Pre-Analysis Plan – Direct Aid Afghanistan

Introduction

Abstract

By 2030, nearly 80% of the world's poor will live in conflict-affected countries, while the number living in proximity to conflict has doubled to over 220 million in the past decade (Corral et al., 2020). International NGOs, donors and organisations face a challenge in these contexts: How can aid be quickly distributed while avoiding capture by criminal groups or hostile governments? We propose to test the effectiveness of one potential solution to this, a digital payments platform called HesabPay in Afghanistan. The intervention will randomise the timing of the disbursement of aid via HesabPay to vulnerable women in three major cities of the country. We will test whether i) this channel of aid delivery succeeds in avoiding capture; ii) the aid helps these women and their households cover their basic needs; and iii) this creates an entrypoint for women to start making broader use of digital financial products.

Motivation

The world's poor are increasingly concentrated in ineffective states and are exposed to conflict. By 2030, nearly 80% of the world's poor will live in conflict-affected countries, while the number living in proximity to conflict has doubled to over 220 million in the past decade (Corral et al., 2020). Such reversals in freedom come about abruptly, as observed recently in, for example, Afghanistan and Myanmar. These countries depend heavily on foreign aid for their functioning. Yet, foreign donors tend to dramatically reduce their imprint when conflict arises due to concerns that aid will be captured by hostile actors, particularly affecting the situation of vulnerable populations.

Afghanistan is a case in point. The abrupt withdrawal of international military, diplomatic, and development presence in August 2021 spurred a sudden, devastating economic crisis as hundreds of thousands of Afghans, including virtually the entire professional class, lost their livelihoods overnight. The UNDP estimates that by mid 2022 an astonishing 97% of Afghans are at risk of falling beneath the poverty line, fueling hunger, malnutrition, a steep decline in access to basic healthcare, and increased migration borne of desperation (UNDP 2021).

We propose to test the effectiveness of a potential aid delivery channel that can help allay concerns of aid-capture by hostile actors (in this case, Taliban members): A digital payments platform called HesabPay. The intervention will be very simple. With the help of our local implementation partners, Uplift Afghanistan and the Community Driven Development Organization (CDDO), we have identified around 2400 vulnerable women in three Afghan cities (Kabul, Herat and Mazar), each of whom will receive four semi-monthly approximately 50 USD payments. We will distribute the payments in two waves, randomly assigning women to the first or second waves, each wave consisting of (around) 1200 women. Participants cannot cash out their balance, though the implementing partner is considering a cash out option in extreme cases. This will allow us to test two broad sets of questions: Can digital

payments platforms be used to deliver aid in a targeted and efficient manner, and what is the effect of these payments on the livelihood of vulnerable recipients?

While Unconditional Cash Transfers (UCTs) have been widely studied (see, e.g. Haushofer & Shapiro, 2016; Egger et al., Forthcoming; Handa et al., 2018), and some have used digital platforms to distribute the transfers (e.g. Londoño-Vélez & Querubín, 2022) our proposed intervention differs from previous studies in several ways. First, the intervention will be conducted in a country ruled by a group hostile to foreign donors, the Taliban. To the best of our knowledge, none of the previous UCT studies has been conducted in a conflict-ridden environment governed by hostile actors. Moreover, our paper will shed light on whether digital payments platforms are a potentially effective tool to efficiently distribute aid without risking it falling into the wrong hands. This is especially important considering the facts mentioned above about the troubling rise in conflict around the world. Second, this will be done in a context of acute poverty, with a particularly vulnerable population. The UNDP estimates that by mid 2022 an astonishing 97% of Afghans are at risk of falling beneath the poverty line. In our own pilot and baseline data, we document that the planned participants have considerable economic, nutrition, and health needs. Furthermore, women have seen their liberties considerably restricted since the Taliban rose to power. Just recently, it was announced that women are not to leave their houses (https://www.theguardian.com/commentisfree/2022/may/10/the-guardian-view-on-afghan-women-the-taliban-turn-the-screws). Understanding how UCTs work among such a population in this type of setting is therefore of policy and of research importance.

Research Questions

The primary *experimental* research question this study seeks to answer is:

1. Can direct digital payments meet the immediate humanitarian needs of recipients?

The study also seeks to provide descriptive evidence regarding the following questions.

2. For how many days after the payment are humanitarian needs alleviated?
3. Are payments immediately (within one week) exchanged for goods and services, or are balances held as savings?
4. Are aid payments made to beneficiaries taxed by local representatives (including possibly the Taliban)?
5. Do other household members influence how women use their aid payments?
6. Can digital payments platforms be a viable and sanctions compliant channel to provide unconditional cash transfers in contexts ridden by conflict and animosity towards foreign donors/organizations?
7. Can digital payments increase more general use of digital payment applications?

8. Are any increases in digital payment application usage sustained after aid payments are stopped?

Research Strategy

Pre-experiment pilots

Prior to starting the experiment with our sample of around 2,400 women, we ran a series of three small pilots (N<50) to i) refine our instruments, ii) work out several logistical processes including how to enroll beneficiaries and iii) identified patterns that needed to be taken into account before the full scale up of the intervention.

The first pilot was conducted with around 30 women in Kabul. The initial idea was to conduct the experiment without any face-to-face interaction. Thus, participants were contacted over the phone, invited to participate and instructed how to open HesabPay accounts. They received smaller payments than in the actual intervention (800 AFA instead of 4000 AFA). A second, similar pilot was conducted a few weeks after the first one. These two initial pilots were intended to evaluate the survey instruments and sort out the logistics for the eventual scale up. From these pilots, it became clear that participants were struggling to create HesabPay accounts and use their funds, as the vast majority of participants had never used mobile money services or apps similar to HesabPay, had never been part of the formal banking system and mostly had feature phones. While HesabPay can be used with feature phones, the process of creating an account is more complicated than when having a smartphone.

Because of these pilots, we decided to instead have in-person registration sessions with around 50 women each, where potential participants would be introduced to the program. Convened by CDDO, a team from HesabPay attended each registration session to help women open their accounts, explain how HesabPay works, and answer any questions the women might have regarding HesabPay. Importantly, during these registration sessions, participants would conduct a test purchase with a local merchant using HesabPay so that they could see how it worked, and were given a brochure with basic information about how to use HesabPay and the phone numbers of some local merchants that accept HesabPay as a payment method. With these steps, we expected familiarity with the app and thus usage, which had been low during our first two pilots, to increase.

All women identified by the CDDO were invited to these registration sessions. They were first asked for their consent to participate in the study and then completed the baseline survey and opened their HesabPay accounts. They were told that all of them would eventually receive the aid payments, with some of them receiving the payments earlier and some later. Randomization takes place after **all** women have gone through

the registration sessions, opened their HesabPay accounts and completed the baseline survey (as the baseline data are used for the stratification of the assignment).

We conducted a third, final pilot with 52 women in Kabul to test the logistics of the full scale up and revise the last versions of the survey instruments (which had been tweaked based on what we had seen in the first two pilots) before conducting the registration sessions with the remaining participants.[1] This included conducting the in-person registration session and several rounds of phone follow-up surveys. This also helped us see whether there were going to be any problems with the Taliban or local community leaders from congregating women in a given place. We observed much higher rates of usage of the funds sent to women and no meaningful problems in this pilot.

We also analysed baseline and follow up data from this pilot of 52 women to assess the quality of our survey questions.

Sampling

Sampling

Our intervention will take place in three large cities of Afghanistan: Kabul, Herat and Mazar. We plan to recruit around 2,400 vulnerable women to participate in our study. Potential participants will be recruited with the help of our local implementation partner, Uplift Afghanistan, a charitable, non-political, humanitarian-focused organisation based in Afghanistan. Uplift works with CDDO (the Community Driven Development Organization) that assists Community Development Councils in a wide-array of local activities. The CDDOs maintain lists of households that are vulnerable and might therefore benefit from this type of program, and CDDO conducts well-being/needs analyses of each community. We draw on Uplift's experience on the ground and its relationship with CDDO to identify and recruit participants. Other than being identified as vulnerable or in need by the CDDO and having at least a feature phone, there are no other restrictions on the selection criteria.

