

The Dynamic Effects of Cash Transfers to Agricultural Households[†]

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Little is known about the evolution and persistence of the effects of one-time cash transfers, especially in rural agricultural settings with limited productive investment opportunities. We use bimonthly phone surveys to estimate dynamic impacts for cash transfer recipients in Liberia and Malawi. We find immediate increases in food security that attenuate over time but do not entirely dissipate even 1.5–2 years later, driven by increased farm investments and production. We find increases in farm profits, reductions in casual off-farm labor, improvements in psychological well-being, and, in Liberia, a reduction in intimate partner violence (IPV). (JEL I31, I32, J43, O12, O13, Q12)

While cash transfers will likely increase the contemporaneous consumption of any normal good, evidence on how these effects evolve over time and whether they persist over the long term is limited. This is particularly relevant for time-limited unconditional cash transfers (UCTs), because realizing sustained and meaningful consumption impacts from temporary infusions of cash necessarily requires making investments in income-generating activities (in contrast to regular transfers, such as universal basic income or disability payments, which will mechanically increase disposable income in every period). It is not obvious, however, if

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such productivity-enhancing opportunities exist for the average rural agricultural household in a developing country.

Evidence from prior experiments on the impact of cash transfers on productive investments is mixed. Several studies show sizable effects on income, at least one to two years postdisbursement, especially when the transfers are targeted around income generation. For example, Blattman, Fiala, and Martinez (2014, 2020) document short- but not long-term effects of cash grants (intended for business investment) that were allocated to groups that had submitted business plans, as do Brudevold-Newman et al. (forthcoming) in the context of cash grants (and business training) among applicants to an entrepreneurship program targeting young women in Nairobi. McIntosh and Zeitlin (2022) document substantial effects over 14 months among underemployed youth in Rwanda. On the other hand, Baird, McIntosh, Özler, and Pape (2024) find that a UCT program designed around business plans increased assets but had no effect on income or consumption in the short or long run in Tanzania. A fairly large literature on cash-drop experiments among existing entrepreneurs shows sizable effects on both investment and profits,¹ and De Mel, McKenzie, and Woodruff (2012) show that these effects persist even five years after disbursement.

However, studies with a more representative sample of individuals in various countries in sub-Saharan Africa who live in rural areas where the primary occupation is farming show more heterogeneous results. Haushofer and Shapiro (2016) show sizable short-run (nine months) effects on assets, income, and consumption, with the latter two impacts having largely dissipated three years after the transfers (Haushofer and Shapiro 2018). Other studies such as Egger et al. (2022) show large gains in assets, but a modest and statistically insignificant effect on income 9–31 months after the first transfer installment. In a multiarmed intervention in Uganda, Sedlmayr, Shah, and Sulaiman (2020) find that respondents receiving cash alone have higher assets but no higher income and consumption relative to a pure control group. There are also a couple of studies that have worked exclusively with farmers. Karlan, Osei, Osei-Akoto, and Udry (2014) find that those in the cash grant arm (average grant amount of \$420) invested more in chemical fertilizer but did not increase total farm investment and saw a small, statistically insignificant increase in harvest output. Beaman, Karlan, Thuysbaert, and Udry (2023) provided cash grants of about \$140 just before planting in one of their experimental arms. Eighteen months later, grant recipients invested more in agriculture, harvested more output, and increased consumption; however, these effects had petered out by the time of their long-term follow-up (though this occurred quite some time—seven years—later). Overall, while the evidence is fairly conclusive that large cash transfers cause households to invest in assets, it is less certain whether income and consumption increase, and even if they do, the time path of these effects is entirely unclear. Also note that in many of these studies, the asset outcome spans productive assets as well

¹ A partial list of papers on this topic include De Mel, McKenzie, and Woodruff (2008); McKenzie and Woodruff (2008); Fafchamps, McKenzie, Quinn, and Woodruff (2014); McKenzie (2017); Bernhardt, Field, Pande, and Rigol (2019); Hussam, Rigol, and Roth (2022); and Crépon, El Komi, and Osman (2024).

as durable consumption goods.² It is possible, therefore, that the observed increases in assets are largely driven by consumption goods, and if so, that may partly explain why income gains have been short-lived at best.

In this paper, we combine detailed information on productive investments with data on consumption, income, and expenditures collected from high-frequency surveys to measure the evolution of effects over time. In particular, we implemented high-frequency phone surveys that continued until an in-person endline survey, conducted 1.5–2 years after the transfers, to study the persistence and dynamics of large, lump-sum UCTs among approximately 6,000 households in rural Liberia and Malawi. In this way, we broaden our understanding of the effect of cash transfers beyond the standard protocol of collecting a single follow-up (or, in some cases, a small number of follow-ups). Moreover, we are also able to measure the accumulated effects over the period immediately following the transfers, which would naturally be the window during which the response to the transfers would be the largest. A temporally distant endline would tend to miss these effects, underestimating the short-term benefits from cash transfer programs.

The context of our evaluation is heavily agrarian and the respondents are extremely poor: Monthly household expenditures in 2019 purchasing power parity (PPP) dollars are only \$120 in Liberia and \$92 in Malawi,³ putting these households well below the global poverty line.⁴ These households earn their livelihood primarily from agriculture: At baseline, 99 percent of households in Malawi and 88 percent in Liberia report engaging in agriculture. While about half the households also report earning supplemental income from other sources, only 20 percent earn income from nonagricultural self-employment. On average, at baseline, the unconditional value of nonagricultural income in the prior month was only \$16.16 PPP in Liberia and \$15.28 PPP in Malawi (of this, \$9.39 and \$5.14 was from self-employment)—that is, around 15 percent of household consumption. Thus, realizing sustained gains in this context will require either productivity increases in agricultural production or the creation of new businesses.

Since our study set out to measure dynamics, we randomly selected 20 percent of study households to participate in a bimonthly phone panel survey. These surveys took about 40 minutes to complete and included questions on food security, income, consumption, savings, credit, transfers, and related topics. To enable participation, we provided households with phones and incentivized them to participate in each call. The surveys began as soon as the cash was disbursed and continued for about

²For example, in Haushofer and Shapiro (2016), nonland assets include livestock, furniture, agricultural tools, radio, TV, roof, savings, and “other” assets; in Egger et al. (2022), assets are made up of livestock, transportation (bicycles, motorcycles, and cars), electronics, farm tools, furniture, other home goods, and net savings.

³According to the CIA’s World Factbook, in 2021, the two countries were ranked 222 and 220 (out of 229) in terms of GDP per capita.

⁴These consumption numbers are comparable to those of the ultrapoor populations studied in recent papers about multifaceted “graduation” programs such as in Bandiera et al. (2017) and Balboni et al. (2022), in Bangladesh (\$52 PPP in 2007) or the six countries in Banerjee et al. (2015) (average \$79 PPP in 2010). These households are, in fact, poorer than those in ultrapoor studies in conflict settings such as Afghanistan (assuming a five member household; documented by Bedoya et al. 2019—\$435 PPP in 2015) and Yemen (Brune, Karlan, Kurdi, and Udry 2022—\$616 PPP in 2010). One study with substantially poorer households is Angelucci, Heath, and Noble (2023) in the Democratic Republic of the Congo (\$25 PPP in 2017).

one year in Liberia and two years in Malawi.⁵ Compliance with the survey was high, though substantially better in Malawi (which had a more advanced cell phone network at the time): We completed about 88 percent of surveys in Malawi and 62 percent in Liberia, and in both countries, compliance was balanced between the treatment and control groups. In addition, we conducted a much longer ($\sim$ three hour) in-person endline survey 18–25 months postdisbursement, which included many more questions, as well as modules on outcomes that do not change at a high frequency, such as agriculture, psychological well-being, and IPV. We use these surveys to both measure the persistence of those effects measured in the phone surveys (and corroborate them with a larger sample), as well as to use the additional questions to examine other outcomes.

Using our high-frequency data, we find that in Malawi, food security improves by about 0.5 standard deviations (SDs) immediately posttransfer but then diminishes to about 0.2 SDs within six months. However, the effect size remains at this elevated level for the remainder of our study period, suggesting lasting effects. By the endline, the measured effect size is 0.11 SDs. In Liberia, dynamics are less apparent; instead, we observe a steady and persistent effect (of about 0.25 SD), both in the phone surveys and in the endline survey.⁶ Yet while we see lasting impacts on food security, this effect is not driven by market transactions: We see a fleeting increase in expenditures, including on large purchases such as durable goods (and this too, in Malawi only), but this effect vanishes within about six months and is, therefore, entirely absent at endline. We see modest effects on nonagricultural income, net savings, and interpersonal transfers throughout the survey period, even when we aggregate results over the full study period.

What accounts for these sustained gains in food security? Our evidence suggests that they are driven by increased agricultural production. In the endline survey, we asked about production decisions for the agricultural season following the transfers and find evidence of substantial effects on agricultural output: The value of harvest output increased by about 35 percent in Liberia and 16 percent in Malawi, and our evidence shows that two-thirds of this increased output is retained for household consumption in Liberia, and one-third is retained in Malawi. In addition, households in both countries report growing a larger number of crops. This increase is driven by investments in both capital and intermediate goods: Input purchases go up by about 25 percent in both countries (although statistically insignificant in Liberia, a context with extremely underdeveloped agricultural input markets) and respondents in the treatment group increased investment in farm tools and in irrigation. There is evidence of more labor being hired on the farm, and this increase is more than 50 percent of the control mean in Liberia. Taken together, our data suggest that farm profits went up by about 33 percent in Liberia and by 15 percent in Malawi. We also find a clear increase in livestock investment of more than 25 percent in both countries.

⁵In Liberia, phone surveys ended up starting slightly before transfers, due to a delay in the start of cash disbursement because of the onset of the COVID-19 pandemic in 2020.

⁶Incidentally, our effect sizes are fairly similar to those documented in other UCT studies—Haushofer and Shapiro (2016) find a 0.26 SD impact on food security at 9 months, and Beaman, Karlan, Thuysbaert, and Udry (2023) find a 0.1 SD effect on their food consumption measure at 18 months.

On the other hand, we find little evidence of any other kind of productive investments, especially in Malawi, where we find no treatment effects on cumulative nonagricultural income, business capital, or investment in education and health. In Liberia, we do observe some mixed evidence on nonagricultural investments and income, including a (statistically insignificant) increase in nonagricultural income of about \$64 over the survey rounds (on a base of \$250), an \$8 increase in enterprise capital (on a \$17 control mean), and a \$27 increase in education expenditures (on a \$129 control mean).

