You Get What You Pay For: The Transitory Effects of Transitory Schooling Support in a Population Vulnerable to Child Labor

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Transitory schooling support is at the center of anti-child labor policy. Forward-looking households should smooth the impact of transitory transfers and extend the impact of transitory assistance beyond the period of support. We reject this hypothesis in an evaluation of a schooling assistance intervention targeted at children associated with carpet factories in the Kathmandu Valley of Nepal. We find that a transfer conditional on school attendance promotes schooling, deters child migration, and reduces child engagement in carpet weaving, a worst form of child labor in Nepal, while the child is supported. The impact of the intervention persists briefly pass the period of support, but we see no effects of the intervention after 15 months.

The U.S. Government (USG) is the largest funder of anti-child labor interventions in 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.[1] Starting in 2001, the Child Labor Education Initiative (EI) placed the promotion of education at the center of project tools to combat child labor. EI projects usually pay school fees and sometimes provide living support to children identified as vulnerable to child labor by the project. This education support is necessarily transitory as USG rules constrain projects to a

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[1] Other USG agencies fund projects that are related to child labor in foreign countries with the same appropriation restrictions as DOL. However, other agencies do not have child labor in their mission statement. We cannot obtain estimates of spending specifically on child labor from other USG agencies.

maximum of five years (31 USSC 1552), and informal review of recent USG solicitations specifically targeted at child labor suggests 3 years is common at the solicitation stage.[2]

We consider the impact of transitory schooling support in a population vulnerable to child labor. Specifically, we examine the impact of a DOL project to promote the education of children associated with workers in the handmade export-oriented carpet sector of the Kathmandu Valley of Nepal. A lottery was used to allocate children to three groups:

1. A control group whose schooling and time allocation would be monitored but who 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 schooling attendance. The stipend was paid as a credit in a local shop that could be used towards food.[3] Most bought rice.

The treatments lasted one academic year. The scholarship treatment was identical to a sponsored education program operated for other children by the NGO implementing the project, the Nepal Goodweave Foundation. Both the scholarship and stipend treatment were similar to other EI projects in what they did as well as how they were administered and structured.

We hypothesize that forward-looking agents smooth transitory education support so that its impact extends beyond the period of support. The scholarship intervention (2) changes the relative price of schooling. The substitution effect should increase schooling and the income effect should be smoothed over multiple periods. The stipend intervention (3) has a larger change in the relative price of schooling than the scholarship and we expect even more smoothing. This hypothesis of effects of project support beyond the period of support is central to the premise of the value of education support to combat child labor.

We find that the impact of schooling support is limited to the period of support. Scholarship funds were largely exhausted at the start of the school year, and the scholarship treatment seems to promote school attendance only at the first part of the school year. The stipend treatment has a large impact on attendance that seems to last even into the weeks past the end of the program. The stipend also leads to decline in child involvement in weaving a few

[2] 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.

[3] USG rules in place at the time of the project prohibited the direct transfer of cash payments to beneficiaries in foreign countries.

weeks past the end of the period of support. Since carpet production can take weeks or even months, we suspect that the effects on weaving extending a few weeks past the end of support is a lingering effect of the reduction in weaving that occurs during the period of support. We find no effects of either intervention 15 months after the end of the program. You get what you pay for, and in this case the USG only paid to support schooling for a single academic year.

While both the scholarship and stipend programs have been studied in other countries, there are two important differences about this study. First, this is the only study we know of where the intervention is explicitly transitory and the evaluation extends substantively beyond the period of the intervention. All USG development projects must be transitory in nature, but this limited support period usually extends to the evaluation. Funding is only available to evaluate the project near its terminus. The present study had the same problem; it was principally a USG funded project. There is data showing a project impact collected at the end of the project, but separate funding had to be obtained to go back to the study population 15 months after the conclusion of the support. Thus, we know of no other study designed to explicitly evaluate the impact of transitory schooling support, the dominant tool in the global fight against child labor. Second, this is the only evaluation we know of a randomized intervention in a population vulnerable to a worst form of child labor. While child involvement in paid employment is often impacted by other schooling interventions (Shultz 2004; Filmer and Schady 2008), it is not incredible to think findings might be different for a population vulnerable to a worst form of child labor. First, the population consists entirely of migrant children living away from most of their extended family (typically only the mother is present if any parent co-resides). Second, the population largely lives inside a factory (or nearby when they do not) where wages paid to youth are substantially large than typically found in the area for uneducated labor. Hence, it is an open question as to whether these types of education support interventions are effective tools to combat worst forms of child labor. The two main questions this study aims to address (what are the impact of the interventions / do transitory programs persist) both fill in missing pieces in the current literature on promoting schooling and combatting child labor.

The next section describes the program. Section II considers the hypothetical impact of the treatments. Section III presents the findings and section IV discusses the results.

I. Background on the Schooling Incentives Project Evaluation (SIPE)

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, especially in the carpet sector, in Nepal. The SIPE study 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 received schooling support for the school year beginning in April 2010 and ending March 2011.

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. The Nepal Goodweave Foundation (NGF) designed and implemented SIPE, under ICF's contract with USDOL and with technical support from the authors.[4] Table 1 outlines the timing of the project and its evaluation. A Nepali NGO, New ERA Ltd. (NE), was contracted to collect data on study subjects for the evaluation.[5] NE was never informed about any subject's status in the project or any details about the project and had no responsibilities to the project or its evaluation other than data collection[6]. Program benefits began in April 2010 and ended in March 2011 (start to finish of one complete Nepali school year.

NGF's involvement in the project was motivated by a desire to improve the efficacy of its efforts to prevent children working on looms. NGF identified 660 children aged 10-16 years old who were the children of workers in licensee establishments and who it considered vulnerable to transitioning into child labor in carpet establishments.

NGF selected 101 establishments for SIPE. The 101 establishments were NGF licensees or subcontractors of NGF licensees as of January 2010. 100 of these establishments engaged in weaving, with 1 establishment reporting finishing work and dyeing wool but no weaving. 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.

SIPE targeted children 10-16 associated with carpet manufacturing establishments and who (a) had attended school within the last 18 months, (b) had not received education support from

[4] Leigh Linden was an important contributor to the early discussions of this project.
[5] New ERA was contracted by ICF Macro for the baseline, midyear, and yearend surveys. It was separately contracted by the authors for the followup survey. New ERA was able to keep staffing largely the same throughout all of its surveys.
[6] NE enumerators were explicitly prohibited from asking the respondent about their treatment status. However, nothing would in principle, prevent the respondent in revealing their treatment. NE reported few incidents of status revealing.

