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Cash Transfers, Food Prices, and Nutrition Impacts on Nonbeneficiary Children

Deon Filmer
Jed Friedman
Eeshani Kandpal
Junko Onishi

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Abstract

Data from a randomized evaluation show that a household-targeted Philippine cash transfer program significantly raised the local prices of perishable protein-rich foods while leaving other food prices unaffected. The price changes are largest in areas with the highest program saturation, where the shock to village income is on the order of 15 percent, and persist more than 2.5 years after program introduction. Through this shift in relative prices, the cash transfer increased the stunting rate of young non-beneficiary children by 11 percentage points, with the greatest increases in the most saturated areas. Failing to consider such local market effects can overstate the net benefits of targeted cash transfers. In areas where individual targeting of social programs covers the majority of the households, offering the program on a universal basis may avoid such negative impacts at moderate additional cost.

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Cash Transfers, Food Prices, and Nutrition Impacts on Nonbeneficiary Children[1]

Deon Filmer, Jed Friedman, Eeshani Kandpal, Junko Onishi

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[1] All authors are with the World Bank. Corresponding author: ekandpal@worldbank.org.

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I. INTRODUCTION

Conditional and unconditional cash transfer programs reach 380 million people in developing countries people around the world (World Bank 2014). While an extensive literature has considered the direct and indirect effects of such programs (Fiszbein and Schady 2009; Baird et al. 2013; Saavedra and Garcia 2012; Hanlon, Barrientos, and Hulme 2010), the study of possible general equilibrium effects has focused on positive channels that operate through the labor market (Muralidharan, Niehaus, and Sukhtankar 2017) or informal insurance and credit markets (Angelucci and De Giorgi 2009). A notable exception is work by Cunha, De Giorgi and Jayachandran (2018), which documents price increases in local markets from a cash infusion into the local economy. The average price increases are modest and do not affect purchasing power but are significantly larger for remote communities, where prices of basic food goods increase on the order of 6 percent. Our paper also identifies local food price increases from an anti-poverty program, but only for perishable and less-easily traded protein rich food goods. We then go on to show that these price changes have large impacts on the health of young children. Specifically, children of nonbeneficiary households have significantly worse nutritional outcomes, an effect that we can trace to a decline in their protein intake following the program. We interpret our results through a conceptual framework that distinguishes between the relative price effects of cash transfers on easily traded and less easily traded foods. [2]

Our paper ties local price effects of cash transfers to welfare outcomes among nonbeneficiaries.[3] We do this using household-level data on health, nutrition, and food prices that

[2] Broadly speaking, we use the terms tradable and non-tradable to distinguish goods that have relatively low versus high transportation and storage costs. We define the terms tradable and non-tradable precisely below.

[3] The unintended consequences of cash transfers have more broadly been studied at three levels: (1) the beneficiary households themselves; (2) participating schools, hospitals and other facilities; and (3) local markets. Within the household, evidence generally suggests the presence of positive spillovers through the information channel on nontargeted siblings' education and work-for-pay, as well as adult health (Ferreira, Filmer, and Schady 2017; Contreras and Maitra 2013). Evidence also suggests that nontargeted outcomes for beneficiaries may be affected in unexpected ways: Bourguignon, Ferreira and Leite (2003) and de Hoop, Friedman, Kandpal, and Rosati (2019) show that conditional cash transfers (CCTs) can increase schooling and work-for-pay by the same children. Within participating facilities, there is robust evidence of peer effects-driven increases in schooling enrollment of nontargeted populations, at least in the case of *PROGRESA/Oportunidades* (Bobba and Gignoux 2019; Bobonis and Finan 2009; Lalive and Cattaneo 2009). Within affected areas, evidence suggests a variety of positive externalities, including improvements in mental health in Kenya (Haushofer and Shapiro 2016) and reductions in crime and political violence in the Philippines (Crost, Felter and Johnston 2014). Finally, there is some evidence that *Pantawid* increased clientelism, which in turn improved political stability (Labonne 2013). In contrast, there is also evidence in other settings of a complete absence of externalities: using data from a Nicaraguan CCT, Macours, Schady and

we collected, and village-level data from the rollout of the Philippine cash transfer program, *Pantawid Pamilya Pilipino Program*. Our identification is as follows. First, we use village-level randomization of the program to estimate its impact on beneficiary households' food budget shares, and child nutrition, education, and food intake.[4] After establishing a demand increase for nutritious foods among beneficiary households, we compare prices in treated and control villages to show that the program increased the prices of key protein rich foods. We show that this effect on prices is larger when saturation is high, where saturation is the proportion of potentially eligible households at baseline in both treated and control villages. Our identification thus leverages the randomized treatment assignment of the program in similarly poor treated and control villages. We confirm that these patterns in the experimental sample are consistent with nationally representative data by comparing village-level unit values for a range of commonly consumed food items as measured in national household expenditure surveys across the phased 6-year rollout of the program.[5]

Our results show that the cash influx increased aggregate income in the treatment villages by 9 percent. This increase in cash resources did not affect the prices of storable and easily traded food goods but increased the local prices of protein-rich perishable foods, such as eggs and fresh fish, by 6 to 8 percent.[6] The rise in relative prices in turn decreases nonbeneficiaries' real income and leads them to substitute away from protein-rich foods, thus creating significant negative effects

Vakis (2012) do not find any evidence of externalities on nonbeneficiaries. Beegle, Galasso and Goldberg (2017) find that a large-scale public-works program in Malawi significantly worsened food security for nonbeneficiary households in treated villages. The authors rule out prices, as well as crowding out of traditional risk-sharing mechanisms, as channels for the impact but are otherwise unable to pinpoint the exact channels. In recent work with Give Directly in Kenya, a study by Egger et al. tests the effects of transfers on prices and wages (Haushofer et al. 2019). However, the context that they study includes lower levels of saturation than the present study and many proximate markets, thus expanding the set of market conditions covered by existing experiments.

[4] As the program is targeted at the household level, eligibility is set through a Proxy Means Test that estimates household resources on the basis of a limited number of observed characteristics. We estimate direct effects on eligible households by comparing across households *below* the treatment threshold in treated and control villages. Spillovers on nonbeneficiaries are calculated by comparing households *above* the treatment threshold treated and control villages. The extent to which a village is defined as a natural market is an open question. Markets in treated and control villages might interfere, and we might be missing even more negative spillovers occurring outside the local area. Both channels would mean that the spillovers we capture, if anything, are a lower bound.

[5] Unit values are defined as the reported expenditure on a particular commodity or commodity group divided by the reported quantity consumed. While unit values combine price effects and quality effects, many of the key goods we examine, including regular commercially-milled rice and eggs, are fairly homogeneous with relatively little scope for quality differentiation.

[6] This magnitude is consistent with the estimates reported by Cunha, De Giorgi, and Jayachandran (2018). Del Boca, Pronzato and Sorrenti (2018) also show, with evidence from Italy, that a CCT increased beneficiaries' demand for more nutritious foods.

on the nutritional status of children in nonbeneficiary households. Young nonbeneficiary children experienced a 0.4 standard deviation decrease in height-for-age z-scores (relative to a control mean of -1.1 standard deviations), which led to a 12-percentage point increase in stunting (relative to a control mean of 32 percent). Children in beneficiary households, which were compensated for the price change by the increase in household income, show— if anything— a gain in nutritional status.

The price increases for non-tradable goods and reductions in child growth were the greatest in villages where over 65 percent of the households were eligible for the transfers (the median saturation level in the study sample), consistent with our interpretation of the stunting increase as an unintended spillover effect of the program. In these villages, the transfers amounted to an inflow of approximately 15 percent of village income and impacts on nonbeneficiary child growth were consequently more severe. The magnitude of the aggregate income transfer and the share of beneficiaries in the local population will be important factors in determining the extent to which local prices shift.

Perhaps surprising at first glance, these impacts on child growth translate to estimated elasticities very much in line with the broader literature, which we discuss in detail later. Our results are also consistent with a nascent literature that highlights the particularly important role of animal-sourced proteins in avoiding child stunting (Heady, Hirvonen, and Hoddinott 2018). In fact, using estimates from the nutrition literature of the elasticity of child height to eggs (Puentes et al. 2016; Iannotti et al. 2017), we show that the observed reduction in the consumption of eggs alone would explain most of the observed decline in height-for-age.

We consider and rule out several alternative explanations for the observed increases in prices and child stunting rates, including the availability of adult care-givers, changes in household composition, and changes in adult and older child productive activity, all of which can affect child health independently of food prices. Further, the observed household consumption patterns are consistent with increases in real income for beneficiaries and decreases for nonbeneficiaries. Beneficiary households spend more on nutrient-rich foods than do nonbeneficiary households (as typically found in CCT programs (Fiszbein and Schady 2009)) and nonbeneficiary children consume less of the expensive protein-rich items, compared with households of similar income levels in control villages. A related local market spillover may arise through greater demand for health services by beneficiaries and reduced health service utilization by nonbeneficiaries. This would be consistent with evidence from an Indonesian cash transfer program that raised prices for health services (Triyana 2016). We indeed find such an effect in the Philippines, but the effects do not vary with saturation, suggesting that it is unlikely to be the only or even the most important causal channel driving the nutrition impacts we document.[7]

The Filipino environment and program present a combination of conditions that allow us to explore spillovers. It is a country with relatively high stunting rates alongside spatial variation in market integration and poverty levels. The RCT allows us to persuasively identify direct and indirect impacts. Further, the *Pantawid* program was rapidly scaled up from 2009 to 2015, which creates village-level variation in prices and saturation that allows us to corroborate the experimental findings with national data. As the experimental study area comprises some of the poorest and most remote parts of the country, the setting allows us to analyze the impact of cash on local markets largely not integrated with the national or regional production base. This is important since the conditions that lead to negative spillovers are not universal—only 4.2 percent of Philippine villages in 2015 reached program coverage levels of 65 percent (the median coverage level in the experimental sample). Nevertheless, this fragmentation of local markets that foster persistent price differences is not exceptional to what one finds in many low-income countries: Atkin and Donaldson (2015) find that the effect of log-distance on trade costs within Ethiopia or Nigeria is four to five times larger than in the US, suggesting that transportation costs significantly restrict spatial price convergence in poor countries. Related studies on the construction of new roads find similar market-level impacts in India (Aggarwal 2018) and Vietnam (Mu and van de Walle 2011).

This paper makes four contributions. First, we identify local price increases from a cash transfer program; this has previously only been documented in Cunha, De Giorgi and Jayachandran (2018). Second, we discuss the conditions under which these price responses arise as they are not observed to be universal in our study setting. Third, we show that these price increases are restricted to a certain class of goods, namely perishable goods with high transport costs. These goods are largely animal-based protein rich foods that recent literature indicate to be a critical input in the production of child height. Fourth, we link these price increases to deleterious

[7] Access to government services, including health clinics, electricity, running water does not vary by saturation, suggesting that our results are not driven by differential access to these services.

and presumably long-lasting health impacts among young children. Since we are able to tie the increase in stunting to a change in the relative price of proteins, the fourth contribution highlights the role of this nutrient for early life growth.

Given these long-term consequences, program design and the choice of benefit targeting mechanism should consider whether local markets are integrated with surrounding regions. This is especially important when a large proportion of the local population is eligible and thus the aggregate increase in local income is broad-based enough to shift local prices. In such situations, a geographic or community-based targeting rule may mitigate the negative impacts documented here.

II. CONCEPTUAL FRAMEWORK

To illustrate how price effects might arise from a cash transfer program, we present a framework that begins with the basic conjecture that an exogenous increase in resources in a local market (village) increases demand for normal goods, including many food goods. In a large integrated economy, a demand shift in either one or a handful of local areas should not significantly affect prices as the local demand increase is too small to influence aggregate demand. However, if markets are not integrated then the local market structure determines whether local demand changes affect local prices. The shape of the local supply curve and the size of the demand shock determine the resulting local equilibrium price level. The supply response to a demand increase may be constrained for a variety of reasons. First, poorer villages tend to suffer from high transport costs for imported goods as the villages tend to be more remote, and the transport cost wedge may offset any marginal gain in profit from importing units to sell at a marginally higher price. Second, if local production markets are oligopolistic (perhaps due to a fixed cost of entry) or competitive but with upward sloping marginal costs, then price increases will also likely arise from a demand shift.

Central to our analysis is the observation that local market characteristics, including characteristics of the local production base and the degree of integration of the local producers with wider networks of producers and consumers, will vary with the type of traded good. Perishable protein rich foods have higher transport and storage costs than storable food goods and thus the local producers and consumers of these goods may be less integrated with national suppliers. Consequently, the price behavior of these goods can diverge from that of more easily traded goods when faced with a shift in local demand.

This conceptual framework is closely related to Cunha, De Giorgi and Jayachandran (2018) who show that cash transfers to rural Mexican households cause a positive but slight increase in the price of a homogenous packaged food good sold by local suppliers. In their context, this price increase is particularly marked in areas where supply responses are constrained by remoteness, which is a proxy for a more closed or uncompetitive local economy. We add to this framework the notion of markets bifurcated by the nature of the good and posit that cash transfers are most likely to affect prices in goods markets where producers are not fully integrated with the broader national production base. This is especially true in poorer and more rural parts of developing countries that rely more on local sourcing.

Price Responses to Increases in Demand

Income gains from cash transfers will increase demand for normal goods as by definition the income elasticities of demand for these goods are positive. Additionally, demand for nutritious foods may become even more elastic since anti-poverty programs such as conditional cash transfers (CCTs) typically broadcast messaging on recommended child feeding practices that can further shift demand. If supply does not fully respond to this demand shift, then prices will rise. Since any potential price change is related to the magnitude of the increase in demand, program saturation will be a relevant program feature. The higher the proportion of beneficiaries in the local economy, the greater the increase in aggregate demand and hence the greater potential for relative price change.

When an increase in demand for a good occurs in a competitive market with constant marginal production costs, then the increased quantity demanded will be accommodated without a change in price. However, even a competitive goods market will translate a demand shift into higher equilibrium prices if the marginal cost of production increases with quantity. Alternatively, if the goods market is supplied by a limited number of producers, then oligopolistic competition can translate a demand increase into higher consumer prices. Consider, for example, a Cournot model with N producers of a homogenous good. We model total quantity demanded, Q, as:

$$Q = F(p, X)$$

where p is the good's price and X a demand shifter. If the demand function is additive in X, such that $Q = g(p) + X$, then the Cournot-Nash solution can be written as follows:

$$\frac{\partial p}{\partial X} = -\frac{1}{N\left(\frac{\partial g(p)}{\partial p}\right)} > 0$$

In other words, a demand shift will raise the price, while the magnitude of the increase depends on the shape of the demand curve and the number of producers in the market (which in turn is partly a function of the fixed cost of entry for potential competitors). This basic result holds for any normal good demand function with an additive shifter X.

Local Market Structure Interaction with Goods Characteristics

We now apply the general framework above to the specific setting of an isolated local market. The same logic applies: if the local market is competitive with constant marginal costs, then prices will not change with an increase in local demand. If instead marginal costs increase in total quantity or the local market is not perfectly competitive, then prices can rise alongside a rise in local demand.

