American Economic Review 2026, 116(9): 3586–3621
https://doi.org/10.1257/aer.20241001

Intergenerational Impacts of Secondary Education: Experimental Evidence from Ghana†

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We provide experimental evidence on the intergenerational impacts of secondary education subsidies in a low-income context, leveraging a randomized controlled trial and 15-year longitudinal follow-up. Secondary school scholarships delay fertility, reduce unwanted pregnancies, and change partner selection. Under-one mortality is reduced by half. We also document cognitive development gains of 0.24 standard deviations of test scores by age five among children of female scholarship recipients. Means-tested secondary school scholarships are a high-return investment, with a conservatively estimated US$4.1 in benefits for each US$1 the policy would cost the government in the long run. (*JEL* H52, I12, I21, I26, J13, J16, O15)

Following the widespread adoption of free primary education in low-income countries and the subsequent surges in primary school enrollment rates, policymakers' attention has shifted to secondary school. The United Nations *Sustainable Development Goals* call for "… free, equitable and quality primary and secondary education leading to relevant and effective learning outcomes" (target 4.1). However, the extent to which secondary education should be publicly subsidized is not settled in the academic literature and is still a very active policy debate in low-income countries. Currently, less than one-third of countries in sub-Saharan Africa offer tuition-free secondary school education. This paper provides experimental evidence on one important aspect of this debate: the extent to which free secondary education has intergenerational impacts. For decades, this claim has been at the heart of the

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†Go to https://doi.org/10.1257/aer.20241001 to visit the article page for additional materials and author disclosure statement(s).

push for girls' education. But despite its prominence in public discourse, rigorous evidence to back it has been scarce.

In Ghana, the setting of this study, debates about whether secondary education should be free have been central to policy discussions over the past 15 years. In 2016, the National Patriotic Party (NPP) won the presidential elections on a promise to make Senior High School (SHS) free for all qualified students and implemented a policy that covered tuition and fees for all Ghanaian students admitted to SHS from the 2017–2018 school year onward. The opposition critiqued the policy as overcommitting resources to the education sector and diluting the quality of secondary education.[1] While the free SHS program is popular among Ghanaians, even NPP politicians have raised concerns over the government's ability to fund the program absent increases in tax revenue.[2]

At the heart of the debate is the fact that secondary education is expensive, and free secondary school implies a transfer to households who are sufficiently well-off to pay to send their children to secondary school. Offsetting these costs are any benefits of secondary education for all those unable to afford it, as well as the possible externalities to society of a more educated population.

In Duflo, Dupas, and Kremer (2026), we investigate the labor market impacts of secondary education and find small impacts on earnings, largely accounted for by hiring in the public sector. In this paper, we investigate one possible source of externality, namely, the fact that more educated individuals, especially women, may choose and be able to have fewer children, delay childbearing, and invest more in the human capital of the children they do have (Becker 1991). This implies that the benefit of providing free education to one cohort of adolescents would benefit future generations as well.

A large literature finds a correlation between education, lower and delayed fertility, and better outcomes for children (Thomas, Strauss, and Henriques 1991; Mohanty, Fink, Chauhan, and Canning 2016; Wietzke 2020; Montenegro, Wodon, Nguyen, and Onagoruwa 2018; Hahn et al. 2018). Moreover, there is a demonstrated causal link between parental inputs in childhood and cognitive scores and performance in school (Walker et al. 2007; Gertler et al. 2014; Attanasio et al. 2022). It is at least plausible that more educated parents provide more of these inputs (Attanasio et al. 2020). However, establishing a causal link between education and future family outcomes is difficult; adolescents who receive more education may be different in various ways, which may in turn explain why their own children would be more educated. Countries that invest in the education of one cohort might continue to invest in future cohorts, which means that educational reforms cannot easily be used as natural experiments.

To fill this gap, we provide what is, to the best of our knowledge, the first experimental evidence on the impact of secondary education on the timing, quantity, and quality of children, leveraging a randomized controlled trial and a very long longitudinal follow-up. The trial began in 2008, several years before free

[1] https://www.ghanaweb.com/GhanaHomePage/NewsArchive/Free-SHS-to-go-Mahama-threatens-689275.

[2] For example, "In recent debates over a controversial E-levy (a tax on mobile money transactions), NPP MPs claimed that the free SHS program would have to be discontinued if the E-levy was not enacted" (https://radiogoldlive.com/review-free-shs-kwame-sefa-kayi-urges-government/).

secondary education was enacted in Ghana. With philanthropic funding and support from the Ghana Education Service, the NGO IPA awarded four-year secondary school scholarships to 682 adolescents, randomly selected among a study sample of 2,064 rural youth who had gained admission to a public high school but did not immediately enroll because they were not able to pay the fee. In Duflo, Dupas, and Kremer (2026), we show that adolescents who received a scholarship were 28 percentage points more likely to complete secondary school, compared to those who did not get a scholarship (with results similar for men and women), and received on average 1.33 more years of education.

Since 2013, that is, right after (potential) graduation from secondary school, we have been regularly following up with the sample ("first generation") to collect data on their occupation, their earnings, and their family formation. In 2017, we began collecting data on the cognitive development of their children ("second generation") at specific milestone ages. We used locally appropriate tests developed by the Harvard Laboratory for Developmental Studies, based on the best available evidence and practice on how to measure cognitive development in young children. The tests were designed to be implemented by regular surveyors, as opposed to trained psychologists (unlike the standard psychometric assessments like the Bailey or MacArthur tests). In the tests, the child plays interactive games that target cognitive abilities that emerge in infancy and remain important through adolescence. To measure parental caregiving behaviors, we complement self-reports with a daylong recording of the child's auditory environment using the Language ENvironment Analysis (LENA) system.

Our first set of results is that receiving a scholarship impacts family formation. At our first follow-up in 2013, female scholarship recipients were 7 percentage points (14 percent) less likely to have had a pregnancy. This effect was driven by a 7 percentage point (17 percent) decrease in unwanted pregnancies. By 2016, they were 12 percentage points (24 percent) less likely to have ever lived with a partner. By 2022, their number of children had mostly caught up (1.8 children among nonrecipients and an insignificant point estimate of -0.135 for the effect of the scholarship); but the catch-up was very gradual. These outcomes are harder to observe and measure for men. Yet the effects for male scholarship recipients, while less precisely estimated, are remarkably similar in percentage terms. Where the results differ starkly between women and men is on partner selection. Among those who had started cohabitating by 2019, the scholarship treatment effect on the partner's level of education is *positive* for women and *negative* for men.

The second result is that children of scholarship recipients were more likely to survive. Among children born to our female control group respondents, 3.5 percent died before the age of one, and 4.2 percent before the age of three. Survival increased by 1.7 and 2.1 percentage points (p-values 0.037 and 0.041), respectively, among children of female scholarship recipients. This represents a halving of under-one and under-three second-generation mortality in our sample. The sample of second-generation children among males in the study is small, and likely negatively selected, because men start having children later than women. This is especially true for outcomes that we can only measure for children born three or more years prior to the end of our study period. We observe a 1.4 percentage point decline in under-1 mortality among children of male scholarship recipients, but this

is entirely driven by children born in the latter half of the sample period. For children born before 2017 to men in our sample, the point estimates on under-one and under-three mortality are (insignificantly) *negative*, consistent with a negative selection into early fertility for the treatment group.

The third set of results concerns cognitive development for second-generation children. To avoid bias stemming from the fact that individuals who received a scholarship started having children later in life, which implies that their children tend to be younger, we collected data on second-generation children at specific age milestones: 18 months, 2 and a half years, 3 and a half years, 5 years, and 7 years. We do not find a significant difference in the cognitive scores of second-generation children at the lower age ranges, but an advantage emerges over time for children of female scholarship recipients. By 5 years of age, a child's aggregate score is 0.24 standard deviation (SD) higher (p-value 0.005), and by 7, it is 0.25 SD higher (p-value 0.034) if their mother received a scholarship. Those are large impacts, found for most of the cognitive domains tested. In contrast, we find no evidence of positive cognitive development impacts for children of male scholarship recipients. The point estimates for the treatment effects are in fact negative for children of male recipients at five and seven years—measurements that could only be done among children born early in the study period.

An important mediator of the impact of the scholarship program on second-generation outcomes seems to be the education of the child's primary caregiver. In most instances, the primary caregiver is the child's mother. This means that children of female scholarship recipients have a much higher chance of having an educated caregiver; but this is not true for children of male scholarship recipients. In fact, male scholarship recipients who start cohabitating early tend to choose partners who are relatively less educated than partners of male respondents in the control group. The opposite is true for partnerships that form later. This could explain why we see consistently large positive impacts on children of female scholarship recipients but heterogeneous impacts on the children of male scholarship recipients.

In turn, the education of the primary caregiver (as well as any other direct impact of the scholarship, for example, lower expenses while in school or greater incentives to stay in school and delay childbearing) could impact child well-being in a number of ways. In our previous work, we do not find large or significant impacts of receiving the scholarship on earnings (including self-employment earnings) in the first 12 years after the scholarships were issued (Duflo, Dupas, and Kremer 2026).[3] The main channel for the intergenerational impacts is thus unlikely to be material well-being. We also don't find any difference in formal schooling inputs for the second generation (time spent in school, age at which they started school) or even educational aspirations, which were very high across the board (81 percent of the mothers in our sample hope their child will go to university, although only 2 percent of the mothers themselves went to university).

What seems to be different are inputs that are not costly but require perhaps more awareness or skills, namely, preventive care and stimulation of children. Children of mothers who received the scholarship have received more preventive care, and

[3] A positive effect on the earnings of female scholarship recipients does emerge after 12 years (Duflo, Dupas, and Kremer 2026), driven by relatively few women who obtain a job in the government sector.

mothers who received a scholarship are more likely to report counting, naming, or drawing things with their children, singing them songs, etc. These self-reports are confirmed by an objective measure of the interaction between young children and their caregivers. At 18 months, children of mothers who received the scholarship produce more vocalizations and have more conversational turns per minute with an adult in the daylong LENA recording. Since programs training parents to spend time playing and interacting with their children have been shown to impact cognitive development and long-run outcomes (Gertler et al. 2014; Walker et al. 2011; Attanasio et al. 2022), the greater stimulation that female scholarship recipients appear to engage in with their young children could explain the children's greater performance on cognitive tests a few years later. Conversely, we find negative effects on the LENA measurements of having a father who has received a scholarship, possibly owing to the fact that children of male scholarship recipients do not have a more educated female caregiver and are less likely to live with their father.

Benefit-cost analyses valuing a life at 100 times the Ghanaian GNI per capita (the medium scenario recommended in the literature) suggest that means-tested secondary school scholarships are a highly valuable investment in contexts similar to Ghana's, with benefits at least 3 times greater than the costs. The estimated marginal value of public funds (MVPF) is 4.1 when both men and women are eligible and 4.6 when scholarships target women only.

This paper makes a number of contributions. First, it contributes to the literature on the impacts of maternal education on fertility. A quasi-experimental study in Nigeria (Osili and Long 2008) and an experiment in Kenya (Duflo, Dupas, and Kremer 2015) find that primary education delays fertility. An experiment in Malawi (Baird, McIntosh, and Özler 2019) finds that conditional cash transfers increase secondary education and reduce fertility among adolescent females. Similarly, Ozier (2018) compares the outcomes of Kenyan students just above and below the cutoff score for attending secondary school and estimates lower rates of teen pregnancy among female students above the cutoff. Consistent with the findings of Baird, McIntosh, and Özler (2019) and Ozier (2018), we find a reduction in teen pregnancy due to an increase in secondary education for female students. However, we are able to track fertility over a longer time period, which enables us to learn that the total fertility of women who receive a scholarship eventually catches up to the total fertility of those who did not, but their children are born later and are more likely to be wanted. A limitation is that our study only estimates the effect for individuals who qualified for secondary school but could not attend (i.e., well prepared and low income).

