Local Spillovers from Cash Transfer Programs:

Food Price Increases and Nutrition Impacts on Non-beneficiary Children

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Draft-- please do not cite; comments welcome

October 18, 2016

Abstract: Cash transfer programs targeted to poor households may increase prices, especially if local markets are not fully integrated into larger regional markets. Using data from the evaluation of a Philippine cash transfer program, we show that the prices of perishable protein rich foods exhibit moderate albeit sustained increases after program introduction. Likely as a result, anthropometric measures of child health, notably stunting rates, worsen among non-beneficiary children. These effects are not short-run but persist up to 31 months after program introduction. Failing to consider the effect of such local price increases on non-beneficiaries' wellbeing can overstate the impact of cash transfers. For very poor areas, where household targeting of cash transfers covers a majority of the households, geographic targeting may avoid the consequences of local market price spillovers and consequently prevent nutritional and other long-run impacts at little additional cost.

JEL Codes: H23; I38; O12; O15; O20

Keywords: Spillovers, cash transfers, price effects, food prices, Philippines

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Introduction

Cash transfer programs provide cash to poor households, whether conditional on the households meeting some pre-specified behavioral criteria (CCTs) – such as investing in their children's health or education – or unconditional (UCTs). These programs reach between 750 million and 1 billion people (DFID, 2011), thus indicating widespread popularity among policy makers. While an extensive economic literature has considered both direct and indirect effects of cash transfer programs, the study of possible program spillovers has focused on the generally positive externalities that operate through social networks (Baird, Bohren, McIntosh, Ozler, 2011) or through informal insurance and credit markets (Angelucci and Di Giorgi, 2009). In contrast, this paper shows that even modest price increases resulting from cash transfers can result in significant detriments among non-beneficiary households (in this case an increase in child stunting rates) that likely result in knock-on effects on human capital. The channel for this impact appears to be price increases of key protein-rich foods, which are particularly pronounced in very poor villages where a majority of households are cash transfer program eligible. This paper thus recommends a reconsideration of targeting mechanisms when designing cash transfer programs in contexts where local markets aren't fully integrated with surrounding regions and a large proportion of households in the village would receive program benefits under a household targeting rule.

The Philippines has implemented a cash transfer conditioned on child health and education since 2008. This CCT, called the *Pantawid Pamilyang Pilipino Program*, or simply *Pantawid*, is targeted to individual households based on a proxy means test for household income with eligibility cut-offs determined by province-specific poverty lines. Starting with an initial pre-pilot of 6000 households, the *Pantawid* pilot reached 4.35 million households by 2015 (DSWD, 2015). Indeed, *Pantawid* has reached more households than many other national CCTs; by way of comparison, the Indonesian *Program Keluarga Harapan (PKH)* covered 1.5 million households after 5 years (Nazara and Rahayu, 2013) and the fully-scaled up Mexican *PROGRESA/Oportunidades* program covered 5.8 million households (World Bank, 2014). The government of the Philippines is currently considering yet another scale expansion of *Pantawid*.

Given the large number of cash transfer programs and the number of households covered by the various programs, it is not surprising that an extensive literature has studied both direct and indirect effects of cash transfers. This literature has been reviewed in Fiszbein and Schady (2009), Baird, Ferreira, Ozler and Woolcock (2013), Saavedra and Garcia (2012) and Hanlon, Barrientos and Hulme (2010). In general, evidence suggests that both UCTs and CCTs improve school enrollment and attendance as well as increase utilization of preventive health services, in addition to increases in total levels of household spending. Evidence also suggests that, holding income constant, beneficiary households spend more on nutrient-rich foods than do untreated households (Fiszbein and Schady, 2009). These results may be unsurprising because enrollment, attendance and preventive health care are typically (a) normal goods whose demand increases as household resources increase, (b) behaviors on which CCTs are conditioned, and (c) both UCTs and CCTs are often accompanied by messaging underlining the importance of investments in human capital. However one might also expect there to be the possibility of spillovers to non-targeted outcomes as a result of the program. Potential spillovers operate at three different levels: within the household, within participating schools, hospitals and other facilities, and within affected local markets.

Spillovers typically arise through two related channels: the information supplied by the program and/or changes in household spending, saving, and activity as a result of the income transfer. Within the household, Contreras and Maitra (2013) exploit program rules of a Colombian CCT to find significantly better health outcomes of non-targeted adults in treatment households than in control households. They present suggestive evidence that this improvement works through information. Ex-ante simulations based on Brazil's *Bolsa Escola* CCT also suggest that there may be changes in child work-for-pay, with full education subsidies generally reducing children's economic activities (Bourguignon, Ferreira and Leite, 2003). On the other hand de Hoop, Friedman, Kandpal, and Rosati (2015) show that the partial education subsidy offered by *Pantawid* was accompanied by an increase in child schooling and work-for-pay, while the larger subsidy, adequate for schooling costs, offered in *PROGRESA/Oportunidades* CCT led to an increase in child schooling but a decrease in children's economic activities. Ferreira, Filmer and Schady (2009) show conceptually and empirically—using data from a Cambodian CCT—that child-specific cash transfers generate positive income effects and negative displacement effects on the schooling of ineligible siblings, leading to an ambiguous overall effect on these siblings' education.

Within participating facilities, the literature provides robust evidence of peer effects-driven increases in schooling enrollment of non-targeted populations, at least in the case of *PROGRESA/Oportunidades* (Bobba and Gignoux, 2014; Bobonis and Finan, 2009; Lalive and Cattaneo, 2009). Of particular relevance for this study, Bobba and Gignoux (2014) use data from *PROGRESA/Oportunidades* to find strong evidence of positive externalities in potential beneficiaries' school enrollment in areas with relatively few beneficiaries but less so in areas with a high proportion of beneficiary households. They surmise that this effect likely arises from greater gains to information sharing in more sparsely treated areas relative to more densely treated areas. They note that this result underlines the importance of estimating program impact on "sufficiently extended geographical areas".

Within affected areas more broadly, evidence suggests a variety of positive externalities: From reductions in crime and political violence in the Philippines (Crost, Felter and Johnston, 2014) to an increase in saturation within social networks leading to higher test scores and lower HIV prevalence among the untreated in Malawi (Baird, Bohren, McIntosh and Ozler, 2011). Finally, there is some evidence that *Pantawid* increased clientelism, which in turn improved political stability (Labonne, 2013). Examples of negative externalities include higher prices for health services resulting from an Indonesian CCT (Triyana, 2014) and production changes resulting in greater deforestation in communities with poor access to markets in Mexico (Alix-Garcia, McIntosh, Sims and Welch, 2013).

Such local-area spillovers can also, in principle, affect non-beneficiaries' consumption, such as through risk-sharing or effects on prices. However, evidence on such externalities is mixed: using data from Nicaraguan and Paraguayan CCTs, respectively, Macours, Schady and Vakis (2008) and Teixeira et al. (2011) do not find any evidence of externalities on non-beneficiaries. In contrast, studying the case of *PROGRESA/Oportunidades*, Angelucci and de Giorgi (2009) find that the CCT increased consumption by ineligibles through enhancing risk-sharing within household networks.

A small literature has also looked explicitly at price externalities from CCTs: Cunha, De Giorgi and Jayachandran (2014) compare in-kind and cash transfers in an intervention in Mexico. They find that both types of transfers generate general equilibrium price effects for the local economy – downward pressure on prices for the transferred good under the in-kind program, and positive pressure on price levels in the case of cash. On average, these price increases are modest and do not affect purchasing power. However, Cunha, Di Giorgi and Jayachandran report more sizeable price increases for remote communities, and indeed find that the cash-versus-kind result is largely driven by price responses in these remote villages.

Beegle, Galasso and Goldberg (2015) study another type of aid program – a public works program (PWP) – to find evidence that a large-scale PWP in Malawi significantly worsened food security for non-beneficiary households in treated villages. Leveraging randomized treatment assignment of communities and households within communities, the authors rule out pecuniary externalities as well as a crowding out of traditional risk sharing mechanisms, although they are unable to pinpoint the exact mechanism driving the negative spillover to untreated households. Our study is therefore one of the few to identify an instance of local general equilibrium price effects from an aid program.

To illustrate how price effects might arise from a cash transfer program, we present a conceptual model showing that increased cash in the village increases demand for normal goods, including many food goods. How supply responds to this demand increase, as well as the shape of the demand curve, determines the resulting local price level. The supply response to a demand increase can be constrained for a variety of reasons. For one, remote villages tend to suffer from high transport costs for imported goods, and the transport cost wedge may counteract the marginal gain in profit from importing more units to sell at a marginally higher price. For another, if local production markets are either oligopolistic (perhaps due to a fixed cost of entry) or competitive with upward sloping MC then price increases will also likely follow a positive shift in demand.

It turns out in this setting that price increases are observed for perishable protein-rich foods – fresh eggs, fresh fish, and other goods with relatively high importation costs from outside the local market – but not for storable food goods such as rice or packaged food that are lower in protein but higher in other nutrients. This increase in relative prices results in a drop in real income for non-beneficiaries and a substitution away from protein-rich foods. The relative price changes and shifts in consumption are not large, but they are apparently enough to be expressed through lower child health as the growth of young children are particularly vulnerable to deficiencies in the intake of protein (Puentes et al., 2014).

The next section discusses the conditions for local price changes arising from cash transfers in more detail, while the subsequent section describes the *Pantawid* program and the data used in this analysis. The presentation of results follows and then the paper concludes with a discussion of the findings and implications for the targeting of social programs.

Possible price responses to cash-transfer induced demand increases

Although direct and indirect effects of cash transfers have been extensively studied, our understanding of the general equilibrium effects, and the economic mechanisms through which the cash and the conditions affect entire communities, remains incomplete. In contrast to previous literature that finds either no or positive spillovers on non-beneficiaries' consumption, we document a case of negative spillovers on the non-targeted population's consumption and nutrition. The relative price change of key protein-rich food goods appears to be the main driver of this outcome. How might this relative price change arise?

For one, the income gain from cash transfers increases demand for normal goods and if supply does not fully respond then prices will rise. In addition, conditional cash transfer programs such as *Pantawid* also broadcast messaging on nutritious foods that may additionally shift relative demand for these goods in particular. As potential price change is related to the magnitude of demand increase, one relevant program aspect that determines the magnitude of price change is what we call program saturation, namely the proportion of the population that benefits from the program. The higher the degree of program saturation, the greater the increase in aggregate demand and hence the greater potential for relative price change. Thus if we are able to observe resultant price increases at all, we should observe them in poorer villages that have a high proportion of program beneficiaries.

Besides the magnitude of the increase in aggregate demand, the price response will also depend on the structure of the market supplying the good. If the markets that supply these goods are perfectly competitive and have access to a broad regional or national (or international) production base, then suppliers should be able to meet any demand increase with relative ease. In this case, the only way price would rise is if the increase in production to meet the new demand raises the marginal cost of production. If the Philippines operated as one integrated market for the good in question, the total increase in aggregate demand due to *Pantawid* would likely have to be very large to raise the marginal cost of production to a noticeable degree (while widespread, program beneficiaries are only a small percentage of the total population). So a price rise in this case is unlikely.

However, the assumption of perfectly competitive food suppliers with access to a broad production base may not be fully applicable in the study villages, at least for all types of food goods. Poor villages can suffer from high transport costs and often have many basic goods locally sourced. This is especially true for perishable goods such as fresh fish or eggs that would need special technologies for transport and storage. For these goods, which we term non-tradable (i.e. across villages) for ease of exposition, the elasticity of supply may be low, especially with respect to relatively small changes in aggregate demand. As a result, price change induced by cash transfer programs can be heterogeneous not only as a result of shifts in relative demand but also due to the degree of integration of the local goods market. Thus another factor determining the price response, besides program saturation, is the tradable nature of the specific good in question. Our study will explore price responses along both these dimensions by contrasting low with high program saturated villages as well as contrasting tradable goods, such as rice and packaged food, with non-tradables such as fresh eggs and fresh fish.

Now with a refined focus on local market price changes for "non-tradable" goods, a rise in demand may or may not result in a price rise depending on the nature of the local market. A competitive local market with a flat MC curve will see no change in price and one with a rising MC curve will see a rise in price even if the local market is able to fully satisfy the new demand with an increase in production. Perhaps a more realistic model of the local market involves imperfect competition as there are relatively few producers of any local good, barriers to entry in terms of fixed costs such as a fishing boat or the cost of livestock, and perhaps even the possibility for collusion. Under imperfect competition, price increases from a rise in demand will occur even if the marginal cost of production is constant.

The price response to a demand shift of a given magnitude under imperfect competition depends on the shape of the demand curve, the number of producers in the market, and the fixed cost of entry for potential competitors. In general, the greater the number of firms operating, the smaller the price increase from a demand shift. Unfortunately we do not observe the number of local producers for perishable food goods, the fixed cost of entry, or the price threshold that may entice traders to import the good into the village. Thus we will not be able to say anything directly about the structure of the local market. Instead we will investigate the observed degree of price change and how the magnitude of change is or is not correlated with the factors that are likely to affect a relative price rise in the case of non-fully integrated markets, i.e. the degree of program saturation and the tradability of the good.

