<small>POLICY RESEARCH WORKING PAPER</small>   8377

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

Deon Filmer
Jed Friedman
Eeshani Kandpal
Junko Onishi

WORLD BANK GROUP

Development Research Group
    &
Social Protection and Labor Global Practice
March 2018

Abstract

Based on a randomized evaluation, the paper shows that a household-targeted Philippine cash transfer program significantly raised the local price of key foods relevant for child nutritional status. This shift in prices increased stunting among young nonbeneficiary children by 34 percent (11 percentage points). Price and stunting effects increase in program saturation; at median saturation, the village income shock is 15 percent. These effects persist 2.5 years after program introduction. The authors confirm the price patterns in their experimental sample against price information from nationally-representative household expenditure surveys across the 6-year rollout of the program. Failing to consider such general equilibrium effects may overstate the net benefits of targeted cash transfers. In areas where targeting of social programs covers a large proportion of the population, offering the program on a universal basis may avoid such long-lasting negative impacts at moderate additional cost.

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

Deon Filmer, Jed Friedman, Eeshani Kandpal, Junko Onishi

I. INTRODUCTION

We leverage a randomized evaluation of the Philippine flagship anti-poverty program, a conditional cash transfer, to investigate impacts on prices of key food goods and implications for nonbeneficiary welfare. Conditional and unconditional cash transfer programs reach 380 million people in developing countries people around the world (World Bank 2014), and 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). However, the study of possible general equilibrium effects has been limited to three potential channels. Evidence suggests significant general equilibrium effects can occur through the labor market (Muralidharan, Niehaus, and Sukhtankar 2017) as well as informal insurance and credit markets (Angelucci and De Giorgi 2009) but that there are no or limited food market responses (Attanasio and Pastorino 2019, Beegle, Galasso, Goldberg 2017; Cunha, De Giorgi and Jayachandran 2019; Haushofer et al. 2019).[2]

A central contribution of our paper is to identify meaningful local food price increases from a targeted cash transfer program, particularly for perishable and less-easily traded protein rich foods. This finding is consistent with a conceptual framework distinguishing between the relative price effects of cash transfers on easily traded and less easily traded goods.[3] We then show that these price increases affect important outcomes: children of nonbeneficiary households have significantly worse nutritional status, an effect that we trace to a decline in protein intake. Child malnutrition, especially stunting, has lifelong economic consequences through its effects on human capital accumulation (Galasso and Wagstaff 2019; Vogl 2014; Currie and Vogl 2013; Hoddinott et al. 2013).

---

[2] Attanasio and Pastorino (2019) show that while cash transfers from Mexico's *PROGRESA/Oportunidades* program did not affect the average price of staples, they did steepen the slope of the price schedule for those goods. 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. Cunha, De Giorgi, and Jayachandran (2019) study the impact of cash transfers on selected food prices in the context of a comparison of in-kind and cash transfers. They do not find that cash transfers led to significant food price effects, including in communities identified as less developed or that have less integrated markets. An earlier version of their paper (Cunha, De Giorgi, and Jayachandran 2015) documented a six percent increase in food prices in remote communities. Ongoing work in Kenya tests the effects of transfers on prices and wages in a context of low treatment saturation and find limited evidence of spillovers (Haushofer et al. 2019).

[3] 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.

Our paper thus ties local price effects of anti-poverty cash transfers to welfare outcomes among nonbeneficiaries.[4] We do this using household-level data on health, nutrition, and food prices that we collected, and village-level data from the rollout of the Philippine cash transfer program, *Pantawid Pamilya Pilipino Program*. Stunting rates are high and there is substantial spatial variation in market integration and poverty levels in the Philippines, creating an environment conducive to these important—and pernicious— spillovers. That said, stunting rates remain high around the world (UNICEF 2019) and the fragmentation of local markets causing persistent price differences is found in many low-income countries.[5]

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 and food intake.[6] After establishing a demand increase for nutritious foods among beneficiary households, we compare prices in treated and control villages to find that the program increased the prices of key protein rich foods. We then show that this price effect increases in saturation, 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

---

[4] 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 et al. (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 Vakis (2012) do not find any evidence of externalities on nonbeneficiaries.

[5] 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).

[6] 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 in 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 interact, 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.

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 the range of commonly consumed food items measured in national household expenditure surveys across the phased 6-year rollout of the program.[7]

The size of the aggregate income transfer at the village level, determined by program saturation, is an important factor in determining the extent to which local prices shift. The transfers amounted to an aggregate increase in village income of about 15 percent. (Median saturation in the study sample is 65 percent, but in some villages as many as 95 percent of the households received the transfer.) This increase in village income did not affect the prices of storable and easily traded food goods but raised the local prices of protein-rich perishable foods, such as eggs and fresh fish, by 6 to 8 percent. The rise in relative prices decreases nonbeneficiaries' real income and leads them to substitute away from protein-rich foods thus creating significant negative effects on the nutritional status of children in nonbeneficiary households.

We show that 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 an 11-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 a gain in nutritional status. The price increases for non-tradable goods and the reductions in child growth both increase in the proportion of households eligible for the transfers. This finding is consistent with our interpretation of the stunting increase as an unintended spillover effect of the program through local food markets.

Perhaps surprising at first glance, these impacts on child growth translate to estimated elasticities very much in line with the broader literature and consistent with changes in overall consumption patterns. 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). 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

---

[7] 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.

in the consumption of eggs alone would explain much of the observed decline in height-for-age. 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.

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. We can also rule out changes in demand for health services by beneficiaries crowding out utilization by nonbeneficiaries.[8]

Our paper makes four main contributions. First, we identify local price increases from a cash transfer program on nutritionally important foods. Second, we discuss the conditions under which these price responses arise as they are not 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 indicates to be a critical input in the production of child height. Fourth, we link these price increases to deleterious 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.

Our findings also contribute to the literature on the benefits and drawbacks of targeting cash transfers (Hanna and Olken 2018; Klasen and Lange 2016; Ravallion 2009). We show that general equilibrium effects can be important when a large proportion of the local population is eligible for targeted benefits and thus the aggregate increase in income is big enough to shift local prices. Of course, such effects may be concentrated in the poorest areas; across the Philippines as a whole in 2015, only 4.2 percent of villages had program coverage of 65 percent or more. Given the important long-term consequences we identify here, the choice of benefit targeting mechanism should consider whether local markets are integrated with surrounding

---

[8] In our study, 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.

regions. In high saturation situations, particularly in remote areas, a geographic or community-based targeting rule may mitigate negative impacts (Elbers et al. 2007).

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 incomes 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 the one in Cunha, De Giorgi and Jayachandran (2019). We add to their 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.

6

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.[9] Additionally, demand for nutritious foods may increase 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.

---

[9] Del Boca, Pronzato and Sorrenti (2018) show that a CCT in Italy increased beneficiaries' demand for more nutritious foods.

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

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

The import cost δ and national price p^n define both an upper and a lower bound on local prices. Which constraint binds depends on whether the local market is a net producer or consumer of the good. 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, dairy, 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.[10]

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. 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, 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

---

[10] 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.

markets. These factors include the degree of program saturation and the degree of tradability of the particular good.

III. THE CASH TRANSFER PROGRAM AND EXPERIMENTAL DESIGN

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.[11] 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).

Our analysis is based on an experimental design implemented during the pilot phase of the program. A village-level randomized evaluation, stratified by eight purposively selected municipalities in four provinces (in some of the poorest areas of the country), was implemented

---

[11] 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.

for the pilot phase of *Pantawid* to inform program scale-up (Chaudhury, Friedman, and Onishi 2013). 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.[12] A follow-up detailed household 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.[13] To study the effects of these price changes on household behavior, we analyze the direct program impacts among eligible households and then estimate the 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

---

[12] 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.

[13] We thus identify impacts of the program by comparing similarly (non) poor households in treated and control villages. Our SUTVA thus relies on a lack of spillovers across villages. However, even if SUTVA is violated across villages, i.e. prices are higher in control villages due to connected markets or something, we might except higher stunting in those villages, leading our estimates to be a lower bound of the impact of Pantawid on prices and stunting. In addition, we have no evidence even of within village effects on wages.

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.[14] 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 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. We use this measure in conjunction with the food goods unit value information from the household budget survey.

We also calculate potential program saturation for the villages in our experimental survey sample, defined 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

---

[14] 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).

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. This figure also suggests that program saturation was balanced across treatment and control villages: a two-sample Kolmogorov-Smirnov test has a p-value of 0.945, suggesting equality of saturation in the two distributions. Highly saturated villages are equally likely to be found in treatment and control suggesting that the saturation distribution is not significantly different.

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 construct a unit value for each food item, and then aggregate these into 15 food categories provided by FIES.[15] 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 relationship between *Pantawid* and food prices.[16]

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

---

[15] 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.

[16] 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.

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). Finally, since the program was randomized at the village level, we can also use the omnibus test of joint orthogonality. The omnibus test has an F-statistic of 2.03, suggesting that the baseline characteristics are jointly unrelated with treatment status.

Because we carry out analyses with interactions between treatment and saturation, 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), and those in the top quartile of saturation (Appendix Table 5). 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 test score of wealth, household head's education, and children's school attendance, are balanced.[17] 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 areas. One standardized mean difference, for household size, exceeds the 25 percent threshold that may imply concern. In areas in the top quartile of saturation, 18 of 28 comparisons, again including proxy means test of wealth, are balanced. In this level of saturation, however, children aged 6 to 11 years in treated villages

---

[17] 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.

are more likely to have been in school than those in control villages. As in above median saturated areas, treated households tend to be a little smaller, although this time the standardized mean difference is below the 0.25 threshold, at -0.19, and are less likely to have strong wall materials, or own a video recorder, stereo or refrigerator. However, treated households in these villages are also more likely to have their own toilet and are less likely to share a water source. 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. Kandpal et al. (2018) show that the program reduced severe stunting among young beneficiary children. Table 1A reproduces these main impacts among program beneficiaries on outcomes related to these targets. 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 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. The estimated impact on the height-for-age z-score suggests an improvement of 0.3 standard deviation (although this is somewhat imprecisely measured). The estimated impacts on the likelihood of stunting suggest a reduction of 2 percentage points, and on the likelihood of severe stunting a reduction of 9.3 percentage points (with the latter being significantly different from zero at the five percent level). 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.[18] For the average beneficiary child there may only have been a limited improvement in nutrition status, but for the most disadvantaged there was a marked improvement.[19]

Direct Expenditure and Consumption Effects

A channel through which the cash transfer may have improved 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 1B reports the program impacts on the household food budget share in column 1 and, in columns 3 to 5, the 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.[20]

In 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 and a 6.9 percentage point increase in dairy 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. We show in Appendix Table 7 that saturation dampens the gains to beneficiary children's anthropometry in treated villages.

---

[18] 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 2006). 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.

[19] de Hoop et al. (2019) document education impacts of *Pantawid* and show that 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.

[20] Protein-rich foods such as eggs, meat, fish, and dairy are particularly important for the linear growth of young children (Baten and Blum 2014; Moradi 2010). Appendix Table 6 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. (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).) 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.

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.[21] 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 with respect to the price of eggs and that of rice. We see that height-for-age decreases in the price of eggs but not in the price of rice, an indication that the price of protein is a factor in determining child height-for-age. Figure 3 shows how the price of eggs co-varies with program saturation in treated and control villages. First, at low levels of saturation, there is no difference in the price of eggs between treated and control villages. In control villages, the relationship between price 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 price of eggs, a key signal protein-rich food, thus covaries with program saturation.

Panel A in Table 2 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.

Price differences emerge more clearly when the program indicator is interacted with saturation. Panel B in Table 2 shows that egg prices increase by 16 percent as we go from zero to

---

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

maximum observed saturation of 94.7 percent. In panel C of Table 3, we see that the interaction effect indicates a price increase of approximately six percentage points in above-median saturated treated villages in comparison to the controls. Although not precisely estimated, the interaction effect of being in a top quartile saturated treated village (panel D in Table 3) is associated with a price increase of 5.4 percent. 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 saturation in program villages, but only for the non-tradable good.

