Payment Mechanisms and Anti-Poverty Programs: Evidence from a Mobile Money Cash Transfer Experiment in Niger

Jenny C. Aker, Rachid Boumnijel, Amanda McClelland and Niall Tierney*

July 2015

Abstract: Cash transfers have become an increasingly important component of social protection policies in both developed and developing countries. While such programs are often implemented electronically in developed countries, in many developing countries with weak financial infrastructure, such transfers are distributed manually, resulting in significant costs to program recipients and the public sector alike. The introduction of mobile money systems in many developing countries offers new opportunities for distributing cash transfers. Using data from a randomized experiment of a mobile money cash transfer program in Niger, we find evidence of benefits of this new system: Household and child diet diversity was 9-16 percent higher among households who received mobile transfers and children ate 1/3 more of a meal per day. These results can be partially attributed to time savings associated with mobile transfers, as program recipients spent less time traveling to and waiting for their transfer. They are also associated with shifts in intra-household bargaining power for women. These results suggest that electronic transfers may address key logistical challenges in implementing cash transfer programs in developing countries, but that sufficient investment in the payments infrastructure is needed.

Keywords: Unconditional cash transfers, mobile money, electronic transfers, Africa
JEL codes: O1, H84, I38, O33

*Jenny C. Aker, The Fletcher School and Department of Economics, Tufts University, 160 Packard Avenue, Medford, MA 02155; Jenny.Aker@tufts.edu. Rachid Boumnijel, Care International, Boumnijel@careinternational.org. Amanda McClelland, International Red Cross, amandamcc1@hotmail.com; Niall Tierney, Department of Foreign Affairs and Aid, Government of Ireland, niall_tierney@hotmail.com. We thank Concern Worldwide in Niger for their support in all stages of this project and would especially like to thank Adamou Hamadou and the data collection team in Niger. We greatly appreciate comments from Randall Akee, Steven Block, Pascaline Dupas, Frederico Finan, Alain de Janvry, Kelsey Jack, Robert Jensen, Charles Kenny, Stephan Klasen, Alan Gelb, Jessica Goldberg, Christopher Ksoll, Jake Kendall, Jeremy Magruder, Craig McIntosh, Pau, Julie Schaffner, Gabrielle Smith, Tavneet Suri, Frank-Borge Wietzke and participants at seminars at the Center for Global Development, UC-Berkeley, MWIDC, UC-Davis, UC-Santa Cruz, UC-Irvine, University of Southern California and the University of Gottingen. We are grateful for financial support from the European Commission, Irish Aid, the Hitachi Center and Tufts University. All errors are our own.

.

Conditional and unconditional cash transfers have become an increasingly common component of social protection policies in both developed and developing countries (World Bank 2009, DFID 2011). As of 2013, 119 developing countries had some type of cash transfer program in place (Gentilini et al 2014). In higher and middle-income countries, such programs are often implemented electronically, either via bank transfers or pre-paid debit cards. Yet in developing countries with limited financial infrastructure, cash transfer programs often require physically distributing cash in small denominations to remote rural areas. This can result in substantial costs for both the implementing agency and program recipients, thereby affecting the potential effectiveness of cash transfers as compared with other anti-poverty programs and resulting in hidden costs to program recipients.

The introduction of mobile phone-based money transfer systems (m-transfers or mobile money) in many developing countries offers an alternative infrastructure for delivering such transfers. By transferring money via the mobile phone, mobile money could potentially reduce the costs and leakage associated with social protection programs (Muralidharan et al 2014). In addition, m-transfer systems may prove easier for transfer recipients to collect their transfers, provided that they have easy access to m-transfer agents. Yet beyond its cost-saving potential, m-transfer systems may have broader implications for economic development by increasing access to informal private transfers (Blumenstock, Eagle and Fafchamps 2014, Jack and Suri 2014) or serving as an alternative savings device (Mas and Mayer 2012, Mbiti and Weil 2011).

This potential "win-win" scenario, in which the public sector could lower the costs of implementing anti-poverty programs and the poor could receive other benefits, is attractive for policymakers, donors and implementing agencies alike (Banerjee et al 2013). In 2012, the "Better than Cash" Alliance was formed, advocating for governmental and non-governmental organizations to move to digital payments for payroll, government benefits and humanitarian aid, citing cost savings, transparency and financial inclusion as potential benefits.[1] This has been echoed by a number of other organizations, such as the Gates Foundation, which has made digital payments a key aspect of its financial inclusion strategy.

[1] http://betterthancash.org/about/. The "Better than Cash" Alliance seeks to "empower people through electronic payments."

What is surprising about the calls for a shift to electronic payments is the scarcity of rigorous empirical evidence to support these claims. A priori, it is not clear that electronic transfers will unambiguously improve welfare. For example, much of the proposed cost savings associated with electronic transfers depends upon the existence of a well-functioning electronic distribution system, such as a mobile agent network that allows recipients to "cash out". While over 140 m-money platforms have been deployed in 80 countries worldwide, adoption has remained surprisingly low in most countries, potentially due to weak agent infrastructures (GSMA 2013, Evans and Pirchio 2015).[2] In the absence of this infrastructure, using m-money to distribute cash transfers might actually *increase* costs for recipients who cannot access m-transfer agents or use the technology. Furthermore, electronic transfers could increase the likelihood of leakage if m-transfer agents can more easily extract the transfer from program recipients than public sector agents.

Using a randomized experiment, we examine the effects of using mobile money in delivering a cash transfer program in Niger. In response to a devastating drought, targeted households in 96 villages received a monthly unconditional cash transfer, with women as the primary beneficiary. The first delivery channel provided the cash transfer manually, whereby cash was distributed in individual envelopes (the standard mechanism). The second delivery channel provided the cash transfer electronically, whereby program recipients received the transfer via the m-transfer system, as well as a m-transfer-enabled mobile phone. The third delivery mechanism was the same as the manual cash mechanism, but households also received a m-transfer-enabled mobile phone. As we were unable to collect data from a pure comparison group, our analysis focuses on the relative costs and benefits of different transfer mechanisms.

Overall, our results provide evidence that the m-transfer system had benefits: Households in the m-transfer group used their cash transfer to buy more diverse types of goods and were more likely to purchase protein and energy-rich foods. These diverse uses of the transfer also resulted in a 9-16 percent improvement in diet diversity, primarily due to increased consumption of beans and fats, and children consumed 1/3 more of a meal per day. We do not

[2] Outside of Kenya, the most notable mobile money success story, m-money adoption has remained surprisingly low in other African countries, estimated at less than 10 percent. Yet m-money adoption and agent penetration are even lower in some of the poorest countries, which arguably could benefit the most from this type of technology.

find evidence that m-transfer households reduced their ownership of other durable and non-durable goods, suggesting that other household members were not decreasing their contribution to household goods as a result of the transfer.

These results can be partially explained by less time spent by m-transfer program recipients in obtaining their transfer, as well as increased bargaining power for women. M-transfer program recipients traveled shorter distances to obtain their transfer as compared with their manual cash counterparts.[3] While the magnitude of average time savings was relatively small – approximately 2.5 days over a five-month period – we believe that this is a conservative lower bound on actual time savings. In addition, this savings occurred during a period of year when opportunity costs were high, implying that the time savings could have enabled m-transfer program recipients to engage in other productive activities or invest more time in child care. While we do not have data on the latter channel, the empirical evidence supports the former claim: M-transfer households were more likely to cultivate marginal cash crops that are primarily grown by women.[4]

In addition to the time savings, we provide additional evidence that the m-transfer mechanism affected intra-household decision-making. Program recipients, all of whom were women, reported that the m-transfer was less observable to other household members, thereby allowing them to temporarily conceal the arrival of the transfer. We find that m-transfer program recipients were more likely to travel to weekly markets, be involved in selling household grains and have higher clothing expenditures than those in the other treatment arms. In addition, the improved diet diversity results persisted six months after the program, well after the cash transfer had been spent. The results, taken together, suggest that the m-transfer technology might have shifted women's bargaining power within the household.

Our paper makes two substantive contributions. First, while there has been substantial literature on the costs of transfer programs (Caldes, Coady and Maluccio 2004, Handa and Davis

[3] This result would not be surprising in a context such as Kenya, with over 50,000 mobile money agents, approximately 10 percent of the total number of agents in sub-Saharan Africa. In West Africa, there are fewer than 45 mobile money agents per 1,000 people in countries such as Senegal and Ivory Coast, two countries with the most well developed m-money platforms. This is less than the number of Western Union or MoneyGram points of sale in such countries, which are in direct competition with mobile money.

[4] Unlike Muralidharan et al (2014), we do not find evidence that the m-transfer mechanism had any impacts on leakage.

2006), there is scant literature on the relative benefits and costs of electronic versus manual transfers. More recently, Muralidharan et al (2014) have found that electronic transfers combined with biometric identification resulted in significant cost reductions for beneficiaries, whereas Blumenstock, Callen, and Ghani (2014) found that mobile salary payments significantly reduced firms' costs in areas where adequate mobile network and agent coverage existed. Second, our experiment adds to a strand of literature on the impact of m-transfer systems on household welfare (Jack and Suri 2014, Blumenstock, Eagle and Fafchamps 2014). That literature has primarily focused on the use of m-money for private transfers. Yet both of these strands of literature are unable to disentangle the impact of the technology from the transfer mechanism. In contrast, our experiment exogenously varies access to both the mobile phone handset and the m-transfer technology, thereby allowing us identify different behavioral responses to each.

Combined with these studies, our results suggest that the use of technology for anti-poverty programs can help to address key logistical challenges in implementing such programs. In our context, the m-transfer intervention also improved program performance: It greatly reduced program recipients' costs and generated important benefits as compared with the manual cash transfer, and variable costs were 20 percent lower per program recipient. Yet this system requires the existence of a robust m-transfer agent infrastructure, which is only available in a handful of developing countries, and often not in the poorest countries where such systems could arguably have the greatest impact in reducing transaction costs. In addition, it is not clear that such systems will improve households' financial inclusion or generate longer-term benefits, as its proponents suggest. Compared to the manual cash transfer mechanisms, the initial costs of the m-transfer delivery system were higher, primarily due to the costs of mobile phones.

The rest of the paper proceeds as follows. Section I describes the context and the experimental design. Section II describes the different datasets and estimation strategy. We discuss the results in Section III before discussing mechanisms (Section IV) and alternative explanations (Section V). Section VI concludes.

I. Setting and Research Design

Niger, a landlocked country located in West Africa, is one of the poorest countries in the world. With a per capita GNP of US$360 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 2011). Rainfall ranges from 200 millimeters (mm) per year in the northern regions to 800 mm in the south and is subject to high intra- and inter-annual variability (Nicholson, Some and Kone, 2000). For example, Niger experienced six droughts between 1980 and 2005 (Government of Niger 2007). In 2009/2010, the time period of this study, Niger experienced both drought and harvest failures, with 2.7 million people classified as vulnerable to extreme food insecurity (FEWS NET 2010).