Existing evidence on the intergenerational impacts of education generally relies on variation induced by policy reforms, such as expansion of education access or compulsory schooling laws. Studies exploiting the expansion of primary education in low-income countries find positive intergenerational effects on educational attainment in Indonesia (Akresh, Halim, and Kleemans 2023) and economic outcomes in Benin (Wantchekon, Klasnja, and Novta 2015). The expansion of tertiary education also appears to have positive intergenerational effects. Suhonen and Karhunen (2019) find an impact of parental tertiary education on a child's educational attainment in Norway, while Currie and Moretti (2003) estimates a positive impact of maternal tertiary education on child health in the United States. Exploiting

variation in college costs during a mother's adolescence, Carneiro, Meghir, and Parey (2013) find important intergenerational effects of maternal education on children's cognitive development in the United States. In contrast, studies exploiting the introduction of compulsory secondary schooling laws in mid-twentieth century Europe find small and insignificant effects of parental education on their children's education (Black, Devereux, and Salvanes 2005; Chevalier 2004). Baird, McIntosh, and Özler's (2019) conditional cash transfer experiment in Malawi provides some experimental evidence on the intergenerational impact of secondary education. While they do not measure child cognitive development, they do estimate noisy but positive effects of the conditional cash transfer on height-for-age among children of beneficiaries born within nine months of the program ending (Baird, McIntosh, and Özler 2019).

Our study diversifies the evidence based on the intergenerational impacts of education, and our identification strategy and data improve upon the existing evidence. To our knowledge, our study is the first to estimate the intergenerational impacts of secondary education on child mortality and cognitive development in a low- or middle-income country.[4] In addition, by randomizing secondary school scholarships to individuals, we avoid the potential identification concerns plaguing past studies. Policy reforms that increase educational attainment for a large cohort (such as expanding access or increasing compulsory schooling years) will increase demand for teachers, affect peer quality, and alter the marriage market. These changes could bias the quasi-experimental estimates of education's intergenerational impacts.

Our rich dataset provides not only uniquely rigorous estimates of the intergenerational impact of secondary education on cognitive development, it also shed lights on the mechanisms driving this relationship. The evidence from LENA recordings and parental reports point toward changes in parenting practices, such as playing and conversing with the child, as the likely mechanism. Our finding that cognitive gains appear only once children are of school age is consistent with recent evidence of complementarity between school-based and home-based investments (Duhon et al. 2024). Finally, by combining administrative data on secondary school fees and government education expenditures with self-reported school expenses and wages during secondary school, we accurately estimate the total social cost of secondary school scholarships, an important component for cost-benefit analyses.

I. Setting, Experimental Design, and Data

In 2008, Duflo, Dupas, and Kremer (2026) sampled 2,064 students who had not enrolled in senior high school because they could not afford to pay the fees, and initiated a randomized controlled trial in which 682 of these students were selected to receive a scholarship. Below, we provide a summary of the important features of the experiment. All data are original; they are available and detailed in a public archive (Duflo, Dupas, Spelke and Walsh 2026).

[4] Walker et al. (2023) estimates intergenerational child mortality impacts of a randomized *health* intervention—school-based deworming—in Kenya and find a 22 percent reduction in under-5 mortality among offspring of individuals exposed to the program in their primary school years. The mechanisms of impact appear somewhat similar to those we identify since not only the health but also the education of the first generation improved.

A. *Sampling*, *Randomization*, *and First-Generation Data*

Secondary school admission in Ghana is conditioned on an exam taken at the end of junior high school (JHS). Based on the exam results and their wishes, students who qualify are assigned to a school by a deferred acceptance algorithm.

The scholarship study sampled students who had been offered a spot to start SHS in fall 2008 but had not yet enrolled (usually due to financial constraints) by the end of the fall quarter (details on the sample construction are provided in Duflo, Dupas, and Kremer 2026). The research team administered a baseline survey to the students themselves as well as to one of their guardians, most commonly the mother. After the survey, each student received a basic mobile phone with a SIM card and was assigned a phone number.

The sampled students participated in a lottery where one-third of students were randomly assigned to the "treatment group" (offered a scholarship) and two-thirds to the "comparison group" (no scholarship), after stratifying by district, senior high school, junior high school, gender, and year of junior high school exit exam.[5] The scholarship covered full tuition and fees for a "day" (i.e., nonboarding) student for four years, paid directly to the school. Students who received the scholarship were only responsible for the cost of school materials, transportation to school, and school meals.

Duflo, Dupas, and Kremer (2026) show that demographic characteristics of study participants were balanced between treatment and control groups. The scholarship-lottery participants were on average 17 years old at the onset of the study and just over 31 at our last follow-up in spring 2023. Students were from poor families in rural areas. At baseline, over 40 percent of the students lived in households with no male head, and 48 percent of household heads had only primary education or less, compared to 24 percent and 35 percent, respectively, in Ghana as a whole (Ghana Statistical Service 2013).

The scholarship study team called the scholarship-lottery participants once a year from 2009 to 2012, to update their contact information and measure key outcomes (education status, family formation). In 2013, a detailed in-person follow-up survey measured schooling, occupation, cognitive skills, labor market expectations, and fertility, among other topics. The cognitive test was an oral test measuring competencies both in reading and in mathematics and the ability of the respondent to apply this knowledge to practical situations. Study participants received a phone upgrade at the end of the 2013 survey. In 2015, 2016, 2017, 2019, 2020, 2022, and 2023, 30-minute phone follow-up surveys were conducted to update participants' contact information and outcomes such as tertiary education, fertility, child survival, cohabitation, and labor market activities.

[5] About 30 percent of the sample is composed of women who had been admitted in SHS for fall 2007 but had not enrolled yet by fall 2008. This group was included to ensure gender balance in the final sample.

B. *Impact of the Scholarship on Education, Cognitive Skills, and Labor Market Outcomes*

Duflo, Dupas, and Kremer (2026) report the impact of the scholarship on recipients' education, cognitive skills, and labor market outcomes. We briefly summarize those results here. Winning a scholarship increased the SHS completion rate (the fraction of the entire group—including those who do not enroll—who graduate from SHS) from 39.8 percent to 67.2 percent among women (a 69 percent increase) and from 49.7 percent to 77.9 percent among men (a 57 percent increase). The effect of scholarships on SHS completion is large and statistically significant at the conventional level at all quartiles of the initial test score distribution. In 2013, scholarship recipients scored 0.16 SD higher on cognitive tests, with gains found in both math and reading. These gains were experienced across the distribution of test scores and were higher for females (0.194) than for males (0.113), although this difference in treatment effects between gender groups is not statistically significant (Duflo, Dupas, and Kremer 2026).

Overall, as of 2022, the scholarship had led to an average increase in total years of education of 1.46 years for women and 1.18 for men. While this increase is mainly due to more years of secondary education, there were significant impacts of the secondary school scholarship on access to tertiary education—but for women only. As of 2019, 12 percent of women in the comparison group had ever enrolled in tertiary education, and 7.8 percent had graduated. Treatment increased enrollment rates by 7.4 percentage points and graduation by 4.0 percentage points. By 2023, the treatment effect on tertiary completion had increased to 10.8 percentage points for women. While average tertiary enrollment was slightly higher among men overall, there was no tertiary education impact of the scholarships for them (Duflo, Dupas, and Kremer 2026).

In contrast to the clear gains in educational achievements and cognitive skills, the labor market impacts are very mixed and delayed. By 2019, on average, no significant impacts on earnings were observed for either males or females (although the earnings data are quite imprecise). For female scholarship recipients, there was a significantly higher likelihood of having a public sector job, though this concerns a very small share of the sample (10.4 percent of scholarship recipients versus 6.3 percent of the control group) (Duflo, Dupas, and Kremer 2026). It is only from 2020 onward that labor market gains emerge for women, with earnings 24 percent higher in 2020 and 30 percent higher by 2023. There is no discernible labor market returns for males up to 2023 (Duflo, Dupas, and Kremer 2026).

The education and labor market effects of the scholarship are nearly identical for the subsample whose offspring form this paper's sample for the analysis of intergenerational cognitive development effects, with one exception (Supplemental Appendix Table A1). Within this subgroup, there is already a significant gap in earnings between women who received the scholarship and those who did not as of 2019.

C. *Child Cognitive Development Test Instruments and Caregiver Surveys*

By 2016, many of the scholarship-lottery participants had children of their own, making it possible to assess whether the scholarship affected the cognitive development of recipients' children.

A first task was to develop cognitive tests for a range of children's ages. Most existing batteries of tests to measure early childhood cognitive development were developed and piloted in high-income economies and were therefore unlikely to be appropriate for Ghanaian children in mostly rural settings.[6] These tests are also expensive because they need to be administered in controlled conditions by a psychologist. An important contribution of our study is the development of a battery of cognitive tests that can be administered to children (i) by trained surveyors with no psychology degrees, (ii) at children's homes, and (iii) in low-income contexts.

The psychology Laboratory of Development Studies at Harvard developed these tests, based on research in cognitive science conducted in multiple cultures and with children at diverse economic levels. The tests consist of interactive "games" targeting cognitive abilities, such as language, attention, working memory, executive function, numerical and spatial reasoning, and social cognitive skills including reasoning through beliefs, perception, and emotions. The tests are meant to be engaging and use rules that are easy for children to understand and easy for surveyors to administer (on a laptop computer for children over age five and using simple concrete materials for the younger children, such as pictures, small objects, and cups). Combined, the games lasted 15 to 20 minutes for the youngest age group and 40–50 minutes for the oldest age group (seven-year-olds). Supplemental Appendix D provides details on the games for each age group. The tests were developed at Harvard and piloted and validated in Ghana (Coffey and Spelke 2023).

Due to the effect of the scholarship on the timing of fertility (Section III), it would have been inappropriate to survey all the children in one survey round because the scholarship recipients' children would have been systematically different. First, they would have been younger on average, and it is difficult to compare health and cognitive outcomes across ages.[7] Second, even after controlling for age at a given date, systematic differences may persist because scholarship recipients who started childbearing early may be negatively selected. Instead, we set up an infrastructure that allowed us to administer the tests, in person, to children of scholarship-lottery participants in specific age windows: 14–22 months old (we refer to these as the "1.5 years" group), 39–45 months old ("3.5 years"), and 60–69 months old ("5 years"). This approach allows us to measure outcomes for children that would be missed with one survey wave and to compare the outcomes of treatment and control children around the same age.

Beginning in June 2017, we administered the child cognitive tests and caregiver surveys whenever children in the sample entered the targeted age windows. To be eligible, the child had to be a biological child of an initial scholarship-lottery participant. A surveyor contacted the primary caregiver of the eligible child to arrange an

[6] In their 2017 *Toolkit for Measuring Early Childhood Development in Low- and Middle-Income Countries*, Fernald, Prado, Kariger, and Raikes (2017) review the weaknesses as well as the strengths of existing child measures and highlight that direct testing of infants (0–18 months) shows little correlation with later child outcomes (p. 31). Oxford Neurodevelopment Assessment (Ox-NDA), an infant development test adapted for use in low- and middle-income (LMIC) settings, was not available in 2017 (when we began assessments). This test also would not have been appropriate for the older age groups (Fernandes 2021).

[7] For example, there is an age gradient in height-for-age z-scores that makes it difficult to compare, e.g., a 6 month-old to an 18 month-old (Aiyar and Cummins 2021).

interview and the child's cognitive test.[8] The caregiver interview covered respondent demographics, respondent education, respondent health, indicators of household socioeconomic status, caregiver beliefs, child health, child health care, child education, cognitive stimulation of the child by household members, child time use, and, for children in the age range 14–22 months, infant language development. After we received approval from the Ghana Health Service in late 2017, we also began measuring the height and weight of children who were 24 months old or more.[9]

We began administering a test for 84- to 96-month-olds ("7 years") in May 2019 and added a 30- to 36-month-old test ("2.5 years") in July 2021. Starting in January 2018, we permitted the field team to survey children slightly above the maximum age for an age window if, due to time constraints among the field team, the child had not yet been surveyed for that age window.[10]

In total, we conducted 15 child measurement rounds from 2017 to 2022. Before each round, a member of the Laboratory of Development Studies met with the enumerators to review videotapes of selected field sessions and discuss ways to improve measurement quality. Then, the enumerators gathered updated location information for the eligible children through a short phone survey with their primary caregivers. Using this location information, local research staff assigned sets of respondents to enumerator teams based on geographic proximity. Finally, enumerator teams tracked and conducted measurements with the respondents over a three- to four-month period. We completed two child measurement rounds in 2017, three in 2018, three in 2019, and one in 2020, before we had to pause fieldwork from March to October 2020 due to the COVID-19 pandemic. We resumed field work in October 2020, completing one round. In 2021, we hired additional surveyors to make up for the missed workdays in 2020. We completed three rounds with this larger team in 2021, three in 2022, and two in 2023 (Supplemental Appendix Figure A1 shows the number of surveys by year). Across all measurement rounds, we administered 3,853 tests to 1,920 unique children.

