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Volunteerism after the Tsunami: The Effects of Democratization

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Abstract

Using three waves of survey data from fishing villages in Aceh, Indonesia for 2005–09, we examine the determinants of local volunteer labor after the tsunami. Volunteer labor is the village public sector labor force for maintenance, clean-up and renovation of public capital. While also examining the effects on volunteerism of village destruction and trauma, pre-existing social capital, diversity, and aid delivery, we focus on effects of democratization. The tsunami and massive international aid effort prompted the settlement of the insurgency movement in Aceh, which had led to suspension of local elections over the prior twenty or more years. Until 2006, village heads who call volunteer days were effectively selected by village elites, who may highly value the public facilities maintained by volunteer labor. With elections, volunteer days fall under the new regime, with democratically elected village heads calling fewer volunteer days, which may appeal more to the typical villager. Identification comes from pseudo-randomized differential timing of elections.

JEL classification: D64, D72, H, O, P16

Key words: volunteerism, social capital, aid, democratization

Volunteer labor in traditional societies is a key part of village life. There is no paid local public labor force to collect litter and sweep the roads, maintain village lands and pathways, and build and repair coastal barriers and irrigation and aquaculture channels. There is also no public labor force to repair or renovate village-provided public buildings such as mosques and village and fishermen's halls. Buildings like schools and health care facilities are government provided. At times, there are construction monies for village-provided buildings such as halls or mosques from outside sources. However, repairs, maintenance, and renovation are a different matter. In Indonesia, such local public labor activities are done through volunteer labor, where village males assemble several times a month, in a response to a call by

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the village head for a volunteer day. While volunteer days may be viewed as informal voluntary taxation (Olken and Siyokal 2011), they are a form of taxation that is central to village life.

This paper studies volunteer labor in 190 coastal fishing villages in Aceh Indonesia in an unusual context, the years following the tsunami of late December 2004. The tsunami wiped out almost all buildings, housing, and boats and significant portions of the population in the villages we study and prompted a massive inflow of private aid to these villages, replacing most lost physical capital by the end of 2007. Most critically for this paper, the tsunami and aid gave high exposure to the twenty-plus year conflict between Aceh and the Indonesian central government and resulted in a settling of that conflict. That settlement prompted the introduction of formal elections in villages starting in 2006, with differential timing on the introduction of those elections from early 2006 to past early 2010.

This differential timing provides a potential opportunity to study the effect of village level democratization on volunteerism as recorded near the end of both 2007 and 2009, with additional recall information on aspects of pre-tsunami volunteerism. While the effect of earlier democratization in other parts of Indonesia on other phenomena has been studied (Henderson and Kuncoro 2011 and Martinez-Bravo 2011), here we focus on a new outcome in the general literature on democratization, volunteer labor. Overall, democratization in Indonesia occurred in 1999, although regular elections of village heads started earlier, albeit in an overall national non-democratic context. But in Aceh the civil conflict and insurgency led to suspension of all formal elections. The tsunami and international exposure led to a declaration of a ceasefire and then a settlement of the conflict situation in Aceh in late 2005, with Aceh achieving considerable autonomy. In early 2006 the first formal, mandated elections for villages in Aceh in at least two decades were authorized, in line with the democratization that had occurred earlier in the rest of Indonesia. However as we will detail below in sections 1b and 3b, implementation of formal elections in villages was delayed in many districts and villages, helping with identification.

What is the effect of democratization on volunteerism? A priori, given work by Dalbo, Foster, and Putterman (2010) or Bardhan (2000), we thought democratization might increase volunteerism.[1] In experimental work, Dal Bo, Foster, and Putterman (2010) find that the effect of a policy on the level of cooperation is greater when the policy is chosen democratically, as opposed to imposed exogenously. We find democratization is associated with reduced volunteerism. Relative to Dalbo et al., we note that pre-democratic volunteerism in our villages was not exogenously imposed; it occurred in a traditional village context.

Prior to election reforms in early 2006, in most villages, village heads were chosen by a variety of informal mechanisms, with elites effectively selecting village heads. As such, volunteer days involved public labor choices made by heads selected by elites. Democratization introduced formal elections, whereby almost all former village heads are replaced. Villages having elections by the two time periods for which we have data (late 2007 and 2009) have about 35% lower volunteerism than villages yet to have elections and effects persist. We will argue that the effect is causal. While there may be competing explanations for the effect, we will argue that volunteerism declines because a democratically elected leader, relative to an elite leader, following preferences of the typical villager chooses fewer projects that involve volunteer labor. Thus the effect follows from a regime change. The outcome is consistent with the work of Martinez-Bravo, Padró-i-Miquel, Qian, and Yaoo (2011), which finds that, in China after

1 While some literature hints that "modernization" and the introduction of formal institutions might reduce social cooperation (Putnam's 1995 lament on "bowling alone," Persson and Tabelli 2000, and Costa and Kahn 2003), these analyses involve gradual changes over long periods of time.

local election reforms, locally elected leaders are more responsive to the wants of villagers than leaders imposed from above.

Is the association of differentially timed elections with lower volunteerism a causal effect of elections on volunteerism?[2] We don't have the context to nail an experimental causal effect and it is not clear under what circumstances such a context would arise. What we have is evidence that is suggestive of a causal relationship. In general, the magnitude of the relationship is the same with no village level controls versus extensive controls that affect volunteerism, suggesting the elections' effects may be orthogonal to other factors. But the key is to argue that, as related to volunteer days, the timing of elections is pseudo-randomized. Formal elections are spread over at least 5 years, driven in part by differential timing of when district and subdistrict governments push villages in their domain to have elections. This aspect is discussed in detail in section 3. The timing of village elections, beyond district fixed effects, is uncorrelated with observable village characteristics. Most critically, elections held in different years have the same effects on volunteerism, suggesting that unobservables affecting election timing do not influence election impacts on volunteerism. Finally, we also have placebo elections, which are informal "elections," typically dominated by village elites held soon after the tsunami to replace village leaders who died. Such elections have no effects on volunteerism.

In doing this analysis we can't and shouldn't completely divorce the paper from the unusual context of massive destruction and aid in post-tsunami Aceh. First, there are potentially direct effects of destruction on volunteerism. Public building that were destroyed were replaced by NGO's with brand new buildings, potentially reducing the need for volunteerism for maintenance, while surviving public buildings generate a need for damage repair and maintenance. Destruction required reconstruction of the coastal line and barriers, mostly by volunteer labor. Second, it may be that differential destruction, differential existing social capital stocks and their destruction, and the massive aid process could all affect volunteerism and could as well influence the timing of elections. At a minimum we want to show that controlling for these factors does not affect the democratization results.

However, the context provides an opportunity to relate to other literatures on volunteerism. When we develop our model of election impacts, we will discuss the general demand and supply factors affecting volunteerism. When we discuss the effects of controls we will talk about explicit hypotheses in the literature that our results relate to. To give a taste of the issues here, we note that in the literature, volunteerism may be affected by village size and diversity (Alesina and La Ferrara 2000; Costa and Kahn 2003). Volunteerism may be affected by the aid and destruction process, with aid directly altering the opportunity costs and benefits of volunteering (Svennson 2000, Labonne and Chase 2008, Knack and Rahman 2007). Social capital and its destruction by the tsunami could affect volunteering by, say, altering the perceived social costs of not volunteering (Putnam 1995, Sobel 2002, Getler, Levine, and Miguel 2009).[3] Later, in a section on controls, we will suggest specific hypotheses that our data may relate to.

