Volunteerism after the tsunami: democratization and aid[1]

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Abstract: Using three waves of survey data from fishing villages in Aceh, Indonesia for 2005-2009, we examine the determinants of local volunteer labor after the tsunami. Pre-existing social capital and the form of aid delivery strongly affect village volunteerism initially, but these effects weaken with time. What persists is the effect of essentially a new institution, formal village elections. While recent work suggests democratization increases cooperation, the differentially timed introduction of elections negatively affects volunteerism, suggesting a regime switch effect where traditional leaders chosen by elites want more volunteer labor projects than democratically elected leaders do. Of course not all villages elect non-elites, but vast majority that do have especially strong, persistent election effects.

Key words: volunteerism, democratization, elections, social capital, aid

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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, build and repair coastal barriers and irrigation and aquaculture channels, and repair or add to village buildings and public infrastructure. In Aceh Indonesia, such activities are done through volunteer labor, where village males assemble several times a month, in a response to a call by the village head for a volunteer day. Volunteer days are enshrined in the Quran and are Islamic volunteer days. They fall under the rubric in Indonesia of *gotong royong* (Bowen, 1986; Thorburn, 2008), which is the concept of mutual assistance promoted by political regimes dating back to the Sultanates to foster notions of moral obligation and generalized reciprocity.

This paper studies volunteer labor in coastal fishing villages in an unusual natural experiment: 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 199 villages we study. There was then a massive inflow of aid to these villages replacing lost physical capital within 2-3 years, but introducing enormous outside influences. However, the relief effort and international scrutiny prompted settlement of the civil conflict in Aceh, the key event for us. That resulted in the first mandated formal local elections in most villages in at least 2 decades. We study how volunteerism is affected by this introduction of formal elections, focusing on late 2007 towards the end of the aid process and in late 2009 after the process is over.

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.[2] In experimental work, Dalbo, 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. However, our experiment is different, with imperfect controls but an experiment that can't be carried out in the lab or field. 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, as well as traditional

[2] 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.

relationships among villagers. Democratization introduced formal elections, with electoral competition and universal suffrage, whereby almost all former village heads are replaced. Elections brought a strong reduction in volunteerism. While there are completing explanations for this correlation, we will argue that volunteerism declines because a democratically elected leader, relative to an elite leader, prefers and chooses fewer projects that involve volunteer labor. The outcome is consistent with the new work of Martinez-Bravo, Padró-i-Miquel, Qian and Yaoo (2011), which finds that, in China after local election reforms, locally elected leaders are more responsive to the wants of villagers than leaders imposed from above.

In our investigation, the differentially timed introduction of elections is significantly correlated with lower volunteerism compared to villages still without elections; and that effect persists.[3] Causality of regime-switch election effects will be argued, recognizing the imperfections of the experiment and relying on the weight of evidence. We have extensive controls in estimation, including pre-tsunami volunteerism. Formal elections will be spread over at least 6 years, driven in part by differential timing of when (sub-) district governments push villages in their domain to have elections. While election timing is insignificantly related to all village observable characteristics except (somewhat weakly) death of the village head in tsunami, a concern in identification must be the possibility that the village unobservables which drive election timing also directly affect volunteerism. Causality will be argued on two main bases. First, elections held in different years have similar effects on volunteerism. When we isolate effects for elected heads who do not come from the elite class, election effects are almost identical in all years, indicating that unobservables affecting election timing do not affect election impacts on volunteerism. Second, we 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.

The introduction of this new institution is in the midst of massive aid following massive destruction, a context that allows us to study also the effect on village level volunteerism of other key considerations discussed in the literature (e.g., Bloch, Genicot, and Ray 2007, Kosfeld, Okada, and Riedl, 2009, Alesina and La Ferrara 2000). After typical controls for village size,

[3] 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).

market opportunities and diversity (Alesina and La Ferrara, 2000; Costa and Kahn 2003), we focus on village pre-existing social capital and the introduction and the method of aid delivery. Villages have different pre-tsunami social capital stocks and have different post-tsunami survival rates affecting survival of social networks. In 2007, we find that such considerations affect the calling of volunteer days (Sobel 2002, Getler, Levine and Miguel, 2006), conditional or not on pre-tsunami village levels of volunteerism.

Village level volunteerism is affected also by the aid process and the extent of influx of foreign influences and market opportunities. Svennson (2000) and Labonne and Chase (2008) argue that increased external aid to a village can lead to a decline in volunteer labor, because people spend more time lobbying for private aid for themselves as opposed to volunteering. Knack and Rahman (2007) further argue that having more donors may erode local bureaucratic quality and capacity, which could reduce villagers' incentives to invest in public goods. Our first order results are consistent with this literature; but a more detailed examination reveals that volunteerism is strongly affected by the type of aid agencies operating in villages, related to the way in which they deliver aid and their investment in working with villagers.

For both surviving social capital and aid influx effects, a critical question is how long these influences last. While they are strong in 2007, unlike election effects, they weaken considerably by 2009. For social capital at least this is not what we expected from the literature (e.g., Guiso Sapienza and Zingales 2008 and Tabelini, 2008). Why might there be diminution of social capital effects? The aid process involved an intrusion in villages by NGO's from outside Aceh and the introduction of widespread new economic opportunities. In addition, in a context where survivors were disproportionately prime adult males, there was widespread new family formation and investment in family life (Alesini and Giuliano 2009 and Ermisch and Gurr 2008). Villages transform physically and socially in a couple of years, potentially weakening the value of pre-tsunami social capital.

The discussion so far has focused on a macro outcome, village level volunteer days, where the key outcome variable is the number of days called by the village head in the month prior to being surveyed. There is an issue about individual participation in those days, which we address directly both theoretically and empirically; but it seems most adult males do participate. In our sample where 25% of villages had no volunteer days in the prior month, participation on days called is high. 75% of our households in 2009 participated at least once in the month prior

to the household survey (which is done separately and often in a different month from the village survey).

Of course there is still variation across households in volunteerism; and in the last part of the paper we briefly explore how household circumstances affect participation at the micro level. For a long time, economics viewed individual volunteerism as "work for nothing" to be done under social pressure (Freedman, 1997) or something that should approach zero as a Nash outcome in a voluntary public labor input game. Recent modeling by Benabou and Tirole (2006) with an adaptation by Carpenter and Myers (2010) and experimental work by Linardi and McConnell (2010) take a more sophisticated view, where altruism plays a central role. Beyond usual opportunity cost considerations (Costa and Kahn, 2003), we explore whether volunteerism is related to personal material incentives in particular social contexts, where social image may play a role (Carpenter and Myers, 2010). We examine whether excuses for not volunteering are utilized differentially in contexts where validity of excuses varies (Benabou and Tirole, 2006 and Linardi and McConnell, 2010). Related, we look at the extent to which volunteerism is related to specific social obligations.

We start by describing the unusual context. Then we present a model of regime-switch effects, discuss the estimating framework, and analyze results at the village and individual level.

1. The Context

We look at the extent of destruction from the tsunami, aid delivery and its form, and aspects of democratization.

1.1 Destruction and aid

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. We also surveyed about 440 fishing families in 2005 in 77 villages (about 30% of surviving boat owners in these villages) and added more fishermen for 2007 and 2009 extending village coverage. In 2007 we have about 700 families in 96 villages and in 2009 after some sorting, attrition, and use of a longer survey we dropped the coverage to about 630 fishing families in 90 villages. All this is discussed in the data Appendix.

For the 199 villages, the intent was to cover the universe of fishing villages, defined as all villages with a significant fishing presence pre-tsunami as certified by the provincial fisherman's association, *Panglima Laot*, as we moved some distance south and north-east of the capital Banda Aceh.[4] The data now cover villages in 31 sub-districts in 5 districts, all affected by the tsunami. In fact, there are no unaffected fishing villages within the local cultural area but a few had only loss of boats and little loss of housing or population. Our villages account for about 30% of all house aid in Aceh, with Banda Aceh accounting for a significant portion of the rest.