D. *LENA Recordings*

To complement caregiver-reported information about the day-to-day environment around young children in the study, for the "1.5 years" age group, we gathered daylong recordings of the auditory environment using a recording device called LENA starting in February 2020. If the caregiver consented to the LENA procedures (80 percent of caregivers did), in the day that followed the cognitive tests measurements and caregiver survey, the child would wear a specially designed shirt with an attached recording device for at least 8 hours. The LENA device uses speech recognition software to process the sounds around the child into count-based metrics such as adult word count, adult–child conversational turns, and child vocalizations (see

[8] The primary caregiver was defined as the person "making the day-to-day decisions about the child's life."

[9] We did not measure the height of children under 24 months old because our measurement tool (a stadiometer) required the child to stand up straight without assistance from an adult.

[10] Starting in January 2018, the surveyors were permitted to survey children up to 25 months old using the 14- to 22-month-old instrument, children up to 55 months old using the 39- to 45-month-old instrument, and children up to 83 months old using the 60- to 69-month-old instrument. The surveyors were permitted to survey children up to 99 months old using the 84- to 96-month-old instrument and up to 39 months old using the 30- to 36-month-old instrument. We control for child age in months in the analysis.

Supplemental Appendix E for more details). Because LENA devices had never been used in Ghana, an environment where it is common for toddlers to spend hours on their mother's back, we first validated the device's accuracy by asking a few individuals (from outside the study sample) to record their activity for a few hours while their child was wearing the device. While a number of pilot studies in the United States have used the LENA device to estimate impacts of parenting interventions (see, for example, Leung, Hernandez, and Suskind 2020) or used LENA to provide feedback to parents (Suskind et al. 2013), we are not aware of any prior RCT having used the LENA device to measure outcomes of interest in a low-income context. Based on the measurement success of this study, a subset of us used the LENA device in another study of determinants of early childhood development in Ghana (Dupas, Falezan, Jayachandran, and Walsh 2025).

II. Empirical Specifications

The analyses follow a preanalysis plan filed on the AEA registry for social experiments.[11] To evaluate the impact of the scholarship, we run intent-to-treat regressions at the scholarship-lottery participant (indexed by j) or the child (indexed by i) level. Since gender differences in the impact of scholarships were a core question of interest at the onset, the randomization of scholarships was stratified by gender, and we study effects separately by gender of the scholarship-eligible individual. As specified in our preanalysis plan, we adjust for multiple hypothesis testing among our prespecified primary outcomes, namely, child survival and child cognitive development.

A. *Fertility and Family Formation*

To study first-generation impacts of the scholarship on fertility and family formation, we run regressions at the scholarship-lottery participant level, straightforwardly regressing outcomes of interest on an indicator for treatment status and controlling for region fixed effects and JHS exit exam score as in Duflo, Dupas, and Kremer (2026).

B. *Second-Generation*: *Child Survival*

To study impacts on child survival among the second generation, our sample consists of children of scholarship-lottery participants. Mechanically given the impact of the scholarship on fertility, children of female scholarship recipients are younger than children of females in the control group (4.71 months younger; $p = 0.067$; Supplemental Appendix Table A4, row 1). We therefore focus on survival to ages 1, 3, and 5 rather than being alive at the time of follow-up. This means we limit the sample to children who, based on their date of birth, had, or would have, reached

[11] The scholarship study started before the AEA RCT registry existed; it was registered immediately upon the creation of the registry in 2013. At the time, we had not anticipated being able to follow up with the children of the initial study participants. We registered a preanalysis plan for the intergenerational impact study in February 2022, after the Spelke lab had investigated construct validity blind to treatment status (Duflo, Dupas, Spelke, and Walsh 2022).

age 1 (or 3 or 5) by the time their scholarship-lottery participant parent was last surveyed. We run the following regression:

$$Y_{ij}^{a} = \alpha_{ij} + \beta_1 T_j + \beta_0 X_{ij} + \epsilon_i, \tag{1}$$

where Y_{ij}^{a} is the outcome (*child survived to age a*) for child i of scholarship-lottery participant j, T_j is an indicator that the scholarship-lottery participant j was randomly selected to receive a scholarship, and X_{ij} is a set of control variables. Because some scholarship-lottery participants had more than one child, we cluster the standard errors at the scholarship-lottery participant level, that is, at the biological mother or father level depending on the scholarship-lottery participant's gender.

Besides the scholarship-lottery-participant-level controls mentioned above (region of birth and junior high school finishing exam scores), we control for the child's birth order and birth year.[12] If we did not control for birth order, our results could be driven by differential composition effects since children of scholarship recipients are more likely to be firstborn children (Supplemental Appendix Table A4, row 2) and survival rates may depend on birth order. If we did not control for year of birth, our results could be driven by general time trends in our outcomes of interest. For example, since infant mortality is falling over time in Ghana, secondary education scholarships would lower infant mortality for the treatment group even if secondary education only delayed fertility and did not affect survival rate conditional on birth cohort. While this (positive) "cohort" effect of education subsidies is important and valuable in itself, we choose to control for it in order to focus our estimation on the effects that would be at play even in an environment with no trend in mortality. We control for the time trend with either a linear control in year of birth or year of birth fixed effects (the results are unchanged).

C. *Child Cognitive Development*

Our sample for child cognitive development outcomes consists of the children of scholarship-lottery participants ever eligible for at least one of our cognitive tests, that is, between 14 months and 7 years old *at some point* between fall 2017 and spring 2023.

To estimate impacts on cognitive development, we use equation (1) but alter the outcome variable and the set of controls. The outcome variable in these regressions is the child's score on the age-appropriate cognitive test. Since we administer different tests to 1.5-year-olds, 2.5-year-olds, 3-year-olds, 5-year-olds, and 7-year-olds, we separately estimate effects for each of these age groups. To control for updates in enumerators' training across measurement rounds, we include round fixed effects. Since the children are in narrow age groups and rounds only last two to four months, round fixed effects also effectively control for year of birth. Additionally, we include a linear control for age in months at the time of measurement, to increase estimate

[12] We cannot control for child gender because we are missing gender for 40 percent of the deceased child sample. This is due to our survey design. To minimize the psychological cost to respondents being asked to recall children who were born alive but passed away, we only asked for the child's birth date or the age when the child passed away and skipped all other questions.

precision.[13] Because the children of female scholarship recipients in the cognitive games sample are more likely to be firstborns (Supplemental Appendix Table A4, row 6), we control for the child's birth order. We also control for child gender as well as the same scholarship-lottery-participant-level variables as in the survival analysis.

Since the caregiver survey administered alongside the child measurements was the same every round, we have multiple caregiver-reported outcomes for a given child who was measured at different age windows. For this reason, we run regressions at the child-age-window level and include age-window fixed effects.

D. *Threats to Validity*

The most important threat to the validity of our estimates is sampling bias. In this section, we discuss sampling bias concerns for each of our main outcomes.

Given high survey rates in our follow-up surveys, there is little risk of sampling bias for the fertility and family formation outcomes: 95 percent of the sample could be surveyed after 11 years (2019), and there was no differential attrition between treatment and control groups (Supplemental Appendix Table A2). Survey rates dropped somewhat in the 2022 (85 percent) and 2023 (80 percent) follow-ups. In 2023, attrition among male participants was higher in the control group (22 percent versus 15 percent, p-value 0.005), so we focus on 2019 and 2022 outcomes when possible.

For child survival, we ensure low attrition rates by relying on data obtained through surveys conducted with first-generation respondents almost yearly between 2009 and 2023. While scholarship-lottery-participant-level attrition in these surveys was minimal (up to 2019), child-level attrition in any single survey round may be nontrivial, as parents may neglect to mention children who passed away years ago. For example, when asked, "Did you (or your partner) ever give birth to a biological child who was born alive but did not survive?", 22 of the 49 respondents who responded *yes* in 2017 responded *no* in 2019. This highlights the benefit of having conducted surveys almost yearly. Drawing upon all follow-up surveys from 2009 to 2023, we have survival status for 93 percent (98 percent) of all children ever mentioned by a female (male) scholarship-lottery participant and who had, or would have, turned at least 1 by the last follow-up survey in spring 2023 (Supplemental Appendix Table A2).

A limitation of our child survival data is that we miss children who would have turned 1 or 3 or 5 after we stopped surveying in spring 2023. Of the children ever mentioned by a scholarship-lottery participant, 8 percent had not turned 1, 24 percent had not turned 3, and 41 percent had not turned 5 by spring 2023. There may also be a sample selection bias among children ever born. Since Ghana has a total fertility rate of $\approx$ 3.4 per woman, it is likely that our sample will have more children over the course of their lives.[14] Of particular concern is the subset of participants

[13] For reference, Supplemental Appendix Figure A2 plots cognitive development score against child age in months by age window among the control group.

[14] Total fertility rate estimates for 2023 as reported in the World Bank Gender Data portal: https://genderdata.worldbank.org/en/indicator/sp-dyn-tfrt-in, last accessed June 30, 2025.

who do not yet have children but may have children in the future. By spring 2023 (when scholarship-lottery participants were 31 years old on average), 75 percent of women and 52 percent of men reported having at least 1 child (Supplemental Appendix Table A2). While these proportions are not significantly different between treatment and control groups, the characteristics of respondents who had at least one child by spring 2023 differ by treatment status among men but not women (Supplemental Appendix Table A3, panel A). In particular, among men who had a child by spring 2023, scholarship recipients are 9.7 percentage points (+25 percent; *p*-value 0.055) more likely to have grown up in a household with no male head compared to those in the control group. This result suggests that male scholarship recipients for whom we can study offspring survival may be negatively selected (in terms of baseline SES) relative to control men. To understand whether this could be driving the results, we perform robustness checks with entropy balancing following Hainmueller (2012).[15]

For the cognitive tests, we had relatively high tracking rates. Tracking rates ranged from 78 to 96 percent for eligible children (Supplemental Appendix Table A2). Some children were never eligible for the cognitive tests. These children were either already past 7 years when we began measurements or did not reach 14 months before the cessation of our measurements. Having children who were too old when assessments began accounts for the largest share of never-eligible parents (5 percent of all female scholarship-lottery participants and 2 percent of all males fall into this category).[16] Overall, we have measures of cognitive development for at least 1 eligible child for 64 percent of female scholarship-lottery participants (86 percent of those who had a child) and 41 percent of male scholarship-lottery participants (79 percent of those who had a child) (Supplemental Appendix Table A2). As with child survival, we use entropy balancing to address the fact that those with children in the cognitive games sample have slightly different characteristics between treatment and control groups (Supplemental Appendix Table A3, panel B.)

III. Results

We start by presenting the impact of the scholarship on family formation and fertility choices. We then show second-generation impacts on survival and cognitive development. Finally, we present evidence on possible mechanisms for the second-generation effects, including parent/caregiver characteristics and behavior.

A. *First-Generation*: *Fertility and Family Formation*

Figure 1 and Table 1 present the impacts of the scholarship on fertility and marriage. We discuss the impact on women and men in turn.

[15] It is possible that men in the treatment group are more likely to be *aware* of, or to *recognize*, children they may have had outside of wedlock. While this is a fascinating question, we are unable to investigate it since we don't know the "true" fertility rate among men.

[16] These respondents had children prior to May 2012 (when they were 21 years old on average) and none later.

