Effects of a Universal Basic Income during the pandemic*

Abhijit Banerjee[†] Michael Faye[‡] Alan Krueger[§]
Paul Niehaus[¶] Tavneet Suri[‖]

December 8, 2020

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 the 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.

Keywords: Universal Basic Income, cash transfers, COVID-19
JEL Classification: I18, I38, O15

*We are indebted to Charles Amuku, Suleiman Asman, Shreya Chandra, Gabriella Fleischman, Preksha Jain, Eunice Kioko, Teresa Lezcano, Bonnyface Mwangi, Simon Robertson, Mansa Saxena, Nikita Sharma, Debborah Wambua and a field team of over 300 people for their tireless efforts to help bring this paper into existence. We gratefully acknowledge funding from the Bill and Melinda Gates Foundation, the Robert Wood Johnson Foundation, and an anonymous donor. We thank Michael Cooke, Frank Schilbach, and seminar audiences at the NBER Summer Institute, MIT Sloan, the University of California San Diego, Georgia State, New York University, Penn State Universityand VDEV for their feedback. Institutional Review Board (IRB) approvals for the data collection were obtained from MIT and Maseno University in Kenya. Princeton University, the University of California San Diego and Innovations for Poverty Action ceded IRB to MIT. Author contributions: Faye led conceptualization supported by Banerjee, Krueger, Niehaus and Suri; all contributed equally to methodology; Niehaus and Suri led formal analysis; Suri led investigation supported by Niehaus; Niehaus led writing supported by Banerjee, Faye and Suri; Suri led visualization; Suri led supervision supported by Niehaus; Suri led project administration; Niehaus and Suri led research funding acquisition; GiveDirectly led by Faye raised funding for the transfers. Niehaus discloses that he serves without compensation as a director of GiveDirectly. Suri is the corresponding author: E62-524, 100 Main Street, Cambridge MA 02142. Email: tavneet@mit.edu

[†]MIT

[‡]GiveDirectly

[§]Princeton

[¶]University of California, San Diego

[‖]MIT Sloan

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 households on the planet. Governments have responded with an unprecedented expansion in their social protection programming. Between 20 March and 18 September 2020, 212 countries introduced 1,179 new social protection measures covering an estimated 1.8 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 on Universal Basic Income ("UBI") that began 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 facet of this debate on which we focus here concerns UBIs role in protecting people against aggregate shocks. While it does not provide insurance in the formal sense of state-contingent payments, UBI may (some argue) protect people from the harshest consequences of uninsurable or unanticipatable risks. This thesis is typically difficult to test, precisely because it involves claims about rare or unforeseeable contingencies. We (sadly) have a unique opportunity to do so here.

Evaluating social protection during a pandemic raises two conceptual issues. First, it is simply unclear whether to expect the kinds of benefits for the recipients themselves that studies conducted during "normal times have typically found (Bastagli et al. 2019). For material outcomes such as food security, supply chain disruptions may limit the usefulness of demand-side interventions like cash transfers. For non-material outcomes such as depression that typically respond to transfers (Ridley et al. 2020), the pandemic may overwhelm this response. On the other hand, transfers might matter most during the hardest times.[2]

Second, transfers during a pandemic may trigger novel public health externalities. Interper-

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

[2] Ohrnberger et al. (2020) find that a conditional cash transfer in Malawi had larger effects on lower quantiles of the mental health distribution, which could be read as consistent with this idea.

sonal interactions such as socializing with neighbors that we normally associate with positive externalities such as information spillovers now risk increasing the rate of contagion. Health facility utilization, which past research has generally interpreted as a private good (Bastagli et al. 2019), could be a public bad if it contributes to anticipated congestion and miscoordination in health services delivery (Hogan and et al 2020).

We examine these issues using data from a household survey conducted in May-June 2020, in the midst of the strictest phase of Kenya's lockdown to date, and focused only on issues related to the pandemic. As we collected these data by phone they cover fewer topics and may be less precise than typical household survey data. Attrition was very low, however, in part because we had distributed phones to all sampled households at baseline. We obtained a 96% response rate overall and hence very small differences across arms. The study population is poor compared to national averages (Kenya National Bureau of Statistics 2018), but includes the entire population of the study villages (as transfers were "universal" within treated villages), which is useful given that social protection responses to the pandemic have been unusually broadly targeted.[3] Finally, the study design includes multiple treatments sized generously but structured differently: lump-sum transfers, long-term transfers scheduled to continue for a further ten years, and short-term transfers that had largely concluded by the time of the survey. At that juncture lump-sum and short-term recipients had received roughly the same amount in total, while long-term recipients had received slightly (16%) more and anticipated receiving more in the future. The short-term/long-term comparison is thus especially relevant to debates over whether cash-strapped governments should continue their emergency social protection measures.[4]

We find that transfers in all arms had statistically significant impacts on measures of recipients private well-being. Recipients were 4.9-10.8 percentage points less likely to report experiencing hunger during the last 30 days relative to a control mean of 68%, i.e. in a context where

[3] As a benchmark, the World Bank estimated in 2018 that an average of 18% of the population in low-income countries and 41% in low-middle-income countries benefited from *any* social protection or labor program. The share who receive direct transfer programs is necessarily lower (Ivaschenko et al. 2018).

[4] Most recently introduced measures were scheduled to last six months or fewer (Gentilini et al. 2020, Figure 3) and some were one-off (such as the transfers sent to all "Jan Dhan" accounts in India).

hunger was widespread. This effect was significantly larger for the long-term arm that was continuing to receive transfers 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 relative to a control mean of 44%. Given COVID-19s very low prevalence at the time of our surveys (12 cases total in Bomet and Siaya) these illnesses were almost surely something else. Recipients were significantly less depressed in the long-term and short-term arm, though they did not report a higher perceived locus of control. Effects on hunger and depression were significantly larger in the long-term arm than in the short-term arm, while effects on measures of health were not. Overall, transfers continued to impact basic measures of well-being typically as in pre-pandemic research, but with effect sizes that are modest relative to the size of the transfers themselves (US $0.75 nominal or $1.78 PPP per day).

Turning to behaviors related to public health, transfers generally had small beneficial effects or null effects. 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 relative to a control mean of 29%, potentially freeing up health system capacity. The reduction in illness we observe is large enough to 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. On the other hand they had (a reasonably precisely estimated) null effect on interaction for commercial purposes such as shopping or work. 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 social protection transfers during the pandemic. Taking a more ex ante perspective, a central issue for the design of social protection is the extent to which an ongoing programme of cash transfers like the one we study affects *resilience* to large and unexpected aggregate shocks. Theoretically this is ambiguous: transfers (and commitments to future transfers) could enable recipients to build up buffer stocks of savings and assets available to smooth consumption in the face of income fluctuations, for example, but could also induce them to increase their exposure to risky venture by, for example, starting a business, especially if they do not anticipate the kind of shock that resulted from the covid-19 lockdown. To examine these issues we need to observe treatment effects both before and during a shock. We therefore take advantage of additional data we collected for several key outcomes from the same sample as part of a full-scale endline survey in Fall 2019, shortly *before* the pandemic.

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 Fall 2019, 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 Summer 2020 these enterprises remained operational for the most part but with no treatment effects on earnings. These new enterprises suffered along with the old: in the control group, non-agricultural enterprise earnings fell 71% from Fall 2019 to Summer 2020.

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, when 39% of the control group experienced hunger. We find no effects, however, in Fall 2019 when only 13% of the control group was experiencing hunger. This suggests that transfers enabled households to reduce their vulnerability to hunger during bad times, which may in turn help rationalize their decision to take on great income risk. Relaxed credit constraints may also contribute to some extent, but the particular pattern of effects we see and the differences between the short-term and long-term arms imply they cannot fully explain the results.[5]

Our papers main purpose is to provide a systematic assessment of the impacts of social protection during the pandemic. We measure impacts in a sample representative of the local population and across a range of indicators for private well-being, public health, and resilience. These findings complement those from a small body of concurrent work. Londoño-Vélez and Queru-

[5]The onset of the pandemic coincided with the transition from rainy to hungry seasons in the local agricultural cycle (and potentially with other shocks), so we interpret this last set of results as referring to this "composite shock rather than solely to the pandemic per se.

bin (2020) examine effects of a smaller, emergency transfer to the extreme poor in Colombia, finding significant impacts on measures of households financial health and education investment but not on food security or mental health, and *increases* in measures of social interaction. Bottan et al. (2020) find effects on hunger in line with ours in a convenience sample of pension recipients in Bolivia.

The paper also reports the first experimental evidence on the effects of Universal Basic Income from a large-scale trial.[6] The most closely related work has focused on labor supply, estimating the effects of long-term transfers non-experimentally in Alaska (Jones and Marinescu 2018) and Iran (Salehi-Isfahani and Mostafavi-Dehzooei 2017) and generally finding neutral or slightly positive effects. We complement this work with experimental identification and a broader set of outcomes measured during and conceptually related to the pandemic, with a more complete assessment of effects outside of the pandemic to follow.

Finally, our results on resilience speak to the issue of risk in the process of development more broadly. Risk can deter profitable investment (Mobarak and Rosenzweig 2013; Karlan et al. 2014; Cai et al. 2015; Cole and Xiong 2017), and yet profitably insuring the risks facing low-income households has proven difficult (Cole and Xiong 2017). This implies a role for public welfare programs as social protection as well as redistribution, and an argument for making them more "shock-responsive (Oxford Policy Management 2017). Yet our results highlight that even "unresponsive social protection measures such as UBI can induce risk-taking when they entail a credible commitment to the future.

