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ABSTRACT

Estimates of the effect of budget changes on food and nutrition for poor households is an important input to the design of efficient programs to reduce poverty and improve nutrition. Yet, good such estimates are challenging to obtain from observational data alone because of potential endogeneity issues. In this paper we estimate the expenditure elasticitity of food using exogenous variation in budget from two unconditional cash transfer programs in rural Kenya, combined with detailed data on food expenditure, nutrition and prices. Our data allow us to estimate a demand system, using the randomized cash transfers as an instrument for total expenditure, and taking into account potential general equilibrium effects of the program on prices. We find that the average income elasticity of food expenditure is 0.87, and of calorie consumption is 0.67. Although these elasticities are higher than those reported in some of the previous studies, they are significantly lower than those obtained using a non-experimental analysis in our context.

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1. Introduction

The response of households to changes in their available budget in terms of food expenditure and calorie consumption is a topic of great interest to both policymakers and economists. It is a critical component in modeling the consumption and nutrition of households and a central factor in designing effective tax and transfers programs, labor markets, and insurance systems (Deaton, 1992; Dubois et al., 2014; Griffith et al., 2018; Hall and Mishkin, 1982; Jappelli and Pistaferri, 2010). It also informs the design of consumption support policies and redistribution programs in developing countries (Fenn et al., 2015; Luseno et al., 2014; Robertson et al., 2013; Fernald and Hidrobo, 2011; Schady and Paxson, 2007; Aguero et al., 2006; Cunha, 2014; Blattman et al., 2013; Aker, 2015). The elasticity of food expenditure has been at the center of the discussions about potential nutrition based poverty traps (Banerjee and Duflo, 2011; Schofield, 2014).

Despite the importance of the questions, it has been difficult to obtain reliable estimates of how nutrition changes with income, as income is rarely exogenous. Even in studies with plausibly exogenous income variation, it has been difficult to combine this with high-quality data on households' food consumption.

In this study, we combine existing data from two previously conducted and published randomized controlled trials providing unconditional cash transfers to low-income households in rural Kenya (Haushofer and Shapiro, 2016; Haushofer et al., 2020), with the collection of new data on households' food consumption and local price levels. Our new data collection, combined with the existing data, has the following advantages.

First, we have unusually detailed data on food expenditure, including both quantities consumed, prices paid, and places of purchase, which enables us to carefully asses households' food consumption. Second, in addition to our detailed measures of quantities, we collected detailed data on the weights of the units in which foods are consumed, such as "bundles" or "bushels". This allows us to calculate calorie intakes, and more generally nutrient consumption, with some confidence. Third, we collected fine-grained data on local prices. On the one hand, we conducted a market price survey, allowing us to control for the prices that households face in the market; and on the other hand, we collected unit values (i.e., the prices that households pay for what they consume), allowing us to assess whether richer households buy higher-quality varieties of the same good.

This setting allows us to use the randomly allocated cash transfers to estimate the causal effect of increased budget on calories and food expenditure. To go beyond the reduced form effects, we estimate a demand system, i.e. we control for prices at the same time as estimating the effect of budget changes on food consumption. We instrument total expenditure with the randomly assigned unconditional cash transfer. Through this demand system estimation, we obtain estimates of the expenditure elasticity for food and calories, while taking into account potential general equilibrium effects of the cash transfers on prices.

Our results indicate an expenditure elasticity of around 0.87 for food expenditure, and an expenditure elasticity of around 0.67 for calorie availability.[1] This is higher than the majority of previous studies that report these elasticities: In a recent meta-analysis covering 66 studies in 48 African countries, Colen et al. (2018) find an average expenditure elasticity of 0.61 and an average calorie elasticity of 0.42. Our estimated elasticities are remarkably similar across the different specifications, ranging from 0.80 to 0.87 for expenditure, and 0.54 to 0.67 for calories. We do not find meaningful differences between elasticities across i) lump-sum and monthly payments, ii) large ($2,956 in 2017-PPP) and small ($782) transfers, and iii) female and male recipients, although a caveat is that for the latter we only have power to detect relatively large differences.

We establish several additional results. First, we investigate whether elasticities derived from observational data are good approximations of elasticities derived from exogenous variation in expenditure in our specific setting. We do this by restricting the sample to control households and estimating observational elasticities without making use of the random assignment of cash transfers. We have a priori reason to doubt such "non-experimental" elasticities, as households with different resources may have different tastes, different opportunities, and face different prices, which complicates interpretation. Indeed, we find that the observational estimates systematically overestimate elasticities: the food expenditure elasticities are usually about 10-12 percentage points larger than our experimental estimates, and the difference is even greater for the calorie

[1] Note that as we do not observe how much the households members eat, we can only report the calorie content of their expenditure (including consumption out of self-production and in-kind), i.e., available calories.

elasticity. This is an important result, as most of the previous literature is based on observational data alone, exploring either the cross-sectional dimension (Deaton and Subramanian, 1996; Jappelli and Pistaferri, 2010; Skoufias, 2003), or the time dimension (Dynarski et al., 1997; Krueger and Perri, 2012, 2006; Browning and Crossley, 2001; Hall and Mishkin, 1982). Our findings suggest that such estimates should be interpreted and used with caution in settings like ours – this includes the above-mentioned core studies, and also previous studies by a subset of the authors (Almås, 2012; Almås et al., 2013; Almås and Kjelsrud, 2017).

Second, we show that estimates that control for local prices are very similar to those that do not, suggesting that lack of price data may not be a significant problem when estimating elasticities in settings such as ours. Relatedly, we can compare the results we obtain when using the prices households face in the market to those we obtain when using unit values. Very few datasets provide this opportunity (see e.g. Gibson and Kim, 2019). Our result suggests that unit values can sensibly be used to estimate expenditure elasticities, at least in our setting.

Third, we investigate the importance of spillovers to non-recipient households. Our main specification leverages random variation in budget *across* villages, which allows us to obtain estimates free of within-village spillovers. When contrasting these estimates with estimates derived from within-village variation alone, we find that the two sets of estimates are almost identical, which suggests that spillover effects are small in our setting, and that elasticities can sensibly be measured within villages. It is important to note, however, that only a small share of households in each of our study villages received transfers; much larger spillover and general equilibrium effects have been found in studies where a larger fraction of households receive transfers (Egger et al., 2022).

We are not aware of previous studies that use randomized unconditional cash transfers to estimate expenditure elasticities of food expenditure and calories, while controlling for local prices. However, a few recent studies come close. Angelucci and Attanasio (2013) and Attanasio et al. (2012) study the effect of conditional cash transfers (CCT) programs in Mexico (PROGRESA/Oportunidades) and Colombia (Familias en Acción) on the food share of expenditures. These studies differ from ours in that the transfers are conditional and made only to women, price data are either unavailable or incomplete, and nutrient elasticities cannot be studied. Attanasio and Lechene (2010) and Hoddinott and Skoufias (2004) use a similar approach for PROGRESA, but have access to price data. Again, the conditionality and the targeting of women makes these studies different from ours. Armand et al. (2020) compare the effects of giving conditional cash transfers to men or women in Macedonia. As the transfers are again conditional, the approach is different from ours. Almås et al. (2020) compare the effect of giving unconditional cash transfers to either men or women in India, but do not have a pure control group. Schady and Rosero (2008) estimate the effect of monthly unconditional cash transfers to women in Ecuador on the food budget share, but as in most of the aforementioned studies, the transfers are made only to women, price data are not available, and nutrient elasticities are not studied. Gangopadhyay et al. (2012) show positive reduced-form impacts on calorie consumption of an unconditional cash transfer program in India, but do not explicitly estimate elasticities. Similarly, Aizawa (2020) shows positive reduced-form effects on food consumption of unconditional cash transfers through the Hunger Safety Net Programme in Kenya without estimating elasticities. In sum, therefore, estimates of food expenditure and calorie elasticities based on unconditional cash transfers are scarce, especially with price controls.

The remainder of the paper is organized as follows. In Section 2, we give an overview of the interventions and the study designs. In Section 3, we describe our data and key variables. We present our econometric framework in Section 4 and discuss our results in Section 5 and 6. We conclude in Section 7.

2. Study design and data collection

Our analysis is based on two existing field experiments in rural Kenya. In this paper, we report new analyses based on combining these existing data with new data on detailed food expenditure, nutrients and prices collected for this project. In this section we describe the existing interventions and discuss the new data. Figure 1 and 2 summarize the study design of the two experiments, and Table 1 provides details on the data collection.

2.1 The Nakuru experiment

The main intervention we make use of took place in Nakuru County from May 2017 to January 2018.[2] The experiment consisted of two treatments: an unconditional cash transfer and a psychotherapy program. We focus solely on the cash transfers in this paper and refer to Haushofer et al. (2020) for a full description of the experiment and other results.

The cash transfers provided KES 50,000 to households, which was equivalent to about $1,076 PPP at the time of study. Before the money was sent out, recipients were contacted and told that the transfer was entirely unconditional. The transfers were then sent to participants through the mobile money service *M-Pesa*, operated by the mobile provider *Safaricom*. Participants who did not already have a personal *M-Pesa* account (the majority did) were offered a cell phone at prevailing retail prices (KES 1,600) and guided on how to set up the account.

Villages and participants were sampled as described below and as illustrated in Figure 1. The survey team first obtained a list of villages in the region, and from this list, 233 villages were randomly chosen. Each village was then randomized into one of four groups: 60 villages were selected to receive the cash treatment only, 60 villages were selected to serve as a pure control group, while the remaining 113 villages were selected to receive either the psychotherapy treatment or the psychotherapy treatment *and* the cash transfer. We only make use of the pure cash treatment villages and the pure control villages in this paper. The 60 cash treatment villages were further cross-randomized into two groups: half of the villages were assigned to receive the transfer as a lump-sum, while the other half was assigned to receive the transfer in five weekly installments.

The experiment was targeted to poor households and eligibility was defined as having a house without brick, stone, or metal walls. The aim was to survey 30 such households in each village, selected as follows. First, ten households were randomly selected from a list of all eligible households. After this, the field officers selected the two (eligible) households that were geographically closest to each of the ten households. This thus gave ten clusters of three households in each

[2]The experiment was organized by the Busara Center for Behavioral Economics. The intervention had to take one month's break in August 2017 due to the Kenyan national election, and two weeks' break in October/November 2017 due to the re-election.

village.[3] Within each cluster, one household was randomly selected to receive the cash transfer, while two households were selected for a within-village control group.

The original data collection effort included two rounds of household surveys: a baseline survey collected from November 2016 to March 2017, and an endline survey collected between August and October 2018, about a year after the intervention. We refer to these surveys as "Nakuru Baseline" and "Nakuru Endline I", respectively. For the villages relevant for our study, the baseline survey gathered data from 540 treatment households, 1,077 control households from the treatment villages, and 1,545 control households from the control villages. The same set of households was targeted for Nakuru Endline I. One person was chosen as respondent within each household. This was usually the household head. If the household head was not available, the survey team chose whoever greeted them at the door.

Nakuru Baseline included only two questions related to household consumption: one question on overall food expenditure, and one question on total expenditure. The Nakuru Endline I survey contained a consumption questionnaire. In this questionnaire, respondents were asked about their expenditure on different food and non-food *categories* (e.g. "fruits", "clothing and shoes"; see Table A1 and Appendix I for details). The survey did not collect data on quantities consumed or local price levels.

Because of these limitations, we organized a new household survey for this paper, implemented over the period August to October 2019. We refer to this survey as "Nakuru Endline II". For this survey, we targeted all treatment households and a random set of 1,000 households from the control villages. Thus, we did not sample control households in treatment villages. We included a detailed consumption questionnaire, in which respondents were asked about expenditure on 75 food items (e.g. rice, carrots, bananas; details in Appendix I); 20 non-food items (e.g. soda, beer, cigarettes); and 10 non-food consumption categories (e.g. "clothing and shoes", "personal items", "medical expenses"). Importantly, we also asked the respondents about the *quantity* consumed

[3] Before randomization of households, ten percent of all respondents, stratified by villages, were removed to act as the treatment group in a study on *M-Pesa*, and these removed households were selected from the pool of households without existing *M-Pesa* accounts. This removal implies that such households are under-represented in our current study, and because of this, we overweight them in our later analysis, but our estimates are almost identical without this ex-post weighting.

of each food item; where they had bought each item; in which units (e.g. a bundle or a kg); and whether they had consumed any food out of home production, including agricultural and livestock production. The information on quantities allows us to impute the nutrient content of households' food expenditure directly, without a detour via quantities and prices (see Section 3.3); and to calculate unit values (as expenditure over quantities, see Section 3.2). For home production, we have information on the quantity consumed, and the self-reported value associated with this consumption, for 27 food items.

We organized two additional surveys to obtain local prices in Nakuru. The first survey, which we call "Nakuru Marketplace Survey I", was conducted in October 2019 (i.e., coinciding with Nakuru Endline II). In this survey, we interviewed 1,133 vendors at 238 local marketplaces to obtain data on food prices. We selected marketplaces based on the information on *where* the study participants had made their food purchases (taken from the Nakuru Endline II household survey).[4] We sent our enumerators to these specific markets and asked them to target five vendors in each market to collect the prices of the same products, in the same quantities, and in the same units as reported in the household survey. To give an example, if a respondent reported a purchase of ten bananas at market X, our enumerators went to market X and asked five vendors about the price of ten bananas. If another respondent reported a purchase of five bananas at the same market, enumerators separately asked the vendors for the price of five bananas as well (to capture any possible quantity discounts or surcharges). In total, we collected data on 49 different food items in this way. We also instructed enumerators to purchase these items and weigh them; we can thus compute the average weight of each product-unit (e.g. a bundle of bananas).

The Nakuru Marketplace Survey I did not gather any non-food prices other than for tobacco and drinks. Because of this, we conducted a second price survey in September 2020, which we term "Nakuru Marketplace Survey II". In this survey, all participants in the Nakuru Endline II were called and asked what they typical pay for eleven different non-food goods and services, at the time of the phone survey and one year before (i.e., at the time of Nakuru Endline II). We picked representative goods and services within each of the non-food consumption categories (except tobacco and drinks). The specific items are listed in Appendix I. This survey allows us to

[4] On average, respondents in each village reported three different marketplaces.

complete the Nakuru Marketplace Survey I data with prices of non-food items *at the same time* as the prices of foods were collected, although it should be noted that some recall bias may be present. However, the average price increase implied by the current (2020) and recalled (2019) prices is 4.4 percent, which is in line with inflation in Kenya (5.2 percent and 5.4 percent in 2019 and 2020, respectively).

2.2 The Rarieda experiment

The other field experiment was conducted in Rarieda district from 2011 to 2013, in cooperation with *GiveDirectly, Inc.* (*GD*), an international NGO that provides unconditional cash transfers to poor households in low-income countries. The intervention and study design are described in detail in Haushofer and Shapiro (2016). We summarize the design briefly below.

The intervention consisted of unconditional cash transfers of either KES 25,200 (equivalent to $782 in 2017-PPP) or KES 95,200 (equivalent to $2,956 in 2017-PPP).[5] The average transfer was KES 42,104 ($1,307 in 2017-PPP). As in Nakuru, participants were informed that the transfer was completely unconditional and one-time. Recipients were provided with a *Safaricom* SIM card and had to register it for *M-Pesa* in the name of the designated transfer recipient.

