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ABSTRACT

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

Could a partial subsidy for child education increase children's participation in paid work? In contrast to much of the theoretical and empirical child labor literature, this paper shows that child work and school participation can be complements under certain conditions. Using data from the randomized evaluation of a conditional cash transfer program in the Philippines, the analysis finds that some children, who were in neither school nor work before the program, increased participation in school and work-for-pay after the program. Earlier cash transfer programs, notably those in Mexico, Brazil, and Ecuador, increased school attendance while reducing child labor. Those programs fully offset schooling costs, while the transfers under the Philippine transfers fall short of the full costs of schooling for a typical child. As a result, some beneficiary children from poor Philippine households increased work to support their schooling. The additional earnings from this work represent a substantive share of the shortfall in the schooling costs net of transfer. The paper rules out several potential alternative explanations for the increase in child labor, including changes in household productive activities, adult labor supply, and household expenditure patterns that, in principle, can arise after a cash transfer and may also affect the supply of or demand for child labor.

JEL Classification: C93, I21, J22, O22

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

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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, 2014; Fiszbein and Schady, 2009). In this paper, we present a counterexample from the experimental pilot of a conditional cash transfer program in the Philippines that shows 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.[1] The randomized evaluation (World Bank, 2013) of *Pantawid* 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 5 percentage points and improving the health and nutrition of eligible 0-14 year olds (World Bank, 2013; Kandpal et al., 2016).[2] 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 5 percentage points, as compared 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.

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

[2] 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.

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 transfer amount per child was approximately US$70 although the households in our sample reported receiving an average education transfer of 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.[3]

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

[3] 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).

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.[4, 5] 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 findings 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.

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

[5] 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).

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’s (2013) 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, 2014; 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 trade-off 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.

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 engage 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

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, 2016). About 85 percent are in school only, 11 percent combine school and work, 3 percent are idle (i.e. in neither in school nor in work), and 2 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).[6] 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).

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 a

[6] 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 3 percent each are idle and in work only.

number of socio-demographic and household asset indicators in order 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)[7] is offered to children aged 6 to 14 who attend primary or secondary school regularly (at least 85 percent of school days in a given month). The education grant is provided for up to 3 children per household and for 10 months a year.[8] 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.[9] 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 the 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

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[7] 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.

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

[9] 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.

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.

The evaluation design

A village-randomized evaluation was designed by the World Bank in conjunction with the Philippines 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, 11 percent lower than the average per capita income in the other program areas from this period.

IV. Data and methods

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.[10]

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

[10] 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.

primary and secondary schools. Finally, we use administrative data on the monthly amounts transferred to beneficiary households over the evaluation period.

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:

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

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 appendices, 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).[11] All standard errors are clustered at the village-level.

[11] We use the following fixed effect specification: $Y_{ivt} = \beta_0 + \beta_1 * T_{vt} + d_i + d_{2008} + d_{2009} + d_{2011} + \varepsilon_{ivt}$. 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. We do not have recall data on schooling and duration worked, so we cannot establish the robustness of those estimates using fixed effects.

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 1,264 children: 637 from 411 households in treatment villages and 627 from 422 households in control villages.[12]

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 (instrumenting for household participation in the cash transfer program using village-level assignment to the program; Appendix Tables 1a-c).

V. Results

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

[12] Household-level attrition from the baseline sample was 11.2% in control and 11.4% in treated households, with no evidence of systematic attrition by baseline characteristics (World Bank, 2013).

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. 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.

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). [13] *Pantawid* thus appears to have significantly increased regular primary school attendance.

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 the channels discussed above. Table 3 explores such impacts.[14] 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.[15] 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. Further, 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

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[13] 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.

[14] 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.

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

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 of these 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)).

Robustness of the reported impacts of Pantawid on child schooling and labor

We now 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:

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

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. Our sensitivity 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, which may lead to concerns around endogenous changes in household composition, and (3) using 12-month recall instead of 7-day recall for work outcomes.

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 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. Appendix Tables 3, 4 and 5 further illustrate that our results are robust 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.

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.[16] 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*. Indeed, there is likely substantial variation in the actual schooling costs faced by individuals. Assuming that 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

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[16] There are 96 children in primary school and work, while only 45 attend secondary school while working. We focus on primary school as a result of the larger sample size. Secondary school expenditures are markedly higher than primary school expenditures, while the reported child labor income is the same regardless of level of school enrollment. Appendix 4 describes how we calculate the total private costs of education.

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.

Results show that the amount earned by children represents a large 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 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 marginal 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.[17]

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).

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[17] 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.

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.

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. This result is also consistent with our assumption in the conceptual framework appendix 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 20 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.

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) 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 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 the home of the child, but commuting to a school near a market (or other 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. 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. Moreover, as we discuss below, similar programs in other contexts, including *Prospera* in Mexico, improved child health without increasing child work.

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 as a result of the transfer (reviewed in de Hoop and Rosati, 2014). However, Table 8 shows that, in virtually all of 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 9 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 Philippine’s *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 have 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, and the schooling of marginal children represented a net cost to the household leading children to make up a substantial share of this cost through paid work. We rule out several alternative explanations for this increase in child labor including changes in household investments, adult labor supply, and household spending patterns. 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. In particular, our findings relate to 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. However, 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.

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 take into account 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.[18]

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 trade-offs. 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

[18] 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, 2011), 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.

translated to a 80.6 percent budget increase.[19,20] 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.

[19] 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.

[20] 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.

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Appendix 1: A Conceptual Framework for the Child Labor Response to a Partial Education Subsidy

This paper concerns the household's response to an offered schooling subsidy that does not fully cover the cost of schooling. Possible responses include an asset drawdown, an increase in adult labor supply, a shift in consumption patterns, or an increase in child labor. For the poorest households, such as those studied here, an increase in child labor may be the only available margin as they lack significant assets, are credit-constrained with consumption patterns are at or near subsistence levels, and (we assume) adults are not able to supply additional labor after the subsidy.[21] Therefore, if the shortfall in education costs after the introduction of the partial schooling subsidy must be met through shifts in household labor, it is the children who were not working prior to the subsidy that will supply this labor.

A question may be why some children were idle prior to the subsidy instead of working. One possibility is that disutility from work outweighs the fairly modest income that could be earned through child labor. Another possibility is that the opportunities for child work are few and not well known, and there is a search cost. The conceptual framework presented in this section considers the first of these two reasons, which is that even poor households would not like their children to work as the returns are not substantial and there is disutility or stigma from paid work by children. While we do not model the second possibility, we explore its applicability in the empirical section of the paper and as we review the literature on other conditional cash transfer program and their effects.

[21] It is also possible that parents value children's education less than do children. In that case, a cash transfer enables children to start attending school, but the cost of additional schooling must be primarily borne by the children themselves. While we have no information to support this hypothesis, such a breakdown in altruism, or parents' myopia resulting in under-investments in children's education would also be consistent with both the estimated results and the broader conceptual framework (Das, Do, and Ozler, 2005).

Households maximize a utility function defined over the child's lifetime income and the disutility of child effort in work or school by deciding (a) whether to send their children to school and the time they spend in education, and (b) whether their children work and the amount of time devoted to it. As we shall see, children might participate in either, both, or neither of these activities. We do not consider substitution between present and future consumption in the household as the optimal arbitrage condition will not alter the essence of the results we want to illustrate here: for this reason, we focus on the maximization of the lifetime utility of the children conditional on current household income *y*.

