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Can Digital Aid Deliver During Humanitarian Crises?

Michael Callen,[a] Miguel Fajardo-Steinhäuser,[a] Michael G. Findley,[b] Tarek Ghani[c,*]

[a] Department of Economics, London School of Economics, London WC2A 3PH, United Kingdom; [b] Department of Government, McCombs School of Business, LBJ School of Public Affairs, University of Texas Austin, Austin, Texas 78712; [c] Olin Business School, Washington University in St. Louis, Saint Louis, Missouri 63130

*Corresponding author

Contact: m.j.callen@lse.ac.uk, https://orcid.org/0000-0002-8408-1404 (MC); m.fajardo-steinhauser@lse.ac.uk, https://orcid.org/0009-0002-8307-9692 (MF-S); mikefindley@utexas.edu, https://orcid.org/0000-0002-4830-1828 (MGF); tghani@wustl.edu, https://orcid.org/0009-0008-9042-2982 (TG)

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Abstract. Can digital payments help reduce extreme hunger? Humanitarian needs are at their highest since 1945, aid budgets are falling behind, and hunger is concentrating in fragile states where repression and aid diversion present major obstacles. In such contexts, partnering directly with governments is often neither feasible nor desirable, making private digital payment platforms a potentially useful means of delivering assistance. We experimentally evaluated digital payments to extremely poor, female-headed households in Afghanistan, as part of a partnership between community, nonprofit, and private organizations. The payments led to substantial improvements in food security and mental well-being. Despite beneficiaries' limited tech literacy, 99.75% used the payments, and stringent checks revealed no evidence of diversion. Before seeing our results, policymakers and experts are uncertain and skeptical about digital aid, consistent with the lack of prior evidence on digital payments for humanitarian response. Delivery costs are under 7 cents per dollar, which is 10 cents per dollar less than the World Food Programme's global figure for cash-based transfers. These savings can help reduce hunger without additional resources, demonstrating how hybrid partnerships utilizing digital payment platforms can help address grand challenges in difficult contexts.

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1. Introduction

Food insecurity is a systemic global challenge where policy obstacles are multiplying (George et al. 2016). Despite progress over the last half-century, global hunger levels have set new records in each of the last three years and most households experiencing food security crises now live in fragile and conflict-affected states like Afghanistan, Democratic Republic of Congo, and Yemen (Townsend et al. 2021; Food Security Information Network 2023; Foreign, Commonwealth and Development Office 2023).[1] In such settings, oppressive state and nonstate actors often seek to control resource flows and can restrict humanitarian access to the most vulnerable populations, raising donor concerns that aid will be diverted from intended beneficiaries (Kurtzer 2019, Cliffe et al. 2023). In such settings, humanitarians face a dilemma: either deliver aid and risk supporting hostile actors and exacerbating conflict (Nunn and Qian 2014) or suspend operations in the face of urgent needs. Moreover, global aid budgets are failing to keep pace with growing needs, creating pressure to reduce the costs of delivering aid while ensuring it reaches the intended beneficiaries directly.[2]

Can digital payment platforms enable more effective humanitarian crisis response? Humanitarians increasingly transfer physical cash to vulnerable individuals, but that modality is expensive and logistically challenging, is subject to diversion by hostile actors that donors do not want to support, and requires in-person contact that is increasingly denied to marginalized populations.[3] Relative to cash, digital payment systems create value by leveraging scalable technology and integrating private firms to facilitate transactions (cf. Dodgson et al. 2015, Wormald et al. 2021, Suri et al. 2023).[4] Delivering aid digitally offers the potential to reduce coordination costs and delays, increase transparency for donors and preserve privacy of beneficiaries, and utilize local supply chains without relying directly on local authorities (Byrd 2023, de Waal 2023). Although digital payment platforms are widely utilized by development programs for poverty alleviation (Muralidharan et al. 2016, Suri and Jack 2016),

adoption by humanitarians has been slowed by market and institutional frictions in fragile states (Williamson 2000, Dorobantu et al. 2017, World Food Programme 2021)—particularly given limited evidence on the efficacy of digital aid to assist vulnerable, hard-to-reach populations (Aker et al. 2016, Pazarbasioglu et al. 2020, Gentilini 2022).[5]

In this study, we conduct a randomized evaluation to study the impacts of a digital aid program in Afghanistan. Specifically, we compare a treatment group receiving digital aid payments with a comparison group which does not receive aid until later. The program was a hybrid partnership involving community, nonprofit, and private organizations. Locally elected community councils identified extremely poor, tech-illiterate, female-headed households in three cities: Kabul, Herat, and Mazar-i-Sharif. Working with an Afghan digital payment platform and a nationwide Afghan nongovernmental organization (NGO), a United States-based nonprofit organization transferred digital value vouchers to beneficiaries' mobile phones that could be spent at participating local private merchants.[6] The intervention transferred payments of $45 USD every two weeks (the equivalent of 3.75 months' worth of average monthly household income in the sample) for two months.[7] The experiment included 2,409 beneficiary households, randomly assigned to treatment and control groups. Given our study's focus on relieving human suffering, we make use of appropriate methods to measure key outcomes and to avoid false positives, including rigid adherence to a preanalysis plan (PAP).[8] Crucially, the program leveraged three enabling conditions: widespread access to mobile phones, the digital platform's robust merchant network, and sufficient market availability of food products to meet demand.

Our experiment yields three results on digital aid's efficacy. First, despite low levels of education and literacy, 99.75% of our sample successfully used their digital payments, and about 80% would not be willing to pay a 2.5% fee to have a physical "cash-out" option. Second, digital payments improved all prespecified measures of nutritional well-being, with an index of these measures increasing by 0.5σ (SE = 0.032; $p \leq 0.0001$), and all prespecified measures of mental and financial health, with an index of these measures improving by 1.5σ (SE = 0.042; $p \leq 0.0001$). Last, rigorous tests reveal no evidence of diversion either from beneficiaries or from merchants accepting digital payments. Our conservative estimates of the cost of delivery—including all aspects of recruitment and facilitation—are 6.7 cents per dollar, or less than 40% of the World Food Programme (2023) global benchmark figure of 17 cents per dollar for cash-based transfers.

Our results exceeded the expectations of experts in this field as elicited prior to the release of the study. To benchmark beliefs, we provided a precise description of the intervention to 55 international analysts, practitioners, and policymakers working on global development issues (including many engaged specifically on hunger in Afghanistan) and 36 academics, many with expertise related to digital payment systems. Asked to predict key outcomes of the study, respondents were skeptical and uncertain. On average, they predicted only 43% of women in our study would be able to use digital payments (SD = 25.67); in practice, 99.75% of women purchased goods at least once. Likewise, they predicted, on average, that local authorities would attempt to tax roughly 40% of beneficiaries (SD = 27.28); in practice, less than 2% of the treatment group reported any diversion attempts, with no statistically significant difference in the control group.

Our work builds on two extensive bodies of social science research on cash transfers—one documenting the usefulness of digital transfers to achieve development outcomes (Bastagli et al. 2019, GiveDirectly 2024) and the second focused mainly on the benefits of distributing physical cash to vulnerable populations during humanitarian crises (Jeong and Trako 2022). Whereas the former literature in development contexts often takes the existence of robust digital payment platforms for granted, the latter literature in humanitarian crises rarely engages the private sector as a delivery partner. In Online Appendix A, we systematically review the intersection of these two literatures and find limited evidence regarding digital payments to hard-to-reach populations in fragile states during humanitarian crises—the closest digital aid papers examine comparatively stable settings including refugee camps and natural disaster response (cf. de Hoop et al. 2019, Mercy Corps and Causal Design 2015). Our primary contribution is to provide clear proof-of-concept that digital transfers can cost-effectively address humanitarian needs for highly vulnerable, hard-to-reach groups while avoiding diversion. In exploring the efficacy of digitization for humanitarian response, we echo the emphasis of George et al. (2021, p. 1000) on both technological innovation and "developing business models that infuse innovations with new purpose." Indeed, the digital payment platform utilized in this study was developed prior to the Taliban's 2021 return to power with the original intent of serving a growing Afghan middle class, and only afterward shifted its focus to assist in aid delivery to the most vulnerable citizens.

Our work also contributes to a nascent strategy-related literature on grand challenges affecting large populations in difficult contexts (cf. Ballesteros et al. 2017, McGahan and Pongeluppe 2023, Fangwa et al. 2024). Scholars are increasingly focused on organizational approaches to grand challenges by analyzing the comparative strengths of public and private actors in addressing social issues (Luo and Kaul 2019, George et al. 2024). The hybrid arrangement we study in this

program—involving community, nonprofit, and private organizations—differs from most previously documented responses to global challenges in that government was not an active partner given donor and humanitarian concerns about engaging the Taliban authorities. This program relied on community councils created by the previous government, which in turn notified local Taliban authorities to ensure the safety of operations. Limiting the Taliban's opportunities to divert aid was a central motivation for digital aid delivery, and we make considerable methodological efforts to measure diversion rigorously. Given increasing awareness that many of the Sustainable Development Goals (SDGs) will not be met in 2030 absent major innovations in fragile states (Ghani and Gordon 2022; Organization for Economic Cooperation and Development (OECD) 2022; Foreign, Commonwealth and Development Office 2023), our findings indicate one hopeful pathway—technological and organizational—for the private sector to help address growing global humanitarian needs.

