Child Schooling and Child Work in the Presence of a Partial Education Subsidy

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Abstract: Could a partial subsidy for child education increase child labor? Using data from the randomized evaluation of a conditional cash transfer program (CCT) in the Philippines, we find that children who were neither in school nor work in the absence of the program not only increased school participation but also increased work for pay. We show suggestive evidence that, because the cash transfer only provided a partial schooling subsidy, children worked to cover the shortfall in schooling fees. Our findings contribute to the increasing evidence that the design of CCTs, in this case transfer size, matters considerably in terms of achieving program goals.

Keywords: Cash transfers, child labor, education, education subsidy, Philippines

JEL codes: C93, I21, J22, O22

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I. Introduction

An extensive literature consistently finds that conditional cash transfer (CCT) programs increase children's school participation (Baird et al., 2014; Fiszbein and Schady, 2009; Saavedra and Garcia, 2012) usually while decreasing their participation in work (de Hoop and Rosati, 2014; Edmonds 2008; Edmonds and Schady, 2012; Fiszbein and Schady, 2009). In this paper, we present a counterexample from the experimental pilot of a conditional cash transfer program in the Philippines showing that cash transfers can, under certain conditions, increase both school enrollment *and* participation in paid work.

The program, *Pantawid Pamilya Pilipino Program* or simply *Pantawid*, supports poor households (those with income less than US $2.15 per capita per day) by providing two transfers, one conditioned on child health-related behavior and the other on schooling.[i] The randomized evaluation of *Pantawid* (World Bank, 2013) demonstrated that the program broadly achieved its primary objectives by increasing school participation of eligible children (those aged 6 to 14 from pre-identified poor households) by almost five percentage points and improving the health and nutrition of eligible 0-14 year olds (World Bank, 2013; Kandpal et al., 2016).[ii] Using data from the *Pantawid* impact evaluation, we find that the increase in school participation was accompanied by a concomitant increase in children's participation in paid work outside the home. This increase is on the order of five percentage points, relative to a control mean of 12 percentage points in the rate of child work-for-pay. In particular, the program appears to have encouraged children who would otherwise be neither in school nor in work to attend school *and* to start working.

We consider and rule out a range of possible explanations for the increase in child labor, including investment of the transfers in household productive activities and changes in adult productive engagement, both of which can increase household demand for child labor, as well as improvements in child health, which could affect the supply of child labor. Instead,

we present evidence suggesting that schooling and work-for-pay were complements in the face of *Pantawid's* partial schooling subsidy. During the evaluation period, education transfers did not fully cover the cost of education and hence the school attendance of compliers, i.e. those who started attending school in response to the program, represented a net cost to the household. The maximum annual education transfer per child was approximately US$70 although the households in our sample received only about US$55. Estimated primary schooling cost was US$86 in treated areas, indicating an average shortfall of US$31 per enrolled child; the shortfall for compliers may have been even greater. We show that the earnings of working children make up for a large portion of this shortfall.[iii]

While we do not estimate the total welfare impact of the increase in children's school attendance and work, which would require knowledge of the full long-run benefits and costs of both school attainment and child work, this paper contributes to our understanding of the relationship between schooling and work-for-pay and argues for the adoption of a broader framework when assessing the cost effectiveness of possible transfer schemes. When discussing program design, the literature typically compares the size of the transfer to household income. However, our findings suggest that the cost of the behavior on which the program is conditioned (in our case school participation) is also a germane metric. A transfer too large may be wasteful if full compliance can be achieved with a smaller transfer amount (or if most transfers are infra-marginal). A transfer too small may not sufficiently compensate potential compliers to modify behavior, even if the presence of positive externalities is an acknowledged motivation for the subsidy. Alternatively, a transfer that does not fully compensate for the cost of adopting the compliant behavior can result in unanticipated consequences as beneficiary households seek to supplement the partial subsidy through a labor response or an asset drawdown.[iv, v] While such compensatory behavior need not arise in all contexts, such as in wealthier populations, the identification of such behavior is relevant

because cash transfer programs are widely implemented, including in settings with markedly lower primary school attendance rates and higher rates of idle children. The phenomenon we document could equally occur in programs encouraging secondary school participation – an issue of increasing policy concern – or providing partial subsidies subject to other behavioral requirements. We thus interpret our findings as an example of an issue of broader concern. Our results also raise questions about the efficiency of spending in such programs as most CCTs with primary school conditions are targeting populations already at very high enrollment levels.

Finally, by documenting compensatory behavior, this paper also contributes to our understanding of the often-significant unintended consequences of CCTs, both beneficial and detrimental. While a comprehensive discussion of this literature is beyond the scope of this paper, a few germane examples include Contreras and Maitra (2013)'s finding that the Colombian CCT significantly improved health outcomes among non-targeted adults in treated households. Ferreira, et al. (2009) and Barrera-Osorio et al. (2011), examining a Cambodian scholarship program and the Colombian CCT respectively, show that child-specific cash transfers may generate negative displacement effects on the schooling of ineligible siblings. Finally, several studies have also found that peer effects can increase school enrollment of non-targeted populations, at least in the case of Mexico's *PROGRESA/Oportunidades/Prospera* CCT (Bobba and Gignoux, 2014; Bobonis and Finan, 2009; Lalive and Cattaneo, 2009).

This paper proceeds as follows. In the next section, we introduce a framework to examine household responses to a partial schooling subsidy. Section III describes the context and program. Section IV discusses the data and our empirical strategy. Section V presents results on the impact of *Pantawid* on children's schooling and work, as well as various alternative channels, and compares the effects of *Pantawid* to those of programs that fully

offset schooling costs, including *Prospera*, to highlight the role of the subsidy size. Section VI offers concluding thoughts.

II. Schooling and Child Work Decisions in the Presence of a Subsidy

The literature exploring household child labor decisions generally treats education and child labor as substitutes. For instance, Basu and Van (1998) assume that children work only to support household subsistence (the so-called luxury axiom), and Baland and Robinson (2000) posit a trade-off between child labor and human capital accumulation. Most of the empirical evidence on schooling and child labor supports this view (Beegle et al., 2006; Bourguignon, Ferreira, and Leite, 2003; Edmonds and Schady, 2012; Ferreira, Filmer and Schady, 2009; Manacorda, 2006; Ravallion and Wodon 2000; Schady et al. 2008). However, as we show, since the time allocated to school and work-for-pay can be adjusted on both the extensive and intensive margins, complementarities can arise when households are offered an education subsidy that only partially offsets education expenditures. In that case, we may observe compensatory behaviors as poor (and adult labor constrained) households need to supplement the partial subsidy if they wish to enroll their children. A brief conceptual framework describes how such compensatory behavior can arise.

