# Does Expanding Female Economic Opportunities Improve Children's Human Capital? Evidence from a Randomized Control Trial in Bangladesh*

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**Abstract**

Asset transfer programs to ultra-poor women in low- and middle-income countries generally show improved outcomes for women, but we know little about intergenerational effects. We use data from BRAC's "Targeting the Ultra Poor" in Bangladesh to examine the effects on children's education and skills. Overall, there is no evidence of positive effects on school attendance or years of education. We examine potential explanations for these results. Although treated households are substantially less likely to motivate non-enrollment with costs or other economic considerations, children's likelihood of working increases substantially. Differential sample attrition does not appear to explain the results.

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# 1 Introduction

Lately, there has been a push to expand economic opportunities for women in low- and middle-income countries through asset transfer programs. The results of these programs are encouraging. Women appear to be moving permanently to higher-income occupations, which leads to more assets, higher expenditures, and better food security (Balboni et al., 2021; Bandiera et al., 2017; Banerjee et al., 2021, 2015; Bedoya et al., 2019). Furthermore, the evaluations cover Afghanistan, Bangladesh, Ethiopia, Ghana, Honduras, India, and Pakistan, showing that asset transfers can work in disparate settings.[1]

The motivation for these asset transfer programs is the belief that a one-time large transfer will help break the poverty trap and pull households out of poverty, ultimately launching a virtuous intergenerational cycle (Banerjee et al., 2021; Raza et al., 2018). A critical component of this virtuous cycle is improving the next generation's human capital, especially health and education. Indeed, child health does improve with the asset transfer (Raza et al., 2018). However, we know little about the effects on children's schooling.

The closest related type of program for which we have information on the effect on school attendance is cash transfers. Conditional cash transfer programs provide smaller but more frequent transfers than asset transfer programs, often with the condition that school-aged children go to school. Generally, these programs show substantial positive effects on educational attainment, even though this does not necessarily translate to better labor market outcomes (Baird et al., 2019, 2014; Molina Millán et al., 2019). One could argue that the improvements in education are driven predominately by the condition that children have to attend school. However, even unconditional cash transfer programs show significant positive effects on school outcomes, suggesting that what is important is the cash itself (Baird et al., 2014).

[1]The one exception to medium-term positive impacts is Uganda's Youth Opportunities Program, which gave substantial cash grants to young people. This program shows positive short-term effects, but most of those dissipate after nine years (Blattman et al., 2020, 2014).

Contrary to cash transfers, standard economic theory suggests that the effect of asset transfer programs on children's education is ambiguous. On the one hand, the positive income effect suggests that we should see increased schooling for children of women who received asset transfers. Similarly, any improvements in women's bargaining power from the asset transfer may also, in turn, improve children's education outcomes (Duflo, 2012; Raza et al., 2018).[2]

On the other hand, if women work more hours or move to activities less compatible with housework, there may be a substitution effect where children do more household chores, potentially negatively affecting their schooling. It is also possible that the return to children's time in market work may increase with the mother's receipt of new assets. For example, if she receives or invests in farm animals, her children may herd and take care of the animals rather than attend school.

Using a randomized control trial from Bangladesh, we examine how BRAC's "Targeting the Ultra Poor" (TUP) program affects children's schooling and market activities. The program targets women in ultra-poor households and provides income-generating assets and livelihood training. This program was one of the first to focus on large asset transfers to poor women.

We first examine the impact of the TUP program on school attendance and years of education completed. There is little evidence of overall improvements in children's school attendance, the highest grade achieved, or skills learned in school. The possible exception is an increase in literacy skills for older boys.

We examine several mechanisms that may explain the absence of an effect on education, such as treatment effects on child labor, tutor use, women's empowerment, and the motivation for not attending school. There is no single explanation, but we do find evidence that children are both more likely to work and work more as a result of the program. Treated households are less likely to report economic motivations for not attending school and are more likely to use private tutors. However, they are also more likely to report that children do not

[2]Identifying whether improvements in child outcomes arise from better future economic opportunities or changes in current bargaining power is often challenging (Folbre, 1984; Rosenzweig and Schultz, 1982).

want to study as the motivation for why they are not in school. Differential selection across treatment and control groups appears to play a minor role in the results.

# 2 The BRAC TUP Program

BRAC set up the TUP program to reach women in ultra-poor households. The motivation was twofold (Hashemi and Rosenberg, 2006; Roy et al., 2015). First, evidence showed that ultra-poor households lack the physical capital and skills necessary to move out of poverty. Second, the existing development programs targeting women in rural areas were unlikely to reach the ultra-poor.

Rather than the smaller, more frequent transfers used in programs such as conditional cash transfer programs, the idea behind BRAC's program is that a significant one-time transfer would help break the vicious cycle of poverty.[3] Therefore, the TUP program provided women in ultra-poor households with income-generating assets valued at approximately USD 560 (2007 PPP terms), almost doubling their wealth (Bandiera et al., 2017, p. 828). The assets could be a combination of cows, goats, poultry, or tree nurseries. The combination depended on the capability of the participants and local conditions, such as grazing grounds access. About 90% of households received at least one cow. The program considered recipients' preferences, but the BRAC officials made the final selection of assets.

The TUP program also provided a comprehensive livelihood development training program. The goal of the livelihood program is to provide training on the use of productive assets, encourage entrepreneurship, and improve their health, nutrition, social, legal, and political awareness. The TUP program did not explicitly encourage children's schooling to the best of our knowledge. To cover the cost of maintaining assets and deter the beneficiaries from selling the transferred assets, BRAC provided consumption support of about $2/week for 3

[3]See Banerjee et al. (2021) for a more detailed discussion of the motivation for this "big push" idea, and Leibenstein (1958, 1957) and Dasgupta and Ray (1987, 1986) for earlier examinations of the idea of a poverty trap. Dasgupta (2000) argues that the intergenerational aspect of the poverty trap is critical.

to 9 months.[4] The value of this support and the training program was also about $560 per beneficiary woman. Two years after the program's start, in 2009, the women graduated from the TUP program and were left to their own devices.

For evaluation purposes, BRAC used a three-step targeting procedure and a cluster randomized control design to identify the TUP beneficiaries (Bandiera et al., 2017; Raza et al., 2018; Roy et al., 2015). First, they identified the poorest 13 districts in Bangladesh using the World Food Program's poverty map. One or two sub-districts within each district were then randomly selected. Only sub-districts with at least two BRAC branches were included, and a BRAC branch typically emcompasses a few villages.

Next, BRAC officials in the sub-districts identified branches with a significant number of ultra-poor households. BRAC randomly selected two branches within each sub-district, with one branch randomly assigned as treated and the other branch assigned as control. The randomization was done at the branch level, instead of the village level, to limit spillover in effects from the treatment group to the control group. As each branch operates in a 3-mile radius and is on average 8 miles apart from another branch, the risk of spillover from treatment to control groups is low.

Finally, all households in treatment and control villages were ranked by relative wealth. If a household was at the bottom rank, that household was considered the poorest. BRAC then conducted a verification survey to ensure that the poorest households were eligible for the TUP program. A household had to satisfy at least three of the following five inclusion criteria to be eligible for the TUP program: total land owned, including homestead, was less than ten decimals; the household had no productive or income-generating assets; there was no adult male income earner in the household; there were women in the household working outside the homestead; and, there were school-aged children who work for pay. To avoid contaminations with other programs, BRAC excluded households if they met any of

[4]The duration of the consumption support was decided based on the anticipated time for the assets to generate returns.

the three following criteria: if a household member was a microfinance participant; if a household member was a recipient of a government anti-poverty program; or there were no adult women present in the household. All households that met the eligibility criteria were in the sample.

# 3 Data and Estimation Strategy

We use three rounds of survey data collected by BRAC's Research and Evaluation Division (See Bandiera et al., 2017; Roy et al., 2015; and Raza et al., 2018 for more detail). The 2007 baseline survey was collected just before the intervention. The second round coincided with the graduation of the TUP beneficiaries in 2009. The third round was conducted in 2011.[5]

Where available, the primary respondent was the female recipient of the assets in treatment households or the female who would have received the assets for control households. She provided the household roster and educational information for all household members, in addition to information about business activities for herself and other household members, expectations about her children's future, household landholdings and sales, business and financial assets and liabilities, empowerment, vulnerability to shocks, health and mortality across all members, and food and non-food consumption for the household. Furthermore, where available, the male household head was interviewed about his and other household members' business activities, expectations about children's future, and land holdings and sales.

The education information collected differ by age. The highest class completed is asked for all household members who are six or older. Current schooling status is asked for household members aged 5 to 25 years, with the possible responses: never attended, currently attending, stopped attending, and expected to start.[6] If a household member is not currently in school

[5]Both Bandiera et al. (2017) and Raza et al. (2018) show balance at the household level for the baseline survey. In the interest of space, we do not reproduce those results here.

[6]The survey does include some information about relatives living outside the surveyed household, but,

or has never attended school, the survey also asked for the main reason for not attending school.[7] For all household members age six and over, the survey collected information on the female respondent's assessment of the member's ability to read and write a letter and ability to keep accounts, representing numeracy skills.

The survey asks whether individuals currently in school have a private tutor. Private tutors are common in Bangladesh, especially among the middle-income and high-income groups. Therefore, expenditure on private tutors can signal increased educational investments in children.

Although there is no comprehensive daily time-use data in the survey, there is information on market work for all individuals. Data include the number of days worked in three business activities and the typical number of hours per working day on each activity. We create two variables on market work: worked over the last year, where 1 represents work and 0 otherwise, and hours worked over the last year.

Because changes in female empowerment might be a path through which children's education outcomes improve, we create an "economic decision" empowerment measure. We use two versions of this measure. First, because 92 percent report being able to influence either three or four of these decisions, we estimate the probability of reporting influence on all four activities, where the outcome takes the value 1 if the respondent reports influence on all of the following decisions: land buying, house repair, source of borrowing, and taking up a new activity. Second, for comparison with prior studies, we also include the estimate on the Z-score of the empowerment index, which is simply the sum of the individual responses to the four questions.

Finally, there are two direct questions about the primary respondent's ability to influence her children's education. The questions ask separately for sons and daughters, whether she

unfortunately, this does not include education information for sons or daughters who are not in the household.

