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Social protection and social distancing during the pandemic: Mobile money transfers in Ghana ☆

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ABSTRACT

We randomized mobile money transfers to a sample of low-income Ghanaians during the COVID-19 pandemic. Treated households received eight transfers that sum to roughly one month's income, while control households only received one transfer. The mere announcement of upcoming transfers has no effect. Once disbursed, transfers increase contemporaneous food expenditure by 8% and income by 20%, but do not affect psychological well-being. Over 40% of the transfers are spent on food. We find suggestive evidence that transfers increased social distancing. The positive effect on income does not persist to two years after the last transfer, and surprisingly, two-year effects on consumption and psychological well-being are negative. Together, we learn that pandemic-era cash transfers can support households economically without diminishing adherence to public health protocols, though with null or negative long-term effects.

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. Many countries responded by expanding social protections, with 3,856 such measures in 223 economies by January 2022 (Almenfi et al., 2020). However, low and middle-income country governments have less financial and institutional capacity for such policy responses. Major challenges range from identifying those most affected by the shock to designing mechanisms to provide the needed support (Aiken et al., 2025; Gerard et al., 2020). Perhaps as a result, per capita spending on these responses was over 90 times higher in high-income countries than low-income countries, and almost twice as high as a percentage of GDP (Almenfi 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., 2025; Crosta et al., 2024). Thus a

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key question is whether cash transfers lead to an increase or decrease in labor supply, and resultant social distancing, during a crisis.

We report results of a randomized evaluation of a pandemic response that delivered a series of cash transfers to low-income households in Ghana. We used 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. We randomly assigned individuals to either treatment or control, and informed each individual of their assignment at the end of a short baseline survey. We told respondents that the transfers were to help cope with the economic impacts of the coronavirus, and that they should spend them however they pleased. Both treatment and control individuals 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 follow-up surveys and one long-term 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 consumption, food security, labor supply, income, social distancing, and psychological well-being, among other outcomes. We discuss effects in the sixth follow-up separately from those in the phone follow-ups since the modality and considerably different survey design renders the levels difficult to compare to the phone-surveys.

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. We cannot say definitively whether this is due to a lack of smoothing in consumption and other behaviors, or a lack of trust that future transfers would arrive on schedule – a point we expand on below.

Second, the transfers yielded moderate contemporaneous improvements in food expenditure, household financial well-being, and income. 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. We also find that transfers led to an increase in household savings. Households that receive our transfers maintain an 18 to 30% higher earned income throughout the economic crisis than those in the control group, though these estimates are imprecise. Some of this effect on income is driven by the extensive margin: treated households are four to five percentage points more likely to have earned a non-zero amount of income during survey weeks in our transfer period. Nevertheless, the positive effects of cash transfers on food spending, savings, and income, co-exist with null effects on psychological well-being and an index measuring food security.

We find mixed evidence regarding the contemporaneous impact of the transfers on social distancing. The treatment group scores 0.08 standard deviations higher on our pre-registered social distancing index – indicating increased adherence to social distancing protocols – driven primarily by an increase in the number of days that treated households stayed at home. However, when we restrict our analysis to (non-pre-registered) measures of social distancing that are less subject to social desirability bias, we find little evidence that the transfers increased social distancing. Given this mixed evidence, our more concrete takeaway is that we find no evidence that the transfers *decreased* adherence to social distancing protocols, as policymakers might fear — particularly in a context like ours where cash transfers increase earned income.

We do not find evidence that cash transfers substitute for plausible pandemic-era coping mechanisms, like motivated beliefs that the pandemic's effects have been exaggerated, or increased religiosity. In particular, transfers do not affect beliefs about the fatality rate from contracting COVID-19, while there is some evidence that transfers *reduce* concerns that the economy will be negatively affected. As for religiosity, we see some evidence that transfers have positive contemporaneous effects on the frequency of prayer, although no evidence of effects on the frequency of reading scripture, or of believing in the "prosperity gospel" – a theology that asserts that God rewards faithful followers with wealth and health.

Third, the effects on income persist to our fifth follow-up survey, eight months after the final transfer. In this wave, participants in the treatment group are four percentage points more likely to report any income, and reported 24% higher income. However, we find no evidence that this positive effect on income persists two years after the final transfer was disbursed. In fact, at this two-year point we find some evidence for a puzzling drop in consumption and mental health of the household head, with these effects being driven by distinct subsamples of our data — urban households in the capital, Accra, drive the negative consumption effects, while female-headed households drive the negative mental health effects. The negative mental health impacts of our cash transfer program echo those of Baird et al. (2025), which finds that a cash transfer program targeted to adolescent Ugandan girls at risk for depression negatively impacted mental health. We discuss this at more length in Section 3.6.

We do not find evidence of meaningful heterogeneity in impacts along most dimensions. One 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. It may be that the especially large contemporaneous impact on female-headed households is linked to the long-run decline in their psychological well-being, described in the previous paragraph, if they felt disappointment or pain from reverting to their "control-group" levels of income and expenditure.

We further find evidence for greater impacts on food expenditure in districts with greater incidence of COVID-19 symptoms. Finally, we explore heterogeneity in impacts by baseline beliefs about the severity of the pandemic on health outcomes and the Ghanaian economy. We find less impact on social distancing and more impact on income generation among those with more pessimistic expectations about COVID-19's fatality rate. The smaller impact on social distancing makes sense, since we find that pessimistic households are already social distancing more to begin with. Otherwise, we find that study participants with more pessimistic beliefs about COVID-19's impact on the economy have a larger contemporaneous social distancing response to transfers, but a smaller response in the 8-month follow-up. This latter finding may be a result of these participants discovering that COVID-19 had a relatively limited impact on the economy.

Together our findings provide mixed support for cash transfers as a mode of pandemic support for poor households in low-income countries. On the positive side, upwards of 40% of the transfer was spent on food, the transfers do not diminish adherence to social distancing protocols (and perhaps improved them), and they bolstered recipients' incomes in a manner that persisted somewhat beyond the termination of the transfers. On the negative side, we see some evidence of long-term negative effects on consumption and psychological well-being of the household head. Since these negative (i.e., negative and not merely fading) impacts are difficult to rationalize with standard models and inconsistent with the shorter term results, we hold the view that future research is needed to determine the replicability of these results and their mechanism.

A number of experiments randomized cash transfers during the COVID-19 crisis. Tables 1 and A1 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 A1 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 study a policy that could be implemented at scale by a local or national government on a sample that is largely 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 implemented via the low-transaction cost medium of mobile money; and our sample was drawn from roughly the bottom half of the income distribution of the nationally representative Ghana Panel Survey.[1] 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).[2] An important exception is Londoño-Vélez and Querubin (2022), a paper on the short-term impact of mobile money transfers on a population enrolled in a welfare program in Colombia. The authors find small positive effects on financial health, imprecise positive effects on psychological well-being, and not statistically significant effects on food security. We build on their work by exploring the effects of mobile money in a lower middle-income country.

Second, 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 positive 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. While we find mixed evidence on social distancing, two experiments (out of the four that measure social distancing) find that cash transfers increase social distancing, and one finds a reduction. Finally, only one other study (out of the three that measure income) finds that transfers increase income (of businesses, Brooks et al. 2022).

We defer detailed discussion of how our results compare to impacts in ordinary times to Section 4. In brief, compared to typical impacts of cash transfer programs in ordinary times, we find similar contemporaneous impacts on food expenditure, substantially larger contemporaneous impacts on income, and less evidence of positive effects on well-being. Furthermore, while normal-times cash programs typically find positive effects that persist after transfers end, we find null or negative effects two years later.

2. Ghana's COVID-19 context and experiment design

Ghana saw its first confirmed cases of COVID-19 in March 2020.[3] 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). 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).[5]

On March 15, 2020, the Government of Ghana closed schools and banned all public gatherings of two or more persons, with an exception for funerals of up to 25 people (Verani et al., 2020). The ban on public gatherings was gradually eased from May to August 2020 (Network, 2020). School reopening began in June 2020 with older students, with most primary-age students not returning to school until January 2021 (Sam, 2021). Borders were closed to human traffic for two years (Mensah, 2022), but trade in goods remained open.

The Government of Ghana introduced a partial lockdown policy on March 30, 2020, covering the two largest cities in Ghana, Accra and Kumasi (Assan et al., 2022). This lockdown included a stay-at-home order, with some exceptions. The lockdown was lifted after three weeks. Our experiment's baseline survey took place after this, meaning that no lockdown policies were in force during our experimental period.

Consistent with the timing of the lockdown, mobility dropped by 40 to 50% in Ghana around April 2020, as measured by Google (Figure A1). Mobility rebounded following the lifting of the lockdown, and was only 10 to 20% below pre-COVID-19 levels by the time of our baseline phone survey. Mobility to workplaces remained below baseline levels for practically the duration of our five follow-up surveys (top panel), though retail and recreational mobility had returned to baseline levels by late-2020 (bottom panel), likely reflecting the easing of restrictions on gatherings, as discussed above. 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. Overall, Ghana had relatively weak restrictions on mobility and economic activity during our experimental time period.

