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Abstract

We study the impact of mobile money transfers to a representative sample of low-income Ghanaians during the COVID-19 pandemic. The announcement of the upcoming transfers affects neither consumption, well-being, nor social distancing. Once disbursed, transfers increase food expenditure by 8%, income by 20%, and a social distancing index by 0.08 standard deviations. Over 40% of the transfers were spent on food. The positive effects on income mostly persist at final measurement, eight months 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 (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 these (and many other COVID-19 emergency relief efforts) was to *reduce* labor supply (in the aims to increase social distancing). Whereas in "normal" times, negative labor supply responses are often a feared consequence of transfer programs (although evidence points either to a null effect or positive effect on labor supply in low-income countries, e.g. see 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 Ghana 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 occurred with considerable delay due to logistical constraints. At the conclusion of each survey, respondents in both treatment and control were given messages about safe pandemic practices.

We report findings from five follow-up surveys. The first 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 follow-ups took place while the treatment group continued to receive ongoing transfers. The fifth follow-up took place roughly eight months after the final cash transfer. We explore effects on food security, labor supply, and self-reported compliance with social distancing.

The value of each transfer was deliberately substantial; each transfer was 90 GHC, which is equal to 65% 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 have three main findings. First, we find little evidence of anticipation effects: at the first followup 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). This is policy relevant, as it implies that the mere act of announcing an impending transfer scheme did not allay the effects of the crisis.

Second, the transfers had a statistically and economically significant contemporaneous effect on household financial well-being and food security, as well as social distancing. Treated households spent about 8% more on food relative to the control mean. We estimate that on average 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. And our social distancing index increased by 0.08 standard deviations in the treatment group, driven primarily by an increase in the number of days that treated individuals stayed home all day. Importantly, we do not find statistically significant evidence that the transfers positively affected psychological well-being.

Third, we find that the effects on income persist to our final followup survey, eight months after the final transfer. In our final survey wave, the treatment group had an approximately 20% higher income on average, albeit our effects are somewhat imprecisely estimated. In contrast, we do not find evidence that the other contemporaneous effects persisted beyond our transfer period.

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 strong 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 meaningfully increased social distancing, and they bolstered recipients' incomes in a manner that persisted well beyond the termination of the transfers.

There are a number of other randomized controlled trials evaluating cash transfers during the COVID-19 crisis. Table 1 catalogues all of 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 crisis. Finally, we reviewed the research page of the NGO GiveDirectly for completed or ongoing cash transfer trials that coincided with the COVID crisis. Table 1 notes the study location, sample characteristics, the design of the cash transfers, and whether or not we found a working or published paper online.

We identified fifteen ongoing or completed studies, five of which currently have working or published papers. Compared with those five, ours is unique in satisfying both of the following important features. First, we study a policy that could be implemented at scale by a local or national government.[1] Second, our study sample is representative of a broad population of policy interest. Specifically our sample is drawn from roughly the bottom half of the income distribution of the nationally representative Ghana Socioeconomic Panel Survey (GSPS).[2] Therefore our study may be particularly valuable in informing future government responses to pandemics and other crises. Finally, compared to all other studies listed in Table 1, ours is the only one to evaluate the anticipation effects of receiving cash transfers in addition to the effects that materialize after receipt of the transfers.

[1]This stands in contrast with Banerjee et al. (2020), Kimani et al. (2020), and Aggarwal et al. (2020), all of which 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 Kimani et al. (2020), and one, two, or three transfers of USD 250 in the case of Aggarwal et al. (2020)). 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. (2020) 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.

[2]Our sample is the lower 55 percent of households in the GSPS with cell phone contact information (93 percent of the GSPS have cell phone access) as ranked by the Poverty Probability Index (https://www.povertyindex.org/about-ppi). This contrasts with Londoño-Vélez and Querubin (2022), which studies the impact of cash transfers on a population enrolled in a welfare program that reaches only about 5% of Colombia and Brooks et al. (forthcoming), which studies the impact of cash transfers on a convenience sample of microenterprise owners in Kenya.

2 Context and Experiment Design

Ghana saw its first confirmed cases of COVID-19 in March of 2020.[3] Roughly a year later, the country had seen nearly 87,000 confirmed cases and 656 deaths, in addition to widespread economic and social disruption. As of May 2020, 84% of Ghanaians in a nationally representative survey reported a drop in income resulting from the COVID-19 crisis,[4] 33% reported a drop in employment, 30% reported reduced access to markets, and 52% reported missed or reduced meals (Egger et al. 2021).

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 Intervention and Timing of Surveys

Our study examines the status of a representative sample of 1,508 relatively low-income Ghanaians in the months following the pandemic's spread to Ghana, as well as households' responsiveness to a cash transfer-based social protection program designed to fit smoothly into the government's current social protection system. Specifically, we conducted a randomized experiment in which 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

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

[4] 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.

($N = 737$) received only the first of these transfers.[5]

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 A2). In particular, the median gap between two adjacent transfers was 20 days, with some variation across transfers (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). 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.[6]

We conducted all surveys by phone. In addition to questions about various household outcomes, all surveys ended with one of three messages relaying various forms of guidance from the World Health Organization about safe pandemic practices (exact scripts in Appendix). 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

[5] 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.

[6] 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).

we examine whether the anticipation of future grants has an impact on household outcomes, in particular labor supply.

We conducted the remaining follow-up surveys in August 2020 (F2), October 2020 (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 follow-up, it is important to note the timing of the transfers relative to each follow-up. Figures A2 and A3 visualize 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 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 below with and without F3.

