Social Protection and Social Distancing During the Pandemic: Mobile Money Transfers in Ghana

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Abstract

We study the impact of both the anticipation and receipt of mobile money transfers to a representative sample of low-income Ghanaians during the COVID-19 pandemic. In the short-run, the mere announcement of upcoming transfers affects neither income, consumption, labor supply, well-being, nor social distancing. Once disbursed, transfers increase contemporaneous food expenditure by 8%, income by 20%, and a social distancing index by 0.08 standard deviations. Over 40% of the transfers are spent on food. The positive effects on consumption and income do not persist to two years after the last transfer. Together, we learn that cash transfers can support households economically while also promoting adherence to public health protocols during a pandemic.

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1 Introduction

The COVID-19 pandemic shock to economic activities affected the poor in developing countries particularly severely, as these citizens were already vulnerable and largely without access to formal government social protection (Egger et al. 2021). The danger of in-person interactions during a pandemic and government encouragement to social distance led individuals to adjust their behavior, including work and consumption patterns. These changes disrupted economic activity for many, especially those in informal sectors. While many rich countries responded by expanding social protections, low and middle-income country governments have less financial and institutional capacity for such policy responses. The mid-pandemic scale up of social protection programs poses major challenges, from identifying those most affected by the shock to designing mechanisms to provide the needed support (Aiken et al. 2023; Gerard et al. 2020).

Mobile money is a transparent and rapidly scalable approach to social protection during a crisis. Such transfers incur relatively low transaction costs and quickly get resources to targeted individuals with minimal social interaction (Amoah et al. 2020). But how effective is such support on immediate, humanitarian outcomes such as food security? Furthermore, a key motivation for such COVID-19 emergency relief efforts was to *reduce* labor supply, with the aim of increasing social distancing. In "normal" times, negative labor supply responses instead tend to be a feared consequence of transfer programs. Existing evidence on this question is mixed – while recent high-powered evidence from the US finds that large transfers reduce labor force participation by two percentage points (Vivalt et al. 2024), evidence for low-income countries points either to a null or a positive effect (Banerjee et al. 2017, 2022; Kaur et al. 2021). Thus a key question is whether cash transfers lead to an increase or decrease in social distancing.

We report results of an experiment in Ghana that randomized a series of cash transfers to low-income households as part of a pandemic response. We use the existing nationally representative Ghana Socioeconomic Panel Survey from 2018 to identify a pool of 1,508 potential transfer recipients in low-income households with access to mobile money accounts across

Ghana. Individuals were randomly assigned to either treatment or control, and informed of their assignment at the end of a short baseline survey. Respondents were told the transfers were to help cope with the economic impacts of the coronavirus, and were told to spend them however they pleased. All individuals (treatment and control) received a single payment of 90 Ghanaian Cedis (GHC, about US$15, or US$42 PPP) after the baseline survey. Treatment individuals then received seven more transfers of 90 GHC. Transfers were intended to be delivered at approximately one-week intervals, although in practice the transfers were delivered roughly every three weeks due to logistical constraints. The value of each transfer was deliberately substantial; each transfer is equal to 65 percent of the median weekly food expenditure of households in our baseline survey. The flagship social welfare program in Ghana, in contrast, provides transfers of less than 10 percent of median food expenditure of recipient households.

We report findings from five phone survey waves and one in-person survey. The first phone survey took place after treatment announcement and after all individuals had received the initial transfer. Any treatment effects at the point of this first follow-up reflect anticipation effects, since this survey was before the treatment group started receiving their additional transfers. The second, third, and fourth phone follow-ups took place while the treatment group continued to receive ongoing transfers. The fifth phone follow-up took place roughly eight months after the final cash transfer. Our sixth follow-up is the fourth wave of the in-person Ghana Panel Survey, administered roughly two years after the final transfer was disbursed. We explore effects on food security, labor supply, income, and self-reported compliance with social distancing, among other outcomes. We discuss effects in the sixth follow-up separately from those in the phone follow-ups since the outcome measures are not fully comparable.

We note that anticipation effects may be especially important to study during a pandemic or other nationwide crises. In such times a government must mobilize responses on many fronts. If the mere assurance that cash transfers were on the way was sufficient to improve mental health or induce behavior changes such as social distancing, then a government could tolerate small delays in the implementation of transfers while prioritizing the implementation of other time-sensitive policies. In contrast, if there are no anticipation effects of cash transfers, then their implementation is more urgent.

We have three main findings. First, we find little evidence of anticipation effects: at the first follow-up when both our treatment and control group had received exactly one transfer but the treatment group had been told they would receive more, we are unable to reject equality across all key outcomes (and in particular labor supply). Thus, governments that wish to use cash transfers to induce behavior change must prioritize their implementation rather than relying on effects materializing from the mere announcement of the policy.

Second, the transfers yielded a statistically and economically significant contemporaneous improvement of household financial well-being and food security, as well as an increase in social distancing. Treated households spent about 8% more on food relative to the control mean. We estimate that upwards of 40% of the transfers were spent on food, while we do not find a statistically significant increase in non-food expenditure. Households that receive our transfers maintain a 20-33% higher income throughout the economic crisis than those in the control group. Some of this effect on income is driven by the extensive margin; treated households are four percentage points more likely to have earned a non-zero amount of income during survey weeks in our transfer period. The treatment group also score 0.08 standard deviations higher on a social distancing index, driven primarily by an increase in the number of days that treated households stayed at home. However, we do not find statistically significant evidence that the transfers positively affected psychological well-being.

Third, the effects on income somewhat persist to our fifth follow-up survey, eight months after the final transfer. In this wave, the treatment group are four percentage points more likely to report any income, and reported 20% higher income on average, although both effects are imprecisely estimated, and not statistically significant at conventional levels. Otherwise, we see little evidence of persistent impacts two years after the final transfer was disbursed, using the fourth wave of the Ghana Panel Survey.

We do not find evidence of meaningful heterogeneity in impacts along most dimensions.

The most notable exception is that female-headed households appear to have a considerably stronger increase in their contemporaneous food expenditure, perhaps due to their heightened vulnerability during the economic crisis. We also find evidence that some microentrepreneurs used the grant to shift towards at-home production.

Together our findings provide support for cash transfers as a mode of pandemic support for poor households in low-income countries. Upwards of 40% of the transfer was spent on food, the transfers increased social distancing, and they bolstered recipients' incomes in a manner that persisted somewhat beyond the termination of the transfers. That said, our two-year follow-up results do not show evidence of enduring effects.

A number of experiments randomized cash transfers during the COVID-19 crisis. Tables 1 and 2 catalogue the ongoing and completed trials documented in the AEA RCT Registry that we found in a three-step process. First, we identified all unconditional cash transfer trials that were produced by searching for the keywords "COVID" and "Cash." Second we searched on Google for "COVID Cash Transfer RCT" and identified any papers or ongoing projects within the first four pages of search results that reported on cash transfer experiments during the COVID-19 crisis. Finally, we reviewed the research page of the NGO GiveDirectly for completed or ongoing cash transfer trials that coincided with the COVID-19 crisis. Tables 1 and 2 note the study location, sample characteristics, the design of the cash transfers, and a summary of the results for the experiments with published or working papers.

We identified fifteen ongoing or completed studies, nine of which currently have working or published papers. Compared with those nine, we make two primary contributions. First, we incorporate a pre-transfer post-announcement "anticipation" test, to examine whether the mere promise of cash led to immediate shifts in economic activities and psychological well-being. While anticipatory consumption responses are limited by savings and borrowing opportunities (and indeed failures of consumption smoothing are well-known), anticipatory well-being responses are not. Nevertheless, we estimate null effects on both types of outcomes.

Second, we study a policy that could be implemented at scale by a local or national government on a sample that is representative of a broad population of policy interest. Specifically we study transfer amounts that are meaningful enough to make a measurable difference but small enough to be scalable as a wide policy, and our sample was drawn from roughly the bottom half of the income distribution of the nationally representative Ghana Panel Survey. Existing papers typically meet at most one of those two criteria: either scalable cash transfer schemes but on a non-representative sample (e.g. Brooks et al. 2022; Jacob et al. 2022; Pilkauskas et al. 2023) or more representative samples but with transfer amounts that would require a significant political economy shift to finance a scaled-up implementation (e.g. Banerjee et al. 2020).[1] An important exception is Londoño-Vélez and Querubin (2022). They study the impact of mobile money transfers on a population enrolled in a welfare program in Colombia, finding small positive effects on household well-being. We build on their work by exploring the effects of mobile money in a low-income country.

Third, as documented in Table 1, we estimate longer-term effects – up to two years after the final transfer – than all existing COVID-19 cash transfer studies with the exception of Banerjee et al. (2020). The latter is an outlier given that the cash transfers in that study were made prior to the pandemic. Our evidence on longer-term effects suggests that pandemic-era cash transfers are unlikely to have enduring effects.

Like our study, four others find that transfers cause a statistically and economically significant increase in food expenditure, with the remaining five not finding statistically significant effects. Also in line with our study, two other experiments (out of the four that measure social distancing) find that cash transfers increase social distancing, though one finds a reduction. Finally, only one other study (out of the three that measure income) finds that transfers increase

[1]Banerjee et al. (2020), Stein et al. (2022), and Aggarwal et al. (2022), all study transfers likely too large to be implemented at scale by a government (USD 0.75 per adult per day for 12 years in the case of Banerjee et al. (2020), a one-time transfer of USD 1000 in the case of Stein et al. (2022), and one, two, or three transfers of USD 250 in the case of Aggarwal et al. (2022)). In all three cases the transfers were implemented by GiveDirectly, an international nonprofit organization. Another important difference between our study and Banerjee et al. (2020) and Aggarwal et al. (2022) is that the latter two papers evaluate a transfer scheme that preceded the COVID-19 crisis and continued throughout it, whereas our study evaluates a cash transfer scheme rolled out in response to the crisis.

income (of businesses, Brooks et al. 2022).

2 Context and Experiment Design

Ghana saw its first confirmed cases of COVID-19 in March 2020.[2] As of May 2020, 84% of Ghanaians in a nationally representative survey reported a drop in income resulting from the COVID-19 crisis,[3] 33% reported a drop in employment, 30% reported reduced access to markets, and 52% reported missed or reduced meals (Egger et al. 2021). Excess deaths during 2020 and 2021 have since been estimated to be 35,900, a mortality rate of 58.3 per 100,000 (Wang et al. 2022).[4]

Mobility dropped rapidly in Ghana around April 2020, as measured by Google (Figure A1). While mobility to workplaces remained below baseline levels for practically the duration of our five follow-up surveys (top panel), retail and recreational mobility had returned to baseline levels by late-2020, and mobility throughout the pandemic was less affected in Ghana than in the USA and India. Otherwise, Ghanaian mobility trends follow quite closely those of Tanzania, a country with leadership known for COVID-19 denialism and the suppression of case count data.

2.1 Sample and Summary Statistics

We sampled households from the Ghana Socioeconomic Panel Survey (Ghana Panel), a nationally representative survey administered every four years since 2009 by researchers at the University of Ghana, Northwestern University, and Yale University. Our sample for the experiment was drawn from the third wave of the Panel, which surveyed 5,673 households in 2018.

[2] Ghana Health Service (GHS). (2020, March). "Ghana confirms two cases of COVID 19" Retrieved from here on 9 March 2021.

[3] Ghana Health Service (GHS). (March 2021). "SITUATION UPDATE, COVID-19 OUTBREAK IN GHANA AS AT 05 March 2021" Retrieved from here on 9 March 2021.

[4] By comparison, the excess death mortality rate estimated for the USA is roughly three times higher, at 179.3 per 100,000.

We pre-registered the experiment with the AEA Registry (#0005861).

