Effects of a Universal Basic Income during the pandemic*

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Abstract

We examine some effects of Universal Basic Income (UBI) during the COVID-19 pandemic using a large-scale experiment in rural Kenya. Transfers significantly improved well-being on common measures such as hunger, sickness and depression in spite of the pandemic, but with modest effect sizes. They may have had public health benefits, as they reduced hospital visits and decreased social (but not commercial) interactions that influence contagion rates. During the pandemic (and contemporaneous agricultural lean season) recipients lost the income gains from starting new non-agricultural enterprises that they had initially obtained, but also suffered smaller increases in hunger. This pattern is consistent with the idea that UBI induced recipients to take on more income risk in part by mitigating the most harmful consequences of adverse shocks.

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1 Introduction

The spread of COVID-19 and of consequent restrictions on economic activity pose a serious threat to the livelihoods of many of the poorest, most vulnerable families on the planet. Governments have responded with an unprecedented expansion in their social protection programming. Between 20 March and 12 June, 195 countries introduced 1,024 new social protection measures covering an estimated 1.7 billion individuals. Cash transfers make up a large share of this expansion, reaching 1.2 billion individuals (Gentilini et al. 2020).

This paper examines the impact of transfers during the pandemic, taking advantage of a unique field experiment with Universal Basic Income ("UBI") which we had begun prior to the pandemic in Bomet and Siaya counties of Kenya. In recent years the merits of UBI have been debated intensely in both developing and developed countries, but without rigorous large-scale experimental evidence in representative populations to inform this debate.[1] One argument made for UBI is to provide a form of insurance against uninsurable or unanticipatable risks. While such arguments are typically difficult to test, precisely because they involve claims about rare or unforeseeable contingencies, we (sadly) have a unique opportunity to do so here.

Several specific design features also make this study relevant for informing policy responses to COVID-19. The study population is poor compared to national averages ((Kenya National Bureau of Statistics 2018)), but includes the entire population of the study communities (since transfers were "universal" within these communities). This is useful since social protection measures introduced since the pandemic aim to reach a much larger swath of the population than normal programming.[2] The study also includes multiple treatment arms including lump-sum transfers, long-term transfers scheduled to continue for ten years, and short-term transfers that had largely concluded by the time we conducted our survey. This is particularly relevant given that most of the recent social protection transfers were initially scheduled to last six months or fewer (Gentilini et al. 2020, Figure 3) including some that are one-off (such as the transfers sent

[1] See Banerjee et al. (2019) for a review of relevant evidence and open questions.

[2] As of 2018 the World Bank estimated that in low-income countries an average of 18% of the population benefitted from *any* social protection or labor program; in low-income countries an estimated 41% do so. The share who receive direct transfers program is lower still (Ivaschenko et al. 2018).

to all "Jhan Dhan" accounts in India), and much of the current policy debate concerns whether and for how long they should continue.

We primarily use data from a short household survey conducted in May in the midst of the strictest phase of Kenya's lockdown to date and focused on the issues most directly related to the pandemic. As we collected these data collected entirely by phone they cover fewer topics and may be less precise than typical household survey data. We were able to achieve low rates of attrition, however, with a 98% response rate overall and hence very small differences across arms. For some outcomes we are also able to combine these data with comparable measures collected as part of a full-scale endline survey conducted shortly before the pandemic onset, from August 2019 to December 2019, which lets us examine pre/post COVID-19 changes in several key variables including earnings, transfers, food security, mental health, and social interaction.

We first examine whether transfers were effective at increasing standard measures of private well-being despite the pandemic. It is unclear whether to expect the kinds of beneficial effects that past evaluations of social protection transfers conducted during "normal' times have typically found (e.g. Bastagli et al. (2019)). For material outcomes such as food security, for example, there are concerns that supply chain disruptions may limit the usefulness of demand-side interventions like cash transfers. For non-material outcomes such as depression, the pandemic may simply be too overwhelming for small transfers to make much difference.

Transfers had statistically significant, economically modest impacts on measures of recipients' private well-being in all three arms. Recipients were 4.9-10.8 percentage points less likely to report experiencing hunger during the last 30 days off of a control mean of 68%, i.e. in a context where hunger was widespread. This effect was significantly larger for the long-term arm that expected to continue receiving transfer than for the others that did not. Recipients were also 3.6-5.7 percentage points less likely to have had a household member sick during the last 30 days off of a control mean of 44%. Given its very low prevalence in Bomet and Siaya at the time of our surveys (12 cases total) these illnesses were almost not surely COVID-19 cases, though they may be comorbidities. Finally, recipients were significantly less depressed in the short-term and long-term arm, though not the lump sum arm. Overall, transfers continued to have the kinds of impacts on basic measures of well-being typically seen in pre-pandemic research.

In addition to private well-being, we also examine how transfers affected behaviors related to public health. These have been less salient in earlier work on social protection, but are central to understanding its role during the pandemic. We examine in particular how transfers affected health, health facility utilization, and social interaction. During "normal" times we might interpret increases in health facility utilization or social interaction as positive outcomes, but during the pandemic they have the potential to generate substantial negative externalities. If transfers increase social interaction that increases the rate of contagion, for example, this could be a significant drawback.

Transfers generally had small beneficial effects or null effects on behaviors related to public health. They reduced the probability that recipients had sought medical attention at a hospital in the last 30 days by 2.8-4.6 percentage points off of a control mean of 29%, potentially freeing up health system capacity. In an accounting sense the reduction in illness we observe can fully explain this reduction in hospital utilization. There is also some evidence that transfers reduced interaction for social activities (specifically, visits to friends or relatives) which could lower the rate of contagion. Estimated impacts on interaction for commercial purposes such as shopping or work are not precise enough to support strong conclusions. Overall, we find no evidence that transfers had harmful effects on public health, and some evidence that they helped.

The results above speak to the immediate policy questions of whether and for how long to continue providing social protection transfers during the pandemic. We also examine, however, what we can learn from this episode about the longer-term impacts of transfer programs on *resilience* to large aggregate shocks. Theoretically this is ambiguous: past transfers (and commitments to continue them) could have enabled recipients to smooth consumption in the face of income fluctuations by purchasing buffer stock assets, for example, or alternatively could have induced them to increase their exposure to risky ventures such as starting a business.

We find that transfers in general, and a commitment to long-term transfers in particular, led to an increase in risk-taking commercial activities and thus in exposure to shocks. As of the endline survey, recipients had diversified their income streams by creating 4.6-4.9 percentage point new non-agricultural enterprises on a control mean of 29%, and saw large corresponding increase in profits from these enterprises, without substantial changes in labor or agricultural earnings. By the time of our phone surveys these enterprises remained operational for the most part but with no treatment effects on earnings, as new enterprises appear to have suffered along with the old (in the control group, non-agricultural enterprise earnings fell 71% from endline to phone survey).

Hunger, on the other hand, was significantly *less* sensitive to shocks in the treatment arms. In addition to the reductions in hunger during the pandemic described above, we find significant reductions during the previous lean season one year earlier, then 39% of the control group experienced hunger. We find no effects, however, at the time of endline when 13% of the control group experienced hunger. Overall this suggests that transfers enabled households to reduce their risk of experiencing hunger during bad times, which in turn helps rationalize their decision to take on great income risk.[3]

Our results provide some of the first evidence of the impacts of social protection programs during the pandemic. In one other early contribution that we are aware of, Bottan et al. (2020) find that a contributory pension scheme in Bolivia reduced the probability of going hungry by 9 percentage points among households with pension-eligible (i.e. 60-year-old) members. More broadly, our results contribute to the large literature on cash transfers by characterizing their effects on private wellbeing and public health outcomes during an unprecedented global shock.

2 Context

Our study is set in two sub-counties in each of Bomet and Siaya counties in Kenya, two of the poorest counties within the country. At baseline, households owned on average 1.7 acres of land, 86% had a phone, 13% had a bank account (though this could be digital), 73% had a farm enterprise, 21% owned a non-farm enterprise, 85% experienced hunger in the year prior to the

[3] Because the onset of the pandemic coincided with the transition from rainy to hungry seasons in the local agricultural cycle (and potentially with other shocks), we interpret this last set of results as referring to this "composite" shock rather than solely to the pandemic per se. In ongoing work we are examining what additional sources of variation may shed light on the relative contributions of various shocks.

baseline and maize consumption was $0.60 per capita per day (scale by approximately 2.5 to get total consumption). In addition, on average sampled villages had 3 markets within 5km of them (approximately an hour and a half walk). Results from other recent work on cash transfers nearby in Siaya also suggest that it is fairly well-integrated into the larger national economy, as even a very large influx of cash equivalent to an estimated 18% of GDP in the treated villages had only modest effects on consumer goods prices on the order of 0.1%-0.2% (Egger et al. 2019).