[6] In the developing world, pilots have been conducted in eight villages in Madhya Pradesh (Davala et al. 2015) and two villages in Namibia, with some analysis but without statistical inference. In the developed world, a number of pilots are in progress (though possibly temporarily suspended), with results available from one: Hämäläinen et al. (2019) study a basic income program in Finland that was targeted only to the initially unemployed (not universally) and that required recipients to forego other social benefits so that on net the treatment group actually received *less* in total assistance than the control group. They are further limited by very low response rates in both treatment (31%) and control (20%) arms.

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% a bank account (including digital accounts), 73% a farm enterprise, and 21% a non-farm enterprise. Eighty-five percent had experienced hunger in the year prior to the baseline and maize consumption (typically around 40% of total consumption) was $0.60 PPP per capita per day. The study area is reasonably well-integrated into the larger economy: on average sampled villages had 3 markets within 5km of them (approximately an hour and a half walk), and recent work nearby in Siaya has found that even a very large influx of cash equivalent to an estimated 18% of GDP in the treated villages had only modest effects, on the order of 0.1%-0.2%, on consumer goods prices (Egger et al. 2019).

Kenya has a relatively well-developed digital payments ecosystem: 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 of Safaricom, the dominant telecommunications and mobile money provider in Kenya, was 3km. This number was presumably lower still by 2018 as the overall number of agents had increased substantially. Mobile money use became even more attractive during the pandemic as Safaricom waived fees for peer-to-peer transactions of 1,000 KSh or less and raised limits on transaction sizes in an effort to encourage contactless transactions, facts which will be relevant for interpreting treatment effects on social interaction.

As in many countries, the pandemic in Kenya has had economic impacts well beyond its 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 2020 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.[7] Over July and early August, some of these restrictions were lifted, but Nairobi county remains restricted. Independent tracking surveys in Siaya County suggest that by the end of June low-income household's per capita earnings had fallen 35% from their February levels, from $2.3 PPP to $1.5 PPP per person per day.[8] 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, were moderate in Kenya compared to other African countries, with 7.3 deaths per million residents as of early August compared to 16.2 for the continent as a whole.[9] That said cases are on the rise and some of the restrictions that were lifted are starting to be brought back, and it appears unlikely that schools will reopen for the 2020 calendar year (which in Kenya coincides with the academic year).

Respondents in our survey (described below) were well aware of the pandemic and the associated restrictions on activity (Table A.8). 100% of respondents reported being aware of COVID-19, though only 27% self-described themselves as familiar with its symptoms. 96% and 97% of respondents were aware that restrictions had been imposed on travel and on public gatherings, respectively. Respondents were most likely to have obtained information about the pandemic from the radio (92%) after which friends and family (28%), television (28%), and social media (14%) were the most important sources.

3 Experimental design

The experiment that we build on was designed to examine the impacts of UBI and compare it to other forms of cash transfer. Specifically, it examines four main conditions: control, long-term streams of transfers, short-term streams of transfers, and one-time lump-sum transfers. We randomly assigned these treatments at the village level. We first mapped and conducted a

[7] https://en.wikipedia.org/wiki/COVID-19_pandemic_in_Kenya, accessed 6 August 2020.

[8] https://www.kenyacovidtracker.org/timeseries.html, accessed 6 August 2020.

[9] 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.

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
- **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.[10] 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. This is the equivalent in net present value terms of the short-term transfers at an annual nominal discount rate of 8%.

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.

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

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 two 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 monthly basis. Figure 1 illustrates the assigned timing of transfers in the different arms as well as measurement activities.[11]

Compliance with the experimental assignment was high. Table A.1 examines this using administrative data on all transfers issued by GD. The control mean for all measures is exactly zero, indicating that no households in control villages were issued transfers. As of the date households were interviewed as part of our Fall 2019 survey, most subjects in the short-term and long-term arm had been issued a transfer during the last 30-40 days, while (as expected) none in the lump-sum arm had. The total amounts issued were essentially the same for the short-term and long-term arm. The long-term arm had been issued slightly more (reflecting the differential effect of delays in registering for mobile money in the arms), but these differences were economically small. As of Summer 2020 we see a similar picture except that now transfers to the short-term arm have also ceased and (as a result) the long-term arm has received 16% more in total transfers than the short-term arm. This is as expected: 95% of villages in the short term had completed transfers by May 2020 and in those few that had not 98% of households had completed transfers.[12]

These "first stage figures have two implications for the interpretation of our results below.

[11]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.

[12]These figures and subsequent results do not include one village that was inadvertently randomized into the short-term arm after GD had already begun piloting long-term transfers in it. We obtain essentially identical results if we include this village (in which case results for the short-term arm should be interpreted as intent-to-treat effects) (Appendix B).

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 arm had recently stopped receiving transfers by the time of our Summer 2020 surveys means that we can reasonably interpret differences in impacts between the short-term and long-term arm as the effects of *continuing* to receive transfers conditional on having received them previously. This comparison is relevant to policy decisions about whether to continue the (costly) emergency social protection measures governments have introduced as part of their initial response to the pandemic.

4 Data and empirical methods

4.1 Data

Table A.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 (the "Summer 2020 round) 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.[13] 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. This very high completion rate in a sample that is itself representative of the local population is a

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

strength relative to other evaluations during the pandemic.[14]

Given the high overall completion rate, imbalances across arms are also small. Column 2 of Table A.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.[15]

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; for these outcomes we add the relevant quantities for the migrants to the totals for their household of origin. We attempted 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.[16] 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 Summer 2020 round, we also make limited use of data from a standard endline survey of households conducted between August and December of 2019 (the "Fall 2019 round). We use data from this survey to assess how occupational choice, earnings, and hunger changed since the onset of the pandemic and whether these changes were differential by treatment arm. We attempted to survey 8,753 households as part of the fall 2019 round, and successfully completed interviews with 8,522, or 97.4% of them. Column 1 of Table A.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.[17]

[14]For example, Londoño-Vélez and Querubin (2020) reach 59% of their sample by phone, while Bottan et al. (2020) use a convenience sample recruited online.

[15]Table A.4 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.

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

[17]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 in fall 2019 an enterprise census and enterprise

As in the Summer 2020 round, 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 conducted these migrant surveys between March and May 2020. 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.[18]

Table A.5 presents descriptive statistics for our sample, focusing on the variables that were pre-specified as primary outcomes in our pre-analysis plan for the Fall 2019 data,[19], 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 average 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 A.5 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 generally aim to follow the methods we had pre-specified in our pre-analysis plan for the Fall 2019 round,[20] as given time constraints we did not create a separate analysis plan for the Summer 2020 round. We focus on a simple intent-to-treat specification

$$Y_h = \alpha + \beta_{LT} LT_{v(h)} + \beta_{ST} ST_{v(h)} + \beta_{LS} LS_{v(h)} + \psi_{s(h)} + \epsilon_h \tag{1}$$

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.

[18]For a handful of households (646, or 7.6% of the total) we were not able to survey one or more family members who had migrated until after 15 March 2020, the date of Kenyas initial lockdown. We include these households as part of our "Fall 2019 round in Section 6 below, but our results are essentially unchanged if we omit them.

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

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

where h indexes households which were living in villages $v(h)$ at baseline; ST, LT, LS are indicators for assignment to the short-term, long-term, and lump-sum treatments respectively; and $\psi_{s(h)}$ are stratum fixed effects. We report standard errors adjusted for spatial autocorrelation following (Conley 1999, 2010); clustered standard errors at the village level yields almost identical results.[21]

We focus on summary statistics for well-being (such as whether the household had experienced hunger) or where possible on aggregate indices equal to the unweighted sum of the underlying variables, following the approach we pre-specified for the Fall 2019 round. In Section 5 the outcomes are those measured in Summer 2020; in Section 6 we also examine effects on outcomes as of Fall 2019 and on the change between survey rounds. In the latter case we restrict estimation to the sample of households observed in both rounds, but results are essentially the same if we include all households.

Changes (whether in levels or in treatment effects) should not be attributed solely to the onset of the pandemic, and in particular the Summer 2020 and Fall 2019 surveys fell at different points in the agricultural cropping cycle. The Fall 2019 round was completed just after the harvest period for the "main" agricultural season, which would typically be a relatively good cash-flow period for households that planted in that season. The Summer 2020 round, 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.[22]

[21]This analysis is a subset of that we pre-specified in our original pre-analysis plan for the Fall 2019 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. Results for these specifications estimated on the full Fall 2019 data will be reported in a separate paper. Note that the village treatment indicator in (1) already captures any within-village spillovers. These likely account for a large share of all spillovers; at the time of our surveys the average study village had 1.7 neighbors receiving treatment, as opposed for example to 3.5 in Egger et al. (2019).

[22]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.

5 Impacts during the pandemic

5.1 Effects on individual well-being

We first examine impacts on core measures of private 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 1 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. The effect size is 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 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 intensity 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 generally similar across arms except that the long-term arm is more likely to meat or fish (Column 5), mirroring the larger extensive-margin effect.

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 B.1), 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 and significantly greater than the other arms) 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 while sustained transfers reduced hunger they did not eliminate it.

