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ABSTRACT

We use a randomized controlled trial to study the effect of large income changes, through unconditional cash transfers, on the food share of expenditures and consumption of calories among poor households in rural Kenya. Our preferred estimate of the food elasticity following USD 709 transfers is 0.78 for expenditure, 0.60 for calories, and 1.29 for protein. Experimental elasticities are lower than cross-sectional estimates. These estimates are unaffected by spillovers or price changes at the village level: results are similar with vs. without an almost ideal demand system, and with a control group in treatment vs. control villages.

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A randomized controlled trials registry entry is available at
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1 Introduction

The response of households in developing countries to income changes in terms of food share of expenditures and calorie consumption is of significant interest to both policymakers and economists. It is a crucial element in modeling the consumption and savings choices of households, and a central ingredient in designing tax and transfers policy, labor market policy, and insurance markets (Deaton 1992; Hall and Mishkin 1982; Jappelli and Pistaferri 2010). In developing countries, it can inform the design of consumption support policies and redistribution programs (Fenn et al. 2015; Luseno et al. 2014; Robertson et al. 2013; Fernald and Hidrobo 2011; Schady and Paxson 2007; Aguero, Carter, and Woolard 2006; Cunha 2014; Blattman, Fiala, and Martinez 2013; Aker 2015; Schofield 2014). A main reason for the importance of such responses is that they provide information about the source of possible poverty traps: If households show a strong response to income changes in terms of calorie consumption, a nutrition-based poverty trap is plausible (Banerjee, Banerjee, and Duflo 2011; Schofield 2014). The potential for nutrition-based poverty traps has received significant attention in the literature (Dasgupta and Ray 1986), and there is debate as to whether they exist (Deaton and Drèze 2009; Behrman and Deolalikar 1987).

The present study revisits the question of the income elasticity of nutrition in the context of the unconditional cash transfer program of the NGO *GiveDirectly, Inc.* (GD), first studied by Haushofer and Shapiro (2016). Between 2011 and 2013, GD sent unconditional cash transfers of at least USD 404, corresponding to at least twice monthly average household consumption, to randomly chosen poor households in Kenya through the mobile money system *M-Pesa*.[1] The transfers were explicitly described to households as fully unconditional, and as short-term windfalls, rather than as a promise of recurring payments for the long term. Within the treatment group, transfer recipient within the household (wife vs. husband), transfer timing (monthly installments over nine months vs. one-time lump sum transfer) and transfer magnitude (USD 404 vs. USD 1520) were randomized. We surveyed randomly selected treatment and control households both before the program and between 1

[1] All USD values are calculated at purchasing power parity, using the World Bank PPP conversion factor for private consumption for KES/USD in 2012, 62.44. The price level ratio of PPP conversion factor (GDP) to KES market exchange rate for 2012 was 0.5. These figures were retroactively changed by the World Bank after 2013; we use those that were current at the time the study was conducted.

and 15 months after it ended. The present study builds on this dataset, but extends it in two ways to obtain nutrition estimates for the purposes of this study: a price survey, and a weight survey to map food weights into nutrients. In combination with the existing dataset, this confers several advantages to our study, which allow us to establish the following results.

First, we identify elasticities by instrumenting total expenditure or consumption with the randomly assigned receipt of the unconditional cash transfer. This feature of our setting allows us to make causal statements with greater confidence than was possible in previous studies, which often relied on cross-sectional estimation of Engel curves and elasticities (Deaton and Subramanian 1996). We find income elasticities that are higher than reported in some previous studies using cross-sectional approaches. For example, in a review of 66 studies containing 1,444 food expenditure elasticity estimates, Colen et al. (2018) report an average expenditure elasticity of 0.61. In contrast, our preferred estimate is 0.78. This estimate makes a nutrition-based poverty trap marginally less implausible than the low previous estimates. However, income would still have to display a surprisingly high elasticity with respect to food expenditure (or calories) to generate a trap. Some nutrients display higher elasticities; e.g. we estimate an elasticity of 1.30 for protein. Thus, if productivity is strongly dependent on protein, a trap may still be possible. .Importantly, we also show that cross-sectional elasticity estimates are unlikely to be reliable: when comparing our experimental estimates to cross-sectional estimates obtained using the same data, we find substantial differences, with the cross-sectional elasticities consistently larger than the experimental ones. For example, the non-experimental elasticity for overall food expenditure is 0.91, i.e. larger than the experimental elasticity of 0.78.

Second, when income changes lead to substitution towards more expensive calories, the income elasticity of expenditure will overestimate the elasticity of calories. To account for this fact, we combine our expenditure data with price data and nutrition tables to obtain estimates of nutrient intake, including calories, protein, fats, carbohydrates, fiber, and iron. This data was obtained through two survey efforts: first, we collected detailed information on local prices of the goods covered by our consumption survey at the market level at the time of the endline, i.e. in 2012. This survey has the additional advantage that it is not collected at the household level, thus ruling out substitution towards more expensive varieties as a possible source of differences in prices between treatment and control groups. Second, a team of surveyors bought and weighed five items from each food category in local stores. These weights enable us to use nutrition tables to compute how many nutrients (calories, fats, etc.) households consumed from a given food category. This data then allows us to estimate not only expenditure, but also nutrition elasticities. We find that households substitute towards more expensive calories, evident in the fact that the nutrition elasticities are consistently lower than the expenditure elasticities: our preferred expenditure elasticity estimate is 0.78, while the calorie elasticity estimate is 0.60. Nevertheless, this elasticity estimate is substantially higher than what is reported in the literature; the review by Colen et al. (2018) finds a mean calorie elasticity of 0.42 averaging over 120 estimates.

Third, estimation of the effect of income changes on expenditure patterns is potentially complicated by general equilibrium effects: if the program is substantial enough to generate significant variation in expenditure, it may also be large enough to move local prices. To deal with this possibility, we use the price data described above to estimate a linearized version of the almost ideal demand system (AIDS; Deaton and Muellbauer 1980), effectively enabling us to estimate elasticities while accounting for changes in prices. We find little effect on the results, suggesting that price effects are an unlikely confound in estimates of consumption elasticities, even with substantial interventions such as this one.

Fourth, when estimating nutrition elasticities using exogenous income changes such as in cash transfer programs, spillovers to non-recipient households are a potential concern. Our setting offers two advantages in this regard. First, our preferred specification uses randomization of villages, rather than households, into treatment and control, allowing us to obtain across-village estimates that are free from within-village spillover effects. Second, because we also surveyed control households in treatment villages, we additionally estimate how important such spillovers at the village level are. We find little evidence that randomization at the household level leads to substantially different estimates from randomization at the village level; our preferred within-village elasticity is 0.79, very close to our preferred across-village elasticity of 0.78. This finding therefore implies that spillover effects are small and elasticities can sensibly be measured within villages.

Finally, our study varies three design features of unconditional cash transfers: frequency (monthly vs. lump-sum), recipient gender (male vs. female), and magnitude (USD 404 vs. USD 1520). In the context of the present study, the randomization of transfer magnitude is especially salient because it allows us to estimate whether Engel curves are linear. We find little evidence of non-linearity; our preferred specification suggests a higher elasticity for small than for large transfers, suggesting some concavity, but the difference between these results is not statistically significant. This result further corroborates the view that a calorie-based poverty trap is unlikely, at least over the range of transfers studied here. We also find little evidence of differences in elasticities across male and female recipient households, suggesting that households are unitary and/or men and women have similar preferences over food consumption.

Our paper contributes to a large literature that aims to estimate the food expenditure and nutrition elasticity from observational data. Previous approaches have used cross-sectional estimates (Deaton and Subramanian 1996; Jappelli and Pistaferri 2010; Skoufias 2003), time-series data (Dynarski et al. 1997; Krueger and Perri 2010; Krueger and Perri 2006; Browning and Crossley 2001; Hall and Mishkin 1982), or natural or policy shocks to study household responses (Johnson, Parker, and Souleles 2006; Souleles 2002; Shapiro and Slemrod 1995). However, estimating the income elasticity of calorie consumption from observational data alone presents significant challenges. In the cross-section, households that have different resources may have different tastes, different opportunities, and face different prices, which complicates the interpretation of estimates of elasticities. Cross-sectional estimates may also be biased by reverse causality, e.g. if calorie intake affects productivity, or simultaneous causality, e.g. if health affects both calorie intake and income. In time series, changes in income are typically accompanied with changes in the economic environment faced by the household (e.g. changes in wages or labor productivity). Finally, because policymakers in developing countries have often been wary of unconditional income transfers, most income redistribution to the poor is either in kind or attached to conditionalities, and therefore few (natural) experiments exist.

We are not aware of previous studies that use randomized unconditional cash transfers to estimate income elasticities of food expenditure and calories while controlling for prices. However, a few recent studies come close. Angelucci and Attanasio (2013) and Attanasio, Battistin, and Mesnard (2012) study the effect of conditional cash transfers (CCT) programs in Mexico (PROGRESA/Oportunidades) and Ecuador (Bono de Desarollo Humano, BDH) on the food share of expenditures, finding an increase in the former study and a small decrease in the latter. These studies differ from ours in that the transfers are conditional, are made only to women, and price data are not available or incomplete, and nutrient elasticities and spillovers cannot be studied. Attanasio and Lechene (2010) and Hoddinott and Skoufias (2004) use a similar approach for PROGRESA, but have access to price data. Attanasio and Lechene (2010) do not estimate elasticities explicitly; Hoddinott and Skoufias (2004) obtain calorie elasticities between 0.3–0.5. Again conditionality and targeting of women makes these studies different from ours. Armand et al. (2016) compare conditional cash transfers to men vs. women in Macedonia, finding that targeting transfers to women increases the expenditure share on food by 4–5 percentage points. In this study, the absence of a pure control group makes it difficult to obtain direct experimental estimates of elasticities. Schady and Rosero (2008) estimate the effect of monthly unconditional cash transfers to women in Ecuador on the food share, finding an increase of 3–4 percentage points. However, a large number of control group households in their experiment received treatment, and there were large baseline differences between treatment and control groups. In addition, transfers again are made only to women, price data are not available, and nutrient elasticities and spillovers are not studied. Gangopadhyay, Lensink, and Yadav (2012) show positive reduced-form impacts of a UCT program in India on calorie consumption, but do not estimate elasticities. Thus, estimates of the income elasticity of food expenditure and calories based on unconditional cash transfers are scarce, especially with price controls.

Finally, the estimates provided in this paper can potentially be combined with estimates of the income elasticity of calorie consumption to assess whether a poverty trap is possible. Most prominently, Schofield (2014) finds effects of providing extra calories over a period of 5 weeks on the productivity of rickshaw drivers in India that correspond to an elasticity of productivity with respect to calorie consumption of around 0.31. This elasticity would still be too low to generate a trap, even in combination with our highest estimates, but differences in setting and timeframe make the two sets of results difficult to compare.

The remainder of the paper is organized as follows. Section 2 gives an overview of the intervention, study design, and datasets. Section 3 lays out the econometric framework. Section 4 presents results, and Section 5 concludes.

2 Intervention, study design, and data

The intervention, experimental design, and econometric approach used in this study have previously been described elsewhere (Haushofer and Shapiro 2016), and are briefly summarized here. We refer the reader to the companion paper for details.