One specific feature of the Kenyan economy that is important for our purposes is its relatively well-developed digital payments ecosystem. In particular, an estimated 96% of households in Kenya have a mobile money account (Suri and Jack 2016) and all the households in our study areas have relatively easy access to one or more outlets for collecting mobile money payments. As of 2014 the median distance from a study village to the nearest Safaricom M-PESA agent was 3km (and this number was presumably lower by 2018 as the number of agents had increased substantially).

As in many countries, the pandemic in Kenya is thought to have had economic impacts well beyond its immediate health impacts due to the reductions in mobility and interaction, both voluntary and mandatory, that it triggered. President Uhuru Kenyatta announced a nation-wide lockdown on 15 March shortly after the first case was confirmed, and subsequently augmented it with additional restrictions on mobility including the closing of all schools, a nationwide curfew and bans on movement into and out of heavily affected regions.[4] Over July and early August, some of these restrictions were lifted, but Nairobi county remains restricted. Independent tracking surveys in Siaya County, one of the two we study, suggest that by the end of June low income household's per capita earnings had fallen relative to their February levels by 35% from 2.3 USD PPP to 1.5 USD PPP per person per day.[5] As we discuss further below some of this likely reflects agricultural seasonality as well as the impacts of the pandemic and lockdown, but regardless of the why it is clear that overall livelihoods have been severely affected. Mortality rates, on the other hand, have been moderate at least compared to other African countries,

[4] `https://en.wikipedia.org/wiki/COVID-19_pandemic_in_Kenya`, accessed 6 August 2020.
[5] `https://www.kenyacovidtracker.org/timeseries.html`, accessed 6 August 2020.

with 7.3 deaths per million residents as of early August compared to 16.2 for the continent as a whole.[6] That said cases are on the rise and some of the restrictions that were lifted are starting to be brought back. In addition, it appears unlikely that schools will open for the 2020 academic year.

3 Experimental design

The experiment on which we build was originally designed to examine the impacts of Universal Basic Income and compare them to the impacts of other forms of cash transfer. Specifically, it examines four main conditions: control, one-time lump-sum transfers, short-term streams of transfers, and long-term streams of transfers. We randomly assigned these treatments at the village level. We first mapped and conducted a census in all villages in two sub-counties of each study county between April and June of 2017, which we used to identify 325 villages that had between 30 and 55 households. We restricted the study to these relatively small villages to limit the cost per village of delivering (universal) transfers, and thus increase the number of randomization units. Of these 325 villages we selected 295 (randomly) to be part of our experiment. Collectively these villages contained 14,674 total households and approximately 34,000 total people at the time of our census.

Prior to conducting our baseline surveys, we sampled 30 households per village to track throughout the study. We will refer to these throughout as the "sample households." Survey enumerators gave each adult in these sampled households a cell phone immediately after the baseline survey in order to make it easier for us to track these households over the following decade.

We then randomized villages to experimental conditions as follows:

- **Control:** 100 villages (approximately 11,000 people) received no additional resource transfers

[6] Source: Our World in Data (https://ourworldindata.org/coronavirus-data-explorer?zoomToSelection=true&deathsMetric=true&interval=total&perCapita=true&smoothing=0&country=KEN~Africa&pickerMetric=location&pickerSort=asc), accessed 6 August 2020.

- **Long-term universal basic income:** in 44 villages (approximately 5,000 people) each adult over the age of 18 receives US \$0.75 per day for 12 years.[7] We calculated this amount as sufficient to cover the most basic needs.
- **Short-term universal basic income:** in 80 villages (approximately 8,800 people) each adult over the age of 18 receives US \$0.75 per day for 2 years.
- **Lump-sum transfers:** in 71 villages (approximately 8800 people) each adult over the age of 18 received one-time payments of about US \$500. We calculated this amount as the equivalent in net present value terms of the short-term transfers assuming an annual nominal discount rate of 9.5%.

In this design, comparisons between the long-term and the short-term arms during the first two years of the study estimate the impacts of *expectations* of future transfers, while comparisons between the short-term and lump-sum arms at or after two years estimate the impacts of tranching structure. Randomization was stratified by location, a geographic unit in Kenya. The average location in our data contains 14 villages, and the average population of villages in our sample is 250 individuals.

After we completed a baseline survey, the NGO GiveDirectly (henceforth GD) conducted enrollment and delivered transfers to adults in each treated village. Individuals who moved to the village after this enrollment campaign were not eligible for transfers. GD delivered all transfers using Safaricom's M-PESA mobile money system, the leading such system in Kenya. Recipients in the lump-sum arm received two equally sized installments three months apart (in order to fit within limits on the size of M-PESA wallet balances), while those in the other two treatment groups received transfers on a biweekly basis. Figure 1 illustrates the assigned timing of transfers in the different arms as well as measurement activities.[8]

[7] In addition, teenagers aged 15-17 in these villages were told that they would begin receiving transfers upon turning 18.

[8] We also cross-randomized (but do not examine here) two "nudges" within the treatment groups and at the household level. The first was a planning nudge, encouraging the household to plan what they were going to spend the transfers on and informing them that GD would ask what these plans were after payments had started. The second was a savings nudge, reminding them that they had the option to save some of their transfers in an interest-bearing "M-Shwari" digital bank account offered by Safaricom since 2012 and providing instructions on how to do so. We stratified the nudge randomization by village.

Compliance with the experimental assignment was high. Columns 1 & 2 of Table 1 shows self-reported receipt of transfers from GD as of our endline survey (described below). In the control group, essentially no one reported receiving transfers from GD in the last 30 days or the last 12 months. In the short-term and long-term transfer arms, respectively, 82% and 78% reported receiving transfers from GD in the last 30 days, and 85% and 81% in the last 12 months.[9] Fewer in the lump-sum arm report receiving transfers which is as expected since the lump sum transfers had largely been completed by June 2018, though a few took longer than this to process. Column 3 reports the analogous 30-day figures as of the our phone survey. Essentially none of the control group or lump-sum recipients and almost none of the short-term recipients report having recently received a transfer in the last 30 days, which is as expected (according to administrative records, 95

These "first stage" figures have two important implications for the interpretation of our results below. First, the fact that compliance was high overall suggests that we can interpret ITT estimates as reasonably close to the overall average treatment effects in this population. Second, the fact that the short-term and lump-sum arms had stopped receiving transfers by the time of our phone surveys means that we can interpret differences in impacts across the arms as differences between the effects of having *received* transfers on the one hand, and having both received transfers and expecting to *continue receiving* them on the other. The comparison between the short-term and long-term arms in particular estimates the effect of anticipated future transfers conditional on having received essentially the same stream of transfers in the past.

[9] To construct this measured we asked respondents "Did you or members of your household receive any of the following assistance(cash/ food/ in-kind) in the last 12 months from the government or a non-governmental institution (such as churches, NGOs, Give Directly, inua jamii etc)?" and then hand-coded responses that mentioned GD.

4 Data and empirical methods

4.1 Data

Table 2 summarizes all data collection conducted to date as part of the overall UBI project. Our focus in this paper is on the data collected in a special round of COVID-19 surveys conducted by phone between late April and late June 2020 and which is summarized in Columns 5 and 6. We attempted to survey all households, including those that had relocated. In each survey we sought to interview the head of each sampled household, or if unavailable then another adult family member competent to answer questions about all aspects of household decision-making. Interviews for this and all other surveys were conducted by staff at Innovations for Poverty Action – Kenya. The household survey covered a relatively short list of five core topics: health (including basic health outcomes, mental health outcomes, and health-seeking behaviors), food security, earnings (including wage and self-employment earnings), transfers and dissavings, and social interaction.[10] We attempted to survey 8,605 household heads as part of this survey, including migrant households who we tracked to survey in their new locations, and successfully completed interviews with 8,427, or 97.9% of them.