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). Like most m-transfer systems, Zap allowed users to store value in an account accessible by the handset, convert cash in and out of the account and make transfers by using a set of text messages, menu commands, and personal identification numbers (PINs) (Aker and Mbiti 2010). While mobile phone coverage has grown substantially in Niger over the past decade, initial coverage, usage and growth of Zap was limited and geographically focused in the capital city (Niamey) and regional capitals. The cost of making a $USD45 transfer using Zap cost USD$3 during this period.[5]

A. Cash Transfer Delivery Mechanisms

In response to the 2009/2010 drought and food crisis in Niger, an international non-governmental organization, Concern Worldwide, designed a short-term social protection program. The program sought to prevent increases in malnutrition and asset depletion by providing unconditional cash transfers to approximately 10,000 drought-affected households during the "hungry season", the five-month period before the harvest.

The first experimental treatment was the *manual cash* intervention (*Cash*), whereby households received an unconditional cash transfer of 22,000 CFA per month (approximately

[5]In 2010, it cost $1.50 to make a transfer between $20-$40, whereas a transfer greater than $USD 40 cost $3.

$US45) over a five-month period.[6] The total value of the transfer was slightly less than 2/3 of the total annual GDP per capita, larger than cash transfer programs in Latin America and sub-Saharan Africa (Handa and Davis 2006, Garcia and Moore 2012).[7] Payments were made on a monthly basis, with cash counted into individual envelopes and transported via armored vehicles to distribution centers.[8] As is common in these types of programs in Niger, one village was chosen as a distribution point for a group of villages, although Concern tried to ensure that the cash distribution points were as close as possible to each village (Niang et al 2012, Hoddinott et al 2014). Program recipients were informed of the date and location of their cash transfer via a phone call, primarily by contacting a point person within the village the day before or the morning of the transfer, and had to travel to their designated location on that given day to receive the cash transfer. The manual cash transfer system was similar to that of other anti-poverty programs in Niger at the time.

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. In the second experimental treatment (*Zap*), program recipients received their cash transfer via the mobile phone. On the day of the cash transfer, *Zap* program recipients would receive a message with a special "beep" on their mobile phone, informing them that the transfer had arrived. After receiving this notification, program recipients had to take the mobile phone to an m-transfer agent located in their village or a nearby village to obtain their transfer.[9] 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. As less than 30 percent of

[6]The value of the transfer varied monthly, with three initial transfers of 20,000 CFA ($US 40) and two transfers of 25,000 CFA ($US 50).

[7]Conditional cash transfer programs in Latin America range from 8-25 percent of average per capita annual income (Handa and Davis 2006). The size of unconditional cash transfer programs in sub-Saharan Africa varies considerably, ranging from US$8 per month in Mali to US$37 and US$42 per month in Kenya and Rwanda, respectively (Garcia and Moore 2012). These represent between 20-40 percent of per capita income in those countries. The program was the same magnitude as the government's current safety net program, which provides 10,000 CFA per month over 12 months, although the timing is concentrated during the hungry season.

[8]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 (Demirguc-Kunt and Klapper 2012).

[9]While the transfer fee and the first withdrawal fee were paid by Concern, program recipients would have had to pay the "cash out" (withdrawal) fee for any additional withdrawals. This would have cost approximately $US .25 for each withdrawal.

households in the region owned mobile phones prior to the program, Concern also provided program recipients with mobile phones equipped with a m-money account and training on how to use the technology. In addition, as Zap was introduced into Niger a few months' prior to the intervention, there were a limited number of Zap agents in rural areas. Concern therefore encouraged the mobile phone operator to register m-money agents within the program area, but did not have any control over the location or density of those agents. The second intervention thereby differs from the *Cash* intervention with respect to the transfer delivery mechanism, as well as the provision of the handset and the m-transfer technology.

In an effort to disentangle the impact of electronic delivery mechanism from that of the mobile phone, we also implemented a third experimental treatment (*Mobile*). The *Mobile* intervention mirrored the manual cash intervention, but program recipients also received a mobile money-enabled mobile phone and training on how to use it.

As these treatments differ in the type of delivery mechanism and technology provided (m-transfer or a mobile phone), comparing outcomes across the different treatments will allow us to estimate the additional costs and benefits of using the m-transfer technology in the context of a social protection program. In particular, comparing outcomes between the *Mobile* and *Cash* groups allows us to measure the additional effect of mobile phone ownership, whereas comparing outcomes between the *Zap* and *Mobile* interventions allows us to estimate the additional effect of the m-transfer delivery mechanism. [10]

Due to the humanitarian nature of the intervention and the political situation at the time, we were unable to collect data from a pure comparison group. Hence, while we can estimate the causal effect of the m-transfer mechanism as compared with the manual cash mechanism, we cannot estimate the causal impact of the cash transfer program.

B. Experimental Design

Prior to the intervention, Concern Worldwide identified 116 intervention villages in one region of Niger. Eligible villages were those classified by the Government of Niger as having

[10]Concern also implemented a seed distribution program in one-third of targeted villages, whereby recipient households could replace two of their cash transfer payments with the equivalent value in seeds (also provided by Concern). There is not a statistically significant difference in the presence of a seed distribution program across treatments.

produced less than 50 percent of their consumption needs during the 2009 harvest.[11] Of these, some villages were prioritized for either the *Cash* or *Zap* intervention based upon their lack of mobile phone coverage (*Cash*) and proximity to the Niger-Mali border (*Zap*), thereby reducing our sample size to 96 eligible villages. Among these villages, we first stratified by administrative division (commune) and then randomly assigned villages to the *Cash*, *Mobile* or *Zap* interventions. In all, 32 villages were assigned to the *Cash* group, 32 to the *Mobile* group and 32 to the *Zap* group.[12]

Prior to program assignment, eligible households within each village were identified by a village-level vulnerability exercise. Using indicators such as livestock ownership, landholdings and the number of household members, households were classified into four vulnerability categories (A, B, C and D), with C and D as the most vulnerable categories. Households from the C and D categories were selected for the program. 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 treatments, the cash transfer (as well as the mobile phone and training in the *Zap* and *Mobile* treatments) was provided to the woman within the household.[13] The study timeline is presented in Figure 1.

C. Why should m-transfers matter?

We expect that the m-transfer delivery mechanism might affect household outcomes through different channels. First, if the m-transfer mechanism reduces program recipients' costs involved in obtaining the transfer or their uncertainty with respect to these costs, then this could reduce program recipients' opportunity costs during a time of year when such costs were relatively higher. Alternatively, if the new technology makes it more difficult for program recipients to access their cash – either due to the limited number of m-transfer agents, difficulty

[11]To calculate a food "deficit", the Government of Niger estimated village-level millet production and compared this with estimated consumption "needs", defined as 190 kg of millet/capita/year. A village that produced less than 50 percent of its estimated consumption needs was considered to be food deficit, and was therefore eligible for assistance in 2009/2010.

[12]The average distance between villages of different treatments was 48 km, with a minimum distance of 3 km.

[13]Concern Worldwide only distributed the cash transfer in the *Cash* and *Mobile* treatments to the female program recipient (after presenting the beneficiary ID card), unless the program recipient had a disability. The requirement was the same for the *Zap* treatment group: m-money agents were instructed to only "cash out" to the intended program recipient, upon presentation of the mobile phone, PIN number and beneficiary ID card. If there were multiple wives within a household (28% of households), the transfer was provided to the first wife.

in using the technology or charging the phone – this could increase costs for the *Zap* households to obtain the cash transfer program. Furthermore, while we might expect such transfers to reduce leakage (Muralidharan et al 2014), electronic transfers could increase the likelihood of leakage if m-transfer agents can more easily extract the transfer from program recipients.

Second, the m-transfer system could simply change the way in which households spend the cash transfer. For example, if *Zap* program recipients obtain their cash from an agent and kiosk-owner within the village, program recipients might be exposed to different products or prices at the kiosk. The m-transfer technology could encourage program recipients to store some of the transfer on their phone, thus increasing the mental costs associated with unplanned expenditures (Dupas and Robinson 2013).

Third, access to the mobile phone technology could affect households' communication with or transfers from members of their social network. The mobile phone handset could increase households' access to information, thereby allowing them to improve their decision-making with respect to agriculture, migration and consumption. Since program recipients in both the *Zap* and *Mobile* treatments received mobile phones, in theory, this should only be a potential channel if *Zap* households used their handsets in different ways, or if women had greater control of the phone. In particular, having access to the m-transfer technology on the mobile phone could have provided households with an alternative means of receiving informal private transfers, thereby helping households to better cope with risks and shocks (Blumenstock, Eagle and Fafchamps 2014, Jack and Suri 2014).

Finally, since m-transfers reduce the observability of the amount and timing of the cash transfer, this could affect women's bargaining power within the household, thus changing the intra-household allocation of resources (Duflo and Udry 2004, Doepke and Tertilt 2014, Ashraf et al 2014).[14] Reducing the transfer's observability could also affect inter-household sharing, thereby leaving more income available for the household (Jakiela and Ozier 2012).

II. Data and Estimation Strategy

[14]Ashraf et al (2014) provide a voucher for concealable contraceptives either to women alone or jointly with their husbands, and find that women who were privately provided a voucher were more likely to use the contraceptives and have fewer births.

A. Data

This paper uses four primary datasets. The first dataset is a household survey of 1,152 program recipients in 96 intervention villages across three rounds. The primary respondent for the household surveys was the program recipient for participating households. The baseline survey was conducted in May 2010, with follow-up surveys in December 2010 and May 2011. The research team located 94 percent of respondents for the follow-up surveys, and attrition was not differential across the experimental arms either in December 2010 or May 2011 (Table A1). The main sample in this paper therefore consists of those households who were located during the follow-up surveys: 1,082 respondents.[15]

The household survey included modules on household demographics, food security agricultural production and sales, mobile phone usage, asset ownership and shocks. For the follow-up surveys, we also included modules on the uses of the cash transfer. As the surveys were conducted during a humanitarian crisis and over a short time frame, we were mindful of the time burden on respondents. As a result, the household surveys did not include a full income and expenditure module, so we are unable to measure the impact of the program on total household expenditures. Rather, we collected data on proxies for well-being, such as asset accumulation (as a wealth proxy) and food security (including a 24-hour recall for diet diversity).[16]

The second dataset is a village-level survey, collected during the same periods of the household-level surveys. The village surveys collected information from a focus group of male and female village residents on topics such as mobile phone coverage, access to markets and the number of *Zap* agents.

The third dataset includes weekly price information for six products in forty-five markets 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 differential effects of the cash transfer delivery mechanism on local market prices and supply.

---

[15]The number of observations in each table is not always equal to 1,082. If the survey team was not able to find the respondent but found another household member, then the team surveyed the other household member, asking only questions household questions (e.g., assets, production, etc). Second, sections that are related to the *uses of the transfer* (e.g., Tables 3, 5 and 6) are conditional on the household having received a transfer, which totals 1,047 households.