Most models of the child labor decision explore the tradeoff between current household income and the future income of the child, as determined by lumpy investments in schooling. Several studies present theoretical explanations for why households may under-invest in children's education and examine how a CCT may affect this investment decision (for instance, Das, Do and Ozler (2005) and Fiszbein and Schady (2009) provide comprehensive overviews of the theoretical underpinnings of CCT design). The central question of this paper is somewhat different in that it concerns the household's response to an offered schooling subsidy after an initial decision on child labor allocation has already been made. Possible responses include an asset drawdown, an increase in adult labor supply, a

shift in consumption patterns, or an increase in child labor. In so far as the only scenario observed is an increase in child labor, we explore a conceptual framework – described in detail in Appendix 1 – that focuses on this scenario and identifies how and for whom this increase might arise.[vi]

We adapt a simple two-period overlapping generation model of a unitary household to highlight how the presence of fixed schooling costs and non-convexity in the time and budget constraints, generated by a minimum amount of time that must be devoted to school attendance, can lead to a complementarity between education and child labor in the presence of a partial schooling subsidy. Salient determining factors are the relative size of the subsidy vis-à-vis the cost of schooling, and the strategies available to the household to take-up the subsidy and enroll the child in school.

It is important to note that this type of behavioral response need not arise in wealthier populations where fewer households require subsidies to enable children's school attendance and the households that do have a more diversified set of strategies to take up a partial subsidy. Therefore, this framework does not attempt to present a global model for all child schooling and work decisions, but rather to understand the observed shifts in child labor after the onset of *Pantawid*.

III. Background and Study Context

A. Education and Child Labor in the Philippines

Recent (2011) ILO survey data show that 95 percent of 10-to-14-year old Philippine children are in school and that 13 percent of children in the same age range are engaged in economic activities (Understanding Children's Work, 2015). About 85 percent are in school only, 11 percent combine school and work, three percent are idle (i.e. in neither in school nor in work), and two percent are in work only. Boys are more likely to work than girls (15

percent versus 10 percent) and somewhat less likely to be in school (93 percent versus 97 percent).[vii] Children in this age range are not legally allowed to engage in economic activities in the Philippines, although the enforcement of such laws has been under-resourced, at least until the establishment, in 2015, of an interagency council to enforce child labor laws (US Department of Labor, 2016).

B. The Program

Pantawid aims to support poor households in satisfying their consumption needs and to encourage investment in their children's education and health. The program began in 2008 with the first enumeration of potential beneficiary households through a listing exercise that collected several socio-demographic and household asset indicators to construct a Proxy Means Test (PMT) score. Households were eligible for the CCT if their baseline PMT score fell below the poverty threshold of approximately US$2.15 per capita per day (in 2011 dollars) and the household included a pregnant woman and/or at least one child under the age of 14. The first beneficiary households enrolled and began receiving benefits in the same year. The program has since been expanded and now covers about 4.5 million households.

Pantawid provides both education and health grants. The monthly education grant of 300 Philippine Pesos (roughly US$7)[viii] is offered to children aged 6 to 14 who attend primary or secondary school regularly (at least 85 percent of school days each month). The education grant is provided for up to 3 children per household and for 10 months a year.[ix] The lump sum monthly health grants of 500 Philippine Peso (roughly US$11.50) are provided to beneficiary households on the condition that pregnant women and children up to the age of 5 regularly attend health clinics, children aged 6 to 14 receive deworming treatment, and the household member receiving the cash transfers (or their spouse) attend "Family Development Sessions" organized by the implementing agency, the Department of Social Welfare and Development.[x] In our study sample, the average household has 2.6 children, which translates

to a maximum monthly transfer of US$30, representing about 20 percent of the average beneficiary's monthly household income (see World Bank, 2013).

Both the theoretical framework and our interpretation of the empirical results rely on the beneficiary's expectation of enforcement of the schooling condition, and not necessarily on the actual enforcement of the condition. While we do not have data on the enforcement of conditions, the program was designed and publicized as conditional. Administrative data show that the average monthly amounts transferred to our sample (US$18.50) were significantly smaller than the US$30 maximum for which households were eligible, which may be indicative of conditions being at least partially enforced. Even if program conditions were not consistently enforced during the first years of the pilot stage program, beneficiaries could not have known with certainty whether conditions would be enforced. Hence, non-compliance would have entailed the risk of loss of benefits in the minds of the study subjects. Finally, as Benhassine et al. (2015) show in Morocco, even a "nudge" or an unenforced condition can be enough to induce beneficiaries to comply.

C. The Evaluation Design

A village-randomized evaluation was jointly designed by the World Bank and the Philippine Department of Social Welfare and Development. In October 2008, 130 villages were randomly allocated to treatment and control arms of 65 villages each stratified by 8 municipalities. The number of villages was chosen based on power calculations for three primary outcomes: school attendance of children aged 6 to 14, household consumption, and health facility visits. Data for the PMT were collected in all 130 villages from October 2008 to January 2009, and in April 2009 eligible households in the treatment villages began receiving transfers.

Since our conceptual framework suggests that the poorest households are the most likely to exhibit increases in child labor *and* education in response to a partial schooling subsidy, it is useful to note that these experimental villages represent the poorest villages in the poorest municipalities in the country: the PMT eligibility threshold of US$2.15 per capita per day is barely above the World Bank's US$1.90 a day poverty line. Indeed, eligible households in the 130 evaluation villages had an average per capita income of approximately $1.50 per capita per day, eleven percent lower than the average per capita income in the other program areas from this period.

IV. Data and Methods

A. Data

We rely on four sources of data collected as part of the *Pantawid* evaluation. Our primary data source is a follow-up household survey conducted in October and November of 2011 – two-and-a-half years after the start of the intervention and during the middle of the 2011/2012 school year. In each of the 130 villages in the evaluation, survey data were collected from a random sample of both eligible and ineligible households in treatment and control communities.[xi]

This survey covered a range of topics including school attendance by children aged 6 to 17, and work by children aged 10 to 17. Questions on children's school participation were addressed to the child's mother, guardian, or main caregiver, while the questions on work were addressed to the child herself. Children were asked not only about current work but also, albeit in less detail, about work prior to the start of the program for the calendar years 2007, 2008, and 2009. We use these recall data to explore baseline balance in child work as the baseline does not contain this information. Appendix 2 explains how we construct our outcome variables based on this data.

The second source of data is the baseline assessment of household-level demographic and socio-economic measures used to construct the PMT score. We use these data to assess balance of key baseline characteristics across treatment and control communities at baseline. Our third data source is a survey administered to village leaders concurrently with the household survey, which includes an assessment of the average daily wage of a male laborer in the village as well as measures of community access to services, such as the distance from the village hall to the nearest public primary and secondary schools. Finally, we use administrative data on the monthly amounts transferred to beneficiary households over the evaluation period.