The CDDO aimed to recruit 2422 women. Baseline data were collected for 2421 women. There were three instances of three women registering with the same phone number due to a bug in the digital payment platform. These eight women were excluded from the analysis, as we cannot uniquely identify an account for them. Out of the remaining women, 2409 could be matched to accounts in the digital payment platform. Some women could not be matched because 1) they completed the baseline survey but for some reason did not open an account or 2) they completed the baseline survey with one phone number but opened the account with a different phone number. Thus, our final sample has 2409 women. Note that for some variables

[1] Note that the full study aimed to recruit around 2400 participants because we had funding for 2500 but 52 were part of this third pilot. The women in this pilot will be excluded from any analysis.

we might have less responses, as we gave women the option of skipping questions if they did not want to answer (although this has rarely happened in pilots).

Statistical Power

The sample size of around 2,400 individuals was decided in collaboration with policy partners based on the size of the philanthropic gift which is paying for the digital aid payments. Our intervention will be randomized at the individual level, with two equally-sized groups. We will stratify at the neighbourhood (nahia) level and on our main outcome variable, total tea and bread meals in the past seven days, as specified below.

For the power calculations, we set a power of 80% and a significance level of 5%. We use Stata's "power twomeans" command, using the mean and (residual) standard deviation of our main outcome variable (total tea and bread meals in the past seven days) at baseline.[2] With these assumptions, we obtain a Minimum Detectable Effect of 0.1903. For comparison, using data from the third pilot, described above, and two rounds of follow-up surveys (after 1 and 2 payments, respectively), we found a decrease in the outcome variable of 2.48 when estimating the pooled regression in the "Main analysis" section of this PAP. Thus, we should be sufficiently powered to detect effects.

If we change the assumptions or consider possible contingencies, our estimates are as follows. With 90% power, the MDE is 0.2128. If we allow for 20% attrition in both groups (much higher than what we observed in the third pilot), the MDE becomes 0.2128. Power calculations do not reflect planned multiple hypothesis test adjustments. We describe below that these are quite limited.

Assignment to Treatment

We aim to recruit around 2400 participants (given funding for 2500 and ~100 participating in piloting activities). We will randomly divide our participants into two different groups, both of which will receive the same aid payments (four semi-monthly 50 USD payments) but at different points in time. That is, we will conduct a randomized phase-in study whereby half are assigned to treatment in wave 1 and the other are assigned to treatment in wave 2.

The protocol is to register all women in the experimental sample during group onboarding sessions. During the onboarding sessions women are given a description

[2] The residualized SD comes from a regression of the outcome variable on the stratification fixed effects.

of the program and provided assistance using the HesabPay app including a nominal test transaction so the beneficiaries can see it work in practice. Women are then assigned to an early (treatment) group or a late (control) group.

Participants will be evenly divided into these two groups. Treatment assignment will be stratified using two variables:[3]

- Neighborhood ("nahia"): individuals from 16 different nahias will be recruited for the study.[4]
- Needs: first, we will take the sum of how many meals in the last 7 days have been only bread and tea (ranging from 0, no bread and tea only meals, to 21, every breakfast, lunch and dinner consisted only of bread and tea). This will be one of our main outcome variables, as specified below. Second, we will create a dummy for whether the participant is above or below the median across the whole sample in terms of their total bread and tea meals, and stratify on this variable. In the pilots, we observed large variation in this measure of needs across subjects.

To randomize participants into treatment and control groups, we will use Stata's randtreat command, and will assign "misfits" independently across strata. Given that we have few strata and a single treatment arm, the number of misfits will be relatively small, so the risk of harming treatment fractions by independently assigning misfits across strata is low. See Carril (2017) for more details.

The first group (our "treatment group") will receive its payments first (four 4000 AFA bi-weekly payments). Two months after the treatment group has received its first 50 USD payment (and hence two weeks after receiving the last of its four 4000 AFA payments), the second group will phase in and start receiving its payments (our "control group"). This will give us a randomly-created control group with no intervention for two months, allowing us to estimate the causal effects of the aid payments on recipients' capacity of meeting basic needs (e.g. nutrition, healthcare, etc).

Ideally, we would have either a pure control group or a larger gap between the time that the first group receives the treatment and the time that the second group (control) is phased into treatment to be able to cleanly assess the longer-term effects of the intervention. However, due to the acute economic and social situation that our target population is experiencing, we decided to balance the research and

[3] Initially, we also planned to stratify on a third variable: whether the participant had a smartphone or not. However, pilots' participants had very low levels of smartphone ownership and thus we decided to not stratify on this variable.

[4] Participants will be recruited from 9 nahias in Kabul (around 900 participants), 3 in Herat (around 800 participants), and 4 in Mazar (around 800 participants).

participants' needs and implement the intervention in this way. The study received IRB approval from LSE (ref. number: 89546).

Attrition from the Sample

We have designed our intervention to be as light-touched and un-intrusive as possible to reduce attrition. Where possible, face-to-face contact with participants has been reduced to avoid drawing unwarranted attention. Only the baseline survey will be conducted face-to-face during the registration sessions organised by CDDO. In these meetings, CDDO and HesabPay will assist participants to open their HesabPay account, show them how to use the app to make transactions/buy products, and give them a list of local merchants that use HesabPay. Given that most of our sample have no experience using mobile payment apps, this initial training will be key to reduce attrition or low usage due to a lack of understanding on how to use the app, as was the case in our initial pilots.

Given that the digital payments platform works through smartphones or feature phones, conducting surveys over the phone is not a major problem, which we have verified in our pilot data collection. Even if participants don't answer their phones to participate in surveys, any in-app activity will be recorded by HesabPay. We have access to HesabPay's transaction data.

To try to avoid issues with differential response to follow-up surveys, we will pay 350 AFA to participants for completing each round of follow-up surveys to compensate them for their time. This payment will be made within a couple of days of completing the follow-up survey. We piloted the logistics of phone-based follow-up surveys during the last pilot, with very high completion rates (over 95%) even among women assigned to the late treatment group. Note that even if participants do not complete a round (or multiple rounds) of follow-up surveys, they will still receive their four approximately 50 USD program payments as scheduled (though they would not receive the 350 AFA payment, which is exclusively an incentive for participation in the survey).

Other than opening a HesabPay account to receive the payments (which we will assist participants to do at registration) and then spending the money that they receive, and responding to surveys, participants will not have to do anything else. Given that the intervention consists in them receiving unconditional cash transfers, that we are paying for surveys, and based on our experience with the pilots, we expect little attrition during the time in which participants are receiving the transfers.

Fieldwork

Instruments

Data will be collected in four ways:

1. Baseline survey/registration: When participants are registered, a short baseline survey will be conducted. This baseline survey gathers the basic demographic information of the respondent, including the variables we use for stratification. This survey includes short modules on income, humanitarian needs, labor provision, mobile money experience, happiness, and a few other variables.

   Most of the questions used for this survey come from surveys used by the Research Team in previous studies in Afghanistan, and have been vetted both by the Research Team and members of our local implementation partners (Uplift Afghanistan, CDDO, and HesabPay) to make sure they are adequate for the local context.

   New questions were developed by the Research Team leveraging their experience conducting RCTs in Afghanistan and elsewhere, consulting our local implementation partners and other scholars who have conducted research in the country.

   We also piloted the survey instruments during our three pilots, tweaking and adding/deleting questions based on the pilots' experiences.

2. Phone surveys: A longer phone survey will be conducted with the participants each month after the first wave receives its initial payment. Similarly to the baseline survey, it includes modules on labor provision, income, familiarity with mobile money, needs, aid capture and the experience using the HesabPay app.

   The questions were developed following the same procedure as the baseline survey.

3. Administrative data from HesabPay: HesabPay will provide the Research Team with their administrative data on all transactions conducted by participants. These will include information on the amount transferred, recipient, etc and will be based on HesabPay's current data collection protocols.

4. Merchant survey conducted by HesapPay: HesabPay is conducting a simple survey of its merchants to capture what beneficiaries are purchasing with their aid payments. We will attempt to use this to corroborate whether payments are indeed used for essentials, though our ability to do so depends on the quality of the data collected by HesabPay.

The different survey instruments can be found at the end of this document.

Empirical Analysis

Variables - Experimental Variation

This section lists outcomes of interest in the experiment and enumerates our hypotheses. We are primarily interested in the effect of the intervention on 1) basic needs, 2) aid capture, 3) economic/wellbeing outcomes and 4) usage of the mobile money platform. We will also provide descriptive information on how participants used their payments and comparisons to another sample of 5000 tech-savvy women that will receive four smaller payments in a non-randomized way.

