Direct and Indirect Effects of Malawi's Public Works Program on Food Security*

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Abstract

Labor-intensive public works programs are important social protection tools in low-income settings, intended to supplement income of poor households and improve public infrastructure. We conduct a unique evaluation of an at-scale government program, the Malawi Social Action Fund, using across- and within-village randomization to estimate the effects of the program on food security and use of agricultural inputs. We find no evidence that the program improves food security, and instead find some evidence of negative spillovers to untreated households. Modifications that offer work during the lean rather than harvest season or increase payment frequency do not improve program impacts.

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1 Introduction

Labor-intensive public works programs, sometimes referred to as workfare programs, are an important social protection tool in low-income settings (Grosh et al. 2008). By requiring participants to work in order to receive a benefit (be it cash or an in-kind transfer), they hold appeal for different reasons. In theory, by design, the offer of low wages acts a screening mechanism to reach households who need it the most, by allowing poor participants to self-select into the program and removing the necessity of implementing complicated targeting schemes on the ground (Besley & Coate 1992). In addition, public works programs have the potential for providing indirect benefits to beneficiaries through the value of assets created by building or repairing community infrastructure, and in some contexts, the value of assets created is necessary to justify a workfare style program instead of transfers to poor households (Murgai, Ravallion & vandeWalle Forthcoming).

Well-known public works programs include the Employment Guarantee Scheme in Maharashtra (Ravallion, Datt & Chaudhuri 1993) and the National Rural Employment Guarantee Act (NREGA) in India (Dutta et al. 2014), and the Productive Safety Net Project in Ethiopia (Hoddinott et al. 2012). Such programs are widespread – albeit on on as large a scale – in sub-Saharan Africa, where McCord & Slater (2009) identified 167 programs across 29 countries.

Recent evidence from the implementation of a tutoring program in Kenya suggests that the same program can have very different effects under different implementation conditions (Bold et al. 2013). Large-scale programs generally operate as public sector programs with high degrees of decentralization, and in many different areas of a country. Despite the pervasiveness of public works in low-income countries and the extensive descriptive and theoretical literature on them, there is scant evidence from rigorous empirical studies on the impact of such programs in African countries, and to our knowledge no information about sub-national variation in the impact of public works programs. Variation in impacts may arise from differences in the implementation itself; differences in local economic conditions; or differences socio-economic conditions of beneficiaries.

There is a growing literature about NREGA in India. Imbert & Papp (2015) find that the availability of the public works program provides an income floor by increasing wages for both participants and non-participants, but crowds out private sector work. (Ravi & Engler 2015) report that when jobs were rationed, households offered work have higher food expenditures and lower food security than households unable to participate. NREGA is intended to function as a true safety net program by guaranteeing employment whenever households need it.[1] Zimmermann (2014) finds evidence that the program functions as

[1] Dutta et al. (2014) documents how rationing of the program limits its effects on the poorest households.

intended by helping households smooth against income shocks.

This paper studies Malawi's large-scale public works program, which operates under the Malawi Social Action Fund (MASAF). The public works program (PWP) has been operational since the mid-1990s and differs from NREGA in that it aims to provide short-term labor-intensive activities to poor, able-bodied households for the purpose of enhancing their food security instead of guaranteeing employment throughout the year. The implementation of the program is highly decentralized, with funding allocated to each of Malawi's 31 districts based on population and poverty estimates. The food security objective is to be achieved mainly through increased access to farm inputs at the time of the planting period. The program is designed to complement Malawi's large-scale fertilizer input subsidy program by synchronizing the availability of public works employment with the availability of fertilizer coupons at planting season. Our experimental design explores the hypothesis that changes to the timing of the program could increase its effect on food security, but potentially at the cost of investment in fertilizer.

Past evaluations have highlighted some of the challenges of undertaking a rigorous study of the impacts of the MASAF-PWP (Chirwa, Mvula & Dulani 2004, Jimat Consult P/L 2008). To this end, the Government of Malawi's Local Development Fund Technical Support Team partnered with researchers to design and implement an impact evaluation that would address these challenges. The effort is unique in at least two dimensions: the evaluation is a randomized-control trial and the effort was embedded in an at-scale program. The evaluation was implemented in 2012-2013, during the main agricultural season.

The design included two levels of randomization, across villages and across households in treated villages. Treatment assignment was stratified at the regional level to enable estimates of the impact of the program across different agro-ecological and socio-economic settings within the country. We use household panel survey data to study the impact of the program (and design variants) on food security and use of agricultural inputs, as well as other measures of household resource allocation and wellbeing in order to understand the mechanisms through which the program affects these two key outcomes.

While Malawi's PWP shares many elements with public works programs in the region, it was not effective in achieving its aim of improving food security during the 2013 lean season. Throughout the country, treated households have no better food security than households in control villages. However, there is notable geographic variation in the indirect effects on untreated households. In the North, these spillovers are negative and meaningful: a summary measure of food security falls by one-third of a standard deviation for untreated households in villages with PWP, relative to households in control villages. In Central Malawi, there is a compelling pattern of negative but imprecisely estimated indirect effects on untreated households. These spillovers are not present in the poorest region, the South.

These results are in contrast to evidence of positive spillover effects of social protection programs in other settings. Imbert & Papp (2015) and Deiniger & Liu (2013) find evidence of a general equilibrium effect of the employment guarantee scheme in India working through an increase in the casual wage rate, with positive spillover effects to the income of the poorest quintiles. Angelucci & DeGiorgi (2009) document positive spillover effects of the Oportunidades program in Mexico to households ineligible for the program living in the same villages. Their indirect effects operate through risk sharing, with ineligible households being able to consume more through an increase in transfers and loans from family and friends in the community. The low levels of risk sharing we detect in our results are inconsistent with the hypothesis of a crowding-out effect of risk sharing networks as a response to the program. Additionally, we do not find evidence of a negative pecuniary externality through price increases which may result from an injection of cash into the community.

A signature feature of Malawi's PWP is its timing: providing work opportunities at planting season is designed to facilitate linkages with the country's input subsidy scheme. We find no evidence that the unconventional schedule is achieving the intended effects. Households invited to participate in the program are more likely to receive the fertilizer coupon (as a result of the interlinkage with the fertilizer subsidy), and hence pay less for the fertilizer they use. However, in two of the three regions, there is no change in the quantity of fertilizer used by households in villages with PWP activities compared to households in control villages. In the North, fertilizer use increases in villages with PWP activities – but by an equal amount for households included in and excluded from the program. This pattern of increased investment by untreated households in the North may help to explain the large negative spillover effects in that region. Regional heterogeneity is key to understanding the mechanisms through which Malawi's PWP has unintended consequences, and to designing better alternatives.

The paper is organized as follows. Section 2 describes the program and the design of the evaluation. Section 3 describes the data and outcomes of interest. In Section 4 we outline the empirical strategy and the identification of the parameters of interest. We explain our analytic strategy in Section 4, and discuss the results in Section 5. Section 6 discusses potential mechanisms for direct and indirect effects of the program. Section 7 concludes.

2 MASAF program and experimental design

The MASAF PWP has been operational since the mid-1990s and aims to provide short-term labor-intensive activities to poor, able-bodied households for the purpose of enhancing their food security, mainly through increased access to farm inputs at the time of the planting period. This program is an important and interesting case, for several reasons. First, the program was designed to be interlinked with Malawi's large-scale fertilizer input subsidy program (known as FISP) through the implementation of the PWP in the planting months of the main agricultural season when the FISP distribution also occurs. The premise behind this is that the PWP facilitates poor, credit-constrained households to be able to finance the purchase of productive inputs (fertilizer). This distinguishes Malawi's program from traditional PWP design with implementation during the lean season.

The MASAF program covers all districts of Malawi with a two-stage targeting approach. In the first stage there is pro-poor geographic targeting, and in the second there is a combination of community-based targeting and self-selection of beneficiaries. The amount of funds given to a district is proportional to the district's population and to the poverty rates as well as other measures of vulnerability. District officials then target a sub-set of extension planning areas (EPAs) based on poverty and vulnerability criteria. Traditional Authorities (TAs) in the EPAs then allocate funds to a subset of selected Group Village Headmen (GVH) who each oversee between 3-10 villages. The GVH determines how many households will participate in each village based on available funding; the GVH then works with the village committees in each village to select participating households.

In 2012, as a response to a large currency devaluation, the program was doubled in size and scaled up to cover about 500,000 households per year. The duration of the project participation increased from 12 days to 48 days, split in two cycles of 24 days each; the cycles are further divided into two consecutive 12-day waves and payments are generally made within one to two weeks of the end of each wave. Projects were mostly road rehabilitation or construction, with some work on afforestation and irrigation projects. The wage rate was MK300/day (US$0.92/day) for a total payment of MK 3,600 for a 12 day wave (US$11.01).

Cycle 1 of PWP is implemented during the planting season (October to December) each year, to align with the timing of distribution of the fertilizer input subsidy program (FISP). Cycle 2 of PWP takes place post-harvest, in June and July.

2.1 Experimental design

We use a randomized controlled trial to test variants of the PWP that are budget-neutral in terms of direct costs. Villages were stratified by geographic region and randomly assigned (by computer) to one of the four treatment groups or a control condition; households within treatment villages were randomly selected for the program.

2.1.1 Village randomization

The villages in our sampling frame were randomly assigned to one of five groups (see Figure 1). The first of these groups is a control group (Group 0) of villages that do not participate in the PWP program in the 2012-2013 Season. Groups 1 through 4 participate in the PWP in the planting season (Cycle 1 of PWP). These four groups vary in terms of the timing of the second cycle of the program and the schedule of payments in both cycles.

Comparing the four treatment groups to the control group measures the impact of the 1st cycle of the program (during the planting season) on consumption patterns and farming investments of households. Among PWP-participating villages, two variants are tested:

- *Timing of the program* PWP is designed to take place for two cycles of 24 days, during planting and during post-harvest season. In our evaluation design, we maintain the first cycle at the planting season, and vary the timing of Cycle 2 to take place pre-harvest, during the lean season (February-March), instead of during/after the harvest season (May onward). Comparing Groups 1-2 and Groups 3-4 measures the consumption smoothing or buffer role of PWP during the lean season.

- *Schedule of the payments* We introduced a variation in the payment approach from a lump-sum payment made after 12 days of work to a split-payment variant. Under the split-payment alternative, participants are paid three days apart, in five equal installments of MK 720 each.[2] The variation of the payment schedule was motivated by extensive qualitative work done in preparation of the design of this project showing that households treat the lump-sum payments of the PWP differently from income generated through short-term casual labor (day-labor activities referred to as "ganyu" in Malawi). Comparing Groups 1 and 3 to Groups 2 and 4 will allow us to compare whether lump-sum payments alter the patterns of consumption and investment during the planting or lean season.[3]

[2] The market price of fertilizer in Malawi at the time of this project was approximately MK 5000 for a 50 kg bag. The national fertilizer subsidy program provided roughly half of households in the country with coupons that allow two bags of fertilizer to be purchased for MK 500 each. Because households face high transaction costs when redeeming their fertilizer coupons, including transportation costs, long wait times, and inflexibility in the days on which fertilizer can be purchased at the government shops, it is substantially more efficient to purchase both bags of subsidized fertilizer at once, for MK 1000 plus transportation costs (which are likely to range between MK 200 and MK 500). While the lump sum of MK 3600 more than covers the cost of purchasing two bags of subsidized fertilizer, a single incremental payment of MK 720 does not.

[3] Note that payments in the study districts were facilitated by the research team for the purposes of the evaluation, with physical delivery of the cash in conjunction with the district officials. Although the split-payment variant slightly increased the cost of implementation, the project variant was introduced to establish to test the proof of concept of whether the timing of the payment would alter the pattern of consumption and investments in this context. Under discussion for the scale-up was an e-payments which would have entailed a small marginal cost of delivery.

> Payments in the study districts were facilitated by the research team for the purposes of the evaluation. This was intended to ensure that payments were made without delay, on specific schedules. Administrative payment records confirm that there are no differences in time lag between work and payment across the districts.

The cross-randomization is designed to test interaction effects between the two design variants, since the payment schedule may have a differential effect depending on the season. While a lump-sum payment may facilitate investment in a lumpy input in December, split payments may help smooth consumption during the lean season. A lump sum in February may be used for staples as well as temptation goods; divided payments can act as a form of commitment savings that will lead to smoother consumption of staples if people otherwise have high temptation to spend or high discount rates even over very short periods of time.

We study a total of 182 villages (EAs) across our five treatment groups (Figure 1). Twenty eight districts are included in the PWP program. For the evaluation, we randomly selected 12 districts, stratifying by the country's three geographic regions. The 12 districts are Blanytyre, Chikwawa, Dowa, Karonga, Lilongwe, Mangochi, Mchinji, Mzimba, Nsanje, Ntchisi, Phalombe, and Zomba. Within selected districts, the list of WP-eligible villages (pre-screened) villages from the District Council and Traditional Authorities was compared to nationally representative survey data from the Integrated Household Survey (IHS3) collected in 2010 and 2011. We identify the overlap between the sampled enumeration areas (EAs) in the IHS3 and the list of communities pre-selected for PWP projects in our 12 districts. For the villages selected for treatment, we randomly chose one project in the in the event that the village are mapped to two, in order to have unique village-project pairs. The geographical targeting of the program is reflected in the regional breakdown of the sample (see Table 1), with about one-half of the sample drawn from the Southern region, which has a higher incidence of poverty and food insecurity(Machinjili & Kanyanda 2012). Random assignment was stratified by region.

2.1.2 Household randomization

The second level of randomization is at the household level. This level of randomization improves statistical power and provides a mechanism for testing for the program's indirect effects on non-participating households. Under the highly decentralized MASAF program, the GVH identifies households that are offered PWP employment. The intention of the program is to target poor households with able-bodied adults. As discussed below, we use the 2010/2011 national household survey as a baseline for this study. By chance, then, it is likely that one or two of the 16 randomly-surveyed households in our villages will be among those chosen by the GVH for the PWP. We term these households as "village chosen beneficiaries".

For this study, we randomly choose ten households from the 16 survey households in the village to be offered the program. To ensure that the experimentally-induced program offer did not affect the village selection process, the list of randomly selected households was distributed two weeks after lists of village chosen beneficiaries were submitted to district councils. We define these randomly-chosen households as "top up" households who are "treated" with the PWP program. Some small share of the "top up" households will also be village chosen beneficiaries. Additionally, given the coverage rate of the status quo program, one or two of the untreated households in PWP communities were likely to be included in the program through the village selection process.

In summary, our study has three groups of households: treated households in PWP villages (top ups who are randomly offered the program), untreated households in PWP villages, and households in non-PWP communities. By focusing on the random offer, we estimate the intent-to-treat effects of the program. By comparing the untreated households in PWP villages to households in non-PWP villages, we are able to measure the indirect (spillover) effects of the program.

3 Data

The data for this study come from a panel household survey. The basis for the panel was the Integrated Household Survey 3 (IHS3) which was fielded in 2010/2011 by Malawi's National Statistics Office. The IHS3 is a cross-section of 12,288 households in 768 enumeration areas (16 households per community) and has extensive household and agricultural modules.

