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ICID POLICY PAPER No 2
2015

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WOMEN'S ECONOMIC CAPACITY AND CHILDREN'S HUMAN CAPITAL ACCUMULATION

ABSTRACT

Programs that increase the economic capacity of women from poor rural backgrounds are multiplying around the world. These programs can have cascading effects on children's participation in school and work that are theoretically undetermined. We present a simple model to describe the potential channels through which the promotion of women's productive capacity may affect children's participation in school and work. Based on a cluster-randomized trial, we examine how a productive intervention targeted at poor rural women in Nicaragua affected children. While the intervention did not aim to address school attendance and child labour, we find that children in beneficiary households are more likely to attend school and less likely to only be working one year after the end of the intervention. An increase in women's influence on household decisions appears to be the primary channel for the program's beneficial effect on school attendance.

JEL CLASSIFICATION D13, I24, J13, J16

AKNOWLEDGMENTS

The authors are grateful to Chrysanthi Hatzimasoura for her contributions to data analysis and the main study report. The authors would like to thank Verónica Aguilera, Soledad Cubas, Amer Hasan, Karen Macours, Marco Manacorda, Enoe Moncada, Ana María Muñoz Boudet, Teresa Suazo and Egda Velez for providing support and advice during the design and implementation of the pilot and study. Finally, the authors would like to extend their gratitude to the team at Fundación Mujer y Desarrollo Económico Comunitario (FUMDEC) who implemented the intervention under the leadership of Rosa Adelina Barahona, Marlene Rodriguez and Milton Castillo.

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1. Introduction

Investment in women's economic capacity is often seen as an important tool not only to achieve gender equality and address poverty, but also to improve children's wellbeing (e.g. World Bank, 2012). The reasoning is that improvements in women's economic capacity will be accompanied by increases in their access to financial resources and in their intra-household bargaining power. And because women are presumed to have stronger preferences for children's wellbeing than men, their increased financial clout and influence on household decisions is expected to translate into beneficial effects on children. Indeed, as discussed in detail in Duflo (2012), there is evidence that policies that actively increase women's access to resources and their influence on household decisions can be advantageous for children. Cash grants appear to have stronger beneficial effects on children's health and education when they are provided to women instead of men. And, even in the absence of an income transfer, household investment in children's education appears to go up if the bargaining power of women vis-à-vis their husband increases.

Yet, evidence on the effects of interventions that aim to sustainably improve women's economic capacity is limited. The lack of analysis is pressing given the popularity of these programs and because the effects of these programs on children's wellbeing are hard to predict and not necessarily favorable. These programs may not only increase women's access to financial resources and thus their bargaining power vis-à-vis their spouse, but may also raise the returns to child work and as a consequence change children's time use. Children may be employed in the new (or expanded) family business or called to replace adults and carry out household chores, as the demand for adult time in the household business increases.

This paper aims to shed more light on the relationship between programs fostering women's productive capacity and children's activities. We begin by presenting a simple model describing the channels through which productive programs targeted at women may affect children's participation in school and work. We then analyze the impact of a program that promotes micro-entrepreneurial activities of vulnerable women in Nicaragua. The program provided productive transfers (a mix of cash and capital), and training in business and agricultural practices to adult women. It also provided gender awareness workshops to members of beneficiary households. We examine the impact of the program on child labour and school attendance one year after the end of the intervention. Identification stems from randomized program assignment across rural communities. Since households in these communities were invited to apply before community randomization, intent-to-treat program impacts can be estimated by comparing outcomes in applicant households in treatment and control communities.

Although the program did not directly aim to address children's participation in school and work, one year after the end of the intervention we find that children in beneficiary households are more likely to attend school and less likely to be working without attending school. We explore the potential mechanisms explaining the observed impacts. Consistent with its stated goals, the program led to changes in employment patterns in beneficiary households. Beneficiary women in particular were more likely to work in small-scale livestock and non-agricultural self-employment activities. The program also increased women's influence on household decision-making, including in domains related to children's outcomes. Given that the changes in women's employment patterns did not lead to substantive increases in

household income, we suggest that the increase in female influence on household decisions offset any potential increase in the returns to children's work and explains the increase in school attendance.

Our results are linked to the literature on the overall effectiveness of interventions aimed at fostering productive employment and raising income-generating capacity among the poor. The evidence, as discussed in several reviews, is mixed. Integrated interventions addressing multiple constraints can be effective, particularly when the interventions tackle capital constraints and are targeted to the poor and vulnerable groups. There is less evidence of interventions providing skills training alone being effective, particularly when targeted to existing micro-enterprise owners.

Evidence on the impact of providing physical capital and skills training on children's time use is scarce and results are similarly varied. Banerjee et al. (2011) find limited effects of the Indian THP (Targeting the Hardcore Poor) program on children's school attendance and labour supply. Bandiera et al. (2013) however, find that a similar program in Bangladesh increased children's work in self-employment. Karlan and Valdivia (2011) find that business training in Peru lowered children's participation in work and increased their participation in school, although these effects are not statistically significant.

Del Carpio and Loayza (2012) study the effects of a conditional cash transfer program complemented with a productive investment grant in Nicaragua. Their study focuses on a different program than the one we analyze in this paper, as well as on a different (although not very dissimilar) region. The authors show that the intervention contributed to reduce overall child participation in household chores and work, but increased child participation in non-traditional activities related to commerce and retail. This is consistent with results in del Carpio and Macours (2010) on the same intervention, who find that the productive investment grant added to a cash transfer reinforced existing specialization in nonagricultural activities and domestic work for girls, but that overall child labour did not increase.

