Income and the Demand for Food among the Poor

Marc F. Bellemare, Eeshani Kandpal, Katherina Thomas

Abstract

How much do the poor spend on food when their income increases? We estimate a key economic parameter—the income elasticity of food expenditures—using data from the randomized evaluations of five conditional cash transfer programs in Mexico, Nicaragua, the Philippines, and Uganda. The transfers provided routine, exogenous increases of 12 to 23 percent of baseline income for at least a year to recipients at or below the global poverty line. Using pooled ordinary least squares and Bayesian hierarchical models, we first show that expenditures on all food categories increase with income. But even among some of the poorest people in the world, all of whom are experiencing high hunger levels, our estimated income elasticity for food is 0.03, i.e., much smaller than many published estimates that either rely on cross-sectional variation or study responses to large income shocks. Next, we run the first credible test of Bennett's Law—the empirical regularity whereby poor households respond to income increases by (i) shifting spending from coarse to fine staples, or (ii) spending more on protein than staples—and find partial support for it. While income increases lead consumers to substitute fine grains for coarse grains and protein for staples, again the estimated shifts are smaller than previous estimates. Quantifying how small and routine income changes affect food demand in low- and middle-income countries can inform the policy discourse on poverty reduction, nutrition, and social protection, as well as the debate on the impact of economic growth on global carbon emission patterns.

KEYWORDS
Food Demand, Elasticities, Conditional Cash Transfers, Low- and Middle- Income Countries

JEL CODES
D12, D13, O12, Q11, Q18, R22

WORKING PAPER 701 · AUGUST 2024

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Marc F. Bellemare, Eeshani Kandpal, and Katherina Thomas. 2024. "Income and the Demand for Food among the Poor." CGD Working Paper 701. Washington, DC: Center for Global Development. https://www.cgdev.org/publication/income-and-demand-food-among-poor.

We thank the authors of the articles whose data we re-analyze for making their data available. We are grateful to Chris Barrett, David Evans, Francis Fang, Deon Filmer, Jed Friedman, Thomas Ginn, John Hoddinott, Hosam Ibrahim, Joan Llull, Karen Macours, Amina Mendez Acosta, and Justin Sandefur for their comments on this work. We thank Jonas Hjort for helpful comments on framing. We also thank Brian Webster for research assistance, and Bernhard Dalheimer, Stephen Pitts, and Witold Wiecek for their help with Bayesian hierarchical models. Bellemare is grateful to NIFA for funding this work through grant MIN- 14-167. Thomas acknowledges financing by the European Research Council through Starting Grant 804989 and 101041741. All remaining errors are ours.

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1 Introduction

On the basis of a handful of uncontroversial assumptions, microeconomic theory predicts that food is a normal good, meaning that the demand for food increases with income. But whether food demand is elastic enough with respect to income for income growth to be a sufficient condition for improved nutrition is an empirical question whose answer depends on many proximate factors (Subramanian and Deaton, 1996; Jappelli and Pistaferri, 2010).[1] Published income elasticity estimates of food demand range from close to zero to greater than one (Deaton, 1974; Behrman and Deolalikar, 1987; Deaton, 1989; Bouis and Haddad, 1992; Bouis, 1994; Jappelli and Pistaferri, 2010; Almås, Haushofer and Shapiro, 2019), but all face important challenges to internal or external validity.[2]

Using data from five randomized control trials (RCTs) of cash transfer programs—*Progresa* (Hoddinott and Skoufias, 2004; Attanasio and Pastorino, 2020; Parker and Vogl, 2023) and *Programa de Apoyo Alimentario* (PAL) (Cunha, De Giorgi and Jayachandran, 2019) in Mexico, *Red de Proteccíon Social* (RPS) (Adato and Roopnaraine, 2004; Barham, Macours and Maluccio, 2024) in Nicaragua, *Pantawid* (Kandpal et al., 2016; Filmer et al., 2023) in the Philippines, and the World Food Program Cash and Food Transfer (Gilligan and Roy, 2013) in Uganda, we estimate the income elasticity of food demand among the poor in LMICs. In each program we study, recipient households were selected at random from a larger pool of eligible households to receive conditional cash transfers. Each pro-

[1]Factors that affect the income elasticity of food demand include the size of income changes and expectations regarding their permanence (Bazzi, Sumarto and Suryahadi, 2015; Pennings, 2021), population wealth (Ravallion, 1990), whether local markets can absorb demand shocks (Filmer et al., 2023), quality upgrading (Angelucci and Attanasio, 2013), bulk discounts (Rao, 2000; Attanasio and Pastorino, 2020), and the duration of the observed consumption impacts (Jappelli and Pistaferri, 2010; Krueger, Malkov and Perri, 2023).

[2]Early estimates of the income elasticity of food demand suggest that income growth should be a sufficient condition for improved nutrition (Deaton, 1974; Strauss and Thomas, 1990; Subramanian and Deaton, 1996). In contrast, Behrman and Deolalikar (1987), Bouis and Haddad (1992), and Bouis (1994) find income elasticities of food demand to be close to or statistically insignificantly different from zero. More recently, Almås, Haushofer and Shapiro (2019) estimate the impacts of a large, exogenous, one-time income shock on the food share of expenditures and calorie consumption. The shock they study is an unconditional cash transfer worth approximately one and a half years of a beneficiary's consumption in one lump-sum grant. Their estimated income elasticity for overall food consumption is 0.87 and 1.29 for protein consumption.

gram's eligibility threshold was at or below the World Bank's global poverty line at the time, meaning that our sample comprises households that would be considered "poor" by the World Bank's global poverty line. Where available, the data also reveal high levels of hunger, with at least half of the sample populations in PAL Mexico, the Philippines, and Uganda reporting having experienced some hunger in the recent past.[3,4] Each program provided recipients with monthly or quarterly transfers of between 12 and 23 percent of their baseline income for an extended period, and evaluation data were collected after at least a year of treatment.[5] We report income elasticities using both a binary indicator for randomly assigned treatment status as well as a continuous indicator that exploits cross-country variation in the relative size of the transfer. In nearly all cases, our estimates of the income elasticity of food demand are positive and statistically significant but relatively small in magnitude, suggesting important limits to the extent to which income growth alone can address hunger and malnutrition in LMICs.

We study conditional cash transfer (CCT) programs, each of which targeted the poorest people within the country. These programs have been widely deployed to simultaneously address both poverty and hunger in LMICs (Fiszbein and Schady, 2009). Such CCT programs reach about 800 million households annually, and supported about 1.36 billion households in the first year of the global COVID-19 pandemic (Gentilini, 2023). Indeed,

[3]Hunger is inconsistently measured across the five programs we study. That said, the respective levels of experiential hunger in the recent past in the control villages is 77.72 percent in the data for the Mexican PAL program, 55.5 percent in the Philippines, and 98 percent in Uganda. Data from *Progresa* in Mexico show that the average household only consumes two meals a day. No relevant data are available for Nicaragua, but Nicaragua is poorer than Mexico or the Philippines, with "alarming hunger levels" during the study period (Global Hunger Index, 2019).

[4]A concern with interpreting our results may arise if preferences are non-homothetic, i.e., if people first use income to consume a minimum amount of food, but once that minimum is met, allocate a greater share to other types of consumption. Our study sample, however, represents a narrow slice near the bottom of the income distribution in each country that is poor both locally and globally, and reports very high levels of hunger. While our findings cannot be used to assess the demand for food across the income distribution, they are externally and internally valid estimates of food demand among the poor in LMICs.

[5]In the Philippines, the program we study has been ongoing for over a decade. The *Progresa* program in Mexico was in place from 1997 until it was discontinued in 2019, while the PAL program has been in place since 2003. In both the Philippines and in Mexico, study data were collected after 24 to 30 months of routine transfers. In Nicaragua, the program lasted five years, with data collected after three years of transfers. Finally, in Uganda, the program lasted a year with the data we use here having been collected shortly after the end of the program. Section 2 describes these programs and the data in greater detail.

the two largest programs we study (i.e., *Progresa* and *Pantawid*) together reached about 47 million people across 11 million households at their peak (World Bank, 2017; Parker and Vogl, 2023; Yaschine et al., 2019; Araujo and Macours, 2021). Cash transfers have also been promoted as nutritional interventions under the assumption that the poor respond to the additional income from the transfer program by increasing all expenditures, including those on protein and fats (Ruel and Alderman, 2013). As a result, many of these programs have two features designed to enhance their nutritional impacts, or to make them "nutrition-sensitive" (Macours, Schady and Vakis, 2012; Fiszbein and Schady, 2009): (i) They distribute the transfers to women in eligible households under the assumption that women spend a greater share of such benefits on food and on investments in their children, and (ii) they provide information on optimal child nutrition. The programs typically make direct transfers of 10 to 25 percent of household income every month or every quarter over a period of several years, which make them akin to increases in permanent income. So, if anywhere, we should expect to see food demand respond to such transfers in the contexts we study.

We establish the impacts of these modest and routine income increases on food demand among the poor in several steps. First, we estimate the income elasticity of demand for various types of food among poor households in these low-income settings. Despite the nutrition-sensitive features of the CCTs we study, our estimated income elasticities are all quite small, but they are also all positive and significantly different from zero.

We estimate an income elasticity for overall food expenditures of 0.03. Coarse staples exhibit the most inelastic demand with an estimated income elasticity that is not statistically significantly different from zero. Animal-sourced protein shows the most elastic demand at 0.10. These results show that food is both a normal good and a necessity (i.e., as income increases, food expenditures also increase, but at a lesser rate) across almost all the categories we consider, viz. staples overall (with or without tubers), fine staples, protein, as well as fruits and vegetables.[6]

Second, we provide the first credible test of Bennett's Law—the empirical regularity whereby poor households seem to respond to increases in income by (i) spending more on fine staples than they do relative to coarse staples, or (ii) spending more on protein relative to staples (Bennett, 1941)—across these five settings to ask whether it is indeed a law or merely the result of correlations. Our estimates provide partial support for Bennett's Law. We find that with an exogenous increase in income, the average household substitutes fine staples for coarse staples, and it substitutes protein for coarse staples, both of which are consistent with Bennett's Law (Bennett, 1941).