The 16 IHS3 households in each EA included in the evaluation were interviewed in four additional rounds: before the public works projects started during the planting season (November 2012), after the first cycle, pre-harvest (February 2013), after the lean season cycle, post-harvest (April-May 2013), and finally after the completion of the 2012/2013 season (November 2013; see Figure 2). Our first survey (before PWP began in all but three villages) is, in effect, a second baseline to complement the IHS3. However, it could be tainted by anticipation effects if households in PWP villages modified their behavior before the program began in expectation of the employment opportunities or other changes it would induce. The complexities of partnering with the Malawian government to both implement the intervention and collect nationally representative data unfortunately preclude the ideal design: true baseline data are not available in all EAs. Twenty three enumeration areas (approximately 13 percent of the EAs in which the experiment was implemented) were incorrectly classified as included in the IHS3, and are therefore areas for which IHS3 data were not collected. We will refer to them as the 'non-IHS3' sample.[4]

3.1 Food security measures

We examine food security outcomes with eight indicators and as well as a composite measure. Our measures include (log) per capita food expenditure and (log) per capita food consumption in the last week (including home consumption). Total household calories is computed based on the caloric value of food consumed. A food consumption score is computed following WFP guidelines and aims to capture both dietary diversity and food frequency; it is the weighted sum of the number of days the household ate foods from eight food groups in the last week.[5] We include a measure of the number of food groups consumed in the last week for seven main groups.[6] We have an indicator for whether the household reported reducing meals in the last seven days. A food insecurity score is constructed according to WFP guidelines and takes on a value of 1, 2, 3 or 4 (a higher number indicates greater insecurity).[7] A coping strategy index is a weighted sum of the number of days in the past 7 days that households had to reduce the quantity and quality of food consumed.[8] Finally, as many of these food security measures are overlapping, we construct a principal components analysis index for these eight measures as our ninth composite food security measure.

4 Analysis

We analyze the intent-to-treat effect of Malawi's public works program using household-level data from two rounds of post-intervention surveys. Recall that our design includes two levels of randomization: village-level randomization varied PWP availability and payment

[4]Households in these enumeration areas were listed and a sample of 16 households was randomly drawn during our November 2012 survey. In these communities, we have one round of baseline data, rather than two. For the IHS3 Households that could not be re-interviewed, the team drew a replacement household from the original listing. About 9 percent of households are replacements for the original IHS3 household.

[5]The score is calculated based on the sum of weighted number of days in the last week the household ate food from eight food groups: (2 * number of days of cereals, grains, maize grain/flour, millet, sorghum, flour, bread and pasta, roots, tubers, and plantains) + (3 * number of days of nuts and pulses) + (number of days of vegetables) + (4 * number of days of meat, fish, other meat, and eggs) + (number of days of fruits) + (4 * number of days of milk products) + (0.5 * number of days of fats and oils) + (0.5 * number of days of sugar, sugar products, and honey). Spices and condiments are excluded. It has a maximum value of 126.

[6]The seven are described in the previous footnote, with exception of the last group (sugars).

[7]Food insecurity score is 1 if in the past seven days, the household reports not worrying about having enough food and reports zero days that they: (a) Rely on less preferred and/or less expensive foods, (b) Limit portion size at meal-times, (c) Reduce number of meals eaten in a day, (d) Restrict consumption by adults in order for small children to eat, or (e) Borrow food, or rely on help from a friend or relative. Food insecurity score is 2 if the household reports worrying about having enough food and reports zero days for actions a-e. Food insecurity score is 3 if the household reports ever relying on less preferred and/or less expensive foods and b-e are zero. Food security score is 4 if the household reports any days for b-e.

[8]Referring to the five actions described in the previous footnote, the coping strategy index is the sum of (a) + (b) + (c) + [3 * (d)] + [2 *(e)]. It has a maximum value of 56.

structure, while household-level randomization varies eligibility to participate conditional on PWP availability. Our main results pool across the four variants of the intervention in order to estimate the effect of having any public works opportunities in one's village, and the additional effect of being a treated household within a PWP village. Therefore, we capture the direct effect of PWP availability on treated households, and the indirect effect of the program on untreated households in PWP villages. The indirect effect is important in the context of rationing.[9]

Using data from the lean season (survey round 2), we pool across treatments and estimate the equation

$$y_{iv} = \alpha + \beta_1 \text{PWP}_v + \beta_2 \text{PWP}_v * \text{Topup}_i + \Gamma_d + \Theta_t + \epsilon_{iv} \tag{1}$$

where the indicator PWP_v is a village-level indicator for the availability ofx any PWP program and Topup_i is a household-level indicator that equals one if the household was randomly selected to be offered ("treated") the program and zero otherwise. The coefficient β_1 captures the indirect effect of the program on untreated households in PWP villages. The coefficient on the interaction term β_2 captures the marginal effect of being randomly selected for PWP in a village that had a PWP program – that is, the effect of being offered the opportunity to participate in program. The sum of the two coefficients β_1 and β_2 captures the total effect of PWP on treated households compared to households with no PWP in their villages. All specifications include district and week-of-interview fixed effects; standard errors are clustered at the village level. As of survey round two (February 2013), all villages with PWP programs had completed Cycle 1.

In half of the PWP villages, Cycle 2 took place in the lean season, after survey round 2 and before survey round 3. Therefore, analyses of the pre-harvest data (survey round 3) estimate the effect of the lean season PWP compared to the standard harvest season program for outcomes just before the harvest takes place.[10] The regression specification becomes

$$y_{iv} = \alpha + \delta_1 \text{Lean}_v + \delta_2 \text{Lean}_v * \text{Topup}_i + \delta_3 \text{Harvest}_v + \delta_4 \text{Harvest}_v * \text{Topup}_i + \Gamma_d + \Theta_t + \epsilon_{iv} \tag{2}$$

The sum $\delta_1 + \delta_2$ is the direct effect of having been offered the lean season program – including one 24-day work cycle at planting time and a second 24-day cycle during the lean season – on

[9]One limitation of our design is that we capture the indirect effect only at the implicit coverage rate as implemented in our study, approximately 18.8%. EAs are defined by NSO to include an average of 200 households. Our randomization ensures that ten of the 16 surveyed households are offered PWP. At the overall coverage rate of 15%, another 28 households, or 15% of the remaining 184, would be included in the program. Therefore, in PWP villages, an average of 37.6 out of 200 households have the opportunity to participate.

[10]Anticipation effects due to the (announced) timing of Cycle 2 could affect survey round 2 outcomes, even though all villages with PWP programs had the same work opportunities as of that survey. In estimates available upon request, we show that the effects of the lean and harvest season variants were very similar in survey round 2, so we prefer the statistical efficiency of the pooled specification using equation (1).

outcomes just before the harvest. The sum $\delta_3 + \delta_4$ captures the direct effect of the standard program timing, under which participants have experienced only one 24-day work cycle at planting season. The difference between the two sums is the marginal effect on household outcomes just before harvest of an additional 24 days of potential PWP work during the lean season.

The parameters we estimate are intent-to-treat, with identification derived from randomized village and household treatment status, rather than the endogenous participation status of households. Intent-to-treat parameters are policy relevant, in that the government can control the coverage rate and offer of PWP but not households' decisions to take it up. The effect of the program on participating households is also of interest, and future research will use instrumental variables specifications to estimate treatment-on-the-treated parameters and will explore heterogeneous effects on village-chosen versus randomly-assigned beneficiaries.

4.1 Balance

To explore the balance between treatment and control villages in terms of pre-treatment covariates and outcomes, we use the IHS3 data from 2010/2011. Although our first round of follow-up survey pre-dates the PWP implementation in all but three villages, the survey was conducted after the intervention was announced in treatment villages. Anticipation of the program may have affected household behaviors.

We first examine the subset of our 182 villages which were not included in the IHS3 national household survey in 2010/2011. These 23 villages were incorrectly identified as having been included in the IHS3. They were not randomly selected from the universe of villages in Malawi by the same procedure as used to select villages for the IHS3. They were, nonetheless, randomly assigned to our treatment/groups (six, six, three, three, and five villages to Groups 0-4 respectively). The differences between the IHS3 and non-IHS3 villages reflects a composition effect and has a bearing on the external validity of the results, but it is orthogonal to treatment assignment. Using our first round of follow-up data, we find that households in the non-IHS3 sample are better off than the IHS3 sample, with better educated household heads, smaller household sizes, and fewer children below the age of 14 (see Table 2).

Unfortunately – and surprisingly, given that randomization was conducted by computer – there is imbalance in pre-program food security at both the village and household levels. This is apparent in the estimates of equation (1) for nine food security measures from the IHS3, reported in Table 3.[11] Untreated households in PWP village had worse food security

[11] IHS3 surveys were conducted from March 2010 until March 2011. Balancing tests control for month and year of survey. To control for pre-treatment levels of outcome variables in subsequent regressions given the strong seasonality in these measures, we use, we use the residual of each measure regressed on month and

than households in control villages according to six of eight measures (columns 1-6, but not 7 and 8) and the PCA index; four of the differences are statistically significant at the 95 percent confidence level. Treated households fared better than their untreated neighbors by all measures, and significantly better in terms of the number of food groups consumed and the PCA index. However, the treated households still have worse food security than households in control villages according to six of eight measures and the PCA index. Four of the differences are statistically significant at the 90 percent confidence level; p-values for the test that treated households have the same food security as households in control villages and 95 percent confidence intervals are displayed below the point estimates.

The nature of the imbalance is markedly different in the North, where treated and untreated households have better baseline food security than households in control villages, than in the Center or South. We report balancing tests for each of the three regions in Appendix Table A1.

The IHS sample is well-balanced for 12 non food-security outcomes reported as outcomes in subsequent tables. Appendix Table A2 reports these tests. Out of 24 coefficients, only one is statistically different from zero. This makes the pre-program differences in food security all the more difficult to understand, but suggests that treated households are not systematically better off than untreated households along multiple dimensions of wellbeing.

Because of the imbalance, we present three sets of results for our analysis of food security: estimates without baseline (IHS3) covariates from the full sample of 182 villages, estimates without covariates from the subsample of 159 IHS3 villages, and estimates with covariates from the IHS3 villages. This strategy makes clear the extent to which changes in point estimates are due to limiting the sample versus controlling for pre-treatment outcomes.

5 Results

5.1 Nation-wide effect on lean-season food security

We begin by presenting results from equation (1) which estimates the direct effect of the program on treated households and the indirect effect on untreated households.[12] The sum of the coefficients $\beta_1 + \beta_2$ (PWP and $PWP * Top-up$) compares households in treatment villages that were themselves included in PWP to households in control villages. This total

year of survey indicators.

[12]Our ITT estimates are of the effect of being randomly chosen for inclusion in the PWP program, not of participating in the program. A small number of households were not randomly chosen to be included in the program but were eligible through village selection procedures. In our specification, these households pooled with the untreated households. One might expect village chosen beneficiary households to have either better outcomes than untreated households, because of the effect of the program, or worse outcomes, if the poorest households were indeed chosen and the program was not enough to offset their relative disadvantage.

effect on top-ups is reported in the tables, along with 95 percent confidence intervals (upper and lower bounds). Table 4 displays the effect of the program on food security measured during the lean season (in survey round 2). Higher values indicate better food security for the first five measures and the PCA index, and lower values indicate better food security for outcomes in columns 6-8. Panel A contains results from the full sample. The total effect of the program on top-up households moves in the direction of worse food security for all measures: negative coefficients for the first five outcomes, positive coefficients for the next three outcomes, and a negative coefficient in column (9) for the PCA index. The effect on the PCA index is equivalent to one-tenth of a standard deviation of that outcome for households in control villages, but the coefficient – like those for all but one of the individual outcomes – is not statistically different from zero.

However, these differences are at least partially driven by pre-existing differences in food security. To account for this imbalance, we first limit the sample to the 159 villages covered by the IHS3 survey (Panel B) and and then include pre-treatment values of the outcomes (Panel C). In subsequent analyses, we will focus on estimates that correspond to Panel C, but we show all three specifications here for transparency.[13]

The pattern and magnitude of the direct effect in the restricted sample in Panel B is very similar to the effects reported in Panel A. Including pre-treatment outcomes (purged of seasonal variation) from the IHS3 reduces the absolute value of the estimated effects by between one quarter and one third (Table 4, Panel C). Treated households have worse food security than households in villages without any public works programs for six of eight measures, but none of the differences are statistically different from zero. The point estimate of the effect on the summary index is is very close to zero, and we can rule out any positive effect of more than 0.1 standard deviations. Overall, a program designed to improve food security did not: households offered the opportunity to participate in public works in November/December 2012 and January 2013 did not have better food security during the lean season than households in villages without a public works program.

The indirect effect of the program on untreated households in villages with PWP is measured by the village-level indicator for public works, PWP. This coefficient captures the difference between mean outcomes for households in control villages and households that were not randomly chosen for the PWP program in villages that had the program. Village-chosen beneficiaries who were selected by their communities rather than the randomization procedure are included among these six untreated households in our estimates. We interpret this as a spillover effect of the PWP.

[13] An alternative to the ANCOVA-style specifications in Panel C is to include the PCA index of food security from the IHS in all of the food security regressions. Results are very similar when controlling for baseline variation in this way; results are available upon request.

In the full sample (Panel A), this spillover effect reduces food security. The differences are statistically significant for the food consumption score, food insecurity measure, and the PCA index. The magnitude of the effect on the latter is equivalent to 0.15 standard deviations. Results are similar in the restricted sample (Panel B). Including pre-treatment values of the outcomes (Panel C) reduces the estimated spillovers by about one half. Indirect effects are negative for five of eight outcomes and statistically significant for two. Not only does PWP fail to improve the food security of households randomly offered the program, but also, there are some indications that it may reduce food security for their neighbors. To further understand the pattern of indirect and direct effects, we turn to separate estimation for each of the country's three regions.

5.2 Heterogeneous effects by geographic region

We estimate equation (1) separately for the North, Central, and South regions of the country to understand whether the program was more effective in some places than others, and to consider whether regional variation provides any insight about the mechanisms for the effects we find. Because of the baseline imbalance, our preferred specifications include pre-treatment outcomes; results for the full sample and the baseline subsample are in the Appendix. As reflected in the sample means reported at the bottom of each panel, the Northern region is the country's wealthiest and most food secure, and the Southern region is both more populous and less food secure than other parts of the country.

5.2.1 North

The program has the biggest impact on food security in the North. The direct effect of the program reduces log per capita food expenditures and consumption; the food consumption score; household caloric intake; and the number of food groups consumed in the previous week. The effects on expenditures and calories consumed by treated households relative to controls are significantly different from zero. The program also appears to reduce food insecurity by three subjective measures: reductions in meals consumed, the food insecurity indicator, and the coping strategy index. For all three outcomes, the direct effect of PWP is statistically significant. The total effect on the PCA index is negative but not statistically different from zero.