The rest of the paper is organized as follows. Section 2 provides a theoretical discussion of the effect of promoting women's productive capacity on children's participation in school and work. Section 3 discusses the setting and presents the program, study design and data. Section 4 discusses the strategy used to identify program effects. Section 5 presents our main results related to children's participation in school and labour and discusses potential mechanisms. Section 6 concludes.

2. Theoretical outline

As discussed above, the effects of programs supporting productive activities of women on children's education and labour supply are theoretically undetermined. These kinds of programs might increase the marginal productivity of child work if capital is a gross complement of child work and/or if it induces adult labour supply shifts towards market activities (which may increase the demand of children's time for performing household chores). Additional income generated through the program will tend to reduce child labour involvement as long as leisure is a normal good. This might lead to an increase in schooling or in children's leisure time. The intervention will also lead to an increase in school attendance as long as households are credit constrained and children's school attendance is suboptimal in the absence of the intervention. Finally, increases in children's schooling or reductions in child labour might also result if (i) productive programs increase adult women's bargaining power within the households and (ii) these same women have stronger preferences for investment in children's education than men.

To highlight these issues more formally, we consider a simplified version of an overlapping generations model, in which adult household members value current household consumption and children's future consumption. The latter is assumed to be a function of household investment in education. Adult labour supply is inelastically fixed, while parents decide about children's time allocation between work and education. Household income is generated through the household production of marketable goods or services, which is a function of the household supply of labour and of physical household capital.

In order to keep the exposition simple, we make several additional assumptions. Households have three members: a mother, a father, and a child. These household members can work only in the household business and no hired labour is used in the household production. More importantly, we assume that households cannot save or borrow. To allow for savings will not alter the results, while the implications of non-binding credit constraints will be discussed later. Finally, we only consider the opportunity costs of children's education:. Allowing for direct costs will not change our results.

More formally, the constraints faced by the households are the following. Children's time (normalized to 1) can be allocated to labour l or to education S:

(1) $S=1-l$

Children's future consumption is assumed to be proportional to the amount of education S received during childhood.

Current household consumption is given by the sum of exogenous income (y) and of the value of the household production:

(2) $C=y+g(l_f+l_m,l,k_f+ k_m)$

where lf and lm, and kf and km respectively indicate the labour supplied by the adult male and female member of the household and the capital stock (both physical and human) owned by the male and female member of the household. We assume that male and female labour and capital are perfect substitutes in the household's

production, but that they separately affect the relative power of the household member as discussed below. We do not consider a unitary household, but assume instead that the two adult members of the household have different utility functions, albeit both defined over the two goods discussed above:

(3) $U_f = U_f(C,S)$ and $U_m = U_m(C,S)$

where m and f indicate respectively the male and female household member.

There are different approaches to derive the demand functions for a non-unitary household. We focus here on a cooperative Nash bargaining solution. Other approaches are possible, like the Pareto efficient models suggested by Chiappori (1988), but in our simple framework they will not lead to different results. We assume, therefore, that the demand functions of the household result from the maximization of the following expression over the only decision variable l:

(4) $\text{Max}\ [(U_f-(U_f)^{-})(U_m-(U_m)^{-})]$

where $(U_f)^{-}$ and $(U_m)^{-}$ indicate, respectively, the female and male fallback utilities, i.e. the utility they would obtain if they would leave the household. We assume that the fallback utilities depend on a set of characteristics X and on the ownership of productive capital k:

(5) $U^{-}_i = U^{-}_i(X_i,k_i) \quad i=f,m$

The optimal level of children's labour supply, l*, is determined by:

(6) $V=(U_{f1}\hat{}'\ g_l\hat{}'-U_{f2}\hat{}')(U_m-(U_m)^{-})+(U_{m1}\hat{}'\ g_l\hat{}'-U_{m2}\hat{}')(U_f-(U_f)^{-})=0$

where the apex refers to the order of differentiation and the numerical subscript to the argument of the function.

As is evident from (6), l* is determined as a weighted average of the levels of child labour supply optimal, respectively, for the mother and the father. The weights are given by their "relative" power. If we assume that women have a stronger preference than men for the education of children (and their future welfare) then in equilibrium $(U_{f1}\hat{}'\ g_l\hat{}'-U_{f2}\hat{}')<0$ and $(U_{m1}\hat{}'\ g_l\hat{}'-U_{m2}\hat{}')>0$. In other words, the equilibrium child labour supply will be lower than that preferred by men and higher than that preferred by women should they have been able to decide by themselves.

In this setup, a program aiming to provide women with additional capital and to empower them, can be analyzed by looking at the impact of a marginal increase in kf. By totally differentiating (6) we obtain:

(7) $(dl^*)/(dk_f) = -((\partial V)/(\partial k_f))/(\partial V/(\partial l^*))$

as the denominator of the right hand side of (7) is negative by second order conditions, sign $(dl^*)/(dk_f)=$sign $(\partial V)/(\partial k_f)$ and

(8) $(\partial V)/(\partial k_f)=(U_{f1}'' g_l' [\![g]\!]_k'+U_{f1}' g_{lk}')(U_m-(U_m)^{-})+(U_{m1}'' g_l' [\![g]\!]_k'+U_{m1}' g_{lk}')(U_f-(U_f)^{-})-(U_{fk})^{-}(U_{m1}' g_l'-U_{m2}')$

Equation (8) allows us to identify three effects of a change of k on education and the supply of child labour. Overall the sign of (8) is undetermined, as it is the result of contrasting effects. The increased availability of capital can affect the productivity of child labour, as shown by the terms in $U_{f1}' g_{lk}'$ and $U_{m1}' g_{lk}'$. In particular, if capital and child labour are gross complements, $g_{lk}'>0$, the supply of child labour will tend to increase (and children's participation in school will decrease) as a result of the increased availability of capital. The opposite will happen if capital is a substitute for child labour.