[1] As we discuss more fully below, though our sample is drawn from this representative population, those who are included in our final sample are selected based on mobile phone ownership and having an MTN mobile money account among several other more minor criteria. Despite these selection criteria, our sample averages of non-mobile phone and mobile money-related variables largely resemble those of the representative sample from which they were drawn.

[2] 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.

[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.

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

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								
Aiken et al. (2025)	Togo; Poorest 100 cantons; $15.50 for women; $13.50 for men; 6x; monthly.	$+^{***}$				n.s.	$+^{***}$	2
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 Paper								
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.

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 5,675 households in 2018. We pre-registered the experiment with the AEA Registry (#0005861), and we describe minor deviations from the pre-registration in Appendix B.

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.[6] 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.[7] The randomized assignments were programmed into the baseline survey but not shared separately with the field team.

Table 2 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).

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 4 to 6), though as expected, they are more likely to have had cell phones and mobile money accounts in 2018.[8] Given the large sample sizes, mean differences between the experimental sample and broader low-food expenditure Ghanaians tend to be statistically significant (column 13), though following Imbens and Rubin (2015), normalized differences tend to be small, at around 0.1σ (with the exception of the cellphone and mobile money account-related variables).

The experimental sample scores similarly to the full Ghana Panel sample in 2018 with respect to the Kessler-6 measure of psychological distress (bottom row, Table 2). 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.

[6] 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.

[7] 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.

[8] 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.

Table 2
Summary statistics from the 2018 Ghana Panel Survey.

	All			Food expenditure eligible			Has phone number			Experiment sample			(4) vs (10)	
	Mean (1)	SD (2)	N (3)	Mean (4)	SD (5)	N (6)	Mean (7)	SD (8)	N (9)	Mean (10)	SD (11)	N (12)	*p* (13)	SD (14)
Household Size	3.36	2.26	5,660	3.81	2.36	3,373	3.86	2.36	3,099	4.09	2.42	1,508	<0.01	0.12
Household Head Female	0.39	0.49	5,660	0.38	0.48	3,373	0.37	0.48	3,099	0.35	0.48	1,508	0.12	0.05
Household Head Age	49.89	17.23	5,660	51.16	17.42	3,373	50.51	17.02	3,099	49.75	15.94	1,508	<0.01	0.08
Monthly Food Exp. p.c. (GHC)	226.11	213.61	5,637	130.72	69.44	3,374	131.53	68.95	3,100	132.17	68.69	1,508	0.5	0.02
Non-Food Consumption (GHC)	111.05	117.13	5,675	98.46	104.07	3,374	101.68	104.65	3,100	117.07	113.97	1,508	<0.01	0.17
Earned Income (GHC)	198.80	622.73	5,669	160.46	542.33	3,372	169.04	560.80	3,098	199.64	594.27	1,508	0.02	0.07
Any Income	0.63	0.48	5,669	0.62	0.49	3,372	0.62	0.48	3,098	0.65	0.48	1,508	0.03	0.07
Lives in Urban Community	0.42	0.49	5,669	0.48	0.50	3,374	0.50	0.50	3,100	0.57	0.50	1,508	<0.01	0.17
HH Has Wage Earner	0.22	0.41	5,669	0.20	0.40	3,374	0.21	0.41	3,100	0.24	0.43	1,508	<0.01	0.09
HH Has Business	0.40	0.49	5,669	0.39	0.49	3,374	0.41	0.49	3,100	0.46	0.50	1,508	<0.01	0.14
HH Has Farmer	0.52	0.50	5,669	0.54	0.50	3,374	0.53	0.50	3,100	0.49	0.50	1,508	<0.01	0.09
HH Head Work Hours	18.25	25.34	5,667	16.75	24.74	3,373	17.31	25.06	3,099	19.18	26.24	1,508	<0.01	0.10
HH Has Cellphone	0.76	0.42	5,662	0.76	0.43	3,374	0.79	0.41	3,100	0.84	0.37	1,508	<0.01	0.20
Any Mobile Money Account	0.71	0.45	5,668	0.68	0.47	3,374	0.71	0.45	3,100	0.81	0.39	1,508	<0.01	0.30
MTN Mobile Money Account	0.59	0.49	5,666	0.58	0.49	3,374	0.61	0.49	3,100	0.74	0.44	1,508	<0.01	0.33
Savings Amount (GHC)	611.21	1,738.68	5,664	437.66	1,368.95	3,373	454.69	1,384.57	3,099	516.05	1,477.20	1,508	0.07	0.06
HH Kessler-6	10.76	3.70	5,627	10.56	3.65	3,351	10.50	3.60	3,077	10.31	3.54	1,496	0.03	0.07

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. Column 13 shows the *p*-value from a t-test of equality of means between the food expenditure eligible and experimental sample (columns (4) and (10)). Column 14 shows the normalized difference between (4) and (10): the difference in means divided by the square root of half the sum of the SDs squared. Monthly Food Exp. p.c. is monthly food expenditure per adult equivalent capita (in Ghanaian Cedis), using a Deaton-Zaidi adult equivalent adjustment. Non-Food Consumption is total weekly household non-food consumption in GHC, winsorized at the top-1%. Earned Income is household weekly earned income (including income from main and secondary employment, non-farm businesses, crop sales, gathering, and animals), winsorized at the top and bottom-1%. Any Income is a dummy variable equal to one if Earned Income is positive. HH Head Work Hours is the estimated weekly working hours of the household head, winsorized at the top-1%. Savings Amount (GHC) is the total amount saved, winsorized at the top-1%. The HH Kessler-6 score from the Ghana Panel Survey data is a 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. HH Kessler-6 is the sum of the six components, averaged across household members. 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).

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).[9]

2.2. Intervention and survey timing

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.[10] 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.

We intended for the program to be designed in a manner that could feasibly be scaled. Conversations with the Ministry of Finance indicated that transfers of GHC 90 were on the upper end of what would be considered at scale. In some ways, utilizing mobile money as a medium of transfer contributes to the scalability of the program due to its very low transaction costs. On the other hand, in 2018 only 71% of the population had access to a mobile money account (Table 2), which means that if this were the only medium of transfer, significant portions of the population would be overlooked. There are also practical barriers to providing cash transfers at scale in the context of a pandemic, most importantly having access to a social registry that identifies and provides contact information for intended recipients. Nevertheless, other countries – e.g. Togo – successfully rolled out transfer schemes via mobile money during the COVID-19 pandemic (see Aiken et al., 2022).

We summarize the timing of the transfers and surveys in Fig. 1, overlaying the time series of confirmed COVID-19 cases per 10,000 in Ghana, the timing of key public policies related to closures and lockdowns, and the timing of agricultural seasons. 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 Fig. 1, and for more detail, Figure A3). In particular, the median gap between two adjacent transfers was 20 days, with some variation across transfers.[11]

The 90 GHC transfer is 65% of the median household's weekly food expenditure reported at baseline, or roughly 45% of weekly earned income. This is considerably larger than transfers from the Livelihood Empowerment Against Poverty (LEAP) program, Ghana's flagship cash transfer social protection program. LEAP provides cash transfers every

[9] Table A2 presents the summary statistics from the fourth wave of the Ghana Socioeconomic Panel Survey, collected in 2022 and 2023. While differences between the 2018 and 2022/2023 waves partially reflect trends in Ghanaian economic and demographic characteristics, they are also partially confounded by our intervention. Nevertheless it may be instructive to review the aggregate statistics. Namely, when comparing the full samples across the two tables — Columns 1–3, we see a near doubling of food and non food expenditure, more than doubling of earned income, and a growth in household savings of 50%. Surprisingly, in the 2022/2023 wave, only 86% of our experimental sample reports having a mobile money account. While we can confirm that 100% of this sample theoretically have access to a mobile money account, by virtue of participating in our study, we suspect that the majority of the households who report not having an account in 2022/2023 either forgot about their account due to inactivity, or the person in their household as of 2018 who controlled the account subsequently exited the household.

[10] 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.

[11] 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.

