IFPRI Discussion Paper 02289

October 2024

Targeting Social Assistance in Fragile Settings

An Experiment on Community-Based Targeting

Kibrom A. Abay

Guush Berhane

Daniel O. Gilligan

Kibrom Tafere

Alemayehu Seyoum Taffesse

Development Strategies and Governance Unit
Innovation Policy and Scaling Unit
Poverty, Gender, and Inclusion Unit

INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE

The International Food Policy Research Institute (IFPRI), a CGIAR Research Center established in 1975, provides research-based policy solutions to sustainably reduce poverty and end hunger and malnutrition. IFPRI's strategic research aims to foster a climate-resilient and sustainable food supply; promote healthy diets and nutrition for all; build inclusive and efficient markets, trade systems, and food industries; transform agricultural and rural economies; and strengthen institutions and governance. Gender is integrated in all the Institute's work. Partnerships, communications, capacity strengthening, and data and knowledge management are essential components to translate IFPRI's research from action to impact. The Institute's regional and country programs play a critical role in responding to demand for food policy research and in delivering holistic support for country-led development. IFPRI collaborates with partners around the world.

AUTHORS

Kibrom A. Abay (K.Abay@cgiar.org) is a Senior Research Fellow with the Development Strategies and Governance Unit of the International Food Policy Research Institute (IFPRI), Washington, DC.

Guush Berhane (Guush.Berhane@cgiar.org) is a Senior Research Fellow with IFPRI's Innovation Policy and Scaling Unit, Washington, DC.

Daniel O. Gilligan (D.Gilligan@cgiar.org) is the Director of IFPRI's Poverty, Gender and Inclusion Unit, Washington, DC.

Kibrom Tafere (K.Tafere@worldbank.org) is an Economist at the Development Research Group at the World Bank, Washington DC.

Alemayehu Seyoum Taffesse (a.seyoumtaffesse@cgiar.org) is a Senior Research Fellow and Program Leader of IFPRI's Ethiopia Program, Addis Ababa, Ethiopia.

Notices

Abstract

Targeting is an important but challenging process in the design and delivery of social and humanitarian assistance programs. Community-based targeting (CBT) approaches are often preferred for their local information advantages, especially when data-driven methods are not feasible. However, how different variants of CBT approaches fare under various constraints and environments remains unclear. For example, it is not obvious whether agents involved in CBT maximize the number of beneficiaries or the intensity of transfers when given different levels of discretion or they face budget constraints. We implemented a clustered randomized control trial among community leaders in 180 villages in Ethiopia to evaluate how community leaders target and allocate resources when they face budget constraints and are in the presence (absence) of discretion. We find that under resource constraints, community leaders prefer to maximize the number of beneficiaries even at the expense of thinly spreading budgets (reducing average transfers to beneficiaries). Community leaders are keen to minimize exclusion errors even at the expense of increased inclusion errors, suggesting that community leaders may be sensitive to potential communal repercussions and hence prefer to accommodate beneficiaries who would otherwise be excluded based on survey-based measures and indicators of poverty. Consistent with this, we find that offering community leaders some level of discretion helps them reduce exclusion errors and include those most deprived or those affected by armed conflicts. Finally, we find that community leaders are more vulnerable to favoritism when real stakes (rather than hypothetical) are involved, budgets are relatively larger, and they lack discretion. We offer nuanced evidence about the implications of implementing CBT designs in the absence of incentives for community leaders to reveal how they use local information.

Keywords: targeting, community-based, social assistance, social protection, Ethiopia

Acknowledgements

This work was supported by the Norwegian Government under the project titled “Learning Support for a Sub-Saharan Africa Multi-Country Climate Resilience Program for Food Security” and by the donors who fund the CGIAR Research Initiative on Fragility, Conflict, and Migration (FCM) and the CGIAR Research Initiative on National Policies and Strategies (NPS), through their contributions to the CGIAR Trust Fund: https://www.cgiar.org/funders/.

We would like to thank Harold Alderman, Agnes Quisumbing, James Allen IV, and Seminar participants at IFPRI for helpful comments. This research experiment was preregistered at the American Economics Association’s RCT registry (AEARCTR-0012795).

1 Introduction

Targeting is an important but challenging step in the design and delivery of social and humanitarian assistance programs. Effective targeting ensures that scarce resources are allocated to those most in need (Coady, Grosh, and Hoddinott, 2004).[1] However, the process is often fraught with challenges including the accurate identification of beneficiaries, minimizing inclusion errors (providing assistance to non-needy individuals) and exclusion errors (overlooking needy individuals), as well as navigating administrative and logistical barriers. Recurring shocks due to conflicts, climate-induced extreme events, and food price fluctuations heighten the demand for shock-responsive social assistance programs, often necessitating more stringent targeting mechanisms. This challenge is particularly acute in conflict-affected areas, where humanitarian organizations face the dual burdens of limited resources and urgent needs (Devereux, 2021; Devereux, Masset, Sabates-Wheeler, Samson, Rivas, and Lintelo, 2017; Maxwell, Young, Jaspars, Frize, and Burns, 2011; Sabates-Wheeler and Szyp, 2021).[2]

The existing literature offers a range of targeting mechanisms, including Community-Based Targeting (CBT), Proxy Means Tests (PMT), geographic targeting, and peer targeting (Coady et al., 2004; Karlan and Thuysbaert, 2019; Premand and Schnitzer, 2021). However, no targeting method is perfect and immune to inclusion and exclusion errors (Alatas, Banerjee, Hanna, Olken, and Tobias, 2012; Brown, Ravallion, and Van de Walle, 2018; Coady et al., 2004; Devereux, 2021; Hanna and Olken, 2018). Thus, the choice of targeting mechanisms under different circumstances remains a persistent policy dilemma, especially in fragile and conflict-affected contexts, where resource scarcity, weak state capacity and administrative inefficiencies, and shifting needs complicate the process (Sabates-Wheeler and Szyp, 2021). While a growing body of research offers some guidance on alternative targeting mechanisms, the conditions under which these alternative targeting mechanisms perform better remains unknown.

CBT designs are often favored for their potential to leverage local knowledge (Alderman, 2002; Coady et al., 2004; Rai, 2002; Trachtman, Permana, and Sahadewo, 2022). However, significant gaps remain in understanding how different CBT designs perform in different contexts, including when community leaders face resource constraints, and varying degrees of discretion in the targeting of beneficiaries, particularly in the absence of credible incentives for community leaders to truthfully reveal how they use local information. CBT outcomes may be more progressive (or regressive) than survey-based targeting approaches, for example, in terms of reaching the needy or the size of transfers allocated. Moreover, allowing some level of discretion, including in setting targeting criteria, pro-

[1] Effective targeting is especially critical in the context of rising fragility and conflict in Africa, where donors and development partners face growing challenges in meeting increasing demand for humanitarian resources (OCHA, 2022a).

[2] This challenge arises in part because of the uncertainty surrounding targeting in response to shocks as the latter (for example, conflicts) can constantly change living conditions while also increasing the urgency to move fast to save lives. These challenges can make targeting costly, and hence not cost-effective than universal coverage (Devereux, 2021), or socially inappropriate (Ellis, 2017).

vides community leaders with the flexibility to adapt to unforeseen circumstances. However, it can also lead to adverse behaviors, such as elite capture or nepotism (Bardhan and Mookherjee, 2005; Basurto, Dupas, and Robinson, 2020; Conning and Kevane, 2002; Devereux, 2021). Conversely, imposing predetermined criteria (hereinafter rule-based approach) may reduce the risk of such behavior but at the cost of inflexibility to changing circumstances. Like policymakers, community leaders, under resource constraints and elevated caseloads, often face the dilemma of reaching fewer high-need households or spread resources more broadly across the community. Thus, it remains unclear what objectives community leaders pursue when tasked with the responsibility of targeting social assistance programs that are resource constrained (Alatas et al., 2012; Conning and Kevane, 2002; Trachtman et al., 2022). Evidence on how each of these CBT designs perform under various conditions can help improve the effectiveness of targeting in complex settings.

This study evaluates the performance of alternative CBT designs in conflict-affected and fragile settings in Ethiopia. Specifically, we address four key questions: (i) How do community leaders target and allocate resources under budget constraints—do they prioritize maximizing the number of beneficiaries or the size of transfers allocated to specific household types (e.g., the poorest of the poor)? (ii) How does the level of discretion granted to community leaders in targeting impact the number and composition of beneficiaries as well as the size of transfers? (iii) Does the nature of the transfers—real versus hypothetical—affect the targeting decisions of community leaders? (iv) How does exposure to conflict mediate targeting outcomes?

To answer these questions, we partnered with communities in 180 villages in Ethiopia and conducted a randomized control trial. Our study builds on a large household survey conducted prior to the outbreak of the recent conflict in Ethiopia, which serves as a baseline for our community-level experiment. Communities were randomly assigned to treatment or control groups, with real cash transfer funds provided to community leaders in the treatment group to allocate among beneficiary households. We conducted an incentivized CBT experiment, in which community leaders were asked to make a one-time transfer to community members after ranking them from the most to the least in need for social assistance transfers. This exercise mimics key aspects of traditional CBT where community leaders' local knowledge is used to target social assistance (Alatas et al., 2012; Basurto et al., 2020; Dupas, Fafchamps, and Houeix, 2022; Trachtman et al., 2022). We then conducted follow-up household and community surveys, revisiting the same communities and households. The surveys employed additional enriched instruments that are designed to gather insights on alternative variants of community-based targeting.

We exogenously varied the total budget available to community leaders for allocation to study how CBT responds to resource constraints. This allows us to identify whether resource scarcity (i) makes CBT more inclusive or exclusive, (ii) changes the composition of beneficiaries, and (iii) leads to increases or decreases in resource leakage. We also exogenously vary the level of discretion granted to community leaders in targeting beneficiaries.

Specifically, community leaders were randomly assigned to one of two groups that were either given discretion to set out own targeting criteria or asked to use set of predetermined targeting criteria. This variation allows us to study how different levels of autonomy given to community leaders affect targeting in CBT. To mimic real world settings in which some transfer funds may be siphoned off, misappropriated or wasted by beneficiary selectors and implementers, our experimental design permits community leaders to retain a limited portion of the budget to cover their "administrative costs". This would allow us to study patterns of prosocial behavior by community leaders under different targeting designs.

The role of conflict in shaping targeting decisions is another key aspect of our study. We link our experimental data with data from the Armed Conflict Location and Event Data (ACLED) project, enabling us to examine how exposure to armed conflict affects the performance of alternative CBT designs. Specifically, we investigate whether conflict exposure leads community leaders to adjust their targeting strategies, either by prioritizing certain households or by altering the size of transfers. Ethiopia is particularly a suitable setting to study targeting in the context of fragility and conflicts. On one hand, the country has experienced a surge in conflicts in recent years, while on the other hand, starting in 2005, it implemented one of Africa's largest social assistance programs, the Productive Social Safety Net Programme (PSNP). The PSNP's targeting requires local officials and community leaders to apply broad targeting guidelines, but also grants some discretion in determining who receives assistance. This context offers a unique opportunity to study targeting in a conflict affect settings.

Our findings can be summarized as follows. First, community leaders are keen to minimize exclusion errors even at the expense of increased inclusion errors, suggesting that community leaders may be sensitive to potential communal repercussions and hence prefer to accommodate beneficiaries who would otherwise be excluded based on traditional well-being measures such as poverty. Although targeting through the CBT appear to be more progressive when viewed from the vantage point of the community leaders' perceived measure of relative economic status of households in the community, targeting is relatively less progressive when measured using conventional well-being measures (consumption and wealth quintiles). These results suggest that community leaders may be using slightly different targeting criteria than survey-based measures (Alatas et al., 2012; Basurto et al., 2020).

Second, and consistent with the above, we find that offering some level of discretion to community leaders enables them to include those most deprived (minimize exclusion errors) or those affected by armed conflicts. Third, we find that when community leaders face budget constraints, they are reluctant to cut the number of beneficiaries but willing to reduce the average size of transfers allocated to each beneficiary. Specifically, while reducing the budget allocated by half decreases the probability of access to transfers by only 8-9 percentage points (about 10 percent), this leads to 43 percent cut in the average transfers made to beneficiaries. Fourth, community leaders are more likely to withhold a portion of the community funds when stakes are real rather than when hypothetical; and this is more so when budget sizes are larger. In other words, community leaders are more likely to be pro-social when budgets are tighter than otherwise. Finally, we document that community leaders are more vulnerable to favoritism when stakes are real (rather than hypothetical), budgets are relatively larger, and they lack discretion.

Overall, four important implications can be drawn from our findings. First, the outcomes of a community-based targeting, measured in terms of inclusion/exclusion errors or well-being, may vary significantly across variants of CBT designs, depending on the degree of control granted to community leaders and the size of available resources against needs in the community. For example, when community leaders are not restricted by a beneficiary quota, they are keen to minimize exclusion errors even at the expense of committing inclusion errors. This is the case even when they face budget constraints. This implies that to the extent that variants of these CBT designs remain important, comparing the outcomes of a generic CBT against other survey-based targeting measures such as PMT may not be intuitive as the objective functions of community leaders and program implementer can deviate. Such comparisons warrant designing a unique CBT that takes into account the nuances of variants of CBT designs (comparable to specific survey-based targeting outcomes). The literature comparing alternative social assistance targeting designs is mute on these important design features of CBT with critical implications to the resulting outcomes. For example, Premand and Schnitzer (2021) compare CBT and PMT in the context of a national cash transfer program in Niger and find that PMT generated slightly higher impacts and more appealing among non-beneficiaries than CBT despite the usual lack of transparency critique presented against PMT-based targeting (Hanna and Olken, 2018). Alatas et al. (2012) point out that CBT may be preferred over data-driven approaches, as "communities seem to have widely shared objective function other than per capita consumption", including earning potential or vulnerability (and CBT can be a source of widespread satisfaction in the community). On the other hand, Trachtman et al. (2022) show CBT may not be suitable to predict short-term distress but rather long-term poverty dynamics. As such, to the best of our knowledge, our paper is the first to present such nuanced comparative results of impacts of within CBT design features on targeting outcomes.

Second, our findings on how community leaders respond to budget constraints offer suggestive evidence on the implications of budget cuts to safety net and humanitarian assistance programs. Under funding gaps, humanitarian programs that rely on community-based targeting may witness larger reductions in the size (intensity) of transfers as community leaders prioritize reaching as many beneficiaries as possible. This may have important implications to the ultimate impacts of transfers. The reduction in transfer size due to budget constraints raises concerns about the cost-effectiveness of cash transfer programs as larger transfers have been shown to improve household welfare more cost-effectively than smaller ones (Banerjee, Duflo, Goldberg, Karlan, Osei, Parienté, Shapiro, Thuysbaert, and Udry, 2015; Deaton, 1989; Kondylis, Loeser, et al., 2021).