We start by describing the unusual context. Then we present a model of volunteerism, discuss the estimating framework, and analyze results.

2 Under the Aceh autonomy laws passed after settlement of the civil conflict, villages receive a small grant (about $11,000 USA) from the provincial government, which in principle they could spend on paid labor. However this goes to villages whether they have had elections or not, so the differential effect of the introduction of elections is not related to government spending crowding out private giving (Andreoni 1993).

3 There is a large literature on the best ways to deliver aid (Collier et al. 1997, Azam and Laffont 2003, Svennson 2003, Murrell 2002, Pederson 2001, Torsvik 2005, Kanbur and Sandler 1999, Easterly 2003, and Paul 2006), which is not really relevant here. The aid process in Aceh was generally unconditional and uncoordinated. The government agency overseeing the process, BRR [Executing Agency for the Rehabilitation and Reconstruction of Aceh and Nias], largely defined its role as (1) a clearing house recording aid and (2) late in the process filling in ex post gaps with monies it administered from the (international) multi-donor fund.

Table 1. Destruction of Population and Housing

	Average	Standard deviation	Min	Max	N
Survival of population (based on government numbers)	0.75	0.45	0.11	3.7	188
Pre-tsunami population	853	901	100	7103	188
Post-tsunami households, official	177	197	20	1516	190
House aid (2009)					
Number of houses survived tsunami, survey	26	112	0	1354	176
Survival rate houses	0.097	0.23	0	0.97	176
Number of permanent aid houses built	200	215	0	2177	190
Replacement rate by late 2009[a]	1.57	1.84	0	20.8	190
Other aid information (2009):					
Survival rate public buildings[b]	0.16	0.23	0	1	190
Replacement rate, public buildings	1.00	0.82	0	5.25	190
Number of different NGO's as first level implementers in RAND in the village	11.0	5.50	1	37	187
Number of different projects (first level implementations) in RAND in the village	31.8	16.2	1	106	189

[a]The replacement rate is the number of houses given in aid divided by the number of surviving households less the number of surviving houses. Treats missing values on survival as zeros for 14 cases for this calculation.

[b]Includes mosques, village halls, fishermen halls, public and Islamic elementary schools, and health facilities.

1. The Context

We look at the extent of destruction from the tsunami, aid delivery, and aspects of democratization relevant to the investigation. To keep a sequence of events, we start with items related to our controls, before turning to democratization.

(a) The Shock: Destruction, Relief, and our Data

The tsunami struck Aceh in late December 2004. After late winter and spring fieldwork, in the summer and fall of 2005, we surveyed village heads and local heads of the fishermen's association (*Panglima Laot*) in 111 fishing villages. In the fall of 2007 and of 2009, we resurveyed the 111 villages and added another 88 villages, which were further away from the capital Banda Aceh and were inaccessible in 2005. The intent was to cover the universe of fishing villages as we moved some distance south and north-east of the capital Banda Aceh, as explained in the appendix. Our villages are in the five districts most affected by the tsunami and account for about 30% of all house aid in Aceh, with urban Banda Aceh accounting for much of the rest. The timing of the tsunami, fieldwork, and surveys are in appendix table A1. One set of surveys focuses on village level characteristics and outcomes. As noted in the appendix, they ask in detail about the public and private capital destruction and the aid process and then ask background questions on village demographic and socio-economic characteristics. Table A2 in the appendix gives statistics on village characteristics we use in estimation. For these questionnaires, in the majority of cases, the official interviewee is the village head, who is also the respondent to the PODES, a tri-annual census of Indonesian village populations and facilities. But often multiple village officials were present and village heads were not shy about asking other prominent villagers to supply or confirm information.[4]

The tsunami devastated coastal cities and villages. Table 1 presents an overview of destruction and aid in 190 of our villages with complete information, using different data sources discussed in the

4 In 2007, for example, 62% of the time the official respondent was the village head, 25% the secretary, and the rest at least both. But often the mullah or head of the local fisherman's association would drop by and participate in the interview. Surveyors generally did not record this, as these are not village officials per se.

appendix. Table 1 reveals the nature of the shock affecting villages. Destruction is massive. In 104 villages around Banda Aceh surveyed in 2005, under 50% of the population survived; in the expanded set used in the paper the survival rate is 75%. The destruction of physical capital in the overall sample was almost universal, given both the earthquake that created the tsunami and the wave following 20–30 minutes later. Survival rates of houses for the overall sample averaged 9.7% and of all public buildings 16%.

The immediacy and extent of aid is impressive. As we will note below, aid was private, most done directly by NGO's with some foreign government monies funneled through a temporary agency in Aceh set up by the national government. District and subdistrict governments did not play a role in organizing and delivering aid, although they are responsible for organizing village elections. In terms of aid, in our villages, 157% of needed houses had been replaced by 2009. Overall in Aceh, aid gave 134,000 houses for 120,000 houses destroyed (Xinhua News Service, February 1, 2009), despite the reduction in population. For public buildings, 100% of destroyed buildings had been replaced by late 2009, a good rate given the loss of village populations. Later we will use information on public buildings destroyed and surviving as measures that might influence the demand for volunteerism. Within three years of the tsunami, the aid process had accomplished what it intended—to replace the entire per household physical capital stock. To deliver aid, private NGO's contracted with villages. Aid delivery takes up village head time and the need and energy for calling of volunteer days, so it may be important to control for the extent of aid. To measure the extent of outside intervention in a village, we use the RAN [Recovery Aceh-Nias] database to count aid projects in a village [http://rand.brr.go.id/RAND/reference]. From table 1 there were on average 11 different "first level implementers," or different aid agencies actually delivering aid on the ground per village, each with different projects, each negotiated separately. The mean number of projects from these aid agencies was about thirty-two in 2009.

(b) Democratization

For twenty years prior to the tsunami, Aceh had an insurgency movement, the Free Aceh Movement [GAM]. The national government imposed effective military rule, with villages caught in the struggle between the army and the insurgents. While free, formal elections in villages in Indonesia commenced in 1979, in Aceh these were suspended. Village heads were chosen and certified by the subdistrict (*kecamatan*) government, in close consultation with the village council of elders (*tuhapeut*), who represent the elites in the village. The few elections that occurred were irregular in timing and informal, usually with lack of contestation, secret ballots, and/or full voting rights; they tended to rubber-stamp the selection of the head chosen by elites. An election, for example, might involve a show of hands by males in a post-Friday prayer meeting at the mosque. The introduction of regularly scheduled, contested, and certified elections with secret ballots and full voting rights followed major election reforms enacted by early 2006, after settlement of the insurgency.

Table 2 shows that elections phased in over time. During 2006 and 2007, 47.4% of our villages had formal elections under the new procedures and another 23.1% in the next two years. Still by the end of 2009, 29.5% still had no formal election. All remaining villages were slated for elections in 2010. This differential timing will be utilized in estimation and arises in part from the differential timing of pressure from subdistrict and district governments to schedule local elections. We devote considerable space to analyzing timing in section 3c. The short version is that no village observables affect timing, other than the district a village is in. Some districts placed greater weight on having earlier elections. While we cannot argue that election timing is exogenous to the village, we will argue that our results indicate that the unobservables affecting timing do not affect volunteerism.

