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Does International Child Sponsorship Work? A Six-Country Study of Impacts on Adult Life Outcomes

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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, where we estimate that 9.14 million children are currently supported through formal international sponsorship organizations. In a study involving original data collection on 10,144 individuals in six countries, we present estimated impacts on adult life outcomes from sponsorship through Compassion International, a leading child sponsorship organization. To generate counterfactuals for identifying program effects, we utilize an age-eligibility rule that was followed from 1980 to 1992 as the program was being introduced into villages in those countries. We find large and statistically significant impacts from child sponsorship on years of completed schooling, primary, secondary, and tertiary school completion, and on the probability and quality of adult employment. We summarize early evidence which suggests that these impacts may be due in part to programming that raises the aspirations and self-expectations of impoverished children.

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1. Introduction

For millions of households in wealthy countries, international child sponsorship represents the most intimate and direct form of involvement with the poor in developing countries. Sponsors typically give $25-40 per month to supplement an impoverished child's education and health expenses or to support programs in which the child participates. We estimate that currently private financial flows to internationally sponsored children exceed US$3 billion annually, yet no published research exists that has gauged these programs' impacts on the life outcomes of sponsored children.

This paper examines whether children sponsored through Compassion International, a leading child sponsorship organization currently serving 1.3 million children in 26 countries, have improved life outcomes when they are adults. Data were collected on the life outcomes of 10,144 individuals over two years from six developing countries that are representative of the Compassion program's work worldwide: Bolivia, Guatemala, India, Kenya, the Philippines, and Uganda.

Identification of the program's impacts rests on three eligibility rules that Compassion used from 1980 to 1992, when those in our study were sponsored. These rules limited the number of children per household that could be sponsored, required sponsored children to be within walking distance of a project, and stipulated that only children below a given age were eligible for the program. Using several estimation strategies that harness these eligibility rules to construct counterfactuals for estimating causal effects, we find that the Compassion sponsorship program significantly increased total years of schooling and completion rates across all levels of schooling. Impacts are especially large for secondary school completion, which increased by 12-18 percentage points over an average baseline of 44.5%. Education impacts are particularly strong in the two African countries. We also find positive and significant impacts on the probability of adult employment and movement into white-collar jobs.

In some respects, Compassion projects are similar to many government and international donor programs that promote education. Sponsors pay for children's school tuition and uniforms, several nutritious meals per week, healthcare, and tutoring. What distinguishes Compassion projects from most government and international donor programs, and from some other child sponsorship programs, is that children spend at least eight hours per week in an intensive after-school program that emphasizes their spiritual, physical, and socio-emotional development. In the sample, the average duration of sponsorship was 9.3 years, so that by the end of their childhood, sponsored children have participated in about 4,000 hours of Compassion programming, including extra activities such as retreats and camps. A primary objective of this extended contact is to raise the child's self-esteem, aspirations, and self-expectations.

Recent work in economics suggests that internal constraints that reflect low aspirations and reference points may lead to poverty traps (Ray, 2006; Dalton, Ghosal, and Mani, 2010; Bernard, Dercon, and Taffesse, 2011). After reporting program impacts on adult life outcomes, we present a short summary of evidence from three follow-up studies that collected data and carried out other research among 1,380 *currently* sponsored Compassion children in Bolivia, Kenya, and Indonesia. These studies find that sponsored children exhibit significantly higher levels of self-esteem, aspirations and self-expectations, and lower levels of hopelessness. While more work is needed to establish a causal link between aspirations during youth and adult life outcomes, a clearer understanding of this relationship may have important implications for the way in which practitioners approach development work.

Two major empirical issues must be addressed to obtain unbiased estimates of the impact of this type of program. First, selection of children into a program may not be random; more needy children may have been chosen (as is directed in the Compassion operations manual), but it is also possible that parents chose the children whom they thought were most likely to succeed. Second, program impacts may spill over onto siblings or non-treated peers in a treated village.

This study uses three program eligibility rules to address these two estimation issues. As Compassion established new projects from 1980 to 1992 in each of the six countries, an age-eligibility rule stipulated that only children 12 years and younger (11 years and under in Uganda and Guatemala) at the time the project started were eligible for sponsorship. This arbitrary rule allows us to compare the adult life outcomes of formerly sponsored children relative to the life outcomes of their ineligible older siblings, who were age 13 or older when the program was started in their village. Moreover, to maximize the number of households benefiting from sponsorships, a second rule set an upper limit on the number of sponsored children per household; this number varied from one to three in the six countries. Finally, a third rule stipulated that, to be sponsored, a child had to reside within walking distance of the program center, which was usually interpreted as at most a thirty-minute walk. In practice, this meant that only children residing within the village where the program was located were chosen for sponsorship. Children from neighboring villages were excluded.

We use the first two rules to address the issue of endogenous child selection. Specifically, we use a vector of dummy variables that indicate the age of an individual when the Compassion program was introduced into that person's village, interacted with his or her sibling order relative to program rollout (oldest sibling 12 or younger when the program began, second-oldest sibling among those 12 or younger when the program began, etc.), as instruments to estimate the probability that an individual was a sponsored child. These instruments are strongly correlated with take-up since children meeting the age requirement were far more likely to be sponsored, and in practice the oldest

age-eligible siblings were most likely to be sponsored. These instruments satisfy the exclusion restriction for instrumental variables because as Compassion programs were introduced in different years in different villages, the age of an individual at the time the program began in his or her village should not be related to his or her life outcomes except via the impact of being sponsored.

We use the third rule to address the issue of program spillovers; we collected data not only on sets of siblings from treated families, but also on sets of siblings from a random sample of non-treated households in Compassion villages and from a random sample of households in neighboring villages where children could not be sponsored due to the walking distance rule. Using the identifying assumptions that program spillovers affect neither older age-ineligible children in treated villages, nor *any* children in non-Compassion villages, we implement both OLS and IV difference-in-differences regressions that estimate direct effects on program participants and can also be used to calculate both intra-household and intra-village spillovers from the Compassion program.

These estimates yield large and statistically significant effects of child sponsorship on education, employment, and leadership outcomes. OLS and IV estimations, with and without the use of household fixed effects, find that child sponsorship resulted in 1.03 to 1.46 additional years of completed schooling for sponsored children over a baseline of 10.19 years for unsponsored children.[1] Impacts on primary schooling range from 4.0 to 7.7 percentage points over an untreated baseline of 88.7%. Impacts on secondary school completion are greater and highly significant, ranging from 11.6 to 16.5 percentage points over a baseline of 44.5%; accounting for marginally significant spillover effects pushes the figure somewhat higher, to 13.7-18.5 percentage points. Tertiary education point estimates of impact are smaller, from 2.1 to 3.6 percentage points, but these are realized over a small baseline of 4.3%. Child sponsorship also appears to be a great "equalizer." Its impacts on the educational outcomes are larger in those countries with lower baseline education outcomes, the two African countries, while impacts in Latin America and Asia are smaller, although still statistically significant. Similarly, in countries where baseline schooling is higher for boys, child sponsorship tends to have a bigger impact on girls; where it is higher for girls, it has a bigger impact on boys.

OLS estimates also find positive impacts on the probability of salaried employment (5.1-6.3 percentage points, over a 35.7% baseline) and white-collar work (6.5-6.7 percentage points, baseline of 18.5%). There is also evidence, albeit mixed, of increases in community and church leadership.[2]

[1] "Years of schooling" denotes highest grade attained. Grade repetition is common in many countries, so years of schooling can exceed grade attained; we have no data on repetition so we cannot account for repeated grades.

[2] Results on adult life outcomes such as age of marriage, fertility, remittances, dwelling quality, and durable good ownership are found in our larger working paper (Wydick, Glewwe, and Rutledge, 2013).

Our results do not necessarily apply to all child sponsorship programs. While some of the other major child sponsorship organizations, such as Children International, use sponsor funding for the nurturing and development of individual children, other large programs, such as those operated by World Vision, Plan USA, and Save the Children, use funding given in the name of a sponsored child more broadly to create village-level public goods. The less-targeted nature of these programs renders potential impacts more diffuse, and thus more difficult to assess.

The remainder of this paper is organized as follows. Section 2 reviews the most relevant previous studies. Section 3 explains the fieldwork and data collection. Section 4 describes the estimation strategy, and Section 5 presents estimates of the program impacts. Section 6 concludes and briefly discusses potential causal mechanisms through the impact of the program on children's aspirations, summarizing results from three follow-up studies of currently sponsored children.

2. Existing Research and Literature

Given the number of individuals involved in child sponsorship relationships and the billions of dollars committed to them, it is surprising that almost no research exists that evaluates the impacts of these programs. One exception is Kremer, Moulin, and Namunyu (2003), who use a randomized experiment to analyze the impacts of a Dutch child-sponsorship program that funded new classroom construction and provided students a $6 uniform and $3.44 worth of textbooks. They find that even these relatively low-cost interventions induced student beneficiaries to attend school a half year longer, and to advance a third of a grade farther in formal education.

More generally, a growing literature attempts to find cost-effective ways to induce parents to invest more in their children's education. Researchers have studied many programs, including cash transfers, free meals, provision of school uniforms, deworming medicine, and free medical treatment.

Perhaps the best known and most frequently evaluated intervention is the *PROGRESA* (later renamed *Oportunidades*) conditional cash transfer (CCT) program. Implemented in 1997 in poor regions of Mexico, *Oportunidades* provides cash incentives for mothers to increase their children's school attendance and obtain health care for younger children. It was initially implemented as a randomized trial to facilitate its evaluation by researchers. Impact evaluations have shown that *Oportunidades* led to higher school enrollment, lower grade repetition, lower dropout rates, and higher school reentry rates among dropouts (Behrman, Sengupta and Todd, 2005; World Bank, 2009). Behrman, Parker and Todd (2007) estimate that receiving *Oportunidades* cash transfers for 5.5 years increased grades completed by 0.8 to 1.0 years. Schultz (2004) estimates that *Oportunidades* increased formal schooling by 0.66 years (0.72 for girls, and 0.64 for boys). He also finds that *Oportunidades* raised enrollment by 3.4 percentage points, averaging over all children in grades one through eight,

with much larger impacts in later grades, not only for girls (14.8 percentage points) but also for boys (6.5 percentage points). Bobonis and Finan (2009) find that enrollment rates in *Oportunidades* communities increased by 5 percentage points, even among those ineligible for the program.

CCT programs have also had positive impacts on education in other countries. For example, Barrera-Osorio et. al. (2008) implemented a randomized experiment to evaluate the *Conditional Subsidies for School Attendance* program in Bogota, Colombia, finding that school attendance increased by 2.8 percentage points on average.

Other programs have funded various school "inputs," such as free or subsidized school meals, uniforms, textbooks, school construction, and teachers. Several focus on nutrition or health. Drèze and Kingdon (2001) find that providing a mid-day meal in India raised girls' school attendance by 15 percentage points. Kremer and Vermeersch (2004) estimate that school attendance rose by 8.5 percentage points in Kenyan preschools that provided free meals, increasing attendance of current students and attracting new students who had never attended preschool. Handa and Peterman (2007) find that South African children's educational attainment is strongly affected by their nutritional status. Glewwe and Miguel (2008) review the impact of health and nutrition on education outcomes.

Many randomized studies of education interventions have been conducted in Kenya. Evans, Kremer, and Ngatia (2008) evaluate a program that selected Kenyan children by lottery to receive free school uniforms. They find that receiving a uniform reduced absenteeism by 39 percent, and by 64 percent for poorer students. In the same area of Kenya, Miguel and Kremer (2004) implemented a randomized de-worming intervention. This intervention not only decreased overall disease transmission but also reduced school absenteeism by 7 percentage points in the treatment schools. They also find positive spillover effects onto children who attended nearby schools that did not participate in the de-worming program. In a follow-up study of former participants ten years after the deworming experiment, Ozier (2011) finds increases in cognitive performance equal to 0.5 to 0.8 years of schooling. A randomized trial that provided sixth grade girls merit scholarships of about $20 for school fees and school supplies increased student attendance by 5 percentage points; surprisingly, it increased both girls' *and* boys' test scores (Kremer, Miguel, and Thornton, 2009). Another study in Kenya provided incentives to teachers to improve their teaching. Glewwe, Ilias, and Kremer (2010) carried out an experiment that provided valuable prizes to teachers based on their students' test scores. Despite the incentives, teacher attendance did not improve; instead teachers held additional prep sessions prior to the exams on which the incentives were based, which led to only short-term increases in test scores.

Methodologically, the empirical strategy of this paper is similar to that of Duflo (2001) in that it uses the ages of former students and geographic placement of a schooling treatment as instruments

to identify program impacts. Using a method similar to that of Pitt, Rosenzweig, and Gibbons (1993), Duflo examines the impact of Indonesia's rapid expansion of school construction from 1973 to 1979. She uses an individual's exposure to the program, as measured by the number of schools built in his or her region of birth, along with age at the time of program implementation, to identify impacts on education and wages. She finds that each new school constructed per 1,000 children led to a 0.12 to 0.19 increase in years of schooling. This implies an average increase of 0.25 to 0.40 years per child beneficiary (about two schools were built per 1000 children), which then resulted in a 3.0 to 5.4 percent increase in wages, suggesting an economic return to education of 6.8 to 10.6 percent. She also finds that those who benefited most were among the poorest.

3. Counting the Sponsored, Program Background, and Fieldwork

3.1 Counting the Sponsored

There have been no reliable figures on the number of internationally sponsored children worldwide; a preliminary task was to estimate this figure. Through internet searches in multiple languages and contact with industry personnel across countries, we tallied 207 organizations that appear to represent nearly all children sponsored through such organizations worldwide. Based on the sponsorship figures claimed by these organizations, we estimate that there are currently 9.14 million internationally sponsored children in the world.[3] Over 90% of this total are sponsored through the ten largest sponsorship organizations. Table 1 contains basic information about these organizations, including years of operation, number of countries served, monthly sponsorship fees, and number of children sponsored. All are based in the United States and Europe, and two of the largest three are faith-based, as are four of the largest ten.

