Understanding Children's Work
An Inter-Agency Research Cooperation Project

Understanding Children's Work Programme Working Paper Series, June 2016

Complementarities between schooling and child work in the presence of an education subsidy

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As part of broader efforts towards durable solutions to child labor, the International Labour Organization (ILO), the United Nations Children's Fund (UNICEF), and the World Bank initiated the interagency Understanding Children's Work (UCW) Programme in December 2000. The Programme is guided by the Roadmap adopted at The Hague Global Child Labour Conference 2010, which lays out the priorities for the international community in the fight against child labour. Through a variety of data collection, research, and assessment activities, the UCW Programme is broadly directed toward improving understanding of child labor and youth employment, their causes and effects, how they can be measured, and effective policies for addressing them. For further information, see the project website at www.ucw-project.org.

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AKNOWLEDGEMENTS

We thank Jorge Avalos, Eric Edmonds, Yusuke Kuwayama, Berk Özler, Aleksandra Posarac, and participants in the 2015 EUDN conference for insightful comments and suggestions.

UCW gratefully acknowledges the support provided by the United States Department of Labor and the Global Affairs Canada for the development of the report*.

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ABSTRACT

Can 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, we show that child work and school participation can be complements. Using data from the randomized evaluation of a conditional cash transfer program in the Philippines, we find that children who were in neither school nor work before the program increased participation in both school and work-for-pay after the program. Earlier cash transfer programs, notably those in Mexico, Brazil, and Ecuador, have been shown to increase school attendance while reducing child labor. Those programs fully offset schooling costs, while the Philippine program only provided a partial subsidy. As a result, children from poor households increased work to support their schooling, and those additional earnings represent a substantive share of the shortfall in schooling costs net of transfer. We rule out several potential alternative explanations for the increase in child labor, including changes in household productive activities, changes in adult labor supply, and changes in other household expenditure patterns that, in principle, can arise after a cash transfer.

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1. INTRODUCTION

1. The theoretical literature that explores household decisions over child labor generally treats education and child labor as substitutes for a child's time and effort. 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 the accumulation of human capital. Most of the empirical literature on the link between schooling and child labor finds evidence to support this view (Edmonds and Schady, 2014; Ferreira, Filmer and Schady, 2009; Schady et al. 2008; Beegle et al., 2006; Manacorda, 2006). In contrast, a few studies, including Bourguignon, Ferreira, and Leite (2003) and Ravallion and Wodon (2000), allow for the theoretical possibility of complementarities between child labor and schooling, although their empirical evidence also points at a substitution effect. In this paper we provide new evidence that in certain situations schooling and work can indeed be complements.

2. Since the time allocated to school and work-for-pay can be adjusted on extensive and intensive margins, complementarity can arise in instances when an offered education subsidy only partially offsets education expenditures. Conditional cash transfer programs often aim to improve educational attainment by providing resources to beneficiaries who comply with stipulated thresholds for eligible children's school attendance. If, however, the cash transfer does not fully offset the costs of attendance, we may observe compensatory behaviors, such as a concomitant increase in child schooling *and* labor, as poor (and adult labor constrained) households attempt to supplement the partial subsidy. Using a theoretical framework and data from a cash transfer program in the Philippines, we show that low-income households compensate for education costs net of the subsidy by also sending to work the children that they enroll in school.

3. The program studied here, the *Pantawid Pamilya Pilipino Program* (henceforth *Pantawid*), supports poor households by providing two transfers: one conditional on household health behavior and the other conditional on child school attendance. The program has been in place since 2008 and currently covers more than 4.5 million poor households. The initial randomized evaluation of *Pantawid* (World Bank, 2013) showed that it broadly achieved its primary objectives. School participation of eligible children (those aged 6 to 14 from pre-identified poor households) increased by almost five percentage points. The program also had beneficial effects on child health and nutrition (Kandpal et al., 2016).[1] Effects on household expenditure beyond education and health spending appear limited.

4. Using data from the randomized impact evaluation of *Pantawid*, we find that the increase in school participation was accompanied by a

[1] Young children (up to age 5) in treated areas had higher height-for-age z scores, and were less likely to be severely stunted, as well as being more likely to eat protein-rich foods and to use of health services, while older children (aged 6 to 14) were more likely to take de-worming medication.

concomitant increase in self-reported child labor: children's participation in work for pay outside the household increased by about 5 percentage points, relative to a control group mean of 20 percent. In particular, the program appears to have encouraged children who would otherwise be neither in school nor in work to attend school *and* to start working. We examine a host of potential explanations for the increase in children's work and suggest that children's school participation and work was complementary in the face of *Pantawid's* (partial) schooling subsidy to poor households. During the period of the evaluation, education transfers did not fully cover the cost of education, suggesting that school attendance of marginal children, who started attending in response to the program, represented a net cost to the household. These marginal children began working for pay to make up a substantial share of this cost. We rule out a range of potential alternative 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 affect the household demand for child labor.

5. While estimating the full welfare effect of the increase in children's school attendance and work is beyond the scope of this paper, the results reported here are critical to our understanding of the relationship between schooling and work-for-pay as well as to an assessment of the cost effectiveness of alternative transfer schemes.[2] Our results demonstrate that a schooling subsidy can crowd in or crowd out child labor depending on the value of the transfer relative to the cost of schooling, the household's budget and time constraints, and demand for child labor in the local market. The results thus run counter to most theoretical approaches in the child labor literature that present school and child work as substitutes. This paper also contributes to the literature on the design of conditional transfer programs. This literature typically compares the size of the transfer to household income but, as we document, the cost of the behavior on which the program is conditioned (in our case school participation) can also be 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 inframarginal. A transfer too small may not sufficiently compensate potential compliers to modify behavior, even if the presence of positive externalities is the original motivation for the subsidy. Alternatively a transfer that doesn't 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.

6. The results we present are related to the extensive literature on the effects of cash transfer programs on schooling outcomes (summarized in

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

several reviews including Baird et al., 2014; Fiszbein and Schady, 2009; Saavedra and Garcia, 2012) and child labor (also summarized in several reviews: de Hoop and Rosati, 2014; Edmonds, 2008; Edmonds and Schady, 2014; Fiszbein and Schady, 2009). This literature generally finds that conditional cash transfer programs increase children's school participation while simultaneously lowering their engagement in economic activities. We discuss how our results compare to this literature. Reviewing this literature, we find that cash transfer programs, including those in Mexico, Brazil, Cambodia, Colombia, Ecuador, and Honduras, typically provide education transfers equal to or in excess of the cost of education. Then, we use the publicly-available data on the Mexican *Prospera* program, which in contrast to *Pantawid* fully subsidized schooling costs, to show that *Prospera* led to a concomitant increase in child schooling and a *decrease* in child labor. This comparison is particularly relevant as the target rural populations of the two programs had similar levels of children's school attendance and participation in child labor.

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

8. The paper proceeds as follows. In the next section, we present a theoretical framework to examine behavioral responses of households to a cash transfer conditional on children's school participation that does not fully cover the cost of education. Section III discusses the context in which *Pantawid* was implemented, including a school participation rates and the prevalence of children's work at the onset of *Pantawid*. It also describes the program and the accompanying evaluation. Section IV presents the data and our empirical strategy. Section V presents the results. It examines the impact of the *Pantawid* program on children's participation in education and economic activities, discusses various channels potentially explaining the observed effects, and finally compares the *Pantawid* results to results found for other programs and Mexico's *Prospera* program in particular. Section VI offers concluding thoughts.

2. THE SCHOOLING AND CHILD WORK DECISIONS

9. We present a simple theoretical framework to highlight how the presence of fixed schooling costs and/or of non-convexity in the time budget constraint, generated by a minimum amount of time that must be devoted to school attendance, can lead to complementarity between education and child labor in the presence of subsidies that do not fully cover the cost of attending school.

10. We consider a two-period overlapping generation model of a unitary household.[3] In our low-income and largely rural setting, we assume that households are credit constrained. Households maximize a utility function defined over children's lifetime income and the disutility of child effort by deciding (a) whether to send their children to school and the time they spend in education, (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 households, 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.

11. 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.[4] Effort can take the form of time devoted to school, e_s, 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:

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

12. Discounted lifetime earnings depend on accumulated human capital that is a function of schooling, S. Schooling, in turn, is a function of the cost of schooling, c, net of any subsidy, p, relative to initial household income, y, as well as the amount of time devoted to schooling, e_s:

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

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

14. The schooling decision is subject to two further conditions:

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

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[3] The theoretical predictions do not depend on a simplifying assumption of a unitary household, but only on the non-convexities from fixed costs of education and/or minimum time requirements for school attendance.

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

15. 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 in order 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[5] to increase human capital implies that enrolling a child in school determines a discontinuity in the time budget of the child.