B. Estimation Strategy

We exploit the cluster-randomized treatment assignment to identify the impact of the cash transfer program on both children's education and work. In our preferred specification, presented below, we estimate the intent-to-treat (ITT) effect of the program by regressing the outcome of interest on the indicator variable for treatment while controlling for municipality, which is the stratification variable, and age dummies where appropriate:

$$(1) \qquad Y_{iv} = \beta_0 + \beta_1 * T_v + \boldsymbol{\beta_2}' \boldsymbol{X_b} + \boldsymbol{\beta_3}' \boldsymbol{X_{ivb}} + \varepsilon_{iv}.$$

Here Y_{iv} is the outcome of interest (e.g. school or work) for child i in village v at follow-up, T_v is the indicator variable taking the value 1 for treatment villages, $\boldsymbol{X_b}$ is a vector of stratification variables measured at baseline, denoted b, $\boldsymbol{X_{ivb}}$ is a vector of age dummies in regressions at the child level, and ε_{iv} is the error term. The coefficient β_1 estimates the intent-to-treat effect of the program using OLS. In a series of appendix tables below, we examine whether the precision of our estimates improves when we include control variables and whether results are robust to using the following alternative models: Probit, Logit, and panel

regressions with individual fixed effects (treating the 2007, 2008, and 2009 recall data as baseline measurements). All standard errors are clustered at the village-level.

B. Sample Definition

We focus on children aged 10 to 14 as they are the youngest children eligible for the education grant for whom both schooling and work data are available. We further restrict our sample to children from households that are below the poverty threshold and therefore eligible to participate in *Pantawid*, which yields a final sample of 1264 children: 637 from 411 households in treatment villages and 627 from 422 households in control villages.[xii]

Appendix 3 examines the validity of the village-level randomized assignment of *Pantawid* across all available baseline individual, household, and community characteristics. We test for balance by regressing the vector of these characteristics on the treatment indicator, clustering standard errors at the village-level. There are no statistically significant mean differences between the treatment and control groups across the covariates considered.

Administrative data show that 605 of 637 (95 percent) of the children from treatment villages are from households that actually participated in the CCT program. In contrast, none of the children from control villages belong to households who participated. Given the high rate of compliance with treatment assignment, the ITT effects reported are not substantively different from estimates of treatment on the treated (discussed in further detail below).

V. Results

A. Descriptive Statistics

Table 1 presents mean values in the control group for the outcome variables considered. School attendance rates among children aged 10 to 14 are high – almost 90 percent attend school, and 80 do so regularly – but lower than the national average because the evaluation study sample was drawn from the poorest areas of the Philippines. Most

children in the 10 to 14 age range are in primary school, although about 20 percent are already in secondary school. A substantial proportion, about 20 percent, worked in the 12 months before the interview and about 16 percent in the 7 days prior to the interview. Conditional on any work, children work about 30 days a year and about 12 hours a week. Children are as likely to report working for pay outside the household as working without pay inside the household. Most of the work carried out by children is unskilled, and most children who work (about 4 in 5) are also in school. A sizeable group of children (about 7 percent) neither worked nor attended school in the 12 months prior to the interview. As we show below, the cash transfer program had a particularly strong effect on the schooling and labor supply of this last group of children.

B. Impact of Pantawid on Education

A key goal of *Pantawid* is to improve children's school participation. Table 2 presents estimates of the effect of the cash transfer program on the school participation of children aged 10 to 14. Overall attendance increased by 4 percentage points relative to a control mean of 89 percent (column (1)). Regular attendance, defined as attendance of at least 85 percent of school days in the two weeks prior to the interview, increased by 9 percentage points (over a control mean of 80 percent, column (4)). This increase occurred especially in primary school (Columns (2) & (3) and Columns (5) & (6)). The reported number of days children attended school in the two weeks prior to the interview increased by approximately a full day, from 7.5 to 8.5 (column (7)). The increase in the number of days children attend school reflects changes in both the probability of school attendance and the number of days attended in the 2 weeks prior to the interview conditional on having attended school at least one day (presented at the bottom of the table).[xiii] *Pantawid* thus appears to have significantly increased regular primary school attendance.

C. *Impact of Pantawid on Child Labor*

While *Pantawid* did not explicitly target child work in its choices of conditions or messaging, the program may have had an impact on child work through a variety of channels, as we discuss in more detail below. Table 3 explores such impacts.[xiv] The probability of 10-to-14-year old children engaging in work in the 12 months before the interview increased by 4 percentage points (column (2)). While not precisely estimated, the point estimate indicates a 20 percent increase over the control mean.[xv] Columns (3) to (5) of Table 3 show that the increase in work is due solely to an increase in work for pay outside the household – a 5 percentage point increase over the control mean of 12 percent, significant at the 5 percent level. Work without pay, inside or outside the household, and work for pay inside the household are not significantly affected. Accordingly, as shown in columns (6) to (8), children increase their participation in laboring and unskilled work, while participation in other work, such as farming and fishing, is not significantly affected. Effects on the number of days worked, including for pay, in the past year are positive but not statistically significant. However, as shown at the bottom of the table, conditional on working, days worked are slightly higher in the treatment group than in the control group. Although we cannot identify working hours separately for children who started working because of the program and those who would work even in the absence of the program, the latter suggests that working hours are similar in both groups.

In Table 4, we examine how *Pantawid* affected the four mutually exclusive combinations of school only, work only, school and work, and neither school nor work (Columns (1) to (4) respectively), and whether children worked while school was in session (Column (5)). We find that *Pantawid* causes a 4-percentage point decrease in the probability of children being neither in school nor work and a 6-percentage point increase in the probability of children both working and attending school. The probability of children

working while school was in session increased by 5 percentage points. These results suggest the most prevalent behavioral shift caused by the program was a transition from being in neither school nor work to being in both school and in work.

We separately estimate the effects on boys and girls by interacting the treatment variable with gender dummies (results available in Appendix Table 2). F-tests do not allow us to reject the null hypothesis that the program impact is similar for boys and girls, with both sexes increasing regular school attendance (8 and 9 percentage points respectively, Column (2)), and the likelihood of being engaged in work for pay outside the household by about 5 percentage points (Column (4)).

D. Robustness of the Reported Impacts of Pantawid on Child Schooling and Labor

We then examine whether the inclusion of control variables increases the power of our hypothesis tests and the extent to which the results presented above are sensitive to the use of alternative estimation procedures, sample trimming, and alternative reference periods. Our specification with control variables augments the original regression specification as follows:

$$(2) \qquad Y_{iv} = \beta_0 + \beta_1 * T_v + \boldsymbol{\beta_2}' \boldsymbol{X_{ivb}} + \boldsymbol{\beta_3}' T_v * (\boldsymbol{X_{ivb}} - \mu(\boldsymbol{X_{vb}})) + \boldsymbol{\beta_4}' \boldsymbol{X_{ivb\ Missing}} + \varepsilon_{iv}.$$

Here $\boldsymbol{X_{ivb}}$ is a vector of individual, household, and village-level control and stratification variables (municipalities) measured at baseline (denoted *b* in the subscript). These controls, described further in Appendix 3, include the interaction of the treatment variable T_v with the vector $(\boldsymbol{X_{ivb}} - \mu(\boldsymbol{X_{vb}}))$ to address concerns of regression adjustment laid out in Freedman (2008a & 2008b) and discussed in Lin (2013). The vector $\mu(\boldsymbol{X_{vb}})$ contains the averages of the control variables across both treatment and control groups. When a control variable is missing for individual *i*, we set both the relevant element of the vector $\boldsymbol{X_{ivb}}$ and the element of

the vector T_v *$(\boldsymbol{X_{ivb}} - \mu(\boldsymbol{X_{vb}}))$ equal to -1. We let the elements of the vector $\boldsymbol{X_{ivb\ Missing}}$ take the value 1 if the relevant control variable is missing and 0 otherwise.