2.2 Summary Statistics and Balance

We sampled households from the Ghana Socioeconomic Panel Survey (Ghana Panel), a nationally representative survey that has been administered to a sample of around 5,000 households 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 5,667 households in 2018.

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 1,050 households that ranked lowest respectively in rural and urban areas. We then randomized the resulting 2,100-household sample equally into treatment and control, stratifying by rural vs. urban status and fine food expenditure cells. [7] We enrolled 1508 households from these poorest 2100 Panel Survey households. The randomized assignments were programmed into the baseline survey but not shared separately with the field team.

The first six columns of Table A1 compare the 2018 characteristics of the full, nationally representative Ghana Panel sample, with the 1,508 households we surveyed at baseline. 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).

The final three columns of the table provide the 2020 baseline level of psychological distress as measured by the Kessler-6 scale for the experimental sample. The experimental sample scores similarly to the full Ghana Panel sample in 2018 with respect to the Kessler-6 score. 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. This heightened degree of psychological distress diminishes with each successive follow-up survey until follow-up 4, when psychological well-being has almost recovered to 2018 levels (Figure A5). Distress was higher in mid-2021, as of our final follow-up 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).

Response rates to the follow-up phone surveys are high at around 90% (Table A2), with the exception of F4 which had a response rate of 75%. Treated households respond at statistically

[7] We held the next 1000 lowest ranked households in reserve in case we were not able to reach our goal of 1500 enrolled households.

significantly higher rates throughout, with these response rate differences larger from F2 onwards, i.e. the first follow-up at which the treatment group had received more transfers then control group. The differences might reflect a mixture of gratitude for the transfers and the misunderstanding that survey response is a prerequisite for continued transfers.

Tables A3 and A4 report that assignment to treatment is consistent with randomization (Panel A). More importantly, the attrited sample is also balanced (Panel B). Though treated households are more likely to respond to the follow-up surveys, this differential response has not created imbalance on observables.

2.3 Specification

We estimate variants of the following specification throughout:

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

where y_{it} is outcome y for household i at follow-up $t \in \{1,2,3,4,5\}$, α_s are randomization strata fixed effects, and y_{i0} is the dependent variable measured at baseline. 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

3.1 Expenditure

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

To estimate anticipation effects, we use data from the first follow-up survey. In principle, forward-looking households might increase spending upon the announcement of future cash transfers, in an attempt to smooth consumption. We see no evidence of this in Panel A – effects on food and non-food expenditure are not statistically significant, and actually negative in the case of food expenditure.

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 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, 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 last transfer was disbursed (Figure A4).

We find no evidence of contemporaneous effects of the cash grants on non-food expenditure (column 2). Together these estimates imply that nearly the full increase in expenditure can be attributed to food expenditure. This 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.

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). 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 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 for in-person economic interactions. 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 3.

As with expenditure, we find no evidence of anticipatory 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).

However, 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).

The treatment effect on attending social gatherings (column 3) is also in the direction of increased distancing, although not statistically significant. 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.

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 3. 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.

3.3 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 (Table 3), they may be doing so by reducing work hours.

Table 4 proves such concerns to be unfounded. While we again find no anticipation effects (Panel A), contemporaneous effects of cash on recent total income are positive and marginally statistically significant (column 1) – at 29 GHC per week (SE: 19) 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. These income increases are not driven by increases in hours worked: our point estimates in column 2 reflect a roughly 5% increase (not statistically significant) in hours worked in the past seven days.

We also examine hours worked from home (column 3), and find neither an anticipation nor a contemporaneous effect. These null effects suggest that the positive effect on social distancing by staying at home is not accompanied by an observed reduction in labor supply outside of the home. Thus the shift suggests social distancing increased by reducing out-of-home social activities (although note that "social gatherings" does not reduce, but that could be capturing more formal gatherings rather than informal).

In contrast to the impacts on expenditure and social distancing, 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.

Table A6 decomposes household income into four components: 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 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.

Finally, we investigate heterogeneity of impacts on income and labor supply. As above, Table A5 explores heterogeneity on 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 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.

3.4 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 5.

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). 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.[8] 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

[8] 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%.

(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 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.

We do not find important heterogeneity on any of these outcomes in Table A5.

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 who own a cell phone. The transfers were 90 GHC, 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 social gatherings rather than a reduction in livelihood generating activities, although we find evidence that small business owners who received our transfers shifted to some extent to home production.

Moreover, we do not see a tradeoff between improved distancing and income in this setting. In fact, transfers had large and persistent (to eight months, albeit noisily estimated) effects on income. Collectively these results suggest that cash relief during a pandemic can promote adherence to public health protocols while bolstering the economic well-being of recipients.