The key difference for a local market is that the transport and storage costs of a good drive a wedge between the local and (presumed competitive) national price for that good. If the national competitive price of a good is p^n, and the import cost into the local economy is δ, then any local good's price, p^l, as determined solely by local market forces, can be sustained as long as

$$p^l \leq p^n + \delta$$

When the local price exceeds the expression to the right of the inequality above, then arbitrage opportunities arise that would compensate for the cost of importation into the village – the local price of the good stabilizes at the national price plus the import and storage cost. This good-specific import and storage cost determines the "tradability" of the good between the local and national markets. For goods whose δ is relatively low, the local market price will never substantially deviate from the national competitive price. However, there is far more scope for local market price deviations for perishable goods, such as fresh eggs or fish, that would need relatively expensive dedicated technologies for transport and storage. For convenience, we term such goods, where $\delta >> 0$, "non-tradables," and conversely "tradables" are goods for which $\delta \approx 0$. Therefore, in addition to program saturation and market structure, the tradable nature of the specific good in question is another factor that should determine the price response.

Imperfect competition is possibly a more realistic model for the local markets of many non-tradables, given the smaller number of local producers for certain goods and the fixed costs to entry in these markets (such as the purchase and maintenance of livestock or fishing boats). We apply the imperfect competition framework above, with a linear shifter in the demand function, to local markets bifurcated by the type of the good, where we index a good by T or NT depending on whether the homogenous good is a tradable or non-tradable good. In this case, the effect of increases in demand (i.e., $\partial X_T > 0$ and $\partial X_{NT} > 0$) on the equilibrium price will depend on the nature of the good and the initial local price level relative to the national price and import cost. For tradables, we have

$$\frac{\partial p_T}{\partial X_T} = 0, p_T^l \cong p_T^n + \delta$$

where we assume that import costs are low enough for these tradable goods to be bought from and sold to large integrated markets. For non-tradables that have a higher cost of import δ,

$$\frac{\partial p_{NT}}{\partial X_{NT}} = -\frac{1}{N_{NT}\left(\frac{\partial g_{NT}(p_{NT})}{\partial p_{NT}}\right)} > 0, p_{NT}^l < p_{NT}^n + \delta$$

or

$$\frac{\partial p_{NT}}{\partial X_{NT}} = 0, p_{NT}^l \cong p_{NT}^n + \delta$$

While arbitrage limits the magnitude of the change in the non-tradable good's price from a demand shift, the price increase may still be enough to affect consumption choice if δ is large enough.[8]

In this framework, a price increase for non-tradables can be sustained indefinitely if the new equilibrium local price does not exceed the national price plus import cost. Cunha, De Giorgi,

[8] For simplicity, we assume no cross-price elasticities between tradables and non-tradables. The basic intuition of differential price changes by goods type holds in a fuller model that allows for the two types of goods to be complements or, more likely in this context, substitutes.

and Jayachandran (2018) find that the price effects of cash transfers persist when measured after 22 months; our results are measured after 31 months of program exposure, suggesting that, even after a considerable amount of time, local supply responses to offset the demand increase from the cash transfer do not return the local price to the pre-program level.

Since 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, we cannot incorporate information about the structure of local markets into our analysis. This framework is used only for illustrative purposes; our empirical analysis focuses on prices pre- and post- program introduction and investigates the degree of good-specific price change and how the magnitude of change is correlated with factors likely to cause a relative price rise in isolated markets. These factors include the degree of program saturation and the degree of tradability of the particular good.

III. THE CASH TRANSFER PROGRAM

The *Pantawid* cash transfer program has been in place since 2008 and is targeted to individual households based on a proxy means test for household income with eligibility cutoffs determined by province-specific poverty lines. Starting with an initial pre-pilot of 6,000 households, the program reached approximately 4.5 million households by 2015 (DSWD 2015). For comparison, the Indonesian *Program Keluarga Harapan* covered 1.5 million households after five years (Nazara and Rahayu 2013) and the fully scaled-up Mexican *PROGRESA/Oportunidades* program covered 5.8 million households (World Bank 2014).

The program gives eligible households cash transfers if the households enroll children in school and use maternal and child health services. Eligibility is granted if a household not only has a proxy means test score below the provincial poverty line but also contains children ages 0 to 14 years or a pregnant woman at the time of assessment. Eligible households receive a combination of health and education grants every two months, ranging from ₱500 to ₱1,400 (approximately US$11 to US$32) per household per month, depending on their number of eligible children and compliance with program conditions.[9] The expected transfer size equaled approximately 23 percent of per capita beneficiary consumption. By way of comparison, the Mexican CCT *PROGRESA* transfers were about 22 percent of beneficiary consumption, while those from the Brazilian CCT, *Bolsa Família*, were about 12 percent (Fiszbein and Schady 2009).

A village-level randomized evaluation, stratified by eight purposively selected municipalities in four provinces, was designed for the pilot phase of *Pantawid* to inform program scale-up (Chaudhury, Friedman, and Onishi 2013). This phase covered some of the poorest areas of the country. The Household Assessment Form to estimate proxy means scores for beneficiary selection was fielded in these eight municipalities between October 2008 and January 2009; we use these data to calculate potential program saturation at baseline. A total of 130 villages in the eight municipalities were then randomly assigned to treatment or control status with equal probability. The following section explores balance across treatment assignment overall and within different saturation levels.[10] A follow-up survey was conducted in October and November 2011, thus allowing for a program exposure period of 30 to 31 months.

IV. EMPIRICAL STRATEGY AND DATA

Empirical Strategy

Our analysis uses variation across villages in treatment assignment to identify program impact on local prices and child nutrition. To study the effects of these price changes on household behavior, we analyze the direct program impacts among eligible households and then estimate the

[9] Poor households with children ages 0 to 14 years or pregnant women receive a lump sum health grant of ₱500 (about US$11) per household per month if (i) all children under age five attend growth monitoring visits at the local health center; (ii) pregnant women seek regular antenatal care; (iii) school-age children (6 to 14 years) accept school-based deworming; and (iv) a household member attends monthly health and nutrition workshops. Households can also receive a monthly education transfer of up to ₱300 (about US$6.50) per child (for three or fewer children) enrolled in and attending at least 85 percent of school days in primary or secondary school for the duration of the school year.

[10] The experimental sample selection occurred in three stages. First, provinces that did not have the program as of October 2008 were selected. Three of the 11 available provinces were excluded due to security concerns. Four of the remaining eight provinces were chosen (Lanao Del Norte, Mountain Province, Negros Oriental, and Occidental Mindoro) to span the three macro areas of the country (North, Visayas, and Mindanao). Second, in each province, two municipalities were selected to represent the average poverty level of areas covered by the program in 2008. Third, villages in each municipality were randomly assigned to treatment or control by a computer based random number generator.

spillover effects among nonbeneficiary households, defined as non-poor households that nonetheless have eligible children. Since potential program saturation is a key mediator of the cash transfer's impact on prices of non-tradable goods, we allow the indirect effect to vary by program saturation. A secondary analysis exploits the staggered rollout of the program in a national sample of villages to estimate the covariation of food prices with village-level program exposure.

Data

We use four data sources in our analysis. Our primary data source is the specialized household survey data from the randomized evaluation of *Pantawid* that contains information on consumption, select food prices, child anthropometry, health care utilization, labor supply, demographic composition and other behavior. We also use village-level administrative data on program saturation, data on unit values of food prices from the national household budget survey, as well as the Household Assessment Form that determined program eligible households in 2008.

<u>Household Data from the 2011 *Pantawid* Impact Evaluation</u>

2,555 households were surveyed in the eight study municipalities during the follow-up survey.[11] With an eye toward investigating potential spillovers on nonbeneficiary households, the study population included 1,418 households that were eligible for the transfers, as well as 1,137 households that had proxy means test scores *above* the provincial threshold but had age eligible children or pregnant women. For anthropometric outcomes, we focus on children ages 6 to 36 months at the time of the follow-up survey, while for food intake, we consider children aged 6 to 60 months. Further details on sample sizes, program take up, anthropometric measures, and the robustness of our results to trimming cutoffs for the anthropometric measures are presented in Annex 1.

<u>Program Saturation</u>

We have comprehensive village-level data on the number of households enrolled in *Pantawid* each year from 2009 to 2015 as reported by the provincial office of the Department of Social Welfare. These data allow us to calculate annual village level program saturation, defined

[11] Household-level attrition from the baseline sample was 11.4 percent (80 of 624 households) in treated villages and 11.2 percent (80 of 634 households) in control villages, with no evidence of a significant difference by treatment status (Chaudhury, Friedman, and Onishi 2013).

as the number of beneficiary households for that year divided by census bureau estimates for the total number of households in the village in 2015. This measure will be used in concordance with the food unit value information from the household budget survey.

We also calculate program saturation for the villages in our experimental survey sample as the number of households with a proxy-means test score below the province-specific poverty threshold divided by the village population. 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 villages, a high proportion of the total population was eligible to receive program benefits. Figure 1 presents a histogram of this proportion of eligible households from among all households in the village for the treatment and control villages. Although there is a good degree of dispersion in this saturation measure, some villages have up to 90 percent of the household population eligible to receive benefits. The median village saturation level in the experimental sample is 65 percent, and the mean is 62 percent.

Household Budget Survey Data

We use the 2009, 2012, and 2015 rounds of the Philippine national household budget survey, the Family Income and Expenditure Survey (FIES), which collected detailed consumption data in 8,500 villages, to calculate unit values of food items. There are 93 food items common across all three rounds of FIES; we aggregate these items into 15 food categories provided by FIES.[12] Finally, we construct a unit value ratio of each category relative to regular rice, treating rice as a numeraire good because it is a tradable staple and by far the most commonly consumed food item. Annex 2 details how we constructed these data. We relate the median village-level unit values for these 15 categories with the village's *Pantawid* program saturation in 2009, 2012, and 2015 to study the effect of *Pantawid* on food prices. [13]

[12] The 15 categories are: regular rice; other rice; roots and tubers; fresh eggs; processed eggs; fresh fish; processed fish; fresh meats; processed meats; fresh fruits; fresh vegetables; processed rice and grains; coffee, cocoa, and tea; sugar; and milk products.

[13] An important distinction between the analysis of FIES data and our core analysis using the *Pantawid* survey is that the FIES analysis exploits the staggered rollout of *Pantawid* throughout the country and relates village level prices with saturation. Unlike our core analysis, which relies on the randomized assignment of villages to treatment, this analysis does not leverage experimental variation.

The Household Assessment Form and the Balance of Baseline Data

The Household Assessment Form survey used to assess household eligibility for *Pantawid* also serves as a baseline for our analysis. This information, which is relatively limited in scope, is primarily used to determine the potential program saturation of each village in the RCT sample as well as assess balance across treatment and control villages for the sociodemographic and economic information collected. We explore baseline balance for beneficiary and nonbeneficiary households respectively. Appendix Tables 1 and 2 present the control means, difference between treated and control means, the corresponding p-values for tests of equality, and standardized mean differences. Overall, the samples of beneficiaries and nonbeneficiaries appear balanced. Among beneficiary households, none of the 28 comparisons is imbalanced. For the nonbeneficiary households, balance is almost as comprehensive: only three of 28 comparisons between treated and control areas are significantly different at the 10% level or less. Nonbeneficiary households in treated villages are less likely to own video recorders or motorcycles, and more likely to have walls made of light materials. However, overall wealth, as measured by the logged proxy means test score, is highly balanced between treated and control areas. Furthermore, no standardized mean difference is greater than 0.25, which again suggests a significant degree of balance (Imbens and Wooldridge, 2009).

We also explore baseline characteristic balance among nonbeneficiary households for those in villages above and below the median saturation level (Appendix Tables 3 and 4). These comparisons confirm the overall balance of the experiment. For nonbeneficiary households in the above-median saturation areas, 22 of 28 comparisons, including the proxy means of wealth, household head's education, and children's school attendance, are balanced.[14] Treated households are slightly smaller and slightly more likely to have light roof and wall materials. They are also slightly less likely to own a video recorder, telephone, or refrigerator than households in control

[14] To rule out differential access to services driving the observed effects on prices and child anthropometry, we compare above median saturated treated and control villages. We do not find evidence of such differential access to health clinics or electricity. Although none of the comparisons are significant, if anything, the point estimates suggest treated villages have slightly higher levels of access: 80 percent of all above-median saturated control villages have a health unit in the village while 90 percent of above-median saturated treated villages do. Similarly, 63 percent of all households in above median saturated control villages and 66 percent in treated villages have access to electricity.

areas. One standardized mean difference, for household size, exceeds the 25 percent threshold that may imply concern. We explore any potential effects of this imbalance in household size in further detail in the next section. In the below-median saturated areas, households are balanced along all 28 dimensions.

V. RESULTS

Impacts on Beneficiaries

Direct Anthropometric Impacts

Pantawid incentivized the health- and education-related behavior concerning children in beneficiary households. Table 1 presents some of the main impacts among program beneficiaries on outcomes related to these targets. Similar to findings from other CCT programs, the schooling behavior of age-appropriate children improves 4 percentage points for enrollment and 2 to 3 percentage points in attendance, depending on the age group analyzed. These improvements are apparent despite an already high level of enrollment and attendance in comparison 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 ages 6 to 36 months, as these children transit a critical developmental period for physical growth—often referred to as "the first 1,000 days." Children in this age range also are likely to have lived most or all their lives exposed to the program. Although there is no precisely estimated impact on the mean height-for-age score (the point estimates suggest an improvement of 0.3 standard deviation in the z-score and reduction in stunting likelihood of 2 percentage points), the program lowered the rate of severe stunting among poor children ages 6 to 36 months by 9.3 percentage points.[15] Stunting is the most commonly used measure of chronic malnutrition, believed to reflect extended periods of inadequate nutritional intake and/or chronic infection. No program impacts were found on other measures of severe or acute malnutrition, such as wasting or severe wasting.[16]

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[15] See Kandpal et al. (2016) for a detailed review of *Pantawid's* effects on beneficiary children's nutrition.

[16] Stunting is measured as height-for-age < -2 standard deviations (SD), and severe stunting is measured as height-for-age < -3 SD, applying the World Health Organization (WHO) Child Growth Standard (WHO 2016). Analogously, wasting is measured as weight-for-age < -2 SD and severe wasting as weight-for-age < -3 SD, applying the same WHO Child Growth Standard.

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.

Direct Expenditure and Consumption Effects

One of the channels through which the cash transfer may have resulted in improvements in child anthropometry is through an increase in the consumption of food 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, second, with the reported food intake of young children. Table 2, column 1, reports the program impacts on the household food budget share. Columns 3 to 5 report impacts on consumption of a selected number of individual food goods by children younger than age 60 months, based on parents' recall over the week before the survey.

In the beneficiary households, the cash transfer increases available resources. Indeed, we find that the total food share of the household budget declines almost three percentage points, indicating that households are moving along the food Engle curve, as would be predicted after a gain in income. Among beneficiary children in *Pantawid* villages, there was an 8.2 percentage point increase in parents feeding their children (ages 6 to 60 months) eggs, as well as some indication of greater frequency of meat and fish consumption (although not precisely estimated) during the previous week compared with children in nonprogram villages. Protein-rich foods such as these are particularly important for the linear growth of young children (Baten and Blum 2014; Moradi 2010). Table 3 shows that the young Filipino children in our control communities eat a diet besides rice, which is almost universally eaten, consisting mainly of eggs, fish, green vegetables, and bananas.[17] The chief sources of protein, then, are eggs, fish, and some meat with little dairy or legumes consumed. These patterns are similar to infant and young child feeding practices reported by Denney et al. (2018) using the Philippine national nutrition survey, suggesting that the children in the *Pantawid* survey are comparable to the average Filipino child in as far as eggs and fish are the most important protein sources for them.