Table 2 reports impacts on measures of physical and mental health. In Column 1 the outcome is an indicator for whether any household member was sick within the last 30 days, where sick means experiencing 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 2 the outcome is an index equal to the sum of indicators for whether any family member has a history of health conditions including smoking, pneumonia, asthma, tuberculosis, other lung diseases, hypertension, diabetes, or being in any way immunocompromised. In Column 5 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 in both Fall 2019 and Summer 2020, and 43.7% of the control group scored 16 or higher, indicating a population that is generally quite depressed. In Column 6 we examine impacts on an index for locus of control.[23] (We discuss the results in Columns 3-4 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, with no significant differences across arms. This almost surely does not indicate 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.[24] The results

[23] In social psychology, locus of control refers to the extent to which individuals believe that they can control events and outcomes in their own lives.

[24] 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. 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 out of 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

thus almost surely indicate a reduction in non-COVID illnesses, perceived or real, though as we discuss below, this may bear indirectly on COVID risks. Treated households also reported 7-15% fewer historical health issues in their families, significant for the short-term and lump sum arms.

Turning to mental health, CES-D scores also fell (indicating improved mental health) in all three arms, significantly so for the short-term and long-term arm. For lump-sum recipients whose transfers ceased long before the survey this effect is not significantly different from zero and is significantly less than the effect on long-term recipients. For context, the effects we find here are similar to and not significantly different from those we see in the Fall 2019 data; the data thus suggest that while transfers help to improve mental health, they do not necessarily help more in a crisis. This improvement in mental health may be related to the improvement in the perception of physical health–there is some evidence that being depressed exacerbates perceptions of ill-health. We see no significant changes on an index of locus of control, on the other hand, in contrast to the pre-crisis results in which it was significantly higher in the long-term and short-term arms.

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 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 only have power to reject the null of equal effects in some cases. Put differently, there is strong evidence that social protection transfers put in place before the pandemic had benefits during it (though we cannot say how much longer this impact would last) and some evidence that incremental transfer delivered during the pandemic had incremental benefits on top of these.

during the last 30 days.

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.

While we do not think there were any reductions in COVID-19 infections per se, the reductions in illness in Table 2 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. A number of chronic health conditions are associated with more severe cases of coronavirus.[25] The estimated effects on our health history index (Column 2 of Table 2) are potentially consistent with reductions in the prevalence of these conditions; the underlying indicators here measure whether whether any household member has a history of smoking, pneumonia, asthma, tuberculosis, other lung diseases, hypertension, diabetes, or being immunocompromised. On the other hand, these are ailments that typically take a long time to express themselves, and so it is also possible that the reductions we see are not effects on disease itself but on the probability of diagnosis. This might be the case for example if treated recipients were less likely to get check-ups because they experience less depression and acute sickness.

Second, we find significant corresponding reductions in the rates at which transfer recipi-

[25]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.

ents sought consultation at a hospital (Table 2, 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 mechanical consequences of lower rates of real or perceived illness in treated households; consistent with this interpretation, if we estimate treatment effects conditional on illness (keeping in mind that this is clearly endogenous) the effects are generally smaller (Table 2, 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 do so (for example they can afford to stay home and rest rather than seeing a doctor for a minor ailment in the hope of getting well sooner).[26] 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. Transfers might increase interaction as recipients move about to collect them and then to spend them, though the fact that transfers were delivered via mobile money in the context of a well-developed digital payments ecosystem meant that recipients might also choose to spend their digital currently remotely. 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: for example it may reduce the need to get help from a friend or relative to pay for essentials. Within the control group, income is moderately positively correlated with our measure of social interaction ($\rho = 0.03$) and our measure of commercial reaction ($\rho = 0.17$). While not causal, this raises the concern that transfers might increase interactions, with potential undesirable health effects.

We find that transfers did not substantially alter interaction for commercial purposes. Col-

[26] 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.

umn 1 of Table 3 reports impacts on an index which sums up the proportion of days out of the last 30 on which the household head left the house for various commercial purposes. While in aggregate this was quite common, with a control mean of 0.83 such trips per day, we find estimated effects very close to zero. These estimates are also reasonably precise; at the 95% confidence level we can reject increases of more than 12%, 8%, and 9% in the long-term, short-term, and lump sum arms respectively. This is bad news relative to a prior that transfers would enable recipients to work less, and good news relative to a prior that recipients would leave the house more often to spend the money.[27] These results make an interesting contrast with those of Londoño-Vélez and Querubin (2020), who find that even when receiving recently introduced mobile money payments recipients in Colombia took more trips to banks and ATMs to withdraw physical cash, with none reporting that they used mobile money to purchase goods. This highlights the importance of the digital payments ecosystem within which transfers take place, as opposed to the nominal modality of the initial transfer.

We do find some evidence that transfers reduced interaction for social purposes during the pandemic. Columns 2-5 of Table 3 report impacts on the share of days out of the last 30 on which a household member visited another household (Column 2) 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 3-5). 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 zero or from the effects in the other arms. 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

[27] We find no significant effects on any of the sub-components of this index, so it does not appear to be the case that we are capturing offsetting positive and negative effects. Consistent with this, we also find no significant effects on time use in the last 24 hours (Table A.9, with the p-value from an F-test that all effects are zero of 0.94.

results thus likely capture the same behavioral change.

Overall, transfers did not meaningfully alter the rate of commercial interaction, and significantly but slightly reduced some forms of social interaction. At a minimum, this suggests that transfers during the pandemic in the form of social support 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, the effects of the intervention on the amount of risk actually borne by the households can go either way. 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.[28]

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.

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 4 report impacts as of Fall 2019 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 in the Fall 2019 round as having had any sales or incurred any cost in the previous 12 months, but in the Summer 2020 round based on whether any household member had been involved in the running of any agricultural enterprise within the previous 30 days (remembering that the Summer 2020 survey was during the lean season); the two measures should thus not be interpreted as exactly comparable.

Prior to the onset of the pandemic (and of the agricultural lean season) we see that households in all three arms are significantly more likely to operate a non-agricultural enterprise. Effects range from 4-5 percentage points on a control mean of 31 percentage points, or 13%-16% in relative terms. For the long-term arm there is an offsetting, marginally significant reduction

[28] 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.

in wage work, while for the other arms we see no significant changes. There is little evidence that they substitute away from agricultural enterprises, which remain ubiquitous (97%) in all arms and if anything slightly more common in the short-term and lump-sum arms. Overall, the pattern shows 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 5 report effects as of Fall 2019 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. [29] As of Fall 2019 (Columns 1, 4, and 7) we see patterns that broadly parallel those for occupational choice: non-agricultural enterprise profits increase, significantly so for the long-term arm; labor earnings fall in the long-term arm; and agriculture earnings do not change except in the short-term arm.

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 no change in the probability of wage employment from Fall 2019 to Summer 2020 (Table 4), and a modest 5 percentage point reduction in the probability of operating a non-agricultural enterprise (29% to 24%). The probability of operating an agricultural enterprise falls substantially (97% to 51%) but this is as one would expect given the definition of "operating and the seasonality of agricultural activity. Earnings from these activities, on the other hand, were down sharply: 54%, 71%, and 42% 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? They were

[29] Also note that since this survey was done by phone, we had to aggregate some of the questions where we had much more detail in the Fall 2019 round 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.

not forced into outright closure. Columns 2, 5, and 8 report level effects on each occupational indicator as of Summer 2020, and Columns 3, 6 and 9 the treatment effects on the change between Fall 2019 and Summer 2020. Treatment effects in Summer 2020 are generally similar to and not statistically distinguishable from those in Fall 2019. Paralleling control group trends, however, we do see meaningful declines in earnings from these new enterprises. In particular, the large and significant increase in non-agricultural enterprise earnings we observed for the long-term arm in Fall 2019 is nearly entirely reversed by Summer 2020, with the change significantly different from zero. It thus appears that these new enterprises were, like other enterprises, not immune to the shock. Consistent with this, we similarly see no significant changes in overall cash earnings as of Summer 2020, though point estimates are generally positive (Table A.10).

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 ($p = 0.09$ and $p = 0.06$, respectively). Since both groups had received roughly the same amount of money (in the same monthly payment structure) at the time of both surveys, this indicates 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 Food security

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

In Fall 2019, 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 Summer 2020 round both to those at the time we conducted the Fall 2019 survey, and also to those one year earlier, i.e. in April-June 2019. The Fall 2019-to-Summer 2020 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 6). In the control group, 39% of households report experiencing hunger in the month in 2019 one year before their Summer 2020 survey. By Fall 2019, when households would have been relatively flush with the proceeds from harvest, this figure had fallen by a factor of 3 to just 12%. By the time of our Summer 2020 round, 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.[30]

Transfers reduced the sensitivity of hunger to these seasonal and/or pandemic-induced fluctuations. We see little evidence of treatment effects in Fall 2019, 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 percentage point decrease at the 95% confidence level. In both the Summer 2020 round (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 differences in the treatment effects across rounds.

6.3 Interpretation

On net, transfer recipients thus saw their income fall *more* when incomes in general fell between late 2019 and 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. Of course, the creation of new non-agriculture enterprises may also reflect to some extent the relaxation of credit constraints. But since the incremental income from these enterprises was wiped out during the pandemic, the enduring effects on hunger must reflect some other form of (self)-insurance. Effects on hunger, labor income, and non-agriculture enterprise profits are also more pronounced for the long-

[30]Note that the Summer 2019 data was collected in Fall 2019 and is therefore from recall, while the Summer 2020 data was collected essentially in real time.

term arm – which had received roughly the same amount of capital as the other two arms and differed only in that it anticipated future transfers – suggesting that the management of future consumption risk plays a role.[31]

As for the specific risk-coping mechanisms that may be at work, our data give us only a partial picture. In all arms we see very small increases in sales of agricultural outputs in Summer 2020 (Column 8 of Table 5). Given that it was the lean season these effects likely represent the sale of items purchased or produced previously and potentially held as buffer stocks. Interestingly, we do not see corresponding effects as of Fall 2019 (Column 7) for the long-term and lump sum arms. This suggests that while productivity may play some role at least part of the effect works through storage: relaxing liquidity constraints allowed treated households to defer the sale of agricultural outputs, as in Burke et al. (2018), or to invest in perennials whose output is by nature deferred.