At the time of the field experiment, *GD* used an eligibility criterion for their cash transfer program that consisted of whether or not households were living in a house with a thatched (rather than metal) roof. The same criterion was used to select participants for the experiment. The selection was done as follows (see illustration in Figure 2). The first step was to identify the 120 villages with the highest proportion of thatched roofs in Rarieda. From this pool of villages, 60 were randomly selected for the treatment group and 60 for the control group. Within the treatment villages, half of all eligible households (i.e., those with a thatched roof) were randomly chosen to be treatment households, while the other half served as control households. Treatment households were further selected into different treatment arms, randomizing i) whether the transfer went to the husband or the wife (in dual-headed households); ii) whether households received a "large"

[5] In PPP terms at the time of study, this corresponds to $404 and $1,525. To facilitate the comparison of our two interventions, we primarily refer to the 2017-PPPs in this paper.

transfer of KES 95,200 per household or a "small" transfer of KES 25,200 per household; and iii) whether households received the transfer as a lump-sum amount or as a series of nine monthly installments.

The original data collection involved a baseline survey before the randomization and an endline survey about a year after the transfers began. The baseline survey, which we term "Rarieda Baseline", included 503 treatment households and 505 control households from the treatment villages. Households from the control villages were not sampled for this survey. The endline survey, which we call "Rarieda Endline", targeted all participants from the baseline, and in addition 432 eligible households from the 60 control villages selected at baseline. Because these households were selected into the sample just before the endline, the thatched-roof criterion was applied to them one year later than to households in the treatment villages. Haushofer and Shapiro (2016) show that the potential bias from this was negligible in practice. We therefore do not control for it further here.

The Rarieda Baseline and Rarieda Endline surveys included the same consumption module.[6] Hence, in contrast to in Nakuru, we have comparable estimates of household expenditure both before and after the intervention. Respondents were asked about their expenditure on 15 food categories (e.g. "cereals", "vegetables", "fruits"; details in Appendix I) and 24 non-food categories (e.g. "personal items", "household items", "medical expenses"). The consumption questionnaire was thus about equally detailed as Nakuru Endline I, but much less detailed than Nakuru Endline II (see Table 1). The original data collection in Rarieda also included a price survey, which we call "Rarieda Price Survey", and which was conducted at the time of the Rarieda Endline survey. This survey gathered prices for 28 common food items (e.g. onions, oranges, potatoes) and 4 common non-food items (firewood, haircut, paraffin and soap bar) from a random subset of 397 study participants, spread over all study villages (see Appendix I for a full list of items). Hence, the Rarieda Price Survey was much smaller in scope than the Nakuru Marketplace Survey I, and the price information was gathered directly from the study participants as opposed to marketplace

[6] The baseline and endline each included two modules: a household module, which collected information about household characteristics, including expenditure; and an individual module, which collected information about psychological wellbeing, intra-household bargaining, and domestic violence. We only make use of the household module in this paper.

vendors like in Nakuru. The price data from Rarieda is thus not fully de-coupled from households' expenditure decisions (richer households may choose to buy more expensive items). Note, however, that participation in the Rarieda Price Survey was limited to within-village control households and pure control households, which should limit the influence of the treatment on the reported prices.

The main limitation of the original data from Rarieda, however, is that we have *expenditure* data for each food item, but no direct information on *quantities* consumed. Without this information, we cannot easily estimate nutrient elasticities (e.g. for calories). To remedy this problem, we calculate implicit quantities, dividing households' reported expenditure on the different food categories by the price of the same categories. We then use these implicit quantities to calculate nutrient availability (see Section 3.3 for a detailed description of this procedure). To enable this calculation, however, it was necessary to conduct two additional surveys. First, the Rarieda Price Survey did not collect prices for all food categories. In the first of the additional surveys, which we term "Rarieda Marketplace Survey I" and which was collected in January 2016, we therefore filled these gaps. In particular, we obtained price data for 31 food items belonging to the consumption categories *non-alcoholic drinks*, *sweets*, and *spices*, from five different markets in the city of Rongo in western Kenya (the same part of the country as Rarieda).

Second, an additional problem was that some prices in the Rarieda Price Survey were in "local" units, which are easy for respondents to understand, but that 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. In the second additional survey, which we call "Rarieda Marketplace Survey II" and which was conducted in January 2016, we therefore obtained estimates of the weight corresponding to each of the "local" units. We did this by visiting five different markets in Rongo and by buying five units of the items in question at each market. From this we obtained the average weight of 31 item-units.

2.3 Integrity of the experiments

All surveys had low attrition. In Appendix C, we find no evidence of differential attrition across treatment and control groups. We also report estimates of baseline balance and find no significant differences in observables between the treatment and the control groups at baseline.

We wrote a new pre-analysis plan (PAP) specifically for the analyses presented in this paper, which is published and time-stamped.[7] Note that this PAP was written before we planned to collect the Nakuru Endline II household survey and marketplace surveys. The main motivation for collecting this data was that we wanted more detailed expenditure and price data, as outlined above. We did not write a new PAP for these additional surveys, as we felt sufficiently constrained by the original document. The PAP contains a detailed description of the set of specifications we planned to run. We broadly follow this description, but we make some minor adjustments to ensure consistency across the different surveys. All these deviations are listed in Table A1 in Appendix B. We also highlight the analyses that are not in the PAP in the main text.

3. Key variables and summary statistics

In this section we describe our key variables and provide summary statistics.

3.1 Consumption expenditure

We calculate monthly household expenditure as the sum of households' reported expenditure on the various food and non-food items.[8] As discussed above, Nakuru Endline II contains a large set of consumption items (e.g. rice, carrots, bananas), while the other household surveys are limited to broader consumption categories (e.g. cereals, vegetables, fruits). For all surveys, we use a weekly recall period for food and a monthly recall period for most non-food items. For some non-food items with larger and less frequent expenditures, such as ceremonies and weddings, we use a

[7]https://www.socialscienceregistry.org/docs/analysisplan/625

[8]We do not make use of the expenditure numbers from the Nakuru baseline survey since they are based on *one* survey question only. Thus, they are not comparable to the expenditures estimates in the other surveys.

yearly recall period. We convert all values to monthly values.[9] We add consumption out of home production, with either a monthly or a yearly recall period, again scaled to monthly equivalents.

We convert the expenditure data to USD 2017-PPP. We do this by deflating the expenditure numbers using a nationwide consumer price index from the *Kenya National Bureau of Statistics*. Note that this adjustment only affects the estimated elasticities in one specification, in which we pool the different household surveys; for the survey-specific elasticities, scaling by a constant does not matter.

3.2 Village prices

We have detailed price data for Nakuru Endline II, and more limited price data for Rarieda Endline. We use this to calculate village-level food and non-food price indices in three steps.

The Nakuru Marketplace Survey I collected prices for the same food items in different units (e.g. "bundles of bananas" and "single banana"), depending on the reported purchases in the Nakuru Endline II household survey. As a first step, we convert these food price observations into common units. We do this based on the *weight* of the different food products-units. To give an example, we convert the price of "bundles of bananas" to the implicit price of "kilos of bananas" based on the measured weight of a bundle of bananas in the Nakuru Marketplace Survey I. In the Rarieda Price Survey, these adjustments are not necessary, as all price observations for a particular food item refer to the same unit; for example, respondents were always asked for the price of a kilo of bananas.[10]

In the second step, we calculate village-level prices for each consumption item (or category). It may appear that this is throwing away information, and that instead of village-level prices, one should use household-specific prices (unit values). However, note that the goal here is to de-couple the prices from the household's decisions: we want an estimate of the prices households face "in

[9] We divide the weekly data by 7 and multiply by 30.5 to convert to monthly. The yearly data are divided by 365 and multiplied by 30.5. See Appendix I for an exact list of consumption items and recall periods in each survey.

[10] Note that we do not make use of the Rarieda Marketplace Survey I when constructing the village prices, as this data was collected in the city of Rongo. Hence, we do not have variation at the level of villages. This data is instead used to compute proxies for quantities consumed of different food items, as explained in Section 3.3.

general" when they go shopping, rather than the prices at which they decide to buy (which could be influenced by treatment).

In Nakuru, we calculate the average price for each food product (converted to a common unit in step 1) at each marketplace. We then merge this with the Nakuru Endline II household survey and compute the median price for each product in each village, i.e., the median price among all households in the village that report consumption of the particular food item.

For non-food in Nakuru, and food and non-food in Rarieda, we have price data at the level of items, but corresponding expenditure data only at the level of consumption categories. We therefore aggregate the item prices to consumption categories. One possible way of doing this is to just calculate the average within each category and village. With missing price observations, however, such averages would be skewed towards the prices of the non-missing items. For instance, if prices of luxury goods are missing in a poor village, the (measured) average price level would be artificially low as compared to other villages. To handle such potential biases, we instead aggregate using the Country-Dummy-Product Method (Summers, 1973), which is a standard method for filling gaps in price data through a simple regression analysis.[11] This analysis is carried out within each consumption category by regressing the logarithm of the item prices on a set of item and village dummies, in which the item dummies take out the price level of each specific item. The overall village price levels of the consumption categories can then be purged from the village dummies. Thus, in contrast to the simple average, the method handles potentially systematic missing observations.

In the third step, we aggregate the set of village-level prices from step 2 to overall food and non-food price indices using the Weighted-Country-Dummy-Product Method. This method, which is attributed to Rao (1990, 2005), is similar to the one described above, except that the regressions are weighted to reflect the importance of each observation. In our case, we use weights equal to the average village-level budget share of each consumption category. The resulting price measures are very closely related to those derived by traditional index number formulae,[12] and just as in

[11]For example, the method has been used by the International Comparison Program (ICP) to construct "basic headings" since the start of the program.

[12]For instance, in the case of two regions, the price index can be shown to be a second order local approximation to the Törnqvist index (see Diewert, 2005).

other price index calculations, the weights ensure that goods with a larger budget share count more in the calculation.

In Appendix F, we present several alternative price indices. In particular, we apply the regression-based method of Deaton et al. (2004) to account for potential quality effects in the price data. This method is based on estimating the expenditure elasticity of quality, as measured by what households of different expenditure levels pay for similar items: for instance, if rich households buy higher-quality meat, they may spend more per unit, but this increased unit value should not be interpreted as a higher price of meat. To correct for it, the method uses the empirically observed relationship between expenditure and unit values to remove price variation that can be attributed to expenditure level differences across areas. Our non-food prices, in particular, cover products that are likely to be heterogeneous in quality (e.g. medical expenses, school fees), as they are self-reported and collected directly from our study participants. This may in theory be problematic, as richer households typically purchase consumption goods of higher quality than poorer households. As shown in the appendix, however, our main findings do not change when we account for such potential quality effects. In Appendix F, we also compute *unit value* indices by dividing households' reported expenditure on different consumption items by their reported quantities purchased, and show that we obtain similar elasticities as our main estimates when using unit values instead of village-level prices to compute elasticities.

3.3 Calories and nutrient availability

We measure households' calorie and nutrient availability based on detailed nutritional composition tables, extracted from the West Africa Food Composition Table 2019.[13] This is straightforward in Nakuru Endline II, as we have data on quantities and weights for both purchased and home-produced food. Since we collected original data on the weight of each product-unit, we can directly align the reported units in the household survey with those in the nutritional composition tables. We can therefore obtain estimates of households' intake of calories, proteins, fat, carbohydrates and fiber by multiplying the reported quantities with the nutritional composition of each food

[13]The database is available at https://www.fao.org/3/ca7779b/CA7779B.PDF

item. This direct pathway from our survey data to nutrient intake is a core advantage of this paper.

In the other household surveys, we do not have information on either expenditure or quantities consumed of the different food items; we only have expenditure data at the level of food categories. Hence, we are unable to directly compute estimates of households' intake of nutrients. However, the new data we gathered in the Rarieda Marketplace Surveys I and II allow us to calculate implicit estimates for households in the Rarieda Endline. We do this as follows. We first convert our price data from the units in which they were collected in the Rarieda Price Survey and the Rarieda Marketplace Survey I into prices *per gram*. To do this, we make use of the weights of the particular item-units, which we collected in the Rarieda Marketplace Survey II (e.g. we convert the price of one avocado into the price per gram of avocado). We then calculate the (unweighted) average price per gram within each food category. As a next step, we divide the expenditure numbers in the Rarieda Endline by these average prices. This gives us a proxy for the quantity consumed (in grams) within each food category, for each household. Finally, we derive estimates of households' nutritional intake by multiplying this quantity with the (unweighted) average nutritional content per gram for the same food category, calculated from the nutritional composition table. In short, the approach therefore imputes a fixed amount of nutrients per dollar spent within each food category. For home-produced food, the Rarieda Endline does have information on quantities consumed at the level of consumption items. For this particular type of consumption, we therefore impute nutrient intake directly, as described above for the Nakuru Endline II.

3.4 Summary statistics and treatment effects

The odd columns of Table 2 present control group means of our key variables. Panel A displays household-level variables. Average household expenditure is KES 11,038 in Rarieda Endline and KES 9,902 and KES 9,691 in Nakuru Endline I and II, respectively. Converted to USD 2017-PPP, these numbers correspond to $328 in Rarieda, and $202 and $187 in Nakuru. Thus, the sampled households in Nakuru are, on average, poorer than those in Rarieda. Given a mean household size of 4.9 (in all samples), the expenditure numbers correspond to about $2.2 PPP per person per day in Rarieda, and about \$1.4 PPP and \$1.3 PPP in Nakuru. The average consumption level in Nakuru is hence below the World Bank's definition of extreme poverty (\$2.15 per person per day). The budget shares spent on food also suggest that we sampled poor households: The average food share is above 70 percent in both surveys from Nakuru, and 65.8 percent in Rarieda. The average calorie availability per day is about 9,400 in Nakuru Endline II (corresponding to roughly 2,000 calories per person), and somewhat lower in Rarieda Endline.[14] About 12-13 per cent of households' food expenditure in Rarieda Endline and Nakuru Endline II comes from home-produced food. This share is somewhat higher in Nakuru Endline I (20 per cent). The share of calories from home-produced food is around 22 per cent in both Nakuru and Rareida.

Panel B displays the village-level price indices. These indices are normalized to unity within each survey, and thus, the overall means within each survey are 1.00 mechanically. The price levels vary quite substantially, with standard deviations ranging from 0.11 to 0.22 for the control villages.

The even columns present treatment effects for the same set of variables. Given that the allocation of the cash transfers was randomized, we can estimate these effects with a minimum set of assumptions. Many of these treatment effects for Nakuru Endline I and Rarieda Endline are discussed in Haushofer and Shapiro (2016) and Haushofer et al. (2020), but not the effects on food share and calories (and not the effects in Nakuru Endline II). The cash transfers have a positive and significant impact on both total expenditure and food expenditure in all three endline surveys. Consistent with the increase in total expenditure and a downward sloping Engel curve for food, we find a negative impact on the budget share spent on food items (insignificant in Nakuru Endline II). The results also show that the recipients of the transfers have higher calorie availability at endline. Finally, we find no significant treatment effects on either home-production or village-level consumption prices.

[14] We top-code the nutrient numbers at the 99th percentile in each data set.

4. Econometric framework

The treatment effects on household expenditure, discussed above, suggest an expenditure elasticity of food expenditure close to but below one. However, to properly estimate elasticities we need to study how endline expenditure is affected by the unconditional cash transfers, and how this again affects endline food budget shares and calories. In this section, we outline an econometric framework for this using the unconditional cash transfers as an instrument for total expenditure. Since we have detailed price data we can also account village-level price differences. For each specification, we also propose a "non-experimental" equivalent.