GiveDirectly, Inc. (*GD*; www.givedirectly.org) is an international NGO founded in 2009 whose mission is to make unconditional cash transfers to poor households in developing countries. At the time of the study, eligibility was determined by living in a house with a thatched (rather than metal) roof. Recipients were informed that they would receive a transfer of KES 25,200 (USD 404 PPP), and that this transfer was unconditional and one-time. Recipients were provided with a *Safaricom* SIM card and had to register it for the mobile money service *M-Pesa* in the name of the designated transfer recipient. They could keep the *M-Pesa* account even after the study, and money could be stored there indefinitely. Anecdotally, most recipients withdrew the money very soon after receipt, and in earlier work we found that *M-Pesa* balances at endline were below USD 5 PPP in all groups.

An overview of the design and timeline is shown in Figure 1. The study was conducted in Rarieda district, Kenya. Consumption levels are relatively low in our setting. As reported in previous work, average monthly food expenditure in the control group at endline was USD 104 PPP, which translates to USD 0.68 PPP per person per day in the average household of five (Haushofer and Shapiro 2016). In line with these low levels of consumption, 23 percent of respondents report having gone to bed hungry at least once in the preceding week.

Among the 120 villages with the highest proportion of thatched roofs in Rarieda, 60 were randomly chosen to be treatment villages. Within these villages, half of all eligible households were randomly chosen to be treatment households, while the other half were control households. A household was eligible if it had a thatched roof. This process resulted in 503 treatment households and 505 spillover households in treatment villages at baseline. Villages had an average of 100 households, of which an average of 19 percent were surveyed, and an average of 9 percent received transfers. The transfers amounted to an average of 10 percent of aggregate baseline village wealth (excluding land).

Among treatment households, we further randomized whether the transfer went to the husband or the wife (in dual-headed households). In addition, 137 households in the treatment group were randomly chosen to receive "large" transfers of KES 95,200 (USD 1,525 PPP, USD 1,000 nominal) per household, while the remaining 366 treatment households received "small" transfers of KES 25,200 (USD 404 PPP, USD 300 nominal) per household. Finally, we randomly assigned the transfer to be delivered either as a lump-sum amount or as a series of nine monthly installments. We only consider the 173 monthly recipient and 193 lump-sum recipient households that did not receive large transfers, because large transfers were not unambiguously monthly or lump-sum. The total amount of each type of transfer was KES 25,200 (USD 404 PPP).

We conducted a baseline survey with all treatment and spillover households before they received the first transfer, and an endline after the end of transfers. Households received the first transfer an average of 9.3 months before endline, the last transfer an average of 4.4 months before endline, and the mean transfer an average of 6.9 months before endline.[2] The order in which villages were surveyed at baseline was randomized, and at endline it followed the same order. In a small number of households, the endline survey was administered before the final transfer was received. These households are nevertheless included in the analysis to be conservative (intent-to-treat).

Control villages were surveyed only at endline; in these villages, we sampled 432 "pure control" households from among eligible households. Because these pure control households were selected into the sample just before the endline, the thatched-roof criterion was applied to them about one year later than to households in treatment villages. This fact potentially introduces bias into the comparison of households in treatment and control villages; however, as shown in Haushofer and Shapiro (2016), this bias was negligible, amounting to 5 households, or 1.1 percent of the sample. We therefore do not control for it further here; this produces results very similar to a number of bounding approaches, as shown in our original paper. The lack of baseline data in the pure control group also implies that we cannot use a first differences approach for this group.

[2]The mean transfer date is defined as the date at which half of the total transfer amount to a given household has been sent.

2.1 Data and Variables

In each surveyed household, we collected two survey modules: a household module, which collected information about assets, consumption, income, food security, health, and education; and an individual module, which collected information about psychological wellbeing, intra-household bargaining and domestic violence, and economic preferences. The two surveys were administered on different (usually consecutive) days. This paper is based on the household survey, which was administered to any household member who could give information about the outcomes in question for the entire household. This was usually one of the primary members.

The household survey contained a detailed consumption expenditure module, which is the main focus of the present paper. It asked for household expenditure in the preceding week on each of 99 foods (e.g. rice, tomatoes, butter), which were aggregated up to 18 food categories (e.g. cereals, vegetables, oils); 46 non-food items with smaller and more frequent expenditures, such as airtime, travel expenses, and firewood, with a weekly recall period, aggregated up to 10 categories; and 26 non-food items with larger and less frequent expenditures, such as weddings, funerals, and home repair, with a 12 month recall period, aggregated up to 11 categories. We use these expenditures to compute monthly totals at purchasing power parity.[3]

To convert food expenditure into calories and other nutrients, we first need to compute the quantities of each consumed item based on expenditure. To do this, we require price data. In the original study, we conducted a separate price survey at the village level, in which we elicited prices for 38 common food and non-food items from 397 individuals, spread over all study villages. This survey allowed us to compute quantities for most consumption categories. However, it did not collect some prices for items in smaller categories: "non-alcoholic drinks", "fats", and "sweets and spices". To complete the price data for these consumption categories, we conducted a new price survey in 2016, in which we collected 2012 prices for 31 food items from these categories. Prices for these items were collected from five different markets in the city of Rongo in western Kenya. Together, our price data therefore allows us to compute monthly quantities consumed of each item in our consumption survey.

An additional problem, however, was that some prices in the price survey were in

[3]The full survey is available at http://www.princeton.edu/haushofer. Weekly data are divided by 7 and multiplied by 30.5 to convert to monthly. Yearly data are divided by 365 and multiplied by 30.5.

"local" units which are easy for respondents to understand, but which do not correspond to those used in standard food composition tables. For example, some prices are given "per bunch". Overall, this was the case for five consumption categories. To convert these units into known weight units, e.g. price per gram, we also obtained estimates of the weight corresponding to the "local" units, by buying five units of the items in question, and obtaining their average weight in grams.

Finally, to allow us to compute nutritional content for each consumption category, we used the detailed nutritional composition tables from the West Africa Food Composition Table 2012, published by Food and Agriculture Organization of the United Nations[4]. Together with knowledge about the quantities consumed from the combination of the household and price surveys, these weights enable us to compute how many nutrients (calories, fats, etc.) households consumed in a given food category. Thus, we first convert the food expenditure data collected at endline into food quantities using the price and weight data, and then convert the weights into calories, protein, fat, carbohydrates, and fiber using nutrition tables. The resulting variables record monthly intake of these nutrients for each household. See Table A.1 for a more detailed descriptions of the variables.

2.2 Integrity of experiment

Earlier results have shown that our study had good baseline balance on most outcomes of interest, and therefore we do not repeat this discussion here (Haushofer and Shapiro 2016). Due primarily to registration issues with *M-Pesa,* 18 treatment households had not received transfers at the time of the endline, and thus only 485 of the 503 treatment households were in fact treated. We deal with this issue by using an intent-to-treat approach. We had low levels of attrition; overall, 940 of 1,008 baseline households (93.3 percent) were surveyed at endline. As shown previously, our results are unlikely to be affected by this attrition (Haushofer and Shapiro 2016). Both for the original study and for this follow-on analysis, we wrote a pre-analysis plan (PAP). The PAP pertinent to this paper is published and time-stamped at https://www.socialscienceregistry.org/docs/analysisplan/625.

[4]The database is available at http://www.fao.org/3/a-i2698b.pdf

3 Econometric framework

3.1 Main estimation: An almost ideal demand specification using across-village variation in treatment

The random variation of the cash transfer across households allows us to identify causal effects of income changes on food shares and nutrient consumption using an instrumental variable approach. Moreover, because we have prices measured at endline, we are able to take account of potential general equilibrium effects on prices. The standard approach for estimating such an elasticity taking account of prices is the almost ideal demand system (Deaton and Muellbauer 1980).[5] It is attractive because it has a structure that is consistent with economic theory in that it is consistent with utility maximization under a budget constraint, that allows for flexible utility functions that are non-homothetic, and that allows for prices to play a role and hence it can account for general equilibrium effects of our intervention. Often there is not sufficient relative price variation to identify all coefficients in the full system, and Deaton and Muellbauer (1980) point out this in their original article. They suggest that in such cases the a linearized version of this system can be used. In particular, they suggest that the Stone price index can be used instead of the translog price index of the full system (see Stone (1953)). The Stone price index is a weighted geometric average of log prices, where the weights are given by the budget share for each good. It is this linearized version of the system that we use in this paper.[6]

[5]Note that for many goods, the quadratic version of this system, the QUAIDS due to Banks, Blundell, and Lewbel (1997) , has been shown to fit data better. However, for food, the AIDS system has proven to be a good application and is the most used. Actually Banks, Blundell, and Lewbel (1997) did not reject the log-linear specification for food although they did for the other goods that they considered.

[6]Note that in the pre-analysis plan we suggested that we would show results for the QUAIDS system in addition to the AIDS system. We pre-specified that we would use lnUCT and squared lnUCT as instruments for this system. However, it turned out during analysis that we do not have a strong enough first stage. Using the Sanderson-Windmeijer multivariate F test, we found lnUCT and its square to be weak instruments for log total expenditure and its square in our specifications. Following Sanderson and Windmeijer (2016), we used the Stock and Yogo (2005) weak instrument critical values for the conditional F-statistics. We defined weak instruments as those that lead to a rejection rate of 10 percent when the true rejection rate is 5 percent and use the corresponding critical value of 7.03. For all of our specifications we cannot reject the null hypothesis of weak identification. (see Table A.2 for the F-statistics and corresponding *p*-values). For this reason we report results only for the linearized AIDS. See Table A.3 for other discrepancies between pre-analysis plan and current specifications.

The linearized AIDS is given by the following equation:

$$\omega_{hv} = \alpha + \beta \ln z^*_{hv} + \gamma(\ln p^f_v - \ln p^n_v) + \xi' \mathbf{X_{hv}} + \varepsilon_{hv} \quad (1)$$

where ω_{hv} is the outcome of interest for household h in village v. z_{hv} denotes the monthly non-durable expenditure, and $\ln z^*_{hv} = \ln \frac{z_{hv}}{a^*(\mathbf{p_v})}$, where $\ln a^*(\mathbf{p_v})$ is the Stone price index: $\ln a^*(\mathbf{p_v}) = \overline{w^f} ln p^f_v + \overline{w^n} ln p^n_v$. p_v is a village price, where superscripts f and n refer to food and non-food prices, respectively. $\overline{w^x}$ is the average budget share for good $x \in \{f, n\}$ in the sample. X_{hv} is a vector of baseline demographic control variables[7], and ε_{hv} is an idiosyncratic error term.

For our main estimation, we use only endline data and apply $\ln UCT_{hv}$ as an instrument for $\ln z^*_{hv}$. $\ln UCT_{hv}$ is log of the randomly assigned cash transfer amount received by household h in village v. To deal with possible zeroes in the expenditure data, we use the inverse hyperbolic sine transform wherever we mention logs (Burbidge, Magee, and Robb 1988; MacKinnon and Magee 1990; Pence 2006). We use treated households from treated villages and households from pure control villages. That is, we do not in the main estimation include the households that we refer to as "spillover households", i.e., the household in treatment villages that did not receive the treatment. These households are included in a next step discussed in Section 3.2. Standard errors are clustered at the village level.

