Beyond Poverty Reduction: Evidence from a Multifaceted Program on Poverty, Nutrition and Child Development ⋆

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Abstract

We study the impacts of a multifaceted program implemented in Burkina Faso that targets ultra-poor households with young children or pregnant women. The design includes a cash transfer program (T1), T1 plus animal transfer (T2) and T2 plus a nutrition bundle which includes fortified flour, a gardening kit and nutrition education (T3). We find that the program reduces extreme poverty in all treatment branches, but only T3 positively impacts child nutrition. T3 also impacts motor and cognitive development of new born children. Our results suggest that while standard multi-faced programs are effective at reducing poverty, nutritionally focused programs are likely necessary to better address children's long-run earning potential via improved cognitive skills.

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JEL: I38,I32, I31, I15

Evidence from multiple studies (e.g., Banerjee et al. 2015; Bandiera et al. 2017; Angelucci et al. 2023) suggests that multifaceted programs targeting ultra-poor households have significant impacts on poverty reduction, asset ownership, household income, consumption, and business investment and revenues. However, whether such programs can effectively address malnutrition—and thereby enhance child cognitive development— remain an open question, echoing previous debates about calorie-income elasticity (Bouis and Haddad, 1992; Deaton and Subramanian, 1996; Colen et al., 2018; Almas et al., 2023), intra-household allocation (Thomas, 1990) and the limited impacts of cash transfers on child nutrition (Manley et al., 2020). Our comprehensive review

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of the cash literature (available online, Table OB.1) confirms that unconditional cash transfers have minimal effects on child nutrition, suggesting that poverty alleviation programs alone may not be sufficient to improve child nutrition. Can multi-faceted programs that integrate nutrition-focused interventions address children's nutrition, cognitive skills and motor development? This broad question is critical not only because malnutrition affects more than 148 million children worldwide—24 percent of whom live in West Africa—but also because malnutrition leads to irreversible cognitive and motor development challenges (UNICEF et al., 2023). Moreover, given the high negative correlation between malnutrition and future earnings as documented by Alderman et al. (2006) and Hoddinott et al. (2008), programs that address immediate poverty without reducing child malnutrition may fail to durably affect intergenerational poverty. To date, studies on multifaceted programs have inadequately investigated this fundamental causal pathway.

We study three models of a multifaceted programs to assess the relative impacts of cash transfers (T1), cash plus asset (livestock) transfers (T2), and a combination of cash, asset, and a nutrition-focused program including nutrition training and nutrient-rich food transfers (T3). Our assessment spans three follow-up surveys: one conducted approximately one year after the initiation of all transfers (referred to as the *1-year follow-up*), another two years after program initiation (*2-year follow-up*) and one year after the program's completion, and a final survey (*3-year follow-up*), capturing effects two years post-program completion. Our randomized controlled trial allocates one of the three program models at the village level, targeting ultra-poor households with young children or pregnant women across 168 villages in two regions of Burkina Faso. Our approach differs from previous studies by integrating nutrition focused transfers and education to a "standard" multi-faceted program. Additionally, our targeting strategy focuses on households with pregnant women and children less than five years old, a particularly vulnerable demographic during early life stages (Black et al., 2017; Hamadani et al., 2014). We hypothesize that multifaceted programs, if targeted to households with young children or pregnant women, could fundamentally transform the early environment of young children, reducing poverty and malnutrition, which would, in turn, improve children's cognitive skills.

We find four main sets of results: on poverty; food insecurity and dietary diversity; malnutrition; and children's motor and cognitive development.[1] First, we find evidence that the program reduced overall poverty. Using a machine learning algorithm (random forest) trained on our baseline survey's qualitative poverty classification, we predict the probability of being ultra-poor in follow-up survey rounds. One year after the beginning of the interventions, the predicted probability of ultra-poverty significantly decreased in all experimental groups, by approximately 5-7 percentage points (pp) relative to the control group. This reduction persisted in the 2-year follow-up (about one year after the end of transfers) and in the 3-year follow-up, exclusively in the T3 branch. Although the ultra-poor prediction is likely underestimated (as the algorithm sensibility is only 83%), these impacts suggest a remarkable reduction of 50-70% in ultra-poverty within one year of intervention. After two years, significant reduction was only observed in T3 but remained substantial (-50%) and persisted in the 3-year follow-up (-28%), nearly two years after all transfers ceased. These substantial poverty reductions stem from significant financial and agricultural

[1]The findings presented in the paper have been pre-specified in a pre-registration report (PRR) that has been peer reviewed and accepted at the Journal of Development Economics in 2018.

investments, particularly pronounced in T3. Treated households exhibited reduced debt (1-year follow-up), increased savings (1- and 2-year follow-up), expanded land cultivation and ownership (1- and 2-year follow-up), augmented investments in agricultural equipment (all follow-up surveys), and have higher agricultural revenue (1-year follow-up). These economic impacts come along with positive shifts in aspirations and stress reduction. In summary, the multifaceted program effectively enhanced the economic and social well-being of households, with discernible impacts on adults' aspirations and mental well-being.

Second, these impacts extend to self-reported household food insecurity and dietary diversity. In the 2-year follow-up survey, severe food insecurity decreased by 5 to 8 pp across all treatment groups, representing a significant reduction of 22% to 34% compared with the control average. Moreover, the program increased dietary diversity, with significant differences observed in the 1-year and 3-year follow-up surveys. In a survey targeting breastfeeding mothers and pregnant women, we also found that women in T3 exhibit significantly better dietary diversity (+0.31 SD), primarily driven by increased consumption of animal proteins and fruits and vegetables rich in Vitamin A. This result together with our measures of aspirations, which reveals particularly large effect on aspiration with regards to children's education attainment, suggest a shift in household priorities towards education and nutrition in T3.

Third, our child-level measurements, focusing on children below five years old, reveal positive impacts on anthropometrics, predominantly concentrated in the T3 group. In the 1-year follow-up survey, T3 exhibits strong effects on all anthropometric measures, addressing both short-term (severe wasting is down by -1.2 pp from a control average of 2.6% i.e. a 46% decline) and chronic malnutrition (severe stunting is down by 3.3 pp from an average of 12.8 i.e. 26% decline). T3 children also have larger arm circumference (another measure of wasting) and are less likely to be severely underweight (-2.6 pp from a control average of 8.3%, i.e. 31% decline). These impacts remain positive, significant, and mostly amplified in the 2-year follow-up survey. Severe stunting is for instance down by 5.4 pp or equivalent to a 33% decline. Two years after the end of the intervention, the impacts remain significant for chronic malnutrition (+0.12 SD), again exclusively in the T3 group.

Fourth, we find evidence that the program positively impacted the motor skills of already-born children and the cognitive development of newborns who benefited from the program *in utero*. Our measures of cognitive and motor development administered to the children age 3-6 in the 2- and 3-year follow-up i.e., those born before the interventions started, indicates no impact on cognitive ability but significant impacts on motor development, only significant in the 2-year follow-up survey (+0.19 SD). As these results are only significant for T3, we attribute them to the improved nutrition provided to these children. Furthermore, we find evidence that children aged 0-3 years in the 3-year follow-up survey exhibit improved cognitive and motor capacities. None of these children were born at baseline; they were either *in utero* when the program started or received the program in their very early years. These findings suggest that the timing of the intervention is crucial to generate significant impacts on cognitive ability with more pronounced impacts when children are either very young or *in utero*. Our results suggest that better nutrition for young mothers during pregnancy and breastfeeding can have enduring effects on the cognitive abilities of young children.

These results contribute to several strands of the literature on poverty alleviation and nutrition. First, they suggest that multifaceted interventions, such as those highlighted in studies like Bandiera et al. (2017) and Banerjee et al. (2015), which have proven effective at stimulating economic activity, may not be sufficient to reduce malnutrition, when implemented without a specific nutrition program. Our findings, therefore, provide causal evidence supporting the conclusions of the calorie-income elasticity literature, which generally suggests a weak relationship between income and calorie intake. As illustrated in the Panel A of the online appendix Table OB.1, the majority of studies reporting positive impacts on anthropometrics involve cash transfers provided conditionally to health visits (Macours et al., 2012; Kandpal et al., 2016; Evans et al., 2014; Akresh et al., 2016; Galiani and McEwan, 2013). In most cases, unconditional cash transfer programs alone are not sufficient to improve anthropometric measures, with the one exception (McIntosh and Zeitlin, 2024) being significant only for the largest cash transfer amounting to $567, almost three times larger than ours. This suggests that unconditional cash transfer policies may only enhance household investments in early nutrition at a very high cost. Our study also reveals distributional consequences of cash transfer programs on nutrition and diet. We find ultra-poor households in our study use cash transfers for food consumption (68% of cash is used for food) rather than investment. However, this consumption does not seem to benefit the youngest members of the households, as anthropometric measures are not affected in T1 or T2. Our results rather demonstrated that the specific program targeting young children—the one received by T3 households—had a meaningful impact on their nutritional outcomes. Second, our study demonstrates that transfers of nutrient-enriched food, combined with nutrition training and the distribution of garden kits, targeted at very poor households with young or soon-to-be-born children, are highly effective in increasing food security, dietary diversity, and anthropometric measures. This finding aligns with the nutrition literature that suggest the impacts of nutrient-enriched foods, homestead gardening programs and focused nutrition education to enhancing nutrition outcomes (see online appendix Table OB.1 Panel B for a review of the main findings).

Last, our study provides suggestive evidence that the relationship between malnutrition and cognition is not as direct as commonly assumed. While improved nutrition may influence motor skills, we do not find evidence that it directly affects cognitive development. Instead, our findings indicate that nutritive supplementation and improved maternal nutrition have positive impacts on the cognitive development only when the program is provided *in utero* to pregnant or lactating women. Although epidemiological studies have demonstrated that better nutrition impacts fetal brain development and cognitive function (Cusick and Georgieff, 2016), there is little evidence that an at-scale nutritional intervention conducted during pregnancy causally affects child cognitive development, as shown in the most recent systematic review available on this subject (Taylor et al., 2017). Our paper brings a valuable contribution to this literature by establishing a clear causal relationship between mother's nutrition, anthropometric measures and children cognitive development only when the program specifically target pregnant and lactating mothers.

In the rest of the paper, we will first describe the context and content of the intervention (Section 2), the design of the experiment (Section 3) and finally the results (Section 4).

2. Context and program's description

Burkina Faso is among the world's poorest nations, facing profound economic and development challenges. Its GDP per capita stood at only $830 in 2022 (equivalent to $2549 PPP), ranking it as the 17th poorest globally, positioned between Mali and Togo. According to the UNDP, Burkina Faso ranks 184th out of 191 countries in terms of the Human Development Index, highlighting its dire situation. A recent report by Burkina Faso's Minister of Health highlights the severity of the country's nutritional crisis (ENN, 2020). Among children aged 6-59 months, approximately 9% suffer from wasting, 25% experience stunted growth, and 18% face malnourishment. In our study, conducted across 168 villages in the East and Boucle du Mouhoun regions, eligible households (i.e., poor or ultra poor) showed even higher levels of deprivation, with 13% suffering from wasting, 34% stunted, and 28% undernourished.

During our study, the region experienced instability. [2] However, our program's implementation was relatively unaffected. As shown in the online appendix (Figure OA.1), violent attacks in the 15 communes where our experimental villages are located escalated after the program's first year (see Figure 1). The violence continued through 2019, impacting the second year of our program and, to a lesser extent, our 1-year follow-up survey. The onset of the COVID-19 pandemic briefly reduced violence, providing a respite during our 2-year follow-up survey in 2019. In an analysis not shown here, we verified whether these attacks had any impacts on the program's delivery but could not find any major disturbance probably because violence did not affect the first year of implementation and our implementing partner's resilience and experience in difficult program implementation contexts.

The multifaceted program we study aims to enhance the resilience of households vulnerable to food and nutritional insecurity in Burkina Faso. This initiative, funded by the European Union, includes a research component with the overarching goal of developing a sustainable resilience model. The program is implemented by two consortia of NGOs, one in the East region coordinated by *Action Contre la Faim*, the other in the Boucle du Mouhoun region coordinated by Terre des Hommes. Both consortia conducted the program simultaneously over a two-year period in 2018 and 2019 (see Figure 1). Prior to the program's inception, we collaborated with both consortia to design three intervention modalities:

(1) **Unconditional cash transfers ("cash")** were provided to households at a rate of $36 per household per month during the four months of the lean season (June-September) in the first year and $27 per household during the four months of the lean season in the second year.[3] These payments, which were designed to alleviate food insecurity during the lean season, were accompanied by training on their appropriate utilization.

[2] Civil unrest in Libya led to competition among international terrorist groups to control drug trafficking routes in Mali and artisanal mining in Burkina Faso. Instability in Burkina Faso was also partly due to the French army's 2014 intervention in Mali (operation "Barkhane"), which pushed terrorist groups into neighboring countries like Burkina Faso and Niger. Since 2016, Burkina Faso has witnessed increasing attacks by these groups across the country.

[3] equivalent to 20,000 and 15,000 FCFA per month using mid-May 2018

(2) **Productive assets** aimed to enhance household productive capacities through animal distribution. Participants received vouchers exchangeable for animals at designated fairs. In the East region, households received a voucher worth \$80 for poultry or \$207 for small ruminants. In Boucle du Mouhoun, households received a coupon worth \$45 for poultry or \$164 for small ruminants. Differences in animal transfer amounts were, in part, due to regional animal price variation. It was estimated that a typical household could acquire 11 poultry or three goats/sheep with the vouchers. The animals underwent a two-week observation period post-distribution to ensure their health and minimize mortality.

(3) **Nutrition interventions** focused on distributing enriched flour (*Farine Misola*) to children aged 6-23 months and pregnant or lactating women. In the East region, each eligible child received 2.5 kg of fortified flour per month for four months, while pregnant or breastfeeding women received 10 sachets of 67g each per month for four months. In Boucle du Mouhoun, households with children aged 6-23 months received 2.5 kg of flour per child for three months, and each pregnant or breastfeeding woman received 30 sachets of 70g flour per month for three months. Additionally, two training sessions were conducted. The first focused specifically on the enriched flour—how to use it, what to cook with it, and why it matters for health and nutrition. The second was a broader session on the nutrition of vulnerable household members, particularly young children and pregnant women. This session was specifically designed to engage husbands, with the aim of involving them more directly in the nutritional well-being of their most vulnerable family members. Finally, the nutrition intervention also included the distribution of a garden kit, which contained seeds, fertilizer, and pesticides to support the cultivation of small household gardens.

In addition to the three main categories of interventions, households in the communes affected by the experiment (control and treatment groups) would also receive community-level interventions.[4]

3. Design, Sampling and method

3.1. Village Randomization and Targeting

Our sample comprises 168 villages in two regions, randomly assigned to one of four groups. Villages were selected from communes where both consortia operated minimal interventions in the five years prior to the intervention.

Before random assignment, our research team conducted a local census in January 2018, collecting basic information on all households in the 168 villages, including asset and household

[4]These were implemented by the communes in the study zone independently of the village's treatment status. The community-based interventions encompassed awareness campaigns, the management of malnutrition cases in collaboration with the local health system, the establishment of accountability mechanisms, the development of a contingency plan for risk management, the creation of a contingency fund, the establishment of early warning committees, the initiation of a risk-early monitoring system, and community support for planning and developing climate change adaptation activities. Since community interventions are executed at the commune level, villages could not be excluded from them, and both treated and control villages in the same commune could potentially benefit from these interventions.

characteristics to create a poverty index. We then randomly allocated 42 villages to treatment 1 (T1) receiving monetary transfers; 41 villages to treatment 2 (T2) receiving monetary and asset transfers; and 42 villages to treatment 3 (T3) receiving the full treatment package, including cash transfers, asset transfers, and nutrition interventions. Additionally, a control group of 43 villages was randomly selected to receive no specific treatment apart from community-level interventions affecting all experimental villages.[5] The randomization was stratified at the commune level.