2. Research Design

We develop, implement, and evaluate an approach to delivering digital transfers to a vulnerable, hard-to-reach population during Afghanistan's humanitarian crisis. We briefly describe the research design in this section, with the supplementary materials providing more details. Specifically, Online Appendix B describes the context, implementation, estimation strategies, deviations from the PAP, and benchmarking of the estimated treatment effects. In addition, Online Appendix C discusses the cost-effectiveness and cost-efficiency estimation and Online Appendix D discusses ethical considerations.

2.1. Theory of Change

In principle, digital payments should enable the transfer of value to vulnerable beneficiaries who can then purchase food and thus alleviate their household's food insecurity. However, several enabling conditions must first be met: vulnerable households must have access to mobile phones, enough merchants must accept digital payments to allow convenient and competitive shopping, and the market must have enough goods to meet demand. Even if these conditions are met, other obstacles might still limit the effectiveness of digital aid: illiterate Afghan women may have difficulty utilizing digital payments, local merchants may find digital transactions overly cumbersome, and the Taliban authorities may decide to interfere.

Following an extensive piloting process, this digital aid program was designed with the goal of ensuring a successful implementation despite the challenges posed by the context and population. Beneficiaries were selected through locally elected Community Development Councils (CDCs), who informed local Taliban authorities of their activities but denied them influence over participant selection. Tech illiteracy was addressed through a careful onboarding process described below, which also ensured that merchants were well prepared to meet the needs of this population. Finally, research procedures were put in place to ensure high-quality data and valid inference procedures to minimize the risk of false positives.

2.2. Partners

The program was a hybrid arrangement designed to minimize interference by Taliban authorities and address common issues facing digital transfers—including technical issues like interoperability and smartphone ownership and social issues like tech literacy and trust. The research team composed of academics designed all research-related components of the project. Locally elected CDCs identified female-headed households as program beneficiaries through community-level, participatory meetings and facilitated their engagement in the program. Uplift Afghanistan, a United States-based nonprofit, received grant funding from a private foundation and managed transfers to beneficiaries. Lastly, HesabPay, a digital payment platform that was compatible across mobile network operators, facilitated transactions through its network of private merchants. This division of labor is consistent with Luo and Kaul (2019), who argue for-profit firms are best positioned to innovate (as in the case of building a digital payment ecosystem), self-governing collectives have a comparative advantage in private ordering (as in determining which potential beneficiaries to prioritize), and nonprofit organizations are well suited for fiduciary roles. All partners shared a common interest in testing whether digital payments were a viable and attractive channel for humanitarian aid.

2.3. HesabPay's Technology and Merchant Network

HesabPay is a digital payment platform that is interoperable across Afghanistan's mobile networks and transfers value either using a smartphone app or via a feature phone using a combination of transaction initiation via QR code and transaction verification via feature phone unstructured supplementary service data (USSD).[9] HesabPay uses the Algorand blockchain as a settlement layer, facilitating finalization and recording of digital transactions; HesabPay users are automatically registered for a custodial Algorand wallet (e.g., HesabPay holds their private keys on their behalf), and all transactions are automatically recorded on the Algorand blockchain without disclosing user identities. HesabPay is licensed by Afghanistan's Central Bank as a financial service provider, similar to the regulatory

framework for HesabPay's main competition from Afghanistan's mobile money operators.

At the time of the study, HesabPay had an active and growing local merchant acceptance network in Afghanistan's major cities, composed of over 1,000 preexisting Afghan businesses providing basic foodstuffs and other household items for sale in the same neighborhoods as beneficiaries. In this program, aid payments took the form of a digital value voucher denominated in local currency that could be exchanged for any available goods from any HesabPay-enrolled merchant. All beneficiaries conducted a test purchase with a nearby merchant immediately following their onboarding session. Although many chose to return to that specific merchant for future purchases, we confirm broader engagement with the acceptance network using beneficiaries' transaction data from the digital payment provider. Online Appendix Table 1 reports the total number of merchants serving beneficiaries in the transaction data, which is consistently larger than the number from onboarding sessions.[10] Online Appendix Figure 1 maps the locations of these merchants for each of the three cities in which we conducted the intervention, visually demonstrating the decentralized distribution network.

2.4. Beneficiary Identification and Onboarding

Our goal was to identify about 2,400 vulnerable women in three Afghan cities (Kabul, Herat, and Mazar-i-Sharif) to be part of the intervention. To do so, we worked with the Community Driven Development Organization (CDDO), an Afghan organization that assists CDCs in a wide array of local activities.[11] Local CDCs identified potential beneficiaries through a well-being analysis in which community members, elders, and mullahs together categorize all community households into different socioeconomic groups (e.g., well-off, middle-income, poor, very poor). Our participants come from the lowest group. Thus, participants are identified through a process relying on the community deliberation regarding who is most vulnerable. We note that ownership of a feature phone was not a binding constraint for any beneficiaries, reflecting high nationwide levels of mobile phone ownership.[12]

After identification, participants were invited to local onboarding sessions where four activities took place.[13] First, the program was described to them. Second, if they wanted to participate, their informed consent was collected and a baseline survey was administered. Third, each woman opened an account with HesabPay, an Afghan commercial digital payment platform. Finally, each woman completed a test purchase with a nearby private merchant using the platform.[14] The CDDO, working together with local CDCs, ensured the safety of staff and beneficiaries by informing local authorities and maintaining independence from external interference in the beneficiary selection process.

2.5. Intervention and Randomization

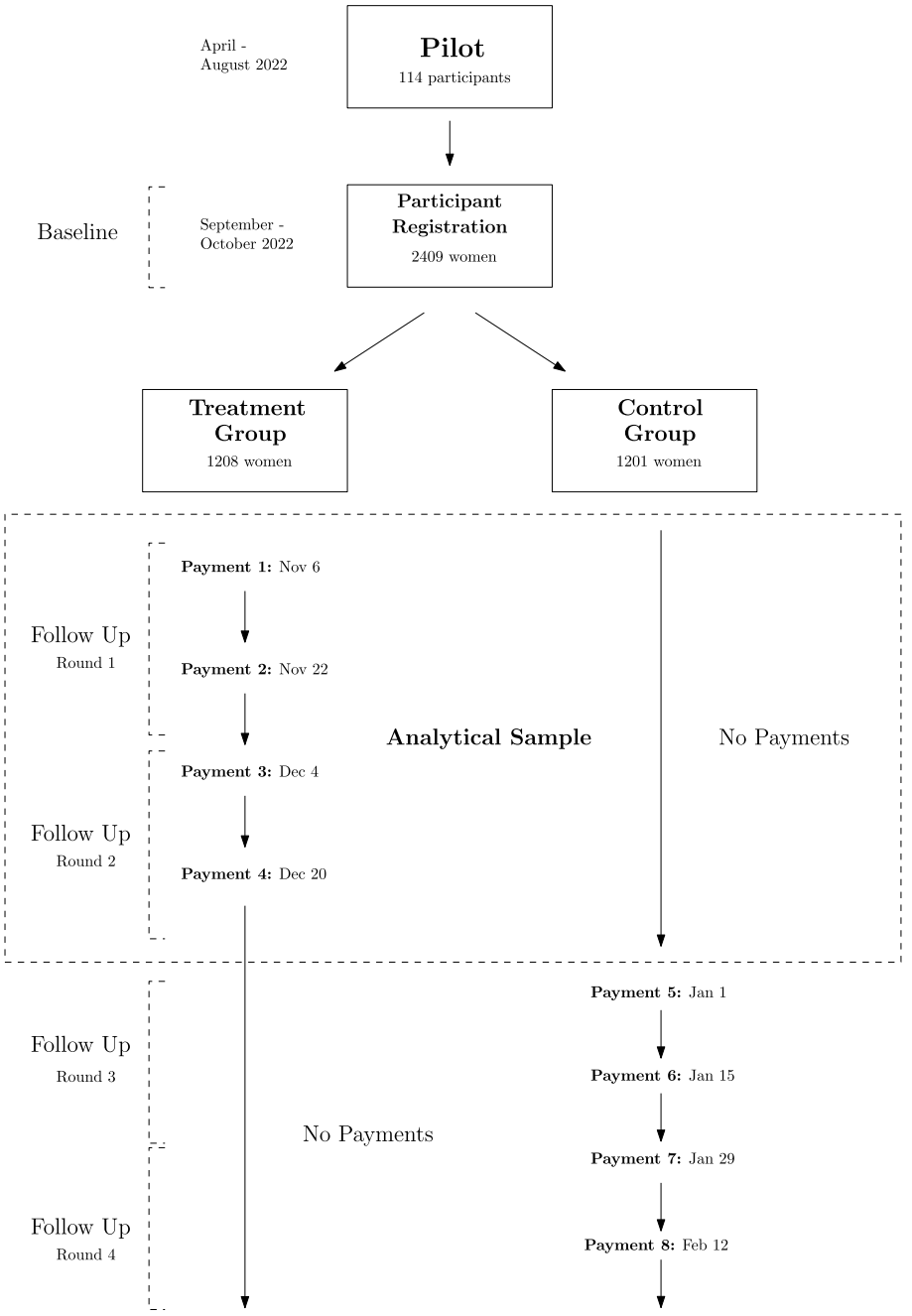
Uplift Afghanistan transferred digital value vouchers to beneficiaries' mobile phones that were redeemable at HesabPay's acceptance network of local merchants for any available goods. Specifically, the intervention transferred 4,000 AFN (approximately $45 USD) every two weeks for two months to households in 16 urban neighborhoods in Kabul, Herat, and Mazar-i-Sharif. From our experimental sample of 2,409 households, we randomly assigned 1,208 households to an "early" group, which received benefits from November 6, 2022, until December 31, 2022 (henceforth, the treatment group). The remaining 1,201 households formed the "late" group, which received benefits from January 1, 2023 (two weeks after the "early" group stopped receiving payments) until February 28, 2023 (henceforth, the control group). The randomization was stratified on two variables: the nahia (neighborhood) in which they registered (our study includes beneficiaries from neighborhoods), and a measure of vulnerability.[15] As can be observed in Online Appendix Table 2, the treatment groups are balanced on 17 of the 18 outcome and heterogeneity variables we collected at baseline.[16]