Cunha et al. (2014) conduct a similar inquiry in rural Mexico and find that cash transfers led to a positive but negligible increase in price.[5] However that is estimated over the entire sample – as described above, price changes can be greater in areas where supply responses are constrained. Indeed the authors find that the price change as a result of the cash program is more pronounced in remote villages, where prices increase for basic foods goods on the order of 6%. Geographic isolation is used in this study as a proxy for how closed an economy is (low elasticity of supply) or for how uncompetitive the market is (low number of producers). In extensions of the analysis we will also use remoteness indicators as a supplementary measure for the integration, or lack thereof, of the local market with wider regional ones.

Any observed price increase may not be too large as otherwise arbitrage opportunities would arise that would compensate for the cost of importation of goods into the village. In this sense the transport cost of goods drives a wedge between local and national prices that may persist over time even if the local price rises from a previous equilibrium price. Cunha et al. find that price effects persist for up to 22 months, suggesting that local supply responses do not (fully) counteract the demand increase from the income transfer. Our results are measured after 31 months of program exposure, and any evidence of a price rise would suggest the same.

The Pantawid program and evaluation design

The Philippines' Conditional Cash Transfer (CCT) Program, *Pantawid Pamilyang Pilipino Program*, provides cash transfers to poor households, conditional upon investments in child education and health as well as use of maternal health services. Eligible poor households are identified by the National Household Targeting System for Poverty Reduction (NHTS-PR) based on a poverty targeting mechanism using a proxy means test, which estimates per capita household income on the basis of observable easily answered information such as household size and the physical conditions of the dwelling. Households with estimated per capita income below the poverty line are classified as poor. From this database of poor households, *Pantawid* identifies and selects eligible households who have children 0-14 years of age and/or a pregnant woman at the time of the assessment. Poor and eligible households receive a combination of health grants and education grants every two months ranging from PhP 500 to

[5] They also look at in-kind transfers, which also increase supply as well as transfer income, and found a 4% decline in prices among the in-kind villages.

PhP 1,400 (approximately 11 USD to 32 USD) per household per month, depending on the number of eligible children in the household. Besides family size, the exact transfer amount is also determined by the compliance behavior of the household with respect to two types of grants:

Health Grants. The health grant is aimed at promoting healthy practices, improving the nutritional status of young children, and increasing the use of health services. Poor households with children 0-14 years old and/or pregnant women receive a lump sum amount of PhP 500 (about US$ 11) per household per month. Households must fulfill the following conditions for the health transfer: (i) all children under the age of five follow the Department of Health (DOH) protocol by visiting the health center or rural health unit regularly; (ii) pregnant women attend the health center or rural health unit according to DOH protocol; (iii) all school-aged children (6-14 years old) comply with the de-worming protocol at schools; and (iv) for households with children 0-14 years old, the household grantee (mother) and/or spouse shall attend Family Development Sessions at least once a month. One major topic of these sessions is family nutrition, which encourages the consumption of fresh protein-rich foods and deemphasizes the consumption of packaged foods.

Education Grants. The education grant is aimed at improving school attendance of children 6-14 years old living in poor households in selected areas[6]. The education transfer is PhP 300 (about US$ 6.50) per child per month (for a period of 10 months/year), for up to a maximum of three children in the household. Beneficiary households receive the education transfer for each child as long as they are enrolled in primary or secondary school and attend 85 percent of the school days every month.

Since its program launch in 2008, *Pantawid* has scaled up rapidly and has become the cornerstone of the Government's social protection strategy. By December 2014, the program had approximately 4.45 million active beneficiary households.

As a requirement of initial donor financing, an evaluation was designed from the early stages of the *Pantawid* program and viewed as an integral part of the program by implementers and policymakers. The first round of the impact evaluation was intended to represent the first implementation phase of the program, as the program's scale-up plan was not yet in place at the time of study design. This first phase covered some of the poorest areas of the country and the study purposively selected eight municipalities to be included in this phase for the evaluation. The Household Assessment Form (HAF) to estimate proxy means scores for beneficiary selection was fielded in these eight municipalities between October 2008 and

[6] The education grants were later extended after the study period to cover students up to 18 years old.

January 2009. This was followed by the implementation of *Pantawid* in the treated villages, with the first payment of cash grants commencing in April 2009.

The evaluation is a randomized control trial, stratified at the municipal level, with randomization at the village (also known as *barangay* in Filipino) level. A total of 130 villages were equally likely to be randomly selected to treatment or control status in the eight municipalities[7] selected for the impact evaluation[8]. A follow-up survey was conducted in October and November 2011, allowing a program exposure period of 30 to 31 months.

Data

A total of 3,742 households were surveyed from the eight study municipalities during the follow-up survey. With an eye toward investigating potential spillovers on non-beneficiary households, the entire study population was divided into four categories using the National Household Targeting Survey database as follows[9]:

- 1,418 Category 1 households that were designated poor households (below the PMT score) with children aged 0-14 or a pregnant mother at the time of the household assessment (the eligible group for *Pantawid*);
- 1,137 Category 2 households designated non-poor households (above the PMT score) with children aged 0-14 or a pregnant mother;
- 556 Category 3 households that were poor households yet without children aged 0-14 or a pregnant mother; and
- 631 Category 4 households that were non-poor but without children aged 0-14 or a pregnant mother.

Direct effects of the *Pantawid* program are estimated by comparing Category 1 households in treated and control villages. Possible spillover effects outside the beneficiary household but

[7] The study sample for the impact evaluation was selected in three stages. First, provinces in which the program had not yet been introduced in some of the eligible municipalities as of October 2008 were selected. Out of the 11 provinces available, 3 provinces were excluded due to security concerns. From the remaining 8 provinces, 4 provinces were chosen to span all three macro areas of the country (North, Visayas, and Mindanao). Second, among the selected four provinces, municipalities were randomly chosen to represent the average poverty level of areas covered by the program. Two municipalities each were selected for the study in the provinces of Lanao Del Norte, Mountain Province, Negros Oriental, and Occidental Mindoro. The set of provinces and municipalities for the RCT was selected jointly by DSWD and the World Bank, and barangay randomization was conducted in October 2008.
[8] The eligible household in the control villages started receiving the program benefits immediately after the survey was completed.
[9] The sample was designed to identify spillover effects to non-beneficiary target groups, as well as to run the RD analysis on the data from RCT sample areas.

within the village can be explored with impact estimates of Category 2, 3, and 4 households. Because of the focus on child nutrition, this paper here will present basic program impacts among Category 1 households and then explore possible spillover effects among Category 2 households.

In terms of program coverage, the impact evaluation survey and program Management Information System (MIS) database yielded slightly different estimates. Although all of the 1,418 households in Category 1 were eligible to become *Pantawid* beneficiaries in 2008, only those in treated villages were offered the program in 2009 by design. Among the 704 Category 1 households sampled in the *Pantawid* villages, 85 percent (581) reported being beneficiaries of the program, while 1 percent (7) in the control villages also reported being beneficiaries. According to the program Management Information System (MIS) database, however, the control villages did not have any beneficiary households, and 91 percent (647) of the 704 sampled Category 1 households in the *Pantawid* villages were considered beneficiaries of the program.[10] Small numbers of households among Categories 2, 3, and 4 (5 percent, 5 percent, and 10 percent, respectively) reported being *Pantawid* beneficiaries, even though none of these households were program beneficiaries according to the program MIS database.

The survey data include complete height-for-age data on 172 non-beneficiary children 6-36 months of age in treated areas and 151 non-beneficiary children of the same age range in control areas. Weight-for-age data were collected for 177 6-36 month old non-beneficiary children in treated areas and 156 non-beneficiary children in control areas. Anthropometric z-scores were calculated based on the WHO (2006) growth standard. Scores of more than 6 standard deviations above or below the reference mean were dropped from the sample (Rutstein, 2006). This trimming resulted in 14 of the 172 treated children and 6 of the 151 control children being dropped from the height-for-age regressions, and 2 of the 177 treated and none of the 156 control children being dropped from the weight-for-age regressions. Annex 1 explores alternative cutoffs for trimming the data, and shows that the results are robust to the data trimming.

[10] The lower percentage of sampled households in *Pantawid* villages that reported being program beneficiaries may be explained in part by the fact that program participation is voluntary. Some households identified as potential beneficiaries may have waived their right to the program. Another possibility is that through the community validation process of NHTS-PR, these households may have been taken off the list of poor households. It is also possible that a potential beneficiary household was unaware of the community assembly where attendance is required for potential beneficiaries to sign up for the program and confirm their basic household information collected for the PMT. Although very small in number, it is more difficult to explain why non-beneficiary households according to the program MIS reported themselves to be *Pantawid* beneficiaries in the survey. There is no official way for a household that was not identified as poor by the NTHS-PR to be registered as a *Pantawid* beneficiary. It is possible that the respondent was thinking of some other program that they received rather than *Pantawid*.

In addition to survey data, this paper uses a time series of item-specific food prices collected by the Bureau of Agricultural Statistics. This data was collected monthly from 2006 – 2014 and reported at the provincial level. In each province, price enumerators visit six markets – four rural and two urban – however the location of these markets are unknown to researchers. As this price data serves as an input to the Consumer Price Index of the Philippines, rigorous field controls are used to ensure the quality comparability of goods assessed (Philippine Statistics Office, 2015).

Experimental Evaluation of Pantawid: Balance and Results on Beneficiaries

Since program saturation is the key mediator of the cash transfer's impact on prices of non-tradables, we examine the expansion of *Pantawid* coverage by looking at the provincial data on the annual change in program saturation of *Pantawid* from 2006 to 2014. Indeed, Figure 1 illustrates the rapid expansion of *Pantawid* in this time period. This high rate of increase in the program suggests that within-village saturation rates may also have been high, generating conditions that may have resulted in prices increases, as discussed above.

<Figure 1 here>

As the municipalities selected at this stage of the program were among the poorest in the Philippines, it is no surprise that in many of the study barangays, a high proportion of the total population was eligible to receive program benefits. Figure 2 presents a histogram of this village level proportion of eligible households from among all households with children in the village – we term this proportion "saturation" as described above – for both treatment and control barangays. While there is a good degree of dispersion in this saturation measure, some villages have up to 90% of the household population eligible to receive benefits and in the typical village the majority of households are eligible. The median barangay saturation level is 65% and the mean 62% for the entire study sample.

<Figure 2 here>

The HAF that determined the household proxy means score constitutes the baseline date for the *Pantawid* evaluation. This information, relatively limited in scope, is primarily used to assess characteristic balance across treatment and control villages for the socio-demographic and economic information collected. Appendix Tables 1 & 2 explore baseline balance for Category 1 and 2 households respectively. These tables suggest that, overall, these categories were balanced. For category 1, only one comparison out of 28 is imbalanced—treated Category 1 household heads are three percentage points less likely to have completed some high school. However, this lack of balance is unlikely to be of import for the analysis presented here, as we primarily rely on comparisons within Category 2. For Category 2, balance is almost as comprehensive: only two out of 28 comparisons between treated and control areas are significantly different. While overall wealth, as measured by the logged PMT score is perfectly balanced between treated and control areas, Category 2 households in treated areas are less likely to own video recorders or motorcycles.

<Appendix tables 1 & 2 here>

Since saturation of the village is an important mediator to assess spillover impact, Appendix Tables 3 & 4 also present baseline characteristic balance in Category 2 households for those above and below the median saturation level. These tables highlight the overall balance of the experiment. For Category 2 households in above median saturation areas, 25 out of 28 comparisons, including the mean proxy of wealth, household head's education, and children's school attendance are completely balanced. Treated households are slightly smaller, and slightly less likely to have strong roof materials or a telephone than households in control areas. Similarly, in below median saturated areas, households are balanced along 26 of 28 dimensions. Treated households are slightly less likely to own their house or a video recorder, although here again the aggregate wealth proxy is balanced.

<Appendix tables 3 & 4 here>

Fielded in October and November of 2011, the follow-up survey was directed both at households and community respondents and covered an extensive range of socio-economic information including child anthropometric measures for ages 6-60 months (which were not assessed at baseline). Therefore, using the baseline characteristics available from the listing data, it appears that the experiment was well balanced.

Pantawid incentivized the health and education related behavior around children in beneficiary households. Table 1 presents some of the main impacts among beneficiaries of the program on outcomes related to these targets. Similar to findings from other CCT programs, the enrollment and attendance of children in the targeted age ranges improves on the order of 4 percentage points in terms of enrollment and 2-3 percentage points in terms of attendance, depending on the age group analyzed. These improvements were identified despite an already high level of enrollment and attendance in the control communities.