Elections appear not to be kind to old leaders, although we don't know the extent to which such leaders stood for election (but then that would be endogenous). At the end of 2009, under 15.5% of old

Table 2. Elections and Old Village Heads

	Percentages
Post-tsunami elections before end of 2007 [N = 190]	47.4
Post-tsunami elections before end of 2009 [N = 190]	70.5
No. of village heads who survive (out of 199)	76.4
If survive: in office at end of 2009 (22 out of 152 survivors; [out of 199])	15.5 [11.8]
& reelected post-tsunami (5 out of 22 still in office; [out of 199])	22.7 [2.5]
% village heads with high school, pre-tsunami	46.7
% village heads with high school in 2009 (out of 199)	62.3
% old village heads with high school in 2009 (out of 22 survivors in office)	36.4

village heads remained in office and under 22.7% of those (i.e., five) were in office after an election (table 2). We believe that this means that formal elections shift power from a narrow set of village elites to the more general population, and the nature of village heads changes. In particular, their education level, as measured by high school completion or not, changes. As the bottom rows of the table show, village heads in 2009 are much better educated than pre-tsunami heads overall, and especially those still in office in 2009.[5] Related but not in the table, in 2009, 66% of heads elected post-tsunami have high school or more, while only 48% of non-elected have high school or more. We will investigate whether this change in education level itself affects volunteerism.

2. Conceptualizing Volunteer Labor Under Democratization

In formulating the approach, we expect the type of village head to differ between a traditional regime and a democratic one. In a traditional regime where the village council has a key role in choosing a village head, we believe that the village head generally is chosen from village elites represented by the council and familiar to the subdistrict government. In contrast, under democracy, the village head is more likely to be chosen from the village at large and to represent more general interests (cf., Martinez-Bravo et al. 2011). These two types of village heads may have very different valuations of projects utilizing volunteer labor, and we examine one characterization. To incorporate individual participation decisions, we adopt a simple version of Foster and Rosenzweig (2005). Although we model a representative village, at the end we note how different variables might represent demand or supply conditions that would vary across villages.

Suppose villagers are heterogeneous in their preferences for any projects requiring public labor. Villagers are ordered uniformly on the unit interval in terms of the value, x, they place on any public service produced with volunteer labor. There is a set of possible public projects where the base valuation of a project i is $C_i \geq 0$, and projects differ in their C_i's from low to high. There are two political regimes, elite controlled (e) and democratic (d). The question is if, say, all high valuation projects are chosen under both regimes, how does the equilibrium lower cut-off value $\underline{C}j$ differ for projects chosen under regime e vs. d? Which regime has a lower cut-off and thus potentially chooses more projects? Any individual has a valuation for a project of $L^{\delta}xC, x \in [0,1], \delta < 1$. L is total volunteer labor devoted to a project and thus gives the endogenous scale of any project.

For the volunteer process, to simplify, we assume a volunteer provides a fixed amount of effort at a cost b. We start by assuming that b is the same for all villagers. Heterogeneity in b will help explain, in an obvious way, individual participation differences and could arise from differences in personal or psychic costs of participation and the like. Of course, heterogeneity in b makes the full village solution more

5 In fact, some districts under the election reforms in principle required village heads to have a high school education. However the village can argue that no available person meets that criterion or that a candidate is worthy anyway. There seems to be a sense that the requirement itself is not binding, but villagers are opting for more educated heads.

difficult. But if, say, b increases with x but at a slower rate, our comparisons of equilibria under the two regimes will not be qualitatively affected.

For the base case, a person volunteers if their private marginal product exceeds cost, or $\delta L^{\delta-1}C_i x \geq b$. If a project is announced, total volunteer labor is $L = (1-s)N$, where N is the mass of people in the village and s is the lowest valuation person to volunteer from the unit interval of valuations x (assuming $\delta L^{\delta-1}C_i > b$). This lowest valuation person to volunteer from the unit interval is given by the equation

$$\delta L^{\delta-1}C_i s \equiv \delta[N(1-s)]^{\delta-1}C_i s = b. \tag{1a}$$

This defines an implicit function

$$s = s_L(C_i, b; N), \frac{ds}{s} = -\frac{(1-s)}{(1-\delta s)}\frac{dC}{C}. \tag{1b}$$

Note also that $db/dC|_s > 0$. One might impose that any project has a minimum required labor $\underline{L}$ or maximum $\bar{s}$,

$$\underline{L} = (1-\bar{s})N. \tag{2}$$

If this is imposed, $\bar{s}$ cannot be exceeded if a project is to be viable.

In the first regime under which projects are proposed, a village head is chosen from village elites. To make our point, we assume village elites have the highest valuations for public projects and occupy the interval $[x_e, 1], x_e > 1/2$. Other work shows that public good composition changes with electoral reforms in the context of caste and landlessness in India (Foster and Rosenzweig 2005 and Munshi and Rosenzweig 2010). In Aceh there are no castes, and the reform is to have elections, not to shift the balance of power within an election regime. More critically, the range of relevant services involved is narrower, mostly maintenance, cleaning, or renovation of village public facilities and areas including docking facilities and aquaculture channels, which have more appeal to elites who intensively use such facilities. This characterization is based on our extensive fieldwork, which also involved professionals who grew up in Aceh; it is broadly consistent the case studies reported in Evers (2000). We assume that, in the elite regime, elites choose all projects desired by the person with preferences x_e. Under a democratic regime, d, we assume the median voter dominates, so $x_d = 1/2$.[6]

The decision maker in each regime can only muster volunteer labor if he also volunteers (after all he must lead the volunteer group). He only does that if the total benefits to him of having the project equal or exceed his cost of volunteering, or $[N(1-s)]^{\delta}C_i x_j \geq b, j = e, d$. This defines a second implicit function defining the lowest valuation project announced by the village head:

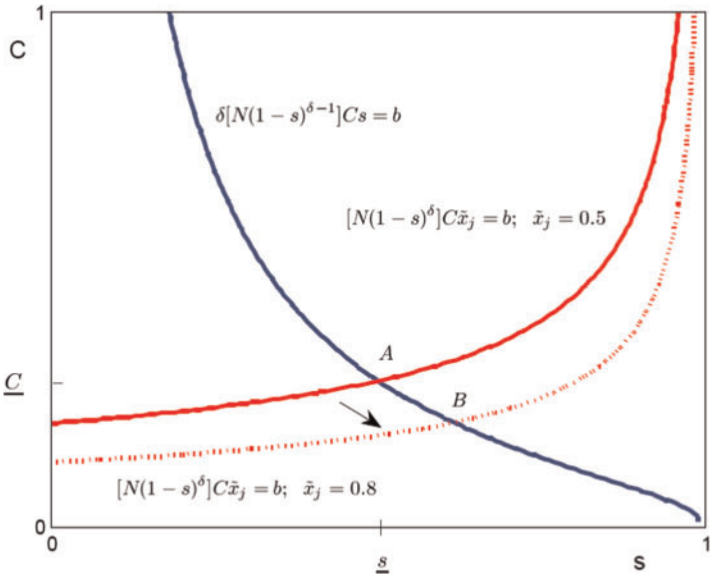
$$C = C(s, b/x_j; n), \frac{dC}{C} = \delta s(1-s)\frac{ds}{s}. \tag{3}$$

Note for this also that $dC/db|s > 0, dC/dx|s < 0$.

