Limiting Elite Capture in Community Driven Development:

Evidence from a Randomized Controlled Trial in Sierra Leone

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ABSTRACT

Using data from a randomized controlled trial in rural Sierra Leone, I examine how appropriate design can limit elite capture in participatory development projects. I show that villages where local elites were prevented from officially managing the projects exhibit lower levels of elite capture as measured by the amount of cash payments budgeted for labor but generally understood to serve as a tithe for the village chief. Excluding elites from project management does not, however, have any effect on the eventual success of the project. Consistently with established theory, I also provide some evidence that villages with high levels of socioeconomic heterogeneity and large populations demonstrate relatively high levels of elite capture.

Keywords: elite capture, participatory development, CDD, randomized controlled trial, Africa, Sierra Leone

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1. INTRODUCTION

Participatory development approaches have been in use – with varying popularity – since the 1950s (Mansuri & Rao, 2004). They were initially influenced by non-economic thinkers such as Mohandas Gandhi (1962) and Paulo Freire (2005), who advocated collective action and self-governance, and later by Robert Chambers (1983), Arturo Escobar (1995) or James Scott (1998), who criticized the top-down policies that dominated mainstream development thinking in the 1980s. It was not until late in the twentieth century that participatory development attracted significant attention from mainstream development economists. Hirshman (1984) demonstrated how collective action on a local level can improve well-being, and Cernea (1985) showed that this can be facilitated even by large intergovernmental organizations like the World Bank, while Ostrom (1990) described the critical role of endogenous institutions in such efforts. Participatory approaches, which were still mostly the domain of small NGOs at the beginning of the 1990s, were taken up by increasingly larger development organizations during that decade. Finally, at the turn of the millennium, they became endorsed even by the World Bank in the form of its own community-driven development or CDD (Narayan & Ebbe, 1997; Narayan-Parker, 2002; World Bank, 2000, 2003). The approach became so popular with the World Bank that between 2001 and 2008, its CDD funding averaged almost USD 2 billion annually (Binswanger-Mkhize et al., 2010, p. 52), and reached USD 7.8 billion in 2010 alone (Mansuri & Rao, 2012).

Broadly defined as a practice of "giving control of development decisions and resources to community groups" (Narayan-Parker, 2002, p. 155), CDD is meant to "make poverty reduction efforts more responsive to demand" and thus help achieve "immediate and lasting results" while targeting "the poorest and most marginal individuals and groups" (Dongier et al., 2002, pp. 303, 307). In other words, the CDD approach assumes that if the recipient communities are given enough control powers and responsibilities in the design and

implementation of development projects, they can manage them more effectively than outsiders.

However, recent research suggests that CDD is hardly a panacea for the many shortcomings of current development practice. Besides concerns about its cost and slow speed of implementation, a common point of critique is the vulnerability of CDD to elite capture – a phenomenon where resources transferred for the benefit of many are usurped by a few, usually politically and/or economically powerful groups (such as traditional leaders, local government officials, landowners or educated people), at the expense of the less influential ones (Dutta, 2009).

The idea that CDD projects are prone to elite capture may seem somewhat counterintuitive. By allowing the intended beneficiaries to play an active role in the management of the projects, the very design of participatory development is supposed to limit the corruption often associated with top-down approaches to development. This 'democratization' of the development process is essentially meant to introduce a control mechanism over potential rent seekers (Fritzen, 2007). There is, nevertheless, some evidence indicating that elite capture in participatory development might be a rather widespread issue.

For example, Platteau and Abraham (2002), Platteau and Gaspart (2003) or Richards (2003) provide anecdotal evidence of elite capture in CDD projects from several countries in sub-Saharan Africa, while Nabi et al. (1999) give examples of elite capture in a more general decentralization setting in Bangladesh. Conversations with development professionals are also source of countless stories about local elites diverting resources not meant for them. On the other hand, only a few quantitative studies deal with the issue, and they generally show mixed results. While some, most notably Olken (2005) and Gallaso and Ravallion (2005) but also D'Exelle (2009), offer evidence of elite capture in CDD and decentralization projects,

others (Dasgupta & Beard, 2007; Duflo, 2005; Fritzen, 2007) do not find any such evidence at all. In an extensive randomized controlled trial study in the Democratic Republic of the Congo, Humphreys, Sanchez de la Sierra and Windt (2012) find some evidence of elite capture in CDD projects, but on a level statistically indifferent from more top-down project designs. The prevalence and the extent of elite capture in CDD projects are thus still topics of academic discussion rather than accepted facts.

Platteau and Abraham (2002) maintain that, especially in the context of lineage-based African societies of which Sierra Leone is a prime example, elite capture of participatory development projects is very likely. In such societies, traditional decision-making processes are typically based on consensus facilitated by the elite. In small and isolated communities where everybody knows each other relatively well and is aware of each other's actions, this ensures that outcomes of such processes are accepted and enforced by virtually the entire community. In a game-theoretical framework, this is a case of an infinitely repeated Prisoner's Dilemma with complete information, which, in the long run, results in full cooperation (Abreu, 1988). An individual who disobeys consensual decisions builds a negative reputation and is likely to be ostracized and sanctioned by the community (Hayami & Gōdo, 2005, p. 322). Platteau and Abraham (2002) argue that once communities increase in size, get more integrated with the outside world and once people start moving in and out of them, the conditions for successfully achieving and enforcing consensual decisions quickly disappear. In many, especially Western societies, traditional decision-making and enforcement mechanisms have evolved into, and been replaced by, representative political and judicial institutions. When these are absent or dysfunctional, the failure of traditional processes to provide effective social regulation in modern integrated market environments allows local elites to capture the benefits of development programs designed with such environments in mind.

Thus arises the question of how to bridge the gap between traditional decision-making and enforcement mechanisms and modern institutions. There is some evidence that enabling the target communities to monitor their representatives or exert enough control over the projects as such can, to varying degrees, limit elite capture (Björkman & Svensson, 2009; D'Exelle, 2009; Olken, 2005; Reinikka & Svensson, 2004). However, how exactly this should be done is a matter of an ongoing debate. Many organizations implementing CDD projects simply integrate basic democratic elements into their project designs by, for example, requiring the target communities to elect one or more representatives to manage the projects, or by having the communities decide on the type of the project through referenda. While there is some evidence that such an approach can limit elite capture (Beath, Christia, & Enikolopov, 2013), several studies voice the concern that in the absence of developed checks and balances, majority votes, referenda and various deliberative fora are prone to domination by elites anyway (Archibald & Richards, 2002; Besley, Pande, & Rao, 2005; Humphreys, Masters, & Sandbu, 2011; J.-P. Platteau & Abraham, 2002). Such elite domination of democratic processes does not necessarily rid them completely of their potential to limit elite capture, but certainly impedes this potential in a severe way (Fritzen, 2007). This is why D'Exelle (2009) suggests that donors get more involved in the selection of community representatives even if that means a compromise on democratic principles. Ideally, this would be done based on good knowledge of each of the target communities. In the absence of it, a random selection of village representatives should help prevent elite domination of project management, which is an approach this study takes. Such an approach may quite logically raise the concern that it would put in charge people with little experience and management capability. However, Duflo (2005) shows that inexperienced people are no less efficient in project management than seasoned leaders, and D'Exelle and Riedl (2008) suggest that they may perform even better than experienced elites.

Besides the design of the projects, the ability of elites to capture project resources and benefits is also likely to depend on endogenous characteristics of the target communities. Previous studies have shown that high levels of wealth and income inequality are often associated with higher levels of elite capture (Galasso & Ravallion, 2005; Khwaja, 2009), which may be due to the often high correlation of wealth and power (Araujo, Ferreira, Lanjouw, & Özler, 2008). It is also in line with the seminal work of Alesina and La Ferrara (2000), who show that high degrees of heterogeneity (both economic and ethnic) within communities lead to lower levels of participation in communal action. By extension, highly heterogeneous communities should be less able to monitor their elites and thus limit their capture of CDD projects. The ability to act collectively can be viewed as one manifestation of the elusive notion of social capital – a concept referred to in volumes of works, but notoriously difficult to define. In this paper, no attempt to propose a suitable definition is made. However, various factors commonly associated with social capital such as trust or respect are made use of in order to control for the potential effects of social capital on elite capture.

Utilizing data from a randomized controlled trial (RCT) in rural Sierra Leone, this work aims at contributing to the discussion by addressing the following questions:

- Is there elite capture of resources in CDD projects?
- What factors influence the extent of elite capture?
- Can appropriate project design limit elite capture?

The rest of the paper is organized as follows: Parts 2 and 3 describe the set-up of the RCT and the data. In part 4, the data are analyzed and a simple econometric model is developed to gauge the effect of project design on elite capture. Part 5 summarizes the results and concludes with some policy implications.

2. SET-UP

This paper draws on data from a randomized controlled trial conducted in 56 rural villages surrounding the Gola Rainforest National Park in south-eastern Sierra Leone. Each village was entitled to receive 2.4 million SLL (about 575 USD at the time) worth of livelihood support. In villages larger than 30 households, the entire sum was used for a community development project, typically involving a construction or a reconstruction of a communal structure such as a latrine, a guest house, a mosque or a court barry.[1] In smaller villages, each household could choose to allocate up to 80,000 SLL to its own consumption, with the rest of the total sum going towards the community project. In this paper, only the community projects are considered.

