Filets Sociaux (Jigisémèjiri) program
Midline Report

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Executive Summary

In 2014 the Government of Mali began implementing its "Filets Sociaux (Jigisémèjiri)" program that aims at reducing poverty and improving human capital accumulation through targeted cash transfers (CT), accompanying measures (or trainings) (AM), and preventive nutrition packages (PNP). In order to rigorously and independently assess the impacts of the program, the Government of Mali has collaborated with research partners, International Food Policy Research Institute (IFPRI) and Institut de recherché pour le développement (IRD), to undertake an impact evaluation. The impact evaluation assesses how the "Jigisémèjiri" program impacts household welfare and child nutrition and health. The impact evaluation is designed as a two-stage randomized control trial in which communes are randomly assigned to treatment or control; then, within a subset of the treatment communes deemed eligible for nutrition packages, villages are randomly assigned to receive the nutrition packages or not receive them. Thus, the impact evaluation will also be able to assess the additional impact of the preventative nutrition packages.

In order to measure impact of the program after 1 to 2 years of exposure, baseline and midline surveys have been implemented that collect detailed information on (1) outcomes on which we expect to see impacts, and (2) intermediate factors that may explain pathways of impact, such as changes in behavior, knowledge, and practices. The baseline data collection occurred September 2014–February 2015, before the start of the program in different regions, and the midline data were collected in August–November 2016, before the control group was rolled into the program. An endline survey is scheduled for 2018, after one year of implementation of the nutrition packages.

Data are collected on two samples: a panel sample that follows across rounds selected households that had a child age 6–23 months at baseline who is 28–47 months at midline, and a cross-section sample that in each round selects a new draw of households with a child age 6–23 months. The panel sample is intended to assess impacts at the household level, while the cross-section sample is intended to assess impacts at the child level. At midline, 2,446 households from the initial panel listing and 1,498 households from the cross-section listing are successfully interviewed. Attrition in the panel is relatively low (4 percent in the two years between baseline and midline) and is uncorrelated with treatment assignment.

This report presents findings from the impact evaluation of the Jigisémèjiri program at midline. It first provides background on the evaluation, including details on the program and interventions. It then relates first-order, second-order, and third-order outcomes of interest in a conceptual framework, which guides our analysis and structures the report. The report next describes the evaluation design, sampling, and data collection process. It then presents statistics on beneficiaries' experience with the CTs and AM. Last, it uses the baseline and midline data, exploiting the randomized design, to estimate the impacts of the program on its beneficiary population at midline, distinguishing between household-level outcomes and child-level outcomes. Given that the baseline report showed that the randomization process was successful at creating similar groups for comparison for the impact evaluation with similar preprogram characteristics, the impacts at midline can be interpreted as truly caused by the program rather than simply correlated with its receipt.

Findings from the midline data collection lead to conclusions in this report that can be divided into four categories: beneficiaries' experience with the CTs, beneficiaries' experience with the AM, program impacts on household-level outcomes, and program impacts on child-level outcomes.

***Beneficiaries' experience with the cash transfers*:** Beneficiary reports reflect that the cash transfer component has been implemented largely as designed, adhering closely to the cluster-randomized control

trial design and following the implementation calendar. In both the panel and cross-section samples, approximately 97 percent of all households in treatment communes report having received a cash transfer in the past 24 months, while virtually no household in the control group reports having received a transfer. Pooled across the five study regions and conditional on receiving at least one transfer, the mean reported number of transfers received by treatment households at midline is just over 5 in both samples; this number ranges from about 4 transfers in the regions of Segou and Mopti to about 6 transfers for the region of Sikasso, as expected from the government's phased implementation calendar. The mean reported amount received in the last transfer is just over 30,000 FCFA, consistent with a small share of households having received a double transfer.

In terms of targeting within the household, the data confirm that the household head is the primary person engaging with the Jigisémèjiri cash transfer. As intended by the program, the household head is listed as the main beneficiary in the vast majority of treatment households (over 80 percent in both samples). Household heads, moreover, represent the largest share of individuals that physically collect the transfer (over 80 percent in both samples). In 77 to 78 percent of cases, the household head is also reported as the only individual that decides how the cash transfer is to be used. Although spouses of the head and adult children also participate in some households, their roles are dominated by the role of the household head.

According to households' self-reports on how they used their last cash transfer, the three largest categories of expenses were food consumption, health, and agricultural investments (about 65 percent, 11 percent, and 4 percent, respectively), corresponding to approximately 80 percent of the last transfer received or 24,000 FCFA. Up to 2 percent of the last transfer is reported to be used for savings and less than 1 percent of the transfer is reported being shared with family and friends. There are small but non-trivial shares reporting concerns about being able to keep the whole transfer. About 8 percent of the cross section sample and 6 percent of the panel sample worry about their safety when returning home from the distribution point with the cash transfer, and similar shares worry about having to share the transfer with non-household members (family and friends) who might know that they just received money. Among treatment households who have received a transfer, the reported average time since receiving the last cash transfer is about 66 days for the panel (2.2 months) and about 63 days for the cross-section (2.1 months).

***Beneficiaries' experience with the accompanying measures*:** With the caveat that beneficiary households may have interpreted survey questions on the Jigisémèjiri AM component to include only the AM session linked to the cash distribution and not the other AM sessions, we analyze beneficiaries' reported experience with the AM component. Beneficiary reports reflect that adherence to the design and coverage was moderate, but lower than for the cash transfers. About 60 percent of treatment households in both cross-section and panel samples reports attending at least one accompanying measure session; it may not be surprising that this is much lower than the share of treatment households that received at least one cash transfer, given that the accompanying measures were not mandatory. The data also indicate some degree of contamination to the control group; 7 to 9 percent of control households report attending at least one accompanying measure session, which is possible, as nothing prevented households in Control communes from attending a session in a neighboring Treatment commune. Out of all the sessions offered before the midline data collection, treatment households in both samples report attending about 4 sessions, on average. This number ranges from about 6 sessions attended in Sikasso to about 1 session attended in Mopti, consistent with the phasing, but reflecting a relatively low share of attendance, assuming that all sessions shown on the phased implementation calendar were, in fact, offered (ranging from about 30 scheduled in Sikasso to about 6 scheduled in Mopti before midline).

In terms of targeting, although the Jigisémèjiri program did not restrict the number of household members that could attend the accompanying measures sessions along with the main beneficiary, in about 60 percent of treatment households that attended any accompanying measure session, only the household head ever attended. In only about 10 percent of cases, the household head and spouse both ever attended, and in about 12 to 15 percent of cases, the mother of the index child in our sample ever attended, which has relevance for the child nutrition outcomes we explore.

Based on self-reports, among treatment households that attended at least one accompanying measure session, their attendance on each of the specific subthemes is, on average, above 60 percent. Given the relatively low number of sessions that households reported attending conditional on any attendance, this suggests that many households may have selectively attended only one session per each group of subthemes or that they may have overreported the number of subthemes to which they were exposed. Alternatively, they may have misunderstood the number of sessions to be specific to the AM session linked to the cash distribution and not the other sessions. For each theme, reported attendance is higher in the panel sample than in the cross-section sample, which may reflect that households in the cross-section sample that have a child 6–24 months are more time constrained. Reported attendance varies slightly across the subthemes; it is highest for the first subtheme (on the use of cash transfers for essential household needs), then decreases and stabilizes at a quite high proportion of treatment household for the remaining topics.

***Program impacts on household-level outcomes*:** Following the conceptual framework, we begin with household-level first-order outcomes: household resources (which we do not have a direct measure of and therefore do not analyze directly) and transfers in and out of the household. Findings show that, in response to receiving the Jigisémèjiri cash transfers, treatment households increase the transfers they give out to others (voluntarily or out of obligation/compulsion). Among treatment households, the intervention causes a 6 percentage-point increase in the share reporting that they gave any cash transfers to family, friends, and others in the village in the past 6 months; the average amount given comes to approximately 2,490 FCFA from each of the 11 percent of households reporting cash transfers out at midline, or about 4 percent of the cash transfer value received by these households over 6 months. The intervention also causes a 5 percentage-point increase in the share of treatment households reporting any food transfers out to family, friends, and others in the village; this represents a 56 percent increase in the share reporting food transfers out, relative to the 9 percent reporting the same in the control group at midline. These findings indicate that some treatment households do not keep the "full" transfer, implying that over all, they receive less from the intervention than was directly delivered to them. While the amount of cash transfer being shared is small, and sharing likely has positive implications for the community, this finding may play a role in the magnitude of effects found on second- and third-order outcomes.

We next turn to household-level second-order outcomes: consumption, as well as savings and investments. There are no statistically significant impacts on the value of consumption from the combined CT and AM intervention; however, point estimates are considerably higher for households that more recently received transfers. In addition, the CT and AM intervention significantly increases the probability of a household purchasing cereals and maize in bulk. These findings suggest a "lumpy" pattern of consumption expenditures, with more spent on purchases (including bulk food) immediately after receiving the transfer than spent several weeks later. Thus, our results suggest that we do not find statistically significant average effects on consumption in part because the midline data were collected on average 66 days after the last transfer and for many households in the sample, the majority of expenditures from transfers occurred prior to the recommended 7 or 30 day recall period for consumption measures. This also implies that households

may have experienced increases in consumption due to the transfers soon after receiving them, but the increases in consumption faded out before the full three months that transfers were intended to cover.

The combined CT and AM intervention also significantly increases household savings and investment. The proportion of households that have any savings increases by 3 percentage points. Given the low rates of saving of households in our sample, this translates to a meaningful 50-percent increase relative to the control. The intervention increases the proportion that invests in small animals by 6 percentage points, with an average amount of about 3,352 FCFA spent in the past 12 months among those who invest. It also increases the proportion that invests in livestock by 11 percentage points, with an average amount of 23,463 FCFA spent in the past 12 months among those who invest. These findings are consistent with households' direct reports of using a substantial share of transfers on "agricultural inputs and investments" (which they may consider to include investments in livestock) and with evidence showing that larger and less frequent transfer amounts lead to greater use of transfers for investment in assets (Haushofer and Shapiro 2016).

We last consider household-level third-order outcomes: household food security and household dietary diversity, poverty, and asset ownership. The combined CT and AM intervention significantly improves households' food security and hunger situation over the preceding four weeks and increases the proportion of households classified as food secure or having little to no hunger. The program reduces household food insecurity by 1.88 points as measured by the Household Food Insecurity Access Scale, representing a 27-percent reduction in food insecurity. It increases the proportion of households classified as food secure by 11 percentage points and decreases the proportion classified as severely insecure by the same magnitude, representing approximately a 38-percent change relative to the midline control mean. The program also leads to a decrease of 0.25 points in the Household Hunger Scale, representing a 52-percent increase from the midline control mean. Changes across the distribution reveal a 7-percentage point increase in the proportion of households classified as having little-to-no hunger and a decrease of the same magnitude in the proportion of households classified as having moderate hunger. These findings are consistent with the self-reported use of the transfer on food and the large increases in the probability that households purchased cereals in bulk in the past 3 months. This suggests that findings on food consumption aggregates are not capturing the full story, likely due to effects fading prior to the recall period for food consumption measures.

The combined CT and AM intervention also significantly improves household dietary diversity as measured by the Dietary Diversity Index (DDI) and Food Consumption Score (FCS). The program increases the DDI by 1 item or nearly a 7-percent increase relative to the control group, and increases the FCS by 3.7 points or a 6-percent increase relative to the midline control mean. In terms of food groups, increases on the extensive margin are found particularly for fruits (a 23-percent increase relative to the control mean), as well as for sweets, and spices/condiments and beverages; increases on the intensive margin are also found particularly for fruits (0.51 more days consumed), as well as for vegetables and main staples. Impacts on other high-value nutritious foods, such as dairy, meat, and seafood, are positive but statistically insignificant.

There are no statistically significant impacts of the combined CT and AM intervention on poverty indicators, based on international poverty lines or the national poverty line, using the poverty headcount ratio, the poverty gap, or the poverty gap index. The poverty gap indicates that the average beneficiary household was so poor in the absence of intervention that the transfer amount was insufficient to lift it above the national poverty line. However, point estimates for program impacts on poverty are negative. Reductions in poverty are larger for households that more recently received transfers, although still not statistically significant, consistent with the findings on consumption expenditures (which are used to construct the poverty indicators). This implies that some households may have experienced reductions in

poverty due to the transfers soon after receiving them, but the reductions faded out before the full three months that the transfers were intended to cover. Thus, overall, our results suggest that we do not find statistically significant average effects on poverty in part because the size of the transfer amount relative to the poverty gap was not sufficiently large on average and in part because, for many households in the sample, the majority of expenditures from transfers occurred prior to the recommended recall period for consumption measures.

The combined CT and AM intervention significantly increases household asset holdings. Relative to the midline control mean, the intervention increases the value of livestock ownership by approximately 61 percent, the value of transport assets by approximately 64 percent, and the value of the total assets by approximately 19 percent. These are meaningful increases in asset ownership and are consistent with findings of increased savings and investments (particularly in livestock). Our evidence suggests that the combined intervention led treatment households to significantly increase their asset base, putting them on a pathway to move sustainably out of poverty.

***Program impacts on child-level outcomes*:** Following the conceptual framework, we begin with the child-level first-order outcome (that does not pertain to PNP): maternal knowledge of child nutrition and health. The combined CT and AM intervention has no statistically significant impact on a summary maternal knowledge score on health and nutrition. Mothers at both baseline and midline answered between 5 and 6 of 10 questions correctly, on average. There is also no significant difference in impacts on responses to the sub-questions contributing to this overall knowledge score, except for the appropriate age to introduce liquids to the infant; mothers in the treatment group are 13 percentage points more likely to answer this question correctly than the control group. The lack of impact of the AM intervention on maternal knowledge of child nutrition and health may be explained by limited direct exposure of mothers to AM. Although the first two quarterly AM sessions covered the themes of breastfeeding practices and complementary feeding, and although beneficiaries of all regions but Mopti were scheduled to be exposed to these messages before the midline data collection, this information may not have reached the mother of the child (typically the child's main caregiver). As mentioned above, only 60 percent of treatment households ever attended any AM session, and among those that ever attended, the mother of the index child attended in only 15 percent of cases in the cross-section. Typically, the household head alone attended, which would require transmission of relevant information within the household to the child's mother. Alternatively, if households misunderstood the questions on AM sessions to be related only to the session linked to the cash distribution, mothers' exposure may have been higher than reported, but the exposure was not enough to improve their knowledge.

We next turn to child-level second-order outcomes (that do not pertain directly to PNP): infant and young child feeding practices (as measured by the World Health Organization (WHO) indicators and as measured by dietary diversity), as well as stimulation practices. The combined CT and AM intervention causes an 11-percentage-point increase in the prevalence of timely introduction of (semi-)solid and soft foods, as well as a 3.6-percentage-point increase in the prevalence of children 6–23 months consuming a minimally acceptable diet (defined by consuming 4 food groups and the recommended number of meals for a specific child age) relative to the control. It is likely that this change in behavior is due to increased resources more than increased knowledge, as more than half of mothers did not know that breast milk intake needs to be complemented with (semi-)solid and soft foods at 6 months of age, and the program did not increase this knowledge. Results show a surprising reduction in the proportion of children that was ever breastfed, by 2.5 percentage points; however, the prevalence of ever breastfed children remains quite high (≥95%) in both study groups at midline. Exclusive breastfeeding could not be measured in this sample because the

cross-sectional sample ranged from 6 to 23 months of age; we did not see any improvement in continuous breastfeeding in older children of 20–23 months, although the small sample of children in this age group reduces our power in detecting impacts.

We do not find any meaningful impact of the combined CT and AM intervention on the mean child dietary diversity score. Breaking down the dietary diversity score into the seven food groups that contribute to the score, we find a small 3-percentage-point decrease in the prevalence of legume and nuts consumption in the treatment group compared to the control group. The lack of impact of the combined CT and AM intervention on the child's DDS may be surprising, given that at the time of the midline survey, beneficiaries of all regions (except for Mopti) were exposed to AM-related group counselling on complementary food practices and, moreover, had access to more resources, allowing for the purchase of nutrient-dense foods that would benefit the young child. It is again possible that the targeting of the CT and AM to household heads, and the limited exposure to AM in general, played some role in limiting impacts.

The combined CT and AM intervention also had no significant impact on child stimulation practices, as measured with 5 key questions used in the Multiple Indicator Cluster Survey to describe interactions between children and their caregivers. This lack of impact could again be explained by the low direct exposure of mothers to the AM component.

We finally consider child-level third-order outcomes: child health status (and associated health-seeking behaviors, which are second-order but analyzed with health status due to how they were asked in the survey) and child nutritional status. The combined CT and AM intervention causes a 4.4-percentage-point reduction in the prevalence of diarrhea in children 6–23 months but no other reduction in child morbidity . The small impact on child diarrhea does not appear to be associated with improvements in caregiver treatment practices in case of child diarrhea, in terms of dietary mitigation strategies, use of oral rehydration solution or zinc, or provision of more liquids and foods. The combined intervention also makes caregivers 10 percentage points more likely to seek treatment if any of the mentioned symptoms occurred in their 6–23-months-old index child. However, there is no program impact on the type of health-seeking behavior; more than half of respondents in both groups report seeking treatment primarily in the informal health sector.

The combined CT and AM intervention also does not result in any impact on child nutritional status and growth. Child anemia, stunting, and wasting remain very high at midline. Several studies that have assessed the effects of CT alone on child nutritional status in Sub-Saharan Africa have similarly found no impact (e.g., Houngbe et al. 2017 in Burkina Faso, Seidenfeld et al. 2014 in Zambia, Haushofer and Shapiro 2013 in Kenya). Given evidence that the AM in this intervention had limited effects on first- and second-order outcomes, such as knowledge or practices related to child feeding practices and health, it is not surprising that there is no meaningful impact on the third-order outcomes of child health and nutrition status.

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Acronyms

ANCOVA	Analysis of Covariance
BPE	bilateral pitting edema
CT	cash transfer
DD	Dietary diversity
DDI	Dietary Diversity Index
DFID	Department for International Development
FCS	Food Consumption Score
FMPOS	Faculty of Medicine, Pharmacy, and Odonto-Stomatology
HDDS	Household Dietary Diversity Score
HFIAS	Household Food Insecurity Access Scale
HHS	Household Hunger Scale
IFPRI	International Food Policy Research Institute
IRD	Institut de recherche pour le développement
IYCF	infant and young child feeding
AM	accompanying measures
MUAC	mid-upper arm circumference
NGO	nongovernmental organization
OLS	Ordinary Least Squares
PNP	preventive nutrition packages
PPS	probability proportional to size
CT	cash transfer
TOR	Terms of Reference
WFP	World Food Programme
WHO	World Health Organization
FCFA	Franc Communauté financière d'Afrique
CSB+	Corn Soya Blend Plus

1 Introduction

1.1 Background

In the last two decades, cash transfer (CT) programs have emerged as a popular approach to long-term poverty alleviation. While the main goal of cash transfer programs is to reduce poverty, they also have the potential to improve many development outcomes related to human capital accumulation (Fiszbein et al. 2009; DFID 2011). Cash transfer programs have been both piloted and implemented at national scale across the developing world, reaching approximately 750 million to one billion people (DFID 2011). While the widespread use of cash transfer programs began in Latin America and the majority of existing evidence on their effectiveness comes from Latin American or Asian contexts, their use is increasing in Africa south of the Sahara as well (Garcia and Moore 2012). However, the context and challenges faced in Africa south of the Sahara are different than those faced elsewhere and require context specific designs, as success in one region does not necessarily imply success in another region (Garcia and Moore 2012).

While many studies, mainly in Latin America and Asia, have investigated the impacts of CTs on poverty and food security and have, for the most part, found positive impacts, less is known about the impacts of CTs in Africa south of the Sahara, and, in particular, West Africa. Given that context and socioeconomic characteristics of the beneficiary population may shape impacts, findings from Latin America will not necessarily generalize to West Africa. Moreover, despite the fact that cash transfers have been shown to lead to decreases in poverty and improvements in household food security, impacts on children's nutritional status (including anthropometric measures) are generally small (Manley, Gitter, and Slavchevska 2013). Consequently, policymakers and governments are left with the question of how to design social safety nets, such as cash transfers, to achieve greater impact on children's diet quality, health, and nutritional status.

Progress in combating child undernutrition and micronutrient deficiencies has stalled in many high-burden areas, including West Africa, leading to long-term, irreversible damage to the cognitive and physical abilities of many people in developing countries—and diminishing those countries' economic productivity (WB 2006; Black et al. 2008). Findings from other CT evaluations have suggested that improvements in income alone may not be sufficient to improve nutritional status (Manley, Gitter, and Slavchevska 2013). Cash transfer programs, when combined with direct nutrition interventions, have great potential to accelerate progress in improving child nutrition, especially when targeted to the critical window of opportunity for nutrition (Ruel et al. 2013) — the first thousand days comprising pregnancy, lactation, and the first two years of the child's life (Bhutta et al. 2013; Hoddinott et al. 2013). Combining cash transfers and nutrition interventions targeting this window may have nutritional benefits above and beyond cash only, if a key constraint to improved nutritional status is insufficient knowledge (for example, the appropriate duration of exclusive breastfeeding or the appropriate frequency and diversity of child feeding). Thus, investing directly in nutrition is an integral component of a coherent social protection system aimed at improving nutrition and human capital accumulation in order to prevent the intergenerational transmission of poverty. However, evidence of this is limited. Although there exists considerable evidence assessing the effectiveness of various cash transfer programs, as well as evidence on the effectiveness of various direct nutritional interventions, little research directly assesses the added impact of a nutrition intervention that is combined with cash transfers—in other words, how an integrated cash transfer and nutrition program might compare with a cash transfer-only program.

The overall goal of this research is to generate evidence on an integrated program implemented by the Government of Mali starting in 2014 that includes a combination of cash transfers and targeted nutrition

interventions. The information generated will inform program implementers and policymakers about best options to improve food security and nutrition among vulnerable groups and individuals in West Africa.

Mali is a compelling setting for studying the impacts of a social transfer program on household food security and on child nutrition and health. Although the hunger situation in Mali has improved over the last two decades, hunger is still classified as a "serious" problem, according to the Global Hunger Index classification (Von Grebmer et al. 2014). Malnutrition is also widespread in Mali. In the five southern regions of Mali, as of 2014, approximately 38.3 percent of children 6–59 months were stunted and 81.7 percent were anemic, with 60.8 percent having moderate/severe anemia (CPS, INSTAT et al. 2014).

1.2 Objectives of Filets Sociaux (Jigisémèjiri) program

The goal of the "Filets Sociaux (Jigisémèjiri)" program is to deliver targeted cash transfers to the poor and vulnerable populations in Mali and to improve their human capital accumulation, thereby reducing the intergenerational transmission of poverty. To accomplish these goals, an integrated model—composed of cash transfers (CT), accompanying measures (AM), and preventive nutrition packages (PNP) targeted to children under 5 years and pregnant women—was developed. Once fully rolled out, the program will operate in six regions (Sikasso, Segou, Mopti, Koulikoro, Kayes, Gao) and the District of Bamako, and the cash transfer component will reach approximately 62,000 poor households.

The short-term and long-term objectives of the program are (Samoura 2014)

- *In the short term:* alleviate current poverty and vulnerability by smoothing and increasing consumption of households through targeted and regular CT;
- *In the medium-long term*: improve human capital of children, thereby reducing intergenerational transmission of poverty by combining CT with AM and/or PNP.

The first component—CT—includes timely, predictable, and regular cash transfers for three years to eligible households, which will allow them to smooth and increase their consumption in addition to protecting their assets from shocks. Predictable transfers also enable households to better plan their investments, allowing households to maximize the use of the cash transfers and helping them to make short-term livelihood investments (e.g., in agriculture) as well as long-term investments in human capital (e.g., investing in their children's health and nutrition). Regular transfers over an extended period may also help the household acquire productive and agricultural assets. The second component—AM—provides complementary information sessions that aim to incentivize households to invest in the human capital of their children. The AM is composed of information sessions that encourage using transfers to fulfill essential needs and promote the adoption of good practices related to infant and young child feeding, maternal nutrition, health, education, family planning, hygiene and sanitation, savings, and investment. The AM component started rolling out in study regions as of the 2nd quarter of 2015. The third component—PNP—is a nutrition package composed of fortified food supplements targeted to pregnant women and to children under the age of 5. The PNP component is scheduled to roll out in the 3rd quarter of 2017 and, during the evaluation period, will only be implemented in approximately 20 percent of program communes.

1.3 Objectives of the impact evaluation

In order to inform the Government of Mali and its partners on the effectiveness of the program—in terms of reducing vulnerability to food insecurity of beneficiary households and improving their resources and

nutrition status—International Food Policy Research Institute (IFPRI), in partnership with Institut de recherche pour le développement (IRD), is conducting an impact evaluation. The impact evaluation is being conducted in 5 of the 6 regions where the program is being implemented: Sikasso, Mopti, Segou, Koulikoro, and Kayes. Drawing on the gaps in existing evidence and in response to the TOR, the impact evaluation will address the following **research questions** using a rigorous randomized design:

1) What is the impact of a combined intervention (cash transfers and accompanying measures) on household and child outcomes? In particular, to what extent does the program affect household consumption, food security, and poverty, as well as child nutritional status and health?
2) To what extent does the program improve local knowledge and practices in the areas of children's nutrition, health, and education?
3) What is the *additional* impact (over and above the impact of cash transfers and accompanying measures) of preventive nutrition packages on all of these outcomes, and in particular, on child nutritional status and early childhood development (ECD)?

While the first two research questions will be addressed in this midline report using data collected at midline (in 2016), the last research question will be addressed in a future endline report using data collected at endline (scheduled for 2018) after the roll out of PNP in 2017. In addition to the midline and planned endline data collection, a process evaluation was conducted in the spring of 2017 to shed light on how the features of the program may shape impacts. The combination of the impact evaluation and process evaluation will inform the Government of Mali and its stakeholders on the impact of the "Filets Sociaux (Jigisémèjiri)" program; present a more in-depth understanding for how and why features of the program shaped the impacts; and provide policy recommendations for improvements and next steps.

1.4 Objectives of midline report

The purpose of this midline report is to summarize the context of this study, describe the interventions and evaluation design, and present findings on program impact from the midline data. The midline report is organized as follows. Chapter 2 describes the "Filets Sociaux (Jigisémèjiri)" program. Chapter 3 describes our theory of change for how the program may affect outcomes of interest. Chapter 4 describes the experimental evaluation design used for studying the impact of the program, the sample design, and the methodologies we use to measure impacts. Chapter 5 summarizes the survey questionnaires, the procedure used to obtain ethical approval, the structure of the fieldwork, and the sample on which we estimate impacts. Chapter 6 reports descriptive statistics on beneficiary households' reports of their experience with the cash transfer and accompanying measure components of the program. Chapter 7 presents estimates of the program's impact on household-level outcomes, including the quantity and quality of food consumption, wealth, and poverty. Chapter 8 presents estimates of the program's impacts on mother- and child-level outcomes, including mothers' knowledge and practices and children's nutritional and health status. Chapter 9 discusses these findings and concludes with policy implications.

2 Intervention

2.1 Targeting

Cash transfers provided by the project were intended to reach about a quarter of the chronic food poor population[1] in Mali over three years. During the life of the project, the cash transfer aimed to reach about 62,000 poor households across six regions (Sikasso, Segou, Mopti, Koulikoro, Kayes, Gao), plus the district of Bamako. With an average of seven members per household, approximately 400,000 poor were expected to benefit from the cash transfer. This corresponded to about a quarter of the people living below the food poverty line according to the 2010 household survey data.