[7]Respondents could provide two reasons in the 2009 and 2011 rounds, but we only use the first reason given to ensure consistency with the 2007 data.

can affect how far the child proceeds with their studies. This question is generic for sons and daughters rather than for specific children in the household. We create two dummy variables, one for sons and one for daughters. For both, 1 represents being able to influence the child's schooling and 0 otherwise.

## 3.1 Empirical Methodology

We examine the impact of the TUP program on a set of schooling outcomes of children using the experimental variation in this RCT. Following the methodology of Bandiera et al. (2017), we use the following difference-in-difference specification for our estimations:

$$Y_{i,j,t} = \alpha + \sum_{t=1}^{2} \beta_t (W_t \times T_i) + \gamma T_i + \sum_{t=1}^{2} \delta_t W_t + \rho_j + \epsilon_{i,j,t}$$

where $Y$ represents the outcome of interest for individual $i$ in subdistrict $j$ at time $t$, which refers to the 2007 baseline ($t = 0$), the 2009 midline survey ($t = 1$), and the 2011 endline survey ($t = 2$). $T_i$ is a dummy variable for treatment status, where 1 represents the treatment group and 0 represents the control group. $W_t$ represents dummy variables for each survey round. The coefficient of the interaction term $\beta_1$ represents the intent-to-treat effect of the TUP program on individual $i$ in ultra-poor households. It is important to note that the above specification controls for time-varying factors common to the households in treatment and control villages and for all time-invariant heterogeneity within subdistricts. $\gamma_j$ represents subdistrict fixed effects to improve estimation efficiency as the randomization was stratified by subdistrict. The error term $\epsilon$ is clustered at the branch level, the unit of randomization.

The impact on education may vary by age and sex of the child. For age, we separate the analysis into two age groups, younger children aged 6 to 15 and older children aged 16 to 20 at the survey. For each age group, we present results for daughters and sons combined estimates and separately by sex.

There are at least three reasons why the effect of treatment may vary by sex. First, households may have different preferences for investing in daughters' and sons' education. Second, daughters typically marry earlier than sons, and the asset transfer may change when they leave the household. Similarly, older sons may leave the household looking for work elsewhere. Finally, especially among the older children, daughters' and sons' opportunity cost of time may change differentially. The mother's opportunity cost from asset transfers is less likely to spill over to sons' opportunity cost than daughters' opportunity cost. Older sons are already more likely to work full-time in the market than older daughters, and their wage is unlikely to change substantially. Older daughters, however, may spend less time in school for two reasons. They either work more in the market because of the increases in the opportunity cost of female time found in other papers, or they spend more time on household chores because their mothers are working more and changing occupations.

# 4 Main Results

Table 1 presents the results for the likelihood of attending school and years of education completed for children aged 6 to 15 and 16 to 20.[8] The dummies for the survey years capture the overall changes across the survey rounds, and the interactions between survey round and treatment capture the impact of the TUP program two years and four years after the intervention. Finally, the table shows the average outcome for children in the control group in the 2007 survey round.

Reflecting the general upward trend in school attendance in Bangladesh, the likelihood of attending school increased substantially over the three survey rounds. This increase especially benefitted girls. Daughters aged 6 to 15 were 16 percentage points more likely to attend school in 2011 than in 2007, and those aged 16 to 20 were 25 percentage points more likely.

[8] Using different age groups, such as 6–14/15–20, 6–13/14–20, or 6–12/13–20, does not change the results in any meaningful way. The Appendix show results for these other age groups. Appendix Figures A.1 and A.2 show the proportion of children currently enrolled in school and the average years of schooling attained across treated and untreated households by age.

Table 1: The Effects of TUP on School Attendance and Years Completed

| | Age 6 to 15 | | | Age 16 to 20 | | |
|---|---|---|---|---|---|---|
| | All | Daughters | Sons | All | Daughters | Sons |
| | Attends School | | | | | |
| Treatment group | 0.01 | 0.03 | −0.01 | −0.01 | −0.00 | −0.02 |
| | (0.02) | (0.03) | (0.02) | (0.01) | (0.01) | (0.02) |
| 2009 | 0.06 | 0.10 | 0.03 | 0.03 | 0.07 | −0.00 |
| | (0.02) | (0.02) | (0.01) | (0.01) | (0.02) | (0.01) |
| 2011 | 0.13 | 0.16 | 0.10 | 0.13 | 0.25 | 0.06 |
| | (0.02) | (0.03) | (0.02) | (0.02) | (0.04) | (0.02) |
| Treatment×2009 | −0.02 | −0.03 | −0.00 | 0.02 | −0.00 | 0.03 |
| | (0.02) | (0.03) | (0.02) | (0.02) | (0.03) | (0.02) |
| Treatment×2011 | −0.00 | −0.03 | 0.03 | 0.00 | −0.03 | 0.02 |
| | (0.02) | (0.03) | (0.02) | (0.03) | (0.04) | (0.03) |
| Observations | 18,103 | 8,447 | 9,656 | 5,014 | 2,201 | 2,813 |
| Mean Control Group 2007 | 0.62 | 0.65 | 0.60 | 0.08 | 0.07 | 0.08 |
| | Years of School Completed | | | | | |
| Treatment group | −0.02 | −0.07 | 0.03 | 0.02 | 0.11 | −0.12 |
| | (0.07) | (0.09) | (0.08) | (0.17) | (0.22) | (0.20) |
| 2009 | 0.15 | 0.15 | 0.15 | 0.52 | 0.98 | 0.37 |
| | (0.08) | (0.08) | (0.09) | (0.14) | (0.20) | (0.22) |
| 2011 | 0.30 | 0.31 | 0.31 | 0.87 | 1.58 | 0.64 |
| | (0.05) | (0.07) | (0.08) | (0.19) | (0.23) | (0.23) |
| Treatment×2009 | −0.08 | −0.13 | −0.03 | 0.14 | −0.05 | 0.27 |
| | (0.08) | (0.09) | (0.10) | (0.18) | (0.24) | (0.26) |
| Treatment×2011 | −0.02 | −0.09 | 0.04 | 0.09 | −0.26 | 0.37 |
| | (0.07) | (0.09) | (0.11) | (0.26) | (0.31) | (0.27) |
| Observations | 18,099 | 8,444 | 9,655 | 5,013 | 2,200 | 2,813 |
| Mean Control Group 2007 | 1.60 | 1.80 | 1.42 | 2.97 | 3.11 | 2.79 |

**Note.** All models are standard linear models and include dummies for subdistricts. Robust standard errors clustered at branch level in parentheses.

For sons, the increases were smaller at 10 and 6 percentage points increases for those aged 6 to 15 and 16 to 20, respectively.

There is no evidence that receiving assets through the TUP program improves the likelihood of attending school. For the youngest age group, the point estimates are generally negative. The exception is sons in 2011, although that estimated effect is still only 3 percentage points with a standard error of 2 percentage points. The oldest age group shows similar effects, with negative but small point estimates for daughters for both rounds and small but positive effects for sons.

Consistent with the secular increase in the likelihood of attending school, the number of years of schooling completed also went up over the three rounds. The increases in the youngest age group were 0.15 years per two years, with little difference across daughters and sons. For the older group, the increases in years of schooling were substantially larger for daughters than for sons, consistent with the much larger increase in the likelihood of going to school for daughters than for sons. The increase in years of schooling for daughters was 0.97 and 1.57 years for the two rounds, while it is 0.37 and 0.64 years for sons.

Similar to the results on school attendance, there is little evidence that being in an asset recipient household positively affects the years of education completed. For the youngest group, the point estimates are negative except for sons in 2011, who show a 0.04 years increase. For the oldest group, the point estimates are negative for the daughters and positive for the sons, although all have large associated standard errors.

It is possible that while schooling does not increase in the short or medium run, households may intend to send their children to school in the future because of the TUP program. Thus, we examine the impact on the likelihood of either currently attending school or having the intent of going to school and find no evidence of any such effect.[9]

Even in the absence of significant schooling improvements, children may gain other types of

[9]The online Appendix shows the results.

Table 2: The Effects of TUP on Skills

| | Age 6 to 15 | | | Age 16 to 20 | | |
|---|---|---|---|---|---|---|
| | All | Daughters | Sons | All | Daughters | Sons |
| | Can Keep Accounts | | | | | |
| Treatment group | −0.01 | −0.01 | −0.00 | 0.01 | 0.01 | 0.00 |
| | (0.03) | (0.03) | (0.03) | (0.01) | (0.01) | (0.02) |
| 2009 | 0.08 | 0.06 | 0.10 | 0.02 | 0.04 | 0.00 |
| | (0.04) | (0.04) | (0.04) | (0.01) | (0.01) | (0.01) |
| 2011 | 0.08 | 0.06 | 0.09 | 0.01 | 0.01 | −0.00 |
| | (0.04) | (0.03) | (0.04) | (0.01) | (0.02) | (0.01) |
| Treatment×2009 | −0.05 | −0.02 | −0.08 | −0.01 | −0.03 | 0.00 |
| | (0.06) | (0.06) | (0.06) | (0.01) | (0.02) | (0.02) |
| Treatment×2011 | 0.03 | 0.04 | 0.02 | 0.01 | 0.00 | 0.03 |
| | (0.04) | (0.05) | (0.04) | (0.01) | (0.02) | (0.02) |
| Observations | 18,103 | 8,447 | 9,656 | 5,014 | 2,201 | 2,813 |
| Mean Control Group 2007 | 0.58 | 0.60 | 0.56 | 0.96 | 0.94 | 0.97 |
| | Can Read and Write a Letter | | | | | |
| Treatment group | −0.03 | −0.02 | −0.04 | −0.03 | −0.00 | −0.06 |
| | (0.02) | (0.02) | (0.02) | (0.03) | (0.04) | (0.04) |
| 2009 | 0.02 | 0.04 | −0.00 | 0.05 | 0.14 | 0.00 |
| | (0.02) | (0.03) | (0.02) | (0.03) | (0.04) | (0.05) |
| 2011 | 0.05 | 0.07 | 0.03 | 0.09 | 0.18 | 0.05 |
| | (0.02) | (0.02) | (0.02) | (0.04) | (0.04) | (0.05) |
| Treatment×2009 | 0.02 | −0.01 | 0.05 | 0.08 | 0.02 | 0.12 |
| | (0.03) | (0.03) | (0.03) | (0.04) | (0.05) | (0.06) |
| Treatment×2011 | 0.04 | 0.01 | 0.06 | 0.06 | 0.03 | 0.10 |
| | (0.03) | (0.03) | (0.04) | (0.05) | (0.06) | (0.06) |
| Observations | 18,103 | 8,447 | 9,656 | 5,014 | 2,201 | 2,813 |
| Mean Control Group 2007 | 0.22 | 0.25 | 0.20 | 0.42 | 0.43 | 0.41 |

**Note.** All models are standard linear models and include dummies for subdistricts. Robust standard errors clustered at branch level in parentheses.

human capital. Hence, another way to examine potential improvements in human capital from program participation is to look directly at skills acquisition. Table 2 show the results for whether the children can keep accounts (numeracy) and whether they can read and write a letter (literacy).[10] A potential downside of these measures is that they reflect the primary respondent's assessment of her children's skills with no verification by the enumerator.

