Community-Based Cash Transfers in Fragile and Conflict-Affected Communities

Abstract

This project evaluates the impact of community-based cash transfers on household wellbeing in a conflict-affected and fragile setting. We aim to address the following questions: (1) Can Community-Based cash transfers improve the food security and nutritional outcomes of beneficiaries in conflict-affected settings? (ii) Can relatively modest community-based cash transfers improve subjective well-being, mental health, and stress in the face of recurrent conflicts? (iii) Do welfare impacts of community-based cash transfers vary by how they are targeted? (iv) Are community-based transfers more impactful for improving welfare outcomes in conflict affected communities? (v) What is the impact of community-based cash transfers on trust in local governance and social cohesion in the presence of conflicts? To address these questions, we design a cluster Randomized Control Trial (RCT) and randomly assign communities into control group and community-based cash transfers involving different targeting approaches.

Introduction

Proliferation of armed conflicts in Africa and other low- and middle-income countries in recent years has increased the demand for humanitarian and social assistance. Besides the rising need for humanitarian assistance, armed conflicts can also significantly limit the reach, breadth, and impact of humanitarian and social assistance programs. The targeting, delivery and access to humanitarian and social assistance programs could be adversely affected by armed conflicts, which can ultimately impact the efficacy of these programs (e.g., Ghorpade, 2017, 2020; Lind et al., 2022). Because of these limitations, there exist important knowledge gaps on: (i) whether and to what extent program impacts vary by the design features of social assistance programs in conflict-settings, and (ii) which targeting approaches are most effective and impactful in these contexts.

A number of studies have documented the positive effects of conditional and unconditional cash transfers in stable settings (e.g., Gertler, 2004; De Janvry et al., 2006; Adato and Hoddinott, 2010; Baird et al., 2011; Galiani and McEwan, 2013; Haushofer and Shapiro, 2016; Aizer et al.,

2016; Parker and Todd, 2017; Evans and Popova, 2017; Millán et al., 2019; Egger et al., 2022; Filmer et al., 2023; Abay et al., 2023). Despite the growing recent evidence on the effect of cash transfers on physical, mental and emotional well-being in stable contexts (e.g., Haushofer et al., 2020; McGuire et al., 2022; Dwyer and Dunn, 2022), there is lack of rigorous empirical evidence on the potential of cash transfers and related interventions to address the adverse consequences of armed conflicts on conflict-affected households and communities. This is particularly important given the widespread adverse effects of violent conflicts on physical, economic, social, mental and emotional well-being of communities (e.g., Kondylis, 2010; Akresh et al., 2011; Shemyakina et al., 2013; Akbulut-Yuksel, 2014; Coupe and Obrizan, 2016; Cheung et al., 2020). Furthermore, the evidence on the impact of cash transfers on some welfare outcomes is mixed, with some studies showing positive direct impacts of cash transfers on nutritional outcomes (Ecker et al., 2023; Kurdi, 2020), food consumption and food security (Bastagli etl al., 2016; Haushofer et al., 2016), and positive consumption spillover effects on non-beneficiaries (Egger et al., 2022) while others find negative impacts of cash transfers in particular and aid in general on social cohesion and related outcomes (Bobonis et al 2013; Crost et al., 2014; Khanna and Zimmerman, 2014; Premand and Rohner, 2023), transition from formal employment (Bergolo and Garvan, 2018) and negative spillover effects on nutritional outcomes of ineligible children (Filmer et al., 2023).

While cash transfers hold significant potential to improve social cohesion by reducing inequalities and promoting inclusion of vulnerable groups (e.g., Idris, 2017; Crost et al., 2016; Valli et al., 2019), these programs can also create social tensions, especially if they are not properly targeted, as is often the case in fragile settings where targeting is prone to corruption as well as significant exclusion and inclusion errors (e.g., Idris, 2017; Della Guardia et al., 2022; Premand and Rohner, 2023). Furthermore, while previous studies from stable settings show nuanced and heterogenous impacts of cash transfers on women's agency and gendered outcomes (e.g., Hidrobo et al., 2013; 2016; UN Women, 2019; Ambler and de Brauw, 2023), their impacts in conflict setting remains unclear. Particularly, the intra-household implications of cash transfers in conflict-affected communities are understudied. Armed conflicts and associated actors thereof may significantly moderate the impact of well-intended social assistance programs.