Given the high overall completion rate, imbalances across arms are also small. Column 2 of Table 3 describes completion rates by arm; relative to the control these were slightly higher in the short-term arm (0.9% higher, $p < 0.10$) and the lump-sum arm (0.9% higher, $p < 0.10$). Given these quantitatively small differences across arms we present simple intent-to-treat comparisons below.[11]

We also attempted to survey all individual adult migrants who had left their household of origin either permanently or temporarily. These individuals are not relevant for outcomes such as mental health that we collected only from the household head, but are for aggregate household outcomes such as earnings, savings, debt and remittances; for these outcomes we add the relevant quantities for the migrants to the totals for their household of origin. We attempted

[10] This and all other survey instruments are available online at `https://www.socialscienceregistry.org/trials/1952`.

[11] Table A.1 reports the results of tests for differences in baseline characteristics among the attritors from the various arms. These differences are significant at the 1% (5%) level for 0 (2) out of 23 outcomes.

to survey 1,396 individual adult migrants and completed interviews with 1,251, or 89.6% of them.

To understand market conditions in the vicinity of our study villages, we contacted the market head in all 105 of the markets located near our study villages and asked whether and when the market had closed as part of the lockdown.[12] Overall, out of 105 markets, only 24% closed at any point and of those only 5 were still closed as of July 2020.

In addition to the COVID-19 surveys, we also make limited use of data from a standard endline survey of households conducted between August and December of 2019, and a survey of migrants from these households conducted between March and May 2020. Data from this survey let us assess how outcomes in the COVID survey have changed since pandemic onset and whether these changes were differential by treatment arm. Specifically, of the outcomes collected in the COVID survey, the endline also collected comparable measures of earnings, transfers, mental health, and social interaction, as well as measures of food security defined in the same way but with reference to the worst 30 days in the last year, as opposed to the most recent 30 days.

We attempted to survey 8,753 households as part of the endline survey, and successfully completed interviews with 8,522, or 97.4% of them. Column 1 of Table 3 describes completion rates by arm; completion rates are balanced across arms except that they are slightly higher (1.7%, $p < 0.01$) for the lump-sum arm relative to the control.[13]

As in our phone surveys, we also attempted to survey all 1,840 individual adults who had migrated temporarily or permanently out of a household that had not itself relocated. We completed surveys of 1,283 of these individuals, or 69.7%. These interviews were originally planned to be conducted in-person but some were shifted to phone calls due to the pandemic.

Table 4 presents descriptive statistics for our sample, focusing on the variables that were

[12] The market head organizes the market, typically setting up stalls, collecting fees if applicable, etc.

[13] In addition to these surveys, we conducted a number of others: a set of baseline surveys of households, spouses, traders, and village elders from June to September of 2017, and at endline an enterprise census and enterprise surveys of enterprises located in or near study villages as well as surveys of spouses, traders, and village elders. We plan to use data from these surveys in subsequent comprehensive analysis of the impacts of UBI.

pre-specified as primary outcomes in our pre-analysis plan for the endline data,[14], or for outcomes that were not available at baseline on the closest available analogue. The sample is generally quite deprived on important socioeconomic measures. Most (86%) households experienced hunger during the past year, for example, and the mean household head is clinically depressed (mean CES-D score of 20, compared to a threshold of 16).

The experiment is also generally well-balanced. Each row in Table 4 reports the F-statistic and p-value for a test that the mean of the variable indicated is the same across all four arms, except the final row which reports the corresponding statistics for a test that the means of *all* variables are the same across all arms. Out of 23 outcomes we reject the null at the 5% level for 1 and at the 10% level for 2, and the p-value on the overall test is 1.00

4.2 Empirical methods

We focus here on three simple specifications: the intent-to-treat effect on (i) outcomes as of the COVID survey and, when available, the effect on (ii) outcomes as of the endline survey and on (iii) changes between survey rounds.

$$\{Y_h^C, Y_h^E, Y_h^C - Y_h^E\} = \alpha + \beta_{ST} ST_{v(h)} + \beta_{LT} LT_{v(h)} + \beta_{LS} LS_{v(h)} + \epsilon \tag{1}$$

where h indexes households which were living in villages $v(h)$ at baseline and ST, LT, LS are indicators for assignment to the short-term, long-term, and lump-sum treatments respectively. We cluster standard errors by village, the unit at which treatment was assigned.

This is a subset of the analysis we pre-specified in our original pre-analysis plan for the endline data, which also included (i) models that interact treatment with the number of adult household members at baseline, and (ii) additional spatial modelling that allows for cross-village neighborhood effects and for spatial autocorrelation in the error terms (Conley 1999, 2010). Work on these specifications and the full set of endline data is ongoing and we will report those complete results in a separate paper.[15]

[14] See `https://www.socialscienceregistry.org/trials/1952`.

[15] The village treatment indicator already captures a large share of the variation in neighborhood treatment in-

Interpreting changes in the outcomes between survey rounds requires some caution. In general we cannot attribute changes over time in any outcome solely to the onset of the pandemic, and in particular the endline and COVID surveys fell at different points in the agricultural cropping cycle, which is an important driver of income fluctuations for many households in our sample. The endline survey was completed around the harvest period for the "short" agricultural season, which would typically be a relatively good cash-flow period for households that planted in that season. The COVID survey, on the other hand, was conducted before the main season harvests, typically a relatively tight cash-flow period. To the extent that measures such as food security deteriorate between rounds, it seems reasonable to expect that at least some of this is attributable to the agricultural cycle and not to COVID.[16]

5 Impacts during the pandemic

5.1 Effects on individual well-being

We first examine impacts on core measures of well-being: food security, physical health, and mental health.

We find statistically significant but economically modest effects on measures of food security during the pandemic. In Column 1 of Table 5 we examine effects on an indicator for whether the household reported experiencing hunger in the past 30 days. Roughly two-thirds of the control group did, indicating that hunger was widespread at this time. All three treatment groups experienced hunger significantly less often, with the effect sizes around twice as large in the long-term arm and significantly different from those in the short-term and lump sum arms. That said, even in the long-term arm the effect size is economically modest, with the rate of

tensity that subjects experience (and in particular the areas we study are substantially less densely populated than those in Egger et al. (2019) who find meaningful cross-village spillovers). It is worth commenting however that treatment is slightly negatively autocorrelated across space because of the spatially stratified randomization, while we expect outcomes to be positively spatially autocorrelated, so we expect the Conley adjustment to yield slightly smaller standard errors, as for example is the case in Egger et al. (2019).

[16] Glennerster and Suri (2019) find extremely large variations in child health (for children under 5) between seasons in rural Sierra Leone, for example, and in a context close to ours Burke et al. (2018) document large seasonal fluctuations in grain prices in Western Kenya.

hunger falling from 68% to 57%.

In Columns 2-5 we examine measures of the intensity of hunger. These were reported *conditional* on experiencing it and so do not let us distinguish between patterns in the type(s) of hunger that transfers eliminated on the one hand, and treatment effects on the intensity of hunger spells that they did not eliminate on the other. That said, they generally suggest that transfers modestly reduce the extremity as well as the incidence of hunger. For example, hungry households were significantly less likely to have a member go without any meals for a full day (Column 2). Effects are also generally larger for the long-term arm, mirroring the results for the extensive margin, and these differences are significant for eating no meals (Column 2) and for eating meat or fish (Column 5).

In the appendix we also examine effects on the *unconditional* likelihood of having a member eat k or fewer meals in a day for $k = 0, 1, 2$ (Table A.2), assuming that households that report not experiencing hunger had no such days. Not surprisingly, the estimates are larger than the conditional ones and in all but one case are statistically significant. The point estimates are still modest, however. For example, the long-term arm (where the effect is largest) was roughly 1% less likely to have a day on which a member ate no meals, compared to a control group mean of 3%. This underscores the point that even sustained transfers reduced hunger but did not eliminate it.

Table 6 reports impacts on measures of physical and mental health. In Columns 1 and 2 the outcomes are an indicator for whether any household members was sick within the last 30 days and the number of household members who were sick within the last 30 days, respectively. “Sick” is defined here to mean that the household member experienced illnesses/symptoms such as fever, nausea, etc. because of which the member did not feel “their usual self” either physically or mentally. In the control group 44% of households had at least one member fall this ill during the last 30 days, indicating a fairly unwell population. In Column 6 we examine impacts on depression as measured using the standard Center for Epidemiological Studies Depression Scale (CES-D) (Radloff 1977). This scale ranges from 0-60 with scores of 16 points or more considered “depressed.” The control group mean in our sample is in fact just over 16 points at both endline and phone survey, and 43.7% of the control group scored 16 or higher, indicating a population that is generally quite depressed. (We discuss the results in Columns 3-7 in the following section.)

