*You get what you pay for: Schooling incentives and child labor*



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# You Get What You Pay For: Schooling Incentives and Child Labor ☆

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**Abstract**

Can schooling promotion deter child participation in hazardous forms of child labor? We examine two interventions intended to promote schooling and deter child labor for children associated with carpet factories in Kathmandu. The first intervention provides scholarships for school-related expenses. The second provides the scholarship and an in-kind stipend conditional on school attendance. Paying for schooling expenses promotes schooling but only at the beginning of the school year when most schooling expenses occur. The scholarship combined with the conditional stipend increases school attendance rates 11 percent, decreases grade failure rates by 46 percent, and reduces carpet weaving by 48 percent. Financial support lasted one year. Effects on schooling and weaving do not persist past the year of support. "You get what you pay for" when schooling incentives are used to combat hazardous child labor.

*Keywords:* Education, school subsidies, conditional transfers, school enrollment, child labor, labor standards, Nepal; JEL: I32, I38, J13, J22, J80

## 1. Introduction

An estimated 264 million children aged 5-17 were economically active in the world in 2012. 85 million were working in activities considered hazardous in their country of residence by virtue of the activity's nature or circumstance (International Labour Organization, 2013). While abundant evidence documents that schooling incentives can promote school attendance, there are unanswered questions about their utility in the fight against hazardous forms of child labor.[1] Hazardous child labor may be fundamentally different than more common forms of work. First, selection into hazardous child labor may be such that the most desperate participate. Subsistence concerns may lead to labor supply that is especially inelastic to changes in net returns to schooling (Dessy and Pallage, 2005). Second, schooling might not be the counterfactual use of time for hazardous child laborers, non-hazardous employment being an obvious alternative. Hence, hazardous employment would not be elastic to net schooling returns in the same way as non-hazardous employment where schooling is the counterfactual use of time. Third, agency issues may mitigate the responsiveness of hazardous child labor to changes in the local economic environment (Rogers and Swinnerton, 2008). In usual labor supply models, labor is free to adjust in response to changes in relative prices. Children in

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[1] Conditional transfers can reduce common forms of child work (Barrera-Osorio et al., 2011; Behrman et al., 2011; Maluccio and Flores, 2005; Schady and Araujo, 2006). However, the magnitudes of the decline in work seem much smaller compared to the increase in schooling (Attanasio et al., 2010; Dammert, 2009; Duryea and Morrison, 2004; Galiani and McEwan, 2011). This has led some to question whether there is a trade-off between common forms of work and schooling (e.g. Ravallion and Wodon (2000)), a question that is difficult to test directly given the joint nature of school and work decisions.

general have limited agency to adjust their own labor supply, but this lack of agency may be even more salient for hazardous activities when they are away from home. The agent making decisions about child labor and schooling may not be informed or responsive to changes in the economic environment at the point of employment. The impact of schooling incentives is an important policy question as combating hazardous child labor through education promotion has been the centerpiece of global anti-child labor policy since 2001.[2] Despite the policy focus, we do not have direct evidence of a connection between hazardous forms of child labor and the net return to education.

This study considers the link between schooling incentives and hazardous child labor through a schooling promotion project for children associated with workers in the handmade, export-oriented carpet sector of the Kathmandu Valley of Nepal. Under ILO Convention 182 each country develops its own list of hazardous forms of child labor in the country. Carpet weaving is on Nepal's list of hazardous activities and prohibited for children under 16 by the 1999 Child Labor Prohibition and Regulation Act. Weaving is on the hazardous list for several reasons including the physical toll of weaving, the health problems associated with prevalent environmental hazards, its incompatibility with schooling, and the prevalence of human trafficking and bonded labor.

In this study, a lottery allocated individual children to three groups:

1. A control group whose schooling and time allocation would be monitored but would receive no education support.
2. A scholarship group whose education-related expenses would be paid directly or reimbursed up to a cap.
3. A stipend group that received the scholarship treatment plus an additional stipend conditional on regular school attendance. The stipend was a third of the cash earnings made by a youth in weaving and paid as a credit in a local shop that could be used towards purchase of food.

The treatments lasted one academic year. The design was motivated by our NGO partner's desire to test whether a conditional stipend would improve the impact of the existing scholarship program (the group 2 treatment) that they were administering to a separate population. Our analysis relies on data collected before, during, immediately after, and 16 months after the end of the project.

We find effects of the two interventions when they are providing support: "You get what you pay for." The scholarship was largely paid out at the start of the school year. It promotes school enrollment and attendance in the beginning of the school year. The effects fade out after two months. By the end of the school year, scholarship recipients attend 84 percent of all school days, insignificant compared to the control group's 81 percent attendance. 11 percent of scholarship recipients and control subjects work as weavers during the year of support. Funds from the stipend intervention lasted throughout the academic year, and it has large effects on school attendance and achievement throughout the school year of support. Compared to the control group, stipend recipients attend 11 percent more days, are 10 percent more likely to have sat for year-end exams, and are 46 percent less likely to have failed their grade. Stipend beneficiaries are 48 percent less likely to have worked as carpet weavers during the period of support. Although gender differences are not statistically significant, effects of the stipend compared to the control are larger in magnitude for girls. During the year of support, 16 percent of girls work in carpet weaving, but stipend recipient girls are 64 percent less likely to weave. The effects of the stipend treatment dissipate when schooling is no longer incentivized. When we return to our study subjects 16 months after the end of support, we cannot reject the null hypothesis of no effect of either treatment on school attendance, school attainment, or carpet weaving.

## 2. Background

### *2.1. Study Population*

Handmade carpets have been one of Nepal's leading commodity exports. In 2011, hand-knotted carpet exports were 8.5% of all exports from Nepal.[3] Nepal exports high-quality hand-knotted carpets to the U.S.

[2] The U.S. Department of Labor (DOL) is the largest funder of anti-child labor programs around the world. The office in the U.S. Department of Labor (DOL) charged with child labor abroad alone has appropriated $800 million to 258 projects in 91 countries since the office's creation in 1995. The influence of DOL far exceeds these appropriations as often DOL is a contributing partner in larger projects or the experience of DOL informs the design of other, non-DOL projects. In 2001, DOL launched the Child Labor Education Initiative (EI) that placed the promotion of education at the center of project tools to combat hazardous child labor.

[3] The share of carpets in total exports has been falling, particularly after 2008. In early 2000s, carpets were about a quarter of all exports from Nepal

and European markets. Child labor in carpet weaving might be the only way U.S. consumers interact with products made with child labor in Nepal. Child labor in Nepali carpets became a major policy issue in the mid-1990s because of working conditions in weaving as well as evidence of human trafficking and forced labor. This study is part of a project funded by the U.S. Department of Labor aimed at understanding child labor in Nepal's carpet sector with the policy goal of eliminating it for children under 18.[4] Under the international convention governing regulation of worst forms of child labor (ILO C182), every country is charged with making a list of hazardous child labor in the country for prohibition for children below 18. Carpet weaving is on the Nepali government's list of hazardous child labor in Nepal. There are an estimated 10,907 children employed in the hand-made, export-oriented carpet sector of Nepal out of a total labor force of 49,538 (ICF 2012).

Our NGO partner in the study is the Nepal GoodWeave Foundation (NGF), formerly known as Rugmark. NGF certifies individual carpets as child labor free and provides support services to children found in establishments involved in the manufacture of certified carpets. In addition, it provides scholarships to vulnerable children, and works with its licensee factories to provide daycare and other support to carpet workers. One of their scholarship programs, the "Sponsored Education Program" (SEP), is the basis for the scholarship treatment in this study. We focus on the population aged 10-16 as most dropouts from school occur at this age range.[5]

For this study, NGF targeted the children whose guardians worked in establishments that were NGF licensees or sub-contractors of licensees.[6] NGF identified 660 children that (a) had attended school within the last 18 months, (b) had not received education support from NGF or other sponsors and (c) were in families who faced an elevated risk of transitioning to child labor. NGF views children as having an elevated risk of transitioning to child labor (weaving) if they met at least one of the following three criteria:

- Family size: A family with 3 or more school-aged children in residence;
- Family income: Total monthly family income (including monetary support from kin/relatives living elsewhere) of NPR 8,500 (USD 119) if housing was not provided free of charge by an establishment or of NPR 7,000 if housing was provided free of charge by the establishment;
- Sibling schooling: Families with one or fewer children attending school or families with a child who dropped out before completing grade 8.[7]

These criteria for identifying a child as vulnerable to child labor were chosen by NGF based on its field experience of implementing other programs.

Most subjects deemed vulnerable to child labor in anti-child labor projects are not actually engaged in child labor as entry deterrence has become the focus of anti-child labor policy. In our case, less than 10 percent of subjects weave at baseline. Hence, weaving was not one of the selection criteria.

NGF's income criterion was based on the minimum wage set by the government of Nepal and in effect in early 2010. The minimum wage is viewed by the government of Nepal as the minimum income needed for subsistence. The minimum wage level per person was NPR 4,600 per month. Assuming 85 percent of children lived in households with an adult earner implied an income of NPR 8,500 per month. The allowance of NPR 1,500 per month for rent when housing was provided free of charge by employers was based on the NGF inspectors' subjective opinion on the value of this free housing. The decision to limit subjects to those recently enrolled in school was based on NGF's opinion that children out of school for longer would require transition education services and other types of support to re-enter school after a long absence.

NGF conducted preliminary interviews with workers in carpet establishments to collect data on the children they have, their family income and other information necessary to determine eligibility of the children. For the 660 children in this study, Table 1 contains counts of the number of subjects who met each of the criteria listed above. All study subjects were 10-16, had attended school within the last 18 months,

[4]While the applicable international conventions imply that hazardous forms of child labor should be prohibited for children under 18, Nepali law prohibits child involvement in carpet weaving for children under 16. Hence, children 16 and 17 are legally allowed to weave in Nepal.

[5]For example, among youths 5-25 who have ever attended school, 63 percent terminated schooling between the ages of 10 and 16 with 14-16 being the most common ages (authors' calculations from the Nepal Living Standards Survey III).

[6]These establishments were NGF licensees or subcontractors of NGF licensees as of January 2010. Of these, 100 were actively engaged in weaving, and one establishment reported no weaving but involvement in carpet finishing and dyeing wool. These 101 establishments had a total of 3,434 employees as of January 2010 and had completed a total of 8,464 square meters of carpet in the last 30 days.

[7]Basic education (grades 1 through 8) is free in community (government) schools of Nepal

Table 1: Eligibility Criteria and SIPE Study Population

| Criteria | Number of Subjects Matching Criteria |
|---|---|
| Family Size | 255 |
| Low income, no housing | 234 |
| Low income, housing provided | 396 |
| Siblings, 1 or fewer in school | 135 |
| Siblings, Dropout | 52 |
| Total | 660 |

Source: Authors' calculations from NGF Eligibility Survey (2010).

and did not receive other education support. Subjects could match multiple criteria. Two-thirds of study subjects met at least two criteria. Only 30 study subjects did not meet the low income criterion. Of these 30, 1 was in the study because of sibling dropouts, 3 were in the study because one or fewer siblings attend school, and the remaining 27 met the family size criteria, having 3 or more eligible children in the family.