Household eligibility was based on a combination of geographical- and community-based targeting mechanisms. In particular, circles and communes were chosen based on geographical targeting, using poverty maps. In total, 18 circles and 106 communes were selected across the six regions plus Bamako. All villages within the communes were then given a quota of households that were eligible, based on population size. Community-based targeting was used to select households within villages. The following criteria were used to select these households, in 2014:

- Food insecurity based on the number of meals eaten and the number of months the household was able to consume from the last harvest
- Agricultural capacity based on land, livestock, and productive assets
- Household size and composition
- Labor and employment

Although cash transfers were targeted, the accompanying measures were not targeted and were available to any household in communes that were selected for the program. The accompanying measures were intended to reach approximately 283,000 households.

Once implemented, preventive nutrition packages (PNP) will occur in 20 program communes that were selected by an expert committee, based on their nutritional needs and the presence of other nutrition programs. Within these communes, pregnant women and children 0–59 months will be targeted.

2.2 Intervention components

The Filets Sociaux (Jigisémèjiri) program consists of cash transfers (CT), accompanying measures (AM), and preventive nutrition packages (PNP). As of the midline survey in August-November 2016, the preventive nutrition packages (PNP) had not been implemented and were scheduled to be rolled out in the third quarter of 2017.

The CT are unconditional transfers of 30,000 FCFA paid every quarter over a 3-year period (10,000 FCFA/month, or USD 18.02/month, which is about 9 percent of beneficiary households' midline consumption for the control group). Heads of household are the main beneficiaries of the cash transfers and are given a beneficiary identity card; a substitute is also identified in case the head of household is not available to receive the money. The CT are paid either via mobile money or banks, depending on the region.

[1] The chronic food poor population is the population below the national food poverty line, which also includes households in chronic food insecurity.

For example, in most of Sikasso (excluding Kibila and Boura), Koulikoro, and Bamako, Orange Money delivers the CT. In Segou and in the remainder of Sikasso (Kibila and Boura only), the CT is delivered by Banque Nationale de Development Agricole (BNDA). In Kayes and Mopti, Banque Malienne de Solidarité (BMS) delivers the CT. In order to receive payments via banks, the beneficiaries or their substitute must present their beneficiary card and identification card to bank agents. For mobile money, an Orange money account is opened for beneficiaries, and beneficiaries receive a free SIM card. Payments are made electronically to mobile accounts, and beneficiaries or their substitutes can withdraw their money at distribution sites upon presentation of their beneficiary card and identification card. In cases where a payment is missed in one quarter, a double payment is made in the following quarter.

For the AM, nongovernmental organizations (NGOs) conduct two training sessions per month in each intervention village. The AM sessions are organized into groups of themes. Each group of themes is covered for a period of 6 months, or 2 quarters. To develop the material for these sessions, the government first designs a curriculum and uses a flip chart to conduct a regional training for the NGO on each group of themes just before the start of the 6-month term for that group. Health center doctors attend the regional trainings to provide information specific to the region. The NGO then adapts the messages to what will be understood by beneficiaries in the local context, translates the material to the local language with the assistance of village-level and circle-level health staff, develops a dissemination strategy (which can include images or radio) that is validated by the government office at the circle level, and trains fieldworkers. Fieldworkers have access to the flip charts developed by the government. The cash transfer distribution is also used as an opportunity to present information, as some beneficiaries who do not attend the sessions are present. In total, four groups of themes had been developed for the first three years of the program (see Table 2.2.1), with different regions receiving different numbers of sessions, depending on when the AM was rolled out.

Table 2.2.1: Accompanying measures topics

Groups of themes	Themes
1	1. Use of Cash Transfer for essential needs
	2. Exclusive Breastfeeding
	3. Prevention of Ebola Virus Disease
2	4. Complementary feeding (including PNP cooking demonstration sessions)
	5. Beneficiaries participation in organizations of the social and solidarity economy (mutual health,, associations and cooperatives)
3	6. Prenatal and postnatal consultations
	7. Nutrition practices of women, in particular pregnant women
	8. Children's rights—Feeding, sick and malnourished children.
	9. Children's rights - Vaccination calendar for children and mothers
	10. Children's rights – Birth registration
4	11. Children's rights – Importance of children and teenagers' education, in particular young girls' education.
	12. Family Economy and stock management
	13. Water –Hygiene and Wash
	14. Respiratory Infections of Children – IRA

	15. Promotion of the free health care services in the health insurance scheme – RAMED for CT beneficiaries.

Once commenced, PNP will also be distributed at roughly the same time as the CT payments, once per quarter over at least a 1-year period. PNP will be composed of fortified food sourced from World Food Programme (WFP) and distributed by local NGOs. For pregnant or lactating mothers, CSB+ (250g per woman per day) and oil (25g per woman per day) will be distributed. For children 6-59 months, CSB++ (200g per child per day) will be distributed.

2.3 Implementation roll out

The Filets Sociaux (Jigisémèjiri) program began in 2014 and was rolled out over time across regions. In all regions the CT and AM will run for at least 3 years, and the PNP will run for approximately 1 year. The roll out of the program in our study regions was the following: CT payments were first rolled out in Sikasso at the end of 2014, followed by Koulikoro and Kayes during the first quarter of 2015, followed by Segou and Mopti during the second and third quarters of 2015. AM was rolled out soon after CT, first in Sikasso, followed by Kayes and Koulikoro in 2015, then in Segou and Mopti in 2016. PNP is scheduled to be rolled out at the same time across all regions in the third quarter of 2017. The baseline survey was conducted before the roll out of the program in five regions of Mali. The midline survey, described in more detail in Section 5, was conducted in August-November 2016. The endline survey is scheduled for 2018. Table 2.3.1 describes the detailed schedule of events between baseline and midline.

Table 2.3.1: Timing of surveys, cash transfers (CT), and accompanying measures (AM), 2014-2016

		2014				2015				2016			
		Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
Sikasso (2è vague)	**Survey**												
	CT				✓	✓	✓		✓✓	✓	✓	✓	✓
	AM						Theme 1		Theme 2		Theme 3		Theme 4
Koulikoro	**Survey**												
	CT				✓	✓	✓		✓✓	✓	✓	✓	✓
	AM							Theme 1		Theme 2		Theme 3	
Kayes	**Survey**												
	CT					✓	✓	✓	✓	✓	✓	✓	✓
	AM							Theme 1		Theme 2		Theme 3	
Segou	**Survey**												
	CT							✓	✓	✓	✓	✓	✓
	AM									Theme 1		Theme 2	
Mopti	**Survey**												
	CT							✓	✓	✓	✓	✓	✓
	AM											Theme 1	

Notes :

= Baseline or midline data collection

✓ = 30,000 FCFA

Theme # = 2 sessions per month covering themes in group #

3 Conceptual Framework

In this section we describe a conceptual framework for how the three components of Jigisémèjiri—unconditional cash transfers, accompanying measures, and preventive nutrition packages—could achieve program objectives related to household welfare, as well as child nutrition and health. Figure 3.1.1 illustrates this conceptual framework, guided by Bastagli et al. (2016). In the framework, we show direct effects triggered by receipt of program components (first-order outcomes), intermediate effects that are consequences of the direct effects (second-order outcomes), and resulting effects on final outcomes of interest (third-order outcomes). If the program components do not have meaningful first- or second-order effects, impacts on third-order effects are very unlikely.

We begin by considering direct effects (first-order outcomes):

- **Unconditional cash transfers** provide financial assistance to beneficiaries with no "strings" attached—that is, recipients can spend the cash in whatever way they wish. The immediate effect is to increase the overall resources available to the household. The resources available to the household will be reduced if the household chooses to or is compelled to share some of the transfer outside the household, such as through voluntary sharing with relatives, friends, and neighbors, or through taxes or other obligations to local authorities; this portion of the transfer will not translate to second- or third-order outcomes within the household, although it may have beneficial effects on the community.
- **Accompanying measures** provide information that encourages use of transfers to fulfill essential needs and promotes adoption of certain practices including those related to child health, child nutrition, and saving behavior. If caregivers in beneficiary households regularly attend these sessions, the behavior change communication messaging may directly affect their knowledge related to infant and young child feeding (IYCF) and child health. Messaging may also affect household decisions on how to use resources, such as to buy more nutritious food or increase savings.
- **Preventive nutrition packages** (PNP), once implemented, will provide knowledge of and access to fortified food to pregnant and lactating mothers, as well as to children 6–59 months. If the PNP distribution is accompanied by broader training on complementary feeding and how to appropriately incorporate PNP into child diets, as indicated by the AM themes, this component may increase caregivers' knowledge related to IYCF and child health as well.

We next consider intermediate effects (second-order outcomes):

- An underlying assumption is that households are budget-constrained, and thus providing more resources will permit them to meet their most pressing needs (Fernald et al. 2012). When cash is transferred in a predictable way directly to households, some of the resources are expected to be used in ways that have immediate effects on household expenditure, such as on food, health and other household needs. It can also affect household behavior related to investments, savings, and credit, if liquidity and credit constraints are alleviated, or if distress sales of existing assets (disinvestments) are reduced. The size and frequency of transfers may affect how transfers are split between these uses; large, less frequent transfers have been shown to increase the share of transfers used toward investment in durable goods and assets compared with smaller, more frequent transfers (Haushofer and Shapiro 2016).
- Children's health care utilization and other child health practices would be expected to increase with increases in household expenditures on health and increased caregiver knowledge related to

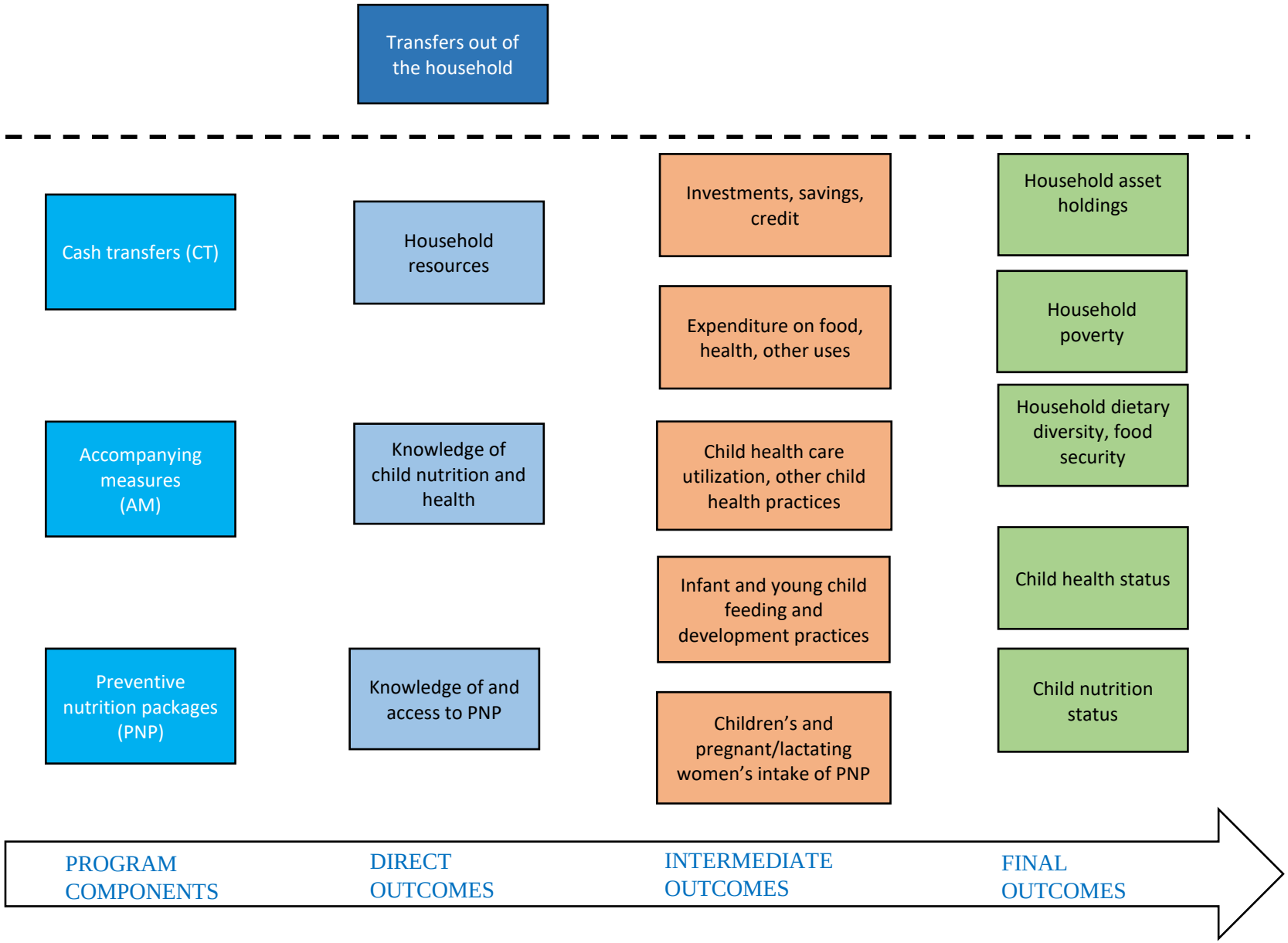
child nutrition and child health. However, this effect depends on the services and quality of services offered by the community health workers and the health centers.

- Infant and young child feeding practices could improve with improvements in caregiver knowledge related to child nutrition and child health as well as with greater household resources, if insufficient knowledge as well as insufficient budget were constraints to these practices prior to the program. Caregiver practices related to infant and young child development may also improve if improved IYCF practices lead to more responsive feeding or more time spent with the child.
- The rationale for providing transfers in the form of fortified food is that it provides households access to a food item that would not necessarily be available to them, thus increasing the consumption of the provided good. Pregnant and lactating women's and children's intake of PNP would be expected to increase, provided that the PNP was in fact distributed to the targeted individuals rather than diverted to other household members.

We then consider final effects (third-order outcomes):

- Household food security would be expected to increase with increases in household food expenditures; household dietary diversity would be expected to increase if households use the transfers to purchase more nutritious foods as a result of the increase in household resources and the increase in knowledge related to nutrition.
- Reductions in household poverty could follow from increases in household expenditures, if transfers cause household expenditures to rise above the poverty lines.
- Household asset holdings would be expected to increase with increases in household investments and savings.
- Improvements in children's health status could follow from improvements in children's health care utilization, other child health practices, IYCF practices, children's consumption of PNP, and pregnant/lactating women's consumption of PNP (if the child is in utero or breastfeeding), as well as reductions in vulnerability related to greater household resources.
- Improvements in children's nutrition status could similarly follow from improvements in children's health care utilization, other child health practices, IYCF practices, children's consumption of PNP, and pregnant/lactating women's consumption of PNP (if the child is in utero or breastfeeding), as well as reductions in vulnerability related to greater household resources.

Figure 3.1.1: Theory of change for effects of the Jigisémèjiri program on household-level and child-level outcomes



4. Evaluation Design

4.1 Experimental design

4.1.1 Experimental design overview

In this section, we describe the design of the overall impact evaluation, to put the midline analysis (the focus of this report) in context of the larger study. The impact evaluation was designed as a cluster randomized trial in order to rigorously estimate the causal impacts of Jigisémèjiri, addressing research questions (1) through (3) in Section 1.3. The overall evaluation will draw on a baseline, midline, and endline survey in addition to a process evaluation (PE). Thus the evaluation will not only address what impacts the program has on various outcomes, but also to shed light on how the features of the program may shape these impacts.

In conceptualizing the impact evaluation design, two important specifications were taken into account: (1) It was possible to retain a control group that would not receive the interventions until after the midline survey; (2) The UTGFS needed to purposively select the communes in which PNP could be delivered, in consultation with a national technical committee. The main reason was that local health centers in some communes were already providing some of these supplements. Therefore the selection of where the government provided PNP could not be randomly assigned at the commune level. However, within the purposively selected communes, villages could be randomly assigned to either receive PNP or not.

Based on these specifications, the following two-stage randomization was designed:

1) **Stage 1:** 96 communes were randomly assigned either to the Treatment Group or to the Control Group.
    a. **Treatment Group**: 76 treatment communes would begin receiving cash transfers (CT) and accompanying measures (AM) after the baseline survey.
    b. **Control Group**: 20 control communes would receive no intervention until after the midline survey (C).
2) **Stage 2:** Within 19 Treatment communes where it was deemed feasible by the government to deliver nutrition packages, villages were randomly assigned to either *additionally* receive preventive nutrition packages (PNP) or to not receive preventive nutrition packages (Non-PNP).

The first-stage randomization of communes to Treatment vs. Control occurred in a July 2014 meeting between government officials and the evaluation team, using colored beads drawn from a bag. For each region, a number of green beads corresponding to the number of intended Treatment communes and a number of red beads corresponding to the number of Control communes were put inside a closed bag. As the name of each study commune in the region was read aloud, officials took turns drawing a bead from the bag without being able to see inside; the commune was assigned to Treatment if green was drawn, and it was assigned to Control if red was drawn.

The second-stage randomization of villages to PNP or Non-PNP occurred in the following months, in public meetings within each PNP-Eligible Treatment commune. To maximize transparency and public acceptance, community leaders, including village chiefs, were in attendance. Government officials supervised these meetings and explained the rationale to all present, with a member of the evaluation team also present in many cases. In each meeting, a number of paper slips corresponding to half the number of total villages in

the commune indicated receipt of PNP in the first year, and a number of paper slips corresponding to the remaining half indicated receipt of PNP the following year. These were placed face down on a table visible to everyone present. As each village name was read aloud, a village leader would come forward and draw a face-down slip of paper which assigned the village to either "PNP" in the first year or to PNP the following year ("Non PNP"), depending on which paper was drawn.

The evaluation design is summarized in Figure 4.1.1.1. Within each region, the breakdown on the number of Treatment and Control communes was chosen to be roughly proportional, so that about 20 percent of each region's communes would be Control, as summarized in Table 4.1.1.1. A map of the distribution of the study communes (Figure 4.1.1.2) reveals that treatment and control communes are well spread out across the five southern regions of Mali.

At midline (August-November 2016), Stage 1 of the randomization had been successfully implemented, with delivery of CT and AM to the treatment group in all five regions. Stage 2 had not yet been implemented, with PNP delivery not scheduled to start in any of the five regions until the third quarter of 2017. Therefore, at midline, the evaluation design will answer research questions (1) and (2) by making the following comparison:

- Comparison 1—**Treatment communes vs. Control communes at Midline**: What is the *absolute* impact on households and children of receiving a combined intervention (CT and AM) relative to no intervention?

At endline, the control group will have started receiving CT and AM in all five regions. To the best of our understanding, Stage 2 will have been implemented, with PNP delivery occurring in the randomly selected villages in all five regions. Therefore, at endline, the intervention design will allow answering research questions (1), (2), and (3) by making the following comparison:

- Comparison 2—**Original Treatment communes vs. Original Control communes at Endline**: What is the *additional* impact on households and children of receiving a combined intervention (CT, AM, and for some households, PNP) for three to four years, relative to receiving it for one to two years?
- Comparison 3—**PNP villages vs. Non-PNP villages at Endline**: In Treatment communes where it is deemed feasible by the government to deliver PNP, what is the *additional* impact on households and children of adding PNP, over and above the impact of CT and AM?

Whereas the baseline survey establishes the reference point of eligible households before the intervention began, the midline survey will reveal the situation of those that received CT+AM (treatment) relative to those that did not (control), after the intervention is underway. The baseline report prepared by IFPRI demonstrates that characteristics of these two groups were balanced at baseline due to randomization; therefore, differences between the two groups at midline can reasonably be interpreted as causal impacts of the CT+AM. Assuming that the control group will also begin receiving program benefits after the midline survey, the endline survey will reveal the situation of those that received CT+AM for three to four years (the original treatment), relative to those that received CT+AM for one to two years (the original control). Again, because characteristics of the two groups were balanced at baseline, differences between the two at endline can reasonably be interpreted as causal impacts of the additional exposure to CT+AM. In between the midline and endline survey, a process evaluation will be conducted, which will allow us to understand details of how the program is being perceived by beneficiaries on the ground. While this component will not make definitive claims about causality, it will help to explain any causal impacts found (or not found)

in the quantitative impact evaluation. This report focuses on results from the midline survey; results from the endline survey and process evaluation will be available in separate reports.

Figure 4.1.1.1: Two-stage randomized evaluation design

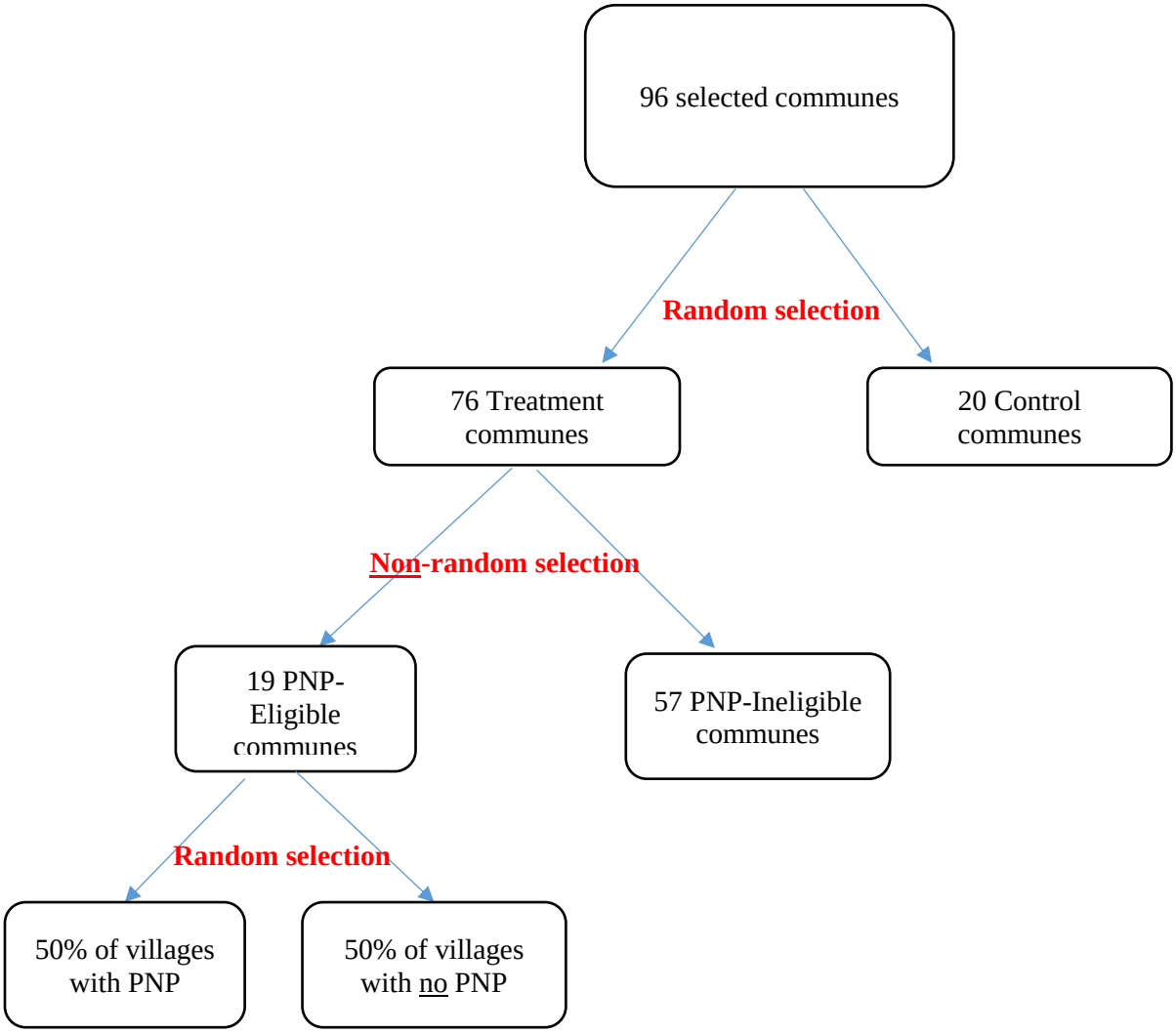
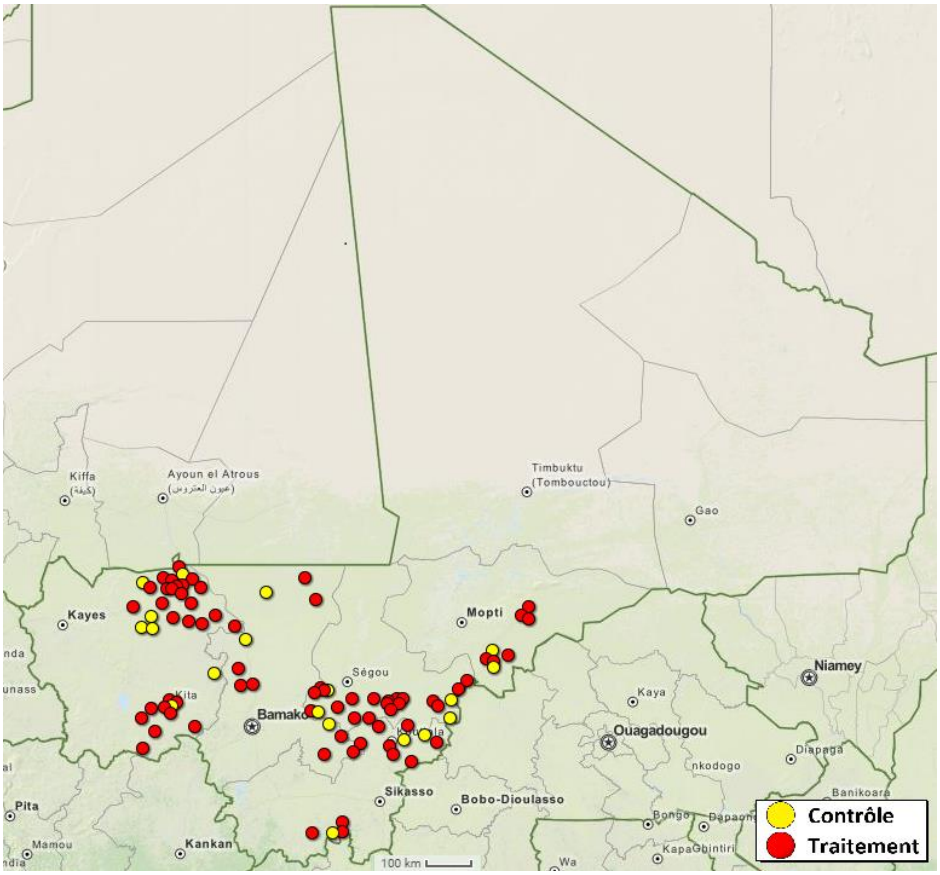
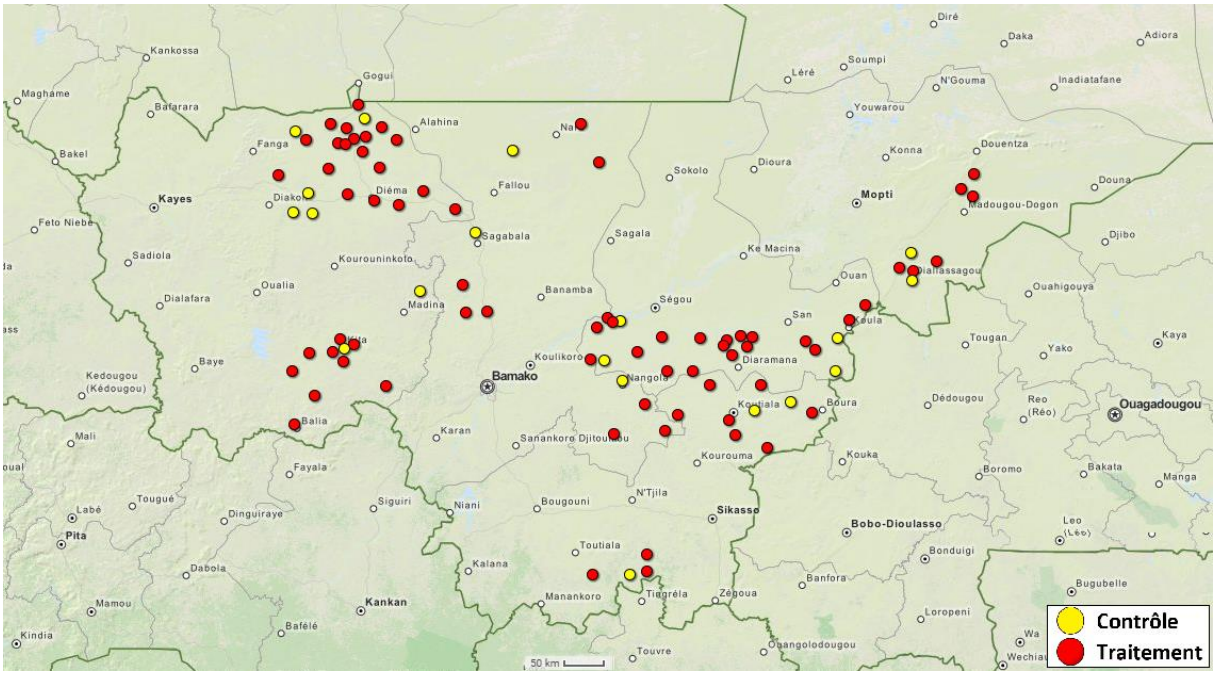


Table 4.1.1.1 Breakdown of treatment and control communes within each region in evaluation design

Region	Total number of communes	Control	PNP-Ineligible Treatment	PNP-Eligible Treatment
Sikasso	13	3	8	2
Kayes	35	7	21	7
Koulikoro	11	3	5	3
Mopti	14	3	8	3
Segou	23	4	15	4
ALL	*96*	*20*	*57*	*19*

Figure 4.1.1.2: Maps of study communes in evaluation design





4.1.2 Rationale for design

The distinct measures of impact can be conceptualized as follows. For the Comparison 1, we can imagine two possible "paths" for a particular household between baseline and midline, depending on whether the household receives no intervention (C) or receives a combined intervention (CT and AM). Figure 4.1.2.1 shows a visualization of these possible paths for a given outcome, with the horizontal axis reflecting time. First, we consider a path in which the household receives no intervention. The red line (Control) gives an example of this possible path, reflecting that the outcome may slightly increase over time despite no intervention, for example, due to general improvements in local infrastructure. Then, we consider an alternate path for the same household, in which after the baseline the household receives the combined intervention. The green line (Treatment) gives an example of this possible path.

