The European Journal of Development Research
https://doi.org/10.1057/s41287-023-00603-y

ORIGINAL ARTICLE

Do Behavioral Interventions Enhance the Effects of Cash on Early Childhood Development and Its Determinants? Evidence from a Cluster-Randomized Trial in Madagascar

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Accepted: 8 June 2023

Abstract

There is growing interest in how best to leverage cash transfers to foster positive impact on children in beneficiary households. We evaluate the effects of interventions based on behavioral science on measures of early childhood socio-cognitive development (and related household-level outcomes) for children from households receiving cash transfers in Madagascar using a multi-arm cluster-randomized trial, where communities were randomized into arms, with 77 communities in each arm and approximately 950 and 1200 households sampled at baseline and midline, respectively. Three behavioral interventions (a 'Mother Leaders' group, either by itself or augmented with a 'self-affirmation' or a 'plan-making' nudge) are layered onto a child-focused cash transfer program targeting the rural poor in Madagascar with children aged 0–6. Approximately 18 months into the implementation of these interventions, we find evidence that households in the behaviorally enhanced arms undertake more desirable parenting behaviors, interact more with their children, prepare more (and more diverse) meals at home, and report lower food insecurity than households that received only cash, and children in these arms perform better than children from households in the cash-only arm on several measures of socio-cognitive development including language learning and social skills. This is promising evidence that behavioral interventions can add significant value to cash transfer programs that aim to improve human development outcomes. (AEARCTR-0000957).

Keywords Behavioral economics · Early childhood development · Welfare · Well-being · Poverty · Development economics · Health

This paper draws from a Working Paper previously submitted in the World Bank Working Paper Series (WPS9747), the working paper series of the institution of the authors.

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Published online: 05 October 2023

Résumé
Il y a un intérêt croissant pour savoir comment mieux exploiter les transferts d'argent pour favoriser un impact positif sur les enfants dans les ménages bénéficiaires. Nous évaluons les effets des interventions basées sur les sciences comportementales sur les mesures du développement socio-cognitif précoce (et les résultats connexes au niveau des ménages) pour les enfants des ménages recevant des transferts d'argent à Madagascar en utilisant un essai randomisé en grappes à plusieurs bras, où les communautés ont été randomisées en bras, avec 77 communautés dans chaque bras et environ 950 et 1 200 ménages échantillonnés à la ligne de base et à la mi-parcours, respectivement. Trois interventions comportementales (un groupe de 'Mères Leaders', soit par lui-même ou augmenté avec une 'auto-affirmation' ou une incitation à 'faire des plans') sont superposées à un programme de transfert d'argent axé sur l'enfant ciblant les pauvres ruraux à Madagascar avec des enfants âgés de 0 à 6 ans. Environ 18 mois après la mise en œuvre de ces interventions, nous trouvons des preuves que les ménages dans les bras améliorés comportementalement entreprennent des comportements parentaux plus souhaitables, interagissent davantage avec leurs enfants, préparent plus (et des repas plus divers) à la maison et signalent une insécurité alimentaire plus faible que les ménages qui n'ont reçu que de l'argent, et les enfants dans ces bras se comportent mieux que les enfants des ménages dans le bras uniquement en espèces sur plusieurs mesures du développement socio-cognitif, y compris l'apprentissage des langues et les compétences sociales. Ceci est une preuve prometteuse que les interventions comportementales peuvent ajouter une valeur significative aux programmes de transfert d'argent qui visent à améliorer les résultats du développement humain. (AEARCTR-0000957).

Resumen
Existe un creciente interés en cómo aprovechar mejor las transferencias de efectivo para fomentar un impacto positivo en los niños de los hogares beneficiarios. Evaluamos los efectos de las intervenciones basadas en la ciencia del comportamiento en medidas de desarrollo socio-cognitivo temprano en la infancia (y resultados relacionados a nivel del hogar) para niños de hogares que reciben transferencias de efectivo en Madagascar utilizando un ensayo de agrupación multi-brazo aleatorizado, donde las comunidades fueron aleatorizadas en brazos, con 77 comunidades en cada brazo y aproximadamente 950 y 1,200 hogares muestreados en la línea de base y en el punto medio, respectivamente. Tres intervenciones conductuales (un grupo de 'Madres Líderes', ya sea por sí solo o aumentado con un 'autoafirmación' o un 'estímulo para hacer planes') se superponen a un programa de transferencia de efectivo enfocado en los niños que apunta a los pobres rurales en Madagascar con niños de 0 a 6 años. Aproximadamente 18 meses después de la implementación de estas intervenciones, encontramos evidencia de que los hogares en los brazos mejorados conductualmente emprenden comportamientos parentales más deseables, interactúan más con sus hijos, preparan más (y más diversas) comidas en casa e informan una menor inseguridad alimentaria que los hogares que solo recibieron efectivo, y los niños en estos brazos se desempeñan mejor que los niños de hogares en el brazo solo de efectivo en varias medidas de desarrollo socio-cognitivo, incluyendo el aprendizaje del lenguaje y las habilidades sociales. Esta es una evidencia prometedora de que las in-

tervenciones conductuales pueden agregar un valor significativo a los programas de transferencia de efectivo que buscan mejorar los resultados del desarrollo humano. (AEARCTR-0000957)

Introduction and Motivation

In recent years, the development community and researchers begun to pay increasing attention to the potential for leveraging cash transfer programs to foster early childhood development (ECD) (World Bank 2018a, b; Black et al. 2017; Currie and Almond 2011). The rationale for the interest in this nexus is driven by two parallel developments in the literature. Firstly, a growing body of research demonstrates that investing in children's health, nutrition, cognition, and socio-emotional development during the 'early years'—defined as the period between gestation and a child's sixth birthday, when there is a high degree of plasticity in children's neurological development—has important cumulative effects for overall development, dramatically improving a range of outcomes, including labor outcomes, later in life (World Bank 2018a; Grantham-McGregor et al. 2007; Black et al. 2017; Currie and Almond 2011; WHO 2018; World Bank 2018b; Gertler et al. 2014; Cunha and Heckman 2007; Cunha et al. 2006). At the same time, the global expansion of cash transfer programs into low-income countries means that such programs—which aim to target the poorest families with children—are among the few public-sector programs in developing countries that directly reach a large number of the very households where deprivations such as chronic malnutrition and other indicators of poor child development are concentrated, though there are still many vulnerable people who do not access them (World Bank 2018a, b). Taken together, these two developments have led to a renewed focus on the possibilities of using cash transfer programs to impact ECD among children experiencing poverty in low- and middle-income countries.

This interest is by no means novel. Pioneering "conditional" cash transfer programs in Latin America had an explicit focus on encouraging families to invest in their children's human capital, often through requirements to take children to health clinics as well as school attendance and enrollment, which arose from the recognition that availability of cash may be a necessary but not always sufficient condition to ensure that the desired outcomes are achieved (Lagarde et al. 2007; Fiszbein et al. 2009). More recently, research emphasizing the importance of parental behaviors on early childhood outcomes has led to a greater emphasis on incorporating ECD programming into cash transfer programs (Vargas-Baron 2009). Evaluations of cash transfer programs have revealed that they have important, measurable effects on a variety of early childhood outcomes beyond impacts on consumption and access to education and health services, which were their initial focus (Bastagli et al. 2016; de Walque et al. 2017). These include positive impacts on mitigating the negative impact of early life shocks (Adhvaryu et al. 2016), food consumption (Adato and Bassett 2009), nutritional diversity (Attanasio et al. 2014; Fernald et al. 2008), food security (see De Groot et al. 2015), cognitive development (Macours et al. 2012),

mixed evidence on stunting and wasting (De Groot et al. 2015), and some effects on reducing infant mortality (Rasella et al. 2013). A summary of recent research on the impact of cash transfer programs in Sub-Saharan Africa by the Transfer Project finds consistently positive impacts of cash on some child outcomes (material well-being, secondary school-age enrollment, spending on school inputs), but weak or inconsistent evidence on others (anthropometric measures, risky sexual behavior among adolescents, child work and labor, morbidity, healthcare-seeking behavior, and early marriage) (Tirivayi et al. 2021).

