

Aid and village public goods: after the tsunami¹

Tiago Freire, Brown University
Vernon Henderson, Brown University
Ari Kuncoro, University of Indonesia

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Abstract: Using survey data on fishermen and fishing villages in Aceh, Indonesia from 2005 and 2007, this paper examines the effect of the December 2004 tsunami and resulting massive aid effort on local public good provision, in particular on public labor inputs, but also public capital choices. Also analyzed are the roles of and changes in local social and political institutions and participation in political and social activities. Such an examination informs not only our understanding of the impacts of aid on villages, but also our understanding of how villages allocate resources to public goods. For public labor inputs, volunteerism is lower in villages with more aid projects, but that is offset if the dominant donor mitigates agency problems by doing its own implementation. Volunteerism is lower in villages with more “democratic” activity such as elections, although that effect is mitigated in villages with higher levels of social capital pre-tsunami. Evidence suggests volunteerism is lower not because of changes in types of leaders with village elections per se, but rather due to heightened internal divisions associated with elections. Correspondingly for public capital, villages with more democratic activity combined with more aid projects tend to emphasize garnering private aid (e.g., houses) at the expense of public aid (e.g., public buildings).

The tsunami of December 2004 devastated major coastal cities and villages of Aceh, Indonesia. In affected coastal areas, well over half the population died and almost all physical capital was destroyed: housing, boats, schools, and local health clinics. Local fishermen’s credit unions and their records were wiped out. The destruction and ensuing publicity resulted in a truly massive aid effort. Beyond the initial clean-up efforts, the infusion of aid replaced almost all lost capital for the remaining population—houses, boats and public buildings within two and half years. Although some types of aid such as coastal environment restoration lagged, what seems distinctive about the aid effort is the multitude of NGO’s with budgets that had to be spent, presenting an almost overwhelming magnitude and array of aid and of aid agencies with which village leaders had

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to negotiate. In fact now at the end of the aid effort, where the official process ended April 16, 2009, there is still \$250m left over (Jakarta Globe March 2, 2009), in a context where, despite massive loss of life and households, aid gave 134,000 houses for 120,000 houses destroyed (Xinhua News Service, February 1, 2009).

Using data from 199 villages, this paper examines the effect of the massive aid effort on village social and political institutions, participation in political and social activities, labor contributions to public goods, and the allocation of aid between private and public capital. The focus will be on the choice of public labor inputs in a village, as influenced by social and political institutions and the form of aid delivery. Examining these issues will inform us about the impact of a disaster and massive relief effort on village institutions and what factors influence village choices over public labor and the allocation of aid to public capital. While this is not a normative paper, understanding the various effects may contribute to shaping better aid delivery processes.

The literature on aid suggests that increased aid may lead to a decline in public good provision, in part because individuals spend more effort lobbying for aid to be distributed in the form of private aid to enrich themselves, as opposed to public goods (Svensson, 2000). Knack and Rahman (2007) further argue more donors may erode local bureaucratic quality and capacity, which could reduce villagers' incentives to invest in public goods and the ability of remaining leaders to marshal public effort. However, the literature suggests that outcomes are affected by the stock of social capital, which may make cooperative outcomes more sustainable (Sobel, 2002). In an empirical paper that relates to some of this paper, Labonne and Chase (2008) examine community driven development programs in the Philippines. They find that in villages where such programs are assigned, compared to the control group, people spend more time "lobbying" for benefits and less time participating in social-network activities. Finally, political economy models suggest the form of village political institutions may have a strong effect on outcomes (Persson and Tabelli, 2000). For example, Foster and Rosenzweig (2004) argue that a greater degree of democracy leads to greater provision of public goods of more universal appeal, although Acemoglu and Robinson (2006) question the effect of greater democratization. The work in Munshi and Rosenzweig (2008) suggests that changes in political institutions may result in villagers seeking different types of

leaders in a Besley and Coate (1997) citizen-candidate framework. This last idea will be important in interpreting results.

We focus on two types of public good inputs. The first involves labor inputs. A almost universal practice in Aceh has been for village heads to call for several “Islamic” volunteer days a month, where villagers volunteer to clean-up the village, dig aquaculture channels, plant mangroves or otherwise fortify the shoreline, construct or repair public buildings, and the like. The village head has to decide whether to call for volunteer days and then individual villagers have to decide whether to participate or not. The second type of public good is physical capital in the form of public buildings. Villages make choices as to whether to focus on garnering private aid (e.g., houses and boats) as opposed to aid for public buildings.