If it were possible to observe both possible paths (i.e., Treatment and Control) for a *given* household, it would also be possible to answer the Comparison 1 research questions by directly comparing across them. The difference between the Treatment and Control outcomes at midline would provide the answer to Comparison 1—the absolute impact on an outcome of receiving a combined intervention relative to receiving no intervention. However, the key complication in impact evaluation is that multiple paths for any household are not observed. For a given household, it is possible to observe only one path, depending on which interventions it actually receives. Therefore, in order to develop a measure of counterfactual scenarios for observed households, proxies must be constructed. Due to the randomized design of Jigisémèjiri, households in the Control communes can serve as the proxy for the counterfactual situation of households in the Treatment communes in the absence of the Jigisémèjiri intervention.

In Figures 4.1.2.2 and 4.1.2.3, analogous visualizations are shown for possible paths at endline, illustrating the impacts described in Comparisons 2 and 3. Again, the randomization of intervention components allows for proxies to be constructed, representing the counterfactual case of households not receiving those intervention components. Figure 4.1.2.2 shows the projected path for the original treatment group after receiving the combined intervention for 3-4 years (green path) and the projected path for the original control group (red line) that begins receiving the combined interventions after the midline survey (yellow line). Comparing the groups at endline will provide the answer to Comparison 2—the *additional* impact on an outcome of receiving a combined intervention for three to four years relative to receiving it for one to two years.

Figure 4.1.2.3 shows the projected path for households in the PNP-eligible Treatment Communes (green path). After midline, households from selected villages will start receiving PNP (purple line), while households from non-selected villages will not receive PNP (blue line). Comparing the groups at endline will provide the answer to Comparison 3—the *additional* impact on an outcome from adding PNP, over and above the impact of CT and AM.

Figure 4.1.2.1: Comparison 1—Different paths of outcomes at midline, for Treatment and Control

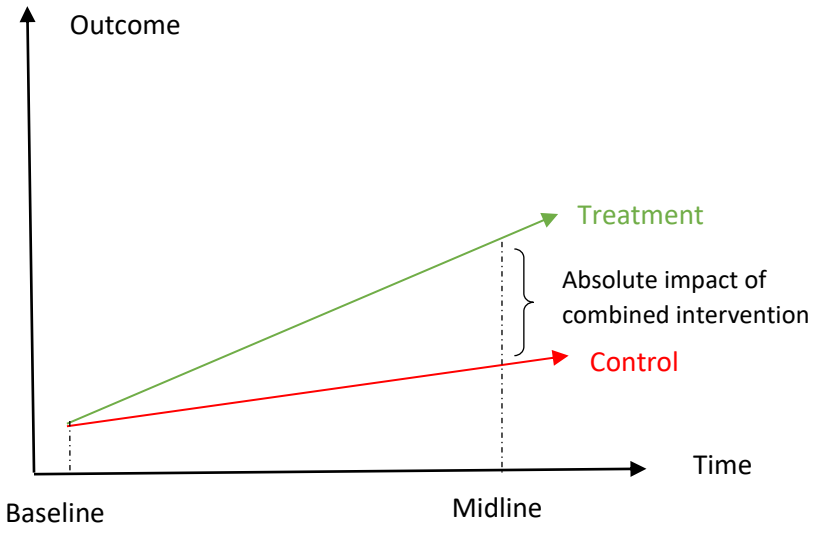


Figure 4.1.2.2: Comparison 2—Different paths of outcomes at endline, for Original Treatment and Original Control

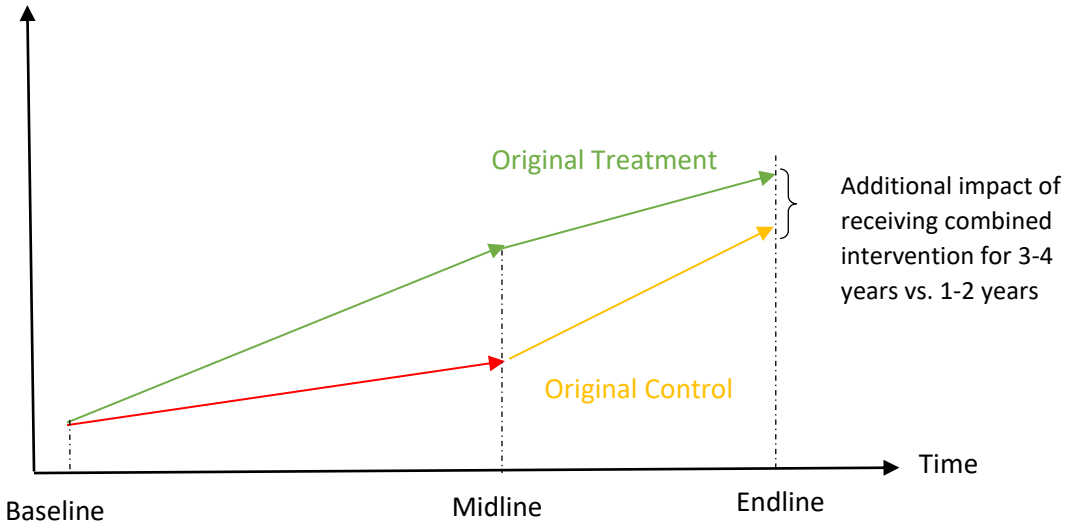
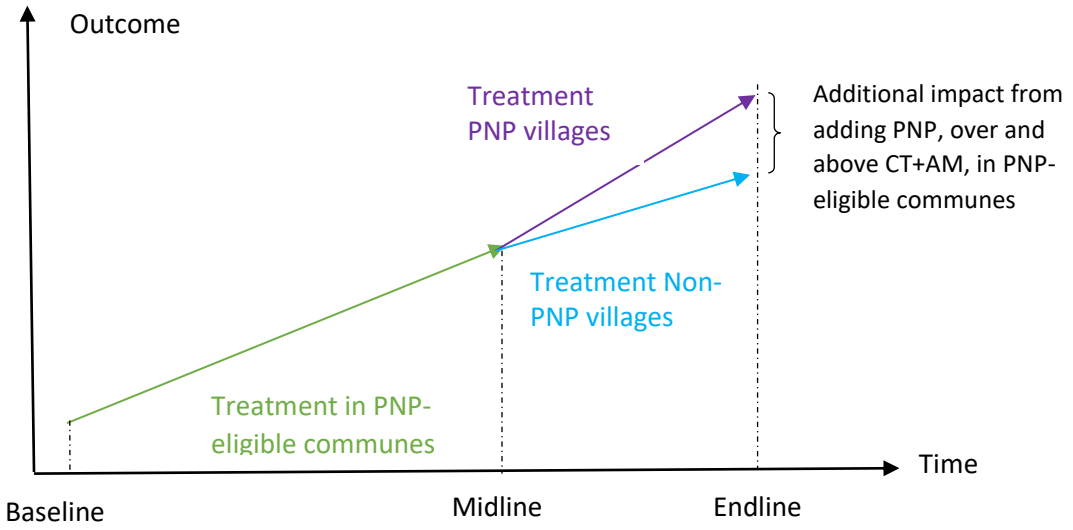


Figure 4.1.2.3: Comparison 3—Different paths of outcomes at endline, for Treatment PNP villages and Treatment Non-PNP villages



4.2 Sample design

4.2.1 Overview

Data for the impact evaluation of Jigisémèjiri consist of a baseline survey that was conducted from September 2014–February 2015, a midline survey that was conducted from August 2016–November 2016, a process evaluation that was conducted from April 2017–May 2017, and an endline survey that is planned for the fall of 2018 (Figure 4.2.1.1).

The **baseline sample** was designed to include households from the 96 communes that are part of the impact evaluation. However, due to security issues, only 90 of the 96 communes were surveyed at baseline. The study sampling frame consisted of households selected as cash transfer beneficiaries that additionally had a child aged 6 to 23 months at the time of the baseline survey. This meant that sample households in both Treatment and Control communes have met all the eligibility criteria for receipt of the cash transfers and are therefore very similar at baseline. The rationale for further restricting the Treatment and Control samples to households with a child aged 6 to 23 months at baseline was to include individuals most likely to be responsive to the interventions. Evidence shows that child nutrition outcomes are most responsive to intervention in the "first 1,000 days" of life (conception to about 2 years of age), but hemoglobin has been found to be a noisy measure for children under the age of 6 months. Given that a primary focus of the Jigisémèjiri intervention is to improve child nutritional status, the sampling frame aims to ensure that all households include at least one child in the most relevant age range.

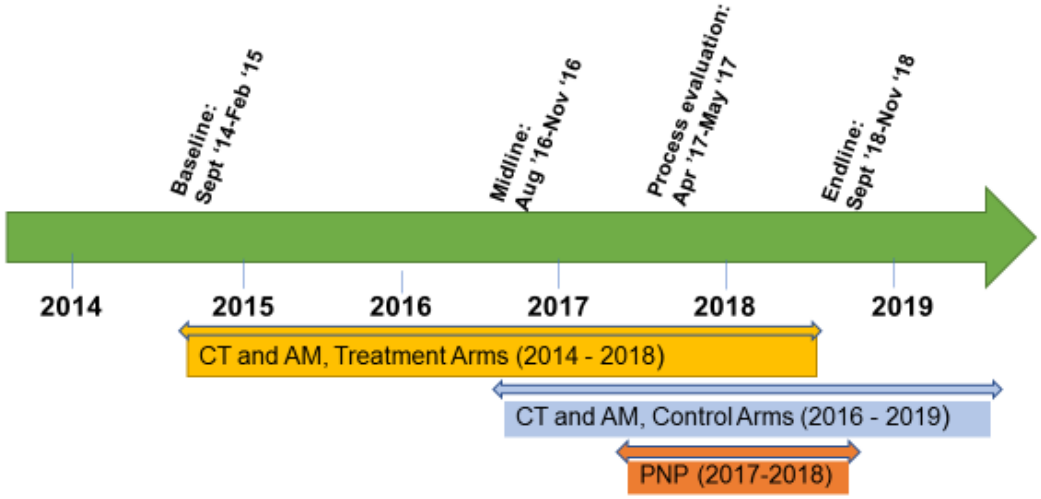
The **midline and endline sample** is (or will be) composed of two parts:

1) A **panel sample** that focuses on capturing impacts at the household level. In each round, this panel sample is a follow-up of a random fraction of the households that were interviewed at baseline. The households in this sample have had maximum exposure to the program. Therefore, they are well-suited to capturing impacts on household consumption, food security, and poverty. In these households, the index child who was aged 6–23 months at baseline is

approximately 28–47 months at midline and will be approximately 52–71 months at endline (assuming 46 to 48 months after baseline). We will follow this index child across survey rounds, but since many of the child-level outcomes will no longer be relevant at the older ages and since many of the index children have experienced shorter exposure to the program during the first 1,000 days of life (for example, none have been exposed in utero or in the first five months of life), we also draw an additional sample ensuring a child in the relevant age range.

2) A **cross-section sample** that focuses on capturing impacts on children aged 6-to-23 months. In each round, the cross-section sample will consist of a new sample of households drawn using the same communes at baseline and the analogous age criteria to the baseline. At midline, these are households selected as cash transfer beneficiaries with a child aged 6-to-23 months at the time of the midline survey; at endline, these will be households selected as cash transfer beneficiaries with a child aged 6-to-23 months at the time of the endline survey. Many of the index children in this cross-section sample have been exposed to the program since conception, and therefore are well-suited for assessing impacts on anthropometry and anemia, as well as impacts on their mothers' knowledge and practices.

Figure 4.2.1.1: Timeline of activities



4.2.2 Power calculations

The sample design for the midline and endline survey is based on "power calculations" that help ensure the ability to statistically detect Jigisémèjiri's impacts on key outcomes. Conducting power calculations for a given outcome requires specifying several parameters: the mean of the outcome, the variability of the outcome (as measured by its standard deviation), the size of the "design effect" or intra-cluster correlation, the number of "clusters" included in each intervention arm, and the number of observations included in each cluster. The calculation returns the magnitude of the maximum detectable impact for each key outcome. All else equal, the larger the sample, the smaller an impact the evaluation is able to successfully detect (meaning that it is more likely to detect an impact of the program). Moreover, outcomes with high variability relative to expected impacts require larger sample sizes for a given detectable impact, since the

more random variation there is in an indicator, the larger the required sample to discern impact from this "noise." The intra-cluster correlation is relevant because of the cluster-based randomization at the commune or village levels. Accounting for the intra-cluster correlation adjusts for the fact that two households sampled within the same commune or village are likely to be more similar than two households sampled across different communes or villages.

Power calculations were conducted for the comparison of outcomes across Treatment and Control communes (First stage randomization), using the baseline data from the Jigisémèjiri evaluation. We assumed that the 90 communes surveyed at baseline would be included in both midline and endline samples. We specified desired statistical power as 80 percent and desired significance level as 0.05, following convention. For the **panel sample**, we specified a sample that would allow detecting a minimum impact of a 30-percent increase in the value of per capita food consumption using an ANCOVA model. For the **cross-section sample**, we specified a sample that would allow detecting a minimum impact of a 0.26 standard deviation increase in the height-for-age Z-score using a single difference. These outcomes are the ones that require the largest samples to detect impact, compared to other household- or child-level outcomes. We then allowed for 10 percent attrition in the panel sample between midline and endline; this means that we adjusted our midline sample to allow the sample size at endline to be sufficient even if 10 percent of households cannot be successfully tracked between the two rounds (for example, due to migration) as is common in panel surveys. The minimum sample needed at endline is 26 households per commune for food consumption outcomes for the panel sample, and 17 households per commune for child-level outcomes for the cross-section sample. The resulting target sample includes **4,140 households at midline** and approximately **3,870 households at endline**. Table 4.2.2.1 shows the proposed breakdown of this target sample across the panel and cross-section.

Table 4.2.2.1: Target sample for midline and endline surveys

Survey round	Panel sample	Cross-section sample	Total sample
Midline survey	2,610 (29 per commune)	1,530 (17 per commune)	4,140
Endline survey	2,340 (26 per commune)	1,530 (17 per commune)	3,870

Power calculations were also conducted for the comparison of child nutrition outcomes across PNP and Non-PNP villages in PNP-Eligible communes (Second stage randomization), using the baseline data from the Jigisémèjiri evaluation. We conducted the power calculation for the **cross-section sample**, since this sample is focused on child-level outcomes. We assumed that 180 villages from PNP-Eligible communes from the baseline survey would be included in both midline and endline samples. We specified desired statistical power as 80 percent and desired significance level as 0.05, following convention. We specified a sample that would allow detecting a minimum impact of a 0.25 standard deviation increase in height-for-age Z-score using an ANCOVA model with an autocorrelation of .8 and intra-cluster correlation of .017. The minimum sample needed was 2 households per village or 360 households in PNP-Eligible communes. This target is taken into account in the sampling procedure described in detail in the section below.

4.2.3 Sampling procedure

The sampling procedure for the **baseline survey** used a two-stage probability proportional to size sampling (PPS) method. In the first stage, villages were sampled based on their "population" size (where population refers to total number of cash transfer beneficiary households), and in the second stage, the same number

of households within each selected village were randomly sampled. With this process, larger villages in terms of total number of beneficiaries had a higher probability of being sampled in the first stage; and in the second stage, beneficiary households in larger villages had a smaller probability of being sampled. The second stage compensated for the first stage, so that each beneficiary household in the commune had the same probability of being sampled.[2] Given that the Second level randomization of PNP was at the village level, sample size calculations for the comparison of child outcomes in PNP villages to Non-PNP villages showed more villages required in PNP-Eligible communes. Thus, the number of villages selected per commune depended on the treatment status of the commune. The final number of villages targeted to be sampled in each commune was 8 in Control communes, 5 in PNP-Ineligible Treatment communes, and 10 in PNP-Eligible Treatment communes. Within each of the selected villages, 5 beneficiary households with children age 6 to 23 months were sampled to meet the baseline sample size requirements (see baseline report for more details). In total, 3,175 households across 96 communes were planned to be sampled at baseline. Due to security issues, only 3,080 households across 90 communes were actually sampled and form part of the baseline data.

The sampling procedure for the **midline survey and planned endline survey** was based on the villages selected for the baseline sample. The procedure was as follows for the two samples:

1) **Panel sample:** The same villages sampled for the baseline survey were sampled for the midline survey.[3] A random sample of households within each village that was interviewed at baseline was selected to be interviewed again at midline. To ensure at least 29 households per commune as prescribed in the power calculations, our rule of thumb was to randomly select 4 households from each village in Control and PNP-Eligible Treatment communes, for approximately 32 households per Control commune (4 households X 8 villages) and 40 households per PNP-Eligible Treatment communes (4 households X 10 villages). Given that only 25 households in PNP-Ineligible Treatment communes were sampled at baseline, all households in PNP-Ineligible Treatment communes were again sampled for the midline survey. The same procedure is planned for the endline survey.
2) **Cross-section sample:** Similar to the panel, the same villages sampled at baseline were sampled for the midline survey. From each village, a new set of households were randomly selected to be surveyed. The sampling inclusion criteria at midline were households selected as cash transfer beneficiaries with a child age 6 to 23 months at the time of the midline survey. To ensure at least 17 households per commune as prescribed in the power calculations, our rule of thumb was to randomly select 3 households from each village in Control and PNP-Eligible Treatment communes for a total of 24 households per Control commune (3 households X 8 villages) and 30 households per PNP-Eligible Treatment communes (3 households X 10 villages). Households in PNP-Eligible Treatment communes were oversampled to ensure there were enough households to compare outcomes for children from PNP villages to those not in PNP villages. In PNP-Ineligible Treatment communes, our rule of thumb was to randomly select 2 households from each village for a total of 10 households per commune (2 households x 5 villages). The oversampling in PNP-Eligible Treatment communes compensated for the under sampling in PNP-Ineligible Treatment communes. The same procedure is planned for the endline survey.

[2] http://www.who.int/tb/advisory_bodies/impact_measurement_taskforce/meetings/prevalence_survey/ psws_probability_prop_size_bierrenbach.pdf

[3] The sampling procedure for villages is described in more detail in the baseline report by Hidrobo et al 2015. Within each of the study communes, village clusters were sampled using PPS methods.

4.3 Empirical methodology

Our approach to estimating Jigisémèjiri's impact on each key outcome will draw on the program's randomized design. The baseline report demonstrated that households in Treatment communes and Control communes were very similar, on average, at baseline, and households in PNP villages and Non-PNP villages were also very similar, on average, at baseline. Given this balance in households across intervention arms prior to any household receiving CT, AM, or PNP, significant differences found between the arms *after* receiving program components can be interpreted as *caused by* the program.

To address the question, "What is the *absolute* impact on households and children of receiving a combined intervention (CT and AM) relative to no intervention (comparison 1)," impact estimation at midline will be conducted using an Analysis of Covariance (ANCOVA) specification. ANCOVA is the preferred specification to adjust for any baseline differences when autocorrelation between baseline and follow-up outcomes is low (McKenzie 2012). Our main household-level outcome—the value of food consumption—is highly variable and has low autocorrelation between baseline and midline. Child-level outcomes in the repeated cross-section also have low autocorrelation, given that different children are being sampled across surveys. ANCOVA estimates will adjust for baseline imbalances according to the degree of correlation between baseline and follow-up, yielding more precise estimates and leading to substantial power gains (McKenzie 2012).

Estimating an ANCOVA model translates to controlling for the lagged outcome variable. We also control for the level of stratification using binary indicators for each region. To be consistent across all outcomes and because of ease of interpretation, we use linear probability models to estimate impacts, clustering our standard error at the level of the commune. We also use sample weights that take into account the oversampling of PNP-Eligible communes.[4] The exact specifications are the following for the panel and cross-section samples:

1) **Panel sample**:

$$Y_{hc1} = \beta_0 + \beta_1 * Treat_c + \beta_2 * Y_{hc0} + \gamma_r + \varepsilon_{hc1},$$

where Y_{hc1} is the outcome of interest for household *h* from commune *c* at midline, and Y_{hc0} is the outcome of interest for the same household *h* from commune *c* at baseline. $Treat_c$ is an indicator that equals one if commune *c* is in treatment group, and γ_r are regional fixed effects. β_1 then measures the impact of the pooled Treatment relative to the Control.

2) **Repeated Cross-Section sample**:

$$Y_{hc1} = \beta_0 + \beta_1 * Treat_c + \beta_2 * \bar{Y}_{c0} + \gamma_r + \varepsilon_{hc1}.$$

The only difference in the repeated cross-section specification compared to the panel specification is that the baseline outcome of interest is $\bar{Y}_{c0}$, which is the mean outcome of interest for commune *c* at baseline.

In all cases, regression analyses are conducted using the statistical software package, Stata 14. For each regression, the results produced by Stata include an estimate of the coefficient (β_1) as well as a "standard error," indicating the noise in the coefficient estimate. The estimate of the coefficient (β_1) represents the

[4] Given that we use probability proportional to size sampling methods, households in our sample are representative at the commune level. However, we over sample PNP-Eligible households in our treatment group. To correct for this oversampling, we use sample weights.

amount of change in outcome, Y, which is due to being assigned to the treatment group. The coefficient (β_1), taken together with its standard error, will indicate both the magnitude of the estimated impact and whether the estimated impact is statistically significant.

This methodology will apply to estimating impacts on any outcome of interest Y. It can be used to estimate impacts on selected "final outcomes" of interest in the evaluation (e.g., poverty or children's nutritional status) or on direct or intermediate outcomes that would lead to the "final outcomes" as explained in the theory of change (Section 3). Therefore, the results of the impact estimation can shed light not only on whether the Jigisémèjiri intervention has an impact on final outcomes, but also on pathways that lead to final outcomes.

5. Midline Survey

5.1 Survey instruments and topics

Between August 2016–November 2016, a midline survey was implemented that consisted of a survey for the panel sample, a survey for the cross-section sample, and a market survey. IRD led the data collection efforts in collaboration with IFPRI. The panel survey focused on household and decisionmaker-level outcomes such as the value of consumption and how decisions are made in the household, the cross-section survey focused on mother- and child-level outcomes such as anthropometrics and mother's nutrition knowledge, and the market survey focused on market prices and unit conversion factors for food items.

5.1.1 Panel and Cross-section surveys

The panel survey is composed of four questionnaires—household, mother-child, decisionmaker, and anthropometry. Household, mother-child, and anthropometry were all part of the baseline survey. The decisionmaker questionnaire was added to the panel survey in order to better understand how the main decisionmaker of the household makes decisions and his/her relationship with other members of the family.

The cross-section survey is composed of three questionnaires—household, mother-child, and anthropometry—which were all part of the baseline survey.

The four questionnaires and the target respondent for each questionnaire are described in more detail below.

- *Household questionnaire:* Given that one of the program's objectives is to improve household welfare, with a particular emphasis on consumption, food security, and diversity of the diet, we collected data on household food consumption, nonfood consumption, and food security. In addition, the study is designed to understand how households use monetary transfers and which household and environmental characteristics determine such use, thus, we also collect information on household composition and dwelling characteristics, in addition to modules on savings, shocks, transfers into and out of the household, and beneficiaries experience with the Jigisémèjiri program. Table 5.1.1.1 lists all the modules in the Household Questionnaire along with whether it was administered in the panel and cross-section survey.
    - *Target respondent:* Whenever possible, the member of the household most knowledgeable on the subject of the respective module was selected as the respondent of the module.
- *Mother-child questionnaire:* Given that another objective of the program is to improve child health, nutrition, and development, this section consists of modules on the index child's health, nutrition, and development. In addition, the study is designed to understand how the program affected intermediate outcomes and thus we also collect information on mother's knowledge, activities, status in the household; infant and young child feeding (IYCF) practices; Water, Sanitation, and Hygiene (WASH) practices; and stress and depression. For the cross-section only, modules on the health, nutrition, and development of the sibling of the index child was also collected. Table 5.1.1.2 lists all the modules in the Mother-Child Questionnaire along with whether it was administered in the panel and cross-section survey.
    - Target respondent
        - *Panel:* The same mother and index child selected in the baseline survey were resurveyed at midline. The index child was 6–23 months at baseline and 28–47 months at midline.

        - *Cross*-section: In each household, one "index child" was randomly chosen out of all children age 6 to 23 months in the household (if there is only one child in this age range, that child is the index child). The mother of the index child (or other primary female caregiver, if the mother is deceased or no longer a household member) was also identified. If the index child's mother had any children age 24 to 59 months, one was additionally randomly selected as the "sibling of the index child."