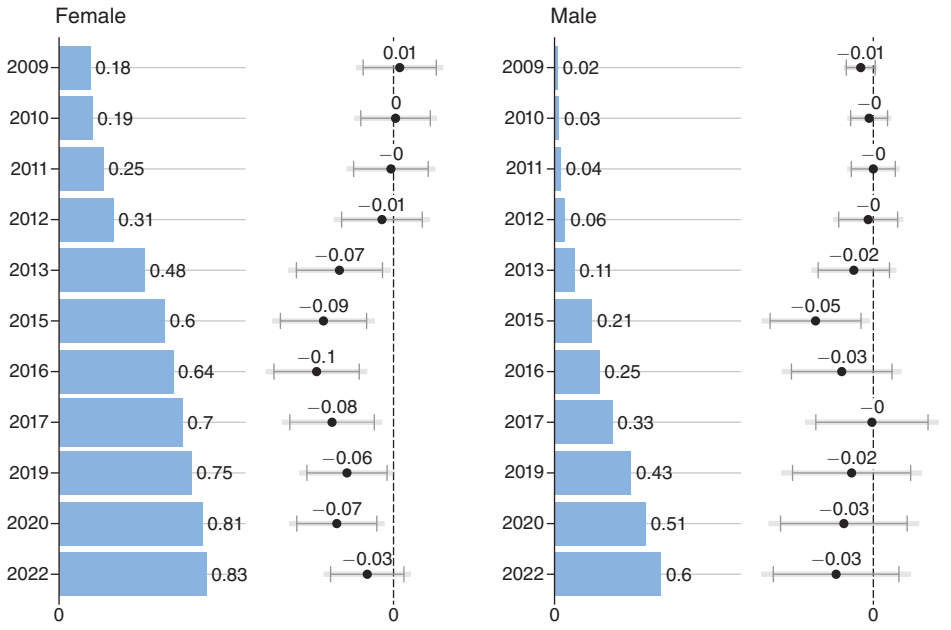


FIGURE 1. IMPACT ON CHILDBEARING ONSET—EVER PREGNANT/EVER HAD A PREGNANT PARTNER

Notes: Data from 2013 in-person follow-up and yearly phone surveys. Survey year indicated on the left. The outcome shown is "Ever pregnant" (for females) and "Ever had a pregnant partner" (for males). Left half of each panel shows means in comparison group; right half shows estimated treatment effects of scholarship. Ninety percent CIs at whiskers, and 95 percent CIs at the end of the confidence bars.

Female Scholarship Recipients.—By 2013, women in the scholarship arm were 7 percentage points less likely (p-value 0.039) to have ever been pregnant (on a base of 48 percent in the control group). Because the great majority of first pregnancies are reported to be unwanted, the fertility decline is almost exclusively a decline in unplanned, out-of-wedlock pregnancies (Table 1, column 2). The delay in childbearing onset is sustained over many years. By 2019, female scholarship recipients are still 6 percentage points less likely to have started childbearing than nonrecipients and had fewer children (−0.15 (11 percent) fewer children, p-value 0.065). These results are consistent with those of an earlier randomized experiment that reduced the cost of access to upper primary school in Kenya and found that the onset of childbearing was also delayed, with no catch-up in the three years following school exit (Duflo, Dupas, and Kremer 2015). They are also consistent with estimates based on natural experiments, such as the discontinuity created by admissions cutoff for secondary school in Kenya (Ozier 2018) or the introduction of free primary and free secondary school in Uganda (Keats 2018; Kazibwe and Li 2025). Eventually, fertility appears to partially catch up for female scholarship recipients. By 2022, women are 4 percentage points less likely to have ever been pregnant, and the number of children ever had is 7 percent smaller, but these differences are not statistically significant.

The finding that the gap in childbearing between treatment and comparison groups persists once the majority of scholarship recipients are out of school suggests that the mechanism is not an "incarceration effect," preventing fertility for a

Table 1—First Generation: Impact of Scholarship on Fertility and Marriage

	Ever pregnant/had a pregnant partner (2013) (1)	Had unwanted pregnancy (2013) (2)	Number of children ever had (2019) (3)	Number of children ever had (2022) (4)	Ever lived with partner (2016) (5)	Currently married or cohabitating (2019) (6)	Still living with parents (2019) (7)	Most recent partner completed tertiary education (2019) (8)
Panel A. Female scholarship-lottery participants								
Treatment	−0.069	−0.067	−0.152	−0.135	−0.121	−0.062	0.003	0.071
	(0.033)	(0.032)	(0.082)	(0.100)	(0.033)	(0.034)	(0.033)	(0.039)
p-value	0.039	0.038	0.065	0.177	0.000	0.067	0.933	0.071
Step-down *p*-value	0.208	0.208	0.250	0.306	0.003	0.250	0.927	0.250
Comparison mean	0.483	0.390	1.332	1.771	0.498	0.475	0.355	0.195
Observations	1,009	985	986	877	1,007	986	986	575
Panel B. Male scholarship-lottery participants								
Treatment	−0.018	−0.012	−0.026	0.022	−0.058	−0.047	0.078	−0.051
	(0.020)	(0.017)	(0.060)	(0.083)	(0.026)	(0.030)	(0.031)	(0.022)
p-value	0.368	0.475	0.671	0.790	0.027	0.117	0.011	0.021
Step-down *p*-value	0.754	0.823	0.853	0.853	0.118	0.360	0.069	0.118
Comparison mean	0.112	0.075	0.568	0.927	0.229	0.291	0.242	0.072
Observations	982	980	965	862	988	965	966	371
p-val mal = fem	0.210	0.136	0.246	0.289	0.138	0.703	0.097	0.008

Notes: An observation is someone enrolled in the 2008 lottery for secondary school scholarships. Panel A shows results for female lottery participants; panel B shows results for male lottery participants. "Treatment" means having won the scholarship lottery for senior high school. Data Sources: surveys conducted in 2013, 2016, 2019, and 2022. Year of survey in parentheses. The last row shows the *p*-values for tests that the effects are identical between males and females. The estimated treatment effects are in the first row; standard errors clustered at the scholarship-lottery participant level are in the second row in parentheses; the third row reports the *p*-values; the fourth row reports the family-wise Romano-Wolf step-down *p*-values; comparison group means are in the fifth row; the sixth row reports the number of observations. Controls include JHS finishing exam score and baseline region fixed effects.

few years while in school (Black, Devereux, and Salvanes 2008). Our rich data help shed light on the relative importance of the alternative mechanisms most discussed in the literature. These include (i) an increase in the opportunity cost of bearing and raising children (Becker 1991); (ii) the ability to control fertility due to better decoding of information (Rosenzweig and Schultz 1989); (iii) changes in the type or preferences of the partner and in the bargaining power of each partner; and (iv) a decrease in the cost of investing in each child's quality (education and health), which in turns affects the demand for the quantity of children (Becker 1991). These channels can of course operate conjointly.

In Duflo, Dupas, and Kremer (2026), we find that, consistent with channel (i), female scholarship recipients are more likely to have regular salaried employment than female nonrecipients, which presumably increases the opportunity cost of a child. We also document increases in learning and cognitive scores for both men and women, which could facilitate channel (ii).

Table 1 documents patterns consistent with channel (iii). First, fertility changes coincide with changes in cohabiting behavior. By 2016 (age 25 on average), female scholarship recipients were 12 percentage points (24 percent of the control mean) less likely to report having ever lived with a partner. As of 2019, they are 6 percentage points (*p*-value 0.067) less likely to be married or cohabiting (a 13 percent drop from a base of 47.5 percent in the control group; see Supplemental Appendix

Figure A3, panel A for the effects year by year). Conditional on having a partner, they are 7 percentage points (36 percent) more likely to have a partner who completed tertiary education (p-value 0.071). The way in which they met their partner has changed, with a significant increase in the role of school and friends and a decrease in workplace matches (Supplemental Appendix Figure A3, panel B). There is, however, no significant change in the age gap with the partner (Supplemental Appendix Figure A3, panel C).

In the rest of the paper, we show evidence that is consistent with either channel (iv) (reduced costs of investing in children quality) or a direct impact of the "wantedness" of children on their quality. Indeed, children of female scholarship recipients are healthier, and they have higher cognitive achievement.

Male Scholarship Recipients.—Much like women, young men who were awarded the scholarship delay family formation. The point estimates for fertility are smaller and not statistically significant, although it is worth noting that they are similar in percentage terms as those found for women because control men report having fewer children. We see a $-1.8/11.2 = 16$ percent decline in ever having a pregnant partner (column 1, panel B of Table 1, p-value 0.37).

One important measurement issue is that parenthood is likely measured with much more error for men than for women. Since many pregnancies are out of wedlock and not all of them lead to marriages, it is possible that male respondents underreport (or are unaware of) births they may have been responsible for. It is also possible that this is differential by treatment status. Some men with scholarships could be more likely to acknowledge paternity (because they are more desirable marriage prospects, for example), which would lead us to underestimate the treatment effect. Some may be less likely, if they are delaying family formation because they are hoping to attend tertiary school. This would lead us to overestimate the treatment effect.

Turning to cohabitation, the magnitude of effects in percentage terms is very similar to that for women, even though men start cohabitating at a later age. We see a 25 percent lower chance of ever having had a partner as of 2016 and a 16 percent lower chance of cohabitation in 2019 (p-values 0.027 and 0.117). Unlike women, however, male scholarship recipients are also significantly more likely to live with their parents ($+$ 8 percentage points, or 30 percent of the control mean, in 2019; p-value 0.011). Among those who had a partner by 2019, the partner is significantly less likely to have attended tertiary education, which is the opposite of the pattern we see for women. This may reflect a negative selection of the men who start cohabitating early, which, as we discussed in Section IID, we also see among men who have children.

Figure 2 shows how the characteristics of partners change across survey years. Male scholarship recipients who started cohabitating early have relatively less educated partners, while those who initiate cohabitation later have relatively more educated partners, with a gap of 31 percentage points in the treatment effect on the likelihood that the partner has completed SHS between those who started cohabitating in the 3 years leading up to 2022 ($+0.11$) versus the 3 years leading up to 2019 (-0.20). We come back to this heterogeneity when we look at outcomes of offspring in the next section.

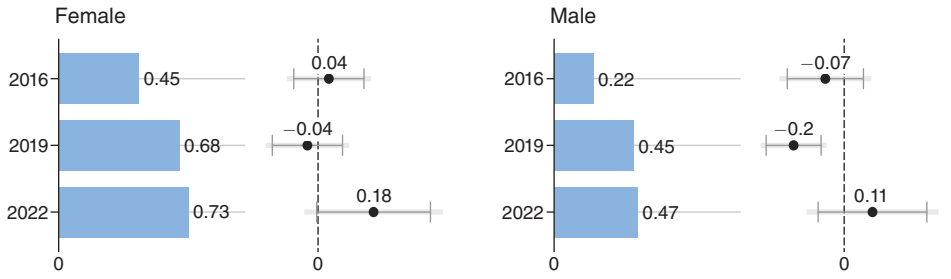


Figure 2. If Started Cohabitating in the Last Three Years: Partner Has Completed SHS

Notes: Observations are female (left panel) and male (right panel) scholarship-lottery participants. The 2016 estimates include all respondents ever cohabiting by 2016. For the 2019 and 2022 estimates, we focus on the subsample who report cohabitating with a partner for the first time in the previous three years. Bars indicate the comparison group mean. Black dots are point estimates of the treatment effects, with 90 percent and 95 percent confidence intervals indicated by the line and whiskers, respectively. The share of study participants who report ever cohabitating by survey year is shown in Supplemental Appendix Figure A3, panel A.