A range of nutrition indicators was investigated, as reducing childhood malnutrition is one of the main goals of *Pantawid*. The considered age group for these indicators in Table 1 is children 6-36 months old as these children transit a critical developmental period for physical growth. Children in this age range also are likely to have lived most or all of their lives exposed to the program. While there is no precisely estimated impact on the mean height-for-age score or other anthropometric measures (the point estimates suggest an improvement of 0.3 standard deviations in the z-score and reduction in stunting likelihood of 2 percentage points), the program lowered the rate of severe stunting[11] among poor children 6-36 months old by 9.3 percentage points. Stunting is a measure of chronic malnutrition, reflecting extended periods of inadequate food intake and/or chronic infection. No program impacts were found on other measures of severe or acute malnutrition such as wasting[12] or severe wasting.[13] For the average beneficiary child there may not have been a noticeable improvement in nutrition status but for the most disadvantaged there was a marked improvement.

<**Table 1**. Program impacts on beneficiaries – education and nutrition - here>

One of the ways in which the cash transfer may have resulted in improvements in child anthropometry is if beneficiary households seek to consume more of the goods associated with increases in child height-for-age. We look for evidence of this in two ways, first with respect to reported spending patterns of various food goods and then with regard to the reported food in-take of young children. The second column of Table 2 reports the program impacts on household food budget share. For a select number of individual food goods, we can also investigate the household reported item consumption for young children under 60 months of age. These results are presented in columns 3-6 of Table 2 and are based on recall over the week before survey.

Among beneficiary households, the cash transfer should increase available resources for spending. Indeed, we find that the total foods share of the household budget actually declines a modest degree (by 2.9 percentage points), indicating that households are moving along the food Engle curve as predicted after a gain in income. Among beneficiary children in *Pantawid* villages, there was a 8.2 percentage point increase in parents feeding their children (0-5 year olds who are fed solid foods) eggs, as well as some indication of higher meat and fish frequency (although not precisely estimated) during the previous week compared to children in non-program villages.

<**Table 2**. Household expenditure impacts for beneficiaries and non-beneficiaries>

Price, expenditure, and consumption effects

The first step in the proposed causal chain leading to nutrition deficits as a result of program pecuniary spillovers to non-beneficiaries is the presence of higher food prices. We investigate this with two sources of price information. First, we analyze price changes with official item-specific price series data. This data is reported at the provincial level for all 81 provinces in the Philippines on a monthly basis and covers a period starting in 2006, two years before the

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[11] Measured as height-for-age <-3SD applying the WHO Child Growth Standard (http://www.who.int/childgrowth/software/en/) accessed March 9, 2012
[12] Weight-for-age <-2SD applying the WHO Child Growth Standard
[13] Weight-for-age <-3SD applying the WHO Child Growth Standard

introduction of *Pantawid*, and extending through the entire scale-up period that concluded in 2014. As the price data is reported only at the provincial level, and not at an administrative level below that, we relate changes in province specific prices to changes in a provincial level saturation measure of *Pantawid* exposure. This measure is calculated as the number of beneficiary households reported by the provincial-level office of the Department of Social Welfare for that year divided by census bureau estimates for the total number of households in the province.

We present prices for three perishable goods – fresh eggs, fish, and chicken – and three tradable goods – rice, snacks and sugar. The average annual provincial price for each good, P_{ipy}, is regressed according to the following specification:

$$P_{ipy} = \gamma_0 + \gamma_1 S_{py} + F_p + F_y + \varepsilon_{ipy}$$

where *S* is the province-year specific saturation measure and *i*, *p*, and *y* index good, province, and year. The specification also includes province and year fixed effects, F_p and F_y respectively. The coefficient of interest, γ_1, captures the good-specific price deviation from its provincial mean level, net of common year effects, as a function of the mean-differenced changes in provincial exposure.

The three "non-tradable" goods all exhibit price increases correlated with changes in program saturation at the provincial level (Table 3). The maximum saturation level in the province level data is .40, suggesting that provincial prices for eggs can rise as much as 7.7% (0.192*0.40) as a result of price spillovers from the *Pantawid* program. Maximum price increases are on the order of 5-6% for fresh fish and chicken. In contrast, the three tradable goods show no significant price co-variation with program saturation as would be predicted if, even in poorer high-saturated villages, traders can access larger more integrated markets to satisfy a rise in food demand as a result of the cash transfer. The magnitude of the non-tradable goods price increase are not large, although it's unlikely that substantially higher increases would be able to sustained as larger increases may lead to arbitrage opportunities.

<**Table 3**. Impact on provincial level prices, here>

Besides province-level price changes, we also examine changes in the unit values of individual food goods reported by the survey respondents. This information was recorded only for three individual goods of standardized quality – eggs, rice, and sugar. The first panel of Table 4 explores how relative price levels vary at the time of survey between program and control villages. None of the prices are significantly different, and the point estimates of the rice and sugar are close to zero as well. While also not precisely estimated, the point estimate for egg price stands at almost 2% higher, indicating some divergence in relative price difference between the storable goods such as rice and sugar and the perishable good, eggs.

Relative price differences emerge much more clearly when the program indicator is interacted with the binary measure of high saturation villages. This interaction effect indicates a relative price increase of 0.36 pesos per egg (0.06*6.015) in saturated villages, a rise of approximately 6%. The price changes for the "tradable" goods rice and sugar are close to zero in magnitude and not precisely estimated. As eggs are the most perishable good in this three good comparison, the price divergences are consistent with the predictions discussed above. We observe a price rise in program villages, but only for the non-tradable good, in highly saturated villages.

It's an open question whether uncompensated price changes of these magnitudes are large enough to shift demand choices of the non-beneficiary households, especially those with children. We return to the survey data to investigate this next question.

<**Table 4**. Impact on village level prices here>

Results on non-beneficiaries

So far, the analysis has demonstrated that the program improved health and education outcomes of children from beneficiary households concomitantly raising children's consumption of protein-rich foods. The analysis has also identified a rise in the price of selected non-tradable goods, but not in more easily traded goods, over the course of *Pantawid* introduction that is correlated with program saturation measures. We identify this general price change pattern with two independent sources of price data. However, such an increase in food prices may also have affected the consumption of these foods by non-beneficiary households.

Table 5 thus presents results that parallel Table 2, but this time contrasting Category 2 in treated and control villages, and reports the program impacts on household food budget share as well as whether the household reported feeding eggs, meat, and fish to children 6-60 months old. For non-beneficiary households in treated villages, food expenditure as a share of household budget significantly increased by 3.6%, which suggests a decline in real income through the rising local prices of perishable foods, and perhaps a substitution away from dairy and eggs and towards cereals. It's difficult to infer too much from the spending data, although the change in patterns between treatment and control villages is consistent with a rise in demand for protein rich foods (as well as greater spending on other child goods) among beneficiary households, and perhaps a substitution away from protein rich foods for non-beneficiaries.

<**Table 5**. Household expenditure and children's food intake impacts for non-beneficiaries>

The program appears to also have had impacts on feeding practices, although not in all aspects. With the provision of cash coupled with parenting education provided during the program's Family Development Sessions, the program was expected to have some impacts on parenting practices, including feeding practices. Indeed, food intake among non-beneficiary children doesn't change nearly as much as a result of the program – the point estimates for the intake of eggs and vegetables are positive although not precisely estimated, suggesting little change in impact. However, as the price changes were seen in highly saturated villages, the food consumption intake of non-beneficiary households in those villages may be appreciably different. The bottom panel of Table 5 thus explores food intake in these villages through fully interacting program exposure with an indicator for above median saturation. Note that in this decomposition, the incidence and quantity of egg consumption among non-beneficiary households is higher in *Pantawid* villages. This may be due to informational spillovers of the program itself and the messaging around nutritious food can also be absorbed by the non-targeted households. Egg consumption also appears greater in highly saturated villages in general. This can be due to various unobserved differences at the village level since high saturation villages are poorer on average and may differ in other key characteristics that determine demand patterns. The interaction term, however, is strongly negative. The lower incidence of egg consumption for these children when compared with children in highly saturated control villages (or compared with children in low saturated but treated villages) is immediately apparent. The same holds for the number of eggs consumed, meat and fish, although these the effects on these three variables are not precisely estimated.

The question then arises: did the price increases and concomitant decreases in non-beneficiary children's consumption of protein-rich foods in highly saturated villages affect their anthropometry or educational outcomes?

Direct and indirect anthropometric impacts

The top panel of Table 6 presents the same schooling and nutrition measures as presented in Table 1 but now contrasts Category 2 households in *Pantawid* and non-*Pantawid* villages. Regarding school related outcomes, non-beneficiary households exhibit little change in the enrollment or attendance of school-age children. These levels are already near universal and substantially higher than the enrollment or attendance of beneficiary children residing in the poorer targeted households in the village. As these children are not enrolled in the program it is perhaps little surprising that schooling-related indicators do not change after program introduction, although it does suggest that there are few schooling specific spillovers in terms of higher fees or increased crowding that may deter the attendance of non-beneficiary children.

It is a different story for child anthropometric measures, here presented for non-beneficiary children 6-36 months of age in the second panel of Table 2. Children in non-beneficiary households are substantially shorter if they reside in program barangays - 0.4 z-scores shorter than their counter-parts in barangays without the program. They are also significantly more likely to be stunted. The stunting rate is estimated at 32% in control barangays compared with

43% in barangays with the *Pantawid* program. While the point estimate for weight-for-age is also negative, it is not precisely estimated, suggesting particularly pronounced effects among longer-term nutritional measures such as child height.

If increases in the prices of protein-rich foods, and the concomitant decrease in non-beneficiary children's consumption of these foods are associated with the worsening nutritional outcomes, then we would also expect to see the strongest nutritional effects in the villages where the price increases are the biggest: villages with the highest rates of program saturation. The second panel of Table 6 presents the impacts of living in an above-median saturated *Pantawid* village on children's schooling and nutrition, while the bottom panel presents the impacts of living in a village with a *Pantawid* saturation rate in the fourth quartile. Indeed, we find that weight-for-age is significantly lower and the likelihood of being underweight significantly higher in program villages that have high rates of saturation. Average height-for-age is lower and stunting rates higher in highly saturated villages, but the coefficients are precisely estimated for only the villages in the fourth quartile. On the other hand, average weight-for-age is significantly lower and the underweight prevalence significantly higher in above median saturated treated villages as well as treated villages in the top quartile of saturation.

<**Table 6**. – Impacts on non- beneficiaries, education and anthro – here>

While the study randomization of program villages resulted in a highly balanced sample across the characteristics assessed at baseline, child growth indicators were not measured. However as child growth is particularly sensitive to nutritional and health conditions in the first 1000 days of life (Hoddinott et al., 2013), we can investigate the age-patterns of child height differences among those who lived much of the first 1000 days under the program compared with somewhat older children born and partially reared before program onset. If the nutritional impacts on non-beneficiary children can be attributed to program presence and not other unobserved factors, then we would not expect to see the same impact among older children.

Figure 2 depicts the proportion of children stunted between treated and control barangays by three age ranges. The stunting prevalence for children 36-60 months, and hence only partially exposed to the program at critical ages for growth, is virtually identical. This is not the same among younger children where the stunting rate is substantially higher in treated barangays both for 6-24 months old, and 24-36 month olds.

<**Figure 2** – Anthro impacts by age, here>

These age differences suggested by Figure 2 are apparent in the impact regressions in Table 7 that now investigate nutrition impacts pooled among 6-60 month olds with the program exposure indicators interacted with the younger age categories of 6-23 months and 24-36 months. HfA z-scores are significantly lower for the 6-23 month olds, i.e. those children who have been exposed to the program for the entirety of their lives (and in-utero as well), on the order of .70 standard deviations. Stunting rates are also higher (15 percentage points) but the impact is not as precisely estimated.

Further, the impact on weight-related nutrition measures, which capture shorter-run measures of health status, also emerge for this age group. Younger non-beneficiary children are significantly more likely to be underweight, on the order of 20 percentage points. For non-beneficiary children 24-36 months, the point estimates of impact also suggest a worsening of nutritional status but to a lesser degree – there is no difference in wasting for example – and the difference not as precisely estimated. Taken altogether, if children in non-beneficiary households suffer growth deficits as a result of the program then we would expect to see a divergence in growth only for those children under an age cut-off when they are most vulnerable to a nutritional deficit. We see this for children under 36 months, and especially for those 6-24 months old at the time of survey.

<Table 7. Anthro impacts on non-beneficiaries by age, here>

At this point in the analysis, we have identified nutrition gains among beneficiary children and deficits among non-beneficiary children as a result of program exposure. Price increases of key foods are also correlated with program exposure, suggesting a key channel for program spillovers to non-beneficiary children. Observed spending patterns and reported food intake of young children are also somewhat consistent with spillovers operating through this channel.

Other channels

We present the evidence above to support the hypothesis that the high saturation of *Pantawid* increased the prices of certain non-tradable foods that are important for the production of child height, leading to increased stunting among non-beneficiary children in these highly saturated areas. In this section, we investigate other competing hypotheses and whether other household behaviors support our hypothesis.