There is a positive income effect on education (negative income effect on work) given by the terms $U_{f1}'' g_l' [\![g]\!]_k'$ and $U_{m1}'' g_l' [\![g]\!]_k'$. If credit markets were perfect, investment in education and consumption decisions would be separable and the income effect would disappear. If, on the other hand, leisure were also valued in the utility function and it were a normal good, then a negative income effect can be present also if capital markets are perfect.

Finally, if the fallback utility of women is positively affected by their increased ownership of capital, $(U_{fk})^{-}>0$, and women value children's education more than men, $U_{m1}' g_l'-U_{m2}'>0$, women's increased bargaining power through the provision of capital will tend to increase consumption of education and reduce children's labour supply.

3. Empirical study design and data

3.1 Nicaraguan country context

Nicaragua is classified by the World Bank as a lower middle income country. In 2010 it had a GDP per capita of about US$1535[1]. In the same year, about 49% of women aged 15 to 64 were economically active, compared to about 82% of men. Nearly 60% of the women who were economically active were self-employed.

School participation is not yet universal among Nicaragua's children. While the country's 2010 net primary school enrollment rate was 92%, it is estimated that only about half of the children who entered primary school would reach the final grade.[2] Concomitantly, the net secondary school enrollment rate was only about 45%. Literacy rates are about 78% in the adult population (15+) and about 87% among youths (15 to 24).[3]

It is common for children to be involved in economic activities, even if they have not yet reached the minimum legal working age of 14.[4] Based on the 2010 Encuesta Continua de Hogares, the Understanding Children's Work programme estimates that nearly 37% of children aged 13 are economically active.[5] About 78% of these children combine work and school. Boys are more likely to work than girls (48% vs. 26%). Rates of participation in economic activities are higher in rural areas (49%) than in urban areas (25%). Boys who are economically active mostly work in agriculture (71%) although it is also common form them to be active in commerce (14%). Girls who are economically active are slightly more likely to work in commerce (35%) than in agriculture (32%).

3.2 The program

In 2009/10, a Nicaraguan NGO (Fundación Mujer y Desarrollo Comunitario, or FUMDEC), implemented a productive transfer program with support from the World Bank[6]. The intervention built on a model in place in other communities in northern Nicaragua since 1996, and had two main objectives: (i) to facilitate income generation and diversification by promoting women's economic activities, and (ii) to foster gender empowerment by improving women's aspirations, their participation in households' economic decisions as well as their social participation.[7]

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[1] The figures in the remainder of this discussion, with the exception of those related to children's economic activities, are drawn from the World Bank's development indicators database: http://data.worldbank.org/country/nicaragua. After correcting for purchasing power parity, the GDP per capita translates to about US$3962.

[2] Latest figure is for 2007.

[3] Latest figure is for 2005.

[4] For more information on legislation, we refer to the website of the US Department of Labour: http://www.dol.gov/ilab/reports/child-labour/nicaragua.htm

[5] http://ucw-project.org/Pages/Tables.aspx?id=1602

[6] See Hatzimasoura, Premand and Vakis (2014) for a more detailed description of the program as well as its overall impacts beyond education and child labour.

[7] For more information about FUMDEC, see http://fumdec.org.

To achieve these two objectives, the program offered households with at least one female member 16 to 60 years old[8] a package of benefits that included capital transfers in the form of cash, seeds, or livestock. The package also included technical assistance and training in business plan development, financial literacy and technical skills to develop or expand small-scale household enterprises, livestock or agricultural activities. Women and some men in beneficiary communities also participated in gender awareness training covering issues such as gender roles and intra-household decision making.[9]

The package had an average value of US$602 per beneficiary[10], divided between US$316 in direct capital transfers (in the form of a mix of cash, seeds and livestock) and US$286 that covered the costs of training and technical assistance provided. More than 80 percent of the targeted households live on average with less than US$2 per capita per day, so that the transfer amounted to around 24% of pre-transfer annual household consumption, a rather sizeable magnitude.

3.3 Beneficiary selection, study design and data

The program operated in Santa Maria de Pantasma, one of Nicaragua's poorest municipalities. For the purpose of evaluating the program, a group of 24 communities was identified[11]. Baseline data were collected in June and July 2009, before households were informed about the program and invited to enroll. Baseline data is available for the universe of eligible applicant and non-applicant households in the 24 communities. Baseline information includes household and dwelling characteristics, household composition and a number of household and individual socio-economic characteristics. For individuals aged 6 years or more, the baseline survey collected information on completed education, school enrollment, school attendance and involvement in economic activities in the week preceding the survey.

Following the completion of the baseline survey, households in all eligible communities were informed about the program and invited to enroll (apply) during a series of community meetings held in July and August 2009. Households were informed that participation in the program was conditional on their community being selected. As households were asked to enroll prior to community randomization, applicant households are known in both treatment and control communities. As such, intent-to-treat program effects can be estimated based on counterfactual outcomes among applicants in the control communities.

A public lottery was then organized in the municipal headquarters to allocate the communities to the treatment and control groups. Communal and municipal leaders were invited along with representatives from each community, from the local NGO

[8] About 95 percent of the households in the treatment communities had a female member in that age range.

[9] One additional component of the program, not yet implemented at the time of the follow-up survey, consisted in the creation of community banks. For this purpose, the program would provide training in management and organization to community leaders and initial technical support. These banks would eventually serve as a sustainable source of credit for the community after the program itself was concluded. Start-up capital of the community banks would consist of the credit repaid by beneficiary households.

[10] In addition, administrative costs for the pilot amounted to US$225 per beneficiary, for a ratio of administrative costs to total transfers of 37%.