While the Ghana Panel sample is nationally representative, the cash transfer intervention under evaluation is geared toward households facing economic difficulties, so we aimed to select the least economically prosperous households from the sample. Furthermore, we expected that epidemiological and socioeconomic characteristics would vary considerably across rural and urban regions. To select the evaluation sample, we therefore sorted rural and urban Ghana Panel households separately by a proxy for economic prosperity—per capita food expenditures using a Deaton-Zaidi (Deaton and Zaidi 2002) adult equivalence adjustment—and selected the 1,550 urban households and 1,550 rural households with the lowest food expenditure, among households that had a valid contact number.[5] We then randomized the resulting 3,100-household sample equally into treatment and control, stratifying by rural vs. urban status and fine food expenditure cells, with each strata comprising roughly 10 households. We enrolled 1,508 households from these 3,100 households.[6] The randomized assignments were programmed into the baseline survey but not shared separately with the field team.

Table 3 compares the 2018 characteristics of our experimental sample with those of several other reference groups. The first three columns present the 2018 characteristics of the full, nationally representative Ghana Panel sample. The next three columns present the characteristics of all households within the sample that meet our food expenditure eligibility criterion; this sample is representative of Ghanaian households that fall below our food expenditure threshold. The following three columns further restrict the sample to those with a valid contact number. The final three columns further restrict the sample to the 1,508 that agreed to participate in our study and had a mobile money account (i.e. our experimental sample).

[5]Households were not included in this sample if they lacked data on rural/urban status, lacked food expenditure data, or reported zero food expenditure. We also excluded 10 households that scored in the top-20% most likely to be non-poor using an IPA Probability of Poverty Index.

[6]The 1,592 non-enrolled households fall into these categories: (i) refused to participate (60 households), (ii) consented but did not have a valid MTN mobile money account (239), (iii) unable to contact respondent (959), (iv) no attempt to contact because target sample of 1,500 households was reached (308), and (v) dropped due to reporting the same mobile account number as a different household in the sample (26). Most non-enrollment was then either due to difficulty in reaching the respondent by phone, or because no contact was attempted.

Compared to the full, representative sample, households in the experimental sample are somewhat larger; gender and age of the household head are similar; and, as expected, food expenditure per adult equivalent is substantially lower (by about 60 percent). There are few meaningful differences between our experimental sample and the representative sample of low-food expenditure Ghanaians (columns 3 to 6), though as expected, they are more likely to have had cell phones and mobile money accounts in 2018.[7]

The experimental sample scores similarly to the full Ghana Panel sample in 2018 with respect to the Kessler-6 score (bottom row, Table 3). These households then exhibited a noticeable increase in distress by the current study's baseline in May to June 2020—i.e., shortly after the COVID-19 pandemic onset. In Figure A2 we track distress levels in the experimental control group from baseline to eight months after the last cash transfer. The heightened distress at baseline diminishes from mid-2020 until the end of 2020, when psychological well-being has almost recovered to 2018 levels. Distress was higher in mid-2021, as of our final phone survey, than at the end of 2020, perhaps due to a new wave of COVID-19 cases (the confirmed case rate was low at the end of 2020).

2.2 Intervention, Timing of Surveys

Our treatment group ($N = 771$) received eight mobile money transfers of 90 Ghanaian Cedis (GHC) each from June 2020 to January 2021, while our control group ($N = 737$) received only the first of these transfers.[8]

Respondents in our treatment group were told that they would receive one transfer every week, however due to logistical constraints, the transfers came less frequently (see Figure A3).

[7] Note that even within our experimental sample, as of 2018 only 84% of respondents own a cell phone and only 81% have a mobile money account. The 16% without a cell phone in 2018 could still be contacted (and enrolled) in 2020 because they left a non-household phone number by which they could be contacted. The 19% without a mobile money account in 2018 could still be enrolled in 2020 because by then they had a mobile money account.

[8] Using administrative data we confirm that by the end of the experiment, control households had each received only one transfer, while treated households had received 7.54 transfers on average.

In particular, the median gap between two adjacent transfers was 20 days, with some variation across transfers.[9] The transfers were framed as transfers from Innovations for Poverty Action to help households cope with the economic effects of coronavirus, and respondents were told that they can spend the money in any way that they want.

The 90 GHC transfer is 65% of the median household's weekly food expenditure reported at baseline. This is considerably larger than transfers from the Livelihood Empowerment Against Poverty (LEAP) program, Ghana's flagship cash transfer social protection program. LEAP provides bi-monthly cash transfers to ultra-poor and vulnerable households across Ghana, focusing on orphaned and vulnerable children, disabled adults unable to work, elderly without support and women who are pregnant or who have children aged under a year. As a result of these strict eligibility criteria, fewer than four percent of our sample are LEAP recipients. LEAP payments represent less than ten percent of average spending of food among LEAP households.[10]

We conducted the baseline and five follow-up surveys by phone. In addition to questions about various household outcomes, these surveys ended with one of three messages relaying various forms of guidance from the World Health Organization about safe pandemic practices.[11] We conducted the baseline survey between May and June 2020, just before the first transfer. We conducted the first follow-up survey (F1) in July 2020 at which time households in treatment and control groups had all received a single transfer and treatment households had been informed that they would receive additional transfers. By comparing the outcomes of households at F1 we examine whether the anticipation of future grants has an impact on household outcomes, in particular labor supply.[12]

We conducted the remaining follow-up phone surveys in August 2020 (F2), October 2020

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[9] The median gap was as low as seven days between the fourth and fifth transfers, and between the sixth and seventh, while it was as high as 55 days between the fifth and sixth transfers.

[10] The weekly value of LEAP payments vary by beneficiary, from 8 GHC for a single recipient to about 13 GHC for families with four or more recipients (paid every two months).

[11] This health messaging was not randomized. See Appendix for the scripts.

[12] Anticipation effects require trust that IPA will send the transfers as promised. While we did not measure trust directly, trust is likely high here: respondents have all been surveyed previously as part of the Ghana Panel Survey, and part of our script emphasizes the link with the Ghana Panel Survey (see the Appendix for the full script).

(F3), November and December 2020 (F4), and July and August 2021 (F5). The outcomes of households in F2 to F4 allow us to evaluate the impact of the cash grants contemporaneous to when transfers were still being made, and F5 examines the persistence of any effect eight months after the final transfer.

To understand the effective treatment at each phone follow-up, it is important to note the timing of the transfers relative to each follow-up. Figure A3 visualizes this timing, while Figure A4 shows more directly the distribution of days since the last transfer for the treatment group at the point of each follow-up phone survey. While each of F2 to F4 were intended to be conducted immediately after the previous transfer, Figure A4 shows that the days since last transfer is somewhat larger on average for F3 than for F2 and F4. This variation in survey timing may matter for treatment effects to the extent that respondents do not fully smooth consumption. Given this, we estimate effects with and without F3.

Separate from our phone surveys, the fourth wave of the Ghana Socioeconomic Panel Survey reached our experimental households from June 2022 to August 2023, or roughly two years after the final cash transfer. We use this data to estimate long-term effects on consumption, income, and depression.

Response rates to the five follow-up phone surveys are high at around 90%, with the exception of F4 which had a response rate of 75%. The response rate for the final in-person survey was 93%. Treated households respond at statistically significantly higher rates to the phone surveys, but not to the final in-person follow-up (Table A1). The differences might reflect a mixture of gratitude for the transfers and the misunderstanding that survey response is a prerequisite for continued transfers. For the most part, we do not see differential attrition by treatment (see the joint F-test p-values, Table A1).

Treatment and control households are well-balanced on baseline characteristics (columns 1 to 4, Table A2). Though treated households are more likely to respond to the follow-up phone surveys, this differential response does not create imbalance on observables – treatment and control participants are balanced on observables even when restricting only to those that answered each follow-up survey (columns 5 to 16 of Table A2, and Table A3). Nevertheless, in case of imbalance on unobservables, we also re-estimate our main contemporaneous effects under various assumptions about the outcomes of attrited households in the treatment and control groups (Table A4). With this table, the reader can find the estimates implied by the missing data assumptions they find the most plausible.

2.3 Specification

We estimate variants of the following specification throughout:

$$\mathrm{y}_{it} = \alpha_s + \beta_0 y_{i0} + \beta_1 \mathrm{Transfers}_i + \varepsilon_{it}$$

where y_{it} is outcome y for household i at follow-up t, α_s are randomization strata fixed effects, and y_{i0} is the dependent variable measured at baseline. $\mathrm{Transfers}_i$ is the key treatment variable – a dummy variable equal to one if the household was randomly assigned to treatment.

To estimate contemporaneous effects of the transfers we pool data from follow-up surveys F2, F3 and F4. In these cases we add survey wave fixed effects and cluster standard errors at the household-level. Otherwise, we estimate robust standard errors.

3 Results

We report effects measured in the five phone follow-up surveys in the next four sub-sections. We then report two-year effects on a narrower set of outcomes measured in the fourth wave of the Ghana Panel Survey.

3.1 Expenditure

We first investigate the impact of our cash transfers on food and non-food expenditure, presented in columns 1 and 2 of Table 4.

To estimate anticipation effects, we use data from the first follow-up survey. In theory, forward-looking households might increase spending upon the announcement of future cash transfers, in an attempt to smooth consumption. In practice, failures to smooth consumption are common, particularly among low-income households (Shapiro 2005; Ganong and Noel 2019; Gerard and Naritomi 2021; Augenblick et al. 2023). Consistent with this, we see no evidence of anticipation effects in Panel A – effects on food and non-food expenditure are not statistically significant, and are actually negative in the case of food expenditure. The lack of anticipation effects could reflect a lack of trust in IPA, or a failure of consumption smoothing due to credit constraints (preventing borrowing) and limited savings (preventing running down savings). We find the latter more plausible in this setting, given that (i) all households had already received one transfer by F1, and (ii) households had answered Ghana Panel Surveys in the past.

We estimate contemporaneous effects in Panels B and C. For Panel B we pool data from the three follow-up surveys (F2, F3, F4) fielded while cash transfers were ongoing. Given the issue of delayed surveys at the third follow-up (Figure A4), for Panel C we pool only the data from F2 and F4.

Households spent a large fraction of the cash transfers on food (column 1, Panels B and C). The point estimate indicates that households in the treatment group spent 12.2 GHC per week (SE: 6.7) more than those in the control group, an 8% increase over the control mean. The estimate is similar when considering only F2 and F4 (Panel C), at 11 GHC per week (SE: 7.8).

On average our transfers arrived 25 days apart from one another. Under the assumption of perfect consumption smoothing the point estimate in Panel B implies that households spent more than 40% of their transfer on food (12.2 x 25/7 = 43.6 GHC on food expenditure every 25 days). If households are not smoothing consumption (as we might expect from the lack of anticipation effects), and instead spend the cash sooner rather than later (Shapiro 2005), then 40% is a lower bound – our follow-up surveys may understate the extent of the consumption response given that they took place typically 20 to 40 days after the most recent transfer was disbursed (Figure A4).

We find no evidence of contemporaneous effects of the cash grants on non-food expenditure (column 2). However, in line with the discussion above, if effects on non-food expenditure are concentrated near the time of the transfer then our data may understate the magnitude of this expenditure response. One possibility is that food expenditure is smoothed much more than non-food expenditure – perishability of food makes it unwise to frontload food expenditures, while the opposite argument holds for durable goods that provide a stream of utility. Regardless of the non-food expenditure response, that such a large fraction of the transfer can be traced to food expenditure may be particularly reassuring from a policy perspective given the drop in food security among Ghanaian households reported in Egger et al. (2021).

We see no evidence of persistent effects on expenditure using the data from the fifth follow-up survey (Panel D). The point estimates for both food and non-food expenditure are actually negative, though imprecisely estimated and we cannot rule out positive estimates in line with the contemporaneous results in Panels B and C. Interpreting these results as nulls, we note that failures of consumption smoothing parsimoniously explain our expenditure findings: jointly rationalizing a lack of anticipation and persistence, and more tentatively rationalizing the larger effects on food than on non-food expenditure.

