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Leader Selection and Service Delivery in Community Groups: Experimental Evidence from Uganda[†]

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In developing countries, NGOs and governments often rely on local groups for the delivery of financial and public services. This paper studies how the design of rules used for group leader selection affects leader identity and shapes service delivery. To do so, we randomly assign newly formed savings and loan groups to select their leaders using either a public discussion procedure or a private vote procedure. Leaders selected with a private vote are found to be less positively selected on socioeconomic characteristics. This results in groups that are more inclusive toward poor members, without being less economically efficient. (*JEL* D72, O16, O17, O22, Z13)

In many developing countries, access to financial and public services is severely limited, especially among the poorest community members. In this context, NGOs and governments often rely on local groups (such as farmer associations, microfinance groups, school management committees, community health groups) to deliver these services (Mansuri and Rao 2012).[1] Delegation of service delivery to local groups has the advantage of lowering program costs as compared to employing external staff and can facilitate outreach and targeting by leveraging local knowledge. At the same time, however, these groups are embedded in local power structures that can enter their governance and create a bias in favor of influential community members. Understanding whom to select as group "leaders" and how to select them therefore has key implications for service delivery at the local level.

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[1]Such groups serve millions of households around the world and have been studied in Björkman and Svensson (2010); Grossman (2014); Baland, Somanathan, and Vandewalle (2015); and Burlando and Canidio (2017) among others.

This paper provides causal evidence of how electoral procedures affect the identity of group leaders and how this in turn shapes the coverage and the targeting of group services. During the creation of 92 new savings and loan groups in northeastern Uganda (Karamoja), we randomly assigned group members to select their leaders using one of two different participatory and inclusive selection rules: (i) a public *discussion* or (ii) a private (secret ballot) *vote*. Our goal is to determine whether the procedure used to select leaders can affect the allocation of services.

The local groups analyzed in this paper were created by the NGO BRAC with the goal of providing access to financial services in a context where such services were previously virtually nonexistent. Each group is formed by up to 30 young local women who deposit savings with the group on a weekly basis and can take loans out of these savings at a 10 percent interest rate. Groups are managed by five elected Committee Members (henceforth, CMs) who receive training from BRAC and are then asked to act as leaders. These CMs play an important role: in addition to managing the regular group activities, they decide how to allocate loans across group members. While explicitly instructed by BRAC not to exclude the poorest members, the difficulty of monitoring the CMs gives them the ultimate power of deciding which individuals are given loans. Their decisions thus have obvious implication on whether access to savings and loans is equitable.

Our experiment creates random variation in the procedure used by groups to select their CMs. In what we will refer to as the *discussion* groups, members are asked to publicly discuss which of them is best suited for the CM positions until reaching an agreement on whom to appoint. In *vote* groups, members hold a similar public discussion which is, however, followed by a private vote. In this vote, group members are asked to secretly indicate their preferred candidate for each committee position, regardless of whether this person was nominated in the public discussion. The person with the most votes is appointed. Aside from the selection procedure, other meeting details are held constant across the two treatments. For instance, the CM selection always takes place in a meeting attended by all group members and where a BRAC staff is present, and the CM positions are always filled sequentially.

The two selection procedures analyzed in this paper are commonly used in direct democracy setups. They differ in the publicness of decision making, a component which can strongly influence the outcome of the decision-making process. On the one hand, publicness allows for coordination, which may lead to decisions of higher quality (Humphreys, Masters, and Sandbu 2006). In our context, this would translate into electing more qualified CMs. On the other hand, when decision making is public, less powerful members can be coerced or intimidated into supporting certain proposals, tilting selection decisions toward outcomes that are less representative of low-income group members' preferences (Hinnerich and Pettersson-Lidbom 2014). In contrast, the opportunity to cast a secret vote has been shown to increase the representation of economically disadvantaged groups (Baland and Robinson 2008), and in our context, this entails more representative CMs. Both the degree of representativeness and the qualifications of the elected CMs are, in turn, likely to affect the nature of service delivery (Pitkin

1967; Osborne and Slivinski 1996; Besley and Coate 1997; Besley, Pande, and Rao 2005).

To estimate how the decision-making process affects the CM choice, and to shed light on the trade-off between CM representativeness and skill, our analysis proceeds in two steps. We begin by comparing characteristics of selected CMs. We then examine group performance by analyzing data on loans, savings, and membership that we collected one year (midline) and three years (end line) after committee selection. A key outcome variable is the group's inclusiveness toward the poor, i.e., the proportion of loans that are assigned to poor members and the likelihood that a poor stays in the group.

We find that selection rules affect the type of group leader chosen. Vote groups select CMs who are more representative of the average group participant in terms of economic status than the CMs selected in discussion groups. Individuals belonging to the group's top wealth quartile (as measured with a wealth score used by BRAC in the field) are indeed 31 percent less likely to be appointed CMs in the vote than in the discussion treatment; those in the bottom quartile are instead 11 percent more likely to be appointed. While CMs in vote groups are more representative, they are also found to be potentially less "qualified," as they have less education and less business training.[2] Overall, these findings reveal differences in CM characteristics between treatments but do not indicate which treatment selects better leaders in terms of service delivery.

We find that vote groups are more inclusive toward poor members than discussion groups: not only do they allow poor members to borrow more money, they are also more likely to retain them. More precisely, in vote groups, poor individuals (belonging to the bottom quartile of their group's wealth-score distribution at baseline) are equally likely as richer members to access loans and to remain in the group. In discussion groups, instead, poor members are 28 percent less likely to be assigned a loan and 16 percent less likely to stay in the group. In line with this, we find suggestive evidence that the treatments also have different effects on wealth distributions over time: the gap in end line wealth score between the initially poorest and the other group members is wider in discussion groups than in vote groups.

The higher inclusiveness observed in vote groups does not come at the expense of efficiency: both treatments have comparable default rates and average loan sizes. If anything, vote groups seem to be more sustainable. Not only do they retain a larger proportion of members, but their probability of collapsing (i.e., retaining none of the original members) in the short run is also significantly lower, at 2 percent versus 15 percent for the discussion groups.

Finally, we show that differences in group outcomes across treatments are most likely explained by a leader *selection effect* (i.e., decision-making rules affect the type of leaders selected), rather than by members perceiving one of the two selection rules as more legitimate. Taken together, the findings of this paper suggest that it is important to limit elite capture at the selection stage, even if at the cost of

[2] Whether any leader selection process can deliver both highly qualified leaders and broad representation fundamentally relies on the distribution of qualifications in the population. In our context, qualifications are rare and mostly concentrated within (what we will refer to as) the "elite."

recruiting a less educated person, and that introducing a secret vote is a successful way to do so.

The paper bridges two recent strands of literature on community-based service delivery in developing countries. First, we contribute to the literature that studies whether the procedure used to select development projects (rather than leaders) affects the type and location of these projects. Recent evidence suggests that projects emerging from more inclusive procedures, that give voice to a larger fraction of the community (e.g., plebiscites), are more likely to benefit the poor than projects emerging under more centralized decision rules (Olken 2010; Beath, Christia, and Enikolopov 2017; Madajewicz, Tompsett, and Habib 2017). Second, we relate to recent evaluations of programs that employ local delivery agents as service providers, either within the context of community groups (microfinance credit officers, leaders of farmer associations) or in non-group settings (health workers, extension workers). These studies show that, in an otherwise identical program, the "type" of agents selected makes a significant difference for how benefits of the program are distributed among community members (Bandiera et al. 2018, Deserranno 2017).[3] We complement these two literatures by showing that, in the context of group-based service delivery, (i) the way leaders are selected influences who is appointed and that (ii) the type of group leaders selected affects the targeting and the coverage of local delivery programs.

Our findings also add to the political economy literature on electoral rules, leader selection, and policy outcomes. Due to the difficulty to find exogenous variation in electoral rules in real world contexts, this literature has been mostly theoretical (see Huber, Ragin, and Stephens 1993; Cox 1997; and Persson and Tabellini 2002, 2005). We are aware of only one experimental work, Beath et al. (2016), that estimates the effect of at-large voting versus voting by districts on leader selection in Afghanistan. More closely related to our paper, Grossman (2014) exploits a natural experiment to show that executive directors of farmer associations act in a more accountable way when elected in a direct rather than an indirect election.[4] Along with the abovementioned studies on development projects (Olken 2010; Beath, Christia, and Enikolopov 2017; Madajewicz, Tompsett, and Habib 2017), Grossman (2014) compares direct to indirect participation rules and hence focuses on the effect of removing restrictions to participation in decision-making fora. In contrast, our paper studies the impact of varying (direct) electoral rules among a comparable set of participants (all the group members). While our findings may not generalize to national elections, they provide new evidence on the design of rules for selecting leaders in community groups which, given the strong presence of such

[3] In the context of an extension agriculture program and two health programs, Ashraf, Bandiera, and Lee (2018); Deserranno (2017); and Bandiera et al. (2018) show that successful targeting of the poorest community members depends on the pro-social motivation and the structure of social preferences of the delivery agent.