The effectiveness of cash transfer programs in fragile and conflict-affected settings depends on a range of factors including the nature of the conflicts, the actors involved and the design of the cash transfer program, such as whether they are government-led or managed by other

entities (e.g., Rohner and Thoenig, 2021; Premand and Rohner, 2023). The targeting of cash transfers is especially important in fragile and conflict-affected settings. If the targeting of cash transfers and related interventions are not designed in ways that can prevent and reduce violence and tension, doing more harm to communal social cohesion (Idris, 2017) or, at best, leading to heterogeneous effects across different contexts. For example, Galiani and McEwan (2013) show that the heterogeneous impact of a conditional cash transfer in Honduras on school enrollment may be explained by differential targeting of poor and non-poor households to the program. Similarly, Premand and Schnitzer (2021) report that Proxy Means Targeting (PMT) yielded marginally higher impacts of a national cash transfer program in Niger than community-based targeting (CBT) and that the former is perceived more legitimate among non-beneficiaries than CBT, despite the typical lack of transparency issues associated with PMT based targeting (Hanna and Olken, 2018).

Another important factor is the objective of the targeting – whether it aims to maximize "impact" or reach to the most "deprived". For example, Haushofer et al. (2022) enquire whether social protection and other development interventions should target based on deprivation or expected impact and document substantial trade-offs between targeting for deprivation versus for impact. Though social transfer programs are typically targeted for deprivation, more recent work have employed impact-based targeting for a variety of intervention including subsidies for anti-malarial bednets (Bhattacharya and Dupas, 2012), a public works program (Bertrand et al., 2021) and job training (Caria et al., 2021). This challenge is, however, complex particularly in conflict-affected settings and remains less explored.

Moreover, while cash transfer programs have varied in their conditionalities, design and transfer size, how the size of transfers affect the effectives of programs is surprisingly underexplored, though there is an increasing recognition that it is one of the reasons for failure of certain programs (Aizer et al. 2016). A handful of studies exploit experimental and non-experimental variations in transfer sizes to study impacts on a range of outcomes, with a mixed result. For example, De Janvry and Sadoulet (2006) exploit the cap in the total amounts to households from the Progresa program in Mexico to study the marginal effect of transfers and find a large positive impact on dropout rate. Filmer and Schady (2011), on the other hand, use eligibility thresholds of a conditional cash transfer program that granted scholarship to students in Cambodia based on predicted probability of dropout and find a sharply diminishing marginal response to transfer size. Haushofer et al. (2016) find results suggestive of diminishing returns to transfer size

in Kenya form a randomized experiment that transferred large cash grants to beneficiaries. A more recent work by Jaroszewicz et al. (2022) in the US finds no differential impact of a large cash transfer (four times the small transfer amount). Besides the paucity of evidence on marginal impacts of cash transfers as well as the mixed findings of the few existing studies, little is known on how variations in transfer amounts function in fragile and conflict-affected settings.

To sum up, empirical evidence on whether and what type of targeting methods are more feasible and effective in fragile settings remains scarce. Similarly, what type and under what conditions alternative targeting and delivering mechanisms reduce or exacerbate social cohesion is not clear. This project aims to fill these important knowledge gaps on the impact of cash transfers on several understudied aspects and outcomes in conflict-affected settings and the implication of alternative targeting methods on households' well-being and social cohesion within communities.

Interventions

While the effectiveness of cash transfers has been widely discussed in the literature, in conflict settings, the choice of targeting methods brings additional challenges to the effectiveness of cash transfers. More specifically, the effectiveness of cash transfers may vary depending on how they are targeted. Over the last decade and half, with support from development partners, Ethiopia has implemented one of largest social assistance programs in Africa - known as the Productive Safety Net Programme (PSNP). The program reaches about 8 million rural people living in food insecure communities in the country. During much of this period, Ethiopia was characterized by relative stability and positive socioeconomic changes. However, it has recently plunged into political unrest and recurrent conflicts, leaving millions in dire emergency and social assistance needs. This surge in the numbers of people in need of assistance, coupled with resource constraints faced by international aid agencies like the WFP and other development partners has necessitated a rethinking of existing targeting approaches, including community-based and other data-driven approaches such as Proxy Means Testing (PMT). The existing literature does not offer clear guidance as to which targeting methods are suitable, cost-effective and impactful in conflict-affected settings where data limitations may affect the feasibility of some targeting approaches. Moreover, it is unclear whether and to what extent household level impacts of transfers are sensitive to the choice of targeting approaches. Our study aims to contribute to the broader discourse on impacts of alternative targeting strategies in conflict-affected and fragile settings.

