IFPRI Discussion Paper 02392

December 2025

Incentives, Administrative Capture and Preference Aggregation in Community-Based Targeting

Kibrom A. Abay
Guush Berhane
Daniel O. Gilligan
Tensay H. Meles
Kibrom Tafere

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 in the Development Strategy and Governance (DSG) Unit of the International Food Policy Research Institute (IFPRI0, Washington, DC.

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

Dan Gilligan (d.gilligan@cgiar.org) is a Senior Research Fellow and Director of IFPRI's Poverty, Gender, and Inclusion (PGI) Unit, Washington, DC.

Tensay H. Meles (tensay.meles1@ucd.ie) is Assistant Professor of Environmental Economics & Policy in the UCD Environmental Policy of the University College Dublin, Ireland.

Kibrom Tafere (ktafere@worldbank.org) is an Economist in the Sustainability and Infrastructure Team of the Development Research Group of the World Bank, Washington, DC.

Abstract

Community-based targeting (CBT), which leverages community leaders to identify eligible beneficiaries, is widely used in social protection programs and development interventions, especially in data-scarce settings. Yet, little is known about how these leaders respond to opportunities for potential resource leakages and elite cap-ture, and whether such behavior is moderated by budget constraints or the level of discretion given to leaders. Similarly, how community leaders involved in CBT aggregate individual preferences into collective decisions remains understudied. We conduct a cluster-randomized experiment in 180 Ethiopian villages to study the effects of incentive structures and discretion on administrative capture—defined as funds retained under the disguise of covering "administrative" costs. Local leaders were tasked with allocating real or hypothetical transfer budgets, with discretion to retain up to 10 percent as "administrative costs". To uncover decision-making and preference aggregation within CBT committees, we elicited these decisions (proposals to retain a portion of the budget) individually as well as collectively. We find that financial incentives significantly increase administrative (elite) capture, and that capture increases with budget size. Group decisions yield higher appropriation than individual proposals, suggesting implicit collusion rather than prosocial restraint in group-based decisions. Moreover, when real stakes are at play, group outcomes are disproportionately shaped by extreme (outlier) preferences, whereas in hypothetical settings without actual transfers, popular preferences dominate. These findings highlight behavioral mechanisms underlying collective decision-making and administrative capture in CBT, which can inform the design of more accountable CBT systems.

Keywords: Community-based targeting, social protection, resource leakage, elite capture, Ethiopia

Acknowledgments

This work was undertaken as part of the CGIAR Research Program on Food Frontiers and Security. We would like to thank all funders who supported this research through their contributions to the CGIAR Trust Fund: https://www.cgiar.org/funders/. This research experiment is registered as AEARCTR-0012677 in the American Economics Association's RCT registry.

1 Introduction

Accurately identifying eligible beneficiaries is a central challenge in the design and delivery of social protection programs in low-and middle-income countries (LMICs), where large informal sectors and limited administrative data hinder effective targeting. Billions of dollars are spent annually on social protection and humanitarian aid in these settings (ILO, 2021; Banerjee et al., 2024), yet the effectiveness of and returns to such investments heavily depend on targeting methods that can direct scarce resources to those most in need (Coady et al., 2004). This challenge has become even more pressing amid growing humanitarian demand and recent donor funding cuts, which have widened the gap between available resources and needs (Kohnert, 2025).[1] Despite the potentially high returns to improved targeting (Coady et al., 2004; Haushofer et al., 2025), implementation remains a daunting task, particularly in the dynamic and rapidly changing environments of low-income countries, which are frequently affected by conflicts, droughts, natural disasters, and pandemics (Beuermann et al., 2025; Alatas et al., 2012; Abay et al., 2023, 2024). In such contexts, traditional targeting methods such as income-based criteria or proxy means tests are often infeasible or unreliable (Banerjee et al., 2024).

Against this backdrop, community-based targeting (CBT) is widely used to identify potential beneficiaries of social protection programs, especially in data-scarce settings and rapidly changing environments (Alatas et al., 2012; Alderman, 2002; Trachtman et al., 2025; Basurto et al., 2020). CBT leverages local information to identify context-specific deprivation, using community leaders or local committees as targeting agents, with the goal of directing resources to those most in need. Moreover, local committees and community leaders are frequently involved in other forms of delegated decision-making and allocation, including for screening of borrowers (Vera-Cossio, 2022) and distribution of agricultural subsidies (Basurto et al., 2020). However, CBT and related decentralized allocations may be prone to local elite capture and subverted to serve private interests (Basurto et al., 2020; Premand and Schnitzer, 2021; Alatas et al., 2019a; Bardhan and Mookherjee, 2005; Conning and Kevane, 2002a; Platteau et al., 2014; Conning and Kevane, 2002b). In this context, donors and program implementers face important trade-offs between having to rely on local leaders to circumvent information asymmetry and the risk of elite capture and self-serving behavior that may arise in the absence of sufficient monitoring of the actions of community leaders (Conning and Kevane, 2002a; Platteau et al., 2014). Means of circumventing elite capture, especially in low-income countries where monitoring and verification is costly and logistically challenging, remain an active area of inquiry (Beath et al., 2018; Fiala and Premand, 2025). Indeed, the optimal level of

[1] For example, the recent cut to foreign aid by the United States, the world's largest humanitarian aid provider (BBC, 2025), has further strained available resources, increasing the urgency of allocating limited funds to those most in need. Even prior to this cut, the global humanitarian funding gap was estimated at approximately US$41 billion in 2023 (UN OCHA, 2023).

decentralization and monitoring needed in the implementation of CBT remains underexplored (Simons, 2022; Fiala and Premand, 2025; Bourguignon and Platteau, 2025).[2] Similarly, the response of local leaders and committees to alternative incentive regimes and constraints, including budget constraints, levels of discretion, and incentives for opportunistic behavior, warrants empirical investigation to effectively understand and address elite capture as well as associated targeting inefficiencies. Finally, how local committees aggregate heterogeneous individual members' preferences into collective decisions under different incentive structures and in the absence of robust verification and accountability mechanisms remains poorly understood, especially in rural Africa. For example, it is not clear how internal power dynamics play out in CBT group decisions, whether powerful individuals sway decisions, or how preferences are aggregated to group-level decisions.

This paper investigates how CBT functions in practice, with a particular focus on local administrative capture by elites and with the objective to understand: (i) how community leaders respond to alternative incentive structures and budget constraints, and (ii) preference aggregation processes of local committees in low accountability settings common to many low-income countries. We conduct a randomized field experiment that exogenously varies the incentive structure, stake size, and decision-making framework to test how community leaders respond to these design features, and where there may be potential opportunities for local elite (administrative) capture. Measuring administrative capture while reflecting practical CBT implementation requires integrated and flexible designs that involve decision-making at different levels. Thus, our study introduces a feature that offers community leaders the flexibility to retain up to 10 percent of total community funds as "administrative costs", creating a scope for examining the potential for administrative capture under CBT when allocating resources under limited monitoring by program implementers. To uncover how community leaders aggregate individual preferences into collective decisions, we elicit individual preferences (and pro-social tendencies) as well as collective decisions of committees. We implement a community-level cluster-randomized experiment across 180 villages in Ethiopia, a setting recently affected by political unrest and recurrent conflict, which has left millions in urgent need of emergency and social assistance. The experimental design allows us to track both individual preferences and negotiated collective decisions among community leaders regarding the share of the transfer budget retained for themselves under different incentive regimes and budget scenarios.

More specifically, we introduced four treatment arms that vary along three dimensions: (i) real financial stakes versus hypothetical budgets; (ii) strictly-defined targeting criteria versus discretionary targeting that allows community leaders to establish the targeting criteria; and (iii) constrained versus relaxed budgets. The resulting four treatment arms

[2]This is surprising given the ubiquitous use of local committees for delivering public goods and services in rural areas across Africa.

are: (1) a rule-based and incentivized targeting involving allocation of 20,000 Ethiopian Birr; (2) a rule-based allocation with a hypothetical 20,000 Ethiopian Birr budget; (3) a rule-based and incentivized allocation of 10,000 Ethiopian Birr; and (4) a discretion-based and incentivized allocation of 20,000 Ethiopian Birr.[3]

The design of our treatment arms is motivated by four key considerations of targeting practices. First, governments in LMICs face increasing fiscal pressure to sustain social protection programs (Almenfi et al., 2020; Gentilini et al., 2025), which reinforces the urgency to optimally allocate limited resources across potential beneficiaries. Hence, we compare targeting under two different budget scenarios:: contrained versus relaxed. Second, although program implementers typically set eligibility criteria (e.g., chronic food insecurity or poverty) (Conning and Kevane, 2002a; Berhane et al., 2013; Asri et al., 2022; Schnitzer and Stoeffler, 2024a), limited monitoring and verification mechanisms may render these criteria irrelevant and ineffective, which usually translates to different forms and levels of elite capture. If community leaders have more updated information about local conditions and relevant criteria, providing some discretion to community leaders may enhance the accuracy of targeting processes. Discretion may introduce opportunities for local elite (administrative) capture, but this mimics real-world scenario and applications.[4] Third, the hypothetical rule-based treatment arm provides a benchmark to understand how community leaders would allocate resources and make targeting decisions in the absence of real financial constraints and limited private incentives to leaders, reflecting decisions made in an ideal setting guided solely by the prescribed criteria.[5] Fourth, committee members involved in targeting of programs come with heterogeneous preferences, for which reason we elicit both individual preferences from each committee member and collective decisions taken and implemented. We build on collective bargaining models to explain collective decision-making processes and preference aggregation in CBT (Duggan et al., 2000; Ambrus et al., 2015).

While much of the literature on CBT focuses on inclusion or exclusion errors in targeting (e.g., Brown et al., 2018; Alatas et al., 2019a; Banerjee et al., 2024; Basurto et al., 2020; Muralidharan et al., 2025), we focus on a more subtle and often overlooked form of elite capture—that is, administrative capture by way of misappopriated funds during implementation under CBT. Specifically, we define administrative capture as community leaders' tendency to favor private interests when allocating public resources. In each treatment arm, community leaders were asked to report how much of the assigned budget they retained to cover "administrative costs".

[3]Birr is the Ethiopian currency. At the time of the data collection for this study, US$1 ≈ 55.5 Birr.

[4]The contrast between rule-based and discretionary targeting forms the basis of our comparison between targeting outcomes across different decision environments.

[5]However, even in hypothetical scenarios, social pressure, community dynamics, or reputational concerns may influence decisions, potentially leading to deviations from ideal rule-based allocations (Charness and Sutter, 2012). Furthermore, the absence of real stakes may weaken incentives for careful screening, resulting in less time and effort devoted to accurately identifying eligible beneficiaries.

We summarize our key findings as follows. First, we find that incentive regimes and stake sizes affect tendencies for administrative capture. Community leaders keep more funds under the guise of covering "administrative costs" when allocations involve real financial stakes, particularly when the overall budget allocation is relatively larger. Second, although one might expect group deliberation to curb self-serving behavior, collective decisions lead to greater appropriation than individual ones, suggesting that group behavior may lead to reduced accountability or preference for personal gain, particularly when individual actions are not observable to the community, and thereby limit prosocial restraint (Zlatev, 2016). Administrative capture averages about 10 percent of the fund in the relaxed-budget treatment, but declines significantly when incentives are removed or budgets are constrained. Third, preference aggregation among committee members varies across incentivized and hypothetical targeting: in the presence of real stakes, extreme voters drive collective decision while popular votes and preferences dominate collective decisions in the absence of real incentives. In the absence of real financial stake, collective decisions reflect convex combinations of individual preferences (weights), while such aggregation fails to hold in the presence of real financial incentives. Finally, while formal tests of irrelevance of extreme preference can be easily rejected in the incentivized targeting process, extreme preferences appear to be irrelevant in the hypothetical targeting exercise.

Our findings contribute to the growing, albeit broader, literature on targeting approaches and implementation challenges in social protection programs. Most existing studies focus on comparing the performance of different targeting methods, such as community-based targeting versus proxy means tests (Alatas et al., 2012; Brown et al., 2018; Alatas et al., 2019a; Basurto et al., 2020; Beuermann et al., 2025; Premand and Schnitzer, 2021; Schnitzer and Stoeffler, 2024b). In settings where local community leaders often have better information than central governments, particularly in fragile environments affected by conflict or natural disasters, CBT is likely to remain a dominant approach. However, there is little evidence on the effectiveness of CBT and on community leaders' response to opportunities for administrative capture or leakage. Our study provides a more nuanced understanding of local CBT implementation by examining how community leaders allocate funds under varying financial incentive structures and decision-making frameworks. We document an overlooked yet policy-relevant form of administrative capture: appropriation under vaguely defined administrative allowances. This form of administrative capture may not be perceived as necessarily illegal or overtly biased, but emerges from incentive-driven behavior in low-accountability settings. By shifting focus from who receives benefits to how funds are allocated and managed by local actors, our study complements existing work on targeting mechanisms with new insights into the behavioral dynamics of local implementation.

Our study also contributes to the literature aiming to explain collective decision-

making and choices of groups (Duggan et al., 2000; Ambrus et al., 2015; Bandiera and Levy, 2011) as well as the experimental literature comparing individual and group decision-making processes and associated aggregation of individual preferences within groups (Levy, 2007; Charness and Sutter, 2012; Ambrus et al., 2015; He and Villeval, 2017; Tan, 2021). A common critique of experimental studies, particularly those conducted in laboratory settings, is their limited external validity due to small stakes and student samples. In contrast, our field setting involves relatively large financial incentives and local community leaders allocating substantial funds to eligible beneficiaries, with a binding group decision following individual proposals. This context provides an opportunity to examine not only whether group decisions differ from individual ones but also how individual preferences are negotiated and aggregated into collective outcomes under varying stake sizes, incentive structures, and decision-making frameworks. Although the debate on whether individuals' or group decisions are self-serving remains unsettled, our findings corroborate that groups engage in more self-interested and self-serving behavior than do individuals (Charness and Sutter, 2012), and more so, when there are real incentives and higher stakes.

