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

This paper concerns the household's response to an offered schooling subsidy that does not fully cover the cost of schooling. Possible responses include an asset drawdown, an increase in adult labor supply, a shift in consumption patterns, or an increase in child labor. For the poorest households, such as those studied here, an increase in child labor may be the only available margin as they lack significant assets, are credit-constrained with consumption patterns at or near subsistence levels, and (we assume) adults are not able to supply additional labor after the subsidy.

[xxii] Therefore, if the shortfall in education costs after the introduction of the partial schooling subsidy must be met through shifts in household labor, it is the children who were not working prior to the subsidy that will supply this labor.

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

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

we focus on the maximization of the lifetime utility of the children conditional on current household income *y*.

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

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

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

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

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

The schooling decision is subject to two further conditions:

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

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

investment needed for schooling to increase human capital implies that enrolling a child in school creates a discontinuity in the time budget of the child.[xxiv]

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.[xxv] 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}.[xxvi] The child wage, w_{child}, is also taken as exogenously fixed and lies below w_{parent}. Given the child wage, the net cost of schooling, c-p, and the level of income, y, children can be in one of 4 states: idle (both $e_s = 0$ and $e_w = 0$, i.e. $e_l = 1$), work only ($e_s = 0$), school only ($e_w = 0$ and $e_s \geq e_{s.min}$), or school and work (both $e_w > 0$ and $e_s \geq e_{s.min}$).

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

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

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

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

The curve denoted U_{Y0} in Appendix Figure 1 presents the possible combinations of school subsidy and current period household income at which the child is indifferent between idleness and joint school and work. If a particular combination of school subsidy and household income falls below this curve, the child either works only or remains idle.[xxvii] Children who both work and study can reallocate the time spent at work to further study as the subsidy level increases beyond p^0 but still

remains below *c*; however, these children cannot exit from work and remain in school as the full schooling cost must be met.

As the subsidy level *p* increases further and approaches *c* then the full cost of schooling is nearly met, and any subsidy in excess of *c* becomes an infra-marginal transfer to total household income. At some point, the return to continuing in child work is surpassed by the discounted total gains from increased attention to school. Call this transition point of lifetime earnings Y^*:

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

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

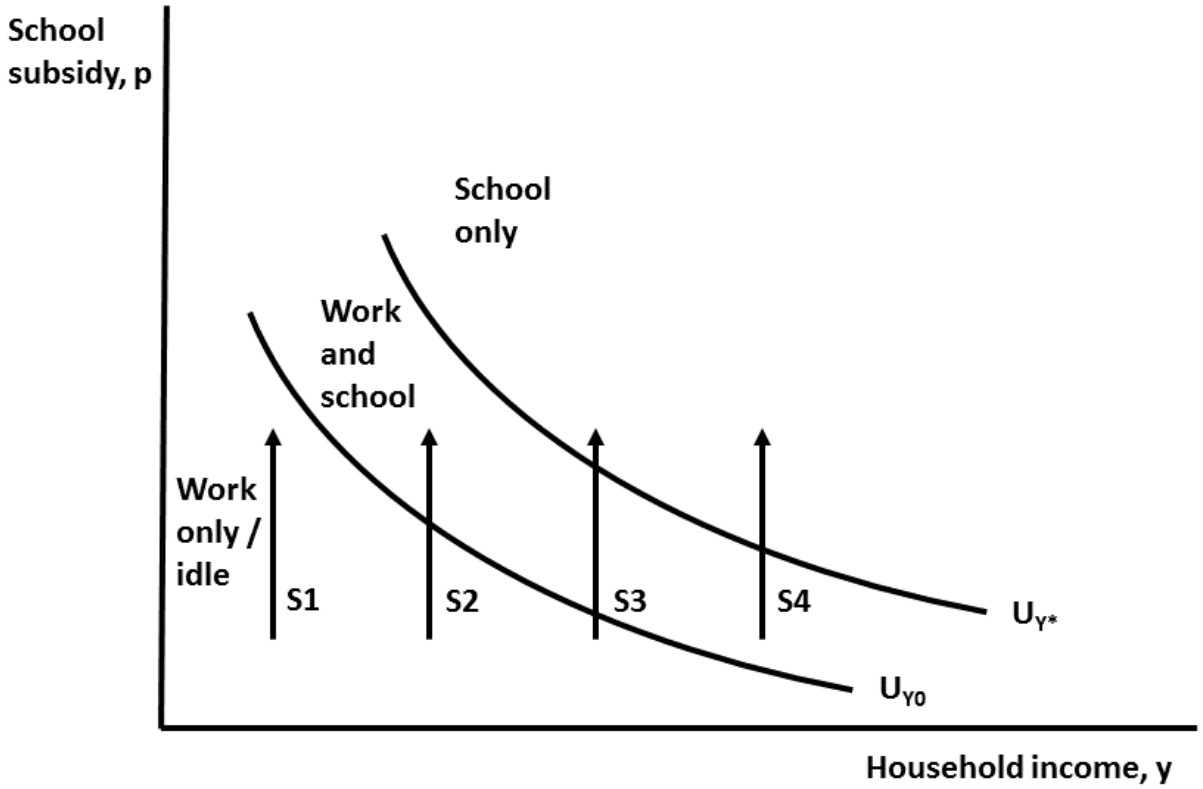
$$U(Y^*, e_s > 0, e_w = 0) \geq U(Y, e_s > 0, e_w > 0) \; for \; all \; p \geq p^*$$