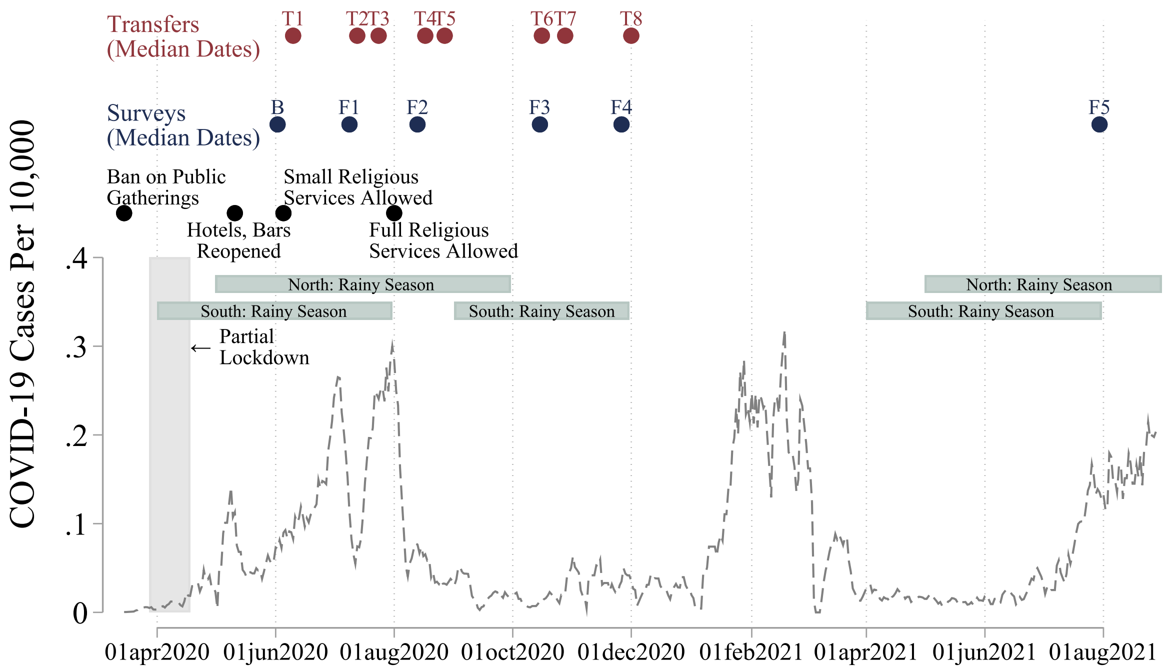

Fig. 1. Timeline of Experiment, COVID-19, and Related Events.
Notes: The figure shows the timing of the experimental phone surveys, cash transfers, key government restrictions, the spread of confirmed cases of COVID-19 (a seven-day moving average), and agricultural rainy seasons (separately for North and South Ghana). The red and blue solid circles reflect the median date of a set of cash transfers (1st to 8th) or phone survey (baseline to the fifth follow-up survey). The sixth (in-person) follow-up survey, fielded August 2022 to June 2023, is not shown. The black circles denote dates of key government restrictions: the banning of all public gatherings on March 15, 2020; the reopening of hotels, bars, and restaurants, with strict social distancing, on May 11th, 2020; the allowing of religious services, funerals, and weddings under reduced capacity, on June 5th, 2020; and the removal of the capacity cap on religious gatherings on August 1st, 2020 (with health protocols like masking maintained). The source for the COVID-19 case data is the Ghana Health Service (Ministry of Health), via the Humanitarian Data Exchange v2.0.5.

two months 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.[12]

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.[13] 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.

We note that anticipation effects require trust that IPA will send future transfers as promised. While we did not measure trust directly, IPA was a familiar organization: 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 Appendix C for the full script). Nevertheless, respondents had not received cash transfers from IPA in the past, and anticipation effects may be limited by the possibility that they did not trust IPA to follow through on its commitment. Furthering this concern, many respondents in our treatment group had already experienced a delay in their second transfer at the time of the first follow-up survey. Our first follow-up survey took place on average 30 days after the first transfer (median – 33 days), at which point no one in the treatment group had received their second transfer yet. Given these concerns, the anticipation effects we characterize may not generalize to settings in which implementation is smoother and trust levels higher.

We conducted the remaining follow-up phone 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 phone follow-up, it is important to note the timing of the transfers relative to the surveys. Figs. 1 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 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 (and other behavioral outcomes). 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 outcomes in our follow-up surveys that have close parallel measures in the Ghana Panel.

We note that our phone surveys were substantially less comprehensive than the Ghana Socioeconomic Panel Survey. In principle, this

12 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).

13 This health messaging was not randomized. See Appendix C for the scripts.

might raise concerns about the reliability of our data – such concerns could even be compounded by frictions inherent in phone surveys, such as difficulty hearing the surveyor over a weak phone signal. Reassuringly, we at least see strong positive wave-to-wave correlations between our various key outcome measures (Table A3). This suggests that our phone-based measures do not lead to substantial measurement error.

Response rates to the five follow-up phone surveys are high at around 90%,[14] with the exception of F4, which had a response rate of 75%. The response rate for the final in-person survey was 93%. Anecdotally, the low response rate to F4 was due to survey fatigue, with some respondents complaining about having to answer identical questions in quick succession (the questions in each phone follow-up survey almost completely overlapped). The F5 response rate of 90% then likely bounced back given that there was a much bigger gap between F4 and F5 (eight months) than between F3 and F4 (one to two months).

Treated households respond at statistically significantly higher rates to the phone surveys, but not to the final in-person follow-up (Table A4). The differences might reflect a mixture of gratitude for the transfers, the misunderstanding that survey response is a prerequisite for continued transfers, and disappointment in the control group from not receiving the transfers. The differential attrition for our contemporaneous surveys (F2 to F4) falls considerably, though remains statistically significant, if we look at whether a household responded to at least one of the contemporaneous surveys (column 7). We make use of this fact below by showing that our results are similar if we collapse the data to each household's average answer to the three contemporaneous surveys, rather than pooling answers from all three follow-ups. For the most part, we do not see differences in the observables that predict attrition by treatment (see the joint F-test p-values, Table A4).

Treatment and control households are well-balanced on baseline characteristics (columns 1 to 4, Table A5). 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 A5, and Table A6). 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 A7). In this table, we assume that the attriters have outcomes with average values ranging from 0.5σ less than responders to .5σ more than responders. We include such a wide range so that the reader can find the estimates implied by the missing data assumptions they find the most plausible. As expected, with a range of missing data assumptions this broad, our estimates in this table vary quite widely. Without more information on the outcomes of attriters, our preferred method for correcting for attrition is to consider the average outcomes over all of the contemporaneous survey waves in which we reached a respondent. As stated above, for the most part these estimates are quite similar to our main estimates, and we reference them as relevant in the following analyses.

[14] These response rates are comparable to other phone surveys conducted by IPA Ghana during COVID-19. For instance, Duflo et al. (2023) reports on a long-running RCT on the impact of secondary education. Prior to and including 2019 those authors followed a protocol of surveying by phone with in-person follow-ups for respondents who were unreachable by phone. In 2019, 11 years into the experiment, the follow-up rate was about 94%. In 2020, switching to a phone-only protocol, the response rate fell to 84%, with attrition being 4.9% higher in the control group.

2.3. Specification

We estimate variants of the following specification throughout:

$$y_{it} = \alpha_s + \beta_0 y_{i0} + \beta_1 \text{Transfers}_i + \epsilon_{it}$$

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. Outcome variables are defined in detail in Appendix Section D, as well as more concisely in the footnotes to tables and figures. 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 five sub-sections. We then report two-year effects on a narrower set of outcomes measured in the fourth wave of the Ghana Panel Survey. Finally we explore several dimensions of heterogeneity.

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 3.

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 the timely receipt of future transfers (potentially exacerbated by our implementation delays), or a failure of consumption smoothing due to credit constraints (preventing borrowing) and limited savings (preventing running down savings).

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 a larger gap between the survey and the last transfer received at F3 (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).[15] These effects are driven by increases in spending on food the last time food was purchased ($p < 0.05$, Table A10), as opposed to increases in the number of days on which food was purchased (not significant). This may be especially reassuring given the policy goal of minimizing the number of trips outside of the home, which we return to in Section 3.4.

[15] To address concerns about differential attrition, Table A8 replicates analysis of the contemporaneous impacts reported in Table 3, but using one observation response per household: the average response across F2, F3, and F4. Estimates for food and non-food expenditure are virtually unchanged, though, as expected, the standard errors increase somewhat and the estimate for the impact of transfers on food expenditure is no longer statistically significant.

Table 3
Impacts on expenditure, income, and labor supply.

	Expenditure (7 days, GHC)		Earned income (7 days)		Working hours (7 days)	
	Food (1)	Non-food (2)	GHC (3)	Any (4)	All (5)	Home (6)
Panel A:	Anticipation: Before Treatment-Only Transfers (F1)					
Treatment	−8.07	0.70	−14.78	0.01	−0.13	0.13
	(13.19)	(7.35)	(17.82)	(0.03)	(1.20)	(0.64)
Observations	1,391	1,383	1,282	1,386	1,435	1,433
Control Mean	209	51	146	.51	21	3.7
Control SD	288	127	328	.5	26	13
Panel B:	Contemporaneous: Between 3rd and Last Transfer (F2, F3, F4)					
Treatment	12.19*	−3.17	25.42	0.04**	1.05	0.19
	(6.70)	(2.84)	(18.90)	(0.02)	(0.89)	(0.46)
Observations	3,711	3,709	3,410	3,709	3,825	3,819
Households	1,427	1,422	1,346	1,422	1,458	1,456
Control Mean	147	32	140	.45	20	3.1
Control SD	167	89	516	.5	23	11
Panel C:	Contemporaneous: Between 3rd and Last Transfer (F2, F4)					
Treatment	10.97	0.33	40.74	0.05**	1.02	−0.10
	(7.79)	(2.82)	(24.99)	(0.02)	(0.97)	(0.49)
Observations	2,429	2,422	2,233	2,427	2,501	2,497
Households	1,397	1,391	1,309	1,391	1,431	1,429
Control Mean	147	28	135	.45	19	3.3
Control SD	171	65	498	.5	23	12
Panel D:	Persistence: 8 Months After Last Transfer (F5)					
Treatment	−20.94	−9.05	45.01*	0.04	−0.58	1.21*
	(21.08)	(10.06)	(26.68)	(0.03)	(1.44)	(0.71)
Observations	1,293	1,296	1,109	1,281	1,349	1,348
Control Mean	257	73	190	.59	26	2.7
Control SD	377	156	381	.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. Earned income (GHC) 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. Earned income (Any) 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.

