof. Information is limited to the location (state and county), year of construction, school name, number of teachers (or home/shop rooms), number of acres of land, insurance valuation, and construction cost. Room additions, as well as complete destructions due to fire or weather, are recorded in handwriting ex-post and included in our data in the relevant year that they take place. Our analysis uses 4,932 schools with the capacity to hold 13,746 teachers in 888 counties.¹²

¹²Official Rosenwald Fund records tally 4,977 schools in 883 counties. Our database

We compute two measures of an individual's exposure to a Rosenwald school from this database. Define T_{tc} as the ratio of the Fund's count of Rosenwald teachers in a county c in year t times an assumed class size of 45¹³ relative to the estimated number of rural Blacks between the ages of 7 and 17 in the county in each year.¹⁴ The population counts in the denominator are computed from the digitized 100 percent 1920 and 1930 Census manuscript files available through Ancestry.com and interpolated for 1919 and 1921 through 1929. Then, E_{tc} is the 10 year average of T_{tc} between Censuses. This measure, which we refer to as "kids exposure," captures the expanded schooling opportunities that women of child-bearing age expect for their (potential) children based on the Rosenwald school building in their community. Alternatively, E_{bc} is the exposure to Rosenwald schools during the time that the women herself was of school-age. We refer to this measure, which is also used in Aaronson and Mazumder (2011), as "own exposure."

starts with 4,972 index cards. We delete 36 of these cards because the project did not involve a schoolhouse (22 cases), contained missing information on cost or teachers (10 cases), or was never built (4 cases). Additionally, we drop the four Missouri projects. We also exclude county training schools because of uncertainty as to whether they housed students or were used for their original purpose to train teachers. However, this restriction has little bearing on our results.

¹³An 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 Rosenwald Fund documents.

¹⁴We confine our analysis to the effects of exposure during the ages of 7 to 13 because we cannot identify schools built after 1926 that contained high school instruction. However, our results are robust to defining exposure over the ages of 7 to 17.

In a small minority of cases, our exposure measure exceeds 1. In such cases, we topcode values at 1.

Census (1910-1930)

We create two samples of Southern women drawn from the 1910, 1920, and 1930 decennial Censuses using the Integrated Public Use Microdata Series (or IPUMS, see Ruggles et al. 2010).¹⁵ Our first sample includes all women aged 25 to 49 who were too old to have attended Rosenwald schools themselves ("older" cohorts). A second sample aged 15 to 22 in 1930 encompasses women who went to school during the Rosenwald era ("younger" cohorts).¹⁶ Both samples are linked to the Rosenwald exposure measures by county of residence and birth year. Importantly in light of the Rosenwald's targeting of rural areas, we can also distinguish between those living in rural or urban areas within a county using the Census Bureau's definition of whether a local community has a population above or below 2,500 people.

Our primary measures of fertility are based on counts of children aged 10 or under.¹⁷ To test the simplest hypothesis, whether fertility is a substitute

¹⁵We use the 1.4 percent sample for 1910, the 1 percent sample for 1920, and a "6 percent" sample for 1930. The latter combines the publicly available 1 percent IPUMS with an early version of the 5 percent sample, with duplicate observations discarded. Since the 1910 data oversamples certain groups, we utilize sample weights in our main estimates. However, our results are not sensitive to weighting.

¹⁶Women born in 1906-07 (aged 23-24 in 1930) could have been old enough to attend a Rosenwald school. But this was exceedingly rare during the first few years that they were in school (1913 to 1916), especially outside of Alabama. Therefore, we exclude them from both samples.

¹⁷This measure is different than the completed fertility measure in Figures 2a and 2b. That is because, unlike the 1900-10 and 1940-50 Censuses, the 1920 and 1930 Census does not include measures of children ever born. There are two advantages to using 10 year fertility rates in the empirical analysis. First, this variation can be linked more cleanly with temporal and cross-sectional variation in exposure to Rosenwald schools. Second, looking across 10 years of fertility allows us to examine changes at low and high levels of fertility – corresponding to the extensive and the intensive margin.

for quality, we use the number of children less than 10 years old. Indicators for whether women had 0, 1, 2, and 3 or more children over the last 10 years are used to test the auxiliary essential complementarity hypothesis. We also summarize the effects of Rosenwald on the intensive margin using the number of children less than 10 years old conditional on having at least one child. These variables are constructed by merging our sample of women with individuals under 10, via their household ID (serial) and the mother’s ID within the household (pernum for the mother and momloc for the child).¹⁸ Using this procedure, we can perfectly replicate the reported count of children (nchild) in the IPUMS. However, we use our procedure, rather than the nchild variable, because it provides the flexibility to drop ambiguous matches and non-biological relationships.¹⁹ Most critically, we can add our measures to the 1930 5 percent sample, which currently excludes nchild and other fertility indicators.

Summary Statistics

Table 1 presents summary statistics of the fertility and Rosenwald exposure measures for our older (Panel A) and younger (Panel B) cohorts of

¹⁸Momloc is an IPUMS constructed variable meant to identify the mother in the household. The links are summarized in the variable momrule, which is equal to one when there is a clear and convincing mother-child link (i.e. a son/daughter linked to a wife/spouse) and greater than one when there are various ambiguities in the relationship.

¹⁹Starting with the sample of mother-child links in the Census, we make the following restrictions: (1) we only include children where the relationship is unambiguous (momrule=1); (2) we drop those under 10 who are not reported to be a child in the family relationship code (relate); (3) we exclude women who are not household heads or spouses (relate); and (4) we drop stepmothers (stepmom). Our preferred fertility measure lowers the average number of kids per mother from 1.45 to 1.42.

women from each of the 1910-1930 Censuses.