[4] Related work on public versus private political voting in heterogeneous and polarized populations indicates that public procedures generate outcomes that are less representative for the preferences of low-income community members (Baland and Robinson 2008, Hinnerich and Pettersson-Lidbom 2014). We compare public to private decision making in relatively homogenous neighborhood-based groups, and whether the same patterns hold in such a setting is an open question.

groups in developing countries, complements studies of the effects of national and district elections on service delivery.[5]

The rest of the paper is organized as follows. Section I discusses the context and the design; Section II lays out the conceptual framework; Section III describes the data; Section IV and V present the treatment effects on leader selection and group performance, respectively; Section VI concludes with a discussion on external validity.

I. Context and Empirical Design

A. *BRAC's Savings and Loan Groups in Karamoja*

This study took place in Karamoja, the poorest region of Uganda with 74 percent of its population living below the local poverty line of 1 USD per day (Ministry of Finance, Planning, and Economic Development 2014). The inhabitants of the region, who traditionally relied on agropastoralism and pastoralism for their livelihood, have recently gotten involved in small-scale market activities due (mainly) to harsher climate conditions. Access to finance, however, remains extremely low: microfinance institutions, village savings, and loan associations (VSLAs) and local credit unions (SACCOs) were almost nonexistent at the time of the study.

In this context, our partner institution BRAC—one of the largest NGOs in the world—introduced a credit program in 2011 in collaboration with UNICEF. The program aims at improving access to basic financial services through the formation of local groups that accumulate savings and extend loans to its members. Like all BRAC's activities, the program has the ultimate goal of empowering poor women. As such, the program is open to young women only and aims to "reach the poorest borrowers and savers (…) who remain unserved by other mainstream financial institutions and banks" (BRAC 2011). In exploratory interviews conducted in 2011 with prospective beneficiaries, before the program was launched, none of the 200 local women interviewed were saving in a bank, a microfinance institution, or a VSLA, and only 4 were members of a SACCO (Czuba 2011, 2012).

We focus on the 92 saving and loan groups created by BRAC in mid-2011 throughout Karamoja.[6] These groups are structured around 9 BRAC branch offices and each of them is composed of 10 to 30 members. Participation is voluntary but open only to participants of BRAC's "youth clubs" that are organized by neighborhood.[7] This implies that the saving and loans groups that we study—pay cut which are embedded in these youth clubs—are presumably not representative of the overall population: members are all relatively young women, who participate in

[5] Our paper contributes to existing research on how to improve the pro-poor targeting of development programs and their inclusiveness (e.g., Galasso and Ravallion 2005; Bardhan and Mookherjee 2006; Alatas et al. 2012; Niehaus et al. 2013; Burlando and Canidio 2017; and Baird, McIntosh, and Özler 2013). Rather than focusing on the identification of poverty and eligibility indicators, we examine the impact of group governance on the access to services delivered by the group, akin to targeting in our context.

[6] The groups were created in the Napak, Nakapiripirit, Moroto, Kotido, and Amudat districts.

[7] These clubs host popular recreational activities such as reading, singing, dancing, and playing games (Bandiera et al. forthcoming) and are composed of young women aged 13 or above. Members are recruited through the canvassing of BRAC staff in the neighborhood and by other members bringing friends.

BRAC's youth clubs and who self-selected into the group. This is important to take into account when analyzing the effect of our electoral rules on group outcomes: our population is one that has little exposure to decision-making fora or to national elections[8] and that may not be able to predict, for instance, the proneness to elite capture of one or the other procedure.

The groups are organized around weekly meetings, held in the hut of the youth club, where savings are collected and placed in a box. Because savings accumulate slowly over time, loans are provided only after the first six months of savings. The typical loan size is 80,000 Ugandan Shillings (UGX), which corresponds to 96 USD (2011 PPP-adjusted). Loans have a 4-month cycle and are repaid to the group at a 10 percent flat interest rate in 4 monthly installments.[9] A member is allowed to have only one outstanding loan at a time. Interest rate payments are accumulated in the group and, by the end of each year, are shared among group members in proportion to the amount of money each person saved.[10] BRAC encourages the loans to be used for productive purposes. In practice, around 85 percent of loans were used for business activity while the remaining loans were used to cover school and medical fees and other household related expenses.

In line with BRAC's antipoverty focus, the loans are not collateralized and groups do not require registration or membership fees, nor a minimum deposit saved. Moreover, even though poor members typically save smaller amounts than richer members, BRAC explicitly asks group leaders not to exclude marginalized individuals from accessing loans. These groups are thus formed with the idea of incentivizing the richest members to save in the group, by offering a positive interest rate on savings.[11] Meanwhile, the poorest members are given the opportunity to save and borrow from the group in a context where formal or semiformal credit is almost nonexistent.

The Role of BRAC Staff and Groups' Committee Members.—In the initial phase of the program, each of the 9 branches in Karamoja was assigned a full-time BRAC staff whose role was to manage and monitor the saving activities of all groups around the branch. In February 2012, before groups started lending to their members, the local BRAC staff instructed each group to select 5 Committee Members (CMs) that would first undergo a financial literacy training provided in the BRAC branch office and thereafter take over

[8] Young women are often excluded from decision-making fora in Karamoja, and with Uganda's legal voting age being 18, the majority of group members had voted in, at most, one national election.

[9] To give a benchmark, this interest rate is in line with the 9 percent–10 percent interest rate offered by most VSLAs in Uganda on similar loans and substantially lower than the 25 percent interest rate imposed by most microfinance institutions in rural Uganda.

[10] Members who leave the group midyear can reclaim their savings, but get no share in the group's profits from accumulated interest rates.

[11] As noted above, the vast majority of members had no access to alternative formal/semiformal sources of savings (banks, SACCOs, or VSLAs). The means of financial saving available to them are saving money at home or with someone, which do not pay any interest rate and are less safe.

group responsibilities. It is at the stage of selection of these committees that our experiment took place.[12]

In addition to managing the regular group activities, the CMs have the key role of deciding which group members are allowed to take loans, and through this, influence members' incentive to stay in the group.[13] Each CM is also assigned extra individual roles: the "chairperson" leads the discussions in every meeting and is the chair of the committee, the "treasurer" makes sure the box is stored in a safe location (typically either at her home or in BRAC's office) and counts the savings contribution at every meeting in front of the group, the "secretary" is responsible for keeping track of all savings and loan transactions in a ledger book, and two "key-holders" are in charge of storing the keys of the saving lock box. We will show that in practice, the effects of our treatments on the selection of CMs is very similar across all committee positions, although slightly stronger for "chairpersons," indicating that these extra individual roles were probably perceived as second order. In most of the analysis, we therefore pool all committee positions together.

The CM position involves no monetary compensation from BRAC. Yet, it is attractive (and indeed all elected CMs accepted the position) because it provides power in influencing who gets a loan, financial skills through the training, and nonfinancial and career incentives (it increases the chances of later being recruited as a permanent staff in the BRAC branch office). Also, the CM position is an open-ended one as long as a CM does not "misbehave" (e.g., by stealing group funds). CMs who quit the position thus "voluntarily" quit but are not forced out.

B. *Empirical Design*

The 92 groups were formed in mid-2011, but the leader selection took place only in February 2012, six months after the group was formed but before it started lending money. In each group, the selection procedure of CMs was divided into two steps.

(1) The BRAC staff informed all members that the group was to select a committee in a meeting to be held the following week. Information was given about the role of the committee and about each committee position. The procedure to be used for selection was not revealed at this point, but instead on the day of the election. This feature is important for two reasons. First, it ensures that the treatment is orthogonal to any campaigning that took place in the week preceding committee selection and orthogonal to both the group composition and the attendance rate on the day of the meeting. Second, it limits spillovers across treatments. The short time that elapsed between elections across groups, along with the geographical dispersion of the groups, makes it unlikely that members heard

[12] After leader selection, the local BRAC staff kept regularly visiting the groups, functioning as a "mentor," and may have remained influential in group governance. We include branch fixed effects in all regressions to account for such unobserved heterogeneity.

[13] Loan applications are made through a simple form that is filled out by the applicant and handed in to the committee at the end of any (weekly) group meeting. It states the date, the applicant's name, and the purpose of the loan. After accumulating enough savings in the group and receiving enough applications, typically monthly, the committee meets and discusses to whom to grant a loan.

about the selection rule used in another group before being asked to elect their own leaders.

(2) Upon arrival at the meeting, group members were instructed on the procedure they would use to select CMs. Half (46) of the groups were randomly assigned to select CMs using the Public Discussion Treatment while the remaining 46 groups were randomly assigned to the Private Vote Treatment. The randomization was stratified at the branch level. In all groups, positions were filled sequentially: for each committee position in turn, groups were first asked to publicly discuss which candidates were suited for the position and willing to fill it. They then proceeded to the selection stage, which varied by treatment as follows.