1. **Basic needs:**
    a. *Primary outcomes:*
        i. Nutrition:
            1. Skipping meals: We will use the responses to questions 7, 8 and 9 in the follow-up survey (and the corresponding question 8 in the baseline for Q7 in the follow-up). For Q7, we will use the total number of meals skipped in the past 7 days. For Q8 and Q9 we will create dummies that will equal to 1 if the answer is “yes” and 0 if the answer is “no”.
            2. Total number of meals in the past seven days that have been only bread and tea, created using responses to questions 9-11 in the baseline and 10A-10C in the follow-up survey. We will take the sum of how many meals in the last 7 days have been only bread and tea (ranging from 0, no bread and tea only meals, to 21, every breakfast, lunch and dinner consisted only of bread and tea).
    b. *Hypothesis:* The primary hypothesis is that the aid payments will help participants meet their basic needs, specifically reducing skipped meals or meals with only bread and tea. Participants in our pilots reported living off of only bread and tea for an average of five days a week and 71% reported being unable to pay for a medical emergency in the last 30 days. We will compare the group receiving the aid payments first (“Wave 1”) to the group receiving the aid payments later (“Wave 2”) using Wave 2’s pre-treatment data.

2. **Informal taxation:**
    a. *Primary outcomes:*

i. Informal taxation: We will use the responses to questions 17A-18B in the follow-up survey and the corresponding questions in the baseline survey (Q13-16). For each of these four questions, we will create dummy variables that equal 1 if the answer is "yes" and 0 if the answer is "no".

b. *Hypothesis:* One concern with delivering aid in contexts like Afghanistan (especially to discriminated groups, like women in this setting) is that a group in control may capture the aid, for example by "taxing" aid recipients. We want to test whether using a digital payment platform (to reduce the visibility of the aid payments) avoids informal taxation by these groups, in this case by the Taliban. Thus, by comparing Wave 1 to Wave 2, we expect to see no increase in informal taxation. The informal taxation questions will allow us to (descriptively) assess whether aid recipients were specifically targeted for their aid or if someone else decided how to use their funds.

3. Economic/wellbeing outcomes:

a. Primary outcomes:

i. Changes in economic situation: We will use the responses to question 5 in the follow-up survey. We will create a dummy that equals 1 if the answer is "slightly better" or "much better" and 0 if the answer is "much worse", "slightly worse" or "same".

ii. Financial health: We will use responses to question 16 in the follow-up survey. We will create a dummy that equals 1 if the answer is "agree a lot" or "somewhat agree" and 0 if the answer is "neither agree nor disagree", "somewhat disagree" or "disagree a lot".

iii. Life satisfaction: We will use responses to question 31 in the follow-up survey and question 24 in the baseline survey. We will use a standardised version of this variable (standardising using the baseline values).

iv. Optimism: We will use responses to question 32 in the follow-up survey and question 25 in the baseline survey. We will create a dummy that equals 1 if the answer is "very happy" or "quite happy" and 0 if the answer is "not very happy" or "not at all happy".

b. Secondary outcomes:

i. Nutrition: We will use the answers to questions 7, total number of meals in the past days that have been only bread and tea (as defined above) and 11-15 in the follow-up to understand how participants' diets have changed. These questions allow us to see whether participants are shifting from not eating, to a poorly nutritional diet (mostly bread and tea), or to a more nutritional diet (with proteins, vegetables and dairy). In our pilots we observed very few vulnerable women eating these foods, we therefore do not expect to have much variation to examine impacts on these food sources. If we had more variation this would be a primary outcome measuring humanitarian need.

ii. Access to medicine: We will use the responses to questions 5A and 5B in the follow-up survey and the corresponding questions 12A and 12B in the baseline survey. More specifically, for those individuals who had a medical emergency in the past 30 days (answered "yes" to Q5A in the follow-up survey/Q12A in the baseline survey) we will create a dummy that equals 1 if the person answers "yes" to Q12B in the follow-up/Q5B in the baseline survey and 0 if they answer "no". In our baseline, we observe very few women purchasing medicine, suggesting it is not easily available at the current time.

iii. Within-household financial decision-making: We will use responses to question 19 in the follow-up survey and question 26 in the baseline survey. We will create a dummy that equals 1 if the woman is part of the decision making at home (i.e. answers "you" and "you/your partner") and 0 if she is not.

Note that we will also check for treatment effect heterogeneity on this measure using the baseline values for the outcomes listed under primary outcomes.

iv. Labor supply: We will use responses to question 2 in the follow-up survey and question 19 in the baseline survey. We will create a dummy that equals 1 if the head of the household is employed (in any of the three categories listed) and 0 if the head of the household is not employed.

v. Income: We will use responses to question 3 in the follow-up survey and question 20 in the baseline survey. There are two interpretations. One, people may mistakenly include the aid payments as income, though the question is phrased to avoid this. Or it may be that nutrition assists making income by meeting

basic caloric needs. We will code income entries between 1 and 10 as missing, as these are likely to come from individuals who believe the variable is being measured in thousands. In the pilot, this was relatively uncommon, however.

c. *Hypotheses:* Given the current level of poverty in general in Afghanistan, and the desperate situation observed among our pilots' participants, we expect the aid payments to improve the primary outcomes, by providing households with financial relief. We will also investigate more traditional measures of economic activity, such as labor supply and income, but due to the existing restrictions on women's liberties in Afghanistan, we believe it is unlikely that this will change.

Descriptive Evidence

Usage of mobile money platform:

We will use HesabPay's administrative transaction data to understand how participants have used the App, who they are transacting with, and potential spillovers. This will shed light on 1) how participants used their funds and 2) whether the intervention integrates participants into the digital finance sector (by analysing whether participants continue to use the app after aid payments have ceased).

More specifically, for each cohort we will aggregate HesabPay transaction data and report the total expenditures in each of the following categories at two-week intervals between payments: airtime purchases, bill payments, transfers to peers (who in many cases are informal merchants, and so indistinguishable in the data), and residual balances (savings). One exception to this, is that HesabPay assisted beneficiaries to conduct a test transaction with a single dedicated merchant, for the onboard session. We can identify these merchants in our data.

Descriptive analysis:

A. *HesabPay user experience:* We will use responses to questions 21-24 in the follow-up survey. These questions will allow us to understand what difficulties participants experienced when using the digital payment platform. For question 21, we will create a dummy variable that equals 1 if the answer is "yes", and 0 if the answer is "no". For question 22, we will create a dummy variable that equals 1 if the answer is "agree a lot" or "somewhat agree", and 0 if the answer is "neither agree nor disagree", "somewhat disagree" or "not agree at all". For question 23, we will create a dummy variable that equals 1 if the answer is "agree a lot" or "somewhat agree", and 0 if the answer is "neither agree nor disagree", "somewhat disagree" or "not agree at all". For question

24, we will create a dummy variable that equals 1 if the answer is “very satisfied” or “somewhat satisfied”, and 0 if the answer is “neither satisfied nor dissatisfied”, “somewhat dissatisfied” or “not satisfied at all”.

B. *Usage of aid payments*: We will use responses to questions 26A-28 in the follow-up survey. These questions will allow us to understand how participants used their aid payments. We expect most participants will use their aid payments to buy food and airtime from merchants (Q27). Given current mobility restrictions on women in Afghanistan, we expect to see some level of transfers to other male individuals to conduct purchases on behalf of the recipient (Q26A and Q26B). Participants receive four 4000 AFA payments every other week over two months, which is considerably more than the self-reported monthly income we observed in the pilots (between 1000 and 1500 AFA). To shed light on whether participants believe this has been enough to cover their needs or not we will use question 30 in the follow-up survey. We will create a dummy variable that equals 1 if the answer is “agree a lot” or “somewhat agree”, and 0 if the answer is “neither agree nor disagree”, “somewhat disagree” or “not agree at all”.

C. *Credit constraints*: We will use responses to questions 17-18 in the baseline survey to analyse how relevant credit constraints are in this context. We expect participants to be credit constrained due to the bad economic situation in Afghanistan. We are specifically interested in credit constraints as we should not observe differences between treatment and control groups given the research design if beneficiaries are not credit constrained.

D. *Banking experience*: We will use responses to question 21 in the baseline survey to check whether participants have previously had experience with the formal banking sector and questions 22-23 to check whether participants have previously used digital payment platforms like HesabPay, creating dummy variables that equal 1 if the answer is “yes” and 0 if the answer is “no”. We expect participants to have very low levels of experience with both types of banking services providers based on the data from the pilots.

