Zap it to Me:
The Impacts of a Mobile Cash Transfer Program

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Abstract: Conditional and unconditional cash transfers have been effective in improving development outcomes in a variety of contexts, yet the costs of these programs to program recipients and implementing agencies are rarely discussed. The introduction of mobile money transfer systems in many developing countries offers new opportunities for a more cost-effective means of implementing cash transfers. This paper reports on the first randomized evaluation of a cash transfer program delivered via the mobile phone. In response to a devastating drought in Niger, households in targeted villages received monthly cash transfers as part of a social protection program. One-third of targeted villages received a monthly cash transfer via a mobile money transfer system (called *zap*), whereas one-third received manual cash transfers and the remaining one-third received manual cash transfers plus a mobile phone. We show that the *zap* delivery mechanism strongly reduced the variable distribution costs for the implementing agency, as well as program recipients' costs of obtaining the cash transfer. The zap approach also resulted in additional benefits: households in zap villages used their cash transfer to purchase a more diverse set of goods, had higher diet diversity, and grew more types of crops, especially marginal cash crops grown by women. These results are robust to the use of a Bonferonni correction for multiple hypothesis-testing. We posit that the primary mechanism underlying these results is the greater privacy of related to the zap mechanism, resulting in changes in intra-household decision-making. This suggests that m-transfers could be a cost-effective means of providing cash transfers for remote rural populations. However, research on the broader welfare effects is still needed.

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1. Introduction

Conditional and unconditional cash transfers have been an important component of social protection policies in developing countries since the 1990s (World Bank 2006, World Bank 2009, DFID 2011). While there is widespread evidence of the effectiveness of such programs in improving development outcomes, the costs of such programs are rarely discussed. Yet many cash transfer programs present logistical, operational and security challenges, especially in countries with limited physical and financial infrastructure, as they require carrying cash in small denominations from urban centers to remote rural areas. These costs can affect the cost effectiveness of cash transfer programs as compared with other types of interventions.

The introduction of mobile phone-based money transfer systems (m-transfers) in many developing countries offers an alternative means of providing such cash transfers. By allowing the money to be transferred via a mobile phone, such programs could potentially reduce the costs to implementing agencies of providing cash transfers to remote populations, especially in areas with few financial institutions. Furthermore, m-transfer systems may also prove easier for cash transfer recipients to collect and use their transfers, provided they have ready access to m-transfer service providers.[1]

Using m-transfer systems to disburse cash may lead to additional impacts on program recipients. By altering the costs involved in obtaining the transfer, the zap program could affect program recipients' time use, alter the timing and location of their purchases and modify investment in productive activities. Access to the m-transfer system

[1] Cost-saving measures may also cause a deterioration in program performance along other dimensions. For example, the switch from cash currency to m-transfers might increase costs for recipients who cannot "cash out" their m-transfer, access m-transfer agents or use the technology.

could increase households' access to financial services and informal private transfers, thereby allowing households to better manage shocks (Morawczynski and Pickens 2009, Blumenstock, Eagle and Fafchamps 2011, Jack and Suri 2011). Access to the mobile phone could enable households to improve their communication with members of their social networks, thereby allowing them to better respond to shocks or improve their decision-making with respect to agriculture and labor markets (Jensen 2007, Aker 2010, Aker and Mbiti 2010). Finally, the greater relative privacy of the m-transfer approach could reduce inter-household sharing of transfers within the village (Jakiela and Ozier 2011) or affect intra-household decision-making with respect to the transfer (Lundberg, Pollack and Wales 1997, Duflo and Udry 2004, Doss 2006).

We report the results of a randomized cash transfer program in Niger, where some program recipients received a cash transfer via a m-transfer system (called Zap). In response to a devastating drought in Niger, households in targeted villages received monthly cash transfers as part of a social protection program. One-third of targeted villages received a monthly cash transfer via a mobile money transfer system (called *zap*), whereas one-third received manual cash transfers and the remaining one-third received manual cash transfers plus a mobile phone. As there was no pure comparison group due to the emergency nature of the intervention and the political situation at the time, this paper can only measure the impact of a different type of micro-cash transfer mechanism – namely, mobile money – as compared to manual cash interventions.

Overall, our results suggest that the technology strongly reduced the variable distribution costs to the implementing agency, as well as costs to the program recipients in accessing their cash transfer. The m-transfer approach also resulted in additional benefits for program recipients: households in *zap* villages spent the cash transfer on more types of items, consumed more diverse foods and cultivated more diverse crops as compared to those

receiving a manual cash transfer. These effects do not appear to be driven by baseline differences in household characteristics, differential attrition, the mobile phone handset or different price effects. Rather we posit that these effects appear to be explained by the greater privacy of the zap transfer mechanism and a shift in women's influence within the household.

We have a large body of evidence indicating that cash transfer programs can be effective, and similarly a large body of empirical work details the impact of mobile phones (Jensen 2007, Aker 2010, Aker and Mbiti 2010, Jack and Suri 2011, Blumenstock, Eagle and Fafchamps 2011). What is less well understood is whether combining these two interventions in ways only recently made possible by technological innovations can provide additional gains. To answer this question, we investigate whether the distribution of cash transfers via mobile phones has impacts above and beyond the distribution of cash transfers and mobile phones. This unique counterfactual reveals the extent to which the use of novel forms of currency is itself specifically beneficial in the provision of humanitarian aid.

While these results suggest important efficiency gains for cash transfer programs, without pre-program data from a randomly assigned comparison group, the welfare effects of this intervention are ambiguous. We therefore assess the spillover effects of the intervention using a within-village control group and anthropometric data collected from comparison villages after the program. Overall, we cannot reject the hypothesis that there was no effect of the program on non-eligible populations. In addition, we find that cash intervention is associated with improvements in child nutritional status.

The rest of the paper proceeds as follows. Section II describes the situation in Niger and m-transfer systems in general. Section III describes the research design. Section IV describes the different datasets and estimation strategy. Section V outlines a theoretical

framework of the impact of mobile money on household well-being, whereas Section VI provides estimates of the impact of the program. Section VII addresses some of the alternative explanations and Section VIII discusses potential mechanisms. Section IX provides a cost-benefit analysis and Section X concludes.

2. Background

2.1. Drought and Food Crises in Niger

Niger, a landlocked country located in West Africa, is one of the poorest countries in the world. With a per capita GNP of US$230 and an estimated 85 percent of the population living on less than US$2 per day, Niger is one of the lowest-ranked countries on the United Nations' Human Development Index (UNDP 2010). As the country spans the Saharan, Sahelian and Sudano-Sahelian agro-ecological zones, rainfall ranges from 200 millimeters (mm) per year in the northern regions to 800 mm in the south. Precipitation varies substantially across the country both within and across years (Nicholson, Some and Kone, 2000). Niger experienced six droughts between 1980 and 2005 (Government of Niger 2007).

A majority of households in Niger depend upon rainfed agriculture, with staple food crops consisting of millet, sorghum and fonio, and cash crops including cowpeas, peanuts, cotton and sesame. Inter-annual deviations in rainfall are positively associated with fluctuations in agricultural output, as yields depend upon the timing and quantity of rainfall.

Because of the correlation between rainfall and grain output, drought is positively correlated with food crises and famine. An estimated one-third of the country's population died during the "great famine" of 1931, with approximately 250,000 drought-related human fatalities occurring in the Sahelian region between 1968-1974 and 1983-84. In 2005, an estimated 2.4 million Nigeriens were affected by severe food shortages, with more than

800,000 of these classified as critically food insecure (FEWS NET 2005). Niger also suffered from both drought and harvest failures in 2009/2010, with 2.7 million people classified as vulnerable to extreme food insecurity (FEWS NET 2010). Rural households in Niger typically deal with such shocks by reduced consumption, asset depletion, tree-cutting and seasonal migration.

2.2. Mobile Money

Mobile phone technology has reduced the costs of communicating information over long distances in many parts of the developing world. This transformation has been particularly dramatic in sub-Saharan Africa, where investments in other infrastructures such as power, roads and landlines are limited. In Niger, there were 13 mobile phone subscribers per 100 people in 2010, as compared to fewer than .2 landline for every 100 people. The road network is equally poor: Despite the fact that Niger is one of the largest countries in Africa, the total road network was estimated to be 15,000 km as of 2005, of which only 8 percent were paved. There is less than 1 bank for every 100,000 people, making it one of the most "unbanked" countries in sub-Saharan Africa (CGAP 2010).

Since 2005, m-transfer systems have emerged in 80 developing countries in Africa, Asia and Latin America. These systems typically involve a set of applications that facilitate a variety of financial transactions via mobile phone, including transmitting airtime, paying bills and transferring money between individuals. Most m-transfer systems allow the user to store value in an account accessible by the handset, convert cash in and out of the stored value account, and transfer value between users by using a set of text messages, menu commands, and personal identification numbers (PINs) (Aker and Mbiti 2010). A "pseudo account" can be established by purchasing "electronic money" (e-money) from an agent, usually a third party or someone who works for the mobile phone operator or bank. The

user can then send e-money to another recipient with a phone, who then withdraws the e-money from their local transfer agent. Fees are generally charged for each transaction.

The first m-transfer system in Niger was introduced in January 2010. Known as Zap, the product was developed by the primary mobile phone service provider (Zain, now Bhartia Airtel) in multiple countries. Initial coverage, usage and growth of Zap was limited in 2010 and geographically focused in the capital city (Niamey) and regional capitals. The cost of making a $USD45 transfer using Zap cost $1.50 during this period, with different costs depending upon the amount transferred.[2]

3. Research Design

In 2010, an international non-governmental organization, Concern Worldwide, developed a humanitarian program in response to the 2009/2010 Niger drought and food crisis. In an attempt to prevent asset depletion and reduce malnutrition among drought-affected households, the program provided unconditional cash transfers to approximately 10,000 households during the "hungry season", the five-month period before the harvest and typically the time of increased malnutrition. Program recipients were to receive an average of 22,000 CFA ($USD 45) per month for five months, for a total of $USD 215. In an effort to facilitate the disbursement of cash in remote areas, Concern decided to implement a pilot study across 116 villages in 6 communes of the Tahoua region.

3.1. Cash Transfer Interventions

Three interventions were ultimately chosen for the pilot program. The reference was the standard *manual cash* intervention, whereby beneficiary households received

[2]To make a transfer between $20-$40 cost $1.50; to transfer more than $USD40 cost $3 per transfer.

unconditional cash transfers of 22,000 CFA per month (approximately $US45).[3] The total value of the transfer over the five-month period was approximately 2/3 of the total annual GDP per capita. Payments were made on a monthly basis, whereby cash was counted into envelopes and transported via armored vehicles to individual recipients. Rather than distributing the cash in each village, as is typical in such programs, a central village location was chosen for groups of 4-5 villages. Program recipients had to travel to their designated location on a given day to receive the cash transfer.[4]

The two additional interventions were variants of the basic intervention, one of which was aimed at reducing the costs of distributing cash to remote, sparsely-populated and in some cases insecure rural areas. Program recipients in the second group (*zap*) received their cash transfer via the mobile phone. After receiving the electronic transfer, recipients had to take the mobile phone to an m-transfer agent located in their village, a nearby village or a nearby market to obtain their cash. As less than 30 percent of households in the region owned mobile phones prior to the program, Concern also provided program recipients with mobile phones equipped with a Zap account, and paid for the transfer charges. The second intervention thereby differs from the manual cash intervention with respect to the transfer delivery *mechanism*, as well as the provision of the handset and the m-transfer technology.[5]

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[3]While the program encouraged program recipients to attend health centers, this was not a condition for receiving the actual transfer. Thus, the program was a de facto unconditional cash transfer program.