To compare the results of this "experimental" analysis to a cross-sectional analysis that resembles many of the previous studies on the income elasticity of consumption, we next repeat the same estimation without instrumenting total expenditure with the randomly assigned cash transfers. To avoid using any experimentally induced variation in expenditure in this analysis, we restrict the sample to households from control villages. Thus, this analysis is based on the cross-section only, and hence comparable in methodology to the majority of previous studies on the topic.

In both cases, we then compute the elasticity as follows:

$$e_{hv} = \frac{\beta}{\omega_{hv}} + 1, \quad (2)$$

[7] For estimations that include treated and spillover households, we control for the baseline number of children and number of adults. For estimation that include treated and pure control households, we use one variable for total number of household members because the number of adults was not registered for pure control households at census.

We present an experimental and a non-experimental version of this elasticity for all empirical specifications we consider. Since the budget share varies across households, there is a question of whose budget share we should use to report the elasticity. We report the elasticity evaluated at the mean and median household budget shares, respectively, and in addition we report the mean and the median elasticities using household-specific shares to find the full distribution of elasticities.

3.2 Within-village analysis in levels

In our second estimation, we restrict the sample to treatment villages and study households that received the treatment (treatment households) and within-village control households ("spillover" households). We use the following specification:

$$\omega_{hv} = \alpha + \beta \ln z_{hv} + \delta_v + \xi' \mathbf{X_{hv}} + \varepsilon_{hv} \tag{3}$$

Again, we estimate based on endline data only, and we use $\ln UCT_{hv}$ as an instrument for $\ln z_{hv}$. The motivation for this analysis is twofold. First, in this within-village analysis, we can include village-level fixed effects, which not only increases precision, but also allows us to not worry about controlling for village-level prices or other village-level controls. The benefit of this approach is that our point estimates and precision will neither be affected by potential measurement error in prices or other control variables, nor will we need to impose the structural assumptions of the demand system. Second, previous work suggests that the spillover effects in this study were small, making the within-village control households a reasonable comparison group (Haushofer and Shapiro 2016). Standard errors are not clustered in this analysis because the randomization occurs at the household level.

In the "non-experimental" version of this specification, the sample is simply restricted to spillover households.

3.3 Within-village analysis in first differences

Third, to investigate the precision of our findings, we estimate the elasticities using a first difference approach. This specification has the potential to be more highly powered with a stronger first stage as the unconditional cash transfer may be more correlated with the change in expenditure from baseline to endline than any level of expenditure. As we only have data for control villages at endline, the first difference results are based on within-village estimations, i.e. the sample consists of treatment and spillover households. Because randomization is within villages, again village-level dummies pick up all variance at the village level, including prices and other village-level differences. Our demand system therefore collapses to:

$$\Delta\omega_{hv} = \alpha + \beta\Delta \ln z_{hv} + \delta_v + \varepsilon_{hv} \tag{4}$$

In the "experimental" version of this analysis, $\Delta \ln z_{hv}$ is again instrumented with the cash transfer. In the "non-experimental" version, we do not instrument, and restrict the sample to the spillover households.

3.4 Across-village analysis without price controls

Fourth, as we are interested in whether general equilibrium effects through prices are important, we also estimate the elasticities using the across-villages comparison, including treatment households and pure control households (i.e. no spillover households), and no price controls:

$$\omega_{hv} = \alpha + \beta \ln z_{hv} + \xi' \mathbf{X_{hv}} + \varepsilon_{hv}. \tag{5}$$

3.5 Differences across treatment arms

The RCT had three cross-randomizations which allow us to study whether results differ when transfer are made to the wife vs. the husband, as a lump-sum vs. in monthly installments, and when transfers are large (USD 1520) vs. small (USD 404). The impact of recipient gender on elasticities is important to inform the discussion around whether aid should be targeted at women when its goal is to improve nutrition for children and families in general. The impact of transfer timing and frequency is important to determine the existence of savings and credit constraints. Finally, the impact of transfer magnitude is informative about the curvature of the effects, and therefore speaks to whether there may be a poverty trap.

We estimate the difference between these treatment arms for each of the four specifications described in the previous sections. For the across-village specifications, we restrict the samples to pure control households and one treatment group, e.g. female recipient households, for each estimation. For example, when estimating the elasticities for female recipients, we restrict the sample to pure control households and treated households with female recipients (leaving out treated households with male recipients and the households that are not two-headed). Analogously, for within-village specifications, we restrict the samples to spillover households and one treatment group for each estimation, etc. In Appendix A we also report a first difference version of this specification. The p-values comparing elasticities across pairs of treatment arms (e.g. female vs. male recipient households) are computed as described in Appendix B.

3.6 Nutrients

To study the elasticities for nutrients, we use a similar set of specifications, with the same sample restrictions, as for the budget share estimations described above:

$$\omega_{hv} = \alpha + \gamma(\ln p_v^c - \ln p_v^n) + \beta \ln z_{hv}^* + \xi' \mathbf{X_{hv}} + \varepsilon_{hv}, \tag{6}$$

$$\omega_{hv} = \alpha + \beta_1 \ln z_{hv} + \delta_v + \xi' \mathbf{X_{hv}} + \varepsilon_{hv}, \tag{7}$$

$$\Delta\omega_{hv} = \alpha + \beta \Delta \ln z_{hv} + \varepsilon_{hv}, \tag{8}$$

$$\omega_{hv} = \alpha + \beta \ln z_{hv} + \xi' \mathbf{X_{hv}} + \varepsilon_{hv}, \tag{9}$$

where ω_{hv} is the budget share for the nutrient of interest for household h in village v, and p_v is a village price, where superscripts c and n refer to the nutrient of interest and all consumption categories other than c, respectively. The price of a particular nutrient is defined as a weighted average of all food prices. The weight for each food category is given by the ratio of the intake of nutrient from one food to the total intake across all food categories. All other variables are as described above, and we also instrument as described above. Again as above, we further test whether there are differences across elasticities for the experimental vs. non-experimental specifications, within vs. across villages, and across treatment arms.

To test the differences between the experimental and non-experimental version of each of the specifications and between treatment arms, we employ a strategy using nested models described in Appendix B.

3.7 Summary

Together, these specifications allow us to answer the following questions:

1. What is the range of the estimated elasticities for our preferred estimations (1 and 2)?
2. Do the experiment reveal different elasticities than the cross-sectional based findings?
3. Are spillover households affected by the treatment, i.e., are there spillovers?
4. Does it matter whether we estimate the elasticities from levels at endline or by taking first differences?
5. Are general equilibrium effects through prices important?
6. Are calorie and protein elasticities lower than expenditure elasticities?
7. Do elasticities differ across treatment arms?

4 Results

We begin by examining non-parametric plots of total expenditure and nutrients, shown in Figures 2 and 3. The categories we consider in the four panels of Figure 2 are total food expenditure; expenditure on meat, fish, dairy, and egg; expenditure on fruit, vegetables, and cereals; and other food. In Figure 3, the five panels show expenditure on total calories, protein, fat, carbohydrates, and fibre, respectively. In each figure, the lines are based on locally weighted scatterplot smoothing (LOWESS) with a bandwidth of 0.8, and show the relationship between the log of total expenditure on the horizontal axis, and category shares of total expenditure on the vertical axis. The blue line uses data from the treatment group, the red line from the pure control group. The lines are thus non-parametric cross-sectional Engel curves; their slope gives an indication of the cross-sectional elasticities. The dots indicate the mean values for the treated and pure control groups: the red dots represent the pure control group average, the blue dots the treatment group average, and the other dots correspond to the different treatment arms. A line through the dots corresponding to the control and treatment groups thus gives an indication of the experimental elasticities.

The plots suggest that the estimated elasticity for food from the experiment is smaller than that of the cross-section: both in Figure 2 and Figure 3, lines drawn through the dots corresponding to the treatment and control group means give steeper downward-sloping Engel curves than either of the cross-sectional Engel curves.

The estimated elasticities are presented in Table 1 and Table 2. As the elasticity is a function not only of the estimated coefficients, but also the budget share for food or nutrients as expressed in equation 2, the elasticities can be evaluated at, e.g., the mean or median budget share, or they can be calculated for each household and then represented by a descriptive statistic from that distribution. In the tables we present elasticities evaluated at the mean and the median, respectively, and we also give mean and median elasticities from the distribution of estimated household-specific elasticities. Table 1, columns (1)–(3), show results from estimating the AIDS in levels and across villages, using equation 1; column (1) shows the results of the experimental estimation, and column (2) of the cross-sectional estimation, with column (3) reporting the p-value of the difference between the two. Columns (4)–(6) report the analogous results for the within-village analysis using levels, as specified by equation 3. In Table 2, the first three columns report results for the within-village first differences approach given by equation 4, and the last three columns report the across-village specification without price controls given by equation 5. All results are very similar when estimated at the mean and median; we focus the following discussion on the mean, but the interpretation of the results for the median is the same.

Our preferred experimental estimates are those in column (1) of Table 1. We find an income elasticity of food expenditure of 0.78. This elasticity is below unity, but larger than some of the early estimates of this elasticity described above. The elasticity for meat, fish, dairy and egg is estimated to be 1.48, suggesting that this consumption category contains luxury goods. The elasticity for fruit, vegetables, and cereals is 0.75 and thus somewhat lower, suggesting that this category may contain staples with a low elasticity. These results are very similar across the different specifications presented in Tables 1 and 2.

The expenditure elasticities are likely to be an upper bound for the elasticities for calories, and a lower bound for more expensive nutrients such as protein.[8] The lower panels of Tables 1 and 2 show elasticities for nutrients. As expected, the calorie elasticity is lower than that for expenditure, with a preferred estimate of 0.60. Also expected, the elasticity for protein is higher, at 1.29, but lower than that for meat, fish, dairy and egg reported above (1.48). In contrast, the elasticity for carbohydrates is lower, at 0.45, again likely owing to the fact that staples contain carbohydrates. Thus, recipients appear to substitute towards more expensive nutrients with fewer calories and carbohydrates, and more protein. The shares of fat and fiber appear not to change dramatically. These results are again comparable across the different specifications, with the exception of a somewhat higher calorie elasticity in the first differences specification.

How do these experimentally obtained elasticities compare to what we would have estimated from the cross-section? Columns (2) and (5) in Tables 1 and 2 show the non-experimental estimates, and columns (3) and (6) show the p-values from the comparison to the experimentally calculated elasticities. Our main finding is that the experimentally estimated elasticities are often different from those obtained in the cross-section. In most cases, the cross-sectional estimates are higher than the experimental estimates, suggesting that the cross-sectional estimates may overestimate the extent to which a nutrition-based poverty trap may exist. In fact, the cross-sectional expenditure elasticity for food in our preferred specification is 0.91 for expenditures and 0.85 for calories. Since both estimates are rather close to unity, relying on the cross-sectional analysis might thus lead us to conclude that a nutrition-based poverty trap is at least possible. The lower experimental elasticities, in contrast, make it somewhat less likely.

We next compare the four different estimation strategies to each other: levels across villages; levels within villages; first differences within villages; and levels across villages without controls for prices. Table 3 reports the p-values of the comparison of the estimates shown in Tables 1 and 2 to each other, with the specific comparison specified in the column headings.

