Does Child Sponsorship Work? Evidence from Uganda using a Regression Discontinuity Design

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Abstract: International child sponsorship is one of the leading forms of direct aid from households in wealthy countries to needy children in developing countries. We analyze the long-term impacts of child sponsorship by drawing a random sample from a list of formerly sponsored children in a program that was rolled out across villages in Uganda in the early 1980s. Through family interviews, we obtain basic education, employment, and health data on the formerly sponsored children and all of their siblings. We obtain estimates of child sponsorship impacts via a regression discontinuity design, where we use the fact that only siblings who were under twelve years old were eligible for the program when it was implemented. By use of a household fixed-effects estimator, our estimates control for genetic characteristics and family environment to obtain impacts of the program on years of formal education, type of employment, community leadership, instances of major disease, and adult fertility. We find that child sponsorship increases yields an increase of approximately 2.9 years of formal education over a base of 8.4 years, increases the probability of formal employment to 72.1 percent from a base of 55.1 percent, and increases the probability of white collar employment to 31.7 percent from 19.1 percent. We find modest evidence that sponsored children live in higher-quality homes as adults, are more likely to use mosquito nets, and are less likely to smoke or drink alcohol.

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1. Introduction

For millions of households in wealthy countries, international child sponsorship programs represent one of the most direct forms of involvement with the poor in the developing world. Child sponsorship programs involve a set of monthly financial contributions to a needy child in a developing country. Depending on the program, monthly contributions from sponsors typically range from US$25-$35. Funds are applied either directly towards the child's education, food, and health expenses, or to support projects or programs in which the child participates and benefits. Often, there is also correspondence between the sponsor and the child consisting of an exchange of letters and photos; many child sponsorship programs encourage sponsors to give their sponsored children gifts for birthdays and Christmas, and even visit their sponsored children in their home countries. This sponsor-child relationship continues either until the child reaches a specific age or attains a certain level of self-sufficiency.

Child sponsorship programs have been in existence since the 1930s, and they have grown to the extent that today 3.5 million children in developing countries are being sponsored through the eight largest child sponsorship programs (see Figure 1 below). This means that a conservative estimate would put the current flow of funds to sponsored children from developed countries at US$1.6 billion per year, with total international transfers over the last two decades at approximately US$30 billion. Given these non-trivial flows of resources, it is surprising that so little work has been carried out to evaluate the impact of child sponsorship programs. An exception is Kremer, Moulin, and Namunyu (2003), who used a randomized field experiment to analyze the short and medium-term impacts of a Dutch child-sponsorship program that funded new classroom construction and provided students in randomly selected schools a $6 uniform and $3.44 in textbooks. They find that even these relatively low-cost interventions resulted in student beneficiaries attending school half a year longer than in control schools, and advancing a third of a grade farther in their education.

But to our knowledge there has never been a rigorous academic study that has assessed the long-term impacts of child sponsorship into adulthood. Our research studies the long-term impacts of a program operated by one of the world's leading child sponsorship organizations that implemented its program in a number of villages in southern Uganda during the 1980s, shortly after the overthrow of Idi Amin. However, because the organization felt it could obtain the most benefit by working with younger children, a rule was implemented dictating that only children 12 years old and younger were eligible for the program. This arbitrary rule suggests the use of a regression discontinuity design in order to obtain estimates of program impact.

Leading International Child Sponsorship Programs

Organization:	Year Established	Number of Sponsored Children*	Contribution per month	Number of Countries
World Vision	1953	921,000	$30	100
Compassion International	1952	880,000	$32	24
Plan USA	1937	700,000	$24	49
Christian Children's Fund	1938	483,000	$24	31
Children International	1980	300,000	$22	11
Save the Children	1932	120,000	$28	50
SOS	1949	70,000	$28	132
Food for the Hungry	1971	16,000	$28	13
Total (Eight Largest)		**3,490,000**		

* Estimated numbers provided by either the organization or extracted from financial reports.
Denotes faith-based program.

Figure 1

To obtain current data on formerly sponsored children and their families, we obtained lists of former alumni from program sites in four villages in southern Uganda. Because our area of study was concentrated in rural villages where there is little household mobility, our research team was able to conduct interviews with virtually all of the families of these formerly sponsored children, now adults in their mid-twenties to early forties. We obtained basic data on formerly sponsored children and all of their siblings: years of formal education, type of current employment, whether or not they held community leadership positions, instances of major diseases, basic information on the quality of their homes and dwelling construction, and ownership of basic consumer goods such as a cell phone, bicycle, motorcycle, or automobile. We also asked these questions to a random sample of other households in the same villages and in other similar non-program villages.

Our impact estimates on our sample of 809 individuals use whether or not a child was eligible to be sponsored as an instrumental variable (young enough to meet the age rule and happening to live in a program village). Typically sponsorship was limited to only one child per family. So to address the possible issue of parental selection of children based on characteristics unobservable to researchers, we observed in the data that in the majority of cases, the first child that was eligible for sponsorship was the one actually chosen to be the sponsored child. Therefore we employed a second "first-eligible" instrumental variable in our estimations to correct for this potential problem.

Our identification strategy is able to control for genetics as well as home environment and program placement through instrumental variable estimations that use household-level fixed effects. First, we test for and find that siblings of formerly sponsored children are insignificantly different than individuals from households without sponsored children. Then we employ an estimation technique that attempts to statistically ascertain whether there are significant differences between sponsored children and their siblings that are greater than the differences between individuals from non-program households and their own respective siblings. Intuitively, we are estimating the extent to which sponsored children significantly "stand out" from their siblings more than other individuals do from their own siblings, as instrumented for by the program eligibility rule.

What we discover from this exercise is that child sponsorship has a powerful and unequivocally positive impact on formal education and an individual's future employment in our Uganda context. Our most conservative estimate using the "first-eligible" instrument suggests that being an internationally sponsored child yields an average treatment effect of nearly 3 additional years of formal education over a base for non-sponsored children of 8.4 years. Other estimates using merely the eligibility rule as an instrument are higher, up to 4.59 additional years. Analyzing impacts on adult employment of these formerly sponsored children, we find using our more conservative instrument that the probability of formal employment increases to 72.1 percent from a base of 55.1 percent, and that the probability of white-collar employment increases to 31.7 percent from a base of 19.1 percent. This results in a tremendous impact on current salary. Although we will discuss caveats with our income data in what follows, our best estimates suggest that former child sponsorship results in approximately a US$23 to $27 increase in monthly income (contingent on employment) on a base of US$112.8 or about a US$9.40 increase per month per additional year of schooling generated by the sponsorship program.

We also carry out impact estimations on health, quality of home construction, and ownership of consumer durables. In these areas, our impact findings are more modest. We provide some evidence that formerly sponsored children are more likely to engage in healthier behaviors (less likely to smoke and drink, and more likely to have mosquito bed nets in their homes), but they are no less likely to have experienced infections from major diseases, and are seemingly even more likely to have had some diseases such as typhoid, perhaps stemming from the additional level of human contact associated with schooling or employment in crowded offices.[1]

While virtually all of our point estimates of impacts on dwelling construction, having indoor plumbing, home electrification are positive, only the impact on home electrification is significant (at the 99% level). Likewise, our impact estimates on ownership of consumer durables nearly all display positive point estimates, but are mostly insignificant with the exception of owning a cell phone, the probability of which increases significantly (at the 95% level) to 72.1 percent from a base of 53.4 percent.

The remainder of our paper continues as follows. Section 2 of the paper reviews some of the literature relevant to our study. Section 3 provides more background on child sponsorship programs,

[1] It is possible that this finding may be due to the increased ability of the formerly sponsored (more highly educated) individuals to more accurately diagnose a past disease, as reported to family members.

the program that we evaluated, and about our sample area in Uganda. Section 4 presents our impact estimation results, and section 5 concludes.

2. Existing Literature

A growing literature has sought to ascertain the most cost-effective methods to induce households to increase investment in children's education. Different programs have used cash transfers, the lure of free meals, the provision of school uniforms, deworming treatments, or free medical treatment in order to provide incentives for families to keep (or send) their children to school.

The widely celebrated (and evaluated) conditional cash transfer program, *Progresa* (later known as *Oportunidades*), was implemented in 1997 by the Mexican government to create financial incentives for families to boost school attendance in geographically and economically poor regions. *Oportunidades* has involved the provision of cash transfer payments to mothers on the condition that their children regularly attend school. The rollout of *Oportunidades* was undertaken randomly to mitigate problems of endogenous program placement and facilitate its evaluation by researchers. Impact evaluations have shown access to the program to be associated with higher school enrollment rates, lower grade repetition and better grade progression, lower dropout rates, and higher school reentry rates among dropouts (Behrman, Sengupta, and Petra, 2005).