[4]Program recipients in cash and placebo villages were informed of the date and location of their cash transfer via a phone call a few days' prior to the transfer, primarily by contacting a point person within the village. While Concern tried to ensure that the cash distribution points were as close as possible to each village, it was not possible to have a distribution point in every village.

[5] While Zap was introduced into Niger in January 2010, there were a limited number of Zap agents in rural areas. Consequently, Concern Worldwide worked with Zain (now Bhartia Airtel) to ensure that Zap agents were registered within the program area. Agents were either registered in the village, in neighboring villages or in nearby markets. On the day of the cash transfer, program recipients would receive a special "beep" on their mobile phone, informing them that the transfer had arrived. The program recipient could then travel to a local agent (at her discretion) and show the value on the phone. The m-transfer agent would then remove the value of the cash transfer and "cash out", paying the value of the cash transfer to the program recipient

In an effort to disentangle the impact of the change in delivery mechanism from that of receiving a mobile phone, the third group (*placebo*) mirrored the manual cash intervention, but also provided a mobile phone. Like the manual cash group, program recipients received $US45 in physical cash on a monthly basis and had to travel to a meeting point to receive their cash. However, like the zap group, program recipients also received a Zap-enabled mobile phone.

Comparing the *placebo* and *cash* groups should allow us to disentangle the additional effect of having a mobile phone. Comparing the *zap* and *placebo* groups should allow us to detect the additional effect of m-transfer delivery mechanism (as compared with the manual cash delivery), since both groups were provided with mobile phones.[6]

3.2. Experimental Design

Prior to the introduction of the program, Concern Worldwide identified 116 "food deficit" villages in the Tahoua region, those classified by the Government of Niger as having produced less than 50 percent of their consumption needs during the 2009 harvest. Of these, some villages were prioritized for the *zap* intervention based upon their population size and proximity to skirmishes near the Niger-Mali border, thereby reducing the sample size to 96 villages. The remaining eligible villages were randomly assigned between the basic (manual cash), placebo and zap interventions. In all, 32 villages were assigned to the cash group, 32 to the placebo group and 32 to the zap group. A map of the project areas is provided in Figure 1.

An ideal evaluation would have also included a pure comparison (non-cash) group, plus a group with access to mobile phones and m-money (but no cash transfer). Due to the

[6]More specifically, we are testing whether the effect of the cash transfer plus the mobile phone (*placebo* group) is the same as the combined effect of receiving the cash transfer via the mobile phone (*zap* group). If the placebo treatment effect is additive, then we will not find a differential effect between the placebo and zap arms.

humanitarian nature of the intervention and the political situation at the time of the crisis, it was not possible to collect data from a comparison group during the program. Hence, while we can estimate the causal effect of alternative delivery mechanisms for cash transfer, we cannot estimate the impact of the *cash* transfer program in this context.[7] Nevertheless, using anthropometric data collect from comparison villages after the program, we are able to provide some insights into the welfare effects of the program. In addition, a substantial body of empirical evidence documents the impact of conditional and unconditional cash transfer programs in a variety of emergency and development contexts (World Bank 2009, Baird, McIntosh and Ozler 2011, DFID 2011).

Within each food deficit village, household-level eligibility was determined by two primary criteria: 1) the level of household poverty (determined during a village-level vulnerability exercise); and 2) whether the household had at least one child under five.[8] The number of recipient households per village ranged from 12 to 90 percent of the village population, covering an average of 45 percent of the population. In all villages, the cash transfer was provided to the woman. The study timeline is presented in Figure 2.

4. Data and Estimation Strategy

4.1. Data

The data come from four primary sources. First, a comprehensive household survey of more than 1,200 program recipients was conducted in all 96 villages. In addition, household surveys were conducted with non-eligible households from a subset of villages. The baseline survey was conducted in May 2010, with follow-up surveys in December 2010

[7]We collected anthropometric and village-level data from comparison villages after the program. We use these data to provide some insights into the welfare impacts of the cash transfer program, and attempt to control for selection on observables using inverse weighting of the propensity score.

[8]Using vulnerability criteria based upon livestock ownership, landholdings and the number of household members, households were classified into four categories (A, B, C and D), with C and D as the lowest categories. Households from the C and D vulnerability categories were selected for the program.

and May 2011. The research team located over 98 percent of households for the follow-up survey. Both the attrition rates and the distribution of attriting households' characteristics were similar across the treatment groups.

The household survey collected detailed information on household food security, demographics, asset ownership, agricultural production and sales, mobile phone ownership and usage, uses of the cash transfer and village and household-level shocks. As the surveys were conducted during a humanitarian crisis and over a short time frame, the research team was mindful of the time burden on respondents. As a result, the household surveys did not include a detailed income and expenditure module. We use the household-level data to measure the impact of the cash delivery mechanism on households' uses of the cash transfer and development outcomes and behaviors.

The second dataset is village-level survey, collected during the same periods of the household-level surveys. The village survey collected detailed information on village-level infrastructure, population and services, including mobile phone coverage, access to markets and the number of zap agents. We use these data to partially estimate the impact of the program on village-level dynamics.

The third dataset includes is weekly agricultural price information from over forty-five markets for a variety of goods between May 2010 and January 2011, as well as the date of each cash transfer in each village. We use these data to test for different effects of the cash transfer delivery mechanism (zap or manual cash) on local market prices, as these price effects could directly and indirectly affect household welfare.

The final dataset is anthropometric data among women and children in both intervention and comparison villages in May 2011. The comparison villages were chosen based upon similar observable characteristics with the intervention villages (including their food deficit status in 2009/2010), and respondents were chosen based upon a vulnerability

exercise similar to that conducted in intervention villages. We use these data to provide some insights into the welfare impacts of the cash transfer intervention for cost benefit analyses.

4.2. Pre-Program Balance of Program Recipients

Table 1 suggests that the randomization was successful in creating comparable groups along observable dimensions. Differences in pre-program household characteristics are small and insignificant. Average household size was nine, and a majority of respondents were members of the Hausa ethnic group. Less than 15 percent of households had any form of education, and 72 percent of households were in monogamous marriages. Less than thirty percent of households owned a mobile phone prior to the start of the program, yet 63 percent of respondents had used a mobile phone in the few months prior to the baseline. Program recipients in the *zap* group were slightly less likely to be from the Hausa group and were older as compared with those in the *placebo* groups. Overall, zap program recipients were older, less educated and more likely to be from the Fulani or Touareg ethnic groups.

Table 2 provides further evidence of the comparability of the different interventions for key outcomes, namely food security, agricultural production, migration and coping strategies. Over 90 percent of households relied upon agriculture as a primary income source, and approximately 50 percent had at least one seasonal migrant in the past year. A strong majority (97 percent) of households had experienced drought, and household diet diversity was 3 (out of 12 categories of foods).[9]

[9]Overall, we made over 75 comparisons and find 3 variables that are statistically significant at the 10 percent level, 2 that are statistically significant at the 5 percent level and none at the 1 percent level. These results are in-line with what we would expect from random assignment (Barrera-Osorio, Bertrand, Linden, and Perez-Calle, 2011).

4.3. Estimation Strategy

To estimate the impact of the *zap* program on a variety of outcomes, we use a simple reduced form regression specification comparing outcomes in the immediate post period (December 2010). This takes the following form:

$$(1) \qquad Y_{iv} = \beta_0 + \beta_1 zap_v + \beta_2 placebo_v + \mathbf{X}'_{iv0}\gamma + \theta_C + \varepsilon_{iv}$$

The variable Y_{iv} represents the outcome of interest (households uses of the cash transfer, food security, assets, agricultural production) of individual or household i in village v. zap_v is an indicator variable for whether the village participated in the m-transfer program, whereas $placebo_v$ is an indicator variable for whether the village was in the placebo group.[10] θ_C are geographic fixed effects at the commune level, which is a geographic grouping that includes approximately twenty villages. To improve precision, we include a vector of household baseline covariates, $\mathbf{X}'_{iv0,}$ such as ethnicity, education and whether the households is female-headed. The error term consists of ε_{iv}, which captures unobserved individual or household characteristics or idiosyncratic shocks. We cluster the error term at the village level to account for the program design and to correct for heteroskedasticity when there is a binary dependent variable. The coefficients of interest are β_1 and β_2, which capture the average impact of the two treatments as compared to the basic cash intervention. We also test whether these coefficients are equal. When data are available for two post periods, we pool the data and include a linear time trend variable.

5. Conceptual Framework

[10]Two villages that were originally assigned to one intervention received a different intervention, due to management oversight. Nevertheless, villages were classified by their initial intervention, so the estimated program effect is the impact of being offered the treatment (intention to treat) not the impact of the treatment itself. Most results are robust to measuring the impact of the treatment itself.

The m-transfer system, as compared with the manual cash transfer, could potentially impact household outcomes through six primary mechanisms. First, by altering the mean and variance of costs involved in obtaining the transfer, the zap program could affect program recipients' time use. If the m-transfer mechanism reduced program recipients' transport and opportunity costs involved in obtaining the transfer, then this would reduce lost income to zap program recipients. If, however, the new technology made it more difficult for program recipients to access their cash – either due to the limited number of m-transfer agents, difficulty in using the technology or phone-charging costs – this could have increased costs for the zap households and reduced participants' ability to access the cash transfer.

Second, as households in *zap* villages did not have to travel to a pre-arranged location – but could "cash out" from any m-transfer agent – households could have changed the location and timing of their cash transfer expenditures. For example, *zap* households could have been more likely to make purchases within the village rather than in an external market, thereby allowing women greater freedom to spend the cash transfer themselves, or having access to different types of goods and services from *zap* transfer agents.

Third, since the zap program was new and involved a training on how to use the technology, program recipients could have simply been better informed about the program, including the time, date and frequency of the cash transfers. This could have enabled households to more efficiently plan their use of the cash transfer throughout the duration of the program.

Fourth, access to the m-transfer technology could have increased households' familiarity with financial transfer services and access to informal private transfers. This could have facilitated households' ability to send or receive transfers outside of the village,

conditional on the availability of the m-transfer service for both parties. Improved access to such transfer mechanisms could thereby help households to better cope with risks and shocks, potentially affecting investment or consumption decisions (Blumenstock, Eagle and Fafchamps 2011, Jack and Suri 2011).

Fifth, access to the mobile phone technology could reduce households' communication costs with members of their social network, thereby increasing access to price and labor market information, enabling households to communicate about shocks and thereby allowing households to improve their decision-making.

Finally, since m-transfers reduced the observability of the amount and timing of the cash transfer, this could have affected inter-household sharing of the cash transfer, thereby leaving more income available for the household (Jakiela and Ozier 2011). The transfer mechanism could also have reduced intra-household observability of the transfer, perhaps increasing women's bargaining power, changing the intra-household allocation of resources (Lundberg, Pollack and Wales 1997, Duflo and Udry 2004, Doss 2006, Doepke and Tertilt 2011).[11]

Whichever of these channels is at work, it is important to note that the impacts outlined in this paper are only for the short-term, ie, immediately after the program. Some of these effects imply behavioral changes that might only be observed several years after the program or are difficult to test using survey data. We provide insights into the mechanisms at work in Section 8.

6. Results

[11]Observing a change in spending based upon the cash transfer mechanism would be consistent with a cooperative bargaining model with different preferences for males and females and different Pareto weights, or a non-cooperative bargaining model with imperfect substitutability between male and female contributions to public goods or different appreciations for public goods (Doepke and Tertilt 2011).

6.1. Did the Zap Intervention Reduce Distribution and Recipients' Costs?

A key motivation for using the zap approach in Niger was to reduce the costs of the implementing agency in providing the cash transfer, as well as those of program recipients in obtaining the cash transfer. Figures 3 and 4 show the relative costs to each party.