Table 2 contains a comparison of the study population to children 10-16 in the country, in the Kathmandu Valley and living in households below the national poverty line in the Nepal Living Standards Survey-III (NLSS-III) of 2010/11. Our pool is more Buddhist and of Tamang and Magar ethnicity than other populations within the country.[8] Our study pool is also younger and more likely to be female than a random sample of children in either of these populations. The mothers and fathers have lower educational attainment compared to population in the Kathmandu valley (including poor households) and the national population. Parental education and literacy is similar to or slightly better than that of the national population living in poverty, although not compared to poor households in Kathmandu. Fathers are less likely to be present in our study population than other populations. To the extent that mothers and fathers respond differently to schooling incentives, findings could differ in our sample compared to a sample where fathers are impacted by schooling incentives. Because of our selection criteria, school enrollment is higher in our study sample compared to the rest of the population, but completed education is lower. Importantly, while slightly less than 10 percent of our sample is involved in weaving at baseline in our sample, participation in this hazardous form of child labor is more prevalent than wage employment in general among the poor in the Kathmandu Valley.

### *2.2. Program Description*

The primary purpose of the Schooling Incentives Project Evaluation (SIPE) study was to evaluate the impact of scholarship and stipend programs in encouraging school enrollment and attendance and reducing child labor in the carpet sector in Nepal. SIPE was the first randomized controlled trial (RCT) impact evaluation of a child labor and schooling intervention to be conducted in Nepal. Subjects were identified in February 2010 and they received schooling support for the school year beginning in April 2010 and ending March 2011.

NGF designed and implemented SIPE with technical support from the authors.[9] NGF's involvement in the project was motivated by a desire to improve the efficacy of its efforts to prevent children from working on looms. The 660 study subjects were grouped into gender - age - establishment size cells, and randomization was used to assign subjects in each cell to one of three groups:

1. The first group is the control group. The children in the control group received no schooling-related assistance.
2. The second group is the scholarship group. For children in this group, NGF reimbursed or paid each child's schooling-related costs up to a maximum of (Nepali rupees) NPR 3,950 per year. This assistance could include all schooling-related costs such as fees, tuition, uniforms, books and other supplies. Once

[8] The carpet exporting industry in Nepal was initially set up amongst Tibetan refugees in Kathmandu. The ethno-linguistic similarity between Tibetan and Tamang and Magar ethnicities explain why these ethnicities and religion are predominant in the carpet sector of Nepal. These ethnicities also have large presence in the districts surrounding Kathmandu Valley

[9] ICF International executed SIPE under its "Research on Children Working in the Carpet Industry of India, Nepal, and Pakistan" contract with the U.S. Department of Labor. NGF, New ERA, and the authors (through International Child Action Research and Evaluation LLC) were all sub-contractors to ICF for this contract. Leigh Linden was an important contributor to the early discussions of this project, in addition to providing invaluable input on an earlier draft of this paper.

Table 2: SIPE Study Population

| | Study Subjects | | National | | Poor (National) | | Kathmandu Valley | | Poor (Valley) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | mean | sd | mean | sd | mean | sd | mean | sd | mean | sd |
| *Demographics* | | | | | | | | | | |
| Age | 12.274 | 6.158 | 12.918 | 2.012 | 12.685 | 2.026 | 13.314 | 1.978 | 13.253 | 1.955 |
| Female | 0.541 | 0.499 | 0.499 | 0.500 | 0.520 | 0.500 | 0.512 | 0.500 | 0.397 | 0.494 |
| *Caste/Ethnicity* | | | | | | | | | | |
| Brahman/Chhetri | 0.070 | 0.255 | 0.300 | 0.458 | 0.227 | 0.419 | 0.325 | 0.469 | 0.110 | 0.316 |
| Newar | 0.053 | 0.224 | 0.053 | 0.223 | 0.022 | 0.146 | 0.347 | 0.476 | 0.329 | 0.474 |
| Tamang/Magar | 0.658 | 0.475 | 0.129 | 0.335 | 0.153 | 0.360 | 0.179 | 0.383 | 0.371 | 0.488 |
| Dalits | 0.020 | 0.139 | 0.137 | 0.344 | 0.226 | 0.418 | 0.029 | 0.167 | 0.111 | 0.317 |
| Others | 0.200 | 0.400 | 0.381 | 0.486 | 0.373 | 0.484 | 0.121 | 0.326 | 0.079 | 0.273 |
| *Religion* | | | | | | | | | | |
| Hindu | 0.439 | 0.497 | 0.831 | 0.375 | 0.821 | 0.384 | 0.784 | 0.412 | 0.792 | 0.410 |
| Buddhist | 0.471 | 0.500 | 0.075 | 0.263 | 0.085 | 0.279 | 0.165 | 0.371 | 0.192 | 0.398 |
| Others | 0.089 | 0.286 | 0.094 | 0.292 | 0.094 | 0.292 | 0.052 | 0.221 | 0.016 | 0.126 |
| *Parent Information* | | | | | | | | | | |
| Live with both parents | 0.641 | 0.480 | 0.664 | 0.472 | 0.730 | 0.444 | 0.715 | 0.452 | 0.729 | 0.449 |
| Mother present in HH | 0.965 | 0.184 | 0.897 | 0.305 | 0.919 | 0.273 | 0.837 | 0.369 | 0.792 | 0.410 |
| Mother literate (if present) | 0.126 | 0.332 | 0.270 | 0.444 | 0.112 | 0.315 | 0.564 | 0.496 | 0.297 | 0.462 |
| Yrs of Schooling (mom) | 0.559 | 1.553 | 1.485 | 3.093 | 0.360 | 1.371 | 4.499 | 5.023 | 0.843 | 2.072 |
| Father present in HH | 0.676 | 0.468 | 0.693 | 0.461 | 0.762 | 0.426 | 0.758 | 0.429 | 0.874 | 0.335 |
| Father literate (if present) | 0.590 | 0.492 | 0.615 | 0.487 | 0.470 | 0.499 | 0.888 | 0.315 | 0.653 | 0.481 |
| Yrs of Schooling (dad) | 2.446 | 2.521 | 4.105 | 4.329 | 2.513 | 3.477 | 7.916 | 4.724 | 3.988 | 4.151 |
| *Migration, Schooling and Work* | | | | | | | | | | |
| Born elsewhere | 0.655 | 0.476 | 0.119 | 0.324 | 0.057 | 0.232 | 0.298 | 0.458 | 0.157 | 0.368 |
| Currently attending school | 0.974 | 0.159 | 0.893 | 0.309 | 0.833 | 0.373 | 0.943 | 0.232 | 0.889 | 0.317 |
| Completed Years of Schooling | 3.932 | 2.256 | 4.727 | 2.663 | 3.662 | 2.440 | 6.180 | 2.425 | 5.285 | 2.600 |
| Wage work in past 7 days | 0.098 | 0.298 | 0.079 | 0.271 | 0.113 | 0.317 | 0.038 | 0.192 | 0.079 | 0.272 |

Source: Authors' calculations from NE-B and NLSS-III. Study subjects refers to the 660 children who were the subject of this evaluation. The rest of the columns refer to children aged 10-16 in the third Nepal Living Standards Survey (NLSS-III) 2010/11. NLSS-III has 5,275 children in the national sample and 527 children in the Kathmandu sample. Poverty classification done based on consumption-based poverty measure of CBS 2011. The SIPE study did not have information on parental literacy when a parent was not present in either sample the scholarship funds were exhausted, no additional support was available to subjects from NGF. This treatment arm was modeled after NGF's Sponsored Education Program.[10] Our calculations from the Nepal Living Standards Survey 2003/04 (NLSS-II) suggested that this scholarship amount was sufficient to cover schooling costs for children if they attended community or government schools in the Kathmandu Valley.

3. The third group is the stipend group. The children in this group received the scholarship and an additional stipend conditional on regular attendance.[11] In addition to the scholarship, NGF provided a stipend of food rations valued at NPR 1,000 per month per subject, provided that the child attended school at least 80 percent of the days his or her school was open for teaching in the previous calendar month. The stipend distribution occurred through local stores. Every child that received the stipend was given an identity card with a picture of the child and their guardian. NGF identified several local stores in the neighborhood of the recipients' residences and arranged for the holder of the identity card to receive the stipend as an in-store credit when the child met the school attendance requirements.[12] Families in the poorest third of the population of the Kathmandu Valley spend slightly more than NPR 1,000 per month on grains and pulses. Hence, we consider these transfers to be infra-marginal. The amount of the stipend is a third of the cash earnings made by a youth in weaving and roughly the same as a child would earn working as a domestic worker, a common occupation of migrant children in Kathmandu.

All subjects assigned to the two treatment arms took up the benefits when offered.

Subjects appear comparable across treatment arms. Table 3 summarizes background characteristics of study subjects by treatment status using data collected before the assignment of subjects to treatment arms. Column 1 contains the control population mean and (in parenthesis) standard deviation for the attribute indicated by the row. Column 2 contains the differences between the scholarship treatment group and the control group and the standard error of this difference in parenthesis. For example, the average subject age in the control group is 11.86 and 11.91 in the scholarship group. Column 3 contains the difference between the stipend group and the control group as well as the standard error of this difference. Column 4 is the difference between the scholarship treatment group and the stipend treatment group and the standard error of the difference. The bottom rows of the table contain the F-Stat and P-Value associated with the null that the individual differences in the column are jointly zero.

There are no statistically significant (at 95%) differences across characteristics, and to our eyes, none of the differences appear substantive either. The only exception is that the difference between the scholarship and stipend groups is significant at 10 percent level for child involvement in carpet weaving. We attribute this to chance, but continue to control for baseline involvement in carpets as one of our controls in all our regression specifications. A few characteristics in Table 3 merit explanation. NGF works with children associated with licensee establishments. The average associated establishment has 41 employees. All subjects will have at least one parent involved in carpet manufacture but fewer than 1 in 10 subjects weave themselves. The fraction of children involved in carpet weaving is almost identical to the fraction of child involvement in wage work (Table 2), suggesting that carpet weaving is the predominant form of child employment in this population. Due to our sample selection criteria, 98 percent of the children have gone to school at some point in the academic year prior to treatment. Therefore, we can view the effect of the treatments as acting through school dropout at the beginning of and throughout the school year. About 95 percent of the subjects live within 500 meters of the establishment, with 59 percent of residing within establishment premises. Two-thirds of the children were born elsewhere, and 66 percent have both parents present at their place of residence.

## 3. Empirical Methods

### *3.1. Timeline*

The two treatments in SIPE were provided to subjects during the school year that began in April 2010 and ended in March 2011. Figure 1 reviews the timeline of the project which ran from April 2010 to March 2011

[10] The scholarship amount, NPR 3,950, was chosen based on the benefits disbursed in NGF's Sponsored Education Program.

[11] Although this group receives both the scholarship and the additional stipend, we refer to this group as the stipend group for economy.

[12] At the time of the project, USG projects were restricted from providing direct cash payments to beneficiaries in foreign countries. Hence, the stipend had to be delivered as a credit. Most recipients bought rice with the credit.