[17] We did not ask about rice consumption of young children, but the Philippine national nutrition survey confirms that virtually all Filipino children eat rice every day (Denney et al. 2018).

Impacts on Local Prices

The second step in the proposed causal chain from a local cash influx to nutrition-related impacts on nonbeneficiaries, after a rise in local aggregate demand, is the presence of higher food prices. We thus explore the covariation between the rollout of *Pantawid* and food prices. We begin by examining changes in the household reported prices of a few individual food goods recorded in the 2011 experimental sample. This information was recorded only for three individual foods of standardized quality – eggs, rice, and sugar.[18] Eggs are therefore the key non-tradable protein in this portion of the analysis, while rice and sugar are storable and hence more easily traded foods.

Figure 2 shows the relationship between height-for-age z-scores of children ages 0-3 years and the price of eggs relative to rice, with this ratio also expressed in standardized units. We see that height-for-age steadily decreases as eggs become relatively more expensive, one indication that the relative price of protein is a factor in determining child height-for-age. Figure 3 shows how the relative prices of eggs (in z-scores) co-vary with program saturation in treated and control villages. First, at low levels of saturation, there is no difference in the relative prices of eggs between treated and control villages. In control villages, the relationship between prices and (potential) program saturation is flat or, if anything, slightly negative, perhaps reflecting that higher saturation control villages are poorer (as higher saturation means more households below the provincial poverty threshold.) However, in treatment villages, as saturation increases, and particularly past the median level of 65 percent, we observe a positive relationship between prices and saturation. The relative price of at least one signal protein rich food covaries with program saturation.

Panel A in Table 4 explores through a regression framework how price levels vary at the time of the survey between program and control villages. None of the price differences are significantly different from zero and the point estimates for rice and sugar are close to zero as well. Although not precisely estimated, the point estimate for the price of eggs stands almost 2 percentage points higher, indicating some divergence in relative price difference between the storable goods, such as rice and sugar, and the perishable good, eggs.

[18] Eggs refer to chicken eggs, rice to commercially milled rice, and sugar to commercially sold brown or white sugar.

Price differences emerge more clearly when the program indicator is interacted with the binary measure of high saturation villages (panel B in Table 4). This interaction effect indicates a price increase of approximately six percentage points in highly saturated treated villages in comparison to the controls. The price changes in high saturation villages for the "tradable" goods, rice and sugar, are small in magnitude and not precisely estimated. As eggs are the most perishable good in this three-good comparison, the price changes are consistent with the predictions discussed above – we observe a price rise in program village, but only for the non-tradable good and only in highly saturated villages.

We continue the exploration of price changes and program exposure with official price data. First, Appendix Table 5 explores the covariation in food prices and program saturation using a monthly time series of item-specific food prices collected in each of the Philippines' 81 provinces by the Philippine Statistics Authority from 2006 to 2014.[19] We present prices for three largely non-tradable goods – fresh eggs, fish, and chicken – and three tradable goods – rice, snacks and sugar. The three largely "non-tradable" goods all exhibit price increases correlated with changes in program saturation at the provincial level, although the estimated coefficients don't reach the standard levels of precision. If we take the point estimates for the coefficients at face value, maximum observed program saturation at the province level data is 0.40, suggesting that provincial prices for eggs can rise as much as 7.7 percent (0.192*0.40) because of price effects from the *Pantawid* program. Maximum price increases are on the order of 5 to 6 percent for fresh fish and chicken. In contrast, the coefficients for the three tradable goods show much smaller price co-variation with program saturation. This pattern of results is also apparent in the first differenced specifications, with covariation in the goods price change and program saturation change for the

[19] In each province, price enumerators visit six markets, four rural and two urban; however, the locations of these markets are unknown to the researchers. Since these price data serve as an input to the Consumer Price Index of the Philippines, rigorous field controls are used to ensure the quality and comparability of goods assessed (Philippine Statistics Authority 2015). In the analysis, the average annual provincial price for each good, P_{ipy}, is related to program expansion 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. Standard errors are clustered at the province level. 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 program exposure. We also explore an alternative specification that regresses the year-on-year difference in a food good price on the contemporaneous change in program exposure, also controlling for common year fixed effects.

three protein-rich food goods but not in the prices of other foods, and here some of these difference estimates reach standard levels of precision.

We now turn from provincial level price data, where inferential power may be limited by the small number of provinces, to price variation at the village level. Specifically, we document the relationship between village-level unit values from FIES and village-level saturation of *Pantawid*. Figure 4 confirms the visual pattern that we saw in the household survey data now with the national expenditure data: the relative price (or unit value) of eggs to rice increases in program saturation, with a steeper slope past approximately 65 percent saturation. Table 5 summarizes regression results not just for fresh eggs but for the prices of 15 categories of food goods from FIES. These include several largely non-tradable goods—fresh eggs, processed eggs, fresh fish, and fresh meat—as well as several tradable ones—processed grain, coffee, tea, cocoa, processed meat, and sugar. All prices are normed relative to the price of regular rice. Consistent with our experimental sample, we relate the unit value, P_{ivy}, for each good i in village v and year y (for the years 2009, 2012, and 2015) to program saturation according to the following specification:

$$P_{ipy}/P_{rpy} = \gamma_0 + \gamma_1 S_{vy} + \gamma_2 D65_{vy} * S_{vy} + \gamma_3 D65_{vy} + F_v + F_y + \varepsilon_{ivy}$$

where S is the village-year-specific saturation measure for v and y. The specification also includes village and year fixed effects, F_v and F_y, respectively. The mean effect of interest is $\gamma_1+\gamma_2$, presented in column 6, which captures the total effect of saturation in high-saturation areas on each relative price.

The results show that the prices of all the largely non-tradable goods increase with program saturation above the 65 percent threshold. Moving from a saturation level of 65 to 100 percent between 2009 and 2015 increases the relative price of eggs by almost 3 percent. The magnitudes of the price changes for the other non-tradables are larger, but less precisely estimated (albeit still significant at the 10 percent level). In contrast, none of the more easily traded goods shows significant price change in program saturation in the highly saturated villages.

Unit values have known interpretive difficulties; most importantly they reflect both quality choices made by the household as well as the prices faced by it. This longstanding problem has been recently explored by McKelvey (2011) and Gibson and Kim (2019) who demonstrate that the traditional method of correcting price elasticity estimates for quality adjustments (Deaton, 1988) does not in fact result in elasticity estimates free of quality choices. Therefore, instead of pursuing a related correction, we first note that the main foods in our study are relatively homogenous and should not exhibit significant quality-income elasticities.

We nevertheless carry out two robustness checks on these results that aim to minimize potential quality variation. First, we calculate median village unit values after trimming values that are smaller or greater than the village mean unit value plus or minus 3 standard deviations. Second, we calculate alternative median unit values only taking into account households in the top village-level quartile of the distribution of per capita consumption expenditures. Even in highly saturated villages, these households should largely be non-beneficiaries and therefore their food quality choices unaffected by the income transfer from program participation.

The results, reported in Annex 2, are consistent with our main findings. In fact, the point estimates for fresh fish, fresh meat, and even fruit are larger than the point estimates based on unit values from the entire sample. Finally, we estimate the same regression specification using the price data from the experimental sample and using unit value price data. Exploiting the cross-sectional variation in saturation in the treatment arm of the experimental sample, we find a coefficient of 0.117 on the interaction between saturation and an indicator for saturation above 0.65 for the relative price of eggs.[20] The corresponding coefficient in the national data (reported in Table 5) is 0.08. The estimated effect from the unit values is smaller than that obtained from the price analysis; if the quality upgrading were significant, we would expect bigger effects from the unit values.

In sum, results from all three independent sources of price data suggest a modest but persistent increase in the relative price of protein rich foods. Are these relatively small magnitudes of uncompensated price changes large enough to shift demand choices of the nonbeneficiary households? We return to the survey data to investigate this question.

Impacts on Nonbeneficiaries

So far, the analysis has demonstrated that the program improved the health and education outcomes of children from beneficiary households while concomitantly raising their consumption of protein-rich foods. The analysis has also identified a rise in the price of selected non-tradable goods, at least for highly saturated areas, but not for more easily traded goods. We document this

[20] The full results from this regression are not reported here but are available from the authors upon request.

general pattern of changing prices with three independent sources of price data. We next explore the consequences of this rise in selected food prices for non-beneficiary households and, especially, children.

Indirect Expenditure and Consumption Effects

Table 6 presents results that parallel those in Table 2, but now contrasts nonbeneficiary households in treated and control villages, and presents the program impacts on household food budget share as well as whether the household reported feeding eggs, meat, and fish to children ages 6 to 60 months. For nonbeneficiary households in treated villages, food expenditure as a share of household budget significantly increased by 3.6 percent. When disaggregated into spending on dairy and eggs, cereals, and other foods, the results suggest that the decline in real income through the rising local prices of perishable foods may lead nonbeneficiaries to substitute away from dairy and eggs and toward cereals (Appendix Table 6). It is difficult to infer too much from the disaggregated spending data, as the coefficients are not precisely estimated. Nevertheless, 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 nonbeneficiaries.[21]

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, the food intake of young beneficiary children has shifted in all treatment villages. The food intake among nonbeneficiary children does not change nearly as much as a result of the program—the point estimates for the intake of eggs and vegetables

[21]Since many households in the Philippines produce food for own consumption as well as purchase food on the market, a natural question concerns whether food producers benefit from the price increase in protein-rich foods. We examine whether the typical nonbeneficiary is a net producer or consumer, using eggs as the signal good because the units are more easily measured and compared than, say, fish and fresh meat. In control villages, the average nonbeneficiary household owns 4.55 chickens (in treated villages, nonbeneficiaries own 4.26 chickens; 60 percent of nonbeneficiary households in both treatment arms own chickens). Assume the average small-flock chicken lays 20 eggs per year, yielding a total annual production of 91 eggs from a flock of 4.55 chickens (Sonaiya and Swan 2004). On average, children ages 6 to 36 months in these households consume 1.78 eggs per week, or 93 per year. Non-beneficiary households in our sample have an average of 1.4 children under 5. Since young children's consumption alone exceeds the average production of nonbeneficiary households, even the average home producer is a net consumer of eggs. In addition to increasing egg consumption, the cash transfer may have induced beneficiaries to diversify out of subsistence farming. Such a switch is consistent with Singh, Squire and Strauss's (1986) model of agricultural households that produce and consume certain goods. This in turn would reduce the total supply of eggs at the village level.

are positive although not precisely estimated, suggesting little change. However, as the price changes were seen in highly saturated villages, the food consumption intake of nonbeneficiary households in those villages is appreciably different. Table 6, panel B, thus explores food intake in these villages through fully interacting program exposure with an indicator for above median saturation. In this decomposition, the incidence and quantity of egg consumption among nonbeneficiary households is higher in *Pantawid* villages, perhaps due to informational spillovers of the program itself as the messaging around nutritious food can be absorbed by nontargeted households. Egg consumption also appears greater in highly saturated villages in general. This may 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. However, the interaction term 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, although the difference on the intensive consumption margin is not precisely estimated. There is also no clear pattern for the consumption incidence of meat and fish.

The results explored so far suggest that, even 31 months after the start of the program, treated villages at high saturation level have higher relative prices for select protein-rich foods, and lower consumption of at least the signal good of fresh eggs. Might there also be evidence of effects on longer run outcomes such as child growth?

Indirect Anthropometric Impacts

Table 7, panel A presents the same nutrition measures as presented in Table 1 but now contrasts nonbeneficiary households in *Pantawid* and non-*Pantawid* villages.[22] The impacts on child anthropometry of nonbeneficiaries are different than those for beneficiaries. Children ages 6 to 36 months in nonbeneficiary households are substantially shorter if they reside in program villages—0.4 points shorter in the height-for-age z-score than their counterparts in villages without the program. They are also significantly more likely to be stunted. The post-program stunting rate

[22] For brevity, we do not show schooling-related outcomes in this table. However, impact estimates on nonbeneficiary households suggest little change in enrollment or attendance. These levels are already near universal and substantially higher than the enrollment or attendance of beneficiary children residing in the targeted households in the village. As nonbeneficiary children are not enrolled in the program, it is perhaps not surprising that schooling-related indicators do not change after program introduction. However, it does suggest that there are few schooling-specific spillovers in terms of higher fees or increased crowding that may have deterred the attendance of these children.

is estimated at 32 percent in control villages compared with 43 percent in treated villages. Although 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 nonbeneficiary 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. Table 7, panel B presents the impacts of living in an above-median saturated *Pantawid* village on children's schooling and nutrition, and panel C presents the impacts of living in a village with a *Pantawid* saturation rate in the top quartile. 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 also lower and stunting rates higher in highly saturated villages, but the coefficients are precisely estimated for only the villages in the fourth quartile of saturation. Growth deficits in height-for-age and weight-for-age among non-beneficiary children are greater when a greater proportion of the village participates in the program.

As child growth is particularly sensitive to nutritional and health conditions in the first 1,000 days of life (Hoddinott et al. 2013), we can investigate the age patterns of child height differences among children who lived much of their first 1,000 days under the program compared with older children born and partially reared before program onset. If the nutritional impacts on nonbeneficiary children are attributed to program presence and not other unobserved factors, then we would not expect to see the same impact among older children.

Figure 5 depicts the proportion of nonbeneficiary children stunted between treated and control villages for three age ranges. The stunting prevalence for children ages 36 to 60 months, and hence only partially exposed to the program at critical ages for growth, is virtually identical. In contrast, among younger children, the stunting rate is substantially higher in treated villages, for children ages 6 to 24 months and those ages 24 to 36 months.

The age differences suggested by Figure 5 are apparent in the impact regressions in Table 8, which investigate the nutrition impacts pooled among children ages 6 to 60 months with the program exposure indicators interacted with the younger age categories of 6 to 23 months and 24 to 36 months. Height-for-age z-scores are significantly lower for children ages 6 to 23 months, that is, those children who have been exposed to the program for the entirety of their lives (and in utero as well), with a deficit on the order of 0.70 standard deviation. Stunting rates are also higher (15 percentage points), but the impact is not as precisely estimated.

Program impacts on weight-related nutrition measures, which capture shorter-run measures of health status, also emerge for this age group. Younger nonbeneficiary children are significantly more likely to be underweight, on the order of 20 percentage points. For nonbeneficiary children ages 24 to 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 is not as precisely estimated.

If children in nonbeneficiary households suffer growth deficits because of the cash transfer, then we would expect to see a divergence in growth only for those children exposed to the program at critical ages when they are most vulnerable to a nutritional deficit. We see this for children younger than 36 months, and especially for those ages 6 to 24 months at the time of the survey.

Relative Sizes of Direct and Indirect Effects of the Transfer on Child Growth

The estimated indirect effect on nonbeneficiary children's height-for-age (-0.397) is larger than the direct effect on beneficiary children's height-for-age (0.257). This result may seem counterintuitive. To understand how a difference of this magnitude may arise, we first note that in highly saturated villages, there are many more beneficiaries than nonbeneficiaries, creating a relatively large price shock and concentrating its effects only on the smaller segment of the population without an income transfer.

Regarding the observed changes in height-for-age per se, while perhaps surprising at first glance, these impacts on child growth translate to estimated elasticities in line with the broader literature. Unfortunately, we do not have the data to estimate price or income elasticities for individual food items at the household level, as we only observe food quantities consumed by young children. However, using observed household consumption expenditure for aggregated food groups, we estimate a price elasticity of demand for dairy and eggs to be -1.3 and an income elasticity of egg demand, estimated for beneficiaries in highly saturated areas, of 0.15. These point estimates are similar to those reported in the literature: Friedman and Levinsohn (2002) estimate a price elasticity of demand for eggs in a nationally representative survey from Indonesia to be -1.