NGF or other sponsors and (c) were in families who faced an elevated risk of transitioning to child labor. NGF viewed children as having an elevated risk of transitioning to child labor if they met at least one of the following three criteria:

- *Family size criteria*: A family with 3 or more school aged children in residence.
- *Family income criteria*: Total monthly family income (including monetary support from kin/relatives living elsewhere) of NPR 8,500 if housing was not provided free of charge by an establishment[7] or of NPR 7,000 if housing was provided free of charge by the establishment
- *Sibling schooling criteria*: Families with 1 or fewer children attending school or children families with a child who dropped out before completing grade 8[8]

These criteria were chosen by NGF based on the field experience of NGF inspectors in its sponsored education program. The income criteria were also 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.[9] Assuming 85 percent of children lived in households with an adult earner implied an income of NPR 8500 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.

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 eligible children, the income cutoff was the most important for children in the SIPE study. For the 660 children in the SIPE study, Table 2 counts 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, and did not receive other education support.

Subjects could match multiple criteria. In fact, nearly two-thirds of study subjects met at least two of the criteria. Only 30 of the study subjects did not meet the low income criteria. Of these 30, 1 was in the study because of sibling dropouts, 3 were in the study because 1 or fewer siblings attend school, and the remaining 27 met the family size criteria, having 3 or more eligible children in the family. Comparing our pool to children in the Kathmandu Valley in the

[7] While there is some volatility of the NPR-USD exchange rate during the period of study, an exchange rate of 71.6NPR per USD is reasonable for the period of study.
[8] Grade 8 is considered the basic level of education and Government of Nepal intends to keep it free.
[9] Round 1 of the Labor Demand Survey in Nepal asked managers about wages necessary to hire new adult workers. Managers reported NPR 4800 per month as the wage required to hire a weaver (adult male or adult female).

2010 Nepal Living Standards Survey, this subject pool is younger and more likely to be female than a random sample of children from the Kathmandu Valley. The mother is less apt to be literate and the father is less likely to be present than is typical for the Valley.

Randomization was used to assign subjects to one of three groups:

- Group #1 is the control group. The children in Group 1 received no schooling-related assistance.
- Group #2 is the scholarship group. The children in Group 2 received a scholarship benefit. NGF reimbursed each child's schooling-related costs up to a maximum of (Nepali rupees) NPR 3,950 per year.[10] This assistance could include all schooling-related costs such as fees, tuition, uniforms, books and other supplies. This program was modeled after NGF's Sponsored Education Program.
- Group #3 is the stipend group. The children in Group 3 received the scholarship as Group #2 and an additional stipend. In addition to the scholarship, NGF provided a stipend of food rations valued at NPR 1,000 per month per child provided that the child attended school at least 80 percent of the days his or her school was open for teaching.[11] 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.

There are 220 subjects in each group. Subjects were stratified based on age, gender, and carpet establishment size before randomization and were allocated to each of the three groups within each cell.[12] All subjects assigned to the two treated groups took-up the benefits.

[10] The scholarship amount, NPR 3,950, was the yearly average cost for supporting one child through the NGF's own school support project in year 2008.

[11] The stipend amount was comparable to the opportunity cost of the child if they chose to work instead. NPR 1,000 per month was a third of the cash earning made by a youth under 20 working in weaving in a carpet establishment and was roughly equivalent to what a child might earn working as a domestic, a common occupation of migrant children in Kathmandu. The amount of the stipend was chosen based on NGF field interviewers. They asked eligible families how much would be required to obviate the need for child labor and felt strongly that NPR 1,000 per month was appropriate. NPR 12,000 per year was slightly less than average expenditures on grains and pulses by the poorest thirty percent of the Kathmandu Valley according to our calculations from the 2010 Nepal Living Standards Survey.

[12] Random assignment within each cell occurred by assigning each child a random number drawn from a uniform distribution bounded between 0 and 1. Within each cell, this random number sorted children. The bottom third were defined as the monitoring population, the middle third received the scholarship, and the highest third received the

We examined the validity of the randomization by comparing each group to the other two groups combined. That is, we examined whether a characteristic in the control population differed from what we observed in the combined populations that received assistance or whether a characteristic in the stipend population differed from the combined control and scholarship populations. Since our focus was on comparing the scholarship treatment to the scholarship plus stipend population, we also tested for differences between those two treated groups (without also including the control population). These comparisons are in Table 3.

Table 3 shows the validity of the randomization across the individual, household, and establishment characteristics collected prior to random assignment. Each cell in columns 1 -3 contains the mean of the indicated (row) variable for the indicated (column) treatment group. Each cell in column 4 contains the P-Value associated with the null hypothesis that the difference in means between columns 2 (the scholarship treatment group) and column 3 (stipend treatment group) is zero. The F-statistic at the bottom of columns 1-3 is for the null that the difference between the means in the column and those of the population in the other columns, taken jointly, is zero. The F-Statistics at the bottom of column 4 is for the null that the difference in means between the scholarship and the stipend groups are jointly zero. The P-value is the p-value associated with the F-statistics.

A few characteristics in Table 3 require explanation. The study contained more females than males. This reflects the population resident in carpet establishments. By the time children reach puberty, males were less likely than females to stay resident with parents in the carpet establishment. Roughly 70 percent lived within the walls of the establishment where they worked. These establishments were large with an average 41 workers. More than half of study children were in establishments whose total output was below the median of the establishments we observe. More than 1 in 5 of the establishments engaged in post-weaving finishing work in addition to weaving. Wool preparation activities were less frequent in weaving establishments. Daycares, scholarships, and health checks existed in the study population.

Table 3 does not present any reason to be considered about problems with the randomization. There do not appear to be any substantive differences in population

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scholarship and stipend combined. There was no guarantee that the number of children in each cell would be divisible by 3. When this happened, children were assigned to treatment populations to make sure that the number of children in group 2 in each cell matched the number of children in group 3. Balance in number between scholarship recipients was prioritized as the comparison between groups 2 and 3.

characteristics across the different treatment groups. Moreover, when the differences are considered jointly, as is appropriate given the non-independence of variables, they are jointly insignificant with P-Values very close to 1.

II. Theory – Permanent Effects of Transitory Education Support

We have in mind a model where a single agent makes decisions about schooling for her single child and consumption. There are two periods. In period one, she chooses her child's schooling and her savings. Her consumption follows out of these two decisions plus exogenous income.[13] In period two, she chooses her child's schooling. Schooling is costly in each period, entailing foregone consumption because of schooling costs and foregone child labor income.