[8] Note, however, that most people in our sample do not have access to refrigeration, and many foods are therefore perishable; expenditure is thus usually fairly close in time to consumption.

Our core specification is the estimation in levels across villages with price controls, as specified in equations 1 and 6. The reason for this choice is that this specification is unaffected by village-level spillovers, and controls for possible price effects of transfers. The comparison of this analysis to the within-village estimation allows us to asses the importance of within-village spillovers. We find very similar elasticities in the two specifications, with all of the individual p-values well higher than conventional levels of statistical significance. Numerically, the largest differences are in the elasticities for fat, which is higher in the within-village specification, and fiber, which is higher in the across-village specification. Overall, however, we conclude that spillovers at the village level are not important in the estimation of our elasticities.

The second comparison of interest is between the within-village levels and within-village first differences specifications; this comparison allows us to assess the importance of baseline data. As shown in column (2) of Table 3, we find no statistically significant differences between the two approaches. Numerically, a few differences emerge: the elasticities for "other foods" and fat are somewhat higher when estimated in first differences, and that for protein is higher when estimated in levels. However, no consistent pattern emerges, and together with the fact that we cannot reject equality for any comparison, we conclude that the first differences approach has little effect on the elasticity estimates.

Finally, we compare the linearized AIDS estimated across villages with the across-village analysis that omits the price controls. This comparison allows us to test whether there might be price effects that distort the estimation of elasticities when prices are not taken into account. Again we find very little evidence of differences: as shown in column (3) of Table 3, the associated p-values are all above conventional levels of statistical significance. Numerically, all estimates are quite close to each other, with the exception of the elasticity for fruit, vegetable, and cereal expenditure, which is somewhat higher when price controls are included. This result suggests that prices may have decreased slightly for this group of items, such that an elasticity estimate that does not take this decrease into account will result in a lower elasticity. On the whole, however, we again conclude that price changes do not strongly affect our estimates, either statistically or economically.

Finally, we turn to the comparison of the various treatment arms in the treatment group: making transfers to women vs. men; monthly vs. lump-sum transfers; and large vs. small transfers. Results are shown in Table 4, using the across-village

AIDS specification.[9] For each pair of treatment arms, the first two columns show the separate elasticity estimates for the two treatment arms relative to the pure control group, and the third column shows the p-value for the difference between the two. Comparing elasticities for male and female recipients shown in columns (1)–(3), we find no significant differences; in fact, for overall food expenditure, the elasticities are almost identical, at 0.720 and 0.715 for female and male recipients, respectively.[10] The calorie elasticities are also similar, at 0.46 and 0.54, respectively. None of the other expenditure categories or nutrients show significant differences, even though some differences are numerically moderate (e.g."other foods", protein, and carbohydrates). Overall, we conclude that there is little evidence of differences in elasticities between female and male recipients, although a more highly-powered study might be able to detect such differences.

Second, when comparing monthly vs. lump-sum transfers in columns (4)–(6), we also find no statistically significant differences. Monthly transfers have slightly higher food expenditure and calorie elasticities (1.10 and 0.83, respectively)[11] than lump-sum transfers (0.83 and 0.65), and this qualitative difference is observed in most expenditure and nutrient categories. However, the standard errors are too large to make these differences statistically significant. The qualitative finding, however, is consistent with monthly transfers being used for consumption and consumption smoothing, while lump-sum transfers are used to make larger investments, e.g. in durables or livestock.

Third, we compare large and small transfers in columns (7)–(9). Again we find no statistically significant differences in this comparison; however, qualitatively, the large transfers generate lower elasticity estimates than the small transfers, consistent with some concavity.

5 Concluding remarks

In this paper we use data from a randomized controlled trial delivering unconditional cash transfers to poor households in Kenya to identify the income elasticity for

[9] See Tables A.4–A.6 for results using the other three specifications.

[10] Note that the average of these two need not amount to our preferred elasticity of 0.78 as we leave out households that are not two-headed.

[11] Note that these elasticities need not average to our preferred elasticity of 0.78 as we here leave out the large transfers that were not randomized into monthly versus lump-sum transfers, but rather only given in installments.

food expenditure and nutrients. In contrast to much of the previous evidence, we can use the random variation in total expenditure induced by the cash transfers to obtain causal estimates for the elasticities. We find that the expenditure and calorie elasticities, at 0.78 and 0.60, respectively, are closer to unity than many previous studies have indicated, but even so they are still substantially below unity. We note, however, that the calorie elasticity for protein is larger than unity. Our findings are relevant to the debate around nutrition-based poverty traps: for such a trap to exist, the food and calorie elasticities cannot be small. The fact that our preferred estimates are below unity suggests that such traps are unlikely unless protein is the most important factor for efficient production. In further support of this conclusion, our causal estimates for food expenditure and calories are smaller than our cross-sectional estimates, suggesting that the cross-sectional estimates in other studies may also be overestimated. In this case, the (unobserved) causal elasticities in these other settings may also be smaller. Thus, across settings, the true expenditure and calorie elasticities may be too small for nutrition-based poverty traps to be plausible.

There are two important caveats to this conclusion. First, when we consider specific food categories (such as meat, fish, dairy, and egg) and nutrients (such as protein), we find some elasticities that are larger than unity. Thus, nutrition-based poverty traps might be possible if work capacity is a function of e.g. protein intake. Second, a nutrition-based poverty trap can occur even with a small calorie elasticity if the elasticity of work capacity with respect to food is substantial. We are unable to study this relationship in the present paper. However, Heather Schofield's recent work provides a window on this question. She finds that providing an extra 700 calories per day to individuals who consume around 2,200 calories per day (a roughly 32 percent increase) leads to an increase in productivity of about 10 percent, suggesting an elasticity of income with respect to calories of 0.31 (Schofield 2014). This estimate is too low to generate a poverty trap in combination with any of our estimates. It is, of course, possible that differences in context (India vs. Kenya), occupation (rickshaw drivers vs. subsistence farmers), or other factors make Schofield's estimates incomparable to ours. Future studies might attempt to study the two elasticities in the same setting to obtain conclusive evidence for the empirical plausibility of a nutrition-based poverty trap.

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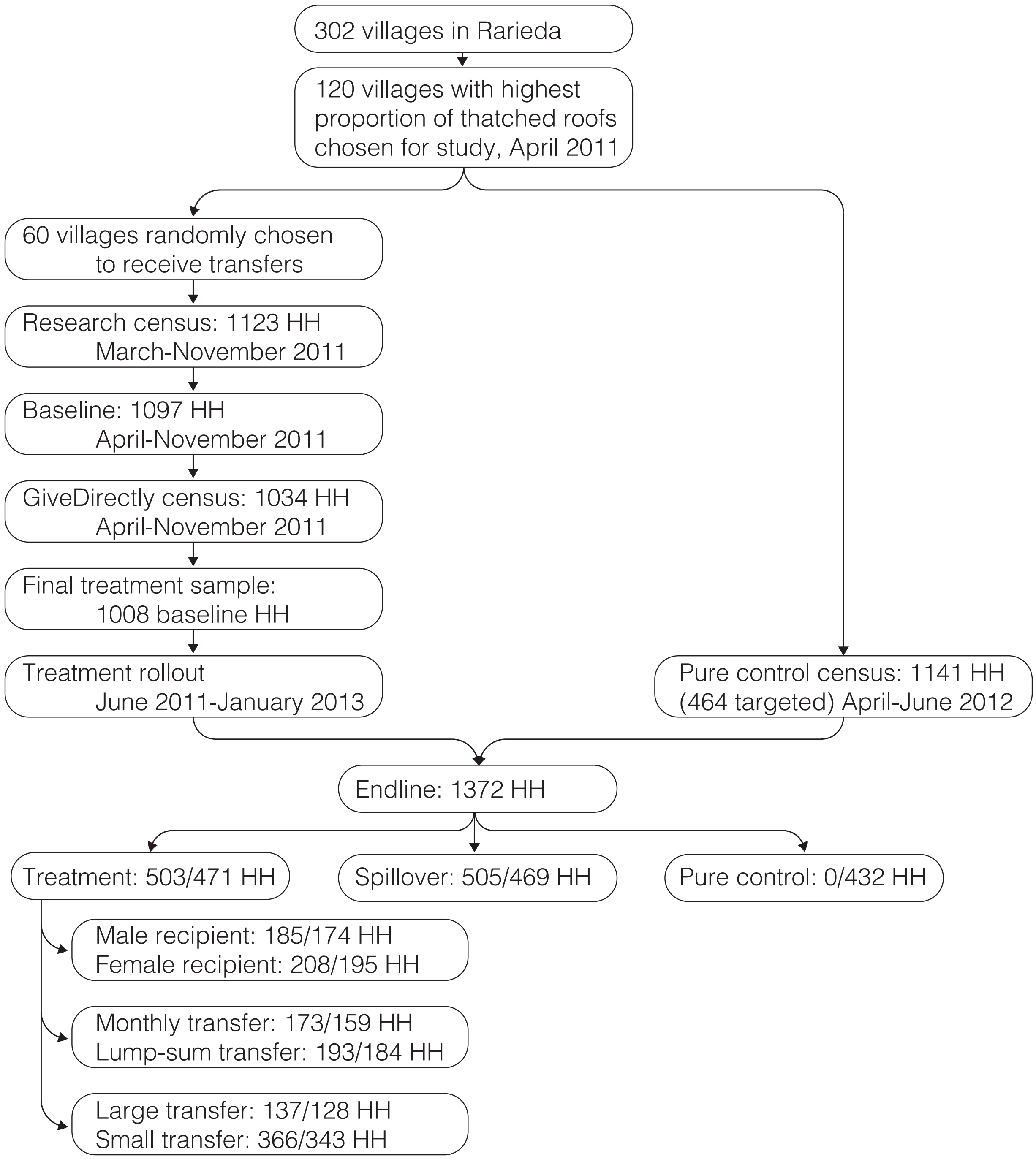
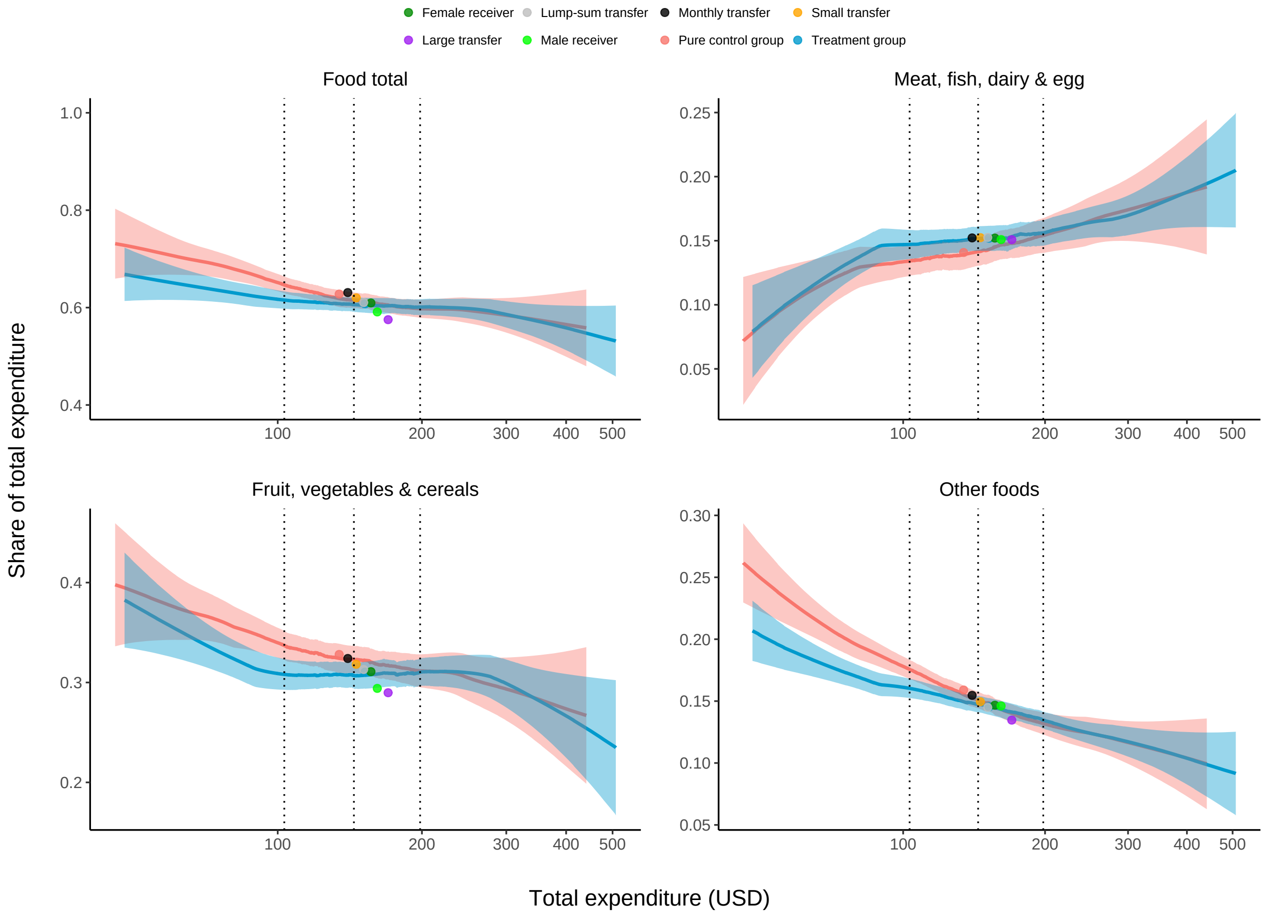
Figure 1: Timeline of study