Pro-poor cash transfer programs targeted at households with young children can thus help to mitigate the detrimental and long-lasting effects that poverty has on child development, supporting human capital accumulation and reducing inequality from early in life via the provision of periodic exogenous income supplements, enabling behaviors that were not previously financially possible for households (Kabeer and Waddington 2015; Tsur 2016, and others).

However, early-childhood-focused cash transfers present a specific set of design and programmatic challenges that mean that some techniques employed by more traditional cash transfers as a means of improving impact may not be as useful when applied to such transfers. For example, early cash transfers in Latin America sought to buttress the effect of the cash with direct 'conditionality,' where the receipt of the cash is conditional on certain actions (e.g., regular health care visits or school attendance), fostering the use of services, notably for education and health (Lagarde et al. 2007; Fiszbein et al. 2009). However, the use of formal or 'hard' conditionality is difficult in the case of ECD-focused programs, where many critical behaviors (e.g., balanced/nutritious feeding, breastfeeding, stimulation to promote socio-cognitive growth, etc.) are private, ongoing and hard to measure, and where investments in children's human capital often depend critically on parents' behavior and less on access to supply side services where attendance or uptake can be tracked.

As a result, as well as due to a growing literature that emphasizes downsides of conditionality, especially in settings with limited implementation and monitoring capacity and where there may be limited availability of resources or facilities needed to enable households to meet conditions, there has been increasing interest in designing and evaluating alternatives to formal conditionality, including the use of "accompanying measures" ranging from nutritional supplementation, family practices training, and other modalities (Cookson 2018; Arriagada et al. 2018). However, while there is some evidence that this 'Cash Plus' approach leads to positive impacts in the short run (Berhman and Hoddinott 2005; Macours et al. 2012), some follow-up studies have found effects to dissipate over time (Attanasio et al. 2014), demonstrating the challenge *of sustainable* behavior change and investments in children's human capital needed to promote long-term welfare.

The question of how best to design ECD-focused cash transfers—in terms of program parameters, delivery mechanisms, and supplemental measures—thus remains open. With conditionality difficult or impossible and mixed results from straightforward informational or awareness-based programming alone, there is room to explore alternative techniques of encouraging the behavior change on the part of caregivers/parents that is critical for ECD-focused transfer programs to achieve their desired ends. Insights from the field of behavioral economics may have a role to play.

Behavioral economics finds that human decision-making is subject to a variety of cognitive biases that cause behavior to diverge from the predictions of neoclassical economics; that many of these biases are activated and exacerbated by small features of decision-makers' contexts; and that while these effects operate for all populations, they are particularly marked for those experiencing poverty (Datta and Mullainathan 2012; Mullainathan and Shafir 2013). Applied to cash transfers, this suggests that even small features of the context in which program beneficiaries operate may impact decisions (and thereby outcomes) by exacerbating the effect of common features of human psychology such as limited self-control, limited attention, present bias, etc., in ways that could reduce the likelihood of beneficiaries taking the kinds of decisions and actions the program aims to generate, *even when beneficiaries can afford to take these steps and are intrinsically motivated to do so.*

Behavioral economics further suggests that small tweaks—or 'nudges'—can improve follow-through on behaviors critical for program success (Datta and Mullainathan 2012). In particular, it suggests that subtle aspects of program design that have traditionally been overlooked can be adjusted or augmented in ways that can help to amplify the effects of cash by aligning program features more closely with intended beneficiaries' cognitive processes (World Bank 2015; Datta and Mullainathan 2012; Mullainathan and Shafir 2013).

Several recent studies test the impact of such 'nudges' on cash transfer beneficiaries' decisions, actions, and outcomes with promising results. For example, Moroccan cash transfer beneficiaries' resource allocation decisions are highly sensitive to how the purpose of the funds they are given is "labeled" (Benhassine et al. 2015). Similarly, a formally unconditional cash transfer accompanied by publicity around school enrollment and health checkups for beneficiaries' children led to substantial increases in school enrollment among the poorest quintile (Oosterbeek et al. 2008). Sedlmayr et al. (2018) find promising impacts from a simple intervention that asks beneficiaries to articulate spending goals before receiving the transfer. Cohen et al. (2017) find that a non-binding commitment to give birth in a high-quality clinic increases the likelihood of transfer-receiving expectant mothers doing so. "Right-timing" transfer payments also improve the likelihood of follow-through on intentions to send children to school, perhaps by reducing the opportunities for spending off-plan between the time of receipt and the time of spending on educational expenses (Barreira-Osorio et al. 2011). At the same time, a meta-analysis of 11 randomized and quasi-experimental studies that supplemented cash with additional transfers, interventions, or services and measured impacts on child outcomes found no overall evidence that such "Cash Plus" programming was more effective than cash alone at improving child well-being, although combining cash with food transfers or primary healthcare may have additive impacts (Little et al. 2021).

However, gaps remain in the application of behavioral economics to cash transfers, and especially to its application to ECD-focused transfers. First, given that parents and caregivers are the lead agents for investments in children's early years, and their behavioral choices and actions have clear impacts on children's development, the application of behavioral insights to parents' engagement in early childhood promises to "uncover approaches that could enhance and support participation and engagement of parents of children who are eligible for early interventions"

(Gennetian et al. 2016). However, there is little evidence of the impact of behaviorally informed interventions on outcomes in ECD-focused transfer programs. Secondly, few studies (with the exception of Sedlmayr et al. 2018) have built on the idea of cognitive bandwidth and slack to explore the potential to exploit a plausible temporary 'slackening' of cognitive bandwidth constraints at the time of receipt of a transfer payment to support beneficiaries to engage in more deliberate or reflective decision-making that could enhance program effectiveness, and which the theory suggests may be more effective at such times (Mullainathan and Shafir 2013; Mani et al. 2013). Further research on the effectiveness of behavioral enhancements to cash transfer programs (especially those that target parenting and ECD outcomes) therefore emerges as a priority.

In this paper, we attempt to fill this gap in the literature on the intersection between cash transfer programs, ECD, and applied behavioral economics by evaluating three variants of a package of behaviorally informed interventions layered onto the Human Development Cash Transfer (HDCT), a child-focused cash transfer program in Madagascar, using a cluster-randomized evaluation design. We evaluate whether and to what extent ECD outcomes for recipient's children are impacted by 'mother leader' groups or 'mother leader' groups alongside a behavioral "nudge" (either plan-making or self-affirmation) 18 months into implementation, as compared to children of recipients who only received cash.