Table 3: Validity of Randomization

| | Control | Scholarship -Control | Stipend -Control | Stipend -Scholarship |
|---|---|---|---|---|
| | mean/(sd) | b/(se) | b/(se) | b/(se) |
| | (1) | (2) | (3) | (4) |
| Age | 11.859 | 0.055 | 0.014 | −0.041 |
| | (2.017) | (0.186) | (0.188) | (0.178) |
| Female | 0.541 | −0.014 | 0.014 | 0.027 |
| | (0.499) | (0.054) | (0.050) | (0.048) |
| # employees in establishment | 41.182 | 0.518 | −0.041 | −0.559 |
| | (25.342) | (1.963) | (1.541) | (1.796) |
| Live inside establishment premise | 0.591 | 0.000 | −0.005 | −0.005 |
| | (0.493) | (0.042) | (0.041) | (0.044) |
| Live with both parents | 0.655 | −0.018 | −0.023 | −0.005 |
| | (0.477) | (0.046) | (0.043) | (0.050) |
| Born elsewhere | 0.659 | 0.027 | −0.041 | −0.068 |
| | (0.475) | (0.037) | (0.041) | (0.045) |
| Household size | 4.586 | 0.077 | −0.023 | −0.100 |
| | (1.299) | (0.120) | (0.116) | (0.111) |
| Completed education | 4.018 | −0.186 | −0.073 | 0.114 |
| | (2.208) | (0.186) | (0.186) | (0.193) |
| Currently going to school | 0.977 | −0.005 | 0.000 | 0.005 |
| | (0.149) | (0.015) | (0.016) | (0.016) |
| Child involved in weaving carpets | 0.095 | −0.027 | 0.018 | 0.045* |
| | (0.295) | (0.026) | (0.029) | (0.024) |
| Educational expenditure ('000 NPR) | 13.325 | −0.478 | 0.016 | 0.494 |
| | (11.241) | (0.839) | (1.034) | (1.161) |
| Total expenditure ('000 NPR) | 108.031 | 1.343 | 1.368 | 0.025 |
| | (58.704) | (5.175) | (5.476) | (5.517) |
| F-stat of test of Joint Significance | | 0.498 | 0.213 | 0.449 |
| p-value of test of Joint Significance | | 0.916 | 0.998 | 0.990 |

$^{*}: p < 0.1$;$^{**}: p < 0.05$;$^{***}: p < 0.01$. Standard errors clustered at the establishment level.

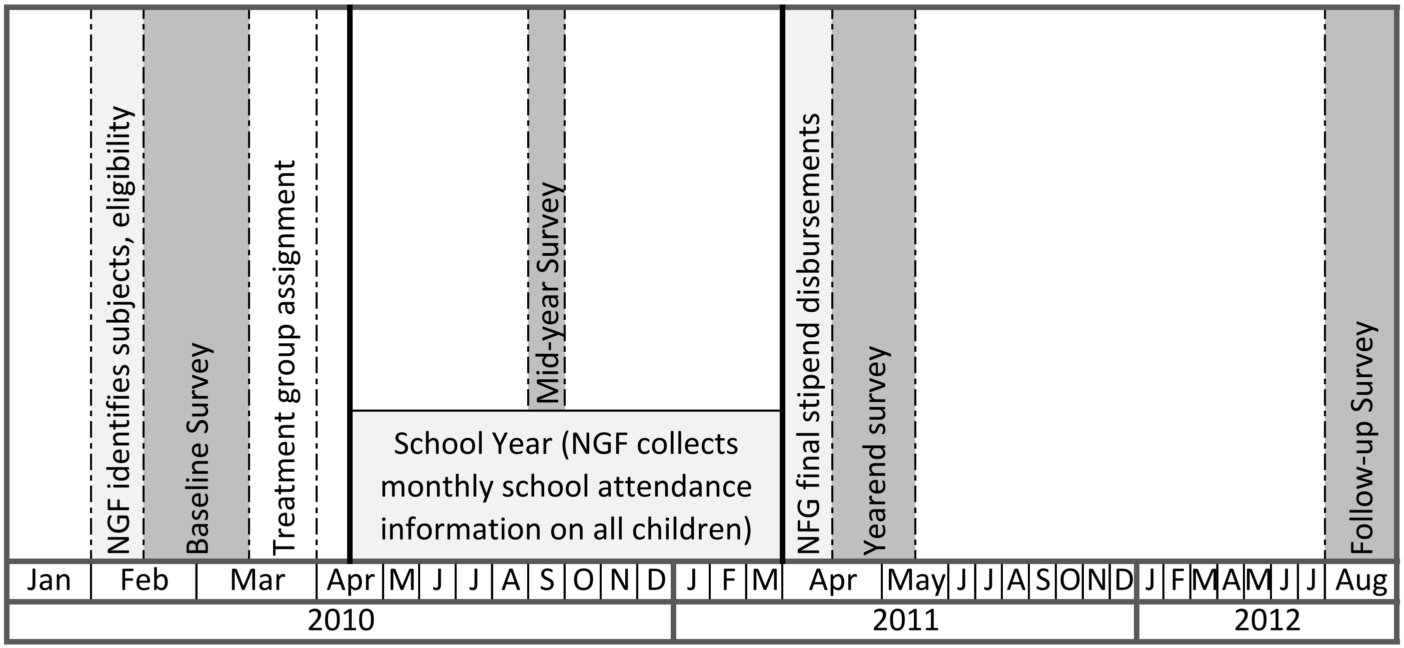
and data collection which spanned February 2010 to August 2012. NGF conducted the initial data collection by identifying the subjects and determining their eligibility for the study. This data was used to determine the subject pool, their stratification into age - gender - establishment size blocks, and randomization within these blocks. Apart from this initial data collection, there are two sources for the data used in this study. Classroom attendance records were gathered by NFG field monitors throughout the support school year.[13] This data was used by NGF to monitor attendance records and determine eligibility for stipend in the stipend group. In addition, NGF collected the yearend exam scores for all the children from their schools. Because of the richness of the monthly attendance measures, we use the administrative data from the school for our school attendance outcomes. In time periods where we also have parent provided information on school attendance, the choice of using the parental response or the school record does not influence our conclusions.

Besides the administrative data collection, we have four rounds of survey data collected by an independent data collection firm New ERA. The enumerators used in these surveys were deliberately not informed about the intervention or the subject's status in the intervention. The enumerators and the data collection firm interacted with NGF only in the initial phase in locating the establishment and introducing them to the establishment owner. This was done to make sure that respondents feel comfortable sharing their schooling and labor information with the enumerators and would not feel that their responses would be shared with the monitors. Small cash incentives were provided to all participants in all rounds of the survey except the followup survey.

The baseline survey was conducted before subjects were informed about their status in the project in

[13]The field monitors normally followed a regular schedule so teachers would know when to expect record collection.

Figure 1: Timeline of Program Implementation and Data Collection



March 2010. The data were not available prior to random assignment so only the NGF eligibility survey collected in February was accessible at the time of randomization. Both the subjects and their parents were interviewed in the baseline survey on their schooling, labor and household consumption, amongst other things. The midterm survey was conducted in September 2010, before the October holidays. Only the parent (or caregiver, if child changed living arrangements) were interviewed in the midterm survey. The number of questions was reduced compared to the baseline survey, and no questions about child involvement in the carpet sector were collected. Data from the midterm survey is not used directly in any of the reported results of this study, but it was critical in the effort to track our migrant population. The yearend survey was conducted after New Year festivities in April and May of 2011. Adult caregivers and subjects were interviewed within a month after the final distribution of benefits. The yearend survey instrument was nearly identical to the baseline instrument. The followup survey was conducted in August 2012, approximately 16 months after the final distribution of benefits. A streamlined survey instrument was administered to adult caregivers, mostly collecting information on child schooling and labor.

*3.2. Empirics*

The SIPE study sample size was chosen to have statistical power to test the null hypothesis that the stipend intervention provided no impact on school attendance relative to the scholarship. This focus was chosen in discussions with NGF, because it was directly relevant to NGF's school promotion efforts. The scholarship portion of SIPE was based on their Sponsored Education Program, or SEP, and this null hypothesis was equivalent to asserting that schooling outcomes would not be improved by providing additional financial support for earnings that would be foregone by attending school rather than working full-time. The existing literature does not provide much evidence as to whether hazardous child labor should be elastic to changes in the net return to schooling like the stipend intervention.[14]

Our ex-ante power calculations suggested we needed a total treated population of 41 children to test this null, so the actually sample size of 220 per treatment group was expected to sufficient to test the null hypothesis separately by gender, by age group, and by carpet establishment size with a simple comparison of means in the yearend data. However, our ex-ante analysis plan stipulated a regression approach throughout our analysis. Because the randomization appears to have achieved balance (Table 3), these controls function to reduce variance only. Specifically, we estimate:

$$y_{ijkt} = \beta A_{i0} + b_0 + b_1 Sc_i + b_2 St_i + \alpha Y_{ijk0} + \pi_{ijk0} + \varepsilon_{ijkt} \tag{1}$$

where $y_{ijkt}$ is outcome $y$ for child $i$ in family $j$ associated with establishment $k$ observed at time $t$. $Sc$ is an indicator that child $i$ was assigned to the scholarship treatment, $St$ is an indicator that child $i$ was assigned to the stipend treatment. All subjects who were offered the opportunity to participate in the scholarship or stipend treatments chose to participate, so there are no issues of take-up associated with interpreting the coefficients on these random assignment variables. $A$ is a vector of age fixed effects, a gender effect, and age$\times$gender effects. $Y_{ijk0}$ is a vector of the outcome variables observed at baseline. Its inclusion means that we identify the impact of the treatments based on changes in $y$ between the baseline period and the yearend or post period. SIPE randomization was conducted after stratifying the population into age group $\times$ gender $\times$ establishment size cells. $\pi$ is a vector of fixed effects denoting the subject's stratum used in randomization. $\varepsilon_{ijkt}$ is a mean zero error term. We cluster standard errors by establishment throughout.[15] There are 101 establishments in our study. We think there is apt to be important clustering of time allocation decisions at

[14]The recent academic literature on child labor supply rarely finds evidence that child labor supply is responsive to the return relative to schooling. First, the theoretical literature on child labor supply emphasizes the primacy of living standards in influencing time allocation. For example, the canonical child labor model of Basu and Van (1998) is built on the luxury axiom which posits that children work when their family cannot meet its basic needs otherwise, regardless of how large or small the return to child labor is. Baland and Robinson (2000) and Ranjan (2001) highlight that constraints on liquidity drive child labor even when relative prices suggest that alternatives are preferred uses of child time. Second, the empirical evidence on the determinants of child labor emphasizes the responsiveness of child labor to changes in family living standards more than any other factor. An extreme example is Edmonds and Schady (2012), where a $15 per month transfer in Ecuador spurs households to forego $80 a month in child labor earnings. Other studies emphasize the importance of child labor in coping with transitory shocks when returns to child labor are presumably lower (Beegle et al., 2006; Duryea et al., 2007; Jacoby and Skoufias, 1997). In contrast, there is little evidence of a response of child labor supply to its net return. Even studies of large economic restructuring, where there should be substantive changes in the employment opportunities open to children, typically only find effects on child labor through family living standards (Cogneau and Jedwab, 2012; Dammert, 2008; Edmonds and Pavcnik, 2005; Edmonds et al., 2010).

[15]The empirical approach stipulated in an analysis plan submitted to ICF before the availability of the midterm data or schooling records (available from authors upon request) was identical to what we use here with regards to the included controls,

Table 4: Sample Size by Survey Round

| Data Source | Control | Scholarship | Stipend |
|---|---|---|---|
| NE Baseline, 3/10 | 220 | 220 | 220 |
| NE Midterm, 9/10 | 207 | 215* | 216** |
| NE Yearend, 5/11 | 219 | 218 | 218 |
| NE Followup, 8/12 | 217 | 217 | 216 |
| NGF School Records, 9/10 | 220 | 218 | 217 |
| NGF School Records, 3/11 | 220 | 218 | 217 |

Each cell contains a count of the number of subjects interviewed in each round of data collection (row) by treatment arm (columns). To test for attrition potential bias, we regress an indicator that a subject is missed in the data collection round indicated by the row on treatment status using the specification from equation (1). Asterisks indicate the p-value range of the test of the null hypothesis that attrition does not differ from between the treatment represented by the column and the control group. * : $p < 0.10$; ** : $p < 0.05$; *** : $p < 0.01$. Attrition rates are never significantly different between the two treatment groups. Source: Authors' calculations from each survey

the establishment level, because subjects are grouped together by establishment. 59 percent of subjects live inside the establishment, but the other 41 percent of subjects typically live proximate to the establishment where their caregiver works.