16. 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.[6] 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}.[7] The child wage, w_{child}, is also taken as exogenously fixed and lies below w_{parent}.

17. Given the child wage, the net cost of schooling, *c*-*p*, and the level of income, *y*, children can be in either of 4 statuses: 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 $e_s \geq e_{s.min}$).

18. 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 a 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.

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

19. 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. We norm this reference utility level of the idle decision to zero. Specifically:

$$U(Y^0, e_s^0, e_w^0) \geq U(0,0,0)$$

20. 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

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[5] 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

[6] We also assume that the subsidy level, *c*, does not appreciably affect the rate of child labor through an increase in returns to child labor due to increased economic activity in the locality, a change in household composition, and the improved health of the child. While we do not discuss this 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, thus supporting the theoretical outline presented here.

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

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.[8] Children who both work and study can reallocate the time spent at work to study as the subsidy level increases beyond p^0 but still remain below c: however these children cannot exit from work if they wish to remain in school as the full schooling cost must be met.

21. 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 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^*:

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

22. 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 UY* curve will devote her time only to school.

23. With this framework we can now categorize several transitions between school, work, and idleness as a function of the level of subsidy and of the current household income y. Let's 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

[8] 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

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

the introduction of the subsidy. Indeed, our results show that higher total household consumption is associated with a greater increase in school enrollment alone, less paid work outside the household, and less concomitant work and schooling after the introduction of the partial subsidy.

24. Well-identified empirical tests of the role of a school subsidy in child work to school transitions remain limited in the literature, partly due to the relative rarity of exogenous variation in subsidy levels for the same population, although the evidence base is growing. This paper compares two cash transfer programs that during a limited period experimentally varied the subsidy for primary and lower secondary school through the randomized introduction of a conditional cash transfer program. The first program, considered in depth, is the *Pantawid* program, which introduced a school subsidy that did not fully cover schooling costs of eligible children. This program is contrasted with several other programs, and in particular the Mexican *Prospera* program, which fully offset all schooling costs.

3. BACKGROUND AND STUDY CONTEXT

25. 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, three percent are idle (i.e. in neither in school nor in work), and two percent are in work only. Boys are more likely to work than girls (15 percent versus 10 percent) and somewhat less likely to be in school (93 percent versus 97 percent). The self-declared reasons for children’s participation in economic activities are varied: a substantial number of children work to help in the household-operated farm or business (53 percent) or to otherwise support family income (20 percent), while a smaller fraction works to gain experience in the labor market (10 percent) or to appreciate the value of work (5 percent). While school attendance rates are comparable between rural and urban areas, rural child employment rates are higher (15 percent versus 8 percent). In rural areas, about 82 percent of children aged 10 to 14 are in school only, 13 percent combine school and work, and three percent each are idle and in work only.

The Program

26. The *Pantawid* program aims to support poor households in satisfying their consumption needs and to encourage investment in their children's education and health. The rollout of the program started in early 2008 with the first enumeration of potential beneficiary households through a listing exercise that collected a number of socio-demographic and household asset indicators in order to construct a Proxy Means Test (PMT) score. Household eligibility was determined on the basis of two criteria: (1) whether the household’s PMT score falls below the province specific poverty threshold, and (2) whether the household includes a pregnant mother and/or at least one child under the age of 14. The first beneficiary households were enrolled and began receiving benefits in the same year, at that point the program was rapidly expanded and currently it covers about 4.5 million households.

27. The program provides both education and health grants. The monthly education grant of 300 Philippine Pesos (roughly US$7)[9] are 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.[10] 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, that children aged 6 to 14 regularly receive deworming treatment,

[9] All amounts related to the Philippines in the remainder of this paper are in 2011 US$.
[10] While the amounts we mention here are monthly, the actual payment is bi-monthly.

and that the household member receiving the cash transfers (or their spouse) regularly attend "Family Development Sessions" organized by the implementing agency, the Department of Social Welfare and Development.[11] The expected average total value of the education and health grants, given family composition, is approximately US$25, equal to about 20 percent of the average beneficiary' monthly household income (see World Bank, 2013). However, the average transfer reported by beneficiaries in our survey data, was substantially lower at approximately US$18.50. This discrepancy is not necessarily surprising, particularly in the early days of the program, as both program officials and households may have still been learning about the program conditions and their implementation.

The evaluation design

28. A village-randomized evaluation was designed by the World Bank in conjunction with the Philippines Department of Social Welfare and Development in order to estimate the effectiveness of the *Pantawid* program and identify areas for program improvement. According to the evaluation design, 130 villages were randomly allocated to a treatment and a control group 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. Village-randomized assignment was conducted in October 2008, data for the PMT were collected in treatment and control villages from October 2008 to January 2009, and in April 2009 eligible households in the treatment villages began receiving transfers.

29. The theoretical framework presented above suggests that the poorest households are the most likely to exhibit the compensatory behavior that results in increases in child labor *and* education in response to a partial schooling subsidy. It is therefore useful to note that the experimental villages represent the poorest rural villages within the poorest municipalities in the country: the per capita income in the 130 evaluation villages was eleven percent lower than the average per capita income in the other program areas from this period (this initial period purposively targeted some of the poorest areas of the country). Moreover, within these villages the eligible households, identified through the proxy means test, had significantly lower baseline mean per capita income than ineligible households (results not displayed).

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[11] 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.

4. DATA AND METHODS

Data

30. We rely on four sources of data collected as part of the *Pantawid* evaluation. Our primary data source is a household survey administered in October and November of 2011, two and a half years after the start of the intervention. The household survey was administered at the end of the Philippines' rainy season / start of the dry season during the middle of the 2011/2012 school year. It comprises a random sample of both eligible and ineligible households in treatment and control communities.[12] The survey covers 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 their current participation in work but also, albeit in less detail, about their participation in work prior to the start of the program for the years 2007, 2008, and 2009. We use this recall data to explore baseline balance in child work as actual baseline data collected did not contain information on children's activities.

31. 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 this data to assess balance of baseline characteristics across treatment and control communities at baseline as well as to construct covariate controls to improve the precision of our primary estimates. Our third data source is a survey administered to village leaders concurrently with the household survey, which includes an assessment of the average daily wage of a male laborer in the village as well as measures of community access to services, such as the distance from the village hall to the nearest public primary and secondary schools. Finally, we use administrative data on the monthly amounts transferred to beneficiary households over the period of the evaluation.

Estimation strategy

32. 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 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 covariates as follows:

[12] The survey was administered in each village to approximately 10 poor households (those with a PMT score below the designated threshold for eligibility) with children aged 0 to 14 and/or a pregnant women (i.e. eligible households), 10 non-poor households with eligible children or pregnant women, 10 poor households without eligible children or pregnant women, and 10 non-poor households without eligible children or pregnant women.

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

33. 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, $\boldsymbol{X_{ivb}}$ is a vector of individual, household, and village-level control and stratification variables (municipalities) measured at baseline (denoted b in the subscript), and ε_{iv} is the error term.

34. Controls and stratification variables are included in the regressions to increase the power of our hypothesis tests; however, results are fully robust to specifications with only municipality fixed effects (presented in Appendix Tables 2-4 and further discussed below). These controls include the interaction of the treatment variable T_v with the vector $(\mathbf{X_{ivb}} - \mu(\mathbf{X_{vb}}))$ to address concerns of regression adjustment laid out in Freedman (2008a & 2008b) and discussed in Lin (2013). The vector $\mu(\mathbf{X_{vb}})$ 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 $T_v {*} (\mathbf{X_{ivb}} - \mu(\mathbf{X_{vb}}))$ 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.

35. The coefficient β_1 estimates the intent to treat effect of the program using OLS. Besides examining whether our results are robust to only using the stratification variables as controls, in a series of appendices we also examine whether the 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).[13] Standard errors are clustered at the village-level in all specifications. Appendix 1 discusses the definition of main outcomes used in the analysis, while Appendix 2 lists all covariates used in the analysis as well as presents balance tests between treatment and control samples for all baseline covariates.

Sample definition

36. This analysis focuses on children aged 10 to 14 as these are, respectively, the youngest children interviewed in the child labor module and the oldest to be eligible for the education grant. Importantly, in the Philippines, children in this age range are not legally allowed to engage in economic activities unless they are carried out "directly under the sole

[13] The fixed effect specification is as follows: $Y_{ivt} = \beta_0 + \beta_1 {*} T_{vt} + d_i + d_{2008} + d_{2009} + d_{2011} + \varepsilon_{ivt}$. 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 which takes the value 1 for treatment villages in 2011 and 0 otherwise, d_i is an individual fixed effect, and d_{2008}, d_{2009}, and d_{2011} are time fixed effects. Information on the intensive margin of work is not available in the recall data, so we cannot establish the robustness of the intensive margin estimates using fixed effects analysis.

responsibility of the child's parents or legal guardian".[14] We further restrict our sample (both in treatment and control villages) to children from households that are below the poverty threshold and therefore eligible to participate in the program. To address concerns around endogenous changes in household composition, we also exclude the 43 children who were neither children nor grandchildren of the household head. Our final sample consists of a total of 1310 children: 654 from treatment villages and 656 from control villages.