Schultz's (2004) research found enrollment rates to be higher in villages with *Oportunidades*, where its impact was positive from grades one through eight. Using difference-in-differences, he finds that on average, enrollment increased by 3.4 percent for all children, finding that the impact in later grades was larger for girls, 14.8 percent relative to 6.5 percent for boys. Benefits have spilled over to non-beneficiaries of *Oportunidades.* Bobonis and Finan (2008) observe a 5 percent increase in enrollment in program communities even among those ineligible for the program.

Studies on similar conditional cash transfer programs, such as *Conditional Subsidies for School Attendance* program in Bogota, Colombia, have shown similar results. Barerra-Osorio et. al. (2008)

implement a randomized field experiment in the context of this program that employs additional treatments for a savings product, and a cash transfer conditional upon tertiary graduation. The two additional treatments, along with the basic treatment, yielded increases in school attendance. The overall average effect of the three combined treatments was in increase in attendance at school by 2.8 percentage points. The students receiving the savings treatment increased enrollment rates by 3.6 percentage points, and those who had the tertiary graduation treatment increased enrollment rates by and 3.3 percent relative to the non-treated control.

Other programs have sought to subsidize different kinds of inputs to schooling. These inputs have ranged from providing free or subsidized school meals, uniforms, textbooks, school construction, and teachers.

Drèze and Kingdon (2001) found that providing a mid-day meal increased female attendance by 15 percent in a study in northern India. Similarly, Kremer and Vermeersch (2004) found that school attendance increased by 8.5 percentage points in preschools that provided free meals, affecting both current students and new students who had never attended school before. Handa and Peterman's (2007) work in South Africa measured the effect of poor nutrition on schooling, finding that educational attainment of children was strongly affected by nutritional status.

Evans, Kremer, and Ngatia, (2008) randomly selected children by lottery to receive free uniforms in a program administered by an NGO operating in Kenya. They found that receiving a school uniform reduced overall school absenteeism by 39 percent and by 64 percent for poorer students who did not previously own a uniform. In a similar geographic area in Kenya, a de-worming medical intervention was implemented in various schools. Miguel and Kremer (2004) discover that this intervention helped to decrease overall disease transmission but also helped to decrease school absenteeism by 25 percent in treatment schools. In addition, they found that there were positive spillover effects to children who attended nearby schools not receiving the de-worming intervention.

In a randomized experiment that provided merit scholarships to girls in the sixth grade of approximately $20 to students to pay for school fees and school supplies, Kremer, Miguel, and

Thornton (2008) found that the intervention caused a 5 percentage point increase in student attendance, and in successful districts, caused a significant increase in both girls' *and* boys' test scores.

Still other experimental studies have attempted to provide incentives to teachers to improve the quality of the education they provide. In response to high teacher absenteeism rates, Glewwe, Ilias, and Kremer (2003) carried out an experimental intervention in Kenya that provided monetary bonuses to teachers based on student test scores. But despite the incentives, teacher attendance did not improve, and instead what they found was that teachers held additional prep sessions prior to an exam, which led only to an unsustainable short-term increase in test scores.

Methodologically, the empirical strategy we use in this paper is similar to that of Duflo (2001) in the sense that we use the age of former students and geographic placement of a schooling treatment as instruments for impact identification. Her study examines the impact of a dramatic expansion of school construction financed by the Indonesian government from 1973-1979. In that time span, over 61,000 schools were constructed. Duflo used an individual's exposure to the program, which was measured by the number of schools built in his or her region of birth, along with age at the time of program inception, to identify impacts on education and wages. She finds the program increased the probability of primary school completion by 12%, and that each new school constructed per 1,000 children contributed to an increase in formal education by 0.12-0.19 years. This implies an increase of between 0.25 and 0.40 years on average per child beneficiary, which then resulted in an increase of between 3 and 5.4 percent in wages, suggesting an economic return to education of 6.8 to 10.6 percent. Moreover, she also finds that those who benefited were among the poorest that would not have had access to a primary school education otherwise.

3. Area of Study and Methodology

3.1 Sample frame

Our child sponsorship partner is a large, faith-based non-profit organization that views itself as a child advocacy ministry with the goal of "releasing children from spiritual, economic, social and physical poverty." Sponsored children in the program receive a number of benefits including subsidized school fees, after-school tutoring, uniforms, school meals, health training, and Christian teaching. In southern Uganda, selection into the sponsorship program is determined at the local church level. The country sponsorship office identifies churches within the country that they choose to partner with in disbursing sponsorship funds. Churches are instructed to select girls and boys from needy, low-income families generally between the ages of three and nine, and children older than twelve are not eligible for sponsorship. Children from both non-Christian and Christian homes are equally eligible to participate in the program. The mean number of years of sponsorship in our sample was 11.33 years.

The sample frame for this research was provided by the child sponsorship program office in Kampala, Uganda. Information on alumni of the sponsorship program was kept at the church-level where the programs operated. Rural areas were selected because there is little migration in these areas, making it easier to locate alumni of the program or family members. The villages ultimately chosen for the study were Jinja, Bugiri, Masaka, Kigasa, Kakooge, and Bombo. Jinja, Bugiri, Masaka, and Kigasa are all treatment villages (where the child sponsorship program has been present for at least 20 years). Kakooge and Bombo are control villages (where the child sponsorship program is not present) but are very similar to the sponsorship villages.

All six areas are within a four-hour drive from Kampala. Jinja is a large village with a significant tourism industry because it lies at the source of the Nile. The other five villages have substantially smaller populations and little tourism. The child sponsorship program developed relationships with local Protestant churches in the four treatment areas, but has yet to establish a presence in the two control villages. The local churches provided a list of former alumni of the program, from which 50 names were randomly selected for interview. In addition, 25 households without sponsored children were randomly chosen who were deemed to be of a similar income group to the sponsored families. The two control villages were chosen based on proximity to treatment villages. Fifty households randomly chosen were surveyed from each control village, also deemed to be highly similar to sponsored families. Indeed, our data show that based on observable indicators these non-sponsored families (apart from the sponsored children) were insignificantly different than those who had the sponsored children. In total we obtained data on 188 adults who were formerly sponsored children, 297 siblings of formerly sponsored children, and 324 adults from non-sponsored families for a total of 809 adults included in the survey.

Table 1 presents summary statistics from each village. Adult family members were interviewed to provide data for adult siblings from each household. These adults were not necessarily the formerly sponsored individuals, but were able to provide basic data concerning the members of the family; those interviewed consisted of parents or other siblings. Data was collected on all siblings in the non-sponsored household families. Data on individuals 18 years of age or younger was discarded from the study because our desire was to focus on the long-term effects of sponsorship.

3.2 Survey

The survey focused on collecting basic information regarding education, occupation, leadership positions within the community, health, dwelling, and ownership of consumer durables including cell phones, bikes, motorcycles, scooters, and automobiles. All questions were designed to be simple and discrete in nature in line with our empirical approach that sought to identify the basic status of adult individuals (level of formal education, employment status--formally employed, white collar employment, agriculture, etc.), dwelling construction of their homes (mud, tin, wood, cement, or brick) past encounters with major local diseases (typhoid, measles, yellow fever, malaria). Except for our income data, we eschewed questions that asked for finely tuned or continuous variable data on individuals, realizing that with this kind of follow-up methodology such data may be unavailable to family members, or inaccurate. Many of our questions were obtained from the Demographic and Health Survey (DHS) that was conducted in Uganda in 2006, and World Health Organization survey questions. We also referenced the World Bank's Living Standards Measurement Study (LSMS) education modules.

We did collect data on the estimated income of family members, but as we expected, in many cases family members were not able to estimate monthly income of one or more employed family members. In response, we broke down occupations into six categories which yielded the respective average monthly wages in $US our sample: (1) Professional Service Sector Workers (US$213.96); (2) Business, Accounting and Technology ($191.87); (3) Retail and Shop Owners ($82.81); (4) Blue Collar Workers (welders and mechanics--$60.46); and (5) Farmers and Fishermen ($50.74); and (6) Simple Services such as maids, custodians, and bicycle taxis. We used these figures in the appropriate occupations to substitute for actual wage data for 209 of the 427 employed individuals in our study. For this reason, we include this estimated income data because we believe these averages substantially reflect the differences in wages associated with different categories of occupations, but it is clearly not as accurate as our discrete data.

3.3 Empirical Strategy

Our empirical strategy flows from a number of steps. First, we seek to ascertain whether the non-sponsored siblings of formerly sponsored children are statistically different than members of non-sponsored families. If they are not, this could be due to either selection effects into the treatment when the program was rolled out, or it could be due to positive or negative spillovers from treated children to their non-treated siblings. If we find no difference between these two groups, then the siblings who were ineligible for the program based on the age rule and the program placement across villages may serve as a reference group for those who were eligible for the program by virtue of their age and geographic location.

Second, we would like to not only examine "naïve" OLS (ordinary least squares) regressions of possible treatment effects on program participation (sponsorship), but also intention to treat (ITT) estimations where we run OLS estimations of impact variables on program eligibility based on the age rule and program placement.