[11] The 24 communities were selected on the basis of 5 criteria (i) they had to be located in a rural area, (ii) they should not have benefitted from related interventions, (iii) they needed to contain a minimum of 20 households, (iv) they needed to be located in an area that was well-known to the local NGO, and (v) the local authorities had to agree with the (potential) implementation of the program in their community.

and the World Bank. Selection of communities was based on block randomization within (10) groups of neighboring communities (2 or 3 depending on the block). The lottery led to the selection of 13 treatment and 11 control communities, containing respectively 405 and 472 eligible households that did apply to the program, 417 and 563 eligible households that did not apply, and 41 and 58 ineligible households (i.e. households without a female member aged 16 to 60). The intervention was implemented between September 2009 and August 2010. By August 2010, all the capital transfers were completed, and all the training modules were fully implemented.

A follow-up survey was administered from June to August 2011 to all the households that had been interviewed at baseline and could be tracked. Tracking was conducted at the household level and households who left the experimental communities or household members who left the household were not followed, although the follow-up survey collects information on households who migrated to other experimental communities and on individuals joining existing households.[12] The follow-up survey was more extensive than the baseline survey. In addition to the questions from the baseline survey, it also collected information on a wider range of outcomes including involvement in economic activities in the 12 months prior to the interview.

[12] Although we have information on individuals who moved into the households (and were not observed at baseline), we leave these individuals out of the analysis.

4. Empirical strategy

4.1 Schooling and child labour outcomes

In the analysis, we focus primarily on the effect of the program on school attendance in the current school year and participation in work in the 12 months prior to the interview. School attendance[13] was measured both in the baseline and follow-up surveys. As the baseline and follow-up questionnaire were administered in the middle of the school year, which runs from February to November, seasonal effects should not be a source of concern.

We classify individuals as working if they participated in any economic activity on own account or as wage workers in the 12 months prior to the interview.[14] This information was collected only as part of the follow-up survey. In order to probe the robustness of the main results, we exploit the information on work in the week prior to the interview, which is available both at baseline and follow-up.[15] To the extent possible, we also examine variations in working hours. For children aged 6 to 16, the follow-up survey asked about usual weekly hours during the school year in up to three main economic activities: we sum the hours worked to measure "total hours worked".

4.2 Sample and attrition

As we study the impact of the productive transfer program on children's work and school participation, we restrict our sample to (households with) children aged 8 to 17 at baseline. This gives a sample of 647 households that applied to participate in the program, with 1923 adults and 1458 children in the relevant age range. Over 95% of these households were observed at follow-up. Because individuals were not tracked if they had left the household, the probability that individuals are observed at follow-up is somewhat lower: about 87% for adults and about 91% for children.

Table A1 in the Appendix examines whether attrition at follow-up is significantly different in households residing in treatment and control communities at baseline. It reports OLS regressions of the indicator for being interviewed at follow-up on the indicator for living in a treatment village at baseline. Standard errors are clustered at village level. Since the number of clusters is small (24), the limiting t-distribution is likely to over-reject the null-hypothesis that the program has no effect. Following Burde and Linde (2012), we therefore calculate statistical significance relative to the small-sample t-distribution with 23 degrees of freedom.

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[13] We consider individuals to be attending school if the answer to the question "is … attending school this school year?" is "yes".

[14] In particular, the activities on own account include agricultural activities (in household plots or backyard production), livestock activities and non-agricultural self-employment in commerce, manufacturing or services. Livestock activities and non-agricultural self-employment activities are grouped in a sub-category in part of the analysis since one of the program objectives was to help participants diversify out of subsistence farming. Wage employment covers both agricultural wage work and non-agricultural wage jobs.

[15] We rely on two questions. The first is whether individuals worked in the week prior to the interview. The second, asked only to individuals who initially respond that they did not work in the week prior to the interview, is whether they participated in any of the following economic activities: (i) sale of goods, (ii) washing, ironing, or sewing for others, (iii) preparing and selling bread, tortillas, sweets, crafts and other items, (iv) work as an apprentice, (v) agricultural work (cultivation or caring for livestock), (vi) tourism, (vii) fishery, or (viii) other economic activities (not further defined). We classify individuals as working if they answer "yes" either to the first or the second question.

Regression results indicate that the attrition rate of applicant households and children was marginally lower in treatment than in control communities. However, when we include baseline covariates (discussed in more detail below) as controls in the regressions, estimates become smaller and not statistically significant, suggesting that controlling for baseline characteristics limits potential bias due to differential attrition.

4.3 Descriptive statistics and estimation strategy

Table 1 displays the mean values of a range of household level[16] baseline covariates in the control communities (column (1)): literacy and gender of the household head, an asset index,[17] a dummy for whether any land is owned by the household, distance to the closest school and to the closest health center, bedrooms per capita, distance to schools and health centers, share of male adults in the household, share of adults in ten-year age groups, and dwelling characteristics. The table illustrates the high level of deprivation in the experimental communities. Nearly 40% of household heads are illiterate. About 10% of households live in a wooden or improvised dwelling and, although nearly 90% of the households own their dwelling, only about 45% have a property title. The main material of the dwelling's floor is typically earth and walls are rarely made of brick or concrete. Over 50% of households rely on rivers for water and nearly 50% of households have no sanitary facilities in the home. Only about 1 in 10 households is connected to the electricity grid.

Table 2 shows the mean values of the outcome variables and of individual covariates for adults and children who lived in control communities at baseline. Nearly 90% of the adults worked in the week prior to the interview. Men are markedly more likely to work (98% of the men in the control group is economically active) than women (79%) (results not displayed). 28% of children from applicant households are engaged in some type of work during the week prior to the interview and 78% of children attend school.