For instance, it possible that the observed increases in child malnutrition among non-beneficiary households may also have been caused by a lack of balance at baseline rather than by *Pantawid*'s impact on prices. As shown in Appendix Table 3, non-beneficiary households in above median saturated treatment areas were significantly smaller (by 0.41 people, particularly adults) than control non-beneficiary households in above median saturated control areas, which in turn may affect the number of caretakers available for young children and thus household responsiveness to child illness or the available household resources that can be devoted to children. In Table 8, we use baseline data on household composition for a differences-in-differences approach to investigate the potential differences in household composition across *Pantawid* and non-*Pantawid* villages among non-beneficiary households with young children. We find no significant differences in household composition or in household dependency ratio, whether overall or with respect to female or male caregivers, in treated areas 31 months after rollout. The triple difference with program saturation, presented in the lower panel of Table 8, also does not suggest differences in household composition, although the average treated household has 0.36 more people. If the lack of baseline balance in household size between non-beneficiaries in saturated villages had been driving difference the increases in child malnutrition, we would have expected to see the triple difference terms to be precisely estimated.

<**Table 8**. Impact on household composition for non-beneficiaries, here>

On the other hand, if real incomes decline for non-beneficiary households as a result of price changes, as predicted by the theoretical framework, adult household members may respond by increasing their labor supply to compensate for the fall in real income, thus reducing the availability of adult caregivers. Table 9 looks at the labor force participation, work-for-pay and full-time work (greater than 40 hours per week) for adult men and women in non-beneficiary households. Overall there is little change in labor force participation or hours worked for either men or women. The second panel, which contains the fully interacted model between the program indicator and village saturation, also shows little evidence of change in male or female labor force participation in above median saturated non-beneficiary households.

<**Table 9**. Impact on adult LFP among non-beneficiaries>

If our hypothesis is correct, then the observed spillovers on non-beneficiary households work through the specific mechanism of increased food prices that affect child growth. While possible confounders may be the lack of caregivers, examined above, and possibly quality of care, examined further below, we should not necessarily see any impacts on non-beneficiary children's education or participation in the labor force. To determine if this is in fact the case, Table 10 looks at the education and child labor impacts of *Pantawid* on non-beneficiary children. The top panel of Table 10 shows the basic impact of the program, while the bottom panel interacts treatment with saturation. None of the estimated effects of *Pantawid* are significant, suggesting that the spillovers caused by *Pantawid* are indeed quite specific.

<**Table 10**. Impact on children's education and LFP among non-beneficiaries>

Finally, we note that child height is determined in early life not only by nutrition but also exposure to infections and other pathogens. Thus another potential channel is through program impacts on the access to early life health services and the quality of those services. This is especially important to investigate since maternal and child care is directly incentivized by *Pantawid.* To the extent that the formal health care sector is able to improve child health, a degradation in either the access to or the quality of health services can also in principle contribute to increased stunting. The *Pantawid* program may degrade the access to, or quality of, care through the crowding-out of available services as a result of an increase in service utilization by beneficiary households. This crowding out mechanism can result either in increased prices for care, through impacts on the quality of available services, or both. In neighboring Indonesia, Triyana (2014) finds that a CCT conditioned on safe-delivery practices results in a 10% increase in fees charged by mid-wives.[14]

Table 11 investigates program impacts on a range of health care seeking behavior relevant for young children for beneficiary and non-beneficiary households, as intended. The first panel of Table 11 indicates that, among beneficiaries, *Pantawid* has increased the use of maternal and child health services such as antenatal care, postnatal care, skilled birth attendance, growth monitoring, and general treatment seeking (the first three measures are based children on 6-36 months old since they refer to care around the birth, while the last two measure of more general care seeking are based on children 6-60 months). The *Pantawid* program has been very successful in getting beneficiary households to increase utilization rates, albeit these households start from a very low base.

The second panel of Table 11 investigates the impact of *Pantawid* on care-seeking by non-beneficiary households. Perhaps worryingly, non-beneficiaries in treated areas had significantly fewer ANC visits skilled birth attendance. However the changes in program utilization for these indicators are unrelated to the program saturation. This suggests that service accessibility around delivery is not related to the growth deficits observed in non-beneficiary children. And in general, the increased utilization from non-beneficiaries is not related to a change in utilization among children of non-beneficiary households. We are unable to look at the prices of such utilization or, perhaps more importantly, the quality of services delivered. However the initial results suggest that changes in health care accessibility are not a major contributor to increased stunting.

<**Table 11**. Impact on health seeking behavior for beneficiaries and non-beneficiaries, here>

Conclusions and discussion

This study investigates the impact of the Philippines flagship anti-poverty program on a range of outcomes for beneficiaries and non-beneficiaries. We show that the direct effects of the program on recipients were consistent with what has been found elsewhere in the literature: improvements in school attendance and enrollment as well as increases in health care utilization. Although many cash transfers are conditioned on nutritional behaviors for young children, the literature finds heterogeneous or modest impacts on nutrition (Gertler, 2004;

[14] There is also a 10% increase in the supply of local mid-wives, but the increase is not sufficient to prevent a price-rise.

Behrman and Hoddinott, 2005), and typically not on anthropometric outcomes (Alatas et al., 2011; Fiszbein et al., 2009). In contrast, we find a direct and positive impact on child nutrition from the *Pantawid* program: severe stunting among 6-36 month old children was 8.5 percentage points lower in treated areas than in control ones.

However, this gain was accompanied by a worsening of nutritional status among non-beneficiary households, particularly for children who are in vulnerable ages (the first 1000 days) when the program rolled out. Moreover, when program exposure at the village level (which we term saturation) is higher, the detrimental impacts on non-beneficiaries are larger. And second, prices for perishable protein-rich foods are higher when saturation is higher; prices are measured both through the survey respondent report of unit values as well as official price data at the provincial level. These effects are not simply short-run effects; the timeline between implementation and survey was 31 months on average.

In theory, the general price effects from a cash transfer are magnified as the number of beneficiaries increases, as well as if the local market that supplies goods is not fully integrated in the wider economy thus allowing local demand and supply conditions to largely determine the price. This lack of integration can arise either in remote areas, with consequent high transport costs into and out of the local market, or for perishable goods that require cooling technologies to transport and store and hence also exhibit higher costs of trade over long distances. In these cases the elasticity of supply to a demand increase may be low, especially with respect to marginal changes in price.

The food goods examined here are a combination of locally produced and largely perishable food goods along with packaged goods largely traded in national markets. It is these perishable food goods that appear to exhibit modest price increases after the introduction of the program, especially in the saturated villages that experienced the largest increases in non-beneficiary child stunting. Taken together, the findings suggest that anti-poverty cash transfer programs may have unanticipated effects operating through the price channel, especially for non-compensated (non-beneficiary) households. These effects will manifest or be greater in local markets that have a high degree of program coverage and/or markets that are less integrated into the national economy.

Besides the food price channel, additional channels that may contribute to the worsening of child anthropometric status include utilization spillovers in the formal health care system, and changes in the availability of young child caretakers. Little evidence was found for spillovers in the health system, although the analysis could not investigate possible quality of care changes. There is some indication that the presence of adults is lessened in the non-beneficiary households in the treated saturated areas – both in terms of a lower number of adults in the household and an increase in the hours worked by adult laborers – but this effect is not precisely estimated and does not appear to be quantitatively large. The analysis cannot completely rule out these complementary channels that may also explain the program spillover on stunting, partly due to the fact that important dimensions of these channels are unobserved. Yet if they arise they would operate alongside the price channel and may, to some extent, but a result of the village-level price changes brought on by the program.

Given the unintended negative consequences for young children in non-beneficiary households, and the fact that these consequences arise in poorer and more remote villages, the question of targeting rules comes to the forefront. The *Pantawid* program is targeted to the individual household on the basis of its proxy-means test score. However, for the subset of villages that are particularly poor and/or remote, a village based targeting scheme would presumably compensate all households for any rise in local prices and thus avert increases in child stunting. However area based targeting, while averting spillovers, would also likely be more expensive.

In order to provide an order of magnitude for the ratio of benefits to costs of extending the program coverage in *barangay*s with high poverty rates to the entire *barangay*, we carry out an exercise relating (i) the discounted value of labor market returns to averting stunting at age 36 months to (ii) the discounted costs of the family transfer associated with adding one household, with one child, to the program roll. Note that the benefits in this exercise are narrowly defined to those associated with lifetime earnings. In order to estimate the impact of stunting on labor market returns, we use the parameter estimate from Hoddinott et al (2011) who find that hourly earnings among adults who were stunted at age 36 months are 0.58 times the hourly earnings of those who were not stunted, after controlling for a number of contextual factors. Based on the average daily adult wages reported in our sample (US$6.3), we assume that the annual earnings of an adult who was stunted as a child are 0.58 times those of an adult who was not stunted as a child in each year that they work (following the method sketched in Hoddinott et al 2013). Since we find that *Pantawid* increased the prevalence of stunting by 12 percentage points among non-beneficiary children, we further multiply this value by 0.12 to estimate only the value of the stunting differential that can be attributed to the program. Using these parameters, we estimate that the discounted lifetime benefits of the program's impact on stunting (manifested in lifetime earnings) equals the discounted program costs when (i) we assume that real wages will grow at a rate of 1.75 percent per year, which is close to the rate observed in 2012 and 2013,[15]and (ii) apply a discount rate of 5 percent. At any *lower* discount rate (holding projected real wage growth constant) the benefit/cost ratio is positive; at any *higher* projected real wage growth (holding the discount rate constant) the benefit/cost ratio is positive.

[15] 1.9 and 1.5 percent real wage growth reported for 2012 and 2013 respectively in ILO (2014), http://www.ilo.org/wcmsp5/groups/public/---asia/---ro-bangkok/---sro-bangkok/documents/publication/wcms_325219.pdf.

To fix ideas, the above estimates assume an annual family transfer of US$132 for when the child is aged 1 to 14 (that is, the basic transfer amount); discounted back to age 0, this amounts to a total per-child value of the transfer (i.e. the cost) of US$1,636. Given the real wage growth estimate of 1.75% and a discount rate of 5%, and attributing 12% of the gap in earnings between adults who were stunted and those who were not stunted to the program, this is also equal to lifetime earnings detriment associated with the program.

Of course an exercise such as this is sensitive to a number of assumptions. In this case in particular, the labor market penalty associated with having been stunted at age 36 months (drawn from Hoddinott et al. 2011) appears to be quite high. We estimate that the program would have a benefit/cost ratio of greater than 1 as long as hourly wages for those who were stunted are 0.75 or less than those who were not stunted. Further we have simplified the cost implications of switching from a household to a village targeting mechanism. Adding more households to the beneficiary rolls would undoubtedly increase total administrative cost to some degree, yet at the same time the adoption of a village-based targeting rule may require far less household information to be collected and so would likely provide savings in this dimension.

Further work needs to be done to estimate more comprehensively the lifetime benefits of averting stunting as well as the programmatic costs of different targeting mechanisms. However the initial estimate here suggests that a national program may wish to consider a hybrid targeting scheme for their anti-poverty programs when faced with the possibility of local market price spillovers to non-beneficiaries in poorer and more remote villages. For these villages, offering the program to every household may be more cost-effective. Other areas of the country that likely will not experience local price spillovers can continue with targeting the program only to the poorest households.