Third, we would like to use the eligibility rule as an IV (instrumental variable) in two-stage least-squares estimations to obtain an estimation of an average treatment effect for those who were formerly sponsored children. We note, however, that there could be issues with using only eligibility criteria as an instrumental variable. Because of the ubiquitous needs of families in the area and limited child sponsorship resources, the child sponsorship program discouraged more than one child per family for being sponsored. Since often more than one child per family was eligible for sponsorship, there conceivably could have been strategic selection among all eligible children in a family towards the child whom parents believed showed the most promise (or in some instances perhaps the opposite case--the child who was the most desperately in need of help). We think the potential for this is somewhat limited as children are at such a young age at the time of enrollment that it may be difficult to ascertain a child's potential, even for parents.

But to correct for this possibility of self-selection, we also carry out estimations using a "first-eligible" instrument. The "first-eligible" instrument is a dummy variable equal to one if the child was the first child eligible for the sponsorship program. For example, when the program was rolled out, our "first-eligible" instrumental variable is equal to one for the oldest child under the age of twelve during the year of rollout, and zero otherwise. If the child sponsorship program in the village already existed before any children in the household were old enough to be sponsored, then the instrumental variable is equal to one for the first child, and zero otherwise. In this way, the "first-eligible" variable satisfies the two criteria for a good instrument. First, the instrument is highly correlated with being a formerly sponsored child ($\rho = 0.710$). Those who happened to have been the first child eligible for sponsorship have a vastly higher probability of being sponsored than other siblings. In fact, 80.1 percent of those children sponsored and our Uganda villages were first-eligible children. Secondly, the instrument satisfies the exclusion restriction: such children are no more likely to have better adult outcomes than their sibling counterparts except via the child sponsorship program after we control for sibling order in second-stage IV estimations.

4. Empirical Results

4.1 Set-up

Our first estimation results are presented in Table 2, where we seek to ascertain whether the siblings of sponsored children are different in terms of outcome variables than individuals from non-sponsored families in our sample.

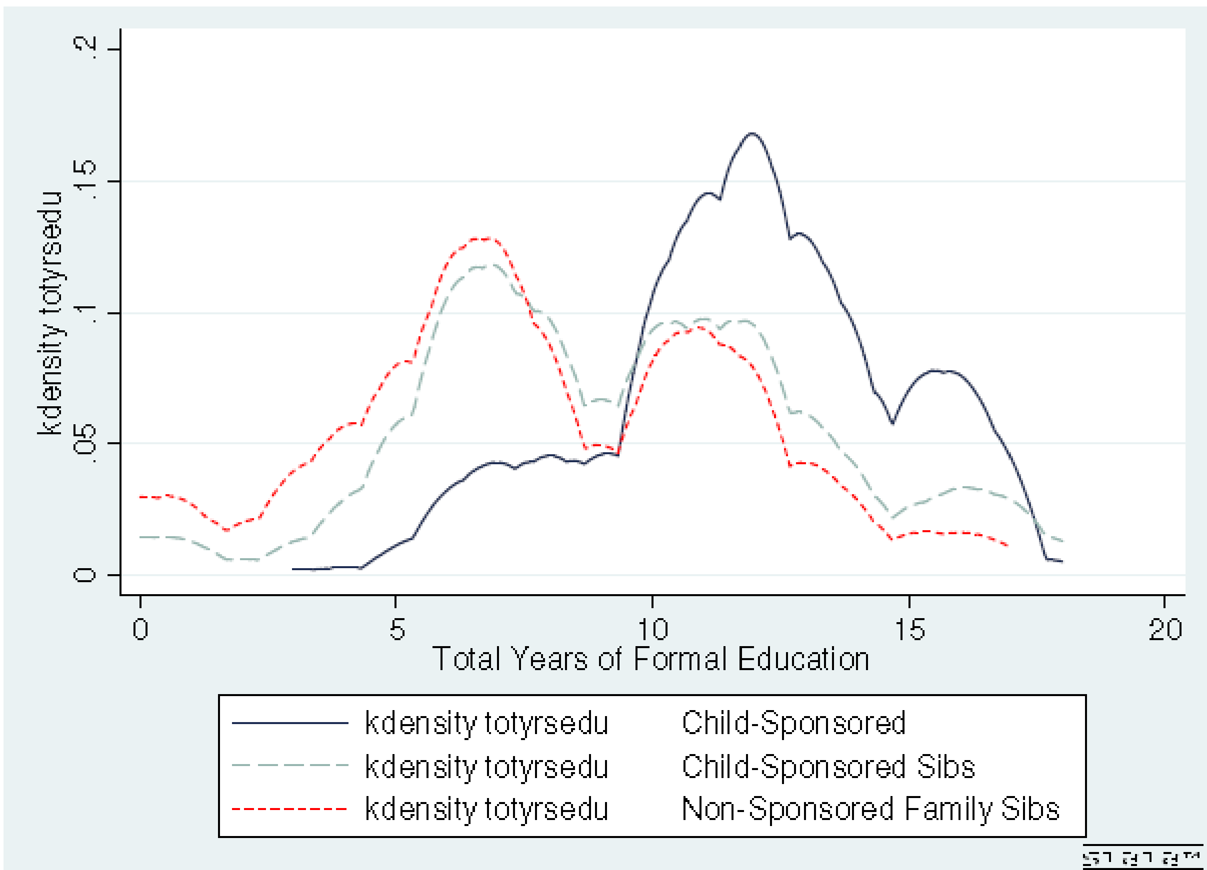

Figure 2

In Figure 2 we plot kernel densities over total years of formal education. Figure 2 gives kernel densities for formerly sponsored children, siblings of formerly sponsored children who were not sponsored, and individuals from families in our villages that did not contain any sponsored children (Epanechnikov kernel, bandwidth=0.75). What is immediately noticeable from Figure 2 is the huge difference in the density function over formal education between formerly sponsored children and everyone else. Because an increase in formal education is a fundamental goal of the child sponsorship program (and obvious from Figure 2), we examine differences between non-sponsored siblings of formerly sponsored children and individuals from families that did not have sponsored children. We can see visually from Figure 2 that there are small differences between these groups in formal education levels. As a result, it would seem that family selection effects into the program, or spillovers from the program to other siblings are small if they do exist, or possibly that they counteract one another.

In Table 2 we statistically test for differences between non-sponsored siblings of formerly sponsored children and individuals from families that did not have sponsored children over formal schooling, employment, and village leadership. Here there are two main reasons we might expect non-sponsored siblings from families with sponsored children to be different than those from families without any sponsored children. The first reason could be household-level selection effects into the program that would be likely to manifest in households with sponsored children either doing better than randomly selected households (via positive self-selection) or worse (if the programs selected children from the most disadvantaged households for participation). The second reason could be from spillover effects of sponsored children to other siblings. Especially in education, we would expect any spillovers would affect *younger* siblings because younger siblings are more likely to be influenced by the educational choices of older siblings than vice versa. Moreover, when younger siblings observe older siblings obtaining more education, they are typically still in school and at an age where it is easier to emulate the educational patterns of a more educated sibling.

In these OLS estimations where we control for gender, age, age-squared, and birth order using village fixed effects, in none of our estimations do we find significant differences between the siblings of sponsored children and siblings in non-sponsored families. We therefore cannot reject the null hypothesis that there is no statistically detectable difference in adult outcomes between siblings of sponsored children and individuals from families that did not have sponsored children. Moreover, while point estimates point to the possibility of positive spillover effects to the younger siblings of sponsored children, we find little statistically significant evidence of positive spillovers in Table 2 in education or employment.

Using these results, we will use the adult outcomes of ineligible siblings of sponsored children as a benchmark counterfactual for what would have been the outcomes of sponsored children had they *not* been sponsored. While it is of course possible that selection effects into sponsorship or spillover effects may exist, because we find no significant evidence of them statistically we feel comfortable using household fixed effects and our eligible and first-eligible instruments in order to identify the effects of the program on treated individuals. By using non-eligible siblings as a *defacto* counterfactual, we are therefore able to control for genetics, parental attributes, and the influence of the household environment on children as they grow up. We do this in our estimations in the latter columns of Tables 3 through 6 via our IV estimations which use our eligibility and first-eligible instruments with household-level fixed effects. In this way, week seek to estimate the average treatment effects of child sponsorship.

Figures 3 and 4 use non-parametric estimations to illustrate the discontinuity in program participation and formal years of schooling, our most important impact variable. We estimate both variables non-parametrically using a locally weighted regression (Epanechnikov kernel, bandwidth = 0.8). The eligibility rule at the time of program rollout in our treatment villages was that children under 12 years of age would be eligible. (Note however, that the probability of program entry declines after age five, which was considered the ideal age of entry.) While we find some "cheating" on this rule (a few children sneaked into the program when they were a year or two older), it is clear that the eligibility rule allows for a "fuzzy" regression discontinuity design. The mean probability of sponsorship among children in eligible villages in our sample was 39.1% if they were under aged 12 when the program was introduced. For those 12 and over the probability was only 2.6%.