Discussion Treatment (*Public Discussion*).—Any group member could nominate a candidate for the position. Other members could then second or oppose the nomination *publicly* until the group agreed on a name.[14]

Vote Treatment (*Private Vote*).—Each group member was asked to *privately vote* for their preferred candidate by writing the name on a piece of paper and then placing it in a basket.[15] The local BRAC staff then compiled the votes and the person with the most votes was elected. Importantly, each person was allowed to vote for any of the group members, irrespective of whether the person was mentioned or not during the discussion that preceded the secret vote. This discussion can hence be considered a standard preelection *nonbinding* discussion with a purely informative goal.

These two selection rules were chosen by BRAC among a set of other potential rules because: (a) they are versions of the globally most common direct participation procedures, (b) they hold relevance also in the Karamoja context, where discussion is used in local village meetings while the secret ballot vote is used in national elections, and (c) they are enforceable in small groups. The lack of consensus about which of the two rules is more legitimate makes the comparison between them relevant.

The members had little or no experience of being active participants in either one of the two decision-making procedures. As explained above, this is because young women are typically excluded from decision-making fora in Karamoja, and the legal voting age in Uganda is 18, implying that the majority of group members has voted in, at most, one national election.

The two treatments differ from each other along two main dimensions. The first one is the degree of publicness of the decision: all support for a candidate is expressed publicly in the discussion treatment while votes are cast privately in the vote treatment. As we discuss further in the next section, this can affect the voting choice of each member as well as the composition of members who actively

[14] The meetings were overseen by the BRAC staff. The BRAC staff also instructed the group on which procedure to be used. Unfortunately, no records were kept from these meetings. However, anecdotal accounts from BRAC staff that observed the meeting confirm that all decisions were made publicly and by discussion rather than by any form of vote.

[15] BRAC staff assisted illiterate members with writing.

participate in the leader selection. The second dimension along which the treatments vary is the proportion of votes that a person needs to receive in order to be elected. In the vote treatment, all group members express their choice through casting a vote, and the person with most votes is elected (*plurality rule*). In the discussion treatment, instead, a leader is elected if a *consensus* is reached, even if only a subset of people decide to actively participate in the discussion. While this simultaneity makes it difficult to disentangle these two mechanisms, we believe that it makes our setup similar to what often occurs in village meetings and other direct democracy contexts.[16]

II. Conceptual Framework

In this section, we discuss the mechanisms through which our selection rules—public discussion versus private vote—can affect committee member (CM) characteristics and how these characteristics, in turn, may affect service delivery and targeting. The literature on political selection has informed our framework, highlighting differences between public and private decision making and a trade-off between leader representativeness and skills.

A. *The Effect of Selection Rules on CM Characteristics*

As explained above, the key difference between our two election rules is the introduction of a secret vote in one but not in the other. Theoretically, the introduction of a secret vote has both advantages and disadvantages. On the one hand, it may reduce elite capture and intimidation, leading to more representative CMs. On the other hand, it may reduce coordination, leading to less qualified CMs. We discuss these in turn.

Elite Capture and the Selection of Less Representative CMs.—Recent work predicts public discussion setups to result in policy outcomes that reflect the interest of more powerful community members through elite capture and intimidation (Hinnerich and Pettersson-Lidbom 2014). The introduction of a secret ballot instead allows disadvantaged citizens to vote according to their true preferences, generating election outcomes that are more representative of the preferences of the electorate (Baland and Robinson 2008).

If this mechanism is at work, we would expect the vote treatment to produce CMs who are more representative (i.e., more similar in characteristics and preferences to the average group member) than CMs emerging in a public discussion. Decision making under the vote treatment is private: each member expresses their opinion through a secret vote that has equal weight in the decision process. The discussion treatment, instead, does not require every member to speak up and thus

[16] These two mechanisms could have been separated by adding a third treatment where a public vote (e.g., a show of hands) determines the final decision. We could not add such a treatment because, due to the divided nature of society in Karamoja, imposing on every member to publicly take sides was deemed problematic by BRAC.

gives more weight to the subset of more powerful members who take active part in the discussion.[17]

Coordination and the Selection of More Qualified CMs.—The introduction of a secret vote may reduce coordination between different members and lead to lower quality decisions (Humphreys, Masters, and Sandbu 2006). This can be either because the private format is less conducive to an informative discussion about advantages and disadvantages of different possible outcomes, or because, in the private format, members may be inclined to vote for candidates they are personally connected to while in public they face pressure to support more qualified candidates. If this mechanism is at work, the privateness of the vote could result in less qualified CMs.

If leader "qualification" is negatively correlated with leader representativeness, the coordination and the elite capture mechanisms both predict the secret vote procedure to lead to more representative but less qualified CMs. This is the case in our context (as in many developing countries): education is rare and the most qualified CMs tend to be richer and therefore less representative. Since the theoretical predictions on observable CM characteristics are the same for the coordination and the elite capture mechanism, we will not be able to separate them in the data. We will, however, be able to test whether the secret vote indeed leads to different types of CMs and then use this variation to analyze how CMs' types shape group outcomes as discussed next.

B. *The Effect of CM Characteristics on Group Outcomes*

As noted by Beath et al. (2016), voters often face a trade-off between candidates who are more competent (qualified) and candidates with policy positions similar to their own (representativeness). This section outlines the way in which qualifications and representativeness of group leaders are likely to play out in our context.

We think of CM qualification as a valence issue, a quality that makes everyone in the group better off regardless of their similarity to the CMs (Besley 2005). A more qualified CM is able to accumulate more savings, allocate more loans, retain more members, and keep the group "active" for a longer period of time.

Representativeness of a CM affects different subgroups of members in different ways, depending on their similarity to the CMs. This can happen either through favoritism, for example, by CMs disproportionally approving loans to members of their own social group, or being more lenient with the repayment from such members, or by committees deciding on policy that caters more to the preferences of

[17] Elite capture can theoretically occur in two ways. First, powerful members may influence election outcomes by exerting their de facto power and intimidating others in the discussion. Second, elite capture may reduce the meeting participation rate of less powerful members, thus changing the composition of meeting participants. In our setting, all group members participated in the meeting, so the second mechanism is shut down. In other settings, in which participation is non-compulsory, the publicness of decision making is found to reduce meeting participation (Olken 2010; Madajewicz, Tompsett, and Habib 2017) with the elite being overrepresented.

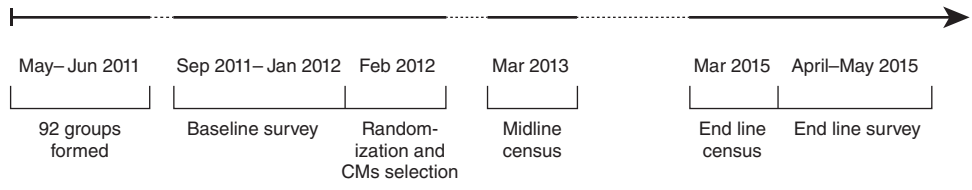

Figure 1. Project Timeline

Notes: The baseline survey covered all members present in the group at baseline (1,483 members). The midline and end line census contains information on whether the baseline respondents are still part of the group in March 2013 and 2015, the number of loans they have received, and how much they have borrowed. The end line surveyed all stayers (members who are still part of the group) and a random 40 percent of leavers (members who dropped out). All the end line results use weights to account for the fact that only a proportion of the leavers were interviewed. The weights are described in online Appendix Table A1.

the CMs' own group (Pitkin 1967; Osborne and Slivinski 1996; Besley and Coate 1997; Besley, Pande, and Rao 2005).

Empirically, we estimate the effect of CM characteristics on group outcomes by comparing post-election group outcomes across treatments. The implicit assumption is that the discussion and vote treatments affect group outcomes only through the selection of a different type of CM. As we will discuss in part C of Section V, alternative channels (such as direct treatment effect on members' satisfaction) are less relevant in our setting.

III. Data and Descriptives

A. *Data and Timeline*

The study was carried out between May 2011 and June 2015 and involves four waves of data (see Figure 1 for a representation of the timeline):

(1) Three months after the groups were formed, in September 2011–January 2012, a baseline survey was administered to all 1,483 group members. At the time of the baseline, members had already started saving but had not elected CMs.

(2) In February 2012, after the baseline was completed, the groups were randomly assigned to either the vote or the discussion treatment, and the CMs were elected in group meetings according to the assigned rule.[18] Unfortunately, the security situation in the Karamoja region in 2011 did not permit us to be present and collect detailed accounts (minutes) from each meeting. Instead, BRAC provided us with the list of group members who were elected CMs, compiled by the local BRAC staff that had supervised the meetings. We use this data, along with the baseline

[18] The committee size deviated from five in four cases: one group had four CMs, two groups had six CMs, and one group had seven CMs. These differences are not significantly correlated with group-treatment status.

data, to compare differences in predetermined characteristics between the CMs selected in the vote versus the discussion treatment.