We work with communities and community leaders in 180 Enumeration Areas (EAs) or villages across Ethiopia. An EA typically comprises 150 to 200 households within a *Kebele*, the lowest administration unit in Ethiopia. We build on a household survey conducted in 2019 comprised of a random sample of 20 households from each of the 180 sampled EAs. The community survey brought together six individuals composed of key *Kebele* leaders, including the *Kebele* chairman and others knowledgeable about the EA, such as community leaders, elders, priests, and teachers. Mimicking the actual targeting practices in Ethiopia and beyond, village leaders were assigned to use of one of different targeting approaches to select beneficiary households from their respective EAs and allocate cash to each beneficiary according to chosen targeting approach. Thus, the intervention in this study occurs at two levels: at the community leaders and at the community members or household levels. In this study, we focus on the household level impacts of these community-level interventions.

The Intervention: allocation of real versus hypothetical cash transfers by community leaders

By way of mimicking traditional community-based targeting, we ask community leaders to rank households in their respective EAs *from the most to the least needy* based on their needs assessment for social assistance. We provide community leaders with a lump-sum of cash that will be transferred to their respective community members (20 households in each EA) ranked based on the targeting approach to which each community is randomly assigned – different budget categories and discretion levels built into the experimental design (see Experimental Design section). That is, we exogenously vary the nature of the transfers (hypothetical vs real), the amount of money available for transfers (low vs high), and the level of discretion given to community leaders (rule-based versus discretionary). These would allow us to study the impact of different levels of transfers made through limited versus relaxed targeting discretion given to community leaders. Specifically, we aim to answer the following research questions.

Research Questions

This research project aims to evaluate the impact of cash transfers disbursed through alternative designs to CBT on the breadth and depth of social assistance transfers on several household level outcomes, including subjective wellbeing, mental health, and perceived stress level and

depression, short-term food insecurity, and dietary diversity. Specifically, we focus on the following key research questions:

1. Do Community-Based cash transfers improve short-term food security, consumption of nutritious diets and dietary diversity?
2. Do Community-based cash transfers improve subjective wellbeing of beneficiaries in conflict-affected settings?
3. Can relatively small community-based cash transfers improve mental health and stress in the face of recurrent conflicts? And to what extent does the size of transfer matter?
4. Do welfare impacts of community-based cash transfers vary by the nature of targeting approaches (e.g., mandated or discretionary)
5. Are community-based transfers more impactful in conflict affected communities?
6. What is the impact of community-based cash transfers on trust in local governance and social cohesion in the presence and absence of conflicts?
7. Does the impact of community-based cash transfers vary by gender, conflict exposure, and variation in the size of transfer?

Primary outcomes

1. Household Dietary Diversity Score (HDDS)
2. Women's dietary diversity score (WDDS)
3. Consumption of nutritious diets
4. Per capita consumption expenditure
5. Household Food Insecurity Experience Scale (FIES)
6. Subjective well-being
7. Perceived stress level
8. Depression scale

Primary outcomes explanation

1. Household Dietary Diversity Score, which will be constructed based on 7-day recall following the guidelines by Food and Agriculture Organization (FAO). The HDDS is a qualitative measure of household-level food security and hence reflects the economic ability

of the household to access a variety of foods (FAO, 2023). Previous studies have shown that an increase in dietary diversity is strongly associated with household food security (e.g., Hoddinott and Yohannes, 2002; Hatloy et al., 2000). Respondents are first asked if household members have consumed one or more of the food groups over the preceding week. Then, the items are re-categorized into 12 groups to arrive at a score which consists of a simple count of food groups consumed, ranging from 0 (no consumption of any group) to 12 (consumption of all groups).

2. The women's dietary diversity score (WDDS) comes from a report of foods consumed in the last 24-hours. The WDDS captures the number of food groups consumed in the previous day by women of reproductive age. The food groups in reference are grouped into 10 groups. The 10 food groups include: (1) grains, roots, and tubers; (2) legumes and beans; (3) nuts and seeds; (4) dairy products; (5) eggs; (6) flesh foods, including organ meat and miscellaneous small animal protein; (7) vitamin A-rich dark green leafy vegetables; (8) other vitamin A-rich vegetables and fruits; (9) other fruits; and (10) other vegetables. The value of DDS ranges from 0 to 10.
3. Consumption of nutritious diets, which is constructed from the set of questions on weather women of reproductive age consumed high-value (nutritious) foods in the 24 hours preceding the survey. For each of the high value consumption groups comprising dairy products, eggs, and meats, the outcome variable is constructed as a dummy indicator that takes value 1 if a woman consumes a high value item (e.g., eggs), and 0 otherwise. Because these measures are related to the WDDS, they will be analyzed together.
4. Consumption expenditure is measured and elicited using household consumption expenditure module, which collects information on households' consumption of various food and non-food items. Household consumption expenditure is widely used as a proxy for household well-being and incomes, based on the assumption that a household's consumption and income are strongly correlated.
5. Food Insecurity Experience Scale (FIES) is a self-reported metric which captures households' access to adequate food and associated difficulties due to financial or other resource constraints. The FIES is an experience-based food insecurity metric developed by the FAO of the United Nations and is widely applied to measure perception and prevalence of food insecurity (FAO, 2014; FAO, 2020). The FIES builds on an eight-question module