Although all participants understood they would eventually receive transfers, randomization took place after all onboarding sessions were completed and we only informed participants in both groups that they were going to start receiving their payments a few days before their first payment. Note that this is not a setting where consumption smoothing was feasible given widespread and acute hunger.[17] We identify causal impact by using the late group as a control for the treated early group during the two months in which the early group was receiving its payments and the late group was not receiving any payments. The study design and CONSORT flow diagram are depicted in Figure 1. Online Appendix D details the ethical considerations addressed in the design and implementation of the study.[18]

2.6. Data Collection

Data were collected four ways. First, a baseline survey was completed during the onboarding session (see above). Second, we had access to participants' transaction data from the mobile payment provider, which we could link to participants' survey data. We obtained permission to do so during the consent process. Third, we conducted four rounds of follow-up surveys over the phone.[19] Fourth, we attempted to survey all 26 merchants who facilitated initial test transactions with beneficiaries, and succeeded in contacting 19, to confirm how beneficiaries had used their payments and to check whether they were asked to provide favors or extralegal taxes to local authorities.

Figure 1. Intervention Timeline and Design



We prespecified all analyses in a PAP, including how the outcome variables would be constructed and what our primary outcome variables were going to be. We divided our outcome variables in three families: basic needs (also called "food security" in the manuscript), well-being, and informal taxation (also called "diversion" in the manuscript). In doing so, we tried to provide a level of detail consistent with the standard articulated in Duflo et al. (2020). This standard requires that two research assistants can take the data and the PAP, and with only these two items separately produce identical analysis. For each of these three families of outcomes, we create a summary index following Kling et al. (2007), called the Kling-Liebman-Katz (KLK) index.

2.7. Methodology

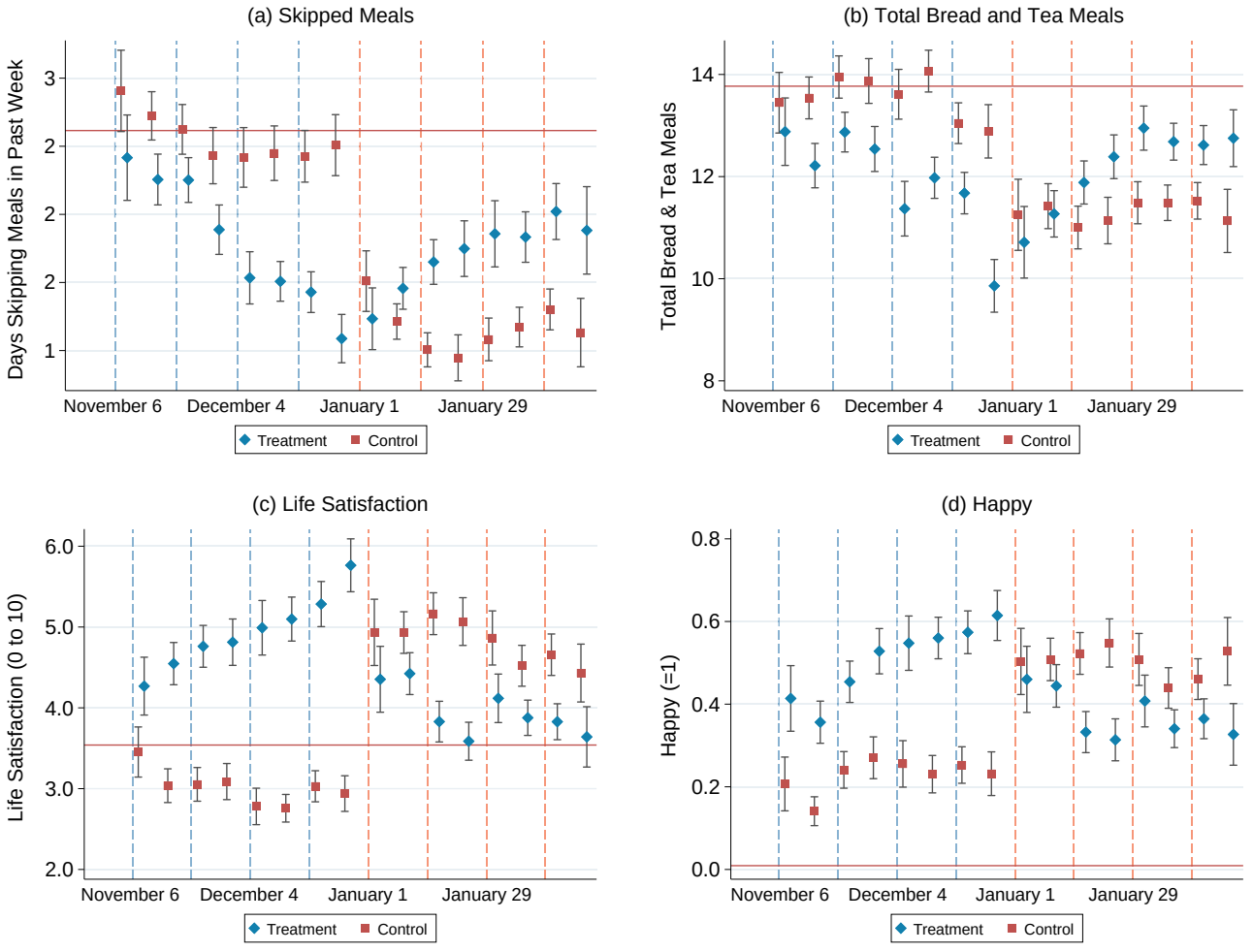
To estimate treatment effects, we use linear regressions (see Online Appendix B.2 for estimating equations). We study the intervention's effect by regressing the prespecified outcome variables on an indicator for treatment assignment (baseline analysis). We control

for the outcome variable's baseline value (if available), stratum fixed effects, and survey round fixed effects. Standard errors are clustered at the level of treatment assignment (the participant). Note that because of very high response rates and no compliance issues, we do not conduct treatment-on-the-treated regressions. To avoid false discoveries due to multiple hypothesis testing, we control for the family-wise error rate (FWER) for each family of primary outcomes and control for the false discovery rate for secondary outcomes as prespecified in our PAP.

We conduct several complementary analyses as well, which were also prespecified. First, we study how the effects change over time by including an interaction term between the treatment assignment indicator and an indicator for the second survey round. We also study dynamic effects graphically by computing the means of given outcome variables at the survey week level for treatment and control groups separately, as shown in Figure 2. Second, we study whether the effects are heterogeneous along several (prespecified) variables, by interacting each of these variables with the treatment assignment indicator.

We check whether experimenter demand effects are present by randomly informing participants about the goals of the study (see Online Appendix B.3 for details). We analyze whether providing information about the goals of the study impacted survey responses in two ways: regressing each outcome on an indicator for the informational assignment indicator, and including an interaction term between an informational assignment indicator and the treatment assignment indicator (to see whether the information affects participants in the treatment and control groups separately).

Figure 2. (Color online) Effects over Entire Duration of Program



Notes. The horizontal line corresponds to the mean of the variable at baseline across all individuals in the sample. Squares (diamonds) show the mean value of the variable among control group (treatment group) respondents divided in two-week bins since the start of the payments. Vertical lines represent the dates in which participants received their payments, where the first four vertical lines represent payments to the treatment group and the next four vertical lines represent payments to the control group. Note that this is the only analysis in which we are using/showing data for the period after the control group starts receiving payments (i.e., whereas the rest of the paper uses only data from survey rounds 1 and 2, this shows in addition data from survey rounds 3 and 4, after the control group starts receiving payments). Bars show 95% confidence intervals.

3. Results

We organize our main results around the following primary research questions (RQs):

- RQ1: Can extremely poor, tech-illiterate women use digital transfers in Afghanistan?
- RQ2: Do digital transfers improve food security and mental well-being?
- RQ3: Are digital transfers diverted?
- RQ4: How much does it cost to deliver digital aid?

3.1. Can Extremely Poor Women Use Digital Transfers?

Can those in need use digital payments? This is a fundamental question for less technology-literate samples such as ours, where 63.3% of the women have no schooling and 33.9% have at most primary education.[20] The results indicate high levels of usage: Nearly all of the women in our treatment group (99.75%) used their digital payments to buy goods.[21]

Ninety-eight percent of the total value transferred in the four payments was spent in the first eight weeks. It is important to note that the degree of success in utilization was largely the result of an effective division of labor between program partners, an emphasis on human-centered design, and extensive piloting (Online Appendix B.1 describes the piloting process). In particular, guiding beneficiaries through a test transaction during onboarding greatly increased comfort with digital payments.