We continue the exploration of price changes and program exposure with official price data. First, Appendix Table 8 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.[22] 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 between price changes and program saturation changes for the

---

[22] 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 foods but not for 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 price (or unit value) of eggs increases in program saturation, with a steeper slope past approximately 65 percent saturation. Table 3 summarizes regression results not just for fresh eggs but for the prices of 6 categories of food goods from FIES. The food goods include several largely non-tradable goods—fresh eggs, fresh fish, fresh meat, fresh vegetables, and milk products—as well as one tradable ones—raw regular milled rice (the full set of results for 15 food goods are reported in Annex 2). All prices are converted into natural logs. Consistent with our experimental sample, we relate the log 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:

$$P_{ipy} = \gamma_0 + \gamma_1 S_{vy} + \gamma_2 * S_{vy} * D_{vy}^c + \gamma_3 D_{vy}^c + F_v + F_y + \varepsilon_{ivy}$$

where *S* is the village-year-specific saturation measure for *v* and *y*. The model allows for the relationship between prices and saturation to vary above and below a cutoff value (we estimate this via an indicator variable D_{vy}^c equal to 1 if saturation exceeds *c* which is included in the model directly as well as interacted with saturation). Alternative models change the cutoff *c* at which we allow the relationship with saturation to vary—setting it at 50, 65, and 75 percent saturation respectively. In addition, we also estimate a simple linear model where log prices are just a function of saturation. All specifications also include village and year fixed effects, F_v and F_y respectively. In each interactive model the mean effect of interest is $\gamma_1 + \gamma_2$ which is reported below the model coefficient estimates—this 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 (recall that 65 percent is the median saturation in the experimental sample). 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 less tradeable goods (except fresh meat) are similar in magnitude (with milk products higher in magnitude but imprecisely measured). In contrast, the price of rice, which is more easily traded is negatively related to saturation above this cutoff. Increasing the cutoff from 65 to 75 percent sharpens these patterns (although the effects for the more tradeable goods are more imprecisely measured). Reducing the cutoff value to 50 percent or estimating a linear model dampens the contrast between more and less tradeable goods somewhat. The results therefore support the notion that saturation leads to pressure on the prices on less tradeable goods, especially when it is high.

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. Second, we note that Attanasio et al. (2013) showed that in Mexico, changes in unit values reasonably track changes in local prices.

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 nonbeneficiaries 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. 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 price of eggs.[23] The corresponding coefficient in the national data (reported in Table 3) 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.[24] 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 outcomes for 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 next explore the consequences of this rise in selected food prices for nonbeneficiary households and, especially, children.

Indirect Expenditure and Consumption Effects

Table 4 reports results that parallel those in Table 1, 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

---

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

[24] While our model shows that a transportation cost-induced wedge may be sustained indefinitely in areas where markets are not perfectly integrated, chicken ownership does not at first glance appear to be difficult to scale up. The question thus arises as to why supply, particularly of eggs, does not respond to a greater degree to the price increase. Morris, Beesabathuni, and Headey (2018) discuss several aspects of egg production in developing countries that may shed light on this puzzle. First, they note that 96 percent of all egg production in the world happens "intensively", i.e. in industrial settings and not "extensively", i.e. by home producers, perhaps explaining why we do not observe more chicken ownership in our data. They also document that eggs tend to be unavailable and expensive, particularly in South Asia and sub-Saharan Africa but also in less wealthy parts of East Asia. Only a few countries, largely in sub-Saharan Africa, have high levels of poultry ownership. Then, they use data from South Asia and sub-Saharan African to calibrate a model and estimate scenarios of price impacts of changes in egg production. They find that egg prices (albeit nationally) largely respond to changes in industrial production rather than home production. Finally, they document that the Philippines has one of the lowest levels of per-person egg availability globally and that it has developed a "very large-scale industrial egg production industry" in recent years (p. 8).

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 9). 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.[25]

With the provision of cash coupled with parenting education provided during the program's Family Development Sessions, the program was expected to impact feeding practices. Indeed, we find that the food intake of young beneficiary children has shifted in all treatment villages. But the estimates in Table 4 suggest that 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 are positive although not precisely estimated, suggesting little change). However, as before, we find that saturation matters. We find that children's consumption of proteins, particularly eggs and dairy, declines significantly in treated villages as program saturation increases. We note that in these models the direct impact of being in a program village on egg and dairy consumption is positive for this sample of nonbeneficiaries (i.e. the coefficient estimate for being in a program village is positive), perhaps due to informational spillovers of the program.[26] However, the interaction term with saturation is strongly negative. The lower incidence of egg and dairy

---

[25]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. Nonbeneficiary 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.

[26] Egg consumption also appears greater in the highest saturation 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.

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 this intensive margin is not precisely estimated. We find similar but imprecisely estimated effects for meat, but no clear pattern for 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 5, panel A reports the same nutrition measures as presented in Table 1A and Appendix Table 7 but now contrasts nonbeneficiary households in program and non-program villages.[27] 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 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. This difference in impact on height-for-age relative to weight-for-age is consistent with the new public health literature on the importance of proteins for linear growth in the first 1000 days of life (Puentes et al., 2016).

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

---

[27] 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.

saturation. Table 5, panel B presents the impact of saturation on children's schooling and nutrition, panel C presents the impacts of living in an above-median saturated program village, and panel D presents the impacts of living in a village with a 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. We therefore do indeed find that growth deficits in height-for-age and weight-for-age among nonbeneficiary 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 in treated versus 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 regressions reported in Appendix Table 10, which investigate the nutrition impacts differentiated across child age categories (ages 6 to 23 months, 24 to 36 months, and 37 to 60 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 in utero and for the entirety of their lives, 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. The youngest nonbeneficiary children are significantly more likely to be underweight, on the order of 20 percentage points. For slightly older

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 do indeed 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 3 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.[28] 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

---

[28] As reported in Denney et al. (2018), data from the National Nutrition Survey of 2013 show that median daily protein intake is 18.6 grams for 6-12 month olds, 23.3 grams for 12-24 month olds, 27.5 grams for 24-36 month olds, and 31.5 grams for 36-60 month olds. An egg contains 7 grams of protein, thus representing between 22 percent (for 36-60 month olds) to 37.6 percent (for 6-12 month olds) of median daily protein intake. If we were to assume a sustained decrease in egg consumption over the entire period (and a linear relation between growth and protein intake), our estimated reduction in consumption of half an egg per week by nonbeneficiary children would imply a deficit of 3.5 grams of protein a week over 30 months (from ages 6 to 36 months). This totals a reduction in protein intake of 420 grams over the 30 month period. The difference in child height in highly saturated treated (average height of 79.53 centimeters) and control areas (average height of 79.95 centimeters) is 0.42 centimeters. By comparison, the Puentes et al. study estimates that an egg a week over an 18-month period, representing a total of 504 additional grams of protein, contributes 0.72 centimeters in linear growth. While this exercise, like the elasticity estimates above, relies on strong assumptions this calibration exercise brackets what one might expect to see as a result of the general equilibrium effects documented here.

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 this cash transfer program 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 price increases for 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.[29]

---

[29] 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.

We thus turn to program impacts on a range of health care seeking behavior relevant to young children for beneficiary and nonbeneficiary households. Panel A of Appendix Table 11 indicates that, among beneficiaries, *Pantawid* increased the timely initiation (i.e. within the first trimester) of antenatal care, postnatal care within 24 hours, institutional or in-facility delivery, children with the mumps, measles, and rubella (MMR) vaccine[30], growth monitoring, and general curative care seeking.[31] We also examine the content of antenatal care using maternal recall and find that *Pantawid* significantly improved the completeness of antenatal care.[32] The *Pantawid* program thus appears to have been successful at increasing utilization and quality of care. However, again, the interactions with saturation, presented in panels B-D of Appendix Table 11, suggest that increases in saturation dampened gains in growth monitoring and postnatal care within 24 hours.

Table 6 examines care seeking by nonbeneficiary households and shows no significant impacts (panel A). A particular threat to our identification would arise from changes in health service utilization that covary in saturation; however, we find no such effects (panels B, C, and D). Indeed, for above median saturated villages, the interaction effect, although never significant, is generally positive (with the exception of growth monitoring and curative cake seeking for young children), if anything, suggesting positive spillovers. These findings thus do not suggest that a decrease in health care constituted a complementary channel to the price increase of protein-rich foods.

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

---

[30] We use the MMR vaccine as a marker for complete immunization as it is the last vaccine in the WHO/Unicef extended immunization schedule for the first year of life (WHO 2003).

[31] The first three of these 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.

[32] Our measure of the content of ANC is the proportion of the ten following actions: measurement of weight, height, and blood pressure, urine and blood samples, administering the tetanus toxoid injection, iron tablets, and deworming pills, and discussing complications, including guidelines on where to go in the case of complications. The *Pantawid* survey elicited these data through maternal recall of ANC received during the last pregnancy (in the preceding two years).

characteristics in highly saturated villages are significantly different (Appendix Tables 3 and 5). Of particular note, nonbeneficiary households in above-median saturated treatment areas were significantly smaller (by 0.41 people—mostly adults— with a standardized mean difference of 0.269) 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 Appendix Table 10). 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 program and non-program 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 12). 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 13). 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 in the Philippines, *Pantawid Pamilya Pilipino Program*.

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, the CCT increased child stunting among nonbeneficiaries (while reducing it for beneficiaries). These 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 local markets. 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, and indeed quality of care, significantly increased among beneficiary households, but the use of health services among nonbeneficiary mothers and children does not vary in treatment or saturation. 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. We 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, we do not necessarily expect impacts in areas with many proximate

markets or where saturation is low.[33] Fortunately, the conditions under which local price spillovers arise 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 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 levels of 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 such general equilibrium effects? 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 such a scheme all households would be offered the program which would compensate everyone for any rise in local prices, thereby averting negative nutrition impacts.

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

---

[33] 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.

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 could continue with offering the program only to the poorest households.

REFERENCES

Almås, Ingvild, Johannes Haushofer and Jeremy P. Shapiro. 2019. "The Income Elasticity for Nutrition: Evidence from Unconditional Cash Transfers in Kenya." No. w25711. National Bureau of Economic Research.

Aggarwal, Shilpa. 2018. "Do Rural Roads Create Pathways Out of Poverty? Evidence from India." *Journal of Development Economics* 133: 375-395.

Angelucci, Manuela and Giacomo De Giorgi. 2009. "Indirect Effects of an Aid Program: How Do Cash Transfers Affect Ineligibles' Consumption?" *The American Economic Review* 99(1): 486-508.

Atkin, David and Dave Donaldson. 2015. "Who's Getting Globalized? The Size and Implications of Intra-national Trade Costs." No. w21439. National Bureau of Economic Research.

Attanasio, Orazio, Vincenzo Di Maro, Valérie Lechene, and David Phillips. 2013. "Welfare Consequences of Food Prices Increases: Evidence from Rural Mexico." *Journal of Development Economics* 104: 136-151.

Attanasio, Orazio, and Elena Pastorino. 2019. "Nonlinear Pricing in Village Economies." *Econometrica* (forthcoming).

Baird, Sarah, Francisco Ferreira, Berk Özler and Woolcock, Michael. 2013. "Conditional, Unconditional and Everything in Between: A Systematic Review of the Effects of Cash Transfer Programmes on Schooling Outcomes." *Journal of Development Effectiveness* 6(1): 1-43.

Baten, Joerg and Matthias Blum. 2014. "Why Are You Tall While Others Are Short? Agricultural Production and Other Proximate Determinants of Global Heights." *European Review of Economic History*, 18(2): 144-165.

Beegle, Kathleen, Emanuela Galasso and Jessica Goldberg. 2017. "Direct and Indirect Effects of Malawi's Public Works Program on Food Security." *Journal of Development Economics* 128:1-23.

Bhutta, Zulfiqar A., Jai K. Das, Arjumand Rizvi, Michelle F. Gaffey, Neff Walker, Susan Horton, Patrick Webb et al. 2013. "Evidence-based Interventions for Improvement of Maternal and Child Nutrition: What Can Be Done and At What Cost?" *The Lancet* 382 (9890): 452-477.

Bobba, Matteo and Jeremie Gignoux. 2019. "Neighborhood Effects in Integrated Social Policies." *World Bank Economic Review* 33(1): 116-139.

Bobonis, Gustavo and Frederico Finan. 2009. "Neighborhood Peer Effects in Secondary School Enrollment Decisions." *Review of Economics and Statistics* 91(4): 695–716.