In figure 1 we graph an example of the general forces at work. The negatively sloped equation (1b) and positively sloped (3) are graphed in figure 1 in s,C space. The graph is drawn for specific parameters, but the results are general. The graph uses $C \in (0, 1)$, $\delta = 0.5, b = 1, N = 10$, and two values of $x, x_d = 0.5$ and $x_e = 0.8$. Equation (3) but not (1a) shifts out as x_j increases. The intersection of the two curves gives the minimum value of C, $\underline{C}j$, for the lowest quality project chosen, and the equilibrium associated $s, \underline{s}j$, for that project (which involves the lowest volunteerism among all projects chosen under regime j). Projects with C's above $\underline{C}j$ are chosen and have higher participation as given by the declining values of s as one moves up the downward sloping curve for equation (1b).

6 Either volunteer labor is the only election decision, or preferences on other election dimensions are ordered in the same fashion on the unit interval.

Figure 1. Equilibrium Relationship in the Model



The graph illustrates the general comparison of the elite versus democratic regime. Under the democratic regime, $x_d = 0.5$, $\underline{s}\,d = 0.5$ and $\underline{C}\,d = 0.283$. Shifting to an elite regime, $x_e = 0.8$, $\underline{s}\,j = 0.615$ and $\underline{C}\,j = 0.202$. Thus, under an elite regime, all projects that occur under a democratic regime are also undertaken (where the level of volunteerism is the same for each specific project across regimes). But more and lower value projects are undertaken in an elite regime, each with lower levels of volunteerism. Thus total projects and total volunteerism are higher in the elite regime. The extent of this shift, or increase in total projects, could be hindered if there is a minimum labor requirement, or $\bar{s}$, since lower value projects induce less volunteerism.

In terms of typical demand and supply factors typically associated with volunteerism, villages could differ in their demand for projects, based on the range and distribution of C's. For example, in our context, if more public buildings survive in a village, those are items where maintenance is required via volunteer labor, relative to a village receiving new facilities that have yet to age. If the village head's energy is diverted to the aid process (akin to Knack and Rahman 2007), that could reduce the efficiency (δ) with which volunteer labor is used. On the supply side, there should be differences in typical costs on volunteering in a village, b, apart from within village differences. Higher stocks of social capital could reduce perceived costs of volunteering or working with others in the village towards common goals. Akin to Svennson (2000) and Labonne and Chase (2008), more aid delivered in a village, although coming from outside aid agencies, could increase market opportunities in the local transport and food service sector, raising costs of volunteering, or divert villagers' energies in negotiating over the division of aid. Higher costs, b, for the village cause both curves in figure 1 to shift up, meaning that the minimum $\underline{C}\,j$ is increased (fewer projects under either regime). While the impact on $\underline{s}\,j$ is ambiguous, total volunteerism declines—fewer projects with less volunteerism for each.

3. The Determinants of Volunteerism across Villages

The key outcome in this study is volunteer days called in the last month, where the village head calls volunteer days up to twice a week during the month. In the village level surveys in 2007 and 2009, we ask whether volunteer days were called in the last month and, if so, how many. In 2007, we also asked how

Table 3. Public Labor: Volunteerism

	Pre-tsunami	2007	2009	Sample	t-stat. on differences	
Proportion with regular volunteer days	.97	.72	.83	190 villages	05–07	07–09
					−6.94	2.70
Avg. volunteer days per month (days in both years)	2.78	2.14		138 villages	−4.7	
		2.19	2.01	120 villages	−1.25	

many days typically were called pre-tsunami in a month. Table 3 gives some basic numbers. The number of villages regularly calling volunteer days declines from a pre-tsunami level of 97% to 72% in 2007, with some rebound to 83% in 2009. For villages that called for volunteer days in both pair-wise years, the average number of days called per month for those reporting also declines post-tsunami.

A concern is whether the village level measure of volunteerism, days called, reflects total volunteerism, given that participation in days called can vary. While volunteer days are typically half days starting in the morning, the actual hours can vary. Second, the fraction of adult males participating can vary. In 2009, we additionally asked the number of hours and number of volunteers in the *most recent* day called. In 2009, for 155 villages reporting numbers, the mean and median number of hours called were 3.0; and the mean number of volunteers was 146, in a sample where the number of households per village averages 217. To see the correlation between the number of days called per month and the numbers of hours and of volunteers in the most recent of those days, we estimated a Poisson regression of number of days called on the log of participants and the log of hours separately. This gives coefficients (s.e.) respectively of −0.031 (0.074) and 0.439** (0.135).[7] These correlations alleviate later measurement concerns, such as village heads calling more volunteer days if they have low participation. If there is a bias in using our measure, it goes the other way. Increased volunteer days may understate increased volunteerism in the sense that village heads wanting and able to call more volunteer days also call longer hours. Note that this is consistent with the model, where more projects means more volunteerism overall. We do not have data on numbers of different volunteer projects overall nor those worked on in any volunteer session.

(a) Formulation

We use a count formulation given the presence of zeroes; results are robust to other formulations (e.g., OLS or Tobit), as we will see. In a count formulation, total volunteer days V_{jt} in village j in time t are modelled as a function $V(X_{jt}\beta_t + \gamma_t E_{jt} + \epsilon_{jt})$, where X_{jt} are village controls to be discussed, E_{jt} is an indicator variable as to whether the village has had a formal election by time t, and ϵ_{jt} is the error term. The error term includes district-time fixed effects, for the five districts and two time periods (2007 and 2009), to capture cultural differentiation across the five districts and to allow for district-level shocks. Since we pool years, we will cluster errors by village to allow for correlation over time. The probability of observing V_{jt} volunteer days under the Poisson is $e^{-\lambda_{jt}}\lambda_{jt}^{V_{jt}}/(V_{jt}!)$, where λ_{jt} is the expected number of volunteer days called per month in village j in time t. The common form for λ_{jt}, which we adopt is

$$\lambda_{jt} = \exp[X_{jt}\beta_t + \gamma_t E_{jt}],$$

which is convenient when defining elasticities. We use Wooldridge robust errors (to the Poisson assumption) for these equations.

In estimation, our base sample is usually 187 of the 190 villages for which we have consistent data. We drop one village dominated by army housing, where reporting is not consistent on what population

7 A control for number of households in the village has no impact on the hours coefficient and for participants changes it to −0.015 (0.078).

Table 4. Basic election results

	Poisson count						
	(1)	(2)	(3)	(4)	(5)	Tobit (6)	OLS (7)
Election-to-date	−0.401***	−0.338**	−0.366**	−0.348**	−0.371**	−0.546*	−0.462**
	(0.140)	(0.151)	(0.155)	(0.156)	(0.153)	(0.289)	(0.215)
Election-to-date * year 2009	−0.049	−0.115	−0.091	−0.108	−0.064	−0.297	−0.281
	(0.171)	(0.178)	(0.186)	(0.189)	(0.187)	(0.349)	(0.289)
Ln(vol days/mon, pre-tsun.)				0.188			
				(0.147)			
Ln(vol days/mon, pre-tsun.) * year 2009				0.007			
				(0.190)			
Base covariates, Base covariates * year 2009	No	No	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	No	No	No	No	No
Aid controls and aid controls * year 2009[a]	No	No	No	No	Yes	No	No
District (5) * year fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Sigma						1.487***	
						(0.067)	
Number of observations	374	374	374	374	372	374	374
Adjusted R^2						0.077	0.185
Log-Likelihood	−599.78	−569.37	−556.10	−554.41	−550.2	−602.37	−604.73

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

[a]These are the count of RAND projects in the village and no. of houses destroyed by the tsunami. And each interacted with a dummy for the year of 2009.

is covered, and two villages with no pre-tsunami government data on population. After that we have missing values on certain survival rates.