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Table 1: COVID-19 Cash Transfer Experiments

Authors	Country	Sample	Transfers in US $'s
Our Experiment			
Karlan et al. (2022)	Ghana	Nationally representative of low income households	$15, 8x, 3 weeks apart
Published			
Brooks et al. (forthcoming)	Kenya	Female, urban microentrepreneurs	$46; 1x
Londoño-Vélez and Querubin (2022)	Colombia	Welfare recipients outside 25% poorest municipalities	$19; 3x; every 5 to 8 weeks
Working Papers			
Aggarwal et al. (2020)	Liberia	Mid-sized villages from 6 districts	$250; 1x, 2x, or 3x; monthly or quarterly
Banerjee et al. (2020)	Kenya	Villages in 2 poor counties	$22.5 per adult; monthly for 12 years
Kimani et al. (2020)	Uganda	Refugee settlement	$1000, 1x
Pre-registered			
Aiken et al. (2022)	Togo	Poorest 100 cantons	$15.5 for women; $13.5 for men; 5x; monthly
Alatas et al. (2021)	Indonesia	Pre-existing vocational training and cash transfer study	Not specified
Badolo et al. (2021)	Burkina Faso	Not specified (overlay on pre-existing pollution study)	Varied by hh size; intended to cover masks and soap
Bertrand and Hallberg (2021)	Chicago (USA)	Low income; 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
Duflo et al. (2021)	India	Elderly, living alone	$13; 1x
García et al. (2021)	South Carolina (USA)	Rural; Near poverty line; affiliated with church org	$200; 24x; monthly
Jacob et al. (2020a)	United States	Supplemental Nutrition Assistance Program (SNAP) recipients	$1000; 1x
Jacob et al. (2020b)	United States	HHs from zip codes with poverty rates > 35%	$1000; 1x

Table 2: Impacts on Expenditure

	Expenditure (last 7 days)	
	Food	Non-Food
	(1)	(2)
Panel A:	Anticipation (F1)	
Transfers	-8.07	0.70
	(13.19)	(7.35)
Observations	1391	1383
Control Mean	209	51
Control SD	288	127
Panel B:	Contemporaneous (F2-F4)	
Transfers	12.19	-3.17
	(6.70)	(2.84)
Observations	3711	3709
Control Mean	147	32
Control SD	167	89
Panel C:	Contemporaneous (F2, F4)	
Transfers	10.97	0.33
	(7.79)	(2.82)
Observations	2429	2422
Control Mean	147	28
Control SD	171	65
Panel D:	Persistence (F5)	
Transfers	-20.94	-9.05
	(21.08)	(10.06)
Observations	1293	1296
Control Mean	257	73
Control SD	377	156

Notes: All regressions 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. 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 spent on food (non-food) on the most recent day food (non-food) was purchased.

Table 3: 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 Effects (Follow-Up 1)						
Transfers	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	1425	1438	1438	1438	1428	1434	1438
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 Effects (Follow-Ups 2, 3 and 4)						
Transfers	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	3782	3831	3831	3831	3792	3814	3831
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 Effects (Follow-Ups 2 and 4)						
Transfers	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	2475	2505	2505	2505	2482	2493	2505
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:	Persistent Effects (Follow-Up 5)						
Transfers	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	1332	1352	1353	1353	1337	1347	1353
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 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. 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.

Table 4: Impacts on Income and Labor Supply

	Total Income (last 7 days)	Working Hours (last 7 days)	
		All	Home
	(1)	(2)	(3)
Panel A:	Anticipation Effects (Follow-Up 1)		
Transfers	-15.89	-0.13	0.13
	(18.49)	(1.20)	(0.64)
Observations	1274	1435	1433
Control Mean	155	21	3.7
Control SD	334	26	13
Panel B:	Contemporaneous Effects (Follow-Ups 2, 3 and 4)		
Transfers	28.82	1.05	0.19
	(19.05)	(0.89)	(0.46)
Observations	3388	3825	3819
Control Mean	147	20	3.1
Control SD	520	23	11
Panel C:	Contemporaneous Effects (Follow-Ups 2 and 4)		
Transfers	47.25	1.02	-0.10
	(25.14)	(0.97)	(0.49)
Observations	2220	2501	2497
Control Mean	141	19	3.3
Control SD	501	23	12
Panel D:	Persistent Effects (Follow-Up 5)		
Transfers	41.65	-0.58	1.21
	(27.23)	(1.44)	(0.71)
Observations	1101	1349	1348
Control Mean	202	26	2.7
Control SD	395	26	11

Notes: All regressions 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. 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) household income earned on the most recent day that it was earned. Transfers are measured similarly: 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. 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%.