The support for the community projects came mainly in the form of construction materials and tools delivered to the villages by the Gola Forest Programme (GFP) – an organization responsible for the management of the National Park.[2] A part of the fund often came in the form of cash officially meant to pay for labor. However, anecdotal evidence from the field as well as general consensus among our enumerators suggest that this money is usually considered a tax levied by the village chief. Though it is conceivable that such fees be considered legitimate to some extent by the chief's subjects – an issue discussed in more detail in section 4 – there is little evidence supporting this. It is therefore safe to assume that the amount of cash payment can be used as a good proxy for elite capture by the chief.

The project to be implemented was decided upon during a village meeting in June 2010. Following the decision, the responsibility for implementing the project was assigned either to the village chief along with the youth and women's leaders (control group), or to a three-

[1] A court barry is an official meeting place in a village. It is typically a roofed structure without outer walls where most communal meetings take place.
[2] The Gola Forest Programme is an international partnership between the Conservation Society of Sierra Leone, the Forestry Department of the Government of Sierra Leone and the Royal Society for the Protection of Birds.

member committee of household heads selected through a lottery (treatment group). Villages were stratified by size and randomly assigned treatment status. Such design allows for testing the hypothesis that bypassing the local elites, i.e. the chiefs during project implementation can limit elite capture.

After a village was assigned to either the control or the treatment group, it was publicly announced that the organizer(s) – i.e. the chief in the control group or the committee members in the treatment group – are responsible for the management of the project, which would include signing for all the delivered materials, organizing the implementation of the project (including asking fellow villagers for help in carrying out the project), and organizing the upkeep and maintenance of the project. The manager (i.e. the chief or the committee) also informed GFP about the village's decision on what materials should be delivered given the available budget. The materials were delivered between September 2010 and February 2011.

3. DATA

The intervention was preceded by a baseline household survey conducted in May and June 2010, and followed by an endline household survey conducted in October and November 2012. In both the baseline and the endline, heads of households within each village were interviewed to collect standard socio-economic indicators, including household demographics, wealth, consumption and social capital. The baseline household survey was supplemented by a village survey to describe village-level characteristics such as remoteness or ethnic and religious heterogeneity. Data about the community projects, including the amount of cash transfers, was compiled from the Memoranda of Understanding (MoU) between the project managers and the GFP. The progress of the projects was monitored through a midline survey in May and June 2011. At this time, additional socio-economic data was collected from a random sample of 16 household heads per village. Finally, the quality and value of projects were assessed by a trained civil engineer at the time of the endline survey.

Unfortunately, as will become evident below, there are some gaps in the data due to various complications in the field. Not all data sources are available for all the villages, and they do not always overlap perfectly. As much as this results in a relatively low estimation power and thus limits the possibilities of econometric analysis, much care was taken to rectify this shortcoming by the use of multiple tests and models.

(a) Elite capture and other outcome variables

As described above, the amount of cash requested for labor in the MoU is used as a proxy for elite capture.[3] Tables 1 and 2 show that while no cash was ordered in four fifths of the

[3] Although this study is limited to only one of the many possible measures of elite capture, data on other outcome indicators was also collected, and will be the focus of a forthcoming larger study.

villages, in the remaining ones, the amounts of cash were quite significant, both in relative and absolute terms.

Table 1
Distribution of cash payment (absolute)

cash	Freq.	Percent	Cum.
0	47	83.93	83.64
100000	3	5.36	89.29
276000	1	1.79	91.07
290000	1	1.79	92.86
322000	1	1.79	94.64
428000	1	1.79	96.43
490000	1	1.79	98.21
504500	1	1.79	100.00
Total	56	100.00	

Table 2
Distribution of cash payment (relative to project size)

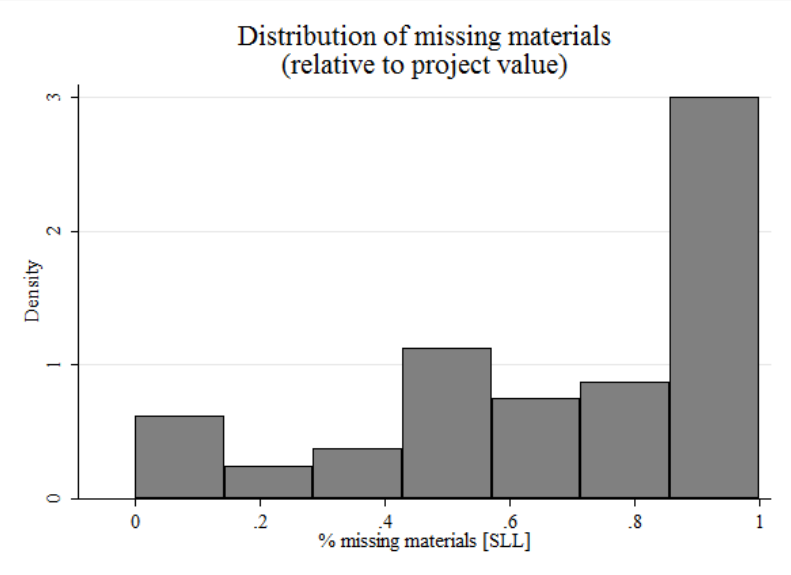
cash_pc	Freq.	Percent	Cum.
0.000	47	83.93	83.93
0.042	1	1.79	85.71
0.057	1	1.79	87.50
0.088	1	1.79	89.29
0.115	1	1.79	91.07
0.121	1	1.79	92.86
0.178	1	1.79	94.64
0.199	1	1.79	96.43
0.204	1	1.79	98.21
0.210	1	1.79	100.00
Total	56	100.00	

Moreover, the engineering assessment of the project conducted at the time of the endline was used to estimate the value of the materials used in the construction of the projects. Adjusted for inflation and enumerator bias, these values were used to approximate the value of materials missing from the projects, and are described in Table 3 in their absolute value *mat* and as fractions of the project value as specified in the MoU *mat_pc*.

Table 3
Distribution of missing materials

	Obs	Mean	Std. Dev.	Min	Max
mat	56	1500952	740212.1	0	2400000
mat_pc	56	0.709477	0.30782	0	1

Figure 1

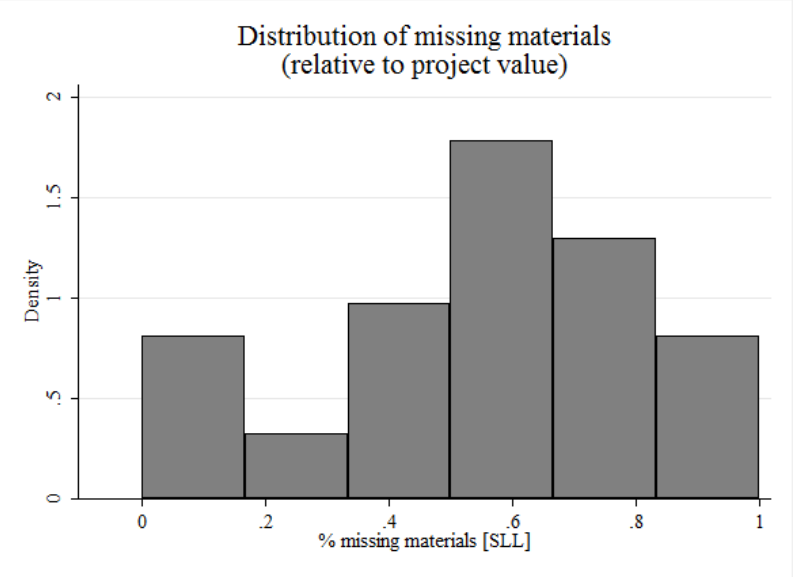


As Table 3 suggests, and as is also evident from Figure 1, the distribution of the *mat_pc* (as well as that of *mat*) is heavily skewed to where of the materials missing from projects. This is due to the fact that in a third of the villages, the projects were not only incomplete upon inspection, but never even started (see Table 4). *mat* and *mat_pc* thus probably pick up not only the diversion of materials from their intended purposes, but also managerial failure of chiefs or committees. Assuming that a project that had not started is a result of poor management and excluding these cases from the sample, it becomes much safer to consider *mat_pc* a good proxy for diversion of materials rather than the incompetence of the managers. The distribution of *mat_pc* then looks as shown in Figure 2, but the sample of course further decreases in size – an issue I address below.

Table 4
Project completion

Project stage	Chief	Committee	Total
Fully completed	3	0	3
Mostly completed	13	8	21
Partially completed	7	6	13
Barely started	0	0	0
Not started	7	12	19
Total	30	26	56

Figure 2



(b) Endogenous village characteristics

Several indicators were measured on the village level in order to control for endogenous village characteristics.[4] Table 5 explains the construction of these variables while Table 6 provides some basic descriptive statistics. The control and treatment groups are well balanced along these characteristics, as can be seen from Table 7, where the two groups are compared by means of a t-test.

As mentioned above, the data gaps in these control variables pose further limits on the already small sample. Fortunately, only a fraction of the positive values of the dependent variable are affected by the gaps, which is evident from Table 8.