Beyond the high levels of usage, three additional results support the argument that this tech-illiterate population can use digital transfers. First, 20.9% of the funds were spent at different merchants than those who facilitated an initial test transaction during onboarding, indicating that beneficiaries understood they could use the payment at any participating merchant. Second, following our PAP, we also checked whether impacts on food security outcomes vary by preintervention need, city, marital status, age, household size, education, and whether the recipient was the primary household financial decision maker. We found no heterogeneity in impacts, consistent with the technology being roughly equally useful for the different groups in our study (Online Appendix Figure 2).

Finally, we asked participants in a hypothetical exercise whether they would prefer to receive their full 4,000 AFN payments digitally, or $4{,}000 - X$ AFN in cash, where X was either 100 AFN, 300 AFN, or 500 AFN. These amounts might reflect the costs required to provide aid as physical cash (see Online Appendix B.2 for details). With a conservative fee of 100 AFN (2.5%), 80% of participants preferred digital aid over cash; with a higher fee of 300 AFN (7.5%), the share choosing digital aid over cash increased to ~95% (Online Appendix Figure 3). Collectively, the results indicate that digital payments are a viable option even when people have limited experience using digital technology.

3.2. Do Digital Transfers Improve Food Security and Mental Well-Being?

The payments reduced all four prespecified measures of food insecurity by meaningful amounts (see Table 1, panel A, for estimates and control means). Beneficiaries, on average, skipped meals on 0.76 fewer days per week (SE = 0.051, $p \leq 0.0001$), children were 11.7 percentage points less likely to have skipped meals over the past week (SE = 0.012, $p \leq 0.0001$), the share of households where everyone was able to eat regularly during the prior week increased by 9.3 percentage points (SE = 0.015, $p \leq 0.0001$), and beneficiaries reduced meals of only bread and tea by 1.608 (SE = 0.121, $p \leq 0.0001$). An index of these four measures—constructed as the average of the standardized measures following Kling et al. (2007)—improved by 0.5 SDs (SE = 0.032, $p \leq 0.0001$).

Under secondary outcomes, we find evidence of a more diverse diet (see Table 1, panel B, for estimates and control means). With a recall period of one week prior to the survey, beneficiaries ate rice on 0.6 days (SE = 0.035, $p \leq 0.0001$), beans on 0.49 days (SE = 0.029, $p \leq 0.0001$), chicken on 0.01 days (SE = 0.006, $p = 0.035$), and dairy on 0.05 days (SE = 0.013, $p \leq 0.0001$) more than the control group. Participants also report an increased ability to purchase medicine when needed. Consistent with preregistered expectations in our PAP, we do not find any increases in outside income, employment, or agency over financial decisions.[22]

Primary food security outcomes kept improving for the two months during which beneficiaries were paid (Table 2), and more modest improvements remain for at least two months after payments conclude (Figure 2). Regarding dietary diversity, estimates reported in Online Appendix Table 3 indicate that consumption of beans and rice is increasing over time.

To assess if these estimated impacts are in line with what we should expect given the size of the payments, we collected data from a subsample of households on what they were purchasing (e.g., wheat flour, cooking oil, and sugar) and price data from merchants. We calculate that the cost of a basket of popular goods to support a family for two weeks is consistent with the aid payment size plus the survey participation incentive (Bete 2022). Although not eliminated, skipped meals declined after each payment (Figure 2(a)). Online Appendix B.5 provides further discussion of the results for magnitudes of needs.

Turning to mental well-being, since the Taliban takeover in 2021, Afghanistan consistently ranks as the country with the lowest levels of happiness (Evans 2022, Ray 2022). Our participants report extremely low

Table 1. Summary Table—Treatment Effects

	Control mean (1)	Control SD (2)	Treatment effect (3)	Standard error (4)	Naïve *p*-value (5)	Adjusted *p*-value (6)	*N* (7)
Panel A: Primary outcomes							
Days skipping meals (past week)	2.569	1.717	−0.76	0.051	0	0.0002	4,761
Children skipping meals (=1)	0.873	0.333	−0.117	0.012	0	0.0002	4,750
Regularly eat twice a day	0.501	0.5	0.093	0.015	0	0.0002	4,736
Total bread and tea meals (past week)	13.639	3.549	−1.608	0.121	0	0.0002	4,763
Food security—KLK index	0	1	0.501	0.032	0		4,763
Better economic situation	0.048	0.213	0.335	0.011	0	0.0002	4,762
Satisfied with fin. situation	0.133	0.34	0.263	0.012	0	0.0002	4,755
Happy	0.154	0.361	0.28	0.014	0	0.0002	4,732
Life satisfaction	3.179	1.707	1.963	0.068	0	0.0002	4,763
Economic/well-being—KLK index	0	1	1.498	0.042	0		4,763
Panel B: Secondary outcomes							
Days eating rice (past week)	0.698	1.058	0.597	0.035	0	0.001	4,763
Days eating beans (past week)	0.518	0.836	0.493	0.029	0	0.001	4,763
Days eating vegetables (past week)	1.394	1.346	−0.003	0.041	0.934	0.453	4,763
Days eating chicken (past week)	0.021	0.147	0.012	0.006	0.035	0.037	4,763
Days eating dairy (past week)	0.074	0.383	0.047	0.013	0.001	0.002	4,763
Able to buy medicine	0.051	0.221	0.034	0.01	0.001	0.002	3,582
Involved in fin. decisions	0.666	0.472	0.017	0.015	0.272	0.185	4,757
Total household income (past month)	876.683	1,581.531	128.482	90.93	0.158	0.118	4,763
Household's head employed (past month)	0.192	0.394	−0.008	0.014	0.578	0.311	4,741

Notes. Stratification fixed effects, survey round fixed effects, and baseline values of dependent variables, if available, are included. Standard errors are clustered at the individual level. Primary outcomes show FWER-adjusted *p*-values within each family outcome (following Romano and Wolf 2005, using 5,000 repetitions), whereas secondary outcomes show false-discovery-rate (FDR)-adjusted *p*-values (following Anderson 2008). The KLK index is created following Kling et al. (2007), and is the equally weighted sum of the standardized component variables. Better economic situation is an index that equals one if the respondent answered that her economic situation compared with 30 days ago is slightly or much better, and zero otherwise. Satisfied with financial situation is a dummy that equals one if the respondent answered that she agrees a lot or somewhat with the statement that she is highly satisfied with her current financial condition, and zero otherwise. Happy is a dummy that equals one if respondent said that she was very happy or quite happy, and zero otherwise. Life satisfaction is the score from 1 (dissatisfied) to 10 (satisfied) in terms of how satisfied the respondent is with her life as a whole these days. Total household income excludes the aid payments. fin., financial.

levels of happiness and financial health. Treatment improved all four prespecified measures of mental and financial health, with an index of these measures improving by 1.5 SDs (Table 1, panel B), although from a very low base and although these impacts disappear as soon as payment stops (Figure 2). Specifically, beneficiaries are 33.5 percentage points more likely to report that they feel the economic situation of their household has improved compared with 30 days ago (SE = 0.011, $p \leq 0.0001$), from a base of just 4.8% in the control group. They are also 26.3 percentage points more likely to report being satisfied with their current financial situation (SE = 0.012, $p \leq 0.0001$). The intervention also increased measures of mental well-being: Beneficiaries are 28 percentage points more likely to report being very or quite happy (SE = 0.014, $p \leq 0.0001$), and report a score 1.96 higher on the Cantril Self-Anchoring Striving Scale, an established measure of life satisfaction that runs from 1 to 10 (SE = 0.068, $p \leq 0.0001$), relative to the control group.[23] These effects are increasing over time, with the effects after receiving three to four aid payments between 40% and 100% larger than after one to two aid payments (Table 2, panel B).[24]

Our results are robust to common concerns in randomized control trials (RCTs). First, we find no evidence of treatment beneficiaries providing part of their payments to control beneficiaries or of control beneficiaries borrowing against payments they would receive in the last two months of the program (see Online Appendix B.3). Second, given that participants had the option of skipping questions, and to keep the sample comparable, we reestimate the results restricting the sample to those who answered all questions relevant for the analysis. Results using this restricted sample are almost identical to those estimated on the complete sample (see Online Appendix Table 5).