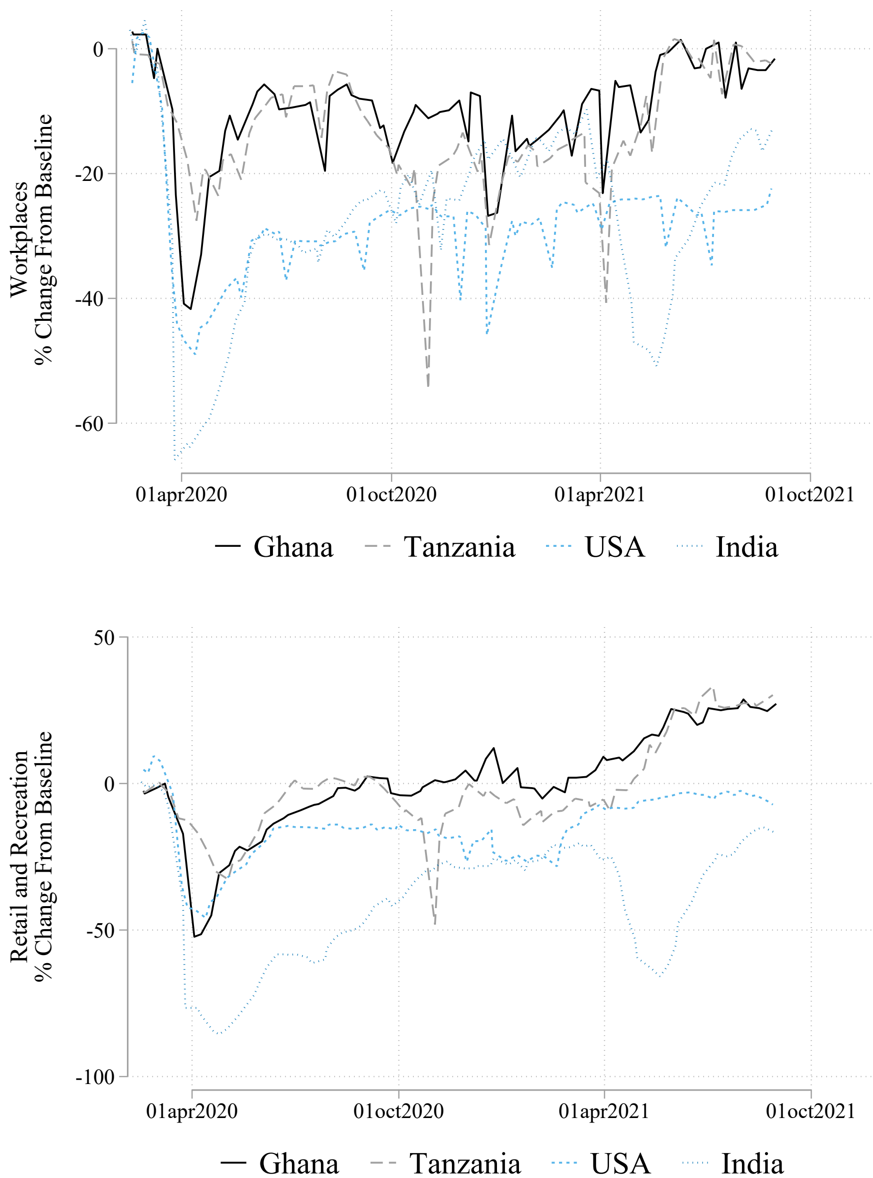
Table 5: Impacts on Beliefs and Well-Being

	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 Effects (Follow-Up 1)				
Transfers	-0.31	0.04	-0.21	-0.05	0.01
	(0.21)	(0.04)	(1.11)	(0.03)	(0.02)
Observations	1438	1438	1218	1438	1438
Control Mean	-12	-3.3	14	3.8	4
Control SD	4.3	.82	22	.53	.5
Panel B:	Contemporaneous Effects (Follow-Ups 2, 3 and 4)				
Transfers	0.12	0.04	-0.12	-0.07	0.04
	(0.17)	(0.03)	(0.74)	(0.03)	(0.02)
Observations	3831	3831	3311	3831	3831
Control Mean	-12	-3.1	11	3.6	4
Control SD	4.5	.86	18	.64	.47
Panel C:	Contemporaneous Effects (Follow-Ups 2 and 4)				
Transfers	0.07	0.04	-0.06	-0.04	0.04
	(0.18)	(0.04)	(0.81)	(0.03)	(0.02)
Observations	2505	2505	2166	2505	2505
Control Mean	-11	-3.1	11	3.6	4
Control SD	4.3	.85	19	.61	.48
Panel D:	Persistent Effects (Follow-Up 5)				
Transfers	-0.01	0.03	-1.31	0.00	0.03
	(0.26)	(0.05)	(1.48)	(0.04)	(0.05)
Observations	1353	1353	1169	1353	1328
Control Mean	-12	-2.8	18	3.6	-3.3
Control SD	4.4	.95	24	.69	.79

Notes: All regressions 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. 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).