(b) Basic Results on Elections

In this section, we start by treating the timing of elections as “exogenous,” being driven by differential subdistrict priorities and capacities, rather than village level unobservables affecting volunteerism. As such, election effects are regime-switch effects. Then we turn to evidence that supports this assumption. In the estimation, as noted, we control for a variety of village characteristics relating to size, diversity, survival and destruction from the tsunami, and village social capital, allowing for 2009 differential effects of each relative to 2007. We discuss the results on these characteristics in Section 3d.

The key variable we focus on in this section is whether the village has had an election to date. This information is taken from the 2009 interviews, where we were careful in questioning to distinguish officially sanctioned elections from informal ones, typically held in the first two years after the tsunami, a distinction we will utilize below. For 2009, an election-to-date is whether the village has had an officially sanctioned election from early 2006 up to the date of interview in late 2009 or early 2010. For 2007, an election-to-date means that the village, when interviewed in a specific month in late 2007 or early 2008, had an official election after 2005 but in or before that interview month in late 2007 or early 2008 (as reported in 2009).

Table 4 contains the basic results. Column 1 contains just election-to-date and its differential effect in 2009, with the only control being a time effect. Those having switched regimes by having an election have 40.1% fewer volunteer days. The differential effect of an election to data on 2009 volunteer days is negative (suggesting a stronger effect) but insignificant; and that pattern holds in all columns in the table. In column 2 we add in district-year fixed effects, which reduce the effect to a 33.8% reduction. Column

3 adds in a full set of eight village characteristics, each with 2009 differential effects, with a then 36.6% reduction in volunteerism. This is our preferred specification.

In columns 4–7 we do some robustness checks on the basic specification. In column 4, we add in a control for typical monthly pre-tsunami volunteer days as recollected by the villagers present at the interview. Obviously this measure has issues to do with both recollection inaccuracy and bias. But it is a control for pre-tsunami village tendencies, which could also be correlated with timing of elections, although, as we will see below, timing is uncorrelated with pre-tsunami volunteer days. The measure of pre-tsunami volunteer days has little impact on the election-to-date coefficient and itself is insignificant. Column 5 adds in measures of the need for and extent of aid: the number of houses destroyed by the tsunami and the count of RAN projects. Coefficients on these (not shown) are completely insignificant and inclusion of these variables has no impact of the election-to-date coefficient. We also experimented with dropping the few villages in which the month of surveying was an election month; that left results from column 3 unchanged (slightly strengthened). Column 6 estimates the specification in column 3 by Tobit with censoring at 0 volunteer days and the dependent variable being the log of volunteer days for those with days, which gives an election-to-date elasticity comparable to column 3. Tobit gives stronger effects. Column 7 estimates by OLS where the dependent variable is the count of volunteer days. There, an election-to-date lowers the number of volunteer days in 2007 by 0.462 from a mean of 1.51 (table A2), a 30.6% reduction, with stronger effects in 2009.

(c) Identification

We now turn to the key analysis of identification, beyond examining election timing in and of itself. Threats to causality include the idea that the type of village head changes because of the aid process (more educated), and effects reflect that, rather than a shift in regime per se. Or it could be that villages that had an election versus those that did not (by the end of 2009) are different and those differences drive the volunteerism differences. We now explore these issues looking at village head education, election timing and differential effects for elections at different times, and placebo elections. The experiments we carry out are reported in tables 5 and 6. We report one at a time.

Village Head Education

We start with the issue that elections generally bring in better-educated village heads, who might desire or only be able to muster fewer volunteer days. As a management style, they might want to produce public goods with fewer volunteer inputs per se. In table 5, a control in column 1 for whether the village head has a high school education or not has a zero effect in both years on volunteerism and no impact on the election magnitude compared to the base case in column 1. This suggests both that volunteerism is not different in villages with more educated heads and that village head education is not driving election results.

Election Timing

The next issue concerns election timing and the comparison between villages with and without an election. First, we show that village observables generally don't affect election timing. We show results for a proportional hazard model of the risk of having an election in year one (2006) through year five (2009), with either censoring or a 2010 date for all other villages (the strongly intended goal). For the hazard, we show the results for an exponential with all elections completed before 2011, but results on covariate coefficients for a censored or Weibull version are similar. We also estimate a Probit on having an election before the end of 2009 or not.

In the hazard and the Probit estimations, in table 6, in columns (1) and (3), only one of the 18 coefficients (an *arisan* group pre-tsunami in the Probit) for village characteristics significantly affects election timing. We will discuss arisan groups in section 3d. Columns 2 and 4 show that additionally there is no

Table 5. Robustness

	(1)	(2)	(3)	(4)
Election-to-date	−0.366**	−0.393**	−0.415***	−0.269*
	(0.158)	(0.173)	(0.160)	(0.146)
Village head high school plus	−0.0005			
	(0.138)			
Village election in 2006		0.156		
		(0.229)		
Village election in 2008 or 2009		0.062		
		(0.188)		
Informal election			−0.147	
			(0.137)	
Year 2009 X Election-to-date	−0.076	−0.172	0.015	0.128
	(0.191)	(0.215)	(0.200)	(0.179)
Year 2009 X Village head high school plus	−0.166			
	(0.180)			
Year 2009 X Village election in 2006		0.116		
		(0.286)		
Year 2009 X Village election in 2008 or 2009		0.120		
		(0.247)		
Year 2009 X Informal election			0.385**	
			(0.182)	
District (5) * year fixed effects	Yes	Yes	Yes	No
Village (190) fixed effects	No	No	No	Yes
Year fixed effects	No	No	No	Yes
Base controls	Yes	Yes	Yes	No
Base controls* year 2009	Yes	Yes	Yes	No
Number of observations	374	374	374	590
Log-Likelihood	−555.3	−554.8	−554.2	−481.3

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

partial correlation with pre-tsunami volunteer days. This is a pretty compelling case that village observables do not affect election timing. Table 2 also suggests reasonable balance in the data between villages that have an election before the end of 2009 versus not, and any imbalance seems to be offset by district fixed effects. These results indicate why we didn't pursue IV work, for example instrumenting for elections with death of the village head. Those deaths only resulted in early informal elections, and didn't affect the timing of formal elections. We struggled to find other predetermined conditions that had a significant influence and don't report the many experiments which bore no fruit, including distance to Banda Aceh, village located on the seashore or not, female versus male survival rate, and whether the dominant occupation is fishing or farming. The timing of village elections seems unrelated to inherent measured village attributes, suggesting a strong random component to election timing.

