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Does Child Sponsorship Pay Off in Adulthood? An International Study of Impacts on Income and Wealth

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Abstract: We estimate the impact of international child sponsorship on adult income and wealth of formally sponsored children using data on 10,144 individuals in six countries. To identify causal effects, we utilize an age-eligibility rule followed from 1980 to 1992 that limited sponsorship to children 12 years old or younger when the program was introduced in a village, allowing comparisons of sponsored children with older siblings who were slightly too old to be sponsored. Estimations indicate that international child sponsorship increased monthly income by $13-19 over an untreated baseline of $75, principally from inducing higher future labor market participation. We also find strong evidence for positive impacts on dwelling quality in adulthood, and modest evidence of impacts on adult ownership of consumer durables, limited to increased ownership of mobile phones.

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1. Introduction

Millions of households in wealthy countries support non-profit organizations whose aim is to alleviate poverty in the developing world. But only recently has a growing body of research in development economics begun to rigorously evaluate the impact of these programs on their intended beneficiaries.[1] International child sponsorship is one of the most popular approaches taken by ordinary households in wealthy countries to help the poor overseas. We estimate that there are currently 9.14 million sponsored children in the world today, the vast majority of whom are sponsored by ordinary households in wealthy countries.[2] Donors typically contribute $25-40 per month to sponsor a child. In many cases the organization uses these funds to provide school uniforms, tuition, nutritious meals, and programming that directly benefits sponsored children. Other types of sponsorship programs pool funds to invest in programming and infrastructure that benefits children in the community more broadly.[3]

For many individuals, child sponsorship represents their most direct contact with the poor in developing countries. Donors are drawn to child sponsorship because of the personalization of the relationship between sponsor and child. But whether child sponsorship actually benefits sponsored children has remained an open question. In Wydick, Glewwe, and Rutledge (2013), a companion paper to this research, we find international child sponsorship to have a statistically significant and positive effect on educational outcomes in all six survey countries (Bolivia, Guatemala, Kenya, Uganda, India, and the Philippines). Sponsorship during childhood increased the probability of secondary school completion by 12-18 percentage points over a 44.5% baseline, and increased completed years of schooling by 1.03-1.45 years. Sponsorship also increased adult white-collar employment by 6.5 percentage points over an 18.5% baseline as well as the probability of being a community leader.

Previous research has studied the impacts of various programs on children's persistence in school in developing countries. Examples include Drèze and Kingdon (2001) and Kremer and Vermeersch (2004) who find positive impacts of school meal programs on school

[1] See for example Cristia et al. (2012) evaluating the One Laptop Per Child Program, Rawlins et al. (2014) on the nutritional impacts of dairy cows and meat goats donated via the Heifer Project, and the analysis of Blattman et al. (2012) on cash transfers.

[2] We estimate this figure based on comprehensive internet search across multiple languages for sponsorship programs. For details on how the 9.12 million figure was compiled, see Wydick, Glewwe, and Rutledge (2013).

[3] Of the top ten child sponsorship organizations, a more direct child-centered approach is taken by Compassion International, ChildFund, Children International, CFCA, and Bornefonden. The community-centered approach is favored by World Vision, Plan International, Kindernothilfe, Save the Children, and SOS Children's Villages.

attendance in India and Kenya, respectively. In a randomized trial, Evans, Kremer and Ngatia (2008) find a nearly 40% reduction in absenteeism from the random provision of free school uniforms, while Kremer, Miguel, and Thornton (2009) estimate that a merit scholarship program for girls boosted attendance by 5 percentage points. Aside from our research, the only other investigation specifically into the impact of international child sponsorship is Kremer, Moulin, and Namunyu (2003). Here the authors assess the impact of a Dutch child-sponsorship program, finding that even a low-cost program focused on the provision of school uniforms and textbooks to each child caused sponsored children to advance a third of a grade farther in schooling completion.

In this paper we present results for the impacts of child sponsorship on the adult income and wealth of children sponsored through one of the leading international child sponsorship organizations. An understanding of these impacts is important for the millions of individuals in wealthy countries involved in international child sponsorship, individuals who are likely to view their contributions as an investment in these overseas children that yields tangible economic returns in their adult future. But it is also important for countries implementing similar programs that work directly with impoverished children, helping us to understand if direct investments in child development are financially sustainable by virtue of the positive impacts on future income of beneficiaries. Thus we ask the question: Does international child sponsorship pay off for children in adulthood?

2. Methodology

Fieldwork for our six-country study took place from 2008 to 2010. We obtained initial enrollment lists from village projects that were rolled out from 1980 to 1992 by Compassion International, the world's third largest sponsorship organization, which currently sponsors over 1.3 million children in 26 countries. Compassion uses its funding to provide tuition fees for children, several nutritious meals per week, basic healthcare, and an after-school tutoring program. The tutoring program not only helps children with homework and gives them some additional academic instruction, but emphasizes spiritual and character formation and the development of life aspirations in sponsored children.

Compassion's child sponsorship program is a very intensive intervention in the lives of impoverished children. Children typically begin sponsorship at age 4-6 and continue into their mid-teens. Many sponsored children attend retreats together with program staff that focus on the nurture of their values and aspirations. Since even in a typical week children typically spend about 8-10 hours per week after school and on Saturdays participating in the program, and because the average duration of sponsorship is 9.3 years, this means that during the course of their childhood, on average sponsored children spend slightly more than 4,000 hours participating in Compassion programming.