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Figures and Tables

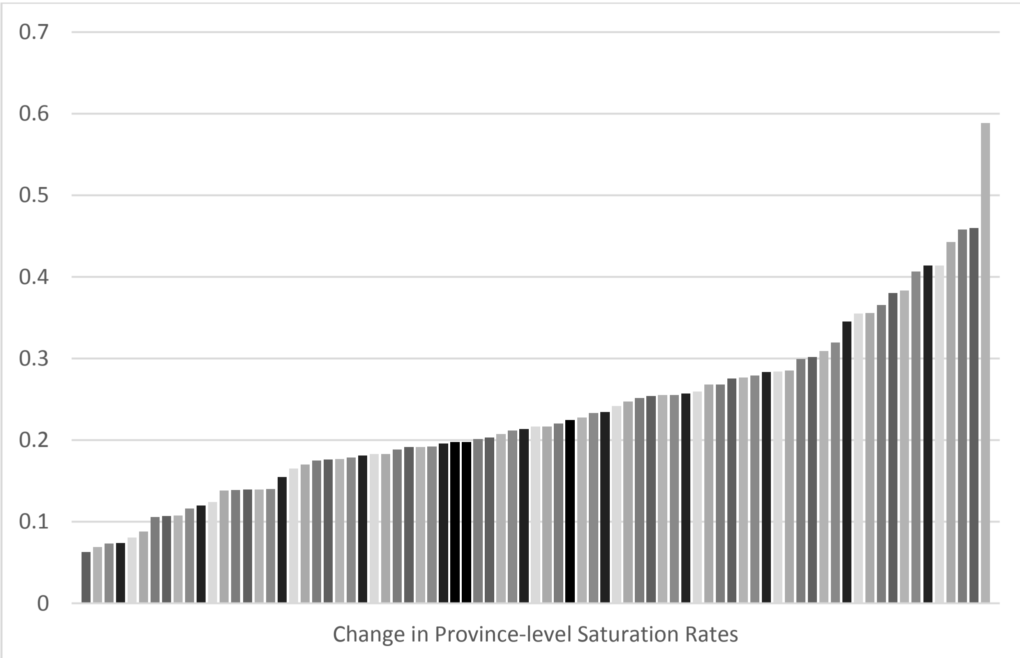

Figure 1: Expansion in the Coverage of Pantawid between 2008 and 2014, by Province

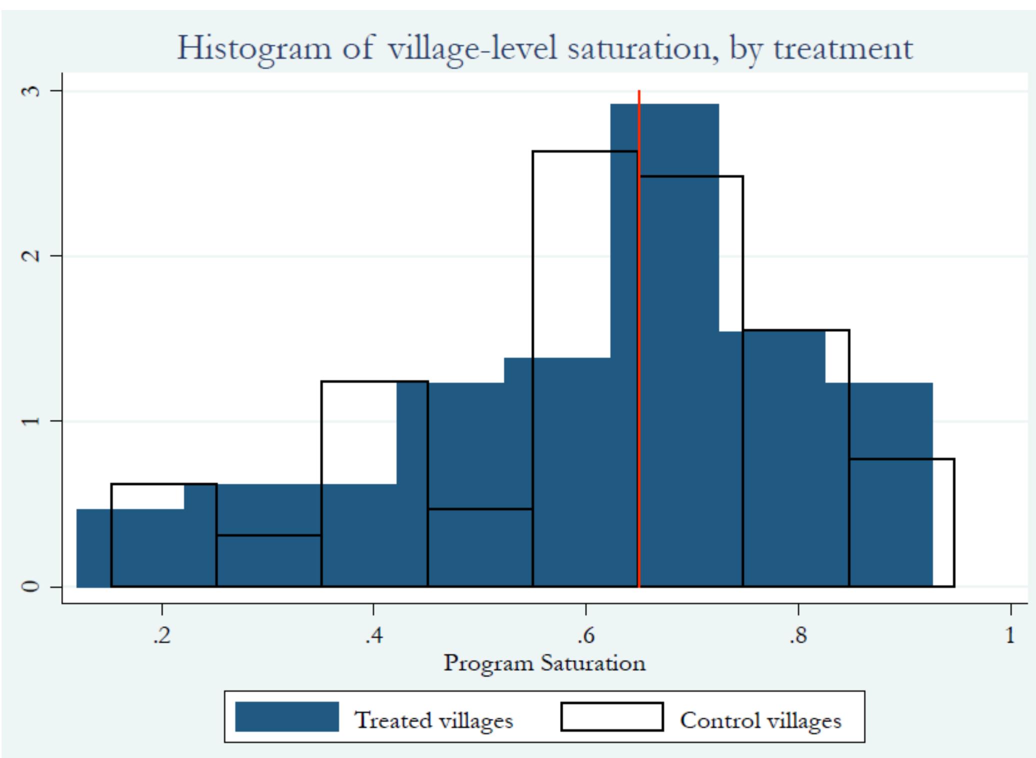

Figure 2: Within-Village Variation in Program Saturation

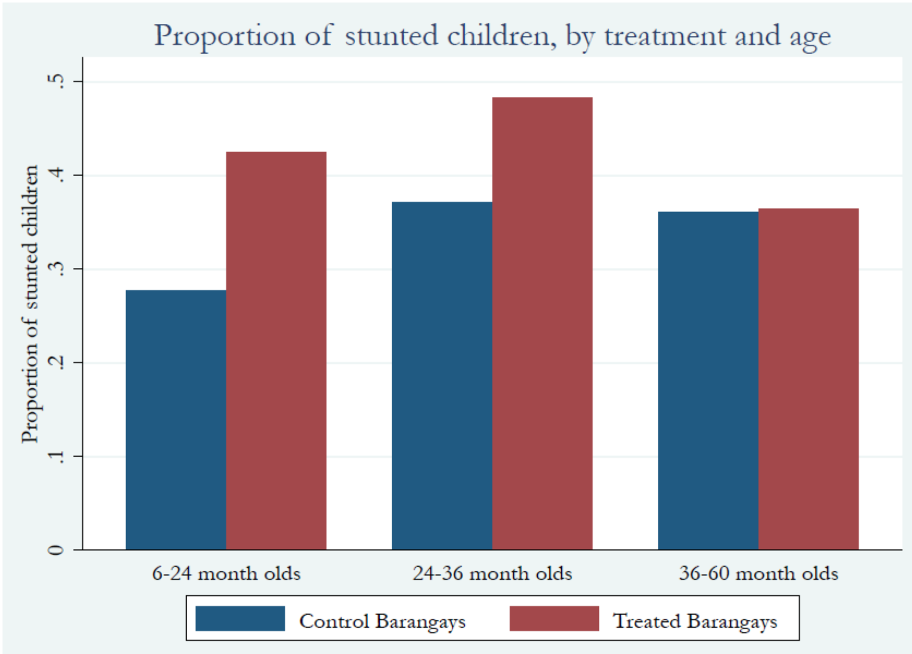

Figure 3: Stunting Prevalence in Children Exposed to Pantawid in First 1000 Days

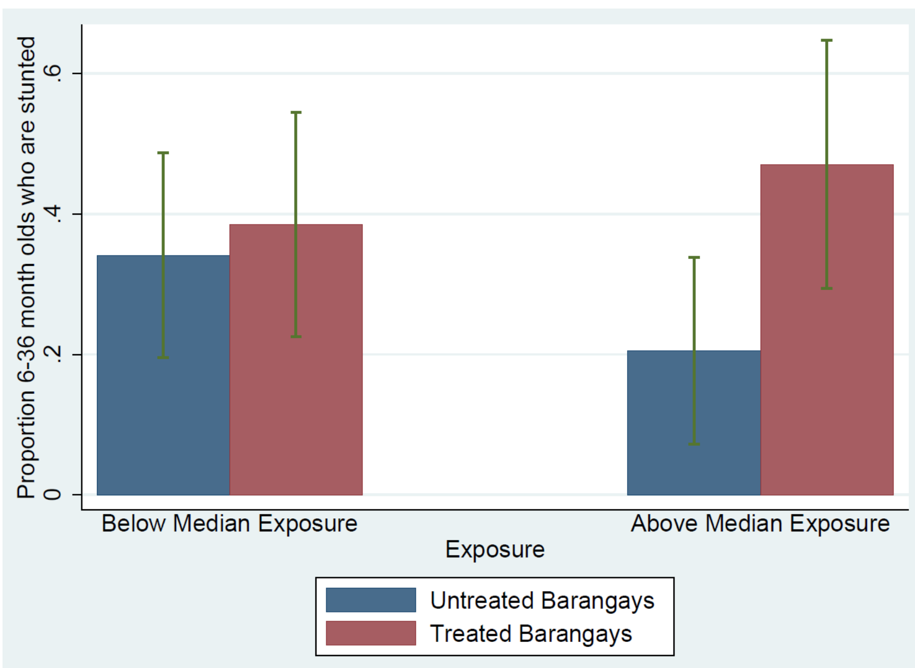

Figure 4: Stunting Rates by Above and Below Median Exposure to Pantawid

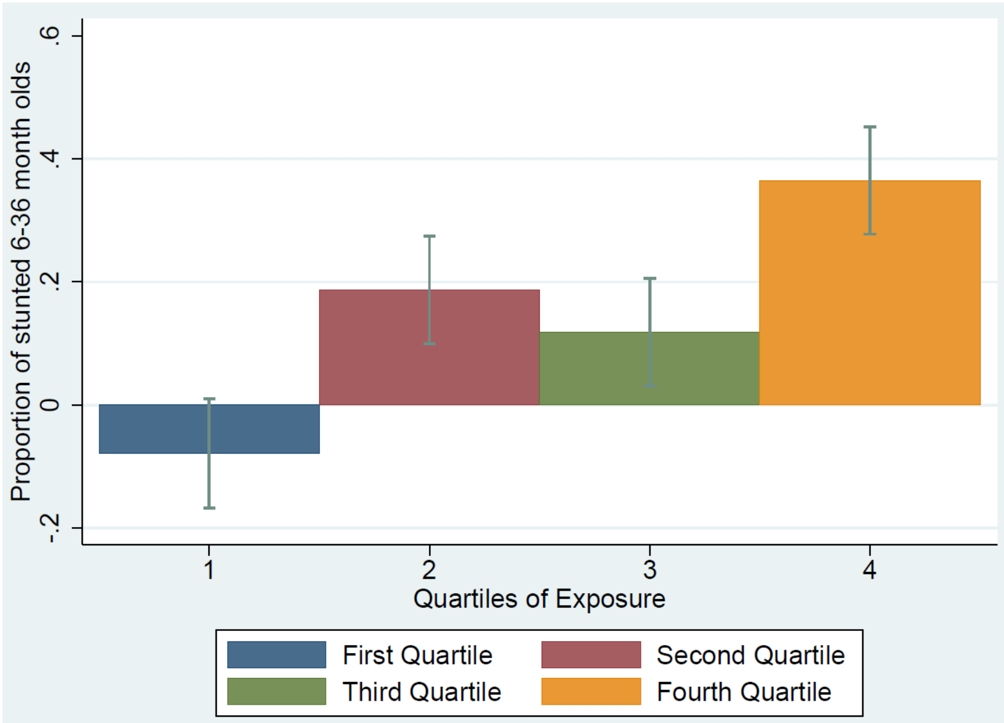

Figure 5: Difference in Stunting Rates (Treatment – Control) among Non-Beneficiaries, by Quartiles of Pantawid Saturation (or "Exposure")

Table 1: Impact of Pantawid on Beneficiary Children: Education and Anthropometry

	EDUCATIONAL OUTCOMES				
	Enrollment of 6-11 year olds	**Enrollment of 12-14 year olds**	**Attendance of 6-11 year olds**	**Attendance of 12-14 year olds**	
Program impact	0.044***	0.039	0.017**	0.025***	
	(0.014)	(0.024)	(0.007)	(0.008)	
Control Observations	770	398	692	324	
Treated Observations	792	411	764	356	
Control Mean	0.934	0.844	0.963	0.962	
	CHILD GROWTH OUTCOMES				
	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program impact	0.257*	-0.035	-0.093**	0.126	-0.024
	(0.152)	(0.052)	(0.041)	(0.150)	(0.048)
Control Observations	162	162	162	186	186
Treated Observations	181	181	181	201	201
Control Mean	-1.800	0.481	0.216	-1.207	0.280

note: *** p<0.01, ** p<0.05, * p<0.1

All specifications include linear controls for child age, and municipality fixed effects; standard errors are clustered at village level.

Table 2: Impact of Pantawid on Budget Share of Food for Beneficiary Households with Children Aged 6-60 months and Children's Food Intake

	Budget Share of Food	**Whether Eggs Were Fed to Child in Past Week**	**Number of Eggs Fed to Child in Past Week**	**Whether Meat Was Fed to Child in Past Week**	**Whether Fish Was Fed to Child in Past Week**
Program impact	-0.029**	0.082**	0.211	0.027	0.029
	(0.015)	(0.034)	(0.165)	(0.040)	(0.026)
Control Observations	328	405	402	406	406
Treated Observation	335	437	434	437	436
Control Mean	0.691	0.704	1.808	0.500	0.852

note: *** p<0.01, ** p<0.05, * p<0.1

All food intake specifications include linear controls for child age. All specifications include municipality fixed effects; standard errors are clustered at village level.

Table 3: Effect of Pantawid Saturation on Food Prices Using Administrative Data from 2007-2014

	LN(Prices)					
	Egg[1]	**Fish[2]**	**Chicken[3]**	**Rice[4]**	**Snacks[5]**	**Sugar[6]**
Saturation level	0.192***	0.152**	0.126**	0.022	0.083	0.023
	(0.05)	(0.051)	(0.055)	(0.047)	(0.052)	(0.045)
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	603	571	602	611	603	611
First Differenced Saturation						
Change in Saturation > 0	0.020**	0.001	0.004	0.009	-0.007	-0.001
	(0.009)	(0.010)	(0.009)	(0.010)	(0.008)	(0.013)
Change in Saturation > 0.05	0.016*	0.013	0.021**	0.008	0.004	0.002
	(0.009)	(0.009)	(0.009)	(0.010)	(0.008)	(0.013)
Change in Saturation> 0.10	0.017***	0.013**	0.012**	0.009	0.000	-0.010
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.008)
Change in Saturation> 0.15	0.011	0.013*	0.014*	0.007	-0.003	0.003
	(0.007)	(0.008)	(0.007)	(0.008)	(0.007)	(0.009)
Province Fixed Effects	No	No	No	No	No	No
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	640	608	608	648	640	648
Provinces	79	75	78	80	79	80
Mean Price (Philippine *pesos*)	5.388	118.949	133.175	24.911	8.161	38.092

note: *** p<0.01, ** p<0.05, * p<0.1

[1]Egg: price of one medium chicken egg

[2]Fish: price of one kilogram of milkfish (*bangus*)

[3]Chicken: price of one kilogram of mixed chicken parts

[4]Rice: price of one kilogram of standard white rice

[5]Snacks: price of one 60 gram foil pack of *Pancit Canton*

[6]Sugar: price of one kilogram of unbranded refined sugar

Table 4: Impact of Pantawid on Reported Village-level Food Prices

	LN(Egg Price Reported by Household)	LN(Rice Price Reported by Household)	LN(Sugar Price Reported by Household)
Program village	0.017 (0.014)	-0.000 (0.006)	0.000 (0.011)
Interaction with Saturation			
Program village	-0.011 (0.019)	0.003 (0.008)	-0.004 (0.015)
Above median saturation	0.007 (0.022)	0.013 (0.010)	0.005 (0.017)
Impact*Above median saturation	0.060** (0.027)	-0.007 (0.012)	0.009 (0.022)
Control Observations	65	65	65
Treated Observations	65	65	65
Control Mean Price (in Philippine *pesos*)	6.015	33.392	38.977

note: *** p<0.01, ** p<0.05, * p<0.1
All specifications include municipality fixed effects; standard errors are clustered at village level.