[16]The household diet diversity index is a standard index developed by the Food and Nutrition Technical Assistance (FANTA) for surveys in developing countries. It includes a list of twelve food categories, including cereals, tubers, legumes, milk, fish, meat, oils, condiments, fruits, vegetables and sugar. FANTA 2006.

The final dataset is anthropometric data among children under five collected in May 2011. These data were collected from program recipient households from a randomly chosen subset of intervention villages, for a total sample of 30 villages and 691 households.

B. Pre-Program Balance of Program Recipients

Tables 1 and 2 present the baseline characteristics for the sample. [17] Column 1 presents the sample mean and standard deviation for a series of characteristics. To test for balance across groups, Columns 2–3 present the coefficient estimates and standard errors of the difference between the baseline mean in the *Zap* or *Mobile* treatments and the mean in the *Cash* treatment. Since randomization was done at the village level, the standard errors are clustered at the village-level and stratification fixed effects are included.

A few characteristics are worth mentioning. The average household size was nine, and a majority of respondents were members of the Hausa ethnic group. 28 percent of households were in polygamous marriages. Less than thirty percent of households owned a mobile phone prior to the start of the program, yet 62 percent of respondents had used a mobile phone in the few months prior to the baseline. 97 percent of households had experienced drought in the past year. 36 percent of villages had a weekly market, and 26 percent had a seed distribution program (Panel E).

Turning to key outcome variables (Table 2), household diet diversity was 3 (out of 12 food categories), and households reported having sufficient food for approximately 2 out of the past 6 months. This is unsurprising, as 97 percent of households relied upon agriculture as a primary income source and all of them had been affected by drought.

Looking at the differences across treatments, the randomization appears to have been successful in creating comparable groups along observable dimensions. Differences in pre-program household characteristics and outcomes are small and generally not statistically significant. *Zap* program recipients were older, less likely to be from the Hausa ethnic group and

[17] Due to an administrative error, the survey team only conducted the baseline survey in 93 villages (rather than 96). Thus, the total number of baseline observations should be 1,116, rather than 1,152; however, there are 10 missing baseline observations.

more likely to raise livestock as compared with the *Mobile* group (Panels A-D).[18] Overall, there are 31 dependent variables and 62 coefficients estimates in Tables 1 and 2. Of these, only two coefficients were statistically significant at the 10 percent level and 1 coefficient at the 5 percent level, comparable with what we would expect with random assignment.

C. Estimation Strategy

To estimate the impact of different cash transfer delivery mechanisms on a variety of outcomes, we use a simple reduced form regression specification of the following form:

$$Y_{iv} = \beta_0 + \beta_1 Zap_v + \beta_2 Mobile_v + \mathbf{X}'_{iv0}\gamma + seed_v + \theta_C + \varepsilon_{iv} \quad (1)$$

The variable Y_{iv} represents the outcome of interest (costs, uses of the cash transfer, food security and assets) of individual or household i in village v after the transfer. Zap_v is an indicator variable for whether the village was assigned to the m-transfer program, whereas $Mobile_v$ is an indicator variable for whether the village was assigned to the *Mobile* group. θ_C are geographic fixed effects at the commune level, the level of stratification. We also include a vector of baseline covariates that differed at baseline, $\mathbf{X}'_{iv0}$, such as age. Finally, we control for the presence of a seed distribution program at the village level.[19] 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 correct for heteroskedasticity. The coefficients of interest are β_1 and β_2, the intent-to-treat (ITT) effect of the different transfer mechanisms (as compared with the basic *cash* intervention) on the outcome of interest, under the assumption that they are conditionally orthogonal to ε_i. Most regression specifications presented in this paper use the December 2010 household data, immediately after the transfer. When household data are available for both December 2010 and May 2011, we pool the data and include a linear time trend. We also conduct separate analyses by time period, which allows us to measure the immediate and longer-term effects of the program.

Equation (1) is our preferred specification for most outcomes, as some of the data were not collected during the baseline. For those outcomes where baseline data are available, we also

[18] In this context the difference in ethnicity and livestock-raising are correlated, as the other ethnic groups in these areas (Fulani and Touareg) have traditionally been pastoralists.
[19] All results are robust to excluding the baseline covariates.

use an Analysis of Covariance (ANCOVA) specification, which controls for baseline value of the outcome variable. In cases where an outcome variable has high variability and low autocorrelation, as is the case with our food security measures, the ANCOVA model is preferred over difference-in-differences (McKenzie 2012).

III. Results

A. Uses of the Transfer

As the cash transfer was unconditional, program 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 (18%), school fees (6%), debt reimbursement (7%) and hiring labor (2%). (Respondents could list more than one use of the cash transfer, so the total can exceed 100%.) Consistent with other studies on cash transfers, fewer than 1% of households used the cash transfer to buy "temptation goods", defined in this context as food from the kiosks (such as doughnuts and cookies) and tea (Evans and Popova 2014). Thus, program recipients primarily used the transfer to ensure immediate consumption needs, as well as make limited agricultural investments and avoid asset depletion. These purchasing patterns, especially purchases of bulk grain, are similar to those found in Hoddinott et al (2014) in the context of a cash transfer program Niger.

As we do not have a full expenditure module, we are unable to show the impact of the transfer on total expenditures or the quantities demanded. Nevertheless, we do have data on the uses of the transfer and the amount spent on a subset of food and non-food items. While this constrains our analysis, these outcomes can be insightful, as the transfer represented a significant income shock to recipient households. Given this fact, as well as the fact that households only had two income sources prior to the program and did not receive external aid from other international organizations, it is reasonable to assume that households' marginal propensity to consume was high, and that the uses of the transfer would approximate overall expenditures during this period.

Table 3 shows the different uses of the cash transfer by treatment group, using data from December 2010.[20] Overall, the results paint a picture of more diverse uses of the cash transfer by *Zap* households, primarily for food items. Households in *Zap* villages purchased .78 more types of food and non-food items as compared with the *Cash* group, and .85 more types of items as compared with the *Mobile* group, with a statistically significant difference between each pair. While the likelihood of purchasing staple grains did not differ by the transfer mechanism, program participants in the *Zap* group were 18-20 percentage points more likely to purchase non-staple grains (rice and corn), 9-10 percentage points more likely to purchase cowpea and 11-18 percentage points more likely to purchase condiments, oil and meat as compared with those in the *Cash* and *Mobile* groups (Panel A). Most of these differences are statistically significant at the 1 percent level. Yet there are no statistically significant differences in the uses of the cash transfer with respect to school fees, health expenses and clothing (Panel B). These patterns are similar when restricting the analysis to the last transfer (Table A2).[21]

B. Food Security and Nutritional Status

While the results in Table 3 suggest that *Zap* households used the cash transfer differently, especially with respect to food items, without a full expenditure module, this would not necessarily indicate a net welfare improvement. In particular, *Zap* program recipients' spouses could have contributed fewer public or private goods to the household as a result of the cash transfer, potentially distorting the uses of the transfer or consumption. While we are unable to assess the impact of the program on household expenditures, Table 4 estimates the impact of

[20]While Table 3 shows the differential effects on the extensive margin, Table A3 shows the differential effects of on the intensive margin. Given potential recall error associated with the initial transfers, we collected data on the amount spent *for the last transfer and for specific food categories* (i.e. those that were easily able to quantify, such as grains or oils, rather than vegetables, meat or condiments). As 91% of *Cash* households purchased a wholesale bag of millet with the last transfer (Table A2), this left approximately $US 8 to purchase other items. *Zap* households spent relatively less on the staple grains as compared with both the *Cash* and *Mobile* groups, but spent slightly more on other grains, cowpea and oil as compared with the *Mobile* and *Cash* groups. While these results are broadly consistent with the findings in Table 3, the magnitudes are relatively small, between $.10 and $1.00.

[21] A potential concern with such self-reported measures is that program recipients could simply list the first (or largest) expenditures made after receiving the transfer, which could differ by treatment groups. Thus, we might see a treatment effect on *measured* expenditures rather than *actual* expenditures. This concern is alleviated by the way in which the question was measured; after program recipients cited specific categories, enumerators were instructed to go through a comprehensive list of all potential categories and ask the recipient if they spent the cash transfer on that particular category.

the different treatments on proxies measures of well-being, namely, household food security and nutritional status.[22]

Table 4 (Panel A) shows the intent to treat estimates of the effect of different transfer delivery mechanisms on household food security, using pooled data from December 2010 and May 2011. *Zap* households had diet diversity scores that were .27-.50 points higher than the *Cash* and *Mobile* households, with a statistically significant difference at the 10 and 5 percent levels, respectively. Focusing on particular food groups, *Zap* households were 6 percentage points more likely to consume beans, 9-11 percentage points more likely to consume fats and 3 percentage points more likely to consume fruits than *Cash* and *Mobile* households. All of these differences are statistically significant at the 1 or 5 percent levels. These results are also robust to using an ANCOVA specification and using only the December 2010 or May 2011 data (Table A4 and Figure A1).[23] Overall, these effects represent a 30-percent increase in consumption of beans and fats, particularly important food groups given the high prevalence of protein-energy malnutrition in Niger (DHS 2012). In addition, the increased food consumption categories are broadly correlated with the more diverse purchases observed in Table 3.[24]

Panel B shows the results of the cash transfer delivery mechanism on child nutritional outcomes, using data from May 2011. While children in *Cash* villages ate 2.33 meals per day, children in *Zap* households ate an additional 1/3 of a meal as compared with those in the *Mobile* and *Cash* groups, with a statistically significant difference at the 5 percent level. Similarly, children in *Zap* households had diet diversity scores that were 12-14 percent higher as compared with those in the *Mobile* and *Cash* groups, although this was only statistically significant for one

[22]In this case, the reference period for household diet diversity was the past 24 hours. A more varied diet is associated with a number of improved outcomes in areas such as birthweight (Rao et. al. 2001), child anthropometric status (Allen et. al., 1991; Hatloy, Hallund, Diarra and Oshaug, 2000; Onyango, Koski and Tucker, 1998) and food expenditures (Hoddinott and Yohannes 2002).

[23]The diet diversity results are consistent when using only the December 2010 or the May 2011 data, suggesting that these results hold in the short and longer-term. While diet diversity is higher in December 2010, it drops in May 2011, consistent with the hungry period. *Zap* households have higher diet diversity than both *Cash* and *Mobile* households in December 2010 and May 2011, although results are slightly stronger in May 2011, six months after the end of the cash transfer program.