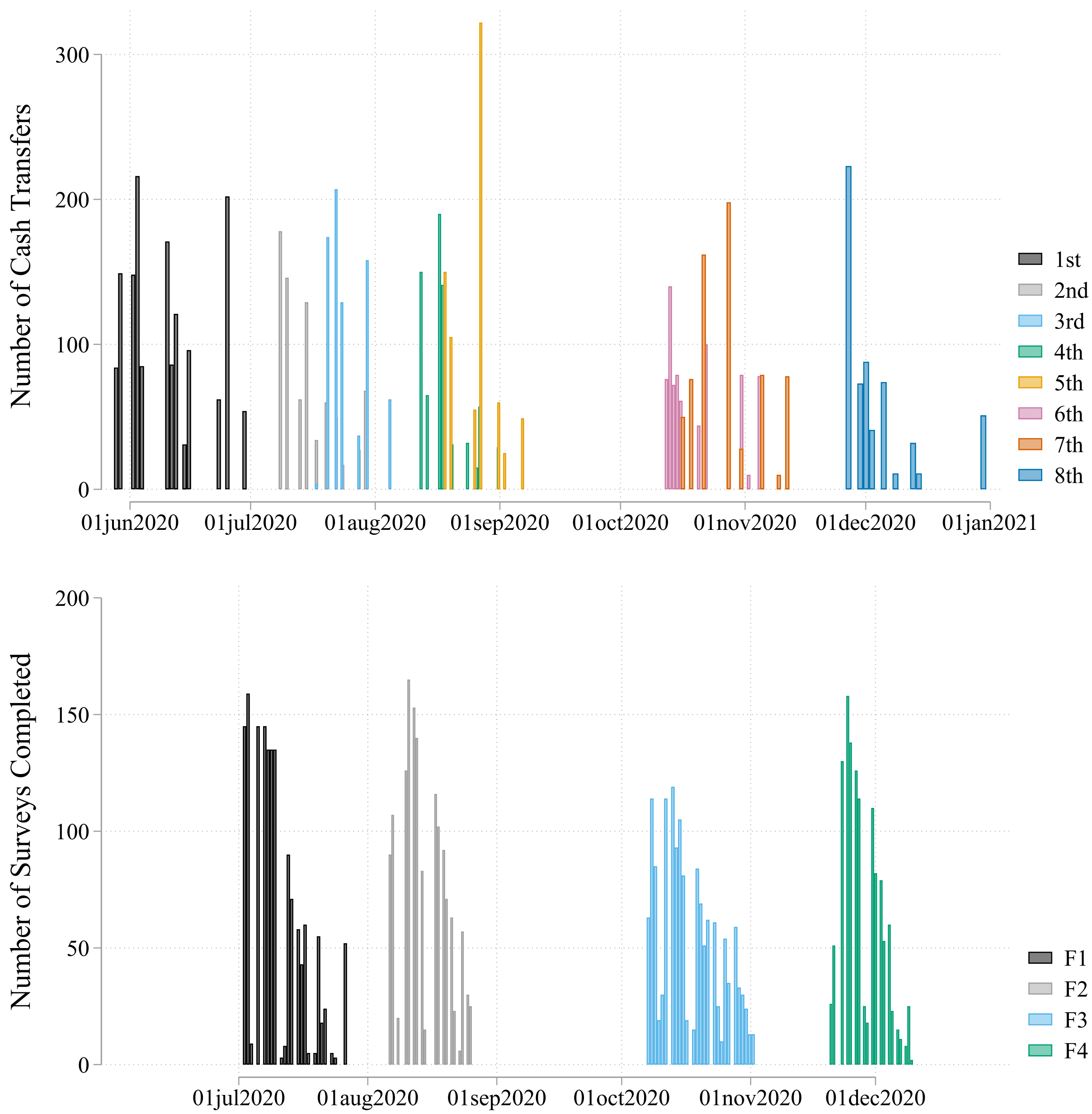
Appendix

Figure A1: Google Mobility Trends



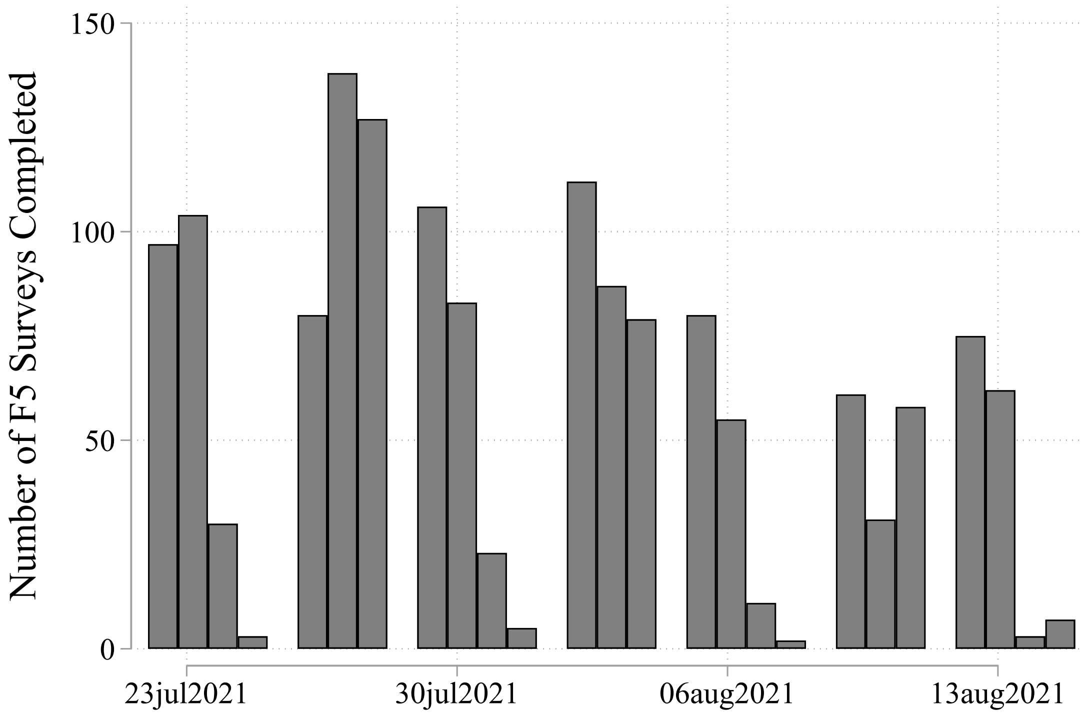
Notes: The figure visualizes COVID-19 Community Mobility Reports from Google (https://www.google.com/covid19/mobility/), collapsed to the weekly-level. The data is based on GPS-linked data collected through the use of Google Maps. Google aggregates the data to show percentage changes in activity across 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: Timing of Surveys and Transfers