Generally speaking, all three treatments significantly improved both physical and mental health during the pandemic. The likelihood of having a family member fall ill fell by 3.6-5.7 percentage points (8.2%-12.9%) depending on treatment arm. CES-D scores also fell (indicating improved mental health) in all three arms, though here the effects appear limited to recipients who had recently received or were continuing to receive transfers; depression falls for lump-sum recipients but this effect is not significantly different from zero, and is significantly less than the effect on long-term recipients.

We do not interpret the effects on rates of illness as indicating that transfer recipients were less likely to become infected with the novel coronavirus itself. The reported number of cases in Kenya at the time on July 1 when we completed our phone survey was just 6,366, or 0.01% of the population, and the counts in Bomet and Siaya counties on June 15 near the end of our survey were just 11 and 1 out of populations of approximately 500,000 and 400,000, respectively.[17] While this is surely a substantial underestimate, even 100 times as high a rate would account for only 2.2% (i.e. 1 percentage point / 44 percentage points) of the illness reported in our control group. Within our sample, 7.7% of all household members experienced at least one of the COVID-like symptoms during the last 30 days. The results thus almost surely indicate a reduction in non-COVID illnesses, though as we discuss below this likely bears indirectly on respondents COVID-related risks.

Overall, our results suggest that transfers were modestly effective at improving core measures of well-being during the pandemic. In interpreting this set of results, it is important to remember that recipients in all arms began receiving transfers well before the pandemic began, and thus that the effects we observe are those of both past and contemporaneous transfers (in the case of the long-term arm) and purely of past transfers (in the case of the lump-sum and

[17]National case counts obtained from `https://ourworldindata.org/coronavirus/country/kenya?country=~KEN`, population from `https://data.worldbank.org/indicator/SP.POP.TOTL?locations=KE`, both accessed 4 August 2020.

short-term arms). Estimated impacts are generally larger in the long-term arm, which is consistent with the idea that contemporaneous transfers have important additional benefits, but we have sufficient power to reject the null of equal effects only in some cases. Put differently, there is strong evidence that social protection transfers put in place before the pandemic had benefits during it, and some evidence that incremental transfer delivered during the pandemic had incremental benefits on top of these.

5.2 Effects on public health

We turn next to impacts on outcomes related to public health. Under more normal circumstances there is substantial evidence that social protection transfers can affect the utilization of health services and some evidence that it affects dietary diversity and child anthropometrics, though less evidence either way on adult health outcomes (Bastagli et al. 2016). More generally, income and health status tend to be strongly positively associated, and there is some evidence that COVID-19 death rates specifically are higher among more deprived populations conditional on other characteristics (Williamson et al. 2020). What is not known is how exogenous increases in income due to transfers and public health policies will interact causally during the pandemic: in particular: whether transfers will tend to increase or decrease the rate of spread of the virus or the capacity of health systems to cope with it.

The reductions in illness discussed above, while likely not reductions in COVID-19 infection per se, are potentially relevant to the pandemic for two reasons. First, depending on the nature of the illness, they may reduce individuals' risk of becoming severely ill or dying if infected with the novel coronavirus.[18] Second, we find significant corresponding reductions in the rates at which transfer recipients sought consultation at a hospital (Table 6, Column 3). This is intriguing as it contrasts with other results in the literature, which have generally found that transfers increase utilization of health services (Bastagli et al. 2016, p. 87). It may simply reflect the me-

[18] A number of chronic health conditions are associated with more severe cases of coronavirus; see for example US Centers for Disease Control and Prevention, (`https://www.cdc.gov/coronavirus/2019-ncov/need-extra-precautions/evidence-table.html`, accessed 10 August 2020 and Davies (2020) who find that diabetes, HIV and tuberculosis predict significantly higher mortality risk from COVID-19 in South Africa. We are not aware of evidence one way or the other on the effects of acute illness such as malaria.

chanical consequences of lower rates of illness in treated households; if we estimate treatment effects conditional on illness (keeping in mind that this is clearly endogenous) the effects largely dissipate, which is consistent with this mechanical interpretation (Table 6, Column 4). Lower levels of depression may also make the households feel more healthy overall. Alternatively, this effect may reflect the fact that all households prefer to reduce their interactions with health services during a pandemic and that households with more financial resources are better able to find alternatives.[19] Either way, this represents an increase in available health system capacity at a time when public health analysts expect large cross-disease interactions due to the limitations of public health systems (e.g. Hogan and et al (2020)).

A second key issue is how transfers affect the frequency of interpersonal interactions that help to transmit the virus. A priori there are many plausible possibilities. On the one hand, transfers might increase interaction as recipients move about to collect them and then to spend them. These effects could in principle be somewhat muted in our case since transfers were made via mobile money, meaning that recipients could potentially use the digital currency they received to pay for items without leaving their homes, but even in this case they would need to take receipt of any purchases. On the other hand, transfers might reduce recipients' need to work either as employees or in self-employment in order to earn money to meet basic needs. Finally, transfers could have various effects on social interactions with friends and family.

We find some evidence that transfers reduced interaction for social purposes during the pandemic. Table 7 reports impacts on the share of days out of the last 30 on which a household member visited another household (Column 1) and on a prespecified social integration index equal to the (normalized) sum of (i) the number of social activities the household participated in and (ii) the number of other households with whom a household member spends at least 1 hour per week (Columns 2-4). Transfers significantly reduced social interaction for the short-term and lump-sum arms, where the frequency of visits to other households fell 14%. Visits also fell in the long-term arm, though the reduction is smaller and not significantly different from

[19] For example, Fu et al. (2015, Figure 10) find that health facility utilization fell sharply in Freetown, the most affluent part of Sierra Leone, during the Ebola crisis.

zero. Short-term and lump-sum transfers also significantly reduced the social integration index by 0.095σ and 0.065σ (Column 3). If we unpack these effects into effects on the components of the index they turn out to be driven entirely by reductions in the number of households with whom the respondent socialized (as opposed to the number of formal activities in which they participated). The visits and social integration results thus likely capture the same behavioral change.

Estimated effects on interaction for commercial reasons, on the other hand, are generally not statistically significant. In Table 8 we examine impacts on measures of the frequency with which the household head left the house to visit commercial sites such as markets, town centers, shopping centers, and bus stops (Columns 1-6) or for work (Columns 6-8). None of the estimates are significant at conventional levels, and the signs of the point estimates do not exhibit strong patterns. There is arguably a tendency towards positive point estimates on trips to markets and town centers, as one might expect if recipients travelled to these places to make purchases, but in each case we can reject large increases. We do see that recipients in all three arms are less likely to go outside their homes for work (Columns 6-7), but again we can reject large changes. In Column 8 we see that few respondents report (remunerated) work from home (control mean of 7.5%) and treatment does not change this significantly.

Overall there seems to be reasonably clear evidence that transfers reduced some forms of interaction for social purposes.The data are not dispositive, however, on whether transfers affected interaction for commercial purposes. At a minimum, this suggests that social protection transfers during the pandemic are not generating large *increases* in interaction that could accelerate disease transmission.

6 Do transfers mitigate or magnify aggregate shocks?

The results above speak to the immediate policy questions of whether and for how long to continue providing social protection transfers during the pandemic. In the longer run, however, the design of effective social insurance policies should reflect how transfers affect *resilience* to aggregate shocks, including both predictable ones such as the agricultural lean season and unpredictable ones such as the pandemic. Policies such as on-demand public employment or publicly provided index insurance, for example, become more (less) attractive if existing redistribution schemes make their recipients more (less) sensitive to shocks.

Theoretically this is ambiguous depending on the outcome and the circumstances. Consider for example a risk-averse household choosing income-generating investments and activities that in turn determine its future consumption stream, and doing so in a context of imperfect credit and insurance markets.