We estimate the impact of our treatments at two points in time: at midline in 2013 and at end line in 2015 (one and three years after the committee selection).

(3) At midline (March 2013), we hired enumerators to visit each group and collect information from the group's ledger book. The ledger book contains useful information because it keeps tracks—on a weekly basis—of individual-level financial transactions from each group member. This allows us to measure: (i) whether each baseline member is still part of the group at midline, (ii) the number of loans each member has received from the group, and (iii) the total loan amount she borrowed.

(4) At end line (March 2015), we have similar data on retention, but we do not have the equivalent individual-level information on loans. This is because some ledger books went missing and/or were incomplete. To fill this gap, we administered an end line survey in April–June 2015 in which we interviewed a sample of baseline members (details on the sampling are provided in the next paragraph). This end line survey provides us with information on respondents' transactions within the group: loans received from the group, default rate, and cumulating savings. Collecting this information was facilitated by the fact that each member kept passbooks at home in which the secretary of their committee weekly recorded their deposits and their outstanding loans. We asked the enumerators to re-transcribe the information from the passbooks. When the passbook was missing, saving and loan information was self-reported and possibly under- or over-reported. Because of this caveat, we always report end line results along with midline results (which are nonself reported and do not suffer from such bias). The end line survey also collected self-reported information on financial transactions and savings outside the groups. These data allow us to estimate whether more saving/loan in BRAC's groups is substituted away from other sources or whether it translates into more credit access.

The high drop-out rate and the difficulty to track members who had left the groups made it difficult to survey all baseline members at end line. To avoid problems of unbalanced attrition, we decided to survey all 345 stayers (baseline members who were still part of the group at end line and who were easy to locate) and a random preselected 40 percent of the 1,105 leavers (baseline members who left the groups and were harder to locate). In total, our sampling strategy thus aimed to interview 787 members (345 stayers and 442 leavers), out of which we interview a total 731 members. To account for the over-sampling of stayers and the nonresponse rate, we use sample probability weights in all end line regressions (online Appendix Table A1 explains how these weights are calculated). The idea is to put more weight on the interviewed leavers who are underrepresented in our end line survey. Online Appendix Table A3 (panel A) shows that attrition is balanced: the final sample of 731 members we interviewed is comparable across treatments.

Finally, for a subset of 69 groups, we have access to group-level data collected by BRAC for auditing purposes. These data include information on overdue loans, which we use in our efficiency discussion in part A of Section V. Although the data is incomplete, we use it as a further check of our defaulting results collected in the

Table 1—Summary Statistics and Balance Checks

Sample	Vote groups			Discussion groups			p-value
	Obs. (1)	Mean (2)	SD (3)	Obs. (4)	Mean (5)	SD (6)	Vote = Discussion (7)
Panel A. Group variables							
Number of group members surveyed at baseline	46	16.26	5.54	46	16.07	5.17	0.86
Panel B. Member variables							
Basic characteristics							
Age (in years)	729	21.95	7.86	730	21.35	7.97	0.49
Married (1 = yes)	738	0.62	0.49	724	0.56	0.50	0.36
Has a job (1 = yes)	747	0.80	0.40	736	0.78	0.41	0.60
Conditional on having a job, main working activity is							
Agriculture/animal husbandry (1 = yes)	600	0.48	0.50	575	0.46	0.50	0.77
Non-agriculture business (1 = yes)	600	0.26	0.44	575	0.26	0.44	0.90
Agriculture casual day work (1 = yes)	600	0.12	0.32	575	0.13	0.33	0.79
Non-agriculture casual day work (1 = yes)	600	0.14	0.35	575	0.15	0.36	0.83
Financial access							
Has savings in BRAC group (1 = yes)	692	0.84	0.37	722	0.83	0.38	0.80
Has savings in a bank (1 = yes)	692	0.00	0.00	722	0.00	0.00	–
Has savings in a SACCO (1 = yes)	692	0.02	0.15	722	0.04	0.19	0.55
Has savings at home or with a person (1 = yes)	692	0.01	0.11	722	0.02	0.13	0.57
Total amount saved (in thousand UGX)	656	17.75	34.55	689	16.43	33.81	0.65
Has a loan from BRAC group (1 = yes)	647	0.00	0.00	663	0.00	0.00	–
Has a loan from a bank (1 = yes)	647	0.00	0.00	663	0.00	0.00	–
Has a loan from a person (1 = yes)	647	0.11	0.31	663	0.13	0.34	0.56
Total amount borrowed (in thousand UGX)	639	7.20	38.12	658	9.02	43.29	0.64
Wealth measures							
Wealth score (0 to 100)	731	23.99	17.22	718	27.44	18.11	0.13
Value of assets owned (in mln UGX)	740	2.53	6.26	727	2.87	6.11	0.54
Competence measures							
Has ever enrolled in school (1 = yes)	732	0.51	0.50	731	0.48	0.50	0.57
Has completed primary school (1 = yes)	732	0.26	0.44	731	0.21	0.41	0.19
Has participated in business training (1 = yes)	718	0.20	0.40	696	0.29	0.45	0.19
Has received advice on earning activities in past year (1 = yes)	747	0.29	0.45	736	0.29	0.45	0.99
Social background							
Has worked/studied outside village for at least a year (1 = yes)	693	0.31	0.46	677	0.30	0.46	0.82
Does not belong to majority tribe (1 = yes)	747	0.44	0.50	736	0.44	0.50	0.97
Share of members who are close friends (in percent)	747	0.10	0.11	736	0.11	0.12	0.52

Notes: This table presents summary statistics. Column 7 reports the p-value of the test of equality of means based on robust standard errors clustered at the group level (level of randomization). "Main working activity" is defined as the most time-consuming earning activity the respondent is engaged in. "Wealth score" is a score from 0 to 100 based on a scale constructed by the Grameen Foundation to measure wealth in Uganda (higher values indicate higher wealth). See Schreiner (2011b) for details on how the score is calculated. "Value of assets owned" is the total value of assets (household, agriculture, and business assets) owned by the respondent's household in millions of UGX and truncated at the top 1 percent to clean for outliers. "Has participated in business training" equals one if the respondent has ever participated in a training on business skills and/or financial literacy offered by BRAC or other providers. "Share of members who are close friends" is a normalized-degree centrality measure. For group member X, this is the percentage of members in the group who report X as being among the two best friends in the group at baseline. The number of observations vary across variables due to the presence of missing values.

end line survey. The two treatments remain balanced in the subsample of 69 groups for which the data are available (online Appendix Table A3, panel B).

B. *Summary Statistics and Balance Checks*

Table 1 reports means and standard deviations for key variables at baseline, separated by treatment. Reassuringly, these characteristics do not differ

significantly across our two treatments, indicating that the randomization yields a balanced sample.

The community groups in our study have an average size of 16 members, aged 22 on average and mostly involved in non-agriculture business. Our baseline survey was conducted after groups had opened, and consequently, most members (83 percent) reported to have savings at baseline, mainly in the group. The average amount saved was 17,000 UGX (21.61 USD, PPP-adjusted). Only 11–13 percent of the group members had an outstanding loan at baseline, and none of these loans are from a bank, a microfinance institution, or a VSLA. This demonstrates that prior to the launch of BRAC's program, there was a lack of access to financial services in the studied context.

Guided by the pro-poor nature of the BRAC program, we define representativeness in terms of observed socioeconomic characteristics such as group members *wealth*, measured using an index constructed by Schreiner (2011a). The index combines information on 10 verifiable wealth indicators and compiles them into a score from 0 to 100 that has been shown to highly correlate with wealth status as measured by expenditure surveys (Schreiner 2011b).[19] Following BRAC targeting guidelines, we define a respondent as being "poor" if she belongs to the bottom quartile of her group's wealth-score distribution. We also proxy wealth with the total value of household, agriculture, and business asset holdings.

We proxy a member's level of *qualification* related to the CM task (which involves accounting skills and experience in handling financial transactions) with three variables. The first variable is the completion of primary school (indicating literacy and basic numeracy), which is limited to 23 percent of the members. The second variable is a dummy for whether the member has business skills, which we proxy with whether she has ever attended a training on "managerial skills and small-scale income-generating activities" organized either by BRAC as part the center's program or by the government. The third variable is whether the member has access to business advice, i.e., whether she reports having received advice on earning activities from someone outside of their household in the past year.

Finally, we collected information on other traits that are positively correlated with the likelihood that a member belongs to a richer and more educated "elite:" whether a member has worked or studied outside her current district (e.g., in the capital) for at least one year and whether a member belongs to the group's minority tribe.[20]

IV. Results: Selection of Committee Members

This section studies how different participatory decision-making methods—i.e., public discussion versus private vote—affect the characteristics of selected

[19] The indicators include house materials (roof, walls, and floor), household members' ownership of shoes and clothes, access to water, sanitation, and power sources (see Schreiner 2011a,b for more details on the 10 indicators). The index is used by various institutions (including the Grameen Foundation and BRAC) to identify poor households and is not constructed for this particular paper.