Third, some of our results highlight positive returns to discretion offered to community leaders, as measured by their relative efficiency in reaching out to the poor and those affected by shocks as well as in terms of their vulnerability to elite capture and favoritism. Finally, the suggestive evidence that community leaders can engage in rent collection and benefit their connections when steaks are real (instead of hypothetical) reinforces the usual caveat associated with elite capture and favoritism in CBT (Bardhan and Mookherjee, 2005; Basurto et al., 2020; Conning and Kevane, 2002) and call for appropriate monitoring and oversight in the implementation of CBT. The fact that these challenges are more evident when community leaders are granted with relaxed budgets than otherwise but interestingly also when they are given with specific rules than when they have the autonomy to control the process, suggests limiting leaders' autonomy on paper may not matter, but in fact can be used as a pretext to override responsibility so long as there is no way to verify that rules have been strictly followed. These findings underscore the need for a deeper understanding of the underlying incentive structures of communities and their leaders and how they use local information when delegated to facilitate targeting in view of the objectives of social assistance programs.

The rest of the paper is organized as follows. In Section 2, we discuss the context and experimental design. Section 3 describes the data and associated descriptive results. Section 4 discusses the estimation strategy. Section 5 presents the main findings. Section 6 concludes.

2 Context and Experimental Design

2.1 Context

Despite the notable economic progress in the last two decades, rural households in Ethiopia face a number of overlapping crises, including violent conflicts, climate-induced shocks, mass displacements, and inflationary pressures (OCHA, 2024; Tefera Taye, Mogus, Carter, Lind, and Sabates-Wheeler, 2024; UNFPA, 2024). Due to lack of capacity to self-insure or cushion impacts, such compounded crises often affect the poor disproportionately, with important implications to humanitarian and social assistance programming. In the presence of shocks, humanitarian and social assistance programs are commonly used to protect the poor, particularly during and after conflicts.

Over the last two decades, Ethiopia has implemented one of the largest social assistance programs in Africa, the Productive Safety Net Program (PSNP). The program reaches about 8 million rural people living in food insecure communities in all regions of the country (Abay, Abay, Berhane, and Chamberlin, 2022; Berhane, Gilligan, Hoddinott, Kumar, and Taffesse, 2014; Gilligan, Hoddinott, and Taffesse, 2009; Hoddinott, Berhane, Gilligan, Kumar, and Seyoum Taffesse, 2012). During much of this period, Ethiopia experienced relative stability and notable improvements in socioeconomic indicators (Berhane et al., 2014;

Hoddinott et al., 2012) and transformation in rural economies (Bachewe, Berhane, Minten, and Taffesse, 2018). However, recent years have witnessed a substantial increase in political unrest and violent conflicts, displacing millions and exacerbating food insecurity beyond the populations traditionally supported by the PSNP.

In addition, as part of the broader climate crisis impacting the Horn of Africa, over the last three years, Ethiopia also faced severe droughts affecting millions, mainly in the conflict-affected north (FEWSNET, 2023). These were compounded by the impacts of the COVID-19 pandemic and the Russia-Ukraine war crises, which respectively, hit in 2020 and 2021. An estimated 29.7 million people, including 4.5 million internally displaced persons, were in dire need of humanitarian assistance in Ethiopia by 2022 (OCHA, 2024). Reports also indicate that humanitarian agencies are struggling to sustain support for increased humanitarian caseloads in the face of severe resource constraints (OCHA, 2022b). In part due to the scale of these challenges, recent studies show that humanitarian assistance in these contexts in Ethiopia remained largely patchy, with limited flexibility to accommodate internally displaced people and lacking coordination with the PSNP itself (Lind, Sabates-Wheeler, Carter, and Tefera Taye, 2024).

This surge in the number of people in need of assistance, coupled with resource constraints faced by international aid agencies and development partners amidst the increase in major conflicts globally, has necessitated a rethink of existing targeting approaches to allocate limited resources more effectively, with important ramifications to livelihood and well-being outcomes. The PSNP uses a combination of geographic (to identify districts) and community-based (to identify beneficiaries) targeting. In our study, we mimic the community-based targeting of PSNP and randomly assign communities to different targeting treatment arms to distribute cash transfers to community members (more on this in the experimental design section). About 30 percent of the households in our sample are PSNP beneficiaries at baseline. Accordingly, one may expect that if community leaders are targeting exclusively based on poverty and assume similar objective function with PSNP implementer, they may reach a comparable share of households in their communities. However, as we discuss below, this is less likely to happen in practice because: (i) community leaders may consider additional criteria whenever allowed, and (ii) they may have slightly different objective function to maximize.

The overlapping humanitarian crises and the recent underfunded emergency appeals means that the need for effective targeting is imperative, which makes Ethiopia a suitable context to study the subject at hand. The context provides a unique opportunity to understand which alternative CBT designs perform better in these conditions.

2.2 Experimental Design

The intervention follows community level clustered randomization in which 180 communities are randomly assigned to one of four treatment arms. The assignment into different treatment and control arm considers:(i) whether communities receive actual or hypothetical transfer funds (control), (ii) the nature of discretion granted to community leaders in the selection of beneficiaries and allocation transfers (discretion versus rule-based) and (iii) the size of the transfer pool available to community leaders to distribute among households within the community (constrained budget of 10,000 Birr (≈ USD 180) versus relaxed budget of 20,000 Birr (≈ USD 360)).[3] In all arms (including the control group), community leaders were instructed to follow a two-step process in making the transfer allocation. First, they rank households based on their social assistance needs. Then, they allocate the budget (real or hypothetical) assigned to the community among the 20 households included in our sample. The treatment arms generated by combinations of the features above are outlined as follows (see Figure 1).

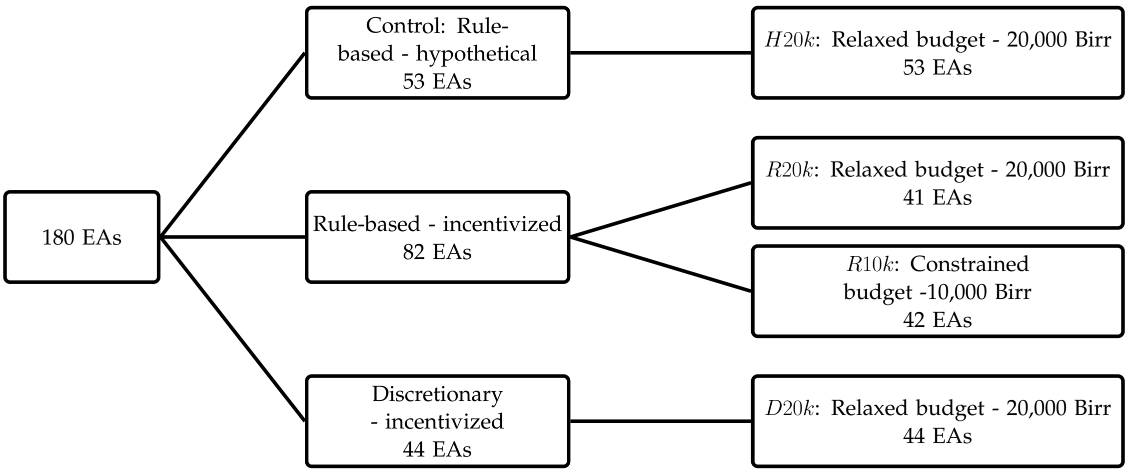

Figure 1: Random assignment of communities across treatment and control arms

(i). Rule-based hypothetical targeting with relaxed budget ($H20k$)

The group of villages assigned to this arm serve as a control cluster. Community leaders were not given any actual funds but were instructed to act as if they have a hypothetical budget of 20,000 Birr to distribute among households in their community. In ranking households, leaders were required to strictly adhere to pre-specified rules provided by the research team. These rules were carefully selected to mimic the targeting criteria used in social assistance programs in Ethiopia including the Productive Safety Net Program (PSNP). More specifically, community leaders were 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. Although we define the criteria for identifying beneficiaries, we did not limit the number of beneficiaries they could identify in each community. Similarly, we have not identified the maximum or minimum transfer that can be transferred to beneficiary households. As

[3]Birr is an Ethiopian currency and at the time of data collection 1 USD was equivalent to approximately 56 Ethiopian Birr.

shown in Figure 1, we assigned about 30 percent of the 180 EAs into the control group.

(ii). Rule-based incentivized targeting with relaxed budget ($R20k$)

A second group of communities were randomly assigned to a cluster that received real transfer funds with a budget of 20,000 Birr. Community leaders in this cluster were asked to rank households based on the same set of five predetermined targeting criteria used in the control group to allocate the transfers. As noted above, these mimic the targeting criteria used by the national safety net program in Ethiopia, the PSNP (e.g., Abay et al., 2022; Gilligan et al., 2009; Hoddinott et al., 2012). However, as in the control group, we did not impose any restriction or limit on the number of beneficiaries they could identify in each community or the maximum or minimum transfer that can be transferred to beneficiary households. Thus, even in the rule-based targeting community leaders had substantial power and role to shape the distribution of the cash transfer.

(iii). Rule-based incentivized targeting with constrained budget ($R10k$)

This group of communities followed similar rules as those in the control group and $R20k$, but they received a constrained budget of 10,000 Birr. Community leaders were 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 were designed to mimic the targeting criteria used by the PSNP. This treatment arm allows us to test the implications of a budget constraint on beneficiary targeting and allocation of cash transfers.

(iv). Discretionary incentivized targeting with relaxed budget ($D20k$)

The fourth group of communities were 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 criteria they collectively establish. The development of these ranking criteria was entirely left to the discretion of the community leaders. It was up to the leaders to determine how the 20,000 Birr would be allocated among the ranked households. As in the rest of the treatment arms, there was no restriction or limit on the number of beneficiaries they could identify in each community or the maximum or minimum transfer that can be transferred to beneficiary households.

Our experimental design allows us to probe what type of objective functions community-leaders maximize. Besides recording the targeting outcome and transfers, we collected three sets of information that can capture the objective functions community leaders are maximizing in their targeting process: (i) for all households they rank, we asked community leaders to identify the three most important reasons that prompted such ranking, (ii) in the rule-based ranking we ask community leaders whether our criteria were comprehensive enough, and (iii) in the discretionary arm we elicited the criteria agreed upon by community leaders.[4] Comparing these responses across treatment arms help us identify the objective functions community leaders maximize.

3 Data and Descriptive Results

3.1 Data

The experimental design in this study leverages a large household survey conducted in 2019 that was designed to evaluate the Feed the Future (FtF) program implemented in 132 *woreda*s or Zones of Influences (ZOI) in Ethiopia. Sampling followed a two-stage stratified cluster sampling design. In the first stage, 264 Enumeration Areas (EAs), or communities, two EAs from each *woreda*, were randomly drawn from a national census frame of the 132 *woreda* using a probability proportional to size sampling design. Next, 20 households were randomly selected from a complete list of households from each of the 264 EAs, generating a total sample of 5,280 households.[5]

Out of the 264 EAs administered in the 2019 survey, 180 EAs were identified to be accessible for the 2023 survey, with the remaining EAs being inaccessible because of the ongoing conflict in some regions of Ethiopia. These 180 EAs and associated households represent the basis for our experimental design. As shown in Figure 1, we randomly assigned these EAs into four arms. To probe the validity of our randomization, we test the balance of the observable characteristics across the four arms using the 2019 characteristics of households. For this purpose we run pairwise t-tests to examine the distribution of key characteristics of households and communities observed at baseline. Table 1 reports these descriptive results, along with t-tests between the control and different treatment arms as well as their pairwise comparisons. We note from the outset that our tests for almost all characteristics are balanced between the control and different treatment arms, suggesting our randomization worked well. Overall, about three-quarters of the sample households are male-headed, with an average family size of five. The average household head is 47 years old and received about three years of education. The majority of the sample comes from rural (with only 19 percent from urban) settings. These contexts are typically characterized by rainfall-dependent smallholder agricultural livelihoods with limited assets and more than 30 percent are below the national poverty line.

[4] Enumerators were given a comprehensive list of items that could be broadly mapped to the list of reasons and criteria that may be associated with each household

[5] An EA typically comprises 150 to 200 households within a *kebele*, the lowest administration unit in Ethiopia.

Table 1: Balance test between treatment arms

	(1) Control/ hypothetical 20k Mean/SE	(2) Rule-based 10K Mean/SE	(3) Rule-based 20K Mean/SE	(4) Discretionary 20k Mean/SE	(1)-(2) Pairwise t-test (p-value)	(1)-(3) Pairwise t-test (p-value)	(1)-(4) Pairwise t-test (p-value)	(2)-(3) Pairwise t-test (p-value)	(2)-(4) Pairwise t-test (p-value)	(3)-(4) Pairwise t-test (p-value)
Male headed household	0.76	0.73	0.75	0.75	0.08*	0.65	0.61	0.20	0.19	0.98
	(0.01)	(0.01)	(0.01)	(0.01)						
Age of the household head	46.38	46.70	46.04	45.59	0.78	0.78	0.55	0.55	0.37	0.72
	(0.91)	(0.78)	(0.79)	(0.95)						
Education of household head	2.95	2.88	2.94	2.74	0.86	0.98	0.56	0.89	0.72	0.61
	(0.26)	(0.31)	(0.30)	(0.25)						
Household size	4.87	4.69	4.89	4.70	0.29	0.87	0.26	0.27	0.99	0.25
	(0.11)	(0.12)	(0.13)	(0.11)						
Poor household (national PV)	0.30	0.35	0.29	0.27	0.30	0.88	0.50	0.26	0.11	0.61
	(0.03)	(0.04)	(0.03)	(0.03)						
Urban	0.19	0.15	0.09	0.11	0.62	0.16	0.28	0.39	0.59	0.72
	(0.05)	(0.05)	(0.04)	(0.05)						
Remote (above 100km distance)	0.56	0.64	0.46	0.51	0.43	0.33	0.60	0.09*	0.21	0.66
	(0.07)	(0.07)	(0.08)	(0.08)						
Wealth index (bottom 40 percent)	0.41	0.40	0.36	0.48	0.88	0.43	0.30	0.57	0.28	0.08*
	(0.04)	(0.05)	(0.05)	(0.05)						
Ranked among the poor by leaders	0.32	0.36	0.30	0.34	0.46	0.67	0.70	0.22	0.69	0.38
	(0.04)	(0.04)	(0.03)	(0.03)						
Household is member of kebele leadership	0.15	0.10	0.11	0.10	0.28	0.42	0.23	0.84	0.96	0.79
	(0.04)	(0.03)	(0.04)	(0.02)						
Household related to kebele leadership	0.12	0.10	0.17	0.15	0.62	0.26	0.43	0.16	0.26	0.68
	(0.02)	(0.03)	(0.04)	(0.03)						
Household related/or member of leadership	0.24	0.17	0.24	0.22	0.24	0.95	0.66	0.26	0.41	0.65
	(0.04)	(0.04)	(0.05)	(0.03)						
Village has access to electricity	0.59	0.42	0.46	0.41	0.10	0.21	0.08*	0.72	0.94	0.67
	(0.07)	(0.08)	(0.08)	(0.08)						
Village has access to market	0.60	0.61	0.58	0.56	0.96	0.82	0.71	0.79	0.69	0.89
	(0.07)	(0.08)	(0.08)	(0.08)						
Number of battles in the last 3 years (20km)	7.11	8.76	8.20	6.88	0.59	0.72	0.93	0.88	0.54	0.67
	(1.73)	(2.56)	(2.54)	(1.74)						
Number of battles in the last 3 years (15km)	3.78	5.97	4.91	5.44	0.29	0.62	0.34	0.70	0.82	0.83
	(0.95)	(1.85)	(2.05)	(1.47)						
No. observations	974	801	787	818						

Notes: This table compares the distribution of observable baseline characteristics of households and communities across the four groups. The first four columns provide mean values (with standard errors in parentheses) while the last four columns report p-values from pairwise comparisons and t-tests. Standard errors, clustered at village (*kebele*) level, are given in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

In terms of implementation, detailed community-level survey was also administered in each of the 180 EAs before the community-based cash transfer was distributed. The community-level survey captured a wide-range of community-level characteristics. About 1-2 months after the implementation of the one-time cash transfer, a follow up and comprehensive household survey was implemented between November 2023 to January 2024 in all EAs.