The total flow of child sponsorship funds to developing countries is non-trivial; indeed it is similar to amounts given by the U.S. Government for international assistance. Assuming an average monthly contribution of $30, funding for child sponsorship is about US$3.29 billion per year, excluding special gifts and travel to visit sponsored children. This is comparable to USAID budgets of $8.72 billion in 2012 for "Global Health and Child Survival," $2.92 billion for "Development Assistance" and the $1.12 billion earmarked for the Millennium Challenge Corporation (OMB, 2012).

International child sponsorship programs arose due to their usefulness as a marketing tool for mobilizing resources in rich countries to reduce poverty in poor countries. As the marketers of these

[3] Because the internet is so vital today for fundraising in the child sponsorship industry, for example posting pictures of children and providing other contact between potential sponsors and potentially sponsored children, we assumed that any such organization of significant scope must have an internet presence. This assumption is the basis for our calculation of 9.14 million sponsored children. If there are child sponsorship programs that do not use the internet, our 9.14 million figure would underestimate the actual number of children sponsored worldwide.

programs have recognized for decades, contact with an individual child creates a commitment device to help donors contribute a fraction of their monthly income to alleviating child poverty in developing countries via a relationship with a *particular child* living in poverty. In this way, international child sponsorship programs mobilize resources by drawing on the psychological and moral instincts people possess to care for their own children. Even in difficult economic times, the commitment of donors to the well-being of "their child" is likely to exceed their commitment to a large, well-intentioned — yet relatively faceless — non-profit organization.[4] Thus even apart from issues of impact and cost-effectiveness, child sponsorship programs may be among the most effective methods for mobilizing resources to benefit children in developing countries.

3.2 The Compassion Child Sponsorship Program

The world's third largest child sponsorship program is Compassion International, a large, faith-based, nonprofit organization whose stated goal is to "release children from spiritual, economic, social, and physical poverty." Compassion staffs its projects locally, and foreign employees are rare. The projects rely on volunteers from local churches and other organizations to carry out its programming. The benefits sponsored children receive vary somewhat by country, and even within countries, and Compassion's approach has evolved over time. Table 2A summarizes, for each country in this study, the benefits the sponsored children received while enrolled in the program. In Uganda and Kenya, and in three of the projects in Bolivia, Compassion operated student centers where sponsored children gathered on Saturday or after school on weekdays. Students participated in structured programs at these centers, receiving academic tutoring, spiritual instruction, healthcare, nutritious meals and school supplies. They also participated in a wide array of games and activities. In most of the projects with student centers, Compassion children also received school fee subsidies and school uniforms. Compassion typically sponsors children through secondary school, although a small number continue to the university level through its leadership development program.

In Guatemala and the Philippines, Compassion programs operated in (Protestant) Christian schools, where students would receive similar benefits, although tutoring was not generally an explicit component of sponsorship. In India and in one project in Bolivia, Compassion collaborated with government programs that gave parents direct cash payments conditional upon the sponsored child's continuation in school. However, these programs differed from standard CCT programs in that children received most of the benefits provided by the other Compassion programs, as well as individual nurturing and care via Compassion's partnership with local Protestant churches.

[4] 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 maintained their monthly financial commitment to sponsored children showed no sign of decline during that period (Kennedy, 2009).

All projects provided basic healthcare benefits. They included regular physical examinations administered by local nurses and doctors at Compassion schools and student centers. Also included was a form of catastrophic health insurance paid through a separate fund operated by Compassion's headquarters in Colorado. If a Compassion child had a serious illness or needed surgery, this fund covered the full cost of the procedure and hospitalization. In the rare cases when children needed such care, this benefit was often reported by formerly sponsored children to be the greatest source of support offered by the program. Aside from this catastrophic insurance, however, all funds directed to children flow from their sponsors' regular monthly contributions.

All children sponsored through Compassion write letters several times per year to their sponsor, and most receive correspondence from their sponsor (71.8% in our study). In addition, about once per year sponsors receive a picture of the child and updates from local Compassion staff on the child's progress in school. Most children (83.7%) also reported receiving birthday gifts from their sponsor. Sponsors can also travel on organized trips to visit their sponsored children and their families; while not uncommon, this was not the norm.

The survey included an open-ended question asking formerly sponsored children which component of the Compassion program had been most beneficial to them. The most common answer was educational support (38.5%). (Within this category, payment of school fees and tutoring were cited most often, and almost equally.) The second-most common response was spiritual and character development (29.4%), followed by economic aid (9.5%--a figure that was no higher in the two countries where parents received direct cash payments), healthcare benefits (2.8%), and gifts received from sponsors (0.8%).

In Compassion projects, selection of children for sponsorship is done locally. Compassion instructs its staff to work with local community members to select children using six criteria:

1) Sponsored children are to be from low-income families within walking distance of a project. The official selection criteria state: "When only a percentage of the children are sponsored from an institution, the school or parent committee should choose children among the neediest families for sponsorship."

2) Orphans, children living with a widowed parent or other family member, and refugees are given special priority.

3) The child cannot have been sponsored by another agency.

4) Children from both Christian and non-Christian families may participate equally, but all families must allow their children to participate in the program's Christian religious instruction.

5) Compassion sponsors a maximum of three children per family. Some countries set a lower limit (one or two per family).

6) Children older than 12 years cannot be sponsored. Children in kindergarten and in first, second, or third grade receive top priority; older children (still age 12 or under) receive lower priority.

The last guideline was intended to lengthen the number of years that a child can be sponsored and was fully operational after programs had operated for several years. However, when a program was first introduced into a village, parents tended to select older eligible children rather than younger eligible children. The mean length of sponsorship in our data is 9.3 years.

3.3 Survey and Fieldwork

The survey work in the six countries of our main study took place from June 2008 to August 2010. Table 2B shows, for each country, a list of villages, rollout years for each village, sample sizes, and survey dates. Some projects started on a large-scale, enrolling up to 100 children in the first year. Others started with fewer children, enrolling only 20 to 30 in the first year. For the larger projects, individuals were randomly selected to be surveyed from the first two or three years of enrollment lists. For smaller projects, data were collected from all children who were enrolled in the first two or three years. To avoid attrition bias, the sample includes both children who were sponsored for many years as well as children who dropped out relatively early.

In some cases the enrollment lists from which we sampled formerly sponsored children were in an electronic database at the country office. In others the only lists were hard copies of computer printouts kept on file at the project sites, which were found after extensive searches of file cabinets or basement boxes. Two of the 19 projects no longer sponsored children through Compassion.

Local assistants were hired to locate the households of formerly sponsored individuals who were on the early enrollment lists. They were usually recommended by project staff, and were known to be responsible, well-respected community members. They also had been raised in the village and so were knowledgeable about the community, but we eschewed hiring enumerators with formal connections with Compassion to avoid bias in responses. We located close to 99% of the targeted households in Uganda, Guatemala, and Kenya, and about 90% in India and Bolivia. In the Philippines slightly less than 80% were located due to a high rate of household mobility following sponsorship. Overall, we located 93.5% of the families of the formerly sponsored children who were on the enrollment lists for the first two to three years the program operated in each village. Families who were not located either had key family members who had passed away, had migrated to unknown areas of the country, or to known areas but without specific details regarding their location.

In addition to these Compassion households, we surveyed 50 to 75 non-Compassion households in each Compassion village, conditional on the presence of an individual in the household being born in the ten years before the Compassion project began operation. We also randomly sampled a similar number of households in neighboring villages without a Compassion program that

were similar to the nearby Compassion villages. Households that did not participate in the program, from either Compassion or non-Compassion villages, were surveyed in order to check for intra-household and intra-village spillovers, as explained below in Section 4. The overall, six-country data set includes information on 1,860 formerly sponsored children, 3,704 of their unsponsored siblings, 2,136 individuals from non-participating families in villages where the Compassion program operated, and 2,444 individuals from similar, nearby villages without the Compassion program.

The samples of non-Compassion households, from both the Compassion and the non-Compassion villages, were randomly selected as follows. A starting point in the village was randomly chosen, and then every third household on the street was selected for possible inclusion in the survey. The household was briefly questioned to see whether any of its members met the sponsorship age criteria. When the end of the street or block was reached, the enumerator turned left and continued with every second or third household, then turned right and proceeded in this way, choosing new random points in the village on different days.

Table 3A shows summary statistics for the outcome variables, and key control variables. The survey questionnaire we used had questions to collect basic information on adult life outcomes of both sponsored children and their siblings, as well as children in non-participating households. These included questions on an individual's level of schooling, type of employment, and whether he or she held various leadership positions. The questionnaire was designed to ask each question sequentially across all siblings by age to avoid focusing on the sibling(s) who had been sponsored by Compassion. The questions were designed to be easily answered in order to obtain data on the basic life outcomes of adults that would be common knowledge among family members. We eschewed questions that asked for detailed data or for exact values of continuous variables, since family members may not be able to provide such information. Many of the questions were obtained from the education modules in the World Bank's Living Standards Measurement Study (LSMS) surveys.

We interviewed all available family members jointly regarding the life outcomes of each formerly sponsored child and his or her siblings. Although, in most cases, several family members answered questions on sibling life outcomes, information was collected on the principal respondent. The most common principal respondents were the parents of the sponsored child (36.6%) and the formerly sponsored child (35.8%), followed by siblings (22.4%) and other relatives (5.2%).

4. Empirical Methodology

We employ a variety of estimation techniques to identify the impact of the Compassion sponsorship program. Important to each approach is the program's 12-years-and-under eligibility rule. Figure 1 shows the probability that a child in a treated household was sponsored as a function

of his or her Age at the Compassion program Introduction (ACI) into his or her village. Averaging over the bars in the figure, a child in a treated household who was between 0 and 12 years old when the program came to his or her village had a 0.458 probability of sponsorship; for a child who was 12-18, the probability was only 0.022. (A few 13-year-old children were sponsored because their photos had been taken and posted for sponsorship when of eligible age, but a sponsor was not found until they were 13.)

Figures 2a and 2b illustrate differences between treated and non-treated households in years of completed schooling and the probability of completing secondary school, respectively, as a function of two-year ACI categories (for visual smoothness). Both figures compare treated households to non-treated households in Compassion villages. In Figure 2a, for treated households the difference between the average outcome for those with ACI ≤ 12 and the average for those with ACI ≥ 13 is 1.47 years of schooling. For non-treated households the difference is 0.89, so the difference-in-differences equals 0.58 years. Dividing this figure by the difference in the probability of sponsorship across these two ACI categories (0.436) suggests a 1.33 year impact of being sponsored, not accounting for controls. The analogous difference in differences in Figure 2b for secondary school completion is 9.0 percentage points; dividing this by 0.436 suggests that sponsorship increases secondary completion by 20.6 percentage points, which could include spillovers onto other eligible children in Compassion households, but likewise does not adjust for control variables.

To estimate the impact of the program in a manner that more carefully controls for individual and household characteristics, we employ four regression specifications: ordinary least squares (OLS), a generalized method of moments instrumental variables estimator (IV-GMM), and both of these techniques adding household fixed effects (OLS-FE and IV-GMM-FE). These estimation methods must address three concerns to identify the impact of the Compassion program: 1) endogeneity in the selection of households into the program; 2) endogeneity in the selection of children within a particular household into the program; and 3) spillover effects from the program onto non-treated individuals in both Compassion and non-Compassion households in Compassion villages.

To address the first concern, our estimation strategy: (*a*) allows for an additive, unobserved difference between selected and non-selected households in Compassion villages, and an additive, unobserved difference between households in villages with and without the Compassion program; (*b*) controls for individual and parental characteristics; and (*c*) allows for unobserved household fixed effects (for two of the four regression specifications).

The second concern is the possibility of endogenous child selection within families. In practice, Compassion staff often select families for sponsorship, after which the families strongly influence which of their children are sponsored. Endogeneity in the choice of children within a

family could bias estimates in either direction; selected children may have been chosen because they seemed of higher ability and so could realize larger gains from the program. Conversely, parents who have preferences for equal outcomes across all their children would select children whom they deem to be more in need of assistance than their siblings. It appeared to us in the field that the latter was more common, which is consistent with Compassion's guidelines for field personnel.

We address possible endogeneity in child selection by constructing instrumental variables based on an individual's age and sibling order relative to the year of program rollout in his or her village (the former are ACI categories; the latter are oldest age eligible child, second oldest age eligible child, etc.). These variables should not be related to adult life outcomes except through program participation, and they are highly correlated with the probability of sponsorship; thus they are valid instruments to address endogenous child selection. We observed that needier children tended to be selected for sponsorship from among age-eligible siblings, so we expect any bias in OLS estimates to be downward. Thus *a priori* we expect IV estimates to be larger than OLS estimates.

To address the third concern, spillover effects, each estimation method incorporates a difference-in-differences estimation strategy that allows for measurement of potential spillover effects onto non-sponsored individuals in both Compassion households and non-Compassion households in Compassion villages. Estimation of potential spillover effects rests on two identifying assumptions. The first is that spillovers do not flow from Compassion villages to non-Compassion villages. The second is that spillovers do not trickle from sponsored younger children up to older age-ineligible children in *any* households in Compassion villages.

While there is little reason to expect violations of the first assumption due to the distance between neighboring communities in our study, one objection to the second assumption is the possibility that sponsorship of younger, age-eligible children affects their older, age-ineligible siblings. Yet while there are good reasons to expect positive externalities to trickle down from sponsored children to younger unsponsored siblings, there is also good reason to expect that spillovers onto older, age-ineligible siblings are much smaller, if not zero. Older siblings tend to be less influenced by their younger siblings' choices than vice versa. More importantly, education opportunities are usually accessible only within a relevant age range, beyond which older siblings have often passed, preventing them from emulating their younger siblings even if they wanted to do so.