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 ($12.2 \times 25/7 = 43.6$ 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 note that despite the substantial increase in food *expenditure*, we do not find that the transfers lead to an increase in food *security*, as measured by an index aggregating our pre-registered measures of food scarcity (Table A9). This suggests that the increase in food expenditure is not heavily concentrated among the households at most danger of food insecurity.

Returning to Table 3, 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. Consistent with this, we find that a household's food expenditure in wave t is more predictive of its food expenditure in wave $t+1$ than non-food expenditure at t is predictive of non-food expenditure at $t+1$ (Table A3). 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). On the other hand, the null effects on our more explicit measures of food insecurity suggest that the cash transfers could have benefited from finer targeting.

We see little 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. That said, we report statistically significant negative effects on (non-food) consumption two years after transfers ended below, in Section 3.6. The negative coefficients for F5 may then reflect true negative effects of consumption after transfers end, that become detectable with the richer data collection of the Ghana Panel Survey.

Finally, in follow-up surveys 3 and 4 we collected data on household savings. Households that received the transfers were 4 to 7 percentage points (SE: 2) more likely to have saved money last month (columns 1 to 3, Table 4). They saved 26% more than the control group (column 4,

Table 4
Cash transfers increase contemporaneous savings.

	Whether saved			Amount saved (GHC)		
	F3 and F4 (1)	F3 only (2)	F4 only (3)	F3 and F4 (4)	F3 only (5)	F4 only (6)
Treatment	0.05*** (0.02)	0.04* (0.02)	0.07*** (0.02)	6.81 (4.39)	2.95 (5.51)	11.85** (5.97)
Observations	2,458	1,326	1,132	2,445	1,320	1,125
Control Mean	.14	.15	.12	26	29	23
Control SD	.35	.36	.32	93	99	86
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	N/A	N/A	Yes	N/A	N/a

Notes: The unit of observation is participant-by-wave. The two savings questions were only asked in follow-up surveys 3 and 4. Whether Saved is a dummy variable equal to one if the respondent's household saved any money last month. Amount Saved (GHC) is the total amount saved by the household last month, winsorized at the top-1%. Columns 1 and 4 pool data from follow-up surveys 3 and 4. Columns 2 and 5 use only follow-up 3, and columns 3 and 6 use only follow-up 4. Standard errors are clustered at the household-level in columns 1 and 4, otherwise standard errors are robust. *** p<0.01, ** p<0.05, * p<0.1.

$p = 0.12$), or 52% more (column 6, $p = 0.05$) when considering only F4, the follow-up with a smaller gap since the last transfer. These positive effects on savings help to account for how the transfers were used: for food spending but not non-food spending (Table 3), and for household savings.

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. Columns 1 and 2 of Table A11 present the results on our main measures of expenditure. 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. As we discuss in the next section, we also see suggestive evidence that female-headed households experience a larger increase in income and working hours in response to our intervention. These 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; Crosta et al., 2024), despite the concerns of some policy-makers. In fact, recent evidence suggests that cash transfers may even increase work effort and income through psychological or productivity channels (Banerjee et al., 2022; Kaur et al., 2025). 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 find some supporting evidence for below), they may be doing so by reducing work hours.

We find no support for concerns of reduced working hours and income (columns 3 to 6, Table 3). While we again find no anticipation effects (Panel A), contemporaneous effects of cash on the past week's earned income are positive, at 25 GHC per week (SE: 19, column 3) or 18% of the control mean when pooling F2 to F4, and 41 GHC per week (SE: 25) or 30% of the control mean when pooling only F2 and F4 (this latter estimate narrowly misses conventional levels of statistical significance, with a p-value of .103).[16] Part of this income effect is driven by the extensive margin of earning any income at all; the likelihood of reporting positive income in the past week increases by 4 to 5 percentage points (SE: 2, column 4, Panels B and C), or 9 to 11% of the control mean. This suggests that cash during a pandemic actually has a *positive* effect on household labor supply.[17] [18]

The impact on household income persists after the transfers end, with an estimated coefficient of 45 GHC per week (SE: 27), which is actually slightly higher than the pooled impacts during the grant disbursement period. Pooling the household income estimates from follow-up waves 2 to 5, the point estimate is 29 GHC (SE: 16) – 19% of the control mean – with a p-value of .06 (not reported in a table). While noisy, the magnitude of this income effect is remarkable, given that we might ex ante expect the income returns to cash to be lower during a pandemic, given additional constraints to both the demand- and supply-sides of household enterprises. Even more striking, we discuss in Section 4 below that these effects of cash on earned income are substantially larger than previously documented effects of cash on earned income in "normal" times.

Columns 1 to 3 of Table A12 decompose household income into its two underlying components: the number of days the household earned income in the last week, and household income on the most recent day in which it earned money. The contemporaneous income effect appears to be mainly driven by a 0.18 increase in the number of

[16] Here, we focus on *earned* income rather than *total* income, which would include our survey measure of transfers received by the household. We pay less attention to effects on transfers given a concern that respondents in the treatment group sometimes interpreted transfers as including the cash transfers from our intervention. Our intervention caused a statistically significant increase in reported transfers received during our intervention period (columns 4 to 6, Panels B and C, Table A12). But we also see that respondents in the treatment group were significantly more likely to report having received exactly 90 GHC as the value of their most recent transfer ($p < 0.01$, column 1, Table A13; recall that 90 GHC is the size of each installment in our cash transfer program). In addition, the impact of our intervention on transfers received in the last week is concentrated in follow up waves 2 and 4 (columns 2 to 4), for which there was a smaller gap between the date of the survey and the date of our most recent transfer, further bolstering the interpretation that treatment group respondents included our cash transfer in this response. Finally, column 5 presents suggestive, direct evidence that treatment group respondents reported receiving more transfers when the most recent installment from our intervention had come more recently.

[17] Similarly, we see no evidence of negative effects on respondent-level labor supply, which we measure in hours (column 5, Table 3). For the most part 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 6 are driven by non-work time.

[18] To address concerns about differential attrition, Table A8 replicates analysis of the contemporaneous impacts reported in Table 3, but using one observation response per household: the average response across F2, F3, and F4. The point estimate for earned income is 70% as large and remains not statistically significant (column 3). Estimates for other outcomes are virtually unchanged in magnitude and statistical significance. In particular, the positive effect on reporting positive income remains five percentage points, with $p < 0.05$.

days a household earned an income in the last seven days (SE: 0.10), a 9% increase over the control mean. The persistent income effect appears to be largely driven by an increase of 10.3 GHC (SE: 5.7) in the household's income on the last day it earned money.

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.[19]

Finally, we investigate heterogeneity of impacts on income and respondent-level labor supply (columns 3 to 6, Table A11). While we find no evidence of heterogeneous impacts by rural/urban status and food expenditure, we find that female headed households who receive the grant are more likely to have had an income in the last seven days (though we do not find statistically significant evidence of heterogeneous impacts on total income generated). The heterogeneous impact on the extensive margin of income generation is consistent with the finding from the previous section that female-headed households also experienced larger treatment effects on food expenditure.

Table A14 explores heterogeneity of impacts based on occupations as measured in the 2018 Ghana Panel Survey: whether the household has a business, whether the household has a wage worker, and whether the household has a farmer. There is 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 contemporaneous impacts on 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.[20]

3.3. Psychological well-being

There is no doubt that the pandemic 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). We test for this 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). Even considering the most positive estimates found for follow-up surveys 2 to 4, our 95% confidence intervals reject positive effects of 0.11 SD or more. In addition, these nulls do not appear to be influenced by selective attrition: estimated effects are almost identical when using the average F2-F4 response for each respondent (Table A15).

The null effects on psychological well-being do not appear to be due to measurement issues, given several validation checks. First, baseline measures of distress and happiness are strongly predictive of follow-up measures ($\hat{\beta}_0 = 0.36$ for distress, and 0.34 for happiness, both with $p < 0.001$, from the regressions in columns 1 and 2 of Panel B, Table 5). Second, in each of the five follow-up surveys, happiness and distress (before reverse-coding) are strongly negatively correlated (maximum $p = 0.01$). Third, respondents scoring 1 SD higher on the COVID-19 symptoms index are 0.16 SD more distressed ($p < 0.001$), though more surprisingly, 0.03 SD happier ($p = 0.05$). Fourth, in each wave, correlations between the six components of the distress index are always positive. With the exception of the symptoms-happiness correlation, these validation checks suggest that mis-measurement does not explain our null effects on well-being.

Table A16 contains several auxiliary, and non-pre-registered, analyses of contemporaneous effects. First we examine heterogeneity in impacts on depression and happiness, based on baseline levels of each (columns 1 and 2). While we do not find any evidence of heterogeneous impacts by baseline levels of depression, we do see heterogeneous impacts in happiness based on baseline happiness. Surprisingly, for respondents that reported being one point happier at baseline (based on a four point scale), the transfer program caused an increase in happiness of an additional 0.15 (SE: 0.05) points. Counter to our priors, the transfers are a complement to baseline happiness, rather than a substitute, in increasing subsequent happiness.