With the aim of understanding and comparing the performance of alternative CBT designs, the experiment involved community leaders from each of the 180 EAs and were tasked to target and allocate the one-off cash transfer to potential beneficiaries among the 20 households selected from each community (see experimental design for additional details). The experiment brought together six individuals composed of key *kebele* leaders, including the *kebele* chairman and other individuals knowledgeable about the village. Specifically, the six committee members included (i) the *kebele* leader or a member of the *kebele* leadership, (ii) Elderly man/woman, (iii) a religious leader, (iv) a women representative, (v) a teacher or a development agent or an extension worker, and (vi) a youth representative. We note that these members are commonly involved in the targeting of social and humanitarian assistance programs in Ethiopia, including the PSNP. The exact composition of community leaders in our sample is presented in Table A1.

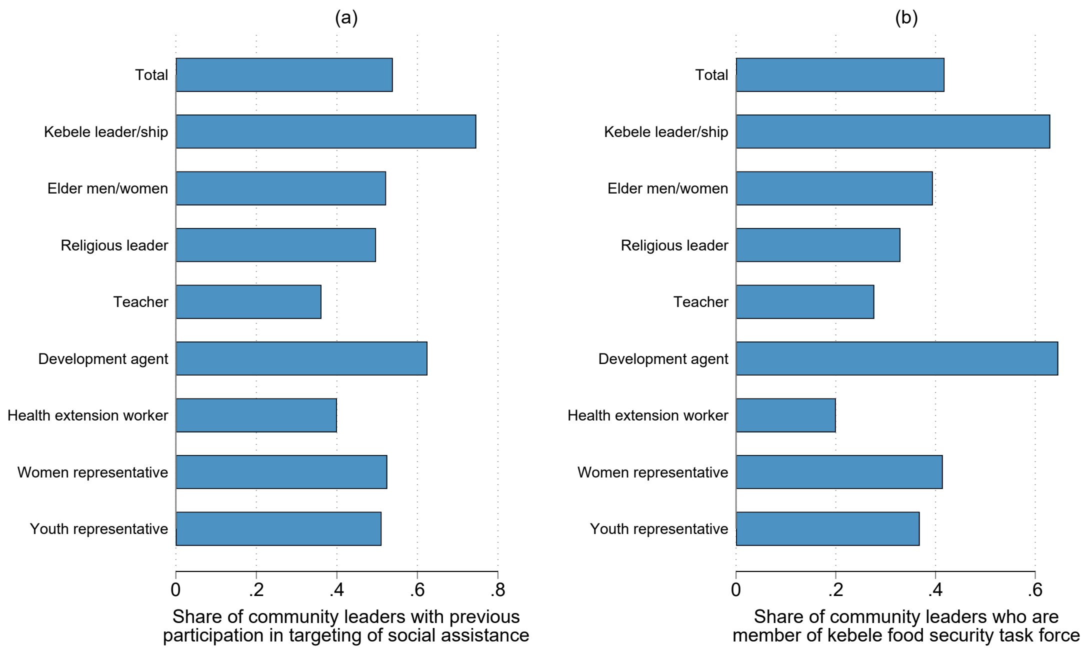

Figure 2: Previous and current targeting experience of community leaders

Community leaders' prior exposure to similar targeting exercises is likely important to CBT. Community leaders were asked two questions regarding experience and participation in *kebele* food security task forces (KFSTF), a local administrative structure tasked with the targeting of the PSNP. Figure 2 panel (a) reports the share of community leaders with prior experience in the targeting of social assistance programs and panel (b) reports the share of community leaders who are members of *kebele* food security task force. The majority of community leaders in our sample (54%) have prior experience in the targeting of social assistance, with 42% reporting they are currently serving in the KFSTF of their respective *kebele*s. However, as shown in Figure 2, there is considerable variation in targeting experience depending on the role that members of the community leadership group play in their village. *kebele* leaders and development agents appear to be the most experienced, followed by women's representatives and village elders (panel (a) of Figure 2). Similarly, these members are more likely to be current participants in local community targeting groups (panel (b) of Figure 2).

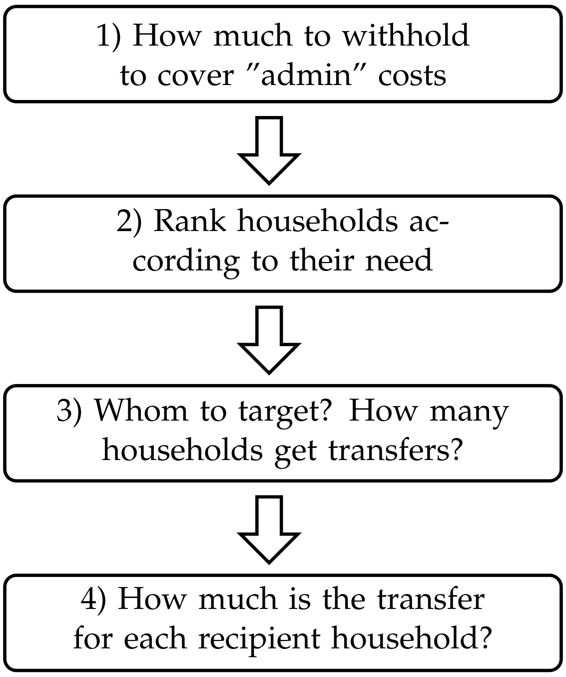

Figure 3: Key decisions made by community leaders

As in actual social assistance programs in Ethiopia, community leaders play important roles in our study, making a series of decisions including identifying the target beneficiaries given the individual and local circumstances as well as making the actual allocation of the transfers (see Figure 3). Community leaders provide these services on voluntary basis as part of their roles in the community and often at no remuneration. However, in reality such services may not be cost-less and may involve trade offs between one's pro-social behavior and the desire to cover private costs. Such trade off is often neglected in community-based social transfer studies, the effect of which remains unknown. In this study, we are also interested to study the extent of *pro-socialness* or vulnerability to elite capture among community leaders in the context of conflict-affected poor communities in Ethiopia. To do this, community leaders are given the option to cut a small portion of the funds allocated to the community for themselves as "administrative cost". This could go up to a maximum of 10% of the funds in 2% increments (0%, 2%, 4%, 6%, 8%, or 10%). That is, after villages are assigned to one of the four control/treatment arms, the first decision community leaders make relates to how much of the funds allocated to the community they would like to reserve for themselves as ”administrative costs”. This would allow us to study the *pro-social behavior* of leaders since the funds could otherwise have been distributed to the poorest members of the community, and community leaders are, often times, likely among the better off in their villages. Although there are not major costs associated with the targeting process, except for their time and costs for calling potential beneficiaries to take the money, we introduced this option to learn about community leaders' pro-social behavior because what is not taken by them is going to be distributed to community members. We note that each member of the committee was given about 4-5 USD in appreciation for their time and they were told about this ahead of the targeting experiment.

Following the decision on ”administrative cost”, community leaders as a group are asked to rank the 20 households in their villages (who also are part of our sample) according to their need for social assistance. This step is expected to inform the identification of beneficiaries of the transfer funds made available to communities and community leaders are expected to converse among themselves regarding their ranking based on need. In the third step, leaders decide on the number of beneficiaries and identify beneficiary households. They are free to allocate the funds to a single person, everyone on the potential beneficiary list, or anything in between. In the final step, conditional on selection for transfers, leaders decide regarding allocation of the remaining transfers to beneficiaries selected based on their ranking exercises. They were instructed to adhere to their ranking in making the allocation, ensuring that households ranked lower do not receive a larger share of the transfer funds. These sets of decisions inform the key questions that our study seeks to address.

Finally, we are interested to understand whether and to what extent variants of CBT consider exposure to conflicts in their targeting and allocation of social cash transfer funds. Specifically, we use the Armed Conflict Location and Event Data (ACLED) to study how exposure to conflict affects access to transfers made across different treatment groups. ACLED provides detailed information on a wide range of conflict events, encompassing both violent and non-violent occurrences. ACLED provides data for six types conflict events mainly battles, protests, riots, explosions/remote violence, violence against civilians, and strategic developments (Raleigh, Linke, Hegre, and Karlsen, 2010). Our analysis uses the *number of battles*, which account for the largest portion of conflict events recorded by ACLED in Ethiopia in the last three years. These battle events are identified using GPS coordinates, which also enables us to merge the data with our survey data. We constructed the cumulative number of battle events that occurred within 10, 15, and 20 km of each household's residence in the last three years before the survey. This approach enables us to examine how proximity to conflict areas influences access to transfers under different treatment groups. The data shows 24 percent of sample were exposed to at least one battle event that occurred within 20 kilometers of radius of the household's residence.

3.2 Descriptive and Non-parametric Results

We first report some descriptive and non-parametric r esults. Table 2 shows the first-stage decision of the community leaders: how much to withhold for covering "administrative costs" out of the total budget. This table presents the shares of funds reserved for "administrative costs" and those that reserved the maximum possible under each treatment group along with t-test comparisons of each against the control. We are particularly interested in exploring three hypotheses in relation to community leaders' behavior. First, if community leaders are given the opportunity to extract some resource from a social assistance that coming to community members in need, how would they behave under different variants and designs of CBT. This is an important question that can inform the level of monitoring needed in community-based targeting. Second, we explore whether community leaders behave differently when stakes hypothetical versus real. Third, would the amount community leaders extract respond to budget size and associated budget constraints.

The results reported in Table 2 reveal some important patterns and behaviors associated with community leaders. First, if community leaders get the opportunity to extract, they would like to withhold a portion of the social assistance that is meant to community members. For example, the share of community groups requesting the maximum allowed administrative cost ranges between 45 percent in the hypothetical to 93 percent in the rule-based with relaxed budget. There are several explanations for this, including the entitlement that community leaders feel to share some of the transfer as well as the fact that the activities may actually involve some cost, including the fact that community leaders are likely to face complaints as consequence of these targeting processes.

Second, community leaders are more pro-social in the hypothetical exercise than in one that involves real incentives and transfers. For example, the share of the budget that community leaders withheld for covering their administrative costs or other purposes ranges from 6 percent in the hypothetical case to 10 percent in the rule-based with relaxed budget (20,000 Ethiopian Birr).

Third, community leaders are more likely to increase the share of budget they would like to withhold when the budget increases from 10,000 ETB to 20,000 ETB. The community leaders withhold on average 8 percent of the budget when they face a constrained budget (10, 000 ETB) while increasing this to 10 percent when they have access to a relaxed budget (20,000 ETB). This clearly suggests that community leaders may increase their share when they get the opportunity and become less pro-social when they face a relaxed budget. These patterns are consistent across the extensive and intensive margin of the share they withhold. For example, the share of groups demanding the maximum share increases from 83 percent in the constrained budget to 93 percent in the relaxed rule-based targeting. Finally, when community leaders are given discretion, they tend to slightly reduce the amount they would like to withhold and the share of groups demanding the maximum allowed rate, although these differences are not statistically different compared to the rule-based targeting with relaxed budget.

Table 2: Pro-social behavior of community leaders

	(1) Control/ hypothetical 20k Mean/SE	(2) Rule-based 10K Mean/SE	(3) Rule-based 20K Mean/SE	(4) Discretionary 20k Mean/SE	(1)-(2) Pairwise t-test (p-value)	(1)-(3) Pairwise t-test (p-value)	(1)-(4) Pairwise t-test (p-value)	(2)-(3) Pairwise t-test (p-value)
Share of budget taken for admin cost	0.06	0.08	0.10	0.09	0.01**	0.00***	0.00***	0.05*
	(0.01)	(0.01)	(0.00)	(0.00)				
Share who took maximum allowed	0.45	0.81	0.93	0.84	0.00***	0.00***	0.00***	0.11
	(0.07)	(0.06)	(0.04)	(0.06)				
Villages	53	42	41	44				

Notes: This table reports the share of budget taken by community leaders to cover "administration" cost. The first four columns provide mean values (with standard errors in parentheses) while the last four columns report p-values from pairwise comparisons and t-tests. Standard errors, clustered at village (*kebele*) level, are given in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Now we move on to describing what goes to the community members and associated distributions. We first assess the distribution of access to transfer across the different variants of community-based targeting approaches. Figure 4 shows that the share of households receiving transfer ranges from 72 percent in the constrained budget to 85 percent in discretionary group. Similarly, the average transfer going to each beneficiary ranges from 665 Birr in the constrained budget to 1234 Birr in the hypothetical scenario where community leaders demanded relatively lower amount and share of the budget for themselves. This clearly shows that the community leaders are keen to reach as many households as possible. In the next sections, we formally test whether budget constraints and discretion leads to significant change in the share of households' receiving the transfer as well as the size of transfer that goes to each beneficiary in the next section.

