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EVIDENCE FROM A FIELD EXPERIMENT IN INDONESIA

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ABSTRACT

In developing countries, identifying the poor for redistribution or social insurance is challenging because the government lacks information about people's incomes. This paper reports the results of a field experiment conducted in 640 Indonesian villages that investigated two main approaches to solving this problem: proxy-means tests, where a census of hard-to-hide assets is used to predict consumption, and community-based targeting, where villagers rank everyone on a scale from richest to poorest. When poverty is defined using per-capita expenditure and the common PPP$2 per day threshold, we find that community-based targeting performs worse in identifying the poor than proxy-means tests, particularly near the threshold. This worse performance does not appear to be due to elite capture. Instead, communities appear to be using a different concept of poverty: the results of community-based methods are more correlated with how individual community members rank each other and with villagers' self-assessments of their own status than per-capita expenditure. Consistent with this, the community-based methods result in higher satisfaction with beneficiary lists and the targeting process.

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I. Introduction

Targeted social safety net programs have become an increasingly common tool used to address poverty (Coady, Grosh and Hoddinott, 2004). In developed countries, the selection of the beneficiaries for these programs ("targeting") is frequently accomplished through means-testing: only those with incomes below a certain threshold are eligible. However, in developing countries, where most potential recipients work in the informal sector and lack verifiable records of their earnings, credibly implementing a conventional income-based means test is much more challenging.

Consequently, in developing countries, there is an increased emphasis on targeting strategies that do not rely on directly observing incomes. In particular, there are two such strategies: proxy means tests (PMTs) and community-based targeting. In a PMT, which has been used in the Mexican Progresa/Oportunidades and Colombian Familias en Acción programs, the government collects information on assets and demographic characteristics to create a "proxy" for household consumption or income, and this proxy is in turn used for targeting. In community-based methods, such as the Bangladesh Food-For-Education program (Galasso and Ravallion, 2005) and the Albanian Economic Support safety net program (Alderman, 2002), the government allows the community or some part of it (e.g. local leaders) to select the beneficiaries. Both methods aim to address the problem of unobservable incomes: in the PMTs, the presumption is that household assets and demographic characteristics are harder to conceal from government surveyors than income; in community-based targeting, the presumption is that wealth is harder to hide from ones' neighbors than from the government.

From the perspective of a central government choosing how to design a targeted transfer program, the choice between the two approaches is generally framed as a tradeoff between the better information that communities might have versus the risk of elite capture in the community process. By focusing on assets, the PMTs aim to capture the permanent component of consumption. In the process, however, they miss out on transitory or recent shocks. For example, a family might have fallen into poverty because one of its members has fallen ill and cannot work, but because the family has a large house, the PMT may still classify it as non-poor. Neighbors, on the other hand, might know the family's true economic situation, either from spending time with them or by merely observing the way they live (e.g. the way they dress, what they buy).[1] If the community perceives that the PMT gets it wrong, political instability and a lack of legitimacy may ensue.[2]

However, while community targeting allows the use of better local information, it also opens up the possibility that targeting decisions may be based on a wide range of factors beyond poverty as defined by the government. This may be due to genuine disagreements about what "poverty" means: the utility function used by the central government to evaluate households might be based only on consumption (i.e., the government allocates transfers to maximize $U^g = \Sigma \lambda_{gi} u_i^g(c)$), whereas the utility function used by local communities may include other factors, such as a household's earning potential or its number of dependants (i.e., the community maximizes $U^c = \Sigma\lambda_{ci} u^c{}_i(c,X)$). Or the weights may be different ($\lambda_g \neq \lambda_c$). In general, it is possible that the community's decision process actually results in outcomes that are closer to what the government really wants (which is to maximize $\Sigma\, \lambda_{gi}\, u_i(c)$ where $u_i(c)$ is the true utility function that the government does not observe) than the government could achieve by maximizing U^g. However, the community process could also favor friends and relatives of the villages elites (in which case, the outcome could be worse than either maximizing U^g or maximizing $\Sigma\, \lambda_{gi}\, u_i(c)$).

[1] Seabright (1996) makes the theoretical argument that greater local information is one of the advantages of the community methods. Alderman (2002) and Galasso and Ravallion (2005) provide empirical evidence that communities may have additional information beyond the PMTs.

[2] See, for example, "Data Penerima BLT di Semarang Membingungkan" (BLT Beneficiary List in Semarang Confuses)" *Kompas* (5/15/08), "Old data disrupts cash aid delivery," *Jakarta Post* (9/6/08); "Poorest still waiting for cash aid," *Jakarta Post* (6/24/08); "Thousands protest fuel plan, cash assistance," *Jakarta Post* (5/22/08).

Given the tradeoffs involved, which method works best is ultimately an empirical question. If elite capture of community targeting is empirically important, then the PMT could dominate community targeting either based on the government's consumption-based metric or a more holistic welfare metric, since the PMT limits the opportunity for capture. If better local information is empirically important, then community targeting could dominate the PMT on both of these metrics. If a different local conception of welfare is empirically important, then the PMT may best match the government's consumption-based metric, while community targeting may work best based on alternative welfare metrics.[3]

To investigate these tradeoffs, we conducted a field experiment in 640 villages in Indonesia in collaboration with the government. In each village, the government implemented a cash transfer program that sought to distribute 30,000 Rupiah (about $3) to households that fell below location-specific poverty lines. In a randomly selected one-third of the villages, the government conducted PMT to target the poor ("PMT Method"). In another third of these villages, once again chosen at random, it employed community-based targeting ("Community Method"). Specifically, the community members were asked to rank everyone from richest to poorest during a meeting, and this ranking determined eligibility. In the remaining villages, a hybrid of the two methods was used ("Hybrid Method"): communities engaged in the ranking exercise, and then the ranks were used to limit the universe of individuals whom the government would survey. Eligibility was then determined by conducting PMT on this limited list. This hybrid method aimed to utilize the communities' knowledge, while at the same time using the

[3] Existing evidence is summarized by Coady, Grosh and Hoddinott (2004), who conduct a meta-analysis of 111 targeted anti-poverty programs in 47 countries, including 7 PMTs and 14 cases of community-based targeting. They find no difference in the performance of these two models, as measured by the fraction of total targeted resources that went to the bottom 40 percent. This may be due, in part, to the small sample sizes in these studies. Moreover, at least two sources of opposing bias may be present. First, the authors suggest that community targeting is often chosen when state capacity is limited and the community functions well together. In such places, the PMT would have fared worse had it been tried. Second, the authors suggest that many relatively small projects have used the community model, but fail to systematically report data. As such, the included examples of community-based targeting tend to be bigger and, potentially, better run than the average. These two types of biases in existing studies suggest that comparing PMT and community targeting *in the same setting* is an open empirical question.

PMT as a check on potential elite capture. The total number of beneficiaries in each village was pre-determined and held constant all three treatment groups.

We begin by evaluating the methods from the perspective of the central government. More specifically, we evaluate which method best targeted the poor according to the central government's welfare function (i.e., consumption-based poverty) and which method produced the highest satisfaction with the beneficiary list.[4] To measure targeting accuracy, we conducted a baseline survey that collected per capita expenditure data from a set of households prior to the experiment and then defined a household as poor if it fell below the common PPP$2 per day cutoff. We find that both the community and hybrid methods perform worse than the PMT: in both methods, there was a 3 percentage point (10 percent) increase in mis-targeting rates relative to the PMT. However, the community-based strategies actually do as well (if not better) at finding the very poor – those with consumption below PPP$1 per day.

Despite the worse targeting outcomes, the community methods resulted in higher satisfaction levels and greater legitimacy of the process along all dimensions that we considered. For example, community targeting resulted in 60 percent fewer complaints than the PMT, and there were many fewer reports of difficulties in distributing the actual funds in the community treatment villages. When asked ex-post about the targeting process, the community treatment villages suggested fewer modifications to the beneficiary list and reported being much more satisfied with the process.

To understand why the community methods exhibit these differences from the PMT, we examine several explanations: elite capture, the role of community effort, local concepts of poverty beyond per-capita expenditure, and local information about poverty.

To test for elite capture in the community treatment, we randomly divided the community

[4] Note that the Indonesian government's definition of poverty is based on the cost of a food and non-food bundle typical for the poor in attaining basic minimum needs.

and hybrid villages so that, in half of these villages, everyone in the community was invited to participate in the ranking meeting, whereas in the other half, only the "elites" (i.e. local community leaders such as the sub-village head, teachers, religious leaders, etc.) were invited. In addition, we gathered data in the baseline survey on which households were related to the local elites. We find no evidence of elite capture. The mis-targeting rates were the same, regardless of whether only the elites attended the meeting. Moreover, we find no evidence that households that are related to the elites are more likely to receive funds in the community treatments relative to the PMT. In fact, we find the opposite: in the community treatments, elites and their relatives are much less likely to be put on the beneficiary list, regardless of their actual income levels.

To examine the role of effort, we randomized the order in which households were considered at the meetings. This allows us to test whether the effectiveness of community targeting differs between those households that were ranked first (when the community members were still full of energy) and those ranked last (when fatigue may have set in). We find evidence that effort matters: at the start of the community meeting, targeting performance is better than in the PMT, but it worsens as the meeting proceeds.

To examine the role of preferences and information, we created and studied alternative metrics of evaluating perceptions of poverty in our baseline survey. First, we asked every survey respondent to rank a set of randomly chosen villagers from rich to poor ("survey ranks"). Second, we asked the head of the sub-village to conduct the same exercise. Finally, and perhaps most importantly, we asked each household we interviewed to subjectively assess its own welfare level. We find that the community treatment produces a ranking of villagers that is much more correlated with these three alternate metrics than the ranking produced by PMT. In other words, the community treatments moved the targeting outcomes away from a ranking based purely on per-capita consumption and towards the rankings one would obtain by polling different classes of villagers or by asking villagers to rate themselves.

There are two ways of explaining these findings: either the community has less information about different household's per-capita consumption than the PMT, or the community's conception of poverty is different from that based solely on per-capita consumption. The evidence suggests that the latter theory is what is predominantly driving the results. First, the correlation of the self-assessments with the survey ranks is higher than the correlation of the self-assessments with per-capita consumption. Thus, in assessing their own poverty, individuals (who presumably have complete information about their own poverty) use a welfare metric that looks more like what community members use to assess each other than their own per-capita consumption. Second, the survey ranks from when community members rank each other contain information about those villagers' per-capita consumption, even controlling for all variables in the PMT; i.e. the community has residual information about consumption beyond the PMT. Finally, when we investigate how the survey ranks differ from consumption, we find that communities place greater weight on factors that predict earnings capacity than would be implied simply by per-capita consumption. For example, conditional on actual per capita consumption levels, the communities consider widowed households poorer than the typical household. The fact that communities employ a different concept of poverty explains why community targeting performance might differ from the PMT, as well as why the community targeting might result in greater satisfaction levels.

The paper proceeds as follows. We discuss the empirical design in Section II, and we describe the data in Section III. In Section IV, we compare how each of the main targeting methods fared in identifying the poor. Section V tests for evidence of elite capture, while Section VI aims to understand the role of effort. In Section VII, we test whether the community and the government have different maximands. Section VIII explores the differences in the community's maximand in greater depth. Section IX concludes.

II. Experimental Design and Data

II.A. Setting

This project occurred in Indonesia, which is home to one of the largest targeted cash transfer programs in the developing world, the Direct Cash Assistance (*Bantuan Langsung Tunai*, or *BLT*) program. Launched in 2005, the BLT program provides transfers of about US $10 per month to about 19.2 million households. The targeting in this program was accomplished through a combination of community-based methods and proxy-means tests. Specifically, Central Statistics Bureau (*Badan Pusat Statistik,* or *BPS*) enumerators met with village leaders to create a list of households who could potentially qualify for the program. The BPS enumerators then conducted an asset survey and a PMT only for the listed households.

Targeting has been identified by policymakers as one of the key problems in the BLT program. Using the common PPP$2 per day poverty threshold, the World Bank estimates that 45 percent of the funds were mis-targeted to non-poor households and 47 percent of the poor were excluded from the program in 2005-2006 (World Bank, 2006).[5] Perhaps more worrisome from the government's perspective is the fact that citizens voiced substantial dissatisfaction with the beneficiary lists. Protests about mis-targeting led some village leaders to resign rather than defend the beneficiary lists to their constituents.[6] In fact, over 2,000 village officials refused to participate in the program for this reason.[7] The experiment reported in this paper was designed and conducted in collaboration with BPS to investigate the two primary targeting issues:

[5] Targeting inaccuracy has been documented in many government anti-poverty programs that offer subsidized rice, basic commodities, health insurance, and scholarships for poor households. See, for example, Olken (2006); Daly and Fane (2002); Cameron (2002); and Conn, Duflo, Dupas, Kremer and Ozier (2008).

[6] See for example: "BLT Bisa Munculkan Konflik Baru" (BLT May Create New Conflicts), *Kompas* (5/17/08), and "Kepala Desa Trauma BLT" (A Village Head's Trauma with BLT) *Kompas* (5/24/08).

[7] See for example: "Ribuan Perangkat Desa Tolak Salurkan BLT" (Thousands of Village Officials Refuse to Distribute BLT), *Kompas* 5/22/08 and "DPRD Indramayu Tolak BLT," (Village Parliament of Indramayu Refuses BLT), *Kompas*, 5/24/08.

improving targeting performance and increasing popular acceptance of the targeting results.

II.B. Sample

The sample for the experiment consists of 640 sub-villages spread across three Indonesian provinces: North Sumatra, South Sulawesi, and Central Java. The provinces were chosen to represent a broad spectrum of Indonesia's diverse geography and ethnic makeup. Within these three provinces, we randomly selected a total of 640 villages, stratifying the sample to consist of approximately 30 percent urban and 70 percent rural locations.[8] For each village, we obtained a list of the smallest administrative unit within it (a *dusun* in North Sumatra and *Rukun Tetangga (RT)* in South Sulawesi and Central Java), and randomly selected one of these sub-villages for the experiment. These sub-village units are best thought of as neighborhoods. Each sub-village contains an average of 54 households and has an elected or appointed administrative head, whom we refer to as the sub-village head.