Through the use of local enumerators, we were able to locate 93.5% of the families of these formerly sponsored children, who by the time of the survey were aged 17 to 43. Our field personnel were unaffiliated with Compassion, so as to reduce bias in the responses of our subjects. We administered our survey first-hand to households pertaining to formerly sponsored children, a random sample of non-participating households in 19 program villages, and a random sample of households in 13 neighboring, non-program villages. The survey questionnaire was administered to family members (typically parents or adult siblings) present at the time of the survey, where survey questions obtained data from all grown siblings in the household cohort. The same questionnaire was administered similarly to collect data from non-sponsored siblings of sponsored children. We also administered the survey to 50-75 randomly selected households with children in a similar cohort age that did not participate in the program in program villages and 50-75 randomly selected households with children in a similar cohort age in nearby non-program villages.

Overall, our data contain information on educational and vocational outcomes, monthly wages, consumer good ownership, and dwelling quality on 1,860 formerly sponsored children, 3,704 of their unsponsored siblings, 2,136 individuals of a similar age from non-participating families in villages where the Compassion program operated, and 2,444 individuals from similar, nearby villages without the Compassion program.

There are several empirical challenges to estimating the program's causal effects on future income and wealth. First, there may be non-random selection of households with eligible children into the program. Second, since a limited number of children per household were eligible for sponsorship (ranging from one in the African countries to three in the Latin American countries), intra-household selection of children for sponsorship may not be random. Third, there may be spillover effects from sponsored children onto their siblings, or onto other children in the village, which complicates the estimation. Lastly, when estimating impacts on future wages, impacts on employment must be separated from impacts on wages, conditional on employment.

To identify causal effects of child sponsorship, we use a program age-eligibility requirement, which stipulated that only children 12 and under could enter the program in any year, including the program's first year in a village. Figure 1 shows the program's strong adherence to this rule. Because a child's age at the time of program rollout in his or her village is independent of adult life outcomes, except via its impact on program participation, the age-eligibility rule can be an instrumental variable that allows one to account (and test) for non-random intra-household selection of children for sponsorship. To address possible endogenous household selection into the program, we employ household fixed effects, which control for unobserved differences in parenting behavior and household environments.[4] Implicitly, this compares life outcomes of children who were age-eligible for sponsorship with their siblings who were too old for sponsorship when the program arrived in their village.

The regression estimates allow for the possibility of spillovers. Dummy variables are included for: (a) Sponsored children, who were 12 or younger when the program started in their villages (denoted by $T = 1$); (b) Program participants' siblings who were 12 or younger when the program began in their villages and, while eligible, were not selected for sponsorship (denoted $D_1^{\leq 12} = 1$); (c) Program participants' siblings age 13-16 when the program arrived in their villages, and thus were ineligible ($D_1^{13-16} = 1$); (d) Individuals in non-Compassion households in program villages who were 12 or younger at program introduction ($D_2^{\leq 12} = 1$); (e) Individuals in non-Compassion households in program villages age 13-16 at program introduction ($D_2^{13-16} = 1$); (f) Individuals 12 or younger in non-Compassion villages when the program started in a neighboring village ($D_3^{\leq 12} = 1$); and (g) Individuals 13-16 in non-Compassion villages when the program began in a neighboring village ($D_3^{13-16} = 1$). Individuals 17 or older in non-program villages are the omitted category.

The household fixed-effects equation for child i in household j is

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

where y_{ij} measures income or wealth, $\boldsymbol{X_{ij}}$ is a vector of controls including age, gender, birth order and oldest child, and θ_j is a household fixed effect. Assuming that spillovers: (a) occur only within villages; and (b) affect age-eligible, but not age-ineligible, siblings of sponsored children, then $[\alpha_1 - \alpha_2] - [\gamma_1 - \gamma_2]$ captures spillovers from sponsored children onto non-

[4] We found that program staff usually selected households for participation, and then parents chose which children to be sponsored.

sponsored siblings age 12 and younger and $[\beta_1 - \beta_2] - [\gamma_1 - \gamma_2]$ captures spillovers onto age-eligible children in non-Compassion households in program villages.[5]

To address possibly endogenous child selection within families we use instrumental variable estimation. The oldest age-eligible sibling was sponsored most often, followed by the second-oldest age-eligible sibling, third oldest, etc., so the instruments are interaction terms between three age-at-program-rollout categories (4 years and under, 5-8, 9-12) and dummy variables for oldest age-eligible sibling, second-oldest age-eligible sibling, and younger age-eligible siblings, yielding a vector of nine instruments.[6] In the first stage, the probability of sponsorship, $\hat{T}_i$, is estimated using these instruments and controls; $\hat{T}_i$ replaces the treatment variable (T_i) in (1) in a second-stage regression.

Estimating the impact of sponsorship on monthly wages involves another challenge: wages are unobserved for the 61% in the sample who were not employed. This situation suggests the use of Heckman (1979) estimation for the wage impact regressions, which uses a probit employment equation to generate an Inverse Mills ratio for each observation in a second-stage wage regression. Given certain assumptions, this removes bias from the censored wage variable (the dependent variable in second equation). This two-equation system is then estimated using maximum likelihood.