We use our data to examine several other key outcomes. First, we find that households reduce their supply of casual labor. The effect size is large and significant in Liberia (a reduction of about 7.6 hours over the month prior to the survey, equivalent to about 45 percent of the control mean) but smaller and insignificant in Malawi.⁷ This finding is related to a large literature that studies the question of whether receipt of cash (via cash transfer programs or as other kinds of welfare payments) may cause beneficiaries to reduce their labor supply, and finds mixed evidence, with most papers finding no effect (see Banerjee, Hanna, Kreindler, and Olken 2017 for a review); although recent work by Vivalt et al. (2024) finds that a guaranteed income program in the US states of Illinois and Texas caused respondents to decrease labor hours and spend more time on leisure activities. Similarly, another such program in California caused recipients to reduce part-time (but not full-time) work (Balakrishnan et al. 2024). Our result is also generally consistent with Fink, Jack, and Masiye (2020), who find that access to credit decreases casual labor off the farm (though increases labor on the farm, in contrast to our result).⁸

We also examine a few noneconomic outcomes, such as IPV and psychological well-being. In theory, cash transfers could impact IPV in either direction. By raising household economic well-being, cash transfers might reduce conflict (Ellsberg et al. 2015); on the other hand, a subliterate on women's economic empowerment suggests that interventions that improve women's financial status could increase IPV due to shifts in power dynamics, for example, as backlash (Buller et al. 2018; Chin 2012) or as a method for male partners to gain control over resources (Bobonis, González-Brenes, and Castro 2013). Several papers have analyzed this question empirically, and by now, there is an emerging consensus that cash transfers generally, though not always, lead to a decrease in IPV (Haushofer and Shapiro 2016; Hidrobo, Peterman, and Heise 2016; Baranov, Cameron, Contreras Suarez, and Thibout 2021; Heath, Hidrobo, and Roy 2020). In our case, we find that IPV is affected only in Liberia but not Malawi. One reason for this is that the contexts are quite different. Liberia has a recent history of civil conflict in which violence against women was widely used as a weapon of war, and baseline IPV rates are extremely high: 38 percent of the control group report IPV over the past year. Malawi has a much lower prevalence of 18 percent. We find a large, statistically significant decline of about 8 percentage

⁷We do not see a subsequent increase in other forms of labor, and so we cautiously attribute this to an increase in leisure, though it's also possible that time saved in casual labor was instead devoted to other forms of labor we did not measure, such as tending to animals.

⁸It is also related to Aggarwal, Brailovskaya, and Robinson (2023) who find that savings accounts (in the form of lockboxes or mobile money) reduce labor supply in business and increase agricultural labor.

points in Liberia, but no effect in Malawi. Consistent with the prior literature, we find improvements in psychological well-being in both countries.

Finally, our paper demonstrates how frequent surveys make it possible to account for the spending of cash transfers over time, which are missed in less frequent surveys. Focusing on Malawi, where we had better coverage in the phone surveys due to the quality of the cellular network and where the COVID-19 pandemic caused fewer disruptions, by the endline, we find no effect on expenditures or interpersonal transfers. Yet aggregating over all the survey rounds, we find an increase in nondurable expenditures of \$734 and in interpersonal transfers of \$28. Though the surveys were not originally designed for this purpose, we can also combine our aggregated results over the survey period with our endline information on stocks, to estimate the total percentage of the cash that can be accounted for. In total, we estimate that we can account for 57 percent of the money in Liberia and 82 percent in Malawi. Such a survey methodology can be useful for the evaluation of interventions that may have “front-loaded” effects, such as large UCTs. While effects on lasting investments such as assets can be measured in a single endline, impacts on nondurable expenditures are more fleeting.

I. Experimental Design

A. *Experimental Context and Design*

The NGO GD implemented a cash transfer program in 300 villages in both Liberia and Malawi in 2019–2021 (600 villages total), with the sample split equally between treatment and control. Targeted counties and districts were identified by GD and the funding partner, USAID, based on poverty levels, mobile phone coverage, and proximity to roads. Villages were eligible if their population (as of the most recent population census) was below a threshold size (100 households in Malawi and 125 in Liberia).⁹ Households were identified in collaboration with GD, which visited every village (both treatment and control) in the sampling frame and recorded every habitation structure with a GPS pin. Even though the households in our study sample are specifically selected on the criterion of being inhabitants of “small” villages, they look very similar to the average rural household in the study region.¹⁰

Amongst the 600 villages, we randomized treatment, stratifying by country and by administrative unit (district in Liberia, traditional authority in Malawi). Treatment villages were randomized into one of three *nominal* amounts: \$250 (\$555 PPP in Liberia; \$678 PPP in Malawi), \$500 (\$1,100; \$1,355 PPP), or \$750 (\$1,665; \$2,033 PPP).¹¹ Targeting was universal: In treatment villages, every household received the transfer. For logistical reasons, transfers were disbursed in one to three tranches. GD capped disbursements at \$250 per tranche, making

⁹ Since the transfers were universal, GD targeted smaller villages in order to cover enough villages while staying within its budget.

¹⁰ See appendix table S4 in Aggarwal, Jeong, et al. (2022) for a comparison between the characteristics of the study households and that of representative households from large-scale surveys done by the World Bank—the Household Income and Expenditure Survey 2016 for Liberia and the Integrated Household Survey 2019 for Malawi.

¹¹ Values for US dollar PPP are calculated based on the PPP conversion factor at baseline (2018 for Liberia and 2019 for Malawi).

additional tranches in the following months; they did this to smooth the amount of money being sent out in a given month. Thus, respondents who received \$250 received their transfer in a single transfer, while those receiving \$500 or \$750 received two or three transfers, respectively. These were given out over consecutive months in Liberia, and every other month in Malawi—that is, transfers were fully given out over one to three months in Liberia and over one to five months in Malawi (depending on the amount).¹²

In addition, in Malawi, we cross-randomized an “input fair” intervention that was designed to reduce transport costs to invest in agricultural inputs. We coordinated with a local agricultural retailer to offer inputs for sale on predesignated days at locations near farmers’ homes (usually in schools) and marketed these as input fairs. We subsidized the cost for farmers to attend these events. At one of our partners’ request, the input fair was only offered in a third of villages: Ultimately, 100 villages received cash only, 50 received market access only, 50 received both, and 100 served as control. Because the input fair was designed solely around agricultural investment, it is unlikely to have major effects beyond that channel. In this analysis, we therefore control for the input fair treatment, and its interaction with cash, but do not report results for the input fair treatment itself.

Within each household, the transfer was made to a beneficiary chosen by the household. As the household beneficiary had to be home at the time of enrollment, the majority of beneficiaries were women. Since beneficiary selection was endogenous and because the mechanics of how the transfer was shared within the household are unknown to us, the household is our unit of analysis. That said, as we will explain in greater detail below in Section IB, the vast majority of our survey respondents are women.

In Liberia, the project was implemented in two waves: a smaller “Wave 1” with 90 villages, in which transfers were disbursed from March 2019 to February 2020; and a larger “Wave 2” with 210 villages, in which transfers were disbursed from March 2020 to July 2021. In Malawi, all 300 villages were enrolled in a single wave (though enrollment was protracted) and transfers were disbursed from July 2019 to February 2020. As village enrollment took several months, the start date of transfers varied across villages (but not across households within a village). We estimate all results relative to the start date in that specific village (while controlling for month fixed effects).

In both countries, transfers were disbursed via mobile money. Households could enroll using a phone they already owned or purchase a cell phone with a mobile-money-enabled SIM during enrollment from GD. GD estimates that the vast majority of households exercised the option of buying a new phone. We know anecdotally that households did not hold the GD transfers as balance in their mobile money accounts, and instead cashed out shortly after receipt.¹³ It is noteworthy that

¹² Within each treatment village in Liberia, transfers were also randomized between being paid either in this “lump-sum” manner, or via quarterly payments spread out over a year (this design is similar to one of the treatment conditions in Haushofer and Shapiro 2016). This randomization was done within, rather than across, villages. Because this payment modality was done only in Liberia, we remove these households from our analysis in this paper. Please see the prior version of this paper (Aggarwal, Aker, et al. 2022) for a discussion of those results.

¹³ In surveys conducted in Malawi shortly after receipt, the treatment group reported having cashed out 92 percent of the transfer. The actual cash-out rate may be even higher if respondents were hesitant to report holding large amounts of cash on hand.

the average household reported spending 3 percent of the total transfer received on mobile money withdrawal fees and 1 percent on transport for the withdrawal.

Some of the fieldwork overlapped with COVID-19 lockdowns, particularly in Liberia. As a result, our in-person data collection and GD's enrollment activities had to be paused in 2020, and the Wave 2 transfers in Liberia were consequently delayed. In Malawi, enrollment ended just before the pandemic started, and in-person data collection even before that. Phone surveys went on as planned throughout the lockdown. However, it is possible that the effects we observe were affected by the general economic environment in the COVID-19 and immediate post-COVID-19 period. For more detail on the dynamics of food security and other outcomes during this time period, please see Aggarwal, Jeong, et al. (2022) which uses the phone surveys to track outcomes pre- and postpandemic.

B. Data

To construct the sample, we randomly sampled 10 GPS pins from the census list that GD collected and conducted baseline surveys with those households (6,000 households across both countries). These surveys took place in November–December 2018 for Liberia Wave 1, November–December 2019 for Liberia Wave 2, and April–July 2019 for Malawi. The surveys took about two to three hours to administer, and included questions on demographics, agriculture (measuring outcomes for the season immediately before cash was disbursed), cash flows, food security, mobile money usage, shocks and resilience, and IPV (among other subjects). Since IPV is a primary outcome, surveys targeted female heads of (dual-headed) households.¹⁴ While we attempted to survey 3,000 households in each country (ten per village), we were only able to enroll fewer than this in some small villages that had less than ten households total, leading to a final sample of 2,715 in Liberia and 2,944 in Malawi.