II.C. Experimental Design

In each sub-village, the Central Statistics Bureau (BPS) and Mitra Samya, an Indonesian NGO, implemented an unconditional cash transfer program, where beneficiary households would receive a one-time, Rp. 30,000 (about $3) cash transfer. The amount of the transfer is equal to about 10 percent of the median beneficiary's monthly per-capita consumption, or a little more than one day's wage for an average laborer.[9]

Each sub-village was randomly allocated to one of the three targeting treatments that are described in detail below.[10] The number of households that would receive the transfer was set in

[8] An additional constraint was applied to the district of Serdang Bedagai because it had particularly large-sized sub-villages. All villages in this district with average populations above 100 households per sub-village were excluded. In addition, five of the originally-selected villages were replaced prior to the randomization due to an inability to reach households during the baseline survey, the village head's refusal to participate, or a conflict.

[9] While the amount of the transfer is substantially smaller than in the national BLT program (which distributed Rp. 100,000 per month), the amount is substantial enough that poor households would want to receive it. In fact, in September 2008, more than twenty people were killed during a stampede involving thousands when a local wealthy person offered to give out charity of Rp. 30,000 per person (*Kompas*, 9/15/08).

[10] Administrative costs of the three programs were $65 per village for the community targeting, $146 for the PMT, and $166 for the hybrid. Including the value of the community members' time, the cost of the community targeting

advance through a geographical targeting approach, which was applied identically in all villages such that the fraction of households in a sub-village that would receive the subsidy was held constant, on average, across the treatments. We then observed how each treatment selected the set of beneficiaries.

After the beneficiaries were finalized, the funds were distributed. To publicize the beneficiary lists, the program staff posted two copies of the list in visible locations such as roadside food stalls, mosques/churches, or the sub-village head's house. They also placed a suggestion box and a stack of complaint cards next to the list, along with a reminder about the program details and the complaint process. Depending on the sub-village head's preference, the cash distribution could occur either through door-to-door handouts or by gathering the recipients at a central location. After at least three days, the program staff collected the suggestion box.

Main Treatment 1: PMT

In the PMT treatment, the government created formulas that mapped easily observable household characteristics into a single index using regression techniques. Specifically, it created a list of 49 indicators similar to those used in Indonesia's registration of the poor in 2008, encompassing the household's home attributes (wall type, roof type, etc.), assets (TV, motorbike, etc.), household composition, and household head's education and occupation. Using pre-existing survey data, the government estimated the relationship between these variables and household per-capita consumption.[11] While it collected the same set of indicators in all regions, the government estimated district-specific formulas due to the high variance in the best predictors of poverty across districts. On average, these PMT regressions had an R^2 value of 0.48 (Appendix Table 1).

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was \$110, the cost of the PMT was \$153, and the cost of the hybrid is \$213. The high cost of targeting in the community and hybrid methods was due in part to high fixed costs of setting up the targeting systems; we expect the administrative costs to fall if the methods were scaled up.

[11] Data from Indonesia's SUSENAS (2007) and World Bank's Urban Poverty Project (2007) datasets were used to determine the weights on the PMT formula.

Government enumerators collected these indicators from all households in the PMT villages by conducting a door-to-door survey. These data were then used to calculate a computer-generated poverty score for each household using the district-specific PMT formula. A list of beneficiaries was generated by selecting the pre-determined number of households with the lowest PMT scores in each sub-village.

Main Treatment 2: Community Targeting

In the community treatment, the sub-village residents determine the list of beneficiaries through a poverty-ranking exercise. To start, a local facilitator visited each sub-village, informed the sub-village head about the program, and set a date for a community meeting. The meeting dates were set several days in advance to allow the facilitator and sub-village head sufficient time to publicize the meeting. Facilitators made door-to-door household visits in order to encourage attendance. On average, 45 percent of households attended the meeting.

At the meeting, the facilitator first explained the program. Next, he displayed a list of all households in the sub-village (from the baseline survey), and asked the attendees to correct the list if necessary. The facilitator then spent 15 minutes having the community brainstorm a list of characteristics that differentiate the poor households from the wealthy ones in their community.[12]

The facilitator then proceeded with the ranking exercise using a set of randomly-ordered index cards that displayed the names of each household in the sub-village. He hung a string from wall to wall, with one end labeled as "most well-off" (*paling mampu*) and the other side labeled as "poorest" (*paling miskin*). Then, he presented the first two name cards from the randomly-ordered stack to the community and asked, "Which of these two households is better off?" Based on the community's response, he attached the cards along the string, with the poorer household placed closer to the "poorest" end. Next, he displayed the third card and asked how this

[12] These characteristics or criteria were not used explicitly in the ranking; they were instead used to help communities think about how they would differentiate households while making the ranking decisions.

household ranked relative to the first two households. The activity continued with the facilitator placing each card one-by-one on the string until all the households had been ranked.[13] By and large, the community reached a consensus on the ranks.[14] Before the final ranking was recorded, the facilitator read the ranking aloud so adjustments could be made if necessary.

After all meetings were complete, the facilitators were provided with "beneficiary quotas" for each sub-village based on the geographic targeting procedure. Households ranked below the quota were deemed eligible. Note that prior to the ranking exercise, facilitators told the meeting attendees that the quotas were predetermined by the government, and that all households who were ranked below this quota would receive the transfer. Facilitators also emphasized that the government would not interfere with the community's ranking.

Main Treatment 3: Hybrid

The hybrid method combines the community ranking procedure with a subsequent PMT verification. In this method, the ranking exercise, described above, was implemented first. However, there was one key difference: at the start of these meetings, the facilitator announced that the lowest-ranked households, those ranked 1.5 times below the "beneficiary quotas," would be independently checked by government enumerators before the list was finalized.

After the community meetings were complete, government enumerators visited the lowest-ranked households to collect the data needed to calculate their PMT score. Beneficiary lists were then determined using the PMT formulas. Thus, it was possible, for example, that some households could become beneficiaries even if they were ranked as slightly wealthier than the beneficiary quota cutoff line on the community list (and vice versa).

[13] When at least 10 households had been ranked, the facilitator began comparing each card to the middle card (or if it was higher than the middle card, to the 75th percentile card), and so on, in order to speed up the process.

[14] If the community did not know a household or if the household's status could not be decided upon, the facilitator and several community members visited the household after the meeting and then added it to the rank list based on the information gained from the visit. In practice, only 2 of the 431 community or hybrid villages had any households that could not be ranked at the meetings (19 out of 67 households at one meeting, all of whom were boarders at a boarding house, and 5 out of 36 households at the second meeting.).

The hybrid treatment aims to take advantage of the relative benefits of both methods. First, as compared to the community method, the hybrid method's additional PMT verification phase may limit elite capture. Second, in the hybrid method, the community is incentivized to accurately rank the poorest households at the bottom of the list, as richer households would later be eliminated by the PMT. Third, as compared to the PMT treatment, the hybrid method's use of the community rankings to narrow the set of households that need to be surveyed may be potentially more cost-effective, in light of the fewer household visits required.

Community Sub-Treatments

We designed several sub-treatments in order to test three hypotheses about why the results from the community process might differ from those that resulted from the PMT treatment: elite capture, community effort, and within-community heterogeneity in preferences.

First, to test for elite capture, we randomly assigned the community and hybrid sub-villages to two groups: a "whole community" sub-treatment and an "elite" sub-treatment. In "whole community" villages, the facilitators actively recruited all community members to participate in the ranking. In the "elite" villages, meeting attendance was restricted to no more than seven invitees that were chosen by the sub-village head. Inviting at least one woman was mandatory and there was some pressure to invite individuals who are usually involved in village decision-making, such as religious leaders or school teachers. The elite meetings are smaller and hence easier to organize and run. Moreover, the elites may have the legitimacy needed (and possibly even better information) to make difficult choices. However, the danger of the elite meetings is that the elites will use their influence to funnel aid to their friends and family (Bardhan and Mookherjee, 2005).

Second, we introduced two treatments to investigate whether the efficacy of the community approach is limited by the ability or willingness of the community to put in the effort needed to make accurate community rankings. First, the order in which households were ranked was randomized in order to compare the accuracy at the start and the end of the meeting.[15] The ranking procedure is tedious: on average, it took 1.68 hours for the community to complete the rankings. For a community with the mean number of households (54), even an optimal sorting algorithm would require making 6 pair-wise comparisons by the time the last card was placed. It is thus plausible that towards the end of the longer meetings, the community members may be too tired to rank accurately. Second, in half of the meetings, the facilitator led an exercise to identify the ten poorest households in the sub-village before the ranking exercise began ("10 poorest treatment"). If effort is important, this treatment should increase accuracy by ensuring that the poor are identified before fatigue sets in.

The third set of hypotheses concerns the role of preferences. If the community results differ from the PMT results because of preferences, it is important to understand whether these preferences are broadly shared or are simply a function of who attends the meeting. Meeting times were therefore varied in order to attract different subsets of the community. Half of the meetings were randomly assigned to occur after 7:30 pm, when men who work during the day could easily attend. The rest were in the afternoon, when we expected higher female attendance.

Randomization Design and Timing

We randomly assigned each of the 640 sub-villages to the treatments as follows (see Table 1). In order to ensure experimental balance across geographic regions, we created 51 geographic strata, where each stratum consists of all villages from one or more sub-districts (*kecamatan*) and is entirely located in a single district (*kabupaten*).[16] Then, we randomly allocated sub-villages to one of the three main treatments (PMT, community, or hybrid), stratifying such that the proportion of sub-villages allocated to each main treatment was identical (up to integer

[15] Any new household cards that were added to the stack during this process were ranked last due to the logistical complexity of re-randomizing the order of the entire stack.

[16] Specifically, we first assigned each of the 68 subdistricts (*kecamatan*) in the sample to a unique stratum. We then took all subdistricts with 5 or fewer sampled subdistricts and merged them with other kecamatans in the same district, so that each of the resulting 51 strata had at least 6 sampled villages. Note that a subdistrict is the next-highest administrative unit above a village, and consist of 5-20 villages and 15,000 – 50,000 inhabitants.

constraints) within each stratum. We then randomly and independently allocated each community or hybrid sub-village to the sub-treatments, with each of these three sub-treatment randomizations stratified by stratum and main treatment.

From November to December 2008, an independent survey company conducted a census in each sub-village and then collected the baseline data. The targeting treatments and the creation of the beneficiary lists started immediately after the baseline survey was completed (December 2008 and January 2009). Fund distribution, the collection of the complaint form boxes, and interviews with the sub-village heads occurred during February 2009. Finally, the survey company conducted the endline survey in late February and early March 2009.

III. Data

We collected four main sources of data: a baseline household survey, household rankings generated by the treatments, data on the community meeting process (in community/hybrid treatments only), and data on community satisfaction. In this section, we describe the data collection effort, and then provide summary statistics and a test of the randomization.

III.A. Baseline Data

We conducted a baseline survey in November and December 2008. The survey was administered by SurveyMeter, an independent survey organization. At this point, there was no mention of the experiment to households.[17] We began by constructing a complete list of all households in the sub-village. From this census, we randomly sampled eight households from each sub-village plus the head of the sub-village, for a total sample size of 5,756 households.[18] To ensure gender balance among survey respondents, in each sub-village, households were randomized as to whether the household head or spouse of the household head would be targeted as the primary respondent. The survey included questions on demographics, their family networks in the sub-

[17] In fact, SurveyMeter enumerators were not told about the targeting experiment.
[18] In four of the sub-villages, there were seven respondents rather than eight due to respondent refusals.

village, participation in community activities, relationships with local leaders, access to existing social transfer programs, and detailed data on the households' per capita consumption.

The baseline survey also included a variety of measures of the household's subjective poverty assessments. In particular, we asked each household to rank the other eight households surveyed in their sub-village from poorest to richest. We also asked each respondent to list the five poorest and five richest households in the sub-village, as well as any households whom they considered formal or informal leaders in the sub-village. To measure "elite connectedness," we asked respondents to identify any household in the sub-village that was related by marriage or blood to those that they identified as poor, rich, or leaders. Finally, we asked respondents several subjective questions to determine how they assessed their own poverty levels.

III.B. Data on treatment results

Each of our treatments – PMT, community, and hybrid – produces a rank ordering of all households in the sub-village ("targeting rank list"). For the PMT treatment, this is the rank ordering of the PMT score, i.e. predicted per capita expenditures. For the community treatment, it is the ranking of households that was constructed during the community meetings. For the hybrid treatment, it is the final ranked list (where all households that were verified are ordered based on their PMT score, while those that were not are ordered based on their rank at the community meeting). For all treatments, we additionally collected data on which households actually received the transfer (i.e. which households fell below each sub-village quota).

III.C. Data on community meetings

For the community and hybrid sub-villages, we collected data on the meetings' functioning. We collected attendance lists, including the number of female participants.[19] The facilitators recorded the household identifiers of the ten poorest households listed in the "10 poorest

[19]The attendance form that was used in the elite sub-treatment was somewhat different than in the whole community treatment, so we verified the attendance results by treatment using data from the endline household survey. All other forms were identical across all sub-treatments.

treatment." After each meeting, the facilitators filled out a questionnaire on their perceptions of the community's interest and satisfaction with the ranking exercise.

III.D. Data on community satisfaction

After the cash disbursement was complete, we collected data on the community's satisfaction level using four different tools: suggestion boxes, sub-village head interviews, facilitator feedback, and household interviews. First, facilitators placed suggestion boxes in each sub-village along with a stack of complaint cards. Each anonymous complaint card asked three yes/no questions in a simple format: (1) Are you satisfied with the beneficiary list resulting from this program? (2) Are there any poor households not included on the list? (3) Are there any non-poor households included on the list? Second, on the day when suggestion boxes were collected, the facilitators interviewed the sub-village heads and asked about complaints submitted to them verbally.[20] Sub-village heads were also asked if they were personally satisfied with the targeting outcome. Third, each facilitator filled out feedback forms on the ease of distributing the transfer payments. Finally, in Central Java province, SurveyMeter conducted an endline survey of three households that were randomly chosen from the eight baseline survey households, using questions that were similar to those asked of the sub-village head.[21]

III.E. Summary statistics

Table 2 provides sample statistics of the key variables. Panel A shows that average monthly per capita expenditures are approximately Rp. 558,000 (about $50).