While the processes driving these two outcomes are related, they have fundamental differences. As we will see, volunteerism seems to be affected by “cohesiveness” in villages, as related to measures concerning village social capital, the advent of elections, and the form in which aid is given. However the choice between public and private buildings seems more affected by the method by which villages make the choices which determine how aid is allocated in a village. Such decisions may be made in village meetings, by the village head, or by external agencies. Surprisingly, the mechanism itself seems generally uncorrelated with village circumstances—political, social capital, loss of leadership, elections, and the like. The decisions of a village head may induce different outcomes from those of a village hall meeting, with both its majority rule and potential for log-rolling.

The tsunami and aid effort had a huge impact on village political institutions and type of leadership. Based on surveying in 2005 and related fieldwork 5-6 months after the tsunami, the survival of village leadership such as the village head and mullah had a strong effect on initial outcomes, with these traditional leaders trying to retain autonomy of the village with the advent of outside aid. However, the type of village head a village wants during a massive relief effort may be very different than the type under traditional business-as-usual. In a Besley and Coate (1997) framework, villagers may want a leader who is more competent in dealing with foreign NGO’s, and may want a leader with different preferences for private versus public aid (Munshi and Rosenzweig, 2008).

In the 2 and ½ years following the tsunami, two thirds of our villages had elections. These usually occurred because the pre-tsunami village head perished, prompting an election; or because a regularly scheduled election occurred -- about 20% of villages are supposed to have an election each year on an overall 5 year election cycle. Reportedly in some cases where village heads survived and no elections were scheduled automatically, disputes in villages over aid allocation prompted elections.

Two things stand out about these post-tsunami elections. First and to an extraordinary degree, 80% of the cases, village heads who survived the tsunami but faced an election lost that election. Being an incumbent did little to stem the tide of change. Second and presumably related, new village heads are much better educated than original ones. What is the impact of this regime switch on villages? We will show that public labor inputs are distinctly lower in villages which had elections. The issue is why. For volunteerism, elections in the face of massive aid may stir up contention within the village making cooperation more difficult and inducing time-intensive political lobbying, both lowering inputs into public labor. Alternatively villagers may vote for a head who desires fewer volunteer days. We will argue in favor of the former mechanism. For public buildings, we will show that in villages where aid allocation decisions are made in village meetings, public relative to private capital falls as the number of aid projects in a village rises. The opposite is the case if the village head is a surviving head who has not faced an election. A traditional head could have a paternalistic view for the village that better internalizes public good externalities; or, alternatively, could benefit more from corruption possibilities in provision of public goods. Village meetings may allow the majority to focus on private aid of immediate benefit to the majority, at the expense of public aid. For example, public aid while improving overall village outcomes in the future may disproportionately improve productivity of a minority.

We first turn to an overview of the data and some background on the project and disaster relief, so as to provide a context for the current paper, and the questions investigated. Part of that context also involves an overview of the whole aid process. We then turn to a conceptual framework for individual and village level public good decisions making. In the third section, we formulate our specific empirical hypotheses,

which require detailing aspects of village institutions and forms of social capital. The fourth and fifth sections give our results.

1. Disaster relief and our data

In this section, we start with a description of our data and then we outline issues in the literature, which also indicate the scope of the project.

1.1 Data

In summer and fall 2005, we surveyed village heads and local heads of the fishermen's association in 111 fishing villages and we surveyed 540 households that identified as fishing boat owning families in 72 of those villages. In summer and fall 2007 we resurveyed the 111 villages and added another 88 villages, which were further away from the capital Banda Aceh, and were inaccessible in 2005. Besides resurveying most of the original 540 fishing families, we added two types of families and expanded coverage of fishing families to 26 more villages (beyond the original 72), mostly further down the coast from Banda Aceh in more traditional fishing areas. We added 151 families that had never owned a boat before but got one through the aid process; and we added about 200 families that were former boat owners, focused on the 26 new villages. We will resurvey these villages and fishing families in late 2009 and again in 2012. By the 2007 survey, as we will see much of the aid effort to villages was "complete".

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, in contiguous districts, as we move south and north-east of Banda Aceh, trying to cover all fishing villages in three districts (Banda Aceh, Aceh Jaya, and Aceh Besar) and those up to a defined geographic limit in two other districts, 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). These include villages on islands offshore for these districts. The data now cover 31 sub-districts in the 5 districts. All villages were affected by the tsunami, although some, such as those in Pidie, were more protected and lost less housing and population. There are no unaffected fishing villages, within the same general cultural area.

Figure 1 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 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 village survey asks questions about education, experience, and survival of village and religious leaders; population composition by sex and age both before and after the tsunami; occupational structure; use of village lands and destruction of lands, seawalls, aquaculture areas, docking areas and mangroves by the tsunami; 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.