Estimates of income elasticity of egg demand in the United States range from 0.04 to 0.11, which are somewhat lower than our estimate of 0.15, although not surprising given the difference in income between the two populations (Okrent and Alston 2012). Finally, using a sample of poor rural households in Kenya, Almås, Haushofer and Shapiro (2019) estimate income elasticity for all protein to be 1.29, which is considerably larger than our estimate for eggs alone and might suggest even greater impacts on child growth than we observe in our setting.

Although eggs are a signal good with a price that we are able to observe, the results presented in Table 5 show that the price increases and thus likely the shift in consumption patterns apply more broadly for other key foods. Nevertheless, if we focus only on eggs, a shortfall in consumption of eggs of the observed magnitude (roughly half an egg a week) extended over the exposure of the program (31 months) would result in a height detriment of approximately 40 percent of the total negative spillover we observe. Informing this calculation are the estimated effects of egg consumption on linear growth from Puentes et al. (2016) and Iannotti et al. (2017). Puentes et al. (2016) estimate an additional 0.72 centimeter in linear growth from consumption of an additional egg per week as a child ages from 6 to 24 months. The converse is also true: the observed increase in egg consumption by beneficiaries over the duration of exposure to the program yields approximately 40 percent of the total height gain for this group. The observed changes in height for the beneficiaries and nonbeneficiaries are therefore consistent with the observed changes in our signal protein-rich food good, eggs. Iannotti et al. (2017) only report the impact on height-for-age z-scores but estimate that an egg a day for six months increased the height-for-age z-score of children ages 6 to 9 months by 0.63 standard deviation relative to a baseline of -1.90 standard deviation. Given the nonlinearity in the height-for-age z-score, these two estimates are not directly comparable; however, they are of the same order of magnitude, suggesting that the observed decline in egg consumption alone explains much of the impact on height-for-age z-scores. Furthermore, the consumption of other protein-rich food goods, such as fresh fish or meat, also plays a role in the production of child height, and price changes for these goods are also correlated with program expansion. The direct and indirect effects of the program on stunting and height-for-age thus appear reasonable given the observed changes in consumption and spending patterns.

Other Possible Mechanisms

Until this point, the evidence presented is consistent with the hypothesis that *Pantawid* increased the prices of certain food goods that are important for the production of child height, with increases especially found in the highly saturated villages, as would be predicted in the presence of a general equilibrium spillover. These price changes in turn likely contributed to an increase in stunting rates among nonbeneficiary children, again especially the children residing in highly saturated villages. Furthermore, the estimated consumption and growth elasticities are in line with the wider literature, suggesting that the observed effects can be expected given the uncompensated relative price increase in key food goods. However, there are alternative transmission mechanisms, complementary to the price channel, that in principle might also result in increased rates of stunting for nonbeneficiary children. In this section, we investigate whether the evidence supports any of these additional channels.

Changes in Health Care Access and Quality

Child height is determined in early life not only by nutrition but also exposure to infections and other health shocks. Thus, another potential channel is through program impacts on 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 can improve child health, a degradation in access to or quality of health services could also in principle contribute to increased stunting. The *Pantawid* program may reduce access or quality of healthcare by crowding out available services due to an increase in service utilization by beneficiary households. This crowding-out mechanism can result in increased prices for care, reductions in the quality of available services, or both. In neighboring Indonesia, Triyana (2016) finds that a CCT conditioned on safe delivery practices resulted in a 10 percent increase in fees charged by midwives.[23]

Table 8 investigates program impacts on a range of health care seeking behavior relevant to young children for beneficiary and nonbeneficiary households. The top panel in Table 8 indicates that, among beneficiaries, *Pantawid* increased the use of maternal and child health services such as antenatal care, postnatal care, skilled birth attendance, growth monitoring, and

[23] There was also a 10 percent increase in the supply of local midwives in that setting, but the increase was not sufficient to prevent a price rise.

general treatment seeking (the first three measures are based on children ages 6 to 36 months, since they refer to care around the birth, while the last two measures of more general care seeking are based on children ages 6 to 60 months). The *Pantawid* program has been successful at increasing utilization.

The bottom panel in Table 8 examines the impact of *Pantawid* on care seeking by nonbeneficiary households. Nonbeneficiaries in treated areas had significantly fewer antenatal care visits and were significantly less likely to have skilled birth attendance (panel B). Although the changes in utilization for these indicators do not significantly vary with program saturation (panel C), the magnitude of the interaction between treatment and above-median saturation for antenatal care visits, at -0.6 visits relative to the control mean of 4.7 visits, is not trivially small. This may suggest that negative spillovers from CCT-driven increased demand for health services around delivery may be related to the growth deficits observed in nonbeneficiary children. Alternatively, the decreased utilization by nonbeneficiaries may be due to a variety of other reasons, such as changes in perceptions of quality or availability of adult time. We are unable to analyze the prices of such utilization or, perhaps more importantly, the quality of services delivered, and thus we cannot definitively point to the underlying mechanism for this change. However, our findings suggest that although a decrease in health care accessibility may contribute to increased stunting—and thus constitute a complementary channel to the price increase of protein-rich foods—the lack of statistical precision in differentiated impacts among highly saturated villages does not support the observed pattern of growth deficits.

Sub-group Imbalance in Baseline Characteristics

Although the baseline characteristics were balanced between treatment and control communities, child height was not one of the measured indicators. Furthermore, as impacts are greatest in highly saturated villages, characteristic balance in this subgroup also needs to be investigated. While all the characteristics in low-saturated villages appear balanced, a handful of characteristics in highly saturated villages are significantly different (Appendix Tables 3 and 4). Of greatest note, nonbeneficiary households in above-median saturated treatment areas were significantly smaller (by 0.41 people, mostly adults) than control nonbeneficiary households. This difference may in turn affect the number of caretakers available for young children and, consequently, factors such as household responsiveness to child illness or the available total household resources partially devoted to children.

We address the possible influence of subgroup imbalance in three ways. First, we note that child height and stunting rates were appreciably the same for children ages 36 to 60 months at the time of the follow-up survey (Figure 5 and Table 9). As these children largely lived their first 1,000 days—a critical growth period in determining child height trajectories—before program introduction, the similar height measures in older nonbeneficiary children between treatment and control villages strongly suggest that this outcome would be balanced at baseline if measured (when these children were 31 months younger). Second, we use baseline data on household composition for a difference-in-differences approach to investigate potential differential changes in household composition across *Pantawid* and non-*Pantawid* villages among nonbeneficiary households with young children. We find no significant differential change in household size or composition in treated villages 31 months after rollout (Annex 3 Table 1). We find this lack of differential change in the full sample and, specifically, with respect to highly saturated villages. Therefore, any baseline difference in household size or composition persists over the study period. If household compositional differences partially determined the child height detriment in comparison villages, then we would also expect to see a persistent height difference among the older child sample, as these children were also raised in somewhat smaller households with somewhat fewer adults. But we do not observe this pattern. Third, we address possible baseline characteristic imbalance more generally by controlling for all baseline characteristics through propensity weighting (Hirano, Imbens, and Ridder 2003). The estimated impacts on child height and weight are appreciably the same as the results presented in Table 6 (Annex 3 Table 2).

Adult Labor Supply Responses to Program Presence

Adult labor market behavior may respond to the *Pantawid* program with knock-on effects for child development. For example, if real incomes decline for nonbeneficiary households because 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. We examine the labor force participation, work-for-pay, and full-time work (greater than 40 hours per week) for adult men and women in nonbeneficiary households (Appendix Table 7). Overall, there is little change in labor force participation or hours worked for men or women. There is also little evidence of change in male or female labor force participation in above-median saturated nonbeneficiary households. Therefore, it appears unlikely that the detrimental impacts among nonbeneficiary children are in part due to shifts in adult labor supply.

Older child educational and labor supply responses to program presence

In addition to adults, the activities of older children in the household, who may play a role in child care, might be affected by the presence of the *Pantawid* program. To determine if this is the case, we look at the education and child labor impacts of *Pantawid* on older nonbeneficiary children (Appendix Table 8). None of the estimated effects of *Pantawid* is significant, suggesting that the availability of older child caregivers was not affected by the program and thus is unlikely to be a channel that impacted nonbeneficiary child growth.

VI. CONCLUSIONS

Aid programs, such as CCTs, often introduce a large amount of resources into small village economies. Such infusions can bring substantial benefits but also risk raising local prices, thus generating negative spillovers on household welfare, particularly for nonbeneficiaries. This paper tests for such local general equilibrium effects on food prices and a range of outcomes for beneficiaries and nonbeneficiaries, using the randomized evaluation of a large CCT program in the Philippines, *Pantawid Pamilya Pilipino*.

Any price response to a demand increase will depend on the market structure of producers and/or suppliers. If the relevant market is principally local and not fully integrated into the wider economy, then the presence of oligopolistic producers or a rising marginal cost curve of local production, if the local market is competitive, will translate the demand increase from the cash transfer into higher prices. We study a combination of locally traded perishable food goods along with staples and packaged goods traded in national markets. It is these locally traded foods that appear to exhibit price increases after the introduction of the program, especially when program exposure at the village level, which we term saturation, is high. In principal, any general equilibrium effect of a cash transfer should be greater as the proportion of beneficiaries increases. This is indeed what we find.

Concomitant with these price changes, *Pantawid* increased child stunting among nonbeneficiaries (while reducing it for beneficiaries). The impacts are observed only for children who were in the vulnerable first 1,000-day period of life when the program was introduced, and not among older children. Moreover, where program saturation was higher, the detrimental impacts on nonbeneficiaries were larger. These are not short-run effects; the transfer program had been in place for 31 months at the time of the follow-up survey. Taken together, the findings suggest that anti-poverty cash transfer programs may have unanticipated effects operating through the price channel. Although participating households are (likely more than) compensated for the price increases, this is not the case for nonbeneficiary households.

In addition to the food price channel, a complementary channel that may contribute to worsening child growth is that of spillovers in the formal health care system. Access to key maternal and child health services significantly increased among beneficiary households, while the use of a few health services declined among nonbeneficiary mothers and children. However, the lack of association with program saturation suggests that this decline did not drive the rise in stunting. We also investigate and rule out possible additional explanations for the nonbeneficiary child growth deficits, including adult and older child behavioral responses to the program that could result in the reduced availability of caregivers, as well as the possible influence of subgroup imbalance in baseline characteristics.

As suggested by the conceptual framework and confirmed by the analysis, we only detect local general equilibrium effects in the highly saturated villages. As saturation and remoteness are key mediators of this effect, so we do not necessarily expect impacts in areas with many proximate markets or where saturation is low.[24] Indeed, the conditions under which local price spillovers arise fortunately are not widespread. By 2015 only 4.2 percent of villages in a nationally representative expenditure survey had a saturation level of 65 percent or higher. We are able to identify such impacts as roughly half the experimental village sample attained such a level of

[24] In extensions of our empirical analysis, we consider remoteness indicators as an additional measure for the integration, or lack thereof, of the local market with wider regional ones. Unfortunately, the *Pantawid* evaluation was not designed to estimate differential impact by remoteness categories. Further, it is not clear what the relevant remoteness metric might be. Annex 4 explores the influence of two remoteness proxies on the main impacts and finds theoretically consistent but imprecise estimates. Given the unambiguous link between price changes, stunting, and program saturation, we believe that in this context the saturation metric is the most comprehensive available measure summarizing the channels through which a cash transfer can lead to general equilibrium impacts.

saturation. In addition, the Philippines has one of the highest incidences of stunting (Bhutta et al. 2013); a similar program might not have such severe consequences in countries where poverty is perhaps less linked with poor nutritional status. Indeed, another reason such negative spillovers have not been detected in other studies may be that cash transfer experiments began in middle-income countries, like Mexico and Brazil, where stunting was less of an issue.

As transfers are increasingly being introduced in poorer countries with higher stunting, the possibility of adverse spillovers to young children in nonbeneficiary households through the local price mechanism merits more consideration. Might a different targeting scheme avoid these consequences from relative price change? The *Pantawid* program is targeted to individual households based on a proxy means test score. An alternative is a village-based targeting scheme for the subset of villages that are particularly poor or remote. In this scheme all households would be offered the program which would compensate everyone for any rise in local prices, thereby averting increases in child stunting.

Of course, area-based targeting or universal access, while averting spillovers, would also likely be more expensive. Using a limited number of assumptions, we consider the costs and benefits of such village-based or geographic targeting in poor areas (as detailed in Annex 5). Note that this is a conservative estimate of program benefit as we only consider the labor market impacts of stunting aversion on subsequent adult wages and not other possible detriments such as lowered intergenerational transmission of human capital. Given such parameter estimates as the average daily adult wages reported in our sample (US$6.3), the stunting differential attributable to *Pantawid*, and the impact of stunting on adult wages (Hoddinott et al. 2013), we estimate that the discounted lifetime benefits of the universal program's impact on stunting, manifested in lifetime earnings, would equate the discounted program costs of expansion to universal coverage at a discount rate of 5 percent. At lower rates of discount, the benefit of local universal targeting exceeds the cost.

While further work needs to be done to estimate more comprehensively the lifetime benefits of improvements in height, as well as the programmatic costs of different targeting mechanisms, this back-of-the-envelope estimate suggests that policy makers consider a hybrid targeting scheme for anti-poverty programs when faced with the possibility of local market price spillovers. For poorer or remote villages, offering the program to every household may be more cost-effective, particularly given the lower administrative costs of geographic targeting through reduced data collection and verification. Other areas of the country that likely will not experience local price spillovers may continue with offering the program only to the poorest households.