[4] As is the case with outcome indicators, data on many other confounding factors was collected, but this study limits itself to the use of those relevant to the model developed below.

Table 5
Description of variables

Name	Label	Description
qual	Perceived quality of chief	An index predicted from first factor loadings from principal factor analysis of data from questions focusing on trust and respect by fellow villagers. The higher the number, the higher social capital.
soccap	Social capital in village	An index predicted from first factor loadings from principal factor analysis of data from questions similar to those used for the index of chief quality, but aimed at relations with neighbors.
pop	Population	Population of the village.
gini	Gini	Gini coefficient of upland farm holdings.
major	% Ethnic majority	Percentage of the largest ethnic group in the village.
muslim	% Muslim	Percentage of Muslims in the village.
heter	Heterogeneity	An index predicted from first factor loadings from a principal factor analysis of *gini*, *major* and *muslim*.[5] The higher the number, the higher is socioeconomic heterogeneity in the village.
pval	Project value	Total value of the project

Table 6
Descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
qual	39	0.843	0.079	0.687	0.967
soccap	39	0.884	0.171	0.353	1.000
pop	48	191	216	11	872
gini	38	0.353	0.141	0.076	0.732
major	37	87.9	13.8	46.3	100.0
muslim	39	97.0	05.2	80.0	100.0
heter	35	0.000	0.725	-0.772	1.675
pval	56	2,083,875	479,676	780,000	2,400,000

[5] Only the composite index *heter* is used in the main model specification. *Gini*, *major* and *muslim* are only used in robustness checks, but are also included in Tables 3, 4 and 5 only for descriptive purposes.

Table 7
Sample balance

	Chief		Committee			
Variable	Obs.	Mean	Obs.	Mean	Diff.	p
qual	18	0.841	21	0.844	0.002	0.923
soccap	18	0.902	21	0.869	-0.033	0.543
pop	25	160	23	222	62	0.335
gini	17	0.368	21	0.340	-0.028	0.562
major	17	90.4	20	85.8	-4.6	0.304
muslim	17	96.8	22	97.1	0.3	0.833
heter	16	-0.050	19	0.042	0.092	0.712
pval	30	2,115,033	26	2,047,923	-67,110	0.605

Table 8
Dependent variable positive values in sub-samples

	Chief		Committee	
Variable	Obs.	# Pos. values cash_pc	Obs.	# Pos. values cash_pc
qual	18	5	21	2
soccap	18	5	21	2
pop	25	7	23	2
gini	17	5	21	2
major	17	5	20	2
muslim	17	5	22	2
heter	16	5	19	2
pval	30	7	26	2

4. ANALYSIS

(a) Elite Capture

Considering the relatively small sample and the fact that there is not always a perfect overlap of data sources, which will limit the sample size even further in most econometric models, let us first consider a simple t-test of the mean amounts of cash ordered by the chiefs (control group) and the randomly selected committees (treatment group). The test (see Tables 9 and 10) reveals that projects managed by a chief requested on average 60,022 SLL more cash than those managed by a committee. While this amount – equivalent to about 14 USD – is not nominally very high, it should be viewed in the perspective of the fact that over 75% of the population of Sierra Leone live on less than 2 USD per day. 60,022 SLL thus amounts to significantly more than a weekly expense of an average Sierra Leonean. Since the cash payment is generally regarded as a tax levied by the chief, this result suggests that limiting chiefs' role in project management also limits the extent of elite capture by the chiefs.

Table 9
Difference in cash payment (absolute)

Group	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
Chief	30	74,483	28,751	157,475	15,681	133,286
Committee	26	14,462	11,145	56,829	-8,492	37,415
combined	56	46,616	16,613	124,322	13,323	79,910
diff		60,022	30,836		-2,435	122,478

diff = mean(Control) - mean(Treatment) t = 1.9465
Ho: diff = 0 degrees of freedom = 37
Ha: diff < 0 Ha: diff != 0 Ha: diff > 0
Pr(T < t) = 0.9704 Pr(|T| > |t|) = 0.0591 Pr(T > t) = 0.0296

Table 10
Difference in cash payment (relative to project value)

Group	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
Chief	30	0.035	0.013	0.071	0.009	0.062
Committee	26	0.006	0.005	0.024	-0.004	0.016
combined	56	0.022	0.007	0.056	0.007	0.037
diff		0.029	0.014		0.001	0.057

diff = mean(Control) - mean(Treatment) t = 2.1269
Ho: diff = 0 degrees of freedom = 36
Ha: diff < 0 Ha: diff != 0 Ha: diff > 0
Pr(T < t) = 0.9798 Pr(|T| > |t|) = 0.0403 Pr(T > t) = 0.0202

A similar test on the value of missing materials in commenced projects yields very similar results (see Table 11).

Table 11
Value of missing materials (relative to project value)

Group	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
Chief	23	0.628	0.056	0.271	0.511	0.745
Committee	14	0.448	0.070	0.262	0.297	0.599
combined	37	0.560	0.046	0.278	0.468	0.653
diff		0.180	0.091		-0.004	0.364

diff = mean(Control) - mean(Treatment) t = 1.9881
Ho: diff = 0 degrees of freedom = 35
Ha: diff < 0 Ha: diff != 0 Ha: diff > 0
Pr(T < t) = 0.9727 Pr(|T| > |t|) = 0.0547 Pr(T > t) = 0.0273

However, as outlined in the introduction, other factors are likely to influence the chiefs' ability to extract cash from the project. These include above all the heterogeneities within the village, the size of the village and social capital. In order to control for their effect, a more comprehensive model is required. It is practically impossible to estimate such a model based on only the observations left after cleaning the missing materials variable *mat_pc* of noise from managerial incompetence, especially given other gaps in the data, which would further reduce the sample. I therefore only use the elite capture proxy of ordered cash *cash_pc* for further analysis.

Since the variable of interest – cash payments to the project – takes on positive values only in a minority of the villages, I use a type I Tobit model censored at zero to assess how significantly these factors affect elite capture. It is conceivable that the confounding factors affect elite capture differently under each treatment, which would require a specification with interaction terms of each of the confounding variables with the treatment dummy:

$$cash\_pc_i = \begin{cases} c_i^* \; if \; c_i^* > 0 \\ 0 \; if \; c_i^* \leq 0 \end{cases}$$

(1) $c_i^* = \beta_0 + \beta_1 treat_i + \beta_2 qual_i + \beta_3 treat_i * qual_i + \beta_4 soccap_i + \beta_5 treat_i * soccap_i + \beta_6 pop_i + \beta_7 treat_i * pop_i + \beta_8 heter_i + \beta_9 treat_i * heter_i + \beta_{10} pval_i + \beta_{11} treat_i * pval_i + \varepsilon_i$

where *treat* is a treatment dummy (equaling one when a committee was in charge, zero when the chief was in charge), *qual* is an index of the perceived quality of the chief, *soccap* is an index of the social capital of the village and *pop* is village size in terms of population. *heter* is an index of socio-economic heterogeneity constructed from a Gini coefficient of upland farm holdings *gini*, and the percentage of the ethnic and religious majorities within the village, *major* and *muslim* respectively. Finally, *pval* is the total value of the project in SLL.

However, such an exhaustive specification cannot be practically estimated given the relatively small sample size and the consequent insufficient number of degrees of freedom. To overcome this issue, I opt for the second best option and estimate several reduced specifications with only one interaction term in each. While this approach somewhat reduces the ability of the models to fully reflect all relationships between the variables, it still allows for a meaningful estimation of the main effects of interest. The following specifications are thus used:

(2) $c_i^* = \beta_0 + \beta_1 treat_i + \beta_2 qual_i + \beta_3 treat_i * qual_i + \beta_4 soccap_i + \beta_5 pop_i + \beta_6 heter_i + \beta_7 pval_i + \varepsilon_i$

(3) $c_i^* = \beta_0 + \beta_1 treat_i + \beta_2 qual_i + \beta_3 soccap_i + \beta_4 treat_i * soccap_i + \beta_5 pop_i + \beta_6 heter_i + \beta_7 pval_i + \varepsilon_i$

(4) $c_i^* = \beta_0 + \beta_1 treat_i + \beta_2 qual_i + \beta_3 soccap_i + \beta_4 pop_i + \beta_5 treat_i * pop_i + \beta_6 heter_i + \beta_7 pval_i + \varepsilon_i$

(5) $c_i^* = \beta_0 + \beta_1 treat_i + \beta_2 qual_i + \beta_3 soccap_i + \beta_4 pop_i + \beta_5 heter_i + \beta_6 treat_i * heter_i + \beta_7 pval_i + \varepsilon_i$

I will first illustrate the methods of analysis using specification (2), and then present the results of the remaining specifications. The estimation results of specification (2) are summarized in Table 12.