B. *Second Generation*: *Survival*

Table 2 presents the results on child survival. The unit of observation in this table is the child of a scholarship-lottery participant. We show two specifications per panel: the basic OLS, for which we adjust for multiple hypothesis testing using the step-down procedure proposed by Romano and Wolf (2005); and the specification with entropy balancing, which reweighs observations so as to obtain balance on 2008 characteristics and thus helps understand whether the estimated effects are driven by differential selection into family formation by treatment status.[17]

Among children of female scholarship-lottery participants, we find a large, significant, and very consistent reduction in mortality in the treatment group. Specifically, we find a 1.7 percentage point increase in the probability of surviving until age one (49 percent decrease in mortality, p-value 0.038, step-down p-value 0.07), an increase of 2.1 percentage points in survival to age three (a 48 percent decrease in mortality, p-value 0.041, step-down p-value 0.07), and an increase of 2.4 percentage points in survival to age five (a 43 percent decrease in mortality, p-value 0.076, step-down p-value 0.085). The results are unchanged with entropy balancing—as expected since there is no imbalance between the recipients who had children and nonrecipients who had children among women (Supplemental Appendix Table A3). Results also barely change when we replace birth year fixed effects with a linear control in birth year, drop birth order fixed effects, add a control for mother's age at birth, or drop all controls for birth year (Supplemental Appendix Table A6).

For children of male scholarship-lottery participants, the sample sizes are considerably smaller, so the estimates are noisier. What is more, since men have children later than women, the sample for which we have survival to age three (even more

[17] The entropy balancing method of Källberg and Waernbaum (2023) is used as a weighting method to match the characteristics of the original scholarship-lottery sample. The characteristics used for balancing are age in 2008, BECE exam performance, no male head in the household, total number of household members, highest education of household head: SHS or more, and highest education of household head: primary or less.

Table 2—Second-Generation Impact: Child Survival

	All children			Born 2009–2016			Born 2017–2022	
Survival to:	1 year	3 years	5 years	1 year	3 years	5 years	1 year	3 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Children of female scholarship-lottery participants								
Treatment	0.017	0.021	0.024	0.015	0.023	0.020	0.024	0.020
	(0.008)	(0.010)	(0.013)	(0.012)	(0.013)	(0.014)	(0.012)	(0.016)
p-value	0.037	0.041	0.076	0.216	0.068	0.155	0.040	0.223
Step-down p-value	0.070	0.070	0.085	0.247	0.109	0.241	0.061	0.241
Entropy balanced								
Treatment	0.017	0.021	0.024	0.015	0.024	0.021	0.024	0.023
	(0.008)	(0.010)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)	(0.016)
p-value	0.038	0.035	0.072	0.196	0.051	0.116	0.043	0.147
Comparison mean	0.965	0.958	0.947	0.967	0.958	0.956	0.963	0.959
Observations	1,707	1,395	1,084	947	935	899	760	460
Panel B. Children of male scholarship-lottery participants								
Treatment	0.014	0.009	0.001	−0.006	−0.007	−0.015	0.029	0.030
	(0.010)	(0.012)	(0.018)	(0.017)	(0.017)	(0.020)	(0.012)	(0.018)
p-value	0.146	0.475	0.972	0.706	0.709	0.450	0.013	0.094
Step-down *p*-value	0.273	0.629	0.975	0.733	0.733	0.577	0.032	0.080
Entropy balanced								
Treatment	0.016	0.013	0.008	−0.004	−0.004	−0.012	0.032	0.038
	(0.009)	(0.011)	(0.017)	(0.015)	(0.015)	(0.018)	(0.011)	(0.016)
p-value	0.066	0.243	0.632	0.795	0.789	0.500	0.002	0.014
Comparison mean	0.970	0.971	0.968	0.979	0.978	0.977	0.964	0.962
Observations	1,016	772	520	420	411	390	596	361
p-val male = fem	0.634	0.403	0.283	0.294	0.160	0.158	0.919	0.652

Notes: An observation is a child of a participant in the lottery for secondary school scholarships. "Treatment" means the child's parent won the scholarship lottery for SHS. Panel A shows results for children of female scholarship lottery participants; panel B shows results for children of male scholarship-lottery participants. Columns 1, 4, and 7 (respectively, columns 2, 5, and 8 and columns 3 and 6) include all children who had reached 12 (respectively, 36 and 60) months as of the 2023 survey. The estimated treatment effects are in the first row; standard errors clustered at the scholarship-lottery participant level are in the second row in parentheses; the third row reports the *p*-values; the fourth row reports the family-wise Romano-Wolf step-down *p*-values; comparison group means are in the fifth row; the sixth row reports the number of observations. Controls include year of birth fixed effects, birth order, region fixed effects, and the JHS finishing exam score of the scholarship-lottery participant.

so for survival to age five) is differentially selected, and comparing across columns (e.g., survival to age five versus survival to age one) does not tell us whether a given child survived to these age milestones, as the columns consider different *samples* of children. For this reason, in columns 4–8 we explore heterogeneity by child birth cohort, to parse out composition from treatment effects.

Turning first to average effects, we estimate an insignificant 1.4 percentage point increase in the probability to survive until age one (47 percent decrease in mortality, *p*-value 0.146, step-down *p*-value 0.273) and an insignificant increase of 0.9 percentage points in survival to age three (a 31 percent decrease in mortality, *p*-value 0.475). While smaller, these estimates are not significantly different from the estimates for female respondents. With entropy balancing, the reduction in infant mortality before age 1 becomes significant at the 10 percent level for children of male scholarship recipients.

Pooling children of female and male scholarship-lottery participants, the treatment effects on survival to ages 1, 3, and 5 are 1.6, 1.6, and 1.6 percentage points, with p-values of 0.012, 0.037, and 0.138 and step-down p-values of 0.016, 0.042, and 0.132 (Supplemental Appendix Table A5).

Heterogeneity across Birth Cohorts.—Turning to the split at the median year of birth (columns 4–8 of Table 2), we find positive survival impacts for both cohorts among children of female scholarship recipients. In contrast, we observe some important heterogeneity for children of male scholarship recipients: The survival effects are completely absent among children born "early" (i.e., before 2017) to male scholarship recipients, but they are quite large and significant for children born 2017 or later (+2.9, p-value 0.013, step-down p-value 0.032). This heterogeneity could reflect differential selection; treatment men who had children relatively early may have been negatively selected. The fact that the point estimates for the earlier birth cohorts become less negative with entropy balancing partially confirms this, though they remain negative. We discuss the role of negative selection of *partners* in Section IV.

In general, the child mortality results for children of male scholarship-lottery participants are more sensitive to perturbations of the data than our other results. This sensitivity is driven by the fact that the sample size is smaller since fewer male participants report having children and few children pass away. Focusing on survival-to-1 among the children of male respondents, only 26/1,016 passed away before age one (20 in the control group; 6 in the treatment group). Among the children of female respondents, 50/1,707 passed away before age one (41 in the control group; 9 in the treatment group). Nonetheless, the stark decline observed in our sample is remarkable.

C. *Second Generation*: *Cognitive Development*

Table 3 shows the results on child cognitive development; once again, the unit of observation is the second-generation child.

For children of female scholarship recipients, the estimated treatment effects are insignificant and slightly negative for 18-month-olds and 2.5-year-olds, and insignificant and slightly positive for 3-year-olds. In contrast, the 5- and 7-year-olds of female scholarship recipients score substantially higher on the cognitive development tests, 0.24 and 0.25 SDs, respectively (p-values 0.005 and 0.034). These results are robust to multiple hypotheses testing adjustment and entropy balancing. They are also robust to excluding measurement round fixed effects (Supplemental Appendix Table A7).

The effects for children of female scholarship recipients at ages five and seven fall between the seventy-fifth and eightieth percentile of effect sizes for the 96 RCTs measuring the impacts of educational interventions on learning in low- and middle-income countries considered in a recent meta-analysis (Evans and Yuan 2022). In terms of early childhood education interventions, these effects are close to those of the most effective rigorously evaluated interventions, such as hiring an additional teacher focused on preschool instruction (0.29 and 0.46 SD increases in math and language scores; Ganimian, Muralidharan, and Walters 2021), offering scholarships for high-quality kindergartens (0.40 SDs; Dean and Jayachandran 2019), and improving

Table 3—Second-Generation: Cognitive Development by Milestone Age Window

	Cognitive ability index score				
Age:	1.5 years (1)	2.5 years (2)	3 years (3)	5 years (4)	7 years (5)
Panel A. Children of female scholarship-lottery participants					
Treatment	−0.072	−0.024	0.025	0.239	0.252
	(0.094)	(0.127)	(0.078)	(0.084)	(0.118)
p-value	0.445	0.852	0.754	0.005	0.034
Step-down *p*-value	0.824	0.925	0.925	0.023	0.122
Entropy balanced					
Treatment	−0.033	−0.060	0.009	0.236	0.256
	(0.081)	(0.112)	(0.080)	(0.081)	(0.107)
p-value	0.681	0.594	0.909	0.004	0.017
Comparison mean	0.013	0.028	−0.015	0.021	0.062
Observations	565	275	633	670	363
Panel B. Children of male scholarship-lottery participants					
Treatment	0.165	−0.218	0.007	−0.216	−0.126
	(0.117)	(0.153)	(0.094)	(0.122)	(0.191)
p-value	0.158	0.157	0.943	0.079	0.510
Step-down *p*-value	0.507	0.507	0.951	0.333	0.767
Entropy balanced					
Treatment	0.132	−0.221	−0.041	−0.259	−0.232
	(0.109)	(0.149)	(0.086)	(0.119)	(0.152)
p-value	0.226	0.139	0.639	0.030	0.126
Comparison mean	−0.022	−0.037	0.039	−0.039	−0.111
Observations	343	208	345	300	175
p-val male = fem	0.217	0.261	0.969	0.006	0.092

Notes: An observation is a child of a participant in the lottery for secondary school scholarships at a given age window (there can be multiple observations per child if the child was surveyed at multiple age windows). See Table 2 notes on how to read the table. All regressions control for child age in months, child gender, child birth order, measurement round fixed effects, and fixed effects for scholarship-lottery participant baseline region and JHS finishing exam score. The latent abilities of the child are estimated using a one parameter logistic item response theory model. The results when we score unattempted questions as zeros instead of missing are shown in Supplemental Appendix Table C1. We have fewer observations at 2.5 years and 7 years because these tests were introduced later (July 2021 and May 2019, respectively).

preschool curricula (0.11–0.26 SDs) (Dillon et al. 2017; Gallego, Näslund-Hadley, and Alfonso 2021; Oreopoulos, Petronijevic, Logel, and Beattie 2020).

Breaking the results for female respondents down by cognitive domain (Supplemental Appendix Tables C2 to C6), we find strong effects on language skills (0.16 SDs for five-year-olds; 0.26 SDs for seven-year-olds), math and numeracy (0.16 SDs; 0.27 SDs), spatial reasoning (0.19 SDs; 0.21 SDs), and executive function (0.24 SDs; 0.19 SDs) but no effect on sociocognitive development.[18, 19]

[18] Note that Coffey and Spelke (2023) document correlations across children and time that suggest that the sociocognitive development tests may not have measured the underlying trait as intended. We keep these in the overall index shown in the main tables since this is what we had prespecified.

[19] We break down the impacts on child cognitive development by child gender in Supplemental Appendix Table A8. The sample sizes become small with this breakdown (especially for children of male respondents), and the results are quite imprecise. We cannot reject equality of the treatment effect by child gender among children of female scholarship-lottery participants.