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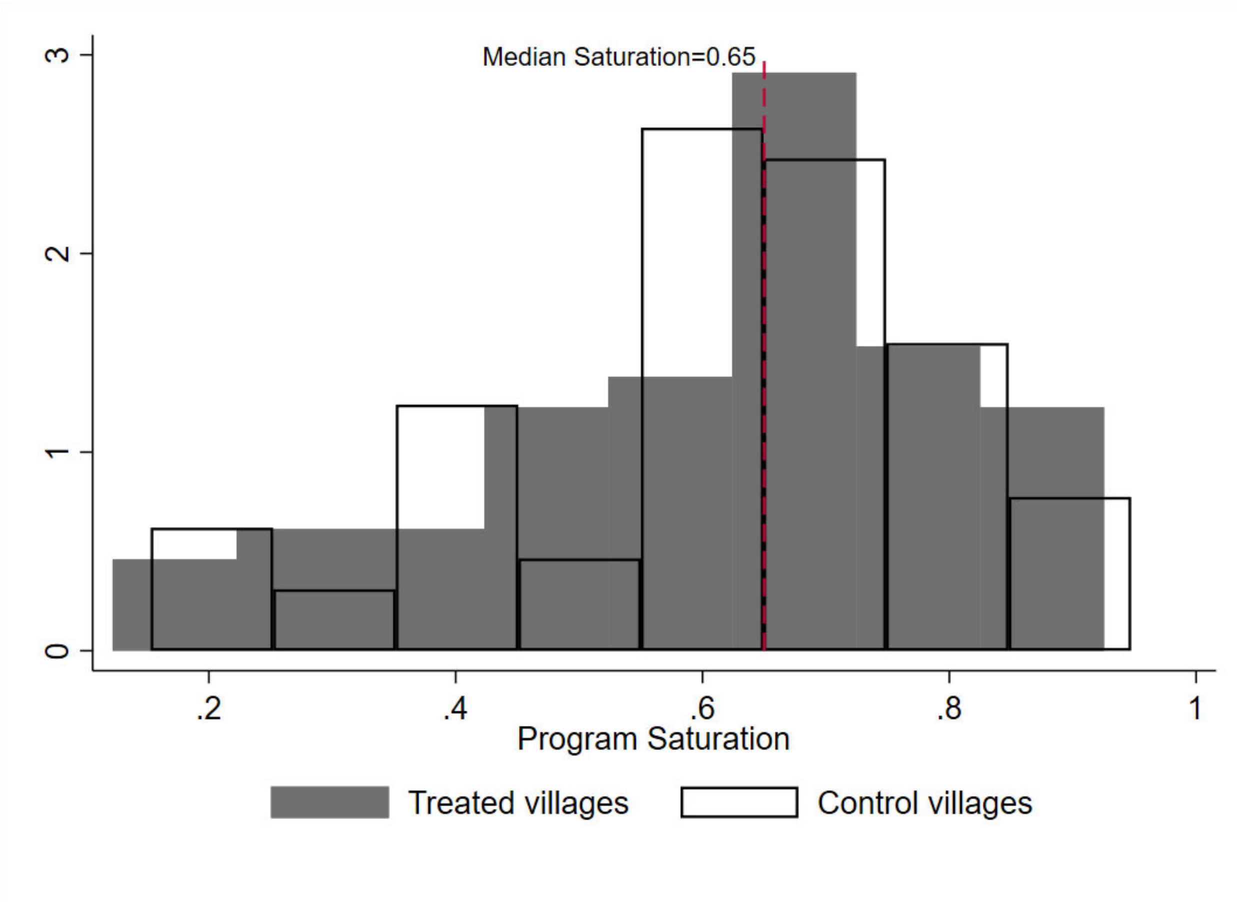
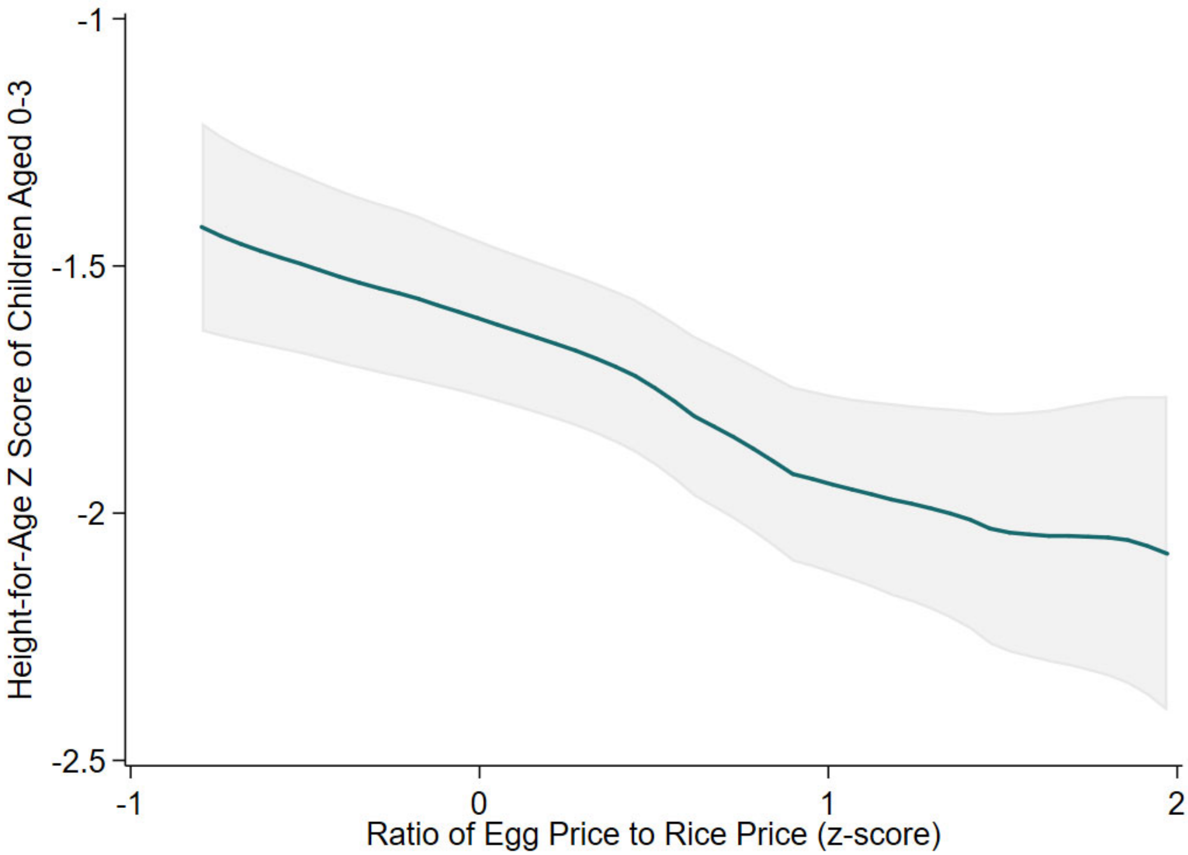


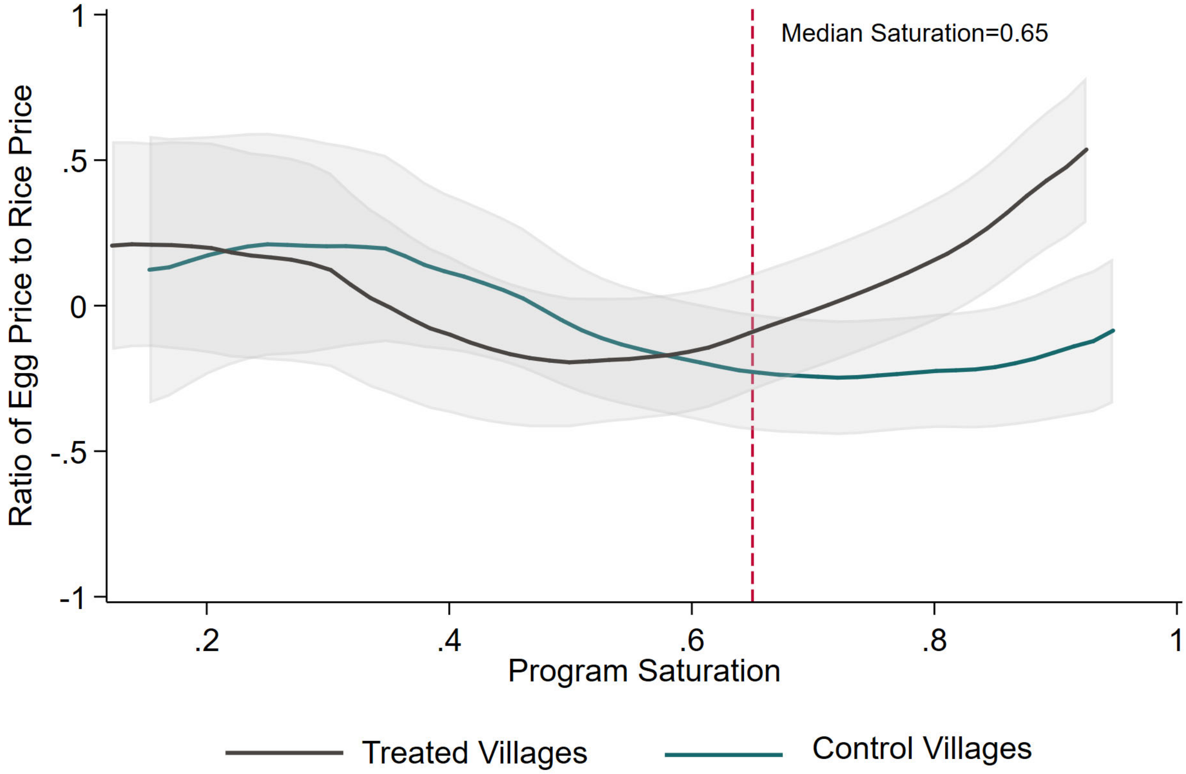
Source: 2008 Household Assessment Form

Figure 1: The across-village variation in program saturation (proportion of households with age appropriate children eligible for the Pantawid program)



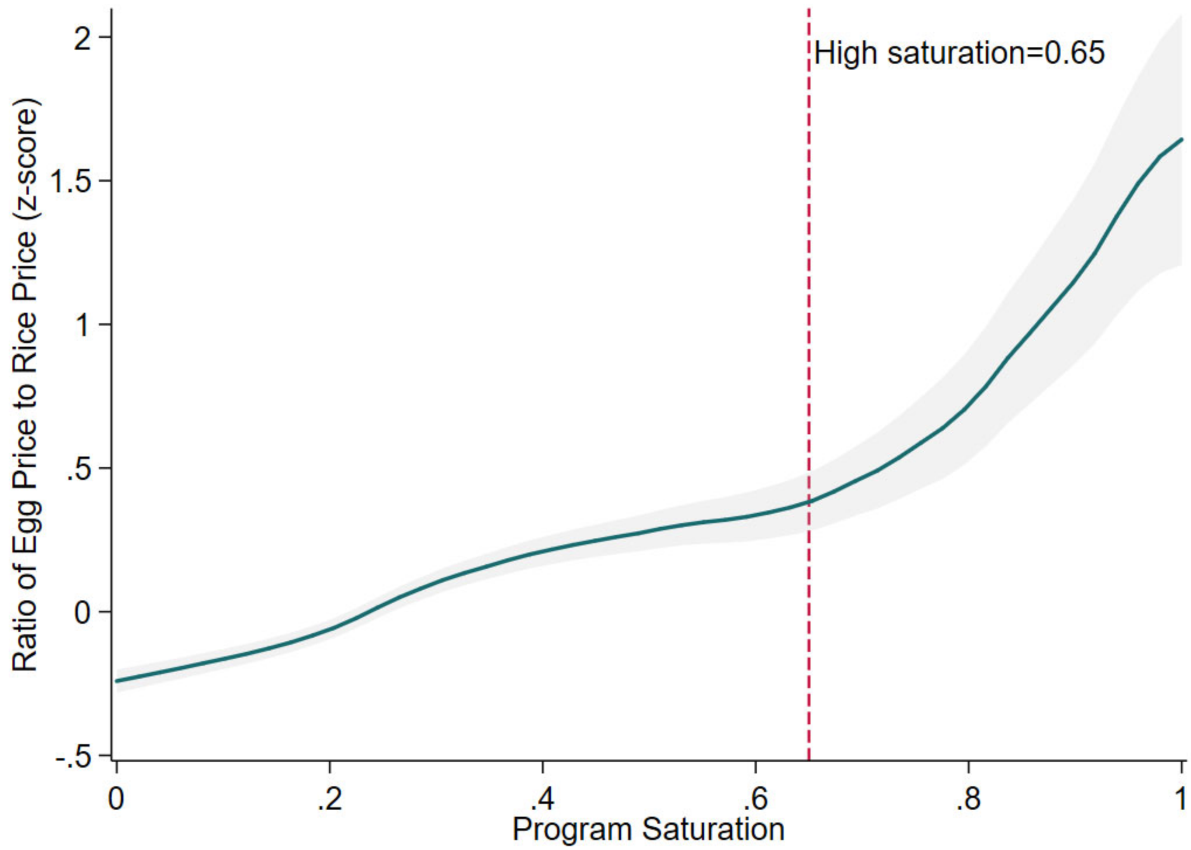
Source: 2011 *Pantawid* impact evaluation survey

Figure 2:Height-for-age Z scores (children aged 0-3 years) and the relative price of eggs



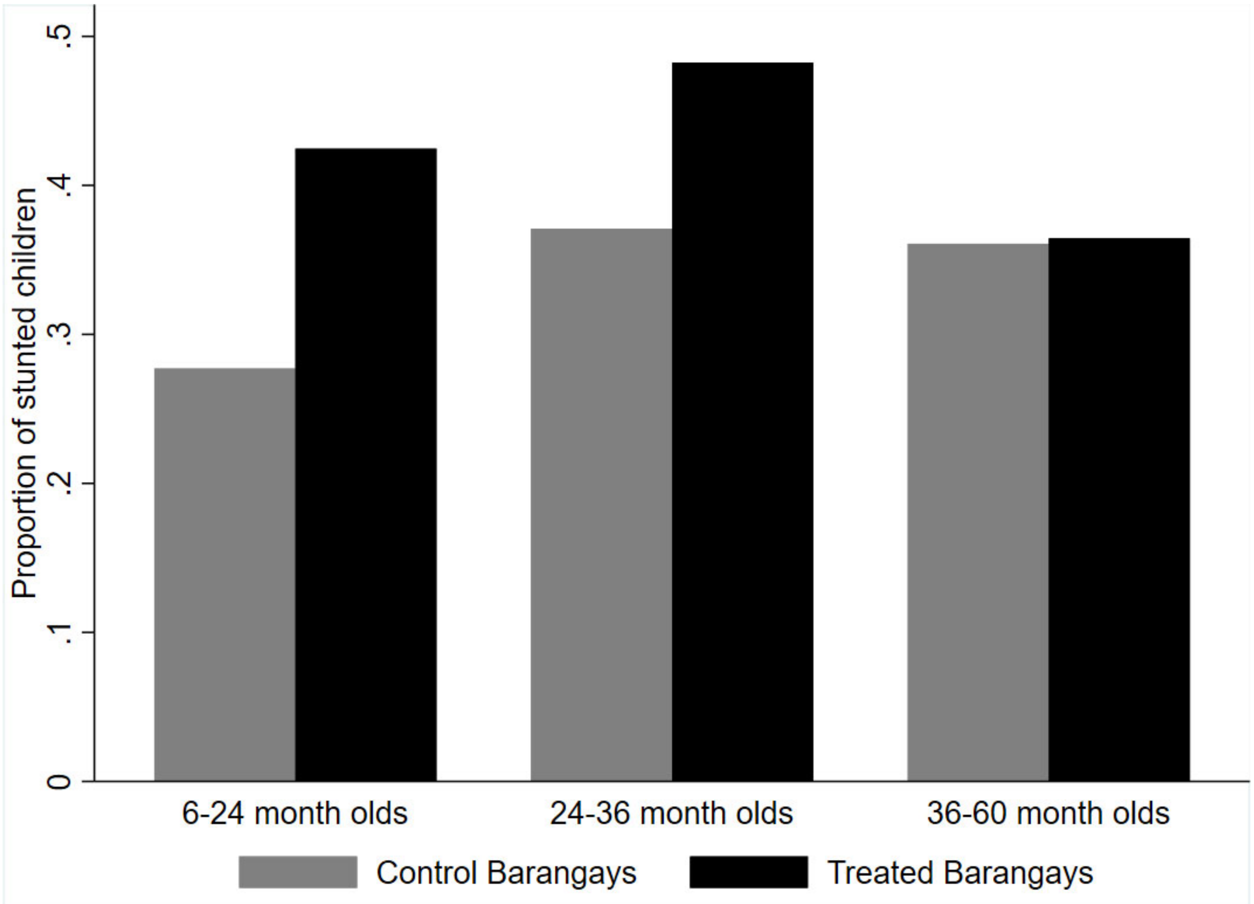
Source: 2011 *Pantawid* impact evaluation survey

Figure 3:The relative price of eggs and program saturation in treated and control villages



Source: 2015 Family and Income Expenditure Survey

Figure 4:The relative price of eggs and program saturation in treated and control villages



Source: 2011 *Pantawid* impact evaluation survey

Figure 5: Stunting prevalence among nonbeneficiary children after 31 months of program exposure by age of child

Table 1: Program impact on beneficiary children: education and child growth

	Educational Outcomes				
	Enrollment of 6-11year 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	
	Growth Outcomes of 6-36-month-old Children				
	Height-for-Age Z score	Stunting	Severe stunting	Weight-for-Age Z score	Underweight
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 (in months), and municipality fixed effects. Standard errors are clustered at village level; there are 130 villages.