Table 12
Model estimation
(2)

		Number of obs	=	35
		F(7, 28)	=	5.51
		Prob > chi2	=	0.0005
Log likelihood =	2.9930	Pseudo R2	=	1.3351
cash_pc	Coef.	Std. Err.	t	P>t
treat	0.299	0.478	0.62	0.537
qual	1.061**	0.472	2.25	0.033
treat*qual	-0.641	0.542	-1.18	0.247
soccap	-0.00384	0.227	-0.02	0.987
pop	5.39e-04***	1.37e-04	3.93	0.001
heter	0.00539	0.0342	0.14	0.890
pval	-3.63e-08	5.64e-08	-0.64	0.525
_cons	-0.933	0.487	-2.47	0.020

* p<0.10, ** p<0.05, *** p<0.01

At the first glance the results of the model estimation seem to suggest that the difference in the use of cash funds between the treatment and control groups is driven by factors other than the treatment itself, most likely the size of the village (*pop*) and the perceived quality of the

chief (*qual*). The coefficient of the treatment dummy is statistically insignificant, and has the wrong sign. However, some caution should be exercised when interpreting the estimated coefficients and their corresponding standard errors in models with interactive terms like this one. As Berry, Golder and Milton (2012) point out, the values of coefficients and standard errors of a constituent of an interactive term can only be estimated given the value of the other constituent. The standard one-dimensional estimation output cannot sufficiently illustrate such relationships. Instead, they propose a simple graphical representation of the marginal effects of each constituent as a function of the second one. In line with their suggestion, Figure 3 shows the marginal effects of treatment conditional on the level of the perceived quality of the chief.

Figure 3

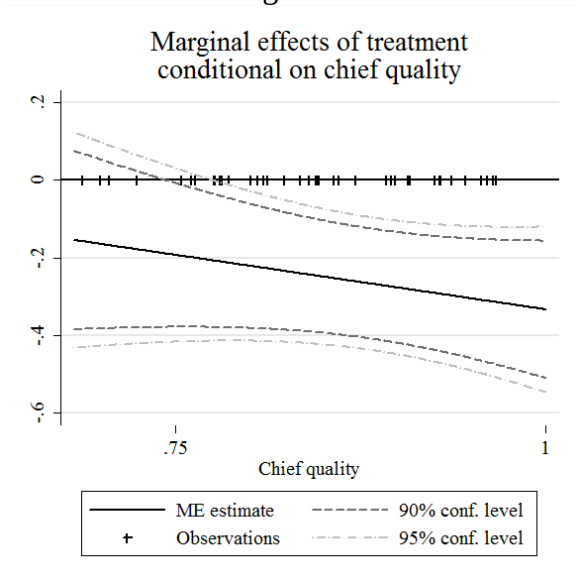


Figure 3 suggests that despite the seemingly the low t-value for the estimated coefficients on *treat* and its seemingly wrong sign, the treatment in fact has a significantly negative impact on ordered cash for a large portion of observed levels of chief quality – 87% of observations at the 5% significance level. In other words, when a randomly selected committee is put in charge of a project, the amount of diverted funds is in most cases significantly lower than when a chief is in charge.

To make the interpretation even easier, it is useful to reverse the treatment dummy so that it is equal to 1 when the chief is in charge of the project (i.e. using (1 – *treat*) instead of *treat* in the model). Such specification naturally does not change the results for the control variables, but yields the following ones for the constituents of the interactive terms (see Table 13 and Figure 4).

Table 13
Model Estimation
(2b)

		Number of obs	=	35
		F(7, 28)	=	5.51
		Prob > chi2	=	0.0005
Log likelihood =	2.9930	Pseudo R2	=	1.3351
cash_pc	Coef.	Std. Err.	t	P>t
(1-treat)	-0.299	0.478	0.62	0.537
qual	0.420	0.652	0.64	0.525
(1-treat)*qual	0.641	0.542	1.18	0.247
soccap	-0.00384	0.227	-0.02	0.987
pop	5.39e-04***	1.37e-04	3.93	0.001
heter	0.00478	0.0342	0.14	0.890
pval	-3.63e-08	5.64e-08	-0.64	0.525
_cons	-0.933	0.487	-2.47	0.020

* p<0.10, ** p<0.05, *** p<0.01

Figure 4[6]

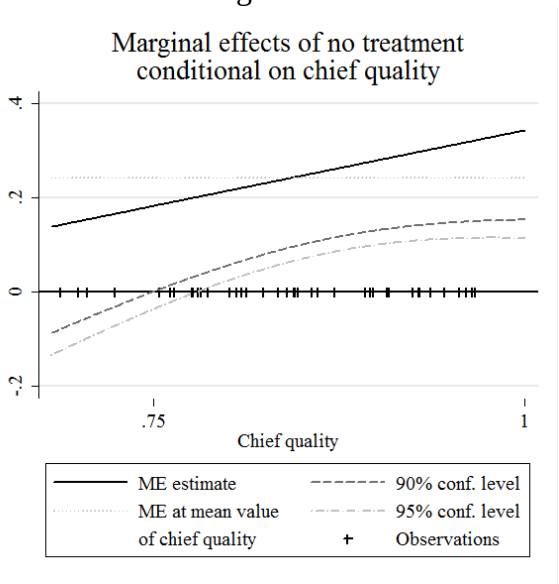


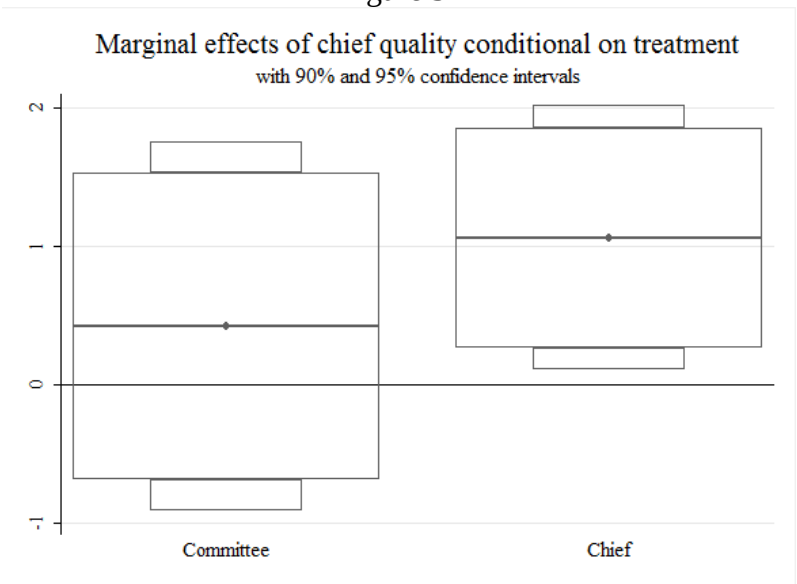
Figure 4 shows that direct and official involvement of the chief in the project is significantly associated with higher amounts of diverted cash than situations when a randomly appointed committee is in charge, and that this amount of cash increases along with the perceived quality of the chief. In other words, not only are chiefs able to extract money from the projects more easily when they are in charge, but the amount that they divert is higher when they are generally more respected. Although the number of usable observations, as compared to the full sample size, decreases drastically in this specification due to the incompleteness of the data, the estimation results are in line with the t-test results.

The interaction plot can also be reversed to show the marginal effects of chief quality given the type of treatment (see Figure 5). It then becomes evident that more cash is ordered in villages where chiefs are in charge of the project and where they are relatively well

[6] Because of the nature of the chief quality index, only the sign and the direction of the slope of the treatment effect, not its magnitude, can be interpreted. The upper bounds of the confidence intervals are therefore redundant to the analysis. For simplicity, they are omitted from this and most similar figures with continuous conditioning variables.

respected.[7] In villages with project committees, there seems to be no statistically significant impact of perceived chief quality on cash extraction.

Figure 5[8]



The estimations of the remaining specifications yield similar results for the treatment, as can be seen from Table 14 and Figures 6 – 9. While Figure 6 would seem to suggest at the first sight that for the marginal effect of treatment is statistically insignificant for a large part of the range of village social capital values, it is important to note that over two thirds of the observations lie in the 95% confidence range, and nearly three quarters lie within the 90% confidence range. This becomes more evident in Figure 7, from which the outliers are omitted and where the treatment marginal effect is plotted at the median instead of the mean value of village social capital.

The results for perceived chief quality also remain similar in all three specifications, though they are logically not as significant as in specification (2) where the *(1-treat)*qual* interaction term is included.

[7] N.B. The magnitude of the effect should not be interpreted in any way as chief quality is only a relative index.
[8] The marginal effects are represented by the center lines (with dots in the middle). The wide boxes represent the 90% confidence intervals, and the narrow boxes represent the 95% confidence intervals. The same representation is used in all similar figures with a binary conditioning variable.