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

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

households should exhibit a reduced labor response, i.e. be less likely to be in school and work than children from poorer households after the introduction of the subsidy.

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



Appendix 2: Definition of outcome measures used in the analysis

Outcomes as defined for the Philippines data

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

For work, we focus on participation in economic activities, days worked in the 12 months prior to the interview (with or without pay), and annual earnings. Work refers to any work on a farm, in the private or public sector, for own account, and 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 inside and outside the household, work without pay inside and outside the household, as well as participation in the following occupations: (i) farmers, forestry workers, and fishermen, (ii) laborers and unskilled workers, and (iii) all other occupations. We check the robustness of the estimated impact on work using the same set of outcome variables, but reported for the seven days preceding the interview (instead of the 12 month recall period). We focus primarily on work in the 12 months prior to the interview, because this outcome variable is less likely to be affected by seasonality concerns.

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

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

Outcomes as defined for the Mexican data

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

Appendix 3: Balance of baseline characteristics

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

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

Appendix Table 12a shows the balance along child (aged 10-14) level controls: age, gender, and an indicator variable taking the value 1 if neither the child's mother nor his/her father lives in the child's household. We consider the latter an important covariate because a large literature shows that parental absence (mostly death) is an important predictor and determinant of schooling outcomes (e.g. Evans and Miguel, 2007) and cash transfers can help compensate for parents' absence (Fitzsimons and Mesnard, 2014). All of these child-level controls are constructed using follow-up data as no child-level information can be derived from the data collected for the proxy-means test. In the absence of differential attrition, these variables are unlikely to be affected by the program (and the lack of significance in the balance tests indeed implies there was no differential attrition unless the characteristics were originally unbalanced at baseline and then experienced a degree of differential attrition that would result in balance at end line, a situation not supported by any supplementary quantitative or qualitative evidence).[vii]

Appendix Table 12b examines the balance of the household level measures: a wealth index (included because it is a key targeting criterion of the program), whether the household head is

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

Appendix Table 12c explores the balance of 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 95^{th} percentile of distance to primary school. We include these variables as the cost of commuting to school is an important component of the overall cost of education (see also Appendix 4).

Appendix Table 12d explores the balance of the child labor recall data measures across treatment and control villages, separately for the years 2007, 2008, and 2009. These variables are not used as controls in our regressions, but exploited in the panel fixed-effects estimates displayed in Appendix Table 4.