The tsunami devastated coastal cities and villages. Table 1 presents an overview of destruction in 190 of our villages with complete information, using different data sources.[5] Destruction is massive. In 104 villages around Banda Aceh surveyed in 2005, under 50% of the population survived; in the expanded set more survived as added villages experienced a weaker wave force. 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 was 9% and of public building was 6%, noting that many public buildings such as mosques and fisherman halls are on the waterfront. For boats based on '05 survey numbers, the survival rate of boats was under 6%.

The immediacy and extent of aid is impressive. By late 2007 in Table 1, overall in our villages, 117% of "needed" houses had been replaced. Need is the number of surviving households less the number of houses that survived. Similarly, for boats the ratio of boats in the water in 2007 to surviving captains recorded in 2005 is 105%. Finally 80% of destroyed public buildings had been replaced by late 2007, a good replacement rate given loss of village populations. By late 2007, the aid process had accomplished what it intended—to replace the entire per household physical capital stock. Yet given the massive international response, there was still money to spend with the official aid process ending April 16, 2009. More public

[4] 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 the 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 sub-district surveyed is Meurah Dua) and moving south into Aceh Barat (the last sub-district surveyed is Meuruebo).

[5] As explained in the Appendix, to cover our villages consistently, for pre and post tsunami populations of villages, we use government counts. 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 boats, there is no record of pre-tsunami boats nor physical evidence of what was destroyed. By 2007 villages tend to heavily exaggerate boat losses. We report numbers for villages we surveyed in 2005, where we got reports not just on boats but captains and owners.

buildings trickled in after 2007 for an eventual 96% replacement rate. While boat aid was mostly over by late 2007, house aid continued with an eventual replacement rate of 145%. Alternatively viewed, overall in Aceh, aid gave 134,000 houses for 120,000 houses destroyed (Xinhua News Service, February 1, 2009), despite the reduction in population. The extra houses were claimed in part by traditional families strategically splintering into their own nuclear residences.

1.2. NGO presence in villages

While there is a large literature on the best ways to deliver aid (Collier et al. 1997, Azam and Laffont 2003, Svennson 2003, and Murrell 2002, Pederson 2001 and Torsvik 2005, Kanbur and Sandler 1999, Easterly 2003, and Paul 2006), the aid process in Aceh was mostly unconditional and largely uncoordinated. Lack of conditionality even at the village level was driven by the huge number of NGO's "competing" to deliver aid, in a context with little co-ordination. 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 recommending, if asked, where an NGO might focus aid and (2) late in the process filling in ex post gaps in aid.

We will see that the aid process affected volunteerism in the short run. We focus on two dimensions which we discuss now: the number of aid projects in a village and the type of agency delivering housing. The number of projects represents a degree of outside intervention in the village that absorbs villager and village head time, which may affect volunteerism. We use the RAN [Recovery Aceh-Nias] database [http://rand.brr.go.id/RAND/reference], a government database set up and maintained now by the UN, to count projects. These numbers indicate that there were 11 different "first level implementers", or different aid agencies actually delivering aid on the ground per village. Implementations are officially recorded distinct aid projects in a village, each negotiated separately; and cover a whole range of hard and soft aid, including for hard aid boats, environmental work, and various types of public buildings and mosques. The mean and median of projects are both about 30 at the end of 2007. By late 2009 that number had increased slightly to 32.[6] This is our measure of the degree of outside intervention.

Form our survey, in Table 2, in over half of the villages, 85% or more of house aid is provided by one agency; and in almost all villages one agency provides the majority of house

[6] We rely on our numbers for actual aid delivered, in part because aid projects as listed in RAN can cover implementations in a number of villages (with still separate negotiations in each village) and it is difficult to disentangle what village gets what; as well there are a number of recording errors in RAN.

aid. Housing as opposed to delivery of aid boats (built in centralized workshops) or building of one particular facility such as a health clinic or a small village cooperative requires the main house agency to be present and highly intrusive for a sustained period of time, as the new spatial layout of the village is planned, houses are built, utilities are hooked up, and families are moved in from camps. We use RAN to classify the main implementer housing agency operating on the ground in each village focus here on one type: donor-implementers.

A donor-implementer is an implementer which is also the project donor.[7] In Table 2, in 97 villages, the largest housing provider reported in our survey is a "donor-implementer". Being a donor-implementer helps solve the donor's agency problem of monitoring and motivating an implementer. Donor-implementers typically have on the ground operations with central offices in Banda Aceh, and large teams of trained people in the field. Fieldwork suggests that such agencies are more likely to seek village "ownership" of the aid process (e.g., Kanbur and Sandler 1999, Easterly 2003, and Paul 2006).

Other types of agencies are implementers, who may be an international NGO, a local NGO, or BRR.[8] With implementers, there is a "disconnect", where donors typically have little or no on-the-ground capacity to monitor implementation at key stages of the project. In a related study, Henderson and Lee (2011) find that housing construction quality is significantly lower for non-donor-implementers. Here we look at the effect on village volunteerism "by luck of the draw" of getting a donor-implementer. Since donor-implementers provide higher quality aid, villagers spend less time trying to monitor aid delivery and dealing with claims associated with bad aid, enhancing the prospects for volunteerism. We note that BRR, the government reconstruction agency which built housing using multi-donor funds (typically monies from foreign governments), was defined to be short-lived, used a top-down approach and was plagued with accusations of corruption. As a temporary agency BRR operates with none of the incentives facing typical NGO donor-implementers. And even implementers who are not donors may feel that their behavior will impact national or international reputations they wish to uphold.

1.3 Democratization

[7] A donor-implementer is a donor agency that, in at least 30% of the villages where it provides housing in our villages according to RAN is also acting as the (first level) implementer in the project, either directly hiring the labor to be used in construction, or else monitoring any sub-contractors.[7] Although we drew a 30% cutoff, almost all agencies we classify as donor-implementers are always both donor and implementer.

[8] While the village head may sometimes name a donor as the agency, we know from RAN if that donor, ever actually does implementation. If not, then it is not a donor-implementer.

For 20 years prior to the tsunami, Aceh had an insurgency movement, the Free Aceh Movement [GAM], with some degree of support from the Acehnese population. 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 sub-district (*kecematan*) government, in close consultation with the village council of elders (*tuhapuet*), who represent the elites in the village. The few elections that occurred were irregular in timing and were informal, usually with lack of contestation, secret ballots, and/or full voting rights; they rubber-stamped the selection of the head chosen by elites. An election for example might involve a show of hands by males in a post-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[9], after settlement of the insurgency.

Table 3 shows that elections phased in over time. After two years (by the end of 2007), less than 60% of villages had had elections; and two years after that almost 25% still had no election. We were told in late 2009 that all remaining villages were slated for elections in 2010. This differential timing will be analyzed later. Elections are not kind to old leaders. At the end of 2009, under 16% of old village heads remained in office and under 23% of those (i.e., 5) had survived an election. We believe this means formal elections shift power from a narrow set of village elites to the more general population; and the nature of village heads change. As the bottom rows of the table show new village heads differ from pre-tsunami ones. New village heads are much better educated, or more likely to have completed high school, than old.[10]

Table 4 gives a somewhat different perspective, the characteristics in 2009 of village heads elected post-tsunami versus those not elected post-tsunami, for the 190 villages later in our estimating sample. While some differences are modest, elected village heads are better educated and correspondingly younger, and less likely to have served previously in village governance

[9] Reforms attempted to democratize the *tuhapuet*; but also left the *tuhapeut* with considerable power. *Tuhapeuts* certify the village head to the sub-district government and will be responsible for organizing future elections.. Council members are supposed to be elected, only serve two 6 year consecutive terms, and have some female members. However, typically, such *tuhapeut* elections to date involve a post-prayer meeting at the mosque with a show of hands. A show of hands means elites are likely to dominate, where non-elites may feel constrained in revealing preferences, because of the informal debt relationships in the village, as well as simply status.

[10] In fact some districts after 2005 in principle required village heads to have a high school education. However the *tuhapuet* can certify 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 is not binding, and villagers are opting for more educated heads.

positions (served on the *tuhapeut*, as village secretary, as leader of the local fishermen's association or on the council of fish captains). Having previously served in village government later will identify current village heads who are more likely to still represent or be influenced by elite interests in the village.