Robustness tests also reconsider the following choices made above: (1) the use of OLS instead of binary models like Probit or Logit, (2) the inclusion of 43 children who were neither children nor grandchildren of the household head, and may lead to concerns around endogenous changes in household composition, (3) reporting village-level intent-to-treat estimates instead of treatment-on-treated effects, and for work outcomes, (4) using a 12-month recall instead of 7-day recall as well as (5) using cross-sectional data on current work rather than fixed effects regressions using recall data on work in previous years.[xvi] We specify the fixed effect regressions as follows:

$$(3) \qquad Y_{ivt} = \beta_0 + \beta_1 * T_{vt} + d_i + d_{2008} + d_{2009} + d_{2011} + \varepsilon_{ivt}.$$

Here, Y_{ivt} is the outcome variable for individual i from village v at time t (i.e. 2007, 2008, 2009, or 2011), T_{vt} is the treatment variable (1 for treatment villages in 2011, 0 otherwise), d_i is an individual fixed effect, and d_{2008}, d_{2009}, and d_{2011} are time fixed effects.

Appendix Table 3 shows the effects on school enrollment and attendance, while Appendix Tables 4 and 5 present the effects on child work and the transition from idleness to joint schooling and work respectively. As the first row of each of these tables shows, point estimates do not change in magnitude or sign but are more likely to be statistically significant when we include covariates. Impact on any work in the 12 months before the interview, for instance, is statistically significant when we include controls. These tables further illustrate the robustness of our results to the use of binary response models instead of OLS, as well as child-level fixed effects using the 2007, 2008, and 2009 recall data as our baseline measurement. While some standard errors are marginally larger, all results are robust in magnitude and precision to the exclusion of children who are neither the biological child nor

grandchild of the household head. Finally, Panel B of Appendix Table 4 confirms that the estimated increase in work is broadly robust to 7-day recall instead of a 12-month recall. For this alternative reference period, participation in work, unskilled work, and work for pay outside the household all increase across most specifications, suggesting that our results are not driven by differential measurement in the longer recall period.

E. Working to Support School Attendance?

To examine potential explanations for the increase in work for pay outside the home, we start by considering transfer sizes, schooling costs, and children's earnings. If children work to make up the shortfall in the net cost of education, we would expect the income they earn to represent a substantial share of this shortfall. For this analysis, we focus on primary school attendance, which increased significantly. School expenditure averages US$73 for every 10-to-14-year old child enrolled in primary school in control communities and US$86 in treatment communities. The difference in school expenditures between control and treatment arms in the full sample suggests that school attendance is costlier for children who switch from idleness to the school-and-work state because of *Pantawid*. Assuming that infra-marginal children in treatment communities, i.e. those who are in school even in the absence of the program, exhibit the same schooling costs as those in control communities, the observed US$13 difference in education costs for the treatment and control groups in the overall sample would reflect an average education cost as high as US$195 for compliers.

Bounded by the interval ($86, $195), total education expenditures for compliers thus appear to be well above the maximum annual per-child education transfer of approximately US$70. The difference between education expenditures and transfers *actually received* by beneficiary households according to the administrative data is higher still. Regressing administrative data on total transfer amounts received by households on the number of children aged 6 to 14 in primary school, in secondary school, and a constant, we find that

households received about US$115 in a calendar year if no children attended primary or secondary school, which is roughly equal to the annualized health grant. Beneficiary households report receiving an additional US$55 for every child in primary school.

The amount earned by children represents a substantive share of the shortfall in schooling costs. Conditional on engaging in any work for pay, enrolled children in control communities report earning US$22 annually. The same value for children in treatment communities is US$43 annually. Assuming that infra-marginal children in control and treatment communities exhibit the same annual income, the observed difference in average earnings of US$21 between treatment and control communities corresponds to average earnings of US$75 by complying children. This income earned by compliers would hence cover roughly half of the upper bound of education costs net of the transfer, approximately equaling US$140.[xvii]

Examining the behavior of siblings of children aged 10 to 14 lends further support to the hypothesis that children work to support their school attendance. We turn first to the eligible older siblings (ages 15 to 17) of the 10-to-14-year olds in our core sample. Panel A of Appendix Table 6 shows that the school participation of these older siblings is not affected, but that these children, too, increase their participation in work for pay outside the household, perhaps helping to offset the education expenditure of their younger siblings. Indeed, Panel A of Table 5 shows larger increases in school and work by 10 to 14 year olds with older siblings than among those without older siblings, a finding comparable to the negative displacement result reported in Barrera-Osorio et al. (2011).

If the lump-sum health grant was used by households to meet some of the schooling cost shortfall left by the education grant, then the lump-sum transfer should be most effective at increasing enrollment and attendance when there are no other school-age children in the household; the greater the number of enrolled children, the greater the dilution in the impact

of the lump-sum health transfer for each child. Consistent with such a dilution, panel B of Table 5 indeed shows that children with no enrolled siblings are more likely to be enrolled in school only and the probability of being enrolled in school decreases with the number of in-school siblings.

F. Alternative Compensatory Behaviors

The evidence thus far is consistent with the shortfall in education costs met by an increase in child labor as well as, perhaps, increased spending from the health grant and shifts in the working patterns of older siblings where applicable. Further analysis suggests that the households did not rely on other compensatory mechanisms to cover the additional cost of schooling. First, adults did not adjust their labor supply, measured for the 7 days prior to the interview, as a result of the CCT: Table 6 examines whether the program affected the probability (i) that any adult household member was involved in agricultural activities, a family-owned non-farm businesses, or fishing and (ii) that adult members in these households worked, and whether they worked for a private household or enterprise, worked for the government, or on their own or household owned farm or non-farm business. Following the estimation strategy outlined above, we find no indication that the program affected household-level micro-entrepreneurial activities or the overall likelihood of adult work. However, there is some evidence of substitution out of self-employment into wage work, which may indicate a need for cash income. These results are consistent with our assumption in the conceptual framework discussed in Appendix 1 that these households are adult labor constrained (60 percent of all adults in these households were already engaged in economic activities in the absence of the program).