[20] See online Appendix Table A2 for correlations between these variables and wealth/education. The Karamojong people are divided into five main tribes/ethnic groups: the Bokora in the west, the Pian in the east, the Matheniko in the south, the Dodoth and Jie in the north, and a non-Karamojong tribe (Pokot) in Amudat. In the locations we analyze, the Bokora tribe is the most represented tribe, on average, but also the "poorest."

Committee Members (CMs). To test this, we compare characteristics of the CMs selected in the public discussion treatment to those selected in the private vote treatment by estimating

$$Y_{igb} = \alpha + \beta Vote_g + \theta_b + \varepsilon_{igb}, \tag{1}$$

where Y_{igb} is an economic or a social characteristic (wealth, education, tribe, etc.), measured at baseline, of CM i elected in group g of branch b; $Vote_g$ equals one if the group g is assigned to the vote treatment and θ_b are branch fixed effects. Throughout the analysis, standard errors are clustered at the group level (level of randomization), and the base category is the discussion treatment.[21]

Table 2 shows that CMs elected in the vote treatment are significantly poorer: their wealth score is 18 percent lower, and their assets are worth 40 percent less. They are also 4 percent less likely to have completed primary school, 33 percent less likely ever to have received business training and 40 percent less likely to have access to business advice through their networks. They also score lower on socioeconomic proxies than their counterpart in the discussion treatment: fewer of them have worked or studied outside the village and more of them belong to the majority tribe (although this difference is not statistically significant). However, they seem to have the same number of friends in their group.[22]

Since the outcomes may be correlated, we follow Kling, Liebman, and Katz (2007) to account for the problem of multiple hypothesis testing and construct four summary indices that aggregate information over multiple outcomes: the first index aggregates the wealth variables, the second aggregates the education and training variables, the third aggregates the connection variables, and the fourth aggregates all variables. Each summary index is defined to be the equally weighted average of z-scores of its components. As shown in Table 2 (columns 9–12), the point estimates for these indices are all negative and significant in the vote treatment. For each of the outcome variables, we also follow List, Shaikh, and Xu (2016) and present p-values adjusted for the multiplicity of null hypotheses.

Altogether, these results suggest that CMs elected in the vote treatment are poorer, less educated, and have fewer market connections on average than CMs selected in the public discussion treatment. This is in line with them being less likely to be part of a local "elite." This is also shown in Figure 2: in discussion groups, the distribution of the wealth score among CMs shifts right relative to the distribution of other regular (non-CM) members, indicating that CMs are positively selected on wealth. In the vote groups, the two distributions follow each other, showing no clear selection on wealth.

As described in the conceptual framework, the presence of a positive selection in the discussion group and the absence of such selection in the vote groups is

[21] An alternative specification consists of using data from all members (elected and non-elected) and studying which member characteristic is a determinant of being elected and whether the predictive power of any characteristic differs across treatments. See online Appendix Table A4 (parts A and B) for results using this specification.

[22] Online Appendix Table A5 replicates the above analysis for each of the five CM positions separately. With the caveat that information is missing in some of the groups, note that we do not find any stark differences in the "type" of leaders selected across different committee positions. In what follows, we therefore pool all positions together.

Table 2—Differences between Committee Members across Treatments

Dependent variable	Wealth		Competence			Social background		
	Wealth score (0 to 100) (1)	Value of assets owned (2)	Has completed primary school (3)	Has ever participated in business training (4)	Has received advice on earning activities in past year (5)	Has worked/studied outside village for at least 1 year (6)	Does not belong to majority tribe (7)	Share of group members who are close friends (8)
Panel A. Wealth, competence, and social background								
Vote	−5.855	−1.350	−0.016	−0.107	−0.087	−0.142	−0.046	−0.004
	(2.295)	(0.584)	(0.055)	(0.056)	(0.047)	(0.059)	(0.043)	(0.013)
Observations (CMs)	312	316	317	310	323	307	323	323
R^2	0.276	0.066	0.067	0.074	0.168	0.123	0.552	0.068
Mean in discussion	32.684	3.349	0.386	0.320	0.333	0.357	0.522	0.156
Multiplicity-adjusted p-values	0.005	0.056	0.617	0.089	0.365	0.017	0.114	0.715

Dependent variable	Wealth index (Col. 1–2) (9)	Competence index (Col. 3–5) (10)	Social index (Col. 6–8) (11)	Aggregate index (Col. 1–2 + 3–5 + 6–8) (12)
Panel B. Indices				
Vote	−0.288	−0.205	−0.099	−0.176
	(0.075)	(0.075)	(0.055)	(0.050)
Observations (CMs)	305	291	323	278
R^2	0.214	0.113	0.160	0.151
Mean in discussion	0.010	−0.016	0.000	−0.014

Notes: This table compares committee members' characteristics across treatments. The sample is restricted to committee members. See notes of Table 1 for more details on each variable. Branch fixed effects are included in all regressions. Robust standard errors clustered at the group level are presented in brackets. "Value of assets owned" is expressed in millions of UGX and truncated at the top 1 percent to clean for outliers. Panel B presents summary indices that aggregate information over multiple outcomes: the first index aggregates the two wealth variables (columns 1 + 2), the second aggregates education and training variables (columns 3 + 4 + 5), the third aggregates connection variables (columns 6 + 7 + 8), and the fourth aggregates all variables (columns 1 to 8). Each summary index is defined to be the equally weighted average of z-scores of its components using the mean and the standard deviation in the discussion groups. "Multiplicity-adjusted p-values" present p-values adjusted for multiple outcomes following the procedure detailed in List, Shaikh, and Xu (2016).

consistent with two possible stories: (i) The vote treatment reduces elite capture or intimidation and gives more weight to the less powerful members of the group—i.e., the poorest members—to influence the political outcome of the vote. This generates leaders who are less likely to belong to the "elite" and instead are more representative of the group as a whole. In a context where education and training are rare and positively correlated with wealth, poorer leaders are also less educated and less trained. (ii) The secret vote allows for less coordination than the public discussion and therefore enables the selection of CMs that appear less qualified for the task (proxied by lower education and training). Although we are not able to fully disentangle these two stories, our treatments create variation in the type of group leaders selected. In Section V, we use this variation to shed light on the important but understudied question of whether it is better to elect more educated but less representative group leaders or vice versa.

For policy reasons, it is important to understand under what circumstances and in which groups the implementation of a secret vote can be a successful way

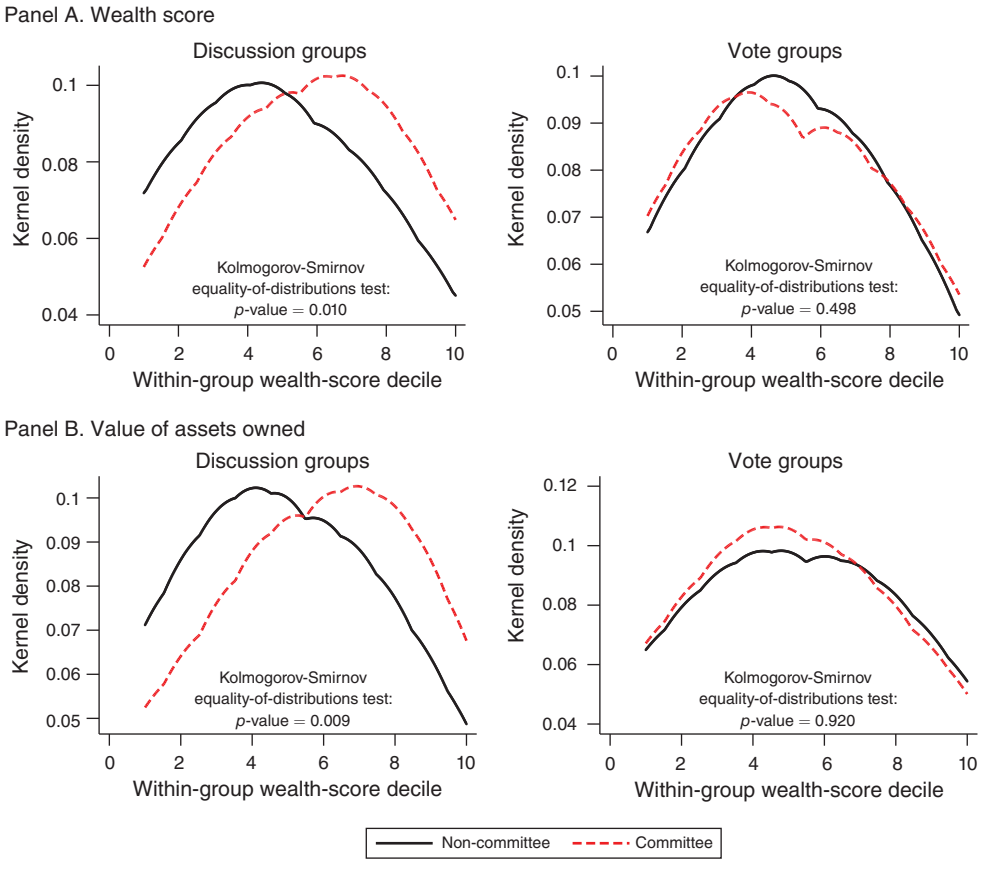

FIGURE 2. POSITION IN THE GROUP WEALTH DISTRIBUTION, COMMITTEE MEMBERS VERSUS NON-COMMITTEE MEMBERS

Notes: This figure shows the "wealth-score" and "asset-value" distribution at baseline separately for regular members (non-committee members) and committee members by treatment. The *x*-axis indicates the position (decile) of an individual in the wealth (panel A) or asset (panel B) distribution of her group. Higher value = less poor. Kernel density plot refers to the Epanechikov Kernel. The *p*-values of the Kolmogorov-Smirnov tests of the equality of distributions (non-committee members versus committee members) are reported at the bottom of each figure. "Wealth score" is a score from 0 to 100 based on a scale constructed by the Grameen Foundation to measure wealth in Uganda (higher values indicate higher wealth). "Assets value" is the value of total assets (household, agriculture, and business) owned by the respondent's household in millions of UGX.