The remainder of the paper is organized as follows. Section 2 describes the study context and experimental design. Section 3 presents the data and descriptive statistics. Section 4 outlines the empirical strategy. Section 5 reports the main results, while Section 6 concludes.

2 Context and Experimental Design

2.1 Context

This study is conducted within the setting of rural households in Ethiopia, who are vulnerable to a range of shocks, including violent conflicts, climate-induced disasters, and inflationary pressures (UN OCHA, 2024; UNFPA, 2024; Tefera Taye et al., 2024). Moreover, Ethiopia's recent conflicts have displaced millions and exacerbated the impacts of recurrent drought (FEWSNET, 2023).[6] These compounded challenges have left millions of households in a state of chronic poverty and food insecurity, necessitating robust social and humanitarian assistance mechanisms to mitigate these adverse impacts.[7]

In response to these vulnerabilities and recurring shocks, the Ethiopian government, along with development partners, introduced the Productive Safety Net Programme

[6]Climate change has exacerbated the frequency and severity of droughts, leading to crop failures and livestock losses, while conflicts in recent years have displaced millions, disrupted markets, and eroded social cohesion. Similarly, inflationary pressures have increased the cost of living, further straining the resilience of rural households.

[7]For example, in 2022 about 29.7 million people were in dire need of humanitarian assistance (UN OCHA, 2024).

(PSNP) in 2005, which has since become one of Africa's largest social protection program (Gilligan et al., 2009; Berhane et al., 2014a; Hoddinott et al., 2012; Abay et al., 2022). The PSNP aims to protect rural households from falling into poverty by providing predictable cash or food transfers in exchange for labor on public works projects, or through direct support for those unable to work. The program has been instrumental in stabilizing the livelihoods of millions of households, reducing food insecurity, and building community assets such as rural roads, irrigation systems, and soil conservation structures (Berhane et al., 2014a; Hirvonen et al., 2022). The program reaches about 8 million rural people living in food insecure districts and communities in all regions of the country (Gilligan et al., 2009; Berhane et al., 2014b). The PSNP is designed to be scalable and responsive to new shocks so that additional households affected by the shocks can be covered (Gilligan et al., 2009). However, despite its scalability, the program faced resource constraints to expand when disasters hit, with available funding often falling short of required levels to cover all eligible households (Sabates-Wheeler et al., 2022; Lind et al., 2024). Reports indicate that humanitarian agencies are struggling to sustain support for increased humanitarian caseloads in the face of severe resource constraints (UN OCHA, 2022). This is due in part to the scale of these challenges—recent studies show that humanitarian assistance in Ethiopia remained largely patchy, with limited flexibility to accommodate internally displaced people and lacking coordination with the PSNP itself (Lind et al., 2024). This increasing funding gap and scarcity of resources means that program implementers need to adjust their targeting strategies to ensure assistance is allocated to those most in need.

The overlapping humanitarian crises and the widening funding gap also means that the need for effective targeting is more imperative than ever. Thus, Ethiopia is a suitable context to study the subject at hand because it provides a unique opportunity to understand which alternative CBT designs perform better, both in terms of efficiency in reaching deserving households and in view of reducing leakage and elite capture. This surge in the number of people in need of assistance, coupled with resource constraints faced by international aid agencies and development partners amid the increase in major conflicts globally, necessitates a rethink of existing targeting approaches to allocate limited resources more effectively. Despite these staggering conditions, targeting of beneficiaries in the PSNP continues to heavily rely on CBT. In the PSNP, community leaders are delegated the responsibility of identifying the most vulnerable households and the size of transfers going to each household (Hoddinott et al., 2012). This involves constituting a community food security task force who are empowered to identify beneficiaries based on certain criteria provided by program implementers. The criteria used typically include household income, asset ownership, labor capacity, and history of exposure to shocks (Sabates-Wheeler et al., 2022).[8] While this method is praised for its inclusivity and lo-

[8]This task force is composed of formal and informal community leaders as well as representatives of

cal relevance, it is often challenged by issues of power dynamics, resource leakage, and the potential for administrative capture influencing targeting processes, raising concerns about fairness and transparency (Premand and Schnitzer, 2021; Grosh et al., 2022). In the absence of effective monitoring and verification of CBT targeting processes, it is not clear how community leaders respond to opportunities for administrative capture.

This study aims to uncover how community leaders behave in allocating and channeling scarce resources to beneficiaries under various contexts and circumstances. The next section describes the experimental design implemented to study the extent of administrative capture in allocating resources.

2.2 Experimental Design

The experimental design and implementation of the targeting process mimics the processes followed by the PSNP in Ethiopia. Six community leaders were selected from each village to represent various official and social roles: (i) a village leader or official; (ii) an elder (man or woman); (iii) a religious figure; (iv) a women's representative; (v) a teacher or development agent; and (vi) a youth representative. In each village, a list of 20 randomly sampled households was assembled, from which community leaders were asked to identify eligible beneficiaries for a prototype social protection program. Considering this process uniformly, we employ a clustered randomization design at the community level, with 180 communities assigned to four distinct experimental arms. The randomization process considered three primary dimensions: i) whether communities receive actual or hypothetical transfer funds; ii) the degree of discretion granted to community leaders in the selection of beneficiaries and allocation of transfer funds; and iii) the total budget available for distribution among households in the communities. Whether a community receives an actual or hypothetical budget determines treatment assignment—with communities in the former group receiving actual funds amounting to either 10,000 Birr (≈$180) or 20,000 Birr (≈$360).[9] One group of community leaders was instructed to strictly follow a set of pre-defined targeting rules in the identification of potential beneficiaries while another group was given discretion to define the criteria based on which beneficiaries were to be targeted. The resulting treatment/control arms are given in Figure 1 and further described below.

different sections of the community (more details given in Data section). It undertakes its duties relying on community information through consultations in community gatherings as well as appeal mechanisms that are put in place by program implementers. The process of community-based targeting often involves a participatory approach, where community members deliberate and collectively decide which households should be included in the program. This process is often guided by quotas or caps on the number of households that can be supported in a given period, which are determined by the available resources and the program's targeting guidelines (Grosh et al., 2022; Berhane et al., 2014a).

[9]We used the official exchange rate at the time of the survey (November 2024). The exchange rate has since changed significantly.

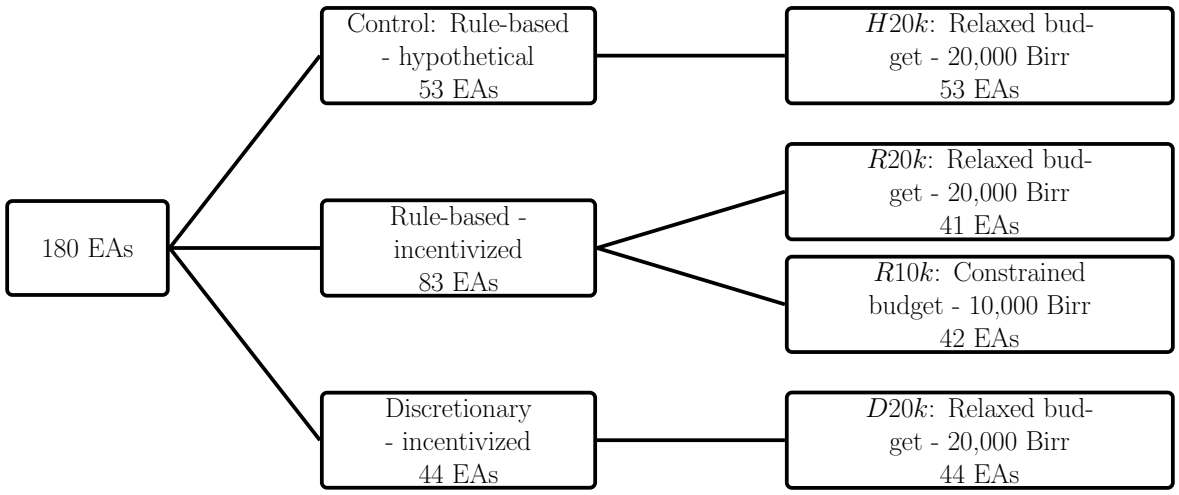

Figure 1: Random assignment of communities across treatment and control arms

Rule-based Hypothetical Targeting ($H20k$)

Communities assigned to this arm did not receive actual transfer. Instead, community leaders were asked to act as if they were in charge of allocating a transfer budget of 20,000 Birr to beneficiaries based on households' need for social assistance. They were asked to follow a set of pre-defined targeting rules, which were designed to mimic targeting criteria used in real-world social protection programs in Ethiopia, such as the PSNP. Specifically, community leaders were asked to prioritize households that: (i) had difficulty satisfying their food needs; (ii) own little to no assets (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. This provides a benchmark for understanding how leaders would allocate resources in the absence of financial constraints or private incentives.

Rule-based Targeting with Relaxed Budget ($R20k$)

This treatment arm is similar to the rule-based hypothetical targeting arm, except the transfer funds are real. Communities in this arm were assigned an actual 20,000 Birr transfer budget. Community leaders were instructed to follow pre-defined targeting rules strictly in their identification of beneficiaries as well as the allocation of funds to selected households. This arm serves as the control group against which we compare the other treatments, varying only one dimension at a time—whether the budget is hypothetical or real, the size of the budget (10,000 vs. 20,000 Birr), or whether targeting is rule-based or discretionary.

Rule-based Targeting with Constrained Budget ($R10k$)

Communities assigned to this arm received a smaller actual budget of 10,000 Birr. Community leaders were asked to identify beneficiaries and distribute the funds based on a pre-determined criteria. This targeting criteria is similar to those in the rule-based hypothetical arm ($H20k$), and mimics the targeting rules used in the PSNP.

Discretionary Targeting with Relaxed Budget ($D20k$)

Communities in this treatment arm received an actual transfer budget of 20,000 Birr. However, unlike the rule-based relaxed budget arm, community leaders were allowed to establish and use their own targeting criteria when selecting beneficiaries and distributing funds. Leaders were encouraged to discuss among themselves to develop targeting criteria they would use in their targeting tasks.

In all treatment arms, community leaders' decision-making protocol involves two stages. In the first stage, each leader independently proposed allocations for how much to keep as "administrative costs" and how much to allocate to beneficiary households in the village. This stage captures individual leader's preferences and decision-making in isolation. In the second stage, which represents the actual binding decision, leaders in each village came together and jointly identified eligible households and decided on the final allocation of funds, including amount to be retained to cover administrative costs as group. This two-stage process provides insights into how individual preferences are consolidated into a collective decision, allowing us to examine group dynamics in the negotiation process, prosocial behavior, and administrative capture across different targeting designs.

Similarly, the targeting of beneficiaries followed a two-tiered process. First, community leaders ranked households in their communities based on the relevant targeting criteria for the arm. Then, they determined who among the ranked households receives transfers. Second, they allocated the available budget—whether real or hypothetical—among selected beneficiary households. Leaders were free to choose as many beneficiaries for the transfers as they saw fit (from among the approximately 20 survey households per community) as well as the transfer amounts distributed to each selected household. Importantly, however, prior to identifying beneficiaries, community leaders were given the opportunity to withhold part of the funds assigned to the community as "administrative cost". This is in addition to payments we made to compensate for their participation in the study. Each member of the committee was given 300 Birr (about $5) in appreciation for their time. The complete outline of the series of decisions taken by community leaders is presented in Figure 2.

After community leaders identified the eligible households and decided on who gets how much, actual transfers were made jointly by the enumerators and the community

leaders. The selected households were informed by community leaders and invited to the village center the following day to collect their transfers. Upon arrival, each beneficiary signed a document confirming receipt of the specific amount transferred.

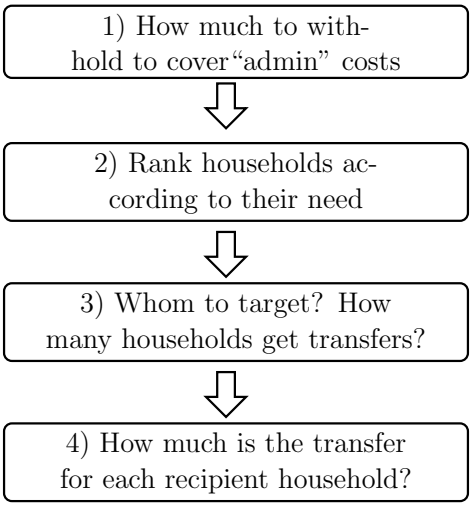

Figure 2: Key decisions made by community leaders

The different groups identified above allow us to test three key hypotheses. First, a comparison between the rule-based hypothetical ($H20k$) and the rule-based targeting with relaxed budget arm ($R20k$) enables us to examine how real stakes shape the decision of community leaders on the share of the transfer funds they withhold for themselves. Since the transfer funds are intended for the poorest, community leaders—typically among the better off in their communities—face a trade-off between financial gains and potential stigma attached to diverting resources meant for the poor. By comparing a treatment arm where such trade-offs are not prevalent (hypothetical) with an arm where they are present (rule-based with relaxed budget), we can understand the choice dynamics that individuals in charge of targeting for social assistance programs face.

