Land grabbing and its consequences for traditional institutions and collective action

Pre-Analysis Plan

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Abstract

In many developing countries, the large-scale expansion of agricultural producers involves the acquisition of vast areas of land from traditional rural communities. Large-scale agricultural investments (LSAIs) may affect livelihoods positively or negatively. In this research project, we examine the consequences of LSAIs on social cohesion. We conduct household surveys and public good games with more than 2500 inhabitants from villages that are more or less affected by oil palm plantations in rural Indonesia. Before deciding how much to contribute to a public good, a randomly chosen subset is reminded of traditional institutions, the others receive a control prime. We investigate if social cohesion differs between villages that are more or less affected by oil palm plantations, and if dysfunctional traditional institutions drive this result. Furthermore, we look into migration history, ethnicity, and village elite membership.

Motivation

Collective action, in particular in rural areas, can be facilitated by traditional institutions. Modernization processes may undermine traditional institutions and thus lead to cooperation failure with potentially adverse development impacts.

Large-scale agricultural investments (LSAIs) continue to be an important phenomenon worldwide. As of 2021, 1,958 concluded deals with a total size of 54,600,821 ha have been reported in the Land Matrix and this number is expected to increase in the coming decades. Many large-scale agricultural investments are located in direct proximity to settlements and affect the availability of land as well as the availability of employment opportunities. Land becoming scarcer may affect livelihoods negatively (e.g. by decreasing nutritional security) (Liversage 2010), employment opportunities that have not existed prior to the investment may arise, constituting a positive shock. While many studies focus on the livelihood effect of LSAIs, less is known on how LSAIs affect *village-level social capital and social cohesion* (Khadjavi et al., 2020).

Theoretically, LSAIs can affect village-level institutions in different ways. The presence of LSAIs can be associated with a greater extent of *market integration*, such as exposure to new technologies and better infrastructure. Considering 118 small-scale rice-producing communities in China, Colombia, Nepal and Thailand, Cárdenas et al. (2017) find that greater market integration reduces contributions to a public good within the community. Similar results have emerged in Liberia (Dietrich et al., 2018). An exception is a study by Khadjavi et al. (2020) who find the presence of LSAIs to be positively associated with village social capital.

LSAI typically lead to a modernization push, increase market integration and competitive pressures on goods and factor markets, particularly land and labor. This “modernization” affects village-level traditional institutions that implement customary law, govern selected economic, social and cultural matters (e.g. property rights, labour exchange) and serve as conflict resolution mechanism. Simultaneously, in- and outmigration become more important and may lead to disruptions in

traditional institutions. Overall, villages affected by LSAI face a complex set of often simultaneous economic and institutional change that depends on initial characteristics and the type of modernization shock. These changes are very difficult to disentangle and causal relationships difficult to identify.

Specific to Indonesia is a certain revival of traditional institutions ("adat") combined with a modernization shock that partly comes with in-migration. All things considered, this reveals a dual ambiguity in the social fabric, one between insiders and outsiders, the other between the winners and losers of modernization.

Besides market integration, LSAIs may influence other village-level characteristics. For example, Braaten (2014) argues that an alteration of the *property rights structure* can play an important role: communities with joint property rights are more dependent on other fellow villagers and more committed to the community.

In our study, we focus on a related channel: the effect of LSAIs on *traditional institutions* more generally. Institutions can regulate issues connected to *culture, law and land*. With the presence of LSAIs, conflict around these issues can naturally emerge (e.g. which land to sell to the investor, which land is sacred land, should the community follow traditional or formal laws etc.). Traditional institutions serve different purposes, but they usually aim at creating harmony within their community. The presence of LSAIs may put these institutions under stress, either because they lose importance or because they need to reunite very conflicting interests.

Analysis Plan

Research questions and Hypotheses

H1: Social cohesion is lower in villages near investment sites.

H2: Dysfunctional traditional institutions drive the lower levels of social cohesion in villages near investment sites.

Outcomes of Interest

We measure social cohesion by adopting an (incentivized) public good game. The primary outcomes relate to the amount a person is willing to contribute to the public good. Secondary outcomes include variables that we will use to investigate mechanisms through heterogenous treatment effects. These relate to changes in wealth (magnitude, fluctuations, uncertainty, working hours), local inequality, personal experience of land expropriation, intra-household bargaining power, indebtedness, conflict experience, job satisfaction, and unfulfilled expectations. Furthermore, we look into migration history, ethnicity, and village elite membership.

Research design

We combine a quasi-experimental set-up with households in control and treatment villages (see below) with a lab-in-the-field experiment (see below). We examine differences in the contribution to a public good between treatment and control villages. Further, we test whether the effect of being reminded of a traditional institution ("adat" as living together in harmony) on social cohesion differs between treatment and control villages.