[24]While *Zap* households purchased more meat with the cash transfer (Table 3), they did not consume more meat (Table 4). This could be partially due to the different reference periods for the outcome measures. Table 3 asked households how they used the cash transfer (the last transfer in October 2010), whereas Table 4 asked about household consumption in the past 24 hours (December 2010). While grains, oils and beans are storable, meat is perishable unless dried. Thus, we would not expect to see an increase in meat consumption two months' after the last cash transfer.

pairwise comparison. Yet these improvements did not translate into changes in nutritional status, as measured by weight-for-height. This could be partially due to the imprecision of the estimates, as nutritional data were only collected for a subset of households, as well as the fact that improved child nutritional status is only positively correlated with diet diversity when diet diversity is greater than 3 food groups (Labadarios et al 2011).

While we would not necessarily expect asset accumulation as a result of the cash transfer program, we test for impacts on household asset ownership to verify that other household members were not reducing their contribution to household public or private goods (Panel C). Excluding mobile phones, *Zap* households owned .12 more asset categories as compared with those in the *Cash* group, and .31 more assets as compared with those in the *Mobile* group, although only the latter comparison is statistically significant at conventional levels. The *Zap* transfer mechanism did not have a strong impact upon households' durable (carts, plows, bikes and motos) or non-durable asset ownership (flashlights, petrol lamps and radios), although non-durable asset ownership was higher in *Zap* households as compared with *Mobile* households. Overall, these results suggest that household members were not reducing their contribution of public or private to the households as a result of the cash transfer. These results are consistent when using an ANCOVA specification or using data from the December or May rounds separately (Table A4).

IV. Potential Mechanisms

One of the core results in this paper is that receiving a cash transfer via mobile money led to different uses of the transfer and increased household diet diversity. Our experimental design allows us to conclude that these results are due to the m-transfer mechanism, and not to the mobile phone. This section presents evidence on the channels through which the observed impacts occurred.

A. Reduced Costs of Obtaining the Transfer

A key claim of those supporting the use of electronic transfers is that they will reduce the costs of implementing the program, including leakage. Yet whether these cost reductions accrue to program recipients depend upon the local electronic payments infrastructure, in this case, m-

money agents. While Concern Worldwide tried to minimize the distance that *Cash* and *Mobile* program recipients had to travel to obtain their transfer, a standard practice in such programs, Concern did not have control over the placement of registered m-money agents, which was managed by the mobile phone operator.

Figure 2 shows the recipients' travel costs related to obtaining the cash transfer. As both the *Mobile* and *Cash* groups received the cash transfer via the same mechanism, we pool the two groups. Overall, program participants in *Zap* villages incurred significantly fewer costs to obtain the transfer. Whereas *Cash* and *Mobile* program recipients travelled an average of 4.04 km (round-trip) from their home village 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.[25] This is equivalent to a travel time savings of approximately 1 hour for each cash transfer, or 5 hours over the entire program. However, this analysis excludes the *Cash* program recipients' waiting time during the transfer, which averaged four hours per cash transfer, as compared with 30 minutes for *Zap* recipients.[26] Including wait time, the average cost savings to *Zap* program recipients over the program period would have been approximately 20 hours. Based upon an average daily agricultural wage of US $3, this would translate into US $7.50 over the life of the program, equivalent to a 20-kg of grain at the time.

While the *Zap* transfer mechanism reduced recipients' costs of obtaining the transfer, contrary to Muralidharan et al (2014), we do not find any effects on leakage. Overall, leakage was very low: 98% of *Cash* recipients reported receiving their transfer and received 95,000 CFA over 4.4 transfers (Table 5). Only one of these differences was statistically significant across the three treatment arms. Part of the difference between the cash transfer balance received ($US 200) as compared with the target ($US 225) can be explained by the seed distribution program, whereby part of cash transfer was replaced by an equivalent value of seeds.[27] Yet even if these

[25]Hoddinott et al (2014) find that the average travel and wait time for cash transfers in Niger was one hour, using a portable ATM system.
[26]While the average wait time for manual cash transfers was four hours, this ranged from 1 to 8 hours. The corresponding wait time for Zap recipients was 15 minutes to 1 hour.
[27]In an effort to reduce the likelihood of theft during the manual cash distribution, either at the bank, en route or during the distribution, Concern Worldwide monitored all activities and hired security to guard the cash during its transport. Those costs represented over 60 percent of the manual cash transfer distribution budget.

differences were due to actual leakage, this would represent between 4-10 percent of the total value of the transfer ($US 1.50 to $7.50).

While the time savings results are small in magnitude, this occurred during the agricultural planting season, a period when program recipients' opportunity costs were relatively higher. This suggests that the estimates are a lower bound, as most program recipients had to travel from their fields – often located 1.5 hours from their home village – before obtaining the transfer. Since *Zap* recipients could cash out at any time, they had greater flexibility in choosing a time to cash out, which could have freed up their time to engage in more productive agricultural activities at a particularly busy time of year or potentially spend more time on childcare or food preparation. While we do not have time use data on the latter, we do have some evidence of the former: *Zap* program recipients were 9-13 percentage points more likely to plant okra and *vouanzdou* than their *Cash* and *Mobile* counterparts (Table A5). There is no systematic observed effect for other staple food and cash crops, and this did not translate into higher production or sales. As okra and *vouandzou* are marginal cash crops grown by women, this suggests that some of the time savings could have been used for agricultural activities, potentially affecting diet diversity.[28]

B. Differential Timing and Location of the "Cash Out" and Expenditures

The observed changes in the uses of the transfer and diet diversity could also be due to changes in the timing and location of households' receipt of the transfer and expenditures. Unsurprisingly, almost all of the *Cash* households received their transfer on the same day that it was available, as most households did not have a choice (Table 6, Panel A). By contrast, *Zap* households were 36-39 percentage points less likely to receive their cash on the same day that it was available, "cashing out" 1-4 days after receiving the notification. While the lag time between being informed of the transfer and actually receiving the transfer was only 1-2 days longer in *Zap* villages, these additional days provided program recipients with greater flexibility during a particularly busy time of year.

[28] This increase in the likelihood of planting these cash crops did not appear to have an income effect, as the amount harvested and sold was extremely small, with no statistically significant difference among the different groups (not shown). As women had to negotiate land with their spouses to cultivate these crops, however, this does suggest a potential shift in intra-household bargaining, which is discussed below.

Zap program recipients also had the option of withdrawing the money in smaller amounts, thereby allowing them to use the mobile phone as a savings device and avoid unplanned expenditures. The cash-out data reveals that this was not the case. For all transfers, over 98% of *Zap* households withdrew the entire amount of their cash transfer at one time (Personal Correspondence with Zain 2011). This could, in part, be due to the fact that *Zap* households would have had to pay a fee for any additional withdrawals. In fact, less than 5 percent of households had any value remaining in their mobile phone one month after the last transfer, and those that did saved less than US $0.15.

The slight change in the timing of the withdrawal could have modified the timing and location of household expenditures (Panel B). Yet 60 percent of *Cash* households spent their transfer all at one time (Table 6, Panel B), without a statistically significant difference in the timing of purchases between the *Zap, Mobile* and *Cash* groups. The results are similar for the location of expenditures: *Zap* households did not have significantly different purchasing patterns as compared with their *Mobile* and *Cash* counterparts (Table 6, Panel C). Overall, these results suggest that while the m-transfer mechanism increased the lag time between learning of and receiving the cash transfer for *Zap* households, it did not change when or where households spent the money.[29]

C. Increased Use of Mobile Phones

With access to the mobile phone handset, *Zap* households could have been better informed about prices, thereby affecting their purchasing decisions and diet diversity. While this channel should, in theory, yield similar results in both the *Zap* and *Mobile* groups, in practice *Zap* program recipients could have felt a greater sense of "ownership" of the mobile phone, as the transfer was specifically linked to the handsets.

Table 7 shows the impact of the different transfer mechanisms on mobile phone usage, using pooled data from December 2010 and May 2011. Unsurprisingly, mobile phone usage was

[29]Finally, 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 over time. Yet only 13 percent of *Cash* 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*, *Cash* and *Mobile* treatments (Table 6, Panel D).

higher in the *Zap* and *Mobile* villages as compared with the *Cash* villages, as well as between the *Zap* and *Mobile* and villages (Panel A). While respondents in *Zap* and Mobile villages were more active mobile phone users, especially for communicating with friends and family members, there were not statistically significant differences in mobile phone usage for commercial reasons or asking for help (Panel A). Thus, while we cannot entirely rule out that increased use of the mobile phone handset affected *Zap* households' outcomes in some unforeseen ways, it did not lead to increased access to market information, at least in the short-term. [30]

Access to the m-transfer technology also did affect *Zap* program recipients' access to private transfers (Table 7, Panel B). While the probability and intensity of seasonal migration was slightly higher among *Zap* households, this did not translate into changes in remittances: There were no statistically significant differences in the frequency or amount of remittances received or the remittance mechanism. These findings are robust when limited to one time period and also when conditioning on those households affected by a shock (N=649) (not shown). These results are not surprising, as the agent network was not widespread and the m-transfer system could not be used for transfers outside of the West African currency zone, the destination of a majority of migrants.[31]

D. Inter- and Intra-Household Dynamics

Unlike the manual cash mechanism, the *Zap* transfer mechanism made it more difficult for others to immediately observe the arrival of the transfer, as the program recipient was notified of the transfer arrival via a discrete beep. This was particularly relevant for *Zap* transfer recipients, many of whom wore their phones around their necks so that they could be notified of the m-transfer at any place or time.[32] In theory, this could have allowed *Zap* program recipients to have private information about the arrival of transfer, thereby affecting their bargaining power with

[30]The regressions in Table 7 (Panel A) were estimated by imputing a zero value for those recipients who had not used a mobile phone. We also estimated this table conditioning on whether the recipient had used a mobile phone since the last harvest (approximately 2/3 of the sample). While the coefficients differ, the results are similar across both specifications.

[31]The regressions in Table 7 (Panel B) were estimated by imputing a zero value if the household did not have any migrants. We also estimated these results conditional on whether the household having a migrant (N=521). The results are largely robust to this specification.

[32]While we do not have administrative data on the actual timing of the m-transfers or the time of the day that the beeps were received, Concern Worldwide transferred the money during the day. These are times when female program recipients would typically be away from home, searching for water and firewood or working in the field.

respect to how the cash transfer was used within or outside of the household.[33] While the *Zap* treatment did not have an impact on the likelihood of inter-household sharing (Table A6), it did appear to affect intra-household dynamics: in focus group interviews, *Zap* program recipients reported that they did not immediately inform their spouses about the arrival of the transfer, but waited until they were in the privacy of their home. By contrast, since *Cash* and *Mobile* program recipients often obtained their transfer with other household members, they immediately provided the cash transfer to that family member, with little opportunity to discuss how the transfer would be used.

