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

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Abstract

Labor-intensive public works programs are an important social protection tool in low-income settings, intended to supplement income of poor households and improve public infrastructure. Despite the recent expansion of public works programs in low-income countries, notably the massive program in India, there is a lack of rigorous empirical studies on the impact of such programs. This paper examines the impact of the large-scale public works program in Malawi in which program availability was randomly assigned across communities and to households within communities. Drawing on household panel survey data, the impact of the program is examined across four dimensions: labor allocation, food security, agricultural inputs, and participation in other programs. There is no evidence that public works participation displaces home production, casual wage labor, or participation in other programs in the context of Malawi's labor markets and social protection environment. While the program does not affect use of fertilizer or improve the food security of treated households, it does lead to perplexing reductions in food security for untreated households in villages with PWP activities.

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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 through short term labor opportunities (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 self-select into the program and removing the necessity of implementing complicated targeting schemes on the ground (Besley & Coate 1992). In addition, unlike unconditional cash schemes, they have the potential for providing indirect benefits to beneficiaries through building or repairing community infrastructure.

Rising commodity and fuel prices and the recent global financial crisis have stimulated interest in the usage and effectiveness of public works in the developed world and in low-income countries. These programs aim to provide consumption smoothing in response to macroeconomic crisis or agro-climatic shocks. Besides rapid scaling up during aggregate shocks, they play an important short term role in providing consumption-smoothing to intra-annual seasonal variation in food consumption when implemented during the lean season, at times of the year when consumption may be the lowest and when other forms of income generation are not as easily available. Short-term programs with a consumption-smoothing and food security objective are widespread and predominant in Sub-Saharan Africa.[1] Alternatively, a PWP can serve as on-going employment/income insurance. These are programs that provide a guarantee to work during the lean season to anyone who is willing to work at the stipulated low wage. Examples include the Employment Guarantee Scheme in Maharashtra (Ravallion, Datt & Chaudhuri 1993) and the National Rural Employment Guarantee Act in India (Dutta et al. 2014), and to a lesser extent the Productive Safety Net Project in Ethiopia (Hoddinott et al. 2012).

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 their impact, with causal inference complicated by unobserved heterogeneity at the village level due to the geographical targeting and at the individual level due to the self-targeting feature of the program. In this paper we study the impact of the large-scale public works program in Malawi in which program was randomly assigned across communities, and within communities randomly assigned to households.

The setting for this study is rural Malawi. Malawi is now entering the fourth phase of its large-scale public works program under the Malawi Social Action Fund (MASAF).

[1] In sub-Saharan Africa, public works range widely in objective, structure, and size. McCord & Slater (2009) identified 167 programs across 29 countries in the region.

The public works program (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. The role of the program design in Malawi to achieve its objective of food security is of particular interest. The food security objective is, in theory, to be achieved mainly through increased access to farm inputs at the time of the planting period. The program is designed to be interlinked with Malawi's large-scale fertilizer input subsidy program through the implementation of the public works scheme in the planting months of the main agricultural season when the fertilizer subsidy distribution also occurs. The program targets areas proportionately to their population and vulnerability criteria, but it is significantly rationed, being able to cover about 15% of the households satisfying the eligibility criteria.

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 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 2012-13. Within the sample of eligible projects during the agricultural season, we randomized the offer of the program at two levels: at the village level and then at the household level within participating villages. Drawing on household panel survey data, the impact of the program is examined across four dimensions: labor allocation, food security, agricultural inputs, and participation in other programs. High frequency data was collected to be able to detect relatively short-term changes in consumption and labor supply patterns as a result of the intervention.

Our results confirm evidence of rationing of the program. However, take-up falls well below 100 percent. The offer of the program is accepted by about 50 percent of the randomly selected households. This confirms that the low wage rate and the (work) conditionality requirement are binding. Contrary to other settings where the opportunity cost of participation is substantial and translates into a lower net income gain to the beneficiaries (Ravallion 1999), we do not find evidence of displacement of casual labor as a result of public works offer. The lack of displacement effect of the program during the planting season, at a time when hours in farming peak during the year, suggests that our setting exhibits significant slack in labor markets. Along the dimensions we examine, there is no evidence that public works is affecting participation in other programs.

There is no evidence that the productive link of public works is realized through the timing of the program during the planting season. Our results show that households 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, but we do not find evidence that they apply more fertilizer.

Finally, we find that the program does not result in improved food security for treated households during the lean season. Within communities with public works, we find negative spillover effect on food security among non-treated households. The results counter the evidence on positive spillover effects of social protection programs in other settings. Imbert & Papp (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 risk sharing responses 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.

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. Section 5 presents the results along the family of outcomes of interest. In Section 6 we provide robustness checks to account for composition effects in the sampling strategy and potential imbalances at baseline. 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 Malawis 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) conditional on the households participation in the FISP. This distinguishes the Malawis program away from traditional PWP design that entail program implementation during the lean season, timing motivated by improving food security outcomes aimed at consumption smoothing, not at improving household production.