Figure 3 shows the total costs of each transfer mechanism for Concern Worldwide, including fixed and variable costs, for each month of the program. The primary initial investment costs of the program included expenses for identifying program recipients, purchasing mobile phones and training recipients in using the mobile phones, the latter of which were only for the zap intervention. Variable costs for the manual cash distributions included transport and security costs, as well as costs associated with organizing the cash into individual envelopes. The average per recipient cost was US$12.76 in cash/placebo villages and US$13.65 in zap villages, or $.90USD more per recipient. Excluding the cost of the mobile phones, the per-recipient cost of the zap intervention falls to $8.80 per recipient. Thus, while the initial costs of the zap program were significantly higher, variable costs were 30 percent higher in the manual cash distribution villages. [12]

Figure 4 shows the recipients' transport and opportunity costs related to obtaining the cash transfer. As both the placebo and manual cash groups received the cash transfer via the same mechanism, we pool the two groups and compare means between the *zap* and *cash/placebo* villages. Overall, program participants in zap villages incurred significantly fewer costs for obtaining the cash transfer. Whereas cash and placebo program recipients travelled an average of 4.04 km round-trip to obtain the transfer, zap program recipients only travelled .9 km to "cash out" at the nearest agent, with a statistically significant difference at the 1 percent level. This is equivalent to an opportunity cost savings of 30

[12]This includes amortized fixed costs for the mobile phones over the program period. If the program had been extended to 12 months, the per-recipient costs would be relatively equal.

minutes for each cash transfer, or 2.5 hours over the entire program.[13] Based upon an average daily agricultural wage of USD $3.60, this time savings would translate into USD $.92 over the cash transfer period. This is equivalent to 2.5-3 kilograms of millet, enough to feed a family of five for one day.

6.2. Did the Zap Program affect Recipients' Uses of the Transfer?

The uses of the cash transfer that the cash transfer recipients reported might be instructive about the kinds of effects of different cash transfer delivery mechanisms that we might expect. As Concern Worldwide did not stipulate that the cash transfer be used for a specific purpose, nor was it conditional on a particular behavior, recipients were free to spend the cash transfer how they wished. Overall households in the manual cash villages used their transfer to purchase 4.40 different categories of goods, including staple grains (99%), cowpeas (42%), meat (40%), oil (70%), condiments (70%), health expenses (28%), seeds (20%), school fees (7%), debt reimbursement (7.4%) and labor costs (2%). (Respondents could list more than one use of the cash transfer, so the total can exceed 100%.) Thus, cash transfer recipients primarily used the transfer to ensure immediate consumption needs, but also to make limited agricultural investments and avoid asset depletion.[14]

Table 3 shows the different uses of the cash transfer by intervention group. Overall, the results paint a picture of more diverse uses of the cash transfer by zap households. Households in zap villages purchased .78 more types of food and non-food

[13]In all likelihood, this is a lower bound of the costs for the manual cash transfer program, as it excludes the program recipients' waiting time (an average of 1 hour per cash transfer). Including wait time, the average cost savings to program recipients over the program period would have been 7.5 hours, or $3.60.

[14]The amount of the cash transfer was slightly higher than the cost of a 100-kg bag of millet ($45 for the monthly cash transfer as compared with US$ 42 for a bag of grain during the period of the transfer). Therefore, a majority of households first purchased a wholesale quantity of millet (100-kg bag) and used the remaining amount for other purchases.

items as compared with the cash group, and .84 more types of items as compared with the placebo group, with a statistically significant difference between each pair. While the likelihood of purchasing staple grains did not differ by the cash transfer mechanism, program participants in the zap group were 20-22 percentage points more likely to purchase non-staple grains (such as rice and corn), 9 percentage points more likely to purchase cowpea, and 13-16 percentage points more likely to purchase meat, condiments and oil as compared with those in the cash and placebo groups (Panel A). All of these differences are statistically significant at the 1 percent level.

Table 3 (Panel B) shows that the uses of the cash transfer for non-food items, namely school fees, health expenses and clothing. While relatively fewer households used the transfer for these purposes, there was no difference between the zap, placebo and cash groups. Thus, while the zap program led to more diverse food purchases, especially those associated with a more diverse diet, it did not lead to different health or education expenditures. This is not surprising given the high marginal utility of income during this period.

6.3. Did the Zap Intervention Affect Food Security?

Table 4 presents the results from a regression of Equation (1) for a variety of food security indicators and asset categories in Niger. The results in Table 4 are correlated with the different uses of the cash transfer observed in Table 3. Household diet diversity is .31 points higher in the zap villages as compared with the cash villages, with a statistically significant difference at the 5 percent level. Diet diversity was also higher in the zap group as compared with the placebo group (Panel A). When assessing the impact on specific food groups, there was no impact of the program on the consumption of staple foods, namely grains. However, households in zap villages were 6-7 percentage points more likely to consume beans, 3 percentage points more likely to consume fruits, and 9-11 percentage

points more likely to consume fats than households in cash and placebo villages. All of these differences are statistically significant at the 1 or 5 percent levels. Overall, these effects represent a 30-percent increase in consumption of beans and fats, and an 80-percent increase in fruit consumption, although this only affects a small percentage of households.

Panel B assesses the impact of the program on asset decapitalization, as asset accumulation would not have been expected during the program. Overall, households in zap villages had .64 more asset categories as compared with those in the cash and placebo groups, 25 percent higher than those in the cash group. These differences are primarily due to increased mobile phone ownership in the zap and placebo groups. Excluding the mobile phone, the program did not have an impact upon durable asset ownership (carts, plows, bikes and mopeds), as there is no statistically significant difference between the zap, placebo and manual cash groups. However, there was a marginal increase in non-durable assets: Excluding mobile phone ownership, households in *zap* villages had .20 more non-durable assets (lamps and flashlights) as compared with the placebo or cash groups, with a statistically significant difference at the 5 and 10 percent levels. This suggests that zap households were selling non-durable assets less frequently than those in placebo or cash villages.

6.4. Did the Zap Intervention affect Agricultural Investment and Production?

Table 5 presents the results of regressions of Equation (1) for a variety of agricultural outcomes, as the reduced costs could have freed up program recipients' time for agricultural investments. Unsurprisingly, the zap program did not have an impact upon the likelihood of cultivating or land ownership. However, the program did affect crop choices: Households in zap villages grew .48 more types of crops than those in the placebo and manual cash villages, 13 percent higher as compared with those in the cash group (Panel A). This

difference is statistically significant at the 1 percent level, although there is not a difference between the zap and placebo groups. These effects are not driven by changes in the likelihood of growing traditional staple food and cash crops, such as millet, sorghum, cowpeas and peanuts. Rather, they were due to the increased cultivation of marginal cash crops: zap households were 9-18 percentage points more likely to grow *vouandzou* and *okra*, two cash crops that are primarily grown by women on marginal lands in Niger.[15] This effect appears to be primarily driven by *zap* households, as the differences are statistically significant as compared with manual cash and placebo groups. As detailed plot-level data are not available, we do not know whether these crops were grown on new or existing lands. However, focus group discussions in targeted villages revealed that women primarily grow these crops on household land or on a separated rented plot (with rental costs between $USD 4-10). In either case, the spouse must give permission to cultivate these crops.

The changes in crop choice did not translate into different production levels or marketing strategies across the three groups (Panel B). There were no differences across the different interventions in the quantity of grains or cash crops produced, the likelihood of selling those crops after the harvest or the quantity sold immediately after the harvest. Thus, while the zap intervention appeared to change households' agricultural investment decision, this did not translate into increases in production or marketing behavior.

7. Alternative Explanations

7.1. Attrition

There are several threats to the validity of the above findings. First, the *zap* intervention could have resulted in differential uptake of the program, thereby affecting the intention to treat estimates. For example, if zap households had more difficulty in finding

[15]In fact, *vouandzou* in Hausa is known as "*gojiya mata*", literally translated as the "women's peanut".

m-transfer agents or using the technology, they might have been less likely to access the cash transfer. Or, if households in *zap* villages were more motivated to stay in the program because of the presence of the new technology, then there could have been differential attrition across the three groups. A means comparison of the three groups for each of these outcomes shows that there are no differential effects in attrition, the probability of receiving the cash transfer or shocks across groups (Table 6).

7.2. Access to Zap Agents

The program introduced new infrastructure into the region, primarily by working with the mobile phone operator to register private kiosk-owners and traders as zap agents. While these agents were already village residents, becoming a zap agent could have enabled them to provide new types of financial or commercial services to households, thereby improving zap households' access to foodstuffs and agricultural inputs. Table 6 shows that this is not the case. Only 3 percent of manual cash villages had a zap agent, without a statistically significant difference between the zap, placebo and manual cash villages. While these results are not very precisely measured, this suggests that zap agents were evenly distributed across different villages, and that differential access to zap agents is not driving the results.

7.3. Migration

The presence of mobile phone technology could have had differential effects upon household seasonal migration, as they could have obtained labor market information via the mobile phone technology. Table A1 shows the effect of the different cash transfer mechanisms on seasonal outmigration. Overall, the probability and intensity of seasonal migration was higher among zap and placebo households as compared to those in the manual cash groups. Nevertheless, there is not a statistically significant difference

between the *zap* and *placebo* groups, suggesting that the impact is primarily driven by the provision of the mobile phone. These changes also did not result in increases in the frequency or amount of remittances received over the time period of the study (Table 9), thereby suggesting that the previous results are not driven by increased informal private transfers. This is discussed in more detail in Section 8.

7.4. Multiple Hypothesis-Testing

In Tables 3-5, we examine the impact of the zap program on 30 different outcomes for three treatment interventions. This raises concerns that the observed effects cannot be attributed to the zap intervention, but are rather simply observed by chance among all of the different outcomes. Following Gibson, McKenzie and Stillman (2010), we use the Bonferroni correction for multiple testing. Using an alpha of 10 percent, and assuming an inter-variable correlation of .08 (for transfer uses) to .35 (for household diet diversity), the Bonferroni p-value would therefore be between .002 and .005.[16] The outcomes that remain significant at these adjusted levels are the cost of obtaining the cash transfer, the different uses of the cash transfer (number of food and non-food items, other grains, condiments and oil) and some of the household diet diversity indicators (namely, overall diet diversity and the likelihood of consuming fats). Thus, we are confident that the zap intervention affected recipients' costs in obtaining the cash transfer, recipients' use of the transfer and some measure of diet diversity. While the likelihood of producing marginal cash crops remains statistically significant when comparing the *zap* and *cash* groups, this is not the case with the *zap-placebo* comparison, suggesting that this effect may be more related to mobile phone ownership than to the *zap* mechanism (Aker and Ksoll 2012).

[16]The Bonferroni correction without accounting for inter-outcome correlation would yield an adjusted p-value of .001. However, in the case of correlated outcome variables, the mean correlation between outcome variables can be included as a parameter in the Bonferroni adjustment. A mean correlation of zero would yield the full Bonferroni adjustment, whereas a mean correlation of one would mean no adjustment.

7.5. Prices

A final alternative explanation to the interpretation of the above findings is the potential differential effects of the cash transfer mechanism on prices. If the manual cash transfer mechanism put greater inflationary pressure on local markets as compared with the zap transfer mechanism, this could reduce the value of the cash transfer in those villages and decrease the number of goods purchased or consumed. Or, if the zap transfer increased prices more quickly and producers were price elastic, this could have encouraged households to consume goods that were less affected by the price increase, or increased incentives for households to cultivate certain crops.[17]

Since the intervention occurred at the village level, village-level price data would be optimal for investigating these impacts (Cunha, Di Giorgi and Jayachandran 2010). Yet as over 60 percent of program recipients' purchases in took place in weekly markets – a majority of which are located outside of the village – weekly market price data were collected. The randomized nature of the program implies that some markets were linked to both manual cash and zap villages, thereby making it difficult to differentiate the impact of each and implying that the stable unit treatment value assumption is almost certainly violated.