Quasi-Experimental Study Set-up

The study applies a two-stage sampling design, first selecting villages, then households. The study area is (for logistical considerations) defined by a 30 km buffer around two roads stretching from the cities Mempawah to Sintang and from Pontianak to Tayan in West Kalimantan.

Villages in the study region are defined to be part of the treatment population if a large-scale oil palm plantation was installed only after 2000. Data on oil palm expansion are from Xu et al. (2020), manually updated, corrected and extended to the current state with additional freely accessible satellite data. These also enable us to remotely identify the implementation year of a plantation (via Google Earth).

The population of control villages are all neighbouring villages without any new or old plantation. From these populations we randomly sample 40 control and 50 treatment villages. We stratify both village samples to have an equal share of Dayak and non-Dayak majority villages[1]. Household sampling of 30 units within each village is done at random. The village-level interview is conducted with 4-6 participants respecting individual availability. Through this village sampling procedure we make sure that the treatment and control villages share comparable geo-physical and climatic conditions as shown in Table 1.

Table 1: Balance table of oil palm suitability and biophysical characteristics

treatment	control	diff	pval	N_t	N_c	sign	label
2351	1690	660.3	0.2522	50	40		hectares suitable for oil palm
0.4138	0.3775	0.0362	0.527	50	40		share of village area suitable for oil palm (in %)
246.6	247.1	-0.4771	0.8656	50	40		average precipitation in mm
16584	16594	-10.42	0.7128	50	40		average solar radiation in kJ m-2 day-1
1.263	1.198	0.0657	0.813	50	40		average slope in percent
49.27	40.89	8.382	0.4253	50	40		average elevation in m
27.1	27.14	-0.0391	0.4274	50	40		average temperature in °C
1.143	1.19	660.3	0.2834	50	40		average wind speed in m/s
50	40						N

Similar conditions in terms of socio-economic characteristics are, in contrast, less obvious. Therefore, we also examine pre-shock characteristics. The pre-shock (i.e. pre-plantation establishment) characteristics of the sampled villages are largely balanced across the treatment and control group which can be confirmed with secondary data for the year 2000 (see Table 2).

Table 2: Balance of selected variables based on PODES 2000 data

treatment	control	diff	pval	N_t	N_c	sign	label
3108	2634	474.2	0.41	48	38		Population
0.8406	0.8737	-0.0331	0.3663	48	38		Share of agricultural households
0.2799	0.3598	-0.0799	0.2109	48	38		Share of households with electricity
0.9375	0.9474	-0.0099	0.8469	48	38		Dummy for firewood being the most important fuel in village
1.146	1.026	0.1195	0.6023	48	38		Number of natural disasters in the past three years
0.7708	0.7632	0.0077	0.9344	48	38		Dummy for critical land

[1] Given that the control village pool did not contain enough non-Dayak villages there are slightly more Dayak (22) than non-Dayak majority (18) villages in the control sample.

treatment	control	diff	pval	N_t	N_c	sign	label
0.1458	0.1053	0.0406	0.5751	48	38		Number of public health centers
0.0055	0.0055	0	0.9822	48	38		Share of households with four-wheeled vehicle
0.0773	0.0645	0.0128	0.3379	48	38		Share of households with two-wheeled vehicle
0.2767	0.2891	-0.0124	0.8092	48	38		Share of households with a health card (kartu sehat), which is targeted at the poor (social safety net)
0.0556	0.0782	-0.0226	0.3679	48	38		Share of households with a poverty card
1083	793.4	289.9	0.4874	48	38		Area of community forest in hectares
30.19	35.45	-5.254	0.6571	48	38		Cases of theft per 100.000 people
54.81	47.3	7.508	0.2189	48	38		Cases of murder per 100.000 people
47.44	47.01	0.4301	0.9463	48	38		Increase in number of members of the civil defense
4.062	3.658	0.4046	0.0384	48	38	*	Village head: highest education achieved
2.875	5.053	-2.178	0.1107	48	38		Village head: length of office (from appointment to enumeration)
43.23	45.03	-1.797	0.3354	48	38		Village head: age
0	0	0	NA	48	38		Village head: gender (all male in both groups)
2.826	3.428	-0.6012	0.8079	48	38		Private kindergardens per 100.000 people
0.2619	0	0.2619	0.3224	48	38		Private middle schools per 100.000 people
103.2	101.6	1.611	0.8791	48	38		Public elementary schools per 100000 people
4.062	12.63	-8.572	0.0608	48	38	*	Public high schools per 100000 people
0.0655	0.0813	-0.0157	0.4454	48	38		share of households with a satellite dish
0.0049	0.0049	0	0.9959	48	38		Share of households with a landline phone
0.2182	0.2297	-0.0115	0.7736	48	38		Share of households with a TV
50	40						N

Most importantly, forest cover data (Hansen et al., 2013) confirm that the treatment assignment as explained above is valid, as treatment villages experienced significantly larger deforestation as compared to control and out-of-sample villages within the study area (see Figure 1 for the entire population of villages in the study area).