Although observables don't influence election timing, there must be village unobservables affecting timing, so that it is not entirely imposed from the outside. While reforms mandated local elections, in many villages formal elections were delayed, with elites remaining in control. Formal elections must be certified and approved by the subdistrict (*kecamatan*) government. Given the mandate to hold elections, what affected the timing of elections after early 2006? Interviews with local election official suggested many factors that are exogenous to specific villages: subdistrict priorities (in a time of massive aid delivery) and capacities to monitor and authorize elections. In our village survey, we asked the key cited reasons for what prompted a post-tsunami election, allowing for multiple factors, although mostly one

Table 6. Election Timing

	Proportional Hazard		Probit: Election by end of 2009 or not	
	(1)	(2)	(3)	(4)
Ln(no. of households post-tsunami)	0.073	0.073	0.317*	0.316*
	(0.078)	(0.077)	(0.164)	(0.164)
Survival rate of population	0.136	0.140	0.194	0.197
	(0.098)	(0.096)	(0.264)	(0.263)
Pre-tsunami *arisan* group	−0.207	−0.207	−0.566**	−0.565**
	(0.140)	(0.140)	(0.256)	(0.257)
Mullah survives tsunami	−0.144	−0.144	−0.378	−0.377
	(0.131)	(0.131)	(0.254)	(0.254)
Village head died from tsunami	0.115	0.111	0.228	0.226
	(0.143)	(0.143)	(0.304)	(0.303)
Diversity index	−0.126	−0.131	−0.485	−0.484
	(0.259)	(0.261)	(0.584)	(0.584)
No. of public bldg. destroyed	0.019	0.019	0.028	0.028
	(0.037)	(0.037)	(0.076)	(0.076)
No. of public bldg. survive	0.066	0.063	0.033	0.031
	(0.080)	(0.080)	(0.161)	(0.161)
Ln(vol. days/month pre-tsunami)		−0.041		−0.025
		(0.118)		(0.245)
District (5) fixed effects	Yes	Yes	Yes	Yes
Number of observations	187	187	187	187
Log-Likelihood	−862.7	−862.6	−86.6	−86.6

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

factor was cited. First are pressure from the subdistrict or district government (55 of 150 cases) and village head died in the tsunami (35 cases). We thought complaints from villagers about the aid delivery process would be important, but that appears to be incorrect. In only four (of 150) cases did villages report that aid disputes of any type prompted the election; and only six report that aid allocations issues were the main election issue. Another set of about 25 villages stated incompetence, forced to resign, and been head too long. There are also 61 villages, which say that the head (or any replacement) had simply completed his term, but this seems to be a more polite phrasing of "been head too long".[8] These last types of responses raise issues such as "been head too long" implies disgruntlement, which could affect volunteerism.

It should be clear we are not claiming election timing is exogenous to all village conditions. We instead argue that unobservables affecting volunteerism don't affect timing of elections, using three pieces of evidence. First, we show the election effect is the same regardless of election timing. While that does not prove the final set of villages (those with elections yet to come after 2009) are different than all other villages, it is strong evidence for regime switch effects. Whatever unobservables differentiate whether elections are held in the years from 2006 through 2009, they have no effect on election coefficients. Second, and related, we show that "placebo elections" have no effect. Third, we will introduce village fixed effects to control for all village conditions, but to do so we must take seriously the information on pre-tsunami volunteerism.

8 We have some information on dates of pre-tsunami elections, with a wave of about 65 elections in 2000–03 after national democratization in 1999. It seems that these were informal elections. Indeed, if they were formal with the proscribed time between elections, they should have had new elections in 2005–09. The overlap between this 65 and the 61 citing term completed is only 25.

For election timing, we can distinguish overall effects for 2007 and 2009 volunteerism from elections in 2006 (13%) and post 2007 (16%)[9] versus no elections by the end of surveying in 2009 (31%). The base case where elections are held in 2007 accounts for 39% of villages. Table 5, column 2 relative to the 2007 election base effect on volunteerism in 2007 shows differential effects of: 2007 elections on 2009 volunteerism, 2006 elections on volunteerism in 2007 and 2009, and 2008/09 elections on volunteerism in 2007 (a placebo effect) and in 2009. None of these differentials are significant, nor is the election-to-date coefficient noticeably changed. Any unobservables driving more precise election timing are not driving the effects of elections on volunteerism.

Placebo Elections

A negative regime-switch effect of elections should only occur for formally mandated elections. To explore this further we note that 24% of our villages called informal elections after the tsunami, usually because the village head was killed in the tsunami or quit office and was temporarily replaced by villagers in an informal election. An informal election is defined by a village that reports a post-tsunami election in 2007 but not in 2009 or reports an election in 2005 prior to election reforms. In the survey and instructions, elections reported in 2009 were supposed to be formal, sanctioned ones. In 2007 that distinction was not made. Again, our presumption is that these unmonitored elections were dominated by elites. We first examined balance in the data for villages with and without informal elections, looking for significant differences in the mean of village characteristics listed in table 7 for those with and without informal elections; no characteristics are significantly different across the two groups. These elections have no effect on volunteerism in either 2007 or 2009, as seen in column 3 of table 5 (note the net effect in 2009 is insignificant). We also ran a version where we narrow the comparison to whether these placebo elections had any effect on volunteerism in 2007 among just the sample of villages that had no formal elections before surveying in 2007. About 45% of these villages had informal elections. Again there are no effects; the informal elections coefficient is −0.076.

Village Fixed Effects

Finally we turn to village fixed effects in table 5 column 4. For elections, the issue is that villages yet to have elections by the end of 2007 or 2009 may have had a history of greater volunteerism pre-tsunami driven by unobservables, and that history could be driving results despite all the controls we use to try to capture village effects. Pre-tsunami versus 2009, villages with elections experienced a 44% drop in volunteer days, while those without only had a 31% drop. If we impose village fixed effects to try to control for the unobservables, what happens? Before answering that we note there are two issues with such a specification. First, imposing fixed effects assumes village characteristics have the same effects over time (in 2007 versus 2009, or pre-tsunami) on volunteerism. As table 7 shows on the following page, for village observables this is clearly not the case in our context, in which case fixed effects is arguably a misspecification. Second, to get identification, we must pool the three years of data on volunteerism (pre-tsunami, 2007, and 2009), using a Poisson model with year and village fixed effects. We need the three years because most elections occur before surveying in 2007, and we can't identify just off of elections that occur in 2008 and 2009. The issue in adding in the third year is that pre-tsunami days are based on recall about what was typical, which might be influenced by the response to current volunteerism. With these caveats in mind, in column 4 of table 5, the fixed effects estimator has an election effect of −0.27, a still strong effect, although smaller than those we found in different specifications in the same row.

9 These are elections actually held in 2008 or 2009. This differs from the higher fraction of villages whose first election was after surveying in 2007.

Table 7. Effects of Village Characteristics

	Base coefficient (1)	Year 2009 differential (2)
Ln(no. of households post-tsunami)	−0.258***	0.143
	(0.088)	(0.116)
Diversity index	−0.075	−0.457
	(0.313)	(0.455)
Pre-tsunami *arisan* group	0.299*	−0.212
	(0.174)	(0.216)
Mullah survives tsunami	0.356***	−0.413**
	(0.132)	(0.178)
Village head survived tsunami	−0.274*	0.150
	(0.150)	(0.183)
Survival rate of population	0.159*	−0.250*
	(0.092)	(0.132)
No. of public bldg. destroyed	−0.004	−0.020
	(0.043)	(0.056)
No. of public bldg. survive	0.016	0.081
	(0.095)	(0.128)

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

(d) Controls in the Basic Specification

The tables contain numerous controls that relate to destruction, aid, and social capital. Here we examine effects conditional on all other controls. We focus the discussion on our preferred specification, column 3 of table 4. Table 7 shows the village controls, base coefficients, and 2009 differentials for this specification. Some controls significantly affect volunteerism in 2007, but no net effects are significant in 2009, a key result. We treat the controls as at least pre-determined relative to late 2007 and some as completely exogenous, as we will note in each case.