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 measures of vulnerability. Within districts, district officials target a sub-set of extension planning areas (EPAs) based on poverty and vulnerability criteria. Traditional Authorities (TAs) 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 crisis response also increased the duration of the project participation 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 are mostly road rehabilitation or construction, with some work on afforestation and irrigation projects. The wage rate is 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 was set to take place at harvest in May-June 2013, but in effect was implemented post-harvest in June-July 2013. Figure 1 lays out the overarching timeline of the PWP and the household surveys.

2.1 Experimental design

We use a randomized controlled trial to test alternative budget neutral variants of the PWP to address two research questions. The four treatment groups result from cross-randomizing the timing of the program and the timing of the payment. The randomization took place at two levels: at the village level and then across households.

2.1.1 Village randomization

The villages in our sampling frame were randomly assigned to one of five groups (see Figure 2). The first of these groups is a pure control group (Group 0) in which villages do not participate in the PWP program in the 2012-2013 Season. Groups 1 through 4 participate in the PWP in the planting season (the first two-wave PWP cycle). 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 distinct variants are tested:

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

- *Schedule of the payments* We compare the current program design whereby the 12 days of works are paid in bulk one week after completion (lump sum schedule) with a split payment variant, where participants were paid three days apart, in five equal installments of MK 720 each. The amount of the each split payment installment was set to be lower than the counterpart funding required for the fertilizer subsidy.[2] The variation of the payment schedule was motivated by extensive qualitative work done in preparation of the design of this project suggests 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 group 1 and 3 vs. group 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.

The cross-randomization is designed to test interaction effects between the two design variants, as we hypothesized that 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. People who receive a large amount of money in December are likely to put some of it towards the purchase of fertilizer and/or seed, but there is not a similarly-profitable investment to be

[2]The market price of fertilizer in Malawi is approximately MK 5000 for a 50 kg bag. The national fertilizer subsidy program provides 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 700 does not.

made in February. 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. Comparing groups 3 and 4 during the lean season allows us to test whether people paid in a lump sum are more likely to spend on temptation goods. Comparing groups 1 and 2 allows us to test whether households are credit constrained in their fertilizer subsidy.

At this stage we pool our analysis across the four variants of the PWP program in order to estimate the direct and indirect effects of the common characteristics of the program: the the impacts of the earnings from and timing of PWP participation. We study a total of 182 villages (EAs) across our five treatment groups (Figure 2).

All 28 districts are included in the PWP program. For the evaluation, we randomly choose 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, we abide by the programs decentralized structure by receiving a list of participating villages from the District Council and Traditional Authorities, who target geographic areas based on Vulnerability Assessment Mapping criteria. These entities designate projects in proportion to the number of planned number of beneficiaries per districts by MASAF for the agricultural season 2012-2013. We identifie the overlap between the sampled enumeration areas (EAs) in the IHS3 and the list of communities pre-selected for PWP projects our 12 districts. We randomly chose one project per enumeration area to have unique EA-project pairs. The geographical targeting of the program is reflected in the regional breakdown of the sample (see Table 4), 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).

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. The highly decentralized MASAF program charges GVH with choosing a small number of households to be offered employment through the PWP. As discussed below, we use the 2010/2011national household survey as a baseline for this study. By chance, then, it is likely that one or two of the 16 survey households in our villages will be among those chosen for the PWP. We define these households as "village chosen beneficiaries."

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. Households in program villages who were not randomly selected for inclusion in the program, are labeled as "untreated."

3 Data

The data for this study are comprised of a panel household survey with multiple rounds of interviews. The basis for the panel was the Integrated Household Survey 3 (IHS3) which was fielded in 2010/2011 by the 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 intent was to use the IHS3 as a baseline survey for our evaluation.

The 16 IHS3 households in each EA included in the evaluation sample were then interviewed in four addition 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). Our first survey (before PWP began) 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 tests: true baseline data are not available in all EAs. The design of the experiment called for building upon the IHS3, and using those data as the baseline. However, 23 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.[3] The IHS3 data pre-date the public works program and capture pre-program characteristics of households. Round 1 of our panel survey was collected after the intervention was announced to the full sample, and after PWP began in three villages, and does not represent a true baseline. Rounds 2 to 4 explore expenditure and investment behaviors after public works payment for treatment households. The project timeline is

[3] Households in these enumeration areas were listed and a sample of 16 households was randomly drawn. 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.

outlined in Table 1.

3.1 Outcomes

We analyze results from three post-intervention rounds of data, collected early in the lean season (round two), just before harvest (round 3) and after the subsequent season's planting (round 4). We focus on four key sets of outcome variables. First, we consider labor supplied to PWP activities and to the private market. The former measure captures take-up of the program; differences between treatment and control villages are a key test of the intent-to-treat framework. The latter captures substitution between work for PWP and in the private market. If PWP is simply a substitute for private employment, then the main scope for impact on other outcomes would come through the wage differential between PWP and market wages. On the other hand, if there is no effect on labor supply to the private market, then there is likely involuntary unemployment.