More formally, the household's problem is to maximize a utility function *U=U(Y,e)* where *Y* is the expected discounted lifetime earnings of the child, including any income earned during childhood, and *e* is total effort expended by the child in the period before adulthood.[22] Effort can take the form of time devoted to school, e_s, and to work, e_w. Time can also be spent in leisure, l. Normalizing total time available during childhood to one, the child faces the following time budget constraint:

$$e_s + \ e_w = 1 - \mathrm{l}$$

Discounted lifetime earnings depend on accumulated human capital, which is a function of the household's choice of schooling level for the child, *S*. Schooling choice, in turn, is a function of the cost of schooling, *c*, net of any subsidy, *p*, relative to current period household income, *y*, as well as the amount of time devoted to schooling, e_s and to work, e_w:

$$Y = f\big(S(c - p, e_s; e_w, ; y)\big)$$

The cost of schooling, *c*, is fixed while the subsidy, *p*, is allowed to vary with the policy choice.

[22] We assume that the utility function has a strictly positive (negative) first derivative and strictly negative (positive) second derivative in income (effort).

The schooling decision is subject to two further conditions:

$$\left(\frac{\partial Y}{\partial \mathrm{y}} | S = s\right) = 0 \text{ and } \frac{\partial Y}{\partial e_s} = 0 \ if \ e_s < e_{s.min}$$

The first condition states that the level of initial income does not have any direct effect on the returns to education; it only influences the decision of whether to attend school and for how long. We assume that household income varies across the population, but do not make any specific assumptions on the characteristics of its distribution. The second condition states that investment in education is lumpy. For schooling to have any impact on earnings, a minimum amount of time, $e_{s.min}$, must be devoted to school; else there are no income gains to education. This assumption, reflecting the minimum time investment needed for schooling to increase human capital implies that enrolling a child in school creates a discontinuity in the time budget of the child.[23]

As this model describes household decision-making related to child schooling and work, we abstract from the adult labor decision and assume there are no complementarities in the relationship between child and adult labor.[24] Since the population we study is low-income and credit constrained, we assume that the adult household member supplies a full unit of labor at the exogenous parent wage, w_{parent}.[25] The child wage, w_{child}, is also taken as exogenously fixed and lies below w_{parent}. Given the child wage, the net cost of schooling, c-p, and the level of income, y, children can be in one of 4

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[23] This assumption is reflected in the general requirement that children are required to attend school for minimum numbers of days during a school year to advance.

[24] We also assume that the subsidy level, c, does not appreciably affect the rate of child labor through a change in the returns to child labor due to increased economic activity in the locality, a change in household composition, or the improved health of the child. While we do not discuss these channels theoretically we explore their empirical relevance in subsequent sections of the paper. None of them appear to play a role in child labor decisions in the *Pantawid* context.

[25] We further assume that when the child enters adulthood she will in turn supply a full unit of labor.

states: idle (both $e_s = 0$ and $e_w = 0$, i.e. $e_l = 1$), work only ($e_s = 0$), school only ($e_w = 0$ and $e_s \geq e_{s.min}$), or school and work (both $e_w > 0$ and $e_s \geq e_{s.min}$).

Denote the minimum level of school subsidy needed for a given household to prefer school and work, as opposed to idleness, as p^0, which in turn defines minimum lifetime earnings, Y^0, that can be attained by restricted combinations of school effort, $e_s^0 \geq e_{s.min}$, and work effort, e_w^0, given an income level, y^0, and a subsidy level, p^0.

$$Y^0 = f(S(c - p^0, y^0, e_s \geq e_{s.min}, e_w > 0))$$

If both attending school and working is to be a viable option for a child, the expected utility from combined school and work needs to exceed the utility from the idle state for a household at the same income level. Specifically:

$$U(Y^0, e_s^0, e_w^0) \geq U\big(\mathrm{Y}(\mathrm{S} = 0, e_w = 0)\big)$$

The curve denoted U_{Y0} in Figure 1 presents the possible combinations of school subsidy and current period household income at which the child is indifferent between idleness and joint school and work. If a particular combination of school subsidy and household income falls below this curve, the child either works only or remains idle.[26] Children who both work and study can reallocate the time spent at work to further study as the subsidy level increases beyond p^0 but still remains below c; however these children cannot exit from work and remain in school as the full schooling cost must be met.

As the subsidy level p increases further and approaches c then the full cost of schooling is nearly met, and any subsidy in excess of c becomes an infra-marginal transfer to total household

[26] For children that do not enroll in school, the decision to work or to remain idle depends on the comparison between the earnings from child work and the disutility of time devoted to work. A child will enter the labor force if there is some level of e_w such that

$$U(Y(S = 0, e_w > 0), e_w) \geq U(Y(S = 0, e_w = 0), 0)$$

income. At some point, the return to continuing in child work is surpassed by the discounted total gains from increased attention to school. Call this transition point of lifetime earnings Y^*:

$$Y^* = f(S(c - p^*, y^*, e_s \geq e_{s.min}, e_w = 0)$$

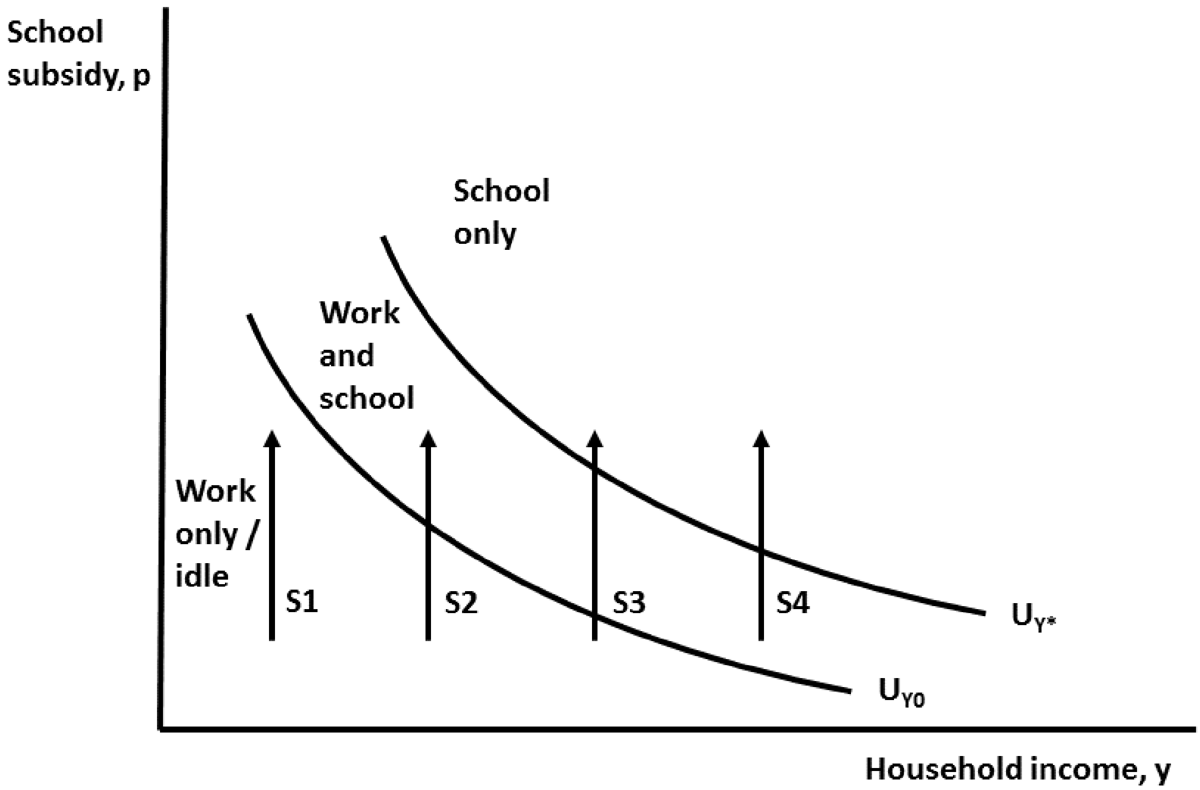
The U_{Y*} curve in Figure 1 denotes the combinations of current income and subsidy value for which a child is just indifferent between attending school and work and only attending school. At any point on or above the U_{Y*} curve, the following holds:

$$U(Y^*, e_s > 0, e_w = 0) \geq U(Y, e_s > 0, e_w > 0) \; for \; all \; p \geq p^*$$

and any child finding herself above the U_{Y*} curve will devote her time only to school.