37. According to the administrative data, 95 percent (624 of 654) of the children from the treatment villages in the Philippines come from households that actually participated in the cash transfer program. In contrast, none of the children from the control villages belong to households who participated in the cash transfer program. Given the high rate of compliance with treatment assignment, the ITT effects on which we focus are not substantively different from the effect of treatment on the treated. This is illustrated by the estimates, presented in Appendix Tables 2 and 3, of the effect of treatment on the treated estimated using 2SLS regressions where we instrument for household participation in the cash transfer program using village-level assignment to the program.

[14] According to Republic Act No. 7658 as described by the Philippine's National Statistics Office (2014: P. 85). An exception is made for children working in the entertainment industry.

5. RESULTS

Descriptive statistics

38. 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 80 percent attend regularly – but not universal. As the evaluation study sample was drawn from the poorer areas of the Philippines, enrollments rates are lower than the national average. Most children in the 10 to 14 age range attend primary school, although about 20 percent already attend secondary school. It is also common for children to be involved in work: about 20 percent worked in the 12 months before the interview and about 16 percent worked in the 7 days before the interview. Conditional on any work, children work about 30 days a year and about 12 hours a week. Children are as likely to report working for pay outside the household as working without pay inside the household. Relatively low proportions of children report working for pay inside the household or working without pay outside the household. Most of the work carried out by children is unskilled.

39. Most children who work (about 4 in 5) are also in school. There is also a sizeable group of children (about 7 percent) that neither works nor attends school. As we will show later, the cash transfer program had a particularly strong effect on the schooling and labor supply of this latter group of children. In Table 2, we present the marginal effects from a multinomial logit regression of the four mutually exclusive combinations of work and school on the full child population in the control villages in order to explore the relation between household characteristics and these key outcomes.[15] 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 decrease in wealth.[16] 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

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[15] 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. For all main estimates, we interact the gender and age indicator variables with each other however we do not do this in the multinomial logit regression. In the multinomial logit regression we include also the dummy variables for missing controls. We do not display the coefficients for these dummy variables.

[16] 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 1000 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).

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.

Impact of Pantawid on education

40. A key goal of the *Pantawid* program is to improve the school participation of children in beneficiary households. Table 3 presents estimates of the effect of the cash transfer program on the school participation of children aged 10 to 14. Overall attendance increased by 5 percentage points (from 88 percent in the control group, column (1)). Regular attendance, defined as attendance for at least 85% of the days school was in session during the two weeks prior to the interview, increased by 9 percentage points (from 79 percent in the control group, column (4)). The point estimates suggest that the increase in (regular) school attendance 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 7.4 to 8.4 (column (3)). The increase in the number of days children attend school reflects both changes in the extensive margin of school attendance (i.e. changes in the probability of attending school at least one day in the two weeks prior to the interview) and in the intensive margin of school attendance (i.e. changes in the number of days children attended school in the 2 weeks prior to the interview conditional on attending school at least one day).[17] The latter can be inferred from the summary statistics conditional on school attendance at the bottom of the table. We thus conclude that *Pantawid* had significantly increased regular primary school attendance.

Impact of Pantawid on child labor

41. While *Pantawid* did not explicitly target child work, the program may have had an impact on child work through the channels discussed in earlier sections. Table 4 explores these possible impacts.[18] The probability that children aged 10 to 14 reported working in the 12 months before the

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[17] Of course, given that regular school attendance is a program requirement, these findings based on self-reported data need to be interpreted with some care (See, for example, 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 for the purpose of checking compliance with program conditions). It is also possible that, by emphasizing children's education, the program stigmatizes child labor in treated villages, thus leading to a downward bias in our estimates.

[18] 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). As shown in Appendix Table 1e, reported pre-intervention participation in child work is *lower* in the treated communities than in the control communities.

interview increased by nearly 5 percentage points, a substantial increase with respect to the 20 percent participation rate in control villages (this impact estimate is significant at the 10 percent level). Columns (2) to (5) of Table 4 show that the increase in work is due solely to an increase in work for pay outside the household (this 5 percent increase is significant at the 5 percent level). Work without pay, either in or out of the household, and work for pay inside the household are not significantly affected. Accordingly, as shown in columns (6) to (8), children increase only their participation in laboring and unskilled work, while participation in other work, such as farming and fishing, is not significantly affected. Effects on the intensive margin of work (days worked in the past year) and work for pay are positive, but not statistically significant. However, as shown at the bottom of the table, conditional on working, days worked are slightly higher in the treatment group than in the control group. Although we cannot identify working hours separately for children who started working as a result of the program and children who would work also in the absence of the program, the latter suggests that working hours are similar in both of these groups.

42. In Table 5, we examine how the program affected the four mutually exclusive combinations of participation in school and work: in school only, in work only, in school and in work, and neither in school nor in work (Columns (1) to (4) respectively). We also examine combinations of being in work and in primary school and in work and in secondary school (Columns (5) and (6)) and whether, in the past year, the child worked while school was in session (Column (7)). The program decreases the probability of children neither in school nor work by four percentage points and increases the probability of children both working and attending school by six percentage points. The increase in school participation *and* work takes place both at the primary and secondary school level. Corresponding to these changes, the probability that children worked while school was in session increased by five percentage points. Overall these results suggest the most prevalent behavioral shift caused by the program was a transition from being neither enrolled in school nor working to being both in school and in work.

43. If boys and girls face different work opportunities or schooling costs, schooling and work impacts may vary by the gender of the child. However, as shown in Appendix Table 1, the effects of the program do not vary significantly by the gender of the child. We separately estimate the effects on boys and girls by interacting the treatment variable with gender dummies. F-tests do not allow us to reject the null hypothesis that the impact of the program is similar for boys and girls aged 10 to 14 for any of the displayed outcome variables. Both boys and girls increased regular school attendance by about 8 to 9 percentage points (Column (2)). As well, for both boys and girls the likelihood of being engaged in work for pay outside the household increased by about 5 percentage points (Column (4)).

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

44. We now examine the extent to which the results presented above may have been influenced by specification and sample trimming. Specifically, we reconsider the four following choices: First, the inclusion of the controls discussed above; second, the exclusion of 43 children who were neither children nor grandchildren of the household head, to address concerns around endogenous changes in household composition. Third, the use of OLS instead of binary models like Probit or Logit. Finally, the estimation of ITT instead of the TOT to mitigate any biases from selective take up.

45. To address the impact of the decisions to include controls and exclude the 43 children who were not children or grandchildren of the household head, we re-estimate the main results without controls and including the 43 excluded children. Appendix Table 2 shows the effects on school enrollment and attendance, while Appendix Tables 3 and 4 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, while some standard errors are marginally larger, all results are robust in significance and magnitude to the exclusion of any controls except municipality fixed effects, as well as to inclusion of these 43 children.

46. Appendix Tables 2, 3 and 4 further illustrate that the results presented in Tables 3, 4 and 5, respectively, are robust to the following alternative econometric specifications: Probit, and Logit that control only for municipality dummies, and child-level fixed effects using the 2007, 2008, and 2009 recall data as our baseline measurement. The estimated coefficients and associated standard errors change little whether we estimate the impact of the transfer on children's education using OLS, Probit, Logit or child fixed effects regressions while only including municipality dummies as controls. Similarly, the TOT effects, estimated using household treatment assignment to the program as an instrument for participation in the program, are equivalent to the presented intent-to-treat effects.

47. Finally, it is possible that the one year recall period led to noisy measurement of child work, leading to falsely inflated *Pantawid* impact on children's participation in economic activities. Panel B of Appendix Table 3 thus examines whether the estimated increase in work is robust to a seven day recall period instead of a 12 month recall period. For this alternative reference period, participation in work, and unskilled work and work for pay outside the household, all, increase significantly across most specifications, suggesting that our results are not driven by the longer recall period.

Working to support school attendance?

48. *Pantawid* appears to have increased not only school enrollment and participation of children 10-14 years, but also the rate of child work for pay outside the home. As outlined above, compliance with a conditional cash transfer scheme that doesn't cover the full cost of schooling will lead the household to make up this shortfall through other means, including potentially an increase in child labor supply.