Columns 3 and 4 of Table A16 present effects on alternative measures of psychological well-being, only measured in follow-up surveys 3 and 4: the participant's assessment of their mental health from 0 (Poor) to 4 (Excellent), and the participant's assessment of where they fall on the "ladder of life," from 0 (the worst possible life) to 10 (the best possible life). Previous studies have found that the ladder

Table 5
Cash transfers do not impact psychological well-being.

	Depression index (-) (1)	Happiness (2)
Panel A:	Anticipation Effects (F1)	
Treatment	−0.31	0.04
	(0.21)	(0.04)
Observations	1,438	1,438
Control Mean	−12	.66
Control SD	4.3	.82
Panel B:	Contemporaneous Effects (F2, F3, F4)	
Treatment	0.12	0.04
	(0.17)	(0.03)
Observations	3,831	3,831
Households	1,460	1,460
Control Mean	−12	.94
Control SD	4.5	.86
Panel C:	Contemporaneous Effects (F2, F4)	
Treatment	0.07	0.04
	(0.18)	(0.04)
Observations	2,505	2,505
Households	1,432	1,432
Control Mean	−11	.89
Control SD	4.3	.85
Panel D:	Persistent Effects (F5)	
Treatment	−0.01	0.03
	(0.26)	(0.05)
Observations	1,353	1,353
Control Mean	−12	1.2
Control SD	4.4	.95

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 outcomes are: (1) Kessler-6 Depression Index (reverse-coded so that higher means less depressed): 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... (0 = Not at all happy, 1 = Not very happy, 2 = Rather happy, 3 = Very happy). *** p<0.01, ** p<0.05, * p<0.1.

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

[20] Figure A5 provides a breakdown of the small businesses in our sample, based on 2 and 4 digit ISIC industry codes, and indicates that the vast majority of businesses in our sample are in retail/trade and manufacturing. While we are not aware of any government mandates directly targeted at retail and manufacturing businesses, several of the broader public mandates to adopt PPE, hygiene protocols, and social distancing were likely constraining on business practices (see e.g. American Chamber of Commerce, 2020). It may be that small business owners were therefore more likely to spend their transfers on investments that allowed them to shift to at-home production to circumvent these constraints.

Table 6
Impacts on social distancing and COVID-19 symptoms.

	Social distancing							Symptoms
	Index (1)	Days at home (2)	Days social gatherings (-) (3)	Keep distance (4)	Days HH home (5)	Days visitors (-) (6)	Worn mask (7)	Index (8)
Panel A:	Anticipation: Before Treatment-Only Transfers (F1)							
Treatment	0.03	0.04	0.05	0.01	0.10	−0.04	−0.01	0.11**
	(0.05)	(0.13)	(0.07)	(0.01)	(0.16)	(0.08)	(0.02)	(0.05)
Observations	1,425	1,438	1,438	1,438	1,428	1,434	1,438	1,438
Control Mean	−.0021	2.4	−.79	.94	3.5	−.68	1.9	−.057
Control SD	.99	2.4	1.4	.24	3.1	1.5	.41	.88
Panel B:	Contemporaneous: Between 3rd and Last Transfer (F2, F3, F4)							
Treatment	0.08*	0.18*	0.04	0.00	0.15	−0.02	−0.01	0.00
	(0.04)	(0.09)	(0.06)	(0.01)	(0.12)	(0.06)	(0.02)	(0.04)
Observations	3,782	3,831	3,831	3,831	3,792	3,814	3,831	3,831
Households	1,453	1,460	1,460	1,460	1,455	1,457	1,460	1,460
Control Mean	−.037	2.1	−1.1	.95	2.8	−.6	1.8	.00024
Control SD	.97	2.3	1.4	.22	2.9	1.4	.48	1
Panel C:	Contemporaneous: Between 3rd and Last Transfer (F2, F4)							
Treatment	0.12***	0.24**	0.06	0.00	0.26**	−0.02	−0.02	−0.01
	(0.05)	(0.10)	(0.07)	(0.01)	(0.13)	(0.07)	(0.02)	(0.04)
Observations	2,475	2,505	2,505	2,505	2,482	2,493	2,505	2,505
Households	1,425	1,432	1,432	1,432	1,427	1,429	1,432	1,432
Control Mean	−.046	2.1	−1.1	.95	2.9	−.61	1.8	.0078
Control SD	.97	2.3	1.4	.23	3	1.5	.46	1.1
Panel D:	Persistence: 8 Months After Last Transfer (F5)							
Treatment	0.05	0.23*	0.01	0.02	−0.12	−0.12	0.02	0.00
	(0.06)	(0.14)	(0.10)	(0.02)	(0.13)	(0.10)	(0.03)	(0.06)
Observations	1,332	1,352	1,353	1,353	1,337	1,347	1,353	1,353
Control Mean	−.0098	2	−1.5	.92	1.7	−.71	1.8	.0074
Control SD	.98	2.3	1.8	.27	2.3	1.5	.53	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 six outcomes in columns 2 to 7, (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) whether wore a mask when near non-family in the past 7 days (0 = No, 1 = At least once, 2 = Always), (8) 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.

of life assessment is more impacted by financial interventions than measures of happiness and mental well-being (e.g. Lindqvist et al., 2020). Consistent with this, we see suggestive positive effects on the ladder of life outcome (0.07σ, $p = 0.13$), but no effect on self-reported mental health (0.04σ, $p = 0.46$). The transfers may then have improved households' perceptions of their success in life, but without spilling over to a more general sense of happiness or a reduction in distress.

Finally, Table A17 explores heterogeneity in our primary well-being outcomes by rural/urban status, gender of the household head, and baseline food expenditure. We do not find statistically significant heterogeneity in impact on these margins.

3.4. 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. Against this backdrop, cash could reduce adherence to social distancing if it is used for in-person transactions, or used to expand household enterprises. Or, cash may increase social distancing by reducing the need to work. It could also 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. 2025), or through reciprocity (Falk, 2007). We test for these possibilities in Tables 6 and 7.

Table 6 presents impacts on our pre-registered components of social distancing, as well as an index combining the components. 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 7).

We find mixed evidence regarding the contemporaneous impact of cash transfers on social distancing. We find an increase in the index combining pre-registered social distancing behaviors, indicating additional adherence to social distancing guidelines among program participants. However, as we return to shortly, we find little evidence for an impact on social distancing among a set of metrics less subject to social desirability bias.

First, transfers increase the social distancing index by 0.08σ (SE: 0.04) when pooling F2 to F4, and by 0.12σ (SE: 0.05) when excluding F3, suggesting that the contemporaneous impact may have been concentrated in the initial weeks after each transfer. Looking at the components of social distancing, the impact is driven mostly by the respondent's 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 3 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. That said, while we find an effect on attending social gatherings in the direction of increased distancing, it is not statistically significant (column 3).

Table 7
Impacts on social distancing, avoiding social desirability bias.

	Days spent outside (1)	Call taken outside (2)	Surveyor guess outside call (3)
Panel A:	Anticipation Effects (F1)		
Treatment	−0.07	−0.03	−0.02
	(0.14)	(0.03)	(0.04)
Observations	1,436	1,438	1,438
Control Mean	3.9	.44	.51
Control SD	2.8	.5	.75
Panel B:	Contemporaneous Effects (F2, F3, F4)		
Treatment	−0.03	−0.01	−0.03
	(0.11)	(0.02)	(0.03)
Observations	3,826	3,831	3,831
Households	1,458	1,460	1,460
Control Mean	3.7	.38	.45
Control SD	2.8	.48	.7
Panel C:	Contemporaneous Effects (F2, F4)		
Treatment	0.04	0.00	0.01
	(0.12)	(0.02)	(0.03)
Observations	2,502	2,505	2,505
Households	1,430	1,432	1,432
Control Mean	3.7	.38	.44
Control SD	2.9	.49	.68
Panel D:	Persistence (F5)		
Treatment	−0.37**	−0.00	0.02
	(0.17)	(0.03)	(0.04)
Observations	1,347	1,353	1,353
Control Mean	3.2	.38	.48
Control SD	2.9	.49	.7

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) number of days spent respondent spent outside the home over the last 7 days, (2) dummy variable for respondent reporting that they took the follow-up survey call from outside their house (rather than inside), (3) the surveyor's guess of where the respondent took the call (0 = At home, 1 = Outside home in a private place, 2 = Outside home in a public place). *** p<0.01, ** p<0.05, * p<0.1.

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. Finally, we do not see an effect on whether the respondent has worn a mask when near non-family members in the past week (column 7).[21]

While cash transfers appear to increase contemporaneous social distancing (measured by the index), 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).

Given that our pre-registered measures of social distancing are self-reported, one concern is that the positive effects we observe 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 emphasized by government directives, like attendance of social gatherings. We do not see this.