Third, to address concerns arising from the likely sampling variation given the small number of groups (i.e., five RCTs) in our study and to distinguish that variation from true heterogeneity in the effect of income on food demand, we complement our pooled ordinary least squares (OLS) estimates with estimates from a Bayesian hierarchical model (BHM) that partially pools information across groups (Meager, 2019). These BHM estimates separate true heterogeneity in estimated elasticities from the sampling variation across the studies and simultaneously use the true heterogeneity to inform the uncertainty on the true elasticities. The pooled OLS and BHM estimates of income elasticities are nearly identical, suggesting that sampling variation does not meaningfully affect our pooled OLS estimates.

Finally, we use food group-specific elasticity estimates to calculate the nutritional impacts of the implied shifts in food expenditures. We show that changes in consumption behavior by beneficiary households increases calorie availability by 11 percent, reflecting a 13-percent increase in protein availability, a 9-percent increase in carbohydrate availability, and a 12-percent increase in fat availability. These estimates, while approximate, are important to consider because the endogeneity of changes in the composition of food bas-

[6]While in some circumstances certain foods may be inferior goods because quantity demanded decreases with income (Jensen and Miller, 2008; Ito, Peterson and Grant, 1989), we do not see evidence of this in our data, probably because we aggregate individual foods in broad categories (e.g., staples, protein, fruits and vegetables).

kets lowers the income elasticity of calories or nutrients relative to the income elasticity of food expenditure, sometimes by a significant amount (Behrman and Deolalikar, 1987). Thus, to the extent that malnutrition remains a policy concern in LMICs, such analyses should assess impacts both on nutrient intake and food expenditures.

Our work contributes to the literature on the estimation of the income elasticity of food demand. Previous estimates in this literature face important limitations to internal or external validity, often relying on cross-sectional variation, leveraging large transitory shocks that likely elicit different behavioral responses than routine income changes, focusing on a handful of commodities, or studying a single narrow context—often a high-income one (Deaton, 1974; Jappelli and Pistaferri, 2010; Krueger and Perri, 2006; Krueger, Malkov and Perri, 2023).[7] By leveraging five RCTs in four LMICs across three continents, we improve upon both the internal and external validity of extant estimates of the income elasticity of food demand.[8]

The shocks we use to identify our elasticity estimates are small and routine. This matters because, in theory, transitory income has different impacts on short-run consumption than does permanent income (Friedman, 1957; Jappelli and Pistaferri, 2010). Empirical evidence suggests that households in LMICs typically exhibit consumption smoothing patterns consistent with permanent income shocks in response to modest and routine cash transfer programs (Bazzi, Sumarto and Suryahadi, 2015).[9] Since income elasticities are

[7]Cross-sectional variation alone cannot identify the effect of income changes on food demand because food intake can both influence and be influenced by income, and these variables may also be jointly affected by yet other factors such as health or labor productivity (Strauss and Thomas, 1998). In addition, cross-sectional estimates cannot account for the effects of differences in prices, quality, and tastes, as well as the ability of local markets to respond to demand shocks (Rao, 2000; Angelucci and Attanasio, 2013; Attanasio and Pastorino, 2020; Filmer et al., 2023). A recent comparison of observational and experimental elasticity estimates within the same sample suggests that observational data lead to overestimates of food expenditure elasticities by about about 11 percentage points (Almås, Haushofer and Shapiro, 2019). In contrast, we estimate elasticities using exogenous variation in income, which allows generating elasticity estimates that are not confounded in the same way as those using cross-sectional data.

[8]Another shortcoming can arise on the construct validity front, because different estimates measure food demand in different ways. Behrman and Deolalikar (1987), for instance, study the relationship between income and food demand as measured in terms of nutrients, whereas Bouis and Haddad (1992) estimate the same relationship for food demand measured in terms of calories. In our analysis, we measure food demand in terms of food expenditures across all five contexts we consider.

[9]Behavioral responses suggest that in high-income countries, cash transfers are more akin to transitory

locally estimated (Ravallion, 1990) and consumption behavior depends on expectations regarding the permanence of the shock (Pennings, 2021), identifying the income elasticity of food demand among the poor calls for modest and sustained income shocks. The only other study that addresses internal validity concerns as we do (i.e., by using an exogenous income shock) studies a transitory and very large income shock (Almås, Haushofer and Shapiro, 2019).[10] In stark contrast, a survey across 69 LMICs shows that the average size of *all* safety net transfers in a country is 23 percent of recipient households' income (Gentilini, Honorati and Yemtsov, 2014). The income shocks we study are 12 to 23 percent of baseline income, making them a much closer approximation of the typical LMIC safety net or indeed of exogenous income changes experienced by the poor.

Our findings inform the policy discourse on food security and nutrition in LMICs, which continues to argue for the central role of income growth in attaining the second Sustainable Development Goal of zero hunger (Manley, Alderman and Gentilini, 2022; World Bank, 2020). While our estimates are consistent with economic intuition that food is both a normal good and a necessity and we find empirical support for Bennett's Law, our estimated income elasticities are quite small. The small size is especially striking because we estimate them from among some of the poorest people in four LMICs and all the programs we study have a pair of features—targeting women and providing information on optimal child feeding practices—that are aimed at increasing their nutritional impacts. These small elasticity estimates suggest that such nutrition targeting notwithstanding, cash transfers are likely to have only a limited impact on food consumption and, in turn,

than permanent income (Friedman, 1957; Hall, 1978). The only permanent cash transfer we know of is that provided since 1982 by the Alaska Permanent Fund studied by Jones and Marinescu (2022). Unfortunately, the universal nature of the Alaskan cash transfer—and the fact that it was introduced over 40 years ago in a high-income setting—makes it impossible to use for our purposes.

[10] In addition to the size of the income transfers, the frequency of the shocks represented by the programs assessed here is different from the one studied by Almås, Haushofer and Shapiro (2019). We study the impacts of monthly or quarterly transfers whereas they study a one-time transfer, one which was known to be such by beneficiaries. Further, Almås, Haushofer and Shapiro (2019) study an unconditional grant while all five programs studied here are conditional transfers. As Almås, Haushofer and Shapiro (2019) note, policy makers are often hesitant to make unconditional transfers while conditional cash transfers reach 800 million people worldwide (Gentilini, 2023).

on nutritional well-being. Our results also suggest that protein demand is the most sensitive to increases in income, but perhaps only a third as responsive to income increases as what was previously estimated. This finding is also relevant for nutrition policy design given the long-established nutritional importance of protein-rich foods, especially for children under the age of five (Scrimshaw and Béhar, 1961).

Our estimates of income elasticity for protein demand among the poor also inform the dialogue on climate change. Income growth has been predicted to lead to diets containing more animal-sourced proteins (Nelson et al., 2018). Some studies have thus raised concerns about the climate impacts of additional demand for animal-sourced proteins due to economic growth in LMICs on greenhouse gas emissions and land use (Kim et al., 2019; Oita et al., 2020). Questions, however, have also been raised about the relationship between income growth and climate change (Speedy, 2003). The extent to which income growth in LMICs significantly affects climate change depends on the extent of the increase in demand for animal-sourced proteins. Our estimated income elasticities suggest that while income growth will indeed lead to increased demand for animal-sourced proteins, these increases may be more muted than previously understood. Our estimated income elasticities also suggest that the demand for animal-sourced proteins in LMICs is unlikely to be a leading contributor to climate change, at least in the short run.

The remainder of this paper is organized as follows. In Section 2, we discuss the data we use for our analysis. Section 3 presents our empirical framework. In Section 4, we present and discuss our estimation results. Section 5 concludes.

2 Data and Summary Statistics

We use publicly available data from the impact evaluations of five RCTs of conditional cash transfer programs. Three of those programs are conditional cash-only transfer programs: the Mexican *Progresa* program (Hoddinott and Skoufias, 2004; Attanasio and Pastorino, 2020), the Nicaraguan *Red de Protección Social* program (Adato and Roopnaraine, 2004; Barham, Macours and Maluccio, 2024), and the Philippine *Pantawid* program (Filmer et al., 2023). Two of these programs deliver both conditional cash and in-kind support: the Ugandan World Food Program Cash and Food transfer program (Gilligan and Roy, 2013) and the *Programa Apoyo Alimentario* (PAL; Cunha, De Giorgi and Jayachandran, 2019), which supplements *Progresa* for remote and poor areas of Mexico, but with only minimal overlap with *Progresa*. For comparability, we only use data on the cash transfer arms of the latter two programs. Each of these datasets provides the necessary information on food groups (i.e., coarse grains, fine grains, tubers, proteins, vegetables, fruits, processed foods, and other food items).

In this section, we first describe the variables we construct and the harmonization process undertaken for our analysis. We next briefly discuss each data set, the identification strategy leveraged in the underlying evaluations, and the harmonization process undertaken to construct the indicators used in our analysis. Finally, we briefly present summary statistics.

2.1 Variable Construction and Harmonization for Analysis

To assess the impacts of income shocks on poor households' food consumption choices and estimate the underlying elasticities for various food groups, the analysis requires a measure of the income shock and budget shares of the relevant food groups. We define the following eligible food groups: coarse staples (or grains), fine staples (or grains), animal-sourced proteins, fruits and vegetables. This allows for estimating both the separate income elasticities for these groups and the impacts on calorie and nutrient availability. These sub-estimations are particularly important for our tests of Bennett's Law, which predicts that households respond to income growth by upgrading their diets. This upgrading occurs by their first substituting fine staples for coarse staples, and then by substituting protein for staples.

