Does Elite Capture Matter?
Local Elites and Targeted Welfare Programs in Indonesia

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Economists tend to be suspicious of local leaders in developing countries. Instead of thinking of the leadership skills that allowed "village heads" to obtain their positions, or the ways to make them more effective, we try to imagine all the ways that they might be scheming to cheat citizens (e.g. Bardhan and Mookherjee 2000; Acemoglu 2006; and Acemoglu, Reed, and Robinsion 2012).

As a consequence of this skepticism, large swaths of development policy have been designed to marginalize local leaders. One example is targeted social programs: local leaders and communities often have better information about who is poor than central governments (Alderman 2002; Galasso and Ravallion 2005; Alatas et al. 2012). However, to combat elite capture, central governments often prefer to forgo this information and instead allocate benefits based on less precise, but centrally administered, proxy-means test systems (Coady, Grosh, and Hoddinott 2004).

Even decentralized programs often are designed to marginalize local leaders. For example, in "community-driven development" programs intended to develop local infrastructure, program decision-making and implementation are often placed directly in the hands of citizens, bypassing existing local leadership structures. (Mansuri and Rao 2013). This can be costly since citizens' implementation ability may be more limited than that of local leaders (Khwaja 2008). Moreover, in the long-run, institutions may suffer as the incentive of local leaders to acquire skills and demonstrate performance is reduced (e.g., Myerson 2009; Shleifer 2012).

In this paper, we test for capture by local leaders in targeted transfer programs, and then estimate whether this capture is quantitatively

large enough to justify the attention it receives.

I. Empirical Design and Data

A. Background

Indonesia runs several national, targeted social assistance programs, including Raskin, a subsidized rice program, and Jamkesmas (now BPJS), free health insurance. They have also periodically implemented a temporary, unconditional transfer program, the Direct Cash Assistance Program (BLT); in 2008, for example, the BLT program distributed about US $10 a month to 19.2 million households for nine months.

Indonesia also introduced a conditional cash transfer program, Program Keluarga Harapan (PKH), where beneficiaries receive cash assistance ranging from US $67-US $250 per year for six years, conditional on family composition, school enrollment and attendance, and a number of health requirements.

Targeting to select beneficiaries for these programs was comprised of both a data-driven component and local input. To determine eligibility for both BLT and PKH, Central Statistics Bureau (BPS) enumerators met with local leaders to create a list of potential households, and then conducted a proxy-means test for these households. For Raskin, beneficiaries are supposed to be selected through community meetings (although often, in practice, the targeting is just done directly by local leaders without involving the entire community). For Jamkesmas, beneficiaries were meant to be determined from the BPS's data-driven targeting list or from a previous data-driven list drawn up by the National Family Planning Board (Sparrow, Suryahadi, and Widyanti 2010). In reality, individuals could also join based on local leaders' discretion (Arifianto et al. 2005).

B. Data

We collected data to test the relative efficacy of different targeting methods in the 2011 PKH expansion. From the 2,500 expansion villages, a sample of 400 was chosen. We drew villages from three provinces (Lampung, South Sumatra, and Central Java), stratified to be 30 percent urban. Within each village, we randomly selected one hamlet (neighborhood), with each having an elected or appointed administrative hamlet head.

In these 400 villages, we conducted a baseline survey from December 2010 to March 2011—before the government announced the PKH expansion or conducted targeting. In each hamlet, we conducted a

census in which we asked questions to determine which households met PKH's demographic requirements. We randomly sampled nine households from those who met this requirement, plus the hamlet head, for a total of 3,998 households.

To assess households' poverty status, the baseline survey included questions on household consumption and the full set of asset and demographic variables that enter the proxy means test (PMT).

To create measures of "elite," we also asked respondents to list the hamlet's leaders, as well as their relatives. We separate formal and informal (*tokoh masyarakat*) leaders since those who enter government service may differ from those who become leaders informally through community service (such as teachers and religious leaders). We classify a household as being a leader if it was mentioned by at least two different baseline respondents. We then classify a household as "elite" if it is a leader itself or if at least two respondents identify that it is related by blood or marriage to a leader.

Finally, we collected data on program access. For the existing programs (Raskin, Jamkesmas, and the BLT), we asked about current program receipt in the baseline. For PKH receipt, we matched the baseline to administrative data from the targeting experiment plus the endline survey that we conducted after the experiments were complete (January to March 2012).