For fishing families, the sample of initial boat owners represents 25% of pre-tsunami boat owners and 45% of surviving boat owners in these villages.² This module asks pre- and post-tsunami information on boat sizes, features, catch, marketing, and boat aid sales, and purchases; family structure, occupations, debt, and earnings, trauma and

² The initial frame for this survey was a random sample from a list of pre-tsunami fishing boat owners assembled by the provincial level fishermen's association-- Panglima Laot [PL] working with an NGO; but the list was amended in the field. Those resurveyed tend to be mostly boat owners who survived the tsunami. The initial survey was carried out under incredibly difficult and traumatic circumstances, with respondents scattered in refugee camps. In the field, there was some over-sampling in a few more accessible villages, although in principle still randomly sampled within that village. Since we know the overall fishing statistics for each village from the village survey (in particular, boat types and catch composition and levels), we can check for representativeness of the sample in terms of boat types and catches, and in terms of observables the sample seems representative,

participation in village social, political, and volunteer activities; and housing conditions and aid, land holdings, and land titling..

These data suggest a number of analyses. We briefly examine a few of these from the literature which inform this paper. In the disaster literature, there is work on whether trauma is debilitating versus spurs a greater focus on the communal activities (Fitz 1961 and Erikson 1976, 1994). But the economics literature asks whether areas return to their equilibrium path, after a disaster (Davis and Weinstein, 2002 and Miguel and Roland, 2006), although a focus of modern aid is “sustainable” development (Brown, Crawford, and Hammil, 2006), which might be explicitly interpreted as altering the equilibrium path and changing village institutions and social and occupational choices (Townsend 1994 and Field 2005). Will the aid-induced RALAS project in Aceh to formally title lands within villages or the decline of volunteerism be sustained over time? Sustainable development also plays out among fishing families at the micro level.³

There is a large literature on the efficacy of and best ways to deliver aid, discussing conditionality and agency questions (Collier et al. 1997, Azam and Laffont 2003, Svennson 2003, and Murrell 2002), strategic considerations (the Samaritan’s dilemma in Pederson 2001 and Torsvik 2005), co-ordination across agencies, village “ownership” of the process and the like (e.g., Kanbur and Sandler 1999, Easterly 2002, and Paul 2006). The aid process in Aceh ended up being unconditional and largely 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 recommending, if asked, where an NGO might focus aid and (2) late in the process filling in ex post gaps in private aid and more particularly public aid. Given a complete lack of conditionality and co-ordination, we will focus more on agency issues.

³ First there is the in the occupational and technology choices of pre-tsunami fishing families. Then there are issues of inequality (Becker and Tomes, 1979, Loury, 1981, Mookherjee and Ray, 2002 and 2006). The tsunami and subsequent aid process was equalizing: almost all private and financial capital in villages was destroyed and villagers largely got the same level of aid. Will the socio-economic rankings of families that existed pre-tsunami re-emerge in ten years? That is, were original divisions driven by wealth differences and capital market imperfections, as opposed to ability differences?

2. Aid in fishing villages

Table 1 presents an overview of destruction and house aid in our villages, where house aid accounts for much of overall aid within the village. There are two samples: in the first column, numbers are reported for 104 villages we surveyed in 2005 for which we have complete 2005 and 2007 information on a variety of characteristics, as well as official government data pre and post tsunami on village households and population. Column 2 reports on 190 villages where there is a complete information requirement for 2007, as well as official government data. Government data on pre-tsunami populations is problematic, given limited survey conditions under the 25 year secessionist movement in Aceh, a conflict only resolved in mid-2006.

Key information in Table 1 is as follows. The survival rate of households is about 58% -- that is 42% of households are effectively wiped out, meaning no surviving independent adults. The survival rate of population in a sub-set of our villages is 45%, and that is similar to the government official survival rate of 49%. Almost all houses are destroyed in fishing villages. If we add together surviving houses and the number of permanent houses given in aid, relative to the number of surviving households, there are now 1.08 permanent houses per surviving houses according to our household count, as opposed to the government post-tsunami count which suggests even more houses per household. In addition, some the houses reported as “temporary” by villages may be viewed as permanent by aid agencies. Houses built of wood (even if well built) are considered temporary in local climatic conditions; however their long term efficacy depends on whether the foundations are cement or not. Regardless, in the aggregate, private house aid looks pretty complete. Of course the amount of aid given varies across village. And villages have had new family formation as people remarry and young adults move out of the house, which has prompted village calls for more housing aid. The last two rows tell us how the number of households claimed in villages has escalated since the tsunami. We examine these issues in a companion paper that is underway; there we show that this completion of private aid appears to also hold for boats, based on numbers of surviving captains.