The surveys all report quantities purchased of each food item in kilograms and weekly expenditures. For consistency, we only use expenditures on purchased items and not estimated values of home production. Expenditures are calculated in nominal terms and then deflated using country-specific consumer price indices.

As the contexts from which we draw data vary greatly, so do the specific food items in each category for each country. There is even some within-country variation in how food groups are defined in the surveys. For instance, in the Mexican *Progresa* data, the only coarse grain is maize, whereas in the Mexican PAL data, coarse grains include both maize and oats. In Nicaragua, coarse grains include oats, maize, and ground maize. Table A1 details the individual food items in each of the food groups in the data sets we use and how we harmonize the items in each group for our analysis.

2.2 Mexico's *Progresa*

The Mexican conditional cash transfer program *Progresa* was launched in 1997 and reached approximately 2.6 million families by the end of 1999, representing the equivalent of about 40 percent of all rural households. By 2000, it had been scaled up nationally and reached 26.6 million individuals in 6.6 million households (Yaschine et al., 2019). It remained in place until 2019 and made monthly transfers to poor households conditional on school attendance and visiting health centers for curative and preventive care-seeking for children younger than five, as well as antenatal care use by pregnant women. The transfer amounted to USD 31 (2011 PPP) each month and represented about 20 percent of baseline consumption in beneficiary households (Skoufias, Davis and Behrman, 1999).

Household eligibility was defined using a proxy means test (PMT) score. Households with a PMT score below a certain threshold were deemed eligible to receive the transfer if they lived in a treated locality.[11] This program has an established evaluative sample dat-

[11] Initially, the definition of poor included 52 percent of households; this was revised to include 78 percent of households before treatment started (Gertler, 2004). We use the broader definition of eligibility in our analysis.

ing back to the initial rollout of the program in 1998 (Skoufias, Davis and Behrman, 1999; Gertler, 2004). Treatment assignment was randomized at the community level across 506 localities in seven states, yielding 320 treated and 186 control units. Localities were randomly assigned into a control arm (186 communities) or a treatment arm (320 communities). In treated localities, eligible households started receiving the transfer in August or September of 1998. In control localities, none of the households received the transfer for approximately 18 months from the start of the program. Initial program uptake was over 90 percent, meaning that the treatment-on-treated and intent-to-treat estimates were very similar (Skoufias, Davis and Behrman, 1999; Araujo and Macours, 2021).

For this analysis, we use the replication data made available by Attanasio and Pastorino (2020), after approximately two to three years of transfers.

2.3 Nicaragua's *Red de Protección* Program

The Nicaraguan *Red de Protección Social* (RPS) was the main national safety net program implemented from November 2000 to December 2005. It was modeled largely after *Progresa* but beneficiary households were only eligible to receive the program for a fixed three-year period. Much like with *Progresa*, it was targeted to households living in poverty in rural Nicaragua and had an evaluation embedded from the design stage onward.

A randomized evaluation was incorporated into the design of the CCT in six rural municipalities from three regions that were intentionally selected for their poverty as well as substandard health and educational outcomes. The poorest 42 of 59 rural localities in these municipalities were selected for treatment. Of the 42, 21 were randomly assigned to early treatment and 21 to late treatment. Households in the early treatment arm received their first transfers in November 2000 and received monthly transfers—if they met program conditions—for up to three years. After randomization, households in the 21 late-treatment localities were informed that the program would start later in their localities. They were enrolled in the program in January 2003 and were also able to receive up to three years of transfers.

Eligible households are poor households identified using a PMT. The program provided USD 36 (2011 PPP) in monthly transfers, representing about 20 percent of the average beneficiary household's consumption expenditures. Program conditions were similar to those used for *Progresa*, with grants tied to children's school attendance, and curative and preventive health care usage by young children.

We use data collected for the experimental evaluation of the program during its pilot stage (Barham, Macours and Maluccio, 2024).

2.4 The Philippines' *Pantawid* Program

The *Pantawid* program is the flagship social safety net intervention in the Philippines. At its peak in December 2016, the program covered around 20 million people in 4.5 million households around the country (World Bank, 2017). While its rolls have not been updated in several years, the program continues to make transfers to poor Filipino households. The program was piloted starting in January 2009. This stage was implemented in 130 randomly-selected villages representing each of the four macro-regions of the country. Of these 130 villages, 65 were randomly assigned to treatment and 65 to control. Households were eligible if identified as poor by a PMT and if they had school-aged or younger children.

In its pilot stage, the program provided USD 78 in monthly transfers (2011 PPP), representing 23 percent of beneficiary household consumption. The program conditions monitored school attendance and enrollment, as well as pregnancy-related care seeking. Limited baseline data were collected before randomization in 2008, and an endline survey was conducted in late 2011, after almost three years of transfers.

This program was evaluated in its pilot stage and we use these data in our analysis (Filmer et al., 2023).

2.5 Mexico's *Programa de Apoyo Alimentario* (PAL)

The PAL provides unconditional in-kind or cash transfers to poor households in the most impoverished areas of Mexico, as a supplement to *Progresa* (Cunha, De Giorgi and Jayachandran, 2019). Villages are eligible for PAL if they have fewer than 2,500 inhabitants, are highly marginalized (as classified by the Mexican Census Bureau), and do not receive aid from *Liconsa*, a Mexican milk subsidy program, or *Progresa*. The PAL villages tend to be poorer and more rural than *Progresa* villages (Cunha, De Giorgi and Jayachandran, 2019). The transfer size is 19 USD a month (in 2011 PPP terms), representing about 11.5 percent beneficiary household consumption.

In 2003, for the evaluation of PAL, 208 localities were randomized into 156 treated units, 104 of which received the in-kind transfer and 52 received the cash transfer. There were 38 control localities while 14 localities were excluded due to implementation difficulties. Eligible households were surveyed in cash-transfer and control communities for a baseline in 2003 and an endline in 2005, representing about two years of transfers.

We use data from the publicly available replication package of Cunha, De Giorgi and Jayachandran (2019).

2.6 The World Food Program's Cash and Food Transfer in Uganda

The World Food Program, in collaboration with other development agencies, implemented a one year long cash and food transfer in Uganda between 2010 and 2011 (Gilligan and Roy, 2013).

For the program, 99 localities were randomized into 66 treated units, each with equal probability assignment to the food or cash arms, and 33 control units. Households that were enrolled in public early childhood development (ECD) centers in these localities were deemed eligible. Within these localities, eligible households were surveyed in cash and control localities. The program provided 30 USD (in 2011 PPP terms) every six weeks if the targeted child attended the ECD at least 80 percent of the time over the previous six week period. The transfer amounted to approximately 13 percent of beneficiary household consumption. Table I summarizes the foregoing discussion of the five RCTs we use in the empirical analysis below.

We use data from the publicly available replication package of Gilligan and Roy (2013).

2.7 Descriptive Statistics

Table II presents mean consumption expenditures (in 2011 PPP terms) in each of those RCTs' control group for food overall, but also for staples, tubers, protein, fruits and vegetables, and other foods. It is noteworthy that in three out of five contexts (i.e., Nicaragua RPS, Philippines *Pantawid*, and Uganda WFP) mean expenditures on staples are larger than mean expenditures on protein, and that the two cases where mean expenditures on protein are larger than mean expenditures on staples are in Mexico. This is consistent with the fact that Mexico is an upper middle-income country, whereas Nicaragua, the Philippines, and Uganda are all lower middle-income countries.

3 Empirical Framework

This section discusses our approach to quantifying the effect of income on food expenditures. Our core approach consists of estimating the effect of (i) being randomized into receiving the CCT (i.e., a dummy variable equal to one if a household is assigned to the treatment group equal to zero otherwise), and (ii) the amount of cash received (i.e., a positive amount for those randomized into receiving the CCT, and zero otherwise) on food expenditures, whether this means food overall or various food categories. In order to improve on the external validity of our results, we then estimate a Bayesian hierarchical version of our core approach.

3.1 Core Approach

The core empirical approach we follow is straightforward. We begin by estimating the equation

$$\ln y_{1ijk} = \alpha_1 + \beta_1 D_{ik} + \delta_{1k} + \epsilon_{1ijk}, \tag{1}$$

where y denotes the expenditures of household i on food category j in the context of RCT $k \in \{1, ..., 5\}$, D is a dummy variable for whether the household is in the treatment group (i.e., whether the household has been randomly assigned to receiving a cash transfer), δ is an RCT fixed effect, and ϵ is an error term with mean zero. We apply the inverse hyperbolic sine (i.e., arcsinh) transformation to the dependent variable to approximate logarithmic values to estimate effects at the extensive and intensive margin by retaining zero-valued observations.[12] We account for this transformation in elasticity calculations following Bellemare and Wichman (2020).

Next, we estimate

$$\ln y_{2ijk} = \alpha_2 + \beta_2 \ln T_{ik} + \delta_{2k} + \epsilon_{1ijk}, \tag{2}$$

where all variables are defined as in Equation 1, but where T_{ik} denotes the amount of the transfer received by household i in the context of RCT k. We apply the inverse hyperbolic sine (or arcsinh) transformation to both the dependent variable and treatment variable T_{ik} to approximate logarithmic values to estimate effects at the extensive and intensive margin by retaining zero-valued observations.