Bourguignon, Francois, Francisco Ferreira and Phillippe Leite. 2003. "Conditional Cash Transfers, Schooling, and Child Labor: Micro-simulating Brazil's Bolsa Escola Program." *The World Bank Economic Review*, 17(2): 229-254.

Chaudhury, Nazmul, Jed Friedman and Junko Onishi. 2013. "Philippines Conditional Cash Transfer Program Impact Evaluation 2012." World Bank.

Contreras, Diana and Pushkar Maitra. 2013. "Health Spillover Effects of a Conditional Cash Transfer Program." Working Paper No. 44-13. Monash University, Department of Economics.

Crost, Benjamin, Joseph Felter and Patrick Johnston. 2014. "Aid Under Fire: Development Projects and Civil Conflict." *The American Economic Review* 104(6): 1833-1856.

Cunha, Jesse, Giacomo De Giorgi and Seema Jayachandran. 2015. "The Price Effects of Cash Versus In-kind Transfers." Staff Reports 735, Federal Reserve Bank of New York.

Cunha, Jesse, Giacomo De Giorgi and Seema Jayachandran. 2019. "The Price Effects of Cash Versus In-kind Transfers." *Review of Economic Studies* 86(1): 240-281.

Currie, Janet, and Tom Vogl. 2013. "Early-life Health and Adult Circumstance in Developing Countries." *Annual Review of Economics* 5(1): 1-36.

de Hoop, Jacobus, Jed Friedman, Eeshani Kandpal and Furio Rosati. 2019. "Child Schooling and Child Work in the Presence of a Partial Education Subsidy." *Journal of Human Resources* 54(2): 503-551.

Deaton, Angus. 1988. "Quality, Quantity, and Spatial Variation of Price." *The American Economic Review*: 418-430.

Del Boca, Daniela, Chiara Pronzato, and Giuseppe Sorrenti. 2018. "The Impact of a Conditional Cash Transfer Program on Households' Well-Being." Working Papers 2018-093, Human Capital and Economic Opportunity Working Group.

Denney, Liya, Imelda Angeles-Agdeppa, Mario Capanzana, Marvin Toledo, Juliana Donohue and Alicia Carriquiry. 2018. "Nutrient Intakes and Food Sources of Filipino Infants, Toddlers and Young Children are Inadequate: Findings from the National Nutrition Survey 2013." *Nutrients* 10 (11): 1730.

DSWD. 2015. *Pantawid Pamilyang Pilipino Program*. Government of the Philippines.

Elbers, Chris, Tomoki Fujii, Peter Lanjouw, Berk Ozler, and Wesley Yin. 2007. "Poverty Alleviation Through Geographic Targeting: How Much Does Disaggregation Help?." *Journal of Development Economics* 83 (1): 198-213.

Ferreira, Francisco H., Deon Filmer and Norbert Schady. 2017. "Own and Sibling Effects of Conditional Cash Transfer Programs: Theory and Evidence from Cambodia" in. S. Bandyopadhyay (ed.) *Research on Economic Inequality* 25: 259-298.

Fiszbein, Ariel and Norbert Schady. 2009. *Conditional Cash Transfers: Reducing Present and Future Poverty*. World Bank, Washington DC.

Friedman, Jed and James Levinsohn. 2002. "The Distributional Impacts of Indonesia's Financial Crisis on Household Welfare: A "Rapid Response" Methodology." *The World Bank Economic Review*, *16*(3): 397-423.

Galasso, Emanuela and Adam Wagstaff. 2019. "The Aggregate Income Losses from Childhood Stunting and the Returns to a Nutrition Intervention Aimed at Reducing Stunting." *Economics & Human Biology* (forthcoming).

Gibson, John, and Bonggeun Kim. 2019. "Quality, Quantity, and Spatial Variation of Price: Back to the Bog." *Journal of Development Economics* 137: 66-77.

Hanlon, Joseph, Armando Barrientos and David Hulme. 2010. *Just Give Money to the Poor: The Development Revolution from the Global South*. Kumarian Press.

Hanna, Rema and Benjamin Olken. 2018. "Universal Basic Incomes Versus Targeted Transfers: Anti-poverty Programs in Developing Countries." *Journal of Economic Perspectives*, *32*(4): 201-26.

Haushofer, Johannes and Jeremy Shapiro. 2016. "The Short-term Impact of Unconditional Cash Transfers to the Poor: Experimental Evidence from Kenya." *The Quarterly Journal of Economics* 131(4): 1973-2042.

Haushofer, Johannes et al. 2019. "General Equilibrium Effects of Cash Transfers in Kenya." AEA RCT Registry. February 18 2019.

Headey, Derek, Kalle Hirvonen and John Hoddinott. 2018. "Animal Sourced Foods and Child Stunting." *American Journal of Agricultural Economics* 100 (5): 1302-1319.

Hirano, Keisuke, Guido Imbens and Geert Ridder. 2003. "Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score". *Econometrica*, 71(4): 1161-1189.

Hoddinott, John, Jere R. Behrman, John A. Maluccio, Paul Melgar, Agnes R. Quisumbing, Manuel Ramirez-Zea, Aryeh D. Stein, Kathryn M. Yount, and Reynaldo Martorell. 2013. "Adult Consequences of Growth Failure in Early Childhood." *The American Journal of Clinical Nutrition* 98(5): 1170-1178.

Hoddinott, John, Harold Alderman, Jere R. Behrman, Lawrence Haddad and Susan Horton. 2013. "The Economic Rationale for Investing in Stunting Reduction." *Maternal and Child Nutrition* 9(Suppl. 2): 69–82.

Iannotti, Lora L., Chessa K. Lutter, Christine P. Stewart, Carlos Andres Gallegos Riofrío, Carla Malo, Gregory Reinhart, Ana Palacios et al. 2017. "Eggs in Early Complementary Feeding and Child Growth: A Randomized Controlled Trial." *Pediatrics* 140(1).

ILO. 2014. "Wages in Asia and the Pacific: Dynamic But Uneven Progress" in *Global Wage Report 2014/15 | Asia and the Pacific Supplement*. Available from: http://www.ilo.org/wcmsp5/groups/public/---asia/---ro-bangkok/---sro-bangkok/documents/publication/wcms_325219.pdf. Bangkok: International Labour Organization Regional Office for Asia and the Pacific Regional Economic and Social Analysis Unit. Accessed 23 May, 2019.

Imbens, Guido and Jeffrey Wooldridge. 2009. "Recent Developments in the Econometrics of Program Evaluation." *Journal of Economic Literature*, 47, 5–86.

Kandpal, Eeshani, Harold Alderman, Jed Friedman, Deon Filmer, Junko Onishi and Jorge Avalos. 2016. "A Conditional Cash Transfer Program in the Philippines Reduces Severe Stunting". *The Journal of Nutrition* 146 (9), 1793-1800.

Klasen, Stephan and Simon Lange. 2016. "How Narrowly Should Anti-poverty Programs be Targeted? Simulation Evidence from Bolivia and Indonesia." No. 213. *Courant Research Centre: Poverty, Equity and Growth-Discussion Papers.*

Labonne, Julien. 2013. "The Local Electoral Impacts of Conditional Cash Transfers: Evidence from a Field Experiment." *Journal of Development Economics* 104: 73-88.

Lalive, Rafael and Mattias Cattaneo. 2009. "Social Interactions and Schooling Decisions." *The Review of Economics and Statistics* 91(3): 457–477.

Macours, Karen, Norbert Schady and Renos Vakis. 2012. "Cash Transfers, Behavioral Changes, and Cognitive Development in Early Childhood: Evidence from a Randomized Experiment." *American Economic Journal: Applied Economics* 4(2): 247-273.

McKelvey, Christopher. 2011. "Price, Unit Value, and Quality Demanded." *Journal of Development Economics*, *95*(2):157-169.

Moradi, Alexander. 2010. "Nutritional Status and Economic Development in Sub-Saharan Africa, 1950–1980." *Economics & Human Biology*, 8(1): 16-29.

Morris, Saul, Kalpana Beesabathuni and Derek Headey. 2018. An Egg for Everyone: Pathways to Universal Access to One of Nature's Most Nutritious Foods. *Maternal & child nutrition*, *14*, e12679.

Mu, Ren and Dominique van de Walle. 2011. "Rural Roads and Local Market Development in Vietnam," *Journal of Development Studies*, 47(5): 709-734.

Muralidharan, Karthik, Paul Niehaus and Sandip Sukhtankar. 2017. "General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence from India." Working Paper.

Nazara, Suahasil and Sri Kusumastuti Rahayu. 2013. "Program Kelyarga Harapan (PKH): Indonesian Conditional Cash Transfer Programme." International Policy Center for Inclusive Growth: Policy Brief No. 42.

Okrent, Abigail and Julian Alston. 2012. The Demand for Disaggregated Food-Away-From-Home and Food-at-Home Products in the United States, ERR-139, U.S. Department of Agriculture, Economic Research Service.

Philippine Statistics Authority. 2015. *Price Indices*. The Government of The Philippines. Web. 10 November 2015.

Puentes, Esteban, Fan Wang, Jere R. Behrman, Flavio Cunha, John Hoddinott, John A. Maluccio, Linda S. Adair, Reynaldo Martorell and Aryeh D. Stein. 2016. "Early Life Height and Weight Production Functions with Endogenous Energy and Protein Inputs." *Economics & Human Biology* 22: 65-81.

Ravallion, Martin. 2009. "How Relevant is Targeting to the Success of an Antipoverty Program?" *The World Bank Research Observer*, 24(2), pp.205-231.

Saavedra, Juan Esteban and Sandra Garcia. 2012. "Impacts of Conditional Cash Transfer Programs on Educational Outcomes in Developing Countries: A Meta-analysis." *RAND Labor and Population Working Paper Series, WR-921-1.*

Singh, Inderjit, Lyn Squire, and John Strauss. 1986. "A Survey of Agricultural Household Models: Recent Findings and Policy Implications." *The World Bank Economic Review* 1.1: 149-179.

Sonaiya, Emmanuel and S. E. J. Swan. 2004. "Small-Scale Poultry Production: Technical Guide." *Food and Agriculture Organization of the United Nations,* Rome.

Triyana, Margaret. 2016. "Do Health Care Providers Respond to Demand-Side Incentives? Evidence from Indonesia." *American Economic Journal: Economic Policy* 8(4): 255-88.

UNICEF 2019. The State of the World's Children 2019. Children, Food and Nutrition: Growing Well in a Changing World. UNICEF, New York.

Vogl, Tom S. 2014. "Height, Skills, and Labor Market Outcomes in Mexico." *Journal of Development Economics* 107: 84-96

World Bank. 2014. *A Model from Mexico for the World.* http://www.worldbank.org/en/news/feature/2014/11/19/un-modelo-de-mexico-para-el-mundo. November 15, 2015.

WHO. 2003. *Selected Vaccine Introduction Status into Routine Immunization*. Geneva: World Health Organization. Available from: www.who.int/immunization_monitoring/routine/schedule_analysis_2003.pdf.