In 2005, we found that house and boat aid at that time, both delivered and promised, were closely connected with survival of village leaders—the village head and

local religious leader [mullah]. Also in 2005, it looked like villages were going to be adopted by specific aid agencies, who offered comprehensive plans for recovery and replacement of village capital. We found that aid and adoption were apparently resisted by surviving leaders, who wanted to retain control of village affairs. By 2007 the aid process was so overwhelming, that adoption was no longer a relevant concept and survival of village leaders, elections and political changes had no effect on the level of private aid delivery.⁴

Table 2 shows the aid effort from a different perspective: the count of aid agencies a village faces. We identified the agencies involved, as perceived by a village, in provision of houses, boats, seven types of public buildings noted below, and replacement of the coast line (seawall, mangroves, trees, etc). The median and average number of agencies listed as being in villages is about 7 -- seven different NGO's that a village head needs to negotiate with and co-ordinate. That number masks a much larger array of donors behind the process and a much larger array of on-the-ground contractors who are sub-contracted to do construction. Government numbers [RAND] suggest 11 different NGO's per village, but perhaps more relevant to our work, RAND identifies the number of different, officially recorded aid projects in a village (each, in principle, negotiated separately). The mean and median are both about 30. In our 198 villages, 117 different agencies are named by village leaders (where we did some consolidation of NGO's named, which have different organizational sub-divisions); and 57 different aid agencies are providing housing. In over half the villages, 85% or more of house aid as perceived by village leaders is provided by one agency, so there is some degree of NGO specialization within villages.

⁴ The exception is boat aid. If the local head of the fishermen's association is also head of the lagoon (the unit at which the PL regulates fishing across villages), a village got more boat aid. Also there is a strategic element that appears which we call "exaggeration". The count of pre-tsunami houses is hard to fudge, because of village collective post-tsunami mapping exercises of all land holdings; because, although the tsunami destroyed house walls, foundations were generally left; and because of government pre-tsunami counts. What villages claim is over a 50% increase in the number of households between 2005 and 2007, an increase that is uncorrelated with post-tsunami demographics (village average household size, males to females, adults to children). In regression work in the companion paper, these "exaggerated" claims are correlated with higher aid levels, *ceteris paribus*. For boats there are no records of pre-tsunami boats; but by 2007 villages are recalling there being 30% more pre-tsunami boats than in 2005. Again such claims seem to be rewarded.

For later reference, we note that, in 69 villages, house aid is provided by a “donor-implementer” where that definition is based on the BRR data base [“RAND”]. A donor-implementer is a donor agency that as a rule does its own implementation in Aceh, either directly hiring the labor to be used in construction, or else monitoring any sub-contractors. This helps solve the agency problem that arises when donors differ from implementers and sub-contractors are used with minimal monitoring. As an example of poor monitoring issues, for some non donor-implementers such as Oxfam, World Bank, Spanish Red Cross and American Red Cross, villages report over 13% of houses-in-aid from these agencies are abandoned/uninhabited (due to poor construction). For boats overall the abandonment rate is about 25%, with major donors like Oxfam and the International Medical Corps, as well as the government (BRR), having very high proportions of unusable boats.

Finally in Table 3, we turn to public buildings. We distinguish two forms of public buildings—mosques, both regular and masjid (great) mosques, and other public buildings which consist of village assembly halls for village meetings, fishermen halls for meetings of fishermen, state elementary schools, Islamic schools (dayah) and health facilities. Mosques are an “essential” part of village life. In many villages, reconstruction using village volunteer labor started before any other permanent reconstruction; and we will examine the use of volunteer labor in mosque reconstruction below. We don’t treat mosques as an entity of choice to be traded-off against private aid. Rather we focus on other public buildings.

Table 3 indicates that replacement rates for either mosques or public buildings are well below 100%. Note there are multiple mosques per village (averaging 2 pre-tsunami). With a loss of 42% of households and over 50% of population, it isn’t clear what an appropriate replacement rate is for any facility. Except for mosques, many villages had no facility of various specific types to begin (column 4). They did without, used facilities of a neighbor village, or used the mosque. Islamic schooling and village meetings might be held in the mosque. For village halls and health facilities in particular, many villages without a facility pre-tsunami got one in the relief effort, to some extent driven by donor demands to build and locate facilities. For example an Australian agency, AIPRD built

49 village halls. BRR is heavily involved in building of facilities, except oddly for state elementary schools which come from an array of private donors.