Second, we compare the $R20k$ with the rule-based targeting with constrained budget ($R10k$) to explore how the size of incentives affect decision-making and choices of community leaders. By doubling the stakes in $R20k$ from that of $R10k$, we can gauge whether the incentives to withhold funds increase proportionally, as leaders weigh financial losses against conforming to societal norms. This will provide valuable insights for the level of oversight necessary in social assistance programs as budgets vary.

Finally, a comparison of the $R20k$ arm with the discretionary targeting with relaxed budget ($D20k$) arm allows us to study whether and the degree to which granting community leaders discretion in the targeting of beneficiaries impacts the share of the transfer funds withheld as "administrative costs" by leaders. We expect that discretion could empower community leaders and create a sense of responsibility towards community members. It could also allow flexibility to consider other local, context-specific criteria for identifying the poor and improve the effectiveness of the targeting exercise. Alter-

natively, given the low likelihood of being verified by program administrators, discretion could create opportunities for favoritism and elite capture. The comparison between $R20k$ and $D20k$ enables us to study the impacts of discretion on targeting outcomes.

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 districts (*woredas*) in Ethiopia. Sampling followed a two-stage stratified cluster sampling design where, in the first stage, 264 enumeration areas (EAs) were randomly selected from a national census frame of 132 *woredas*, with two EAs per *woreda* chosen using probability proportional to size sampling. In the second stage, 20 households were randomly selected from each of the 264 EAs, resulting in a total sample of 5,280 households.[10] Of the 264 EAs from the 2019 survey, 180 were identified to be accessible for the 2023 experimental study, with the remaining being inaccessible due to ongoing conflicts. These 180 EAs and the sample households from each EA form the basis of our experimental design, and were randomly assigned into four arms (Figure 1). With the aim of understanding and comparing the performance of alternative CBT designs, community leaders from each of the 180 EAs were tasked with targeting and allocating the one-time cash transfer to potential beneficiaries among the 20 selected households selected from each community. 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) an elderly man/woman; (iii) a religious leader; (iv) a women's representative; (v) a teacher, development agent, or extension worker' and (vi) a youth representative. These members are commonly involved in targeting social and humanitarian assistance programs in Ethiopia, such as the PSNP. The composition of community leaders in our sample is reported in Table A.1.

To validate randomization, we tested the balance of observable characteristics of community leaders across the arms using pairwise t-tests as shown in Table 1. These observable characteristics were captured through a survey to community leaders that was conducted before the targeting experiment. Prior to the community-based cash transfer distribution, a detailed community-level survey was administered in each of the 180 EAs to capture a range of community-level characteristics. These tests indicate balance across nearly all characteristics between the control and treatment arms, suggesting successful

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

randomization.

Table 1: Balance test across treatment groups

	(1)	(2)	(3)	(4)	(5)	(3)-(2)	(4)-(2)	(5)-(2)
	All	Relaxed budget 20K (control)	Hypothetical 20K	Constrained budget 10K	Discretionary 20K	Pairwise t-test	Pairwise t-test	Pairwise t-test
Variables	Mean/(SE)	Mean/(SE)	Mean/(SE)	Mean/(SE)	Mean/(SE)	P-value	P- value	P-value
Gender (1=female)	0.19	0.19	0.19	0.17	0.19	0.71	0.30	0.92
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)			
Age in years	41.90	41.55	41.49	42.21	42.45	0.95	0.55	0.42
	(0.37)	(0.83)	(0.69)	(0.72)	(0.76)			
Highest grade completed	8.51	8.56	8.66	8.35	8.45	0.82	0.66	0.83
	(0.17)	(0.32)	(0.33)	(0.34)	(0.39)			
Married	0.89	0.87	0.90	0.89	0.88	0.30	0.50	0.88
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)			
Residence in community (years)	34.28	34.58	34.17	32.77	35.56	0.78	0.29	0.53
	(0.55)	(1.12)	(0.94)	(1.27)	(1.10)			
Participated in targeting of beneficiaries	0.54	0.60	0.49	0.52	0.55	0.05*	0.21	0.46
	(0.02)	(0.04)	(0.03)	(0.04)	(0.04)			
Ranking of own livelihood	0.27	0.26	0.28	0.31	0.24	0.62	0.39	0.74
(1=below average)	(0.02)	(0.04)	(0.04)	(0.04)	(0.03)			
Government should reduce inequality	2.06	2.08	2.06	2.06	2.03	0.86	0.87	0.73
(5 Likert-scale)	(0.05)	(0.09)	(0.08)	(0.09)	(0.10)			
Primary focus of local leaders	0.51	0.49	0.51	0.55	0.51	0.81	0.42	0.82
(1=fairness; 0=max overall)	(0.02)	(0.05)	(0.04)	(0.05)	(0.05)			
Pro-social behavior (index)	0.00	-0.01	-0.10	0.06	0.07	0.69	0.79	0.73
	(0.08)	(0.17)	(0.15)	(0.17)	(0.16)			
Access to electricity	0.49	0.46	0.60	0.43	0.43	0.18	0.75	0.77
	(0.04)	(0.08)	(0.07)	(0.08)	(0.08)			
Access to health center	0.59	0.59	0.60	0.60	0.57	0.86	0.93	0.87
	(0.04)	(0.08)	(0.07)	(0.08)	(0.08)			
Experienced conflict within 10km	0.52	0.46	0.51	0.50	0.59	0.66	0.74	0.24
	(0.04)	(0.08)	(0.07)	(0.08)	(0.07)			
Experienced battle within 10km	0.41	0.34	0.40	0.43	0.45	0.59	0.42	0.29
	(0.04)	(0.07)	(0.07)	(0.08)	(0.08)			
Observations	1080	246	318	252	264	564	498	510
Cluster/Villages	180	41	53	42	44	94	83	85

Note: Table 1 reports means, with standard errors clustered at the village levels given in parentheses, of baseline characteristics across treatment arms. The first column shows full sample mean values while the following four columns report means values across the four treatment arms. The remaining. columns report pairwise comparsions and tests. *** p<0.01, ** p<0.05, * p<0.1

Community leaders were asked about their prior exposure to similar targeting exercises and participation in *kebele* food security task forces (KFSTF), a local administrative structure tasked with the targeting of the PSNP. Panel (a) of Figure 3 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 a KFSTF. The majority of community leaders in our sample (54%) have prior experience in the targeting of social assistance, with 42 percent reporting they are members of the KFSTF of their respective *kebeles*. However, as shown in Figure 3, 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

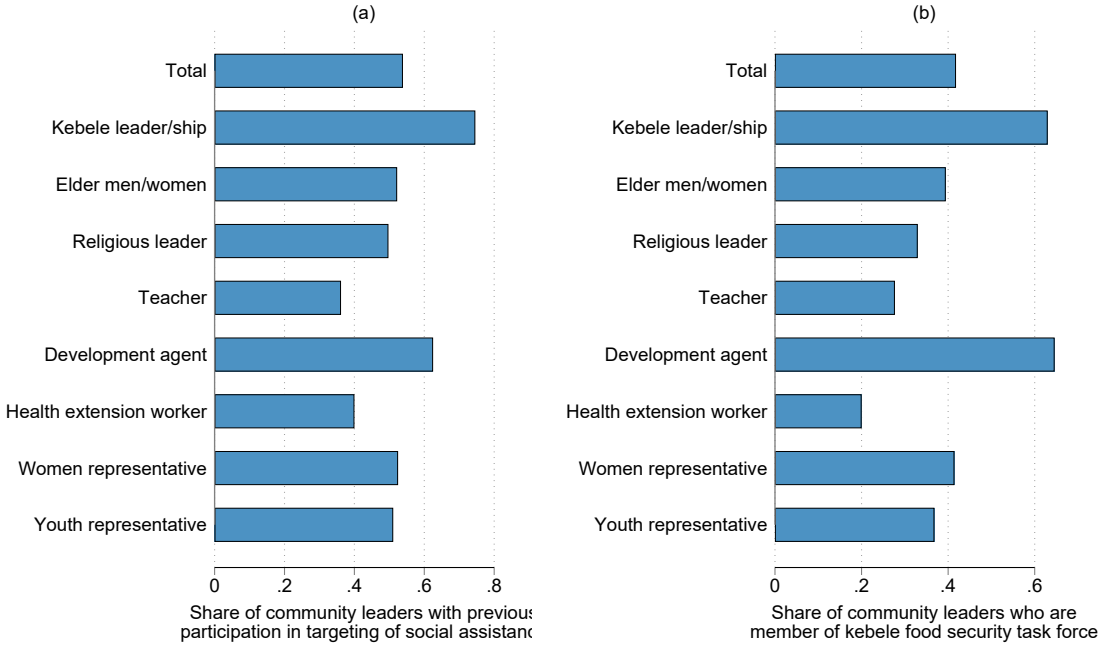

Figure 3: Previous and current targeting experience of community leaders

Figure 3). Similarly, these members are more likely to be current participants in local community targeting groups (panel (b) of Figure 3).

Consistent with the practices in Ethiopian social assistance programming, community leaders in our study identified target beneficiaries by considering individual and local circumstances (see Figure 2). 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 involve trade-offs between one's pro-social behavior of serving the people and the desire to cover private costs, or even tendencies of elite capture and diversion. Such trade-off is often neglected in community-based social transfer studies, the effect of which remains unknown. Platteau et al. (2014) show how local elites in the context of decentralized aid programs may be guided by selfish interest to make their own interests predominate, including as remuneration for leadership roles, leading to elite capture. In this study, we are interested to understand the extent to which *pro-social behavior* or vulnerability to elite capture plays a role in allocating scarce resources to the poor in the context of Ethiopia. To facilitate this, community leaders are given the option to retain a small portion of the allocated funds for themselves as "administrative cost". This could go up to a maximum of 10 percent of the funds in 2 percent increments (0%, 2%, 4%, 6%, 8%, or 10%). That is, after villages are assigned to one of the four control/treatment arms, community leaders decide the proportion of the allocated budget they would like to reserve for themselves as "administrative costs". This allows us to study potential administrative capture and *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 among the better off in their villages. With the exception of community leaders' time and the cost of outreach to potential beneficiaries, there are no major costs associated with the targeting process. By introducing the option to retain a portion of allocated funds for "administrative costs", we are able to learn about the extent to which community leaders would like to misappropriate funds. We note that each member of the committee was given about $4-5 in appreciation for their time, and they were told about this ahead of the targeting experiment.

Following the decision on "administrative cost", community leaders are asked as a group to rank the 20 households in their villages according to their need for social assistance. This step is expected to inform the identification of beneficiaries, and community leaders are expected to converse among themselves regarding beneficiaries' ranking. 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, 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.

3.2 Descriptive Results

We begin by presenting descriptive and non-parametric results. Table 2 shows the first-stage decision of the community leaders: how much of the total budget to withhold for "administrative costs". This table presents the shares of funds withheld for "administrative costs" and those that withheld the maximum possible under each treatment group along with t-test comparisons of each against the control group. 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 resources from the budget allocated to the community, 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 are hypothetical versus real. Third, would the amount community leaders extract respond to budget constraints and degree of discretion provided?

We summarize the results in Table 2. First, when leaders are given the opportunity to allocate community resources with no or minimal supervision, community leaders tend to withhold a portion of the social assistance budget designated for beneficiaries in the community. For instance, the proportion of communities with community leaders revealing interest to cut the maximum permissible "administrative cost" ranges between 45 percent in the hypothetical to as high as 93 percent under the rule-based with relaxed

budget arm. Several factors may contribute to these behaviors, such as the perceived entitlement among community leaders to a share of the transfers and the acknowledgment that implementation activities incur certain costs, including in the form of potential complaints or scrutiny arising from the targeting process.

Second, community leaders demonstrate less pro-social behavior in the real setting compared to the hypothetical. For example, the share of the budget that community leaders withheld ranges from 6 percent in the hypothetical case to 10 percent in the rule-based with relaxed budget (20K Birr). Third, collective decisions involve larger administrative capture than individual proposals, implying that group deliberations attenuate pro-social tendencies reported by individuals. The formal tests of equality of transfer reported at the bottom of Table 2 show statistically significant differences for the full sample as well as almost all treatment arms.

Fourth, community leaders are more likely to increase the share of budget they withhold when the budget increases from 10K Birr to 20K Birr. On average community leaders withhold 8 percent of the budget when they face a constrained budget (10K Birr), but this increases to 10 percent when the budget is relaxed (20K Birr). This clearly suggests that community leaders may increase their share the opportunity arises and become less pro-social when they face a relaxed budget. These patterns are consistent across the extensive and intensive margins 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 those demanding the maximum possible rate shrinks, although these differences are not statistically different compared to the rule-based targeting with relaxed budget.

Table 2: Comparison of administrative cost preferences and allocations across treatment groups

	(1)	(2)	(3)	(4)	(5)	(3)-(2)	(4)-(2)	(5)-(2)
	All	Relaxed budget 20K (control)	Hypothetical 20K	Constrained budget 10K	Discretionary 20K	Pairwise t-test	Pairwise t-test	Pairwise t-test
Variables	Mean/(SE)	Mean/(SE)	Mean/(SE)	Mean/(SE)	Mean/(SE)	P-value	P- value	P-value
Percent wish to take (individual)	7.06	8.28	5.94	6.91	7.39	0.00***	0.01***	0.09*
	(0.21)	(0.29)	(0.44)	(0.41)	(0.42)			
Percent taken (collective)	8.27	9.61	6.49	8.43	9.00	0.00***	0.05*	0.21
	(0.25)	(0.26)	(0.53)	(0.55)	(0.41)			
Wish to take maximum percent allowed (individual)	0.54	0.65	0.40	0.52	0.60	0.00***	0.06*	0.47
	(0.03)	(0.05)	(0.05)	(0.05)	(0.05)			
Maximum amount taken (collecive)	0.74	0.93	0.45	0.81	0.84	0.00***	0.11	0.22
	(0.03)	(0.04)	(0.07)	(0.06)	(0.06)			
P-value for differences in share (individual vs collective)	0.000***	0.000***	0.06*	0.001***	0.000***			
Observations	1080	246	318	252	264	564	498	510
Cluster/Villages	180	41	53	42	44	94	83	85

Note: Table 2 reports means, with standard errors clustered at the village levels in parentheses, of the amount retained as administrative costs (in %) across treatment arms, both individually and collectively. *** p<0.01, ** p<0.05, * p<0.1.