We then identified eligible ultra-poor households in each village by conducting a Household Economic Assessment (HEA) (see Figure 1, *HEA*). The HEA, a quantitative and qualitative participatory targeting approach, involved two phases:

(1) **Community-Based Classification (CSE)**: Villagers engaged in a discussion during a community meeting to determine a village-specific definition of poverty. Two selection committees nominated by the community determined criteria and weights, then classified households into socioeconomic categories (ultra-poor, poor, average, or wealthy). Disagreements were resolved through committee meetings and appeals.

(2) **Eligibility determination**: Conducted by the research team based on CSE classification and quantitative census data. Eligible households were classified as poor or ultra-poor with a pregnant woman and/or a child under five. Due to budget constraints, a maximum of 21 households per village could benefit from the program. In villages with more eligible households, the 21 poorest were selected using the poverty index. Conversely, extra spots were redistributed to villages with additional eligible households in the same community.[6]

Each consortium conducted the HEA in its assigned treatment villages, while our research team applied the same protocol in the control villages. The procedure was implemented uniformly across all sites. Importantly, during the HEA, the consortia did not know in which treatment branch the villages had been randomly assigned. Although the HEA was not conducted by the same organization in treatment and control areas, potentially introducing imbalance, we find little evidence of systematic differences between groups (see Table OB2). Furthermore, our double LASSO estimations confirm the robustness of both intermediary and final impacts (see Appendix C). Lastly, in results not provided here, we find no evidence that imbalance differs in the non-eligible group[7] compared to the eligible group, suggesting that the HEA procedure did not alter the composition of eligible households across treatment arms.

[5]This represents a slight deviation from the protocol as we were originally planning to conduct the randomization in two steps.

[6]This procedure was applied uniformly across all experimental groups. It implicitly increased support to villages that reported a higher proportion of poor households. We believe this approach more accurately reflects what would have occurred in the absence of the experiment. In the context of a national scale-up, we would expect a similar pattern—villages with higher poverty levels would naturally receive more support.

[7]Non-eligible households were also identified, though they are not the focus of this paper. As outlined in the pre-registration report (PRR), this group provides a relevant comparison to assess potential spillover effects. While the data were collected and the analysis conducted, most estimated spillover effects are statistically indistinguishable from zero. To maintain brevity and avoid overloading the reader with null results, we have chosen not to report these findings in the main text. However, the results are available upon request.

3.2. *Data and survey*

Following randomization, we conducted four household surveys, each administered between April and June: the baseline survey (2018), and follow-up surveys at one year (2019), two years (2020), and three years (2021) (see Figure 1). All surveys included similar modules covering household revenues, expenditures, investments and assets, savings, shocks, and aspirations.

With the exception of the one-year follow-up, each survey also included a children's questionnaire. This questionnaire comprised standard anthropometric measurements and two cognitive tests: the CREDI for children under age 3 and the MELQO for children aged 3 to 5. The CREDI assesses cognitive and socio-emotional development through caregiver responses and had recently been validated in its full format before the start of our study (Waldman et al., 2021). Unfortunately, a coding error affected the CREDI test in the two-year follow-up survey, rendering the data unusable. As a result, only MELQO scores are available for cognitive outcomes in that year.

The MELQO is an open-source instrument that directly assesses the cognitive and socio-emotional skills of children over age 3. Unlike CREDI, it is administered directly to children. It was developed through collaboration among several international organizations and has been validated in a range of low-income country contexts, including a recent study in Laos (Gomez et al., 2022).

The sample includes approximately 3,500 eligible households at baseline, around 4,000 eligible children under the age of five, and a total of 28,700 household members (see Table OB1). As specified in our pre-registered report, we calculated an ex-ante minimum detectable effect (MDE) of 0.21 standard deviations for comparisons between two experimental branches at the 5% significance level. Ex-post, our realized statistical precision is notably higher, with detectable impacts as small as 0.10 standard deviations. This suggests that the study is sufficiently powered to identify relatively small differences not only between treatment and control groups but also across treatment branches.

3.3. *Protocol validation*

In online appendix Table OB.2, we show that our data does not exhibit significant differential attrition issues in the one and two-year follow-up surveys. Overall attrition rates in the treatment groups are not significantly different from those observed in the control group, with attrition rates around 10% in both the one and two-year follow-up surveys, primarily driven by household attrition (i.e. households that could not be located in surveyed villages). However, in the three-year follow-up survey, attrition rates rise to 22%, mainly due to the tense security situation at that time. This increased attrition is driven by both village attrition (villages not surveyed) and household attrition from those who fled the region for security reasons. To address this, we organized surveys in neighboring villages and transported eligible households from the experimental village, mechanically reducing our ability to survey households. While overall attrition remains not significantly differential, we do observe differential village attrition in group T2, which may affect the validity of our results in T2 in the three-year follow-up sample.

We also verify sample balance using our balancing data. In Table OB2, we present how our main indexes relate to treatment variables at baseline. Although we observe some weakly significant imbalances, after adjusting for multiple hypothesis testing using the false discovery rate

(Benjamini et al., 2006), none of the q-values are significant. However, these results suggest occasional imbalance. As a robustness test for our main results, we will conduct a double LASSO estimation using all baseline indexes, their squares, and their cubes in the algorithm to control for potential imbalances.

3.4. Empirical Method

We provide intention-to-treat (ITT) results, estimated using strata fixed effects (commune fixed effects used for stratification) and clustering at the village level (the level of randomization) for each treatment (*T1, T2, T3*). Given the multifaceted nature of the intervention, Local Average Treatment Effect (LATE) interpretation becomes challenging, particularly in T3, where the intervention consists of several primary components (cash, asset, nutrition) with varying levels of compliance. Given the high compliance observed (see Section 1), the ITT results closely approximate the potential LATE in any case.

We present results controlling only for strata fixed effects (commune fixed effects).[8] To account for potential baseline imbalances, we adopt a double LASSO as a robustness test.[9] We include in the double LASSO seven primary indices defined at baseline. To handle missing values in these indices, we impute them with the mean of the respective index, and we introduce an indicator variable assigning imputed observations a value of one. Additionally, we include the second and third-degree polynomials of these variables. In the double LASSO algorithm (Belloni et al., 2013), we use all these variables (22 in total, including the imputation indicators). In addition, we constrain the algorithm to retain the strata fixed effect. When available, we add the corresponding baseline outcome: for instance, when we measure the impact of the program on height-for-age, we add the measure of baseline height-for-age to the list of control variable in the double LASSO algorithm.

Initially, in our PRR, we planned to conduct heterogeneity analyses based on network proximity and a wealth index. However, considering the risk of multiple hypothesis testing and the limited sample size within each treatment group, we believe that exploring heterogeneous impacts within sub-divisions of an already economically deprived population may not yield highly informative results. Therefore, in this paper, we prioritize presenting the main impacts on all eligible households.

Finally, all specifications in our analyses include standard errors that account for village clustering. Acknowledging the challenge of multi-hypothesis bias arising from the numerous tests conducted (3 treatments over 4 surveys and across various dimensions of poverty), we adopt a two-fold strategy following Anderson (2008). We first address this issue by reducing the dimensionality of our tests through the creation of aggregated indices and sub-indices. In line with our pre-analysis plan, we predefined a set of indices based on data collected at each survey round. Each

[8]This specification closely follows specification (1) in the PRR, but replaces region fixed effects with commune fixed effects. This adjustment is more consistent with the randomization strategy, which was stratified at the commune level.

[9]Using the double LASSO approach represents a deviation from the original PRR specifications, which relied on a pre-determined set of baseline covariates. However, double LASSO has become a more widely accepted method since the PRR was written, offering improved transparency and enhanced precision in variable selection and estimation.

index captures a distinct dimension of capital accumulation and is formed by averaging standardized individual items. Specifically, we compute four individual indices of capital accumulation:

(1) The *agricultural equipment* index consolidates measures of agricultural equipment owned by households (e.g., the number of pickaxes owned).

(2) The *livestock* index combines measures of animals owned (e.g., the number of chickens owned)

(3) The *farming* index aggregates various measures related to the agricultural property of the household (e.g., the number of parcels, overall size of agricultural property).

(4) The *saving* index encompasses different measures of net savings (e.g., the number of saving accounts, amount saved).

Using these four indexes, we create an aggregated index of wealth, referred to as the *wealth aggregated index*, consolidating information from all individual indexes. To form these indexes, as per the pre-registration report (PRR), we follow (Kling et al., 2007) i.e. we standardize each item by survey year and then take their average.

We use indices to reduce the dimensionality of our dataset our dataset, but each survey round still entails numerous hypothesis tests, potentially leading to multi-hypothesis testing bias. To address this concern, we calculate q-values for key analyses using the false discovery rate (FDR) method (Benjamini et al., 2006).[10] We control for multi-hypothesis testing annually, recognizing that impacts over time are highly correlated and essentially measure the same outcome. Additionally, we exclude the aggregated index from multi-hypothesis testing, as it represents the average of the sub-indices.

While we control for multi-hypothesis testing when analyzing our indices and sub-indices, we do not do so for the more granular analysis of our impacts, i.e., when analyzing the effect of items composing each index. Controlling for the false discovery rate (FDR) at this level could be impractical due to the large number of tests and potential lack of statistical power. However, presenting specific results is crucial for analysis. Therefore, in addition to presenting results without controlling for multi-hypothesis testing, we track, by treatment group and survey rounds, the share of significant hypotheses reported. To be conservative, we exclude compliance-related hypotheses from this analysis, as they are expected to be positive and strongly significant. Instead, we focus on hypotheses for which the sign and significance are *a priori* undetermined.To strengthen the analysis, we re-estimated all hypothesis using a double-LASSO algorithm.[11] Similarly, we track

[10]This method suits our context well, assuming strong positive correlation among final outcomes and minimal negative correlation.

[11]The set of baseline variables used for double-LASSO is composed of all available baseline variables, to which we subtract text variables and collinear variables (perfectly or with correlation above 90%). Since double-LASSO does not handle missing values, we impute each missing value and create a dummy variable taking value 1 if the observation is imputed. We include these dummy variables to the set of covariates. Finally, we take the square of all non binary variables.

for every double-LASSO estimates, the share of impacts significant at 10 %. We present the share of hypothesis significant at 10%, with and without double-LASSO controls in Table 8 and we will analysis our findings in the result section below.

4. Results

We present the results based on the hypotheses laid out in the pre-registration report (PRR) where we estimate program model impacts from the participation decision, analyzed using several compliance measures (sub-section 4.1), to children's cognitive impacts (sub-section 4.4). Between compliance and cognition, we first analyze how the program impacted household welfare by measuring effects on poverty status, wealth and asset (sub-section 4.2) and then its impact on nutrition, cognition and child development (sub-section 4.3).

4.1. Compliance

Table 1 provides the compliance level for the interventions. For our main interventions, compliance is very high for cash (about 95%), slightly less satisfactory for asset (about 75% of the T2/T3 households received animals) and nutrition (about 65% of T3 households received enriched flour). The control group received minimal equivalent interventions even when considering programs offered by other NGOs or government entities. We attribute the lower compliance in T2 to security concerns in certain communities. In some villages, the distribution of animals could not be carried out safely. In both T2 and T3 villages where animal transfers were not feasible, it was decided to provide eligible households with agricultural inputs of equivalent value—such as fertilizer, seeds, cereals, or pesticides. The imperfect compliance in the nutrition branch is likely due to the restricted target population for enriched flour distribution, which included children between 6 and 23 months and pregnant women while all families with a pregnant woman or a child below five were eligible. Despite minor deviations, interventions strictly adhered to the experimental protocol.

Table 1 also reveals higher cereal transfers in the treatment groups, particularly in T2 and T3. The larger T3 effects could stem from households considering enriched flour as a form of cereal transfer, possibly leading to reporting inaccuracies. Moreover, the larger cereal transfers in T2 and T3 may be due to distributing cereals in communes where animal transfers were impractical due to the security situation. Similarly, the larger transfer of inputs (including hybrid seeds, fertilizer or pesticides), especially in T2 and T3, results from interventions provided to villages unable to receive animals and, to a lesser extent, for specific interventions like the lowland management assistance program.[12] Lastly, T2 and T3 households are more likely to attend training programs compared to C and T1 groups, due to training associated with animal distribution in T2 and T3 and nutrition programs in T3.

In Online Appendix Figure OA.3, we show that approximately half of the transferred animals died within a year of the transfer. Among the animals distributed in T2 and T3 households, only half survived after one year. This decline corresponds to a mortality rate of 29% for sheep and 59%

[12] Lowland management program, part of the commune level interventions, consists in helping villagers to manage irrigation systems in lowlands. This intervention was provided to all treatment villages with access to lowlands.

for chickens. In contrast, goats, distributed less frequently and with lower value, exhibited a lower mortality rate of 13% after one year. While avian influenza during the intervention period may have contributed to this high mortality rate, factors like households' lack of experience and limited access to veterinarian care also play a role. These findings, alongside distribution challenges in some communes, raise concerns about the effectiveness of animal transfers in regions with limited access to veterinary care. Addressing these challenges is vital for the success and sustainability of similar interventions in the future.

Lastly, we asked household heads about their cash transfer utilization. In the Online Appendix Figure OA.2, we present cash utilization across all treatment groups in the same pie chart, as no statistical differences were found between them. The responses show that the majority of the cash (68%) was used for food purchases, followed by investments in agriculture (11%), health (9%), and education (4%). "Other spending" (8%) includes items like clothing, non-agricultural equipment, with cellphones being prominent and celebrations. The substantial allocation for food purchases unsurprising given that we targeted extremely impoverished households, often struggling to meet basic needs. Given this cash utilization pattern, we have reasons to anticipate potential impacts of the program on food security and nutrition.

4.2. Poverty, Wealth and Assets

Poverty — We begin our analysis by examining the program's effects on household poverty. Using the qualitative and quantitative categorization established during the HEA and the extensive baseline dataset collected before the beginning of interventions, we predict the probability of being classified as ultra-poor (instead of simply poor) in successive surveys. This approach ensures transparency and grounds the poverty assessment largely on the qualitative categorization determined by households themselves during the HEA. We derive the predicted probability of ultra-poverty from a selected set of baseline variables possessing two key properties:

(i) they must have been consistently collected in all surveys and;

(i) they should be potentially affected by the intervention.

Property (i) excludes variables like child cognitive tests, not administered in the 1-year follow-up survey, and measures of aspirations collected only in later surveys. Property (ii) excludes baseline variables that are unlikely to be affected by the intervention, such as adult education or literacy levels. The final set of baseline variables used in the model comprises 84 variables, including 50 original ones to which we add their polynomial of degree 2 and 3 and drop those which are multicollinear.[13]

We use the 84 variables to predict the expected poverty categorization among eligible households in follow-up surveys. As eligible households are all either ultra-poor or poor, the prediction focuses on identifying the ultra-poor households among them. To make the best prediction possible, we divided our sample into *training* and *test* sets and compared the known categorization at baseline with the predicted one using different algorithms (Logit Lasso, logit elasticity net, and

[13]We exclude perfectly and imperfectly multicollinear variables (i.e., correlated above 90%) to enhance the algorithm's performance.

random forest). Accuracy, defined as the proportion of households correctly categorized at baseline, is our decision criterion. Parametric methods (LASSO and Elasticity net) showed similar and poor performance, regardless of the selection method used (cross-validation, plugin, BIC, or adaptive), with an accuracy rate of 61.3%, just above chance. In contrast, random forest demonstrated significantly better results with an accuracy rate of 82.6%, leading us to adopt it as our primary predictive strategy.