We find that one or more of the behaviorally enhanced arms have significant positive impacts on all measured behaviors compared with the corresponding behaviors for those receiving only cash. Households in one of the enhanced arms also see higher increases in the number of meals prepared as well as greater reductions in 12-month food insecurity, while households in all three enhanced arms see greater reductions in past-week food insecurity, than households in the cash-only group. The children in some of the enhanced arms saw greater development in language learning and social skills than children in the cash-only group. We do not find significant differences between any of the enhanced arms, and cannot distinguish between the effects of the Mother Leaders component and additive effect of the additional 'nudges.'

The "Background: Madagascar and the Human Development Cash Transfer" section of this paper provides background and motivation. The "Interventions: Rationale and Design" section describes the interventions, rationale, and the theory of change. "Evaluation Design, Data and Estimation Strategy" section discusses data sources, variables, and experimental strategy. Finally, the "Results: Behavioral enhancements vs Cash Only" section presents the main results and the "Conclusion" section concludes by discussing the results, limitations, and directions for further work.

Background: Madagascar and the Human Development Cash Transfer

Madagascar's Economic and Human Development Trajectory

Although Madagascar has sustained consistently high rates of economic growth each year since 2013, this has yet to make a significant dent on poverty: the country

remains one of the world's poorest, with, the poverty rate inching down from 77.7% in 2014 to 75.1% in 2018 (World Bank 2017, 2018b). Almost 80% of the population lives on less than US$1.90 per day.[1] This makes Madagascar the country with the sixth highest number of people in poverty in the world.

Many of the rural poor are deprived in multiple dimensions including consumption, literacy, education, basic household assets, and access to public services such as electricity. Madagascar also has the world's fourth highest rate of chronic malnutrition,[2] with one child in two under five years old suffering from stunting.

Social Protection and the Human Development Cash Transfer Program

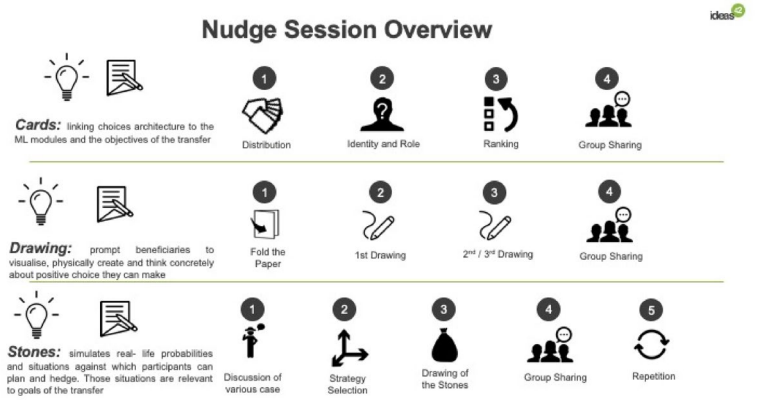
Starting in 2016, the Government of Madagascar began seeking to address these high levels of poverty and low levels of human development by implementing a Human Development Cash Transfer (HDCT) program in partnership with the World Bank and UNICEF. Under this program, cash payments set at about 30% of average household consumption in program areas are made to households with children under 12 once every two months, reaching 97,000 children in 7 districts with particularly low levels of human development. Households are selected for inclusion based on a proxy means test to identify the poorest households. Payments are made to the children's mother, who was expected to be the primary caregiver. While a portion of the cash transfer for households with children over 6 years old is conditioned on regular primary school attendance, households with younger children receive an unconditional transfer. Since the purpose of this study is to evaluate the effects of behavioral interventions in addition to an unconditional cash transfer for early childhood development, the sample includes only the households with children under six (i.e., those receiving the unconditional transfer).

Interventions: Rationale and Design

Our study tests the additionality on ECD and antecedent behaviors and outcomes relevant to these from one of three variants of a suite of behavioral interventions layered onto the cash itself. All the variants incorporate 'Mother Leader' groups, which we will refer to as ML groups, where a set of beneficiaries from a program village are organized into a group headed by a beneficiary mother elected by the members (see "Behaviorally Enhanced Arm 1: Mother Leaders groups only" section below for details). In addition to this, the second and third 'enhanced arms' use trained facilitators to deliver a "nudge" intervention—either 'plan-making' or 'self-affirmation'—to ML groups on cash transfer days. This "nudge" part of the second and third enhanced arms began with the fourth cash transfer tranche, or about

[1] The headcount measuring the percent of the population under the extreme poverty rate of US$1.90 (in PPP terms) was 76.2% in 2017 (http://www.worldbank.org/en/country/madagascar/overview).

[2] Chronic malnutrition affects 47.3% of children in Madagascar between 6 and 59 months (World Food Programme Country Brief, May 2018).



Plan Making			
Forms	**Psychology**	**Desired outcome**	**Description**
	Commitment device, concrete plan-making, salient reminder	Women are better able to visualize the goals they want to achieve with the transfer, as well as draw out the concrete intermediate steps to reach those goals.	Beneficiaries draw: (1) Current state, (2) Future goal, and (3) Intermediate steps linking (1) and (2) (4) Volunteers share goals and plans with the group
	Choice architecture, visual reminder, concrete plan-making	Curating the choice set on how to spend the transfer sets social norms on what to do and provides women with concrete suggestions on important investments.	(1) Beneficiaries name their primary identity, (2) Beneficiaries rank purchases in order of importance, (3) Volunteers share their first choice and rationale with the group
	Locus of control, self-efficacy	Women internalize stronger sense of control over their environment and ability to prevent bad outcomes	(1) Beneficiaries discuss plans to prepare for bad but uncertain outcomes (child falling ill), (2) Beneficiaries simulate outcomes (7 bad to 3 good), (3) Beneficiaries discuss plans to *prevent* bad outcomes (4) Beneficiaries simulate outcomes (3 bad to 7 good)
Self Affirmation			
Intervention	**Psychology**	**Desired outcome**	**Description**
	Locus of control, self-efficacy, positive self-concept	Women make concrete links between their expenditure choices and their direct impact / consequence on family members. This primes their caretaker identity and provides positive feedback on the impact of their choices.	Beneficiaries draw: (1) Positive choice made with last transfer, and (2) How that choice affected their family (3) Volunteers share goals and plans with the group
	Self-affirmation, identity, priming, salient visual reminder	Identifying a specific positive value women believe to be important and thinking through how they exemplify it can affirm them to think more positively about themselves.	(1) Beneficiaries name their primary identity, (2) Beneficiaries rank values in order of personal importance, (3) Volunteers share their first choice and example of such behavior with the group

Fig. 1 "Nudge" designs

	Locus of control, self-efficacy	Women internalize stronger sense of control over their environment and ability to prevent bad outcomes	(5) Beneficiaries discuss plans to prepare for bad but uncertain outcomes (child falling ill), (6) Beneficiaries simulate outcomes (7 bad to 3 good), (7) Beneficiaries discuss plans to *prevent* bad outcomes (8) Beneficiaries simulate outcomes (3 bad to 7 good)

Fig. 1 (continued)

14 months before the midline, supplementing the ML program which had been ongoing since program implementation began approximately 18 months before the midline. Behaviorally enhanced arms 2 and 3 were delivered immediately before the payment and consisted of group sessions lasting between 15 and 30 min, co-facilitated by the ML and an externally hired facilitator (with the aim of gradually transitioning full leadership responsibility to the ML). The motivation and design of these interventions are described more fully below, and further details on the content of the "nudges" a found in Fig. 1. No evidence of harms was found from any of the intervention iterations.