related to respondents' experiences and associated difficulties to access sufficient and nutritious food in the last 30 days. The aggregate FIES is constructed by summing the responses to the eight questions. Its value ranges from zero to eight, zero standing for those households reporting no experience of food insecurity across all eight dimensions of food insecurity. Based on the various indicators and questions used to measure Food Insecurity Experience Scale (FIES), we also aim to generate an indicator variable assuming a value of 1 if the household experiences one or more types of food insecurity and 0 otherwise.

6. Subjective Well-being: this is measured using an ordered indicator of overall life satisfaction. This scale ranges from 1 ("completely dissatisfied") to 10 ("completely satisfied").
7. Perceived stress level. We use two measures of perceived stress level. One that captures the overall perceived stress of respondents related to everything in their life, like work, family, health, and so on. This is a scale ranging from 1 (not stressed at all) to 10 (extremely stressed). A second and more comprehensive stress measure is constructed from 10 questions in the standard stress assessment instrument Perceived Stress Scale (PSS). Respondents are asked 10 questions on their feelings and thoughts over the last month.
8. Depression scale. We measure depression scale using responses to the Patient Health Questionnaire (PHQ-9). The PHQ-9 consists of 9 questions that ask respondents how often they have been bothered by a case-finding problem item (e.g., little interest or pleasure in doing things) in the past two weeks. Each item is scored on as 0 – not at all, 1 – several days, 2 – more than half days, and 3 – nearly every day. These scores are then summed to generate a depression scale.

Secondary outcomes

1. Trust in local governance, trust in community leaders and other stakeholders in the village
2. Social cohesion among households

Experimental design

The intervention follows community level clustered randomization in which the 180 communities are randomly assigned into control group or one of three treatment arms. The treatment assignment is based on (i) whether communities receive actual transfer or hypothetical (control); (ii) the size

of the transfer pool available to community leaders to be distributed among households within the community (constrained budget involving 10,000 Birr versus relaxed budget of 20,000 Birr); and (iii) the level of discretion given to community leaders (rule-based or discretionary). Effectively, the first stage randomization involves assigning the 180 communities into a control and cash transfer group. We assign about 30 percent of the communities to control group and the remaining 70 percent into treatment group. We further split the treatment group communities (enumeration areas) into one of three treatment arms: Relaxed budget with rule-based approach for distributing resources (i.e., ETB 20,000), relaxed budget with some discretion on the criteria to distributing cash transfer (i.e., ETB 20,000); constrained budget with rule-based approach for targeting (i.e., ETB 10,000). Figure 1 shows the random assignment and associated control and treatment groups.