To measure spillover effects onto non-sponsored age-eligible siblings in treated (Compassion) households, we compare differences in life outcomes between age-eligible children and their older age-ineligible siblings in Compassion households with the same differences between these two groups in nearby, non-treated villages. To estimate spillover effects from Compassion onto non-Compassion households in program villages, we compare differences in life outcomes between age-eligible siblings

and their older age-ineligible siblings in non-treated households in Compassion villages with the same differences between these two groups in nearby non-Compassion villages. This is done by including dummy variables in the regression analysis that represent each of these groups.

More formally, we assign all individuals who were 16 or younger when the Compassion program began in their villages (or a neighboring village) into seven mutually exclusive categories:

1.) Sponsored children, denoted by $T = 1$, who were 12 or younger when the program started in their villages;
2.) Siblings of program participants who were 12 or younger when the program was introduced into their villages (denoted by $D_1^{\leq 12} = 1$); they were eligible, but not selected, for the program;
3.) Siblings of program participants who were 13-16 when the program was introduced into their villages and thus were ineligible for the program ($D_1^{13-16} = 1$);
4.) Individuals in non-Compassion households in program villages who were 12 or younger when the program was introduced ($D_2^{\leq 12} = 1$);
5.) Individuals in non-Compassion households in program villages who were 13-16 when the program was introduced ($D_2^{13-16} = 1$);
6.) Individuals in non-Compassion villages who were 12 or younger when the program was introduced in a neighboring village ($D_3^{\leq 12} = 1$); and finally
7.) Individuals in non-Compassion villages who were 13-16 when the program was introduced in a neighboring village ($D_3^{13-16} = 1$).[5]

These categories, and the associated notation, lead to the following regression equation:

$$y_i = \alpha_1 D_{1i}^{\leq 12} + \alpha_2 D_{1i}^{13-16} + \tau\left(D_{1i}^{\leq 12} * T_i\right) + \beta_1 D_{2i}^{\leq 12} + \beta_2 D_{2i}^{13-16} + \gamma_1 D_{3i}^{\leq 12} + \gamma_2 D_{3i}^{13-16} + \delta C_h + \theta C_v + \boldsymbol{X_i}\boldsymbol{\phi} + \epsilon_i \quad (1)$$

where y_i, is the adult outcome of interest for person i, C_h is a dummy variable indicating a household with a sponsored child, C_v is a dummy variable indicating residence in a village with the Compassion program, and $\boldsymbol{X_i}$ is a vector of controls that include gender, age, age-squared, birth order, number of siblings in a family, and mother's and father's education.[6]

[5] Individuals 17 or older in program households are accounted for by the sum of the program household dummy variable and the program village dummy variable in equation (1), and individuals 17 or older in program villages are accounted for by the program village dummy variable. Individuals 17 or older in non-program villages are the omitted category. Note also that $D_1^{\leq 12} = 1$ for both sponsored children and their age-eligible siblings who were not chosen to be sponsored.

[6] A more flexible specification would allow τ to vary for each ACI category (ACI = 12, ACI = 11, etc.) and similarly for α_1, α_2, β_1, β_2, γ_1 and γ_2. We tested the restrictions in equation (1) that τ is the same for all sponsored children (*i.e.* does not vary by ACI) and the analogous restrictions for α_1, α_2, β_1, β_2, γ_1 and γ_2, and the same restrictions in equation (3) below, and we cannot reject the hypothesis that these restrictions hold. Unrestricted estimates, as well as the tests of these restrictions, are available from the authors upon request.

This framework allows us to estimate the causal impacts of the Compassion program under different assumptions about spillover effects. If we assume that: *i*) differences between villages with and without the Compassion program can be fully represented by θC_v and the observed covariates $\boldsymbol{X}$; *ii*) differences between participating and non-participating households in villages where Compassion has a program can be represented by δC_h and the observed covariates $\boldsymbol{X}$; *iii*) there are no intra-family, intra-village, and inter-village spillover effects; and *iv*) intra-family child selection is random, then OLS estimates of equation (1) would consistently estimate τ, the impact of the sponsorship program on y_i. Moreover, we can test whether these assumptions are reasonable. For example, continuing to assume no spillovers onto individuals with ACI ≥ 13, assumptions *i*) and *ii*) imply that $\alpha_2 = \beta_2 = \gamma_2$ (C_h, C_v and the observed covariates $\boldsymbol{X}$ fully account for differences between individuals with ACI of 13-16 in Compassion households, non-Compassion households in Compassion villages and households in non-Compassion villages, so there is no need for these three parameters to differ), and adding assumption *iii*) implies that $\alpha_1 = \beta_1 = \gamma_1$ (without intra-household or intra-village spillovers, there is also no reason for these three parameters to differ).

Alternatively, if assumptions *i*) and *ii*) but assumption *iii*) does not, intra-household spillovers onto non-treated eligible siblings can be estimated as explained above by the difference-in-differences $[\alpha_1 - \alpha_2] - [\gamma_1 - \gamma_2]$, and intra-village spillovers among children 12 and younger can be estimated by $[\beta_1 - \beta_2] - [\gamma_1 - \gamma_2]$. If spillovers exist, then the full treatment effect is no longer estimated by τ alone. If there are intra-household spillovers, the full program effect on the treated can be estimated by $\tau + [\alpha_1 - \alpha_2] - [\gamma_1 - \gamma_2]$. That is, τ estimates the impact of the program on a treated child relative to his or her siblings of similar age, but since those siblings experienced spillover effects, one needs to add this spillover effect to τ to obtain the full program impact on the treated child.

Our instrumental variable estimations use a vector of instruments comprised of interactions between dummy variables for a child's age at program introduction (ACI) and dummy variables for sibling order relative to program rollout (*SORR*).[7] These dummy variables have strong predictive power for the child(ren) chosen by parents for the program due to parents' tendency to choose the oldest age-eligible siblings for sponsorship.[8] Moreover, they plausibly satisfy the exclusion restriction for equation (1), since there is no reason why, after controlling for characteristic variables ($\boldsymbol{X}$), a child's age at the time of program rollout interacted with sibling order relative to program rollout should affect adult life outcomes except via its effect on the probability of sponsorship.

[7] SORR consists of three dummy variables: oldest sibling among age-eligible siblings at time of program roll-out, second oldest of such siblings at time of program roll-out, and third or higher oldest of such siblings at time of program roll-out.

[8] Among children in Compassion households, the probability of the two oldest age-eligible children being sponsored was 51.5 percent (average over all six countries), compared to only 20.4 percent for all other age-eligible children.

In addition to aggregating age categories (12 or less and 13-16), we also aggregated our instrumental variables.[9] This is done to avoid potential problems with including large numbers of instruments that individually may be weak, and to provide more reliable asymptotic results given that the sample is divided into 32 clusters (villages). We maintained the distinction between children who were the oldest eligible sibling, the second oldest eligible sibling, and all other eligible siblings, but we grouped individuals by ACI into three categories: 5 or younger, 6-8, and 9-12, yielding nine instruments of the form $\boldsymbol{Z_{1i}^{ACI}} = (\boldsymbol{D_{1i}^{ACI}} \otimes \boldsymbol{SORR})$. Other variations in the age aggregation of the instruments yield very similar results.

Thus for IV estimates the first-stage estimation is

$$T_i = \tilde{\alpha}_1 D_{1i}^{\leq 12} + \tilde{\alpha}_2 D_{1i}^{13-16} + \tilde{\beta}_1 D_{2i}^{\leq 12} + \tilde{\beta}_2 D_{2i}^{13-16} + \tilde{\gamma}_1 D_{3i}^{\leq 12} + \tilde{\gamma}_2 D_{3i}^{13-16} + \tilde{\delta} C_h + \tilde{\theta} C_v + \boldsymbol{X_i}\tilde{\boldsymbol{\varphi}} + \boldsymbol{Z_{1i}^{ACI}}\tilde{\boldsymbol{\pi}} + e_i \quad (2)$$

and the second stage is

$$y_i = \alpha_1 D_{1i}^{\leq 12} + \alpha_2 D_{1i}^{13-16} + \tau\left(D_{1i}^{\leq 12}\hat{T}_i\right) + \beta_1 D_{2i}^{\leq 12} + \beta_2 D_{2i}^{13-16} + \gamma_1 D_{3i}^{\leq 12} + \gamma_2 D_{3i}^{13-16} + \delta C_h + \theta C_v \boldsymbol{\varepsilon} + \boldsymbol{X_i}\boldsymbol{\varphi} + \epsilon_i. \quad (2')$$

where $\hat{T}_{ij}$ is the predicted probability of being sponsored. Equations (2) and (2′) are estimated using generalized method of moments (GMM), which is more efficient than standard IV estimates and allows one to carry out *J*-tests of over-identifying restrictions to check the validity of the instruments.

We also present OLS and (GMM) IV household-fixed-effect estimates, which control more directly for inter-household unobservables that could affect child selection. The main disadvantage is that the large number of fixed effects may reduce the precision of the estimates. The OLS household fixed-effects (OLS-FE) estimate for child *i* in household *j* is

$$y_{ij} = \alpha_1 D_{1ij}^{\leq 12} + \alpha_2 D_{1ij}^{13-16} + \tau\left(D_{1ij}^{\leq 12} * T_i\right) + \beta_1 D_{2ij}^{\leq 12} + \beta_2 D_{2ij}^{13-16} + \gamma_1 D_{3ij}^{\leq 12} + \gamma_2 D_{3ij}^{13-16} + \boldsymbol{X_{ij}}\boldsymbol{\varphi} + \theta_j + \epsilon_{ij} \quad (3)$$

where θ_j is a household fixed effect and other variables are as defined previously. For the IV fixed-effect estimations (IV-GMM-FE), the first stage equations are given by

$$T_{ij} = \tilde{\alpha}_1 D_{1ij}^{\leq 12} + \tilde{\alpha}_2 D_{1ij}^{13-16} + \tilde{\beta}_1 D_{2ij}^{\leq 12} + \tilde{\beta}_2 D_{2ij}^{13-16} + \tilde{\gamma}_1 D_{3ij}^{\leq 12} + \tilde{\gamma}_2 D_{3ij}^{13-16} + \boldsymbol{X_{ij}}\tilde{\boldsymbol{\varphi}} + \boldsymbol{Z_{ij}^{ACI}}\tilde{\boldsymbol{\pi}} + \tilde{\theta}_j + \tilde{\epsilon}_{ij} \ , \quad (4)$$

where $\tilde{\theta}_j$ is also a household fixed effect, and $\boldsymbol{Z_{ij}^{ACI}}$ is the same vector of instruments used in equation (3). The second-stage equation of the household-fixed-effects estimation is

$$y_{ij} = \alpha_1 D_{1ij}^{\leq 12} + \alpha_2 D_{1ij}^{13-16} + \tau\left(D_{1ij}^{\leq 12}\hat{T}_i\right) + \beta_1 D_{2ij}^{\leq 12} + \beta_2 D_{2ij}^{13-16} + \gamma_1 D_{3ij}^{\leq 12} + \gamma_2 D_{3ij}^{13-16} + \theta_j + \boldsymbol{X_{ij}}\boldsymbol{\varphi} + \epsilon_{ij}. \quad (4')$$

[9] Results are similar when we retain all 54 instrumental variables.

5. Empirical Results

This section presents estimates of the impact of child sponsorship on completed years of schooling and on the probabilities of completing primary, secondary, and tertiary education. It also summarizes estimates of impacts on employment and leadership, and presents robustness checks.

5.1 Estimates of Impact on Education

Table 3A provides, by country, descriptive statistics of the outcome variables and the main control variables. Table 3B shows how the outcome variables differ between all sponsored children (first column), all non-sponsored children in the sample (middle column), and non-sponsored siblings of sponsored children (last column). Simple *t*-tests in Table 3B that do not account for the influence of control variables show statistically significant differences in all of these adult life outcomes, including formal years of schooling, where sponsored children realize 1.38 more years of schooling than their unsponsored siblings and 1.79 more years of schooling than their unsponsored peers.

Tables 4A-4D provide estimates of equations (1) through (4), that is, estimates based on OLS, IV-GMM, OLS with household-level fixed effects, and IV-GMM with household-level fixed effects. The education outcomes differ for each table; years of completed schooling is in Table 4A, and primary, secondary, and university completion are in Tables 4B, 4C and 4D, respectively. OLS estimates of the impact of Compassion sponsorship on completed years of formal schooling in the first column of Table 4A show a highly significant estimated direct impact (τ) of a little over one year (1.03 years). The coefficients on the other categories of individuals in the next six rows of the table measure differences in educational outcomes between those individuals and individuals in the same type of household who were 17 or older when the Compassion program was introduced in their village (or a nearby village). For example, the second (third) row suggests that children who were 12 and younger (13-16) in Compassion households when the program was introduced but did not participate in the program eventually attained about 0.42 (0.17) more years of schooling than their siblings who were 17 or older when the program began, but neither difference is statistically significant. Similarly, rows 4 and 5 compare younger household members to those aged 17 and older in non-Compassion households in Compassion villages, and rows 6 and 7 do the same among those in non-Compassion villages. The assumption that the general specification used in equations (1) - (4) is reasonable – more specifically that C_h, C_v and the observed covariates in $\boldsymbol{X}$ adequately account for differences between individuals with ACI of 13-16 across Compassion households, non-Compassion households in Compassion villages and households in non-Compassion villages, which implies that $\alpha_2 = \beta_2 = \gamma_2$ – is tested in the eighth row of Table 4A and is not rejected.

The results in Table 4A also allow one to test for spillover effects, as explained in Section 4. The first check for possible spillovers is to test the assumption of "parallel trends" among non-sponsored students with ACI ≤ 12 in the three types of households, which implies $\alpha_1 = \beta_1 = \gamma_1$; this is not rejected (ninth row). The tenth row of Table 4A directly checks for intra-household spillovers by comparing the difference in years of schooling between non-participating individuals with ACI 12 or below and those with ACI 13-16 in Compassion households with the same difference for individuals in nearby non-Compassion villages. The insignificance of this double difference, $(\alpha_1-\alpha_2)$ - $(\gamma_1-\gamma_2)$, yields no evidence for intra-household spillovers from sponsored children to age-eligible non-sponsored siblings in Compassion households. The eleventh row tests for spillovers within villages by comparing the difference between individuals with ACI 12 or below and those with ACI 13-16 in non-Compassion households in Compassion villages with the same difference in non-Compassion villages, $(\beta_1-\beta_2)$ - $(\gamma_1-\gamma_2)$. Thus there is no evidence of intra-village spillovers from Compassion households onto age-eligible children in non-Compassion households in Compassion villages.