As for school attendance and years of education, there is no evidence of positive effects on numeracy from participation in the TUP program. The effects on numeracy are either negative or small and positive for both age groups. The one upside is that it does appear that numeracy rates for children aged 6 to 15 in recipient households caught up between 2009 and 2011, rather than falling further behind after the initial decrease in 2009. With numeracy rates for the oldest age group approaching 100% in 2007, it is not surprising that we see little change with program participation or between rounds.

The picture is more complicated for literacy, with some positive program effects on sons but not daughters. While there is no apparent effect for the youngest daughter group, sons aged 6 to 15 in recipient households see increases of 5 and 6 percentage points for 2009 and 2011, respectively. Similarly, daughters experience little effect while sons see 12 and 10 percentage points increases for the oldest age group. However, it is worth pointing out that girls in the oldest age groups see significant secular improvements in literacy rates, with 14 and 18 percentage points increases from 2007 to 2009 and 2011, respectively.

# 5 Why No Effect on Education?

As the TUP program does not significantly impact the children's likelihood of attending school or the total number of years of schooling, this section examines potential explanations for the lack of an effect. We focus first on time spent working and then on investments in

[10] Appendix Figures A.3 and A.4 show the proportion of children who can read and write a letter and who can keep accounts across treated and untreated households by age.

human capital.

If the asset transfer provides better income-generating opportunities for the family, this may affect the use of child labor. The top panel of Table 3 shows the effect of the TUP program on the annual number of hours worked in business activities by children in the two age groups. Unfortunately, there is no information on other time uses, such as household work, so we cannot examine changes in other forms of work, especially non-market work.

Receiving assets does appear to increase the likelihood of engaging in business-related activities, especially in the short run. In the youngest age group, daughters are 17 percentage points and sons 14 percentage points more likely to work two years after the baseline survey. These effects decline for 2011, but daughters of TUP recipients are still 13 percentage points and sons 8 percentage points more likely to work. Similarly, for the oldest daughter group, the likelihood of working increases by 15 and 17 percentage points, respectively, over 2009 and 2011 with the receipts of assets. The effect for sons is more muted, which is not surprising given their high base level of participation in market activities.

We repeat the same analysis for total hours spent on business-related activities over the last year, shown in the middle panel of Table 3. The results are broadly consistent with the likelihood of working results but substantially noisier. The noise likely arises from the phrasing of the business activity questions. The primary female respondent was asked how many days each household member worked across up to three activities during the last year and how many hours they spent on each activity on a typical working day.

The TUP program does cause a significant increase in market work two and four years after baseline. The increased market work of children may partly explain why we do not observe an increase in children's schooling for the TUP households. The caveat is that because we do not have information on other time uses, we cannot establish whether the large increases constitute an increase in total non-school hours spent, a shift from household chores to market activities, or a combination thereof.

Table 3: The Effect of TUP on Working and Tutor Use

| | Age 6 to 15 | | | Age 16 to 20 | | |
|---|---|---|---|---|---|---|
| | All | Daughters | Sons | All | Daughters | Sons |
| | Likelihood of Working Over Last Year | | | | | |
| Treatment group | −0.03 | −0.02 | −0.03 | −0.04 | −0.02 | 0.02 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.04) | (0.02) |
| 2009 | −0.01 | −0.00 | −0.01 | 0.17 | 0.21 | −0.01 |
| | (0.03) | (0.03) | (0.03) | (0.02) | (0.05) | (0.02) |
| 2011 | −0.02 | −0.02 | −0.02 | 0.15 | 0.18 | −0.05 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.06) | (0.03) |
| Treatment×2009 | 0.15 | 0.17 | 0.14 | 0.11 | 0.14 | 0.04 |
| | (0.04) | (0.05) | (0.04) | (0.04) | (0.07) | (0.03) |
| Treatment×2011 | 0.10 | 0.13 | 0.08 | 0.13 | 0.17 | 0.04 |
| | (0.05) | (0.06) | (0.05) | (0.05) | (0.08) | (0.04) |
| Observations | 18,103 | 8,447 | 9,656 | 5,014 | 2,225 | 2,789 |
| Mean Control Group 2007 | 0.22 | 0.18 | 0.25 | 0.57 | 0.31 | 0.87 |
| | Hours Worked Over Last Year | | | | | |
| Treatment group | −61.1 | −51.8 | −72.1 | −143.5 | −48.4 | −88.3 |
| | (24.2) | (30.4) | (32.2) | (56.3) | (61.4) | (78.4) |
| 2009 | −81.5 | −121.7 | −49.6 | 229.3 | 184.7 | −105.4 |
| | (20.5) | (23.7) | (29.4) | (64.3) | (58.6) | (99.8) |
| 2011 | −100.8 | −107.6 | −93.9 | 232.1 | 206.9 | −175.4 |
| | (34.6) | (38.5) | (44.1) | (79.6) | (110.5) | (101.9) |
| Treatment×2009 | 95.9 | 114.8 | 80.7 | 158.6 | 105.2 | 192.4 |
| | (25.6) | (29.2) | (37.8) | (92.2) | (87.0) | (122.1) |
| Treatment×2011 | 57.7 | 58.0 | 55.6 | 276.8 | 120.1 | 280.7 |
| | (41.0) | (47.9) | (50.9) | (109.2) | (137.6) | (140.8) |
| Observations | 18,103 | 8,447 | 9,656 | 5,014 | 2,225 | 2,789 |
| Mean Control Group 2007 | 333.3 | 262.2 | 397.8 | 1,016.7 | 424.0 | 1,715.0 |
| | Uses Private Tutor if Currently in School | | | | | |
| Treatment group | −0.01 | −0.02 | −0.01 | −0.08 | −0.11 | −0.07 |
| | (0.02) | (0.02) | (0.03) | (0.09) | (0.11) | (0.12) |
| 2009 | −0.01 | 0.01 | −0.03 | 0.00 | −0.03 | 0.00 |
| | (0.02) | (0.02) | (0.02) | (0.11) | (0.13) | (0.18) |
| 2011 | 0.09 | 0.10 | 0.08 | 0.01 | −0.11 | 0.11 |
| | (0.02) | (0.02) | (0.03) | (0.11) | (0.10) | (0.16) |
| Treatment×2009 | 0.02 | 0.02 | 0.03 | 0.17 | 0.18 | 0.17 |
| | (0.03) | (0.03) | (0.03) | (0.13) | (0.15) | (0.19) |
| Treatment×2011 | 0.01 | 0.01 | 0.01 | 0.24 | 0.29 | 0.18 |
| | (0.04) | (0.04) | (0.05) | (0.13) | (0.13) | (0.18) |
| Observations | 12,540 | 6,223 | 6,317 | 599 | 302 | 297 |
| Mean Control Group 2007 | 0.15 | 0.13 | 0.16 | 0.28 | 0.31 | 0.26 |

**Note.** All models are standard linear models and include dummies for subdistricts. Robust standard errors clustered at branch level in parentheses.

Despite the substantial increase in the likelihood of working and a null effect on school attendance, there is little effect on skills, suggesting that some other factor may be compensating. The bottom panel in Table 3 shows the effect of being a TUP beneficiary on the likelihood of using a private tutor following the same setup as in the prior analyses but conditioning on being in school. While there are no discernable effects for the youngest age group, the oldest daughters are substantially more likely to have a private tutor at 18 and 29 percentage points for the two rounds. The effects are 17 and 18 percentage points for the oldest sons, but these estimates are very noisy. However, an important caveat to these results is the small sample sizes of only 302 daughters and 297 sons or only about 100 per round. The results on tutoring are in line with significantly higher expenditures on education for treated than untreated households in 2011.[11]

As mentioned, an implicit motivation for programs like the BRAC TUP program is to increase women's bargaining power and, thereby, positively influence future outcomes for children. We, therefore, present the effects of the program on women's empowerment in Table 4, focusing on the women's ability to influence economic decisions and the progression of her children's studies.[12] The sample is women with children, although they do not have to be school-aged. The top panel shows results for all women, while the bottom panel shows results for women living in a household with a male head. Conditioning on having children in school does not substantially change the results below (estimates are available upon request)

The economic decisions include the timing of land buying, the timing of house repairs, the source of borrowing, and the respondent taking up a new activity. There is some evidence that women who receive assets have increased economic power in 2009, where the program's effect is a 15 percentage points increase in the probability of being able to influence all the decisions listed above. However, this effect was substantially lower in 2011 at only

[11]The results are available upon request.

[12]An alternative approach is to sum across all twelve empowerment questions asked in the survey and calculate a Z-score. The effect of treatment in 2011 is then an increase of 0.077 standard deviations with a standard error of 0.056 (Bandiera et al., 2017, Table VI and Online Appendix).