We present our findings in Table 2.[14] In the 1-year follow-up survey—conducted approximately nine months after the initial transfers—the predicted probability of being ultra-poor declined by 5-7 percentage points across all treatment arms. Compared to the control groupâĂŹs estimated 10.4% predicted probability (which is likely slightly underestimated, given the 84% sensitivity at baseline[15]), these reductions suggest that between 50% and 70% of treated households exited ultra-poverty within a year.

However, these effects did not persist in the T1 and T2 groups: reductions in ultra-poverty were no longer statistically significant beyond the first follow-up. In contrast, the T3 group sustained significant reductions in predicted poverty nine months after the final transfers (i.e., at the 2-year follow-up), though the effect size was smaller. By the 3-year follow-up—nearly two years after the last transfer—ultra-poverty remained 29% lower in the T3 group relative to the control. However, under the double LASSO specification, this final effect is no longer statistically significant (p-value = 11.2%).

In Online Appendix Figure OA.7, we present the top 10 variables used in generating the 1000 trees of the random forest prediction model. As expected, the algorithm frequently includes various poverty and asset indices, such as wealth, farming, and animal ownership. It also often includes baseline outcomes related to nutrition, such as food expenditures, the anthropometrics index and its cube. This selection of variable sheds light on why this poverty measure is more affected in T3 than in other branches. It reflects the notion that poverty, as defined by households themselves during the HEA, is closely tied to the perceived ability of village members to provide food for themselves and their families.

Wealth Indexes — Alongside using random forest to measure poverty, we use a more conventional approach to assess asset accumulation and investment in Table 2. Our aggregated index (*wealth_index*) indicates a significant treatment effect about a year after transfers began (+0.2 SD in T3), more pronounced and lasting in T2 and T3. These impacts stem from substantial 1-year increase in agricultural assets (+0.45 SD in T3), livestock (+0.38 SD in T3), and to a lesser extent, farming (+0.15 SD). After two years, impacts diminish but remain significant in T3, suggesting that the cash-only program has a short-lived effect, consistent with several other results in the literature (e.g., Baird et al. (2019)). By the 3-year follow-up, most impacts become statistically insignificant in all groups except for the agricultural asset index, remaining significant in T3. Controlling for multi-hypothesis testing does not alter our main results, which remain significant in the first-year follow-up survey, less so after one year, and generally not significant in the 3-year

[14]Importantly, this approach to measuring poverty was not included in our pre-registered analysis plan. For this reason, we also report a more traditional wealth index in the subsequent analysis.

[15]Sensitivity, or the true positive rate, refers to the proportion of ultra-poor households correctly classified in the test sub-sample, representing 30% of the baseline sample.

follow-up.

To verify the robustness of our wealth findings, we use a double LASSO approach outlined in Section 3.4. The results of the double LASSO are presented in the Online Appendix Table OC.1. While the magnitudes of the impacts on wealth are generally slightly lower, the significance of our tests improves due to a substantial reduction in standard errors. In the first follow-up survey, the results in Table OC.1 closely align with those presented earlier. This time however T3 impact is significantly larger than T2 and T2 significantly larger than T1 (bold coefficients). In the 2-year follow-up survey, impacts are actually more significant with the double LASSO approach, especially for the farming index, which is significantly larger in T2 and T3 (although only marginally when accounting for multi-hypothesis testing). In the 3-year follow-up survey, results are generally not significant, except for agricultural equipment in T3.

Asset accumulation — In more granular results, the significant impact on the agriculture index mainly stems from tool purchases (e.g., rake, shovel, sickle) during the experiment's initial two years (results not given here). Consistent with the impacts on the index and the experiment's design, these effects, more pronounced in T2 and T3 during the first two follow-ups and persist significantly in T3 during the 3-year follow-up survey. Additionally, Online Appendix Table OB.3 shows that livestock increases by about 4 additional animals per household in T2 and T3 during the experiment's first year, while, as expected, the T1 group is unaffected. However, after two years, livestock holdings are only slightly higher in the treatment groups compared to the control group, likely due to high animal mortality rates during the initial implementation period. Table OB.3 also shows that the selling price of animals declined in the T2 and T3 groups during the study period. While this outcome was not pre-specified in our pre-registration report (PRR) and should therefore be interpreted with caution due to the risk of selective reporting, the observed decline is substantial—around 30% after one year—and is limited to the T2 and T3 groups, suggesting it may be a direct consequence of the animal distribution intervention. After two years, the price drop persists in the T2 group, though to a lesser extent (-22%), raising concerns about potential unintended effects of livestock transfers on local market prices.

Lastly, we examine the program's effects on individual items that composed the farming index. Table OB.4 reveals increases in the number of cultivated crops, fertilizer plots, and agricultural revenue in the one-year follow-up. In the 2-year follow-up survey, the number of cultivated plots and the size of the cultivated plots increase, primarily significant in T3, albeit marginally, but these impacts are generally not sustained over time. These impacts are sizable, though: after two years for instance, the property size of the T3 households increases by 0.38 hectare, a 14% rise compared to the control group. Similarly, despite no overall impact on the saving index, we observe isolated impacts on savings, especially after one year (Online Appendix Table OB.5). Treatment households appear more likely to save, to reimburse outstanding loans, and to limit contracting new loans in this period. While these effects are uniform across treatment groups initially, suggesting a direct effect of the cash transfers, they diminish after one year, except possibly in T3, where households indicate a higher propensity to save in the subsequent follow-up surveys.

Finally, we provide in the appendix two additional sets of results that were pre-specified in the PRR, focusing on exposure to shocks (Table OB9) and business activity (Table OB8). We find no significant impacts on the number of shocks experienced by households and almost no effects on business income, profits, or asset values. In additional (non-pre-specified) analyses not reported in the paper, we also find no impacts on self-reported crimes or on an index of social cohesion.[16]

In summary, our measures of wealth and poverty indicates positive impacts of the program on treated households' financial well-being. They exhibit reduced poverty, increased wealth, higher savings, and a greater likelihood of loan reimbursement. Moreover, they possess more assets, including larger agricultural properties, increased livestock, and more agricultural equipment. These findings suggest that a portion of the transfers was invested in household economic activities, aligning with expectations. Economic impacts are more pronounced and long-lasting in branches with larger transfers. To ensure these impacts are not artifacts of multi-hypothesis testing, we track the proportion of significantly different hypotheses at 10% significance level in Table 8. It confirms that T3 interventions consistently influence final outcomes across the one-year (65% of significant hypotheses), two-year (46%), and even three-year follow-up surveys (16%). While T1 and T2 exhibit lower shares of significant hypotheses, they remain above 10% in the first and second follow-up surveys but goes below 10% in the 3-year survey. Across treatment branches, our results are as expected: the T3 branch displays a larger number of significant results than T2 and T1 branches (column *T3-T1* and *T3-T2*). The difference between T1 and T2 being only marginally above the 10% significance bar (column *T2-T1*). Results from the double LASSO analysis, designed to address initial imbalances, actually reinforce these findings, particularly in the last follow-up surveys where T1 and T2 also demonstrate an overall significant effect on final outcomes. T2 also appears to outperform T1 in all survey rounds.

While positive, the economic impacts of this program are weaker than those observed in comparable studies, notably Banerjee et al. (2015), Balboni et al. (2022), and Banerjee et al. (2021a). Moreover, our effects appear to be more short-lived, although longer-term follow-up would be necessary to draw definitive conclusions.

We believe several factors help explain the relatively limited impacts on economic activity. First, as noted and discussed below (Section 4.5), the overall size of our intervention was at the lower end of the spectrum compared to other comparable programs—approximately $880 PPP, whereas others range between $1,000 and $3,000 (see Section 4.5). Second, the animal transfer component did not proceed as intended: some villages never received animals, and in others, substitute programs had to be implemented (see Section 4.1). Third, as previously discussed, a significant number of animals died within the first year of distribution. While avian influenza may have contributed, the broader challenges of the northern Burkina Faso context likely made livestock maintenance particularly difficult. Finally, given the severity of poverty in the region, most households appear to have used the cash primarily for food consumption (Figure OA2), investing only a small portion in productive assets. While direct comparisons are difficult, we believe our study population was substantially more deprived than those in comparable interventions, with fewer opportunities for income generation and entrepreneurial activity and with a transfer of smaller magnitude.

This interpretation is consistent with the findings of Balboni et al. (2022), who show that

[16] Some other pre-specified outcomes could not be collected. This includes anemia, which was dropped due to budget constraints, and a measure of social networks, which was ultimately only collected at baseline because of its length and the logistical burden of implementation.

escaping poverty traps requires a critical threshold of capital accumulation. In our context—characterized by extreme poverty and an imperfect, short-lived asset transfer—most households likely failed to reach this threshold and thus may remain trapped in poverty. That said, the strong results on nutrition, anthropometrics, and early cognitive development presented below suggest that the T3 bundle may generate longer-term benefits through alternative pathways—namely, investments in human capital.

Aspirations — To delve deeper into how the impacts on poverty and wealth influenced household well-being, we administered an aspiration test in the last two follow-up surveys, where households assess their own socio-economic status and the level they aspired to reach in the future. After two years, treatment households assessed their current status as more favorable than the control group, particularly in terms of land area and education and they also *aspire* to even better economic situation. In comparison to the control group, T3 households hope to acquire an additional 0.5 hectares of land and 1.4 more years of education for their children. This suggests a higher level of optimism among treated households regarding their future, again specifically concentrated among T3 households.

The substantial increase in education-related aspirations, despite the program not being primarily focused on education, may indicate a shift in household priorities toward education, childcare, and broader human capital investments. However, at the three-year mark, intention-to-treat estimates show that aspiration levels were no longer significantly different from zero across all treatment groups. Nevertheless, estimates using the double LASSO approach (see Online Appendix Table OC.2) confirm that the impacts on aspirations remained large and statistically significant at two years and even increased in magnitude (but not significantly so) by the third year—again, only among T3 households.

4.3. Food security, Nutrition and Anthropometrics

In addition to highlighting significant improvements in wealth and poverty reduction, this paper focuses on exploring the potential consequences of these outcomes on food security, children's nutrition, and cognitive development. With 68% of the cash reportedly spent on food consumption, we may reasonably expect notable impacts on nutrition. However, in T1, where only unconditional cash distribution occurred, there is no guarantee that more food was provided to the members who may benefit the most from a better nutrition, such as pregnant women, breastfeeding mothers or children below 3 years old. Similarly, while poverty impacts are larger in T2, there is no guarantee either that the reduction in economic poverty went hand in hand with a better nutrition. This section delves into the program's impact on nutrition, starting with food security outcomes and then children's nutritional status using anthropometrics measures.

Nutrition — We present the results on food insecurity, dietary diversity, and the nutrition of pregnant and breastfeeding mothers in Table 4. In the first follow-up survey, we observe a significant reduction in food insecurity by -6.2 percentage points in T3 and lower and insignificant impacts in T1 and T2. We also find impacts on dietary diversity but this time concentrated in T1 and T2. By the second follow-up, the reduction in food insecurity become significant in T1 and T2 but is not anymore in T3 (-4.8 pp, albeit close to significance level with p-value=10.5%). However, the coefficients across treatment branches show no significant differences. By the third follow-up, impacts diminish, except for potential effects on dietary diversity, only significant in T2. Interestingly, using the double LASSO approach (see Online Appendix Table OC.3), results are robust and more precisely estimated. For instance, after one year, severe insecurity decreases by 5.4 pp in T3, highly significantly. After two years, reductions in severe insecurity are observed across all treatment branches (around - 4pp or -18% compared to the control mean), with again high significance level. In terms of food diversity yet again, the T1 and T2 groups show stronger impacts than T3. In conclusion, severe food insecurity has improved but necessarily more in T3, THE nutrition-focused treatment arm.

Although improvements in perceived food security are insightful, they are supposed to capture household level food insecurity and do not necessarily gauge the impact on pregnant or lactating women or young children's nutrition. To address this issue, in the 2-year follow-up survey, we administered a tailored questionnaire module directly to pregnant/lactating women to assess their nutrition over the past seven days. This module is less prone to response bias than the food security questionnaire and better targeted to the population of interest. Table 4 highlights significant impacts on the nutrition practices of pregnant and lactating women, particularly evident in T3 (+0.31 SD or 0.32 SD when estimated using double LASSO as shown in the Online Appendix Table OC.3). We attribute this effect to the nutrition training sessions that these women received in T3, covering nutrition during pregnancy and breastfeeding. Additionally, results not presented here show that the impact is driven by increased intake of foods particularly beneficial during pregnancy and breastfeeding, such as meat, fish, vitamin A-rich legumes and fruits, cereals, and fruits and vegetables in general. This suggests that in T3, nutrition information combined with cash and asset transfers altered maternal nutrition practices. We consider this result as a crucial pathway to explaining our impacts on children's anthropometric measures.

The fact that food diversity did not improve in T3, while the nutritional status of pregnant and lactating women did, suggests that although all program branches may have contributed to general improvements in household food security, only T3 had a targeted nutritional impact on pregnant women—and, by extension, on young children. This pattern indicates that T3 may have shifted household priorities toward enhancing early childhood nutritional intake and, more broadly, increasing investment in early human capital. This interpretation is consistent with the aspiration results discussed above, which also point to a reorientation of household goals toward education.

Anthropometrics — We present our anthropometric measures in Table 5, revealing robust, significant, and lasting impacts of the T3 intervention. After one year, severe wasting decreases by almost half, severe underweight by 31%, and severe stunting by 26% for T3 children, while other treatment children are unaffected, indicating that the nutrition program, including training and enriched flour, had substantial and significant effects on early nutrition and growth. These positive impacts persist and strengthen over two years, with severe stunting decreasing by approximately 33% and 41%, respectively, compared to the control group. The anthropometrics impacts in T3 are also quasi-systematically significantly larger than the ones in T1, suggesting that cash alone is unlikely to directly affect nutrition and child development. The comparison between T3 and T2 yields a more nuanced interpretation: point estimates in T2 are roughly half the size of those in T3 and the differences in coefficients are not always statistically significant. This may indicate that the T2 package had some effect on child anthropometrics, but at a magnitude too small to be reliably detected in our study. This being said, in our context, the nutrition intervention emerged as a crucial factor in achieving these impacts. Once again, results are reinforced when using a double LASSO approach (Online Appendix Table OC.4): in the 3-year follow-up survey, for instance, while the effect was barely significant for chronic malnutrition in T3 in Table 5, the impact is of similar magnitude (+0.115 SD) but this time strongly significant.

The impacts reported in year 3 may appear more disappointing at first glance, with T3 impacts becoming insignificant for weight for age and only barely significant for height for age. While it is reasonable to expect that weight-for-age gains might diminish whenever the transfers are interrupted, the effects on height-for-age are typically considered more persistent. However, we believe the year 3 impacts are likely misleading due to cohort dynamics. In year 1, the intervention began approximately nine months prior to the survey, meaning that virtually all children aged 0-6 had already been exposed to the nutrition package—some in utero, others directly. The same applies to year 2 results: with the second phase of the intervention also implemented nine months before the survey (see Timeline Figure 1), nearly all children in the 0-6 age range were exposed to the nutrition component—either in utero, during one lean season, or across both lean seasons. By contrast, in year 3, a substantial portion of the sample no longer had direct exposure to the T3 intervention. Specifically, children conceived after August 2019 or born after June 2020, which represents 11.3% of the sample, were not directly affected, as they were conceived after the final distribution of enriched flour. On these children, in results not reported here, we find for instance zero impacts on height for age while those born before June 2020 gain 12.8% of a SD. In addition, children who were age three or older at baseline—and thus received full exposure to the intervention—had aged out of the 0-6 measurement window by year 3. Given these shifts in sample composition, the fact that the height-for-age impact in year 3 (11.3%) is not substantially lower than in year 2 (18.2%) suggests that there may, in fact, be no meaningful fadeout effect at all. Rather, the attenuation is likely an artifact of the changing cohort structure.