Next, we assess the distribution of access to and amount of transfers across house-

holds. Before making any decision on the allocation of the transfer, community leaders are asked to rank the 20 households within each community according to their need for potential social assistance from the most needy to the least. Following the ranking, community leaders choose "eligible" households to receive transfer and associated amount of transfer. In all treatment arms, community leaders were given the discretion to decide on the number of households who should receive the transfer in each village as well as corresponding amounts of transfers. Table 3 shows the share of households receiving the transfer as well as the amount of transfer going to "eligible" households in each group. The results in Table 3 offer an important insight on how community leaders respond to different incentive regimes and constraints, including budget constraint. Despite the major differences in the design of the treatment arms (including budget and incentive structures), we do not observe significant differences in the share of beneficiary households across the treatment arms, rather than the amount of transfer going to beneficiary households. As shown by Abay et al. (2024), community leaders respond to budget constraint by cutting the size of transfer rather than reducing the number of beneficiaries. Intuitively, this implies that we can assume that the administrative cost of the cash transfers are likely to be similar across treatment groups. Finally, 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 A.1 shows a strong inverse relationship between: (a) access to the transfer and households' rank, and (b) size of transfer to each beneficiary and households' rank. These patterns suggest that community leaders have followed the ranking exercise for distributing the cash to those deemed deserving.

Table 3: Share of beneficiary households and amount allocated across treatment groups

	(1)	(2)	(3)	(4)	(5)	(3)-(2)	(4)-(2)	(5)-(2)
	All	Relaxed budget 20K (control)	Hypothetical 20K	Constrained budget 10K	Discretionary 20K	Pairwise t-test	Pairwise t-test	Pairwise t-test
Variables	Mean/(SE)	Mean/(SE)	Mean/(SE)	Mean/(SE)	Mean/(SE)	P-value	P-value	P-value
Share of beneficiaries	0.79	0.80	0.81	0.72	0.85	0.88	0.17	0.36
	(0.02)	(0.04)	(0.03)	(0.04)	(0.04)			
Average amount allocated per beneficiary	1071.57	1162.84	1234.15	665.64	1142.19	0.38	0.00***	0.80
	(31.25)	(59.02)	(55.49)	(39.11)	(57.95)			
Observations	180	41	53	42	44	94	83	85

Note: Table 3 reports means, with standard errors clustered at the village levels in parentheses, of beneficiaries share and amount allocated across treatment arms. *** p<0.01, ** p<0.05, * p<0.1.

3.3 Measuring "Administrative Capture"

We define administrative capture in the context of social protection programs as the diversion of resources from their intended purpose by administrative structures mainly driven by community leaders' self-interest, an important challenge in CBT (Banerjee

et al., 2024; Muralidharan et al., 2025). We focus on a specific form of leakage: funds captured—that is, retained by those in charge of the administration of these programs—beyond reasonable administrative needs, constituting misappropriation of resources in low accountability settings.[11] This focus is motivated by experiences and reports elite and political capture in the implementation of social safety net programs (Alatas et al., 2019b), including in Ethiopia (Caeyers and Dercon, 2012). More recently, reports of aid diversion (by military and political actors) in humanitarian services in Ethiopia led to suspension of humanitarian aid by major actors (Devex, 2023). These instances and reports suggest that cash or in-kind transfers may not fully reach intended beneficiaries due to the risk of appropriation or capture by village administrators or other local frontline actors.

In our experimental design, community leaders were granted discretion to retain up to 10 percent of the funds as an "administrative cost", creating an opportunity for potential leakage or administrative capture. We leverage this feature of the experiment to analyze how such discretion affects resource allocation and the extent to which local implementers appropriate funds under varying incentive structures in low-accountability environments. Specifically, we define administrative capture as cases where claimed administrative costs exceed a benchmark estimate of implementation costs, capturing a subtle yet policy-relevant form of leakage: the opportunistic overuse of loosely defined administrative privilege.

We compute the size of administrative capture under three alternative definitions and scenarios, reflecting potential measures and indicators of administrative capture of funds in decentralized CBT. In our first scenario, we assume that community leaders have been paid for their time and hence we treat all declared administrative costs as administrative capture. Each of the six community leaders received a 300 Birr ($\approx$ \$5.41) survey participation fee, which amounts to 1,800 Birr in total for each group, mainly for their time spent attending committee meetings for identifying beneficiary households. We assume that this payment compensates for their implementation effort under the CBT exercise. This assumption is plausible given that beneficiaries are supposed to come to the center to collect the funds and given the relatively short distance between the center and household's residence (about a kilometer). In our second and third scenarios, we relax this assumption by constructing benchmarks for "legitimate" administrative expenditures. Potential cost components include communication costs and associated transport costs needed to inform eligible households. Our second scenario allows for additional communication costs (beyond the amount offered as "participation fee") as community leaders will have to call beneficiaries to let them come to the center to collect the transfer. We expect about 10 minutes of mobile call time to inform each identified

[11] This is slightly different from the widely studied form of leakage and elite capture that arise in the form of nepotism where local leaders channel resources to their relatives or favored individuals.

household, including an introductory conversation before conveying the purpose of the call. At Ethio Telecom's peak-hour tariff rate of 0.63 Birr per minute, this results in an estimated communication cost of approximately 6.30 Birr per household.[12] Considering the airtime costs from Ethio Telecom, we assume communication costs of 6.30 Birr per beneficiary, corresponding to 10 minutes of airtime. In our third scenario, we instead account for transport costs incurred when a community leader travels to a beneficiary's residence to deliver the information and invite eligible households to collect their transfers. The transport costs depend on the distance between beneficiary households and the village center. Based on GPS coordinate data collected during the study, the median distance between the center of the village and households' residence is about 1.3 kilometers, which is usually within walking distance or involves the lowest transportation fare. We therefore compute the transport cost as 5 Birr per kilometer, multiplied by average village distance and the number of beneficiaries per village. These scenarios and definitions provide a comprehensive range for quantifying and interpreting administrative capture, allowing us to analyze the sensitivity of our results to different assumptions about the implementation costs of CBT.

The estimated average benchmark administrative cost for covering communication would be about 95 Birr, while the transport cost amounts to about 162 Birr. These are substantially lower than the amounts retained, which average 1,457 Birr across all arms, allowing us to identify self-appropriated surplus beyond reasonable costs. We construct reasonable administrative cost benchmarks for all treatment arms, including the hypothetical treatment arm, to maintain consistency in defining administrative capture. In the hypothetical treatment, where no actual cash transfer occurred, the benchmark represents the estimated costs community leaders would have incurred had the allocation exercise been realized. Administrative capture in this context, therefore, captures counterfactual appropriation behavior, even in the absence of actual financial stakes or implementation responsibilities.

To probe the sensitivity of our computations and interpretations associated with these alternative definitions, we assess whether claimed administrative costs reflect actual implementation effort by examining whether retained amounts (in %) vary with observable factors such as the number of identified beneficiaries and the distance between these beneficiary households from the village center. We find no relationship: the correlation is close to zero and statistically insignificant. However, the amounts retained as "administrative costs" differ systematically across treatment arms. On average, community leaders in the relaxed-budget treatment arm retained 1,922 ETB, compared to 1,298 ETB in the hypothetical arm, 843 ETB in the constrained-budget arm, and 1,800 ETB in the discretionary arm. These correspond to 9.6 percent, 6.5 percent, 8.4 percent, and 9.0 percent of the total allocated funds in each respective treatment arm, with the highest shares of

[12] See, https://www.ethiotelecom.et/getting-started/.

retention found in the relaxed-budget and discretionary treatments.

Table 4: Measures of administrative capture and summary statistics by treatment arms

	(1)	(2)	(3)	(4)	(5)	(3)-(2)	(4)-(2)	(5)-(2)
	All	Relaxed budget 20K (control)	Hypothetical 20K	Constrained budget 10K	Discretionary 20K	Pairwise t-test	Pairwise t-test	Pairwise t-test
Variables	Mean/(SE)	Mean/(SE)	Mean/(SE)	Mean/(SE)	Mean/(SE)	P-value	P- value	P-value
Total claimed administrative costs (in ETB)	1456.67	1921.95	1298.11	842.86	1800.00	0.00***	0.00***	0.21
	(51.24)	(52.67)	(106.07)	(54.76)	(81.74)			
Estimated calling costs (in ETB)	95.16	97.57	95.45	87.00	100.37	0.75	0.15	0.70
	(2.42)	(5.07)	(4.19)	(5.19)	(4.99)			
Estimated transport costs (in ETB)	161.97	134.11	194.71	151.15	158.82	0.12	0.64	0.44
	(13.64)	(22.48)	(31.40)	(28.14)	(22.88)			
Case 1: Administrative capture (in ETB)	1456.67	1921.95	1298.11	842.86	1800.00	0.00***	0.00***	0.21
	(51.24)	(52.67)	(106.07)	(54.76)	(81.74)			
Case 2: Administrative capture (in ETB)	1370.64	1827.45	1215.98	768.61	1705.93	0.00***	0.00***	0.20
	(49.63)	(51.19)	(102.71)	(50.61)	(78.26)			
Case 3: Administrative capture (in ETB)	1316.83	1800.43	1135.18	715.95	1658.56	0.00***	0.00***	0.15
	(50.43)	(57.11)	(101.33)	(54.63)	(77.99)			
Number of observations	180	41	53	42	44	94	83	85

Note: Table 4 reports mean values (with standard errors clustered at village level given in parentheses) for alternative measures of administrative capture across treatment arms. Benchmark administrative costs are estimated based on communication and transport costs required to disburse funds to eligible households. Administrative capture is constructed under three cases: Case 1 treats all declared administrative costs as capture, given that community leaders were paid participation fees for the exercise; Under the second case we compute administrative capture after deducting communication expenses. In our third scenario we compute administrative capture as claimed costs beyond estimated transport expenses. *** p<0.01, ** p<0.05, * p<0.1.

4 Empirical Strategy

4.1 Evaluating the Impact of Incentive Regimes and Stake Size

The empirical strategy of this study exploits the randomized assignment of communities to the hypothetical or one of the three treatment arms involving actual cash transfer, as described in Section 2.2. The four groups allow us to test four key hypotheses: i) the role of incentive regimes (real versus hypothetical stakes) on community leaders' decision-making processes and outcomes, including the amount withheld for themselves; ii) whether and to what extent pro-social behavior and tendencies for administrative capture are sensitive to the size of budgets allocated to communities; iii) whether the amount community leaders withheld as "administration costs" varies by the level of discretion given to community leaders; and (iv) the extent to which individual community leaders' preferences to withheld funds converge or diverge against group preferences across different incentive regimes. We will also exploit the distinction between individual preferences and group decisions on the amount taken as "administration costs" to explore potential mechanisms that underlie the decision process. In this sub-section we focus on testing the first three hypotheses, and the next section addresses the fourth hypothesis.

The primary outcomes of interest are the share of the transfer budget that community leaders indicated they would "extract" for themselves to recover "administrative costs" before they start the distribution of the remaining budget to "eligible" community members. Interestingly, we elicited these preferences individually as well as collectively, which

facilitates comparison across these decisions. Both the individual and collective amounts withheld are measured in percentage points, as a share of the transfer budget allocated to the community, which act as measures and indicators of "leakage" in delivering cash transfers through decentralized CBT. To examine how the randomized assignment to the different treatment arms impacts individual preference to withhold a share of the budget for themselves, we start with the following empirical specification:

$$W_{ic} = \beta_0 + \beta_1 H20k_c + \beta_2 R10k_c + \beta_3 D20k_c + \beta_4 X_{ic} + \gamma_r + \epsilon_{ic} \quad (1)$$

where W_{ic} is the amount community leader i in community c wishes to withhold as an administrative cost, and $H20k_c$, $R10k_c$, and $D20k_c$ are dummy variables indicating whether community c is assigned to the hypothetical, rule-based with constrained budget, or discretionary treatment arms, respectively. The term X_{ic} represents a vector of community leaders' characteristics, γ_r stands for region fixed effects, and ϵ_{ic} is a residual term that absorbs all variations not captured by the treatment dummies and leader characteristics. It is assumed to be orthogonal to the included variables.

In equation 1, the reference group is the $R20k$ treatment arm, which allows us to directly test our key hypotheses. The coefficient associated with $H20k_c$, β_1 measures the differences in the share of budget community leaders wish to withhold between those assigned to the hypothetical group and those assigned to the rule-based with similar stake ($R20k$). A negative β_1 would indicate that community leaders are likely to ask for a larger share of the funds for themselves when the targeting process involves real stakes compared to the hypothetical. The coefficient on $R10k_c$, β_2, measures the impact of change in the size of real incentives on the decision of community leaders, that is, the share of the budget they wish to withhold for themselves. We expect β_2 to be negative to reflect the fact that with higher budget the return to withholding decisions is greater. Finally, β_3 measures the effect of offering discretion in the targeting of cash transfers. If discretion empowers leaders, and hence increases a sense of responsibility, we expect β_3 to be negative. Conversely, if discretion creates adverse incentives and encourages leaders to focus on their self-interest, β_3 will be positive.