49. We now turn to an examination of transfer sizes, schooling costs, and children's earnings; if our hypothesis is correct, the shortfall in schooling costs should be approximately equal to the amount earned by children. We focus on children in primary school, for whom we observe a pronounced increase in (regular) school attendance.[19] Indeed, we show that the average transfer received from *Pantawid* fell short of the average primary education expenditure and that the additional income generated by children covers a substantive share of the increase in outlays for primary education, which is consistent with our hypothesis that the child labor increase is in response to the partial school subsidy. We also find that adult labor supply and household economic activity did not change in response to the program and that expenditures on non-educational items did not fall, thus further confirming that households who wished to enroll children in school as a result of the partial subsidy made up for the gap in schooling costs largely through an increase in child labor and not by other observed behavioral responses. A contrasting example, transfers from Mexico's *Prospera* program substantially exceeded education expenditures and were not accompanied by an increase in child labor.

50. Appendix 3 describes how we estimate the total private costs of education. This expenditure measure averages US$73 for every 10 to 14 year old child enrolled in primary school in control communities and somewhat higher – US$86 – in treatment communities. The difference in school expenditures between control and treatment communities suggests that school attendance is more costly for those who start attending school in response to the program.

51. Thus, total education expenditures appear to be about US$16 above the maximum annual transfer amount per child of approximately US$70, but the difference between the education expenditures and the education transfers *actually received* by beneficiary households according to the administrative data is roughly twice as high. 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

[19] There are 96 children who attend primary school and work, while there are only 45 children who attend secondary school and work. We decided not to examine primary and secondary school children jointly as secondary school expenditures are markedly higher than primary school expenditures, making the comparison of school expenditures with children's income hard to interpret.

find that that households received on average about US$115 in a calendar year if no children attended primary or secondary school, which is roughly equal to the annualized health grant, as well as an additional US$55 for every child in primary school, for an average shortfall of just over US$30 per school attending child in treatment communities.

52. If children work to make up the shortfall in the net cost of education, the income earned by them should represent a substantial share of this shortfall. 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, which is slightly greater than the calculated shortfall of US$30.

53. This comparison is based on reported average schooling costs and earnings. An alternative, and perhaps more appropriate comparison would be between the estimated education costs and income for a child who switches from being idle into school and work. We can call these children "compliers". Due to the lack of individual-level data on baseline enrollment, we cannot identify compliers. However, we can make the assumption that "always takers" in treatment communities exhibit the same schooling costs and earnings as their counterparts in the control communities. If this assumption holds, the observed US$13 difference in education costs for the treatment and control groups would reflect an average education cost of US$195 for compliers. The observed difference in average earnings of US$21 would then correspond to average earnings of US$75 by compliers. The income earned by compliers would hence cover about half of the net cost of education of US$140. Note that this shortfall is likely an upper bound on the true shortfall because always takers may have increased their school expenditure or reduced their participation in work for pay.

54. 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 older siblings (aged 15 to 17) of the 6 to 14 year olds who are eligible for the education transfer. As shown in Panel A of Appendix Table 5, the school participation of these older siblings is not affected, but these children, too, increase their participation in work for pay outside the household, suggesting that older children may work to help offset the education expenditure of their younger siblings. This interpretation is also consistent with the larger increases in school-and-work by 10 to 14 year olds without older siblings than among those with older siblings (Table 6, Panel A), as well as a similar result reported in Barrera-Osorio et al. (2011) for Colombia.

55. Moreover, as we show in Table 6 Panel B, treatment impacts are heterogeneous by the number of enrolled siblings aged 6 to 14. If the lump-sum health grant was used by households to meet some of the 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 is the dilution in the impact of the lump-sum transfer for each child. Children with one or two enrolled siblings are significantly less likely to be in school only, and more likely to be in school and work. We find no significant changes on either education or child labor behavior of children with three or more enrolled siblings, which is consistent with (a) the education grant being provided for up to three children, and (b) a dilution of the ability of the lump-sum transfer to contribute to schooling costs for any one child.

Alternative explanations for the rise in child work

56. This next section explores alternative explanations for the observed increase in child labor. As mentioned above, the income transfer might affect the returns to children's work, inducing households to increase children's labor supply. This may arise if households invest (part of) the transfers in productive assets or if adult household members adjust their labor supply as a result of the transfer program. Table 7 focuses on eligible (poor) households with children aged 10 to 14 and 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 in the 7 days prior to the interview, 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.[20] To do so we follow the estimation strategy outlined in the methodology section, adjusting for baseline characteristics in accordance with the level of the outcome considered (household or adult). We find no evidence that the program affected household-level productive activities or the overall likelihood of adult work (although there is some evidence of substitution out of self-employment and into wagework). We also find that ownership of various farm animals and the profitability of the household farm and non-farm businesses did not change as a result of the program (results not presented, available on request).[21]

[20] For adults no data were collected on work in the past 12 months.

[21] The participation in work and sub-divisions of work carried out by adults, with the exception of work for a private household or enterprise, were not significantly affected either. We examined a range of other indicators for adult work including employment and industry (results not displayed). We find no evidence that the program significantly affected these outcomes. We also separately examined participation in work by youths aged 18 to 24 and find no evidence that the program affected individuals in this age range (results not displayed). The findings presented in Table 7 are in accordance with World Bank (2013), which notes that as a result of the program beneficiary households did not "work less or make less effort to obtain more work". However, they are not

57. Table 8 explores the relative expenditure pattern of treated households with 10-14 year old children. We do not find evidence of significant changes in expenditure patterns with one exception—health expenditure. No other expenditure category shows a statistically significant increase as a result of the program. The point estimate for education expenditures is relatively large, although not precise, which may indicate somewhat of an increase in out-of-pocket spending on education. The lack of a larger increase in education spending indicates the resource constrained situation of these households. Moreover, total household consumption and household savings were not significantly affected.

58. Despite the lack of observed large increases in aggregate spending in the data, the presence of a cash transfer program and subsequent cash infusion in the village economy can lead to general equilibrium effects, as reported for *Prospera* (Angelucci and DeGiorgi, 2009) and *Pantawid* (Filmer et al., 2016). Such effects in turn may lead to increased child labor if the growth in economic activity raised the returns to labor. To test for this, we examine whether the program affected village-level adult male wages (the assumption being that any sector experiencing a demand shock would employ adults as well as children). The result in Column (1) of Table 9 indicates that adult male wages were unchanged after program introduction. We also explored possible changes in economic activity for non-poor households, i.e. households with a PMT score above the poverty line, with young children. The results of this analysis are displayed in columns (2) to (8) of Table 9. All of the estimates are small in magnitude and none is statistically significant, indicating that the program did not result in large enough changes in the local economy to raise the child wage and draw more children into the labor force from these households. Finally, we also examined the effects of the program on 10-17 year old children from ineligible households from the same areas to see whether similar trends were observed. Panel B of Appendix Table 5 shows that the program does not affect the school participation or participation in work among ineligible children, thus supporting our hypothesis that *Pantawid* drew additional into concomitant school and work.

59. Another potential mechanism concerns households responding to the cash transfer program by altering the household composition. The additional income available to the household, for instance, may induce increased fertility or may encourage other relatives to move into the household. Such changes in household composition can in turn affect children's participation in work for several reasons. For instance, as women spend more time with young children, older children may have to take over some activities. Or, perhaps, if the household takes on an increased number of dependents in order to maximize the transfer amount, older children may be forced to partially compensate for the additional costs of a larger

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entirely identical to those presented in World Bank (2013), as we restrict our sample to households with children aged 10 to 14.

household through increase work. We deem such effects unlikely, as benefits are paid only for children living in the household at the time of program enrollment (the survey period for the proxy means test). Indeed, as shown in Table 10, we find no evidence that household composition altered in response to the cash transfer program. The total number of individuals in the household, the number of children aged 0 to 5, the number of children aged 6 to 14, and the number of children aged 15 to 17 is not significantly different across treatment and control groups.

60. Kandpal et al. (2016) finds that *Pantawid* helps to keep children healthy, one of the stated aims of the program. The strongest evidence for this concerns children up to the age of 5 for whom extensive health data, including anthropometric indicators, were collected. In treatment villages, children in this age range were less likely to be stunted, more likely to eat protein rich food, and more likely to receive preventative health services. Moreover, household expenditure on protein rich foods increased substantially. Although we have no direct evidence of beneficial impacts on the health of 10-14 year olds, due to the lack of child-specific health information for those in this age-range, it is possible that physical health improved for such children. Improved physical health can theoretically lead to increased child labor supply (as well as increased school attendance and school performance – it is an open question whether healthier children are more likely to work or to go to school). Unfortunately we cannot directly investigate the influence of this mechanism in the Philippine data due to the limited information. However, as we discuss below, similar programs in other contexts, such as *Prospera* in Mexico, also included conditions on child health in their design and led to improvements in child health yet did not increase children's participation in work.