The indirect effect of the program in the North is quite pronounced. Untreated households in PWP villages have worse food security than households in control villages for six of eight measures (the PWP coefficient in Table 5, Panel A) , with reductions in per capita food consumption and the food consumption score significant at the 90 percent level and increases in the need to reduce meals and the WFP food insecurity indicator significant at the 95 percent level. The PCA index falls by 0.580, or one third of a standard deviation for households in control villages in the region. That decline is significant at the 90 percent level.

5.2.2 Central

In the Central region, the direct effect of the program is not statistically different from zero for any of the food security outcomes. The pattern and absolute value of point estimates are close to those for the full sample: food security falls by six of eight measures, and the effect on the PCA index is small and negative.

The indirect effects are also imprecisely estimated, though greater in absolute value than the direct effects. For seven of the eight outcomes, untreated households in PWP villages have worse food security than households in control villages. The impact on the PCA index is -0.189, or almost one-tenth of a standard deviation of the PCA index for households in control villages, but not statistically significant at conventional levels. The overall pattern of results is suggestive and troubling in this region: there is no evidence that the program improves lean season food security for treated households and a pattern of coefficients that while not statistically significant, consistently points towards negative effects on untreated households.

5.2.3 South

PWP has very little impact on food security for treated or untreated households in the South. The point estimate of the effect is small and positive for four outcomes and small and negative for the other four; the effect on the PCA index is small and positive. The pattern of indirect effects is similarly unclear, with five outcomes showing small improvements three showing small reductions in food security. The indirect effect on the PCA index is 0.083, which is both small relative to the standard deviation in the control group and relative to point estimates in other regions.

5.3 Effects of program variants in the pre-harvest period

As designed by the government, the second PWP cycle takes place during the harvest period, beginning in May. This timing is suboptimal if marginal utility of consumption is higher and opportunity cost of working lower during the lean season. To determine whether changing the seasonality of the program could improve its effectiveness, our evaluation randomly assigned half of treated villages to move the second cycle, making public works start sooner, in March 2013. Survey round 3 took place after the March-April Cycle 2 and just before harvest.

In this round, villages assigned to the lean season treatment had completed work cycles in November-January and March, but those assigned to the standard harvest season treatment had not yet begun their second work cycle. The survey captures food security and other outcomes at the very end of the lean season, before the new harvest has begun. Estimating equation (2) allows us to test the marginal effect of rescheduling the second cycle of PWP for the lean season instead of the harvest season.

We report national results in Table 6. As before, Panel A includes the full sample and no controls for pre-treatment outcomes; Panel B limits the sample to observations for which pre-treatment outcomes are available; and Panel C incorporates those pre-treatment outcomes in the regressions. In addition to the coefficients from equation (2), we report the total direct effect of the lean and harvest season programs ($\delta_1 + \delta_2$ and $\delta_3 + \delta_4$, respectively); the 95% confidence interval for those effects; and p-values for the tests that the direct effect of the lean season treatment equals the direct effect of the harvest season treatment and that the corresponding indirect effects are equal.

The direct effect of the lean season program on treated households is small and generally not significantly different from zero. We focus on Panel C to account for baseline imbalance. The absolute value of the effect on treated households is very small for all outcomes, and there is no consistent pattern to the direction of the effect. Similarly, spillover effects on untreated households in lean season PWP villages are close to zero (the point estimates in the row labeled "lean").

The effect on treated households in villages whose second PWP cycle is scheduled for the harvest season (soon after survey round 3) is also small for most outcomes, but the pattern of coefficients is in the direction of worse food security than households in control villages: food security falls for six of eight measures and the PCA index, which falls by about 0.05 standard deviations. The indirect effect (coefficients in the the "harvest" row) is negative for five of the eight outcomes, and the magnitude of the (negative) indirect effect on the PCA index is half that of the direct effect.

We cannot reject that the effect of PWP on households treated with the lean season program equals that of households treated with the harvest season for any individual outcomes. This is despite the additional 24 days of work offered to the former group of households. The gap in effects on the PCA index is 0.08 standard deviations – with the lean season PWP (which had offered two cycles of PWP) slightly improving and the harvest season PWP (only one cycle offered as of survey round 3) slightly worsening food security compared to villages with no PWP. The p-value for the test that the direct effects of those two variants on the PCA index are equal is 0.22. Differences in indirect effects of the two program variants are even less precisely estimated and show no consistent pattern. In short, there is little evidence that changing the timing of Cycle 2 is effective in improving food security, even after treated households in the lean season program have been offered 48 days of work compared to 24 days for households in the harvest season treatment.

5.3.1 North

As with the effects on lean-season food security, estimating equation (2) on the full sample obscures important regional heterogeneity. In the North, the direct effect of the lean season program is small, with point estimates that are positive for six of eight outcomes and the PCA index (Table 7, Panel A). The indirect effect of the lean season program, though, is negative for all outcomes and significantly so for dietary diversity and the PCA index, which falls by 0.45 SD relative to households in the control group. The harvest season program performs even worse. The direct effect is small and the pattern is inconsistent, but the point estimate for the PCA index is negative. The indirect effect is negative for all outcomes, including significant reductions in dietary diversity and significant increases in all three measures of food insecurity. The negative effect on the PCA index is equivalent to 0.48 SDs of the control group.

5.3.2 Central

In the Central region, the lean season program improves food security for both treated and untreated households in PWP villages (Table 7, Panel B). The direct effect is positive for seven of eight outcomes and the PCA index, which increases by 0.17 SDs though none of the differences relative to households in the control villages are statistically significant at conventional levels. The pattern and magnitude of indirect effects are very similar: food security improves by all eight measures, including an imprecisely-estimated 0.17 SDs of the PCA index.

Impacts of the harvest season program in this region are puzzling. The direct effects are, if anything, negative. Food security falls along six of eight measures, though none significantly. The magnitude of the effect on the PCA index is close to zero. The indirect effects, however, are positive. Food security improves by seven of eight outcomes, including a significant decline in the number of days in which meals were reduced. The PCA index is 0.22 SD higher for untreated households in villages with PWP than for households in control villages, though the difference is not statistically significant. Since even the treated households did not work in the period leading up to survey round 3, it is conceivable that the indirect effects are due to anticipatory behavior by untreated households who are not on the cusp of another 24 day work cycle, relative to treated households whose next work opportunity is pending.

5.3.3 South

Finally, the lack of effects of PWP in the South we observed in survey round 2 continues in survey round 3. For the lean season program, the pattern of direct effects is weak, with food security falling for five of eight outcomes and the PCA index. Indirect effects are no more conclusive, and the point estimate of the indirect effect on the PCA index is very close to zero. The harvest season program has negative direct effects on food security for six of eight outcomes and reduces the PCA index by 0.10 SDs; none of the changes are significant. Indirect effects are negative for five of eight outcomes, but the reductions in per capita consumption and per capita caloric intake are significant. The decline in the PCA index is -0.1 SDs (as the negative impacts on the first five food security measures are somewhat offset by imprecise positive effects on the three measures of food insecurity).

Each panel of Table 7 includes tests that the direct effect of the lean season program (where treated households were offered 48 days of work as of survey round 3) equals the direct effect of the harvest season program (which offered 24 days of work before survey round 3), and that the corresponding indirect effects are equal. We reject equality of the two program variants for very few outcomes, but the overall pattern of results for food security in all three regions is much more favorable – or at least less damaging – for the lean season program (which offered 48 days of PWP) than the harvest season program (as yet offered 24 ays of PWP).

6 Mechanisms

Malawi's PWP shifts household budget constraints out by providing paid work that is offset by reductions in other labor supply. Despite this, households offered PWP do not have better food security or use more agricultural inputs as a result of the program, and food security worsens among untreated households in PWP villages compared to their counterparts in villages with no program. We discuss, and when possible test, potential mechanisms for the lack of positive direct effects in any region, and for the negative spillovers in the Northern and Central regions.

6.1 Study design

6.1.1 Low power

Lack of statistical power is one possible explanation for a null main effect. It would be a more plausible explanation had we found positive but imprecisely estimated point estimates, however. Instead, we find predominantly negative effects that are not statistically different from zero. This is true at not only the national level but also within each region.

We calculate confidence intervals for the total effect on treated households using the delta method. In most cases, we can rule out meaningful positive effects of the program: nationally, the upper bound (at the 95 percent confidence level) of the improvement in food security for any of the eight individual indicators is always less than 0.2 standard deviations of the outcome in the control group. In the full sample, the standard deviation of the PCA index for food security is 2.018, and the confidence interval for the effect of the public works program on treated households is [-0.476, 0.087]. In the IHS3 subsample, the standard deviation in the control group is 2.051, and the confidence interval for the effect is [-0.337, 0.204].

By region, we can rule out effects greater than 0.16 SDs in the North, 0.21 SDs in the Center, and 0.24 SDs in the South. Thus, even moderate direct effects are outside the confidence intervals in each region. It does not appear that lack of statistical power explains failure to identify positive effects of the program.

6.1.2 Low take-up

The second possibility is that household-level intervention, which chose households for inclusion in PWP at random, resulted in low take-up and therefore small intent-to-treat (ITT) estimates. The ITT estimates are not biased, but they are – by construction – smaller in absolute value than the treatment on the treated (TOT) effects. Since assignment to the treatment group increases MASAF participation, the TOT and ITT effects have the same sign. Therefore, discussion of the TOT does not offer an explanation for coefficients with unanticipated direction of impact.

As designed and implemented by the government, the program is targeted to vulnerable households, who might participate at higher rates than randomly selected households. Across the full study, 57 percent of treated (i.e. randomly-selected) households in our study participated in MASAF (sum of coefficients in column 1 of Table 8). Since there are considerable within-village spillovers, using household treatment status as an instrument violates the Stable Unit Treatment Value Assumption (SUTVA) and is not a valid specification. Instead, we can instrument for PWP participation using village randomization, where the first stage equation is

$$\text{Any PWP participation}_{iv} = \delta_0 + \delta_1 \text{PWP}_v + \Gamma_d + \Theta_t + \nu_{iv} \tag{3}$$

In this specification, the treatment effect incorporates direct and within-village spillover effects; the assumption is that there are no across-village spillovers within our sample and is justified by the distance between study villages. Participation is less than 100 % by design: $\delta_1 = 0.145$, and the first stage F-statistic 182.44. The national treatment-on-the-treated

(TOT) parameters reported in Table 9 are larger in magnitude than the weighted average of the direct and indirect effects reported in Table 4, but imprecisely estimated and always in the direction of reducing food security.

Participation varies somewhat by region at 58 percent of treated households in the North, 42 percent in the Center, and 65 percent in the South. Regional TOT estimates are reported in Table 10, and indicate worse food security for PWP participants in all regions. Low take-up does not explain the lack of impact of PWP on food security.

6.2 Design of PWP

6.2.1 Value of transfer

A key design feature that may contribute to the lack of direct effect is low total value of PWP income. The wage rate for the program is set by the government at MK 300 ($0.92) per day, with total possible earnings of MK 14,400 ($44.16). The wage rate is low by international standards, but in a country with gross national income per capita of $320 ($730, adjusted for purchasing power parity), $44 is non trivial. Local political constraints made it infeasible to vary the wage rate for this study, so our experimental design does not allow us to speak to the effect of PWP with higher wages. Despite extensive expenditure data in our surveys, we are not able to detect increased spending on any category: food, agricultural inputs or business investments, non-food consumption, or durables. This limitation is shared with many studies on micro-finance, which similarly fail to detect the effect of increased cash on household consumption or expenditures.

The lack of beneficial effects of the lean season program variant on survey round 3 outcomes also undermines the idea that a more generous program would transform the effects. Treated households in villages selected for the lean season treatment were eligible for 24 additional days of work in March (for a total of 48 days from November to March). Yet in surveys conducted the following month, their food security is no better than either households in control villages or treated households in the harvest season villages (offered 24 days from November to January).

6.2.2 Timing

A second hypothesis related to the design of the PWP program concerns timing of the program. First, the program covers periods where the opportunity cost of time is potentially high. Perhaps work on PWP activities crowded out labor supplied to the household's own farm or to the wage labor market. We do not have data about hours of work in household agriculture, but note that since survey round 2 is conducted *before* the harvest, any reduction in food security due to reduced harvests from households' own plots cannot explain the results and would instead exacerbate the negative effect on food security measured later in the year.

We report the effect of the public works program on labor supply in daily wage markets("ganyu") in Table 8 (columns 3 and 4). Both the direct and indirect effects of the program are small and not statistically significant at the extensive margin of participation; at the intensive margin, PWP if anything crowds in wage labor, though standard errors are large. It appears that households have excess supply of labor, a finding consistent with Goldberg (Forthcoming).

The planting season begins earlier in the South than in the North, and the Government activated PWP activities earlier in the South. PWP and the fertilizer subsidy program are administered separately and are not perfectly synchronized. In three study districts, fertilizer subsidy coupon distribution took place between the first and second 12-day waves of PWP activities and in the remaining nine districts, fertilizer coupon distribution overlapped with PWP work and payment. The three districts without overlap were Blantyre (South); Dowa (Center); and Karonga (North). The North accounts for a smaller fraction of total population and therefore of our sample, so discordant timing in one study district represents a larger share of beneficiaries in that region than in the Center or South.

6.2.3 Targeting

Regressive or ineffective targeting potentially explains both lack of direct effects and negative indirect effects. PWP is intended for the able-bodied poor, and uses a combination of community wealth ranking exercises and low wages to target the program. In practice, the characteristics of participants may differ from the eligibility criteria because of differences in how local officials select beneficiaries and in the opportunity cost of participation. There are two types of targeting that may modulate impacts. The first is the selection of village-chosen beneficiaries. As noted above, some untreated households in our study were village-chosen beneficiaries. We examine the correlation between baseline per capita food consumption and participation of these households as an indication of whether the village selection procedures targeted poorer households. Our preferred measure of baseline food security comes from the IHS3, because unlike round 1 of the evaluation data, the IHS3 data were collected before the intervention was announced. We cannot assess targeting on short-term food security, because survey round 1 data were collected after the program was announced and may be affected by anticipated PWP earnings. However, if PWP is used locally in response to short-term shocks, the lag between the IHS3 and program implementation may explain lack of correlation between time-varying characteristics such as food security and participation.

Nationally, Figure 3 illustrates that there is very little correlation between food security in 2010/2011 and participation through the village selection process. This may suggest that the village selection process either responds to short-term food security or relies on criteria that are orthogonal to long term consumption.[14]

As shown in Figure 4, the relationship between food security and PWP participation varies by region. In the North and Central regions, participation of untreated households was uniform across the distribution of baseline food consumption.[15] In the South, households with lower baseline food consumption were if anything *less* likely to be chosen by GVH and participate in PWP.

The second type of targeting is self-targeting, captured by participation by randomly-selected households from different points of the distribution of baseline food consumption. This mimics an unrationed PWP like NREGA. Among treated households, the correlation between accepting PWP work and IHS3 food consumption is negative in the North (indicating pro-poor targeting and self-selection of the poorest households), but not in the Central or Southern regions. Though self-targeting seems more prevalent in the North, there is no evidence of displacement of work in the market for casual wage labor ("ganyu") in any region.