Behaviorally Enhanced Arm 1: Mother Leaders Groups Only

The first behaviorally enhanced arm simply layers the 'Mother Leaders'(ML) intervention onto the cash. The 'Mother Leaders' intervention is a norm-oriented behavioral intervention relying on peer influence, building support systems in small groups and providing positive support to others in the group. Its introduction was motivated by experience from a Colombian cash transfer program "*Familias en Accion,*" where elected, mostly female beneficiary leaders work with groups of beneficiary mothers to augment cash transfer program activities through home visits and community activities, which was found to have contributed to improved child development (Attanasio et al. 2014). Its design also draws upon the finding that mother/peer group-based interventions are effective in improving dietary diversity and meal frequency, and reduce wasting in children under 5 when paired with home visits (Janmohamed et al. 2020).

In the HDCT version of this intervention, social norms, peer influence, and community dynamics are leveraged through Mother Leaders. These women are beneficiaries of the program elected by their peers to lead a group of other beneficiaries in their home village to a one-year term. Mother leaders are responsible for keeping beneficiaries informed about the program's rules, responsibilities, and procedures including payment schedules and complaint mechanisms. Second, they are responsible for organizing bimonthly meetings to learn about and discuss issues of food/nutrition for pregnant and breastfeeding mothers and children, prenatal consultation, exclusive breastfeeding, children compulsory vaccination, food diversification and nutrition, hand washing, use of potable water, use of latrines, and ECD. These meetings also serve as a place to share

opinions and information among community members in the same environment and facing the same challenges. Mother Leaders further supplement these bimonthly meetings with home visits to follow up on the adoption of practices discussed.[3]

Behaviorally Enhanced Arm 2: Mother Leaders and Plan-Making

The design of the 'nudges' layered onto the ML component followed from extensive field research, which uncovered two major groupings of potential barriers to effective allocation decisions (and actions), and two corresponding interventions. The first of these was situational impediments to forming and executing a plan for using transfer funds that inhibited their use for the achievement of beneficiary goals. While beneficiaries tended to have clear high-level goals for their participation in the program, such as seeing their children graduate from secondary school or becoming salaried professionals, they either lacked the cognitive bandwidth needed to plan realistic steps that they could take to achieve their goals, they experienced difficulty executing these once they received the money, or both. In line with the literature on cognitive scarcity (see Shah et al. 2012; Mani et al. 2013), they were more focused on routine needs that the transfer could support than forward-thinking investments. This tendency was exacerbated by the setting of the transfer payments, which took place among an attention-sapping milieu crowded with social interaction and local market vendors, many of whom had sprung up precisely to take advantage of the transfer, further increasing the potential for spending off-plan (as in Baumeister et al. 1994; Gollwitzer and Moskowitz 1996; Kuhl 1984; Loewenstein 1996; Mischel et al. 1996; Rachlin 1995; Thaler 1994; Wegner 1994). This treatment arm incorporated a "nudge" that used plan-making to link the transfer to goals, enabling women to better adopt a longer-term perspective with concrete goals they wish to achieve with the cash and identify concrete risk mitigation strategy to help them reach their goals.

Behaviorally Enhanced Arm 3: Mother Leaders and Self-affirmation

The second key finding from field research was that negative "mindsets" also limited beneficiary aspirations, goals, and actions. Holding positive mindsets have been found to be an important input in anti-poverty programs, particularly ones that hinge on the need to take risks with long-term future payoff (Campos et al. 2017). We found that HDCT beneficiaries seemed to *externalize* their own role in taking decisions and improving their circumstances, a marker of potentially negative psychological mindset factors including loss of self-efficacy, locus of control, and psychological well-being, making it more difficult to come up with specific action steps and carry these out at the right time and at the expense of more immediate needs (Ghosal et al. 2013). To address this, an additional arm incorporated a 'self-affirmation' intervention, which involved activities aimed at enabling women to define what they want, to make

[3] Further details about the mother leader sessions and facilitators can be found here: http://www.womensworldbanking.org/wp-content/uploads/2019/06/CaseStudy-Madagascar.pdf.

decisions about the well-being of the family, and reinforcing their identity as guardians and the power they have to improve the lives of their children. These sessions reinforce women confidence that they can have positive influence of their family's happiness.

Operational Details

The activities carried out on transfer day for behaviorally enhanced arms 2 and 3 consisted of group sessions lasting between 15 and 30 min with about 20 beneficiaries, co-facilitated by the ML and an externally hired facilitator trained by ideas42, with the aim of gradually transitioning full leadership responsibility to the ML. To reinforce the link between beneficiaries' receipt of their transfer amount and their resultant decisions and actions (for instance about engaging in child stimulation, purchasing nutritious foods, and other human capital-enhancing behaviors) as directly as possible while eliminating additional time or travel requirements for beneficiaries, each of the nudges occurred in the hours immediately preceding the transfer payment itself while beneficiaries waited to receive their cash at the transfer siter. Beneficiaries were organized by their ML group while waiting for the payment and brought into an adjacent area, where the facilitator led the group in a set of activities. Figure 1 presents design specifications for each activity set.

Theory of Change

Our broad theory of change builds on the argument laid out in Gennetian et al. (2016). In our theory of change, interventions are hypothesized to affect beneficiary *behaviors*, which then lead to changes in *proximate outcomes*, which in turn are hypothesized to lead to changes in *longer-term outcomes*. Broadly, a host of parenting and nutrition-related behaviors are hypothesized to lead to proximate outcomes such as more diverse diets and lower food insecurity in the short term, and through these to longer-term outcomes such as better physical and cognitive child development. We summarize these three classes of variables and the specific outcomes we measured in the "Key Variables" section below. By and large, we should think of there being a temporal dimension to this classification. We expect that the behaviors we are assessing can change quickly and are typically ongoing. The proximate outcomes are likely driven by behavior changes, and are likely to take longer to materialize (perhaps weeks or months). Finally, long-term outcomes such as child development measures are likely driven by both sustained behaviors and changes in proximate outcomes, and may take months or years to change. While the relationship between individual variables is complex and multi-dimensional, an example might help to clarify the reasoning here: higher food consumption is posited to lead to lower food insecurity, and over time to better physical development.

Evaluation Design, Data and Estimation Strategy

Evaluation Design

In this paper, we report the results of an evaluation 'embedded into' a broader evaluation of the cash transfer program itself. The broader evaluation (whose results are in the Appendix Tables 6, 7, 8) assessed the impacts of receiving the transfer vs. not receiving the transfer. For this, 51 communes were randomized (to either receive cash or no cash) through a lottery at the national office in Antananarivo, where Ministry of population and FID (Fonds d'Intervention pour le Développement) senior officials met and put pieces of paper with the names of all of the communes in a bag, and randomly drew the 38 communes who would receive the cash transfer.

The goal of the 'embedded' evaluation whose results we report here was to estimate the additive impact of the enhanced behaviorally informed treatments relative to the receipt of cash alone. This embedded evaluation is in the spirit of those included in the Little et al. (2021) meta-analysis of Cash Plus programming, which similarly asked whether additional programming led to measurable additive effects on child outcomes over and above those from the provision of cash alone. For this 'embedded' evaluation, 309 villages in six districts (Tomasina II, Mahanoro, Vohipeno, Ambohimahasoa, Betioky Sud, and Faratsiho) that were randomly selected to receive the cash transfer were pooled[4] and re-randomized into three groups corresponding to the three "enhanced" treatments (MLs and behavioral variants), plus a "cash only" condition, which serves as the control group for the purposes of this evaluation. The randomization of these 309 villages was completed by the researchers using Stata to randomly generate treatment assignments. Communities then each participated in the allocated program, but were not informed of the specific differences between each arm. Note that for reference, the results of the evaluation of the cash transfer program itself—i.e., the comparison of households in the communes which received cash only to households in the communes randomized to be 'pure controls' (no cash)—are found in Appendix Tables.