WHO Multicentre Growth Reference Study Group. 2006. *WHO Child Growth Standards: Length/Height-for-age, Weight-for-age, Weight-for-length, Weight-for-height and Body Mass Index-for-age: Methods and Development*. Geneva: World Health Organization. Available from: https://www.who.int/childgrowth/standards/technical_report/en/.
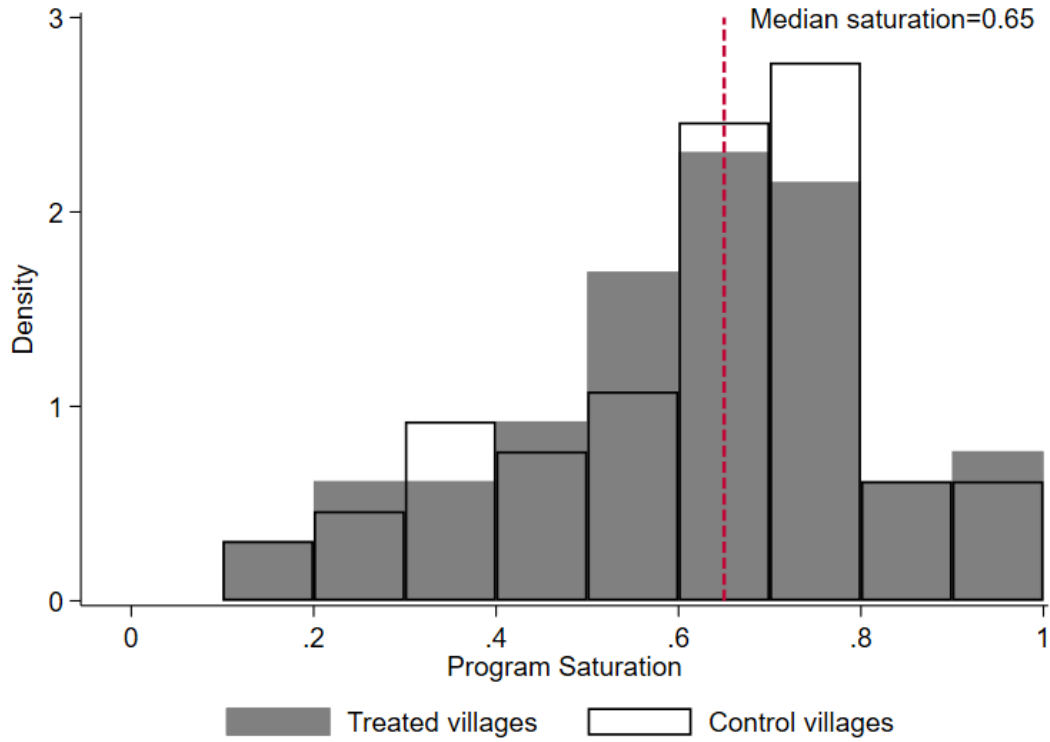
Source: 2008 Household Assessment Form

Figure 1: The across-village variation in program saturation (proportion of households with age appropriate children eligible for the Pantawid program)
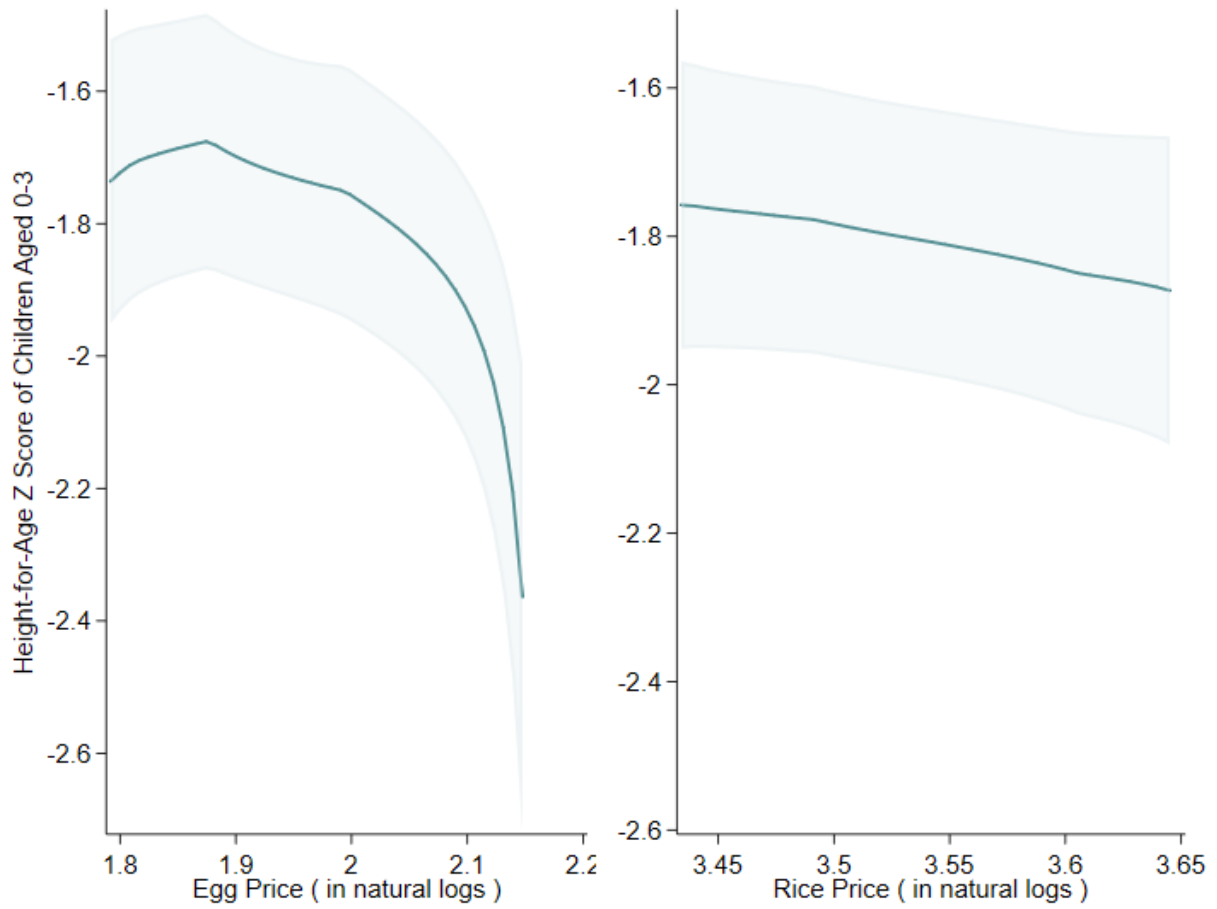
Source: 2011 *Pantawid* impact evaluation survey

Figure 2:Height-for-age Z scores (children aged 0-3 years) and the prices of eggs and rice
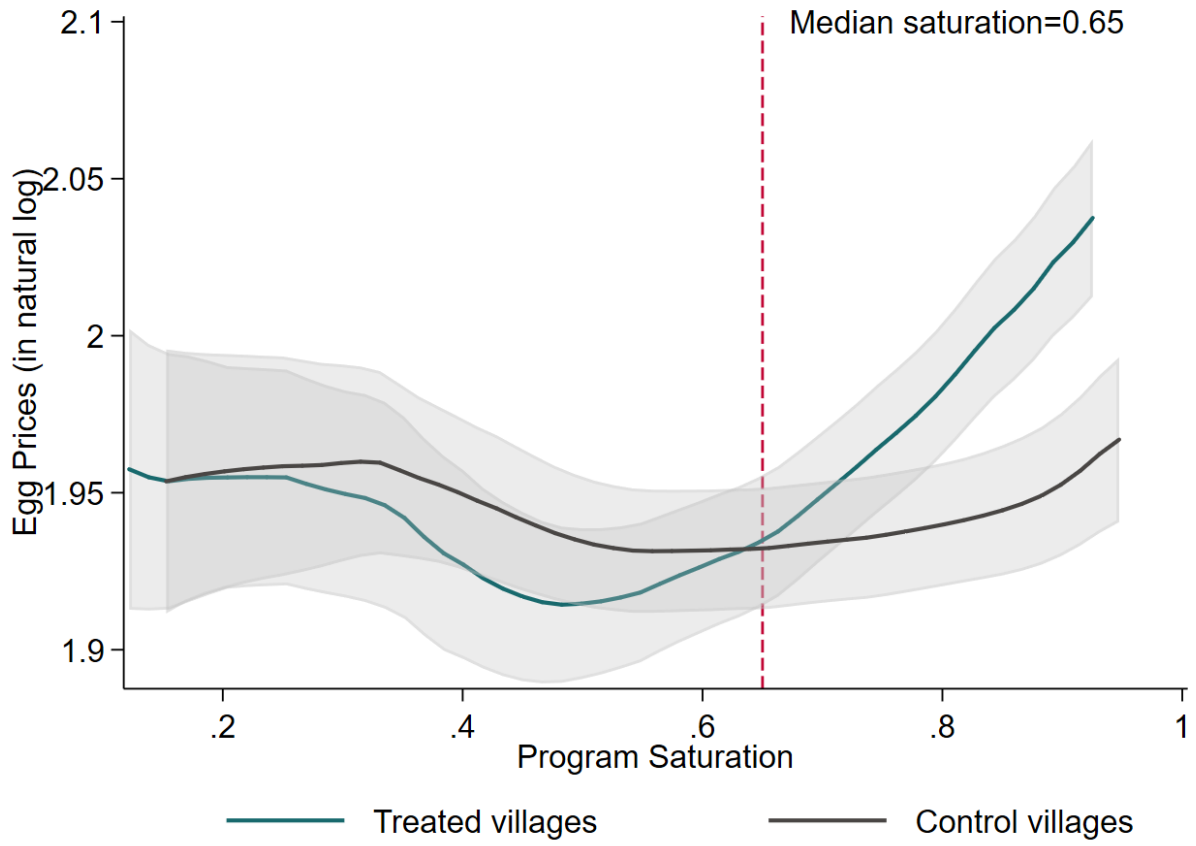
Source: 2011 *Pantawid* impact evaluation survey

Figure 3:The price of eggs and program saturation in treated and control villages
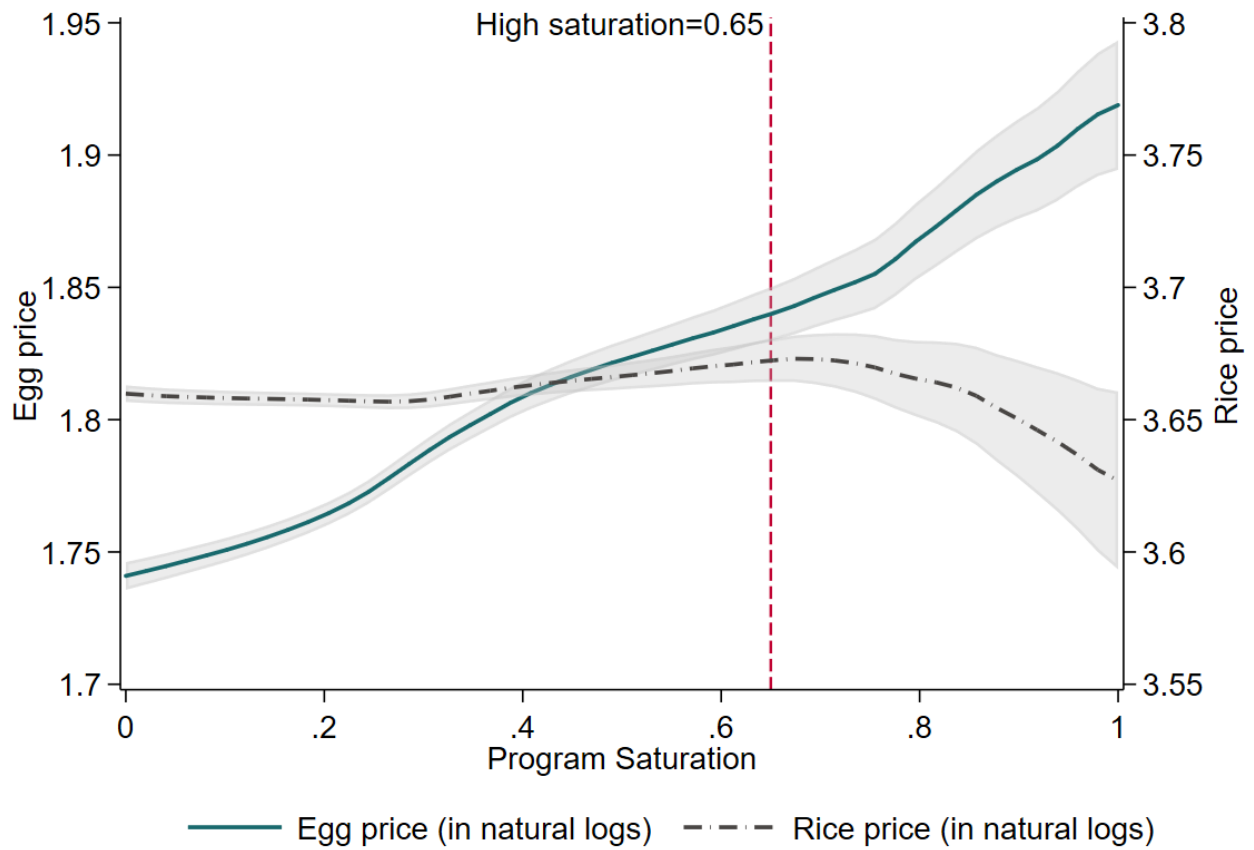
Source: 2015 Family and Income Expenditure Survey