C. Experimental Design

We experimentally varied elite control over the targeting for the PKH expansion. Specifically, for our sample of 400 villages, we randomly assigned half to have PKH targeting outcomes determined as usual: everyone who was surveyed in the last targeting survey in 2008 was considered as a potential interviewee, along with households suggested by the local village leaders. Central government officials then verified the poverty status of everyone on this list by conducting the PMT.

In the remaining villages (community-input villages), the ultimate beneficiaries were determined through a community meeting where a poverty-ranking exercise was conducted, with no additional verification by the central government.

To vary the level of elite control within the community-input meetings, we randomly varied who was invited to them: in half of the villages, we asked the hamlet head to invite 5-8 local leaders, both formal and informal ("elite"). In the other half, the full community was invited to the meetings so that they could potentially provide a check on elite capture.

On average, 15 percent of households in the hamlet attended the meetings in the elite sub-treatment, while 59 percent did so in the "full" sub-treatment.

III. How Large is Capture?

In Table 1, we first test for elite capture in targeted social programs by estimating:

Eq 1: $Beneficiary_{ivs} = \beta_0 + \beta_1 Elite_{ivs} + \beta_2 \ln(PerCapCons)_{ivs} + \alpha_s + \varepsilon_{iv}$

where $Beneficiary_{ivs}$ indicates whether the household receives a program and $Elite_{ivs}$ indicates whether it is or is related to a leader. We include the log of per capita consumption, so that β_1 provides an estimate of the effect of being elite, conditional on one's economic status. All regressions are estimated by OLS, include sub-district fixed effects (α_s), and are clustered by village.

We provide evidence for existing programs (BLT 2008, Jamkesmas and Raskin) for formal (Panel A) and informal elites (Panel B).[1] In Panels C and D, we consider whether the household received PKH as measured from our endline survey. In these two panels, we test for elite capture in the PMT (Column 1) and the community-input treatments (Column 2), and then report the differential effect of elite by whether the full community or just local leaders were randomly invited to the decision meeting (Column 3).

Starting with the existing government programs, in Panel A and B, we find that for the most part, conditional on per capita consumption, formal leaders and their relatives are more likely to be beneficiaries, while informal elites are less likely to. For example, formal leaders and their relatives are almost 5 percentage points more likely to receive BLT, the direct cash assistance program (Column 1); 8 percentage points more likely to receive Jamkesmas, the health insurance program for the poor (Column 2); and 3 percentage points more likely to receive the subsidized rice program, Raskin (Column 3). In contrast, informal elites and their relatives are about 6 to 7 percentage points less likely to receive the programs, conditional on consumption levels.

Turning to the PKH experiment, we find no evidence of elite capture in any of the treatments. If anything, formal leaders and their relatives may be less likely to receive benefits relative to their consumption level. One potential reason for the difference between these results and the other existing programs is that the threshold for being a beneficiary is much lower: only about 12 percent of households in our data received PKH, compared to 36 percent for BLT, 42

[1] Appendix Table 1 provides the results for all elites, regardless if formal or informal.

percent for Jamkesmas, and 75 percent for Raskin. While elites may be able to plausibly pretend to be in the middle of the income distribution, it may be much harder to plausibly pretend to be in the bottom decile.

III. Welfare Consequences

Is capture by formal elites economically significant? How large are its potential welfare losses relative to other types of targeting errors?

We first calculate a back-of-the-envelope measure of capture by considering how different the average consumption level of beneficiaries would be if the advantage that formal elites have is eliminated. We note that formal leaders and their relatives are on average only 9 percent richer than non-elites and are at most about 8 percentage points (the highest is in the case of health insurance) more likely to receive benefits at a given consumption level. This implies that consumption would be 0.15 (population share of elites) × (0.08 / 0.42) (relative increase in elite's probability of receipt) × 0.09 (elite relative to non-elite consumption) = 0.003 higher, or about three tenths of a percentage point higher with elite capture than without it.[2]

To calculate the impact of elite capture more formally, we consider the following welfare calculation. We assume a CRRA utility function with $\rho = 3$, and calculate utility for all sample households without the program. Next, we calculate expected utility from each program, both with and without elite capture. To do so, for each household, we first calculate their predicted probability of receiving benefits from a given program as estimated using equation (1), and their predicted probability of receiving benefits from the same econometric model setting the ELITE variable equal to 0 (that is, without an elite premium).[3] Since the sum of the probabilities will be slightly different with and without elite capture, we scale the probabilities for the non-elites in such way that the fraction of the population getting the benefit is in both scenarios is identical. We then assign each household the per-capita monthly benefits from a given program with probability equal to the predicted probability, and then calculate the total utility under the program with and without elite capture using