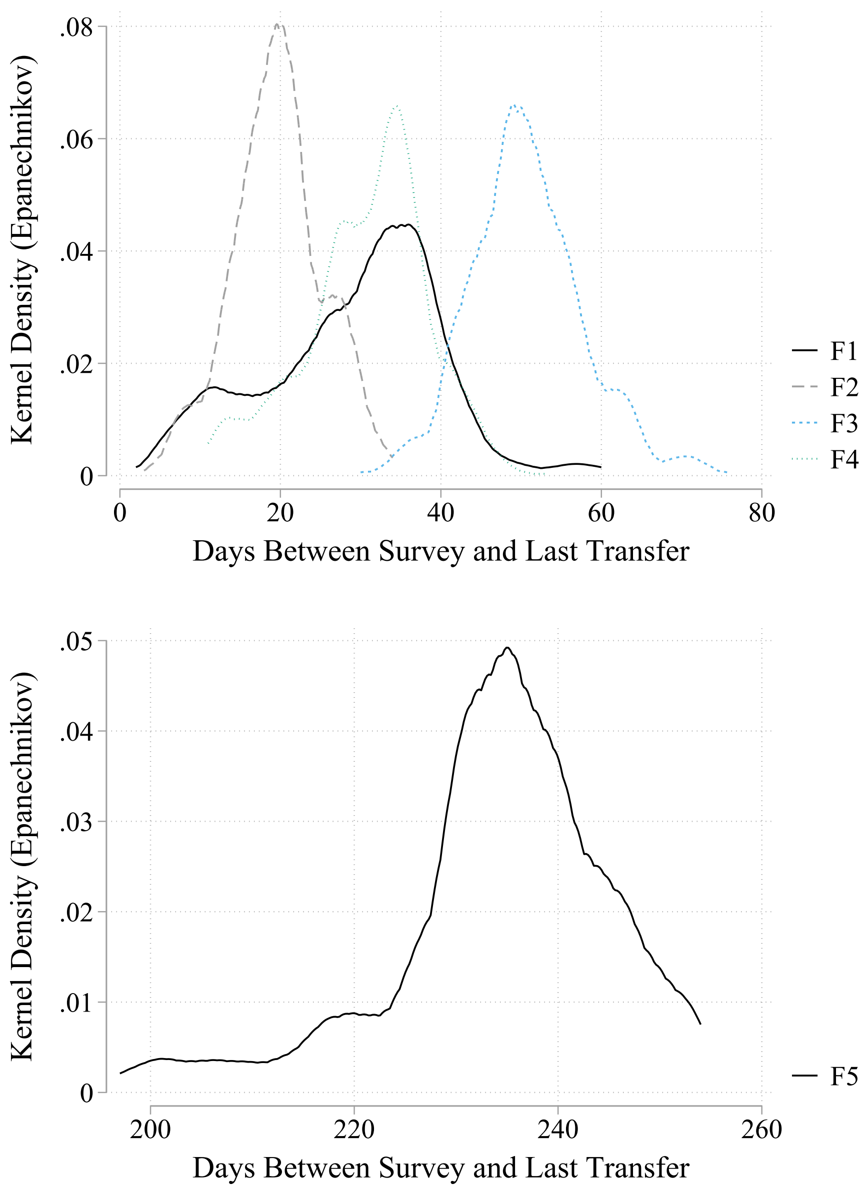
Notes: The top panel shows the timing of the cash transfers to recipients. The first transfer was made to both treatment and control recipients during June 2020. Subsequent transfers were made only to treatment recipients. The bottom panel shows the timing of the first four follow-up surveys. The first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

Figure A3: Timing of Follow-up 5 Surveys



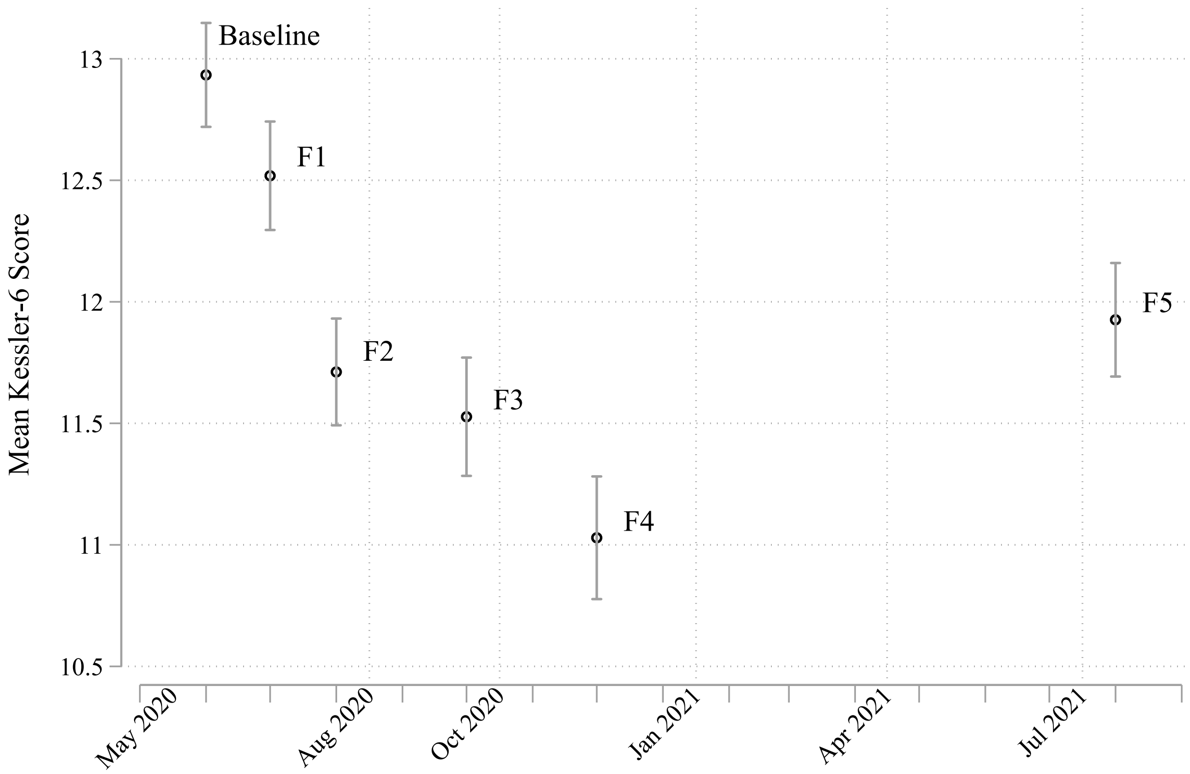
Notes: The figure shows the timing of the fifth follow-up survey, which occurred seven to eight months after the last transfer was disbursed.