Panel B provides statistics on the mis-targeting rates. By construction, about 30 percent of the households received the cash transfer. We calculate how many households were mis-targeted using the per capita consumption data from the baseline survey to determine eligibility. Specifically, we calculated the per-capita consumption level in each province (separately by

[20]We intended to randomly re-assign facilitators' designated sub-villages after the fund distribution so that no facilitator would collect the sub-village head's feedback from an area that he or she had already visited. While this proved logistically impossible in North Sumatra, the re-assignment was implemented in the other provinces.
[21]Time and budget constraints prevented the possibility of surveying in North Sumatra and South Sulawesi.

urban and rural areas) that corresponded to the percentage of households who were supposed to receive the transfer. This threshold level is approximately equal to the PPP$2 poverty line.[22] We defined "mis-target" to be equal to 1 if either the household's per capita consumption was below the threshold line and it did not receive the transfer (exclusion errors) or if it was above the threshold line and did receive it (inclusion errors). We also calculate mis-target for subsets of the population: those below the threshold, whom we call the "poor" (divided in half into the "very poor," with per-capita consumption below approximately PPP$1, and the "near poor," with per-capita consumption between approximately PPP$1 and PPP$2) and those above the threshold, whom we call the "non-poor" (divided in half into "middle income" and "rich"). As shown in Panel B, 32 percent of the households were mis-targeted. Twenty percent of the non-poor households received it, while 53 percent of the poor were excluded. Specifically, the rich are the least likely to be mis-targeted (14 percent), while the near poor are the most likely (59 percent).[23]

Panel C provides summary statistics for several alternative metrics that can be used to gauge targeting: the rank correlation for each sub-village between one of four different metrics of household well-being and results of the targeting experiment ("targeting rank list"). By using rank correlations, we can flexibly examine the relationship between the treatment outcomes and various measures of well-being on a comparable scale. First, we compute the rank correlation with per capita consumption, which tells us how closely the final outcome is to the government's metric of well-being. Second, we compute the rank correlation with the ranks provided by the eight individual households during the baseline survey. This allows us to understand how close

[22] To see this, note that adjusting the 2005 International Price Comparison Project's PPP-exchange rate for Indonesia for inflation through the end of 2008 yields a PPP exchange rate of PPP$1 = Rp. 5549 (author's calculations based on World Bank 2008 and the Indonesian CPI). The PPP$2 per day per person poverty line therefore corresponds to per-capita consumption of Rp. 338,000 per month. In our sample, the average threshold below which households should have received the transfer is Rp. 320,000 per month, or almost exactly PPP$2 per day. The slight discrepancy is due to different regional price deflators used in the geographic targeting procedure.

[23] Note that measurement error in our consumption survey means that these mis-targeting rates are likely over-estimates of the "true" mistargeting rates. This measurement error will be identical in treatment and control, however, so it will not affect our estimate of *changes* in mistargeting across treatment conditions.

the targeting rank list is to the community member's individual beliefs about their fellow community members' well-being. Third, we compute the rank correlation with the ranks provided by the sub-village head in the baseline survey, which does the same thing for the sub-village head's views. Finally, we compute the rank correlation with respondents' self-assessment of poverty, as reported in the baseline survey.[24] This allows us to understand how closely the treatment result matches individuals' beliefs about their own well-being.

While targeting rank lists are associated with consumption rankings, they are more highly associated with the community's rankings of well-being. While the mean rank correlation between the targeting rank lists and the consumption rankings is 0.41, the mean correlation of the targeting rank list with the individual community members' ranks is 0.64, and the correlation with the sub-village head's ranks is 0.58. Finally, we observe a 0.40 correlation between the ranks from the targeted lists with the individuals' self assessments of their own poverty.

III.F. Randomization Balance Check

Before turning to the results, we first examine whether the randomization for the main treatments appears balanced across covariates. We chose ten variables for this check prior to obtaining the data from the experiment.[25] Specifically, we examined the following characteristics from the baseline survey: per capita expenditures, years of education of the household head, calculated PMT score, the share of households that are agricultural, and the years of education of the sub-village head. We also examined five village characteristics from the 2008 PODES, a census of villages conducted by BPS: log number of households, distance to district center in kilometers, log size of the village in hectares, the number of religious buildings per household, and the number of primary schools per household. We present the results from this analysis in Table 3.

[24]Specifically, each household was asked "Please imagine a six-step ladder where on the bottom (the first step) stand the poorest people and on the highest step (the sixth step) stand the richest people. On which step are you today?" Each respondent responded with a number from 1 to 6, and we use the rank of this response among respondents in a village in computing the rank correlation.

[25]In fact, we specified and documented all of the main regressions in the paper before examining the data (April 3, 2009). The document is available from the authors upon request.

In Columns 1, 2, and 3, we present the mean of each variable for the sub-villages assigned to the PMT, community, and hybrid treatments, respectively. Standard deviations are listed below the means in brackets. We present the difference in means between the community and the PMT groups in Column 4, between the hybrid and the PMT in Column 5, and between the hybrid and the community in Column 6. In Columns 7 – 9, we replicate the analysis shown in Columns 4-6, but additionally control for stratum fixed effects. Robust standard errors are shown in parentheses in Columns 4 – 9. All variables are aggregated to the sub-village level; thus each regression includes 640 observations. In the final row of Table 3, we provide the p-value of a test of joint significance of the difference across each of the outcome variables.

The sub-villages appear to be generally well-balanced across the ten characteristics. Out of the sixty individual differences presented, three are statistically significant at the 5 percent level – precisely what one would expect from random chance. All of these significant differences are in Column 9, which compares the community and hybrid methods, controlling for stratum fixed effects. Specifically, controlling for stratum fixed effects, households in community locations have less education and are less likely to be agriculturists than households in the hybrid treatment, and hybrid villages have 8 percent fewer households than community villages. Looking at the joint significance tests across all ten variables considered, without stratum fixed effects, the only jointly significant difference is between the hybrid and the community (Column 6, p-value 0.089); with stratum fixed effects (Column 9), the p-value is 0.028. All results in this paper are robust to specifications that include these additional ten control variables.

IV. Results on Targeting Performance and Satisfaction

We begin by evaluating the treatments from the government's perspective. Specifically, we examine (1) how the treatments performed in terms of targeting the poor based on per-capita consumption, the metric the government uses to assess poverty, and (2) how the treatments performed in terms of satisfaction with and legitimacy of the targeting results.

IV.A. Targeting performance based on per-capita consumption

We begin by comparing how the different targeting methods performed based on per-capita consumption levels, the metric of poverty used by the government. Specifically, as discussed above we compute location-specific poverty lines based on the PPP$2 per day consumption threshold, and then classify a household as mis-targeted if its per capita consumption levels is below the poverty line and it was not chosen as a beneficiary, or if it was above the poverty line and it was identified as a recipient (MISTARGET_{ivk}). We then examine which method minimized mis-targeting by estimating the following equation using OLS:

$$\text{MISTARGET}_{ivk} = \alpha + \beta_1 \text{COMMUNITY}_{ivk} + \beta_2 \text{HYBRID}_{\text{ivk}} + \gamma_k + \varepsilon_{ivk} \qquad (1)$$

where *i* represents a household, *v* represents a sub-village, *k* represents a stratum, and γ_k are stratum fixed effects.[26] Note that the PMT treatment is the omitted category, so β_1 and β_2 are interpretable as the impact of the community and the hybrid treatments relative to the PMT treatment. Since the targeting methods were assigned at the sub-village level, the standard errors are clustered to allow for arbitrary correlation within a sub-village.

The results, shown in Table 4, indicate that the PMT method outperforms both the community and hybrid treatment in terms of the mis-target rate that is based on consumption. Under the PMT, 30 percent of the households are mis-targeted (Column 1).[27] Both the community and hybrid methods increase the mis-targeting rate by about 3 percentage points —or about 10 percent— relative to the PMT method (significant at the ten percent level).[28]

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[26] For simplicity of interpretation, we use OLS / linear probability models for all dependent variables in Table 4. Using a probit model for the binary dependent variables produces the same signs of the results, and the same levels of statistical significance.

[27] Fluctuations in consumption between the date of the baseline survey and that of targeting could lead to overinflated mistargeting rates. To minimize this, we ensured that the targeting quickly followed the baseline survey: the average time lapse was only 44 days. We also ensured that the time elapsed between the baseline survey and the targeting was orthogonal to the treatment. Appendix Table 2 also shows that the time between survey and targeting date has no effect empirically on the mis-targeting rates, and that the interaction of time elapsed with the treatment dummies is never significant.

[28] The community treatment provides a relative ranking of households; it does not provide any indication of the absolute level of poverty. As a result, we chose the fraction of households in each sub-village that would become beneficiaries of the program through geographic targeting. For consistency, we use the geographic targeting across

Adding a rich household to the list may have different welfare implications than adding a household that is just above the poverty line. To examine this, Figure 1 graphs the log per capita consumption distribution of the beneficiaries (left panel) and non-beneficiaries (right panel) for each targeting treatment. The vertical lines in the graphs indicate the PPP$1 and PPP$2 per day poverty lines. Overall, the graphs confirm that all methods select relatively poorer households: for all methods the mode per-capita consumption for beneficiaries is below PPP$2 per day, whereas it is above PPP$2 per day for non-beneficiaries.

Examining the impact of the treatments, the left panel shows that the consumption distribution of beneficiaries derived from the PMT is centered to the left of the distribution under the community and hybrid methods. Thus, on average, the PMT identifies poorer individuals. However, the community methods select a greater percentage of beneficiaries whose log daily per-capita consumption is less than PPP$1 (the leftmost part of the distribution). Thus, the figures suggest that despite doing worse on average, the community methods may capture more of the *very* poor. Moreover, the figures suggest that all three methods contain similar proportions of richer individuals (with log income greater than about 6.5). The difference in mis-targeting across the three treatments is driven by differences in the near poor (PPP$1 to PPP$2) and the middle income group (those above the PPP$2 poverty line, but with log income less than 6.5).

We more formally examine the findings from Figure 1 in the remaining columns of Table 4. In Columns 2 and 3, we examine mis-targeting separately for the poor and the non-poor. In Columns 4 and 5, we disaggregate the non-poor into rich and middle, and in Columns 6 and 7, we disaggregate the poor by splitting them into near poor and very poor. The results confirm that much of the difference in the error rate between the community methods and the PMT occurs

all three treatments. However, by imposing this constraint on the PMT, we do not take full advantage of the fact that the PMT provides absolute measures of poverty. Taking advantage of this information, the error in the PMT is further reduced by 3-percentage points. Thus, the full-information PMT would perform 6-percentage points (or 20 percent) better that the community methods in selecting the poor. This analysis is available upon request.

near the cutoff for inclusion. Specifically, the community and hybrid methods are respectively 6.7 and 5.2 percentage points more likely to misclassify the middle non-poor (Column 5, both statistically significant at 5 percent). They are also more likely to misclassify the near poor by 4.9 and 3.1 percentage points, respectively, although these results are not individually statistically significant. In contrast, we observe much less difference between the PMT and community methods for the rich and the very poor, and in fact the point estimate suggests that the community method may actually do better among the very poor.[29]

In Column 8, we examine the average per capita consumption of beneficiaries across the three groups. As expected, given that the community treatment selects more of the very poor and also selects more individuals who are just above the PPP$2 poverty line, the average per capita consumption of beneficiaries is not substantially different between the various treatments. This suggests that even though the community treatments are more likely to mis-target the poor as defined by the PPP$2 cutoff, the welfare implications of the three methods appear similar based on the consumption metric.[30]

To end this sub-section we explore the heterogeneity in the results along several key sub-village characteristics.[31] First, we examine whether the community methods do worse in urban areas, where individuals may not know their neighbors as well. Our sample was specifically stratified along this dimension. Second, we hypothesized that the community methods may work better in areas with higher inequality, where inequality is defined as the range between the 20th and the 80th percentile per capita consumption levels, since greater inequality implies that the

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[29] In results not reported in the table, we find that the difference in the community treatment's impact on mis-targeting rates between the very poor and the non-poor is statistically significant at the 10 percent level.

[30] To maximize social welfare, the targeting method should select households with the highest average marginal utility. If utility is quadratic in per-capita consumption, marginal utility is exactly equal to per-capita consumption, so the regression in column (8) shows that there are no difference in average marginal utility across the three treatments based on this metric. In results not reported in the table, we have also confirmed that the average marginal utility of beneficiaries is the same across treatments using alternate specifications for the utility function as well, including CRRA utility with $\rho = 1$ (log), 2, 3, 4, and 5.

[31] Note that we specified these hypotheses regarding the heterogeneity of the treatment prior to obtaining the data.

rich and poor are more sharply differentiated. Third, we hypothesized that in areas where many people are related to one other by blood or marriage, they have more information about their neighbors, so the community method should work better. The results are presented in Appendix Table 3. There was *less* mis-targeting in the community treatment (relative to the PMT) in urban areas, in areas with high inequality, and in areas where many households are related. However, these effects are not significant at conventional levels.

IV.B. Satisfaction

In Table 5, we study the impacts of the treatments on the communities' satisfaction levels and the legitimacy of the targeting. Panel A presents data from the endline household survey. Panel B presents data from the follow-up survey of sub-village heads. Panel C presents the results from the anonymous comment box, the community's complaints to the village head, and the facilitator comments on the ease of distributing the transfer payments.[32]

The results from the endline survey (Panel A) show that individuals are much more satisfied with the community treatment than with the PMT or hybrid treatments. For example, in the community treatment, respondents wish to make fewer changes to the beneficiary list; they would prefer to add about one-third fewer households to the list of beneficiaries (Column 4) and subtract about one-half as many households (Column 5) than in the PMT or the hybrid treatments. Individuals in the community treatment are more likely to report that the method used was appropriate (Column 1) and are also more likely to state that they are satisfied with the program (Column 2). A joint test of the dependent variables in Panel A indicates that the community treatment differences are jointly statistically significant (p-value < 0.001).

Sub-village heads are also much more satisfied (Panel B). The sub-village head was 38 percentage points more likely to say that the targeting method was appropriate when community-

[32] For simplicity of interpretation, we use OLS/linear probability models for all dependent variables in Table 5. Using ordered probit for categorical response variables and probit for binary dependent variables produces the same signs of the results, and the same levels of statistical significance.

based targeting was used and 17 percentage points less likely to name any households that should be added to the list.