61. Children who attend school may also be better informed about labor market opportunities, and thus be more likely to work. To rule out better information about labor market opportunities from going to school as the driving mechanism, we would need to examine networks of children in which some work and some do not. While we do not have the data to carry out this type of network analysis, the villages in our sample are small, with an average of 215 households, and learning about labor market opportunities through school attendance appears unlikely. Any information effects are thus likely to be secondary to the effect of economic need. Moreover, since we do not expect the Philippines to be unique in this regard, were information a likely channel, we would also expect to see the increased schooling and work-for-pay effect in some of the other countries that run similar programs, but as we will discuss below, this is not the case.

62. Finally, the model assumes that households view the education transfer as a partial subsidy for regular school attendance, and not as an unconditional transfer. This of course is how the program was described to recipient households at program onset. While we do not have data on the enforcement of schooling conditions and cannot verify whether they were

enforced, the fact that the actual amounts transferred to our sample were significantly smaller than the maximum potential transfers suggests that conditions were in fact enforced. Indeed, the program was designed and publicized to be conditional, and particularly during the pilot stage— the period analyzed here— program beneficiaries may not have known with certainty whether conditions would be enforced. Moreover, as Benhassine et al. (2015) show, even a nudge or an unenforced condition can be enough to induce beneficiaries to comply.

The impact of more generous education subsidies

63. The evidence presented above suggests that the increase in child work is the result of a partial grant for the full cost of education. The observed increase in children's participation in work for pay contrasts with studies of other cash transfer programs (for an overview see de Hoop and Rosati, 2014). As summarized in Appendix Table 6, a review of the literature on conditional cash transfer programs and education costs shows that in virtually all of the programs studied, the transfer amount exceeded the full cost of education. In addition, all the studies reviewed in Appendix Table 6 document either a significant decrease or no change in child labor as a result of the transfer.

64. To further investigate the hypothesis that *Pantawid* increases child schooling and work-for-pay because it only partially subsidizes education costs, we compare the effects of the *Pantawid* to those of *Prospera* in Mexico when it was first implemented in the late 1990s (Parker, Rubalcava, and Teruel, 2008). We believe that the comparison with *Prospera* is particularly insightful for three reasons. First, the rural target populations of the two programs were quite similar in terms of children's school attendance and participation in child labor. Data from the 1996 ENIGH present a similar picture in Mexico at the onset of *Prospera*: nationwide, about 84 percent of children aged 12 to 14 were in school and while 15 percent were engaged in economic activities. 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. As in the Philippines, boys were almost twice as likely to be engaged in economic activities as girls, and participation in work appears to have been particularly common in rural areas (over 53% of reported child economic activity was in agriculture).

65. Second, many other cash transfer programs, including *Pantawid*, were modeled after *Prospera*. Like the *Pantawid* pilot, *Prospera* provided conditional education and health/nutrition grants to poor households. The experimental phase of *Prospera*, analyzed here, was targeted at the poorest communities in the country and household poverty was established by means of a proxy means test established based on survey data (World Bank, 2014). Education grants were provided for up to 3 children up to the age of 18 conditional on 85 percent school attendance. The health/nutrition grants

were provided conditional on regular participation in health clinics, regular attendance of health talks, prenatal visits by pregnant women, and the use of nutritional supplements by pregnant and lactating women. The total value of the transfers equaled about 20 percent of average beneficiary household income (Barber and Gertler, 2008; Skoufias and Parker, 2001).

66. However, as we establish here, in contrast to *Pantawid*, the education grants from *Prospera* fully covered the cost of schooling. Further, the level of the education grant 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), but the size of the transfer depended on the grade attended by the child. In 1998, education grants ranged from 70 Mexican pesos (US$7.65 in 1998 dollars) for male and female students in grade 3 of primary school to 225 Mexican pesos (US$24.62) for male students in grade 3 (the final grade) of lower secondary school and 255 Mexican pesos (US$27.90) for female students. Interestingly, to our knowledge, no study compares the *Prospera* education grants to the *direct* cost of education. Appendix 3 describes how we estimate this full cost for our sample of Mexican beneficiary households.

67. 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). The early treatment group started receiving the program in the beginning of 1998. The late treatment group started receiving the program at the end of 1999, with take up rates of approximately 97 percent (Ozer et al., 2011). Hence, the TOT effect for *Prospera* are likely similar to the ITT effects presented below. We rely on three sources of information collected as part of the evaluation to establish the effects of *Prospera*: a baseline household survey administered in November of 1997 (known under the acronym ENCASEH), a follow-up household survey administered in November of 1999 (known under the acronym ENCEL), and 1999 locality level information collected from local officials. We can construct outcome and control variables that are broadly similar to those we used to examine the effects of *Pantawid* based on this data. These outcome and control variable definitions are described in Appendices 1 and 2.

68. Replicating the estimation procedure we use for the Philippines cash transfer program as closely as possible, we estimate the effects of *Prospera* on children's schooling and work based on regression specification (1). Table 11 presents our estimates of the effects of *Prospera* on participation in school and work by children aged 10 to 14. These estimates are consistent with those presented in Ranzani and Rosati (2013), Rubio-Codina (2010), Schultz (2004), and Skoufias and Parker (2001). Like the

Pantawid cash transfer program, *Prospera* increased school attendance and regular school attendance, defined as not having missed school for a day over the two weeks prior to the interview, respectively by about six percentage points (Columns (1) and (2)). However, in contrast with the *Pantawid* program, *Prospera* reduced the probability of children working for pay by about one percentage point (Columns (3) and (4)). Most of this impact seems to have been on children who would have been in neither school nor work without the transfer from *Prospera* (Columns (5) to (8)): the probability that children are neither in school nor in work decreases by about 4.5 percentage points, while the probability that children are in school increases by 5.2 percentage points.

69. Various other studies show that *Prospera* resulted in effects that, at least in theory, can lead to increased children's participation in work: it increased household investment in productive activities (Gertler, Martinez, and Rubio-Codina, 2012), resulted in economic benefits for ineligible households (Angelucci and De Giorgi, 2009), and improved children's health (see among others Gertler, 2004; Rivera et al. 2004). The fact that *Prospera* nonetheless *lowered* children's participation in work is consistent with the hypothesis that the relative value of the education subsidy compared to the cost of schooling plays a dominant role in the beneficiary household’s decision making around child labor. The alternative mechanism that the *Pantawid* related analysis above could not directly address involves the possible link between improvements in child health and child work incidence. The finding from *Prospera* related to health – child health improved but labor participation actually fell – 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.

6. DISCUSSION AND CONCLUSIONS

70. This paper illustrates how a partial subsidy for a socially desirable good can elicit unanticipated compensatory behavior from complier households. We use data from a Philippine cash transfer program that partially subsidized schooling to provide evidence of such compensatory behavior in the form of concomitant increases in schooling and participation in paid work by the same children. This result stands in contrast with most other cash transfers, including the Mexican *Progresa*, which increased schooling while decreased paid work by children. We rule out potential alternative explanations for the increase in child labor by showing that the *Pantawid* program did not increase total labor supply of adult household members nor did it increase household investment in productive assets, which could in turn have increased household productivity; moreover, non-education expenditures did not decrease or adjust once child enrollment increased as a result of the program. The only margin of the household budget constraint to have changed is the supply of child labor.

71. The program effects we estimate are sizeable in magnitude. We find that the Philippine CCT increased children's participation in work for pay outside the household increased by about 5 percentage points, relative to a control group mean of 20 percent. We find that most of this effect appears to have been driven by children who would otherwise be neither in school nor in work but start attending school *and* start working. We present evidence that children's school participation and work was complementary in the face of *Pantawid's* partial schooling subsidy to poor, labor-constrained households. School attendance of marginal children represented a net cost to the household, and these children began working for pay to make up a substantial share of this cost.

72. This observed behavioral response to *Pantawid* subsidy level is also consistent with a theoretical framework that frames child labor as a complement to school participation when the subsidy does not cover the full cost of schooling (but is high enough to make part time child work a useful supplementary strategy). This view of child work as complementary to school enrollments runs counter to most theoretical treatment of child labor, which presents schooling and work as strict substitutes, but we argue arises in the conditions studied here. In particular, our findings speak to the luxury axiom to the child labor model presented in Basu and Van (1998). The luxury axiom 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 extensive and intensive margins, complementarity can arise in the presence of a partial education subsidy.