Table 1: Summary Statistics

The 10 year fertility rates vary substantially between race, across urban and rural populations and over time. By multiplying by the constant 3.5, these 10 year fertility rates roughly approximate the better known total fertility rate (TFR). We find rapid declines in this approximated TFR between 1910 and 1930 for rural Blacks (5.3 to 4.0) and rural Whites (5.8 to 4.6) for the older cohorts. Fertility rates among urban populations are much lower, but they also trend downwards during this time-period. Consistent with Figure 2, declining fertility is present at both low and high fertility levels. For instance, the percentage of 25 to 49 year old rural black women without any children during the last 10 years increases from about 45 to 58 percent.

Over this same time period, there is a rapid increase in exposure to Rosenwald schools. Our kids exposure measure rises from 0 in 1910 to roughly 20 percent by 1930 (19.2 percent for rural black women). The own exposure measure also rises for our younger cohort, averaging about 12 percent for 15 to 22 year old rural black women in 1930. Both measures exhibit significant cross-county dispersion in the program's coverage.

4 The Effect of the Rosenwald Schools Initiative

4.1 The Econometric Model

To examine how changes in Rosenwald exposure affect fertility, we rely on the empirical strategy of Aaronson and Mazumder (2011). The basic regression model is:

$$y_{ibct} = f(\text{black}_i, \text{rural}_i, X_{it}, \text{age}_{it}, t, c) + (\gamma_0 + \gamma_1 \text{black}_i + \gamma_2 \text{rural}_i + \gamma_3 (\text{black}_i * \text{rural}_i)) \times E_{tc} + \varepsilon_{ibct} \quad (3)$$

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. The coefficient γ_3 , what we refer to as the triple difference estimator, reflects how y_{ibct} correlates with exposure E_{tc} across counties and time among rural Blacks relative to rural and urban Whites and urban Blacks.

To interpret γ_3 as causal, we need to restrict how the conditional expec-

tation of ε_{ibct} varies with race and rural status and with Rosenwald exposure. In particular, we need to assume that variation in Rosenwald exposure was not driven by variation in the demand for schooling specific to the rural black population after removing variation in the demand specific to both Blacks and to the rural population in each county. In particular, suppose the error term is structured as $\varepsilon_{ict} = \omega_{t,c}^y + \omega_{t,c,Race_i}^y + \omega_{t,c,Rural_i}^y + \omega_{t,c,(Rural_i,Race_i)}^y + \chi_{i,c,t}$, where, by definition, $\chi_{i,c,t}$ is orthogonal to the variables in the conditioning set. By interacting race and urban/rural indicators with E_{tc} , any correlation between Rosenwald exposure and the error terms $\omega_{t,c,Rural_i}^y$ and $\omega_{t,c,Race_i}^y$ is absorbed. This design controls for shocks to the rural economy or to all Blacks in general that happen to coincide with the construction of schools. For identification of γ_3 , our identification assumption is that $\omega_{t,c,(Rural_i,Race_i)}$ is uncorrelated with E_{tc} . That is, there are no unobservable factors that are correlated with our Rosenwald measure that only affect rural Blacks.

We also consider two alternative estimators. The first compares how Rosenwald exposure affected outcomes for rural rather than urban Blacks. Identification here requires $\left(\omega_{t,c,(Rural_i,Black_i)}^y + \omega_{t,c,Rural_i}^y \right)$ to be uncorrelated with E_{tc} . This assumption is weaker than that required for the triple difference in that we do not restrict how unobservables vary among Whites, but it is stronger in that we do not control for variation in unobservables that are common to Blacks and Whites living in rural areas. Likewise, we can estimate how Rosenwald varies across Whites and Blacks conditional on rural status. In this case, identification requires $\left(\omega_{t,c,(Rural_i,Race_i)}^y + \omega_{t,c,Race_i}^y \right)$ to be

orthogonal to the variation in E_{tc} . In the empirical section, we will show the results from all three differencing procedures but our favored specification is the triple difference estimator, γ_3 .

Since the exposure variables can take on values between 0 and 1, we interpret the coefficients in equation (3) as the effect of going from no Rosenwald exposure in one's county to complete exposure.

4.2 Empirical Results

Older cohorts

We begin with our sample of women aged 25 to 49 who were too old to have gone through Rosenwald schools but were of a child-bearing age during the prior decade.

Table 2 here

The top of the table presents the γ coefficients estimated from equation (3) where the dependent variable is the number of children under 10. Columns are differentiated by included covariates. All specifications include a full set of age dummies, fully interacted controls for race and rural/urban status, year dummies, and where applicable separate race and rural trends to accommodate secular changes in fertility. Columns 2 and 3 include county fixed effects and column 3 allows for state-specific time trends. Across specifications, our three key estimators (γ_3 , black rural-urban, and rural black-white) suggest

little consistent evidence that rural black fertility varies in an economically and statistically meaningful way with Rosenwald exposure. For example, the triple differenced estimator, γ_3 , from our most complete specification column (3) is 0.014 (with a standard error of 0.051). Thus, the data do not support the naive hypothesis that expanded education opportunities induced parents to substitute quality for quantity.²⁰

To examine the essential complementarity hypothesis, we estimate linear probability models of the probability of having a specific number of children under 10. By examining the probability of having no children, we can consider how exposure affected fertility behavior at low levels of fertility. The impact on women with high levels of fertility can be found by examining the probability of having 2, 3, or more children within 10 years. As a summary measure on the intensive margin, we also consider the impact of Rosenwald exposure on average 10-year fertility conditional on having at least one child. The evidence from these regressions is presented in Table 3.

Table 3 here

We find support for the essential complementarity hypothesis. The point estimates indicate that moving from no to complete Rosenwald exposure decreases the probability of having no children within the prior decade by 2.7 to 4 percentage points and increases the probability of having one child

²⁰The results are broadly similar if we restrict age to under 45 or use children less than 5 as the main outcome variable.

by a comparable magnitude. Further, the incidence of families with three or more kids falls. Based on γ_3 , the average number of children born to women that gave birth to at least one child (column 5) is estimated to decline by roughly 0.156 (0.085) upon complete exposure to Rosenwald.