E. *Potential spillovers*: One concern is that control women could potentially know treatment women and the treatment women could share their aid payments with the control women to help them meet their basic needs (participants know

they will eventually receive payments). While we expect this not to be a common problem, we will use questions 31A and 31B from the follow-up to shed light on this issue. These will give us a sense of i) how common it is for participants to know other participant women and ii) check whether treated households are sharing their aid payments with control households.

F. *Within household aid capture and dynamics:* We will use responses to questions 25A and 25B in the follow-up survey. For each of these two questions, we will create dummy variables that equal 1 if the answer is not missing or "no" and 0 if the answer is "no".

Comparison to non-experimental group:

In a separate effort, HesabPay is recruiting a group of around 5000 digitally-literate women via digital channels (e.g. Facebook campaigns) to receive four smaller, 800 AFA, semi-monthly aid payments ("Cohort 2"). We do not have influence over this part of the program and therefore are limited in what we can commit for potential analysis. To the extent that it is feasible, we expect that by making comparisons between the ~2400 women in our intervention ("Cohort 1") and these ~5000 women ("Cohort 2") we will learn about demographic characteristics of the two groups.

Ex ante, we expect Cohort 1 participants to be less educated (baseline question 6, creating a dummy for "some education": this equals 0 if the woman has no education and 1 otherwise), have less experience using mobile money services or with formal banking services (baseline questions 21-23, using the same indicators as above), worse nutrition, access to medicine and economic indicators (income and employment) as Cohort 2 participants.

This comparison will shed light on whether CDDO is effective at detecting vulnerable women in local communities. It may also shed light on possible transportation of inferences from Cohort 1 types to Cohort 2 types.

Balancing Checks

Treatment assignment balance

At the randomization stage, balance will be checked on the (primary) outcome variables defined above for which we have baseline data and variables used for heterogeneity analysis: marital status (baseline question 7, we will create a dummy that equals 1 if the answer is "married" and 0 for any other non-missing category),

Pashtun ethnicity (baseline question 30, we will create a dummy that equals 1 if the answer is “Pashtun” and 0 for any other non-missing category), some education (baseline question 6, we will create a dummy that equals 0 if the answer is “no education” and 1 for any other non-missing category), by city (Kabul, Mazar, and Herat), by age (baseline question 4, above and below median age), whether the woman is (partly) the household’s financial decision maker (baseline question 26, we will create a dummy that equals 1 if the answer is “you” or “you and your partner” and 0 otherwise), and whether the household is below or above the median household size (baseline question 27, we will create a dummy that equals 1 if the reported number of household members is above or 0 if it is below the median age).

To check for balance, we will run the following regression:

$$y_{i0n} = \gamma_0 + \gamma_1 Treated_{in} + v_{i0}$$

Where y_{i0} is the outcome of woman *i* in nahia *n* at time *t* = 0 (baseline).There is no need to cluster this cross-sectional regression as treatment is assigned at the individual level. We will report the mean and the standard deviation of each y_{i0n} for the treated and control groups as well as the p-value of γ_1, which measures the difference in y_{i0n} between the women in the treatment and control groups.

In total, this means we will check the balance of 18 variables (as for some outcome variables we don’t have baseline information). At a 5% significance level, this means we would expect to see p-values of less than 0.05 in 2 of the variables. This is the algorithm we will follow to generate the randomization:

1. Select a random seed number, check the number of coefficients for the variables specified here with p-value below 0.05.
2. If the number in Step 1 is more than 2, then go back to Step 1 with a different seed number. Otherwise, set that randomization.

Below is the balance table generated following this approach. We obtained these results with the first seed we tried. There is only one variable for which the p-value of the difference between the treatment and control groups is significant: whether a community leader has asked for any kind of assistance from the participants in the last days. Note that this is simply because the four individuals who answered yes to this question were assigned to the control group. Given that there is only one unbalanced variable out of 18, we stick to this randomization.

Table 1: Baseline Balance Check

Variable	Whole Sample Mean (1)	SD (2)	Treatment Mean (3)	Control Mean (4)	p-value Difference (5)
Panel A. Outcome Vars					
1. Total bread-tea meals	13.76	2.56	13.74	13.77	0.79
2. Skipped meals	2.62	1.52	2.63	2.62	0.86
3. Inf tax gov (others)	0	0.06	0	0	0.47
4. Inf tax leaders (others)	0	0.02	0	0	0.32
5. Inf tax gov (you)	0	0	0	0	1
6. Inf tax leaders (you)	0	0.04	0	0	0.05
7. Life satisfaction	3.53	1.17	3.51	3.54	0.59
8. Happy	0.01	0.1	0.01	0.01	0.99
Panel B. Heterogeneity Vars					
1. Married	0.34	0.47	0.34	0.34	0.88
2. Pashtun	0.1	0.3	0.11	0.09	0.15
3. Some education	0.37	0.48	0.37	0.36	0.37
4. Balkh	0.33	0.47	0.33	0.33	0.77
5. Herat	0.33	0.47	0.33	0.33	0.85
6. Kabul	0.34	0.47	0.34	0.34	0.95
7. Above median age	0.56	0.5	0.56	0.56	0.81
8. Fin. decision maker	0.66	0.47	0.66	0.66	0.99
9. Above median HH size	0.64	0.48	0.64	0.65	0.6
10. Able to leave house	0.65	0.48	0.64	0.66	0.56
Number of individuals	2409		1208	1201	

Attrition balance

Once we have finished the initial two rounds of follow-up surveys (meaning the treatment group has finished receiving all its aid payments), we will check for attrition by estimating the following regression:

$$y_{itn} = \alpha_1 Treated_{itn} + \alpha_n + u_{it}$$

Where y_{itn} is a dummy variable equal to 1 for if individual *i* in nahia *n* does not respond to survey wave t. $Treated_{it}$ is a dummy variable equal to one for treated recipients in periods after the baseline. α_n are nahia fixed effects. We will evaluate the same variables that we examine in the baseline balance analysis and we will cluster standard errors at the individual level.

Lee bounds

We will also calculate Lee bounds for the results if attrition is differential by treatment group based on our attrition analysis.

Treatment Effects - Main Analysis

Intent to Treat

Pooled regressions

To estimate impacts on the outcomes mentioned in the previous section, we estimate

$$y_{itn} = \beta_0 + \beta_1 1[Wave\ 1]_{in} + \beta_2 X_{i0n} + \beta_3 y_{i0n} + \beta_4 1[t = 2] + \varepsilon_{itn}$$

Where y_{itn} is the outcome of woman *i* in nahia *n* at time *t*. Note that *t* can be either 0 (baseline survey), 1 (first round of follow-up survey, after the treated have received 1 or 2 aid payments) or 2 (second round of follow-up survey, after the treated have received 3 or 4 aid payments).

Thus, we are only using the first two months of intervention for all primary analyses ($t = 1$ and $t = 2$), as those are the months in which we have clear experimental variation and the months where we expect potential attrition (survey non-response) to be low. X_{i0n} are the stratification variables (nahia fixed effects, and baseline needs). $1[t = 2]$ is a dummy for the second survey round period (round fixed effect). The coefficient of interest is β_1, which measures the causal effect of the intervention for those in Wave 1 (group that receives aid payments first) relative to those in Wave 2 (group that receives aid payments later), after Wave 1 starts receiving the aid payments (for some variables, we will have pre-intervention data from the baseline survey). For variables for which we have values at baseline, we control for the baseline values y_{i0n}. Standard errors will be clustered at the individual level.

Short vs. longer run effects

We will survey participants twice over a two month period. This means that the first round of follow-up surveys will take place after treated individuals have received either 1 or 2 payments, while the second round of follow-up surveys will take place after treated individuals have received either 3 or 4 payments. To test whether treatment effects differ after receiving 1 or 2 vs. 3 or 4 payments, we will estimate the following regression:

$$y_{itn} = \delta_0 + \delta_1 1[t = 2]_{itn} + \delta_2 1[t = 2]_{itn} \times 1[Wave\ 1]_{in} + \zeta_i + \epsilon_{itn}$$

Where $1[t = 2]$ equals 1 if the data correspond to the second round of the follow-up surveys and 0 if the data correspond to the first round of the follow-up surveys. ζ_i are individual fixed effects. The coefficient of interest is δ_2, which measures whether the treatment effects are growing over time. Standard errors will be clustered at the individual level.