4.2 Estimates of Impact on Education

In Tables 3 and 4 we present estimates on the effects of child sponsorship on formal education. Column 1 in Table 3 presents the "naïve" OLS estimation that regresses total years of formal education on the child sponsorship treatment controlling for gender, age, age-squared, birth order, number of siblings, and mother's and father's schooling with fixed effects at the village level. This estimation shows a coefficient of 2.59 additional years of formal schooling from child sponsorship, significant at the 1 percent level. Columns 2 and 3 show OLS regressions on our two instruments, with the "first-eligible" instrument significant at the 1 percent level for the latter estimation, carrying a coefficient of 1.83 years. In Column 4 we show our IV estimations using the "eligible" instrument with village-level fixed effects, where the point estimate on formal years of education is 3.17. All of our instrumental variable estimations use bootstrapped standard errors with standard errors clustered at the either the respective village or household level.

Column 5 uses the "first-eligible" instrument with village-level fixed effects, where here we obtain a very similar average treatment effect point estimate of 3.01 additional years of formal education. In all of our estimations using village fixed effects in Table 3, mother's and father's education have a strong, significant, and equal effect on children's education.

Columns 6 and 7 in Table 3 employ our most robust estimation method, where we use 2SLS to estimate the following first and second-stage equations in (1) and (2):

$$T_{ij} = \boldsymbol{X}'_{\boldsymbol{ij}}\boldsymbol{\phi} + \lambda z_{ij} + u_{ij} \tag{1}$$

where T is the probability of treatment for individual i in family j (of being sponsored), X is a vector of control variables for gender, age, age-squared, and birth order in the family, z is one of our two instrumental variables, $\boldsymbol{\phi}$ is a vector of coefficients on control variables and λ is the coefficient that measures the effect of the instrument on take-up, and u_{ij} is the error term. Our second-stage impact estimation is given by

$$y_{ij} = \alpha_j + \boldsymbol{X}'_{\boldsymbol{ij}}\boldsymbol{\beta} + \gamma\hat{T}_{ij} + \varepsilon_{ij} \tag{2}$$

where y is a relevant impact indicator, α_j is a household fixed effect, $\hat{T}$ is the instrumented probability of receiving the treatment, $\boldsymbol{\beta}$ is a vector of coefficients that show the effects of the control variables on our impact variable, γ is measures the impact of child sponsorship on y, and ε_{ij} is the error term in (2).

Column 6 uses the "eligible" instrument to estimate equation (2), where the average treatment effect increases to 4.59, significant at the 5 percent level. What we believe to be our strongest estimate of the average treatment effect is in Column 7, where we employ the "first-eligible" instrument with household-level fixed effects given in (2) and obtain an estimate of 2.88 additional years of schooling from child sponsorship. We therefore feel comfortable concluding conservatively that child sponsorship lead to an increase of nearly three years of additional formal education in our sample over a mean of 9.18 and a base for non-sponsored children of 8.37 years, a substantial and fairly dramatic impact on years of formal education.

Table 4 estimates equation (2) for school completion at various levels using our two different instruments in conjunction with household-level fixed effects. The first group of columns uses the more simple eligibility instrument; the second group of columns uses the "first-eligible" instrument. Using the "first-eligible" instrument, sponsorship significantly increases an individual's probability of completing primary school by 20.7 percentage points over a mean of 63.8 percent completion in our sample, and is significant at the 95 percent level. Using the simple eligibility instrument, we find no effect on the completion of ordinary secondary education (trade-oriented rather than university preparation), but we do find significant effects using the "first-eligible" instrument--an increase in the probability of 48.4 percentage points over a strikingly small mean probability of 14.4 percent. The simple eligibility instrument in Column 1 yields an increased probability of advanced secondary school completion of 0.38, while the point estimate for the "first-eligible" instrument in Column 2 is 0.19, both significant at the 99 percent level. Neither type of IV estimation points to a significant increase in the probability of university completion. This makes some sense, because child sponsorship is typically terminated after the secondary school years.

4.3 Impact on Economic Outcomes

Table 5 summarizes impacts on education and includes impact estimations on economic outcomes, leadership, health, dwelling, and consumer purchases. Each cell in Tables 5 and 6 presents the estimated average treatment effect from a single regression. Each of these regressions includes controls for gender, age, age-squared, and birth order. Columns 1 and 2 use fixed effects at the village level and so are able to control for household-specific variables; Column 1 includes controls for number of siblings, and mother and father's education, while Column 2 includes controls for only the former. Column 1 presents the "naïve" OLS regression of the impact variable on the child sponsorship treatment (which we find is not often different from the 2SLS estimates in Column 5), while as in Table 4, Columns 2 and 3 present what is essentially an unweighted estimate of the intention-to-treat, unweighted because the treated group (formerly sponsored children) are over-weighted in the sample relative to their actual weight in the population. Columns 4 and 5 give our IV estimations in equation (2).

The first row variable under *economic outcomes* in Table 5 shows the estimated impact of child sponsorship on formal employment in adulthood. Only 55.1 percent of our simple of 809 adults were formally employed. Some of these were women working in the home, but the formal employment rate of males was only 59.5 percent, not too much higher than that for women, 51.1 percent. Those who were "unemployed" sought casual jobs around the village, but at the time had no steady job that paid a cash wage. Our IV estimations in Columns 4 and 5 estimating (2) yield somewhat similar point estimates, with an estimated increase in the probability of formal employment using the "first-eligible" instrument rising by 17.0 percentage points, significant at the 1 percent level.

Child sponsorship also displays a strong effect on the probability of white-collar employment during adulthood. The mean rate of white-collar employment in our sample was only 19.1 percent. These kinds of jobs typically included working in a business, as a government official, or as a teacher. Estimations using both of our instruments are positive and significant at the 10 percent level, the simple eligibility instrument yielding an increase of 45.4 percentage points and the "first-eligible" instrument yielding a more modest point estimate of 12.6 percentage points, which is much more in line with the simple OLS estimation in Column 1 of 14.2 percentage points. Since white-collar employment is clearly a measure of upward mobility into what might be called the "middle-class" in Uganda, we take this as a strong and significant impact of the program.

The mean monthly salary for an employed individual in our survey is $112.30 per month, or about $1,348 per year. We present our impact estimations on income with the understanding that our income data are not as individually precise as other data in the survey. However to the extent that our income data reflect wage differences between the six previously mentioned occupational categories we felt it was important to show how these differences in occupational employment result in significant increases in income as a result of the program.

Our estimate on the impact of sponsorship on monthly salary in the regression in Column 1 yields a statistically significant increase of US$23.62 per month. This is similar to the point estimate using our "first-eligible" estimate in Column 5 of US$25.88, however the latter is statistically insignificant. Given the increased probability of white-collar employment, however, both of these estimates appear reasonable. Column 1 and Column 5 also yield similar estimates on the probability of sending remittances to other family members, an increase of approximately 4 percentage points on a base probability of 26.8 percent, but with only the former being significant (at the 1% level).

4.4 Impact on Community Leadership

We find some evidence that child sponsorship is associated with greater levels of community leadership, however, although point estimates are virtually always positive, they are for the most part statistically insignificant in our IV estimations. OLS estimations in Columns 1, 2, and 3 show "Community Leader" to be correlated with program participation and program eligibility. Village leadership (a higher level of leadership) is also significantly correlated with program eligibility, and is also significant at the 5 percent level using the eligibility instrument. In the basic OLS estimation, church leadership (pastor, teacher, lay leadership) is strongly correlated with child sponsorship, which is expected given the Christian background and perspective of the sponsorship organization. IV estimations on church leadership all carry positive signs but are not significant.

4.5 Impact on Health

One of the stated goals of the child sponsorship organization we worked with was to promote health and preventative hygiene among children. We do find evidence that formerly sponsored children take greater precautionary health measures as adults, but we do not find reduced instances of major diseases. All of the estimations in Table 5 on "Smoker" carry a negative sign although none are statistically significant. Most of the estimations on "Drinks Alcohol" also carry a negative sign, but only estimations in Columns 1 and 2 are significant (at the 10% level). There is fairly strong evidence that being a formerly sponsored child is associated with much higher use rates of mosquito net usage, this in a region where malaria was so widely reported among those involved in our study that we were unable to find sufficient individual variation in it to use it as an impact variable. The mean probability in the sample that a household has a mosquito net in their home is 41.3 percent. IV estimations using our "first-eligible" instrument show an increase in the mosquito net use rate of 17.3 percentage points from child sponsorship, significant at the 1% level.