Table 4: The Effects of TUP on Women's Empowerment

| | Influence Decisions on: | | | |
|---|---|---|---|---|
| | Economic Decisions | | Studies for:[c] | |
| | Prob(All)[a] | Z-score[b] | Daughters | Sons |
| | Any Household Head | | | |
| Treatment group | −0.07 | −0.07 | 0.05 | 0.03 |
| | (0.04) | (0.06) | (0.06) | (0.06) |
| 2009 | 0.59 | 0.70 | −0.00 | −0.07 |
| | (0.06) | (0.09) | (0.08) | (0.07) |
| 2011 | 0.26 | 0.31 | 0.07 | 0.08 |
| | (0.06) | (0.11) | (0.07) | (0.06) |
| Treatment×2009 | 0.15 | 0.18 | 0.00 | 0.01 |
| | (0.07) | (0.12) | (0.09) | (0.08) |
| Treatment×2011 | 0.06 | 0.11 | −0.01 | −0.03 |
| | (0.09) | (0.14) | (0.08) | (0.07) |
| Observations | 16,231 | 16,231 | 8,452 | 9,389 |
| Mean Control Group 2007 | 0.17 | −0.36 | 0.77 | 0.77 |
| | Male Household Head | | | |
| Treatment group | −0.01 | 0.02 | 0.03 | 0.01 |
| | (0.04) | (0.08) | (0.06) | (0.06) |
| 2009 | 0.69 | 0.85 | 0.01 | −0.06 |
| | (0.06) | (0.11) | (0.08) | (0.07) |
| 2011 | 0.28 | 0.33 | 0.05 | 0.04 |
| | (0.07) | (0.15) | (0.07) | (0.06) |
| Treatment×2009 | 0.11 | 0.11 | 0.02 | 0.04 |
| | (0.08) | (0.14) | (0.10) | (0.08) |
| Treatment×2011 | 0.08 | 0.14 | 0.01 | 0.03 |
| | (0.09) | (0.18) | (0.08) | (0.07) |
| Observations | 11,709 | 11,709 | 6,689 | 7,228 |
| Mean Control Group 2007 | 0.04 | −0.57 | 0.79 | 0.81 |

**Note.** All models are standard linear models and include dummies for sub-districts using only women with children. Robust standard errors clustered at branch level in parentheses.

[a] Takes the value 1 if the respondent report influence on all of the following decisions: land buying, house repair, source of borrowing, and taking up a new activity, and 0 otherwise.

[b] The Z-score of the sum of the following decisions: land buying, house repair, source of borrowing, and taking up a new activity, where each takes the value 1 if the respondent report influence and 0 otherwise.

[c] The question is "Can you influence the decision on how far your son/daughter proceeds with his/her studies?" Takes the value 1 if the respondent answers yes.

just over 6 percentage points. The background for these results is a large general increase in empowerment between 2007 and 2009, although the change between 2007 and 2011 is somewhat smaller. Using the Z-score index, treatment leads to increases of 0.18 and 0.11 standard deviations for 2009 and 2011, although the standard errors are large at 0.12 and 0.14. Conditioning on having a male household head does not qualitatively change these results.

The education questions ask whether respondents, who have sons(s)/daughter(s) currently in school, can influence how far their son/daughter proceeds with their studies. Despite the positive impact of the TUP program on general economic empowerment, there is no evidence of any effect on women's ability to influence how long their children continue with their studies. The point estimates are uniformly close to zero and even negative for 2011, although the standard errors are substantial. Part of the explanation may be the relatively high initial ability to affect children's schooling. In the baseline survey, 77 percent of the control group women report being able to influence their children's schooling. The results are marginally better in male-headed households but still very small.

Another approach to understanding why we do not observe any treatment effects on children's education is to examine the motivation for not attending school, which we do in Table 5. We present three different possible motivations. First, in the top panel, the motivation is that the household either cannot afford education for the child or that the child has to work either at home or in the market. In the middle panel, the motivation is that the child does not want to study, and in the bottom panel, that marriage interfered with attending school.

In line with the increased use of tutoring and higher expenditures on education we discuss above, treated households are less likely than untreated households to report cost or work-related reasons for their children not attending school. The point estimates are almost uniformly negative, with the reductions in the likelihood of reporting cost or work as a reason for not being in school between 6 and 10 percentage points in the last round. Again,

Table 5: The Effects of TUP on Reason for Not in School

| | Age 6 to 15 | | | Age 16 to 20 | | |
|---|---|---|---|---|---|---|
| | All | Daughters | Sons | All | Daughters | Sons |
| | Due to Cost/Work Related | | | | | |
| Treatment group | 0.01 | −0.00 | 0.01 | −0.03 | −0.01 | −0.02 |
| | (0.03) | (0.04) | (0.04) | (0.04) | (0.04) | (0.05) |
| 2009 | −0.08 | −0.12 | −0.05 | 0.03 | 0.02 | −0.00 |
| | (0.04) | (0.04) | (0.05) | (0.03) | (0.05) | (0.04) |
| 2011 | −0.11 | −0.17 | −0.08 | 0.04 | 0.06 | −0.01 |
| | (0.05) | (0.05) | (0.06) | (0.05) | (0.08) | (0.06) |
| Treatment×2009 | −0.06 | −0.00 | −0.09 | −0.03 | 0.01 | −0.05 |
| | (0.05) | (0.06) | (0.07) | (0.06) | (0.08) | (0.06) |
| Treatment×2011 | −0.09 | −0.07 | −0.10 | −0.06 | −0.09 | −0.07 |
| | (0.06) | (0.07) | (0.07) | (0.06) | (0.09) | (0.07) |
| Observations | 5,563 | 2,205 | 3,358 | 4,415 | 1,902 | 2,513 |
| Mean Control Group 2007 | 0.53 | 0.57 | 0.51 | 0.68 | 0.63 | 0.74 |
| | Does Not Want to Study | | | | | |
| Treatment group | 0.00 | 0.03 | −0.02 | −0.00 | −0.01 | 0.01 |
| | (0.03) | (0.03) | (0.04) | (0.04) | (0.04) | (0.05) |
| 2009 | 0.01 | 0.05 | −0.03 | 0.02 | 0.03 | 0.00 |
| | (0.04) | (0.03) | (0.05) | (0.04) | (0.05) | (0.04) |
| 2011 | 0.20 | 0.28 | 0.14 | 0.02 | 0.02 | −0.01 |
| | (0.04) | (0.04) | (0.06) | (0.06) | (0.08) | (0.06) |
| Treatment×2009 | 0.02 | −0.04 | 0.06 | 0.06 | 0.04 | 0.07 |
| | (0.05) | (0.05) | (0.07) | (0.06) | (0.07) | (0.06) |
| Treatment×2011 | 0.05 | −0.00 | 0.09 | 0.11 | 0.11 | 0.10 |
| | (0.06) | (0.06) | (0.07) | (0.07) | (0.09) | (0.07) |
| Observations | 5,563 | 2,205 | 3,358 | 4,415 | 1,902 | 2,513 |
| Mean Control Group 2007 | 0.26 | 0.20 | 0.32 | 0.20 | 0.18 | 0.24 |
| | Due to Marriage | | | | | |
| Treatment group | −0.00 | −0.01 | 0.00 | 0.02 | 0.01 | −0.00 |
| | (0.00) | (0.01) | (0.00) | (0.02) | (0.03) | (0.01) |
| 2009 | 0.00 | −0.00 | 0.01 | −0.05 | −0.04 | −0.01 |
| | (0.00) | (0.01) | (0.00) | (0.02) | (0.04) | (0.00) |
| 2011 | −0.00 | −0.00 | 0.00 | −0.08 | −0.11 | −0.00 |
| | (0.00) | (0.01) | (0.00) | (0.02) | (0.03) | (0.01) |
| Treatment×2009 | −0.00 | 0.00 | −0.00 | −0.02 | −0.04 | 0.00 |
| | (0.01) | (0.01) | (0.00) | (0.02) | (0.05) | (0.01) |
| Treatment×2011 | 0.00 | 0.00 | 0.00 | −0.02 | −0.00 | 0.00 |
| | (0.00) | (0.01) | (0.00) | (0.02) | (0.04) | (0.01) |
| Observations | 5,563 | 2,205 | 3,358 | 4,415 | 1,902 | 2,513 |
| Mean Control Group 2007 | 0.01 | 0.01 | 0.00 | 0.09 | 0.17 | 0.01 |

**Note.** All models are standard linear models and include dummies for subdistricts. Robust standard errors clustered at branch level in parentheses.

however, the standard errors are large.

While economic constraints on schooling appear to ease with treatment, there is a corresponding increase in the likelihood of reporting that the child does not want to study with treatment. For children not in school in 2011, treated households are about 10 percentage points more likely to report that the child does not want to study for both daughters aged 16 to 20 and sons no matter the age, while there is no difference for the youngest daughters. As for the cost/work-related motivation, the standard errors are about the size of the point estimates.

We expect that marriage would impede schooling for the oldest age group daughters, with 17 percent of the control group reporting this as the motivation for their older daughters not being in school in 2007. There is, however, no discernible difference between treated and untreated households for this motivation, possibly because there has been a general decrease in this motivation over time.

Fertility effects could also explain the results. Just as the theoretical effect on child schooling, the predicted effect on fertility is ambiguous. On the one hand, the changes in employment type and the associated higher opportunity cost of time should lead to lower fertility. On the other, the income effect from treatment on fertility is likely positive. Furthermore, even if there is no change in overall fertility, the positive economic shock could accelerate fertility timing, thereby affecting the older children's education (Alam and Pörtner, 2018). However, we find little evidence of an effect on fertility from treatment, with treated women having between 0 and 0.07 children more depending on specification (results shown in Appendix Table A.4). Although young women—those aged 15 to 20 in the baseline—do appear to have more births, all other age groups up to age 40 show negative treatment effects.

The final question is whether the selection effects discussed above might drive the results. A first indication that this is unlikely to be the case is that the average age differs little across rounds or between treatment and control groups within a round (see Appendix Table A.5).

Second, the proportion of children in treatment households relative to control remains close to constant across the three waves, as shown in Appendix Figures A.5 and A.6. The only group with any tangible movement is the oldest females, where the proportion treated first increases and then decreases. However, even here, the change is 3 percentage points or less and not statistically significant in a two-sample test of proportion.