This approach allows one to decompose overall income impacts of child sponsorship into the impact from formerly sponsored children obtaining employment and the impact on wages conditional on employment. These two effects are seen by differentiating the expected average wage, $E(w)$, where

$$E(w) = \Phi(\boldsymbol{z}'\boldsymbol{\gamma}) \cdot w(\boldsymbol{x}'\boldsymbol{\beta}), \tag{2}$$

and $\Phi(z'\gamma)$ is the probability that an individual works and earns a wage, based on characteristics $\boldsymbol{z}$, and $w(\boldsymbol{x}'\boldsymbol{\beta})$ is the individual's wage, based on characteristics $\boldsymbol{x}$. To estimate $\boldsymbol{\beta}$ without assuming arbitrary functional forms, $\boldsymbol{z}$ should have one or more variables that are excluded from $\boldsymbol{x}$; we use the individual's number of children, which strongly affects employment but should not affect wages. Both $\boldsymbol{x}$ and $\boldsymbol{z}$ include the sponsorship (treatment) variable, T. Differentiating (2) with respect to T, and setting all variables to their means, gives

$$\frac{\partial E(w)}{\partial T} = \frac{\partial \Phi(z'\gamma)}{\partial T} \cdot w(\bar{\boldsymbol{x}}'\boldsymbol{\beta}) + \frac{\partial w}{\partial T} \cdot \Phi(\bar{\boldsymbol{z}}'\boldsymbol{\gamma}) \tag{3}$$

[5] Spillovers onto non-sponsored siblings may reflect extra income available from sponsorship, role model effects, and parental reallocation of assistance to non-sponsored children.

[6] An identical set of instruments are used in Wydick *et al.* (2013).

The first term gives the impact of sponsorship on income from its employment effect; the second is the impact of sponsorship on wages, conditional on employment.

Both terms in (3) are obtained using Heckman's method; $\Phi(z'\gamma)$ is estimated using a probit specification, and $w(\boldsymbol{x}'\boldsymbol{\beta})$ is essentially equation (1). To calculate the standard errors of the employment effect (the first term in (3)), a bootstrapping procedure is used; estimates of $\frac{\partial\Phi(z\prime\gamma)}{\partial T}$ and of the average wage are obtained from a random draw (with replacement) from the sample. These two estimates are multiplied for each bootstrap iteration and (household-level clustered) standard errors are obtained from 500 bootstrapped replications. Similarly, the impact of sponsorship on wages (the second term in (3)) is the product of the estimate of $\frac{\partial w}{\partial T}$ and mean labor market participation; for each bootstrapped sample (with replacement), the entire estimation procedure, which combines estimates of the probit equation with those of the wage equation, is implemented, and 500 replications are used to obtain correct standard errors.

For all 10,144 individuals in the study, interviewers attempted to obtain current monthly wages. For 83%, they or their family members reported whether they received a salary and, if so, the monthly salary. For the remaining 17% no one could provide salary data, but for nearly all of these individuals, family members knew their completed schooling and current occupation, if applicable. Using data on education, occupation, gender and age (and country fixed effects), wages were imputed for all individuals in the sample. Two estimates of income impacts from sponsorship were thus implemented; one drops the 17% of the sample without wage data, and the other imputes wage values on the full sample.[7] Assuming that any imputation errors are independent of the explanatory variables in equation (1), estimates using imputed values are consistent and unbiased (Wooldridge, 2010). To carry out extended estimations by country and gender we use the imputed wages, which yield slightly lower (yet more precise) wage impact estimates than do our directly reported wage data.

To examine the impact of child sponsorship on adult wealth, we examine two broad categories: indicators of current dwelling quality, and current ownership of common consumer goods. The dwelling quality measures include the presence of an indoor toilet, electricity, walls constructed of sturdy materials (e.g. wood, concrete, rather than mud or sticks), high quality roofs (constructed from tile, concrete, or high-quality wood, rather than thatch, leaves, or low-quality corrugated iron), and high quality floors (concrete, wood, or tile, rather than dirt floors

[7] Those whose imputed wages are less than or equal to zero are assigned non-working status.

or floors made from other natural materials). For the second wealth proxy, information was collected on ownership of mobile phones, bicycles, motorcycles, automobiles, and land.

To address issues of over-testing and joint-testing of related hypotheses, two types of indices were created. The first simply weights each of the five variables within a group equally; OLS and GMM IV estimations are then carried out on these simple indices. Secondly, for each of these two groups of variables we created an Anderson (2008) summary index. This index is created by de-meaning each of the dependent variables in the respective group j ($j \in$ dwelling, consumer goods), then weighting each observation by the sum of its row entries across the inverted variance-covariance matrix of the dependent variables in the group. Specifically, each observation i in group j receives a weight (index score) of $\bar{s}_{ij} = (\mathbf{1}'\Sigma^{-1}\mathbf{1})^{-1}(\mathbf{1}'\Sigma^{-1}y_{ij})$, where $\mathbf{1}$ is a m x 1 column vector of 1's, Σ^{-1} is the m x m inverted covariance matrix, and y_{ij} is the m x 1 vector of outcomes for individual i. Relative to the simple index, the Anderson Index gives more weight to dependent variables within the grouping that are least correlated with other variables, and hence embody the greatest degree of unique information.

3. Results

Table 1 provides summary statistics for the data. Monthly income is $16.67 higher among those who were sponsored as children ($p < 0.01$). This mainly reflects a higher employment rate (54.5% to 47.9%) among formerly sponsored individuals ($p < 0.01$). This is evident in Figure 2; conditional on positive wages, distributions of (log) income are nearly identical, but formerly sponsored individuals show dramatically more non-zero wages. Figure 3 illustrates the program's impact in a discontinuity diagram; non-parametric estimation shows that wages are generally higher for individuals in untreated (relative to treated) households among those 13 and older when the program began. However, among those 12 or younger when the program rolled out, income is higher in treated households.