As described in section 3.1, the maximum allowable amount that community leaders can withhold for themselves is 10 percent of the transfer budget. Thus, we use both the continuous values as well as binary indicator variables indicating preference for withholding the maximum allowable share. For this purpose, we construct a dummy variable that takes the value 1 if a community leader wishes to take 10 percent and 0 otherwise. We then estimate a similar specification to equation 1 with this indicator as the outcome variable.

After individual community leaders express their preferred share of funds to withhold, they are then guided to conduct a group discussion with their fellow group members to

settle on the actual to be withheld. To examine how incentives and discretion affect the actual amount withheld collectively by community leaders, we estimate the following (aggregated) empirical specification:

$$W_c = \beta_0 + \beta_1 H20k_c + \beta_2 R10k_c + \beta_3 D20k_c + \gamma_r + \epsilon_c \tag{2}$$

where W_c is the actual percentage of transfer funds the group collectively decides to withhold for themselves. All other variables are as defined above.

4.2 Testing Aggregation of Individual Preferences into Collective Decisions

To understand how preferences of individual community leaders aggregate into collective group decisions, we follow Ambrus et al. (2015) and characterize the relationship between the individual-level proposals and group-level decisions (retention rates). This characterization of group decisions builds on Duggan et al. (2000)'s collective bargaining model to explain collective choices and preferences of interest, namely: (i) uncovering the type of aggregation rule that best explains collective decisions, and (ii) understanding and identifying whether such aggregation processes vary across incentive regimes (hypothetical versus incentivized targeting) as well as the size of the stake (overall budget). For this purpose, we conduct simple and parametric regressions with the dependent variable being the percentage of the total budget retained as "administrative costs" in the collective decision (Stage 2). The key independent variables are the six ranked retention amounts from the individual decision (Stage 1), ordered from the lowest (X_{1c}) to the highest (X_{6c}) within each group. W_c denotes the share of the budget retained as administrative costs in the collective decision for community c, and $X_{1c}, \ldots, X_{6c}$ represent the six individual retention amounts, ranked from lowest to highest within community c. The model is specified as:

$$W_c = \alpha + \theta_1 X_{1c} + \theta_2 X_{2c} + \theta_3 X_{3c} + \theta_4 X_{4c} + \theta_5 X_{5c} + \theta_6 X_{6c} + \epsilon_c \tag{3}$$

To formally assess which aggregation rule best explains collective decisions, we conduct several joint hypothesis tests following Ambrus et al. (2015). We begin by testing whether the group outcome corresponds to a convex combination of individual proposals. This involves testing the null hypothesis: $\theta_1 + \theta_2 + \theta_3 + \theta_4 + \theta_5 + \theta_6 = 1$. Another hypothesis relates to the "mean hypothesis", which implies that all members' opinions matter equally, which in turn involves testing $\theta_1 = \theta_2 = \theta_3 = \theta_4 = \theta_5 = \theta_6$. Similarly, we also test the "median hypothesis", which entails testing whether median member's opinion drives collective decisions, which in turn involves testing $\theta_1 = \theta_2 = \theta_5 = \theta_6$=0. Finally, we test whether extreme opinions are ignored in collective decisions by conducting the *extreme*

irrelevance hypothesis, which involves testing the null hypothesis: $\theta_1 = \theta_6 = 0$.

Alternatively, we investigate which individuals are more closely reflected in the group outcome by examining the absolute difference between each individual's proposed retention amount and the final group decision. Following Ambrus et al. (2015), we regress this absolute difference (ΔW_{ic}) on a set of independent variables indicating the relative position of the individual choice within a group (i.e., $p_{ic}^{(1)}, \ldots, p_{ic}^{(6)}$). Formally, this is specified as follows:

$$\Delta W_{ic} = |W_c - X_{ic}| = \alpha_0 + \alpha_1 p_{ic}^{(1)} + \alpha_2 p_{ic}^{(2)} + \alpha_3 p_{ic}^{(3)} + \alpha_5 p_{ic}^{(5)} + \alpha_6 p_{ic}^{(6)} + \epsilon_{ic} \tag{4}$$

The variables $p_{ic}^{(j)}$ denotes the (tie-weighted) relative position of individual i in community c. If it is the unique j^{th} lowest within the group, $p_{ci}^{(j)}$ equals one, and all $p_{ci}^{(j')}$ for $j' \neq j$ are zero. If individuals $i, \ldots, m$ are tied at positions $j, \ldots, j+m$ then all $p_{ci}^{(j)}$ for $j \in \{j, \ldots, j+m\}$ are equal to $1/|\{j, \ldots, j+m\}|$ (that is, number of tied positions), and all other $p_{ic}^{(j')}$ are set to zero. The base category is the relative position of the fourth-lowest proposer, $p_{ic}^{(4)}$, so all estimated coefficients reflect differences in proximity to the group decision relative to this position.

Our randomization involves assignment of villages (and community leaders) into different treatment groups. Community leaders coming from the same community are likely to experience similar shocks, market conditions, and cultural environments, 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 hence recommended level of clustering (Abadie et al., 2023).

5 Results and Discussion

5.1 Treatment Effects on Administrative Capture

This subsection presents treatment effect results of the different CBT designs. As described earlier, community leaders' decisions to retain funds as "administrative costs" were made at two levels: first, each community leader was asked to independently propose how much of the funds to retain and how much to allocate to eligible households; and second, all community leaders were asked to do the same collectively as a group. We first present the average treatment effects on the share of funds retained at each of these levels. We then present the average treatment effects after deducting some reasonable amounts to recover administrative costs, including communication costs for beneficiary outreach.

Table 5 reports average treatment effects both at individual and group levels. Columns

1–2 report estimates for individual community leader decisions across all treatment arms, while Columns 3–4 present the corresponding estimates for collective decisions. The dependent variable is expressed as a percentage of the total funds assigned. Since funds retained means reduced amount of funds available to transfer to beneficiaries, we interpret higher retention as a proxy for less pro-social behavior. In all columns the rule-based, incentivized relaxed-budget (20K Birr) treatment arm serves as the reference group. Specifications progressively include region fixed effects.[13]

Table 5: Average treatment effects on percentage retained as "administrative costs"

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables	Individual decisions:				Collective decisions:			
	Proposed share (%)		Proposed max (10%)		Retained share (%)		Retained max (10%)	
Hypothetical 20K	-2.33***	-2.33***	-0.26***	-0.26***	-3.12***	-3.12***	-0.47***	-0.48***
(Ref: Relaxed-budget 20K)	(0.52)	(0.52)	(0.07)	(0.07)	(0.59)	(0.61)	(0.08)	(0.08)
Constrained-budget 10K	-1.36***	-1.29***	-0.13*	-0.13*	-1.18*	-1.17*	-0.12	-0.12
	(0.49)	(0.46)	(0.07)	(0.07)	(0.61)	(0.61)	(0.07)	(0.07)
Discretionary 20K	-0.88*	-0.81*	-0.05	-0.05	-0.61	-0.55	-0.09	-0.08
	(0.51)	(0.48)	(0.07)	(0.07)	(0.49)	(0.49)	(0.07)	(0.07)
Constant	8.28***	8.24***	0.65***	0.65***	9.61***	9.59***	0.93***	0.93***
	(0.28)	(0.27)	(0.05)	(0.05)	(0.26)	(0.28)	(0.04)	(0.04)
Region fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Mean of base group	8.28	8.28	0.65	0.65	9.61	9.61	0.93	0.93
Adjusted R^2	0.05	0.08	0.04	0.06	0.12	0.12	0.17	0.18
Observations	1080	1080	1080	1080	180	180	180	180

Note: Table 5 reports average treatment effects on (i) the share of funds retained as "administrative costs" and (ii) an indicator equal to one if the maximum allowable share (10%) was retained. Outcomes are measured for individual community leaders (columns 1–4) and collective decisions (columns 5–8). The reference category is the rule-based targeting with a relaxed budget group. Standard errors clustered at the village levels are in parentheses. Results are shown with and without region fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Under standard economic theory, rational and self-interested decision makers are expected to maximize their own benefits. In the context of community-based targeting, that would imply appropriating the maximum allowable share of the funds for personal use. However, empirical evidence suggests that real-world decision-making also reflects social preferences such as fairness concerns, reputational motives, and adherence to social norms, leading some community leaders to retain less than the full 10 percent share (Fehr and Fischbacher, 2002; Fehr and Charness, 2025).

In the hypothetical 20K treatment arm in which no real financial stakes are involved, leaders individually proposed to retain approximately 2.3 percentage points less than those in the control group, across all specifications. With the control group (relaxed-budget 20K arm) mean retention rate of 8.3 percent, this difference suggests that financial incentives induce a shift away from pro-social allocation toward self-serving behavior. In other words, leaders allocate a larger share of the budget to community members when their retention is hypothetical, and more selfishly when retention decisions involve actual stake.

[13] Our preferred specification includes region fixed effects. Overall, the inclusion of spatial fixed effects has little impact on the coefficient estimates.

The constrained budget (10K) treatment arm, which involves real stakes, similarly leads to significantly lower proposed retention than the relaxed budget—by 1.3. This suggests that the size of the financial incentive influences behavior: lower stakes dampen the incentive to appropriate and may intensify competition for resources between selfish and pro-social motives. In contrast, as the material payoff from appropriation increases, the opportunity cost of generosity rises, crowding out prosocial behavior in favor of self-interest and resulting in higher retention under larger stakes (relaxed-budget 20K arm).

The discretionary incentivized 20k treatment arm produced a modest and weakly significant reduction in retention, which becomes statistically insignificant once additional controls are included. This indicates that granting discretion to community leaders appears to moderate self-serving behavior as community leaders may tend to feel responsible, although the effect does not appear to be strong. One possible interpretation is that community leaders, even under discretionary criteria, apply practical criteria similarly to those of their rule-based counterparts, thereby limiting the scope for behavioral divergence.

Columns 3–4 of Table 5 report the treatment effects on community leaders' probability of demanding the maximum allowable administrative costs. These results show broadly consistent patterns. Community leaders in the hypothetical arm are 26 percentage points less likely to request the maximum allowable administrative cost, compared to the benchmark group with real transfers. Similarly, community leaders exposed to the constrained budget are 13 percentage points less likely to demand the maximum administrative cost allowed.

Columns 5–8 of Table 5 characterize collective decisions of community leaders and hence the share of the budget retained as "administrative costs" collectively by community leaders. As described earlier, in the second stage of the decision-making process, the group of six community leaders met and jointly determined the final decision: how much to retain and how much to allocate to eligible households. As in the individual-level decisions, higher retention reflects more self-interest and less pro-social behavior. Similarly, our reference category is the incentivized relaxed-budget 20K treatment arm. The results of the collective decision largely mirror those observed at the individual level. Groups in the hypothetical and constrained-budget treatments retained significantly less than the control group, by 3.1 and 1.2 percentage points, respectively. The impacts of incentives in driving administrative capture appears to be slightly higher in collective decisions than individual-level proposals.[14] The coefficient on the discretionary treatment is again negative but not statistically significant. As with the individual-level findings, a plausible explanation for the null effect is behavioral convergence: leaders in the discretionary arm

[14]We formally test the equality of the coefficients associated with the hypothetical arm across the individual (column 1–4) and collective (column 5–8) decisions and find statistically significant differences, implying that incentives trigger larger increase in administrative capture in collective than individual decisions.

appear to apply targeting criteria similar to those in the rule-based treatments. Appendix Table A.2 shows that 38 percent of leaders in the discretionary treatment cited food insecurity as the primary rationale, with other responses, such as lack of land or labor, closely tracking rule-based guidelines. Limited statistical power may also contribute to the statistical insignificance.

While the direction of treatment effects is consistent across both individual and collective decisions, average retention is higher in the collective decision. For example, in the control group, leaders retained 9.6 percent of the budget collectively, compared to 8.3 percent individually. This pattern suggests that group decision-making facilitated greater self-interested behavior: leaders retained more for themselves and allocated less to eligible households. That is, the collective process appears not to moderate but reinforce leaders' selfish group behavior, potentially by paving the way to free ride and pursue personal gains at the expense of the group, particularly when actions are not individually identifiable(Zlatev, 2016). Clearly, this outcome is contrary to the presumed gains in community-based targeting, including leveraging prosocial behavior in community participation to improve the effectiveness of targeting.

We further compare the incentivized (real stakes) treatments to the hypothetical arm by pooling the three incentivized treatment arms and comparing them with the hypothetical benchmark. Table 6 reports the treatment effects on the percentage of the budget retained as "administrative costs" through individual (Columns 1–4) and collective (Columns 5–8) decisions by community leaders. Relative to the hypothetical task of allocating funds between administrative costs and transfers to eligible households, community leaders retained approximately 1.6 percent more individually and 2.5 percent more collectively when faced with real financial stakes.[15] These differences are robust even when region fixed effects are included, underscoring one of the central behavioral findings of the study: real stakes systematically reduce pro-social behavior and associated allocation. Leaders tend to behave more selfishly when the retention decision involves direct personal financial consequences. This extends previous findings and studies showing significant discrepancy in stated and revealed behaviors in individual decision-making (Alem et al., 2018; Bertrand and Mullainathan, 2001) by demonstrating that such discrepancies hold even in group decisions.

Next, we present results associated with administrative capture after allowing for some reasonable amount of funds to cover administrative costs. Here, we define administrative capture as the share of the assigned budget that is retained beyond reasonable administrative costs. This measure captures a broader form of appropriation and elite capture. Since such behavior directly reduces the share of funds reaching intended beneficiaries, we interpret higher administrative capture as indicative of less pro-social—and therefore

[15] A formal test of differences in these effects across individual and collective differences show statistically significant differences.