We also examined whether there is a relationship between treatment status and the absence of information on children's schooling. This absence may, for example, be because they have left the household or their mother simply did not provide the information. Generally, treatment status is associated with a higher likelihood that we have information about children's schooling (Appendix Table A.6 shows the results). However, the point estimates are small, especially for the youngest group. The one exception is for the group of oldest boys, where treatment is associated with a 4.5 percentage points increase in the likelihood of education data available, although the standard error is 3.1 percentage points. Considering the number of the oldest sons and daughters that leave the households as part of life events such as marriage, the difference across treatment status does not appear to explain the education results.

# 6 Conclusion

Despite the various positive effects of TUP programs documented in the literature, we find no effects of receiving assets on children's schooling or skills. If anything, the effects on the likelihood of attending school and the years of school completed may even be negative, especially for daughters. The most likely explanation for these results is that the value of children's time increases with asset transfers, consistent with economic theory. The likelihood of working increases substantially for daughters, especially the oldest, as do the hours worked, although those results are noisy.

Interestingly, treated households invest more in their children, as evidenced by the increasing

use of private tutors for children still in school. However, there appears to be no effect on the children's reported skill levels, except some improvements in literacy among boys. Consistent with the increased use of tutoring, treated households are much less likely to report economic motivations for their children not being in school. The flipside is that the increase in the proportion of treated households reporting that their children do not want to study as the motivation for them not going to school more than matches the decline in the economic motivation for not going to school.

The absence of positive effects on schooling does not preclude positive intergenerational effects of TUP programs, as evidenced by the better health outcomes for children discussed above. As shown by prior research, the return to working has increased for the mothers. Hence, it is possible that what we observe is an optimal response to this increased return to working with parents and children expecting a higher return to labor market participation and responding by investing more in labor market skills (Beegle et al., 2009; Rosenzweig and Schultz, 1982). This explanation would be consistent with the increased likelihood of working and the changing motivation for not attending school.

Finally, the secular increases in schooling over time, especially for older girls, is encouraging, even if the TUP program did not appear to contribute. Understanding the contributing factors behind this increase in education for children of ultra-poor mothers is an important future area of research.

# Appendix

Table A.1: The Effects of TUP on Schooling Intent

| | Age 6 to 15 | | | Age 16 to 20 | | |
|---|---|---|---|---|---|---|
| | All | Daughters | Sons | All | Daughters | Sons |
| | Attends School or Intents to Attend | | | | | |
| Treatment group | 0.01 | 0.02 | −0.01 | −0.01 | −0.00 | −0.02 |
| | (0.01) | (0.02) | (0.02) | (0.01) | (0.01) | (0.02) |
| 2009 | 0.06 | 0.09 | 0.03 | 0.03 | 0.07 | −0.00 |
| | (0.01) | (0.02) | (0.01) | (0.01) | (0.02) | (0.01) |
| 2011 | 0.13 | 0.16 | 0.11 | 0.13 | 0.25 | 0.06 |
| | (0.01) | (0.02) | (0.02) | (0.02) | (0.04) | (0.02) |
| Treatment×2009 | −0.01 | −0.02 | 0.01 | 0.02 | −0.00 | 0.03 |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.03) | (0.02) |
| Treatment×2011 | 0.01 | −0.01 | 0.02 | 0.00 | −0.03 | 0.02 |
| | (0.02) | (0.02) | (0.02) | (0.03) | (0.04) | (0.03) |
| Observations | 18,111 | 8,450 | 9,661 | 5,019 | 2,202 | 2,817 |
| Mean Control Group 2007 | 0.68 | 0.71 | 0.66 | 0.08 | 0.07 | 0.08 |

**Note.** All models are standard linear models and include dummies for subdistricts. Robust standard errors clustered at branch level in parentheses.

Table A.2: The Effects of TUP on School Attendance with Different Age Groups

| | Attends School | | | | | |
|---|---|---|---|---|---|---|
| | All | Daughters | Sons | All | Daughters | Sons |
| | Age 6 to 14 | | | Age 15 to 20 | | |
| Treatment group | 0.01 | 0.02 | −0.00 | −0.00 | 0.02 | −0.02 |
| | (0.02) | (0.03) | (0.02) | (0.01) | (0.02) | (0.01) |
| 2009 | 0.04 | 0.08 | 0.01 | 0.05 | 0.11 | 0.01 |
| | (0.02) | (0.03) | (0.01) | (0.01) | (0.02) | (0.01) |
| 2011 | 0.11 | 0.15 | 0.09 | 0.13 | 0.25 | 0.06 |
| | (0.02) | (0.03) | (0.01) | (0.02) | (0.03) | (0.02) |
| Treatment×2009 | −0.01 | −0.02 | 0.00 | −0.01 | −0.05 | 0.02 |
| | (0.02) | (0.03) | (0.02) | (0.02) | (0.03) | (0.02) |
| Treatment×2011 | −0.00 | −0.03 | 0.02 | −0.01 | −0.03 | 0.01 |
| | (0.02) | (0.03) | (0.02) | (0.02) | (0.04) | (0.02) |
| Observations | 16,977 | 7,928 | 9,049 | 6,153 | 2,724 | 3,429 |
| Mean Control Group 2007 | 0.66 | 0.68 | 0.64 | 0.11 | 0.11 | 0.10 |
| | Age 6 to 13 | | | Age 14 to 20 | | |
| Treatment group | 0.01 | 0.02 | −0.01 | −0.01 | 0.01 | −0.02 |
| | (0.02) | (0.03) | (0.02) | (0.01) | (0.02) | (0.02) |
| 2009 | 0.06 | 0.09 | 0.03 | 0.08 | 0.16 | 0.04 |
| | (0.02) | (0.02) | (0.01) | (0.01) | (0.02) | (0.01) |
| 2011 | 0.13 | 0.15 | 0.11 | 0.17 | 0.30 | 0.10 |
| | (0.02) | (0.02) | (0.01) | (0.02) | (0.04) | (0.02) |
| Treatment×2009 | −0.02 | −0.03 | −0.00 | −0.01 | −0.04 | 0.01 |
| | (0.02) | (0.03) | (0.02) | (0.01) | (0.03) | (0.02) |
| Treatment×2011 | −0.01 | −0.03 | 0.01 | 0.01 | −0.01 | 0.03 |
| | (0.02) | (0.03) | (0.02) | (0.03) | (0.05) | (0.03) |
| Observations | 15,151 | 7,145 | 8,006 | 7,979 | 3,507 | 4,472 |
| Mean Control Group 2007 | 0.69 | 0.71 | 0.67 | 0.14 | 0.15 | 0.13 |
| | Age 6 to 12 | | | Age 13 to 20 | | |
| Treatment group | 0.01 | 0.03 | 0.00 | −0.01 | 0.00 | −0.01 |
| | (0.02) | (0.03) | (0.02) | (0.01) | (0.02) | (0.01) |
| 2009 | 0.05 | 0.08 | 0.03 | 0.08 | 0.16 | 0.03 |
| | (0.02) | (0.02) | (0.02) | (0.01) | (0.02) | (0.02) |
| 2011 | 0.12 | 0.14 | 0.11 | 0.17 | 0.30 | 0.11 |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.04) | (0.02) |
| Treatment×2009 | −0.02 | −0.04 | −0.01 | −0.01 | −0.03 | 0.00 |
| | (0.02) | (0.03) | (0.02) | (0.02) | (0.03) | (0.02) |
| Treatment×2011 | −0.01 | −0.03 | 0.01 | 0.01 | −0.01 | 0.02 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.04) | (0.03) |
| Observations | 14,013 | 6,594 | 7,419 | 9,117 | 4,058 | 5,059 |
| Mean Control Group 2007 | 0.70 | 0.72 | 0.69 | 0.18 | 0.20 | 0.17 |

**Note.** All models are standard linear models and include dummies for subdistricts. Robust standard errors clustered at branch level in parentheses.

Table A.3: The Effects of TUP on Years of Schooling with Different Age Groups

| | Years of School Completed | | | | | |
|---|---|---|---|---|---|---|
| | All | Daughters | Sons | All | Daughters | Sons |
| | Age 6 to 14 | | | Age 15 to 20 | | |
| Treatment group | −0.01 | −0.05 | 0.03 | −0.03 | 0.01 | −0.12 |
| | (0.07) | (0.08) | (0.08) | (0.15) | (0.18) | (0.17) |
| 2009 | 0.16 | 0.16 | 0.16 | 0.56 | 1.00 | 0.45 |
| | (0.08) | (0.08) | (0.09) | (0.10) | (0.17) | (0.15) |
| 2011 | 0.34 | 0.38 | 0.32 | 0.81 | 1.36 | 0.69 |
| | (0.05) | (0.07) | (0.07) | (0.17) | (0.19) | (0.21) |
| Treatment×2009 | −0.10 | −0.12 | −0.09 | 0.14 | −0.12 | 0.30 |
| | (0.08) | (0.09) | (0.10) | (0.15) | (0.21) | (0.19) |
| Treatment×2011 | −0.02 | −0.10 | 0.04 | 0.14 | −0.15 | 0.38 |
| | (0.07) | (0.08) | (0.11) | (0.23) | (0.27) | (0.24) |
| Observations | 16,973 | 7,925 | 9,048 | 6,152 | 2,723 | 3,429 |
| Mean Control Group 2007 | 1.46 | 1.60 | 1.32 | 3.04 | 3.32 | 2.72 |
| | Age 6 to 13 | | | Age 14 to 20 | | |
| Treatment group | −0.02 | −0.06 | 0.02 | 0.00 | 0.03 | −0.04 |
| | (0.07) | (0.08) | (0.08) | (0.15) | (0.19) | (0.15) |
| 2009 | 0.06 | 0.06 | 0.06 | 0.47 | 0.79 | 0.40 |
| | (0.09) | (0.09) | (0.10) | (0.11) | (0.16) | (0.12) |
| 2011 | 0.17 | 0.19 | 0.15 | 0.76 | 1.25 | 0.63 |
| | (0.05) | (0.06) | (0.08) | (0.16) | (0.20) | (0.17) |
| Treatment×2009 | −0.07 | −0.09 | −0.06 | 0.12 | −0.03 | 0.21 |
| | (0.09) | (0.09) | (0.11) | (0.16) | (0.21) | (0.16) |
| Treatment×2011 | 0.00 | −0.06 | 0.07 | 0.07 | −0.21 | 0.28 |
| | (0.08) | (0.08) | (0.11) | (0.20) | (0.26) | (0.19) |
| Observations | 15,147 | 7,142 | 8,005 | 7,978 | 3,506 | 4,472 |
| Mean Control Group 2007 | 1.33 | 1.47 | 1.21 | 3.02 | 3.32 | 2.71 |
| | Age 6 to 12 | | | Age 13 to 20 | | |
| Treatment group | −0.04 | −0.04 | −0.03 | −0.00 | −0.02 | 0.01 |
| | (0.07) | (0.08) | (0.08) | (0.13) | (0.16) | (0.14) |
| 2009 | 0.09 | 0.11 | 0.08 | 0.41 | 0.71 | 0.35 |
| | (0.07) | (0.06) | (0.09) | (0.12) | (0.16) | (0.12) |
| 2011 | 0.18 | 0.23 | 0.13 | 0.66 | 1.04 | 0.58 |
| | (0.05) | (0.07) | (0.08) | (0.14) | (0.19) | (0.14) |
| Treatment×2009 | −0.06 | −0.10 | −0.02 | 0.10 | −0.03 | 0.17 |
| | (0.07) | (0.07) | (0.10) | (0.15) | (0.20) | (0.15) |
| Treatment×2011 | 0.00 | −0.10 | 0.10 | 0.07 | −0.13 | 0.22 |
| | (0.08) | (0.09) | (0.11) | (0.17) | (0.24) | (0.16) |
| Observations | 14,009 | 6,591 | 7,418 | 9,116 | 4,057 | 5,059 |
| Mean Control Group 2007 | 1.17 | 1.26 | 1.09 | 3.06 | 3.37 | 2.73 |