A) Impact on Income

Heckman estimations are provided in Table 2 in columns (1) through (12). The first row of Table 2 gives marginal effects from selection into positive wage realizations from child sponsorship. Column (1) gives estimates without household fixed effects and omitting missing wage observations, column (2) adds household fixed effects, column (3) uses household fixed effects with imputed wages, and column (4) provides the bootstrapped IV-Heckman estimates. These columns yield estimated coefficients of 0.087, 0.079, 0.068, and 0.186 for the marginal effect of selection into employment. All are significant at $p < 0.01$ except the latter which is significant at $p < 0.05$. Hausman tests cannot reject the null hypothesis that the standard Heckman estimate is consistent (p = 0.136). The second row provides estimates of $\frac{\partial\Phi(z'\gamma)}{\partial T} \cdot w(\bar{\boldsymbol{x}}'\boldsymbol{\beta})$ in (3), the increased income from sponsorship via greater employment. These impacts range from \$12.81 per month in column (3) to 17.25 in column (1).

The third row provides selection-corrected estimates of $\frac{\partial w}{\partial T} \cdot \Phi(\bar{\boldsymbol{z}}'\boldsymbol{\gamma})$, the impact of sponsorship on wages conditional on employment. Only the \$6.06 estimate in column (3) is significant ($p < 0.05$); the others in columns 1-4 are insignificant, and two are negative. Over the whole sample there is weaker evidence that sponsorship raises incomes *conditional* upon employment, and the income impacts in the fourth row are insignificant except for the \$2.64 estimate ($p < 0.10$) in column (3). These estimates confirm the density functions in Figure 2—the main impact of child sponsorship on income is primarily via increased employment, rather than via increased wages among those already employed in the overall sample. Column 4 reports estimates from an instrumental variables (IV) estimation in which we regress treatment on our vector of instruments and controls in the first stage and then carry out the Heckman estimation in the second stage, bootstrapping clustered standard errors at the household level for the entire process with 500 replications. Our impact estimates with the bootstrapped IV-Heckman procedure increase to a monthly income impact of \$35.31 ($p < 0.05$), yet a Hausman test still fails to reject the efficiency of the standard Heckman estimate (t = 1.49).

In columns (5) and (6) we divide our standard Heckman estimations by gender, and find that while monthly wage effects from selection into employment are nearly identical (\$12.60 for boys, \$12.75 for girls, both $p < 0.01$), there is a positive impact on wages conditional on employment for boys (\$6.74, $p < 0.01$), but this effect is zero for girls. Thus sponsorship yields an increase in girls' future wages of \$12.75, resulting solely from greater labor market participation. But the total wage impact from sponsorship on boys' future wages is \$19.34-- \$12.60 from higher labor market participation and \$6.74 from higher wages conditional upon labor market participation.

Columns (7) through (12) in Table 2 show impacts of the employment selection effect from child sponsorship, replicating those in column (3) by country. Impacts are highest in India (\$37.61, $p < 0.01$), Guatemala (\$27.63, $p < 0.05$), and the Philippines (\$17.01, $p < 0.10$). And although estimates are positive in every country, they are moderate in Bolivia (\$8.19, but not statistically significant), and low and statistically insignificant in Uganda ($7.19) and Kenya.[8]

We test for wage spillovers onto unsponsored siblings and other children of eligible age within program villages children using a joint test of the significance of the linear combinations $[\alpha_1 - \alpha_2] - [\gamma_1 - \gamma_2]$ and $[\beta_1 - \beta_2] - [\gamma_1 - \gamma_2]$ in (2), but find no evidence of either positive or negative spillover effects in either case (p = 0.987, 0.195, respectively). Thus we conclude that the benefits of international sponsorship on adult income appear limited to the sponsored child.

Figure 4 presents non-parametric estimations of the wage trajectories of sponsored (upper line) and unsponsored (lower line) individuals; the impact of sponsorship appears to increase over time. While differences in wages are small in the twenties and thirties, they grow as individuals reach their forties. A similar non-parametric estimation in Figure 5 compares the wages of sponsored and non-sponsored children by maternal education. Wage differences are lowest for both children of mothers without formal education and children of highly educated mothers. The largest impact, about $25 per month, is among individuals whose mothers had completed only primary school.

B) Wealth Impacts

Finally, we consider the impact of child sponsorship on indicators of wealth in adulthood. Individuals who were sponsored as children live in better houses as adults. Both the simple index and the Anderson index indicate significant impacts of sponsorship on adult dwelling quality. Specifically, OLS (linear probability model) household fixed-effect estimates in Table 3 indicate that sponsorship increases the probability that a home has electricity by 2.9 percentage point, raises the probability of having improved walls by 2.5 percentage points, and increases having improved floors by 1.9 percentage points. GMM-IV estimates are smaller and insignificant for specific improvements, but larger and significant for both dwelling indices.

Does child sponsorship increase consumer durable ownership in adulthood? The only asset with a statistically significant effect is the probability of owning a mobile phone, an increase of 5.4 percentage points in OLS estimations and 18.3 percentage points in IV

[8] Other estimation models yield similar estimates including Ordinary Least Squares (OLS), which over a variety of specifications, listwise deletion, and wage imputation yields significant ($p < 0.01$) whole-sample estimates ranging from $16.60 to $19.05. While OLS captures both employment selection and marginal wage effects, it exhibits a downward bias because it omits the Inverse Mills ratio, included in the Heckman estimation, as a right-hand-side regressor. Tobit estimates, which are not preferred because they assume a homogeneous vector of explanatory variables for selection and marginal wage effects yield significant ($p < 0.01$) estimates between $12.53 and $24.51.

estimations (baseline of 76.8%). We find no evidence that sponsorship increased ownership of bicycles, motorcycles, vehicles, or land; coefficients on both wealth summaries are insignificant.