We test the success of the randomization by regressing selected baseline characteristics on the treatment dummy among households and individuals observed at follow-up. Tables 1 and 2 report the coefficients on the treatment dummy (column (2)) together with the clustered standard errors (column (3)). There are a few imbalances in baseline characteristics, but, by and large, these characteristics are not significantly correlated with the treatment dummy. These results suggest that the experiment was balanced. Coupled with the observation that that attrition did not differentially affect the composition of the treatment and control groups (and more so when we include additional baseline controls), it gives us reasonable confidence in the internal validity of the experiment.[18]

We rely on randomized assignment to identify the program's impact by employing a simple reduced form model. Formally, we estimate cross-section regressions as follows:

$$(9) \qquad Y_{ic1} = \beta_0 + \beta_1 TREAT_{c1} + \beta_2 X_{ic0} + e_{ic1}$$

[16] All figures reported in this subsection are for households and individuals that were observed also at follow-up.

[17] This is computed as the first principal component of 13 assets.

[18] Administrative data also show that the application rate was similar in treatment and control communities.

where Y_{ic1} is the outcome of interest for individual i in community c at follow-up (denoted with the subscript 1), $TREAT_{c1}$ is a dummy that takes the value 1 for treatment communities, and X_{ic0} is a vector of baseline (denoted 0) controls. Baseline controls include all covariates and outcome variables displayed in Tables 1 and 2 as well as the "randomization blocks".[19] We estimate regression (1) for individuals from applicant households only to obtain the intent-to-treat effects of the program[20]. We cluster the standard errors at the community level and compute statistical significance relative to the small-sample t-distribution with 23 degrees of freedom.

[19] If a covariate is not reported for an individual or household we code it with the value -1. We then include a dummy variable taking the value 1 for all individuals or households for whom the corresponding covariate is missing.

[20] We also examined potential spillover effects on non-applicant households. School participation of children in those households was not significantly affected. There may have been an increase in participation in work among children from non-applicant households. However, the estimated effect is only marginally significant and, given the absence of other spillover effects, we decided not to focus on this outcome in the present paper.

5. Main results

5.1 Children's work and schooling

Table 3 shows the impact of the program on children from applicant households. As shown in Panel A, school attendance increases by about 8 percentage points, with no significant impact on work during the 12 months prior to the interview. There is no evidence of any significant changes in hours of work in economic activities either (Panel B)[21].

Panel C examines the impact of the program on four mutually exclusive combinations of work and school attendance: attending school only, working only, both working and attending school, neither attending school nor working. The share of children who are only working falls by about 6 percentage points as a result of the program. Concurrently, the share of children engaged in both activities rises by about 4 percentage points, and the share of children that are in school only increased by 3.5 percentage point. These two effects are not significant, but contribute to explain the overall decrease in the share of children working only. It appears that children who were only working, begin to combine school and work as a result of the program.

Panel D analyzes whether the program led to changes in the work undertaken by children, by showing program impacts on different forms of work. Results suggest that children from applicant households switched from agricultural activities related to crop production into livestock and non-agricultural self-employment, the type of activities encouraged by the intervention., This change is not associated with an increase in overall child labour, as highlighted above, but seems to indicate that complementarity effects between capital, adult and child work were at play.

Table 4 examines whether the effects of the program are heterogeneous along household and individual baseline characteristics (by interacting the treatment dummy with the relevant baseline characteristics). The increase in school attendance for children in applicant households holds for both boys and girls. However, the effect of the program on school attendance appears to be concentrated among older children (aged 14-17 at baseline), those living closer to schools (1 km at most), and those living in households with a literate head. Interestingly, the increase in school participation is particularly pronounced among children who were not in school at baseline, indicating that the program might have led some children to (re-) enter school.[22] This finding is consistent with the earlier observation that children who otherwise only work, begin to combine school and work as a result of the program. No statistically significant effect on work is observed for any subgroup of children from applicant households.

5.2 Robustness

To examine the robustness of our main results we exploit the panel nature of the data and estimate the following individual fixed effect regressions:

$$(10) \qquad Y_{ict} = \delta_0 + \delta_1 TREAT_{ct} + \mathrm{d_t} + \mathrm{d_i} + e_{ict}$$

[21] No changes are found in household chores either (results not displayed).

[22] None of the differences displayed in Table 4 (such as between the impact of the program on boys and girls or the impact of the program on older or younger children) is statistically significant.

where Y_{ict} is the outcome of interest for individual i in community c at time t (0 baseline and 1 follow-up), $TREAT_{ct}$ is a dummy that takes the value 1 for treatment communities at follow-up and it is equal to 0 otherwise, and d_t and d_i are respectively time and individual fixed effects. As mentioned, information on work in the past 12 months was not collected at baseline, therefore we use as outcome variable work in the *week* prior to the interview. The results are displayed in Table 5. The impact estimates for school attendance and work in the week prior to the interview are remarkably similar to those for work in the year prior to the interview discussed above. Panel A examines children's school attendance and participation in work. Similar to the cross-sectional results, we find a 7 percentage point increase in school participation among children from applicant households and a reduction in work only by 6 percentage points. These effects are again driven by a reduction in the share of children only working.

5.3 Channels

The model we presented above (section 2) highlights three potential channels through which the program may affect children's participation in school and work (see equation (8) in particular): changes in the returns to children's work (given by the term g'_{lk}), increased household income (given by the term g'_k), and female bargaining power ($\overline{U_{fk}} > 0$). Because the randomized program assignment constitutes a single instrument, we cannot definitively establish the extent to which each of the three channels explains the increase in school participation and the absence of any effect on children's labor supply. However, following for instance Pop-Eleches and Urquiola (2013), we can examine whether the program affected any of these three channels, allowing us to exclude as likely mechanisms the ones that were not affected.