Consider first the effects of short-term transfer, as in the short-term and lump-sum arms in our experiment. If the household simply saves this money (or similarly purchases relatively low-productivity but liquid, buffer-stock assets) then this may have no effect on the sensitivity of its earnings to shocks, but reduce the sensitivity of its consumption to shocks, since it can draw down the additional savings to offset them. If on the other hand the household uses the money to finance risky investments, perhaps accompanied by a change in occupation (e.g. shifting from employment to self-employment), this could *increase* the sensitivity of both earnings and consumption to shocks. This would be particularly likely if the transfer "crowds in" investment from other sources of funds, concentrating the households income risk.[20]

Now consider the effect of a long-term commitment to future transfers, as in the long-term arm in our study. For a given path of earned income this may help to reduce the sensitivity of consumption to income shocks, at least for essential items measured here such as food, as the household is more likely to be able to meet basic needs even in adverse circumstances. Anticipating this, however, the household may also be more likely to invest in relatively high-risk, high-reward income generating activities. On net the sensitivity of consumption to shocks may or may not change meaningfully, while the sensitivity of income could increase.

[20] Balboni et al. (2020) find for example that consumption fell initially after receipt of a one-time asset transfer in Bangladesh before eventually rising substantially, consistent with crowding-in.

6.1 Earnings

Turning to our data, the results are broadly consistent with the idea that transfers in general, and a commitment to long-term transfers in particular, led to an increase in risk-taking commercial activities that were more sensitive to large shocks.

Beginning with occupational choices, Columns 1, 4 and 7 of Table 9 report impacts as of endline on an indicator for any wage employment, one for operating any non-agricultural enterprise, and one for operating any agricultural enterprise. For non-agricultural enterprises we define "operational" as having incurred any costs or earned any revenue in the preceding 30 days. For agricultural enterprises we define "operational" at endline as having had any sales or incurred any cost in the previous 12 months, but at phone survey based on whether any household member had been involved in the running of any agricultural enterprise within the previous 30 days; the two measures should thus not be interpreted as exactly comparable.

Prior to the onset of the pandemic (and of the agricultural dry season) we see that households in all three arms are significantly more likely to operate a non-agricultural enterprise. Effects range from 4.6 to 4.9 percentage points on a control mean of 29.2 percentage points, or 16%-17% in relative terms. There is suggestive evidence that some of these enterprises were staffed through reductions in wage work, though these effects are smaller and only (marginally) significant for the long-term arm. There is little evidence that they substitute away from agricultural enterprises, which remain ubiquitous (9̃8%) in all arms and if anything slightly more common in the short-term and lump-sum arms. Overall, the pattern is an increase in own-account activity.

Effects on earnings show a similar pattern of initial diversification into higher-return (but also potentially higher-risk) activities. Columns 1, 4 and 7 of Table 10 report effects as of endline on earnings from wage employment (i.e. wage income), non-agricultural enterprises (i.e. profits), and the sale of agricultural outputs (i.e. agricultural revenue), as well as measures of total household income. We focus on agricultural revenue rather than profit since we are asking about the last 30 days and this, being the pre-harvest season, is probably not the period during which much of the relevant costs were incurred. [21] At endline (Columns 1, 4, and 7) we see

[21]Also note that since this survey was done by phone, we had to aggregate some of the questions where we

patterns that broadly parallel those for occupational choice: non-agricultural enterprise profits increase for all three arms, significantly so for the long-term and lump-sum arms, while there are no significant changes in labor earnings and a weakly significant increase in agricultural earnings only for the short-term group.

In the control group, the onset of the pandemic (and correlated shocks) was associated with modest changes in occupational structure but large reductions in earnings. We see little change (Table 9) in the probability of wage employment from endline to phone survey (53% to 50%), and a modest 5% reduction in the probability of operating a non-agricultural enterprise (29% to 24%). The probability of operating an agricultural enterprise falls substantially (69% to 49%) but this is arguably as one would expect given the seasonality of agricultural activity. Earnings from these activities, on the other hand, were down sharply: 54%, 71%, and 41% for labor, non-ag enterprise, and agricultural enterprise respectively. Again, the reduction in agricultural earnings is unsurprising given seasonality, but the fall in earnings from other sources is striking.

How did the enterprises newly created due to treatment fare during this shock? There is little evidence that they were forced into outright closure. Columns 2, 5, and 8 report level effects on each occupational indicator as of the phone survey, and Columns 3, 6 and 9 the treatment effects on the change between endline and phone survey. Treatment effects as of the phone survey endline are generally similar to and not statistically distinguishable from those at endline. Paralleling control group trends, however, we do see meaningful declines in earnings from these new enterprises. In particular, the large and significant increases in non-agricultural enterprise earnings we observed for the long-term and lump-sum arm at endline are nearly entirely reversed by the phone survey, with the changes significantly different from zero. It thus appears that these new enterprises were, like other enterprises, not immune to the shock.

Interestingly, both the initial increase in non-agricultural profits and the subsequent reversal are (marginally) significantly larger for the long-term than the short-term arm ($p = 0.11$ and $p = 0.09$, respectively). Since both groups had received roughly the same amount of money

had much more detail in the endline as the phone surveys have to be kept short in length. We plan to collect a second round of phone surveys and will collect data on harvests and all the relevant costs once the harvests end in September.

at the time both surveys, this suggests that the expectation of a basic income guarantee in the *future* stimulated greater investment in non-agricultural enterprise, but that these investments were vulnerable like others to the effects of lockdown (as well as other correlated shocks).

6.2 Essential consumption

Did recipients' increased exposure to income risk translate into consumption volatility, and in particular for essentials such as food?

At endline, prior to the pandemic, we asked households for each month in the preceding year whether or not they experienced hunger in that month. We can thus compare rates of hunger at the time of the phone survey both to those at the time we conducted the endline survey, and also to those one year earlier, i.e. in April-June 2019. The endline-to-phone comparison is analogous to the one we perform for income above, and thus informative about how income and consumption volatility are related. The year-on-year comparison is informative about the *nature* of the shock households were experiencing: in particular, it sheds some light on whether hunger during the pandemic was excessive relatively to what we might expect at that time of year due to agricultural seasonality.

Seasonal hunger appears to be a substantial problem for this population, and the 2020 hungry season was also unusually bad, potentially reflecting the effects of the pandemic (Table 11). In the control group, 39% of households report experiencing hunger in the month in 2019 one year before their phone survey. By the time of our endline in late 2019, when households would have been relatively flush with the proceeds from harvest, this figure had fallen by a factor of 3 to just 13%. By the time of our phone survey in April-June 2019, however, it had shot back up to 68%. By this metric, hunger was thus 74% higher than at the same time in the previous year.

Transfers reduced the sensitivity of hunger to these seasonal and/or pandemic-induced fluctuations. We see little evidence of treatment effects at endline, when hunger in the control group was low (Column 2). Point estimates are negative but small, and we can reject effects larger than a 4.1-4.5 percentage point decrease at the 95% confidence level. In both the phone survey (as we have already seen) and during the previous lean season, however, when hunger in the control group was common, we see significant effects of all treatment arms on hunger. As a result we also see significant (in most cases) differences in the treatment effects across rounds.

On net, transfer recipients thus saw their income fall *more* when incomes fell from late 2019 to early 2020, but saw hunger increase *less*. This is consistent with the idea that transfers induced recipients to undertake relatively risky income-generating activities, knowing that they could still hedge risks to their most basic needs.

Our data do not let us pin down exactly how this hedging worked. For the long-term arm, where reductions in hunger were largest, it is likely in part simply due to the fact that they were still receiving transfers as of our phone survey. For the other arms, however, transfers had ceased large (short-term arm) or entirely (lump-sum arm). In these cases it seems more likely that households had used part of their transfers, or the income generated from them, to acquire liquid assets that they could draw down. This is one interpretation of the increase in sales of agricultural goods that we see in all three arms (Column 8 of Table 10). These effects are unlikely to represent increased agricultural productivity, since we do not see similar effects at endline (Column 7) with the possible exception of the short-term arm. They may rather reflect households' holding storable commodities such as grain or acquiring buffer-stock agricultural assets such as livestock which they then sold during the pandemic to smooth their consumption of essentials.

7 Conclusion

The unprecedented COVID pandemic raises new questions about old approaches to social protection, and in particular about cash transfers. Will supply chain disruptions and other circumstances reduce their efficacy? How will they affect the severity and the transmission of the disease, both for those who receive them and for those who do not through public health externalities? And how have pre-existing programs affected resilience to the current shock?