The above notwithstanding, we do not find contemporaneous effects on social distancing when using three non-pre-registered measures with reduced concerns of social desirability bias (Table 7). The first of the three measures is the most important: in the survey section on labor and employment, several questions before the social distancing section, we asked respondents about the number of days they spent outside of the home. This is essentially the inverse of the question reported in Column 2 of Table 6 – one of the main outcomes for which we find statistically significant impacts.[22] However, in this instance, it may be less prone to social desirability bias as it was asked within the context of employment questions rather than explicitly as a measure of adherence to social distancing protocols. Our second measure is whether the respondent reported taking the follow-up survey call outside of their home, which may be less prone to social desirability bias as again the question was not part of a section explicitly connected to "social distancing," and because manipulating the response to this question would require explicitly lying about a respondent's current location. Our final measure of social distancing behavior is less prone to social desirability bias as it is not self reported – it is a variable indicating the surveyor's best guess for whether the respondent took the call from their home, coded as a 0, from a private place outside of their home, coded as a 1, or from a public place, coded as a 2. Across all of these measures we find no anticipation, contemporaneous, or persistent effects of the transfers, with the exception of a reduction of 0.37 days (SE: 0.17) spent outside of the home in the past week for the fifth follow-up survey taking place 8 months after the transfers concluded (Panel C, column 1). This significant effect suggests that the transfers may have persistently increased social distancing, a finding we do not see using our pre-registered measures of social distancing.

On net, we remain agnostic as to whether the cash transfer program increased adherence to social distancing protocols. Reassuringly, however, we find no evidence that cash transfers *reduced* adherence to social distancing protocols.

If the cash transfers *did* increase adherence to social distancing protocols, this 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 6. 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 again a form of social desirability bias – respondents unsure that they would continue to receive transfers could report more symptoms.[23]

[21] To address concerns about differential attrition, Table A18 replicates analysis of the contemporaneous impacts reported in Table 6, using each respondent's average answer given across F2 to F4. Most importantly, the effect on the social distancing index and its statistical significance is identical. Estimates on individual components are also near-identical, though the coefficient on days spent at home falls from 0.18 to 0.16, and is no longer statistically significant at the 10% level.

[22] Specifically, that survey question asked respondents "In the past 7 days, on how many days did you... Stay at home all day (without going out at all)." The survey question in the labor and employment section, which we consider to be less subject to social desirability bias, asked "How many days did you spend outside the home over the last 7 days?" The answers to the two questions are strongly negatively correlated (coefficient: −0.45, SE: 0.01, from a regression that pools F1 to F5 and includes wave fixed effects, clustering standard errors at household-level).

[23] Table A19 reports impacts separately for each of the ten components of the symptoms index. In the anticipation wave, the three reported symptoms

Table A20 tests for heterogeneous treatment effects in our pre-registered measures of social distancing by rural/urban status, whether the household has a female head, and low/high food expenditure at baseline. Of 48 interaction terms, only one is significant, and only at the 10% level. We conclude that impacts on social distancing and symptoms are not meaningfully heterogeneous by these three dimensions.

In Table A14 we found that households with a business were more likely to increase at-home working hours, perhaps because they used the investment to facilitate a transition to at-home operations. Consistent with this, Table A21 tests for heterogeneity in contemporaneous impacts on social distancing by occupation, and confirms that households with a business have a significantly larger increase in social distancing in response to the transfers. In fact, the table suggests that households with only a farmer or only a wage earner do not exhibit increases in social distancing at all.

3.5. Beliefs about COVID-19 and religious practices

In principle, cash transfers may substitute for existing coping mechanisms employed during the pandemic: whether motivated beliefs that COVID-19 is not particularly harmful (Bénabou and Tirole, 2016; Engelmann et al., 2024), or investments in religious beliefs and practices (Sinding Bentzen, 2019; Bentzen, 2021). We test for these ideas in Table A22.

We do not see any evidence that the cash transfers substituted for the coping mechanisms of motivated or religious beliefs. In column 1, 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.[24] 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.

Turning to religious beliefs, contemporaneous transfers actually somewhat increase the frequency of prayer (Panels B and C, column 3). 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 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. In any case, we do not find a similar increase in respondents' likelihood to read scripture, or any effect on belief in the "prosperity gospel" (columns 4 and 5).[25] [26]

Finally, Table A25 reports impacts of the transfers on respondents attitudes toward various COVID-19-policies: whether people should cancel gatherings, whether people should refrain from shaking hands, whether non-essential shops should be closed, whether the government should impose a general lockdown, and whether the government's general reaction to the COVID-19 pandemic has been sufficiently extreme, coded such that higher values indicate a desire for more extreme policy responses. We find little evidence that transfers moved policy attitudes in an anticipatory, contemporaneous, or persistent manner. Across all panels, the treatment effect on an index aggregating these attitudes is small and not statistically significant. Across the individual outcomes, we find a statistically significant anticipatory effect, indicating that respondents who expected to receive more transfers believed the government should have a less extreme policy reaction to the COVID-19 pandemic (Panel A, column 6), and we find a contemporaneous 3 percentage point increase (SE: 2) in the likelihood that transfer recipients support general lockdowns (Panel B, column 5). Across all other outcomes we find no statistically significant impacts.

3.6. 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, working hours, savings, and depression symptoms in Table 8. These outcomes are chosen to parallel the main phone follow-up outcomes reported above, to the extent possible. The typical difference is that outcomes are measured more carefully in the Ghana Panel Survey than in our phone-based follow-up surveys. For instance, food expenditure in the Ghana Panel is measured by aggregating reported spending over the past month on roughly 94 food types. In contrast, we measured food expenditure in our phone follow-up surveys with two questions: "How many days did your household spend money on food over the last 7 days?" and, "What was the total amount spent on food on the most recent day on which food was purchased?" Earned income is similarly measured in a more comprehensive manner in the Ghana Panel Survey. Appendix D lays out all key outcome variable definitions.

Two-year impacts are not statistically significant, with the exceptions of consumption and depression. Treatment reduces consumption by roughly 7% ($p = 0.05$).[27] This effect is driven by non-food consumption; we estimate a precise null effect on food consumption (rows 2 and 3, Table 8). Along with a drop in consumption, we also find a statistically significant deterioration in the depression score of the household head of 0.13 to 0.14σ (the final two rows). We might expect this decline in mental health to be connected to the drop in consumption, though as we discuss below, the two impacts are driven by different subpopulations.

To unpack the surprising negative effect on consumption, we estimate effects on the underlying components of consumption in Table A26. A drop in health expenditures drives roughly one third of the drop in overall consumption expenditure, and a drop in miscellaneous expenditures covers roughly another 20%.[28] Optimistically, it might be that the reduced health expenditure reflects a lower prevalence of COVID-19 symptoms (or even of long-COVID), due to increased social distancing; or due to lower expenditures on child health, due to nutritional benefits of higher food expenditure while cash transfers were ongoing. However, we estimate null effects of cash transfers on health outcomes at the two-year point (Panel A, Table A28), and the reductions in health expenditure are primarily driven by spending

that are significantly impacted by the transfers are the incidence of dry cough, both for the respondent and for members of their household, and loss of taste for the respondent (columns 2, 4, and 7). While loss of taste is strongly indicative of COVID-19, a dry cough could derive from many common illnesses and assessing whether one has a dry cough may require more subjective judgment from the respondent. Thus it may be more prone to distortions due to social desirability bias, though of course this is speculative.

[24] 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%. Control mean perceived fatality rates range from 11 to 18% (Table A22).

[25] We do not find any consistent pattern of heterogeneous treatment effects on COVID-19 beliefs or religiosity (Table A23).

[26] To address concerns about differential attrition, Table A24 replicates analysis of the contemporaneous impacts reported in Table A22 using household-level averages across F2 to F4. Estimates are virtually unchanged.

[27] The treatment effects we observe are not due to differential timing of the surveying of treatment and control households – we cannot reject the null that they were surveyed at the same time (Figure A6).

[28] Table A27 provides a breakdown of the impacts on the main components that comprise the miscellaneous category. The categories with significant negative effects of treatment are barbers and beauty shops, communications (phone, email etc.) and owner-occupying housing rent, although individually each of these reductions is very small.

Table 8
Two-year impacts of COVID-19 cash transfers.

	Treatment effects (SE) (1)	Control mean (2)	Control SD (3)	observations (4)
Consumption (weekly, GHC)	−36.54** (18.30)	540.65	372.04	1,408
Food Consumption (weekly, GHC)	1.54 (8.80)	252.54	164.58	1,408
Non-Food Consumption (weekly, GHC)	−39.54*** (12.30)	285.35	260.63	1,408
Income Aggregate (weekly, GHC)	−3.17 (72.73)	451.11	1197.13	1,408
Earned Income (weekly, GHC)	3.10 (72.06)	415.52	1179.53	1,408
Any Income (0/1)	0.003 (0.02)	0.80	0.40	1,408
HH Head Work Hours (weekly)	0.89 (1.91)	26.28	34.70	1,408
Transfers (weekly, GHC)	−0.42 (3.29)	30.38	61.35	1,408
Any Savings (0/1)	−0.013 (0.03)	0.58	0.49	1,406
Savings Amount (GHC)	−79.69 (110.87)	866.60	1984.74	1,406
Mobile Money Balance (GHC)	−15.69 (13.20)	91.62	257.83	1,406
Kessler 10 Depression (-)	−0.54 (0.34)	−17.29	5.88	1,393
Kessler 6 Depression (-)	−0.35 (0.22)	−10.65	3.86	1,393
Kessler 10 Depression HH Head (-)	−0.84** (0.41)	−17.40	6.16	1,232
Kessler 6 Depression HH Head (-)	−0.52* (0.27)	−10.70	4.08	1,232

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, and control 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. When missing (which is only rarely), these lagged dependent variables are set to the mean. Consumption is total weekly household consumption in Ghanaian Cedis. Food Consumption is total weekly household food consumption valued in Ghanaian Cedis (including food purchased, produced, or received as a gift). Non-Food Consumption is Consumption minus Food Consumption. 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). Any Income is a dummy variable equal to one if Earned Income is positive. HH Head Work Hours is the estimated weekly working hours of the household head. Transfers is weekly household transfers of income received from persons and organizations outside of the household. Any Savings is a dummy variable equal to one if the household has any savings, while Savings Amount is the total amount saved, and Mobile Money Balance is the current mobile money balance. Kessler 10 is a depression score summed across 10 symptoms, reverse-coded such that a higher number reflects less depression. 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 follow-up phone surveys. The HH Head version of each measure is the score for just the household head, more closely following the measurement in our phone follow-up surveys. All outcomes other than Any Income and the depression measures are winsorized at the top 1%. Income Aggregate and Earned Income are also winsorized at the bottom 1%, given the possibility of large negative outliers. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

on adults, not spending on children (Panel B). On the other hand, consistent with the optimistic hypothesis, we do see that the health expenditure reduction is primarily driven by a reduction in health expenses for illness, as opposed to spending on preventative care or injury (Panel C).