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

In Appendix Table 13, we present the results of a multinomial logit regression of the four mutually exclusive combinations of work and school on the household characteristics for which we carried out balance checks in the control villages. We estimate the multinomial logit both for our primary sample of children from eligible poor households (columns (5) – (8)) and, to highlight the role of income in the probability that children work and/or attend school, for the full sample of children observed in the control villages (columns (1) – (4)).[viii] 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.[ix] The probability of being neither in work nor in school, on the other hand, increases with distance to school. The younger the child is, the more likely she is to be in school only and the less likely to be in work only. Boys are generally less likely to be in school only than girls. Children are less likely to be in school if the household head never attended school. Children are also less likely to attend school only and more likely to work only if the household is engaged in agricultural activities.

Baseline balance in the Mexican data

For the analysis of child work and schooling in the Mexican data, we tested for balance along the following individual and household characteristics: age, gender, and an indicator variable taking the value 1 if neither of the child's parents live in the household, a wealth index (with weights derived from the first principal component of the following dwelling characteristics: electricity, three indicators for roof material ((i) sheets made of metal, fiber glass, or plastic, (ii) sheets made of cardboard, or (iii), concrete), indicators for wall material ((i) wood, (ii) bricks, or (iii) adobe), dwelling owned by the household, the household has no access to toilet facilities used exclusively by the household, the household has access to piped water on the household's dwelling or plot, and ownership of the following assets: TV, video, stereo, blender, refrigerator, washing machine, fan, gas stove, gas heater, car, and truck.), whether the household belongs to an indigenous people group, whether the household head ever attended school, whether the household is engaged in (non-livestock) agricultural activities, the total number of household members, and the number of household members aged 0 to 5 and 6 to 17. Finally, we also test for balance in whether there is a primary school in the locality and whether there is a secondary school in the locality. The locality level characteristics are established using November 1999 follow-up data. We found that there is one statistically significant difference between the treatment and control group: treatment localities are about 4 percentage points more likely to have a primary school than control localities. We do not present these balance tests here, as numerous other studies have investigated the balance of the Mexican data. The most notable of these is Behrman and Todd (1999), who find minor but statistically significant imbalances when using the household level (instead of locality level) data.

Appendix 4: Estimates of schooling costs

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

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

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

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

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Appendix tables

Appendix Table 1. Attrition Baseline to Follow-up in *Pantawid* data

	Households Originally Sampled		**Replacement Sample**	
	Treatment	Control	Treatment	Control
Poor with children in eligible age (Sample 1)	624 (88.6%)	634 (88.8%)	80 (11.4%)	80(11.2%)

Reproduced from World Bank (2013). The replacement sample refers to the households that needed to be replaced due to attrition between the baseline and follow-up surveys.

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

	Education		Work past 12 months		Mutually exclusive combinations			
	Attends	Attends regularly	Any work	Work for pay, outside own household	In school only	In work only	In school and in work	Neither in school nor in work
	(1)	(2)	(3)	(4)				
OLS:								
Impact on boys	0.040	0.088**	0.046	0.050*	-0.021	-0.011	0.059*	-0.027
	(0.029)	(0.034)	(0.038)	(0.028)	(0.041)	(0.018)	(0.035)	(0.023)
Impact on girls	0.050**	0.099***	0.032	0.050**	0.010	-0.007	0.041	-0.044***
	(0.020)	(0.027)	(0.032)	(0.023)	(0.034)	(0.011)	(0.031)	(0.016)
Additional information:								
P-value F-test (impact boys = impact girls)	0.697	0.762	0.803	0.862	0.612	0.854	0.749	0.538
Number of observations	1,264	1,264	1,264	1,264	1,264	1,264	1,264	1,264
Mean in control group, boys	0.864	0.840	0.249	0.122	0.668	0.053	0.196	0.083
Mean in treatment group, boys	0.899	0.940	0.293	0.170	0.648	0.042	0.251	0.060
Mean in control group, girls	0.914	0.841	0.145	0.055	0.790	0.021	0.124	0.066
Mean in treatment group, girls	0.964	0.933	0.185	0.109	0.791	0.013	0.172	0.023