Finally, survey bias in the absence of objective measures of well-being (such as anthropometric or biometric measures) is a concern, especially because participants might respond strategically if they believe this could influence their benefits. To check for potential strategic reporting by participants, we primed half of our respondents with an explicit description of the study's purpose. Specifically, in the second round of follow-up surveys, we randomly assigned individuals into two groups. A "primed" group hears the following

Table 2. Short vs. Long Treatment Effects—Food Security and Well-Being Outcomes

	(1)	(2)	(3)	(4)	(5)
	Panel A: Food security				
	Days skipping meals	Children skipping meals (=1)	Regularly eat twice a day (=1)	Total bread and tea meals	KLK index
β_1: treated × round 2	−0.580***	−0.096***	−0.005	−0.948***	0.309***
	(0.094)	(0.021)	(0.028)	(0.219)	(0.056)
β_2: round 2	−0.195***	0.025*	0.072***	−0.322**	0.096**
	(0.072)	(0.013)	(0.020)	(0.151)	(0.038)
β_3: treated	−0.470***	−0.069***	0.096***	−1.134***	0.346***
	(0.072)	(0.015)	(0.020)	(0.162)	(0.042)
Control mean	2.569	0.873	0.501	13.639	0.000
Observations	4,761	4,750	4,736	4,763	4,763
$(\beta_1+\beta_2+\beta_3)/\beta_3$	2.650	2.040	1.700	2.120	2.170
	Panel B: Well-being				
	Better economic situation	Satisfied with financial sit.	Happy	Life satisfaction	KLK index
β_1: treated × round 2	0.219***	0.200***	0.102***	0.842***	0.837***
	(0.021)	(0.024)	(0.026)	(0.127)	(0.078)
β_2: round 2	−0.012	−0.036***	0.030*	−0.223***	−0.073*
	(0.009)	(0.014)	(0.017)	(0.075)	(0.040)
β_3: treated	0.226***	0.163***	0.229***	1.542***	1.079***
	(0.015)	(0.017)	(0.019)	(0.093)	(0.055)
Control mean	0.048	0.133	0.154	3.179	0.000
Observations	4,762	4,755	4,732	4,763	4,763
$(\beta_1+\beta_2+\beta_3)/\beta_3$	1.910	2.010	1.580	1.400	1.710

Notes. This table reports estimated impacts of treatment separately for the first and second survey round. Households were surveyed once per month for two months. Each of these months constitutes a survey round. All specifications control for stratum fixed effects and the baseline value of the dependent variable, if available. Standard errors are clustered at individual level. The outcome variables follow the primary outcomes shown in Table 1. sit., situation.

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

statement just before the questions related to needs: "I would now like to ask you a few questions about how you and your family are doing. The goal of the CDDO and HesabPay program is to help you and your family meet basic needs, such as buying food, and we would like to see how you are doing in this regard. We will share what we learn from interviewing participants like yourself, with international organizations who are trying to help Afghans deal with these difficult times." Thus, this group is explicitly told what we are expecting to find. The "not primed" group hears this placebo statement instead: "I would now like to ask you a few questions about how you and your family are doing." We find no evidence that the prime influenced responses (see Online Appendix B.3 and Online Appendix Table 4).

3.3. Are Digital Transfers Diverted?

An important concern about distributing aid in fragile settings is diversion, especially to regimes with poor human rights records and to those that sponsor international terror. Reports of aid diversion across multiple fragile countries have emerged in the past year (Chiappa 2023, O'Donnell 2023, UN News 2023) and have been documented extensively for decades (Barnett 2013) with scholarly evidence suggesting that diversion prolongs conflict (Nunn and Qian 2014, Findley 2018). This is especially a concern in Afghanistan. In July of 2023, the U.S. House of Representatives passed a bill that would bar the Secretary of State and the U.S. Agency for International Development (USAID) Administrator from giving any funds to Afghanistan if they directly or indirectly supported the Taliban (Binesh 2023). Reports of Taliban aid requirements and infiltration of United Nations (UN) assistance (O'Donnell 2023, Ott and Yousafzai 2023) are deepening these concerns, adding further pressure to cut assistance. Therefore, the humanitarian mandate to address hunger depends partly on avoiding diversion.

There are at least four ways that hostile regimes might capture aid. First, they might influence who is eligible, including by creating fictitious "ghost" beneficiaries. Second, they can ask recipients to hand over aid transfers. Third, they can capture aid while in transit. Fourth, they can ask merchants or payment platforms who are serving aid beneficiaries to pay bribes or additional tax. We organize the presentation of results around these four potential diversion strategies and discuss whether digital delivery might constrain them.

Such diversion strategies are widely documented both in Afghanistan (Sopko 2023, Special Inspector General for Afghanistan Reconstruction 2023) and in other countries (see Online Appendix A.1 for a discussion of diversion of humanitarian aid in the literature).

3.3.1. Influencing Eligibility. In our study, beneficiaries were identified by local elected community councils in consultation with communities using a "well-being analysis" described in Online Appendix B. The prevalence of mobile phones enables phone surveys as a means of checking that beneficiaries are indeed vulnerable, even at large scale. Our surveys confirm that beneficiaries were quite vulnerable (see Online Appendix Table 6). The digital payment platform also requires Know Your Customer identity verification, which can automatically compare beneficiary names against sanction lists to further guard against capture by hostile actors. Evidence from a diverse set of global contexts (Guggenheim and Petrie 2022, Samii 2023) and the pre-2021 Afghanistan National Solidarity Program (NSP) specifically (Beath et al. 2018, Burde et al. 2023) also indicates that most diversion occurs at national and regional levels and primarily through bureaucratic and partisan channels. At the local level, in contrast, CDCs develop transparency and monitoring mechanisms to prevent diversion, making it more challenging for local authorities to interfere in influencing eligibility.

3.3.2. Taxing Beneficiaries. To check for taxation, we directly asked beneficiaries whether they have been asked for informal assistance. Specifically, we asked them whether local community leaders or government officials have asked them for any kind of assistance, such as food or money. Given that participants may be reluctant to disclose doing this themselves, we first asked them whether they know someone in their community who has been asked to do so (Reinikka and Svensson 2006), and then whether they themselves have been asked. Results are presented in the first four columns of Table 3, panel A, which show that the treatment group does not report informal payments to authorities in larger proportion than the control group regardless of how the question was framed (all coefficients are insignificant at traditional levels, and precisely estimated). When looking at the KLK index combining the four individual questions (column 5), the coefficient is marginally significant at the 10% level. However, this is driven by a single individual who answered yes to three of the four questions, which,

Table 3. Are Digital Payments Diverted?

	Gov. off. others (1)	Comm. leader others (2)	Gov. off. you (3)	Comm. leader you (4)	KLK index (5)	Yes to any question (6)
Panel A: Baseline						
Treated	0.001	0.002	0.002	0.002	0.074*	0.003
	(0.001)	(0.002)	(0.001)	(0.003)	(0.042)	(0.003)
Observations	4,558	4,596	4,631	4,626	4,648	4,509
Control mean	0.002	0.002	0.001	0.007	0.012	0.009
Panel B: Long run						
β_1: treated × round 2	−0.000	0.006*	0.002	−0.001	0.091	0.004
	(0.003)	(0.003)	(0.003)	(0.005)	(0.084)	(0.006)
β_2: round 2	−0.002	−0.003	−0.003*	−0.008**	−0.137***	−0.010**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.045)	(0.004)
β_3: treated	0.001	−0.007	−0.001	0.003	−0.062	−0.004
	(0.005)	(0.005)	(0.005)	(0.009)	(0.140)	(0.011)
$(\beta_1+\beta_2+\beta_3)/\beta_3$	−0.770	0.500	1.880	−1.680	1.740	2.550
Observations	4,558	4,596	4,631	4,626	4,648	4,509

Panel C: List experiment	All sample	Late sample	Early sample
Treated list	0.011	0.019	0.008
	(0.023)	(0.032)	(0.032)
Observations	2,358	1,171	1,187
Mean items mentioned	0.746	0.691	0.800

Notes. In panels A and B, stratification fixed effects, survey round fixed effects, and baseline values of dependent variables, if available, are included. These are answers to questions of the type "Have you/someone in your community been asked to provide informal assistance (for example money or food) to local community leaders/government officials in the past month?" The outcome in column 6 was not prespecified. Panel C shows the results of a list experiment where the treatment group received the following additional statement: "I have been approached by government officials or community leaders to provide them with any kind of assistance, like food or money, in the past month." This includes a control for surveyor fixed effects. Standard errors are clustered at the individual level. Gov., government; Comm., community; off., official.

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$.

because of the way the index is constructed, receives an extremely high index value of 34 standard deviations. Removing this observation leads the results using the KLK index to lose statistical significance at conventional levels (column 4 in Online Appendix Table 7).

We find very low levels of informal taxation: Overall, only 27 beneficiaries in the treatment group answered yes to any of these four questions since payments started, compared with 21 beneficiaries in the control group. When using an indicator for whether the respondent answered yes to any of the four informal taxation questions, the difference is statistically insignificant (Table 3, column 6).[25]

Estimates in Table 3, panel B, indicate that the results are also not increasing over time, when beneficiaries could have become more visible to local authorities. Based on qualitative debriefs with a subsample of our respondents, one potential reason for limited diversion is that participants are too poor for local authorities to ask them for payments. Moreover, it is politically and logistically costly to tax vulnerable beneficiaries after they have received their payments.

Other surveys in Afghanistan indicate that significant shares of Afghans are comfortable reporting corrupt behaviors, at least before the Taliban takeover (Asia Foundation 2019). Even so, given the authoritarian context and the fact that these are questions that may be sensitive for participants (Blair et al. 2020), there could be a lack of positive responses because of fear of reporting inappropriate behavior by local authorities. We therefore conducted a list experiment, an established method to measure the presence of sensitive behaviors, with beneficiaries to gauge the extent of informal taxation (see Online Appendix B.2 for details). Table 3, panel C, shows the results of the list experiment. Regardless of whether we analyze the whole sample or either of the two treatment groups, individuals who receive the longer list, which includes the informal taxation statement, do not report experiencing more items on the list than those receiving the shorter list. The effects are precisely estimated and small in magnitude, supporting the argument that diversion was minimal.