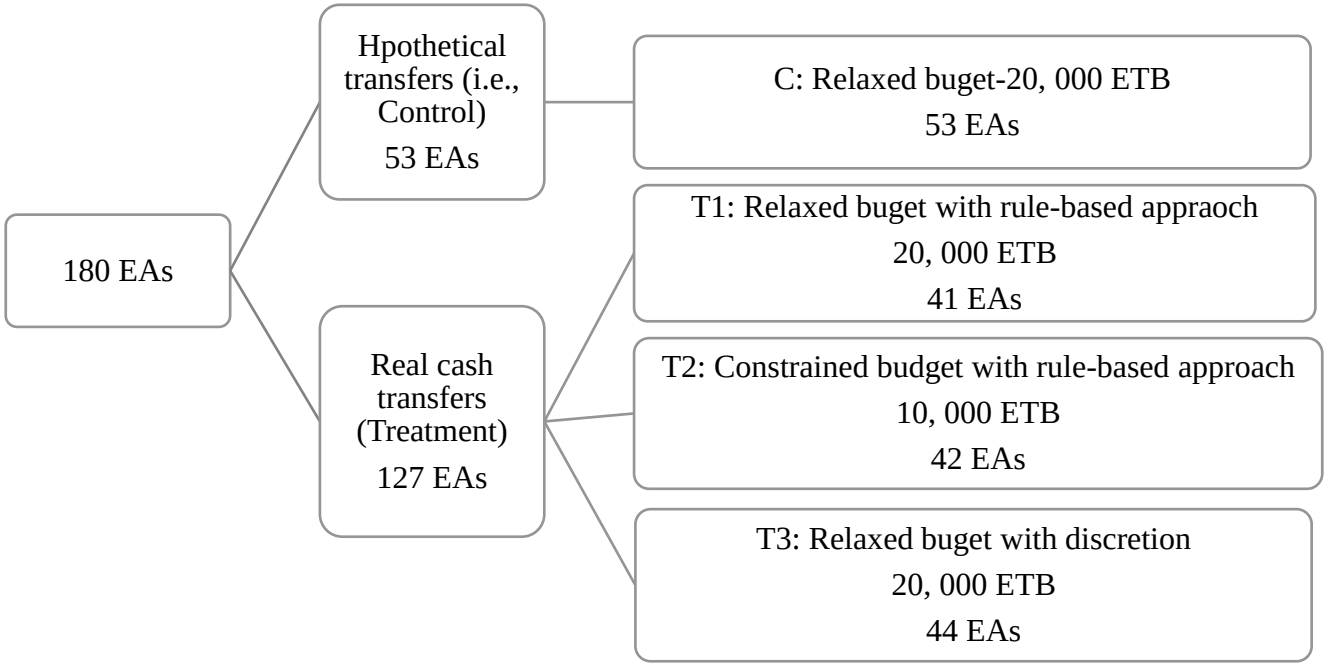

Figure 1. Random assignment of communities across treatment and control arms

(1) ***Control: Rule-based targeting using hypothetical transfer of 20,000 Birr (C)***: This group serves as a control cluster where community leaders are not given any actual funds but are instructed to act as if they have a hypothetical budget of 20,000 Birr to distribute among households in their community. Community leaders are first asked to rank households based on their need for social assistance. They are then asked to allocate this notional budget among the 20 households included in our sample. During this ranking process, leaders are required to strictly adhere to pre-defined rules provided by the research team. These rules are carefully selected to

mimic the targeting criteria used in actual social assistance programs in Ethiopia. More specifically, community leaders are asked to prioritize those households who: (i) had difficulty satisfying their food needs; (ii) own no or little asset (e.g., livestock, land); (iii) have limited income-generating activities or capacity; (iv) have lost productive assets due to shocks (e.g., conflict, drought); and (v) have lost family members recently. Effectively, for the current purpose of the analysis, this group receives no actual transfer and hence serves as our control group.

(2) ***Rule-based targeting with relaxed budget (T1)***: Another group of communities are randomly assigned to a cluster that receives real transfer funds with a budget of 20,000 Birr (about 360 USD). In this cluster (i) Community leaders are required to rank households based on five pre-determined targeting criteria and allocate the transfers. These criteria are similar to those in the control group and mimic the targeting criteria used by the national safety net program in Ethiopia (e.g., Gilligan et al., 2009; Hoddinott et al., 2012; Abay et al., 2022).

(3) ***Rule-based incentivized targeting with constrained budget (T2)***: This group of communities follows similar rules as those in control group, but they receive a constrained budget of 10,000 Birr (about 180 USD). Community leaders are required to rank households based on the five criteria outlined above and allocate the 10, 000 Birr to the community members in our sample. These criteria are designed to mimic the targeting criteria used by the national safety net program in Ethiopia, the PSNP. This treatment arm serves to test the implication of budget constraint.

(4) ***Discretionary targeting (T3)***: The fourth group of communities are provided a budget of 20,000 Birr to distribute as social assistance to households identified as in need. Here, community leaders rank households based on their own criteria they collectively agree upon. The establishment of these ranking criteria is entirely left to the discretion of the community leaders. It is up to the leaders to determine who among the ranked households gets how much of the 20,000 Birr transfer assigned to the community.

Randomization Method

The randomization was done at the village level using the baseline list of villages. We initially selected those villages that are accessible for a survey, and we randomly assigned these villages

into four groups. A reserve list was also prepared in case some of the villages are not accessible because of conflict.

Randomization Unit

Village or community level

Was the treatment clustered:

Yes

Planned number of observations:

180 communities and about 3,060 households

Number of clusters by treatment arm:

C (Control): 53 villages

T2 (Rule-based, 20, 000 ETB): 41 villages

T3 (Rule-based, 10, 000 ETB): 42 villages

T4 (Discretionary, 20, 000 ETB): 44 villages

Statistical Power: Minimum Detectable Effect Size for Primary Outcomes

We compute the number of clusters needed for the primary outcomes described above, assuming that there are a known and fixed number of households in each cluster (village). The baseline sample includes an average of 20 households in each village. We assume an attrition rate of 15 percent and hence we expect to revisit an average of 17 households per village. Our power calculations aim to achieve 80 percent power at a significance level of 5 percent. Power calculations are performed only for the primary outcomes described above. Given that we have primary outcomes, we computed the number of clusters and associated sample size needed for each primary outcome separately, and then selected the maximum sample needed to detect impacts across these outcomes.