All results tables follow a similar structure. For each outcome of interest, we report the mean of the outcome for the control population and its standard deviation in column 1. In column 2, we report $b_1$ and its standard error. This tests the null that outcome $y$ does not differ with the scholarship group compared to the control. Ex-ante, we expected modest effects of the scholarship, and we do not believe that our sample size is sufficient to detect effects of the scholarship in general even with the control function approach specified above. In column 3, we report $b_2$ and its standard error. This tests the null that outcome $y$ does not differ with the stipend group compared to the control. In column 4, we report the difference between the two treatment groups ($b_2 - b_1$) and the associated standard errors. We report findings for the pooled sample and bifurcated by gender.[16] Random assignment was conducted at the individual level, so spillovers are a distinct possibility and discussed in section 4. $b_1$and $b_2$ are both identified net of any spillovers. To the extent there are any positive spillovers to schooling and reduction of child labor, the estimates presented here serve as a lower bound of the impact of the program.

With two-thirds of subjects as migrants, attrition was an ex-ante concern in this study, and substantial resources were devoted to tracking subjects in every round except for the midyear survey.[17] Table 4 contains counts of the number of subjects by treatment arm for each source of data. In the baseline, we had 660 subjects evenly split across arms. For the NGF school records at the end of the year of support, we had records for 655 subjects. The same 655 were interviewed in the yearend survey in May 2011 and 650 participated in the followup survey in August of 2012.[18]

We regard this 98.5 percent retention rate as an achievement in a migrant population. Table 4 contains results of a test for whether attrition is associated with treatment status. Specifically, for each of the 660 subjects and each round of data collection indicated by the row, we regress an indicator that the subject is not interviewed in that round on treatment status using the approach of equation (1). We only report results for the tests that the scholarship and stipend groups differ from the control group as we can never

the functional form of these controls, and the clustering of the residuals. The only difference in specification is that the ex-ante analysis plan specified the scholarship dummy to be one for both the scholarship and stipend treatments so that the coefficient on the stipend indicator directly tested differences between the scholarship and the stipend. This difference in specification is only relevant for the presentation of results and is not substantive.

[16]While the design would allow bifurcation by age group and establishment size, we do not present those results herein. Establishment size differences were almost never substantive, and we do not think the value-added to the present discussion of the age group differences was sufficient to justify their inclusion.

[17]While the project funder wanted a midyear survey for their own goals, an important part of the midyear survey was to collect tracking information on individuals before the October holidays where many families go home. Hence, we did not feel it was worth the resources at that time to track down families that had already migrated.

[18]We have some information on 7 of the 10 missing children. One child died. One child is from a family who is in hiding because of unpaid debts. Two children came from a closed factory. Their location was unknown by any of their provided contacts or former work-mates. Three children (all unrelated) are in an unknown location after their mothers left their fathers (all abroad) for another man.

reject the null of no difference in attrition between the two treatments. It is only the midterm survey where there is evidence of differential attrition by treatment arm, and we do not use the midterm survey in any of the results reported in the next section. For rounds of data collection other than the midterm, the difference in attrition probabilities are insignificant and never above 1.4 percentage points.

## 4. Main Findings

### *4.1. Schooling*

School attendance appears to have been positively impacted by both the stipend treatment and the scholarship treatment while support lasted. Because scholarship funds were largely exhausted at the start of the school year, we see smaller overall effects of the scholarship on schooling when compared to the scholarship.

The impact of the scholarship on having attended school during the year of support and the attendance rate overall is evident in Table 5. We have two different data sources to measure school outcomes: reported schooling during the surveys and administrative data collected from the schools. Each data source is likely to be affected by different measurement errors. Self-reported schooling is likely to suffer from social desirability bias if parents perceive sending children to school as socially desirable outcome. It also suffers from recall bias when parents are asked to report their child's schooling at the beginning of the support school year. Administrative data from schools is free from these biases, but teachers may face pressure from parents to misreport attendance as school records are used for program eligibility.[19] Overall, the results from parental report and administrative records are qualitatively similar. We use administrative data for most of our schooling results because of the richness of attendance information.

The first row in Table 5 contains results where the dependent variable is a parental reported indicator that the subject attended school during the academic year of support. 92 percent of control subjects attended school during the year of support. This indicates a 6 percentage points fall in school attendance compared to the year before support. The scholarship increased school attendance by a statistically insignificant 2.3 percentage points compared to the control. The stipend increased this by a statistically significant 4.9 percentage points. The second row contains a schooling attendance measure based on the school records. The outcome is the fraction of months in which the subject attended school during the year of support. Control subjects attended school at some point during the month in 88 percent of months. The stipend intervention increases the share of months with some attendance compared to the control. The third row reports the total attendance rates (total number of days attended in school in the school year divided by the total number of days the school was open for teaching) in the administrative school data. Children who did not attend school at any point in the year are coded to have zero attendance. Control subjects attended school 4 out of every 5 days the schools was open for teaching. Stipend subjects attended school more than scholarship and control groups, more than 90 percent of days.

The month by month impact of the treatments on attendance rates during the year of support is pictured in Figure 2. The difference between the stipend treatment and control treatment is statistically significant in every month. The difference between the scholarship treatment and control is not statistically significant in any month. The difference between the stipend subjects and the control or scholarship populations is greatest in month 7. This is a month with two important festivals where many families migrate back to their place of origin in rural Nepal. We specifically timed the midterm survey before this festival season in order to make sure that we had the most recently available information to track mobile subjects. The stipend significantly reduced the proportion of days missed by subjects during the festival season.

Figure 2 combines the extensive margin of attendance (attend school at all in the month) with the intensive margin (number of days attended conditional on attendance). By combining the two, the figure mixes something the scholarship treatment can affect (the extensive margin - it makes school free until funds are exhausted) with something it does not affect (the intensive margin - the cost of attending school on any given day doesn't change once the extensive margin costs have been paid). Figure 3 pictures the impact of both treatments on the extensive margin of attendance for the month. We find similar treatment effects on the extensive margin for the two treatments at the start of the year. The scholarship group is 5 percentage points more likely than the control group to attend school. The impact of scholarship differs significantly from zero on the extensive margin for the first two month but not in later months. By the yearend the probability of a scholarship child attending school is virtually identical to that of the control group. The gap

[19]There were more than 150 different schools where the subjects attended

Table 5: Treatment Effects on Schooling Outcomes during Year of Support

| | Control | Scholarship -Control | Stipend -Control | Stipend -Scholarship |
|---|---|---|---|---|
| | mean/(sd) | b/(se) | b/(se) | b/(se) |
| | (1) | (2) | (3) | (4) |
| Attendance in last year (parent) | 0.918 | 0.023 | 0.049** | 0.027 |
| | (0.275) | (0.029) | (0.019) | (0.023) |
| Share of months with any attendance (admin) | 0.881 | 0.033 | 0.071*** | 0.038* |
| | (0.295) | (0.027) | (0.024) | (0.020) |
| School year attendance rate (admin) | 0.811 | 0.026 | 0.091*** | 0.065*** |
| | (0.281) | (0.026) | (0.023) | (0.020) |
| Sat for final exams | 0.873 | 0.017 | 0.088*** | 0.071*** |
| | (0.334) | (0.034) | (0.026) | (0.026) |
| Normalized schooling measure | 0.000 | 0.112 | 0.280*** | 0.169*** |
| | (0.885) | (0.086) | (0.065) | (0.061) |
| Failed current grade (if took exams) | 0.135 | −0.064* | −0.062** | 0.002 |
| | (0.343) | (0.035) | (0.029) | (0.026) |
| Score in final exams (standardized) if not fail | −0.000 | −0.077 | 0.040 | 0.117 |
| | (1.000) | (0.099) | (0.108) | (0.101) |

* : $p < 0.1$;** : $p < 0.05$;*** : $p < 0.01$. Each row is from a different regression with the row variable as the dependent variable. All regressions include age × gender fixed effects as well as dummies for randomization strata and controls for baseline time allocation. Standard errors are clustered by establishment. Column (1) is the mean and standard deviation of the row variable for the control group. Column (2) indicates the coefficient and standard error on an indicator for scholarship treatment group. Column (3) indicates the coefficient and standard error on an indicator for stipend treatment group. Column (4) presents difference between coefficients in columns (2) and (3) and associated standard error. The first row is based on parental report and the rest comes from administrative school data collected by NGF. School attendance in second row is defined as the fraction of months attended school. Attendance rate in third row is defined as the fraction of days in a year present in school. Non-attendance and dropouts coded as 0.

between the stipend and control widens throughout the year. The divergence in treatment effects between the stipend and scholarship is particularly prominent after the seventh month (month of holidays). The increasing effect of stipend on the extensive margin seems to be driven by the children in the control group dropping out of the school as the school year progresses.

It is important to understand the distribution of school costs throughout the school year in this context to understand the effect of scholarship. The bulk of the schooling costs fall in the beginning of the school year. Students typically have to pay for textbooks and related supplies, uniforms, and admissions fee at the beginning of the school year. A child aged 10-16 going to community schools in the Nepal Living Standards Survey 2010/11 spends 75 percent of her direct educational expenditures on theses fixed costs; the share for the poor child is even higher at 82 percent. Consistent with this, we see parents claiming most of the scholarship amounts in the first month of the school year. Students also have to pay monthly fees, but the fees are small in community (government) schools where most of poor children attend. Those who still had scholarship to claim after the start of the school year largely claimed those funds in the last months of the school year. Given this, it seems reasonable to think of schooling as approximately free once the fixed cost has been paid. However, we see large effects of the scholarship on attendance in the beginning of the school year which fades out as the school year progresses.