Despite these limitations, our analysis treats each market as a local economy and examines food prices as the outcome. Using weekly price data between May and December 2010 from over 45 markets in the region, we estimate the impact of the cash transfer delivery mechanism on weekly prices by using the following regression:

$$ln(p_{ij,t}) = \alpha + \beta_1 zap_{j,t} + \beta_2 cash_{j,t} + \theta_t + \theta_j + \varepsilon_{ij,t} \quad (2)$$

[17]For a discussion of the potential impact of mobile money on the velocity of money and inflation, see Jack, Suri and Townsend (2010).

where $ln(p_{ij,t})$ is the log price of agricultural good i in market j at week t, $zap_{j,t}$ is an indicator variable equal to one if a village within a 10 km-radius of the market received a transfer via zap during week t, 0 otherwise; $cash_{j,t}$ is an indicator variable equal to one if a village within a 10-km radius of the market received a manual cash transfer during week t, 0 otherwise; θ_t represents a full set of time fixed effects, either monthly or weekly, whereas θ_j are a set of market-level fixed effects, which will capture characteristics such as market size, road quality and infrastructure. We also include a variable for the number of villages within a certain radius of the market receiving the cash or zap transfer at that time, as we would expect larger income effects in markets where a higher density of cash transfers took place. To partially account for non-stationarity, we estimate equation (2) via first differences. Standard errors are clustered at the market level. To control for differential price trends across markets during the period of interest, we also include market-specific time trends.

Table 7 presents the results of these regressions, first for any cash transfer and then by the type of cash transfer delivery mechanism. Overall, the presence of a cash transfer in a particular market area during the week did not appear to have had a statistically significant impact upon staple food prices. The cash transfer program did not have an impact upon retail or wholesale prices for staple grains (Columns 1-3), rice or vegetable oil. For cowpea, the effect is only observed for large quantities: The presence of a cash transfer in a nearby village during a particular week increased cowpea prices by 2 percent, with a statistically significant effect at the 10 percent level. The absence of results suggests that local supply on these markets was able to absorb the potential additional demand from the cash transfer program, especially for staple food crops.

Panel B measures the impact the cash transfer delivery mechanism on weekly food prices. Overall, the results are consistent with those in Panel A. The presence of the zap or manual cash transfer program in a nearby village did not have an impact upon staple grain prices (Columns 1, 2 and 3), and there is no statistically significant difference between the two cash transfer mechanisms. The same is true for cowpeas (Columns 4 and 5) and vegetable oil (Column 8). The one difference was for retail rice prices: the presence of a zap transfer during a particular week increased rice prices by 3 percent, with a statistically significant difference between the zap and cash transfer mechanisms. This is consistent with the finding that zap households were more likely to purchase other cereals, primarily rice and corn (Table 3).[18]

As all households were net consumers of local and imported grains during this period, an increase in rice prices would have reduced consumer welfare for recipient and non-recipient households living near those markets affected by the zap transfer.[19] Thus, the price effects suggest that the previous results are a lower bound for the impacts of the zap intervention on recipient households. While the price effects could have also had adverse effects on consumption of non-recipients living in the village and region, overall, the price analysis suggests that were not strong price effects due to the program.

8. Potential Mechanisms

8.1. Did the Zap Intervention Change Awareness, Location and Timing of Expenditures?

[18]Given the price of a bag of millet, households typically purchased small quantities (e.g., 1 kg) of non-staple grains (such as rice). This explains the different results on retail and wholesale quantities rice prices.
[19]While the increase in rice prices could potentially increase incentives to produce rice, rice is not produced in this area of Niger.

Table 8 provides some evidence of the location, timing and uses of the cash transfer. For example, the lower costs related to accessing cash via the zap distribution mechanism could have changed the purchase patterns of program recipients, allowing them to purchase goods closer to home. Or, the m-transfer system could have enabled recipient households to better spread their expenses over multiple periods, as they were able to "cash out" at their leisure. Alternatively, the "innovation" of the m-transfer technology could have increased program recipients' awareness of the cash transfer program, thereby reducing uncertainty about the cash transfer and allowing households to more optimally allocate expenses across different months.

Overall, the data do not allow us to conclude that the program impact is driven by one of these mechanisms. Forty-three percent of recipients in the cash villages spent their transfer at a kiosk within the village, whereas sixty-one percent spent the cash transfer at a market outside of the village. While this percentage was higher in zap villages, there is not a statistically significant difference between the pairs (Table 8, Panel A). Over 50 percent of manual cash households spent their transfer all at once, without a statistically significant difference between zap, placebo and cash groups (Table 8, Panel A). Only 25 percent of cash and placebo program participants could correctly cite the total amount or duration of the cash transfer prior to the program, with no statistically significant difference between the zap and cash/placebo treatments (Table 8, Panel B).

8.2. Did the Zap System Increase Households' Access to Private Transfers?

By having access to the m-transfer (zap) technology, program recipients in zap villages could have received informal private transfers from migrants and other family members more easily, thereby augmenting the income increase derived from the transfer and

increasing the diversity of their purchases or diets. Table 9 shows the results of regressions for a variety of indicators related to private money transfers (person-to-person). While households in both zap and placebo villages were more likely to receive remittances (correlated with higher rates of migration), this did not affect the frequency or amount of remittances transferred to the household during this period. Moreover, households in all villages primarily received remittances via Western Union (34 percent) or friends (53 percent), with less than 1 percent of households receiving remittances via the m-transfer technology (Panel A). This is supported by data on the percentage of program recipients who used zap to make money transfers – very few used the m-transfer technology, and there were no statistically significant differences between the three groups. These results are similar for those households affected by a shock in 2010, whom we would expect to use the m-transfer technology (Table 9, Panel B). Thus provides evidence that the results are not driven, at least in the short-term, by the impact of the m-transfer technology on private transfers.

8.3. Did the Zap Program Change Mobile Phone Usage?

It is possible that the zap program could have changed zap households' access to information via the mobile phone, thereby affecting agricultural practices and diet diversity. Furthermore, even if the zap program did not affect households' likelihood of sending or receiving private transfers via the mobile phone, the handset could have facilitated communications with migrants and their ability to request remittances via other channels.

Table 10 shows the impact of the program on mobile phone ownership and usage for each type of intervention. Unsurprisingly, the zap program increased mobile phone ownership and the frequency of usage since the last harvest, with a statistically significant

difference between the zap and cash villages, as well as between the zap and placebo villages. While respondents in zap villages were more likely to make calls or send and receive beeps as compared to placebo and cash villages, there was not a statistically significant difference in their probability of receiving a call, writing or receiving SMS or transferring money via the m-transfer system (Panel A). Overall, households in zap villages were more likely to communicate with friends and family members within Niger and to communicate a shock, but there is not a statistically significant difference between the *zap* and *placebo* groups (Panel B). Taken together, these results suggest that households in zap villages used the phone in more active ways as compared to those in the placebo and cash groups, although the operations are primarily associated with the *zap* program – such as beeping, receiving a *zap* transfer or receiving a SMS. Thus, while zap recipients used the mobile phone in more active ways, this did not necessarily translate into impacts on other outcomes during the time of the program.

8.4. Did the Zap Intervention Change Inter-Household Sharing?

As transfers via the m-transfer system were more difficult for outsiders to observe, this could have changed inter-household sharing within villages, thereby leaving more disposable income available for recipient households (Jakiela and Ozier 2011). Table 11 shows the impact of the program on sharing of the transfer and goods purchased from the transfer with different groups within and outside of the village.

Overall, the zap mechanism did not appear to have a strong effect on inter-hosuehold share. Eighteen percent of households shared their cash transfer with another household, whereas 60 percent of households shared goods purchased from the cash transfer with another household. However, there was no difference in sharing across households in each of the three groups. The one exception was sharing outside of the village: zap households

were 1-2 percentage points more likely to share their transfer with friends or family members outside of the village. These results suggest that while the zap transfer mechanism did not affect the likelihood of inter-household sharing, it did affect the allocation of sharing across villages.

8.5. Did the Zap Intervention Change Intra-Household Decision-Making?

Unlike the manual cash mechanism, the zap transfer mechanism made it more difficult for program recipients' spouses to observe the arrival of the transfer, as the program recipient was notified of the transfer arrival via a SMS message and a discrete "beep". This could have allowed zap program recipients to spend the cash transfers themselves, rather than providing it to their spouses, or provided program recipients with greater bargaining power with respect to the use of the cash transfer.[20] The program was implemented an area of Niger where socio-cultural norms do not permit younger, married women belonging to the Hausa ethnic group to travel to markets, either individually or in groups.[21] For this reason, we would not expect to find strong average effects of the program on women's visible control over the cash transfer, but might find heterogeneous effects across different ethnic groups or across female-headed and male-headed households.

Table 12a provides evidence of the impact of the zap program on a variety of direct and indirect measures of intra-household decision-making.[22] Overall, 53 percent of program

[20]When the analyses are restricted to the purchases from the last cash transfer, the results are largely similar. This suggests that intra-household privacy did not reduce over the time period of the cash transfers.
[21]This is in contrast to cultural norms in the Fulani and Touareg groups, where women often travel to markets to sell dairy products and purchase food items.
[22] In order to formally test for differences in intra-household decision-making, we would ideally want to test outcomes across each of the three interventions between households with male and female program recipients. As all program recipients were women, we are unable to do this, and thus can only compare proxy indicators for intra-household decision-making across the three interventions, conditional on the program recipient being a woman.

recipients in the manual cash villages reported that they were responsible for spending the cash transfer, with no statistically significant difference among the three groups (Panel A). Among the remaining households, the transfer was primarily spent by the program recipients' husband or son. Yet almost all recipients (99 percent) stated that they were consulted on the use of the cash transfer, with no statistically significant difference between the three groups.

Panel B uses alternative measures of intra-household decision-making, namely, how households made decisions regarding children's education, agricultural planting, inter-household transfers and clothing purchases. Overall, zap households made decisions in a more collaborative manner as compared to the cash households: men in zap households were 8-9 percentage points less likely to make decisions on education, planting and transfers by themselves (Panel B), with a statistically significant difference between the zap and cash groups. While the results are in general stronger for zap households, there is not a statistically significant difference between the zap and placebo groups, suggesting that the mobile phone handset could have changed intra-household bargaining along these dimensions.

Panel C shows the impact of the program on the share of money spent on men's clothing for Muslim festivals, as compared with children's and women's clothing. Overall, while the share of children's to men's clothing was higher in the zap households as compared with those in the placebo and cash groups, the result not statistically significant at conventional levels.

Table 12b attempts to provide insights into the question of intra-household bargaining by looking at heterogeneous impacts of the program by female-headed households and ethnicity, including interactions between the cash transfer mechanism and each of these groups. If intra-household bargaining is truly the mechanism at work, then the zap

program should have a relatively stronger impact on male-headed households (Panel A). Furthermore, as different ethnic groups in the region have different intra-household roles and responsibilities, we would expect to see a relatively stronger effect among those ethnic groups where women have relatively weaker bargaining power -- in this case, the Hausa group (Panel B).

Overall, the results suggest that male-headed households in zap villages were more likely to use the cash transfer to purchase cowpea, condiments and oils than female-headed households, with a statistically significant difference between the zap and placebo groups. While zap program recipients in Hausa households were more likely to have more diverse uses of the transfer, to jointly decide about inter-household transfers and to devote a higher share of their clothing expenditures to children (as compared with men), none of these results are statistically significant at conventional levels given the lack of precision of the estimates.