We focus on quantifying the impact of the community-based cash transfers on short-term welfare outcomes. Our outcomes of interest include: households' dietary diversity score (HDDS), women's dietary diversity score (WDDS), consumption of nutritious foods, food security,

consumption expenditure, subjective well-being, and perceived stress level. We compiled mean and standard deviation of these primary outcomes as well as minimum detectable effects (MDEs) for each outcome using the baseline sample as well as other external data and evidence from previous studies. The mean HDDS in our baseline stands at a very low level of 4.5 food groups. Previous studies that evaluated comparable cash transfers programs report average impacts ranging from 0.12 to 0.33 or a 6-12 percent increase (Hidrobo et al., 2014; Savy et al., 2020; Leight et al., 2023). In our study, an assumed 11 percent increase in HDDS requires 51 control clusters and 120 treatment communities. As we are comparing the hypothetical arm, where households receive no actual transfers, with the rest of the treatment arms which involve actual transfers to households, this comparison is sufficiently powered since we can combine the three treatment arms with actual transfers. The 180 clusters (53 control and 127 treatment group) allow us to detect the assumed MDEs for 6 of the 7 primary outcome variables listed in Table 1. Table 1 summarizes our power calculations involving several primary outcomes meant to test alternative hypotheses.

Table 1: Household-level primary outcomes and associated sample size

	HDDS	WDDS	Woman consumes nutritious food	Food secure	log (daily) Per capita consumption	Subjective well-being	Stress level
Baseline mean	4.5	1.8	0.7	0.45	3.5	3.9	5.3
Baseline SD	2.12	1.2	0.46	0.5	0.78	2.2	2.7
Adjusted SD	1.78	1.08	0.42	0.42	0.70	1.94	2.59
ICC	0.25	0.22	0.16	0.2	0.2	0.21	0.21
MDE	11%	12%	10%	11 pp	16%	12%	11%
Attrition	15%	15%	15%	15%	15%	15%	15%
Size of cluster (adjusted after attrition)	17	17	17	17	17	17	17
Control clusters	51	76	44	53	54	52	50
Treatment clusters	120	171	99	120	122	113	113
Total clusters needed	171	247	143	173	176	165	163
Adjustment factor*	0.84	0.9	0.91	0.86	0.9	0.88	0.96

*The adjustment factor accounts for the reduction in the variance of the outcome variable that arises from controlling for stratifying and related geographic controls. We follow McKenzie's guidance in the following blog to compute this adjustment factor. https://blogs.worldbank.org/impactevaluations/six-questions-about-doing-power-calculations

Empirical Estimation Strategy

The community-based cash transfers we introduced in this project entails transfer of significant resources to households. For example, the 20,000 Birr assigned to communities amounts to about 360 USD. Although we do not yet know the average transfer that goes to beneficiary households, these transfers are sizeable, especially relative to monthly per capita consumption in our baseline sample (see Table 1) as well as compared to the value of conventional PSNP transfers in Ethiopia. If, for instance, community leaders distribute the 20,000 ETB equally between the 17 households in the community, the average transfer to each household is above the monthly PSNP transfer usually reported by several studies (e.g., Gilligan et al., 2009; Hoddinott et al., 2012; Berhane et al., 2014; Abay et al., 2022). The average transfer going to relatively poorer households is likely to be higher than this. Thus, we anticipate that these transfers can improve short-term welfare outcomes, especially for poorer households, who may have higher chance of receiving relatively larger transfers and for whom these transfers may represent significant share of their consumption expenditure.