The lumpiness of investments in human capital implies thresholds in both the utility from school enrollment and school cost that determine whether a child is enrolled. Our model thus categorizes four transitions between school, work, and idleness as a function of the level of subsidy and of the current household income. We now consider a relatively modest increase in the school subsidy and the set of children currently not enrolled in school. If household income is low enough that the partial subsidy still does not make the expected gains in utility from school enrollment and part-time work an attractive option, then the child will not leave the idle or work only state. This situation is labeled S1 in Figure 1. However, at a higher level of current household income, the additional subsidy combined with part-time child work fully offsets the remaining school costs, leading to the child enrolling in school and beginning to work; this transition is labeled S2 in Figure 1. At higher levels of household income, the same partial subsidy might induce idle or working children to transition directly to the school only state (S3), or for children in school and work to transition into school alone (S4). This framework thus predicts that children of higher current income households should exhibit a reduced labor response, i.e. be less likely to be in school and work than children from poorer households after the introduction of the subsidy.

Figure 1. Combinations of school and work depending on current household wealth and school subsidy value



Appendix 2: Definition of outcome measures used in the analysis

Outcomes as defined for the Philippines data

The analysis concentrates on children's participation in education and work. For education, we consider current school attendance (in primary or secondary school), regular school attendance, and days of school attendance in the two weeks prior to the interview. We define regular school attendance as attending school for at least 85 percent of the days that school was in session in the two weeks prior to the interview (self-reported).

For work, we focus on participation in economic activities, days worked in the 12 months prior to the interview (with or without pay), and annual earnings. Work without pay refers to any work without pay on a farm, work in the private or public sector, work for own account, and work in a business belonging to the child or the household. Work without pay does not include household chores. We separately examine participation in work for pay outside the household, work without pay both inside and outside the household, work for pay inside the household, as well as participation in the following occupations: (i) farmers, forestry workers, and fishermen, (ii) laborers and unskilled workers, and (iii) all other occupations. We check the robustness of the estimated impact on work using the same set of outcome variables, but reported for the seven days preceding the interview (instead of the 12 month recall period). We focus primarily on work in the 12 months prior to the interview, because this outcome variable is less likely to be affected by seasonality concerns.

To calculate annual earnings, we first estimate children's individual hourly wage rate by dividing the last pay they received by the hours worked over the period covered by the last pay. We multiply this hourly wage rate by the estimated number of days worked in the last year and "usual" hours worked per day in the job. Finally, to examine summary shifts in child behavior as a result of the program, we analyze four mutually exclusive combinations of school attendance and work in the last 12 months: in school only, in work only, in work and in school, and neither in work nor in school.

To complement the analysis with these last outcomes, we additionally examine whether, in the past year, children worked while school was in session. To construct this outcome variable, we rely on the following two questions asked to *working* children: "Were you enrolled in the past 12 months?" and "Did you sometimes work [in this occupation] while also attending school (i.e. during the school year)?"

Outcomes as defined for the Mexican data

We classify children as attending school if they "currently" attend school, regardless of the level attended. We classify them as attending school regularly if they currently attend school and did not miss any school days during the 4 weeks prior to the interview. We classify them as working if, during the week prior to the interview, they worked, had a job but did not work, or worked in the household business, on the household property, or on the household farm. We classify them as working for pay if they worked in the week prior to the interview for a wage or salary.

Appendix 3: Balance of baseline characteristics

Balance of baseline characteristics and variable definitions, used in the Philippine data

This Appendix describes the individual, household, and community characteristics we include as controls in the regressions presented in Appendix Tables 3, 4, and 5. We constructed these characteristics using the baseline Proxy Means Test survey, unless noted otherwise. We briefly describe why these characteristics are appropriate potential covariates and present balance tests to assess the validity of the village-level randomized assignment. In each balance test, we regress the vector of covariates on the treatment indicator and cluster the standard errors at the village level. There are no statistically significant differences between the treatment and control groups across the range of outcomes considered.

Appendix Table 11a shows the balance along child (aged 10-14) level controls: age, gender, and an indicator variable taking the value 1 if neither the child's mother nor his/her father lives in the child's household. We consider the latter an important covariate because a large literature shows that parental absence (mostly death) is an important predictor and determinant of schooling outcomes (e.g. Evans and Miguel, 2007) and cash transfers can help compensate for parents' absence (Fitzsimons and Mesnard, 2014). All of these child-level controls are constructed using follow-up data as no child-level information can be derived from the data collected for the proxy-means test. In the absence of differential attrition, these variables are unlikely to be affected by the program (and the lack of significance in the balance tests indeed implies there was no differential attrition unless the characteristics were originally unbalanced at baseline and then experienced a degree of differential attrition that would result in balance at end line, a situation not supported by any supplementary quantitative or qualitative evidence).[27]

[27] In the impact estimates, we do not include the age variable as a linear control, but rather indicator variables for age equal to 10, 11, 12, and 13 interacted with the indicator variable for being male.

Appendix Table 11b examines the balance of the household level measures: a wealth index (included because it is a key targeting criterion of the program), whether the household head is Muslim and whether the household belongs to an indigenous ethnic group (included to account for differences in education outcomes across population groups), whether the household head ever attended school (included because it is commonly considered as a determinant of the well-documented intergeneration link between parents and children's life outcomes, e.g. Chevalier, 2004), whether the household is engaged in agricultural activities (included because most child labor (62%) in the Philippines takes place in agriculture according to Understanding Children's Work, 2016) and household size and demographic composition (number of members aged 0 to 5, 6 to 14, and 15 to 17, included because the program is partly targeted based on the number of children in these age ranges). The variables for Muslim household head and indigenous household are based on the follow-up data, all remaining household indicators were measured at baseline in the Proxy Means Test survey. The wealth index is defined as a normalized measure with weights from the first principal component of the following dwelling and asset characteristics: electricity, strong roof, strong walls, dwelling owned by the household, the household has no access to toilet facilities, the household's main source of water is located in the household's own dwelling or plot, and ownership of the following assets: TV, video, stereo, refrigerator, washing machine, air conditioning, living room furniture set, dining room furniture set, car, phone, PC, microwave, and motorcycle.

Appendix Table 11c explores the balance of the two key village level characteristics: whether the distance from the village hall to nearest public primary or secondary school, respectively, is more than 2 kilometers, which is the 95th percentile of distance to primary school. We include these variables as the cost of commuting to school is an important component of the overall cost of education (see also Appendix 4).

Appendix Table 11d explores the balance of the child labor recall data measures across treatment and control villages, separately for the years 2007, 2008, and 2009. Importantly, these variables are not used as controls in our regressions, but exploited in the panel fixed-effects estimates displayed in Appendix Table 4.

Across all of these balance tests, not one indicator for treatment assignment is significant at standard levels of precision, suggesting that the randomization process, stratified by municipality, resulted in a well-balanced sample at baseline. As such, any estimated impact of the program is unlikely to be caused by unobserved confounders. Finally, to further rule out a lack of balance between treated and control areas at baseline driving our results, we examine schooling and work for 10-to-17-year-old children from program ineligible households (i.e. those with imputed income above the eligibility threshold) and find no differences in schooling or work among ineligible children (results displayed in Panel B of Appendix Table 6).