- *Decisionmaker questionnaire (panel survey only):* The goal of the decisionmaker questionnaire is to better understand how the main decisionmaker of the household makes decisions and his/her relationship with other members of the family, especially his or her spouse. Thus, modules in this questionnaire were administered on the main decisionmaker and her or her spouse and consisted of modules on productive activities, decisionmaking with respect to productive activities and revenue, relationships and disputes, stress and anxiety, cognitive tests, time preference, self-efficacy, and female status. Table 5.1.1.3 lists all the modules in the Decisionmaker Questionnaire along with targeted respondent.
    - *Target respondent:* The decisionmaker questionnaire was administered on the main decisionmaker of the household and his or her spouse. The main decisionmaker was the individual who in practice makes decisions about spending and household income. If the decisionmaker was over 70 years old, then the questionnaire was administered to the individual who makes decisions on household finances in the absence of the principal decisionmaker. The spouse of the decisionmaker also had to be under 70 years of age to be administered the questionnaire. If there was more than one eligible spouse, then the mother of the index child was excluded and the spouse was then randomly selected from the rest.
- *Anthropometry questionnaire:* The weight, height, and hemoglobin concentration of participants were collected using electronic balances, height boards, and HemoCues. In addition, the date of birth of the measured child was also collected.
    - Target respondent
        - *Panel:* index child and mother as defined above.
        - *Cross-section*: index child, mother, and sibling of the index child as defined above.

All four questionnaires were programmed into SurveyCTO by an external consultant and tested by the IFPRI team. Questionnaires were administered by enumerators using Samsung tablets with a computer assisted personal interview (CAPI). The CAPI enabled enumerators to easily access pre-loaded data, follow interview skip patterns according to interviewee responses, and back-up survey data to a cloud server after each day of interviews.

Table 5.1.1.1: Household questionnaire

	Module	Survey
A	Identification	Panel and Cross section
B	Household roster	Panel and Cross section
C	Activities and labor force participation	Panel and Cross section
D	Eligibility for anthropometry and decisionmaker questionnaires	Panel and Cross section
E	Dwelling characteristics	Panel and Cross section
F	Assets	Panel and Cross section
G	Agriculture (crops produced and inputs)	Panel and Cross section
H	Food security	Panel and Cross section
I	Nonfood expenditures	Panel and Cross section
J	Food consumption	Panel and Cross section
K	Shocks	Panel and Cross section
L	Access to services and markets	Panel and Cross section
M	Savings and credit	Panel and Cross section
N	Transfers in and out of household	Panel and Cross section
O	Experience with the program	Panel and Cross section

Table 5.1.1.2: Mother-child questionnaire

	Module	Survey
MB	Mother's knowledge	Panel and Cross section
MC	Mother's activities	Panel and Cross section
MR	Child development, child 6-23 months	Cross section only
MO	Child development, child 36 months or older	Panel and Cross section
ME	IYCF practices, child 6-23 months	Cross section only
MF	Child health, child 6-23 months	Cross section only
MG	IYCF practices, child 24-59 months	Panel and Cross section
MH	Child health, child 24-59 months	Panel and Cross section
MI	Women's emotional status, part 1	Panel and Cross section
MJ	Women's emotional status, part 2	Panel and Cross section
ML	Women's empowerment in Agriculture: Nutrition	Panel and Cross section
MP	Women's empowerment in Agriculture: Access to productive capital	Cross section only
MQ	Women's empowerment in Agriculture: Mobility	Cross section only
MM	Mother's consumption and transfers	Panel and Cross section
MN	Knowledge of Jigisémèjiri	Panel and Cross section
MD	Women's status	Panel and Cross section

Table 5.1.1.3: Decisionmaker questionnaire

	Module	Survey
IA	Identification	Decisionmaker & spouse
IB	Production and Income activities, part 1	Decisionmaker & spouse
IB	Production and Income activities, part 2	Female respondent only
IC	Household's relationship across spouses and co-wives	Decisionmaker & spouse
ID	Cognitive tests, part 1	Decisionmaker only
IE	Cognitive tests, part 2	Decisionmaker only
IF	Stress	Decisionmaker only
IG	Women's Consumption and transfers	Female respondent only
IH	Pro-WEAI	Female respondent only
II	Self-efficacy	Decisionmaker only
IJ	Temporal preference	Decisionmaker only
IK	Women's status	Female respondent only

5.1.2 Market survey

A market questionnaire was designed to collect market-level food data in each commune. This survey was designed to determine the price of a list of food products sold in local markets, the units in which those products were sold, and the equivalence of common units in kilograms. The food products and units listed in the questionnaire were the same as the food items listed in the household food consumption module in order to be able to construct aggregate food consumption measures. Dedicated market enumerators were hired to collect data for the main markets in each commune. When enumerators could not be present during market days, data were collected from shops in the main village of the commune. Due to security issues especially in Mopti and Segou, not all communes were completed. In total, approximately 48 communes were covered by the market survey. Data were initially collected using a paper questionnaire and then entered into a database using the software EpiData 3.1. Once this step was completed, the data were exported into a statistical data analysis software (Stata) to conduct analysis.

5.2 Ethics approval

The initial study design and the survey instruments were submitted for ethics review and approved by both the Ethics Committee of the Faculty of Medicine, Pharmacy and Odonto-Stomatology (FMPOS) of the University of Science, Techniques, and Technologies of Bamako (Ministry of Higher Education and Scientific Research) on August 27, 2014, and the IFPRI Institutional Review Board on August 22, 2014. Both committees were allowed the opportunity to comment on the design and content of the baseline survey instrument before it was fielded.

In 2016, before the midline survey, a request for modification of the study protocol was submitted to both the ethics committee of FMPOS and IFPRI. Modifications consisted of (1) revising the date of the midline survey from Fall 2015 to Fall 2016; (2) adding the cross-section sample; (3) adding a process evaluation component; and (4) adding the decisionmaker questionnaire. Approval of the modifications was obtained on July 27, 2017, from FMPOS and August 3, 2016, from IFPRI.

5.3 Enumerator teams and training

5.3.1 Enumerator training

Enumerator training was organized by IRD and was jointly directed by IRD and IFPRI. Training was conducted in Bamako over 14 days starting from July 30, 2016, and consisted of separate training sessions for household enumerators, anthropometry enumerators, supervisors, and census enumerators. In total, 66 enumerators attended the training. Representatives of the Jigiséméjiri program were present during the opening session of training and provided an overview of the project to enumerators and supervisors.

The first ten days of household enumerator training included a comprehensive participatory discussion of the different modules of the household, mother, and decisionmakers' questionnaires, simulations of interviews using the paper questionnaire first and later the electronic questionnaire (CAPI), and procedures for questionnaire application (such as consent forms). Supervisors also partook in the first eight days of the household enumerator trainings. During the last two days of household enumerator training, separate training sessions were held for supervisors to discuss their specific tasks, the organization of fieldwork, and the uploading of the electronic questionnaires to an online server.

Although the selected anthropometry enumerators had previous experience with the collection of anthropometric data, all selected anthropometry enumerators underwent a comprehensive ten-day training, including sessions dedicated to the collection of hemoglobin measurements. The trainings consisted of an overview of, and practice for, the collection of height and weight information, the principals of hemoglobin measurement and HemoCue administration techniques, and the standardization of height and weight measurements. Three sessions for the standardization of the anthropometric measures were conducted as prescribed by Cogill (2003). During a session, 10 woman-child dyads were measured twice by every anthropometrist and by the trainers. By evaluating the intra- and inter-rater agreement of the results, trainers could improve the technique of every anthropometrist. During the last session, all anthropometrist results showed acceptable precision and the absence of any systematic under- or overreporting compared to golden standard measurement performed by the trainers.

During the eleventh day of training, the panel survey was piloted in the municipality of Siby with households that were not beneficiary households and thus would not be selected for the midline evaluation. IRD and IFPRI staff participated in the field pilot to evaluate questionnaire content as well as enumerator conduct and understanding of the questionnaire. This was followed by two days of more training that included a feedback session, a refresher on challenging modules, and a fieldwork logistics session. On the last day of training a pilot was conducted on the cross-section survey.

5.3.2 Field teams

In total, 56 enumerators (30 males and 26 females) were selected for the fieldwork. Field staff were recruited based on their field expertise, local language skills, performance during the training sessions, and experience in conducting socioeconomic and health surveys. 7 field teams were formed and consisted of 6 household enumerators, 1 anthropometry enumerator, and 1 supervisor responsible for organizing the teams and monitoring progress. One household enumerator in each team was also trained on anthropometrics in order to assist the anthropometrist in measuring children's height, weight and hemoglobin. The teams were led by a field coordinator from IRD.

A team of 6 census enumerators were also formed to conduct a census in each selected village for the cross-section sample.

5.4 Fieldwork (dates + calendar + drop outs due to security issues)

5.4.1 Census

For the cross-section sample, a census was conducted in order to identify a new set of beneficiary households with a child 6-23 months to be surveyed. Census enumerators were sent to each sampled village prior to the midline survey to identify whether or not each beneficiary household had a child age 6 to 23 months (based on a birth certificate or vaccination card).[5] The subset of beneficiary households that did have a child age 6 to 23 months made up the sampling frame. Within this sampling frame, census enumerators randomly selected 3 households per selected village in Control and PNP-Eligible Treatment communes and 2 households per selected village in PNP-Ineligible Treatment communes. The list of selected households in each village was given to the village chief or mayor who would then give the list to the field team supervisor upon arrival for the cross-section survey.

5.4.2 Midline survey

Data collection occurred from August 19–November 18 2016, with a break between September 10-September 17, 2016, to respect the Tabaski holiday, which is traditionally different in terms of activities and food consumption and thus not a recommended time to collect information on households' consumption and labor patterns. Data collection began in Sikasso from August 19–September 10, 2016. After Tabaski, data collection occurred in Koulikoro from September 18 to 26, followed by Kayes from September 26 to October 21, and Mopti from October 21 to November 12, and finally Segou from November 12 to 18. Table 5.4.2.1 reveals the data collection schedule and how it overlaps with the seasons across the five regions. For all five regions, data collection occurred either during the rainy season (Sikasso, Kayes, and Koulikoro) or harvest season (Mopti, Segou). Although the data collection occurred over 4 months, no data collection occurred during the dry season. Households where data collection occurred during the lean season are those surveyed in August and September in Sikasso, Koulikoro, and Kayes.

Table 5.4.2.1: Seasonal calendar and midline data collection for 2016-2017

	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
Sikasso			Dry	Dry	Rain	Rain	Rain	Rain	Rain	Rain		
Koulikoro			Dry	Dry	Dry	Rain	Rain	Rain	Rain	Rain		
Kayes			Dry	Dry	Dry	Rain	Rain	Rain	Rain	Rain		
Mopti			Dry	Dry	Dry	Dry	Rain	Rain	Rain	Rain		
Segou			Dry	Dry	Dry	Rain	Rain	Rain	Rain	Rain		

Legend	
Dry	Dry season
Rain	Rainy season
	Midline data collection
	Harvest of different crops

[5] The sampling procedure for villages is described in more detail in the baseline report by Hidrobo et al 2015. Within each of the study communes, village clusters were sampled using PPS methods

For the panel survey, supervisors were provided with a list of households to be surveyed in each selected village along with location identifiers and the name of the selected mother and index child from baseline. The list was composed of households randomly sampled in each village (detailed in section 4.2.3) that comprised the main sampling list (called "initial"), and "replacement" households in each village that were to be surveyed only if the "initial" households were not found. For the cross-section survey enumerators were provided with the list of selected households from the sampling procedures conducted by the census enumerators (detailed in section 5.4.1). If a village did not have the required number of households with a child 6-23 months to sample, oversampling was done in another village of the same treatment status.

Before beginning a household survey, enumerators read the respondent a brief description of the study that was being conducted, informed them that their participation in the study was voluntary and that they could discontinue participating at any time, and asked whether they agreed to respond to participate in the survey. On average across all regions, the cross-section survey took 3 hours to complete and the panel survey took 4.5 hours to complete.

At the end of data collection in each commune, completed questionnaires were uploaded to an online server and routinely checked for data quality. To this end, a program to perform data checks was developed by IFPRI staff to promptly validate question responses and identify incomplete information through a set of consistency checks. Incomplete or unsatisfactory questionnaires were returned to the relevant supervisors, and errors were corrected by enumerators during brief revisits to identified households.

5.4.3 Anthropometrics and hemoglobin

Measures of height, weight, and hemoglobin concentration were taken twice for all index children, mothers, and siblings of index children (cross-section only) who did not have a physical disability. All field supervisors were provided with referral slips for local health clinics for identified cases of severe anemia (hb<7g/dL) among children and mothers, and acute malnutrition among children defined by a mid-upper arm circumference (MUAC) <125mm, a weight-for-length Z-score (WHZ) <-2, or the presence of bilateral pitting edema.

- *Weight* was measured using electronic balances. When measuring weight, the electronic balance was first placed on a flat wooden platform. The mother was then asked to remove heavy outer garments and shoes and stand in the center of balance, along demarcated lines. After her weight was recorded, the anthropometry enumerator then tared the balance and asked the mother to carry the index child. Once the weight of the index child was recorded, the process was repeated for the sibling of the index child.
- *Height and length* was measured using length boards. When measuring the length of the index child under the age of 2, recumbent length was measured by placing the length board horizontally on a flat surface. The child was then placed supine on the board, and an assistant enumerator was asked to hold the child's head such that the tip of the head touched against the base of the length board and the child's chin was facing straight up. The anthropometry enumerator then placed the rest of the child's body flat against the base of the length board, and gently held the child's knees, to prevent them from flexing, while ensuring that the child's toes pointed directly upward. The child's length was then recorded using the measuring rod. When measuring the height of the mother and siblings over the age of 2, the base of the board was placed on a flat surface, and the back of the board was positioned upright against a flat wall surface. The mother or sibling over the age of 2 was then asked to remove heavy outer garments and shoes and stand with heels, buttocks, and head

positioned flat on the backboard. The mother or sibling was then asked to look straight, and the anthropometric enumerator recorded the height using the measuring rod.

- *MUAC* was measured on children's left arm using a colored measurement tape that identified children's nutritional status (red zone indicating that a child is severely undernourished). To measure the MUAC, the mid-point between the elbow and the shoulder of a child's left arm is determined and the MUAC tape is placed. The measurement is then read from the window of the tape.
- *Bilateral pitting edema (BPE)* is the presence of excessive amounts of fluid in the intracellular tissue. Bilateral pitting edema is diagnosed by applying thumb pressure to the back of the foot. The impression of the thumb will remain for 3-4 seconds when edema is present. A BPE case is identified if both feet are affected. BPE is a consequence of kwashiorkor, a type of severe acute malnutrition. Being undernourished, children with BPE present a higher body weight. Therefore, it is necessary to diagnose BPE when calculating the prevalence of undernutrition or wasting. Children with BPE are automatically classified as severely and acutely malnourished without considering their WLZ scores. The BPE prevalence is typically very low in most settings, but it is strongly associated with mortality.
- *Hemoglobin concentration* was measured using HemoCue devices. To measure the hemoglobin concentration, the participant's finger was first cleaned with alcohol and punctured using an automatic lancet. The first drop of blood was then discarded, and the subsequent drop was collected using a HemoCue microcuvette. The concentration was then analyzed by the anthropometry enumerator using a HemoCue device. The accuracy of all HemoCue device was verified weekly using a reference solution.

5.4.4 Security issues

Baseline data collection did not occur in the circle of Youwarou in Mopti, given the high safety risk following attacks in February 2015. In total, the circle of Youwarou included six study communes—1 Control commune, 4 PNP-Ineligible Treatment communes, and 1 PNP-Eligible Treatment communes. Given continuing security issues, it was decided that the circle of Youwarou would also not be included in the midline data collection. Consequently, the initial number of communes to be included in the impact evaluation was revised down from 96 communes to 90.

Security issues also arose in Kayes during the midline survey. Enumerators were advised not to enter the village, Guiré, due to security concerns. In order to reach households in Guiré, enumerators stayed in a nearby village, Gueniebé, and the field coordinator organized sample households from Guire to be transported to Gueniebé and interviewed in Gueniebé. Households were compensated for transportation costs and other related expenses.

5.5 Sample description

In total, 2,536 households were successfully interviewed for the panel survey and 1,498 for the cross-section survey (Table 5.5.1). Of these, 2,244 in the panel survey and 1,490 in the cross-section survey had complete household questionnaires, mother-child questionnaires, and anthropometry questionnaires; 2,224 households in the panel survey had also complete decisionmaker questionnaires.

Table 5.5.1: Sample size, by questionnaires

	Complete HH	Complete HH+MC	Complete HH+MC+Anthro	Complete HH+MC+DM+Anthro
Panel	2,536	2,282	2,244	2,224
Cross-section	1,498	1,498	1,490	

Note: HH is defined as household, MC is defined as Mother-Child, DM is defined as decisionmaker, and Anthro is defined as Anthropometry. The Anthro questionnaires are considered complete when at least one individual in the household has been measured. The Decisionmaker questionnaires are considered complete when at least one individual in the household has been interviewed.

5.5.1 Panel sample

As described in Section 4.2.3, the midline panel sample was designed to survey 4 households per village from Control and PNP-Eligible Treatment communes and 5 households per village from PNP-Ineligible Treatment communes. However, not all villages in the baseline survey had the required number of households per village or the required number of villages per commune. Thus, the sample list at midline included 2,560, which is slightly less than the designed sample of 2,653 households (Table 5.5.1.1). Of the 2,560 households in the midline sample list, 2,536 households were successfully resurveyed. Of these 2,446 households were part of the initial households that were randomly selected, and 90 were part of the replacement sample. To determine whether there was differential attrition across our treatment groups, we look at how many households surveyed were from the initial sample and compare it to the midline sample list (last column of Table 5.5.1.2). As revealed across intervention arms, 94-97 percent of households in the midline sample list were successfully resurveyed as part of the "Initial sample." In all subsequent analysis on the panel data, we use the 'Initial sample" as our primary sample for analysis.

Table 5.5.1.1: Number of households, by intervention assignment, in midline panel data vs. sample design

	Baseline actual	Midline sample list	Midline actual
Control	770	592	586
PNP-Ineligible Treatment	1,387	1,233	1,215
PNP-Eligible Treatment	923	735	735
Total	3,080	2,560	2,536

Table 5.5.1.2: Attrition Midline

	Midline sample list	Midline actual		Initial sample over midline sample list (percent)
		INITIAL sample	Replacement sample	
Control	592	559	27	94
PNP-Ineligible Treatment	1,233	1,177	38	96
PNP-Eligible Treatment	735	710	25	97
Total	2,560	2,446	90	96

5.5.2 Cross-section sample

As described in Section 4.2.3, the cross-section sample was designed to survey 3 households per village for the Control and PNP-Eligible Treatment communes and 2 households per village for the PNP-Ineligible communes, for a total of 1,526 households (Table 5.5.2.1). Of the 1,526 households to be surveyed, 1,498

were actually surveyed for a success rate of 98 percent. PNP-Ineligible Treatment group had a higher success rate compared to the other two intervention arms, likely due to the lower requirement of eligible households per village.

Table 5.5.2.1: Number of households by intervention assignment, in midline cross-section data vs. sample design

	Midline design	Midline Actual	Midline Actual over Midline design (percent)
Control	456	443	97
PNP-Ineligible Treatment	540	534	99
PNP-Eligible Treatment	530	521	98
Total	1,526	1,498	98

6 Experience with the "Filets Sociaux (Jigisémèjiri)" program

In this chapter, we discuss beneficiaries reported experience with the Jigisémèjiri program. We focus on cash transfers and accompanying measures, since these were rolled out before the midline survey.

6.1 Cash transfers

6.1.1 Treatment adherence

According to the study design described in Section 2, all treatment households were scheduled to start receiving Jigisémèjiri cash transfers on a rolling basis between baseline and midline data collection. Control households were to receive no Jigisémèjiri cash transfers during this period.

Our data indicate high adherence to this study design. In our midline survey, in the panel and cross-section samples, households in both the treatment and control groups were asked if they were aware of the Jigisémèjiri program. If so, they were asked if they believed their household was on Jigisémèjiri's beneficiary list. If they again said yes, they were asked whether they had received any transfers from Jigisémèjiri in the past 24 months. We summarize these responses in Table 6.1.1.1. Here we make the conservative assumption that a household had never received a transfer from Jigisémèjiri if (1) the household's respondent had not heard of Jigisémèjiri[6] or (2) the respondent had heard of Jigisémèjiri but did not believe their household was on Jigisémèjiri's beneficiary list.[7] In these two cases, we assume that the household received no transfers. With this assumption, approximately 97 percent of all households in treatment communes (for both panel and cross-section) reported having received a transfer in the past 24 months. Virtually no household in the control group (in panel or cross-section) reported having received a transfer. Thus, the data indicate that the randomization of the program was implemented as designed, creating a strong basis for evaluation.

Table 6.1.1.1 Proportion of households receiving a Jigisémèjiri cash transfer in past 24 months

	Panel		Cross-section	
Received cash transfer	**Proportion**	**N**	**Proportion**	**N**
Control	0.00	559	0.00	442
Treatment	0.97	1,887	0.97	1,056

6.1.2 Total number of transfers and days since last transfer

According to the implementation calendar, treatment households were to receive one cash transfer of 30,000 FCFA every quarter (or in some cases, two transfers in a single quarter if a previous quarter's transfer was missed). With the phased roll-out across regions as shown in Figure 2.3.1, this came out to a plan of distributing between 5 and 7 transfers between baseline and midline, depending on the region.

[6] Less than 1 percent of treatment households across both samples have never heard of Jigisémèjiri, and between 2 percent and 4 percent of control households in the panel and cross section samples, respectively, have never heard of the program.

[7] Fewer than 3 percent of treatment households report not being on the beneficiary list; 4 percent of panel and approximately 8 percent of cross-section control households also did not believe that their household was a Jigisémèjiri beneficiary.

Findings in our data are consistent with these plans. At midline, in the panel and cross-section, if households reported receiving any transfers from Jigisémèjiri in the past 24 months, they were asked how many in total they had received and what was the amount of the last transfer. Table 6.1.2.1 summarizes responses from households in the treatment group, conditional on the household reporting receipt of at least one transfer in the last 24 months. Reports from panel and cross-section households are similar to each other. Pooled across the five study regions, and conditional on receiving at least one transfer, the mean reported number of transfers received at midline is just over 5 in both samples. Figure 6.1.2.1 shows that the mean number of transfers by study region is also in that range in both samples, between 4 (for the regions of Segou and Mopti) and 6 (for the region of Sikasso). The mean amount received in the last transfer is just over 30,000 FCFA (consistent with some having received a double transfer). Again, these reports indicate that, for beneficiaries that received at least one cash transfer, the transfers were administered largely as planned.

Table 6.1.2.1 Number of Jigisémèjiri cash transfers received and total amount received

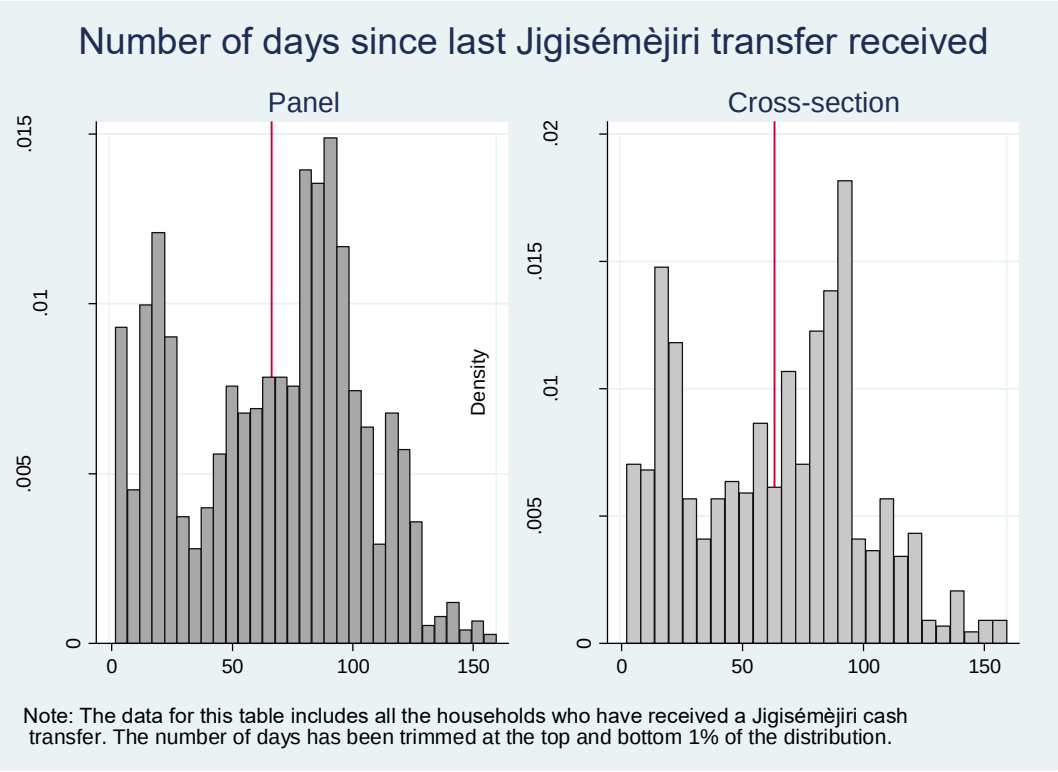
	Panel			Cross-section		
	Mean	Standard error	N	Mean	Standard error	N
Number of transfers received in the last 24 months	5.37	0.04	1,816	5.29	0.06	1,007
Amount received at the last Jigisémèjiri payment	30,270.12	147.83	1,816	30,150.96	106.98	1,007

Note: The data for this table include all the households that are beneficiaries of Jigisémèjiri program and have received any transfer in the past 24 months.

Figure 6.1.2.1 Number of transfers, by region

For beneficiary households that reported receiving a transfer in the last 24 months, the midline questionnaire also asked the date on which the household received its last transfer. Using this date and the date on which the household was administered the questionnaire, we calculated the number of days since the last transfer. Figure 6.1.2.2 reveals that, for households that received at least one transfer, the number of days since the last transfer ranges from 1.5 to 160. The mean time between the last transfer and the midline interview is 66 days for the panel (2.2 months) and 63 days for the cross-section (2.1 months). While each transfer is intended to cover three months, it is possible that households spend most of the transfer soon after receiving it rather than distributing expenditures uniformly over the three months. If this is the case, there are potential implications for what consumption patterns the midline survey will pick up.

Figure 6.1.2.2 Days since the last transfer at the time of midline interview



Note: The data for this table includes all the households who have received a Jigisémèjiri cash transfer. The number of days has been trimmed at the top and bottom 1% of the distribution.

6.1.3 Beneficiaries, recipients, and decisionmakers of the transfer

The cash transfers are targeted in the following way: each beneficiary household has a beneficiary card that lists a main beneficiary; in most cases, this is the household head. The beneficiary card also lists a secondary person who can collect the money in case the main beneficiary is not available. Only the main beneficiary or secondary beneficiary is allowed to receive the transfer on behalf of the household.

Table 6.1.3.1 shows responses from the midline survey on the identity of the main beneficiary, among treatment households that report being a beneficiary. The data confirm that, in both panel and cross-section households, household heads represented the vast majority of individuals listed as the main beneficiaries on the beneficiary card (in 88 percent and 83 percent of panel and cross-section households, respectively), as intended by the program. In small shares of households, the main beneficiary was, instead, the spouse of the household head (3 percent and 4 percent of households, in the panel and cross-section, respectively); the adult child of the household head (2 percent of both samples); or another household member (4 percent

and 3 percent of the panel and cross-section, respectively). There is also a small share of both samples (3 percent of each) reporting that the main beneficiary is a non-household member; this is unexpected and may reflect a household splitting over time or misreporting.

Table 6.1.3.1 Main beneficiary of the Jigisémèjiri cash transfer (based on beneficiary card)

	Panel		Cross-section	
	Proportion	N	Proportion	N
Household head	0.88	1,839	0.88	1,031
Spouse of household head	0.03	1,839	0.04	1,031
Adult child of household head	0.02	1,839	0.02	1,031
Other household members	0.04	1,839	0.03	1,031
Other non-household members	0.03	1,839	0.03	1,031

Note: The data for this table include all the households who are beneficiaries of Jigisémèjiri program.