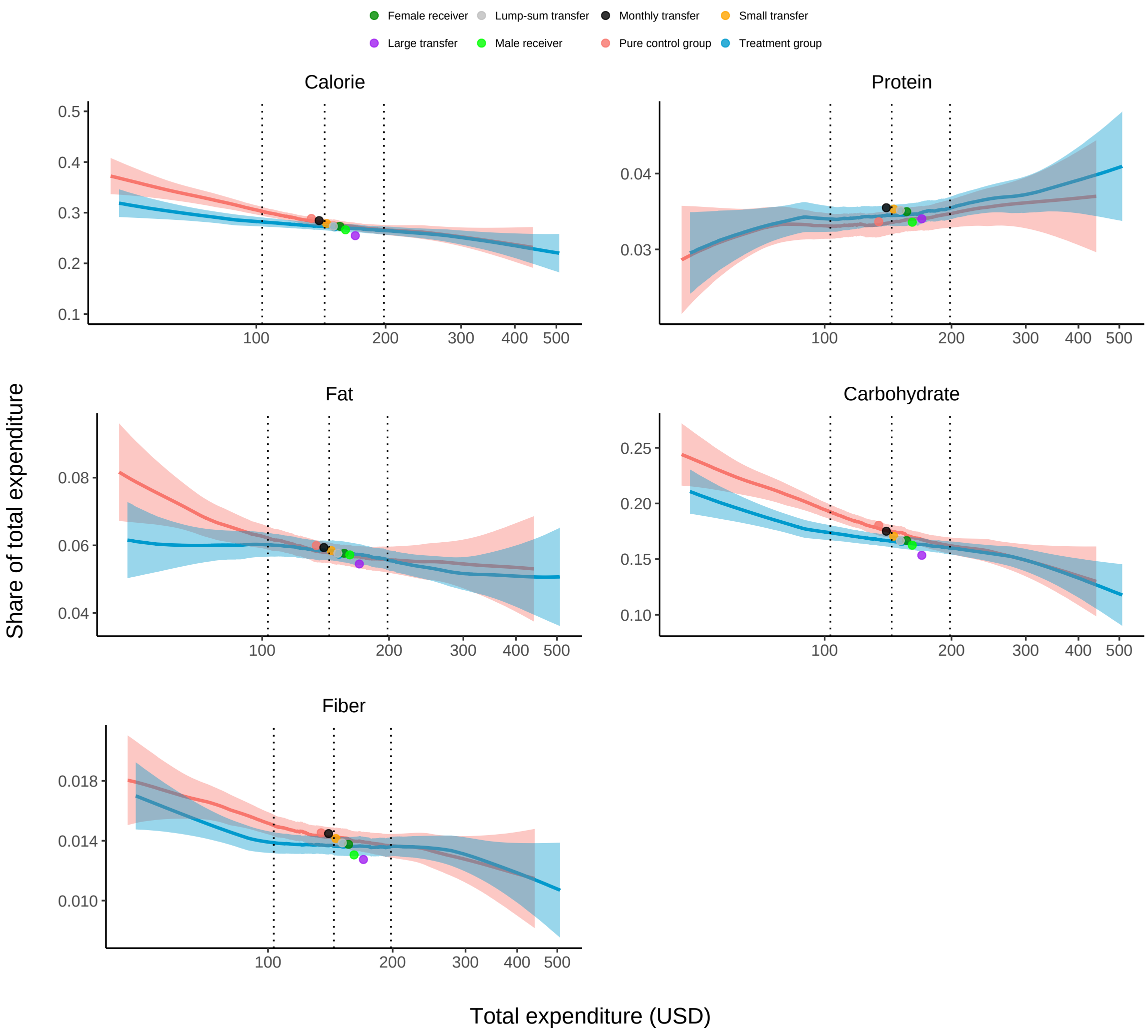
Notes: Timeline and treatment arms. Numbers with slashes designate baseline/endline number of households in each treatment arm. Male vs. female recipient was randomized only for households with co-habitating couples. Large transfers were administered by making additional transfers to households that had previously been assigned to treatment. The lump-sum vs. monthly comparison is restricted to small transfer recipient households.

Figure 2: LOWESS estimates for expenditure shares



Notes: Locally weighted smoothed scatterplots between category shares of total expenditure and log of total expenditure, for bandwidth equal to 0.8. Bandwidth refers to the share of total observations used for calculating smoothed values for each household (except for the end points). The shaded areas show the two-standard error bands for the nonparametric regressions. The standard errors are obtained by bootstrapping, with 100 replication for each of the regression curves. The blue dot shows the average total expenditure and category share for the treatment group, and the red dot for the control group. Other dots correspond to other treatment groups, as described by the legend. The vertical lines show the sample quartiles.

Figure 3: LOWESS estimates for calorie and nutrient shares



Notes: Locally weighted smoothed scatterplots between calorie and nutrients' shares of total expenditure and log of total expenditure, for bandwidth equal to 0.8. Bandwidth refers to the share of total observations used for calculating smoothed values for each household (except for the end points). The shaded areas show the two-standard error bands for the nonparametric regressions. The standard errors are obtained by bootstrapping, with 100 replication for each of the regression curves. The blue dot shows the average total expenditure and category share for the treatment group, and the red dot for the control group. Other dots correspond to other treatment groups, as described by the legend. The vertical lines show the sample quartiles.

Table 1: Income elasticities of food expenditure and nutrient availability I

	Across village In levels Linearized AIDS			Within village In levels		
	(1) Experimental (N = 903)	(2) Non-experimental (N = 432)	(3) Difference p-value	(4) Experimental (N = 940)	(5) Non-experimental (N = 469)	(6) Difference p-value
Food expenditure						
Food total						
At mean	0.780 (0.126)	0.913 (0.029)	0.287	0.793 (0.071)	0.976 (0.024)	0.015
At median	0.782 (0.125)	0.914 (0.029)	0.287	0.794 (0.071)	0.976 (0.024)	0.015
Mean	0.767 (0.134)	0.907 (0.031)	0.288	0.779 (0.076)	0.974 (0.026)	0.015
Median	0.782 (0.125)	0.914 (0.029)	0.287	0.794 (0.071)	0.976 (0.024)	0.015
Meat, fish, dairy & egg						
At mean	1.478 (0.287)	1.244 (0.082)	0.429	1.490 (0.190)	1.291 (0.055)	0.314
At median	1.511 (0.306)	1.272 (0.092)	0.454	1.530 (0.206)	1.327 (0.062)	0.345
Mean	1.711 (0.426)	1.367 (0.124)	0.437	1.732 (0.284)	1.441 (0.083)	0.326
Median	1.500 (0.300)	1.255 (0.086)	0.430	1.515 (0.200)	1.310 (0.059)	0.325
Fruit, vegetables & cereals						
At mean	0.751 (0.214)	0.877 (0.049)	0.539	0.607 (0.124)	0.936 (0.039)	0.011
At median	0.745 (0.220)	0.876 (0.050)	0.533	0.595 (0.128)	0.935 (0.040)	0.011
Mean	0.698 (0.261)	0.850 (0.060)	0.541	0.518 (0.152)	0.921 (0.049)	0.012
Median	0.745 (0.220)	0.876 (0.050)	0.533	0.595 (0.128)	0.936 (0.040)	0.011
Other foods						
At mean	0.239 (0.244)	0.699 (0.057)	0.044	0.535 (0.136)	0.786 (0.050)	0.085
At median	0.180 (0.263)	0.682 (0.060)	0.042	0.500 (0.147)	0.771 (0.054)	0.082
Mean	0.007 (0.318)	0.588 (0.078)	0.051	0.397 (0.177)	0.710 (0.068)	0.099
Median	0.180 (0.263)	0.682 (0.060)	0.042	0.501 (0.146)	0.771 (0.054)	0.083
Calorie and nutrient availability						
Calorie						
At mean	0.600 (0.167)	0.854 (0.035)	0.113	0.682 (0.087)	0.941 (0.031)	0.005
At median	0.597 (0.168)	0.853 (0.035)	0.114	0.681 (0.087)	0.941 (0.031)	0.005
Mean	0.569 (0.180)	0.842 (0.038)	0.114	0.652 (0.095)	0.934 (0.034)	0.005
Median	0.597 (0.168)	0.853 (0.035)	0.114	0.681 (0.087)	0.941 (0.031)	0.005
Protein						
At mean	1.291 (0.201)	1.085 (0.041)	0.324	1.183 (0.112)	1.132 (0.034)	0.667
At median	1.303 (0.210)	1.089 (0.043)	0.325	1.188 (0.116)	1.135 (0.034)	0.656
Mean	1.351 (0.243)	1.103 (0.050)	0.322	1.222 (0.137)	1.161 (0.041)	0.670
Median	1.303 (0.210)	1.089 (0.043)	0.325	1.188 (0.116)	1.135 (0.034)	0.656
Fat						
At mean	0.545 (0.292)	0.790 (0.085)	0.403	0.794 (0.150)	1.010 (0.053)	0.174
At median	0.496 (0.323)	0.761 (0.096)	0.414	0.779 (0.161)	1.011 (0.057)	0.175
Mean	0.376 (0.400)	0.723 (0.112)	0.386	0.691 (0.225)	1.016 (0.084)	0.176
Median	0.496 (0.323)	0.761 (0.096)	0.414	0.779 (0.161)	1.011 (0.057)	0.175
Carbohydrate						
At mean	0.453 (0.204)	0.835 (0.050)	0.041	0.553 (0.110)	0.886 (0.040)	0.004
At median	0.434 (0.211)	0.832 (0.050)	0.040	0.536 (0.114)	0.883 (0.041)	0.004
Mean	0.071 (0.346)	0.608 (0.118)	0.085	0.460 (0.133)	0.852 (0.052)	0.006
Median	0.434 (0.211)	0.832 (0.050)	0.040	0.536 (0.114)	0.883 (0.041)	0.004
Fiber						
At mean	0.706 (0.241)	0.836 (0.050)	0.574	0.599 (0.131)	0.901 (0.043)	0.028
At median	0.697 (0.248)	0.832 (0.051)	0.571	0.581 (0.137)	0.898 (0.044)	0.028
Mean	0.637 (0.297)	0.795 (0.062)	0.581	0.511 (0.159)	0.879 (0.052)	0.028
Median	0.698 (0.248)	0.832 (0.051)	0.571	0.581 (0.137)	0.898 (0.044)	0.027