Specifically, the agent maximizes utility by choosing consumption c_t, and education e_t in period $t = \{1,2\}$

$$(1) \qquad U(c_1,c_2,e_1,e_2) = u(c_1) + \delta u(c_2) + v(e_1) + \delta v(e_2)$$

where δ is the discount factor. We assume additive separability of utility across time and between consumption and education to simplify comparative statics. We assume that parental utility from education in each period incorporates any future economic return to education that the child or the agent incurs in the child's adult life.

Consumption in period 1 follows exogenous income Y_1 and child labor income $(1-e_1)w_1$ as well as any savings s:

$$(2) \qquad c_1 = Y_1 + (1-e_1)w_1 - s - p_1 e_1$$

where p_1 is the direct cost of schooling so that the total cost of schooling is $p_1 + w_1$. Modeling child labor as the residual claimant on child time outside of schooling abstracts from all of the nuances of how children allocate their time (Edmonds 2007), but adding that richness to the model does not enhance our understanding of how the two treatments influence time allocation in this setting. Savings in period 1 yields additional income of $1+r$ in period to 2. Hence, consumption in period 2 is:

$$(3) \qquad c_2 = Y_2 + (1-e_2)w_2 + (1+r)s - p_2 e_2 .$$

Hence, the agent's problem is to pick savings and education consumption in each period.

[13] It is standard in the child labor literature to abstract from parental labor supply decisions (e.g. Basu and Van 1998, Baland and Robinson 2001).

Savings is chosen to equalize the present discounted value of consumption in the two periods. Schooling in each period is chosen so that it's marginal utility is equal to the marginal utility of the resulting foregone consumption. The solution to the agent's problem then implies:

$$(4) \quad \frac{\delta}{1+r}\left(\frac{p_1+w_1}{p_2+w_2}\right)=\frac{\partial v/\partial e_1}{\partial v/\partial e_2}.$$

This equation models the intertemporal substitutability of education.

We model the treatments as occurring in period one. Both treatments change the price of schooling in period one and leave the price of schooling in period 2 unchanged. We assume child labor wages, interest rates, and discount rates do not change with treatment. Totally differentiating equation (4) shows that a decline in the price to school in period 1 induces agents to shift schooling forward to period 1. The decline in schooling in period 2 relative to 1 depends on the shape of preferences, the present discounted value of the future total cost of schooling, and the change in the period 1 price. Absent an income effect of either treatment, the decline in the price of schooling in period one induces a shift in schooling to the period of support.

We hypothesize that the income effect of both the scholarship and stipend treatments should lead to higher schooling in both periods. Both the substitution effects and income effects of the decline in the price of period one schooling lead to more schooling in period one. Period two schooling effects come through savings and are increasing in level of schooling before the price change. Put another way, the more the child attends school without the price decline, the more the treatments induce higher savings and a rise in schooling in period 2. If the scholarship group largely attends school even without the treatment, the scholarship in effect is a cash transfer that should increase savings and lead to more schooling (and consumption) in the second period. Additional schooling in the second period should lead to less child labor.

The scholarship is a smaller price decline than the stipend treatment. Hence, we expect larger schooling changes in the first period and larger savings responses. Of course, the pressure to substitute schooling forward to period one as modeled in equation (4) is also greater with the stipend compared to the scholarship. In the end, the assumption behind EI type policies is that the incentive to shift schooling forward in equation (4) is small compared to the income effect of the price decline. Hence, we proceed with testing this assumption in the next section.

It is worth discussing why the treatment design was as it is given that we model both treatments the same (just different magnitudes). The reason why treatment one was defined to cover school fees is that NGF felt strongly that the direct costs of schooling were a substantive barrier to schooling and that families were more sensitive to direct schooling prices than would be expected given the apparent return to education and the costs of fees. In many ways, their view was consistent with the finding of extremely high price elasticities of demand for preventative health services found in the case of the de-worming (Kremer and Miguel 2004) and bed nets interventions (Cohen and Dupas 2010). One way to incorporate NGF's view within the current model is to assume liquidity constraints. Approximately half of schooling costs are in text books and admission related fees that the agent incurs at the start of the school year. Illiquidity might make it difficult for agents to cope with these expenses.

III. Findings

A. Data

Our evaluation of the impact of the SIPE intervention uses five different data sources. NGF collected schooling records for the purpose of documenting compliance with the attendance requirements for the stipend intervention. We refer to these data as *NGF School Records*. They are attractive in our evaluation, because they contain monthly school attendance records for every child from April 2010 until March 2011. However, the NGF School Records were collected by program administers, and there are incentives for inaccuracy given that they are used to determine program benefits.

We have four sources of data that we use in evaluating the program's impact that were collected by an independent NGO, New ERA Ltd., who was not aware of the details of the intervention or any subject's status in the intervention. The *baseline survey* was conducted before subjects were informed about their status in the project. Both subjects and their adult caregivers were interviewed in the baseline survey. The *midterm survey* was conducted in September, before the October holidays. Only adult caregivers were interviewed in the midterm survey, and the number of questions was reduced compared to the baseline survey. The *yearend survey* interviewed adult caregivers and subjects approximately two months after the final distribution of benefits. The yearend survey instrument was nearly identical to the baseline

instrument. The *post survey* was conducted approximately 17 months after the final distribution of benefits. A streamlined instrument was administered to adult caregivers.

The subject population consisted almost entirely of migrant children. Each survey other than the post survey collected detailed re-contact information including the name, address, and phone numbers of family relatives and friends who were likely to know the subject's future whereabouts. As a result, despite a highly mobile subject pool, attrition seems to be a minimal concern in the data.

Table 4 tabulates how many of the 660 subjects were captured in each of the different sources of data. 650 of the 660 study subjects were interviewed in the post survey conducted 15 months after the end of the project, a 1.5 percent attrition rate.[14] Overall, attrition is uncorrelated with treatment. The only round of data collection where there appears to be any basis for concern is the midterm survey where 13 control children are missing compared to 5 scholarship and 4 stipend subjects. The recapture rate is lowest in the midterm survey, because the data collection contract explicitly excluded the data collection sub from pursuing subjects that moved outside of the Kathmandu Valley. The yearend and post surveys explicitly followed movers, as did the NGF School Records.

B. Empirical Methods

The SIPE study was small in scale because of limited resources. The sample size was chosen to have statistical power with regards to answering one question. Specifically, SIPE was engineered to test the null hypothesis that the stipend intervention provided no impact on school attendance relative to just 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 identical to 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 sample size of 220 per treatment group was chosen to test the null hypothesis separately by gender, by age group, and by carpet establishment size.

[14] 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. 2 children came from a closed factory. Their location was unknown by any of their provided contacts or former classmates. 3 children (all unrelated) are in an unknown location after their mothers left their fathers (all abroad) for another man.