The picture that emerges from these patterns is that of a more complicated relationship between opportunities to invest into children’s human capital and fertility. As prices for quality decline, two effects work against each other: quality and quantity complement each other at low levels of fertility but they substitute for each other at high levels. On balance, the evidence from Rosenwald suggests that the improved schooling opportunities did not change overall fertility levels, but children were born into families of a smaller size.

Younger Cohorts

As we discussed in Section 2, as opportunity costs of women increase, the cost of rearing children, τ_q , rises and this channel is likely to reduce fertility along both the extensive and intensive margins. The data from Rosenwald allows us to investigate this hypothesis since rural black girls schooled in counties with higher Rosenwald exposure were more likely to acquire human capital and thus have higher opportunity cost of time as adults.

Table 4 here

Table 4 reports results for Southern women that are 15 to 22 years of age

in 1930, a cohort young enough to have potentially gone through the Rosenwald schools but old enough to be of child-bearing age. The specifications are analogous to those in Table 2 but to contrast the direct price effect with the endowment effect, we use the own exposure measure computed when these women were of school-age (7 to 13).²¹ Since fertility changes rapidly with age, we display results for all women aged 15-22 and separately for 15 to 17, 18 to 20, and 21 to 22 year olds. Table 4 illustrates a drop in fertility, especially among the 21 to 22 year olds. Our triple differenced estimate, γ_3 , suggests that complete exposure to Rosenwald leads to 0.15 (0.07) and 0.96 (0.30) fewer children among 18-20 and 21-22 year olds. The 21-22 year old estimates are not as stable across different differencing strategies, in particular the black-white rural estimate is smaller. However, even there the point estimate is negative and of an economically relevant magnitude.

It is important to emphasize that because we cannot extend the analysis beyond the 1930 Census with current data,²² we cannot determine to what extent our results on the Rosenwald-educated women reflect changes to timing of fertility or completed fertility. However, we find a strong association between fertility at young and old ages. In particular, we constructed a data set of the average number of children under 10 by state of birth, race, and

²¹Still, it must be based on the county of current residence not the county of residence when the woman was in school. To lessen concern about migration, we also report results for women who live in their birth state.

²²It might be possible once later Censuses are released. However, even then, we would expect some attenuation as migration pulls individuals away from the county in which they went to school and potentially in a selective way. That is another reason why we show our results for women who do not leave their state of birth.

birth cohort from the 1900 to 1950 Censuses. The correlation between the fertility of 18-22 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 0.87.²³ For Rosenwald only states, the adjusted correlation is 0.81. Therefore, we view our measure of fertility as a useful proxy for completed fertility.

Table 5 here

Finally, Table 5 presents estimates by specific fertility levels for 18 to 22 year olds. Relying on γ_3 , the fraction of women with 0 children rose and the fraction with three or more children decline as women themselves are exposed to more Rosenwald schooling. The decrease in fertility along the extensive margin stands in contrast to the results in Table 3 for older cohorts and is consistent with the interpretation that own exposure raised the opportunity costs of having children at all levels of fertility. However, similar to Table 4,

²³To 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 $corr(\phi_g^y, \phi_g^o) =$

$$\frac{\widehat{cov}(f_g^y, f_g^o)}{\left(\widehat{var}(f_g^y) - \frac{1}{G} \sum_g \left(\frac{1}{N_g^y} \hat{s}_{y,i \in g}^2\right)\right)^{1/2} \left(\widehat{var}(f_g^o) - \frac{1}{G} \sum_g \left(\frac{1}{N_g^o} \hat{s}_{o,i \in g}^2\right)\right)^{1/2}}$$

where $\hat{s}_{y,i \in g}^2 = \frac{1}{N_g^y - 1} \sum_g \left((e_{i \in g}^y)^2\right)$ 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, $\hat{s}_{o,i \in g}^2$. A derivation is available from the authors upon request.

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

the extensive margin results are smaller and statistically insignificant when comparing rural black and white women.

Overall, the qualitative results of a change in Rosenwald exposure on various fertility measures can be summarized as follows. Rosenwald exposure had no discernible effect on average fertility among the older parent generation (Table 2). However, fertility patterns varied in response to the Rosenwald Initiative in accordance with the predictions from Becker and Lewis (1973) – i.e. fewer women had large numbers of children – and essential complementarity – i.e. fewer women had no children (Table 3). Together, these two effects together implied that children living in areas treated more intensely by the Rosenwald Initiative were, on average, raised in smaller families. Finally, girls growing up in high Rosenwald exposure communities had significantly lower fertility levels as young adults regardless of whether we consider the extensive or the intensive margin (Tables 4 and 5). A plausible explanation for this finding is that the additional resources available to these women during their childhood raised their value of time as young women and thus made it more attractive to postpone having children.

4.3 How much did Rosenwald affect fertility?

To explore the aggregate impact of Rosenwald, we compute how rural black fertility changed in response to Rosenwald exposure rising from 0 to the average kids exposure rate of 19.2 percent in 1930.

Consider first the impact on our older cohort of women that did not attend Rosenwald schools. Using the $\gamma_3 = -0.032$ from column 1 in Table 3, we estimate that the fraction of women that did not have a child in the previous decade declined by about 0.6 percentage points ($-0.032 * 0.192$). Relative to the roughly half of rural black women who did not have children within the last 10 years, this is a modest effect. Moreover, the direction runs counter to the historical trend between 1910 and 1930. On the intensive margin (column 5 of Table 3), we estimate that the number of children among those households with any children under 10 declines by 0.03 or about 30 percent of the 1910-1930 decline for rural black women. The effect, however, is small compared to the average size of 2.6 young children in rural black households. Therefore, while the impact of kids exposure on the intensive margin may be sizable compared to contemporaneous trends, they are moderate compared to average fertility measures of the time. Moreover, the offsetting intensive and extensive effects (aggregated in Table 2) imply that none of the overall decline in Southern fertility can be explained by kids exposure to Rosenwald schools.