Notes: Estimates of income elasticities for food expenditure (top panel) and calorie and nutrient availability (bottom panel). Columns (1)–(3) show estimates using the linearized AIDS across villages, estimated in levels; columns (4)–(6) show reults from the within-village specification estimated in levels. Columns (1) and (4) present experimentally estimated elasticities, and columns (2) and (5) cross-sectional estimates. Columns (3) and (6) report p-values of the difference between the experimental and non-experimental estimates. Each elasticity is evaluated both at the mean and median household budget shares, and we report the mean and the median elasticities using household-specific shares. Standard errors are shown in parentheses.

Table 2: Income elasticity of food expenditures and nutrients availability II

	Within village In first-differences			Across village In levels No price control		
	(1) Experimental (N = 940)	(2) Non-experimental (N = 469)	(3) Difference p-value	(4) Experimental (N = 903)	(5) Non-experimental (N = 432)	(6) Difference p-value
Food expenditure						
Food total						
At mean	0.769 (0.093)	1.069 (0.029)	0.002	0.746 (0.142)	0.914 (0.030)	0.229
At median	0.776 (0.090)	1.067 (0.028)	0.002	0.748 (0.141)	0.914 (0.030)	0.229
Mean	0.751 (0.100)	1.075 (0.031)	0.002	0.730 (0.151)	0.908 (0.032)	0.230
Median	0.777 (0.090)	1.067 (0.028)	0.002	0.748 (0.141)	0.914 (0.030)	0.229
Meat, fish, dairy & egg						
At mean	1.380 (0.268)	1.207 (0.079)	0.534	1.526 (0.323)	1.244 (0.077)	0.402
At median	1.429 (0.303)	1.246 (0.094)	0.563	1.562 (0.345)	1.273 (0.086)	0.422
Mean	1.543 (0.383)	1.296 (0.113)	0.536	1.781 (0.479)	1.367 (0.116)	0.408
Median	1.380 (0.268)	1.212 (0.080)	0.547	1.549 (0.337)	1.255 (0.081)	0.402
Fruit, vegetables & cereals						
At mean	0.568 (0.170)	1.108 (0.049)	0.002	0.613 (0.254)	0.890 (0.052)	0.242
At median	0.543 (0.180)	1.114 (0.051)	0.002	0.602 (0.261)	0.889 (0.052)	0.239
Mean	0.377 (0.246)	1.164 (0.074)	0.002	0.529 (0.309)	0.866 (0.063)	0.243
Median	0.546 (0.179)	1.113 (0.051)	0.002	0.603 (0.260)	0.889 (0.052)	0.239
Other foods						
At mean	0.755 (0.143)	0.920 (0.039)	0.268	0.295 (0.250)	0.678 (0.054)	0.107
At median	0.737 (0.154)	0.913 (0.043)	0.271	0.241 (0.270)	0.659 (0.057)	0.102
Mean	0.670 (0.193)	0.892 (0.053)	0.269	0.081 (0.327)	0.558 (0.074)	0.123
Median	0.739 (0.153)	0.914 (0.042)	0.269	0.241 (0.270)	0.659 (0.057)	0.102
Calorie and nutrient availability						
Calorie						
At mean	0.763 (0.106)	1.072 (0.032)	0.005	0.563 (0.175)	0.848 (0.036)	0.084
At median	0.765 (0.106)	1.072 (0.031)	0.005	0.559 (0.176)	0.847 (0.037)	0.084
Mean	0.731 (0.121)	1.082 (0.036)	0.005	0.528 (0.188)	0.835 (0.039)	0.085
Median	0.766 (0.105)	1.071 (0.031)	0.005	0.559 (0.176)	0.847 (0.037)	0.084
Protein						
At mean	1.037 (0.145)	1.180 (0.046)	0.345	1.299 (0.215)	1.079 (0.040)	0.323
At median	1.037 (0.144)	1.181 (0.046)	0.340	1.311 (0.224)	1.083 (0.042)	0.324
Mean	1.053 (0.208)	1.277 (0.070)	0.308	1.361 (0.260)	1.095 (0.048)	0.322
Median	1.037 (0.144)	1.180 (0.046)	0.342	1.311 (0.224)	1.083 (0.042)	0.324
Fat						
At mean	1.112 (0.186)	0.898 (0.057)	0.271	0.580 (0.282)	0.780 (0.082)	0.478
At median	1.122 (0.204)	0.889 (0.061)	0.274	0.535 (0.312)	0.751 (0.093)	0.491
Mean	1.165 (0.274)	0.854 (0.081)	0.278	0.424 (0.387)	0.711 (0.108)	0.458
Median	1.122 (0.203)	0.890 (0.061)	0.274	0.535 (0.312)	0.751 (0.093)	0.491
Carbohydrate						
At mean	0.573 (0.157)	1.115 (0.040)	0.001	0.408 (0.210)	0.829 (0.052)	0.027
At median	0.569 (0.158)	1.115 (0.040)	0.001	0.387 (0.218)	0.826 (0.053)	0.026
Mean	0.267 (0.269)	1.201 (0.071)	0.001	-0.005 (0.357)	0.595 (0.124)	0.057
Median	0.571 (0.157)	1.112 (0.040)	0.001	0.387 (0.218)	0.826 (0.053)	0.026
Fiber						
At mean	0.610 (0.188)	1.076 (0.055)	0.017	0.614 (0.267)	0.836 (0.051)	0.377
At median	0.570 (0.208)	1.087 (0.063)	0.017	0.602 (0.275)	0.832 (0.053)	0.375
Mean	-0.216 (0.587)	1.355 (0.258)	0.014	0.524 (0.329)	0.794 (0.064)	0.382
Median	0.572 (0.207)	1.086 (0.062)	0.017	0.603 (0.275)	0.832 (0.053)	0.375

Notes: Estimates of income elasticities for food expenditure (top panel) and calorie and nutrient availability (bottom panel). Columns (1)–(3) show estimates using the first-differences specification within villages; columns (4)–(6) show reults from the across-village specification estimated in levels, omitting price controls. Columns (1) and (4) present experimentally estimated elasticities, and columns (2) and (5) cross-sectional estimates. Columns (3) and (6) report p-values of the difference between the experimental and non-experimental estimates. Each elasticity is evaluated both at the mean and median household budget shares, and we report the mean and the median elasticities using household-specific shares. Standard errors are shown in parentheses.

Table 3: Comparison between models

	Across village linearized AIDS vs. Within village	Within village levels vs. Within village first-differences	Across village linearized AIDS vs. Across village no price control
	(1) Difference p-value	(2) Difference p-value	(3) Difference p-value
Food expenditure			
Food total			
At mean	0.922	0.840	0.826
At median	0.923	0.874	0.826
Mean	0.925	0.823	0.826
Median	0.923	0.877	0.826
Meat, fish, dairy & egg			
At mean	0.971	0.739	0.908
At median	0.958	0.784	0.908
Mean	0.966	0.693	0.908
Median	0.965	0.687	0.908
Fruit, vegetables & cereals			
At mean	0.486	0.855	0.595
At median	0.482	0.817	0.595
Mean	0.477	0.627	0.595
Median	0.480	0.824	0.595
Other foods			
At mean	0.250	0.266	0.859
At median	0.250	0.264	0.859
Mean	0.246	0.297	0.859
Median	0.249	0.259	0.859
Calorie and nutrient availability			
Calorie			
At mean	0.604	0.555	0.846
At median	0.599	0.538	0.846
Mean	0.629	0.608	0.846
Median	0.599	0.534	0.846
Protein			
At mean	0.612	0.427	0.977
At median	0.605	0.413	0.977
Mean	0.618	0.497	0.977
Median	0.605	0.412	0.977
Fat			
At mean	0.394	0.184	0.920
At median	0.377	0.186	0.920
Mean	0.442	0.182	0.920
Median	0.377	0.186	0.920
Carbohydrate			
At mean	0.624	0.916	0.857
At median	0.629	0.864	0.857
Mean	0.224	0.520	0.857
Median	0.629	0.856	0.857
Fiber			
At mean	0.640	0.963	0.744
At median	0.624	0.964	0.744
Mean	0.654	0.231	0.744
Median	0.623	0.970	0.744

Notes: Each column shows the p-values of the difference between elasticities generated by the models listed in the column headers. Column (1) shows the difference between the across-village vs. within-village linearized AIDS, both estimated in levels. Column (2) shows the difference between the within-village levels and the within-village first differences specification. Column (3) shows the difference between the across-village specification in levels, estimated with vs. without price controls. Separate p-values are reported for the comparisons at the mean and median household budget shares, and of the mean and the median elasticities using household-specific shares.