This contrasts with the findings in studies where reducing fixed costs of schooling through uniforms reduces dropouts for younger children (Evans et al., 2008) and adolescent girls (Duflo et al., 2011) in Kenya. One key difference between the setting in Kenya and our context is the opportunity cost of child time. In our setting, child access to child labor opportunities is arguably much higher and therefore presents a bigger opportunity cost of child time in school. But this explanation alone is not sufficient to explain why they go to school at the beginning of the school year in the first place. One possibility is that enrolling in school in the beginning of the school year provides an option value to the child. If child labor opportunities are high or there are consumption shocks during the year, then the child can always drop out of school to work had she enrolled in school. But if child labor opportunities are low throughout the year, then the child cannot go to school in the middle of the school year if she never enrolled in school. This explanation is consistent with our finding of high enrollment at the beginning which gradually drops off as households face increased child labor opportunities or shocks. The drastic drop in schooling after the holidays could be a result of large

Figure 2: Treatment Effects on School Attendance by Month during Year of Support

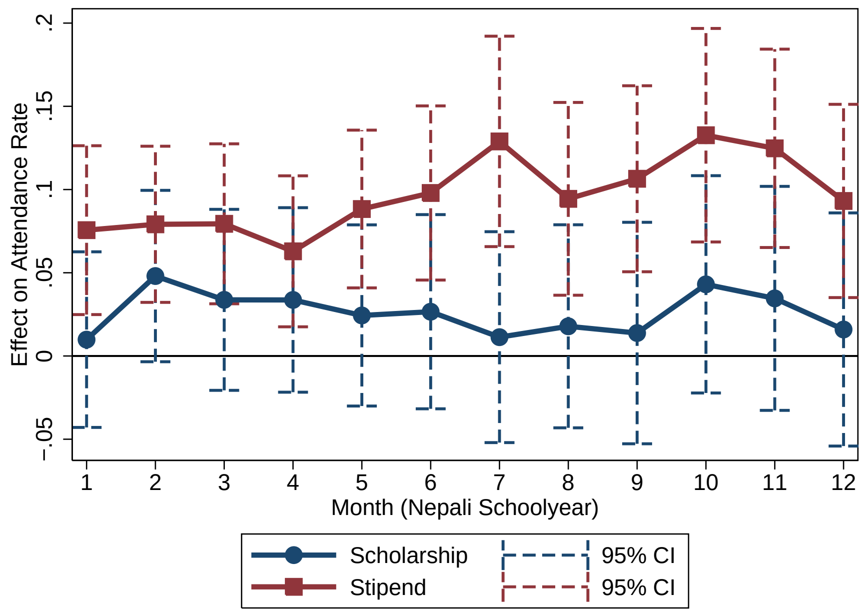



Figure 3: Treatment Effects on Whether Attends School in a Month

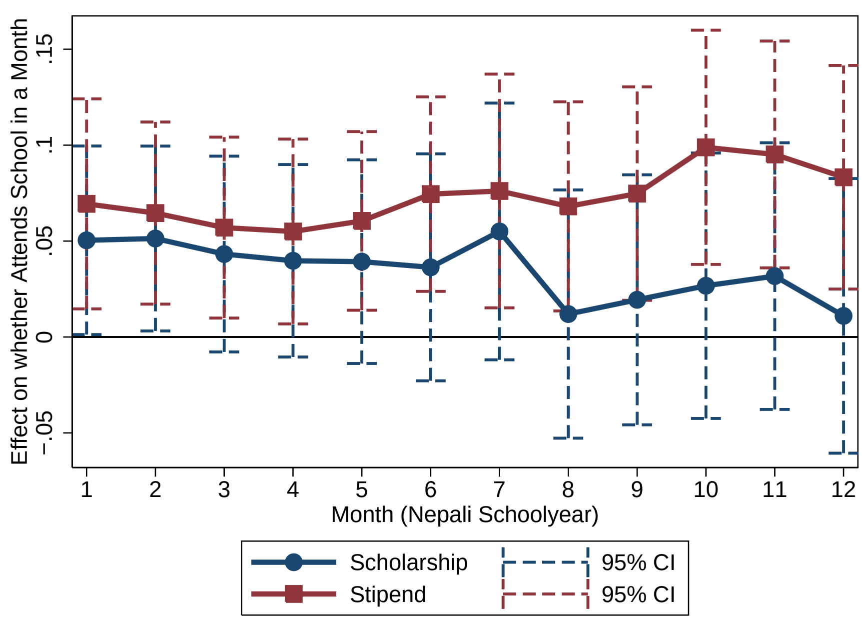
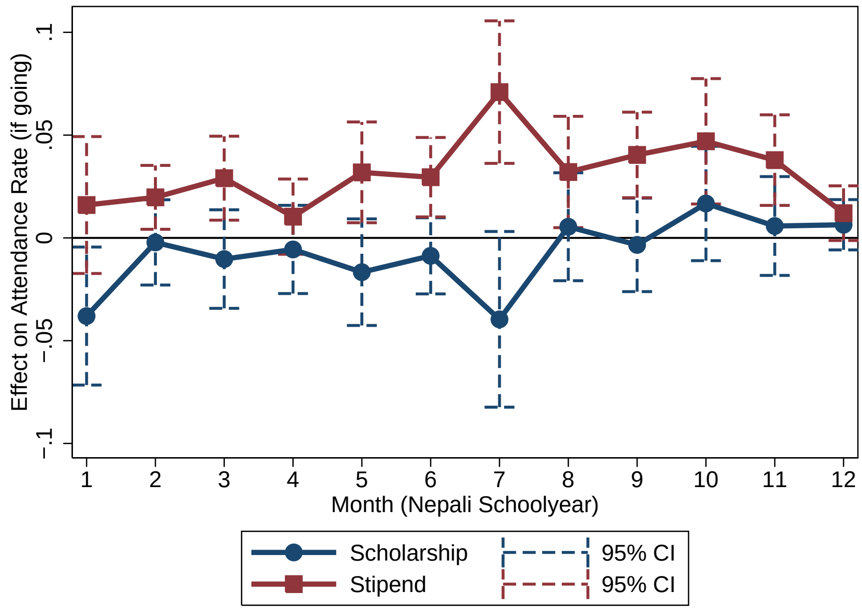


Figure 4: Treatment Effects on School Attendance Conditional on Any Attendance in Month




expenditure shock during the October holidays. For the stipend group, the additional income from stipends lowers the net return to child labor and provides additional income to buffer small shocks. Alternatively, any positive schooling habits formed decayed after a relatively long (at least 15 days) break in schooling for the holidays for the scholarship group.[20] The stipend group had continued incentives to keep going to school and was not affected by the holidays.

Figure 4 illustrates the impact of each treatment by month conditional on attending school. As we would expect, here we see no effect of the scholarship on attendance compared to the control. Given that the stipend pays for regular attendance, we would expect to see large intensive margin effects for the stipend treatment, and we see such, especially in month 7. The scholarship reduces the cost of attendance on the extensive margin without any impact on the intensive margin. Hence, the two treatments impact attendance along the lines of "You get what you pay for." The scholarship induces attendance along the extensive margin until it is exhausted. The stipend induces attendance along both the intensive and extensive margin as both are incentivized.

Another measure of attendance is whether the child sits for final exams as this requires attendance in the last month of school. The stipend leads to a nearly 10 percent increase in the probability of taking the test compared to the control and the scholarship group. (Table 5). We aggregate the four schooling attendance measures in Table 5 in the fifth row of the table, "Normalized Schooling". To create normalized schooling, we standardize the four preceding schooling outcomes by the control group mean and standard deviations, average across outcomes, and regress on treatment status using the same approach we have throughout (equation 1). The stipend treatment is associated with 0.28 standard deviations higher attendance compared to control and 0.17 higher attendance compared to the scholarship treatment.

The additional days at school throughout the school year due to stipend seems to reduce the probability that the child fails the current grade. Row 6 of Table 5 looks at the impact of our treatments on an indicator that is 1 if the child fails the current grade. We limit the sample in row 6 to children who took exams as we do not want to confound the mechanical effect of attendance (cannot pass the test if you are not in school to take it) with the impact of having been in the classroom more. Conditional on taking the exam, the stipend reduces the probability of failing by almost half. The scholarship treatment also seems to have large effect in reducing failure. Given that the stipend induces individuals to take exams who otherwise would not, it would be a mistake to assume that scholarship reduces failure by the same amount as stipend does as the

[20]This is consistent with the phenomenon in Acland and Levy (2010) in case of health forming habits. In an experimental setting similar to Charness and Gneezy (2009), but with longer term followup, the authors find substantial decay in regularity of gym attendance after winter break in the post-incentive period.

Table 6: Treatment Effects on Schooling Outcomes after Year of Support

| | Control mean/(sd) (1) | Scholarship -Control b/(se) (2) | Stipend -Control b/(se) (3) | Stipend -Scholarship b/(se) (4) |
|---|---|---|---|---|
| Enrolled in School (Year + 1) | 0.931 | −0.002 | 0.013 | 0.016 |
| | (0.254) | (0.023) | (0.019) | (0.022) |
| Did not fail enrolled grade (Year + 1) | 0.915 | −0.003 | −0.023 | −0.019 |
| | (0.280) | (0.027) | (0.026) | (0.028) |
| Enrolled in School (Year + 2) | 0.816 | 0.048 | 0.013 | −0.034 |
| | (0.389) | (0.034) | (0.032) | (0.034) |
| Attended school in current month (Year+2) | 0.779 | 0.050 | 0.012 | −0.039 |
| | (0.416) | (0.037) | (0.034) | (0.038) |
| Completed Years of Schooling (Year+2) | 6.645 | −0.129 | 0.065 | 0.194 |
| | (2.295) | (0.137) | (0.123) | (0.124) |

$^{*}: p < 0.1;^{**}: p < 0.05;^{***}: p < 0.01$. Each row is from a different regression with the row variable as the dependent variable. All regressions include age × gender fixed effects as well as dummies for randomization strata and controls for baseline time allocation. Standard errors are clustered by establishment. Column (1) is the mean and standard deviation of the row variable for the control group. Column (2) indicates the coefficient and standard error on an indicator for scholarship treatment group. Column (3) indicates the coefficient and standard error on an indicator for stipend treatment group. Column (4) presents difference between coefficients in columns (2) and (3) and associated standard error. All variables are reported by parents during the Followup Survey.

counterfactual pass rate would differ between the stipend and scholarship groups. Suppose students who did not take the final exams would have been more likely to fail than those who took the exams. Then, the stipend effects underestimate the effect of the treatment on pass rates as lower ability students are induced to take the test. An extreme assumption would be to assume that all those who did not take the exam would have failed in any case, then scholarship would have reduced the failure by 8 percentage points (31 percent) whereas stipend would have reduced the failure by 14 percentage points (57 percent). We can reject the null of same effect of the two treatments at 10 percent significance under this assumption. We have to be cautious in interpreting the effect on failure rates as an increase in learning, as the tests are designed and administered by the schools themselves. Conditional on passing, we cannot reject the null of no effect of either treatment on test scores (bottom row of Table 5).

The "You get what you pay for" result evident in the attendance effects of the scholarship treatment extends to both treatments when we look at schooling outcomes after the year of support. We cannot reject the null of no effect of either treatment on schooling past the year of support. These findings are in Table 6. In the year after support, we do not observe a substantive difference in enrollment between the treatment groups (row 1). The scholarship and stipend groups are slightly less likely to pass their grade of enrollment in the year after support (although these negative coefficients are not statistically significant, row 2).[21] Enrollment falls off from one year past support to two years past support, and we cannot reject a null of no difference in school enrollment for the year or attendance in the month of the follow-up survey (two academic years after the year of support) across treatment groups. Although stipend subjects were 44 percent less likely to fail school during the year of support (Table 5), we are not able to reject the null hypothesis that they have no higher schooling attainment after one and a half year. Hence, there do not appear to be lasting effects of the treatment beyond the period of support for schooling.

During the year of support, the effects of the stipend are largest for girls. Table 7 bifurcates the sample by gender. It is apparent that most of the statistically significant effects of the stipend in Table 5 came from the response of girls to the stipend (although boys show significantly higher attendance rates as well). The impact of the stipend is especially large for the failure rate of girls (reducing the probability of failing by 66 percent). While the magnitudes of the effect of the scholarship are larger for girls, we cannot reject the null

[21] Since we do not have administrative records past the year of support, school attendance and failure rates in Table 6 is based on self reported measures. We suspect the difference in failure rates in the control sample in the year after support owes to this difference in data source. Further, the Year + 1 measures in this table likely suffer from recall bias as respondents were asked about schooling status up to 15 months prior to the followup survey. Hence, the control group attendance in Table 6 appears to be slightly larger than self reported attendance measure for the program year in Table 5.