We also have access to the beneficiaries' transaction data from the digital payment provider, which, coupled with the fact that they could not cash out their payments and that they spent almost all the money they received, provide a clear picture of how beneficiaries spent their funds. Beneficiaries spent 74.2% of their funds at the merchants they visited during the beneficiaries' onboarding sessions, 20.9% at other registered merchants, 3.5% at individually registered accounts, and 0.1% on airtime purchases. Combined with the fact that almost no respondents indicate providing assistance to local authorities, this also supports the argument that there was little diversion. Moreover, only 6.7% of beneficiaries reported that someone else decided how to use the aid payments—all of whom were other household members.

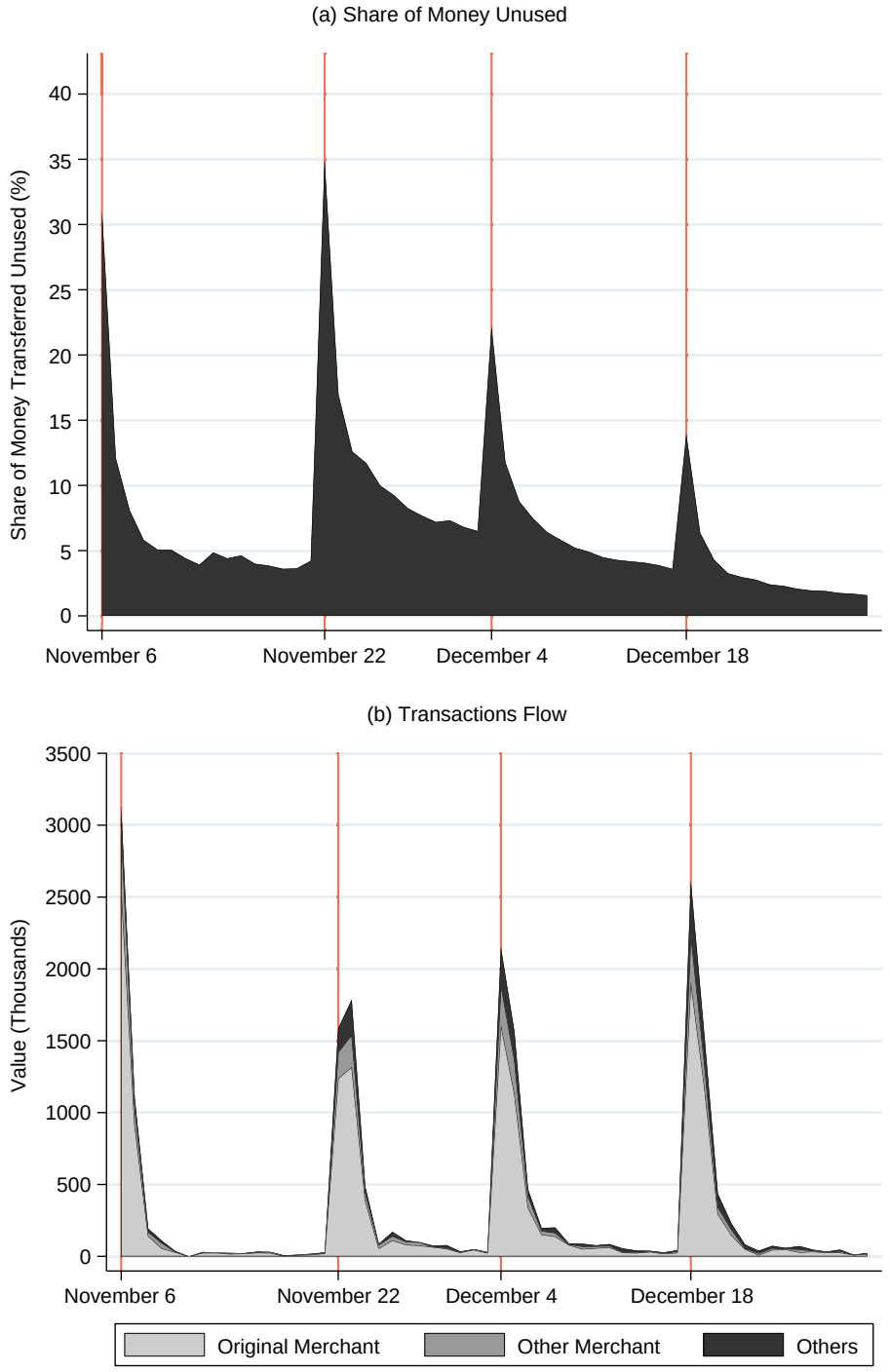
Importantly, once beneficiaries are onboarded, distribution does not require travel outside the home as it might with food or cash distributions. The distribution of digital aid is therefore less visible than cash or in-kind distribution. Finally, the distribution of digital payments is instantaneous, with recipients able to access their funds immediately. Figure 3(a) displays the share of the cumulative funds participants have received that remains unspent over time, and shows that participants spent over 70% of the funds they received the day they received their first payment. This drops quickly over the next few days to below 5% of the received funds. A similar pattern holds every time participants receive their funds. Figure 3(b) displays how participants spent their money on a daily basis.

3.3.3. Capturing Aid in Transit. Relative to in-kind or physical cash payments, which rely on intermediaries who staff convoys or physical distribution points, digital payments may reduce the opportunities for in-transit theft by transferring aid directly to beneficiaries. Also, with automatically generated transaction data, donors and humanitarian agencies gain increased transparency on the delivery and utilization of cash-based assistance programs that can be used for auditing and real-time programming adjustments. The digital payment platform in this study utilizes the Algorand blockchain, so all payments and purchases were automatically recorded on an immutable ledger that facilitates external auditing. Finally, phone surveys also provide a scalable means of confirming the delivery of benefits, as also demonstrated in Muralidharan et al. (2021).

3.3.4. Taxing Merchants or Intermediaries. The government has the legal authority to tax both merchants and digital payment platforms. To check for merchant taxation, we surveyed 26 merchants who assisted beneficiaries with an initial test transaction to ask about extralegal taxes (merchants do need to pay ordinary sales tax on any transactions). None of the merchants reported paying extralegal taxes, being asked to provide favors, or being aware of other merchants paying extralegal taxes or providing favors.

In our study the digital payment provider reported no attempts at extralegal taxation. This could plausibly change for a larger initiative. In this scenario, a single tax would be levied on a single provider. Economic theory suggests a single, negotiated tax is preferable to numerous, less-organized attempts at interception, potentially reducing overall diversion (Shleifer and Vishny 1993).

Figure 3. (Color online) Account Usage over Time



Notes. Panel (a) shows the cumulative share of money that participants in the treated group have not spent. Panel (b) shows where participants spent their funds. "Original merchant" is the merchant a participant visited during the onboarding session to conduct their test purchase. "Other merchant" is another account that belongs to either a different test merchant or another merchant that did not participate in the onboarding sessions.

3.4. How Much Does It Cost to Deliver Digital Aid?

The cost-efficiency of digital aid, or the program costs required to serve a given number of beneficiaries, outperforms comparable estimates. The total cost per beneficiary (CPB) of delivering the entire \$180 (across four disbursements) to our beneficiaries is \$2.44 USD excluding recruitment costs or \$12 USD including recruitment costs. Correspondingly, the cost to deliver a single dollar including the transferred dollar, termed

the total cost transfer ratio (TCTR), is either \$1.014 (\$182.44/\$180.00) or \$1.067 (\$192.00/\$180.00). Excluding the transferred dollar, the cost transfer ratios (CTRs) are either 1.4 cents when recruitment costs are excluded or 6.7 cents when recruitment costs are included. For comparison, the global CTR of the World Food Programme (WFP) for providing cash-based humanitarian aid is 17 cents per dollar (World Food Programme 2023). To contextualize this difference, we estimate that if the WFP had delivered all \$357M of its 2022 cash-based assistance in Afghanistan digitally, the savings would be sufficient to support an additional 77,000 households or 538,075 individuals for the four-month lean season.

We also computed the costs required to produce a given level of program impact, a measure of cost-effectiveness. Online Appendix C details these cost-efficiency, scale, and cost-effectiveness estimates and reports ranges of estimates based on more or less conservative assumptions. The cost-efficiency estimates compare favorably to other cash-based transfer programs in humanitarian contexts. Although cost analysis is rarely performed for humanitarian cash programs (Gentilini 2016, Jeong and Trako 2022), we identified three organization-specific global estimates and 12 individual studies with estimates. When cost categories are comparable, all of these organizations and studies have higher costs of delivery than our digital aid intervention.

Digital aid offers further advantages to donors and humanitarian agencies in terms of increased decentralization and transparency compared with status quo modalities of assistance. Delivery of in-kind food or physical cash incurs transport and mobilization costs associated with each distribution, and is subject to potential diversion or disruption at key chokepoints, such as airports and border crossings. Furthermore, beneficiaries bear the time and travel costs associated with reaching physical cash distribution points and may be subject to harassment or extortion during this process.

By contrast, digital payment platforms enable instantaneous, private transfers of value, which can then be exchanged for goods at local merchants—or potentially "cashed out" for physical currency—without need to visit a central distribution location each time. When payments are delivered digitally like in this study, there is less need to accumulate cash at intermediate locations to facilitate beneficiary cashout, further reducing delivery costs and opportunities for diversion. This is particularly important when considering women in food-insecure settings as existing research documents substantial benefits to channeling resources to women directly (Field et al. 2021, Riley 2024). Beneficiaries in our study used their digital transfers across the merchant acceptance network, increasing convenience and privacy while also ensuring competitive pressure on any single merchant who might otherwise seek to engage in price gouging.