Treatment on the Treated

We do not expect high levels of non-compliance. However, if there is non-compliance, we will estimate the following regression:

$$y_{itn} = \eta_0 + \eta_1 1[\,Received\ Payment]_{itn} + \eta_2 X_{i0n} + \eta_3 y_{i0n} + \eta_4 1[t = 2]_{itn} + \mu_{itn}$$

Where $1[Received\ Payment]_{itn}$ equals 1 if the individual received at least one payment during that round of follow-up surveys. We will instrument $Received\ Payment\ _{itn}$ with $1[Wave\ 1]_{itn}$, the (random) treatment assignment. Standard errors will be clustered at the individual level.

Heterogeneous Effects

Intent to Treat

First, following standard regression-based approaches, we will look for differential (linear) effects of treatment on outcomes by subgroup. For a subgroup defined by $Heterogeneity_{in}$, we will test for heterogeneous effects by interacting our treatment variables with the heterogeneity variable. For instance, the heterogeneous version of specification 1 would estimate:

$$y_{itn} = \psi_0 + \psi_1 Heterogeneity_{i0n} \times 1[Wave\ 1]_{in} + \kappa_i + \psi_2 1[t = 2]_{itn} + \omega_{itn}$$

where ψ_1 is a test for heterogeneity and we include individual and survey round fixed effects. Standard errors will be clustered at the individual level. In this "standard" approach, we will test for heterogeneity by the following characteristics:

Primary

- Baseline observations of basic needs primary outcomes.
- By city (Kabul, Mazar, and Herat). Given that restrictions on women's freedom vary across Afghanistan, and that CDDO teams and HesabPay merchant acceptance network differ across cities, this will allow us to test whether there are geographical differences in the effectiveness of the intervention.

Secondary

- Able to leave the house (baseline question 28, we will create a dummy that equals 1 if able to leave the house and 0 otherwise). This would be a primary dimension of heterogeneity, but this is a challenging question to get respondents to all understand uniformly.

- Marital status (baseline question 7, we will create a dummy that equals 1 if the answer is “married” and 0 for any other non-missing category).
- Pashtun ethnicity (baseline question 30, we will create a dummy that equals 1 if the answer is “Pashtun” and 0 for any other non-missing category).
- Some education (baseline question 6, we will create a dummy that equals 0 if the answer is “no education” and 1 for any other non-missing category).
- Age (baseline question 4, we will create a dummy that equals 1 if above the median age and 0 otherwise).
- Financial decision-maker in the household (baseline question 26, we will create a dummy that equals 1 if the woman is part of the decision making process [i.e. answers “you” or “you and your partner”] and 0 otherwise).
- Household size (baseline question 27, we will create a dummy that equals 1 if above the median household size and 0 otherwise), this will allow us to test whether the effectiveness of the intervention attenuates in larger households.

Treatment on the Treated

We don’t expect high levels of non-compliance. However, if there is non-compliance, we will estimate the following regression:

$$y_{itn} = \tau_0 + \tau_1 1[t = 2]_{itn} + \tau_2 Heterogeneity_{i0n} \times 1[Received\ Payment]_{itn} + \chi_i + \mu_{itn}$$

Where $1[Received\ Payment]_{itn}$ equals 1 if the individual received at least one payment during that round of follow-up surveys. We will instrument $Heterogeneity_{i0n} \times 1[Received\ Payment]_{itn}$ with $1[Wave\ 1]_{in} \times Heterogeneity_{i0n}$, the (random) treatment assignment and the interactions between the (random) treatment assignment and the heterogeneity dimension. We include individual and survey round fixed effects. Standard errors will be clustered at the individual level.

Standard Error Adjustments

We will cluster standard errors at the individual level. Participants will be invited to a registration session in community centres where they will provide their consent to participate in the study, complete the baseline survey, and receive a primer on how to use the HesabPay app. We expect around 50 participants per registration session. After finishing all registration sessions and having baseline data for all participants,

we will randomly assign participants to the treatment and control group, as specified above. Given that the randomisation is at the individual level, that is the level we will cluster our standard errors at.

Multiple Hypothesis Tests:

Our main experimental hypothesis is that direct aid payments will reduce immediate humanitarian needs. We will therefore control for the Family Wise Error Rate (FWER) for the family of outcomes related to humanitarian need: skipping meals and the total number of meals that are bread and tea. We are conducting relatively few hypothesis tests, we are well-powered, and our primary concerns relate to falsely rejecting the null that the program had no impact on humanitarian needs. We therefore control the FWER rather than the False Discovery Rate (FDR). We will treat primary outcomes related to basic needs as a family. We will treat primary outcomes related to informal taxation as another family and those related to economic outcomes as another family. These primary outcomes, and their construction, are enumerated above. For each family, we will also create a summary index following Katz, Kling, and Liebman (2007) and perform a single unadjusted test. We will control for the False Discovery Rate for all secondary outcomes.

We will also analyze our experimenter demand, described below, primes adjusting for FWER and FDR in the same way that we do for our treatment impact estimates. But we want to be conservative about the possibility of experimenter demand effects, so we will also report unadjusted p-values.

Addressing Experimenter Demand Effects

Our primary outcomes are self-reported survey data. Moreover, subjects cannot be blinded to their treatment status. As such, there is clear potential for experimenter demand effects.

In order to assess whether this is a problem in this setting, in the last of the follow-up surveys ($t = 2$) we will "prime" participants by telling them what we are expecting to find to see how that affects their responses. This exercise is similar in spirit to the work by de Quidt et al. (2018). More specifically, we randomly assign individuals into two groups: a "primed" group is told "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." before the questions related to needs (Q4) in one of the follow-up surveys. Thus, this group is explicitly told what we are expecting to find.[5] The "not primed" group is simply told "I would now like to ask you a few questions about how you and your family are doing." in the same place.

We stratify the random assignment to the primed and not primed group by treatment status and the enumerator that will conduct the survey.

To test whether primed individuals give different answers than not primed individuals, we run the following regression:

$$y_{itn} = \theta_0 + \theta_1 1[Primed]_{in} + \theta_2 X_{i0n} + \theta_3 y_{i0n} + e_{itn}$$

Note that here the set of control variables also includes enumerator fixed effects. $1[Primed]_{in}$ equals 1 if the individual has been assigned to the follow-up survey with the prime and 0 if the individual has been assigned to the follow-up survey without the prime. Standard errors will be clustered at the individual level. This regression will allow us to test whether there are experimenter demand effects by checking the coefficient θ_1. We will only focus on the primary need outcomes specified above for brevity, as these are the ones mentioned in the primed message.

We expect that the prime will impact our subjective measures of financial and mental well-being. But, we hypothesize it will not affect concrete measures such as the number of meals during which respondents eat only tea and bread and the number of meals skipped. If the prime impacts a variable, we will discount the corresponding estimated effects of treatment on that variable.

Another way of testing for experimenter demand effects is by comparing survey answers to measures derived from administrative sources or with no room for measurement error/misreporting. In our setting, there is limited scope for doing this due to the situation on the ground and our need to rely on phone surveys. Our intervention emphasizes the usage of the HesabPay app. Thus, it might be the case that participants feel the need to tell us that they have been using the app even when that is not the case. To test whether this is the case, we ask question 4 in the follow-up data. We can then check this variable against administrative records from HesabPay to see whether participants are over- or under-reporting the usage of the HesabPay app, providing further evidence on experimenter demand effects.

We also sought to prevent inducing any inadvertent demand effects by working with the implementing partners (Uplift/CDDO) to be sure that their messaging in the registration sessions does not raise false hope/fear about continuation of the program. We verified that the implementer only offers information about the program at hand and never discusses possible future funding. The implementer trains its

[5] This is closer to the "weak" than to the "strong" prime in de Quidt et al. (2018).

enumerators to hold this line in official presentations, in question and answer, and in any survey or data collection.

Treatment Effects - Response Timing

We will survey participants twice while they are receiving aid payments. The first round of follow-up surveys will take place once they have received 1 or 2 aid payments, and the second round will take place once they have received 3 or 4 aid payments. This means that some individuals can be interviewed the day in which they receive a payment, two weeks after receiving a payment, or anything in between. To investigate whether answers/needs vary according to how many days have passed since receiving the last aid payment, we will randomise the date in which the participants are contacted to complete the follow-up surveys. We will then measure whether responses vary along this dimension.