Source: 2011 *Pantawid* impact evaluation survey

Table 2: Program impact on budget share of food and children's food intake for beneficiary households with children aged 6-60 months

	(1) Budget Share of Food	(2) Whether Eggs Were Fed to Child in Past Week	(3) Number of Eggs Fed to Child in Past Week	(4) Whether Meat Was Fed to Child in Past Week	(5) 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 specifications include linear controls for child age (in months), and municipality fixed effects. Standard errors are clustered at village level; there are 130 villages.

Source: 2011 *Pantawid* impact evaluation survey

Table 3: Food intake patterns of young children in control villages

Food item	Percentage of children who consumed the food item in the past week	
	Children aged 0-5 years	Children aged 0-3 years
Sweet potatoes	49.15	47.27
Eggs	73.11	71.92
Fish	83.58	82.37
Meat	56.45	53.98
Dairy	27.86	28.55
Green Vegetables	86.86	85.02
Carrot	13.5	13.1
Banana	81.75	81.12
Papaya	36.74	36.35
Mango	20.32	19.34

Source: 2011 *Pantawid* impact evaluation survey

Table 4: Program impact on reported village-level food prices

	Egg Price Reported by Household	Rice Price Reported by Household	Sugar Price Reported by Household
Panel A: Program Impact			
Program village	0.017	-0.000	0.000
	(0.014)	(0.006)	(0.011)
Panel B: Interaction with Saturation			
Program village	-0.011	0.003	-0.004
	(0.019)	(0.008)	(0.015)
Above median saturation	0.007	0.013	0.005
	(0.022)	(0.010)	(0.017)
Impact*Above median saturation	0.060**	-0.007	0.009
	(0.027)	(0.012)	(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; there are 130 villages. All prices are in natural logs.
Source: 2011 *Pantawid* impact evaluation survey

Table 5: Relationship between local unit values of food and program saturation in nationally representative data

Food item/group	(1) Average share of food expenditures	(2) Saturation	(3) Saturation*Above 0.65	(4) Above 0.65	(5) Saturation + Saturation*Above 0.65	(6) Observations
Rice, raw regular milled	0.191	..	..	..	..	8257
Other raw rice and grains	0.074	-0.063**	0.003	-0.122	-0.060	8101
		(0.027)	(0.215)	(0.164)	(0.212)	
Roots and tubers	0.011	0.252***	0.067	-0.088	0.320	8045
		(0.056)	(0.466)	(0.354)	(0.459)	
Eggs (fresh)	0.018	-0.003	0.080***	-0.060***	0.077***	8129
		(0.003)	(0.024)	(0.019)	(0.024)	
Eggs (processed)	0.001	0.002	0.182*	-0.150*	0.184*	6211
		(0.010)	(0.108)	(0.083)	(0.108)	
Fish (fresh)	0.058	-0.176***	1.038**	-0.905**	0.862*	8080
		(0.056)	(0.503)	(0.379)	(0.497)	
Fish (processed)	0.023	-0.153	1.473	-1.493*	1.320	8024
		(0.121)	(1.071)	(0.817)	(1.057)	
Meats (fresh)	0.035	-0.327***	1.181**	-1.081***	0.854*	8057
		(0.066)	(0.512)	(0.390)	(0.504)	
Meats (processed)	0.011	0.570***	-1.437*	0.928	-0.867	7855
		(0.085)	(0.766)	(0.584)	(0.758)	
Fruits (fresh)	0.021	-0.324***	0.543*	-0.342	0.218	8060
		(0.039)	(0.288)	(0.221)	(0.283)	
Vegetables (fresh)	0.046	-0.027	0.091	-0.102	0.064	8109
		(0.018)	(0.139)	(0.106)	(0.137)	
Rice and grains processed	0.021	0.000	0.002	-0.002	0.002	8090
		(0.000)	(0.002)	(0.001)	(0.002)	
Coffee, cocoa, tea	0.025	-0.001	0.002	-0.001	0.001	8103
		(0.001)	(0.005)	(0.004)	(0.005)	
Sugar	0.016	-0.527***	0.778***	-0.554***	0.251	8092
		(0.034)	(0.253)	(0.193)	(0.249)	
Milk Products	0.007	-0.001	-0.006	0.003	-0.007	7699
		(0.001)	(0.007)	(0.005)	(0.007)	

note: *** p<0.01, ** p<0.05, * p<.1. Standard errors in parenthesis. Each row corresponds to a separate regression. Observations are at the village-food item-year level and pooled across the years 2009, 2012, and 2015. All models include year and village fixed effects.
Source: 2009, 2012, and 2015 Family Income and Expenditure Surveys and Department of Social Welfare Administrative Data on *Pantawid* Program Enrollment

Table 6: Program impact on food budget share for nonbeneficiary households with 6-60-month-old children and children's food intake

	(1)	(2)	(3)	(4)	(5)
	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
Panel A: Program Impact					
Program village	0.036*	0.041	-0.147	-0.054	0.007
	(0.021)	(0.038)	(0.170)	(0.046)	(0.037)
Panel B: 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 (in months). All specifications include municipality fixed effects. Standard errors are clustered at village level; there are 130 villages.
Source: 2011 *Pantawid* impact evaluation survey

Table 7: Program impact on 6-36-month-old nonbeneficiary children's physical growth, overall and by program saturation

	Height-for-Age Z score	Stunting	Severe stunting	Weight-for-Age Z score	Underweight
Panel A: Program Impact					
Program village	-0.397**	0.113*	0.042	-0.102	0.057
	(0.176)	(0.060)	(0.032)	(0.166)	(0.043)
Panel B: Interaction with Above Median Saturation					
Program village	-0.314	0.061	0.039	0.257	-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)
Panel C: 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 (in months), and municipality fixed effects. Standard errors are clustered at village level; there are 130 villages.
Source: 2011 *Pantawid* impact evaluation survey

Table 8: Program impact on health seeking behavior of households

	Beneficiary Households				
	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
Panel A: Program impact					
Program village	0.596*	0.102**	0.024	0.148***	0.128***
	(0.346)	(0.049)	(0.061)	(0.030)	(0.038)
Control Observations	224	223	227	450	440
Treated Observations	238	241	242	470	459
Control Mean	4.147	0.296	0.449	0.247	0.479
	Nonbeneficiary Households				
	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
Panel B: Program impact					
Program village	-0.609**	0.000	-0.102*	0.024	-0.003
	(0.308)	(0.054)	(0.058)	(0.030)	(0.042)
Panel C: Interaction with Saturation					
Program impact	-0.319	-0.018	-0.096	0.063	0.011
	(0.384)	(0.076)	(0.064)	(0.039)	(0.054)
Above median saturation	-0.830*	-0.029	-0.291***	-0.014	-0.082
	(0.469)	(0.076)	(0.079)	(0.042)	(0.068)
Program Village*Above median saturation	-0.622	0.042	-0.004	-0.086	-0.031
	(0.604)	(0.119)	(0.099)	(0.058)	(0.087)
Control Observations	174	178	180	288	273
Treated Observations	202	198	203	323	312
Control Mean	4.736	0.371	0.667	0.191	0.511

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

Child level outcomes include linear controls for child age (in months), and all specifications include municipality fixed effects. Standard errors are clustered at village level; there are 130 villages.

Source: 2011 *Pantawid* impact evaluation survey

Table 9: Program impact on nonbeneficiary 6-60-month-old children's physical growth by age category

	Height-for-Age Z score	Stunted	Severely stunted	Weight-for-Age Z score	Underweight
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 measured to the day of birth, therefore age categories are not overlapping
All specifications include linear controls for child age (in months), and municipality fixed effects. Standard errors are clustered at village level; there are 130 villages.
Source: 2011 *Pantawid* impact evaluation survey

Appendix Table 1: Characteristic balance at baseline: beneficiary households

Baseline survey variables	Control Mean	Difference (Treated-Control)	Standardized Mean Difference	p-value
LN Proxy Means Test Score	9.093	-0.014	0.072	0.494
Household composition:				
Household size	5.828	0.004	0.001	0.973
Children 5 years old and below	1.063	0.069	-0.065	0.244
Children between 6 and 14 years old	1.718	-0.022	0.018	0.742
Primary occupation: Farming and livestock	0.685	0.040	-0.096	0.335
Educational attainment of the household head:				
No grade completed	0.078	-0.002	0.002	0.913
Some elementary school	0.433	-0.006	0.000	0.856
Completed elementary school	0.214	0.014	-0.044	0.585
Some high school	0.132	-0.029	0.104	0.105
High school graduate	0.100	0.004	0.000	0.841
Some college	0.031	0.008	-0.036	0.444
College graduate	0.015	0.009	-0.057	0.206
School Attendance:				
Children between 6 to 11 years	0.857	0.002	0.000	0.925
Children between 6 to 11 years	0.767	0.038	-0.094	0.216
Housing Amenities:				
Strong roof materials	0.307	-0.041	0.111	0.164
Strong wall materials	0.191	-0.023	0.080	0.276
Light roof materials	0.478	0.050	-0.130	0.107
Light wall materials	0.445	0.017	-0.065	0.577
Owns a house and lot	0.345	-0.034	0.093	0.239
House has no toilet	0.388	-0.018	0.013	0.632
Shares a water source	0.200	-0.023	0.066	0.463
Household Assets:				
Electricity in house	0.407	0.021	-0.016	0.542
Owns a television	0.203	-0.004	0.030	0.879
Owns a video recorder	0.090	-0.020	0.091	0.241
Owns a Stereo/CD player	0.100	-0.006	0.015	0.755
Owns a refrigerator	0.012	-0.004	0.044	0.430
Has a telephone/cellphone	0.072	-0.013	0.064	0.292
Owns a motorcycle	0.025	0.002	-0.002	0.822

note: *** p<0.01, ** p<0.05, * p<0.1.
Estimates include municipality fixed effects. Standard errors clustered at the village level; there are 130 villages.
Source: 2008 Household Assessment Form

Appendix Table 2: Characteristic balance at baseline: nonbeneficiary households

Baseline survey variables	Control Mean	Difference (Treated-Control)	Standardized Mean Difference	p-value
LN Proxy Means Test Score	9.871	0.008	0.026	0.703
Household composition:				
Household size	4.240	-0.146	0.108	0.149
Children 5 years old and below	0.566	-0.015	0.032	0.702
Children between 6 and 14 years old	0.952	-0.037	0.051	0.547
Primary occupation: Farming and livestock	0.312	-0.000	0.007	0.990
Educational attainment of the household head:				
No grade completed	0.028	0.001	-0.005	0.899
Some elementary school	0.230	0.029	-0.083	0.320
Completed elementary school	0.171	-0.008	-0.004	0.751
Some high school	0.135	-0.001	0.016	0.944
High school graduate	0.195	-0.018	0.059	0.402
Some college	0.122	-0.002	0.012	0.923
College graduate	0.121	-0.001	0.018	0.951
School Attendance:				
Children between 6 to 11 years	0.947	-0.008	0.041	0.689
Children between 12 to 14 years	0.873	-0.009	-0.010	0.804
Housing Amenities:				
Strong roof materials	0.662	-0.046	0.125	0.160
Strong wall materials	0.552	-0.039	0.111	0.217
Light roof materials	0.232	0.027	-0.097	0.352
Light wall materials	0.220	0.047*	-0.145	0.066
Owns a house and lot	0.537	-0.030	0.085	0.369
House has no toilet	0.177	-0.036	0.080	0.219
Shares a water source	0.173	0.001	-0.007	0.962
Household Assets:				
Electricity in house	0.797	0.001	0.036	0.989
Owns a television	0.570	-0.017	0.061	0.687
Owns a video recorder	0.400	-0.088**	0.213	0.014
Owns a Stereo/CD player	0.349	-0.052	0.114	0.110
Owns a refrigerator	0.312	-0.012	0.052	0.682
Has a telephone/cellphone	0.273	-0.054	0.133	0.104
Owns a motorcycle	0.173	-0.042*	0.130	0.068

Note: *** p<0.01, ** p<0.05, * p<0.1.
Estimates include municipality fixed effects. Standard errors clustered at the village level; there are 130 villages.
Source: 2008 Household Assessment Form

Appendix Table 3: Characteristic balance at baseline: nonbeneficiary households in above median saturated areas

Baseline survey variables	Control Mean	Difference (Treated-Control)	Standardized Mean Difference	p-value
LN Proxy Means Test Score	9.816	0.003	-0.070	0.910
Household composition:				
Household size	4.088	-0.429**	0.269	0.016
Children 5 years old and below	0.491	-0.047	0.099	0.344
Children between 6 and 14 years old	0.894	-0.078	0.127	0.397
Primary occupation: Farming and livestock	0.372	-0.036	0.143	0.566
Educational attainment of the household head:				
No grade completed	0.031	0.002	-0.080	0.931
Some elementary school	0.231	0.059	-0.110	0.236
Completed elementary school	0.200	-0.043	0.135	0.264
Some high school	0.124	-0.013	-0.003	0.653
High school graduate	0.213	-0.032	0.104	0.412
Some college	0.107	0.008	-0.030	0.794
College graduate	0.093	0.019	-0.060	0.499
School Attendance:				
Children between 6 to 11 years	0.931	0.022	-0.073	0.498
Children between 12 to 14 years	0.851	-0.037	0.057	0.579
Housing Amenities:				
Strong roof materials	0.611	-0.086	0.182	0.135
Strong wall materials	0.482	-0.079	0.164	0.149
Light roof materials	0.221	0.095*	-0.148	0.087
Light wall materials	0.212	0.093**	-0.139	0.032
Owns a house and lot	0.389	0.031	-0.061	0.573
House has no toilet	0.208	0.009	0.024	0.827
Shares a water source	0.195	0.032	-0.087	0.612
Household Assets:				
Electricity in house	0.735	0.036	-0.049	0.301
Owns a television	0.562	-0.051	0.110	0.334
Owns a video recorder	0.389	-0.099*	0.241	0.071
Owns a Stereo/CD player	0.367	-0.068	0.154	0.231
Owns a refrigerator	0.398	-0.091**	0.148	0.028
Has a telephone/cellphone	0.279	-0.109**	0.225	0.037
Owns a motorcycle	0.204	-0.051	0.147	0.235

*** p<0.01, ** p<0.05, * p<0.1.
Estimates include municipality fixed effects. Standard errors clustered at the village level; there are 65 villages.
Source: 2008 Household Assessment Form

Appendix Table 4: Characteristic balance at baseline: nonbeneficiary households in below median saturated areas