In this context, $\hat{\beta}_1$ and $\hat{\beta}_2$ are intent-to-treat (ITT) estimates respectively capturing the effect of (i) being randomly assigned to the treatment group, or (ii) receiving a cash transfer on expenditures on food category j for the average household in our data. By looking at expenditures on specific food categories, Equations 1 and 2 allow testing whether those

[12] Aihounton and Henningsen (2021), Mullahy and Norton (2022), and Chen and Roth (2024) all note that the inverse hyperbolic sine transformation can be problematic in that a variable subject to it can be rescaled arbitrarily to get a specific coefficient estimate. To avoid both this problem and that of nonrandomly dropping zero-valued observations, we do not rescale any of the variables subject to an inverse hyperbolic sine transformation in our application.

categories of foods are normal goods ($\hat{\beta}_1 > 0$ and $\hat{\beta}_2 > 0$), inferior goods ($\hat{\beta}_1 < 0$ and $\hat{\beta}_2 < 0$), or neither ($\hat{\beta}_1 = 0$ and $\hat{\beta}_2 = 0$). Because the randomization unit across all five RCTs we consider is the village, clustering is a design rather than sampling issue, so we cluster standard errors at the village level following the recommendations in Abadie et al. (2023).

We next estimate the equations

$$\ln\left(\frac{y_{3ijk}}{y_{3i\ell k}}\right) = \alpha_3 + \beta_3 D_{ik} + \delta_{3k} + \epsilon_{3ij\ell k}, \tag{3}$$

and

$$\ln\left(\frac{y_{4ijk}}{y_{4i\ell k}}\right) = \alpha_4 + \beta_4 T_{ik} + \delta_{4k} + \epsilon_{4ij\ell k}, \tag{4}$$

where all right-hand side variables are defined as before, but where the dependent variables are now the ratio of expenditures on food categories j and ℓ. By looking at expenditure ratios, Equations 3 and 4 allow testing whether cash transfers cause expenditures to increase faster ($\hat{\beta}_3 > 0$ and $\hat{\beta}_4 > 0$), slower ($\hat{\beta}_3 < 0$ and $\hat{\beta}_4 < 0$), or at the same rate ($\hat{\beta}_3 = 0$ and $\hat{\beta}_4 = 0$) in food category j relative to food category ℓ. We apply the inverse hyperbolic sine transformation to the dependent variable and cluster standard errors at the village level here as well.

What hypothesis tests are required to test Bennett's Law (Bennett, 1941)? Recall that Bennett's Law makes two explicit, testable predictions:

1. As the income of poor households increases, they will spend relatively more on fine staples relative to coarse staples, and

2. As the income of those same households increases further, they will spend relatively more on protein relative to staples.

Implicitly, Bennett's Law also posits that as the income of poor households increases, they will not spend less on fine staples or protein. In other words, Bennett's Law implies that neither fine staples nor protein are inferior goods, although it leaves open the possibility that coarse staples may be an inferior good.[13]

3.2 Bayesian Aggregation

Our core empirical approach pools the data from the five RCTs we retain for analysis into one set of results. Another approach consists in estimating Equations 1, 2, 3, and 4 piecemeal for each RCT. While this latter approach would help us learn about individual (i.e., RCT-specific) contexts, it would not help with our goal of making an externally valid statement about the relationship between conditional cash transfers and the demand for food, which is provided by the former approach.

Yet another approach, one perhaps more fruitful than either estimating Equations 1 to 4 piecemeal or by pooling the data together, would be to estimate a Bayesian hierarchical model (BHM). As in Meager (2019), we use the BHM to estimate a pooled estimate from several experiments in the presence of external validity concerns by jointly estimating both the average effect and the heterogeneity in effects across experiments.

When pooling estimates from different studies, we want to separate true heterogeneity in our estimated effects from sampling variation. A piecemeal approach would give us a vector $\hat{\tau}$ of treatment effect estimates, such that $\hat{\tau} = \{\hat{\tau}_1, ..., \hat{\tau}_5\}$, where the subscripts denote specific contexts, with an associated vector of standard errors $\hat{\sigma} = \{\hat{\sigma}_1, ..., \hat{\sigma}_5\}$.

In contrast, and as laid out in Meager (2019), a Bayesian aggregation allows for the partial pooling of the distinct treatment effect estimates and their associated standard errors, incorporating additional information about the sampling variation in each group and improving on our pooled estimates. It is this incorporation of extra information that

[13]One might be tempted to estimate each of Equations 1 to 4 as part of systems of seemingly unrelated regressions for all food categories j (i.e., all categories j as part of a system for Equation 1, and the same for Equations 2, 3, and 4, see Zellner (1962); Zellner and Huang (1962)). But the fact that all of the j regressions in each such system would regress its dependent variable on the same set of covariates as the other $-j$ regressions in the same system makes the seemingly unrelated regression estimator equivalent to estimating the separate regressions results we present here, as discussed in Greene (2003), which would make seemingly unrelated regressions redundant.

makes this approach Bayesian. This is done at two levels, which makes the model hierarchical: the first level accounts for treatment effect heterogeneity, and the second level accounts for sampling variation. This approach allows a parameter $\hat{\theta}$ (where θ is either τ or σ) to incorporate information from each $\hat{\theta}_k$ and vice versa, which means that each $\hat{\theta}_k$ also then (indirectly) incorporates information from each $\hat{\theta}_{-k}$.

We follow Meager (2019) in estimating a summary (μ,τ) pooling version of the BHM just described. We use RCT-specific (i.e., piecemeal) treatment effect estimates and their associated standard errors as priors and generate estimates using a Markov chain Monte Carlo algorithm with eight chains and 10,000 iterations.

4 Results and Discussion

We now turn to our empirical results, which we divide into two sets of results. The first set consists of results for our core approach, or Equations 1 to 4. The second set consists of results for the Bayesian hierarchical variant of our core results.

4.1 Core Results

Before discussing our core parametric results, we briefly discuss some nonparametric results. In Figure I, the left panel plots the value food expenditures (in 2011 PPP dollars) against total expenditures and the right panel plots the Engel curve for food, i.e., the budget share of food as a percentage of total expenditures, also against total expenditures. Each panel presents the data for treated households with a solid line and control households with a dashed line. As total expenditures increase, food expenditures also generally increase monotonically. The Engel curve then shows that the budget share of food is generally stable along the same conditioning domain, at about 45 percent of total expenditures. While the patterns are largely similar for treated and control households, treated households have a somewhat greater food share at higher levels of total household expenditure, which may be tied to the greater salience of food and nutrition resulting from the nutritional messaging accompanying the cash in each of the transfer programs.

Figure II then shows how expenditures on protein as a share of food expenditures change as total food expenditures increase in both the treatment (i.e., blue line) and control (i.e., green line) groups, along with the relevant confidence intervals. Both lines have a similar appearance, with the average household in the treatment group generally devoting a higher share of expenditures to protein than the average household in the control group.

Panel A of Table III presents estimation results for Equations 1 looking respectively at all staples (column 1), coarse staples (column 2), fine staples (column 3), protein (column 4), fruits and vegetables (column 5), and food overall (column 6). Each column regresses the inverse hyperbolic sine of food expenditures on a given food category on a dummy for whether a household is treated (i.e., randomized into receiving the conditional cash transfer) and on a vector of RCT fixed effects.

In all specifications in Table III, the ITT effect of being assigned to receiving the conditional cash transfer is positive and statistically significant at conventional levels. In other words, an exogenous increase in income increases the demand for overall staples, coarse staples, fine staples, protein, and fruits and vegetables. Given that, it is not surprising that an exogenous increase in income also increases the demand for food overall. In terms of magnitude, our estimates indicate that, on average across the five RCTs we study, being assigned to receiving a cash transfer increases expenditures on staples by 27 percent, expenditures on coarse staples by 16 percent, expenditures on fine staples by 31 percent, expenditures on protein by 52 percent, expenditures on fruits and vegetables by 33 percent, and expenditures on food overall by 16 percent.

Panel B of Table III presents estimation results for Equation 2 looking respectively at all staples (column 1), coarse staples (column 2), fine staples (column 3), protein (column 4), fruits and vegetables (column 5), and food overall (column 6). Each column regresses the inverse hyperbolic sine of food expenditures on a given food category on a variable measuring the size of the cash transfer received (i.e., a positive amount for those assigned to treatment, and zero otherwise) and on a vector of RCT fixed effects.

In all but one of the specifications in Table III, the ITT effect of receiving the conditional cash transfer is positive and statistically significant at the conventional levels. Each estimated coefficient in the transfer size line is also an elasticity, measuring the percentage change in food expenditures caused by a one percent increase in income. Given that, staples, fine staples, protein, fruits and vegetables, and food overall are all normal goods since their estimated income elasticities are all statistically significantly different from zero and between zero and one. Furthermore, while the income elasticity of expenditures on food overall is relatively small at 0.03, the same elasticity is higher for staples generally, fine staples, protein, and fruits and vegetables. Interestingly, with an income elasticity of 0.10, expenditures on protein respond most to a change in income. Expenditures on coarse staples respond least to a change in income, with an estimated elasticity of 0.03 that is not statistically significant at any of the conventional levels.

To characterize what happens away from the mean of our data, we also present results for two quantile regressions of the ITT effect of the cash transfer on food expenditures, at the second and third quartiles of the distribution of overall household expenditure. Panel A of Table IV shows results with a dummy variable for whether a household is assigned to treatment as the treatment variable, while Panel B shows results with transfer size as the treatment variable. Results in panel A for the second quartile show that households respond to being treated by increasing expenditures on staples, protein, fruits and vegetables respectively by about 25, 48, and 29 percent, and by increasing expenditures on food by about 14 percent. Similarly, results for the third quartile show that households respond to being treated by increasing expenditures on staples, fine staples, protein, fruits and vegetables respectively by about 18, 21, 22, and 21 percent, and by increasing expenditures on food by about 11 percent. Looking at results in Panel B of Table IV, results of the second quartile show that a 1-percent increase in transfer size translates into a 0.06 percent increase in expenditures on staples, a 0.11 percent increase in expenditures on protein, a 0.06 percent increase in expenditures on fruits and vegetables, and a 0.03 percent increase in expenditures on food overall. Results for the third quartile tend to be somewhat smaller in magnitude, except for fine staples: Whereas a 1-percent increase in transfer size did not translate into a significant increase in expenditures on fine staples at the second quartile, it translates into a 0.04 percent increase in expenditures on fine staples at the third quartile.