It is noteworthy that a treatment effect emerges only once children reach age five, increases from age five to seven, and focuses primarily on cognitive skills that underlie, and are enhanced by, learning to read and calculate in school. These findings suggest that having a more educated mother leads to gains in children's readiness for learning in school, perhaps because an educated mother is more likely to bridge the gap between school and home. Another, more mechanical, interpretation could be that the cognitive tests are more robust at older ages and that the tests of language, math, and executive function are more robust than the tests of socioemotional development. Coffey and Spelke (2023) tested construct validity of our tests, by measuring overall correlations between game scores within the same domain cross-sectionally and longitudinally, and indeed the 5- and 7-year-old games appear to be significantly more reliable measures of the targeted cognitive abilities than the 1.5-year-, 2.5-year-, and 3.5-year-old games.[20] It is thus possible that the null effects on younger age groups are driven by noisy measurement. In other words, we cannot exclude the possibility that the mother's receipt of the scholarship mattered for the younger children on dimensions that we either didn't measure or measured less effectively, due to limitations on the number of questions young children would sit through and on the number of response options they were able to consider for each question. On the other hand, we see null effects on caregiver-reported language outcomes (Supplemental Appendix Table A9).[21] The most plausible interpretation of the results may be that impacts on cognitive development only emerge after a few years. Given the treatment effect on mortality, it could also be that marginal children in the treatment group start with a cognitive deficit (say, because they survived a cerebral malaria episode, so they are alive but weakened), and it takes time for the impact of maternal education on child cognitive development to overcome this initial deficit.[22]

Turning to children of male scholarship-lottery participants, we find no significantly positive impacts on cognitive development at any age, and the point estimates are in fact negative at all ages except for 1.5 years old, and even marginally significantly negative for 5-year-olds (Table 3, point estimate −0.22, p-value 0.079, step-down p-value 0.333). The difference in effect sizes for children of male scholarship-lottery participants compared to children of female scholarship-lottery participants is significant for 5-year-olds (Table 3, column 4, p-value 0.006). Note

[20] If the tests are measuring a cognitive domain accurately, performance on one of the tests should predict performance on a subsequent test in the same domain. We find that, for children who took both sets of tests, the 5-year-old game scores are highly correlated with 7-year-old game scores (0.53), while 1.5-year-old game scores have little correlation with 2.5- or 3.5-year-old game scores (0.15 and 0.07, respectively—see Supplemental Appendix Figure A4.)

[21] Dupas, Falezan, Jayachandran, and Walsh (2025) found significant positive effects of a light-touch infant-directed speech information treatment among infants aged 0–24 months in Northern Ghana using our caregiver-reported language measure, suggesting this measure has some signal.

[22] One could also think that the treatment effect is observed at older ages because of a *selection effect*; due to delay in the onset of childbearing for the treatment group, children eligible for the 5- or 7-year tests include, in the control group, *children who would not have been born* had their mother received a scholarship. This cannot explain the results, however, because we did not start testing children for cognitive development until 2017, which means that we missed the "window" to administer the 5- (7-) year-old test to children born before 2012 (2010). Thus, even though women in the treatment group had 0.11 fewer children aged at least 7 by 2023 compared to women in the control group (p-value 0.06), there is no significant difference between treatment and control arms in the number of children administered the age-7 cognitive test. The same goes for age 5 (results available upon request). Thus, these effects are by construction coming from the "intensive" margin of a similar sample of children born in treatment and control group, doing better in the treatment group.

that almost all children who could be tested at 5 or 7 years old were born before 2017, so, for children born to male scholarship recipients, the nonpositive effect on cognitive scores is consistent with the nonpositive effect on survival observed in Table 2, columns 4–6.

Recall that for male scholarship-lottery participants, selection into parenthood is large since only about half report that they ever had a child (Supplemental Appendix Table A2), and we see some imbalance in 2008 baseline characteristics between treatment and control groups within that subsample (Supplemental Appendix Table A3). However, the results on offspring cognitive scores are qualitatively unchanged when we use entropy balancing, suggestive that the results are not due to negative selection.

Overall, these results suggest that investing in universal female secondary school education improves the cognitive abilities of the next generation, especially those that are most directly tied to learning in school, while additional investments in males' education alone does not appear to have the same impacts.

IV. Mechanisms

What are the likely explanations for the positive impacts for children of female scholarship recipients and the lack of effects for children of male recipients born before 2017?

A. *Parental Education*

Maternal and paternal education do not have the same effect on the education of a child's primary caregiver. For 84 percent of the children, the primary caregiver is the child's mother (Supplemental Appendix Table A10). As a result, the primary caregiver for the children of female scholarship recipients is 25 percentage points more likely to have completed secondary school and 5 percentage points more likely to have completed tertiary (Table 4, columns 1 and 2). In contrast, children of male scholarship recipients have caregivers with the same level of education as children of males in the control group.

While the caregivers are almost exclusively females, the partners of caregivers are almost exclusively males. Mechanically, there is a large treatment effect on the secondary school status of partner of caregivers for children of male scholarship recipients (i.e., the partner of the caregiver is typically the scholarship recipient father). There is also a positive (and quite large in percentage term) effect on the education levels of partners of caregivers of children of female scholarship recipients, though not significant (columns 3 and 4 of Table 4).[23] Controlling for partner's education does not change the treatment effects for children of female scholarship recipients, however, suggesting that maternal education, not partner education, is the main driver (Supplemental Appendix Table A11).

Another reason why paternal and maternal education may not have symmetric impacts on a child's environment is that children of male scholarship recipients

[23] The effect on the education of partners of caregivers is smaller than the effect on partners of scholarship recipients in Table 1, likely due to the fact that the sample in Table 4 is selected on children being old enough to be tested, and this likely includes scholarship recipients most likely to have found a partner while enrolled in tertiary.

Table 4—Caregiver Characteristics

	Caregiver completed secondary education (1)	Caregiver completed tertiary education (2)	Caregiver's partner completed secondary (3)	Caregiver's partner completed tertiary (4)	Earn income (5)	Public sector job (6)	SES index (7)
Panel A. Children of female scholarship-lottery participants							
Treatment	0.249	0.054	0.063	0.039	0.023	0.052	0.068
	(0.040)	(0.021)	(0.049)	(0.037)	(0.030)	(0.022)	(0.063)
p-value	0.000	0.009	0.202	0.287	0.447	0.017	0.278
Step-down *p*-value	0.001	0.065	0.567	0.616	0.616	0.085	0.616
Comparison mean	0.218	0.035	0.432	0.155	0.748	0.039	−0.005
Observations	2,756	2,756	2,308	2,308	2,756	2,756	2,718
Panel B. Children of male scholarship-lottery participants							
Treatment	0.010	−0.002	0.204	0.025	−0.049	−0.027	0.041
	(0.036)	(0.013)	(0.055)	(0.029)	(0.034)	(0.010)	(0.075)
p-value	0.785	0.858	0.000	0.400	0.153	0.009	0.590
Step-down *p*-value	0.945	0.945	0.014	0.858	0.589	0.094	0.932
Comparison mean	0.196	0.026	0.453	0.070	0.818	0.033	0.002
Observations	1,536	1,536	1,341	1,341	1,536	1,536	1,516
p-val male = fem	0.000	0.025	0.064	0.634	0.128	0.002	0.641

Notes: An observation is a child of a participant in the lottery for secondary school scholarships at a given age window (there can be multiple observations per child if the child was surveyed at multiple age windows). All regressions control for child age in months, child gender, baseline region fixed effects, and the junior high school finishing exam score of the scholarship-lottery participant. Components of the SES index are shown in Supplemental Appendix Table B1.

appear significantly less likely to live with their father (−8.7 percentage points, *p*-value 0.024, column 5 of Supplemental Appendix Table A10). We see no such effect on the probability that children of female scholarship recipients live with their mother. Overall, the probability a child lives with their scholarship-recipient parent is much lower for children of male compared to female scholarship recipients (71 percent versus 923 percent).

To understand the heterogeneity in treatment effects on survival across birth cohorts among children of male scholarship lottery participants, we cannot rely on the caregiver survey data, which is, by construction, only available if a child survived to age 1.5 and was included in the cognitive test sample. Instead, we use data on scholarship-lottery participants and their partners. Here again, maternal education appears to be the key factor. Indeed, recall from Figure 2 that treatment effects on partner selection changed over time for male scholarship recipients, going from negative to positive. Assuming that, for the majority of cases, the partner of the father is/was the mother/caregiver, this would mean that the children born early to male scholarship recipients have mothers with somewhat lower education (which could explain the negative point estimate on survival if maternal education is the key factor), while those born later have more educated mothers (consistent with the positive point estimate on survival if maternal education is the key factor).[24]

[24] Education and other characteristics of scholarship-lottery participants and their partners, and associated treatment effects, broken down by child birth cohort for the survival-to-1 sample, are shown in Supplemental Appendix Figure A5.

B. *How Does Maternal Education Affect Children's Outcome?*

Maternal education could affect child survival and cognitive development through lower or delayed fertility, higher, less volatile parental income, greater health knowledge, better parenting skills, higher valuation of a child's education or health, and/or higher bargaining power for women/improved marriage market prospects.

Maternal Age.—The striking reduction in child mortality among female scholarship recipients could be a direct result of an increase in maternal age. Using DHS data from 55 low- and middle-income countries, Finlay, Özaltin, and Canning (2011) document a strong correlation between maternal age at birth and child health outcomes, including survival, even after controlling for socioeconomic status. They conclude that there is likely a causal effect of the biological maturity of the mother. Child marriage bans improve child health (Le, Molina, Ibuka, and Goto 2024) and educational outcomes (Chari, Heath, Maertens, and Fatima 2017), with higher maternal age at marriage and first birth being a likely mechanism. In our setting, maternal age at birth increases by 0.35 years for children born to female scholarship recipients (p-value 0.147) compared to the control group (column 5 of Supplemental Appendix Table A6). For firstborn children, the gap in maternal age at birth between treatment and control group rises to 0.55 years (p-value 0.074, Supplemental Appendix Table A12, column 3). We are underpowered to detect mortality effects on firstborn children (which make up 44 percent of the sample), but the point estimates in Supplemental Appendix Table A12 are somewhat *smaller* compared to the overall mortality effects in Table 2, suggesting that maternal age at birth may not be the primary driver of the mortality results in our sample. Another piece of evidence suggesting a minor role for maternal age at birth in our context is found in the results for children of male scholarship recipients: Maternal age at birth falls for them (Supplemental Appendix Table A6, −0.46 years; p-value 0.22), yet the point estimate on survival is positive.

Resources.—Duflo, Dupas, and Kremer (2026) report no significant treatment effect on income until 2019, but by 2020, women who received a scholarship start showing 24 percent higher earnings than women who did not receive a scholarship. This means that the children of scholarship recipients who are old enough to have been tested at the ages of 5 or 7 by 2023 likely did not have improved economic resources in their early years of life, though their mother may have achieved better material circumstances by the time we conducted the tests (recall that we conducted the children tests from 2017 to 2023). Consistent with this, we see that children of female scholarship recipients are more likely to have a caregiver who has a public sector job (column 6 of Table 4).[25] Yet we see no difference in the prespecified SES index that includes information on per adult equivalent food spending and dwelling

[25] For the sample of children for whom we have information on survival, we also see that mothers in the treatment group are more likely to have a partner with a public sector job compared to mothers in the control group (Supplemental Appendix Figure A5).

characteristics (column 7 of Table 4). It could be that our SES index misses some of the resources available, such as savings for emergencies.

Resources *per child* could also be greater since female scholarship recipients started bearing children later. We do not find any evidence for a quality-quantity trade-off, however. In fact, surprisingly, children of scholarship recipients do *not* have caregivers with fewer children to care for (column 7 of Supplemental Appendix Table A10). This is surprising since we know female scholarship recipients had fewer children when we began the cognitive assessments. Possible explanations for this include (i) the survival impact documented earlier; (ii) the fact that the sample of scholarship recipients who do have at least one child old enough to be tested is positively selected in terms of fertility; and (iii) children born from unwanted teen pregnancies may be more likely to be fostered by grandparents, and therefore, the older children of nonrecipients may not be living with their mothers. We do not have data on the location of children older than seven, so we cannot test whether this is a factor. We can, however, rule out yet a fourth explanation, namely, that scholarship recipients are more likely to foster children who are not their biological children, e.g., nephews and nieces; the (lack of) effect in column 7 of Supplemental Appendix Table A10 is found even among biological children.

Caregiver Practices, Behaviors, and Beliefs.—Table 5 turns to parental/caregiver behavior, and a number of meaningful differences emerge. Turning first to health investments, we find that children of female scholarship recipients are significantly more likely to receive prenatal care (column 1); prenatal care is quasi-universal, so the 1 percentage point increase in prenatal care for the treatment group corresponds to almost a halving of the odds of *not* having received prenatal care. We see a positive and significant (p-value 0.068) effect on preventive health behaviors (column 2).[26] Moreover, we observe an improvement in caregiver-reported child health (Supplemental Appendix Table A13 and Supplemental Appendix Table B3). Finally, for the subset of children for which anthropometric outcomes could be measured (see breakdown in Supplemental Appendix Table B4), we find that stunting and wasting are not differential across treatment and comparison groups—if anything, they are worse for children of female scholarship recipients, which may be due to the fact that the frailest children of nonrecipients were more likely to pass away prematurely.