Tests for household and village level spillovers show no significant effects on aggregated dwelling indicators (p = 0.237 and p = 0.523, respectively) or consumer durables (p = 0.333 and p = 0.536). Although our research on educational impacts provides evidence for spillovers onto younger siblings, particularly in secondary school completion (Wydick, Glewwe, and Rutledge, 2013), we find no evidence of income or wealth spillovers in our data.

4. Conclusion

International child sponsorship is arguably the leading form of individual contact between ordinary people in developed countries with the poor in developing countries, yet little has been known about the impact of these programs on the economic outcomes in adulthood of sponsored children. Our more conservative Heckman estimates from a six-country study of 10,144 individuals show that child sponsorship is responsible for increases in monthly income of $13-19 over an unconditional baseline of $75, or an increase of 17.3-25.3%. This effect of child sponsorship on future wages is due principally to sponsored children entering the labor market as adults who would not have done so otherwise, particularly for girls. We find that boys realize an added $6.74 of monthly income from higher wages conditional upon employment, but that this added impact on wages to be estimated at zero for girls.

Given that the cost of sponsorship to sponsors was $28 per month during the time in the 1980s and 1990s when the individuals in our study were sponsored, and that the average length of sponsorship was 9.5 years, a monthly income increase of $13-19 over an average lifetime of work implies a modest financial rate of return to child sponsorship of 3.7-5.6%.

Our estimations of impacts on wealth in adulthood find significant impacts on adult dwelling quality from child sponsorship on proxies for adult wealth, where we find that sponsored children are more likely as adults to live in better housing, homes with electricity, and with walls and floor made of superior construction. Impacts on adult consumer good ownership, however, are more modest and appear to be limited to substantially greater ownership of mobile phones.

In other research we find that child sponsorship may improve adult incomes not merely through relieving *external* constraints that improve schooling access, nutrition, and health, but through addressing *internal* constraints related to imparting a greater level of hopefulness about the future and instilling greater aspirations for schooling and adult vocation. Work related to this research (Glewwe, Ross, and Wydick, 2013) on *currently* sponsored children finds a causal link between child sponsorship and elevated educational and vocational aspirations among children in Kenya, and finds higher levels of happiness, self-efficacy, and hopefulness based on a quantitative analysis of children's self-portrait drawings in Indonesia. Although it is yet impossible to definitively identify these increased aspirations as a causal channel to the positive impact from sponsorship on income and wealth we find in this study, what is clear is that child sponsorship increases aspirations and that child sponsorship also improves adult economic outcomes. Yet taken together, these results suggest that development programs that relieve tangible external constraints, while simultaneously addressing the internal constraints faced by many among the poor, may realize stronger impacts than programs that address external constraints alone, thus providing a basis for interesting and important future research.

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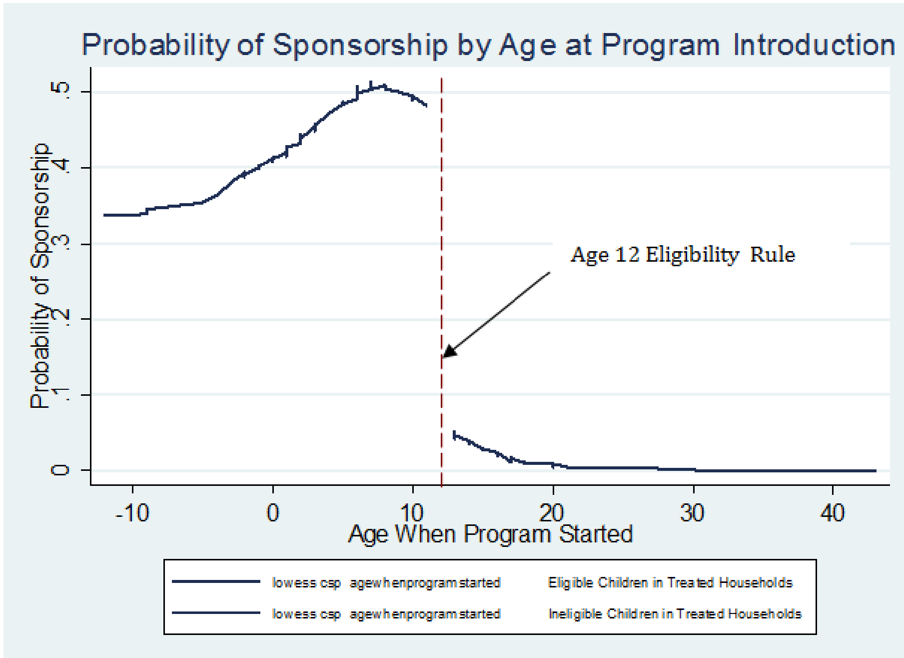