Figure 4:The relative price of eggs and program saturation in treated and control villages
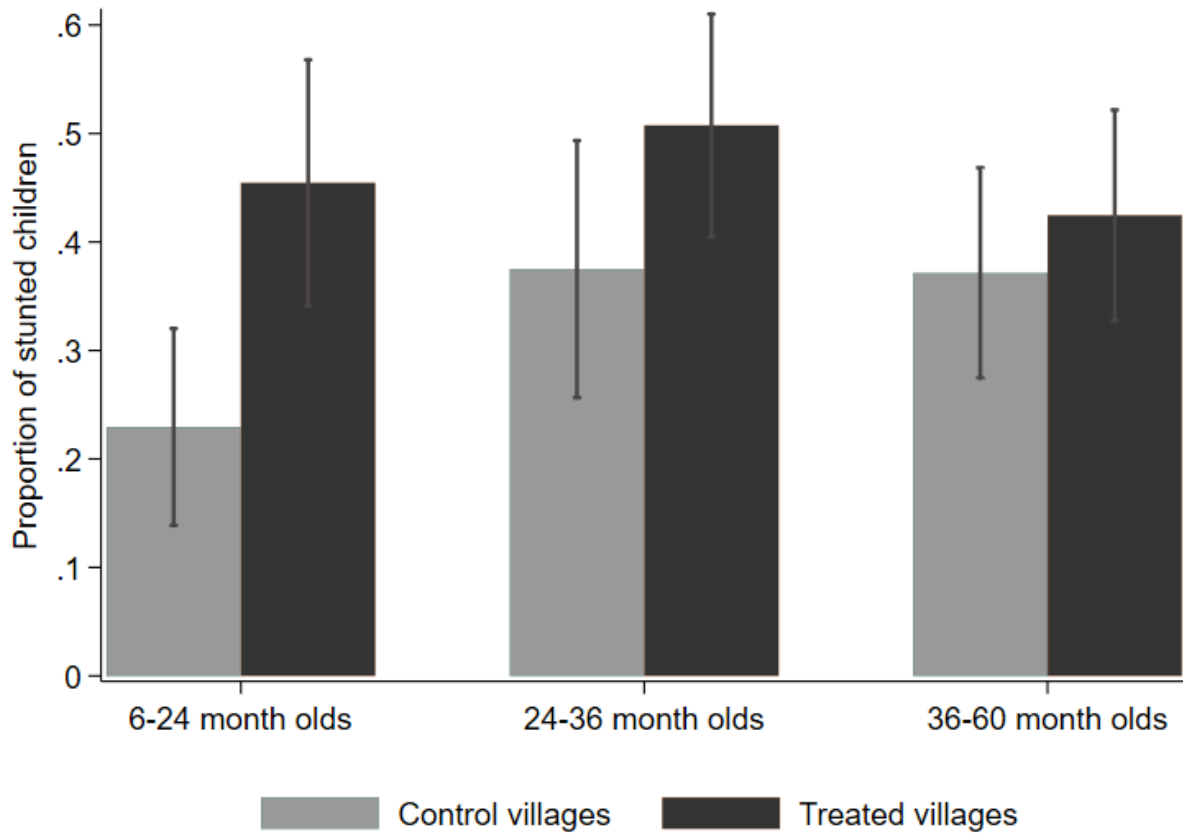
Source: 2011 *Pantawid* impact evaluation survey

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

Table 1A: Program impact on anthropometry of beneficiary children aged 6-36 months

	(1) Height-for-Age Z score	(2) Stunting	(3) Severe stunting	(4) Weight-for-Age Z score	(5) Underweight
Panel A: Program impact					
Program village	0.256*	-0.035	-0.093**	0.125	-0.023
	(0.15)	(0.05)	(0.04)	(0.15)	(0.05)
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
Source: Kandpal et al. (2018)					

note: *** p<0.01, ** p<0.05, * p<0.1
All specifications include municipality fixed effects. Prices are in natural logs. Child-level specifications include quadratic terms for child age (in months). Standard errors are clustered by village; there are 130 villages. Source: 2011 *Pantawid* impact evaluation survey

Table 1B: Program impact on the 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	(6) Whether Dairy Was Fed to Child in Past Week
Panel B: Program impact						
Program village	-0.029**	0.082**	0.21	0.027	0.029	0.069*
	(0.02)	(0.03)	(0.17)	(0.04)	(0.03)	(0.04)
Control observations	328	405	402	406	406	399
Treated observations	335	437	434	437	436	433
Control mean	0.691	0.704	1.808	0.5	0.852	0.248

Table 2: 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	-0.083*	-0.009	-0.020
	(0.046)	(0.020)	(0.037)
Saturation	-0.007	0.017	0.004
	(0.062)	(0.027)	(0.049)
Program village* saturation	0.164**	0.014	0.034
	(0.072)	(0.031)	(0.057)
Panel C: Interaction with above median 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)
Program village *Above median saturation	0.060**	-0.007	0.009
	(0.027)	(0.012)	(0.022)
Panel D: Interaction with fourth quartile saturation			
Program village	-0.011	-0.001	0.005
	(0.027)	(0.006)	(-0.012)
Above median saturation	0.030	0.014	0.027
	(0.032)	(0.015)	(0.021)
Program village *Above median saturation	0.054	-0.003	-0.018
	(0.038)	(0.017)	(0.025)
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. All prices are in natural logs.

Source: 2011 *Pantawid* impact evaluation survey

Table 3: Selected food prices (ln) and program saturation in nationally representative data

	Rice, raw regular milled	Eggs (fresh)	Fish (fresh)	Meats (fresh)	Vegetables (fresh)	Milk Products
Panel A: Model with cutoff at saturation of 50 percent						
Saturation	0.061***	0.083***	-0.016	0.002	0.030	0.140**
	(0.014)	(0.018)	(0.015)	(0.013)	(0.018)	(0.063)
Saturation*Above cutoff	0.088*	0.185***	0.090*	0.053	0.180***	-0.008
	(0.049)	(0.062)	(0.054)	(0.044)	(0.062)	(0.237)
Above cutoff	-0.022	-0.094**	-0.042	-0.007	-0.061	-0.095
	(0.030)	(0.038)	(0.033)	(0.027)	(0.038)	(0.144)
Saturation + Saturation*Above cutoff	0.149***	0.268***	0.074	0.055	0.210***	0.132
	(0.047)	(0.059)	(0.051)	(0.042)	(0.058)	(0.227)
Panel B: Model with cutoff at saturation of 65 percent						
Saturation	0.078***	0.089***	-0.009	0.019	0.054***	0.071
	(0.013)	(0.016)	(0.014)	(0.012)	(0.017)	(0.058)
Saturation*Above cutoff	-0.252***	0.154	0.332***	0.029	0.160	0.527
	(0.088)	(0.110)	(0.095)	(0.080)	(0.109)	(0.429)
Above cutoff	0.245***	-0.070	-0.249***	-0.004	-0.060	-0.456
	(0.067)	(0.085)	(0.072)	(0.062)	(0.085)	(0.328)
Saturation + Saturation*Above cutoff	-0.174**	0.242**	0.323***	0.048	0.214**	0.598
	(0.086)	(0.107)	(0.093)	(0.079)	(0.107)	(0.422)
Panel C: Model with cutoff at saturation of 75 percent						
Saturation	0.095***	0.097***	-0.014	0.023**	0.064***	0.055
	(0.012)	(0.016)	(0.013)	(0.011)	(0.016)	(0.056)
Saturation*Above cutoff	-0.489***	0.220	0.194	0.070	0.154	0.309
	(0.147)	(0.185)	(0.158)	(0.132)	(0.184)	(0.743)
Above cutoff	0.440***	-0.139	-0.116	-0.047	-0.058	-0.250
	(0.127)	(0.160)	(0.137)	(0.114)	(0.161)	(0.643)
Saturation + Saturation*Above cutoff	-0.394***	0.317*	0.180	0.093	0.217	0.364
	(0.146)	(0.183)	(0.157)	(0.131)	(0.183)	(0.737)
Panel D: Linear model						
Saturation	0.095***	0.114***	0.001	0.028***	0.087***	0.062
	(0.011)	(0.015)	(0.012)	(0.010)	(0.015)	(0.053)

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. Prices are calculated as unit values; see text for details.
Source: 2009, 2012, and 2015 Family Income and Expenditure Surveys and Department of Social Welfare Administrative Data on *Pantawid* Program Enrollment

Table 4: Program impact on food budget share for nonbeneficiary households with 6-60-month-old children and children's food intake

	(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	(6) Whether Dairy Was Fed to Child in Past Week
Panel A: Program impact						
Program village	0.036*	0.037	-0.154	-0.058	0.002	0.011
	(0.021)	(0.038)	(0.170)	(0.046)	(0.036)	(0.047)
Panel B: Interaction with saturation						
Program village	-0.016	0.301**	0.743	0.102	-0.015	0.382***
	(0.066)	(0.127)	(0.640)	(0.131)	(0.113)	(0.135)
Saturation	0.089	-0.071	-0.216	-0.385**	-0.313**	0.171
	(0.104)	(0.212)	(0.963)	(0.173)	(0.152)	(0.200)
Program village*Saturation	0.085	-0.439**	-1.495	-0.253	0.045	-0.627***
	(0.117)	(0.215)	(1.056)	(0.223)	(0.178)	(0.226)
Panel C: Interaction with above median saturation						
Program village	0.033	0.123**	0.063	-0.047	0.005	0.064
	(0.024)	(0.049)	(0.221)	(0.058)	(0.048)	(0.066)
Above median saturation	0.032	0.100	0.184	-0.105	-0.114**	0.058
	(0.041)	(0.068)	(0.311)	(0.064)	(0.052)	(0.080)
Program village*Above median saturation	0.007	-0.203***	-0.504	-0.024	-0.006	-0.125
	(0.044)	(0.077)	(0.350)	(0.095)	(0.067)	(0.097)
Panel D: Interaction with top quartile of saturation						
Program village	0.031	0.053	-0.022	-0.026	0.002	0.054
	(0.024)	(0.044)	(0.184)	(0.051)	(0.039)	(0.054)
Fourth quartile of saturation	0.005	0.033	0.516	0.042	-0.144**	0.012
	(0.047)	(0.085)	(0.476)	(0.073)	(0.071)	(0.092)
Program village*Fourth quartile of saturation	0.026	-0.092	-0.846	-0.176	0.055	-0.212**
	(0.057)	(0.099)	(0.516)	(0.120)	(0.093)	(0.108)
Control Observations	214	269	268	269	269	269
Treated Observation	230	295	292	292	293	292
Control Mean	0.596	0.755	2.131	0.665	0.818	0.342

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

All child-level specifications include quadratic terms 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 5: 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	-0.431**	0.123**	0.044	-0.111	0.06
	(0.17)	(0.06)	(0.03)	(0.17)	(0.04)
Panel B: Interaction with saturation					
Program village	0.043	-0.137	-0.038	1.557***	-0.381**
	(0.63)	(0.22)	(0.14)	(0.58)	(0.15)
Saturation	-0.892	0.197	0.068	0.249	-0.291
	(0.79)	(0.24)	(0.15)	(0.63)	(0.21)
Program village*saturation	-0.773	0.428	0.136	-2.786***	0.745***
	(1.00)	(0.34)	(0.23)	(0.94)	(0.25)
Panel C: Interaction with above median saturation					
Program village	-0.314	0.061	0.039	0.257	-0.046
	(0.24)	(0.08)	(0.04)	(0.22)	(0.05)
Above median saturation	-0.397	0.074	0.048	0.066	-0.075
	(0.29)	(0.08)	(0.04)	(0.26)	(0.07)
Program village*Above median saturation	-0.196	0.117	0.006	-0.798**	0.231***
	(0.35)	(0.11)	(0.07)	(0.34)	(0.09)
Panel D: Interaction with top quartile of saturation					
Program village	-0.244	0.041	0.012	0.127	0.018
	(0.20)	(0.06)	(0.03)	(0.18)	(0.05)
Fourth quartile of saturation	0.073	0.02	-0.009	0.498*	-0.125
	(0.37)	(0.09)	(0.07)	(0.27)	(0.09)
Program village*Fourth quartile of saturation	-0.740*	0.319**	0.139	-1.320***	0.245**
	(0.41)	(0.13)	(0.10)	(0.37)	(0.11)
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 quadratic terms 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 6: Program impact on health seeking behavior of nonbeneficiary households