The impact on program participants that includes possible spillover effects, $\tau+(\alpha_1-\alpha_2)$ - $(\gamma_1-\gamma_2)$, is in the twelfth row of Table 4A. This "full" program impact is somewhat smaller than the direct program impact (0.76 vs. 1.03), and is statistically insignificant. Yet the lack of statistical significance is due to adding the four statistically insignificant parameters that attempt to capture spillovers to the estimate of the direct impact. Because the spillover estimates are insignificant, our preferred estimate of the Compassion program's impact on years of schooling is the direct impact (τ), which is 1.03.

The second column of Table 4A presents IV estimates of the impact of the Compassion program on years of schooling, which are somewhat higher (direct impact of 1.38 years). Note that the exclusion restrictions are not rejected by the over-identification test (*p*-value = 0.232), and the *F*-test of the explanatory power of the excluded instruments is quite large (60.03). However, the Hausman test does not reject the hypothesis that the OLS and IV estimates are equal (*p*-value = 0.969). As with the OLS estimates, there is no evidence of intra-household or intra-village spillovers, and the "full" program effect on participants (1.34 years) is very similar to the direct effect (1.38), though it is significant only at the 10% level due to the imprecision of the estimated spillover effects.

The third and fourth columns carry out the same estimates in the first two columns, but add household fixed-effects. Estimates of direct impacts (τ) are slightly higher, at 1.12 and 1.46 years, and although the program effect including spillovers, $\tau+(\alpha_1-\alpha_2)$ - $(\gamma_1-\gamma_2)$, is estimated to be somewhat higher (1.36 and 1.59 years, respectively), there is no significant evidence of spillovers, so the estimates of the direct impacts in Table 4A are our preferred measures of impact.[10]

[10] There is marginally significant evidence that the parallel trends assumption is violated in the IV-GMM-FE specification (*p*-value of 0.048), but since the seven other tests of parallel trends in Table 4A do not reject that assumption, and the Hausman test does not indicate a need for IV estimation, we do not view this as a major cause for concern.

Estimation results for primary school completion are shown in Table 4B. The estimates of the direct impact (τ) are generally significant, but small, probably due to the relatively high rate of baseline primary school completion in these villages (88.7%). These estimates are similar across columns, ranging from 4.0 (OLS estimates) to 7.7 percentage points (IV estimates without fixed effects). All except for the fixed-effect IV estimate in the last column are significant at the 1% level. There is no evidence of spillovers either within households or within Compassion villages.

Table 4C presents estimates of the probability of completing secondary school. Because the Compassion program typically sponsors children to the end of secondary school, this is a natural level of school completion to examine. OLS estimates (first column) indicate that the direct effect of the program (τ) raises the probability of completing secondary school by 13.2 percentage points. The IV estimates (second column) show a 16.5 percentage point effect. Neither of these estimates shows significant spillover effects. Household fixed-effects (third and fourth columns), yield marginally significant estimates of household spillovers onto other age-eligible siblings in secondary school completion, about 7 percentage points (p-value 0.109) in the OLS estimates and about 10 percentage points (p-value 0.110) in the IV estimates. (Another indication of spillovers is the rejection of parallel trends for individuals with ACI $\leq$ 12; p-value of 0.011.) Incorporating these estimated spillovers increases the full program effect to 18.5 percentage points (OLS-FE) and 15.9 percentage points (IV-GMM-FE) on sponsored individuals, both of which are highly significant. The spillover point estimates, if valid, also indicate a 7 to 10 percentage point impact on other age-eligible siblings.

Estimated impacts on completion of tertiary education are in Table 4D. OLS estimates with and without household fixed effects in the first and third columns yield statistically significant direct impacts (τ) of 2.4 and 2.1 percentage points, respectively. Given a baseline completion rate of 4.3%, these point estimates, while small, reflect an approximate 50% increase over this baseline. The IV point estimates in columns 2 and 4 are somewhat larger, but are of low statistical significance; moreover, Hausman tests cannot reject the consistency of the OLS estimates. Point estimates of intra-household spillovers are small, but positive, such that the "full" impacts of the program (including spillovers) range from 3.2 to 5.1 percentage points, each significant at the 5% level. But due to little direct evidence for household spillovers, we consider the more conservative estimates (on τ only) to be the best estimate of program impact. The IV-GMM estimate finds evidence of positive intra-village spillovers that is significant at the 5% level, but since this is the only significant evidence of such spillovers out of 16 estimates in Tables 4A-4D, it could simply reflect random chance. (At the 5% significance level one would expect 1 out of 20 estimates of a parameter that equals zero to be significantly different from zero.)

Table 5 presents estimated direct impacts (τ) separately for each of the six countries, focusing on the OLS and OLS with household fixed effects estimates.[11] While the program impacts are positive and statistically significant in each country, a striking feature of these estimates is the variation across countries. The estimated impacts of sponsorship are highest in Uganda; the OLS estimates are 2.47 years more years of schooling, a 10.6 percentage point increase in primary school completion, and 25.3 and 7.7 percentage point increases for secondary and university completion, respectively (household fixed-effect estimates are quite similar). The second-highest impacts are most often found for Kenya, where point estimates are 1.16 years for formal schooling, with impacts (in percentage points) of 3.3 for primary completion, 12.2 for secondary completion, but no significant impact for university completion. Guatemala and the Philippines have high estimated impacts for secondary school completion, at 14.0 and 11.7 percentage points, respectively. In Bolivia, nearly all of the significant impact occurs at the university level, where sponsored individuals are 5.1 percentage points more likely to acquire a university education. In India, impacts are smaller and confined to the lower levels of education. Overall the magnitude of educational impacts across educational levels is much higher in the African countries than in the Latin American and Asian countries. Thus, the impact of child sponsorship appears to be greatest where counterfactual levels of education are lowest; Kenya and Uganda have the lowest rates of secondary school completion, and are second and third lowest in terms of years of completed schooling, among the six countries in our data.

Table 6 shows estimated impacts on total years of formal schooling by country and by gender, based on the OLS fixed-effect specification (OLS estimates without fixed effects are very similar). The most striking feature of these results is that the impacts are generally larger for the gender with the lowest baseline education levels. In particular, in the Philippines and India, where baseline schooling is higher for girls among non-sponsored children, the impact of sponsorship is higher for boys. Similarly, in Uganda, Guatemala, and Bolivia, where baseline schooling is higher for boys among non-sponsored children, point estimates of program impact are higher for girls.

5.2 Impacts on Employment and Leadership

Along with educational goals, two other major objectives of the Compassion program are to prepare sponsored children for employment and community leadership. We briefly discuss program impacts on these outcomes; the estimates are given in the appendix. Two employment outcomes are

[11] We omit country specific instrumental variable estimates for space consideration, and because Hausman tests never reject the consistency of the OLS estimates (with and without household fixed effects) in the combined sample. Also, the GMM-IV results are generally much less precise than the OLS estimates, and this imprecision is worsened by the small samples for each country. Finally GMM IV estimation requires the number of instruments be less than the number of clusters (villages) to obtain estimates of clustered standard errors. This is not an issue for estimates that combine all six countries (which have 32 villages, and use 8 instruments), but for the individual countries we have only 4 to 7 villages.

considered: 1) the probability of formal employment, defined as "currently employed at a steady salary," which rules out itinerant laborer work; and 2) the probability of "white collar" employment.[12] Table A.1 presents basic descriptive statistics for the employment and leadership variables. OLS (OLS with fixed-effects) estimates in Table A.2 show a significant direct impact (τ) of 5.1 (6.3) percentage points over a 35.7% baseline level of formal employment. The IV-GMM estimates of τ are much larger, but the "full impact" estimates that adjust for negative spillovers (which may reflect an inelastic supply of salaried jobs in the local community) are closer to the OLS and OLS-FE estimates. OLS estimates for white-collar employment in Table A.3 show impacts of 6.5 and 6.7 percentage points over a baseline of 18.5%. Multinomial logit estimates of program impacts on movement into the different job categories in Table A.4 show that the increase in white collar work is not from a large movement into high-paying jobs; it primarily consists of movement toward relatively modestly-paid white-collar work, particularly teaching. With a 3.4 percentage point marginal effect, and a baseline level of 5.4%, Compassion sponsorship increases the probability that a child becomes a teacher by 63%. This may be because many Compassion children are from families with little exposure to white-collar work, so that teachers may serve as primary role models. Individual country estimations in Table A.7 show that employment impacts tend to be highest in countries where economic growth is higher, specifically in the Philippines, India, and Guatemala.

A third Compassion goal is to develop leadership skills. OLS and fixed-effects OLS estimates yield impacts of 2.2 to 0.9 percentage points, respectively, on the probability of being a community leader in adulthood over a baseline of 2.9%, but instrumental variable estimations are near zero or even negative (Table A.5). OLS and OLS with fixed-effects estimates (Table A.6) yield 6.0 and 3.5 percentage point increases, respectively, on the probability of becoming a church leader, over an 8.6% baseline, while instrumental variable estimations are negative and imprecisely estimated.

5.3 Robustness Checks

We conducted a number of robustness checks on these results. Estimates that limit the sample to those over age 25 yield similar point estimates and significance for the education (and employment) outcomes. We also tried several different sets of instruments, interacting our instrumental variables with three other variables to obtain (ACI group $\otimes$ SORR $\times$ gender), (ACI group $\otimes$ SORR $\times$ age), (ACI group $\otimes$ SORR $\times$ birth order). Some of these instruments offered a higher first-stage *F*-statistics, others slightly lower. Results from using different instruments yield

[12] We divided occupations into 14 types: 1-agriculture, 2-construction, 3-clerical/sales, 4-blue collar work, 5-personal services, 6-teaching, 7-public administration, 8-small business, 9-pastoral/religious ministry, 10-finance/large business, 11-police/army/security, 12-professional (doctors, lawyers, engineers, etc.), 13-less-skilled technical work (e.g. call centers), and 14-nursing/public health. A "white collar" job is defined as categories 3, 6, 7, 9, 10, and 12-14.

nearly identical estimates for the educational outcomes. Some instrument sets yielded slightly larger and more significant impacts on employment and leadership outcomes, while others were slightly weaker. Overall, our chosen instrument set is in about the middle of all the instrument sets we tried in terms of both first- and second-stage significance, and thus it provides reasonably stable estimates of the impact of the Compassion program.

A third robustness check verified that the estimated impacts do not reflect negative spillovers onto older siblings. Estimates for the education (and employment) variables on sponsored children who had no older siblings yields coefficients that are generally similar to those provided here. We also considered the possibility of reporting bias that favored the program. To avoid this bias, the enumerators had no formal ties to the sponsorship program. We also find no evidence of reporting bias; separate regressions for each type of principal respondent (parent, sponsored child, sibling, etc.) yield essentially no differences in point estimates or significance of estimated impacts. Lastly, although we found 93.5% of the families of the formerly sponsored children, could the results be significantly different for the 6.5% whom we were unable to locate? This is unlikely since the strongest results are for the two African countries, where we located 99% of the formerly sponsored children's families.

6. Conclusion

We estimate that the Compassion child sponsorship program increases years of completed schooling by 1.03 to 1.46 years over a baseline of 10.19 years, increases the probability of primary school completion by 4.0 to 7.7 percentage points (baseline 88.7%), secondary school completion by 11.6 to 16.5 percentage points (baseline 44.5%), and university completion by 2.1 to 2.4 percentage points (baseline 4.3%). We also find impacts of 5.1 to 6.3 percentage points on the probability of salaried employment in adulthood and a 6.5 to 6.7 percentage point increase in the probability of white collar employment, as well as modest evidence for causal impacts on community and church leadership.

One can compare these estimated impacts of Compassion's child sponsorship program on education outcomes to recent estimates for other educational interventions. For example, Aaronson and Mazumder (2011) examine the introduction of Rosenwald schools from 1913 to 1931 to foster the education among rural blacks in the U.S. south. They conclude that the introduction of these schools increased secondary school completion among rural blacks by 8.3 percentage points and increased formal schooling outcomes by 1.2 years, estimates that are strikingly similar to ours. Our results for the Compassion program compare favorably to the 0.66 year increase in years of schooling found by Schultz (2004) on the *PROGRESA/Oportunidades* CCT program in Mexico, and the 0.12 to

0.19 increase in years of schooling that Duflo (2001) estimates as the impact of the large school construction program in Indonesia; she also estimates a 6 percentage point increase in primary school completion, an estimate similar to ours, but finds a slightly negative impact on secondary completion.

We conclude by discussing a possible causal mechanism behind these impacts that subsequent work has begun to explore. The most salient characteristic that distinguishes Compassion's program from comparable interventions is its emphasis on raising children's self-esteem, reference points, and aspirations. As such, it aims to simultaneously relieve both internal and external constraints that can impede progress in education.[13] Indeed, the role of psychological factors has gained increasing attention in development economics.[14] Recent research has explored the role of psychological factors on credit decisions (Bertrand et al., 2010), health (Dupas, 2010), technology adoption (Duflo, Kremer and Robinson, 2011), and education (Kremer, Miguel and Thornton, 2009).

Three follow-up studies conducted in Bolivia, Kenya, and Indonesia investigate whether adult life outcomes may have been shaped by the Compassion program's focus on developing self-esteem and nurturing aspirations during childhood. Unlike this paper, which examines formerly sponsored subjects who are now adults, these three follow-up studies focus on *currently* sponsored children.