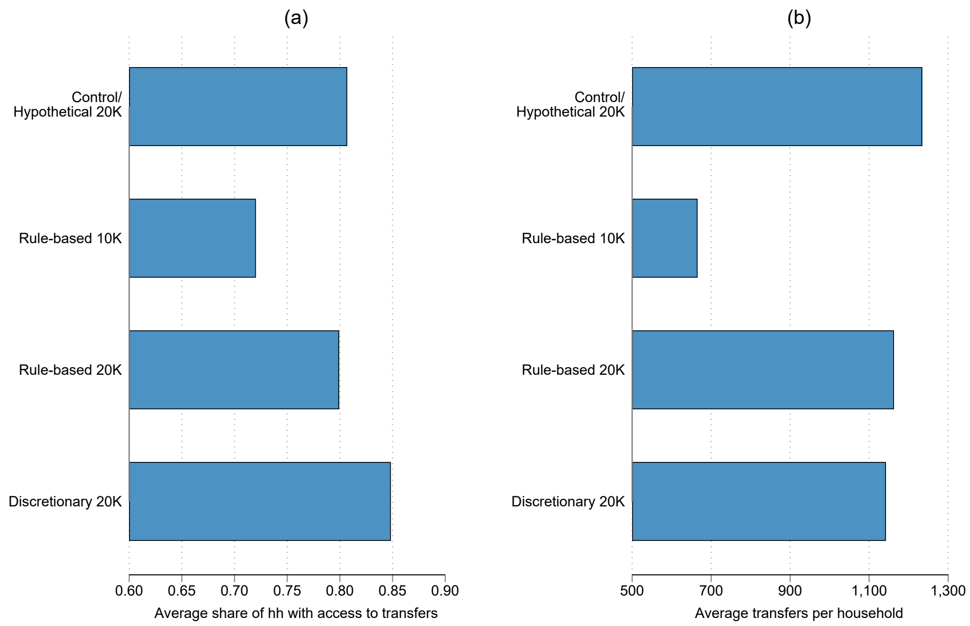

Figure 4: Access to and size of transfers by treatment group

Next we assess the distribution of transfers across households' rank by the community leaders. Before making any decision on the size of transfer, community leaders are asked to rank the 20 households within each community according their need for potential social assistance, starting with the most needy to the least. We expect that households ranked at the top and deemed needy are more likely to receive the transfer. To test this, we plot the probability of access to the transfer (and the size of transfer) as a function of household rank assigned by the community leaders. As expected Figure 5 shows strong inverse relationship between: (a) access to the transfer and households' rank, and (b) size of transfer going to each beneficiary and household rank. These patterns suggest that community leaders have followed the ranking exercise they did for distributing the cash to those deemed deserving.

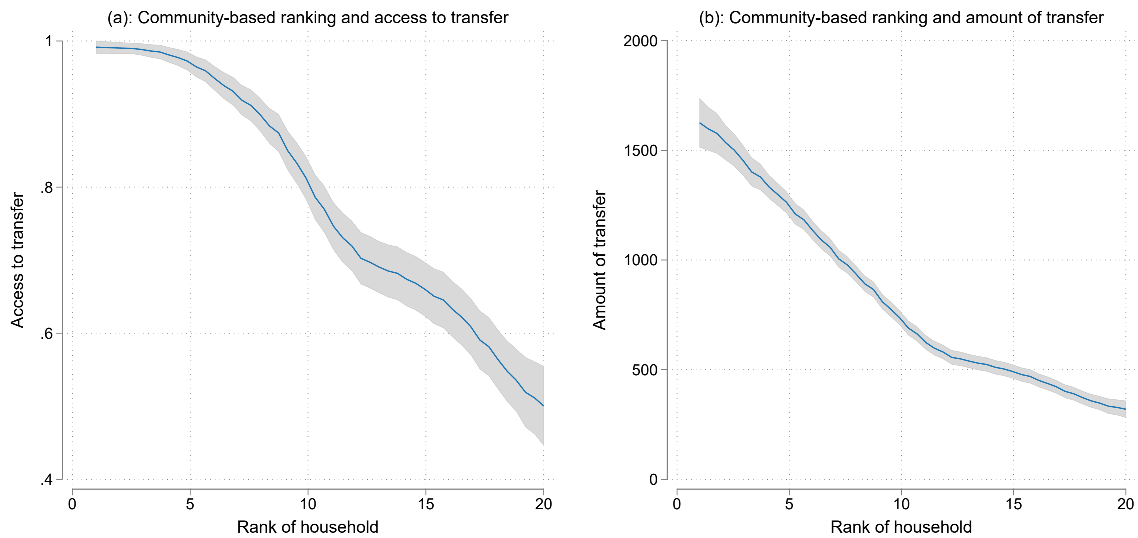

Figure 5: Correlation between access and size of transfers with rank of households

4 Empirical Strategy

To compare the performance of alternative variants of CBT, we exploit the random variation generated by the intervention that assigned villages and hence community-leaders into a control and three treatment arms. Communities were randomly assigned into these groups based on: (i) whether community-leaders receive hypothetical or actual transfer funds to distribute among households (i.e., hypothetical ranking/distribution exercise or incentivized ranking exercise); (ii) the nature of discretion granted to community leaders when ranking households and allocating transfers (i.e., rule-based versus discretion); and (iii) the size of transfers community leaders are given to distribute to households (i.e., 10,000 Birr versus 20,000 Birr).

The randomly generated treatment variations produce the following treatment arms:

(i) Rule-based hypothetical ranking/distribution ($H20k$): a group that receives no real transfer but community leaders are asked to imagine a hypothetically allocation of 20,000 Birr transfer while ranking households in these communities. However, leaders are required to strictly follow predefined rules in ranking households. (ii) Rule-based incentivized targeting with relaxed budget ($R20k$): a group where community leaders receive a real 20,000 Birr transfer budget but are asked to rank households and distribute transfers based on pre-defined criteria. (iii) Rule-based incentivized targeting with constrained budget ($R10k$): a group where community leaders receive a 10,000 Birr transfer fund and are asked to rank households and distribute transfers based on pre-defined criteria. This group is similar to $R20k$ with the only difference being the smaller transfer budget. (iii) Discretionary incentivized targeting ($D20k$): this group receives a one-off 20,000 Birr transfer as social assistance to help needy households based on some criteria that is left to the discretion of the community leaders.

As described earlier, we employ two types of outcomes to compare and evaluate how alternative variants of community-based targeting perform. The first group of outcomes are measured at the community level and relate to the distribution or concentration of the transfers across the entire community as well as across the poor members of the community. We examine the first set of outcomes non-parametrically, including using Kolmogorov-Smirnov tests. The second set of outcomes are measured at the household level and relate to the extent and intensity of the community-based transfers. These outcomes include the number of beneficiary households, whether a household receives a transfer, and the size of transfer going to each household.

While the random assignment of villages into treatment and control groups generates unbiased average treatment effects using simple mean differences, the availability of observable geographic and baseline household characteristics facilitates more structured and powered estimations. The empirical specifications we estimate vary depending on which research question we aim to address. For example, to answer how CBT responds to budget constraints, we would like to compare the those assigned to the rule-based targeting with constrained budget and those assigned to the rule-based relaxed budget because the only difference between the two groups is the budget. Similarly, to evaluate the impact of the discretionary targeting, we need to compare households those assigned to the discretionary and those assigned to the rule-based with relaxed budget. Considering these two comparisons for addressing the impact of budget constraint and discretion, we start estimating the following empirical specification:

$$Y_{ic} = \beta_0 + \beta_1 H20k_c + \beta_2 R10k_c + \beta_3 D20k_c + \beta_4 X_{ic} + \epsilon_{ic} \tag{1}$$

where Y_{ic} measures access to community-based cash transfers of household i living in community c. We measure households' access to community-based cash transfers at the extensive margin using a binary indicator for access to transfers and at the intensive margin using the size of transfers going to each beneficiary household. $H20k_c$, $R10k_c$, and

$D20k_c$ stand for indicator variables of those households assigned to the hypothetical arm, incentivized rule-based, with constrained budget, and discretionary community-based targeting, respectively. X_{ic} captures a vector of observable pre-intervention characteristics of households (including gender, age and education of the household head as well as other socio-economic characteristics of households such as poverty status and participation in the PSNP) as well as a geographic characteristics, including an indicator for rural-urban location. ϵ_{ic} represents an idiosyncratic error term.

Given our interest in understanding the impact of a budget constraint and discretion in targeting, we consider the rule-based with relaxed budget category as our reference arm. This facilitates the interpretation of all coefficients in equation 1. For example, β_1 identifies the difference in the behavior of community leaders when transfers are hypothetical versus real. If community leaders are likely to be more generous in distributing to community members when transfers are hypothetical, we expect β_1 to be positive and statistically significant. Similarly, β_2 captures the impact of budget constraints on household targeting outcomes. Depending on whether community leaders maximize access to the program or size of transfers, we expect either a negative or statistically insignificant β_2. Whether community leaders are keen to reduce the number of beneficiaries or the size of transfer going to each beneficiary in response to a budget constraint is important to uncover. If a budget constraint leads community leaders to exclude households, we expect β_2 to be negative and statistically significant in the estimation involving extensive margin of access to transfers. On the other hand, if community leaders are keen to maximize the number of beneficiaries by reducing the size of transfers going to each beneficiary, we expect β_2 to be statistically insignificant in the estimation involving extensive margin of access to transfers and negative and statistically significant in the estimation involving the intensive margin of access to transfers. Finally, β_3 captures the impact of allowing discretion to community leaders on targeting outcomes.

Another important policy question is which of the CBT targeting approaches are pro-poor? This is a crucial question directly related to the targeting error involved in each targeting approach. As we have consumption-based poverty measure, we can directly test this by interacting the treatment indicators specified in equation 2 as follows:

$$\begin{aligned} Y_{ic} =& \alpha_0 + \alpha_1 H20k_c + \alpha_2 R10k_c + \alpha_3 D20k_c + \alpha_4 Poor_{ic} \\ &+\alpha_5 H20k_c \times Poor_{ic} + \alpha_6 R10k_c \times Poor_{ic} + \alpha_7 D20k_c \times Poor_{ic} + \omega_{ic}. \end{aligned} \tag{2}$$

where $Poor_{ic}$ is an indicator variable assuming a value of 1 for those households whose per capita consumption falls below the national poverty line at baseline (in the 2019 survey). All other variables are as defined above in equation 1 . The coefficients associated with the interaction terms between the alternative CBT and poverty status (α_5, α_6 and α_7) allow us to test whether some variants of the community-based targeting approaches are particularly more effective in serving the poor. For example, if either of the targeting approaches are pro-poor and enable the transfers of a large share of the budget to poor households, we expect α_5 or α_6 or α_7 to be positive and statistically significant.

Given the widespread prevalence of conflict in Ethiopia in the last few years, we also examine which of the variants of CBT are more responsive to conflict. For this purpose, we estimate the following specification by interacting the exposure to conflict and treatment assignment.

$$\begin{aligned} Y_{ic} =& \delta_0 + \delta_1 H20k_c + \delta_2 R10k_c + \delta_3 D20k_c + \delta_4 Battles_{ic} \\ &+ \delta_5 H20k_c \times Battles_{ic} + \delta_6 R10k_c \times Battles_{ic} + \delta_7 D20k_c \times Battles_{ic} + \phi_{ic} \end{aligned} \tag{3}$$

where all terms except $Battles_{ic}$ are as defined in equation 1-2. $Battles_{ic}$ captures households' exposure to armed conflict, which is measured using both the intensive and extensive margin of exposure to armed conflict. We focus on exposure to battles in the last three years given that battles represent more than half of the conflict events reported in the last few years in Ethiopia (Abay, Tafere, Berhane, Chamberlin, and Abay, 2023). If there are significant differences in the way the different variants of CBT are more likely to increase access to or the level of transfers to households affected by the armed conflict, we expect statistically significant values of δ_5 or δ_6 or δ_7. For example, we anticipate community leaders to use the discretion offered to them under the discretionary targeting and hence include households affected by armed conflicts. In that case, we expect δ_7 to be positive and statistically significant.

A major limitation associated with CBT is related to its vulnerability to elite capture and favoritism (Alatas et al., 2012; Bardhan and Mookherjee, 2005; Conning and Kevane, 2002; Galasso and Ravallion, 2005). To explicitly test and compare the vulnerability of the four variants of CBT to favoritism and elite capture, we collected information on households' relationship with community leaders. This information was collected before community leaders make decisions on the various steps that comprise the targeting of beneficiaries described in Section 2 (see Figure 3).

$$\begin{aligned} Y_{ic} =& \gamma_0 + \gamma_1 H20k_c + \delta_2 R10k_c + \gamma_3 D20k_c + \gamma_4 Connected_{ic} + \gamma_5 H20k_c \times Connected_{ic} \\ &+ \gamma_6 R10k_c \times Connected_{ic} + \gamma_7 D20k_c \times Connected_{ic} + \Omega_{ic} \end{aligned} \tag{4}$$

where all terms except $Connected_{ic}$ are as defined in equation 1 -3. $Connected_{ic}$ captures households' connection with community leaders and leadership, which is measured using a connection index capturing the intensive margin of connectedness as well as using binary indicators of connection with community leaders. If households with varying level of connection and association with community leaders have differential access to the transfers, we expect statistically significant values of γ_5 or γ_6 or γ_7. For example, if community leaders are more vulnerable to favoritism when stakes or incentives are real instead of hypothetical, we anticipate negative and statistically significant value of γ_5. Similarly, if community leaders are less vulnerable when budgets are relatively small, we expect negative and statistically significant value of γ_6. Finally, γ_7 serves to test whether discretion mitigates or aggravates favoritism and elite capture.

Households living in the same community are assigned to the same treatment group and they are likely to face similar shocks, market conditions, and food security environment, which could generate spatial correlation of unobserved effects (error terms) across households within the same community. To account for this, standard errors are clustered at the community level, which is the level of treatment and recommended in our case (Abadie, Athey, Imbens, and Wooldridge, 2023).

5 Results

5.1 Comparing the performance of variants of CBT

We start by assessing whether (and which of) the targeting approaches among the different CBT variants are progressive. For this purpose, we assess the distribution of access to transfers across four indicators reported in Table 3 : (i) quintiles of pre-transfer consumption per capita expenditure (Panel A), (ii) quintiles of post-transfer consumption per capita expenditure (Panel B), (iii) quintiles of wealth index (Panel C), and (iv) across groups of five-level community-leader assessed economic well-being (Panel D). While in Panel A shows comparison using pre-treatment indicators, we note that there was four years gap between the consumption values measured in 2019 and the targeting exercise, which was conducted in late 2023. To complement this, we also use the post-transfer consumption expenditure measured in 2023, though this might have been affected by the transfer itself. Community leaders may not target based on poverty but using long-term economic status such as wealth (e.g., Trachtman et al., 2022) for which reason we also assess the distribution of access to transfers based on quintiles of wealth index (Panel C). Lastly, we asked community leaders to assess each household's relative economic standing in the village and categorize them into five groups: (i) Among the poorest, (ii) Poorer than many, (iii) About average, (iv) Richer than many, and (v) Among the richest.