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

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

	Attends	Attends primary school	Attends secondary school	Attends regularly	Attends primary school regularly	Attends secondary school regularly	Days attended school past 2 weeks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OLS with controls:	0.050***	0.035	0.014	0.097***	0.071***	0.024	1.019***
	(0.019)	(0.023)	(0.019)	(0.024)	(0.026)	(0.020)	(0.237)
Probit without controls:	0.041**	0.033	0.005	0.087***	0.069**	0.016	
	(0.019)	(0.026)	(0.023)	(0.025)	(0.029)	(0.024)	
Logit without controls:	0.040**	0.033	0.005	0.086***	0.069**	0.017	
	(0.019)	(0.026)	(0.023)	(0.025)	(0.029)	(0.023)	
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)

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

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

		Pay and location				Types of occupations			Intensity	
	Any work	Work for pay, outside own household	Work for pay, inside own household	Work without pay, outside own household	Work without pay, inside own household	Laborers and unskilled workers	Farmers, forestry workers, and fishermen	Other	Days worked past year / Hours worked past week	Days worked past year / Hours worked past week *for pay*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Work in the past 12 months										
OLS with controls:	0.046*	0.052***	-0.002	-0.000	0.009	0.050**	-0.004	0.006	1.794	1.426
	(0.026)	(0.019)	(0.010)	(0.010)	(0.021)	(0.024)	(0.016)	(0.006)	(1.731)	(1.270)
Probit controlling only for municipality:	0.041	0.045**	-0.001	-0.007	0.006	0.045	-0.004**	0.002		
	(0.031)	(0.019)	(0.008)	(0.010)	(0.018)	(0.025)	(0.013)	(0.005)		
Logit controlling only for municipality:	0.043	0.042**	-0.002	-0.007	0.009	0.044*	-0.003	0.003		
	(0.031)	(0.017)	(0.007)	(0.010)	(0.017)	(0.024)	(0.011)	(0.004)		
Panel fixed effects based on recall data	0.049	N.A.	N.A.	N.A.	N.A.	-0.006	0.054**	0.006	N.A.	N.A.
	(0.033)					(0.019)	(0.026)	(0.006)		
OLS excluding children not directly related to household head	0.039	0.047**	-0.001	-0.009	0.012	0.043	-0.003	0.004	2.044	1.989
	(0.030)	(0.022)	(0.010)	(0.011)	(0.022)	(0.027)	(0.016)	(0.007)	(1.802)	(1.403)
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 only controlling for municipality and child age:	0.046*	0.024	-0.002	-0.006	0.023	0.048**	-0.008	0.003	0.040	0.152
	(0.027)	(0.016)	(0.008)	(0.010)	(0.019)	(0.022)	(0.015)	(0.002)	(0.410)	(0.248)
OLS with controls:	0.052**	0.027*	-0.004	0.002	0.021	0.051**	-0.006	0.003	0.029	0.076
	(0.025)	(0.015)	(0.008)	(0.008)	(0.018)	(0.021)	(0.014)	(0.002)	(0.364)	(0.231)
Probit controlling only for municipality:	0.047*	0.021	-0.001	-0.005	0.022	-0.007	0.047**	0.003		
	(0.027)	(0.014)	(0.006)	(0.008)	(0.017)	(0.013)	(0.021)	(0.004)		
Logit controlling only for municipality:	0.047*	0.021	-0.001	-0.005	0.020	-0.005	0.044**	0.003		
	(0.026)	(0.013)	(0.006)	(0.008)	(0.015)	(0.012)	(0.020)	(0.004)		
OLS excluding children not directly related to household head	0.051*	0.024	0.000	-0.006	0.027	0.049**	-0.005	-0.006	0.073	0.170
	(0.027)	(0.016)	(0.008)	(0.010)	(0.019)	(0.023)	(0.015)	(0.032)	(0.413)	(0.246)
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)