Second, we examine food security outcomes with eight indicators and a ninth composite measure. Our measures include (log) per capita food expenditure and (log) per capita food consumption in the last week based on a list of about 100 food items. Total household calories is computed based on the caloric value of quantity consumed of this same list. A food consumption score is computed following WFP guidelines; it is the weighted sum of the number of days the household ate foods from eight food groups in the last week.[4] We includes a measure of the number of food groups consumed in the last week for seven main groups.[5] 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 (higher number indicates greater insecurity).[6] 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.[7] Finally, we construct a principal components

[4]The food consumption 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.

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

[6]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.

[7]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.

analysis index for these eight measures as our ninth food security measure.

Third, we study the use of agricultural inputs during the planting season. While improved food security is a universal goal for public works programs, Malawi's PWP is somewhat unique on its emphasis on linkages to the country's fertilizer subsidy program. The rationale for offering work activities during the planting season – a season that is likely to have high opportunity cost of time and low marginal utility of consumption – is to enable beneficiaries to increase their use of subsidized and unsubsidized fertilizer and other agricultural inputs, therefore improving long run food security. We include an indicator for whether the household uses any commercial fertilizer; the number of kilograms used; and the household's total expenditure on fertilizer.

Finally, we consider households' participation in other government or NGO benefit schemes. These measures are particularly important for understanding the mechanisms through which village-level assignment to PWP may operate. For example, if local leaders or NGOs compensate control villages with other programs, then the treatment effect we estimate is the effect of PWP relative to alternative programs. We use indicators for inclusion in free food programs; school feeding or supplemental nutritional programs targeted towards women and children; educational scholarship programs; cash transfer programs; and the fertilizer subsidy scheme.

4 Analysis

We analyze the intent-to-treats effects Malawi's public works program using household-level data from three rounds of follow-up surveys.

4.1 Regression specifications

Our design includes two levels of randomization: village-level randomization varied program availability and design, while household-level randomization varies eligibility to participate conditional on program availability. Our main results pool across the variants of the program 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 program village. Therefore, we capture the direct effect of program eligibility on treated households, and the indirect effect of the program on untreated households in villages with the program. The indirect effect is important in the context of rationing, but one limitation of our design is that we capture the indirect effect only at the implicit coverage rate as implemented in our study, approximately18.8%.[8]

We 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 of 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 ("untreated"). Because both village- and household-level eligibility were randomly determined, a main effect for Topup is not necessary. 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 in fact had a PWP program – that is, the marginal 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. All specifications include district and week-of-interview fixed effects; standard errors are clustered at the village level.

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 the program but not households' decisions to take it up. However, 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.

4.2 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 four villages, the survey was conducted after the intervention was announced in treatment villages. Anticipation of the program may affect 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

[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.

having been included in the IHS3. While the total group of 182 communities overlapped with the master list of PWP villages, these 23 non-IHS3 were not randomly selected from the universe of villages in Malawi by the same procedure as used to choose villages for the IHS3. They were, nonetheless, randomly assigned to our treatment/groups (six, six, three, three, and five villages to groups 0-5 respectively). 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 a smaller number of children below the age of 14 (see Table 1). 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. Robustness checks are done to confirm that main findings are robust to exclusion of the non-IHS3 villages.

Turning to baseline balance, we present estimates of equation (1) using the IHS3 to examine nine food security outcomes. The estimates in Table 2 show some imbalance. Specifically, untreated households in PWP villages had statistically worse outcomes for three measures; other outcomes, although not statistically significant, also tend to show worse food security. Treated households in PWP villages had statistically better food security for three measures compared to their untreated counterparts in the same village. Generally, the net difference between treated households in PWP village and households in control villages (the sum of the PWP and interaction terms) is not statistically significant.

5 Results

We begin by discussing survey round 2 outcomes of the village-level randomization. Because these outcomes are measured in January, where all treatment groups have participated in one cycle of PWP, the regressions capture the short-term effects of the first cycle of PWP in Malawi's lean season.

Household assignment had a strong effect on participation. As indicated in Table 3, treated households in villages with PWP are 37 percentage points more likely to work in the public employment program than others in their villages, and they work 4.7 more days (see columns (3) and (4)). In total, 49 percent of treated households in villages with PWP programs participate (the sum of the coefficients in column (2)), compared to 12 percent of other residents of those villages. This latter result reflects the national PWP coverage rate. To clarify, the coefficient on the PWP variable captures the effect of the program on the six (of 16) households randomly not offered the PWP program. In fact, some of these households may have been eligible for the program through the village selection process. Given the overall coverage rate of the program, we expect that an average of one or two of the 16 total households interviewed would have been included in the program through the village selection procedure, with an average of less than one household not chosen as a top-up beneficiary nonetheless eligible. Therefore, the coefficient on the variable PWP captures the effect on untreated households more precisely averages across the effect on the households not treated in our study and the small number of households that were chosen by their villages but not randomly selected in our household-level randomization. Household treatment status does not affect participation in the private market for casual wage labor ("ganyu").