Turning to potential channels for the effects on cognitive development, we see no significant impact on a child investment index—monetary investments in food or education-related supplies (see column 3 of Table 5, with breakdown shown in Supplemental Appendix Table B5) nor on a schooling index (column 4 of Table 5, with breakdown shown in Supplemental Appendix Table B6). However, among children of female scholarship recipients, the caregiver reports more interacting with the child in stimulating ways (column 5 of Table 5, with breakdown in Supplemental Appendix Table B7).

[26] The preventive health behaviors in the index are shown in Supplemental Appendix Table B2. We see significant increases in access to bed nets and private toilets but no change in water treatment. The most common source of drinking water for the children in our sample is sachet/bottled water (main source for 55.3 percent of children).

Table 5—Parental/Caregiver Behavior, Aspirations, and Beliefs

	Received prenatal care (1)	Preventive health behaviors index (2)	Child investment index (3)	Schooling index (4)	Child stimulation index (5)	Beliefs: Role of parental stimulation index (6)	Aspiration: Child's years of education (7)
Panel A. Children of female scholarship-lottery participants							
Treatment	0.010	0.121	0.010	0.020	0.147	0.046	0.025
	(0.004)	(0.066)	(0.053)	(0.069)	(0.059)	(0.075)	(0.039)
p-value	0.024	0.066	0.850	0.770	0.013	0.547	0.527
Step-down *p*-value	0.160	0.314	0.952	0.952	0.105	0.936	0.936
Comparison mean	0.989	0.014	0.047	0.065	−0.006	0.060	16.753
Observations	2,977	2,743	2,742	1,833	2,739	2,741	2,726
Panel B. Children of male scholarship-lottery participants							
Treatment	0.007	0.007	−0.047	0.062	−0.097	0.002	0.098
	(0.009)	(0.080)	(0.074)	(0.098)	(0.083)	(0.072)	(0.070)
p-value	0.440	0.932	0.525	0.527	0.241	0.973	0.165
Step-down *p*-value	0.948	0.999	0.957	0.957	0.823	0.999	0.739
Comparison mean	0.981	−0.019	−0.079	−0.130	0.020	−0.105	16.562
Observations	1,654	1,528	1,528	941	1,528	1,524	1,522
p-val male = fem	0.851	0.260	0.576	0.678	0.019	0.621	0.363

Notes: The unit of observation for column 1 is the child, and the data come from surveys with scholarship-lottery participants. The unit of observation for columns 2 to 5 is a caregiver-child pair, and the data come from surveys with caregivers. Standard errors clustered at the scholarship-lottery participant level. All regressions control for baseline region fixed effects, the junior high school finishing exam score of the scholarship-lottery participant, and birth order of the child. Columns 2–5 also control for child age in months and child gender. Refer to Supplemental Appendix B for components of the "Preventive health behaviors index" (Supplemental Appendix Table B2), "Child investment index" (Supplemental Appendix Table B5), "Schooling index" (Supplemental Appendix Table B6), and "Child stimulation index" (Supplemental Appendix Table B7). For the "Schooling index," the sample is restricted to caregiver-child pairs with children over 36 months old. Beliefs on the role of parental stimulation index: A higher value means that the caregiver is more aware of the positive impact of parental stimulation on infant brain development (see components in Supplemental Appendix Table B8). "Aspiration" shows the answer to the question, "What is the highest level of education that you would like [child name] to complete?"

Interestingly, we see this difference in reported behavior despite no effect on *knowledge* about the role of parental stimulation in children's cognitive development (columns 7 and 8 of Table 5). We also see no significant differences in caregiver's aspirations for their children—likely because they all believe "the sky is the limit," with an average expected 16 years of education.

Drawing upon our objective measurements of adult–child interactions through daylong recordings of the child's auditory environment via the LENA (Table 6), we confirm the presence of increased adult–child engagement for children of female scholarship recipients. In particular, we see increases in conversational turns per minute (0.068 effect size, a 20 percent increase; *p*-value 0.005) and child vocalization per minute (0.32 effect size, a 17 percent increase, *p*-value 0.014).[27]

Summing up, we do not find clear evidence that female scholarship recipients invest greater financial resources in their children; but they seek more preventive care

[27] We have nontrivial attrition in the LENA measurements, as shown in Supplemental Appendix Table A2, but we find no imbalance in child characteristics (Supplemental Appendix Table A4) nor in 2008 baseline characteristics or current household environment (Supplemental Appendix Table A14) for female respondents. As expected given the imbalances in Supplemental Appendix Table A3, there is imbalance on 2008 baseline characteristics for male respondents. We show results with entropy balancing in Supplemental Appendix Table A15.

Table 6—Objectively Measured Child Language and Stimulation: LENA Measurements

	Child vocalizations per min (1)	Conversational turns per min (2)	Percent Meaningful speech (3)	Adult word count per min (4)	Female adult word count per min (5)	Male adult word count per min (6)
Panel A. Children of female scholarship-lottery participants						
Treatment	0.324	0.068	0.009	0.423	0.371	0.052
	(0.131)	(0.024)	(0.008)	(0.725)	(0.619)	(0.291)
p-value	0.014	0.005	0.265	0.560	0.549	0.859
Step-down *p*-value	0.053	0.027	0.564	0.814	0.814	0.875
Comparison mean	1.956	0.336	0.156	12.952	9.814	3.138
Observations	560	560	560	560	560	560
Panel B. Children of male scholarship-lottery participants						
Treatment	−0.198	−0.053	−0.017	−2.590	−1.927	−0.662
	(0.154)	(0.030)	(0.008)	(0.904)	(0.748)	(0.339)
p-value	0.201	0.072	0.047	0.005	0.011	0.052
Step-down *p*-value	0.210	0.135	0.135	0.015	0.041	0.135
Comparison mean	2.210	0.380	0.171	14.260	10.677	3.584
Observations	391	391	391	391	391	391
p-val male = fem	0.009	0.002	0.020	0.012	0.023	0.118

Notes: An observation is a child of a participant in the lottery for secondary school scholarships. Only children aged 14–22 months old between January 2020 and February 2022 were eligible to be recorded using the LENA device. "Treatment" means the child's parent won the scholarship lottery for SHS. The analysis is restricted to recording times between 7AM to 6PM included. Only files with at least five hours of recording between 7AM and 6PM are kept in the analysis. All regressions control for child age in months, child gender, baseline region fixed effects, and the junior high school finishing exam score of the scholarship-lottery participant. Conversational turns per minute are measured by the number of times there is one utterance by an adult/child and then one by child/adult in response (within five seconds). "Percent meaningful speech" is the share of the audio categorized as vocalizations from the target child or speech/vocalizations from adults or other children near the target child.

and engage in more of the parent-child interactions that help children develop—not because they are more informed that such interactions are particularly important but likely because their greater level of education enables them to interact with others (including their children) in a different way.

Consistent with the fact that caregivers of children of male scholarship recipients do not have more education than their counterparts in the control group, the impacts on caregiver behavior for children of male respondents are never positive—all the coefficients in Table 5 are insignificant. Turning to LENA measures, we find *negative* treatment effects for children of male scholarship recipients (Table 6), consistent with the finding that they are less likely to live with their father (Supplemental Appendix Table A10). Absence of the father mechanically reduces exposure to male adult words and may also cause reduced exposure to female adult words if single mothers have less time on hand to verbally engage with each child individually. The negative LENA results for children of male scholarship recipients still hold with entropy balancing (Supplemental Appendix Table A15).

V. Cost-Effectiveness, Marginal Value of Public Funds, and Benefit-Cost Ratio

Intergenerational impacts rarely factor into policy debates around subsidizing secondary education. Yet our findings of substantial intergenerational impacts suggest

TABLE 7—COST-EFFECTIVENESS, MVPF, AND BENEFIT-COST RATIO

	Cost-effectiveness		Value of a statistical life: Marginal value of public funds			Value of a statistical life: Benefit-cost ratio		
	US$ per death averted (1)	US$ per SD of cognitive gain (2)	Low (3)	Medium (4)	High (5)	Low (6)	Medium (7)	High (8)
Panel A. Second-generation benefits								
Scholarships for all	23,016	2,686	0.9	3.1	4.9	0.7	2.5	4.1
	[14,935, 41,192]	[1,182, 6,934]	[0.6, 1.2]	[1.8, 4.2]	[2.8, 6.8]	[0.4, 0.9]	[1.4, 3.5]	[2.3, 5.5]
Scholarships for girls	17,391	1,208	1.1	3.9	6.2	0.9	3.3	5.2
	[10,404, 39,411]	[722, 2,743]	[0.6, 1.6]	[1.9, 5.6]	[2.9, 8.9]	[0.4, 1.3]	[1.5, 4.6]	[2.5, 7.4]
Panel B. First- and second-generation benefits								
Scholarships for all			1.2	3.4	5.2	0.9	2.8	4.3
			[0.7, 1.5]	[2.0, 4.6]	[3.0, 7.1]	[0.5, 1.3]	[1.6, 3.8]	[2.4, 5.8]
Scholarships for girls			1.5	4.3	6.5	1.3	3.6	5.6
			[0.9, 2.1]	[2.1, 6.1]	[3.2, 9.4]	[0.7, 1.7]	[1.8, 5.1]	[2.7, 7.9]
Panel C. First- and second-generation benefits with full fertility								
Scholarships for all	16,574	1,935	1.4	4.1	6.4	1.1	3.4	5.3
	[10,755, 29,663]	[852, 4,993]	[0.8, 1.8]	[2.4, 5.7]	[3.6, 8.8]	[0.6, 1.5]	[1.9, 4.7]	[3.0, 7.3]
Scholarships for girls	15,367	1,067	1.6	4.6	7.1	1.4	3.9	6.1
	[9,193, 34,824]	[638, 2,423]	[0.9, 2.2]	[2.3, 6.6]	[3.4, 10.2]	[0.7, 1.9]	[1.9, 5.5]	[2.9, 8.6]

Notes: Ten percent bootstrapped confidence intervals are shown in brackets. Panel A considers only the second-generation benefits from averting child deaths (Table 2). Panel B includes the benefits in panel A as well as the first-generation benefits. The number of births used for panels A and B is equal to the sample births in the treatment group as of 2023. Panel C includes the same benefits as panel B but assumes a total fertility rate of 2.1 children for both men and women. Columns 3 to 8: For the "Medium" columns, we use 100 × GNI per capita in Ghana for the value of a statistical life (VSL). In the "Low" columns, we present a less optimistic scenario using 33×GNI per capita. In the "High" columns, we present a more optimistic scenario using 160 × GNI per capita. Refer to Supplemental Appendix F for more details on these calculations.

that such impacts may be the strongest basis for public subsidies of education. We estimate the cost per child death averted, cost per SD improvement in child cognitive development, the benefit-cost ratio, and the marginal value of public funds (Hendren and Sprung-Keyser 2020) for a program providing means-tested secondary education scholarships. While such an exercise is, as always, sensitive to a host of assumptions, it provides a helpful sense of the relative magnitude of the benefits.

In Table 7, we present these estimates for a means-tested scholarship program open to both genders or open only to female students. We assume that the at-scale scholarship program would be means-tested in a manner similar to the program we evaluated (i.e., only those facing some difficulty paying for SHS on their own are eligible).

A. *Cost-Effectiveness*

We first compute the cost per child death averted and the cost per SD improvement in child cognitive development. For this exercise, we consider only the scholarship costs and estimated administrative costs associated with the program if it were implemented at scale. We estimate a cost of US$565 per scholarship recipient for 4 years of SHS. Since we are looking at impacts on *second-generation children*, we divide these costs by the number of children per scholarship recipient to get the cost per second-generation child. Note that this is conservative since we don't

observe complete fertility yet and thus underestimate the fertility rate. Given this, we also show in panel C the cost-effectiveness when we impute a total fertility of 2.1 (the replacement rate). This is still much below the current total fertility rate of 3.4 for Ghana.