Table 8 tests for the impact of the *Zap* intervention on intra-household decision-making more formally.[34] Overall, 52 percent of program recipients in the *Cash* villages reported that they were responsible for spending at least part of the cash transfer, and almost all recipients (99 percent) stated that they were consulted on how to spend the cash transfer. There was no statistically significant difference in either of these indicators among the three groups (Panel A).[35]

Panels B and C assesses the impact of the m-transfer on other proxy indicators of intra-household decision-making, many of which are common in much of the female empowerment literature (Deopke and Tertilt 2014, Bobonis 2009, Attanasio and Lechene 2002). *Zap* program recipients were more likely to travel to weekly markets and to be involved in the sale of household grains than *Mobile* or *Cash* households (Panel B). The *Zap* intervention also affected the extensive and intensive margin of women's and children's clothing expenditures for Muslim festivals (Panel C): *Zap* households were 6-9 percentage points more likely to spend money on

[33]Because the program was implemented an area of Niger where socio-cultural norms often do not permit younger, married women of the Hausa ethnic group to travel to markets (Coles and Mack 1991). As most of the women in our sample are married and less than 45 years old, we would not expect to find strong effects of the m-transfer mechanism on women's visible control over the cash transfer, such as spending it on their own.

[34]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. In addition, a large number of zero expenditures for males does not allow us to compute the ratio of women to male expenditures.

[35]Program recipients who were primarily responsible for spending the cash transfer were female-headed households (13 percent of the sample) and those of the Fulani and Touareg ethnic groups, for whom the travel restriction was not a primary constraint. Nevertheless, even in these cases, fewer than 2 percent of these households were solely responsible for spending all of the cash transfer. We do not have sufficient variation to estimate heterogeneous results by female-headed household or ethnicity.

women's and children's clothing and spent 30 percent more as compared with the *Cash* and *Mobile* treatments.[36]

Taken together, the results in Table 8 suggest that the m-transfer mechanism affected intra-household decision-making, thereby affecting households' uses of the transfer and food security. This could have happened in one of three ways. First, male household members in *Zap* households could have been less likely to spend the transfer on temptation goods or more willing to invest extra effort in finding lower prices for staple foods, thereby freeing up income. Second, women might have been better able to hide the transfer amount from their husbands, allowing them to purchase more diverse foodstuffs on their own. Or finally, women in *Zap* households might have been better able to convince their husbands to spend more on higher quality foods.[37] As we do not have data on temptation good spending, we are unable to rule out the first hypothesis. The second hypothesis, while likely, is not supported by the data on the location of expenditures (Table 6).[38] We posit that the third mechanism is potentially at work, although we only have qualitative data to support this, as mentioned above.

V. **Alternative Explanations**

There are several potential confounds to the above findings. A first potential confounding factor could emerge if the registration of Zap agents (who were already village residents or traders) provided new types of financial or commercial services to households, thereby improving *Zap* households' access to foodstuffs and agricultural inputs. Table 9 (Panel A) shows that this is not the case. Only 3 percent of *Cash* villages had a Zap agent, and there was not a statistically significant difference in the likelihood or number of Zap agents between the *Zap*, *Mobile* and *Cash* villages. While these results are not very precisely measured, they

[36]While much of the literature calculates the ratio of male to female clothing expenditures (Lundberg, Pollak and Wales 1997), very few households spent money on male clothing during this period, so we concentrate on women's clothing expenditures.

[37]An additional test of the intra-household bargaining channel would be to measure the impacts of the *Zap* program for male and female-headed households. If the program truly changed intra-household bargaining, then the effects should only be apparent for male-headed households. While we conduct this analysis, we cannot conclude that there is a statistically significant difference between the two groups. This could be partly due to the limited number of female-headed households in our sample (less than 15 percent), as well as the fact that the male and female-headed households differ in observable and unobservable ways.

[38]We have suggestive evidence that men in *Zap* households were willing to travel 3 km farther to larger markets, suggesting that the third mechanism could have taken place.

suggest that Zap agents were evenly distributed across different villages, and that differential access to m-transfer agents is not driving the results.

In Figure 2 and Tables 3 and 4 (our primary outcomes of interest), we examined the impact of the *Zap* program on 22 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 Sankoh et al (1997), we use a Bonferroni correction that adjusts for the mean correlation among outcomes, focusing on the key outcomes of interest. 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 (adjusting for correlation) would therefore be between .002 and .005.[39] The outcomes that remain significant at these adjusted levels are program recipients' cost of obtaining the cash transfer (Figure 2), the different uses of the cash transfer in Table 3 (with the exception of cowpea and condiments) and the increased consumption of fats and higher diet diversity (for the *Zap* group as compared with the *Mobile* group) in Table 4. Thus, we are confident that the *Zap* intervention affected recipients' costs in obtaining the cash transfer, recipients' uses of the transfer, fat consumption and household diet diversity.[40]

A final potential confounding factor is the differential effect of the transfer mechanism on prices. If the manual cash transfer mechanism put greater inflationary pressure on local markets as compared with the m-transfer mechanism, this could have reduced the value of the cash transfer in those villages and decreased the number of goods purchased or consumed. Or, if the m-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 traders to supply those goods to the market.[41] Table A7 shows that

[39]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 (Sankoh et al 1997, Simple Interactive Statistical Analyses). A mean correlation of zero would yield the full Bonferroni adjustment, whereas a mean correlation of one would mean no adjustment.

[40]A comparison of the original p-value and Bonferroni-adjusted p-value for Figure 2 and Tables 3 and 4 is available upon request. A less restrictive test than the Bonferroni adjustment would be the use of False Discover Rate (FDR) techniques (Anderson 2008).

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

different cash transfer delivery mechanisms did not have differential impacts on the number of wholesalers and semi-wholesalers or the number of products available on the market.

As 63 percent of program recipients' purchases in took place in weekly markets, primarily outside of the village, we collected weekly market price data between May and December 2010 from 45 markets in the region. The randomized nature of the program implies that some markets were linked to both *Cash* and *Zap* villages, almost certainly violating the stable unit treatment value assumption.

Table A8 presents the results of a regression of the log of weekly prices in market on the cash transfer delivery mechanisms, controlling for market and time fixed effects.[42] Overall, the presence of a cash transfer in a particular market during the week did not have a statistically significant impact upon food prices (Panel A). The absence of an impact 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, although this is not a causal interpretation.[43]

Panel B measures the impact of the cash transfer delivery mechanism on weekly food prices. Overall, the results are consistent with those in Panel A. The presence of the m-transfer or manual cash transfer program in a nearby village did not have an impact upon staple grain, cowpea or vegetable oil prices, and there is no statistically significant difference between the cash transfer mechanisms.[44] Overall, the analysis suggests that were not strong price effects due to the different transfer mechanisms.

VI. Conclusion

[42]The regression estimated in Table A8 is the following: $ln(p_{ij,t}) = \alpha + \beta_1 zap_{j,t} + \beta_2 cash_{j,t} + \theta_t + \theta_j + \varepsilon_{ij,t}$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 capture market characteristics. 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. To partially account for non-stationarity, the equation is estimated via first differences. Standard errors are clustered at the market level.

[43]While the results in Table A8 suggest that markets were able to respond to increased demand, these results only show relative changes in prices. Welfare could have decreased on these markets due to the cash transfer if the influx of cash increased prices higher than average prices during this period of year. This requires a comparison of average prices on these markets during the previous (non-drought) marketing season, which we do not have.

[44]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 manual cash transfer mechanisms. This is consistent with the finding that *Zap* households were more likely to purchase other cereals. Given the price of a bag of millet, households typically purchased small quantities (e.g., 1 kg) of rice. Rice is not produced in these areas of Niger, so the increase in rice prices could not have increased incentives to produce rice.

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 cost-effectiveness as compared with other types of interventions. Yet understanding the costs and benefits of implementing these programs is particularly important in countries where distributing cash involves significant logistical, operational and security costs, as is the case in many countries sub-Saharan Africa.

An intervention that provided a cash transfer via the mobile phone strongly reduced the costs of program recipients in obtaining the cash transfer. Yet a key question for the introduction of a new technological payment infrastructure is the cost of such programs to the implementing agency.[45] While the initial costs of the *Zap* program were significantly higher, primarily due to the purchase of mobile phones, the per-transfer costs of the *Zap* transfer mechanism were approximately 20 percent lower than per-transfer cost of the manual cash distribution. The average per recipient cost was US$16.43 in *Cash* and *Mobile* villages, well within the range of per recipient costs for other cash transfer programs in sub-Saharan Africa (DFID 2011), as compared with an average per recipient cost in *Zap* villages of US$24.14, or US$7.70 more per recipient.[46] Excluding the cost of the mobile phones, the per-recipient cost of the *Zap* intervention fell to $6.78 per recipient (Figure A2).[47] This suggests that mobile phones could be a simple and low-cost way to deliver cash transfers once the necessary infrastructure is in place.

Yet beyond the cost reductions, distributing cash transfers electronically also affected household behavior: *Zap* households bought more types of food items and increased their diet

[45] The initial investment costs of the Zap program included expenses for identifying program recipients, purchasing mobile phones and training recipients in how to use them, 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 counting the cash into individual envelopes. Equivalent variable costs for the m-transfer mechanism included the fees associated with making the transfers and staff time in managing the transfer process. The key variable costs for the *Zap* intervention included the transfer cost to the program recipients and program recipients' withdrawal fees.

[46] The annual per recipient costs of cash transfer programs in sub-Saharan Africa range from US$7 (Malawi) to US$35 (Ethiopia) (DFID 2011).

[47] The cost comparison focuses on the cost to the Concern Worldwide for implementing the program, rather than the costs to the private sector (such as the cost of maintaining the agent network, which were managed solely by the mobile phone operator). While this might omit some important costs, this is fairly standard in cost comparisons; for example, most electronic transfer programs do not compare the costs for maintaining a banking sector or SmartCards infrastructure after the initial investment.

diversity, all while retaining their durable and non-durable assets. These observed 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.

While these results are promising, they suggest that electronic transfers may not lead to improved financial inclusion for all households or in all contexts, as proponents might suggest. Unlike the mobile money "revolution" in Kenya, mobile money registration and usage has not grown substantially in other parts of sub-Saharan Africa, including Niger. This suggests that substantial investment to register clients and agents would be required to establish mobile payment systems. In addition, while program recipient households in our study used mobile money to receive their transfer, they did not use it to receive remittances or to save, two important aspects of financial inclusion. This is potentially related to the limited m-money agent network in the country, a common issue in other West African countries.