Table 7: Treatment Effects on Schooling Outcomes by Gender

| | Control<br>mean/(sd)<br>(1) | Scholarship<br>-Control<br>b/(se)<br>(2) | Stipend<br>-Control<br>b/(se)<br>(3) | Stipend<br>-Scholarship<br>b/(se)<br>(4) |
|---|---|---|---|---|
| *Boys* | | | | |
| Attendance in last year (parent) | 0.911 | 0.045 | 0.038 | −0.007 |
| | (0.286) | (0.043) | (0.029) | (0.032) |
| Share of months with any attendance (admin) | 0.881 | 0.049 | 0.053 | 0.004 |
| | (0.297) | (0.042) | (0.035) | (0.033) |
| School year attendance rate (admin) | 0.801 | 0.041 | $0.079^{**}$ | 0.038 |
| | (0.278) | (0.041) | (0.034) | (0.034) |
| Sat for final exams | 0.871 | 0.019 | $0.073^{*}$ | 0.054 |
| | (0.337) | (0.053) | (0.037) | (0.041) |
| Normalized schooling measure | −0.000 | 0.144 | $0.233^{**}$ | 0.089 |
| | (0.921) | (0.140) | (0.095) | (0.097) |
| Failed current grade (if took exams) | 0.136 | −0.071 | −0.041 | 0.030 |
| | (0.345) | (0.051) | (0.048) | (0.036) |
| Score in final exams (standardized) if not fail | −0.071 | −0.004 | 0.019 | 0.023 |
| | (0.963) | (0.158) | (0.158) | (0.157) |
| Enrolled in School (Year + 1) | 0.930 | −0.008 | 0.024 | 0.032 |
| | (0.256) | (0.043) | (0.032) | (0.036) |
| Did not fail enrolled grade (Year + 1) | 0.900 | −0.032 | −0.036 | −0.003 |
| | (0.302) | (0.056) | (0.053) | (0.050) |
| Enrolled in School (Year + 2) | 0.810 | 0.018 | −0.006 | −0.024 |
| | (0.394) | (0.055) | (0.056) | (0.056) |
| Attended school in current month (Year+2) | 0.770 | 0.030 | −0.047 | −0.077 |
| | (0.423) | (0.054) | (0.056) | (0.058) |
| Completed Years of Schooling (Year+2) | 6.790 | −0.367 | −0.051 | 0.316 |
| | (2.311) | (0.248) | (0.174) | (0.197) |
| *Girls* | | | | |
| Attendance in last year (parent) | 0.924 | 0.004 | $0.055^{*}$ | $0.050^{*}$ |
| | (0.267) | (0.040) | (0.030) | (0.029) |
| Share of months with any attendance (admin) | 0.882 | 0.026 | $0.087^{***}$ | $0.060^{**}$ |
| | (0.295) | (0.032) | (0.031) | (0.023) |
| School year attendance rate (admin) | 0.820 | 0.019 | $0.102^{***}$ | $0.083^{***}$ |
| | (0.284) | (0.031) | (0.029) | (0.022) |
| Sat for final exams | 0.874 | 0.016 | $0.097^{***}$ | $0.081^{***}$ |
| | (0.333) | (0.039) | (0.033) | (0.028) |
| Normalized schooling measure | −0.000 | 0.087 | $0.310^{***}$ | $0.223^{***}$ |
| | (0.849) | (0.098) | (0.087) | (0.062) |
| Failed current grade (if took exams) | 0.135 | −0.063 | $-0.089^{**}$ | −0.026 |
| | (0.343) | (0.043) | (0.034) | (0.037) |
| Score in final exams (standardized) if not fail | 0.060 | −0.108 | 0.092 | 0.200 |
| | (1.032) | (0.164) | (0.151) | (0.157) |
| Enrolled in School (Year + 1) | 0.932 | 0.002 | 0.004 | 0.003 |
| | (0.253) | (0.025) | (0.023) | (0.024) |
| Did not fail enrolled grade (Year + 1) | 0.927 | 0.011 | −0.028 | −0.039 |
| | (0.262) | (0.035) | (0.034) | (0.032) |
| Enrolled in School (Year + 2) | 0.821 | 0.064 | 0.022 | −0.042 |
| | (0.385) | (0.046) | (0.043) | (0.038) |
| Attended school in current month (Year+2) | 0.786 | 0.055 | 0.046 | −0.008 |
| | (0.412) | (0.049) | (0.042) | (0.039) |
| Completed Years of Schooling (Year+2) | 6.521 | 0.041 | 0.138 | 0.097 |
| | (2.284) | (0.214) | (0.194) | (0.174) |

$^{*}: p < 0.1; ^{**}: p < 0.05; ^{***}: p < 0.01$. Each row is from a different regression with the row variable as the dependent variable. All regressions include age × gender fixed effects as well as dummies for randomization strata and controls for baseline time allocation. Standard errors are clustered by establishment. The top panel refers to boys and and the bottom panel refers to girls. Column (1) is the mean and standard deviation of the row variable for the control group. Column (2) indicates the coefficient and standard error on an indicator for scholarship treatment group. Column (3) indicates the coefficient and standard error on an indicator for stipend treatment group. Column (4) presents difference between coefficients in columns (2) and (3) and associated standard error. Variable sources are as described in previous tables.

of no gender difference in the stipend or scholarship treatment effects for any outcome in the table. As with the pooled sample, we cannot reject the null hypothesis that the impact of the stipend on schooling for girls lasted only during the year of support.

*4.2. Child Labor*

The purpose of this intervention, from the funder's perspective, was to stop children from weaving. Weaving is considered a hazardous form of child labor because of its association with long-term spinal injuries, arthritis, respiratory ailments, and eyesight damage as well as the prevalence of forced and bonded labor in the sector. We find that the stipend intervention substantively reduced weaving among treated girls during the period of support.

Our measure of child labor is self-reported. We do not have data from other sources, for instance the employers, to validate this. Given that employing children under 14 is illegal in Nepal and since the establishments are under inspections of NGF, it is unlikely that we would obtain credible estimates from the employers. One could be concerned that respondents provided socially desirable response of not sending children to work, and also that treatment respondents would be scared of losing privileges from NGF if they revealed their children to be working in the carpet sector. A few observations mitigate this concern in our case. Even at baseline, 9.5 percent of our sample are reported to be involved in the carpet sector, which is close to the 9.8 percent of our children involved in any form of wage work. This suggests that our respondents do not consider under-reporting carpet-related work to be any more socially desirable than other less-scrutinized forms of child work. This rate, as discussed earlier, is three times higher than wage involvement of poor children living in Kathmandu Valley. Furthermore, the respondents are told that their responses would be kept confidential and would not be shared with any other organization, and no question in the questionnaire pertain directly to the benefits they may be receiving from NGF. Most importantly, no questions pertaining to involvement in the carpet sector were asked while subjects were receiving support. Since participants were explicitly told that the benefit would last only for a year and benefits were never tied to child labor, there is no reason that they should misreport with hopes of getting continued benefit from NGF. Hence, we proceed with the self-reported measure of child labor.

At yearend, 11.4 percent of control children participated in weaving at some point during the year of support. 12.9 percent of control children participated in weaving 16 months past the year of support. It may seem surprising that an intervention aimed at preventing a hazardous form of child labor would deal with subjects whose probability of entering into the activity are far less than one during the period of support. However, this is not at all unusual for education interventions aimed at deterring entry into hazardous child labor.[22] These illegal activities are rare, so it would be surprising if we were were able to identify a subject pool with a transition probability close to 1. Given that the probability a child works in any form for pay in Kathmandu is 3.8 percent, we think it is appropriate to view children with counterfactual probabilities of being involved in a hazardous form of child labor of more than 10 percent as children who are vulnerable to hazardous child labor.

Table 8 contains the impact of the scholarship and stipend treatments on weaving in the pooled sample. We can measure child involvement in weaving at 4 points in time: in the 7 days before the interview in the yearend survey, in the 12 months before the yearend survey (including the 7 days before the interview and including most of the year of support), in the 30 days before the follow-up survey, and in the 12 months before the follow-up survey (which would include the 30 days). We begin the table with the 7 days before the interview measure, because we also have hours worked data to match that recall period. 7.3 percent of control subjects weaved in the 7 days prior to the yearend survey. On average, those that weaved did so for 30 hours over the previous 7 days. It is useful to note that this is not total time working, only time spent weaving. The stipend is associated with a 4.3 percentage point or 59 percent reduction in weaving. The decline in hours worked associated with the stipend is greater than would be predicted by the decline in participation alone. The decline in participation in the stipend population, using the control's hours as a counterfactual, implies that the coefficient on the stipend treatment in row 2 should be -1.253 instead of the observed -1.497. The larger observed treatment effect might reflect a treatment effect on the intensity of weaving, or it might reveal that children whose weaving is most influenced by the stipend are those who would have woven more time-intensively absent the treatment.

The 7 days prior to the yearend survey does not include the period where conditionality was enforced for the stipend. However, we think the reduction in weaving in the 7 days prior to the yearend survey may

[22] The U.S. Department of Labor's Office of Child Labor, Forced Labor, and Human Trafficking regular publishes research on its website that report on interventions aim at populations vulnerable to hazardous child labor.

Table 8: Treatment Effects on Child Labor

| | Control<br>mean/(sd)<br>(1) | Scholarship<br>-Control<br>b/(se)<br>(2) | Stipend<br>-Control<br>b/(se)<br>(3) | Stipend<br>-Scholarship<br>b/(se)<br>(4) |
|---|---|---|---|---|
| Child weaved in past 7 days (yearend) | 0.073 | $-0.011$ | $-0.043^{**}$ | $-0.032^{*}$ |
| | (0.261) | (0.025) | (0.020) | (0.018) |
| Hours weaved in past 7 days (yearend) | 2.178 | $-0.585$ | $-1.497^{*}$ | $-0.912$ |
| | (9.431) | (0.990) | (0.761) | (0.612) |
| Child weaved in past 12 months (yearend) | 0.114 | $-0.007$ | $-0.055^{**}$ | $-0.048^{*}$ |
| | (0.319) | (0.028) | (0.027) | (0.028) |
| Child weaved in past 30 days (Year+2) | 0.092 | $-0.011$ | $-0.016$ | $-0.006$ |
| | (0.290) | (0.024) | (0.026) | (0.025) |
| Child weaved in past 12 months (Year+2) | 0.129 | $-0.009$ | $-0.026$ | $-0.017$ |
| | (0.336) | (0.026) | (0.028) | (0.027) |

$^{*}: p < 0.1$;$^{**}: p < 0.05$;$^{***}: p < 0.01$. Each row is from a different regression with the row variable as the dependent variable. All regressions include age × gender fixed effects as well as dummies for randomization strata and controls for baseline time allocation. Standard errors are clustered by establishment. Column (1) is the mean and standard deviation of the row variable for the control group. Column (2) indicates the coefficient and standard error on an indicator for scholarship treatment group. Column (3) indicates the coefficient and standard error on an indicator for stipend treatment group. Column (4) presents difference between coefficients in columns (2) and (3) and associated standard error. All variables are reported during the Followup and Yearend Survey.

reflect the reduced labor supply during the period of support. The mode carpet size being manufactured in Kathmandu at the time of our study was 8 foot x 10 foot with 100 knots per square inch. Carpets are knotted in teams who work in a line. The consistency of knotting in the product requires that the team is held constant throughout the manufacturing process. Thus, it would not be unusual for a team to spend more than a month dedicated to an 8x10 carpet during normal work periods. The yearend survey was conducted around the Nepali New Year's celebrations. It would not be surprising to learn that carpets being manufactured at the time of our yearend survey were started when conditionality was enforced in the stipend intervention. Hence, the impact of the stipend on weaving participation in the past 7 days during the yearend survey should reflect a combination of the impact of the stipend while schooling was incentivized and whatever decisions were being made after the end of support.