Panel A of Table V presents estimation results for Equation 3 looking respectively at the fine-to-coarse-staples expenditure ratio (column 1), the protein-to-staples expenditure ratio (column 2), the protein-to-coarse-staples expenditure ratio (column 3), the protein-to-fine-staples expenditure ratio (column 4), and the protein-to-fruits-and-vegetables food expenditure ratio (column 5). Here, the only significant results are for the fine-to-coarse-staples and the protein-to-coarse-staples food expenditure ratios, which are both positive and statistically significant at conventional levels. What this means in practice is that as a result of being randomly assigned to receiving a cash transfer, expenditures on fine staples rise faster than expenditures on coarse staples, and expenditures on protein rise faster than expenditures on coarse staples.

Finally, Panel B of Table V presents estimation results for Equation 4, again looking respectively at the fine-to-coarse-staples expenditure ratio (column 1), the protein-to-staples expenditure ratio (column 2), the protein-to-coarse-staples expenditure ratio (column 3), the protein-to-fine-staples expenditure ratio (column 4), and the protein-to-fruits-and-vegetables food expenditure ratio (column 5). Once again, the only significant results are for the fine-to-coarse-staples and the protein-to-coarse-staples food expenditure ratios, which are both positive and statistically significant at conventional levels. In practice, this means that as a result of receiving additional income, expenditures on fine staples rise faster than expenditures on coarse staples, and expenditures on protein rise faster than expenditures on coarse staples.

Turning to Bennett's Law, recall that the two testable predictions we laid out in Section 3 were:

1. As the income of poor households increases, they will spend relatively more on fine staples relative to coarse staples, and

2. As the income of those same households increases further, they will send relatively more on protein relative to *all* staples.

Unambiguous support for Bennett's Law would thus consist of finding that the fine-to-coarse-staples, the protein-to-staples, the protein-to-coarse staples, and the protein-to-fine-staples ratios are all positive and statistically significant. Here, we find only partial support for Bennett's Law in that (i) the first requirement is partially supported by the result in Table V that expenditures on fine staples rise faster than expenditures on coarse staples, and (ii) the second requirement of Bennett's Law is supported by the result in Table V that expenditures on protein rise faster than expenditures on coarse staples, but without support for the hypotheses that expenditures on protein rise faster than expenditures on staples overall or for the hypothesis that expenditures on protein rise faster than expenditures on fine staples.

4.2 Bayesian Aggregation

Turning to our Bayesian hierarchical modeling results, for all categories we analyze—overall staples, coarse staples, fine staples, protein, and fruits and vegetables—in Figures A2 to A5. In each figure, the left panel shows treatment effect estimates (and related confidence intervals) by RCT, while the right panel shows forest plots of the prior (i.e., input) and posterior (i.e., estimated) treatment effects for all staples for each RCT as well as across all contexts, with the latter labeled "hypermean" treatment effect.

Across all food categories, Bayesian hierarchical modeling results are qualitatively very similar to our frequentist results, as shown by the forest plots in odd-numbered figures. Comparing each hypermean treatment effect with its frequentist analog, we note that the BHM estimated ITT effect is 0.18 for all staples (versus 0.24 for the frequentist estimate), 0.11 for coarse staples (versus 0.15 for the frequentist estimate), 0.26 for fine staples (versus 0.27 for the frequentist estimate), 0.41 for protein (versus 0.42 for the frequentist estimate), and 0.25 for fruits and vegetables (versus 0.29 for the frequentist estimate).[14]

4.3 Nutrient Availability Impacts

We now turn to back-of-the-envelope calculations of the implied nutrient availability impacts of the estimated food expenditure responses to income changes. Our data do not allow measuring actual calories and nutrients consumed—only calorie and nutrient availability. If there is a significant loss of calories or nutrients due to waste or cooking methods, availability may differ from actual intake. We have no *ex ante* reason, however, to believe this to be the case.

For these calculations, we estimate weekly calorie (i.e., kcal) and nutrient availability at the household level. Considering only meals at home, the average non-recipient household consumed 29,433 kcal over the course of a week, or 4,200 kcal each day.[15] These households having an average of 3.5 members, this means that approximately 1,200 kcal were available daily per capita.

Table A2 presents these estimated nutrient availability impacts. Results show that cash transfer recipient households see an 11-percent increase in overall available calories, representing an additional 3,231 calories per week relative to the control mean of 29,433

[14]Our BHM results cannot account for clustering due to the inability of statistical package used to generate BHM results to handle clustering. We therefore do not comment on the BHM standard errors, preferring instead to put our trust in the standard errors generated by our core frequentist approach when assessing statistical significance.

[15]We do not have complete data on meals away from home, although these may represent an important source of calories and nutrients and may also increase in importance with the positive income shock from the cash transfers.

calories. The largest nutrient availability impact is for proteins, with a 13-percent increase (i.e., an additional 114 grams of protein availability each week, relative to a control mean of 856 grams) as well as a 9-percent increase in carbohydrate availability (i.e., an additional 470 grams of carbohydrate availability each week, relative to a control mean of 4,988 grams), and a 12-percent increase in fat availability (i.e., an additional 104 grams of fat availability each week, relative to a control mean of 869 grams).

5 Summary and Concluding Remarks

Aggregating data from five conditional cash transfer RCTs in four countries across three continents, we have looked at the effect of income on food demand, as proxied by food expenditures. Our results show that food is a normal good and a necessity across all categories, i.e. coarse and fine staples, protein, and fruits and vegetables. Of all food categories, protein responds most to an exogenous increase in income. In contrast to the income elasticities previously reported in the literature, our estimates are causal because they leverage randomized assignment to the CCT treatment, and they are based on behavioral responses to small, regular, and sustained income shocks as opposed to a large one-time shock as in Almås, Haushofer and Shapiro (2019).

We also conduct the first credible test of Bennett's Law (Bennett, 1941), the empirical regularity whereby the poor first substitute fine staples for coarse staples as their income increases, and then substitute protein for staples as their income increases further still. Our results show that with additional income, the poor on average substitute fine staples for coarse staples, and they substitute protein for coarse staples, but we do not find that they substitute protein for staples overall, nor do we find that they substitute protein for fine staples. These results constitute partial support for Bennett's Law.

Lastly, we estimate the effect of an exogenous increase in income on calorie availability, finding that recipient households see an 11-percent increase in overall available calories as well as a 9-percent increase in available carbohydrates, a 13-percent increase in available protein, and a 12-percent increase in available fat.

Our findings come with a few caveats. First, while we have harmonized the five data sets we retain for analysis, important differences may persist across contexts. In particular, there may be important quality differences that are not accounted for in the data. For instance, households may have different tastes for the different grades of rice and these grades may have significant price differences, such that a positive income shock may cause a household to consume a higher grade of rice. We generally do not observe such granularity in the data and assume that all rice is one grade, but households could be spending more on a higher quality version of a coarse staple in addition to the observed changes in allocation across food groups. Such quality upgrading would be consistent with Bennett's Law and would result in underestimating the impacts of the income shock.

Similarly, the conditional cash transfers we study may enable otherwise cash-constrained households to purchase food items in bulk, potentially leading to lower total expenditures on staples than if they were only able to purchase smaller quantities. This in turn may free up more income for proteins and finer grains, further increasing demand for such categories. Such compensatory behavior by households is part of the estimated income effect underlying Bennett's Law.

Finally, while conditional cash transfers provide exogenous variation in income, we assume that recipient households view this income as permanent income. If instead they view transfer income as transitory, the permanent income hypothesis would predict a smaller consumption response, which in turn would imply that our elasticity estimates might be a lower bound. That said, evidence from LMICs suggests that households treat transfer income as permanent income and exhibit at least some consumption smoothing behavior (Bazzi, Sumarto and Suryahadi, 2015; Gertler, Martinez and Rubio-Codina, 2012). Such behavior is also consistent with evidence on consumption behavior with transfer income in high-income countries (Jappelli and Pistaferri, 2010).

Our elasticity estimates are relevant for development policy. If a policy maker's goal is to improve nutrition, our results show that cash transfers may be a cost-effective way of doing so, particularly in comparison to nutrient supplementation or in-kind transfers. This is especially so if the goal is to either increase the number of calories consumed or increase the consumption of protein. Nonetheless, our estimates also show that the extent of the impact—especially in response to a modest, routine income shocks—may be lower than has been previously reported in the literature. To fully eradicate hunger and malnutrition, governments in LMICs will likely need to increase the size or frequency of cash transfers, or rely on complementary and direct nutritional interventions.

Finally, given that the land use requirements, greenhouse gas emissions, and overall carbon footprint of animal-sourced proteins are higher than for other food categories (Nijdam, Rood and Westhoek, 2012), an increasing policy concern is that as households in LMICs get wealthier, increasing amounts of land are likely to be dedicated to the production of such animal-sourced protein, and greenhouse gas and carbon emissions are likely to rise (Delgado, 2003). Similarly, there are concerns that consumers in high-income countries will have to reduce their consumption of animal-sourced protein in order to offset the effects of increased animal-sourced protein consumption in LMICs (Henchion et al., 2021). While the results also point to such shifts, our relatively small estimates of income elasticity for protein suggest that the climate change impacts arising from the changing food choices of poor households may be of limited magnitude.