Two of the ten households from every village were further sampled to answer a monthly phone survey designed to measure a predefined set of outcomes. Each respondent sampled for this activity received a phone (worth \$10–\$15). Of these two, we called one household per village in even-numbered months, and the other in odd-numbered months. This results in a monthly village-level panel and a bimonthly household-level panel. The surveys included questions on food security, expenditures, income, labor supply, transfers, savings, and credit.¹⁵ The phone surveys were administered from July 2019 to August 2021 in Malawi, from February 2019 to September 2020 for Liberia Wave 1, and from January 2020 to October 2021 for Liberia Wave 2. The responses to this panel survey form our high-frequency data, which we use to study dynamics.

We collected a fuller set of outcomes from the entire sample via in-person end-line surveys, which were conducted in late 2020 for Liberia Wave 1 (18–20 months after disbursement), late 2021 for Liberia Wave 2 (18–22 months after disbursement), and April–July 2021 in Malawi (21–25 months after disbursement). These

¹⁴ Male heads were interviewed only when the female was absent and unreachable.

¹⁵ During COVID-19 lockdowns, we added questions aimed at measuring their impact. Aggarwal, Jeong, et al. (2022) documents the impact of COVID-19 in these two countries.

surveys were similar to the baselines in length and scope.¹⁶ Supplemental Appendix Figure A1 provides a timeline of project-related activities.

As shown in Supplemental Appendix Table A1, there was little attrition from the endline survey in either country (96 percent of households completed the endline in Liberia, and 94 percent in Malawi), and attrition was balanced between treatment and control. Attrition in the phone survey, shown in Supplemental Appendix Table A2, is more heterogeneous across countries. In Malawi (panel A), over 95 percent of the sample participated in early rounds; though this percentage fell over time, we still successfully interviewed 80 percent or more until the twelfth round (approximately two years after cash was disbursed). However, in Liberia, attrition is substantially higher, largely due to the country's inferior phone network. Also, in Wave 1, we noticed that households in the treatment group were more likely to switch to the SIM card provided by GD, thus making it more difficult to reach these respondents. Therefore, we drop Liberia Wave 1 from the phone survey analysis. In Wave 2, we took proactive steps to avoid this problem and managed to achieve a balanced sample, but the overall level of attrition is still substantially higher than Malawi: Survey completion peaks at 72 percent, but falls below 60 percent by the seventh round (14 months after cash disbursement).¹⁷

C. Summary Statistics and Randomization Check

Table 1 presents summary statistics and a randomization balance check. Columns 1 and 4 show the means and SDs of the control group in Liberia and Malawi, respectively. Columns 2 and 5 show the p -values for a t -test of equality between treatment and control, and columns 3 and 6 report the p -values for an F -test of equality of means across the three subtreatments (\$250, \$500, \$750) and control. The underlying regressions control for strata fixed effects and cluster standard errors by village.

Because we targeted women for surveys, the sample skews female: 77 percent of the sample in Liberia and 94 percent in Malawi are women. In Liberia, 85 percent are married, and 67 percent are married in Malawi. The average age (about 40 years) is similar in the two countries. Education levels are low, averaging 2.9 years in Liberia and 4.7 years in Malawi. On average, households have nearly five members. There are no statistically significant differences across experimental arms.

Panel A also includes a few measures relevant to the context. In prior evaluations with GD, the presence of a thatched roof was a targeting criteria, but only a minority in Liberia (23 percent) and about half in Malawi have a thatch roof (despite, as we will see, the high level of poverty).¹⁸ In terms of occupation, 88 percent of households in

¹⁶ See Jeong et al. (2023) and Park et al. (forthcoming) for analyses of cross-randomized survey experiments on survey length and interview modality of IPV, respectively, in these baseline and endline surveys.

¹⁷ We examine correlates of attrition in Supplemental Appendix Table A3, in which we regress the percentage of rounds completed on various covariates. We find a few common predictors: In general, older and richer respondents are more likely to complete surveys, likely reflective of better cell phone network coverage (or perhaps access to power) or that older respondents may be more likely to be at home for the calls. We replicate our main results using only those respondents who completed every round, though the sample size is dramatically reduced (results on request).

¹⁸ Liberia is exceptionally rainy, so metal roofs are the norm, even among the poor. In the Liberia Demographic and Health Survey 2019–2020, 82 percent of households had a roof made of zinc, metal, or aluminum.

TABLE 1—BASELINE SUMMARY STATISTICS AND EXPERIMENTAL BALANCE

	Liberia			Malawi		
	Control mean [SD] (1)	<i>p</i> -value: pooled treatment = control (2)	<i>p</i> -value: equality over four arms (3)	Control mean [SD] (4)	<i>p</i> -value: pooled treatment = control (5)	<i>p</i> -value: equality over four arms (6)
<i>Panel A. Demographics</i>						
=1 if female	0.77	0.770	0.780	0.94	0.648	0.600
=1 if currently married or has partner	0.85	0.347	0.717	0.67	0.166	0.534
Age	39.23 [13.92]	0.329	0.636	40.65 [15.05]	0.439	0.873
Years of education	2.89 [3.74]	0.427	0.163	4.72 [3.40]	0.572	0.712
Number of household members	4.62 [2.20]	0.338	0.688	4.79 [2.10]	0.427	0.698
=1 if housing roof material is thatch	0.23	0.265	0.366	0.50	0.764	0.221
=1 if planted/harvested any crop (past season)	0.88	0.704	0.693	0.99	0.128	0.171
=1 if earns any nonagricultural income	0.50	0.830	0.450	0.47	0.733	0.522
=1 if operates own business	0.23	0.983	0.628	0.21	0.512	0.570
<i>Panel B. Baseline measures of key outcomes</i>						
FSI (z-score, past year)	0.00 [1.00]	0.718	0.002	−0.00 [0.99]	0.462	0.762
Food expenditure (past month)	50.55 [40.82]	0.551	0.700	39.04 [41.04]	0.833	0.795
Total expenditure (past month)	120.14 [94.35]	0.485	0.632	92.35 [86.74]	0.978	0.952
Net value of durables, livestock, financial assets	254.26 [513.86]	0.463	0.577	251.47 [524.84]	0.373	0.784
Nonagricultural income (past month)	16.16 [38.46]	0.116	0.391	15.50 [40.25]	0.907	0.175
Observations		1,843			2,784	

Notes: Columns 1 and 4 present the mean for the control groups; columns 2 and 5 report the *p*-values for testing difference between the pooled cash treatment and control groups; columns 3 and 6 report the *p*-values for testing difference across individual treatment arms by cash amounts (i.e., \$250, \$500, or \$750) and the control group. SDs are in square brackets in columns 1 and 4 and standard errors clustered at village level in parentheses in columns 2, 3, 5, and 6. Monetary outcomes are in US PPP dollars and winsorized at the ninety-ninth percentile.

Liberia and 99 percent in Malawi earn income from farming, while about half earn any nonagricultural income. A little less than a quarter own a business.

Panel B shows baseline measures of some important primary outcomes: a food security index (FSI),¹⁹ expenditures, assets, and income (all values in 2019 PPP dollars). Overall, total monthly expenditures in the control group are \$51 in Liberia and \$39 in Malawi, with food expenditures being about 42 percent of the total in each country. The total value of durables, livestock, and financial assets is approximately \$250 in both countries. Total nonagricultural incomes (\$16) are also fairly similar in both contexts. Again, we observe sample balance on each of these attributes across

¹⁹The FSI is comprised of standardized scores of the Household Dietary Diversity Score (HDDS), measured over the past 24 hours; the Food Consumption Score (FCS), measured over the past seven days; and the Household Hunger Scale (HHS), measured over the past month. The baseline and endline FSI also includes a fourth measure, the Food Insecurity Experience Scale (FIES), which we didn't measure in the phone surveys because it has a one-year recall. A z-score is calculated using inverse covariance weighting (Anderson 2008).

treatment and control, other than in the case of the FSI over the disaggregated treatment arms. Overall, randomization appears successful; in any case, all regressions were prespecified as analysis of covariance (ANCOVA) and we control for baseline measures throughout the paper.

II. Results

A. Dynamic Treatment Effects

We estimate time-varying treatment effects using the phone survey data as follows:

$$(1) \quad Y_{ivst} = \sum_t \beta_t \text{Cash}_{vs} D_{ivs} + \gamma Y_{ivs0} + \delta I_{vs} + \eta I_{vs} \times \text{Cash}_{vs} + \phi_m + \lambda_s + \varepsilon_{ivst},$$

where Y_{ivst} is an outcome for individual i in village v and strata s at time t , which is defined as the number of months since cash transfers began (defined for each stratum and thus, able to take on values for both treatment and control). Cash_{vs} is a binary variable equal to 1 for villages assigned to any cash transfer, and 0 otherwise; D_{ivs} is a binary variable equal to 1 for a survey that occurred t months since transfers began; Y_{ivs0} is the baseline value of the outcome variable; I_{vs} is an indicator for the input fair treatment (in Malawi only); and ϕ_m and λ_s are calendar month and strata fixed effects, respectively. We cluster standard errors by village, the level of randomization.

Figure 1 plots the coefficient and confidence intervals from equation (1) for four important outcomes: food security, nondurable expenditures, large purchases, and nonagricultural income. The figures pool two months together so that the comparison across points in the graph is for the same set of respondents. In Malawi, we observe a spike in food security, expenditures, and large purchases immediately after disbursement, but no effect on nonagricultural income. For these three outcomes, we also observe clear evidence of time-varying treatment effects. Specifically, food security increased by over 0.5 SDs in the first six months (statistically significant at 1 percent) but then fell to approximately 0.25 SDs by the eighth month. These levels persist for the duration of the survey period (24–26 months after the initial transfer).²⁰ For nondurable expenditures and large purchases, treatment effects are substantial initially, but then fall to being indistinguishable from zero within 10–12 months. The result that large purchases increase for about eight months indicates that these larger outlays were not all spent immediately postdisbursement but spread out over some period of time. Nonagricultural income shows little effect throughout.

The picture is slightly different for Liberia. While there is also a clear increase in food security immediately posttransfer, the magnitude is smaller (0.25–0.4 SDs), and the effect persists for the duration of surveying.²¹ The effect on nondurable

²⁰ Supplemental Appendix Figure A4 shows food security results by each of the components of the index (HDDS, FCS, and HHS) and shows that the time path is similar for each.

²¹ Figure 1 suggests that food security outcomes in the treatment group improved in the round *before* the first payment. Although not statistically significant, it is possible that effects manifest before the recorded date because of measurement error in the date of the first GD transfer, or because treatment households could have started spending in anticipation.