2. Conceptualizing volunteer labor

The effects of social capital and aid interventions on public labor have been modeled variously elsewhere in the literature. We explored several approaches, including one based on Svennson's (2000) modeling of volunteerism in the face of people's alternative use of time which is to lobby for more aid.[11] We also looked at elections being prompted by aid disgruntlement,[12] although as we see will see later that does not seem to be an important factor. But in the end to simplify, we focused in modeling on explaining the new dimension: the election effect. The role of social capital and aid delivery will be summarized by allowing heterogeneity in costs of volunteering across villages.

In formulating the approach, we expect the type of village head to differ between a traditional regime and an election context. In a traditional regime where the village council, or *tuhapuet*, 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 sub-district 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 head may have very different valuations of projects utilizing volunteer labor, and we construct a model of an admittedly, polar case. There is also the issue of who responds to a call for a

[11] In one regime, villagers lobby a traditional village head who potentially holds the position indefinitely. In the second regime with elections, there are at least two potential candidates to lobby — either during an election time or after an election in anticipation of the next regularly scheduled election. It is easy to construct examples where having two or more people to lobby in a democratic environment relative to an indefinite village head increases total time people spend lobbying and hence reduces the time they spend volunteering.

[12] We did work through one model where election timing and ex post (as in our data) election disgruntlement could be related. In that model, results depend on the assumption that increasing disgruntlement increases the chances of an early election. In order to minimize the chance of complaints and an election being called early during the intense aid distribution years (2006 and 2007), elites may retain control by maintaining reasonable equality in aid distribution. However if disgruntlement leads to an election, elites are replaced by non-elites who have no incentive to maintain reasonable equality in aid distribution, steering aid away from elites. Disgruntled elites then have a high cost to volunteerism which could lead to a decline in volunteerism in the village, associated with an election. We don't have the village level data on equality of within villages of aid distribution to investigate the notion.

volunteer project. To capture elements of both, we experimented with a very simple adaptation of Foster and Rosenzweig (2004).

Suppose villagers are heterogeneous in their preferences for any public goods requiring public labor. Villagers are ordered uniformly on the unit interval is terms of the value, x, they place on any public service produced with volunteer labor. There is a discrete set of possible public projects where the base valuation of project i is $C_i \geq 0$ and projects differ in their C_i 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 two 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. For now we assume b is the same for all villagers, noting that, if, say, b increases with x but at a slower rate, our comparisons of equilibria under the two regimes will not be qualitatively affected. 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

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

This defines an implicit function

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

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

$$\underline{L} = (1-\overline{s})N. \qquad (2)$$

If this is imposed, $\overline{s}$ 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 construction of village public facilities and areas including docking facilities and aquaculture channel construction, which might have more appeal to elites who intensively use such facilities. We assume 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 $x_d = 1/2$ [13].

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 $[N(1-s)]^{\delta} C_i x_j \geq b, \quad j = e,d$. This defines a second implicit function defining the lowest valuation project announced by the village head:

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

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

The negatively sloped equation (1b) and positively sloped (3) are graphed in Figure 1 in s, C space, for $C \in (0,1)$, $\delta = 0.5$, $b = 1$, $N = 10$, 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 all have higher participation as given by the declining values of s as one moves up the downward sloping curve for equation (1b) .

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

Under the democratic regime where $x_d = 0.5$, $\underline{s}_d = 0.500$, $\underline{C}_d = 0.283$. Shifting to an elite regime where $x_e = 0.8$, $\underline{s}_j = 0.615$, $\underline{C}_j = .202$. 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). However more and lower value projects are undertaken in an elite regime, each with lower levels of volunteerism. However total projects and total volunteerism are obviously higher in the elite regime. This extent of this shift, or increase in total projects could be hindered if there is a minimum labor requirement, or $\overline{s}$, since lower value projects induce less volunteerism.

There are two additional considerations that we note. First once out of office, elites' ability to monopolize village facilities maintained by public labor may decline somewhat and their valuations may weaken. But it seems unlikely that the average villager's valuations will shift up significantly: educational, water, and health facilities are not provided through volunteer labor. While participation in use of facilities related to volunteer labor by non-elites may increase, in general we expect elites to still hang out in village facilities and dominate use of docking and aquaculture channels.

Second, we think that the cost of volunteer labor varies across villages, given outside market opportunities arising from aid delivery, the psychic costs of volunteering that might be related to village social capital, and psychic costs related to disgruntlement over aid delivery. Higher costs, b 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 village 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 many days typically were called pre-tsunami in a month. Table 5 gives some basic numbers, first looking at village level data and then individual. Comparing the answer about pre-tsunami days and current days in 2007, the number of villages regularly calling volunteer days declines dramatically post-tsunami in 2007, as does the average

number of days called per month for those reporting having volunteer days in both time periods. Between 2007 and 2009, there is resurgence in the number of villages calling volunteer days, although villages calling days in both 2007 and 2009 do not have an increase in average number of days. Since the 2004 baseline has obvious measurement issues, we focus on cross-village variation in volunteerism post-tsunami. Later we look at individual participation, where from the lower part of Table 5, there is a sharp rise in volunteerism between 2007 and 2009. The 2009 proportion of families volunteering in the prior month is 73%, up from 50% in 2007. The average number of family member-days rises from .74 to 1.5, almost a doubling.

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, 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 are 3.0; and the mean number of volunteers is 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 -.031 (.074) and .439** (.135).[14] These correlations alleviate later measurement issues 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 this is not inconsistent with the model. We do not have data on numbers of different projects overall nor those worked on in any volunteer session. In the model, more projects mean more overall volunteerism. Here it seems our measure of increased volunteerism does not overstate true increases in volunteerism.

3.1 Formulation

[14] A control for number of household in the village has no impact on the hours coefficient and for participants changes it to -.015 (.078).

We use a count formulation; results are robust to other formulations (e.g. OLS, Tobit, Probit on volunteer days or not). In a count formulation, the expected number of volunteer days called per month in village *j* in time *t*, λ_{jt} , is

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

a form convenient for defining elasticities. X_{jt} are village characteristics including measures of village size, social capital, aid delivery, and elections. In the equation a key assumption validated by the data is that coefficients change over time—what is relevant to determining volunteerism in 2007 in the turmoil of aid, reform, and new family formation will differ than what is relevant 2 years later, after the aid process is over. We estimate separately by year.

3.1.1 Sample

To use consistent data across all villages on count of households and population, we use pre and post tsunami government data (Table 1). For 8 villages, the PODES post-tsunami count grossly undercounts households, meaning the number of households in 2009 is more than fivefold the PODES count.[15] A ninth village is dominated by army housing and reporting is not consistent on what population is covered. There are also 2 villages with no pre-tsunami government data on population. That brings us to 188 of 199 villages. After that depending on the specification we may have 1-2 missing values for variables such as recorded aid projects and distance to Banda Aceh where we are still missing GPS readings for the village center for 2 villages. We discuss results when we add back in the 8 villages with poor PODES information.

3.1.2 Non-Election Covariates

While our focus will be on elections, there are a number of other covariates affecting volunteerism. We present and discuss those first, so as to proceed to the larger agenda. Two basic controls are the number of households post-tsunami and the population survival rate as measures of village size and of trauma and disturbance of social relations. Village size post-tsunami is recorded in 2005, before households may later split to strategically garner more housing aid. We also control for pre-tsunami social capital and at times for the degree and form of aid intervention. Above, we had a detailed discussion about aid delivery based on Table 3, but not of social capital measures.

[15] The ratios are 11, 7.5, 312, 23, 50, 21, 5.2 and 6.9. The absolute counts are just too low, with one exception all in the range 2-20. In 2 of the 8 cases where we have 2005 data, our survey counts versus the PODES are 212 vs. 80 and 136 vs. 20.