In Appendix Table 12, we present the results of a multinomial logit regression of the four mutually exclusive combinations of work and school on the household characteristics for which we carried out balance checks in the control villages. We estimate the multinomial logit both for our primary sample of children from eligible poor households (columns (5) – (8)) and for the full sample of children observed in the control villages (columns (1) – (4)) to highlight the role of income in the probability that children work and/or attend school.[28] Several results are as expected: the probability of children being in school decreases with the distance to school and increases with household wealth (here we can interpret wealth as a proxy for the role that household income plays in the model in Section II). The probabilities of being in work only, combining work and school, and being idle all

[28] We estimate the multinomial logit for the full sample of children in the data in control villages (not only for children from the eligible poor households) to highlight the role of income in the probability that children work and/or attend school. Municipality fixed effects are included. We do not display the coefficients for these dummy variables.

decrease in wealth.[29] The probability of being neither in work nor in school, on the other hand, increases with distance to school. The younger the child is, the more likely she is to be in school only and the less likely to be in work only. Boys are generally less likely to be in school only than girls. Children are less likely to be in school if the household head never attended school. Children are also less likely to attend school only and more likely to work only if the household is engaged in agricultural activities.

Baseline balance in the Mexican data

For the analysis of child work and schooling in the Mexican data, we tested for balance along the following individual and household characteristics: age, gender, and an indicator variable taking the value 1 if neither of the child's parents live in the household, a wealth index (with weights derived from the first principal component of the following dwelling characteristics: electricity, three indicators for roof material ((i) sheets made of metal, fiber glass, or plastic, (ii) sheets made of cardboard, or (iii), concrete), indicators for wall material ((i) wood, (ii) bricks, or (iii) adobe), dwelling owned by the household, the household has no access to toilet facilities used exclusively by the household, the household has access to piped water on the household's dwelling or plot, and ownership of the following assets: TV, video, stereo, blender, refrigerator, washing machine, fan, gas stove, gas heater, car, and truck.), whether the household belongs to an indigenous people group, whether the household head ever attended school, whether the household is engaged in (non-livestock) agricultural activities, the total number of household members, and the number of household members aged 0 to 5 and 6 to 17. Finally, we also test for balance in whether there is a

[29] We get similar results if we include annual baseline per capita income instead of the wealth index: the probability of being in school only increases significantly with 1.7 percentage points for every additional 1,000 Philippine Peso of per capita income, while the probability of being in work only, in school and in work, or idle each decreases by about half a percentage point (statistically significant at the 5% level).

primary school in the locality and whether there is a secondary school in the locality. The locality level characteristics are established using November 1999 follow-up data. We found that there is one statistically significant difference between the treatment and control group: treatment localities are about 4 percentage points more likely to have a primary school than control localities. We do not present these balance tests here, as numerous other studies have investigated the balance of the Mexican data. The most notable of these is Behrman and Todd (1999), who find minor but statistically significant imbalances when using the household level (instead of locality level) data.

Appendix 4: Estimates of schooling costs

The *Pantawid* data contain information on a range of education expenditures for individual pupils including expenditure on school fees, exam fees, fees for extracurricular activities, school materials, uniforms, books, pocket money and snacks, transport, and other expenditures are reported for each child in school. Because reference periods for these expenditures may differ, we converted all of these to annual expenditures. In the calculation of annual expenditure on pocket money, snacks, and transport, we assume that children who are in school attend school 98 percent of the academic calendar's 204 school days, based on the average self-reported number of days that children attended school in the 2 weeks prior to the interview and the number of days that school was in session in the 2 weeks prior to the interview.

The *Pantawid* survey collected data on “the total cost to go to school one way” without clarifying whether students make this commute on every school day. To limit the probability of overestimating transport costs for boarders or students who otherwise live closer to the school during part of the school year, we exclude children who (i) live more than 50km from their school or (ii) spend more than US$1.15 a day, which is the ninetieth percentile, on transport to and from school. These restrictions lead to the exclusion of 24 children aged 10 to 14 from eligible households attending primary school. We further exclude the children with non-transport education expenditure in the highest percentile.

The *Progresa* data do not contain information on the cost of education. Instead, we used the 1998 Mexican National Survey of Household Income and Expenditure (ENIGH) to assess household education expenditures. ENIGH contains information on fees and subscription, education services, overnight stays, additional education, special education, transport to school, purchase and maintenance of books and other school materials. Because the ENIGH does not provide all of this information at the child level, we regressed total household expenditure on education in the month before the interview on the number of children in primary and secondary school grades.

To be consistent with the Philippines impact evaluation sample of poor households, we restricted our analysis to rural households with total expenditure in the national bottom quartile. We also restrict to households that do not have individuals attending other school grades, or individuals over 18 attending school, to limit the probability that we are picking up other household education expenditures in the regression. The regression does not include a constant, as we assume that the cost of education is zero if no one in the household attends school. The estimated monthly household expenditure on education increases by US$3 for every child in primary school and US$5 for every child in secondary school. The direct cost of education was thus substantially lower than even the minimum value of the *Prospera* education grants.

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 childen 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 houshold	Work for pay, inside own houshold	Work without pay, outside own houshold	Work without pay, inside own houshold	Laborers and unskilled workers	Farmers, forestry workers, and fishermen	Other	Days worked	Days worked for pay, outside own houshold
	(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 regularly	Any work	Work for pay, outside own houshold	In school only	In work only	In school and in work	Neither in school nor in work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Heterogeneous effects on core sample (10-14), by older siblings							
No siblings aged 15-17	0.040	0.033	0.019	-0.004	-0.015	0.048	-0.029
	(0.033)	(0.042)	(0.037)	(0.043)	(0.018)	(0.037)	(0.020)
One or more siblings aged 15-17	0.134***	0.067**	0.085***	-0.030	0.003	0.063*	-0.037
	(0.036)	(0.033)	(0.032)	(0.040)	(0.014)	(0.033)	(0.024)
Number of observations:							
P-value F-test (impact no siblings = impact siblings)	0.005	0.617	0.151	0.716	0.491	0.409	0.246
Number of observations	1,193	1,212	1,212	1,212	1,212	1,212	1,212
Mean in control group, no siblings	0.758	0.211	0.091	0.712	0.040	0.171	0.077
Mean in treatment group, siblings	0.888	0.247	0.148	0.707	0.034	0.213	0.046
Mean in control group, one or more siblings	0.841	0.190	0.091	0.743	0.036	0.154	0.067
Mean in treatment group, one or more siblings	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	0.264*	-0.075	-0.077	0.275**	-0.154	0.079	-0.200*
	(0.135)	(0.103)	(0.092)	(0.128)	(0.098)	(0.063)	(0.117)
One or two enrolled siblings aged 6-14	0.066**	0.068**	0.071**	-0.065*	0.003	0.064**	-0.003
	(0.031)	(0.033)	(0.028)	(0.033)	(0.014)	(0.032)	(0.017)
Three or more enrolled siblings aged 6-14	0.044*	0.027	0.051*	-0.016	0.014	0.013	-0.010
	(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)	0.147	0.187	0.117	0.011	0.115	0.841	0.094
P-value F-test (impact 0 siblings = impact 3+ siblings)	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)	0.568	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
Mean in control group, no enrolled siblings	0.296	0.280	0.200	0.260	0.240	0.040	0.460
Mean in treatment group, enrolled siblings	0.571	0.217	0.130	0.522	0.087	0.130	0.261
Mean in control group, one or two enrolled siblings	0.801	0.193	0.084	0.749	0.032	0.161	0.058
Mean in treatment group, one or two enrolled siblings	0.870	0.239	0.144	0.706	0.034	0.206	0.055
Mean in control group, three or more enrolled siblings	0.881	0.196	0.078	0.787	0.004	0.191	0.017
Mean in treatment group, three or more enrolled siblings	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: Summary of the Literature on Conditional Cash Transfer Programs and Education Costs