Ross (2010) examines the life aspirations of 270 children living near Compassion sponsorship projects in Bolivia. In response to the question, "What level of education would you say is sufficient for one to be successful today?" sponsored children's answers yield village-fixed-effect estimates averaging 0.89 years higher than unsponsored children (average $p < 0.05$), a figure just under the estimated impact on years of schooling found in this paper. Sponsored children also appear to have higher self-expectations for future vocations. When asked "What occupation do you *realistically expect* to have in the future?", sponsored children were 10.1 to 25.3 percentage points more likely than unsponsored children to respond with a white-collar occupation (average $p < 0.10$).

In a second follow-up study, Ross and Wydick (2011) surveyed 570 children aged 10 to 18 in three villages in Kenya, using an instrumental variable strategy that exploits the same age-eligibility and limited-children-per-family sponsorship rules used in this paper. Estimates indicate that sponsorship raises educational expectations by 0.23 standard deviations of the distribution of those expectations ($p < 0.10$), as well as raising the probability that a child expects to have a white-collar job by 12.5 percentage points. These changes in expectations about future education and vocation are similar to the estimated impacts among the adults found in this paper.

A third follow-up study, Glewwe and Wydick (2012), examines data from 540 poor children in the slums of Jakarta, Indonesia, finding impacts on schooling aspirations from sponsorship that

[13] Dalton et al. (2010) provides an excellent theoretical treatment of internalized poverty constraints.

[14] Mullainathan (2006) and Bernard et al. (2011) review the role of psychology in development economics.

parallel the findings of the previous two studies. In addition, a new box of 24 colored pencils was placed in front of each child, who was then asked to "Draw a picture of yourself in the rain." Using factor analysis with a varimax rotation on twenty dummy variables that relate drawing characteristics to five measures of hopefulness and self-esteem, three latent factors were identifiable: Happiness, Self-Efficacy, and Hopelessness.[15] Regressions controlling for age, gender, family selection, and neighborhood reveal a 0.27 standard deviation increase in happiness among the Compassion-sponsored children ($t = 2.40$), a 0.33 standard deviation increase in self-efficacy ($t = 3.11$), and a 0.52 standard deviation *decrease* in hopelessness ($t = 5.19$).

While further work is required to establish a causal link between aspirations and adult life outcomes, the possibility that nurturing aspirations can have important effects on economic development has intriguing implications. Traditionally, development economics--and indeed the practice of economic development--has focused on the relief of *external* constraints such as school quality, infrastructure, and credit. But it may be that the *internal* constraints of the poor also contribute to poverty traps in important ways. Further observational and experimental research should seek to better understand the internal constraints faced by the poor, and how development efforts that seek to release internal constraints can complement purely economic interventions and incentives.

[15] There is a large literature on interpreting children's drawings to gauge their psychological well-being (Koppitz, 1968; Klepsch and Logie, 1982; Furth, 2002). Researchers have found empirical correlations between aspects of child's human figure drawings to a variety of professionally diagnosed disorders, including anxiety and emotional insecurity (missing mouth, frowning, use of dark colors), low self-esteem (tiny figure, poor integration of body parts, missing arms or hands), and low self-efficacy (tiny head, short arms).

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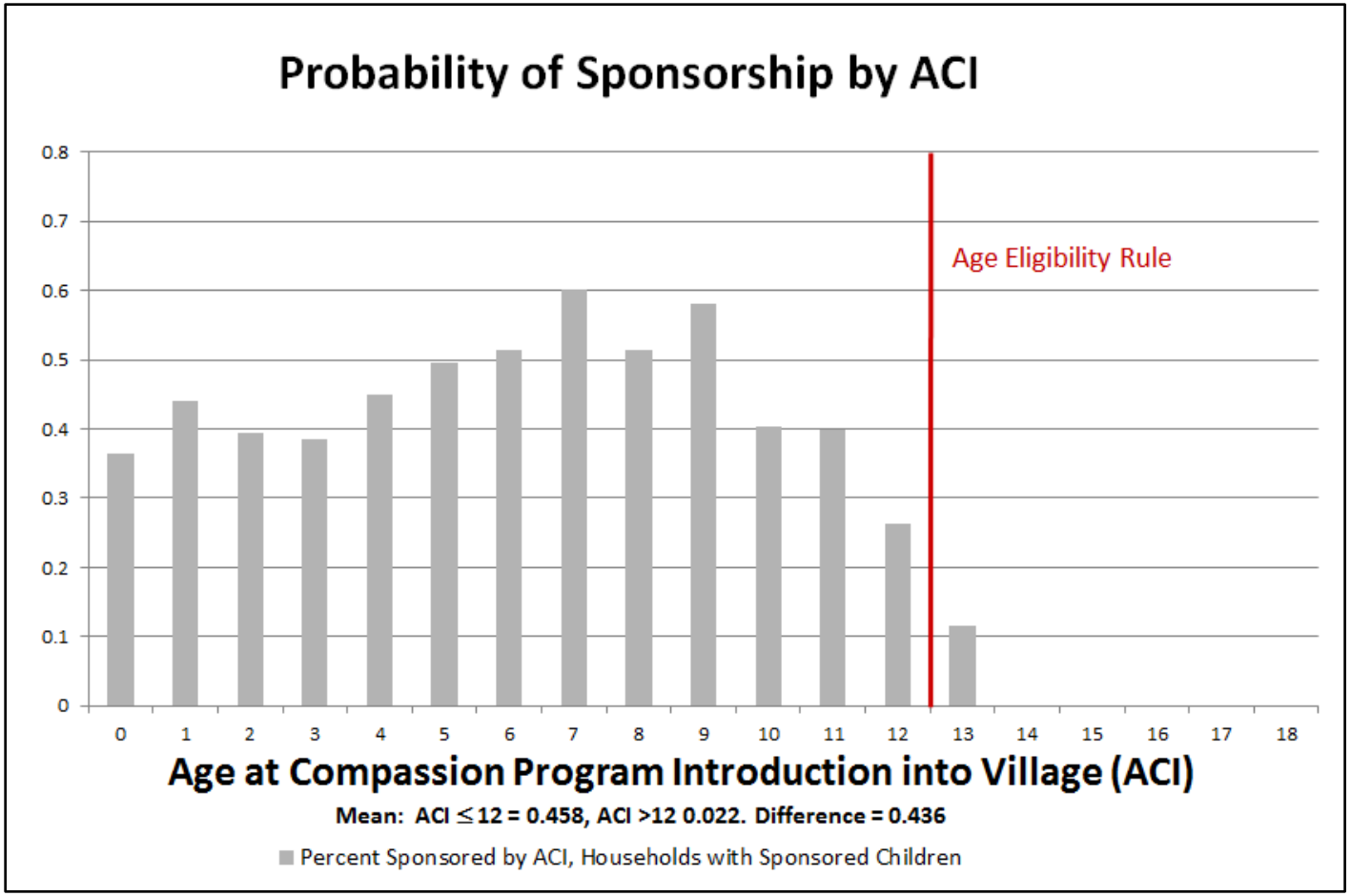

Figure 1: Discontinuity in Sponsorship by Age at Time of Program Introduction

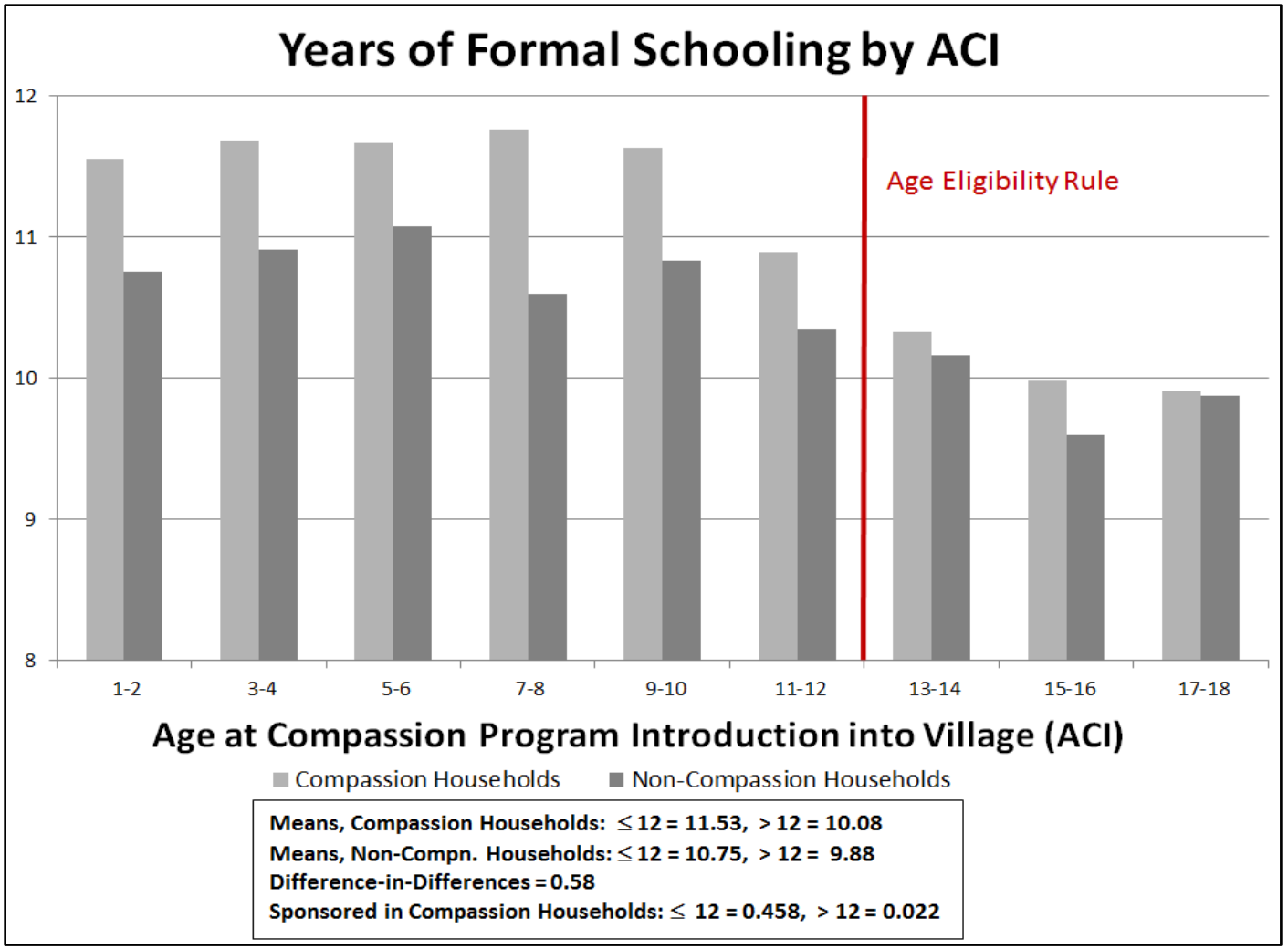

Figure 2a: Total Years of Formal Schooling by Age when the Compassion Program was Introduced into a Village (ACI)

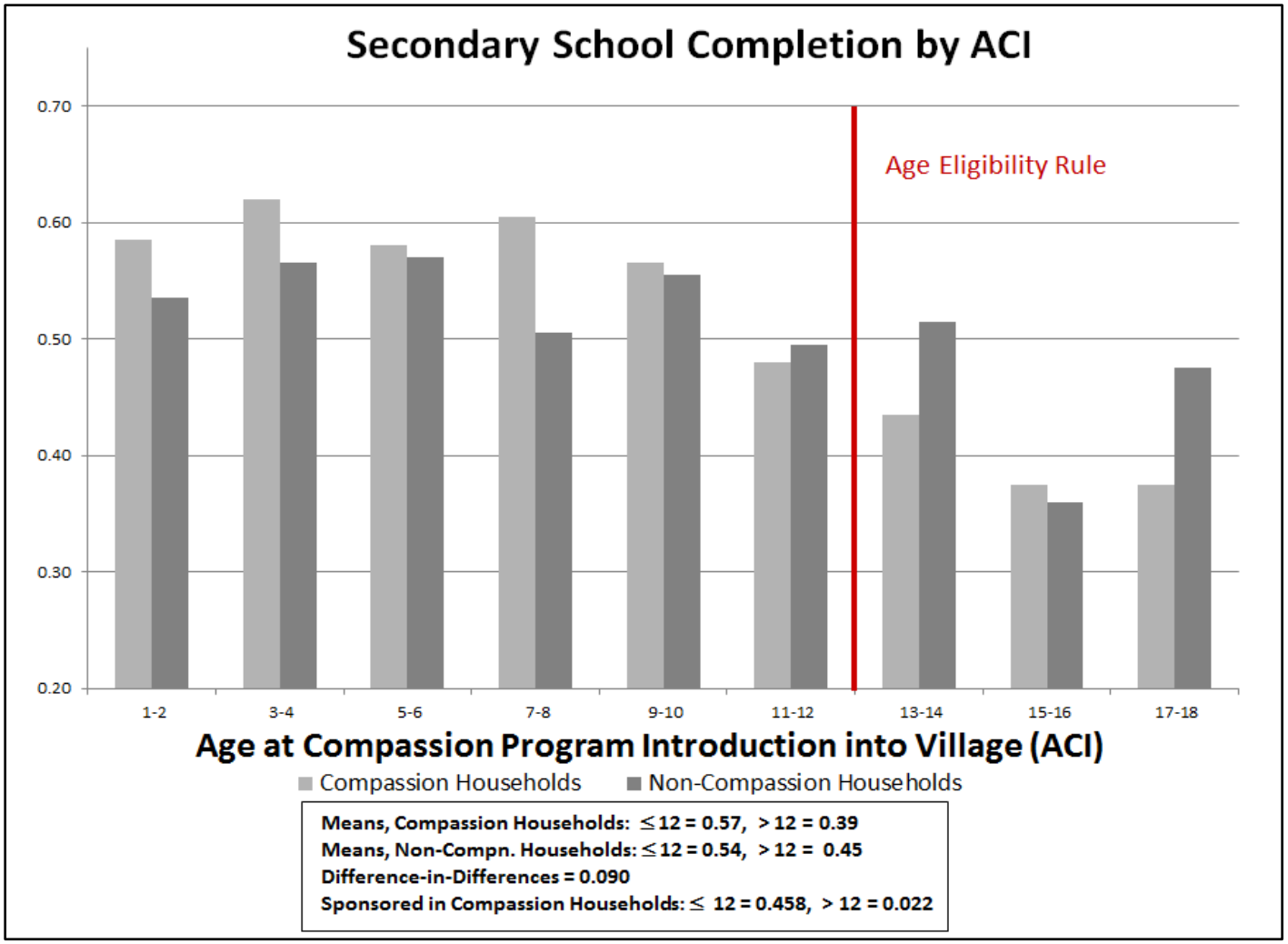

Figure 2b: Secondary School Completion by Age when the Compassion Program was Introduced into a Village (ACI)

Table 1: The Ten Leading International Child Sponsorship Programs

Organization:	International Headquarters	Year Founded	No. of Countries	Contribution per month	No. of Sponsored Children*
1. World Vision†	USA	1953	100	$30	4,100,000
2. Plan USA	USA	1937	49	$24	1,500,000
3. Compassion International†	USA	1952	26	$38	1,288,632
4. ChildFund International	USA	1938	31	$24	510,000
5. Children International	USA	1980	11	$22	340,000
6. Christian Foundation for Children and Aging †	USA	1981	23	$30	291,262
7. Kindernothilfe†	Germany	1959	28	$30	145,814
8. Save the Children	USA	1932	50	$28	120,000
9. SOS Children's Villages	USA	1949	132	$28	80,000
10. Bornefonden	Denmark	1972	5	$34	72,473
Others* (197)					692,979
Total					**9,141,160**

*Child sponsorship organizations by donating country: USA (43), UK (41), France (18), Canada (10), Italy (10), Australia (9), Denmark (7), Spain (7), Norway (6),Germany (5), Sweden (4), Others (16).