Results in Table 4 show a robust and, for four of the eight measured outcomes, significant negative impact on the food security of untreated households in PWP villages. That is, these households not only have lower food security than treated households in villages with the program, but also have lower food security than households in control villages where no public works activities took place. This is an unanticipated result which we discuss in more detail below.

Treated households fare somewhat better than their untreated neighbors: the marginal effect of being a treated household, conditional on having PWP activities in the village, is to improve food security as measured by all eight outcomes, with statistically significant effects on three of those eight and on the PCA index summarizing the individual measures. However, the net effect of the program, obtained by summing the coefficients on the village and household level treatment indicators, is still negative relative to control villages for six of the eight outcome measures and for the PCA index. The program did not improve the food security of treated households and appears to have reduced it for untreated households in villages where others had the opportunity to participate.

Recall that increased use of agricultural inputs was a major objective of the PWP (and the motivation for the planting season work activities). In results not shown, we find that the program had no detectable effect on the amount of fertilizer used by either treated or untreated households. Also, ownership of durable goods does not increase.

It is important to isolate the mechanism for this negative indirect effect. Conceptually, there are several possibilities. One is that that increased demand from treated households whose budget sets have shifted outward drives up the price of food, which reduces food security among other households. A second is that the program led to a breakdown in sharing norms, reducing the transfers to untreated households as treated households exited risk-sharing networks. Relatedly, contacts outside the village might reduce transfers to ineligible households or expect increased transfers from such households, based on the misperception that they were benefiting from PWP.

However, analysis of village-level price indices shows no differences in price levels between treatment and control villages, ruling out the first mechanism. Specifically, we compute the price index as the quantity-weighted average price of the five most commonly purchased goods – dried fish, tomatoes, cooking oil, sugar, and salt. Since this is constructed at the village level, estimates of equation (1) are not informative. Instead, we compare prices in PWP and control villages. The value of the price index is an economically small and not statistically significant three percent lower in PWP villages than controls.

Table 5 provides some evidence against the second hypothesis. Specifically, the reduction in transfers given to other households is concentrated among the untreated households in PWP villages.[9] Therefore, these households were not erroneously targeted for increased contributions to others (or decreased contributions from others) in their networks.

Differential participation in other programs could explain the negative spillovers if control *villages* were targeted for other benefits, perhaps by politicians or organizations who observed the absence of public works activities. However, treated and untreated households in PWP villages participate in food, education, and cash benefit schemes at the same rates as households in control villages. Treated households are approximately 10 percent more likely to receive fertilizer subsidy coupons, a result consistent with the intended linkage between the PWP and fertilizer subsidy scheme but not a finding the explains the decrease in food security for the untreated households. Identifying the mechanism responsible for the negative externality experienced by ineligible households in treatment villages remains a priority for future analysis.

5.1 Pre-harvest outcomes

We collect additional survey data in April and May, before the harvest begins. In analyzing round 3 data, it becomes important to look separately at the effects of the lean and harvest treatments, because villages assigned to the lean treatment have had both 24-day cycles of PWP activities by the time these data are collected, but villages assigned to the harvest treatment have only had one cycle. Pooling across the two treatments is no longer appropriate.[10] We consider three types of villages in round 3: control villages with no PWP, "lean" treatment villages that had completed two 24-day work cycles, and "harvest" treatment villages that had completed one 24-day cycle, with the second scheduled for several weeks after the round 3 survey. Therefore, we run the regression

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

[9] In results not shown, we find the decline is driven by changes in transfers to contacts outside of the village, rather than within the village.

[10] In results not shown, the indirect effects of the harvest season treatment in round 2 are more negative than the indirect effects of the lean season treatment, but we cannot reject that the two treatments have equal direct and indirect effects.

As in the previous analysis, the reference group are households in control villages. The coefficient δ_1 measures the indirect effect of the lean season treatment – 24 days of PWP activities at planting time and 24 additional days during the lean season – on untreated households, relative to households in control villages. The sum of $\delta_1 + \delta_2$ captures the direct effect of the lean season treatment on treated households relative to households in control villages. The coefficients δ_3 and δ_4 are the corresponding effects for the harvest season treatment, which as of round 3 data collection is one 24 day cycle of PWP activities at planting time.

To compare the indirect effects of the lean season treatment to those of the harvest season treatment, we test whether δ_1 is equal to δ_3. To test for equal direct effects, we compare the sum $\delta_1 + \delta_2$ to $\delta_3 + \delta_4$.