Throughout our analysis we focus on three primary dimensions of heterogeneity: rural or urban, male or female household head, and baseline poverty (specifically, above or below median household per capita adult-equivalent food expenditure at baseline). We include all three interactions in the same regression, such that rural/urban treatment effect heterogeneity, for example, should be considered heterogeneity by rural/urban status conditional on the gender of the household head and on baseline poverty. Table A5 presents the results. While we do not find evidence of heterogeneous impacts on spending for rural/urban households or households with above/below median food expenditure, we do find that female-headed households have a larger increase in food expenditure than male-headed households. This may reflect female-headed households' heightened vulnerability during the crisis.

3.2 Income and Labor Supply

Cash transfers in the developing world typically do not reduce working hours (Banerjee et al. 2017), despite the concerns of some policy-makers. In fact, recent evidence suggests that cash transfers may even increase work effort and income through psychological channels (Banerjee et al. 2022; Kaur et al. 2021). In the context of COVID-19, these results may not generalize – in particular, if recipients use the cash to facilitate distancing at home (as we will show evidence for below), they may be doing so by reducing work hours.

We find no support for such concerns (columns 3 to 6, Table 4). While we again find no anticipation effects (Panel A), contemporaneous effects of cash on recent total income are positive, at 29 GHC per week (SE: 19, column 3) or 20% of the control mean when pooling F2 to F4, and 47 GHC per week (SE: 25) or 33% of the control mean when pooling only F2 and F4. Part of this income effect is driven by the extensive margin of earning any income at all; the likelihood of reporting any positive income in the past week increases by 5 percentage points (SE: 2, column 4, Panel C), or 11% of the control mean. This suggests that cash actually has a *positive* effect on household labor supply.[13]

In contrast to the impact on expenditure, the impact on household income persists somewhat after the transfers end, with the albeit imprecisely estimated coefficient of 42 GHC per week (SE: 27) nearly unchanged relative to the pooled impacts during the grant disbursement period. In addition, the extensive margin effect on reporting any positive income remains at 4 percentage points (SE: 3) or 7% of the control mean.

[13]Similarly, we see no evidence of negative effects on respondent-level labor supply, which we measure in hours (column 5, Table 4). We also do not see positive effects on hours worked at home (column 6), suggesting that the social distancing effects we report below in Table 5 are driven by non-work time.

Table A6 decomposes household income into the four components from which our income variable is constructed: the number of days the household earned an income in the last week, the household income on its most recent day in which it earned money, the number of days that the household received or made a transfer to or from another household, and the net size of the most recent transfer. The contemporaneous income effect appears to be mainly driven by a 0.18 increase in the number of days a household earned an income in the last seven days (SE: 0.10), which represents about a 10% increase relative to the control mean.

Surprisingly, we also detect *positive* and statistically significant effects on the days and value of transfers received (columns 3 and 4). We do not have a clear explanation for why our intervention would increase the net transfers received by households, but we note that these are economically small effects that account for little of the positive effects on total income in Panels B and C in Table 4. For example, in Panel B of Table 4, 87% of the 28.82 impact on total income comes from increases in earned income, and only 13% from the impact on transfers.

The persistent income effect appears to be largely driven by an increase of 10.3 GHC (SE: 5.7) in the household's income the most recent day it earned money, and an increase in the frequency of transfers of 0.05 days in the last 7 (SE: 0.02), while the effect on the size of the transfers received is no longer statistically significant.

What accounts for the increase in income resulting from our transfers? While our data do not allow us to pin down a single mechanism, these results are consistent with our transfers enabling households to start new businesses and reinvigorate old ones, which could account for the increase in income on both the intensive and extensive margins.[14]

Finally, we investigate heterogeneity of impacts on income and respondent-level labor supply. As above, Table A5 explores heterogeneity by whether households are rural or urban, whether the household head is male or female, and whether the household's level of food expenditure is above or below median at baseline. Table A7 explores heterogeneity of impacts

[14]Unfortunately, we did not collect data on source of income and so cannot provide direct support for this hypothesis.

based on occupations: whether the household has a business, whether the household has a wage worker, and whether the household has a farmer. Table A7 reveals some important heterogeneity by occupation. First, households with a farmer experience a significantly smaller increase in their income in response to the grants (column 1). Second, while we do not find heterogeneity in impacts for total working hours, we find that small business owners who received our grants experience a significant increase in their at-home working hours (column 3). This suggests that our grants may have helped entrepreneurs shift to at-home production.

3.3 Social Distancing and COVID-19 Symptoms

A concern during the pandemic has been that social distancing may be near impossible for low-income households in developing countries. Without the option to work from home, social distancing may only be possible by reducing work hours. But this may not be viable for those with low savings in countries without a social safety net. Cash may then increase social distancing by reducing the need to work. Otherwise, cash may increase social distancing through more behavioral mechanisms: perhaps by increasing the cognitive bandwidth to exhibit costly prosocial behaviors like social distancing (Dean et al. 2017, Kaur et al. 2021), or through reciprocity (Falk 2007). On the other hand, if cash is used for in-person transactions, cash transfers may even reduce distancing. We test for these possibilities in Table 5.

As with expenditure, we find no evidence of anticipation effects on social distancing (Panel A), with no economically meaningful impacts on either our overall social distancing index (column 1), or its underlying components (columns 2 to 6). Unlike the expenditure case, limited borrowing and savings opportunities need not prevent anticipation effects on social distancing – households could plausibly feel less pressure to work or look for work, knowing that money is on the way, and thus social distance more. This null anticipation effect (and also the null we discuss below on psychological well-being) is then a more surprising null than that on expenditure.

Unlike the anticipation period, treatment households exhibited statistically significantly more social distancing during the transfer program. The social distancing index increased by 0.08σ (SE: 0.04) when pooling F2 to F4, and by 0.12σ (SE: 0.04) when excluding F3, suggesting that the contemporaneous impact may have been concentrated in the initial weeks after each transfer.

Looking at the individual metrics of social distancing, the impact is driven mostly by the respondents' and their households' propensity to stay at home all day. Using the estimates in Panel C, the former increased by 11% (0.24 days, SE: 0.1, column 2), while the latter increased by 9% (0.26 days, SE: 0.13, column 5). Our income results in Table 4 suggest that this effect on staying at home is not accompanied by an observed reduction in labor supply outside of the home. Rather social distancing likely increased by reducing out-of-home non-work activities – while we find an effect on attending social gatherings in the direction of increased distancing it is without statistical significance (column 3).

We do not find effects on whether respondents try to keep a distance of at least one meter from anyone outside of their immediate family (column 4), although here we are limited by ceiling effects – 95% of the control group reports trying to keep a distance. We also do not see effects on the number of days the respondent has had visitors to their home from outside of their immediate family (column 6). In this case, we might anyway expect this dimension of social distancing to be less controllable by the household receiving the transfers.

Two primary channels might drive increases in social distancing. First, respondents may reciprocate the generosity of the transfers by following social distancing guidelines more closely. Alternatively, respondents may spend less time outside looking for work, given that they have less need for cash. The concentration of effects on days spent at home, as opposed to the component related to attending social gatherings, is most consistent with the latter channel.

Given that our measures are self-reported, one concern is whether positive effects could be due in part to experimenter demand effects – with treatment households exaggerating the extent to which they are social distancing. Two facts speak against this. First, the positive effects are estimated only with the surveys contemporaneous with the transfers. If the response was due only to experimenter demand, we might also expect a positive effect during the anticipation wave and during the long-term follow-ups. Second, if experimenter demand drove the effects, we would expect some subcomponents of the social distancing index to be the most impacted — in particular, those emphasised by government directives, like attendance of social gatherings. We do not see this.

While cash transfers increase contemporaneous social distancing, the distancing is not habit-forming – the persistent impact of cash transfers on the social distancing index is only 0.03σ and not statistically significant (Panel D).

In principle, greater social distancing could curtail the spread of COVID-19. To explore this, we examine the impact of cash transfers on an index of self-reported symptoms in the final column of Table 5. The only statistically or economically significant treatment effect is an increase in reported symptoms of 0.11σ (SE: 0.05) at the time of the first follow-up. In the absence of other evidence of behavior change associated with the anticipation of future transfers, this result is perhaps a consequence of increased salience of the pandemic, or a form of social desirability bias–respondents unsure that they would continue to receive transfers could report more symptoms.

We find no evidence of heterogeneity along the three covariates tested above (Table A5). This suggests a uniformly positive treatment effect on social distancing behavior across the population.

In summary, we find that our cash transfers did induce a significant increase in social distancing behaviors, and this effect was not accompanied by a reduction of working hours or income generating activities.

3.4 Psychological Well-Being and Beliefs

There is no doubt that the pandemic has caused global psychological distress. To the extent that the distress in Ghana is driven by the economic impacts of the pandemic, we might expect cash transfers to improve psychological well-being (Haushofer and Shapiro 2016). In addition, cash transfers may substitute for existing coping mechanisms: whether motivated beliefs that COVID-19 is not particularly harmful (Bénabou and Tirole 2016; Engelmann et al. 2019), or investments in religious beliefs and practices (Sinding Bentzen 2019; Bentzen 2021). We test for these ideas in Table 6.

Transfers had neither anticipatory, contemporaneous, nor persistent effects on psychological well-being. We see this using the Kessler-6 psychological distress scale (column 1) and also with self-reported happiness (column 2).[15] Consistent with these null effects, we do not see any evidence that the cash transfers substituted for the coping mechanisms of motivated or religious beliefs (columns 3 to 5).

Specifically, we see no impact on the perceived fatality rate of COVID-19, and the mean belief is in any case far higher than the actual fatality rate.[16] Second, there is actually some evidence that transfers *reduce* the perceived impact of the pandemic on the Ghanaian economy (Panel B, column 2), perhaps because treated respondents infer from the transfers that organizations are taking action to mitigate the economic impacts of the pandemic.

Third, contemporaneous transfers actually somewhat increase the frequency of prayer (Panels B and C, column 5). Though inconsistent with the idea of prayer as a coping mechanism (Bentzen 2021), this finding is reminiscent of positive effects of income on religious participation in Ecuador (Buser 2015). In the Ecuadorian context, a positive income shock increases church attendance, but does not affect self-reported religiousness. Buser suggests that these

[15] We also do not see evidence for heterogeneous treatment effects on psychological distress (column 5, Table A5).

[16] As of April 14, 2022, Ghana has had 161,086 confirmed COVID-19 cases, and only 1,445 confirmed COVID-19 deaths (see https://covid19.who.int/region/afro/country/gh). If cases are under-reported more than deaths, this places an upper bound on the fatality rate of 0.9%.

results are consistent with Evangelical churches being social clubs where participation is costly. Since prayer is costless, our findings cannot easily be rationalized by the same story.

3.5 Two-Year Impacts

The fourth wave of the Ghana Panel Survey was fielded from August 2022 to June 2023, or roughly two years after the final transfer had been disbursed to our experimental sample. We use this survey wave to explore long-term effects of the cash transfers on consumption, income, and depression symptoms in Table 7. Effects are not statistically significant, with the exception of the estimated effect on consumption, which is actually negative, roughly 6% of the control mean, and statistically significant at the 10% level. This effect is driven mainly by health expenditures (second row, Table 7), which we explain further below. While consumption drops, effects on income are positive but not statistically significant (rows 4 and 5), suggesting that treated households save a higher fraction of their income.[17]

To unpack the surprising negative effect on consumption, we estimate effects on the underlying components of consumption in Table A8. A drop in health expenditures drives roughly half of the drop in overall consumption expenditure (Panel A), with this drop in turn driven by a reduction in health expenses for illness (Panel B), as opposed to spending on preventative care or injury, or unclassified health spending. A consumption reduction driven by health spending related to illness is difficult to conclusively interpret. One possible interpretation would be that the cash transfers reduce long-term illness, perhaps by greater social distancing reducing the contraction of COVID-19 or other contagious diseases, leading to lower spending. However, the delayed timing of the effect, and the null effects we estimate on contemporaneous COVID-19-related symptoms above, make this interpretation speculative.

[17] We note that the control mean income in the two-year-later wave four data (Table 7) is almost double the level in the eight-months-later phone survey (Table 4). While part of the difference may be due to rising incomes over the 16 months between the two, we expect that much of the difference is due to the differences in how income is measured across the two surveys – with a much more comprehensive module in the Ghana Panel Survey than in our follow-up phone surveys.