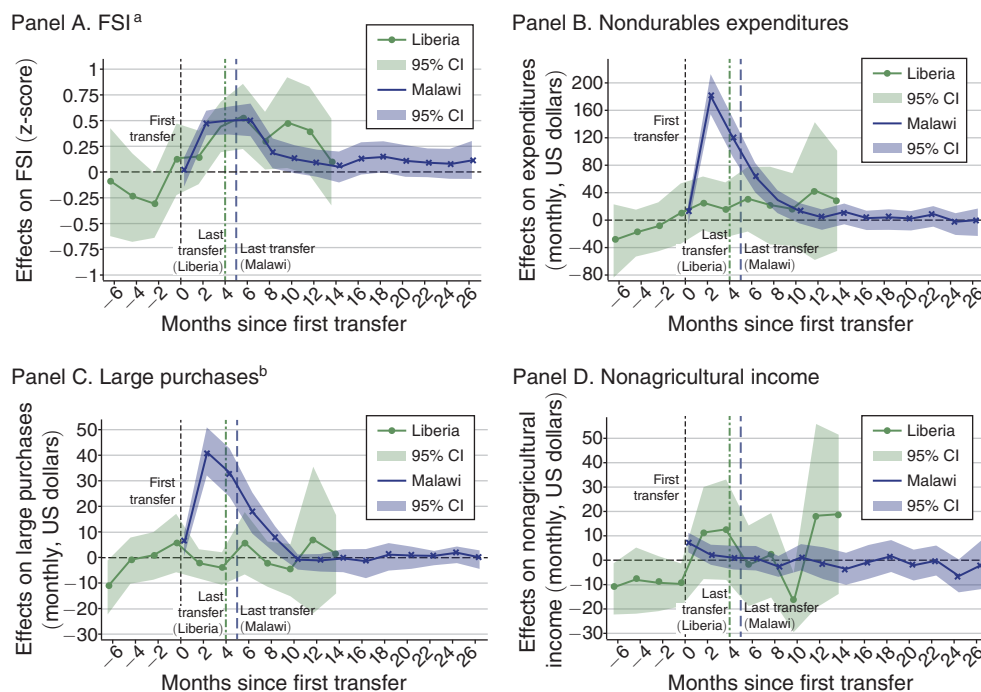


FIGURE 1. DYNAMIC EFFECTS OF CASH ON FOOD SECURITY, EXPENDITURES, LARGE PURCHASES, AND INCOME

Notes: Regressions include baseline measurement of outcome and strata fixed effects. Standard errors clustered at village level. First transfer for each treatment household was made across July–October 2019 for Malawi and March–September 2020 for Liberia (Wave 2). Sample includes only those households receiving “lump-sum” transfers (see text for details). Sample includes 596 households in Malawi and 253 in Liberia (Wave 2).

^aOutcome variable is FSI, a restandardized z-score of HDDS, FCS, and HHS (negatively weighted), per Anderson (2008).

^bPurchases of health preventatives (e.g., bed nets, family planning, water purification), durables (e.g., furniture, electronics, livestock), and farm investments (e.g., farm tool, fertilizer/hybrid seeds).

expenditure is fairly modest relative to Malawi (and to the total transfer size) and indistinguishable from zero. There is also no strong evidence of an effect on nonagricultural income, though the results are noisier than in Malawi.

Figure 2 shows effects on other outcomes measured in the phone surveys, specifically savings, debt, and interpersonal transfers. We report savings as a stock variable, measured as the total balance held by the household at the time of the survey, and it is remarkable for its low baseline value: The average control household reports holding only \$5–\$23 in savings over this period in Liberia and \$12–\$24 in Malawi. We see minimal treatment effects on reported savings, as well as on outstanding debt on the date of each survey round. The lack of effects suggests that the vast majority of income is spent, rather than used to build savings or discharge debt, and suggests a marginal propensity to consume close to 1. In regards to transfers, we see small but positive point estimates on transfers sent in Liberia (of a few dollars per month) and no effect whatsoever on those received. In Malawi, we observe no effect on either at any point in time. These results suggest that little or none of the cash transfer amount is directly shared with others (which may be because the transfers were targeted

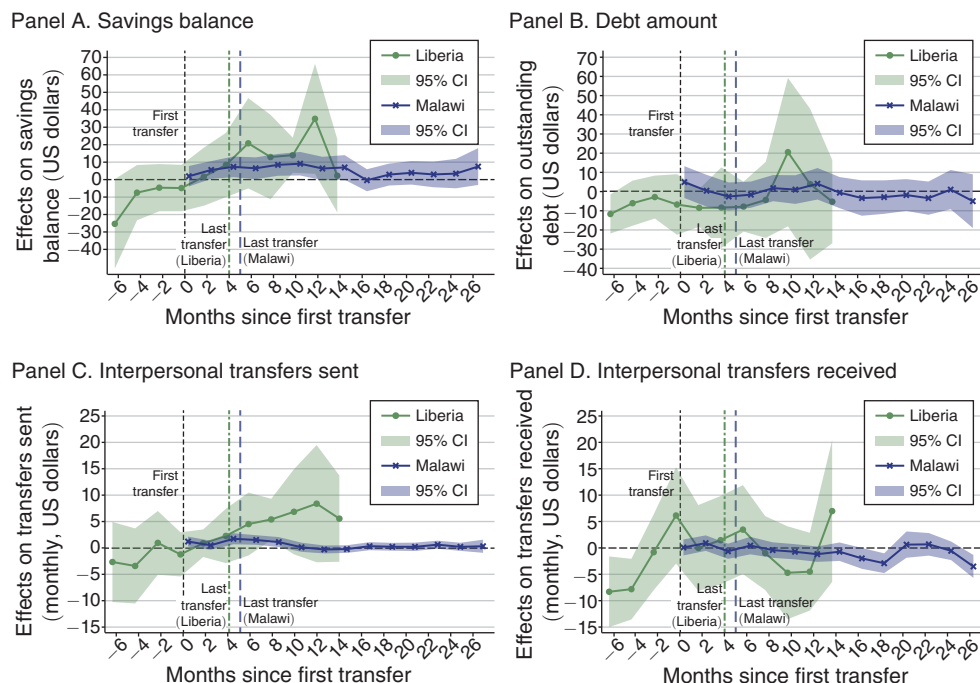


FIGURE 2. DYNAMIC EFFECTS OF CASH ON INTERPERSONAL TRANSFERS, SAVINGS, AND DEBT

Notes: Data come from phone surveys, and transfers are measured over the past month. Regressions include baseline measurement of outcome and strata fixed effects. Standard errors clustered at village level. First transfer for each treatment household was made across July–October 2019 for Malawi and March–September 2020 for Liberia (Wave 2). Sample includes only those households receiving “lump-sum” transfers (see text for details). Sample includes 596 households in Malawi and 253 in Liberia (Wave 2).

universally within villages and so, unlike studies such as Angelucci and De Giorgi (2009), there is little scope for sharing, at least within village networks).

Finally, in Figure 3 we show results separately by the different grant sizes (\$250, \$500, or \$750), for the main outcomes shown in Figure 2. We perform this analysis in Malawi only, because that is the country in which we observe the more interesting dynamics. We can clearly see here that effects on food security and expenditures are larger and last longer if transfers are larger. The overall pattern is similar across the different grant amounts, however, suggesting that the qualitative type of investment choices is similar across the grant amounts.

B. Cumulative Effects over the Study Period

For the phone survey sample, we are able to calculate cumulative effects over the full period over which we followed households. In fact, we can create a household balance sheet of sorts by summing all the inflows received by households and outflows spent by households over all the phone survey rounds and the endline.²² To

²² Each household was surveyed every two months, and for months between surveys, results are interpolated.

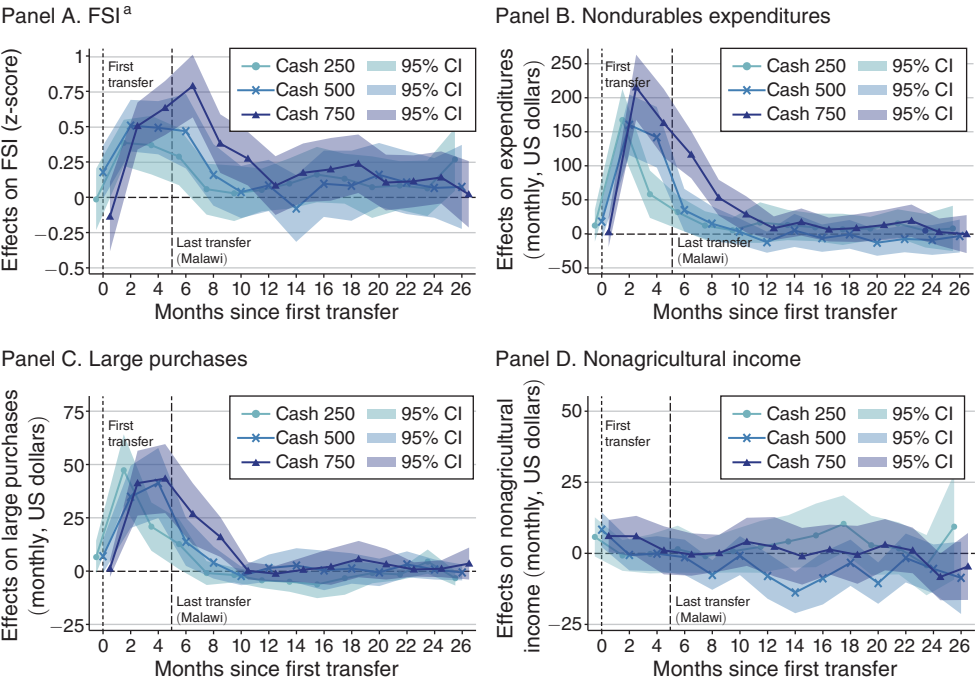


FIGURE 3. EFFECTS OF DIFFERENT GRANT SIZES ON DYNAMICS OF MAIN OUTCOMES (MALAWI ONLY)

Notes: Sample includes data from Malawi only (596 households). Regressions include baseline measurement of outcome, strata fixed effects, and treatment indicators for market access and cash times market access. Standard errors are clustered at village level. First transfer for each treatment household was made during July–October 2019 for Malawi.