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

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

	Mutually exclusive combinations				
	In school only	In work only	In school and in work	Neither in school nor in work	Worked while school was in session
	(1)	(2)	(3)	(4)	(5)
OLS with controls:	-0.009	-0.013	0.059**	-0.037**	0.047**
	(0.028)	(0.011)	(0.025)	(0.014)	(0.021)
Probit controlling only for municipality:	-0.004	-0.008	0.047	-0.032**	0.029
	(0.034)	(0.010)	(0.029)	(0.015)	(0.021)
Logit controlling only for municipality:	-0.007	-0.008	0.050*	-0.032**	0.030
	(0.034)	(0.009)	(0.027)	(0.014)	(0.019)
OLS excluding children not directly related to household head	-0.006	-0.009	0.048*	-0.033**	0.031
	(0.032)	(0.011)	(0.028)	(0.015)	(0.023)
2SLS TOT controlling only for municipality:	-0.010	-0.014	0.057	-0.032**	0.046
	(0.046)	(0.013)	(0.044)	(0.015)	(0.029)

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

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

	Education		Work past 12 months		Mutually exclusive combinations			
	Attends	Attends regularly	Any work	Work for pay, outside own household	In school only	In work only	In school and in work	Neither in school nor in work
	(1)	(2)	(3)	(4)				
Panel A: Effects on older siblings (15-17) of children in core sample (10-14) from eligible households								
OLS without controls:	-0.025	0.031	0.077	0.107***	-0.093*	0.016	0.060	0.017
	(0.048)	(0.051)	(0.047)	(0.039)	(0.052)	(0.040)	(0.041)	(0.037)
Additional information:								
Number of observations	474	461	395	395	394	394	394	394
Observations in control group	234	224	189	189	188	188	188	188
Observations in treatment group	240	237	206	206	206	206	206	206
Mean in control group	0.632	0.563	0.323	0.175	0.403	0.180	0.238	0.180
Mean in treatment group	0.600	0.591	0.417	0.291	-0.108	0.025	0.068	0.015
Panel B: Effects on children (10-17) from ineligible households								
OLS without controls:	0.012	0.022	0.034	0.014	-0.034	-0.021*	0.055**	-0.001
	(0.019)	(0.025)	(0.023)	(0.018)	(0.026)	(0.013)	(0.022)	(0.016)
Additional information:								
Number of observations	1,277	1,237	1,162	1,162	1,162	1,162	1,162	1,162
Observations in control group	663	633	607	607	607	607	607	607
Observations in treatment group	614	604	555	555	555	555	555	555
Mean in control group	0.861	0.815	0.216	0.120	0.720	0.066	0.150	0.064
Mean in treatment group	0.857	0.820	0.247	0.132	0.681	0.049	0.198	0.072

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

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

		Ineligible households with children aged 6 to 14						
	Village level	Household level, past 12 months. Any household members involved in:			Adult level, past 7 days			
	Wages of adult male laborers	Farming	Non-farm business	Fishing	Worked	Worked for private household or establishment	Worked for government	Self-employed, employer, or worked on household farm or business
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OLS only controlling for municipality and child age.	9.306	0.020	-0.015	-0.032	-0.010	0.001	0.003	-0.016
	(6.422)	(0.030)	(0.016)	(0.024)	(0.013)	(0.014)	(0.010)	(0.011)
Additional information:								
Number of observations	127	2,323	2,322	2,323	5,403	5,403	5,403	5,403
Observations in control group	62	1182	1180	1181	2802	2802	2802	2802
Observations in treatment group	65	1141	1142	1142	2601	2601	2601	2601
Mean in control group	142	0.615	0.131	0.110	0.620	0.257	0.061	0.108
Mean in treatment group	150	0.648	0.114	0.074	0.611	7.000	0.065	0.090

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

Appendix Table 8. *Pantawid* program impact on household composition from a difference-in-differences regression using baseline data.