[2] To see this, define c_b to be the consumption of the beneficiaries with no differential elite capture, and c_e to be the consumption of elites, β to be the average probability that people in the population receive benefits and $\Delta\beta$ to be additional probability that elites receive benefits, and α_e the population share of elites. With elite capture the average consumption of beneficiaries is $\frac{\beta c_b + \alpha\Delta\beta c_e}{\beta + \alpha\Delta\beta}$, and without elite capture the average consumption of beneficiaries is just c_b. The percent difference in average consumption is just $\frac{\frac{\beta c_b + \alpha\Delta\beta c_e}{\beta + \alpha\Delta\beta} - c_b}{c_b} = \frac{\alpha\Delta\beta\frac{(c_e - c_b)}{c_b}}{\beta + \alpha\Delta\beta} < \alpha\frac{\Delta\beta}{\beta}\frac{(c_e - c_b)}{c_b}$, which is the percentage calculated in the text.

[3] Since we will be predicting probabilities, for this exercise we use probit specifications and also include additional controls. The results (presented in Appendix Table 2) are similar to the OLS estimates.

the two different predicted probabilities (one with ELITE = 1 for elites, and one under the hypothetical counterfactual that there was no differential benefit for elites, i.e., setting the ELITE variable to 0).

We also compute two benchmarks. First, we calculate what the utility would be if we chose the poorest households to become beneficiaries based on their baseline consumption levels. This is effectively the utility from the program under "perfect" targeting. However, it is near impossible to achieve perfect targeting in the real world, since it is impossible to conduct a reliable consumption survey in a high-stakes environment when it is being used for targeting. Thus, as a second benchmark, we calculate utility if the government conducted a high-quality proxy-means test for the entire population. To do so, we calculate each household's predicted proxy-means test score based on the PMT formula used in PKH, using the asset variables that we observe in our survey, which was conducted by a highly reputable survey company. Then, we assign the benefits to those households with the lowest PMT score. This amounts to just assuming our high-quality survey firm did the targeting, rather than the central government's enumerators.

Table 2 presents these calculations for formal elites (we provide results for all elites in Appendix Table 3). The first column shows the simulations for PKH, and each subsequent column shows a different simulation for a different program. For each program, the first 5 rows show (1) the utility without the program, (2) the utility with the program given the actual elite effects we estimate in the data, (3) with the program but without any differential effect for elites, (4) under more complete proxy-means test targeting where the benefits go to those with lowest proxy-means test scores, and (5) under perfect consumption targeting (giving benefits to those with lowest consumption). The final three rows show the share of the possible utility gain each scenario obtains, where 0 is the utility without the program and 100% is the maximum utility with the program under perfect consumption targeting.

The loss due to elite capture can be seen by comparing the share of the possible utility gain with the elite-related premium on to the share of possible utility gain with elite effect turned off. The difference is small. For the elite sub-treatment in the PKH experiment, the difference is -0.12 percentage points (29.60% with elite-related premium on vs. 29.48% with elite-related premium off); for Jamkesmas, the difference is 1.18 percentage points (61.75%

vs. 62.93%); for Raskin, 0.44 percentage points (88.27% vs. 88.71%); for BLT 08, 0.78 percentage points (60.20% vs. 60.98%). Averaging across all 4 programs, eliminating formal elite capture entirely would improve welfare by about 0.55 percentage points, or about half of one percent.

For perspective, it is worth considering how large the potential scope for improvement is by simply improving overall targeting. The first benchmark assumes that the proxy-means test algorithm was implemented perfectly–that is, *all the poorest households* based on their proxy-means test scores using data from our survey received benefits. Our data suggest that welfare from Jamkesmas would improve by 17 percentage points (27 percent); welfare from Raskin by 7 percentage points (8 percent); welfare from PKH by 15 percentage points (57 percent) and welfare from BLT08 by 17 percentage points (28 percent).

Note that this better-implemented proxy-means test is different from the actual procedure along two dimensions. First, it treats all households as eligible for the programs, rather than just those who are chosen to be interviewed by the village government. Second, it uses data from a reputed survey company rather than from government enumerators. For PKH, we can also explore the welfare gains if only those the government suggested were interviewed, but we used the asset data that comes from our (plausibly higher-quality) baseline survey rather than the government survey to calculate the PMT scores. These results, which are presented in Appendix Table 4, show that most of the welfare gains that can be achieved with the perfectly implemented PMT are due to imperfect data quality, and not due to the inclusion of the households excluded by the village government: in the PMT treatment, the welfare gain from the perfectly implemented, full census PMT is 41.71 percent while the welfare gain of just the perfectly implemented PMT over the selected sample that is interviewed in the national survey is 36.99 percent.