The higher levels of satisfaction were manifested in fewer complaints (Panel C). There were on average 1.09 fewer complaints in the comment box for the community treatment sub-villages relative to the PMT sub-villages, and 0.55 fewer complaints in the hybrid sub-villages relative to the PMT (Column 2). The sub-village head also reported receiving 2.68 and 2.01 fewer complaints in the community and hybrid treatment, respectively (Column 3).

The higher satisfaction levels in the community treatment led to a smoother disbursal process. First, the facilitators who distributed the cash payment were 4-6 percentage points less likely to experience difficulties while doing so in sub-villages assigned to the community or hybrid method (Panel C, Column 4). Second, the sub-village heads had a choice of how the facilitator would conduct the disbursals: they could do so in an open community meeting or, if the head felt that they would encounter problems in the village, the facilitator could distribute the transfer door-to-door. Facilitators were 8 percentage points more likely to distribute the cash in an open meeting in the sub-villages assigned to the community treatment (Panel D, Column 5). They were also 5 percentage points more likely to do so in sub-villages that were assigned to the hybrid treatment, but this result is not significant at conventional levels.[33]

[33] An important question is whether these differences in satisfaction represent changes from the act of directly participating in the process (as in Olken (forthcoming)), from knowing that some local process was followed, or from changes in the final listing of beneficiaries. Two pieces of evidence shed light on this question. First, we find no differences in our measures of satisfaction between the whole community treatments (when 48 percent of households attended the meeting) and elite community treatments (when only 17.6 percent of households attended the meeting). This finding suggests that it is either differences in the list or knowing that some type of local process was followed that drives the differences in satisfaction. Second, we computed an approximate PMT score for each individual who was surveyed in the baseline, regardless of treatment, and then we computed the rank correlation between this score and the targeting rank list that resulted from the experiment. This gives us a measure of how close the community's list would "match" the PMT. We also created a dummy variable that indicates a high correlation between these two measures, and interacted this variable with the community and hybrid treatments. There is no discernable difference across the different satisfaction measures; this implies that the higher satisfaction that was observed in the community treatment was not affected by the degree to which the community's list would match the PMT. Thus, this suggests that knowing that some type of local process was followed seems to drive the satisfaction levels. These tables were omitted for brevity, but are available from the authors upon request.

IV.C Understanding the differences between PMT and community targeting

The findings present an interesting puzzle. The results on mis-targeting suggest that the community-based methods actually do somewhat worse at identifying the poor. However, the community method results in much greater satisfaction among both citizens and the sub-village head. The following sections explore alternative explanations of why the PMT and the community methods differ: elite capture, community effort problems, heterogeneity in preferences within the villages, and differences in information.

V. Elite Capture

Community-based targeting may involve a tradeoff: it allows the government to make use of local knowledge, but it also potentially opens the door for elite capture. To the extent that elites have different social welfare weights from the community as a whole ($\lambda_e \neq \lambda_c$), greater elite control over the process should lead to more resources being directed at those households with high λ_e and worse overall targeting performance. The increased latitude for elite capture is one potential explanation for why the community targeting fared worse than the PMT.

We test for elite capture by examining the community sub-treatments that vary the level of elite control. Specifically, we would expect less elite capture in the hybrid treatment, where there is ex-post verification of the community's ranking. We would also expect more elite capture in the elite sub-treatment, when only elite members were invited to participate in the rankings. We start by re-estimating equation (1), including a dummy for the ELITE sub-treatment and, in some specifications, the interaction of ELITE and HYBRID. The results are presented in Table 6.

We first verify that the treatment had an impact on meeting attendance. Columns 1 and 2 calculate the attendance rate using data collected at the meeting, while Columns 3 and 4 calculate it using the data from the endline household survey.[34] Both measures confirm that the whole community meetings were substantially better attended than the elite-only meetings. For example, the survey data (Column 3) show that 48 percent of households attended the targeting meetings in the whole community treatment, compared to 18 percent in the elite sub-treatment.

Despite these differences in attendance, the mis-targeting rate for the elite treatment was not significantly different than for the whole community treatment (Column 5 of Table 6). In Column 6, we examine the interaction of elite and hybrid. We would expect less elite capture in hybrid treatment, where the government verifies the results. In fact, if anything we find more mis-targeting in the hybrid methods when only the elites are invited.

Overall, while the whole community meetings were more inclusive than the "elite" meetings, it does not appear that the presence of the full community affected the degree of elite capture. However, while the evidence presented in Table 6 is consistent with no elite capture, it is also consistent with the elite dominating the whole community meetings, leading to the result that both types of meetings reflect their preferences.[35] To test this, we examine whether the elites and their relatives (those with high λ_e) were more likely to be selected in both the whole community and elite meetings relative to the PMT in Table 7. Specifically, we estimate the following equation:

$$\text{MISTARGET}_{ivk} = \alpha + \beta_1 \text{COMMUNITY}_{ivk} + \beta_2 \text{HYBRID}_{ivk} + \beta_3 \text{ELITE}_{ivk} + \beta_4 \text{CONN}_{ivk} + \beta_5 (\text{COMMUNITY}_{ivk} \times \text{CONN}_{ivk}) + \beta_6 (\text{HYBRID}_{ivk} \times \text{CONN}_{ivk}) + \beta_7 (\text{ELITE}_{ivk} \times \text{CONN}_{ivk}) + \gamma_k + \varepsilon_{ivk} \qquad (2)$$

[34] Since the data in columns 1 and 2 come from the actual meetings, they are only available for the community and hybrid treatments. Since the data in columns 3 and 4 come from questions about generic targeting meetings, it is possible to report having attended a meeting (such as a meeting during the socialization of the program or a meeting about another targeted related activity) even though our project held no ranking meeting in their villages.

[35] This second story seems unlikely: the facilitators report that a few individuals dominated the conversation in only 15 percent of the meetings, and that otherwise the meetings were a full community affair.

where CONN_{ivk} is an indicator that equals one if the household is related to any of the sub-village leaders/elites, or is one of the leaders themselves.[36] Columns 1 and 2 examine the mis-targeting rate as the dependent variable, and columns 3 and 4 examine whether a household received the transfer as the dependent variable. We find little evidence of elite capture. In fact, the point estimates suggest the opposite: the elite connected households are less likely to be mis-targeted in the community and elite treatments, although the effect is not significant at conventional levels. In fact, we find that elites are actually penalized in the community meetings: elites and their relatives are about 6.7 to 7.8 percent less likely to be on the beneficiary list in the community meetings relative to PMT meetings (Columns 3 and 4).[37]

Overall, these findings suggest that the reason that the mis-targeting is worse under the community method is not due to increased elite capture of the community process.

VI. Problems with Community Effort

The community-based ranking process requires human effort to make each comparison. For example, ranking 75 households would require making at least 363 pair-wise comparisons.[38] One might imagine that the worse targeting in the community methods could result simply from fatigue as the ranking exercise progresses. We introduced two treatments to investigate the role of effort: randomization of the order in which the ranking happened and the 10 poorest treatment.

[36] Specifically, we defined an "elite connected" household as any household where 1) we interviewed the household and found that a household member held a formal leadership position in the village, such as village or sub-village head, 2) at least two of the respondents we interviewed identified the household as holding either a formal or informal (*tokoh*) leadership role in the village, or 3) a household connected by blood or marriage to any household identified in (1) or (2).

[37] It is possible that elite connected households are more likely to be connected to other households in the sub-village in general. In this case, the penalty in columns (3) and (4) may not be due to the fact that they are elite, but instead be due to the fact that the community believes that they will be "taken care of" by their relatives. In Appendix Table 4, we re-run the specifications in Table 7, now including both main effects and interacted effects of the households' general connectedness within the village (specifically, a dummy variables for whether the household is related by blood or marriage to any other household in the village) as well as elite connectedness. The elite results stay robust (both in magnitude and significance) when controlling for general connectedness.

[38] The community sorting algorithm facilitators were instructed to use is called a binary insertion sort, which has been shown to require in expectation $\log_2(n!)$ pairwise comparisons to sort a list of size n, which is the theoretical lower bound for comparison sorting (Knuth, 1998). In practice, the community may not perfectly implement this algorithm, so the number of comparisons may be even higher.

Figure 2 graphs the relationship between mis-targeting and the randomized rank order from a non-parametric Fan regression, with cluster-bootstrapped 95 percent confidence intervals shown as dashed lines. The mis-targeting rate is lowest for the first few households ranked, but then rises sharply by the 20th percentile of households. The magnitude is substantial – the point estimates imply that mis-targeting rates are between 5-10 percentage points lower for the first household than for households ranked in the latter half of the meeting.

Table 8 reports results from investigating these issues in a regression framework. Column 1 reports the results from estimating the relationship between the mis-targeting rate and the randomized rank order, which varies from 0 (household was ranked first) to 1 (the household was ranked last). The point estimate is positive, indicating a higher mis-targeting rate for households ranked later, but it is not statistically significant. In Column 2, we interact the order with the hybrid treatment. The results show that in the community treatment, there is substantially more mis-targeting at the end of the process: the first household ranked is 5.9 percentage points less likely to be mis-targeted than the last household ranked (p-value 0.11). On net, the community treatment actually does slightly better than the PMT in the beginning, but substantially worse towards the end. This effect is completely undone in the hybrid, where the random rank order and the mis-targeting rate appear unrelated. Columns 5 and 6 examine how the rank order affects whether a household receives the transfer. The results show that on average, households ranked at the end of the meeting are 4.9 percentage points more likely to be on the beneficiary list than those ranked at the start (significant at the 10% level). The additional mis-targeting from being late in the list thus comes largely from richer households ranked toward the end of the process being more likely to be on the list.

On the other hand, the ten poorest treatments, in which the poorest were identified first, had no effect on mis-targeting as shown in Columns 3 and 4. However, this may be due to the fact that most of the mis-targeting error was comprised of the near poor rather than the very poor.

Overall, what is striking is that despite conventional wisdom which emphasizes the risks of elite capture, the main weakness of the community treatment appears instead to have been the amount of sustained attention it requires from the participants in order to be effective.

VII. Does the Community Have a Different Maximand?

A third potential reason why the community produced a different outcome than the PMT is that the community is actually doing its best to identify the poor, but has different ideas about how to define a poor population. The next section tries to explain why the community's views on poverty might differ from that of per-capita consumption.

VII.A. Alternative welfare metrics

We begin by examining how the targeting outcomes compare not just against the government's metric of welfare u_g (captured by r_g,, the ranking based on per-capita consumption), but also against alternative welfare metrics. In our baseline survey, we asked eight randomly chosen members of the community to confidentially rank each other from poorest to richest. We average the ranks to construct each household's wealth rank according to the other community members, denoted r_c. To capture welfare as measured from an elite perspective, denoted r_e, we examine how the sub-village head ranked these eight other households. To measure how people assess their own poverty, denoted r_i, we asked all respondents to rate their own poverty level on a scale of 1 to 6. We computed the percentile rank of each measure to put them on the same scale.

Table 9 presents the matrix of rank correlations between these alternative welfare metrics. The correlation matrix shows that while all of the welfare metrics are positively correlated, they clearly capture different things. Of particular note is the bottom row, which shows the correlations with self-assessments. While the rank correlation of self-assessments (r_i) with consumption (r_g) is only 0.26, that with community survey ranks (r_c) is 0.45 and with the sub-village head survey ranks (r_e) is 0.41. Thus, the community and sub-village head ranks appear to capture how individuals feel about themselves better than per capita consumption.

To assess the poverty targeting results against these alternative welfare metrics, we compute the rank correlation between targeting rank list derived from the experiment and each of four welfare metrics. We then examine the effectiveness of the various targeting treatments against these different measures of well-being by estimating:

$$\text{RANKCORR}_{vkR} = \alpha + \beta_1 \text{ COMMUNITY}_{vk} + \beta_2 \text{ HYBRID}_{vk} + \gamma_k + \varepsilon_{vkR} \quad (3)$$

where RANKCORR_{vkR} is the rank correlation between the targeting rank list and the well-being measure R in sub-village v. Stratum fixed effects (γ_k) are included. The results are reported in Table 10. As the data is aggregated to the village level, each regression has 640 observations.[39]

The results provide striking evidence that per capita consumption as we measure it does not fully capture what the community calls welfare. Column 1 confirms the mis-targeting results that are shown in Table 4: both the community and hybrid treatment result in lower rank correlations with per-capita consumption than the PMT. Specifically, they are 6.5-6.7 percentage points, or about 14 percent, lower than the rank correlations obtained with PMT. However, they move away from consumption in a very clear direction – the community treatment increases the rank correlation with r_c by 24.6 percentage points, or 49 percent above the PMT level. The hybrid also increases the correlation with r_c but the magnitude is about half that of the community treatment. Thus, the verification in the hybrid appears to move the final outcome away from the community's perception of well-being. These differences are statistically significant at the 1 percent level. Results using the rank list obtained in the survey from the sub-village head (our measure of r_e) are virtually identical to the survey list obtained by the

[39]Self-assessments have 637 observations due to non-response on the self-assessment question in several villages.

community, and are also statistically significant at the 1 percent level. This provides further evidence that the community at large and the elite broadly share similar assessments of welfare.

Perhaps most importantly, we find that the community treatment increases the rank correlation between the targeting outcomes and the individual self-assessments of their own poverty (r_s) by 10.2 percentage points, or about 30 percent of the level in the PMT (significant at 1 percent). The hybrid treatment increases the same rank correlation by 7.5 percentage points. The community targeting methods are thus more likely to conform with people's self-identified welfare status.

VII.B. Are these preferences broadly shared?

The results above suggest that the ranking exercise moves the targeting process towards a welfare metric identified by community members. An important question is the degree to which this reflects the view of one group within the community about who is poor. One experimental sub-treatment was designed precisely to get at this question. In Table 11, we report the effect of changing the composition of the meeting by holding the meeting during the day, when women are more likely to be able to attend. We also consider the other sub-treatments (elite and 10 poorest) in this analysis, as they could also plausibly have affected the welfare weights of those at the meeting.

We begin by investigating the impact of having a daytime meeting on attendance. This treatment does not change the share of households in the village that attend (Columns 1 and 2). However, Column 3 confirms that the percentage of households that are represented by women is about 10 percentage points (for a total of 49 percent) higher in the day meetings than during the evening meetings.