The effects are larger once we look at the cohort of rural black women who were of an age to have potentially gone through Rosenwald schools. Using the estimate derived from γ_3 (column 1 of Table 4), we find the number of children born to women aged 15-22 declines by about 1.9 percentage points, corresponding to a drop in fertility of 7.8 percent relative to the average (0.246) number of children born to this age-group in 1930. Using the smaller

estimate derived from the black-white rural comparison, the effect is still almost 3 percent relative to the average family size of this cohort. Note that the fertility effects rise monotonically with age. If we consider the magnitudes for 21-22 year old rural black women (column 4 of Table 4), the estimated fertility decline is particularly sharp, ranging from 7.3 (black-white rural estimate) to 30.6 (γ_3) percent of the average fertility level for 21-22 year olds in 1930.²⁴ We therefore view the fertility effect arising from former Rosenwald students delaying, and potentially reducing, fertility to be the quantitatively more important effect in explaining aggregate fertility declines during this episode.

As a final comparison, we compare our estimates to the education effects reported in Aaronson and Mazumder (2011). They report that complete exposure to Rosenwald in a county increases completed schooling by 1.2 to 1.4 years. Multiplying the mid-range of those estimates by the same 19.2 percent coverage rate we use above implies an increase in education of 1/4 year or 3.5 percent relative to the baseline of 7.1 years of average schooling among Southern rural Blacks. This increase in average education of 3.5% is of roughly the same magnitude as the 7.8% decline in fertility found among 15-22 year olds in response to a similar level of exposure to Rosenwald schools.

²⁴For example, the Black-White rural estimate multiplies the -0.228 fertility estimate in column 4 of Table 4 by average Rosenwald exposure of .192 and then divides that by the average fertility of 21-22 year old rural Black women in 1930 of 0.603.

4.4 Conclusion

This paper examines the fertility transition through a new lens: the extensive margin. Parents with high levels of children might substitute quality for quantity as the constraints on quality relax or those on quantity tighten. However, along the extensive margin, the quantity-quality trade-off cannot operate. At low levels of fertility, we expect quality and quantity to be essential complements.

To derive what essential complementarity implies for explanations of the fertility transition, we adapt Galor’s (2010) fertility model to include an extensive margin. We find that many candidate explanations, including declines in child and infant mortality, improvements in adult longevity, and skill-biased technical change that raises standards of living for all education levels, fail to predict the empirically observed declines in fertility along both the intensive and extensive margin. By contrast, technological change of the type proposed by Galor and Weil (2000) is capable of generating these patterns. Similarly, increased opportunity costs of fertility, maybe due to changes in the value of female time, also generates a decline in fertility along both the extensive and the intensive margin.

Empirically, we consider new evidence from a particular episode in the history of the American South: the Rosenwald School Initiative. The fertility patterns observed in response to this intervention generally support the idea of essential complementarity: we find that increased schooling opportunities lead to reductions in fertility among women with high fertility levels, while

at the same time inducing higher levels of fertility among women with low levels of fertility. The magnitude of the fertility changes induced in the parent generation is however small compared to the changes in fertility induced by the Rosenwald intervention among women that were themselves treated by the intervention. The evidence from the Rosenwald intervention therefore suggests that changes in female opportunity costs induced by increased educational attainment might be among the most important driving forces of the fertility transition.

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6 Appendix

This appendix briefly describes results from Aaronson and Mazumder (2011). They run a series of statistical models similar to equation (3):

$$y_{ibct} = f(\text{black}_i, \text{rural}_i, X_{it}, \text{age}_{it}, t, c) + (\gamma_0 + \gamma_1 \text{black}_i + \gamma_2 \text{rural}_i + \gamma_3 (\text{black}_i * \text{rural}_i)) \times E_{bc} + \varepsilon_{ibct} \quad (4)$$

that likewise take advantage of the explicit targeting of the Rosenwald schools to rural Blacks, while allowing other demographic groups to potentially serve as controls, and that exploit variation in Rosenwald school coverage over time and cohorts to control for unobservable county characteristics. The dependent variable y_{ibct} is measures of human capital acquisition, including school attendance, young adult (aged 15 to 22) literacy, high school attendance and completion, years of completed schooling, cognitive ability measured by a test used to assign occupations during World War II, and migration to the

North. School attendance and literacy regressions are estimated from the 1900 to 1930 Censuses. Years of completed schooling, including the high school measures, and cognitive ability are based on samples of male World War II enlistees obtained from the National Archives and Records Administration. Finally, completed schooling and Northern Migration results are estimated using the 1940 IPUMS.

A version of the school attendance results, stratified by gender, is reproduced in Table A1 (Table 8 of Aaronson and Mazumder). Based on the triple difference estimator γ_3 , we find that going from no Rosenwald schools to full exposure raises female school attendance by 7.5 percentage points (standard error of 1.9 percentage points), nearly identical to the estimated impact on males. In the aggregate, going from no exposure to the mean level of Rosenwald exposure for rural Blacks in 1930 ($E_{bc}=0.27$) raised school attendance of female rural Blacks by about 2 to 3.5 percentage points, depending on the estimator used. We find similar results on literacy, completed years of schooling, and cognitive ability. Across all these outcomes, the effects are sharply higher among students schooled in the most disadvantaged communities, as measured by proclivity to slavery or pre-Rosenwald levels of black schooling. Finally, the results are robust to a series of tests meant to limit selection bias that might arise from a number of sources, including a community's desire to obtain a Rosenwald school, endogenous migration, and nonrandom selection into the military.