to limit elite capture. To shed light on this question, we test for the presence of heterogeneous effects by a number of factors that may be correlated with the propensity for elite capture, namely: group size, group fractionalization (the degree to which group members belong to different tribes), and whether the group is part of a "non-pastoralist" society which have been shown to be less economically egalitarian and more socially stratified (Bollig 2000, Borgerhoff Mulder et al. 2010). Online Appendix Table A6 reveals the limited heterogenous effect by group size and tribal fractionalization. Consistent with the intuition that the public discussion format is likely to be more prone to capture in hierarchical societies, column 6 indicates that the secret vote is more effective in limiting capture (relative to the discussion

treatment) in non-pastoralist societies with more salient power structures. Finally, columns 7–10 show that the results generalize irrespective of members' experience of secret ballot voting.[23] We take this to indicate that our results can generalize to groups irrespective of their previous exposure to the specific decision-making procedure used.

V. Results: Group Inclusiveness and Sustainability

This section studies which electoral rule leads to more inclusive and sustainable groups, where inclusiveness is defined as the likelihood that a poor member is assigned a loan and retained in the group, while sustainability refers to the likelihood that the group remains operational.

A. *Member-Level Results*

To study the treatment effects on loan delivery, we estimate

$$Y_{igb} = \alpha_1 + \beta_1 Vote_g + \theta_b + \varepsilon_{igb}, \tag{2}$$

where Y_{igb} is the number of loans that member i has ever received or the total amount ever borrowed from group g (in branch b) at midline and end line. Standard errors are clustered at the group level (level of randomization), and the base category is the discussion treatment.[24]

Table 3 shows that the percentage of members that have ever received a loan is equivalent across the treatments with levels of 17 percent at midline (part A) and 39 percent at end line (part B). Similarly, the average number of loans received and the average amount of money borrowed from the group is comparable across treatments: 14,000 versus 15,000 UGX borrowed at midline in discussion and vote treatments respectively (part A), and 50,000 versus 46,000 UGX at end line (part B). While these results provide evidence that BRAC's program has substantially increased overall access to financial services between baseline and end line (more than doubling the proportion of borrowers and more than tripling the amount borrowed), the two selection rules appear to have been equally successful at achieving this.

Given our special focus on inclusiveness, we next separately identify the effects for "poor" versus "less poor" members:

$$Y_{igb} = \alpha_2 + \beta_2 Vote_g + \gamma_2 Poor_i + \delta_2 Poor_i \times Vote_g + \theta_b + \varepsilon_{igb}, \tag{3}$$

[23] This is measured in two ways: (i) with the share of group members who were old enough to vote in the 2011 secret ballot presidential election and (ii) with the distance between group location and the closest polling station in the 2011 presidential election (with the idea that the likelihood of voting decreases with distance).

[24] All end line regressions include sample weights to account for the over-sampling of stayers.

Table 3 (Part A)—Treatment Effects on Loans and Savings at Midline

Dependent variable	= 1 if received a loan in the past year from BRAC group			Number of loans received in the past year from BRAC group			Amount borrowed in the past year from BRAC group		
TRAIT (at baseline)	– (1)	Poor (2)	Has no loan (3)	– (4)	Poor (5)	Has no loan (6)	– (7)	Poor (8)	Has no loan (9)
Panel A. Midline results (2013)									
Vote	0.005	−0.008	−0.174	−0.030	−0.071	−0.385	0.977	−2.222	−6.213
	(0.039)	(0.039)	(0.106)	(0.060)	(0.062)	(0.182)	(4.770)	(5.784)	(12.118)
TRAIT		−0.051	−0.189		−0.145	−0.382		−11.133	−14.783
		(0.025)	(0.086)		(0.045)	(0.165)		(4.533)	(7.894)
Vote × TRAIT		0.045	0.201		0.120	0.395		9.085	9.425
		(0.041)	(0.098)		(0.060)	(0.170)		(5.735)	(10.852)
Observations (members)	1,427	1,394	1,260	1,445	1,411	1,278	1,365	1,334	1,210
R^2	0.074	0.072	0.077	0.080	0.079	0.091	0.039	0.045	0.049
Mean dep. var. in discussion	0.174	0.174	0.174	0.250	0.250	0.250	14.018	14.018	14.018
Mean dep. var. in discussion and NO-TRAIT		0.183	0.349		0.283	0.595		16.928	25.342
Coefficient (TRAIT + Vote × TRAIT)		−0.006	0.012		−0.024	0.013		−2.048	−5.358
p-value (TRAIT + Vote × TRAIT)		0.850	0.800		0.528	0.823		0.556	0.519

Notes: This table compares loan access, membership, and savings across treatments. "Poor" is a dummy equal to 1 if an individual belongs to the bottom 25 percent of the group wealth-score distribution at baseline. "Has no loan" is a dummy equal to one if an individual had no loans at baseline. "Amount borrowed in the past year from BRAC group" (in thousand UGX) is the total value of loans taken in the past year from BRAC group (takes a value of zero if no loans were received). All regressions include branch fixed effects and robust standard errors clustered at the group level.

where $Poor_i$ equals one if member i belongs to the bottom quartile of the group's wealth distribution, as measured with the wealth score.[25]

Table 3 shows that the composition of people who have been able to borrow from the groups substantially varies across treatments, with poorer members having greater access to loans in the vote treatment than in the discussion treatment. At midline, the poorest members (bottom quartile of their group's wealth distribution) of the discussion groups have indeed received 51 percent fewer loans and borrowed 66 percent less money than the remaining, richer members (part A). The same pattern is observed at end line, although slightly attenuated: in discussion groups, the poorest members receive 29 percent fewer loans and borrow 42 percent less money (part B).

The results for savings paint a similar picture. Since the formation of the groups until end line (2011 to 2015), the initially poor members of discussion groups saved a total of 106,000 UGX per person, while the initially poor vote group members saved 127,000 UGX per person. This corresponds to 20 percent lower savings among the poor in discussion groups. In comparison to other group members, initially poor members in discussion groups save 10 percent less while in the vote treatment they save only 3 percent less than others. Taken together, these results indicate that the introduction of a secret vote that selects more representative CMs

[25] In online Appendix Table A7, we show that results are robust, although less precise, to defining poverty as belonging to the bottom half of the wealth-score distribution or using a continuous measure of poverty (inversely proportional to the wealth score).

Table 3 (Part B)—Treatment Effects on Loans and Savings at End Line

Dependent variable	= 1 if received a loan in the past year from BRAC group			Number of loans received in the past year from BRAC group		
TRAIT (at baseline)	– (1)	Poor (2)	Has no loan (3)	– (4)	Poor (5)	Has no loan (6)
Panel B. End line results (2015)						
Vote	−0.040	−0.094	−0.198	−0.079	−0.167	−0.497
	(0.050)	(0.051)	(0.098)	(0.104)	(0.100)	(0.209)
TRAIT		−0.097	−0.205		−0.223	−0.507
		(0.051)	(0.070)		(0.092)	(0.164)
Vote × TRAIT		0.175	0.180		0.347	0.481
		(0.069)	(0.087)		(0.147)	(0.204)
Observations (members)	731	714	639	730	713	638
R^2	0.126	0.132	0.150	0.137	0.138	0.161
Mean dep. var. in discussion	0.394	0.394	0.394	0.713	0.713	0.713
Mean dep. var. in discussion and NO-TRAIT		0.423	0.522		0.766	1.087
TRAIT versus NO-TRAIT in vote treat.						
coefficient (TRAIT + Vote × TRAIT)		0.078	−0.025		0.124	−0.026
p-value (TRAIT + Vote × TRAIT)		0.099	0.645		0.287	0.840