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6 Figures and Tables

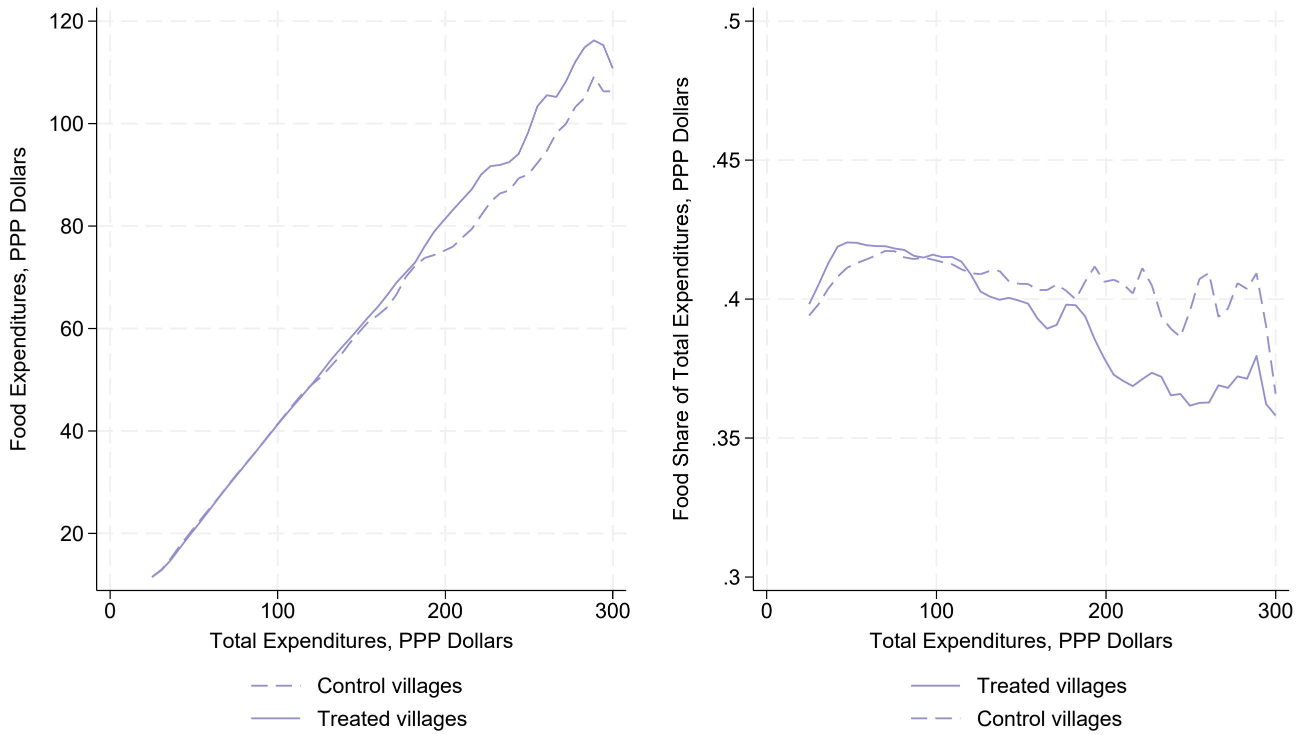


FIGURE I: Engel Curves for overall food demand pooling data from 5 conditional cash transfer programs in Mexico, Nicaragua, Philippines, and Uganda

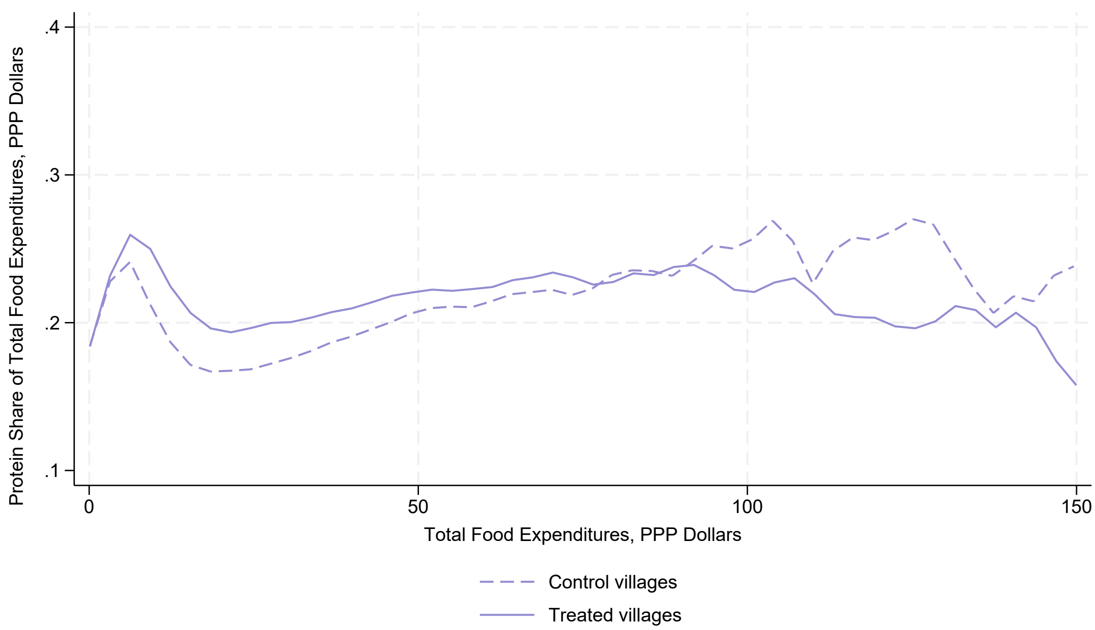


FIGURE II: Protein share and overall food expenditures pooling data from 5 conditional cash transfer programs in Mexico, Nicaragua, Philippines, and Uganda

TABLE I: Overview of the five randomized controlled trials of conditional cash transfer programs included in the study, conducted in Mexico, Nicaragua, the Philippines, and Uganda

Country	Program	Study period	Sample size	Transfer type	Transfer ratio
Mexico	Progresa	1998-1999	18,351	CCT	20%
Mexico	Programa de Apoyo Alimentario (PAL)	2003-2005	2,866	UCT	11.5%
Nicaragua	Red de Protección Social (RPS)	2000-2002	1,433	CCT	20%
Philippines	Pantawid Pamilyang Pilipino Program (PPPP)	2009-2011	1,401	CCT	23%
Uganda	WFP	2010-2011	1,777	UCT	13%

Note: Mexico's PAL and Uganda's WPF included an in-kind transfer arm, but only the cash-transfer arm data are used in this paper. The sample size consists of households at endline. Transfer size describes the transfer-to-consumption ratio.

TABLE II: Mean weekly food consumption expenditure in the control group by program

	Mean expenditures of control group					
	Food	Staples	Tubers	Protein	Vegetables + fruits	Other
Mexico PAL	52.30	5.52	0.72	15.55	5.66	21.56
Mexico Progresa	31.35	4.61	0.89	5.99	3.13	18.45
Nicaragua RSP	47.81	15.73	0.84	6.12	1.67	23.23
Philippines PPPP	48.63	21.61	0.67	14.39	2.18	9.79
Uganda WFP	17.41	5.60	0.66	3.53	0.86	6.56
Number of observations	22,232	22,232	21,986	22,231	22,232	19,513

Note: All expenditures are expressed in USD (2011 PPP terms).

TABLE III: Income elasticities of food demand estimated using a binary treatment indicator and pooled OLS on all five programs

Panel A: Elasticities estimated using an indicator variable for treatment

	Staples	Coarse staples	Fine staples	Protein	Vegetables + fruits	Overall food
Coefficient on treatment indicator:						
Treated	0.243***	0.147*	0.270***	0.419***	0.288***	0.151***
	(0.067)	(0.085)	(0.047)	(0.058)	(0.057)	(0.038)
Constant	2.757***	1.451***	1.969***	2.974***	2.586***	5.359***
	(0.059)	(0.071)	(0.038)	(0.045)	(0.043)	(0.032)
Elasticity calculated using different formulas:						
$\exp(\hat{\beta}) - 1$	0.275***	0.158	0.310***	0.521***	0.334***	0.164***
	(0.083)	(0.099)	(0.059)	(0.084)	(0.069)	(0.046)
$\exp(\hat{\beta} - 0.5\hat{Var}(\hat{\beta})) - 1$	0.273***	0.155*	0.309***	0.519***	0.332***	0.163***
	(0.075)	(0.088)	(0.063)	(0.085)	(0.076)	(0.040)
Number of observations	55,744	48,326	55,079	55,739	55,744	55,081
Number of clusters	843	715	715	843	843	715

Panel B: Elasticities estimated using transfer size

	Staples	Coarse staples	Fine staples	Protein	Vegetables + fruits	Overall food
Transfer size	0.049***	0.029	0.058***	0.101***	0.064***	0.034***
	(0.014)	(0.018)	(0.010)	(0.012)	(0.012)	(0.008)
Constant	2.764***	1.458***	1.969***	2.943***	2.579***	5.355***
	(0.050)	(0.065)	(0.034)	(0.045)	(0.040)	(0.028)
Number of observations	55,744	48,326	55,079	55,739	55,744	55,081
Number of clusters	843	715	715	843	843	715

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Each row corresponds to a separate regression. All specifications include RCT fixed effects. Standard errors are clustered at the village level (randomization unit) and bootstrapped with 100 reps. Elasticities are calculated using the formulas indicated in the table, with displaying $\frac{P}{100} = formula$.