† Faith-Based Organization.

Table 2A: Compassion Program Benefits by Country

Country	Uniforms	Tutoring	School Materials	Spiritual Instruction	Healthcare	Gifts from Sponsors	Cash to Family
Uganda	Yes	Yes	Yes	Yes	Yes	Yes	No
Guatemala	Yes	Limited	Yes	Yes	Yes	Yes	No
Philippines	Limited	Limited	Yes	Yes	Yes	Yes	No
India	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Kenya	Yes	Yes	Limited	Yes	Yes	Yes	No
Bolivia	Limited	Yes	Yes	Yes	Yes	Yes	Limited

Table 2B: Survey Information by Country

Country	Treatment Villages and Cities (year the program began)	Non-Treatment Villages and Cities	Sample Size	Time of Survey
Uganda	Jinja (1980), Bugiri (1981), Masaka (1989)	Kakooge, Bombo	809	June-August 2008
Guatemala	San Pedro La Laguna (1991), San Juan La Laguna (1992), San Pedro Necta (1992)	San Pablo La Laguna, Santiago Chimaltenango	1,762	May-July 2009
Philippines	Quezon City (1986), Bacolod (1986)	Skybag, Handumanan	1,428	Nov. 2009 - February 2010
India	Tuticorn (1980), Sawyerpuram (1980), Bangalore (1986)	Eral, Bangalore	1,622	March-April 2010
Kenya	Cianthia (1986), Cierria (1986), Nderu (1990), Thigio (1990)	Riakingenyi, Kerwa, Rusigeti	3,056	April-June 2010
Bolivia	Chulla (1992), Los Olivios (1990), Puntiti (1991), Pongonhuyo (1980)	Pairumani-Iscaypata, Igrana	1,467	June-August 2010
Six Countries	**19 Compassion Programs**	**13 Non-Treatment Areas**	**10,144**	**June 2008-August 2010**

Table 3A: Summary Statistics
(Means; Standard Deviations in Parentheses)

	Uganda	Guatemala	Philippines	India	Kenya	Bolivia	**All Six Countries**
Sponsored as a Child	0.232	0.213	0.168	0.138	0.178	0.198	**0.183**
	(0.423)	(0.409)	(0.374)	(0.345)	(0.383)	(0.398)	**(0.387)**
Years Sponsored	11.325	6.717	7.469	11.065	10.207	9.510	**9.287**
	(3.067)	(2.467)	(4.629)	(3.230)	(3.332)	(3.747)	**(3.790)**
Total Years of Education	9.185	8.859	12.180	11.696	10.442	10.750	**10.566**
	(4.003)	(4.295)	(1.994)	(3.345)	(3.078)	(4.155)	**(3.654)**
Completed Primary	0.841	0.795	0.994	0.937	0.953	0.847	**0.905**
	(0.366)	(0.404)	(0.080)	(0.244)	(0.211)	(0.360)	**(0.2930)**
Completed Secondary	0.269	0.486	0.745	0.602	0.338	0.537	**0.485**
	(0.444)	(0.500)	(0.436)	(0.490)	(0.473)	(0.499)	**(0.500)**
Completed Tertiary	0.077	0.022	0.019	0.047	0.040	0.117	**0.049**
	(0.266)	(0.148)	(0.135)	(0.212)	(0.195)	(0.321)	**(0.216)**
Age	29.968	26.635	29.054	32.575	30.806	29.113	**29.827**
	(8.642)	(6.468)	(8.751)	(8.813)	(7.919)	(7.893)	**(8.232)**
Gender	0.476	0.511	0.502	0.500	0.515	0.501	**0.505**
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	**(0.500)**
Number of Siblings	4.580	4.838	4.389	3.991	6.307	4.908	**5.076**
	(2.339)	(2.195)	(2.089)	(1.944)	(2.237)	(2.003)	**(2.308)**
Mother's Education	5.385	2.105	9.682	6.982	3.862	2.783	**4.853**
	(3.778)	(2.909)	(3.120)	(3.645)	(4.116)	(3.059)	**(4.338)**
Father's Education	7.024	3.496	10.032	7.629	5.538	4.727	**6.166**
	(4.044)	(3.645)	(3.203)	(3.635)	(4.318)	(3.381)	**(4.310)**

Table 3B: Summary Means and T-tests of Education Variables

	Mean, Sponsored individuals (std. dev.)	Mean, All non-sponsored individuals (std. dev.)	Difference *t*-test (std. error)	Mean, Non-sponsored siblings in sponsored households	Difference *t*-test (std. error)
Total Years of Education	12.03	10.24	1.79***	10.65	1.38***
	(2.790)	(3.74)	(0.093)	(3.41)	(0.092)
Completed Primary	0.984	0.887	0.096***	0.927	0.057***
	(0.127)	(0.316)	(0.0075)	(0.260)	(0.0064)
Completed Secondary	0.646	0.449	0.196***	0.460	0.185***
	(0.478)	(0.497)	(0.013)	(0.498)	(0.014)
Completed University	0.078	0.043	0.035***	0.049	0.029***
	(0.268)	(0.203)	(0.0056)	(0.216)	(0.007)

Full sample size = 10,011. Number of formerly sponsored individuals = 1,834. Number of individuals in non-treated households = 4,560. Number of non-sponsored individuals in treated households = 3,617.

Table 4A: OLS and IV Estimates of Equations (1), (2), (3), and (4) Years of Schooling

VARIABLES	OLS	IV-GMM	OLS-FE	IV-GMM-FE
Program participant (τ)	1.034***	1.383***	1.118***	1.455***
	(0.152)	(0.441)	(0.121)	(0.407)
Compassion hh: ACI ≤ 12 (α_1)	0.421	-0.069	0.720	0.765
	(0.555)	(0.330)	(0.432)	(0.475)
Compassion hh: ACI 13-16 (α_2)	0.169	-0.236	0.039	0.082
	(0.417)	(0.251)	(0.265)	(0.207)
Program village ACI ≤ 12 (β_1)	0.588	0.476	0.897**	0.916
	(0.611)	(0.437)	(0.436)	(0.579)
Program village ACI 13-16 (β_2)	-0.013	-0.205	0.414	0.405
	(0.533)	(0.430)	(0.459)	(0.468)
Nonprog. village ACI ≤ 12 (γ_1)	0.690	0.248	-0.085	0.047
	(0.734)	(0.697)	(0.504)	(0.435)
Nonprog. village ACI 13-16 (γ_2)	0.161	0.041	-0.525**	-0.499***
	(0.523)	(0.486)	(0.197)	(0.185)
F-tests/χ^2 tests of parallel trends:				
$\alpha_2 = \beta_2 = \gamma_2$	0.08	0.26	2.26	6.08**
	[0.926]	[0.877]	[0.121]	[0.048]
$\alpha_1 = \beta_1 = \gamma_1$	0.07	1.54	1.03	1.38
	[0.935]	[0.464]	[0.368]	[0.501]
Intra-Household Spillovers (α_1-α_2) - (γ_1-γ_2)	-0.277	-0.039	0.240	0.137
	(0.686)	(0.686)	(0.516)	(0.555)
Intra-Village Spillovers (β_1-β_2) - (γ_1-γ_2)	0.072	0.474	0.043	-0.036
	(0.631)	(0.584)	(0.540)	(0.529)
Program Impact including household Spillovers τ +(α_1-α_2) - (γ_1-γ_2)	0.757	1.344*	1.359**	1.592***
	(0.678)	(0.713)	(0.506)	(0.446)
Hausman Test [*p*-value]	--	[0.969]	--	[0.922]
Over-identification (J-test) [*p*-value]	--	9.30	--	6.05
		[0.232]		[0.534]
Weak IV Test (*F*-statistic)	--	60.03	--	48.93
Observations	9954	9954	9,954	9,954
R-squared	0.253	0.250	0.064	0.047

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, status as oldest child, mother's education, father's education, mother's education missing and father's education missing.

Table 4B: OLS and IV Estimates of Equations (1), (2), (3), and (4)
Primary School Completion

VARIABLES	OLS	IV-GMM	OLS-FE	IV-GMM-FE
Program participant (τ)	0.0404***	0.0771***	0.0493***	0.0503
	(0.0086)	(0.0282)	(0.0097)	(0.0453)
Compassion hh: ACI ≤ 12 (α_1)	0.0171	-0.0133	0.0199	0.0358
	(0.0540)	(0.0288)	(0.0382)	(0.0284)
Compassion hh: ACI 13-16 (α_2)	0.0042	-0.0243	-0.0062	0.0069
	(0.0437)	(0.0303)	(0.0261)	(0.0175)
Program village ACI ≤ 12 (β_1)	0.0367	0.0062	0.0657**	0.0468
	(0.0574)	(0.0493)	(0.0311)	(0.0374)
Program village ACI 13-16 (β_2)	-0.0217	-0.0507	0.0205	0.0119
	(0.0480)	(0.0402)	(0.0312)	(0.0337)
Nonprog. village ACI ≤ 12 (γ_1)	0.0658	-0.0079	0.0133	-0.0058
	(0.0773)	(0.0714)	(0.0493)	(0.0448)
Nonprog. village ACI 13-16 (γ_2)	0.0019	0.0157	-0.0288	-0.0355*
	(0.0550)	(0.0479)	(0.0211)	(0.0194)
F-tests/χ^2 tests of parallel trends:				
$\alpha_2 = \beta_2 = \gamma_2$	0.16	1.20	0.98	3.20
	[0.856]	[0.548]	[0.388]	[0.202]
$\alpha_1 = \beta_1 = \gamma_1$	0.17	0.22	0.74	0.72
	[0.846]	[0.894]	[0.487]	[0.698]
Intra-Household Spillovers				
(α_1-α_2) - (γ_1-γ_2)	-0.0511	0.0347	-0.0160	-0.0007
	(0.0719)	(0.0708)	(0.0534)	(0.0532)
Intra-Village Spillovers				
(β_1-β_2) - (γ_1-γ_2)	-0.0056	0.0805	0.0030	0.0053
	(0.0693)	(0.0666)	(0.0568)	(0.0451)
Program Impact				
including household Spillovers	-0.0106	0.1118	0.0332	0.0495
τ +(α_1-α_2) - (γ_1-γ_2)	(0.0726)	(0.0745)	(0.0530)	(0.0429)
Hausman Test [*p*-value]	--	[0.888]	--	[0.830]
Over-identification (J-test) [*p*-value]	--	9.47	--	8.23
		[0.220]		[0.313]
Weak IV Test				
(*F*-statistic)	--	60.03	--	48.93
Observations	9954	9954	9954	9954
R-squared	0.123	0.104	0.031	0.030

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, status as oldest child, mother's education, father's education, mother's educ. missing and father's educ. missing.