	Pregnant women/last pregnancy in the last 36 months					Children younger than 60 months old		
	Any antenatal care	Timely initiation of ANC	Content of care in ANC	Postnatal care within 24 hours	Institutional Delivery	MMR	Growth Monitoring	Treatment seeking
Panel A: Program impact								
Program village	-0.039	0.004	0.000	0.000	-0.061	0.032	0.020	0.001
	(0.029)	(0.059)	(0.202)	(0.054)	(0.060)	(0.048)	(0.032)	(0.044)
Panel B: Interaction with saturation								
Program village	-0.074	0.022	-0.819	-0.087	-0.204	-0.170	0.144	0.117
	(0.060)	(0.229)	(0.592)	(0.207)	(0.195)	(0.143)	(0.110)	(0.161)
Saturation	-0.168	-0.227	-2.577***	-0.285	-0.631***	-0.125	-0.088	-0.168
	(0.123)	(0.301)	(0.860)	(0.262)	(0.231)	(0.220)	(0.133)	(0.224)
Program village*saturation	0.066	-0.018	1.498	0.159	0.267	0.345	-0.208	-0.191
	(0.110)	(0.363)	(0.994)	(0.333)	(0.312)	(0.249)	(0.172)	(0.273)
Panel C: Interaction with above median saturation								
Program village	-0.043	-0.061	-0.154	-0.018	-0.068	-0.029	0.056	0.015
	(0.031)	(0.082)	(0.228)	(0.076)	(0.080)	(0.063)	(0.043)	(0.052)
Above median saturation	-0.075	-0.192**	-0.791**	-0.029	-0.164*	-0.137*	-0.014	-0.088
	(0.049)	(0.093)	(0.338)	(0.076)	(0.090)	(0.083)	(0.046)	(0.072)
Program village*above median saturation	0.011	0.146	0.347	0.042	0.021	0.142	-0.083	-0.031
	(0.044)	(0.115)	(0.381)	(0.119)	(0.115)	(0.102)	(0.063)	(0.094)
Panel D: Interaction with top quartile of saturation								
Program village	-0.030	0.037	0.114	-0.010	-0.048	0.046	0.031	0.019
	(0.024)	(0.063)	(0.216)	(0.062)	(0.069)	(0.055)	(0.037)	(0.046)
Fourth quartile of saturation	-0.107	0.032	-0.380	0.092	-0.079	0.022	-0.039	0.144
	(0.082)	(0.143)	(0.482)	(0.118)	(0.087)	(0.079)	(0.051)	(0.108)
Program village*fourth quartile of saturation	0.019	-0.153	-0.329	-0.011	-0.012	-0.078	-0.038	-0.135
	(0.091)	(0.160)	(0.544)	(0.142)	(0.121)	(0.108)	(0.065)	(0.137)
Control observations	172	180	169	178	180	239	268	258
Treated observations	202	203	184	198	203	267	299	291
Control mean	0.965	0.472	5.533	0.371	0.439	0.473	0.209	0.508

note: *** p<0.01, ** p<0.05, * p<0.1
Child level outcomes include quadratic terms 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

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 12 to 14 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 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 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: Characteristic balance at baseline: nonbeneficiary households in top quartile of saturated areas

Baseline survey variables	Control Mean	Difference (Treated-Control)	Standardized Mean Difference	p-value
LN Proxy Means Test Score	9.797	-0.013	-0.037	0.642
Household composition:				
Household size	4.096	-0.742**	-0.189	0.025
Children 5 years old and below	0.521	-0.143*	-0.150	0.071
Children between 6 and 14 years old	0.877	-0.168	-0.106	0.196
Primary occupation: Farming and livestock	0.425	-0.037	-0.027	0.739
Educational attainment of the household head:				
No grade completed	0.055	0.021	0.040	0.618
Some elementary school	0.247	0.049	0.046	0.566
Completed elementary school	0.247	-0.109***	-0.210	0.013
Some high school	0.082	0.019	0.031	0.701
High school graduate	0.192	-0.054	-0.082	0.311
Some college	0.110	0.020	0.032	0.692
College graduate	0.068	0.055	0.113	0.168
School Attendance:				
Children between 6 to 11 years	0.882	0.090*	0.149	0.076
Children between 12 to 14 years	0.762	-0.081	-0.042	0.608
Housing Amenities:				
Strong roof materials	0.507	-0.021	-0.017	0.834
Strong wall materials	0.466	-0.148*	-0.134	0.104
Light roof materials	0.233	-0.010	-0.009	0.908
Light wall materials	0.192	0.106	0.114	0.164
Owns a house and lot	0.315	0.063	0.069	0.392
House has toilet	0.192	0.167**	0.205	0.015
Shares a water source	0.288	-0.160**	-0.172	0.040
Household Assets:				
Electricity in house	0.781	-0.072	-0.122	0.138
Owns a television	0.548	-0.084	-0.091	0.266
Owns a video recorder	0.466	-0.283***	-0.251	0.004
Owns a Stereo/CD player	0.411	-0.153*	-0.138	0.094
Owns a refrigerator	0.397	-0.144**	-0.185	0.027
Has a telephone/cellphone	0.274	-0.086	-0.077	0.345
Owns a motorcycle	0.192	-0.093	-0.098	0.227

*** 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 6: 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
Fish	83.58	82.37
Green Vegetables	86.86	85.02
Banana	81.75	81.12
Eggs	73.11	71.92
Meat	56.45	53.98
Sweet potatoes	49.15	47.27
Papaya	36.74	36.35
Mango	20.32	19.34
Dairy	27.86	28.55
Carrot	13.5	13.1

Source: 2011 *Pantawid* impact evaluation survey

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

	Height-for-Age Z score	Stunted	Severely stunted	Weight-for-Age Z score	Underweight
Panel A: Interaction with saturation					
Program village	0.324	-0.369**	-0.099	-0.394	0.086
	(0.442)	(0.153)	(0.113)	(0.471)	(0.146)
Saturation	-0.264	-0.231	0.363*	-1.384**	0.243
	(0.678)	(0.249)	(0.195)	(0.595)	(0.203)
	-0.115	0.532**	0.016	0.827	-0.175
Program village*Saturation	(0.691)	(0.248)	(0.182)	(0.744)	(0.233)
Panel B: Interaction with above median saturation					
Program village	0.098	-0.084	-0.095*	0.099	0.020
	(0.206)	(0.074)	(0.055)	(0.197)	(0.068)
Above median saturation	-0.033	-0.127	0.019	-0.050	-0.016
	(0.297)	(0.096)	(0.072)	(0.244)	(0.077)
Program village*Above	0.313	0.097	0.004	0.052	-0.086
median saturation	(0.295)	(0.107)	(0.082)	(0.302)	(0.094)
Panel C: Interaction with top quartile of saturation					
Program village	0.335*	-0.071	-0.120**	0.022	-0.014
	(0.191)	(0.062)	(0.050)	(0.163)	(0.056)
	0.225	-0.065	0.013	-0.104	0.018
Fourth quartile of saturation	(0.265)	(0.087)	(0.081)	(0.258)	(0.073)
Program village*Fourth	-0.264	0.120	0.092	0.343	-0.032
quartile of saturation	(0.332)	(0.125)	(0.093)	(0.361)	(0.107)
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

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

All specifications include municipality fixed effects. All specifications include quadratic terms for child age (in months). Standard errors are clustered by village; there are 130 villages.

Source: 2011 *Pantawid* impact evaluation survey

Appendix Table 8: 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 9: 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 Saturation						
Program village	0.152	0.141	0.295	0.003	0.160	0.139
	(0.107)	(0.215)	(0.373)	(0.207)	(0.304)	(0.450)
Saturation	-0.138	0.098	0.091	-0.533***	-0.348	-0.747
	(0.138)	(0.305)	(0.535)	(0.182)	(0.293)	(0.520)
Program village*saturation	-0.221	-0.234	-0.451	-0.062	-0.230	-0.160
	(0.173)	(0.354)	(0.577)	(0.322)	(0.475)	(0.701)
Panel C: 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)
Panel D: Interaction with top quartile of saturation						
Program impact	0.036	-0.003	0.037	-0.022	0.046	0.093
	(0.029)	(0.069)	(0.129)	(0.050)	(0.076)	(0.141)
Fourth quartile of saturation	0.069	0.204	0.322	0.092	0.135	0.356*
	(0.061)	(0.147)	(0.247)	(0.085)	(0.100)	(0.201)
Program Village* fourth quartile of saturation	-0.059	0.024	-0.035	-0.105	-0.147	-0.320
	(0.075)	(0.163)	(0.231)	(0.112)	(0.141)	(0.277)
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 10: 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	-0.016	0.012	0.025	0.058	-0.030
	(0.148)	(0.071)	(0.042)	(0.125)	(0.046)
Dummy: 6-24 months old	0.213	-0.049	-0.059	0.290	-0.038
	(0.445)	(0.156)	(0.083)	(0.416)	(0.115)
Dummy: 24-36 months old	0.159	-0.112	-0.006	0.262	0.039
	(0.301)	(0.108)	(0.055)	(0.252)	(0.081)
Program village*6-24 dummy	-0.622**	0.131	0.062	-0.166	0.198**
	(0.259)	(0.098)	(0.060)	(0.247)	(0.078)
Program village*24-36 dummy	-0.165	0.097	-0.040	-0.157	-0.035
	(0.255)	(0.095)	(0.063)	(0.247)	(0.082)
Control Observations	242	242	242	265	265
Treated Observations	265	265	265	287	287
Control Mean	-1.305	0.335	0.070	-0.966	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 quadratic terms 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 11: Program impact on health seeking behavior of beneficiary households

	Pregnant women/last pregnancy in the last 36 months					Children younger than 60 months old		
	Any antenatal care	Timely initiation of ANC	Content of care in ANC	Postnatal care within 24 hours	Institutional Delivery	MMR	Growth Monitoring	Treatment seeking
Panel A: Program impact								
Program village	0.039	0.107**	0.719***	0.102**	0.018	0.056	0.152***	0.134***
	(0.031)	(0.049)	(0.201)	(0.049)	(0.048)	(0.039)	(0.032)	(0.039)
Panel B: Interaction with saturation								
Program village	-0.052	0.440**	1.133	0.235	-0.125	-0.072	0.321***	0.043
	(0.104)	(0.175)	(0.694)	(0.172)	(0.143)	(0.121)	(0.110)	(0.139)
Saturation	-0.450***	0.376	-2.485***	-0.657***	-0.873***	-0.260	0.047	-0.401**
	(0.173)	(0.232)	(0.960)	(0.198)	(0.199)	(0.167)	(0.113)	(0.179)
Program village*saturation	0.144	-0.526**	-0.613	-0.214	0.226	0.204	-0.275*	0.142
	(0.193)	(0.265)	(1.132)	(0.252)	(0.222)	(0.198)	(0.165)	(0.207)
Panel C: Interaction with above median saturation								
Program village	0.014	0.166**	0.716***	0.124*	-0.020	0.034	0.179***	0.070
	(0.030)	(0.071)	(0.261)	(0.067)	(0.070)	(0.050)	(0.045)	(0.057)
Above median saturation	-0.072	0.101	-0.783***	-0.072	-0.187**	-0.001	0.051	-0.131**
	(0.052)	(0.086)	(0.296)	(0.073)	(0.074)	(0.070)	(0.053)	(0.065)
Program village*above median saturation	0.046	-0.110	0.004	-0.047	0.065	0.048	-0.053	0.127
	(0.060)	(0.102)	(0.400)	(0.094)	(0.100)	(0.080)	(0.070)	(0.078)
Panel D: Interaction with top quartile of saturation								
Program village	0.029	0.169***	0.762***	0.154***	0.029	0.044	0.195***	0.115**
	(0.028)	(0.057)	(0.209)	(0.055)	(0.059)	(0.044)	(0.039)	(0.048)
Fourth quartile of saturation	-0.083	0.213**	-0.771*	-0.094	-0.148*	-0.020	0.070	-0.036
	(0.063)	(0.093)	(0.400)	(0.081)	(0.080)	(0.081)	(0.051)	(0.066)
Program village*fourth quartile of saturation	0.033	-0.201*	-0.109	-0.161*	-0.031	0.041	-0.153**	0.066
	(0.088)	(0.105)	(0.501)	(0.093)	(0.100)	(0.097)	(0.071)	(0.083)
Control observations	221	227	205	223	227	376	411	403
Treated observations	233	242	226	241	242	408	443	436
Control mean	0.910	0.410	4.932	0.296	0.291	0.436	0.175	0.409