Like many field experiments, the generalizability of our results may be limited. Our study exploits variation in the transfer mechanism during a food crisis, when the marginal utility of income can be high. In addition, as we do not have full income and expenditure data, we are unable to estimate the Engel curves of these households, a relevant question for many cash transfer programs (Attansio et al 2012). And finally, since Niger is one of the poorest countries in the world, with low rates of literacy, financial inclusion and mobile money adoption, the context might be different from other countries where governments are considering electronic payments. Nonetheless, Niger's educational, financial and mobile money indicators are not vastly different from other countries in West Africa, suggesting that our results might be informative for those contexts (UNESCO 2012, Demirguc-Kunt and Klapper 2012).[48]

Despite these caveats, the widespread growth of mobile phone coverage and m-transfer 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 road infrastructure, low literacy rates and high financial exclusion -- suggests that the approach could

[48]For example, countries in the West Africa region have some of the lowest educational indicators in the world, and fewer than 10 percent of individuals had used mobile money in 2012, ranging from 1-2% in Togo and Ghana to 19% in Liberia (Demirguc-Kunt and Klapper 2012).

thrive in less marginalized countries and during periods when the marginal utility of income is lower. This is particularly the case in other areas of West Africa, such as Burkina Faso, Mali, northern Ghana and Senegal, which have similar education and mobile money indicators. Nevertheless, transferring these responsibilities to the private sector could potentially increase the likelihood of corruption or leakage, especially if m-transfer agents exert some type of power vis-à-vis program recipients. In addition, such programs could potentially crowd out certain segments of the private sector, especially smaller traders and shopkeepers who are unable register as agents.

References

Aker, Jenny C. 2010. "Information from Markets Near and Far: Mobile Phones and Agricultural Markets in Niger." *American Economic Journal: Applied Economics*, 2(3): 46–59.

Aker, Jenny C., Christopher Ksoll and Travis Lybbert. "Can Mobile Phones Improve Learning? Evidence from a Field Experiment in Niger." Forthcoming, *American Economic Journal: Applied Economics*.

Aker, Jenny C. and Isaac M. Mbiti. 2010. "Mobile phones and economic development in Africa." *Journal of Economic Perspectives*, 24(3): 207–232.

Ashraf, Nava. 2009. "Spousal Control and Intra-Household Decision Making: An Experimental Study in the Philippines." *American Economic Review* 99, no. 4.

Ashraf, Nava, Erica Field, and Jean Lee. 2014. "Household Bargaining and Excess Fertility: An Experimental Study in Zambia." *American Economic Review* 104, no. 7 (July 2014).

Attanasio, O., Battistin, E. and Mesnard, A. 2012. "Food and Cash Transfers: Evidence from Colombia." *The Economic Journal*, 122: 92–124.

Atanassova, Antonia, Marianne Bertrand, Sendhil Mullainathan and Paul Niehaus. "Targeting with Agents." *American Economic Journal: Economic Policy*, forthcoming.

Baird, Sarah, Craig McIntosh and Berk Ozler. 2011. "Cash or Condition? Evidence from a Randomized Cash Transfer Program." *Quarterly Journal of Economics*.

Banerjee, Abhijit, Esther Duflo, Rachel Glennerster and Cynthia Kinnan. 2013. "The Miracle of Microfinance? Evidence from a Randomized Evaluation." *NBER Working Paper* No. 18950.

Barrera-Osorio, Felipe, Marianne Bertrand, Leigh L. Linden, and Francisco Perez-Calle. 2011. "Improving the Design of Conditional Transfer Programs: Evidence from a Randomized Education Experiment in Colombia." *American Economic Journal: Applied Economics,* **3(2): 167–95.**

Barrientos, Armando, Miguel Niño-Zarazúa and Mathilde Maitrot. 2010. *Social Assistance in Developing Countries Database: Version 5.0*. DFID and the Brooks World Poverty Institute, The University of Manchester, United Kingdom.

Blumenstock, Joshua, Nathan Eagle and Marcel Fafchamps. 2014. "Motives for Mobile-Based Giving: Evidence in the Aftermath of Natural Disasters." Unpublished manuscript.

Blumenstock, Joshua, Michael Callen and Tarek Ghani. 2014. "Violence and Precautionary Savings: Experimental Evidence from Mobile Phone-Based Salaries in Afghanistan" Unpublished Working Paper.

Bobonis, Gustavo J. 2009. "Is the Allocation of Resources within the Household Efficient? New Evidence from a Randomized Experiment." *Journal of Political Economy*. 117 (3): 453–503.

Caldes, N., Coady, D. and Maluccio, J. 2004. "The Cost of Poverty Alleviation Transfer Programmes: A Comparative Analysis of Three Programmes in Latin America." FCND Discussion Paper N. 174, IFPRI, Washington DC

Coles, Catherine and Beverly Mack, eds. 1991. *Hausa Women in the 20th Century*. Madison, WI: University of Wisconsin Press

Cunha, Jesse M., Giacomo De Giorgi and Seema Jayachandran. 2013. "The Price Effects of Cash Versus In-Kind Transfers." Unpublished manuscript.

Dhaliwal, Iqbal, Esther Duflo, Rachel Glennerster, Caitlin Tulloch. August 2011. "Comparative Cost-Effectiveness Analysis to Inform Policy in Developing Countries: A General Framework with Applications for Education." Abdul Jameel Poverty Action Lab.

De Janvry A, Sadoulet E, Pantelis Solomon and Renos Vakis. 2006. **"**Uninsured Risk and Asset Protection: Can Conditional Cash Transfer Programs Serve as Safety Nets?" *World Bank Discussion Paper 0604*. World Bank Publications: Washington, D.C.

DFID. April 2011. "Cash Transfers Evidence Paper." *Evidence Paper Policy Division*. London, UK.

Doepke, Matthias and Michele Tertilt. 2014. "Does Female Empowerment Promote Economic Development?" Unpublished manuscript.

Demirguc-Kunt, Asli and Leora Klapper. 2012. "Measuring Financial Inclusion: The Global Findex Database." *Policy Research Working Paper 6025*. Washington, D.C. The World Bank.

Demographic and Health Surveys. 2012. *Enquete Demographique et de Sante et Indicateurs Multiples du Niger*. *(EDSN-MICS IV)*. *Rapport Preliminaire*. Niamey, Niger and Calverton, MD: 2012.

Doss, Cheryl. 2006. "The Effects of Intra-household Property Ownership on Expenditure Patterns in Ghana." *Journal of African Economies* 15 (1): 149–180.

Duflo, Esther, and Christopher Udry. 2004. May. "Intra-household Resource Allocation in Cote d'Ivoire: Social Norms, Separate Accounts and Consumption Choices." Working paper 10498, National Bureau of Economic Research.

Dupas, Pascaline and Jonathon Robinson. 2013. "Why don't the poor save more? Evidence from Health Savings Experiments." *American Economic Review*. 103(4), pp. 1138-71, June 2013.

Engle, Patrice L. 1993. "Influences of Mothers' and Fathers' Income on Children's Nutritional Status in Guatemala." Social Science Medicine 37 (11): 1303– 1312.

Evans, David K. and Anna Popova. 2014. "Cash Transfers and Temptation Goods: A Review of Global Evidence." *World Bank Policy Research Working Paper 6886*. Washington, D.C.: The World Bank Group.

Evans, David S. and Alexis Pirchio. 2015. **"**An Empirical Examination of Why Mobile Money Schemes Ignite in Some Developing Countries But Flounder in Most." *The University of Chicago, Institute for Law and Economics Working Paper Series*.

Famine Early Warning Systems Network (FEWS NET). 2005. *Niger: An evidence base for understanding the current crisis*. Washington DC: FEWS NET.

Famine Early Warning Systems Network (FEWS NET). 2010. *Niger Food Security Outlook 2010*. Washington DC: FEWS NET.

Food and Nutrition Technical Assistance (FANTA. 2006. *Household Dietary Diversity Score (HDDS) for Measurement of Household Food Access: Indicator Guide*. AED: Washington, D.C.

Ferraz, Claudio and Frederico Finan. 2008. "Exposing Corrupt Politicians: The Effect of Brazil's Publicly Released Audits on Electoral Outcomes", with Claudio Ferraz, *Quarterly Journal of Economics*, 123(2): 703-745.

Garcia, Marito and Charity M.T. Moore. 2012. *The Cash Dividend: The Rise of Cash Transfer Programs in Sub-Saharan Africa*. The World Bank: Washington, D.C.

Gentilini, Ugo, Maddalena Honorati and Rsulan Yemtsov. 2014. *The state of social safety nets 2014*. Washington, DC : The World Bank Group.

Gibson, John, David McKenzie and Steven Stillman. 2011**.** "The Impacts of International Migration on Remaining Household Members: Omnibus Results from a Migration Lottery Program." *Review of Economics and Statistics*. 93(4): 1297-1317.

Government of Niger. 2007. *Plan National de Contingence: Volet Sécurité Alimentaire et Nutritionnelle*. Niamey, Niger.

GSMA. 2013. **"**State of the Industry: Results from the 2012 Global Mobile Money Adoption Survey".

Handa, Sudhanshu and Benjamin Davis. 2006. "The Experience of Conditional Cash Transfers in Latin America and the Caribbean." *Development Policy Review*. 24 (5): 513-536

Hoddinott, John, and Lawrence Haddad. 1995. "Does Female Income Share Influence Household Expenditures? Evidence From Cote D'Ivoire." *Oxford Bulletin of Economics and Statistics* 57 (1): 77–96.

Hoddinott, John, Susanna Sandstrom and Joanna Upton. 2014. "The Impact of Cash and Food Transfers: Evidence from a Randomized Intervention in Niger." *IFPRI Discussion Paper 01341*. IFPRI: Washington, D.C.

Hoddinott, John and Yisehac Yohannes. 2002. "Dietary Diversity as a Food Security Indicator." *FCND Discussion Paper No*. *136*. IFPRI: Washington, D.C.

Jack, William, Tavneet Suri and Robert Townsend. 2010. "Monetary Theory and Electronic Money: Reflections on the Kenyan Experience." *Economic Quarterly*. Volume 96, Number 1: 83–122

Jack, William and Tavneet Suri. 2014. "Risk-Sharing and Transaction Costs: Evidence from Kenya's Mobile Money Revolution." *American Economic Review*. 104(1): 183-223.

Jakiela, Pamela and Owen Ozier. 2012. "Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies." Unpublished manuscript.

Jensen, Robert. 2007. "The Digital Provide: Information (Technology), Market Performance, and Welfare in the South Indian Fisheries Sector." *Quarterly Journal of Economics*, 122(3): 879–924.

Kendall, J., N. Mylenko, and A. Ponce. 2010. Measuring Financial Access around the World. World Bank Policy Research Working Paper 5253.

Labadarios, Demetre, Zandile June-Rose Mchiza, Nelia Patricia Steyn, Gerda Gericke, Eleni Maria Winifred Maunder, Yul Derek Davids and Whadiah Parker. 2011. "Food Security in South Africa: A Review of National Surveys." *Bulletin of the World Health Organization*. 89:891-89**.**

Lundberg, Shelly J., Robert A. Pollak, and Terence J. Wales. 1997. "Do Husbands and Wives Pool Their Resources? Evidence from the United Kingdom Child Benefit." *The Journal of Human Resources*. 32 (3): 463–480.

Mas, Ignacio and Mayer, Colin. "Savings as Forward Payments: Innovations on Mobile Money Platforms." March 2012. Working Paper.

Mbiti, Isaac M. and David Weil. 2011. "Mobile Banking: The Impact of M-Pesa in Kenya." *NBER Working Paper No. w17129*.