Table 5: Impact of Pantawid on Budget Share of Food for Non-Beneficiary Households with Children Aged 6-60 months and Children's Food Intake

	Budget Share of Food	Whether Eggs Were Fed to Child in Past Week	Number of Eggs Fed to Child in Past Week	Whether Meat Was Fed to Child in Past Week	Whether Fish Was Fed to Child in Past Week
Program impact	0.036*	0.041	-0.147	-0.054	0.007
	(0.021)	(0.038)	(0.170)	(0.046)	(0.037)
Interaction with Saturation					
Program village	0.033	0.124**	0.066	-0.047	0.006
	(0.024)	(0.049)	(0.220)	(0.059)	(0.050)
Above median saturation	0.032	0.099	0.181	-0.108*	-0.117**
	(0.041)	(0.067)	(0.311)	(0.065)	(0.052)
Program village*Above median saturation	0.007	-0.196**	-0.494	-0.013	0.004
	(0.044)	(0.077)	(0.348)	(0.095)	(0.068)
Control Observations	214	269	268	269	269
Treated Observation	230	295	292	293	292
Control Mean	0.596	0.755	2.131	0.665	0.818

note: *** p<0.01, ** p<0.05, * p<0.1

All food intake specifications include linear controls for child age. All specifications include municipality fixed effects; standard errors are clustered at village level.

Table 6: Impact of Pantawid on 6-36 month Old Non-Beneficiary Children's Anthropometry by Saturation

	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.397**	0.113*	0.042	-0.102	0.057
	(0.176)	(0.060)	(0.032)	(0.166)	(0.043)

	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Interaction with Above Median Saturation					
Program village	0.061	0.040	0.257	-0.046	-0.046
	(0.242)	(0.078)	(0.038)	(0.216)	(0.053)
Above median saturation	-0.397	0.074	0.048	0.066	-0.075
	(0.287)	(0.084)	(0.044)	(0.261)	(0.074)
Program village*Above median saturation	-0.196	0.117	0.006	-0.798**	0.231***
	(0.347)	(0.111)	(0.066)	(0.344)	(0.087)
Interaction with top quartile of saturation					
Program village	-0.244	0.041	0.012	0.127	0.018
	(0.196)	(0.062)	(0.034)	(0.180)	(0.047)
Fourth quartile of saturation	0.073	0.020	-0.009	0.498*	-0.125
	(0.366)	(0.093)	(0.067)	(0.273)	(0.087)
Program village*Fourth quartile of saturation	-0.740*	0.319**	0.139	-1.320***	0.245**
	(0.411)	(0.128)	(0.098)	(0.373)	(0.111)
Control Observations	145	145	145	156	156
Treated Observations	158	158	158	175	175
Control Mean	-1.124	0.317	0.069	-0.922	0.186

note: *** p<0.01, ** p<0.05, * p<0.1
All specifications include linear controls for child age, municipality fixed effects; standard errors are clustered at village level.

Table 7: Impact of Pantawid on Non-Beneficiary Children's Anthropometry by Age

	Height-for-Age Z score	**Stunted 6-60 month olds**	**Severely stunted 6-60 month olds**	**Weight-for-Age Z score**	**Underweight 6-60 month olds**
Program village	0.060	-0.001	0.024	-0.034	-0.031
	(0.154)	(0.071)	(0.042)	(0.121)	(0.046)
Dummy: 6-24 months old	-0.027	-0.207	-0.068	-0.348*	-0.047
	(0.234)	(0.156)	(0.078)	(0.184)	(0.112)
Dummy: 24-36 months old	-0.352	-0.063	-0.004	-0.259	0.042
	(0.219)	(0.108)	(0.052)	(0.164)	(0.080)
Program village*6-24 dummy	-0.702**	0.151	0.063	-0.073	0.199**
	(0.274)	(0.101)	(0.061)	(0.242)	(0.078)
Program village*24-36 dummy	-0.249	0.118	-0.039	-0.051	-0.034
	(0.262)	(0.094)	(0.064)	(0.225)	(0.082)
Control Observations	264	242	242	288	265
Treated Observations	291	265	265	314	287
Control Mean	-1.243	0.335	0.070	-0.884	0.181

note: *** p<0.01, ** p<0.05, * p<0.1
Age was defined in days, therefore the categories are not overlapping
All specifications include linear controls for child age, municipality fixed effects; standard errors are clustered at village level.

Table 8: Impact of Pantawid on the Household Composition of Non-Beneficiary Households with 6-60 Month Old Children

	Household Size	**Dependency Ratio (Children 0-15/Adults 15-59)**	**Dependency Ratio-Female (Children 0-15/Females 15-59)**	**Dependency Ratio-Male (Children 0-15/Males 15-59)**
Program village	-0.006	-0.026	-0.031	-0.013
	(0.148)	(0.045)	(0.087)	(0.090)
After	1.096***	-0.062***	-0.115***	-0.140***
	(0.092)	(0.019)	(0.032)	(0.034)
Program village*After	0.116	0.010	0.062	0.022
	(0.137)	(0.026)	(0.043)	(0.051)

	Household Size	**Dependency Ratio (Children 0-15/Adults 15-59)**	**Dependency Ratio (Children 0-15/Females 15-59)**	**Dependency Ratio (Children 0-15/Males 15-59)**
Program village	0.360*	0.033	0.088	0.128
	(0.196)	(0.060)	(0.121)	(0.128)
After	1.077***	-0.081***	-0.106**	-0.122**
	(0.124)	(0.025)	(0.048)	(0.048)
Above median saturation	-0.263	-0.065	-0.196	-0.169
	(0.227)	(0.069)	(0.127)	(0.136)
Program village*After	-0.030	0.008	0.051	-0.040
	(0.170)	(0.035)	(0.065)	(0.077)
Program village*Above median saturation	-0.830***	-0.131	-0.264	-0.314*
	(0.297)	(0.092)	(0.178)	(0.185)
After*Above median saturation	0.058	0.047	-0.014	-0.036
	(0.182)	(0.042)	(0.061)	(0.068)
Program village*After*Above median saturation	0.315	-0.001	0.010	0.121
	(0.282)	(0.051)	(0.083)	(0.096)
Control Observations	418	417	409	398
Treated Observations	451	447	445	433
Control Mean	4.488	0.625	1.208	1.189

note: *** p<0.01, ** p<0.05, * p<0.1
All specifications include municipality fixed effects; standard errors are clustered at the household level.

Table 9: Labor Force Participation of Non-Beneficiary Households with 6-60 Month Old Children

	MALES				FEMALES			
	Participates in labor force	**Participates in labor force for more than 40 hours per week**	**Works for Pay**	**Works for pay more than 40 hours per week**	**Participates in labor force**	**Participates in labor force for more than 40 hours per week**	**Works for Pay**	**Works for pay more than 40 hours per week**
Program impact	0.015	0.027	0.055	-0.023	0.003	0.055	-0.019	0.116
	(0.026)	(0.051)	(0.060)	(0.077)	(0.022)	(0.066)	(0.075)	(0.092)
Interaction with above median saturation								
	MALES				**FEMALES**			
	Participates in labor force	**Participates in labor force for more than 40 hours per week**	**Works for Pay**	**Works for pay more than 40 hours per week**	**Participates in labor force**	**Participates in labor force for more than 40 hours per week**	**Works for Pay**	**Works for pay more than 40 hours per week**
Program village	-0.006	-0.012	0.016	-0.044	-0.007	0.112	0.062	0.197*
	(0.026)	(0.064)	(0.074)	(0.100)	(0.024)	(0.077)	(0.083)	(0.105)
Above median saturation	-0.037	-0.128	-0.217***	-0.039	-0.040	0.060	-0.076	-0.005
	(0.050)	(0.082)	(0.081)	(0.110)	(0.035)	(0.107)	(0.101)	(0.145)
Program village* Above median saturation	0.050	0.095	0.088	0.050	0.023	-0.169	-0.193	-0.233
	(0.061)	(0.099)	(0.098)	(0.146)	(0.054)	(0.144)	(0.150)	(0.196)
Control observations	292	226	136	92	293	112	71	53
Treated observations	300	238	146	100	310	117	82	62
Control Mean	0.928	0.624	0.691	0.685	0.935	0.607	0.775	0.660

note: *** p<0.01, ** p<0.05, * p<0.1; all specifications include municipality fixed effects. Standard errors are clustered by village

Table 10: Education and Child Labor Impacts of Pantawid on Non-Beneficiary Children

	Enrollment of 6-11 year olds	**Enrollment of 12-14 year olds**	**Attendance of 6-11 year olds**	**Attendance of 12-14 year olds**	**Children 10-14 years old who worked in the last week**	**Children 10-14 years old who worked for pay in the last week**
Program impact	-0.004	0.007	-0.004	-0.001	0.031	0.010
	(0.010)	(0.018)	(0.006)	(0.010)	(0.027)	(0.018)
Interaction with Saturation						
Program village	-0.001	0.006	-0.010	-0.001	0.043	0.006
	(0.013)	(0.022)	(0.008)	(0.011)	(0.041)	(0.028)
Above Median Saturation	0.010	-0.025	-0.008	-0.005	0.016	-0.033
	(0.010)	(0.029)	(0.006)	(0.012)	(0.038)	(0.027)
Program village*Above Median Saturation	-0.007	0.002	0.015	-0.001	-0.031	0.012
	(0.018)	(0.039)	(0.011)	(0.019)	(0.049)	(0.030)
Control Observations	355	181	342	163	301	301
Treated Observation	341	185	332	176	295	297
Control Mean	0.986	0.950	0.985	0.985	0.103	0.033

note: *** p<0.01, ** p<0.05, * p<0.1

All specifications include linear controls for child age and municipality fixed effects. Standard errors are clustered by village

Table 11: Impact of Pantawid on Health Seeking Behavior of Households

	BENEFICIARIES				
	Pregnant women/last pregnancy in the last 36 months			***Children younger than 60 months old***	
	Number of ANC visits	**PNC within 24 hours**	**Skilled Birth Attendant**	**Growth monitoring**	**Treatment Seeking**
Program impact	0.596*	0.102**	0.024	0.157***	0.134***
	(0.346)	(0.049)	(0.061)	(0.033)	(0.039)
Control Observations	224	223	227	411	403
Treated Observations	238	241	242	443	436
Control Mean	4.147	0.296	0.449	0.175	0.409
	NON-BENEFICIARIES				
	Pregnant women/last pregnancy in the last 36 months			***Children younger than 60 months old***	
	Number of ANC visits	**PNC within 24 hours**	**Skilled Birth Attendant**	**Growth monitoring**	**Treatment Seeking**
Program village	-0.609**	0.000	-0.102*	0.019	0.002
	(0.308)	(0.054)	(0.058)	(0.032)	(0.043)
Interaction with Saturation					
Program impact	-0.319	-0.018	-0.096	0.057	-0.011
	(0.384)	(0.076)	(0.064)	(0.036)	(0.051)
Above median saturation	-0.830*	-0.029	-0.291***	-0.054	-0.123*
	(0.469)	(0.076)	(0.079)	(0.041)	(0.065)
Program Village*Above median saturation	-0.622	0.042	-0.004	-0.078	0.025
	(0.604)	(0.119)	(0.099)	(0.055)	(0.084)
Control Observations	174	178	180	341	328
Treated Observations	202	198	203	376	363
Control Mean	4.736	0.371	0.667	0.191	0.506

Appendix Table 1: Balance using baseline data: Category 1 households

Baseline survey variables	Control Mean	Difference (Treated-Control)	Standard Error for Difference	Kolmogorov (p-value)
LN Proxy Means Test Score	9.093	-0.014	0.020	0.574
Household composition:				
Household size	5.828	0.004	0.110	0.187
Children 5 years old and below	1.063	0.069	0.059	1.000
Children between 6 and 14 years old	1.718	-0.022	0.068	0.849
Primary occupation: Farming and livestock	0.685	0.040	0.041	0.458
Educational attainment of the household head:				
No grade completed	0.078	-0.002	0.018	1.000
Some elementary school	0.433	-0.006	0.033	1.000
Completed elementary school	0.214	0.014	0.026	1.000
Some high school	0.132	-0.029	0.018	0.979
High school graduate	0.100	0.004	0.019	1.000
Some college	0.031	0.008	0.011	1.000
College graduate	0.015	0.009	0.007	1.000
School Attendance:				
Children between 6 to 11 years	0.857	0.002	0.026	1.000
Children between 6 to 11 years	0.767	0.038	0.030	0.995
Housing Amenities:				
Strong roof materials	0.307	-0.041	0.029	0.122
Strong wall materials	0.191	-0.023	0.021	0.189
Light roof materials	0.478	0.050	0.031	0.038
Light wall materials	0.445	0.017	0.031	0.191
Owns a house and lot	0.345	-0.034	0.029	0.080
House has no toilet	0.388	-0.018	0.037	1.000
Shares a water source	0.200	-0.023	0.031	0.934
Household Assets:				
Electricity in house	0.407	0.021	0.034	1.000
Owns a television	0.203	-0.004	0.025	0.955
Owns a video recorder	0.090	-0.020	0.017	0.026
Owns a Stereo/CD player	0.100	-0.006	0.019	0.668
Owns a refrigerator	0.012	-0.004	0.005	0.847
Has a telephone/cellphone	0.072	-0.013	0.012	0.521
Owns a motorcycle	0.025	0.002	0.008	1.000

Note: *** p<0.01, ** p<0.05, * p<0.1; includes municipality fixed effects; standard errors clustered at the village level.