Three plausible and complementary explanations can account for the T3 anthropometric impacts. The first, and like the most influential, is that the enriched flour, distributed during the pregnancy and during the lean season, was particularly effective at improving children's anthropometrics. The literature on enriched flour and food supplementation does indicate that enriched flour is a valuable strategy to improve nutrition, as shown in the Nutrition panel of the Online Appendix Table OB4. Yet, the size and duration of the impacts seem to suggest that enriched flour by itself may not entirely explain all the results. Another complementary explanation is that the nutrition training, combined with the flour distribution, possibly spurred improved household nutrition practices, enhancing anthropometric outcomes. The evidence of improved nutrition practices among pregnant and lactating women supports this mechanism. Lastly, the emphasis on nutrition in T3 interventions may have shifted intra-household priorities. While T1 and T2 households may have distributed resources more evenly across household members, T3 households may have prioritized more vulnerable individuals, specifically those targeted by the nutrition component. This interpretation is further supported by aspiration measures reported above which reveal a significant shift toward education aspirations, despite the program not being explicitly designed to influence this dimension.

4.4. Children's cognitive development

Our cognitive measurement relies on two separate tests that we originally planned to administer in the 2- (nine months after the end of all transfers) and 3-year follow-up survey (almost two years after the end of all transfers): the CREDI, administered to caregivers for children aged between 0 and 36 months, and the MELQO, administered directly to children aged above 36 months and up to 6 years old. While we had prior experience with the MELQO test, a coding error in the CREDI rendered our 2-year follow-up results unusable.[17] Consequently, in Table 6, we chose to omit the CREDI 2-year follow-up results from our analysis.

CREDI — Using the 3-year CREDI test, we observe moderate-to-small impacts across all dimensions except for mental health. The aggregated CREDI score and the language sub-index show significant impacts, particularly in group T3, which experienced substantial and lasting anthropometric effects due to nutrition interventions. These findings align with our original theory of change that connects nutrition and cognition. As depicted in the Online Appendix Figure OA.4, the cohort of children whose caregivers took the 3-year follow-up CREDI test (born between July 2017 and July 2020) were either *in utero* (born between June 2020 and October 2017) or very young (born before December 2018) during the program's implementation. This suggests that the nutrition intervention is particularly effective when implemented very early, during lactation or pregnancy. Once again, using the double LASSO approach, impacts are similar and more significant (see Online Appendix Table OC.5).

The administration of the CREDI directly to caregivers could introduce bias in our context, potentially leading to over-reporting of positive developmental steps if mothers who provided more food during infancy tend to be more optimistic. As children are less than 36 months, they can not report for themselves, so parental respondents are the best alternative as objective measurement from enumerators would also be subject to potential misreporting. This concern has not been reported as a primary issue in the CREDI test validation (Waldman et al., 2021), our context, where households report higher levels of aspiration and optimism (see Table 3), warrants consideration of this potential bias. The absence of CREDI impacts in T1 and T2 partly alleviates this concern, as increased optimism would likely affect all treatment branches. However, the caregiver may have modified their answers depending on whether they received the nutrition package or not. since the nutrition training did cover the relationship between nutrition and development, the concern may still persists. Some of results in Table 6 should alleviate the remaining concerns however. First, the fact that the CREDI mental health sub-score is unaffected in T3 suggests that caregivers did not overestimate overall developmental progress, as the nutrition package is unlikely to have impacted mental health. If caregivers overestimated their children's developmental steps, they should have done so for all sub-categories of the test, including mental health. Second, analyzing program impacts in T3 by age group reveals expected treatment heterogeneity, supporting again the notion that CREDI scores were not uniformly inflated. For instance, we find no treatment effect in the 3-year follow-up CREDI test on children below nine months old. This is exactly what we would have expected since these children were too young to have benefited from the program either *in*

[17]The coding mistake related to the specific stopping rule used in the CREDI.

utero or post-natal. Once again, if the CREDI scores were driven upward, we would have expected all children to show positive score, not only the ones who benefited from the program the most.

MELQO — Results from the MELQO test mostly show non-significant differences from zero, as depicted in Table 7. However, in the 2-year follow-up survey, we observe one positive impact in motor development, once again in T3 and in a domain (motor skills) expected to be influenced by nutrition interventions. Interestingly, children assessed in the 2-year follow-up were already between 13 and 35 months old when transfers began and while they benefited from the interventions during 16 months, they were not treated prenatal or during the breastfeeding stages. The limited impacts on these children support the notion that nutrition interventions are most effective when initiated early, ideally during pregnancy.

In the 3-year follow-up survey, results are similarly not significantly different from zero. In comparison to the two years test, the children who took the three years tests were younger (between 4 and 26 months old) when the program started but they still did not benefit from the program during their mothers' pregnancy (see Online Figure OA.6). We further investigated whether younger children (typically below 48 months and who benefited from the program early) were more affected but could not identify any treatment variation by age. This further suggests that the *in utero* period may be particularly crucial for the efficacy of the nutrition program.

4.5. Relative Bundle Values

Finally, we provide in Table 9 the perceived value of each intervention over two years of implementation, as reported by respondents. Across all three treatments, households reported receiving approximately \$214 in cash over two years (or \$227 for those who received any cash), fairly aligning with the objective to distribute \$252 per household. For those who received cash, this corresponds to \$604 PPP in 2018, slightly below typical cash amounts transferred in comparable multifaceted programs.[18] Animal transfers were valued at \$74 (in ITT terms) for T2 and T3 households, or \$97 for households in T2 and T3 reporting receiving at least one animal. Although slightly lower than expected values (estimated around \$129 by our implementation partners), it is consistent with the program's design.[19] The value of other interventions appears more marginal: \$15 for enriched flour transfer in T3 (\$23 for households receiving it), about \$3 for cereals, and between \$3 and \$11 for input transfers. It is worth noting however that these input, cereals and flour transfer do not include implementation cost which may be high in this context.[20] Overall, treatment households estimate the program to be worth about \$275 over two years, significantly smaller in T1 (\$213) and significantly larger in T3 (\$328). Given the 2018 national poverty line was \$284 per year (INSD, Janvier 2022), and assuming most households in our sample fall below it, our intervention is at least equivalent to one poverty line delivered over two years, slightly less in T1 and a bit more in T3. Compared to other multifaceted programs implemented elsewhere,

[18]In the six experiments included in their analysis, Banerjee et al. (2015) indicate that the cash transfers vary between \$700 and \$2048 PPP.

[19]Households may face lower prices than our implementation partners. It is also possible, given the high mortality rate, that households account for lost property when assessing the value of the transferred animals.

[20]Unfortunately, many of the NGOs which conducted this program left Burkina Faso soon after the program's implementation making the collection of cost data almost impossible.

the T3 intervention, which mimics the program that would have been implemented absent the experiment, is equivalent to $874 PPP, significantly lower than typical multifaceted programs where direct transfer costs range between $1131 and $3091 PPP (Banerjee et al., 2015).

Table 9 also reveals that all treatment groups are significantly different from each other in terms of perceived cost: T3 is significantly more costly than than T1 (+80 USD) and even T2 (+43 USD). The overall cost of the additional nutrition-focused intervention is relatively small compared to the cost of the other bundles, with T3 being only about 20% more costly than T2. Given T3's substantially greater and longer-lasting impacts on metrics such as predicted poverty, anthropometric measurements, and cognitive development, these results suggest that incorporating nutrition-focused interventions into multifaceted programs can significantly enhance human capital accumulation and support long-term outcomes at a relative low cost.

Conclusion

The literature on multifaceted programs has established the potential effectiveness of such programs to reduce ultra-poverty. Yet, whether these programs can have lasting impacts on nutrition and child cognitive development remain understudied. In this article, in addition to contributing to the multifaceted literature, we formally test the hypothesis that such program, targeted to ultra poor households, improves young children anthropometric outcomes and cognitive development.

Our study aims to better understand whether adding animal transfers and nutrition-focused programs to "standard" ultra-poverty programs improves poverty reduction or malnutrition. We first find that, one year after the start of the intervention, ultra-poverty was reduced by 50 to 70%, with this effect gradually diminishing over time but remaining significant nearly two years after all transfers ended in the nutritionally focused treatment (T3). This reduction stemmed from substantial positive impacts on livestock, agricultural equipment, number of parcels cultivated and improve financial situation. Second, only the nutrition-focused group demonstrated significant and lasting impacts on children's anthropometric measures. Beyond the direct effects of enriched flour on anthropometrics, our findings suggest that the nutrition intervention may have prompted households to allocate more resources specifically to vulnerable groups like young children, pregnant women, and breastfeeding women. Lastly, our analysis indicates that the nutrition intervention is the sole contributor to positive impacts on child cognition, particularly pronounced when targeted at pregnant women. Children born to mothers who received these interventions exhibited significant and compelling improvements in cognitive development.

To mitigate the risk of over-claiming significant results when there are multiple outcomes, we first reduced the dimensionality of our tests using summary indices as suggested by Anderson (2008). We then employed the False Discovery Rate (FDR) to control for multiple hypothesis testing when analyzing indices. Moreover, we systematically tracked the number of hypotheses tested and the proportion of significant hypotheses. This approach revealed compelling evidence of significant impacts of the T3 branch across all survey rounds, while T1 and T2 showed significance mainly in the two first follow-up surveys. The T3 group also generated more significant hypotheses than T1 and T2, confirming the possible complementarity between interventions. Finally, using a double LASSO algorithm reinforces our findings, with the proportion of significant hypotheses supported reaching 43% after one year and remaining above 10% in all follow-up surveys and across all treatment branches.

Our results highlight the potential of ultra-poverty programs that integrate nutritional interventions to impact not only malnutrition, but also the cognitive development of children if properly targeted. While these results confirm those available in the epidemiological literature that demonstrated large cognitive benefits from improved nutrition early on and during pregnancy (Cusick and Georgieff, 2016), our study offers causal evidence of the effectiveness of this approach through social protection program implemented at scale and amid escalating conflict in a low-income country. Given the high costs and uncertain effectiveness of formalized early education program (Bouguen et al., 2018; Berkes et al., 2024) in low-income countries, the relative cost effectiveness of nutrition interventions (in our case valued by the household head at $24 per household) provide a strong case for targeted nutritional investments.

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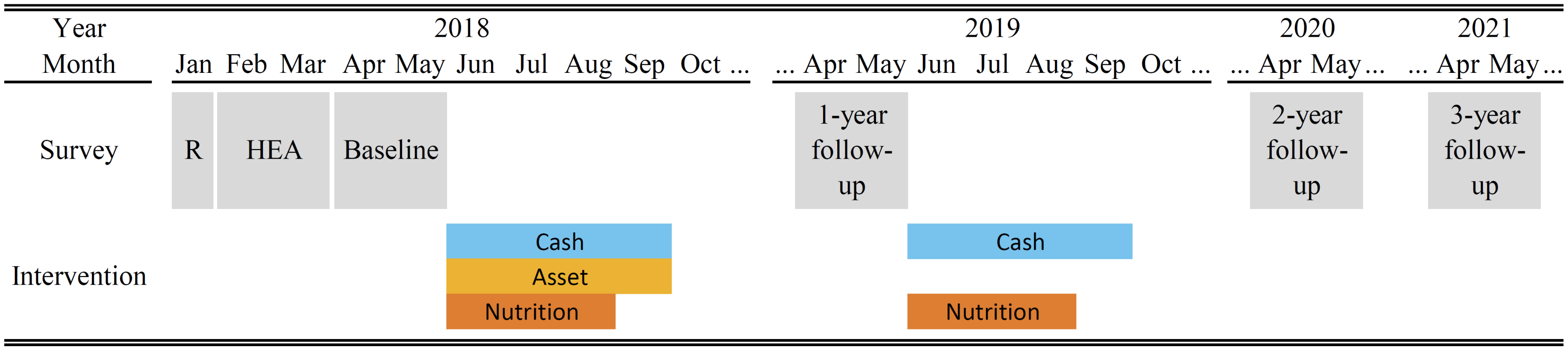
5. Figures

Figure 1: Timeline - Interventions and Surveys



The graph provides the interventions period as well as the timing of each surveys.

6. Tables

Table 1: Participation in Program's interventions over two years

	Obs	C	T1-C	T2-C	T3-C
Main program 's interventions					
Cash,	3,219	0.042	0.893***	0.890***	0.916***
		[0.200]	(0.023)	(0.022)	(0.020)
Animals	3,186	0.001	**0.028**	**0.684*** **	**0.820*** **
		[0.033]	**(0.027)**	**(0.062)**	**(0.041)**
... #	3,274	0.003	**0.093**	**3.726*** **	**4.581*** **
		[0.097]	**(0.212)**	**(0.501)**	**(0.489)**
Flour	3,196	0.003	**0.002**	**-0.001**	**0.640*** **
		[0.057]	**(0.011)**	**(0.012)**	**(0.029)**
Other program's interventions					
Cereals	3,188	0.032	**0.117*** **	**0.235*** **	**0.369*** **
		[0.176]	**(0.034)**	**(0.037)**	**(0.045)**
Inputs	3,188	0.000	**0.020**	**0.239*** **	**0.303*** **
		[0.000]	**(0.027)**	**(0.041)**	**(0.042)**
Training	3,188	0.005	**0.017**	**0.186*** **	**0.210*** **
		[0.074]	**(0.018)**	**(0.038)**	**(0.034)**
Other unrelated interventions					
Other	3,274	0.039	-0.003	-0.009	0.020
		[0.193]	(0.019)	(0.019)	(0.025)

Table 1 presents participation rates for each component of the program. Column C reports the average rate in the control group. The remaining columns display the differences between each experimental group, estimated using strata fixed effects. Below each coefficient, we report robust standard errors clustered at the village level (in parentheses) and standard deviations (in square brackets). Coefficients that are significantly different (at the 10% level) from one of the two other experimental branches are shown in **light bold** ; those significantly different from both branches are shown in **dark bold**. *** 1% **5 % * 10% significance level

Table 2: Wealth Indexes and Poverty Impacts

	1-year results				*2-year results*				*3-year results*			
	C	**T1-C**	**T2-C**	**T3-C**	**C**	**T1-C**	**T2-C**	**T3-C**	**C**	**T1-C**	**T2-C**	**T3-C**
Indices												
Agriculture	0.000	**0.167****	**0.270*****	**0.445*****	0.000	**0.042**	**0.146****	**0.409*****	0.000	0.128*	0.092	0.194***
	[1.000]	**(0.075)**	**(0.072)**	**(0.089)**	[1.000]	**(0.065)**	**(0.061)**	**(0.087)**	[1.000]	(0.071)	(0.069)	(0.072)
		[0.023]	[0.001]	[0.001]		[0.902]	[0.099]	[0.001]		[0.782]	[1.000]	[0.168]
Livestock	0.000	**0.096**	**0.313*****	**0.382*****	0.000	0.003	0.075	0.099	0.000	0.059	0.066	0.049
	[1.000]	**(0.065)**	**(0.071)**	**(0.118)**	[1.000]	(0.073)	(0.079)	(0.093)	[1.000]	(0.093)	(0.081)	(0.106)
		[0.071]	[0.001]	[0.002]		[1.000]	[0.819]	[0.724]		[1.000]	[1.000]	[1.000]
Farming	0.000	0.102	0.298	0.231**	0.000	0.085	0.204	0.142	0.000	0.111	0.033	0.042
	[1.000]	(0.096)	(0.258)	(0.096)	[1.000]	(0.126)	(0.187)	(0.104)	[1.000]	(0.088)	(0.088)	(0.097)
		[0.131]	[0.120]	[0.016]		[0.902]	[0.724]	[0.584]		[1.000]	[1.000]	[1.000]
Saving	0.000	0.028	0.020	0.020	0.000	-0.059	0.030	-0.012	0.000	-0.042	-0.007	-0.038
	[1.000]	(0.038)	(0.043)	(0.037)	[1.000]	(0.044)	(0.069)	(0.044)	[1.000]	(0.035)	(0.034)	(0.039)
		[0.206]	[0.246]	[0.246]		[0.584]	[0.987]	[1.000]		[1.000]	[1.000]	[1.000]
Aggregated indices												
Wealth	-0.001	**0.100****	**0.229*****	**0.272*****	0.001	**0.019**	0.118	**0.167*****	0.000	0.064	0.046	0.062
	[0.684]	**(0.048)**	**(0.071)**	**(0.060)**	[0.698]	**(0.053)**	(0.072)	**(0.054)**	[0.667]	(0.055)	(0.050)	(0.059)
Poverty†	0.104	-0.060***	-0.068***	-0.054***	0.061	-0.007	-0.008	-0.030*	0.081	-0.010	-0.012	-0.023*
	[0.305]	(0.015)	(0.017)	(0.016)	[0.239]	(0.016)	(0.014)	(0.016)	[0.272]	(0.017)	(0.016)	(0.013)

Table 2 provides the impacts on indices and aggregated indices for each components of the program for eligible households. The *Poverty* aggregated index is predicted using the Machine Learning approach described in Section 4.2. Column C gives the average in the control group. The other column gives the respective differences between each experimental groups, estimated using strata fixed effect. Below in parenthesis, we provide the standard error of the coefficient and in square bracket the standard deviation of the mean. The square brackets below the standard errors provide the FDR q-value. Standard errors are robust and clustered at village level. Coefficients that are significantly different (at the 10% level) from one of the two other experimental branches are shown in **light bold** ; those significantly different from both branches are shown in **dark bold**.