Table 14
Model estimations
(dependent variable *cash_pc*)

	(6)	(7)	(8)
(1-treat)	1.342	0.412**	0.306***
	(1.442)	(0.159)	(0.0922)
qual	1.035**	1.000**	1.153**
	(0.447)	(0.458)	(0.469)
soccap	1.071	0.0166	-0.0684
	(1.389)	(0.238)	(0.248)
(1-treat)*soccap	-1.117		
	(1.436)		
pop	5.69e-04***	7.13e-04***	6.03e-04***
	(1.65e-04)	(1.98e-04)	(1.38e-04)
(1-treat)*pop		-2.78e-04	
		(2.27e-04)	
heter	0.00223	0.0262	0.0529**
	(0.0356)	(0.0374)	(0.0252)
(1-treat)*heter			-0.0695
			(0.0473)
pval	-3.50e-08	-2.35e-08	-4.48e-08
	(5.02e-08)	(5.86e-08)	(6.06e-08)
N	35	35	35
p	6.95e-04	0.00761	0.936e-04
r2_p	1.335	1.349	1.348

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 6

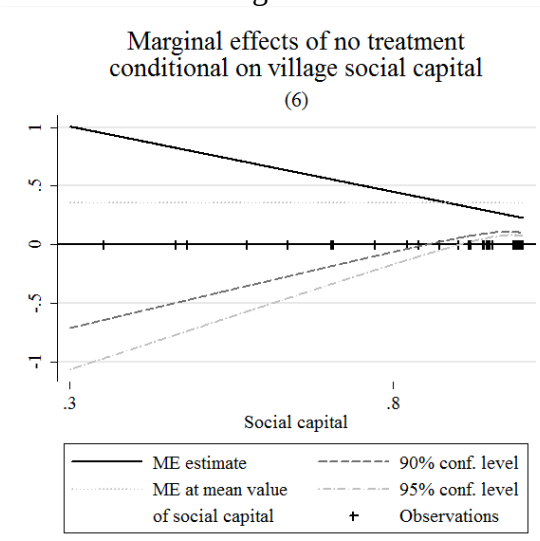


Figure 7

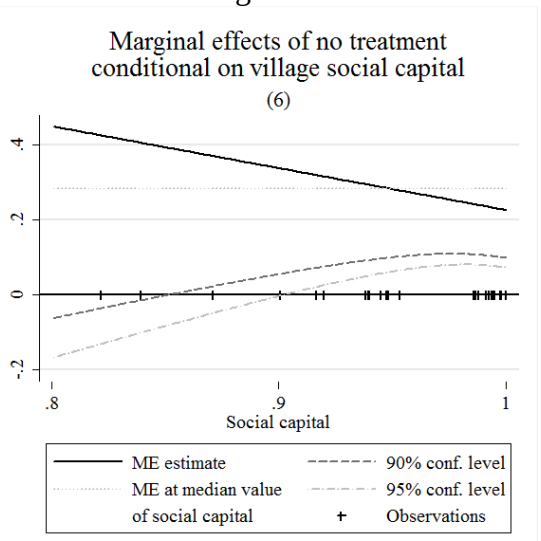


Figure 8

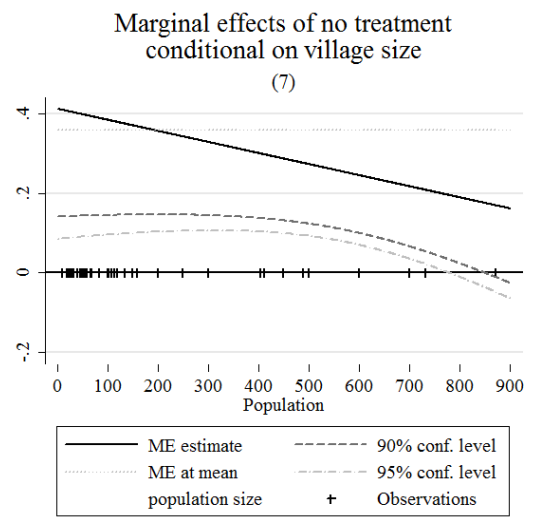
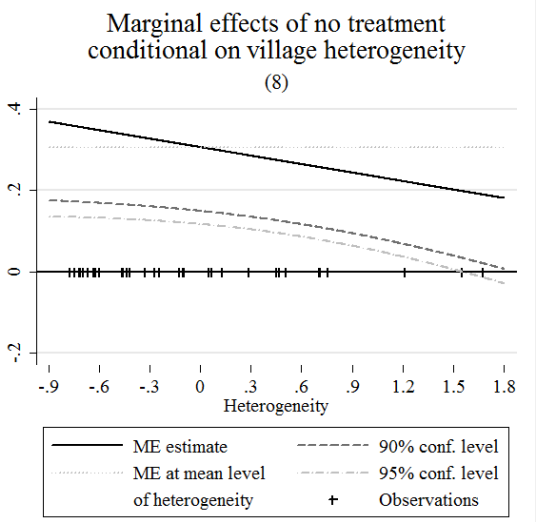


Figure 9



The obvious interpretation of these findings would be that well-respected chiefs take advantage of the respect they command to extract cash from CDD projects. However, one should be cautious before jumping to such conclusion. While the abuse of respect by the traditional elite seems like a realistic scenario,[9] a somewhat less pessimistic explanation is also conceivable:

Chiefs habitually exercise a customary right to impose various taxes and fines on their subjects or to require 'voluntary' labor from them in order to provide – or often under the pretext of providing – various public goods from road maintenance to law enforcement (Archibald & Richards, 2002; Jackson, 2005, 2007; Richards, Bah, & Vincent, 2004). From this perspective, chiefs and possibly even their subjects may view the diversion of funds from CDD projects as their legitimate right – a compensation for organizing the public works. Well-respected chiefs might thus be able to extract more cash from the projects as they are expected to manage their implementation relatively well. As such, elite capture could be considered perfectly legitimate if not desirable by the recipient communities, especially in those with well-respected chiefs who may 'deserve' remuneration.

[9] See for instance Archibald and Richards (2002) or Richards, Bah and Vincent (2004) for anecdotal evidence.

The line between theft and reward is unfortunately too fine in this setting to be established using the quantitative methods at hand. Whether or not it is justified, the matter of fact remains that chiefs tend to extract more cash from CDD projects when they are in charge. In light of the strong anecdotal evidence suggesting that such cash is normally kept by the chiefs, along with the fact that in villages without a project committee, more materials are missing from started projects than in villages with a committee, these results strongly point to a case of elite capture by chiefs when they are in charge of CDD projects.

(b) Other Findings

One could argue that elite capture is of little relevance considering that the chiefs seem to be more effective at managing the projects than committees. Only half of the committee-managed projects had started over a year after material delivery as opposed to three quarters of those managed by the traditional elites (see Table 4). However, this apparent difference disappears when the confounding factors – all of which should affect project completion similarly to elite capture – are taken into account. This can be seen from a logistic regression of *start*, a dummy equaling 1 when the project had started by the time of the inspection and 0 otherwise (see various estimation results in Table 15 and the corresponding interaction plots in Figures 10 – 13). There is thus little evidence that, ceteris paribus, traditional elites manage the projects better than randomly appointed committees – a result similar to previous findings (Duflo, 2005).

Table 15
Model estimations
(dependent variable *start*)

	(9)[10]	(10)	(11)	(12)
(1-treat)	-18.88**	4.176	1.819	0.422
	(8.886)	(3.714)	(1.120)	(0.848)
qual	0.885	8.055	9.570	8.326
	(6.518)	(6.281)	(6.342)	(6.277)
(1-treat)*qual	23.51**			
	(11.29)			
soccap	0.756	0.294	-0.463	-0.358
	(2.593)	(2.841)	(2.742)	(2.546)
(1-treat)*soccap		-4.187		
		(4.253)		
pop	0.00106	0.00151	0.00461	0.00136
	(0.00244)	(0.00245)	(0.00412)	(0.00249)
(1-treat)*pop			-0.00917*	
			(0.00479)	
heter	0.415	0.178	0.148	0.399
	(0.590)	(0.564)	(0.670)	(0.833)
(1-treat)*heter				-0.341
				(1.222)
pval	-1.38e-06*	-0.862e-06	-1.21e-06	-1.09e-06
	(0.793e-06)	(0.715e-06)	(0.739e-06)	(0.790e-06)
N	35	35	35	35
p	0.283	0.542	0.231	0.625
r2_p	0.167	0.0973	0.162	0.0864

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[10] Although the coefficients on *(1-treat)* and *(1-treat)*qual* are each statistically significant by themselves, they effectively cancel each other out, becoming in the end insignificant as Figure 10 Shows.