Our study does not find child sponsorship to be associated with lower reported rates of major diseases common to the area. Adults who were sponsored as children seem to have a higher probability of having had the measles, even in the IV estimation shown in Column 4. We find a similar result for the estimations on typhoid in Column 5 using the "first-eligible" instrument. After consulting with public health experts, we believe this result could be due to two factors. It is possible that increased human contact from more schooling could leave more educated individuals more exposed to contagious diseases. This may continue into adulthood when these individuals work more closely with people in leadership positions and in offices where contagion is more likely, and in some respects these more highly educated individuals also may be under more stress. A second possibility is that more educated people are better able to diagnose their own diseases, or to be able to afford to have their infirmities diagnosed by a physician. For these reasons we are obviously hesitant to contend that child sponsorship is associated with a greater probability of disease, but the correlated nature of this relationship in our data may make investigation of this phenomenon worthwhile.

4.6 Impact on Dwelling and Consumer Good Ownership

We interviewed family members about the type of homes in which their siblings lived, focusing on material used in their home construction, which is often an outward sign of upward mobility from poverty. All of our estimations carry positive signs for an increased probability of a formerly sponsored child living in a (better constructed) house made of concrete or bricks, although none are statistically significant. The base probability in our sample of having a home with electricity is 25.3 percent. Our IV estimation using the "first-eligible" instrument shows an impact from child sponsorship with an increased probability of 15.9 percentage points, significant at the 1% level.

Although our study region is economically very poor, cell phone ownership is quite common in all villages, 53.4% in our sample. In all estimations in Columns 1 through 5, cell phone ownership is strongly correlated with being a formerly sponsored child. However, the point estimate from our IV estimation using the simple eligibility rule in Column 4 of 45.6 percentage points seems unreasonably large. Estimated coefficients in Columns 1 and 5 are equivalent, both suggesting an added probability increase of 18.7 percentage points, both being significant at the 1% level. While bicycle and motorcycle or scooter ownership are significantly correlated with child sponsorship in the OLS regressions in Column 1, none of these (or automobile ownership) are positively correlated with child sponsorship in the IV estimations in Columns 4 and 5.

4.7 Impact of Education on Dependent Variables

While the focus of this research is understanding the impact of child sponsorship, because child sponsorship leads to significant increases in formal education, we can therefore look at the impact increased years of formal education may have had on some of our dependent variables. In Table 6, we use our instrumental variables of program eligibility and "first-eligibility" to examine the impact of *education* itself, as stimulated by the child sponsorship program.

In the section under economic outcomes in Table 6, we present various estimates of the impact of education on monthly salary in U.S. dollars. Point estimates of increases in salary per additional year of formal education range from \$9.42 and \$13.83 per month, with only OLS estimations significant at the 1% level. This is consistent with our finding in Table 5 of an increase in salary of about \$25 per month because child sponsorship appears to increase formal schooling by about three years. In the row below, we present results of what is essentially a Mincerian wage regression, in which we estimate log salary as a function of schooling and our controls that include age and age-squared, which may be close proxies for experience. Our OLS regressions using the village and household fixed effects, respectively yield point estimates that suggest a 10.7 and 10.5 percent increases in salary per additional year of education, on the upper end of the range found in Duflo (2001). Our IV estimation using the "first-eligible" instrument in Column 4 yields a point estimate of 5.7 percent, just below the lower end of her range, yet not statistically significant.

We generally find point estimates from the impact of education on health variables to replicate those of the child sponsorship program itself in sign and magnitude, increasing the prevalence of health-promoting behaviors such as reduced smoking and drinking and the increased use of mosquito nets. However, we also continue to see higher probabilities of reported disease associated with higher levels of education.

Greater levels of education are also strongly correlated with electrification of a household, with the probability of having electricity in a home increasing by 5.7 points with every year of formal education in our "first-eligible" IV estimation (significant at the 1% level), although other point estimates are lower. While we find increases in the probability of cell phone ownership that are similar to those we found in Table 5, yet we find only positive point estimates on most of our transportation goods, with none achieving statistical significance.

5. Summary and Concluding Comments

Although international child sponsorship has come to represent one of the most widespread points of contact for households in wealthy countries with the poor in developing countries, there have been no studies to date that have rigorously analyzed the long-term impacts into adulthood on the child beneficiaries of these programs. This research presents a first attempt at measuring these impacts. We study a leading child sponsorship program that began operating in Uganda in the 1980s, where early entrants into the program are now adults in their mid-twenties to early forties.

The technique that we use to obtain statistical identification of impacts examines whether the differences in adult outcomes between sponsored children and their siblings are significantly greater than the differences in these outcomes between individuals in families without a formerly sponsored children and their own siblings. Through this use of a household fixed-effects estimator, we are able to control for genetics, household environment, and other outside factors that are common to siblings to try to isolate the impact of sponsorship. To correct for possible endogeneity over sibling choice into treatment, we instrument for child sponsorship using an age (and village) eligibility rule, and the fact that in the majority of cases the sibling that was actually sponsored was the first child who was eligible for sponsorship based on child's age when the program happened to enter his or her particular village.

Do child sponsorship programs work? The evidence that we present in this paper suggests that indeed they do, and have large-magnitude impacts, especially in the areas of education and future employment. What we believe to be our best instrumental variable estimation, our "first-eligible" instrument, suggests that child sponsorship in our Uganda study is associated with an average increase in formal schooling of approximately 2.88 years. Our other instrument, simple eligibility for the program, puts this average treatment effect at 4.59 years. We find that child sponsorship increases the probability of formal employment to 72.1 percent from a base of 55.1 percent, and increases the probability of white collar employment to 31.7 percent from 19.1 percent. Contingent on being formally employed and controlling for age and gender, child sponsorship appears to increase monthly salary by about $25 per month over a monthly income base in our sample of $112. All of these estimates are robust even when we add a variable for "oldest child" in addition to sibling order in our specifications to account for the fact that oldest children may have special outcomes even independent of a numerical sibling order. We find that in most cases our "first-eligible" IV point estimates are fairly close to our OLS estimates for our significant impacts. Thus our results concur with Duflo (2001) who finds that selection bias may not be as great of an issue in education estimations as may have been previously believed.

Our estimates of the impact of child sponsorship on formal schooling are substantially greater than those found by Kremer, Moulin, and Namunyu (2003), who report about 0.30 years of additional schooling as a result of sponsorship. But their impact results examine a much less costly intervention than that undertaken by the child sponsorship program we analyze. Whereas the program they analyze mandated basic provisions to sponsored children, principally uniforms and school textbooks, the program we study provided students with a much more comprehensive and holistic support structure: not only basic school provisions, but also tutoring services, school fees, health education and basic healthcare, and Christian teaching, nurture and development opportunities. The cost of these services was about $28 per month to the sponsor, and the mean number of years of sponsorship in our sample was 11.33 years. Thus the total (non-discounted) cost of sponsoring a single child to the average sponsor therefore was approximately US$3,806.

Using our more conservative impact estimate of 2.88 additional years of schooling from child sponsorship, this puts the average cost of an additional year of schooling from the program at $1,321 per year, per sponsored child. While it is clear that the impacts of the child sponsorship extend far beyond added years of schooling, it is worth comparing this figure to the costs of increasing formal schooling from other interventions. For example, Skoufias (2005) estimates that *Oportunidades* program in Mexico increased formal schooling by an average of 0.66 years (0.72 for girls, and 0.64 yrs for boys). Conditional cash transfer payments in his study ranged anywhere from $4.62 to $21.67 per month, and are received from the third to the eleventh years of school. Assuming the average of these two figures, or $13.15, eight years of conditional cash transfers would cost $1,420 and would result in an average cost per additional year of school of $2,151.

While child sponsorship appears cheaper than these conditional cash transfer cost estimates, both of these figures are dramatically higher than the $3.50 per added year of schooling estimated from child de-worming treatments (Miguel and Kremer, 2007), or the cost per added schooling year from the free provision of school uniforms (Evans, Kremer, and Ngatia, 2008). However, these latter estimates are based on increased attendance during a given schooling year rather than higher levels of grade completion, and the two may not be comparable since the opportunity cost of child schooling increases with age. But the point remains that while our estimates indicate that international child sponsorship programs have some rather dramatic impacts on schooling and employment, we make no claims in this research that child sponsorship represents the most cost-effective path to these ends.

When addressing issues of cost efficiency it is important to understand that the development of international child sponsorship programs fundamentally arose from their usefulness as a marketing tool for mobilizing resources in rich countries to fight poverty in poor countries. As administrators in these programs have recognized for decades, contact with an individual child creates a focal point to nudge donors of modest incomes towards contributing part of their income to alleviating world poverty. In fact, it is likely that many of these resources may not have been mobilized at all if it were not for the ability of international child sponsorship to foster a commitment to poverty alleviation via a wealthy household's commitment to a particular overseas *child*. In this way, international child sponsorship programs may mobilize these additional resources by drawing heavily upon the same group of psychological and moral instincts people possess to care for their own children. Even in difficult economic times, the commitment of donors to the wellbeing of "their child" is likely to be much greater than their commitment to a large well-intentioned, but faceless, non-profit organization. There is at least anecdotal evidence of this: During the first year of the 2008-09 recession when giving to most U.S. charities declined sharply, World Vision reported that the percentage of those who remained faithful with their monthly donations to sponsored children showed no sign of decline during this period (Kennedy, 2009). In summary, while child sponsorship programs are unlikely to be the most cost-effective method to increase child schooling, they may be among the most effective methods to *mobilize resources* that significantly increase child schooling.