** 1% **5 % * 10% significance level.

† indicate outcomes that was not pre-specified in the PRR.

Table 3: Aspiration

	2-year results				3-year results		
	C	**T1-C**	**T2-C**	**T3-C**	**T1-C**	**T2-C**	**T3-C**
Land area (ha)							
own	2.688	0.348*	0.145	0.360*	0.264	0.106	0.204
	[2.514]	(0.203)	(0.173)	(0.212)	(0.162)	(0.150)	(0.170)
aspired	4.882	0.500	0.226	0.587*	0.426	0.239	0.403
	[3.468]	(0.346)	(0.258)	(0.336)	(0.359)	(0.297)	(0.381)
Cattle size (#)							
own	6.646	0.468	1.140	0.305	-0.032	0.325	0.047
	[7.784]	(0.653)	(0.846)	(0.658)	(0.630)	(0.624)	(0.692)
aspired	25.01	-0.456	-1.553	-1.829	4.255	2.089	-0.338
	[25.42]	(2.224)	(2.635)	(2.358)	(3.100)	(2.031)	(1.901)
Education (years)							
own	2.841	0.181	0.354	0.504**	0.015	0.021	0.448
	[3.340]	(0.275)	(0.299)	(0.255)	(0.302)	(0.315)	(0.273)
aspired	10.58	**0.457**	0.788*	**1.373*****	0.290	0.356	0.323
	[3.714]	**(0.376)**	(0.415)	**(0.411)**	(0.360)	(0.386)	(0.338)
Aspiration index	0.002	0.124	0.104	0.222**	0.175*	0.114	0.090
	[1.000]	(0.098)	(0.100)	(0.100)	(0.097)	(0.087)	(0.090)

Table 3 provides measures of aspiration with regards to land size, cattle size and education. For each category we ask the household heads own level and his desired level. We aggregate the answers by standardizing each dimension using the control group and taking their average. Column C gives the average in the control group. The other columns give the respective differences between each experimental groups, estimated using strata fixed effect. Below in parenthesis, we provide the standard error of the coefficient and in square bracket the standard deviation. Standard errors are robust and clustered at village level. Coefficients that are significantly different (at the 10% level) from one of the two other experimental branches are shown in **light bold** ; those significantly different from both branches are shown in **dark bold**
*** 1%, ** 5%, * 10% significance level

Table 4: Food security and dietary diversity

	1-year results				*2-year results*			*3-year results*		
	C	**T1-C**	**T2-C**	**T3-C**	**T1-C**	**T2-C**	**T3-C**	**T1-C**	**T2-C**	**T3-C**
Household food insecurity										
insecure	0.557	-0.014	-0.011	0.013	**0.015**	**-0.075***	-0.009	0.037	0.060	-0.005
	[0.497]	(0.038)	(0.040)	(0.042)	**(0.042)**	**(0.043)**	(0.043)	(0.041)	(0.043)	(0.043)
Severely insecure	0.305	-0.011	-0.026	-0.062*	-0.053*	-0.075**	-0.048	-0.023	-0.001	-0.003
	[0.461]	(0.036)	(0.033)	(0.037)	(0.028)	(0.033)	(0.029)	(0.033)	(0.039)	(0.036)
Food Diversity										
Poor diversity	0.210	**-0.074*****	**-0.067****	**-0.012**	-0.003	0.011	-0.021	-0.042	**-0.093*****	**-0.031**
	[0.408]	**(0.026)**	**(0.030)**	**(0.026)**	(0.031)	(0.039)	(0.033)	(0.030)	**(0.032)**	**(0.028)**
Pregnant/lactating women nutrition										
Index	.	.	.	.	**0.036**	**0.096**	**0.310*****	.	.	.
	.	.	.	.	**(0.123)**	**(0.111)**	**(0.114)**	.	.	.

Table 4 provides Column C gives the average in the control group. The other column gives the respective differences between each experimental groups, estimated using strata fixed effect. Below in parenthesis, we provide the standard error of the coefficient and in square bracket the standard deviation. Standard errors are robust and clustered at village level. Coefficients that are significantly different (at the 10% level) from one of the two other experimental branches are shown in **light bold** ; those significantly different from both branches are shown in **dark bold**

*** 1%, ** 5%, * 10% significance level

Table 5: Anthropometrics measures - children between 0 and 5 years old

	1-year results				*2-year results*			*3-year results*		
	C	*T1-C*	*T2-C*	T3-C	*T1-C*	*T2-C*	T3-C	*T1-C*	*T2-C*	T3-C
Acute malnutrition										
Weight-for-height	-0.800	-0.019	-0.081*	0.044	-0.084	-0.053	0.005	0.077*	0.085*	0.035
	[1.106]	(0.052)	(0.045)	(0.053)	(0.057)	(0.054)	(0.063)	(0.044)	(0.051)	(0.047)
. . . wasting	0.134	0.007	0.006	-0.024	0.025**	0.026**	0.000	-0.016	-0.013	-0.020
	[0.341]	(0.015)	(0.015)	(0.015)	(0.013)	(0.013)	(0.012)	(0.013)	(0.014)	(0.014)
. . . severe wasting	0.026	-0.002	-0.002	-0.012*	0.002	-0.005	-0.005	0.001	0.003	-0.003
	[0.160]	(0.006)	(0.007)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
MUAC	-1.068	-0.039	0.047	0.109*	-0.056	-0.020	0.061	-0.003	0.019	0.043
	[0.909]	(0.054)	(0.054)	(0.059)	(0.045)	(0.044)	(0.054)	(0.058)	(0.059)	(0.056)
. . . wasting, MUAC	0.148	-0.004	-0.014	-0.022	0.026*	0.012	0.012	-0.004	-0.004	-0.003
	[0.355]	(0.016)	(0.018)	(0.016)	(0.014)	(0.014)	(0.016)	(0.024)	(0.027)	(0.023)
Chronic malnutrition										
Height-for-age	-1.454	-0.051	0.067	0.147**	-0.034	0.076	0.182**	-0.069	-0.011	0.118*
	[1.433]	(0.063)	(0.074)	(0.067)	(0.068)	(0.070)	(0.070)	(0.070)	(0.062)	(0.063)
. . . stunting	0.338	0.002	-0.024	-0.044*	0.020	-0.018	-0.054**	0.038	0.023	-0.021
	[0.473]	(0.021)	(0.024)	(0.023)	(0.022)	(0.021)	(0.022)	(0.025)	(0.023)	(0.022)
. . . severe stunting	0.128	-0.007	-0.011	-0.033**	0.003	0.001	-0.036**	0.012	0.005	-0.011
	[0.334]	(0.015)	(0.016)	(0.016)	(0.018)	(0.016)	(0.016)	(0.012)	(0.012)	(0.011)
Underweight										
Weight-for-age	-1.376	-0.067	-0.036	0.113**	-0.100*	-0.008	0.139**	-0.033	0.022	-0.036
	[1.158]	(0.054)	(0.053)	(0.056)	(0.057)	(0.052)	(0.060)	(0.109)	(0.127)	(0.099)
. . . underweight	0.281	0.021	0.005	-0.030	0.031	-0.009	-0.041**	0.026	-0.002	-0.030
	[0.450]	(0.021)	(0.022)	(0.020)	(0.019)	(0.020)	(0.019)	(0.023)	(0.026)	(0.021)
. . . severe underweight	0.083	0.002	-0.019	-0.026**	0.023**	0.010	-0.013	0.010	-0.002	-0.013
	[0.276]	(0.012)	(0.013)	(0.012)	(0.011)	(0.011)	(0.010)	(0.012)	(0.011)	(0.009)
Observations	1,576	1,457	1,207	1,313	1,719	1,377	1,510	1,700	1,331	1,698

Table 5 reports several anthropometric measures for eligible children aged 0âĂŞ5 years. Column C presents the average for the control group. The remaining columns display the differences between each experimental group, estimated using strata fixed effects. Standard errors (in parentheses) are robust and clustered at the village level; standard deviations of the control group are shown in square brackets. Coefficients that are significantly different (at the 10% level) from one of the two other experimental branches are shown in **light bold**; those significantly different from both branches are shown in **dark bold**. *** 1%, ** 5%, * 10% significance level.

Table 6: CREDI Test results - 0-36 months Children

	3-year follow-up			*3-year follow-up & <9 months*		
	T1-C	***T2-C***	***T3-C***	***T1-C***	***T2-C***	***T3-C***
Cognition	0.006	**-0.062**	**0.129***	-0.098	0.044	-0.001
	(0.072)	**(0.076)**	**(0.071)**	(0.097)	(0.103)	(0.121)
Langage	0.014	**-0.068**	**0.150****	-0.029	-0.020	-0.014
	(0.075)	**(0.077)**	**(0.071)**	(0.084)	(0.076)	(0.094)
Motor	0.019	**-0.043**	**0.135***	**-0.081**	**0.066**	0.026
	(0.069)	**(0.076)**	**(0.070)**	**(0.089)**	**(0.092)**	(0.115)
Socio-emotional	**-0.002**	**-0.047**	**0.133***	**-0.091**	**0.059**	0.008
	(0.068)	**(0.074)**	**(0.070)**	**(0.087)**	**(0.092)**	(0.108)
Mental health	0.040	0.165	0.025	0.146	0.043	0.237**
	(0.110)	(0.111)	(0.087)	(0.110)	(0.142)	(0.099)
Score global	0.024	**-0.037**	**0.145****	-0.063	0.032	-0.016
	(0.068)	**(0.075)**	**(0.070)**	(0.100)	(0.101)	(0.117)
Observations	509	411	463	133	129	116

Table 6 provides impacts measures of the CREDI. Column C gives the average in the control group. The other columns give the respective differences between each experimental groups, estimated using strata fixed effect. Below in parenthesis, we provide the standard error of the coefficient and in square bracket the standard deviation. Standard errors are robust and clustered at village level. Coefficients that are significantly different (at the 10% level) from one of the two other experimental branches are shown in **light bold** ; those significantly different from both branches are shown in **dark bold**

*** 1%, ** 5%, * 10% significance level

Table 7: MELQO Test results - 36-59 months children

	2-year results			*3-year results*		
	T1-C	***T2-C***	**T3-C**	***T1-C***	***T2-C***	**T3-C**
Math score	**-0.066**	0.037	**0.109**	**0.033**	**-0.091**	-0.003
	(0.077)	(0.093)	**(0.088)**	**(0.060)**	**(0.065)**	(0.051)
Cognitive score	-0.045	0.062	0.010	**0.082**	**-0.046**	0.053
	(0.061)	(0.077)	(0.078)	**(0.066)**	**(0.073)**	(0.063)
Language score	-0.104	-0.063	-0.088	**0.049**	**-0.060**	0.009
	(0.087)	(0.100)	(0.088)	**(0.061)**	**(0.064)**	(0.055)
Motor Score	**0.018**	**-0.118**	**0.191****	**0.020**	**-0.178***	**0.041**
	(0.104)	**(0.114)**	**(0.096)**	**(0.104)**	**(0.099)**	**(0.098)**
Overall score	-0.042	0.043	0.001	**0.055**	**-0.065**	0.014
	(0.062)	(0.076)	(0.073)	**(0.054)**	**(0.059)**	(0.047)
Observations	558	488	510	599	456	587

Table 7 provides measures of cognitive development(MELQO) in the 2-year follow-up survey. Column C gives the average in the control group. The other columns give the respective differences between each experimental groups, estimated using strata fixed effect. Below in parenthesis, we provide the standard error of the coefficient and in square bracket the standard deviation. Standard errors are robust and clustered at village level. Coefficients that are significantly different (at the 10% level) from one of the two other experimental branches are shown in **light bold** ; those significantly different from both branches are shown in **dark bold**. *** 1%, ** 5%, * 10% significance level

Table 8: Hypothesis

	Main specification						
	T1-C	**T2-C**	**T3-C**	**T-C**	**T2-T1**	**T3-T1**	**T3-T2**
1-year effect							
# of hypothesis tested	20	20	20	60	20	20	20
# of hypothesis with p-value<10%	4	6	13	23	2	9	5
% sigificant hypothesis	20.0%	30.0%	65.0%	38.3%	10.0%	45.0%	25.0%
2-year effect							
# of hypothesis tested	33	33	33	99	33	33	33
# of hypothesis with p-value<10%	6	5	15	26	3	14	9
% significant at 10%	18.2%	15.2%	45.5%	26.3%	9.1%	42.4%	27.3%
3-year effect							
# of hypothesis tested	38	38	38	114	38	38	38
# of hypothesis with p-value<10%	3	3	6	12	5	12	1
% significant at 10%	7.9%	7.9%	15.8%	10.5%	13.2%	31.6%	2.6%
	Double LASSO						
	T1-C	**T2-C**	**T3-C**	**T-C**	**T2-T1**	**T3-T1**	**T3-T2**
1-year effect							
# of hypothesis tested	20	20	20	60	20	20	20
# of hypothesis with p-value<10%	4	9	13	26	7	13	11
% sigificant hypothesis	20.0%	45.0%	65.0%	43.3%	35.0%	65.0%	55.0%
2-year effect							
# of hypothesis tested	33	33	33	99	33	33	33
# of hypothesis with p-value<10%	9	9	21	39	13	19	18
% significant at 10%	27.3%	27.3%	63.6%	39.4%	39.4%	57.6%	54.5%
3-year effect							
# of hypothesis tested	38	38	38	114	38	38	38
# of hypothesis with p-value<10%	6	9	10	25	9	14	18
% significant at 10%	15.8%	23.7%	26.3%	21.9%	23.7%	36.8%	47.4%

Table 8 tracks by survey rounds and treatment branches the number of hypothesis tested, the number of null hypothesis rejected at 10% and the share of significant at 1-% hypothesis. We exclude from this analysis the hypothesis that relates to compliance (i.e. the one evoked in Section 4.1). We highlight with a graded color scale the share of hypothesis above 10% (in green) and the share of hypothesis below 10% in red.