73. These findings have important ramifications for the policy maker's choice of transfer amount in a conditional cash transfer program. A transfer amount set too high may transfer substantial resources to households who would be compliant with the conditions even without the transfer (so called inframarginal households) and thus raises the question of the spending

efficiency of high transfer values. In addition to the results from *Pantawid*, we review other cash transfer programs studied in the literature to note that these programs all fully offset schooling costs. On the other hand a lower transfer amount may not induce all eligible population to take up the preferred behavior or be too low to induce households to adopt compensatory activities such as an increase in child labor. Therefore the question of an efficient subsidy level is one that involves a consideration of program costs, 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 of course stand the anticipated benefits of increased school participation and human capital.

74. While a full accounting of costs and benefits is beyond the scope of this paper, a back-of-the-envelope calculation of the increase in program costs from increasing the *Pantawid* education grant to a full schooling subsidy can illustrate these tradeoffs. We estimated 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 and \$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 (from \$45,780 to \$55,590 if the costs were \$85, and from \$45,780 to \$127,530 if the costs were \$195) in the outlay for the education grant. During the pilot phase, education grants comprised approximately half of the potential transfer (\$12.50 of a maximum of \$25), 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 have thus translated to a 9.5 percent increase in the overall 2015 program budget of US\$ 1.3 billion, while a 179 percent rise in outlays for education grants would have translated to a 80.5 percent budget increase. Without knowing the nature of the work done by children, we cannot estimate welfare effects, but note that eliminating the modest increase in child labor reported by this paper would have come at a substantial increase in program costs.

75. We close by noting that the broader issue of compensatory behaviors arising from partial subsidies merits further consideration. While our framework predicts that transfers smaller than the cost of education can lead to compensating behavior such as increases in child labor, this need not always be the case as impacts of partial subsidies may depend on a variety of factors. For instance, if rigidities in minimum working hours and school attendance requirements make it difficult for children to combine work-for-pay and school (see for instance Edmonds and Schady, 2011), children may not combine work and school even in the presence of a partial subsidy. Similarly, we do not argue that *Pantawid* resulted in a nationwide increase in child labor. The experimental communities in our dataset represent the poorest communities in the country. Since poverty is a key factor of the proposed mechanism, we are not likely to observe similar

changes in wealthier communities phased into the program during scale up. Indeed, regression discontinuity estimates from both pilot and scale up phases of the program, comparing households that were just poor enough to enter the program to those that were just excluded from the program, find no effect on the incidence of child labor and in fact report a decrease in days worked in the past year (World Bank, 2014; World Bank 2016).

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APPENDIX 1: DEFINITION OF OUTCOME MEASURES USED IN THE ANALYSIS

Outcomes as defined for the Philippines data

76. 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).

77. 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.

78. 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. 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 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. The 7-day recall for child work is included for robustness purposes.

79. 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

80. 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 2: Covariate definition and balance of baseline characteristics

Balance of baseline characteristics and other covariates, as well as variable definitions, used in the Philippine data

81. Appendix Tables 7a to 7e present balance tests assessing the validity of the village-level randomized assignment of *Pantawid* across the individual, household, and community characteristics that we include as controls in our regressions. Most of these characteristics were assessed in the baseline Proxy Means Score survey. 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.

82. Appendix Table 7a examines whether the stratification variable – the municipality – is balanced by regressing treatment assignment on each of the 8 municipalities in which the experiment was implemented.

83. Appendix Table 7b 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. The child-level controls are based on 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 available quantitative or qualitative evidence). 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.

84. Appendix Table 7c examines the balance of the household level measures: a wealth index, whether the household head is Muslim, whether the household belongs to an indigenous ethnic group, whether the household head ever attended school, whether the household is engaged in agricultural activities, and household size and demographic composition (number of members aged 0 to 5, 6 to 14, and 15 to 17). 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 Score survey. The wealth index is defined as 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.

85. Appendix Table 7d 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.

86. Finally, Appendix Table 7e explores the balance of the child labor recall data measures across treatment and control villages, separately for the years 2007, 2008, and 2009. Across all of these balance tests in the five tables, 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.

Covariates definition and baseline balance in the Mexican data

87. For the analysis of child work and schooling in the Mexican data, we include the following individual characteristics as controls: age, gender, and an indicator variable taking the value 1 if neither of the child's parents live in the household. We include the following household level characteristics: a wealth index (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 include the following locality level characteristics: whether there is a 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.

88. Following the procedure described above, we examined the balance of these characteristics and find 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 3: ESTIMATES OF SCHOOLING COSTS

89. 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.

90. 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 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. Based on these assumptions and restrictions, the estimated annual household expenditure on each child’s educations averages US$73 for every 10 to 14 year old child enrolled in primary school in control communities and just over US$86 in treatment communities.

91. 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 (results not displayed).

92. 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 restricted 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.

FIGURES AND TABLES

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

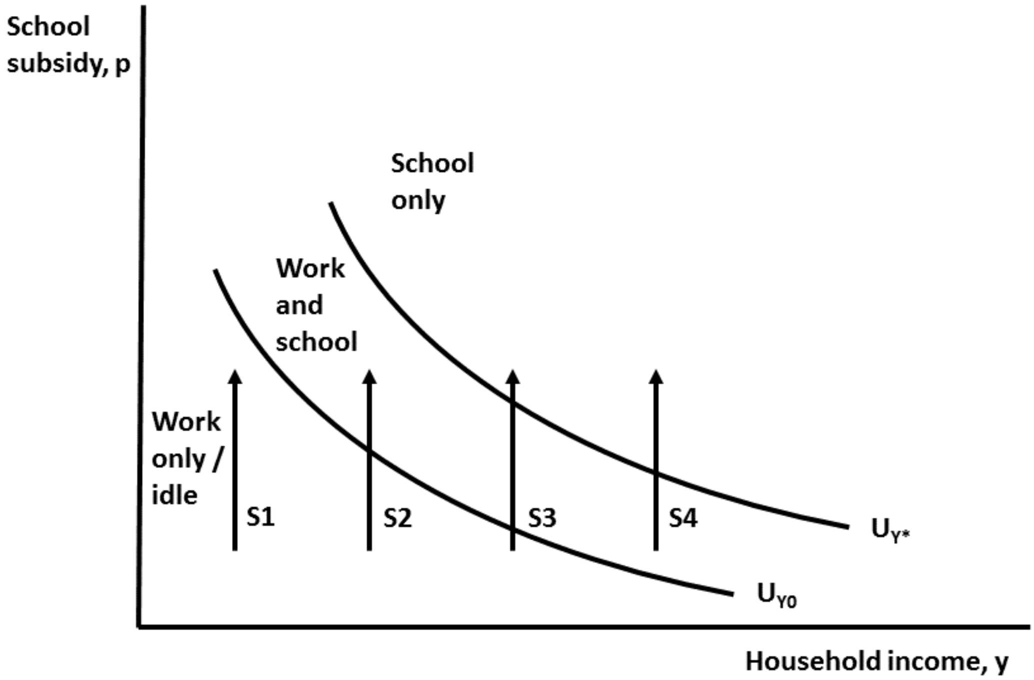

Table 1. Descriptive statistics: mean values for children from *Pantawid* control communities	
Extensive margin:	
Attends	0.882
Attends primary school	0.651
Attends secondary school	0.231
Attends regularly	0.793
Attends primary school regularly	0.579
Attends secondary school regularly	0.215
Worked in 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. Determinants of mutually exclusive combinations of work and school for children from *Pantawid* control communities

	In school only	In work only	In school and in work	Neither in school nor in work
Child aged 10	0.479***	-0.386***	-0.062	-0.032
	(0.053)	(0.065)	(0.038)	(0.026)
Child aged 11	0.162***	-0.045***	-0.073***	-0.045**
	(0.031)	(0.015)	(0.028)	(0.019)
Child aged 12	0.081***	-0.036***	-0.011	-0.033*
	(0.028)	(0.013)	(0.022)	(0.020)
Child aged 13	0.088***	-0.031**	-0.010	-0.047**
	(0.029)	(0.014)	(0.024)	(0.021)
Male	-0.094***	0.022**	0.048**	0.024
	(0.022)	(0.010)	(0.022)	(0.016)
Neither biological mother nor biological father lives in the household	0.033	-0.008	-0.064	0.038
	(0.048)	(0.028)	(0.051)	(0.029)
Wealth index	0.033***	-0.011**	-0.011*	-0.010
	(0.009)	(0.005)	(0.006)	(0.007)
Distance to nearest public primary school from town hall > 2 Km	-0.139**	0.044	0.027	0.068**
	(0.065)	(0.035)	(0.065)	(0.027)
Distance to nearest public secondary school from town hall > 2 Km	0.022	0.018	-0.013	-0.027
	(0.035)	(0.015)	(0.028)	(0.027)
Household head is muslim	0.191***	-0.047	-0.184**	0.040
	(0.072)	(0.033)	(0.079)	(0.039)
Household belongs to indigenous people group	0.025	-0.012	0.015	-0.028
	(0.044)	(0.012)	(0.046)	(0.020)
Household head never attended school	-0.090	0.022	-0.013	0.080***
	(0.057)	(0.020)	(0.050)	(0.029)
Household engaged in agricultural activities	-0.087***	0.024**	0.050*	0.014
	(0.030)	(0.012)	(0.026)	(0.020)
Total number of household members	-0.004	0.007*	-0.003	-0.001
	(0.011)	(0.004)	(0.009)	(0.006)
Number of children aged 0 to 5	-0.017	0.017**	-0.013	0.013
	(0.031)	(0.008)	(0.026)	(0.014)
Number of children aged 6 to 14	0.000	-0.002	-0.001	0.002
	(0.015)	(0.004)	(0.014)	(0.009)
Number of children aged 15 to 17	0.020	-0.017*	-0.018	0.015
	(0.027)	(0.010)	(0.020)	(0.017)
Additional information:				
Number of observations	972			