For differential village "social capital", we utilize the pre-tsunami existence or not of a rotating saving and credit association (RoSCA), called *arisan* group. Such groups, usually composed of women meet regularly, with each member contributing a fixed sum to a pot and then taking the pot on a rotating schedule. In Indonesia, a village having an *arisan* group indicates the existence of a volunteer association outside the mosque and governance structure. While the original theoretical 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 in a village 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. Table 6 shows 136 of 199 villages report the existence of an *arisan* group pre-tsunami. The number falls post-tsunami, but there is persistence: 97 villages out of 136 who had *arisan* group pre-tsunami report one in 2009. There is also change: between 2007 and 2009, 33 of 123 villages report losing their *arisan* group, while another 34 form one. Given the rapid formation of families post tsunami with wives from outside sub-district (given survival rates of women were lower than men), village level changes are not surprising

We asked about other social capital activities 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.

The second social capital related variable is survival or not (about 2/3) of the key village spiritual leader, the mullah. While the volunteer days are called by the village head, they are "Islamic" days; and survival of the mullah may play a key role in fostering a sense of purpose, community and continuity, especially (from fieldwork) in the months immediately following the tsunami. We originally thought survival of village heads would also be important. But given the rapid political changes, their survival is simply not directly relevant to volunteerism.

We treat these base covariates of volunteer days as pre-determined. Survival rates and post-tsunami village size variables seem plausibly exogenous. However having had an *arisan* or

any other group/institution pre-tsunami is a proxy for pre-existing village social capital, which would be correlated with other unobserved measures of persistent social capital which might influence village cohesion and the inclination and ability of the village head post-tsunami to call and utilize volunteer days. 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. Of course those conditions could affect volunteer days pre-tsunami and persist in affecting volunteer days post-tsunami. To deal with this, we show specifications where we control for recollected volunteer days called pre-tsunami.

In terms of aid measures, most aid in our villages is complete by the time of our surveys, so we are asking whether pre-determined aid activity affects volunteerism, not whether the arrival of aid today has a contemporaneous effect on volunteer days. A key concern is whether villages with, say, better unobserved tendencies to volunteer days attract more aid. Henderson and Lee (2011) 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. Most house aid seems driven by observables connected with need and supply conditions (like access of NGO's to the village and extent of destruction). However Henderson and Lee show that village social capital (but not elections) affects the form of aid delivery, making it more likely that villages with an *arisan* group pre-tsunami got a donor-implementer for housing, which results in better quality housing. That is, a "nicer" village doesn't get more private aid, but it may get better quality aid. Thus there are direct (after controlling for aid conditions) and indirect (not controlling for aid conditions) of *arisan* group.

We start with a basic formulation that deals with the role of social capital and effects of aid received, and then turn to main results on the impact of elections.

3.2 Basic Results: social capital and aid

Results from a Poisson count model with Wooldridge robust errors are in Table 7. In Table 7, results are divided by sets of columns into those for 2007 and those for 2009. The post-tsunami count of households hint at either a negative effect of size on volunteerism (free-riding) or scale effects in returns to volunteer labor in larger villages: proportionately less is needed to maintain

the same level of services. If we add in the 8 villages with low PODES household counts, this negative effect is strengthened in all columns.[16]

3.2.1 Social Capital.

Results on social capital for 2007 before the introduction of aid covariates are in column 1 of Table 7. Whether the village had an *arisan* group or not has a strong positive effect, raising the number of days called by 60%, before controls on aid are added on. Second for social capital, higher population survival rates mean more social structures in a village remained intact and the village suffered less trauma. A one standard deviation increase (.41) in the survival rate raises volunteer days by 29% in column 1. Beyond column 1 while the coefficient remains positive, the variable is insignificant. Regardless, the idea that more trauma leads to more volunteerism (Bellows and Miguel, 2009) does not hold here. Finally the survival of the mullah raises volunteer days in 2007 by 35%.

What happens to these effects when (1) we add in more covariates and (2) we move to 2009? In column 3 relative to column 1 for 2007 adding in NGO variables reduces the effect of *arisan* group by 22%, although it is still strongly significant. Adding NGO variables removes the indirect effect of *arisan* on what type of aid agency a village gets, where better aid agencies are associated with both more volunteerism and existence of *arisan* groups as noted above. The mullah effect is not weakened for 2007 from adding in more covariates. Both the mullah and *arisan* group effects are minimally reduced by adding the control in column 5 for volunteer days called pre-tsunami. A pre-tsunami tradition of calling more volunteer days which controls for persistent unobservables has positive effects in 2007 as expected.

In 2009, the perhaps startling results are that the social capital effect of mullah survival no longer holds, the pre-tsunami *arisan* group effect declines by 50% although it remains significant, and the effect of pre-tsunami traditions of calling volunteer days is also reduced by about 50%. These declines in the effects of pre-tsunami social capital and village life variables in the context of the massive shake-up of villages may not seem surprising, but goes against the spirit of incredible persistence found in the growth literature. It suggests here that these conditions mattered in the earlier traumatic days of village recovery and infusion of aid agencies. But in the new order in village life their effects are much weakened.

[16] To illustrate for the column 5 and 6 specifications with 8 villages with bad PODES data added in have for household counts and survival rates coefficients for 2007 are -.175 (.099) and .230 (.113) and for 2009 are -.135 (.065) and -.026 (.114).

3.2.2 Aid

As noted above, we have two dimensions concerning aid. For the official count of different aid projects contracted in a village, we use 2007 counts for both 2007 and 2009, since almost all projects are started before the end of 2007. Results for 2007 are in columns 3 and 5. There, having more projects has significant negative effects on volunteerism. There are several interpretations including Svennson's (2000) time spent lobbying for the aid. But we think more projects reflect opportunity cost effects, where more projects mean more temporary employment opportunities, drawing away from volunteerism. We explore that possibility in the next table. WE note more aid projects may mean less need for volunteer labor with less need for maintenance of new public buildings. On the other hand, more village clean-up (the most important use of volunteer labor) may be required with more projects.

The negative effect of having more aid projects on volunteerism is mostly offset for a typical count of projects in a village, if the housing provider is a donor-implementer. A donor-implementer may free up time and energy for volunteerism, by reducing time spent monitoring, given mitigation of the agency problem between donors, implementers and contractors. They may be also more cognizant of village conditions, work to minimize squabbling over allocations, and impose sharper limits (given sharper monitoring) on realized inequality. However for BRR effects are negative, relative to the base (non-donor implementers). BRR has a reputation for corruption, provision of lower quality housing, and no monitoring.

Again, while these effects hold in 2007, in 2009 well after the end of the relief effort, there is no effect of aid conditions on volunteerism.[17] The effects are short-lived.

3.2.3 Other considerations

In column 1, Table 8 for 2007, we see that besides our aid variables, distance to Banda Aceh and whether the NGO housing provider employs local labor (15% of villages) matter. Greater distance from Banda Aceh which was the NGO operations center reduces the value of potential job opportunities in the aid sector, with an elasticity of .28 for increased volunteerism with distance. Using local paid labor in housing aid also reduces volunteerism but the effect is statistically weak. Given aid is well over by 2009, there are no effects in 2009 (not shown).

[17] By relief effort we mean hard aid. There is some continuing soft aid (especially through the Australian government), but it seems to have little presence in our villages.

Next we look for within village diversity effects on volunteerism (Alesina and LaFerrara 2000, Costa and Kahn, 2003). Villagers are not diverse in ethnicity or race, but there is occupational diversity of households. 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%). For diversity 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* in village *i*. As we see in columns 2 and 3 in Table 8, diversity has the expected negative effect but it is at best weakly significant. We also show in these two columns a control for elections, which are discussed next. We include the controls now to show that election effects are not tied with diversity effects.

3.3 Elections

We now turn to our main results and the analysis of elections. We start by treating the timing of elections in the specification as "exogenous", being driven by differential sub-district priorities and capacities, rather than village level unobservables affecting volunteerism. In that case, election effects would be regime-switch effects. In Table 9 in column 1, we start with a dummy variable for whether a village had an election post-tsunami before the end of 2007, as recorded at the end of 2009. Having an election significantly reduces volunteer days in 2007 in column 1 by 43% which declines to a 28% reduction in column 3 when the full set of covariates are added in. As we saw above, adding a diversity control has no effect on election coefficients. Elections before the end of 2007 reduce volunteer days in 2009 by 31% in column 2 and by 25% in column 4. These past election effects are very persistent. Comparing columns 3 and 4 with a full set of controls, the election effect in 2007 is 28% versus 25% in 2009, virtually the same point estimate. For 2009, making the election variable be elections held before the end of 2009 produces a somewhat stronger negative effect, as shown in the table.