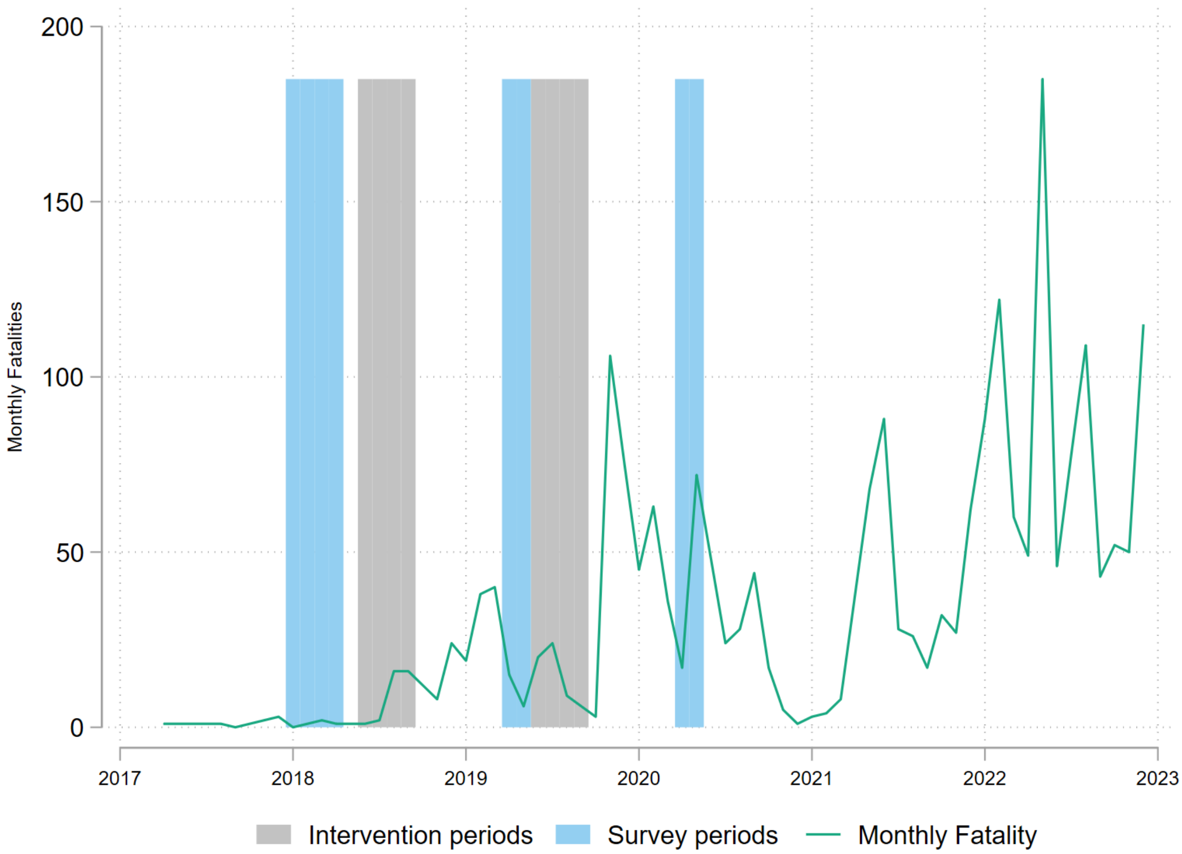
Table 9: Values of the intervention in USD equivalent

	Obs	C	T1-C	T2-C	T3-C
Cash transfer	2,932	13.13	192.0***	205.4***	205.7***
		[86.64]	(11.77)	(10.37)	(10.29)
Animals transfer	2,932	0.157	**3.092**	**64.97*** **	**82.38*** **
		[4.638]	**(4.937)**	**(8.918)**	**(10.48)**
Enriched flower	2,932	0.026	-0.173	-0.419	**15.42*** **
		[0.596]	(0.425)	(0.462)	**(1.607)**
Cereals transfer	2,932	0.506	**0.454**	**1.784*** **	**3.025*** **
		[4.510]	**(0.478)**	**(0.551)**	**(0.470)**
Inputs transfer	2,932	0.000	**0.263**	**3.248*** **	**11.35*** **
		[0.000]	**(1.044)**	**(1.082)**	**(2.826)**
Total value	2,932	13.82	**195.7*** **	**275.0*** **	**317.9*** **
		[86.79]	**(12.71)**	**(13.77)**	**(14.67)**

Table 9 provides the compliance rates for each components of the program for eligible households. Column C gives the average in the control group. The other column gives the respective differences between each experimental groups, estimated using strata fixed effect. Below in parenthesis, we provide the standard error of the coefficient and in square bracket the standard deviation. Standard errors are robust and clustered at village level.
*** 1%, ** 5%, * 10% significance level

Online Appendix A: Supplementary Figures

Figure OA1: Violent Events Fatalities - Experimental Communes



The graph shows the number of monthly fatalities due to terrorist attacks from 2017 (one year before the beginning of the interventions) to 2022 (one year after) in the 15 communes where the experiments was conducted.
source: ACLED

Figure OA2: Cash Utilization

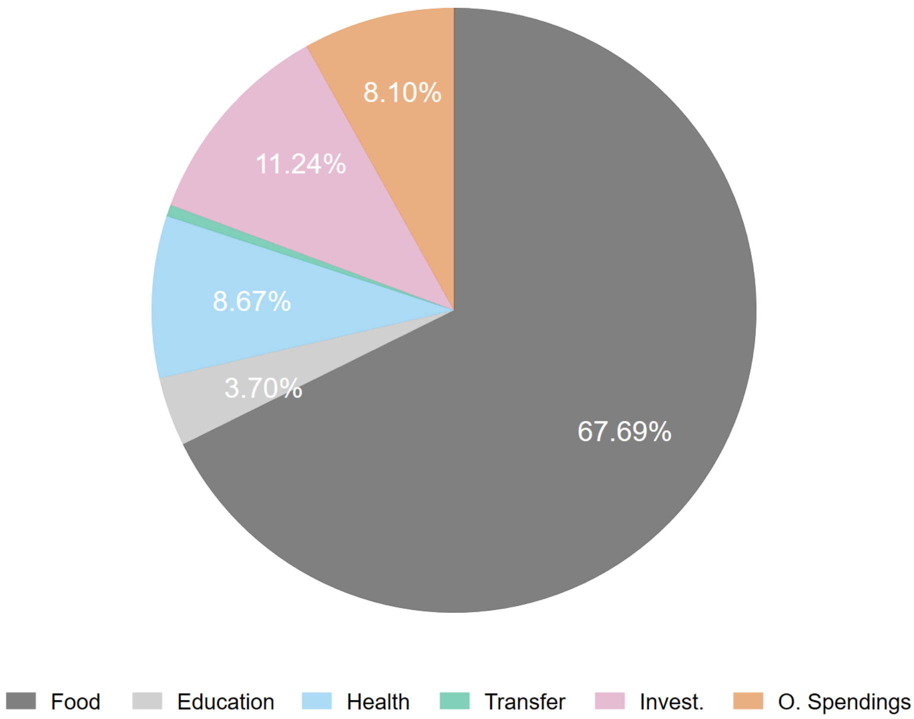
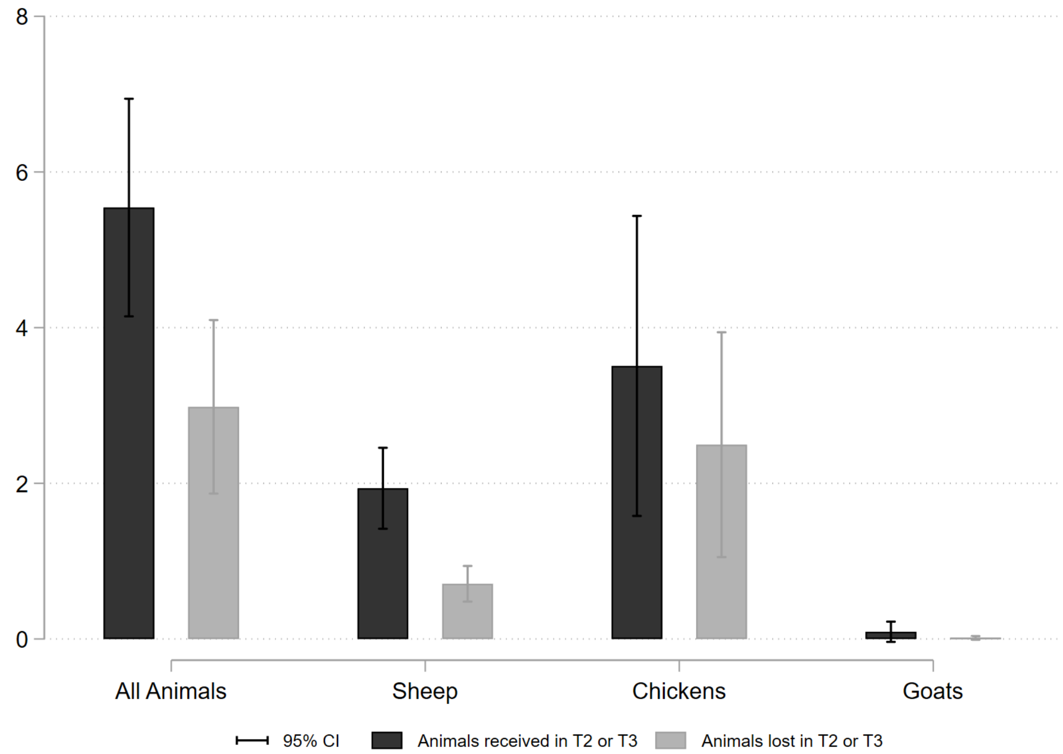


Figure OA3: Animal Transfers and Mortality - T2 & T3 branches jointly, conditional on benefiting from an animal transfer



Number of animals transferred as reported by respondent conditional on being in an asset branch (T2 or T3) and receiving at least one animal. Since households only benefited from one type of animal transfer, the average number for sheep, chickens and goats includes many zeros while the averages for all animals only includes positive numbers.

Figure OA4: Treatment Exposure and Start Date - CREDI cohort - 3-year follow-up

	Month conceived	Birthday	age (months) April 2021	Treatment dosage in-utero	Treatment dosage direct	Treatment start age	Treatment intensity
CREDI	Jul-20	Apr-21	0	0	0	NA	Not directly impacted
	Jun-20	Mar-21	1	0	0	NA	
	May-20	Feb-21	2	0	0	NA	
	Apr-20	Jan-21	3	0	0	NA	
	Mar-20	Dec-20	4	0	0	NA	
	Feb-20	Nov-20	5	0	0	NA	
	Jan-20	Oct-20	6	0	0	NA	
	Dec-19	Sep-20	7	0	0	NA	
	Nov-19	Aug-20	8	0	0	NA	
	Oct-19	Jul-20	9	0	0	NA	
	Sep-19	Jun-20	10	1	0	NA	only in-utero impacted
	Aug-19	May-20	11	2	0	NA	
	Jul-19	Apr-20	12	3	0	NA	
	Jun-19	Mar-20	13	4	0	NA	
	May-19	Feb-20	14	5	0	NA	
	Apr-19	Jan-20	15	6	0	NA	
	Mar-19	Dec-19	16	7	0	NA	
	Feb-19	Nov-19	17	8	0	NA	
	Jan-19	Oct-19	18	9	0	NA	
	Dec-18	Sep-19	19	9	1	1	In-utero \& directly impacted
	Nov-18	Aug-19	20	9	2	1	
	Oct-18	Jul-19	21	9	3	1	
	Sep-18	Jun-19	22	9	4	1	
	Aug-18	May-19	23	9	5	1	
	Jul-18	Apr-19	24	9	6	1	
	Jun-18	Mar-19	25	9	7	1	
	May-18	Feb-19	26	8	8	1	
	Apr-18	Jan-19	27	7	9	1	
	Mar-18	Dec-18	28	6	10	1	
	Feb-18	Nov-18	29	5	11	1	
	Jan-18	Oct-18	30	4	12	1	
	Dec-17	Sep-18	31	3	13	1	
	Nov-17	Aug-18	32	2	14	1	
	Oct-17	Jul-18	33	1	15	1	
	Sep-17	Jun-18	34	0	16	1	directly impacted, not in-utero
	Aug-17	May-18	35	0	16	2	
	Jul-17	Apr-18	36	0	16	3	

The graph shows the treatment intensity and treatment starting age for 0-36 children whose caregiver took the 3-year follow-up CREDI test. The treatment intensity and start age is given by age. Column *Month conceived* gives the approximate month the child was conceived and column *Birthday* the approximate birthday of the child. Columns *Treatment dosage* give the number of months the child benefited from the treatment both *in utero* and post-natal (direct). We consider as the *treatment* the period between June 2018 (dates of the first transfers) until September 2019 (date of the last transfers), see Figure 1 for more details about the timeline of the interventions.

Figure OA5: Treatment Exposure and Start Date - MELQO cohort - 2-year follow-up

Month conceived	Birthday	age (months) April 2020	Treatment dosage in-utero	Treatment dosage direct	Treatment start age	Treatment intensity
Aug-16	May-17	37	0	16	13	directly impacted not in-utero
Jul-16	Apr-17	38	0	16	14	
Jun-16	Mar-17	39	0	16	15	
May-16	Feb-17	40	0	16	16	
Apr-16	Jan-17	41	0	16	17	
Mar-16	Dec-16	42	0	16	18	
Feb-16	Nov-16	43	0	16	19	
Jan-16	Oct-16	44	0	16	20	
Dec-15	Sep-16	45	0	16	21	
Nov-15	Aug-16	46	0	16	22	
Oct-15	Jul-16	47	0	16	23	
Sep-15	Jun-16	48	0	16	24	
Aug-15	May-16	49	0	16	25	
Jul-15	Apr-16	50	0	16	26	
Jun-15	Mar-16	51	0	16	27	
May-15	Feb-16	52	0	16	28	
Apr-15	Jan-16	53	0	16	29	
Mar-15	Dec-15	54	0	16	30	
Feb-15	Nov-15	55	0	16	31	
Jan-15	Oct-15	56	0	16	32	
Dec-14	Sep-15	57	0	16	33	
Nov-14	Aug-15	58	0	16	34	
Oct-14	Jul-15	59	0	16	35	

Same as Figure OA4 but for MELQO in the 2-year follow-up survey i.e. for children aged between 36 and 60 months.

Figure OA6: Treatment Exposure and Start Date - 3-year MELQO test cohort

Month conceived	**Birthday**	**age (months) April 2021**	**Treatment dosage**		**Treatment start age**	**Treatment intensity**
			in-utero	direct		
Jun-17	Mar-18	37	0	16	4	directly impacted not in-utero
May-17	Feb-18	38	0	16	5	
Apr-17	Jan-18	39	0	16	6	
Mar-17	Dec-17	40	0	16	7	
Feb-17	Nov-17	41	0	16	8	
Jan-17	Oct-17	42	0	16	9	
Dec-16	Sep-17	43	0	16	10	
Nov-16	Aug-17	44	0	16	11	
Oct-16	Jul-17	45	0	16	12	
Sep-16	Jun-17	46	0	16	13	
Aug-16	May-17	47	0	16	14	
Jul-16	Apr-17	48	0	16	15	
Jun-16	Mar-17	49	0	16	16	
May-16	Feb-17	50	0	16	17	
Apr-16	Jan-17	51	0	16	18	
Mar-16	Dec-16	52	0	16	19	
Feb-16	Nov-16	53	0	16	20	
Jan-16	Oct-16	54	0	16	21	
Dec-15	Sep-16	55	0	16	22	
Nov-15	Aug-16	56	0	16	23	
Oct-15	Jul-16	57	0	16	24	
Sep-15	Jun-16	58	0	16	25	
Aug-15	May-16	59	0	16	26	

Same as Figure OA4 but for the 3-year MELQO test i.e. for children aged between 36 and 60 months.