While we believe that the results from our research are internally valid in the case of the child sponsorship program we study in southern Uganda, further study must be carried out in other contexts to ascertain their external validity to other contexts. We analyze the case of a child sponsorship program operating in an area with only modest opportunities for schooling and subsequent employment. We believe impact studies of child sponsorship programs implemented among at-risk children in areas with greater opportunities for schooling and economic activity are likely to show smaller impacts on levels of formal schooling, but perhaps greater impacts on subsequent employment and income.

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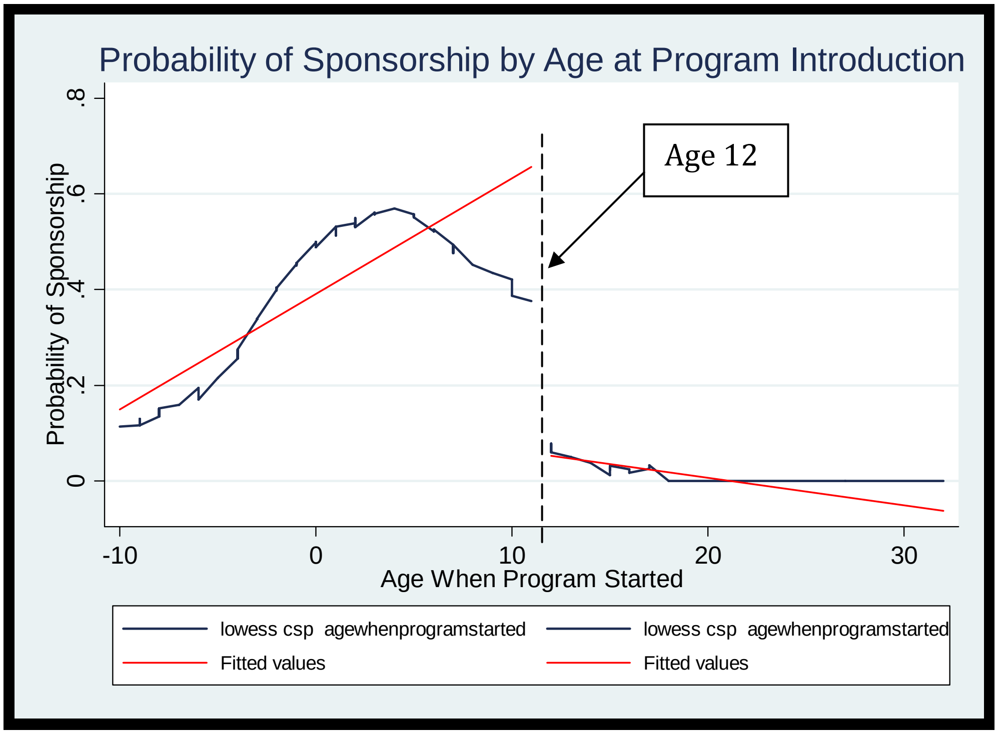

Figure 3: Discontinuity of Program Participation at Age 12
(Locally Weighted Regression, Epanechnikov kernel, bandwidth = 0.8)

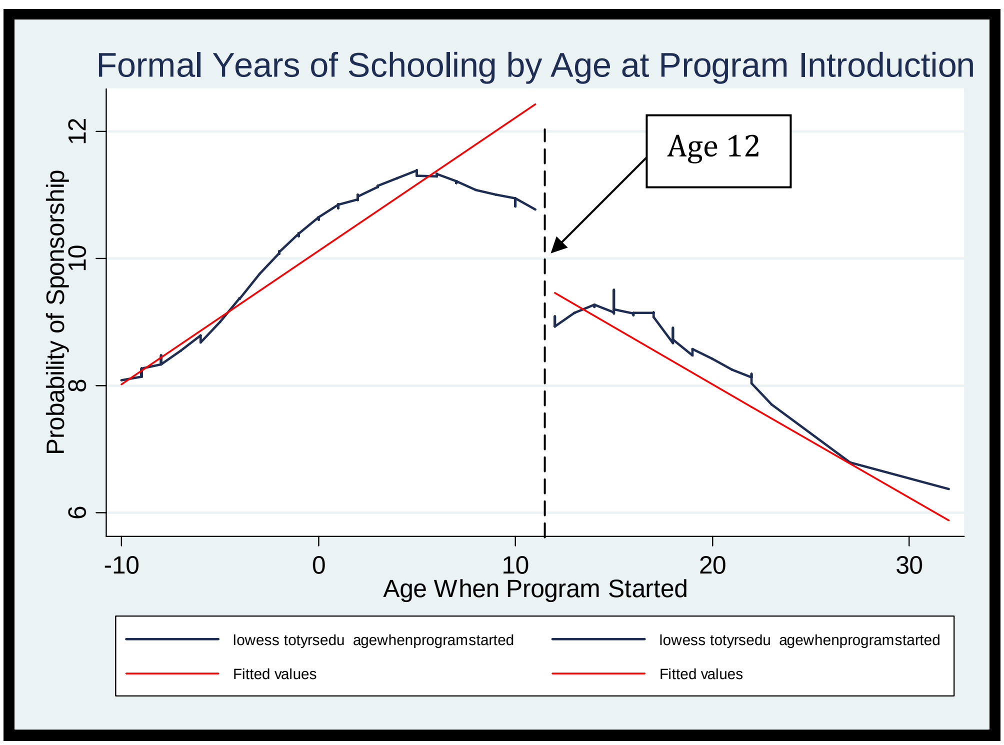

Figure 4: Discontinuity of Formal Education at Age 12
(Locally Weighted Regression, Epanechnikov kernel, bandwidth = 0.8)

Table 1: Summary Statistics

Means with standard deviations in parentheses

Variable	Kigasa	Bugiri	Jinja	Kakooge	Bombo	5 Villages
Num of Households	64	40	74	47	36	261
Num of Observations	231	138	148	189	103	809
Demographic Variables:						
age	27.965	26.348	24.088	33.434	33.544	28.968
	(7.524)	(6.331)	(4.553)	(10.707)	(8.050)	(8.642)
sex	0.489	0.572	0.446	0.450	0.408	0.476
	(0.501)	(0.497)	(0.499)	(0.499)	(0.494)	(0.500)
totyrsedu	8.922	8.899	12.750	6.571	9.835	9.185
	(3.540)	(3.575)	(2.876)	(4.004)	(2.873)	(4.003)
employed	0.641	0.543	0.399	0.514	0.647	0.551
	(0.481)	(0.500)	(0.491)	(0.501)	(0.480)	(0.498)
Treatment Variables:						
csp	0.281	0.341	0.514	0	0	0.232
	(0.451)	(0.476)	(0.502)	0	0	(0.423)
sibparticipated	0.589	0.616	0.514	0	0	0.367
	(0.493)	(0.488)	(0.502)	0	0	(0.482)
Household Variables:						
married	0.481	0.587	0.216	0.451	0.618	0.461
	(0.501)	(0.494)	(0.413)	(0.499)	(0.488)	(0.499)
electricity	0.136	0.099	0.676	0.079	0.388	0.253
	(0.343)	(0.300)	(0.470)	(0.270)	(0.490)	(0.435)
numchildren	1.680	1.674	0.642	3.344	3.602	2.122
	(1.927)	(2.030)	(1.599)	(3.086)	(3.454)	(2.658)
medfacdist	3.435	2.723	1.318	3.483	2.487	2.773
	(4.477)	(2.538)	(1.031)	(2.834)	(5.466)	(3.628)
land	0.471	0.348	0.095	0.189	0.155	0.274
	(0.500)	(0.478)	(0.295)	(0.393)	(0.364)	(0.446)
numsibs	5.532	5.638	2.392	5.053	3.767	4.639
	(3.437)	(2.614)	(2.059)	(2.643)	(2.045)	(2.984)
Health Variables:						
anemic	0.091	0.043	0.095	0.200	0.233	0.127
	(0.288)	(0.205)	(0.294)	(0.401)	(0.425)	(0.333)
yellowfever	0.285	0.114	0.041	0.401	0.277	0.234
	(0.452)	(0.319)	(0.198)	(0.492)	(0.450)	(0.424)
measles	0.722	0.493	0.500	0.880	0.738	0.678
	(0.449)	(0.502)	(0.502)	(0.326)	(0.442)	(0.468)
typhoid	0.127	0.118	0.189	0.439	0.184	0.215
	(0.334)	(0.324)	(0.393)	(0.498)	(0.390)	(0.411)
vitamina	0.868	0.663	0.608	0.708	0.990	0.752
	(0.340)	(0.475)	(0.490)	(0.456)	(0.100)	(0.432)
washhands	0.995	0.962	0.905	0.971	0.990	0.965
	(0.073)	(0.192)	(0.294)	(0.167)	(0.099)	(0.183)
netcount	0.469	0.610	0.534	0.429	0.612	0.515
	(0.716)	(0.740)	(0.514)	(0.604)	(0.768)	(0.668)
Leadership Variables:						
leader_comm	0.069	0.080	0.110	0.090	0.157	0.095
	(0.254)	(0.273)	(0.313)	(0.288)	(0.365)	(0.293)
leader_chu~h	0.148	0.162	0.184	0.145	0.178	0.160
	(0.356)	(0.369)	(0.389)	(0.353)	(0.385)	(0.367)
leader_vil	0.035	0.036	0.014	0.053	0.119	0.046
	(0.184)	(0.188)	(0.116)	(0.225)	(0.325)	(0.209)

Table 2: Are Siblings of Sponsored Children Different than Others in the Sample?