The instrumental variable approach rests on two key assumptions. First, the first stage needs to be strong enough, i.e., the cash transfers must affect total consumption at endline. Second, the exclusion restriction needs to hold, i.e., the transfer needs to affect food expenditure only through the treated participants being richer at endline and not through any other effects of the cash transfers. We return to the plausibility of the two assumptions in our setting in Section 5.

4.1 Main specifications: Across village variation with price controls

We use the Almost Ideal Demand System (Deaton and Muellbauer, 1980) to estimate food expenditure elasticities. This simple demand system is attractive because it has a structure that is consistent with economic theory; specifically, it is consistent with utility maximization under a budget constraint. Also, it allows for prices to play a role, and hence, it can account for differences in price levels across villages in our sample.

We use the linearized version of this system and estimate the following specification:

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

where ω_{hv} is the budget share of interest (primarily food in our case) for household h in village v. z_{hv} denotes monthly expenditure, and $\ln z^*_{hv} = \ln z_{hv} - \ln a^*(\mathbf{p_v})$, where $\ln a^*(\mathbf{p_v})$ is a Stone price index (see Stone, 1953), defined as: $\ln a^*(\mathbf{p_v}) = \overline{\omega^f} lnp^f_v + (1 - \overline{\omega^f}) lnp^n_v$. p_v denotes our village price indices, where the superscripts f and n refer to food and non-food prices, respectively, and $\overline{\omega^f}$ is the average budget share for food. z^*_{hv} is thus a measure of monthly expenditure, deflated by prevailing village price levels. Finally, X_{hv} is a vector of controls including the number of adults and the number of children at baseline, and ε_{hv} is an idiosyncratic error term.[15]

By differentiating (1), we can obtain the food expenditure elasticity from the following expression:

$$e_{hv} = \frac{\beta}{\omega_{hv}} + 1. \tag{2}$$

The elasticity e will typically vary across households since the budget shares differ. In the main text, we focus on elasticities evaluated at the mean household budget shares, ω.

We estimate elasticities for calorie and nutrient availability based on the following specification:

$$\ln c_{hv} = \alpha + \beta \ln z^*_{hv} + \gamma(\ln p^f_v - \ln p^n_v) + \xi' \mathbf{X_{hv}} + \varepsilon_{hv}, \tag{3}$$

where c_{hv} is the availability of calories or nutrients for household h in village v. Since we use log of the level of calories (or nutrients) on the left-hand side in this specification, as opposed to budget shares, the elasticity is simply equal to the β-coefficient.

We use the receipt of the cash transfers (amounts) as an instrument for expenditure to identify the causal effect of expenditure in (1) and (3).[16] In our main specifications, we exclude control households located in the treatment villages (because they may be affected by spillovers). These

[15]Note that in the pre-analysis plan we suggested to show results for the QUAIDS system (Banks et al., 1997) in addition to the AIDS system. We pre-specified that we would use log transfers and squared log transfers as instruments for this system. For the Nakuru Endline I and II, we are unable to estimate the quadratic demand system, as we do not have variation in the *size* of the transfer. For the Rarieda Endline, it turned out during the analysis that we do not have a strong enough first stage to estimate the quadratic system. For these reasons, we only report results for the linearized AIDS. See Table A1 in Appendix B for other discrepancies between the pre-analysis plan and the current specifications.

[16]We transform the cash amounts using the inverse hyperbolic sine to deal with zeroes (Burbidge et al., 1988; MacKinnon and Magee, 1990; Pence, 2006). A recent paper by Chen and Roth (2022) highlights a number of concerns with such transformations. In our case, however, the cash transfer variable is the only transformed variable that includes zeros and we are therefore not too worried about these issues. In fact, for the Nakuru experiment the transformation has *no* impact on our estimates, as the transfer is of identical magnitude for every recipient. For the Rarieda experiment, and in the pooled regression (see Section 4.3), the transfers vary somewhat in magnitude, but we obtain very similar estimates if we instead of using the inverse hyperbolic sine construct the instrument as the transfer in levels, or as the log of the transfer plus one.

households are instead included in one of the alternative specifications (see Section 4.3). Standard errors are clustered at the village level, since this is the level at which the randomization took place (given that we exclude controls households in treatment villages).

We compare the results of this experimental analysis to a standard cross-sectional analysis by estimating similar specifications without instrumenting for household expenditure. When doing this, we restrict the sample to households from the control villages to avoid using any of the experimentally induced variation. We then test the differences between the experimental and the non-experimental elasticities by estimating nested models (see Appendix A for details).

We estimate several alternative specifications, as outlined in the remainder of this section. The motivation for this is twofold: to increase the precision of our estimates, and to explore their robustness.

4.2 Across-village variation without price controls

As a first alternative setup, we estimate our baseline specifications without any of the price controls. This regression can thus be written as:

$$y_{hv} = \alpha + \beta \ln z_{hv} + \xi' \mathbf{X_{hv}} + \varepsilon_{hv}, \tag{4}$$

where y_{hv} denotes either the budget share, ω_{hv}, or the logarithm of calories, $\ln c_{hv}$. We derive elasticities in the same way as for our baseline specification. To obtain more statistical power, we also run a version of (4) where we pool all household surveys, i.e., the Nakuru Endline I and I, and the Rarieda Endline.

4.3 Within-village specifications

In Rarieda Endline and Nakuru Endline I, we have data on control households in treatment villages. We use these data to estimate specifications comparing treatment and control households within villages, using village fixed effects. This setup has the potential to be more highly powered, as it absorbs all time-invariant village characteristics (including price levels). The cost of the approach is that within-village spillover effects are baked into the estimates.

We restrict the sample to treatment villages and estimate two different specifications. The first specification, which resembles our main specification, can be written as follows:

$$y_{hv} = \alpha_v + \beta \ln z_{hv} + \xi' \mathbf{X_{hv}} + \varepsilon_{hv}, \tag{5}$$

where α_v denotes the village fixed effects, and y_{hv} denotes either ω_{hv} or $\ln c_{hv}$. We estimate (5) separately for Rarieda Endline and Nakuru Endline I. As in our main specifications, we instrument for expenditure using the unconditional cash transfers. We do not cluster the standard errors in this regression, as the randomization in this case occurred at the household level (our unit of observation). In the non-experimental version, we do not make use of the instrument and restrict the sample to the control households in the treatment villages.

The second specification is a first difference regression. We are able to estimate such a regression since we have expenditure data in both Rarieda Baseline and Rarieda Endline for the treatment villages (treated and control households). The specification can be written as:

$$\Delta y_{hv} = \alpha_v + \beta \Delta \ln z_{hv} + \xi' \mathbf{X_{hv}} + \varepsilon_{hv}, \tag{6}$$

where Δ denotes changes from baseline to endline, and α_v, as before, denotes the village fixed effects. y_{hv} denotes either ω_{hv} or $\ln c_{hv}$. This is an attractive specification with plausibly a high degree of precision, as it takes account of household heterogeneity directly and because the instrument (the transfer) should be a good predictor of the change in expenditure between baseline and endline.

4.4 Differences across treatment arms

The Nakuru intervention had one cross-randomization (transfer frequency) and the Rarieda intervention had three (gender, transfer magnitude, and frequency). We use this to study whether the estimated elasticities depend on the transfers i) being made to the wife or the husband; ii) being handed out as a lump sum or in monthly (in Rarieda) or weekly (in Nakuru) installments; and iii) being "large" (KES 95,200) or "small" (KES 25,200).

We estimate elasticities for the different treatment arms based on the main across-village specifications in (1) and (3). When doing this, we restrict the sample to control households from the control villages and one treatment group for each estimation. For example, when estimating the elasticities for female recipients, we restrict the sample to control households and treated households with female recipients (leaving out treated households with male recipients and households that are not two-headed). We test the differences between the different treatment arms by estimating nested models, similarly as for the experimental versus non-experimental comparison (see above).

5. Main results

We start by briefly discussing the reduced form effects of the unconditional cash transfers on total consumption (the first stage) as well as the validity of the IV estimation. We then present our main results.

5.1 First stage

In Section 3.4, we documented significant treatment effects of the cash transfers on total expenditure at endline. This is essentially our first stage. In Table 3, we display first-stage F-statistics and the number of observations in each specification. For the main specification, we find that the first stage is strongest in the Nakuru Endline I (F-statistic of 39), and weaker in the Nakuru Endline II (F-statistic of 12) and the Rarieda Endline (F-statistic of 10). The relatively stronger first-stage in Nakuru Endline I (i.e., the stronger relationship between receiving the cash transfer and total household expenditure) can plausibly be explained by the typical transfer in Rarieda being smaller ($1,076 PPP vs. $782 PPP, with only a small share of recipients in Rarieda receiving the very large transfer of $2,956 PPP), and the timing of the Nakuru Endline I (one year after the transfer) versus Nakuru Endline II (two years after the transfer). Note that the specification in which we pool all surveys has a much stronger first stage than any of the individual household surveys (F-statistic of 65). In sum, the randomized cash transfers strongly increase total expenditure and are thus a good instrument in terms of relevance.

The fact that the first stage is strong is important and somewhat interesting in light of discussions related to both modality and timing of transfers. According to the permanent income hypothesis, the transfer should affect consumption in all periods after such a transfer as households would smooth the expenditure gain across all future consumption (Meghir and Pistaferri, 2011). According to hand-to-mouth based theories, on the other hand, the transfer should only affect the same period expenditure and our first stage would be much weaker (see e.g., Kaplan et al., 2014, for a discussion of hand-to-mouth households).

5.2 The effect of the transfer

The exclusion restriction requires that the transfers affect food expenditure only through the treated participants being richer at endline, i.e., through total expenditure at endline and not through other effects of the cash transfers. Another way to think about this is that the IV analysis interprets changes in food consumption as movements *along* an underlying Engel curve.

It is fair to say that the main effect of unconditional cash transfers is that people become richer. This is quite intuitive and it is also clear from the full set of reduced form results reported in Haushofer and Shapiro (2016) and Haushofer et al. (2020). In particular, there is no sign that the treatment changes the primary source of income for recipient households. Even so, previous research suggest that cash transfer programs in other settings may have had a direct impact food consumption *beyond* the effect through expenditure. For instance, Angelucci and Attanasio (2013) and Attanasio et al. (2012) argue that *PROGRESA/Oportunidades* in Mexico, and *Familias en Acción* in Colombia, induce an upward shift in the Engel curve for food expenditure, i.e. that recipients of the cash transfers consume relatively more food for given levels of consumption. A key difference from our setting is that these programs explicitly target mothers which might lead to a shift in power in decision-making within the household and increased food consumption (see Angelucci and Attanasio, 2013). Another difference to our setting is that the above programs are multi-faceted and conditional on activities that plausibly could have a direct effect on food consumption. In PROGRESA, for instance, the cash transfers are conditional on attending a number of courses, including courses in nutritional practices.

The cash transfers studied in this paper were completely unconditional and not targeted to women only. We thus find and IV analysis more plausible in our setting than in the aforementioned studies (that used other identification strategies to discuss consumption responses). For completeness and comparability to the previous literature, we discuss the form of the Engel curves for the cross sections of control and treated in the Appendix D (see also Grönqvist et al. (2020) for a similar discussion of whether treatment changes the functional form of the relationship of interest).

We now turn to our main results based on the IV estimation.

5.3 Experimental elasticities

Figure 3 presents elasticities derived through the different specifications, for food expenditure (left panel) and calorie availability (right panel). The dark shaded bars show experimental estimates identified using the exogenous variation in expenditure, while the light shaded bars show the corresponding non-experimental estimates. We start by describing the former.

The first set of bars displays estimates from Nakuru Endline II, which is the survey with the most detailed expenditure and price data. We find an expenditure elasticity of food expenditure of 0.872. This elasticity is likely to be an upper bound of the elasticity of calorie availability since households typically switch from calorie-intense food to food with other attributes, such as taste, when they become richer. As expected, therefore, we find a smaller calorie elasticity of 0.668.

The following two sets of bars present corresponding elasticities from Rarieda Endline and Nakuru Endline I. Overall, the estimates are very similar to those from Nakuru Endline II. The food expenditure elasticity in Nakuru Endline I is almost exactly the same (0.860), while the food expenditure elasticity in Rarieda Endline is somewhat smaller (0.795).[17] The elasticity of calorie availability is also slightly smaller (as discussed in Section 3.3, the calorie numbers used for this estimation are less reliable than in Nakuru Endline as they are imputed from information on expenditure and prices). In the fourth row, we report the food expenditure elasticity obtained by pooling the three household surveys. We find an elasticity of 0.845. As mentioned, the first stage in the pooled regression is much stronger than for any of the individual surveys, and as a consequence, we obtain tighter confidence intervals.[18]

Panel B of the figure displays elasticities from the within-village specifications. The comparison group in these regressions is the control households from the treatment villages, while households from the pure control villages are excluded. We start by estimating the regression in levels, as displayed in Equation (5). We find remarkably similar elasticities as before, ranging from 0.796 to 0.836, despite the fact that we use a completely different control group. The confidence intervals are however tighter than in the across-village specifications. We next estimate the first-difference specification from Equation (6). We are able to run this regression because we have (comparable) expenditure data in Rarieda Baseline and Rarieda Endline. Again, we find estimates that are very similar to the across-village estimates: for food expenditure, the estimate is almost exactly the same, while the calorie elasticity is about 4 percentage points lower.

Haushofer and Shapiro (2016) concluded that the spillover effects to non-recipient households were of limited importance in the Rarieda experiment. The above results suggest that this conclusion holds also for the Nakuru experiment. This observation is important, we believe, as spillover effects constitute a general concern when estimating treatment effects in social sciences. Our results suggests that expenditure and nutrient elasticities, at least in our setting, can sensibly be estimated from within-village variation alone.

Most of the earlier literature estimate elasticities that are smaller than those reported here. As mentioned in the Introduction, the meta-study of Colen et al. (2018) reports an average food

[17] Note that we do not have separate price data for Nakuru Endline I. Because of this, we use the price indices from Nakuru Endline II for this survey.

[18] We do not include relative price controls in the pooled regression, as the price indices are relative *within* each survey. We also estimated the pooled regression with survey fixed effects. The advantage of this specification is that it accounts for time-varying effects (e.g. price levels). Yet, we obtained almost exactly the same point estimate; results are available upon request.

expenditure elasticity of 0.61 and an average calorie elasticity of 0.42 based on a sample of 66 studies from African countries. Our estimates – based on the exogenous variation in expenditure induced by the unconditional cash transfers – are much larger than this, and this finding is robust to a variety of different specifications.

We present several additional analyses in the appendix. In Appendix E, we show elasticities for sub-groups of food expenditure (see Table A7) and for different nutrients, including proteins, fats, carbohydrates, fibers, and irons (see Table A8). All the nutrient elasticities are lower than our estimated food expenditure elasticities, and the point estimates are mostly below unity (although not always significantly so). Like for calories, this suggests that households switch to food with other attributes (for instance taste) when they become richer. The expenditure elasticities of the different food sub-groups are generally also below unity, with the exception of the category *meat, fish and dairy*, which suggests that this category contains luxury goods.