Two standard controls in the literature are village size, or the number of households post-tsunami, and village diversity. Size could reduce volunteerism by aggravating free-rider problems and increased diversity could reduce cohesiveness and raise the psychic costs of volunteering (Alesina and LaFerrara 2000, Costa and Kahn, 2003). Village size post-tsunami is recorded in 2005 from the PODES, before households may later split to strategically garner more housing aid. Diversity is measured from our survey in 2007. Villagers are not diverse in ethnicity or race, but there is occupational diversity of households. We form the index $1 - \sum_{j=1}^{9} s_{ij}^2$, where s_{ij}^2 is the squared share of employment in occupation j across 9 occupations in village i.[10] In 2007 in table 7, increased size does reduce volunteerism consistent with free rider problems with an elasticity of −0.258 in 2007 in column 1, although the net effect is smaller and insignificant in 2009 (from combining columns 1 and 2). Diversity has the anticipated negative sign in both tables but is statistically weak.

We then turn to a set of variables that could be interpreted as measures of social capital, although they may have other channels of influence. Greater social capital should reduce the psychic costs of volunteering (Sobel 2002, Getler, Bellows, and Miguel, 2009). Foremost in our data is the pre-tsunami existence or not of a rotating saving and credit association (RoSCA), called *arisan* group. Such groups,

10 Occupations with their average shares are: fishing (28%), aquaculture (5%), agriculture (29%), trade (8%), transport (4%), construction (6%), public service (9%), other (6%), and unemployed (6%).

usually composed of women who meet regularly, with each member contributing a fixed sum to a pot and then taking the pot on a rotating schedule. *Arisan* groups are a volunteer association outside the mosque and governance structure. While the original work was on RoSCA's role in alleviating credit market imperfections (Besley, Coate, and Loury 1994), empirical work suggests a strong social component, with participation rising with wealth and complementing rather than substituting for credit institutions, at least in Indonesia (Varadharajan 2004). We view the existence of an *arisan* group as an indicator of a higher level of social capital and spirit of mutual assistance pre-tsunami. In 2005, male villagers repeatedly identified women as the social "glue" which facilitated village unity and purpose. 68% of our villages report the existence of an *arisan* group pre-tsunami.[11] Clearly the treatment effect here is not to say, if one randomly formed an *arisan* group pre-tsunami, that would give the effect we see. It is the pre-tsunami conditions in the village, which led to the formation of an *arisan* group, that we are trying to represent—the village social capital.

The control has a positive effect in 2007, as the literature suggests, with the pre-tsunami existence of an *arisan* group pre-tsunami increasing volunteer days by 30%. A surprise is that by 2009 in table 7, the *arisan* group effect is close to zero. This goes against the spirit of persistence of social capital effects found in the growth literature (e.g., Guiso Sapienza and Zingales 2008 and Tabelini 2008). But our context is unusual. Survivors in villages were disproportionately prime adult males, so there was widespread new family formation, investment in family life, and a restructuring of village female networks (Alesini and Giuliano 2009 and Ermisch and Gurr 2008). Villages transformed physically and socially, potentially weakening the value of pre-tsunami social capital.

There are additional controls, which could be interpreted as relating to social capital. They all have to do with destruction and would then be interpreted as measuring greater or less destruction of social capital. That idea is in contrast to the notion that destruction could also be a rallying point for villagers, so that more trauma leads to more volunteerism (Bellows and Miguel 2009 and Zylberberg 2010). First consider survival or not of the key village spiritual leader, the mullah. Survival of the mullah (65% in table A2) may play a key role in fostering a sense of purpose, community, and continuity, especially (from fieldwork) in the months immediately following the tsunami. Mullah survival could be interpreted as triggering the social capital reserves of the village, as opposed to the villagers rallying together in the face of the death of their mullah. Mullah survival has a strong positive effect in 2007, increasing volunteer days by 36%; but that effect is gone by 2009. This would be consistent with the idea that in the more immediate post-tsunami period the mullah was a force to draw villagers together to undertake social projects.

The other variables concern the survival of the village head, another potential leader, and survival of the overall population. Village head survival has seemingly oddly a negative, although a statistically weak effect, which is gone by 2009. The initial negative could have several explanations: traditional heads faced strong strains in terms of the coming democratization and were also often overwhelmed post-tsunami, feeling personal responsibility to get villages back on their feet. Village population survival hints at positive effects in 2007 consistent with the social capital story but not the rallying one in Bellows and Miguel (2009). However there is no effect in 2009.

Then there are controls relating to survival and destruction of public buildings that are generally maintained by the village: great and regular mosques, village halls, fishermen's halls, and Islamic schools. Survival, relative to obtaining a brand new building through aid, could create a demand for

11 We asked about other social capital indicators also dominated by women such as Quran recitation groups and PKK groups, but almost all villages report such activities both pre-and post tsunami, so there is effectively no village level variation. In addition, Quran recitation is a religious activity sponsored by the mosque, and PKK groups are sponsored by the national government offering "guidance for family and welfare" and have political-social overtones, not mutual assistance ones.

public labor. In table 7, we see no effects; nor, less surprisingly, does the destruction rate of housing have effects.

In table 4 column 5, as noted above, we experimented with adding a measure of the extent of aid projects in the village. For the latter, most aid in our villages is complete by late 2007, so we were asking mostly whether predetermined aid activity affects volunteerism, not whether the arrival of aid today has a contemporaneous effect on volunteer days. Our aid measure is the total number of aid projects in the village. The literature suggests that more projects would create higher opportunity costs of volunteering and greater incentives to spend time lobbying for aid (Svennson 2000 and Labonne and Chase 2008). Knack and Rahman (2007) argue that having more donors erodes local bureaucratic quality and capacity, which could reduce villagers' incentives to volunteer. Here we suggest that the village head may be distracted, and volunteer days may not be well planned or may be put off.[12] Regardless, there are no effects: coefficients (s.e.) on the count of RAN projects and the 2009 differential are respectively −0.0061 (.0050) and 0.0038 (0.0075).

4. Conclusions

The introduction of a new institution, elections for village head, is associated with reduced volunteerism in villages. Old village heads were long-term heads chosen by and from village elites. New heads may represent more the choices for the median voter. The reduction in volunteer days seems to occur largely because these different types of village heads chosen under different regimes choose different numbers of projects requiring volunteer labor.

Data Appendix

The Surveys

The village surveys in summer and fall 2005, fall 2007, and fall 2009 ask questions about education, experience, and survival of village and religious leaders; population composition by sex and age both before and after the tsunami; migration; occupational structure; destruction of village lands, seawalls, aquaculture areas, docking areas and mangroves; pre- and post-tsunami data on political, legal, and social institutions; pre and post tsunami information on physical capital (houses, boats, public buildings); detailed information on initial and ongoing operations of NGO's, local governments, and relief agencies providing housing, boats, public buildings, and restoration of the coast line; and detailed information on the village fishing industry pre- and post-tsunami, including questions on marketing, fishing fleet composition, catch composition and boat replacement. The 2005 survey of 111 villages focused on benchmarking destruction and village conditions. The 2007 and 2009 surveys of 199 villages (including the original 111) focused on aspects of the aid effort and institutional transformation of villages, such as the democratic evolution and quality of aid as related to different types of aid agencies.