We start by examining the first potential channel, namely whether the program affected the returns to children's work. We examine whether the program altered adult labor supply and the type of economic activities in which adults are involved. As discussed, such changes in household and adult economic activity might directly generate opportunities for the gainful employment of children or lead to increased demand for children's time in activities that would otherwise be carried out by adults.

The intervention led applicant households in treatment communities to start new economic activities and/or expand their existing ones. As shown in Panel A of Table 6, the probability that adults in applicant households worked in the 12 months prior to the interview is 3 percentage points higher. Consistent with the intended consequences of the program, women from applicant households - i.e. direct beneficiaries of the program - experience the most pronounced increase in work (4 percentage points). Men from applicant households in treatment communities are also marginally more likely to work in the previous 12 months by 1 percentage point. As for the findings on children discussed earlier, results for adults are robust to using difference-in-differences on employment outcomes in the previous week (see appendix table A.3).

Table 6 (Panel B) also shows the impact of the program on the different forms of work for adults. The main increase of employment is driven by additional work in livestock and non-agricultural self-employment activities for women. These impacts are in line with the core activities promoted by the program. The observed change in

the sectoral composition of children's employment, discussed earlier, is consistent with the shift in the composition of adult employment and offer support to the hypothesis of complementarity between child labour, adult labour and capital.

The second potential channel relates to increased household income. Table 7 (Panel A) documents program impact on total per capita income. Despite an overall increase in household economic activity and employment, no overall impacts on household income are measured. As such, income effects at the household level cannot explain the positive program impacts on school attendance and the observed decrease of children working only.

Changes in female bargaining power constitute the third potential channel. Table 7 (Panel B) documents impacts on female bargaining power and intra-household decision-making in relation to children's outcomes. Results show that beneficiary women and their husbands are more likely to make decisions jointly on children's activities. In particular, they are 10 percentage points more likely to make joint decision on children's school attendance. This increase is associated with a corresponding decrease in husbands' sole decision-making in that domain. Similar results are found for other decisions relevant to children, including purchase of clothes for children, or visits to health centers.[23] Results in table 7 suggest that the increase in child school attendance and the decrease in children working only may be driven by increases in bargaining power, in particular an increased role of women in intra-household decision-making. Such effects may have contributed to offset potential increase in child labour driven by substitution effects.

[23] Hatzimasoura, Premand and Vakis (2014) provide a broader discussion on the impacts of the program on intra-household decision-making and gender empowerment.

6. Conclusion

We have analyzed the effects of a program that aims both to empower and to increase the productive capacity of women in rural Nicaragua on children's school attendance and work. The program offered technical assistance and training plus capital transfers to eligible women in beneficiary households, along with gender awareness training. As shown in our theoretical outline, these kinds of programs can affect children's time allocation through a variety of channels: the possible complementarity between capital and child labour might generate increased demand for child labour that can be counterbalanced by income effects and by the increased power of women within the household.

We find robust evidence that children in applicant households were more likely to be enrolled in school, and less likely to be only working. A modest shift away from agricultural work and towards livestock and non-agricultural self-employment activities is observed among children. These changes offset each other and as a consequence children's overall labour supply did not change.

We provide evidence on the channels that may explain these effects on children's activities. Consistent with its stated goals, the program led to changes in employment patterns among beneficiary households, particularly women. Beneficiary women were more likely to work in small-scale livestock and non-agricultural self-employment activities. This shift seems to have been mirrored in the structure of children's employment, indicating that a change in children's labour demand did likely happen. The changes in employment patterns, however, did not lead to an increase in household income. This seems to exclude the possibility that positive income effects explain the expansion of school attendance and counterbalanced the possible increase in the demand for child work. The program apparently did succeed in empowering women, as indicated by the substantial increase in joint decision making within the household especially with reference to children's related issues. In absence of any relevant impact on household income and given the indications that complementarity effects might have increased the demand for child labour, we conclude that women's influence on decision making did play a crucial role in ensuring that the program had a positive effect on children's human capital.

The evidence presented here confirms, albeit indirectly, that promoting household businesses can potentially have unwanted effects on children's labour supply (see the references quoted in the introduction). However, the improved bargaining power of women brought about by the targeting of the program seems to have more than counterbalanced any negative effects.