In Appendix F, we present various robustness tests. We test whether our findings are driven by differences in household composition by using equivalence scales; whether our findings are driven by increased use of *M-Pesa* by removing households that obtained access to the mobile money services through the intervention; we assess the robustness of our results to the inclusion of a larger set of household controls (see Table A9); and we evaluate the food expenditure elasticities at different points of the food budget share distribution (see Table A10). All of these robustness specifications give comparable elasticities as those reported in this section. Finally, in Appendix G we evaluate whether the estimated elasticities in Rarieda differ depending on the recipients initial expenditure level. We find no meaningful heterogeneity.

5.4 Non-experimental elasticities

The light shaded bars in Figure 3 display the non-experimental elasticities. We derive these by estimating the same specifications as above, but without instrumenting for expenditure. We also limit the sample to households from the control villages to avoid using any of the variation induced by the cash transfers.

The main take-away is that the non-experimental elasticities are consistently larger than those identified through the experiments. In our main across-village specification, the food expenditure and the calorie elasticities are about 10-12 percentage points larger. The standard errors for the estimates in Rarieda Endline and Nakuru Endline II are too large to make this difference to the experimental estimates statistically significant, while in Nakuru Endline I and in the pooled regression the difference is statistically significant, with p-values of 0.057 and 0.007, respectively.[19] The within-village specifications give even larger differences, especially for the calorie elasticities, and these differences are significant at the 5 percent level in most cases.

The specifications used to derive these non-experimental elasticities resemble typical specifications from the literature on food elasticities based on observational data. Such specifications do not have a straightforward interpretation, as differences in households' consumption levels and patterns may stem from difference in tastes or opportunities. The fact that we find significant differences between our experimental and non-experimental elasticities suggest that we should treat estimates based on cross-sections with caution.[20]

6. Alternative specifications

The two experiments and our rich household survey data enable us to establish several additional results. We outline these below.

6.1 Relative prices

The summary statistics in Table 2 revealed large differences in both food and non-food prices across the villages in our study. An important question is whether such price differences matter to accurately estimate food elasticities. We investigate this question by estimating the across-village specification without the price controls.

[19] The difference in the pooled regression remains significant at the 5 percent level in the pooled regression with survey fixed effects.

[20] A commonly used approach to deal with the endogeneity of household expenditure is to use household income or assets as an instrument. In Appendix H, we show that this is of little help, at least in our setting.

Panel A of Figure 4 displays elasticities with and without controls for relative prices (see Table 4 for first-stage F-statistics and the number of observations). The two sets of estimates barely differ. In Rarieda Endline, the estimated elasticities become somewhat smaller when we disregard the price controls (0.734 vs. 0.795), but in Nakuru Endline II, where we have the best price data, we obtain almost exactly the same elasticities (0.867 vs. 0.872). This is an important finding, we believe, as researchers often do not have access to proper price measures and are forced to ignore local price variation. Our results suggest that this might not be a serious problem in settings like ours.

In Appendix E and F, we explore the robustness of this conclusion by using alternative price measures. First, we apply a correction to remove (potential) quality differences in the goods underlying our price data (see also Section 3.2) and show that this does not affect the elasticities (see Table A12). Second, we construct distinct measures for the price of calories in each village and show that this gives rise to similar calorie elasticities as reported in our main specification (see Table A8). Third, we compute unit values by dividing households' reported expenditure on different consumption items by their reported quantities purchased. We then use the unit values as proxies for prices and compute alternative price indices, which gives about similar elasticities as our main estimates (see Table A12). Very few studies are able to make this comparison, as most datasets do not include unit values and actual prices for the same households (see e.g. Gibson and Kim, 2019). Our result suggests that unit values can sensibly be used to estimate expenditure elasticities in settings like ours.

6.2 Cross-randomization: Transfer frequency, transfer size, and gender targeting

Finally, we briefly report results based on the different cross-randomizations in the two experiments. Estimates based on our main across-village specification are shown in Panel B-D of Figure 4.

In Panel B, we show elasticities derived from cash transfers given as installments (monthly in

Rarieda and weekly in Nakuru) and transfers given as lump-sum. The point estimates for installments are relatively larger in both Rarieda Endline and Nakuru Endline II, but smaller in Nakuru Endline I.[21] The standard errors of the estimates are however too large to make the differences between the two sets of estimates statistically significant.

In Panel C, we display elasticities for large and small transfers in Rarieda. We find that the large transfers generate lower food expenditure elasticities than the small transfers, but again, the standard errors are too large to statistically distinguish the two sets of estimates. Qualitatively, however, the estimates suggest some concavity in the relationship between food budget shares and total expenditure.

In Panel D, we show elasticities separately for male and female recipients in Rarieda Endline. We estimate these elasticities by limiting sample to two-headed households.[22] We find no significant differences. In fact, the point estimates are very similar (0.706 vs. 0.772 for food expenditure and 0.546 vs. 0.539 for calories, respectively). This result echoes the findings of Almås et al. (2018), who document that male and female recipients had identical consumption and nutrition responses to unconditional cash transfers in India. The result also suggests that the unitary household model is appropriate to accurately estimate elasticities in our setting. More highly powered studies (along this dimension) using unconditional cash transfers and high quality expenditure data are however needed to assess the plausibility of the unitary assumptions in different settings across the world.

7. Concluding remarks

In this paper we use data from two randomized controlled trials delivering unconditional cash transfers to poor households in Kenya to identify the expenditure elasticity for food expenditure and nutrition. In contrast to much of the previous literature, we derive estimates through random variation in total expenditure, which enables us to obtain causal estimates of the different

[21] Note that the elasticities in Rarieda Endline need not average to our preferred elasticity of 0.795 as we leave out the large transfers. These transfers were delivered as add-on monthly installments to small lump-sum or monthly transfers, and thus cannot be unambiguously considered lump-sum or monthly.

[22] Again, note that these elasticities need not average to our main estimated elasticity in Rarieda Endline, as we leave out households that were not two-headed.

elasticities.

We find that the expenditure elasticity for food is higher than most previous studies have indicated, and our estimated elasticities are robust to a range of specification- and robustness checks. The estimated elasticities using exogenous variation in budget are significantly different from those estimated using non-experimental data.

We find that the expenditure elasticity for calories is lower than that for food expenditure, which is plausible given that as households get richer they may adjust their food consumption from calorie-dense food to more expensive food with better taste. The expenditure elasticities for most food groups are below unity, with the exception of meat, fish, and diary, where we find an elasticity larger than one.

Can the results reported here be used to enlighten us about the possible existence of poverty traps? Note first that the necessary condition for a nutrition-based poverty trap to exist is that the product of two elasticities – the income elasticity of nutrition with respect to income, and the elasticity of income with respect to nutrition (e.g. driven by the productivity increase from eating more or better food) – needs to be higher than unity over some range. Our estimated calorie elasticity is higher than in previous work, but below unity. This means that for a poverty trap to exist, the elasticity of income with respect to calorie consumption would have to be very high. We deem this somewhat unlikely; for instance, Schofield (2014) finds significant but moderately-sized effects of an increase in calorie consumption on productivity in India. Thus, in our view, our results make a simple calorie-based poverty trap unlikely.

However, even if a calorie-based poverty trap does not exist, it is important to note that poor households in Kenya are responding to higher budgets by consuming more and better food. This is an important insight for policy makers considering transfer schemes as a potential tool to improve the lives of poor people and the health and nutrition of children, adolescents, and adults.

Last, note that we have studied the causal effect on nutrition and food consumption of a specific policy tool – with unconditional cash transfers of substantial size paid out during a limited period of time. Our instrument is the random allocation of cash 1 to 3 years prior to the study period. The allocation of money is indeed random – and we do have a strong first stage – but it is still worth mentioning that the estimated elasticities should be interpreted within the setting of previous random transfers. The recent substantial literature evaluating cash transfer programs of different sorts, has been concerned with the duration of the effect from various programs and aspects important for the effect, such as context, modality, multi-faceting, conditionality as well as gender-, and other-, targeting (see e.g. Banerjee et al., 2015; Bazzi et al., 2015; Fiszbein and Schady, 2009; Haushofer and Shapiro, 2016; Parker and Vogl, 2018; Meghir and Pistaferri, 2011; Millán et al., 2019, 2020, for in-depths discussions and results on these issues). In order to estimate expenditure elasticities for food and nutrition for other policy interventions it is necessary to combine plausible exogenous variation with high quality data on consumption and prices for the context of the interventions, and we therefore see this paper as one contribution in a whole series of existing and future contributions to more fully understand how households respond to budget changes and transfers.

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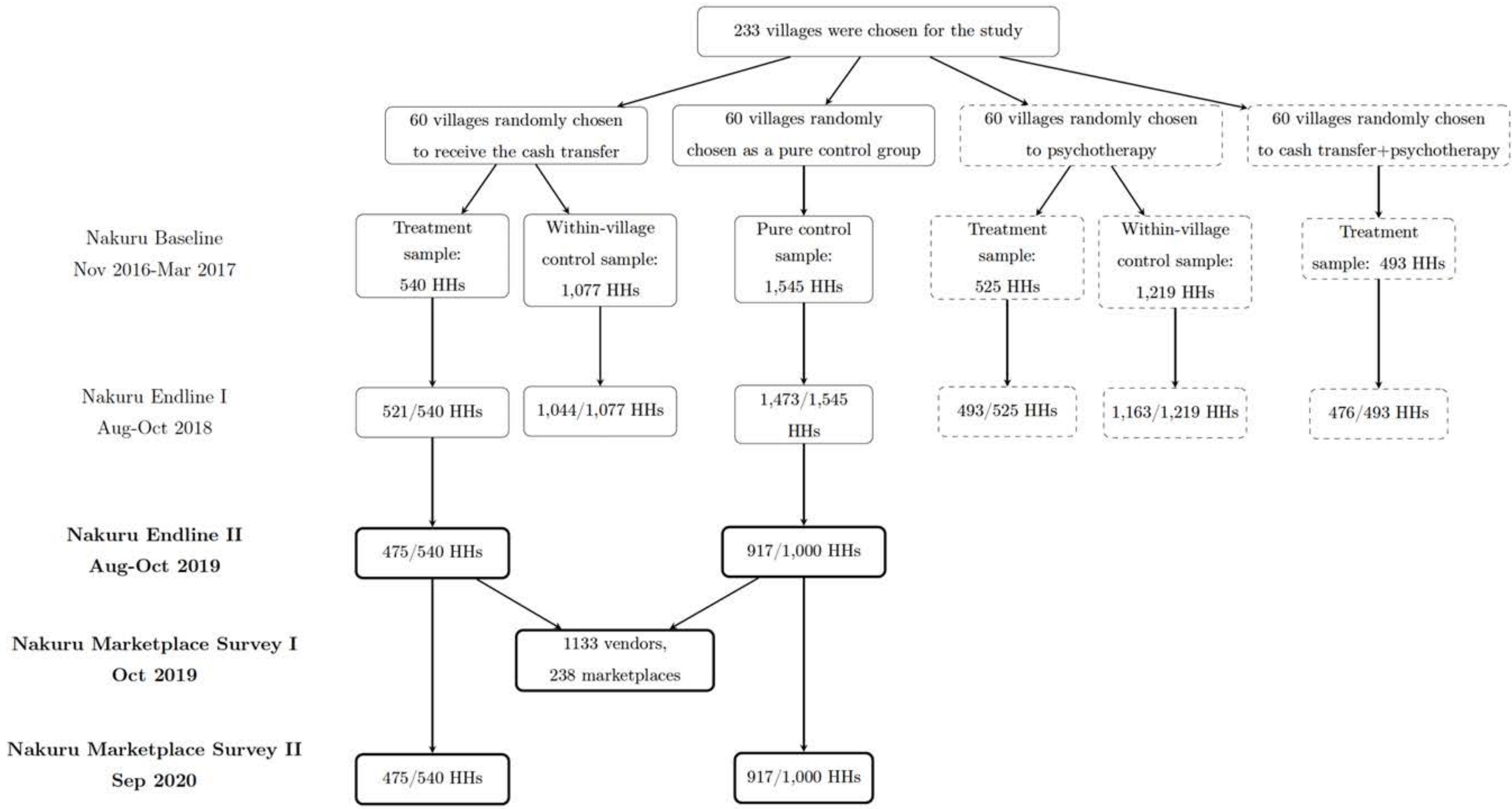
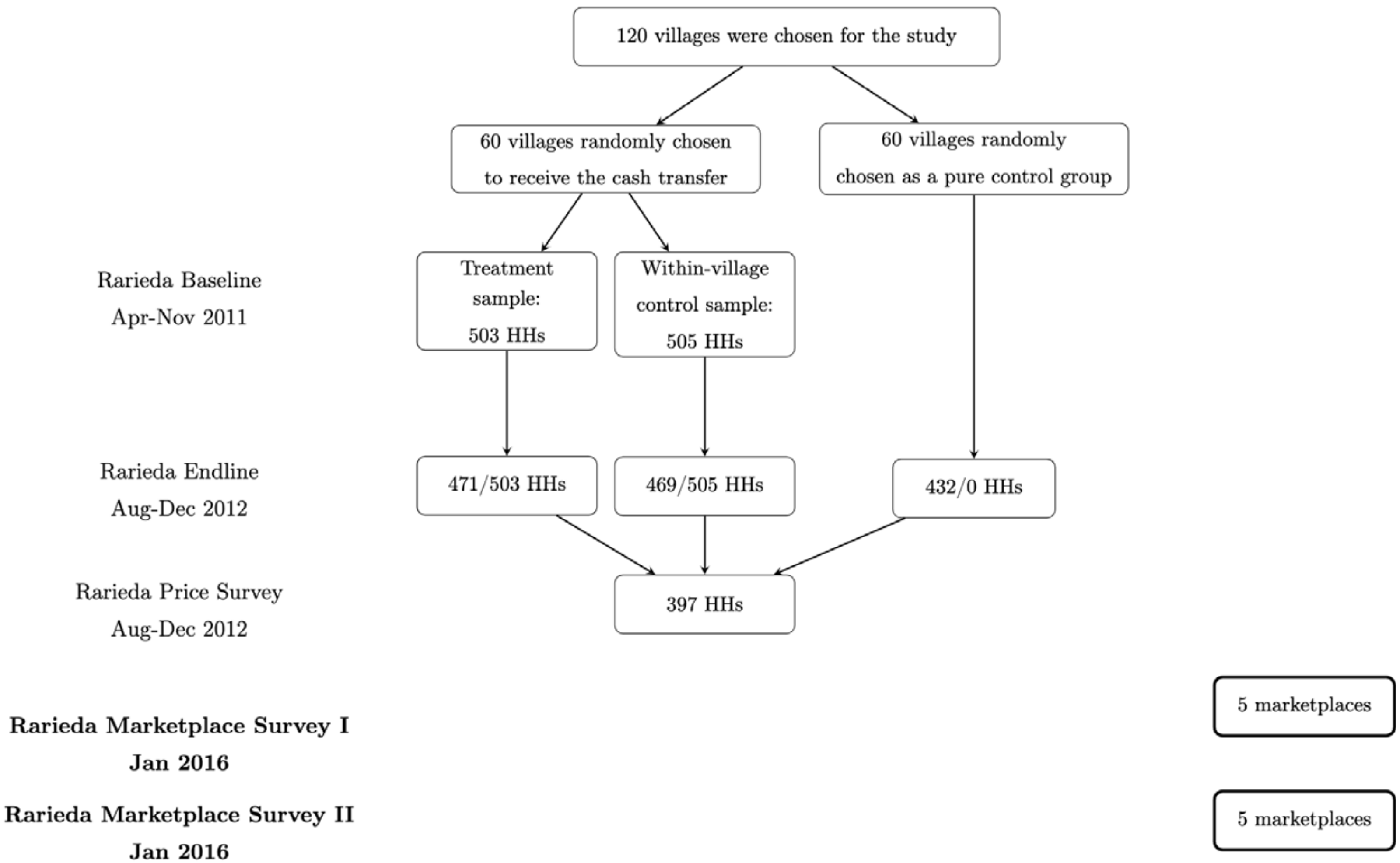
Figure 1: Study design and data collection in Nakuru