Overall, Malawi's PWP is rationed and not targeted towards the food-insecure. Mistargeting could explain the lack of improvement in food security if the program employed people who had lower marginal propensity to consume food, but the geographic heterogeneity in targeting does not seem to explain the regional heterogeneity in results. PWP was, if anything, slightly better targeted in the region where it led to the most pronounced negative spillovers.

6.2.4 Project type

PWP activities included road building and tree-planting, which conceptually could have required different effort levels or induced differential selection by participants. While project type is unlikely to explain the lack of direct effect, different project types across the three regions could lead to heterogeneous effects. In fact, the mix of projects was similar across the three regions. We have limited anecdotal information about the day-to-day work activities of beneficiaries, with no evidence of systematic differences by region. As discussed previously, PWP activities do not displace wage work in any region. We conclude that any differences in work activities are unlikely to explain regional differences in program impacts.

[14] The autocorrelation between log per capita food consumption in the IHS3 and survey round 1 in control villages 0.30. Over shorter horizons, between any two adjacent survey rounds, the autocorrelation in control villages is close to 0.5.

[15] Food consumption data from the IHS3, accounting for seasonality by detrending by week-of-interview.

6.3 Equilibrium price effects

Spillovers could operate through markets. If increased demand by treated households drove up the price of food or other goods, the program may have reduced the purchasing power of both treated households and their untreated neighbors. A change in price level has the potential to explain both the lack of positive effects for treated households and negative effects on their untreated neighbors, though differences in market conditions across the three regions would be necessary to explain why the negative spillovers were concentrated in the North and Central regions but not the South.

We test whether village-level prices were different in treatment and control villages, using a price index constructed from households' reported prices for the five most commonly purchased goods. The specification for village level differences is

$$y_{iv} = \alpha + \beta \text{PWP}_v + \Gamma_d + \Theta_t + \epsilon_{iv} \tag{4}$$

Note that this specification estimates the effect of PWP on prices at the coverage rate in the experiment. As reported in Table 16, the difference in the price index between treatment and control villages is neither economically nor statistically significant. Nationally, prices in treatment villages are nearly identical – within one percent – to those in control villages. Neither is there any evidence of price effects at the regional level. In the North, the magnitude of the coefficient on the PWP indicator is near zero; prices are about seven percent lower for treatment villages than controls in the Center and seven percent higher in the South. None of the differences are statistically significant.

6.4 Change in transfers from institutions or individuals

Other institutions – government or NGO programs – or individuals could have changed their behavior towards untreated households or otherwise compensated control villages with other programs. In this case, the untreated households in PWP villages, which benefited from neither PWP nor these alternative programs, may be worse in comparison. We have data about receipt of five types of benefits: food assistance or maize distribution, school-based nutrition programs, scholarships or other help with school fees, cash transfers, and fertilizer coupons. As shown in Table 14, there is no evidence that untreated households differ in receipt of these benefits from either treated households or households in control villages. (Indeed, the only economically or statistically meaningful difference is in fertilizer coupons, which are more likely to be available to treated households in accordance with the designed linkage between PWP and the national fertilizer subsidy scheme).

A related hypothesis is that treated households were not the final beneficiaries of the program, but instead shared their income with other households or used it to replace or repay loans. We examine four outcomes in Table 15: whether the household received any transfers, whether it made any transfers to others, whether it received any new loans, and whether it made any loan payments in the previous month. PWP does not significantly affect any of these indicators, and the magnitudes of the direct effects are small as well as imprecisely estimated. For example, treated households were about ten percent less likely to receive any transfers but twenty percent less likely to make any transfers to others.

It is also possible that other households reduced transfers to – or increased demands for transfers from – households in villages with PWP. Even untreated households in PWP villages could be affected. For example, family members living in other locations may know that PWP was active in a village, but have inaccurate information about who actually benefited from the program. They may falsely believe that untreated households received income from PWP and therefore do not need support through social networks or even have funds they could share with others. Results reported in Table 15 indicate that untreated households are significantly *less* likely than households in control villages to make transfers to others, ruling out the possibility of misguided pressure to share income. Untreated households do not significantly differ from controls (or treated households) in their probabilities of receiving transfers or accessing loans, and effects are not different across the three regions (results available upon request).

6.5 Behavioral responses by treated and untreated households

Finally, households offered PWP may make choices that offset or limit its effect on food security. If households behave according to the predictions of the permanent income hypothesis (PIH), then the temporary increase in income will be allocated to consumption in all future periods, and the change in consumption in any one period would be diminishingly small, as in Chen, Mu & Ravallion (2009). This could account for the lack of positive effects on food security of treated households, but cannot explain negative spillovers to untreated households. Perhaps more importantly, the myriad credit market imperfections in Malawi that lead to well-documented seasonal variation in consumption make it unlikely that the PIH holds in this context.

Another possibility is that treated households responded to the program by voluntarily reducing their food consumption in order to pool existing resources and wages from the program in order to make indivisible purchases of durables or investments in fertilizer or other agricultural inputs. We test this hypothesis by estimating the effect of PWP on durable ownership (Table 11) and use of agricultural inputs (Table 12). We consider three measures of durable ownership: the probability of owning at least one of 32 goods covered in the survey, the total number of durable goods owned, and a PCA index of durable ownership. The total effect of PWP on treated households is not significantly different from zero for any of the three outcomes, and the imprecise estimates for the number of goods and PCA index are negative. There is no evidence that households that are covered by PWP use program earnings or other resources to purchase durable goods, and these effects do not differ by region (results available upon request).

Use of agricultural inputs is important as an outcome in its own right – since complementarities with the fertilizer subsidy scheme drive the design of the program and increased fertilizer use is a major stated goal – as well as a potential explanation for failure to improve food security. In Malawi, fertilizer is applied twice to both maize (the staple crop) and tobacco (the main cash crop). We examine the probability that a household uses any chemical fertilizer during the 2012/2013 season; the log of expenditure on fertilizer for each of the first and second applications; and the log of quantity of fertilizer used in each of the first and second applications. For the expenditure and quantity variables, we use the inverse hyperbolic sine transformation in order to be able to take logs of variables where some observations are zeros. The national point estimates of both the direct and indirect effects on any of these outcomes are close to zero, and none of the coefficients are statistically significant.

However, results reported in Table 13 indicate important heterogeneity by region: while fertilizer use is unchanged in the Central and Southern regions, it increases for both treated and untreated households in PWP villages compared to non-PWP villages in the North. Recall that negative direct and indirect effects on food security are also most pronounced in the North. Households in that region may have devoted PWP income and other resources to purchase of fertilizer rather than purchase of food. We highlight this result, because it is the only evidence we have of behavior that is consistent with the unexpected negative spillovers on food security and matches the geographic pattern of direct and indirect results.

6.6 Remaining possibilities

One potential explanation for the observed negative spillovers to untreated households is strategic self reporting by untreated households who do not experience real reductions in food security but choose to under-report consumption in order to signal their potential eligibility and increase their chances of being offered PWP or other benefits in the future. We argue that two features of our research design make it less likely in this study than in many other randomized controlled trials that rely on similar data. First, data collection and program implementation are conducted by different institutions. The National Statistics Office is responsible for survey work, and the Local Development Fund with assistance from Innovations for Poverty Action manages PWP activities and payments. Respondents do not report their food consumption or other outcomes to people who are employed by or connected to the administration of the public works program. Second, households in our study[16] were included in the IHS3, a nationally-representative survey that is not connected to the provision of any public or private benefits. These households had been administered the same survey instrument by the same institution (NSO) before the study began, and had not received any benefits that depended on their answers. These features distinguish our evaluation from RCTs that rely on data collected by employees of the same organization that administers the intervention, and from evaluations of subjects who have no reason *not* to infer that surveys are collected for the purpose of allocating benefits.

We are not able to assess the reliability of our various outcome measures in order to determine whether perceived or self-reported food security may have fallen absent a change in objective consumption. While the eight different measures of nutrition we examine are all related to food security, they measure different aspects of the concept. Therefore, we cannot use the correlations between items in the IHS3 and subsequent survey rounds as an indication of item reliability, because the program itself may have affected some measures of food security differently than others. If strategic misreporting is a concern, it will not be identified by comparing so-called "subjective" measures to "objective" measures (such as expenditure).

While we cannot directly rule out strategic misreporting, we think it less likely to influence results in our evaluation than in many other studies relying on self-reported data. The remainder of our discussion focuses on explanations for real negative spillovers. We cannot rule out that PWP changed the behavior of untreated households in some way that worsened food security. Such a mechanism would have to take a very specific form to explain both the result and be consistent with the behaviors we observe. One possibility is as follows: PWP eligibility causes a change in the behavior of treated households that is not captured by our survey (either because it falls outside the recall period, or because it is too small to be measured precisely). Untreated households respond to the behavior or consequences of the behavior by the treated households. For example, treated households might spend their earnings by increasing consumption immediately after payment, which would not be captured by a the seven-day recall period of the survey which takes place a few weeks after the payment. Untreated households respond by similarly increasing very short run consumption – to "keep up" up with their neighbors, or because their reference point was affected – but then have to reduce consumption in subsequent periods because their short-term splurge was not financed by PWP earnings. This explanation requires peculiar behavior by the untreated households,

[16] Because of the error in NSO's original village listing, about 20 percent of villages we study were not part of the IHS3. As discussed previously, we do not observe differences in the impact of PWP across IHS3 and non-IHS3 villages.

however – either their marginal utility of "keeping up" with their neighbors is high enough to offset reduced consumption later on, or their preferences are time-inconsistent, or they have erroneous expectations about future income.

An alternative explanation requiring less complicated behavior by the untreated households is that immediately after they are paid, treated households purchase food in bulk quantities for storage and later consumption. These purchases are not captured in the survey recall period and therefore do not translated into increased food security from the program, but they do reduce the availability of commodities at local markets for untreated households. The equilibrium effect is on the quantity of goods, not prices, perhaps because of frictions in the wholesale market. Food security for untreated households falls because of supply-side factors.

We see some evidence of this behavior in the fertilizer purchases of untreated households in the North. Treated and untreated households increase their expenditure on and use of fertilizer by about the same amount – nearly double – relative to controls. For treated households, the MK 6,000 increase in spending across the basal and top dressing fertilizer applications could have come entirely from PWP earnings. Untreated households, though, did not have extra income to use for the fertilizer. They did not have the opportunity to earn wages from public works activities and did not work more in the casual wage labor market. If the increase in use of fertilizer had not been accompanied by a reduction in food security, we would suspect that treated households had shared the spoils of the program with their untreated neighbors. We see no evidence of this in our direct measures of cash and in-kind transfers. The pattern of food security and fertilizer use results makes it more likely that instead, untreated households in the North modeled their behavior on that of their treated neighbors. We cannot distinguish between an adjustment of the reference point about appropriate fertilizer use or incorrect expectations that PWP or other opportunities would soon be available to untreated households.

Relatedly, Bazzi, Sumatro & Suryahadi (2015) find that a large-scale CCT in Indonesia does not affect consumption growth for households that receive transfers on schedule, but reduces consumption growth for households whose second transfer is delayed. In our context, untreated households in PWP villages may behave like Indonesian households whose transfers were delayed: their expectations about future income changed, but were unfulfilled (at least within the survey recall period).

7 Conclusion

Public works programs can stabilize income and improve food security of beneficiaries by providing earnings opportunities, and can achieve targeting through low wages and work requirements that promote self-selection. While Malawi's PWP offers households the opportunity to earn approximately \$22 – a substantial sum compared to the country's per-capital GDP of \$226[17] – at planting season and an additional \$22 later in the year, it does not have a measurable short-term effect on lean season food security for treated households. Even improving the structure of the program by rescheduling the second work cycle from the harvest season to the lean season does not generate measurable improvements in the food security of treated households. This may be because households spread the income over a large number of different expenses, with increases in any individual category too small to detect with expenditure surveys conducted two to four weeks after payments are distributed.

In Malawi, the public works program is designed with an additional objective: it is timed to coincide with the planting season to promote take-up of the country's fertilizer subsidy scheme. However, our results do not support the hypothesis that the two programs are complementary. While households included in PWP are more likely to receive fertilizer coupons (consistent of the policy of interlinkage with the fertilizer subsidy), and hence pay less for the fertilizer they use, they do not use more fertilizer than untreated households in their villages. In the Central and Southern regions, treated households do not increase fertilizer use relative to households in control villages, and in the North, treated and untreated households both increase their fertilizer use by the same amount. This may suggest that other constraints, including lack of knowledge of the correct amount of fertilizer to use (Duflo, Kremer & Robinson 2008), are more important than credit constraints in limiting use of fertilizer.

The program is rationed nationally, with funding available to cover only 15 percent of households satisfying eligibility criteria. When coverage is extended to additional households chosen randomly rather than through the community wealth ranking exercises, take-up is about fifty percent. However, participation is higher among less food-secure households only in the North, suggesting that the program is not achieving pro-poor targeting (as measured by these indicators) through self-selection at the current wage rate. Despite this, the program does not displace labor supplied to households' own farms or to the market for casual day labor, likely because of slack labor markets even during peak agricultural periods.

[17] In their study of the cash transfer project in one district of Malawi in 2007, Miller, Tsoka & Reichert (2011) find large, positive effects on beneficiary households in program villages compared to households in control villages screened as eligible but not given the program. In this program the size of the benefit is significantly larger, with transfers totaling \$168 per household over the course of a year (equivalent to about \$250 in 2012 price levels), an amount more than five times what households received from PWP 2012/2013.

We find the surprising result that the indirect effects of the public works program are small or negative. In Northern and Central Malawi, food security for untreated households in villages with PWP programs is not only lower than food security for their treated neighbors, but also lower than food security in control villages without PWP activities. This is in contrast to expectations and to the effects of other large-scale transfer programs. For example, Oportunidades, the conditional cash transfer program in Mexico, generated positive effects on the consumption of treated households and positive externalities to non-beneficiary households (Angelucci & DeGiorgi 2009).

An explanation for this unexpected finding has proven elusive. Cash transfers in Mexico generated equilibrium effects on commodity prices (Cunha, DeGiorgi & Jayachandran 2014), but we find no evidence of price increases in Malawian villages with PWP compared to those without. Pressure to share money could have explained the negative effect on untreated households if relatives mistakenly believed that because PWP was present in the village, even the untreated households had benefited and could contribute to the social network. However, there is no evidence of increased income sharing from PWP villages and if anything, untreated households made fewer contributions to their networks than households in control villages.

A possibility we cannot exclude is that untreated households reduce food consumption in reaction to an unobserved change in the behavior of treated households or to erroneous expectations of their own future income. We discuss two examples of this: untreated households over-spend to match a short term and thus unobserved increase in consumption by their treated neighbors, and compensate by reducing consumption during the period we do observe, or a combination of stock-outs and sticky prices reduces food availability. The matching and substantial increases in fertilizer use by both treated and untreated households in the North, the region with the strongest negative spillovers, are consistent with this hypothesis. We lack direct evidence to test related behaviors, but present them as examples of the types of mechanisms that could explain the unexpected finding that Malawi's public works program reduces the food security of untreated households in villages with PWP activities. Identifying the mechanism remains a priority for both understanding household spending patterns and for informing policy.