Dependent variable	Amount borrowed in the past year from BRAC group			Amount ever saved in BRAC group		
TRAIT (at baseline)	– (7)	Poor (8)	Has no loan (9)	– (10)	Poor (11)	Has no loan (12)
Vote	−4.742	−11.528	−51.647	12.216	13.898	−3.750
	(9.434)	(10.270)	(20.324)	(19.561)	(21.107)	(27.845)
TRAIT		−24.295	−52.784		−10.981	−33.985
		(7.791)	(17.979)		(13.873)	(21.435)
Vote × TRAIT		27.905	55.956		6.701	36.401
		(11.770)	(19.298)		(24.956)	(26.611)
Observations (members)	679	664	594	642	628	560
R^2	0.120	0.126	0.158	0.224	0.224	0.262
Mean dep. var. in discussion	50.483	50.483	50.483	115.219	115.219	115.219
Mean dep. var. in discussion and NO-TRAIT		58.521	91.860		117.230	119.595
TRAIT versus NO-TRAIT in vote treat.						
coefficient (TRAIT + Vote × TRAIT)		3.610	3.172		−4.280	2.417
p-value (TRAIT + Vote × TRAIT)		0.687	0.716		0.840	0.867

Notes: This table compares loan access, membership, and savings across treatments. "Poor" is a dummy equal to 1 if an individual belongs to the bottom 25 percent of the group wealth-score distribution at baseline. "Has no loan" is a dummy equal to one if an individual had no loans at baseline. "Amount borrowed in the past year from BRAC group" (in thousand UGX) is the total value of loans taken in the past year from BRAC group (takes a value of zero if no loans were received). "Amount ever saved in BRAC group" is the amount saved since group formation (in thousand UGX). All regressions include branch fixed effects and robust standard errors clustered at the group level. End line results also include sample weights to account for the fact that across relevant subgroups, different proportions of the baseline members were interviewed at end line (see online Appendix Table A1 for more details).

leads to more inclusive groups. Interestingly, vote groups seem to be more inclusive not only toward the poorest members but also, more generally, toward members who had no loan experience and were hence financially excluded at baseline (Table 3 (part B) columns 3, 6, and 9). Vote groups thus succeeded better in boosting financial inclusion.

Differential Dropout or Differential Loan Assignment?—The finding that poor members receive fewer loans in discussion groups is likely explained by two mechanisms: (i) *differential drop-out*: poor members are more likely to exit the groups; (ii) *differential loan assignment*: poor members are less likely to be granted a loan, conditional on staying in the groups. Online Appendix Table A8 indeed shows that poor members (in the bottom quartile of the baseline wealth distribution) are more likely to leave the groups in the discussion treatment, and this partly explains why they receive fewer loans.[26] In a context with very limited access to financial services, this result has important implications: poor members remain financially more isolated in discussion than vote groups. Online Appendix Table A9 (parts A and B) instead provides suggestive evidence on loan assignment *conditional* on staying, with the obvious caveat that we are conditioning on an endogeneous variable. In discussion groups, "retained" poor members borrow 41 percent less money than other members at midline (significant at the 5 percent confidence level), while the corresponding difference in vote groups is 20 percent and not statistically significant. This indicates that, conditional on "being a stayer," a poor member is less likely to be given access to loans in discussion groups.[27]

Finally, differences in loan access between poor and richer members across treatments could also be explained by CMs being systematically more likely to assign loans to themselves than to other regular members, irrespective of the treatment. Because elected CMs are poorer in the vote treatment, this would result in a larger proportion of loans given to "poor" members in such groups. We find that such patterns do not fully explain the results: even within the sample of non-CMs, the poorest members receive significantly fewer loans in the discussion treatment (see online Appendix Table A10, parts A and B). To further corroborate this, online Appendix Table A11 compares access to loans for CMs and non-CMs. While CMs are indeed more likely to assign loans to themselves than to regular group members, this seems to be equally the case in the vote and the discussion treatment.[28]

More Inclusiveness or Lower Efficiency?—We find that in groups that elect leaders with a secret vote, as compared to public discussion groups, poor members are more likely to gain loan access. While this may be the result of the more representative CMs in vote groups making group services more equitable, another possible explanation is that the CMs elected in discussion groups—who are more educated and potentially more qualified—did not lend to poor members because doing so is inefficient. This would be the case if, for instance, poor members are

[26] Discussion groups are disportionally less likely to retain the poor; i.e., poor members drop out 7 and 2.6 percentage points more than other richer members at midline and at end line, respectively. Meanwhile, in vote groups, the likelihood to stay is the same regardless of wealth. Notice that retention rates are low in all groups, but especially so in the discussion groups (41 percent versus 55 percent in the vote treatment).

[27] The difference between the treatments has attenuated at end line, and the difference between poor and less poor members is around 45 percent (statistically significant only in the discussion treatment). For a more in-depth analysis of whether poor members are excluded in the discussion treatment, one needs to consider the ratio between the proportion of loans assigned to the poor and the proportion of the retained members who are poor. As indicated in the next section, the ratio is indeed lower in discussion than vote groups.

[28] Columns 5–6 show that, in both vote and discussion groups, CMs remain in the group substantially longer than regular members. Differences in retention across treatments are thus less stark among CMs than among regular members.

disproportionally more likely to default on their loans, or if they are more likely than other members to be given a loan outside the group.

Three sets of results suggest that this is not the case. First, end line data on the defaulting behavior of each respondent show that default rates are comparable across treatments and across wealth status (Table 4). More precisely: 10 percent of borrowers (leavers or stayers) report having fully or partially defaulted on any BRAC loan in the discussion treatment versus 10.1 percent in the vote treatment (column 1).[29] Moreover, borrowers report having defaulted on 8.9 percent versus 7.8 percent of BRAC loans in the discussion versus vote treatment, respectively (column 5). On the intensive margin, borrowers in the discussion treatment default on 7 percent of the total amount borrowed, as compared to 8.1 percent in the vote treatment (column 9). While the differences across treatments in all three default measures are small and statistically insignificant, it is important to note that the confidence intervals are wide (e.g., for "having ever defaulted on a BRAC loan," the point estimate is 0.009 with a 95 percent confidence interval ranging from −7 to 9 percentage points). Our data are thus too noisy to rule out a default effect with enough precision. Reassuringly, we find no support for the default rate in the vote treatment being strictly *higher* than in the discussion treatment (the *p*-value of a one-sided test is 0.987, see the bottom of Table 4). Also, Table 4 consistently indicates that poor members are not more likely to default than less poor members.

Second, online Appendix Table A12 shows that poor members are not more likely than richer members to get credit outside BRAC, if anything the opposite seems to hold. In a context where only 15 percent of the respondents declare having savings or loans from any non-BRAC sources at end line, we show that limiting the credit access from BRAC groups thus translates into lower overall credit access (column 6).

Finally, group-level administrative data, available for a subset of 69 groups, show no significant difference between discussion and vote groups in the amount of loans outstanding in a given month (see online Appendix Table A13). The point estimates for vote groups are consistently negative suggesting, if anything, a more efficient outcome for vote groups. Results are robust to controlling for the volume of financial activities in the group, defined either as volume of loans disbursed or volume of savings.[30]

[29] This is higher than the default rate found in other saving groups in Uganda (e.g., 3 percent in Burlando and Canidio 2017), which alleviates the concern that the default rate may be underreported.

[30] Providing credit to poor members could also backfire if this increases the chances that the least poor members exit the group (e.g., if they do not trust the poor members to repay on time), leading to a reduction in the group's total savings and cumulative loans (Burlando and Canidio 2017). We can reject this possibility in our setting: vote groups retain more poor members without reducing the number of other members retained and thus end up being significantly larger both at midline and at end line (see next section for more details on this). Moreover, as discussed above, group members save and borrow more money on average in the vote than in the discussion treatment (although not significantly more).