Table 6 reports the estimates of equation (2) for the food security outcomes. Consider first the effects of the lean season PWP. Untreated households had lower food security than households in control villages according to all eight food security outcomes and the composite index, though none of the effects are statistically different from zero. Treated households in villages with the lean season program fare better than their untreated counterparts according to five of the eight measures and the composite; the total effect of the program on the food security of treated households in villages that received the second cycle of PWP activities during the lean season was very close to zero. This result is in itself a surprise, since wage employment is scarce and consumption low at this time of year and one would expect an outward shift in the budget constraint to lead to higher food security.

Households in villages with harvest season PWP programs received no additional intervention between rounds 2 and 3 of the survey. Untreated households in these villages had worse food security than households in control villages according to five of eight measures (with significant reductions in per capita food expenditures, consumption, and the food consumption score) and as measured by the PCA index. Unexpectedly, treated households fared even worse than their untreated counterparts according to seven of the eight measures and the index. The harvest season PWP – that is, the status quo timing – significantly reduced food security of treated households relative to households in villages without any PWP according to five of the eight individual measures and the composite index. This result is perplexing and is the focus of future analysis.

6 Robustness

In Section 4.2 we present evidence of balanced pre-treatment food consumption between households in the control villages and treated households in program villages, but imbalance between untreated households in program villages and both treated households in program villages and households in control villages. We explore whether our results are driven by baseline imbalance by including baseline controls and estimating equations (1) and (2) for the non-random subset of EAs for which we have data from 2010/2011.

6.1 Early lean season

The top panel of Table A1 mirrors Table 4 but limits the sample to only those observations for which pre-treatment outcomes are available. The pattern of coefficients for the effect of PWP estimated among this subsample are similar to those for the full sample. Food security is lower for untreated households in program villages than control households according to seven of the eight individual measures, though the differences are not always statistically significant. Food security for treated households is not significantly different from households in control villages.

The pattern persists with the inclusion of pre-treatment outcomes from the IHS3 as additional regressors, shown in panel A.2. The indirect effect of PWP appears to reduce food security for untreated households in program villages for six out of eight indicators, though the effect is statistically different from zero for only two of those measures. The pattern is very similar to the previous results without pre-treatment controls. The magnitude of the effect on the PCA index (shown in column (9)), is -0.193, nearly identical as in the specification using the full sample without baseline covariates, though somewhat smaller in absolute value than the -0.274 in the subsample regression without pre-treatment outcomes (panel A.1).

Treated households, on the other hand, have food security neither better nor worse than that of households in control villages. The coefficient on the interaction between the village and household treatment indicators moves in the direction of improved food security, and the net effect of the program on these households is close to zero.

The evidence in Table A1 suggests that our findings are robust despite baseline imbalance. Analysis of the subsample for which baseline data are available leads to the same conclusion (with or without the inclusion of pre-treatment outcomes in the regression specification): the indirect effect of the program is to reduce food security for untreated households in villages with PWP relative to households in villages without any such program and there is no evidence of a direct effect which improves the food security of treated households.

6.2 Pre-harvest

We perform similar robustness checks for the analysis of round 3 data (in Table A2). In the subsample, the indirect effect of the lean season PWP on untreated households is essentially zero. The direct effect on treated households is close to zero for most outcomes. The program does lower the probability of having to reduce meals and the value of the food insecurity scores by statistically significant amounts. The harvest season program has negative and sometime statistically significant effects on untreated and treated households.

After including pre-treatment outcomes, we find essentially no effect of the lean season program on untreated households. Treated households see food security decline along four measures and improve along three (and as above, the improvements the chance of having to reduce meals and in the food insecurity score are statistically different from zero); the effect on the index combining all eight measures is positive but very small in magnitude. This is consistent with predictions if the program is simply too small to change the food security of beneficiary households.

In villages that have the second cycle of the public works program during the harvest season, the results are puzzling. Recall that these villages implemented the first 24-day PWP cycle in December or January. In round 3 of the survey, they are visited in April, which is several weeks *before* their second cycle commences. Outcomes worsen for untreated households by five of eight measures and the PCA index. The magnitudes are smaller than those estimated in the full sample without pre-treatment outcomes; the coefficient on the PCA index, -0.105, is about half the magnitude of the corresponding coefficient in Table 6. Even more surprisingly, the marginal effect of being a topup household *worsens* food security by six of eight measures and according to the PCA index. The marginal effect of being a treated household in a village with a harvest season PWP cycle is -0.071, and the net effect on treated households, -0.176, is about eighty percent the magnitude of the effect as measured in the full sample without pre-treatment outcomes. Therefore, baseline imbalance is not responsible for the unanticipated finding that the harvest season program reduces food security for treated households.

7 Conclusion

Public works programs can stabilize income and improve food security of beneficiaries by providing earnings opportunities for vulnerable households, and can achieve targeting through low wages and work requirements that promote self-selection. 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. While the program is rationed nationally, with funding available to cover only 15 percent of households satisfying eligibility criteria, its wage rate and work requirements do generate some of the desired self-selection. When offered to randomly-selected households, about half choose to participate.