What is the election effect? Our model focuses on a regime switch in political determination of public goods. But the regime switch also brings in different types of people as heads. A key issue is that elections generally bring in better educated village heads, who, based on their education, might desire or can only muster fewer volunteer days. As a management style, they might want to produce public goods with fewer volunteer inputs per se. In Table 9 in columns 1 and 3 for 2007 and 2009 respectively whether the village head has a high school education or not has no significant impact on volunteer days and the coefficients on the election

variable are virtually unchanged. This result in part motivated the interpretation to results given by the model.

The threat to causality is that villages which had an election before 2007 (or 2009) versus those which did not are different and those differences drive the volunteerism differences. We note that underlying divisions as represented by diversity in occupations, low pre-tsunami volunteer days or lack of pre-tsunami social capital, don't impact let alone take away the election effect. But there obviously can be other circumstances.

3.3.2 Identification

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 sub-district (*kecamatan*) government. Given the mandate to hold elections, why did elections occur sooner or later after early 2006? Interviews with local election official suggested factors that are exogenous to specific villages: sub-district priorities (in a time of massive aid delivery) and capacities to monitor and authorize elections. In our village survey, the key cited reasons for what prompted a post-tsunami election are pressure from the sub-district 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 4 (of 150) cases did villages report that aid disputes of any type prompted the election; and only 6 report that aid allocations issues were the main election issue. Another set of about 20-25 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".[18] These last types of responses raise issues – "been head too long" implies disgruntlement which could affect volunteerism.

Regardless, it is clear we can't claim election timing is exogenous to village conditions. To argue that unobservables affecting volunteerism don't affect timing of elections, as noted earlier, we provide two 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

[18] We have some information on dates of pre-tsunami elections, with a wave of about 65 elections in 2000-2003 after national democratization in 1999. It seems these were informal elections. Indeed if they were formal with the proscribed time between elections, they should have had new elections in 2005-2009. The overlap between this 65 and the 61 citing term completed is only 25.

regime switch effects. Whatever unobservables differentiate whether elections are held in the years from 2006 through 2009, they have no effect on election coefficients. In fact as we will see the election timing results have a neat twist, pointing to regime-switch effects. Second we show that "placebo elections" have no effect.

Election Timing

For election timing, we can distinguish effects from elections in 2005 (7.5% of villages), 2006 (13%), 2007 (39%), and post 2007 (17%) versus no elections by the end of 2009 (24%). In Table 10, we see the results in column 3 and 4 for 2007 and 2009 volunteer days respectively for the differential effect of elections held in different years. We present the results to show effects of past elections on *current* volunteerism by year of election, relative to villages that have yet to have elections. There are several key points.

First, except for 2005, elections in years prior to the current volunteer days are associated with negative relative reductions in volunteerism, compared to villages still yet to hold elections. Elections in 2005 were before the election reforms, perhaps less formal, and few in number.

Second, consistent with the pure regime switch story, any two point estimates of the effect of elections on volunteerism in different years are never significantly different from each other (except for the 2005 effect on 2007 volunteerism). Specifically, we can compare pairs of coefficients in Table 10; but we can reformulate also with a common election coefficient (elections before end of '07 for '07 volunteerism and elections before end of '09 for '09 volunteerism) and ask if there are deviations from that main effect. The answer is no. For '07 volunteerism, the '07 effect and common coefficient (s.e.) for column 5 is -.385 (.159) while the differential for 06 is just -.041 (.210). For '09 volunteerism, the '07 effect and common coefficient (s.e.) for column 6 is -.487 (.163), while the differentials for '06 and '08-'09 are respectively .231 (.199), and .174 (.188).

Third, building on these results, while the election effects on 2007 volunteerism are essentially identical for 2006 and 2007, when we get to 2009, the 2006 effect in column 4 (relative to column 3) weakens considerably. Does this suggest evidence against the regime switch model? In a small sense it does, but in larger sense the case is bolstered. We know whether the current village head served in elite village positions previously (served on the *tuhapeut*, as village secretary, as leader of the local fishermen's association or on the council of fish captains). Those who did not serve in such positions we judge much less likely to come from

or speak for village elites. Having previously held an elite position occurs in just under 1/3 of the cases. While such newly elected heads may profess change, with the test of time, such villages may start to move back to elite domination and do not get a full persistent effect of the regime switch. In column 5 of Table 7, we add a control for whether the village head is from the elites defined in this fashion and that term interacted with each election year variable. As footnoted in the table, cell counts get very small and it is hard to nail the magnitude of the interactive effects. However they are all uniformly positive. More to the point, pulling these cases out of the overall election effect for villages sharpens the base results. Now in column 5 for 2009 volunteerism, 2006 and 2007 coefficients (for villages with non-elite heads) return to being identical, large, and significant.

Fourth, there is a difference-in-difference possibility here. We have volunteer days for ’07 and ’09 and a set of 77 villages with no elections before the end of ’07, which can be split into the 31 that have elections over the next 2 years and the 46 that don’t. The mean difference in volunteer days (’09-’07) for those with elections in that time period is -.032. The mean difference for those with no elections is .283. While this is exactly the differential result we expect with a regime-switch, the standard errors are large and differences aren’t significant.

Placebo Elections

Consistent with regime switch effects, it is only formally mandated elections which create the negative regime-switch effect. In our sample, 18% of our villages called informal elections after the tsunami, usually because the village head was killed in the tsunami and was temporarily replaced by villagers in an informal election.[19] Our presumption is that these unmonitored elections were dominated by elites. Informal elections are more likely to be conducted by a show of hands by males after Friday services at the mosque, rather than by secret ballot with participation of all villagers. In addition the sub-district might be likely to only go along (temporarily) with this informally chosen head if he came from known members of the elite. These elections have no effect on volunteerism in either 2007 or 2009. Adding to columns 1 and 2 in Table 9, a dummy for having had an informal election yields coefficients (s.e.) for 2007 and 2009 of respectively -.074 (.171) and .156 (.181).

Other considerations.

[19] An informal election is defined by a village which reports a post-tsunami election in 2007 but not in 2009. In the survey and instructions, elections reported in 2009 were supposed to be formal sanctioned ones. In 2007 that distinction was not made.

If elections effects are pure regime switch, then effects in villages where elections are held for different stated reasons should have similar effects. It is not clear exactly what to make of the self reported village information, especially with the idea that many villages just wanted new heads. But we might think a test would be to look at the set of villages forced to have an election by pressure from the sub-district or district government as reported in our surveys. If we are prepared to treat this as exogenous to the village, then under a regime switch model, the outcomes should be the same as those where problems in villages led to elections. For a specification based on column 2 of Table 9 where the base election effect is -.42, the coefficients (s.e.) for election before the end of 2009 and forced election (conditional on election being held) are respectively -.371 (.148) and -.227 (.158). This hints at stronger negative effects in villages where elections are forced from above before 2009 as opposed to other villages, an unexpected direction of bias. But the differential is not significant.

3.4 Observables and elections

In this sub-section we briefly examine the determinants of election timing. We show results for a Probit on whether a village had an election before the end of 2007. We also estimated proportional hazard model of the risk of having an election in year 1 (2005) through year 5 (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 expotential with all elections completed before 2011, but results on covariate coefficients for a censored or Weibull version are similar. We start with simple barebones specifications in columns 1 and 3 of Table 11. Results for the Probit and hazard are qualitatively similar.

No village characteristics affect the likelihood of an election except for the death of the village head. The village head effect is not as sharp as we hoped (in order to pursue IV work), because dead village heads were often first replaced informally. 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, on the seashore or not, female versus male survival, and dominated by fishing or farming as an occupation. The timing of village elections seems unrelated to inherent measured village attributes, suggesting a strong random component to election timing. In columns 2 and 4 of Table 11 we add on post tsunami variables on 2007 occupational diversity and the nature of aid intervention (which also affect volunteerism). BRR as the main house agency is significantly associated with an increased

chance of an election. Having had the governmental agency as a village's provider may institutionally have increased the likelihood of elections (greater enforcement of the election reform).