Table 4C: OLS and IV Estimates of Equations (1), (2), (3), and (4) Secondary School Completion

VARIABLES	OLS	IV-GMM	OLS-FE	IV-GMM-FE
Program participant (τ)	0.1324***	0.1648*	0.1160***	0.0624
	(0.0275)	(0.0937)	(0.0172)	(0.0846)
Compassion hh: ACI ≤ 12 (α_1)	-0.0266	-0.0574	0.0934*	0.1669***
	(0.0523)	(0.0582)	(0.0553)	(0.0638)
Compassion hh: ACI 13-16 (α_2)	-0.0236	-0.0495*	-0.0060	0.0219
	(0.0385)	(0.0267)	(0.0339)	(0.0238)
Program village ACI ≤ 12 (β_1)	-0.0113	-0.0092	0.1126	0.1655
	(0.0589)	(0.0387)	(0.0751)	(0.1094)
Program village ACI 13-16 (β_2)	-0.0324	-0.0551	0.0554	0.0810
	(0.0615)	(0.0447)	(0.0764)	(0.0873)
Nonprog. village ACI ≤ 12 (γ_1)	-0.0219	-0.0134	-0.0323	-0.0076
	(0.0602)	(0.0579)	(0.0495)	(0.0445)
Nonprog. village ACI 13-16 (γ_2)	-0.0143	-0.0082	-0.0624*	-0.0561*
	(0.0449)	(0.0432)	(0.0309)	(0.0294)
F-tests/χ^2 tests of parallel trends:				
$\alpha_2 = \beta_2 = \gamma_2$	0.03	0.83	1.37	6.56**
	[0.970]	[0.660]	[0.270]	[0.038]
$\alpha_1 = \beta_1 = \gamma_1$	0.04	0.53	2.33	8.95**
	[0.962]	[0.765]	[0.114]	[0.011]
Intra-Household Spillovers $(\alpha_1-\alpha_2) - (\gamma_1-\gamma_2)$	0.0046	-0.0027	0.0694*	0.0965
	(0.0554)	(0.0645)	(0.421)	(0.0603)
Intra-Village Spillovers $(\beta_1-\beta_2) - (\gamma_1-\gamma_2)$	0.0286	0.0511	0.0271	0.0360
	(0.0555)	(0.0484)	(0.0480)	(0.0531)
Program Impact including household Spillovers $\tau + (\alpha_1-\alpha_2) - (\gamma_1-\gamma_2)$	0.1371**	0.1621**	0.1854***	0.1589***
	(0.0581)	(0.0642)	(0.0425)	(0.0503)
Hausman Test [*p*-value]	--	[0.959]	--	[0.785]
Over-identification (J-test) [*p*-value]	--	6.18	--	6.85
		[0.519]		[0.444]
Weak IV Test (*F*-statistic)	--	60.03	--	48.93
Observations	9954	9954	9954	9954
R-squared	0.164	0.161	0.033	0.019

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, status as oldest child, mother's education, father's education, mother's educ. missing and father's educ. missing.

Table 4D: OLS and IV Estimates of Equations (1), (2), (3), and (4)
University Completion

VARIABLES	OLS	IV-GMM	OLS-FE	IV-GMM-FE
Program participant (τ)	0.0243***	0.0356*	0.0213**	0.0496
	(0.0090)	(0.0213)	(0.0085)	(0.0345)
Compassion hh: ACI ≤ 12 (α_1)	0.0164	0.0085	0.0331*	0.0224
	(0.0139)	(0.0113)	(0.0174)	(0.0162)
Compassion hh: ACI 13-16 (α_2)	0.0094	0.0038	0.0150	0.0137
	(0.0117)	(0.0099)	(0.0129)	(0.0123)
Program village ACI ≤ 12 (β_1)	0.0300*	0.0301**	0.0241	0.0150
	(0.0170)	(0.0120)	(0.0228)	(0.0279)
Program village ACI 13-16 (β_2)	0.0105	0.0060	0.0131	0.0044
	(0.0190)	(0.0120)	(0.0183)	(0.0193)
Nonprog. village ACI ≤ 12 (γ_1)	-0.0036	-0.0010	0.0021	0.0006
	(0.0161)	(0.0142)	(0.0199)	(0.0189)
Nonprog. village ACI 13-16 (γ_2)	-0.0031	0.0001	-0.0069	-0.0065
	(0.0163)	(0.0147)	(0.0114)	(0.0111)
F-tests/χ^2 tests of parallel trends:				
$\alpha_2 = \beta_2 = \gamma_2$	0.24	0.11	1.17	1.99
	[0.786]	[0.948]	[0.324]	[0.370]
$\alpha_1 = \beta_1 = \gamma_1$	1.60	8.23**	1.07	0.96
	[0.218]	[0.016]	[0.356]	[0.619]
Intra-Household Spillovers $(\alpha_1-\alpha_2) - (\gamma_1-\gamma_2)$	0.0075	0.0058	0.0091	0.0015
	(0.0128)	(0.0125)	(0.0126)	(0.0164)
Intra-Village Spillovers $(\beta_1-\beta_2) - (\gamma_1-\gamma_2)$	0.0200	0.0252**	0.0020	0.0035
	(0.0145)	(0.0119)	(0.0206)	(0.0207)
Program Impact including household Spillovers $\tau +(\alpha_1-\alpha_2) - (\gamma_1-\gamma_2)$	0.0318**	0.0414**	0.0304**	0.0511**
	(0.0137)	(0.0178)	(0.0149)	(0.0247)
Hausman Test [*p*-value]	--	[0.625]	--	[0.304]
Over-identification (J-test) [*p*-value]	--	9.45	--	2.73
		[0.222]		[0.909]
Weak IV Test (*F*-statistic)	--	60.03	--	48.93
Observations	9954	9954	9954	9954
R-squared	0.021	0.020	0.012	0.006

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, status as oldest child, mother's education, father's education, mother's educ. missing and father's educ. missing.

Table 5: OLS and OLS Household Fixed Effect Estimates for Education by Country

VARIABLES						
TOTAL YEARS OF EDUCATION	***Uganda***	***Guatemala***	***Philippines***	***India***	***Kenya***	***Bolivia***
Program participant (τ), OLS	2.472***	0.528***	0.573***	0.658**	1.156***	0.668**
	(0.236)	(0.113)	(0.064)	(0.226)	(0.142)	(0.184)
R^2	0.332	0.426	0.213	0.246	0.156	0.307
Program participant (τ), HH FE	2.216***	0.830***	0.631***	0.768**	1.312***	0.733**
	(0.232)	(0.166)	(0.008)	(0.171)	(0.141)	(0.228)
R^2	0.127	0.147	0.114	0.062	0.075	0.128
Baseline, Untreated	*8.37*	*8.12*	*12.11*	*11.45*	*10.22*	*10.32*
PRIMARY COMPLETION	***Uganda***	***Guatemala***	***Philippines***	***India***	***Kenya***	***Bolivia***
Program participant (τ), OLS	0.1061**	-0.0112*	0.0030	0.0298*	0.0334*	0.0380
	(0.0283)	(0.0048)	(0.0016)	(0.0115)	(0.0152)	(0.0299)
R^2	0.206	0.329	0.060	0.093	0.036	0.231
Program participant (τ), HH FE	0.0966**	0.0270*	0.0054	0.0293*	0.0517**	0.0491
	(0.0339)	(0.0116)	(0.0028)	(0.0128)	(0.0162)	(0.0313)
R^2	0.076	0.120	0.009	0.027	0.014	0.170
Baseline, Untreated	*0.795*	*0.744*	*0.993*	*0.926*	*0.948*	*0.816*
SECONDARY COMPLETION	***Uganda***	***Guatemala***	***Philippines***	***India***	***Kenya***	***Bolivia***
Program participant (τ), OLS	0.2532**	0.1403***	0.1173***	0.0833*	0.1218***	0.0134
	(0.0568)	(0.0106)	(0.0176)	(0.0455)	(0.0207)	(0.0144)
R^2	0.196	0.293	0.151	0.195	0.095	0.204
Program participant (τ), HH FE	0.2176**	0.1320***	0.1203***	0.0869*	0.1154**	0.0259
	(0.0580)	(0.0206)	(0.0026)	(0.0464)	(0.0345)	(0.0235)
R^2	0.075	0.099	0.060	0.039	0.037	0.052
Baseline, Untreated	*0.192*	*0.406*	*0.732*	*0.568*	*0.314*	*0.503*
UNIVERSITY COMPLETION	***Uganda***	***Guatemala***	***Philippines***	***India***	***Kenya***	***Bolivia***
Program participant (τ), OLS	0.0771**	0.0079	0.0023	0.0010	0.0109	0.0509*
	(0.0188)	(0.0207)	(0.0089)	(0.0290)	(0.0106)	(0.0245)
R^2	0.107	0.063	0.024	0.050	0.058	0.083
Program participant (τ), HH FE	0.0780***	0.0205	0.0071	-0.0021	0.0056	0.0531*
	(0.0154)	(0.0111)	(0.0088)	(0.0242)	(0.0052)	(0.0218)
R^2	0.037	0.030	0.045	0.015	0.015	0.036
Baseline, Untreated	*0.055*	*0.016*	*0.020*	*0.042*	*0.037*	*0.102*
Observations	**809**	**1,656**	**1,390**	**1,591**	**3,050**	**1,458**

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, status as oldest child, number of siblings, mother's and father's education, mother's education missing, and father's education missing.

Table 6: OLS Fixed-Effect Estimates for Total Years of Schooling by Country and Gender

VARIABLES			BOYS			
TOTAL YEARS OF EDUCATION	***Uganda***	***Guatemala***	***Philippines***	***India***	***Kenya***	***Bolivia***
Program participant (τ)	1.463**	0.496	0.706***	0.671**	1.486***	0.481***
	(0.518)	(0.361)	(0.041)	(0.229)	(0.189)	(0.104)
Observations	385	848	697	795	1,569	730
R^2	0.169	0.141	0.071	0.064	0.069	0.134
Baseline, Untreated	*8.39*	*8.48*	*11.86*	*11.24*	*10.29*	*10.55*
			GIRLS			
TOTAL YEARS OF EDUCATION	***Uganda***	***Guatemala***	***Philippines***	***India***	***Kenya***	***Bolivia***
Program participant (τ)	2.735***	1.190***	0.395***	0.423	0.935***	0.695
	(0.405)	(0.173)	(0.053)	(0.328)	(0.124)	(0.517)
Observations	424	808	693	796	1,481	728
R^2	0.163	0.214	0.154	0.110	0.081	0.165
Baseline, Untreated	*8.35*	*7.74*	*12.37*	*11.66*	*10.14*	*10.08*

*** Significant at 1%; ** significant at 5%; * significant at 10%. Estimations include fixed-effects at the household level. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, and status as oldest child.

APPENDIX

Table A.1: Summary Means and T-tests of Employment and Leadership Variables

	Mean, Sponsored individuals (std. dev.)	Mean, All non-sponsored individuals (std. dev.)	Difference *t*-test (std. error)	Mean, Non-sponsored siblings in sponsored households	Difference *t*-test (std. error)
Formally Employed	0.423	0.359	0.064***	0.341	0.082***
	(0.494)	(0.480)	(0.013)	(0.474)	(0.014)
White-Collar Employed	0.274	0.187	0.086***	0.180	0.094***
	(0.446)	(0.390)	(0.011)	(0.384)	(0.012)
Community Leader	0.046	0.029	0.017***	0.026	0.020***
	(0.209)	(0.168)	(0.0047)	(0.159)	(0.005)
Church Leader	0.161	0.087	0.073***	0.095	0.065***
	(0.367)	(0.282)	(0.0079)	(0.293)	(0.0094)

Full sample size = 10,144. Number of formerly sponsored individuals = 1,860. Number of individuals in non-treated households = 4,580. Number of non-sponsored individuals in treated households = 3,704.

Table A.2: OLS and IV Estimates of Equations (1), (2), (3), and (4)
Formal Employment

VARIABLES	OLS	IV-GMM	OLS-FE	IV-GMM-FE
Program participant (τ)	0.0505*	0.1869***	0.0628***	0.2888***
	(0.0264)	(0.0667)	(0.0148)	(0.0712)
Compassion hh: ACI ≤ 12 (α_1)	0.1399	0.0405	0.1149***	0.0009
	(0.0887)	(0.0698)	(0.0387)	(0.0451)
Compassion hh: ACI 13-16 (α_2)	0.0425	0.0434	0.0458	0.0368
	(0.0542)	(0.0406)	(0.0313)	(0.0230)
Program village ACI ≤ 12 (β_1)	0.1197	0.0822	0.1517***	0.1521***
	(0.0992)	(0.0621)	(0.0412)	(0.0368)
Program village ACI 13-16 (β_2)	0.1021	0.0859**	0.1078**	0.1230***
	(0.0688)	(0.0427)	(0.0466)	(0.0387)
Nonprog. village ACI ≤ 12 (γ_1)	-0.0169	-0.0385	0.0867	0.0889
	(0.1002)	(0.0710)	(0.0646)	(0.0608)
Nonprog. village ACI 13-16 (γ_2)	-0.1111*	-0.1235***	-0.0261	-0.0208
	(0.0582)	(0.0421)	(0.0497)	(0.0481)
F-tests/χ^2 tests of parallel trends:				
$\alpha_2 = \beta_2 = \gamma_2$	9.23***	23.06***	2.40	7.05**
	[0.001]	[0.000]	[0.108]	[0.030]
$\alpha_1 = \beta_1 = \gamma_1$	3.02*	2.91	0.66	11.89***
	[0.063]	[0.234]	[0.522]	0.003
Intra-Household Spillovers (α_1-α_2) - (γ_1-γ_2)	0.0033	-0.0879*	-0.4360	-0.1456***
	(0.0458)	(0.0477)	(0.0370)	(0.0480)
Intra-Village Spillovers (β_1-β_2) - (γ_1-γ_2)	-0.0765	-0.0886*	-0.0688	-0.0807**
	(0.0583)	(0.0463)	(0.0466)	(0.0381)
Program Impact including household Spillovers τ +(α_1-α_2) - (γ_1-γ_2)	0.0538	0.0990*	0.0192	0.1431***
	(0.0545)	(0.0557)	(0.0384)	[0.0467]
Hausman Test [*p*-value]	--	[0.255]	--	[0.073]
Over-identification (J-test) [*p*-value]	--	3.19	--	7.86
		[0.867]		[0.447]
Weak IV Test (*F*-statistic)	--	75.31	--	50.09
Observations	9497	9497	9,497	9,497
R-squared	0.124	0.115	0.073	0.036

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, status as oldest child, mother's education, father's education, mother's educ. missing and father's educ. missing.