In this paper we have examined these issues, taking advantage of a pre-existing field experiment in rural Kenya designed to evaluate the impact of different cash transfer designs, including

Universal Basic income (UBI). In terms of individual well-being, we find modest but positive impacts on measures of food security and physical and mental health, indicating that recipients still benefit from transfers in spite of the COVID crisis. Turning to public health externalities, transfers reduce hospital utilization (potentially increasing capacity for COVID patients), the incidence of illness, and some but not all measures of social distancing. With respect to resilience, transfer recipients saw greater losses of non-agricultural enterprise income during the shock, as previous gains they had made through the creation of new businesses were reversed, but smaller increases in hunger.

Turning to policy implications, this paper shows that, in the context of a large unanticipated shock like COVID-19, access to a generous pre-existing UBI has modest positive effects on a range of measures of well-being. While reassuring in itself, it makes it clear that UBI is unlikely to be the tool of choice in such contexts, and probably for good reason. The theory behind UBI emphasizes the idea that it encourages risk-taking and investment and it is not hard to imagine situations where both of those increases exposure to the shock. Indeed this is one interpretation of what we observe in the present crisis where the very real pre-pandemic income gains from UBI are wiped out during the pandemic. This is not a failing of UBI, just a warning that it is not designed to deal with such extreme situations.

However the other message is that the ability to access income supplements helped during the pandemic. This strengthens the case for building the infrastructure for making universal cash transfers that can be activated at short notice and can be used to deliver additional cash in response to unanticipated crises like the one we are currently experiencing.

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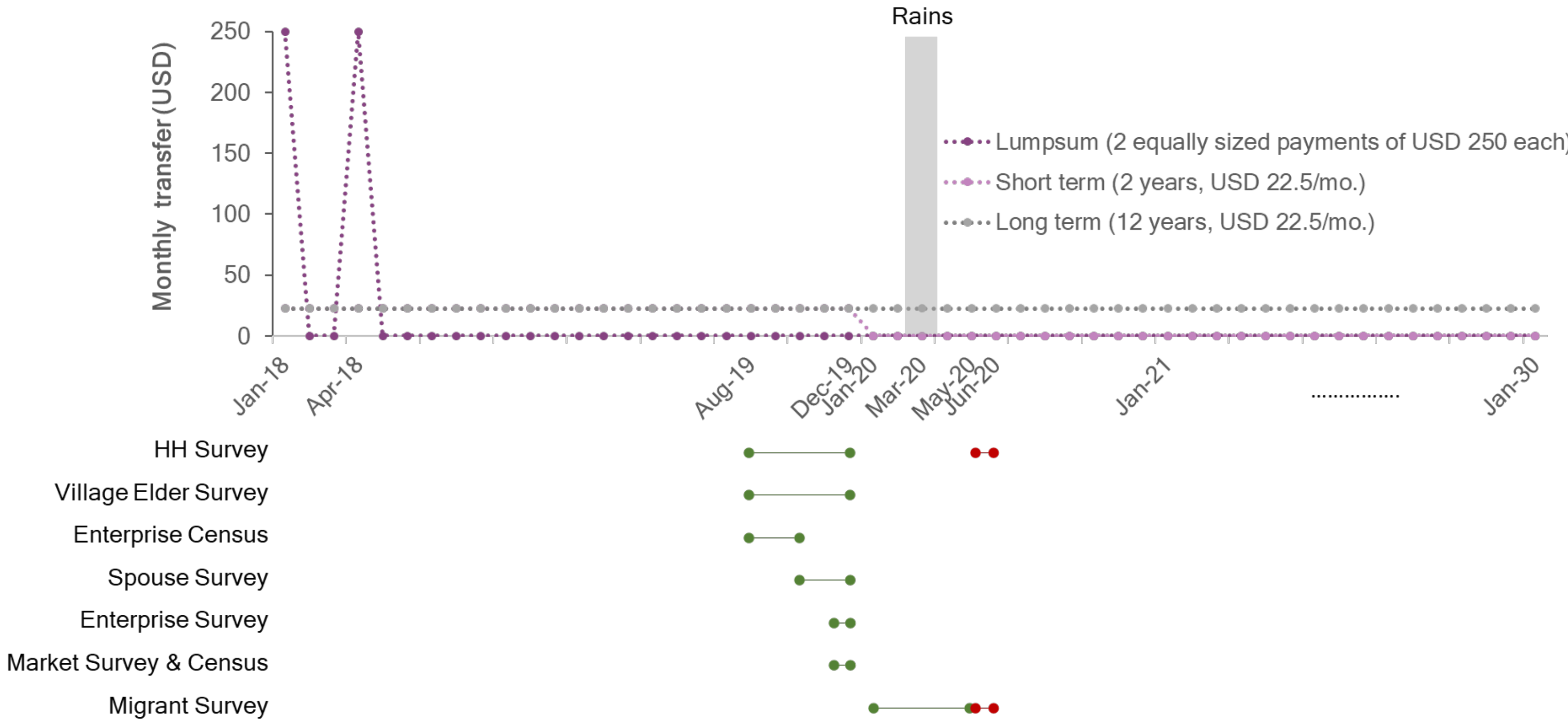
Figure 1: Timing: Transfers, Surveys and Rain



This figure illustrates the relative timing of a typical transfer trajectory in each of the three transfer arms (top panel) and of data collection activities (bottom panel). Note that the actual timing of transfer onset (and therefore transfer cessation) varied somewhat across households.

Table 1: Compliance with experimental assignment

	Received in last 30 days (Endline) (1)	Received in last 12 months (Endline) (2)	Received in last 30 days (Phone Survey) (3)
Long Term Arm	0.803***	0.775***	0.593***
	[0.017]	[0.020]	[0.027]
Short Term Arm	0.843***	0.821***	0.012***
	[0.013]	[0.014]	[0.005]
Lumpsum Arm	0.138***	0.015*	0.005
	[0.012]	[0.008]	[0.004]
R-squared	.622	.694	.531
Control Mean	0.004	0.003	0.002
P-value: ST = LT	0.063*	0.062*	0.000***
P-value: ST = LS	0.000***	0.000***	0.105
P-value: LS = LT	0.000***	0.000***	0.000***
Observations	8483	8483	8392

This table reports treatment effects on an indicator equal to one if the household reported receiving transfers during the specified period at endline (Columns 1-2) or phone survey (Column 3). All regressions include strata fixed effects. Standard errors clustered at the village level are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 2: Completion rates by survey

	Baseline		Endline		Phone Survey	
	Target # (1)	% Complete (2)	Target # (3)	% Complete (4)	Target # (5)	% Complete (6)
Household survey	8850	98.90	8753	97.36	8605	97.93
Spouse survey	5925	73.69	5606	96.6		
Village elder survey	295	99.66	295	100		
Market survey	105	100	105	98.10		
Enterprises census			14659	93.74		
Enterprise survey			3341	93.09		
Adult migrant survey			1840	69.73	1396	89.61

This table reports the target sample size (Columns 1, 3 and 5) and successful completion rate (Columns 2, 4 and 6) for each survey conducted.

Table 3: Household survey completion rates by arm

	Endline (1)	Phone Survey (2)
Long Term Arm	0.006	-0.003
	[0.007]	[0.005]
Short Term Arm	0.010	0.009**
	[0.007]	[0.004]
Lumpsum Arm	0.017***	0.009**
	[0.006]	[0.004]
R-squared	.014	.007
Control Mean	0.967	0.976
P-value: ST = LT	0.525	0.017**
P-value: ST = LS	0.149	0.966
P-value: LS = LT	0.039**	0.019**
Observations	8753	8605

This table reports treatment effects on an indicator equal to one if the household was surveyed. All regressions include strata fixed effects. Standard errors clustered at the village level are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 4: Descriptive statistics and balance

	Mean	F-Stat	P-Value
Household Demographics			
Number of household members	4.916	2.273	.105
Fraction of males	.484	.395	.674
Household head age	49.141	.92	.4
Household experienced hunger	.852	.988	.374
Height for age zscore	-.656	1.301	.274
Weight for age zscore	-1.001	2.656	.072
Social Integration Index	0	.402	.669
CES-Depression Scale	19.822	1.522	.22
Domestic Violence Index	.015	.701	.497
Remittance sent in last 2 months	16.797	1.002	.368
Consumption			
Maize in last 7 days (USD PPP)	19.903	.346	.708
Meat in last 7 days (USD PPP)	1.606	3.045	.049
Outside food in last 7 days (USD PPP)	7.822	.923	.399
Non-food consumption in last 30 days (USD PPP)	76.842	.337	.714
Education in the last 12 months (USD PPP)	543.309	.363	.696
Assets			
Value of assets	19234.53	1.397	.249
Employment			
Household member employed	.648	.952	.387
Monthly paid employment wages (USD PPP)	167.374	1.024	.36
Owns a non-ag enterprise	.213	.501	.607
Monthly non-agricultural enterprise sales (USD PPP)	81.953	1.409	.246
Owns agricultural enterprise	.732	1.547	.215
Sold agricultural output	.478	2.252	.107
Annual agricultural enterprise sales (USD PPP)	151.743	1.645	.195
F-test of Joint Significance		0.04	1.00

This table reports descriptive statistics as of our baseline household survey and tests for balance in these across treatment arms. Column 1 reports the mean of the variable indicated. Column 2 reports the F-statistic from a test of the joint null that the coefficients on all three treatment indicators in Equation 1 are equal to zero, and Column 3 reports the corresponding p-values. In the last row we report the F-statistic and p-value for a test that all coefficients on all treatment indicators for all outcomes are zero.