Table 6.1.3.2 shows responses from the midline survey on who in the household usually collected the transfer, within treatment households that reported receiving at least one transfer. We look specifically at the proportion of these individuals who are household heads, spouses of the household head, or adult children of the household head. Compared with the main beneficiary results presented in the previous paragraph, there is more variation in who collects the Jigisémèjiri transfer. Across the two samples, household heads represent the largest share of individuals receiving the transfer (about 83 percent of panel households and 85 percent of cross-section households). The adult child is the second most often cited individual to receive the transfer (around 7 percent in the panel and about 6 percent in the cross-section), followed by spouse and other household member. Both the spouse and adult child of the household head are more likely to be listed as the individual collecting the Jigisémèjiri transfer than to be listed as its main beneficiary. Unlike the previous table, there are no outside household members collecting the transfer (and are thus not part of Table 6.1.3.2).

Table 6.1.3.2 Main recipient of the Jigisémèjiri cash transfer

	Panel		Cross-section	
	Proportion	N	Proportion	N
Household head	0.83	1,827	0.85	1,023
Spouse of the household head	0.05	1,827	0.04	1,023
Adult child of household head	0.07	1,827	0.06	1,023
Other household members	0.05	1,827	0.05	1,023

Note: The data for this table include all the households who are beneficiaries of Jigisémèjiri program and have received any transfer in the past 24 months.

Table 6.1.3.3 shows responses on who is the main decisionmaker regarding the Jigisémèjiri transfer, in all treatment households that reported receiving at least one transfer. The question in the midline asked, “Who in the household usually takes decisions on how to use the transfers received?” The households interviewed could report up to five individuals that are part of the household. We first analyze the proportion of household where only one person decides. We then classify responses into the following categories: “Household head ONLY,” “Spouse of household head ONLY,” “Adult child of household head ONLY,” “Household head and spouse ONLY,” “Household head and adult child of household head ONLY,” and “Other.” The data show that in the large majority of households (more than 80 percent of households), in the panel and cross section data, only one person is reported as deciding on how the cash transfer is to be

used. In 77 to 78 percent of all cases, this is the household head only. This means that, in both panel and cross-section samples, the household head is the person to receive and decide about the transfer in the large majority of treated households that report receiving any transfers. The spouse or adult child of the household head is the sole decisionmaker in fewer than 5 percent of the treated households for both panel and cross-section samples. In the remaining 14–16 percent of cases where there are two or more decisionmakers, the household head and spouse predominantly make the decisions; however, this accounts for only about 5 percent of households in the panel and cross-section overall. The adult child of the household head plays even less of a role in decisionmaking, taking decisions alongside the household head in 3 percent or fewer households in the two samples.

Thus, the data collected confirm the position of the household head as the primary person engaging with the Jigisémèjiri cash transfer (whether as main beneficiary, recipient, or decisionmaker). Although spouses of the head and adult children also participate in some households, their roles are dominated by the role of the household head.

Table 6.1.3.3 Main decisionmakers on the uses of the Jigisémèjiri cash transfer

	Panel		Cross-section	
	Proportion	**N**	**Proportion**	**N**
Only one person decides	0.84	1,827	0.86	1,023
Household head ONLY	0.77	1,827	0.78	1,023
Spouse of household head ONLY	0.02	1,827	0.02	1,023
Adult child of household head ONLY	0.02	1,827	0.02	1,023
Household head and spouse ONLY	0.05	1,827	0.05	1,023
Household head and adult child of household head ONLY	0.03	1,827	0.02	1,023
Other	0.11	1,827	0.10	1,023

Note: The data for this table include all the households who are beneficiaries of Jigisémèjiri program and have received any transfer in the past 24 months.

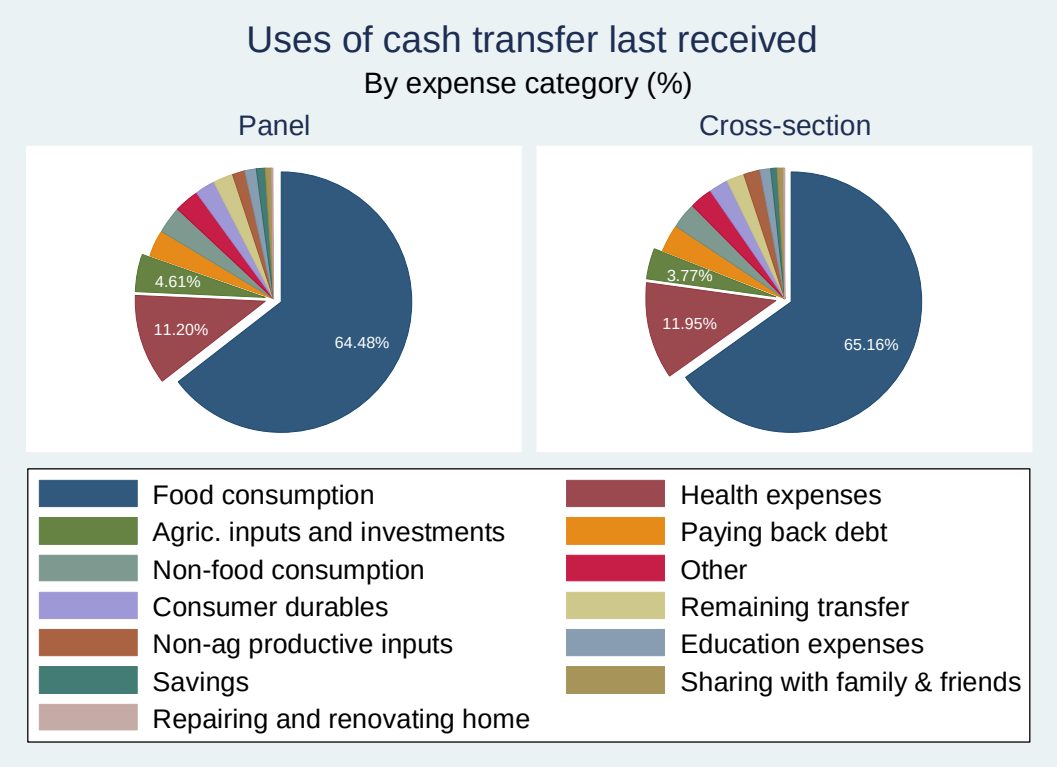
6.1.4 Use of the transfer

We analyze how households report spending the cash transfer, among treatment households that report receiving a transfer in the last 24 months. Households were first asked how much they received in total in their last transfer, then asked how they split up this total among various uses: food consumption, nonfood consumption, health and education, agricultural inputs and investments, debt repayment, consumer durables acquisition, nonagricultural productive inputs, savings, home renovation and repair, and sharing with family and friends.

Figure 6.1.4.1 summarizes household reports on how the last cash transfer received was spent, among treatment households that report ever receiving a transfer. Looking at households' reported uses of the last cash transfer, we see that the largest share of the transfer is reported to be devoted to food consumption, which amounts to approximately 65 percent (64.5 percent in panel and 65.2 percent in cross-section) of the last Jigisémèjiri transfer. The second expense category is health at approximately 11 percent (11.2 percent in panel and 12 percent in cross section). The third largest share of transfer goes to agricultural inputs and investments and it represents 4 to 5 percent of the last Jigisémèjiri transfer. The three largest categories of expenses (food consumption, health and agricultural investments) correspond to approximately 80 percent of the last transfer received (or 24,000 FCFA). Conversely, all other types of expenses make up 20 percent of the last Jigisémèjiri transfer for both panel and cross-section households. It is worthwhile to look more closely at two specific categories: savings and sharing outside the household. Up to 2 percent of the last

transfer is reported to be used for savings and less than 1 percent of the Jigisémèjiri is reportedly being shared with family and friends.

Figure 6.1.4.1 Use of the last Jigisémèjiri cash transfer



6.1.5 Worries regarding the Jigisémèjiri cash transfer

Last, we analyze whether households were worried about their safety when returning home from the distribution point with the cash transfer and whether they were worried about having to share the transfer with non-household members (family and friends) who might know that they just received money. The proportion of such households who worry about their safety or having to share transfer with others is 8 percent in the cross-section sample, and 6 percent of panel households (Table 6.1.5.1). Although the percentage is not large, it is still also not trivial.

Table 6.1.5.1 Proportion of treatment households with worries regarding the Jigisémèjiri cash transfer

	Panel		Cross-section	
	Proportion	**N**	**Proportion**	**N**
Worried about your safety when returning home with the transfer	0.06	1,827	0.08	1,023
Worried about having to share transfer with others who know you received money	0.06	1,827	0.08	1,023

Note: The data for this table include all the households who are beneficiaries of Jigisémèjiri program and received the transfer in the last 24 months.

6.2 Accompanying measures

6.2.1 Treatment adherence

According to the study design described in Section 2, all treatment households were scheduled to receive accompanying measures sessions between baseline and midline data collection. Accompanying measure sessions were not rolled out in Control communes, so their exposure should be minimal; however, there was nothing preventing households from Control communes from attending a session in a neighboring Treatment commune. In the midline questionnaire, we ask about respondents' experience with the AM sessions by first asking whether or not the respondent had heard of the AM sessions of the Jigisémèjiri program. Although enumerators were instructed to clarify what AM sessions we referred to, we note that it is possible that the respondents misunderstood this question and the ones following it to be related only to the AM session that accompanied the cash distribution and not the other AM sessions that were open to the community. Our analysis proceeds with this caveat.

Table 6.2.1.1 indicates moderate adherence to this study design, compared to adherence to the Jigisémèjiri cash transfer treatment. In the panel and cross-section samples, households in both treatment and control groups were asked if they had heard about the accompanying measures of the Jigisémèjiri program. Then, they were asked if anyone in the household had participated in the accompanying measures sessions. Here we make the conservative assumption that a household had never attended the sessions if the household's respondent had never heard of the accompanying measures of Jigisémèjiri. Under this assumption, 60 percent of the treatment households in both cross-section and panel samples have attended at least one accompanying measure session. The data also indicate some degree of contamination to the control group, with 7 to 9 percent of control households having also attended at least one accompanying measure session. Unlike what we observed previously for the Jigisémèjiri cash transfer, we see that there is a lower adherence overall, which may not be surprising, given that the accompanying measures were not mandatory.

Table 6.2.1.1 Proportion attending the accompanying measures sessions in past 24 months

	Panel		Cross-section	
Ever attended accompanying measures	**Proportion**	**N**	**Proportion**	**N**
Control	0.09	559	0.07	442
Treatment	0.60	1,887	0.60	1,056

6.2.2 Total number of sessions

According to the implementation calendar (Figure 2.3.1) in Chapter 2, the AM sessions were phased-in across regions, with Sikasso being the first to receive the AM sessions and Segou and Mopti being the last. Treatment households were to attend two sessions per month for the 4 group of themes established by the government.

In the midline questionnaire, if households reported having attended at least one accompanying measure, they were subsequently asked how many sessions they attended and what themes they attended. Using the initial list of accompanying measures themes, we compute the number of themes exposed to during the sessions. Table 6.2.2.1 presents these findings for treatment households, conditional on the household having attended at least one accompanying measure session during the study period. Cross-section and panel household members attend, on average, approximately 4 sessions. As different topics are covered during each session, the number of themes explored is much larger than the number of sessions. Indeed,

households were exposed to most themes, with a mean of 8.7 themes in the panel and 7.5 themes in the cross-section.

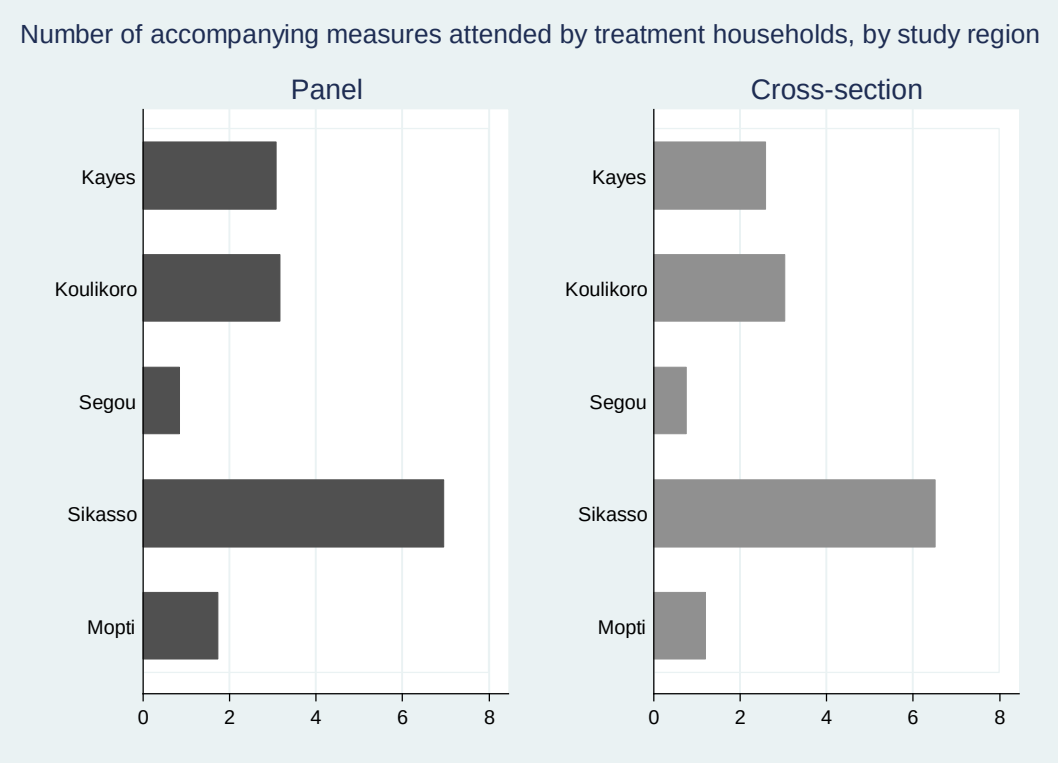
Table 6.2.2.1 Number of accompanying measures sessions attended

	Panel			Cross-section		
	Mean	Standard error	N	Mean	Standard error	N
Number of accompanying measures sessions attended	4.32	0.11	1,134	3.98	0.15	634
Number of different topics attended by household	8.67	0.08	1,134	7.49	0.14	634

Note: The data for this table include all the households who are beneficiaries of Jigisémèjiri program and attended at least one AM session.

Figure 6.2.2.1 presents evidence on the variation in the number of sessions attended across study regions. The difference between regions is consistent with the implementation calendar presented in Section 2; the mean number of sessions is at least twice as large for Sikasso as it is for the other regions, with an average slightly over 6 sessions in both panel and cross-section samples. For the regions of Mopti and Segou, which started implementing the accompanying measures last, the average number of sessions is approximately 1 during the period studied.

Figure 6.2.2.1 Number of sessions attended by treatment households, by region



6.2.3. Attendance and participation to accompanying measure sessions

The Jigisémèjiri program did not restrict the number of individuals who could attend the accompanying measures sessions, thus the main beneficiary and anyone else from the household could attend. Table 6.2.3.1 reports the number of household members that attended any accompanying measure session, conditional on the household being in the Treatment group and having ever attended a session. In both panel and cross-section samples, only one person usually attends the sessions. This represents approximately 76 to 77 percent of treatment households in the panel and cross-section samples. This leaves approximately 20 percent of households across the two samples reporting two members attending the sessions, and fewer than 5 percent reporting three members or more attending.

Table 6.2.3.1 Number of individuals in the household attending the accompanying measures

	Panel		Cross-section	
	Proportion	**N**	**Proportion**	**N**
Only one person	0.77	1,136	0.76	641
Two people	0.18	1,136	0.20	641
Three or more people	0.05	1,136	0.04	641

Note: The data for this table include all the households who are beneficiaries of Jigisémèjiri program and have attended at least one accompanying measure.

In Table 6.2.3.2, we analyze which individuals within the households attended the sessions. In both the cross-section and panel households, it is overwhelmingly the household head only that attends (61 percent and 63 percent, respectively), followed by other (13 to 14 percent), and the household head and spouse (10 percent in both samples). The smallest share of households sends only the spouse of the household head (between 4 percent of the panel households and 5 percent of the cross-section households) or the household head and his/her adult child. For our child nutrition outcomes, we are interested in whether the mother of the index child attended the session. In 12 to15 percent of the cases, the mother of the index child is present among the household members attending the accompanying measures in the panel and cross-section samples, respectively (not shown in tables). While the attendance rates of household members other than heads is low, it is possible that the respondents considered only the AM session that was linked to the distribution of the cash transfer and not the other AM sessions.

Table 6.2.3.2 Proportion of different household members going to accompanying measures

	Panel		Cross-section	
	Proportion	**N**	**Proportion**	**N**
Household head ONLY	0.63	1,136	0.61	641
Adult child of household head ONLY	0.06	1,136	0.06	641
Spouse ONLY	0.04	1,136	0.05	641
Household head and spouse ONLY	0.10	1,136	0.10	641
Household head and adult child of household head ONLY	0.04	1,136	0.04	641
Other	0.13	1,136	0.14	641

Note: The data for this table include all the households who are beneficiaries of Jigisémèjiri program and have attended at least one accompanying measure.

6.2.4 Topics of the accompanying measure sessions

As described in Chapter 2, the AM sessions were grouped into four themes and within each group, subthemes. According to the calendar (Table 2.3.1), all regions should have been exposed to the first theme,

which included subthemes on use of the transfer, breastfeeding, and prevention of Ebola. Exposure to the other theme groups varied by region.

To analyze the different themes the households were exposed to during the AM sessions, we look at attendance by subtheme for treatment households that attended at least one session. For each of these subthemes, the households reported if they attended a session on the theme (presented below in Table 6.2.4.1). The high attendance previously noted in the number of themes explored by treatment households across samples is further confirmed by looking at the attendance by theme; in both panel and cross-section households, attendance for each specific theme is, on average, above 60 percent. Interestingly, for each theme, attendance is higher in the panel sample than the cross-section sample, which may reflect that households in the cross-section sample that have a child 6–24 months are more time constrained. The attendance varies slightly across the subthemes, is highest for the first theme, but decreases and stabilizes at a quite high proportion of treatment household for the remaining topics.

Table 6.2.4.1 Topics of the sessions

	Panel		Cross-section	
	Proportion	**N**	**Proportion**	**N**
Use of cash transfers for essential household needs	0.95	1,136	0.85	641
Prevention of Ebola virus disease	0.79	1,136	0.60	641
Exclusive breastfeeding	0.86	1,136	0.79	641
Complementary feeding	0.89	1,136	0.82	641
Participation to social economy	0.85	1,136	0.73	641
Participation to pre- and postnatal visits	0.87	1,136	0.77	641
Birth registration and civil status	0.87	1,136	0.72	641
Respecting mother and child vaccination calendar	0.86	1,136	0.76	641
Children's rights	0.87	1,136	0.75	641
Development of income-generating activities	0.86	1,136	0.70	641

Note: The data for this table include all the households who are beneficiaries of Jigisémèjiri program and have attended at least one accompanying measure.

7 Impact Estimates on Household-level Outcomes

In this chapter, we assess the impact of the combined CT and AM intervention on outcomes measured at the household level. Given that the household is the unit of interest for these outcomes, we use only the panel survey, in which we track the same household over time. Our analysis follows the conceptual framework in Chapter 3. We do not have a direct measure of total household resources, thus limit our analysis of direct (first-order) outcomes to transfers in and out of the household. We then look at intermediate (second-order) outcomes related to consumption and expenditures, savings, credit, and investments. Last, we analyze impacts on final (third-order) outcomes of food security, dietary diversity, and asset holdings. For all outcomes, we present baseline means and midline means across the pooled treatment and control group, and then we present the coefficient of the impact estimate at midline, which is reported in the "Impact at midline" column. This coefficient, taken together with its standard error, indicates both the magnitude of the estimated impact of the combined CT and AM intervention at midline and whether the estimated impact is statistically significant.

7.1 First order household-level outcomes

7.1.1 Transfers in and out of the household

Although beneficiary households received 30,000 FCFA every quarter, they may not actually "take home" the full amount. The beneficiary household may share some of the transfer or other household resources, such as food, with others outside the household (Jakiela and Ozier 2016). Receiving the intervention transfers could also change the flow of other transfers in and out of the household; the household may receive less other assistance (e.g., from family outside the household, friends, neighbors, NGOs) than it would have received in the absence of the CT. This could be considered "crowding out" of private transfers by the public transfers (Albarran and Attanasio 2003). If these increases in transfers out or reductions in other transfers in occur, the overall effect on total household resources may be smaller than the amount directly delivered by the intervention.

In the baseline and midline surveys, households are asked about various types of cash and food transfers they have given and received in the past 6 months. In Table 7.1.1.1, we compare the reports of transfers in and out of the household across control and pooled treatment households. The table shows the proportion of households reporting that these occurred as well as their average reported value across all households (filling in zero for those that did not report such transfers). For transfers in, we consider those from family and friends (but exclude any category that might have been interpreted to include the Jigisémèjiri transfers); for transfers out, we consider those to family, friends, and others in the village. We see no significant differences between the reports of cash in or food in between the pooled treatment and control. The proportion receiving these transfers in, as well as the average amounts received, are very similar between the two groups in both rounds. The share of households receiving cash goes from about 12 percent to about 20 percent in both groups between baseline and midline, while the share of households receiving food stays at 3 percent in both groups in both rounds.

However, we see significant differences in transfers out. The intervention causes a 6-percentage-point increase among treatment households in the share reporting any cash transfers out to family, friends, and others in the village, and this is significant at the 1 percent level. Relative to the 4 percent of households reporting any cash out in both groups at baseline, and still only 5 percent reporting such transfers out in the control group at midline, this represents more than a doubling of the proportion giving cash out due to the intervention. The average increase in the value of cash given out by treatment households is 274 FCFA

(including those who gave no cash transfers), significant at the 10 percent level. Making a rough calculation, this comes to an average of approximately 2,490 FCFA given by each of the 11 percent of households reporting cash transfers out at midline, or about 4 percent of the cash transfer value received by these households over 6 months.

The intervention also causes a 5-percentage-point increase in the share of treatment households reporting any food transfers out to family, friends, and others in the village, significant at the 1-percent level. This represents a 56-percent increase in the share reporting food transfers out, relative to the 9 percent reporting the same in the control group at midline. The intervention's estimated effect on the average value of food transfers out is positive; however, not statistically significant.

Table 7.1.1.1 Comparison of transfers in and out of household in cash or in food in the past 6 months, control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Cash in from family and friends - any	0.12	0.12	0.19	0.20	-0.01 (0.02)	2,466
Cash in from family and friends - value	3,119.96	3,689.35	9,167.13	8,269.50	1,031.41 (1,603.58)	2,466
Food in from family and friends - any	0.03	0.03	0.03	0.03	0.00 (0.01)	2,446
Food in from family and friends - value	156.06	189.53	292.27	251.79	41.36 (118.48)	2,446
Cash out to family, friends and others in the village - any	0.04	0.04	0.11	0.05	0.06 (0.01)***	2,446
Cash out to family, friends and others in the village - value	287.75	317.77	744.01	500.81	274.37 (146.26)*	2,411
Food out to family, friends, and others in the village - any	0.07	0.07	0.14	0.09	0.05 (0.02)***	2,446
Food out to family, friends, and others in the village - value	331.27	391.05	773.44	628.84	164.80 (169.64)	2,402

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account midline sample weights.

The findings in Table 7.1.1.1 suggest that, in response to receiving the Jigisémèjiri cash transfers, the treatment households increased the transfers they themselves gave to others, in the form of both cash and, to a lesser extent, food. This means that they did not keep the "full" transfer, implying that the increase in resources available for a household's own use is likely less than the transfer amount. While sharing with others in the community reveals strong sharing norms, and the amount shared is small relative to the total transfer size, this finding may play a modest role in the magnitude of effects found on second- and third-order outcomes.

7.2 Second-order household-level outcomes

7.2.1 Household consumption

As the conceptual framework shows, poor households may put some portion of their increased resources toward food consumption. Global evidence shows that many cash transfer programs significantly increase

household food consumption (Fiszbein et al. 2009; Hidrobo et al 2017), but the size and frequency of transfers may affect how transfers are allocated. Large, less frequent transfers, such as those in Jigisémèjiri, have been found to increase the share of transfers put toward uses other than consumption (e.g., investment in durable goods and assets) compared with smaller, more frequent transfers (Haushofer and Shapiro 2016). We assess expenditures on food, focusing on the total value of food consumed from all sources: out-of-pocket expenses for purchased food, as well as the imputed value of food consumed from households' own production or existing stocks, gifts, loans, payments in kind, barter, or harvest from the wild. At baseline and midline, we construct this aggregate value of food consumption following Deaton and Zaidi (2002), using data collected on food consumed from all sources in the 7 days preceding the survey.[8] We note that, while 7-day recall is the recommended recall period, since it improves respondent accuracy on food consumption relative to a longer recall period, food consumption expenditures made several weeks preceding the survey will not be captured. Information on food consumed is collected regarding 74 food items at baseline and 79 food items at midline.[9] To impute prices for food consumed from sources other than market purchases, median prices from purchased food are used. Calculated total values of food consumed in the last seven days are extrapolated to the past 30 days. To minimize the effect of outliers that emerge due to the large number of items added together, enumerator error, and imputations made, two steps are taken. First, for each component of food consumption (the value of food consumed from purchases, the value of food consumed from own stock, and the value of food consumed from gifts and other sources), the top 2% of the distribution is trimmed and replaced with the median. This limits the effect of outliers observed disproportionately in the upper tails of the distributions for each of these components. Second, after these adjusted components are added together into a total value of food consumed in the last 30 days, this total value is trimmed at the bottom and top 1% of the distribution to reduce noise.

Households could also increase expenditures on nonfood consumption needs. We collect information on household expenditures in the past 30 days on 13 items at baseline and 18 items at midline (for which expenditures tend to be frequent) related to health, energy, transport, personal hygiene, and tobacco.[10] At both baseline and midline, we also collect information on household expenditures in the past 12 months on 18 items (for which expenditures are less frequent) related to housing, durable goods, clothing, and social events. Additionally, only at midline, we ask each household member about educational expenses (tuition fees, school supplies, and transport) incurred in the previous school year. Following convention (Deaton and Zaidi 2002), we split nonfood consumption expenditures into health, education, and all other nonfood consumption items. We leave out lumpy infrequent expenditures on marriages, baptisms, new home purchases, and funerals from the other nonfood consumption aggregate. As with food consumption, we take two steps to limit the influence of outliers. First, for each component of nonfood consumption (health expenditures, education expenditures, and all other nonfood consumption expenditures), we trim the top 2% of the distribution and replace with the median. Again, this limits the effect of outliers observed disproportionately in the upper tails of the distributions for each of these components. Second, after these adjusted components are added together into a total value of nonfood consumption in the last 30 days, this total value is trimmed at the bottom and top 1% of the distribution to reduce noise. The trimmed value of food consumption and trimmed value of nonfood consumption are then added together to construct the total value of household consumption.

[8] At both baseline and midline, we ensured that interviews were not conducted within seven days after Tabaski to avoid measuring atypical food consumption patterns.
[9] Food items were added at midline because of feedback from field staff that some common food items were missing from the baseline list.
[10] Several items related to health and energy expenses were added at midline.

Table 7.2.1.1 reveals that the value of all categories of consumption increases from baseline to midline, which is in part due to the new consumption items added at midline. However, there are no statistically significant program impacts on consumption from the combined CT and AM intervention at midline. Estimated program impacts on food consumption, nonfood consumption, and total consumption are positive and fairly large, but statistically insignificant given the large standard errors. Estimated program impacts on health and education expenditures are near zero. To the extent that there are positive point estimates on food consumption, although not statistically significant, Table 7.2.1.2 shows that impacts are larger for the value of food consumed from households' own stock (which includes both the households' own production and any food purchased by the household prior to the past 7 days).