From a different perspective, in terms of potential IV work, for 2007 volunteer days, we only have death of the village head as a predictor of elections which might meet the exclusion restriction. In 2SLS work based on column 1 of Table 9, in the linear probability [LP] first stage, the village head death coefficient is actually not significant at the 5% level and the partial *F*-statistic is 3.7, too weak an instrument to work with. For 2009 volunteerism, we arguably could add whether BRR was the major housing provider as an instrument for whether an election had been held by 2009. The BRR variable is significant in the LP first stage (but again not death of the village head), but the *F*-statistic is still only 4.8. There the OLS election coefficient (on count of days) is -.76, while the 2SLS's one is -2.7; but the latter has a huge standard error (2.8).

4. Individual volunteer days

To help inform certain debates in the literature, we brefily examine within village variation in participation in volunteer days amongst traditional fishing families in 2009, controlling for all village level conditions by village fixed effects. We also present panel estimates with household fixed effects for changes between 2007 and 2009.

4.1 Baseline results

We have information for families in about 90 villages; but with village fixed effects in a Poisson, we remove villages with just one surveyed family and villages where all families report zero days. The final sample in 2009 is 545 families in 76 villages. We record volunteer days in the last month for all family members. We focus on those for the whole family (mean of 1.50). In column 1 of Table 12, we show baseline effects for 2009 and in column 2 panel results. We delay discussion of columns 3 and 4.

We start with a usual set of controls on family demographics and income. Not surprisingly, in columns 1 and 2, larger households are more likely to volunteer. Having a new baby appears to reduce volunteerism in 2009, which might reflect opportunity cost. Having a new baby between 2007 and 2009 in the panel formulation does not reduce volunteerism between the two years. This variable suffers from measurement issues such as timing of pregnancy and at what age to cycle children out of the defined variable. Columns 1 and 2 suggest

strong income effects, with the column 1 control for (time invariant) education having no effect. Income effects are, perhaps surprisingly, non-monotonic. Compared to middle income families (50% of sample), both low and high income families volunteer less. Clearly an opportunity cost of time story can't be all that is at work: low income families may simply not value public projects as much as other families (as the model might suggest).

Asset ownership, on the surface, presents a puzzle. Farm land ownership which does not vary for households between years is associated with increased volunteerism in 2009. However, owning a boat in 2009 has a negative but insignificant effect and having just gotten a boat between 2007 and 2009 significantly reduces volunteerism. One explanation would be that families with land may be more vested in the village and more dependent on the village head in their operations, unlike fishing boat owners. For boat owners, the production opportunity cost effects of owning boats on volunteerism seem to dominate.

In 2009, conditional on boat ownership, having received a boat on aid typically 2-3 years earlier is not associated with higher volunteerism, implying no persistent pay-back issues. However in the panel, getting a boat between 2007 and 2009 is associated with higher volunteerism, suggesting immediate effects. Social obligation notions are enforced by the associations with whom a person applied for boat aid originally. If a family is one of the 15% of fishing families that applied for a boat through the village head (rather than more typically through *Panglima Laot*), they volunteer more in 2009 (usually about 3 years later). By exploring interactive effects we note this increase occurs, *regardless* of whether the family got a boat on not or whether that boat was high or low quality. We think the application to the village head is simply an indicator of more personal ties to the village head, especially given boat allocations in the end were generally done by *Panglima Laot*.

Finally we record trauma: people experiencing panic attack-like symptoms ("nausea, racing heartbeat, difficulty in breathing, sweating") from memories of the tsunami. In 2009, for the 4% of families where the household head is still experiencing those symptoms there is no effect, nor is there in the panel where a much higher fraction (12%) report trauma in 2007.

4.2 Recent considerations in the literature

In the last two columns of Table 12, for 2009, we examine two ideas that have been formulated in the recent literature. While we focus on household participation in column 3, we also look at participation of the household head in column 4, for the second idea where the effect may be

personal to the head. The first idea in Benabou and Tirole (2006) is that excuses to not volunteer are validated by circumstances. Having had a new baby may be an excuse not to volunteer, but the validity of that excuse may decline in villages where more people have just had new babies (i.e., the village still needs volunteers). Column 3 suggests this is the case. While volunteerism is less with a new baby, the effect is smaller in villages where there are more babies. We don't show a panel version of this for data reasons (lack of variation over a limited sample of villages).

The second idea concerns Carpenter and Myers (2010) idea that image concerns affect volunteerism. In our sample 35% of households have debt, usually to other villagers such as shop keepers and fish wholesalers. People with debt may be more burdened and less likely to volunteer, but they may also want to volunteer to promote community goodwill and perhaps gain forbearance. That is, they have a personal incentive to volunteer. Debt on its own has no significant effect on volunteer days. However debt interacted with how many days a week a head goes to mosque (average of 4.5 with a maximum of 7) does. Column 3 and especially column 4 for the household head suggests those with debt volunteer, but that declines with mosque attendance. One might interpret this in a Carpenter and Myers framework. Those who go to mosque more and show greater public devotion may be cultivating an image of devoutness, and the incentive to volunteer more to gain forbearance declines with image concerns. While the material incentive to volunteer is there, responding to that (as perceived by the community) if image is also a primary concern, detracts from image.

5. 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 of 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.

We also find strong but, surprisingly, not highly persistent social capital effects. Second we find a variety of aid conditions strongly affect volunteerism in 2007 but their effects are gone by 2009. Villages with more official projects had temporarily reduced volunteerism unless the dominant housing agency was a donor-implementer. Donor-implementer agencies are associated with higher quality aid, fewer resulting disputes, and more attention of the agency to village life.

Data Appendix

Our 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.

The fishermen surveys ask about family structure, occupations, social status, income and aspect of debt and wealth, housing and boat destruction and aid, fishing productivity, and family participation in village activities. The 2005 survey focused on original boat owners and captains, benchmarking family destruction of people, housing and boats, as well as pre-tsunami productivity. The 2007 and 2009 surveys follow these families, marking their rebuilding of families, new occupational choices, aid received, reestablishment or not of fishing activities, and evolving family participation in village life. One focus is on the quality of aid received and response to low quality boat aid.

The survey area

Figure A1. Map of survey area

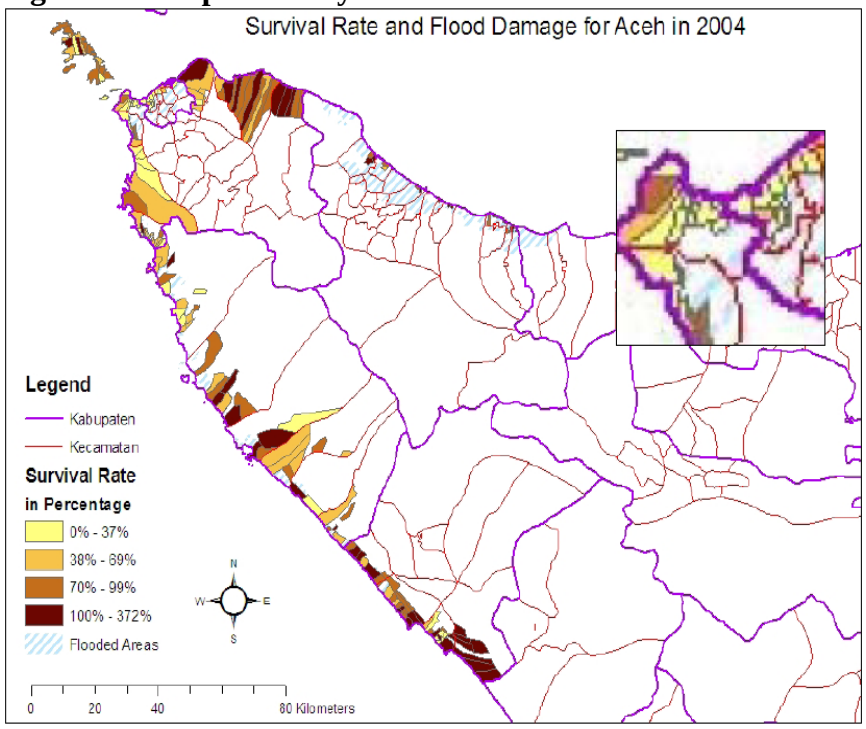

Figure A1 shows a map of the survey area, with a blow-up (right side in figure) of the Banda Aceh area (upper-left part of coastal area). The map shows household survival rates by village (yellow being the worst). Unfortunately, the map is based on the government rendering, post-tsunami, of village boundaries. 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.