Figure 10

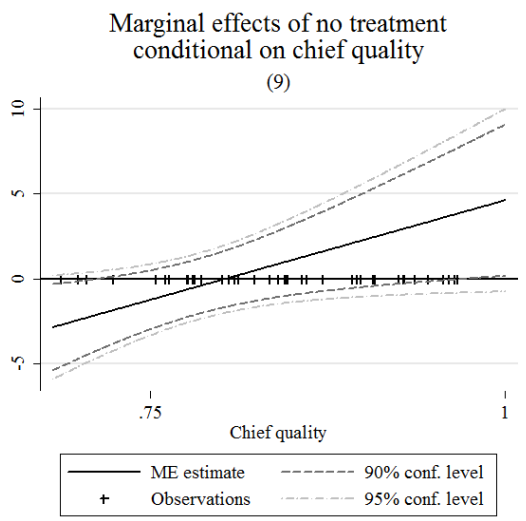


Figure 11

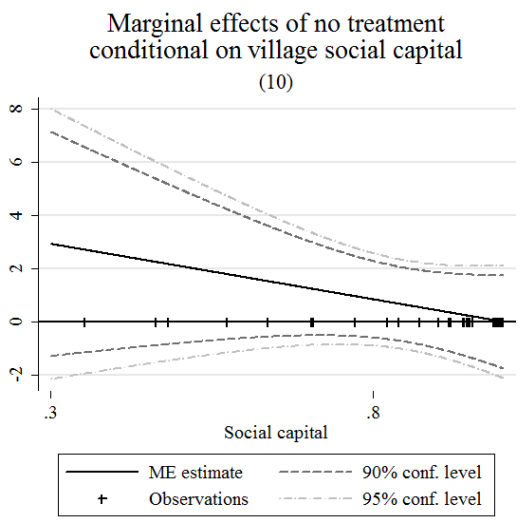


Figure 12

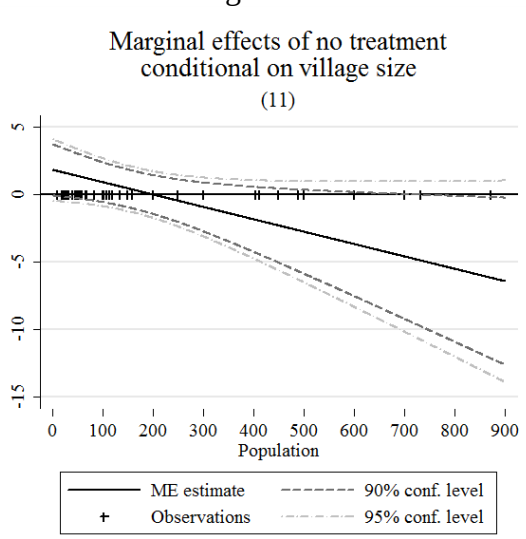
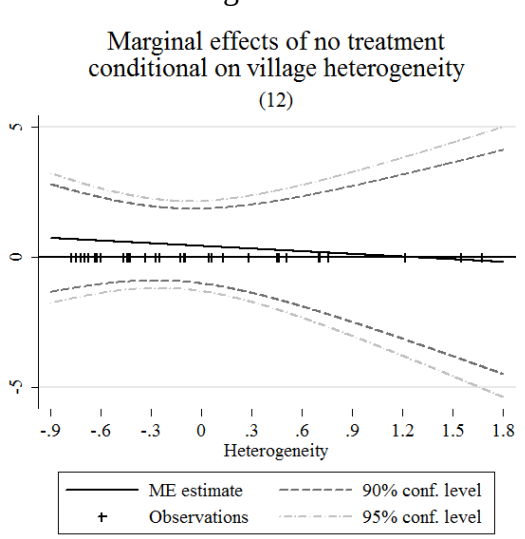


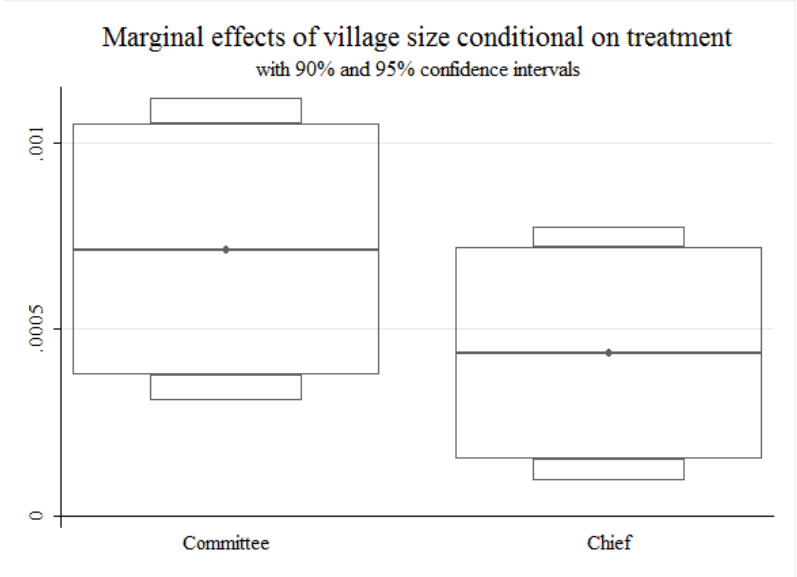
Figure 13



The results also indicate that the amounts of cash extracted from projects tend to grow with an increasing population size. This is also evident in the reversed version of the interaction plot from specification (4) (see Figure 14), which, moreover, shows that the effect is stronger in villages with project committees, suggesting that it may be harder for the committees to reign in elite capture in larger communities. Such a finding is in line with Platteau's and Abraham's (2002) proposition that traditional mechanisms of social control tend to break apart as communities grow in size. A threshold of 150 people, beyond which social control

becomes increasingly difficult, has been suggested by Gladwell (2000, p. 179), but I find no evidence of any such exact value.

Figure 14

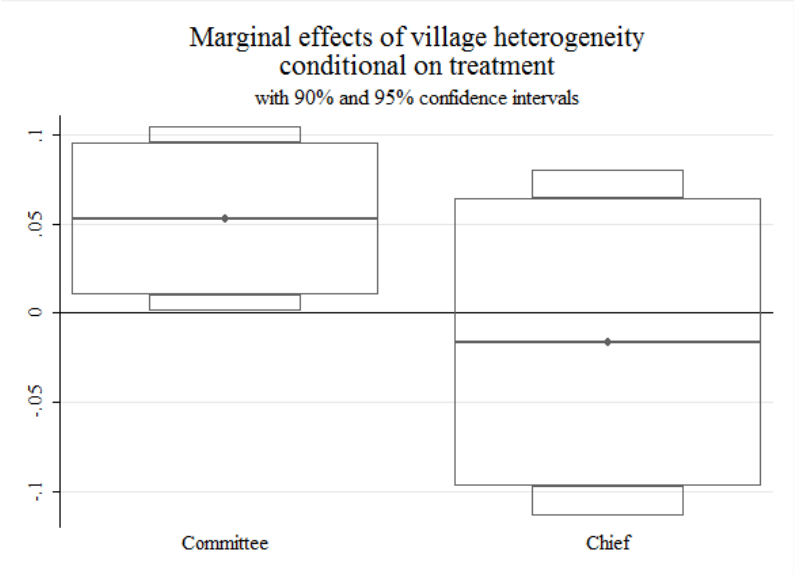


Contrary to theoretical predictions and to previous findings (Alesina & La Ferrara, 2000; Galasso & Ravallion, 2005; Khwaja, 2009), the estimations of the initial specifications (2), (3) and (4) do not indicate that elite capture should be influenced by socioeconomic heterogeneity within villages. However, when interacted with treatment, it becomes evident that socioeconomic heterogeneity does indeed have the expected effect, but only in villages with a project committee (see Figure 15).

The last two findings could be explained in two different ways. It is possible that the control mechanisms – described by Platteau and Abraham (2002) – erode with increased population size and socioeconomic heterogeneity more readily when applied to a new organizational situation (project committee) than under the status quo with traditional elites in charge. It is, however, also possible that the control mechanisms erode similarly in both kinds of villages, but are ineffective against capture by traditional elites in the CDD setting, and can only make a difference when the elites are removed. Considering that elite capture does generally tend to

take place more often in chief-managed projects than in those managed by a committee, the later explanation seems more likely.

Figure 15



As the mechanisms through which high socioeconomic heterogeneity and large population are likely to affect elite capture are similar, the two might well reinforce each other's effects. To verify this, I estimate one more model specification (13) with an interaction term between *pop* and *heter*.

(13) $$c_i^* = \beta_0 + \beta_1(1 - treat)_i + \beta_2 qual_i + \beta_3 soccap_i + \beta_4 pop_i + \beta_5 heter_i + \beta_6 pop_i * heter_i + \beta_7 pval_i + \varepsilon_i$$

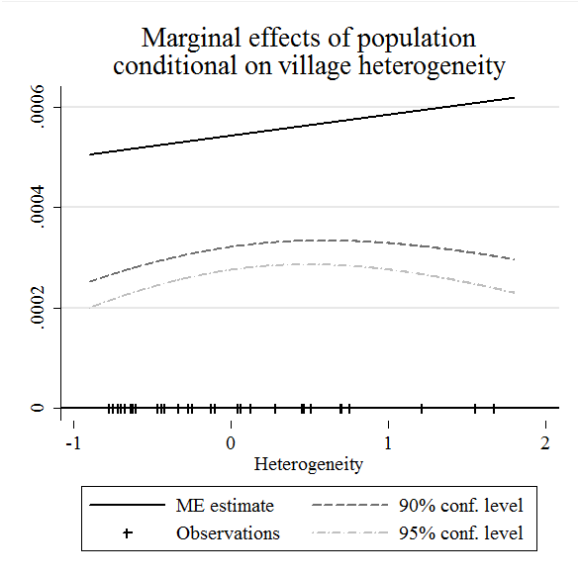
As expected, the results (see table 16 and Figure 16) show that the effect of village size on the amount of extracted cash increases along with increasing socioeconomic heterogeneity in the village.