Morawczynski, Olga and Mark Pickens. 2009. "Poor People Using Mobile Financial Services: Observations on Customer Usage and Impact from M-PESA." **CGAP: Washington, D.C.**

Muralidharan, Karthik, Paul Niehaus and Sandip Sukhtankar. 2014. "Payment Infrastructure and the Performance of Public Programs: Evidence from Biometric Smartcards in India." Technical Paper, BREAD.

Niang, Ousmane, Véronique Mistycki and Soukeynatou Fall. 2012. "The impact of safety nets on the resilience of vulnerable households in Niger." 55. *Humanitarian Exchange Magazine*.

Nicholson, S.E. , B. Some and B. Kone. 2000. "An Analysis of Recent Rainfall Conditions in West Africa, Including the Rainy Seasons of the 1997 El Nino and the 1998 La Nina Years." *Journal of Climate*. Vol. 13.

Olken, Benjamin. 2006. "Corruption and the Costs of Redistribution." *Journal of Public Economics* 90 (4-5), pp. 853-870.

Olken, Benjamin. 2007. "Monitoring Corruption: Evidence from a Field Experiment in Indonesia" *Journal of Political Economy*, vol. 115, issue 2, April 2007, pp. 200-249.

Reinikka, Ritva, and Jakob Svensson. 2004. "Local Capture: Evidence From a Central Government Transfer Program in Uganda." *The Quarterly Journal of Economics*. 119(2): 678–704.

Sankoh AJ, Huque MF, Dubey SD. "Some comments on frequently used multiple endpoint adjustments methods in clinical trials. *Statistics in Medicine* 1997;16:2529-254."

Thorne-Lyman, Andrew, Natalie Valpiani, Kai Sun, Richard D. Semba, Christine L. Klotz, Klaus Kraemer, Nasima Akhter, Saskia de Pee, Regina Moench-Pfanner, Mayang Sari and Martin W. Bloem. 2010. "Household Dietary Diversity and Food Expenditures Are Closely Linked in Rural Bangladesh, Increasing the Risk of Malnutrition Due to the Financial Crisis." *Journal of Nutrition,* January 2010. 140: 182S-188S.

UNDP. 2011. Human Development Report 2011: Sustainability and Equity: A Better Future for All. UNDP: New York.

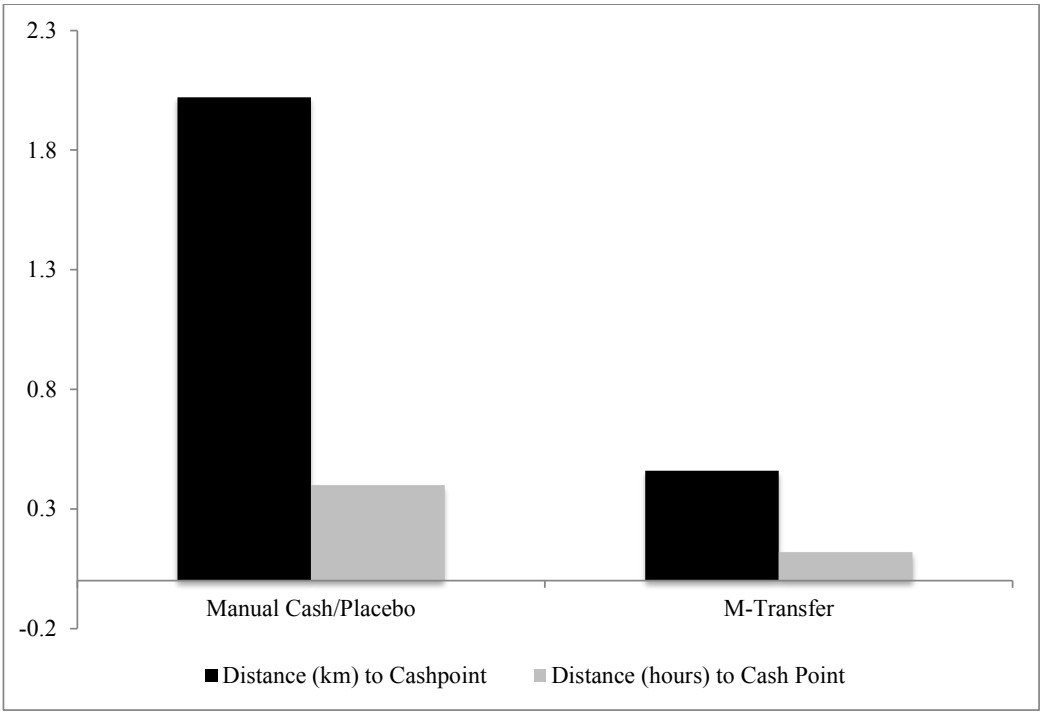
UNESCO. 2012. *Education for All Global Monitoring Report*. http://www.unesco.org/new/en/education/themes/leading-the-international-agenda/efareport/statistics/.

World Bank. 2009. *Conditional Cash Transfers: Reducing Present and Future Poverty*. World Bank Publications: Washington, DC.

Figure 1. Timeline of Data Collection and 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 and planting 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
Nutrition survey in 30 villages</td><td colspan="4">Hungry season and planting season</td><td colspan="2">Harvest period</td><td></td></tr>
</table>

Figure 2. Program Recipients' Costs of Obtaining the Cash Transfer by Cash Transfer Delivery Mechanism



Notes: This figure shows the mean cost (in km and hours), by transfer mechanism, for program recipients' travel to the nearest cash point to obtain their cash transfer. 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 Treatment Status)

	(1)	(2)	(3)	(4)
	Cash	**Zap-Cash**	**Mobile-Cash**	**Zap-Mobile**
	Mean (s.d.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)
Panel A: Socio-Demographic Characteristics				
Age of respondent	33.22	1.91	-0.90	2.81**
	(11.05)	(1.21)	(1.24)	(1.24)
Polygamous household	0.28	0.04	0.02	0.01
	(0.45)	(0.04)	(0.04)	(0.04)
Respondent is member of Hausa ethnic group	0.81	-0.05	0.08	-0.13
	(0.40)	(0.08)	(0.06)	(0.08)
Number of household members	9.3	-0.40	-0.21	-0.18
	(4.95)	(0.63)	(0.52)	(0.50)
Number of household members under 15 years	5.65	-0.35	-0.11	-0.24
	(3.42)	(0.38)	(0.34)	(0.34)
Percentage of Household Members with Some Education	0.59	-0.01	0.04	-0.04
	(0.32)	(0.03)	(0.03)	(0.03)
Panel B: Household Income Sources and Assets				
Agriculture is an income source	0.97	-0.01	-0.01	-0.00
	(0.16)	(0.01)	(0.01)	(0.01)
Livestock is an income source	0.62	0.06	-0.03	0.09*
	(0.49)	(0.06)	(0.06)	(0.05)
Remittances are an income source	0.36	-0.01	-0.05	0.04
	(0.48)	(0.04)	(0.04)	(0.04)
Number of asset categories owned (out of 12)	3.6	0.07	-0.15	0.22
	(1.58)	(0.18)	(0.16)	(0.14)
Panel C: Mobile Phone Ownership and Usage				
Household owns mobile phone	0.29	0.04	-0.03	0.06
	(0.45)	(0.05)	(0.04)	(0.04)
Respondent has used mobile phone since last harvest	0.62	0.06	0.00	0.05
	(0.49)	(0.05)	(0.05)	(0.04)
Respondent made or received call since last harvest	0.62	0.06	-0.00	0.06
	0.49	(0.05)	(0.05)	(0.04)
Respondent sent or received m-money transfer since last harvest	0	0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Panel D: Shocks				
Household experienced drought in past year	0.97	-0.00	0.01	-0.01
	(0.15)	(0.01)	(0.01)	(0.01)
Household experienced crickets in past year	0.82	-0.02	-0.04	0.01
	(0.39)	(0.04)	(0.05)	(0.05)
Number of household observations		1106		

Panel E: Village-Level Covariates				
Market located within the village	0.36	-0.03	0.01	-0.04
	(0.48)	(0.13)	(0.13)	(0.12)
School located within village	0.97	0.02	-0.04	0.05
	(0.16)	(0.04)	(0.05)	(0.05)
Presence of a seed distribution program	0.26	0.04	-0.04	0.08
	(0.44)	(0.08)	(0.07)	(0.09)
Village population (number of households)	177	-22	-12	-10
	(127)	(29)	(34)	(26)
Number of village observations		93		

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 difference in conditional means between the different treatments and the cash households. Column 4 shows the conditional difference in means between the *Zap* and *Mobile* treatment households. All regressions control for commune-level fixed effects and the presence of a seed program in the village, the level of stratification prior to randomization. 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 Treatment Status)

	(1)	(2)	(3)	(4)
	Cash	**Zap-Cash**	**Mobile-Cash**	**Zap-Mobile**
	Mean	Coeff	Coeff	Coeff
	(s.d.)	(s.e.)	(s.e.)	(s.e.)
Panel A: Food Security Outcomes and Coping Strategies				
Number of months of household food provisioning (scale of 6)	1.89	0.19	0.14	0.06
	(1.56)	(0.15)	(0.14)	(0.14)
Household diet diversity index (scale of 12)	3.07	0.04	-0.08	0.12
	(2.03)	(0.19)	(0.18)	(0.16)
Panel B: Migration and Remittances				
One household member migrated since the last harvest	0.5	0.03	0.02	0.01
	(0.50)	(0.05)	(0.05)	(0.05)
Number of remittances received since the past harvest	0.87	0.23	0.08	0.16
	(2.63)	(0.19)	(0.17)	(0.19)
Received remittance via m-money transfer (zap)	0.01	0.00	0.01	-0.00
	(0.12)	(0.01)	(0.01)	(0.01)
Panel C: Agricultural Production and Livestock				
Cultivate land	0.98	-0.01	-0.01	-0.00
	(0.13)	(0.01)	(0.01)	(0.01)
Produce millet	0.97	-0.01	0.01	-0.01
	(0.17)	(0.01)	(0.01)	(0.01)
Quantity of millet produced (kg)	267	19.97	-16.25	36.22
	(362)	(52.86)	(43.77)	(40.03)
Produce cowpea	0.87	-0.03	-0.07*	0.05
	(0.34)	(0.03)	(0.04)	(0.03)
Quantity of cowpea produced (kg)	9.06	2.34	0.98	1.36
	(30)	(2.44)	(2.73)	(2.49)
Produce vouandzou or okra	0.54	0.00	-0.02	0.02
	(0.50)	(0.06)	(0.05)	(0.05)
Number of observations		1106		