Country	Reference(s)	Impact on School Enrollment	Impact on Child Labor	Subsidy Relative to Schooling Costs	Notes
Brazil	Ferro et al. (2010)	Positive (2.5 percentage points)	Negative (3 percentage points)	Full subsidy	Although Ferro et al. (2010) do not explicitly mention the cost of education, one of the authors kindly confirmed that the transfers will have exceeded the cost of education. Children in the examined cash transfer program would typically attend public schools, which are free of charge. The government provides textbooks and uniforms and in rural areas there are school buses.
Cambodia	Ferreira et al. (2009)	Positive (20 percentage points)	Negative (10 percentage points)	Full subsidy	Noted on page 24.
Colombia	Barrera-Osorio et al. (2011); Attanasio ()	Positive (3-5 percentage points)	Negative on students in grades 6-10 (30 percent reduction), no effect on those in grade 11	Full subsidy	Noted on page 171.
Costa Rica	Duryea and Morrisson (2004)	Positive (2.9 to 8-7 percentage points, depending on method)	No effect		The conditional transfer program in Costa Rica was an in-kind transfer.
Ecuador	Edmonds and Schady (2011)	Positive (19 percentage points)	Negative (9.9 percentage points)	Full subsidy	The authors note on page 118 that the size of the transfer is greater than the average increase in schooling costs between primary and secondary school. While the transfer program in Ecuador was unconditional, it was accompanied by marketing activities advocating for the relevance of schooling and that part of the beneficiaries perceived the program as conditional on school participation.
Honduras	Glewwe and Olinto (2004); Galiani and McEwan (2013)	Positive (1-2 percentage points-- Glewwe and Olinto; 8 percentage points-- Galiani and McEwan)	No effect (Glewwe and Olinto); negative (3 percentage points-- Galiani and McEwan)	Full subsidy	We infer that the transfer amount exceeded the cost of education from Fiszbein and Schady (2009, P.182-183) and Rawlings and Rubio (2005, P.34).
Indonesia	Sparrow (2007)	Positive (13 percentage points)	Negative (4 percentage points)	Full subsidy through junior secondary; marginally lower than senior secondary costs	Noted on page 105.
Jamaica	Levy and Ohls (2007)	Positive on attendance, enrollment not reported (38.5-50.6 percentage points)	No effect	Full subsidy	Noted on page 7
Mexico	Skoufias and Parker (2001); Schultz (2004); Rubio-Codina (2010)	Positive (girls: 1.3 percentage points in primary school and 7.1 pp in secondary school. Boys: 1.2 percentage points in primary school, 5.2 pp in secondary-- Schultz); Positive for girls (4.9 percentage points--Rubio-Codina)	Negative (1.2 percentage points for girls, 1.4 percentage points for boys-- Schultz); Negative for girls (8.4 percentage points-- Rubio-Codina)	Full subsidy	Inferred from Fiszbein and Schady (2009, P.182-183) and Rawlings and Rubio (2005, P.34), and confirmed in own calculations reported in the paper.
Nepal	Edmonds and Shrestha (2013)	Positive (4.9 percentage points for full subsidy plus stipend) 2.3 percentage points but insignificant for full subsidy alone.	Negative for full subsidy plus stipend (5.3 percentage points); no effect of full subsidy alone.	Full subsidy in one arm; full subsidy plus an additional stiped in another	Noted in footnote 7 on page 7.
Nicaragua	Dammert (2008); Thomas (2010); Barham, Macours and Maluccio (2013)	Positive for ex-ante enrolment (19 percentage points-- Thomas), ex-post early enrolment (14.2 percentage points-- Barham et al.), ex-post attendance (12 percentage points for girls, 18 percentage points for boys-- Dammert), and long-term attainment (half a year-- Barham et al.)	Negative (1 percentage point for girls, 11 percent points for boys-- Dammert)	Full subsidy	Inferred from Barham et al. (2013) and Thomas 2010-- primary education is free and the fees transfer was designed to offset all other schooling costs.

Table 9. *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

Appendix tables

Appendix Table 1a. *Pantawid* program impact on education outcomes, alternative specifications

	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)
2SLS TOT controlling only for municipality:	0.044**	0.039	0.002	0.088***	0.073**	0.014	1.004***
	(0.019)	(0.029)	(0.028)	(0.025)	(0.032)	(0.028)	(0.269)
Number of observations	1,351	1,351	1,351	1,351	1,351	1,351	1,351
Observations in control group	678	678	678	678	678	678	678
Observations in treatment group	673	673	673	673	673	673	673
Mean in control group	0.879	0.649	0.230	0.789	0.576	0.213	7.426
Mean in treatment group	0.921	0.686	0.232	0.879	0.651	0.227	8.387
Conditional mean in control group							8.647
Conditional mean in treatment group							9.142

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

Appendix Table 1b. *Pantawid* program impact on the extensive margin of work, alternative specifications and alternative reference period

		Pay and location					Types of occupations			Intensity	
	Any work	Work for pay, outside own houshold	Work for pay, inside own houshold	Work without pay, outside own houshold	Work without pay, inside own houshold		Laborers and unskilled workers	Farmers, forestry workers, and fishermen	Other	Days worked	Days worked for pay, outside own houshold
	(1)	(2)	(3)	(4)	(5)		(6)	(7)	(8)	(9)	(10)
Panel A: Work in the past 12 months											
2SLS TOT controlling only for municipality:	0.043	0.053*	0.001	-0.009	0.012		0.002	0.042	0.006	2.056	0.021
	(0.048)	(0.031)	(0.012)	(0.011)	(0.032)		(0.025)	(0.037)	(0.008)	(1.577)	(1.129)
Panel B: Work in the past 7 days											
2SLS TOT controlling only for municipality:	0.043	0.053*	0.001	-0.009	0.012		0.002	0.042	0.006	-0.197	-0.085
	(0.048)	(0.031)	(0.012)	(0.011)	(0.032)		(0.025)	(0.037)	(0.008)	(0.525)	(0.315)
Additional information, work in the past 7 days:											
Number of observations	1,264	1,264	1,264	1,264	1,264	1,265	1,264	1,264	1,264	1,261	1,263
Observations in control group	627	627	627	627	627	628.000	627	627	627	625	626
Observations in treatment group	637	637	637	637	637	637.000	637	637	637	636	637
Mean in control group	0.201	0.108	0.116	0.091	0.032	0.038	0.144	0.078	0.008	5.906	2.851
Mean in treatment group	0.242	0.122	0.155	0.141	0.030	0.030	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. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 1c. *Pantawid* program impact on children's participation in economic activities, alternative specifications

	Mutually exclusive combinations						
	In school only	In work only	In school and in work	Neither in school nor in work	In primary school and in work	In secondary school and in work	Worked while school was in session
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TOT controlling only for municipality:	-0.010	-0.014	0.057	-0.032**	0.037	0.019	0.046
	(0.046)	(0.013)	(0.044)	(0.015)	(0.030)	(0.019)	(0.029)
Additional information:							
Number of observations	1,264	1,264	1,264	1,264	1,264	1,264	1,264
Observations in control group	627	627	627	627	627	627	627
Observations in treatment group	637	637	637	637	637	637	637
Mean in control group	0.724	0.038	0.163	0.075	0.113	0.049	0.094
Mean in treatment group	0.716	0.028	0.214	0.042	0.113	0.069	0.138