**Note.** All models are standard linear models and include dummies for subdistricts. Robust standard errors clustered at branch level in parentheses.

Table A.4: Impact of TUP Program on Total Births

| Outcome variable: | | Ages | | | | | |
|---|---|---|---|---|---|---|---|
| Total births | 15–45 | 15–20 | 21–25 | 26–30 | 31–35 | 36–40 | 41–45 |
| | All Women | | | | | | |
| Treatment group | −0.03 | −0.30 | −0.05 | 0.10 | 0.35 | 0.04 | 0.06 |
| | (0.05) | (0.08) | (0.07) | (0.08) | (0.10) | (0.11) | (0.15) |
| 2009 | 0.16 | 0.45 | 0.30 | 0.23 | 0.20 | −0.03 | −0.01 |
| | (0.03) | (0.08) | (0.06) | (0.05) | (0.05) | (0.09) | (0.10) |
| 2011 | 0.27 | 0.64 | 0.51 | 0.34 | 0.14 | 0.16 | −0.07 |
| | (0.04) | (0.08) | (0.07) | (0.07) | (0.07) | (0.08) | (0.12) |
| Treatment×2009 | 0.07 | 0.13 | −0.01 | 0.00 | −0.05 | 0.08 | 0.26 |
| | (0.04) | (0.09) | (0.08) | (0.06) | (0.08) | (0.11) | (0.13) |
| Treatment×2011 | 0.07 | 0.36 | −0.03 | −0.02 | −0.02 | −0.08 | 0.24 |
| | (0.05) | (0.12) | (0.09) | (0.08) | (0.09) | (0.10) | (0.16) |
| Observations | 13,352 | 1,107 | 2,027 | 3,614 | 2,288 | 2,453 | 1,859 |
| Mean Control Group 2007 | 2.9 | 1.3 | 2.0 | 2.8 | 3.7 | 3.9 | 4.4 |
| | Married Women | | | | | | |
| Treatment group | −0.08 | −0.29 | −0.06 | 0.08 | 0.22 | 0.03 | −0.22 |
| | (0.06) | (0.07) | (0.07) | (0.09) | (0.10) | (0.14) | (0.22) |
| 2009 | 0.26 | 0.48 | 0.30 | 0.24 | 0.23 | 0.15 | 0.02 |
| | (0.04) | (0.07) | (0.06) | (0.06) | (0.06) | (0.16) | (0.13) |
| 2011 | 0.37 | 0.67 | 0.53 | 0.35 | 0.21 | 0.33 | −0.15 |
| | (0.05) | (0.07) | (0.08) | (0.09) | (0.08) | (0.12) | (0.15) |
| Treatment×2009 | 0.00 | 0.06 | −0.00 | −0.00 | −0.07 | −0.13 | 0.23 |
| | (0.05) | (0.08) | (0.08) | (0.07) | (0.09) | (0.18) | (0.17) |
| Treatment×2011 | 0.01 | 0.21 | −0.03 | 0.00 | −0.07 | −0.32 | 0.29 |
| | (0.06) | (0.09) | (0.09) | (0.10) | (0.10) | (0.14) | (0.21) |
| Observations | 9,431 | 1,021 | 1,833 | 2,916 | 1,480 | 1,365 | 816 |
| Mean Control Group 2007 | 2.9 | 1.3 | 2.0 | 2.8 | 3.7 | 3.9 | 4.4 |

**Note.** Robust standard errors clustered at branch level in parentheses; Age is at baseline.

Table A.5: Average Age by Round and Treatment Status

| | Age 6 to 15 | | Age 16 to 20 | |
|---|---|---|---|---|
| | Control | Treatment | Control | Treatment |
| | Schooling Variables | | | |
| 2007 | 10.0 | 10.0 | 18.0 | 18.2 |
| 2009 | 10.2 | 10.1 | 17.8 | 17.9 |
| 2011 | 10.5 | 10.4 | 17.6 | 17.6 |
| | Work Variables | | | |
| 2007 | 10.0 | 10.0 | 18.0 | 18.2 |
| 2009 | 10.2 | 10.1 | 17.8 | 17.9 |
| 2011 | 10.5 | 10.4 | 17.6 | 17.6 |
| | Tutoring Variable | | | |
| 2007 | 9.4 | 9.4 | 17.0 | 16.9 |
| 2009 | 9.8 | 9.7 | 17.1 | 17.1 |
| 2011 | 10.2 | 10.2 | 17.0 | 17.2 |
| | School Absence Variables | | | |
| 2007 | 11.0 | 11.1 | 18.1 | 18.3 |
| 2009 | 11.0 | 10.9 | 17.9 | 18.0 |
| 2011 | 11.1 | 11.1 | 17.8 | 17.7 |

Table A.6: The Effects of TUP on Absence of School Information for Children

| | Age 6 to 15 | | | Age 16 to 20 | | |
|---|---|---|---|---|---|---|
| | All | Daughters | Sons | All | Daughters | Sons |
| Treatment group | −0.001 | −0.003 | 0.001 | 0.000 | −0.006 | 0.007 |
| | (0.004) | (0.007) | (0.004) | (0.012) | (0.020) | (0.013) |
| 2009 | 0.063 | 0.096 | 0.032 | 0.267 | 0.438 | 0.115 |
| | (0.009) | (0.012) | (0.010) | (0.026) | (0.038) | (0.024) |
| 2011 | 0.067 | 0.084 | 0.051 | 0.329 | 0.533 | 0.157 |
| | (0.010) | (0.012) | (0.012) | (0.020) | (0.026) | (0.030) |
| Treatment×2009 | −0.008 | −0.012 | −0.003 | −0.026 | −0.058 | −0.015 |
| | (0.010) | (0.014) | (0.011) | (0.027) | (0.043) | (0.027) |
| Treatment×2011 | −0.009 | 0.003 | −0.020 | −0.019 | −0.006 | −0.045 |
| | (0.011) | (0.014) | (0.013) | (0.025) | (0.038) | (0.031) |
| Observations | 18,922 | 9,001 | 9,921 | 6,326 | 3,218 | 3,108 |
| Mean Control Group 2007 | 0.005 | 0.005 | 0.005 | 0.010 | 0.013 | 0.006 |

**Note.** All models are standard linear models and include dummies for subdistricts. Robust standard errors clustered at branch level in parentheses.