Government versus survey numbers

We view our 2005 survey numbers on pre-tsunami and post-tsunami populations as more reliable than government numbers which tend to under-count by 10-15%. However 2007 survey numbers on pre-tsunami populations per se can be hazy with the influx of new village heads; and, relative to 2005, 2007 counts of village households (but not pre-tsunami houses) become more strategic with villages splintering surviving households to claims more need for aid. In estimation, we rely on 2005 and earlier government numbers to have complete coverage of all 199 villages, removing some obvious outliers with bad information as explained in the text. But as an aside we comment on our versus government number on pre-tsunami populations and survival as reported in 2005. Official population counts pre-tsunami are from the P4B, a 2004 government pre-election census. For some individual villages, P4B counts and our pre-tsunami counts diverge markedly. We did intensive field surveying in 10 villages where numbers diverged a lot, to ascertain that our numbers seemed much more accurate, based on specific types of village recordings of population (e.g., the number of *zakat fitrah* payers in 2004, which is a Islamic poll tax on all living people in the villages; the number of votes in the 2004 elections recorded by the official local tabulator; a count by a mid-wife of village population just before the tsunami, etc.). We believe our numbers are fairly accurate compared to the P4B which was conducted in the insurgency period. For post-tsunami numbers, we use the 2006 PODES counts. The PODES is a tri-annual government inventory of village populations and facilities. The 2006 PODES in Aceh was conducted in the Spring 2005. It has lower counts of population and households compared to our 2005 survey (Summer and Fall, 2005). This may be partly a “9/11 phenomenon”; as time goes on more missing families are discovered.

Summary statistics for estimating sample

Village level

	Mean	**Standard deviation**
Count vol days 07	1.52	1.46
Count vol day 09	1.56	1.34
Ln (number of households, post tsunami)	4.84	.78
Survival rate of population	.75	.45
Arisan group pre-tsunami	.68	.47
Mullah survive	.65	.48
Number of aid projects	31.9	16.2
Housing provider is donor-implementer	.53	.50
Housing provider is BRR	.14	.35
Ln (voldays pre-tsunami +1)	1.18	.48
Ln(distance to Banda Aceh)	3.61	1.26
Occupational diversity 07	.54	.15
Election post-tsunami before end of 07	.59	.49
Election after 2007	.17	.37
Village head high school or more 2009	.61	.49
Village head survive tsunami	.77	.42
Ratio: farm hh's to land owning hh's	.96	2.48
Village meet regularly	.49	.50
Vol labor to rebuild mosque (N=165)	.25	.43

Fishermen

	2009 mean	**2007 mean**	**2009 s.d.**
Vol. days head	1.01	.52	1.06
Vol. days family	1.50	.72	1.85
h.h. size	4.04	3.88	1.67
h.h. head high school or more	.20	.17	.40
Own land	.27	.31	.44
Own boat	.47	.62	.50
New baby (child 3 or young present)	.49	.33	.50
trauma	.044	.097	.21
Get aid boat	.53	.48	.50
Low income (<250,000 rupiah per week	.36	.27	.48
High income (>500,000 rupiah per week)	.14	.22	.35
Apply village head	.16		.36
pray	4.37	4.49	2.56
debt	.33	.21	.47
Fraction population under 6			

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Figure 1. Equilibrium relationship in the model

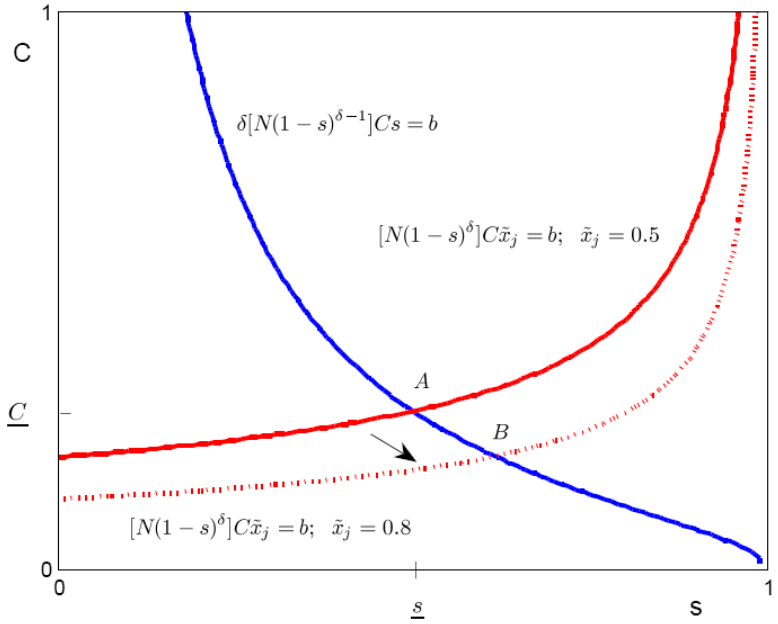

Table 1. Destruction of population and housing

	N=190 ('07 sample)
Survival	
Pre-tsunami population [a]	171783 (official)
Survival rate of population [b] [original 05 villages, 104 covered]	**65%** [49%]
Post-tsunami households, official	32876
House aid	
Number of houses survive tsunami, survey	5399
Survival rate houses	**9%**
Number of temporary aid houses built ('07 survey)	6529
Number of permanent aid houses built ('07 survey)	32277
Replacement rate by late 2007 [c]	**117%**
Number of permanent aid houses built by late 2009	39899
Other aid	
Survival rate public buildings [d]	**6%**
Replacement rate, public buildings by late 2007	**80%**
Replacement rate, public buildings by late 2009	96%
Survival rate of boats ['05 sample of villages]	**[6%]**
Replacement rate, boats [2007 survey for 96 villages surveyed in '05] [e]	**[105%]**

a. Official population counts pre-tsunami are from the P4B, a 2004 government pre-election census.

b. 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.

c. The replacement rate is the number of houses given in aid divided by the number of surviving households less the number of surviving houses.

d. Includes mosques, village halls, fishermen halls, public and Islamic elementary schools, health facilities.

e. Defined as boats on water by late 2007/surviving captains 2005.

Table 2. Aid effort—diversity and specialization

	N=198
Avg. (median) number of different NGO's as first level implementers in RAND in the village	11 (10)
Avg. (median) number of different projects (first level implementations) in RAND in the village	**30 (29)**
No. of villages where one NGO provides =.> 50% of housing	170
No. of villages where one NGO provides =.> 85% of housing	105
No. of villages where largest housing provider is "donor-implementer"	97
No. of villages where largest housing provider is BRR	23
No. of villages with no housing destruction	9
No. of villages with destruction yet to receive "permanent" housing aid [a]	15

a. These villages have several features: very high population survival rates (and in two cases probably no housing damage, based on 2005 information), and, when housing is destroyed, unusual levels of "temporary" housing. There is a sometimes a dispute between villages versus BRR and NGO's about what is temporary versus permanent.

Table 3. Elections and old village heads

	Percentages
Post-tsunami elections before end of 2007 (out of 199 villages)	58.8
Post-tsunami elections before end of 2009 (out of 199)	75.4
No. of village heads who survive (out of 199)	76.4
Survive & in office at end of 2007 (out of 152 survivors; [out of 199])	46.7 [35.7]
& reelected post-tsunami (out of 71 in office; [out of 199])	38.0 [13.6]
Survive & in office at end of 2009 (out of 152 survivors; [out of 199])	15.5 [11.8]
& reelected post-tsunami (out of 22 still in office; [out of 199])	22.7 [2.5]
% village heads with high school, pre-tsunami (out of 194, recalled in '07)	46.7
% village heads with high school, in 2007 (out of 199)	61.8
% 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

Table 4. Elected versus non-elected village heads in 2009

	Percent with high school	Average age	Held customary position in past*
Head elected post-tsunami	66.0	42.3	16.6
Head not-elected post tsunami	48.1	48.1	21.7

*Previously member of *tuhapeut* or fish captain's council, village secretary, or head of local fishermen's association.