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

Table 3. *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 with controls:	0.053***	0.032	0.018	0.092***	0.064***	0.027	1.046***
	(0.018)	(0.022)	(0.018)	(0.023)	(0.024)	(0.019)	(0.244)
Additional information:							
Number of observations	1,308	1,308	1,308	1,276	1,276	1,276	1,296
Observations in control group	654	654	654	634	634	634	649
Observations in treatment group	654	654	654	642	642	642	647
Mean in control group	0.882	0.651	0.231	0.793	0.579	0.215	7.435
Mean in treatment group	0.922	0.688	0.231	0.879	0.651	0.226	8.393
Conditional mean in control group							8.631
Conditional mean in treatment group							9.141

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

		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)
OLS with controls:	0.045*	0.048**	-0.001	-0.002	0.008	0.047*	-0.002	0.006	1.975	1.638
	(0.027)	(0.020)	(0.010)	(0.010)	(0.022)	(0.025)	(0.016)	(0.006)	(1.742)	(1.239)
Additional information:										
Number of observations	1,223	1,223	1,223	1,224	1,224	1,223	1,223	1,223	1,220	1,223
Observations in control group	604	604	604	605	605	604	604	604	602	604
Observations in treatment group	619	619	619	619	619	619	619	619	618	619
Mean in control group	0.202	0.091	0.031	0.040	0.091	0.144	0.079	0.008	5.919	2.225
Mean in treatment group	0.239	0.136	0.031	0.029	0.103	0.184	0.078	0.013	7.979	4.174
Conditional mean in control group									29.692	24.436
Conditional mean in treatment group									33.544	30.762

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 5. *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	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)
OLS with controls:	-0.007	-0.014	0.059**	-0.039***	0.031*	0.028**	0.045**
	(0.029)	(0.011)	(0.026)	(0.015)	(0.019)	(0.013)	(0.021)
Additional information:							
Number of observations	1,223	1,223	1,223	1,223	1,223	1,223	1,223
Observations in control group	604	604	604	604	604	604	604
Observations in treatment group	619	619	619	619	619	619	619
Mean in control group	0.725	0.038	0.164	0.073	0.114	0.050	0.094
Mean in treatment group	0.719	0.027	0.212	0.042	0.142	0.069	0.136

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 6. 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	-0.079	0.033	-0.001	-0.039	0.007	0.025	0.007
	(0.063)	(0.059)	(0.047)	(0.064)	(0.026)	(0.056)	(0.043)
One or more siblings aged 15-17	0.237***	0.079	0.109***	-0.001	-0.025	0.105**	-0.079**
	(0.058)	(0.049)	(0.040)	(0.059)	(0.026)	(0.052)	(0.037)
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	1,225	1,174	1,174	1,174	1,174	1,174	1,174
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	-0.005	-0.007	-0.120	0.020	-0.092	0.084	-0.012
	(0.128)	(0.151)	(0.109)	(0.155)	(0.123)	(0.117)	(0.150)
One or two enrolled siblings aged 6-14	0.065	0.082**	0.072**	-0.065*	-0.002	0.083**	-0.016
	(0.041)	(0.035)	(0.029)	(0.038)	(0.022)	(0.034)	(0.023)
Three or more enrolled siblings aged 6-14	0.049	0.022	0.048	-0.016	0.010	0.011	-0.006
	(0.041)	(0.047)	(0.032)	(0.051)	(0.017)	(0.049)	(0.023)
Number of observations:							
P-value F-test (impact 0 siblings = impact 1 or 2 siblings)	0.573	0.564	0.079	0.585	0.470	0.993	0.979
P-value F-test (impact 0 siblings = impact 3+ siblings)	0.708	0.852	0.160	0.831	0.428	0.575	0.968
P-value F-test (impact 1 or 2 siblings = 3+ impact siblings)	0.820	0.339	0.617	0.483	0.731	0.268	0.800
Number of observations	1,225	1,174	1,174	1,174	1,174	1,174	1,174
Mean in control group, no enrolled siblings	0.244	0.286	0.214	0.214	0.262	0.024	0.500
Mean in treatment group, enrolled siblings	0.250	0.250	0.083	0.250	0.167	0.083	0.500
Mean in control group, one or two enrolled siblings	0.804	0.195	0.084	0.748	0.033	0.162	0.057
Mean in treatment group, one or two enrolled siblings	0.873	0.232	0.135	0.715	0.031	0.201	0.053
Mean in control group, three or more enrolled siblings	0.880	0.197	0.079	0.786	0.004	0.192	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. Included controls in the OLS estimates for work in the past 7 days are listed in section 3.1. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table 7. *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 with controls:	0.039	0.001	0.006	0.024	0.049**	0.006	-0.030
	(0.029)	(0.019)	(0.028)	(0.020)	(0.024)	(0.008)	(0.024)
Additional information:							
Number of observations	849	849	847	2,428	2,428	2,428	2,428
Observations in control group	435	435	435	1,244	1,244	1,244	1,244
Observations in treatment group	414	414	412	1,184	1,184	1,184	1,184
Mean in control group	0.637	0.083	0.117	0.621	0.281	0.026	0.295
Mean in treatment group	0.667	0.077	0.124	0.628	0.321	0.030	0.262

Note. Estimates of program impact on household and adult level economic activities. Sample restricted to eligible households with children aged 10-14. Household level estimates include only village and household level controls, except the indicator for whether there are any adults working in agriculture. Adult level estimates include all village, household and individual controls, except the dummies for whether the individual's biological mother and father live in the same household. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table 8. *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 with controls:	-0.044	-0.071	0.429**	0.250	-0.077	0.237
	(0.052)	(0.054)	(0.197)	(0.205)	(0.089)	(0.206)
Additional information:						
Number of observations	873	873	870	870	873	861
Observations in control group	447	447	447	446	447	439
Observations in treatment group	426	426	423	424	426	422
Mean in control group	9.370	8.927	2.970	4.485	1.129	-0.641
Mean in treatment group	9.344	8.855	3.127	4.613	0.990	-0.579

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

Table 9. *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 with controls:	9.306	0.022	-0.011	-0.028	-0.001	0.009	0.003	-0.017
	(6.422)	(0.028)	(0.015)	(0.022)	(0.012)	(0.013)	(0.007)	(0.017)
Additional information:								
Number of observations	127	2,323	2,322	2,323	6,603	6,603	6,603	6,603
Observations in control group	62	1,182	1,180	1,181	3,388	3,388	3,388	3,388
Observations in treatment group	65	1,141	1,142	1,142	3,215	3,215	3,215	3,215
Mean in control group	142	0.615	0.131	0.110	0.594	0.228	0.055	0.300
Mean in treatment group	150	0.648	0.114	0.074	0.586	0.231	0.057	0.285

Note. Estimates of program impact on household and adult level economic activities in ineligible households. Village level controls include only municipality dummies. Household level estimates include all village and household level controls. Adult level estimates include all village, household, and individual level controls, except the dummies for whether the individual's biological mother and father live in the same household. Standard errors are clustered at the village level *** p<0.01, ** p<0.05, * p<0.1

Table 10. *Pantawid* program impact on beneficiary household composition

	Total number of individuals in the household	Number of children aged 0 to 5	Number of children aged 6 to 14	Number of children aged 15 to 17
	(1)	(2)	(3)	(4)
OLS with controls:	0.095	0.022	0.026	0.027
	(0.103)	(0.049)	(0.044)	(0.034)
Additional information:				
Number of observations	850	850	850	850
Observations in control group	435	435	435	435
Observations in treatment group	415	415	415	415
Mean in control group	6.763	0.747	2.276	0.664
Mean in treatment group	6.928	0.790	2.434	0.677