Figures

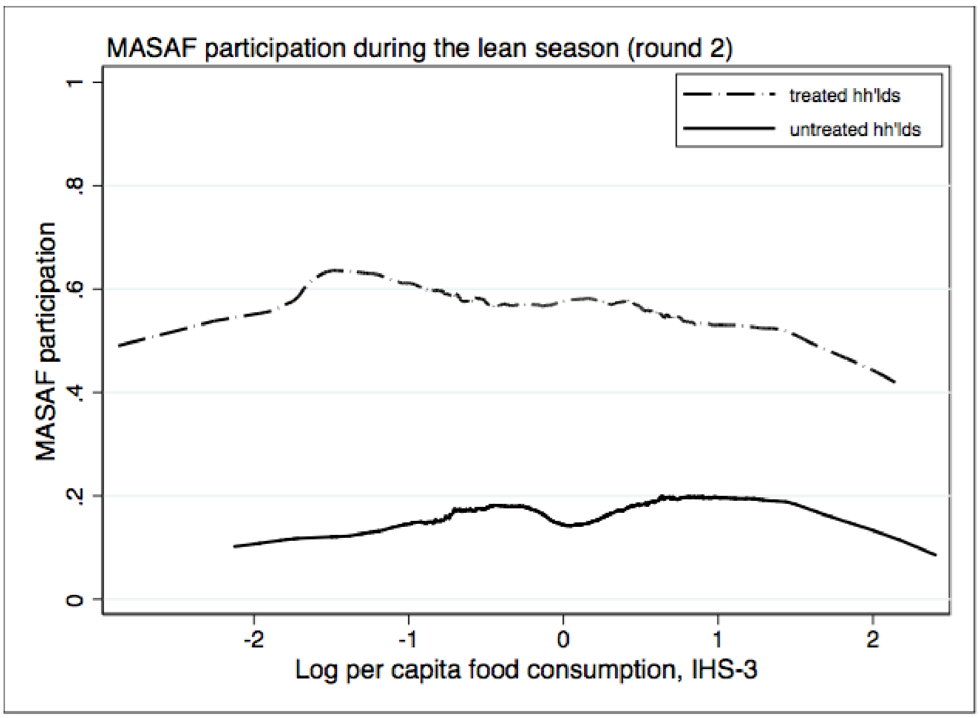
Figure 1: Experimental design

	Treatment at the EA level	
	Cycle 1: Planting season PWP Cycle 2: Harvest season PWP	Cycle 1: Planting season PWP Cycle 2: Lean season PWP
No PWP	Group 0 [38 EAs]	
PWP with lump sum payment	Group 1 (status quo) [40 EAs]	Group 3 [35 EAs]
PWP with split payment	Group 2 [34 EAs]	Group 4 [35 EAs]

Figure 2: Timeline

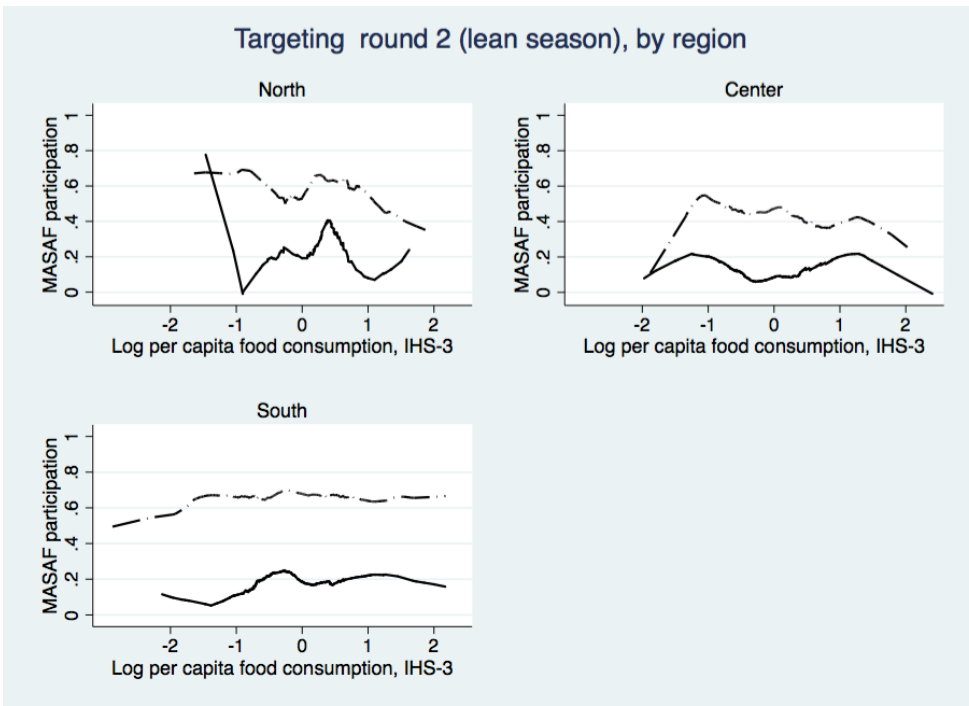
Year-round	Pre-planting	Planting	Lean	Lean	Early harvest	Harvest	Pre-planting
Mar. 2010-Mar. 2011	Oct.-Nov. 2012	Wave 1: Nov.-Dec. Wave 2: Jan. 2013	Feb. 2013	Wave 1: Feb.-Mar. Wave 2: Mar.-Apr.	Apr.-May 2013	Wave 1: May-Jun. Wave 2: Jul.-Aug.	Oct. 2013
IHS3	Survey round 1 (post announcement of treatment)	PWP Group 1 PWP Group 2 PWP Group 3 PWP Group 4	Survey round 2	PWP Group 3 PWP Group 4	Survey round 3	PWP Group 1 PWP Group 2	Survey round 4

Figure 3: National targeting based on IHS3 food security



MASAF participation is an indicator for whether any member of the household has participated in PWP in the month preceding survey round 2 (dashed line is for treated households, and solid line for untreated households. Log per capita food consumption from IHS3.

Figure 4: Regional targeting based on IHS3 food security



MASAF participation is an indicator for whether any member of the household has participated in PWP in the month preceding survey round 2 (dashed line is for treated households, and solid line for untreated households. Log per capita food consumption from IHS3.

Tables

Table 1: Regional breakdown of treatment assignment

	Control EA	PWP EA	Total
North	4	18	22
	10.5%	12.5%	12.1%
Central	14	50	64
	36.8%	34.7%	35.2%
South	20	76	96
	52.6%	52.8%	52.7%
Total	38	144	182
	100%	100%	100%

Table 2: Descriptive statistics of households (survey round 1) by IHS3 status

	Non IHS3 EAs	IHS3 EAs	p-value: non IHS3 = IHS3
Female headed household	0.304	0.260	0.800
	(0.461)	(0.439)	
Highest level of education, HoH	6.337	6.191	0.066
	(3.831)	(3.334)	
Head attended secondary school	0.230	0.186	0.003
	(0.422)	(0.389)	
Household size	4.380	4.969	0.000
	(1.998)	(2.291)	
Number of children under 14	2.149	2.388	0.005
	(1.568)	(1.718)	
Number of EA	[23]	[159]	

Standard deviations in parentheses.

Table 3: Food security: balancing tests (IHS3)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) per adult equivalent calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
PWP	-0.193**	-0.128**	-3.864**	-1762.095	-0.380***	0.016	-0.041	-0.712	-0.348**
	(0.089)	(0.051)	(1.279)	(1841.631)	(0.098)	(0.030)	(0.097)	(0.586)	(0.148)
PWP * Top-up	0.066	0.047	1.345*	405.184	0.170**	-0.012	-0.118*	-0.112	0.183**
	(0.053)	(0.034)	(0.780)	(1382.837)	(0.062)	(0.017)	(0.062)	(0.288)	(0.081)
R-squared	0.09	0.15	0.12	0.17	0.13	0.19	0.23	0.21	0.19
Total effect on top-ups	-0.127	-0.082	-2.519	-1356.911	-0.210	0.005	-0.158	-0.823	-0.165
P-value for non-zero effect on top-ups	0.13	0.08	0.04	0.43	0.02	0.88	0.09	0.17	0.25
Upper bound (total effect)	0.037	0.009	-0.169	2022.784	-0.031	0.064	0.025	0.336	0.118
Lower bound (total effect)	-0.291	-0.173	-4.870	-4736.605	-0.389	-0.055	-0.342	-1.982	-0.448
Observations	2260	2274	2270	2274	2270	2274	2274	2274	2256
Mean of dep. var. in control group	5.27	6.16	45.33	64728.56	5.08	0.22	2.34	4.57	0.05

OLS estimates. Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.

Table 4: Food security (survey round 2)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) per adult equivalent calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
Panel A: full sample, no covariates									
PWP	-0.040	-0.071	-2.026*	-96.593	-0.116	0.065*	0.181**	0.566	-0.293**
	(0.080)	(0.044)	(1.072)	(61.277)	(0.106)	(0.034)	(0.080)	(0.635)	(0.148)
PWP * Top-up	-0.002	0.017	0.151	8.060	0.025	-0.048*	-0.129**	-0.156	0.099
	(0.050)	(0.031)	(0.665)	(50.639)	(0.068)	(0.025)	(0.061)	(0.489)	(0.096)
R-squared	0.15	0.17	0.14	0.13	0.18	0.08	0.08	0.09	0.16
Total effect on top-ups	-0.041	-0.054	-1.874	-88.533	-0.091	0.017	0.052	0.409	-0.195
P-value for non-zero effect on top-ups	0.59	0.20	0.07	0.13	0.38	0.62	0.52	0.51	0.18
Upper bound (total effect)	0.108	0.028	0.163	24.807	0.113	0.083	0.210	1.631	0.087
Lower bound (total effect)	-0.191	-0.135	-3.911	-201.874	-0.295	-0.049	-0.106	-0.812	-0.476
Observations	2792	2837	2825	2835	2825	2808	2800	2802	2738
Mean of dep. var. in control group	5.83	6.58	38.30	2341.49	4.32	0.49	3.12	9.09	0.13
Panel B: baseline subsample, no covariates									
PWP	0.004	-0.069	-2.149*	-64.478	-0.077	0.073**	0.216**	0.221	-0.258
	(0.092)	(0.050)	(1.183)	(68.238)	(0.115)	(0.035)	(0.084)	(0.671)	(0.160)
PWP * Top-up	0.034	0.026	0.676	-12.607	0.091	-0.051*	-0.162**	0.115	0.135
	(0.057)	(0.034)	(0.753)	(57.816)	(0.080)	(0.030)	(0.070)	(0.573)	(0.108)
R-squared	0.16	0.19	0.16	0.15	0.21	0.10	0.10	0.10	0.18
Total effect on top-ups	0.038	-0.043	-1.473	-77.086	0.014	0.022	0.055	0.337	-0.124
P-value for non-zero effect on top-ups	0.66	0.34	0.20	0.21	0.90	0.53	0.50	0.61	0.42
Upper bound (total effect)	0.208	0.046	0.759	42.916	0.230	0.090	0.215	1.612	0.174
Lower bound (total effect)	-0.132	-0.132	-3.706	-197.088	-0.201	-0.046	-0.105	-0.939	-0.421
Panel C: baseline sub-sample, including pre-treatment outcomes									
PWP	0.043	-0.042	-1.427	-50.716	0.005	0.070**	0.209**	0.242	-0.148
	(0.085)	(0.048)	(1.099)	(68.695)	(0.107)	(0.035)	(0.082)	(0.656)	(0.148)
PWP * Top-up	0.018	0.014	0.349	-19.091	0.046	-0.050*	-0.152**	0.127	0.082
	(0.056)	(0.034)	(0.732)	(57.822)	(0.080)	(0.030)	(0.070)	(0.574)	(0.106)
R-squared	0.21	0.24	0.21	0.17	0.25	0.10	0.10	0.11	0.26
Total effect on top-ups	0.061	-0.028	-1.079	-69.807	0.051	0.020	0.056	0.370	-0.067
P-value for non-zero effect on top-ups	0.44	0.52	0.31	0.26	0.62	0.55	0.48	0.56	0.63
Upper bound (total effect)	0.215	0.058	0.992	51.181	0.250	0.088	0.214	1.607	0.204
Lower bound (total effect)	-0.092	-0.114	-3.149	-190.796	-0.149	-0.047	-0.101	-0.868	-0.337
Observations	2165	2216	2201	2215	2201	2201	2193	2195	2123
Mean of dep. var. in control group	5.78	6.60	38.82	2329.06	4.32	0.48	3.12	9.32	0.15

OLS estimates. Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.

Table 5: Regional food security (survey round 2)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) per adult equivalent calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
Panel A: North									
PWP	0.038	-0.208*	-3.987*	66.063	-0.172	0.203**	0.511**	1.406	-0.580*
	(0.122)	(0.121)	(2.148)	(224.061)	(0.202)	(0.061)	(0.179)	(0.995)	(0.302)
PWP * Top-up	-0.224	0.051	-0.615	-447.895**	0.024	-0.309***	-0.713**	-2.126**	0.305
	(0.136)	(0.091)	(1.793)	(140.374)	(0.200)	(0.079)	(0.187)	(0.905)	(0.267)
R-squared	0.36	0.20	0.16	0.15	0.19	0.18	0.09	0.11	0.26
Total effect on top-ups	-0.185	-0.157	-4.602	-381.832	-0.148	-0.106	-0.202	-0.720	-0.275
P-value for non-zero effect on top-ups	0.07	0.50	0.15	0.00	0.73	0.00	0.01	0.06	0.95
Upper bound (total effect)	0.034	0.048	-0.155	-6.362	0.254	0.006	0.121	0.903	0.261
Lower bound (total effect)	-0.405	-0.363	-9.050	-757.302	-0.551	-0.217	-0.524	-2.343	-0.810
Observations	307	310	307	310	307	308	307	307	301
Mean of dep. var. in control group	6.87	7.38	52.97	3230.30	5.95	0.43	2.66	5.43	2.24
Panel B: Central									
PWP	-0.068	-0.055	-1.267	-176.239	-0.074	0.029	0.199	-0.427	-0.189
	(0.154)	(0.084)	(1.884)	(107.781)	(0.205)	(0.052)	(0.145)	(0.917)	(0.283)
PWP * Top-up	0.188	0.037	0.437	142.813*	0.160	0.018	-0.082	0.745	0.123
	(0.117)	(0.055)	(1.414)	(81.181)	(0.154)	(0.047)	(0.133)	(0.843)	(0.199)
R-squared	0.09	0.09	0.07	0.04	0.08	0.09	0.06	0.05	0.10
Total effect on top-ups	0.120	-0.018	-0.831	-33.426	0.087	0.047	0.118	0.318	-0.066
P-value for non-zero effect on top-ups	0.13	0.86	0.87	0.45	0.37	0.41	0.88	0.47	0.87
Upper bound (total effect)	0.386	0.135	2.448	157.891	0.452	0.141	0.389	2.066	0.435
Lower bound (total effect)	-0.146	-0.172	-4.109	-224.742	-0.279	-0.046	-0.154	-1.430	-0.567
Observations	787	822	819	821	819	809	808	808	771
Mean of dep. var. in control group	5.38	6.47	34.33	1997.38	3.95	0.36	2.87	7.75	-0.17
Panel C: South									
PWP	0.179	0.046	0.019	-13.063	0.162	0.048	0.082	-0.331	0.083
	(0.123)	(0.067)	(1.545)	(99.449)	(0.135)	(0.056)	(0.109)	(1.100)	(0.189)
PWP * Top-up	-0.035	-0.016	0.570	-16.477	-0.036	-0.029	-0.044	0.333	-0.012
	(0.059)	(0.048)	(0.955)	(86.951)	(0.099)	(0.039)	(0.078)	(0.942)	(0.135)
R-squared	0.08	0.14	0.16	0.10	0.17	0.03	0.05	0.04	0.16
Total effect on top-ups	0.144	0.030	0.589	-29.541	0.126	0.018	0.038	0.002	0.072
P-value for non-zero effect on top-ups	0.45	0.87	0.57	0.76	0.64	0.89	0.97	0.84	0.82
Upper bound (total effect)	0.381	0.148	3.611	150.841	0.391	0.129	0.255	2.046	0.438
Lower bound (total effect)	-0.092	-0.088	-2.433	-209.922	-0.138	-0.093	-0.178	-2.042	-0.294
Observations	1071	1084	1075	1084	1075	1084	1078	1080	1051
Mean of dep. var. in control group	5.78	6.49	38.41	2342.21	4.16	0.59	3.44	11.56	-0.19

OLS estimates. All specifications include pre-treatment outcomes from the IHS3, residualized to allow for week-of-survey effects in the IHS3 data. Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.