The distribution of transfers shown in Table 3 show three important patterns worth noting. First, targeting appears to be generally progressive using all indicators of household well-being, though not as strong as program implementers may want it to be. For example, Panel A shows that 75 percent of the poorest (first quintile) households assigned to the constrained budget received cash transfers while this declines to 60 percent among those in the fifth quintile. Although targeting in all four arms appear to be generally progressive, the fact that many non-poor households received the transfer means that there is substantial inclusion error, a topic we address next. Second, the patterns in Panel A and B are comparable, mainly because the transfers did not significantly change average consumption expenditures. The distribution of the transfers across wealth quintiles shows similar progressive targeting, but again more than half of the households in the last wealth

Table 3: Economic status of households selected for transfers

	Panel A: Consumption quintiles (2019)				
	Q1	Q2	Q3	Q4	Q5
Control/hypothetical 20k	0.87	0.77	0.84	0.79	0.76
Rule-based 10K	0.75	0.75	0.73	0.75	0.60
Rule-based 20K	0.82	0.82	0.79	0.79	0.78
Discretionary	0.95	0.90	0.87	0.83	0.72
	Panel B: Consumption quintiles (2023)				
	Q1	Q2	Q3	Q4	Q5
Control/hypothetical 20k	0.88	0.86	0.82	0.75	0.74
Rule-based 10K	0.77	0.81	0.78	0.72	0.66
Rule-based 20K	0.94	0.85	0.83	0.75	0.78
Discretionary	0.94	0.94	0.87	0.90	0.77
	Panel C: Wealth quintiles (2023)				
	Q1	Q2	Q3	Q4	Q5
Control/hypothetical 20k	0.85	0.86	0.85	0.80	0.68
Rule-based 10K	0.81	0.84	0.77	0.67	0.63
Rule-based 20K	0.94	0.87	0.84	0.89	0.70
Discretionary	0.97	0.97	0.91	0.85	0.69
	Panel D: Community leaders' relative ranking (of economic status)				
	Among the poorest	Poorer than many	About average	Richer than many	Among the richest
Control/hypothetical 20k	0.93	0.90	0.66	0.43	0.25
Rule-based 10K	0.87	0.83	0.50	0.56	0.19
Rule-based 20K	0.97	0.91	0.57	0.47	0.13
Discretionary	0.96	0.95	0.74	0.59	0.13

Notes: This table reports the share of households selected to receive cash transfer across quintiles of observable socioeconomic indicators, including consumption expenditure (Panel A and B), wealth (Panel C) and relative economic status of households assessed by community leaders.

quintile receive the transfer. Finally, the results in Panel D show more progressive targeting patterns. For example, about 90 percent of those households deemed to be among the poorest received transfers in the constrained budget while this goes down to 13 percent among those households deemed among the richest. Indeed, the results in Panel D of Table 3 show that the distribution of transfers appears to be more progressive in view of community leaders' assessment of households' economic standing, suggesting that community leaders may be using slightly different definition of poverty and the poor. In a similar experiment, Alatas et al. (2012) argue that community leaders and those involved in community targeting may have a slightly different concept and definition of the poor or the most needy.[6] This can generate important differences in the distribution of transfers across the metrics that community leaders are using and across the conventional measures of poverty and wealth. To test whether community leaders are using slightly different criteria when targeting, we asked them to tell us what the most important reason for why each household was given a specific size of transfer. For those groups in the rule-based targeting, we asked them to choose among the list of five criteria we gave them while for those groups, which were given discretion to come up with their own criteria, we mapped their responses across a comprehensive list of criteria we identified.

The patterns in Table A2 shows that there are not significant differences in the criteria

[6]Community leaders may also target based on expected benefits or returns from the transfers as shown by Basurto et al. (2020).

across those three arms which were given rules and criteria, with difficulty of food needs being the most important followed by lack of assets (land and livestock). Interestingly, in the discretionary group, community leaders added new criteria, which appeared to be important to justify transfer to specific households, including lack of able-bodied members and household size (see Table A2, Appendix A). This suggests that community leaders are more likely to use discretion to add new targeting criteria based on which they allocate transfers.

In Figure 6 we present the distribution of baseline (pre-transfer) consumption for beneficiary and non-beneficiary households across the four targeting approaches. While the distribution of consumption for beneficiary households appears similar across the targeting methods, non-beneficiary households exhibit higher consumption under the discretionary targeting. Two mechanisms may explain this pattern. First, as shown in Figure 4 the discretionary targeting led to inclusion of slightly more households, partly because community leaders considered additional targeting criteria as reported in Table A2. This suggests that fewer poor households were left out of the transfers. Second, as we empirically examine below, the discretionary targeting may be more effective in reaching poorer households, implying that most non-beneficiary households are less likely to be poor.

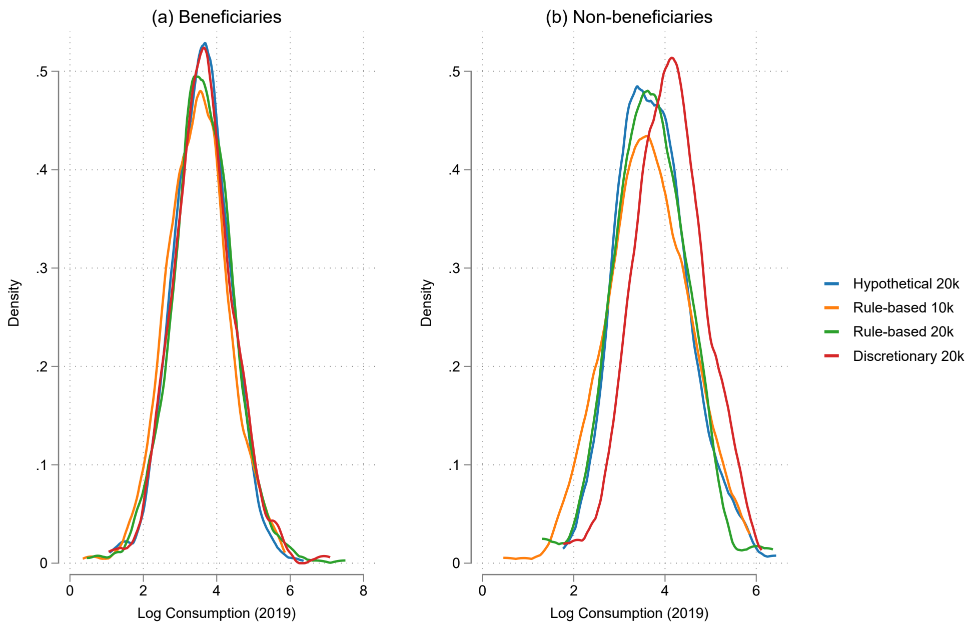

Figure 6: Distribution of consumption (2019) of alternative targeting approaches

We now turn to explicit comparison of the relative performance of variants of community-based targeting approaches using the inclusion and exclusion rates associated with each method. Program implementers may have different objectives to maximize, including minimizing targeting errors. Hence, we calculate both types of errors, namely inclusion and exclusion errors, and compute rates using national poverty lines and consumption values coming from the pre-transfer survey in 2019. We also present these using the post-transfer consumption module in 2023 for comparison purposes. [7] If the transfers increase per capita consumption in the 2023 survey, we expect larger inclusion errors when using the 2023 data compared to the pre-transfer (2019) data. Using both measures of consumption per capita and hence poverty helps to probe the robustness of our computations.

Table 4: Inclusion and exclusion errors across different groups

	(1) Control/ hypothetical 20k Mean/SE	(2) Rule-based 10K Mean/SE	(3) Rule-based 20K Mean/SE	(4) Discretionary 20k Mean/SE	(1)-(2) Pairwise t-test (p-value)	(1)-(3) Pairwise t-test (p-value)	(1)-(4) Pairwise t-test (p-value)	(2)-(3) Pairwise t-test (p-value)	(2)-(4) Pairwise t-test (p-value)	(3)-(4) Pairwise t-test (p-value)
Inclusion error-2019	0.80	0.70	0.80	0.82	0.11	0.92	0.67	0.11	0.07*	0.75
	(0.04)	(0.05)	(0.04)	(0.04)						
Inclusion error-2023	0.77	0.73	0.79	0.85	0.51	0.76	0.13	0.40	0.04**	0.32
	(0.04)	(0.05)	(0.05)	(0.04)						
Exclusion error-2019	0.16	0.25	0.20	0.07	0.20	0.53	0.03**	0.57	0.01***	0.03**
	(0.03)	(0.05)	(0.05)	(0.03)						
Exclusion error-2023	0.13	0.22	0.11	0.06	0.13	0.71	0.13	0.06*	0.01***	0.24
	(0.04)	(0.05)	(0.03)	(0.02)						

Notes: This table reports inclusion and inclusion errors associated with each targeting approach. The values are computed using information on: (i) whether a household was selected to receive transfer by community leaders, and (ii) whether the per capita consumption expenditure for such a household falls below the national poverty line. We compute these inclusion and exclusion rates using national poverty lines and consumption values coming from the pre-transfer survey in 2019 and the post-transfer consumption module in 2023. While the first four columns report inclusion and exclusion rates, the remaining four columns report p-values from pairwise comparisons and t-tests.

In Table 4, we report these inclusion and exclusion errors using the poverty indicators along with access to the program across the different targeting approaches. While the first four columns of the table show inclusion and exclusion errors the remaining six columns provide p-values associated with comparison of the inclusion and exclusion errors for each pair of targeting approaches. The inclusion and exclusion rates reported in Table 4 as well as the comparison across the four targeting approaches highlight some important patterns. First, we note that community leaders appear to be keen to minimize exclusion errors rather than inclusion errors. Indeed, the exclusion error we found is relatively small while the inclusion error appears to be relatively large compared to previous rates in Ethiopia and other countries (Alatas et al., 2012; Brown et al., 2018). For example, Brown et al. (2018) estimated inclusion and exclusion rates associated with PMT targeting across several African countries and report an inclusion error ranging from 50-70 percent. Similarly, Alatas et al. (2012) comparable inclusion rates associated with PMT and community-based targeting, with the former being relatively more effective at identifying the poor. The exclusion rates reports in Table 4 are generally smaller than those reported in other previous studies (Alatas et al., 2012; Brown et al., 2018), suggesting that community leaders in our experiment may be minimizing exclusion error at the expense of inclusion error.

Second, the inclusion and exclusion errors computed using the 2019 and 2023 consumption expenditure are comparable in most of the cases, confirming the robustness of these estimates. Third, we observe significant differences in the performance of the different variants of community-based targeting. The budget constraint is associated with the

[7] Note that both of these datasets have some limitations, the 2019 consumption data being relatively old and the 2023 consumption data being influenced by the transfer itself, although the average effect of the community-based transfer on household consumption appears to be statistically insignificant.

lowest inclusion error but relatively higher exclusion error while the discretionary targeting is associated with the highest inclusion error and lowest exclusion error. This suggests that budget constraints may force community leaders to reduce inclusion error at the expense of increasing exclusion error. More specifically, the discretionary targeting leads to the lowest level of exclusion error, which appears to be 7 percent when using 2019 consumption (poverty) data and 6 percent when using 2023 consumption (poverty) data. But again the discretionary targeting generates the largest inclusion error. As shown in the last few columns, some of these differences in targeting errors are statistically significant, especially when comparing the discretionary targeting with the rest of the others. This suggests that community leaders use their discretion to minimize exclusion errors.

We also assess the distribution of the transfers across households. We do so using aggregate metrics such as Gini coefficient as well as using non-parametric tests of the distribution of transfers across households in communities. We test equality in each pair of distribution of transfers using Kolmogorov-Smirnov tests (Kolmogorov, 1933; Smirnov, 1933). These non-parametric tests (reported in Table A3) suggest that we can reject the null hypothesis of no significant differences between almost all pairs of distributions associated with different targeting approaches. Figure 7 shows a Lorenz curve depicting the relationship between the cumulative distribution of transfers against the cumulative distribution of beneficiaries for each type of targeting approach. The 45 degree line shows equal distribution of resources across all beneficiaries and deviation from this line shows increasing inequality in the distribution of transfers across beneficiaries. Comparing the Lorenz curves associated with the different targeting approaches suggests that budget constraints force community leaders to distribute the transfer unevenly across households. On the other hand, the Lorenz curve associated with the smallest inequality is the one for the discretionary group, suggesting that community leaders use their discretion to minimize inequality in the distribution of the transfers.

We formally test the differences in Gini coefficients associated with alternative targeting approaches in Table 5. Although some of these differences are not statistically significant, we note that budget constraint forces community leaders to be more discriminatory while discretion encourages to reduce inequality in the distribution of transfers across households.

Table 5: Comparison of Gini coefficients from different targeting approaches

	(1) Control/ hypothetical 20k Mean/SE	(2) Rule-based 10K Mean/SE	(3) Rule-based 20K Mean/SE	(4) Discretionary 20k Mean/SE	(1)-(2) Pairwise t-test (p-value)	(1)-(3) Pairwise t-test (p-value)	(1)-(4) Pairwise t-test (p-value)	(2)-(3) Pairwise t-test (p-value)	(2)-(4) Pairwise t-test (p-value)	(3)-(4) Pairwise t-test (p-value)
Gini	0.35	0.39	0.30	0.27	0.47	0.25	0.08*	0.09*	0.02**	0.60
	(0.23)	(0.24)	(0.24)	(0.24)						
Gini=0	0.02	0.05	0.12	0.11	0.43	0.04**	0.05*	0.23	0.27	0.91
	(0.14)	(0.22)	(0.33)	(0.32)						

Notes: This table reports *Gini coefficients* associated with the distribution of the cash transfer within each village and across targeting approaches. While the first four columns report mean values of *Gini coefficients*, the remaining four columns report p-values from pairwise comparisons and t-tests.