4. Experts' Beliefs and Methodological Safeguards

Humanitarian operations are complex and expensive, and the stakes can involve life or death. If a new approach fails, the consequences can be severe, both financially and in terms of human suffering. This may lead to a reasonable reluctance to adopt new technologies, especially if they are untested or perceived as risky. To better understand these issues, we surveyed experts, asking them to predict the treatment impacts we would later estimate. To help address the same issues, we took several methodological steps to safeguard against false positives.

We measure the expert views by surveying 55 international analysts, practitioners, and policymakers working on global development issues (including many engaged specifically on hunger in Afghanistan) and 36 academics, many with expertise related to digital payment systems.[26] The survey provided a precise description of the intervention and asked respondents to predict the corresponding treatment impacts for several key outcomes we report here; see Online Appendix Table 8 for the full text of the survey prompt.

Table 4 compares the actual values of the intervention's components (column 1) to the respondents' predicted values (column 2), and shows the p-value of a test of equality (column 3). Across all four components, differences between expert predictions and the values from our data are statistically significant. These respondents predicted, on average, that only 43% of the women in our study would be able to use digital payments (SD = 25.67 percentage points). In practice, as described above, 99.75% of women purchased goods at least once. They also predicted, on average, that local authorities would attempt to tax roughly 40% of beneficiaries (SD = 27.28). In practice, less than 2% of the treatment group reported any diversion attempts, with no statistically significant difference in the control group. Moreover, respondents had little agreement in their views, reflected in the large variance across predictions. A combination of skepticism and uncertainty about potential efficacy might partly explain an unwillingness to adopt innovations, especially when stakes are high. Experts did a better job of predicting cost-efficiency, guessing on average that delivery costs would be 10.65 cents on the dollar, whereas the actual delivery costs were 6.7 cents on the dollar, indicating a more common belief that digital aid delivery is relatively cheap.

Because of the potential stakes, we implemented three methodological safeguards against false positives (whereby the null hypothesis that an innovation has no impact is incorrectly rejected in favor of the

Table 4. Actual Values vs. Experts' Predictions

Variable	Actual values (1)	Predicted values (2)	*p*-value predicted = actual (3)
How many bread and tea meals in past week?	11.96	10.32 (0.45)	<0.001
Percent reporting diversion attempts	1.99	39.85 (2.98)	<0.001
Percent able to use digital payments	99.75	43.82 (2.75)	<0.001
Delivery costs (cents per $1 delivered)	6.7	10.65 (0.74)	<0.001

Notes. The first column shows the actual values of different elements of the intervention. The second column shows the mean predicted value by our sample of experts (with standard errors in parentheses). The third column shows the *p*-value of a test of the mean predicted value being equal to the actual value (i.e., a test of equality of columns 1 and 2).

alternative that it is effective). First, we conducted an RCT, which provides unbiased causal evidence by establishing an independent control group. This is particularly important in humanitarian settings, where the circumstances on the ground can change quickly and often worsen. Simpler analyses, such as comparing outcomes before and after an intervention, can therefore be highly misleading (Wolfe 2020). Second, we strictly followed our PAP, which, although limiting our capacity to learn and adapt, helps ensure that *p*-values are correctly calculated, providing additional protection against false positives (Olken 2015, Duflo et al. 2020). We also adjust our estimates to take into account the multiple hypotheses we are testing following the PAP. Third, as we rely on self-reported measures and not on anthropometrics or biometric markers because of surveying limitations, survey bias and strategic responses by participants are a source of concern. In our case, we followed the literature on testing and avoiding survey response bias (De Quidt et al. 2018) and took the most conservative approach we could think of to test for survey bias (see Online Appendix B.3).

5. Discussion

We evaluate a digital aid program and show that extremely poor, tech-illiterate women can receive humanitarian aid through digital payment systems to meet basic food security and mental well-being outcomes. We also find no evidence of diversion. Accounting for all aspects of facilitation, the estimated cost of delivery at 6.7 cents per dollar is less than 40% of the WFP's global figure of 17 cents per dollar for cash-based transfers. Our estimates are also better than the predictions of policy and research experts. Why did digital aid outperform these expectations?

The hybrid partnership that delivered the program was carefully designed to address common issues working with poor, tech-illiterate populations in a politically fragile context. Vulnerable households were identified by locally elected CDCs, which ensured the safety of staff and beneficiaries by informing local authorities and maintained independence from interference in the beneficiary selection process. A United States-based nonprofit provided fiduciary oversight. The digital payment platform was compatible across mobile operators and did not require a smartphone, ensuring beneficiaries could use existing phones to access transfers. In addition, user-centered design principles helped minimize potential confusion: Most importantly, beneficiaries were trained to use the payment platform by completing a test transaction with a merchant.

We expect that whether the findings obtained from this context and population generalize to other settings will depend most critically on three key enabling conditions. First, vulnerable households must have access to phones. Second, there must be enough merchants who accept digital payments to allow convenient and competitive shopping. And third, markets must have enough goods to meet demand. Transitioning from in-kind to cash and digital support can increase competition for goods and so may increase market prices and thereby displace hunger to other vulnerable groups. Studies of famine highlight that in extreme cases of sudden and widespread deprivation—as in Gaza at the time of writing (Chotiner 2024)—increasing food supply is critical, but that response to most famines historically required complementary strategies to redistribute resources (Dreze and Sen 1990, Sen 1999).

Our study is motivated by the challenges facing populations who are isolated by the state in complex crisis situations like Afghanistan, where basic human survival—not sustained poverty reduction—is the immediate policy goal. Evidence on whether digital humanitarian aid can help achieve this objective is limited. Most cash-based programs are carried out in more stable contexts, contributing to a rich evidence base on outcomes like poverty reduction, education, health, financial inclusion, and social protection (Bastagli et al. 2019, GiveDirectly 2024).

Based largely on this research, humanitarian cash-based programming is growing and digital delivery is increasingly popular (Urquhart et al. 2023), but research during humanitarian crises with populations facing extreme cultural and political constraints is still nascent. In Online Appendix A.1, we review the literature and find 23 RCTs of cash for humanitarian applications; 10 such RCTs in contexts categorized as "not free" by Freedom House, 3 of those 10 having some digital component; and none, that we can find, targeting populations that the government actively oppresses. This knowledge gap is consequential as these contexts and groups are increasingly affected by acute hunger (Foreign, Commonwealth and Development Office 2023). As our expert survey demonstrates, our results are not readily inferred from prior studies: Illiterate Afghan women could have been unable to utilize digital payments, local merchants could have refused to accept digital transactions, and the Taliban authorities could have interfered. Our study thus provides an "existence result" on the feasibility of digital aid in humanitarian response, with insight into the organizational and technological factors that contributed to success.

The exigencies of this context and the fact that we work with a hard-to-reach group, however, necessitated three design choices that affect our generalizability. First, our experimental data cover only two months before the early group is treated. This time frame, however, is consistent with our learning objective: can digital payments cost-effectively reduce acute hunger. As such, we prespecified this as the relevant time frame, and relatedly that we did not expect changes in outcomes like income or employment. Such impacts were unlikely—and did not realize—given the large humanitarian needs, prohibitions on women's employment, and relatively modest transfer size. Second, working with our sample required approval of the de facto authorities. Our partners could not justify seeking Taliban permission to onboard women to the digital payment app without eventually providing humanitarian assistance. We thus opted for a short, staggered program where every participant received some assistance during the lean season. Although the control group knew they would receive transfers, they were unaware when these would start, and we find no evidence of borrowing or other anticipatory changes in behavior. Our late group thus provides a valid counterfactual. Third, because we could not visit our participants, and to avoid survey fatigue over the phone, we focused on a small set of questions and could not take measurements that would yield deeper insights (such as anthropometrics). Thus, other relevant topics (e.g., within-household dynamics, validated mental health measures, and impacts on other household members) were not covered.

Nonetheless, this exercise provides proof-of-concept that digital aid represents a potential cost-effective complement to existing modalities. We see five important directions for future research to help humanitarians select strategies to address hunger. First, relative to physical goods and cash, digital payments enable humanitarians to more easily vary when aid is delivered, its frequency, and the amount. Research can help identify how to optimally take advantage of this added flexibility. Second, more evidence is needed on the costs and benefits of digital relative to cash delivery. Some prior work addresses this question (Aker et al. 2016), but we think more of this work in fragile contexts controlled by hostile actors and where the vulnerable are hard to reach will be relevant given the rise in hunger among such groups. This is best pursued in experiments which randomize both digital delivery and cash delivery against a control group. Third, humanitarians need further evidence on how key enabling conditions—such as mobile phone penetration and the availability of merchants who accept digital payments—affect these relative costs and benefits, and how best to organize future hybrid partnerships based on differences in enabling conditions. Fourth, additional focus is needed on approaches to targeting, onboarding, and monitoring beneficiaries in hostile environments that minimize risks to participants (Aiken et al. 2022, Jeong and Trako 2022). In particular, the one point of physical contact in our study between humanitarians and beneficiaries was during onboarding. Further innovations might eliminate the need for contact between humanitarians and those in need altogether and apply automated screening of data from digital transactions and phone surveys to monitor user experience and identify potential fraudulent patterns. And fifth, implementing digital payments at scale may affect diversion strategies, market prices, and the broader use of digital payment technologies for other financial transactions. It may also spur the transformation of humanitarian organizations into leaner organizational models to best complement the capacities of private and community organizations. Future work with large-scale experiments should explore such changes.