Table 6: Food security (survey round 3)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) per adult equivalent calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
Panel A: full sample, no covariates									
Lean	-0.036	-0.073	-0.715	-22.351	-0.052	0.011	0.032	0.441	-0.129
	(0.121)	(0.051)	(1.202)	(21.604)	(0.094)	(0.028)	(0.093)	(0.404)	(0.156)
Lean season PWP * topup	-0.039	0.058	-0.453	8.070	0.007	-0.061**	-0.161*	-0.353	0.126
	(0.098)	(0.049)	(1.047)	(17.825)	(0.074)	(0.027)	(0.086)	(0.472)	(0.133)
Harvest	-0.227**	-0.105**	-2.010*	-36.834	-0.127	-0.012	-0.032	-0.189	-0.218
	(0.113)	(0.041)	(1.061)	(22.333)	(0.091)	(0.027)	(0.085)	(0.366)	(0.137)
Harvest season PWP * topup	-0.042	0.029	-0.513	-13.509	-0.116	0.001	0.030	0.641	-0.089
	(0.095)	(0.041)	(0.881)	(18.024)	(0.083)	(0.032)	(0.097)	(0.444)	(0.130)
R-squared	0.10	0.16	0.13	0.09	0.18	0.13	0.18	0.21	0.21
Total effect (lean PWP)	-0.076	-0.015	-1.168	-14.281	-0.045	-0.050	-0.129	0.088	-0.002
Upper bound (total effect, lean PWP)	0.143	0.072	1.073	24.259	0.132	0.000	0.031	0.797	0.274
Lower bound (total effect, lean PWP)	-0.294	-0.102	-3.410	-52.822	-0.222	-0.101	-0.290	-0.621	-0.279
Total effect (harvest PWP)	-0.269	-0.076	-2.523	-50.343	-0.244	-0.011	-0.002	0.452	-0.307
Upper bound (total effect, harvest PWP)	-0.060	0.008	-0.482	-10.519	-0.072	0.038	0.150	1.126	-0.051
Lower bound (total effect, harvest PWP)	-0.477	-0.161	-4.565	-90.167	-0.415	-0.060	-0.153	-0.222	-0.562
p-value: direct (lean) = direct (harvest)	0.05	0.16	0.15	0.05	0.02	0.15	0.12	0.37	0.02
p-value: indirect (lean) = indirect (harvest)	0.10	0.51	0.21	0.52	0.40	0.46	0.51	0.17	0.55
Observations	2783	2804	2797	2804	2797	2779	2774	2776	2746
Mean of dep. var. in control group	5.37	6.76	44.30	730.26	4.78	0.29	2.63	5.15	0.16
Panel B: baseline subsample, no covariates									
Lean	0.019	-0.014	-0.207	-5.417	-0.057	0.005	0.014	0.268	-0.057
	(0.140)	(0.063)	(1.342)	(24.936)	(0.109)	(0.033)	(0.099)	(0.472)	(0.176)
Lean season PWP * topup	-0.061	0.019	-0.070	-5.162	0.012	-0.078**	-0.191**	-0.317	0.114
	(0.111)	(0.056)	(1.190)	(19.086)	(0.086)	(0.033)	(0.094)	(0.540)	(0.148)
Harvest	-0.214	-0.089*	-1.575	-33.882	-0.098	-0.007	-0.015	-0.430	-0.200
	(0.135)	(0.052)	(1.274)	(26.173)	(0.111)	(0.035)	(0.101)	(0.466)	(0.168)
Harvest season PWP * topup	0.027	0.042	-0.087	-9.093	-0.102	-0.020	-0.053	0.626	-0.004
	(0.115)	(0.050)	(1.100)	(20.479)	(0.098)	(0.041)	(0.113)	(0.527)	(0.158)
R-squared	0.11	0.16	0.13	0.08	0.18	0.12	0.17	0.19	0.20
Total effect (lean PWP)	-0.042	0.004	-0.276	-10.579	-0.044	-0.072	-0.177	-0.049	0.057
Upper bound (total effect, lean PWP)	0.199	0.102	2.099	33.403	0.161	-0.017	-0.008	0.768	0.368
Lower bound (total effect, lean PWP)	-0.283	-0.094	-2.652	-54.560	-0.249	-0.127	-0.346	-0.866	-0.253
Total effect (harvest PWP)	-0.187	-0.047	-1.662	-42.976	-0.200	-0.028	-0.068	0.196	-0.204
Upper bound (total effect, harvest PWP)	0.052	0.051	0.583	3.120	-0.000	0.029	0.098	1.004	0.095
Lower bound (total effect, harvest PWP)	-0.427	-0.146	-3.907	-89.072	-0.400	-0.085	-0.233	-0.612	-0.502
p-value: direct (lean) = direct (harvest)	0.19	0.28	0.17	0.09	0.09	0.13	0.22	0.58	0.06
p-value: indirect (lean) = indirect (harvest)	0.10	0.23	0.29	0.27	0.69	0.75	0.80	0.20	0.41
Panel C: baseline sub-sample, including pre-treatment outcomes									
Lean	0.020	0.002	0.041	-6.366	0.005	0.005	0.018	0.313	-0.018
	(0.125)	(0.059)	(1.311)	(23.770)	(0.109)	(0.033)	(0.099)	(0.477)	(0.172)
Lean season PWP * topup	-0.085	0.008	-0.449	-8.811	-0.031	-0.077**	-0.178*	-0.291	0.046
	(0.112)	(0.055)	(1.223)	(19.892)	(0.092)	(0.033)	(0.092)	(0.536)	(0.150)
Harvest	-0.136	-0.051	-0.871	-28.704	-0.021	-0.008	-0.005	-0.363	-0.110
	(0.117)	(0.046)	(1.208)	(24.837)	(0.107)	(0.035)	(0.101)	(0.466)	(0.158)
Harvest season PWP * topup	-0.006	0.022	-0.395	-7.981	-0.131	-0.020	-0.047	0.630	-0.056
	(0.110)	(0.050)	(1.072)	(19.509)	(0.096)	(0.041)	(0.115)	(0.525)	(0.154)
R-squared	0.19	0.22	0.17	0.15	0.21	0.13	0.18	0.20	0.26
Total effect (lean PWP)	-0.065	0.010	-0.408	-15.178	-0.026	-0.071	-0.160	0.023	0.027
Upper bound (total effect, lean PWP)	0.139	0.095	1.826	27.517	0.172	-0.016	0.007	0.849	0.317
Lower bound (total effect, lean PWP)	-0.270	-0.075	-2.643	-57.873	-0.223	-0.126	-0.328	-0.803	-0.262
Total effect (harvest PWP)	-0.143	-0.029	-1.266	-36.684	-0.152	-0.028	-0.053	0.267	-0.167
Upper bound (total effect, harvest PWP)	0.064	0.060	0.883	6.515	0.045	0.029	0.112	1.082	0.113
Lower bound (total effect, harvest PWP)	-0.349	-0.118	-3.415	-79.883	-0.348	-0.084	-0.217	-0.547	-0.446
p-value: direct (lean) = direct (harvest)	0.44	0.39	0.39	0.28	0.17	0.14	0.23	0.58	0.15
p-value: indirect (lean) = indirect (harvest)	0.24	0.38	0.48	0.38	0.80	0.73	0.84	0.21	0.59
Observations	2167	2197	2190	2197	2190	2179	2175	2177	2138
Mean of dep. var. in control group	5.37	6.77	44.48	732.59	4.82	0.32	2.71	5.69	0.15

OLS estimates. Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.

Table 7: Regional food security (survey round 3)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) per adult equivalent calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
Panel A: North									
Lean	-0.074	-0.244	-5.326	-138.945	-0.611**	0.082	0.360	1.542	-0.858*
	(0.306)	(0.151)	(4.440)	(291.417)	(0.232)	(0.053)	(0.247)	(1.189)	(0.452)
Lean season PWP * topup	0.224	0.281	7.132**	281.108	0.502**	-0.122	-0.549**	-0.938	0.957**
	(0.363)	(0.189)	(2.538)	(300.595)	(0.165)	(0.087)	(0.248)	(0.949)	(0.448)
Harvest	-0.327	-0.142	-5.158	63.711	-0.564**	0.244**	0.641**	2.668*	-0.914**
	(0.246)	(0.091)	(3.856)	(149.937)	(0.241)	(0.101)	(0.265)	(1.371)	(0.404)
Harvest season PWP * topup	0.418	0.159	2.534	-58.121	0.175	-0.330**	-0.777**	-2.283	0.762*
	(0.272)	(0.099)	(1.570)	(147.801)	(0.123)	(0.125)	(0.333)	(1.397)	(0.383)
R-squared	0.28	0.24	0.18	0.14	0.29	0.09	0.07	0.04	0.25
Total effect (lean PWP)	0.149	0.036	1.806	142.162	-0.109	-0.041	-0.190	0.604	0.098
Upper bound (total effect, lean PWP)	0.590	0.271	9.348	409.881	0.394	0.083	0.160	2.512	0.888
Lower bound (total effect, lean PWP)	-0.292	-0.198	-5.735	-125.556	-0.613	-0.165	-0.539	-1.304	-0.691
Total effect (harvest PWP)	0.091	0.016	-2.624	5.590	-0.390	-0.086	-0.136	0.384	-0.151
Upper bound (total effect, harvest PWP)	0.485	0.248	4.914	386.242	0.034	0.011	0.244	2.444	0.595
Lower bound (total effect, harvest PWP)	-0.303	-0.215	-10.161	-375.063	-0.813	-0.183	-0.516	-1.675	-0.898
p-value: direct (lean) = direct (harvest)	0.83	0.89	0.08	0.53	0.18	0.52	0.83	0.81	0.58
p-value: indirect (lean) = indirect (harvest)	0.44	0.44	0.96	0.47	0.78	0.16	0.38	0.45	0.90
Observations	303	306	303	306	303	306	305	306	299
Mean of dep. var. in control group	6.33	7.35	54.20	3109.72	6.26	0.21	2.29	3.55	1.80
Panel B: Center									
Lean	0.027	0.076	1.252	22.502	0.190	-0.085**	-0.205	-0.561	0.311
	(0.269)	(0.106)	(2.464)	(149.156)	(0.194)	(0.041)	(0.173)	(0.514)	(0.315)
Lean season PWP * topup	0.058	-0.039	0.503	-46.508	-0.012	0.023	-0.093	0.110	-0.014
	(0.173)	(0.075)	(2.045)	(110.372)	(0.156)	(0.038)	(0.154)	(0.654)	(0.229)
Harvest	-0.030	0.098	1.959	-9.626	0.325	-0.081**	-0.167	-0.718	0.400
	(0.234)	(0.087)	(2.292)	(133.070)	(0.201)	(0.036)	(0.146)	(0.538)	(0.277)
Harvest season PWP * topup	-0.081	-0.122*	-2.587	-113.317	-0.327*	0.069	0.164	1.216*	-0.463*
	(0.189)	(0.068)	(1.849)	(121.448)	(0.185)	(0.050)	(0.176)	(0.668)	(0.251)
R-squared	0.11	0.09	0.12	0.06	0.05	0.06	0.16	0.07	0.12
Total effect (lean PWP)	0.085	0.037	1.755	-24.006	0.178	-0.062	-0.298	-0.451	0.297
Upper bound (total effect, lean PWP)	0.496	0.201	5.767	233.485	0.513	0.007	-0.019	0.545	0.803
Lower bound (total effect, lean PWP)	-0.325	-0.127	-2.257	-281.498	-0.157	-0.130	-0.577	-1.448	-0.208
Total effect (harvest PWP)	-0.111	-0.024	-0.627	-122.943	-0.002	-0.012	-0.003	0.499	-0.063
Upper bound (total effect, harvest PWP)	0.326	0.137	3.527	103.946	0.359	0.060	0.268	1.461	0.463
Lower bound (total effect, harvest PWP)	-0.547	-0.185	-4.782	-349.833	-0.363	-0.083	-0.274	-0.463	-0.590
p-value: direct (lean) = direct (harvest)	0.20	0.34	0.16	0.36	0.26	0.22	0.04	0.11	0.10
p-value: indirect (lean) = indirect (harvest)	0.81	0.81	0.76	0.82	0.48	0.93	0.83	0.80	0.75
Observations	803	817	816	817	816	815	814	814	799
Mean of dep. var. in control group	5.15	6.72	43.69	2440.25	4.48	0.20	2.33	3.11	0.13
Panel C: South									
Lean	0.106	0.039	1.004	-21.687	0.031	0.062	0.104	0.643	0.008
	(0.143)	(0.074)	(1.486)	(113.464)	(0.144)	(0.057)	(0.136)	(0.837)	(0.223)
Lean season PWP * topup	-0.263*	-0.027	-3.051*	-13.066	-0.193	-0.144**	-0.150	-0.375	-0.114
	(0.151)	(0.075)	(1.548)	(112.308)	(0.120)	(0.053)	(0.125)	(0.944)	(0.203)
Harvest	-0.109	-0.134**	-1.632	-157.059*	-0.161	-0.026	-0.059	-0.945	-0.191
	(0.161)	(0.062)	(1.339)	(91.914)	(0.130)	(0.052)	(0.142)	(0.746)	(0.194)
Harvest season PWP * topup	-0.089	0.082	0.060	134.343	-0.091	0.013	0.023	1.108	-0.012
	(0.149)	(0.075)	(1.550)	(105.327)	(0.130)	(0.055)	(0.146)	(0.792)	(0.196)
R-squared	0.13	0.17	0.14	0.12	0.13	0.08	0.08	0.13	0.21
Total effect (lean PWP)	-0.157	0.012	-2.047	-34.753	-0.161	-0.081	-0.046	0.268	-0.107
Upper bound (total effect, lean PWP)	0.112	0.124	0.407	151.498	0.097	0.011	0.187	1.668	0.267
Lower bound (total effect, lean PWP)	-0.426	-0.100	-4.502	-221.004	-0.420	-0.174	-0.279	-1.132	-0.481
Total effect (harvest PWP)	-0.198	-0.052	-1.572	-22.716	-0.252	-0.013	-0.036	0.163	-0.203
Upper bound (total effect, harvest PWP)	0.063	0.063	0.516	158.527	0.006	0.085	0.198	1.568	0.140
Lower bound (total effect, harvest PWP)	-0.460	-0.168	-3.661	-203.959	-0.509	-0.111	-0.269	-1.242	-0.546
p-value: direct (lean) = direct (harvest)	0.78	0.33	0.70	0.91	0.48	0.15	0.93	0.89	0.58
p-value: indirect (lean) = indirect (harvest)	0.24	0.04	0.12	0.27	0.17	0.14	0.30	0.08	0.38
Observations	1061	1074	1071	1074	1071	1058	1056	1057	1040
Mean of dep. var. in control group	5.28	6.66	42.46	2650.42	4.69	0.44	3.12	8.24	-0.31

OLS estimates. All specifications include pre-treatment outcomes from the IHS3, residualized to allow for week-of-survey effects in the IHS3 data. Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.