Although the day meeting treatment affected the gender composition of the meetings, Columns 4 - 8 show that it did not affect the targeting outcomes. The elite treatment also did not affect the rank correlations with any of the various welfare metrics. Interestingly, the only sub-treatment that affected the rank correlations was the 10 poorest treatment, which increased the correlation of the treatments with ranks from self assessments. Overall, there seems to be no evidence that the identity of the subgroup doing the ranking mattered.

VIII. Understanding the Community's Maximand

The evidence so far suggests that the community has a systematic, broadly shared, notion of welfare that is not based on per-capita consumption, and that the community-based targeting methods reflect this different concept of welfare. This raises several key questions: Is the community simply mis-measuring consumption? Or does it value something other than consumption in evaluating individual welfare, i.e. is $u^g \neq u^c$? And is that the whole story or does the community also weigh the welfare of some households more than others due to other social or political reasons– i.e., are the differences also because $\lambda_g \neq \lambda_c$?

VIII.A. Does the Community Lack Information to Evaluate Consumption?

While there is no definitive way to prove that the community has all the information that is available in the PMT, the fact that those ranked early in the process were ranked at least as well as in the PMT suggests that information is not the main constraint. We can, however, test whether the community has information about consumption beyond that in the PMT. Specifically, we estimate:

$$\text{RANKIND}_{ijvk} = \alpha + \beta_1 \text{ RANKCONSUMPTION}_{ivk} + \beta_2 \text{ RANKPMTSCORE}_{jvk} + \nu_j + \varepsilon_{ijvk} \qquad (4)$$

where RANKIND_{ijvk} is household *j*'s rank of household *i* (all ranks are in percentiles), $\text{RANKCONSUMPTION}_{\text{ivk}}$ is the rank of household *i*'s per capita consumption in village *v*, and $\text{RANKPMTSCORE}_{ivk}$ is the rank of household *i*'s PMT score that is computed using the baseline data. Fixed effects for the individual providing the ranking are included (ν_j), and standard errors are clustered at the village level. The results of this analysis are presented in Column 1 of Table 12. In Column 2, we instead include all of the variables that enter the PMT score separately rather than including the rank of the PMT score.

Table 12 illustrates that the community has residual information. Consumption is still highly correlated with individuals' ranks of other households from the baseline survey even after we control for the rank from the PMT. Controlling for the rank from the PMT, a one percentile increase in consumption rank is associated with a 0.132 percentile increase in individual household ranks of the community (Column 1). This is significant at the 1 percent level. In the more flexible specification presented in Column 2, the correlation between consumption rank and survey rank remains positive (0.088) and significant at the 1 percent level.

The findings in Table 12 suggest that the community has residual information about consumption beyond that contained in the PMT score or even in the PMT variables. Moreover, the fact that almost all the PMT variables enter into the community ranks with plausible signs and magnitudes suggests that the community has most of the information in the PMT as well, but chooses to aggregate it in different ways. While we cannot completely rule out that the community lacks some information that is present in the PMT, the evidence here suggests that differences in information are not the primary drivers of the different results.

VIII.B. A Different View of Individual Welfare

Table 13 explores the relationship between the welfare metrics (community survey rank r_c, elite survey rank r_e, and self-assessment rank r_s), the targeting results in PMT, community, and hybrid villages, and a variety of household characteristics that might plausibly affect either the welfare functions (u) or the social welfare weights used in targeting (λ). In Columns 1 - 3, we present results of specifications where the dependent variable is the within-village rank of each household in the baseline survey according to different survey-based welfare metrics. In Columns 4 – 6, the dependent variable is the treatment rank, put on a corresponding metric where the lowest ranked (poorest) household in the dataset in each village is ranked 0 and the highest ranked (richest) household in the dataset in each village is ranked 1.[40] We control for the log of per capita consumption in all regressions, and therefore the coefficients can be interpreted as conditional on per-capita consumption. Thus, we identify where the community rankings deviate from ranking based on consumption. The current sub-section focuses on some of the possible deviations that come from sources of differences in *u*, while the next sub-section deals with potential differences in the λs.

We find several dimensions of differences in the *u*'s. First, we find adjustments for equivalence scales. The PMT in our setting is explicitly defined using per-capita consumption. Thus, it makes no adjustment for economies of scale in the household. By contrast, all of the community welfare functions (Columns 1-3) reveal that the community believes that there are household economies of scale, so that conditional on per-capita consumption, those in larger households are considered to have higher welfare (as in Olken, 2005). Likewise, the same is true for the community ranking – which assigns almost an identical household size premium (Column 5). Interestingly, for a given household size and consumption, all methods rank households with more kids as poorer, even though children generally cost less than adults (Deaton, 1997).

Second, the community may know more about current consumption than the PMT, which aims to capture the permanent component of consumption. For example, if two families have the same per capita consumption, the one that is more elite connected may, for example, worry less about bad shocks because it can expect to get help from rich relatives and hence have higher welfare. The community might therefore feel that elite connected households are richer than their consumption indicates. Whether or not this is the correct theory, it aligns perfectly with what we find. The community survey ranks put about a 9 percentage point premium on being elite connected, and even the elite and self-assessed survey ranks place a 4.4 and 2.5 percentage point

[40] Note that some of the variables included as explanatory variables – including household size, share of kids, household head education, and widowhood – were explicitly included in the PMT regression, which may explain why some of these variables are significant predictors of targeting in the PMT regressions.

premium, respectively. The community treatment ranks place a 5.1 percentage point premium on elite connectedness.

Similarly, there appears to be a premium for being better connected to the financial system. While total savings does not affect the rank, households that have a greater share of savings in a bank are classified as richer in both the individual surveys (Column 1-3) and the community meeting (Column 5).

Finally, households with family outside the village (who can presumably send remittances), are ranked as less poor in terms of individual ranks, sub-village head ranks and the self-assessment, though not in the community meetings.

VIII.C. A Different Weighting of Individual Welfares

There are multiple reasons why all families may not get the same effective weights (λ) in the social welfare function. The simplest story is that of discrimination against ethnic or religious minorities or other marginal community members. We find no evidence of this: ethnic minorities are more likely to be ranked as poor in the community treatment, suggesting perhaps that even extra care is paid to them in the interest of social harmony (Column 5). In addition, we find no evidence of favoring families that are more engaged with the community. Contributing labor to village projects does not affect a family's status. However, those who contribute money are viewed as rich (Column 1-3), though they are also likely to be ranked as richer by the PMT (Column 4).

Another form of discrimination may result from the community's desire to provide the "right" incentives to households. For example, a transfer may have less of a distortionary effect for "hardworking" or "deserving" families than for those that are below their earning potential. In this case, the former should get a higher weight in the community's utility function. To test this theory, we first look at the education level of the household head. Households where the household head has a primary education or less rank 2-4 percentage points poorer, conditional on their actual consumption. Similarly, households headed by a widow, those with a disability, and those where there is a serious illness are all rated poorer, conditional on actual consumption. The adjustment for widowhood is also reflected in the community treatment ranking, but not the disability and serious illness adjustments (Column 5).[41] Finally and rather interestingly, the village does not penalize those who spend a lot of money on smoking and drinking. Families with these attributes are actually ranked lower both in community surveys (Column 1-3), and community meetings (Column 5), suggesting that the village treats these preferences as problems for the family as a whole rather than as behaviors that should be punished.

IX. Conclusion

The debate regarding decentralization in targeting is usually framed in terms of the benefits of utilizing local information versus the costs of some form of malfeasance, such as elite capture. While we started with an experiment that took both of these ideas very seriously, our results point to a third (and possibly a fourth) factor as being very important: the community seems to have a widely shared objective function that the government does not necessarily share, and implementing this objective is a source of widespread satisfaction in the community. Moreover, what makes this objective function different is neither nepotism (elite capture) nor majoritarian prejudices. Rather, these preferences appear to be informed by a better understanding of factors that affect the earning potential or vulnerability of the household, such as the returns to scale within the family, incentives, and insurance, as compared to relying purely on consumption as the government does. Nor is there any evidence that the community lacks the information to identify the poor effectively—before fatigue sets in, the community process does at least as well as the PMT. The main constraint on the community's effectiveness in identifying poor families

[41] There are, of course, two interpretations of these findings. One interpretation is that households are conditioning on earnings ability – i.e., if you are highly educated but do not earn much, that is your fault and you should not receive subsidies for it. Another interpretation, however, is that education is merely another signal of poverty that is more easily observable to the community than actual consumption, though communities would need to be over-weighting this signal for this effect to produce a negative coefficient conditional on actual consumption.

based on the consumption metric appears to be the onset of fatigue. Future designs of community based methods will need to contend with this factor.

Given these findings, if targeting the poor based on consumption is the only objective, the PMT does dominate the community methods. However, it is not evident that there is a strong enough case to overrule the community's preferences in favor of the traditional consumption metric of poverty, especially given the gain in satisfaction and legitimacy. On the other hand, what is clear is that based on our evidence, there is no case for the intermediate hybrid method: it resulted in both poor targeting performance and low legitimacy. This may be because its main theoretical advantage—preventing elite capture—was not important in our setting. It is possible that perhaps alternative hybrid designs that allow the community to add some very poor households to the PMT might perform better than those that limit the universe to the PMT surveys, as the community does better at identifying those under PPP$1 per day.

The findings in this paper raise several interesting questions for further research. First, while we found little evidence of elite capture or general malfeasance of the targeting methods, it is possible that this might change over time as individuals learn to better manipulate the system. Manipulation over time has been shown to occur in some kinds of PMT systems (Camacho and Conover, 2008), but whether it would occur when the per-village allocation is fixed, and whether it would be more or less severe in community-targeted systems, are important open questions. Second, given how well the community outcomes match individual self-assessments, an important question is whether some form of self-targeting system (perhaps connected to an ordeal mechanism as in Nichols and Zeckhauser (1982)) could provide a more cost-effective method of targeting the poor. We regard these as important questions for future research.

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Table 1: Randomization Design

<table>
<tr><td colspan="3" rowspan="2">Community/Hybrid Sub-Treatments</td><td colspan="3">Main Treatments</td></tr>
<tr><td>Community</td><td>Hybrid</td><td>PMT</td></tr>
<tr><td rowspan="4">Elite</td><td rowspan="2">10 Poorest First</td><td>Day</td><td>24</td><td>23</td><td rowspan="8"></td></tr>
<tr><td>Night</td><td>26</td><td>32</td></tr>
<tr><td rowspan="2">No 10 Poorest First</td><td>Day</td><td>29</td><td>20</td></tr>
<tr><td>Night</td><td>29</td><td>34</td></tr>
<tr><td rowspan="4">Whole Community</td><td rowspan="2">10 Poorest First</td><td>Day</td><td>29</td><td>28</td></tr>
<tr><td>Night</td><td>29</td><td>23</td></tr>
<tr><td rowspan="2">No 10 Poorest First</td><td>Day</td><td>28</td><td>33</td></tr>
<tr><td>Night</td><td>20</td><td>24</td></tr>
<tr><td colspan="3">TOTAL</td><td>214</td><td>217</td><td>209</td></tr>
</table>

Notes: This table shows the results of the randomization. Each cell reports the number of sub-villages randomized to each combination of treatments. Note that the randomization of sub-villages into main treatments was stratified to be balanced in each of 51 strata. The randomization of community and hybrid subvillages into each sub-treatment (elite or full community, 10 poorest prompting or no 10 poorest prompting, and day or night) was conducted independently for each sub-treatment, and each randomization was stratified by main treatment and geographic stratum.

Table 2: Summary Statistics

Variable	Obs	Mean	Std. Dev.
Panel A: Consumption from baseline survey			
Per capita consumption (Rp. 000s)	5753	557.501	602.33
Panel B: Mis-targeting variables:			
On beneficiary list	5756	0.32	0.46
Mis-target	5753	0.32	0.47
Mis-target -- nonpoor (rich + middle)	3725	0.20	0.40
Mis-target -- poor (near + very poor)	2028	0.53	0.50
Mis-target -- rich	1843	0.14	0.35
Mis-target -- middle income	1882	0.27	0.44
Mis-target -- near poor	1074	0.59	0.49
Mis-target -- very poor	954	0.46	0.50
Panel C: Rank correlations between treatment results and…			
Per capita consumption	640	0.41	0.34
Community (excluding sub-village head)	640	0.64	0.33
Sub-village Head	640	0.58	0.41
Self-Assessment	637	0.40	0.34

Table 3: Testing Balance Between Treatment Groups

	Means			Differences, No Fixed Effects			Differences, Controlling for Stratum Fixed Effects		
	PMT	Community	Hybrid	Community - PMT	Hybrid - PMT	Hybrid - Community	Community - PMT	Hybrid - PMT	Hybrid - Community
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Average per capita expenditure (Rp. 000s)	558.576	550.579	564.295	-7.997	5.719	13.716	-1.331	11.980	13.312
	[245.845]	[220.237]	[337.172]	(22.728)	(28.535)	(27.416)	(20.661)	(25.973)	(24.913)
Average years of education of household	7.360	7.566	7.087	0.206	-0.273	-0.4785*	0.219	-0.255	-0.4739**
head among survey respondents	[2.616]	[2.644]	[2.627]	(0.256)	(0.254)	(0.254)	(0.204)	(0.200)	(0.209)
PMT score	12.467	12.519	12.474	0.052	0.007	-0.045	0.053	0.011	-0.043
(calculated from Baseline survey)	[0.436]	[0.414]	[0.423]	(0.041)	(0.042)	(0.040)	(0.037)	(0.037)	(0.037)
Pct. of households that are agricultural	45.827	42.887	48.438	-2.940	2.612	5.5515*	-3.7806*	1.264	5.0442**
	[34.889]	[33.789]	[35.038]	(3.343)	(3.391)	(3.318)	(2.060)	(2.096)	(2.027)
Years of education of RT head	8.856	8.860	8.604	0.003	-0.253	-0.256	0.033	-0.206	-0.238
	[4.018]	[4.244]	[3.796]	(0.402)	(0.379)	(0.388)	(0.352)	(0.336)	(0.335)
Log number of households	3.832	3.895	3.810	0.063	-0.022	-0.0853*	0.057	-0.028	-0.0846**
	[0.491]	[0.489]	[0.460]	(0.048)	(0.046)	(0.046)	(0.044)	(0.043)	(0.041)
Distance to kecamatan in km	0.444	0.416	0.482	-0.028	0.039	0.067	-0.029	0.038	0.0673*
	[0.652]	[0.473]	[0.431]	(0.056)	(0.054)	(0.044)	(0.050)	(0.046)	(0.037)
Log size of villages in hectares	3.105	3.271	3.282	0.166	0.177	0.011	0.1435*	0.1376*	-0.006
	[1.278]	[1.197]	[1.187]	(0.121)	(0.120)	(0.115)	(0.075)	(0.075)	(0.076)
Religious building per household	0.0070	0.0060	0.0060	-0.0004	-0.0004	-0.0001	-0.0004	-0.0005	-0.0001
	[0.0050]	[0.0050]	[0.0050]	(0.0005)	(0.0005)	(0.0005)	(0.0004)	(0.0004)	(0.0003)
Primary school per household	0.0030	0.0030	0.0030	0.0001	-0.0002	-0.0003	0.0000	-0.0002	-0.0003
	[0.0030]	[0.0030]	[0.0020]	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
P-value from joint test				0.275	0.689	0.089	0.165	0.322	0.028

Notes: An observation is a sub-village, and therefore, there are 640 observations. Standard deviations are shown in brackets in columns (1) – (3); robust standard errors are shown in parentheses in columns (4) – (9).