Taken together, we do not see strong evidence of long-term effects of the transfers.

4 Discussion

Our results highlight the promise of cash assistance, delivered over mobile money as a form of economic relief during future pandemics and perhaps other crises. We provided cash transfers to a representative sample of low-income Ghanaians with a mobile money account. The transfers were 90 GHC, and were delivered about once every three weeks with some variation in timing due to logistical constraints. Despite the unpredictability of their timing, these transfers eased food insecurity. About 40% of the value of transfers were spent on food, and households who received our transfers had about 8% higher food expenditure on average.

Our transfers also improved a social distancing index by 0.08 standard deviations. This effect was largely driven by a reduction in leisure time outside of the home, rather than a reduction in livelihood generating activities. Moreover, we do not see a tradeoff between improved distancing and income in this setting. In fact, transfers had large, though noisily estimated, positive effects on income.

While we cannot definitively pin down a single mechanism that drives our findings, the results may all follow from transfers having eased the household budget constraint. Households that no longer had as much concern about food security had more flexibility (and mental bandwidth) to adhere to social distancing guidelines. And households may have spent part of the transfer on starting new businesses and reinvigorating old ones, explaining the increase in incomes on both the intensive and extensive margins.

Regardless of the mechanism, our results suggest that cash relief during a pandemic can promote adherence to public health protocols while bolstering the economic well-being of recipients. Mobile money transfers are also highly scalable, especially in Ghana where mobile phone coverage is high and growing, with 0.86 mobile money accounts per adult in 2021 (Andersson-Manjang 2021). While recent evidence suggests that cash transfers at scale have large multiplier effects with minimal effects on inflation (Egger et al. 2022), an open question is whether cash transfers at scale during a pandemic would have a similar effect. Given the greater likelihood of supply-side bottlenecks, inflationary pressure may be higher than during regular times. We leave this question to future work.

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Table 1: Results From COVID-19 Cash Transfer Experiments in Other Contexts

Paper	Experiment Design	Food Expenditure	Non-Food Expenditure	Social Distancing	Income	Labor Supply	Subjective Wellbeing	Follow-up (Months)
Published								
Aggarwal et al. (2022)	Liberia & Malawi; Mid-sized villages from 6 districts; $250; 1x, 2x, or 3x; monthly or quarterly.	+*						7
Brooks et al. (2022)	Kenya; Female, urban microentrepreneurs; $46; 1x.	+**	+***	+***	+***	+**		3
Jacob et al. (2022)	US; HHs from zip codes with poverty rates > 35%; $1000, 1x.	n.s.			n.s.	n.s.	n.s.	2
Londoño-Vélez and Querubin (2022)	Colombia; Welfare recipients outside 25% poorest municipalities; $19; 3x; every 5 to 8 weeks.	n.s.	n.s.	−***		n.s.	n.s.	1
McKelway et al. (2023)	India; Age 55+, living alone; $13; 1x.	n.s.	n.s.				+*	3
Pilkauskas et al. (2023)	US; SNAP recipients; $1000; 1x.	n.s.	n.s.			n.s.	n.s.	3
Stein et al. (2022)	Uganda; Refugee settlement; $1000; 1x.	n.s.		n.s.			+***	5
Working Papers								
Aiken et al. (2023)	Togo; Poorest 100 cantons; $15.50 for women; $13.50 for men; 6x; monthly.	+***				n.s.	+***	2
Banerjee et al. (2020)	Kenya; Villages in 2 poor counties; $22.50 per adult; monthly; 24x (ST) or 144x (LT), or $550 per adult; 1x (LS).	LT: +*** ST: +** LS: +***		LT: n.s. ST: +** LS: +**	LT: n.s. ST: n.s. LS: n.s.	LT: n.s. ST: −* LS: n.s.	LT: +*** ST: +*** LS: n.s.	26

Notes: All transfer numbers are in nominal USD. Some papers measured food insecurity rather than food expenditure. In these cases, a decrease in food insecurity was interpreted as an increase in food expenditure. Outcomes in Brooks et al. (2022) are measured at the business-level. Follow-up (Months) is the number of months between the last cash transfer and the last outcome measurement (8 for our study). The number for Banerjee et al. (2020) is an outlier given that the cash transfers were made pre-pandemic. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Other Pre-registered COVID-19 Cash Transfer Experiments

Authors	Country	Sample	Transfers in USD
Alatas et al. (2021)	Indonesia	Pre-existing vocational training and cash transfer study	Not specified
Badolo et al. (2021)	Burkina Faso	Not specified (overlaid on pre-existing pollution study)	Varied by hh size; intended to cover masks and soap
Bertrand and Hallberg (2021)	USA	Low income in Chicago; facing hardship; applied for $	$1000; 1x
Bird and Freier (2020)	Peru	Venezuelan migrants residing in Peru	Not specified
Carney et al. (2021)	India	Low income; Tamil Nadu	$65; 1x
García et al. (2021)	USA	Rural South Carolina; Near poverty line; affiliated with church org	$200; 24x; monthly

Table 3: Summary Statistics From the 2018 Ghana Panel Survey

	All			Food Expenditure Eligible			Has Phone Number			Experiment Sample		
	Mean (1)	SD (2)	N (3)	Mean (4)	SD (5)	N (6)	Mean (7)	SD (8)	N (9)	Mean (10)	SD (11)	N (12)
Household Size	3.36	2.26	5,654	3.81	2.36	3,371	3.86	2.36	3,097	4.09	2.42	1,508
Household Head Female	0.39	0.49	5,654	0.38	0.48	3,371	0.37	0.48	3,097	0.35	0.48	1,508
Household Head Age	49.90	17.23	5,654	51.16	17.42	3,371	50.52	17.02	3,097	49.75	15.94	1,508
Monthly Food Exp. p.c. (GHC)	226.14	213.68	5,632	130.72	69.44	3,374	131.53	68.95	3,100	132.17	68.69	1,508
Lives in Urban Community	0.42	0.49	5,656	0.48	0.50	3,374	0.50	0.50	3,100	0.57	0.50	1,508
HH Has Wage Earner	0.22	0.41	5,673	0.20	0.40	3,374	0.21	0.41	3,100	0.24	0.43	1,508
HH Has Business	0.40	0.49	5,673	0.39	0.49	3,374	0.41	0.49	3,100	0.46	0.50	1,508
HH Has Farmer	0.52	0.50	5,673	0.54	0.50	3,374	0.53	0.50	3,100	0.49	0.50	1,508
HH Has Cellphone	0.76	0.43	5,673	0.76	0.43	3,374	0.79	0.41	3,100	0.84	0.37	1,508
Any Mobile Money Account	0.71	0.46	5,673	0.68	0.47	3,374	0.71	0.45	3,100	0.81	0.39	1,508
MTN Mobile Money Account	0.59	0.49	5,673	0.58	0.49	3,374	0.61	0.49	3,100	0.74	0.44	1,508
HH Kessler Sum	10.75	3.70	5,615	10.56	3.65	3,349	10.50	3.60	3,075	10.31	3.54	1,496

Notes: Columns 1 to 3 show data for all households in Wave 3 (2018) of the Ghana Panel Survey. Columns 4 to 6 show the Ghana Panel Survey data only for those households eligible for the cash transfers experiment based on their food expenditure per adult equivalent capita. Columns 7 to 9 drop those without any cell phone number reported in the Ghana Panel Survey, leaving us with the 3,100 households we attempted to enroll in the experiment. Columns 10 to 12 include the 1,508 households successfully enrolled in the experiment. Monthly Food Exp. p.c. is monthly food expenditure per adult equivalent capita (in Ghanaian Cedis), using a Deaton-Zaidi adult equivalent adjustment. Kessler scores from the Ghana Panel Survey data are household-level averages, since multiple members for some Ghana Panel households were asked the Kessler scale questions. The Kessler-6 index asks respondents *During the past 7 days, about how often did you feel ...* for six different versions (nervous / hopeless / restless or fidgety / that everything was an effort / so sad that nothing could cheer you up / worthless). Responses are 1=None of the time, 2=A little of the time, 3=Some of the time, 4=Most of the time, or 5=All of the time. Higher scores indicate a higher likelihood of distress. The HH Kessler sum adds the six components. The maximum score for the sum would be 30, i.e., if someone answers *All of the time* to all six questions. The baseline survey average of the Kessler-6 index in the experimental sample of 1,508 respondents is 12.93 (SD = 4.24).

Table 4: Impacts on Expenditure, Income, and Labor Supply

	Expenditure (7 days, GHC)		Income (7 days, GHC)		Working Hours (7 days)	
	Food	Non-Food	Total	Any	All	Home
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:	Anticipation: Before Treatment-Only Transfers (F1)					
Treatment	-8.07	0.70	-15.89	0.01	-0.13	0.13
	(13.19)	(7.35)	(18.49)	(0.03)	(1.20)	(0.64)
Observations	1,391	1,383	1,274	1,386	1,435	1,433
Control Mean	209	51	155	.51	21	3.7
Control SD	288	127	334	.5	26	13
Panel B:	Contemporaneous: Between 3rd and Last Transfer (F2-F4)					
Treatment	12.19*	-3.17	28.82	0.04**	1.05	0.19
	(6.70)	(2.84)	(19.05)	(0.02)	(0.89)	(0.46)
Observations	3,711	3,709	3,388	3,709	3,825	3,819
Households	1,427	1,422	1,339	1,422	1,458	1,456
Control Mean	147	32	147	.45	20	3.1
Control SD	167	89	520	.5	23	11
Panel C:	Contemporaneous: Between 3rd and Last Transfer (F2, F4)					
Treatment	10.97	0.33	47.25*	0.05**	1.02	-0.10
	(7.79)	(2.82)	(25.14)	(0.02)	(0.97)	(0.49)
Observations	2,429	2,422	2,220	2,427	2,501	2,497
Households	1,397	1,391	1,302	1,391	1,431	1,429
Control Mean	147	28	141	.45	19	3.3
Control SD	171	65	501	.5	23	12
Panel D:	Persistence: 8 Months After Last Transfer (F5)					
Treatment	-20.94	-9.05	41.65	0.04	-0.58	1.21*
	(21.08)	(10.06)	(27.23)	(0.03)	(1.44)	(0.71)
Observations	1,293	1,296	1,101	1,281	1,349	1,348
Control Mean	257	73	202	.59	26	2.7
Control SD	377	156	395	.49	26	11

Notes: All regressions are OLS and include strata fixed effects and the baseline-measured dependent variable. Standard errors are robust (Panels A and D) or clustered at the household-level (Panels B and C). Panels B and C additionally include survey wave fixed effects. F1 denotes the first phone follow-up survey. Treatment is a dummy variable equal to one if the household was randomly assigned to receive the full set of mobile money transfers. Food (non-food) expenditure is the number of days the household purchased food (non-food) items over the last 7 days multiplied by the top-1% winsorized amount (in Ghanaian Cedis) spent on food (non-food) on the most recent day food (non-food) was purchased. Total Income is the sum of earned income and transfers in Ghanaian Cedis. Earned income is measured as the number of days the household earned income over the past 7 days multiplied by the (top-1% winsorized) household income earned on the most recent day that it was earned. Transfers are measured as the number of days the household received transfers over the past 7 days multiplied by the (top-1% winsorized) total value of transfers on the most recent day they were received. Any Income is a dummy variable equal to one if the number of days the household earned income over the past 7 days is greater than zero. All working hours is the number of days the respondent worked for income over the last 7 days multiplied by the number of hours worked on the most recent working day, and this number is then winsorized at the top-1%. Home working hours is the number of days the respondent worked for income over the last 7 days multiplied by the number of hours worked from home on the most recent working day, and this number is then winsorized at the top-1%. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Impacts on Social Distancing and COVID-19 Symptoms