The welfare effects of negative consumption may depend on their seasonal timing. Could the negative impacts be concentrated during the lean season? This does not seem to be the case. We follow (Breza et al., 2021) in using the fraction of households that have earned zero income recently to proxy for lean season periods. Households who were surveyed later in the fourth wave of the Ghana Panel Survey are significantly less likely to have earned an income in the last month (Panel A, Figure A7). This relationship is similar in the north and south of Ghana (Panel B), despite differences in agricultural seasons (Fig. 1). Panel C demonstrates that this impact is driven by rural areas — there is no such relationship in urban areas. These facts suggest that we can use the lateness of the Wave 4 survey date to proxy for lean seasonality. Correspondingly, Table A29 investigates heterogeneity in the impacts of the transfers on our main outcomes of interest, based on the date at which households were surveyed in the fourth wave of the Ghana Panel Survey. We find no evidence of heterogeneity for effects on consumption, income, and savings, and only suggestive evidence of heterogeneity for depression: with more positive effects during lean season ($p < 0.1$).

Next, we investigate heterogeneity in two-year impacts by region. Table A30 subdivides the impacts into urban Accra (Ghana's capital city, comprising roughly 10% of the sample; Panel A), urban areas other than Accra (Panel B), and impacts in rural areas (Panel C).[29] This analysis reveals that the negative impacts on consumption are largely driven by urban Accra. The negative impact of the transfers on consumption in Accra is large – GHC 101 (SE: 43) for non-food expenditure, and GHC 41 (SE: 25) for food expenditure – 22% and 13% reductions relative to the control mean. Though noisy, we also find a large positive point estimate on savings for the urban Accra sub-sample — taken literally, our point estimate suggests a 37%, or 0.2σ increase in savings relative to the control group. One possible explanation for this result then is that our intervention heightened sensitivity to

[29] Note that we drop strata fixed effects from these regressions, as the subsamples are sufficiently small that we lose variation in treatment within strata, and strata fixed effects are not necessary for identification.

and concerns about the pandemic, leading to more social distancing, reduced discretionary spending, fewer trips to crowded health clinics, and higher savings, with these effects most pronounced in urban Accra where population density is highest. Consistent with this interpretation, we also find that urban Accra drives our positive contemporaneous effects on social distancing, and in those contemporaneous surveys we even see suggestive evidence of negative effects on consumption for those in urban Accra (Table A31). However, ultimately we are not able to confidently pin down a mechanism for the negative impact on consumption.

In contrast to the consumption effects, we find that the negative impacts on psychological well-being are similar in urban Accra and urban areas outside of Accra (Panel B). This suggests that any negative impacts on psychological well-being are not likely to be driven by reduction in consumption expenditure, as we do not observe a reduction in consumption expenditure for urban areas outside Accra. Instead, we find that the negative mental health effects are driven by female-headed households (column 6, Table A32). These households are not the ones driving the drop in expenditure (columns 1 and 2), indicating that these two results are likely not to be directly related. Given that female-headed households had significantly larger contemporaneous effects on food expenditure and employment (columns 1 and 4, Table A11), one possible interpretation of the long-term negative effect on mental health is a reference-dependence type effect: where the removal of much-appreciated transfers leads to lower mental health later, as households feel a loss relative to the reference point of their past selves.

Negative impacts of cash on psychological well-being are rare (Crosta et al., 2024), though one point of comparison is Baird et al. (2025). They find negative well-being effects of cash transfers targeted to adolescent Ugandan girls at risk for depression. Unlike our intervention, their transfers were disbursed shortly before the COVID-19 pandemic, and the authors suggest the mechanism may be that recipients were disappointed that they needed to allocate their grants to meet pandemic needs rather than allocating the grant as they originally intended. That mechanism would not apply in our setting, as our cash transfers were distributed after the onset of the pandemic, and explicitly framed as assistance for coping with the pandemic. However, that mechanism could be clubbed with the one we suggest above, under the header of "frustrated expectations."

Taken together, in our two-year follow-up data we find evidence of a drop in consumption, although this is concentrated in only the 10% of our sample residing in urban Accra. We also find evidence of a drop in psychological well-being for household heads, driven by female-headed households. That the negative impact on consumption is driven by a different subsample than the negative impact of psychological well-being of household heads suggests that the two results may be independent phenomena. We do not have a definitive explanation for these results — we leave this as a topic for further research.

3.7. Heterogeneity in impacts by disease prevalence and beliefs about the pandemic

In this section we consider heterogeneity of impacts by the severity of the pandemic in terms of disease prevalence, and by two dimensions measuring how severe people believed the COVID-19 pandemic would be for public health and the economy. In doing so, we may inform policy responses for future public health crises based on their severity and perceptions thereof.

3.7.1. Heterogeneity by COVID-19 prevalence

We first examine the heterogeneous impacts of our cash transfers by the intensity of the COVID-19 pandemic as measured by district-level disease prevalence.[30] We utilize our symptoms index, aggregating a respondent's self reported experience of fever, dry cough, difficulty breathing, loss of taste, and whether they sought medical care, reported both for the respondent themselves as well as anyone in their household. As an alternative measure of COVID-19 intensity, we utilize the fraction of respondents who report having lost their sense of taste, as this is one of the most distinctive symptoms of COVID-19.[31] We repeat our main regression analysis, with the treatment indicator interacted with a district-level measure of COVID-intensity, for contemporaneous effects on each of our five main outcomes of interest: food spending, our social distancing index, total earned income, an indicator for whether the respondent earned any income, and the Kessler-6 depression index.[32]

The heterogeneity term in Panel A of Table A34 corresponds to the average symptoms index in a given district, measured from follow-up surveys 2 to 4, and in Panel B corresponds to the fraction of respondents that report having lost their sense of taste in a given district, once again averaged over follow-up surveys 2 to 4. For both measures, we find strong evidence that the positive impact of our transfers on food expenditure is concentrated in areas with high COVID-19 prevalence (column 1), and that these tend to be districts with lower food expenditure in the control group (see coefficients on Symptoms Index and on Loss of Taste). The estimate in Panel A indicates that a one standard deviation increase in the district-level average symptoms index – 0.43 – corresponds to an additional GHC 25.9 in weekly food expenditure, which is about 28% of the transfer amount. Similarly, the estimate in Panel B indicates that a one standard deviation increase in the district-level average loss of taste index – 0.07 – corresponds to an additional GHC 30.3 in weekly food expenditure, or about 34% of the transfer amount. Both interaction terms are statistically significant at the 1% level.

We do not find statistically significant heterogeneity across our other outcomes of interest. Nevertheless, as food expenditure is one of the primary outcomes impacted by the cash transfer program, these results indicate that the cash transfers were more effective in areas more impacted by COVID-19.

3.7.2. Heterogeneity by beliefs about pandemic severity

We next examine heterogeneous treatment effects by baseline beliefs about the severity of the pandemic. To elicit severity in beliefs about the public health consequences of COVID-19, at baseline we asked what fraction of people who contracted the illness would ultimately die. Heterogeneity by this belief in the impacts of transfers on our main outcomes are presented in Table A35, with contemporaneous impacts presented in Panel A and persistent impacts, as of follow-up 5, in Panel B. For contemporaneous impacts, we find that those who believed the health consequences to be more severe were statistically significantly less likely to social distance as a result of our transfers (column 2). This interaction effect can be rationalized by the fact that those with more pessimistic beliefs are already socially distancing more to begin with (see coefficient on the level term in Table A35).

Otherwise, those with pessimistic beliefs are statistically significantly more likely to generate an income as a result of our transfers (column 4), in contrast to the hypothesis that the pessimistic may be more cautious to use the cash to operate businesses and the like. We do

[30] A complementary analysis might look at heterogeneous impacts by district-level measures of lockdown intensity. We are unable to conduct such an analysis, as there were no lockdowns in place during our study. See Section 2 for further discussion.