Moreover, 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.

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[11] – it does not have a measurable short-term effect on lean season food security for 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 other countries, social protection schemes with different types of conditionalities – school attendance requirements, rather than work – have generated positive effects on treated households and positive externalities to non-beneficiary households. In Malawi, though, we find the surprising result that the indirect effects of the public works program are negative. 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.

An explanation for this unexpected finding has proven elusive. Oportunidades, the conditional cash transfer program in Mexico, generated equilibrium effects on commodity prices, 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 sharing from PWP villages and if anything, untreated households made fewer contributions to their networks than households in control villages.

One explanation for the reduction in food security among the untreated households in PWP villages 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 in the seven-day recall period of the survey. Untreated households respond by similarly increasing very short run consumption – to "keep up" up

[11] 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.

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 windfall earnings. This explanation requires peculiar behavior by the untreated households, 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 requiring less complicated behavior by the untreated households is that treated households purchase food in bulk quantities for storage and later consumption immediately after they are paid. These purchases are not captured in the survey recall period and therefore do not translated into increased food security for treated households relative to control villages, but they do reduce the availability of commodities at local markets when untreated households attempt routine shopping. 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.

Explaining the negative direct impact for the subset of households who get the second cycle later in the season, during the harvest season, is also challenging. Offering the second cycle of PWP at harvest time can only improve lean season food security if it improves households' abilities to borrow, but with a rational expectations framework, there is no reason that the availability of 24 days of work at planting time and 24 days at harvest time should reduce food security in the intervening lean season, relative to no program at all. The pattern we observe could be explained by households forming incorrect expectations following the first cycle and not smoothing consumption properly because they anticipated receiving additional PWP opportunities during the lean season rather than later, after harvest. However, if over-consumption earlier in the lean season is responsible for the reductions in food security that we observe in round 3 data among treated households in villages with the status-quo PWP timing, we would have expected to see food security rise (relative to control households) at some previous point. Instead, food security was lower for these households than households in control villages. Any increase in consumption would have had to occur in the unobserved period between the second and third rounds of data collection.

We lack direct evidence to test either of these hypotheses or related phenomena, 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: Timeline

Pre-planting	Planting	Lean	Lean	Early harvest	Harvest
Oct. - Nov. 2012	Cycle 1: Nov.-Dec. Cycle 2: Jan.	Jan. 2013	Cycle 1: Feb.-**Mar.** Cycle 2: **Mar.**-Apr.	Apr.-May	Cycle 1: May-Jun. Cycle 2: Jul.-Aug.
Baseline survey	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

Figure 2: 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	
PWP with lump sum payment	Group 1 (status quo)	Group 3
PWP with split payment	Group 2	Group 4

Figure 3: Treatment assignment

	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	38 EAs	
PWP with lump sum payment	40 EAs	35 EAs
PWP with split payment	34 EAs	35 EAs

Figure 4: Regional breakdown of treatment assignment

Treatment at the EA level			
	Control EA	PWP EA	Total
North	4	18	22
	10.53%	12.50%	12.09%
Central	14	50	64
	36.84%	34.72%	35.16%
South	20	76	96
	52.63%	52.78%	52.75%
Total	38	144	182
	100%	100%	100%

Tables

Table 1: Descriptive statistics (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)	
HH size	4.380	4.969	0.000
	(1.998)	(2.291)	
Number of children (under 14) in HH	2.149	2.388	0.005
	(1.568)	(1.718)	

Standard deviations in parentheses. Round 1 data, tabulations based IHS3 status.

Table 2: Food security: balancing tests (IHS3)

Dependent variable:	(1) ln (per cap exp.) last week	(2) ln (per cap 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
PWP	-0.088	-0.109**	-2.722*	-1190.828	-0.299**	0.001	-0.107	-1.016	-0.195
	(0.097)	(0.048)	(1.401)	(2239.665)	(0.101)	(0.032)	(0.106)	(0.649)	(0.163)
PWP * Top-up	0.061	0.049	1.298*	748.540	0.166**	-0.011	-0.115*	-0.123	0.181**
	(0.053)	(0.034)	(0.769)	(1415.848)	(0.061)	(0.017)	(0.061)	(0.288)	(0.082)
Observations	2260	2274	2270	2274	2270	2274	2274	2274	2256
R-squared	0.07	0.15	0.11	0.14	0.13	0.18	0.23	0.19	0.17
Mean of dep. var. in control group	5.27	6.16	45.33	69475.03	5.08	0.22	2.34	4.57	0.05
P-value for non-zero effect on top-ups	0.77	0.86	0.94	0.92	0.78	0.60	0.01	0.10	0.39

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.
All estimates include district and week-of-interview fixed effects.