Table 16
Model estimation
(13)

		Number of obs	=	35
		F(7, 28)	=	5.19
		Prob > F	=	0.0007
Log likelihood =	2.9253	Pseudo R2	=	1.3275
cash_pc	Coef.	Std. Err.	t	P>t
(1-treat)	0.260***	0.082	3.16	0.004
qual	1.048**	0.402	2.60	0.015
soccap	-0.033	0.231	-0.14	0.889
pop	0.001***	0.000	4.17	0.000
heter	-0.002	0.052	-0.03	0.975
pop*heter	4.18e-05	7.76e-05	0.54	0.594
pval	-3.80e-08	5.37e-08	-0.71	0.485
_cons	-0.933	0.487	-3.03	0.005

* p<0.10, ** p<0.05, *** p<0.01

Figure 16



Interestingly, the results do not provide much evidence suggesting that social capital within the village should have any effect on elite capture, which is contrary to what theory would predict. However, it is possible that social capital effects are not picked up due to the very small sample size, so the low t-values on the social capital coefficients should not be taken as evidence contrary to previous findings.

(c) Robustness

To check the robustness of the results, I also estimate several stripped down versions of model specification (2) which allow for a slightly larger sample size and thus for somewhat higher power. Table 17 and Figures 13 – 18 show the results of several alternative specifications which include the interactive term, and thus implicitly assume that the chiefs can better influence project decisions when directly in charge.

Table 17
Alternative specifications with interaction term
(dependent variable *cash_pc*)

	(14a)	(15a)	(16a)	(17a)	(18a)	(19a)
(1-treat)	-1.112*	-1.190*	-1.343*	-0.365	-0.372	-0.227
	(0.573)	(0.605)	(0.756)	(0.444)	(0.418)	(0.474)
qual	0.102	-0.222	-0.270	0.447	0.385	0.543
	(0.363)	(0.465)	(0.452)	(0.366)	(0.422)	(0.500)
(1-treat)*qual	1.439**	1.527**	1.709*	0.724	0.729	0.560
	(0.676)	(0.710)	(0.879)	(0.525)	(0.506)	(0.529)
soccap		0.337	0.361	-0.0457	-0.0272	-0.0199
		(0.249)	(0.286)	(0.196)	(0.216)	(0.206)
pval			6.76e-08		-3.92e-08	
			(8.42e-08)		(5.38e-08)	
pop				5.30e-04***	5.59e-04***	5.03e-04***
				(1.58e-04)	(1.64e-04)	(1.33e-04)
heter						0.0117
						(0.0309)
N	39	39	39	38	38	35
p	0.0507	0.0106	0.0674	0.0182	0.0270	5.42e-04
r2_p	0.349	0.390	0.420	1.278	1.301	1.315

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The marginal effects of treatment on extracted cash remain significant only for higher levels of chief quality in all specifications of the models which do not control for village size (7a, 8a and 9a). There are, however, strong theoretical reasons for village size to be an important catalyst of elite capture (J.-P. Platteau & Abraham, 2002), and when controlled for, the marginal effects of treatment are significant for most observed levels of social capital. The slope of the marginal effects line flattens out as the model becomes more specific, but remains positive in all specifications.

Figure 17

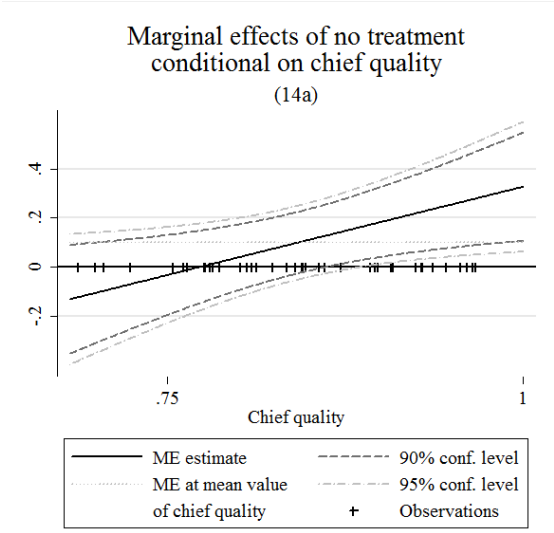


Figure 18

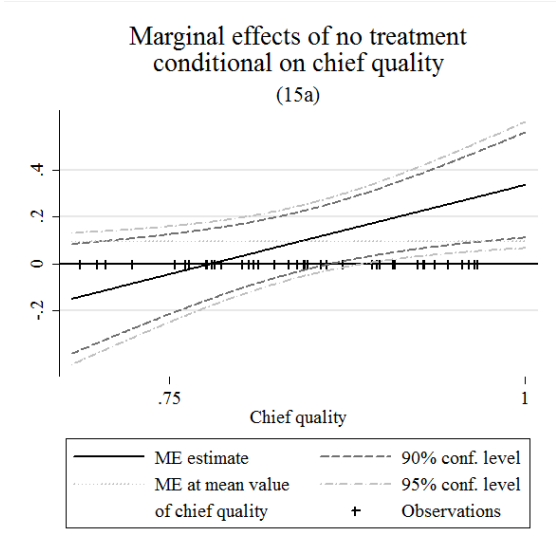


Figure 19

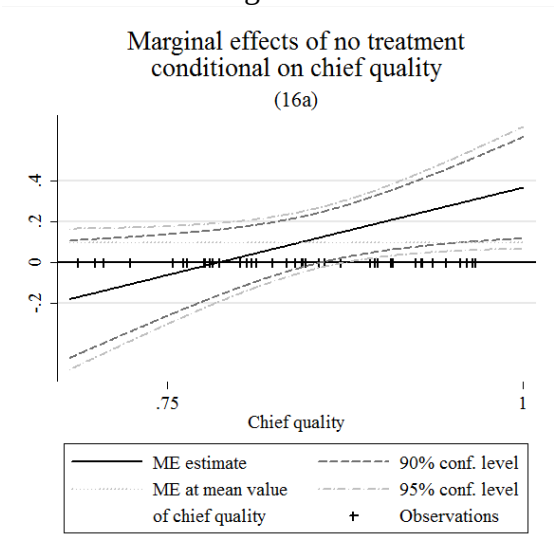


Figure 20

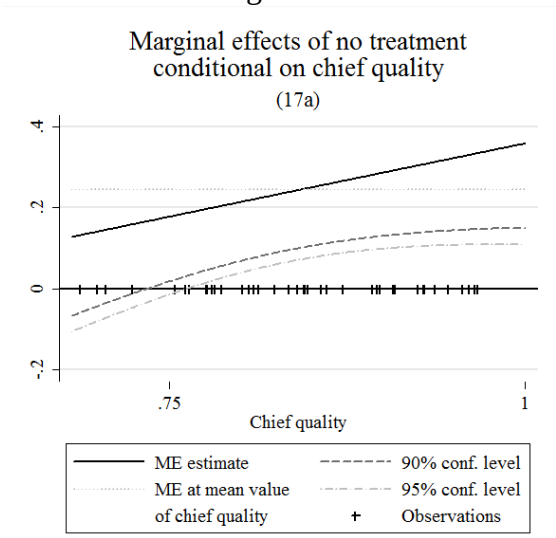


Figure 21

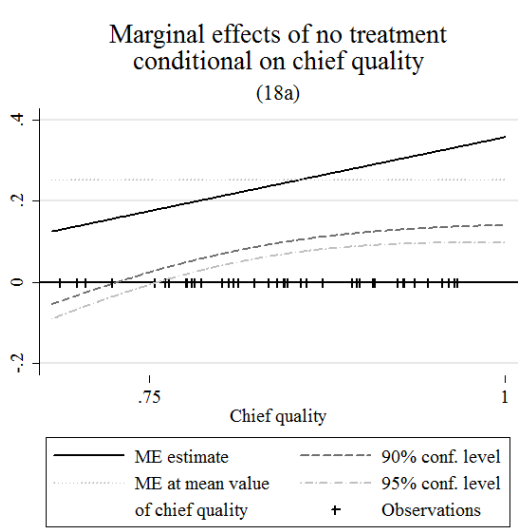


Figure 22

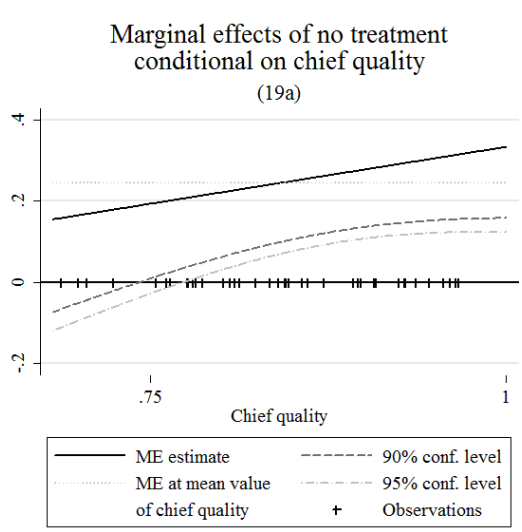


Table 18 provides estimation results of the same stripped down specifications, but without the interaction term. As long as one controls for project value *pval* and village size *pop* (9b – 12b), both of which can theoretically affect elite capture (J.-P. Platteau & Abraham, 2002), the results are largely similar to the ones discussed above (though they provide slightly less insight due to the missing interaction terms).[11]

[11] As there is no interaction term to offset their sign, the estimated coefficients for (1-treat) are now positive, directly indicating that more money is requested from projects when chiefs are in charge.