Figure 1: Sponsorship as a Function of Age When Program Started

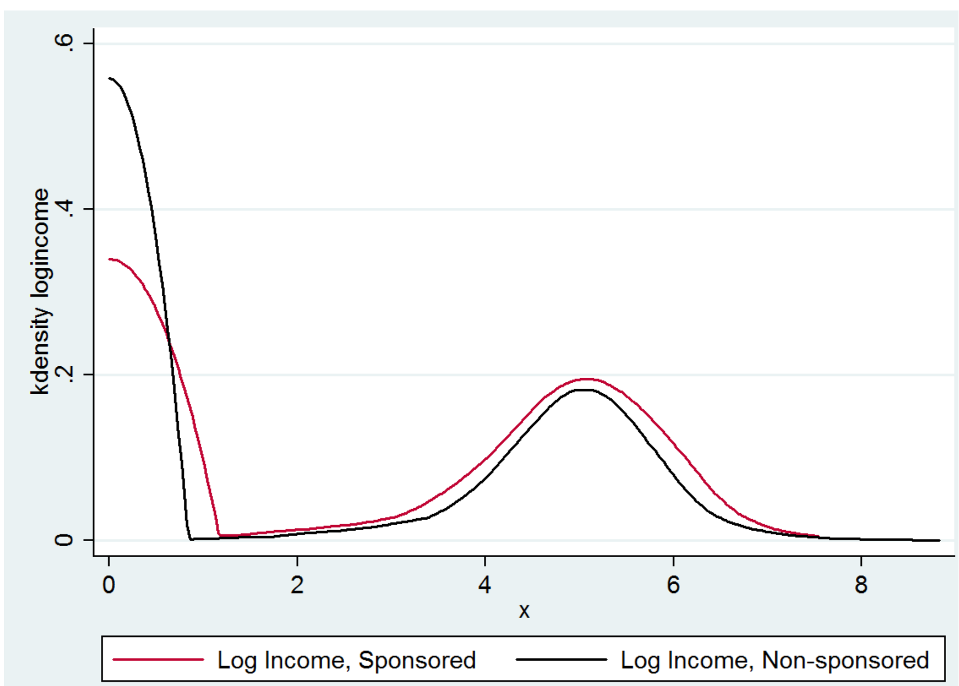

Figure 2: Differences in Log Income, Sponsored vs. Unsponsored

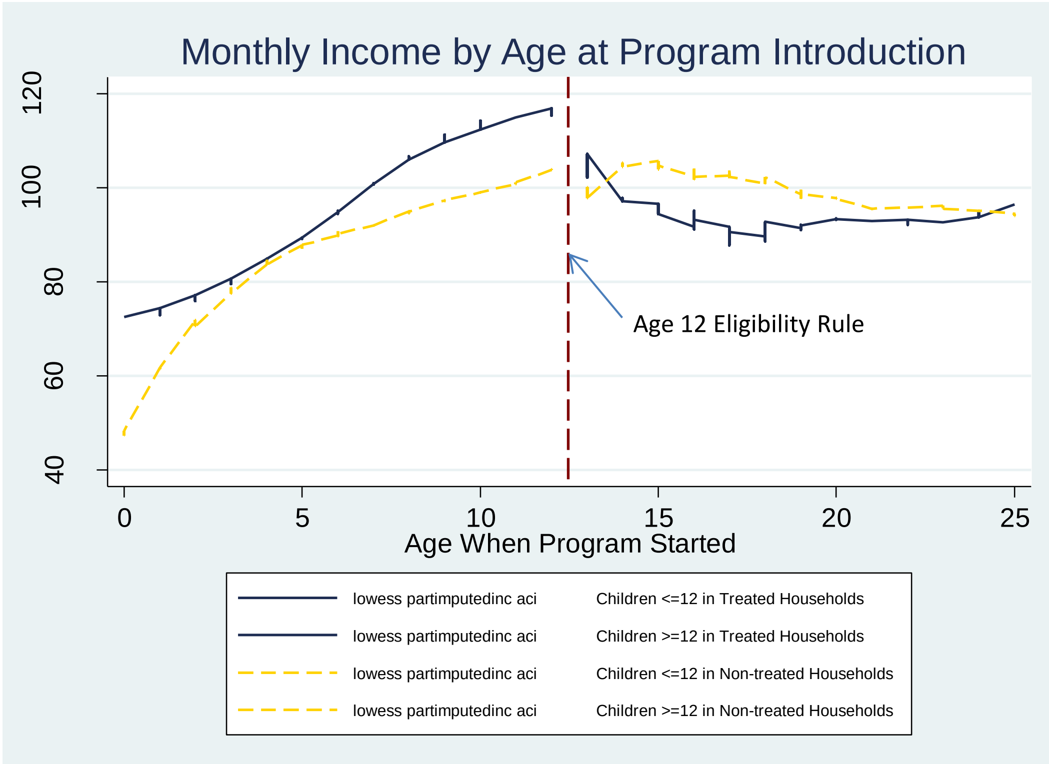

Figure 3: Monthly Income as a Function of Eligibility

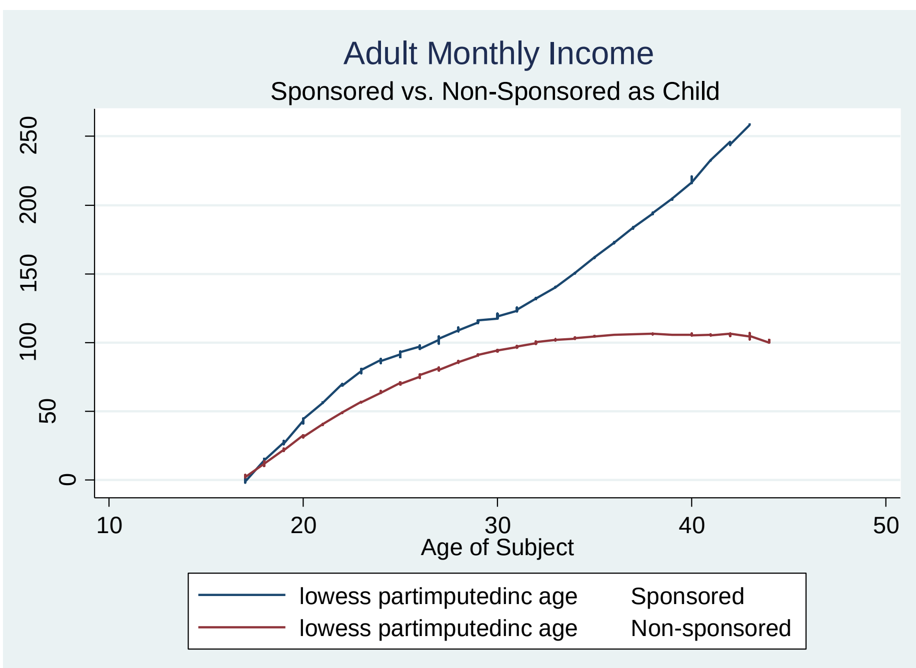

Figure 4: Growth in Wage Gap Over Time, Sponsored vs. Non-Sponsored (Bandwidth = 1.0)