Table 3: Labor supply (round 2)

Dependent variable:	(1) Indicator: any HH ganyu	(2) Total HH person-days of ganyu	(3) Indicator: any HH MASAF	(4) Total person-days of MASAF
PWP	0.032	0.324	0.118***	1.553***
	(0.032)	(0.636)	(0.026)	(0.351)
PWP * Top-up	-0.016	0.172	0.373***	4.673***
	(0.019)	(0.425)	(0.025)	(0.343)
Observations	2813	2813	2813	2813
R-squared	0.03	0.04	0.26	0.21
Mean of dep. var. in control group	0.42	4.47	0.04	0.52
P-value for non-zero effect on top-ups	0.99	0.44	0.00	0.00

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 4: Food security (round 2)

Dependent variable:	(1) ln (per cap exp.) last week	(2) ln (per cap 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
PWP	-0.040	-0.076*	-2.026*	-2634.799	-0.116	0.065*	0.181**	0.566	-0.294**
	(0.080)	(0.043)	(1.072)	(1883.529)	(0.106)	(0.034)	(0.080)	(0.635)	(0.148)
PWP * Top-up	-0.002	0.020	0.151	-729.650	0.025	-0.048*	-0.129**	-0.156	0.097
	(0.050)	(0.031)	(0.665)	(1452.967)	(0.068)	(0.025)	(0.061)	(0.489)	(0.094)
Observations	2792	2837	2825	2837	2825	2808	2800	2802	2740
R-squared	0.15	0.16	0.14	0.10	0.18	0.08	0.08	0.09	0.15
Mean of dep. var. in control group	5.83	6.59	38.30	65256.98	4.32	0.49	3.12	9.09	0.14
P-value for non-zero effect on top-ups	0.67	0.54	0.21	0.12	0.64	0.52	0.52	0.78	0.60

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.
All estimates include district and week-of-interview fixed effects.

Table 5: Transfers and loans (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)
Observations	2840	2840	2840	2840
R-squared	0.11	0.18	0.04	0.03
Mean of dep. var. in control group	0.24	0.22	0.20	0.06
P-value for non-zero effect on top-ups	0.15	0.17	0.23	0.79

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 6: Food security (round 3)

Dependent variable:	(1) ln (per cap exp.) last week	(2) ln (per cap 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
Lean season PWP	-0.036	-0.064	-0.715	-1280.558	-0.052	0.011	0.032	0.441	-0.115
	(0.121)	(0.050)	(1.202)	(2023.274)	(0.094)	(0.028)	(0.093)	(0.404)	(0.154)
Lean season PWP * topup beneficiary	-0.039	0.050	-0.453	-391.823	0.007	-0.061**	-0.161*	-0.353	0.113
	(0.098)	(0.047)	(1.047)	(1581.325)	(0.074)	(0.027)	(0.086)	(0.472)	(0.131)
Harvest season PWP	-0.235**	-0.102**	-2.010*	-3376.622	-0.127	-0.012	-0.032	-0.189	-0.213
	(0.113)	(0.041)	(1.061)	(2097.369)	(0.091)	(0.027)	(0.085)	(0.366)	(0.136)
Harvest season PWP * topup beneficiary	-0.033	0.018	-0.513	-1977.985	-0.116	0.001	0.030	0.641	-0.098
	(0.095)	(0.041)	(0.881)	(1577.765)	(0.083)	(0.032)	(0.097)	(0.444)	(0.129)
Observations	2783	2804	2797	2804	2797	2779	2774	2776	2746
R-squared	5.37	6.77	44.30	72955.52	4.78	0.29	2.63	5.15	0.16
P-value for equal indirect effects for lean and harvest PWP	0.09	0.44	0.21	0.29	0.40	0.46	0.51	0.17	0.50
P-value for non-zero effect on lean season top-ups	0.51	0.74	0.31	0.38	0.62	0.05	0.12	0.81	0.99
P-value for non-zero effect on harvest season top-ups	0.01	0.05	0.02	0.01	0.01	0.67	0.98	0.19	0.02
Mean of dep. var. in control group	0.11	0.16	0.13	0.09	0.18	0.13	0.18	0.21	0.21

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.

All estimates include district and week-of-interview fixed effects.

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Appendix

Table A1: Food security (round 2, baseline subsample)