As indicated above, one feature of the 7-day recall period for food consumption and 30-day recall period for nonfood consumption is that these will not capture consumption expenditures made more than seven and 30 days preceding the survey, respectively. The Jigisémèjiri transfers are distributed every three months, and Figure 6.1.2.2 shows that the average time since treatment households received their last transfer was 66 days at midline. If most of treatment households' expenditures occurred immediately after the last transfer was distributed, rather than the transfer being spent evenly over the course of the three months, our measures of consumption may not fully capture these expenditures. To assess this possibility, we conduct two additional analyses. First, we compare impacts among households that had received transfers more recently to impacts among those that had received transfers less recently. Specifically, we construct a variable that indicates whether a household's days since last transfer is above or below the mean number of days for its own region. With the caveat that days since last transfer is not randomly assigned and so the results must be interpreted as suggestive rather than conclusive, Table 7.2.1.3 indicates that estimated impacts on consumption are indeed higher for those who had more recently received the transfer. For example, the point estimate for the program impact on monthly value of food consumption is 6,632 CFA for households whose number of days since last transfer was below the mean for its region (more recent), while it is 1,427 CFA for households whose number of days since last transfer was higher on average for its region (less recent). The former number is consistent with households reporting that they spent about 65% of their last transfer (10,000 CFA per month) on food. Thus, our results give some support to the hypothesis that households spent most of the transfer soon after receiving it. For many households in the sample, expenditures from transfers appear to have occurred prior to the recall period for the consumption measures. Several weeks after a transfer, a large part of households' food consumption may draw from foods that were purchased in bulk soon after receiving the transfer and are now in the households' stock. Table 7.2.1.4 separately estimates impacts on the components of food consumption by whether the household's days since the last transfer were above or below the mean for its region. The point estimate on the value of food consumed from purchases is larger for the subset of households that more recently received transfers, supporting the possibility that households tended to spend transfers on food purchases soon after receipt rather than spending them evenly throughout the three months.

Table 7.2.1.1 Comparison of consumption aggregates in the past 30 days, control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Household size	10.08	9.90	11.65	11.62	-0.18 (0.32)	2446
Value of food consumption in the past 30 days	63,970.35	59,297.66	92,413.71	89,660.06	1,644.50 (5,012.36)	2356
Value of health expenses in the	5,861.25	6,212.08	8,826.09	8,735.94	-26.27	2348

past 30 days					(573.47)	
Value of education expenses in the past 30 days	0.00	0.00	486.78	531.61	-49.09 (66.99)	2398
Value of other non-food consumption in the past 30 days	8,353.20	8,846.53	16,810.30	16,294.84	798.91 (1,198.25)	2348
Value of total consumption in the past 30 days	77,831.16	73,778.86	118,047.38	115,212.16	1,788.14 (6,003.41)	2273

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account midline sample weights. All aggregates are trimmed at the top and bottom 1 percent. Education expenses are not reported at baseline.

Table 7.2.1.2: Comparison of components of food consumption aggregate in the past 30 days, control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Value of food consumed from purchases in the past 30 days	20,902.25	22,954.38	31,923.96	31,906.86	828.92 (2,103.01)	2356
Value of food consumed from own stock in the past 30 days	40,562.23	33,898.33	54,918.21	52,956.29	941.87 (3,542.25)	2356
Value of food consumed from gifts in the past 30 days	2,505.87	2,444.95	5,571.54	4,796.90	714.66 (678.35)	2356

* SE in parentheses clustered at the commune level. * p<0.10, ** p<0.05, *** p<0.01. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account midline sample weights. All aggregates are trimmed at the top and bottom 1 percent.

Table 7.2.1.3: Comparison of consumption aggregates in the past 30 days, control vs. pooled treatment, by number of days since last transfer

	Midline control mean	Midline below mean nb days since transfer	Midline above mean nb days since transfer	Impact at midline – nb days below the mean	Impact at midline – nb days above the mean	N
Household size	11.62	12.32	11.46	0.22 (0.37)	-0.28 (0.35)	2035
Value of food consumption in the past 30 days	89,660.06	96,214.85	92,073.67	6,632.47 (5,557.19)	1,427.37 (5,331.94)	1958
Value of health expenses in the past 30 days	8,735.94	9,317.66	7,809.38	470.00 (612.07)	-1,048.23 (643.48)	1954
Value of education expenses in the past 30 days	531.61	442.13	545.41	-101.14 (72.11)	14.13 (78.38)	1997
Value of other non-food consumption in the past 30 days	16,294.84	18,066.12	16,490.70	1,610.64 (1,368.99)	821.87 (1,261.38)	1954

Value of total consumption in the past 30 days	115,212.16	123,387.40	116,442.23	8,067.86 (6,617.37)	670.75 (6,366.40)	1890

* SE in parentheses clustered at the commune level. * p<0.10, ** p<0.05, *** p<0.01. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account midline sample weights. All aggregates are trimmed at the top and bottom 1 percent. Education expenses are not reported at baseline..

Table 7.2.1.4: Comparison of components of food consumption aggregate in the past 30 days, control vs. pooled treatment, by number of days since last transfer

	Midline control mean	Midline below mean nb days since transfer	Midline above mean nb days since transfer	Impact at midline – nb days below the mean	Impact at midline – nb days above the mean	N
Value of food consumed from purchases in the past 30 days	31,906.86	34,936.22	29,513.09	2,933.30 (2,311.51)	-540.94 (2,267.16)	1958
Value of food consumed from own stock in the past 30 days	52,956.29	54,892.52	57,627.97	3,042.64 (3,969.56)	2,877.03 (3,860.26)	1958
Value of food consumed from gifts in the past 30 days	4,796.90	6,386.11	4,932.61	1,452.81 (759.52)*	182.76 (754.30)	1958

* SE in parentheses clustered at the commune level. * p<0.10, ** p<0.05, *** p<0.01. ""Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account midline sample weights. All aggregates are trimmed at the top and bottom 1 percent.

Next, we look at the impact of the combined CT and AM intervention on the probability of households making bulk purchases of cereals or maize in the last 3 months. In regions where markets are far from households and prices vary by season, bulk purchases of grains may be common. Moreover, in poor households grains make up a large share of food consumption. Consequently, households may use cash transfers to purchase grains in bulk to satisfy their daily caloric needs. Hoddinott et al (2014) provide evidence of this pattern occurring in Niger. Table 7.2.1.5 reveals that 53 percent of control households purchased cereals or grains in quantities greater than 50 kg in the past 3 months. The percentage is significantly higher for households in the treatment group at midline, revealing a 10 percentage point increase as a result of the intervention.

Table 7.2.1.5: Purchase of large quantities of cereals or maize in the last 3 months, control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Past three months, household purchased large quantities of cereals or maize (>50 kg)?	0.38	0.45	0.63	0.53	0.10 (0.04)**	2,446

*SE in parentheses clustered at the commune level. * *p*<0.10, **p<0.05, ***p<0.01, Difference from regression reports the coefficient on treatment of an OLS regression of the outcome of interest on treatment variable, controlling for baseline outcome level and region. All estimates take into account midline sample weights.

Taken together, the findings in this section show that the combined CT and AM intervention did not significantly increase our estimates of the value of consumption, but that point estimates are considerably higher for households that more recently received transfers. In addition, the CT and AM intervention significantly increased the probability of a household purchasing cereals in bulk. These findings suggest a "lumpy" pattern of consumption expenditures, with more spent on purchases (including bulk food purchases) immediately after receiving the transfer than spent several weeks later. Thus, our results suggest that we do not find statistically significant average effects on the value of consumption in part because, for many households in the sample, the majority of expenditures from transfers occurred prior to the recommended recall period for consumption measures. This also implies that households may have experienced increases in consumption due to the transfers soon after receiving them, but the increases in consumption faded out before the full three months that transfers were intended to cover.

7.2.2 Savings, credit, and investments

As shown in the conceptual framework, not all cash received from transfers will necessarily be used for consumption by households. Some portion of the transfers could be put toward savings, repayment of debt, or investments for the future; all of these can reduce poor households' vulnerability. Accumulating savings is an important dimension of building resilience. Savings allow households to smooth consumption in the face of negative income shocks (Deaton 1989; Alderman and Paxson 1994), without resorting to distress sales of assets. They also allow households to build up funds over time toward larger purchases—such as productive assets or consumer durables—that may have long-term benefits (Gertler, Martinez, and Rubio-Codina 2012). Although having access to credit is an important dimension of poverty reduction, holding large amounts of revolving debt can trap households into a cycle of borrowing and repaying interest (Schicks 2014). Making investments in productive assets that can increase future income generation can help households move more sustainably out of poverty (Gertler, Martinez, and Rubio-Codina 2012).

We begin by looking at savings and credit. In both the baseline and midline surveys, households are asked whether they hold any savings or credit in various forms at the time of the survey, including whether they participated in a *tontine* (a group in which each member pays an agreed-upon sum and thereafter receives an annuity). Table 7.2.2.1 shows the estimated impacts of the combined CT and AM intervention on whether households hold savings, owe credit, or participate in *tontines* (which can be viewed as a type of savings). We see that the intervention causes a 3-percentage-point increase in the proportion of households with savings, significant at the 10-percent level. Although this is a small change in absolute terms, it is meaningful. At baseline, only 6 percent of treatment households and 5 percent of control households held savings. Thus, this represents about a 50-percent increase in the share of households with savings due to the combined intervention.[11]

The estimated impact on whether households owe credit is not statistically significant. Although it is negative, reflecting that there was a smaller increase between rounds in the share of households owing credit in treatment households than in control households, the difference is not estimated precisely.[12] Similarly, the estimated effect on participation in any *tontine* is negative, but is not statistically significant.

[11] We note that effects on holding any savings represent impacts at the extensive margin. It is also possible that there could be impacts on the intensive margin, that is, changes in amount saved among the share of households that already had savings at baseline. However, given that such a small share of households hold any saving, we focus on the extensive margin.

[12] This impact captures the extensive margin of whether households hold any debt, although it is possible that there are effects on the intensive margin if the amount of debt is reduced.

Table 7.2.2.1 Comparison of savings and credit behavior, control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Any savings	0.06	0.05	0.10	0.07	0.03 (0.02)*	2,446
Any credit	0.09	0.07	0.13	0.15	-0.03 (0.03)	2,446
Any tontine	0.20	0.25	0.24	0.27	-0.03 (0.03)	2,446

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account midline sample weights.

We next look at impacts on investments. In the midline questionnaire, households were asked whether in the last 12 months, they had made any expenditures on small animals (such as poultry), livestock (such as goats, sheep, cattle, or donkeys), agricultural land, agricultural equipment, or nonagricultural equipment (used for self-employment or a microenterprise, excluding bicycles, motorcycles, or durable goods).[13] If so, they were asked the total amount in FCFA spent on each category. Table 7.2.2.2 compares these responses between the pooled treatment and control groups at midline, using single-difference estimation.[14] The table shows the proportion of households reporting each type of investment as well as its average value across all households (filling in zero for households that did not report such investments). We see that the combined intervention causes large increases in the proportion of households reporting any investment in small animals (6 percentage points, or a 67-percent increase relative to the midline control mean) and any investment in livestock (11 percentage points, or an 85-percent increase relative to the midline control mean). These increases are also reflected in terms of the value of the investments: over the course of the past 12 months, the average treatment household invested 503 FCFA more on small animals and 5,631 FCFA more on livestock. Making a rough calculation based on the proportion that reported investing at all in each, on average, this comes out to approximately 3,352 FCFA more in the past 12 months on small animals for the 15 percent of treatment households that invested in them, and approximately 23,463 FCFA more in the past 12 months on livestock for the 24 percent that invested in them. Compared against the size of the transfers over 12 months (about 120,000 FCFA), these increases in investments represent meaningful shares of the total, among those who make them. We note that results also suggest a 1-percentage-point increase in investment in nonagricultural productive equipment, significant at the 10-percent level; however, this finding is less strong, given its small magnitude as well as the fact that there is no significant change in the value of investment in nonagricultural productive equipment.

Table 7.2.2.2 Comparison of investment behavior and investment value, control vs. pooled treatment

	Midline mean T	Midline mean C	Impact at midline	N
Any investment in small animals	0.15	0.09	0.06 (0.02)***	2,446
Value of investment in small animals	904.28	396.20	502.87 (106.83)***	2,425
Any investment in livestock	0.24	0.13	0.11 (0.02)***	2,446

[13] Given field time constraints, it was not possible to ask households about an exhaustive list of possible investments.

[14] We did not collect information on investments at baseline and thus cannot run the ANCOVA specification.

Value of investment in livestock	10,732.98	5,280.81	5,631.48 (1,117.09)***	2,423
Any investment in agricultural land	0.01	0.01	0.00 (0.00)	2,446
Value of investment in agricultural land	5.99	3.61	2.10 (5.07)	2,423
Any investment is agricultural equipment	0.14	0.14	-0.00 (0.02)	2,446
Value of investment in agricultural equipment	1,115.05	1,339.59	-234.98 (252.66)	2,422
Any investment in nonagricultural equipment	0.03	0.02	0.01 (0.01)*	2,446
Value of investment in nonagricultural equipment	341.23	302.51	6.01 (149.29)	2,421

Note: SE in parentheses clustered at the commune level. * $p<0.10$, **p<0.05, ***p<0.01. "Impact at midline" reports the coefficient on treatment from a single-difference specification, which estimates midline differences in the outcome of interest controlling for the baseline region. All estimates take into account midline sample weights. All estimates are trimmed at the 1 percent tail and converted in log values.

Taken together, the results in Tables 7.2.2.1 and 7.2.2.2 indicate that the combined CT and AM intervention increases the proportion of households that have any savings, increases the proportion that invest in small animals, and increases the proportion that invest in livestock. The latter two investments appear sizeable in value among those who make them, particularly in the case of livestock. They are also consistent with households' direct reports in Section 6.1.4 of using a substantial share of transfers on "agricultural inputs and investments," which they may consider to include investments in livestock. As described in the conceptual framework, the use of transfers for investment in assets is consistent with delivery of larger and less frequent transfer amounts.

7.3 Third-order household-level outcomes

7.3.1 Food security

According to the conceptual framework in Chapter 3, if households use part of the CT to buy food, then we should expect to see improvements in households' access to food and food security. We use two different measures of household food security: the Household Food Insecurity Access Scale (HFIAS) and the Household Hunger Scale (HHS). The HFIAS reflects a broad range of household food insecurity that encompasses the following three domains: "(1) anxiety about household food supply; (2) insufficient quality, which includes variety, preferences, and social acceptability; and (3) insufficient food supply and intake and the physical consequences" (Ballard et al. 2011). HFIAS is constructed from 18 questions that consist of 9 occurrence questions on whether or not the household experienced a specific food insecurity (access) condition in the past four weeks, and 9 "frequency-of-occurrence" questions that are asked as a follow-up to determine how often the condition occurred. For each condition, a household scores a range of 0-3 with 0 representing the condition not occurring; 1, the condition occurring rarely; 2, the condition occurring sometimes; and 3, the condition occurring frequently. Scores are summed up across all 9 conditions for a total score that ranges from 0-27, with higher scores indicating that a household is more food insecure. Households are then categorized into four levels of household food insecurity: food secure, and mildly, moderately, and severely food insecure. While a food secure household experiences none of the food insecurity (access) conditions, or just experiences worry, but rarely, a severely food insecure household has cut back on meal size or number of meals often, and/or experienced any of the three most severe conditions (running out of food, going to bed hungry, or going a whole day and night without eating) (Coates, Swindale, and Bilinsky 2007). The HHS, on the other hand, reflects more severe household food

insecurity and has been validated for cross-cultural use (Ballard et al. 2011). The HHS is constructed using the three most severe conditions used in the HFIAS, and coding the frequency conditions from 0-2 instead of 0-3 (where 0 is the condition never occurring, 1 is the condition occurring rarely or sometimes, and 2 is the condition occurring often), thus the range of the HHS is 0-6, with the higher the score indicating that a household is more food insecure.

Table 7.3.1.1 reveals that the Jigisémèjiri program led to a large and significant reduction in a household's food insecurity of 1.88 points as measured by the HFIAS. Compared to the midline mean of the control group, this is a 27-percent reduction in food insecurity. In terms of moving households across the distribution, the Jigisémèjiri program increased the proportion of households classified as food secure by 11 percentage points and decreased the proportion of households classified as severely insecure by the same magnitude. These represent approximately a 38-percent change compared to the midline mean of the control group. Table 7.3.1.2 reveals a similar pattern; the Jigisémèjiri program led to a decrease of .25 points in the Household Hunger Scale, which represents a 52 percent increase from the midline control mean. Changes across the distribution reveal a 7-percentage-point increase in the proportion of households classified as having little-to-no hunger and a decrease of the same magnitude in the proportion of households classified as having moderate hunger.

Table 7.3.1.1 Comparison of Household Food Insecurity Access Scale, by control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
HFIAS Score (Range 0-27)	5.59	6.50	5.07	7.05	-1.88 (0.43)***	2,446
Food Secure	0.33	0.23	0.35	0.24	0.11 (0.03)***	2,446
Mildly Insecure	0.12	0.11	0.14	0.09	0.06 (0.02)***	2,446
Moderately Insecure	0.31	0.43	0.32	0.38	-0.06 (0.04)	2,446
Severely Insecure	0.25	0.23	0.18	0.29	-0.11 (0.03)***	2,446

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account midline sample weights.

Table 7.3.1.2 Comparison of Household Hunger Scale, by control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Household Hunger Scale Score (Range 0-6)	0.41	0.41	0.23	0.48	-0.25 (0.07)***	2,446
Little-to-no hunger	0.89	0.89	0.93	0.86	0.07 (0.02)***	2,446
Moderate hunger	0.10	0.11	0.06	0.13	-0.07 (0.02)***	2,446
Severe hunger	0.01	0.01	0.00	0.01	-0.01 (0.00)	2,446

*SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights.

Taken together, the results in Tables 7.3.1.1 and 7.3.1.2 indicate that the combined CT and AM intervention improved households' food security and hunger situation and increased the proportion of households classified as food secure or having little to no hunger. These findings are consistent with the self-reported use of the transfer on food (Figure 6.1.4.1), and suggest that our food consumption aggregates are not capturing the full story.

7.3.2 Dietary Diversity

Although the HFIAS and HHS measure food insecurity and access, they do not measure dietary quality. Households may use the CT to buy more nutritious foods and improve their diets if they are aware of the benefits or if they have a preference toward these high value foods but were previously cash constrained to purchase them. Thus, in addition to analyzing food security indicators, we analyze dietary diversity indicators that are important for household nutrition and have been shown to be associated with improvement in a number of outcomes, such as birth weight, child anthropometrics, and hemoglobin concentrations, as well as reduced risk of hypertension, cardiovascular disease, and cancer (Hoddinott and Yohannes 2002). We construct three separate measures for dietary quality: the Dietary Diversity Index (DDI), the Household Dietary Diversity Score (HDDS), and the Food Consumption Score (FCS). The most straightforward of these measures, the Dietary Diversity Index, sums the number of distinct food items consumed by the household in the previous seven days. The household questionnaires at baseline and midline cover 74 and 79 such food items, respectively, and thus the DDI ranges from 0 (no consumption at all) to 74 at baseline and from 0 to 79 at midline. Hoddinott and Yohannes (2002) show that the DDI correlates well with both household dietary quantity and quality, and thus provides a useful summary point of comparison within the measured sample. The HDDS captures a similar element of food access, although it differs from DDI in that frequency is measured across standardized food groups, instead of individual food items. The score is calculated by summing the number of food groups consumed in the previous seven days from the following 12 groups (Kennedy, Ballard, and Dop 2011): cereals, roots/tubers, vegetables, fruits, meat/poultry/offal, eggs, fish/seafood, pulses/legumes/nuts, milk/milk products, oils/fats, sweets, spices/condiments/beverages. Given that the HDDS indicates a household's economic access to food, items that require household resources to obtain, such as condiments, sugar and sugary foods, and beverages, are included in the score. Last, WFP measures food insecurity using a proxy indicator called the food consumption score (FCS). The FCS is calculated by summing the number of days each food group (staples, pulses, vegetables, fruit, meat/fish, milk/dairies, sugar/honey, oils/fats) was consumed by a household, multiplying these by weighted frequencies that represent the nutritional value of the food group, and summing across categories to obtain a single proxy indicator. The FCS ranges from 0–112, with the following typical cut-offs: 0–21 represent poor food consumption, 21.5–35 represent borderline food consumption, and 35+ represent acceptable food consumption (WFP 2008). The FCS has been found to correlate well with caloric availability at the household level (Wiesmann et al. 2009) and thus reflects the quality of the diet in terms of energy and diversity.

Table 7.3.2.1 reveals that the Jigisémèjiri program led to small but significant improvements in the number of food items consumed as measured by the DDI. In particular, the program increased the DDI by 1 item or nearly a 7-percent increase from the control group's midline mean. Across both the treatment and control groups, the household DDI increased from baseline to midline, which is likely a result of adding more food items to the survey at midline. However, the increase of the treatment group was larger than the increase of

the control group. In terms of food groups, Table 7.3.2.2 reveals positive but insignificant improvements in the number of food groups consumed as measured by the HDDS. However, there are large and significant increases in the proportion of households that consumed fruits, sweets, and spices, condiments, and beverages. The increase in the proportion of households consuming fruits is particularly large, representing a 23-percent increase from the midline control mean. The proportion of households consuming the cereals food group or the vegetables food group in a given week is very high, and thus we should not expect to see impacts on the extensive margin, but rather on the intensive margin. The FCS indicator captures both the frequency of consumption and the nutritional value of the food group. Table 7.3.2.3 reveals significant impacts of the Jigisémèjiri program on FCS of 3.7 points or a 6-percent increase from the midline control mean. Significant impacts are found in the number of days households consumed main staple and vegetables, in addition to impacts on the number of days households consumed fruits.

Table 7.3.2.1 Comparison of household Dietary Diversity Index (DDI), by control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
DDI (Range 0 to 79)	11.41	10.88	15.22	13.99	1.01 (0.58)*	2,446

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights. The number of food items varies between baseline and midline surveys.

Table 7.3.2.2 Comparison of Household Dietary Diversity Score (HDDS) and proportion of households consuming each category in last 7 days, by control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
HDDS (Range 0 to 12)	6.61	6.45	7.67	7.34	0.28 (0.17)	2,446
Cereals	0.99	0.99	1.00	1.00	0.00 (0.00)	2,446
White tubers and roots	0.16	0.13	0.14	0.15	-0.02 (0.03)	2,446
Vegetables	0.90	0.89	0.97	0.97	0.00 (0.01)	2,446
Fruits	0.23	0.19	0.38	0.31	0.07 (0.03)**	2,446
Meat	0.39	0.45	0.52	0.53	-0.01 (0.05)	2,446
Eggs	0.01	0.00	0.03	0.03	0.01 (0.01)	2,446
Fish and other seafood	0.59	0.55	0.69	0.62	0.06 (0.04)	2,446
Legumes, nuts, and seeds	0.82	0.78	0.90	0.86	0.02 (0.02)	2,446
Milk and milk products	0.23	0.24	0.43	0.38	0.05 (0.05)	2,446
Oils and fats	0.53	0.52	0.74	0.68	0.05 (0.04)	2,446
Sweets	0.79	0.75	0.89	0.85	0.04	2,446

Spices, condiments, and beverages	0.96	0.97	0.99	0.97	(0.02)* 0.01 (0.01)*	2,446

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** p<0.05, *** p<0.01. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights.

Table 7.3.2.3 Comparison of Food Consumption Score (FCS) and mean days consumed in past 7 days, by control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
FCS (Range 0 to 112)	57.13	55.66	69.36	64.73	3.70 (1.97)*	2,446
Poor (FCS<=21)	0.04	0.07	0.01	0.01	-0.00 (0.01)	2,446
Borderline (FCS<=35)	0.11	0.11	0.07	0.09	-0.02 (0.02)	2,446
Acceptable (FCS>35)	0.84	0.82	0.92	0.90	0.02 (0.02)	2,446
Days consumed in the past 7 days						
Main staples	6.83	6.72	6.97	6.88	0.09 (0.03)***	2,446
Pulses	5.01	4.70	5.72	5.34	0.27 (0.16)	2,446
Vegetables	5.93	5.82	6.59	6.41	0.17 (0.09)*	2,446
Fruits	1.09	0.86	2.07	1.52	0.51 (0.18)***	2,446
Meat & fish	3.57	3.55	4.36	3.99	0.27 (0.21)	2,446
Milk and milk products	0.92	0.98	1.89	1.70	0.19 (0.31)	2,446
Sugar	4.42	4.17	5.50	5.13	0.32 (0.22)	2,446
Oils and fats	2.51	2.43	3.73	3.33	0.38 (0.27)	2,446
Condiments	6.64	6.76	6.87	6.77	0.09 (0.06)	2,446

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** p<0.05, *** p<0.01. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights.

Taken together, the results in Tables 7.3.2.1, 7.3.2.2, and 7.3.2.3 indicate that the combined CT and AM intervention improved a household's dietary diversity as measured by the DDI and FCS. In terms of food groups, increases on the extensive margin are found for fruits, sweets, and spices/condiments and beverages, and on the intensive margin for fruits, vegetables, and main staples. Impacts on other high-value nutritious foods, such as dairy, meat, and seafood, are positive but insignificant, suggesting that there is room for improvement in these categories.

7.3.3 Poverty

Poverty indicators are based on "poverty lines," which reflect a level of expenditure below which it is estimated that individuals are unable to meet their minimum nutritional, clothing, and shelter needs (Coudouel et al., 2002). In Section 7.2.1, we see that the combined intervention of CT and AM does not lead to statistically significant increases in consumption expenditures on average. However, it is possible that the intervention could lead to fewer households with per-capita consumption expenditures below the threshold defining the poverty line.

We assess the impacts of the combined intervention on poverty using three different constructions of the poverty line: (1) the international poverty line of $1.25 per person per day based on 2005 purchasing power parity (PPP) calculations, which was used by the World Bank prior to October 2015 (Ravallion et al., 2009) and was used in the baseline report, (2) the updated international poverty line of $1.90 per person per day based on 2011 PPP, used by the World Bank since October 2015 (Ferreira et al., 2015), and (3) Mali's national poverty line, which was 479.45 FCFA per person per day for midline (175,000 FCFA per person per year in 2016, according to Institut National de la Statistique du Mali, 2017) and 476.50 FCFA per person per day for baseline (173,922 FCFA per person per year in 2013, according to Gacko et al., 2014). For each, we construct the poverty headcount ratio. We also construct the poverty gap for the national poverty line, which represents the average extent to which individuals fall below the poverty line, as well as the poverty gap index which divides the poverty gap by the poverty line and represents the average percentage shortfall from the poverty line (World Bank Institute, 2005). In order to construct each of these indicators, we compare the value of total household consumption expenditures per capita per day against the respective poverty line.

Table 7.3.3.1 shows that, at baseline, the share of poor households in our sample was about 63-65% based on the international poverty line using the 2005 PPP, about 79% based on the international poverty line using the 2011 PPP, and about 86% based on the national poverty line. According to each of the two international poverty lines and the national poverty line, the impact of the combined intervention on the percentage of poor households has a negative point estimate. However, none of these program impacts is statistically significant. Based on all three definitions, the percentage of poor households decreases between baseline and midline, but the reductions are not statistically different between the treatment and control groups.