^aOutcome variable is FSI, a restandardized z-score of HDDS, FCS, and HHS (negatively weighted), per Anderson (2008).

this, we can then add in data collected on “stock” variables, such as assets, at endline, to get a measure of the net change in these stocks over the study period. Having created these variables, we run the following regressions:

$$(2) \quad Y_{ivs} = \beta Cash_{vs} + \gamma Y_{ivs0} + \delta I_{vs} + \eta I_{vs} \times Cash_{vs} + \lambda_s + \varepsilon_{ivs},$$

where Y_{ivs} is either the cumulative value of the flow value, or the measured endline stock. Standard errors are clustered by village. For each outcome, we show results from two separate regressions: one with all cash treatments pooled, and a second with treatment effects disaggregated by grant size. For the latter, we report p -values from a test for the linearity of the effect size in the dollar value of the cash amount, as well as from a test for equality of the three effect sizes.

The household balance sheet is presented in Table 2. Columns 1–2 show our two measures of cash income: nonagricultural income (measured as a flow) and estimated agricultural sales (measured at endline). Columns 3–4 show expenditures of nondurables (measured as a flow) and durables (at endline). Finally, columns 5–6 show net values for interhousehold transfers and savings, both measured as a flow.

TABLE 2—CUMULATIVE HOUSEHOLD BALANCE SHEET

	Income		Expenditures		Net transfers ^d	Net savings ^e
	Nonag income ^a	Agri sales (US dollars)	Nondurables ^b	Durables ^c		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Liberia</i>						
<i>Pooled cash treatment:</i>						
Cash	63.16 (60.04)	101.51 (99.08)	151.44 (195.62)	498.10 (294.82)	24.76 (24.30)	43.27 (39.41)
<i>Individual treatments by cash amount:</i>						
Cash \$250 (β_1)	21.97 (82.01)	94.06 (195.95)	−263.69 (223.36)	60.68 (287.58)	9.27 (30.49)	−28.07 (35.20)
Cash \$500 (β_2)	138.40 (107.15)	160.19 (114.78)	213.96 (270.93)	397.28 (427.82)	7.11 (49.21)	55.09 (41.50)
Cash \$750 (β_3)	24.06 (97.08)	48.06 (153.51)	477.89 (393.06)	1,015.93 (589.05)	57.92 (27.64)	97.82 (106.02)
Control mean	249.77	205.47	1,716.86	1,101.77	−58.51	45.05
Control SD	305.31	626.65	1,171.09	1,543.95	144.47	123.30
<i>p-values:</i>						
$\beta_1 = \beta_2 = \beta_3$	0.624	0.808	0.147	0.275	0.347	0.188
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.604	0.631	0.219	0.673	0.787	0.330
Observations	231	231	231	231	231	231
<i>Panel B. Malawi</i>						
<i>Pooled cash treatment:</i>						
Cash	−59.80 (51.29)	67.89 (47.21)	734.79 (133.39)	352.64 (265.99)	27.88 (14.44)	4.52 (6.05)
<i>Individual treatments by cash amount:</i>						
Cash \$250 (β_1)	27.68 (71.15)	3.63 (54.99)	549.22 (163.72)	141.08 (336.02)	26.09 (17.47)	6.87 (8.54)
Cash \$500 (β_2)	−127.88 (59.15)	21.77 (51.62)	675.92 (182.97)	170.75 (312.70)	12.13 (18.55)	−2.74 (8.09)
Cash \$750 (β_3)	−78.86 (71.31)	181.87 (79.38)	989.28 (175.00)	758.53 (393.04)	46.27 (18.20)	9.58 (7.67)
Control mean	589.06	116.97	2,232.07	1,967.56	−36.17	−5.72
Control SD	433.53	379.71	1,424.18	2,886.00	150.41	61.38
<i>p-values:</i>						
$\beta_1 = \beta_2 = \beta_3$	0.113	0.064	0.073	0.250	0.170	0.414
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.224	0.215	0.331	0.567	0.430	0.448
Observations	560	560	560	560	560	560

Notes: Sample restricted to those in both phone surveys and endline survey. Regressions include baseline measurement of the dependent variable (ANCOVA) and strata fixed effects; for Malawi, regressions also include treatment indicators for market access and cash times market access. Results in the subpanels on pooled and individual treatments are from separate regressions. Monetary outcomes are in US dollars PPP and winsorized at the ninety-ninth percentile. \$1 = \$2.7 PPP for Malawi and \$2.2 PPP for Liberia. Standard errors clustered at the village level in parentheses.

^aMonthly flow amounts reported in the phone surveys (14 months in total in Liberia; 26 months in Malawi) are aggregated. While each household was surveyed every other month, the values for the unsurveyed months are interpolated.

^bExpenditures including nondurable expenditures aggregated over monthly flows, agricultural input purchases (past season), expense on hired labor for agriculture (past season), education expenditures (annualized), and health expenditures (annualized).

^cStock variables at endline including durables, livestock, owned housing, business capital, and farm tools and irrigation equipment.

^dTransfers are coded as positive for outflows and negative for inflows.

^eTotal value of savings balances minus outstanding debt amount, both at endline.

Starting with Liberia, we observe statistically insignificant increases of about \$63 PPP in nonagricultural income and \$102 PPP in the value of agricultural sales. This amounts to about a \$165 increase in income, on a base of \$455 (or about 36 percent). We find an increase of about \$151 PPP in nondurables and \$498 in durables, and relatively small effects on interpersonal transfers (about a \$25 increase, which corresponds to an increase in net outflows) and in net savings (\$43). The bottom of the panel shows *p*-values for the test of equality of the regression coefficients, as well as for a test of whether effects are linear in the grant size. Though we do generally see larger point estimates for larger grants sizes, especially for expenditures, neither test rejects the null for any variable.

Turning to Malawi, we find a very different pattern for income: We actually observe a negative point estimate of $-\$60$ for nonagricultural income, but a borderline significant increase of \$68 for agricultural sales. Thus, we find an overall increase in income of only about \$8, on a base of \$706. However, we are able to account for much of the transfer (\$1,350 PPP on average) in the form of increased expenditures: From columns 3–4, we see that nondurable expenditures increase by \$735 PPP and durable expenditures by \$353 PPP, though from columns 5–6, we again see relatively small effects on transfers and on savings. We reject equality of grant sizes for nondurables expenditures (with a *p*-value of 0.073).

Accounting for Inflows and Outflows.—Though it was not the original intention of these surveys, we can use the information in this table to examine the percentage of the cash transfer for which we are able to account. Note, however, that this is only illustrative, in particular, because we did not collect information on the home consumption of agricultural production, especially livestock. With that caveat in mind, in Liberia, the value of the cash transfer was \$1,100 PPP, and we observe an increase in cash receipts via increased income of \$165.²³ We account for \$151 in nondurables, \$498 in durables, \$25 in transfers, and \$43 in savings. Thus, total outflows amount to about $(151 + 498 + 25 + 43)/(1100 + 165) = 57\%$ of inflows. In Malawi, the cash transfer is worth \$1,355 PPP, we observe a negligible effect on income of \$8, and thus, the percent we can account for is $(735 + 353 + 28 + 5)/(1355 + 8) = 82\%$.

C. Effects at Endline

We now turn to using the endline to look at effects on some of our main outcomes at 18–24 months from when the transfers were first disbursed. This analysis is similar to those in prior evaluations of cash transfers, giving a snapshot of these variables at a single point in time, allowing for benchmarking with the remainder of the literature on UCTs. For this analysis, we are able to use the entire study sample, and not just the 20 percent with whom we ran the phone surveys. However, in this analysis, all

²³Note that this \$165 is not the only increased “income” generated by the transfers as the increase in the value of harvest is much larger (see Table 4 and the corresponding discussion). However, we focus only on cash because we did not measure home consumption.

the flow variables (such as expenditures and income) have limited recall, as they are only measured over the period immediately preceding the endline survey.

Table 3 shows effects for the seven outcomes shown in Figure 1 and Figure 2 (we do not show the endline snapshot for large purchases, as we did not collect information for this outcome at endline. We instead collected information for individual asset classes at this time). In the first row of each panel, we start by showing effects pooled across the different amounts. We continue to see significant improvements in food security, even this long after the transfers were disbursed: Households in treated villages reported a 0.29 and 0.11 SDs improvement in the FSI in Liberia and Malawi respectively, both statistically significant. We also observe an increase in nondurable expenditures and savings in Liberia, and the effects are sizable: a \$15 increase on a \$143 base for expenditures and a \$21 increase on a \$49 base for savings. The only significant treatment effect we find for Malawi is on savings, which went up by \$6 on a base of \$18.²⁴

If we had relied solely on the endline to estimate treatment effects, we would have confirmed the existence of several lasting effects, such as on food security, but we see that we would have captured only a small fraction of the overall treatment effect. For example, while we observe a 0.11–0.29 SD effect on food security at this point, the effects immediately following the transfer, as shown in Figure 1, are at least twice as large and continue to be at this level for several months thereafter. Thus, our results also point to the importance of measurement timing as an important consideration of experimental design, especially when outcomes may be dynamic in nature.

D. Mechanisms

Agricultural Investment.—An important finding to emerge out of Figure 1 and Table 3 is that while there are persistent increases in food security (present even at the endline), they are accompanied neither by an increase in expenditures, nor by any in nonagricultural income. What then explains these persistent effects?

Since this is a heavily agrarian context, any potential investment and income increases driving persistent effects on food security would most likely be driven by agriculture. Therefore, we next examine farm production decisions in the wake of receiving the transfers.

We start by analyzing the output side. Table 4 shows effects on agriculture, covering one full agricultural season after transfers were disbursed. In column 1, we start by showing that indeed there was a large increase in the value of harvest (aggregated over all crops and using average prices as reported in our survey data). We find an increase of \$311 in harvest value in Liberia (a 35 percent increase, on a base of \$894) and an increase of \$101 in Malawi (a 16 percent increase, on a base of \$613). We also observe in column 8 that the value of livestock goes up in both countries by 26–28 percent. It is, thus, possible that persistent food security increases are driven by increased home production.

²⁴ As above, the bottom of the table shows tests for whether effects are linear in the grant amount, as well as those for equality of coefficients. In Liberia, we reject equality for two coefficients: food security and nondurable expenditures. In Malawi, we reject it only for nonagricultural income.