More specifically, we randomly assign individuals to be called on a certain date (no stratification). Based on our experience in the pilots, while participants are contacted on the date they are assigned to be contacted on, it might not be possible to conduct the survey on that date (e.g. the participant's phone might be off, or the participant doesn't have time to complete the survey). Thus, we plan to estimate the following regression:

$$y_{itn} = \pi_0 + \pi_1 \, Days\ Since\ Last\ Payment\ Realised_{itn} + \pi_2 1[t = 2] + \phi_i + \rho_{itn}$$

Where $Days\ Since\ Last\ Payment\ Realised$ is the number of days passed between receiving the last aid payment and completing the follow-up survey. Note that this ranges from 0 to 14. Due to the potential for imperfect compliance, we will instrument $Days\ Since\ Last\ Payment\ Realised_{itn}$ with $Days\ Since\ Last\ Payment\ Assigned_{itn}$, the number of days passed between receiving the last aid payment and the date on which the individual was supposed to complete the follow-up survey, which again is randomly assigned. Overall, in our last pilot over 80% of the surveys were conducted on the day they were supposed to be conducted, and the rest were completed the following day, so we expect the IV to have a strong first stage. We will cluster standard errors at the individual level. The coefficient of interest is π_1, and we expect that participants will report better outcomes when the survey is conducted closer to the date of receipt of the last payment.

Beliefs of practitioners

We are planning to conduct a survey of practitioners regarding what they believe the project's impacts will be. More specifically, we are going to ask them about their beliefs regarding six components of our project:

1. Beliefs about the effect of the intervention on the number of meals that are bread and tea.
2. Beliefs about the effect of the intervention on the number of skipped meals.
3. Beliefs about the effect of the intervention on the share of respondents who report giving some money to the Taliban.
4. Beliefs about the cost per dollar to create a direct aid payment.
5. Beliefs about the fraction of users who will not be able to use their payment at all to get goods and services.
6. Beliefs about the share of beneficiaries who will continue to use Hesabpay regularly after the payments end.

We will then compare the practitioners' beliefs and the actual estimates from the intervention.

Spillovers

One potential concern is that of spillovers, more precisely that people in the treatment group, who start receiving money early on, give money to people in the control group, presumably to be paid later on when they start receiving the money. It seems unlikely that people in the control group are able to borrow money from other sources, as over 99% of the sample answered at baseline that it would be "very difficult" or "somewhat difficult" to raise 1500 AFA within a month.

While we believe that spillovers are unlikely to be the case in our setting, we have taken some steps to diminish the risk of spillovers and assess whether they are a reason to be concerned. First, to avoid concentrations of treated individuals in any given geographical area, we stratified the treatment at the neighborhood level. Second, we ask in the follow-up survey i) whether they know any other individual participating in the program, and ii) whether they have received any money or assistance from these individuals so far (questions 31A and 31B). These questions will help assess whether spillovers are present in our sample. We can also use the administrative transaction data to see whether households transacted with each other. Finally, one avenue through which individuals could have interacted is during the onboarding sessions. While we stratify at the neighborhood level, there is some (exogenous) variation in the amount of treated and control individuals in each onboarding session (as sometimes there were multiple onboarding sessions within the same neighborhood). Thus, we can use this variation to check whether the outcomes of control individuals are affected by the number of treated participants that participated in their onboarding session.

Timing of the Pre-Analysis Plan

We are filing the pre-analysis plan on November 1, 2022. We expect beneficiaries will receive their first aid payments on November 6, 2022. The first phone survey calls will begin around November 10, 2022. The research team has looked at baseline data, and data from a pilot of 52 beneficiaries in Kabul before filing this pre-analysis plan. We have not looked at any post-intervention data (it does not yet exist).

Research Team

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- Research Manager: Shahim Kabuli

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Appendix

Survey Instruments

Baseline Survey

Demographics	
1A. What mobile phone number did you use to register for HesabPay?	
1B. If you have another phone number, what is it?	
2A. Is this a smartphone?	1. Yes 2. No -98 Refused to answer -99 Don't know
2B. [**IF NOT "YES" IN Q2A**] Do you or anyone in your household own a smartphone?	1. Yes 2. No -98 Refused to answer -99 Don't know
3A. What is your first name?	
3B. What is your last name?	
4. What is your age?	[10-99] -98 Refused to answer -99 Don't know
5A. In which province do you currently live?	Options
5B. In which district do you currently live?	Options
5C. In which nahia/village do you currently live?	[Open field]

6. What is the highest level of education you have completed?	0. No education 1. Less than primary 2. Primary 3. Lower Secondary 4. Upper Secondary 5. Certificate 6. Diploma 7. University Degree 8. Higher than university degree -98 Refused to answer -99 Don't know
7. What is your marital status?	1. Single 2. Married / Cohabitation 3. Separated 4. Divorced 5. Widowed -98 Refused to answer -99 Don't know
Needs and Vulnerability	
8. Over the past seven days, how many days did you or any other adults in your household skip meals because there were not enough resources for food?	[0-7 days] -98 Refused to answer -99 Don't know
9. Over the past **seven days**, how many days did your household eat only bread and tea for breakfast because you only had tea and bread available?	[0-7 days] -98 Refused to answer -99 Don't know
10. Over the past **seven days**, how many days did your household eat only bread and tea for lunch because you only had tea and bread available?	[0-7 days] -98 Refused to answer -99 Don't know
11. Over the past **seven days**, how many days did your household eat only bread and tea for dinner because you only had tea and bread available?	[0-7 days] -98 Refused to answer -99 Don't know
12A. In the last 30 days, did you have any medical needs to buy medicine?	1. Yes 2. No -98 Refused to answer -99 Don't know

12B. [**IF "YES" IN Q12A**] Were you able to pay for the medicine for these medical needs?	1. Yes 2. No -98 Refused to answer -99 Don't know
13. In the last 30 days, do you know anyone in your community who has been approached by government officials to provide them with any kind of assistance, such as food or money?	1. Yes 2. No -98 Refused to answer -99 Don't know
14. In the last 30 days, do you know anyone in your community who has been approached by a local community leader to provide them with any kind of assistance, such as food or money?	1. Yes 2. No -98 Refused to answer -99 Don't know
15. In the last 30 days, have you been approached by government officials to provide them with any kind of assistance, such as food or money?	1. Yes 2. No -98 Refused to answer -99 Don't know
16. In the last 30 days, have you been approached by a local community leader to provide them with any kind of assistance, such as food or money?	1. Yes 2. No -98 Refused to answer -99 Don't know
17. Imagine that you have an emergency and you need to come up with 1,500 Afghani. How difficult is it that you could come up with this amount within the next 1 month? Would you say it is very difficult, somewhat difficult, somewhat easy, or very easy?	1. Very difficult 2. Somewhat difficult 3. Somewhat easy 4. Very easy -98 Refused to answer -99 Don't know
18. How would you come up with this money within the next 1 month? (Do not prompt) (Select all)	1. Use savings 2. Borrow from my social network (family, friends, relatives, etc) 3. Borrow from formal source 4. Borrow from informal

	moneylender 5. Borrow from informal savings group 6. Sell household durable asset 7. Sell productive asset 8. Money from working 9. Receive money from my social network without the expectation of paying back 10. Other -98 Refused to answer -99 Don't know
Labor	
19. In the last 30 days, did the head of your household work for any organization, individual or on own-account?	1. Yes - worked for organisation 2. Yes - worked for individual 3. Yes - worked for self 4. No - did not work -98 Refused to answer -99 Don't know
Income	
20. In the last 30 days, how much income did all the members of your household earn from economic activity in total? (eg. Wages/Salaries from work including profit from your business, etc).	Amount in Afs -98 Refused to answer -99 Don't know