Table 4: Results of Different Targeting Methods on Mis-targeting Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		By Income Status		By Detailed Income Status				
Sample:	Full population	Non-poor	Poor	Rich	Middle income	Near Poor	Very Poor	Per-capita consumption of beneficiaries
Community treatment	0.031*	0.046**	0.022	0.028	0.067**	0.49	-0.013	9.933
	(0.017)	(0.018)	(0.028)	(0.021)	(0.027)	(0.038)	(0.039)	(18.742)
Hybrid treatment	0.029*	0.037**	0.009	0.020	0.052**	0.031	-0.008	-1.155
	(0.016)	(0.017)	(0.027)	(0.020)	(0.025)	(0.037)	(0.037)	(19.302)
Observations	5753	3725	2028	1843	1882	1074	954	1719
Mean in PMT treatment	0.30	0.18	0.52	0.13	0.23	0.55	0.48	366

Notes: All regressions include stratum fixed effects. Robust standard errors are shown in parentheses, adjusted for clustering at the village level. All coefficients are interpretable relative to the PMT treatment, which is the omitted category. The mean of the dependent variable in the PMT treatment is shown in the bottom row. All specifications include stratum fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Satisfaction

	(1)	(2)	(3)	(4)	(5)	(7)
	Panel A: Household Endline Survey					
	Is the method applied to determine the targeted households appropriate? (1=worst,4=best)	Are you satisfied with P2K08 activities in this sub-village in general? (1=worst,4=best)	Are there any poor HH which should be added to the list? (0=no, 1 = yes)	Number of HH that should be added from list	Number of HH that should be subtracted from list	P-value from joint test
Community treatment	0.161***	0.245***	-0.189***	-0.578***	-0.554***	<0.001
	(0.056)	(0.049)	(0.040)	(0.158)	(0.112)	
Hybrid treatment	0.018	0.063	0.020	0.078	-0.171	0.762
	(0.055)	(0.049)	(0.042)	(0.188)	(0.129)	
Observations	1089	1214	1435	1435	1435	
Mean in PMT treatment	3.243	3.042	0.568	1.458	0.968	
	Panel B: Sub-village Head Endline Survey					
	Is the method applied to determine the targeted households appropriate? (0=no, 1=yes)	In your opinion, are villagers satisfied with P2K08 activities in this sub-village in general? (1=worst,4=best)	Are there any poor HH which should be added to the list? (0=no, 1=yes)	Are there any poor HH which should be subtracted from the list? (0=no, 1=yes)		
Community treatment	0.378***	0.943***	-0.169***	-0.010		<0.001
	(0.038)	(0.072)	(0.045)	(0.020)		
Hybrid treatment	0.190***	0.528***	-0.065	-0.019		<0.001
	(0.038)	(0.071)	(0.043)	(0.019)		
Observations	636	629	640	640		
Mean in PMT treatment	0.565	2.456	0.732	0.057		
	Panel C: Comment forms and fund disbursement results					
	Number of comments in the comment box	Number of complaints in the comment box	Number of complaints received by sub-village head	Did facilitator encounter any difficulty in distributing the funds? (0=no, 1=yes)	Fund distributed in a meeting (0=no, 1=yes)	
Community treatment	-0.944	-1.085***	-2.684***	-0.062***	0.082**	0.0014
	(0.822)	(0.286)	(0.530)	(0.023)	(0.038)	0.177
Hybrid treatment	-0.364	-0.554**	-2.010***	-0.045*	0.051	
	(0.821)	(0.285)	(0.529)	(0.026)	(0.038)	
Observations	640	640	640	621	614	
Mean in PMT treatment	11.392	1.694	4.34	0.135	0.579	

Notes: All estimation is by OLS with stratum fixed effects. Using ordered probit for multiple response and probit models for binary dependent variables produces the same signs and statistical significance as the results shown. These results are available from the authors upon request.

Table 6: Elite Treatments

	(1)	(2)	(3)	(4)	(5)	(6)
	Attendance (Meeting Data)		Attendance (Survey Data)		Mis-target Dummy	
Community treatment			0.367***	0.360***	0.029	0.044**
			(0.038)	(0.044)	(0.018)	(0.020)
Hybrid treatment	0.021	0.019	0.370***	0.378***	0.027	0.013
	(0.029)	(0.036)	(0.037)	(0.043)	(0.018)	(0.020)
Elite sub-treatment	-0.062**	-0.064	-0.301***	-0.287***	0.004	-0.025
	(0.029)	(0.041)	(0.034)	(0.050)	(0.016)	(0.023)
Elite × hybrid		0.004		-0.029		0.058*
		(0.058)		(0.068)		(0.032)
Observations	431	431	287	287	5753	5753
Mean in PMT treatment	N/A	N/A	0.11	0.11	0.30	0.30

Notes: In columns (1) – (4), an observation is a village. In columns (1) and (2), the dependent variable is the number of households attending the meeting (as observed on the meeting attendance list) divided by the number of households in the village. In columns (3) and (4), the dependent variable is the share of households surveyed in the endline survey where at least 1 household member attended a targeting meeting. The PMT mean in columns (3) and (4) is not zero, because the question was worded generically to be about any targeting meeting, not just meetings associated with our project. The dependent variable in column (5) and (6) is the mis-targeting dummy, as in column (1) of Table 4. Robust standard errors in parentheses, and standard errors are adjusted for clustering at the village level in columns (5) and (6). All specifications include stratum fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Are Households Connected to Elites Treated Differently?

	(1)	(2)	(3)	(4)
	Mis-target dummy		On beneficiary list dummy	
Elite connectedness	-0.025	-0.025	-0.063***	-0.063***
	(0.021)	(0.021)	(0.021)	(0.021)
Elite connectedness	-0.015	-0.013	-0.067**	-0.078**
× community treatment	(0.035)	(0.038)	(0.033)	(0.036)
Elite connectedness	0.010	0.010	-0.013	-0.001
× hybrid treatment	(0.033)	(0.035)	(0.033)	(0.035)
Elite connectedness	-0.029	-0.034	0.041	0.064
× elite treatment	(0.031)	(0.047)	(0.030)	(0.042)
Elite connectedness		0.003		-0.047
× elite treatment × hybrid		(0.063)		(0.060)
Observations	5753	5753	5756	5756

Notes: All specifications include dummies for the community, hybrid, and elite treatment main effects, as well as stratum fixed effects; columns (2) and (4) also include a dummy for elite × hybrid. Robust standard errors in parentheses, adjusted for clustering at the village level. Dependent variable in columns (1) and (2) is the mis-target dummy for the full sample, as in column (1) of Table 4. Dependent variable in columns (3) and (4) is a dummy for being a beneficiary of the program. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Effort

	(1)	(2)	(3)	(4)	(5)	(6)
	Mis-target dummy				On beneficiary list dummy	
Household order in ranking	0.030	0.059			0.049*	0.048*
(percentile)	(0.026)	(0.037)			(0.026)	(0.029)
Household order in ranking		-0.056				0.001
× hybrid		(0.052)				(0.028)
Poorest 10 framing sub-treament			-0.006	-0.007		
			(0.016)	(0.023)		
Poorest 10 framing sub-treatment				0.002		
× hybrid				(0.031)		
Observations	3784	3784	3874	3874	3785	3785

Notes: All specifications are limited to community and hybrid villages. Columns (1) – (4) include a hybrid dummy and stratum fixed effects; columns (5) and (6) include stratum fixed effects since the total number of beneficiaries is constant in all treatments. The dependent variable in columns (1) – (4) is the mis-target dummy for the full sample, as in column (1) of Table 4. The dependent variable in columns (5) and (6) is a dummy for being chosen as a recipient, as in column (3) of Table 6.*** p<0.01, ** p<0.05, * p<0.1

Table 9: Rank correlation matrix of alternative welfare metrics

	(1)	(2)	(3)	(4)
	Consumption (r_g)	Community survey ranks (r_c)	Sub-village head survey ranks(r_e)	Self-Assessment (r_s)
Consumption (r_g)	1.000			
Community survey ranks (r_c)	0.376	1.000		
Sub-village head survey ranks(r_e)	0.334	0.737	1.000	
Self-Assessment(r_s)	0.263	0.445	0.407	1.000

Notes: This table reports the correlation matrix between the within-village ranks of the four variables listed. All correlations are statistically significantly different from 0 at the 1% level.

Table 10: Assessing targeting treatments using alternative welfare metrics

	(1)	(2)	(3)	(4)
	Consumption (r_g)	Community survey ranks (r_c)	Sub-village head survey ranks(r_e)	Self-Assessment (r_s)
Community treatment	-0.065**	0.246***	0.248***	0.102***
	(0.033)	(0.029)	(0.038)	(0.033)
Hybrid treatment	-0.067**	0.143***	0.128***	0.075**
	(0.033)	(0.029)	(0.038)	(0.033)
Observations	640	640	640	637
Mean in PMT treatment	0.451	0.506	0.456	0.343

Notes: The dependent variable is the rank correlation between the treatment outcome (i.e., the rank ordering of households generated by the PMT, community, or hybrid treatment) and the welfare metric shown in the column, where each observation is a village. Robust standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Do community meetings reflect broadly shared preferences?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Rank Correlations with:			
	Attend Meeting (Meeting Data)	Attend Meeting (HH Data)	Female Attends (Meeting Data)	Mis-target	Consumption	Community (excluding sub-village head)	Sub-village Head	Self-Assessment
Community treatment		0.349***		0.027	-0.089**	0.232***	0.180***	0.072
		(0.042)		(0.021)	(0.045)	(0.040)	(0.052)	(0.044)
Hybrid treatment	0.020	0.353***	0.008	0.026	-0.089**	0.130***	0.064	0.046
	(0.029)	(0.041)	(0.017)	(0.021)	(0.044)	(0.039)	(0.051)	(0.044)
Day meeting treatment	-0.021	0.013	0.104***	0.008	0.019	0.004	0.055	0.014
	(0.029)	(0.033)	(0.017)	(0.016)	(0.033)	(0.029)	(0.038)	(0.033)
Elite treatment	-0.064**	-0.300***	-0.085***	0.005	-0.004	-0.023	0.034	-0.017
	(0.029)	(0.033)	(0.017)	(0.016)	(0.033)	(0.029)	(0.038)	(0.033)
10 Poorest treatment	0.022	0.023	-0.010	-0.006	0.031	0.047	0.044	0.062*
	(0.029)	(0.034)	(0.018)	(0.016)	(0.033)	(0.029)	(0.038)	(0.032)
Observations	431	287	428	5753	640	640	640	637
Mean in PMT treatment		0.110		0.300	0.451	0.506	0.456	0.343

Notes: For column (3), the dependent variable is the percentage of households in the village in which a female attends the meeting, using data collected from the meeting attendance lists. *** p<0.01, ** p<0.05, * p<0.1