The study size was chosen to be able to detect differences in school attendance between the scholarship and scholarship+ stipend treatments using a simple comparison of means in the yearend data. This comparison of means can be written in regression form as:

(5) $$y_{i,j,k,t} = \beta_0 + \beta_1 S_i + \beta_2 SS_i + \varepsilon_{it}$$

where $y_{i,j,k,t}$ is the outcome for child i in family j associated with establishment k at time t. S_i is an indicator that the child receives scholarship and SS_i is an indicator that the child also receives the stipend. ε_i is a mean zero error term.

When y is schooling attendance, β_0 is mean schooling attendance for children in the control group. $\beta_0 + \beta_1$ is mean schooling for children receiving scholarships. $\beta_0 + \beta_1 + \beta_2$ is mean schooling for children receiving the scholarship and the stipend together. Hence, the interpretation of coefficient β_1 is the impact of the scholarship on outcome y for the child at yearend. The impact of the scholarship and stipend combined on outcome y compared to the control group of children is given by $\beta_1 + \beta_2$. The interpretation of β_2 is the impact of the additional stipend on the outcome compared to just receiving scholarship. Hence, sample size was chosen to be able to test the null hypothesis $\beta_2 = 0$ in the case of school attendance separately by gender, age group, and factory size.

We test hypotheses on subjects other than the impact of the stipend on school attendance. Most other hypotheses are underpowered in the comparison of raw means. Moreover, tabulations from the baseline survey suggest that the data used to inform our study design overstated the lack of schooling in our study subjects. Hence, even the most basic comparison that was the basis for our choice of sample size may be underpowered in reality. To address this, we add additional controls to our regression work. Because the randomization was valid and the baseline data appeared balanced, these controls function to reduce variance and improve statistical power only.

Specifically, we modify (5) as:

(6) $$y_{i,j,k,t} = \beta_0 + \beta_1 S_i + \beta_2 SS_i + \pi_1 ST_i + \pi_2 A_{i,t=0} + \pi_3 \left(A_{i,t=0} * F_i\right) + \alpha y_{i,j,k,t=0} + \varepsilon_{kt}$$

where $y_{i,j,k,t}$ is the outcome for child i in family j associated with establishment k at time $t, t \in \{1,2\}$. $y_{i,j,k,t=0}$ is the value of the outcome variable at baseline. Its inclusion means that we identify the impact of S and SS based on changes in y between the baseline period and either the midyear, yearend, or post period. We do not pool post, yearend, or midyear data in our empirical work. SIPE randomization was conducted after stratifying the population into age group * gender

* establishment size cells, we include a vector of dummies ST_i to denote each of the age*gender*establishment size strata. Even within each stratum, we have the prior that our outcomes are correlated with gender and age. To incorporate that in our specification, we include dummies for age at baseline, $A_{i,t=0}$ and include age-gender interaction ($A_{i,t=0} * F_i$) as a control for all the outcomes that we consider. We cluster errors at the establishment level for each time period.[15]

C. Findings during Period of Support

Table 5 contains results from estimating equation (6) on outcomes covering periods where the program was running. Each row is from a separate regression. Column (1) contains estimates of the impact of the scholarship (compared to the control) on the row variable. Column (2) contains estimates of the additional stipend (compared to the scholarship) on the row variable. Column (3) contains estimates of the impact of the combined scholarship and stipend intervention (treatment 2, compared to control) and the F-statistics for the null hypothesis that treatment 2 does not differ from the control. Column 4 contains the control mean of the row variable, measured at the same time as the treatment variables.

The outcome in row one is an indicator that is 1 if the child has moved at midterm since the baseline. 18 percent of control children have moved during the first 5 months of support, and both treatments reduce the probability a child has moved by 6 percentage points, approximately a third. The treatments do not require the child to stay in the factory, with the same employer, in the carpet sector, or even in Kathmandu. In practice, both seem to have reduced child migration.

There are several different attendance measures in the data. In the Midterm Survey, we have attendance in the month before the survey (row 2). From the NGF School Records, we know whether the child attended school during the first month of the school year, the fifth month of the school year (same time as Midterm Survey), the attendance rate for the entire academic year, and the number of months absent during the supported year. Both the scholarship and stipend treatments improved school attendance during the first month of the year. The NGF and Midterm Survey present a slightly different story about attendance in the fifth month. The Midterm Survey shows an increase in attendance in the stipend treatment, but the NGF School

[15] This empirical approach was stipulated in an analysis plan submitted to ICF Macro before the availability of the midterm data or schooling records (Edmonds and Shrestha 2010).

records show a larger increase for the stipend treatment, an increase for the scholarship treatment, and do not reject the null that scholarship and stipend treatments have the same effect on school attendance. Much of the difference between the Midterm Survey findings and the NGF School record findings at midterm can be explained by the lower attendance rate document in the NGF School Records compared to the Midterm Survey for the control group.

The detailed monthly data from NGF can be used to see how the effect of the treatment varies within the program year. Figure 1 plots the monthly attendance rates of the three treatment groups. These records show higher schooling attendance for both treatment groups during the first few months of the program. The effect of the scholarship, however, starts to falter as we move along the school year. Particularly on and after month 7 (around October), the period of holidays, the effect of scholarship becomes weaker over time. Towards the end of the school year, the attendance in the scholarship group looks very similar to that of the control group. The stipend group tapers off for the holiday, but regains attendance after the festivals are over.

The attendance rate described by Figure 1 is a composite effect on the extensive (attend school) and intensive (number of days) margin. Figure 2 plots these effects separately. The panel to the left shows the effect on whether a child attends school in a particular month. It shows that the phenomenon described above is mostly driven by the effect on attendance. The scholarship group starts out with school attendance rates as high as the stipend group and slowly declines towards the control group by year-end. The gap particularly widens after month 7. To confirm this, the regression table shows that the stipend treatment is associated a 13 percent increase in school attendance and a 71 percent decline in the number of months missed during the school year. We cannot reject the null that the scholarship had no effect on school attendance for the year as a whole compared to the control group.

The right panel of Figure 2 shows attendance rates conditional on going to school at least one day in a month. Here, the story is slightly different. Conditional on going to school, children in scholarship group have attendance rates almost identical to the control group, whereas the children in stipend group have a consistent and high attendance rates. The gap between the stipend group and other groups closes down towards the year-end, possibly because of nearing final exams. This suggests that the conditionality of the minimum attendance requirement pushed students to attend more days in school. Figure 3 shows the histograms of student attendance rates for the three treatment groups for the entire year. This plot shows that the distribution of student

attendance rates lower than 80 percent almost disappears in the stipend group, whereas the distribution seems smoother in the other two groups. This suggests the impact of stipend in making children go to school in any given month, making regular children even more regular (as suggested by the frequency of students in highest attendance group) and also making irregular children go to school more regularly. In total children in stipend group attend 12 more days of school in the school year compared to scholarship children and attend 18 more days compared to the control group. Even when we restrict to children who attended school at least some days, children in stipend group attend 13 more days of school compared to the control and the scholarship group.