Table 1: Summary Statistics: 1910-1930

Panel A: Women aged 25 to 49		1910				1920		
	Rural	Urban	Rural	Urban	Rural	Urban	Rural	Urban
	<u>Black</u>	<u>Black</u>	<u>White</u>	<u>White</u>	<u>Black</u>	<u>Black</u>	<u>White</u>	<u>White</u>
Number of Children < 10 yo	1.509	0.607	1.666	0.959	1.344	0.509	1.519	0.801
Number of Children < 10 yo if N>0	2.739	2.028	2.541	2.116	2.624	1.972	2.395	1.917
% with kids < 10 = 0	0.449	0.701	0.344	0.547	0.488	0.742	0.366	0.582
% with kids < 10 = 1	0.139	0.133	0.166	0.172	0.144	0.122	0.181	0.186
% with kids < 10 = 2	0.125	0.076	0.176	0.132	0.123	0.066	0.185	0.126
% with kids < 10 = 3+	0.287	0.091	0.313	0.148	0.246	0.070	0.269	0.105
Kids Rosenwald exposure	0.000	0.000	0.000	0.000	0.012	0.012	0.012	0.008
Std Deviation of Rosenwald Exposure	[0.000]	[0.000]	[0.000]	[0.000]	[0.026]	[0.036]	[0.037]	[0.030]
% Literate	0.535	0.702	0.858	0.901	0.665	0.787	0.934	0.968
Sample size	12,128	4,650	29,944	14,718	7,699	3,334	21,562	11,052
Panel A. cont'd: Women aged 25 to 49		1930						
	Rural	Urban	Rural	Urban				
	<u>Black</u>	<u>Black</u>	<u>White</u>	<u>White</u>				
Panel A: Women aged 25 to 49								
Number of Children < 10 yo	1.127	0.464	1.321	0.720				
Number of Children < 10 yo if N>0	2.648	2.095	2.270	1.818				
% with kids < 10 = 0	0.575	0.778	0.418	0.604				
% with kids < 10 = 1	0.121	0.096	0.194	0.192				
% with kids < 10 = 2	0.097	0.058	0.169	0.121				
% with kids < 10 = 3+	0.208	0.068	0.219	0.083				
Kids Rosenwald exposure	0.192	0.247	0.199	0.218				
Std Deviation of Rosenwald Exposure	[0.182]	[0.253]	[0.228]	[0.247]				
% Literate	0.784	0.872	0.959	0.986				
Sample size	51,446	31,491	140,163	81,919				

Panel B: Women aged 15 to 22

	1930			
	Rural	Urban	Rural	Urban
	<u>Black</u>	<u>Black</u>	<u>White</u>	<u>White</u>
Number of Children < 10 yo	0.246	0.146	0.239	0.144
Number of Children < 10 yo if N>0	1.677	1.557	1.497	1.373
% with kids < 10 = 0	0.853	0.906	0.840	0.895
% with kids < 10 = 1	0.080	0.057	0.099	0.073
% with kids < 10 = 2	0.042	0.024	0.045	0.025
% with kids < 10 = 3+	0.024	0.012	0.016	0.006
Own Rosenwald Exposure	0.121	0.136	0.125	0.124
Std Deviation of Own Rosenwald Exposure	[0.154]	[0.193]	[0.196]	[0.194]
Kids Rosenwald exposure	0.194	0.243	0.201	0.221
Std Deviation of Rosenwald Exposure	[0.181]	[0.243]	[0.231]	[0.246]
% Literate	0.893	0.955	0.980	0.993
Sample size	36,932	79,668	14,448	34,216

Table 2: The Effect of Rosenwald Exposure on the Number of Children Under 10 Women Aged 25 to 49

	(1)	(2)	(3)
	Basic	Add County Fixed Effects	Add County Fixed Effects and State Time Trends
γ_0	0.017 [0.059]	0.095*** [0.027]	-0.008 [0.030]
γ_1	-0.015 [0.046]	0.026 [0.028]	0.033 [0.028]
γ_2	0.085 [0.053]	0.040 [0.027]	0.043 [0.027]
γ_3 (B-W Rur - B-W Urb)	0.006 [0.092]	0.012 [0.051]	0.014 [0.051]
Black, Rural-Urban ($\gamma_2 + \gamma_3$)	0.091 [0.079]	0.052 [0.047]	0.057 [0.048]
B-W Rural ($\gamma_1 + \gamma_3$)	-0.009 [0.082]	0.038 [0.043]	0.047 [0.043]
N	410,106	410,106	410,106
R ²	0.113	0.133	0.133

Sample includes women **25-49** year old women. All estimated specifications contain race and rural specific trends in addition to full sets of age and year dummies. All specifications control for literacy, which has a strong negative effect on fertility. Columns 2 and 3 include county fixed effects and column 3 includes state-specific time trends. Robust standard errors in brackets. Standard errors are clustered at the county level. Stars indicate probability values:

*** p<0.01, ** p<0.05, * p<0.1

Table 3: The Effect of Rosenwald Exposure on the Specific Number of Kids Under 10 Women Aged 25 to 49

	(1)	(2)	(3)	(4)	(5)
	0 Kids	1 Kid	2 Kids	3+ Kids	# Children if Positive
γ_0	-0.003 [0.011]	-0.005 [0.009]	0.020** [0.008]	-0.012 [0.008]	0.001 [0.038]
γ_1	-0.008 [0.012]	-0.005 [0.009]	-0.000 [0.007]	0.013* [0.007]	0.104 [0.064]
γ_2	0.004 [0.010]	-0.016* [0.008]	-0.008 [0.008]	0.019** [0.007]	0.045 [0.034]
γ_3 (B-W Rur - B-W Urb)	-0.032* [0.018]	0.038*** [0.013]	0.005 [0.011]	-0.012 [0.013]	-0.156* [0.085]
Black, Rural-Urban ($\gamma_2 + \gamma_3$)	-0.027* [0.016]	0.023** [0.011]	-0.002 [0.010]	0.007 [0.012]	-0.111 [0.082]
B-W Rural ($\gamma_1 + \gamma_3$)	-0.04*** [0.014]	0.034*** [0.01]	0.005 [0.009]	0.001 [0.011]	-0.052 [0.057]
N	410,106	410,106	410,106	410,106	200,351
R ²	0.115	0.014	0.025	0.092	0.111