Table 4: Comparison of treatment arms: linearized AIDS across villages

	Female vs. male recipient (N = 703)			Monthly vs. lump-sum transfer (N = 775)			Large vs. small transfer (N = 903)		
	(1) Female recipient	(2) Male recipient	(3) Difference p-value	(4) Monthly transfers	(5) Lump-sum transfers	(6) Difference p-value	(7) Large transfers	(8) Small transfers	(9) Difference p-value
Food expenditure									
Food total									
At mean	0.720	0.715	0.981	1.097	0.834	0.330	0.700	0.912	0.214
	(0.204)	(0.194)		(0.288)	(0.192)		(0.105)	(0.199)	
At median	0.718	0.718	0.999	1.098	0.837	0.338	0.704	0.912	0.216
	(0.205)	(0.192)		(0.292)	(0.188)		(0.104)	(0.197)	
Meat, fish, dairy & egg									
At mean	1.614	1.442	0.699	1.995	1.441	0.596	1.338	1.651	0.543
	(0.511)	(0.386)		(0.954)	(0.533)		(0.247)	(0.513)	
At median	1.674	1.471	0.677	2.071	1.462	0.586	1.375	1.690	0.566
	(0.561)	(0.412)		(1.027)	(0.557)		(0.274)	(0.544)	
Fruit, vegetables & cereals									
At mean	0.642	0.496	0.676	1.154	0.969	0.707	0.587	1.016	0.195
	(0.346)	(0.338)		(0.541)	(0.337)		(0.191)	(0.361)	
At median	0.627	0.473	0.673	1.157	0.967	0.706	0.569	1.016	0.198
	(0.361)	(0.353)		(0.553)	(0.364)		(0.200)	(0.380)	
Other foods									
At mean	-0.010	0.509	0.268	0.223	0.048	0.767	0.255	0.073	0.641
	(0.466)	(0.262)		(0.570)	(0.464)		(0.192)	(0.461)	
At median	-0.085	0.458	0.282	0.164	-0.067	0.720	0.186	0.005	0.664
	(0.501)	(0.289)		(0.614)	(0.520)		(0.210)	(0.495)	
Calorie and nutrient availability									
Calorie									
At mean	0.461	0.542	0.797	0.827	0.651	0.567	0.519	0.695	0.395
	(0.298)	(0.267)		(0.342)	(0.264)		(0.137)	(0.264)	
At median	0.446	0.538	0.772	0.830	0.648	0.553	0.513	0.696	0.374
	(0.307)	(0.269)		(0.337)	(0.266)		(0.139)	(0.264)	
Protein									
At mean	1.399	1.046	0.268	1.943	1.466	0.603	1.062	1.652	0.183
	(0.336)	(0.302)		(0.872)	(0.468)		(0.173)	(0.451)	
At median	1.429	1.046	0.252	2.001	1.486	0.596	1.067	1.686	0.184
	(0.361)	(0.304)		(0.926)	(0.488)		(0.186)	(0.475)	
Fat									
At mean	0.642	0.670	0.951	0.316	0.368	0.954	0.577	0.390	0.728
	(0.489)	(0.377)		(0.983)	(0.580)		(0.224)	(0.607)	
At median	0.607	0.658	0.918	0.282	0.297	0.987	0.514	0.353	0.776
	(0.537)	(0.392)		(1.032)	(0.645)		(0.256)	(0.644)	
Carbohydrate									
At mean	0.151	0.384	0.591	0.706	0.522	0.680	0.353	0.550	0.473
	(0.426)	(0.324)		(0.472)	(0.335)		(0.183)	(0.335)	
At median	0.121	0.357	0.598	0.703	0.504	0.661	0.345	0.535	0.501
	(0.442)	(0.338)		(0.477)	(0.348)		(0.185)	(0.345)	
Fiber									
At mean	0.600	0.420	0.652	1.204	0.942	0.672	0.459	1.041	0.141
	(0.394)	(0.412)		(0.646)	(0.419)		(0.227)	(0.422)	
At median	0.589	0.398	0.643	1.206	0.938	0.672	0.449	1.042	0.147
	(0.405)	(0.428)		(0.654)	(0.445)		(0.231)	(0.436)	

Notes: Experimental estimates of income elasticities for food expenditure (top panel) and calorie and nutrient availability (bottom panel). Columns (1)–(3) show separate elasticity estimates for female and male recipient households, and the *p*-values of the difference between them; columns (4)–(6) show analogous estimates for monthly vs. lump-sum transfers, and columns (7)–(9) for large vs. small transfers. Estimates in columns (1)–(9) are obtained using the linearized AIDS across villages, estimated in levels. Each elasticity is evaluated both at the mean and median household budget shares. Standard errors are shown in parentheses.

Appendix

A Additional tables

Table A.1: Variable Descriptions

Outcome variables	
Food total (share)	The share of household expenditure on meat, fish, dairy and egg, fruits, vegetables, pulses, roots and tubers, cereals, fats, spices, sugars, non-alcoholic drinks, and sweets.
Meat, fish, dairy & egg (share)	The share of household expenditure on meat, fish, dairy and egg.
Fruit, vegetables & cereals (share)	The share of household expenditure on fruits, vegetables, pulses, roots and tubers, and cereals.
Other food (share)	The share of household expenditure on fats, spices, sugars, non-alcoholic drinks, and sweets.
Nutrient variables	Each of the nutrient variables is the total nutrient available based on household's monthly expenditures on meat, fish, dairy and egg, fruits, vegetables, pulses, roots and tubers, cereals, fats, spices, sugars, non-alcoholic drinks and sweets.
Other variables	
Total expenditure	The total monthly spending on food and other non-food expenditures. Non-food expenditures include temptation goods, medical care, education expenditures, social expenditures and other non-durable expenditures.
Temptation expenditure	The total monthly spending on alcohol, tobacco and lottery.
Medical expenditure	The total monthly spending on medical care for all household members including consultation fees, medicines, hospital costs, lab test costs, ambulance costs, and related transport.
Education expenditure	The total monthly spending for household members on school/college fees, uniforms, books, and other supplies.
Social expenditure	The total monthly spending by all members of the household on ceremonies, weddings, funerals, dowries / bride prices, charitable donations, village elder fees, and recreation or entertainment.
Other non-durable expenditure	The total monthly spending on airtime, travel, clothing, personal items, household items, firewood, electricity and water.

Table A.2: Sanderson-Windmeijer multivariate test for weak instruments

	Across village In levels Linearized AIDS	Within village In levels	Within village In first-differences	Across village In levels No price control
Log total expenditure				
F-statistic	1.008	1.445	4.677	1.001
Prob > F	0.317	0.230	0.031	0.319
Log total expenditure squared				
F-statistic	1.004	1.438	2.988	0.992
Prob > F	0.318	0.231	0.084	0.321

Notes: The table shows the F-statistics and p-values of the Sanderson-Windmeijer multivariate test for weak instruments. We test the joint significance of included instruments, lnUCT and its square, in the first stage for each of our four specifications. The table shows the conditional first-stage F-statistic and corresponding p-value for each of the endogenous variables, log total expenditure and its square.

Table A.3: Pre-analysis plan discrepancies

Pre-analysis Plan	Modification	Reason
Specifications		
Expenditure elasticities with naïve approach; QUAIDS; nutrient elasticities with naïve approach and price controls	Omitted quadratic term.	LnUCT and its square are weak instruments when we include quadratic term.
Expenditure elasticities with linearized QUAIDS; nutrient elasticities with price controls	Omitted first-difference version.	Space constraints; results similar.
Nutrient elasticities with naïve approach and price controls	Changed from log-log to budget share form.	Equivalent estimation; consistency with expenditure elasticities.
Estimation samples		
Experimental estimation sample	Added specifications across villages in addition to the pre-specified within-village specifications.	No potential for confounds any possible within-village spillover effects.
Non-experimental estimation sample	Added specifications using pure control sample in addition to the pre-specified spillover sample	No potential for confounds any possible within-village spillover effects.
	In within-village specifications, changed from using baseline spillover household data to using endline data	No time confound in comparing to experimental estimation.

Table A.4: Comparison of treatment arms: within village in levels

	Female vs. male recipient (N = 742)			Monthly vs. lump-sum transfer (N = 812)			Large vs. small transfer (N = 940)		
	(1) Female recipient	(2) Male recipient	(3) Difference p-value	(4) Monthly transfers	(5) Lump-sum transfers	(6) Difference p-value	(7) Large transfers	(8) Small transfers	(9) Difference p-value
Food expenditure									
Food total									
At mean	0.778	0.754	0.856	0.998	0.807	0.338	0.711	0.880	0.203
	(0.099)	(0.085)		(0.151)	(0.131)		(0.078)	(0.107)	
At median	0.777	0.758	0.884	0.998	0.811	0.347	0.715	0.881	0.205
	(0.099)	(0.084)		(0.153)	(0.128)		(0.077)	(0.107)	
Meat, fish, dairy & egg									
At mean	1.460	1.312	0.627	1.756	1.472	0.607	1.376	1.610	0.491
	(0.227)	(0.201)		(0.458)	(0.306)		(0.179)	(0.288)	
At median	1.504	1.333	0.603	1.814	1.494	0.587	1.417	1.647	0.529
	(0.249)	(0.214)		(0.493)	(0.320)		(0.199)	(0.306)	
Fruit, vegetables & cereals									
At mean	0.621	0.466	0.505	0.771	0.622	0.674	0.495	0.670	0.462
	(0.161)	(0.167)		(0.281)	(0.218)		(0.136)	(0.195)	
At median	0.605	0.442	0.501	0.767	0.591	0.636	0.473	0.652	0.471
	(0.168)	(0.175)		(0.287)	(0.236)		(0.142)	(0.205)	
Other foods									
At mean	0.414	0.760	0.145	0.742	0.551	0.639	0.436	0.618	0.488
	(0.181)	(0.154)		(0.318)	(0.256)		(0.137)	(0.223)	
At median	0.371	0.735	0.158	0.723	0.497	0.613	0.384	0.590	0.467
	(0.194)	(0.170)		(0.342)	(0.286)		(0.150)	(0.240)	
Calorie and nutrient availability									
Calorie									
At mean	0.629	0.672	0.787	0.864	0.725	0.551	0.576	0.778	0.212
	(0.122)	(0.098)		(0.174)	(0.156)		(0.095)	(0.131)	
At median	0.619	0.670	0.752	0.866	0.723	0.538	0.570	0.779	0.200
	(0.126)	(0.099)		(0.172)	(0.157)		(0.096)	(0.131)	
Protein									
At mean	1.168	0.948	0.258	1.434	1.215	0.548	1.080	1.318	0.264
	(0.147)	(0.127)		(0.306)	(0.197)		(0.107)	(0.185)	
At median	1.181	0.948	0.252	1.460	1.224	0.539	1.085	1.334	0.270
	(0.158)	(0.127)		(0.325)	(0.205)		(0.114)	(0.194)	
Fat									
At mean	0.796	0.885	0.727	0.759	0.872	0.800	0.743	0.843	0.724
	(0.196)	(0.165)		(0.363)	(0.261)		(0.150)	(0.239)	
At median	0.776	0.881	0.703	0.747	0.858	0.817	0.705	0.833	0.676
	(0.215)	(0.171)		(0.381)	(0.291)		(0.173)	(0.254)	
Carbohydrate									
At mean	0.466	0.563	0.621	0.788	0.580	0.471	0.417	0.653	0.255
	(0.152)	(0.124)		(0.212)	(0.196)		(0.124)	(0.167)	
At median	0.447	0.544	0.632	0.786	0.564	0.453	0.409	0.642	0.274
	(0.157)	(0.129)		(0.214)	(0.203)		(0.125)	(0.172)	
Fiber									
At mean	0.560	0.406	0.535	0.823	0.655	0.654	0.425	0.711	0.253
	(0.176)	(0.174)		(0.298)	(0.227)		(0.146)	(0.203)	
At median	0.547	0.383	0.521	0.821	0.633	0.628	0.414	0.701	0.267
	(0.181)	(0.181)		(0.302)	(0.241)		(0.149)	(0.211)	

Notes: Experimental estimates of income elasticities for food expenditure (top panel) and calorie and nutrient availability (bottom panel). Columns (1)–(3) show separate elasticity estimates for female and male recipient households, and the *p*-values of the difference between them; columns (4)–(6) show analogous estimates for monthly vs. lump-sum transfers, and columns (7)–(9) for large vs. small transfers. Estimates in columns (1)–(9) are obtained using the within-village specification estimated in levels. Each elasticity is evaluated both at the mean and median household budget shares. Standard errors are shown in parentheses.