TABLE IV: Impact of cash transfers on food consumption for the second and third quartiles of expenditure using pooled OLS on all five programs

	Staples	Coarse staples	Fine staples	Protein	Vegetables + fruits	Overall food
Panel A: Estimation using an indicator variable for treatment						
Second quartile:						
Treated	0.248***	0.000	0.098***	0.476***	0.290***	0.137***
	(0.056)	(0.000)	(0.025)	(0.070)	(0.045)	(0.030)
Number of observations	55,744	48,326	55,079	55,739	55,744	55,081
Third quartile:						
Treated	0.189***	0.140	0.208***	0.220***	0.209***	0.114***
	(0.064)	(0.135)	(0.042)	(0.033)	(0.042)	(0.023)
Number of observations	55,744	48,326	55,079	55,739	55,744	55,081
Panel B: Estimation using transfer size						
	Staples	Coarse staples	Fine staples	Protein	Vegetables + fruits	Overall food
Second quartile						
Transfer size	0.055***	0.000	0.006	0.111***	0.064***	0.031***
	(0.013)	(0.000)	(0.004)	(0.015)	(0.011)	(0.006)
Number of observations	55,744	48,326	55,079	55,739	55,744	55,081
Third quartile						
Transfer size	0.038***	0.029	0.043***	0.049***	0.046***	0.027***
	(0.014)	(0.029)	(0.011)	(0.007)	(0.009)	(0.005)
Number of observations	55,744	48,326	55,079	55,739	55,744	55,081

Note: Standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Each row and column correspond to a separate regression. All specifications include RCT fixed effects. Standard errors are clustered at the village level (randomization unit).

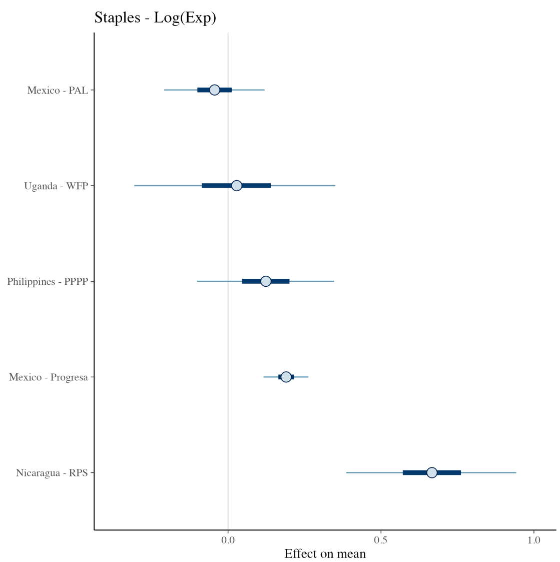
TABLE V: Impact of cash transfers on food expenditure ratios (in natural log terms) using pooled OLS on all five programs

Panel A: Estimation using an indicator variable for treatment					
	Fine/ Coarse	Protein/ Staples	Protein/ Coarse	Protein/ Fine	Protein/ Veg+fruits
Treated	0.089*	0.009	0.124**	0.002	0.000
	(0.049)	(0.040)	(0.050)	(0.025)	(0.025)
Constant	-1.111***	0.406***	-0.259***	0.985***	0.737***
	(0.040)	(0.031)	(0.042)	(0.018)	(0.019)
Number of observations	12,766	38,344	14,143	34,735	42,144
Number of clusters	650	839	694	677	831
Panel B: Estimation using transfer size					
	Fine/ Coarse	Protein/ Staples	Protein/ Coarse	Protein/ Fine	Protein/ Veg+fruits
Transfer size	0.020*	0.002	0.024**	0.001	-0.000
	(0.011)	(0.008)	(0.010)	(0.005)	(0.005)
Constant	-1.114***	0.407***	-0.254***	0.985***	0.738***
	(0.040)	(0.031)	(0.041)	(0.018)	(0.019)
Number of observations	12,766	38,344	14,143	34,735	42,144
Number of clusters	650	839	694	677	831

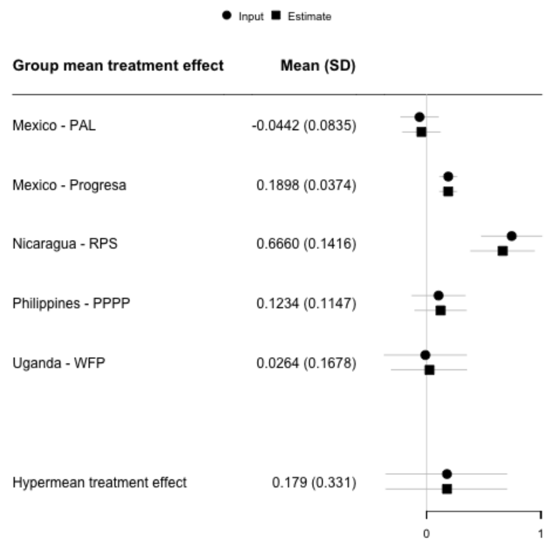
Note: Standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Each row corresponds to a separate regression. All specifications include RCT fixed effects. Standard errors are clustered at the village level (randomization unit).

Appendix Figures and Tables

FIGURE A1: Treatment effects on staples demand aggregated using Bayesian Hierarchical Modeling

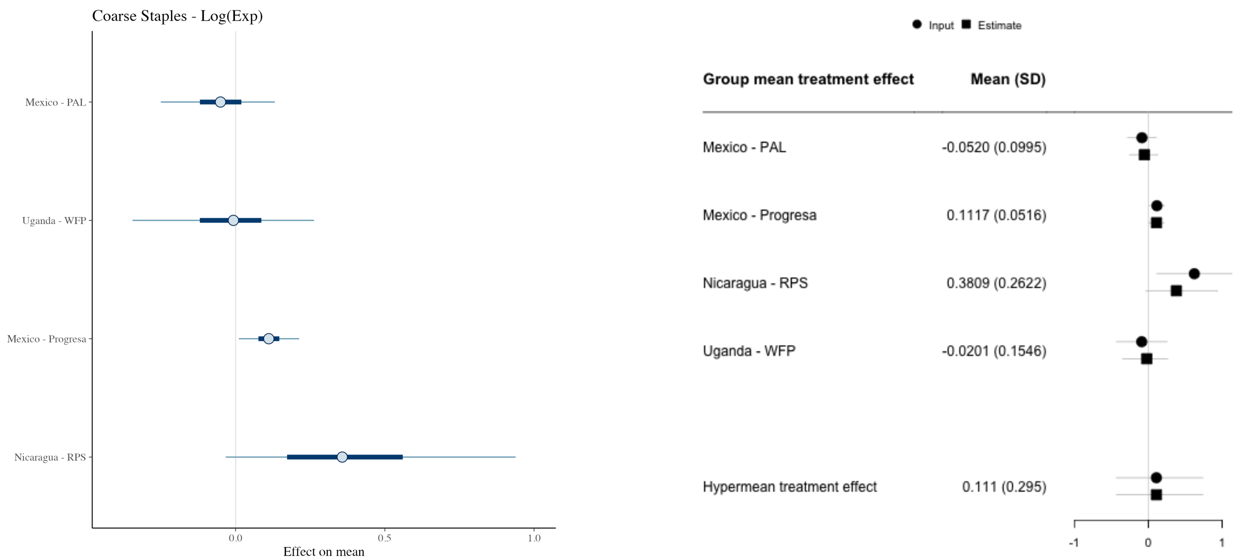


(a) RCT-wise treatment effects



(b) Hypermean treatment effect

FIGURE A2: Treatment effects on coarse staples demand aggregated using Bayesian Hierarchical Modeling

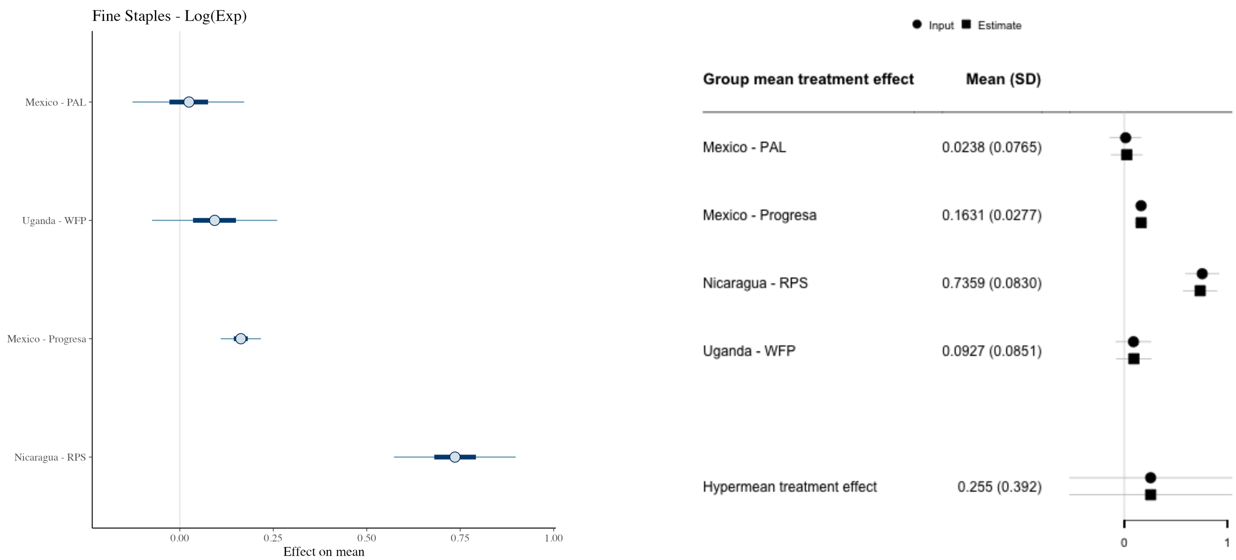


(a) RCT-wise treatment effects

(b) Hypermean treatment effect

Note: Results for the Philippines are omitted because we cannot differentiate between coarse and fine staples in that context.