	Household Size	Dependency Ratio	Female Dependency Ratio	Male Dependency Ratio	Children Aged 0-14
	(1)	(2)	(3)	(4)	(5)
Treated*After	-0.040	-0.001	0.084	-0.016	0.030
	(0.116)	(0.043)	(0.083)	(0.086)	(0.033)
Additional information					
Number of observations	664	664	664	664	664
Observations in control group	336	336	336	336	336
Observations in treatment group	328	328	328	328	328
Mean in control group	6.193	1.180	2.460	2.350	3.005
Mean in treatment group	6.313	1.287	2.592	2.582	3.107

Note. The Dependency Ratio is calculated as the proportion of individuals aged 0-15 and older than 60 to those aged 16-59 in the household. The Female (Male) Dependency Ratio is the proportion of 0-15 year olds and those older than 60 to females (males) aged 16-59. Difference-in-differences estimates of program impact on the composition of households with 10-14 year olds in study sample. Standard errors are clustered at the village level. OLS only controlling for municipality and child age. *** p<0.01, ** p<0.05, * p<0.1

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

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

Note. Estimates of travel time and travel costs to nearest market on the school attendance and work for 10-14 year old children from eligible households. Here, school refers to current school attendance and work refers to any work in the past 12 months. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

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

			Pay and location				Types of occupations			Days worked	
	Any work	Any work for pay	Work for pay, outside own 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)	(11)
Child was offered deworming pills during last school year	-0.025	-0.043	-0.014	-0.032	-0.002	-0.003	0.021	-0.025	-0.014	-1.458	-1.018
	(0.050)	(0.041)	(0.024)	(0.029)	(0.032)	(0.019)	(0.027)	(0.044)	(0.011)	(1.747)	(0.779)
Additional information:											
Number of observations	546	546	546	546	546	546	546	546	546	544	546
Observations not offered deworming	133	133	133	133	133	133	133	133	133	133	133
Observations offered deworming	413	413	413	413	413	413	413	413	413	411	413
Mean in group not offered deworming	0.241	0.165	0.060	0.105	0.105	0.038	0.075	0.173	0.015	6.083	1.564
Mean in group offered deworming	0.167	0.092	0.027	0.063	0.075	0.034	0.077	0.109	0.002	3.314	0.475

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

Appendix Table 11: Summary of the Literature on Conditional Cash Transfer Programs and Education Costs

Country	Reference(s)	Impact on School Enrollment	Impact on Child Labor	Subsidy Relative to Schooling Costs	Notes
Brazil	Ferro et al. (2010)	Positive (2.5 percentage points)	Negative (3 percentage points)	Full subsidy	Although Ferro et al. (2010) do not explicitly mention the cost of education, one of the authors kindly confirmed that the transfers will have exceeded the cost of education. Children in the examined cash transfer program would typically attend public schools, which are free of charge. The government provides textbooks and uniforms and in rural areas there are school buses.
Cambodia	Ferreira et al. (2009)	Positive (20 percentage points)	Negative (10 percentage points)	Full subsidy	Noted on page 24.
Colombia	Barrera-Osorio et al. (2011); Attanasio ()	Positive (3-5 percentage points)	Negative on students in grades 6-10 (30 percent reduction), no effect on those in grade 11	Full subsidy	Noted on page 171.
Costa Rica	Duryea and Morrisson (2004)	Positive (2.9 to 8-7 percentage points, depending on method)	No effect		The conditional transfer program in Costa Rica was an in-kind transfer.
Ecuador	Edmonds and Schady (2011)	Positive (19 percentage points)	Negative (9.9 percentage points)	Full subsidy	The authors note on page 118 that the size of the transfer is greater than the average increase in schooling costs between primary and secondary school. While the transfer program in Ecuador was unconditional, it was accompanied by marketing activities advocating for the relevance of schooling and that part of the beneficiaries perceived the program as conditional on school participation.
Honduras	Glewwe and Olinto (2004); Galiani and McEwan (2013)	Positive (1-2 percentage points-- Glewwe and Olinto; 8 percentage points-- Galiani and McEwan)	No effect (Glewwe and Olinto); negative (3 percentage points-- Galiani and McEwan)	Full subsidy	We infer that the transfer amount exceeded the cost of education from Fiszbein and Schady (2009, P.182-183) and Rawlings and Rubio (2005, P.34).