21. Have you or anyone in your household ever had a bank account?	1. Yes 2. No -98 Refused to answer -99 Don't know
Mobile Money Experience	
22. In the last 30 days have you personally transferred airtime to or received airtime from a relative or friend living in a different area inside Afghanistan through a mobile phone?	1. Yes - transferred airtime 2. Yes - received airtime 3. No -98 Refused to answer -99 Don't know
23. In the last 30 days, have you, personally, transferred money to or received money from a relative or friend living in a different area inside Afghanistan through a mobile phone?	1. Yes - transferred money 2. Yes - received money 3. No -98 Refused to answer -99 Don't know
Life satisfaction and Optimism	
24. All things considered, how satisfied are you with your life as a whole these days?	From 1 (dissatisfied) to 10 (satisfied) -98 Refused to answer -99 Don't know
25. Taking all things together, would you say you are:	1. Very happy 2. Quite happy 3. Not very happy 4. Not at all happy -98 Refused to answer -99 Don't know
Additional Demographics and Merchants	
26. Who handles your household's financial decisions, for example how much money to save and what to buy with the household's money?	1. You 2. Your husband/partner

	3. You AND your partner together 4. Some other male household member 5. Some other female household member -98 Refused to answer -99 Don't know
27. Including yourself, how many people are there in total in your household, living and eating together in the same house?	[1-25] -98 Refused to answer -99 Don't know
28. Are you currently able to leave the house to complete day-to-day tasks like buying groceries and medicine?	1. Yes 2. No -98 Refused to answer -99 Don't know
29. What is the name of the supermarket closest to where you live?	
30. Which ethnic group do you belong to?	1. Pashtun 2. Tajik 3. Uzbek 4. Hazara 5. Turkmen 6. Baloch 7. Kirghiz 8. Nuristani 9. Aimak 10. Arab 97. Other (Specify): _____ -98 Refused to answer -99 Don't know

Follow-up Survey

Demographics	
1. Has your household moved to a different city or nahia in the last 30 days?	1. Yes 2. No -98 Refused to answer -99 Don't know
Labor	
2. In the last 30 days, did the head of your household work for any organization, individual or on own-account (in a business enterprise belonging to the household or member of the household, - e.g. as a trader, barber, shop owner, dressmaker, carpenter, taxi driver, etc)?	1. Yes - worked for organisation 2. Yes - worked for individual 3. Yes - worked for self 4. No - did not work -98 Refused to answer -99 Don't know
Income	
3. In the last 30 days, how much income did all the members of your household earn from economic activity in total? (eg. Wages/Salaries from work including profit from your business, etc). If you have received any HesabPay payments, please exclude it from this total.	Amount in Afs -98 Refused to answer -99 Don't know
Recall	
4. In the last seven days, have you used your HesabPay account to pay for something or transfer money to someone?	1. Yes 2. No
Needs	
[IF PRIMED] I would now like to ask you a few questions about how you and your family are doing. [This first sentence could be in both versions of the survey]. 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.	
[IF NOT PRIMED] I would now like to ask you a few questions about how you and your family are doing.	
5. How do you compare the overall economic situation of the household with 30 days ago?	1. Much worse 2. Slightly worse

	3. Same 4. Slightly better 5. Much better -98 Refused to answer -99 Don't know
6A. In the last 30 days, did you have any medical needs to buy medicine?	1. Yes 2. No -98 Refused to answer -99 Don't know
6B. [**IF "YES" IN Q6A**] Were you able to pay for the medicine for these medical needs?	1. Yes 2. No -98 Refused to answer -99 Don't know
7. Over the past seven days, how many days did you or any other adults in your household skip meals because there were not enough resources for food?	[0-7 days] -98 Refused to answer -99 Don't know
8. In the last 30 days, were your children ever forced to skip a meal because there wasn't enough money for food?	1. Yes 2. No -98 Refused to answer -99 Don't know
9. Does everyone in the household regularly eat at least two meals a day?	1. Yes 2. No -98 Refused to answer -99 Don't know
10A. Over the past **seven days**, how many days did your household eat only bread and tea for breakfast because you only had tea and bread available?	[0-7 days] -98 Refused to answer -99 Don't know
10B. Over the past **seven days**, how many days did your household eat only bread and tea for lunch because you only had tea and bread available?	[0-7 days] -98 Refused to answer -99 Don't know
10C. Over the past **seven days**, how many days did your household eat only bread and tea for dinner because you only had tea and bread available?	[0-7 days] -98 Refused to answer -99 Don't know
11. In the last seven days how many days did you eat rice?	[0-7 days]

	-98 Refused to answer -99 Don't know
12. In the last seven days how many days did you eat beans?	[0-7 days] -98 Refused to answer -99 Don't know
13. In the last seven days how many days did you eat vegetables?	[0-7 days] -98 Refused to answer -99 Don't know
14. In the last seven days how many days did you eat any meat, like chicken, cow or sheep?	[0-7 days] -98 Refused to answer -99 Don't know
15. In the last seven days how many days did you eat milk or yoghurt?	[0-7 days] -98 Refused to answer -99 Don't know
Financial health self-assessment	
16. How much do you agree or disagree with the following statement: I am highly satisfied with my present financial condition.	1. Agree a lot 2. Somewhat agree 3. Neither agree nor disagree 4. Somewhat disagree 5. Not agree at all -98 Refused to answer -99 Don't know
Aid Capture and HesabPay Experience	
17A. In the last 30 days, do you know anyone in your community who has been approached by government officials to provide them with any kind of assistance, such as food or money?	1. Yes 2. No -98 Refused to answer -99 Don't know
17B. In the last 30 days, do you know anyone in your community who has been approached by a local community leader to provide them with any kind of assistance, such as food or money?	1. Yes 2. No -98 Refused to answer -99 Don't know

18A. In the last 30 days, have you been approached by government officials to provide them with any kind of assistance, such as food or money?	1. Yes 2. No -98 Refused to answer -99 Don't know
18B. In the last 30 days, have you been approached by a local community leader to provide them with any kind of assistance, such as food or money?	1. Yes 2. No -98 Refused to answer -99 Don't know
19. Are you currently able to leave the house to complete day-to-day tasks like buying groceries and medicine?	1. Yes 2. No -98 Refused to answer -99 Don't know
20. Who handles your household's financial decisions, for example how much money to save and on what to spend the household's money?	1. You 2. Your husband/partner 3. You AND your partner together 4. Some other male household member 5. Some other female household member -98 Refused to answer -99 Don't know
21. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] You should have received payment for 4000 AFG as part of your participation in this study. Did you experience any difficulty receiving this payment via the HesabPay app?	1. Yes 2. No -98 Refused to answer -99 Don't know
22. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] How much do you agree or disagree with the following statement: Using HesabPay's app was easy and intuitive.	1. Agree a lot 2. Somewhat agree 3. Neither agree nor disagree 4. Somewhat disagree 5. Not agree at all

	-98 Refused to answer -99 Don't know
23. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] How much do you agree or disagree with the following statement: It has been easy to pay merchants using the HesabPay app.	1. Agree a lot 2. Somewhat agree 3. Neither agree nor disagree 4. Somewhat disagree 5. Not agree at all -98 Refused to answer -99 Don't know
24. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] How satisfied are you with the HesabPay app?	1. Very satisfied 2. Somewhat satisfied 3. neither satisfied nor dissatisfied 4. Somewhat dissatisfied 5. Not satisfied at all -98 Refused to answer -99 Don't know
25A. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] You should have received a payment for 4000 AFG, did anybody approach you for part of that payment?	1. No 2. Yes, my husband/partner 3. Yes, another male household member 4. Yes, another female household member 5. Yes, a government official 6. Yes, other (specify) -98 Refused to answer -99 Don't know

25B. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] Did anyone else decide how to spend your direct aid payment?	1. No 2. Yes, my husband/partner 3. Yes, another male household member 4. Yes, another female household member 5. Yes, a government official 6. Yes, other (specify) -98 Refused to answer -99 Don't know
26A. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] Have you transferred part or the whole 4000 AFG payment to someone else so that they can buy things for the household on your behalf?	1. Yes 2. No -98 Refused to answer -99 Don't know
26B. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION & YES TO Q26A**] Who have you transferred money to?	1. Husband/partner 2. Brother 3. Father 4. Son 5. Sister 6. Mother 7. Daughter 8. Other male household member 9. Other female household member 10. Other male not member of household 11. Other female not member of household -98 Refused to answer -99 Don't know

27. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] From the 4000 AFG payment you received, have you used all or part of it for the following:	
27A. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To buy directly from merchants using HesabPay	1. Yes 2. No -98 Refused to answer -99 Don't know
27B. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To transfer money to someone else inside the household	1. Yes 2. No -98 Refused to answer -99 Don't know
27C. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To transfer money to someone else outside the household	1. Yes 2. No -98 Refused to answer -99 Don't know
27D. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To buy airtime	1. Yes 2. No -98 Refused to answer -99 Don't know
27E. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To pay for bills	1. Yes 2. No -98 Refused to answer -99 Don't know
27F. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To buy food	1. Yes 2. No -98 Refused to answer -99 Don't know
27G. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To buy clothes	1. Yes 2. No -98 Refused to answer -99 Don't know
27H. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To buy medicine	1. Yes 2. No -98 Refused to answer -99 Don't know