Second, household expenditure, other than on health and education, does not appear to have changed, suggesting that changes in household consumption patterns are not driving our results. Table 7 explores the relative expenditures of households with children in our core

sample. The point estimates for education and health expenditures are relatively large (suggesting increases of 18 and 22 percent) although these are not precisely estimated. Approximately twenty percent of these households had any savings, and the average amount saved was $11, suggesting that this is a savings-constrained population that would find it difficult to cover additional education expenditures from savings. All told, these findings indicate that households did not use other compensatory behavior to cover the shortfall in child schooling costs.

G. Alternative Explanations for the Rise in Child Work

This section examines a range of potential alternative explanations for the increase in child work. A first possibility is that the inflow of cash led to changes in the local economy (Angelucci and DeGiorgi (2009), for instance, document positive spillovers on non-beneficiaries living in *Prospera* villages) that increase the returns to work or labor demand in treatment communities. To understand whether the program resulted in such general equilibrium effects, we examined whether wages and economic activity of ineligible households were affected by the program, but find no evidence for such an effect (Appendix Table 7). A second possibility is that household composition changes in response to the cash transfer. For instance, the additional income available to the household may induce increased fertility, in turn decreasing adult female labor supply and increasing demand for child work, but we do not find any evidence that *Pantawid* affected family composition (Appendix Table 8). A third possibility is that school attendance opens up new opportunities for children to work. This may occur if there are few employment opportunities close to the home of the child, but commuting to a school near a market (or another economic hub) allows the child to work. However, Appendix Table 9 shows that, if anything, longer distance to the nearest market is associated with a higher probability of being in work. A fourth option would be that children learn about work opportunities from their peers in school. However, given that the

villages in our sample are small (215 households on average in the baseline PMT data), this mechanism appears unlikely.

Finally, cash transfer programs may improve children's health, thus increasing their capacity for work *and* school participation. Indeed, Kandpal et al. (2016) find that *Pantawid* helps to keep the youngest children healthy, one of the stated aims of the program. In treatment villages, children up to the age of 5 (for whom extensive health data, including anthropometric indicators, were collected) were less likely to be stunted, more likely to eat protein-rich food, and more likely to receive preventative health services. Older children's health may have improved due to increased household expenditure on health and nutrition. The program also required regular deworming for older children, which may have improved schooling outcomes (Baird et al., 2016; Bleakley, 2007; Miguel and Kremer, 2004). If this mechanism were driving our results, we would expect improvements in child health to have similar effects on work for pay outside the household and work without pay inside the household. However, we do not observe an impact on the latter. In addition, Appendix Table 10 shows no significant association between parent-reported offer of deworming pills at school to children aged 10 to 14 and child labor supply in the control areas.[xviii] Moreover, as we discuss below, similar programs in other contexts, including *Prospera* in Mexico, improved child health without increasing child work.

H. The Impact of More Generous Education Subsidies

The evidence presented above suggests that the increase in child work is largely the result of a partial grant for the full cost of education. This observed increase in children's participation in paid work contrasts with evidence from other cash transfer programs, which document either a significant decrease or no change in child labor because of the transfer (reviewed in de Hoop and Rosati, 2014). However, Appendix Table 11 shows that, in virtually all the programs studied, the transfer amount exceeded the full cost of education.

The Philippines thus appears to be the first CCT program to experience a slight rise in the rate of child work, and is one of the few that did not fully cover the cost of education.

To further illustrate this contrast between *Pantawid* and more generous CCT programs, we examine the schooling and child labor effects of the Mexican *Prospera* program when it was first implemented in the late 1990s (Parker, Rubalcava, and Teruel, 2008). This comparison is insightful for three reasons. First, the rural target populations of the two programs had comparable levels of school attendance and child labor: the 1996 Mexican National Survey of Household Income and Expenditure (ENIGH) shows that 84 percent of children aged 12 to 14 were in school while 15 percent were engaged in economic activities with boys, especially in rural areas, being almost twice as likely to work as girls. About 76 percent of children were in school only, 9 percent were idle, 8 percent combined school and work, and 7 percent were in work only. Second, *Pantawid* was explicitly modeled after *Prospera* in terms of both conditions and relative transfer size to household income (Barber and Gertler, 2008; Skoufias and Parker, 2001). The experimental phases of both programs were targeted at the poorest communities with household poverty defined by a PMT (World Bank, 2013). However, in contrast to *Pantawid*, the education grant from *Prospera* was explicitly "set to cover the opportunity costs for students, estimated on the basis of observed children's incomes" (Fiszbein and Schady, 2009, p. 182) and the maximum-possible education grant covered about two-thirds of the earnings of a full-time working child (Schultz, 2004). Third, the pilot phase of the (then *Progresa*, and now) *Prospera*, program was accompanied by a similar cluster-randomized evaluation design (Parker, Rubalcava, and Teruel, 2008). In the late 1990s, 495 rural localities were randomly allocated to an early treatment group (313 localities) and a late treatment group (182 localities), with take up rates of approximately 97 percent (Ozer et al., 2011). We use the baseline household survey administered in 1997, follow-up household survey administered in

1999, as well as 1999 locality level information, to construct variables comparable to those used in the *Pantawid* analysis above (variable construction is described in Appendices 2 and 4).

Replicating the *Pantawid* estimation procedure, we estimate the effects of *Prospera* on children's schooling and work based on regression specification (1). Table 8 presents our estimates of the effects of *Prospera* on participation in school and work by children aged 10 to 14. As established in previous studies (Rubio-Codina, 2010; Schultz, 2004; and Skoufias and Parker, 2001), we find that *Prospera* increased school enrollment and attendance by about six percentage points each (Columns (1) and (2)). However, in contrast with *Pantawid*, *Prospera* reduced the probability of children working for pay by about 1 percentage point (Column (4)) and the probability of children being in neither school nor work by 4 percentage points (Column (8))

Various other studies show that *Prospera* resulted in benefits and behaviors that, at least in theory, can increase children's participation in work, including greater household investment in productive activities (Gertler, Martinez, and Rubio-Codina, 2012), higher consumption by ineligible households (Angelucci and De Giorgi, 2009), and improved children's health (Gertler, 2004; Rivera et al. 2004). The fact that *Prospera* nonetheless *lowered* children's participation in work is consistent with our hypothesis that the size of the education subsidy relative to schooling cost influences the child schooling and work decision. Further, the fact that *Prospera* improved child health while reducing their labor participation suggests that, at least in the case of rural Mexico, the relative value of the education subsidy dominates the health channel when it comes to child work decisions.

VI. Discussion and Conclusions

This paper illustrates how a partial subsidy for a socially desirable good can elicit unanticipated compensatory behavior from complier households. We show that the Philippines' *Pantawid* cash transfer program, which partially subsidized schooling during its early implementation, generated compensatory behavior in the form of concomitant increases in schooling and participation in paid work by the same children. In particular, *Pantawid* increased children's participation in work for pay outside the household by about 5 percentage points, over a control mean of 20 percent. This result appears to been driven by children who would otherwise neither be in school nor in work, and stands in contrast with most other cash transfers, including the Mexican *Prospera*, which increased schooling while decreasing paid work by children. Unlike these other CCTs, *Pantawid* only partially subsidized schooling. Compliance by children who would not be in school in the absence of the program represented a net cost to the household leading these children to make up a substantial share of the shortfall through paid work. We rule out several alternative explanations for the increase in child labor including changes in household investments, adult labor supply, and alternative compensatory behaviors, such as reduced household spending. We also address the role of child health and possible declines in the search cost for child jobs, but do not find evidence consistent with these channels principally driving the observed results on work-for-pay.