Appendix Table 11 continued: Summary of the Literature on Conditional Cash Transfer Programs and Education Costs

Country	Reference(s)	Impact on School Enrollment	Impact on Child Labor	Subsidy Relative to Schooling Costs	Notes
Indonesia	Sparrow (2007)	Positive (13 percentage points)	Negative (4 percentage points)	Full subsidy through junior secondary; marginally lower than senior secondary costs	Noted on page 105.
Jamaica	Levy and Ohls (2007)	Positive on attendance, enrollment not reported (38.5-50.6 percentage points)	No effect	Full subsidy	Noted on page 7
Mexico	Skoufias and Parker (2001); Schultz (2004); Rubio-Codina (2010)	Positive (girls: 1.3 percentage points in primary school and 7.1 pp in secondary school. Boys: 1.2 percentage points in primary school, 5.2 pp in secondary--Schultz); Positive for girls (4.9 percentage points--Rubio-Codina)	Negative (1.2 percentage points for girls, 1.4 percentage points for boys--Schultz); Negative for girls (8.4 percentage points-- Rubio-Codina)	Full subsidy	Inferred from Fiszbein and Schady (2009, P.182-183) and Rawlings and Rubio (2005, P.34), and confirmed in own calculations reported in the paper.
Nepal	Edmonds and Shrestha (2013)	Positive (4.9 percentage points for full subsidy plus stipend) 2.3 percentage points but insignificant for full subsidy alone.	Negative for full subsidy plus stipend (5.3 percentage points); no effect of full subsidy alone.	Full subsidy in one arm; full subsidy plus an additional stiped in another	Noted in footnote 7 on page 7.
Nicaragua	Dammert (2008); Thomas (2010); Barham, Macours and Maluccio (2013)	Positive for ex-ante enrolment (19 percentage points-- Thomas), ex-post early enrolment (14.2 percentage points--Barham et al.), ex-post attendance (12 percentage points for girls, 18 percentage points for boys-- Dammert), and long-term attainment (half a year--Barham et al.)	Negative (1 percentage point for girls, 11 percent points for boys-- Dammert)	Full subsidy	Inferred from Barham et al. (2013) and Thomas 2010--primary education is free and the fees transfer was designed to offset all other schooling costs.

Appendix Table 12a. Balance of child characteristics in *Pantawid* data

	Study sample		
	Age	Male	Neither father nor mother lives in household
	(1)	(2)	(3)
Panel A: OLS without controls on study sample:	0.043	-0.011	0.007
	(0.059)	(0.030)	(0.013)
Additional information:			
Number of observations	1,264	1,264	1,264
Observations in control group	627	627	627
Observations in treatment group	637	637	637
Mean in control group	11.968	0.537	0.040
Mean in treatment group	12.013	0.526	0.047
Panel B: OLS without controls on all eligible households:	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
Panel C: OLS without controls on all households in baseline:	0.017	0.013	0.015
	(0.051)	(0.019)	(0.014)
Additional information:			
Number of observations	2,184	2,184	2,184
Observations in control group	1,114	1,114	1,114
Observations in treatment group	1,070	1,070	1,070
Mean in control group	11.955	0.521	0.094
Mean in treatment group	11.972	0.535	0.109

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

Appendix table 12b. Balance of household characteristics in *Pantawid* data

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

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

Appendix Table 12c. 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 12d. Balance of pre-intervention child work measures, recall data for children aged 10 to 14 at endline interview in *Pantawid* data

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

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

Appendix Table 13. Determinants of mutually exclusive combinations of work and school for children from *Pantawid* control communities

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

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

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

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

[xxiv] 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.

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

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

[xxvii] 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

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

[vii] 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.

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

[ix] 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).