Table 8: Labor supply (survey round 2)

Dependent variable:	(1) Indicator: any HH MASAF	(2) Total person-days of MASAF	(3) Indicator: any HH ganyu	(4) Total HH person-days of ganyu
PWP	0.118***	1.531***	0.032	0.329
	(0.026)	(0.348)	(0.032)	(0.632)
PWP * Top-up	0.374***	4.663***	-0.016	0.160
	(0.025)	(0.339)	(0.019)	(0.421)
R-squared	0.26	0.21	0.03	0.04
P-value for non-zero effect on top-ups	0.00	0.00	0.62	0.43
Upper bound (total effect)	0.547	6.979	0.076	1.713
Lower bound (total effect)	0.437	5.410	-0.045	-0.735
Observations	2820	2836	2818	2836
Mean of dep. var. in control group	0.04	0.51	0.41	4.43

OLS estimates. Standard errors clustered at the EA level. All estimates include district and week-of-interview fixed effects. $*p < 0.10, **p < 0.05, ***p < 0.001$

Table 9: Food security, TOT (round 2)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) per adult equivalent calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
HH participated in MASAF	-0.124	-0.171	-5.580*	-82.769*	-0.299	0.110	0.319	1.283	-0.682
in past 30 days	(0.225)	(0.122)	(3.089)	(50.278)	(0.302)	(0.093)	(0.228)	(1.748)	(0.423)
R-squared	0.15	0.16	0.12	0.07	0.17	0.07	0.07	0.09	0.13
Observations	2776	2820	2805	2820	2805	2789	2781	2783	2724

2SLS estimates. HH MASAF participation instrumented with PWP.
Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.

Table 10: Regional food security, TOT (survey round 2)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) per adult equivalent calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
Panel A: North									
HH participated in PWP in past 30 days	-0.509	-0.447*	-12.501**	-549.642	-0.361	-0.098	0.004	-1.639	-0.811
	(0.375)	(0.236)	(5.060)	(531.664)	(0.444)	(0.185)	(0.409)	(2.550)	(0.728)
R-squared	0.24	0.00	0.00	0.09	0.09	0.06	0.03	0.05	0.12
Observations	336	338	337	338	337	336	335	335	332
Panel B: Center									
HH participated in PWP in past 30 days	-0.349	-0.257	-7.461	-351.598	-0.375	0.253	0.670	2.507	-1.189
	(0.527)	(0.275)	(5.551)	(277.377)	(0.636)	(0.183)	(0.532)	(3.223)	(0.900)
R-squared	0.03	0.03	0.00	0.00	0.05	0.03	0.00	0.02	0.00
Observations	945	983	981	983	981	960	959	959	921
Panel C: South									
HH participated in PWP in past 30 days	0.149	-0.050	-1.268	-257.588	-0.162	0.062	0.118	0.380	-0.270
	(0.262)	(0.148)	(3.773)	(218.456)	(0.375)	(0.116)	(0.237)	(2.324)	(0.498)
R-squared	0.01	0.05	0.08	0.05	0.07	0.03	0.04	0.05	0.04
Observations	1495	1499	1487	1499	1487	1493	1487	1489	1471

2SLS estimates. HH PWP participation instrumented with PWP and PWP * Top-up.
Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.

Table 11: Durable goods (survey round 2)

Dependent variable:	(1) Indicator: HH owns at least 1 of the 32 durable goods	(2) Number of durable goods owned	(3) PCA index for durable goods
PWP	-0.034	-0.256	-0.084
	(0.022)	(0.209)	(0.191)
PWP * Top-up	0.009	-0.033	-0.095
	(0.019)	(0.120)	(0.101)
R-squared	0.04	0.06	0.03
P-value for non-zero effect on top-ups	0.24	0.15	0.34
Upper bound (total effect)	0.017	0.107	0.190
Lower bound (total effect)	-0.067	-0.685	-0.548
Observations	2831	2831	2253
Mean of dep. var. in control group	0.82	2.89	-0.03

OLS estimates. Standard errors clustered at the EA level.
The PCA index for durable goods is only defined for HHs that own at least one good.
Estimates include district and week-of-interview fixed effects.
$*p < 0.10, **p < 0.05, ***p < 0.001$

Table 12: Agricultural inputs (survey round 2)

Dependent variable:	(1) Indicator: HH used any fertilizer	(2) ln (kg fertilizer, 1st application)	(3) ln (kg fertilizer, 2nd application)	(4) ln (exp. fertilizer, 1st application)	(5) ln (exp. fertilizer, 2nd application)
PWP	0.010	0.020	0.051	-0.084	0.009
	(0.032)	(0.148)	(0.165)	(0.263)	(0.283)
PWP * Top-up	0.003	0.040	0.082	0.038	0.135
	(0.018)	(0.083)	(0.106)	(0.152)	(0.178)
R-squared	0.24	0.24	0.11	0.23	0.11
Total effect on top-ups	0.013	0.060	0.133	-0.046	0.145
P-value for non-zero effect on top-ups	0.68	0.68	0.40	0.86	0.58
Upper bound (total effect)	0.074	0.343	0.438	0.453	0.660
Lower bound (total effect)	-0.048	-0.223	-0.173	-0.545	-0.371
Observations	2764	2764	2764	2764	2764
Mean of dep. var. in control group	0.68	3.07	1.98	5.26	3.41

OLS estimates. Standard errors clustered at the EA level.
Estimates include district and week-of-interview fixed effects.
$*p < 0.10, **p < 0.05, ***p < 0.001$

Table 13: Regional agricultural inputs (survey round 2)

Dependent variable:	(1) Indicator: HH used any fertilizer	(2) ln (kg fertilizer, 1st application)	(3) ln (kg fertilizer, 2nd application)
Panel A: North			
PWP	0.236**	0.964*	1.083**
	(0.111)	(0.490)	(0.470)
PWP * Top-up	-0.064	-0.294	-0.274
	(0.058)	(0.244)	(0.329)
R-squared	0.16	0.22	0.16
Total effect on top-ups	0.171	0.670	0.809
P-value for non-zero effect on top-ups	0.13	0.18	0.04
Upper bound (total effect)	0.385	1.616	1.521
Lower bound (total effect)	-0.042	-0.276	0.097
Observations	278	278	278
Mean of dep. var. in control group	0.42	1.90	1.44
Panel B: Center			
PWP	-0.005	0.009	0.175
	(0.035)	(0.195)	(0.227)
PWP * Top-up	0.042	0.202	0.282
	(0.033)	(0.159)	(0.222)
R-squared	0.11	0.15	0.12
Total effect on top-ups	0.037	0.211	0.458
P-value for non-zero effect on top-ups	0.25	0.27	0.03
Upper bound (total effect)	0.100	0.583	0.869
Lower bound (total effect)	-0.026	-0.161	0.046
Observations	779	779	779
Mean of dep. var. in control group	0.78	3.67	2.00
Panel C: South			
PWP	0.009	0.103	-0.238
	(0.060)	(0.268)	(0.289)
PWP * Top-up	0.001	0.017	0.138
	(0.026)	(0.120)	(0.163)
R-squared	0.37	0.36	0.17
Total effect on top-ups	0.010	0.120	-0.100
P-value for non-zero effect on top-ups	0.87	0.64	0.70
Upper bound (total effect)	0.123	0.622	0.416
Lower bound (total effect)	-0.103	-0.381	-0.616
Observations	1006	1007	1007
Mean of dep. var. in control group	0.61	2.68	1.84

OLS estimates. All specifications include pre-treatment outcomes from the IHS3, residualized to allow for week-of-survey effects in the IHS3 data. Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$ Estimates include district and week-of-interview fixed effects.

Table 14: Other programs (survey round 2)

Dependent variable:	(1) Indicator: any free food or maize	(2) Indicator: any school feeding	(3) Indicator: any scholarship	(4) Indicator: any cash transfer	(5) Indicator any fertilizer coupon
PWP	0.002	0.004	-0.002	-0.000	0.047
	(0.025)	(0.030)	(0.004)	(0.005)	(0.032)
PWP * Top-up	-0.013	-0.009	-0.002	0.004	0.018
	(0.013)	(0.017)	(0.003)	(0.005)	(0.021)
R-squared	0.24	0.24	0.01	0.02	0.15
P-value for non-zero effect on top-ups	0.67	0.84	0.19	0.51	0.04
Upper bound (total effect)	0.037	0.053	0.002	0.015	0.124
Lower bound (total effect)	-0.058	-0.065	-0.010	-0.007	0.005
Observations	2840	2840	2840	2840	2840
Mean of dep. var. in control group	0.14	0.22	0.01	0.01	0.50

OLS estimates. Standard errors clustered at the EA level.
Estimates include district and week-of-interview fixed effects.
$*p < 0.10, **p < 0.05, ***p < 0.001$

Table 15: Transfers and loans (survey round 2)

Dependent variable:	(1) Indicator: any transfers received	(2) Indicator: any transfers given	(3) Indicator: any loans received	(4) Indicator: any loan payments
PWP	0.005	-0.053**	-0.032	0.003
	(0.025)	(0.020)	(0.024)	(0.013)
PWP * Top-up	-0.028	0.007	-0.002	0.001
	(0.020)	(0.016)	(0.016)	(0.011)
R-squared	0.11	0.18	0.04	0.03
P-value for non-zero effect on top-ups	0.33	0.02	0.13	0.74
Upper bound (total effect)	0.023	-0.009	0.010	0.027
Lower bound (total effect)	-0.070	-0.084	-0.079	-0.019
Observations	2840	2840	2840	2840
Mean of dep. var. in control group	0.24	0.22	0.20	0.06

OLS estimates. Standard errors clustered at the EA level.
Estimates include district and week-of-interview fixed effects.
$*p < 0.10, **p < 0.05, ***p < 0.001$

Table 16: Price index (survey round 2)

	(1) National	(2) North	(3) Center	(4) South
PWP	-3.335	-2.404	-36.293	30.750
	(16.613)	(17.735)	(23.572)	(27.488)
R-squared	0.27	0.63	0.10	0.26
Observations	2793	339	959	1495
Mean of dep. var. in control group	485.25	556.2	512.531	452.08

OLS estimates. Standard errors clustered at the EA level.
Estimates include district and week-of-interview fixed effects.
$*p < 0.10, **p < 0.05, ***p < 0.001$

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Online Appendix (not for publication)

Table A1: Regional food security: balancing tests (IHS3)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) per adult equivalent calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
Panel A: North									
PWP	-0.217	-0.062	-1.430	-380.847	-0.098	-0.226*	-0.712	-5.217*	0.294
	(0.236)	(0.109)	(2.142)	(244.381)	(0.183)	(0.130)	(0.420)	(2.736)	(0.379)
PWP * Top-up	0.097	0.051	2.258	164.287	0.370**	0.033	0.147	0.941	0.174
	(0.132)	(0.080)	(2.141)	(117.519)	(0.153)	(0.046)	(0.159)	(0.662)	(0.152)
R-squared	0.10	0.03	0.09	0.06	0.12	0.11	0.09	0.13	0.06
Total effect on top-ups	-0.120	-0.011	0.828	-216.559	0.272	-0.193	-0.565	-4.276	0.468
P-value for non-zero effect on top-ups	0.59	0.91	0.72	0.38	0.10	0.14	0.19	0.13	0.24
Upper bound (total effect)	0.315	0.173	5.313	255.711	0.584	0.055	0.253	1.071	1.221
Lower bound (total effect)	-0.555	-0.195	-3.658	-688.829	-0.039	-0.441	-1.383	-9.623	-0.285
Observations	317	318	316	318	316	318	318	318	315
Mean of dep. var. in control group	5.61	6.34	45.15	2874.98	5.18	0.32	2.35	6.58	0.22
Panel B: Central									
PWP	-0.171	-0.027	-1.630	122.477	-0.360**	0.061**	0.271*	0.122	-0.307
	(0.186)	(0.089)	(2.411)	(117.671)	(0.172)	(0.028)	(0.148)	(0.790)	(0.283)
PWP * Top-up	0.068	0.041	2.227*	-31.790	0.141	-0.016	-0.191*	-0.357	0.206
	(0.088)	(0.057)	(1.322)	(90.828)	(0.107)	(0.026)	(0.105)	(0.407)	(0.145)
R-squared	0.04	0.09	0.12	0.19	0.08	0.03	0.04	0.05	0.08
Total effect on top-ups	-0.103	0.014	0.597	90.687	-0.218	0.045	0.080	-0.235	-0.101
P-value for non-zero effect on top-ups	0.55	0.86	0.81	0.41	0.18	0.12	0.54	0.76	0.70
Upper bound (total effect)	0.233	0.172	5.328	306.205	0.098	0.101	0.332	1.268	0.417
Lower bound (total effect)	-0.440	-0.143	-4.135	-124.831	-0.535	-0.011	-0.172	-1.739	-0.618
Observations	846	850	849	850	849	850	850	850	845
Mean of dep. var. in control group	5.11	6.20	45.08	2512.70	4.98	0.10	1.89	3.26	0.22
Panel C: South									
PWP	-0.060	-0.148*	-2.043	-133.039*	-0.327**	0.035	-0.094	0.262	-0.317
	(0.133)	(0.075)	(1.880)	(74.876)	(0.133)	(0.035)	(0.108)	(0.744)	(0.206)
PWP * Top-up	0.053	0.055	0.576	62.504	0.152*	-0.028	-0.150*	-0.391	0.207*
	(0.075)	(0.050)	(1.040)	(77.293)	(0.083)	(0.027)	(0.084)	(0.461)	(0.124)
R-squared	0.05	0.14	0.09	0.09	0.13	0.21	0.32	0.23	0.21
Total effect on top-ups	-0.008	-0.093	-1.467	-70.535	-0.175	0.007	-0.245	-0.129	-0.110
P-value for non-zero effect on top-ups	0.95	0.20	0.42	0.30	0.17	0.84	0.02	0.86	0.59
Upper bound (total effect)	0.241	0.048	2.069	62.959	0.070	0.074	-0.040	1.254	0.284
Lower bound (total effect)	-0.256	-0.233	-5.004	-204.029	-0.419	-0.061	-0.449	-1.512	-0.503
Observations	1097	1106	1105	1106	1105	1106	1106	1106	1096
Mean of dep. var. in control group	5.29	6.07	45.57	2482.46	5.12	0.28	2.68	5.03	-0.11

OLS estimates. Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.