For the long-term arm, where reductions in hunger were largest, the mechanical fact that they were still receiving transfers as of the Summer 2020 round was surely important. We also see some evidence of substitution across coping mechanisms, however. Long-term recipients were significantly less likely to sell tangible assets or to receive remittances from friends or family during the previous 30 days (Table A.6, Columns 2 and 4) and significantly more likely to withdraw liquid savings (Column 1). They were also significantly less likely to have paid formal tax or informal contributions to local government (Table A.7). The overall pattern is suggestive of a shift away from informal and social insurance mechanisms towards reliance on private savings.

For the other arms, in which transfers had ceased largely (short-term arm) or entirely (lump-sum arm), we see less evidence of changes in participation in either private (Table A.6) or public (Table A.7) sharing mechanisms. This is consistent with the idea that these relatively informal risk-sharing mechanisms are made incentive-compatible by the fact that one may need them again in the future. Such incentives were likely reduced for the long-term arm relative to the

[31] Also related, Egger et al. (2019) find little evidence of an investment response to lump-sum transfers in nearby parts of Siaya.

others.

One nuance that arises when comparing effects on Fall 2019 and Summer 2020 outcomes is that the former were measured in-person while the latter were measured by phone. A common reason to question comparability between phone and field surveys is that phone surveys elicit lower response rates, but that was not the case here: the response rate to our Summer 2020 survey was 98%, actually slightly higher than in Fall 2019.[32] (Not surprisingly, we get very similar results if we estimate the same models on the full sample of households observed in either round.) We also largely used the same question phrasings in both surveys (and note any exceptions). The concern would thus have to be that respondents answer the same questions differently when they are asked by phone and in person.

While there is no way to rule this possibility out entirely, several pieces of circumstantial evidence support the reliability of the phone data. For example, we see high self-reported levels of and significant treatment effects on depression, an outcome that is thought to be hard to elicit by phone. Perhaps most importantly, our core comparative finding is that levels of and effects on enterprise earnings and hunger move in *opposite* directions across rounds. It would take a very specific form of "differential differences across measurement technologies and across outcomes to generate this pattern.

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

[32] As a benchmark, Suri (2018) and Bharadwaj et al. (2019) obtained response rates of 60% and 69%, respectively, to similar phone surveys in Kenya.

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 interaction. 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, that hit during the lean season (a partly anticipated shock), 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 increase exposure to shocks. 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, especially with basics like food and health. 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.

References

Balboni, Clare, Oriana Bandiera, Robin Burgess, Maitreesh Ghatak, and Anton Heil, "Why Do People Stay Poor?," Technical Report DP14534, CEPR March 2020.

Banerjee, Abhijit, Paul Niehaus, and Tavneet Suri, "Universal Basic Income in the Developing World," *Annual Review of Economics*, 2019, *11* (1), 959–983.

Bastagli, Francesca, Jessica Hagen-Zanker, Luke Harman, Valentina Barca, Georgina Sturge, and Tanja Schmidt, "Cash transfers: what does the evidence say?," Technical Report, Overseas Development Institute July 2016.

_ , _ , _ , _ , _ , and _ , "The Impact of Cash Transfers: A Review of the Evidence from Low- and Middle-income Countries," *Journal of Social Policy*, 2019, *48* (3), 569594.

Bharadwaj, Prashant, William Jack, and Tavneet Suri, "Fintech and Household Resilience to Shocks: Evidence from Digital Loans in Kenya," Working Paper 25604, National Bureau of Economic Research February 2019.

Bottan, Nicholas, Bridge Hoffmann, and Diego A. Vera-Cossio, "Building resilience during the pandemic: evidence from a non-contributory pension program in Bolivia," Technical Report 2020.

Burke, Marshall, Lauren Falcao Bergquist, and Edward Miguel, "Sell Low and Buy High: Arbitrage and Local Price Effects in Kenyan Markets*," *The Quarterly Journal of Economics*, 12 2018, *134* (2), 785–842.

Cai, Jing, Alain De Janvry, and Elisabeth Sadoulet, "Social Networks and the Decision to Insure," *American Economic Journal: Applied Economics*, April 2015, *7* (2), 81–108.

Cole, Shawn A. and Wentao Xiong, "Agricultural Insurance and Economic Development," *Annual Review of Economics*, September 2017, *9* (1), 235–262.

Conley, Timothy G., "GMM estimation with cross sectional dependence," *Journal of Econometrics*, 1999, *92* (1), 1 – 45.

_ , "Spatial Econometrics," in Steven N. Durlauf and Lawrence E. Blume, eds., *Microeconometrics*, London: Palgrave Macmillan UK, 2010, pp. 303–313.

Davala, Sarath, Renana Jhabvala, Soumya Mehta, and Guy Standing, *Basic Income, A Transformative Policy for India*, Bloomsbury Academic, 2015.

Davies, Mary-Ann, "HIV and risk of COVID-19 death: a population cohort study from the Western Cape Province, South Africa.," *medRxiv*, 2020.

Egger, Dennis, Johannes Haushofer, Edward Miguel, Paul Niehaus, and Michael W Walker, "General Equilibrium Effects of Cash Transfers: Experimental Evidence from Kenya," Working Paper 26600, National Bureau of Economic Research December 2019.

Fu, Ning, Rachel Glennerster, Kristen Himelein, Nina Rosas, and Tavneet Suri, "The Socio-Economic Impacts of Ebola in Sierra Leone," Technical Report, World Bank Group 2015.

Gentilini, Ugo, Mohamed Bubaker Alsafi Almenfi, Pamela Dale, Robert Palacios, Harish J. Natarajan, Guillermo Alfonso Galicia Rabadan, Yuko Okamura, John D. Blomquist, Miglena Abels, Gustavo C. Demarco, and Indhira Vanessa Santos, "Social Protection and Jobs Responses to COVID-19 : A Real-Time Review of Country Measures (September 18, 2020)," Technical Report, World Bank Group 2020.

Glennerster, Rachel and Tavneet Suri, "Agriculture and Nutrition in Sierra Leone," Technical Report, MIT 2019.

Hämäläinen, Kari, Ohto Kanninen, Miska Simanainen, and Jouko Verho, "Employment effects for the first year of the basic income experiment," in Olli Kangas, Signe Jauhiainen, Miska Simanainen, and Minna Ylikännö, eds., *The basic income experiment 20172018 in Finland: Preliminary results*, 2019.

Hogan, Alexandra B and et al, "Potential impact of the COVID-19 pandemic on HIV, tuberculosis, and malaria in low-income and middle-income countries: a modelling study," *The Lancet Global Health*, August 2020.

Ivaschenko, Oleksiy, Claudia P. Rodriguez Alas, Marina Novikova, Carolina Romero, Thomas Bowen, and Linghui Zhu, *The State of Social Safety Nets 2018*, World Bank Group, 2018.

Jones, Damon and Ioana Marinescu, "The Labor Market Impacts of Universal and Permanent Cash Transfers: Evidence from the Alaska Permanent Fund," Working Paper 24312, National Bureau of Economic Research February 2018.

Karlan, Dean, Robert Osei, Isaac Osei-Akoto, and Christopher Udry, " Agricultural Decisions after Relaxing Credit and Risk Constraints *," *The Quarterly Journal of Economics*, 02 2014, *129* (2), 597–652.

Kenya National Bureau of Statistics, "Basic report: 2015/2016 Kenya Integrated Household Budget Survey (KIHBS)," Technical Report, Kenya National Bureau of Statistics March 2018.

Londoño-Vélez, Juliana and Pablo Querubin, "The Impact of Emergency Cash Assistance in a Pandemic: Experimental Evidence from Colombia," Technical Report 2020.

Mobarak, Ahmed Mushfiq and Mark R. Rosenzweig, "Informal Risk Sharing, Index Insurance, and Risk Taking in Developing Countries," *American Economic Review*, May 2013, *103* (3), 375–80.

Ohrnberger, Julius, Eleonora Fichera, Matt Sutton, and Laura Anselmi, "The worse the better? Quantile treatment effects of a conditional cash transfer programme on mental health," *Health Policy and Planning*, 09 2020.

Oxford Policy Management, "Shock-Responsive Social Protection Systems Research: Literature review (2nd Edition)," Technical Report 2017.

Radloff, Lenore Sawyer, "The CES-D Scale: A Self-Report Depression Scale for Research in the General Population," *Applied Psychological Measurement*, 1977, *1* (3), 385–401.

Ridley, Matthew, Gautam Rao, Frank Schilbach, and Vikram Patel, "Poverty, depression, and anxiety: Causal evidence and mechanisms," *Science*, 2020, *370* (6522).