While the results will provide suggestive evidence of potential intra-household bargaining, we cannot rule out the possibility that unobserved differences between male and female-headed households, or Hausa and Fulani/Touareg households, could be driving the results.[23] Nevertheless, these results, taken together with more diverse uses of the cash transfer, greater diet diversity and increased cultivation of women's cash crops, provide some suggestive evidence that the zap mechanism could have changed intra-household decision-making, thereby allowing women to have greater control over the spending of the cash transfer and engage in consumption and production decisions. Nevertheless, these results are suggestive at best.

[23]When splitting the sample, the zap results are strongly statistically significant for all of the variables for the Hausa group. However, we cannot reject the equality of zap coefficients across the Hausa and Fulani/Touareg groups.

9. Cost-Benefit Analysis of the Zap Intervention

A natural question related to the use of a new approach is whether the expected benefits outweigh the additional costs. This is especially the case in a country such as Niger, where, despite widespread growth in mobile phone coverage over the past decade, m-transfer systems are still relatively new. Thus, using an m-transfer system to distribute cash transfers can require significant up-front investments for governmental and non-governmental organizations, including ensuring access to the mobile phone handset and m-transfer technology. It also implies that a network of m-transfer agents already exists in the distribution areas, or can be developed, thereby requiring partnerships with mobile phone operators. In this section, we explore whether a m-transfer program should be a priority for governmental and non-governmental institutions for cash transfer programs by assessing the relative costs and benefits of the program. [24]

A full cost-benefit analysis of the zap program would require estimates of both the social and private returns to the cash transfer program. As we do not have information on household income and expenditures, we instead focus on the value of a narrow range of benefits. In addition, a proper cost-benefit analysis would use a causal estimate of the impact of the cash transfer program. As we do not have a pure comparison group from before the program, we unable to estimate the impact of the cash transfer intervention. However, using data from comparison households within the intervention villages, as well

[24]Dhaliwal, Duflo, Glennerster and Tulloch (2011) outline the relative advantages and disadvantages of cost benefit and cost effectiveness analyses. While the cost effectiveness analysis (showing the program effect on one outcome measure) is preferred in many contexts, we use a cost benefit analysis for several reasons. First, as the program was an unconditional cash transfer, it had effects on multiple outcomes, which are difficult to capture in one indicator. Second, as m-transfers were new in Niger and in the program region, the program required significant initial investments (including purchase mobile phones), which would not be required in contexts where m-transfer services have been in existence for several years.

as nutritional data from comparison villages immediately after the program, we estimate the impact of the cash program.[25]

As outlined in the Figure 3, the average per recipient cost over the life of the project was US$12.76 in cash/placebo villages and US$13.65 in zap villages, or $.90USD more. While there was a range of benefits from the zap intervention, we focus on two for the cost-benefit analysis: the impact on child nutritional status, as measured by weight-for-height z-scores in children under 5. Table 13 shows the results of a regression on a variety of measures of child nutritional status on any cash transfer program, as well as different types of cash transfer mechanisms. Overall, children's weight-for-height z-scores were .18 s.d. in the cash transfer villages, with a statistically significant difference at the 10 percent level (Column 1). While the z-scores were relatively higher in each of the cash transfer groups, it was relatively higher in the zap group (Column 2). The program also reduced the prevalence of wasting in the population by 7 percentage points, with a statistically significant difference at the 10 percent level. These results, combined with the previous findings with respect to time usage and diet diversity, suggests that the additional costs of the zap intervention yielded an equivalent or higher benefit for zap program recipients. If the program yields benefits in the longer-term, perhaps by allowing households to send and receive more informal transfers or access formal financial services, this could potentially yield a higher rate of return.

10. Conclusion

[25]As described in earlier sections, the intervention villages were chosen based upon the Government of Niger's "food deficit list". The government calculations are highly unreliable, due to outdated census data and the timing of the data collection for production statistics (which occurs prior to the actual harvest). As a result, NGOs and donors have often complained that the vulnerability criteria do not reflect actual needs. A comparison of household and village-level time-invariant characteristics collected in control villages in May 2011 suggests that the two groups were largely similar along observable time-invariant dimensions. While we cannot argue that the control villages were randomly assigned, we compare non-eligible households in treatment villages with non-eligible households in control villages to test for potential spillover effects of the program.

Cash transfer programs are an important part of the social protection policies in many developing countries. While there is significant evidence on the impact of such programs on improving specific outcomes, there is more limited evidence on their impact in humanitarian contexts, as well as their cost-effectiveness as compared with other types of interventions. This is particularly important in countries where distributing cash involves significant logistical, operational and security costs.

An intervention that provided a cash transfer via the mobile phone strongly reduced the costs of program recipients in obtaining the cash transfer, and reduced the implementing agency's variable costs associated with distributing cash. This suggests that mobile telephony could be a simple and low-cost way to deliver cash transfers. In addition, we observe that those in the m-transfer group bought more types of food and non-food items, increased their diet diversity, depleted their non-durable assets at a slower rate and produced a more diverse basket of agricultural goods. These differences are primarily due to the m-transfer intervention, and not to the presence of the mobile phone, suggesting that a program that jointly distributes mobile phones and cash transfers would not yield the same impacts. This effects appear to be due to the reduced costs of the program and the greater privacy of the m-transfer mechanism, which are potentially linked with changes in intra-household decision-making.

The m-transfer approach may be limited in its application to all contexts. First, it will only be effective in cases where telecommunications infrastructure currently exists, which could limit its utility in remote areas. Second, in areas with high rates of illiteracy – as is the case in Niger – program recipients might not able use the m-transfer technology on their own, implying that they might need help from other family members, friends or m-transfer agents. This could potentially limit the use of the technology by program recipients for informal private transfers or in accessing other mobile financial services, but

could be beneficial for the household as a whole. And finally, the short-term impacts of the program might not persist in the longer-term.

Despite these caveats, the widespread growth of mobile phone coverage, cheaper mobile phone handsets and m-money services in developing countries suggests that these constraints could be easily overcome. In addition, the benefits of the program in a context such as Niger -- a country with limited investment in power, roads and landlines, low literacy rates and one of the highest rates of financial exclusion in sub-Saharan Africa -- suggests that the approach could thrive in less marginalized contexts.

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Figure 1. Map of Villages in the Project Area

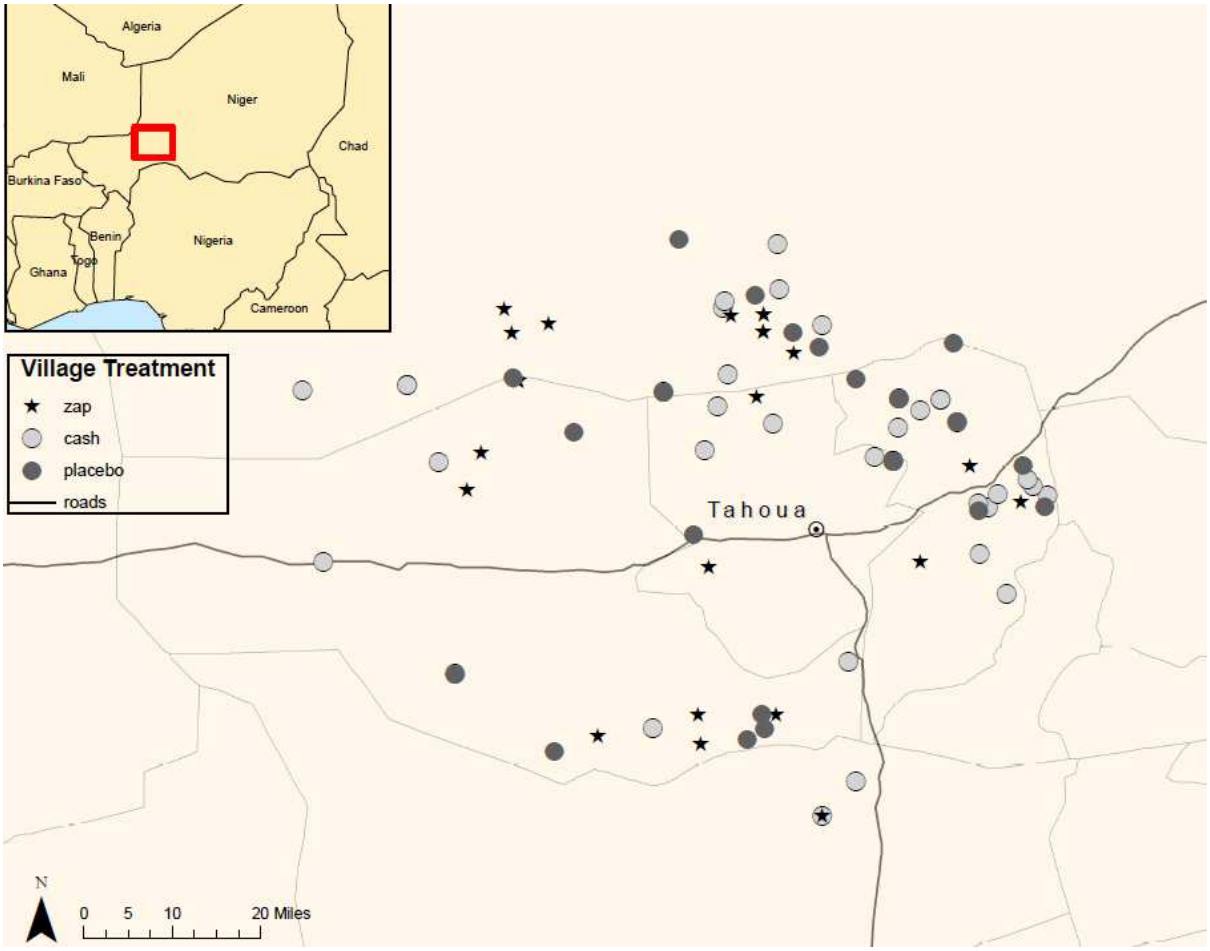
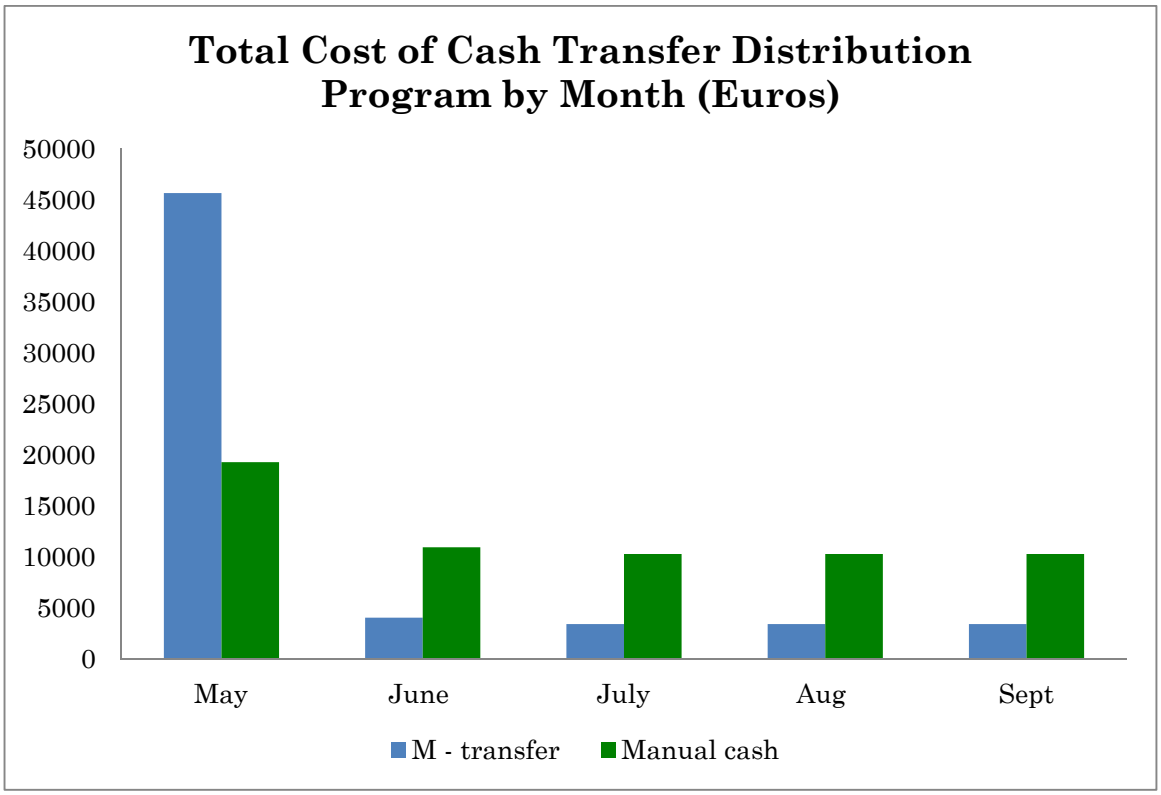