	Social Distancing						Symptoms
	Index	Days At Home	Days Social Gatherings (-)	Keep Distance	Days HH Home	Days Visitors (-)	Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:	Anticipation: Before Treatment-Only Transfers (F1)						
Treatment	0.03	0.04	0.05	0.01	0.10	-0.04	0.11**
	(0.05)	(0.13)	(0.07)	(0.01)	(0.16)	(0.08)	(0.05)
Observations	1,425	1,438	1,438	1,438	1,428	1,434	1,438
Control Mean	-.0027	2.4	-.79	.94	3.5	-.68	-.057
Control SD	.99	2.4	1.4	.24	3.1	1.5	.88
Panel B:	Contemporaneous: Between 3rd and Last Transfer (F2-F4)						
Treatment	0.08*	0.18*	0.04	0.00	0.15	-0.02	0.00
	(0.04)	(0.09)	(0.06)	(0.01)	(0.12)	(0.06)	(0.04)
Observations	3,782	3,831	3,831	3,831	3,792	3,814	3,831
Households	1,453	1,460	1,460	1,460	1,455	1,457	1,460
Control Mean	-.042	2.1	-1.1	.95	2.8	-.6	-.00069
Control SD	.97	2.3	1.4	.22	2.9	1.4	1
Panel C:	Contemporaneous: Between 3rd and Last Transfer (F2, F4)						
Treatment	0.12***	0.24**	0.06	0.00	0.26**	-0.02	-0.01
	(0.04)	(0.10)	(0.07)	(0.01)	(0.13)	(0.07)	(0.04)
Observations	2,475	2,505	2,505	2,505	2,482	2,493	2,505
Households	1,425	1,432	1,432	1,432	1,427	1,429	1,432
Control Mean	-.052	2.1	-1.1	.95	2.9	-.61	.0078
Control SD	.98	2.3	1.4	.23	3	1.5	1.1
Panel D:	Persistence: 8 Months After Last Transfer (F5)						
Treatment	0.03	0.23*	0.01	0.02	-0.12	-0.12	0.00
	(0.06)	(0.14)	(0.10)	(0.02)	(0.13)	(0.10)	(0.06)
Observations	1,332	1,352	1,353	1,353	1,337	1,347	1,353
Control Mean	.0015	2	-1.5	.92	1.7	-.71	.0074
Control SD	.99	2.3	1.8	.27	2.3	1.5	1

Notes: All regressions are OLS and include strata fixed effects and the baseline-measured dependent variable. Standard errors are robust (Panels A and D) or clustered at the household-level (Panels B and C). Panels B and C additionally include survey wave fixed effects. F1 denotes the first phone follow-up survey. The outcome variables are: (1) the standardized first principal component of the five outcomes in columns 2 to 6, (2) number of days the respondent spent at home all day out of the past 7, (3) -1*number of days the respondent attended social gatherings out of the past 7, (4) dummy variable for trying to keep a distance of at least one meter from non-family members, (5) number of days other members of respondent's household stayed at home all day out of the past 7, (6) -1*number of days with non-family visitors to the respondent's home out of the past 7, (7) the standardized first principal component of ten binary measures of COVID-19 symptoms: five symptoms (fever, dry cough, difficulty breathing, lost sense of taste, sought medical treatment) asked both of the respondent and the respondent's household. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Impacts on Beliefs and Well-Being

	Psychological Well-Being		COVID-19 Beliefs		Religiosity
	Depression Index (-)	Happiness	Fatality Rate	Effect On Economy	Prayer Frequency
	(1)	(2)	(3)	(4)	(5)
Panel A:	Anticipation: Before Treatment-Only Transfers (F1)				
Treatment	-0.31	0.04	-0.21	-0.05	0.01
	(0.21)	(0.04)	(1.11)	(0.03)	(0.02)
Observations	1,438	1,438	1,218	1,438	1,438
Control Mean	-12	-3.3	14	3.8	4
Control SD	4.3	.82	22	.53	.5
Panel B:	Contemporaneous: Between 3rd and Last Transfer (F2-F4)				
Treatment	0.12	0.04	-0.12	-0.07***	0.04**
	(0.17)	(0.03)	(0.74)	(0.03)	(0.02)
Observations	3,831	3,831	3,311	3,831	3,831
Households	1,460	1,460	1,301	1,460	1,460
Control Mean	-12	-3.1	11	3.6	4
Control SD	4.5	.86	18	.64	.47
Panel C:	Contemporaneous: Between 3rd and Last Transfer (F2, F4)				
Treatment	0.07	0.04	-0.06	-0.04	0.04**
	(0.18)	(0.04)	(0.81)	(0.03)	(0.02)
Observations	2,505	2,505	2,166	2,505	2,505
Households	1,432	1,432	1,267	1,432	1,432
Control Mean	-11	-3.1	11	3.6	4
Control SD	4.3	.85	19	.61	.48
Panel D:	Persistence: 8 Months After Last Transfer (F5)				
Treatment	-0.01	0.03	-1.31	0.00	0.03
	(0.26)	(0.05)	(1.48)	(0.04)	(0.05)
Observations	1,353	1,353	1,169	1,353	1,328
Control Mean	-12	-2.8	18	3.6	-3.3
Control SD	4.4	.95	24	.69	.79

Notes: All regressions are OLS and include strata fixed effects and the baseline-measured dependent variable. Standard errors are robust (Panels A and D) or clustered at the household-level (Panels B and C). Panels B and C additionally include survey wave fixed effects. F1 denotes the first phone follow-up survey. The survey questions for each column are: (1) Kessler-6 Depression Index (reverse-coded): the sum of answers to six questions like During the past 7 days, about how often did you feel hopeless? (1 = None of the time, 2 = A little of the time, 3 = Some of the time, 4 = Most of the time, 5 = All of the time), (2) Taking all things together, would you say you are... (1 = Very happy, 2 = Rather happy, 3 = Not very happy, 4 = Not at all happy) (reverse-coded), (3) If 100 people were infected with the coronavirus, how many do you think would die? (0 to 100), (4) How severely do you think that the coronavirus will affect the Ghanaian economy? (1 = Not at all, 2 = A little bit, 3 = Moderately so, 4 = Extremely so), (5) During the past 7 days, about how often did you pray? (1 = I didn't pray, 2 = I prayed, but less than once a day, 3 = Once a day, 4 = Several (2-5) times a day, 5 = Many (6+) times a day). *** p<0.01, ** p<0.05, * p<0.1.

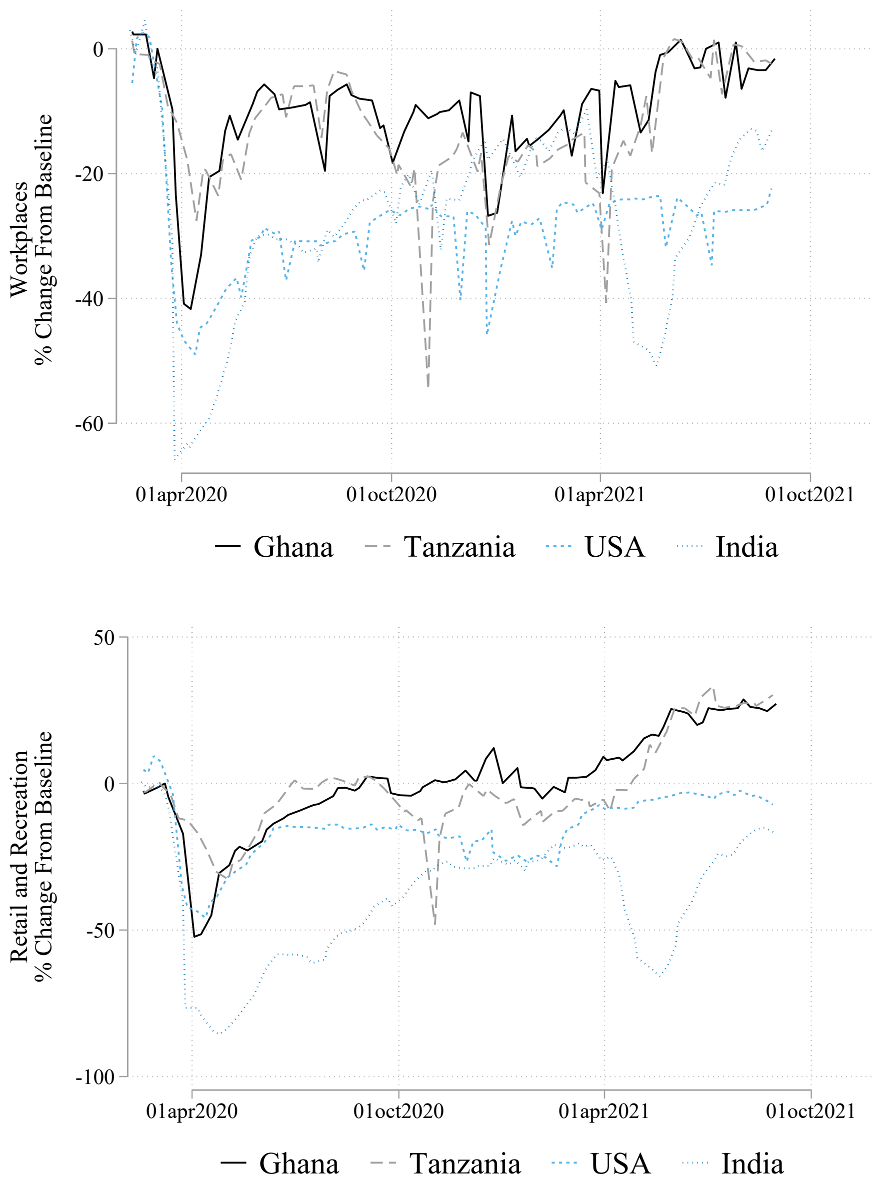
Table 7: Two-Year Impacts of COVID-19 Cash Transfers

	Treatment Effects (SE) (1)	Control Mean (2)	Control SD (3)	Observations (4)
Consumption (weekly, GHC)	-29.44*	524.38	340.51	1,347
	(17.55)			
Health Expenditures	-13.60***	40.60	92.81	1,347
	(4.53)			
Food Consumption (weekly, GHC)	1.71	251.80	164.29	1,347
	(9.09)			
Income Aggregate (weekly, GHC)	30.12	407.85	1121.87	1,347
	(70.68)			
Earned Income (weekly, GHC)	30.83	377.94	1119.83	1,347
	(70.75)			
Transfers (weekly, GHC)	-0.96	29.91	61.11	1,347
	(3.40)			
Kessler 10 Depression (-)	-0.56	-17.29	5.93	1,323
	(0.35)			
Kessler 6 Depression (-)	-0.33	-10.54	3.81	1,323
	(0.22)			

Notes: The regressions estimate long-term effects of the cash transfer treatment on outcomes measured in the fourth wave of the Ghana Panel Survey (roughly two years after the final transfer). All regressions include strata fixed effects. Each regression controls for lagged dependent variables (or closest equivalents) from Wave 3 and from the baseline survey for the cash drop experiment of the Ghana Panel Survey. The outcomes for rows 1, and 3 to 6 are winsorized at the top 1%. The outcomes for rows 4 and 5 are also winsorized at the bottom 1%, given the possibility of large negative outliers. Consumption is total weekly household consumption in Ghanaian Cedis. Health Expenditures is total weekly household health expenses in Ghanaian Cedis. Food Consumption is total weekly household food consumption valued in Ghanaian Cedis (including food purchased, produced, or received as a gift). Income Aggregate is the sum of weekly earned and transfer household income in Ghanaian Cedis. Earned Income is household weekly earned income (including income from main and secondary employment, non-farm businesses, crop sales, gathering, and animals). Transfers is weekly household transfers of income received from persons and organizations outside of the household. Kessler 10 is a depression score summed across 10 symptoms, reverse-coded such that more depressed individuals have lower scores. The score is measured at the individual-level and then averaged to give the household-level score. The Kessler 6 is the same, but includes only the sum over 6 symptoms, paralleling the measure in our cash drop endline surveys. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