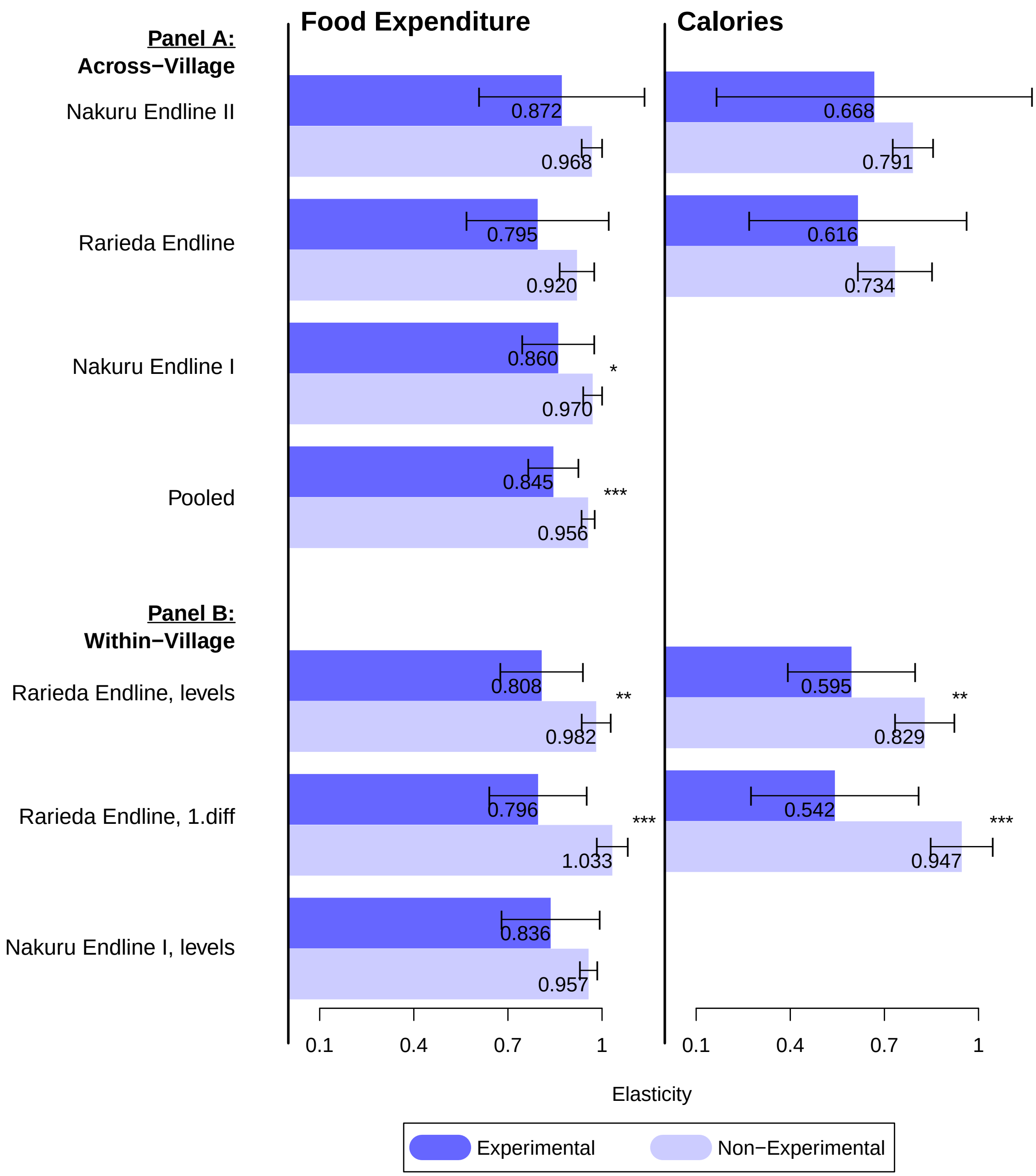
Notes: The figure displays the study design of the Nakuru experiment. Numbers with slashes designate surveyed endline households/baseline households in each treatment arm. The bold squares indicate the new surveys conducted for this paper.

Figure 2: Study design and data collection in Rarieda



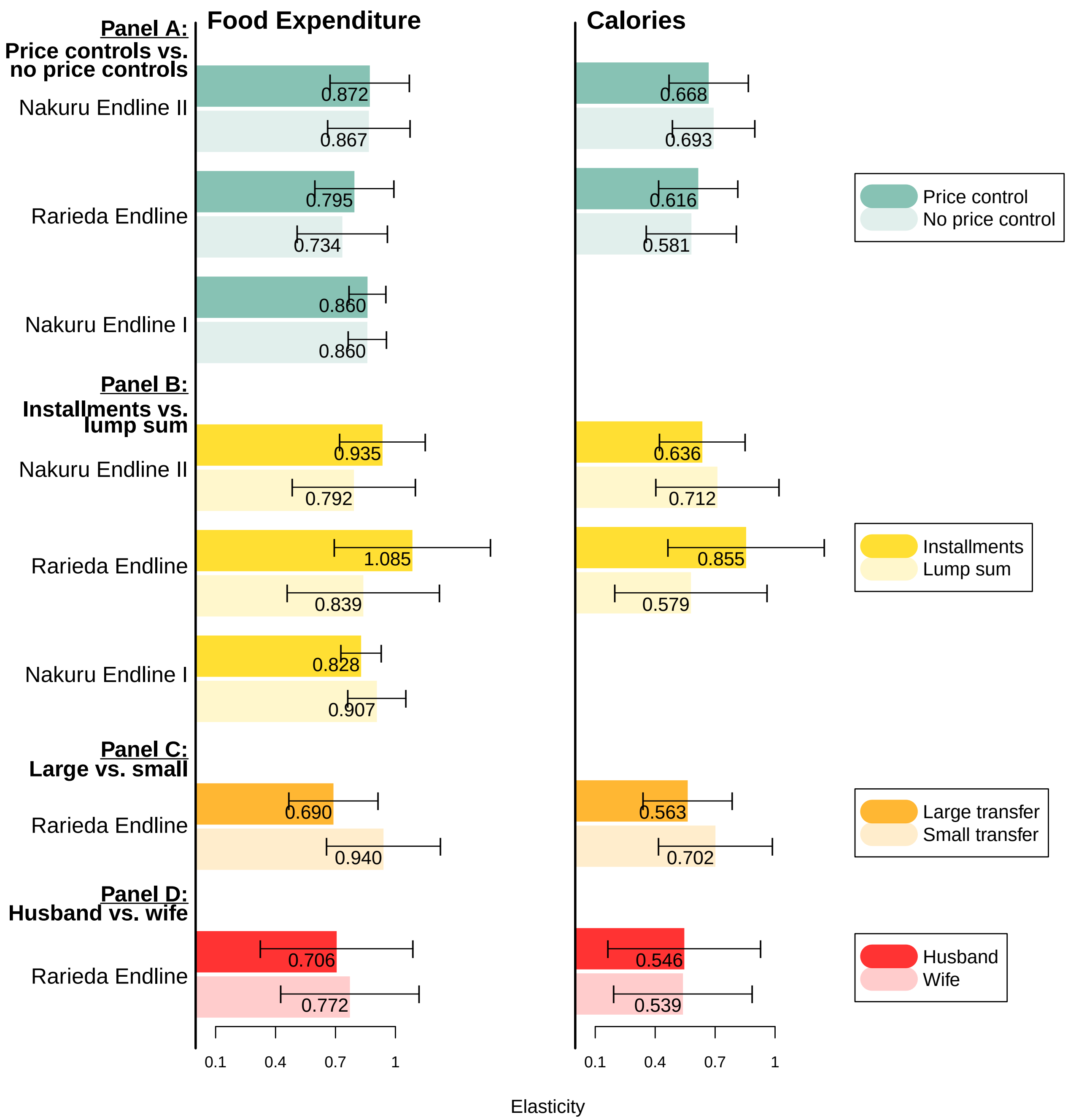
Notes: The figure displays the study design of the Rarieda experiment. The bold squares indicate the new surveys conducted for this paper.

Figure 3: Experimental vs. non-experimental elasticities



Notes: The dark shaded bars display elasiticities derived from the exogenous variation in expenditure induced by the unconditional cash transfers, while the light shaded bars display the corresponding non-experimental elasticities. The bracket lines show 95 percent confidence intervals, while the stars denote the significance level of the difference between the experimental and non-experimental estimates for each specification.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4: Prices vs. no prices, treatment arms, and individual elasticities



Notes: The figure displays (experimental) elasticities from various across-village specifications. The bracket lines show 95 percent confidence intervals. Panel A displays elasticities with and without price controls; Panel B displays elasticities by lump sum and installment transfers; Panel C displays elasticities by large and small transfers; and Panel D displays elasticities by male and female recipients.

Table 1: Survey details

	Rarieda		Nakuru		
	Baseline	Endline	Baseline	Endline I	Endline II
Amount spent on food	✓	✓	✓	✓	✓
# of categories	15	15	1	13	
# of items					75
Food quantities consumed					✓
Consumption of home-produced food	✓	✓	✓	✓	✓
Amount spent on non-food	✓	✓	✓	✓	✓
# of categories	24	24	1	13	10
# of items					20
Local food prices		✓			✓
# of items		24			49
Local non-food prices		✓			✓
# of items		4			11
Other-village control group	✓	✓	✓	✓	✓
Same-village control group	✓	✓	✓	✓	

Notes: The table summarizes the number of consumption items/categories for each household survey.

Table 2: Treatment effects

	Rarieda		Nakuru			
	Endline		Endline I		Endline II	
	Control mean (SD)	Treatment effect	Control mean (SD)	Treatment effect	Control mean (SD)	Treatment effect
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Household-level						
Monthly expenditure KES	11,038.3	2,426.0**	9,902.0	2,873.2***	9,691.4	1,192.7**
	(14,008.4)	(1,125.6)	(11,716.3)	(786.7)	(6,198.9)	(524.9)
Monthly expenditure USD (2017-PPP)	327.9	72.1**	202.2	58.7***	187.1	23.0**
	(416.1)	(33.4)	(239.3)	(16.1)	(119.7)	(10.1)
Food budget share	65.8	-2.0*	71.8	-2.7**	70.6	-1.2
	(14.3)	(1.1)	(18.0)	(1.1)	(17.1)	(1.3)
Calories per day	7,134.3	826.4**			9,383.1	824.2
	(4,225.5)	(341.9)			(5,850.4)	(539.9)
Home-production, share of food exp.	13.3	0.4	20.2	-0.4	12.2	-0.5
	(10.5)	(0.9)	(19.7)	(1.5)	(12.5)	(1.1)
Home-production, share of calories	21.5	0.4			21.5	-2.2
	(14.1)	(1.4)		(20.5)	(2.1)	
Number of households	903		1,994		1,392	
Panel B: Village-level						
Food prices	1.00	0.00			1.00	-0.00
	0.17	0.03			0.11	0.02
Non-food prices	1.00	-0.05			1.00	-0.03
	0.17	0.03			0.22	0.04
Number of villages	123				120	

Notes: The table presents OLS estimates of treatment effects. The outcome variables are listed on the left. Columns (1), (3) and (5) display averages and standard deviations for the control group, while Columns (2), (4) and (6) display treatment effects. We derive the treatment effects by comparing treatment households to control households in the pure control villages. Standard errors, clustered on villages, are shown in the parentheses. The unit of observation is households in Panel A, and villages in Panel B.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: First stage and number of observations, Figure 3

	Experimental (1)	Non-experimental (2)
Panel A: Across-village		
Nakuru Endline II		
First Stage coeff., log(UCT)	0.018***	
	(0.005)	
Observations	1,392	917
First Stage F-statistic	11.7	
Rarieda Endline		
First Stage coeff., log(UCT)	0.020***	
	(0.006)	
Observations	903	432
First Stage F-statistic	10.2	
Nakuru Endline I		
First Stage coeff., log(UCT)	0.034***	
	(0.006)	
Observations	1,994	1,473
First Stage F-statistic	39.4	
Pooled		
First Stage coeff., log(UCT)	0.035***	
	(0.005)	
Observations	4,289	2,822
First Stage F-statistic	65.3	
Panel B: Within-village		
Rarieda Endline, levels		
First Stage coeff., log(UCT)	0.027***	
	(0.005)	
Observations	940	469
First-stage F-statistic	30.9	
Rarieda, 1. Diff		
First Stage coeff., log(UCT)	0.033***	
	(0.006)	
Observations	939	469
First-stage F-statistic	28.4	
Nakuru Endline I, levels		
First Stage coeff., log(UCT)	0.021***	
	(0.004)	
Observations	1,565	1,044
First-stage F-statistic	24.3	

Notes: The table presents the number of observations and the first-stage F-statistic for each specification from Figure 3. "First Stage coeff., log(UCT)" denotes the effect of log cash transfer on log household expenditure.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Number of observations and F-statistic, Figure 4

	Panel A		Panel B		Panel C		Panel D	
	Price controls	No price controls	Install-ment	Lump sum	Large	Small	Husband	Wife
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Nakuru Endline II								
Observations	1392	1392	1145	1164	–	–	–	–
First-stage F-statistic	11.7	10.7	10.3	4.8				
Rarieda Endline								
Observations	903	903	591	616	560	775	529	508
First-stage F-statistic	10.2	9.0	3.1	2.6	15.7	3.9	3.9	4.2
Nakuru Endline I								
Observations	1994	1994	–	–	–	–	–	–
First-stage F-statistic	39.4	35.6						

Notes: The table presents the number of observations and the first-stage F-statistic for each specification from Figure 4.

Appendix for Online Publication

A. Testing differences across specifications using nested models

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

$$\begin{aligned}\omega_{hv} = \alpha_0 + \alpha_1 s_1 + \beta_1 \ln(z^*_{hv}) s_1 + \beta_2 \ln(z^*_{hv}) s_2 + \gamma_1 (\ln p^f_v - \ln p^n_v) s_1 \\ + \gamma_2 (\ln p^f_v - \ln p^n_v) s_2 + \xi_1' X_{hv} s_1 + \xi_2' X_{hv} s_2 + \varepsilon_{hv}\end{aligned} \tag{A.1}$$

Here, s_1 and s_2 are indicator variables denoting the sample to which each observation belongs. Equation (A.1) is estimated on a sample that combines the two sample restrictions. One of the two interactions terms between log expenditure and the 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 let 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 $ln(UCT_{hv})s_1$. β_1 is then equivalent to β in Equation (1) in the main paper 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 the differences between the different treatment arms.

B. Deviations from the Pre-Analysis Plan, and Analyses Not Pre-Specified

We wrote a pre-analysis plan (PAP), which is published and time-stamped. The document is available at https://www.socialscienceregistry.org/docs/analysisplan/625. Table A1 lists the differences between the analyses we implemented and the registered PAP.