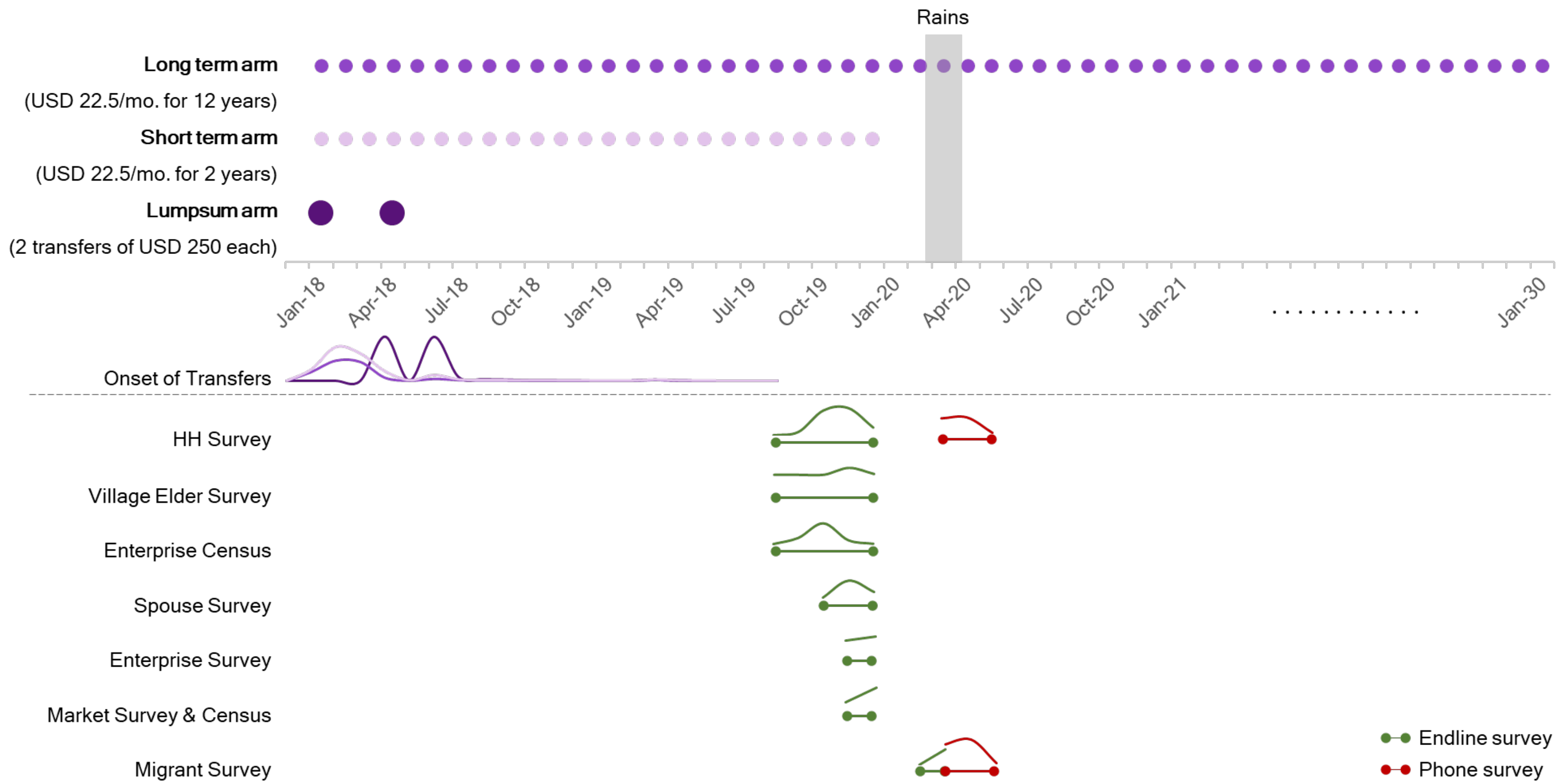
Salehi-Isfahani, Djavad and Mohammad Mostafavi-Dehzooei, "Cash transfers and labor supply: evidence from a large-scale program in Iran," Working paper 1090, Economic Research Forum 2017.

Suri, Tavneet, "Impacts of Access to the Internet," AEA RCT Registry 2018.

_ **and William Jack**, "The long-run poverty and gender impacts of mobile money," *Science*, 2016, *354* (6317), 1288–1292.

Williamson, Elizabeth J., Alex J. Walker, Krishnan Bhaskaran, and et al, "Factors associated with COVID-19-related death using OpenSAFELY," *Nature*, 2020.

Figure 1: Timing: Transfers, Surveys and Rain



This figure illustrates the relative timing of transfers and data collection activities. The top panel illustrates the assigned transfer trajectory in each of the three treatment arms. The bottom panel illustrates the distribution of the actual timing of transfer onset (purple), in-person surveys (green), and phone surveys (red). Note that since the actual timing of transfer onset varied across household the timing of transfer cessation did as well.

Table 1: 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	-0.11***	-0.01***	-0.02	-0.03	0.02***
	[0.02]	[0.00]	[0.01]	[0.02]	[0.01]
Short Term Arm	-0.05**	-0.01**	-0.01	0.00	0.01
	[0.02]	[0.00]	[0.01]	[0.01]	[0.00]
Lumpsum Arm	-0.06***	-0.01**	-0.02*	-0.03*	0.01
	[0.02]	[0.00]	[0.01]	[0.01]	[0.01]
R-squared	.08	.02	.04	.04	.06
Control Mean	0.68	0.04	0.17	0.35	0.06
P-value: ST = LT	0.01***	0.10	0.37	0.10	0.07*
P-value: ST = LS	0.43	0.71	0.32	0.06*	0.61
P-value: LS = LT	0.02**	0.04**	0.79	0.79	0.26
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 adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^*p < .10, ^{**}p < .05, ^{***}p < .01$.

Table 2: Health

	Any member	Health	Consulted hospital		CES-Depression	Locus of Control
	sick	history	unconditional	conditional	Scale	Score
	(1)	(2)	(3)	(4)	(5)	(6)
Long Term Arm	-0.06***	-0.04	-0.04*	0.00	-1.69***	0.02
	[0.02]	[0.03]	[0.02]	[0.04]	[0.45]	[0.13]
Short Term Arm	-0.04***	-0.08**	-0.05***	-0.05**	-1.07***	0.10
	[0.02]	[0.03]	[0.02]	[0.03]	[0.38]	[0.13]
Lumpsum Arm	-0.04***	-0.07**	-0.03*	-0.02	-0.41	-0.01
	[0.01]	[0.03]	[0.02]	[0.03]	[0.37]	[0.15]
R-squared	.12	.06	.07	.02	.15	.07
Control Mean	0.44	0.55	0.29	0.65	16.05	13.11
P-value: ST = LT	0.48	0.20	0.54	0.16	0.11	0.59
P-value: ST = LS	0.64	0.78	0.22	0.21	0.06*	0.46
P-value: LS = LT	0.25	0.36	0.74	0.64	0.00***	0.84
Observations	8398	8398	8398	3465	8105	8115

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); an index equal to the sum of indicators for whether any family member has a history of health conditions including smoking, pneumonia, asthma, tuberculosis, other lung diseases, hypertension, diabetes, or being in any way immunocompromised (Column 2); an indicator for whether the household consulted a care provider at a hospital in the last 30 days (Column 3), and the same indicator conditional on having had a sick household member (Column 4). Dependent variables related to mental health are the head of households CES-D depression score (Column 5) and locus of control score (Column 6). Depression is measured on a scale from 00to 60 with values above 16 indicating clinical depression. Locus of control is measured on a scale from 0 to 24 with higher values indicating higher external locus of control. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 3: Interpersonal interaction

	Commercial interaction	Social interaction	Social Integration Index		
			Fall 2019	Summer 2020	Change
	(1)	(2)	(3)	(4)	(5)
Long Term Arm	0.50	-0.01	0.00	-0.03	-0.03
	[1.20]	[0.01]	[0.04]	[0.04]	[0.05]
Short Term Arm	0.26	-0.02**	0.05**	-0.09***	-0.15***
	[0.86]	[0.01]	[0.03]	[0.03]	[0.04]
Lumpsum Arm	0.54	-0.02**	-0.02	-0.06**	-0.05
	[1.01]	[0.01]	[0.02]	[0.03]	[0.03]
R-squared	.01	.01	.02	.02	.01
Control Mean	24.98	0.13	-0.00	0.02	0.02
P-value: ST = LT	0.84	0.27	0.19	0.14	0.02**
P-value: ST = LS	0.81	0.91	0.01***	0.27	0.00***
P-value: LS = LT	0.98	0.30	0.63	0.44	0.80
Observations	8395	8398	8476	8395	8369

This table reports treatment effects on measures of interpersonal interaction. The dependent variables are the sum of the number of days out of the last 30 on which the household head went to (i) a market to sell items, (ii) a market to buy items, (iii) a town center, (iv) a shopping center, (v) a matatu station or bus stop, and the number of days on which (s)he (v) worked outside of the house (Column 1); the share of days out of the last 30 on which the household head visited a friend or relative (Column 2); and an index of social integration as of Fall 2019 (Column 3), as of Summer 2020 (Column 4), and the difference (Column 5). 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 adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table 4: Occupational choice