TABLE 3—SNAPSHOT OF TREATMENT EFFECTS AT ENDLINE

	FSI ^a (1)	Non- durables expenditures (2)	Non- agricultural income (3)	Inter- personal transfers sent (4)	Inter- personal transfers received (5)	Savings balance (6)	Debt amount (7)
<i>Panel A. Liberia</i>							
<i>Pooled cash treatment:</i>							
Cash	0.29 (0.06)	15.00 (5.63)	4.10 (4.05)	−1.67 (1.03)	−1.13 (1.63)	20.70 (9.08)	0.28 (2.89)
<i>Individual treatments by cash amount:</i>							
Cash \$250 (β_1)	0.15 (0.09)	−6.13 (8.68)	5.80 (7.91)	−3.20 (1.21)	−1.88 (1.76)	16.90 (12.85)	0.27 (4.90)
Cash \$500 (β_2)	0.18 (0.08)	29.03 (7.65)	5.71 (6.21)	−0.37 (1.28)	−2.06 (1.64)	23.29 (12.27)	0.44 (3.64)
Cash \$750 (β_3)	0.56 (0.09)	20.27 (8.99)	0.69 (5.19)	−1.59 (1.47)	0.63 (3.38)	21.57 (17.45)	0.13 (4.86)
Control mean	0.00	142.66	21.04	5.18	6.83	48.70	13.66
Control SD	1.00	111.41	71.25	29.41	37.34	168.76	51.79
<i>p-values:</i>							
$\beta_1 = \beta_2 = \beta_3$	0.001	0.004	0.757	0.102	0.731	0.925	0.998
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.128	0.040	0.614	0.024	0.475	0.732	0.997
Observations	1,843	1,843	1,843	1,843	1,843	1,843	1,843
<i>Panel B. Malawi</i>							
<i>Pooled cash treatment:</i>							
Cash	0.11 (0.05)	2.14 (3.12)	0.38 (2.81)	0.10 (0.22)	0.57 (0.67)	6.07 (2.89)	−2.22 (3.20)
<i>Individual treatments by cash amount:</i>							
Cash \$250 (β_1)	0.06 (0.06)	1.03 (3.72)	4.62 (4.10)	0.16 (0.27)	0.26 (0.84)	2.69 (4.72)	−1.10 (3.77)
Cash \$500 (β_2)	0.12 (0.06)	0.33 (4.03)	−4.27 (3.31)	−0.17 (0.23)	0.38 (0.83)	−1.42 (4.36)	−6.48 (3.45)
Cash \$750 (β_3)	0.16 (0.07)	5.06 (4.43)	0.77 (3.59)	0.31 (0.29)	1.06 (0.94)	16.89 (7.81)	0.90 (4.34)
Control mean	0.00	61.35	26.92	1.21	2.89	18.01	21.43
Control SD	1.00	63.22	58.62	5.28	12.36	47.11	56.41
<i>p-values:</i>							
$\beta_1 = \beta_2 = \beta_3$	0.265	0.578	0.081	0.127	0.683	0.140	0.118
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.982	0.746	0.087	0.132	0.924	0.087	0.087
Observations	2,784	2,784	2,784	2,784	2,784	2,784	2,784

Notes: The endline was conducted about 18–22 months after first transfers were received in Liberia and 21–25 months in Malawi (see Supplemental Appendix Figure A1 for more detail). Regressions include baseline measurement of the dependent variable (ANCOVA) and strata fixed effects; for Malawi, regressions also include treatment indicators for market access and cash times market access. Results in the subpanels on pooled and individual treatments are from separate regressions. Monetary outcomes are in US dollars PPP and winsorized at the ninety-ninth percentile. \$1 = \$2.7 PPP for Malawi and \$2.2 PPP for Liberia. Standard errors clustered at the village level in parentheses.

^aFSI is standardized z-score of HDDS, FCS, HHS (negatively weighted), and FIES (negatively weighted), using inverse covariance weighting (Anderson 2008) relative to the control mean and SD in each country.

The next few columns examine effects on crop and input choices that may be driving the increase in harvest output observed in column 1. Column 2 shows crop choice. We find that beneficiaries grow more crops in both countries: 0.34 more crops in Liberia and 0.15 more in Malawi, on a base of about 2 crops in both countries. Columns 3–5

TABLE 4—AGRICULTURAL INVESTMENT AND PRODUCTION

	Value of harvest (US dollars) (1)	Number of crops grown (2)	Input purchase value (US dollars) (3)	Hired labor (US dollars) (4)	Invest in farm tools & irrigation (US dollars) (5)	Agri profit ^a (US dollars) (6)	Land under cultivation (acre) (7)	Livestock value (US dollars) (8)
<i>Panel A. Liberia</i>								
<i>Pooled cash treatment:</i>								
Cash	311.42 (108.08)	0.34 (0.10)	4.00 (4.69)	49.08 (16.16)	6.48 (2.45)	263.54 (100.63)	−0.20 (0.97)	25.86 (12.81)
<i>Individual treatments by cash amount:</i>								
Cash \$250 (β_1)	404.54 (186.13)	0.32 (0.17)	−2.48 (4.62)	19.77 (11.89)	5.36 (3.91)	389.94 (179.32)	1.72 (2.44)	20.14 (20.13)
Cash \$500 (β_2)	227.55 (152.01)	0.34 (0.13)	3.97 (6.05)	35.49 (18.73)	5.51 (3.75)	206.91 (136.92)	−1.36 (0.58)	34.50 (20.14)
Cash \$750 (β_3)	312.26 (179.60)	0.35 (0.16)	10.37 (9.77)	92.57 (38.09)	8.65 (3.84)	202.05 (163.04)	−0.79 (0.93)	21.98 (20.60)
Control mean	893.80	1.83	18.12	81.38	30.61	788.18	1.72	99.50
Control SD	1,662.86	1.69	69.06	228.79	44.23	1,583.24	18.97	243.65
<i>p-values:</i>								
$\beta_1 = \beta_2 = \beta_3$	0.740	0.990	0.307	0.144	0.768	0.648	0.350	0.847
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.284	0.451	0.486	0.670	0.810	0.212	0.218	0.621
Observations	1,843	1,843	1,843	1,843	1,843	1,843	1,843	1,843
<i>Panel B. Malawi</i>								
<i>Pooled cash treatment:</i>								
Cash	101.23 (40.09)	0.15 (0.05)	13.08 (2.93)	5.06 (4.28)	65.68 (31.88)	78.18 (43.84)	−0.68 (0.47)	40.46 (16.37)
<i>Individual treatments by cash amount:</i>								
Cash \$250 (β_1)	59.76 (50.19)	0.13 (0.07)	5.26 (3.31)	0.17 (5.40)	34.24 (35.82)	46.86 (54.05)	−0.82 (0.55)	2.24 (18.05)
Cash \$500 (β_2)	96.56 (49.01)	0.10 (0.07)	16.42 (3.82)	6.31 (5.24)	70.05 (39.65)	47.64 (53.86)	−0.95 (0.48)	61.64 (21.29)
Cash \$750 (β_3)	147.46 (50.03)	0.21 (0.07)	17.56 (3.82)	8.71 (5.73)	92.74 (47.30)	139.84 (57.76)	−0.26 (0.63)	57.57 (23.44)
Control mean	613.26	2.23	52.49	19.27	140.99	513.31	1.96	139.40
Control SD	752.20	1.02	55.24	88.83	474.76	713.73	8.22	345.75
<i>p-values:</i>								
$\beta_1 = \beta_2 = \beta_3$	0.204	0.282	0.001	0.338	0.421	0.201	0.472	0.006
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.967	0.421	0.370	0.830	0.979	0.648	0.162	0.179
Observations	2,784	2,784	2,784	2,784	2,784	2,784	2,784	2,784

Notes: The endline was conducted about 18–22 months after first transfers were received in Liberia and 21–25 months in Malawi. Regressions include baseline measurement and strata fixed effects; for Malawi, regressions also include treatment indicators for market access and cash times market access. Results in the subpanels on pooled and individual treatments are from separate regressions. Monetary outcomes are in US dollars PPP and winsorized at the ninety-ninth percentile. \$1 = \$2.7 PPP for Malawi and \$2.2 PPP for Liberia. Standard errors clustered at the village level in parentheses.

^aAgricultural profit is calculated as the value of harvest minus value of input purchases, hired labor, and investments in tools and irrigation; we assume the tools and irrigation investments depreciate annually by 20 percent. Family labor is not included.

show investment in physical and human capital for crop agriculture—specifically, inputs such as fertilizer, seeds, and pesticides (column 3); hired labor (column 4); and investments in farm tools and irrigation at endline (column 5). We find that these treatment effects are positive across the board and also statistically significant in the

case of tools and irrigation in both countries, labor in Liberia,²⁵ and inputs in Malawi. The effect on fertilizer, seeds, and pesticides represents a 25 percent increase, despite relatively high baseline input usage (\$52) due to the country's farm input subsidy program. We use the dollar values of the farm input investments reported in columns 3–5 and subtract them from the harvest values reported in column 1 to impute farm profits.²⁶ We report the coefficients from a regression of these imputed profits on treatment in column 6 (note, however, that these profits are not net of own and family labor as we did not measure those for the agricultural season). We find that farm profits for the agricultural season immediately following the transfers go up by \$264 PPP in Liberia and by \$78 PPP in Malawi. These numbers suggest a significant return on transfer dollars, especially in Liberia, where the increase in farm profits alone is worth 24 percent of the average transfer amount (\$1,100 PPP). In Malawi, farm profit increase is about 6 percent of the transfer amount. It is worth emphasizing, however, that these profits correspond to just one agricultural season, whereas the farm investments made by the households, especially those in tools and irrigation, would likely generate returns in subsequent years as well.

Next, we examine if treatment households also changed their area under cultivation. Column 7 shows land under cultivation and shows no effect.²⁷

Finally, as noted above, we show effects on the stock of livestock owned by the study households at endline. In both countries, the treatment group reports about a 25 percent increase in the value of livestock owned. Livestock products may also partly explain the increases in food security experienced by the treatment households. The increase in livestock holdings is a particularly noteworthy finding in that livestock transfers have been a central element of the multifaceted “graduation” (out-of-poverty) programs that were tried out in many countries.²⁸ Our results suggest that similar outcomes may be attainable just by giving households cash alone, which can be delivered at a fraction of the cost of graduation programs, which require costly administration. Moreover, UCTs enable households to also make other complementary investments (such as in agriculture in this case), whose possibility is precluded by in-kind transfers.