This behavioral response to *Pantawid* is consistent with a theoretical framework that posits child labor as a complement to school participation when the offered subsidy does not cover the full cost of schooling, but is high enough to render part-time child work a useful supplemental strategy. This view of child work as complementary to schooling runs counter to most theoretical treatments of child labor, which presents the two as strict substitutes. Specifically, our findings contrast with the luxury axiom in to the child labor model presented

in Basu and Van (1998), which stipulates that child labor occurs only if families could not subsist without child labor. Since time allocated to school and work-for-pay can be adjusted on both extensive and intensive margins, complementarity can arise in the presence of a partial education subsidy, as we observe here.

This study is not without caveats. First, we lack the baseline data to definitively show that children who were idle at baseline switched into paid work *and* schooling after the cash transfer. However, we show balance at baseline along a rich set of other household characteristics. We also do not have data on the health outcomes of the 10-to-14-year-old children in our sample, and therefore can only rule out the potentially positive effects of health improvements through the cash transfer on child labor supply through an intent-to-treat analysis of children offered deworming and by comparison to *Prospera*. Finally, we do not have data on children's social networks, and therefore cannot completely rule out that going to school provided children with information about job opportunities, which, rather than the size of the transfer, led to the increase in child work.

Findings of this nature present complications for the policy maker's choice of transfer amount in a CCT program. A transfer amount set too high may allocate substantial resources to households who would comply with the conditions even without the transfer thus raising concerns about the efficiency of the transfer. On the other hand, a lower transfer amount may not induce all eligible population to take up the preferred behavior or may induce households to adopt compensatory activities such as an increase in child labor. The consideration of an efficient subsidy level should consider not only program costs but also additional costs incurred by financing such a program (such as deadweight loss) and any cost of private behavior change taken in response to the program. Against these costs stand the anticipated benefits of increased school participation and reduced income poverty.[xix]

A back-of-the-envelope calculation of the increase in program costs from increasing the *Pantawid* education grant to a full schooling subsidy demonstrates these tradeoffs. We estimate above that schooling costs ranged between $85 and $195, while the reported annual education grant received by families during the pilot was $70. To provide a full subsidy, *Pantawid* would thus have had to pay between an additional $15 to $125 per child in school per year. For our sample of 654 children, this would have meant an increase ranging between 21 and 179 percent in the disbursements for the education grant (the outlay would have gone from $45,780 to $55,590 for these 654 children if the costs were $85, and to $127,530 if the costs were $195). During the pilot phase, education grants comprised approximately half of the potential total transfer value ($12.50 of a maximum of $25 per month), and the Philippine government reported spending 90 percent of its *Pantawid* budget to the health and education grants (DSWD, 2015). An increase of 21 percent in the outlay for the education grant would thus have translated to a 9.5 percent increase in the overall 2015 program budget of US$ 1.3 billion, while a 179 percent rise in outlays for education grants would have translated to a 80.6 percent budget increase.[xx,xxi] Without knowing the nature of the work done by children, we cannot estimate welfare effects, but note that eliminating the increase in child labor reported by this paper would have come at a substantial increase in total program costs.

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Tables

Table 1. Descriptive statistics: mean values for children from *Pantawid* control communities

Extensive margin:	
Attends	0.882
Attends primary school	0.651
Attends secondary school	0.231
Attends regularly	0.793
Attends primary school regularly	0.579
Attends secondary school regularly	0.215
Worked in past 12 months	0.202
Pay and location:	
For pay, outside own household	0.091
For pay, inside own household	0.031
Without pay, outside own household	0.040
Without pay, inside own household	0.091
Types of occupations:	
Laborers and unskilled workers	0.144
Farmers, forestry workers, and fishermen	0.079
Other	0.008
Worked in past 7 days	0.158
Pay and location:	
For pay, outside own household	0.066
For pay, inside own household	0.017
Without pay, outside own household	0.028
Without pay, inside own household	0.080
Types of occupations:	
Laborers and unskilled workers	0.098
Farmers, forestry workers, and fishermen	0.068
Other	0.000
Mutually exclusive combinations of school and work	
In school only	0.725
In work only	0.038
In school and in work	0.164
Neither in school nor in work	0.073
Worked while school was in session	0.094
Intensive margin:	
Days attended school past 2 weeks	
Unconditional	7.4
Conditional on attending school	8.6
Days worked in past 12 months	
Unconditional	5.9
Conditional on any work	29.7
Days worked for pay, outside own household in past 12 months	

Unconditional	2.2
Conditional on any work	24.4
Hours worked in past 7 days	
Unconditional	1.9
Conditional on any work	12.4
Hours worked for pay, outside own household in past 7 days	
Unconditional	0.7
Conditional on any work	10.6

Note. Estimates based on 656 children aged 10-14 from eligible households in control villages.

Table 2. *Pantawid* program impact on school attendance

	Attends	Attends primary school	Attends secondary school	Attends regularly	Attends primary school regularly	Attends secondary school regularly	Days attended school past 2 weeks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OLS only controlling for municipality and child age.	0.044**	0.039	0.004	0.094***	0.076***	0.016	0.955***
	(0.019)	(0.024)	(0.021)	(0.025)	(0.027)	(0.021)	(0.243)
Additional information:							
Number of observations	1,264	1,264	1,264	1,243	1,243	1,243	1,263
Observations in control group	627	627	627	611	611	611	626
Observations in treatment group	637	637	637	632	632	632	637
Mean in control group	0.887	0.665	0.222	0.795	0.589	0.206	7.502
Mean in treatment group	0.929	0.700	0.228	0.888	0.663	0.223	8.457
Conditional mean in control group							8.648
Conditional mean in treatment group							9.131

Note. Estimates of program impact on self-reported education outcomes of children aged 10 to 14 from eligible households . Standard errors are clustered at the village level. Conditional means are means conditional on any school days attended. *** p<0.01, ** p<0.05, * p<0.1

Table 3. *Pantawid* program impact on children's participation in economic activities