We have two types of household-level outcomes that are of interest: those with baseline information and those without. However, the baseline was conducted four years ago, implying that the temporal autocorrelation in welfare outcomes can be low. In the presence of low temporal autocorrelations, McKenzie (2012) demonstrates that an analysis of covariance (ANCOVA) offers more efficiency and power than standard differences-in-differences. Thus, we start by examining the impact of being assigned into either the control or the treatment arms involving actual cash transfer and then disaggregate these treatment arms using the following simple specifications:

$$W_{hct=1} = \gamma_0 W_{hct=0} + \gamma_1 T_{hc} + \gamma_2 HH_{hct=0} + \mu_{hc} \quad (1)$$

$$W_{hct=1} = \theta_0 W_{hct=0} + \theta_1 T1_{hc} + \theta_2 T2_{hc} + \theta_3 T3_{hc} + \theta_4 HH_{hct=0} + \theta_5 CM_{ct=0} + \mu_{hc} \quad (2)$$

Where $W_{hct=1}$ stands for welfare indicators such as dietary diversity score, subjective wellbeing and perceived stress and depression. $W_{hct=0}$ captures baseline values of corresponding outcomes, in our case values at the 2019 round. T_{hc} stands for a binary indicator assuming a value of 1 for those communities and households assigned to receive an actual transfer and 0 for those communities assigned to the hypothetical arm and hence control group. The rest of the terms are as defined before. $T1_{hc}$, $T2_{hc}$, and $T3_{hc}$ stand for indicator variables for those communities and

households assigned to receive relaxed budget cash transfer with rule-based, those receiving with constrained budget and with additional discretion in targeting, respectively. Note that those communities and households not receiving an actual transfer (which were assigned to the hypothetical ranking/ distribution) serve as control group and are the base outcome. As communities and households were randomly assigned into the treatment arms, γ_1 captures the impact of this assignment on household welfare while $\theta_1 - \theta_3$ identify the impact of assignment to the three treatment arms associated with each variant of the community-based targeting approach.

Comparing the size of the parameters $\theta_1, \theta_2 \; and \; \theta_3$ helps to test several important hypotheses related to role of targeting methods and size of transfers in moderating the impact of cash transfers. Previous studies show some heterogeneous impacts of conditional and unconditional cash transfers to the efficiency and effectiveness of alternative targeting methods (e.g., Galiani and McEwan, 2013; Premand and Schnitzer, 2021; Haushofer et al., 2022). For example, comparing the size of $\theta_1 \; and \; \theta_2$ allows to understand the implication of budget constraint and hence the size of transfer on welfare outcomes. Similarly, comparing the size of $\theta_1 \; and \; \theta_3$ helps to understand the implication of discretionary versus rule-based targeting approaches. For example, if discretionary or rule-based targeting approach facilitate the identification of the most deprived or those households for whom the expected impact is higher, then we expect differential impacts driven by the different targeting approach even if the budget remains the same.

However, we note that not every household assigned to the treatment arms involving actual cash transfers will receive transfer. In that sense, the empirical specification in equation (1)-(2) and associated estimates (γ_1 and $\theta_1 - \theta_3$) cannot be interpreted as average treatment effects, rather as intention to treat (ITT). To quantify the impact of the actual transfers, we estimate 2SLS equation by using the exogenous assignment into the treatment arms as an instrument for whether a household receives a transfer as well as the actual transfer households receive. The impacts of the transfers are likely to vary across households and communities. For example, the cash transfers may be more useful and effective for poorer households or those facing economic shocks, including those triggered by conflict. We probe these heterogeneities by extending the empirical specification in equation (1) using interaction terms and sample splits. For example, we hypothesize that the community-based cash transfers may be more impactful for those

communities affected by recent conflicts. To explore this hypothesis, we will interact the treatment assignment with community-level exposure to conflict (an information coming from the ACLED project).

$$W_{hct=1} = \beta_0 W_{hct=0} + \beta_1 T_{hc} + \beta_2 Conflict_{hc} + \beta_3 T_{hc} * Conflict_{hc} + \beta_4 HH_{hct=0} + \varepsilon_{hc} \quad (3)$$

Where all terms except $Conflict_{hc}$ are as defined above. $Conflict_{hc}$ stands for a measure of community-level exposure to conflict, which we aim to compile from the ACLED database and community-level survey. The ACLED is a widely used database to study the consequences of conflicts in different settings and records event-based information for different types of conflicts, including battles, attacks against civilians, remote violence, and protests and riots. Some of these conflict events have intensified recently. For example, more than half of the incidents recorded by ACLED during August 2019-May 2021 represent battles (Abay et al., 202). Thus, we aim to estimate equation (2) using different types of conflicts.