Figure OA7: Top 10 Variables used in Random Forest

Appendix OB: Supplementary Tables

Table OB1: Sample sizes

	Total	C	T1	T2	T3
Household dataset					
Baseline	3,465	1,005	858	806	796
Midline	3,170	908	801	697	764
Endline	3,020	913	737	658	712
Follow-up	2,836	870	696	580	690
Household member dataset					
Baseline	28,699	8,274	7,257	6,628	6,540
Midline	27,073	7,641	6,972	5,836	6,624
Endline	26,288	7,949	6,568	5,626	6,145
Follow-up	30,478	9,374	7,707	6,092	7,305
Anthropometrics dataset, 0-59 months					
Baseline	6,082	1,760	1,567	1,343	1,412
Midline	5,556	1,576	1,457	1,207	1,316
Endline	6,281	1,856	1,629	1,346	1,450
Follow-up	6,686	1,952	1,700	1,336	1,698
CREDI test score, 0-35 months					
Baseline	4,069	1,177	1,017	944	931
Midline	0	0	0	0	0
Endline	0	0	0	0	0
Follow-up	1,979	596	509	411	463
MELQO test score, 36-59 months					
Baseline	0	0	0	0	0
Midline	0	0	0	0	0
Endline	2,200	644	558	488	510
Follow-up	2,352	710	599	456	587

Table OB1 provides the sample sizes by experimental group, for each dataset and for each survey round.

Table OB2: Index Balancing

	Obs	C	T1-C	T2-C	T3-C
Agriculture	3,465	0.000	0.152***	0.050	0.070
		[1.000]	(0.055)	(0.059)	(0.056)
			[0.151]	[0.647]	[0.510]
Animals	3,465	0.000	0.108	0.134	0.070
		[1.000]	(0.072)	(0.084)	(0.090)
			[0.455]	[0.439]	[0.668]
Farming	3,465	-0.011	0.041	0.158*	0.047
		[0.592]	(0.042)	(0.083)	(0.048)
			[0.599]	[0.380]	[0.599]
Saving	3,465	0.000	-0.022	0.078	-0.018
		[1.000]	(0.041)	(0.083)	(0.041)
			[0.795]	[0.599]	[0.795]
Anthropometrics	3,050	-1.124	-0.003	-0.003	0.052
		[0.880]	(0.055)	(0.052)	(0.052)
			[0.967]	[0.967]	[0.599]
Food security	3,465	0.000	-0.093*	-0.053	-0.064
		[0.776]	(0.050)	(0.052)	(0.051)
			[0.380]	[0.599]	[0.510]
Aggreagated	3,465	-0.003	0.072*	0.100*	0.056
		[0.570]	(0.037)	(0.055)	(0.041)
			[0.380]	[0.380]	[0.510]
Observations	3,465	1,005	858	806	796
Clusters	168	43	42	41	42

Table OB2 provides the initial differences between the experimental groups using the wealth indexes. Column *C* gives the average in the control group while the other columns give the difference between the treatment groups and the control group. We control for commune fixed effect and standard errors are robust and clustered at the village level. Below the balancing coefficients, we provide in square parenthesis the FDR q-value that accounts for all the hypothesis tested at baseline for all treatment branches i.e. 21 hypothesis.

*** 1%, ** 5%, * 10% significance level

Table OB3: Predicted Poverty based on HEA classification

	C	***T1-C***	***T2-C***	**T3-C**	***T2-T1***	***T3-T1***	**T3-T2**
Midline	0.104	-0.060***	-0.068***	-0.054***	-0.008	0.006	0.014
	[0.305]	(0.015)	(0.017)	(0.016)	(0.013)	(0.013)	(0.015)
Endline	0.061	-0.007	-0.008	-0.030*	-0.002	-0.023	-0.021
	[0.239]	(0.016)	(0.014)	(0.016)	(0.014)	(0.016)	(0.014)
Follow-up	0.081	-0.010	-0.012	-0.023*	-0.002	-0.013	-0.011
	[0.272]	(0.017)	(0.016)	(0.013)	(0.019)	(0.016)	(0.015)

Table OB3 provides a measure of poverty reduction based on the baseline prediction of the HEA categorization. Column C gives the average in the control group at Midline, the average at Endline and Follow-up is not provided but is constant overtime. The other columns give the respective differences between each experimental groups, estimated using commune fixed effect. Below in parenthesis, we provide the standard errors and in square bracket the standard deviation of the control group. Standard errors are robust and clustered at village level. Light bold coefficients indicate that the treatment coefficient is different from one other treatment coefficient, bold coefficients indicate that the treatment coefficient is different from both other treatment groups.

*** 1%, ** 5%, * 10% significance level

Table OB4: Literature Review Cash, Nutrition and Multi-faceted comparable Experimental Studies

Panel A: Conditional and Unconditional Cash litterature

Study	Country	HAZ	WHZ	age	intervention
Premand and Barry (2022)	Niger	0	0	6-59 m	UCT
McIntosh and Zeitlin (2024)	Rwanda	+	NA	0-5 yr	UCT
Baird et al. (2019)	Malawi	0	NA	13-22 yr	UCT
Houngbe et al. (2017)	Burkina Faso	0	0	24-39 m	UCT
Akresh et al. (2016)	Burkina Faso	0	0	0-5 yr	UCT
Paxson and Schady (2010)	Ecuador	0	NA	0-6 yr	UCT
Baird et al. (2019)	Malawi	0	NA	13-22 yr	CCT
Evans et al. (2016)	Philippines	+	NA	6-36 m	CCT
Kandpal et al. (2016)	Tanzania	0	0	0-4 yr	CCT
Akresh et al. (2016)	Burkina Faso	+	+	0-5 yr	CCT
Galiani and McEwan (2013)	Honduras	+	NA	0-6 yr	CCT
Macours et al. (2012)	Nicaragua	+	NA	0-5 yr	CCT
Maluccio and Flores (2005)	Nicaragua	+	0	0-5 yr	CCT

Panel B: Multifaced program litterature

Study	Country	Food security	Asset	Health	Last survey
Angelucci et al. (2023)	Congo	**NA**	**+**	0	1 year
Soofi et al. (2022)	Pakistan	NA	NA	+	1.5 years
Banerjee et al. (2022)**	Ghana	+	+	NA	2 years
Banerjee et al. (2021b)*	India	+	+	+	10 years
Bandiera et al. (2017)	Bangladesh	NA	+	NA	7 years
Balboni et al. (2022)	Bangladesh	NA	+	NA	11 years
Banerjee et al. (2015)	Multiple	+	+	0	2 years

Continued

Panel C: Nutrition Program

Study	Country	WHZ	HAZ	Dev. Skills	Program Type
Wegmüller et al. (2022)	Kenya	NA	+	NA	NA
Olney et al. (2019)	Burundi	+	+	+	MMN+Training
Barffour et al. (2019)	Lao PDR	0	0	NA	MMN
Maleta et al. (2015)	Malawi	0	0	NA	LNS
Hess et al. (2015)	Burkina Faso	+	+	NA	LNS
Chang et al. (2013)	China	NA	NA	+	MMN
Aboud and Akhter (2011)	Bangladesh	NA	NA	+	MMN
Sazawal et al. (2010)	India	+	+	NA	MMN
Makrides et al. (2010)	Vietnam	NA	NA	0	Fish Oil
Li et al. (2009)	China	NA	NA	+	MMN
Manger et al. (2008)	Thailand	0	0	0	MMN
Adu-Afarwuah et al. (2008)	Ghana	+	+	NA	MMN
Tofail et al. (2008)	Bangladesh	NA	NA	0	MMN
McGrath et al. (2006)	Tanzania	NA	NA	+	Multivitamin

Table OB4 provides the main impacts of the most prominent experiments on the cash, nutrition and multifaceted literature, conducted in the last 20 years. Column *HAZ* gives the Height for age z-score impacts,*WHZ*, the Weight for Height z-score impacts, age gives the age of the child, *Skills*, the effect on developmental skills. For the multifaceted literature, we provide the results on food security, asset and health index. Column intervention gives the type of program implemented: UCT stands for unconditional cash transfer, CCT conditional cash transfer, MMN for Multiple-micro-nutrient supplementation, LNS for liquid-based nutrient supplementation

* long-term follow-up of a previously listed experiment. ** long-term follow-up of a previously listed experiment. Already included in the multi-site article. + indicates positive and significant effect, *NA* not reported/collected, *0* no significant effect.

Table OB4: Attrition

	Obs	C	T1-C	T2-C	T3-C	T-C
Midline						
overall attrition	3,483	0.096	-0.030	0.038	-0.054	-0.015
		[0.295]	(0.045)	(0.064)	(0.041)	(0.042)
... Villages	3,483	0.000	0.000	0.037	0.000	0.012
		[0.000]	(0.000)	(0.036)	(0.000)	(0.012)
... Households	3,483	0.096	-0.030	0.001	-0.054	-0.027
		[0.295]	(0.045)	(0.055)	(0.041)	(0.040)
Endline						
overall attrition	7,109	0.098	0.063	0.084	0.032	0.060
		[0.297]	(0.055)	(0.063)	(0.051)	(0.040)
... Villages	7,109	0.030	0.056	0.094	0.026	0.059
		[0.171]	(0.049)	(0.063)	(0.047)	(0.037)
... Households	7,109	0.068	0.007	-0.010	0.006	0.001
		[0.251]	(0.032)	(0.022)	(0.026)	(0.020)
Follow-up						
overall attrition	7,109	0.224	0.030	0.102	-0.003	0.042
		[0.417]	(0.065)	(0.078)	(0.066)	(0.053)
... Villages	7,109	0.046	0.052	0.159**	0.025	0.078*
		[0.209]	(0.052)	(0.075)	(0.051)	(0.043)
... Households	7,109	0.178	-0.022	-0.057	-0.028	-0.036
		[0.383]	(0.050)	(0.048)	(0.051)	(0.040)
Observations	7,109	1,923	1,777	1,695	1,714	5,186
Clusters	169	44	43	41	42	126

Table OB4 gives attrition rate for the control group (column C) and the differential attrition between the treatment branches and the control group. Column T-C compares all treatment groups with the control group. Regression results are controlled for commune fixed effect. Standard errors are clustered at the village level and robust to heteroskedasticity. *** 1%, ** 5%, * 10% significance level

Table OB5: Livestock

		1-year results			2-year results			3-year results		
	C	***T1-C***	***T2-C***	**T3-C**	***T1-C***	***T2-C***	**T3-C**	***T1-C***	***T2-C***	**T3-C**
Livestock										
# of animals	10.75	1.041	3.394***	4.140***	0.039	0.921	1.213	0.722	0.804	0.599
	[10.83]	(0.706)	(0.766)	(1.275)	(0.893)	(0.975)	(1.144)	(1.136)	(0.988)	(1.295)
Average price	31.51	1.109	-3.769*	-1.155	1.431	-1.774	-1.111	0.092	-3.919	-1.514
	[34.36]	(2.251)	(2.043)	(2.094)	(2.268)	(2.041)	(2.049)	(2.534)	(2.613)	(2.793)
Total Value	244.7	11.34	2.880	46.24*	1.083	-14.91	4.034	10.85	13.20	-21.92
	[357.5]	(23.28)	(21.43)	(27.43)	(20.97)	(24.59)	(25.68)	(25.86)	(32.49)	(29.84)
Animals sold										
# sold	2.865	0.311	0.214	0.385	0.106	0.089	0.030	-0.005	-0.080	-0.046
	[5.791]	(0.361)	(0.352)	(0.421)	(0.315)	(0.336)	(0.334)	(0.636)	(0.529)	(0.647)
Average price	28.88	-2.527	-10.81***	-6.547*	1.827	-5.546**	0.095	.	.	.
	[50.55]	(3.232)	(2.966)	(3.411)	(3.131)	(2.581)	(2.975)	.	.	.
Value	42.17	-0.165	-10.22*	-4.607	-0.007	-10.40**	-3.320	.	.	.
	[104.4]	(5.555)	(5.334)	(6.558)	(4.882)	(4.491)	(4.741)	.	.	.

Table OB5 provides the impacts on outcomes related to livestock ownership and price. Column C provides the control average for 1-year results while the other columns give the respective differences between each experimental groups, estimated using commune fixed effect.Below in parenthesis, we provide the standard errors and in square bracket the standard deviation of the control group. Standard errors are robust and clustered at village level.

*** 1%, ** 5%, * 10% significance level

Table OB6: Farm size and revenue

	1-year results			*2-year results*			*3-year results*		
	T1-C	***T2-C***	**T3-C**	***T1-C***	***T2-C***	**T3-C**	***T1-C***	***T2-C***	***T3-C***
# of plots									
... cultivated	-0.044	0.180	0.177	-0.083	0.052	0.229*	0.171	0.127	0.087
	(0.110)	(0.116)	(0.138)	(0.124)	(0.136)	(0.137)	(0.116)	(0.109)	(0.105)
... owned	-0.071	0.054	0.166	-0.169	-0.054	0.267*	0.110	0.128	0.087
	(0.117)	(0.101)	(0.149)	(0.121)	(0.119)	(0.138)	(0.124)	(0.116)	(0.113)
... fertilized	0.126	0.065	0.164*	0.033	0.072	0.115	0.032	-0.012	0.088
	(0.080)	(0.113)	(0.094)	(0.099)	(0.103)	(0.093)	(0.080)	(0.080)	(0.097)
... irrigated	0.011	0.078	0.007	0.024	0.055	-0.003	0.010	0.003	-0.002
	(0.015)	(0.075)	(0.016)	(0.018)	(0.041)	(0.008)	(0.006)	(0.006)	(0.006)
# of cultivated crops	0.248	0.182	0.345**	0.078	-0.023	0.268	0.148	0.057	-0.013
	(0.166)	(0.176)	(0.171)	(0.159)	(0.183)	(0.181)	(0.147)	(0.148)	(0.162)
Plot size (in ha)	0.278	0.035	0.225	0.317	0.096	0.384*	0.377**	0.216	0.202
	(0.215)	(0.170)	(0.217)	(0.200)	(0.179)	(0.213)	(0.166)	(0.155)	(0.180)
Agricultural revenue	-3.131	-17.14	31.94*	-5.046	-23.09	6.312	-5.830	-10.98	-1.802
	(14.07)	(12.95)	(17.99)	(15.30)	(14.05)	(17.01)	(12.59)	(10.49)	(11.61)
Observation	801	697	764	737	658	712	696	580	690

Same as Table OB6 but with a specification including baseline outcome variables.
*** 1%, ** 5%, * 10% significance level

Table OB7: Loans and Saving

	1-year results				*2-year results*			*3-year results*		
	C	***T1-C***	***T2-C***	**T3-C**	***T1-C***	***T2-C***	**T3-C**	***T1-C***	***T2-C***	**T3-C**
Tontine										
Tontine, (yes=1)	0.068	-0.020	-0.013	-0.009	-0.039*	0.005	0.010	0.008	0.023*	0.021
	[0.252]	(0.016)	(0.018)	(0.015)	(0.022)	(0.024)	(0.024)	(0.015)	(0.013)	(0.016)
Tontine, amount	0.167	0.041	0.191	0.026	0.024	0.161	0.016	0.978	-0.085	0.351
	[1.027]	(0.078)	(0.147)	(0.080)	(0.162)	(0.154)	(0.156)	(0.689)	(0.367)	(0.270)
Savings										
saving, (yes=1)	0.281	0.032	0.051	0.070*	0.025	0.052	0.107***	0.046	0.047	0.056*
	[0.450]	(0.039)	(0.038)	(0.039)	(0.037)	(0.037)	(0.039)	(0.034)	(0.030)	(0.031)
saving, amount	5.328	2.122*	2.739**	1.407	0.123	3.102	3.003	1.547	2.417*	1.335
	[129.6]	(4.577)	(5.389)	(4.671)	(1.800)	(2.122)	(1.868)	(1.118)	(1.322)	(1.337)
Loans										
has loan, (yes=1)	0.445	-0.096**	-0.041	-0.023	-0.006	-0.009	0.045	-0.005	0.011	-0.013
	[0.497]	(0.042)	(0.042)	(0.035)	(0.034)	(0.037)	(0.036)	(0.034)	(0.036)	(0.038)
loan, amount	27.69	-9.076***	-8.576**	-6.776*	-0.003	-0.009	0.049	-0.002	0.007	-0.021
	[77.48]	(3.267)	(3.474)	(3.616)	(0.035)	(0.039)	(0.039)	(0.038)	(0.041)	(0.042)
loan, remaining	15.92	-6.350**	-4.100	-4.443*	-1.784	-5.921	-0.385	2.620	-2.210	-0.440
	[65.70]	(2.555)	(3.080)	(2.481)	(4.944)	(3.875)	(4.071)	(4.353)	(3.419)	(4.247)
Observations	908	801	697	764	737	658	712	696	580	690

Table OB7 provides the impacts on outcomes related to household finance. Column C provides the control average for 1-year results while the other columns give the respective differences between each experimental groups, estimated using commune fixed effect. Below in parenthesis, we provide the standard errors and in square bracket the standard deviation of the control group. Standard errors are robust and clustered at village level.