Combining these cost estimates with the child mortality reduction (Table 2) gives us a cost per under-3 death averted of around US$23,000 for scholarships given to both genders, and of US$17,500 for scholarships given to females only (column 1, panel A of Table 7). Both of these estimates go down to around US$15,000 when we take into consideration future fertility. We use the ITT estimate on mortality, which means that the cost of subsidizing inframarginal students who would go to school anyway is taken into account. To put these estimates in perspective, we convert Stenberg et al.'s (2021) estimates of the cost per healthy life year delivered by 36 WHO-recommended neonatal and/or child health interventions into cost per under-3 death averted, using the WHO's 2019 recommendations. Stenberg et al.'s (2021) converted estimates imply that the median intervention recommended by WHO costs US$2,300 to US$8,200 per under-3 death averted, the twenty-fifth percentile intervention costs US$3,600 to US$16,100, and the tenth percentile intervention costs US$11,300 to US$33,200.[28] Thus, our estimates imply that scholarship could be in the recommended set of interventions to reduce child mortality, even if they had no other impacts.

In column 2 of Table 7, we evaluate the cost-effectiveness of secondary school scholarships in terms of improving children's cognitive development. As our estimate of the impact on cognitive development, we take the intergenerational impacts of the scholarships on cognitive scores for seven-year-olds (Table 3). Using this estimate yields a cost per SD increase in early childhood cognitive test scores of US$2,686 if the program covers both genders and US$1,208 if it covers females only (Table 7).

B. *Marginal Value of Public Funds and Benefit-Cost Ratio*

This subsection estimates the MVPF following the methodology proposed by Hendren and Sprung-Keyser (2020), as well as the more traditional Benefit-Cost Ratio (BCR). While the BCR considers the up-front government expenditure, the MVPF focuses on the long-run benefits per dollar of *net* government expenditure. Specifically, the MVPF is calculated as the ratio of two numbers: the benefits that a policy provides to its recipients divided by the policy's costs *net* of long-run government savings, such as additional tax revenue raised from higher earners. The components of each of these two numbers are shown in Figure 3, with benefits in orange and costs in blue. We detail how each of these are computed/estimated in Supplemental Appendix F.

MVPF: Benefits.—The numerator for the MVPF—the dark orange bar in Figure 3—adds the ways in which the policy improves the lives of its

[28] The ranges reflect differing estimates across region (Stenberg et al. 2021 evaluates interventions in East Africa or South Asia) and assumed coverage rate (50 percent, 80 percent, or 95 percent). The lower bound assumes a 95 percent coverage rate in East Africa. The upper bound assumes a 50 percent coverage rate in South Asia.

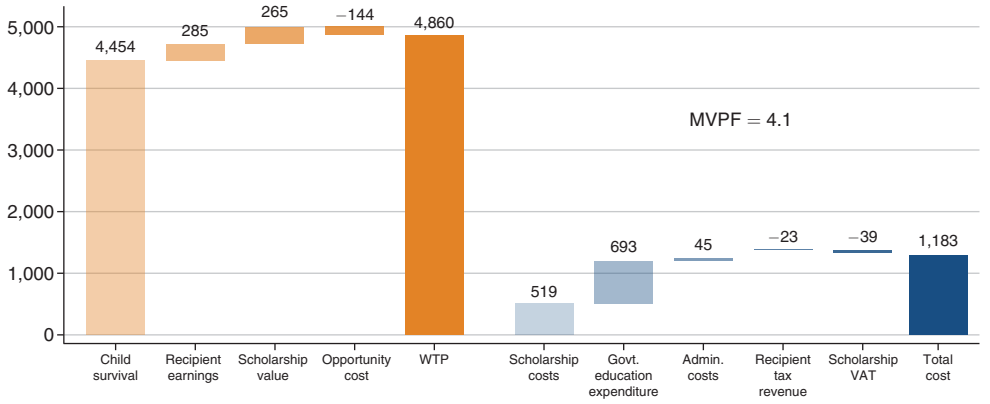


FIGURE 3. MEANS-TESTED SCHOLARSHIPS FOR ALL WITH FULL FERTILITY: BREAKDOWN OF MVPF

Notes: Estimates correspond to column 4, row 1, panel C of Table 7. The figure shows in light orange the components of the *numerator* (dark orange) and in blue the components of the *denominator* (dark blue) in the marginal value of public funds calculation. The costs presented assume a scaled implementation of the program. Administrative costs as incurred by the research team were US$181 per beneficiary, leading to a total net cost of US$1,319 and an MVPF of 3.7. See details in Supplemental Appendix F.

beneficiaries; Hendren and Sprung-Keyser (2020) call this sum the individuals' "Willingness to Pay (WTP)."[29] We measure the benefits of the scholarship program from averted child deaths and greater earnings for the scholarship recipients. To be conservative, we ignore the value of early childhood cognitive gains.[30] We also ignore the other effects of the scholarship (e.g., effects on caregiver-reported child health or first-generation life satisfaction) because we expect these benefits to be smaller in magnitude than the three benefits we consider, and we want to avoid double counting benefits (for example, improved child health may result in lower child mortality).

We calculate the total benefits from averted child deaths using estimates of the value of statistical life (VSL) for our sample. We follow Robinson, Hammitt, and O'Keeffe's (2019) recommendation for conducting VSL sensitivity analyses. The three methods proposed by Robinson, Hammitt, and O'Keeffe (2019) suggest that we multiply Ghana's GNI per capita by 100, 33, or 160, yielding VSLs of US$235,000 (refer to "Medium VSL" column in Table 7), US$76,862 (refer to "Low VSL" column), and US$376,000 (refer to "High VSL" column), respectively. Note that all these VSLs (even the "high" one) are very low compared to the numbers used in the United States.[31]

Since Duflo, Dupas, and Kremer (2026) find no impact on earnings until 2020, we model the first-generation labor market benefits as a stream of increased income

[29] This is what individuals would be willing to pay for the program if they were maximizing their lineage well-being, absent any market or information frictions.

[30] If one wanted to quantify those, one would need to project the effect of these gains on adult earnings and taking the present value of this income stream. This is challenging because there are no estimates of the relationship between early childhood cognitive development and adult earnings in our context.

[31] For example, the Environmental Protection Agency's (EPA) website recommends using a VSL of US$(2006)7.4 million, updated to the year of the analysis (see https://www.epa.gov/environmental-economics/mortality-risk-valuation#means, last accessed July 8, 2024.)

beginning at age 30 and continuing for the remaining 34 years of working life. Following Duflo, Dupas, and Kremer (2026), we attribute the observed earnings gains to a 6.7 percentage point increase in public sector employment for women. Thus, we ignore any potential within-sector wage increases. To estimate the impact, we calculate average wages in the public and nonpublic sectors by gender using sample data and project earnings over time using ILO lifetime earnings profiles for Ghana. We then calculate the annual earnings increase for public sector workers and multiply the net present value of this stream by the program's estimated ITT effect on public sector employment.

MVPF: Costs.—The denominator for the MVPF (dark blue bar labeled "Total cost") departs from the cost value used in the cost-effectiveness section above in that it (a) is net of tax revenues gained in the long run, and (b) it includes *all* associated costs incurred by the government, not only the scholarship costs. In particular, we include (i) the cost of administering a means-tested scholarship program, including the cost of identifying eligible youths;[32] (ii) the per student government education expenditure in secondary school (this includes, for example, teacher salaries and lab equipment); and (iii) the per student government education expenditure in tertiary programs (since some of the secondary school scholarship recipients go on to tertiary).

MVPF.—The MVPF estimates are shown in columns 3–5 of Table 7. In panel A, we consider the second-generation survival benefits, considering only children born within the study period. In panel B, we add the first-generation labor market benefits estimated in Duflo, Dupas, and Kremer (2026). In panel C, we consider the second-generation survival benefits assuming 2.1 children per scholarship recipient (a conservative estimate since the average total fertility rate in Ghana was estimated at 3.5 for 2022) and the first-generation labor market benefits.

Our calculations indicate that secondary school scholarships produce substantial benefits, with an at-scale MVPF of 4.1 (meaning that each dollar spent on the policy generates 4.1 dollars in benefits) in the medium VSL scenario. The MVPF is higher (4.6) when scholarships are targeted to female students, and higher still if we consider the higher VSL value (7.1).

Importantly, the benefits of secondary education for the next generation outstrip the benefits to the first generation. As one can see by comparing panels A and B of Table 7, the benefits to the second generation account for almost all of the numerator in the MVPF. This is important to note, since the intergenerational impacts are likely to be much less sensitive to possible general equilibrium impacts than the first-generation impacts. Duflo, Dupas, and Kremer (2026) show that the first-generation labor market impacts are primarily due to access to scarce public

[32] To select scholarship eligible students, Duflo, Dupas, and Kremer (2026) conducted school and home visits after the start of the school year to identify students who were admitted to secondary school but had not enrolled by the end of the first semester of the school year; 95 percent of these students cited financial difficulties as the reason they did not enroll. At scale, we would expect a government to identify eligible youths by exploiting administrative records and seeking inputs from school principals. We consider two costs scenarios: the administration costs incurred during the study ("pilot") and the administration costs that we estimate would be incurred by the government at "scale".

sectors job and therefore may not obtain if the program was generalized. In contrast, the second-generation estimates probably reflect the impacts of improved child-rearing human capital, and we would not expect negative externalities on other parents. This result underscores the importance of accounting for intergenerational effects when considering investments in secondary education.

C. *Benefit-Cost Ratio*

We provide estimates of the benefit-cost ratio as more typically estimated in the public health literature in columns 6–8 of Table 7. The details are provided in Supplemental Appendix F. Under the medium VSL valuation, and considering total expected fertility, the benefits of scholarships for all are 3.4 times their costs.

VI. Conclusion

The UN *Call to Action on Education Investment* states that governments in low- and lower-middle-income countries "shall allocate at least 4–6 percent of GDP and at least 15–20 percent of total public expenditure to education, protecting public education budgets from the constrained fiscal environment resulting from the COVID 19 pandemic and the global economic crisis." Yet the IMF estimates that the median education budget in sub-Saharan Africa was equal to about 3.5 percent of gross domestic product in 2020.[33] As sub-Saharan governments struggle to find financing to maintain, let alone expand, free education, understanding the long-term gains from such investments is key. While mothers' education has long been assumed to have a range of positive impacts on children, rigorous evidence to back this claim had been lacking.

By studying the long-term impacts of a randomized scholarship program in Ghana, this paper provides evidence that secondary school education for women does indeed have strong positive impacts on the next generation. Given the size of the child mortality and cognitive development gains, this externality should be considered when governments or international donors consider whether to fund the expansion of free secondary education, particularly in environments where women are disproportionately likely to drop out absent this policy. Our results indicate the primary mechanism through which women's education benefits the next generation is by raising nonmonetary investments (child care and cognitive stimulation) by the primary caregiver of the child. Our rich data show that access to secondary education causes these caregivers to gain the skills to safeguard their children's health and stimulate their children's cognitive development. One interesting question for future research is whether these parenting aptitudes were improved directly by secondary school instruction, indirectly through secondary education enabling students to learn how to learn, or improving students' cognitive abilities. Alternatively, they could be learned from secondary school peers who were generally of higher SES than the marginal students in our study.

[33] https://www.imf.org/en/Blogs/Articles/2024/04/25/sub-saharan-africas-growth-requires-quality-education-for-growing-population (last accessed April 29, 2024).

Impacts on the children of male scholarship recipients are harder to estimate due to data censoring; even after 15 years, only about half of the men in the sample report having had a child, and there appears to be heterogeneous impacts in household formation, limiting our ability to make strong causal claims at this time. What appears clear is that there are no systematic, large positive impacts such as those observed for children of female scholarship participants within that time frame. Duflo, Dupas, and Kremer (2026) discusses the labor market outcomes for scholarship recipients and how the expectation that women have a lower labor force participation rate might mean that the marginal woman induced to attend secondary school by the scholarship would have higher returns to secondary school than the marginal man. Future work is needed to ascertain whether the non-labor market returns for the marginal men were also lower than for the marginal women.

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