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



Notes: The figure visualizes the number of days between a respondent taking a follow-up 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.

Figure A5: Trends in Psychological Distress



Notes: The figure visualizes the average Kessler-6 psychological distress score (higher = more distressed) in the experimental sample at the point of the baseline survey and each subsequent follow-up. 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).

Table A1: Baseline Summary Statistics

	Ghana Panel Survey 2018								
	All			Experiment			Experiment Baseline		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Household Size	3.35	2.26	5,667	4.09	2.42	1,508			
Household Head Female	0.39	0.49	5,667	0.35	0.48	1,508			
Household Head Age	49.85	17.27	5,667	49.75	15.94	1,508			
Food Expenditure Per Capita	229.83	217.37	5,259	132.17	68.69	1,508			
Lives in Urban Community	0.22	0.41	5,259	0.21	0.41	1,508			
HH Has Wage Earner	0.22	0.41	5,673	0.24	0.43	1,508			
HH Has Business	0.40	0.49	5,673	0.46	0.50	1,508			
HH Has Farmer	0.52	0.50	5,673	0.49	0.50	1,508			
HH Has Cellphone	0.76	0.43	5,673	0.84	0.37	1,508			
HH Has Any Mobile Money Account	0.71	0.46	5,673	0.81	0.39	1,508			
HH Has MTN Mobile Money Account	0.59	0.49	5,673	0.74	0.44	1,508			
HH Kessler Sum	10.76	3.70	5,627	10.31	3.54	1,496	12.93	4.24	1,508

Notes: Columns 2 to 4 show the Ghana Panel Survey Wave 3 data (2018) for the full sample. Columns 5 to 7 show the Ghana Panel Survey Wave 3 data only for those households also included in the baseline survey for the mobile money experiment. Columns 8 to 10 show the baseline survey data itself, collected during May to June 2020. Food Expenditure (Adult Equivalent) is food expenditure per adult equivalent capita, 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. Kessler scores from the experimental baseline are at the respondent-level. The Kessler-6 index asks respondents *During the past 7 days, about how often did you feel ...* for each of the six categories listed above. 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 other household-level demographic variables were not collected in the experimental baseline.

Table A2: Response Rate by Follow-Up Survey

	Whether Responded to Follow-Up Survey (=0/1)				
	F1 Jul 2020 (1)	F2 Aug 2020 (2)	F3 Oct 2020 (3)	F4 Nov-Dec 2020 (4)	F5 Jul-Aug 2021 (5)
Transfers	0.02 (0.01)	0.10 (0.02)	0.13 (0.02)	0.14 (0.02)	0.07 (0.02)
Observations	1508	1508	1508	1508	1508
Control Mean	0.94	0.86	0.81	0.68	0.86

Notes: Each regression includes strata fixed effects. Standard errors are robust.

Table A3: Baseline Balance: Economic Outcomes and Social Distancing

	Expenditure			COVID-19		Income	Working Hours	
	Food	Non-Food	Transfers	Social Distancing Index	Symptoms Index	Total	All	Home
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A:				Full Sample Balance Test				
Transfers	17.26	10.22	-1.33	0.01	-0.00	15.24	-0.74	-0.20
	(17.25)	(12.06)	(1.30)	(0.05)	(0.05)	(25.39)	(1.37)	(0.72)
Observations	1477	1472	1498	1500	1508	1401	1507	1505
Control Mean	209	57	5.7	-.0033	.0078	191	21	4
Panel B:				Attrited Sample Balance Test (Follow-Ups 2, 3 and 4)				
Transfers	15.34	6.55	-1.65	0.02	-0.03	6.72	-0.38	-0.58
	(15.26)	(12.22)	(1.28)	(0.05)	(0.05)	(24.04)	(1.31)	(0.69)
Observations	3711	3709	3799	3782	3831	3388	3825	3819
Control Mean	204	60	6	-.022	.03	188	21	4.3

Notes: All regressions include strata fixed effects. See main tables for outcome variable definitions. Panel A tests for balance using only the household-level baseline data, with robust standard errors. In Panel B, the unit of observation is the household-by-wave. These regressions test for balance among those that answered the equivalent question in follow-ups 2, 3, and 4, with wave fixed effects added, and standard errors clustered at the household-level. For example, column 1 includes all household-by-wave observations from follow-ups 2, 3, and 4 with non-missing data for food expenditure, with baseline food expenditure as the dependent variable.