We prefer the measure of child weaving the 12 months prior to the yearend survey as our measure of the impact of the stipend treatment on weaving during the period of support. The stipend reduces the probability the child was engaged in weaving during the year of support by 5.5 percentage points or 48 percent. The scholarship does not appear to be associated with a reduction in weaving, and we can reject the null of no difference between the scholarship and stipend treatments at 10 percent. The magnitude of the reduction in weaving is comparable to the magnitude of increase in schooling during the program year (5.5 percentage point decline in weaving compared to a 4.9 percentage point increase in having attended school). This finding is different from most of the literature discussed above which finds comparatively small impact on child labor relative to the impact on schooling. Given our setting where children live inside or very close to the establishment, children have better access to work opportunities. The set of people that the stipend induced to go to school (or not drop out) are more likely to be the same set of children who faced the trade-off between schooling and work. However, 16 months past the end of support, we cannot reject the null of no impact of either treatment on child engagement in weaving.

The reduction in child labor is concentrated in girls. Results bifurcated by gender are in Table 9. Boys are less likely to be involved in weaving in the last year compared to girls (6 percent vs. 16 percent), and even when they participate in weaving, boys do so for fewer hours on average than participating girls. Hence, we do not find it surprising that we observe larger effects of the stipend among girls. Girls' involvement in weaving in the year of support declines by 10 percentage points or 64 percent. Unlike schooling outcomes, where we are not able to reject the null of no gender difference, we reject that null for participation in weaving in the last 7 days (at 10 percent) and the last 12 months (5 percent). Hence, female involvement in weaving is more impacted by the stipend than is male involvement. Despite the larger initial effects, we cannot reject the null of no effect for boys or girls in the followup survey, 16 months past the end of the intervention.

Table 9: Treatment Effects on Child Labor by Gender

| | Control<br><br>mean/(sd)<br>(1) | Scholarship -Control<br>b/(se)<br>(2) | Stipend -Control<br>b/(se)<br>(3) | Stipend - Scholarship<br>b/(se)<br>(4) |
|---|---|---|---|---|
| *Boys* | | | | |
| Child weaved in past 7 days (yearend) | 0.040 | 0.013 | −0.012 | −0.025 |
| | (0.196) | (0.024) | (0.020) | (0.029) |
| Hours weaved in past 7 days (yearend) | 1.129 | 0.013 | −0.003 | −0.017 |
| | (6.067) | (0.539) | (0.841) | (0.639) |
| Child weaved in past 12 months (yearend) | 0.059 | 0.034 | 0.006 | −0.028 |
| | (0.238) | (0.033) | (0.030) | (0.041) |
| Child weaved in past 30 days (Year+2) | 0.050 | 0.023 | −0.016 | −0.039 |
| | (0.219) | (0.029) | (0.024) | (0.031) |
| Child weaved in past 12 months (Year+2) | 0.060 | 0.034 | −0.014 | −0.048 |
| | (0.239) | (0.033) | (0.028) | (0.032) |
| *Girls* | | | | |
| Child weaved in past 7 days (yearend) | 0.102 | −0.033 | $-0.076^{**}$ | −0.043 |
| | (0.304) | (0.038) | (0.031) | (0.027) |
| Hours weaved in past 7 days (yearend) | 3.076 | −0.932 | $-2.815^{**}$ | $-1.883^{**}$ |
| | (11.510) | (1.709) | (1.210) | (0.948) |
| Child weaved in past 12 months (yearend) | 0.161 | −0.037 | $-0.103^{**}$ | −0.066 |
| | (0.369) | (0.042) | (0.042) | (0.040) |
| Child weaved in past 30 days (Year+2) | 0.128 | −0.045 | −0.023 | 0.021 |
| | (0.336) | (0.039) | (0.042) | (0.040) |
| Child weaved in past 12 months (Year+2) | 0.188 | −0.053 | −0.046 | 0.007 |
| | (0.392) | (0.042) | (0.047) | (0.046) |

$^{*}: p < 0.1; ^{**}: p < 0.05; ^{***}: p < 0.01$. Each row is from a different regression with the row variable as the dependent variable. All regressions include age × gender fixed effects as well as dummies for randomization strata and controls for baseline time allocation. Standard errors are clustered by establishment. The top panel refers to boys and and the bottom panel refers to girls. Column (1) is the mean and standard deviation of the row variable for the control group. Column (2) indicates the coefficient and standard error on an indicator for scholarship treatment group. Column (3) indicates the coefficient and standard error on an indicator for stipend treatment group. Column (4) presents difference between coefficients in columns (2) and (3) and associated standard error. Variable sources are as described in previous tables.

## 5. Spillovers and Household Consumption

### *5.1. Spillovers*

As noted in the empirical section, randomization was at the child level. If there are important spillovers in time allocation or schooling within household, our estimates of the effect of the stipend or scholarship treatment will be biased. We suspect two potential types of spillovers. First, we expect family resources to be shared. The financial support implicit within both interventions should lead to attenuated treatment effects above. Second, we think it is plausible that the change in the relative return to schooling implicit with the stipend could induce the control children to work more and attend school less, implying that our findings above exaggerate the impact of the stipend. A similar dynamic is documented in the context of child labor laws by Manacorda (2006).

To examine spillovers, we limit our sample to the 220 children in the control group. We modify equation (1) to control for the number of subjects in the family (which is non-random), $N_j$. Conditional on $N_j$, the presence of a scholarship winner $ScW_j$or a stipend winner $StW_j$in the family will be random. We estimate:

$$y_{ijkt} = \beta A_{i0} + b_0 + \delta_0 N_j + \delta_1 ScW_j + \delta_2 StW_j + \alpha Y_{ijk0} + \pi_{ijk0} + e_{ijkt} \tag{2}$$

where $\delta_1$ is how outcome $y$ differs for a control child when there is a scholarship winner in the family (compared to no scholarship winner), and $\delta_2$ is how outcome $y$ differs for a control child when there is a stipend winner in the family. Estimates of $\delta_0$, $\delta_1$, and $\delta_2$ are reported in Table 10. Since the number of subjects in the family is non-random, $\delta_0$ will not have a causal interpretation. We focus on columns (2) and (3).

Table 10: Within Household Spillovers on Control Children

| | Number of Subjects in HH b/(se) (1) | Has Scholarship child in HH b/(se) (2) | Has Stipend child in HH b/(se) (3) |
|---|---|---|---|
| Share of months with any attendance (admin) | 0.061 | 0.054 | 0.022 |
| | (0.056) | (0.064) | (0.076) |
| School year attendance rate (admin) | 0.045 | 0.031 | 0.017 |
| | (0.055) | (0.061) | (0.072) |
| Sat for final exams | 0.062 | 0.062 | 0.037 |
| | (0.058) | (0.071) | (0.078) |
| Normalized schooling measure | 0.046 | 0.302 | 0.210 |
| | (0.163) | (0.193) | (0.214) |
| Failed current grade (if took exams) | 0.065 | −0.036 | −0.078 |
| | (0.055) | (0.078) | (0.084) |
| Score in final exams (standardized) if not fail | −0.172 | −0.147 | 0.071 |
| | (0.197) | (0.262) | (0.294) |
| Enrolled in School (Year + 1) | −0.038 | 0.000 | 0.068 |
| | (0.054) | (0.054) | (0.072) |
| Did not fail enrolled grade (Year + 1) | −0.043 | 0.095 | 0.037 |
| | (0.055) | (0.060) | (0.090) |
| Enrolled in School (Year + 2) | −0.077 | 0.064 | 0.106 |
| | (0.058) | (0.070) | (0.075) |
| Attended school in current month (Year+2) | −0.109 | 0.066 | 0.155* |
| | (0.074) | (0.091) | (0.092) |
| Completed Years of Schooling (Year+2) | 0.092 | 0.296 | 0.178 |
| | (0.225) | (0.370) | (0.391) |
| Child weaved in past 7 days (yearend) | 0.074 | 0.013 | −0.078 |
| | (0.054) | (0.053) | (0.053) |
| Hours weaved in past 7 days (yearend) | 3.009 | −0.895 | −2.527 |
| | (2.081) | (2.162) | (1.915) |
| Child weaved in past 12 months (yearend) | 0.033 | 0.089 | −0.022 |
| | (0.051) | (0.055) | (0.055) |
| Child weaved in past 30 days (Year+2) | 0.003 | −0.006 | 0.008 |
| | (0.036) | (0.047) | (0.042) |
| Child weaved in past 12 months (Year+2) | −0.041 | 0.100* | 0.055 |
| | (0.042) | (0.058) | (0.044) |

$^{*}: p < 0.1$;$^{**}: p < 0.05$;$^{***}: p < 0.01$. Sample restricted to control children only. Each row is from a different regression with the row variable as the dependent variable. All regressions include age × gender fixed effects as well as dummies for randomization strata and controls for baseline time allocation. Standard errors are clustered by establishment. Column (1) indicates the coefficient and standard error on the number of study subjects in the household. Column (2) indicates the coefficient and standard error on an indicator for whether the household has a child from scholarship group. Column (3) indicates the coefficient and standard error on an indicator for whether the household has a child from stipend group. Variable sources are as in previous tables.

We cannot reject the null of no spillovers for either the scholarship or stipend interventions. This lack of spillover is not what readers of studies documenting family spillovers in school attendance from conditional cash transfer programs would expect (e.g. Angelucci and De Giorgi, 2009; Bobonis and Finan, 2009; and Barrera-Osorio et al., 2011). The transitory nature or total economic value of the support provided in this context could explain the difference between our findings and that of published studies on spillovers which all consider enduring social programs. Overall, we do not believe spillovers are generating our treatment effects herein.

*5.2. Household consumption and expenditure*

One of the channels through which transitory support could have lasting impact is through support smoothing behavior. Though we did not see any effect on the schooling and child labor behavior in the years following schooling, there could be smoothing going on in other expenditure margins which may not affect schooling and child time allocation in the future years. To explore the impact on household expenditure, we use the data collected during the yearend survey.[23] The top panel of Table 11 examines the effect on household expenditures for all households, whereas the bottom panel restricts to households with only one program child. Though the bottom panel has a clearer interpretation of the effect of the treatment for these household level variables, the sample size is substantively smaller and single subject households are substantively different (they are smaller, for example).

Ex-ante, our expectation was that forward-looking households would exhibit permanent income behavior: treat the one year of support as a transitory income shock, and save a significant portion of the transfer, at least for the stipend treatment. The stipend was paid as in-kind store credit, but since households would consume more rice and pulses than what the transfers offered, we interpret the transfers as being infra-marginal. Hence, there would be ample means to save the stipend.

Additional savings does not appear to have materialized. Only 17 percent of households report having some savings (20 percent of single subject families). Stipend recipients do not appear more likely to save. The lack of saving may reflect that these households do not have appropriate savings vehicles. Most people who report savings, report doing so in form of cash in the household as less than 10 percent of our sample have bank accounts at baseline. We do not think the in-kind nature of the stipend explains the lack of savings effect, because we do not think it will be binding for our subjects. In absence of proper savings mechanisms, purchase of asset or durables could be another way that households could save. We cannot reject the null of no change in whether the household purchases assets and durables for either treatment (coefficients are actually negative). However, conditional on spending positive amounts on assets or durables, stipend households spend 80 percent more than control households. The extensive margin dominates the intensive margin for assets and durables so that the combined effect cannot reject the null of no change in expenditures on assets and durables (not shown), but there is at least some margin of asset accumulation associated with the stipend for households that are accumulating assets.

Some of the stipend seems to have been spent, particularly by households with many program children. In our full sample (top panel), stipend-receiving household spends 6 percent more on food and 9 percent more on total expenditure when including consumer cost of the in-kind transfer in expenditures. This translates to 26 percent of the stipend being spent on food and 78 percent of the total transfers being spent. The corresponding elasticities are slightly lesser when the sample is restricted to households with only one program child and the data do not reject the null of no change in food or total expenditures in single subject households.