	Wage Employment			Operated non-ag enterprise			Operated ag enterprise		
	Fall 2019 (1)	Summer 2020 (2)	Change (3)	Fall 2019 (4)	Summer 2020 (5)	Change (6)	Fall 2019 (7)	Summer 2020 (8)	Change (9)
Long Term Arm	-0.04*	-0.01	0.03	0.04**	0.03	-0.01	0.00	0.03	0.03
	[0.02]	[0.03]	[0.04]	[0.02]	[0.02]	[0.02]	[0.01]	[0.03]	[0.03]
Short Term Arm	-0.01	-0.03	-0.02	0.05**	0.04***	-0.01	0.01*	0.00	-0.01
	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.02]	[0.00]	[0.02]	[0.02]
Lumpsum Arm	0.00	-0.03	-0.03	0.04***	0.05***	0.00	0.01	0.02	0.02
	[0.02]	[0.02]	[0.03]	[0.02]	[0.02]	[0.02]	[0.00]	[0.03]	[0.03]
R-squared	.07	.02	.03	.03	.03	.01	.01	.06	.06
Control Mean	0.53	0.53	0.00	0.31	0.26	-0.05	0.97	0.51	-0.47
P-value: ST = LT	0.20	0.41	0.13	0.65	0.51	0.85	0.29	0.38	0.28
P-value: ST = LS	0.44	0.92	0.78	0.86	0.61	0.49	0.65	0.37	0.33
P-value: LS = LT	0.06*	0.49	0.10*	0.78	0.31	0.46	0.40	0.92	0.78
Observations	8376	8375	8375	8376	8376	8376	8376	8376	8376

This table reports treatment effects on measures of occupational choice. In Columns 1, 4 and 7 the dependent variable is the outcome as of Fall 2019; in Columns 2, 5 and 8 it is the outcome as of Summer 2020; and in Columns 3, 6 and 9 it is the change in the outcome from Fall 2019 to Summer 2020. 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 adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 5: Earnings by occupation

	Wage income			Non-ag enterprise profits			Agricultural sales		
	Fall 2019 (1)	Summer 2020 (2)	Change (3)	Fall 2019 (4)	Summer 2020 (5)	Change (6)	Fall 2019 (7)	Summer 2020 (8)	Change (9)
Long Term Arm	-20.22*	-13.82**	6.40	57.21**	3.22	-54.00**	-1.73	16.85***	18.58**
	[10.95]	[6.12]	[11.72]	[25.14]	[3.48]	[23.91]	[5.29]	[5.92]	[7.37]
Short Term Arm	24.58	-6.10	-30.68*	13.38	4.46	-8.92	15.95**	11.83***	-4.12
	[17.51]	[6.35]	[15.83]	[25.49]	[3.41]	[24.72]	[6.35]	[3.84]	[6.05]
Lumpsum Arm	12.79	-1.48	-14.25	72.25	8.55	-63.71	0.77	8.63**	7.86
	[10.67]	[6.04]	[10.69]	[45.35]	[5.42]	[41.00]	[5.43]	[3.43]	[5.01]
R-squared	.03	.02	.02	.01	.01	.01	.04	.03	.01
Control Mean	159.84	74.42	-85.42	55.90	16.10	-39.80	51.57	29.38	-22.19
P-value: ST = LT	0.02**	0.27	0.04**	0.09*	0.74	0.06*	0.01**	0.38	0.00***
P-value: ST = LS	0.49	0.53	0.32	0.19	0.46	0.18	0.02**	0.32	0.03**
P-value: LS = LT	0.01**	0.07*	0.10	0.68	0.25	0.77	0.67	0.13	0.11
Observations	8376	8375	8375	8374	8376	8374	8376	8376	8376

This table reports treatment effects on earnings from various occupations. In Columns 1, 4 and 7 the dependent variable is the outcome as of Fall 2019; in Columns 2, 5 and 8 it is the outcome as of Summer 2020; and in Columns 3, 6 and 9 it is the change in the outcome from Fall 2019 to Summer 2020. 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 adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table 6: Food security dynamics

	Summer 2019 (1)	Fall 2019 (2)	Summer 2020 (3)	Change $(3-2)$ (4)	Change$(3-1)$ (5)
Long Term Arm	-0.06***	-0.02	-0.11***	-0.09***	-0.05*
	[0.02]	[0.01]	[0.02]	[0.02]	[0.03]
Short Term Arm	-0.06***	-0.02	-0.05**	-0.03	0.01
	[0.01]	[0.01]	[0.02]	[0.03]	[0.02]
Lumpsum Arm	-0.04**	-0.02	-0.06***	-0.05**	-0.03
	[0.02]	[0.01]	[0.02]	[0.02]	[0.02]
R-squared	.08	.03	.08	.08	.01
Control Mean	0.39	0.12	0.68	0.55	0.28
P-value: ST = LT	0.79	0.89	0.01***	0.01**	0.03**
P-value: ST = LS	0.31	0.75	0.43	0.32	0.13
P-value: LS = LT	0.25	0.90	0.02**	0.06*	0.49
Observations	8268	8268	8376	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 month one year before the Summer 2020 survey as measured Fall 2019 (Column 1), during the most recent month as measured in Fall 2019 (Column 2), and during the most recent 30 days as measured in Summer 2020 (Column 3). The dependent variables in Columns 4 and 5 are the differences between the dependent variable in Column 3 and that in Columns 2 and 1, respectively. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

A Supplemental exhibits

Table A.1: Compliance with experimental design

	Fall 2019			Summer 2020		
	Last 30 days (1)	Last 40 days (2)	Amount recd (3)	Last 30 days (4)	Last 40 days (5)	Amount recd (6)
Long Term Arm	0.89***	0.91***	1944.74***	0.67***	0.83***	2602.87***
	[0.02]	[0.01]	[54.92]	[0.05]	[0.03]	[71.80]
Short Term Arm	0.84***	0.92***	1946.07***	0.01	0.01*	2250.20***
	[0.03]	[0.01]	[31.28]	[0.01]	[0.01]	[34.38]
Lumpsum Arm	0.01	0.00	2141.80***	0.00	0.00	2151.89***
	[0.01]	[0.00]	[40.28]	[0.01]	[0.00]	[41.91]
R-squared	.78	.86	.55	.64	.79	.55
Control Mean	0.00	0.00	0.00	0.00	0.00	0.00
P-value: ST = LT	0.18	0.63	0.98	0.00***	0.00***	0.00***
P-value: ST = LS	0.00***	0.00***	0.00***	0.24	0.03**	0.02**
P-value: LS = LT	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***
Observations	8362	8362	8362	8362	8362	8362

This table examines compliance with the experimental assignment using administrative data on mobile money transfers issued by GiveDirectly. Columns 1-3 report treatment effects on an indicator for transfer issuance during the previous 30 days, an indicator for transfer issuance during the previous 40 days, and the cumulative amount transferred to date, respectively, as of the date of the respondents Fall 2019 survey. Columns 4-6 report the corresponding figures as of the date of the respondents Summer 2020 survey. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table A.2: Completion rates by survey

	Fall 2017		Fall 2019		Summer 2020	
	Target # (1)	% Complete (2)	Target # (3)	% Complete (4)	Target # (5)	% Complete (6)
Household survey	8850	98.90	8753	97.36	8605	97.93
Relocations			292	64.38	292	69.52
Spouse survey	5925	73.69	5606	96.6		
Adult migrant survey			1840	69.73	1396	89.61
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		

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 A.3: Household survey completion rates by arm

	Fall 2019 (1)	Summer 2020 (2)
Short Term Arm	0.01	0.01*
	[0.01]	[0.01]
Long Term Arm	0.01	0.00
	[0.01]	[0.01]
Lumpsum Arm	0.02***	0.02***
	[0.01]	[0.01]
R-squared	.01	.01
Control Mean	0.97	0.96
P-value: ST = LT	0.49	0.16
P-value: ST = LS	0.15	0.23
P-value: LS = LT	0.05*	0.02**
Observations	8753	8753

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

Table A.4: Differential composition of attrition

	F-Stat	P-Value
Household Demographics		
Number of household members	0.64	0.53
Fraction of males	0.84	0.43
Household head age	0.79	0.46
Household experienced hunger	0.61	0.55
Height for age zscore	4.42	0.01
Weight for age zscore	1.15	0.32
Social Integration Index	0.65	0.52
CES-Depression Scale	3.73	0.03
Domestic Violence Index	0.93	0.39
Remittance sent in last 2 months (USD PPP)	2.07	0.13
Consumption		
Maize in last 7 days (USD PPP)	1.48	0.23
Meat in last 7 days (USD PPP)	2.27	0.1
Outside food in last 7 days (USD PPP)	2.31	0.1
Non-food consumption in last 30 days (USD PPP)	1.06	0.35
Education in the last 12 months (USD PPP)	0.35	0.71
Assets		
Value of assets (USD PPP)	1.64	0.2
Employment		
Household member employed	0.99	0.37
Monthly paid employment wages (USD PPP)	0.02	0.98
Owns a non-ag enterprise	0.19	0.83
Monthly non-agricultural enterprise sales (USD PPP)	2.55	0.08
Owns agricultural enterprise	1.42	0.24
Sold agricultural output	0.15	0.86
Annual agricultural enterprise sales (USD PPP)	1.81	0.17
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.5: Descriptive statistics and balance

	Mean	F-Stat	P-Value
Household Demographics			
Number of household members	4.92	2.27	0.1
Fraction of males	0.48	0.39	0.67
Household head age	49.14	0.92	0.4
Household experienced hunger	0.85	0.99	0.37
Height for age zscore	−0.66	1.3	0.27
Weight for age zscore	−1	2.65	0.07
Social Integration Index	0	0.4	0.67
CES-Depression Scale	19.82	1.52	0.22
Domestic Violence Index	0.01	0.7	0.5
Remittance sent in last 2 months	16.8	1	0.37
Consumption			
Maize in last 7 days (USD PPP)	16.8	1	0.37
Meat in last 7 days (USD PPP)	1.61	3.04	0.05
Outside food in last 7 days (USD PPP)	7.82	0.92	0.4
Non-food consumption in last 30 days (USD PPP)	76.84	0.34	0.71
Education in the last 12 months (USD PPP)	543.31	0.36	0.7
Assets			
Value of assets	19 234.53	1.4	0.25
Employment			
Household member employed	0.65	0.95	0.39
Monthly paid employment wages (USD PPP)	167.37	1.02	0.36
Owns a non-ag enterprise	0.21	0.5	0.61
Monthly non-agricultural enterprise sales (USD PPP)	81.95	1.41	0.25
Owns agricultural enterprise	0.73	1.55	0.21
Sold agricultural output	0.48	2.25	0.11
Annual agricultural enterprise sales (USD PPP)	151.74	1.64	0.19
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 A.6: Liquidity