(OLS Estimations; Standard Errors Clustered at the Village Level)

	(1) Years of Formal Schooling	(2) Completed Primary School	(3) Completed Adv. Sec. School	(4) Formally Employed	(5) White Collar Employed	(6) Estimated Salary ($US)	(7) Village Leader	(8) Comm. Leader
Sibling Sponsored	-0.041	-0.020	-0.053	0.124	0.049	-7.129	-0.014	-0.057
	(0.596)	(0.094)	(0.056)	(0.077)	(0.059)	(19.432)	(0.035)	(0.046)
Older Sib Sponsored	0.346	0.069	0.056	-0.124*	-0.112**	-9.890	0.042	0.092**
	(0.570)	(0.090)	(0.053)	(0.074)	(0.056)	(20.281)	(0.034)	(0.044)
Sex (1 = male)	0.069	0.044	0.041	0.091**	-0.055**	-14.421	-0.001	0.046**
	(0.286)	(0.045)	(0.027)	(0.037)	(0.028)	(9.892)	(0.017)	(0.022)
Age	0.262***	0.031**	0.017**	0.063***	0.038***	4.450	0.009	0.004
	(0.090)	(0.014)	(0.008)	(0.012)	(0.009)	(3.144)	(0.005)	(0.007)
Age-Squared	-0.004***	-0.000**	-0.000**	-0.001***	-0.000***	-0.053	-0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.039)	(0.000)	(0.000)
Birth Order	0.105	0.016	0.012	0.014	0.014*	0.371	-0.008	-0.014**
	(0.081)	(0.013)	(0.008)	(0.011)	(0.008)	(3.032)	(0.005)	(0.006)
Constant	4.119**	0.095	-0.198	-0.843***	-0.527***	33.747	-0.128	-0.058
Observations	621	618	618	610	621	319	597	607
R-squared	0.03	0.01	0.01	0.16	0.07	0.02	0.03	0.06

* significant at 10 are; ** significant at 5%; *** significant at 1%. Clustered standard errors at the village level in parentheses.

Table 3: Total Years of Formal Education as a Function of Child Sponsorship

(OLS and Instrumental Variable Estimations; Clustered Standard Errors)

	(1) OLS Village FE	(2) OLS Village FE	(3) OLS Village FE	(4) IV-Eligible Village FE	(5) IV-1st Eligib Village FE	(6) IV-Eligible HH FE	(7) IV-1st Elig HH FE
Sponsored as Child	2.586***			3.171	3.006***	4.596**	2.877***
	(0.349)			(3.031)	(0.621)	(1.803)	(0.503)
Eligible for Spons.		1.280					
		(0.854)					
First Eligible			1.829***				
			(0.374)				
Sex (1 = male)	-0.049	-0.152	-0.130	-0.024	-0.031	0.160	0.144
	(0.273)	(0.285)	(0.280)	(0.352)	(0.346)	(0.271)	(0.248)
Age	0.399***	0.487***	0.440***	0.373**	0.380***	0.037	0.080
	(0.109)	(0.114)	(0.112)	(0.184)	(0.128)	(0.112)	(0.079)
Age-Squared	-0.006***	-0.007***	-0.007***	-0.006**	-0.006***	-0.001	-0.001*
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)
Birth order (1=oldest)	0.082	0.019	0.121	0.098	0.093	-0.039	-0.060
	(0.085)	(0.088)	(0.089)	(0.194)	(0.142)	(0.157)	(0.158)
Number of Siblings	0.071	-0.002	0.025	0.088	0.083		
	(0.053)	(0.055)	(0.054)	(0.103)	(0.062)		
Mother Schooling	0.163***	0.155***	0.154***	0.164***	0.164***		
	(0.037)	(0.038)	(0.037)	(0.034)	(0.029)		
Father Schooling	0.166***	0.160***	0.166***	0.168***	0.167***		
	(0.036)	(0.037)	(0.037)	(0.039)	(0.043)		
Constant	0.202	-0.824	0.115	0.244	0.232	7.673***	7.500***
	(1.775)	(1.941)	(1.823)	(1.642)	(1.822)	(2.436)	(2.039)
Observations	557	557	557	557	557	809	809
R-squared	0.22	0.14	0.17				

* Significant at 10%; ** significant at 5%; *** significant at 1%. Clustered standard errors at the village (household) level in parentheses.

Table 4: Probability of School Completion modeled
(Household-level Fixed Effects)

	(1)				(2)			
	Primary	Ordinary Secondary	Advanced Secondary	University	Primary	Ordinary Secondary	Advanced Secondary	University
Spons. as Child	0.440*	0.111	0.381***	-0.0511	0.207***	0.484***	0.193***	0.0252
	(0.227)	(0.277)	(0.126)	(0.174)	(0.0702)	(0.0963)	(0.0697)	(0.0517)
Sex (1 = male)	0.0437	0.0710*	0.0735**	-0.0115	0.0404	0.0745*	0.0717**	-0.0108
	(0.0357)	(0.0414)	(0.0343)	(0.0187)	(0.0393)	(0.0382)	(0.0345)	(0.0194)
Age	0.00426	-0.0468	-0.0129	0.0109	0.0100	-0.0561	-0.00817	0.00898
	(0.0159)	(0.0345)	(0.0125)	(0.0107)	(0.0146)	(0.0368)	(0.00962)	(0.00834)
Age-Squared	-7.42e-05	0.000295	0.000125	-0.000122	-0.000164	0.000439	5.17e-05	-9.26e-05
	(0.000186)	(0.000268)	(0.000154)	(0.000138)	(0.000160)	(0.000306)	(0.000115)	(0.000104)
Birth order (1=oldest)	0.00826	-0.0791	-0.0104	0.000549	0.00530	-0.0746	-0.0127	0.00148
	(0.0194)	(0.0507)	(0.0133)	(0.0114)	(0.0207)	(0.0469)	(0.0121)	(0.0102)
Constant	0.544*	1.700*	0.378	-0.110	0.523	1.737*	0.359*	-0.102
	(0.328)	(0.944)	(0.255)	(0.186)	(0.332)	(0.913)	(0.204)	(0.167)
Observations	805	806	806	806	805	806	806	806
R^2	0.048	0.043	0.104	0.003	0.052	0.071	0.100	0.002

*** p<0.01, ** p<0.05, * p<0.1 Bootstrapped clustered standard errors in parentheses,
(1) Instrument used: *totaleligibility* (2) Instrument used: *firsteligible*

Table 5: Child Sponsorship Impacts

-- Intention to Treat (ITT) and Average Treatment Effects on the Treated (TET) --