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

Child level outcomes include quadratic terms 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

Appendix Table 12: 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 saturation								
Program village	-0.003	0.049	-0.047	0.072	0.042	0.407**	0.386*	0.367
	(0.073)	(0.159)	(0.204)	(0.242)	(0.073)	(0.197)	(0.221)	(0.344)
Saturation	-0.166	-0.344	-0.763***	-0.177	-0.095	0.147	-0.311	0.105
	(0.150)	(0.210)	(0.281)	(0.288)	(0.110)	(0.275)	(0.316)	(0.538)
Program village* saturation	0.036	-0.027	0.181	-0.162	-0.064	-0.629*	-0.691*	-0.451
	(0.142)	(0.268)	(0.324)	(0.418)	(0.137)	(0.365)	(0.401)	(0.599)
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)
Panel B: Interaction with top quartile of saturation								
Program village	-0.009	0.007	0.065	0.007	-0.007	0.079	-0.004	0.172*
	(0.023)	(0.057)	(0.061)	(0.085)	(0.022)	(0.072)	(0.079)	(0.094)
Fourth Quartile of saturation	-0.086	-0.118	-0.097	0.079	-0.067	0.063	-0.048	0.023
	(0.070)	(0.096)	(0.090)	(0.114)	(0.052)	(0.119)	(0.123)	(0.158)
Program village* fourth quartile of saturation	0.115	0.091	-0.079	-0.194	0.042	-0.130	-0.113	-0.361
	(0.079)	(0.129)	(0.138)	(0.222)	(0.079)	(0.181)	(0.177)	(0.292)
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 13: 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	0.006	-0.062	-0.043**	-0.015	0.006	-0.004
	(0.028)	(0.065)	(0.019)	(0.019)	(0.109)	(0.073)
Saturation	0.006	-0.090	-0.034*	-0.018	-0.055	-0.184**
	(0.037)	(0.055)	(0.020)	(0.024)	(0.118)	(0.084)
Program village*saturation	-0.017	0.129	0.069**	0.024	0.043	0.024
	(0.047)	(0.112)	(0.029)	(0.034)	(0.163)	(0.105)
Panel B: Interaction with above median 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)
Panel B: Interaction with top quartile of saturation						
Program village	-0.008	0.015	-0.009	-0.003	0.021	0.008
	(0.011)	(0.020)	(0.007)	(0.011)	(0.031)	(0.021)
Fourth quartile of saturation	-0.007	-0.001	-0.017	0.007	-0.038	-0.017
	(0.019)	(0.030)	(0.012)	(0.013)	(0.026)	(0.021)
Program village*fourth quartile of saturation	0.028	-0.022	0.030**	0.011	0.065	0.011
	(0.024)	(0.049)	(0.013)	(0.013)	(0.052)	(0.025)
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.[34] 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

---

[34] 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	-0.415	0.152	0.017	-0.488*	0.180**
median saturation	(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	-0.253	0.129	0.007	-0.626*	0.208**
median saturation	(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	-0.443	0.146	0.046	-0.798**	0.231***
median saturation	(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	-0.648	0.146	0.046	-0.853**	0.231***
median saturation	(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	-0.771**	0.146	0.046	-0.812**	0.231***
median saturation	(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	-0.000	-0.005	-0.033	0.518*	-0.125
saturation	(0.525)	(0.091)	(0.068)	(0.279)	(0.087)
Program village*Fourth	-0.883	0.329**	0.159	-1.322***	0.245**
quartile of saturation	(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.[35] We aggregate items into each of these categories using as weights the national average food expenditure share of each item in 2009:[36]

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

Finally, we estimate the models using the natural log of these estimated unit values.

<u>Full set of results</u>

Table 5 of the paper presents selected results from various specifications relating unit values to saturations. The models allow for the relationship to vary above and below a cutoff point, with the cutoff set in turn at 50, 65, and 75 percent saturation. An additional model estimates a simple linear model. Annex 2 Tables 1 to 4 report the full set of results from these specifications (that is, the results for all the different foods).

<u>Robustness of unit values results</u>

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 2 Tables 5 and 6 as compared to Annex 2 Tables 2 and 4 (that is comparing the interaction models with a cutoff at 65 percent, as well as the linear models) the results are only minimally affected.

---

[35] 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.
[36] Regular rice and sugar are reported as single unit values and don't require this aggregation.

Annex 2 Table 1: Food prices (ln) and program saturation in nationally representative data; Model with cutoff at saturation of 50 percent

	Share of food expenditures	Saturation	Saturation*Above cutoff	Above cutoff	N	Saturation + Saturation*Above cutoff
Rice, raw regular milled	0.191	0.061 ***	0.088 *	-0.022	8370	0.149 ***
		(0.014)	(0.049)	(0.030)		(0.047)
Other raw rice and grains	0.074	-0.030	-0.301	0.034	8555	-0.332
		(0.111)	(0.371)	(0.228)		(0.351)
Roots and tubers	0.011	0.247 ***	0.357 *	-0.196	8487	0.604 ***
		(0.055)	(0.197)	(0.120)		(0.187)
Eggs (fresh)	0.018	0.083 ***	0.185 ***	-0.094 **	8551	0.268 ***
		(0.018)	(0.062)	(0.038)		(0.059)
Eggs (processed)	0.001	0.097 ***	0.135	-0.076	6449	0.232
		(0.026)	(0.155)	(0.091)		(0.152)
Fish (fresh)	0.058	-0.016	0.090 *	-0.042	8530	0.074
		(0.015)	(0.054)	(0.033)		(0.051)
Fish (processed)	0.023	-0.075 *	0.162	-0.096	8479	0.087
		(0.042)	(0.152)	(0.092)		(0.144)
Meats (fresh)	0.035	0.002	0.053	-0.007	8510	0.055
		(0.013)	(0.044)	(0.027)		(0.042)
Meats (processed)	0.011	1.779 ***	-5.163 ***	2.781 ***	8242	-3.384 **
		(0.359)	(1.383)	(0.836)		(1.334)
Fruits (fresh)	0.021	-0.211 ***	0.275 **	-0.106	8512	0.064
		(0.034)	(0.118)	(0.072)		(0.111)
Vegetables (fresh)	0.046	0.030	0.180 ***	-0.061	8561	0.210 ***
		(0.018)	(0.062)	(0.038)		(0.058)
Rice and grains processed	0.021	0.169 ***	-0.509 ***	0.290 ***	8555	-0.340 **
		(0.054)	(0.180)	(0.110)		(0.170)
Coffee, cocoa, tea	0.025	0.093 **	-0.023	0.040	8559	0.070
		(0.046)	(0.156)	(0.096)		(0.148)
Sugar	0.016	-0.541 ***	0.390 ***	-0.163 ***	8529	-0.151 *
		(0.029)	(0.097)	(0.060)		(0.092)
Milk Products	0.007	0.140 **	-0.008	-0.095	8117	0.132
		(0.063)	(0.237)	(0.144)		(0.227)

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. Prices are calculated as unit values, see text for details.
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: Food prices (ln) and program saturation in nationally representative data; Model with cutoff at saturation of 65 percent

	Share of food expenditures	Saturation	Saturation*Above cutoff	Above cutoff	N	Saturation + Saturation*Above cutoff
Rice, raw regular milled	0.191	0.078 ***	-0.252 ***	0.245 ***	N	-0.174 **
		(0.013)	(0.088)	(0.067)	8370	(0.086)
Other raw rice and grains	0.074	-0.157	0.474	-0.456		0.318
		(0.100)	(0.654)	(0.506)	8555	(0.640)
Roots and tubers	0.011	0.241 ***	0.771 **	-0.519 *		1.012 ***
		(0.050)	(0.351)	(0.270)	8487	(0.344)
Eggs (fresh)	0.018	0.089 ***	0.154	-0.070		0.242 **
		(0.016)	(0.110)	(0.085)	8551	(0.107)
Eggs (processed)	0.001	0.093 ***	0.424	-0.284		0.517 *
		(0.025)	(0.284)	(0.218)	6449	(0.283)
Fish (fresh)	0.058	-0.009	0.332 ***	-0.249 ***		0.323 ***
		(0.014)	(0.095)	(0.072)	8530	(0.093)
Fish (processed)	0.023	-0.066 *	0.638 **	-0.512 **		0.572 **
		(0.038)	(0.290)	(0.223)	8479	(0.285)
Meats (fresh)	0.035	0.019	0.029	-0.004		0.048
		(0.012)	(0.080)	(0.062)	8510	(0.079)
Meats (processed)	0.011	1.652 ***	-4.552 *	2.523		-2.901
		(0.330)	(2.692)	(2.064)	8242	(2.657)
Fruits (fresh)	0.021	-0.178 ***	0.091	0.017		-0.087
		(0.031)	(0.207)	(0.160)	8512	(0.203)
Vegetables (fresh)	0.046	0.054 ***	0.160	-0.060		0.214 **
		(0.017)	(0.109)	(0.085)	8561	(0.107)
Rice and grains processed	0.021	0.176 ***	-0.429	0.213		-0.253
		(0.049)	(0.316)	(0.245)	8555	(0.310)
Coffee, cocoa, tea	0.025	0.114 ***	-0.262	0.219		-0.149
		(0.042)	(0.277)	(0.214)	8559	(0.271)
Sugar	0.016	-0.504 ***	0.590 ***	-0.360 ***		0.086
		(0.026)	(0.174)	(0.135)	8529	(0.170)
Milk Products	0.007	0.071	0.527	-0.456		0.598
		(0.058)	(0.429)	(0.328)	8117	(0.422)

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. Prices are calculated as unit values, see text for details.
Source: 2009, 2012, and 2015 Family Income and Expenditure Surveys and Department of Social Welfare Administrative Data on *Pantawid* Program Enrollment

Annex 2 Table 3: Food prices (ln) and program saturation in nationally representative data; Model with cutoff at saturation of 75 percent

	Share of food expenditures	Saturation	Saturation*Above cutoff	Above cutoff	N	Saturation + Saturation*Above cutoff
Rice, raw regular milled	0.191	0.095 ***	-0.489 ***	0.440 ***	8370	-0.394 ***
		(0.012)	(0.147)	(0.127)		(0.146)
Other raw rice and grains	0.074	-0.193 **	-1.071	1.003	8555	-1.264
		(0.096)	(1.103)	(0.963)		(1.093)
Roots and tubers	0.011	0.243 ***	1.315 **	-1.005 **	8487	1.558 ***
		(0.048)	(0.579)	(0.500)		(0.574)
Eggs (fresh)	0.018	0.097 ***	0.220	-0.139	8551	0.317 *
		(0.016)	(0.185)	(0.160)		(0.183)
Eggs (processed)	0.001	0.092 ***	-0.134	0.229	6449	-0.042
		(0.025)	(0.509)	(0.440)		(0.508)
Fish (fresh)	0.058	-0.014	0.194	-0.116	8530	0.180
		(0.013)	(0.158)	(0.137)		(0.157)
Fish (processed)	0.023	-0.080 **	0.939 *	-0.773 *	8479	0.859 *
		(0.036)	(0.486)	(0.418)		(0.481)
Meats (fresh)	0.035	0.023 **	0.070	-0.047	8510	0.093
		(0.011)	(0.132)	(0.114)		(0.131)
Meats (processed)	0.011	1.476 ***	-6.996	5.103	8242	-5.520
		(0.321)	(4.295)	(3.667)		(4.268)
Fruits (fresh)	0.021	-0.154 ***	1.281 ***	-1.099 ***	8512	1.127 ***
		(0.030)	(0.349)	(0.304)		(0.346)
Vegetables (fresh)	0.046	0.064 ***	0.154	-0.058	8561	0.217
		(0.016)	(0.184)	(0.161)		(0.183)
Rice and grains processed	0.021	0.148 ***	-1.073 **	0.848 *	8555	-0.925 *
		(0.047)	(0.534)	(0.466)		(0.530)
Coffee, cocoa, tea	0.025	0.126 ***	-0.381	0.306	8559	-0.256
		(0.040)	(0.466)	(0.406)		(0.462)
Sugar	0.016	-0.489 ***	0.993 ***	-0.750 ***	8529	0.505 *
		(0.025)	(0.289)	(0.252)		(0.286)
Milk Products	0.007	0.055	0.309	-0.250	8117	0.364
		(0.056)	(0.743)	(0.643)		(0.737)