In each panel, we also show disaggregated results by transfer amount. We cannot reject equality of the effects on harvest quantities, sales, or profits. While the pattern of coefficients suggests that those receiving the larger transfers may have been able to make slightly larger farm investments, even for these, we cannot reject equality of coefficients other than in the case of input purchases and

²⁵ We measured retrospective information on labor inputs during different phases of the agricultural season in the endline.

²⁶ To do so, we assume that capital goods—that is, tools and irrigation facilities—would depreciate over five years and, therefore, consider only 20 percent of the value reported by the household to compute annual profits.

²⁷ In Supplemental Appendix Table A5, we examine whether the increase in output is generally commensurate with increases in inputs. In particular, we regress, for the control group only, the value of harvest (as measured in the endline survey) on the various measures of agricultural investment and livestock in Table 4. The measured changes in inputs and investments explain about 77 percent of the observed increase in output in each country. However, note that the R2 of this cross-sectional correlation is on the low side.

²⁸ See Balboni et al. (2022) and Banerjee, Duflo, and Sharma (2021) for evidence on the long-term effectiveness of these programs in Bangladesh and India respectively.

livestock in Malawi. Finally, for all statistically significant outcomes, we cannot reject that effects are linear in grant size.

Nonagricultural Investment.—Table 5 shows results on physical and human capital investments outside of agriculture to identify other sources of persistent gains.

Consistent with the prior literature, column 1 shows sizable increases in investment in housing and durable goods of (\$197) 24 percent in Liberia and \$324 (20 percent) in Malawi. Since these are PPP amounts, this translates to 18 percent of the transfer in Liberia and 24 percent in Malawi.²⁹ Next, columns 2 and 3 show spending on education and health. In Liberia, we see an increase in education spending (\$27, base \$129) but no effect in Malawi (on a base only half as large). We attribute this to the fact that primary school is free in Malawi but not in Liberia and, thus, is an important investment channel in Liberia. We see no effect on health expenditures in either country.

Finally, we examine enterprise outcomes. Columns 4–6 display enterprise starts, investment, and income, and show some evidence of an increase in enterprise activity in Liberia: The probability of starting an enterprise increases by 3 percentage points (significant at 10 percent), on a base of only 8 percent, and the value of business capital increases by \$8 on a base of \$17 (also significant at 10 percent). Enterprise income also increases by about 50 percent (on a very low base), but is not statistically significant. In Malawi, on the other hand, we find no evidence of any entrepreneurial activity in response to receiving cash (in fact, point estimates are negative).³⁰

Turning to disaggregated effects by grant size, what jumps out is the fact that the “large” (both \$500 and \$750) grant recipients made similar and large outlays on housing and durables (coefficients are between \$270 and \$290 in Liberia and between \$400 and \$490 in Malawi), but we observe virtually no effects for the \$250 grant size; however, we only reject equality in Malawi. We observe a similar pattern for education expenses in Liberia, and here too, we reject equality. We reject linearity for only one outcome (the value of self-owned capital in Liberia), which shows a puzzling pattern that effects are actually decreasing in grant size, and we formally reject linearity. One possible explanation for this is that, as discussed, we do not see a statistically significant increase in housing and durable wealth in the \$250 arm, in either country, and perhaps some of this investment was put into the business in Liberia.

In Supplemental Appendix Table A7, we examine whether these education investments that we observe in Table 5 translate into schooling outcomes. In Liberia, only 48 percent of primary-age children are enrolled in school (compared to 93 percent in Malawi), and we see a 10 percentage point enrollment increase due to cash. We

²⁹ Note that these are just a subset of the nonmonetary assets already shown in Table 2, but we show them here for the sake of completeness.

³⁰ A possible reason for the modest effects on enterprise income is that effects may be larger for those that already own a business (as in Banerjee, Breza, Duflo, and Kinnan 2024), but only a minority of people in our samples have a business at baseline (21–23 percent of our sample). To investigate this, we examine heterogeneity in impacts by whether the respondent has a business at baseline in Supplemental Appendix Table A6. However, we do not observe effects for existing entrepreneurs in either country (in fact, in Malawi, the point estimate is actually negative, though only significant at 10 percent.)

TABLE 5—NONAGRICULTURAL INVESTMENT AND ENTERPRISE INCOME

	Housing & durables value (at endline) (1)	Education expend ^a (annualized) (2)	Health expend ^b (annualized) (3)	=1 if started enterprise since baseline (4)	Self-own enterprise capital value (at endline) (5)	Enterprise income (past month) (6)
<i>Panel A. Liberia</i>						
<i>Pooled cash treatment:</i>						
Cash	197.03 (93.09)	27.40 (16.00)	5.17 (4.16)	0.03 (0.02)	7.91 (4.78)	3.33 (2.85)
<i>Individual treatments by cash amount:</i>						
Cash \$250 (β_1)	19.69 (113.16)	-11.53 (22.39)	0.03 (4.81)	0.02 (0.02)	20.39 (9.54)	7.85 (6.33)
Cash \$500 (β_2)	287.78 (156.90)	64.24 (27.15)	9.99 (6.92)	0.03 (0.02)	9.21 (5.46)	3.58 (3.81)
Cash \$750 (β_3)	271.24 (172.91)	24.20 (25.00)	4.90 (7.03)	0.04 (0.03)	-5.73 (6.49)	-1.35 (2.32)
Control mean	823.92	128.80	27.98	0.08	16.82	7.34
Control SD	1,667.81	251.58	70.32	0.27	85.50	60.86
<i>p-values:</i>						
$\beta_1 = \beta_2 = \beta_3$	0.247	0.081	0.429	0.797	0.034	0.197
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.638	0.148	0.598	0.974	0.015	0.220
Observations	1,843	1,332	1,843	1,843	1,843	1,843
<i>Panel B. Malawi</i>						
<i>Pooled cash treatment:</i>						
Cash	323.77 (127.53)	-7.40 (6.45)	-1.66 (1.10)	-0.02 (0.01)	-4.02 (2.49)	-1.61 (1.51)
<i>Individual treatments by cash amount:</i>						
Cash \$250 (β_1)	77.43 (156.37)	-7.28 (8.90)	-1.76 (1.40)	0.00 (0.02)	-5.17 (2.79)	-1.65 (1.79)
Cash \$500 (β_2)	406.50 (186.32)	-12.92 (7.50)	-0.49 (1.43)	-0.03 (0.02)	-5.37 (3.30)	-4.25 (1.85)
Cash \$750 (β_3)	487.89 (146.28)	-2.19 (8.04)	-2.71 (1.23)	-0.01 (0.02)	-1.52 (3.20)	1.05 (2.41)
Control mean	1,621.78	61.86	7.11	0.10	14.39	6.77
Control SD	2,690.94	130.01	22.80	0.30	60.03	34.21
<i>p-values:</i>						
$\beta_1 = \beta_2 = \beta_3$	0.038	0.328	0.287	0.165	0.453	0.090
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.700	0.179	0.360	0.220	0.138	0.057
Observations	2,784	2,158	2,784	2,784	2,784	2,784

Notes: All outcomes are in US dollars PPP and winsorized at the ninety-ninth percentile. The endline was conducted about 18–22 months after first transfers were received in Liberia and 21–25 months in Malawi. Regressions include baseline measurement and strata fixed effects; for Malawi, regressions also include treatment indicators for market access and cash times market access. Results in the subpanels on pooled and individual treatments are from separate regressions. Standard errors clustered at the village level in parentheses.

^aSample restricted to households with any school-aged children (age 6–18).

^bIncludes investments in health preventatives.

also see negative point estimates on days of school missed in the past year (total and due specifically to lack of money) in both countries, as well as an increase in the proportion of days attended in the past week. Interestingly, in Malawi, we also see positive effects on school attendance.

E. Other Results

In this subsection, we discuss a few other key results. For an exhausting list of the outcomes prespecified in our preanalysis plan, please see the older version of this paper (Aggarwal, Aker, et al. 2022).

Labor Supply.—The effect of transfers on labor supply is an important question in regard to cash transfers generally, but also in this context of agricultural households in which households may choose between supplying labor in multiple occupations, and cash may affect this choice in various ways. For example, Fink, Jack, and Masiye (2020) show that credit reduces casual labor in favor of on-farm labor, and Aggarwal, Brailovskaya, and Robinson (2023) show that savings accounts given to entrepreneurs reduce labor supply in the primary business and increase agricultural labor. We show results in Table 6. In Liberia, we find a sizable decline in casual labor of 7.5 hours (almost a 50 percent decline on the control mean of 17 hours per week), and we cannot reject that effects are linear in the grant amount. However, unlike prior papers, we do not detect an increase in labor on the farm, in the business, or in other jobs, suggesting an overall decline in labor supply. In Malawi, we observe no evidence of an effect on labor supply.

IPV and Psychological Well-Being.—A key aim of this study was the measurement of IPV and psychological well-being, and we set up data collection with the goal of measuring IPV as an outcome. To the extent possible, we enrolled the female head of the household in our surveys, the entire enumeration team was female, and we extensively piloted out the IPV modules in our survey questionnaire to ensure comprehension. As part of this endeavor, we also set up the most up-to-date interview protocols to confidentiality as well as respondent confidence.³¹

We report these results in Table 7. We start by noting that these contexts differ dramatically in reported IPV—38 percent of women in Liberia report IPV of some sort in the past year (column 4), compared to 18 percent in Malawi. In Liberia, where IPV is much more prevalent, we see a large reduction in reported IPV of 8 percentage points. There are declines in all subcategories of IPV (emotional, physical, and sexual), and effects are increasing in the grant size. However, we cannot reject equality (nor linearity) for any outcome. By contrast, we see no effect in Malawi.

We also measured psychosocial well-being, and we report here our two key measures: depression as measured by the PHQ-9 index (a widely used nine-question scale designed to screen, diagnose, and monitor depression) and a “happiness index” that is based on the happiness questions that are included in the World Values Survey.³² The happiness questions measure overall life satisfaction, but also feelings of control over

³¹ During this exercise, we became aware of an open measurement issue in this area, wherein the evidence on how best to ask these types of sensitive questions is inconclusive. Therefore, we also conducted an individual-level measurement experiment in which we asked about IPV experience using either face-to-face interviewing (FTFI) or audio-computer-assisted self interviewing (ACASI). The results of that experiment are reported in Park et al. (forthcoming); the key result is that our evidence suggests that data collected via FTFI are more reliable than ACASI in this setting. As such, we rely only on FTFI reports here.