We do not observe significant effects of either treatment on wage employment in the week prior to the midterm for either treatment group. Overall, wage employment in the week before the survey is low, 4 percent of children in the control group. The magnitude of the decline associated with the stipend treatment is extremely large (38 percent) but also imprecise. We do not have information on carpet work during the midterm survey, because the data contractor was concerned that such questions would escalate tensions between it and Maoist workers groups and potentially jeopardize its access to subjects.

D. Findings @ Yearend

Table 6 contains our findings regarding data covering the period immediately after support ended. The final stipend distribution occurred in early March 2011, and subjects knew that the program and its requirements ended at that time. Hence, table 6 contains information collected in the NGF School Records after support ended. Table 6's layout is identical to table 5.

Neither treatment has a statistically significant effect on the child's mobility since midterm. 29 percent of control children moved between the midterm survey and the yearend survey (an 8 month period). The scholarship treatment group and the stipend treatment groups are both roughly 5 percent less likely to move, but we cannot reject the null that these children move at the same rate as the control group. This finding is useful in interpreting the migration result from midterm. It implies that the reduced mobility of both treatment groups is not reversed by a higher post program migration rate. The declined mobility of children in both treatment groups is sustained into yearend.

The higher attendance rates of the stipend treatment group documented in Table 5 appear in Table 6 as an increased probability of sitting for the final exam (based on the Yearend survey),

a higher score on the final exam (from the NGF School Records), and a reduced probability of failing the school year (from the NGF School Record). Interestingly, though the effects of the scholarship treatment on attendance did not seem to persist past the October holidays, we cannot reject the null that the increase in yearend scores and passing the grade is the same for the scholarship and stipends. The change in failure rates is incredibly large. 14 percent of control children fail the grade. Both the scholarship and the stipend reduce failure rates by 6 percentage points or 43 percent.

Despite higher attendance rates, test scores, and passing rates, the treatments do not appear to lead to higher enrollment rates in the year after support. In the Yearend Survey, we asked respondents whether they intended to enroll in school in the coming school year. 97 percent of children answered in the affirmative in the control group. We observe a similar response in the stipend treatment group, and a slightly lower affirmative response rate in the scholarship treatment group (significant at 10 percent, small in magnitude). We are not sure of the meaning of this question; we were only able to ask about intent to enroll, not actual enrollment because of the timing of the survey. We will see in the next section that actual enrollment results are similar to the responses to this question in the Yearend Survey (with a lower control mean)

The stipend treatment group is less likely to work for wages in the 7 days before the yearend survey, but as at midterm, the effect is not statistically significant compared to either the scholarship treatment group or the control group. The difference between the stipend group and either the scholarship or control groups is large in magnitude (10 percent), but there is enough variation and a small enough sample that we are underpowered.

Weaving is a worst form of child labor in Nepal according to national law. The impact of the stipend treatment on weaving is large. 7 percent of control children were involved in weaving in the week before the Yearend Survey, and 11 percent weaved in the 12 months prior. The stipend treatment group was 4 percentage points less likely to weave in the week before the Yearend Survey and 6 percentage points less likely to have weaved in the previous 12 months. These more than 50 percent reductions in weaving, concentrated in girls, differ between the stipend and scholarship treatments (at 10 percent significance). We suspect that the effect on weaving in the last 7 days reflects an earlier reduction in weaving coming during the period of

support. Otherwise, it is difficult to reconcile the findings on weaving with the results in the next section.

E. Findings @ Follow-up

Table 7 contains our findings on the impact of the program from the Follow-up Survey, completed 17 months after the conclusion of support and 15 to 16 months after the Yearend Survey. They layout of the table is identical to the previous two. "Supported Year+1" refers to the school year that began in April/May 2011 (support ended March 2011) and ended March 2012. "Supported year +2" refers to the academic year that began April/May 2012. Hence, information on this year, we call it the +2 year, is collected just 4 months into the school year whereas the +1 year data cover the entire year.

Migration between yearend and follow-up is uncorrelated with treatment. Hence, the change in location status induced at midterm by the program endures for two years past the findings at midterm. This is the only evidence of any differential effect associated with treatment, and owes entirely to a reduced probability of migration in the first 5 months of the program.

School attendance, including attendance rate in the survey month of the +2 year, does not differ with treatment status. There does not even appear to be an effect on grade of enrollment. These insignificant differences across treatment groups are not just imprecision. Even the estimated magnitudes of the differences are small compared to Tables 5 and 6.

Weaving is lower in the stipend treatment group but the differences are small in magnitude between groups and not statistically significant. Overall the rate of involvement of weaving is higher at follow-up than at yearend, but the reduced participation in weaving evident immediately after the program's end does not seem to have persisted.

IV. Discussion

Combatting child labor through education promotion is at the center of anti-child labor policy. Financial education support in USG funded programs is required to be transitory by federal law. We evaluate the impact of a USG anti-child labor aimed at children vulnerable to child labor in the hand-made export-oriented carpet sector of Nepal. 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 stipend conditional on school attendance in the previous month.

We have found small, positive effects of a scholarship program that covered out of pocket schooling expenses on schooling at the start of the period of support, effects that include substantive reductions in the probability the child migrates away. These treatment effects on schooling dissipate quickly such that we found no effects on school attendance for the year of support (although scholarship recipients were more likely to pass the school year). We found larger, substantive effects of the combined scholarship and stipend program that covered school expenses and provided a large stipend conditional on school attendance. The impact of the stipend treatment appeared to extend a few weeks past the period of support with reduced child involvement in carpet weaving, a worst form of child labor in Nepal.

However, after 15 months, we can find no evidence of the program in schooling attendance, grade level, or child labor. These findings are inconsistent with the hypothesis underlying USG anti-child labor policy that transitory education interventions are an effective tool to combat child labor in the long run. Our findings suggest an impact on child labor and schooling during the period of support, but it decays quickly after support ends. You get what you pay for.

An interesting question is why. In the present case, subjects knew that the support was transitory. We suspect that is not always the case in anti-child labor programs, but the implementing NGO went to great lengths to make sure that recipients knew the support was transitory. One possible explanation is that households are entirely myopic. We think that is unlikely to be the case in the present population as myopic agents would likely have their children weave rather than attending school. Work in the carpet sector is readily available; we frequently heard complaints from employers that they could not find workers. It is also well compensated compared to other paid jobs available to young people in the Kathmandu Valley. Myopic households should not choose schooling, but most control children attend school.