There is also no statistically significant impact on the poverty gap or poverty gap index for the national poverty line, although the point estimate is negative. At baseline, the poverty gap based on the national poverty line is about 215 to 219 FCFA per day, and the poverty gap index reveals an average shortfall of about 45 to 46 percent. By midline the poverty gap and poverty gap index have decreased in both the treatment and control groups, but the reductions are not statistically different between treatment and control. The poverty gap does however shed light on the amount of funds needed to bring the average household in our sample out of poverty. At midline, the poverty gap for the control group is on average 161 FCFA, reflecting that, on average, daily consumption expenditures per capita of control households is 161 FCFA below the national poverty line. This implies a shortfall per individual of about 4830 FCFA per month (= 161 FCFA per day * 30 days). Referring to Table 7.2.1.1, we see that treatment and control households at midline include 11.6 individuals on average. Thus, at the household level, consumption expenditures per month would need to increase by about 56,028 FCFA (= 4830 FCFA per individual * 11.6 individuals) in order to bring the average household up to the national poverty line. This implies that Jigisémèjiri's monthly household transfer of 10,000 FCFA is a fraction of what is needed to eliminate poverty for the average household in our sample. Although it is possible that some less-poor-than-average households had

expenditures closer to the poverty line, such that 10,000 per month may have been enough to move them out of poverty, the poverty gap indicates that the *average* beneficiary household was so poor in the absence of intervention that the transfer amount was insufficient to lift it to the national poverty line.

Table 7.3.3.2 shows impacts only on households that were characterized as poor at baseline according to each definition, in order to assess whether there are effects concentrated among those who were initially poor. We find no statistically significant impacts within this subgroup, although again point estimates are negative.

Given our observation in Section 7.2.1 that impacts on consumption expenditures appear larger (although still not statistically significant) for households that received their last cash transfer more recently, we disaggregate impacts on poverty by the number of days since the last transfer. Table 7.3.3.3 shows that, consistent with the results on consumption expenditures, the estimated reductions for households receiving the transfer more recently are larger than for those receiving the transfer less recently (although neither are statistically significant).

Table 7.3.3.1: Percentage of households with daily consumption expenditures per capita below the poverty line, in national and international terms – control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Percent of households below international poverty line (2005 PPP)	62.65	65.17	43.95	46.62	-1.65	2273
					(3.81)	
Percent of households below international poverty line (2011 PPP)	78.70	78.84	63.71	65.41	-0.89	2273
					(3.83)	
Percent of households below national poverty line	86.09	86.14	75.54	78.20	-1.78	2273
					(2.73)	
Poverty gap (for national poverty line)	214.46	219.69	153.67	161.01	-4.25	2273
					(11.14)	
Poverty gap index (for national poverty line)	45.00	46.10	32.05	33.58	-0.89	2273
					(2.32)	

*SE in parentheses clustered at the commune level. * $p<0.10$, **p<0.05, ***p<0.01. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification of the outcome of interest on treatment variable, controlling for baseline outcome level and region. All estimates take into account midline sample weights. Aggregates were trimmed at the top and bottom 1 percent before being converted into daily values. The proportion of individuals under the international poverty line is using the 2011 PPP dollar conversion rate of 210.193 to $1 from the ICP group, and the poverty line of $1.90 per person a day.

Table 7.3.3.2: Percentage of households with daily consumption expenditures per capita below the poverty line, in national and international terms, among those who were poor at baseline – control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Percent of households below international poverty line (2005 PPP)	100.00	100.00	48.69	50.00	-0.34	1440
					(4.61)	
Percent of households below	100.00	100.00	65.68	68.41	-2.40	1792

international poverty line (2011 PPP)						
					(3.64)	
Percent of households below national poverty line	100.00	100.00	76.65	79.95	-3.24	1958
					(2.70)	
Poverty gap (for national poverty line)	249.10	255.03	157.81	168.13	-7.81	1958
					(11.14)	
Poverty gap index (for national poverty line)	52.27	53.51	32.92	35.07	-1.63	1958
					(2.32)	

Table 7.3.3.3: Percentage of households with daily consumption expenditures per capita below the poverty line, in national and international terms, by number of days since last transfer – control vs. pooled treatment

	Midline control mean	Midline below mean nb days since transfer	Midline above mean nb days since transfer	Impact at midline - nb days below the mean	Impact at midline - nb days above the mean	N
Percent of households below international poverty line (2005 PPP)	46.62	43.02	44.38	-4.12	-1.01	
				(4.15)	(4.07)	1890
Percent of households below international poverty line (2011 PPP)	65.41	63.42	63.55	-1.58	-1.43	
				(4.17)	(4.09)	1890
Percent of households below national poverty line	78.20	73.90	76.76	-3.84	-0.96	
				(3.02)	(3.00)	1890
Poverty gap (for national poverty line)	161.01	153.04	153.02	-8.16	-4.71	
				(12.35)	(11.64)	1890
Poverty gap index (for national poverty line)	33.58	31.92	31.92	-1.70	-0.98	
				(2.57)	(2.43)	1890

*SE in parentheses clustered at the commune level. * $p<0.10$, **p<0.05, ***p<0.01. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification of the outcome of interest on treatment variable, controlling for baseline outcome level and region. All estimates take into account midline sample weights. Aggregates were trimmed at the top and bottom 1 percent before being converted into daily values. The proportion of individuals under the international poverty line is using the 2011 PPP dollar conversion rate of 210.193 to $1 from the ICP group, and the poverty line of $1.90 per person a day.

Taken together, the results in Tables 7.3.3.1, 7.3.3.2, and 7.3.3.3 indicate that there are no statistically significant impacts of the combined intervention on poverty based on the value of consumption expenditures per capita per day. The poverty gap indicates that, on average, beneficiary households were so poor in the absence of intervention that the transfer amount was insufficient to lift them above the national poverty line. However, point estimates for program impacts on poverty are negative, and reductions in poverty appear larger (although still not statistically significant) for households that received transfers more recently. These patterns are consistent with the pattern of estimated impacts on consumption expenditures, as we would expect, given that poverty indicators are based on consumption expenditures. This implies that some households may have experienced reductions in poverty due to the transfers soon after receiving them, but the reductions faded out before the full three months that the transfers were intended to cover. Thus, overall, our results suggest that we do not find statistically significant effects on poverty in part because the size of the transfer amount relative to the poverty gap was not sufficiently large

on average and in part because, for many households in the sample, the majority of expenditures from transfers occurred prior to the recommended recall period for consumption measures.

7.3.4 Assets

The theory of change shows that investment and savings behavior could increase asset holdings. Asset ownership is an important dimension of putting households on a pathway to move sustainably out of poverty (Moser 1998). Benefits are multidimensional. Assets allow households to build wealth over the longer term. They serve as stores of value and can increase in value with further investment. Productive assets can generate new income. Consumer durables can provide a flow of consumption benefits, while retaining their value. Assets can serve as collateral for access to financial services. They can also be sold for income when needed (Rosenzweig and Wolpin 1993). As such, asset ownership provides security through emergencies as well as opportunities during periods of stability.

In both the baseline and midline, households are asked whether they own each of 35 types of assets. If so, they are asked what value they would expect to receive if they sold all of that asset. We use these responses to construct an estimated value of households' asset ownership. Asset types are grouped into the following categories: (1) Consumer durables—tables, beds, lamps, pots and pans, radios, etc.; (2) Livestock ownership—poultry, rabbits, sheep, goats, beef cattle, cows, donkeys, etc.; (3) Transport assets, which could serve as nonagricultural productive assets—carts, bicycles, motorcycles/mopeds, boats, etc.; (4) Agricultural productive assets—plows, sickles, hoes, seeders, shovels, rakes, machetes, axes, etc.; and (5) Total over all of these. We construct the total value of assets in each of these categories. Because respondents' estimates of the hypothetical value of their assets are very noisy, with many clear outliers, we trim the top and bottom 1 percent of the distribution, then transform the constructed values to their natural logs, which reduces the variation caused by extreme reports. However, using a log outcome changes the interpretation of the treatment coefficient derived from the estimation equation in Section 4.3. Rather than this coefficient being interpreted as the magnitude of the estimated impact on the outcome, it is the magnitude of the estimated impact on the *log* outcome. This magnitude is approximately equivalent to the percent change in the outcome due to the intervention, relative to the control group.

Table 7.3.4.1 compares the log value of asset ownership in each of the five categories between the pooled treatment and control. We see that, for the treatment group, the intervention causes a 0.61 increase in the log value of livestock ownership (significant at the 5-percent level), a 0.64 increase in the log value of transport assets (significant the 5-percent level), no significant effects on consumer durables or agricultural productive assets, and a 0.19 increase in the log value of the total over all these categories (significant at the 1-percent level). This translates to the intervention increasing the treatment group's value of livestock ownership by approximately 61 percent relative to the midline control mean, their value of transport assets by approximately 64 percent, relative to the midline control mean, and their value of the total overall listed assets by approximately 19 percent relative to the midline control mean.

Table 7.3.4.1 Comparison of assets aggregates in log, control vs. pooled treatment

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Value owned (in log): Consumer durables	9.76	9.55	10.62	10.48	0.11 (0.08)	2,403
Value owned (in log): Livestock	9.82	10.30	11.49	11.02	0.61 (0.31)**	2,404
Value owned (in log): Transport	7.98	7.83	9.42	8.77	0.64	2,403

					(0.29)**	
Value owned (in log): Ag productive	8.61	8.28	9.55	9.33	0.18	2,407
					(0.17)	
Value owned (in log): Consumer durables, transport, ag productive, livestock	12.14	12.22	12.86	12.70	0.19 (0.07)***	2,374

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account midline sample weights. All aggregates are in logs, and have been trimmed prior to being transformed into logs.

The changes in asset ownership due to the intervention are meaningful. They are also consistent with findings in Section 7.2.2 of increased savings and increased investments (particularly in livestock).[15] Taken together, our evidence suggests that the combined intervention led treatment households to significantly increase their asset base.

[15] Investments in transport assets were not asked about.

8 Impact Estimates on Child-level Outcomes

In this chapter, we assess the impact of the combined CT and AM intervention on outcomes situated at the child level. Child health, nutritional status, and feeding practices depend heavily on child age. Therefore, we use the repeated cross-sectional study design to assess the impact in a midline sample of children 6–23 months of age. This age range is identical to that of the baseline sample to minimize the influence of different child age on the impact analysis. We focus on this specific age range because from 6 months onward, the child is expected to receive complementary feeding in addition to breastfeeding as per recommendation. The choice of complementary foods depends to a large extent on the available household budget, availability of food in markets, and maternal knowledge and attitudes on child feeding practices. In addition, because the study also planned the evaluation of the PNP intervention, no children below 6 months of age were included in the sample. Below 6 months, it is recommended to offer only breast milk to a child. On the other hand, we limit the age range to 23 months, because this is the period of greatest vulnerability to linear growth retardation (Victora et al., 2010) and of potential to benefit from targeted nutrition interventions (Bhutta et al. 2013).

8.1 First-order child-level outcomes

8.1.1 Maternal knowledge of child nutrition and health

The objective of this section is to assess the impact of the combined CT and AM intervention on maternal knowledge of child nutrition and health, including on questions related to breastfeeding and complementary feeding. It is hypothesized that caregivers who have been exposed to the program's accompanying measures (AM), including group counselling on child health and nutrition, increase their knowledge on child feeding practices and health. These practices include appropriate breastfeeding, the timely introduction of (semi-) solid foods at the age of 6 months, the inclusion of nutrient dense foods into the complementary diet and appropriate hand washing to avoid the possibility of the child getting contaminated by pathogens.

Table 8.1.1.1 shows that there was no statistically significant program impact on the summary maternal knowledge score on health and nutrition. Mothers at baseline and midline answered, on average, between 5 and 6 of 10 questions correctly. Concerning the sub-questions that contributed to this overall knowledge score, we noticed no difference in knowledge between mothers of both study groups, except for the appropriate age to introduce liquids to the infant that was 13 percentage points ($P<0.01$) more likely to be answered correctly by mothers belonging to the treatment group as compared to the control group.

When comparing the midline to the baseline results, we notice that knowledge on general breastfeeding practices remained similarly high at midline as compared to baseline. Knowledge levels on complementary feeding and nutritional mitigating behavior during child illness and breastfeeding problems were generally lower in the midline sample as compared to the baseline. Of interest, caregivers of the midline survey were more knowledgeable of foods rich in vitamin A compared to caregivers of the baseline survey (0.73 vs. 0.56); this positive evolution in both treatment and control groups is likely due to interventions outside the Jigisémèjiri program.

The first two quarterly AM sessions treated the themes of breastfeeding practices (with the focus on exclusive breastfeeding) and complementary feeding. With the exception of Mopti, beneficiaries of all regions were scheduled to be exposed to these messages before the collection of the midline data. However, it should be stressed that the knowledge questions were posed to the main caregiver of the child who was very often the mother of the child. In Chapter 6, we noted that 60 percent of households belonging to the

treatment group attended an AM session. Among those attending, it was mainly the head of the household—very often the father. Spouses participated only in 15 percent of the cases. Hence, with the caveat that respondents may have misinterpreted questions about AM sessions to include only those linked to the cash transfer distribution, it is possible that there was little exposure of mothers to AM directly. If so, this can explain the lack of impact of the AM intervention on maternal knowledge on child nutrition and hygiene.

Table 8.1.1.1 Maternal knowledge of hygiene and nutrition for children 6–23 months of age

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Maternal knowledge on health and nutrition[a]	5.99	5.94	5.65	5.61	0.01 (0.16)	1,490
Newborn breastfed within one hour after delivery	0.84	0.83	0.83	0.83	-0.00 (0.03)	1,490
Mother should feed child colostrum	0.93	0.94	0.94	0.91	0.02 (0.02)	1,490
Go to health center or breastfeed more if not enough breast milk	0.25	0.31	0.19	0.20	-0.01 (0.02)	1,490
Baby should receive other liquids at 6 months	0.63	0.63	0.64	0.53	0.13 (0.04)***	1,490
Baby should receive semi-solids at 6 months	0.37	0.40	0.33	0.32	0.02 (0.03)	1,490
Sick children under 2 years should breastfeed more	0.48	0.42	0.39	0.43	-0.05 (0.04)	1,490
Sick children 6-24 months should drink more	0.54	0.52	0.44	0.46	-0.03 (0.04)	1,490
Sick children 6-24 months should eat more	0.53	0.50	0.42	0.43	-0.02 (0.04)	1,490
Can identify foods rich in vitamin A	0.56	0.57	0.72	0.75	-0.02 (0.04)	1,239
Can identify appropriate moments for hand washing	0.96	0.95	0.95	0.96	-0.01 (0.02)	1,490

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights. Covariates for child sex and child age are included in the regression.

[a] The maternal knowledge score is composed out of ten questions, each contributing one point; the maximal score is 10 out of 10.

8.2 Second-order child-level outcomes

8.2.1 Infant and Young Child Feeding practices

Next, we assess the program's impact on the Infant and Young Child Feeding (IYCF) indicators proposed by WHO (WHO, 2008) (Table 8.2.1.1). At midline, the timely introduction of (semi-)solid and soft foods was 11 percentage points more prevalent in the treatment group as compared to the control group. There were also 3.6 percentage points ($P<0.05$) more treatment children that consumed a minimally acceptable diet (defined by consuming four food groups and the recommended number of meals for a specific child age) compared to control peers. These changes in practices, albeit very small, are important to ensure children's timely introduction to energy and nutrient dense foods in addition to breast milk to support their development and growth. It is likely that this change in behavior is due to increased resources more than

increased knowledge, as more than half of the caregivers did not know that at 6 months of age, (semi-)solid and soft foods need to complement the intake of breast milk (see Table 8.1.1.1).

To our surprise, we observed that the proportion of children at midline that was ever breastfed was slightly lower (-2.5 percentage points; $P<0.10$) in children belonging to the treatment group compared to the control group. Nevertheless, the prevalence of ever breastfed children remained quite high (≥95%) in both study groups at midline as well as at baseline, hence this very small difference seems not very meaningful.

The first quarterly session of the AM consisted of group counselling on optimal breastfeeding practices with a focus on exclusive breastfeeding for children below the age of 6 months. The proportion of children with recommended exclusive breastfeeding could not be measured in this sample because the cross-sectional sample ranged for 6-to-23 months of age. We did not see any improvement in continuous breastfeeding in older children of 20–23 months, although the small sample of children in this age group reduces our power in detecting impacts.

Table 8.2.1.1 IYCF indicators for children 6–23 months of age

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Continued breastfeeding at 12–15 months	0.94	0.96	0.95	0.94	0.008 (0.023)	351
Children ever breastfed (6–23 months)	0.96	0.95	0.95	0.98	-0.025 (0.010)**	1,490
Continued breastfeeding at 20–23 months	0.58	0.50	0.58	0.45	0.105 (0.064)	302
Adequate milk feeding frequency for non-breastfed (6–23 months)	0.04	0.08	0.07	0.07	0.015 (0.036)	225
Introduction of solid, semi-solid, soft foods (6–8 months)	0.42	0.44	0.38	0.28	0.110 (0.057)*	298
Child ate solid, semi-solid, or soft food in past 24 hours	0.82	0.81	0.79	0.81	-0.028 (0.022)	1,468
Minimum meal frequency (6-23 months)	0.52	0.51	0.45	0.44	0.010 (0.046)	1,471
Minimum dietary diversity (>=4) (6–23 months)	0.12	0.13	0.15	0.12	0.025 (0.020)	1,486
Minimum acceptable diet (6-23 months)	0.07	0.08	0.08	0.05	0.036 (0.014)**	1,488
Consumption of iron-rich foods/supplements (6–23 months)	0.44	0.44	0.42	0.38	0.052 (0.037)	1,482

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights. Covariates for child sex and child age are included in the regression.

Several studies have demonstrated a consistent positive association between the child DD indicator (WHO, 2008) and the probability of adequacy of several essential micronutrients(Kennedy et al., 2007; Moursi et al., 2008; Wondafrash et al., 2016). In other words, the higher the DD, the higher the likelihood that children can meet their requirements for essential micronutrients, including vitamins and minerals.

Table 8.2.1.2 shows the treatment effect on the mean DD and the consumption of specific food groups by study children. We did not find any meaningful impact of the combined CT and AM intervention on the mean child dietary diversity score. Breaking down the DD into the seven food groups that contribute to it, we notice a small decrease in the prevalence of legume and nuts intake in the treatment group (-3 percentage points; $P<0.05$) compared to the control group.

The lack of impact of the combined CT and AM intervention on the child's DDS is an unexpected result because at the time of the midline survey, beneficiaries of all regions (except for Mopti) were exposed to AM-related group counselling on complementary food practices. Both at baseline and midline, children 6 to 23 months of age only consumed on average 2 food groups per day. One of the key messages is the use of a diversity of food groups to prepare both child-targeted and family foods to ensure an optimal (micro-) nutrient intake. In addition, it was hypothesized that the distributed cash would allow for the purchase of more nutrient dense foods that would benefit the young child. A recent dietary assessment study assessing the impact of the seasonal unconditional cash transfer program, MAM'Out on children's dietary intake conducted in neighboring Burkina Faso, found that children in the cash intervention group consumed, more often, flesh foods, dairy, and iron-rich foods compared to children from the control group (Tonguet-Papucci et al., 2017).

Table 8.2.1.2 Dietary diversity for children 6–23 months of age

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Mean dietary diversity score	2.07	2.04	2.00	1.96	0.03 (0.08)	1,486
Consumed grains, roots, and tubers in past 24 hours	0.81	0.80	0.79	0.79	-0.00 (0.02)	1,484
Consumed legumes and nuts in past 24 hours	0.12	0.10	0.06	0.09	-0.03 (0.02)**	1,483
Consumed dairy in past 24 hours	0.18	0.26	0.25	0.22	0.04 (0.03)	1,480
Consumed flesh food in past 24 hours	0.42	0.39	0.37	0.33	0.04 (0.04)	1,483
Consumed eggs in past 24 hours	0.02	0.01	0.02	0.02	0.01 (0.01)	1,483
Consumed vitamin A-rich fruit and vegetable in past 24 hours	0.43	0.40	0.43	0.44	-0.01 (0.04)	1,483
Consumed other fruit and vegetable in past 24 hours	0.09	0.08	0.08	0.08	-0.00 (0.02)	1,482

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights. Covariates for child sex and child age are included in the regression.

8.2.2 Stimulation practices for children

In this section, we assess the impact of the combined CT and AM intervention on stimulation practices by caregivers for children. One potential reason why cash transfers may lead to more stimulation practices is that members of households that benefit from the cash intervention may substitute time invested in revenue related activities for more time to interact with and care about the youngest children to support their early development. In addition, income could indirectly improve the psychological well-being of family

members, through reductions in subjective feelings of financial strain and deprivation. The AM sessions may also improve child stimulation if these issues were part of the subthemes.

To analyze impacts on stimulation practices, we use five key questions used by the Multiple Indicator Cluster Survey (MICS[16]) to describe to what extent interaction occurs between caregivers and their children. These questions are originally intended for children older than 24 months; however, they can also be used in this context to assess an impact in the treatment group relative to a control group. Table 8.2.2.1 shows that the combined CT and AM intervention did not have any impact on how caregivers or other persons older than 15 years in household stimulate the child's development. This lack of impact could be explained by the possibility that mainly the head of household attended the AM group counselling, while the mother and other household members who may spend more time on childcare than the household head attended much less frequently (see Chapter 6).

Table 8.2.2.1 Stimulation practices for children 6–23 months of age

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Told stories to child	0.27	0.26	0.29	0.27	0.01 (0.04)	1,490
Sang songs to or with child, including lullabies	0.65	0.60	0.60	0.59	0.01 (0.04)	1,490
Took child outside the home	0.50	0.49	0.53	0.53	-0.00 (0.04)	1,490
Played with child	0.81	0.77	0.83	0.86	-0.03 (0.03)	1,490
Named, counted, or drew things for or with child	0.24	0.30	0.32	0.33	-0.02 (0.03)	1,490

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights. Covariates for child sex and child age are included in the regression.

8.3 Third-order child-level outcomes

In this section, we assess the effect of combined CT and AM on two third-order child level outcomes: (1) child health and (2) child nutritional status. These two outcomes are intricately linked to each other. Malnourished children have a weakened immune system that makes them less resistant to infections, while ill children typically have higher nutrient requirements and a reduced appetite. Consequently, the latter are more prone to become malnourished. According to the conceptual model presented in Chapter 3, we hypothesized that if CT+AM improve first- and second-order nutrition and health-inspired child-level outcomes, this would result in a positive impact on child health and nutritional status.

8.3.1 Child health

In this section, we assess the impact of combined CT and AM on morbidity symptoms and health-seeking behavior in case of illness. The hypothesis is that the cash could reduce the financial barriers to health care. Therefore, beneficiary caregivers are expected to attend health center consultations sooner if they notice their child develops an illness. In addition, the increased purchasing power would allow for more financial

[16] UNICEF (2017) Multiple Indicator Survey 6 (http://mics.unicef.org/).

access to medication. Although health-seeking behavior is a second-order outcome, we place this outcome with the third-order outcome of child health because in the survey the questions on health seeking behavior were asked to the subsample of caregivers that reported any morbidity symptoms in their children.

As a first step, we assess the effect of the combined CT and AM intervention on child morbidity signs (Table 8.3.1.1). With the exception of a 4.4 percentage point ($P<0.10$) greater reduction in the prevalence of diarrhea in treatment of children relative to the control peers at midline, we find no evidence of an intervention-related impact on child morbidity. We further assess if this small impact on child diarrhea was associated with better caregiver treatment practices in the treatment group. However, when asking caregivers what mitigation strategies they would apply in case of child diarrhea, caregivers of the treatment group did not display better practices compared to the control caregivers. Relative to the control group, treatment mothers were not more likely to mention the use of oral rehydration solution or zinc to treat the diarrhea. Furthermore, they did not report that in case of diarrhea, a child should be provided with more liquids and foods.

Overall, there was a trend to less child morbidity symptoms reported at midline compared to baseline for diarrhea, fever, and cough. This could be the consequence of having collected part of the baseline data in a different season than the midline data. Indeed, part of the baseline data collection happened during the cold season (January–February) that is characterized by the presence of dust winds—called *Harmattan*. The inhalation of small dust particles is typically associated with a higher incidence of respiratory infections (Owusu et al., 2014), which could explain the higher prevalence of cough and breathing difficulties at baseline as compared to midline.

Table 8.3.1.1 Morbidity symptoms presented in children between 6–23 months of age

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Vomiting	0.15	0.16	0.11	0.12	-0.00 (0.02)	1,486
Drank very little	0.08	0.09	0.08	0.07	0.01 (0.02)	1,484
Blood in stool	0.05	0.06	0.10	0.08	0.02 (0.02)	1,487
Diarrhea	0.18	0.14	0.11	0.14	-0.04 (0.02)*	1,481
Loss of appetite	0.13	0.11	0.11	0.13	-0.02 (0.02)	1,478
Languid or lazy	0.14	0.15	0.13	0.16	-0.03 (0.03)	1,479
Fever	0.35	0.40	0.28	0.33	-0.04 (0.04)	1,487
Persistent cough	0.23	0.20	0.13	0.10	0.02 (0.02)	1,483
Breathing difficulties	0.07	0.09	0.05	0.04	0.01 (0.01)	1,484

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights. Covariates for child sex and child age are included in the regression.

We also assessed if the combined CT and AM intervention changed the health-seeking behavior of the program's beneficiaries in case child morbidity signs present themselves. Table 8.3.1.2 shows that caregivers of the treatment group were 10 percentage points ($P<0.05$) more likely to search for a treatment if any of the mentioned symptoms occurred in their 6–23-months-old index child as compared to the control group. No difference in the type of health-seeking behavior was found between study groups. More than half of the respondents declared seeking treatment primarily in the informal health sector, while a third would rather seek treatment at a CSCom or public hospital.

While it is positive that caregivers in the treatment group seek, more often, treatment in case of child illness, this result does not entail a shift toward more consultations in the formal health sector. The AM-intervention did not have a specific module that sensitized caregivers to consult formal first-line health services.

There are only a few studies to which we can compare our results on health-seeking behavior and child morbidity. Most CT studies evaluated health service attendance in terms of preventive health clinics that are typically offered for free (Bastagli et al., 2016). There are also few studies that measured the impact of CT on child morbidity. Two previous studies assessing the effect of an unconditional CT (without AM) reported a positive impact on child morbidity and health-seeking behavior. In Burkina Faso, the MAM'Out study found that children in the cash treatment group had a 21-percent lower risk of self-reported acute respiratory infections, while no impact was found on fever and diarrhea prevalence. On the other hand, in the Give Directly Program in Kenya, the proportion of illnesses for which a professional medical staff was consulted was 7 percent higher in the intervention group as compared to the control group (Haushofer & Shapiro, 2013).

Table 8.3.1.2 Health seeking behavior toward first-line health services in case of morbidity symptom presented

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Sought treatment for any of the morbidity symptoms	0.45	0.55	0.51	0.44	0.10 (0.04)**	632
CSCOM or public hospital	0.32	0.33	0.37	0.38	-0.01 (0.06)	347
Private health sector	0.02	0.00	0.01	0.03	-0.02 (0.02)	347
(Private) pharmacy	0.01	0.03	0.02	0.01	0.01 (0.01)	347
ASC or health post	0.11	0.12	0.10	0.09	0.01 (0.04)	347
Community health services (Relais)	0.00	0.00	0.03	0.01	0.01 (0.01)	347
Informal health sector	0.53	0.52	0.47	0.48	0.00 (0.06)	347

Note: SE in parentheses clustered at the commune level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights. Covariates for child sex and child age are included in the regression. The number of observations for each row corresponds to the number of households who presented any of the symptoms for the first variable. For the following variables, the number of observations correspond to the total number of households who presented the symptoms and who sought treatment.