Encouragingly, aid agencies are increasing their use of digital payments. The WFP is using insights from our study to scale digital payments to over 100,000 households in Afghanistan in partnership with Hesab-Pay, which has been able to maintain its license to operate despite the dynamic strategic and political conditions in the country. As this program scales, it will need to grapple with the complex interdependencies between humanitarian agencies, digital payment platforms, and governments in fragile states. Although local authorities could still choose to block digital aid

provision, doing so might impede future humanitarian commitments and draw increased public attention to the issues of aid diversion and vulnerable populations. The centralized management of digital payments relative to cash or food distribution also reduces the need to engage with local actors who might complicate aid delivery and increases humanitarians' bargaining power. The existence of key enablers for the success of a program like this in other fragile settings (e.g., the presence of CDCs, mobile phone availability, and digital payment platforms), as shown in Online Appendix Table 9, where Afghanistan ranks as a particularly challenging case, suggests that this approach could increase food security in other fragile states with high levels of hunger.

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Endnotes

[1] In 2022, 11 countries—Afghanistan, the Central African Republic, Chad, Democratic Republic of the Congo, Haiti, Nigeria, South Sudan, Sudan, Somalia, Syria, and Yemen—accounted for over 50% of the quarter-billion facing acute hunger and potential starvation (those with a food insecurity score of three or higher among the roughly 1.350B people analyzed) (Food Security Information Network 2023).

[2] Humanitarian food security budgets fell 40% from $85 USD per person in 2018 to $51 USD per person in 2021 (Food Security Information Network 2023).

[3] For example, the share of the WFP's assistance delivered as cash-based transfers (rather than in-kind aid contributions) rose from 2% in 2010 to 34% in 2021 (McDonough 2022), a move supported by research evidence (Jeong and Trako 2022).

[4] The United Nations Better than Cash Alliance defines a digital payment as "the transfer of value from one payment account to another using a digital device or channel. This definition may include payments made with bank transfers, mobile money, QR codes, and payment instruments such as credit, debit, and prepaid cards." In this study, we examine a specific digital payment platform in Afghanistan, HesabPay, which is available as an app on iOS and Android and is also accessible to feature phone users via USSD or combined with a QR code card.

[5] GiveDirectly (2024) catalogs the vast literature on cash transfers, including important contributions by Haushofer and Shapiro (2016), Bedoya Arguelles et al. (2019), Banerjee et al. (2020), Muralidharan et al. (2021), and McIntosh and Zeitlin (2022). Whereas prior cash transfer studies focus mainly on sustained poverty reduction (i.e., SDG 1), we are concerned with basic human survival (i.e., SDG 2); Online Appendix A discusses prior cash transfer studies focused on humanitarian goals.

[6] These value vouchers have a denominated local currency value redeemable at participating merchants for any available goods. The World Food Programme (Skovbye 2023) treats value vouchers as an official form of a cash-based transfer, unlike commodity vouchers, which are tied to both specific merchants and goods.

[7] The Afghanistan Cash and Voucher Working Group (CVWG) estimates a Basic Food Basket for a family of seven costs approximately $96 USD per month (Bete 2022), roughly equivalent to two biweekly direct aid transfers of 4,000 AFN and a monthly survey incentive of 350 AFN. This basket was composed of 89 kg wheat flour, 21 kg domestic rice, 7 liters vegetable oil, 9 kg pulses, and 1 kg salt at prevailing exchange rates in August 2022.

[8] The preanalysis plan is registered at https://www.socialscienceregistry.org/versions/160809/docs/version/document.

[9] HesabPay was founded in 2016 by entrepreneur Sanzar Kakar. For more details, see the Algorand Foundation case study at https://algorand.co/case-studies/how-hesabpay-became-the-first-and-only-interoperable-digital-payments-platform-in-afghanistan.

[10] Note that this likely underestimates the true number of merchants in any given area, because (i) we only contacted numbers with which our beneficiaries transacted more than 10 times; (ii) among those numbers with which our participants transacted more than 10 times, not all could be reached; and (iii) there are more merchants in the areas that accept the digital vouchers but that our participants did not visit.

[11] The CDCs were established through local elections as part of the National Solidarity Program starting in 2004 (Beath et al. 2013; 2016; 2017a, b; 2018), where their primary job was to oversee block grants of development funding, and they were given a much broader range of local administrative authorities under the Citizens' Charter, starting in 2016.

[12] Mobile phone ownership in Afghanistan has grown rapidly over the past two decades, from approximately 25,000 subscribers in 2002 to over 22 million subscribers in 2021 (World Bank 2023). In a nationally representative survey, 91% of respondents reported at least one member of their household owned a mobile phone (66% of respondents report personally using a mobile phone), and 46% of that subgroup reported having an internet connection (Asia Foundation 2019).

[13] These took place during September 2022 in each of the three cities, with each session having between 24 and 80 participants.

[14] During pilots we conducted prior to the actual intervention, it became clear that participants needed help at first using the digital payment platform, as most of them were illiterate and had no prior experience with mobile money. This is why we decided to use the onboarding sessions to both help participants create their accounts and resolve any remaining questions and also to have participants complete a test purchase with a nearby merchant using the mobile payment platform.

[15] Participants in the same nahia had access to the same merchant acceptance network, motivating this decision. We discuss the stratification procedure in detail in our preanalysis plan.

[16] As we note on p. 15 of the PAP, with 18 balance variables and a 5% significance level, "we would expect to see p-values of less than 0.05 in 2 of the variables." We thus committed to implement the first randomization draw with fewer than two variables with a p-value below 0.05. This condition was met on the first random seed we tried, resulting in one unbalanced variable out of 18.

[17] We prespecified (on p. 13 of our preanalysis plan) that our sample would need to be credit constrained in order to interpret differences between our treatment and control groups. Empirically, we asked if women could borrow—only 0.29% of our sample indicated that they could—and we found no evidence of borrowing against future payments in the control group.

[18] We obtained IRB approval on May 4, 2022, from the London School of Economics (#89546). During the study, we submitted an amendment with plans to carry out a survey of experts (November 20, 2022), which the IRB approved (Study #145636 on November 29, 2022). A collaboration of practitioners, local grassroots organizations, the digital payment provider, and academics codesigned the study. Locally and internationally based Afghans either led or worked with each of these collaborative organizations and fully participated in all decision making, helping to ensure representation of the views of the participants, sensitivity to possible risks, and fair distribution of the program's benefits and costs. The study went through a due diligence phase of several months in which the team met weekly to assess the feasibility of implementing the program ethically. After launch, the entire team continued to meet every week to assess progress and implement any changes deemed necessary. The team was committed to early termination of the program, the evaluation, or both, if adverse events were to occur. Notably, although the research team conducted multiple rounds of surveying, the main NGO partner also conducted its own internal evaluations, which also involved interviews and surveys with participants, which they reported as independent checks on the research team's evaluation. See Online Appendix D for more details, including considerations specific to insecure, humanitarian crisis environments (Wood 2006, Campbell 2017, Puri et al. 2017, Wolfe 2020).

[19] All surveys were conducted by female enumerators, consistent with local norms. Participants were informed that the survey was completely voluntary and would not affect their aid payments, and they could skip any question they did not want to answer. Participants received a 350 AFN (~$4 USD) payment for completing each survey (see Online Appendix D). Overall, we have response rates of about 99% across survey rounds, with no difference in response rates between the treatment and control groups.

[20] Using the 2015 Demographic and Health Surveys (DHS) data, we compare our sample to a representative sample of similarly aged women in urban areas of Kabul, Balkh, and Mazar. The DHS sample has higher educational attainment: 56.8% have no schooling and 13.9% have at most primary education.

[21] Three women in the treatment group never used the funds transferred to their accounts. Tracking efforts indicate that each migrated to new cities during the period between enrollment and the start of payments.

[22] Regarding income and employment, we write in our PAP: "Due to the existing restrictions on women's liberties in Afghanistan, we believe it is unlikely that [these] will change."

[23] Although these results may seem unrealistically large, this is attributable to low base levels, as is seen in other surveys in contemporary Afghanistan (Evans 2022, Ray 2022).

[24] Our food security results complement a broader literature demonstrating mostly positive impacts, whereas our mental well-being results contribute newer insights to a more nascent literature. As discussed in Online Appendix A.2, our review of 23 cash-based programs in humanitarian contexts found that 21 included food security outcomes, with 16 of those 21 finding any positive effect. Less attention has focused on the mental impacts of cash-based programs, with only seven of the 23 studies considering a relevant outcome and six finding any positive effect.

[25] Note that this categorical variable for whether the respondent answered yes to any of the four diversion questions was not prespecified in our PAP. In Online Appendix B.4 we describe all deviations from the PAP.

[26] The policy organizations represented include the USAID, the UN World Food Program, UNICEF, the World Bank, GiveDirectly, Brookings, and BRAC, and the academic institutions included Brown, Duke, Georgetown, the London School of Economics, Princeton, Stanford, the University of Chicago, UC Berkeley, and UC San Diego.

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