Table A1: Deviations from the Pre-Analysis Plan, and Analyses Not Pre-Specified

PAP	Actual Study	Reason for Deviation
Data Sources		
We pre-specified that we would use the sample of households from the Rarieda experiment, which took place between 2011 and 2013.	In addition to the Rarieda experiment, we make use of a similar experiment in Nakuru. Since this experiment is more recent, we were able to re-interview the original study participants. In total, we organized five new surveys for this paper: i. Rarieda Marketplace Survey I; ii. Rarieda Marketplace Survey II; iii. Nakuru Endline II household survey; iv. Nakuru Marketplace Survey I, and v. Nakuru Marketplace Survey II.	The detailed consumption data in the Nakuru Endline II household survey enables us to more credibly measure households' food consumption and nutrient intakes. Similarly, the additional price data allows us to credibly account for potential general equilibrium effects on prices. Also, the use of different data sources from different experiments allows us to make statements about the generalizability of the findings across different settings.
Analysis		
We pre-specified two main types of specifications: a linear Almost Ideal Demand System (AIDS) and a Quadratic Almost Ideal Demand System (QUAIDS). We stated that AIDS would become our preferred model if the quadratic term in the QUAIDS was insignificant.	We only report AIDS and not QUAIDS.	For the Rarieda experiment, the first-stage is too weak when we estimate the QUAIDS, and the quadratic term is insignificant. For the Nakuru experiment, we are unable to estimate the quadratic demand system as we do not have variation in the size of the transfer (our instrument).
We pre-specified that we would use a first-difference specification as our main specification and that we would use this to test the differences across treatment arms etc. We also noted that we would pool within-village control households and pure control households in most specifications.	We use a across-village specification as our main specification, in which we exclude within-village control households.	The Nakuru Baseline did not collect (detailed) consumption data, which implies that we are unable to apply the first-difference specification for this experiment. We report the first-difference regression for Rarieda as an alternative specification. We do not report specifications where we pool within-village control households and pure control households, as these specifications always give about identical estimates as the corresponding specifications reported in the paper.

We pre-specified that we would test the differences across treatment arms by interacting the instrument with treatment status.	We estimate separate elasticities for each treatment arm and restrict the estimation sample to one treatment arm at a time and households in control villages. We test for differences between the treatment arms by estimating nested models.	The two approaches give equivalent results. We chose the nested model approach for consistency, as it also can be used to test the differences between our "experimental" and "non-experimental" elasticities.
We pre-specified a number of concrete robustness tests.	Presented in Appendix F and not in the main paper. We also report a number of additional robustness tests by computing alternative measures of local price levels (in Appendix F.5).	Space constraints. The opportunity to calculate a rich set of price measures arose with the collecting of the Nakuru Endline II household survey and the Nakuru Marketplace Survey I.
We pre-specified that we would estimate specifications where we add heterogeneity in baseline expenditure.	Presented in Appendix G, and not in the main paper	Space constraints.
We pre-specified that we would conduct an extension in which we allow consumption responses to price changes to the transfer to vary by baseline expenditure.	We do not report this extension.	Space constraints, and also because we are only able to implement this for the Rarieda experiment, in which we find very little price responses in general, and no heterogeneity in terms of baseline expenditure.

C. Attrition and sample balance

In this section, we investigate baseline balance between the treatment and the control groups and test whether they had differential attrition.

We start by analyzing attrition. To do this, we first construct a binary variable indicating whether household h was surveyed at baseline but not at endline. For Nakuru, we also construct a binary variable denoting whether households were surveyed at baseline but are missing in either Endline I or Endline II. We use these dummy variables as dependent variables in the following regression:

$$attrit_{hv} = \beta_0 + \beta_1 T_{hv} + \epsilon_{hv}, \tag{C.1}$$

where T_{hv} denotes the treatment status of household h in village v.

Estimates are presented in Table A2. In the first column we investigate attrition in Nakuru Endline II. In the following two columns we investigate attrition in Endline I: Column (2) is based on the full sample of eligible households, while Column (3) is based on the smaller sample of households that were targeted for Endline II. In the fourth column, we explore attrition in Endline I and II combined (*attrit* equal to unity if respondent is missing in either of the surveys), and in the fifth column we investigate attrition in Rarieda Endline. Overall, the estimates give no evidence of differential attrition by treatment and control groups.

We next investigate the sample balance at baseline. We run the following regression:

$$y_{hv} = \beta_0 + \beta_1 T_{hv} + \epsilon_{hv}, \tag{C.2}$$

where y_{hv} denotes the household characteristic of interest, while T_{hv}, as before, denotes the treatment status of the household. Table A3 displays estimates for the Nakuru Endline II sample, Table A4 displays estimates for Nakuru Endline I sample, and Table A5 displays estimates for Rarieda Endline sample. The results suggest that the treatment and control groups are balanced on observables.

Table A2: Attrition: Difference in attrition in treatment vs. control groups

	Nakuru Endline II	Nakuru Endline I		Nakuru Endline I, II	Rarieda Endline
	(1)	(2)	(3)	(4)	(5)
Treated household	0.034	-0.009	-0.011	0.027	-0.008
	(0.021)	(0.013)	(0.014)	(0.022)	(0.016)
Control household in treatment village			-0.017		
			(0.011)		
Mean of dep.var	0.101	0.040	0.046	0.110	0.068
Observations	1,540	3,162	1,540	1,540	1,007

Notes: The dependent variable in Column (1) is a dummy that is equal to one if the respondent was surveyed in Nakuru Baseline but not in Nakuru Endline II. In Columns (2) and (3), the dummy variable is equal to one if the respondent was surveyed in Nakuru Baseline but not in Nakuru Endline II. Column (2) is based on the full sample, while Column (3) is based on the sample eligible for Nakuru Endline II: all treatment households and 1000 randomly selected households from the control villages. The dependent variable in Column (4) is a dummy that is equal to one if the respondent was surveyed in Nakuru Baseline but is missing in either Nakuru Endline I or Endline II. Finally, the dependent variable in Column (5) is a dummy that is equal to one if the respondent in Rarieda was surveyed at baseline but is missing in the endline survey. Standard errors in Columns (1) to (4) are clustered at the level of villages.

Table A3: Baseline balance, Nakuru Endline II sample

	Control mean (SD) (1)	Treatment effect (2)
Age (respondent)	43.21	1.01
	(16.20)	(1.11)
Female (respondent)	0.64	0.02
	(0.48)	(0.03)
Marital status (respondent)	0.65	0.02
	(0.48)	(0.03)
Years of education (respondent)	7.38	-0.19
	(4.16)	(0.26)
Number of children	1.95	0.02
	(2.02)	(0.13)
Household size	4.88	0.07
	(2.52)	(0.17)
Land owned (ha)	1.32	0.08
	(1.72)	(0.17)
Asset index	0.52	-0.06
	(0.50)	(0.04)
Log total expenditure	8.03	0.03
	(0.82)	(0.05)
Log food expenditure	7.54	0.08
	(0.82)	(0.06)
M-Pesa access	0.78	-0.01
	(0.42)	(0.03)
Observations	917	475

Notes: The table presents OLS estimates of baseline differences between treatment and control groups. Outcome variables are listed on the left. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports mean and standard deviation for the control group. Column (2) compares treatment households to control households. Standard errors, clustered at the level of villages, are shown in the parentheses.

Table A4: Baseline balance, Nakuru Endline I sample

	Control mean (SD)	Treatment effect	Control HH treatment village
	(1)	(2)	(3)
Age (respondent)	43.38	-1.48	2.56
	(16.58)	(1.06)	(1.85)
Female (respondent)	0.64	-0.02	0.04
	(0.48)	(0.03)	(0.02)
Marital status (respondent)	0.63	0.03	0.01
	(0.48)	(0.03)	(0.03)
Years of education (respondent)	7.17	-0.08	0.06
	(4.09)	(0.25)	(0.23)
Number of children	1.96	0.01	0.05
	(2.05)	(0.13)	(0.12)
Household size	4.89	0.08	0.09
	(2.56)	(0.17)	(0.15)
Land owned (ha)	1.40	0.05	-0.03
	(1.77)	(0.18)	(0.15)
Asset index	0.52	-0.06	-0.02
	(0.50)	(0.04)	(0.04)
Log total expenditure	8.04	0.02	0.01
	(0.81)	(0.05)	(0.04)
Log food expenditure	7.56	0.05	0.03
	(0.80)	(0.06)	(0.05)
M-Pesa access	0.76	0.01	-0.00
	(0.43)	(0.02)	(0.02)
Observations	1,473	521	1,044

Notes: The table presents OLS estimates of baseline differences between treatment and control groups. Outcome variables are listed on the left. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports mean and standard deviation for the control group. Column (2) compare treatment households to pure control households, and Column (3) control households in treatment villages to pure control households. Standard errors, clustered at the level of villages, are shown in the parentheses.

Table A5: Baseline balance, Rarieda Endline sample

	Control mean (SD) (1)	Treatment effect (2)
Age (respondent)	35.35	-1.26
	(14.13)	(0.87)
Female (respondent)	0.62	-0.00
	(0.48)	(0.03)
Marital status (respondent)	0.78	-0.00
	(0.42)	(0.03)
Years of education (respondent)	8.53	0.28
	(2.95)	(0.18)
Number of children	2.88	0.05
	(1.92)	(0.12)
Household size	4.94	0.03
	(2.16)	(0.13)
Land owned (ha)	1.31	-0.02
	(1.57)	(0.11)
Log value of non-land assets	6.26	-0.03
	(0.96)	(0.06)
Log total expenditure	5.73	-0.02
	(0.68)	(0.04)
Log food expenditure	5.27	0.01
	(0.80)	(0.05)
Observations	502	505

Notes: The table presents OLS estimates of baseline differences between treatment and control groups. Outcome variables are listed on the left. For each outcome variable, we report the coefficients of interest and their standard errors in parentheses. Column (1) reports mean and standard deviation for the control group. Column (2) compares treatment households to control households. Standard errors are shown in the parentheses.

D. Engel curves for treatment and control

Our instrument variable approach implicitly assumes that the unconditional cash transfers only affect endline food budget shares through total expenditure. One way to think about this assumption is that the analysis interprets changes in food consumption as movements *along* a constant underlying Engel curve for food.

Clearly, the exclusion restriction cannot be tested. Still, for completeness and comparability to other studies on (conditional) cash transfers (see e.g. Angelucci and Attanasio, 2013; Attanasio et al., 2012; Schady and Rosero, 2008), we investigate whether the unconditional cash transfers in this study led to changes in the Engel curves.

We follow Attanasio and Lechene (2010) and estimate the following specification:

$$\omega_{hv} = \alpha + \delta T + (\beta + \phi T) \ln z^*_{hv} + \gamma(\ln p^f_v - \ln p^n_v) + \xi' \mathbf{X_{hv}} + \varepsilon_{hv}, \tag{D.1}$$

where ω_{hv}, as before, denotes the budget share for food, and z^*_{hv} denotes total household expenditure adjusted for village-level prices. The indicator variable, T, denotes treated households. We estimate the specification for our main across-village specification using OLS, i.e., we do not use the unconditional cash transfers as an instrument.

Estimates are shown in Table A6. The even numbered columns display the main coefficients of interest from Equation (D.1), while the odd numbered columns – for comparison – display the β-coefficients from a similar specification without the indicators for treatment households.

In contrast to much of the literature on conditional cash transfers, we find no signs of shifts in the Engel curves: the intercepts increase somewhat in all three data sets ($\delta > 0$), while the slopes become slightly steeper ($\phi < 0$), but all the estimated coefficients are close to zero and they are not statistically significant. To be clear, the results presented in this section do not *prove* that the Engel curves were unaffected by the cash transfers in our study, but they are still – if nothing more – consistent with such an interpretation.

Table A6: Tests of shifts in Engel curves for food expenditure

	Nakuru Endline II		Nakuru Endline I		Rarieda Endline	
	(1)	(2)	(3)	(4)	(5)	(6)
β	-0.052***	-0.049***	-0.030***	-0.024**	-0.027***	-0.025**
	(0.010)	(0.017)	(0.009)	(0.011)	(0.010)	(0.012)
δ		0.015		0.064		0.007
		(0.104)		(0.090)		(0.097)
ϕ		-0.004		-0.016		-0.003
		(0.019)		(0.018)		(0.019)
Observations	903	903	1994	1994	1392	1392

Notes: The table presents OLS estimates based on treatment and pure control households. The even columns present the key coefficients of interest from Equation (D.1), while the odd columns display the β-coefficients from a similar specification without the indicators for treatment households.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

E. Elasticities for sub-categories of food and nutrients

In this section, we present expenditure elasticities for five sub-groups of food and four different nutrients.

E.1 Food sub-categories

We start by exploring the following sub-groups of food expenditure: i) *Cereals*; ii) *Fruits, vegetables, pulses and roots*; iii) *Meat, fish, diary products*, and iv) *Other food*. We compute the overall budget share for each of these categories as household expenditure on the particular category over total household expenditure. We then run our baseline regression from Equation (1) in the main paper and compute the expenditure elasticities according to the formula in Equation (2). Results are presented in Table A7.

The first row in the table reproduces the expenditure elasticities for overall food consumption. The second row displays estimates for the sub-category *Cereals*. This elasticity is estimated to be 0.78 in Nakuru Endline II and 0.70 in Rarieda Endline, which is lower than for overall food expenditure, suggesting that the category contains staples. The elasticity for *Fruits, vegetables, pulses and roots* is close to unity in both surveys, while the one for *Meat, fish, diary products* is estimated to be 1.50 in Nakuru Endline II and 1.38 in Rarieda Endline, consistent with this category containing luxury goods. Finally, the elasticity for the category *Other food* is estimated to be 0.43 in Nakuru Endline II and 0.86 in Rarieda Endline.

In Column (3), we present estimates based on regressions that controls for relative price measures *specific* for each sub-category of food. We are able to compute such sub-category price measures for the Nakuru Endline II household survey, as we have a rich set of item prices linked to this survey. We first construct price indices for each category following the same steps as for the overall food price index (see Section 3.2 of the main paper). We then calculate relative price measures (within each village) as the price index of the particular food category relative to all other consumption goods and plug this into the budget share regression. To give an example, for the category *Cereals*, the budget share regression controls for relative prices of cereals versus other goods (food and non-food), as opposed to food versus non-food in the baseline specification. By comparing Column (1) and (3), we see that we obtain very similar elasticities using this alternative specification.

Table A7: Expenditure elasticities of sub-food expenditure

	Main price controls		Category-specific price controls
	Nakuru Endline II (1)	Rarieda Endline (2)	Nakuru Endline II (3)
Cereals	0.779 (0.247)	0.697 (0.382)	0.777 (0.245)
Fruits, vegetables, pulses and roots	0.975 (0.452)	1.034 (0.302)	0.889 (0.449)
Meat, fish, diary products	1.496 (0.458)	1.384 (0.318)	1.480 (0.452)
Other food	0.793 (0.260)	0.202 (0.264)	0.801(0.252)
Observations	1,392	903	1,392

Notes: The table displays elasticities derived from the across-village specification with price controls. The elasticities in Column (3) are based on the sub-category specific price controls. Each elasticity is evaluated at the mean household budget share for the particular sub-category. Standard errors, clustered at the village level, are shown in parentheses.