Liquidity constraints are a possibility. Other studies have found liquidity constraints to be an important influence on child labor and schooling (e.g. Edmonds 2005). However, ordinarily we think of liquidity constraints as limiting an agent's ability to borrow against future income rather than to find a way to save current income. Financial access among migrant workers in the Kathmandu Valley is not perfect, but 10 percent of families report having a

savings account at baseline, that rises to 18 percent at yearend (spread evenly between treatment groups and control), and all respondents report being able to borrow funds if necessary. We do not think that this is a setting where agents are obviously liquidity constrained, but we cannot rule out the possibility of liquidity constraints preventing families from transferring transitory income forward. In fact, the communal living arrangements of most of our subjects probably make it hard to safely save outside of financial institutions, as security is difficult. Consistent with liquidity constraints, if we replicate Table 7 on the population that had savings accounts at baseline, we see migration and school enrollment effects (in support year +1 only) for both the scholarship and stipend treatment groups. However, households with savings accounts are distinct from those without and randomization is not balanced within the savings account sub-sample. Hence, we do not feel that we can push this data further.

The model of section III also suggests a more pragmatic reason for why there may be no forward smoothing of transitory support. Perhaps income effects are small relative to the importance of changes in the price of schooling. While we do not have a formal test of this hypothesis, structural models that attempt to decompose the causes of changes in time allocation with conditional cash transfers often emphasize the importance of the conditionality over the import of the income support. In our model of section III, this finding would imply potentially a dominant role for equation (4) in leading to an intertemporal shift in education. We do not observe lower education in the treatment groups post treatment, but perhaps the non-effects could reflect the income effects being offset by the intertemporal substitution of education toward the period of price reduction.

These findings that the impact of EI type support is limited to the period of support has important implications for child labor policy. While providing support may be beneficial to recipients, it cannot be presumed that such education support will have a lasting effect even on those who receive the support, let alone child labor in general. Moreover, these findings that the main effect of EI support is coming through the relative price decline for schooling mirrors the findings of work on conditional cash transfers that emphasize the importance of the relative price changes for the findings that conditional cash transfers improve schooling (Todd and Wolpin 2006; de Brauw and Hoddinott 2011; Baird et al 2012). While we cannot evaluate whether similar changes in time allocation during the period of support would have come through unconditional transfers, our findings raise the question of whether an inability or unwillingness

to smooth income over time could play a role in why the relative price effects appear so important in conditional cash transfer evaluations.

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Table 1: Timing of the SIPE Project and Evaluation

Date	Project Task	Evaluation Task
Early February 2010	NGF identifies subjects	NGF eligibility survey collects background information on subjects and associated carpet establishments
Late Feb / Early March 2010		NE collects baseline survey data
Late March 2010	NGF assigns subjects to treatment groups and notifies subjects of their status	
Late April 2010	School year starts	
May 2010	NGF begins collecting monthly school attendance data	
September 2010		NE midyear survey
February 2011	NGF ends collecting monthly school attendance data	
March 2011	NGF makes final disbursements. Subjects take final exams	
Late April / May 2011		NE yearend survey
August 2012		NE post survey

NGF=Nepal Goodweave Foundation. NE=New ERA Ltd.

Table 2: Determinants of eligibility for SIPE, 660 study subjects

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, Drop out	52
Total	660

Source: ICF International (2012).

Table 3: Child, Family and Establishment Characteristics by Treatment Status

	Control (1)	Scholarship (2)	Scholarship + Stipend (3)	P-Value for Sch. = Sch. + Stipend (4)
Age	12.27	12.37	12.31	0.76
Female	53.64	54.55	54.55	1.00
Completed Education	4.25	4.12	4.18	0.80
Lives in Establishment	71.36	70.45	69.09	0.76
Number of Children	2.36	2.28	2.35	0.44
Match Child Selection Criteria	40.91	34.55	40.45	0.20
Receives Free Housing	63.64	63.64	60.45	0.49
Family Income	4.67	4.93	4.72	0.40
Match Income Selection Criteria	96.82	94.55	95	0.83
Number of Children School Age	2.26	2.19	2.23	0.64
Has Drop Out	7.27	7.73	8.64	0.73
Matches Sibling Schooling Selection Criteria	19.09	21.36	20.91	0.91
Establishment Characteristics				
Number of Workers	41.18	41.7	41.14	0.83
Weaving Output in Sq. Meters in Last 30 Days	92.71	95.29	94.24	0.86
Estab. has Above Median Output	46.36	47.27	47.27	1.00
Pre-weaving activities in establishment	7.27	9.09	7.27	0.49
Post-weaving activities in establishment	22.27	23.64	20.91	0.49
Day Care Available to Estab. Workers	11.36	13.64	13.64	1.00
Scholarship Available to Some in Estab.	11.82	14.55	12.27	0.49
Health Checks available to Estab. Workers	2.73	4.09	3.64	0.81
F-Stat on Joint Significance of Covariates above	0.41	0.55	0.18	0.34
P-Value associated with F-Stat	0.99	0.95	1.00	1.00

Source: Authors' calculations using the NGF Eligibility Survey

Table 4: Attrition by Data Source and Project Status

Data Source:	Control Group	Scholarship Treatment	Stipend Treatment
NE Baseline, 3/10	220	220	220
NE Midterm, 9/10	207	215	216
NE Yearend, 5/11	219	218	218
NE Post, 8/12	217	217	216
NGF School Records, 9/10	220	218	217
NGF School Records, 3/11	220	218	217

Source: Authors' calculations from each survey.