Table A.5: Comparison of treatment arms: within village in first differences

	Female vs. male recipient (N = 742)			Monthly vs. lump-sum transfer (N = 812)			Large vs. small transfer (N = 940)		
	(1) Female recipient	(2) Male recipient	(3) Difference p-value	(4) Monthly transfers	(5) Lump-sum transfers	(6) Difference p-value	(7) Large transfers	(8) Small transfers	(9) Difference p-value
Food expenditure									
Food total									
At mean	0.747	0.584	0.439	0.784	0.867	0.745	0.746	0.824	0.630
	(0.104)	(0.184)		(0.218)	(0.135)		(0.089)	(0.134)	
At median	0.755	0.598	0.442	0.790	0.871	0.745	0.754	0.829	0.629
	(0.101)	(0.178)		(0.211)	(0.131)		(0.086)	(0.129)	
Meat, fish, dairy & egg									
At mean	1.353	1.240	0.831	1.628	1.064	0.456	1.523	1.265	0.590
	(0.317)	(0.426)		(0.642)	(0.400)		(0.278)	(0.388)	
At median	1.397	1.256	0.807	1.675	1.070	0.460	1.545	1.290	0.621
	(0.356)	(0.454)		(0.690)	(0.438)		(0.290)	(0.425)	
Fruit, vegetables & cereals									
At mean	0.524	0.265	0.520	0.438	0.889	0.370	0.498	0.697	0.499
	(0.198)	(0.351)		(0.441)	(0.243)		(0.166)	(0.243)	
At median	0.498	0.208	0.502	0.406	0.883	0.370	0.457	0.684	0.463
	(0.209)	(0.378)		(0.465)	(0.257)		(0.180)	(0.253)	
Other foods									
At mean	0.790	0.725	0.830	0.906	0.704	0.643	0.749	0.776	0.918
	(0.157)	(0.257)		(0.353)	(0.252)		(0.128)	(0.231)	
At median	0.782	0.700	0.801	0.899	0.685	0.644	0.728	0.761	0.906
	(0.163)	(0.281)		(0.377)	(0.269)		(0.139)	(0.247)	
Calorie and nutrient availability									
Calorie									
At mean	0.740	0.571	0.479	0.931	0.892	0.888	0.682	0.900	0.237
	(0.119)	(0.208)		(0.228)	(0.160)		(0.107)	(0.151)	
At median	0.743	0.575	0.479	0.931	0.894	0.892	0.685	0.901	0.240
	(0.118)	(0.206)		(0.227)	(0.157)		(0.105)	(0.150)	
Protein									
At mean	0.958	0.752	0.485	1.303	1.140	0.684	0.943	1.195	0.313
	(0.166)	(0.244)		(0.333)	(0.222)		(0.133)	(0.211)	
At median	0.959	0.761	0.489	1.297	1.142	0.696	0.945	1.194	0.312
	(0.162)	(0.235)		(0.326)	(0.225)		(0.128)	(0.210)	
Fat									
At mean	1.028	0.996	0.928	1.508	1.249	0.658	0.923	1.326	0.243
	(0.207)	(0.283)		(0.503)	(0.304)		(0.167)	(0.303)	
At median	1.031	0.995	0.925	1.559	1.275	0.661	0.917	1.361	0.243
	(0.235)	(0.302)		(0.554)	(0.336)		(0.180)	(0.335)	
Carbohydrate									
At mean	0.629	0.427	0.527	0.700	0.730	0.941	0.497	0.719	0.395
	(0.161)	(0.277)		(0.343)	(0.217)		(0.151)	(0.212)	
At median	0.628	0.408	0.505	0.692	0.732	0.923	0.469	0.719	0.348
	(0.162)	(0.286)		(0.352)	(0.215)		(0.159)	(0.212)	
Fiber									
At mean	0.551	0.297	0.541	0.836	1.003	0.724	0.391	0.919	0.099
	(0.212)	(0.358)		(0.390)	(0.268)		(0.197)	(0.252)	
At median	0.510	0.237	0.546	0.819	1.003	0.721	0.362	0.913	0.106
	(0.231)	(0.388)		(0.430)	(0.284)		(0.206)	(0.272)	

Notes: Experimental estimates of income elasticities for food expenditure (top panel) and calorie and nutrient availability (bottom panel). Columns (1)–(3) show separate elasticity estimates for female and male recipient households, and the *p*-values of the difference between them; columns (4)–(6) show analogous estimates for monthly vs. lump-sum transfers, and columns (7)–(9) for large vs. small transfers. Estimates in columns (1)–(9) are obtained using the within-village specification estimated in first-differences. Each elasticity is evaluated both at the mean and median household budget shares. Standard errors are shown in parentheses.

Table A.6: Comparison of treatment arms: across villages, no price controls

	Female vs. male recipient (N = 703)			Monthly vs. lump-sum transfer (N = 775)			Large vs. small transfer (N = 903)		
	(1) Female recipient	(2) Male recipient	(3) Difference p-value	(4) Monthly transfers	(5) Lump-sum transfers	(6) Difference p-value	(7) Large transfers	(8) Small transfers	(9) Difference p-value
Food expenditure									
Food total									
At mean	0.656	0.657	0.995	1.077	0.758	0.379	0.648	0.880	0.283
	(0.262)	(0.231)		(0.363)	(0.273)		(0.125)	(0.247)	
At median	0.654	0.662	0.976	1.078	0.763	0.387	0.652	0.880	0.286
	(0.264)	(0.228)		(0.367)	(0.267)		(0.124)	(0.246)	
Meat, fish, dairy & egg									
At mean	1.696	1.475	0.659	2.227	1.610	0.665	1.322	1.836	0.438
	(0.587)	(0.418)		(1.258)	(0.756)		(0.269)	(0.672)	
At median	1.764	1.507	0.639	2.322	1.639	0.654	1.358	1.887	0.453
	(0.644)	(0.447)		(1.355)	(0.791)		(0.298)	(0.713)	
Fruit, vegetables & cereals									
At mean	0.498	0.257	0.619	0.924	0.683	0.708	0.465	0.786	0.450
	(0.437)	(0.452)		(0.683)	(0.510)		(0.217)	(0.480)	
At median	0.477	0.223	0.616	0.923	0.657	0.691	0.441	0.775	0.456
	(0.456)	(0.473)		(0.697)	(0.552)		(0.226)	(0.505)	
Other foods									
At mean	-0.072	0.624	0.225	0.286	0.085	0.782	0.291	0.147	0.755
	(0.556)	(0.276)		(0.671)	(0.609)		(0.203)	(0.528)	
At median	-0.152	0.585	0.234	0.231	-0.025	0.747	0.226	0.084	0.775
	(0.598)	(0.305)		(0.722)	(0.683)		(0.222)	(0.566)	
Calorie and nutrient availability									
Calorie									
At mean	0.359	0.518	0.659	0.739	0.538	0.595	0.468	0.626	0.520
	(0.358)	(0.258)		(0.378)	(0.346)		(0.154)	(0.300)	
At median	0.341	0.515	0.638	0.743	0.535	0.581	0.461	0.627	0.498
	(0.368)	(0.259)		(0.373)	(0.348)		(0.156)	(0.300)	
Protein									
At mean	1.422	1.040	0.243	2.012	1.525	0.634	1.047	1.719	0.202
	(0.358)	(0.299)		(0.988)	(0.559)		(0.181)	(0.538)	
At median	1.454	1.041	0.230	2.075	1.548	0.627	1.051	1.756	0.202
	(0.385)	(0.300)		(1.049)	(0.582)		(0.194)	(0.566)	
Fat									
At mean	0.653	0.715	0.895	0.368	0.463	0.918	0.606	0.449	0.770
	(0.495)	(0.361)		(0.989)	(0.590)		(0.218)	(0.601)	
At median	0.619	0.704	0.867	0.336	0.403	0.946	0.548	0.415	0.815
	(0.543)	(0.375)		(1.039)	(0.656)		(0.250)	(0.638)	
Carbohydrate									
At mean	0.026	0.363	0.485	0.579	0.345	0.655	0.298	0.442	0.641
	(0.479)	(0.299)		(0.498)	(0.429)		(0.193)	(0.371)	
At median	-0.008	0.336	0.491	0.574	0.320	0.634	0.289	0.424	0.672
	(0.496)	(0.311)		(0.503)	(0.445)		(0.195)	(0.383)	
Fiber									
At mean	0.488	0.282	0.661	1.078	0.757	0.656	0.383	0.899	0.250
	(0.444)	(0.457)		(0.737)	(0.534)		(0.247)	(0.497)	
At median	0.473	0.254	0.652	1.079	0.742	0.648	0.372	0.895	0.259
	(0.457)	(0.476)		(0.746)	(0.567)		(0.251)	(0.515)	

Notes: Experimental estimates of income elasticities for food expenditure (top panel) and calorie and nutrient availability (bottom panel). Columns (1)–(3) show separate elasticity estimates for female and male recipient households, and the *p*-values of the difference between them; columns (4)–(6) show analogous estimates for monthly vs. lump-sum transfers, and columns (7)–(9) for large vs. small transfers. Estimates in columns (1)–(9) are obtained using the across-village specification estimated in levels. Each elasticity is evaluated both at the mean and median household budget shares. Standard errors are shown in parentheses.

B Testing differences across specifications using nested models

To test the differences between the experimental and non-experimental version of equation 1, we use the following setup, within which both versions are nested:

$$\begin{aligned}\omega_{hv} = \alpha_0 + \alpha_1 s_1 + \beta_1 \ln(z^*_{hv})s_1 + \beta_2 \ln(z^*_{hv})s_2 + \gamma_1(\ln p^f_v - \ln p^n_v)s_1 \\ + \gamma_2(\ln p^f_v - \ln p^n_v)s_2 + \xi_1' X_{hv} s_1 + \xi_2' X_{hv} s_2 + \varepsilon_{hv}\end{aligned} \tag{10}$$

Here, s_1 and s_2 are indicator variables denoting the sample to which each observation belongs. Equation 10 is estimated on a sample that combines the two sample restrictions. One of the two interactions terms between log expenditure and sample indicator is instrumented. As an illustration, let s_1 be an indicator variable for the sample in the experimental version (treatment and pure control households) and s_2 be an indicator variable for the sample in the non-experimental version (pure control households only). The interaction term $\ln(z^*_{hv})s_1$ is then instrumented with $ln(u_{hv})s_1$. β_1 is then equivalent to β in equation 1 estimated experimentally, and β_2 is equivalent to β in equation 1 estimated non-experimentally. A test of equivalence between $\frac{\beta_1}{\omega_{hv}} + 1$ and $\frac{\beta_2}{\omega_{hv}} + 1$ produces the difference p-values we report. We proceed analogously for testing differences between experimental and non-experimental versions of the remaining specifications, and between treatment arms such as male vs. female recipient households.