*** 1%, ** 5%, * 10% significance level

Table OB8: Small Business

	T1-C	***T2-C***	**T3-C**	***T1-C***	***T2-C***	**T3-C**	***T1-C***	***T2-C***	**T3-C**
# of active business	0.092	0.163***	0.043	-0.053	-0.131*	-0.130**	0.019	0.113***	0.038
	(0.060)	(0.052)	(0.068)	(0.072)	(0.073)	(0.064)	(0.033)	(0.039)	(0.043)
# of days worked	2.997*	2.525**	2.120**	53.40	6.837	5.622	0.067	0.975*	0.702*
	(1.568)	(1.264)	(0.843)	(49.93)	(9.613)	(9.098)	(0.367)	(0.511)	(0.387)
# of active months	0.550	0.103	0.059	-0.188	-1.032	-0.274	0.286	0.741**	0.437
	(0.543)	(0.407)	(0.553)	(0.960)	(0.841)	(0.791)	(0.312)	(0.348)	(0.359)
Total income value	3.151	-5.017	-5.136	-19.15	-28.53	-23.41	-0.170	3.542	0.644
	(4.540)	(3.464)	(3.111)	(20.89)	(18.76)	(18.31)	(1.776)	(2.719)	(1.501)
Total profit	-0.003	-0.416	-1.180	-1.561	-3.041	-1.214	-0.035	0.899	0.210
	(1.195)	(1.182)	(0.771)	(2.018)	(1.938)	(1.840)	(0.522)	(0.688)	(0.508)
Total asset value	-7.496	-16.90	-13.67	-2.302	-0.848	2.496	-0.588	0.476	0.637
	(10.29)	(12.03)	(9.148)	(3.338)	(3.278)	(3.598)	(0.729)	(0.855)	(1.256)
Observations	260	250	310	306	272	330	705	598	696

Table OB8 provides the impacts on outcomes related to household small business activity. Columns (*T1-C*, *T2-C* and *T3-C*) give the respective differences between each experimental groups, estimated using commune fixed effect. Below in parenthesis, we provide the standard errors and in square bracket the standard deviation of the control group. Standard errors are robust and clustered at village level. *** 1%, ** 5%, * 10% significance level

Table OB9: Shocks

	1-year follow-up			
	C	***T1-C***	***T2-C***	**T3-C**
# of shocks last 12 mo	2.372	-0.545**	-0.264	-0.387
	[2.531]	(0.241)	(0.298)	(0.270)
# of shocks last 4 mo	0.366	-0.047	-0.059	-0.051
	[0.860]	(0.070)	(0.058)	(0.064)
Observations	908	801	697	764
	2-year follow-up			
# of shocks last 12 mo	2.344	0.275	-0.359	0.266
	[2.827]	(0.281)	(0.266)	(0.308)
# of shocks last 4 mo	0.194	0.001	0.007	0.025
	[0.641]	(0.048)	(0.057)	(0.057)
Observations	913	737	658	712
	3-year follow-up			
# of shocks last 12 mo	2.880	-0.204	-0.241	-0.368
	[3.248]	(0.248)	(0.302)	(0.262)
# of shocks last 4 mo	0.475	-0.067	-0.080	-0.048
	[0.998]	(0.063)	(0.081)	(0.070)
Observations	908	801	697	764

Table OB9 provides the impacts on outcomes related to shocks affecting the households. Column C provides the control average while the other columns give the respective differences between each experimental groups, estimated using commune fixed effect. Below in parenthesis, we provide the standard errors and in square bracket the standard deviation of the control group. Standard errors are robust and clustered at village level.

*** 1%, ** 5%, * 10% significance level

Appendix C: Robustness tests

Table OC1: INDEXES - Double LASSO controls

	1-year results			*2-year results*			*3-year results*		
	T1-C	**T2-C**	**T3-C**	**T1-C**	**T2-C**	**T3-C**	**T1-C**	**T2-C**	**T3-C**
Ag. Equipment	**0.092***	**0.214*****	**0.410*****	**-0.028**	**0.077****	**0.365*****	0.037	0.047	**0.147*****
	(0.047)	**(0.039)**	**(0.039)**	**(0.038)**	**(0.038)**	**(0.046)**	(0.048)	(0.047)	**(0.048)**
	[0.030]	[0.001]	[0.001]	[0.671]	[0.117]	[0.001]	[1.000]	[1.000]	[0.040]
Livestock	**0.035**	**0.233*****	**0.356*****	-0.049	0.018	0.062	0.002	0.005	0.015
	(0.041)	**(0.045)**	**(0.048)**	(0.042)	(0.046)	(0.048)	(0.049)	(0.048)	(0.048)
	[0.152]	[0.001]	[0.001]	[0.388]	[0.717]	[0.372]	[1.000]	[1.000]	[1.000]
Farming	**0.053**	0.210***	0.208***	0.042	0.114**	0.111**	0.080	-0.004	0.019
	(0.050)	(0.070)	(0.046)	(0.059)	(0.056)	(0.047)	(0.051)	(0.052)	(0.049)
	[0.117]	[0.003]	[0.001]	[0.671]	[0.117]	[0.098]	[1.000]	[1.000]	[1.000]
Saving	0.028	0.003	0.023	-0.055	-0.019	-0.009	-0.042	-0.007	-0.038
	(0.037)	(0.051)	(0.036)	(0.041)	(0.047)	(0.038)	(0.038)	(0.036)	(0.041)
	[0.162]	[0.357]	[0.184]	[0.366]	[0.717]	[0.773]	[1.000]	[1.000]	[1.000]
Aggregated index	**0.053****	**0.164*****	**0.251*****	**-0.022**	**0.048**	**0.139*****	0.016	0.011	0.035
	(0.026)	**(0.030)**	**(0.026)**	**(0.028)**	**(0.029)**	**(0.029)**	(0.030)	(0.028)	(0.029)
Poverty Prediction	-0.051***	-0.064***	-0.050***	-0.002	-0.003	**-0.027****	-0.006	-0.007	-0.022
	(0.013)	(0.014)	(0.014)	(0.013)	(0.014)	**(0.012)**	(0.015)	(0.016)	(0.014)

Same as Table 2 but with a specification including baseline outcome variables selected using double LASSO.
*** 1%, ** 5%, * 10% significance level

Table OC2: Aspiration and Stress Measures - Double LASSO

	2-year results				*3-year results*		
	C	**T1-C**	**T2-C**	**T3-C**	**T1-C**	**T2-C**	**T3-C**
Land area (ha)							
own	2.688	**0.220****	**-0.025**	**0.275****	0.264	0.106	0.204
		(0.106)	**(0.101)**	**(0.112)**	(0.162)	(0.150)	(0.170)
aspired	4.882	**0.343****	**0.024**	**0.484*****	0.426	0.239	0.403
		(0.161)	**(0.147)**	**(0.165)**	(0.359)	(0.297)	(0.381)
Cattle size (#)							
own	6.646	0.146	0.679	0.072	-0.032	0.325	0.047
		(0.362)	(0.427)	(0.370)	(0.630)	(0.624)	(0.692)
aspired	25.01	-0.798	-2.025*	-2.091*	**4.255**	2.089	**-0.338**
		(1.191)	(1.227)	(1.179)	**(3.100)**	(2.031)	**(1.901)**
Education (years)							
own	2.841	0.076	0.265	0.425***	**0.015**	**0.021**	**0.448**
		(0.156)	(0.165)	(0.161)	**(0.302)**	**(0.315)**	**(0.273)**
aspired	10.58	**0.410****	**0.765*****	**1.343*****	0.290	0.356	0.323
		(0.182)	**(0.204)**	**(0.183)**	(0.360)	(0.386)	(0.338)
Aspiration index	0.002	**0.082***	**0.058**	**0.195*****	0.175*	0.114	0.090
		(0.045)	**(0.048)**	**(0.045)**	(0.097)	(0.087)	(0.090)

Same as Table 3 but with a specification including baseline outcome variables selected using double LASSO.
*** 1%, ** 5%, * 10% significance level

Table OC3: Food security and dietary diversity - Double LASSO

		1-year results			*2-year results*			*3-year results*		
	C	**T1-C**	**T2-C**	**T3-C**	**T1-C**	**T2-C**	**T3-C**	**T1-C**	**T2-C**	**T3-C**
Household food insecurity										
insecure	0.557	0.001	0.007	0.022	0.029	**-0.062*** **	0.000	0.054**	0.071***	**0.005**
		(0.022)	(0.023)	(0.023)	(0.024)	**(0.024)**	(0.024)	(0.025)	(0.027)	**(0.025)**
Severe insecure	0.305	0.002	-0.013	**-0.054*** **	-0.042**	-0.065***	-0.040**	-0.012	0.010	0.005
		(0.021)	(0.022)	**(0.021)**	(0.019)	(0.020)	(0.019)	(0.021)	(0.024)	(0.022)
Food Diversity										
Poor diversity	0.210	-0.071***	-0.063***	**-0.011**	-0.003	0.012	-0.021	-0.038*	**-0.091*** **	-0.028
		(0.017)	(0.019)	**(0.018)**	(0.019)	(0.020)	(0.018)	(0.022)	**(0.024)**	(0.021)
Pregnant/lactating women nutrition										
Index	.	.	.	.	0.073	0.074	**0.323*** **	.	.	.
	.	.	.	.	(0.101)	(0.094)	**(0.096)**	.	.	.

Same as Table 4 but with a specification including baseline outcome variables selected using double LASSO.

Table OC4: Anthropometrics measures - Double LASSO

	1-year results				2-year results			3-year results		
	C	T1-C	T2-C	T3-C	T1-C	T2-C	T3-C	T1-C	T2-C	T3-C
Acute malnutrition										
Weight-for-height	-0.800	-0.029	-0.137***	0.018	-0.077*	-0.019	0.001	0.101**	0.121**	0.050
		(0.044)	(0.046)	(0.043)	(0.044)	(0.046)	(0.044)	(0.044)	(0.047)	(0.042)
. . . wasting	0.134	0.019	0.019	-0.019	0.036***	0.035***	0.008	-0.007	-0.006	-0.011
		(0.014)	(0.015)	(0.013)	(0.012)	(0.013)	(0.012)	(0.014)	(0.015)	(0.013)
. . . severe wasting	0.026	-0.002	-0.009	-0.007	0.008	0.000	-0.005	0.000	0.007	0.002
		(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)
MUAC	-1.068	-0.024	0.038	0.093***	-0.027	-0.034	0.073*	0.076	0.047	0.129***
		(0.033)	(0.035)	(0.034)	(0.041)	(0.043)	(0.043)	(0.052)	(0.053)	(0.048)
. . . wasting, MUAC	0.148	-0.012	-0.003	-0.009	0.028	0.021	0.020	0.010	0.019	-0.016
		(0.015)	(0.017)	(0.016)	(0.019)	(0.020)	(0.019)	(0.028)	(0.030)	(0.025)
Chronic malnutrition										
Height-for-age	-1.454	-0.011	0.089*	0.146***	-0.016	0.060	0.161***	-0.080*	-0.044	0.115***
		(0.049)	(0.052)	(0.048)	(0.047)	(0.053)	(0.049)	(0.045)	(0.048)	(0.042)
. . . stunting	0.338	0.015	-0.025	-0.035*	0.026	-0.016	-0.044**	0.049***	0.037*	-0.025
		(0.019)	(0.021)	(0.019)	(0.018)	(0.019)	(0.018)	(0.018)	(0.019)	(0.016)
. . . severe stunting	0.128	-0.018	-0.009	-0.035**	-0.007	-0.005	-0.030**	0.005	0.003	-0.019**
		(0.014)	(0.015)	(0.014)	(0.012)	(0.013)	(0.012)	(0.011)	(0.012)	(0.010)
Underweight										
Weight-for-age	-1.376	-0.036	-0.040	0.109***	-0.088**	-0.007	0.126***	-0.033	0.044	-0.044
		(0.038)	(0.039)	(0.037)	(0.036)	(0.039)	(0.037)	(0.048)	(0.052)	(0.044)
. . . underweight	0.281	0.019	0.010	-0.040**	0.043**	0.004	-0.032*	0.022	-0.012	-0.032*
		(0.018)	(0.019)	(0.018)	(0.017)	(0.018)	(0.016)	(0.018)	(0.019)	(0.016)
. . . severe underweight	0.083	-0.009	-0.021*	-0.032***	0.020**	0.007	-0.014	0.010	0.006	-0.011
		(0.012)	(0.012)	(0.011)	(0.010)	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)
Observations	1,576	1,457	1,207	1,313	1,719	1,377	1,510	1,700	1,331	1,698

Same as Table 5 but with a specification including baseline outcome variables.
*** 1%, ** 5%, * 10% significance level

Table OC5: CREDI Test Results - 0-36 months Children - 3-year follow-up survey - Double LASSO

	3-year follow-up		
	T1-C	***T2-C***	***T3-C***
Cognition	0.008	-0.062	**0.129***
	(0.072)	(0.076)	**(0.071)**
Langage	0.017	-0.068	**0.150****
	(0.075)	(0.077)	**(0.071)**
Moteur	0.021	-0.043	**0.135***
	(0.069)	(0.076)	**(0.070)**
Socio-emotional	0.000	-0.047	**0.133***
	(0.068)	(0.074)	**(0.070)**
Mental health	0.040	**0.165**	0.025
	(0.110)	**(0.111)**	(0.087)
Score global	0.026	-0.037	**0.145****
	(0.069)	(0.075)	**(0.070)**
Observations	510	411	463

Same as Table 6 but with a specification including baseline outcome variables selected using double LASSO.

Table OC6: MELQO Test results - 36-59 months children - Double LASSO

	2-year results			*3-year results*		
	T1-C	***T2-C***	**T3-C**	***T1-C***	***T2-C***	**T3-C**
Math score	**-0.059**	**0.041**	**0.096****	**0.033**	**-0.091****	**-0.003**
	(0.043)	**(0.049)**	**(0.047)**	**(0.041)**	**(0.046)**	**(0.041)**
Cognitive score	**-0.040**	**0.066**	0.002	**0.082****	**-0.048**	**0.053**
	(0.034)	**(0.040)**	(0.038)	**(0.041)**	**(0.046)**	**(0.041)**
Language score	-0.096**	-0.058	-0.102**	**0.049**	**-0.061**	0.009
	(0.041)	(0.046)	(0.043)	**(0.040)**	**(0.043)**	(0.039)
Motor Score	**0.025**	**-0.114****	**0.179*****	**0.020**	**-0.174*****	**0.041**
	(0.054)	**(0.057)**	**(0.053)**	**(0.057)**	**(0.061)**	**(0.055)**
Overall score	**-0.037**	**0.046**	-0.010	**0.055**	**-0.066***	**0.014**
	(0.033)	**(0.038)**	(0.036)	**(0.035)**	**(0.038)**	**(0.034)**
Observations	558	488	510	599	456	587

Same as Table 7 but with a specification including baseline outcome variables selected using double LASSO.