Table 5: Impact of Scholarship and Scholarship+Stipend Interventions on Child Outcomes during Period of Support

	Scholarship b/(se) (1)		Stipend b/(se) (2)		Scholarship + Stipend b/(F) (3)		Control Mean (4)
Child has migrated by midterm since baseline	-0.062 (0.034)	*	0.005 (0.034)		-0.057 (3.013)	*	0.177
Child attends school at midterm	-0.010 (0.015)		0.034 (0.015)	**	0.025 (3.578)	*	0.971
Child attended school in month 1 of the school year	0.060 (0.024)	**	0.021 (0.017)		0.081 (10.482)	**	0.895
Child attended school in month 5 of the school year	0.048 (0.027)	*	0.025 (0.020)		0.072 (11.847)	**	0.895
Child works for wages in the last 7 days at midterm	0.018 (0.018)		-0.033 (0.020)	*	-0.015 (0.953)		0.040
Attendance rate during supported academic year	0.033 (0.026)		0.069 (0.019)	**	0.102 (23.519)	**	0.811
Months absent from school during supported year	-0.500 (0.320)		-0.490 (0.229)	**	-0.990 (14.084)	**	1.423

* significant at 10%. ** Significant at 5%. Each row is from a different regression with the row variable as the dependent variable. All regressions include age*gender*establishment size fixed effects and controls for baseline time allocation. Standard errors are clustered by establishment. Column (1) includes the coefficient standard error on the indicator that the child receives a scholarship. Column (2) includes the coefficient and standard error on an indicator that the child receives a stipend. It tests the null hypothesis that the additional stipend has no effect beyond the scholarship. All children in treatment group 2 receive both scholarships and stipends. Column (3) contains the sum of columns 1 and 2 and the F-Statistic associated with the null hypothesis that the combine scholarship and stipend treatment (group 2) does not differ from the control group. Column (4) is the mean of the row variable for the control group.

Table 6: Impact of Scholarship and Scholarship+Stipend Interventions on Child Outcomes at Yearend

	Scholarship b/(se) (1)		Stipend b/(se) (2)		Scholarship + Stipend b/(F) (3)		Control Mean (4)
Child migrate since midterm	-0.053 (0.041)		-0.005 (0.040)		-0.057 (2.053)		0.286
Child sat for final at end of supported year	0.007 (0.027)		0.049 (0.025)	**	0.056 (7.164)	**	0.900
Score on yearend exams	2.787 (2.082)		1.661 (2.063)		4.448 (4.534)	**	49.701
Child failed supported school year	-0.064 (0.035)	*	0.002 (0.026)		-0.062 (4.713)	**	0.135
Child attended school in month 12 of the school year	0.013 (0.035)		0.074 (0.026)	**	0.087 (11.116)	**	0.873
Child will enroll in next school year (supported year + 1)	-0.032 (0.019)	*	0.032 (0.021)		0.000 (0.000)		0.973
Child works for wages in the last 7 days	0.025 (0.029)		-0.041 (0.026)		-0.016 (0.409)		0.110
Child worked in wage employment in the last 12 months	0.004 (0.036)		-0.034 (0.039)		-0.031 (0.763)		0.237
Child weaved in the last 7 days	-0.011 (0.025)		-0.032 (0.018)	*	-0.043 (4.656)	**	0.073
Child weaved in the last 12 months	-0.007 (0.028)		-0.048 (0.028)	*	-0.055 (4.285)	**	0.114

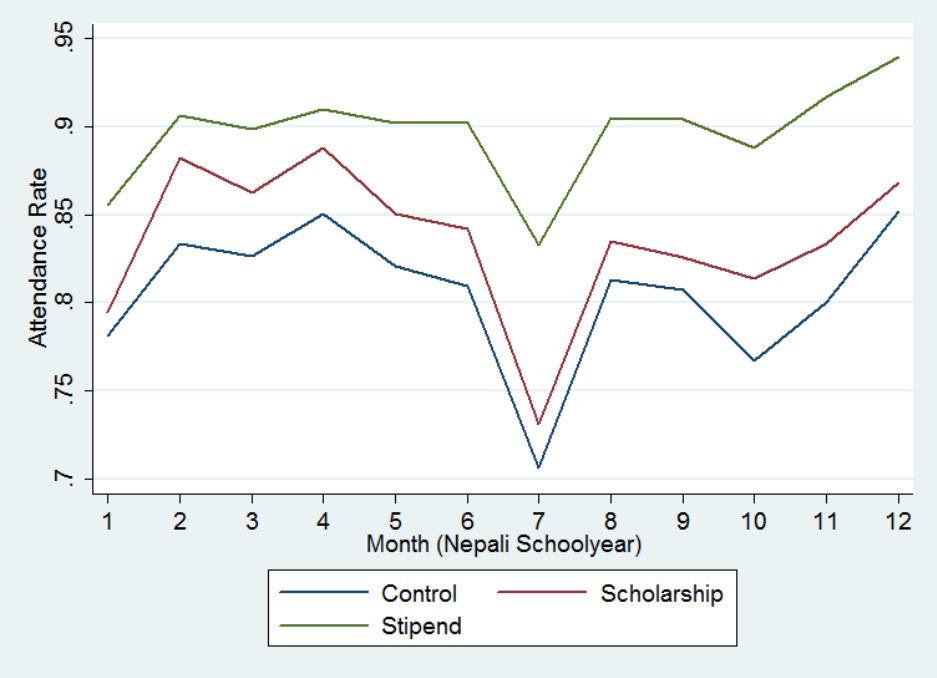
* significant at 10%. ** Significant at 5%. Each row is from a different regression with the row variable as the dependent variable. All regressions include age*gender*establishment size fixed effects and controls for baseline time allocation. Standard errors are clustered by establishment. Column (1) includes the coefficient standard error on the indicator that the child receives a scholarship. Column (2) includes the coefficient and standard error on an indicator that the child receives a stipend. It tests the null hypothesis that the additional stipend has no effect beyond the scholarship. All children in treatment group 2 receive both scholarships and stipends. Column (3) contains the sum of columns 1 and 2 and the F-Statistic associated with the null hypothesis that the combine scholarship and stipend treatment (group 2) does not differ from the control group. Column (4) is the mean of the row variable for the control group.

Table 7: Impact of Scholarship and Scholarship+Stipend Interventions on Child Outcomes at Follow-up

	Scholarship b/(se) (1)	Stipend b/(se) (2)	Scholarship + Stipend b/(F) (3)	Control Mean (4)
Child migrate since yearend	0.017	-0.012	0.005	0.173
	(0.037)	(0.036)	(0.024)	
Child attended school in supported year + 1	-0.002	0.016	0.013	0.931
	(0.023)	(0.022)	(0.488)	
Child passed school in supported year + 1	-0.003	-0.019	-0.023	0.915
	(0.027)	(0.028)	(0.746)	
Child attended school in supported year +2	0.048	-0.034	0.013	0.816
	(0.034)	(0.034)	(0.176)	
Attendance rate during current month (supported year +2)	0.050	-0.039	0.012	0.779
	(0.037)	(0.038)	(0.116)	
Grade of enrollment, supported year+1	-0.174	0.187	0.013	6.871
	(0.117)	(0.115)	(0.012)	
Grade of enrollment, supported year+2	-0.169	0.188	0.019	7.542
	(0.120)	(0.120)	(0.023)	
Child work for pay in the last 30 days	-0.004	0.003	-0.001	0.240
	(0.042)	(0.036)	(0.000)	
Child work for pay in the last 12 months	-0.034	0.007	-0.028	0.318
	(0.043)	(0.038)	(0.408)	
Child weaved in the last 30 days	-0.011	-0.006	-0.016	0.092
	(0.024)	(0.025)	(0.386)	
Child weaved in the last 12 months	-0.009	-0.017	-0.026	0.129
	(0.026)	(0.027)	(0.863)	