	Withdrew Savings (1)	Sold Assets (2)	Sent Remittance (3)	Received Remittance (4)	Borrowed (5)	Lent (6)
Long Term Arm	0.07***	-0.03**	0.02	-0.05*	-0.00	-0.01
	[0.03]	[0.01]	[0.03]	[0.03]	[0.02]	[0.01]
Short Term Arm	0.02	-0.02	-0.04*	-0.01	-0.03**	-0.01
	[0.02]	[0.01]	[0.02]	[0.02]	[0.01]	[0.01]
Lumpsum Arm	0.02	-0.00	0.01	-0.03*	-0.03*	0.01
	[0.02]	[0.01]	[0.02]	[0.02]	[0.01]	[0.01]
R-squared	.01	.02	.01	.02	.01	.01
Control Mean	0.34	0.10	0.45	0.40	0.52	0.18
P-value: ST = LT	0.08*	0.49	0.03**	0.20	0.21	0.82
P-value: ST = LS	0.98	0.31	0.05*	0.46	0.74	0.15
P-value: LS = LT	0.08*	0.10*	0.51	0.43	0.36	0.21
Observations	8398	8398	8398	8398	8398	8398

This table reports treatment effects on measures of cash flow from various potential sources of / uses of liquidity. The outcome in each column is a binary indicator equal to one if the respondent took the indicated action within the last 30 days. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Table A.7: Public contributions

	Paid taxes (1)	Paid VE (projects) (2)	Paid VE (personal use) (3)	Received money from VE (4)
Long Term Arm	-0.02**	-0.03***	-0.01*	-0.01
	[0.01]	[0.01]	[0.01]	[0.01]
Short Term Arm	-0.02***	-0.00	-0.01	-0.01
	[0.01]	[0.01]	[0.00]	[0.01]
Lumpsum Arm	-0.01	0.01	-0.00	-0.01***
	[0.01]	[0.01]	[0.01]	[0.00]
R-squared	.01	.02	0	.01
Control Mean	0.07	0.07	0.03	0.03
P-value: ST = LT	0.98	0.01***	0.49	0.94
P-value: ST = LS	0.15	0.06*	0.87	0.12
P-value: LS = LT	0.22	0.00***	0.46	0.27
Observations	8398	8398	8398	8398

This table reports treatment effects on measures of contribution to or from local sources of public funds. The outcome in each column is a binary indicator equal to one if the respondent took the indicated action within the last 30 days. "VE refers to the village elder, a formally appointed position within local government. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.8: Covid-19 awareness and sources of information

	Mean	SD	N
Awareness			
Aware of Coronavirus	1	0.05	2808
No. of symptoms aware of	2.64	1.22	2882
Aware if govt imposed any restrictions on travel	0.96	0.2	2808
No. of months restrictions on travel expected to last	4.07	4.2	1919
No. of travel restrictions aware of	2.06	1.38	2882
Aware if govt imposed restrictions on public gatherings	0.97	0.17	2808
No. of months restrictions on public gatherings expected to last	3.9	3.69	1976
No. of restrictions on public gatherings aware of	2.4	1.37	2882
Sources			
Friends/family	0.28	0.45	2800
Local school teacher	0	0.06	2800
Newspapers	0.04	0.2	2800
Radio	0.92	0.28	2800
Social media	0.14	0.35	2800
Television	0.28	0.45	2800
Local church leader / pastor	0.01	0.1	2800
Village elder	0.05	0.23	2800
Community health worker	0.04	0.19	2800
Local administration	0.11	0.31	2800

This table reports descriptive statistics on awareness of the pandemic and sources of this information within the control group.

Table A.9: Time use

	Work (1)	Household Activities (2)	Leisure (3)	Religious and community activities (4)	School (5)	Job Search (6)	Other activities (7)	Sleep (8)
Long Term Arm	0.24	-0.14	0.11	0.02	-0.00	-0.04	0.01	0.06
	[0.16]	[0.11]	[0.14]	[0.02]	[0.00]	[0.02]	[0.03]	[0.08]
Short Term Arm	0.03	-0.05	0.05	-0.01	0.00	-0.00	0.04	0.03
	[0.12]	[0.08]	[0.11]	[0.02]	[0.00]	[0.02]	[0.03]	[0.07]
Lumpsum Arm	0.01	-0.05	-0.05	0.02	-0.00	-0.01	0.04	0.11
	[0.13]	[0.09]	[0.12]	[0.02]	[0.00]	[0.02]	[0.03]	[0.07]
R-squared	.01	.04	.01	0	.01	0	.01	.02
Control Mean	3.84	2.05	6.30	0.07	0.00	0.10	0.12	8.86
P-value: ST = LT	0.18	0.47	0.67	0.33	0.63	0.21	0.41	0.73
P-value: ST = LS	0.91	0.96	0.43	0.15	0.15	0.64	0.97	0.23
P-value: LS = LT	0.21	0.46	0.25	0.77	0.82	0.43	0.42	0.52
Observations	8398	8398	8398	8398	8398	8398	8398	8398

This table reports treatment effects on the number of hours spent by the household head in the indicated activities over the previous 24 hours. "Work includes farm activities, livestock and other working hours. "Household activities include time spent on cleaning the house, washing clothes, collecting firewood, cooking for the household, fetching water for home use, looking after children and milling. "Leisure includes time spent on shopping, socializing and other leisure activities. "School includes time spent in school, completing school homework and walking to and from school. "Other activities include attending a funeral, visiting hospital etc. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table A.10: Total earnings

	Mean	25th percentile	50th percentile	75th percentile
	(1)	(2)	(3)	(4)
Long Term Arm	5.89	-0.00	6.20	9.42
	[8.79]	[1.62]	[4.19]	[11.24]
Short Term Arm	9.71	-0.00	3.72	9.91
	[9.30]	[1.40]	[4.09]	[7.50]
Lumpsum Arm	15.04*	-0.00	3.72	13.63*
	[9.08]	[1.45]	[3.82]	[7.86]
Constant	120.41***	0.00	132.61***	237.46***
	[5.40]	[3.10]	[21.82]	[7.90]
R-squared	.03	.02	.03	.03
Control Mean	120.28	120.28	120.28	120.28
P-value: ST = LT	0.70	1.00	0.58	0.97
P-value: ST = LS	0.59	1.00	1.00	0.64
P-value: LS = LT	0.32	1.00	0.56	0.71
Observations	8398	8398	8398	8398

This table reports treatment effects on total cash earnings, equal to the sum of labor earnings, non-agricultural enterprise profits, and proceeds from the sale of agricultural outputs as in Table 5. This is not a full accounting of income as it does not account for the costs of agricultural outputs sold. Estimation is via ordinary least squares with standard errors adjusted for spatial autocorrelation in Column 1, and via quantile regression with standard errors clustered at the village level in Columns 2-4. All regressions include strata fixed effects. Statistical significance is denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

B Robustness checks

This section examines the sensitivity of the main results to sample changes. Table B.1 replicates Table 1 but includes all households in Columns 2-4, assuming that those that had not experienced hunger always ate 3 meals per day. Tables B.2-B.7 replicate Tables 1-6 but include the "pilot village in which GD delivered long-term transfers but which the research team inadvertently assigned to the short-term arm.

Table B.1: 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	-0.11***	-0.01***	-0.03***	-0.05***	0.02***
	[0.02]	[0.00]	[0.01]	[0.01]	[0.00]
Short Term Arm	-0.05***	-0.01***	-0.01*	-0.02	0.01
	[0.02]	[0.00]	[0.01]	[0.01]	[0.00]
Lumpsum Arm	-0.06***	-0.01***	-0.02***	-0.04***	0.01
	[0.02]	[0.00]	[0.01]	[0.01]	[0.01]
R-squared	.08	.04	.06	.06	.06
Control Mean	0.68	0.03	0.11	0.24	0.06
P-value: ST = LT	0.01***	0.01***	0.06*	0.01***	0.08*
P-value: ST = LS	0.43	0.88	0.15	0.05**	0.67
P-value: LS = LT	0.04**	0.01**	0.42	0.31	0.31
Observations	8398	8394	8388	8377	5309

This table reports treatment effects on measures of food security. Outcomes are as in Table 1 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 adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table B.2: 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	-0.11***	-0.01***	-0.02	-0.03	0.02***
	[0.02]	[0.00]	[0.01]	[0.02]	[0.01]
Short Term Arm	-0.05**	-0.01**	-0.01	-0.00	0.01
	[0.02]	[0.00]	[0.01]	[0.01]	[0.00]
Lumpsum Arm	-0.06***	-0.01**	-0.02*	-0.03*	0.01
	[0.02]	[0.00]	[0.01]	[0.01]	[0.01]
R-squared	.08	.02	.04	.04	.06
Control Mean	0.68	0.04	0.17	0.35	0.06
P-value: ST = LT	0.00***	0.10	0.38	0.11	0.06*
P-value: ST = LS	0.42	0.70	0.33	0.07*	0.58
P-value: LS = LT	0.02**	0.04**	0.79	0.80	0.25
Observations	8427	5334	5328	5317	5332

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 adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**}p < .05,^{***}p < .01$.