		Pay and location				Types of occupations			Days worked	
	Any work	Work for pay, outside own household	Work for pay, inside own household	Work without pay, outside own household	Work without pay, inside own household	Laborers and unskilled workers	Farmers, forestry workers, and fishermen	Other	Days worked	Days worked for pay, outside own household
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OLS only controlling for municipality and child age.	0.038	0.050**	-0.003	-0.007	0.010	0.045*	-0.005	0.004	1.812	1.728
	(0.029)	(0.021)	(0.010)	(0.011)	(0.021)	(0.026)	(0.016)	(0.007)	(1.765)	(1.370)
Additional information:										
Number of observations	1,264	1,264	1,264	1,264	1,264	1,264	1,264	1,264	1,261	1,263
Observations in control group	627	627	627	627	627	627	627	627	625	626
Observations in treatment group	637	637	637	637	637	637	637	637	636	637
Mean in control group	0.201	0.116	0.032	0.038	0.088	0.144	0.078	0.008	5.906	2.851
Mean in treatment group	0.242	0.155	0.030	0.030	0.102	0.188	0.077	0.013	7.884	4.666
Conditional mean in control group									29.766	23.182
Conditional mean in treatment group									32.771	27.519

Note. Estimates of program impact on work by children aged 10 to 14 from eligible households in the 12 months prior to the interview. Standard errors are clustered at the village level. Conditional means are means conditional on positive days worked or positive days worked for pay outside own household. *** p<0.01, ** p<0.05, * p<0.1

Table 4. *Pantawid* program impact on mutually exclusive combinations of work and schooling

	Mutually exclusive combinations				
	In school only	In work only	In school and in work	Neither in school nor in work	Worked while school was in session
	(1)	(2)	(3)	(4)	(5)
OLS only controlling for municipality and child age.	-0.003	-0.010	0.047*	-0.034**	0.031
	(0.031)	(0.012)	(0.027)	(0.015)	(0.022)
Additional information:					
Number of observations	1,264	1,264	1,264	1,264	1,252
Observations in control group	627	627	627	627	623
Observations in treatment group	637	637	637	637	629
Mean in control group	0.724	0.038	0.163	0.075	0.087
Mean in treatment group	0.716	0.028	0.214	0.042	0.119

Note. Estimates of program impact on mutually exclusive combinations of work in the 12 months prior to the interview and current school attendance for children aged 10 to 14 from eligible households. Here, school refers to current school attendance and work refers to any work in the past 12 months. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Heterogeneous *Pantawid* program impacts on schooling and work by household composition

	Education		Work past 12 months		Mutually exclusive combinations			
	Attends	Attends regularly	Any work	Work for pay, outside own household	In school only	In work only	In school and in work	Neither in school nor in work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Heterogeneous effects on core sample (10-14), by older siblings								
No siblings aged 15-17	0.038	0.033	0.033	0.019	-0.004	-0.015	0.048	-0.029
	(0.026)	(0.033)	(0.042)	(0.037)	(0.043)	(0.018)	(0.037)	(0.020)
One or more siblings aged 15-17	0.040	0.138***	0.067**	0.085***	-0.030	0.003	0.063*	-0.037
	(0.029)	(0.035)	(0.033)	(0.032)	(0.040)	(0.014)	(0.033)	(0.024)
Number of observations:								
P-value F-test (impact no siblings = impact siblings)	0.902	0.005	0.617	0.151	0.716	0.491	0.409	0.246
Number of observations	1,193	1,193	1,212	1,212	1,212	1,212	1,212	1,212
Mean in control group, no siblings	0.891	0.758	0.211	0.091	0.712	0.040	0.171	0.077
Mean in treatment group, siblings	0.929	0.888	0.247	0.148	0.707	0.034	0.213	0.046
Mean in control group, one or more siblings	0.876	0.841	0.190	0.091	0.743	0.036	0.154	0.067
Mean in treatment group, one or more siblings	0.916	0.868	0.231	0.122	0.732	0.020	0.210	0.037
Panel B: Heterogeneous effects on core sample (10-14), by eligible siblings								
No enrolled siblings aged 6-14	0.342**	0.264*	-0.075	-0.077	0.275**	-0.154	0.079	-0.200*
	(0.131)	(0.135)	(0.103)	(0.092)	(0.128)	(0.098)	(0.063)	(0.117)
One or two enrolled siblings aged 6-14	0.007	0.066**	0.068**	0.071**	-0.065*	0.003	0.064**	-0.003
	(0.022)	(0.031)	(0.033)	(0.028)	(0.033)	(0.014)	(0.032)	(0.017)
Three or more enrolled siblings aged 6-14	-0.005	0.044*	0.027	0.051*	-0.016	0.014	0.013	-0.010
	(0.015)	(0.026)	(0.047)	(0.031)	(0.046)	(0.009)	(0.047)	(0.011)
Number of observations:								

P-value F-test (impact 0 siblings = impact 1 or 2 siblings)	0.011	0.147	0.187	0.117	0.011	0.115	0.841	0.094
P-value F-test (impact 0 siblings = impact 3+ siblings)	0.009	0.107	0.349	0.196	0.031	0.094	0.399	0.104
P-value F-test (impact 1 or 2 siblings = 3+ impact siblings)	0.651	0.568	0.450	0.626	0.367	0.501	0.339	0.709
Number of observations	1,264	1,264	1,264	1,264	1,264	1,264	1,264	1,264
Mean in control group, no enrolled siblings	0.296	0.296	0.280	0.200	0.260	0.240	0.040	0.460
Mean in treatment group, enrolled siblings	0.633	0.571	0.217	0.130	0.522	0.087	0.130	0.261
Mean in control group, one or two enrolled siblings	0.903	0.801	0.193	0.084	0.749	0.032	0.161	0.058
Mean in treatment group, one or two enrolled siblings	0.912	0.870	0.239	0.144	0.706	0.034	0.206	0.055
Mean in control group, three or more enrolled siblings	0.968	0.881	0.196	0.078	0.787	0.004	0.191	0.017
Mean in treatment group, three or more enrolled siblings	0.961	0.917	0.247	0.139	0.743	0.017	0.229	0.010

Note. Estimates of heterogeneous program impact on work by children aged 10 to 14 from eligible households. Only municipality and child age fixed effects are included. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table 6. *Pantawid* program impact on other economic activities of beneficiary households

	Household level, past 12 months. Any household members involved in:			Adult level, past 7 days			
	Farming	Non-farm business	Fishing	Worked	Worked for private household or establishment	Worked for government	Self-employed, employer, or worked on household farm or business
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OLS only controlling for municipality	0.042	0.001	0.006	0.007	0.038*	0.007	-0.037
	(0.033)	(0.019)	(0.028)	(0.020)	(0.023)	(0.007)	(0.023)
Additional information:							
Number of observations		830	832	2,480	2,480	2,480	2,480
Observations in control group		422	422	1,251	1,251	1,251	1,251
Observations in treatment group		408	410	1,229	1,229	1,229	1,229
Mean in control group	0.608	0.077	0.134	0.627	0.295	0.023	0.291
Mean in treatment group	0.668	0.075	0.123	0.635	0.333	0.028	0.256

Note. Estimates of program impact on household and adult level economic activities. Sample restricted to eligible households with children aged 10-14. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table 7. *Pantawid* program impact on household expenditure