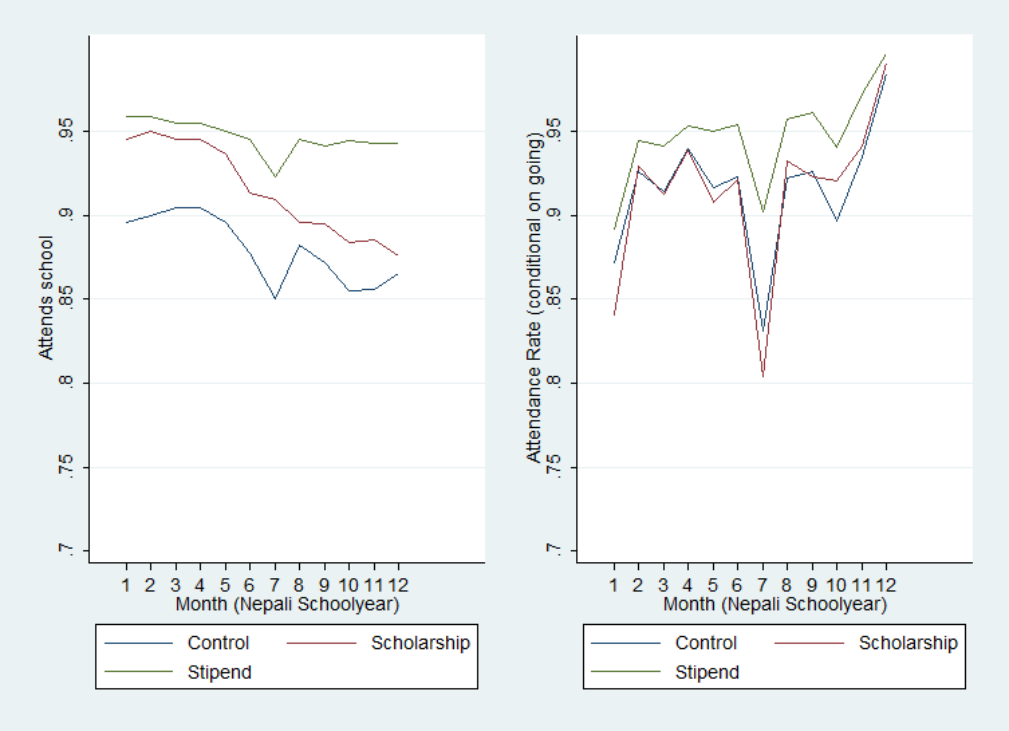
* significant at 10%. ** Significant at 5%. Each row is from a different regression with the row variable as the dependent variable. All regressions include age*gender*establishment size fixed effects and controls for baseline time allocation. Standard errors are clustered by establishment. Column (1) includes the coefficient standard error on the indicator that the child receives a scholarship. Column (2) includes the coefficient and standard error on an indicator that the child receives a stipend. It tests the null hypothesis that the additional stipend has no effect beyond the scholarship. All children in treatment group 2 receive both scholarships and stipends. Column (3) contains the sum of columns 1 and 2 and the F-Statistic associated with the null hypothesis that the combine scholarship and stipend treatment (group 2) does not differ from the control group. Column (4) is the mean of the row variable for the control group.

Figure 1: Attendance Rates by treatment group



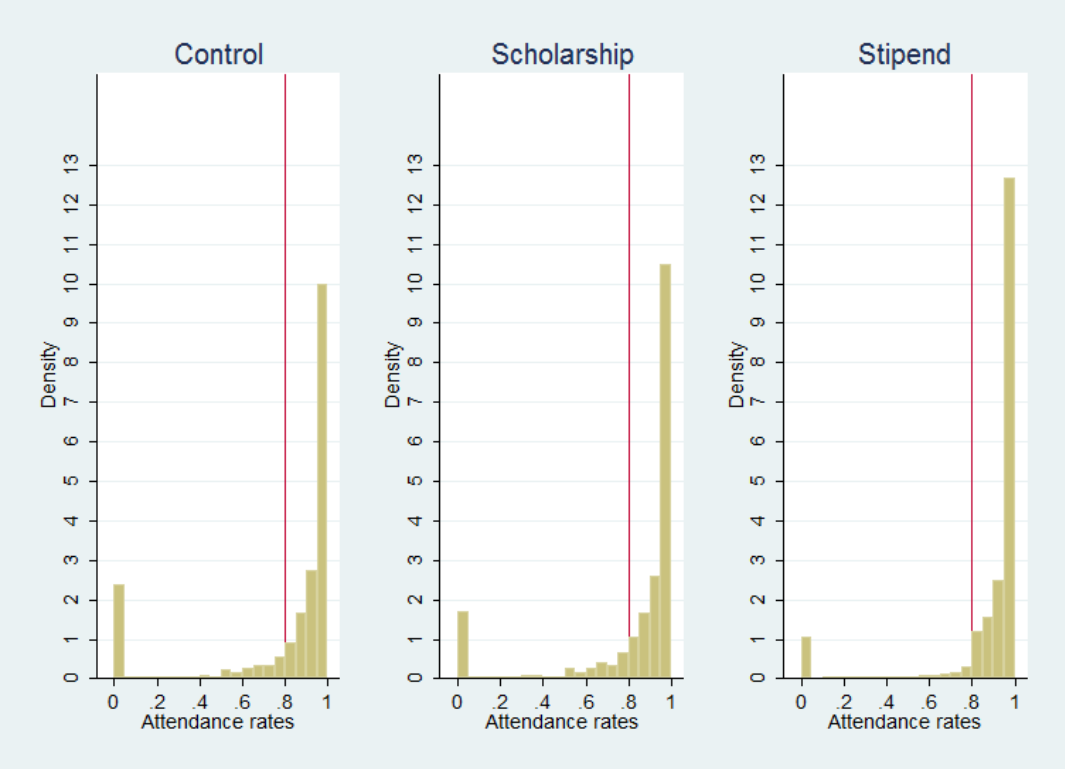
Source: Authors' calculations from the NGF School records

Figure 2: Presence in school and conditional attendance by treatment group



Source: Authors' calculations from the NGF School records

Figure 3: School attendance rates by treatment group



Source: Authors' calculations from the NGF School records