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TABLES

Table 1. Balance of household level baseline covariates

	Control (1)	Treatment - control (2)	(s.e.) (3)
Household head:			
Female	0,134	-0,054**	(0,023)
Illiterate	0,391	-0,050	(0,043)
Wealth indicators:			
Asset index: poorest 1/4	0,228	-0,009	(0,055)
Asset index: richest 1/4	0,259	0,010	(0,051)
Any land owned	0,543	0,062	(0,048)
Bedrooms per capita	0,268	-0,019	(0,015)
Location:			
Distance to school >1km	0,333	-0,107**	(0,049)
Distance to health center >1km	0,946	0,011	(0,037)
Household composition:			
% male adults	0,498	0,017	(0,011)
% adults 18-19	0,071	0,014	(0,010)
% adults 20-29	0,268	-0,013	(0,021)
% adults 30-39	0,345	0,038	(0,030)
% adults 40-49	0,178	-0,009	(0,015)
% adults 50-59	0,081	-0,021*	(0,011)
% adults 60-69	0,036	-0,014**	(0,006)
% adults >70	0,021	0,006	(0,006)
Dwelling:			
Type: House	0,910	0,031	(0,030)
Wooden	0,040	-0,001	(0,022)
Improvised	0,050	-0,033	(0,020)
Ownership: With title	0,464	0,013	(0,047)
Without title	0,410	-0,020	(0,052)
Other	0,126	0,007	(0,030)
Walls: Brick	0,031	0,012	(0,019)
Concrete	0,062	0,034	(0,048)
Mud	0,159	0,017	(0,101)
Wood	0,695	-0,066	(0,115)
Wood and concrete	0,012	0,007	(0,010)
Rubble	0,022	-0,009	(0,011)
Other	0,019	0,004	(0,018)
Floor: Wood	0,022	-0,015	(0,011)
Tiles	0,150	-0,010	(0,047)
Bricks	0,016	0,008	(0,012)
Earth	0,810	0,021	(0,053)
Other	0,003	-0,003	(0,003)
Roof: Zink	0,788	0,135***	(0,036)
Tiles	0,012	-0,006	(0,010)
Waste	0,009	-0,006	(0,007)
Plastic	0,187	-0,130***	(0,037)
Other	0,003	0,007	(0,006)
Water: Piped	0,090	0,148	(0,104)
Public place	0,031	0,009	(0,017)
Well	0,277	-0,115	(0,081)
Source or river	0,533	-0,036	(0,075)
Other	0,069	-0,006	(0,035)
Sanitation: latrine	0,492	0,091	(0,112)
No service	0,498	-0,085	(0,114)
Other	0,009	-0,006	(0,009)
Light: Electricity grid	0,118	-0,072	(0,070)
Generator	0,019	0,015	(0,015)
Kerosene	0,576	0,032	(0,087)
None	0,090	-0,014	(0,042)
Other	0,196	0,040	(0,074)

Notes. Columns entitled "Control" shows the mean in the control group. Columns entitled "Treatment - Control" and "(s.e.)"respectively show the coefficient and standard error of OLS regressions of the baseline covariates in the stub column on the indicator for living in a treatment village at baseline. Sample restricted to households with non-attriting children. The asset index is the first principal component of a group of 13 assets. Standard errors (in parentheses) clustered at the village level. Significance levels calculated against the T distribution with 23 degrees of freedom. *** p<0.01, ** p<0.05, * p<0.1

Table 2. Balance of individual level baseline activities and covariates

	Control (1)	Treatment - control (2)	(s.e.) (3)
Panel A: Adults (18 and older at follow-up)			
Work (last week)	0,884	-0,023	(0,030)
Age (at baseline)	36,964	-1,224**	(0,506)
Illiterate	0,335	-0,061*	(0,032)
No basic education	0,371	-0,046	(0,027)
Some primary education	0,522	0,034	(0,024)
Education beyond primary	0,107	0,012	(0,023)
Panel B: Children (8-17 at follow-up)			
School attendance (current school year)	0,775	0,000	(0,039)
Work (last week)	0,278	0,032	(0,038)
Age (at baseline)	10,192	-0,018	(0,112)
Male	0,516	0,021	(0,023)
Illiterate	0,355	-0,056	(0,048)
No basic education	0,352	-0,059	(0,040)
Some primary education	0,606	0,057	(0,038)
Education beyond primary	0,042	0,001	(0,011)

Notes. Columns entitled "Control" shows the mean in the control group. Columns entitled "Treatment - Control" and "(s.e.)"respectively show the coefficient and standard error of OLS regressions of the baseline covariates and activities in the stub column on the indicator for living in a treatment village at baseline. Sample restricted to households with non-attriting children. Standard errors (in parentheses) clustered at the village level. Significance levels calculated against the T distribution with 23 degrees of freedom. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Program impact on children's activities

	(1)
Panel A: School-attendance and work	
School attendance (current school year)	0,079***
	(0,025)
Any work (past 12 months)	-0,014
	(0,020)
Panel B: Intensive margin of work	
Usual hours of work per week	-0,939
	(1,036)
Panel C: Combinations of school-attendance and work in the past 12 months	
School only	0,035
	(0,023)
Work only	-0,063***
	(0,016)
Both in work and in school	0,043
	(0,026)
Neither in work nor in school	-0,014
	(0,019)
Panel D: Type of Employment	
Agriculture (non-livestock)	-0,031*
	(0,018)
Livestock and non-agricultural self-employment	0,038*
	(0,022)
Wage Employment	0,043
	(0,027)

Notes. Impact estimated using cross-section regressions with controls. Controls include all the baseline covariates and activities listed in Table appendix 2 (with age at baseline converted to dummies for years). Standard errors (in parentheses) clustered at the village level. Significance levels calculated against the T distribution with 23 degrees of freedom. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Heterogeneous program impacts on children's activities

Dependent variable	Any work (1)	In school (2)
Panel A: Gender		
Boys	-0,029	0,086**
	(0,030)	(0,032)
Girls	0,002	0,071**
	(0,036)	(0,031)
Panel B: Age (at follow-up)		
8-13 at follow-up	-0,002	0,037
	(0,030)	(0,032)
14-17 at follow-up	-0,026	0,128**
	(0,037)	(0,050)
Panel C: Distance to school		
Less than 1 km	-0,017	0,109***
	(0,024)	(0,028)
1 km or more	-0,004	0,003
	(0,050)	(0,048)
Panel D: Literacy of household head		
Literate	0,022	0,099**
	(0,034)	(0,039)
Illiterate	-0,072*	0,048
	(0,038)	(0,037)
Panel E: School attendance at baseline		
Not in school	0,013	0,122**
	(0,032)	(0,050)
In school	-0,022	0,066**
	(0,025)	(0,028)

Notes. Results from the estimation of model (1), i.e. cross section regressions with baseline controls, where the indicator for village treatment is interacted with baseline covariates as displayed in the stub column. Controls include all the baseline covariates and activities listed in Table appendix 2 (with age at baseline converted to dummies for years). Standard errors (in parentheses) clustered at the village level. Significance levels calculated against the T distribution with 23 degrees of freedom. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Robustness of measured program impact on children's activities