In sum, eliminating elite capture will not improve welfare as much as improving administrative capabilities to administer the PMT. In particular, the results suggest that the possible welfare gains from improved targeting implementation are more than *20 times* larger than the potential welfare gains from the complete elimination of elite capture in these programs, and therefore suggest that investing in higher quality data collection mechanisms is important.

IV. Conclusions

Formal elites in Indonesia appear to use their influence to capture some of the benefits from targeted welfare programs. But, this type of elite capture is not economically large, and in fact is small relative to the targeting error resulting from limited administrative capabilities. This implies that starting from the current level of program oversight, further focusing on limiting elite capture may be less important than improving administrative practices.

Even more importantly, in designing effective public delivery mechanisms, there is often a reluctance to engage local leaders for fear of capture. However, we should take seriously the possibility that improving the administrative and management skills of local leaders may contribute more to welfare than cutting them out of the whole process to avoid capture, even if this means that the elites sometimes pocket some of the resources. At least in the context we study, the consequences of elite capture are simply not bad enough to give up everything else to prevent it. Given the importance of national leaders (Jones and Olken 2005), and the fact that national leaders often start as local leaders (e.g., Myerson 2009), the small welfare costs of elite capture documented here suggest worrying less about capture by local elites.

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TABLE 1— ELITE CAPTURE BY FORMAL VERSUS INFORMAL ELITES

	(1)	(2)	(3)
Panel A. Government Transfers– Formal Elites			
	Receives Benefits		
	BLT 08	Jamkesmas	Raskin
Elite	0.047***	0.082***	0.032**
	(0.018)	(0.018)	(0.014)
Observations	3,985	3,996	3,996
Dep. Var. Mean	0.387	0.425	0.751
Panel B. Government Transfers – Informal Elites			
	Receives Benefits		
	BLT 08	Jamkesmas	Raskin
Elite	-0.066***	-0.064***	-0.061***
	(0.021)	(0.023)	(0.017)
Observations	3,985	3,996	3,996
Dep. Var. Mean	0.387	0.425	0.751
Panel C. PKH Experiment – Formal Elites			
	Receives PKH		
	PMT	Community	Community
Elite	-0.034**	-0.042***	-0.021
	(0.015)	(0.015)	(0.023)
Elite x Elite Sub			-0.042
			(0.031)
Observations	1,863	1,936	1,936
Dep. Var. Mean	0.110	0.142	0.142
Panel D. PKH Experiment – Informal Elites			
	Receives PKH		
	PMT	Community	Community
Elite	-0.033*	-0.020	-0.018
	(0.017)	(0.018)	(0.026)
Elite x Elite Subt			-0.004
			(0.038)
Observations	1,863	1,936	1,936
Dep. Var. Mean	0.110	0.142	0.142

Notes: Each column shows an OLS regression of benefit receipt or benefit targeting on elite status and log per capita consumption. Stratum fixed effects are included in all regressions. Standard errors clustered at the village level are listed in parentheses. An F-test on the difference between the elite-related coefficient in Panel C, Columns (1) and (2), yields: F(1, 393) = 0.15 Prob > F = .7023. The same test in Panel D yields: F(1, 393) = 0.29 Prob > F = .5931.

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

TABLE 2— SIMULATED SOCIAL WELFARE UNDER DIFFERENT LEVELS OF CAPTURE (FORMAL ELITES)

	(1) PKH Experiment	(2) BLT 08	(3) Jamkesmas	(4) Raskin
Utility…				
Without program	-6.689	-6.689	-6.689	-6.689
With Elite on	-6.600	-6.268	-6.664	-6.471
With Elite off	-6.601	-6.263	-6.663	-6.470
Under perfect PMT	-6.550	-6.149	-6.657	-6.455
Consumption targeting	-6.354	-5.991	-6.648	-6.442
Share of possible utility				
With Elite on	26.41%	60.21%	61.75%	88.23%
With Elite off	26.18%	61.01%	62.92%	88.68%
Under perfect PMT	41.37%	77.26%	78.38%	94.86%

Notes: Utility is calculated as a monotonically increasing function of log per capita consumption (note that, under this formula, all utility is defined to be negative). Simulations are created with a probit model of benefit receipt, using our baseline calculations of consumption and PMT score, and a list of covariates. The probit model is shown in Appendix Table 2.