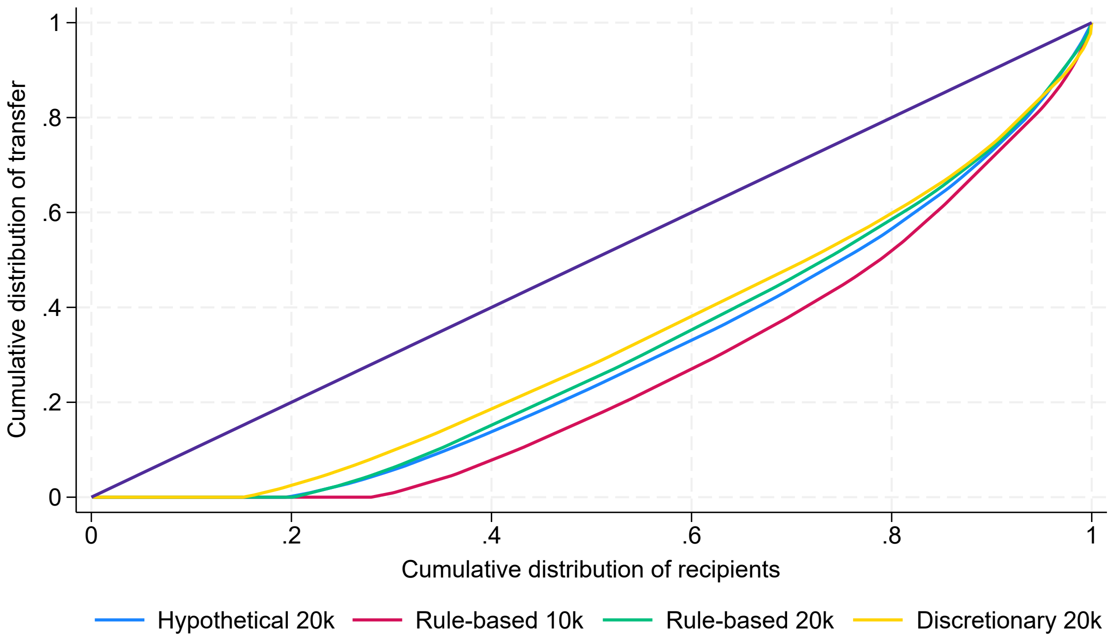

Figure 7: Lorenz curves using alternative targeting approaches

5.2 Effects of Budget Constraint and Discretion

To estimate the effects of budget constraint and discretion, we estimate the empirical specification given in equation 1. This specification allows us to quantify the impact of budget constraint and discretion by comparing them to the rule-based relaxed budget arm. As described in section 5.1, we estimate the impact of budget constraint and discretion on households' access to the transfer, both at the extensive and intensive margins. By estimating impacts at the extensive and intensive margins, we are able to understand whether budget constraints lead to reduction in the number of beneficiaries or size of transfers. This is an important question that allows us to assess the implication of emerging pressures and budget constraints humanitarian organizations continue to face because of the mismatch between the demand for humanitarian assistance and donor funding.

Table 6 shows the coefficients associated with the extensive margin of access to transfer while Table 7 provides impacts on the intensive margin of the distribution focusing on the size of transfer going to those selected to receive the transfer. The results in Table 6 show that halving the total budget allocated to the community leads to only a marginal 8 percentage point (10 percent) reduction in the number of beneficiaries while the effect of discretion appears to be statistically insignificant. If community leaders were keen in maintaining a reasonable size of transfer to each beneficiary, they would have reduced the total number of beneficiaries when they face the budget constraint. On the other hand, the results in Table 7 show that reducing the total budget by half leads to a reduction in the size of transfers going to each beneficiary by about 500 ETB (about 43 percent). This is a large effect on the size of transfers, mainly because community leaders were reluctant to

Table 6: Effects of budget constraints on access to transfers

	(1) Access	(2) Access	(3) Access	(4) Access
Rule-based hypothetical 20k	0.008	0.014	-0.006	0.004
	(0.050)	(0.047)	(0.044)	(0.042)
Rule-based 10k	-0.079	-0.080	-0.093*	-0.083*
	(0.057)	(0.055)	(0.050)	(0.050)
Discretionary 20k	0.049	0.059	0.030	0.033
	(0.053)	(0.049)	(0.045)	(0.041)
Constant	0.799***	0.795***	0.811***	0.860***
	(0.039)	(0.037)	(0.033)	(0.043)
Region FE	No	Yes	Yes	No
Zone FE	No	No	No	Yes
Controls	No	No	No	Yes
R-squared	0.01	0.05	0.13	0.17
Mean of base (Rule-based 20k)	0.80	0.80	0.80	0.80
Observations	3380	3380	3380	3380

Notes: this table reports coefficients on the effects of different variants of CBT on access to the transfers. The dependent variable in all regressions is an indicator variable capturing whether a household is selected to receive a transfer or not. The first column reports results from unconditional regression while we can control for region and zone fixed effect in the second and third columns, respectively. In the fourth column we control for baseline household characteristics. Standard errors, clustered at village level in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

cut the number of beneficiaries.

If community leaders were fully adjusting to a 50 percent budget cut by only reducing the size of transfers going to each beneficiary, keeping the number of beneficiaries unchanged, we would expect a proportional reduction in the average size of transfers to each beneficiary. The 43 percent reduction reported in Table 7 is not too far off the proportional (50 percent) reduction, implying that the main margin of adjustment in response to the budget constraint was reduction in the size of transfers going to beneficiaries. These findings have important implications for our understanding of the extent to which transfer entitlements for each beneficiary can be diluted so as to reach larger number of households under resource constraints. This finding offers important implications for humanitarian organizations and governments in low-and-middle income countries, which are witnessing an ever increasing funding gap to finance humanitarian and social assistance programs. For example, the United Nations Office for the Coordination of Humanitarian Affairs (UN OCHA) estimates that the global humanitarian funding gap was about USD 41 billion in 2023 (OCHA, 2023).[8] Our finding suggests that, for humanitarian organizations facing increasing budget shortfalls and relying on community-based targeting, this translates to reducing the intensity of transfer without significantly reducing the number of potential beneficiaries, which may have important implications to the ultimate impacts

[8]Similarly, low-and-middle income countries are experiencing significant fiscal pressures to finance their responses to recurring crises, including the COVID-19 pandemic and other shocks (Gentilini, 2022).

Table 7: Effect of budget constraint on the size of transfers to households

	(1) Amount of transfer	(2) Amount of transfer	(3) Amount of transfer	(4) Amount of transfer
Rule-based hypothetical 20k	71.312	44.885	87.991	39.138
	(80.391)	(78.400)	(77.150)	(71.558)
Rule-based 10k	-497.200***	-500.788***	-458.718***	-509.580***
	(70.176)	(70.153)	(68.180)	(66.054)
Discretionary 20k	-20.648	-28.844	36.568	29.630
	(82.006)	(82.009)	(81.365)	(73.808)
Constant	1162.841***	1173.463***	1134.910***	1258.900***
	(58.490)	(57.755)	(52.692)	(67.653)
Region FE	No	Yes	No	No
Zone FE	No	No	Yes	Yes
Controls	No	No	No	Yes
R-squared	0.10	0.17	0.26	0.31
Mean of base	1162.84	1162.84	1162.84	1162.84
Observations	2686	2686	2686	2686

Notes: this table reports coefficients on the effects of different variants of CBT on the amount of transfer that is assigned to households. The dependent variable in all regressions is the value of transfer that is assigned to a household. The first column reports results from unconditional regression while we can control for region and zone fixed effect in the second and third columns, respectively. In the fourth column we control for baseline household characteristics. Standard errors, clustered at village level in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

of transfers (Kondylis et al., 2021). Reducing the intensity of transfer may also impact the cost-effectiveness of cash transfer programs as larger cash transfers are arguably likely to improve household welfare more cost-effectively than smaller transfers (Banerjee et al., 2015; Deaton, 1989; Kondylis et al., 2021).[9]

5.3 Effects of variants of CBTs on access to and sizes of transfers in the face of poverty and conflicts

The performance and efficiency of the different CBT approaches can be evaluated by their relative ability to identify and serve poor households or their flexibility to respond to emerging shocks that affect households' well-being. Returning to the results on the performance of different CBT approaches in serving the poor, we rely on the poverty status constructed based on baseline (2019) per capita consumption of households and corresponding national poverty line. We interact a poverty indicator variable with assignment to the different treatment arms to quantify the probability of poor households' access to transfers across the different targeting methods. Table 8 clearly shows that poor households have relatively higher likelihood of access to the transfers under the discretionary

[9]This has been one of the justifications for the literature proposing "Big Push" interventions to support poverty reduction efforts, especially in contexts where poor households suffer from poverty trap (Banerjee et al., 2015).

approach relative to the rule-based approach involving similar budget, suggesting different CBT designs can lead to different targeting and allocation outcomes. More importantly, discretion given to community leaders seems to allow the flexibility to target the poorest. For example, the results in Table 8 show that poor households have about 9-11 percentage point higher probability of access to the transfers in the discretionary targeting approach. We find comparable results when using wealth indicators instead of poverty (see Table A4). Although we are not directly comparing discretionary CBT with PMT or any other standard targeting approach here, given our findings come from a more nuanced or disaggregated CBT designs, it suggests that slight changes in the design of CBT may enable them perform less or better than other measures. In short, different CBT approaches can lead to heterogeneous targeting outcomes depending on poverty status. Thus, CBT designs that allow leaders the flexibility to consider their own criteria, may not necessarily perform less than other approaches.

Table 8: Effects of variants of CBT on access to transfers by poverty status

	(1) Access	(2) Access	(3) Access	(4) Access
Rule-based hypothetical 20k	-0.006	0.000	-0.016	-0.008
	(0.054)	(0.049)	(0.048)	(0.045)
Rule-based 10k	-0.098	-0.098*	-0.098*	-0.086*
	(0.060)	(0.057)	(0.053)	(0.052)
Discretionary 20k	0.019	0.032	0.006	0.006
	(0.060)	(0.054)	(0.050)	(0.046)
Poor in 2019	-0.005	-0.003	0.015	0.007
	(0.045)	(0.043)	(0.041)	(0.037)
Hypothetical 20k × Poor in 2019	0.045	0.043	0.032	0.041
	(0.055)	(0.052)	(0.053)	(0.048)
Rule-based 10k × Poor in 2019	0.055	0.050	0.014	0.015
	(0.067)	(0.066)	(0.059)	(0.055)
Discretionary 20k × Poor in 2019	0.112*	0.098*	0.090*	0.096*
	(0.062)	(0.058)	(0.054)	(0.050)
Constant	0.801***	0.797***	0.807***	0.870***
	(0.040)	(0.037)	(0.035)	(0.043)
Region FE	No	Yes	No	No
Zone FE	No	No	Yes	Yes
Controls	No	No	No	Yes
R-squared	0.02	0.06	0.14	0.17
Mean of base	0.80	0.80	0.80	0.80
Observations	3380	3380	3380	3380

Notes: this table reports coefficients on the effects of different variants of CBT on access to the transfers. The dependent variable in all regressions is an indicator variable capturing whether a household is selected to receive a transfer or not. The first column reports results from unconditional regression while we can control for region and zone fixed effect in the second and third columns, respectively. In the fourth column we control for baseline household characteristics. Standard errors, clustered at village level in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Various CBT approaches may respond differently to variations in the context in which transfers occur. In this section, we explore the role of exposure to armed conflict in households' access to social transfers. Table 9 reports results on how different CBT approaches serve households affected by armed conflict events, more precisely battles that broke out in the last three years.[10] The results in Table 9 show some differences in the way the different CBT approaches respond to conflict. First, an increase in the number of battle events (intensity of conflict) is associated with reduced access to transfers - because of the covariate nature of conflicts, in areas that saw greater conflict intensity, getting resources to the communities would be harder as is reaching those eligible for transfers. As a result, access decreases with increases in conflict intensity. Second, and interestingly, community leaders prioritize conflict-affected households when they are given greater discretion in how to allocate and distribute transfer funds. This may be because community leaders feel more empowered or empathetic and exert more effort to reach potentially deserving or affected households. Indeed, these results are interesting and imply that when given discretion, community leaders may identify those affected by various types of shocks and prioritize them in their targeting processes.[11]

5.4 Vulnerability to Elite Capture and Favoritism

Finally, we evaluate and compare the performance of the different variants of the CBT in terms of their vulnerability to elite capture and favoritism. For this purpose, we estimate equation 4 based on an information set related to the relationship between community members and community leaders. We captured the relationship between the community leaders involved in the targeting exercise and the community members (households) meant to potentially benefit from the cash transfer using the following indicators: (i) whether the household being considered is a member of village *Kebele* leadership, (ii) whether the household is related to village *Kebele* leadership, and (iii) whether the household is a family or friend of the community leaders. Based on these variables, we construct an index using principal component analysis, which captures the level of connection the household has with members of the village leadership. To facilitate interpretation, we standardize this index to assume a mean of zero and standard deviation of 1. We then interact this variable with the treatment assignment and estimate equation (3). The results associated with the intensive margin of access to transfer are given in Table 10 while those related to the intensive margin of access to the transfers are given in Table 11.[12]

The results in Tables 10 and 11 show important patterns on the differential vulnera-

[10] We have also done this analysis for various distance buffers including 5km, 10km, and 20km, and we find consistent results to those reported in Table 9.

[11] We conduct a range of robustness checks of the heterogeneous effect of conflict by varying the way battle exposure is defined. In Table A5 we use battle exposure dummy, and in Table A6 we use a dummy for the highest battle exposure quartile. The findings are consistent with those reported in Table 9.

[12] We also construct a binary indicator variable to capture connection with community leaders and estimate equation 4 by interacting this indicator variable with the treatment assignment. These results are reported in Table A7 and Table A8. These estimates show consistent findings with those reported in Table 10 and Table 11.