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 *Mobile* treatment households. All regressions control for commune-level fixed effects and the presence of a seed program in the village. 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) **Mobile-Cash**	(4) **Zap-Mobile**
	Mean			
Panel A: Uses of Cash Transfer for Food Items	(s.d.)	Coeff(s.e.)	Coeff(s.e.)	Coeff(s.e.)
Number of food and non-food items purchased with cash transfer	4.42	0.78***	-0.07	0.85***
	(2.48)	(0.24)	(0.24)	(0.25)
Transfer used to buy staple grains (millet, sorghum)	0.99	-0.01	-0.00	-0.01
	(0.05)	(0.01)	(0.01)	(0.01)
Transfer used to buy other grains (corn, rice)	0.58	0.18***	-0.02	0.20***
	(0.50)	(0.05)	(0.05)	(0.06)
Transfer used to buy cowpea	0.42	0.09*	-0.01	0.10**
	(0.49)	(0.05)	(0.05)	(0.05)
Transfer used to buy condiments	0.69	0.11**	-0.02	0.12***
	(0.46)	(0.05)	(0.04)	(0.04)
Transfer used to buy oil	0.69	0.13***	-0.01	0.15***
	(0.46)	(0.05)	(0.05)	(0.05)
Transfer used to buy meat	0.40	0.16***	-0.02	0.18***
	(0.49)	(0.04)	(0.04)	(0.04)
Panel B: Uses of Cash Transfer for Non-Food Items				
Transfer used to pay school fees	.07	-0.02	-0.01	-0.01
	(0.26)	(0.02)	(0.02)	(0.02)
Transfer used to pay health expenses	.30	0.00	-0.02	0.02
	(0.46)	(0.03)	(0.04)	(0.03)
Transfer used to buy clothes	.05	0.01	0.00	0.00
	(0.21)	(0.02)	(0.03)	(0.02)
Number of observations		1047		

Notes: This table presents a simple difference comparison of households 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 Mobile treatment households. All regressions control for geographic-level fixed effects and for the presence of a seed distribution program in the village. 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 and Nutritional Status

	(1) **Cash average** Mean (s.d.)	(2) **Zap-Cash** Coeff (s.e.)	(3) **Mobile-Cash** Coeff (s.e.)	(4) **Zap-Mobile** Coeff (s.e.)
Panel A: Food Security				
Household diet diversity score (out of 12)	3.16	0.27*	-0.19	0.50***
	(1.70)	(0.15)	(0.13)	(0.14)
Consumption of:				
Grains	0.99	0.00	0.00	-0.00
	(0.11)	(0.01)	(0.01)	(0.01)
Beans	0.18	0.06**	-0.01	0.06**
	(0.39)	(0.03)	(0.02)	(0.03)
Fats	0.29	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	-0.08**	0.07*
	(0.48)	(0.05)	(0.04)	(0.04)
Fruit	0.03	0.03**	-0.01	0.03***
	(0.16)	(0.01)	(0.01)	(0.01)
Number of observations		2168		
Panel B: Child Nutritional Status				
Number of meals eaten by children under 5 in past 24 hours	2.33	0.33**	0.05	0.28**
	(1.80)	(0.15)	(0.14)	(0.12)
Diet diversity of children under 5	2.33	0.20	-0.22	0.42**
	(1.80)	(0.23)	(0.18)	(0.17)
Weight-for-height z-score	-1.09	0.06	-0.03	0.09
	(0.98)	(0.12)	(0.15)	(0.13)
Number of observations		691		
Panel C: Durable and Non-Durable Goods				
Number of asset categories owned (out of 11, excluding mobile phones)	3.05	0.12	-0.19*	0.31***
	(1.27)	(0.11)	(0.11)	(0.09)
Durable assets (plows, carts, bikes and motos)	0.78	-0.01	-0.07**	0.05
	(0.49)	(0.04)	(0.03)	(0.03)
Non-durable assets (flashlights, petrol lamps and radios)	1.63	0.12	-0.08	0.20***
	(0.87)	(0.07)	(0.07)	(0.07)
Number of observations		2219		

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, 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 Mobile treatments. All regressions control for commune-level fixed effects and the presence of a seed program in the village. 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. Panels A and C include pooled data from the December and May rounds, whereas Panel B only includes data from the May round.

Table 5: Leakage

	(1) **Cash average**	(2) **Zap-Cash**	(3) **Mobile-Cash**	(4) **Zap-Mobile**
Program recipient received cash transfer	0.98	-0.02	0.02	-0.04**
	(0.16)	(0.02)	(0.01)	(0.02)
Number of transfers received	4.43	-0.10	-0.02	-0.08
	(1.28)	(0.11)	(0.13)	(0.13)
Amount of money received (CFA)	95,372	-263.95	-236.25	-27.70
	(31209)	(2,761.47)	(3,137.18)	(2,904.74)
Number of observations		1079		

Notes: This table presents a simple difference comparison of households 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 Mobile treatment households. All regressions control for geographic-level fixed effects and for the presence of a seed distribution program in the village. 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: Location, Knowledge and Timing of Cash Transfer Expenses

	(1) **Cash average**	(2) **Zap-Cash**	(3) **Mobile-Cash**	(4) **Zap-Mobile**
Panel A: Timing of Receipt of Transfer				
Obtained transfer the same day	0.89	-0.39***	-0.03	-0.36***
	(0.32)	(0.06)	(0.03)	(0.05)
Panel B: Timing of Expenditures				
Spent money all at once	0.60	-0.03	-0.03	0.00
	(0.49)	(0.04)	(0.04)	(0.04)
Spent money at least two times	0.40	0.03	0.03	-0.00
	(0.49)	(0.04)	(0.04)	(0.04)
Pane C: Location of Expenditures				
Spent transfer at kiosk in village	0.42	0.04	-0.04	0.08
	(0.49)	(0.06)	(0.05)	(0.06)
Spent transfer at market within village	0.23	-0.01	0.03	-0.04
	(0.42)	(0.09)	(0.08)	(0.08)
Spent transfer at market outside village	0.63	0.03	-0.00	0.03
	(0.48)	(0.08)	(0.08)	(0.08)
Panel D: Knowledge of Cash Transfer				
Knew correct amount of cash transfer	0.13	0.01	-0.01	0.02
	(0.33)	(0.03)	(0.02)	(0.03)
Knew correct duration of cash transfer	0.12	0.02	-0.02	0.04
	(0.33)	(0.03)	(0.02)	(0.03)
Number of observations		1047		

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, 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 the *Zap* and *Mobile* treatments. All regressions control for commune-level fixed effects and the presence of a seed program in the village. 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: Mobile Phone Ownership and Usage

	(1) **Cash average**	(2) **Zap-Cash**	(3) **Mobile-Cash**	(4) **Zap-Mobile**
	Mean (s.d.)	Coeff (s.e.)	Coeff (s.e.)	Coeff (s.e.)
Panel A: Mobile Phone Ownership and Usage				
Program recipient used mobile phone since last harvest	0.52	0.33***	0.15***	0.18***
	(0.50)	(0.04)	(0.04)	(0.04)
Made or received calls	0.26	0.30***	0.15***	0.15***
	(0.44)	(0.04)	(0.04)	(0.04)
Sent or received a "beep"	0.07	0.12***	0.04***	0.08***
	(0.25)	(0.02)	(0.01)	(0.02)
Received credit via Zap	0.01	0.19***	0.03**	0.16***
	(0.12)	(0.02)	(0.01)	(0.02)
Communicate with family/friends inside Niger	0.39	0.28***	0.13***	0.15***
	(0.49)	(0.04)	(0.03)	(0.04)
Communicate with family/friends outside Niger	0.40	0.09***	0.02	0.07***
	(0.49)	(0.03)	(0.02)	(0.02)
Communicate with commercial contacts inside Niger	0.00	0.01**	0.01***	-0.00
	(0.00)	(0.00)	(0.00)	(0.01)
Used mobile phone to communicate death/ceremony	0.14	0.12***	0.08***	0.03
	(0.35)	(0.03)	(0.02)	(0.02)
Used mobile phone to obtain price information	0.02	-0.01	0.01	-0.02
	(0.14)	(0.01)	(0.02)	(0.02)
Used mobile phone to ask for help/support	0.14	0.03	0.03	0.00
	(0.35)	(0.02)	(0.02)	(0.02)
Panel B: Migration, Remittances and Mobile Transfers				
At least one household member migrates	0.39	0.08*	0.05	0.03
	(0.48)	(0.05)	(0.05)	(0.04)
Percentage of household members who migrated	0.05	0.02*	0.01	0.01
	(0.08)	(0.01)	(0.01)	(0.01)
Household received remittances as income	0.21	0.05	0.01	0.04
	(0.41)	(0.03)	(0.03)	(0.03)
Amount of remittances received for last transfer (CFA)	4177	482.45	235.90	246.55
	(12334)	(830.78)	(867.13)	(817.47)
Number of remittances since last harvest	0.51	0.19	-0.00	0.19
	(2.44)	(0.16)	(0.12)	(0.14)
Received remittance via Western Union	0.06	-0.01	-0.02	0.02
	(0.24)	(0.01)	(0.01)	(0.01)
Received remittance via friend	0.10	0.04	0.03	0.01
	(0.30)	(0.02)	(0.02)	(0.02)
Received remittance via Zap	0.00	0.00	-0.00	0.00
	(0.04)	(0.01)	(0.00)	(0.00)
Number of observations	2219			

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, 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 the Zap and Mobile treatments. All regressions control for commune-level fixed effects and the presence of a seed program in the village. The total sample is 1052, but most regressions have a sample size of 666 observations (conditional on whether the respondent or household had used a mobile phone). 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 8: Intra-Household Decision-Making

	(1) **Cash average** Mean (s.d.)	(2) **Zap-Cash** Coeff(s.e.)	(3) **Mobile-Cash** Coeff (s.e.)	(4) **Zap-Mobile** Coeff(s.e.)
Panel A: Decision-Making Regarding Cash Transfer				
Program recipient responsible for spending part of cash transfer	0.52	-0.01	-0.03	0.01
	(0.50)	(0.03)	(0.04)	(0.04)
Program recipient involved in deciding how transfer	0.99	0.01	0.01	0.00
	(0.09)	(0.01)	(0.01)	(0.01)
Number of observations		1047		
Panel B. Women's Involvement in Agriculture				
Program recipient visited market in past week	0.19	0.09**	-0.04	0.14***
	(0.39)	(0.04)	(0.04)	(0.04)
Program recipient involved in selling grains for household	0.25	0.04*	-0.02	0.06**
	(0.43)	(0.02)	(0.03)	(0.03)
Number of observations		2124		
Panel C: Clothing Expenditures for Muslim Festivals				
Household spent money on women or children's clothing for festivals	0.43	0.09*	0.03	0.06*
	(0.50)	(0.05)	(0.05)	(0.04)
Amount spent on children's clothing for festivals	6351	2,117.55*	-258.52	2,376.07**
	(11250)	(1,152.53)	(1,102.29)	(1,152.52)
Number of observations		1062		

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, 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 the Zap and Mobile treatments. All regressions control for commune-level fixed effects and the presence of a seed program in the village. The data only include observations from male-headed 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.