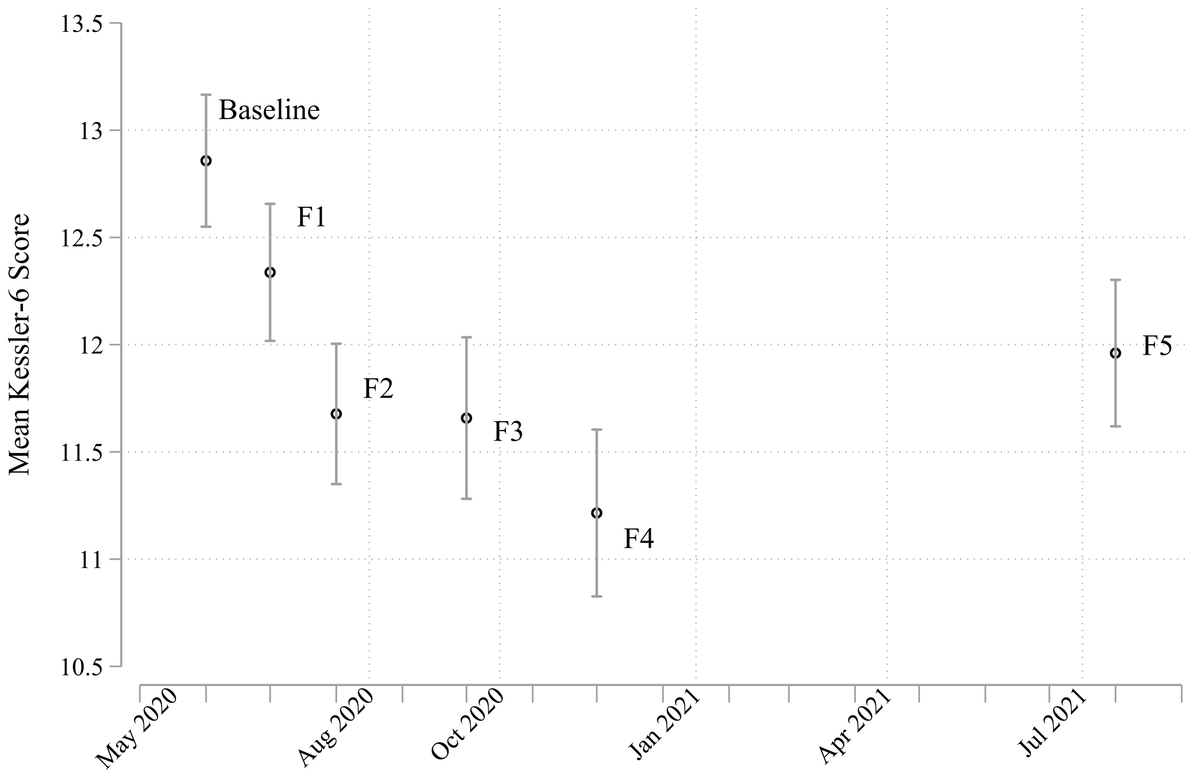
Appendix

Figure A1: Google Mobility Trends



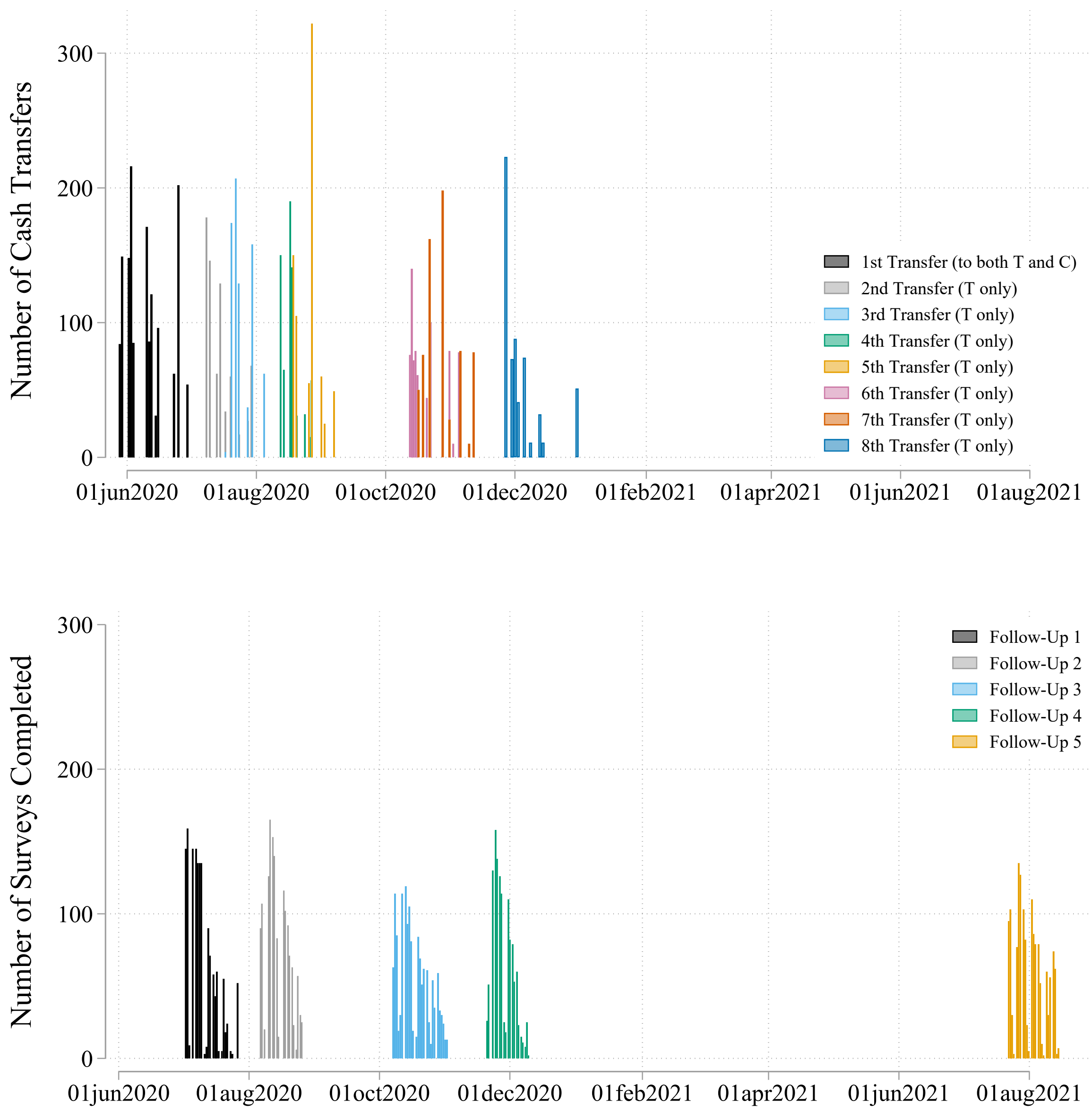
Notes: The figure visualizes COVID-19 Community Mobility Reports from Google (https://www.google.com/covid19/mobility/), collapsed to the weekly-level. The data is based on GPS-linked data collected through the use of Google Maps. Google aggregates the data to show percentage changes in activity across six categories: retail and recreation, grocery and pharmacy, parks, transit stations, workplaces, and residential. The figure shows trends for the retail and workplaces categories.

Figure A2: Control Group Trends in Psychological Distress



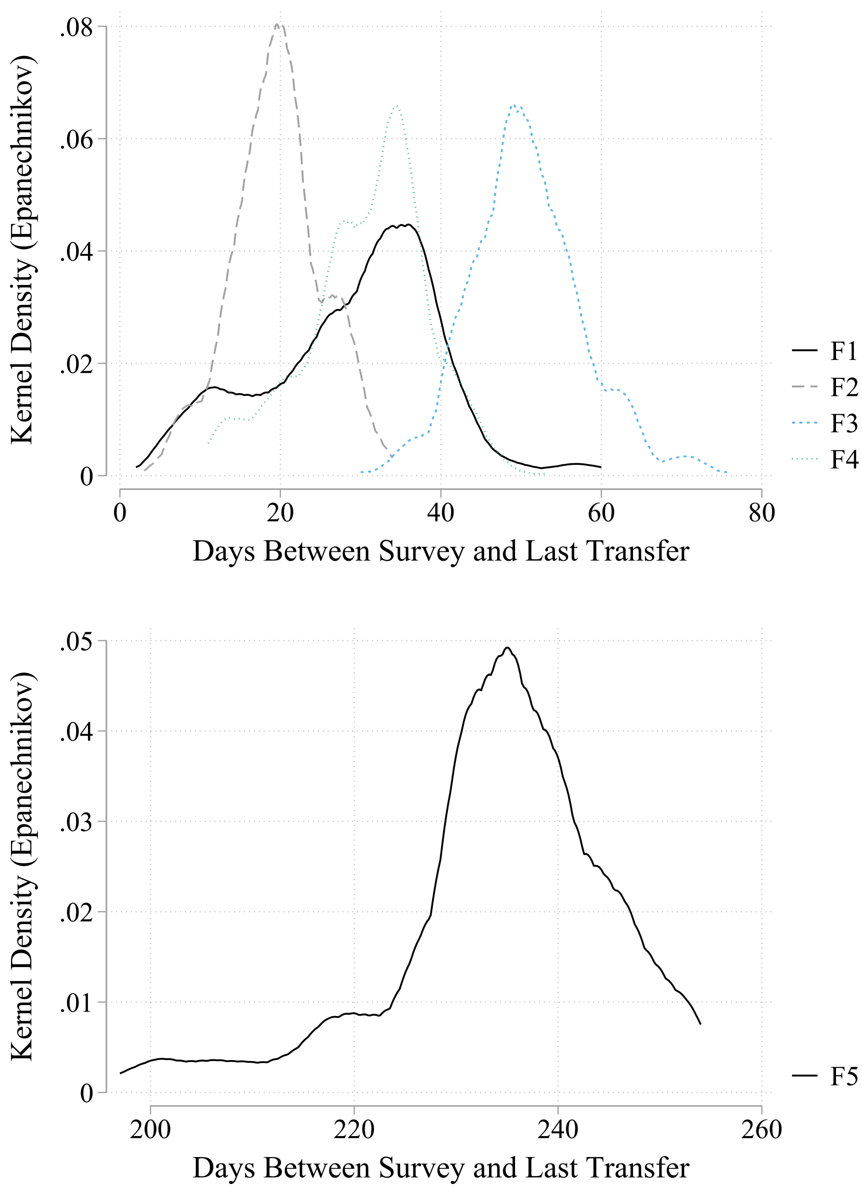
Notes: The figure visualizes the average Kessler-6 psychological distress score (higher = more distressed) and 95% confidence interval in the control group at the point of the baseline survey and each subsequent phone follow-up survey. The score is the sum of answers to six questions like “During the past 7 days, about how often did you feel hopeless?” (1 = None of the time, 2 = A little of the time, 3 = Some of the time, 4 = Most of the time, 5 = All of the time).

Figure A3: Timing of Phone Surveys and Transfers



Notes: The top panel shows the timing of the cash transfers to recipients. The first transfer was made to both treatment and control recipients during June 2020. Subsequent transfers were made only to treatment recipients. The bottom panel shows the timing of the five phone follow-up surveys. The first follow-up survey was timed to be after the treatment was announced and the first transfer received, but before any subsequent transfers.

Figure A4: Days Elapsed Between Transfers and Follow-up Phone Surveys



Notes: The figure visualizes the number of days between a respondent taking a follow-up phone survey relative to the date they last received a cash transfer. The top panel shows kernel densities for the first four follow-up surveys, the bottom panel shows the same for the fifth follow-up survey.