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. Prices are calculated as unit values, see text for details.
Source: 2009, 2012, and 2015 Family Income and Expenditure Surveys and Department of Social Welfare Administrative Data on *Pantawid* Program Enrollment

*Annex 2 Table 4: Food prices (ln) and program saturation in nationally representative data;
Linear model*

	Share of food expenditures	Saturation	N
Rice, raw regular milled	0.191	0.095 ***	8370
		(0.011)	
Other raw rice and grains	0.074	-0.185 **	8555
		(0.089)	
Roots and tubers	0.011	0.290 ***	8487
		(0.045)	
Eggs (fresh)	0.018	0.114 ***	8551
		(0.015)	
Eggs (processed)	0.001	0.102 ***	6449
		(0.024)	
Fish (fresh)	0.058	0.001	8530
		(0.012)	
Fish (processed)	0.023	-0.062 *	8479
		(0.034)	
Meats (fresh)	0.035	0.028 ***	8510
		(0.010)	
Meats (processed)	0.011	1.238 ***	8242
		(0.305)	
Fruits (fresh)	0.021	-0.136 ***	8512
		(0.028)	
Vegetables (fresh)	0.046	0.087 ***	8561
		(0.015)	
Rice and grains processed	0.021	0.111 ***	8555
		(0.043)	
Coffee, cocoa, tea	0.025	0.114 ***	8559
		(0.037)	
Sugar	0.016	-0.444 ***	8529
		(0.023)	
Milk Products	0.007	0.062	8117
		(0.053)	

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. Prices are calculated as unit values, see text for details.
Source: 2009, 2012, and 2015 Family Income and Expenditure Surveys and Department of Social Welfare Administrative Data on *Pantawid* Program Enrollment

Annex 2 Table 5: Alternative estimates of relationship between food prices (ln) and program saturation in nationally representative data-- unit values derived after trimming values +/- 3 SD from mean.

	Model with cutoff at saturation of 65 percent				Linear Model
	Saturation	Saturation*Above cutoff	Above cutoff	Saturation + Saturation*Above cutoff	Saturation
Rice, raw regular milled	0.078 ***	-0.251 ***	0.245 ***	-0.173 **	0.095***
	0.013	0.088	0.067	0.086	0.011
Other raw rice and grains	-0.157	0.475	-0.456	0.318	-0.185**
	0.100	0.654	0.506	0.640	0.089
Roots and tubers	0.241 ***	0.767 **	-0.515 *	1.008 ***	0.290***
	0.050	0.351	0.270	0.344	0.045
Eggs (fresh)	0.089 ***	0.154	-0.070	0.242 **	0.114***
	0.016	0.110	0.085	0.107	0.015
Eggs (processed)	0.093 ***	0.424	-0.284	0.517 *	0.102***
	0.025	0.284	0.218	0.283	0.024
Fish (fresh)	-0.009	0.332 ***	-0.249 ***	0.323 ***	0.001
	0.014	0.095	0.072	0.093	0.012
Fish (processed)	-0.066 *	0.638 **	-0.512 **	0.572 **	-0.062*
	0.038	0.290	0.223	0.285	0.034
Meats (fresh)	0.019	0.029	-0.004	0.048	0.028***
	0.012	0.080	0.062	0.079	0.010
Meats (processed)	1.652 ***	-4.552 *	2.523	-2.901	1.238***
	0.330	2.692	2.064	2.657	0.305
Fruits (fresh)	-0.178 ***	0.091	0.017	-0.087	-0.136***
	0.031	0.207	0.160	0.203	0.028
Vegetables (fresh)	0.054 ***	0.161	-0.061	0.214 **	0.087***
	0.017	0.109	0.085	0.107	0.015
Rice and grains processed	0.176 ***	-0.429	0.213	-0.253	0.111***
	0.049	0.317	0.245	0.310	0.043
Coffee, cocoa, tea	0.114 ***	-0.262	0.219	-0.149	0.114***
	0.042	0.277	0.214	0.271	0.037
Sugar	-0.504 ***	0.590 ***	-0.360 ***	0.086	-0.444***
	0.026	0.174	0.135	0.170	0.023
Milk Products	0.071	0.527	-0.456	0.598	0.062
	0.058	0.429	0.328	0.422	0.053

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. Prices are calculated as unit values, see text for details.
Source: 2009, 2012, and 2015 Family Income and Expenditure Surveys and Department of Social Welfare Administrative Data on *Pantawid* Program Enrollment

Annex 2 Table 6: Alternative estimates of relationship between food prices (ln) and program saturation in nationally representative data-- unit values derived from households in top quartile of per capita consumption-expenditures.

| | Model with cutoff at saturation of 65 percent | | | | Linear Model |
	Saturation	Saturation*Above cutoff	Above cutoff	Saturation + Saturation*Above cutoff	Saturation
Rice, raw regular milled	0.081 ***	-0.238 ***	0.231 ***	-0.158 *	0.096***
	0.013	0.090	0.069	0.089	0.012
Other raw rice and grains	-0.167 *	0.495	-0.462	0.328	-0.190**
	0.100	0.652	0.505	0.639	0.088
Roots and tubers	0.248 ***	0.686 *	-0.423	0.934 **	0.308***
	0.058	0.405	0.311	0.397	0.052
Eggs (fresh)	0.088 ***	0.128	-0.051	0.216 *	0.112***
	0.017	0.113	0.087	0.111	0.015
Eggs (processed)	0.099 ***	0.413	-0.277	0.511 *	0.108***
	0.026	0.300	0.231	0.299	0.026
Fish (fresh)	-0.012	0.271 ***	-0.212 ***	0.259 ***	-0.008
	0.014	0.095	0.073	0.094	0.013
Fish (processed)	-0.061	0.642 **	-0.507 **	0.581 **	-0.054
	0.038	0.293	0.226	0.288	0.034
Meats (fresh)	0.015	0.016	0.015	0.030	0.027***
	0.012	0.083	0.064	0.082	0.011
Meats (processed)	1.646 ***	-4.532 *	2.511	-2.887	1.233***
	0.330	2.689	2.062	2.654	0.304
Fruits (fresh)	-0.178 ***	0.117	-0.020	-0.061	-0.144***
	0.031	0.208	0.161	0.204	0.028
Vegetables (fresh)	0.053 ***	0.190 *	-0.078	0.243 **	0.089***
	0.017	0.110	0.085	0.107	0.015
Rice and grains processed	0.176 ***	-0.326	0.146	-0.150	0.119***
	0.049	0.318	0.246	0.311	0.043
Coffee, cocoa, tea	0.128 ***	-0.337	0.253	-0.209	0.116***
	0.042	0.276	0.214	0.270	0.037
Sugar	-0.493 ***	0.564 ***	-0.346 **	0.071	-0.436***
	0.026	0.175	0.136	0.171	0.023
Milk Products	0.087	0.394	-0.362	0.481	0.072
	0.059	0.434	0.331	0.427	0.054

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. Prices are calculated as unit values, see text for details.
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)	(0.09)
After	1.096***	-0.062***	-0.115***	-0.140***
	(0.092)	(0.019)	(0.032)	(0.034)
Program village*After	0.116	0.01	0.062	0.022
	(0.137)	(0.026)	(0.043)	(0.051)
Panel B: Triple difference interaction with saturation				
Program village	0.942*	0.239	0.488	0.515
	(0.499)	(0.158)	(0.304)	(0.321)
After	1.101***	-0.084	0.036	-0.093
	(0.274)	(0.056)	(0.105)	(0.119)
Saturation	-1.499**	-0.154	-0.505	-0.538
	(0.641)	(0.185)	(0.365)	(0.367)
Program village*after	-0.246	-0.083	-0.209	-0.22
	(0.39)	(0.091)	(0.153)	(0.191)
Program village*saturation	-1.558*	-0.440*	-0.862*	-0.873*
	(0.803)	(0.262)	(0.488)	(0.51)
After*Saturation	0.295	0.038	-0.261	-0.078
	(0.47)	(0.101)	(0.166)	(0.191)
Program village*after*saturation	0.683	0.156	0.464**	0.404
	(0.672)	(0.148)	(0.236)	(0.287)
Panel C: Triple difference interaction with above median saturation				
Program village	0.360*	0.033	0.088	0.128
	(0.196)	(0.06)	(0.121)	(0.128)
After	1.077***	-0.081***	-0.106**	-0.122**
	(0.124)	(0.025)	(0.048)	(0.048)
Above median saturation	-0.263	-0.065	-0.196	-0.169
	(0.227)	(0.069)	(0.127)	(0.136)
Program village*After	-0.03	0.008	0.051	-0.04
	(0.17)	(0.035)	(0.065)	(0.077)
Program village*Above median saturation	-0.830***	-0.131	-0.264	-0.314*
	(0.297)	(0.092)	(0.178)	(0.185)
After*Above median saturation	0.058	0.047	-0.014	-0.036
	(0.182)	(0.042)	(0.061)	(0.068)
Program village*After*Above median saturation	0.315	-0.001	0.01	0.121
	(0.282)	(0.051)	(0.083)	(0.096)
Panel D: Triple difference interaction with top quartile of saturation				
Program village	0.142	0.007	0.051	0.097
	(0.166)	-0.049)	-0.098)	(0.102)
After	1.325***	-0.070***	-0.119***	-0.131***
	(0.102)	(0.022)	(0.037)	(0.038)
	-0.473	-0.087	-0.183	-0.123

Fourth quartile of saturation	(0.306)	(0.087)	(0.171)	(0.183)
Program village*After	0.051	0.006	0.062	-0.019
	(0.156)	(0.03)	(0.052)	(0.059)
Program village*fourth quartile of saturation	-0.604*	-0.136	-0.357*	-0.498**
	(0.36)	-(.118)	(0.212)	(0.221)
After*fourth quartile of saturation	-0.292	0.049	0.027	-0.037
	(0.247)	(0.048)	(0.055)	(0.089)
Program village*after*fourth quartile of saturation	0.547	0	-0.014	0.161
	(0.355)	(0.055)	(0.071)	(0.106)
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 saturation					
Program village	-0.076	-0.039	0.015	1.377**	-0.437***
(0.242)	(0.740)	(0.240)	(0.164)	(0.619)	
Saturation	-0.757	0.159	0.130	0.456	-0.437*
	(0.937)	(0.273)	(0.186)	(0.677)	(0.231)
Program village*saturation	-0.638	0.279	0.056	-2.522**	0.813***
	(1.141)	(0.374)	(0.262)	(0.994)	(0.259)
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	-0.076	-0.017	-0.033
	(0.055)	(0.025)	(0.045)
Above median saturation	-0.092	0.008	-0.053
	(0.071)	(0.033)	(0.059)
Above Median Distance to Nearest Market	-0.172*	-0.068	-0.091
	(0.093)	(0.043)	(0.077)
Treated*Saturation	0.195**	0.025	0.063
	(0.092)	(0.042)	(0.076)
Treated*Above Median Travel Time to Nearest Market	-0.053	0.038	0.032
	(0.099)	(0.045)	(0.082)
Saturation* Above Median Travel Time to Nearest Mkt	0.307**	0.089	0.165
	(0.133)	(0.061)	(0.110)
Treatment* Saturation * Above Med. Travel Time to Nearest Market	0.004	-0.047	-0.067
	(0.150)	(0.069)	(0.124)
Program village	0.014	-0.004	0.001
	(0.020)	(0.009)	(0.017)
Fourth quartile of saturation	-0.037	0.021	-0.017
	(0.060)	(0.027)	(0.050)
Above Median Distance	0.013	-0.014	0.008
to Nearest Market	(0.024)	(0.011)	(0.020)
Treated*fourth quartile of saturation	0.103**	0.006	0.008
	(0.042)	(0.019)	(0.034)
Treated*Above Median	-0.017	0.014	0.003
Travel Time to Nearest Market	(0.031)	(0.014)	(0.025)
Fourth quartile of saturation*	0.087***	0.020	0.038
Above Median Travel Time to Nearest Mkt	(0.034)	(0.015)	(0.028)
Treatment*Fourth Quartile of Saturation *	-0.101*	-0.020	-0.036
Above Med. Travel Time to Nearest Market	(0.058)	(0.026)	(0.048)
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[37], 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.

---

[37] 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.