E.2 Expenditure elasticities of nutrients

We next estimate expenditure elasticities for the following nutrients: *Protein*, *Fat*, *Carbohydrates* and *Fiber*.

The first row of Table A8 reproduces the estimates for calories presented in the main paper. For proteins we find an elasticity of 0.95 in Rarieda Endline and an elasticity as small as 0.50 in Nakuru Endline II. This relatively low estimate in Nakuru is somewhat surprising. In our setting, it could – at least partly – be explained by households reducing their (relative) consumption of maize grain, which is the main food staple in rural Kenya and which contains relatively many proteins. The effect of this seems to dominate the effect from increased consumption of meat and diary products, which contain much more protein. For fat we find an elasticity of 0.78 in Nakuru Endline II and 0.66 in Rarieda Endline, and for carbohydrates an elasticity of 0.65 and 0.54, respectively. Finally, the estimated elasticity for fiber is 0.39 in Nakuru Endline II and 0.85 in Rarieda Endline.

The estimated nutrient elasticities thus differ somewhat across the two experiments. Our preferred estimates are those from Nakuru Endline II, as these estimates are based on a much richer consumption questionnaire than those from Rarieda Endline.

We also estimate an alternative specification in which we explicitly control for the cost of different nutrients.[23] We are able to do this for the Nakuru Endline II household survey, using the following steps. In the first step, we compute the cost per nutrient for each food item, as the village market price (from our main price index) over the nutrient content of the same food item. We can do this since the village prices already refer to a common unit for each product, i.e., each village price refer to a product of a particular weight. This step thus gives us a price per nutrient for each food item in each village. In the next step, we aggregate the nutrient prices into overall indices based on the Weighted-Country-Dummy-Product-Method (Rao, 1990, 2005), using product-village as the unit of observation. As weights in this regression we use the share of total intake of each nutrient obtained from the particular food item, averaged over the full sample. To give an example, the weight of rice in the calorie price index is the average share of total calories households obtain from eating rice.

The procedure gives us a set of village-level indices for each nutrient (calories, proteins, fats, carbohydrates and fibers), which we plug into the different regressions instead of the relative food vs. non-food price measures. The resulting expenditure elasticities are shown in Column (3) of Table A8. As can be seen, the estimates are very similar to those in Column (1).

F. Robustness analysis

In this section, we conduct several robustness tests of our main estimates. We estimate elasticities using different equivalence scales; we change the set of household controls; and we calculate elasticities at different points of the food budget share distribution. In all tests, we focus on our main across-village specification, which explores village-level variation in exposure to the unconditional cash transfers.

[23] This analysis was not specified in the PAP.

Table A8: Expenditure elasticities of nutrients

	Main price controls		Nutrient price controls
	Nakuru Endline II (1)	Rarieda Endline (2)	Nakuru Endline II (3)
Calories	0.668 (0.256)	0.616 (0.177)	0.631 (0.275)
Protein	0.503 (0.352)	0.949 (0.199)	0.423 (0.379)
Fat	0.776 (0.288)	0.662 (0.205)	0.800 (0.298)
Carbohydrates	0.648 (0.301)	0.539 (0.217)	0.622 (0.323)
Fiber	0.389 (0.401)	0.847 (0.258)	0.366 (0.431)
Observations	1,392	903	1,392

Notes: The table displays elasticities derived from the across-village specification with price controls. The elasticities in Column (3) are based on the nutrient specific price controls. Standard errors, clustered at the village level, are shown in parentheses.

F.1 Equivalence scales

In our baseline specifications, we use the log of *total* household expenditure as the main independent variable. As a first set of robustness tests, we replace this with the log of household expenditure adjusted for household size and composition. We adjust the unconditional cash transfer (the instrument) in a similar fashion. Estimates are shown in Columns (1)-(4) of Table A9.

In the first column, we divide total expenditure and the cash transfer by the number of individuals in the household. This does not affect our estimates much. In the second column, we make use of the OECD equivalence scale. This equivalence scale gives a weight of 1 for the first household member aged 14 years and over; a weight of 0.7 to each additional household member aged 14 years and over; and a value of 0.5 to each child who is under 14 years old. In the third column, we use the modified OECD scale, which gives a weight of 1 for the first household member aged 14 years and over; a weight of 0.5 to each additional household member aged 14 years and over; and a weight of 0.3 to each child who is under 14 years old. In the fourth column, we adjust total household expenditure by the square root of the number of household members. Overall, our estimates are robust to the choice of equivalence scale: the estimates from the Nakuru Endline I and II are very similar to our baseline estimates, while those from Rarieda Endline are somewhat more sensitive.

F.2 Household controls

We next explore the robustness to the choice of household controls. In Column (5) of Table A9, we remove the baseline household controls completely (the number of adults and the number of children at baseline), and in Column (6) we add additional controls (age and gender of the primary respondent, marital status of the primary respondent, highest level of education attained by the primary respondent, and the amount of land owned by the household). Unsurprisingly, given that the allocation of cash transfers was randomized, and given that we have baseline balance (see Section C), this does not affect the estimated elasticities much. The only exception is the calorie elasticity obtained in the Rarieda Endline, which becomes somewhat smaller when we include the large set of household controls.

F.3 *M-Pesa* access

In the final column of Table A9 we exclude households that did not have an account at the money service *M-Pesa* at baseline. The motivation for this sensitivity test is that the cash transfers were sent through *M-Pesa*, and those without a personal *M-Pesa* account were offered a cell phone at prevailing retail prices and guided on how to set up the account. As explained in the main paper, this applies for relatively few households. Comfortingly, removing these households does not change our estimated elasticities much: the food expenditure elasticity becomes somewhat smaller in Nakuru Endline I and II, while the calorie elasticity in Nakuru Endline II increases slightly. We do not report similar estimates for Rarieda Endline, as very few of the participants in this experiment had *M-Pesa* at baseline (in 2011). Haushofer and Shapiro (2016) document that the cash transfer treatment (which included access to *M-Pesa*) did not change the use of the money service, suggesting that our estimates are not driven by *M-Pesa* access in a meaningful way.

F.4 Different evaluation points for expenditure elasticities

Since the expenditure elasticity depends on the budget share for food, there is a question of where in the distribution of budget shares to to report the elasticities. For our baseline estimates we use the average budget share within each estimation sample. In Table A10, we present four alternatives to this.

In Column (2), we present elasticities evaluated at the median budget share for food within each sample. This does not affect our estimates. In Column (3), we use the lowest decile of the distribution of budget shares, and in Column (4) we use the highest decile. Unsurprisingly, the elasticities are smaller in the first case, but the differences are not large. Finally, we calculate elasticities using household-specific budget shares to find the full distribution of elasticities. In Column (5), we present the average elasticity from this distribution. Again, the estimates are very similar to our baseline estimates.

In all, we therefore conclude that the estimated elasticities are robust to different evaluations of the budget share for food.

Table A9: Robustness analysis I: Equivalence scales and household controls

	Per capita	OECD equivalence scale	OECD modified scale	Square root scale	No HH controls	Extra HH controls	Excl. HHs without M-Pesa
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Nakuru Endline II							
Food expenditure	0.850	0.851	0.866	0.860	0.872	0.869	0.799
	(0.150)	(0.149)	(0.144)	(0.144)	(0.132)	(0.129)	(0.142)
Calories	0.613	0.619	0.634	0.647	0.678	0.656	0.714
	(0.285)	(0.284)	(0.277)	(0.274)	(0.261)	(0.251)	(0.249)
Observations	1392	1392	1392	1392	1392	1392	1178
First-stage F-stat	9.7	9.8	10.3	10.6	9.7	11.6	11.5
Rarieda Endline							
Food expenditure	0.789	0.887	0.900	0.789	0.828	0.809	
	(0.124)	(0.060)	(0.054)	(0.121)	(0.089)	(0.130)	
Calories	0.601	0.786	0.808	0.603	0.688	0.558	
	(0.189)	(0.095)	(0.087)	(0.185)	(0.143)	(0.212)	
Observations	903	903	903	903	903	903	
First-stage F-stat	8.9	33.5	39.2	9.5	13.6	7.4	
Nakuru Endline I							
Food expenditure	0.853	0.855	0.854	0.856	0.861	0.867	0.805
	(0.061)	(0.060)	(0.060)	(0.060)	(0.057)	(0.058)	(0.067)
Observations	1,994	1,994	1,994	1,994	1,994	1,994	1,672
First-stage F-stat	38.8	39.2	39.5	39.4	38.0	37.9	29.9

Notes: The table displays elasticities derived from the across-village specification with price controls. The food expenditure elasticities are evaluated at the mean household budget share for food. Column (1) is based on a regression where we convert household expenditure and the cash transfers to per capita. Columns (2)-(4) similarly use different equivalence scales. Column (5) ignores the household baseline controls, while Column (6) is based on a more extensive list of household baseline controls. Column (7) is based on the exclusion of households without *M-Pesa* at baseline. Standard errors, clustered at the village level, are shown in parentheses.

Table A10: Robustness analysis II: Food expenditure elasticities using different evaluation points

	Mean budget share food	Median budget share food	1. decile budget share food	10. decile budget share food	Mean elasticity
	(1)	(2)	(3)	(4)	(5)
Nakuru Endline II	0.872	0.877	0.804	0.899	0.854
	(0.135)	(0.129)	(0.205)	(0.106)	(0.153)
Rarieda	0.795	0.799	0.712	0.838	0.767
	(0.116)	(0.113)	(0.162)	(0.091)	(0.131)
Nakuru Endline I	0.860	0.867	0.785	0.892	0.838
	(0.059)	(0.056)	(0.090)	(0.046)	(0.068)

Notes: The table displays food expenditure elasticities derived from the across-village specification with price controls. Column (1) reproduces our main estimates, where we evaluate the food expenditure elasticities at the mean household budget share for food. In Column (2) we evaluate elasticities at the median budget share. In Columns (3)-(4) we evaluate the elasticities based on first decile and tenth decile of the budget share distribution, respectively. In Column (5) we calculate household specific elasticities and present the mean food expenditure elasticity. Standard errors, clustered at the village level, are shown in parentheses.

F.5 Alternative price measures

In the main paper, we construct price indices based on data from local marketplaces. In this section, we present several alternative price measures and explore whether our estimates are sensitive to them.

Quality-adjusted prices

We start by computing price measures that adjust for quality effects in the underlying consumption goods. Rich households may purchase items of higher quality than poor households. In our case, this may pose a challenge especially for the non-food price measures, as these prices cover products that are likely to be heterogeneous in terms of quality.

The main challenge is that product quality is unobserved. We therefore use the regression-based method suggested by Deaton et al. (2004). For each item i, we run the following regression:

$$\text{ln} p_{ihv} = \beta \text{ln} y_{hv} + \sum_{v} d_v D_v + \epsilon_{ij}, \tag{F.1}$$

where p_{ihv} denotes the price of item i paid by household h, y denotes total household expenditure, while D_v denotes a set of village dummies. The β-coefficient in the regressions can be interpreted as an expenditure elasticity of quality. We derive the quality-adjusted prices from the estimated d_v-coefficients, plus the expenditure term evaluated at the median of the sample. We then aggregate to overall price indices, just as before.

Summary statistics of this price measure are shown in Table A11. As can be seen, both the food and non-food index exhibit a somewhat smaller variance across villages as compared to our unadjusted price measure. In Column (2) of Table A12, we present the food expenditure and the calorie elasticities we derive when using these price measures in our main regressions. The estimates are almost identical to our baseline estimates, which are reproduced in the first column of the table.

Unit values

Many studies lack *price* data (i.e., prices households face in the market), and therefore use *unit values* (i.e., what households actually pay for specific items) as an alternative. It is therefore useful to compare unit values with actual prices, since we have access to both in our setting. We do this by computing unit value price measures for the Nakuru Endline II household survey. This survey data includes information on expenditure and quantities for all food items and we use this to calculate unit values as expenditure over quantities for each item (see e.g. Deaton and Dréze, 2002).

We compute two different price indices based on the unit values. For the first index, we use the median unit value for each item within each village. We then use the Weighted-Country-Dummy-Product-Method for aggregation to overall price indices, as for our main price index. For the second index, we adjust the unit values for potential quality differences using the framework described above. This adjustment is likely to be of more importance for the unit values than for our market prices, as the unit values are explicitly linked to purchases of households with varying expenditure.

Summary statistics of the unit value indices are shown in Table A11, while the subsequent elasticities are shown in Columns (3) and (4) of Table A12. All elasticities are similar to our baseline estimates, except the calorie elasticity based on the unadjusted unit value indices. In sum, the results still suggest that unit values can sensibly be used to estimate elasticities, at least in our setting.

Table A11: Summary price statistics

	Nakuru Endline II			Rarieda Endline II		
	SD (1)	Min (2)	Max (3)	SD (4)	Min (5)	Max (6)
Food prices						
Main	0.11	0.55	1.39	0.22	0.56	1.68
Quality-adjusted	0.10	0.61	1.32	0.19	0.58	1.53
Unit values	0.10	0.68	1.44			
Unit values quality-adjusted	0.10	0.69	1.36			
Non-food prices						
Main	0.22	0.56	1.68	0.10	0.68	1.44
Quality-adjusted	0.19	0.58	1.53	0.10	0.69	1.36

Notes: All price indices are normalized to unity. Columns (1)-(3) are based on 120 villages in Nakuru, and Columns (4)-(6) are based on 120 villages in Rarieda.

Table A12: Robustness analysis: Alternative price measures

	Market prices, main (1)	Market prices, adjusted (2)	Unit values (3)	Unit values, adjusted (4)
Nakuru Endline II				
Food expenditure	0.872	0.869	0.861	0.871
	(0.135)	(0.138)	(0.149)	(0.136)
Calories	0.668	0.684	0.756	0.664
	(0.256)	(0.260)	(0.262)	(0.243)
Observations	1,392	1,392	1,392	1,392
First-stage F-statistic	11.7	11.3	10.1	12.0
Rarieda Endline				
Food expenditure	0.795	0.797		
	(0.116)	(0.112)		
Calories	0.616	0.574		
	(0.177)	(0.172)		
Observations	903	903		
First-stage F-statistic	10.2	11.0		

Notes: The table displays elasticities derived from the across-village specification with different price controls. The food expenditure elasticities are evaluated at the mean household budget share for food. Column (1) reproduces our main estimates. Column (2) is based on the quality-adjusted prices. Column (3) is based on the unit value price indices, and Column (4) is based on the quality-adjusted unit value indices. Standard errors, clustered at the village level, are shown in parentheses.