FIGURE A3: Treatment effects on fine staples demand aggregated using Bayesian Hierarchical Modeling

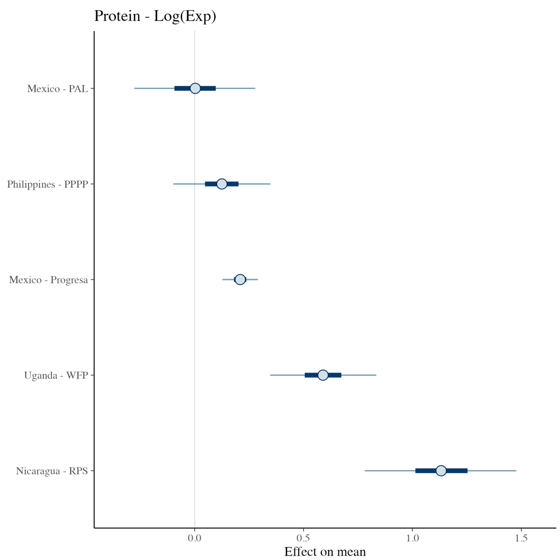


(a) RCT-wise treatment effects

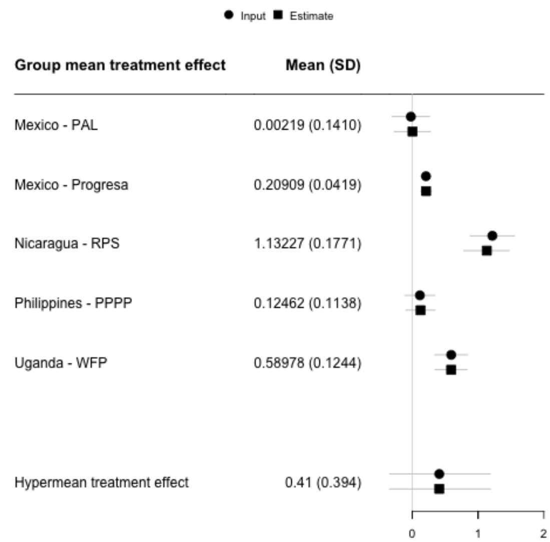
(b) Hypermean treatment effect

Note: Results for the Philippines are omitted because we cannot differentiate between coarse and fine staples in that context.

FIGURE A4: Treatment effects on protein demand aggregated using Bayesian Hierarchical Modeling

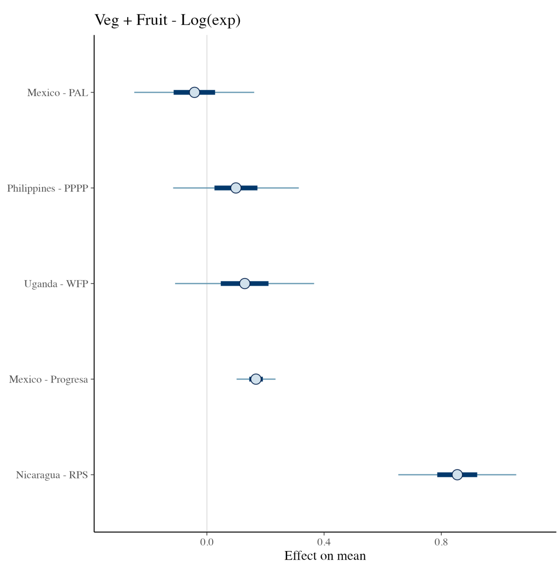


(a) RCT-wise treatment effects

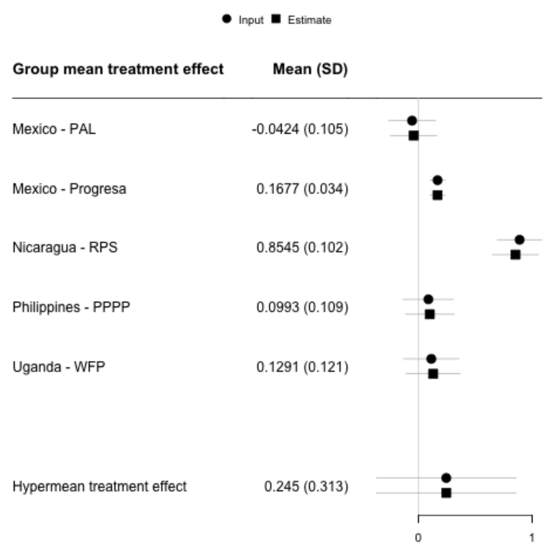


(b) Hypermean treatment effect

FIGURE A5: Treatment effects on vegetables and fruits demand aggregated using Bayesian Hierarchical Modeling



(a) RCT-wise treatment effects



(b) Hypermean treatment effect

TABLE A1: Overview of food types surveyed and categorization in food groups by RCT

Staples (coarse + fine):	
Pal	Corn, Oats + rice, white bread, sweet bread, box bread
Progresa	Corn + rice, white bread, sweet bread, box bread
Nicaragua	Oats, corn, grained corn + rice, bread, sweet bread
Philippines	Cereals (rice, corn, bread, biscuits, flour etc.)
Uganda	Corn, grained corn, sorghum, millet + rice, bread
Tubers:	
Pal	Potato
Progresa	Potato
Nicaragua	Potato, yucca
Philippines	Roots (potato, cassava, sweet potato etc.)
Uganda	Potato, sweet potato, cassava, dry cassava
Protein:	
Pal	Chicken, beef, pork, sheep, goat, fish, sardines, tuna, eggs, sausages, milk, yogurt, powdered milk
Progresa	Chicken, beef, pork, sheep, goat, fish, eggs, milk
Nicaragua	Beef, pork, bones, chicken, fish, shrimps, tuna, sausage, egg, fried fish, milk, powdered milk
Philippines	Fish, meat, dairy (fresh chicken, fresh beef, fresh pork, corned beef etc.)
Uganda	Beef, pork, goat, other red meat, blood, white meat, fish, eggs, milk, powdered milk
Vegetables and fruits:	
Pal	Tomato, carrot, leafy vegetables, cactus, squash, chayote, guayaba, mandarin, papaya, orange, banana, apple, lemon, watermelon
Progresa	Tomato, carrot, leaf vegetables, cactus, orange, banana, apple, lemon
Nicaragua	Pepper, tomato, salad, cucumber, carrot, banana, avocado, citrus fruits, tropical fruits, other fruits
Philippines	Vegetables and fruits (fresh fruits, leafy vegetables , coconut etc.)
Uganda	Tomato, orange color vegetables, leafy greens, other vegetables, banana, avocado, orange fruits, other fruits

Note: In the Philippines we cannot distinguish between coarse and fine staples and so we use the data only for overall staples.

TABLE A2: Income elasticities of food demand and household-level nutrition impacts using a binary treatment indicator and pooled OLS on all five programs

		Weekly consumption in control households				*Impact of Conditional Cash Transfer:*			
Food item	Elasticity	Calories	Carbs (g)	Protein (g)	Fat (g)	Calories	Carbs (g)	Protein (g)	Fat (g)
Staples and tubers:									
Corn	0.008 (0.103)	9049.28	1967.69	344.08	142.05	68.78	14.96	2.62	1.08
Rice	0.161 (0.039)***	2340.55	517.80	42.50	3.40	376.58	83.31	6.84	0.55
Bread	0.339 (0.058)***	708.99	141.80	19.34	6.45	240.36	48.07	6.56	2.19
Potato	0.359 (0.076)***	379.81	86.32	10.11	0.44	136.27	30.97	3.63	0.16
Protein:									
Chicken	0.434 (0.085)***	740.44	0.00	89.97	40.04	321.42	0.00	39.06	17.38
Pork	0.197 (0.050)***	506.07	0.00	32.52	40.79	99.77	0.00	6.41	8.04
Beef	0.244 (0.054)***	247.30	0.00	44.96	6.13	60.23	0.00	10.95	1.49
Fish	0.071 (0.024)***	36.77	0.00	8.03	0.27	2.60	0.00	0.57	0.02
Eggs	0.217 (0.048)***	1155.74	5.82	101.83	76.86	250.46	1.26	22.07	16.66
Milk	0.170 (0.055)***	65.66	5.14	3.61	3.50	11.17	0.87	0.61	0.60
Beans	0.075 (0.033)**	2116.17	375.34	129.75	16.71	158.60	28.13	9.72	1.25
Vegetables and fruits:									
Tomato	0.188 (0.043)***	174.48	37.71	8.53	1.94	32.75	7.08	1.60	0.36
Leafy greens	0.056 (0.014)***	6.87	1.08	0.85	0.12	0.38	0.06	0.05	0.01
Onion	0.160 (0.027)***	251.39	58.70	6.91	0.63	40.16	9.38	1.10	0.10
Banana	0.253 (0.060)***	421.44	107.96	5.16	1.56	106.51	27.29	1.30	0.39
Other:									
Cookies	0.086 (0.020)***	395.27	57.22	5.07	15.61	34.02	4.92	0.44	1.34
Sugar	0.132 (0.051)***	6273.26	1620.67	0.00	0.00	825.29	213.21	0.00	0.00
Oil	0.103 (0.054)*	4521.85	0.00	0.00	511.52	463.61	0.00	0.00	52.44
Total		29391.34	4983.25	853.22	868.02	3228.96	469.51	113.53	104.06
% change						10.99	9.42	13.31	12.00

Note: Standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Each row is a separate regression. All specifications include RCT fixed effects. Standard errors clustered at village (randomization unit) and bootstrapped (100 reps). Elasticities are calculated using the formula: $\frac{P}{100} = \exp(\hat{\beta}) - 1$. Increase calculated using the estimated elasticity, average weekly consumption in control households (kg) and average nutrients by kg.