Table A4: Baseline Balance: 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:	Full Sample Balance Test				
Transfers	-0.14	-0.07	-0.54	-0.00	0.01
	(0.23)	(0.04)	(1.28)	(0.03)	(0.03)
Observations	1508	1508	1361	1508	1508
Control Mean	-13	-3.3	15	3.8	3.9
Panel B:	Attrited Sample Balance Test (Follow-Ups 2, 3 and 4)				
Transfers	0.05	-0.04	-1.40	-0.01	-0.01
	(0.22)	(0.04)	(1.28)	(0.03)	(0.03)
Observations	3831	3831	3311	3831	3831
Control Mean	-13	-3.3	15	3.8	4

Notes: All regressions include strata fixed effects. See main tables for outcome variable definitions. Panel A tests for balance using only the household-level baseline data, with robust standard errors. In Panel B, the unit of observation is the household-by-wave. These regressions test for balance among those that answered the equivalent question in follow-ups 2, 3, and 4, with wave fixed effects added, and standard errors clustered at the household-level. For example, column 1 includes all household-by-wave observations from follow-ups 2, 3, and 4 with non-missing data for the depression index, with baseline depression as the dependent variable.

Table A5: Heterogeneity of Impacts

	Food Spending (1)	Social Distancing Index (2)	Total Income (3)	Working Hours (4)	Depression Index (-) (5)
Panel A:	Contemporaneous Effects (Follow-Ups 2, 3 and 4)				
Transfers	1.42	0.06	52.32	-0.39	0.20
	(12.22)	(0.07)	(28.01)	(1.69)	(0.28)
Transfers × Rural	6.23	-0.02	-63.98	0.96	0.01
	(15.75)	(0.09)	(49.77)	(1.94)	(0.39)
Transfers × Female Household Head	27.86	0.03	28.96	2.67	0.18
	(14.50)	(0.09)	(42.01)	(2.05)	(0.39)
Transfers × Low Food Expenditure	-2.71	0.03	-6.69	0.39	-0.28
	(15.71)	(0.09)	(50.11)	(1.98)	(0.37)
Observations	3711	3782	3388	3825	3831
Control Mean	147	-.042	147	20	-12
Panel B:	Persistent Effects (Follow-Up 5)				
Transfers	-19.47	-0.03	80.22	-2.91	-0.72
	(38.21)	(0.10)	(42.03)	(2.55)	(0.42)
Transfers × Rural	-51.02	0.19	-7.85	-3.86	0.58
	(44.88)	(0.14)	(59.79)	(3.34)	(0.59)
Transfers × Female Household Head	-9.61	0.05	-44.22	4.65	0.49
	(41.13)	(0.13)	(58.70)	(3.08)	(0.57)
Transfers × Low Food Expenditure	52.92	-0.08	-37.38	4.54	0.60
	(44.41)	(0.14)	(57.46)	(3.36)	(0.59)
Observations	1293	1332	1101	1349	1353
Control Mean	257	.0015	202	26	-12

Notes: All regressions 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. 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.

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 Effects (Follow-Up 1)			
Transfers	-0.05	-2.42	0.01	1.16
	(0.14)	(3.68)	(0.02)	(1.65)
Observations	1386	1282	1432	1426
Control Mean	2.3	34	.11	7.5
Control SD	2.7	69	.34	28
Panel B:	Contemporaneous Effects (Follow-Ups 2, 3 and 4)			
Transfers	0.18	2.53	0.05	1.92
	(0.10)	(4.25)	(0.02)	(0.89)
Observations	3709	3413	3817	3799
Control Mean	1.9	36	.077	4.7
Control SD	2.5	122	.35	24
Panel C:	Contemporaneous Effects (Follow-Ups 2 and 4)			
Transfers	0.19	6.93	0.07	3.10
	(0.11)	(4.95)	(0.02)	(1.03)
Observations	2427	2235	2495	2485
Control Mean	1.9	32	.067	4.1
Control SD	2.5	102	.3	22
Panel D:	Persistent Effects (Follow-Up 5)			
Transfers	0.18	10.31	0.05	3.61
	(0.16)	(5.73)	(0.02)	(2.29)
Observations	1281	1111	1345	1337
Control Mean	2.8	43	.085	7
Control SD	2.7	80	.39	36

Notes: All regressions 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. 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? (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? (top 1% winsorized).

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 Effects (Follow-Ups 2, 3 and 4)		
Transfers	93.79	0.32	-1.53
	(41.43)	(1.85)	(0.93)
Transfers × HH Has Business	-37.81	-0.72	3.15
	(45.30)	(1.96)	(1.01)
Transfers × HH Has Wage Earner	-36.49	1.01	-0.21
	(51.28)	(2.28)	(1.03)
Transfers × HH Has Farmer	-76.07	1.75	0.89
	(39.28)	(1.97)	(0.96)
Observations	3370	3807	3801
Control Mean	147	20	3.2
Panel B:	Persistent Effects (Follow-Up 5)		
Transfers	115.82	6.46	2.45
	(67.71)	(2.89)	(1.42)
Transfers × HH Has Business	-73.69	-3.73	0.94
	(67.56)	(3.12)	(1.60)
Transfers × HH Has Wage Earner	-34.28	-5.55	-3.54
	(70.07)	(3.60)	(1.72)
Transfers × HH Has Farmer	-64.77	-8.40	-1.76
	(62.88)	(3.12)	(1.56)
Observations	1096	1343	1342
Control Mean	201	26	2.6

Notes: All regressions 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. 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) household income earned on the most recent day that it was earned. Transfers are measured similarly: 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. 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%.

Covid-19 Messaging Accompanying Our Surveys

All households in our sample were randomized to receive one of the following three messages:

Note 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.

Note 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).

Note 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