	(1)
Panel A: Children's school-attendance and work	
School attendance (current school year)	0,072*
	(0,035)
Any work (past week)	-0,005
	(0,049)
Panel B: Children's combinations of school attendance and work in the past week	
School only	0,019
	(0,048)
Work only	-0,061**
	(0,027)
Both in work and in school	0,056
	(0,047)
Neither in work nor in school	-0,014
	(0,026)

Notes. Impact estimated using individual fixed effects panel regressions. Standard errors (in parentheses) clustered at the village level. Significance levels calculated against the T distribution with 23 degrees of freedom. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Program impact on household activities and work carried out by adults

	All adults (1)	Male adults (2)	Female adults (3)
Panel A: Adult level outcomes (past 12 months)			
Any work (past 12 months)	0,034***	0,009*	0,047***
	(0,006)	(0,005)	(0,018)
Panel B: Types of employment (past 12 months)			
Self-employment in Agriculture (non-livestock)	0,022	0,022	0,009
	(0,023)	(0,025)	(0,040)
Livestock and non-agricultural self-employment	0,005	-0,062*	0,081***
	(0,022)	(0,035)	(0,031)
Wage employment	-0,010	-0,032	-0,011
	(0,027)	(0,044)	(0,029)

Notes. Impact estimated using cross-section regressions with controls. Sample restricted to households with children observed at baseline and follow-up. Controls include all the baseline covariates and activities listed in Table appendix 2 (with age at baseline converted to dummies). Standard errors (in parentheses) clustered at the village level. Significance levels calculated against the T distribution with 23 degrees of freedom. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Program impact on household income and decision-making on children's outcomes

	(1)
Panel A: Income	
Household per capita income (log)	0,037
	(0,177)
Panel B: Empowerment and Intra-Household Decisionmaking	
Beneficiary and husband make joint decisions on chidren's school participation	0,093***
	(0,030)
Beneficiary and husband make joint decisions on purchase of clothes for children	0,182***
	(0,040)
Beneficiary and husband make joint decisions on children's health center visits	0,149***
	(0,031)

Notes. Impact estimated using cross-section regressions with controls. Controls include all the baseline covariates and activities listed in Table appendix 2 (with age at baseline converted to dummies for years). Estimates for work as a professional among non-applicant children are missing as none of these children worked as a professional. Standard errors (in parentheses) clustered at the village level. Significance levels calculated against the T distribution with 23 degrees of freedom. *** p<0.01, ** p<0.05, * p<0.1

Appendix. Tables

Appendix table 1. Sample attrition

	Control (1)	Treatment - control (2)	(s.e.) (3)
Panel A: Simple difference			
Households	0,955	0,019*	(0,009)
Adults	0,863	0,006	(0,025)
Children	0,896	0,036*	(0,019)
Panel B: With controls included			
Households		0,013	(0,012)
Adults		-0,006	(0,019)
Children		0,027	(0,019)

Notes. Columns entitled "Control" shows the mean in the control group. Columns entitled "Treatment - Control" and "(s.e.)" show the coefficient and standard error of OLS regressions of the indicator for being interviewed at follow-up on the indicator for living in a treatment village at baseline without controls (Panel A) and with the controls displayed in Table appendix 2 (Panel B). The sample is restricted to households with non-attriting children. Standard errors (in parentheses) clustered at the village level. Significance levels calculated against the T distribution with 23 degrees of freedom. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 2. Mean values of outcome variables in the control group at follow-up

	(1)
Panel A: Adults	
Work (all, last year)	0,946
Work (men, last year)	0,975
Work (women, last year)	0,916
Work (all, last week)	0,648
Work (men, last week)	0,933
Work (women, last week)	0,353
Self-employment in agriculture (all, last year)	0,730
Self-employment in agriculture (men, last year)	0,898
Self-employment in agriculture (women, last year)	0,556
Self-employment in livestock and non-agricultural activities (all, last year)	0,613
Self-employment in livestock and non-agricultural activities (men, last year)	0,386
Self-employment in livestock and non-agricultural activities (women, last year)	0,847
Wage employment (all, last year)	0,390
Wage employment (men, last year)	0,540
Wage employment (women, last year)	0,234
Panel B: Children	
School attendance (all, current school year)	0,680
School attendance (boys, current school year)	0,622
School attendance (girls, current school year)	0,742
Work (all, last year)	0,768
Work (boys, last year)	0,854
Work (girls, last year)	0,677
Usual hours of work per week (all, last year)	9,434
Work (all, last week)	0,380
Combinations of school attendance and work (last year)	
School only	0,182
Work only	0,273
Both in work and in school	0,498
Neither in work nor in school	0,047
Combinations of school attendance and work (last week)	
School only	0,490
Work only	0,190
Both in work and in school	0,191
Neither in work nor in school	0,130
Self-employment in agriculture (last year)	0,467
Self-employment in livestock and non-agricultural activities (last year)	0,405
Wage employment (last year)	0,244

Notes. The table reports means for outcome variables in the control groups at follow-up. Sample restricted to households with non-attriting children.

Appendix table 3. Robustness of measured program impact

	All adults (1)	Male adults (2)	Female adults (3)
Panel A: Adults			
Any work (past week)	0,064**	0,012	0,106**
	(0,025)	(0,017)	(0,053)

Notes. Impact estimated using individual fixed effects panel regressions. Standard errors (in parentheses) clustered at the village level. Significance levels calculated against the T distribution with 23 degrees of freedom. *** p<0.01, ** p<0.05, * p<0.1