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. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Table 2. Heterogeneity of *Pantawid* program impact on education and work outcomes by gender

	Education		Work past 12 months	
	Attends	Attends regularly	Any work	Work for pay, outside own houshold
	(1)	(2)	(3)	(4)
OLS:				
Impact on boys	0.040	0.088**	0.046	0.050*
	(0.029)	(0.034)	(0.038)	(0.028)
Impact on girls	0.050**	0.099***	0.032	0.050**
	(0.020)	(0.027)	(0.032)	(0.023)
Additional information:				
P-value F-test (impact boys = impact girls)	0.697	0.762	0.803	0.862
Number of observations	1,264	1,264	1,264	1,264
Mean in control group, boys	0.864	0.840	0.249	0.122
Mean in treatment group, boys	0.899	0.940	0.293	0.170
Mean in control group, girls	0.914	0.841	0.145	0.055
Mean in treatment group, girls	0.964	0.933	0.185	0.109

Note. Estimates of program impact on education and work outcomes by gender for children aged 10 to 14 from eligible households. Impact estimated using only municipality dummies as controls. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 3. *Pantawid* program impact on education outcomes, alternative specifications

	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 with controls:	0.050***	0.035	0.014	0.097***	0.071***	0.024	1.019***
	(0.019)	(0.023)	(0.019)	(0.024)	(0.026)	(0.020)	(0.237)
Probit without controls:	0.041**	0.033	0.005	0.087***	0.069**	0.016	
	(0.019)	(0.026)	(0.023)	(0.025)	(0.029)	(0.024)	
Logit without controls:	0.040**	0.033	0.005	0.086***	0.069**	0.017	
	(0.019)	(0.026)	(0.023)	(0.025)	(0.029)	(0.023)	
OLS excluding children not directly related to	0.041**	0.033	0.005	0.087***	0.069**	0.017	0.941
household head	(0.019)	(0.026)	(0.023)	(0.025)	(0.029)	(0.024)	(0.255)

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

Appendix Table 4. *Pantawid* program impact on the extensive margin of work, alternative specifications and alternative reference period

		Pay and location				Types of occupations			Intensity	
	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 past year / Hours worked past week	Days worked past year / Hours worked past week *for pay*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Work in the past 12 months										
OLS with controls:	0.046*	0.052***	-0.002	-0.000	0.009	0.050**	-0.004	0.006	1.794	1.426
	(0.026)	(0.019)	(0.010)	(0.010)	(0.021)	(0.024)	(0.016)	(0.006)	(1.731)	(1.270)
Probit controlling only for municipality:	0.041	0.045**	-0.001	-0.007	0.006	0.045	-0.004**	0.002		
	(0.031)	(0.019)	(0.008)	(0.010)	(0.018)	(0.025)	(0.013)	(0.005)		
Logit controlling only for municipality:	0.043	0.042**	-0.002	-0.007	0.009	0.044*	-0.003	0.003		
	(0.031)	(0.017)	(0.007)	(0.010)	(0.017)	(0.024)	(0.011)	(0.004)		
Panel fixed effects based on recall data	0.049	N.A.	N.A.	N.A.	N.A.	-0.006	0.054**	0.006	N.A.	N.A.
	(0.033)					(0.019)	(0.026)	(0.006)		
OLS excluding children not directly related to household head	0.039	0.047**	-0.001	-0.009	0.012	0.043	-0.003	0.004	2.044	1.989
	(0.030)	(0.022)	(0.010)	(0.011)	(0.022)	(0.027)	(0.016)	(0.007)	(1.802)	(1.403)
Panel B: Work in the past 7 days										
OLS only controlling for municipality and child age:	0.046*	0.024	-0.002	-0.006	0.023	0.048**	-0.008	0.003	0.040	0.152
	(0.027)	(0.016)	(0.008)	(0.010)	(0.019)	(0.022)	(0.015)	(0.002)	(0.410)	(0.248)
OLS with controls:	0.052**	0.027*	-0.004	0.002	0.021	0.051**	-0.006	0.003	0.029	0.076
	(0.025)	(0.015)	(0.008)	(0.008)	(0.018)	(0.021)	(0.014)	(0.002)	(0.364)	(0.231)
Probit controlling only for municipality:	0.047*	0.021	-0.001	-0.005	0.022	-0.007	0.047**	0.003		
	(0.027)	(0.014)	(0.006)	(0.008)	(0.017)	(0.013)	(0.021)	(0.004)		
Logit controlling only for municipality:	0.047*	0.021	-0.001	-0.005	0.020	-0.005	0.044**	0.003		
	(0.026)	(0.013)	(0.006)	(0.008)	(0.015)	(0.012)	(0.020)	(0.004)		
OLS excluding children not directly related to household head	0.051*	0.024	0.000	-0.006	0.027	0.049**	-0.005	-0.006	0.073	0.170
	(0.027)	(0.016)	(0.008)	(0.010)	(0.019)	(0.023)	(0.015)	(0.032)	(0.413)	(0.246)

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

Appendix Table 5. *Pantawid* program impact on children's participation in economic activities, alternative specifications

	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 with controls:	-0.009	-0.013	0.059**	-0.037**	0.047**
	(0.028)	(0.011)	(0.025)	(0.014)	(0.021)
Probit controlling only for municipality:	-0.004	-0.008	0.047	-0.032**	0.029
	(0.034)	(0.010)	(0.029)	(0.015)	(0.021)
Logit controlling only for municipality:	-0.007	-0.008	0.050*	-0.032**	0.030
	(0.034)	(0.009)	(0.027)	(0.014)	(0.019)
OLS excluding children not directly related to household head	-0.006	-0.009	0.048*	-0.033**	0.031
	(0.032)	(0.011)	(0.028)	(0.015)	(0.023)

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

Appendix Table 6. Heterogeneity of *Pantawid* program impact on schooling and work by household composition

	Education		Work past 12 months	
	Attends	Attends regularly	Any work	Work for pay, outside own houshold
	(1)	(2)	(3)	(4)
Panel A: Effects on older siblings (15-17) of children in core sample (10-14) from eligible households				
OLS without controls:	-0.025	0.031	0.077	0.107***
	(0.048)	(0.051)	(0.047)	(0.039)
Additional information:				
Number of observations	474	461	395	395
Observations in control group	234	224	189	189
Observations in treatment group	240	237	206	206
Mean in control group	0.632	0.563	0.323	0.175
Mean in treatment group	0.600	0.591	0.417	0.291
Panel B: Effects on children (10-17) from ineligible households				
OLS without controls:	0.008	0.015	0.033	-0.001
	(0.019)	(0.026)	(0.024)	(0.010)
Additional information:				
Number of observations	1,277	1,237	1,162	1,162
Observations in control group	663	633	607	607
Observations in treatment group	614	604	555	555
Mean in control group	0.861	0.815	0.216	0.120
Mean in treatment group	0.857	0.820	0.247	0.132

Note. Estimates of program impact on education and work outcomes by gender for children aged 10 to 14 from eligible households. Etimates include village and household level controls described in Appendix 2. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 7. *Pantawid* program impact on the local economy

	Village level	Ineligible households with children aged 6 to 14						
		Household level, past 12 months. Any household members involved in:			Adult level, past 7 days			
	Wages of adult male laborers	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)	(8)
OLS only controlling for municipality and child age.	9.306	0.020	-0.015	-0.032	-0.010	0.001	0.003	-0.016
	(6.422)	(0.030)	(0.016)	(0.024)	(0.013)	(0.014)	(0.010)	(0.011)
Additional information:								
Number of observations	127	2,323	2,322	2,323	5,403	5,403	5,403	5,403
Observations in control group	62	1182	1180	1181	2802	2802	2802	2802
Observations in treatment group	65	1141	1142	1142	2601	2601	2601	2601
Mean in control group	142	0.615	0.131	0.110	0.620	0.257	0.061	0.108
Mean in treatment group	150	0.648	0.114	0.074	0.611	7.000	0.065	0.090