Table A2: Balance (IHS survey)

	(1) Indicator: any HH ganyu	(2) Total HH person-days ganyu	(3) Indicator: HH used any fertilizer	(4) ln (kg fertilizer 1st application)	(5) ln (kg fertilizer, 2nd application)	(6) Indicator: any food or maize	(7) Indicator: any school feeding	(8) Indicator: any scholarship	(9) Indicator: any cash transfer	(10) Indicator: HH owns at least 1 of the 32 durable goods	(11) Number of durable goods owned	(12) PCA index for durable goods
PWP	0.055	8.202	-0.043	-0.265	0.050	0.032**	-0.016	-0.000	0.004	-0.026	-0.120	-0.012
	(0.035)	(5.543)	(0.034)	(0.165)	(0.194)	(0.016)	(0.039)	(0.003)	(0.005)	(0.027)	(0.239)	(0.151)
PWP * Top-up	-0.039	-2.819	0.010	0.109	0.031	-0.016	0.006	-0.001	-0.006	0.024	-0.036	-0.012
	(0.024)	(4.068)	(0.016)	(0.084)	(0.112)	(0.011)	(0.012)	(0.002)	(0.004)	(0.019)	(0.114)	(0.063)
R-squared	0.07	0.14	0.38	0.37	0.13	0.08	0.19	0.01	0.03	0.03	0.06	0.06
Total effect on top-ups	0.016	5.383	-0.033	-0.156	0.081	0.016	-0.010	-0.001	-0.002	-0.002	-0.155	-0.025
P-value for non-zero effect on top-ups	0.63	0.31	0.30	0.32	0.66	0.27	0.79	0.65	0.65	0.94	0.50	0.87
Upper bound (total effect)	0.081	15.731	0.030	0.152	0.441	0.045	0.066	0.004	0.006	0.048	0.293	0.265
Lower bound (total effect)	-0.049	-4.966	-0.096	-0.464	-0.279	-0.012	-0.086	-0.006	-0.009	-0.052	-0.604	-0.315
Observations	2274	2274	2125	2126	2126	2274	2274	2274	2274	2274	2274	1850
Mean of dep. var. in control group	0.46	37.73	0.70	3.43	1.49	0.04	0.15	0.00	0.00	0.82	2.78	-0.68

OLS estimates. Standard errors clustered at the EA level. Estimates include district and week-of-interview fixed effects.
The PCA index for durable goods is only defined for HHs that own at least one good. $*p < 0.10, **p < 0.05, ***p < 0.001$

Table A3: Food security (Northern region, survey round 2)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) per adult equivalent calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
Panel A: full sample, no covariates									
PWP	-0.085	-0.177	-3.189	-4.790	-0.161	0.086	0.271	0.191	-0.369
	(0.160)	(0.116)	(2.113)	(214.986)	(0.182)	(0.068)	(0.168)	(0.915)	(0.318)
PWP * Top-up	-0.149	0.047	-1.626	-357.906**	0.052	-0.233**	-0.534**	-1.426	0.197
	(0.140)	(0.083)	(2.091)	(102.649)	(0.205)	(0.080)	(0.188)	(0.913)	(0.287)
R-squared	0.26	0.08	0.06	0.14	0.10	0.10	0.06	0.07	0.15
P-value for non-zero effect on top-ups	0.18	0.24	0.03	0.08	0.60	0.04	0.12	0.16	0.60
Upper bound (total effect)	0.093	0.081	-0.665	30.276	0.291	-0.013	0.056	0.420	0.459
Lower bound (total effect)	-0.559	-0.340	-8.964	-755.669	-0.507	-0.281	-0.582	-2.890	-0.802
Observations	337	339	338	339	338	337	336	336	333
Mean of dep. var. in control group	6.84	7.35	52.39	3210.67	5.92	0.45	2.70	5.63	2.16
Panel B: baseline subsample, no covariates									
PWP	-0.052	-0.226*	-5.210**	48.901	-0.241	0.163**	0.468**	0.869	-0.573
	(0.147)	(0.128)	(2.101)	(225.883)	(0.203)	(0.073)	(0.189)	(1.063)	(0.347)
PWP * Top-up	-0.194	0.068	0.227	-428.807**	0.131	-0.297**	-0.694**	-1.955**	0.356
	(0.146)	(0.098)	(1.759)	(142.158)	(0.193)	(0.079)	(0.192)	(0.903)	(0.286)
R-squared	0.26	0.08	0.07	0.14	0.12	0.14	0.08	0.08	0.16
Total effect on top-ups	-0.246	-0.158	-4.983	-379.906	-0.110	-0.134	-0.226	-1.086	-0.217
P-value for non-zero effect on top-ups	0.13	0.17	0.04	0.07	0.62	0.08	0.22	0.26	0.53
Upper bound (total effect)	0.059	0.058	-0.457	3.088	0.315	0.007	0.123	0.750	0.453
Lower bound (total effect)	-0.550	-0.375	-9.509	-762.899	-0.535	-0.275	-0.575	-2.923	-0.887
Panel C: baseline sub-sample, including pre-treatment outcomes									
PWP	0.038	-0.208*	-3.987*	66.063	-0.172	0.203**	0.511**	1.406	-0.580*
	(0.122)	(0.121)	(2.148)	(224.061)	(0.202)	(0.061)	(0.179)	(0.995)	(0.302)
PWP * Top-up	-0.224	0.051	-0.615	-447.895**	0.024	-0.309***	-0.713**	-2.126**	0.305
	(0.136)	(0.091)	(1.793)	(140.374)	(0.200)	(0.079)	(0.187)	(0.905)	(0.267)
R-squared	0.36	0.20	0.16	0.15	0.19	0.18	0.09	0.11	0.26
Total effect on top-ups	-0.185	-0.157	-4.602	-381.832	-0.148	-0.106	-0.202	-0.720	-0.275
P-value for non-zero effect on top-ups	0.07	0.50	0.15	0.00	0.73	0.00	0.01	0.06	0.95
Upper bound (total effect)	0.034	0.048	-0.155	-6.362	0.254	0.006	0.121	0.903	0.261
Lower bound (total effect)	-0.405	-0.363	-9.050	-757.302	-0.551	-0.217	-0.524	-2.343	-0.810
Observations	307	310	307	310	307	308	307	307	301
Mean of dep. var. in control group	6.87	7.38	52.97	3230.30	5.95	0.43	2.66	5.43	2.24

OLS estimates. Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.

Table A4: Food security (Central region, survey round 2)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) per adult equivalent calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
Panel A: full sample, no covariates									
PWP	-0.161	-0.078	-2.287	-146.233*	-0.184	0.088	0.265*	0.830	-0.422*
	(0.145)	(0.072)	(1.497)	(85.239)	(0.169)	(0.053)	(0.144)	(0.891)	(0.243)
PWP * Top-up	0.133	0.011	0.679	82.720	0.155	-0.033	-0.152	-0.092	0.168
	(0.110)	(0.049)	(1.189)	(78.535)	(0.119)	(0.043)	(0.120)	(0.777)	(0.176)
R-squared	0.05	0.05	0.04	0.02	0.06	0.07	0.05	0.04	0.04
P-value for non-zero effect on top-ups	0.84	0.35	0.26	0.41	0.86	0.27	0.44	0.41	0.28
Upper bound (total effect)	0.239	0.072	1.169	86.055	0.293	0.152	0.398	2.500	0.206
Lower bound (total effect)	-0.296	-0.206	-4.386	-213.081	-0.351	-0.042	-0.173	-1.024	-0.713
Observations	952	990	988	988	988	967	966	966	926
Mean of dep. var. in control group	5.44	6.48	34.41	2037.26	3.98	0.36	2.88	7.39	-0.11
Panel B: baseline subsample, no covariates									
PWP	-0.088	-0.063	-1.755	-179.367*	-0.131	0.041	0.225	-0.318	-0.291
	(0.160)	(0.083)	(1.878)	(105.339)	(0.211)	(0.052)	(0.146)	(0.943)	(0.278)
PWP * Top-up	0.202*	0.044	0.788	139.589*	0.188	0.015	-0.094	0.682	0.170
	(0.115)	(0.055)	(1.451)	(82.978)	(0.152)	(0.047)	(0.132)	(0.835)	(0.197)
R-squared	0.06	0.05	0.04	0.02	0.06	0.08	0.05	0.04	0.04
Total effect on top-ups	0.115	-0.019	-0.967	-39.778	0.057	0.056	0.131	0.364	-0.120
P-value for non-zero effect on top-ups	0.43	0.81	0.56	0.67	0.77	0.24	0.34	0.70	0.64
Upper bound (total effect)	0.398	0.134	2.299	143.285	0.432	0.148	0.400	2.180	0.375
Lower bound (total effect)	-0.169	-0.172	-4.233	-222.841	-0.317	-0.036	-0.138	-1.452	-0.615
Panel C: baseline sub-sample, including pre-treatment outcomes									
PWP	-0.068	-0.055	-1.267	-176.239	-0.074	0.029	0.199	-0.427	-0.189
	(0.154)	(0.084)	(1.884)	(107.781)	(0.205)	(0.052)	(0.145)	(0.917)	(0.283)
PWP * Top-up	0.188	0.037	0.437	142.813*	0.160	0.018	-0.082	0.745	0.123
	(0.117)	(0.055)	(1.414)	(81.181)	(0.154)	(0.047)	(0.133)	(0.843)	(0.199)
R-squared	0.09	0.09	0.07	0.04	0.08	0.09	0.06	0.05	0.10
Total effect on top-ups	0.120	-0.018	-0.831	-33.426	0.087	0.047	0.118	0.318	-0.066
P-value for non-zero effect on top-ups	0.13	0.86	0.87	0.45	0.37	0.41	0.88	0.47	0.87
Upper bound (total effect)	0.386	0.135	2.448	157.891	0.452	0.141	0.389	2.066	0.435
Lower bound (total effect)	-0.146	-0.172	-4.109	-224.742	-0.279	-0.046	-0.154	-1.430	-0.567
Observations	787	822	819	821	819	809	808	808	771
Mean of dep. var. in control group	5.38	6.47	34.33	1997.38	3.95	0.36	2.87	7.75	-0.17

OLS estimates. Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.

Table A5: Food security (Southern region, survey round 2)

Dependent variable:	(1) ln (p.c. food exp.) last week	(2) ln (p.c. food cons.) last week	(3) food cons. score	(4) total HH calories	(5) # food groups consumed	(6) any day w/reduced meals	(7) food insecurity score	(8) coping strategy index	(9) PCA index cols. 1-8
Panel A: full sample, no covariates									
PWP	0.090	-0.030	-0.760	-129.604	-0.027	0.035	0.064	0.109	-0.134
	(0.108)	(0.066)	(1.591)	(95.622)	(0.159)	(0.050)	(0.100)	(1.036)	(0.215)
PWP * Top-up	-0.051	0.014	0.262	49.405	-0.060	-0.018	-0.023	0.055	0.040
	(0.051)	(0.045)	(0.855)	(74.091)	(0.089)	(0.032)	(0.068)	(0.739)	(0.125)
R-squared	0.01	0.06	0.09	0.06	0.09	0.03	0.04	0.05	0.04
P-value for non-zero effect on top-ups	0.71	0.80	0.75	0.39	0.58	0.73	0.68	0.87	0.65
Upper bound (total effect)	0.249	0.104	2.562	102.727	0.220	0.115	0.238	2.095	0.311
Lower bound (total effect)	-0.169	-0.136	-3.558	-263.126	-0.393	-0.081	-0.156	-1.767	-0.499
Observations	1503	1508	1499	1508	1499	1504	1498	1500	1479
Mean of dep. var. in control group	5.89	6.49	38.22	2382.57	4.24	0.59	3.36	10.94	-0.10
Panel B: baseline subsample, no covariates									
PWP	0.161	0.001	-0.378	-28.741	0.095	0.049	0.076	-0.343	-0.010
	(0.131)	(0.074)	(1.705)	(98.328)	(0.151)	(0.056)	(0.114)	(1.117)	(0.221)
PWP * Top-up	-0.023	-0.002	0.704	-4.712	0.005	-0.031	-0.057	0.312	0.041
	(0.064)	(0.050)	(0.970)	(85.672)	(0.103)	(0.039)	(0.078)	(0.945)	(0.142)
R-squared	0.02	0.07	0.09	0.07	0.10	0.03	0.04	0.03	0.05
Total effect on top-ups	0.138	-0.001	0.326	-33.453	0.100	0.018	0.018	-0.031	0.032
P-value for non-zero effect on top-ups	0.28	0.99	0.85	0.72	0.51	0.75	0.87	0.98	0.89
Upper bound (total effect)	0.387	0.130	3.673	147.250	0.396	0.130	0.240	2.066	0.460
Lower bound (total effect)	-0.110	-0.131	-3.021	-214.155	-0.196	-0.093	-0.204	-2.128	-0.397
Panel C: baseline sub-sample, including pre-treatment outcomes									
PWP	0.179	0.046	0.019	-13.063	0.162	0.048	0.082	-0.331	0.083
	(0.123)	(0.067)	(1.545)	(99.449)	(0.135)	(0.056)	(0.109)	(1.100)	(0.189)
PWP * Top-up	-0.035	-0.016	0.570	-16.477	-0.036	-0.029	-0.044	0.333	-0.012
	(0.059)	(0.048)	(0.955)	(86.951)	(0.099)	(0.039)	(0.078)	(0.942)	(0.135)
R-squared	0.08	0.14	0.16	0.10	0.17	0.03	0.05	0.04	0.16
Total effect on top-ups	0.144	0.030	0.589	-29.541	0.126	0.018	0.038	0.002	0.072
P-value for non-zero effect on top-ups	0.45	0.87	0.57	0.76	0.64	0.89	0.97	0.84	0.82
Upper bound (total effect)	0.381	0.148	3.611	150.841	0.391	0.129	0.255	2.046	0.438
Lower bound (total effect)	-0.092	-0.088	-2.433	-209.922	-0.138	-0.093	-0.178	-2.042	-0.294
Observations	1071	1084	1075	1084	1075	1084	1078	1080	1051
Mean of dep. var. in control group	5.78	6.49	38.41	2342.21	4.16	0.59	3.44	11.56	-0.19

OLS estimates. Standard errors clustered at the EA level. $*p < 0.10, **p < 0.05, ***p < 0.001$
Higher values indicate worse food security of columns 6, 7, and 8. Total calories are Winsorized at the 10th and 90th percentiles.
Estimates include district and week-of-interview fixed effects.