Table 18
Alternative specifications without interaction term
(dependent variable *cash_pc*)

	(14b)	(14c)	(15b)	(16b)	(17b)	(18b)	(19b)
(1-treat)	0.136	0.139	0.137	0.143*	0.259***	0.255***	0.258***
	(0.0848)	(0.0866)	(0.0848)	(0.0831)	(0.0819)	(0.0828)	(0.0814)
qual	0.883**		0.635	0.681	1.050**	1.005**	1.017**
	(0.425)		(0.523)	(0.550)	(0.391)	(0.416)	(0.397)
soccap		0.578**	0.384	0.381	-0.0381	-0.0182	-0.0127
		(0.281)	(0.319)	(0.356)	(0.198)	(0.216)	(0.207)
pval				5.06e-08		-3.86e-08	
				(7.61e-08)		(4.92e-08)	
pop					5.39e-04***	5.65e-04***	5.03e-04***
					(1.48e-04)	(1.53e-04)	(1.29e-04)
heter							0.0181
							(0.0289)
N	39	39	39	39	38	38	35
p	0.0973	0.0445	0.0500	0.103	0.0129	0.0257	0.00119
r2_p	0.267	0.256	0.305	0.323	1.262	1.285	1.306

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

At the first sight, one might worry about the results of specifications (14b), (14c) and (15b) where the estimated coefficients for chief quality *qual* and village social capital *soccap* are statistically significant when used separately in two different specifications, but not when included together. This could suggest that the two variables are highly collinear and their effect on the ordered cash may be misestimated. However, none of the regressors has a variance inflation factor larger than 1.5, which practically rules out the case of severe multicollinearity based on even the most stringent rules of thumb (O'Brien, 2007).

Despite the small sample and all the problems that come with it, the results for diverted cash thus hold to several robustness checks.

Similarly to the above, I also estimate several stripped down specifications of model (9) to verify that the insignificant findings for management ability are not due only to the decreased

sample size in the full specification. The results of the estimations are summarized in Tables 19 and 20 and Figures 23 – 26. In all the specifications, the effect of treatment on whether or not projects had been started remains insignificant.

Table 19
Alternative specifications with interaction term
(dependent variable *start*)

	(20a)	(21a)	(22a)	(23a)
(1-treat)	-17.92**	-17.79**	-16.11**	-15.45*
	(8.090)	(8.079)	(7.790)	(7.973)
qual	-1.034	0.396	0.935	1.052
	(5.463)	(6.216)	(6.147)	(6.984)
(1-treat)*qual	22.59**	22.49**	20.54**	19.23**
	(9.904)	(9.895)	(9.586)	(9.658)
soccap		-1.119	-1.228	-0.559
		(2.303)	(2.290)	(2.460)
pop			0.00125	0.000333
			(0.00214)	(0.00232)
heter				0.194
				(0.626)
N	39	39	38	35
p	0.0755	0.0877	0.194	0.210
r2_p	0.127	0.131	0.128	0.116

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 23

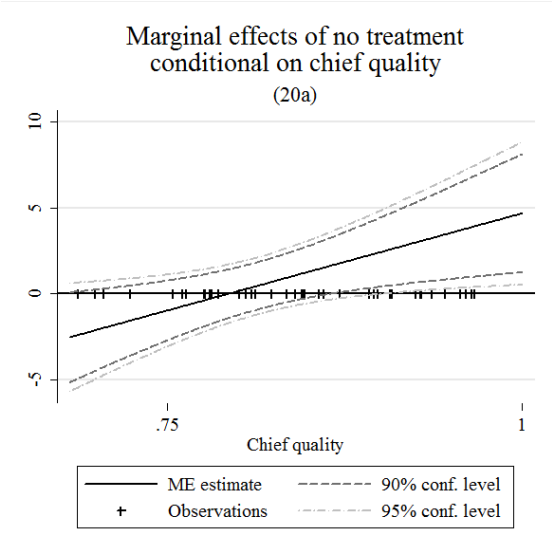


Figure 24

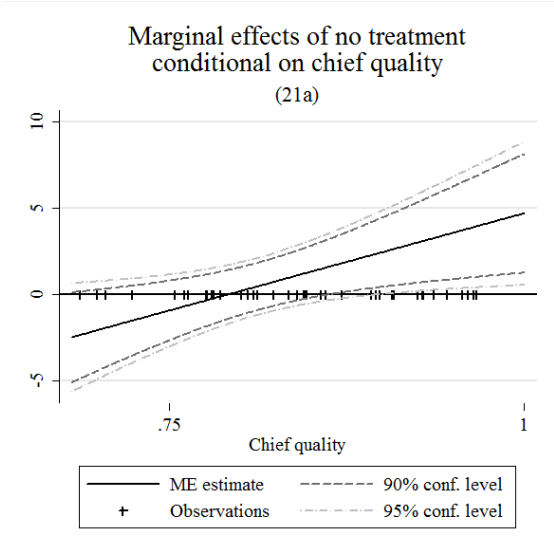


Figure 25

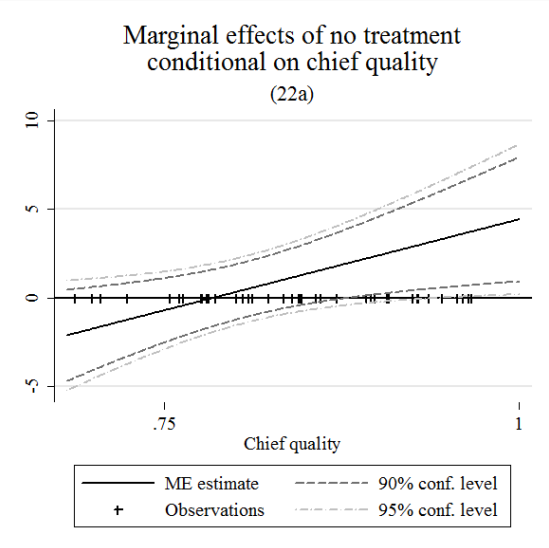


Figure 26

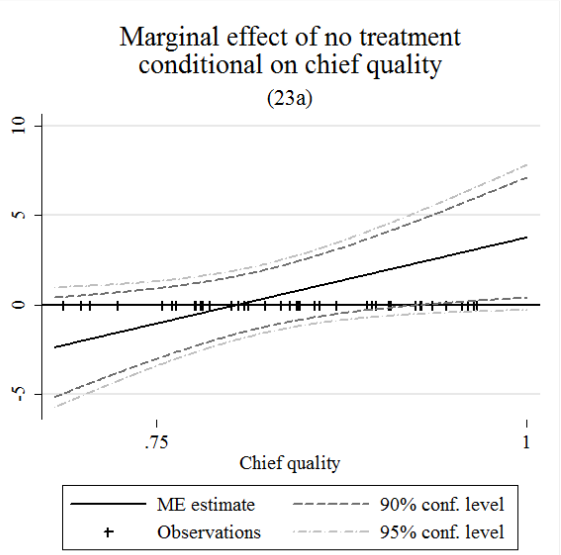


Table 20
Alternative specifications without interaction term
(dependent variable *start*)

	(20b)	(20c)	(21b)	(22b)	(23b)	(9b)
(1-treat)	0.696	0.663	0.760	0.842	0.421	0.447
	(0.689)	(0.695)	(0.685)	(0.783)	(0.834)	(0.843)
qual	4.914		6.740	6.848	7.679	8.180
	(4.771)		(5.952)	(5.803)	(6.347)	(6.239)
soccap		0.134	-1.532	-1.640	-1.187	-0.493
		(1.871)	(2.263)	(2.310)	(2.437)	(2.543)
pop				0.00163	0.000783	0.00147
				(0.00216)	(0.00238)	(0.00247)
heter					0.146	0.231
					(0.550)	(0.563)
pval						-9.75e-07
						(7.06e-07)
N	39	39	39	38	35	35
p	0.295	0.631	0.452	0.664	0.856	0.564
r2_p	0.0442	0.0191	0.0525	0.0616	0.0529	0.0848

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5. CONCLUSIONS

In this paper, I have offered an examination of the results of a field experiment in rural Sierra Leone designed to investigate a possible design-based solution to limiting elite capture in participatory development projects. I examined the effects of excluding local elites – in particular village chiefs – from project management on their ability to request cash from a project instead of materials, which, in the context of rural Sierra Leone, essentially amounts to elite capture.

The evidence suggests that limiting the chiefs' control over the projects by appointing a project management committee composed of randomly selected members of the community significantly lowers the level of elite capture from the projects as measured by the amount of cash requested instead of materials – money assumed to typically go straight into the chief's pocket. This seems to be especially true in communities with strong and well-respected chiefs, where the chiefs otherwise tend to extract more cash out of the projects than their weaker counterparts. Measuring elite capture in terms of the value of materials missing from the project gives similar results. To what extent does this happen despite of or with the approval of the chiefs' subjects is however a question beyond the scope of this research, and should be subject to further investigation. Nonetheless, this result has obvious implications for the design of future participatory development projects, which may be able to curb elite capture by opting to use other intermediaries than the traditional elites, especially since there is no evidence that traditional elites manage the projects better than others. Furthermore, there is evidence that controlling elite capture is more difficult in large and socioeconomically heterogeneous communities than in small, homogeneous ones, suggesting that it may be easier to avoid elite capture in small-scale projects than in larger ones.

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