Note. Estimates of program impact on composition of eligible households. Household level estimates include all village and household level controls. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Table 11. *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 with controls:	0.059***	0.057***	-0.007	-0.011***	0.052***	-0.013***	0.006	-0.045***
	(0.011)	(0.012)	(0.007)	(0.004)	(0.011)	(0.004)	(0.005)	(0.009)
Additional information:								
Number of observations	10,821	10,805	10,883	10,875	10,771	10,771	10,771	10,771
Observations in control group, boys	4,142	4,135	4,177	4,176	4,121	4,121	4,121	4,121
Observations in treatment group, boys	6,679	6,670	6,706	6,699	6,650	6,650	6,650	6,650
Mean in control group, boys	0.841	0.805	0.049	0.028	0.828	0.034	0.014	0.124
Mean in treatment group, boys	0.900	0.863	0.041	0.018	0.881	0.021	0.019	0.079

Note. Estimates of *Prospera* program impact on education and work outcomes of children aged 10 to 14 from eligible households. Included controls are listed in Appendix 1. Standard errors are clustered at the level of localities. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX TABLES

Appendix Table 1. 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	0.035	0.081**	0.045	0.050*
	(0.029)	(0.034)	(0.039)	(0.029)
Impact on girls	0.048**	0.093***	0.033	0.044*
	(0.021)	(0.027)	(0.035)	(0.027)
Additional information:				
P-value F-test (impact boys = impact girls)	0.697	0.762	0.803	0.862
Number of observations	1,308	1,276	1,223	1,223
Mean in control group, boys	0.859	0.751	0.253	0.123
Mean in treatment group, boys	0.891	0.830	0.295	0.170
Mean in control group, girls	0.909	0.842	0.143	0.054
Mean in treatment group, girls	0.957	0.933	0.176	0.097

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 2. *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 controlling only for municipality:	0.043**	0.035	0.006	0.092***	0.072**	0.019	0.947***
	(0.019)	(0.026)	(0.023)	(0.025)	(0.029)	(0.024)	(0.255)
Probit controlling only for municipality:	0.044**	0.035	0.006	0.092***	0.072**	0.019	
	(0.019)	(0.026)	(0.023)	(0.025)	(0.029)	(0.024)	
Logit controlling only for municipality:	0.043**	0.035	0.006	0.091***	0.073**	0.019	
	(0.018)	(0.026)	(0.023)	(0.025)	(0.029)	(0.024)	
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 3. *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	Days worked for pay, outside own household
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Work in the past 12 months										
OLS controlling only for municipality:	0.040	0.051**	-0.003	-0.007	0.011	0.046*	-0.003	0.004	1.954	1.838
	(0.029)	(0.021)	(0.010)	(0.010)	(0.021)	(0.026)	(0.015)	(0.007)	(1.763)	(1.392)
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	0.044**	-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)		
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										
OLS with controls:	0.055**	0.027*	-0.003	0.002	0.024	0.051**	-0.004	0.003	0.040	0.174
	(0.025)	(0.015)	(0.008)	(0.008)	(0.018)	(0.022)	(0.014)	(0.002)	(0.366)	(0.194)
Additional information, work in the past 7 days:										
Number of observations	1,222	1,225	1,225	1,222	1,222	1,222	1,222	1,222	1,215	1,223
Observations in control group	603	606	606	603	603	603	603	603	599	605
Observations in treatment group	619	619	619	619	619	619	619	619	616	618
OLS controlling only for municipality:	0.048*	0.025	-0.001	-0.006	0.025	0.049**	-0.006	0.003	0.073	0.249
	(0.027)	(0.016)	(0.008)	(0.010)	(0.019)	(0.022)	(0.014)	(0.002)	(0.413)	(0.217)
Probit controlling only for municipality:	0.047*	0.021	-0.001	-0.005	0.022	0.047**	-0.005			
	(0.027)	(0.014)	(0.006)	(0.008)	(0.017)	(0.021)	(0.012)			
Logit controlling only for municipality:	0.047*	0.021	-0.001	-0.005	0.020	0.044**	-0.007			
	(0.026)	(0.013)	(0.006)	(0.008)	(0.015)	(0.020)	(0.013)			
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	1,264	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	0.108	0.116	0.091	0.032	0.144	0.078	0.008	5.906	2.851
Mean in treatment group	0.242	0.122	0.155	0.141	0.030	0.188	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 4. *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)
OLS controlling only for municipality:	-0.006	-0.009	0.050*	-0.034**	0.029	0.020	0.043*
	(0.031)	(0.011)	(0.027)	(0.015)	(0.020)	(0.013)	(0.022)
Probit controlling only for municipality:	-0.005	-0.008	0.048*	-0.033**	0.027	0.016	0.041*
	(0.033)	(0.010)	(0.028)	(0.014)	(0.019)	(0.011)	(0.021)
Logit controlling only for municipality:	-0.007	-0.008	0.050*	-0.032**	0.027	0.014	0.039*
	(0.034)	(0.009)	(0.027)	(0.014)	(0.018)	(0.009)	(0.020)
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 5. 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 with controls:	-0.027	0.029	0.099**	0.105***
	(0.040)	(0.041)	(0.039)	(0.032)
Additional information:				
Number of observations	691	672	572	572
Observations in control group	353	338	285	285
Observations in treatment group	338	334	287	287
Mean in control group	0.629	0.556	0.337	0.193
Mean in treatment group	0.580	0.566	0.432	0.286
Panel B: Effects on children (10-17) from ineligible households				
OLS with controls:	0.008	0.015	0.033	-0.001
	(0.019)	(0.026)	(0.024)	(0.010)
Additional information:				
Number of observations	1,145	1,107	1,036	1,036
Observations in control group	598	569	545	545
Observations in treatment group	547	538	491	491
Mean in control group	0.868	0.826	0.218	0.119
Mean in treatment group	0.856	0.822	0.228	0.120

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 6: Summary of the Literature on Conditional Cash Transfer Programs and Education Costs

Country	Reference(s)	Impact on Child Labor	Subsidy Relative to Schooling Costs	Notes
Brazil	Ferro et al. (2010)	Negative	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)	Negative	Full subsidy	Noted on page 24.
Colombia	Barrera-Osorio et al. (2011)	Negative on students in grades 6-10, no effect on those in grade 11	Full subsidy	Noted on page 171.
Ecuador	Edmonds and Schady (2011)	Negative	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)	No effect (Glewwe and Olinto); negative (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)	Negative	Full subsidy through junior secondary; marginally lower than senior secondary costs	Noted on page 105.
Jamaica	Levy and Ohls (2007)	No effect	Full subsidy	Noted on page 7
Mexico	Skoufias and Parker (2001); Schultz (2004); Rubio-Codina (2010)	Negative	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.

Appendix Table 7a. Balance tests of households across *Pantawid* municipalities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS without controls:	0.011	0.024	0.008	-0.004	-0.020	-0.008	-0.002	-0.010
	(0.049)	(0.075)	(0.066)	(0.068)	(0.055)	(0.055)	(0.055)	(0.051)
Additional information:								
Number of observations	850	850	850	850	850	850	850	850
Observations in control group	435	435	435	435	435	435	435	435
Observations in treatment group	415	415	415	415	415	415	415	415
Mean in control group	0.074	0.207	0.172	0.177	0.094	0.085	0.099	0.092
Mean in treatment group	0.084	0.231	0.181	0.173	0.075	0.077	0.096	0.082

Note. Balance of households across the 8 municipalities. Estimates based on OLS regressions without controls. Sample restricted to eligible households with children aged 10 to 14. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 7b. Balance of child characteristics in *Pantawid* data

	Age	Male	Neither father nor mother lives in household
	(1)	(2)	(3)
OLS without controls:	0.005	0.006	0.012
	(0.064)	(0.028)	(0.011)
Additional information:			
Number of observations	1,310	1,310	1,310
Observations in control group	656	656	656
Observations in treatment group	654	654	654
Mean in control group	11.997	0.529	0.029
Mean in treatment group	12.002	0.535	0.041

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 7c. 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
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
OLS without controls:	-0.167	0.069	-0.009	0.018	0.001	0.021	0.027	0.117	-0.034
	(0.122)	(0.054)	(0.055)	(0.028)	(0.049)	(0.151)	(0.042)	(0.081)	(0.053)
Additional information:									
Number of observations	811	850	850	806	811	811	811	811	811
Observations in control group	413	435	435	410	413	413	413	413	413
Observations in treatment group	398	415	415	396	398	398	398	398	398
Mean in control group	-0.756	0.076	0.166	0.080	0.705	6.499	0.387	2.240	0.700
Mean in treatment group	-0.922	0.145	0.157	0.098	0.706	6.520	0.415	2.357	0.666

Note. Estimated differences in household covariates across treatment and control villages. Estimates based on OLS regressions without controls. Sample restricted to eligible households with children aged 10 to 14. 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 7d. 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 7e. 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