	Log per capita expenditure	Log per capita food expenditure	Log per capita medical expenditure	Log per capita education expenditure	Log per capita alcohol and tobacco expenditure	Log per capita savings
	(1)	(2)	(3)	(4)	(5)	(6)
OLS only controlling for municipality	0.005	-0.042	0.178	0.221	-0.079	0.169
	(0.043)	(0.044)	(0.191)	(0.153)	(0.081)	(0.184)
Additional information:						
Number of observations	833	833	830	830	833	822
Observations in control group	422	422	422	421	422	415
Observations in treatment group	411	411	408	409	411	407
Mean in control group	9.357	8.917	2.969	4.453	1.087	-0.666
Mean in treatment group	9.345	8.859	3.108	4.652	0.988	-0.551

Note. Estimates of program impact on household expenditure or savings. Sample restricted to eligible households with children aged 10-14. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table 8. *Prospera* program impact on education and work outcomes

	Education		Work past week		Mutually exclusive combinations			
	Attends	Attends regularly	Any work	Work for pay	In school only	In work only	In school and in work	Neither in school nor in work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS only controlling for municipality and child age	0.058***	0.058***	-0.009	-0.010***	0.053***	-0.014***	0.005	-0.044***
	(0.012)	(0.014)	(0.007)	(0.004)	(0.012)	(0.004)	(0.006)	(0.010)
Additional information:								
Number of observations	10,821	10,805	10,886	10,867	10,774	10,774	10,774	10,774
Observations in control group, boys	4142	4135	4177	4171	4121	4121	4121	4121
Observations in treatment group, boys	6679	6670	6709	6696	6653	6653	6653	6653
Mean in control group, boys	0.841	0.805	0.051	0.028	0.827	0.035	0.015	0.124
Mean in treatment group, boys	0.900	0.863	0.042	0.018	0.880	0.021	0.020	0.079

Note. Estimates of *Prospera* program impact on education and work outcomes of children aged 10 to 14 from eligible households. Standard errors are clustered at the level of localities. *** p<0.01, ** p<0.05, * p<0.1

Notes

[i] The program has been in place since 2008 and now covers over 4.5 million poor households.

[ii] Children younger than age 5 in treated areas had higher height-for-age z scores, were less likely to be severely stunted, and more likely to eat protein-rich foods and use health services. Older children (aged 6 to 14) were more likely to be offered de-worming medication.

[iii] The compensatory behavior we document is particularly likely to occur in ultra-poor populations and when the price of school participation exceeds the value of the subsidy by a substantive margin. A later evaluation identifying the local effect of *Pantawid* on the wealthiest beneficiaries (exploiting the poverty means test based on which the program is allocated) did not document a similar impact on child work (World Bank, 2013).

[iv] From an efficiency standpoint, it may be optimal to induce a small amount of child labor, particularly since evidence suggests only a partial negative tradeoff between child labor and human capital formation (Akabayashi and Pscharapoulous, 1999).

[v] A few studies have examined how labor supply, including by children, can help households respond to income shocks. Jacoby and Skoufias (1997) find that households smooth seasonal fluctuations in consumption by drawing upon their children's labor, and that such fluctuations have negligible average effects on human capital. Banerjee, Karlan, and Zinman (2015) show that a relatively small loan to acquire an expensive durable good may lead to complex adjustments in household consumption and labor supply. Various other studies find that micro-credit programs may increase children's participation in productive activities (Augsburg et al., 2012; Nelson, 2011).

[vi] All Appendices are available online at http://jhr.uwpress.org.

[vii] The self-declared reasons for children's participation in economic activities are varied: a substantial number of children work to help in the household-operated farm or business (53 percent) or to otherwise support family income (20 percent), while a smaller fraction works to gain experience in the labor market (10 percent) or to appreciate the value of work (5 percent). While school attendance rates are comparable between rural and urban areas, rural child employment rates are higher (15 percent versus 8 percent). In rural areas, about 82 percent of children aged 10 to 14 are in school only, 13 percent combine school and work, and three percent each are idle and in work only.

[viii] All amounts related to the Philippines in the remainder of this paper are in 2011 US$, the year in which the follow-up data were collected.

[ix] While the amounts mentioned above here are monthly, payment is made every two months.

[x] The focus of these sessions rotates on a monthly basis but covers topics such as good parenting practices, general health and nutrition, and household management.

[xi] In each village, the survey was administered to 10 poor households (those with a PMT score below the eligibility threshold) with children aged 0 to 14 and/or a pregnant woman, 10 non-poor households with eligible children and/or pregnant women, 5 poor households without eligible children or pregnant women, and 5 non-poor households without eligible children or pregnant women.

[xii] Household-level attrition from the baseline sample was 11.4 percent (80 out of 624 households) in treated villages and 11.2 percent (80 out of 634 households) in control villages, with no evidence of a significant difference by treatment status (See also Appendix Table 1, reproduced from World Bank (2013)).

[xiii] Of course, given that regular school attendance is a program requirement, these self-reported data need to be interpreted with some care (Baird and Özler, 2012). Households may misreport school attendance to ensure that they are not removed from the program even if the responses to the questionnaire were treated as confidential and were not used to check compliance. By emphasizing children's education, the program may also have stigmatized child labor in treated villages, thus leading to a downward bias in our estimated effects on child labor.

[xiv] The outcome measures for work are observed for about 93.4 percent of children (94.6% in the treatment group and 92.1% in the control group). Appendix Table 11e shows that reported pre-intervention child work is *lower* in treated communities than in control communities.

[xv] This coefficient reaches traditional levels of significance if we include covariates to increase precision (as shown in Appendix Table 4).

[xvi] We do not have recall data on schooling and duration worked, so cannot establish the robustness of those estimates using fixed effects.

[xvii] Note that this shortfall may be an upper bound on the true shortfall if infra-marginal children increased their school expenditure or reduced their participation in work for pay.

[xviii] Deworming was offered at school, with 75 percent of 10-14 year olds in control areas being offered them. We do not find any effect of the deworming offer on regular school attendance (result not displayed).

[xix] While our framework predicts that transfers smaller than the cost of education can lead to compensatory behavior, policy makers should also consider a variety of contextual factors. For instance, rigidities in minimum working hours and school attendance requirements might make it difficult for children to combine work-for-pay and school (see Edmonds and Schady, 2012), even in the presence of a partial subsidy. Household resource availability is another mediating factor: since the lack of asset wealth is a key factor of the proposed mechanism, we do not expect similar changes in the wealthier communities phased into *Pantawid* as the program continued to expand its coverage.

[xx] The risk of child labor may be exacerbated over time because transfer sizes have not kept up with inflation, so the current shortfall in education costs may be higher than estimated here.

[xxi] Since the rise in child labor appears to be concentrated among the poorest households, an alternative approach might be to introduce a differentiated subsidy that falls in value as the estimated income of the beneficiary household rises. While this would mean more complex program implementation, it would decrease the additional program costs required to avoid the increase in paid work by children.