Table 9: Effects of variants of CBT on access to transfers by conflict experience

	(1) Access	(2) Access	(3) Access	(4) Access
Rule-based hypothetical 20k	-0.016	-0.003	-0.029	-0.013
	(0.052)	(0.049)	(0.045)	(0.043)
Rule-based 10k	-0.084	-0.090	-0.099*	-0.085
	(0.062)	(0.059)	(0.054)	(0.054)
Discretionary 20k	0.017	0.031	-0.012	-0.005
	(0.059)	(0.053)	(0.047)	(0.042)
Number of battles 15km	-0.004***	-0.003**	-0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)
Hypothetical 20k $\times$ Number of battles 15km	0.005	0.004	0.005	0.004
	(0.004)	(0.004)	(0.004)	(0.004)
Rule-based 10k $\times$ Number of battles 15km	0.002	0.002	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Discretionary 20k $\times$ Number of battles 15km	0.006**	0.005**	0.007**	0.006**
	(0.003)	(0.003)	(0.003)	(0.003)
Constant	0.818***	0.810***	0.820***	0.865***
	(0.040)	(0.039)	(0.034)	(0.043)
Region FE	No	Yes	No	No
Zone FE	No	No	Yes	Yes
Controls	No	No	No	Yes
R-squared	0.02	0.06	0.14	0.17
Mean of base	0.80	0.80	0.80	0.80
Observations	3375	3375	3375	3375

Notes: this table reports coefficients on the effects of different variants of CBT on access to the transfers. The dependent variable in all regressions is an indicator variable capturing whether a household is selected to receive a transfer or not. The first column reports results from unconditional regression while we can control for region and zone fixed effect in the second and third columns, respectively. In the fourth column we control for baseline household characteristics. Standard errors, clustered at village level in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

bility of the different variants of CBT to elite capture and favoritism. Focusing on the estimates related to the extensive margin of access to transfer, three important points are worth highlighting. First, the coefficient associated with connection with leaders indicates that households with some level of connection with community leaders may have slightly better access to the transfer, especially when the community leaders have relatively relaxed budget. For example, one standard deviation increase in the level of connection with community leaders is associated with 4-5 percentage point increase in the probability of receiving the transfer for those households assigned to the rule-based targeting with a relaxed budget (ETB 20,000). This is consistent with the literature arguing that community-based targeting may be prone to elite capture and favoritism (Alatas et al., 2012; Bardhan and Mookherjee, 2005; Basurto et al., 2020; Schüring, 2014; Trachtman et al., 2022)

Second, community leaders appear to be more vulnerable to favoritism when the tar-

Table 10: Vulnerability of variants of CBT to elite capture and favoritism

	(1) Access	(2) Access	(3) Access	(4) Access
Rule-based hypothetical 20k	0.014	0.019	-0.002	0.009
	(0.051)	(0.048)	(0.044)	(0.042)
Rule-based 10k	-0.075	-0.078	-0.086*	-0.077
	(0.058)	(0.056)	(0.050)	(0.050)
Discretionary 20k	0.056	0.066	0.035	0.038
	(0.054)	(0.050)	(0.044)	(0.040)
Connection index (std)	0.049***	0.044***	0.036**	0.037**
	(0.016)	(0.016)	(0.017)	(0.015)
Hypothetical 20k $\times$ Connection index (std)	-0.072***	-0.074***	-0.072***	-0.065***
	(0.027)	(0.027)	(0.024)	(0.022)
Rule-based 10k $\times$ Connection index (std)	-0.082**	-0.085**	-0.065*	-0.065*
	(0.040)	(0.040)	(0.035)	(0.036)
Discretionary 20k $\times$ Connection index (std)	-0.035	-0.036	-0.039	-0.044*
	(0.023)	(0.024)	(0.024)	(0.023)
Constant	0.792***	0.788***	0.804***	0.853***
	(0.040)	(0.037)	(0.033)	(0.043)
Region FE	No	Yes	No	No
Zone FE	No	No	Yes	Yes
R-squared	0.02	0.06	0.14	0.17
Mean of base	0.80	0.80	0.80	0.80
Observations	3380	3380	3380	3380

Notes: this table reports coefficients on the effects of different variants of CBT on access to the transfers. The dependent variable in all regressions is an indicator variable capturing whether a household is selected to receive a transfer or not. The first column reports results from unconditional regression while we can control for region and zone fixed effect in the second and third columns, respectively. In the fourth column we control for baseline household characteristics. Standard errors, clustered at village level in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

geting involves actual transfer of resource to beneficiaries than in the hypothetical case. For example, relative to the rule-based targeting involving relaxed budget, households in the hypothetical targeting with one standard deviation higher connection with community leaders are about 7 percentage point less likely to receive the transfer. In other words, one standard deviation increase in connection and social capital generates 7 percentage points higher probability of inclusion in the transfer in the rule-based targeting involving actual transfer than hypothetical targeting. These results are consistent with those in Table 2, which show that community leaders are likely to behave pro-socially in the hypothetical exercise than the incentivized targeting. This finding resonates with the broader literature documenting strong discrepancy in stated and revealed behavior (Alem, Eggert, Kocher, and Ruhinduka, 2018; Bertrand and Mullainathan, 2001).

Third, budget constraint forces community leaders to be relatively less vulnerable to favoritism. The results in Table 10 show that households with strong connection with community leaders in the constrained budget are relatively less likely to receive the transfer compared to similar households in the relaxed budget. The coefficients associated with the interaction terms between the constrained budget and connection with community leaders suggest that households with one standard deviation higher connections are 7-9 percentage points less likely to receive transfer in the constrained budget, compared to similar households in the relaxed budget.

Table 11: Vulnerability of variants of CBT to elite capture and favoritism

	(1) Amount of transfer	(2) Amount of transfer	(3) Amount of transfer	(4) Amount of transfer
Rule-based hypothetical 20k	70.277***	66.011***	75.696***	66.411***
	(19.352)	(19.674)	(22.573)	(21.800)
Rule-based 10k	-447.194***	-451.996***	-438.394***	-444.595***
	(12.094)	(12.598)	(14.351)	(17.124)
Discretionary 20k	45.249**	40.080*	49.307**	48.393**
	(20.686)	(20.508)	(22.744)	(23.107)
connection_std	22.529	16.972	20.360	38.895**
	(15.830)	(16.162)	(17.511)	(16.862)
Hypothetical 20k × Connection Index (std)	-19.370	-18.734	-29.081	-33.099
	(40.353)	(40.508)	(43.084)	(42.077)
Rule-based 10k × Connection Index (std)	-29.556	-25.957	-29.272	-30.563
	(22.908)	(23.191)	(23.993)	(23.196)
Discretionary 20k × Connection Index (std)	-65.190***	-63.623***	-65.935***	-71.307***
	(23.955)	(24.046)	(25.234)	(25.094)
Constant	925.894***	929.603***	921.076***	1057.948***
	(9.708)	(10.336)	(11.835)	(63.468)
Region FE	No	Yes	No	No
Zone FE	No	No	Yes	Yes
R-squared	0.08	0.08	0.08	0.12
Mean of base	0.80	0.80	0.80	0.80
Observations	3380	3380	3380	3380

Notes. This table reports coefficients on the effects of different variants of CBT on amount transfers to eligible households. The dependent variable in all regressions is amount of transfer assigned to each beneficiary household. The first column reports results from unconditional regression while we can control for region and zone fixed effect in the second and third columns, respectively. Standard errors, clustered at village (kebele) level, are given in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

The results in Table 11 provide two additional insights on the distribution of the transfers using the size of transfer. First, we find that for those households with average level of ”connection”, community leaders are more likely to assign slightly larger transfer to households in the hypothetical targeting, mainly because they take less to themselves to cover administrative costs as reflected in Table 2 . For example, the coefficients associated with the hypothetical targeting show that households with average level of elite connection receive 66-76 Birr higher transfer than comparable households in the rule-based incentivized targeting. This corroborates the findings in Table 10 using the extensive margin of access to transfer. Second, the discretionary targeting led to reduced transfer to those households with strong elite connection compared to the rule-based targeting. For example, one standard deviation increase in connection with community leaders is associated with 64-71 Birr lower transfer in the discretionary group than in the rule-based targeting. Again, this suggests that community leaders may be using their discretion to reach out to those deemed needy by reducing the size of transfers going to those connected with them. This is consistent with those results in Section 5.1, which broadly show that the discretionary targeting led to slightly higher inclusion of poor and conflict-affected.

6 Conclusion

In the absence of up-to-date local information, targeting of social assistance programs proves challenging, particularly in conflict-affected and dynamic settings. Community-based targeting approaches are routinely recommended for their information advantage and cost considerations over those of program implementers. However, in the absence of clear incentives to truthfully reveal and use available information, it is not clear what community leaders - those tasked with targeting and distribution of funds - maximize (minimize) when they allocate resources to beneficiaries. Moreover, community leaders may need a certain degree of discretion to use local information, involving trade-offs between policymakers' control over program designs vis-a-vis gains from local information. Thus, given resource constraints, it is not obvious how community leaders act when given the discretion to use local information to set their own targeting criteria and allocate resources accordingly.

Using a lab-in-the-field experiment, this paper tested these hypotheses by randomly assigning communities (and community leaders) into various community-based targeting designs, which allow us to evaluate how community leaders target and allocate resources under constrained versus relaxed budgets, rule-based versus discretionary targeting, and real versus hypothetical transfers. Using survey data from 180 villages in Ethiopia, and randomized variation in budgets and targeting criteria, we assess the inclusivity, beneficiary composition, and resource leakages of community-based targeting under different settings.

We first provide descriptive insights regarding the basis on which community leaders target beneficiary households across the different CBT approaches, and using different well-being measures. We assess whether the resulting targeting outcomes are progressive (regressive) and the extent of inclusion/exclusion errors in view of the survey-based objective measures against which program designers would have ideally wanted to compare. Descriptive results suggest that targeting and transfer sizes are generally progressive using the survey-based objective measures of well-being, including consumption or poverty and wealth quintiles, but not as progressive as program designers would have liked. Community leaders are keen to minimize exclusion errors even at the expense of increased inclusion errors, suggesting community leaders may be sensitive to potential communal repercussions. However, compared with the objective measures of well-being, targeting appears to be more progressive when viewed from the stand point of the community leaders' (perceived) economic status of community members. These results may rather underscore the differences in the two measures used to assess targeting, suggesting community leaders may be using slightly different and more dynamic targeting criteria, which would not be captured by the commonly used survey-based measures. These findings are consistent with previous findings by Alatas et al. (2012) who pointed out community leaders may differ in the way the conceptualize the "the needy" from that of traditional survey-based measures. Further triangulation of this question using data collected on targeting criteria set under the discretionary arm suggests that community leaders indeed come up with additional set of criteria based on which they target and allocate funds to specific community members. These include the size of the household and availability of able-bodied member in the household, which are important given recent conflicts might have affected households' labor composition and ability to feed their families. This is also evidenced by the fact that compared to rule-based, giving discretion to community leaders to set their own criteria more likely accommodates those affected by conflicts.

Coming to the main empirical results on the performance of variants of CBT approaches (i.e., relaxed vs constrained budget, and rule-based vs discretionary), we find that community leaders are reluctant to decrease the number of beneficiaries in response to reduction in budget sizes, rather they are willing to reduce the size of transfers allocated to each beneficiary. We also note that compared to the rule-based approach, the discretionary approach is more likely to accommodate poorer households, leading to lower exclusion errors, in addition to those affected by conflicts. These findings underscore the importance of community leaders' discretion in mitigating targeting challenges in post-conflict settings while also reducing targeting costs.

A common drawback in CBT approaches is that it may be subverted to serve elite interests, or be driven by nepotism or work along connections. Our study explored some of these features involving some of these hypotheses in our experimental designs. Specifically, when allowed to extract some rents from community funds, say in the form of "administrative costs", we find that community leaders are more likely to withhold a portion of the community funds when stakes are real, and this portion increases with increase in the budgets. We also observe that discretionary targeting may be more pro-social than traditional rule-based approaches. Elite capture may also manifest in the form of connections within the community. Consistent with other findings in the literature, we find that households with some level of connection with community leaders had slightly higher access to the transfers, especially when the community leader have relatively relaxed budgets. But more interestingly, we find that community leaders were more vulnerable to favoritism when: (i) the stakes are real than hypothetical, (ii) budgets are relatively larger, and (iii) communities lack discretion.

Overall, we find important evidence reinforcing that community leaders' local information advantage over program implementers can be harnessed to address some of the targeting challenges in social and humanitarian assistance programs in post-conflict and fragile settings. Among variants of CBT approaches, providing community leaders with some degree of discretion to set out targeting rules (rather than limiting them to predetermined ones) brings important benefits, including lower exclusion errors through accommodating potentially dynamic welfare changes in post-conflict settings as well as providing leaders with incentives to become more pro-social in administering funds, thereby improving overall targeting efficiency and cost-effectiveness.

References

Abadie, A., S. Athey, G. W. Imbens, and J. M. Wooldridge (2023). When should you adjust standard errors for clustering? *The Quarterly Journal of Economics 138*(1), 1–35.

Abay, K. A., M. H. Abay, G. Berhane, and J. Chamberlin (2022). Social protection and resilience: The case of the Productive Safety Net Program in Ethiopia. *Food Policy 112*, 102367.

Abay, K. A., K. Tafere, G. Berhane, J. Chamberlin, and M. H. Abay (2023). Near-real-time welfare and livelihood impacts of an active war: Evidence from Ethiopia. *Food Policy 119*, 102526.

Alatas, V., A. Banerjee, R. Hanna, B. A. Olken, and J. Tobias (2012). Targeting the poor: evidence from a field experiment in indonesia. *American Economic Review 102*(4), 1206–1240.

Alderman, H. (2002). Do local officials know something we don't? decentralization of targeted transfers in Albania. *Journal of public Economics 83*(3), 375–404.

Alem, Y., H. Eggert, M. G. Kocher, and R. D. Ruhinduka (2018). Why (field) experiments on unethical behavior are important: Comparing stated and revealed behavior. *Journal of Economic Behavior & Organization 156*, 71–85.

Bachewe, F. N., G. Berhane, B. Minten, and A. S. Taffesse (2018). Agricultural transformation in africa? assessing the evidence in ethiopia. *World Development 105*, 286–298.

Banerjee, A., E. Duflo, N. Goldberg, D. Karlan, R. Osei, W. Parienté, J. Shapiro, B. Thuysbaert, and C. Udry (2015). A multifaceted program causes lasting progress for the very poor: Evidence from six countries. *Science 348*(6236), 1260799.

Bardhan, P. and D. Mookherjee (2005). Decentralizing antipoverty program delivery in developing countries. *Journal of public economics 89*(4), 675–704.

Basurto, M. P., P. Dupas, and J. Robinson (2020). Decentralization and efficiency of subsidy targeting: Evidence from chiefs in rural Malawi. *Journal of public economics 185*, 104047.

Berhane, G., D. O. Gilligan, J. Hoddinott, N. Kumar, and A. S. Taffesse (2014). Can social protection work in africa? the impact of Ethiopia's Productive Safety Net Programme. *Economic Development and Cultural Change 63*(1), 1–26.

Bertrand, M. and S. Mullainathan (2001). Do people mean what they say? implications for subjective survey data. *American Economic Review 91*(2), 67–72.

Brown, C., M. Ravallion, and D. Van de Walle (2018). A poor means test? econometric targeting in Africa. *Journal of Development Economics 134*, 109–124.

Coady, D., M. E. Grosh, and J. Hoddinott (2004). Targeting of transfers in developing countries: Review of lessons and experience.

Conning, J. and M. Kevane (2002). Community-based targeting mechanisms for social safety nets: A critical review. *World development 30*(3), 375–394.

Deaton, A. (1989). Saving and liquidity constraints.

Devereux, S. (2021). *Targeting*, Chapter 8. Edward Elgar Publishing Limited.

Targeting.

Handbook on social protection systems, p.150.

Devereux, S., E. Masset, R. Sabates-Wheeler, M. Samson, A. Rivas, and D. Lintelo (2017). The targeting effectiveness of social transfers. *Journal of Development Effectiveness*.