Appendix Table 2: Balance using baseline data: Category 2 households

Baseline survey variables	Control Mean	Difference (Treated-Control)	Standard Error for Difference	Kolmogorov (p-value)
LN Proxy Means Test Score	9.871	0.008	0.021	0.888
Household composition:				
Household size	4.240	-0.146	0.100	0.094
Children 5 years old and below	0.566	-0.015	0.038	1.000
Children between 6 and 14 years old	0.952	-0.037	0.061	0.678
Primary occupation: Farming and livestock	0.312	-0.000	0.034	1.000
Educational attainment of the household head:				
No grade completed	0.028	0.001	0.011	1.000
Some elementary school	0.230	0.029	0.029	0.872
Completed elementary school	0.171	-0.008	0.024	1.000
Some high school	0.135	-0.001	0.018	1.000
High school graduate	0.195	-0.018	0.022	0.999
Some college	0.122	-0.002	0.020	1.000
College graduate	0.121	-0.001	0.021	1.000
School Attendance:				
Children between 6 to 11 years	0.947	-0.008	0.021	1.000
Children between 12 to 14 years	0.873	-0.009	0.035	1.000
Housing Amenities:				
Strong roof materials	0.662	-0.046	0.033	0.263
Strong wall materials	0.552	-0.039	0.031	0.354
Light roof materials	0.232	0.027	0.029	0.703
Light wall materials	0.220	0.047*	0.025	0.221
Owns a house and lot	0.537	-0.030	0.033	0.691
House has no toilet	0.177	-0.036	0.029	0.970
Shares a water source	0.173	0.001	0.031	1.000
Household Assets:				
Electricity in house	0.797	0.001	0.040	1.000
Owns a television	0.570	-0.017	0.042	0.960
Owns a video recorder	0.400	-0.088**	0.035	0.007
Owns a Stereo/CD player	0.349	-0.052	0.032	0.404
Owns a refrigerator	0.312	-0.012	0.029	0.997
Has a telephone/cellphone	0.273	-0.054	0.033	0.319
Owns a motorcycle	0.173	-0.042*	0.023	0.579

Note: *** p<0.01, ** p<0.05, * p<0.1; includes municipality fixed effects; standard errors clustered at the village level.

Appendix Table 3: Balance using baseline data: Group 2 households in above median saturated areas

Baseline survey variables	Control Mean	Difference (Treated-Control)	Standard Error for Difference	Kolmogorov (p-value)
LN Proxy Means Test Score	9.816	0.003	0.022	0.242
Household composition:				
Household size	4.088	-0.429**	0.173	0.174
Children 5 years old and below	0.491	-0.047	0.049	0.995
Children between 6 and 14 years old	0.894	-0.078	0.092	0.837
Primary occupation: Farming and livestock	0.372	-0.036	0.063	0.652
Educational attainment of the household head:				
No grade completed	0.031	0.002	0.018	1.000
Some elementary school	0.231	0.059	0.049	0.952
Completed elementary school	0.200	-0.043	0.038	0.920
Some high school	0.124	-0.013	0.029	1.000
High school graduate	0.213	-0.032	0.038	0.989
Some college	0.107	0.008	0.031	1.000
College graduate	0.093	0.019	0.029	1.000
School Attendance:				
Children between 6 to 11 years	0.931	0.022	0.032	1.000
Children between 12 to 14 years	0.851	-0.037	0.065	1.000
Housing Amenities:				
Strong roof materials	0.611	-0.086	0.057	0.292
Strong wall materials	0.482	-0.079	0.054	0.411
Light roof materials	0.221	0.095*	0.055	0.709
Light wall materials	0.212	0.093**	0.042	0.797
Owns a house and lot	0.389	0.031	0.055	1.000
House has no toilet	0.208	0.009	0.041	1.000
Shares a water source	0.195	0.032	0.062	0.999
Household Assets:				
Electricity in house	0.735	0.036	0.034	1.000
Owns a television	0.562	-0.051	0.052	0.868
Owns a video recorder	0.389	-0.099*	0.054	0.098
Owns a Stereo/CD player	0.367	-0.068	0.056	0.564
Owns a refrigerator	0.398	-0.091**	0.040	0.588
Has a telephone/cellphone	0.279	-0.109**	0.051	0.240
Owns a motorcycle	0.204	-0.051	0.042	0.852

Note: *** p<0.01, ** p<0.05, * p<0.1; includes municipality fixed effects; standard errors are clustered at the village level.

Appendix Table 4: Balance using baseline data: Group 2 households in below median saturated areas

Baseline survey variables	Control Mean	Difference (Treated-Control)	Standard Error for Difference	Kolmogorov (p-value)
LN Proxy Means Test Score	9.911	0.023	0.028	0.193
Household composition:				
Household size	4.348	0.064	0.118	0.531
Children 5 years old and below	0.620	0.026	0.053	1.000
Children between 6 and 14 years old	0.994	0.023	0.084	0.992
Primary occupation: Farming and livestock	0.269	0.039	0.042	0.922
Educational attainment of household head:				
No grade completed	0.025	-0.007	0.012	1.000
Some elementary school	0.229	0.010	0.036	1.000
Completed elementary school	0.150	0.008	0.034	0.968
Some high school	0.143	0.004	0.022	1.000
High school graduate	0.182	-0.002	0.027	1.000
Some college	0.134	-0.008	0.028	1.000
College graduate	0.140	-0.004	0.032	1.000
School Attendance:				
Children between 6 to 11 years	0.956	-0.020	0.028	1.000
Children between 12 to 14 years	0.889	0.007	0.042	1.000
Housing Amenities:				
Strong roof materials	0.699	0.019	0.035	0.019
Strong wall materials	0.601	0.021	0.032	0.021
Light roof materials	0.241	-0.041	0.032	-0.041
Light wall materials	0.225	0.000	0.029	0.000
Owns a house and lot	0.642	-0.064	0.040	-0.064
House has no toilet	0.155	-0.063	0.039	-0.063
Shares a water source	0.158	-0.034	0.033	-0.034
Household Assets:				
Electricity in house	0.842	-0.007	0.046	0.943
Owns a television	0.576	0.016	0.052	1.000
Owns a video recorder	0.408	-0.063	0.042	0.112
Owns a Stereo/CD player	0.335	-0.029	0.037	0.958
Owns a refrigerator	0.250	0.046	0.035	1.000
Has a telephone/cellphone	0.269	-0.014	0.043	0.998
Owns a motorcycle	0.152	-0.039	0.027	0.961

Note: *** p<0.01, ** p<0.05, * p<0.1; includes municipality fixed effects; standard errors are clustered at the village level.

Annex 1: Robustness of results to data trimming

Given the relatively small number of children for whom we have anthropometric data, a concern may be that the estimated impacts of Pantawid on non-beneficiary children's height-for-age and weight-for-age are driven by outliers in the data. This annex explores this issue in further depth by considering varying thresholds at which to trim the data, using -3SD and 3 SD, -4SD and 4 SD, -6 and 6 SD, and -7 and 7SD as alternative thresholds instead of the WHO (2006) recommended -5 and 5 SD. These results, presented in Annex 1 Tables 1 through 3 show that the results presented above are completely robust for all three specifications discussed in Table 6 above—simple program impact, interaction with above median saturation, and the interaction with the fourth quartile of saturation. We also consider Winsorizing five and ten percent of the anthropometry data. As shown in Annex 1 Tables 4 and 5, the results are robust to this alternative method of treating the outliers in the anthropometry data as well.

Annex 1 Table 1: Varying Trimming Threshold

Trimming >-3 and <3 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.262*	0.090	0.008	-0.170	0.066*
	(0.139)	(0.058)	(0.008)	(0.145)	(0.040)
Control Observations	126	126	126	171	171
Treated Observations	156	156	156	183	183
Control Mean	-1.317	0.341	0.000	-1.035	0.222
Trimming >-4 and <4 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.335**	0.113*	0.045	-0.152	0.065
	(0.159)	(0.060)	(0.029)	(0.159)	(0.043)
Control Observations	151	151	151	184	184
Treated Observations	176	176	176	195	195
Control Mean	-1.667	0.450	0.166	-1.171	0.272
Trimming >-6 and <6 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.533***	0.129**	0.067*	-0.102	0.057
	(0.188)	(0.059)	(0.037)	(0.166)	(0.043)
Control Observations	167	167	167	188	188
Treated Observations	184	184	184	202	202
Control Mean	-1.903	0.497	0.240	-1.251	0.287
Trimming >-7 and <7 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.396*	0.129**	0.067*	-0.134	0.057
	(0.204)	(0.059)	(0.037)	(0.175)	(0.043)
Control Observations	172	167	167	189	188
Treated Observations	187	184	184	202	202
Control Mean	-2.035	0.497	0.240	-1.278	0.287

note: *** p<0.01, ** p<0.05, * p<0.1. All specifications include linear controls for child age, municipality fixed effects; standard errors are clustered at village level.

Annex 1 Table 2: Varying Trimming Threshold with Above Median Saturation Interaction

Trimming >-3 and <3 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.077	0.022	0.001	0.042	-0.011
	(0.178)	(0.074)	(0.002)	(0.178)	(0.049)
Above median saturation	-0.077	0.024	-0.004	0.042	-0.094
	(0.225)	(0.083)	(0.004)	(0.226)	(0.061)
Program village*Above median saturation	-0.415	0.152	0.017	-0.488*	0.180**
	(0.273)	(0.107)	(0.017)	(0.294)	(0.079)
Control Observations	126	126	126	171	171
Treated Observations	156	156	156	183	183
Control Mean	-1.317	0.341	0.000	-1.035	0.222

Trimming >-4 and <4 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.226	0.056	0.043	0.128	-0.028
	(0.211)	(0.077)	(0.031)	(0.203)	(0.052)
Above median saturation	-0.362	0.069	0.053	0.000	-0.062
	(0.250)	(0.084)	(0.036)	(0.251)	(0.074)
Program village*Above median saturation	-0.253	0.129	0.007	-0.626*	0.208**
	(0.306)	(0.110)	(0.055)	(0.330)	(0.087)
Control Observations	151	151	151	184	184
Treated Observations	176	176	176	195	195
Control Mean	-1.667	0.450	0.166	-1.171	0.272

Trimming >-6 and <6 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.349*	0.058	0.036	0.127	0.018
	(0.208)	(0.062)	(0.040)	(0.180)	(0.047)
Above median saturation	0.488	-0.005	-0.033	0.498*	-0.125
	(0.552)	(0.091)	(0.068)	(0.273)	(0.087)
Program village*Above median saturation	-1.079*	0.329**	0.159	-1.320***	0.245**
	(0.573)	(0.126)	(0.102)	(0.373)	(0.111)
Control Observations	167	167	167	188	188
Treated Observations	184	184	184	202	202
Control Mean	-1.903	0.497	0.240	-1.251	0.287

Trimming >-7 and <7 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.103	0.063	0.047	0.251	-0.046
	(0.282)	(0.075)	(0.046)	(0.216)	(0.053)
Above median saturation	-0.076	0.054	0.020	0.088	-0.075
	(0.315)	(0.082)	(0.050)	(0.268)	(0.074)
Program village*Above median saturation	-0.648	0.146	0.046	-0.853**	0.231***
	(0.396)	(0.110)	(0.075)	(0.359)	(0.087)

Control Observations	172	167	167	189	188
Treated Observations	187	184	184	202	202
Control Mean	-2.035	0.497	0.240	-1.278	0.287

note: *** p<0.01, ** p<0.05, * p<0.1. All specifications include linear controls for child age, municipality fixed effects; standard errors are clustered at village level.