Baseline survey variables	Control Mean	Difference (Treated-Control)	Standardized Mean Difference	p-value
LN Proxy Means Test Score	9.911	0.023	0.082	0.416
Household composition:				
Household size	4.348	0.064	-0.002	0.586
Children 5 years old and below	0.620	0.026	-0.005	0.621
Children between 6 and 14 years old	0.994	0.023	0.004	0.781
Primary occupation: Farming and livestock	0.269	0.039	-0.094	0.357
Educational attainment of household head:				
No grade completed	0.025	-0.007	0.065	0.556
Some elementary school	0.229	0.010	-0.063	0.780
Completed elementary school	0.150	0.008	-0.103	0.813
Some high school	0.143	0.004	0.030	0.874
High school graduate	0.182	-0.002	0.025	0.932
Some college	0.134	-0.008	0.041	0.785
College graduate	0.140	-0.004	0.069	0.892
School Attendance:				
Children between 6 to 11 years	0.956	-0.020	0.110	0.466
Children between 12 to 14 years	0.889	0.007	-0.048	0.877
Housing Amenities:				
Strong roof materials	0.699	0.019	0.086	0.595
Strong wall materials	0.601	0.021	0.078	0.514
Light roof materials	0.241	-0.041	-0.061	0.202
Light wall materials	0.225	0.000	-0.148	0.994
Owns a house and lot	0.642	-0.064	0.196	0.113
House has no toilet	0.155	-0.063	0.125	0.110
Shares a water source	0.158	-0.034	0.056	0.302
Household Assets:				
Electricity in house	0.842	-0.007	0.107	0.874
Owns a television	0.576	0.016	0.027	0.762
Owns a video recorder	0.408	-0.063	0.194	0.135
Owns a Stereo/CD player	0.335	-0.029	0.085	0.436
Owns a refrigerator	0.250	0.046	-0.024	0.186
Has a telephone/cellphone	0.269	-0.014	0.071	0.740
Owns a motorcycle	0.152	-0.039	0.116	0.357

*** p<0.01, ** p<0.05, * p<0.1.
Estimates include municipality fixed effects. Standard errors clustered at the village level; there are 65 villages.
Source: 2008 Household Assessment Form

Appendix Table 5: Log food price impacts of program saturation at the province level, 2006-2014

	Egg	Fish	Chicken	Rice	Snacks	Sugar
Panel A: Covariation with Saturation						
Program saturation	0.192	0.152	0.126	0.022	0.083	0.023
	(0.124)	(0.112)	(0.103)	(0.060)	(0.096)	(0.077)
Province Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	640	608	608	648	640	648
Panel B: Covariation with First Differenced Saturation						
Change in Saturation > 0	0.020**	0.001	0.004	0.009	-0.007	-0.001
	(0.008)	(0.007)	(0.008)	(0.008)	(0.009)	(0.014)
Change in Saturation > 0.05	0.015*	0.011	0.012*	0.010	-0.002	-0.016
	(0.009)	(0.010)	(0.006)	(0.010)	(0.006)	(0.010)
Change in Saturation> 0.10	0.018	0.012	0.014	0.007	0.002	0.003
	(0.013)	(0.010)	(0.008)	(0.009)	(0.008)	(0.016)
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*, 2006)	5.388	118.949	133.175	24.911	8.161	38.092

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

[1]Egg: log price of one medium chicken egg

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

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

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

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

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

Source: Provincial Consumer Price Index Data 2006-2014

Appendix Table 6: Disaggregated food consumption by beneficiary and nonbeneficiary households with 6-60-month-old children

	Beneficiary Households			*Nonbeneficiary Households*		
	Log Per Capita Dairy and Egg Consumption	Log Per Capita Cereal Consumption	Log Per Capita Other Food Consumption	Log Per Capita Dairy and Egg Consumption	Log Per Capita Cereal Consumption	Log Per Capita Other Food Consumption
Panel A: Program Impact						
Program village	0.019	-0.002	0.020	-0.042	0.018	0.032
	(0.029)	(0.066)	(0.113)	(0.046)	(0.065)	(0.125)
Panel B: Interaction with above median saturation						
Program impact	0.056	0.051	0.119	-0.018	0.090	0.156
	(0.036)	(0.076)	(0.145)	(0.069)	(0.089)	(0.172)
Above median saturation	0.027	0.087	0.184	-0.039	0.055	0.154
	(0.048)	(0.106)	(0.193)	(0.069)	(0.099)	(0.202)
Program Village*Above median saturation	-0.082	-0.112	-0.207	-0.058	-0.168	-0.290
	(0.057)	(0.134)	(0.226)	(0.089)	(0.130)	(0.240)
Control Observations	328	328	328	214	214	214
Treated Observations	334	335	334	230	230	230
Control Mean	0.214	1.007	5.227	0.333	0.886	5.209

note: *** p<0.01, ** p<0.05, * p<0.1.
All specifications include municipality fixed effects. Standard errors are clustered by village; there are 130 villages.
Source: 2011 *Pantawid* impact evaluation survey

Appendix Table 7: Labor force participation of nonbeneficiary 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
Panel A: Program Impact								
Program village	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)
Panel B: Interaction with above median saturation								
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; there are 130 villages.
Source: 2011 *Pantawid* impact evaluation survey

Appendix Table 8: Education and child labor program impacts of on nonbeneficiary 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
Panel A: Program Impact						
Program village	-0.004	0.007	-0.004	-0.001	0.031	0.010
	(0.010)	(0.018)	(0.006)	(0.010)	(0.027)	(0.018)
Panel B: 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 (in months) and municipality fixed effects. Standard errors are clustered by village; there are 130 villages.
Source: 2011 *Pantawid* impact evaluation survey

Annex 1: Program take-up, anthropometric outcome construction, and the robustness of results to data trimming

In terms of program saturation, the impact evaluation survey and program Management Information System (MIS) database yield slightly different estimates. 1,418 households surveyed were eligible to become *Pantawid* beneficiaries in 2008, yet only those in treated villages were offered the program in 2009 by design. Among the 704 beneficiary eligible 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 beneficiary eligible households in the *Pantawid* villages were considered participating beneficiaries of the program.[25] Five percent of the households not eligible for the program 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 out of 186 nonbeneficiary children 6 to 36 months of age in treated areas and 151 of 163 nonbeneficiary children of the same age range in control areas. Weight data were collected for 177 6-to-36-month-old nonbeneficiary children in treated areas and 156 nonbeneficiary children in control areas. Anthropometric z-scores were calculated based on the WHO (2006) growth standard. Scores of more than 5 standard deviations above or below the reference mean were dropped from the sample (WHO, 2006). This trimming resulted in 14 of the 172 treated children and 6 of the 151

[25] The lower percentage of households in *Pantawid* villages that reported being program beneficiaries in the survey may be explained in part by the fact that program participation is voluntary. Some households that 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 eligible 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 nonbeneficiary 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*.

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.

Given the relatively small number of children for whom we have anthropometric data, a concern may be that the estimated impacts of *Pantawid* on nonbeneficiary 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 -3 and 3 SD, -4 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 2 Tables 1 through 3, show that the results presented above are highly 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 5 and 10 percent of the anthropometry data. As shown in Annex 2 Tables 4 and 5, the results are robust to this alternative method of treating the outliers in the anthropometry data.

Annex 1 Table 1: Varying trimming threshold on anthropometry data for 6-36-month olds

	Height-for-Age Z score	Stunting	Severe stunting	Weight-for-Age Z score	Underweight
Trimming >-3 and <3 SD					
Program village	-0.262*	0.089	0.008	-0.169	0.066*
	(0.139)	(0.058)	(0.008)	(0.145)	(0.040)
Control Observations	131	131	131	145	145
Treated Observations	142	142	142	151	151
Control Mean	-1.271	0.341	0.001	-0.821	0.160
Trimming >-4 and <4 SD					
Program village	-0.335**	0.113*	0.045	-0.152	0.065
	(0.159)	(0.060)	(0.029)	(0.159)	(0.043)
Control Observations	140	140	140	153	153
Treated Observations	156	156	156	172	172
Control Mean	-1.371	0.382	0.166	-0.947	0.206
Trimming >-6 and <6 SD					
Program village	-0.533***	0.129**	0.067*	-0.102	0.057
	(0.188)	(0.059)	(0.037)	(0.166)	(0.043)
Control Observations	147	147	147	156	156
Treated Observations	164	164	164	175	175
Control Mean	-1.452	0.402	0.106	-0.986	0.217
Trimming >-7 and <7 SD					
Program village	-0.396*	0.129**	0.067*	-0.134	0.057
	(0.204)	(0.059)	(0.037)	(0.175)	(0.043)
Control Observations	149	149	149	156	156
Treated Observations	164	164	164	176	176
Control Mean	-1.457	0.402	0.106	-1.003	0.216

*** p<0.01, ** p<0.05, * p<0.1. All specifications include linear controls for child age (in months). Estimates include municipality fixed effects. Standard errors clustered at the village level; there are 130 villages
Source: 2011 *Pantawid* impact evaluation survey

Annex 1 Table 2: Varying trimming threshold on anthropometry data for 6-36-month-olds with above median saturation interaction

	Height-for-Age Z score	Stunting	Severe stunting	Weight-for-Age Z score	Underweight
Trimming >-3 and <3 SD					
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	131	131	131	145	145
Treated Observations	142	142	142	151	151
Control Mean	-1.271	0.341	0.001	-0.821	0.160
Trimming >-4 and <4 SD					
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	140	140	140	153	153
Treated Observations	156	156	156	172	172
Control Mean	-1.371	0.382	0.166	-0.947	0.206
Trimming >-6 and <6 SD					
Program village	-0.333	0.063	0.047	0.257	-0.046
	(0.253)	(0.075)	(0.046)	(0.216)	(0.053)
Above median saturation	-0.214	0.054	0.020	0.066	-0.075
	(0.306)	(0.082)	(0.050)	(0.261)	(0.074)
Program village*Above median saturation	-0.443	0.146	0.046	-0.798**	0.231***
	(0.381)	(0.110)	(0.075)	(0.344)	(0.087)
Control Observations	147	147	147	156	156
Treated Observations	164	164	164	175	175
Control Mean	-1.452	0.402	0.106	-0.986	0.217
Trimming >-7 and <7 SD					
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	149	149	149	156	156
Treated Observations	165	165	165	176	175
Control Mean	-1.457	0.402	0.106	-1.003	0.217

*** p<0.01, ** p<0.05, * p<0.1. All specifications include linear controls for child age (in months). Estimates include municipality fixed effects. Standard errors clustered at the village level; there are 130 villages. Source: 2011 *Pantawid* impact evaluation survey

Annex 1 Table 3: Varying trimming threshold on anthropometry data for 6-36-month-olds with fourth quartile of saturation interaction

	Height-for-Age Z score	Stunting	Severe stunting	Weight-for-Age Z score	Underweight
Trimming >-3 and <3 SD					
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	131	131	131	145	145
Treated Observations	142	142	142	151	151
Control Mean	-1.271	0.341	0.001	-0.821	0.160
Trimming >-4 and <4 SD					
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	140	140	140	153	153
Treated Observations	156	156	156	172	172
Control Mean	-1.371	0.382	0.166	-0.947	0.206
Trimming >-6 and <6 SD					
Program village	-0.349*	0.058	0.036	0.127	0.018
	(0.208)	(0.062)	(0.040)	(0.180)	(0.047)
Fourth quartile of saturation	0.488	-0.005	-0.033	0.498*	-0.125
	(0.552)	(0.091)	(0.068)	(0.273)	(0.087)
Program village*Fourth quartile of saturation	-1.079*	0.329**	0.159	-1.320***	0.245**
	(0.573)	(0.126)	(0.102)	(0.373)	(0.111)
Control Observations	147	147	147	156	156
Treated Observations	164	164	164	175	175
Control Mean	-1.452	0.402	0.106	-0.986	0.217
Trimming >-7 and <7 SD					
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	149	149	149	156	156
Treated Observations	164	164	164	176	176
Control Mean	-1.457	0.402	0.106	-1.003	0.216

*** p<0.01, ** p<0.05, * p<0.1
All specifications include linear controls for child age (in months). Estimates include municipality fixed effects. Standard errors clustered at the village level; there are 130 villages. Source: 2011 *Pantawid* impact evaluation survey

Annex 1 Table 4: Winsorizing five percent of anthropometry data on 6-36-month-olds

	Height-for-Age Z score	Stunting	Severe stunting	Weight-for-Age Z score	Underweight
Panel A: Program Impact					
Program village	-0.380*	0.129**	0.067*	-0.163	0.057
	(0.210)	(0.059)	(0.037)	(0.169)	(0.043)
Panel B: Interaction with Above Median Saturation					
Program village	-0.036	0.063	0.047	0.205	-0.046
	(0.249)	(0.075)	(0.046)	(0.206)	(0.053)
Above median saturation	-0.189	0.054	0.020	0.087	-0.075
	(0.298)	(0.082)	(0.050)	(0.261)	(0.074)
Program village*Above median saturation	-0.771**	0.146	0.046	-0.812**	0.231***
	(0.382)	(0.110)	(0.075)	(0.349)	(0.087)
Panel C: Interaction with Top Quartile of Saturation					
Program village	-0.191	0.058	0.036	0.066	0.018
	(0.229)	(0.062)	(0.040)	(0.184)	(0.047)
Fourth quartile of saturation	-0.000	-0.005	-0.033	0.518*	-0.125
	(0.525)	(0.091)	(0.068)	(0.279)	(0.087)
Program village*Fourth quartile of saturation	-0.883	0.329**	0.159	-1.322***	0.245**
	(0.579)	(0.126)	(0.102)	(0.396)	(0.111)
Control Observations	151	151	151	156	156
Treated Observations	172	172	172	177	177
Control Mean	-1.593	0.402	0.106	-1.01	0.217

*** p<0.01, ** p<0.05, * p<0.1
All specifications include linear controls for child age (in months). Estimates include municipality fixed effects.
Standard errors clustered at the village level; there are 130 villages.
Source: 2011 *Pantawid* impact evaluation survey

Annex 1 Table 5: Winsorizing ten percent of anthropometry data on 6-36-month-olds

	Height-for-Age Z score	Stunting	Severe stunting	Weight-for-Age Z score	Underweight
Panel A: Program Impact					
Program village	-0.380*	0.129**	0.067*	-0.163	0.057
	(0.210)	(0.059)	(0.037)	(0.169)	(0.043)
Panel B: Interaction with Above Median Saturation					
Program village	-0.036	0.063	0.047	0.205	-0.046
	(0.249)	(0.075)	(0.046)	(0.206)	(0.053)
Above median saturation	-0.189	0.054	0.020	0.087	-0.075
	(0.298)	(0.082)	(0.050)	(0.261)	(0.074)
Program village*Above median saturation	-0.771**	0.146	0.046	-0.812**	0.231***
	(0.382)	(0.110)	(0.075)	(0.349)	(0.087)
Panel C: Interaction with Top Quartile of Saturation					
Program village	-0.191	0.058	0.036	0.066	0.018
	(0.229)	(0.062)	(0.040)	(0.184)	(0.047)
Fourth quartile of saturation	-0.000	-0.005	-0.033	0.518*	-0.125
	(0.525)	(0.091)	(0.068)	(0.279)	(0.087)
Program village*Fourth quartile of saturation	-0.883	0.329**	0.159	-1.322***	0.245**
	(0.579)	(0.126)	(0.102)	(0.396)	(0.111)
Control Observations	151	151	151	156	156
Treated Observations	172	172	172	177	177
Control Mean	-1.588	0.402	0.106	-1.010	0.218

*** p<0.01, ** p<0.05, * p<0.1

All specifications include linear controls for child age (in months). Estimates include municipality fixed effects. Standard errors clustered at the village level; there are 130 villages.