Dupas, P., M. Fafchamps, and D. Houeix (2022). Measuring relative poverty through peer rankings: Evidence from Côte d'Ivoire. National Bureau of Economic Research, No. w29911.

Ellis (2017). We are all poor here": Economic difference, social divisiveness, and targeting cash transfers in Sub-Saharan Africa. *Journal of Development Studies*. doi:10.1080/00220388.2011.625408.

FEWSNET (2023). Drought-induced crop failure leads to emergencey in conflict-affected north. Web: https://fews.net/east-africa/ethiopia/food-security-outlook-update/december-2023.

Galasso, E. and M. Ravallion (2005). Decentralized targeting of an antipoverty program. *Journal of Public economics 89*(4), 705–727.

Gentilini, U. (2022). Cash transfers in pandemic times. *Evidence, practices, and implications from the largest scale up in history*.

Gilligan, D. O., J. Hoddinott, and A. S. Taffesse (2009). The impact of Ethiopia's Productive Safety Net Programme and its linkages. *The journal of development studies 45*(10), 1684–1706.

Hanna, R. and B. Olken (2018). Universal basic incomes versus targeted transfers: Anti-poverty programs in developing countries. *Journal of Economic Perspectives*.

Hoddinott, J., G. Berhane, D. O. Gilligan, N. Kumar, and A. Seyoum Taffesse (2012). The impact of Ethiopia's Productive Safety Net Programme and related transfers on agricultural productivity. *Journal of African Economies 21*(5), 761–786.

Karlan, D. and B. Thuysbaert (2019). Targeting ultra-poor households in honduras and Peru. *The World Bank Economic Review*.

Kolmogorov, A. N. (1933). Sulla determinazione empirica di una legge didistribuzione. *Giornale dell' Istituto Italiano degli Attuari 4*, 83–91.

Kondylis, F., J. Loeser, et al. (2021). *Intervention size and persistence*. World Bank.

Lind, J., R. Sabates-Wheeler, B. Carter, and M. Tefera Taye (2024). Conflict disruptions to social assistance in a multi-hazard context: Assessing responses to the northern ethiopia crisis (2020–22). Technical report, Institute of Development Studies.

Maxwell, D., H. Young, S. Jaspars, J. Frize, and J. Burns (2011). Targeting and distribution in complex emergencies: Participatory management of humanitarian food assistance. *Food Policy*.

OCHA (2024). Ethiopia: Humanitarian response plan, 2024. Technical report, United Nations Office for the Coordination of Humanitarian Affairs.

OCHA, U. (2022a). Global humanitarian overview. Web: https://2022.gho.unocha.org/.

OCHA, U. (2022b). Humanitarian response plan ethiopia 2022. Web: https://reliefweb.int/report/ethiopia/ethiopia-humanitarian-response-plan-

2022-july-2022.

OCHA, U. (2023). Global humanitarian overview 2023, october update (snapshot as of 31 october 2023). Web: https://www.unocha.org/publications/report/world/global-humanitarian-overview-2023-october-update-snapshot-31-october-2023: :text=The

Premand, P. and P. Schnitzer (2021). Efficiency, legitimacy, and impacts of targeting methods: Evidence from an experiment in niger. *The World Bank Economic Review 35*(4), 892–920.

Rai, A. S. (2002). Targeting the poor using community information. *Journal of Development Economics 69*(1), 71–83.

Raleigh, C., R. Linke, H. Hegre, and J. Karlsen (2010). Introducing acled: An armed conflict location and event dataset. *Journal of peace research 47*(5), 651–660.

Sabates-Wheeler, R. and C. Szyp (2021). Key considerations for targetig social assitance in situations of protracted crises. BASIC Research Working Paper 12, Brighton: Institute of Development Studies, DOI: 10.19088/BASIC.

Schüring, E. (2014). Preferences for community-based targeting-field experimental evidence from Zambia. *World Development 54*, 360–373.

Smirnov, N. V. (1933). Estimate of deviation between empirical distribution functions in two independent samples. *Bulletin Moscow University 2*(2), 3–16.

Tefera Taye, M., F. Mogus, B. Carter, J. Lind, and R. Sabates-Wheeler (2024). Conflict, displacement, and social assistance in three districts of Ethiopia. BASIC Research Working Paper 27, Brighton: Institute of Development Studies, DOI: 10.19088/BASIC.2024.009.

Trachtman, C., Y. Permana, and G. Sahadewo (2022). How much do our neighbors really know? the limits of community-based targeting. *University of California, Berkeley Working Paper*.

UNFPA (2024). Ethiopia humanitarian response situation report 2024. https://reliefweb.int/report/ethiopia/unfpa-ethiopia-humanitarian-response-situation-report-march-2024: :text=As

A Appendix Tables

Table A1: Role of community leaders in the village

Role in the village	Frequency	Share	Total
Kebele leader/ship	180	16.67	16.67
Elder men/women	179	16.57	33.24
Religious leader	181	16.76	50.00
Teacher	128	11.85	61.85
Development agent	45	4.17	66.02
Health extension worker	8	0.74	66.76
Women representative	180	16.67	83.43
Youth representative	179	16.57	100.00
Total	1,080	100	

Table A2: Most important reasons for allocation transfers to households

	Control/ Hypothetical 20k	Rule-based 10k	Rule-based 20k	Discretionary 20k	
Had difficulty satisfying its food need	57.00	55.81	52.15	Had difficulty satisfying its food need	37.90
Own no or little asset (e.g., livestock)	18.83	17.68	17.65	Own no/only few livestock	10.95
Limited income-generating activities or	12.21	15.25	17.49	Own no/little land	16.28
Lost productive assets due to shocks	6.36	7.45	6.68	Had harvest failure	2.59
Lost family members recently	5.60	3.81	6.04	Lost productive assets due to conflict	1.44
				Sustained damage due to conflict	1.44
				Experience violence due to conflict	0.29
				Lost family members recently	2.45
				Has large household size	7.49
				Lack able bodied household members	12.97
				Woman headed household	4.47
				Other (please specify)	1.73

Table A3: Kolmogorov-Smirnov tests for differences between distributions of transfers

Comparison	**K-S value**	**P-value**
Hypothetical 20k vs Rule-based 10k	0.4807	0.000
Hypothetical 20k vs Rule-based 20k	0.0851	0.004
Hypothetical 20k vs Discretionary 20k	0.1048	0.000
Rule-based 10k vs Rule-based 20k	0.4896	0.000
Rule-based 10k vs Discretionary 20k	0.5293	0.000
Rule-based 20k vs Discretionary 20k	0.0582	0.132

Table A4: Effects of variants of CBT on access to transfers by wealth status (bottom 40%)

	(1) Access	(2) Access	(3) Access	(4) Access
Hypothetical 20k	-0.005	-0.001	-0.019	-0.005
	(0.057)	(0.053)	(0.049)	(0.047)
Rule-based 10k	-0.114*	-0.117*	-0.121**	-0.112**
	(0.063)	(0.060)	(0.056)	(0.056)
Discretionary 20k	-0.020	-0.002	-0.022	-0.016
	(0.066)	(0.061)	(0.053)	(0.049)
Bottom 40% baseline wealth	0.030	0.037	0.034	0.014
	(0.049)	(0.049)	(0.045)	(0.041)
Hypothetical 20k × Bottom 40%	0.028	0.029	0.022	0.013
	(0.066)	(0.063)	(0.061)	(0.060)
Rule-based 10k × Bottom 40%	0.084	0.090	0.068	0.068
	(0.075)	(0.073)	(0.066)	(0.064)
Discretionary 20k × Bottom 40%	0.138**	0.116*	0.100*	0.097*
	(0.067)	(0.067)	(0.060)	(0.057)
Constant	0.789***	0.783***	0.799***	0.862***
	(0.043)	(0.040)	(0.036)	(0.043)
Region FE	No	Yes	No	No
Zone FE	No	No	Yes	Yes
Controls	No	No	No	Yes
R-squared	0.03	0.07	0.14	0.18
Mean of base	0.80	0.80	0.80	0.80
Observations	3380	3380	3380	3380

Standard errors, clustered at village level, given in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A5: Effects of variants of CBT on access to transfers by households' exposure to battle (dummy)

	(1)	(2)	(3)	(4)
	Access	Access	Access	Access
Rule-based hypothetical 20k	-0.029	-0.027	-0.035	-0.002
	(0.058)	(0.055)	(0.048)	(0.046)
Rule-based 10k	-0.069	-0.076	-0.081	-0.068
	(0.074)	(0.073)	(0.063)	(0.065)
Discretionary 20k	-0.019	0.000	-0.029	-0.011
	(0.074)	(0.065)	(0.054)	(0.051)
Battle dummy 15km	-0.044	-0.035	0.026	0.058
	(0.084)	(0.083)	(0.082)	(0.077)
Hypothetical 20k $\times$ Battle dummy 15km	0.099	0.107	0.070	0.008
	(0.105)	(0.097)	(0.095)	(0.090)
Rule-based 10k $\times$ Battle dummy 15km	-0.010	0.000	-0.025	-0.042
	(0.116)	(0.112)	(0.099)	(0.096)
Discretionary 20k $\times$ Battle dummy 15km	0.149	0.128	0.104	0.063
	(0.108)	(0.100)	(0.093)	(0.087)
Constant	0.815***	0.809***	0.803***	0.844***
	(0.044)	(0.043)	(0.036)	(0.044)
Region FE	No	Yes	No	No
Zone FE	No	No	Yes	Yes
Controls	No	No	No	Yes
R-squared	0.02	0.06	0.14	0.17
Mean of base	0.80	0.80	0.80	0.80
Observations	3375	3375	3375	3375

Standard errors, clustered at village level, given in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A6: Effects of variants of CBT on access to transfers by households' exposure to battles (quartile dummy)

	(1) Access	(2) Access	(3) Access	(4) Access
Rule-based hypothetical 20k	-0.038	-0.021	-0.032	-0.016
	(0.051)	(0.048)	(0.043)	(0.042)
Rule-based 10k	-0.118*	-0.121*	-0.120**	-0.101*
	(0.063)	(0.062)	(0.055)	(0.056)
Discretionary 20k	-0.000	0.017	-0.011	0.001
	(0.057)	(0.052)	(0.044)	(0.040)
Last Quartile dummy 15k	-0.181*	-0.175*	-0.059	-0.020
	(0.103)	(0.102)	(0.111)	(0.111)
Hypothetical 20k × 4^{th} quartile dummy 15km	0.223*	0.181	0.115	0.083
	(0.126)	(0.118)	(0.122)	(0.118)
Rule-based 10k × 4^{th} quartile dummy 15km	0.190	0.196	0.120	0.076
	(0.129)	(0.124)	(0.118)	(0.118)
Discretionary 20k × 4^{th} quartile dummy 15km	0.235*	0.203*	0.169	0.121
	(0.121)	(0.114)	(0.114)	(0.111)
Constant	0.835***	0.830***	0.823***	0.868***
	(0.038)	(0.037)	(0.032)	(0.042)
Region FE	No	Yes	No	No
Zone FE	No	No	Yes	Yes
Controls	No	No	No	Yes
R-squared	0.02	0.06	0.14	0.17
Mean of base	0.80	0.80	0.80	0.80
Observations	3375	3375	3375	3375

Standard errors, clustered at village level, given in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A7: Effects of variants of CBT on access to transfers by connection with leadership

	(1)	(2)	(3)	(4)
	Access	Access	Access	Access
Hypothetical 20k	0.045	0.053	0.036	0.045
	(0.056)	(0.052)	(0.047)	(0.045)
Rule-based 10k	-0.039	-0.039	-0.058	-0.052
	(0.061)	(0.059)	(0.052)	(0.051)
Discretionary 20k	0.072	0.082	0.053	0.055
	(0.058)	(0.054)	(0.046)	(0.042)
Connected with leadership (dummy)	0.120**	0.108**	0.099*	0.092*
	(0.049)	(0.048)	(0.054)	(0.048)
Hypothetical 20k $\times$ Connected dummy	-0.154**	-0.163**	-0.171**	-0.164**
	(0.073)	(0.071)	(0.069)	(0.066)
Rule-based 10k $\times$ Connected dummy	-0.185	-0.192*	-0.141	-0.121
	(0.116)	(0.115)	(0.104)	(0.110)
Discretionary 20k $\times$ Connected dummy	-0.092	-0.092	-0.095	-0.093
	(0.070)	(0.073)	(0.071)	(0.065)
Constant	0.770***	0.769***	0.786***	0.833***
	(0.044)	(0.042)	(0.035)	(0.045)
Region FE	No	Yes	No	No
Zone FE	No	No	Yes	Yes
Controls	No	No	No	Yes
R-squared	0.02	0.06	0.14	0.17
Mean of base	0.80	0.80	0.80	0.80
Observations	3380	3380	3380	3380

Standard errors, clustered at village level, given in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A8: Effects of variants of CBT on transfer amount by connection with leadership

	(1)	(2)	(3)	(4)
	Amount	Amount	Amount	Amount
Hypothetical 20k	71.160***	68.187***	79.714***	70.770***
	(25.307)	(25.481)	(28.620)	(26.445)
Rule-based 10k	-429.482***	-434.944***	-420.910***	-427.968***
	(15.746)	(16.700)	(18.988)	(19.980)
Discretionary 20k	64.260***	58.952**	64.622**	64.949**
	(24.285)	(24.069)	(25.378)	(25.024)
Connected with leadership (dummy)	43.059	32.101	19.962	57.969
	(37.234)	(37.093)	(44.295)	(41.608)
Hypothetical 20k × Connected dummy	-18.635	-18.703	-27.900	-44.170
	(82.095)	(82.030)	(87.870)	(81.635)
Rule-based 10k × Connected dummy	-100.868*	-94.475*	-112.545**	-117.825**
	(53.556)	(54.804)	(56.998)	(56.470)
Discretionary 20k × Connected dummy	-108.442*	-105.200*	-91.153	-100.441*
	(55.844)	(55.210)	(59.919)	(57.597)
Constant	918.936***	924.342***	919.612***	1050.468***
	(12.230)	(13.173)	(15.963)	(63.120)
Region FE	No	Yes	No	No
Zone FE	No	No	Yes	Yes
Controls	No	No	No	Yes
R-squared	0.08	0.08	0.08	0.12
Mean of base	0.80	0.80	0.80	0.80
Observations	3380	3380	3380	3380

Standard errors, clustered at village level, given in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

ALL IFPRI DISCUSSION PAPERS

All discussion papers are available **here**

They can be downloaded free of charge

INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE
www.ifpri.org

IFPRI HEADQUARTERS
1201 Eye Street, NW
Washington, DC 20005 USA
Tel.: +1-202-862-5600
Fax: +1-202-862-5606
Email: ifpri@cgiar.org