Figure 2. Timeline of Data Collection and Project Implementation

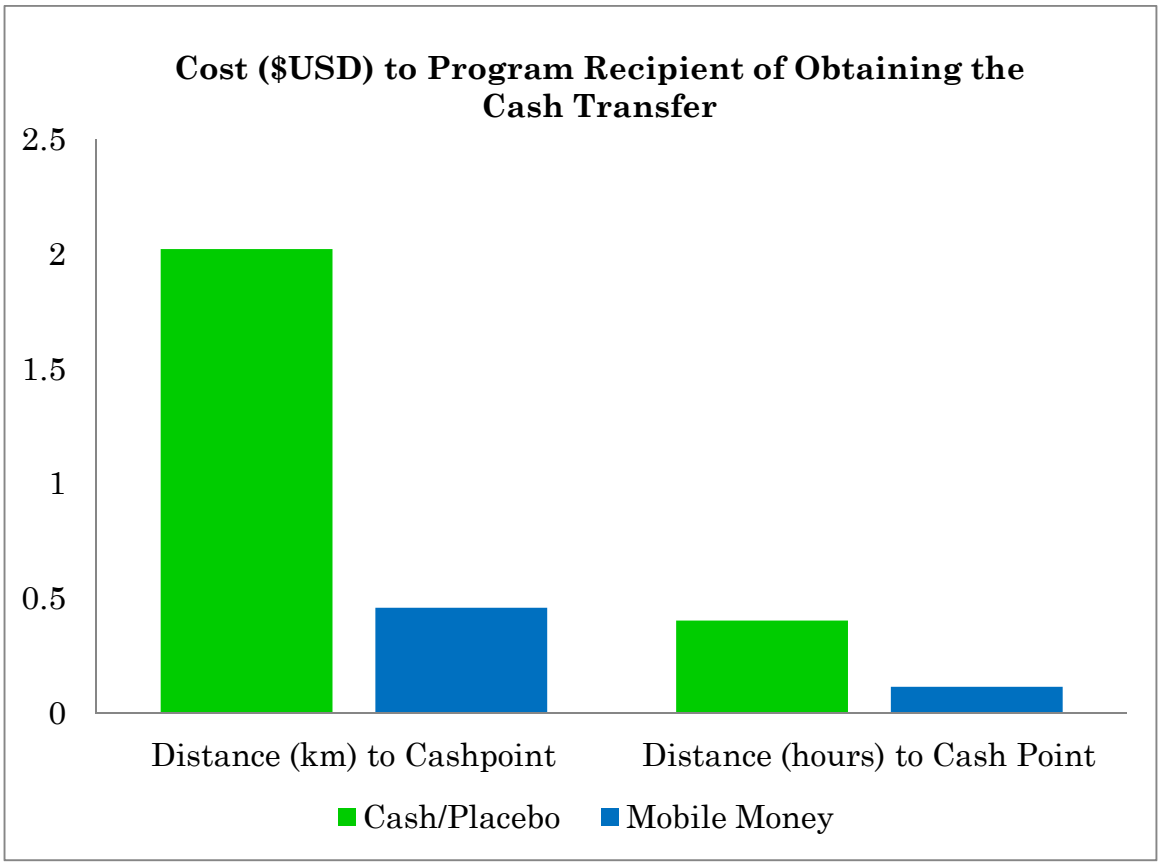
<table>
<tr><th>Year</th><th>January</th><th>March</th><th>April</th><th>May</th><th>June</th><th>July</th><th>August</th><th>September</th><th>October</th><th>November</th><th>December</th></tr>
<tr><td rowspan="2">2010</td><td rowspan="2">Village selection</td><td rowspan="2">Identification and selection of program recipients</td><td rowspan="2">Program preparation</td><td rowspan="2">Baseline household and village-level survey in 96 treatment villages</td><td colspan="4">Hungry season
Monthly cash transfers in 116 villages</td><td colspan="2">Harvest period</td><td>Midterm household and village-level survey in 96 treatment villages</td></tr>
<tr><td colspan="7">Price data collected in 45 markets in all communes</td></tr>
<tr><td>2011</td><td></td><td></td><td></td><td>Final household and village-level survey in 96 treatment villages and 25 control villages</td><td colspan="4">Hungry season</td><td colspan="2">Harvest period</td><td></td></tr>
</table>

Figure 3. Impact of the Cash Transfer Delivery Mechanism on Distribution Costs ($USD per program recipient)



Notes: This figure shows the total costs (in USD) for the manual cash and m-transfer mechanisms for each month of the program. For the m-transfer mechanism, costs include the fixed costs of purchasing the mobile phones, training program recipients in how to use the mobile phones and the variable costs associated with the monthly distribution. For the manual cash mechanism, costs include primarily variable costs associated with manually counting the cash, transport, security and staff time.

Figure 4. Impact of the M-Money Program on Program Recipients' Costs of Obtaining the Cash Transfer



Notes: This figure calculates the cost in program recipients distance and hours to the nearest cash point for each cash distribution. Data were obtained from the household surveys and Concern Worldwide's list of distribution points for the manual cash villages.

Table 1: Baseline Individual and Household Covariates (by Program Status)

	Cash Mean (s.d.)	**Zap-Cash** Coeff (s.e.)	**Placebo-Cash** Coeff (s.e.)	**Zap-Placebo** Coeff (s.e.)
Panel A: Socio-Demographic Characteristics				
Age of respondent	33.73	1.78	-1.17	2.95*
	(11.12)	(1.60)	(1.46)	(1.59)
Respondent is household head	0.13	0.05	0.01	0.04
	(0.34)	(0.03)	(0.04)	(0.03)
Polygamous household	0.28	0.02	0.00	0.02
	(0.45)	(0.04)	(0.04)	(0.04)
Respondent is member of Hausa ethnic group	0.83	-0.06	0.08	-0.14*
	(0.38)	(0.08)	(0.07)	(0.08)
Number of household members	9.34	-0.64	-0.40	-0.24
	(4.92)	(0.62)	(0.46)	(0.56)
Number of household members over 15	3.53	0.07	-0.05	0.12
	(2.09)	(0.25)	(0.19)	(0.24)
Average years of household education	0.15	-0.08**	-0.03	-0.05*
	(0.36)	-0.03	(0.03)	(0.03)
Panel B: Household Income Sources and Assets				
Agriculture is an income source	0.97	-0.02	-0.01	-0.01
	(0.16)	(0.02)	(0.02)	(0.02)
Livestock is an income source	0.62	0.01	-0.08	0.09
	(0.48)	(0.05)	(0.06)	(0.06)
Remittances are an income source	0.35	-0.03	-0.04	0.01
	(0.48)	(0.04)	(0.04)	(0.04)
Number of income source categories	2.46	-0.03	-0.19	0.16
	(1.07)	(0.10)	(0.11)	(0.10)
Number of asset categories	3.59	-0.04	-0.18	0.14
	(1.56)	(0.17)	(0.17)	(0.17)
Panel C: Mobile Phone Ownership and Usage				
Household owns mobile phone	0.29	-0.01	-0.06	0.05
	(0.45)	(0.04)	(0.05)	(0.05)
Respondent is owner of mobile phone	0.25	-0.05	-0.07	0.00
	(0.43)	(0.07)	(0.06)	(0.00)
Respondent has used mobile phone since last harvest	0.63	-0.02	-0.05	0.03
	(0.48)	(0.05)	(0.05)	(0.05)
Used phone to make call since last harvest	0.29	-0.07*	-0.06	-0.01
	(0.45)	(0.04)	(0.05)	(0.05)
Used phone to send or receive m-money transfer	0.01	0.01	0.01	0.01
	(0.00)	(0.01)	(0.01)	(0.01)
Panel D: Shocks				

Household experienced drought in past year	0.99	-0.00	0.01	-0.01
	(0.12)	(0.01)	(0.01)	(0.01)
Household experienced crickets in past year	0.82	-0.06	-0.07	0.01
	(0.27)	(0.05)	(0.05)	(0.05)
Panel E: Village-Level Covariates				
Market located within the village	0.25	-0.04	-0.11	0.08
	(0.44)	(0.10)	(0.09)	(0.09)
Village population (number of households)	177.82	-21.02**	-1.44	-19.58**
	(132.00)	(8.24)	(9.84)	(7.62)

Notes: This table presents a comparison of individual and household covariates in each of the different treatment areas. Column 1 shows the mean and s.d. of the basic treatment (cash) households, whereas Columns 2 and 3 show the average difference between the different treatments and the cash households. Column 4 shows the average difference between the zap and placebo treatment households. Heteroskedasticity-consistent s.e. are clustered at the village level(for Panels A-C) are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 2: Baseline Individual and Household Outcomes (by Program Status)

	Cash Mean (s.d.)	**Zap-Cash** Coeff (s.e.)	**Placebo-Cash** Coeff (s.e.)	**Zap-Placebo** Coeff (s.e.)
Panel A: Food Security Outcomes and Coping Strategies				
Number of months of household food provisioning (scale of 6)	1.9	0.12	0.03	0.08
	(1.56)	(0.16)	(0.17)	(0.16)
Household diet diversity index (scale of 12)	3.07	-0.10	-0.31	0.21
	(2.04)	(0.21)	(0.19)	(0.21)
Reduced meals	0.46	0.06	0.10	-0.04
	(0.50)	(0.09)	(0.08)	-0.08
Panel B: Migration and Remittances				
One household member migrated since the last harvest	0.49	0.01	0.01	-0.01
	(0.50)	(0.06)	(0.05)	(0.05)
Number of household members who migrated since last harvest	0.64	0.05	0.06	-0.01
	(0.80)	(0.10)	(0.08)	(0.10)
Number of remittances received	2.11	-0.28	-0.18	-0.09
	(1.27)	-0.17	(0.22)	(0.22)
Received remittance via m-money transfer (zap)	0.05	-0.00	0.01	-0.02
	(0.21)	(0.03)	(0.03)	(0.03)
Panel C: Agricultural Production and Livestock				
Cultivate land	0.98	-0.01	-0.00	-0.01
	(0.13)	(0.01)	(0.01)	(0.01)
Produce millet	0.97	-0.00	0.01	-0.02
	(0.18)	(0.01)	(0.01)	(0.01)
Quantity of millet produced (kg)	269	4.20	-35.42	39.63
	(354)	(48.70)	(46.00)	(44.66)
Produce cowpea	0.87	-0.00	-0.02	0.02
	(0.34)	(0.04)	(0.04)	(0.04)
Quantity of cowpea produced (kg)	10.81	2.03	0.48	1.55
	(32)	(2.87)	(3.19)	(3.51)
Sold millet	0.04	-0.00	0.00	-0.01
	(0.19)	(0.02)	(0.02)	(0.02)
Sold cowpea	0.00	0.02**	0.01*	0.01
	(0.00)	(0.01)	(0.00)	(0.01)