Table 6: Average treatment effects on percentage retained as "administrative costs" across incentivized and hypothetical arms

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Individual decisions:				Collective decisions:			
	Proposed share (%)		Proposed max (10%)		Retained share (%)		Retained max (10%)	
Incentivized treatments	1.58***	1.62***	0.19***	0.20***	2.52***	2.54***	0.41***	0.41***
	(0.49)	(0.49)	(0.06)	(0.06)	(0.58)	(0.59)	(0.08)	(0.08)
Constant	5.94***	5.91***	0.40***	0.39***	6.49***	6.47***	0.45***	0.45***
	(0.43)	(0.44)	(0.05)	(0.05)	(0.53)	(0.54)	(0.07)	(0.07)
Region fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Mean of base group	5.94	5.94	0.40	0.40	6.49	6.49	0.45	0.45
Adjusted R^2	0.04	0.07	0.03	0.05	0.11	0.11	0.17	0.18
Observations	1,080	1,080	1,080	1,080	180	180	180	180

Note: Table 6 reports the average treatment effects on (i) the percentage of funds retained as "administrative costs" and (ii) an indicator equal to one if the maximum allowable percentage (10%) was retained, across incentivized and hypothetical treatment arms. The reference category is the hypothetical treatment arm. Standard errors clustered at the village levels are in parentheses. Results are shown with and without region fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

more self-serving—behavior.

Table 7 reports the treatment effects on administrative capture, measured both in absolute terms and as a percentage of the total funds allocated. Columns 1–6 present results in Ethiopian Birr, and Columns 7–12 report results in percentage terms. As before, the relaxed-budget 20K arm serves as the reference group. The results closely correspond with the findings from the collective administrative cost retention decisions reported in Table 5. Administrative capture is significantly lower in the hypothetical and constrained-budget arms relative to the control group (relaxed rule-based 20,000 Birr). Community leaders in the hypothetical arm requested 540 Birr less in administrative capture, while those in the constrained-budget arm extracted approximately 1,078 Birr less. Expressed as a percentage of the assigned budget, these differences amount to 2.7 and 2.5 percentage points, respectively, which are statistically and economically meaningful. These findings reinforce and extend the earlier analysis of administrative cost retention. The consistency across both outcomes suggests that financial incentives systematically reduce pro-social behavior—more so in collective decision-making settings. This has important implications for the design of community-based targeting approaches, particularly in low-capacity environments where enforcement is weak and administrative discretion is high.

Table 7: Average treatment effects on administrative capture

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Administrative capture in ETB						Administrative capture in % of the total fund assigned					
Variables	Case 1		Case 2		Case 3		Case 1		Case 2		Case 3	
Hypothetical 20K	-623.84***	-623.94***	-611.48***	-612.49***	-665.24***	-668.33***	-3.12***	-3.12***	-3.06***	-3.06***	-3.33***	-3.35***
(Ref: Relaxed-budget 20K)	(118.57)	(120.92)	(114.89)	(117.32)	(116.44)	(117.79)	(0.59)	(0.61)	(0.57)	(0.59)	(0.58)	(0.59)
Constrained-budget 10K	-1,079.09***	-1,077.58***	-1,058.84***	-1,057.83***	-1,084.48***	-1,086.38***	-1.18*	-1.17*	-1.45**	-1.45**	-1.84***	-1.86***
	(75.91)	(77.63)	(71.92)	(73.52)	(78.96)	(79.50)	(0.61)	(0.61)	(0.57)	(0.57)	(0.62)	(0.61)
Discretionary 20K	-121.95	-110.53	-121.52	-112.06	-141.86	-134.13	-0.61	-0.55	-0.61	-0.56	-0.71	-0.68
	(97.19)	(97.55)	(93.47)	(93.78)	(96.61)	(95.75)	(0.49)	(0.49)	(0.47)	(0.47)	(0.48)	(0.48)
Constant	1,921.95***	1,918.84***	1,827.45***	1,825.20***	1,800.43***	1,799.89***	9.61***	9.59***	9.14***	9.13***	9.00***	9.00***
	(52.62)	(55.22)	(51.14)	(53.62)	(57.05)	(58.55)	(0.26)	(0.28)	(0.26)	(0.27)	(0.29)	(0.29)
Region fixed effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean of base group	1,921.95	1,921.95	1,827.45	1,827.45	1,800.43	1,800.43	9.61	9.61	9.14	9.14	9	9
Adjusted R^2	0.36	0.36	0.37	0.36	0.38	0.38	0.12	0.12	0.13	0.12	0.14	0.15
Observations	180	180	180	180	180	180	180	180	180	180	180	180

Note: Table 7 reports the average treatment effects on administrative capture in ETB (Columns 1-6) and as a percentage of the total fund assigned (Columns 7-12), estimated with and without region fixed effects. Administrative capture is constructed under three cases: Case 1 treats all declared administrative costs as capture, given that community leaders received participation fees; Case 2 defines capture as claimed costs beyond estimated communication expenses; and Case 3 defines capture as claimed costs beyond estimated transport expenses. Standard errors clustered at the village levels are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

5.2 Individual versus Group Decisions and Preferences

Our results on the share of funds retained as "administrative costs" under individual and collective decision-making reveal substantial variation, with higher amounts retained in collective decisions. This is consistent with prior research showing that groups make more rational, cognitively sophisticated, and self-interested decisions than individuals (Charness and Sutter, 2012). This is contrary to the pro-social behavior often used to justify use of community-based groups to facilitate targeting of social programs. To understand how preferences of individual community leaders aggregate into collective group decisions, we regress the group-level retention rate on the individual retention proposals, following Ambrus et al. (2015). Table 8 presents the regression results. Columns 1 and 2 report estimates for the full sample, with and without controlling for region fixed effects. Columns 3–6 split the sample into the incentivized treatment arms and the hypothetical arm, allowing us to test whether the aggregation process differs when financial incentives are at stake.[16]

The full sample results in Table 8 indicate that extreme preferences—particularly those proposing the highest—play a dominant role in shaping the outcome of the final group. In the full sample, both those proposing the lowest (X_{1c}) and the highest (X_{6c}) are statistically significant, suggesting that group decisions are not simply driven by averages or median preferences, but instead reflect a compromise between the extremes. The coefficient on X_{6c} is particularly large and strongly significant, highlighting strong upward pressure from the group member proposing the highest—potentially the individual with highest preference for self-interest. The constant term is statistically insignificant, indicating no systematic shift in group decisions that is independent of individual proposals. Instead, group outcomes appear to be shaped by the distribution of individual preferences.

When we split the sample by incentive structure, the patterns diverge sharply. In the incentivized treatments, the coefficient on X_{6c} remains large and highly significant, highlighting the importance of extreme proposals in the incentivized targeting. Strikingly, the corresponding coefficient on X_{6c} is almost zero and statistically insignificant in the hypothetical arm. Indeed, in the hypothetical treatment, a more balanced aggregation emerges. The coefficients on mid-ranked members X_{3c}, X_{4c}, and X_{5c} are statistically significant and of similar magnitude, while the effect of X_{6c} is small and insignificant.

These results suggest that in the presence of real financial stakes, group decisions are likely to be driven by preferences for higher self-interest, consistent with predictions of models of strategic bargaining that are skewed towards the upper end of the distribution (Duggan et al., 2000; Charness and Sutter, 2012). To further examine heterogeneity in

[16]Note that the number of observations in specifications with region fixed effects is slightly lower, as one region contains only a single group after aggregating the four treatment arms into two categories (incentivized vs. hypothetical).

Table 8: OLS regressions of group choices on individual choices

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Full sample		Incentivized treatments		Hypothetical treatment	
X_{1c}	0.09*	0.11**	0.07	0.08	0.17	0.25*
	(0.05)	(0.05)	(0.05)	(0.06)	(0.15)	(0.15)
X_{2c}	0.12	0.12	0.16	0.16	-0.07	-0.16
	(0.15)	(0.16)	(0.17)	(0.18)	(0.26)	(0.25)
X_{3c}	-0.07	-0.05	-0.24	-0.22	0.68*	0.66**
	(0.21)	(0.23)	(0.21)	(0.23)	(0.35)	(0.32)
X_{4c}	0.29	0.24	0.59**	0.55*	-0.56*	-0.55*
	(0.24)	(0.24)	(0.30)	(0.30)	(0.33)	(0.30)
X_{5c}	0.07	0.11	-0.69*	-0.66	0.90***	1.01***
	(0.34)	(0.33)	(0.39)	(0.41)	(0.23)	(0.20)
X_{6c}	0.50**	0.49**	1.09***	1.07***	-0.14	-0.20
	(0.23)	(0.22)	(0.17)	(0.17)	(0.13)	(0.17)
Constant	0.35	0.22	0.50	0.51	0.47	0.34
	(0.47)	(0.48)	(0.93)	(1.10)	(0.41)	(0.56)
Region fixed effects	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.53	0.53	0.46	0.44	0.73	0.73
Observations	180	180	127	127	53	53

Note: Table 8 reports OLS regressions of group choices on individual choices, ranked from the lowest (X_{1c}) to the highest (X_{6c}) proposed percentage to retain as "administrative costs". Columns 1–2 present estimates for the full sample, with and without region fixed effects. Columns 3–4 restrict the sample to incentivized treatments, while Columns 5–6 restrict to the hypothetical treatment. Standard errors clustered at the village levels are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

the aggregation process across treatments, we estimate the same regression model separately for each of the four treatment arms. While a fully interacted specification would allow for formal testing of coefficient differences across arms, such a model is difficult to interpret given the number of interactions. Estimating separate regressions by treatment provides a more transparent approach, though at the cost of reduced statistical power due to reduced sample sizes. The disaggregated results, presented in Table A.3 in the Appendix, broadly reinforce the main patterns observed in Table 8 while also uncovering treatment-specific dynamics. Results for the hypothetical treatment (Columns 3–4) are discussed earlier. In the relaxed-budget 20K arm (Columns 1–2), none of the individual coefficients are statistically significant, while the constant is large and positive, indicating a systematic upward shift in group decisions independent of individual choices. This may reflect reduced accountability when actions are not individually identifiable or group members may pursue personal gain at the group's expense, or prioritization of in-group over out-of-group members' benefits especially in high-stake settings (Zlatev, 2016). In contrast, in the constrained budget 10K treatment arm (Columns 5–6), coefficients on

both moderate (X_{4c}) and extreme (X_{6c}) proposals are large and statistically significant, suggesting bargaining over scarce resources drive group outcomes. Finally, in the discretionary 20K treatment arm (Columns 7–8), the influence appears to come from those proposing highest retention (X_{6c}), indicating that even in the absence of formal rules, assertive preferences can dominate. These treatment-specific results reinforce the broader insight that incentive structures, discretion, and resource constraints shape not only individual behavior but also how preferences are negotiated and aggregated into collective decisions.

To formally assess which aggregation rule best explains collective decisions, we conduct several joint hypothesis tests following Ambrus et al. (2015). These results are reported in Table 9. We first discuss the post-estimation test results for the full sample, and then the disaggregated results across the treatment arms. We begin by testing whether the group outcome corresponds to a convex combination of individual proposals. The null hypothesis that the sum of coefficients equals one cannot be rejected ($p = 0.56$) for the full sample, although we observe some important differences across the incentive regimes (hypothetical versus incentivized arms). This supports the view that collective decisions reflect weighted combinations of individual preferences. However, we reject the hypothesis that all six individual preferences have equal influence ($p = 0.01$), known as the weak mean hypothesis, as well as the strong mean hypothesis that each coefficient equals $\frac{1}{6}$ ($p = 0.02$), thereby ruling out simple averaging. We next test whether the group decision aligns with a median-based rule, assigning greater weight to centrally ranked preferences. Both the weak median ($p < 0.001$) and strong median ($p < 0.001$) hypotheses are rejected, indicating that group decisions are not centrally dominated. In contrast, we reject the extreme-irrelevance hypothesis ($p = 0.007$), confirming that both the lowest and highest individual retention choices meaningfully influence group decisions.

When we disaggregate the results and tests across the hypothetical and incentivized arms, some interesting patterns emerge. While the aggregation process in the hypothetical targeting can be characterized by a convex combination of individual proposals, this hypothesis can be easily rejected in those groups involving actual transfers. Similarly, extreme-irrelevance tests cannot be rejected in the hypothetical arm, suggesting that in the absence of financial consequences, group deliberation leads to aggregation rules that give more weight to central preferences. On the other hand, extreme values and preferences appears to be major drivers in the presence of incentives, and hence, we can easily reject that the null of the extreme-irrelevance hypothesis. These patterns show distinct differences in collective decision-making and aggregation processes in the presence and absence of actual incentives.