Geographic Coverage

Overall, in terms of village coverage, we cover all villages in three contiguous districts (Banda Aceh, Aceh Jaya, and Aceh Besar) going south and north-east of the capital Banda Aceh, as well as off-shore islands of these districts. We also covered fishing villages in two other districts, up to a defined geographic limit moving east from Banda Aceh into Pidie (the last subdistrict surveyed is Meurah Dua) and moving south into Aceh Barat (the last subdistrict surveyed is Meuruebo).

12 A key issue in interpreting aid effects concerns whether villages with unobserved tendencies to volunteer days attract differential aid. Henderson and Lee (2014) show that the level of house aid seems uncorrelated with any observed measures of leadership survival, social capital, elections, or the overall RAN count of aid projects in a village. House aid, at least, seems driven by need and supply conditions (like access of NGO's to the village and extent of destruction).

Table A1. Timeline on Tsunami and Village Surveys

Event	Date	Notes
Tsunami	12/26/2004	
Field work trips	04, 05 and 06 2005	2 people for 15 days; and then 6 people for about 15 days
First survey	09/16/05–12/22/05	92 of 112 completed by 10/15/05. Rest were down the coast and very hard to reach (road destroyed). Surveying interrupted in October by Ramadan.
Field work	05 and 06 2007	6 people for about 2 weeks and then 3 people for about 2 weeks. [Also 1 person for about a month in late 2007.]
Second survey	03/08/07–12/22/2007	Surveying interrupted in September by Ramadan
Field work	08 2009	7 people for about 3 weeks
Third survey	30/09/09–04/02/2010	Surveying delayed to end of September by Ramadan. 186 completed by mid-February

Table A2. Stats on Village Characteristics and Balance in the Data (Estimating Sample of 187)

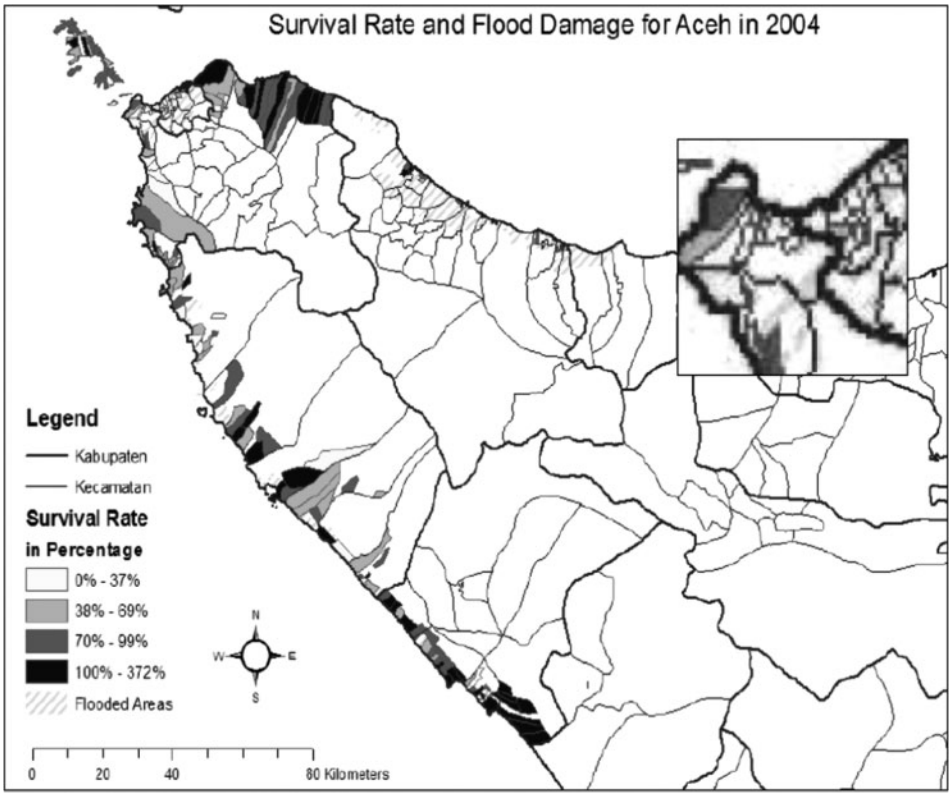
	Mean	Standard deviation	Min	Max	Difference in means: no election by 2009 minus rest. T-stats
Ln(no. of households post-tsunami)	4.85	0.78	3.04	7.73	0.90
Survival rate of population	0.75	0.48	0.11	3.72	2.13
Mullah survives tsunami	0.65	0.48	0	1	2.20
Village head survived tsunami	0.76	0.43	0	1	1.88
Arisan group	0.68	0.47	0	1	1.95
Diversity index	0.60	0.18	0.096	1	0.27
No. of public bldg. destroyed	3.65	1.76	0	9	−0.94
No. of public bldg. survive	0.58	0.75	0	4	1.22
Vol. days/month pre-tsunami	2.58	1.51	0	8	2.10
Volunteer days in 2007	1.52	1.46	0	4	n.a.
Volunteer days in 2009	1.56	1.35	0	5	n.a.
District (5) fixed effects					6.36, 2.14, −2.91, −0.24, −3.44

Figure A1 shows a map of the survey area, with a blow-up (right side in figure) of the Banda Aceh area (upper-left part of coastal area). The map shows household survival rates by village (yellow being the worst). Unfortunately, the map is based on the government rendering, post-tsunami, of village boundaries. In that dimension the map is grossly inaccurate. We took GPS readings of the center (the mosque) of the living area of each village. In only 6% of the cases is that GPS reading within the village boundaries. In 15% of the cases, it is over 10 kilometers away. Coastal villages are drawn as non-coastal and vice-versa which explains why, in parts of the map, a yellow (low survival) village may be shown next to a supposed coastal village, which is dark (high survival). Nevertheless the map pictures the general survey area.

The Survey Area

Sources of Population and Building Stock Numbers. To cover our villages consistently, for pre- and post-tsunami populations of villages, we use government counts. For the 88 villages added to our survey after 2005, 2007 recall of pre-tsunami populations tended to be strategic or unreliable (the village head reporting those numbers is usually new to office). Official population counts pre-tsunami are from the P4B, a 2004 government pre-election census. The official survival rate is the 2006 PODES count divided by the count in P4B. The PODES is a tri-annual government inventory of village populations and facilities. The 2006 PODES in Aceh was conducted in the Spring 2005. For eight villages, the PODES post-tsunami count grossly undercounts households, where for example the number of households in our 2009 survey is more than fivefold the PODES count. The ratios are 11, 7.5, 312, 23, 50, 21, 5.2 and 6.9. The absolute counts are just too low. In two of the eight cases where we have 2005 data, our survey counts versus the PODES are 212 versus 80 and 136 versus 20. Not surprisingly, we get slightly sharper size and survival results if we drop these observations, but they do not affect election results.

Figure A1. Map of Survey Area



For house and public building counts before and after the tsunami, we use our survey numbers (and there are not reliable government numbers); what was destroyed is well recorded by the remaining foundations, as well as village mapping exercises conducted soon after the tsunami. For survival rate of the mullah and village head, we use 2005 numbers with 2007 numbers for our added villages. Given that the deaths of prominent figures are known to all villagers, we are not so worried about recall here.

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