Notes: This table presents a pre-treatment comparison of individual and household outcomes in each of the different treatment areas. Column 1 shows the mean and s.d. of the basic treatment (cash) households, whereas Columns 2 and 3 show the average difference between the different treatments and the cash households. Column 4 shows the average difference between the zap and placebo treatment households. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 3: Uses of the Cash Transfer

	(1) **Cash average**	(2) **Zap-Cash**	(3) **Placebo-Cash**	(4) **Zap-Placebo**
Panel A: Uses of Cash Transfer for Food Items	Mean (s.d.)	Coeff(s.e.)	Coeff(s.e.)	Coeff(s.e.)
Number of food and non-food items purchased with cash transfer	4.39	0.78***	-0.06	0.84***
	(2.45)	(0.24)	(0.25)	(0.25)
Transfer used to buy staple grains (millet, sorghum)	0.99	-0.01	-0.00	-0.00
	(0.05)	(0.01)	(0.01)	(0.01)
Transfer used to buy other grains (corn, rice)	0.58	0.20***	-0.02	0.22***
	(0.50)	(0.05)	(0.05)	(0.06)
Transfer used to buy cowpea	0.42	0.09*	-0.02	0.11**
	(0.49)	(0.05)	(0.05)	(0.05)
Transfer used to buy condiments	0.7	0.13***	0.00	0.13***
	(0.46)	(0.05)	(0.04)	(0.05)
Transfer used to buy oil	0.7	0.16***	0.00	0.16***
	(0.46)	(0.05)	(0.05)	(0.05)
Transfer used to buy meat	0.4	0.15***	-0.02	0.17***
	(0.49)	(0.04)	(0.04)	(0.05)
Panel B: Uses of Cash Transfer for Non-Food Items				
Transfer used to pay school fees	.07	-0.00	-0.01	0.01
	(0.25)	(0.02)	(0.02)	(0.02)
Transfer used to pay health expenses	.29	0.02	-0.02	0.05
	(0.45)	(0.03)	(0.04)	(0.04)
Transfer used to buy clothes	.04	0.01	0.02	-0.01
	(0.19)	(0.01)	(0.02)	(0.02)
Observations		1104	1104	1104

Notes: This table presents a simple difference comparison of households in each of the different treatment areas using the December 2010 data. Column 1 shows the mean and s.d. of the basic treatment (cash and placebo) households, whereas Column 2 shows the difference between the zap treatment and the cash/placebo treatment. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 4: Impact on Food Security, Coping Strategies and Assets

	(1)	(2)	(3)	(4)
	Cash average	**Zap-Cash**	**Placebo-Cash**	**Zap-Placebo**
	Mean	Coeff	Coeff	Coeff
	(s.d.)	(s.e.)	(s.e.)	(s.e.)
Panel A: Food Security				
Household diet diversity score (out of 12)	3.14	0.31**	-0.19	0.50***
	(1.69)	(0.16)	(0.13)	(0.14)
Consumption of:				
Grains	0.98	-0.01	-0.01	-0.01
	(0.11)	(0.01)	(0.01)	(0.01)
Beans	0.17	0.07**	0.01	0.06**
	(0.38)	(0.03)	(0.02)	(0.03)
Fats	0.28	0.09***	-0.02	0.11***
	(0.45)	(0.03)	(0.03)	(0.03)
Meat	0.06	0.02	-0.00	0.03*
	(0.24)	(0.02)	(0.02)	(0.01)
Condiments	0.36	-0.00	-0.07*	0.07*
	(0.48)	(0.05)	(0.04)	(0.04)
Fruit	0.03	0.02*	-0.01	0.03***
	(0.16)	(0.01)	(0.01)	(0.01)
Number of observations	697	2124	2124	2124
Panel B: Durable and Non-Durable Goods				
Number of asset categories owned (out of 12)	3.28	0.64***	0.15	0.50***
	(1.45)	(0.14)	(0.13)	(0.12)
Durable assets	0.16	-0.00	-0.05*	0.05
	(0.45)	(0.04)	(0.03)	(0.03)
Non-durable assets	1.61	0.12*	-0.07	0.19**
	(0.88)	(0.07)	(0.08)	(0.07)
Number of observations		2203	2203	2203

Notes: This table presents the simple difference estimates for each of the different treatment areas. Column 1 shows the mean and s.d. of the basic treatment (cash) households in the pre-treatment period, whereas Columns 2 and 3 show the difference in means between the different treatments and the cash households. Column 4 shows the difference in means for zap and placebo treatments. Column 5 compares the zap treatment with the joint placebo/cash treatment. All regressions control for commune-level fixed effects. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 5: Impact on Agricultural Outcomes

	(1) **Cash average** Mean (s.d.)	(2) **Zap-Cash** Coeff (s.e.)	(3) **Placebo-Cash** Coeff (s.e.)	(4) **Zap-Placebo** Coeff (s.e.)
Panel A: Agricultural Production				
Cultivated in past growing season	0.98	0.01	0.01	0.01
	(0.13)	(0.01)	(0.01)	(0.01)
Types of crops grown	3.66	0.48***	0.21	0.27
	(1.65)	(0.18)	(0.20)	(0.20)
Grow staple grains (millet or sorghum)	1.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Grow cowpea	0.89	0.04	-0.01	0.05*
	(0.30)	(0.03)	(0.03)	(0.03)
Grow vouandzou or gombo	0.53	0.18***	0.09*	0.09*
	(0.50)	(0.05)	(0.05)	(0.05)
Quantity cowpea produced (kg)	21	1.97	0.83	1.14
	(38)	(4.65)	(4.31)	(6.58)
Quantity vouandzou and gombo produced	4.38	-0.56	1.19	-1.75
	(11.51)	(2.36)	(3.35)	(4.13)
Panel B: Agricultural Marketing				
Sell staple grains	0.15	0.04	0.01	0.03
	(0.36)	(0.04)	(0.04)	(0.04)
Sell cowpea	0.01	0.02	0.02**	-0.01
	(0.00)	(0.01)	(0.01)	(0.02)
Sell vouandzou or gombo	0.01	0.04	0.08	-0.04
	(0.10)	(0.03)	(0.05)	(0.05)
Number of observations		1,079	1,079	1,079

Notes: This table presents the simple difference estimates for each of the different treatment areas. Column 1 shows the mean and s.d. of the basic treatment (cash) households in the pre-treatment period, whereas Columns 2 and 3 show the difference in means between the different treatments and the cash households. Column 4 shows the difference in means for zap and placebo treatments. Column 5 compares the zap treatment with the joint placebo/cash treatment. All regressions control for commune-level fixed effects. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 6: Alternative Explanations

	(1) **Cash average**	(2) **Zap-Cash**	(3) **Placebo-Cash**	(4) **Zap-Placebo**
	Mean (s.d.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)
Panel A: Household-Level Outcomes				
Beneficiary received cash transfer	0.97	-0.02	0.01	-0.03
	(0.16)	(0.02)	(0.01)	(0.03)
Respondent was present for follow-up survey	0.94	-0.01	0.03	-0.04
	(0.23)	(0.05)	(0.02)	(0.05)
Household was affected by drought in 2010/2011	0.66	0.02	-0.03	0.05
	(0.48)	(0.07)	(0.06)	(0.08)
Household was affected by illness in 2010/2011	0.74	-0.09	-0.05	-0.04
	(0.44)	(0.06)	(0.05)	(0.06)
Panel B: Village-Level Outcomes				
Zap agent in village	0.03	-0.03	0.04	-0.06
	(0.18)	(0.03)	(0.06)	(0.05)
Number of Zap agents in village	0.07	-0.07	-0.00	-0.06
	(0.35)	(0.07)	(0.08)	(0.04)

Notes: This table presents the difference in difference estimates for each of the different treatment areas. Column 1 shows the mean and s.d. of the basic treatment (cash) households in the pre-treatment period, whereas Columns 2 and 3 show the DD estimator between the different treatments and the cash households. Column 4 shows the DD estimator for zap and placebo treatments. Column 5 compares the zap treatment with the joint placebo/cash treatment. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 7: Impact of Cash Transfers on Agricultural Prices

Dependent variable: Log of prices	(1) **Millet (tia)**	(2) **Millet (100-kg bag)**	(3) **Sorghum (tia)**	(4) **Cowpea (tia)**	(5) **Cowpea (60-kg bag)**	(6) **Rice (1 kg)**	(7) **Rice (50-kg)**	(8) **Vegetable Oil (1 liter)**
	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)
Panel A: Impact of Any Cash Transfer								
Any cash transfer	-0.00	0.00	0.01	-0.00	0.02*	0.01	-0.00	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Panel B: Impact by Cash Transfer Mechanism								
Zap	0.00	0.01	0.03	0.00	0.01	0.03**	0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Cash/Placebo	-0.01	0.00	-0.01	-0.01	0.03*	-0.01	-0.01	-0.01
	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
H0: Zap-Cash/Placebo								
P-value of F-statistic	0.76	0.30	0.35	0.70	0.16	0.03	0.28	0.29
Mean Price (CFA) of Non-Cash Transfer Markets	516	20,927	466	794	15923	435	19,376	1033
Number of observations	476	412	370	469	343	495	427	459

Notes: Each row shows a separate regression. "Any cash transfer" is a binary variable equal to 1 at time t if there was a cash transfer during that week, 0 otherwise. "Zap transfer" is equal to 1 in week t if there was a zap transfer during that week, 0 otherwise. Column 1 shows the mean and s.d. of the agricultural commodity over the period, whereas Column 2 shows the impact of any transfer on prices. Columns 3 and 4 shows the coefficients for the zap and cash transfers, respectively. Heteroskedasticity-consistent s.e. clustered at the market-level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 8: Location, Knowledge and Timing of Cash Transfer Expenses

	(1) Cash average	(2) Zap-Cash	(3) Placebo-Cash	(4) Zap-Placebo
Panel A: Location and Timing of Expenditures				
Spent transfer at kiosk in village	0.43	0.03	-0.05	0.09
	(0.49)	(0.06)	(0.05)	(0.06)
Spent transfer at market within village	0.22	0.06	0.06	-0.00
	(0.42)	(0.08)	(0.07)	(0.08)
Spent transfer at market outside village	0.61	-0.04	-0.05	0.01
	(0.49)	(0.08)	(0.07)	(0.08)
Obtained transfer the same day	0.98	-0.76***	-.018	-0.74***
	(0.15)	(.044)	(.014)	(.043)
Spent money all at once	0.61	-0.04	-0.04	-0.00
	(0.50)	(0.04)	(0.05)	(0.04)
Panel B: Knowledge of Cash Transfer				
Knew amount of cash transfer	0.27	0.03	0.03	-0.00
	(0.44)	(0.06)	(0.05)	(0.06)
Knew duration of cash transfer	0.26	-0.01	-0.08	0.07
	(0.44)	(0.05)	(0.05)	(0.06)

Notes: This table presents the simple difference estimates for each of the different treatment areas. Column 1 shows the mean and s.d. of the basic treatment (cash) households in the pre-treatment period, whereas Columns 2 and 3 show the difference in means between the different treatments and the cash households. Column 4 shows the difference in means for zap and placebo treatments. Column 5 compares the zap treatment with the joint placebo/cash treatment. All regressions control for commune-level fixed effects. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 9. M-Money and Private Transfers