The sample includes **25 to 49** year old women. Columns 1 through 4 display coefficient estimates from linear probability models for having the number of children specified in the column titles within the last 10 years. Column 5 shows the impact of exposure on the number of children among those women with positive fertility levels. The estimated specification is similar to the one in column 3 of Table 2. Robust standard errors in brackets. Standard errors are clustered at the county level. Stars indicate probability values:

*** p<0.01, ** p<0.05, * p<0.1

**Table 4: The Effect of Own Exposure to Rosenwald Schols on the Number of Children Under 10
Women Aged 15 to 22 in 1930**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Residing in State of Birth			
	All	Age 15-17	Age 18-20	Age 21-22	All	Age 15-17	Age 18-20	Age 21-22
γ_0	0.213*** [0.020]	0.072*** [0.023]	-0.038 [0.077]	0.273 [0.379]	0.215*** [0.023]	0.066*** [0.024]	-0.046 [0.085]	0.443 [0.436]
γ_1	0.066*** [0.020]	-0.005 [0.011]	0.040 [0.044]	0.734*** [0.256]	0.056** [0.023]	0.002 [0.012]	-0.009 [0.048]	0.651** [0.317]
γ_2	-0.261*** [0.015]	-0.008 [0.008]	-0.053 [0.040]	0.090 [0.136]	-0.263*** [0.017]	-0.001 [0.009]	-0.069 [0.043]	0.055 [0.164]
γ_3 (B-W Rur - B-W Urb)	-0.101*** [0.028]	-0.008 [0.014]	-0.151** [0.065]	-0.962*** [0.295]	-0.106*** [0.031]	-0.020 [0.016]	-0.093 [0.069]	-0.875** [0.356]
Black, Rural-Urban ($\gamma_2 + \gamma_3$)	-0.362*** [0.027]	-0.016 [0.013]	-0.204*** [0.059]	-0.872*** [0.280]	-0.369*** [0.029]	-0.021 [0.015]	-0.162*** [0.062]	-0.819** [0.328]
B-W Rural ($\gamma_1 + \gamma_3$)	-0.034* [0.020]	-0.013 [0.010]	-0.111** [0.050]	-0.228 [0.173]	-0.050** [0.022]	-0.018* [0.011]	-0.101* [0.053]	-0.224 [0.187]
N	165,264	64,202	62,805	38,257	140,814	56,229	53,272	31,313
R ²	0.135	0.046	0.074	0.092	0.137	0.047	0.078	0.097

The full sample includes women less than 23. The table displays coefficient estimates from a regression of the number of children less than 10 years old on exposure to Rosenwald experienced by women from the indicated age-ranges during their own childhood. Columns 5 through 8 use the sample of women residing in their state of birth. The estimates are obtained using a specification mirroring the one in column 3 of Table 2, but omitting the literacy control. For details refer to the notes in that table. Robust standard errors in brackets. Where appropriate standard errors are clustered at county level. Stars indicate probability values:

*** p<0.01, ** p<0.05, * p<0.1

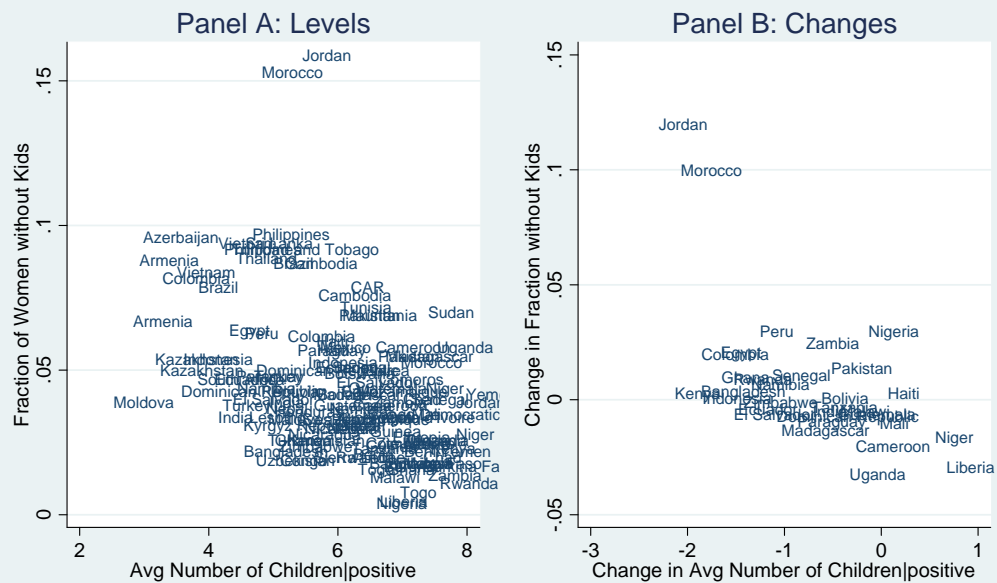
Table 5: The Effect of Own Exposure to Rosenwald Schols on the Specific Number of Children Under 10
Women Aged 15 to 22 in 1930