Table B.3: Health

	Any member	Health	Consulted hospital		CES-Depression	Locus of control
	sick	history	unconditional	conditional	Scale	score
	(1)	(2)	(3)	(4)	(5)	(6)
Long Term Arm	-0.06***	-0.04	-0.03*	0.00	-1.68***	0.03
	[0.02]	[0.03]	[0.02]	[0.04]	[0.45]	[0.12]
Short Term Arm	-0.04***	-0.08**	-0.05***	-0.05*	-1.08***	0.11
	[0.02]	[0.03]	[0.02]	[0.03]	[0.38]	[0.13]
Lumpsum Arm	-0.03***	-0.07**	-0.03*	-0.02	-0.41	-0.01
	[0.01]	[0.03]	[0.02]	[0.03]	[0.37]	[0.15]
R-squared	.12	.06	.07	.02	.15	.07
Control Mean	0.44	0.55	0.29	0.65	16.05	13.10
P-value: ST = LT	0.55	0.21	0.53	0.18	0.12	0.58
P-value: ST = LS	0.56	0.82	0.21	0.24	0.05*	0.40
P-value: LS = LT	0.26	0.37	0.74	0.65	0.00***	0.78
Observations	8427	8427	8427	3478	8133	8290

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); an index equal to the sum of indicators for whether any family member has a history of health conditions including smoking, pneumonia, asthma, tuberculosis, other lung diseases, hypertension, diabetes, or being in any way immunocompromised (Column 2); an indicator for whether the household consulted a care provider at a hospital in the last 30 days (Column 3), and the same indicator conditional on having had a sick household member (Column 4). Dependent variables related to mental health are the head of households CES-D depression score (Column 5) and locus of control score (Column 6). Depression is measured on a scale from 0 to 60 with values above 16 indicating clinical depression. Locus of control is measured on a scale from 0 to 24 with higher values indicating high external locus of control. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**} p < .05, ^{***} p < .01$.

Table B.4: Interpersonal interaction

	Commercial interaction (1)	Social interaction (2)	Social Integration Index: Fall 2019 (3)	Social Integration Index: Summer 2020 (4)	Social Integration Index: Change (5)
Long Term Arm	0.57	-0.01	0.00	-0.03	-0.04
	[1.20]	[0.01]	[0.04]	[0.04]	[0.05]
Short Term Arm	0.17	-0.02**	0.05**	-0.09***	-0.14***
	[0.86]	[0.01]	[0.03]	[0.03]	[0.04]
Lumpsum Arm	0.59	-0.02**	-0.02	-0.07**	-0.05
	[1.01]	[0.01]	[0.02]	[0.03]	[0.03]
R-squared	.01	.01	.02	.02	.01
Control Mean	24.98	0.13	-0.00	0.02	0.02
P-value: ST = LT	0.75	0.24	0.18	0.17	0.03**
P-value: ST = LS	0.72	0.83	0.01***	0.35	0.00***
P-value: LS = LT	0.99	0.29	0.63	0.44	0.81
Observations	8424	8427	8506	8424	8398

This table reports treatment effects on measures of interpersonal interaction. The dependent variables are the sum of the number of days out of the last 30 on which the household head went to (i) a market to sell items, (ii) a market to buy items, (iii) a town center, (iv) a shopping center, (v) a matatu station or bus stop, and the number of days on which (s)he (v) worked outside of the house (Column 1); the share of days out of the last 30 on which a household member visited a friend or relative (Column 2); and an index of social integration as of Fall 2019 (Column 3), as of Summer 2020 (Column 4), and the difference (Column 5). 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 adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table B.5: Occupational choice

	Wage Employment			Operated non-ag enterprise			Operated ag enterprise		
	Fall 2019 (1)	Summer 2020 (2)	Change (3)	Fall 2019 (4)	Summer 2020 (5)	Change (6)	Fall 2019 (7)	Summer 2020 (8)	Change (9)
Long Term Arm	-0.04*	-0.01	0.03	0.04**	0.03	-0.01	0.00	0.03	0.03
	[0.02]	[0.03]	[0.04]	[0.02]	[0.02]	[0.02]	[0.01]	[0.03]	[0.03]
Short Term Arm	-0.01	-0.03	-0.03	0.05**	0.04***	-0.01	0.01*	0.00	-0.01
	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]	[0.02]	[0.00]	[0.02]	[0.02]
Lumpsum Arm	0.00	-0.03	-0.03	0.04***	0.05***	0.00	0.01	0.03	0.02
	[0.02]	[0.02]	[0.03]	[0.02]	[0.02]	[0.02]	[0.00]	[0.03]	[0.03]
R-squared	.07	.02	.03	.03	.03	.01	.01	.06	.06
Control Mean	0.53	0.53	0.00	0.31	0.26	-0.05	0.97	0.51	-0.47
P-value: ST = LT	0.23	0.35	0.12	0.76	0.51	0.73	0.29	0.35	0.26
P-value: ST = LS	0.38	0.83	0.81	0.98	0.61	0.58	0.62	0.34	0.30
P-value: LS = LT	0.06*	0.48	0.10*	0.80	0.31	0.45	0.40	0.91	0.77
Observations	8405	8404	8404	8405	8405	8405	8405	8405	8405

This table reports treatment effects on measures of occupational choice. In Columns 1, 4 and 7 the dependent variable is the outcome as of Fall 2019; in Columns 2, 5 and 8 it is the outcome as of Summer 2020; and in Columns 3, 6 and 9 it is the change in the outcome from Fall 2019 to Summer 2020. 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 adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10, ^{**}p < .05, ^{***}p < .01$.

Table B.6: Earnings by occupation

	Wage income			Non-ag enterprise profits			Agricultural sales		
	Fall 2019 (1)	Summer 2020 (2)	Change (3)	Fall 2019 (4)	Summer 2020 (5)	Change (6)	Fall 2019 (7)	Summer 2020 (8)	Change (9)
Long Term Arm	-19.90*	-13.54**	6.36	57.21**	3.24	-53.98**	-1.59	16.95***	18.55**
	[10.98]	[6.12]	[11.70]	[25.09]	[3.48]	[23.86]	[5.28]	[5.92]	[7.36]
Short Term Arm	24.13	-6.49	-30.62*	13.38	4.43	-8.95	15.76**	11.69***	-4.07
	[17.47]	[6.35]	[15.77]	[25.43]	[3.40]	[24.67]	[6.34]	[3.84]	[6.04]
Lumpsum Arm	13.03	-1.28	-14.29	72.26	8.57	-63.69	0.87	8.70**	7.84
	[10.67]	[6.04]	[10.70]	[45.29]	[5.41]	[40.95]	[5.44]	[3.43]	[5.01]
R-squared	.03	.02	.02	.01	.01	.01	.04	.03	.01
Control Mean	159.84	74.42	-85.42	55.90	16.10	-39.80	51.57	29.38	-22.19
P-value: ST = LT	0.02**	0.32	0.04**	0.08*	0.75	0.06*	0.01**	0.36	0.00***
P-value: ST = LS	0.52	0.48	0.33	0.19	0.45	0.18	0.02**	0.35	0.03**
P-value: LS = LT	0.01**	0.07*	0.10	0.68	0.26	0.77	0.67	0.13	0.11
Observations	8405	8404	8404	8403	8405	8403	8405	8405	8405

This table reports treatment effects on earnings from various occupations. In Columns 1, 4 and 7 the dependent variable is the outcome as of Fall 2019; in Columns 2, 5 and 8 it is the outcome as of Summer 2020; and in Columns 3, 6 and 9 it is the change in the outcome from Fall 2019 to Summer 2020. 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 adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.

Table B.7: Food security dynamics

	Summer 2019 (1)	Fall 2019 (2)	Summer 2020 (3)	Change $(3-2)$ (4)	Change$(3-1)$ (5)
Long Term Arm	-0.06***	-0.02	-0.11***	-0.09***	-0.05*
	[0.02]	[0.01]	[0.02]	[0.02]	[0.03]
Short Term Arm	-0.06***	-0.02*	-0.05**	-0.03	0.01
	[0.01]	[0.01]	[0.02]	[0.03]	[0.02]
Lumpsum Arm	-0.04**	-0.02	-0.06***	-0.05**	-0.03
	[0.02]	[0.01]	[0.02]	[0.02]	[0.02]
R-squared	.08	.03	.08	.08	.01
Control Mean	0.39	0.12	0.68	0.55	0.28
P-value: ST = LT	0.79	0.79	0.01***	0.01***	0.03**
P-value: ST = LS	0.31	0.63	0.42	0.28	0.13
P-value: LS = LT	0.25	0.92	0.02**	0.06*	0.49
Observations	8297	8297	8405	8297	8297

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 month one year before taking the Summer 2020 survey as measured in Fall 2019 (Column 1), during the most recent month as measured in Fall 2019 (Column 2), and during the most recent 30 days as measured at Summer 2020 (Column 3). The dependent variables in Columns 4 and 5 are the differences between the dependent variable in Column 3 and that in Columns 2 and 1, respectively. All regressions include strata fixed effects. Standard errors adjusted for spatial autocorrelation are reported in brackets, with statistical significance denoted as: $^{*}p < .10,^{**} p < .05,^{***} p < .01$.