Table A1: Analysis of Attrition

	Whether Responded to Follow-Up Survey (=0/1)					
	F1 July 2020 (1)	F2 Aug 2020 (2)	F3 Oct 2020 (3)	F4 Nov-Dec 2020 (4)	F5 Jul-Aug 2021 (5)	F6 Aug 2022- Jun 2023 (6)
Panel A:						
Treatment	0.02*	0.10***	0.13***	0.14***	0.07***	-0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Observations	1,508	1,508	1,508	1,508	1,508	1,508
Rand. Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B:						
Treatment	0.017	0.094***	0.127***	0.145***	0.069***	-0.007
	(0.011)	(0.015)	(0.018)	(0.023)	(0.017)	(0.014)
Treatment × Food Expenditure	-0.000	0.000***	0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment × Non-Food Expenditure	-0.000	-0.000	-0.000	-0.000*	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment × Transfers	-0.001	-0.001**	-0.000	0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × Social Distancing Index	0.000	0.005	0.028	0.005	-0.005	0.033**
	(0.014)	(0.018)	(0.020)	(0.026)	(0.018)	(0.016)
Treatment × Symptoms Index	-0.016	-0.011	-0.004	-0.042*	-0.018	0.002
	(0.011)	(0.016)	(0.019)	(0.024)	(0.017)	(0.013)
Treatment × Total Income	-0.000	-0.000	-0.000	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment × Any Income	0.008	0.023	0.034	0.005	0.006	-0.020
	(0.028)	(0.037)	(0.044)	(0.057)	(0.037)	(0.031)
Treatment × Depression Index (-)	0.002	0.004	0.007	0.014**	-0.002	0.002
	(0.003)	(0.004)	(0.005)	(0.006)	(0.004)	(0.003)
Treatment × Happiness	0.005	0.058***	0.085***	-0.017	0.024	-0.007
	(0.017)	(0.021)	(0.026)	(0.034)	(0.024)	(0.018)
Treatment × Belief: Fatality Rate	0.000	-0.001	-0.001	-0.001	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treatment × Belief: Effect On Economy	-0.003	0.065**	-0.001	-0.032	0.035	-0.024
	(0.023)	(0.032)	(0.039)	(0.048)	(0.035)	(0.028)
Treatment × Prayer Frequency	0.012	-0.032	0.021	-0.031	0.022	-0.006
	(0.023)	(0.029)	(0.032)	(0.049)	(0.028)	(0.022)
Observations	1,508	1,508	1,508	1,508	1,508	1,508
Control Mean	0.94	0.86	0.81	0.68	0.86	0.93
Uninteracted Covariates	Yes	Yes	Yes	Yes	Yes	Yes
p-val Joint F-Test	0.67	0.03	0.10	0.27	0.82	0.86
Rand. Strata FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Treatment is a dummy variable equal to one if the household was randomly assigned to receive the full set of mobile money transfers. Regressions are OLS and standard errors are robust. In panel B, the 12 covariates from Food Expenditure and Prayer Frequency are set to zero when missing (7 of 12 are sometimes missing), and missingness dummies are included. These 12 covariates and 7 missingness dummies are then demeaned. The regression includes interactions between the Treatment dummy and each of the 12 demeaned covariates (coefficients shown) and their demeaned missingness dummies (not shown). The joint F-test tests for joint significance of 12 covariates and their missingness dummies interacted with treatment. *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Baseline Balance (Part 1 of 2)

	Answered Baseline				Answered Follow-Up 1				Answered Follow-Up 2				Answered Follow-Up 3			
	Treat Mean (1)	Control Mean (2)	p-value (3)	Obs (4)	Treat Mean (5)	Control Mean (6)	p-value (7)	Obs (8)	Treat Mean (9)	Control Mean (10)	p-value (11)	Obs (12)	Treat Mean (13)	Control Mean (14)	p-value (15)	Obs (16)
Food Expenditures	217.81	208.85	0.32	1477	219.16	212.88	0.42	1408	221.37	204.52	0.19	1345	217.81	208.85	0.32	1477
Non-Food Expenditures	72.62	57.18	0.40	1472	74.17	58.22	0.40	1402	75.01	58.62	0.44	1341	72.62	57.18	0.40	1472
Transfers	4.23	5.66	0.30	1498	3.95	5.77	0.25	1429	3.95	6.45	0.10	1363	4.23	5.66	0.30	1498
Social Distancing Index	0.00	-0.00	0.92	1500	-0.00	-0.01	0.92	1430	-0.01	-0.02	0.88	1367	0.00	-0.00	0.92	1500
Symptoms Index	-0.01	0.01	0.93	1508	-0.01	0.02	0.80	1438	0.00	0.03	0.90	1373	-0.01	0.01	0.93	1508
Total Income	199.42	190.70	0.55	1401	200.98	193.58	0.42	1337	199.44	193.24	0.72	1275	199.42	190.70	0.55	1401
Any Income	0.54	0.52	0.70	1469	0.54	0.52	0.53	1403	0.53	0.51	0.67	1337	0.54	0.52	0.70	1469
Depression Index (-)	-13.01	-12.86	0.54	1508	-13.04	-12.95	0.66	1438	-12.99	-12.93	0.74	1373	-13.01	-12.86	0.54	1508
Happiness	-3.31	-3.27	0.10	1508	-3.31	-3.27	0.08	1438	-3.31	-3.30	0.32	1373	-3.31	-3.27	0.10	1508
Fatality Rate	13.64	14.62	0.67	1361	13.53	14.60	0.64	1295	13.71	15.18	0.48	1245	13.64	14.62	0.67	1361
Effect on Economy	3.77	3.79	1.00	1508	3.77	3.78	0.96	1438	3.78	3.78	0.77	1373	3.77	3.79	1.00	1508
Prayer Frequency	3.95	3.94	0.84	1508	3.95	3.94	0.68	1438	3.94	3.95	0.80	1373	3.95	3.94	0.84	1508
p-val joint F-Test			0.93				0.88				0.84				0.93	

Notes: All regressions use household-level data. All expenditures and income related variables are weekly and in Ghanaian cedis. The table shows treatment and control means for 12 baseline covariates for the full baseline sample (columns 1 and 2), for those that answered the first follow-up survey (columns 5 and 6), the second (columns 9 and 10), and the third (columns 13 and 14). Each p-value is from a regression of the baseline covariate on the treatment dummy and randomization strata fixed effects, with robust standard errors, keeping only the relevant sample (e.g. in column 7, keeping only those that responded to the first follow-up). The joint F-test p-value comes from a regression of treatment on the 12 baseline variables, dummies for missing, strata fixed effects, with robust standard errors. The joint test is for the significance of the 12 baseline variables (not including the missingness dummies). See main tables for outcome variable definitions. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Baseline Balance (Part 2 of 2)

	Answered Follow-Up 4				Answered Follow-Up 5				Answered Follow-Up 6			
	Treat Mean (1)	Control Mean (2)	p-value (3)	Obs (4)	Treat Mean (5)	Control Mean (6)	p-value (7)	Obs (8)	Treat Mean (9)	Control Mean (10)	p-value (11)	Obs (12)
Food Expenditures	193.44	193.98	0.55	1108	218.23	215.33	0.53	1326	223.43	211.57	0.28	1382
Non-Food Expenditures	62.44	61.68	0.65	1105	72.44	56.82	0.54	1323	73.03	56.64	0.44	1373
Transfers	4.42	6.04	0.57	1125	4.32	6.21	0.22	1344	3.81	5.47	0.25	1398
Social Distancing Index	0.00	-0.01	0.68	1125	-0.01	-0.00	0.92	1347	0.01	-0.02	0.70	1400
Symptoms Index	-0.05	0.06	0.08	1132	-0.01	0.01	0.96	1353	0.00	0.01	0.95	1408
Total Income	171.86	172.44	0.98	1048	198.54	194.84	0.61	1258	206.57	192.49	0.37	1309
Any Income	0.53	0.51	0.74	1101	0.53	0.52	0.76	1318	0.54	0.52	0.60	1374
Depression Index (-)	-12.95	-13.27	0.18	1132	-13.07	-12.90	0.72	1353	-13.08	-12.90	0.36	1408
Happiness	-3.34	-3.32	0.32	1132	-3.32	-3.29	0.17	1353	-3.31	-3.27	0.21	1408
Fatality Rate	13.46	15.67	0.21	1026	13.86	14.92	0.77	1227	13.51	14.65	0.64	1272
Effect on Economy	3.77	3.81	0.61	1132	3.77	3.78	0.88	1353	3.77	3.79	1.00	1408
Prayer Frequency	3.95	3.96	0.55	1132	2.05	2.06	0.75	1353	3.94	3.94	0.90	1408
p-val joint F-Test			0.72				0.98				0.92	

Notes: All regressions use household-level data. All expenditures and income related variables are weekly and in Ghanaian cedis. The table shows treatment and control means for 12 baseline covariates for those that answered the fourth follow-up (columns 1 and 2), for those that answered the fifth follow-up survey (columns 5 and 6), and for those that answered the sixth, which is the fourth wave of the Ghana Panel Survey (columns 9 and 10). Each p-value is from a regression of the baseline covariate on the treatment dummy and randomization strata fixed effects, with robust standard errors, keeping only the relevant sample (e.g. in column 7, keeping only those that responded to the fifth follow-up). The joint F-test p-value comes from a regression of treatment on the 12 baseline variables, dummies for missing, strata fixed effects, with robust standard errors. The joint test is for the significance of the 12 baseline variables (not including the missingness dummies). See main tables for outcome variable definitions. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4: Contemporaneous Treatment Effects Under Varying Missing Data Assumptions

	Lower Bounds				Unadjusted Treatment Effect	Upper Bounds			
	0.5 sd	0.25 sd	0.1 sd	0.05 sd		0.05 sd	0.1 sd	0.25 sd	0.5 sd
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Food Spending	-33.81***	-12.57**	0.18	4.43	12.19*	12.93**	17.18***	29.93***	51.18***
	(5.74)	(5.54)	(5.48)	(5.48)	(6.70)	(5.47)	(5.48)	(5.53)	(5.71)
Social Distancing Index	-0.09**	-0.01	0.04	0.06*	0.08*	0.09***	0.11***	0.16***	0.24***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Total Income	-67.70***	-21.83	5.69	14.86	28.82	33.21**	42.38***	69.90***	115.76***
	(15.36)	(15.01)	(14.90)	(14.88)	(19.05)	(14.85)	(14.85)	(14.90)	(15.14)
Any Income	-0.03**	0.01	0.03*	0.04**	0.04**	0.06***	0.06***	0.09***	0.13***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Depression Index (-)	-0.49***	-0.14	0.07	0.14	0.12	0.28*	0.35**	0.56***	0.91***
	(0.15)	(0.15)	(0.14)	(0.14)	(0.17)	(0.14)	(0.14)	(0.15)	(0.15)

Notes: Each cell is from a different OLS regression using data from phone follow-ups 2, 3, and 4. Each regression includes strata and survey wave fixed effects and the baseline-measured dependent variable. Standard errors are clustered at the household-level. See main tables for outcome variable definitions. Columns are: (1) imputes mean minus 0.5 s.d. of the nonattrited treatment distribution to attrited in treatment group, mean plus 0.5 s.d. of the nonattrited control distribution to attrited in control group. (2) to (4) are similar, though with 0.25, 0.1, and 0.05 s.d. (5) is the treatment effect for the nonattrited, replicating the core results. (6) imputes mean plus 0.05 s.d. of the nonattrited treatment distribution to attrited in treatment group, mean minus 0.05 s.d. of the nonattrited control distribution to attrited in control group. (7) to (9) are similar, though with 0.1, 0.25, and 0.05 s.d.