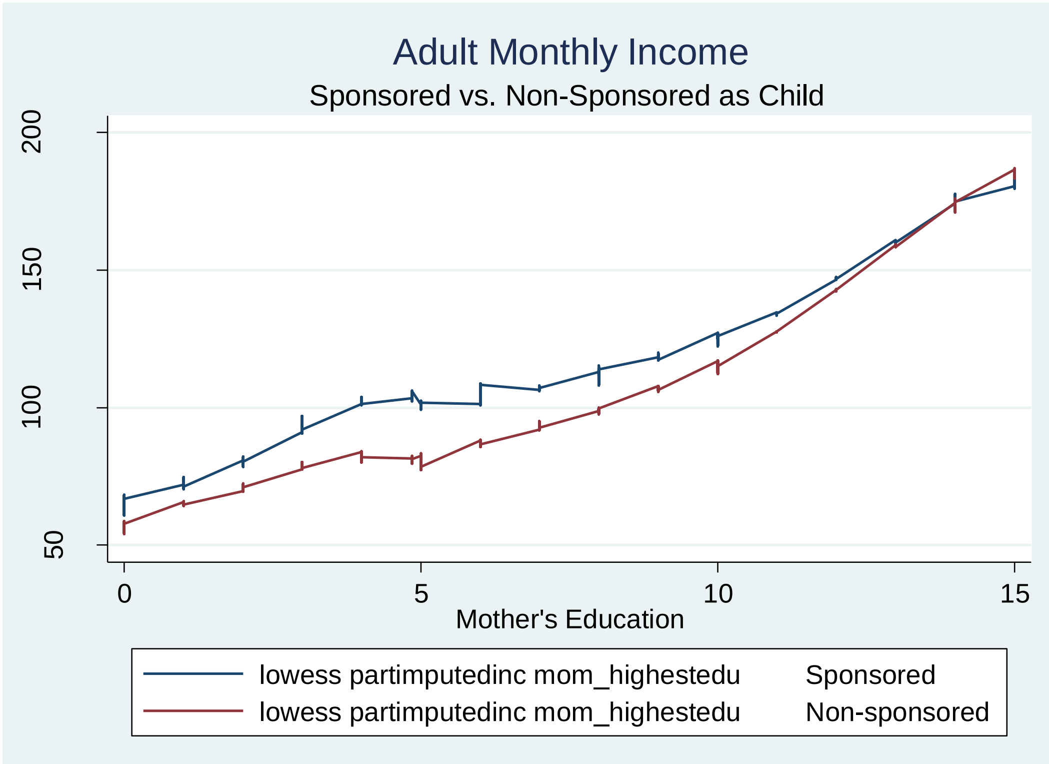

Figure 5: Wage Impact between Sponsored and Non-Sponsored by Mother's Education (Bandwidth = 0.5)

Table 1: Summary Statistics

Variable	Full Sample	Sponsored	Unsponsored	Difference	*p*-value
Age	29.82	30.56	26.51	4.05	0.000
Sex	0.504	0.481	0.509	-0.028	0.013
Birth Order	3.041	3.006	3.049	-0.043	0.389
Mothers Educ. (Years)	4.85	4.94	4.83	0.11	0.298
Uganda	0.080	0.102	0.075	0.027	0.001
Guatemala	0.167	0.192	0.162	0.030	0.002
Philippines	0.141	0.129	0.143	-0.014	0.122
India	0.159	0.119	0.168	-0.049	0.000
Kenya	0.304	0.296	0.306	-0.010	0.418
Bolivia	0.145	0.158	0.142	0.016	0.089
Working = 1	0.491	0.545	.479	0.066	0.000
Monthly Income ($US)	77.96	91.53	74.86	16.67	0.008
Monthly Income ($US), Working=1	198.13	194.25	199.24	-4.99	0.649
Monthly Income (Imputed, $US)	90.63	104.13	87.62	16.51	0.000
Monthly Income (Imp, $US), Working=1	170.25	177.48	168.43	9.05	0.009
Housing Quality Index (Simple)	2.81	2.88	2.80	0.08	0.0143
Consumer Good Index (Simple)	1.26	1.25	1.27	-0.02	0.378
Sample Size	**10,011**	**1,819**	**8,192**		