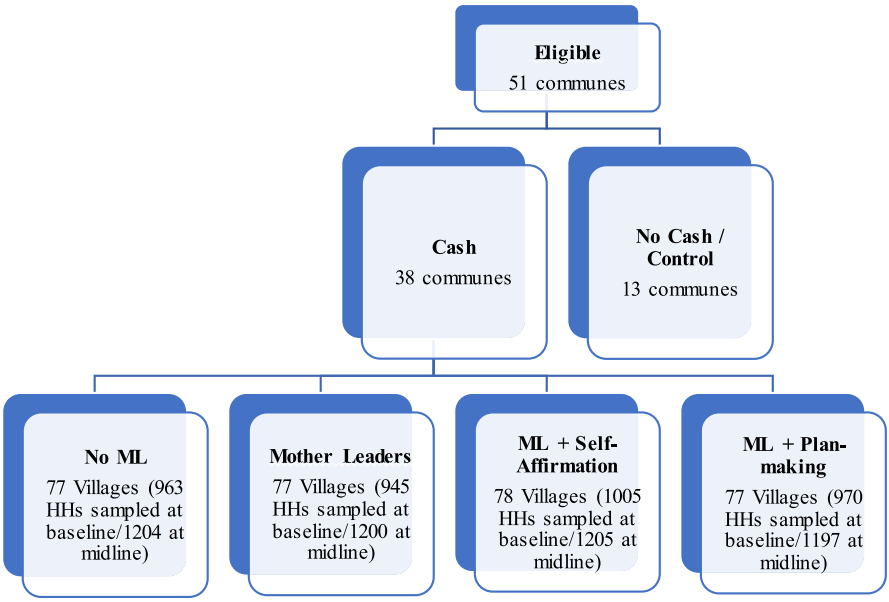
It is worth noting that despite the fact that we survey respondents at two points in time, we do *not* have a true panel. We conducted a limited baseline—surveying a randomly selected 12–13 households per village, for a total of 3883 households evenly split between the four arms—solely for the purposes of verifying balance across experimental groups. To evaluate outcomes, we surveyed a larger number of households from the same villages, this time targeting 16 households per village, for a total of 4806 households.[5] Households were randomly selected based on the most updated lists of beneficiaries.

[4] An inherent weakness of this pooling feature is that the design ignores the potential for within-commune spillovers between "enhanced" treatment conditions. This potential was judged to be low owing to the subtle nature of the differences in intervention types at the transfer payment point.

[5] This sample size was determined based on power calculations following standard economics assumptions of 5% significance and 80% power. We assumed a minimum detectable effect of 0.2 standard deviations, as is typically found for behavioral interventions, and assumed a standard intra-cluster correlation of 0.1.

While the households surveyed at baseline were included in the list of eligible households for the midline survey, they were not prioritized for inclusion. However, a subset of them were sampled at midline as well, giving us 3189 households for which we have data on at least some outcome variables at both points in time. This allows us to run a specification where we control for baseline values, albeit only for the portion of this restricted sample for which all relevant variables are available. However, throughout, we emphasize results from the full midline sample, either with or without demographic controls. We planned to conduct a further endline survey in 2020; however, this was never conducted due to the COVID-19 pandemic.

The following figure summarizes the design:



Data Sources

Baseline and outcome data come from household surveys. Data were collected in two waves, with the baseline ($N=3883$) occurring about 30 days before the first payments in each district, in September 2018, and the midline ($N=4806$) after about 20 months (May 2018), at beneficiaries homes. The nudges were delivered approximately every two months between the baseline and midline surveys. The survey featured modules on household consumption, assets, food security, educational attendance, and parenting behaviors taken from prior national Demographic and Health Surveys.[6] Prior to data collection, participants were informed that they would be

[6] See https://www.instat.mg/ for more details.

participating in a research study should they consent to be surveyed. However they were not specifically informed which arm of the trial they were in, and did not know what treatments the other arms were receiving. Those completing data analysis were not blind to which arm participants were in.

For child development progress, the Malawi Development Assessment Test (MDAT) pioneered by Gladstone et al. (2010) was adapted to the local context under the supervision of one of the test's original creators and a local child development expert. The MDAT is designed to measure skills in the areas of speech/language, motor skills, and social interaction. An ECD specialist spent a year adapting and testing the tool for the Malagasy population, changing items where necessary and exploring how to best adapt assessment rules.

The MDAT is only used for one child per household within the age range that the test is designed for. If there was more than one eligible child in the household, one was chosen at random. As such, the sample size for the MDAT and its component indices is smaller than the total number of households surveyed since not all households have children in the appropriate age range, resulting in a sample of 2757 for the child development outcomes.

Key Variables

Key outcome variables are defined as below. As discussed earlier in the theory of change, we broadly classify our outcome variables into Behaviors, Proximate Outcomes, and Long-Term Outcomes. The list below reflects this classification.

Behaviors

Positive Parenting Behavior How many of three positive parenting behaviors (following up on the education of the child, checking up on the child's health, and playing with children) the parent reports engaging in over the preceding twelve months. It ranges in value between 0 and 3.

Interaction With Children How many of 6 parenting behaviors (read a book, told stories, sang songs, went for a walk, played with, taught to count or draw) the parent reports engaging in with their two youngest children over the preceding 3 months. Each of these behaviors is coded as a 0–1, so that this variable ranges in value between 0 and 6.

Preparing Diverse Meals A binary variable taking the value 1 if the household reports having usually prepared 'diverse meals' including fruit over the preceding 12 months, and 0 otherwise.

Proximate Outcomes

Food Diversity The sum of the number of ten different categories of food (cereals, flours, legumes, vegetables, fruits, etc) that the household reports consuming over the last 7 days takes a maximum value of 10 if household reports consuming all the categories of food. Dietary diversity was included instead of attempting

to measure nutrition directly, since it has been found to be a good predictor of micronutrient intake in Madagascar (Moursi et al. 2008).
Number of Meals Prepared Number of meals the household reports preparing on the day prior to the survey.
Food Insecurity, 7 Day The *mean* number of days over the preceding 7-day period that the household reportedly experienced seven types of food insecurity. (The 7 potential types include: cooked food they did not like, was not able to properly diversify food, had to reduce quantity per meal, had to reduce the number of meals, adults had to reduce amount of food eaten to give to children, had to borrow food or rely on friends and family, had nothing to eat).
Food Insecurity, 12 Month The number of months in the past 12 months the household reported experiencing not having enough food.

Long-Term Outcomes (Child Development)

Language Learning/Fine Motor Skills/Social Skills Normalized scores over the relevant sections of the Malagasy-adapted MDAT.
Composite Development The normalized aggregate score over all sections of the Malagasy-adapted MDAT.