Table A5: Heterogeneity of Impacts

	Food Spending (1)	Social Distancing Index (2)	Total Income (3)	Any Income (4)	Depression Index (-) (5)
Panel A:	Contemporaneous: Between 3rd and Last Transfer (F2-F4)				
Treatment	1.42	0.06	52.32*	0.04	0.20
	(12.22)	(0.07)	(28.01)	(0.03)	(0.28)
Treatment × Rural	6.23	-0.02	-63.98	-0.01	0.01
	(15.75)	(0.09)	(49.77)	(0.04)	(0.39)
Treatment × Female Household Head	27.86*	0.03	28.96	0.08*	0.18
	(14.50)	(0.09)	(42.01)	(0.04)	(0.39)
Treatment × Low Food Expenditure	-2.71	0.03	-6.69	-0.03	-0.28
	(15.71)	(0.09)	(50.11)	(0.04)	(0.37)
Observations	3,711	3,782	3,388	3,709	3,831
Households	1,427	1,453	1,339	1,422	1,460
Control Mean	147	-.042	147	.45	-12
Panel B:	Persistence: 8 Months After Last Transfer (F5)				
Treatment	-19.47	-0.03	80.22*	0.08	-0.72*
	(38.21)	(0.10)	(42.03)	(0.05)	(0.42)
Treatment × Rural	-51.02	0.19	-7.85	-0.11	0.58
	(44.88)	(0.14)	(59.79)	(0.07)	(0.59)
Treatment × Female Household Head	-9.61	0.05	-44.22	0.07	0.49
	(41.13)	(0.13)	(58.70)	(0.07)	(0.57)
Treatment × Low Food Expenditure	52.92	-0.08	-37.38	-0.04	0.60
	(44.41)	(0.14)	(57.46)	(0.07)	(0.59)
Observations	1,293	1,332	1,101	1,281	1,353
Control Mean	257	.0015	202	.59	-12

Notes: All regressions are OLS and include strata fixed effects (implicitly controlling for rural location), a dummy variable for female head of household and low food expenditure, and the baseline-measured dependent variable. Standard errors are clustered at the household-level in Panel A, and robust in Panel B. Panel A additionally includes survey wave fixed effects. F1 denotes the first phone follow-up survey. Low food expenditure is a dummy variable equal to one if the household's per capita adult-equivalent food expenditure in the third wave of the Ghana Panel Survey (2018) is below the median. See main tables for outcome variable definitions. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Impacts on Components of Income

	Earned Income		Transfers Received	
	Days Earned Out Of Last 7	Household Income Last Day Earned	Days Received Out Of Last 7	Total Value Last Day Received
	(1)	(2)	(3)	(4)
Panel A:	Anticipation: Before Treatment-Only Transfers (F1)			
Treatment	-0.05	-2.42	0.01	1.16
	(0.14)	(3.68)	(0.02)	(1.65)
Observations	1,386	1,282	1,432	1,426
Control Mean	2.3	34	.11	7.5
Control SD	2.7	69	.34	28
Panel B:	Contemporaneous: Between 3rd and Last Transfer (F2-F4)			
Treatment	0.18*	2.53	0.05***	1.92**
	(0.10)	(4.25)	(0.02)	(0.89)
Observations	3,709	3,413	3,817	3,799
Households	1,422	1,347	1,455	1,450
Control Mean	1.9	36	.077	4.7
Control SD	2.5	122	.35	24
Panel C:	Contemporaneous: Between 3rd and Last Transfer (F2, F4)			
Treatment	0.19*	6.93	0.07***	3.10***
	(0.11)	(4.95)	(0.02)	(1.03)
Observations	2,427	2,235	2,495	2,485
Households	1,391	1,310	1,427	1,422
Control Mean	1.9	32	.067	4.1
Control SD	2.5	102	.3	22
Panel D:	Persistence: 8 Months After Last Transfer (F5)			
Treatment	0.18	10.31*	0.05**	3.61
	(0.16)	(5.73)	(0.02)	(2.29)
Observations	1,281	1,111	1,345	1,337
Control Mean	2.8	43	.085	7
Control SD	2.7	80	.39	36

Notes: All regressions are OLS and include strata fixed effects and the baseline-measured dependent variable. Standard errors are robust (Panels A and D) or clustered at the household-level (Panels B and C). Panels B and C additionally include survey wave fixed effects. F1 denotes the first phone follow-up survey. The survey questions for each column are: (1) How many days did your household earn income over the last 7 days?, (2) What was your total household income on the most recent day on which income was earned? (Ghanaian Cedis, top 1% winsorized), (3) How many days did your household receive in-kind or cash transfers over the last 7 days, either from the government, an NGO, a religious organization or anyone else outside your family?, (4) What was the total value of these in-kind and cash transfers on the most recent day on which they were received? (Ghanaian Cedis, top 1% winsorized). *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Heterogeneity of Impacts on Income

	Total Income (last 7 days)	Working Hours (last 7 days)	
		All	Home
	(1)	(2)	(3)
Panel A:	Contemporaneous: Between 3rd and Last Transfer (F2-F4)		
Treatment	83.66**	0.19	-1.55*
	(42.61)	(1.88)	(0.94)
Treatment × HH Has Business	-19.29	-0.49	3.01***
	(46.10)	(1.97)	(0.98)
Treatment × HH Has Wage Earner	-35.21	1.04	-0.23
	(51.24)	(2.28)	(1.03)
Treatment × HH Has Farmer	-71.62*	1.79	0.86
	(38.96)	(1.97)	(0.97)
Observations	3,370	3,807	3,801
Households	1,333	1,452	1,450
Control Mean	147	20	3.2
Panel B:	Persistence: 8 Months After Last Transfer (F5)		
Treatment	100.29	6.01**	2.39*
	(64.34)	(2.92)	(1.44)
Treatment × HH Has Business	-45.64	-2.79	1.01
	(62.93)	(3.08)	(1.56)
Treatment × HH Has Wage Earner	-32.99	-5.47	-3.53**
	(70.11)	(3.60)	(1.72)
Treatment × HH Has Farmer	-56.88	-8.20***	-1.76
	(61.02)	(3.11)	(1.56)
Observations	1,096	1,343	1,342
Control Mean	201	26	2.6

Notes: All regressions are OLS and include strata fixed effects, dummy variables for HH Has Business, HH Has Wage Earner and HH Has Farmer, and the baseline-measured dependent variable. Standard errors are clustered at the household-level in Panel A, and robust in Panel B. Panel A additionally includes survey wave fixed effects. F1 denotes the first phone follow-up survey. Total income (column 1) is the sum of earned income and transfers. Earned income is measured as the number of days the household earned income over the past 7 days multiplied by the (top-1% winsorized) Ghanaian Cedis household income earned on the most recent day that it was earned. The transfer component of Total income is measured similarly: the number of days the household received transfers over the past 7 days multiplied by the (top-1% winsorized) total Ghanaian Cedis value of transfers on the most recent day they were received. All working hours (column 2) is the number of days the respondent worked for income over the last 7 days multiplied by the number of hours worked on the most recent working day, and this number is then winsorized at the top-1%. Home working hours (column 3) is the number of days the respondent worked for income over the last 7 days multiplied by the number of hours worked from home on the most recent working day, and this number is then winsorized at the top-1%. HH Has Business is a dummy variable equal to one if the household had at least one owner of, or contributor to, a household non-farm enterprise in the 2018 Ghana Panel Survey. HH Has Wage Earner is similar, but with the household having at least one paid employed worker. HH Has Farmer is similar, but with the household having at least one owner of, or contributor to, a household farm plot. The three HH Has dummy variables are not mutually exclusive. The omitted category includes households with only people who are students, retired, incapacitated, full-time home makers, or looking for work but with no work to do. *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Long-Term Impacts on Components of Weekly Consumption

	Treatment Effects (SE) (1)	Control Mean (2)	Control SD (3)	Observations (4)
Panel A: Components of Weekly Consumption (weekly, GHC)				
Food Consumption	1.71	251.80	164.29	1,347
	(9.09)			
Clothes	-0.58	22.67	24.90	1,347
	(1.46)			
Miscellaneous	-6.99*	69.30	70.82	1,347
	(3.66)			
Fuel Consumption	-6.70	92.02	121.74	1,347
	(6.50)			
Education	-3.42	47.20	72.25	1,347
	(3.44)			
Health Insurance	-0.02	0.79	0.75	1,347
	(0.04)			
Health Expenditures	-13.60***	40.60	92.81	1,347
	(4.53)			
Panel B: Components of Health Expenditures				
Preventative Care	-0.64	3.65	25.59	1,347
	(1.88)			
Illness	-8.32***	17.70	61.37	1,347
	(3.04)			
Injury	-2.17	4.98	54.90	1,347
	(1.65)			
Other and Unreported	-2.60	14.26	34.01	1,347
	(2.14)			

Notes: All regressions include strata fixed effects. The regressions estimate long-term effects of the cash transfer treatment on outcomes measured in the fourth wave of the Ghana Panel Survey (roughly two years after the final transfer). Each regression controls for lagged dependent variables (or closest equivalents) from Wave 3 of the Ghana Panel Survey and from the baseline survey for the cash drop experiment. All outcomes are winsorized at the top 1%. Outcomes in Panel A are the components of total weekly household consumption in Ghanaian Cedis. Outcomes in Panel B are the components of weekly household Health Expenditures in Ghanaian Cedis. Preventative care includes check-ups, prenatal care, postnatal care, and vaccination. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Script Introduction

Our survey script emphasizes the link between IPA and the Ghana Panel Survey:

"Hello. I'm [enumerator name] from Innovations for Poverty Action, a non-profit organization dedicated to finding innovative solutions to development issues in various countries. We have offices in Accra and in Tamale. We work with a group of researchers who conduct the Ghana Socioeconomic Panel Survey, which studies how the lives of individuals and households in Ghana are affected by the process of economic change. We understand that you have consented to being a part of this survey in this past. We are contacting you now because we are interested in having you participate in a different study that is taking place as a phone survey, which is why we have contacted you by phone rather than in-person."

While most respondents would not have had experience with IPA, all respondents would have had experience with the Ghana Panel Survey.

COVID-19 Messaging Accompanying Our Surveys

While we did not randomize COVID-19 messaging, we included messaging for ethical and public health reasons.

Respondents received Message Set 1 below if they said yes to any of the questions about COVID-19 symptoms. They received Message Set 2 if they reported anything other than perfect social distancing in the social distancing module. All respondents received Message Set 3.

Message Set 1

You are almost at the end of the survey! We would just like to share the following guidance from the World Health Organization on Protecting Yourself and Others from the Spread of COVID-19.

Make sure you, and the people around you, follow good respiratory hygiene. This

means covering your mouth and nose with your bent elbow or tissue when you cough or sneeze. Then dispose of the used tissue immediately and wash your hands. Why? Droplets spread virus. By following good respiratory hygiene, you protect the people around you from viruses such as cold, flu and COVID-19.

Stay home and self-isolate even with minor symptoms such as cough, headache, mild fever, until you recover. Have someone bring you supplies. If you need to leave your house, wear a mask to avoid infecting others. Why? Avoiding contact with others will protect them from possible COVID-19 and other viruses.

If you have a fever, cough and difficulty breathing, seek medical attention, but call by telephone in advance if possible and follow the directions of your local health authority. Why? National and local authorities will have the most up to date information on the situation in your area. Calling in advance will allow your health care provider to quickly direct you to the right health facility. This will also protect you and help prevent spread of viruses and other infections.

Message Set 2

You are almost at the end of the survey! We would just like to share the following guidance from the World Health Organization on Protecting Yourself and Others from the Spread of COVID-19.

Maintain at least 1 metre (3 feet) distance between yourself and others. Why? When someone coughs, sneezes, or speaks they spray small liquid droplets from their nose or mouth which may contain virus. If you are too close, you can breathe in the droplets, including the COVID-19 virus if the person has the disease.

Avoid going to crowded places. Why? Where people come together in crowds, you are more likely to come into close contact with someone that has COVID-19 and it is more difficult to maintain physical distance of 1 metre (3 feet).

Message Set 3

You are almost at the end of the survey! We would just like to share the following guidance from the World Health Organization on Protecting Yourself and Others from the Spread of COVID-19.

Regularly and thoroughly clean your hands with an alcohol-based hand rub or wash them with soap and water. Why? Washing your hands with soap and water or using alcohol-based hand rub kills viruses that may be on your hands.

Avoid touching eyes, nose and mouth. Why? Hands touch many surfaces and can pick up viruses. Once contaminated, hands can transfer the virus to your eyes, nose or mouth. From there, the virus can enter your body and infect you.

Keep up to date on the latest information from trusted sources, such as WHO or your local and national health authorities. Why? Local and national authorities are best placed to advise on what people in your area should be doing to protect themselves