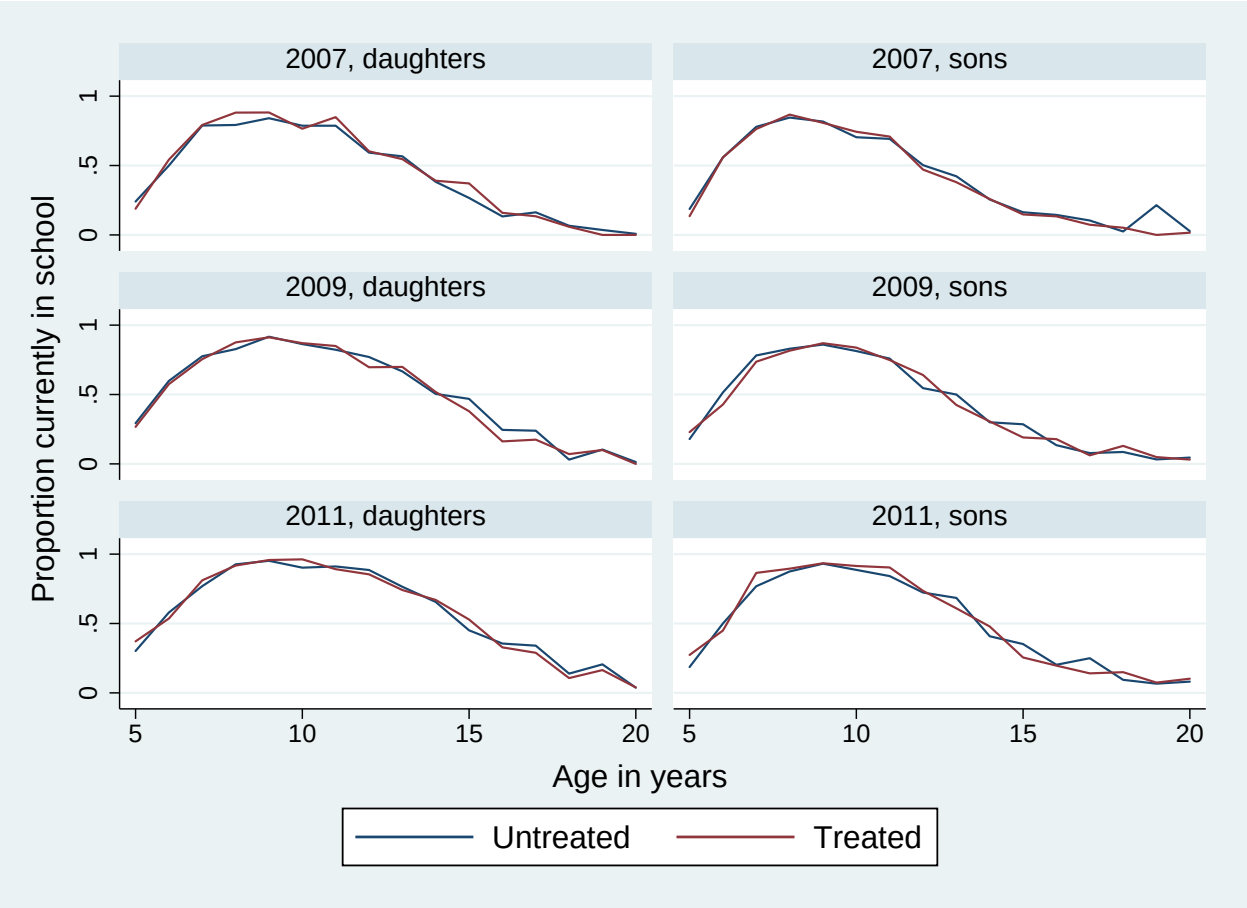


Figure A.1: Proportion of children currently attending school by survey round, sex, age, and treatment status

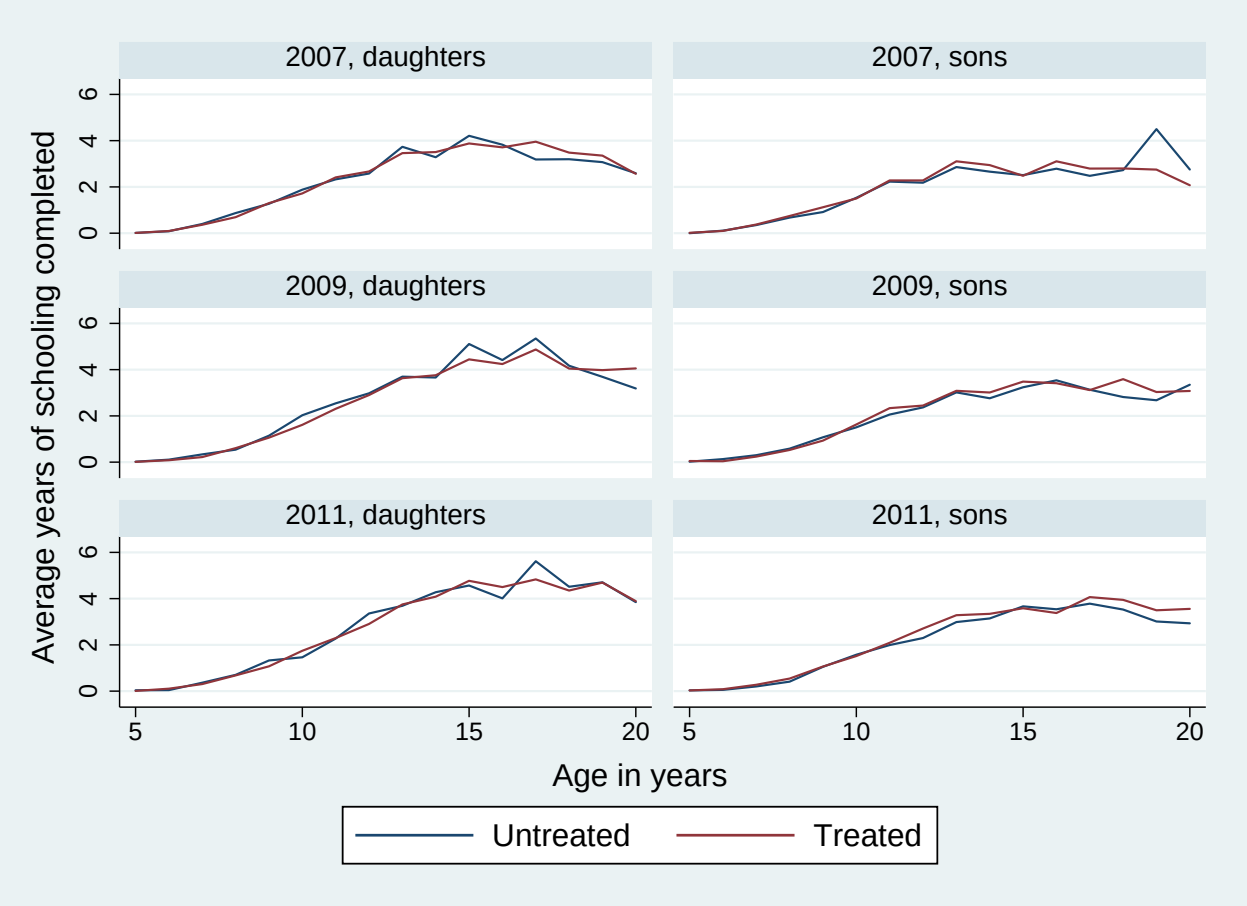


Figure A.2: Average years of schooling by survey round, sex, age, and treatment status

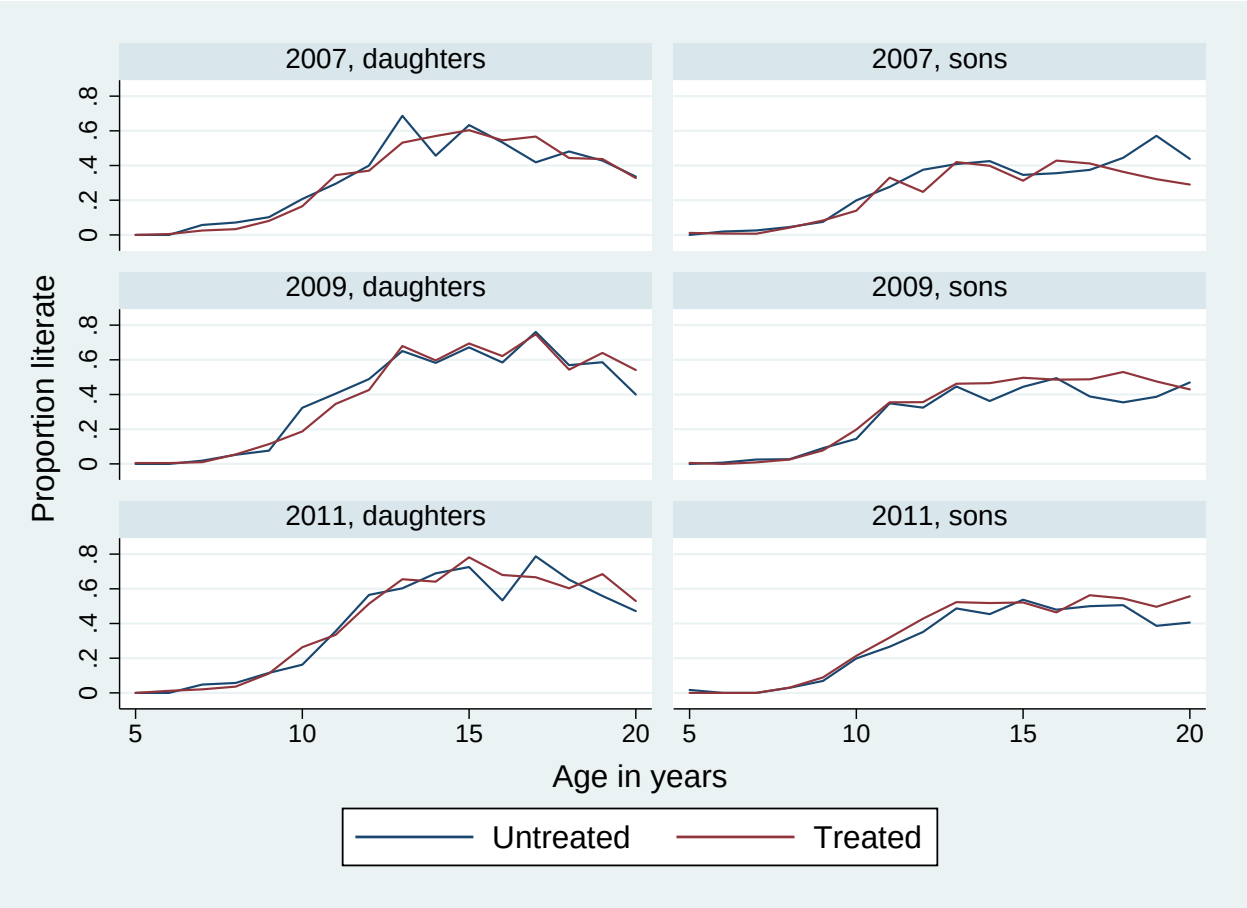


Figure A.3: Proportion of children who can read and write a letter by survey round, sex, age, and treatment status