Source: 2011 *Pantawid* impact evaluation survey

Annex 2: Deriving village-level program saturation and unit values from national data and robustness checks on unit value results

We explore whether the association between prices and program saturation is detectable beyond our experimental sample by combining a number of data sources to derive estimates of village-level program saturation and of unit values (which we use as proxies for prices) for various food categories. The following describes the steps we take to generate these variables.

Beneficiary and Census data

Program administrative data, reported by provincial offices of the Department of Social Welfare, provide village-level information on the number of beneficiary households enrolled in *Pantawid* in each year from 2009 to 2015 (B_{vy}). In order-to estimate village-level program saturation, we scale these by an estimate of the total number of households in each village. To generate this estimate we combine data from the Census for 2015 and from the Family Income and Expenditure Survey (FIES) for 2009, 2012, and 2015. The census provides an estimate of the number of individuals in each village for 2015 (P_{v2015}); the FIES provides an estimate of the average number of individuals per household in each village in each year ($\bar{m}_{vy}$); and the ratio of these is our estimate of households per village ($P_{v2015} / \bar{m}_{vy}$). Dividing the program data on beneficiary households per village by the estimated total number of households per village yields our estimate of village- and year-specific program saturation:

$$S_{vy} = \frac{B_{vy}}{P_{v2015} / \bar{m}_{vy}}$$

Unit-values (or price proxies)

We do not observe prices at the level of each village. Instead, we use unit values of food items derived from the 2009, 2012, and 2015 rounds of the Philippine national household budget survey (the FIES). There are 93 food items common across all three rounds of FIES. We derive the household-level unit value for each food item by dividing household expenditure on that item by the quantity consumed. The village-level unit value is defined as the median unit value observed for the village. We aggregate the 93 items into 15 broad food categories provided by FIES: regular rice; other rice; roots and tubers; fresh eggs; processed eggs; fresh fish; processed fish; fresh meats; processed meats; fresh fruits; fresh vegetables; processed rice and grains; coffee, cocoa, and tea; sugar; milk products.[26] We aggregate items into each of these categories using as weights the national average food expenditure share of each item in 2009:[27]

$$P_{ivy} = \sum_{j=1}^{n} w_{j2009} \times P_{jvy}$$

Finally, we construct the price ratios of each category relative to regular rice for each year of observation, which means we treat rice as the numeraire good (that is, we define the relative prices as P_{ipy}/P_{rpy}).

Robustness of unit values results

As discussed in the main text of this paper, unit values have the potential drawback of possibly conflating quality effects with the price estimate. We implement two variants on our derivation of village-level unit values in order to minimize the potential variability in quality. First, we restrict the range of household-level values considered by only including those that are within three standard deviations of the village mean unit value. Second, we consider unit values only for households that are within the top quartile of the per capita household consumption expenditures distribution. The majority of these households are not potential program beneficiaries and hence any quality choice in food consumption decisions should not be affected by the increase in income from program participation. As documented in Annex Tables 1 and 2 (as compared to Table 5 of the paper) the results are only minimally affected.

[26] Our analysis excludes the processed fruit, juice powders and concentrate, chocolate, and "other foods" categories, since these were not consistently defined over time, or were not homogenous enough to constitute a category.
[27] Regular rice and sugar are reported as single unit values and don't require this aggregation.

Annex 2 Table 1: Alternative estimates of relationship between food prices and program coverage—unit values derived after trimming values +/- 3 SD from mean

Food item/group	(1) Saturation	(2) Saturation* Above 0.65	(3) Above 0.65	(4) Saturation + Saturation*Above 0.65	(5) Observations
Rice, raw regular milled	..	..	..	..	8257
Other raw rice and grains	-0.063**	0.003	-0.122	-0.061	8101
	(0.027)	(0.215)	(0.164)	(0.212)	
Roots and tubers	0.245***	0.084	-0.115	0.329	8045
	(0.056)	(0.465)	(0.354)	(0.459)	
Eggs (fresh)	-0.003	0.080***	-0.060***	0.077***	8129
	(0.003)	(0.024)	(0.019)	(0.024)	
Eggs (processed)	0.002	0.182*	-0.150*	0.184*	6211
	(0.010)	(0.108)	(0.083)	(0.108)	
Fish (fresh)	-0.176***	1.038**	-0.905**	0.862*	8080
	(0.056)	(0.503)	(0.379)	(0.497)	
Fish (processed)	-0.153	1.468	-1.489*	1.314	8024
	(0.121)	(1.071)	(0.818)	(1.057)	
Meats (fresh)	-0.327***	1.176**	-1.077***	0.849*	8057
	(0.066)	(0.513)	(0.390)	(0.504)	
Meats (processed)	0.570***	-1.437*	0.928	-0.867	7855
	(0.085)	(0.766)	(0.584)	(0.758)	
Fruits (fresh)	-0.325***	0.542*	-0.342	0.217	8061
	(0.039)	(0.288)	(0.221)	(0.283)	
Vegetables (fresh)	-0.027	0.089	-0.100	0.062	8109
	(0.018)	(0.139)	(0.106)	(0.137)	
Rice and grains processed	0.000	0.002	-0.002	0.002	8090
	(0.000)	(0.002)	(0.001)	(0.002)	
Coffee, cocoa, tea	-0.001	0.002	-0.001	0.001	8103
	(0.001)	(0.005)	(0.004)	(0.005)	
Sugar	-0.526***	0.775***	-0.552***	0.249	8096
	(0.034)	(0.253)	(0.193)	(0.249)	
Milk Products	-0.001	-0.006	0.003	-0.007	7699
	(0.001)	(0.007)	(0.005)	(0.007)	

note: *** p<0.01, ** p<0.05, * p<.1. Standard errors in parenthesis.
Each row corresponds to a separate regression. All models include year and village fixed effects.
Source: 2009, 2012, and 2015 Family Income and Expenditure Surveys and Department of Social Welfare Administrative Data on *Pantawid* Program Enrollment

Annex 2 Table 2: Alternative estimates of relationship between food prices and program Coverage—unit values derived from households in top quartile of per capita consumption-expenditures

Food item/group	(1) Saturation	(2) Saturation* Above 0.65	(3) Above 0.65	(4) Saturation + Saturation*Above 0.65	(5) Observations
Rice, raw regular milled	..	..	..	..	8254
Other raw rice and grains	-0.085***	0.036	-0.120	-0.049	8098
	(0.027)	(0.208)	(0.159)	(0.205)	
Roots and tubers	0.268***	0.295	-0.196	0.563	8044
	(0.075)	(0.582)	(0.447)	(0.572)	
Eggs (fresh)	-0.004	0.070***	-0.053***	0.066***	8132
	(0.003)	(0.025)	(0.019)	(0.025)	
Eggs (processed)	-0.003	0.195*	-0.156*	0.192*	6204
	(0.010)	(0.111)	(0.085)	(0.111)	
Fish (fresh)	-0.222***	1.582***	-1.344***	1.360***	8076
	(0.058)	(0.494)	(0.373)	(0.488)	
Fish (processed)	-0.161	1.943*	-1.815**	1.782*	8021
	(0.121)	(1.039)	(0.797)	(1.023)	
Meats (fresh)	-0.372***	1.461***	-1.260***	1.089**	8054
	(0.068)	(0.510)	(0.391)	(0.501)	
Meats (processed)	0.565***	-1.552**	1.050*	-0.987	7849
	(0.085)	(0.735)	(0.563)	(0.726)	
Fruits (fresh)	-0.331***	0.848***	-0.595***	0.517*	8057
	(0.040)	(0.286)	(0.220)	(0.280)	
Vegetables (fresh)	-0.039**	0.224	-0.181*	0.185	8107
	(0.019)	(0.138)	(0.105)	(0.136)	
Rice and grains processed	0.000	0.002	-0.002*	0.003	8087
	(0.000)	(0.002)	(0.001)	(0.002)	
Coffee, cocoa, tea	0.000	0.000	0.001	-0.001	8100
	(0.001)	(0.005)	(0.004)	(0.005)	
Sugar	-0.548***	0.916***	-0.640***	0.368	8076
	(0.034)	(0.248)	(0.190)	(0.243)	
Milk Products	-0.001	-0.007	0.004	-0.008	7696
	(0.001)	(0.007)	(0.005)	(0.007)	

note: *** p<0.01, ** p<0.05, * p<.1. Standard errors in parenthesis.
Each row corresponds to a separate regression. All models include year and village fixed effects.
Source: 2009, 2012, and 2015 Family Income and Expenditure Surveys and Department of Social Welfare Administrative Data on *Pantawid* Program Enrollment

Annex 3: Robustness of main effects to differences in observable baseline population characteristics

Although our identification leverages the randomized treatment assignment of villages, one may still be concerned may be that differences in outcomes attributed to saturation interacted with treatment are correlated with other factors that affect nutritional outcomes.

Regarding household characteristics, as shown in Appendix Table 3, at baseline, nonbeneficiary households in above median saturated treated areas had slightly smaller household sizes than nonbeneficiary households in control above median saturated areas as well as a handful of other differential household characteristics. Using differences-in-differences (Panel A) and triple differences (Panel B), we show in Annex 3 Table 1 that the household size and the dependency ratios do not significantly vary over time across saturation or treatment categories. Any difference among high saturation treatment villages is consistent over time. That older child heights show no difference between treatment and control villages (Table 7) even though these children were raised in the same household structure as the younger children with significantly different heights indicates the baseline difference in household size and composition likely does not cause the higher stunting rates for younger children in treatment villages.

It may still be that the lack of baseline balance more generally is driving the effects on nonbeneficiary young children's physical growth. This annex then further explores the robustness of the results presented in Table 6 after covariate balancing using the relatively rich set of observables from the baseline. To balance observable characteristics, we estimate propensity scores as a function of baseline population characteristics, and then use these propensity scores to approximate what the outcome would have been if the treated population had exhibited the same baseline characteristics as the control population (following the approach discussed in Hirano, Imbens and Ridder 2003). Results presented in Annex 3 Table 2 are nearly identical to those presented in Table 6. Taken together, the lack of differential stunting among older children and the robustness tests presented in Annex 3 Tables 1 and 2 suggest that small differences in household size and a few other characteristics are not driving the estimated effects of *Pantawid* on nonbeneficiary children's growth.

Annex 3 Table 1: Program impact on the household size and composition of nonbeneficiary 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)
Panel A: Difference-in-differences				
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)
Panel B: Triple difference interaction with above median saturation				
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	-0.263	-0.065	-0.196	-0.169
saturation	(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	-0.830***	-0.131	-0.264	-0.314*
village*Above median saturation	(0.297)	(0.092)	(0.178)	(0.185)
After*Above median	0.058	0.047	-0.014	-0.036
saturation	(0.182)	(0.042)	(0.061)	(0.068)
Program	0.315	-0.001	0.010	0.121
village*After*Above median saturation	(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 village level; there are 130 villages.

Source: 2011 *Pantawid* impact evaluation survey

Annex 3 Table 2: Covariate balanced estimates of program impact on nonbeneficiary children's anthropometry

	Height-for-Age Z score	Stunting	Severe stunting	Weight-for-Age Z score	Underweight
Panel A: Program Impact					
Program village	-0.388**	0.104*	0.040	-0.064	0.044
	(0.180)	(0.061)	(0.033)	(0.174)	(0.044)
Panel B: 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)
Panel C: 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

*** p<0.01, ** p<0.05, * p<0.1. Estimates include municipality fixed effects and are propensity weighted following Hirano et al. (2003). Standard errors clustered at the village level; there are 130 villages.
Source: 2011 *Pantawid* impact evaluation survey

Annex 4: 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-easily traded perishable foods, for which local markets are not as well integrated with wider regional markets as for tradables. If this hypothesis is correct, we may expect to see the greatest increase in reported prices for remote highly saturated villages if remoteness is a good proxy for local market structure and the degree of integration with wide regional markets. We can examine the relationship between distance and price increases using the prices of individual food goods reported by the survey respondents. Annex 4 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 travel time to the nearest market. As we only have 130 villages in the sample, the estimates are largely imprecise, but the estimated coefficients suggest that the price increases are indeed somewhat larger for treated villages that are above median saturated and have an above median travel time to the nearest market, at least for the signal protein-rich good, eggs. For more easily tradable goods such as rice and sugar, the triple interaction coefficient of treatment, above median saturation and above median travel time to the nearest market is close to zero.

In theory, a local market is less likely to be integrated with wider regional markets if the village is remote. However, the *Pantawid* evaluation was not designed to estimate differential impact by remoteness categories. Indeed, in this context, it is not clear what the relevant remoteness metric might be. For example, we also conducted similar analysis to Annex 4 Table 1, but using above median travel time to the municipal center as an alternative metric of remoteness; the results are largely the same as those presented in the table. Given the clear linkage between price changes, stunting, and program saturation, we believe that the saturation metric is the most comprehensive available measure that encapsulates the potential for general equilibrium impacts in this context.

Annex 4 Table 1: Remoteness and food prices reported by households (in natural logs)

	Egg Price	Rice Price	Sugar Price
Program village	0.015	0.001	0.001
	(0.024)	(0.011)	(0.019)
Above median saturation	-0.017	0.013	-0.011
	(0.027)	(0.012)	(0.022)
Above Median Distance to Nearest Market	0.014	-0.013	0.004
	(0.033)	(0.015)	(0.027)
Treated*Above median saturation	0.050	-0.013	0.005
	(0.040)	(0.018)	(0.032)
Treated*Above Median Travel Time to Nearest Market	-0.061	0.009	-0.011
	(0.039)	(0.018)	(0.032)
Above Median Saturation* Above Median Travel Time to Nearest Mkt	0.039	0.007	0.027
	(0.043)	(0.019)	(0.035)
Treatment*Above Med. Saturation * Above Med. Travel Time to Nearest Market	0.027	0.004	0.003
	(0.056)	(0.025)	(0.046)
Control Observations	65	65	65
Treated Observations	65	65	65
Control Mean	6.015	33.392	38.977

*** p<0.01, ** p<0.05, * p<0.1

Estimates include municipality fixed effects. Standard errors clustered at the village level; there are 130 villages.

Source: 2011 *Pantawid* impact evaluation survey

Annex 5: Costs and benefits of area (or village) targeting in the poorest areas

To provide an order of magnitude for the ratio of benefits to costs of extending program saturation in the poorest villages to the entire village, 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 a household, with one 6-12 month old child, to the program roll. Note that the benefits in this exercise are narrowly defined to those associated with lifetime earnings. To estimate the impact of stunting on labor market returns, we use the parameter estimate from Hoddinott et al (2013) 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. Since we find that *Pantawid* increased the prevalence of stunting by 12 percentage points among nonbeneficiary children, we further multiply this value by 0.12 to estimate only the value of the stunting differential attributable 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[28], and (ii) apply a discount rate of 5 percent. At any *lower* discount rate (holding projected real wage growth constant) the benefit/cost ratio exceeds one; at any *higher* projected real wage growth (holding the discount rate constant) the benefit/cost ratio also exceeds one.

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 percent and a discount rate of 5 percent and attributing 12 percent 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.

[28] 1.9 and 1.5 percent real wage growth reported for 2012 and 2013, respectively, in ILO (2014).

Of course, an exercise such as this is sensitive to several assumptions. In particular, the labor market penalty associated with having been stunted at age 36 months (drawn from Hoddinott et al. 2013) appears to be quite high. We estimate that the program would have a benefit/cost ratio of greater than one if 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. Finally, other welfare benefits from stunting aversion, such as increased longevity and non-pecuniary benefits from increased educational attainment and higher cognitive ability, are not accounted for in this exercise but would serve to increase benefits without affecting the cost of program expansion.