Column 5 of Table 3 shows the share of villages which had a facility (perhaps multiple for mosques) before the tsunami that was destroyed and has been replaced. Mosques have the highest replacement rate at 88%, followed by schools at 79%. Others are in the 63-79 range, except for fishermen's halls at 33%. The last column shows that some villages where facilities existed and were not damaged, got either an additional one or a new one. There is a lot of action in the public building numbers of non-replacement and of new acquisition. We will argue that rates of public building acquisition are not independent of village choices or recruitment efforts at the margin between public and private aid; and these choices are affected by the local socio-political situation. However we note the overall aid process officially only ends in April 2009. In mid-2007 BRR officials characterized the aid process as being largely complete; and most remaining BRR activity would be focused on evaluation. But we do expect between 2007 and 2009 there has been more construction of public facilities so we may be looking at interim choices.

3. Sketching a conceptual framework for public good choices (incomplete)

We outline a simple framework, where villagers must decide public labor contributions, lobbying for private capital, and choice of village head in a citizen-candidate election paradigm. We start with the situation where a village has an existing village head not subject to re-election during the aid process and then add in elections. Note this framework is not a general political economy framework, rather one tailored to thinking about what happens if elections occur at the same time as a massive aid effort. We model villager utility as the sum of income received and a function $B(\cdot)$ relating to the utility benefits of volunteerism. Villager i 's income is produced with private labor where labor endowment is 1, private capital and public goods. In turn, public goods are produced according to a function $g(\cdot)$ based on aid allocated to village public capital, K_g and total volunteer labor inputs of villagers, $\sum_j g_j$ (which detract from private labor inputs). Private capital for a villager k_i is villager i 's share of village private aid capital,

K ; and the share is a function of villager relative lobbying effort $s(l_i) / \sum_j s(l_j)$, where lobbying also detracts from private labor inputs.

The cost of lobbying has a fixed component $\gamma \geq 0$, the cost of setting up a lobbying relationship with a specific person, and a variable component l_i indicating effort at lobbying, for a total input cost of $\tilde{l}_i = \gamma + l_i$. A villager's utility, $U_i(\cdot)$, is then

$$w[(1 - \tilde{l}_i - g_i), K \cdot (s(l_i) / \sum_j s(l_j)), g(K_g, \sum_j g_j)] + B(g_i, \bar{g}_{-i}, \hat{e}; \theta, SC) \quad (1)$$

The $B(\cdot)$ function is increasing in g_i , so that, apart from the opportunity cost, there is enjoyment from participating in volunteer efforts. That enjoyment may be affected by social pressure and participation of others, characteristics of the village head (θ), effort (\hat{e}) of the village head to rally participants, and village social capital, SC . For the discussion at the moment, we don't add villager heterogeneity. Such heterogeneity could be critical to modeling longer term within village divisions created by elections, as well as differences in desired levels of public goods.

When we introduce elections, suppose there are two candidates at this stage. The villager has to decide how much to lobby each, in a context where, say, private aid allocation decisions will be made immediately upon selection of the new village head. If we have candidates 1 and 2, with respective probabilities of being elected of p_1 and $p_2 = (1 - p_1)$, then the villager's preference specification becomes

$$p_1 \left(w[(1 - \tilde{l}_{1i} - \tilde{l}_{2i} - g_{1i}), K \cdot (s_{1i}(l_{1i}) / \sum_j s_{1j}(l_{1j}))], g(K_{1g}, \sum_j g_{1j})] + B(g_{1i}, \bar{g}_{-1i}, \hat{e}_1; \theta_1, SC) \right) \quad (2)$$

$$+ p_2 \left(w[(1 - \tilde{l}_{1i} - \tilde{l}_{2i} - g_{2i}), K \cdot (s_{2i}(l_{2i}) / \sum_j s_{2j}(l_{2j}))], g(K_{2g}, \sum_j g_{2j})] + B(g_{2i}, \bar{g}_{-2i}, \hat{e}_2; \theta_2, SC) \right)$$

Note the village does lobbying pre-election, incurring two variable l_1, l_2 and two fixed costs 2γ ; while the rest is decided post-election.

Assuming symmetry where villagers are identical and the 2 candidates are identical with the same probability of winning, same θ , and same post-election choices, it is easy to establish some baseline possibilities. If we assume, say, a well-behaved log

linear specification, compared to a village with no election, in a village with elections: (1) total lobbying cost is higher; (2) public labor inputs are lower and (3) in both regimes, lobbying increases and volunteerism decreases as social capital or village head effort declines..