Estimation Strategy

Given random assignment to treatment, we construct intent-to-treat estimates for treatment impacts from the following OLS specification:

$$y_{\mathrm{ic}} = \beta_0 + \beta_1 T_{\mathrm{ic}} + \beta_2 Pre_{\mathrm{ic}} + \mathbf{X}'\boldsymbol{beta} + \varepsilon_{\mathrm{ic}},$$

where i indexes households in cluster c. y_i are target outcomes, T_i is treatment assignment, $\boldsymbol{X'}$ is a vector of time-invariant demographic characteristics, and Pre_{ic} denotes outcomes at baseline, when available. When considering an individual-level outcome, as in the case of child development measures, i instead indexes individuals. We report results from three specifications: specification (I) with no additional controls; specification (II) with demographic controls including household size, gender of household head, age of household head, education of household head, distance of the household from the nearest school, weeks since the last payment and age of the youngest child, and finally a specification (III), which includes both demographic controls and controls for values at baseline where available. As explained earlier, we have a sub-sample of the population who were sampled at both baseline and midline; however, one of the key control variables (weeks since the last payment) is missing for a portion of the population, so the sample in specifications II and III is lower than the full sample. As a robustness check, we have run all analyses without the inclusion of this variable and see no major effect on the results. For these purposes, we will focus results from Specifications I and II when discussing results.

Table 1 Sample means and balance

	Cash only (mean)		Mother leaders		Mother leaders and affirmation		Mother leaders and planning	
	Baseline sample	Midline sample	Baseline sample	Midline sample	Baseline sample	Midline sample	Baseline sample	Midline sample
Gender of HH head (% male)	0.22	0.23	− 0.03	− 0.02	0.01	0.02	− 0.03	− 0.02
	(− 0.02)	(− 0.01)						
Age of HH head	45.12	45.04	− 0.11	− 0.53	0.27	− 0.11	0.02	− 0.11
	(0.47)	(0.38)						
Level of school attended by HH head	2.98	2.97	0.07	0.04	0.07	0.06	− 0.03	− 0.06
	(0.03)	(0.03)						
Distance to nearest school (km)	1.03	1.04	0.02	0.02	− 0.05	− 0.05	− 0.12	− 0.03
	(0.06)	(0.05)						
HH size	6.63	6.42	0.11	− 0.05	− 0.14	− 0.27***	− 0.21	− 0.14
	(0.09)	(0.07)						
Total food consumption in the last 30 days (*ariary*) at baseline	843,000	–	− 14,800	–	− 105,000*	–	− 50,800	–
	(21,347.97)							
Non-food consumption in the last 30 days (*ariary*) at baseline	197,000	–	− 1617.28	–	− 6865.85	–	− 16,500	–
	(6628.61)							
General Consumption in the last 30 days (*ariary*) at baseline	1,110,000	–	− 21,000	–	− 110,000	–	− 73,300*	–
	(25,691.30)							
Livestock Index at baseline	4.80	–	0.35	–	0.09	–	0.37	–
	(− 0.27)							
N	778	1204	771	1200	825	1205	815	1197

The coefficients for the Cash only represent the mean for the group. The figures for all treatment groups represent the added difference from control mean. Standard deviations are in parentheses. The last five variables are only reported for baseline because there is a chance the intervention may have impacted the value of those variables

*Denotes significance at $p \leq 0.1$; **at $p \leq 0.05$

Table 2 Sample means and balance for MDAT participants

	Cash only (mean)		Mother leaders		Mother leaders and affirmation		Mother leaders and planning	
	Baseline sample	Midline sample	Baseline sample	Midline sample	Baseline sample	Midline sample	Baseline sample	Midline sample
Male (%)	0.46	0.48	− 0.04	− 0.01	− 0.06*	− 0.02	0.00	0.01
	(− 0.03)	(− 0.02)						
Age	46.11	39.10	− 1.75	− 1.80	2.04	− 1.19	0.57	− 0.89
	(− 1.55)	(− 1.36)						
HH size	7.03	6.82	0.17	− 0.05	0.06	− 0.17	− 0.04	− 0.05
	(− 0.12)	(− 0.09)						
Distance to school (KM)	0.94	1.00	− 0.12	− 0.04	− 0.15	− 0.09	− 0.21*	− 0.09
	(− 0.07)	(− 0.06)						
School attended by HH head	0.70	0.64	− 0.03	− 0.04	0.03	− 0.02	0.01	− 0.03
	(− 0.02)	(− 0.02)						
N	447	695	440	677	461	680	491	705

The coefficients for the Cash only represent the mean for the group. The figures for all treatment groups represent the added difference from the control mean. Standard deviations are in parentheses

*Denotes significance at $p \leq 0.1$; **at $p \leq 0.05$

Table 3 Effects of enhanced treatments on behaviors

Treatment effects:	Parenting behavior		Interaction with children		Preparing diverse meals	
	(I)	(II)	(I)	(II)	(I)	(II)
Mother leaders	0.04	0.08*	0.10	0.17	0.13**	0.12**
	(0.04)	(0.04)	(0.14)	(0.12)	(0.03)	(0.04)
Mother leaders and affirmation	0.04	0.06	0.13	0.18	0.11**	0.11**
	(0.05)	(0.04)	(0.15)	(0.12)	(0.03)	(0.04)
Mother leaders and planning	0.09**	0.11**	0.32**	0.19	0.16**	0.15**
	(0.04)	(0.05)	(0.15)	(0.15)	(0.04)	(0.04)
Intercept	2.37**	2.34**	2.60**	1.24**	0.34**	0.47**
	(0.03)	(0.07)	(0.10)	(0.23)	(0.02)	(0.05)
N	4055	3521	4806	4061	4462	3801
R-squared	0.00	0.03	0.00	0.38	0.01	0.02

Standard errors in parentheses. Model (I) is a simple regression with clustered standard errors, model (II) adds demographic and explanatory controls including household size, gender of household head, age of household head, education of household head, age of youngest child, weeks since last payment, and distance of the household from the nearest school, and model (III) adds additional baseline controls

*Denotes significance at $p < 0.1$ level, **at $p < 0.05$

Given the number of hypotheses tested and the heterogeneity of constructs measured, even though we pre-specified the analyses in a published pre-analysis plan,[7] we also present and discuss sharpened *q*-values as described in Anderson (2008) in Supplement 4 as a sensitivity analysis. Standard errors are clustered at the level of the unit of randomization.

Balance Across Arms

Table 1 describes the sample and provides full balance statistics for both the full group sampled at midline as well as the smaller group sampled at both baseline and midline, while Table 2 provides balance statistics for the population sampled for the MDAT (test of socio-cognitive development) for both groups. Note generally balanced means across the evaluation groups, indicating that random assignment was successful.

Results: Behavioral Enhancements vs Cash Only

In this section, we describe the results of regressions that compare outcomes for households in each of the three behaviorally enhanced arms to the outcomes of households in the cash-only arm, with the treatment effect capturing this difference, which can be thought of as the 'additional effect' of adding the behavioral enhancements. We group indicators into three categories—behaviors, proximate outcomes, and long-term outcomes. While we find significant improvements of each

[7] Pre-analysis plan registered at the American Economic Association and available at https://www.socialscienceregistry.org/trials/957/history/6111.

of the behaviorally enhanced arms when compared to the 'cash only' arm, we do not find any significant differences between the behaviorally enhanced arms. While this paper does not intend to investigate the effects of cash alone, it is worth noting that the effects of the behaviorally enhanced arms when compared to the 'cash only' arm are more often significant and of higher magnitude than the effects of cash.