Note. Estimates of program impact on household and adult level economic activities in ineligible households. Standard errors are clustered at the village level *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 8. *Pantawid* program impact on household composition

	Household Size	Dependency Ratio	Female dependency Ratio	Male dependency Ratio	Children Aged 0-14
	(1)	(2)	(3)	(4)	(5)
Treated	0.120	0.107	0.132	0.233*	0.102
	(0.163)	(0.065)	(0.131)	(0.129)	(0.130)
After	0.905***	-0.150***	-0.308***	-0.185***	0.055**
	(0.092)	(0.028)	(0.062)	(0.055)	(0.024)
Treated*After	-0.040	-0.001	0.084	-0.016	0.030
	(0.116)	(0.043)	(0.083)	(0.086)	(0.033)
Additional information					
Number of observations	664	664	664	664	664
Observations in control group	336	336	336	336	336
Observations in treatment group	328	328	328	328	328
Mean in control group	6.193	1.180	2.460	2.350	3.005
Mean in treatment group	6.313	1.287	2.592	2.582	3.107

Note. Estimates of program impact on composition of households with 10-14 year olds in study sample Standard errors are clustered at the village level. OLS only controlling for municipality and child age. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 9. The Effect of Remoteness Child on Work and Schooling

	Mutually exclusive combinations							
	Control Households				Treated Households			
	In school only	In work only	In school and in work	Neither in school nor in work	In school only	In work only	In school and in work	Neither in school nor in work
Travel time to nearest market	-0.002***	0.000	0.002***	-0.000***	-0.003**	0.000**	0.003**	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Number of observations	530	530	530	531	567	567	567	567
Travel fare to nearest market	-0.001	0.000	0.001*	-0.000	0.001*	-0.000	-0.001**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of observations	602	602	602	602	616	616	616	616

Note. Estimates of travel time and travel costs to nearest market on the school attendance and work for 10-14 year old children 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

Appendix Table 10. The Effects of Deworming on Child Work-for-Pay on Eligible Children in Control Areas

			Pay and location				Types of occupations			Days worked	
	Any work	Any work for pay	Work for pay, outside own houshold	Work for pay, inside own houshold	Work without pay, outside own houshold	Work without pay, inside own houshold	Laborers and unskilled workers	Farmers, forestry workers, and fishermen	Other	Days worked	Days worked for pay, outside own houshold
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Child was offered deworming pills during last school year	-0.025	-0.043	-0.014	-0.032	-0.002	-0.003	0.021	-0.025	-0.014	-1.458	-1.018
	(0.050)	(0.041)	(0.024)	(0.029)	(0.032)	(0.019)	(0.027)	(0.044)	(0.011)	(1.747)	(0.779)
Additional information:											
Number of observations	546	546	546	546	546	546	546	546	546	544	546
Observations not offered deworming	133	133	133	133	133	133	133	133	133	133	133
Observations offered deworming	413	413	413	413	413	413	413	413	413	411	413
Mean in group not offered deworming	0.241	0.165	0.060	0.105	0.105	0.038	0.075	0.173	0.015	6.083	1.564
Mean in group offered deworming	0.167	0.092	0.027	0.063	0.075	0.034	0.077	0.109	0.002	3.314	0.475

Note. Estimates of 10-14 year old children being offered deworming at school in the past 12 months on any work in the past 12 months. OLS only controlling for municipality and child age. Standard errors are clustered at the village level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Table 11a. Balance of child characteristics in *Pantawid* data

	Study sample			All eligible households			All households in survey sample		
	Age	Male	Neither father nor mother lives in household	Age	Male	Neither father nor mother lives in household	Age	Male	Neither father nor mother lives in househol d
				(1)	(2)	(3)	(1)	(2)	(3)
OLS without controls:	0.043	-0.011	0.007	0.005	0.006	0.012	0.017	0.013	0.015
	(0.059)	(0.030)	(0.013)	(0.064)	(0.028)	(0.011)	(0.051)	(0.019)	(0.014)
Additional information:									
Number of observations	1,264	1,264	1,264	1,310	1,310	1,310	2,184	2,184	2,184
Observations in control group	627	627	627	656	656	656	1,114	1,114	1,114
Observations in treatment group	637	637	637	654	654	654	1,070	1,070	1,070
Mean in control group	11.968	0.537	0.040	11.997	0.529	0.029	11.955	0.521	0.094
Mean in treatment group	12.013	0.526	0.047	12.002	0.535	0.041	11.972	0.535	0.109

Note. Estimated differences in individual covariates measured in the endline survey for children aged 10-14 from eligible households. Estimates based on OLS regressions without controls. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Appendix table 11b. Balance of household characteristics in *Pantawid* data

	Wealth index	Household head is muslim	Household belongs to indigenous people group	Household head never attended school	Household engaged in agricultural activities	Total number of household members	Number of children aged 0 to 5	Number of children aged 6 to 14	Number of children aged 15 to 17	Children, 10-14, enrolled in school
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: OLS without controls on study sample:	-0.035	0.075	0.004	0.004	0.012	0.115	0.009	0.087	-0.030	-0.031
	(0.035)	(0.054)	(0.053)	(0.027)	(0.048)	(0.152)	(0.071)	(0.088)	(0.051)	(0.023)
Additional information:										
Number of observations	796	833	833	791	796	796	796	796	796	796
Observations in control group	400	422	422	397	400	400	400	400	400	400
Observations in treatment group	396	411	411	394	396	396	396	396	396	396
Mean in control group	9.036	0.070	0.149	0.091	0.698	6.420	0.898	2.323	0.553	0.830
Mean in treatment group	9.001	0.148	0.153	0.096	0.710	6.535	0.907	2.409	0.523	0.785
Panel B: OLS without controls on all eligible households:	-0.023	0.071	-0.017	-0.000	0.044	-0.003	0.056	-0.016	-0.044	-0.044
	(0.029)	(0.051)	(0.058)	(0.021)	(0.046)	(0.139)	(0.058)	(0.086)	(0.037)	(0.046)
Additional information:										
Number of observations	1,330	1,167	1,167	1,325	1,330	1,330	1,330	1,330	1,330	1,330
Observations in control group	670	585	585	667	670	670	670	670	670	670
Observations in treatment group	660	582	582	658	660	660	660	660	660	660
Mean in control group	9.093	0.072	0.149	0.078	0.685	5.828	0.906	1.743	0.475	0.830
Mean in treatment group	9.071	0.143	0.137	0.078	0.729	5.826	0.962	1.727	0.430	0.785
Panel C: OLS without controls on all households in baseline:	-0.017	0.062	-0.005	0.004	0.028	-0.102	0.017	-0.031	-0.029	-0.031
	(0.031)	(0.042)	(0.046)	(0.017)	(0.036)	(0.088)	(0.028)	(0.044)	(0.020)	(0.023)
Additional information:										
Number of observations	3,595	2,350	2,350	3,575	3,595	3,595	3,595	3,595	3,595	3,595
Observations in control group	1,817	1,191	1,191	1,806	1,817	1,817	1,817	1,817	1,817	1,817
Observations in treatment group	1,778	1,159	1,159	1,769	1,778	1,778	1,778	1,778	1,778	1,778
Mean in control group	9.523	0.055	0.142	0.086	0.565	4.489	0.482	0.966	0.361	0.830
Mean in treatment group	9.506	0.117	0.137	0.090	0.593	4.388	0.498	0.936	0.332	0.785