8.3.2 Child nutrition status

In this section, we assess the impact of the combined CT and AM intervention on child nutritional status. By providing the CT and attending AM sessions on the use of transfers and complementary feeding, we hypothesized that the treatment households would have better access to more nutrient dense foods for their growing children. Previous studies have also shown that greater child dietary diversity is associated with lower stunting (Arimond & Ruel, 2004).

Table 8.3.2.1 shows that providing extra cash and organizing AM group counselling did not result in any impact on child anthropometric indicators or Hb/anemia. In addition, child anemia, stunting, and wasting prevalences remained very high at midline. Particularly, the high levels of child wasting are a reason for concern as they are equal to or above the 15-percent threshold that the World Health Organization proposed to denominate the nutritional situation of a region or country as *critical* (WHO, 2000).

Table 8.3.2.1: The impact of the intervention on child nutritional status in children 6-23 months of age

	Baseline mean T	Baseline mean C	Midline mean T	Midline mean C	Impact at midline	N
Length/height-for-age Z-score	-1.32	-1.31	-1.40	-1.27	-0.11 (0.10)	1,471
Weight-for-length/height Z-score	-0.82	-0.83	-0.90	-0.91	0.03 (0.11)	1,466
Child stunting	0.29	0.28	0.31	0.27	0.03 (0.03)	1,471
Child severe stunting (HAZ<-3)	0.09	0.08	0.10	0.09	0.01 (0.02)	1,471
Child wasting	0.14	0.15	0.16	0.15	0.01 (0.03)	1,466
Child severe wasting	0.03	0.03	0.04	0.04	0.00 (0.01)	1,466
Hemoglobin level (g/dl)	8.96	8.78	9.25	9.11	0.10 (0.13)	1,476
Anemia Hb<11g/dL	0.92	0.94	0.89	0.89	0.01 (0.02)	1,476
Anemia Hb<7g/dL	0.10	0.14	0.07	0.09	-0.02 (0.02)	1,476

Note: SE in parentheses clustered at the commune level. * p<0.10, ** p<0.05, *** p<0.01. "Impact at midline" reports the coefficient on treatment from an ANCOVA specification, which estimates midline differences in the outcome of interest controlling for the baseline outcome level and region. All estimates take into account sample weights. Covariates for child sex and child age are included in the regression. For the acute malnutrition indicators, "Impact at midline" reports the coefficient on treatment from a single-difference specification, which estimates midline differences in the outcome of interest controlling for child sex and age and baseline region.

The results on the impact of the combined CT and AM intervention on nutritional status are in line with the few studies that assessed the impact of CT on child nutritional status. For instance, the recent MAM'Out study in Burkina Faso did not find any impact on child nutritional status as compared to a control group (Houngbe et al., 2017). The UCT Zambian Child Grant Program also had no effect on child wasting (or mean weight-for-height Z-score) or stunting (or mean height-for-age) among children under 5 years compared to the control group (Seidenfeld et al., 2014). The Kenyan Give-directly program pooled child nutritional status outcomes with child health outcomes into a "health index," but did not find any CT impact

on this index (Haushofer & Shapiro, 2013). In contrast to this study, however, the cited studies did not offer AM in addition to the CT. According to the theory of change that we presented in Chapter 3, the lack of AM that aims at inducing a behavior change related to child feeding practices, health and nutrition makes it conceptually more difficult to expect an important impact on these third-order outcomes.

9 Conclusion

This report presents the background, design, and midline findings of the impact evaluation of the "Filets Sociaux (Jigisémèjiri)" program implemented by the Government of Mali. The program aims at reducing poverty and improving human capital accumulation through targeted cash transfers (CT), accompanying measures (AM), and preventive nutrition packages (PNP).

In order to rigorously and independently assess the impacts of the program, the Government of Mali has collaborated with research partners, International Food Policy Research Institute (IFPRI) and Institut de recherché pour le développement (IRD), to undertake an impact evaluation. The impact evaluation is designed as a two-stage randomized control trial in which we randomly assigned communes to treatment or control; then, within a subset of the treatment communes deemed eligible for nutrition packages, we randomly assigned villages to receive the nutrition packages or not receive them. To measure impact of the program on household welfare and child health and nutrition, a baseline and midline survey were implemented. The baseline data collection occurred September 2014–February 2015, before the start of the program in different regions, and the midline occurred August 2016–November 2016, before the control group was rolled into the program. An endline survey is planned for the fall of 2018.

At midline, the treatment group had received a combined intervention of CT and AM for 1–2 years, the randomized control group had not yet entered the program, and PNP had not yet been introduced. Midline results compare the impacts of the pooled treatment (those receiving CT and AM) on household-level and child-level outcomes, relative to the control group. A panel sample, which follows the same households over time, is used to assess household-level outcomes, and a repeated cross-section sample, which consists of a new sample of households with a child 6–23 months, is used to assess child-level outcomes. The midline survey successfully collected data on 2,446 households from the initial panel listing and 1,498 households from the cross-section listing. Findings from the midline data collection lead to conclusions in this report that can be divided into four categories: beneficiaries' experience with the cash transfers, beneficiaries' experience with the accompanying measures, program impacts on household-level outcomes, and program impacts on child-level outcomes. Policy implications and recommendations follow the summary of findings.

9.1 Summary of midline findings

***Beneficiaries' experience with the cash transfers*:** Beneficiary reports reflect that the cash transfer component has been implemented largely as designed, adhering closely to the cluster-randomized control trial design and following the implementation calendar. In both the panel and cross-section samples, approximately 97 percent of all households in treatment communes report having received a cash transfer in the past 24 months, while virtually no household in the control group reports having received a transfer. Pooled across the five study regions and conditional on receiving at least one transfer, the mean reported number of transfers received by treatment households at midline is just over 5 in both samples; this number ranges from about 4 transfers in the regions of Segou and Mopti to about 6 transfers for the region of Sikasso, as expected from the government's phased implementation calendar. The mean reported amount received in the last transfer is just over 30,000 FCFA, consistent with a small share of households having received a double transfer.

In terms of targeting within the household, the data confirm that the household head is the primary person engaging with the Jigisémèjiri cash transfer. As intended by the program, the household head is listed as

the main beneficiary in the vast majority of treatment households (over 80 percent in both samples). Household heads, moreover, represent the largest share of individuals that physically collect the transfer (over 80 percent in both samples). In 77 to 78 percent of cases, the household head is also reported as the only individual that decides how the cash transfer is to be used. Although spouses of the head and adult children also participate in some households, their roles are dominated by the role of the household head.

According to households' self-reports on how they used their last cash transfer, the three largest categories of expenses were food consumption, health, and agricultural investments (about 65 percent, 11 percent, and 4 percent, respectively), corresponding to approximately 80 percent of the last transfer received or 24,000 FCFA. Up to 2 percent of the last transfer is reported to be used for savings and less than 1 percent of the transfer is reportedly being shared with family and friends. There are small but non-trivial shares reporting concerns about being able to keep the whole transfer. About 8 percent of the cross-section sample and 6 percent of the panel sample worry about their safety when returning home from the distribution point with the cash transfer, and similar shares worry about having to share the transfer with non-household members (family and friends) who might know that they just received money. Among treatment households who have received a transfer, the reported average time since receiving the last cash transfer is about 66 days for the panel (2.2 months) and about 63 days for the cross-section (2.1 months).

***Beneficiaries' experience with the accompanying measures*:** With the caveat that beneficiary households may have interpreted survey questions on the Jigisémèjiri AM to include only the AM session linked to the cash distribution and not the other AM sessions, we analyze beneficiaries' reported experience with the AM component. Beneficiary reports reflect that adherence to the design and coverage was moderate, but lower than for the cash transfers. About 60 percent of treatment households in both cross-section and panel samples report attending at least one accompanying measure session; it may not be surprising that this is much lower than the share of treatment households that received at least one cash transfer, given that the accompanying measures were not mandatory. The data also indicate some degree of contamination to the control group; 7 to 9 percent of control households report attending at least one accompanying measure session, which is possible, as nothing prevented households in Control communes from attending a session in a neighboring Treatment commune. Out of all the sessions offered before the midline data collection, treatment households in both samples report attending about 4 sessions, on average. This number ranges from about 6 sessions attended in Sikasso to about 1 session attended in Mopti, consistent with the phasing, but reflecting a relatively low share of attendance, assuming that all sessions shown on the phased implementation calendar were, in fact, offered (ranging from about 30 scheduled in Sikasso to about 6 scheduled in Mopti before midline).

In terms of targeting, although the Jigisémèjiri program did not restrict the number of household members that could attend the accompanying measures sessions along with the main beneficiary, in about 60 percent of treatment households that attended any accompanying measure session, only the household head ever attended. In only about 10 percent of cases did the household head and spouse both ever attended, and in about 12 to 15 percent of cases, the mother of the index child in our sample ever attended, which has relevance for the child nutrition outcomes we explore.

Based on self-reports, among treatment households that attended at least one accompanying measure session, their attendance in at least one session on each of the specific subthemes is, on average, above 60 percent. Given the relatively low number of sessions that households reported attending, conditional on any attendance, this suggests that many households may have selectively attended only one session per each group of subthemes or that they may have overreported the number of subthemes to which they were exposed. Alternatively, they may have misunderstood the number of sessions to be specific to the AM

session linked to the cash distribution and not the other sessions. For each theme, reported attendance is higher in the panel sample than in the cross-section sample, which may reflect that households in the cross-section sample that have a child 6–24 months of age are more time-constrained. Reported attendance varies slightly across the subthemes; it is highest for the first subtheme (on the use of cash transfers for essential household needs), then decreases and stabilizes at a quite high proportion of treatment household for the remaining topics.

***Program impacts on household-level outcomes*:** Following the conceptual framework, we begin with household-level first-order outcomes: household resources (which we do not have a direct measure of and therefore do not analyze directly) and transfers in and out of the household. Findings show that, in response to receiving the Jigisémèjiri cash transfers, treatment households increase the transfers they give out to others (voluntarily or out of obligation/compulsion). Among treatment households, the intervention causes a 6-percentage-point increase in the share reporting that they gave any cash transfers to family, friends, and others in the village in the past 6 months; the average amount given comes to approximately 2,490 FCFA from each of the 11 percent of households reporting cash transfers out at midline, or about 4 percent of the cash transfer value received by these households over 6 months. The intervention also causes a 5-percentage-point increase in the share of treatment households reporting any food transfers out to family, friends, and others in the village; this represents a 56-percent increase in the share reporting food transfers out, relative to the 9 percent reporting the same in the control group at midline. These findings indicate that some treatment households do not keep the "full" transfer, implying that overall, they receive less from the intervention than was directly delivered to them. While the amount of cash transfer being shared is small, and sharing likely has positive implications for the community, this finding may play a role in the magnitude of effects found on second- and third-order outcomes.

We next turn to household-level second-order outcomes: consumption, as well as savings and investments. There are no statistically significant impacts on the value of consumption from the combined CT and AM intervention; however, point estimates are considerably higher for households that more recently received transfers. In addition, the CT and AM intervention significantly increases the probability of a household purchasing cereals in bulk. These findings suggest a "lumpy" pattern of consumption expenditures, with more spent on purchases (including bulk food) immediately after receiving the transfer than spent several weeks later. Thus, our results suggest that we do not find statistically significant average effects on consumption in part because the midline data were collected on average 66 days after the last transfer and for many households in the sample, the majority of expenditures from transfers occurred prior to the recommended 7 or 30 day recall period for consumption measures. This also implies that households may have experienced increases in consumption due to the transfers soon after receiving them, but the increases in consumption faded out before the full three months that transfers were intended to cover.

The combined CT and AM intervention also significantly increases household savings and investment. The proportion of households that have any savings increases by 3 percentage points. Given the low rates of saving of households in our sample, this translates to a meaningful 50-percent increase relative to the control. The intervention increases the proportion that invest in small animals by 6 percentage points, with an average amount of about 3,352 FCFA spent in the past 12 months among those who invest. It also increases the proportion that invest in livestock by 11 percentage points, with an average amount of 23,463 FCFA spent in the past 12 months among those who invest. These findings are consistent with households' direct reports of using a substantial share of transfers on "agricultural inputs and investments" (in which they may consider including investments in livestock) and with above-mentioned evidence showing that

larger and less frequent transfer amounts lead to greater use of transfers for investment in assets (Haushofer and Shapiro 2016).

We last consider household-level third-order outcomes: household food security and household dietary diversity, poverty, and asset ownership. The combined CT and AM intervention significantly improves households' food security and hunger situation over the preceding four weeks and increases the proportion of households classified as food secure or having little to no hunger. The program reduces household food insecurity by 1.88 points as measured by the Household Food Insecurity Access Scale, representing a 27-percent reduction in food insecurity. It increases the proportion of households classified as food secure by 11 percentage points and decreases the proportion classified as severely insecure by the same magnitude, representing approximately a 38-percent change relative to the midline control mean. The program also leads to a decrease of 0.25 points in the Household Hunger Scale, representing a 52-percent increase from the midline control mean. Changes across the distribution reveal a 7-percentage-point increase in the proportion of households classified as having little-to-no hunger and a decrease of the same magnitude in the proportion of households classified as having moderate hunger. These findings are consistent with the self-reported use of the transfer on food and the large increases in the probability that households purchased cereals in bulk in the past 3 months. This suggests that findings on food consumption aggregates are not capturing the full story, likely due to effects fading prior to the recall period for food consumption measures.

The combined CT and AM intervention also significantly improves household dietary diversity as measured by the Dietary Diversity Index (DDI) and Food Consumption Score (FCS). The program increases the DDI by 1 item or nearly a 7-percent increase relative to the control group, and increases the FCS by 3.7 points or a 6-percent increase relative to the midline control mean. In terms of food groups, increases on the extensive margin are found particularly for fruits (a 23-percent increase relative to the control mean), as well as for sweets, and spices/condiments and beverages; increases on the intensive margin are also found particularly for fruits (0.51 more days consumed), as well as for vegetables and main staples. Impacts on other high-value nutritious foods, such as dairy, meat, and seafood, are positive, but statistically insignificant.

There are no statistically significant impacts of the combined CT and AM intervention on poverty indicators, based on international poverty lines or the national poverty line, using the poverty headcount ratio, the poverty gap, or the poverty gap index. The poverty gap indicates that the average beneficiary household was so poor in the absence of intervention that the transfer amount was insufficient to lift it above the national poverty line. However, point estimates for program impacts on poverty are negative. Reductions in poverty are larger for households that more recently received transfers, although still not statistically significant, consistent with the findings on consumption expenditures (which are used to construct the poverty indicators). This implies that some households may have experienced reductions in poverty due to the transfers soon after receiving them, but the reductions faded out before the full three months that transfers were intended to cover. Thus, overall, our results suggest that we do not find statistically significant average effects on poverty in part because the size of the transfer amount relative to the poverty gap was not sufficiently large on average and in part because, for many households in the sample, the majority of expenditures from transfers occurred prior to the recommended recall period for consumption measures.

The combined CT and AM intervention significantly increases household asset holdings. Relative to the midline control mean, the intervention increases the value of livestock ownership by approximately 61 percent, the value of transport assets by approximately 64 percent, and the value of the total assets by approximately 19 percent. These are meaningful increases in asset ownership and are consistent with

findings of increased savings and investments (particularly in livestock). Our evidence suggests that the combined intervention led treatment households to significantly increase their asset base, putting them on a pathway to move sustainably out of poverty.

***Program impacts on child-level outcomes*:** Following the conceptual framework, we begin with the child-level first-order outcome (that does not pertain to PNP): maternal knowledge of child nutrition and health. The combined CT and AM intervention has no statistically significant impact on a summary maternal knowledge score on health and nutrition. Mothers at both baseline and midline answered between 5 and 6 of 10 questions correctly, on average. There is also no significant difference in impacts on responses to the sub-questions contributing to this overall knowledge score, except for the appropriate age to introduce liquids to the infant; mothers in the treatment group are 13 percentage points more likely to answer this question correctly than the control group. The lack of impact of the AM intervention on maternal knowledge of child nutrition and health may be explained by limited direct exposure of mothers to AM. Although the first two quarterly AM sessions covered the themes of breastfeeding practices and complementary feeding, and although beneficiaries of all regions but Mopti were scheduled to be exposed to these messages before the midline data collection, this information may not have reached the mother of the child (typically the child's main caregiver). As mentioned above, only 60 percent of treatment households ever attended any AM session, and among those that ever attended, the mother of the index child attended in only 15 percent of cases in the cross-section. Typically, the household head alone attended, which would require transmission of relevant information within the household to the child's mother. Alternatively, if households misunderstood the questions on AM sessions to be related only to the session linked to the cash distribution, mothers' exposure may have been higher than reported, but the exposure was not enough to improve their knowledge.

We next turn to child-level second-order outcomes (that do not pertain directly to PNP): infant and young child feeding practices (as measured by the WHO indicators and as measured by dietary diversity), as well as stimulation practices. The combined CT and AM intervention causes an 11-percentage-point increase in the prevalence of timely introduction of (semi-)solid and soft foods, as well as a 3.6-percentage-point increase in the prevalence of children 6–23 months consuming a minimally acceptable diet (defined by consuming four food groups and the recommended number of meals for a specific child age) relative to the control. It is likely that this change in behavior is due to increased resources more than increased knowledge, as more than half of mothers did not know that breast milk intake needs to be complemented with (semi-)solid and soft foods at 6 months of age, and the program did not increase this knowledge. Results show a surprising reduction in the proportion of children that was ever breastfed, by 2.5 percentage points; however, the prevalence of ever breastfed children remains quite high (≥95%) in both study groups at midline. Exclusive breastfeeding could not be measured in this sample because the cross-sectional sample ranged from 6-to-23 months of age; we did not see any improvement in continuous breastfeeding in older children of 20–23 months, although the small sample of children in this age group reduces our power in detecting impacts.

We do not find any meaningful impact of the combined CT and AM intervention on the mean child dietary diversity score. Breaking down the dietary diversity score into the seven food groups that contribute to the score, we find a small 3-percentage-point decrease in the prevalence of legume and nuts consumption in the treatment group compared to the control group. The lack of impact of the combined CT and AM intervention on the child's DDS may be surprising, given that at the time of the midline survey, beneficiaries of all regions (except for Mopti) were exposed to AM-related group counselling on complementary food practices and, moreover, had access to more resources allowing for the purchase of nutrient dense foods

that would benefit the young child. It is again possible that the targeting of the CT and AM to household heads, and the limited exposure to AM in general, played some role in limiting impacts.

The combined CT and AM intervention also had no significant impact on child stimulation practices, as measured with five key questions used in the Multiple Indicator Cluster Survey to describe interactions between children and their caregivers. This lack of impact could again be explained by the low direct exposure of mothers to the AM component.

We finally consider child-level third-order outcomes: child health status (and associated health-seeking behaviors, which are second-order but analyzed with health status due to how they were asked in the survey) and child nutritional status. The combined CT and AM intervention causes a 4.4-percentage-point reduction in the prevalence of diarrhea in children 6–23 months, but no other reduction in child morbidity signs. The small impact on child diarrhea does not appear to be associated with improvements in caregiver treatment practices in case of child diarrhea, in terms of dietary mitigation strategies, use of oral rehydration solution or zinc, or provision of more liquids and foods. The combined intervention also makes caregivers 10-percentage points more likely to seek treatment if any of the mentioned symptoms occurred in their 6–23 months old index child. However, there is no program impact on the type of health-seeking behavior; more than half of respondents in both groups report seeking treatment primarily in the informal health sector.

The combined CT and AM intervention also does not result in any impact on child nutritional status and growth. Child anemia, stunting, and wasting remains very high at midline. Several studies that have assessed the effects of CT alone on child nutritional status in Sub-Saharan Africa have similarly found no impact (e.g., Houngbe et al. 2017 in Burkina Faso, Seidenfeld et al. 2014 in Zambia, Haushofer and Shapiro 2013 in Kenya). Given evidence that the AM in this intervention had limited effects on first- and second-order outcomes, such as knowledge or practices related to child feeding practices and health, it is not surprising that there is no meaningful impact on the third-order outcomes of child health and nutrition status.

9.2 Policy implications and recommendations

A number of policy implications and recommendations emerge from these findings. We divide them into those related to household-level outcomes and those related to child-level outcomes.

9.2.1 Policy implications and recommendations related to household-level outcomes

The objectives of the Jigisémèjiri program of reducing the vulnerability of poor households have been met. The combined CT+AM intervention leads to significant improvements in household food security, increasing the proportion of households classified as food secure or having little to no hunger. It also significantly improves household-level dietary diversity. These are findings consistent with much of the literature on cash transfers. Additionally, the Jigisémèjiri program reduces determinants of poverty and increases resilience, through strengthening households' savings and asset base. Savings have been shown to allow households to smooth consumption in the face of negative income shocks without resorting to distress sales of assets, as well as allow households to build up funds over time toward larger purchases (Deaton 1989; Gertler, Martinez, and Rubio-Codina 2012). The increases in asset holdings emerge particularly in livestock and transport assets, which evidence shows can generate new income, serve as stores of wealth or collateral, and be sold for income if needed (Moser 1998). These improvements imply long-term benefits for households.

However, we find no statistically significant impacts on the value of households' food and nonfood consumption or on rates of poverty which are constructed from the value of consumption. This is despite households' self-reports of using more than half of their last transfer on food. One potential explanation is that the midline survey interviewed many of them toward the end of a transfer cycle (about 2 months after the last transfer, on average), at which time beneficiaries may have been "running out" of the transfer. Implicitly this suggests there may not be "continuous" improvements in household consumption, but rather somewhat cyclical improvements with the transfer receipt. This pattern is likely to occur in settings where there are barriers to using credit and savings or imperfect food markets, preventing households from smoothing their consumption over time. This could make it challenging for households to spread increases in food consumption throughout the 3-month cycle.

Another potential explanation for limited effects on consumption relates to the dominant role of household heads in engaging with the transfer (from being the named beneficiary to physically receiving it to making decisions about it), to the exclusion of other household members. Household heads are primarily male, and women are engaged with the transfers in only a relatively small share of households. Although there is no conclusive global evidence that targeting transfers to women rather than men causes significant differences in how transfers are spent, there is evidence that women having a greater income share significantly increases the share of household expenditures devoted to household food consumption (Hoddinott and Haddad 1995).

It is also worth noting that a review of evidence on transfer programs in Africa suggests that findings of statistically significant impacts depend on transfer size. Widespread transformative impacts tend to emerge when the size of transfers is about 20 percent or more of beneficiary households' consumption in the absence of intervention (Davis and Handa, 2015). The monthly transfer amount for the CT in Jigisémèjiri is approximately 9 percent of the midline control mean's monthly household consumption, which is below the threshold described. It is also well below the amount needed to bring the average beneficiary household above the poverty line, according to the poverty gap statistic.

Recommendations

- If an objective of the program is to create more "continuous" increases in food consumption, a potential option would be to make the transfers more frequent than quarterly: e.g., monthly such as many other transfer programs globally. This would mean 10,000 CFA monthly rather than 30,000 CFA every three months. Even if households still used most of the cash right away, this could potentially lead to a shorter period between transfers when effects on food consumption had faded out. However, it is worth noting that the frequency of the transfer distribution involves tradeoffs. As Haushofer and Shapiro (2016) show, more frequent transfers could lead to more being spent on consumption, while less frequent transfers could lead to more being spent on lumpy investments in durables and assets. Thus, the choice of frequency may depend on government priorities (i.e., greater priority on increasing food consumption or building assets).
- It may be worth reconsidering the targeting of transfers exclusively to the household head. At minimum, it may be useful to sensitize household heads to view the transfers as intended for the "household" as a whole and to engage other household members in deciding how to use them.
- A larger transfer size would be expected to have larger impacts; a target of 20 percent of monthly household consumption is recommended in the literature. Raising the transfer amount would increase its size relative to the poverty gap and bring it closer to eliminating poverty for the average beneficiary.

Also related, adjusting transfer values for inflation may be considered. The Jigisémèjiri transfer amount has not changed between baseline and midline, while there has been some inflation, which would reduce the "real" value of the transfer.

9.2.2. Policy implications and recommendations related to child-level outcomes

According to our midline survey results, the CT+AM intervention caused a small reduction in the prevalence of child diarrhea, but did not significantly change other measures of child health and nutritional status (including stunting, wasting, and anemia prevalence). We hypothesize that the limited impact on these third-order outcomes is a consequence of a lack of meaningful changes on the first- and second-order child-level outcomes, in particular, maternal knowledge and practices related to child nutrition and health. The only significant effect of the CT+AM intervention on knowledge relates to the appropriate age to introduce liquids to the infant; the only two significant effects on practices relate to the timely introduction of solid, semi-solid, and soft foods and to the minimum acceptable diet indicator.

One possible explanation for the limited effect on first- and second-order outcomes is the low direct involvement of mothers in the AM. Evidence from unconditional CT impact studies (Haushofer & Shapiro, 2013; Houngbe et al., 2017; Seidenfeld et al., 2014) suggests that CT programs without well-attended trainings on child feeding and nutrition are less likely to result in meaningful improvements in child health and nutrition. The data from our midline survey on AM attendance suggest fairly low exposure, with only 60 percent of households having attended even one AM session in the last two years. Among households that ever attended, the mean number of sessions attended is only about 4 over the course of 1-2 years. Moreover, only 15 percent of mothers reported attendance if a household participated. Although some of the nutrition and health messages could be transmitted within the household from the household head to the mother of the index child, we suspect that the low level of direct involvement of children's mothers limited the program's impact on maternal knowledge and IYCF indicators. Alternatively, if households misunderstood our survey questions on AM sessions to be related only to the session linked to the cash distribution, mothers' exposure may have been higher than reported, but the exposure was not enough to improve their knowledge.

Recommendations

- Strategies should be considered to increase the effectiveness of AM sessions at improving mothers' knowledge. This could include re-assessing the curriculum and mode of message delivery. If attendance or exposure among mothers is an underlying issue, potentially the upcoming introduction of the child- and mother-centered PNP component can be used as a rationale for mothers to attend the AM more often and thus make the AM more inclusive. The AM strategy to improve child nutrition and health knowledge and practices could be made more intense by combining multiple delivery channels. Recent evaluations of the Alive and Thrive programs implemented in Ethiopia, Bangladesh and Vietnam show that an intense behavior change model using mass communication, interpersonal and group counselling, community mobilization as compared to standard model improved breastfeeding and complementary feeding practices significantly (Menon et al. 2016a, Menon et al. 2016b; Kim et al. 2016). General behavior change models are not limited to the dissemination of knowledge, but prescribe frequent contacts between provider and recipient to address the barriers and acknowledge of achievements of the desired behavior. AM aimed at changing behavior are thus ideally organized at a sufficiently high frequency with appropriate child age-specific messages on optimal breastfeeding and age-dependent complementary feeding practices.

- If a core aim of the government is to shape the use of transfers using the AM, the two may need to be more closely linked. The AM should ideally offer messages explicitly on the use of CT toward better child health, nutrition, and development. Embedding messages regarding specific recommended behaviors using cash in the thematic AM might encourage participating household members to use their transfer resources accordingly toward their children. Sessions on the use of the transfer, which are typically attended by household heads, should include spouses and other members of the family. Evidence from Bangladesh shows that cash transfers that were combined with intensive high-quality, nutrition behavior change communication involving multiple household members as a "soft" condition were able to significantly reduce child stunting, while the cash transfers alone had no significant impact on child anthropometry (Ahmed et al. 2016). This finding suggests potential for a transfer program to be effective in improving child nutrition if nutrition-specific complementary activities are well-attended, carefully designed, and closely tied to the transfers.

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