We also aim to explore whether the community-based cash transfers are more impactful for poorer households. For this purpose, we use pre-intervention consumption-based poverty measure and directly test this hypothesis by interacting the treatment indicators specified in equation (3) as follows:

$$W_{hc} = \delta_0 W_{hct=0} + \delta_1 T_{hc} + \delta_2 Poor_{hct=0} + \delta_3 T_{hc} * Poor_{hct=0} + \delta_4 HH_{hc} + \epsilon_{hc} \quad (4)$$

where all terms except $Poor_{hct=0}$ are as defined before. $Poor_{hc}$ is an indicator variable assuming a value of 1 for those households whose per capita consumption (in the baseline, 2019 round) falls below the national poverty line. The coefficient associated with the interaction terms between access to cash transfers and poverty status (δ_3) allows us to test whether the community-based cash transfers are particularly impactful for poor households. We also aim to expand the empirical specification in equation (1) by disaggregating the treatment group into the three arms involving different targeting method and size of transfers. This can serve to test if either of the targeting approaches are pro-poor and transfers a large share of the budget to poor households, which may lead to higher impact of the cash transfers on poor households. For some outcomes (e.g., perceived stress level, depression symptoms), we collected information from the primary male and female respondent in the household. Thus, we also estimate differential impacts of these community-based

transfers across genders. This entails similar empirical specification as equation (4), but the variable that is being interacted becomes the gender of the respondent.

Finally, we are interested in evaluating whether the alternative variants of community-based targeting, especially those involving actual cash transfers, can affect social cohesion among members of community. We measure this by eliciting respondents' trust on community leaders, government officials, neighbours and local institutions such as health and agricultural extension centres, banks and Non-Governmental Organizations (NGO). We elicit these measures after we inform households about the community-based targeting we conduct. We also asked respondents about their preferences for future targeting for social assistance programs and we expect to detect some differences across those households who may have felt unhappy and dissatisfied by the targeting process in their community. We anticipate that the temporal autocorrelation in these outcomes is likely to be strong and hence standard difference-in-difference may be more powered than ANCOVA. Thus, besides the ANCOVA specifications spelled out above, we also aim to estimate the following standard difference-in-difference fixed effects equation:

$$Y_{hct} = \alpha_h + \alpha_0 Round_t + \alpha_1 T_{hct} + \alpha_2 HH_{hct} + \omega_{hct} \quad (5)$$

$$Y_{hct} = \alpha_h + \varphi_0 Round_t + \varphi_1 T1_{hct} + \varphi_2 T2_{hct} + \varphi_3 T3_{hct} + \varphi_4 HH_{hct} + \vartheta_{hct} \quad (6)$$

Where Y_{hct} stands for our measure of social cohesion, captured using trust in different entities. α_h stands for household fixed effect that captures all time-invariant differences across households, and $Round_t$ is a dummy variable that takes a value of 0 for baseline and 1 for midline. Note that we define $T_{hct}, T1_{hct}, T2_{hct}\ and\ T3_{hct}$ as time-varying indicators and hence assume a value of 0 for all households in the baseline while assuming a value of 1 only for those households assigned to the treatment arms in the midline survey. Our difference-in-difference (ITT) estimates are $\alpha_1, \varphi_1, \varphi_2, and\ \varphi_3,$ which capture the impact of the random assignment of communities (and households) to the community-based cash transfers and the different targeting approaches. The impact of cash transfers on social cohesion remains contested (Premand and Rohner, 2023; Crost et al., 2014). Although some studies show positive impacts (e.g., Idris, 2017; Crost et al., 2016; Valli et al., 2019), they can also create social tensions, especially if they are not properly targeted (e.g., Della Guardia et al., 2022; Premand and Rohner, 2023). If cash transfers improve or erode social cohesion, we expect α_1 to be statistically significant (e.g., Alik-Lagrange et al., 2021; Evans et al., 2019). Similarly, if alternative targeting of social assistance triggers differential implications

on trust and social cohesion, we expect $\varphi_1, \varphi_2, and\ \varphi_3$ to be statistically significant. The impacts of the community-based cash transfers may be moderated by underlying social structures, existing inequalities and fragilities within communities. Thus, we also aim to extend the empirical specification in equation (5)-(6) by adding interaction terms, including with exposure to conflict and existing inequalities.

Households living in the same community are likely to face similar treatment as well as similar shocks, markets and food security environment, which can generate spatial correlation of unobserved effects (error terms) across households from the same community. Thus, standard errors will be clustered at the village level, which is the level of treatment in our case and hence the usually recommended level of clustering for standard errors (Abadie et al., 2023).

Theory of Change

As the community-based targeting exercise described above entails transfer of some resource to beneficiary households, we anticipate significant impacts on household welfare, especially on short-term welfare indicators. As described above, these transfers are sizeable, especially relative to monthly per capita consumption in our baseline sample as well as compared to the value of conventional social assistance transfers in Ethiopia. The community-based cash transfers were offered about 3-4 months before the household survey. Thus, we expect that households can use these transfers to satisfy their consumption and related needs. This is ultimately expected to improve beneficiary households' welfare.

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