Note. Estimated differences in household covariates across treatment and control villages. Estimates based on OLS regressions without controls. All variables come from the baseline measurements taken to determine household eligibility for the transfer program with two exceptions: religion of the household head and household members belonging to an indigenous group, which come from the endline survey. The dwelling and asset index is the first principal component of the following dwelling characteristics: electricity, strong roof, strong walls, dwelling owned by the household, the household has no access to toilet facilities, the household's main source of water is located in the household's own dwelling or plot, and ownership of the following assets: TV, video, stereo, refridgerator, washing machine, air conditioning, living room furniture set, dining room furniture set, car, phone, PC, microwave, and motorcycle. Standard errors are clustered at the barangay level. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 11d. Balance of pre-intervention child work measures, recall data for children aged 10 to 14 at endline interview in *Pantawid* data

	2009				2008				2007			
	Any work	Laborers and unskilled workers	Farmers, forestry workers, and fishermen	Other	Any work	Laborers and unskilled workers	Farmers, forestry workers, and fishermen	Other	Any work	Laborers and unskilled workers	Farmers, forestry workers, and fishermen	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
OLS with controls:	-0.011	-0.022	0.011	-0.003	-0.012	-0.014	0.003	-0.002	-0.016	-0.015	-0.000	-0.002
	(0.032)	(0.026)	(0.016)	(0.002)	(0.016)	(0.013)	(0.009)	(0.002)	(0.014)	(0.012)	(0.007)	(0.002)
Additional information:												
Number of observations	1,210	1,210	1,210	1,210	1,164	1,164	1,164	1,164	1,165	1,165	1,165	1,165
Observations in control group	603	603	603	603	580	580	580	580	582	582	582	582
Observations in treatment group	607	607	607	607	584	584	584	584	583	583	583	583
Mean in control group	0.124	0.103	0.033	0.003	0.053	0.043	0.016	0.002	0.043	0.034	0.012	0.002
Mean in treatment group	0.114	0.081	0.044	0.000	0.041	0.029	0.019	0.000	0.027	0.019	0.012	0.000

Note. Estimated differences in recall data between the treatment and the control villages for children aged 10-14 from eligible households. Estimates based on OLS regressions without controls. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 11c. Balance of village characteristics in *Pantawid* data

	Distance to nearest public primary school from town hall > 2 Km	Distance to nearest public secondary school from town hall > 2 Km
	(1)	(2)
OLS without controls:	0.067	0.083
	(0.051)	(0.106)
Additional information:		
Number of observations	120	86
Observations in control group	60	41
Observations in treatment group	60	45
Mean in control group	0.050	0.561
Mean in treatment group	0.117	0.644

Note. Estimated differences in village level covariates taken from the endline questionnaire. Estimates based on OLS regressions without controls. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 12. Determinants of mutually exclusive combinations of work and school for children from *Pantawid* control communities

	All households				Eligible households only			
	In school only	In work only	In school and in work	Neither in school nor in	In school only	In work only	In school and in work	Neither in school nor in
Child aged 10	0.483***	-0.381***	-0.064*	-0.039	1.237***	-1.760***	0.463***	0.060
	(0.051)	(0.063)	(0.038)	(0.027)	(0.246)	(0.344)	(0.126)	(0.068)
Child aged 11	0.161***	-0.044***	-0.068**	-0.049**	0.116***	-0.060**	-0.015	-0.042*
	(0.030)	(0.014)	(0.028)	(0.019)	(0.043)	(0.023)	(0.040)	(0.024)
Child aged 12	0.084***	-0.038***	-0.015	-0.031	0.048	-0.037*	0.041	-0.052*
	(0.028)	(0.013)	(0.021)	(0.020)	(0.037)	(0.019)	(0.027)	(0.031)
Child aged 13	0.097***	-0.032**	-0.016	-0.048**	0.050	-0.035*	0.035	-0.050*
	(0.027)	(0.013)	(0.023)	(0.020)	(0.035)	(0.020)	(0.031)	(0.027)
Male	-0.096***	0.024**	0.039*	0.033**	-0.084***	0.022	0.037	0.025
	(0.020)	(0.010)	(0.021)	(0.015)	(0.027)	(0.015)	(0.026)	(0.021)
Neither biological mother nor biological father lives in the household	0.039	-0.017	-0.048	0.026	1.006***	-1.686***	0.508***	0.171*
	(0.043)	(0.028)	(0.043)	(0.024)	(0.225)	(0.317)	(0.141)	(0.092)
Wealth index	0.032***	-0.012**	-0.011*	-0.010	0.024	-0.015	0.001	-0.009
	(0.008)	(0.005)	(0.006)	(0.006)	(0.016)	(0.010)	(0.012)	(0.010)
Distance to nearest public primary school from town hall > 2 Km	-0.130*	0.042	0.033	0.055*	-0.264***	0.049	0.109	0.106***
	(0.068)	(0.033)	(0.064)	(0.028)	(0.086)	(0.052)	(0.080)	(0.041)
Distance to nearest public secondary school from town hall > 2 Km	0.019	0.018	-0.019	-0.017	0.055	0.020	-0.035	-0.039
	(0.033)	(0.015)	(0.026)	(0.026)	(0.048)	(0.024)	(0.038)	(0.038)
Household head is muslim	0.177**	-0.044	-0.170**	0.037	0.364***	-0.061	-0.272*	-0.030
	(0.071)	(0.033)	(0.078)	(0.035)	(0.132)	(0.053)	(0.151)	(0.051)
Household belongs to indigenous people group	0.030	-0.011	0.011	-0.030	-0.006	-0.021	0.047	-0.020
	(0.043)	(0.011)	(0.043)	(0.021)	(0.053)	(0.019)	(0.060)	(0.024)
Household head never attended school	-0.104*	0.022	-0.012	0.094***	-0.161**	0.035	0.005	0.121***
	(0.054)	(0.018)	(0.047)	(0.027)	(0.077)	(0.028)	(0.074)	(0.038)
Household engaged in agricultural activities	-0.078***	0.028**	0.045*	0.005	-0.070	0.047**	0.039	-0.016
	(0.030)	(0.012)	(0.025)	(0.019)	(0.044)	(0.020)	(0.041)	(0.024)
Total number of household members	-0.001	0.006*	-0.003	-0.003	-0.002	0.011**	0.001	-0.010
	(0.010)	(0.004)	(0.009)	(0.006)	(0.016)	(0.005)	(0.013)	(0.011)
Number of children aged 0 to 5	-0.016	0.016**	-0.012	0.012	-0.036	0.024**	-0.016	0.028
	(0.030)	(0.007)	(0.025)	(0.013)	(0.039)	(0.012)	(0.030)	(0.019)
Number of children aged 6 to 14	-0.003	-0.002	0.000	0.005	-0.014	-0.005	0.012	0.007
	(0.015)	(0.004)	(0.013)	(0.009)	(0.019)	(0.005)	(0.018)	(0.014)
Number of children aged 15 to 17	0.015	-0.016*	-0.014	0.015	0.018	-0.024*	-0.019	0.025
	(0.026)	(0.009)	(0.019)	(0.016)	(0.037)	(0.013)	(0.028)	(0.026)
Additional information:								
Number of observations	1032				627			

Note. Coefficients represent marginal effects estimated on the basis of a multinomial logit regression. Standard errors are clustered at the village level. The estimation sample includes children aged 10 to 14 from all households, those eligible and those ineligible. The estimated specification includes indicator variables for municipalities and for missing observations. The coefficients for these indicator variables are not displayed in the table *** p<0.01, ** p<0.05, * p<0.1