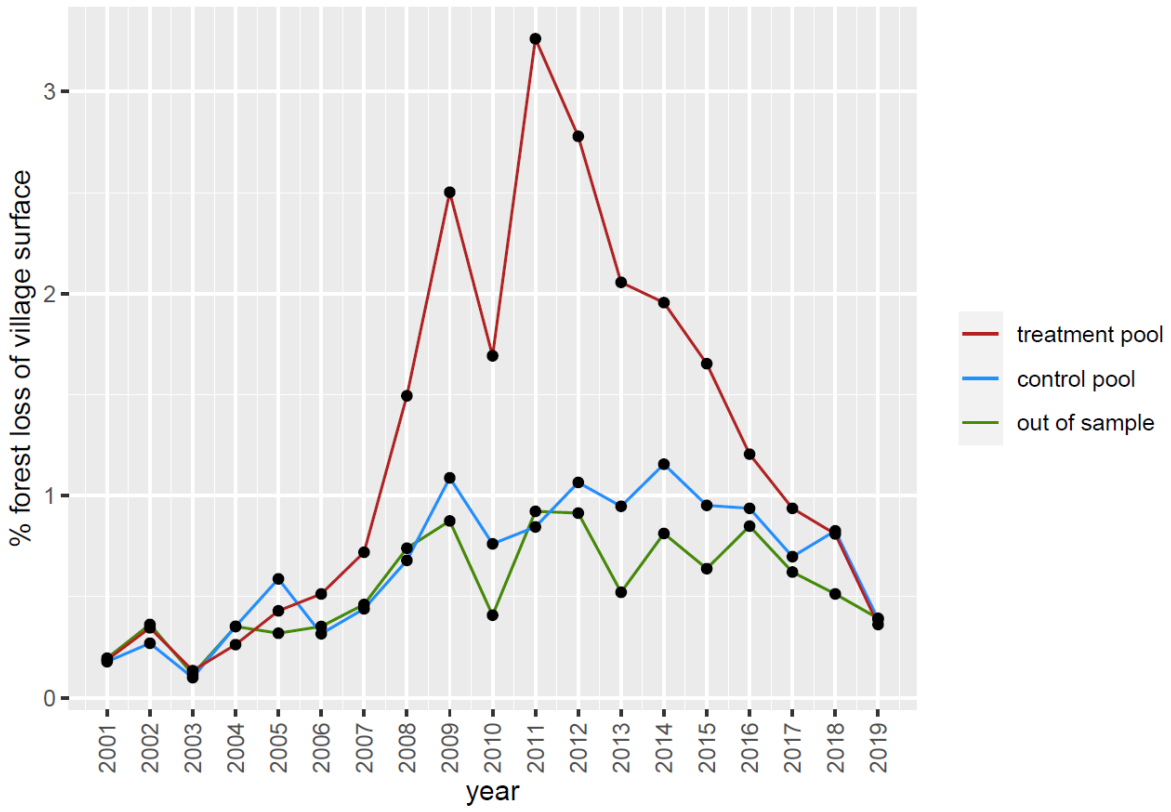

Figure 1: Deforestation across village populations

Experimental Design

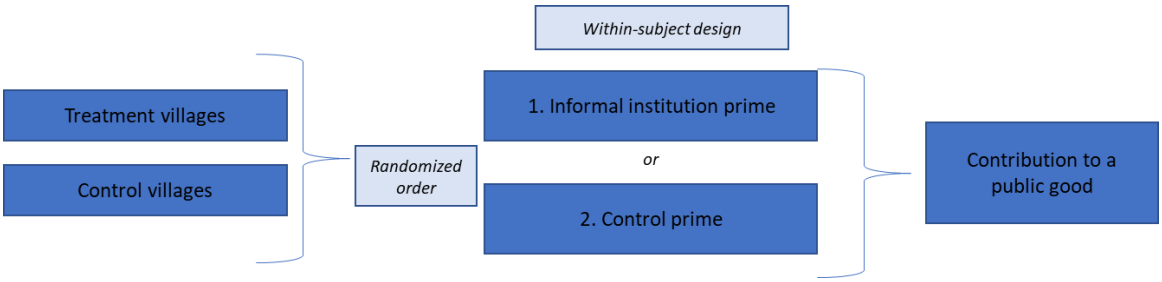

Figure 2: Draft experimental design

We conduct experiments in treatment and control villages with village leaders and the general population. Participants will contribute to a public good. They will be assigned to groups of four people. They will only know that they interact with people from their village, but the identity is kept anonymous. Our participants will be respondents from a survey that has been carried out on the same day. They are invited to join research activities on decision-making. The study context (lab-in-the-field study; non-university subjects, some of them presumably with low literacy and numeracy skills) requires a simple experiment.