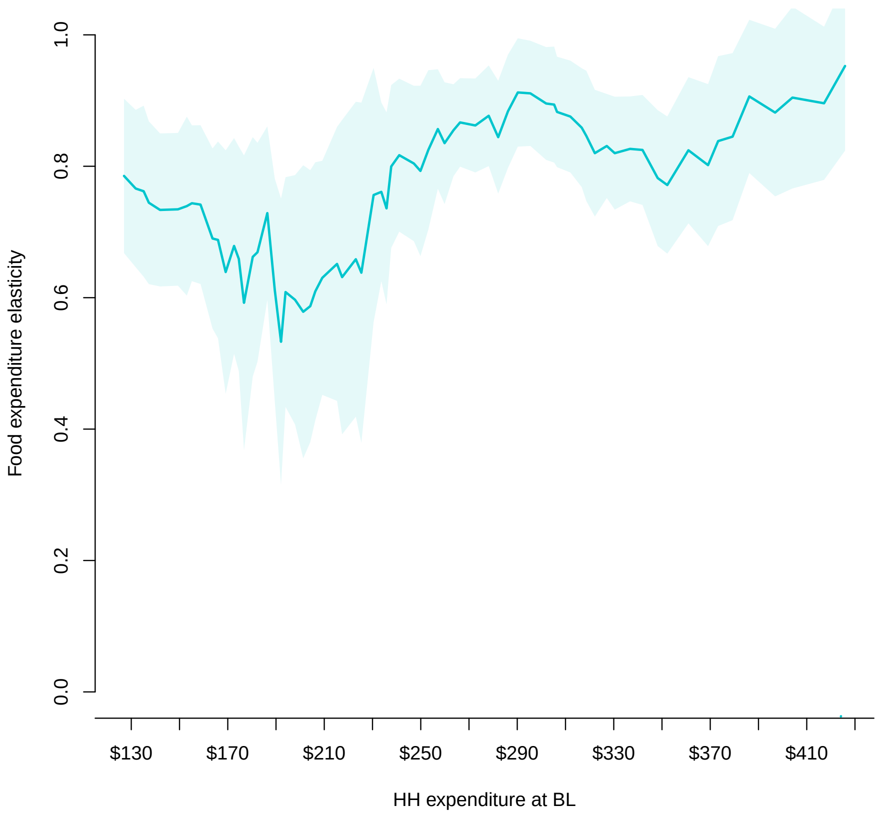
G. Heterogeneous effects

In this section, we consider the possibility that the elasticities differ for households at different positions along the expenditure distribution (Strauss and Thomas, 1990). We can address this question in the household data from Rarieda, as we have information on household expenditure in the Rarieda Baseline survey.

We proceed as follows. We begin by restricting the sample to the bottom 30 percent of households in terms of baseline expenditure, and compute the elasticity for that subsample. We then gradually slide this window of 30 percentage points width across the entire baseline expenditure distribution in steps of 1 percentage point. At each point, we compute the elasticity. We repeat this procedure until the window covers households between the 71st and 100th percentiles of the baseline expenditure distribution.

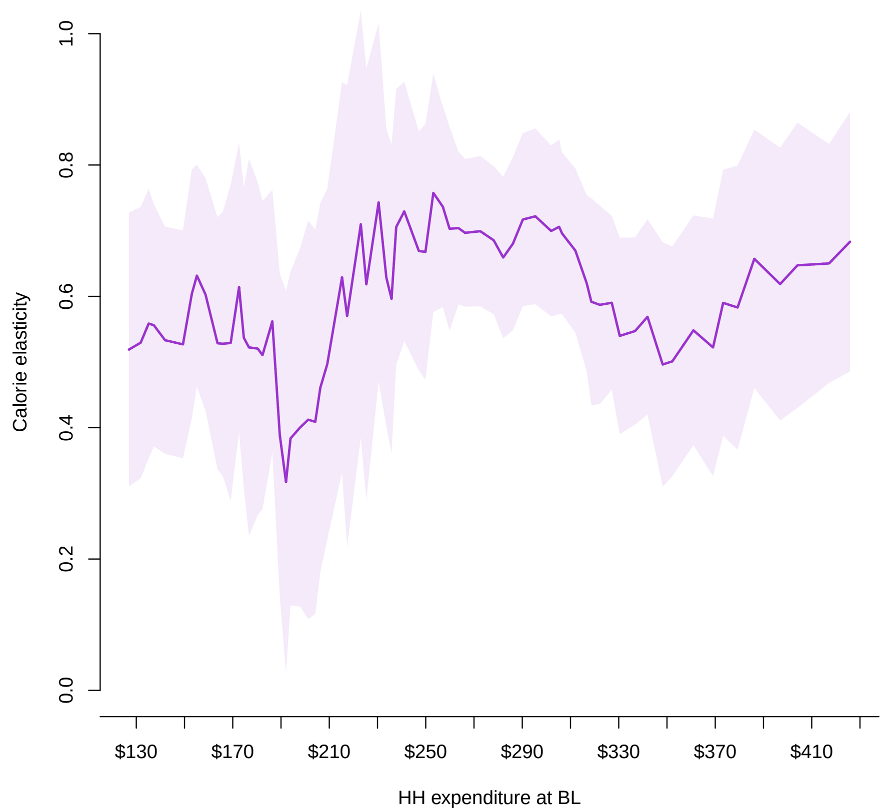
Figure A1 shows this running estimate for the expenditure elasticity of food expenditure, while Figure A2 shows similar estimates for the calorie elasticity. The x-axis denotes the center of the sliding window, from the 15th to the 85th percentile of the baseline expenditure distribution. We find that the elasticities vary somewhat across the distribution. Perhaps surprisingly, we find that the estimated food expenditure elasticity tends to be somewhat greater for households with higher baseline expenditure, as compared to households with lower baseline expenditure. The differences are not large, however. The estimated calorie elasticity does not exhibit the same pattern; the elasticity is largest for households around the middle of the distribution.

Figure A1: Food expenditure elasticity by baseline expenditure, Rarieda Endline



Notes: Food expenditure elasticity by baseline expenditure. The graph shows the elasticity as a function of household expenditure at baseline, obtained by restricting the sample to subsamples of 30 percent, beginning with the bottom 30 percent of households (i.e., from the 1st to the 30th percentiles) and then moving the window in steps of 1 percentage point to the top 30 percent of households (i.e., from the 71st to the 100th percentiles). At each step, we estimate the expenditure elasticity and evaluate it at the average budget share for food. Shaded areas denote one standard error.

Figure A2: Calorie elasticity by baseline expenditure, Rarieda Endline



Notes: Calorie elasticity by baseline expenditure. The graph shows the calorie elasticity as a function of household expenditure at baseline, obtained by restricting the sample to subsamples of 30 percent, beginning with the bottom 30 percent of households (i.e. from the 1st to the 30th percentiles) and then moving the window in steps of 1 percentage point to the top 30 percent of households (i.e., from the 71st to the 100th percentiles). At each step, we estimate the calorie elasticity. Shaded areas denote one standard error.

H. Income and assets as instruments

The Nakuru Endline I household survey includes detailed data on household income and asset holdings. In this section, we use this information to construct alternative instruments for total household expenditure. This is a useful exercise, as the previous literature, which often cannot capitalize on random variation from cash transfers, has used income and assets as instruments for expenditure (Babatunde et al., 2010; Fashogbon and Oni, 2013; Skoufias et al., 2012).

The first two columns of Table A13 present food expenditure elasticities using total wage income as an instrument. Column 1 is based on the full sample, while Column 2 is restricted to pure control households. The instrument is only weakly related to household expenditure and the first-stage F-values are well below conventionally used thresholds. The point estimate of the food expenditure elasticity is also above unity in both samples.

We next use total asset holdings of households as an instrument. We construct the asset variable using data on 30 asset categories, including productive assets, vehicles, household durables, livestock, and financial assets. The value of the different types of assets is defined as reported by the respondent (see Haushofer et al., 2020, for more details). As seen from the table, the asset measure is a much stronger instrument than wage income, with an F-value of 17 in the full sample and 12 in the sample with control households only. Yet, the point estimates of the food expenditure elasticity is much closer to the non-experimental elasticities reported in the main paper than to our preferred experimental estimates.

We conclude from this exercise that information on income or assets is of little help to precisely estimate elasticities in our setting.

Table A13: Elasticities using income or assets as instruments

	Income		Assets	
	All HHs	Control HHs	All HHs	Control HHs
	(1)	(2)	(3)	(4)
Nakuru Endline I				
Food expenditure	1.068	1.238	1.028	1.094
	(0.178)	(0.321)	(0.070)	(0.088)
Observations	1994	1473	1994	1473
First-stage F-statistic	6.9	2.7	17.0	11.7

Notes: The table displays elasticities derived from the across-village specification with price controls. The food expenditure elasticities are evaluated at the mean household budget share for food. Columns (1)-(2) use wage income as an instrument for household expenditure. Column (1) is based on treatment households and pure control households, while Column (2) is based on only the control households. Columns (3)-(4) use an asset index as an instrument for household expenditure. Column (3) is based on treatment households and pure control households, while Column (4) is based on only the control households. Standard errors, clustered at the village level, are shown in parentheses.

I. Survey details

In this section, we provide details on the consumption expenditure questionnaire used in Nakuru and Rarieda, respectively.

Table A14 displays the consumption categories and items used in the Nakuru questionnaire. The Endline I household survey collected expenditure data on the consumption categories only, while the Endline II household survey collected expenditure numbers for each item. Table A15 similarly presents the consumption categories used in the Rarieda Baseline and Endline surveys. Table A16, A17 and A18 present the consumption items in the Nakuru Marketplace Survey (I and II), the Rarieda Price Survey, and the Rarieda Marketplace Survey (I and II), respectively.

Table A14: Nakuru expenditure questionnaire

Categories	Items
Food, weekly recall period, level of expenditure: items	
Cereals	rice, maize grain, green maize, bread, wheat grain, other cereals
Roots	potatoes, sweet potatoes, cassava, other roots
Pulses	beans, grams, peas, cowpeas, other pulses
Vegetables	onions/leeks, cabagges, carrots, tomatoes, spinach, kale, other vegetables
Fruits	ripe banana, cooking banana, oranges, avocado, mangoes, melons, other fruits
Meat & fish	beef, mutton/goat, chicken, fresh fish, dried fish, offals, matumbo, kidney, liver, sausages, other meat and fish
Dairy	milk, eggs, other dairies
Oils	margarine, cooking fat, cooking oil, other oils
Sugars	sugar, sugar cane, other sugars
Jam	jam, maramlade, honey, chocolate, sweets, chewing gum
Spices	salt, tomato sauce, chili sauce or powder, baking powder, yeast, mustard, vinegar, pickles, pepper
Processed food	Vendor food, cafes/take-away, kiosks, restaurants, other processed food
Other food	tinned beans or pulses, soups, tinned fish, baby food, other
Non-food, weekly recall period, level of expenditure: items	
Non-alcoholic drinks	preserved fruit juice, tea, coffee, soda, soya drink, health drink, drinking chocolate, mineral water, other non-alcoholic drinks
Alcoholic drinks	spirits, wine, beer, brews, cider, other alcoholic drinks
Tobacco	cigarettes, cigars, tobacco, snuff, khatt or miraa
Non-food, monthly recall period, level of expenditure: categories	
Lottery tickets/gambling	
Clothing and shoes	
Personal items	
Household items	
Non-food, yearly recall period, level of expenditure: categories	
Fixing home damage, improving home	
Religious expenses or other ceremonies	
Weddings	
Funerals	
School fees, uniforms, books, other supplies	
Medical expenses	

Notes: The table displays the consumption categories and items used in the Nakuru questionnaire. Endline I collected expenditure data on the consumption categories only, while Endline II collected expenditure numbers for each item.

Table A15: Rarieda expenditure questionnaire

Categories
Food, weekly recall period
Cereals
Roots
Pulses
Vegetables
Fruits
Meat
Animal products
Fish
Dairy/eggs
Oils/fats
Sugar
Jam/sweets
Spices
Processed food
Other food
Non-food, weekly recall period
Non-alcoholic drinks
Alcoholic drinks
Tobacco
Non-food, monthly recall period
Airtime, internet, other phone expenses
Travel, transport, hotels
Lottery tickets/gambling
Clothing and shoes
Recreation/entertainment
Personal items
Household items
Firewood, kerosene, charcoal
Electricity
Water
Non-food, yearly recall period
House rent/mortgage
Fixing home damage, improving home
Religious expenses or other ceremonies
Weddings
Funerals
School fees, uniforms, books, other supplies
Medical expenses
Household durables
Bride price

Notes: The table displays the consumption categories used in the Rarieda Baseline and Endline.

Table A16: Nakuru Marketplace Survey I and II data

Nakuru Marketplace Survey I	
Avocado	Maizegrain
Babyfood	Mangos
Beans	Margarinee
Beef	Melons
Beer	Milk
Bread	Miwa
Brewsbusaa	Muttongoat
Cabagges	Offalsmatu
Carrots	Onionsleek
Cassavaand	Oranges
Chakulayam	Peas
Chicken	Potatoes
Cigars	Restaurant
Cookingban	Rice
Cookingfat	Ripebanana
Cookingoil	Soups
Cowpeas	Spinach
Driedfish	Spirits
Eggs	Sugar
Freshfish	Sweetpotat
Grams	Tobacco
Kale	Tomatoes
Kiosks	Wheatgrain
Maharagwey	Wine
Mahindimbi	
Nakuru Marketplace Survey II	
Shoes (1 pair)	School textbook
Haircut	School fees (1 semester)
Toothpaste	Doctor consultation fee
Soap bar	Wedding
Matches	Funeral
Hiring somebody to repair a leaky roof	

Notes: The upper panel of the table displays the items used the Nakuru Marketplace Survey I. The bottom panel shows the items in the Nakuru Marketplace Survey II.

Table A17: Rarieda Price Survey data

Arrowroot	Oranges
Avocados	Paraffin
Beans	Passion fruit
Cabbage	Pawpaw (papaya)
Cassava	Pilipili
Cooking banana	Pineapple
Cowpeas	Potato
Eggplant	Pumpkin
Firewood	Small banana
Haircut	Soap bar
Kale	Spinach
Large banana	Sugar
Maizegrain	Sweetpotato
Mangos	Tilapia
Mudfish	Tomatoes
Onions	Watermelon

Notes: The table displays the consumption items the Rarieda Price Survey.

Table A18: Rarieda Marketplace Survey I and II data

Rarieda Marketplace Survey I	
Preserved fruit juice (Delmonte)	Pepper
Tea (Ketepa)	Jam (Zesta)
Coffee (Nescafe)	Marmalade (Zesta)
Soda (Coke)	Honey
Soya Drink	Chocolate (Cadburys)
Health Drink (Mwarubaini)	Sweets (Tropical)
Drinking Chocolate (Cadburys)	Chewing gum (Orbit)
Mineral Water (Dasani)	Avocado
Salt (Kensalt)	Beans (1kg)
Tomato Sauce (Peptang)	Sugar (1kg)
Chilli sauce powder	Pilipili (One bunch)
Baking powder	Eggs (One dozen)
Yeast	Meat (1kg of beef)
Mustard	Margarine (1kg of Blueband)
Vinegar	Cooking fat (1kg of kasuku cooking fat)
Pickles	
Rarieda Marketplace Survey II	
Bunch of kales	Medium avocado
Bunch of spinach	Bunch of passion fruits
4 medium tomatoes	Medium pawpaw
4 medium onions	Medium water melon
Medium head of Lettuce	Orange
Medium head of cabbage	5 medium cassavas
Medium pumpkin	5 medium arrow roots
Bunch of chillies	5 medium potatoes
Medium piece of eggplant	5 medium sweet potatoes
Medium mango	Kunde- cowpea leaves
Dozen small (ripe) banana	Mchicha-amaranthus
Dozen large (ripe) banana	Mrenda
Dozen cooking banana	Managu
Medium pineapple	1 kg sugar
Packaging polethene	Luggage boy
Gunia	

Notes: The table displays the consumption items the Rarieda Marketplace Survey I and II. The former survey collected prices of the different items, while the latter collected data on the weight of the different items-units.

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