Table 12: Information

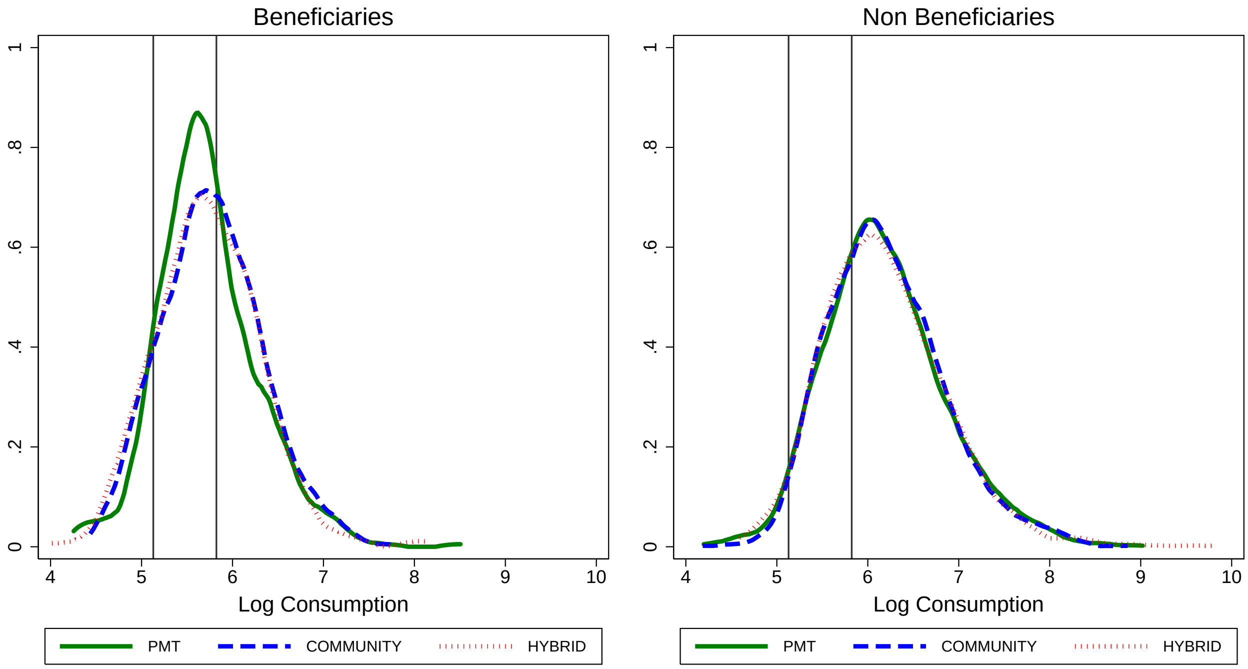
	Survey rank (1)	Survey rank (2)		Survey rank (2 continued)
Rank per capita consumption within village in percentiles	0.132***	0.088***		
	(0.014)	(0.012)		
Rank per capita consumption from PMT within village in percentiles	0.368***			
	(0.014)			
Household floor area per capita		0.001***	Has this Household ever got credit?	0.027**
		0.000		(0.011)
Not earth floor		0.060***	Number of children 0-4	0.000
		(0.010)		(0.006)
Brick or cement wall		0.065***	Number of Children in Elementary School	0.003
		(0.007)		(0.005)
Private toilet		0.047***	Number of Children in Junior High School	0.007
		(0.008)		(0.007)
Clean drinking water		0.008	Number of Children in Senior High School	0.022***
		(0.009)		(0.008)
PLN electricity		0.064***	Highest Education Attainment within HH is Elem. School	0.007
		(0.008)		(0.016)
Concrete or corrugated roof		0.027*	Highest Education Attainment within HH is Junior School	0.01
		(0.014)		(0.016)
Cooks with firewood		0.031***	Highest Education Attainment within HH is Senior High or higher	0.051***
		(0.008)		(0.017)
Own house privately		0.034***	Total Dependency Ratio	0.004
		(0.008)		(0.006)
Household size		0.004	AC	0.049**
		(0.006)		(0.023)
Household Size Squared		-0.001	Computer	0.045***
		(0.001)		(0.011)
Age of head of household		0.011***	Radio / Cassette Player	0.001
		(0.002)		(0.006)
Age of head of household squared		-0.000***	TV	0.043***
		0.000		(0.010)
Head of household is Male		0.047**	DVD/VCD player	0.017**
		(0.019)		(0.007)
Head of household is married		0.119***	Satellite dish	0.021*
		(0.022)		(0.011)
Head of household is male and Married		-0.043*	Gas burner	0.030***
		(0.026)		(0.008)
Head of household works in agriculture sector		-0.006	Refrigerator	0.069***
		(0.041)		(0.008)
Head of household works in industry Sector		-0.043	Bicycle	-0.004
		(0.042)		(0.007)
Head of household works in service Sector		-0.018	Motorcycle	0.078***
		(0.042)		(0.007)
Head of household works in formal sector		0.071	Car / Mini-bus / Truck	0.116***
		(0.045)		(0.012)
Head of household works in informal sector		0.048	HP	0.014*
		(0.045)		(0.007)
Education Attainment of HH Head is Elementary School		0.008	Jewelry	0.034***
		(0.008)		(0.006)
Education Attainment of HH Head is Junior School		0.036***	Chicken	-0.001
		(0.010)		(0.006)
Education Attainment of HH Head is Senior High School or higher		0.041***	Caribou / Cow	0.065***
		(0.011)		(0.012)
Observations	40398	38336		

Table 13: What is the community maximizing?

	Rank according to welfare metric…			Targeting Rank List in…		
	Community survey ranks (r_c)	Sub-village head survey ranks(r_e)	Self-Assessment (r_s)	PMT villages	Community villages	Hybrid villages
	(1)	(2)	(3)	(4)	(5)	(6)
Log PCE	0.176***	0.145***	0.087***	0.132***	0.197***	0.162***
	(0.008)	(0.008)	(0.004)	(0.013)	(0.014)	(0.014)
Log HH size	0.164***	0.134***	0.073***	-0.028	0.154***	0.078***
	(0.011)	(0.010)	(0.006)	(0.019)	(0.019)	(0.021)
Share kids	-0.125***	-0.094***	-0.037***	-0.296***	-0.068*	-0.141***
	(0.021)	(0.021)	(0.012)	(0.035)	(0.041)	(0.039)
HH head with primary	-0.028***	-0.025***	-0.037***	-0.108***	-0.011	-0.066***
education or less	(0.009)	(0.009)	(0.005)	(0.017)	(0.018)	(0.017)
Elite connected	0.092***	0.044***	0.025***	0.062***	0.051***	0.043***
	(0.008)	(0.009)	(0.005)	(0.016)	(0.015)	(0.015)
Ethnic minority	-0.024*	-0.019	-0.003	0.012	-0.051**	-0.011
	(0.014)	(0.014)	(0.008)	(0.026)	(0.025)	(0.024)
Religious minority	0.012	-0.007	-0.014*	-0.018	0.025	0.012
	(0.018)	(0.017)	(0.008)	(0.030)	(0.032)	(0.033)
Widow	-0.104***	-0.083***	-0.012	0.009	-0.108***	-0.026
	(0.014)	(0.014)	(0.008)	(0.027)	(0.024)	(0.028)
Disability	-0.045***	-0.037***	-0.026***	-0.079***	0.009	0.012
	(0.016)	(0.014)	(0.008)	(0.027)	(0.026)	(0.027)
Death	-0.041*	-0.031	-0.010	-0.111***	-0.013	-0.059
	(0.025)	(0.025)	(0.015)	(0.042)	(0.048)	(0.043)
Sick	-0.038***	-0.041***	-0.028***	0.007	-0.018	-0.044**
	(0.011)	(0.011)	(0.006)	(0.018)	(0.019)	(0.019)
Recent shock to	-0.001	-0.005	-0.013**	-0.019	0.009	-0.012
income	(0.009)	(0.009)	(0.005)	(0.016)	(0.016)	(0.017)
Tobacco and alcohol	-0.0002***	-0.0002***	-0.0001***	-0.0002***	-0.0002***	-0.0001***
consumption	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Total savings	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Share of savings in a	0.096***	0.069***	0.052***	0.121***	0.103***	0.075***
bank	(0.011)	(0.010)	(0.006)	(0.018)	(0.021)	(0.020)
Share of debt	0.005***	0.004***	0.002***	0.002	0.007***	0.008***
	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)
Connectedness	-0.039***	-0.021**	-0.015***	-0.016	-0.019	-0.054***
	(0.010)	(0.009)	(0.005)	(0.017)	(0.017)	(0.019)
Number of family members	0.012***	0.010***	0.006***	0.020***	0.001	0.001
outside sub-village	(0.004)	(0.003)	(0.002)	(0.006)	(0.006)	(0.006)
Participation in religious	0.027***	0.033***	0.014**	0.033**	0.012	0.029
groups	(0.010)	(0.010)	(0.006)	(0.016)	(0.017)	(0.017)
Participation through work to	0.002	0.021**	0.005	0.000	0.010	0.003
community projects	(0.011)	(0.010)	(0.006)	(0.018)	(0.019)	(0.019)
Participation through money	0.061***	0.041***	0.024***	0.056***	0.058***	0.034*
to community projects	(0.009)	(0.009)	(0.005)	(0.016)	(0.016)	(0.018)
Observations	5337	4680	5724	1814	1876	1889

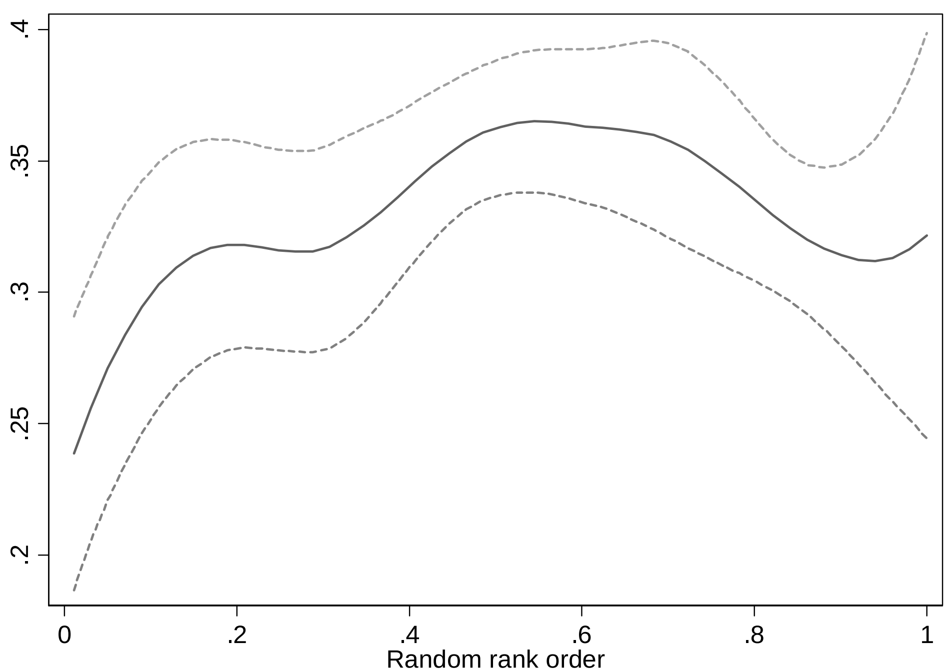
Notes: Note that the children and household head education variables are explicitly included in the PMT regression (see Table 12). The PMT regression also includes dummies for the household head being male, married, and male * married, which together will be closely correlated with the widow variable.

Figure 1: PDF of log per-capita consumption of beneficiaries and non-beneficiaries, by treatment status



Notes: The left panel shows the PDF of log per-capita consumption for those households chosen to receive the transfer, separately by each treatment. The right panel shows the PDF of log per-capita consumption for those households not chosen to receive the transfer, separately by treatment. The vertical lines show the PPP$1 and PPP$2 per day poverty lines (see footnote for more information on the calculation of these poverty lines.)

Figure 2: Effect of order in ranking meeting on mis-target rate



Notes: This figure graphs the relationship between mis-targeting and the randomized rank order from a non-parametric Fan regression. The dashed lines represent cluster-bootstrapped 95th percentile confidence intervals.

Appendices: Not For Publication

Appendix Table 1: PMT Regressions by district

District	Humbang Hasundutan	Serdang Bedagai	Pematang Siantar	Purba-lingga	Wonogiri	Demak	Kendal	Semarang	Bone	Enrekang	Tana Toraja	Makasar
Indicators												
Type of place		-0.086		-0.077	-0.068	-0.095	-0.230				0.112	
(1=Urban 0=Others)		(0.023)		(0.025)	(0.025)	(0.020)	(0.017)				(0.031)	
Percapita Floor			0.004			0.001		0.001	0.001	0.001	0.002	0.003
			(0.001)			(0.001)		(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Type of Floor	-0.100	0.113	0.149	0.118	0.133	0.111	0.096	0.169		-0.108		0.088
(1=Not earth 0=Others)	(0.024)	(0.026)	(0.031)	(0.018)	(0.017)	(0.018)	(0.015)	(0.027)		(0.021)		(0.028)
Type of Wall	0.104		0.053		0.055		0.033		0.119	0.059	0.114	
(1=Brick/Cement 0=Others)	(0.027)		(0.022)		(0.018)		(0.018)		(0.026)	(0.028)	(0.038)	
Toilet Facility	0.056	0.094	0.184	0.127	0.066	0.094	0.123	0.073	0.103	0.033	0.087	0.140
(1=Private 0=Others)	(0.015)	(0.022)	(0.034)	(0.017)	(0.019)	(0.019)	(0.015)	(0.020)	(0.017)	(0.014)	(0.019)	(0.022)
Drinking Water source	0.035		0.112	0.044			0.064	0.100	-0.047	0.100	0.028	0.117
(1=Clean 0=Other)	(0.014)		(0.036)	(0.017)			(0.014)	(0.019)	(0.018)	(0.013)	(0.016)	(0.020)
Electricity (1=PLN 0=Others)		0.113	0.294	0.112	0.177	0.125	0.286	0.286	0.190	0.093		
		(0.033)	(0.074)	(0.034)	(0.077)	(0.043)	(0.081)	(0.123)	(0.021)	(0.021)		
Type of Roof	0.078	0.085	0.108		-0.208	-0.121	-0.037	-0.075	0.178	0.093	0.095	0.122
(1=Concrete/Corrugated 0=Others)	(0.034)	(0.042)	(0.046)		(0.021)	(0.024)	(0.018)	(0.021)	(0.057)	(0.032)	(0.053)	(0.028)
Fuel for Cooking	0.178	0.155	0.274	0.188	0.172	0.155	0.168	0.152	0.229	0.145	0.074	0.188
(1=Not Firewood 0=Other)	(0.033)	(0.021)	(0.019)	(0.027)	(0.038)	(0.023)	(0.018)	(0.018)	(0.030)	(0.021)	(0.036)	(0.018)
Ownership of house	0.060			0.080	0.076		0.102	0.077				0.087
(1=Private 0=Others)	(0.021)			(0.042)	(0.035)		(0.022)	(0.019)				(0.021)
Having Micro Credit		0.129		0.098	0.165	0.209	0.069	-0.106		0.045	0.227	0.304
		(0.073)		(0.036)	(0.050)	(0.045)	(0.023)	(0.049)		(0.022)	(0.060)	(0.111)
Household Size	-0.287	-0.247	-0.261	-0.314	-0.330	-0.254	-0.277	-0.378	-0.249	-0.250	-0.209	-0.293
	(0.018)	(0.033)	(0.026)	(0.025)	(0.021)	(0.027)	(0.025)	(0.021)	(0.017)	(0.015)	(0.019)	(0.018)
Household Size Squared	0.017	0.012	0.014	0.019	0.021	0.014	0.016	0.026	0.014	0.015	0.012	0.016
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Age of the head of household	0.010	0.013	0.004	0.012			0.012		0.009	0.007	0.004	0.007
	(0.004)	(0.005)	(0.001)	(0.004)			(0.004)		(0.004)	(0.003)	(0.001)	(0.004)
Age of the head of household Squared	0.000	0.000		0.000	0.000	0.000	0.000	0.000	0.000	0.000		0.000
	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)		(0.000)
Head of household gender	0.153	0.193	0.109	0.092	0.098	0.171	0.153		0.070	0.145	0.101	0.135
(1=Male 0=Female)	(0.037)	(0.073)	(0.057)	(0.026)	(0.037)	(0.049)	(0.040)		(0.024)	(0.029)	(0.038)	(0.036)
Head of household is Married	0.166				-0.066	-0.086						0.119
	(0.083)				(0.034)	(0.050)						(0.068)
Head of household is Male*Married	-0.207	-0.141					-0.074			-0.060	-0.075	-0.230
	(0.089)	(0.071)					(0.038)			(0.026)	(0.035)	(0.076)
Sector of HH Head is Agriculture		-0.066			-0.071				-0.080			-0.138
		(0.021)			(0.022)				(0.021)			(0.058)
Sector of HH Head is Industry	0.240		-0.060	0.105				-0.121		-0.108	-0.101	-0.110
	(0.073)		(0.029)	(0.020)				(0.019)		(0.031)	(0.044)	(0.036)
Sector of HH Head is Service	0.248			0.145	0.098	0.107	0.053			0.089		-0.065
	(0.028)			(0.021)	(0.027)	(0.020)	(0.019)			(0.026)		(0.027)
Sector of HH Head is in Formal			0.082			0.036	0.076	0.058	0.056	0.083	0.089	0.040
Sector			(0.023)			(0.019)	(0.018)	(0.018)	(0.031)	(0.031)	(0.031)	(0.020)
Sector of HH Head is in Informal	0.045		0.045						0.077	0.051	0.140	
Sector	(0.024)		(0.023)						(0.020)	(0.020)	(0.020)	
Education Attaintment of HH Head	0.053	0.152		0.031	0.061	0.183	0.041	0.115	0.053		0.063	
is Elementary School	(0.023)	(0.040)		(0.028)	(0.025)	(0.035)	(0.018)	(0.028)	(0.019)		(0.032)	
Education Attaintment of HH Head	0.072	0.164	0.198	0.166	0.153	0.164	0.128	0.204	0.099	0.081	0.080	0.156
is Junior School	(0.027)	(0.040)	(0.031)	(0.032)	(0.028)	(0.036)	(0.026)	(0.054)	(0.030)	(0.016)	(0.031)	(0.035)
Education Attaintment of HH Head	0.096	0.194	0.143	0.140	0.113	0.088	0.198	0.317	0.099	0.129	0.170	0.196