The other side of the coin concerns the behavior and selection of the village head. We might think of a potential village head receiving a fixed income, y , plus monies he siphons off from aid, faced with m aid agencies. Siphoned off money is the gap between aid received, $\sum_t^m A_t(e_t, \theta)$ which is a function of his effort, \bar{e} , and characteristics, θ , and his allocations to village private and public capital K and K_g . The village head has a cost of effort $f(\sum_t^m (\kappa + e_t), \hat{e})$, $f_1, f_2 > 0, f_{11}, f_{22} > 0$ in securing aid. And he has receives benefits from enhancing the utility of villagers and from siphoning off less of the aid money, in a function $P[\sum_t^m A_t(e_t, \theta) - K - K_g, \sum_j^n \omega_j(l_j)U_j(\cdot)]$, $P_1 < 0, P_2 > 0$. It seems straightforward to construct examples where the $\omega_j(l_j)$ function and the $s_j(l_j)$ function in (1) are consistent. Thus the village head's preferences are given by

$$y + \left[\sum_t^m A_t(e_t, \theta) - K - K_g \right] - f \left(\sum_{t=1}^m (\kappa + e_t), \hat{e} \right) + P \left(\sum_t^m A_t(e_t, \theta) - K - K_g, \sum_j^N \omega(l_j)U_j(\cdot) \right) \quad (3)$$

Once elected the village head chooses $K, K_g, \hat{e}, \bar{k}, \bar{e}$.

Villagers have preferences over village head candidates, which depend on the candidates' θ 's and the associated post-election choices of both villagers and the candidate if he wins. So high θ 's (ability to deal with NGO's) might be associated with increases in aid. The challenges lie in modeling what villager(s) run for election and modeling how elections might create more permanent tensions or divisions in a village.

4. Social and public goods participation and village institutions

The previous section suggests that activities which might be construed as having a lobbying component will go up in the face of an aid effort, while those that have a public flavor will go down. We look at the basic patterns in our data. We start with social and

political participation and contributions to village public goods. Then we turn to a description of political institutions.

4.1 Social and political participation and contributions to village public goods.

Social capital. For “social capital” measures, or activities representing investment in village networks we have two main items in our data. First rotating saving and credit associations (RoSCA), called arisan groups in Indonesia, are popular. 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. While, the original theoretical work on such associations (Besley, Coate, and Lounsbury, 1994) emphasized that they existed to alleviate credit market imperfections, empirical work suggests a strong social component, with participation rising with wealth and complementing (in terms of correlations) rather than substituting for credit institutions, at least in Indonesia (Varadharajan, 2004). We view the existence of arisan groups in a village as a measure of social capital, indicating cohesiveness. In 2005, an interesting aspect of fieldwork was that, in a context where many females spouses died, male villagers identified women as the “glue” which held villages together and provided purpose in life. Similarly, with a social component involving females, but with more social pressure to participate, are Quran recitation groups. In Table 4 we look at pre and post-tsunami participation in these two activities.

Table 4 shows the number of villages reporting the existence of arisan groups from village level surveys. Second it reports individual pre-and post tsunami participation on a matched sample of households reporting on pre-tsunami participation in 2005 and post-tsunami participation in 2007. For the latter, we report for the whole sample, based on fishing families where generally the male household head survived. However many wives who would have participated pre-tsunami in arisan groups died; and most household head have since remarried. Thus we also look at a sub-sample of households where the wife survived and there is an adult female present in 2007. Both the number of arisan groups and participation in arisan groups drops, although only significantly so for the sub-sample.

For Quran recitation groups, most villages claim to have such a group pre and post tsunami. However post-tsunami participation of households falls dramatically, from

almost universal participation pre-tsunami. For the overall household sample, the change is significant at the 5% level and for the sub-sample of surviving wives at the 10% level. **“Lobbying”**. For measures of political participation which could be construed as measures of efforts to garner resources, we are more limited. For fishing boat owners in 2005 versus 2007, we know their participation in the regularly scheduled “Friday meeting” in which affairs of the local fishermen’s association, including potentially boat aid and reconstruction of mangroves, are discussed. In Table 5 we show that such participation rises, although not significantly. Second, at a village level, we know about participation of fishing captains as representatives in higher levels of the fishermen’s association, Panglima Laot [PL]. Villages can have representatives at the lagoon level (covering several villages), district level, and or provincial level. PL was critical in the distribution of boat aid and formulation of NGO policies in the distribution of such aid. Greater village participation in the higher levels of the PL could generate significant gains in private aid received for the village as well as for the individual participants. For 102 villages with information in 2005 and 2007, we see that, both the mean and median number of representatives at higher levels of PL rose significantly. Finally, we know attendance at the last village hall meeting, but we have no pre-tsunami information. We can only comment that rates seem high.

Volunteerism. A key outcome in this study is (Islamic) volunteer days, where the village head calls so many volunteer days for the month and then households decide whether to send an adult male member to participate. Table 6 gives some basic numbers. The number of villages regularly calling volunteer days each month declines dramatically post-stunami, as does the average number of days called per month for those having volunteer days. We didn’t ask individuals to recollect their pre-tsunami participation; but we note post-tsunami, 48% of our matched households participated in volunteer work in the prior month.