The regression results in Tables 8 and A.3 show how group decisions are formed by aggregating individual preferences, highlighting the influence of central versus extreme proposals within the group. To explicitly quantify the role of popular opinions under

Table 9: Results from post-estimation hypothesis tests, p-values

	(1)	(2)	(3)
Hypothesis	Full sample	All incentivized	Hypotehtical 20K
No systematic shift:	0.463	0.594	0.253
(H_0: $\alpha = 0$)			
Convex combination:	0.853	<0.001***	0.689
(H_0: $\theta_1 + \theta_2 + \theta_3 + \theta_4 + \theta_5 + \theta_6 = 1$)			
Mean hypothesis:	0.002***	<0.001***	0.011**
(H_0: $\theta_1 = \theta_2 = \theta_3 = \theta_4 = \theta_5 = \theta_6$)			
Median hypothesis:	<0.001***	<0.001***	0.003***
(H_0: $\theta_1 = \theta_2 = \theta_5 = \theta_6 = 0$)			
Extreme irrelevance:	0.031**	<0.001***	0.265
(H_0: $\theta_1 = \theta_6 = 0$)			

Note: Table 9 reports p-values from post-estimation hypothesis tests of alternative aggregation rules across different sample groups. The hypotheses test whether group decisions correspond to a convex combination of individual choices, mean or median-based aggregation, a systematic shift independent of individual choices, or whether extreme preferences are irrelevant. Standard errors clustered at the village levels are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

different incentive regimes, we extend the specification in equation 2 by identifying modal values (within each group) and interacting them with the incentive regimes. As shown in Table A.5, we find that popular opinions are more likely to drive collective decisions in the hypothetical arm than the incentivized arms.

Finally, we investigate which individuals are more closely reflected in the group outcome by examining the absolute difference between each individual's proposed retention amount and the final group decision. Following Ambrus et al. (2015), we regress this absolute difference (ΔW_{ic}) on a set of independent variables indicating the relative position of the individual choice within a group (i.e., $p_{ic}^{(1)}, \ldots, p_{ic}^{(6)}$). Table 10 reports the ordinary least squares regression of the difference between the group and individual choice on indicators for the individual's relative position within the group. These are estimated for the full sample, incentivized and hypothetical treatment arms.[17] This analysis helps to understand whether the distance between the group and an individual's choice systematically varies with their relative position within the group. The coefficients on the relative position indicators capture whether individuals proposing relatively lower or higher retention amounts tend to be closer to or farther from the final collective decision, compared to those in the fourth lowest position (reference position). The constant coefficient reflects the average deviation from the group outcome for individuals in this reference position.

The results in Table 10 provide further insights into how group decisions to retain

[17]We also present separate regressions for each of the four treatment groups in Table A.4. For each case, we present three specifications: a model with only a constant term, and models including relative position indicators, both with and without region fixed effects.

Table 10: OLS regressions of the difference between group and individual choice

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	Full sample		Incentivized treatments		Hypothetical treatment	
Relative position (Ref: $p_{ic}^{(4)}$)						
$p_{ic}^{(1)}$	4.85***	4.85***	5.20***	5.20***	3.65***	3.65***
	(0.55)	(0.56)	(0.53)	(0.54)	(1.30)	(1.31)
$p_{ic}^{(2)}$	2.49***	2.49***	2.56***	2.56***	1.75	1.75
	(0.61)	(0.61)	(0.63)	(0.63)	(1.40)	(1.41)
$p_{ic}^{(3)}$	1.09*	1.09*	1.53***	1.53***	-0.12	-0.12
	(0.60)	(0.60)	(0.57)	(0.57)	(1.42)	(1.43)
$p_{ic}^{(5)}$	-0.53	-0.53	-0.36	-0.36	-0.77	-0.77
	(0.78)	(0.78)	(0.88)	(0.88)	(1.79)	(1.81)
$p_{ic}^{(6)}$	-0.15	-0.15	-1.98**	-1.98**	2.37	2.37
	(0.88)	(0.88)	(0.80)	(0.81)	(1.68)	(1.70)
Constant	0.71	0.71	0.88*	0.88*	0.76	0.76
	(0.52)	(0.52)	(0.48)	(0.48)	(1.28)	(1.29)
Region fixed effects	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.22	0.24	0.29	0.32	0.13	0.14
Observations	1080	1080	762	762	318	318

Note: Table 10 reports OLS regressions of the difference between group and individual choice on a set of relative positions of the individual choice within a group. Regressions are presented for the full sample, incentivized and hypothetical treatment arms, each with and without region fixed effects. The constant term reflects the average deviation from the group decision for individuals in the fourth-lowest position, which serves as a reference position. Standard errors clustered at the village levels are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

funds diverge from those proposed at individual level along the relative positions within the group. Negative coefficients indicate that the individual's proposal is, on average, closer to the final group decision than the reference (fourth-lowest) position, while positive coefficients imply greater divergence. Across all treatment arms, individual choice at the lower end, particularly those in the lowest position ($p_{ic}^{(1)}$), exhibits significantly larger deviations from the group outcome than the reference position. This suggests that more restrained or pro-social proposals are systematically overridden during group deliberations. This pattern is more pronounced in the treatments with substantial financial incentives (Columns 2–3 and 11–12 of Table A.4), where the coefficients are large and statistically significant at the 1% level.

At the opposite end, individuals in the highest relative position ($p_{ic}^{(6)}$)—those proposing the most to retain as "administrative costs"—tend to be significantly closer to the final group decision, particularly in the relaxed-budget and discretionary treatments. This indicates a disproportionate influence of assertive members, whose choices may anchor or dominate collective outcomes. Those individuals with relative positions closer to the reference ($p_{ic}^{(3)}$, and $p_{cic}^{(5)}$) tend to have smaller and less consistent deviations across treatments. These relative positions are somehow far from the group outcome in some

treatments, while in others their influence is limited, and mostly remain statistically insignificant. Overall, these findings provide a complementary perspective to the regression results on the aggregation process (Table 8) by quantifying how far individuals' preferences are from collective outcomes and identifying individuals are overridden or heard in real-world group decision-making under the hypothetical and incentivized community-based targeting processes.

6 Conclusion

Community-based targeting (CBT) is widely used to identify eligible beneficiaries in social assistance programs in low-income countries where information needed for targeting is not readily available. Yet, little is known about how community leaders themselves respond to alternative incentive schemes, discretion, or stake sizes in beneficiary targeting. Specifically, little is clear about how the micro-foundations of collective decision-making within community committees—heterogeneous member preferences, power dynamics, and social norms—are aggregated into collective decisions despite their centrality for understanding when and how CBT succeeds or fails. This paper provides new evidence on the behavior of community leaders responsible for CBT of cash transfers. Specifically, we examine how community leaders target and allocate funds to eligible households and whether and to what extent they are prone to administrative capture, constituting an overlooked but policy-relevant form of leakage in targeting of interventions. The type of administrative capture we examine arises from discretionary appropriation of funds during implementation. To understand committee-based decision-making processes in CBT, we also examine how individual choices and preferences are aggregated to form collective decisions.

Using a community-level randomized experiment across 180 villages in Ethiopia, we show that the financial stakes and targeting criteria faced by community leaders significantly shape both their individual and collective behavior in community-based targeting and associated allocation of cash transfers. Our two-stage decision protocol—first eliciting individual preferences in isolation, then requiring a binding group decision—reveals that community leaders systematically retain larger share of the budget as "administrative costs" in their collective decisions than individual preferences. Rather than moderating selfish behavior, collective decision-making amplifies it, facilitating more appropriation through implicit coordination. This aligns with prior findings that groups tend to behave in ways more consistent with self-interested rationality, retain more for themselves, and allocate less to beneficiaries. Administrative capture, defined as the share of the budget retained beyond reasonable administrative costs, averages about 10 percent of the total funds in the high-stakes incentivized treatment and declines significantly when incentives are removed or budgets are tightened. This implies that incentive regimes as well as

stake size shape administrative capture, with real stakes and a relaxed budget triggering self-serving motivations, and hence increasing administrative capture.

We also find that preference aggregation among heterogeneous committee members varies across incentivized and hypothetical targeting. In the hypothetical targeting, collective decisions can be characterized by convex combinations of individual proposals (preferences) while such aggregation fails to hold in the presence of real stakes. Similarly, extreme opinions (voters) drive collective decisions in the presence of real incentives, while popular votes and preferences dominate collective decisions in the absence of real incentives. Finally, while formal tests of relevance of extreme preferences can not be rejected in the incentivized targeting process, extreme preferences appear to be irrelevant in the hypothetical targeting exercise.

While most of the literature on targeting focuses on misidentification of beneficiaries—inclusion or exclusion errors—or elite capture in the form of nepotism and favoritism, our findings underscore a complementary and underexplored behavioral margin: the discretionary appropriation of funds during the allocation process under vaguely defined administrative cost allowances. This channel is subtle, difficult to monitor, and potentially widespread where accountability is limited. It provides important insights about the critical roles micro-foundations of community committees play in the design of CBT, with implications to the trade-offs between harnessing local information and safeguarding against capture. Policymakers and program implementers aiming to leverage local knowledge in targeting must therefore consider not just who makes allocation decisions, but under what constraints and incentive structures these decisions take place.

These findings suggest that modest design changes, such as limiting stake size, clarifying allowable administrative costs, or allowing discretion within broadly accepted criteria, can reduce administrative capture while leveraging local participation and knowledge to address structural information problems in targeting. Future research should explore how formal accountability mechanisms (e.g., audits, reporting requirements, or performance-based rewards) interact with local discretion to improve implementation in social protection programs.

References

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

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

Abay, K.A., Berhane, G., Gilligan, D.O., Tafere, K., Taffesse, A.S., 2024. Targeting social assistance in fragile settings: An experiment on community-based targeting. Intl Food Policy Res Inst.

Abay, K.A., Berhane, G., Hoddinott, J., Tafere, K., 2023. Covid-19 and food security in ethiopia: do social protection programs protect? Economic Development and Cultural Change 71, 373–402.

Alatas, V., Banerjee, A., Hanna, R., Olken, B.A., Purnamasari, R., Wai-Poi, M., 2019a. Does elite capture matter? Local elites and targeted welfare programs in Indonesia, in: AEA Papers and Proceedings, American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203. pp. 334–339.

Alatas, V., Banerjee, A., Hanna, R., Olken, B.A., Purnamasari, R., Wai-Poi, M., 2019b. Does elite capture matter? local elites and targeted welfare programs in indonesia, in: AEA Papers and Proceedings, American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203. pp. 334–339.

Alatas, V., Banerjee, A., Hanna, R., Olken, B.A., Tobias, J., 2012. Targeting the poor: Evidence from a field experiment in Indonesia. American Economic Review 102, 1206–1240.

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

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

Almenfi, M., Breton, M., Dale, P., Gentilini, U., Pick, A., Richardson, D., 2020. Where is the money coming from? ten stylized facts on financing social protection responses to covid-19 .

Ambrus, A., Greiner, B., Pathak, P.A., 2015. How individual preferences are aggregated in groups: An experimental study. Journal of public economics 129, 1–13.

Asri, V., Michaelowa, K., Panda, S., Paul, S.B., 2022. The pursuit of simplicity: Can simplifying eligibility criteria improve social pension targeting? Journal of Economic Behavior & Organization 200, 820–846.

Bandiera, O., Levy, G., 2011. Diversity and the power of the elites in democratic societies: Evidence from indonesia. Journal of Public Economics 95, 1322–1330.

Banerjee, A., Hanna, R., Olken, B.A., Sverdlin Lisker, D., 2024. Social protection in the developing world. Journal of Economic Literature 62, 1349–1421. URL: https://www.aeaweb.org/articles?id=10.1257/jel.20241646.

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

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

BBC, 2025. What is USAID and why is Trump reportedly poised to close it? https://www.bbc.com/news/articles/clyezjwnx5ko. Accessed January 07, 2025.

Beath, A., BenYishay, A., d'Adda, G., Grosjean, P., Weber, R.A., 2018. Can vouchers reduce elite capture of local development projects? experimental evidence from the solomon islands. Journal of Public Economics 160, 117–131.

Berhane, G., Gilligan, D.O., Hoddinott, J., Kumar, N., Taffesse, A.S., 2014a. Can social protection work in Africa? The impact of Ethiopia's productive safety net programme. Economic Development and Cultural Change 63, 1–26.

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

Berhane, G., Hoddinott John, K.N., Taffesse A Seyoum, D.T.M., Yohannes Yisehac, S.W.R., Handino Mulugeta, L.J., Mulugeta, T., Feyera, S., 2013. Evaluation of Ethiopia's food security program: documenting progress in the implementation of the Productive Safety Nets Programme and the Household asset Building Programme. Technical Report. IFPRI.

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

Beuermann, D.W., Hoffmann, B., Stampini, M., Vargas, D.L., Vera-Cossio, D., 2025. Shooting a moving target: Evaluating targeting tools for social programs when income fluctuates. Journal of Development Economics 172, 103395.

Bourguignon, F., Platteau, J.P., 2025. Aid allocation with optimal monitoring: Theory and policy. Journal of Development Economics 172, 103364.

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

Caeyers, B., Dercon, S., 2012. Political connections and social networks in targeted transfer programs: Evidence from rural ethiopia. Economic Development and Cultural Change 60, 639–675.

Charness, G., Sutter, M., 2012. Groups make better self-interested decisions. Journal of economic perspectives 26, 157–176.

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

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

Conning, J., Kevane, M., 2002b. Community-Based Targeting Mechanisms for Social Safety Nets: A Critical Review. World Development 30, 375–394.

Devex, 2023. USAID pauses food aid to Ethiopia's Tigray region due to theft. Https://www.devex.com/news/usaid-pauses-food-aid-to-ethiopia-s-tigray-region-due-to-theft-105457.

Duggan, J., et al., 2000. A bargaining model of collective choice. American Political Science Review 94, 73–88.

Fehr, E., Charness, G., 2025. Social preferences: fundamental characteristics and economic consequences. Journal of Economic Literature 63, 440–514.

Fehr, E., Fischbacher, U., 2002. Why social preferences matter–the impact of non-selfish motives on competition, cooperation and incentives. The economic journal 112, C1–C33.

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.

Fiala, N., Premand, P., 2025. Community monitoring and social accountability in development projects: Experimental evidence from Uganda. Journal of Development Economics , 103537.