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	18-20 years old				21-22 years old				21-22 years old, Residing in State of Birth			
	0 Kids	1 Kid	2 Kids	3+ Kids	0 Kids	1 Kid	2 Kids	3+ Kids	0 Kids	1 Kid	2 Kids	3+ Kids
γ_0	0.032 [0.050]	-0.016 [0.043]	-0.030 [0.028]	0.013 [0.013]	-0.150 [0.194]	0.010 [0.157]	0.122 [0.124]	0.018 [0.093]	-0.241 [0.219]	0.029 [0.174]	0.177 [0.142]	0.035 [0.108]
γ_1	-0.029 [0.030]	0.015 [0.026]	0.014 [0.017]	-0.000 [0.005]	-0.238** [0.099]	0.057 [0.072]	0.049 [0.054]	0.131** [0.053]	-0.127 [0.113]	-0.014 [0.074]	-0.009 [0.060]	0.151** [0.066]
γ_2	0.020 [0.027]	0.016 [0.024]	-0.032** [0.014]	-0.004 [0.006]	-0.030 [0.074]	-0.015 [0.059]	0.013 [0.048]	0.031 [0.029]	0.018 [0.086]	-0.052 [0.068]	-0.005 [0.056]	0.040 [0.037]
γ_3 (B-W Rur - B-W Urb)	0.062 [0.043]	-0.024 [0.036]	-0.007 [0.024]	-0.031*** [0.010]	0.279** [0.127]	-0.032 [0.096]	0.008 [0.078]	-0.255*** [0.064]	0.177 [0.142]	0.033 [0.099]	0.064 [0.086]	-0.274*** [0.077]
Black, Rural-Urban ($\gamma_2 + \gamma_3$)	0.082** [0.039]	-0.008 [0.032]	-0.038* [0.022]	-0.036*** [0.009]	0.250** [0.114]	-0.047 [0.083]	0.021 [0.070]	-0.224*** [0.060]	0.195 [0.123]	-0.020 [0.081]	0.059 [0.075]	-0.234*** [0.070]
B-W Rural ($\gamma_1 + \gamma_3$)	0.033 [0.032]	-0.009 [0.028]	0.007 [0.018]	-0.032*** [0.008]	0.042 [0.084]	0.025 [0.065]	0.057 [0.056]	-0.124*** [0.041]	0.050 [0.090]	0.019 [0.069]	0.054 [0.061]	-0.123*** [0.045]
N	62,805	62,805	62,805	62,805	38,257	38,257	38,257	38,257	31,313	31,313	31,313	31,313
R ²	0.074	0.050	0.048	0.038	0.089	0.059	0.064	0.069	0.095	0.068	0.072	0.077

See notes to Table 3. The estimated specification is similar to the ones in that table. Robust standard errors in brackets. Stars indicate probability values:

*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Changes in Fraction Childless vs. Avg. Fertility
 Avg Fertility measured conditional on having at least 1 child



Using the first and the most recent surveys available from DHS

STATA™

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

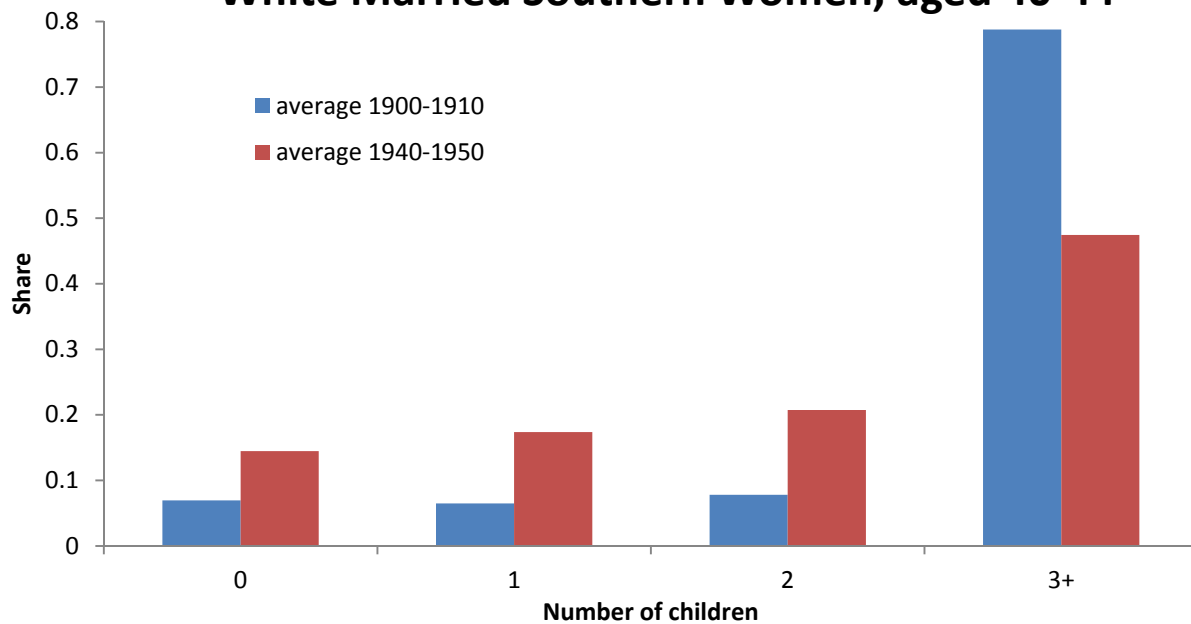


Figure 2b
Distribution of Number of Children Ever Born,
Black Married Southern Women, aged 40-44