[31] We considered using COVID-19 case count data from the Ghana Ministry of Health to conduct this analysis, downloaded from https://data.humdata.org/dataset/ghana-coronavirus-covid-19-subnational-cases?force_layout=desktop on June 18, 2025. However, we find that these case count data tend to be *negatively* correlated with our data on self-reported COVID-19 symptoms, both when using only cross-sectional (across-district) variation, and when using variation across time (within-district, Table A33). We believe, therefore, that these case data are unreliable and may be more indicative of a district's health capacity and therefore their ability to record COVID-19 cases in the official statistics.

[32] Our data include 166 of the 216 districts spanning Ghana.

not find statistically significant heterogeneity on other outcomes, nor do we find heterogeneity in the persistent impacts of the transfers.

The second dimension of heterogeneity we study is how severely the pandemic would affect the economy. Specifically, at baseline we elicited this belief on a four point scale, with 1 corresponding to “not at all”, and 4 corresponding to “extremely so.” Heterogeneity in impacts by this belief is presented in Table A36. In the contemporaneous survey waves, those with more severe concerns about the economy were more responsive to the cash transfers in terms of social distancing. As with the results on fatality rate beliefs, this heterogeneity can be rationalized by the fact that those with more severe concerns are socially distancing less to begin with, perhaps because they perceive a greater need to work to compensate for a loss of economic activity.

Surprisingly, in the follow-up 5 data, we find that those same individuals were *less* responsive to the cash transfers in terms of social distancing. One possibility is that this group experienced a “backlash” in terms of social distancing, once they realized their worst fears about the trajectory of the economy did not materialize. We do not find statistically significant heterogeneity for other outcomes.

4. A comparison to cash transfer programs in non-pandemic times

How do our estimated impacts compare to the impacts of cash transfers outside of pandemics? To assess this question, we rely on estimates drawn from two recent meta-analyses of unconditional cash transfer RCTs: Crosta et al. (2024), which aggregates 115 studies of 72 distinct transfer programs in low and middle income countries, looking at effects on a broad set of outcomes; and Kondylis et al. (2024), which aggregates 17 RCTs to study effects on consumptions.

The results of the Bayesian meta-analysis conducted in Crosta et al. (2024) allow us to contextualize the impacts we find on expenditure, income, labor supply, and psychological well-being. We focus on estimates drawn from cash transfer programs that, like ours, involved streams of payments over several installments rather than a single lump sum. There are 77 studies of such programs in Crosta et al. (2024). The median monthly transfer amount in these stream programs is US$35 PPP/month. Our transfers were approximately US$42 PPP every 3 weeks, or approximately US$56 PPP/month.[33]

First, examining contemporaneous impacts on food expenditure, we find that our estimates are largely in line with prior studies in the literature. For all of the following estimates on the contemporaneous impacts of cash transfer schemes, we draw on the relevant estimates from Crosta et al. (2024) contained in Table 3, Panel B. Rescaling those estimates to a cash transfer scheme of US$ 56 PPP/month (we use $ rather than US$ PPP from now), the average effect size on food expenditure is $26.24, with a 95% credibility interval spanning $19.36 to $33.60. Our estimate of 12.19 GHC/week implies a monthly food expenditure of $23.33/month – nearly at the center of the credibility interval. Moreover, the estimates from Crosta et al. (2024) on total expenditure are nearly the same as those for food expenditure, implying very low spending on non-food expenditure, just as we find.

Turning to income, our estimates, though imprecise, far outrange the average impacts arising from the analysis of Crosta et al. (2024). Again rescaling the estimates to a cash transfer scheme of $56/month, the average effect size on monthly income is $13 with a 95% credibility interval spanning $8.80 to $17.60. In contrast, our estimates of 25 to 41 GHC/week (column 3, Panels B and C, Table 3) imply an increase in monthly income of $48.70 to $77.80 – substantially above the average estimates from reference studies, albeit where the 95% confidence interval for our estimates includes zero (only our follow-up 5 estimate, and our estimate from pooling impacts from follow-up surveys 2 to 5 reach statistical significance, at the 10% level.)

Next we consider labor supply, where once again our estimates are more in line with estimates from Crosta et al. (2024). Rescaling their estimates as above, the average effect size on labor force participation is 9.1 percentage points, with a 95% credibility interval of 3.4 to 15 percentage points. The closest estimate from our study is on the binary outcome of whether the respondent earned any income in the last week, which increased by 4 to 5 percentage points (column 4, Panels B and C, Table 3), toward the low end of the credibility interval of existing studies.

Regarding psychological well-being, Crosta et al. (2024) finds the average effects of (rescaled) monthly transfers on psychological well-being to be 0.33σ. Our null effects then differ considerably from the average effects found in previous studies. However, the estimates of previous studies are nevertheless highly variable across contexts—(Crosta et al., 2024) finds the Bayesian pooling factor to be low for effects on psychological well-being, lower than the pooling factor for any other outcome domain considered. Relatedly, of the 24 estimated well-being effects of streams of transfers that they consider, eight have a t-statistic below two — i.e. no statistically significant effects on psychological well-being, like ours, are not uncommon, and thus do not necessarily tell us that cash transfers during pandemic times are less effective for improving well-being than during regular times.

As a final note on contemporaneous impacts, Crosta et al. (2024) find that the transfer programs targeted to women have statistically significantly larger impacts on food expenditure than do non-targeted programs, mirroring our findings on expenditure.

Perhaps our largest point of contrast to existing work is in the persistence of impacts. Crosta et al. (2024) finds evidence of significant persistent impacts, defined as any outcome measured after the transfers ceased – mostly 12 to 48 months after the conclusion of the transfers. Scaling the estimates from their Table 4, Panel B as above, their estimates imply persistent effects of $28.50/month on household consumption (95% credibility interval: 13.1 to 44.5), $10.30/month on household income (95% credibility interval: 0.5 to 20.7), and 0.06σ on psychological well-being (95% credibility interval: −0.17 to 0.34). Similarly, Kondylis et al. (2024) aggregate the evidence from 17 RCTs, finding that on average cash recipients consume 30 to 35% of their cash transfer on an ongoing basis, and that the consumption estimates are highly persistent, diminishing by about 8% each year after the stream of transfers has ended.

Turning to our estimates, we find a large persistent impact on income in our 8 month follow-up — scaling the estimate in column 3, Panel E, of Table 3 we find an impact of $86/month, significant at the 10% level. However, we fail to find persistent impacts on other dimensions in the 8 month follow-up. And in our two year follow-up, we no longer see persistent impacts on income or any key outcome variables, except for a statistically significant *decrease* in consumption and well-being of the household head.

What explains our relatively large contemporaneous impact on income that also exhibits less persistence than findings from much of the rest of the literature? While we can only speculate, one possibility is that the pandemic opportunities for investment that were high-return but relatively context specific — for instance the ability to purchase masks or other personal protective equipment that allow one to work productively outside of the home, or the ability to pivot a business to staples or consumables that are especially in demand during the pandemic. Catering ones income-generating activities to those opportunities during the pandemic may have yielded high returns that did not persist when the economy largely reverted to its original state.

[33] To reach these figures we applied the average GHC PPP conversion factors for 2020 (2.03) and 2021 (2.15), drawn from https://data.worldbank.org/indicator/pa.nus.ppp?end=2024&locations=GH&start=1990&utm_source=chatgpt.com&view=chart, accessed July 23, 2025.

5. Discussion

Our results shed light on the impacts 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 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 had meaningful contemporaneous impacts. 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. The transfers increased savings, and had large, though noisily estimated, positive impacts on income. We find mixed evidence about the transfers' impact on social distancing, but importantly we do not find any evidence that transfers reduced social distancing. And we find no contemporaneous impacts on psychological well-being.

Our contemporaneous impacts can be largely explained by a story whereby households used our transfers to invest in income-generating opportunities, increasing contemporaneous income and expenditure. We find evidence that households with businesses used the cash transfers to shift to at-home operations, and consistent with this result we find that households with businesses also drive the impacts we observe on our pre-registered index of social distancing behaviors.

We find no evidence for a persistent impact on food expenditure, income generation, or social distancing. We do find a puzzling drop in non-food expenditure and psychological well-being, driven by different sub–samples of our data. We offer above some speculative explanations for these results, but ultimately we leave a definitive explanation of these persistent results for future research.

Our results suggest that cash relief during a pandemic can bolster the economic well-being of recipients without deteriorating adherence to public health protocols; although with signs of negative long-term effects. Mobile money transfers are 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.

CRediT authorship contribution statement

Dean Karlan: Writing – original draft. **Matt Lowe:** Writing – original draft. **Robert Osei:** Writing – original draft. **Isaac Osei-Akoto:** Writing – original draft. **Benjamin N. Roth:** Writing – original draft. **Christopher Udry:** Writing – original draft.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: The authors report that financial support was provided by IGC, The Abdul Latif Jameel Poverty Action Lab, and IPA. Given Dean Karlan's role as a co- editor at JDE, he had no involvement in the peer review of this article and had no access to information regarding its peer review. Full responsibility for the editorial process for this article was delegated to another journal editor. Otherwise, the authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2025.103657.

Data availability

Replication code and data are available at https://doi.org/10.5281/zenodo.17246896.

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