Table A.3: OLS and IV Estimates of Equations (1), (2), (3), and (4)
White-Collar Employment

VARIABLES	OLS	IV-GMM	OLS-FE	IV-GMM-FE
Program participant (τ)	0.0654***	0.0638	0.0665***	0.0891*
	(0.0224)	(0.0571)	(0.0157)	(0.0509)
Compassion hh: ACI ≤ 12 (α_1)	0.0525	0.0657	0.0837***	0.0870**
	(0.0394)	(0.0438)	(0.0301)	(0.0384)
Compassion hh: ACI 13-16 (α_2)	0.0113	0.0097	0.0197	0.0201
	(0.0283)	(0.0203)	(0.0223)	(0.0182)
Program village ACI ≤ 12 (β_1)	0.1098**	0.1408***	0.1167**	0.1252***
	(0.0487)	(0.0300)	(0.0447)	(0.0329)
Program village ACI 13-16 (β_2)	0.0949**	0.0797**	0.1161**	0.1180***
	(0.0420)	(0.0336)	(0.0568)	(0.0460)
Nonprog. village ACI ≤ 12 (γ_1)	0.0167	0.0633	0.0694	0.0931**
	(0.0591)	(0.0457)	(0.0452)	(0.0423)
Nonprog. village ACI 13-16 (γ_2)	-0.0204	0.0165	0.0017	0.0311
	(0.0496)	(0.0423)	(0.0414)	(0.0392)
F-tests/χ^2 tests of parallel trends:				
$\alpha_2 = \beta_2 = \gamma_2$	1.77	2.69	1.71	5.09*
	[0.188]	[0.261]	[0.197]	[0.078]
$\alpha_1 = \beta_1 = \gamma_1$	2.87*	7.29**	0.43	0.89
	[0.072]	[0.026]	[0.657]	[0.640]
Intra-Household Spillovers	0.0041	0.0092	-0.0037	0.0050
$(\alpha_1-\alpha_2) - (\gamma_1-\gamma_2)$	(0.0316)	(0.0374)	(0.0230)	(0.0362)
Intra-Village Spillovers	-0.0221	0.0143	-0.0670	-0.0547*
$(\beta_1-\beta_2) - (\gamma_1-\gamma_2)$	(0.0503)	(0.0420)	(0.0418)	(0.0301)
Program Impact	0.0695*	0.0730*	0.0627**	0.0941***
including household Spillovers	(0.0360)	(0.0439)	(0.0267)	(0.0276)
$\tau + (\alpha_1-\alpha_2) - (\gamma_1-\gamma_2)$				
Hausman Test [*p*-value]	--	[0.688]	--	[0.664]
Over-identification (J-test) [*p*-value]	--	9.90	--	12.61
		[0.195]		[0.126]
Weak IV Test	--	68.56	--	41.08
(*F*-statistic)				
Observations	9596	9596	9,596	9,596
R-squared	0.091	0.089	0.027	0.026

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, status as oldest child, mother's education, father's education, mother's educ. missing and father's educ. missing.

Table A4: Impacts on Vocation: Marginal Effects, Multinomial Logit Estimates

Occupational Category	Six Countries MN Logit Coefficients	Six Countries Marginal Effects	Mean Untreated	Occupational Category	Six Countries MN Logit Coefficients	Six Countries Marginal Effects	Mean Untreated
1 Agriculture	0.532**	0.0164	***0.051***	8 Small Business	0.0316	-0.0096	***0.049***
	(0.217)	(0.0147)			(0.144)	(0.0077)	
2 Construction, Day Labor	1.085***	0.0147*	***0.024***	9 Ministry, Pastoral	0.567	0.0023	***0.007***
	(0.406)	(0.0088)			(0.444)	(0.0032)	
3 Clerical, Sales	0.623**	0.0232	***0.050***	10 Finance and Large Business	0.574**	0.0106	***0.029***
	(0.250)	(0.0142)			(0.268)	(0.0083)	
4 Blue Collar	0.457**	0.0141	***0.069***	11 Police, Army, Security, Fire	0.963***	0.0084**	***0.011***
	(0.198)	(0.0110)			(0.307)	(0.0040)	
5 Personal Services	0.314	0.0034	***0.052***	12 Professional, Doctor., Lawyer	-0.078	-0.0052	***0.019***
	(0.209)	(0.0106)			(0.353)	(0.0049)	
6 Teaching	0.808***	0.0341***	***0.054***	13 Less-Skilled Tech, Call Centers	0.636**	0.0086	***0.020***
	(0.160)	(0.0120)			(0.294)	(0.0067)	
7 Government	0.897*	0.0059	***0.007***	14 Nursing, Public Health, Hospital	0.505	0.0026	***0.011***
	(0.509)	(0.0048)			(0.322)	(0.0032)	

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Marginal effects, *dy/dx*, are from corresponding multinomial logit estimates; control variables are gender, age, age^2, birth order, oldest child and a dummy variable for household with sponsored child. The base category is unemployment. Number of observations = 8,911. Psuedo R^2 = 0.0470, Chi-squared $p < 0.0001$.

Table A.5: OLS and IV Estimates of Equations (1), (2), (3), and (4)
Community Leader

VARIABLES	OLS	IV-GMM	OLS-FE	IV-GMM-FE
Program participant (τ)	0.0218***	0.0007	0.0093**	-0.0461***
	(0.0063)	(0.0149)	(0.0041)	(0.0133)
Compassion hh: ACI ≤ 12 (α_1)	0.0373*	0.0501***	0.0351**	0.0623***
	(0.0214)	(0.0185)	(0.0169)	(0.0157)
Compassion hh: ACI 13-16 (α_2)	0.0226	0.0327***	0.0230	0.0210***
	(0.0155)	(0.0103)	(0.0146)	(0.0076)
Program village ACI ≤ 12 (β_1)	0.0534*	0.0577**	0.0413**	0.0419***
	(0.0292)	(0.0247)	(0.0171)	(0.0121)
Program village ACI 13-16 (β_2)	0.0567**	0.0736***	0.0493**	0.0474***
	(0.0268)	(0.0239)	(0.0192)	(0.0121)
Nonprog. village ACI ≤ 12 (γ_1)	-0.0147	-0.0138	0.0121	0.0120
	(0.0430)	(0.0411)	(0.0201)	(0.0186)
Nonprog. village ACI 13-16 (γ_2)	-0.0306	-0.0323	0.0117	0.0104
	(0.0353)	(0.0337)	(0.0237)	(0.0216)
F-tests/χ^2 tests of parallel trends:				
$\alpha_2 = \beta_2 = \gamma_2$	2.36	8.32**	1.24	7.91**
	[0.111]	[0.016]	[0.304]	[0.019]
$\alpha_1 = \beta_1 = \gamma_1$	1.39	3.28	0.94	6.42**
	[0.263]	[0.194]	[0.401]	[0.040]
Intra-Household Spillovers (α_1-α_2) - (γ_1-γ_2)	-0.0012	-0.0011	0.0118	0.0397**
	(0.0155)	(0.0156)	(0.0197)	(0.0186)
Intra-Village Spillovers (β_1-β_2) - (γ_1-γ_2)	-0.0191	-0.0343*	-0.0083	-0.0071
	(0.0242)	(0.0179)	(0.0259)	(0.0162)
Program Impact including household Spillovers τ +(α_1-α_2) - (γ_1-γ_2)	0.0206	-0.0004	0.0212	-0.0065
	(0.0157)	(0.0146)	(0.0197)	(0.0158)
Hausman Test [*p*-value]	--	0.493	--	0.091
Over-identification (J-test) [*p*-value]	--	10.84	--	3.03
		[0.146]		[0.933]
Weak IV Test (*F*-statistic)	--	61.79	--	40.87
Observations	9,495	9,495	9,495	9,495
R-squared	0.017	0.015	0.018	0.002

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, status as oldest child, mother's education, father's education, mother's educ. missing and father's educ. missing.

Table A.6: OLS and IV Estimates of Equations (1), (2), (3), and (4)
Church Leader

VARIABLES	OLS	IV-GMM	OLS-FE	IV-GMM-FE
Program participant (τ)	0.0603***	-0.0642*	0.0352***	-0.0715
	(0.0142)	(0.0374)	(0.0113)	(0.0452)
Compassion hh: ACI ≤ 12 (α_1)	-0.0453	-0.0109	-0.0076	0.0443
	(0.0338)	(0.0198)	(0.0238)	(0.0322)
Compassion hh: ACI 13-16 (α_2)	-0.0384*	-0.0508***	-0.0113	-0.0155
	(0.0223)	(0.0119)	(0.0198)	(0.0152)
Program village ACI ≤ 12 (β_1)	-0.0548	-0.0829*	-0.0271	-0.0148
	(0.0764)	(0.0427)	(0.0514)	(0.0377)
Program village ACI 13-16 (β_2)	-0.0148	-0.0407*	0.0327	0.0088
	(0.0418)	(0.0232)	(0.0321)	(0.0256)
Nonprog. village ACI ≤ 12 (γ_1)	-0.0501	-0.0680*	-0.0352	-0.0219
	(0.0463)	(0.0368)	(0.0248)	(0.0244)
Nonprog. village ACI 13-16 (γ_2)	-0.0522	-0.0669**	-0.0213	-0.0171
	(0.0405)	(0.0341)	(0.0212)	(0.0208)
F-tests/χ^2 tests of parallel trends:				
$\alpha_2 = \beta_2 = \gamma_2$	0.44	0.51	1.49	1.05
	[0.645]	[0.776]	[0.241]	[0.592]
$\alpha_1 = \beta_1 = \gamma_1$	0.02	7.57**	0.77	3.97
	[0.980]	[0.023]	[0.470]	[0.137]
Intra-Household Spillovers	-0.0090	0.0410*	0.0176	0.0647**
(α_1-α_2) - (γ_1-γ_2)	(0.0260)	(0.0242)	(0.0175)	(0.0303)
Intra-Village Spillovers	-0.0421	-0.0411	-0.0459	-0.0187
(β_1-β_2) - (γ_1-γ_2)	(0.0420)	(0.0333)	(0.0321)	(0.0219)
Program Impact	0.0513*	-0.0231	0.0528***	-0.0068
including household Spillovers	(0.0266)	(0.0275)	(0.0191)	(0.0239)
τ +(α_1-α_2) - (γ_1-γ_2)				
Hausman Test [*p*-value]	--	[0.007]	--	[0.144]
Over-identification (J-test) [*p*-value]	--	4.59	--	9.31
		[0.710]		[0.317]
Weak IV Test	--	63.46	--	40.18
(*F*-statistic)				
Observations	9,468	9,468	9,468	9,468
R-squared	0.018	0.001	0.005	0.056

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, status as oldest child, mother's education, father's education, mother's educ. missing and father's educ. missing.

Table A7: OLS and OLS Fixed Effect Estimates for Employment and Leadership by Country

VARIABLES						
EMPLOYED WITH SALARY	***Uganda***	***Guatemala***	***Philippines***	***India***	***Kenya***	***Bolivia***
Program participant (τ)	0.048	0.133**	0.089***	0.143**	0.039**	0.025
	(0.031)	(0.045)	(0.011)	(0.040)	(0.015)	(0.040)
R^2	0.192	0.137	0.136	0.313	0.049	0.134
Program participant (τ), HH FE	0.042	0.081	0.086***	0.145**	0.047**	0.017
	(0.025)	(0.063)	(0.014)	(0.037)	(0.018)	(0.039)
R^2	0.157	0.091	0.101	0.319	0.029	0.085
Observations	798	1,503	1,341	1,552	2,942	1,361
Baseline, Untreated	*0.544*	*0.281*	*0.425*	*0.646*	*0.136*	*0.353*
WHITE-COLLAR EMPLOYED	***Uganda***	***Guatemala***	***Philippines***	***India***	***Kenya***	***Bolivia***
Program participant (τ)	0.099	0.113***	0.139***	0.065***	0.027***	0.047
	(0.055)	(0.004)	(0.020)	(0.008)	(0.004)	(0.038)
R^2	0.104	0.160	0.093	0.074	0.048	0.065
Program participant (τ), HH FE	0.095	0.122***	0.128***	0.068**	0.025**	0.038
	(0.052)	(0.011)	(0.014)	(0.015)	(0.009)	(0.034)
R^2	0.066	0.104	0.062	0.039	0.018	0.041
Observations	809	1,506	1,376	1,591	2,942	1,372
Baseline, Untreated	*0.184*	*0.182*	*0.298*	*0.362*	*0.056*	*0.149*
COMMUNITY LEADER	***Uganda***	***Guatemala***	***Philippines***	***India***	***Kenya***	***Bolivia***
Program participant (τ)	0.035	0.035	0.024***	0.016	0.0070*	0.023*
	(0.029)	(0.023)	(0.004)	(0.008)	(0.0035)	(0.010)
R^2	0.101	0.036	0.026	0.037	0.019	0.072
Program participant (τ), HH FE	0.043*	0.009	-0.0048*	0.005	0.001	0.021
	(0.018)	(0.009)	(0.0020)	(0.003)	(0.008)	(0.011)
R^2	0.091	0.011	0.013	0.018	0.017	0.049
Observations	793	1,487	1,359	1,543	2,942	1,371
Baseline, Untreated	*0.085*	*0.016*	*0.019*	*0.021*	*0.016*	*0.057*
CHURCH LEADER	***Uganda***	***Guatemala***	***Philippines***	***India***	***Kenya***	***Bolivia***
Program participant (τ)	0.169***	0.054	0.076	-0.020	0.063**	0.027
	(0.032)	(0.040)	(0.038)	(0.012)	(0.020)	(0.028)
R^2	0.080	0.071	0.054	0.036	0.021	0.037
Program participant (τ), HH FE	0.112*	0.015	0.048**	0.001	0.038**	0.011
	(0.052)	(0.050)	(0.015)	(0.014)	(0.015)	(0.036)
R^2	0.040	0.024	0.024	0.011	0.009	0.014
Observations	773	1,491	1,354	1,542	2,942	1,366
Baseline, Untreated	*0.119*	*0.138*	*0.086*	*0.097*	*0.061*	*0.062*

*** Significant at 1%; ** significant at 5%; * significant at 10%. Clustered standard errors at village level in parentheses. Each regression includes controls for age at program introduction (ACI) at age 12 and below and age 13-16, age, age-squared, birth order, gender, status as oldest child, number of siblings, mother's and father's education, mother's education missing, and father's education missing. Fixed-effects are at the household level.