Intervention

- Each participant receives, independent of her decision, a ”show-up fee“
- Additionally, each participant receives an endowment for the experimental part

- Participants will be able to contribute to a public good in groups of four
- Their contribution is doubled and then equally shared among the group
- Each participant makes one decision
- Additionally, participants will be randomly assigned to one of two treatments
    - (1) **Traditional institution treatment:** *The idea of "adat" is that everyone in this village lives together in harmony. Do you agree, disagree or are you undecided?*
    - (2) **Control treatment:** *Everyone is talented at something but no one is talented at everything. Do you agree, disagree or are you undecided?*
- At the end of the experimental procedures, participants receive the fixed "show-up fee" + the variable experimental earnings

Randomization

The random assignment is done by the PIs with a replicable procedure using statistical software.

Estimation

Treatment effects are obtained from multivariate regressions; in particular OLS. The study applies two distinct empirical approaches to shed light on the potential causal effect of large-scale land acquisitions on village institutions.

Approach 1: Approach 1 leverages the sampling and stratification and takes into account that about 55 percent of villages are villages that are directly affected by large-scale land acquisitions, while about 45 percent of villages are not directly affected by large-scale land acquisitions. Identification of treatment effects is obtained from estimating equation 1 via OLS.

$$Y_{iv}=\gamma T_v + \beta + X'_{iv}\delta + W'_{v}\sigma + \varepsilon_{iv} \quad (1)$$

where:

- Y: refers to the outcome variable of individual i in village v
- T: refers to the treatment indicator (village-level)
- X: refers to individual and household-specific controls. Controls are selected based on stratification criteria.
- W: refers to village-level control variables. Control variables refer to pre-land acquisition variables, in particular population size and ethnic composition prior to the start of land acquisitions
- ε is the error term, while β refers to the constant.

We are aware of potential endogeneity issues in the form of reverse causality in this analysis. We attempt to address these issues with different approaches. Firstly, we control for observables where available and for unobservables with variables that are potentially correlated with these (e.g. district or year fixed effects). We test robustness with different estimation strategies, for instance with matching, by using subsamples, or by investigating the heterogeneities in γ.

Approach 2: Approach 2 makes use of the experimental intervention (priming strategy) in combination with the sampling and stratification strategy. Let's denote with P the priming treatment. The prime

aims at stimulating the individual's attitude towards the traditional (adat) institution's role in enhancing harmony. Stimulation occurs via a statement with the question about the degree of agreement. $T_v = 1$ denotes villages that directly experienced large-scale land acquisitions. Treatment effects are obtained by estimating equation 2 using OLS. The coefficient of interest is gamma.

$$Y_{iv}=\gamma PT_{iv} + T_v\delta + P_{iv}\pi + \beta + X'_{iv}\delta + W'_v\sigma + \varepsilon_{iv} \quad (2)$$

where:

- Y: refers to the outcome variable of individual i in village v
- T: refers to the treatment indicator (village-level)
- P: refers to the priming treatment (individual-level)
- X: refers to individual and household-specific controls. Controls are selected based on stratification criteria.
- W: refers to village-level control variables. Control variables refer to pre-land acquisition variables, in particular population size and ethnic composition prior to the start of land acquisitions
- ε is the error term, while β refers to the constant.

In addition, in estimating heterogenous treatment effects, we will conduct split-sample analysis with respect to treatment villages (villages with and without large-scale oil palm plantations).

Randomization Checks

We will report the extent to which observable covariates are balanced across treatment conditions, as we expect to be the case. The following variables are examined: age, gender, marital status, education, religion, risk preferences, number of children, work status, wealth, land ownership, mental health indicators, within-village exposure to land transaction as part of land acquisitions.

Heterogeneous Effects

We will report heterogeneous effects for each outcome and treatment-control group comparison, either by using separate samples or interactions.

Spillovers

We will report specifications in which standard errors are adjusted for spatial correlations (Conley-type of standard errors).

Mechanisms

We will perform mediation analysis in order to understand the mechanisms underpinning our effects of interest. The relevant variables we will use to investigate mechanisms through heterogenous treatment effects. These relate to changes in wealth (magnitude, fluctuations, uncertainty, working hours), local inequality, personal experience of land expropriation, intra-household bargaining power, indebtedness, conflict experience, job satisfaction, and unfulfilled expectations. Furthermore, we look into migration history, ethnicity, and village elite membership.

Compliance

Compliance with treatment assignment is guaranteed by the study design.

Attrition

Not applicable.

Manipulation Checks

We will conduct several checks to see if our interventions perform as expected. As part of the training and pre-test process we verified that the applied primes are able to stimulate subject's mental activities.

Cited Literature

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