Table 5. 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
Households, proportion volunteering	n.a.	.495	.736	545 h.h.'s in '07 and '09	7.83
Households, average family-member days in month	n.a.	.741	1.52	545 h.h.'s in '07 and '09	9.11

Table 6. Village life and social capital

	Pre-tsunami	2007	2009
Arisan group exists	136	123	124
Overlap '05		116	97
Overlap 07			90

Table 7. Volunteer days per month called by village head

	Social capital 2007	Social capital 2009	Aid effect, 2007	Aid effect, 2009	Pre-tsunami public labor, 2007	Pre-tsunami public labor, 2009
Ln (no. households post tsunami)	-.142 (.094)	-.148* (.083)	-.085 (.099)	-.130 (.091)	-.080 (.100)	-.121 (.094)
"Social capital"						
Survival rate population	.290** (.142)	.045 (.116)	.208 (.153)	.033 (.118)	.186 (.126)	-.019 (.114)
Village had pre-tsunami arisan group	.603** (.160)	.219* (.134)	.469** (.184)	.279** (.139)	.422** (.175)	.251* (.139)
Mullah survive tsunami	.345** (.184)	-.054 (.138)	.384** (.150)	-.073 (.136)	.361** (.142)	-.085 (.134)
Aid level and form						
Official number of aid projects in village			-.0091** (.0046)	-.0034 (.0053)	-.011** (.0043)	-.0042 (.0052)
major housing provider is donor-implementer			.322** (.157)	-.041 (.149)	.258* (.151)	-.067 (.147)
Major housing provider is BRR			-.648** (.190)	-.014 (.181)	-.559** (.179)	.044 (.178)
Volunteer day history						
Ln (Volunteer days per month pre-tsunami+1)					.514** (.166)	.283* (.147)
N	187	187	186	186	186	186
Pseudo Rsq	.055	.001	.078	.011	.073	.100

Table 8. Other social capital, opportunity cost, and diversity conditions

	2007	2007	2009
Official number of aid projects in village	-.0089* (.0046)		
Majority housing provider is donor-implementer	.243* (.143)		
Majority housing provider is BRR	-.535** (.186)		
Ln (distance to Banda Aceh)	.279** (.068)		
Housing provider uses local labor, 2007	-.227 (.191)		
Election, post-tsunami Before end of 2007		-.422** (.133)	-.258** (.128)
Occupational diversity in village		-.441 (.380)	-.714* (.370)
Other controls	See col. 3 Table 7	See col 1 Table 9	See col 2 Table 9

Table 9. Volunteer days: elections

	Basic covariates		Extended covariate list	
	2007	2009	2007	2009
Ln (no. households post tsunami)	-.161* (.086)	-.151* (.083)	-.091 (.098)	-.125 (.095)
Survival rate population	.277** (.122)	.021 (.114)	.183 (.114)	-.024 (.112)
Village had pre-tsunami arisan group	.618** (.182)	.218* (.133)	.433** (.172)	.257* (.140)
Mullah survive tsunami	.305* (.156)	-.087 (.136)	.331** (.139)	-.112 (.133)
Official number of aid projects in village			-.010** (.0044)	-.0034 (.0053)
Major housing provider is donor-implementer			.254* (.149)	-.076 (.147)
Major housing provider is BRR			-.514** (.180)	.088 (.180)
Elections				
Election, post-tsunami Before end of 2007 [2009]	-.430** (.129)	-.305** [-.423**] (.126) [(.147)]	-.282** (.136)	-.246** [-.360**] (.136) [(.137)]
Ln (Volunteer days per month pre-tsunami)			.435** (.175)	.221 (.151)
N	187	187	186	186
Pseudo Rsq	.075	.019	.108	.025

Table 10. Volunteer days: elections year-by-year and education [a]

	2007	2009	20 07	2009	2009[b]
Village election before end of 2007	-.413** (.133)	-.278** (.131)			
Village head, high school or more	-.106 (.142)	-.138 (.143)			
Village elect. in 2005			.044 (.229)	-.188 (.234)	-.255 (.268)
Village elect. in 2006			-.504** (.191)	-.310 (.209)	-.568** (.252)
Village elect. in 2007			-.540** (.150)	-.552** (.156)	-.576** (.185)
Village elect. in 2008-2010				-.361** (.184)	-.427* (.245)
VH "elite" (held prior village positions)					.050 (.241)
Village elect. in 2005* VH "elite"					.424 (.504)
Village elect. in 2006* VH "elite"					.568 (.407)
Village elect. in 2007* VH "elite"					.061 (.347)
Village elect. 2008/09* VH "elite"					.131 (.379)
N	188	187	187	187	187

a. The variables listed are added to the column 1 and 2 formulations in Table 7. Adding in all controls in the last columns in Table 7 does not enhance the education effect. Adding in all controls in columns 3 and 4 of this table little impact, other than as in Table 7 to reduce elections effects by typically under 20%. For 2007, the coefficients (s.e.) on 2006 and 2007 election become -.425 (.199) and -.385 (.159). For 2009 the 2006, 2007 and 2008/09 coefficients (s.e) become -.256 (.210), -.487 (.163), and -.313 (.189).

b. Cells sizes get small. For the interaction of VH elite with elections in 2005, 2006, 2007, and 2008/09 the count of 1's are 2, 9, 22, and 12.

Table 11. Probits (marginal effects, robust errors)
and proportional hazard for election timing

	Formal election before end 2007		Formal election in years 1-6	
	Probit		Hazard ratio: t-stat reported	
Ln (no. households post tsunami)	-.023 (.050)	-.028 (.057)	1.02 (0.29)	.981 (-0.23)
Survival rate population	.039 (.077)	.056 (.074)	.962 (-0.36)	.990 (-0.10)
Village had pre-tsunami arisan group	-.025 (.081)	.015 (.091)	.848 (-1.34)	.891 (-0.88)
Mullah survive tsunami	-.096 (.078)	-.110 (.080)	.891 (-0.95)	.896 (-0.87)
Village head killed in tsunami	.181** (.082)	-.188** (.083)	1.33** (2.09)	1.37** (2.31)
Official number of aid projects in village		.0019 (.0028)		1.00 (0.95)
Major housing provider is donor-implementer		-.145* (.083)		.840 (-1.51)
Major housing provider is BRR		.282** (.083)		1.56** (2.15)
Occupational diversity in village		.495* (.265)		1.62 (1.39)
N	187	181	186	179

12. Count of family volunteer days (robust errors)

	Village FE	Panel: HH fixed effects	Village FE	Village FE
	Family 2009	Family 2007 & 2009	Family 2009	HH head 2009
Household size	.134** (.024)	.212** (.047)	.128** (.025)	-.049 (.032)
New-born baby (age0-3)	-.241** (.083)	.0022 (.117)	-.579** (.144)	-.214 (.165)
*ratio of child under 6 to village population			3.45** (1.20)	2.12 (1.39)
HH head high school of more	.053 (.103)		.041 (.104)	.100 (.120)
Low income family (under 250,000)	-.194** (.093)	-.165* (.097)	-.177* (.093)	-.154 (.114)
High family income (> 500,000 per month)	-.419** (.128)	-.248** (.127)	-.383** (.130)	-.280* (.151)
Own land	.225** (.105)		.233** (.106)	.226* (.125)
Own a boat currently	-.140 (.090)	-.498** (.105)	-.117 (.093)	-.063 (.112)
Received boat on aid	.028 (.093)	.310** (.117)	.043 (.094)	.024 (.113)
Apply to village head for boat aid	.273** (.112)		.255** (.113)	.310** (.139)
Suffers trauma	-.145 (.187)	-.128 (.159)	-.146 (.189)	-.143 (.228)
Have current debt			.248 (.174)	.402* (.228)
*Number times at evening prayer last week			-.075** (.037)	-.100** (.045)
Number of times at evening prayer last week			.0036 (.019)	.017 (.023)
N [villages}	545 [76]	940	545 [76]	545 [76]