Table 4—Defaulting on Loans by Treatment and by Poverty

Dependent variable	End line results (2015)							
	Ever defaulted on a BRAC loan				Percent of BRAC loans on which member has defaulted			
Sample	All members (stayers or leavers) who have received at least one loan		Stayers who have received at least one loan		All members (stayers or leavers) who have received at least one loan		Stayers who have received at least one loan	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vote	0.009	−0.002	0.020	−0.004	−0.011	−0.025	−0.013	−0.037
	(0.040)	(0.048)	(0.067)	(0.081)	(0.034)	(0.039)	(0.058)	(0.064)
Poor		−0.071		−0.055		−0.073		−0.071
		(0.050)		(0.143)		(0.037)		(0.099)
Vote × Poor		0.056		0.078		0.063		0.091
		(0.071)		(0.183)		(0.051)		(0.124)
Observations (see "sample")	285	278	172	169	285	278	172	169
R^2	0.045	0.050	0.084	0.086	0.028	0.036	0.046	0.051
Mean dep. var. in discussion	0.101	0.101	0.162	0.162	0.089	0.089	0.140	0.140
Mean dep. var. in discussion for Poor = 0		0.112		0.169		0.103		0.153
p-values: Vote > Discussion	0.987		0.981		0.998		0.995	
p-values: Poor + Vote × Poor = 0		0.782		0.834		0.777		0.784

Dependent variable	End line results (2015)			
	Percent of total amount borrowed never repaid			
Sample	All members (stayers or leavers) who have received at least one loan		Stayers who have received at least one loan	
	(9)	(10)	(11)	(12)
Vote	0.011	−0.007	0.008	−0.037
	(0.025)	(0.034)	(0.038)	(0.059)
Poor		−0.061		−0.112
		(0.027)		(0.080)
Vote × Poor		0.069		0.163
		(0.054)		(0.129)
Observations (see "sample")	284	277	171	168
R^2	0.054	0.059	0.094	0.106
Mean dep. var. in discussion	0.07	0.07	0.119	0.119
Mean dep. var. in discussion for Poor = 0		0.083		0.134
p-values: Vote > Discussion	0.99		0.998	
p-values: Poor + Vote × Poor = 0		0.862		0.598

Notes: This table compares default rates across treatments using end line data. Ever defaulted on a BRAC loan: a dummy equals one if the member has ever defaulted, partially or fully, on a loan taken in the group. "Poor" is a dummy equal to 1 if an individual belongs to the bottom 25 percent of the group wealth-score distribution at baseline. All regressions include branch fixed effects and robust standard errors clustered at the group level. Regressions also include sample weights to account for the fact that across relevant subgroups, different proportions of the baseline members were interviewed at end line (see online Appendix Table A1 for more details).

B. *Group-Level Results*

In order to better understand the sustainability of the groups and the inclusiveness of their activities over time, we aggregate individual-level data and examine group-level differences across treatments.

Group Size and Sustainability.—While groups in both treatments start out similar in size, vote treatment groups end up being statistically larger. Retention rate is indeed larger in the vote than in the discussion treatment: 54 percent versus 41 percent at midline and 26 percent versus 21 percent at end line (online Appendix Table A14, columns 1 and 4). Vote groups are also more sustainable, i.e., less likely to have "collapsed" by losing all their members. At midline, about 1 percent of vote groups had lost all their members versus 15 percent of discussion groups, although the difference converges over time (columns 2 and 5). Finally, groups differ in member composition, with vote ones retaining proportionally more poor members. Individuals who initially belonged to the bottom quartile of the distribution end up representing 28 percent and 32 percent of the whole group at midline and end line, respectively. In contrast, in the discussion treatment, their share falls to 21 percent at midline and 19 percent at end line (columns 3 and 6).[31]

Group-Level Allocation of Loans.—In discussion groups, poor members comprise 20 percent of their groups and are assigned 15 percent of the amount borrowed from the group by end line (online Appendix Table A14, column 10). In vote groups, they instead comprise 32 percent of their groups and are assigned 30 percent of the total amount borrowed. The ratio between borrowings assigned to the poor and the proportion of poor members is thus smaller in discussion groups: 75 percent $(= 15/20)$ versus 94 percent $(= 30/32)$ in vote groups. Moreover, conditional on remaining in the group, poor members save almost twice as much money in the vote than in the discussion treatment (column 11, panel B). This provides further evidence that vote groups are more inclusive toward vulnerable members.

Group-Level Wealth Inequality.—In online Appendix Table A15, we examine whether higher financial inclusion in the vote treatment reduces the wealth gap between the most vulnerable group members and the rest. In columns 1–3, we report group dispersion in wealth at end line, as measured with the interquartile range for three variables: wealth score, asset value, and net wealth (calculated as assets owned plus total amount saved anywhere minus the value of outstanding loans minus interests to be paid on the loans, which we assume to be at 10 percent).[32] For all three variables, the interquartile range goes down in vote groups, although significantly so only for assets and net wealth. This provides suggestive evidence that the introduction of a secret vote reduces wealth inequality. Consistent with this, columns 4–9 show that the individual-level gap in the wealth score between the initially poorest

[31] Further evidence of the change in group size and composition over time is shown in online Appendix Figure A1, where we plot the kernel density of the number of poor members over time and across treatments.

[32] The results do not change if we exclude interests on outstanding loans.

members (bottom quartile) and the less poor members is larger in the discussion than in the vote treatment, although this difference is precisely estimated only in column 5. For poor members: the wealth score is 2.5 points (out of 100) higher in the vote than discussion treatment, and the net wealth (which accounts for savings and liabilities) is 0.429 million UGX higher in the vote treatment. In contrast, for less poor members, both the wealth score and the net wealth are lower in the vote treatment. Overall, the results suggest that wealth inequality in community groups can be reduced by the introduction of a secret vote at the leader selection stage.

C. *Leader Types and Treatment Effects*

This section shows that the individual- and group-level differences highlighted so far are most likely explained by a leader selection effect (i.e., selection rules affect the type of leaders selected), rather than by other mechanisms through which the treatments may affect group outcomes independently of leader types. The first piece of evidence is presented in online Appendix Table A16, which shows that CMs' characteristics correlate with group outcomes: committees with at least one "poor" member (versus none) more than doubles the percentage of loans assigned to the poor, while those with at least one educated member reduces it by a factor of two (column 2).

Rather than through leader selection, our treatments could also potentially affect group outcomes if one of the two procedures is perceived as more *legitimate*. This could lead to higher member satisfaction and involvement (and hence lower drop-out) in one of the two treatments, holding leader type constant. To examine this possibility we use data collected at the time of the end line survey on perceived legitimacy ("Which of the two selection procedures do you find more fair?"). We interviewed two samples of respondents: our regular end line respondents (who had been present during the leader selection in their own group) and a random subsample of 400 non-saving group members (who had experienced neither of the two leader selection procedures).[33] We find no support for the *legitimacy* channel: perceived legitimacy does not differ significantly across treatments, nor does it differ between richer and poorer group members (online Appendix Table A17). This is true both across and within treatment and both among group members (i.e., the group that had observed how the decision making unfolded) and among non-group members.[34] Differential perceived legitimacy is thus unlikely to explain the observed differences across treatments.

[33] The latter group was selected from the pool of women enrolled in the BRAC *youth clubs* at end line. We asked this question at end line rather than at baseline because asking about perception of a rule before the election took place could have affected electoral outcomes.

[34] In other words, no societal consensus exists in our context regarding which electoral rule leads to the best aggregate outcome. This is likely due to our respondents having limited experience of being active participants in any of the two procedures.

VI. Conclusion

This paper estimates the causal effect of leader selection rules on group governance and service delivery. Ninety-two saving and loan groups created through a poverty alleviation program were randomly assigned to choose their committees either publicly or in a secret ballot vote.

We find that the way leaders are selected affects both leader types and group services. The secret vote procedure generates more representative leaders, while the public discussion procedure results in leaders that are positively selected in terms of education and training. Furthermore, the introduction of a secret vote—under which more representative but less educated leaders are selected—creates groups that are more inclusive toward poor members. In our context, this shows up by vote groups displaying a higher share of loans being allocated to poor members, higher retention rates among poor members and higher saving levels among the poor in the group.

The more inclusive access to savings and credit in vote groups is not accompanied by lower efficiency: default rates on loans are similar across treatments, and there is no deterioration of the savings or lending activity in vote groups. Vote groups are also significantly less likely than discussion groups to collapse during the first year following leader selection. Overall, our findings point to the benefit of finding electoral rules that minimize elite capture, even if it comes at the expense of leader qualifications.

Moreover, the observation that introducing a secret vote and resulting changes in governance can increase short-run sustainability may provide a first step toward a solution of the well-documented problem of low sustainability of development programs (Mansuri and Rao 2012, Fishman et al. 2017).

To the best of our knowledge, this paper is the first to provide experimental evidence on the causal effects of electoral rules in group settings. As with most experimental work, internal validity is achieved within a specific context, and this may raise questions about the external validity of our results. In our case, two features are of note. First, we analyze the effect of selection rules in the context of community groups. Our results are thus of direct relevance for decisions made in community groups, among individuals who know each other well, while they may not generalize to broader settings with more asymmetric information (i.e., national elections). Second, the pool of group members we focus on are all young women with little experience being active participants in either one of the two decision-making procedures. Members with more experience could have reacted differently; for instance, by dropping out sooner from discussion groups had they expected elite capture. We have shown, however, that our results appear to hold—at least in our setting—irrespective of members' previous exposure to secret ballot voting.

Keeping these external validity concerns in mind, we believe our results provide insights for public policy in developing countries, complementing research about the effect of political selection on service delivery. Specifically, our study can inform the selection of local delivery agents who provide members of their own communities with specific services (for instance, within health and agricultural extension). Our results suggest that the way in which such agents are selected may

result in a different "type" of agent recruited and can affect service delivery and outreach. They also indicate that the success of "community driven development" projects, which consists in involving the community in decisions about public services, hinges on the method of decision making. In fact, community involvement using an inadequate decision-making rule could lead to worse outcomes than having an NGO or the government making decisions without community participation, especially in situations where there is risk of elite capture at the local level.

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