³² Responses to the five happiness and life satisfaction questions from the World Values Survey are standardized into a z-score per Anderson (2008).

TABLE 6—LABOR SUPPLY

	Number of labor hours supplied on (past month):			
	Casual labor (1)	Own farm (2)	Own enterprise (3)	Other job (4)
<i>Panel A. Liberia</i>				
<i>Pooled cash treatment:</i>				
Cash	-7.54 (1.79)	-0.04 (3.64)	-0.17 (2.00)	0.64 (0.79)
<i>Individual treatments by cash amount:</i>				
Cash \$250 (β_1)	-5.29 (2.92)	1.03 (5.77)	1.13 (3.46)	-1.24 (0.63)
Cash \$500 (β_2)	-8.28 (2.40)	-2.93 (4.89)	-2.72 (2.44)	1.29 (1.29)
Cash \$750 (β_3)	-8.94 (2.04)	2.07 (5.30)	1.37 (3.10)	1.78 (1.55)
Control mean	16.98	43.50	9.09	1.98
Control SD	39.93	70.13	38.66	14.84
<i>p-values:</i>				
$\beta_1 = \beta_2 = \beta_3$	0.486	0.709	0.422	0.044
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.472	0.707	0.408	0.026
Observations	1,843	1,843	1,843	1,843
<i>Panel B. Malawi</i>				
<i>Pooled cash treatment:</i>				
Cash	0.45 (2.37)	1.08 (0.92)	0.79 (1.29)	0.19 (1.17)
<i>Individual treatments by cash amount:</i>				
Cash \$250 (β_1)	2.13 (2.95)	-0.45 (0.98)	2.21 (1.79)	1.33 (1.49)
Cash \$500 (β_2)	-0.23 (2.90)	2.00 (1.36)	0.34 (1.61)	0.01 (1.57)
Cash \$750 (β_3)	-0.55 (2.95)	1.69 (1.27)	-0.17 (1.65)	-0.78 (1.37)
Control mean	21.90	9.85	6.21	3.69
Control SD	47.52	19.48	29.29	25.84
<i>p-values:</i>				
$\beta_1 = \beta_2 = \beta_3$	0.623	0.089	0.465	0.354
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.665	0.294	0.402	0.487
Observations	2,784	2,784	2,784	2,784

Notes: Labor supply hours are summed up between female and male household heads. Regressions include baseline measurement and strata fixed effects; for Malawi, regressions also include treatment indicators for market access and cash times market access. Results in the subpanels on pooled and individual treatments are from separate regressions. Standard errors clustered at the village level in parentheses. Nonbinary outcomes are winsorized at the ninety-ninth percentile.

one's circumstances as well as over one's financial situation. These subjective perceptions of well-being, especially those in financial matters, could arguably be impacted by cash transfers.

In Liberia, we find a 0.76 point decline in the PHQ-9 (about 0.18 SD) and a 0.38 SD increase in the happiness index. In Malawi, we find a much smaller and statistically insignificant negative point estimate in PHQ-9 and a smaller but significant 0.1

TABLE 7—IPV AND PSYCHOLOGICAL WELL-BEING

	Emotional IPV (1)	Physical IPV (2)	Sexual IPV (3)	Any IPV (4)	Depression (PHQ-9) index (5)	Happiness index ^a (6)
<i>Panel A. Liberia</i>						
<i>Pooled cash treatment:</i>						
Cash	−0.10 (0.03)	−0.04 (0.03)	−0.04 (0.02)	−0.08 (0.04)	−0.76 (0.27)	0.38 (0.05)
<i>Individual treatments by cash amount:</i>						
Cash \$250 (β_1)	−0.06 (0.05)	−0.02 (0.04)	−0.03 (0.02)	−0.04 (0.06)	−0.24 (0.47)	0.26 (0.08)
Cash \$500 (β_2)	−0.10 (0.05)	−0.05 (0.04)	−0.04 (0.02)	−0.10 (0.05)	−1.15 (0.36)	0.40 (0.08)
Cash \$750 (β_3)	−0.14 (0.05)	−0.04 (0.05)	−0.06 (0.02)	−0.10 (0.06)	−0.87 (0.42)	0.47 (0.07)
Control mean	0.34	0.23	0.10	0.38	4.76	0.00
Control SD	0.48	0.42	0.31	0.49	4.44	1.00
<i>p-values:</i>						
$\beta_1 = \beta_2 = \beta_3$	0.551	0.802	0.568	0.659	0.259	0.104
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.972	0.921	0.787	0.844	0.363	0.315
Observations	867	867	867	867	1,298	1,843
<i>Panel B. Malawi</i>						
<i>Pooled cash treatment:</i>						
Cash	0.01 (0.02)	−0.01 (0.01)	0.00 (0.01)	0.01 (0.02)	−0.15 (0.20)	0.10 (0.05)
<i>Individual treatments by cash amount:</i>						
Cash \$250 (β_1)	0.02 (0.02)	−0.01 (0.01)	0.01 (0.01)	0.02 (0.02)	−0.02 (0.27)	0.04 (0.06)
Cash \$500 (β_2)	0.00 (0.02)	0.00 (0.01)	0.01 (0.01)	0.01 (0.02)	−0.02 (0.25)	0.14 (0.07)
Cash \$750 (β_3)	0.00 (0.02)	−0.01 (0.01)	−0.01 (0.01)	−0.00 (0.02)	−0.42 (0.25)	0.12 (0.07)
Control mean	0.14	0.08	0.07	0.19	4.56	0.00
Control SD	0.35	0.27	0.25	0.39	4.22	1.00
<i>p-values:</i>						
$\beta_1 = \beta_2 = \beta_3$	0.701	0.582	0.146	0.598	0.252	0.294
$\beta_1 = \frac{1}{2}\beta_2 = \frac{1}{3}\beta_3$	0.627	0.674	0.226	0.534	0.535	0.634
Observations	1,754	1,754	1,754	1,754	2,733	2,784

Notes: IPV is measured over the past year, while psychological well-being is over the past two weeks. For IPV, IPV was measured using FTFI. Regressions include baseline measurement and strata fixed effects; for Malawi, regressions also include treatment indicators for market access and cash times market access. Results in the subpanels on pooled and individual treatments are from separate regressions. Standard errors clustered at the village level in parentheses.

^aResponses to World Values Survey happiness questions are standardized using inverse covariance weighting (Anderson 2008).

SD increase in happiness. Effects are generally increasing in the grant size, though we cannot reject equality for any outcome (nor linearity).

While important in their own right, we view these effects as further indication of a lasting effect of the transfers at the time of the endline, 18–25 months after the cash transfers, which is consistent with other evidence shown earlier.

III. Conclusion

We use high-frequency panel data and in-person surveys to measure the evolution and persistence of the effect of UCTs among farming households in rural Liberia and Malawi. We find evidence that effects on food security and expenditures attenuate after about six months, but that persistent positive effects on food security are achieved for up to two years posttransfer. Effects appear to be driven by increased agricultural output, rather than through enterprise income or other sources of cash income.

Our results confirm the commonly discussed but rarely tested notion that the measurement of treatment effects of interventions such as cash will vary over time. Our results suggest that the somewhat arbitrary timing of many endlines around a year from disbursement may actually capture lasting food security;³³ at the same time, we document that much of the impact on food security and other outcomes is missed without higher frequency data. An open question for future work is what the effects become in the next period. It is unclear whether higher harvest income and further investment will cause these effects to accrete over time, or if the effects may attenuate as the new capital stocks depreciate.

This paper raises several questions for future work. First, what size transfer would be most effective to generate lasting effects for agricultural households such as those in our study?³⁴ Theoretically, larger transfers may be better at spurring investment and therefore, generating persistent effects either if investments are lumpy (Balboni et al. 2022) or if households are close to subsistence, and therefore, a large proportion of the “smaller” transfers ends up being consumed soon after receipt—that is, if the marginal propensity to consume is diminishing in transfer size. In this paper, we find that larger transfers were more effective in inducing certain kinds of investment responses, such as in livestock and irrigation (in the case of Malawi) and farm labor (in the case of Liberia), as well as in durable assets such as housing. Yet, many other outcomes are similarly sized—for example, harvest gains are similar for the small as well as the large transfers.

We also note that in the setting that we study (and other similar contexts), households often face other constraints that may prevent them from making productive investments, such as limits in entrepreneurial ability (Banerjee, Breza, Duflo, and Kinnan 2024; Beaman, Karlan, Thuysbaert, and Udry 2023; Maitra et al. 2017), missing markets for risk mitigation (Cole, Giné, and Vickery 2017; Emerick, de Janvry, Sadoulet, and Dar 2016; Ghosh and Vats 2023; Karlan, Osei, Osei-Akoto, and Udry 2014), or, in remote contexts, poor access to markets, such as those for farm inputs (Aggarwal, Giera, et al. 2024; Cedrez, Chamberlain, Guo, and Hijmans 2020; Minten, Koru, and Stifel 2013). Therefore, one way to increase the efficacy of cash transfers may be to combine them with complementary “cash-plus” interventions. Since our study was set in an agrarian context with limited market access, we

³³For example, the endlines in Haushofer and Shapiro (2016) and McIntosh and Zeitlin (2024) were 9 and 13 months after the final transfers respectively. The first endline in Blattman, Fiala, and Martinez (2014) was after two years, a timeline similar to ours.

³⁴See Kondylis and Loeser (2024) for a discussion on this subject, structured around a meta-analysis of the UCT and ultra-poor graduation literature.

cross cut our treatment arms with an “input fair” treatment in Malawi,³⁵ which we describe in greater detail in our companion paper Aggarwal et al. (2024). Broadly, we find that the input fair treatment boosts the effect of cash-alone on input expenditures by 50 percent. While effective, market access is only one out of a large set of potential cash-plus interventions, such as entrepreneurial training and mentorship, insurance or access to savings devices. Testing and identifying the most effective of these may be a fruitful avenue for future research in this area.

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³⁵ We had originally planned to conduct the input fair intervention at both our study sites, but we missed the relevant planting season in Liberia due to the pandemic.

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