Summary. The raw data are consistent with our hypotheses. Participation in social group activities falls; lobbying-type activities increase or at least seem very high; and public good inputs in the form of volunteer days decline. A more nuanced look at the effects of political, social capital and aid differentials on public good inputs across villages awaits econometric analysis below. Before proceeding, we turn to village politics.

4.2 Local politics

Elections in Aceh pre-tsunami were problematic because of the secessionist movement of the last 25 years. The movement and army reaction delayed the local democratic reforms that occurred earlier in other parts of Indonesia. Some basics on political institutions in Aceh are given in Table 7a, and indicate some issues. For example, while 78% of villages now report they use direct democratic vote (universal suffrage with secret ballot) as they are supposed to, that still leaves 22% which use another election method such as voting at a village hall meeting (in which women are less likely to participate) by either secret ballot or, worse, by a show of hands. Moreover, 27% report changing their election method post-tsunami. Although under reforms all villages are supposed to have elections (on a differential schedule) every 5 years and there is a mass point at 5 years, there is an equal size one at 6 years and a smaller one at 8, with numbers ranging up to 20 years. Indonesia has gone through changes in election cycle timing; so, while 5 years is the current standard, in the past 6 and 8 years have been standards and villages may be slow to adjust. However 37 villages have had no election since 1999, beyond any notion of a regular democratic cycle. Elections are generally reported to be competitive, with multiple candidates in the final round. Villages generally have a village council, although they are perceived as larger ineffective and are a council of “elders” (read village elites). Journalist writings suggest that during the secessionist period many of these councils performed no function. Village elected councils with strong powers that are supposed to be part of Indonesian democratization have yet to take shape in Aceh.

We key on three items. First there are villages which had no election during the aid process and ones that did. For those without, these are generally villages with surviving village heads who have not faced an election since 2004. These village heads made aid decisions, potentially paternalistically (corruptly?), not subject to the immediate pressure and scrutiny of an election. In contrast are the two-thirds of villages which had an election post 2004 by the time they are interviewed in the summer of 2007; almost all of these are post 2005. Which villages had an election? In principle if a village head dies, there is a replacement process until the next election. However it seems in the data, that the (exogenous) death of the village head from the tsunami prompts an election (in 87%

of the cases). Then there are villages where the village head survives but faces a scheduled election after 2004 and before mid-2007. The issue raised above is whether exogenous election cycles drive the advent of these elections, or whether there is also an element of elections being prompted by aid disputes. For surviving village heads, 60% have an election in the 2.75 years after the tsunami, which seems like an appropriate rate on a 5-6 year cycle. Table 7b shows a simple probit, where, after standard controls used in later econometric work (which don't significantly affect the probability of an election), village head survival significantly reduces the chances of an election, while being on a 5 or 6 year election cycle significantly increases it.

Second, as Table 7a reveals, surviving village heads who face re-election lose in 81% of the cases. That is a phenomenal rate of loss for incumbents in any election process. This indicates the desire of villagers for a new type of leadership, post-tsunami. Third and related, from Table 7c, new village heads have dramatically better education than those surviving heads who are still in office. Overall, only 41 of surviving village heads who are still in office have a high school degree or more. In contrast and significantly higher, 72% of village heads who are new post-tsunami have completed at least high school, with little difference between village where replacement is prompted by death of the original village head or by election cycles.

5. The determinants of volunteer labor

In our villages we have two types of volunteerism, which are observed. First and foremost are (Islamic) volunteer days, which are called by the village head for purposes of using volunteers to provide public goods and public investment. We observe these in two ways: the number of volunteer days called at the village level in the month prior to the 2007 survey and, for individual families in 88 villages, the number of volunteer days devoted by the family in the past month. Counts run from 0 to 8. In both cases, we looked at the problem both as a discrete choice over days versus no days and as a count. Results are similar but we use the count formulation because it is more informative. We focus on the village level results but also look in the Appendix at individual family choices. The other type of volunteer labor occurred in the early months following the tsunami. A

number of villages started to first rebuild their mosques, using village volunteer labor. We examine which villages did this and which did not.

5.1 Volunteer days called by the village head

We start with a basic count formulation which models the expected number of volunteer days called per month in village j , λ_j , as

$$\lambda_j = \exp[X_j \beta], \quad (1)$$

a form which is convenient for defining elasticities. X_j are village characteristics including measures of social capital which might enhance village cooperation, aid received which could have the adverse effects, and politics and indicators of “lobbying”, as described below.