Behaviors

As the first two columns of Table 3 show, parents in two of the three behaviorally enhanced arms undertook a significantly larger number of the three targeted positive parenting practices than parents in the cash-only arm, with the difference being statistically significant both with and without demographic controls for the "Mother Leaders and Planning" arm. Parents in the behaviorally enhanced arms similarly undertook a larger number of the six kinds of parent–child interactions (read a book, told stories, sang songs, went for a walk, played with, taught to count or draw) over the preceding three months than did parents in the cash-only arm, with the difference being statistically significant for the "Mother Leaders and Planning" treatment arm. Finally, parents in all three behaviorally enhanced arms were significantly more likely to report having prepared diverse meals over the last twelve months than parents in the cash-only arm.

Overall, one or more of the behaviorally enhanced treatment arms led to greater adherence to desirable parenting- and nutrition-related behaviors than receiving cash alone did, although the size and significance of the treatment effect varies by arm and the specification used.

Proximate Outcomes

In Table 4, we see that there are no significant effects from any of the treatments (in any specification) on the fraction of households reporting that they ate diverse meals over the past 12 months. Food diversity—at least as we measure it here—does not appear to benefit from the adding on of behavioral enhancements. However, turning to other measures of nutrition and food security, we see that households in the "Mother Leaders and Affirmation" treatment arm report having prepared a significantly larger number of meals than those in the cash-only arm (treatment effects from the other enhanced arms are positive but not statistically significant in any specification).

Households in both the "Mother Leaders" arm and the "Mother Leaders and Affirmation" arm report experiencing significantly fewer dimensions of food insecurity over the past seven days, with the effect generally being the largest for those in the Cash + Mother Leaders arm. Finally, while point estimates all suggest that households in the three behaviorally enhanced arms all reported experiencing fewer months without enough food during the preceding twelve months than households in the cash-only arm, this effect is only statistically significant for households in the arm where the cash is supplemented by "Mother Leaders and Planning." Overall, self-reported food insecurity is significantly reduced when cash is supplemented by the Mother Leader groups and additional behavioral nudges.

Table 4 Effects of enhanced treatments on proximate outcomes

Treatment effects:	Food diversity			Number of meals prepared			Food insecurity, 7-day			Food insecurity, 12-month		
	(I)	(II)	(III)	(I)	(II)	(III)	(I)	(II)	(III)	(I)	(II)	(III)
Mother leaders	0.01	− 0.01	− 0.06	0.08	0.09	0.08	− 0.37**	− 0.40**	− 0.38**	− 0.25	− 0.28	− 0.20
	(0.09)	(0.09)	(0.10)	(0.06)	(0.06)	(0.05)	(0.13)	(0.13)	(0.15)	(0.19)	(0.19)	(0.20)
Mother leaders and affirmation	− 0.08	− 0.06	− 0.09	0.11*	0.11**	0.12**	− 0.25 *	− 0.26*	− 0.34**	− 0.11	− 0.20	− 0.26
	(0.09)	(0.09)	(0.10)	(0.06)	(0.06)	(0.05)	(0.14)	(0.14)	(0.15)	(0.19)	(0.19)	(0.20)
Mother leaders and planning	− 0.01	0.00	− 0.06	0.04	0.07	0.07	− 0.18	− 0.23	− 0.28*	− 0.28	− 0.38**	− 0.34*
	(0.10)	(0.10)	(0.11)	(0.06)	(0.06)	(0.06)	(0.14)	(0.14)	(0.15)	(0.18)	(0.19)	(0.20)
Intercept	8.32**	8.05**	7.63**	2.65**	2.86**	2.33**	1.15**	0.12	− 0.19	4.53**	3.05**	2.63**
	(0.07)	(0.12)	(0.20)	(0.04)	(0.07)	(0.10)	(0.10)	(0.17)	(0.21)	(0.14)	(0.31)	(0.34)
N	4806	4061	2762	4806	4061	2762	4652	3927	2483	4806	4061	2762
R-squared	0.00	0.01	0.02	0.01	0.05	0.10	0.01	0.05	0.08	0.00	0.02	0.03

Standard errors in parentheses. Model (I) is a simple regression with clustered standard errors, model (II) adds demographic and explanatory controls including household size, gender of household head, age of household head, education of household head, age of youngest child, weeks since last payment, and distance of the household from the nearest school, and model (III) adds additional baseline controls

*Denotes significance at $p < 0.1$ level, **at $p < 0.05$

Table 5 Effects of enhanced treatments on long-term outcomes

Treatment effects	Language learning			Fine motor skills			Social skills			Composite development		
	(I)	(II)	(III)	(I)	(II)	(III)	(I)	(II)	(III)	(I)	(II)	(III)
Mother leaders	0.14**	0.11*	0.10	0.01	− 0.01	− 0.03	0.14**	0.09	0.12	0.13*	0.09	0.08
	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.08)	(0.06)	(0.07)	(0.09)	(0.07)	(0.07)	(0.08)
Mother leaders and affirmation	0.10	0.10	0.09	− 0.05	− 0.03	− 0.04	0.09	0.11*	0.18**	0.07	0.08	0.09
	(0.07)	(0.07)	(0.08)	(0.07)	(0.07)	(0.08)	(0.06)	(0.06)	(0.08)	(0.07)	(0.07)	(0.08)
Mother leaders and planning	0.15**	0.12*	0.09	− 0.02	− 0.03	− 0.05	0.12*	0.09	0.11	0.11	0.09	0.05
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.07)	(0.07)	(0.09)	(0.07)	(0.07)	(0.08)
Intercept	− 0.06	0.06	0.24	0.04	− 0.06	0.03	− 0.04	0.06	− 0.01	− 0.04	0.04	0.15
	(0.04)	(0.13)	(0.17)	(0.05)	(0.13)	(0.17)	(0.04)	(0.11)	(0.16)	(0.04)	(0.12)	(0.16)
N	2757	2395	1402	2757	2395	1402	2757	2395	1402	2757	2395	1402
R-squared	0.00	0.01	0.07	0.00	0.01	0.05	0.00	0.01	0.04	0.00	0.01	0.08

Standard errors in parentheses. Model (I) is a simple regression with clustered standard errors, model (II) adds demographic and explanatory controls including household size, gender of household head, age of household head, education of household head, age of youngest child, weeks since last payment, and distance of the household from the nearest school, and age of the child (in months), and model (III) adds additional baseline controls to Model (II)

*Denotes significance at $p < 0.1$ level, **at $p < 0.05$

Long-Term Outcomes

As Table 5 shows, only the treatment arm where cash is augmented with the Mother Leaders groups shows statistically significant positive differences in overall child development as measured by the MDAT, as adapted to the Malagasy context. When we look at the component indices that make up the overall MDAT score, we see that children in several of the enhanced arms have significantly higher social skills than children in the cash-only arm. Similarly, children in the arms where cash was augmented by Mother Leader groups as well as the arm where this was further augmented by the Planning tool display significantly higher language learning than children in the cash-only group. The differences engendered by the addition of the "Affirmation" nudge are generally positive but not large enough to be statistically significant.

Finally, we see no statistically significant evidence of improved fine motor skills from any of the behaviorally enhanced arms.

Conclusion

Effectiveness

At midline, we see promising indications that behavioral enhancements can enhance the effectiveness of a cash transfer program. Results of cash transfer programs, counterintuitively, have been observed to take years to materialize (Evans et al. 2016), leading to the potential that estimated effects will be larger in the future. Nevertheless, a number of observations are already possible to make.