	(1) **Cash average**	(2) **Zap-Cash**	(3) **Placebo-Cash**	(4) **Zap-Placebo**
	Mean (s.d.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)
Panel A: Full Sample				
Received remittances as income	0.23	0.06	0.00	0.05
	(0.41)	(0.04)	(0.04)	(0.03)
Amount of remittances received for last transfer (CFA)	21156	-537.89	-1,324.95	787.07
	(14618)	(2,516.85)	(2,393.23)	(1,761.78)
Number of remittances since last harvest	2.47	0.28	-0.41	0.69
	(4.94)	(0.65)	(0.53)	(0.45)
Received remittance via Western Union	0.38	-0.05	-0.08	0.03
	(0.48)	(0.07)	(0.05)	(0.06)
Received remittance via friend	0.53	0.04	0.08	-0.04
	(0.50)	(0.08)	(0.06)	(0.07)
Received remittance via Zap	0.01	0.00	-0.00	0.01
	(0.10)	(0.01)	(0.00)	(0.01)
Transferred credit via Zap	0	0.01	0.01	-0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Panel B: Households affected by drought				
Received remittances as income	0.18	0.07	0.05	0.02
	(0.39)	(0.05)	(0.05)	(0.05)
Amount of remittances received for last transfer (CFA)	18887.00	1,854.68	1,348.90	505.78
	(12527)	(2,646.07)	(2,521.07)	(2,234.02)
Number of remittances since last harvest	2.55	0.38	-1.21	1.59**
	(6.48)	(1.02)	(0.79)	(0.66)
Received remittance via Western Union	0.32	0.01	-0.01	0.02
	(0.48)	(0.09)	(0.07)	(0.08)
Received remittance via friend	0.61	0.00	0.06	-0.06
	(0.50)	(0.11)	(0.09)	(0.09)
Received remittance via Zap	0.02	-0.02	-0.01	-0.00
	(0.16)	(0.01)	(0.01)	(0.00)
Transferred credit via Zap	0	0.01	0.01	-0.00
	(0.00)	(0.01)	(0.01)	(0.01)

Notes: This table presents the simple difference estimates for each of the different treatment areas. Column 1 shows the mean and s.d. of the basic treatment (cash) households in the pre-treatment period, whereas Columns 2 and 3 show the difference in means between the different treatments and the cash households. Column 4 shows the difference in means for zap and placebo treatments. Column 5 compares the zap treatment with the joint placebo/cash treatment. All regressions control for commune-level fixed effects. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 10: Impact of Mobile Money on Mobile Phone Ownership and Usage

	(1) **Cash average**	(2) **Zap-Cash**	(3) **Placebo-Cash**	(4) **Zap-Placebo**
	Mean (s.d.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)
Panel A: Mobile Phone Ownership				
Respondent owns a mobile phone	0.25	0.71***	0.53***	0.18**
		(0.07)	(0.08)	(0.09)
Used mobile phone since last harvest	0.63	0.31***	0.13**	0.18***
		(0.05)	(0.05)	(0.05)
Made calls	0.29	0.33***	0.21***	0.12*
		(0.06)	(0.06)	(0.06)
Wrote or received SMS	0.01	0.01*	0.008	0.003
		(0.01)	(0.01)	(0.01)
Sent or received a "beep"	0.06	0.14***	0.04*	0.09***
		(0.03)	(0.03)	(0.03)
Transferred credit via Zap	0.00	-0.00	-0.00	-0.00
		(0.01)	(0.01)	(0.01)
Received credit via Zap	0.01	0.97***	0.01	0.95***
		(0.01)	(0.02)	(0.01)
Panel B: Uses of Mobile Phones				
Communicate with family/friends inside Niger	0.24	0.18***	0.13**	0.04
		(0.06)	(0.05)	(0.06)
Communicate with commercial contacts inside Niger	0.00	-0.00	0.01	-0.01
		(0.01)	(0.01)	(0.01)
Communicate with family/friends outside Niger	0.46	0.01	0.03	-0.02
		(0.07)	(0.07)	(0.07)
Used mobile phone to Communicate death/ceremony	0.27	0.16***	0.15***	0.00
		(0.05)	(0.05)	(0.05)
Used mobile phone to share general information	0.59	0.03	0.07	-0.04
		(0.06)	(0.06)	(0.07)
Used mobile phone to ask for help/support	0.27	0.08	0.07	0.01
		(0.05)	(0.05)	(0.05)

Notes: This table presents the simple difference estimates for each of the different treatment areas. Column 1 shows the mean and s.d. of the basic treatment (cash) households in the pre-treatment period, whereas Columns 2 and 3 show the difference in means between the different treatments and the cash households. Column 4 shows the difference in means for zap and placebo treatments. Column 5 compares the zap treatment with the joint placebo/cash treatment. All regressions control for commune-level fixed effects. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 11: Impact of Mobile Money on Inter-Household Sharing

	(1) **Cash average**	(2) **Zap-Cash**	(3) **Placebo-Cash**	(4) **Zap-Placebo**
	Mean	Coeff	Coeff	Coeff
	(s.d.)	(s.e.)	(s.e.)	(s.e.)
Shared cash transfer	0.18	0.05	0.02	0.03
	(0.38)	(0.04)	(0.04)	(0.04)
Shared cash transfer with friend or family within village	0.91	0.01	0.03	-0.02
	(0.30)	(0.05)	(0.06)	(0.06)
Shared cash transfer with friend or family outside of village	0.00	0.02*	0.00	0.01*
	0	(0.01)	(0.00)	(0.01)
Shared goods from cash transfer	0.60	-0.01	-0.03	0.02
	(0.30)	(0.04)	(0.04)	(0.05)

Notes: This table presents the simple difference estimates for each of the different treatment areas. Column 1 shows the mean and s.d. of the basic treatment (cash) households in the pre-treatment period, whereas Columns 2 and 3 show the difference in means between the different treatments and the cash households. Column 4 shows the difference in means for zap and placebo treatments. Column 5 compares the zap treatment with the joint placebo/cash treatment. All regressions control for commune-level fixed effects. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 12a: Intra-Household Decision-Making

	(1) **Cash average** Mean	(2) **Zap-Cash**	(3) **Placebo-Cash** Coeff	(4) **Zap-Placebo**
Panel A: Decision-Making Regarding Cash Transfer	(s.d.)	Coeff(s.e.)	(s.e.)	Coeff(s.e.)
Respondent responsible for spending cash transfer	0.53	-0.01	-0.02	0.01
	(0.49)	(0.03)	(0.04)	(0.04)
Respondent involved in decision-making of transfer	0.99	0.01	0.01	0.00
	(0.10)	(0.01)	(0.01)	(0.01)
Panel B: Decision-Making on Other Household Issues				
Children's education (=1 if husband decides alone)	0.22	-0.06*	-0.04	-0.03
	(0.42)	(0.03)	(0.03)	(0.03)
Agricultural crops (=1 if husband decides alone)	0.45	-0.08*	-0.09**	0.01
	(0.50)	(0.04)	(0.04)	(0.04)
Transfers to other households (=1 if husband decides alone)	0.44	-0.09**	-0.08*	-0.01
	(0.50)	(0.04)	(0.04)	(0.04)
Panel C: Clothing Expenditures for Muslim Festivals				
Ratio of Children's to Men's Clothing Expenditures (=1 if >=1)	0.46	0.03	-0.05	0.07
	(0.50)	(0.06)	(0.06)	(0.06)
Ratio of Women's to Men's Clothing Expenditures (=1 if >=1)	0.52	-0.01	-0.02	0.02
	(0.50)	(0.06)	(0.07)	(0.07)

Notes: This table presents the simple difference estimates for each of the different treatment areas. Column 1 shows the mean and s.d. of the basic treatment (cash) households in the pre-treatment period, whereas Columns 2 and 3 show the difference in means between the different treatments and the cash households. Column 4 shows the difference in means for zap and placebo treatments. Column 5 compares the zap treatment with the joint placebo/cash treatment. All regressions control for commune-level fixed effects. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 12b: Intra-Household Decision-Making: Heterogeneous Effects

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	**Uses of cash transfer** Coeff (s.e.)	**Purchased Condiments** Coeff (s.e.)	**Purchased Oil** Coeff (s.e.)	**Inter-Household Transfer** Coeff (s.e.)	**Child/Male Clothing Expenses** Coeff (s.e.)
Panel A: Decision-Making by Male-Headed Households					
Zap*Male-headed household	0.48	0.14*	0.18**		
	(0.51)	(0.08)	(0.09)		
Cash*Male-headed household	0.43	0.17*	0.22**		
	(0.57)	(0.10)	(0.11)		
Zap	0.42	0.01	-0.01		
	(0.47)	(0.08)	(0.08)		
Cash	-0.34	-0.14	-0.19*		
	(0.56)	(0.10)	(0.11)		
Male-headed household	0.50	-0.06	-0.08		
	(0.38)	(0.06)	(0.07)		
Number of observations	1,029	1,024	1,024		
R-squared	0.16	0.12	0.15		
H_0: Zap*Male-Headed-Cash*Male-Headed					
P-value of F-Test:	.61	0.72	0.69		
Panel B: Decision-Making by Ethnicity					
Zap*Hausa household	0.18	-0.00	0.02	-0.12	0.41
	(0.64)	(0.10)	(0.10)	(0.11)	(0.99)
Cash*Hausa household	-0.26	-0.15	-0.13	0.10	-0.96
	(0.61)	(0.10)	(0.09)	(0.10)	(1.36)
Zap	0.70	0.13	0.14	0.08	-0.59
	(0.60)	(0.09)	(0.09)	(0.09)	(0.98)
Cash	0.31	0.14	0.11	-0.00	0.72
	(0.58)	(0.09)	(0.08)	(0.09)	(1.35)
Hausa household	0.60	0.11	0.06	0.03	0.44
	(0.50)	(0.07)	(0.08)	(0.08)	(0.93)
Number of observations	1,095	1,018	1,018	890	211
R-squared	0.13	0.12	0.12	0.02	0.05
H_0: Zap*Male-Headed-Cash*Male-Headed					
P-value of F-Test:	0.42	0.12	0.07	0.03	0.17

Notes: Each column represents a separate regression. All regressions control for commune-level fixed effects. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.

Table 13: Impact of the Cash Program

	(1)	(2)	(3)	(4)
Dependent variable	**Weight for height z-score**	**Weight for height z-score**	**Prevalence of wasting**	**Prevalence of wasting**
	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)
Any cash	0.18*		-0.07*	
	(0.10)		(0.04)	
Zap		0.28***		-0.07*
		(0.09)		(0.04)
Placebo		0.17		-0.06
		(0.14)		(0.06)
Cash		0.12		-0.03
		(0.14)		(0.05)
Mean of comparison group	-1.22	-1.22	0.168	0.168
Additional controls	Yes	Yes	Yes	Yes
Number of observations	1,092	1,092	1,092	1,092
R-squared	0.02	0.02	0.01	0.01

Notes: This table presents the regression results using the May 2011 data. "Any cash" is a village that received any cash intervention in 2010, 0 otherwise. Zap, placebo and cash are defined as previously. The comparison group is a village that did not receive any cash intervention in 2010. All regressions control for commune-level fixed effects. Heteroskedasticity-consistent s.e. clustered at the village level are presented in parentheses. Panel A shows the results for non-eligible households, whereas Panels B and C show the regression results for eligible households only. *** significant at the 1 percent level, ** significant at the 5 percent level, * significant at the 10 percent level.