Gentilini, U., Andrews, C., Evans, M., Tesliuc, E.D., Rodriguez Alas, C.P., Zafar, U., Martinez Cordova, A.S., Immervoll, H., Ozer, C., Bloom, D.E., et al., 2025. How scale-up happens .

Gilligan, D.O., Hoddinott, J., Taffesse, A.S., 2009. The Impact of Ethiopia's Productive Safety Net Programme and its Linkages. Journal of Development Studies 45, 1684–1706.

Grosh, M., Leite, P., Wai-Poi, M., Tesliuc, E., 2022. Revisiting targeting in social assistance: A new look at old dilemmas. World Bank Publications.

Haushofer, J., Niehaus, P., Paramo, C., Miguel, E., Walker, M., 2025. Targeting impact versus deprivation. American Economic Review 115, 1936–1974.

He, H., Villeval, M.C., 2017. Are group members less inequality averse than individual decision makers? Journal of Economic Behavior & Organization 138, 111–124.

Hirvonen, K., Machado, E., Simons, A., Taraz, V., 2022. More than a safety net: Ethiopia's flagship public works program increases tree cover. Available at SSRN 3791624 .

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

ILO, 2021. World Social Protection Report 2020–22: Social protection at the crossroads–in pursuit of a better future. Technical Report. International Labour Organization. URL: https://www.ilo.org/publications/world-social-protection-report-2020-22-social-protection-crossroads-%E2%80%93. accessed June 2025.

Kohnert, D., 2025. Aid in retreat: The impact of US and European Aid cuts on Sub-Saharan Africa .

Levy, G., 2007. Decision making in committees: Transparency, reputation, and voting rules. American economic review 97, 150–168.

Lind, J., Sabates-Wheeler, R., Carter, B., Tefera Taye, M., 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.

Muralidharan, K., Niehaus, P., Sukhtankar, S., 2025. Identity verification standards in welfare programs: Experimental evidence from India. Review of Economics and Statistics 107, 372–392.

Platteau, J.P., Vincent Somville, V., Wahhaj, Z., 2014. Elite capture through information distortion: A theoretical essay. Journal of Development Economics 106, 250–263.

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

Sabates-Wheeler, R., Hirvonen, K., Lind, J., Hoddinott, J., 2022. Expanding social protection coverage with humanitarian aid: Lessons on targeting and transfer values from Ethiopia. The Journal of Development Studies 58, 1981–2000.

Schnitzer, P., Stoeffler, Q., 2024a. Targeting social safety nets: evidence from nine programs in the sahel. The Journal of Development Studies 60, 574–595.

Schnitzer, P., Stoeffler, Q., 2024b. Targeting social safety nets: Evidence from nine programs in the Sahel. The Journal of Development Studies 60, 574–595.

Simons, A.M., 2022. What is the optimal locus of control for social assistance programs? evidence from the productive safety net program in ethiopia. Journal of Development Economics 158, 102897.

Tan, C.H., 2021. The effects of group decision-making on social preferences: An experimental study. Journal of Economic Behavior & Organization 190, 134–153.

Tefera Taye, M., Mogus, F., Carter, B., Lind, J., Sabates-Wheeler, R., 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., Permana, Y.H., Sahadewo, G.A., 2025. How much do our neighbors really know? The limits of community-based targeting. Journal of Development Economics , 103555.

UN OCHA, 2022. Ethiopia: Humanitarian Response Plan 2022. Technical Report. United Nations Office for the Coordination of Humanitarian Affairs. URL: https://reliefweb.int/report/ethiopia/ethiopia-humanitarian-response-plan-2022-july-2022. accessed April 2025.

UN OCHA, 2023. Global Humanitarian Overview 2023: October Update. Technical Report. United Nations Office for the Coordination of Humanitarian Affairs. URL: https://www.unocha.org/publications/report/world/global-humanitarian-overview-2023-october-update-snapshot-31-october-2023. accessed April 2025.

UN OCHA, 2024. Ethiopia: Humanitarian Response Plan 2024. Technical Report. United Nations Office for the Coordination of Humanitarian Affairs. URL: https://reliefweb.int/report/ethiopia/ethiopia-humanitarian-response-plan-2024. accessed April 2025.

UNFPA, 2024. Ethiopia Humanitarian Response Situation Report 2024. Technical Report. United Nations Population Fund. URL: https://reliefweb.int/report/ethiopia/unfpa-ethiopia-humanitarian-response-situation-report-march-2024. accessed April 2025.

Vera-Cossio, D., 2022. Targeting credit through community members. Journal of the European Economic Association 20, 778–821.

Zlatev, M., 2016. Selfishly benevolent or benevolently selfish: When self-interest undermines versus promotes prosocial behavior. Organizational Behavior and Human Decision Processes 137, 112–122.

Appendix A Figures and Tables

Table A.1: 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 A.2: Most important reasons for allocation of 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 capacity	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 A.3: OLS regressions of group choices on individual choices by treatment arms

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Relaxed-budget 20K		Hypotehtical 20K		Constrained-budget 10K		Discretionary 20K	
X_{1c}	-0.01	-0.03	0.17	0.25*	0.25*	0.32	-0.01	0.01
	(0.08)	(0.08)	(0.15)	(0.15)	(0.15)	(0.22)	(0.02)	(0.05)
X_{2c}	0.51	0.51	-0.07	-0.14	0.04	-0.04	0.01	0.09
	(0.37)	(0.40)	(0.26)	(0.24)	(0.30)	(0.32)	(0.04)	(0.10)
X_{3c}	-0.40	-0.37	0.68*	0.70**	-0.37	-0.33	0.16	0.03
	(0.37)	(0.40)	(0.35)	(0.31)	(0.34)	(0.43)	(0.20)	(0.15)
X_{4c}	-0.07	-0.03	-0.56*	-0.71**	0.99**	0.92*	0.17	0.30
	(0.63)	(0.83)	(0.33)	(0.27)	(0.38)	(0.49)	(0.29)	(0.32)
X_{5c}	-0.04	-0.18	0.90***	1.08***	-1.09**	-1.11*	-0.84	-1.19
	(0.85)	(1.41)	(0.23)	(0.16)	(0.46)	(0.54)	(0.92)	(1.05)
X_{6c}	-0.27	-0.30	-0.14	-0.19	1.43***	1.66***	1.39**	1.67**
	(0.22)	(0.53)	(0.13)	(0.16)	(0.28)	(0.38)	(0.58)	(0.71)
Constant	13.08***	14.06***	0.47	0.58	-1.74	-3.31	1.26	1.20
	(1.73)	(2.56)	(0.41)	(0.54)	(2.05)	(2.81)	(1.14)	(0.98)
Region fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.26	0.17	0.73	0.76	0.42	0.37	0.65	0.65
Observations	41	40	53	52	42	42	44	43

Note: Table A.3 reports OLS regressions of group choices on individual choices, ranked from the lowest (X_{1c}) to the highest (X_{6c}) proposed percentage to retain as "administrative costs", by treatment arm. Standard errors clustered at the village levels are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

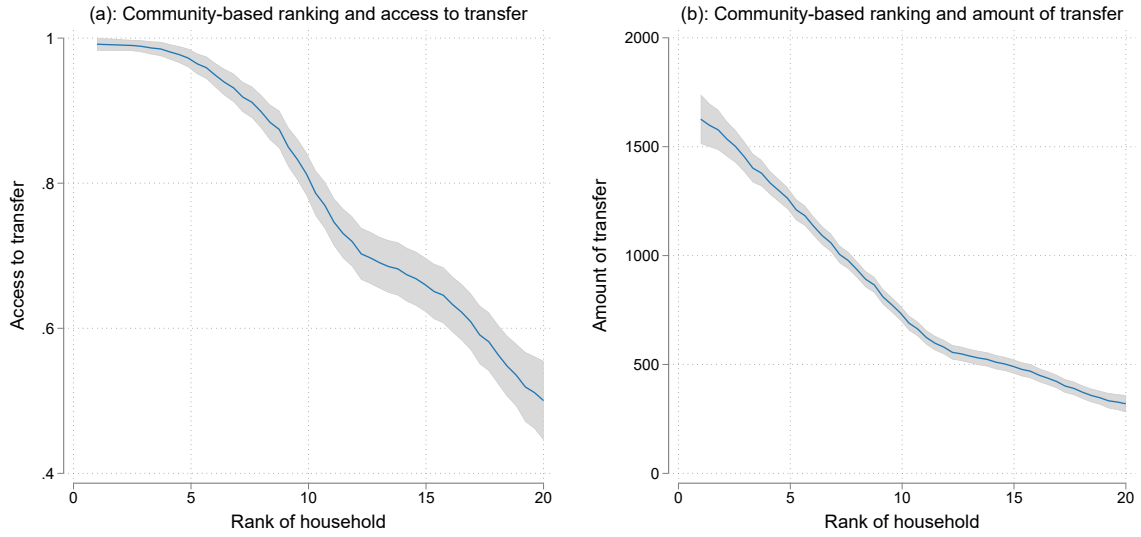

Figure A.1: Correlation (relationship) between access and size of transfers with rank of households

Table A.4: OLS regressions of the difference between group and individual choice across treatment groups

	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Variables	Relaxed-budget 20K			Hypothetical 20K			Constrained-budget 10K			Discretionary 20K		
Relative position (Ref: $p_{ic}^{(4)}$)												
$p_{ic}^{(1)}$		5.38***	5.38***		3.65***	3.65***		4.94***	4.94***		5.23***	5.23***
		(0.87)	(0.88)		(1.30)	(1.31)		(1.02)	(1.03)		(0.80)	(0.81)
$p_{ic}^{(2)}$		2.18*	2.18*		1.75	1.75		2.49**	2.49**		3.08***	3.08***
		(1.11)	(1.12)		(1.40)	(1.41)		(1.11)	(1.12)		(0.89)	(0.90)
$p_{ic}^{(3)}$		2.36*	2.36*		-0.12	-0.12		1.39	1.39		1.13	1.13
		(1.22)	(1.23)		(1.42)	(1.43)		(1.16)	(1.17)		(0.77)	(0.78)
$p_{ic}^{(5)}$		0.22	0.22		-0.77	-0.77		0.32	0.32		-1.61	-1.61
		(0.75)	(0.75)		(1.79)	(1.81)		(1.59)	(1.61)		(1.31)	(1.32)
$p_{ic}^{(6)}$		-1.82*	-1.82*		2.37	2.37		-1.32	-1.32		-3.13**	-3.13**
		(0.94)	(0.95)		(1.68)	(1.70)		(1.43)	(1.45)		(1.16)	(1.18)
Constant	1.76***	0.37	0.37	1.91***	0.76	0.76	2.55***	1.24	1.24	1.82***	1.04	1.04
	(0.30)	(0.64)	(0.63)	(0.23)	(1.28)	(1.29)	(0.34)	(0.87)	(0.87)	(0.29)	(0.71)	(0.70)
Region fixed effects	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Adjusted R^2	0	0.3	0.35	0	0.13	0.14	0	0.19	0.24	0	0.41	0.45
Observations	246	246	246	318	318	318	252	252	252	264	264	264

Note: Table A.4 reports OLS regressions of the difference between group and individual choice on a set of relative positions of the individual choice within a group. Regression is presented for the four treatment groups. We estimate three models for each: 1) a simple model with only a constant term, 2) a model including relative position indicators without region fixed effects, and 3) a model including relative position indicators with region fixed effects. The constant term reflects the average deviation from the group decision for individuals in the fourth-lowest position, which serves as a reference position. Standard errors clustered at the village levels are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Effects of modal individual proposals on collective administrative costs

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Modal (max) of proposed share (%)	0.57***	0.57***	0.74***			
	(0.08)	(0.08)	(0.10)			
Incentivized treatments			4.58***			4.31***
			(1.45)			(1.33)
Modal (max) of proposed share*Incentivized treatments			-0.39**			
			(0.16)			
Modal (min) of proposed share (%)				0.51***	0.50***	0.70***
				(0.08)	(0.08)	(0.11)
Modal (min) of proposed share*Incentivized treatments						-0.36**
						(0.15)
Constant	3.68***	3.72***	1.44	4.57***	4.59***	2.13**
	(0.78)	(0.80)	(0.90)	(0.71)	(0.72)	(0.95)
Region fixed effects	No	Yes	Yes	No	Yes	Yes
Adjusted R^2	0.35	0.33	0.41	0.34	0.32	0.42
Observations	180	180	180	180	180	180

Note: Table A.5 reports the effects of the modal individual proposal (maximum or minimum) for the share retained as "administrative costs" on collective outcomes, across incentivized and hypothetical treatment arms. The reference category is the hypothetical treatment arm. Standard errors clustered at the village levels are in parentheses. Results are shown with and without region fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Share of proposed administrative costs by community leaders across treatment

Variables	(1) Relaxed budget 20K	(2) Hypothetical 20K	(3) Constrained budget 10K	(4) Discretionary 20K	(5) Total	(6) Observations
Kebele leader/ship	7.71	5.74	6.48	6.86	6.63	180
Elder men/women	7.71	6.15	7.37	7.68	7.16	179
Religious leader	7.85	6.15	7.07	6.86	6.93	181
Teacher, Development agent or Health extension worker	8.83	5.58	6.48	7.38	6.97	181
Women representative	8.83	5.92	7.14	7.91	7.36	180
Youth representative	8.73	6.11	6.95	7.67	7.28	179
Total	8.28	5.94	6.91	7.39	7.06	1,080
Collective final decision	9.61	6.49	8.43	9.00	8.27	

Note: Table A.6 reports summary statistics of the proposed share of administrative costs by community leaders across treatment arms.

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