Taken together, the various enhanced arms lead to outcomes that are significant improvements on a variety of behaviors, proximate outcomes, and long-term outcomes related to child development than corresponding outcomes for cash alone. Seeing effects, albeit somewhat inconsistently, on measures of child development, is particularly striking given that the existing evidence on "cash plus" interventions principally finds effects on behaviors and proximate outcomes, and relatively little on longer-term outcomes (de Groot 2015).

Secondly, there are few significant pairwise differences between the MLs-only condition and either "nudge" variants, suggesting that MLs drive most of the improvements that are due to the enhanced treatments. Nevertheless, the arms augmented with behavioral 'nudges' do lead to significant effects over the cash-only condition in the case of several outcomes where the Mother Leader program by itself does not lead to a significant improvement in outcomes.

Limitations and Directions for Future Research

A limitation of this study is that the time between baseline and measurement is insufficient for the full effects of interventions—particularly those that relate to child development—to play out, while possibly overstating the importance of some of the

proximate behavioral variables where change may be quick but not durable. However, a key limitation we will not be able to address is the question of whether the 'nudges' have sufficient value *absent* the mother leaders intervention. This is a key question for future research, but one we are unable to address in the present study where it was not possible, due to operational constraints, to deliver the 'nudges' absent mother leader groups. In addition, in the absence of a full panel dataset, the specification that accounts for the most individual differences, including the value of outcomes at baseline, has a limited number of observations and thus lower power than the remainder of the specifications. Future research should also seek to unpack the mechanisms through which the Mother Leaders produce impact.

In closing, the positive findings from this study are in line with the growing body of research that behavioral designs layered on top of cash transfer programs can enhance impact at little additional cost. Given that the designs and assessments were customized to the Malagasy context, external validity using the specific designs may be limited. However, social protection programs in many countries target similar populations, and positive impacts of behavioral designs on other outcomes of cash transfer programs suggest that such designs are likely to be effective in other contexts after sufficient research to ensure they are relevant for the context. This is the first of such studies that focuses on the specific application of such interventions to parenting practices and ECD outcomes, and points to a yet untapped opportunity to integrate behavioral interventions into cash transfer programs with parenting elements to enhance outcomes from such programs.

Appendix

See Tables 6, 7, and 8.

Table 6 Effects of cash on behaviors

	Parenting behavior		Interaction with children			Preparing diverse meals	
	(I)	(II)	(I)	(II)	(III)	(I)	(II)
Treatment effect (cash)	0.19**	0.18**	0.33*	0.25*	0.33**	0.02	0.02
	(0.09)	(0.09)	(0.17)	(0.14)	(0.16)	(0.05)	(0.04)
Intercept	2.17**	2.31**	2.27**	0.84**	0.59	0.32**	0.51**
	(0.07)	(0.11)	(0.12)	(0.27)	(0.36)	(0.04)	(0.08)
N	1840	1840	2404	2404	1268	2163	2163
R-squared	0.01	0.03	0.00	0.38	0.39	0.00	0.01

Standard errors in parentheses. Model (I) is a simple regression with clustered standard errors, model (II) adds demographic and explanatory control including household size, gender of household head, age of household head, education of household head, age of youngest child, weeks since last payment, and distance of the household from the nearest school, and model (III) adds additional baseline controls

*Denotes significance at $p < 0.1$ level, **at $p < 0.05$

Table 7 Effects of cash on proximate outcomes

	Food diversity			Number of meals prepared			Food insecurity, 7-day			Food insecurity, 12-month		
	(I)	(II)	(III)	(I)	(II)	(III)	(I)	(II)	(III)	(I)	(II)	(III)
Treatment effect (cash)	0.30*	0.27*	0.32*	− 0.06	− 0.03	0.00	0.21	0.14	0.08	− 0.36	− 0.4	− 0.60**
	(0.15)	(0.15)	(0.17)	(0.10)	(0.10)	(0.07)	(0.24)	(0.21)	(0.23)	(0.31)	(0.29)	(0.29)
Intercept	8.03**	8.01**	6.99**	2.71**	2.72**	2.07**	0.94**	0.37	0.19	4.89**	3.79**	3.64**
	(0.11)	(0.20)	(0.34)	(0.07)	(0.11)	(0.14)	(0.14)	(0.31)	(0.39)	(0.20)	(0.39)	(0.50)
N	2404	2404	1268	2404	2404	1268	2343	2343	1155	2404	2404	1268
R-squared	0.02	0.04	0.06	0.00	0.05	0.09	0.01	0.05	0.08	0.00	0.02	0.04

Standard errors in parentheses. Model (I) is a simple regression with clustered standard errors, model (II) adds demographic and explanatory controls including household size, gender of household head, age of household head, education of household head, age of youngest child, weeks since last payment, and distance of the household from the nearest school, and model (III) adds additional baseline controls

*Denotes significance at $p < 0.1$ level, **at $p < 0.05$

Table 8 Effects of cash on long-term outcomes

	Language learning			Fine motor skills			Social skills			Composite development		
	(I)	(II)	(III)	(I)	(II)	(III)	(I)	(II)	(III)	(I)	(II)	(III)
Treatment effect (cash)	0.08	0.07	0.18**	0.14	0.11	0.09	0.14*	0.14*	0.03	0.14*	0.13*	0.14**
	(0.08)	(0.07)	(0.07)	(0.09)	(0.08)	(0.07)	(0.07)	(0.07)	(0.09)	(0.07)	(0.07)	(0.07)
Intercept	− 0.15**	− 0.2	− 0.09	− 0.1	− 0.25*	− 0.2	− 0.18**	− 0.31*	− 0.18	− 0.18**	− 0.32**	− 0.17
	(0.06)	(0.15)	(0.24)	(0.08)	(0.15)	(0.17)	(0.05)	(0.16)	(0.25)	(0.05)	(0.14)	(0.22)
N	1353	1353	624	1353	1353	625	1353	1353	625	1353	1353	624
R-squared	0.00	0.01	0.10	0.00	0.02	0.06	0.00	0.01	0.02	0.01	0.02	0.07

Standard errors in parentheses. Model (I) is a simple regression with clustered standard errors, model (II) adds demographic and explanatory controls including household size, gender of household head, age of household head, education of household head, age of youngest child, weeks since last payment, and distance of the household from the nearest school, and age of the child (in months), and model (III) adds additional baseline controls

*Denotes significance at $p < 0.1$ level, **at $p < 0.05$

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1057/s41287-023-00603-y.

Acknowledgements The trial was pre-registered at the American Economic Association's RCT Registry as RCT ID AEARCTR-0000957. We gratefully acknowledge helpful comments and suggestions from Alaka Holla, Jeffrey Tanner, and Benedicte Leroy De La Briere at the World Bank; Keetie Roelen at the Institute of Development Studies (IDS) at the University of Sussex; and Lawrence Aber at New York University. Participants at the World Bank's Social Protection and Jobs Seminar Series and IDS's seminar on "Putting Children First" where earlier versions of this paper were presented gave helpful feedback and suggestions. We thank Attila Cidam for his help with analysis. Any remaining errors are ours. This work does not necessarily reflect the views of the World Bank, its Board of Executive Directors, or the governments they represent.

Funding This work was supported by the Wellspring Philanthropic Fund and the Strategic Impact Evaluation Fund at the World Bank.

Data Availability The data underlying this article will be shared on reasonable request to the corresponding author.

Declarations

Conflict of interest On behalf of all authors, the corresponding author states that there is no conflict of interest.

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