Dependent variable:	(1) ln (per cap exp.) last week	(2) ln (per cap 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.1: no covariates									
PWP	0.004	-0.079	-2.149*	-2197.878	-0.077	0.073**	0.216**	0.221	-0.274*
	(0.092)	(0.049)	(1.183)	(2214.176)	(0.115)	(0.035)	(0.084)	(0.671)	(0.159)
PWP * Top-up	0.034	0.031	0.676	-765.651	0.091	-0.051*	-0.162**	0.115	0.142
	(0.057)	(0.034)	(0.753)	(1718.144)	(0.080)	(0.030)	(0.070)	(0.573)	(0.107)
R-squared	0.16	0.18	0.16	0.10	0.21	0.10	0.10	0.10	0.18
P-value for non-zero effect on top-ups	0.53	0.78	0.60	0.22	0.49	0.59	0.40	0.66	0.96
Panel A.2: including pre-treatment outcomes									
PWP	0.033	-0.055	-1.595	-1776.406	0.027	0.073**	0.224**	0.316	-0.193
	(0.082)	(0.045)	(1.043)	(2162.605)	(0.104)	(0.034)	(0.080)	(0.643)	(0.135)
PWP * Top-up	0.019	0.017	0.349	-1015.815	0.043	-0.049*	-0.150**	0.135	0.086
	(0.056)	(0.034)	(0.731)	(1674.042)	(0.080)	(0.030)	(0.070)	(0.573)	(0.104)
R-squared	0.21	0.25	0.21	0.14	0.25	0.10	0.11	0.11	0.26
P-value for non-zero effect on top-ups	0.50	0.73	0.52	0.20	0.44	0.63	0.54	0.56	0.91
Observations	2165	2216	2201	2216	2201	2201	2193	2195	2124
Mean of dep. var. in control group	5.78	6.61	38.82	65578.86	4.32	0.48	3.12	9.32	0.16

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.

All estimates include district and week-of-interview fixed effects.

Table A2: Food security (round 3, baseline subsample)

Dependent variable:	(1) ln (per cap exp.) last week	(2) ln (per cap 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.1: no covariates									
Lean	0.019	-0.010	-0.207	-138.667	-0.057	0.005	0.014	0.268	-0.050
	(0.140)	(0.062)	(1.342)	(2324.790)	(0.109)	(0.033)	(0.099)	(0.472)	(0.173)
Lean season PWP * topup beneficiary	-0.061	0.012	-0.070	-1499.421	0.012	-0.078**	-0.191**	-0.317	0.103
	(0.111)	(0.054)	(1.190)	(1676.922)	(0.086)	(0.033)	(0.094)	(0.540)	(0.145)
Harvest	-0.214	-0.091*	-1.575	-2880.469	-0.098	-0.007	-0.015	-0.430	-0.195
	(0.135)	(0.052)	(1.274)	(2492.352)	(0.111)	(0.035)	(0.101)	(0.466)	(0.167)
Harvest season PWP * topup beneficiary	0.027	0.033	-0.087	-2070.789	-0.102	-0.020	-0.053	0.626	-0.018
	(0.115)	(0.050)	(1.100)	(1788.229)	(0.098)	(0.041)	(0.113)	(0.527)	(0.156)
R-squared	5.37	6.78	44.48	73417.96	4.82	0.32	2.71	5.69	0.16
P-value for equal indirect effects for lean and harvest PWP	0.10	0.19	0.29	0.23	0.69	0.75	0.80	0.20	0.39
P-value for non-zero effect on lean season top-ups	0.73	0.97	0.82	0.46	0.67	0.01	0.04	0.91	0.74
P-value for non-zero effect on harvest season top-ups	0.13	0.26	0.15	0.03	0.05	0.34	0.42	0.63	0.16
Panel A.2: including pre-treatment outcomes									
Lean	0.019	0.007	0.041	91.402	0.005	0.005	0.018	0.313	-0.009
	(0.125)	(0.058)	(1.311)	(2192.146)	(0.109)	(0.033)	(0.099)	(0.477)	(0.169)
Lean season PWP * topup beneficiary	-0.085	0.000	-0.449	-2048.188	-0.031	-0.077**	-0.178*	-0.291	0.033
	(0.112)	(0.054)	(1.223)	(1732.773)	(0.092)	(0.033)	(0.092)	(0.536)	(0.148)
Harvest	-0.136	-0.053	-0.871	-2374.964	-0.021	-0.008	-0.005	-0.363	-0.105
	(0.117)	(0.046)	(1.208)	(2367.212)	(0.107)	(0.035)	(0.101)	(0.466)	(0.157)
Harvest season PWP * topup beneficiary	-0.006	0.015	-0.395	-2135.412	-0.131	-0.020	-0.047	0.630	-0.071
	(0.110)	(0.050)	(1.072)	(1687.454)	(0.096)	(0.041)	(0.115)	(0.525)	(0.152)
R-squared	5.37	6.78	44.48	73417.96	4.82	0.32	2.71	5.69	0.16
P-value for equal indirect effects for lean and harvest PWP	0.24	0.31	0.48	0.28	0.80	0.73	0.84	0.21	0.57
P-value for non-zero effect on lean season top-ups	0.53	0.86	0.72	0.36	0.80	0.01	0.06	0.96	0.87
P-value for non-zero effect on harvest season top-ups	0.18	0.40	0.25	0.04	0.13	0.34	0.53	0.52	0.21
Observations	2167	2197	2190	2197	2190	2179	2175	2177	2138
Mean of dep. var. in control group	0.19	0.22	0.17	0.16	0.21	0.13	0.18	0.20	0.26

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.
All estimates include district and week-of-interview fixed effects.