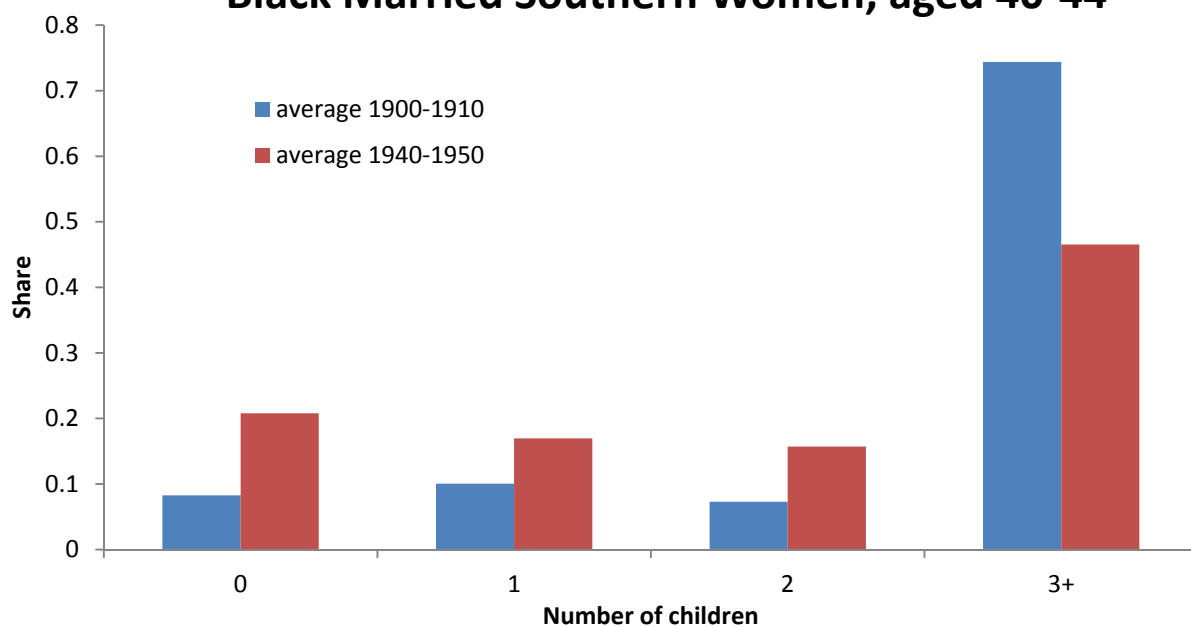


Figure 3: Exposure to Rosenwald Schools, from Aaronson and Mazumder (2011)

Figure 2d: Exposure to Rosenwald Schools, as of 1932

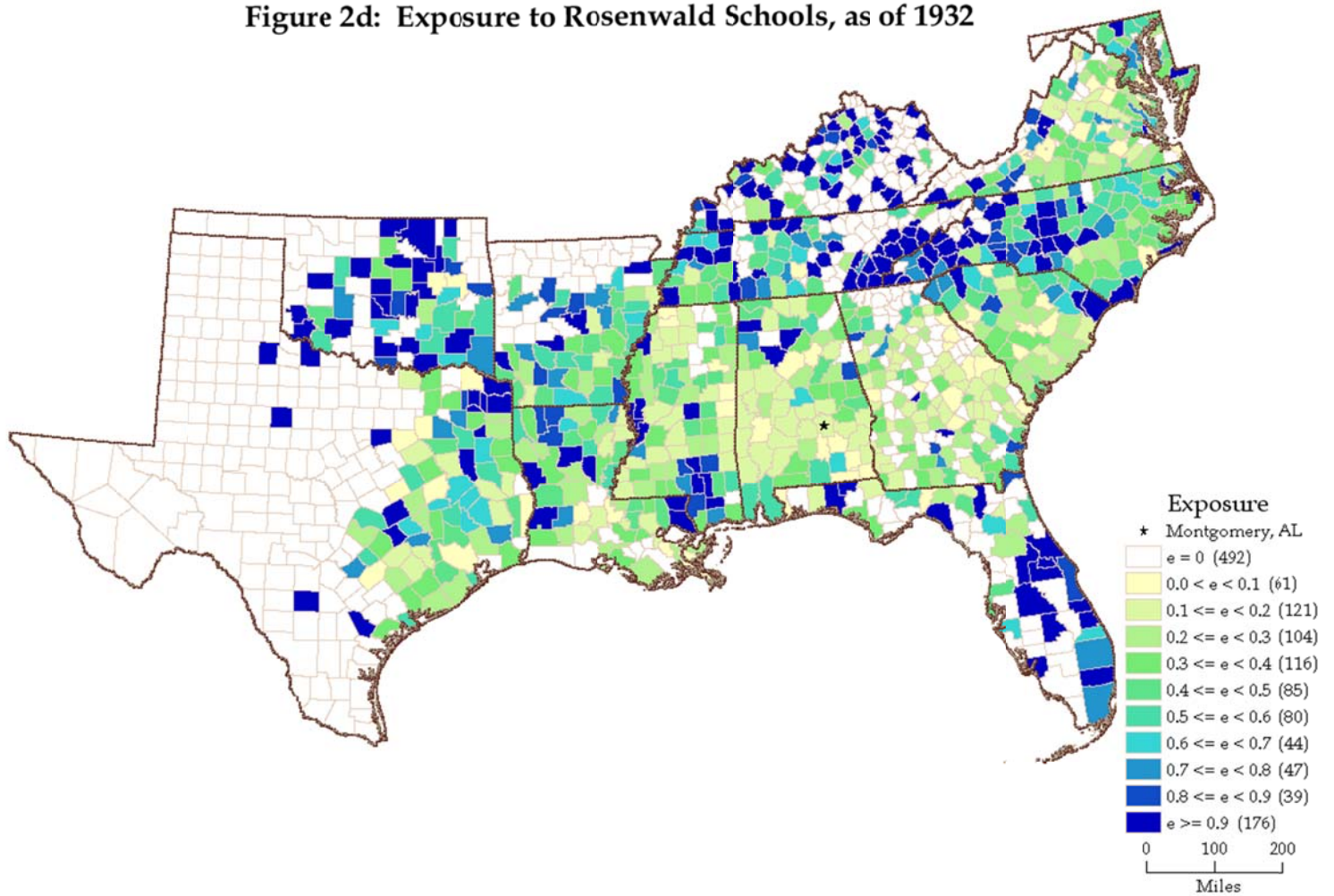


Table A1: School Attendance Results from Aaronson and Mazumder (2011)
Ages 7 to 17 in the 1900-1930 Censuses

	(1)	(2)
	Male	Female
γ_0	0.015 (0.016)	-0.010 (0.015)
γ_1	0.052 *** (0.019)	0.054 *** (0.013)
γ_2	-0.015 (0.012)	0.002 (0.012)
γ_3 (B-W Rural - B-W Urban)	0.075 *** (0.022)	0.075 *** (0.019)
Black, Rural-Urban ($\gamma_2 + \gamma_3$)	0.060 *** (0.026)	0.077 *** (0.020)
Black-White Rural ($\gamma_1 + \gamma_3$)	0.127 *** (0.014)	0.129 *** (0.013)
N	324,141	319,122

Notes: See Aaronson and Mazumder (2011).

The specification corresponds to equation (3) where the controls include county fixed effects, age interactions by state and year, father's and mother's literacy, father's occupational score, and father's home ownership. Standard errors clustered on county are shown in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%

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