Annex 1 Table 3: Varying Trimming Threshold with Fourth Quartile of Saturation Interaction

Trimming >-3 and <3 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.118	0.025	-0.000	-0.012	0.048
	(0.150)	(0.059)	(0.002)	(0.160)	(0.044)
Fourth quartile of saturation	0.181	0.020	-0.002	0.348	-0.097
	(0.346)	(0.089)	(0.005)	(0.259)	(0.075)
Program village*Fourth quartile of saturation	-0.774**	0.302**	0.043	-0.939***	0.136
	(0.385)	(0.127)	(0.042)	(0.308)	(0.090)
Control Observations	126	126	126	171	171
Treated Observations	156	156	156	183	183
Control Mean	-1.317	0.341	0.000	-1.035	0.222
Trimming >-4 and <4 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.153	0.035	0.011	0.057	0.029
	(0.174)	(0.062)	(0.028)	(0.174)	(0.047)
Fourth quartile of saturation	0.045	0.045	0.026	0.357	-0.098
	(0.364)	(0.094)	(0.062)	(0.261)	(0.085)
Program village*Fourth quartile of saturation	-0.836**	0.324**	0.139	-1.157***	0.218**
	(0.410)	(0.129)	(0.096)	(0.353)	(0.109)
Control Observations	151	151	151	184	184
Treated Observations	176	176	176	195	195
Control Mean	-1.667	0.450	0.166	-1.171	0.272
Trimming >-6 and <6 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.416**	0.068	0.034	0.121	0.013
	(0.203)	(0.061)	(0.040)	(0.177)	(0.047)
Fourth quartile of saturation	0.515	-0.009	-0.032	0.497*	-0.126
	(0.586)	(0.093)	(0.068)	(0.272)	(0.086)
Program village*Fourth quartile of saturation	-1.156*	0.341***	0.156	-1.323***	0.242**
	(0.611)	(0.128)	(0.101)	(0.373)	(0.111)
Control Observations	167	167	167	188	188
Treated Observations	184	184	184	202	202
Control Mean	-1.903	0.497	0.240	-1.251	0.287
Trimming >-7 and <7 SD	**Height-for-Age Z score**	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**

Program village	-0.194	0.058	0.036	0.071	0.018
	(0.227)	(0.062)	(0.040)	(0.199)	(0.047)
Fourth quartile of saturation	0.583	-0.005	-0.033	0.543*	-0.125
	(0.545)	(0.091)	(0.068)	(0.294)	(0.087)
Program village*Fourth quartile of saturation	-1.214**	0.329**	0.159	-1.237***	0.245**
	(0.564)	(0.126)	(0.102)	(0.397)	(0.111)
Control Observations	172	167	167	189	188
Treated Observations	187	184	184	202	202
Control Mean	-2.035	0.497	0.240	-1.278	0.287

note: *** p<0.01, ** p<0.05, * p<0.1. All specifications include linear controls for child age, municipality fixed effects; standard errors are clustered at village level.

Annex 1 Table 4: Five percent of data Winsorized

	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.337**	0.129**	0.067*	-0.143	0.057
	(0.161)	(0.059)	(0.037)	(0.157)	(0.043)
	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.134	0.063	0.047	0.191	-0.046
	(0.212)	(0.075)	(0.046)	(0.192)	(0.053)
Above median saturation	-0.133	0.054	0.020	0.095	-0.075
	(0.245)	(0.082)	(0.050)	(0.249)	(0.074)
Program village*Above median saturation	-0.449	0.146	0.046	-0.739**	0.231***
	(0.312)	(0.110)	(0.075)	(0.327)	(0.087)
	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.170	0.058	0.036	0.062	0.018
	(0.180)	(0.062)	(0.040)	(0.173)	(0.047)
Fourth quartile of saturation	0.277	-0.005	-0.033	0.490*	-0.125
	(0.330)	(0.091)	(0.068)	(0.265)	(0.087)
Program village*Fourth quartile of saturation	-0.904**	0.329**	0.159	-1.210***	0.245**
	(0.377)	(0.126)	(0.102)	(0.356)	(0.111)
Control Observations	172	167	167	189	188
Treated Observations	187	184	184	202	202
Control Mean	-1.981	0.497	0.240	-1.237	0.287

note: *** p<0.01, ** p<0.05, * p<0.1. All specifications include linear controls for child age, municipality fixed effects; standard errors clustered at village level.

Annex 1 Table 5: Ten percent of data Winsorized

	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**

Program village	-0.276**	0.129**	0.067*	-0.137	0.057
	(0.131)	(0.059)	(0.037)	(0.137)	(0.043)

	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.130	0.063	0.047	0.152	-0.046
	(0.168)	(0.075)	(0.046)	(0.164)	(0.053)
Above median saturation	-0.171	0.054	0.020	0.110	-0.075
	(0.195)	(0.082)	(0.050)	(0.215)	(0.074)
Program village*Above median saturation	-0.325	0.146	0.046	-0.642**	0.231***
	(0.248)	(0.110)	(0.075)	(0.284)	(0.087)

	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.132	0.058	0.036	0.041	0.018
	(0.143)	(0.062)	(0.040)	(0.150)	(0.047)
Fourth quartile of saturation	0.118	-0.005	-0.033	0.442*	-0.125
	(0.244)	(0.091)	(0.068)	(0.225)	(0.087)
Program village*Fourth quartile of saturation	-0.725**	0.329**	0.159	-1.059***	0.245**
	(0.302)	(0.126)	(0.102)	(0.303)	(0.111)
Control Observations	172	167	167	189	188
Treated Observations	187	184	184	202	202
Control Mean	-1.920	0.497	0.240	-1.237	0.287

note: *** p<0.01, ** p<0.05, * p<0.1. All specifications include linear controls for child age, municipality fixed effects; standard errors clustered at village level.

Annex 2: Robustness of results to differences in observable baseline population characteristics

As shown in Appendix Table 3, at baseline, non-beneficiary households in treated above median saturated areas had slightly smaller household sizes than non-beneficiary households in control above median saturated areas. Using differences-in-differences, we show in Table 8 that the household size and overall dependency ratio as well as with respect to male and female caregivers separately do not vary significantly between treated and control non-beneficiary households after treatment, including in above median saturated areas. However, we only examine a few of the available variables in Table 8, and it may still be that the lack of baseline balance at baseline is still driving the effects on non-beneficiary children's anthropometry. This annex further explores the robustness of the results presented in Table 6 to covariate balancing using the relatively rich set of observables from the baseline as well as linearly controlling for these observables. To balance observable characteristics, we estimate propensity scores as a function of baseline population characteristics, and then use these propensity scores to weight approximate what the outcome would have been if the treated population had exhibited the same baseline characteristics as the control population. Results presented in Annex 2 Table 1 are nearly identical to those presented in Table 6. An alternative to covariate balancing is to linearly control for baseline characteristics; the estimates from this approach, presented in Annex 2 Table 2 are also very close, although somewhat less precise, to those presented in Table 6; the slight lack of precision of these estimates is perhaps not surprising given the large number of control variables added to the specification. Taken together, the robustness tests presented in Annex 2 Tables 1 and 2 suggest that the lack of baseline balance in household size among above median saturated areas is not driving the effects of *Pantawid* on non-beneficiary children's anthropometry.

Annex 2 Table 1: Covariate Balanced Estimates of Pantawid Impact on Non-Beneficiary Children's Anthropometry

	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.388**	0.104*	0.040	-0.064	0.044
	(0.180)	(0.061)	(0.033)	(0.174)	(0.044)

	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Interaction with Above Median Saturation					
Program village	-0.294	0.049	0.033	0.294	-0.072
	(0.242)	(0.247)	(0.082)	(0.042)	(0.220)
Above median saturation	-0.434	0.072	0.035	0.101	-0.088
	(0.303)	(0.095)	(0.050)	(0.286)	(0.074)
Program village*Above median saturation	-0.206	0.122	0.016	-0.785**	0.255***
	(0.356)	(0.117)	(0.068)	(0.359)	(0.085)
Interaction with top quartile of saturation					
Program village	-0.227	0.028	0.010	0.157	0.002
	(0.198)	(0.063)	(0.034)	(0.185)	(0.048)
Fourth quartile of saturation	-0.001	0.010	-0.020	0.604**	-0.139
	(0.347)	(0.096)	(0.064)	(0.280)	(0.086)
Program village*Fourth quartile of saturation	-0.730*	0.343***	0.147	-1.338***	0.264**
	(0.392)	(0.126)	(0.092)	(0.381)	(0.116)
Control Observations	145	145	145	156	156
Treated Observations	158	158	158	175	175
Control Mean	-1.124	0.317	0.069	-0.922	0.186

note: *** p<0.01, ** p<0.05, * p<0.1. All specifications include linear controls for child age, municipality fixed effects; standard errors are clustered at village level.

Annex 2 Table 2: Estimates of Pantawid Impact on Non-Beneficiary Children's Anthropometry with Linear Controls for Observable Household Characteristics at Baseline

	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Program village	-0.451**	0.126**	0.049	-0.134	0.044
	(0.183)	(0.064)	(0.034)	(0.165)	(0.046)

	Height-for-Age Z score	**Stunted 6-36 month olds**	**Severely stunted 6-36 month olds**	**Weight-for-Age Z score**	**Underweight 6-36 month olds**
Interaction with Above Median Saturation					
Program village	-0.394	0.096	0.067	0.207	-0.060
	(0.242)	(0.265)	(0.088)	(0.045)	(0.234)
Above median saturation	-0.449	0.089	0.080	-0.020	-0.075
	(0.320)	(0.093)	(0.050)	(0.287)	(0.086)
Program village*Above median saturation	-0.122	0.065	-0.039	-0.725**	0.226**
	(0.373)	(0.124)	(0.073)	(0.354)	(0.097)
Interaction with top quartile of saturation					
Program village	-0.342	0.066	0.020	0.096	0.006
	(0.217)	(0.071)	(0.037)	(0.188)	(0.053)
Fourth quartile of saturation	-0.006	0.016	-0.016	0.619**	-0.156*
	(0.333)	(0.108)	(0.068)	(0.277)	(0.091)
Program village*Fourth quartile of saturation	-0.470	0.251*	0.136	-1.333***	0.247**
	(0.413)	(0.148)	(0.099)	(0.408)	(0.117)
Control Observations	145	145	145	156	156
Treated Observations	158	158	158	175	175
Control Mean	-1.124	0.317	0.069	-0.922	0.186

note: *** p<0.01, ** p<0.05, * p<0.1
All specifications include linear controls for child age, municipality fixed effects; standard errors are clustered at village level.

Annex 3: Remoteness and Prices

This paper presents evidence suggesting that the increased cash flow from *Pantawid* in highly saturated villages led to the increase in prices of non-tradable perishable foods, for which markets are not as well integrated as for tradables. If this hypothesis is correct, we would expect to see the greatest increase in reported prices for remote highly saturated villages. We are able to examine the relationship between distance and price increases using the unit values of individual food goods reported by the survey respondents. Annex 3 Table 1 presents the estimates of *Pantawid* impact on unit values for treated and control villages that are above median saturation as well as above median distance to the municipal capital. As we only have 130 villages in the sample, the estimates are largely imprecise, but suggest that price increases are indeed somewhat larger for treated villages that are above median saturated and above median distance to the municipal capital.

Annex 3 Table 1: Remoteness and Prices

	LN(Price of Egg Reported by Household)	**LN(Price of Rice Reported by Household)**	**LN(Price of Sugar Reported by Household)**
Program village	-0.004	0.012	0.037*
	(0.027)	(0.012)	(0.022)
Above median saturation	0.024	0.010	0.004
	(0.027)	(0.012)	(0.022)
Above Median Distance	0.073**	0.006	0.050**
to Municipal Capital	(0.031)	(0.014)	(0.025)
Impact*Above median saturation	0.036	-0.016	-0.030
	(0.036)	(0.016)	(0.028)
Treated*Above Median	-0.010	-0.014	-0.071**
Distance	(0.037)	(0.017)	(0.029)
Above Med. Sat.*	-0.046	0.006	0.012
Above Med. Distance	(0.041)	(0.019)	(0.033)
Treatment*Above Med. Sat. *Above	0.065	0.015	0.054
Med. Dist.	(0.057)	(0.026)	(0.045)
Control Observations	65	65	65
Treated Observations	65	65	65
Control Mean	6.015	33.392	38.977

Note: *** p<0.01, ** p<0.05, * p<0.1; all specifications include municipality fixed effects; standard errors clustered at village level.