	Mean (σ)	(1) OLS on Sponsorship Village FE	(2) OLS-Eligible Village FE (ITT)	(3) OLS-Eligible HH FE (ITT)	(4) IV-Eligible HH FE (TET)	(5) IV-1st Elig. HH FE (TET)
Education:						
Total Years of Formal Education	9.18	2.586***	1.599**	2.166***	4.596***	2.877***
	(4.01)	(0.349)	(0.721)	(0.713)	(1.633)	(0.598)
Completed Primary School	0.745	0.262***	0.102	0.207**	0.440**	0.207***
	(0.52)	(0.058)	(0.106)	(0.099)	(0.215)	(0.074)
Completed Ordinary Sec. School	0.454	0.341***	0.043	0.052	0.111	0.484***
	(0.57)	(0.059)	(0.115)	(0.126)	(0.396)	(0.104)
Completed Advanced Sec. School	0.213	0.205***	0.203***	0.180**	0.381***	0.193***
	(0.41)	(0.040)	(0.076)	(0.083)	(0.126)	(0.072)
Completed University Degree	0.789	0.030	0.026	-0.024	-0.051	0.025
	(0.27)	(0.030)	(0.054)	(0.058)	(0.158)	(0.065)
Economic Outcomes:						
Formally Employed	0.551	0.087*	0.010	0.063	0.135	0.170***
	(0.49)	(0.048)	(0.096)	(0.099)	(0.216)	(0.064)
White-Collar Employment	0.191	0.142***	0.068	0.214**	0.454*	0.126*
	(0.39)	(0.041)	(0.079)	(0.090)	(0.239)	(0.071)
Monthly Salary in U.S. dollars	112.3	23.629*	14.233	27.762	74.021	25.880
	(91.4)	(13.426)	(21.572)	(24.753)	(69.922)	(37.267)
Sends Remittances to Family	0.268	0.040***	-0.042	0.008	0.015	0.039
	(0.44)	(0.012)	(0.033)	(0.031)	(0.059)	(0.033)
Leadership:						
Community Leader	0.095	0.063**	0.186***	0.131**	0.274	0.042
	(0.29)	(0.030)	(0.060)	(0.065)	(0.215)	(0.045)
Village Leader	0.045	0.023	0.078*	0.094*	0.194**	-0.012
	(0.21)	(0.020)	(0.043)	(0.048)	(0.094)	(0.032)
Church Leader	0.165	0.235***	0.105	0.107	0.221	0.083
	(0.37)	(0.038)	(0.079)	(0.081)	(0.145)	(0.068)
Health:						
Is a Smoker	0.018	-0.012	-0.006	-0.022	-0.048	-0.007
	(0.14)	(0.015)	(0.029)	(0.031)	(0.098)	(0.013)
Drinks Alcohol	0.094	-0.056*	-0.106*	0.024	0.052	-0.044
	(0.31)	(0.029)	(0.060)	(0.068)	(0.246)	(0.044)
Has Mosquito Net(s) in House	(0.443)	0.239***	0.109	0.015	0.031	0.173***
	(0.49)	(0.056)	(0.110)	(0.073)	(0.105)	(0.057)
Has had Measles	0.677	0.070	0.233**	0.127**	0.270**	0.043
	(0.47)	(0.049)	(0.093)	(0.061)	(0.137)	(0.064)
Has had Yellow Fever	0.233	0.009	0.118	-0.024	-0.052	0.055
	(0.42)	(0.043)	(0.085)	(0.066)	(0.147)	(0.048)
Has had Typhoid	0.241	0.093**	0.159*	0.006	0.012	0.137**
	(0.42)	(0.042)	(0.082)	(0.056)	(0.099)	(0.062)
Dwelling:						
Home Concrete or Brick	0.653	0.071	0.133	0.096	0.203	0.061
	(0.48)	(0.049)	(0.093)	(0.071)	(0.176)	(0.058)
Home has Indoor Toilet	0.090	0.011	0.016	0.021	0.039	0.028
	(0.28)	(0.028)	(0.054)	(0.031)	(0.027)	(0.035)
Home has Electricity	0.253	0.087**	-0.052	-0.001	-0.002	0.159***
	(0.435)	(0.039)	(0.085)	(0.061)	(0.168)	(0.062)
Consumer Goods:						

Owns Mobile Phone	0.534	0.187**	0.232**	0.481**	0.456***	0.187**
	(0.49)	(0.093)	(0.098)	(0.196)	(0.075)	(0.093)
Owns Bike	0.205	0.070*	0.087	0.113	0.233	0.032
	(0.41)	(0.038)	(0.080)	(0.079)	(0.183)	(0.053)
Owns Motorcycle or Scooter	0.048	0.059***	0.029	0.092*	0.190	0.057
	(0.21)	(0.023)	(0.044)	(0.053)	(0.182)	(0.051)
Owns Automobile	0.021	0.015	-0.048	-0.070**	-0.145	0.043
	(0.14)	(0.019)	(0.030)	(0.035)	(0.172)	(0.037)

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors in parentheses in (1), (2), and (3). Bootstrapped clustered standard errors in parentheses in (4) and (5). Each cell presents the estimated treatment effect from a single regression, which includes controls for gender, age, age-squared, and birth order where (1) also includes number of siblings, and mother's and father's education, and (2) also includes number of siblings. Number of observations is equal to 557 in column (1) and (depending on missing data) 726 to 809 for estimations in (2), (3), (4), and (5). First stage estimations: Coefficient in (4) on sponsorship from eligibility = 0.471 (asymptotic *t*-value = 5.04). Coefficient in (5) on sponsorship from first child eligible = 0.565 (asymptotic *t*-value = 16.51).

Table 6: Impacts of Additional Years of Formal Education
(OLS and Instrumental Variable Estimations)

	Mean (σ)	(1) OLS Village FE	(2) OLS Household FE	(3) IV-Eligible Household FE	(4) IV-1st Eligible Household FE
Economic Outcomes:					
Formally Employed	0.551	-0.001	-0.010*	0.029	0.059*
	(0.49)	(0.006)	(0.006)	(0.074)	(0.033)
White-Collar Employment	0.191	0.027***	0.022***	0.099	0.044
	(0.39)	(0.005)	(0.005)	(0.099)	(0.034)
Monthly Salary in U.S. dollars	112.3	11.091***	9.646***	13.839	9.419
	(91.4)	(1.497)	(1.751)	(19.437)	(15.21)
Log Monthly Salary in U.S. dollars	4.30	0.107***	0.105***	0.250	0.057
	(1.06)	(0.017)	(0.018)	(0.282)	(0.171)
Sends Remittances to Family	0.268	0.001	0.002	0.003	0.015
	(0.44)	(0.001)	(0.002)	(0.017)	(0.012)
Leadership:					
Community Leader	0.095	0.008**	0.009**	0.060	0.014
	(0.29)	(0.004)	(0.004)	(0.128)	(0.019)
Village Leader	0.045	0.003	0.002	0.044	-0.004
	(0.21)	(0.002)	(0.003)	(0.042)	(0.013)
Church Leader	0.165	0.022***	0.020***	0.057	0.028
	(0.37)	(0.005)	(0.005)	(0.090)	(0.033)
Health:					
Is a Smoker	0.018	-0.004**	-0.001	-0.010	-0.002
	(0.14)	(0.002)	(0.002)	(0.111)	(0.005)
Drinks Alcohol	0.094	-0.013***	-0.011***	0.011	-0.015
	(0.31)	(0.003)	(0.004)	(0.110)	(0.023)
Has Mosquito Net(s) in House	(0.443)	0.030***	0.008*	0.008	0.072**
	(0.49)	(0.007)	(0.005)	(0.073)	(0.030)
Has had Measles	0.677	-0.010*	0.001	0.059**	0.015
	(0.47)	(0.006)	(0.004)	(0.029)	(0.019)
Has had Yellow Fever	0.233	0.008	0.005	-0.011	0.019
	(0.42)	(0.005)	(0.004)	(0.043)	(0.019)
Has had Typhoid	0.241	-0.005	0.007**	0.003	0.047*
	(0.42)	(0.005)	(0.003)	(0.057)	(0.026)
Dwelling:					
Home Concrete or Brick	0.653	0.015***	0.008*	0.044	0.021
	(0.48)	(0.006)	(0.004)	(0.048)	(0.023)
Home has Indoor Toilet	0.090	0.010***	0.004**	0.008	0.010
	(0.28)	(0.003)	(0.002)	(0.008)	(0.015)
Home has Electricity	0.253	0.012**	0.013***	-0.001	0.057***
	(0.435)	(0.005)	(0.003)	(0.043)	(0.016)
Consumer Goods:					
Owns Mobile Phone	0.534	0.035***	0.025***	0.105	0.160***
	(0.49)	(0.005)	(0.006)	(3.415)	(0.048)
Owns Bike	0.205	-0.005	0.005	0.058	0.011
	(0.41)	(0.004)	(0.005)	(0.191)	(0.021)
Owns Motorcycle or Scooter	0.048	0.003	0.002	0.041	0.020
	(0.21)	(0.003)	(0.003)	(0.059)	(0.020)
Owns Automobile	0.021	0.009***	0.007***	-0.031	0.015
	(0.14)	(0.002)	(0.002)	(0.145)	(0.012)

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors in parentheses in (1) and (2). Bootstrapped clustered standard errors in parentheses in (3) and (4). Each cell presents the estimated treatment effect from a single regression, which includes controls for gender, age, age-squared, and birth order where (1) also includes number of siblings, and mother's and father's education, and (2) also includes number of siblings. Number of observations is equal to 557 in column (1) and (depending on missing data) 726 to 809 for estimations in (2), (3), (4), and (5). First stage estimations: Coefficient in (3) on years of education from eligibility = 2.16 (asymptotic *t*-value = 3.04). Coefficient in (4) from first child eligible = 1.62 (asymptotic *t*-value = 5.29).