Remittances seem to respond to the stipend. Most of the workers in our families are migrants from rural Nepal and do not identify their current residence as their permanent "home". These migrant households also have members who migrate elsewhere. We did not collect information on extended household members or members that were absent from the household. However, in our attempt to collect consistent information on household members throughout the survey period, we found that 15 percent of the control households had a member who had migrated abroad (excluding India) in the 14 months between the baseline and the yearend survey. Households in our sample are net receivers of remittances. In the control sample, the average household receives 14 percent of total expenditure as remittances. Table 11 shows that stipend households receive less remittance than the control sample: 57 percent lower for the full sample and 40 percent lower for the single subject sample. This displacement of remittances is similar to what Jensen (2004) finds in South Africa.

[23]Unfortunately, we do not have data on expenditure after the support year to see if the treatment have effects on educational expenditures past the year of support.

Table 11: Treatment Effect on Household Spending

| | Control | Scholarship -Control | Stipend -Control | Stipend -Scholarship |
|---|---|---|---|---|
| | mean/(sd) | b/(se) | b/(se) | b/(se) |
| | (1) | (2) | (3) | (4) |
| *All Households* | | | | |
| Household reports savings | 0.169 | 0.028 | −0.001 | −0.029 |
| | (0.376) | (0.034) | (0.039) | (0.041) |
| Amount savings (NPR) | 391.781 | −49.372 | −113.516 | −64.144 |
| | (1611.776) | (124.963) | (130.779) | (95.169) |
| Log(education expenses, scholarship inc) | 9.112 | 0.502*** | 0.584*** | 0.082 |
| | (1.886) | (0.159) | (0.163) | (0.092) |
| Log(food expenses, stipend inc) | 10.935 | 0.031 | 0.064*** | 0.033 |
| | (0.312) | (0.028) | (0.024) | (0.030) |
| Log(total expenses, benefits inc) | 11.644 | 0.043 | 0.092*** | 0.049* |
| | (0.364) | (0.027) | (0.024) | (0.028) |
| HH spends on assets and durables | 0.265 | −0.015 | −0.015 | −0.000 |
| | (0.442) | (0.037) | (0.040) | (0.043) |
| Log(expenses in assets and durables) if spent | 6.457 | 0.486 | 0.811*** | 0.325 |
| | (1.443) | (0.308) | (0.275) | (0.333) |
| Net remittance sent ('000 NPR) | −16.169 | −3.168 | 6.953* | 10.120** |
| | (50.834) | (3.712) | (4.127) | (5.058) |
| Log (total labor income) | 11.120 | 0.091 | 0.072 | −0.018 |
| | (0.999) | (0.071) | (0.074) | (0.055) |
| *Households with only 1 program child, N=274* | | | | |
| Household reports savings | 0.202 | 0.004 | −0.086 | −0.091 |
| | (0.404) | (0.066) | (0.060) | (0.065) |
| Amount savings (NPR) | 576.404 | 19.968 | −152.477 | −172.445 |
| | (2304.542) | (197.998) | (153.009) | (191.314) |
| Log(education expenses, scholarship inc) | 8.844 | 0.526*** | 0.594*** | 0.067 |
| | (1.658) | (0.175) | (0.198) | (0.125) |
| Log(food expenses, stipend inc) | 10.861 | −0.001 | 0.022 | 0.024 |
| | (0.335) | (0.049) | (0.036) | (0.048) |
| Log(total expenses, benefits inc) | 11.552 | 0.002 | 0.060 | 0.058 |
| | (0.371) | (0.056) | (0.044) | (0.052) |
| HH spends on assets and durables | 0.326 | −0.077 | −0.130** | −0.053 |
| | (0.471) | (0.063) | (0.065) | (0.070) |
| Log(expenses in assets and durables) if spent | 6.428 | 0.575 | 0.912* | 0.338 |
| | (1.428) | (0.655) | (0.484) | (0.590) |
| Net remittance sent ('000 NPR) | −13.043 | −3.721 | 7.795 | 11.516*** |
| | (34.804) | (5.335) | (5.310) | (3.377) |
| Log (total labor income) | 10.997 | 0.160 | 0.179 | 0.019 |
| | (1.334) | (0.177) | (0.177) | (0.086) |

$^{*}: p < 0.1$; $^{**}: p < 0.05$; $^{***}: p < 0.01$. Each row is from a different regression with the row variable as the dependent variable. All regressions include age × gender fixed effects as well as dummies for randomization strata and controls for baseline time allocation. Standard errors are clustered by establishment. Column (1) is the mean and standard deviation of the row variable for the control group. Column (2) indicates the coefficient and standard error on an indicator for scholarship treatment group. Column (3) indicates the coefficient and standard error on an indicator for stipend treatment group. Column (4) presents difference between coefficients in columns (2) and (3) and associated standard error. All variables are reported in Yearend Survey.

Overall, we see that the stipend is associated with changes in spending in the subject household and redistribution of resources across family members living elsewhere. Between the two, it is not surprising that we do not observe any savings of the stipend in subject households (although it could have resulted in observed changes in savings in other households involved in remittance flows). This finding of no savings is consistent with our observation that the impact of the stipend on recipients does not persist past the period of support.

## 6. Conclusion

Combating child labor through education promotion is at the center of anti-child labor policy aimed at ending the involvement of 85 million children worldwide in hazardous child labor. We evaluate the impact of a scholarship program designed to cover direct, out-of-pocket schooling expenses and a stipend program that included the scholarship plus an additional in-kind stipend conditional on school attendance in the previous month aimed at children vulnerable to child labor in the hand-made export-oriented carpet sector of Nepal.

We have found small, positive effects of a scholarship program that covered out-of-pocket schooling expenses on whether subjects attended school at all at the start of the period of support. These treatment effects on schooling dissipate gradually over the year with a distinct drop in attendance after October holidays. We found larger, substantive effects of the combined scholarship and stipend program that covered school expenses and provided a stipend conditional on school attendance. The stipend treatment appeared to reduce child involvement in carpet weaving, a worst form of child labor in Nepal. The impact of the stipend is largest on girls, reducing the involvement of girls in weaving by 75 percent and reducing the probability that a girl failed her grade by 66 percent.

However, after 16 months, we find no evidence of the program in schooling attendance, schooling attainment, or child labor.

These findings have important implications for child labor policy. This is the first field experiment targeted specifically at a hazardous form of child labor, carpet-weaving. In fact, the project was funded by the U.S. Department of Labor as a pilot for using field experiments to improve child labor policy. Since 2001, the promotion of education has been central in efforts to reduce hazardous child labor around the world. Despite the policy focus, we do not have direct evidence of a connection between hazardous forms of child labor and the net return to education. Our finding that scholarship alone is not sufficient to keep children in school throughout the school year suggests that foregone child labor earnings are an important determinant of time allocation decision for children vulnerable to hazardous forms of child labor. The effectiveness of additional stipend in keeping children in school and reducing hazardous child labor by a magnitude comparable to the increase in schooling suggests that at least in case of vulnerable children, who have easy access to work opportunities, educational support can effectively deter hazardous child labor.

That said, our findings also illustrate that the impact of education support on child labor need not persist past the period of support. Almost all funding to combat child labor is project funding. Project funding is by definition transitory.[24] The idea of combating child labor with project funding is based on the idea that there are permanent effects of transitory support. In a classical labor supply model, transitory changes in the net return to schooling would induce a transitory change in time allocation. Hence, it is not obvious why transitory support should have permanent effects, but there are several potential arguments that would imply permanent effects of transitory project funding. First, there is the hope that the impact of the support will endure. For example, if liquidity constrained families pull children out of school in response to temporary shocks, but some cost of rejoining school prevents children from going to school after the temporary shock has abated (as in De Janvry et al., 2006), the temporary support can have lasting effects by buffering the temporary shock. Second, permanent-income theory suggests that the marginal propensity to consume out of transitory income should be zero. Hence, the recipients should smooth the impact of transitory support over time. It is, however, not clear whether smoothing would necessarily imply increased schooling or reduced child labor in future periods. Third, being in school one more year due to support induces an increase in preference for schooling. This might stem from changing expectations of returns to education (Attanasio and Kaufmann, 2014), new information that alters parental aspirations (Chiapa et al.,

[24]For example, the largest funder of child labor projects is the U.S. government. USG rules constrain projects to a maximum of 5 years. Our informal review of recent USG solicitations specifically targeted at child labor suggests 3 years is common at the solicitation stage. A 3 year contract would translate into at most 2 years of support for children as 3 years would include both start up and reporting time. The project examined below was solicited as 3 years, extended to 5, and provided 1 year of support to children identified as vulnerable to child labor.

2012), or habit formation (for instance, when agents who are provided high powered incentives to visit the gym regularly continue doing so even after the incentives have stopped, (Charness and Gneezy, 2009)). Our study is unusual in being able to follow up with subjects substantively past the period of support. We do not find any impact of schooling support on schooling and child labor after a year past the completion of the program. Hence, with regards to transitory education support to promote schooling and deter child labor in Nepal's carpet sector, you get what you pay for.

An obvious question is whether this "you get what you pay for" finding generalizes to other child labor policy contexts. USDOL is the largest single-source funder of anti-child labor projects in the world, and the project evaluated herein was funded by USDOL as a pilot of using impact evaluations to improve project efficacy. The interventions were chosen, because they are central in most USDOL funded projects directed at deterring entry into hazardous forms of child labor. The subject pool itself is not representative of the poor in Kathmandu or child laborers worldwide. However, their circumstances are similar to what the authors have experienced in working with other child labor projects. Children in projects aimed at deterring entry into hazardous child labor often have backgrounds as migrants and, in our experience, typically cohabitate with an adult caregiver. Transition probabilities into hazardous forms of child labor (absent a project) are often far from one for project beneficiaries, although all beneficiaries are coded as "rescued" child laborers in project reporting. Hence, we believe the results of this study are relevant to the broader child labor policy community. Our finding of substantive effects of schooling incentives while benefits are being distributed coupled with no detectable effect past the period of support raises important questions about the utility of transitory education support in projects aimed at combating child entry into hazardous child labor over the long term. We hope to see more research focusing explicitly on the transitory nature of education support in the future.

Our findings also have implications for future studies of hazardous forms of child labor. First, our findings illustrate the utility of the classical human capital literature in discussing child labor. Based on the existing literature, there is a question as to whether child labor responds to changes in the net return to attending school. We find clear evidence of a response of child labor and schooling to changes in the net return to schooling. In fact, it is striking how similar our findings are to the prediction of a static labor supply model. We can think of both interventions as inducing more schooling and less child labor by changing the relative price of schooling and shifting a portion of the budget curve. When support ends, the budget curves return to their pre-project state and so too does time allocation. The child labor literature has largely focused on deviations from the classical labor supply model to explain child labor supply, but it is straightforward to anticipate our findings in the classical model without the need for any of the additional mechanics developed in the academic literature. Second, we illustrate the value of evaluating projects substantively past the conclusion of the project. It is not typical for field projects to have follow-ups that extend substantively past the end of the project because of the interconnection of project and evaluation funds. In the present case, the policy's goal was to have lasting effects on child labor through transitory project support, and very different conclusions about the impact of this project emerge from the project funded yearend survey and the separately funded follow-up survey. Especially for policy oriented evaluations like this one, we think our findings illustrate the importance of considering the time horizon of data collection when assessing the impact of development projects.

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