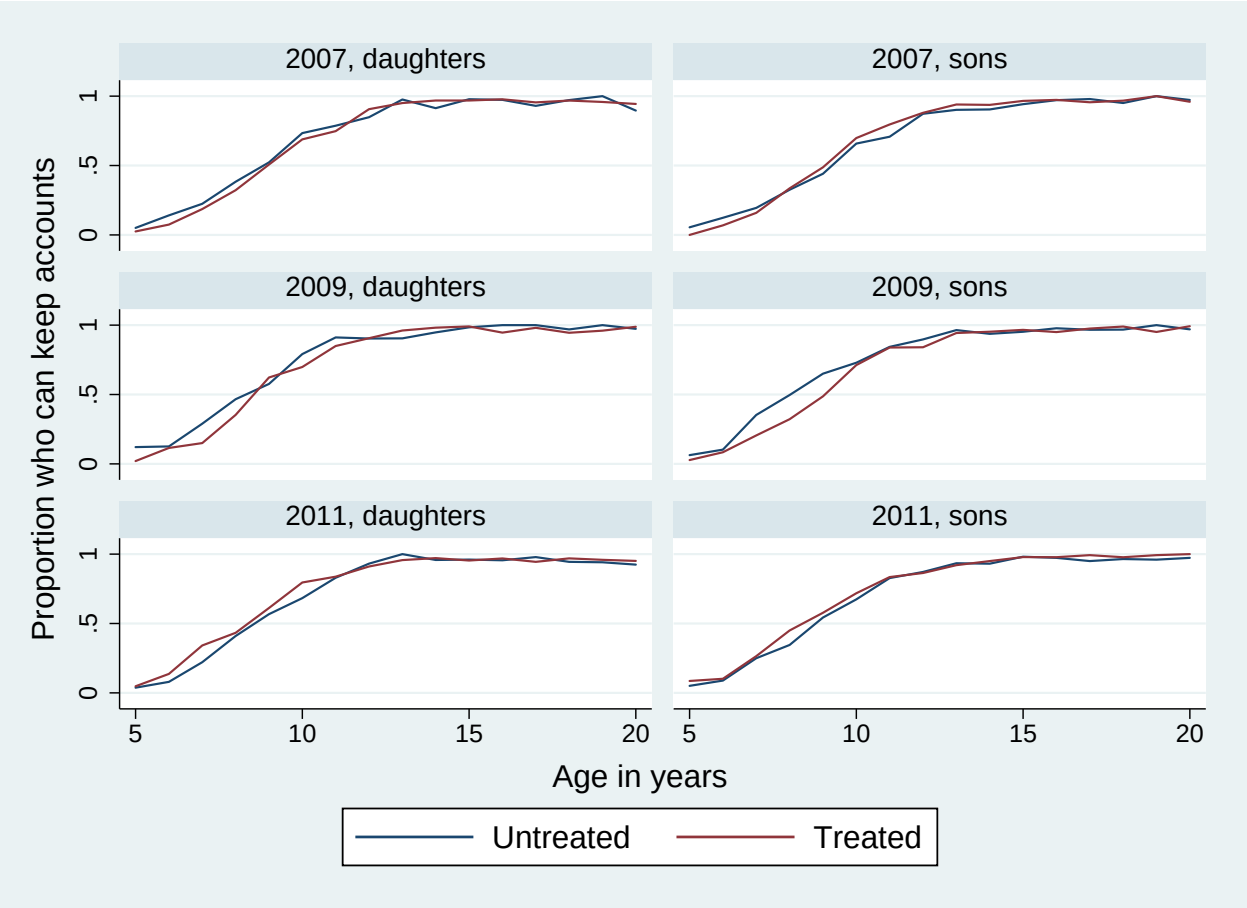


Figure A.4: Proportion of children who can keep accounts by survey round, sex, age, and treatment status

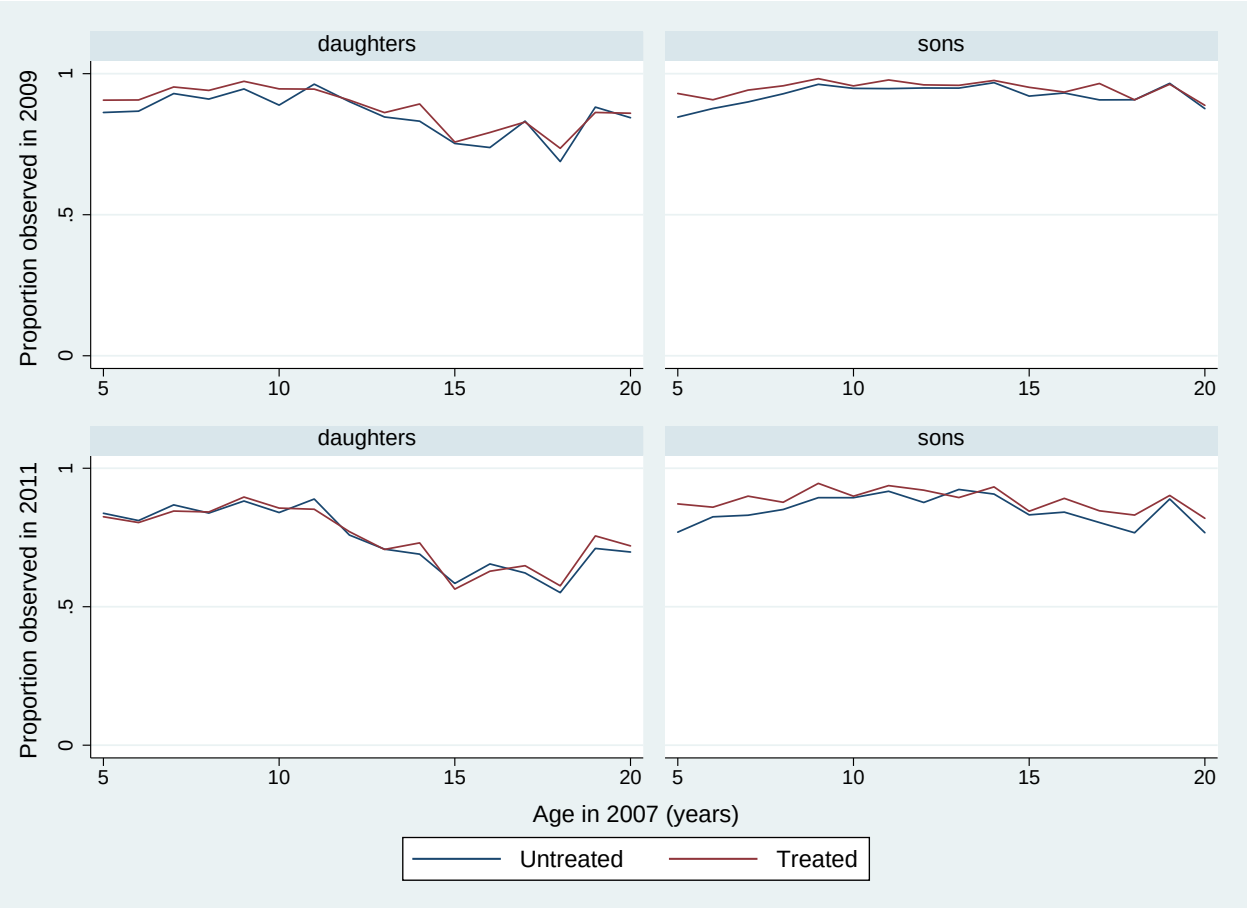


Figure A.5: Proportion of children observed in 2007 who are observed in subsequent rounds by sex, age, and treatment status

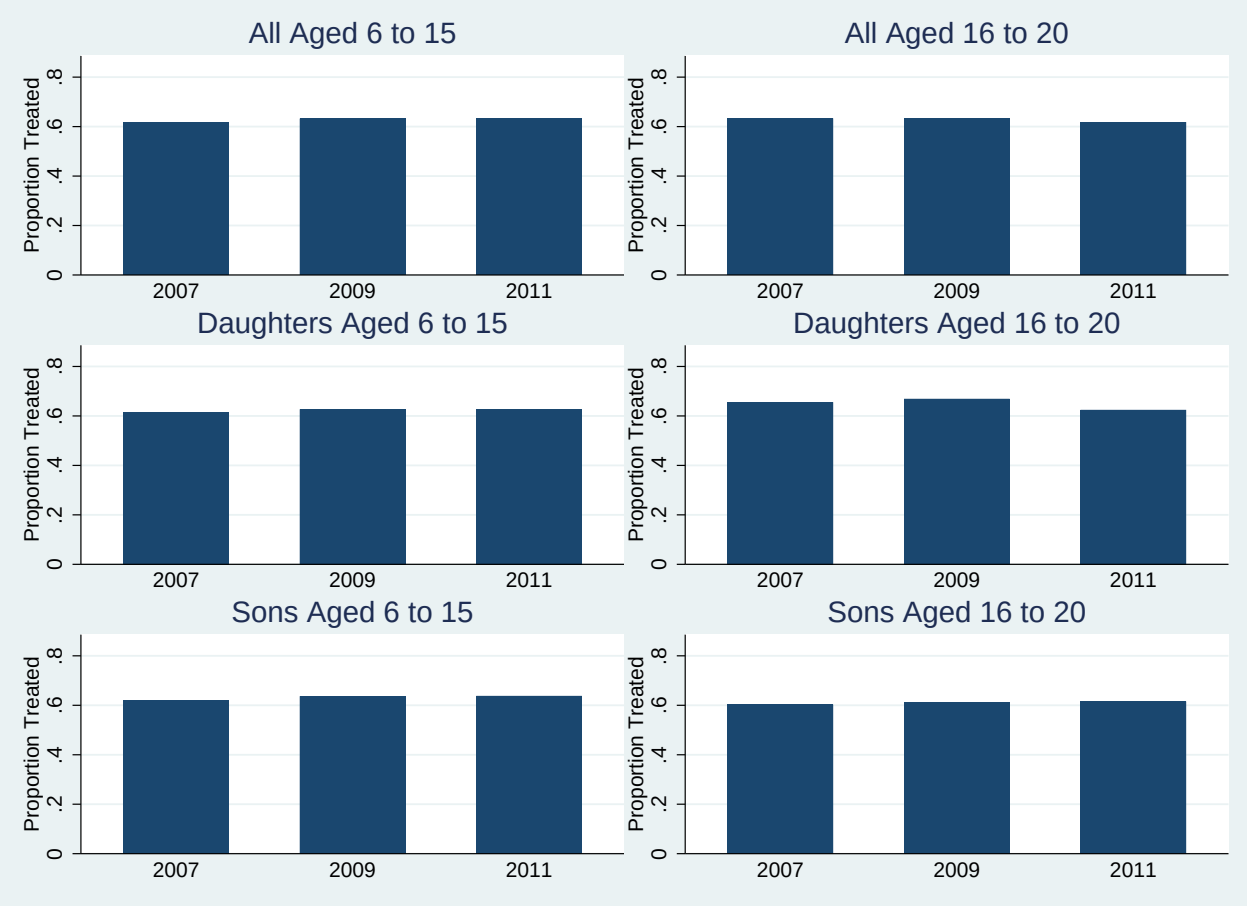


Figure A.6: Proportion of children in treatment households by age group and survey round

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