is Senior +	(0.033)	(0.045)	(0.032)	(0.051)	(0.036)	(0.045)	(0.032)	(0.035)	(0.035)	(0.020)	(0.036)	(0.053)
Number of children 0-4	-0.043							-0.078	-0.029	-0.044		-0.028
	(0.011)							(0.017)	(0.013)	(0.010)		(0.016)
Number of Children in Elementary School												
Number of Children in Junior H.School		0.056		0.056	0.086	0.072	0.045	0.073				
		(0.022)		(0.018)	(0.020)	(0.019)	(0.017)	(0.020)				
Number of Children in Senior H.School		0.076	0.069	0.165	0.150	0.135	0.116	0.167		0.029	0.049	0.109
		(0.023)	(0.017)	(0.024)	(0.024)	(0.024)	(0.022)	(0.015)		(0.014)	(0.018)	(0.012)
Highest Education Attaintment of HH Members is Elementary School	0.053	0.152			0.061	-0.045	0.105	0.133	0.064		0.063	0.077
	(0.023)	(0.040)			(0.025)	(0.019)	(0.028)	(0.054)	(0.026)		(0.032)	(0.031)
Highest Education Attaintment of HH Members is Junior School	0.077	0.058	0.113	0.122	0.153	0.164	0.070	0.221	0.120	0.081	0.050	0.170
	(0.025)	(0.032)	(0.083)	(0.034)	(0.028)	(0.036)	(0.030)	(0.032)	(0.035)	(0.016)	(0.024)	(0.051)
Highest Education Attaintment of HH Members is Senior +	0.110	0.135	0.211	0.317	0.267	0.281	0.133	0.310	0.170	0.129	0.109	0.231
	(0.033)	(0.044)	(0.082)	(0.044)	(0.042)	(0.043)	(0.035)	(0.054)	(0.041)	(0.020)	(0.032)	(0.039)
Dependency Ratio		-0.039	-0.034	-0.027		-0.075			-0.034	-0.022	-0.040	-0.074
		(0.018)	(0.015)	(0.016)		(0.017)			(0.014)	(0.009)	(0.011)	(0.018)
Distance to District		-0.004	-0.025		-0.003	-0.004	-0.007				-0.004	-0.004
		(0.001)	(0.010)		(0.001)	(0.001)	(0.001)				(0.000)	(0.002)
Existence of SD		-0.224	0.183					-1.438			0.093	
		(0.040)	(0.102)					(0.057)			(0.041)	
Existence of SLTP	-0.150		-0.051	-0.088	0.041				0.053			
	(0.019)		(0.028)	(0.020)	(0.017)				(0.016)			
Existence of Puskesmas/Pustu	-0.047	-0.116	0.100	0.032							0.049	0.038
	(0.020)	(0.024)	(0.031)	(0.017)							(0.020)	(0.019)
Existence of Polindes	-0.054	-0.114			-0.048					0.029		
	(0.017)	(0.028)			(0.016)					(0.015)		
Existence of Posyandu	-0.062	-0.081			-0.184				0.174		-0.205	
	(0.018)	(0.040)			(0.073)				(0.038)		(0.038)	
Availability of Doctor						-0.050	-0.080	0.092				0.085
						(0.021)	(0.018)	(0.023)				(0.025)
Availability of Bidan	0.082	0.089	-0.144		-0.065	0.072	0.093				-0.068	0.084
	(0.025)	(0.035)	(0.068)		(0.027)	(0.036)	(0.034)				(0.021)	(0.025)
Road type is Asphalt	0.101	0.132	-0.280	0.137	-0.042	0.053			-0.114	0.057		-0.247
	(0.015)	(0.023)	(0.057)	(0.018)	(0.018)	(0.017)			(0.018)	(0.015)		(0.066)
Existence of Semi permanent market place			0.276	0.049			0.065		-0.090		-0.099	0.048
			(0.098)	(0.021)			(0.018)		(0.018)		(0.034)	(0.021)
Existence of Credit Facility		0.055						-0.072	-0.040	-0.031	-0.185	
		(0.022)						(0.018)	(0.017)	(0.014)	(0.022)	
Constant	12.839	12.884	12.131	12.119	13.287	12.756	12.344	14.008	12.577	12.852	13.082	13.098
	(0.106)	(0.150)	(0.218)	(0.123)	(0.127)	(0.076)	(0.131)	(0.149)	(0.109)	(0.082)	(0.087)	(0.118)
Observations	1920	2239	1824	2112	2208	2208	2208	2496	2016	1824	1920	2208
R-squared	0.606	0.284	0.486	0.457	0.436	0.363	0.471	0.516	0.474	0.556	0.478	0.583

Notes: Each column reports the result from a separate regression for that district. The dependent variable is log per capita consumption. Following standard BPS procedure, for each district, a first regression was run with all variables listed. A second regression was then run retaining only those variables that were statistically significant at the 10% level in the first regression. The results above present the results of this second regression, which were used for the PMT calculation. All variables above are statistically significant at least at the 10% level.

Appendix Table 2: Results of Different Targeting Methods on Mis-targeting Rate - Time elapsed between survey and targeting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		By Income Status		By Detailed Income Status				Per-capita consumption of beneficiaries
Sample:	Full population	Non-poor	Poor	Rich	Middle income	Near Poor	Very Poor	
Community treatment	0.088	0.098	0.042	0.090	0.102	0.127	-0.072	68.008
	(0.072)	(0.074)	(0.129)	(0.086)	(0.111)	(0.170)	(0.178)	(78.501)
Hybrid treatment	0.018	0.074	-0.226*	0.023	0.117	-0.252	-0.227	5.139
	(0.072)	(0.071)	(0.125)	(0.081)	(0.108)	(0.166)	(0.176)	(90.750)
Time elapsed	0.000	-0.000	0.001	-0.001	-0.000	0.004	-0.002	0.759
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(1.552)
Time elapsed x	-0.001	-0.001	-0.001	-0.001	-0.001	-0.003	0.002	-1.358
Community Treatment	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(1.852)
Time elapsed x	0.000	-0.001	0.005	0.000	-0.001	0.005	0.005	-0.322
Hybrid Treatment	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(2.049)
Observations	5595	3617	1978	1791	1826	1052	926	1687
Mean in PMT treatment	0.30	0.18	0.52	0.13	0.23	0.55	0.48	366

Notes: All regressions include stratum fixed effects. Robust standard errors are shown in parentheses, adjusted for clustering at the village level. All coefficients are interpretable relative to the PMT treatment, which is the omitted category. The mean of the dependent variable in the PMT treatment is shown in the bottom row. All specifications include stratum fixed effects. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 3: Results of Different Targeting Methods on Mis-targeting Rate - Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		By Income Status		By Detailed Income Status				Per-capita
Sample:	Full population	Non-poor	Poor	Rich	Middle income	Near Poor	Very Poor	consumption of beneficiaries
Community treatment	0.069**	0.005	0.145**	-0.052	0.068	0.218***	0.042	-44.804
	(0.035)	(0.039)	(0.058)	(0.050)	(0.053)	(0.079)	(0.083)	(36.192)
Hybrid treatment	0.087**	0.017	0.130**	0.041	-0.009	0.200**	0.092	-22.408
	(0.037)	(0.042)	(0.060)	(0.054)	(0.054)	(0.078)	(0.087)	(40.155)
Urban village	-0.010	-0.098***	0.128***	-0.088**	-0.113**	0.231***	0.035	-20.668
	(0.030)	(0.035)	(0.046)	(0.043)	(0.047)	(0.063)	(0.062)	(36.623)
Inequality	-0.004	-0.026	0.057	-0.029	-0.015	0.043	0.091*	-3.963
	(0.026)	(0.027)	(0.040)	(0.033)	(0.039)	(0.055)	(0.053)	(33.960)
General	0.043*	-0.010	0.000	0.009	-0.046	0.036	-0.043	-20.957
Connectedness	(0.026)	(0.031)	(0.039)	(0.039)	(0.039)	(0.053)	(0.056)	(29.451)
Urban village x	-0.049	0.026	-0.110*	0.056	-0.029	-0.184**	-0.041	25.062
Community treatment	(0.036)	(0.039)	(0.060)	(0.045)	(0.058)	(0.078)	(0.083)	(41.936)
Urban village x	-0.051	0.014	-0.069	-0.001	0.020	-0.149*	-0.015	11.794
Hybrid treatment	(0.036)	(0.039)	(0.059)	(0.045)	(0.055)	(0.079)	(0.082)	(47.509)
Inequality x	-0.016	0.022	-0.134**	0.027	0.052	-0.128*	-0.117	38.764
Community treatment	(0.035)	(0.039)	(0.055)	(0.047)	(0.057)	(0.075)	(0.076)	(39.728)
Inequality x	-0.021	-0.002	-0.069	-0.036	0.061	0.006	-0.157**	13.022
Hybrid treatment	(0.034)	(0.037)	(0.054)	(0.045)	(0.054)	(0.073)	(0.077)	(41.603)
General Connectedness x	-0.018	0.022	-0.007	0.069	-0.025	-0.038	0.040	44.665
Community treatment	(0.035)	(0.041)	(0.058)	(0.050)	(0.056)	(0.078)	(0.082)	(39.438)
General Connectedness x	-0.050	0.018	-0.081	-0.007	0.034	-0.166**	-0.010	17.931
Hybrid treatment	(0.035)	(0.040)	(0.058)	(0.050)	(0.052)	(0.080)	(0.078)	(40.290)
Observations	5753	3725	2028	1843	1882	1074	954	1719
Mean in PMT treatment	0.30	0.18	0.52	0.13	0.23	0.55	0.48	366

Notes: All regressions include stratum fixed effects. Robust standard errors are shown in parentheses, adjusted for clustering at the village level. All coefficients are interpretable relative to the PMT treatment, which is the omitted category. The mean of the dependent variable in the PMT treatment is shown in the bottom row. All specifications include stratum fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Table 4: Are elite results driven by social connections?

	(1)	(2)	(3)	(4)	(5)	(6)
	Mis-target dummy		On beneficiary list dummy		Mis-target dummy	
Elite connectedness	-0.034	-0.034	-0.078***	-0.078***	0.083**	-0.074***
	(0.021)	(0.021)	(0.023)	(0.023)	(0.040)	(0.023)
Connectedness	0.041*	0.041*	0.067***	0.067***	-0.051	0.049**
	(0.023)	(0.023)	(0.022)	(0.022)	(0.041)	(0.024)
Elite connectedness	-0.010	-0.002	-0.064*	-0.075**	0.068	-0.035
× community treatment	(0.035)	(0.039)	(0.034)	(0.037)	(0.064)	(0.036)
Elite connectedness	0.003	-0.004	-0.022	-0.010	0.035	0.018
× hybrid treatment	(0.034)	(0.036)	(0.034)	(0.037)	(0.062)	(0.036)
Elite connectedness	-0.032	-0.050	0.040	0.062	-0.067	0.018
× elite treatment	(0.031)	(0.047)	(0.031)	(0.043)	(0.059)	(0.032)
Elite connectedness		0.030		-0.050		
× elite treatment × hybrid		(0.064)		(0.061)		
Connectedness	-0.002	-0.026	0.008	0.019	0.004	0.010
× community treatment	(0.038)	(0.043)	(0.036)	(0.041)	(0.066)	(0.041)
Connectedness ×	0.041	0.064	0.055	0.042	-0.041	0.043
hybrid treatment	(0.037)	(0.041)	(0.035)	(0.037)	(0.073)	(0.036)
Connectedness ×	-0.000	0.043	-0.004	-0.029	-0.044	-0.004
elite treatment	(0.035)	(0.051)	(0.032)	(0.047)	(0.067)	(0.035)
Connectedness × elite treatment ×		-0.090		0.050		
hybrid treatment		(0.071)		(0.065)		
Observations	5753	5753	5756	5756	2028	3725

Notes: All specifications include dummies for the community, hybrid, and elite treatment main effects, as well as stratum fixed effects; columns (2) and (4) also include a dummy for elite × hybrid. Robust standard errors in parentheses, adjusted for clustering at the village level. Dependent variable in columns (1) and (2) is the mis-target dummy for the full sample, as in column (1) of Table 4. Dependent variable in columns (3) and (4) is a dummy for being a beneficiary of the program. *** p<0.01, ** p<0.05, * p<0.1