Table 2: Impact on Monthly Income: Heckman Estimates

	(1) Heckman Misg. Omt. No FE	(2) Heckman Misg. Omt. HH FE	(3) Heckman Obs. Impt. HH FE	(4) IV-Heckman Obs. Impt. HH FE	(5) Heckman All Obs Imp. HH FE--boys	(6) Heckman All Obs Imp. HH FE--girls
Heckman Selection $=\frac{\partial\Phi(z'\gamma)}{\partial T}$	0.096***	0.079***	0.068***	0.186**	0.063***	0.073***
	(0.017)	(0.0155)	(0.014)	(0.080)	(0.023)	(0.022)
Selection Impact on Income $=\frac{\partial\Phi(z'\gamma)}{\partial T}\cdot w(\bar{x}'\beta)$	**$17.25*** **	**$15.63*** **	**$12.81*** **	**$35.31** **	**$12.60*** **	**$12.75*** **
	(3.12)	(3.08)	(2.71)	(15.33)	(4.44)	(3.90)
Marginal wage Impact | *w* > 0 $=\frac{\partial w}{\partial T}$	-$1.17	-$5.39	$6.06**	$10.23	$12.90***	$0.43
	(9.85)	(10.79)	(3.10)	(15.21)	(4.14)	(4.62)
Marginal wage Impact on Income $=\frac{\partial w}{\partial T}\cdot\Phi(\bar{z}'\gamma)$	**-$0.46**	**-$2.12**	**$2.64***	**$4.46**	**$6.74*** **	**$0.15**
	(4.02)	(4.25)	(1.38)	(6.69)	(2.21)	(1.65)
Lambda	-18.85***	-56.12	-10.55***	-10.31	-5.41***	-35.9***
	(3.94)	(41.19)	(3.60)	(7.50)	(1.71)	(6.19)
Observations	8,389	8,389	10,004	10,004	5,048	4,956
Mean *w*, untreated	$74.86	$74.86	$74.86	$74.86	$100.70	$47.54
	(196.07)	(196.07)	(196.07)	(196.07)	(185.69)	(114.82)
Mean *w* | *w* > 0, untreated	$199.24	$199.24	$199.24	$199.24	$201.89	$175.02
	(278.47)	(278.47)	(278.47)	(278.47)	(220.71)	(162.01)

BY COUNTRY: Selection Impact on Income $=\frac{\partial\Phi(z'\gamma)}{\partial T}\cdot w(\bar{x}'\beta)$	(7) Col. (4) OLS HH FE **Uganda**	(8) Col. (4) OLS HH FE **Guatemala**	(9) Col. (4) OLS HH FE **Philippines**	(10) Col. (4) OLS HH FE **India**	(11) Col. (4) OLS HH FE **Kenya**	(12) Col. (4) OLS HH FE **Bolivia**
Sponsored	**$7.19**	**$27.63** **	**$17.01***	**$37.61*** **	**$1.61**	**$8.19**
	(7.82)	(8.35)	(9.54)	(6.47)	(3.57)	(6.09)
Observations	809	1,680	1,407	1,599	3,051	1,458
Mean *w*, untreated	$36.90	$56.65	$115.33	$131.96	$31.22	$57.73
	144.30	104.66	253.02	167.22	89.62	120.46
Mean *w* | *w* > 0, untreated	$154.99	$193.34	$301.78	$198.28	$111.62	$165.36
	264.39	104.68	333.85	169.90	140.65	154.36

Heckman estimations include controls for age, gender, sibling order, and oldest sibling. Selection on Impact multiplies marginal effect of first-stage tobit by E[*w* | *w* > 0] for sample ($178.07). First-stage *F*-test for instrumental variable estimation yields $F = 95.82$ ($p < 0.001$). Hausman test for efficiency of Heckman vs. IV Heckman fails to reject null of non-instrumented Heckman efficiency ($p = 0.1363$).
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Impact on Wealth

	--------------------------------Dwelling Quality						---------------------------------
VARIABLES	(1) indoor toilet	(2) electricity in home	(3) improved walls	(4) improved roof	(5) improved floor	(6) Simple Dwelling Index	(7) Anderson Dwelling Index
OLS, Household FE:							
Sponsored	0.009	0.029***	0.025**	0.004	0.019**	0.082***	0.034*
	(0.006)	(0.007)	(0.012)	(0.006)	(0.009)	(0.017)	(0.020)
Observations	9,477	9,490	7,863	8,554	8,614	10,004	10,004
R-squared	0.006	0.008	0.011	0.004	0.012	0.013	0.009
GMM-IV, Household FE:							
Sponsored	-0.017	0.041	0.006	-0.001	0.070	0.232**	0.192*
	(0.028)	(0.035)	(0.049)	(0.030)	(0.049)	(0.093)	(0.106)
Observations	9,477	9,490	7,863	8,554	8,614	10,004	10,004

	-------------------------------- Consumer Durables						---------------------------------
VARIABLES	(8) mobile phone	(9) owns bike	(10) owns motorcycle	(11) owns car	(12) owns land	(13) Simple Consumer Index	(14) Anderson Consumer Index
OLS, Household FE:							
Sponsored	0.054***	0.015	0.010	-0.000	0.003	0.089***	0.003
	(0.012)	(0.010)	(0.009)	(0.007)	(0.009)	(0.025)	(0.029)
Observations	9,884	9,856	9,906	9,880	9,444	10,004	10,004
R-squared	0.047	0.044	0.036	0.023	0.047	0.097	0.047
GMM-IV, Household FE:							
Sponsored	0.183***	0.004	0.019	0.000	-0.006	-0.004	0.024
	(0.055)	(0.052)	(0.040)	(0.036)	(0.046)	(0.128)	(0.145)
Observations	9,883	9,856	9,906	9,880	9,444	10,004	10,004
R-squared	0.085	0.060	0.106	0.029	0.063	0.149	0.077

Regressions include controls for age, gender, and sibling order. OLS and IV estimations incorporate household fixed effects.). First-stage *F*-test for instrumental variable estimation yields $F = 95.82$ ($p < 0.001$). Clustered standard errors at the household level in parentheses.

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