Table 5: Food security

	Experienced Hunger (1)	Share of days (0 Meals/day) (2)	Share of days (1 Meal/day) (3)	Share of days (2 Meals/day) (4)	Ate Meat/Fish (5)
Long Term Arm	-0.108***	-0.012***	-0.021	-0.031	0.016***
	[0.021]	[0.003]	[0.013]	[0.020]	[0.005]
Short Term Arm	-0.049***	-0.006**	-0.008	0.000	0.006
	[0.018]	[0.003]	[0.009]	[0.015]	[0.004]
Lumpsum Arm	-0.064***	-0.005*	-0.017**	-0.026*	0.009
	[0.017]	[0.003]	[0.009]	[0.014]	[0.006]
R-squared	.082	.021	.036	.038	.059
Control Mean	0.676	0.043	0.169	0.349	0.058
P-value: ST = LT	0.007***	0.069*	0.370	0.143	0.076*
P-value: ST = LS	0.428	0.728	0.357	0.096*	0.670
P-value: LS = LT	0.043**	0.036**	0.784	0.800	0.313
Observations	8398	5311	5305	5294	5309

This table reports treatment effects on measures of food security. The dependent variables are an indicator for experience hunger in the last 30 days (Column 1); the share of days out of the last 30 on which at least one household member ate k or fewer meals for $k = 0, 1, 2$ (Columns 2,3 4); and the share of days out of the last 30 on which the household ate meat or fish (Column 5). All regressions include strata fixed effects. Standard errors clustered at the village level are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 6: Health

	HH Member	Number of members	Consulted		CES-Depression Scale		
	Sick	Sick	Hospital (unconditional)	Hospital (conditional)	Endline	Phone Survey	Change
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Long Term Arm	-0.056***	-0.047	-0.035*	0.002	-2.017***	-1.687***	0.337
	[0.018]	[0.030]	[0.020]	[0.040]	[0.437]	[0.427]	[0.549]
Short Term Arm	-0.043***	-0.047*	-0.046***	-0.052*	-1.823***	-1.071***	0.806
	[0.015]	[0.026]	[0.017]	[0.031]	[0.372]	[0.392]	[0.499]
Lumpsum Arm	-0.036**	-0.047*	-0.028*	-0.018	-1.121***	-0.414	0.714
	[0.014]	[0.024]	[0.016]	[0.029]	[0.373]	[0.413]	[0.522]
R-squared	.116	.109	.067	.018	.2	.151	.012
Control Mean	0.441	0.587	0.287	0.650	16.497	16.047	-0.411
P-value: ST = LT	0.492	0.987	0.600	0.207	0.672	0.165	0.411
P-value: ST = LS	0.634	0.995	0.299	0.291	0.063*	0.127	0.861
P-value: LS = LT	0.281	0.982	0.752	0.635	0.052*	0.008***	0.528
Observations	8398	8398	8398	3465	8330	8105	7955

This table reports treatment effects on measures of physical and mental health. Dependent variables related to health are an indicator for whether any household member was sick in the last 30 days (Column 1); the number of household members who were sick during the last 30 days (Column 2); an indicator for whether the household consulted a care provider at a hospital in the last 30 days (Column 3). Dependent variables related to mental health are the head of household's CES-D depression score as of endline (Column 4), as of phone survey (Column 5), and the change from endline to phone survey (Column 6). All regressions include strata fixed effects. Standard errors clustered at the village level are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 7: Social interaction

	Visited Friends/Relatives (1)	Social Integration Index: Endline (2)	Social Integration Index: Phone Survey (3)	Social Integration Index: Change (4)
Long Term Arm	-0.008	0.002	-0.031	-0.034
	[0.011]	[0.038]	[0.041]	[0.047]
Short Term Arm	-0.019**	0.052*	-0.095***	-0.146***
	[0.008]	[0.027]	[0.030]	[0.037]
Lumpsum Arm	-0.018**	-0.018	-0.065**	-0.046
	[0.007]	[0.026]	[0.029]	[0.037]
R-squared	.011	.018	.021	.015
Control Mean	0.131	-0.000	0.023	0.023
P-value: ST = LT	0.298	0.205	0.125	0.018**
P-value: ST = LS	0.898	0.016**	0.339	0.008***
P-value: LS = LT	0.318	0.614	0.418	0.799
Observations	8398	8476	8395	8369

This table reports treatment effects on measures of social interaction. The dependent variables are the share of days out of the last 30 on which a household member visited a friend or relative (Column 1) and an index of social integration as of endline (Column 2) and phone survey (Column 3) as well as the change from endline to phone survey (Column 4). The index of social integration is the (normalized) sum of (i) the number of social activities the household participated in and (ii) the number of other households with whom a household member spends at least 1 hour per week. All regressions include strata fixed effects. Standard errors clustered at the village level are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 8: Commercial interaction

	Went to					Went Outside for Work		Working from
	Market to Buy (1)	Market to Sell (2)	Town Center (3)	Shopping Center (4)	Matatu/Bus Stn (5)	Current (6)	Usual (7)	Home (Y/N) (8)
Long Term Arm	0.009	0.002	0.005	0.010	-0.009	-0.001	-0.023	0.059
	[0.009]	[0.005]	[0.010]	[0.017]	[0.007]	[0.015]	[0.017]	[0.055]
Short Term Arm	0.008	0.001	-0.004	0.002	0.003	-0.001	-0.003	-0.033
	[0.008]	[0.004]	[0.008]	[0.014]	[0.006]	[0.012]	[0.014]	[0.038]
Lumpsum Arm	0.006	0.001	0.008	0.006	0.006	-0.009	-0.010	0.027
	[0.008]	[0.003]	[0.009]	[0.014]	[0.006]	[0.013]	[0.017]	[0.041]
R-squared	.094	.015	.027	.044	.011	.011	.02	.075
Control Mean	0.112	0.021	0.087	0.273	0.055	0.284	0.423	0.323
P-value: ST = LT	0.904	0.832	0.306	0.675	0.072*	0.991	0.264	0.057*
P-value: ST = LS	0.826	0.925	0.142	0.804	0.578	0.579	0.707	0.114
P-value: LS = LT	0.762	0.880	0.803	0.834	0.029**	0.627	0.521	0.557
Observations	8396	8398	8398	8397	8398	8398	8388	2952

This table reports treatment effects on measures of commercial interaction. The dependent variables in Columns 1-6 are the share of days out of the last 30 on which the household head visited the indicated locations. The dependent variables in Column 6 and 7 are the share of days out of the last 30 days and out of a typical 30 days on which the respondent left home to work. All regressions include strata fixed effects. Standard errors clustered at the village level are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 9: Occupational choice

	Wage Employment			Operated Non-ag Enterprise			Agricultural Enterprises		
	Endline (1)	Phone Survey (2)	Change (3)	Endline (4)	Phone Survey (5)	Change (6)	Endline (7)	Phone Survey (8)	Change (9)
Long Term Arm	-0.040**	-0.003	0.036	0.046***	0.026	-0.019	0.001	0.024	0.023
	[0.019]	[0.025]	[0.033]	[0.017]	[0.018]	[0.020]	[0.007]	[0.031]	[0.031]
Short Term Arm	-0.010	-0.030	-0.020	0.049***	0.037**	-0.013	0.007*	0.000	-0.006
	[0.016]	[0.023]	[0.027]	[0.016]	[0.015]	[0.017]	[0.004]	[0.026]	[0.026]
Lumpsum Arm	0.005	-0.030	-0.033	0.047***	0.045***	-0.002	0.008*	0.025	0.018
	[0.017]	[0.024]	[0.028]	[0.015]	[0.017]	[0.017]	[0.004]	[0.027]	[0.028]
R-squared	.069	.018	.033	.031	.034	.008	.009	.063	.058
Control Mean	0.529	0.497	-0.033	0.292	0.242	-0.049	0.977	0.486	-0.491
P-value: ST = LT	0.154	0.312	0.108	0.874	0.585	0.793	0.307	0.441	0.362
P-value: ST = LS	0.382	0.995	0.638	0.906	0.663	0.544	0.854	0.349	0.381
P-value: LS = LT	0.037**	0.312	0.044**	0.953	0.377	0.438	0.266	0.965	0.885
Observations	8434	8397	8331	8434	8398	8332	8434	8398	8332