27I. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To invest in a business	1. Yes 2. No -98 Refused to answer -99 Don't know
27J. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To pay for transport	1. Yes 2. No -98 Refused to answer -99 Don't know
27K. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To pay rent	1. Yes 2. No -98 Refused to answer -99 Don't know
27L. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To pay debt	1. Yes 2. No -98 Refused to answer -99 Don't know
27M. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] To save	1. Yes 2. No -98 Refused to answer -99 Don't know
28. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] Have you used some of the 4000 AFG payment you received for something else? Specify	1. Yes, specify: _____ 2. No -98 Refused to answer -99 Don't know
29A. Do you share your HesabPay account with someone else?	1. Yes 2. No -98 Refused to answer -99 Don't know
29B. [**IF YES IN Q29A**] Who do you share your HesabPay account with?	1. Husband/Partner 2. Parents 3. Sons 4. Other household member 5. Someone outside your household

	6. Other (specify) -98 Refused to answer -99 Don't know
30. [**IF INDIVIDUAL HAS RECEIVED THE INTERVENTION**] How much do you agree or disagree with the following statement: The 4000 AFG payment has been enough to cover my household's basic needs, such as food consumption, healthcare expenses and electricity bills?	1. Agree a lot 2. Somewhat agree 3. Neither agree nor disagree 4. Somewhat disagree 5. Not agree at all -98 Refused to answer -99 Don't know
31A. Do you know any household that receives aid payments via HesabPay?	1. Yes 2. No -98 Refused to answer -99 Don't know
31B. [**IF YES IN Q31A**] Did you receive any kind of assistance (e.g. food or money) from any of the other households you know receive aid payments via HesabPay?	1. Yes 2. No -98 Refused to answer -99 Don't know
Life satisfaction and Optimism	
32. All things considered, how satisfied are you with your life as a whole these days?	From 1 (dissatisfied) to 10 (satisfied) -98 Refused to answer -99 Don't know
33. Taking all things together, would you say you are:	1. Very happy 2. Quite happy 3. Not very happy 4. Not at all happy -98 Refused to answer -99 Don't know
34. Who is the head of your household?	1. You 2. Your husband/partner

	3. Some other male household member 4. Some other female household member -98 Refused to answer -99 Don't know

Merchant Survey

1. What is the name of your store?

2. When did you start accepting HesabPay for purchases at your store? (Month & Year)

3. On a typical week before the CDDO onboarding session, how many customers did you serve?

4. Before the CDDO onboarding, how many of your weekly customers were using HesabPay?

5. On a typical week after the CDDO onboarding session, how many customers did you serve?

6. After the CDDO onboarding, how many of your weekly customers were using HesabPay?

7. Do your HesabPay customers only use QR code cards, or do some also use the HesabPay smartphone app? (Choose one answer: QR codes only, or QR codes and smartphone app)

8. Before the CDDO onboarding, how did you typically use your HesabPay balance:
    - 8.1. Did you purchase supplies from wholesalers using HesabPay? (Yes/No)
    - 8.2. Did you purchase goods from another merchant? (Yes/No)
    - 8.3. Did you purchase airtime using HesabPay? (Yes/No)
    - 8.4. Did you pay electricity bills using HesabPay? (Yes/No)
    - 8.5. Did you send money to others using HesabPay? (Yes/No)
    - 8.6. Did you cash out from the HesabPay office? (Yes/No)

8.7. [If No to each of the above] Did you typically have a HesabPay balance? (Yes/No)

8.7.1. [If yes to 8.7] How else did you use your HesabPay balance?

9. After the CDDO onboarding, how do you now typically use your HesabPay balance:

9.1. Do you purchase supplies from wholesalers using HesabPay? (Yes/No)

9.2. Do you purchase goods from another merchant? (Yes/No)

9.3. Do you purchase airtime using HesabPay? (Yes/No)

9.4. Do you pay electricity bills using HesabPay? (Yes/No)

9.5. Do you send money to others using HesabPay? (Yes/No)

9.6. Do you cash out from the HesabPay office? (Yes/No)

9.7. [If No to each of the above] Do you typically have a HesabPay balance? (Yes/No)

9.7.1. [If yes to 9.7] How else do you use your HesabPay balance?

10. For each of the following goods/services - please tell us if you sell it, whether it can be purchased with HesabPay at your store, and if CDDO women have been buying it:

10.1. Does your store sell dried goods like flour, pasta, rice, beans, sugar, tea? (Yes/No)

10.1.1. [If Yes to 10.1]: Can dried goods be purchased using HesabPay? (Yes/No)

10.1.2. [If Yes to 10.1.1] Are the CDDO women buying dried goods? (Yes/No)

10.2. Does your store sell fresh fruit and vegetables? (Yes/No)

10.2.1. [If Yes to 10.2]: Can fresh fruit and vegetables be purchased using HesabPay? (Yes/No)

10.2.2. [If Yes to 10.2.1] Are the CDDO women buying fresh fruit and vegetables? (Yes/No)

10.3. Does your store sell dairy items like milk, yogurt, cheese? (Yes/No)

10.3.1. [If Yes to 10.3]: Can dairy items be purchased using HesabPay? (Yes/No)

10.3.2. [If Yes to 10.3.1] Are the CDDO women buying dairy items? (Yes/No)

10.4. Does your store sell bread? (Yes/No)

10.4.1. [If Yes to 10.4]: Can bread be purchased using HesabPay? (Yes/No)

10.4.2. [If Yes to 10.4.1] Are the CDDO women buying bread? (Yes/No)

10.5. Does your store sell eggs? (Yes/No)

10.5.1. [If Yes to 10.5]: Can eggs be purchased using HesabPay? (Yes/No)

10.5.2. [If Yes to 10.5.1] Are the CDDO women buying eggs? (Yes/No)

10.6. Does your store sell meat like sheep, cows, calf and chicken,? (Yes/No)

10.6.1. [If Yes to 10.6]: Can meat be purchased using HesabPay? (Yes/No)

10.6.2. [If Yes to 10.6.1] Are the CDDO women buying meat? (Yes/No)

10.7. Does your store sell mobile airtime? (Yes/No)

10.7.1. [If Yes to 10.7]: Can airtime be purchased using HesabPay? (Yes/No)

10.7.2. [If Yes to 10.7.1] Are the CDDO women buying airtime? (Yes/No)

10.8. Does your store sell medicine? (Yes/No)

10.8.1. [If Yes to 10.8]: Can medicine be purchased using HesabPay? (Yes/No)

10.8.2. [If Yes to 10.8.1] Are the CDDO women buying medicine? (Yes/No)

10.9. Does your store sell toiletries like soap, shampoo, etc? (Yes/No)

10.9.1. [If Yes to 10.9]: Can toiletries be purchased using HesabPay? (Yes/No)

10.9.2. [If Yes to 10.9.1] Are the CDDO women buying toiletries? (Yes/No)

10.10. Does your store sell clothes? (Yes/No)

10.10.1. [If Yes to 10.10]: Can clothes be purchased using HesabPay? (Yes/No)

10.10.2. [If Yes to 10.10.1] Are the CDDO women buying clothes? (Yes/No)

11. What category of goods have the CDDO women been buying most often? (Choose one answer: Dried Goods, Fresh Fruit and Vegetables, Dairy Items, Bread, Eggs, Meat, Mobile Airtime, Medicine, Toiletries, Clothes, Other)

12. Do you sell anything not on the list above that the CDDO women have been buying frequently? If so, please describe.

13. If you don’t allow customers to pay for any of the goods on the list above using HesabPay, please explain:

14. Do you charge any additional fees for customers to pay using HesabPay? (Yes/No)
    14.1. [If Yes to 14], please explain what fees you charge:

15. How satisfied are you with the HesabPay system? (Choose one answer: Very Satisfied, Somewhat Satisfied, Neither Satisfied Nor Dissatisfied, Somewhat Dissatisfied, Not Satisfied At All)

16. Please tell us, what changes to the HesabPay system could help support your business?