This table reports treatment effects on measures of occupational choice. In Columns 1, 4 and 7 the dependent variable is the outcome as of endline; in Columns 2, 5 and 8 it is the outcome as of phone survey; and in Columns 3, 6 and 9 it is the change in the outcome from endline to phone survey. The outcomes are an indicator for whether any household member earned income from wage employment in the previous 30 days (Columns 1-3), an indicator for the whether the household operates a non-agricultural enterprise (Columns 4-6), and an indicator for whether the household operates an agricultural enterprise (Columns 7-9). All regressions include strata fixed effects. Standard errors clustered at the village level are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 10: Earnings by occupation

	Income from Wage Employment			Non-ag Enterprise Profits			Agricultural Sales		
	Endline (1)	Phone Survey (2)	Change (3)	Endline (4)	Phone Survey (5)	Change (6)	Endline (7)	Phone Survey (8)	Change (9)
Long Term Arm	-17.686	-13.964**	2.259	58.506**	3.189	-54.094**	-3.247	16.666***	18.604**
	[12.446]	[5.505]	[12.675]	[25.514]	[3.910]	[24.798]	[5.792]	[5.998]	[7.548]
Short Term Arm	24.611	-6.264	-31.046*	13.774	4.388	-8.838	13.712*	11.583***	-4.047
	[19.570]	[6.464]	[18.247]	[26.173]	[4.278]	[25.015]	[7.220]	[4.010]	[7.420]
Lumpsum Arm	13.129	-1.754	-15.837	71.994*	8.450*	-63.947*	-0.906	8.360**	7.931
	[11.503]	[6.085]	[10.666]	[42.133]	[5.079]	[38.368]	[5.084]	[3.511]	[5.352]
R-squared	.029	.021	.016	.012	.006	.012	.035	.03	.012
Control Mean	161.789	74.695	-85.893	56.030	16.098	-39.996	54.125	29.483	-22.367
P-value: ST = LT	0.036**	0.225	0.087*	0.089*	0.778	0.082*	0.033**	0.416	0.013**
P-value: ST = LS	0.524	0.498	0.375	0.164	0.430	0.153	0.042**	0.420	0.092*
P-value: LS = LT	0.015**	0.044**	0.157	0.692	0.238	0.760	0.696	0.140	0.140
Observations	8434	8397	8331	8434	8398	8332	8434	8398	8332

This table reports treatment effects on earnings from various occupations. In Columns 1, 4 and 7 the dependent variable is the outcome as of endline; in Columns 2, 5 and 8 it is the outcome as of phone survey; and in Columns 3, 6 and 9 it is the change in the outcome from endline to phone survey. The outcomes are earnings from wage labor during the previous 30 days (Columns 1-3), profit from operating a non-agricultural enterprise (Columns 4-6), and revenue from sales of produce from an agricultural enterprise (Columns 7-9). All regressions include strata fixed effects. Standard errors clustered at the village level are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table 11: Food security dynamics

	Hungry @ Phone Survey (1)	Hungry @ Endline Survey (2)	Change $(1-2)$ (3)	Hungry one year before phone survey (4)	Change $(1-4)$ (5)
Long Term Arm	-0.108***	-0.018	-0.093***	-0.061***	-0.050*
	[0.021]	[0.015]	[0.024]	[0.020]	[0.029]
Short Term Arm	-0.049***	-0.020*	-0.028	-0.056***	0.008
	[0.018]	[0.012]	[0.022]	[0.017]	[0.024]
Lumpsum Arm	-0.064***	-0.016	-0.049**	-0.037**	-0.029
	[0.017]	[0.013]	[0.023]	[0.017]	[0.025]
R-squared	.082	.028	.078	.077	.011
Control Mean	0.676	0.123	0.553	0.392	0.284
P-value: ST = LT	0.007***	0.859	0.008***	0.798	0.053*
P-value: ST = LS	0.428	0.718	0.337	0.269	0.124
P-value: LS = LT	0.043**	0.914	0.094*	0.206	0.496
Observations	8398	8365	8268	8268	8268

This table reports treatment effects on measures of food security at different points in time and on changes in food security over time. The primary dependent variables are an indicator for whether the household experienced hunger during the previous 30 days as measured at phone survey (Column 1), during the most recent month as measured at endline (Column 2), and during the month 1 year before taking the phone survey as measured at endline (Column 4). The dependent variables in Columns 3 and 5 are the differences between the dependent variable in Column 1 and that in Columns 2 and 4, respectively. All regressions include strata fixed effects. Standard errors clustered at the village level are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

A Supplemental exhibits

Table A.1: Differential composition of attrition

	F-Stat	P-Value
Household Demographics		
Number of household members	.638	.529
Fraction of males	.841	.432
Household head age	.788	.456
Household experienced hunger	.607	.546
Height for age zscore	4.418	.013
Weight for age zscore	1.148	.319
Social Integration Index	.65	.523
CES-Depression Scale	3.732	.025
Domestic Violence Index	.933	.395
Remittance sent in last 2 months (USD PPP)	2.065	.129
Consumption		
Maize in last 7 days (USD PPP)	1.483	.229
Meat in last 7 days (USD PPP)	2.271	.105
Outside food in last 7 days (USD PPP)	2.305	.102
Non-food consumption in last 30 days (USD PPP)	1.062	.347
Education in the last 12 months (USD PPP)	.345	.708
Assets		
Value of assets (USD PPP)	1.638	.196
Employment		
Household member employed	.987	.374
Monthly paid employment wages (USD PPP)	.021	.979
Owns a non-ag enterprise	.19	.827
Monthly non-agricultural enterprise sales (USD PPP)	2.546	.08
Owns agricultural enterprise	1.417	.244
Sold agricultural output	.148	.862
Annual agricultural enterprise sales (USD PPP)	1.811	.165
F-test of Joint Significance	1.18	0.142

This table reports tests for balance in the composition of attrition across treatment arms. Underlying each row is a regression of an indicator for attrition on a full set of treatment indicators interacted with the given baseline covariate. Column 1 reports the F-statistic from a test of the joint null that the coefficients on all three of the interaction terms in this regression are equal to zero, and Column 3 reports the corresponding p-values.

Table A.2: Food security (with imputation)

	Experienced Hunger (1)	Share of days (0 Meals/day) (2)	Share of days (1 Meal/day) (3)	Share of days (2 Meals/day) (4)	Ate Meat/Fish (5)
Long Term Arm	-0.108***	-0.011***	-0.028***	-0.053***	0.016***
	[0.021]	[0.002]	[0.008]	[0.013]	[0.005]
Short Term Arm	-0.049***	-0.006***	-0.011*	-0.015	0.006
	[0.018]	[0.002]	[0.007]	[0.011]	[0.004]
Lumpsum Arm	-0.064***	-0.006***	-0.021***	-0.039***	0.009
	[0.017]	[0.002]	[0.006]	[0.011]	[0.006]
R-squared	.082	.037	.063	.064	.059
Control Mean	0.676	0.029	0.114	0.236	0.058
P-value: ST = LT	0.007***	0.006***	0.062*	0.006***	0.076*
P-value: ST = LS	0.428	0.875	0.154	0.045**	0.670
P-value: LS = LT	0.043**	0.013**	0.417	0.309	0.313
Observations	8398	8394	8388	8377	5309

This table reports treatment effects on measures of food security. Outcomes are as in Table 5 except that in Columns 2-4 we assume that households reporting never experiencing hunger during the previous 30 days in Column 1 also never had a household member eat fewer than k meals during those days. All regressions include strata fixed effects. Standard errors clustered at the village level are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$.