A key issue conceptually, although not in terms of actual results, concerns the sample of villages used. Of the 199 villages, there are 2 villages with missing data on key variables (government measures of the count of households before and after the tsunami); and a third village which is dominated by army housing and we exclude it since at times it is hard to tell what numbers relate to indigenous villagers, versus army residents. Of the remaining 196, 8 villages in our sample experienced no house damages or loss of life, although some lost some boats. They typically face very different circumstances, like having swelled populations including refugees and displaced relatives. We do not include them in estimation; but report results with them added back in. We also have another 12 villages which report house destruction but report no permanent house aid. These villages have unusually high levels of “temporary” housing. In general temporary housing is wood housing; by now most temporary houses have either been torn down and the wood used for other purposes, or converted to things like storage sheds. However, we believe there may be a dispute between BRR and these specific villages over what is truly temporary versus permanent; for example wood houses with a cement foundation are viewed as permanent. Results on volunteer days when we exclude or include these 12 villages are very similar and we generally include them. Finally, for survival rates and post-tsunami number of houses in a village, we use our 2005 survey data for the 110 villages for which we have it. For villages new to the survey in 2007 we use government data but exclude 7 outliers for which the 2006 PODES counts of households are non-

credibly low. These villages distort any results related to the extent to which a village has been serviced by aid (in fact yielding “false positives”). We also footnote basic results for the whole sample.

We start by treating our determinants of volunteer days as exogenous. Survival rates, location, and size type variables seem plausibly exogenous to current volunteer days called by the village head. Our key measure of social capital, apart from village survival rates is whether the village had an arisan group (RoSCA) pre-tsunami which applies to 68% of villages, along with the village household survival rate. Obviously having had an arisan group pre-tsunami is a crude proxy for pre-existing village social capital, which would be correlated with other unobserved measures of social capital which might influence village cohesion and the inclination and ability of the village head post-tsunami to call and utilize volunteer days. But it is the one measure we have where there are cross-village differentials.

The next issue is whether aid measures are exogenous to volunteer days. There are two parts to a response. First, most aid in our villages is complete by the time of our survey, so we are asking whether pre-determined aid activity affects volunteerism, not whether the arrival of aid today has a temporary effect on volunteer days. Second and more critically is the issue of whether villages with, say, better unobserved inherent tendencies to volunteer days attract more aid. House and boat aid in regression work seems uncorrelated with any observed measures of leadership survival, social capital, and political circumstances. Most aid seems driven by observables connected with need (e.g., boats and houses lost and families remaining) and supply conditions (like access of NGO’s to the village), with a random component of “luck”—what aid agencies landed where. That is a “nicer” village, a more social village, or a village which has better or consistent leadership doesn’t get more private aid, or a better type of aid agency. Aid happened and now we see the following effect on volunteerism. Third, the prime measure of aid is not counts of housing or boats, although we look at these. Rather we measure aid by the number of projects (“first level implementers”) approved by BRR that a village has garnered. This is a measure of the number of separate activities, with which a village head has to contend and the array of on-the-ground implementers parading through the village.

We start with a basic formulation that emphasizes the role of social capital, add in the effects of aid received, and then turn to the impact of political processes.

5.2 Results

Results on counts are in Table 8a. There is a set of basic controls which we use throughout—whether a village is coastal; whether it is in an “urban area”; and what its post-tsunami number of houses was in 2005. None of these are significant.

5.2.1 Social Capital.

Results on social capital before the introduction of aid covariates are in column 1 of Table 8a. Our key index of village pre-tsunami social capital is whether the village had an arisan group or not; that has a strong positive effect, raising the number of days called by 52%. Second, higher 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 12%.

Third concerns whether the mullah survived or not. Based on what we saw in 2005 in the field and in surveying we thought there would be a strong role for survival of traditional village leaders, who provide continuity and enforcement of traditional social conventions, relevant to volunteerism. Foremost is the mullah, who plays a central role in village spiritual life in a context where most men go to mosques regularly and most women attend Quran recitation groups. After all, although called by the village head, volunteer days are “Islamic” volunteer days, enshrined in interpretations of the Quran. Survival of the mullah raises volunteer days by 29%. However the coefficient in different formulations can be statistically weak; and, as we discuss below, there are issues in the data with correlations among survival rates for the village overall, the mullah and the pre-tsunami village head.

5.2.2 Aid

We have two dimensions concerning aid. First as discussed above, there is the official count of different aid projects in a village, our measure of the overall level of aid for the village. The mean is 30 per village and the maximum is 76. Second concerns whether the major housing provider in the village is a donor-implementer. We believe donor-implementers are NGO’s that solve some of the agency problems among donors, implementers and sub-contractors, facing all donors. Having a donor-implementer, we

believe (1) mitigates disputes within the village between either villagers or villages and NGO's and (2) presents an NGO to the village that will be aware of and responsive to village conditions and (3) reduces villager monitoring costs involved when there are shoddy or fly-by-night contractors who start and never finish jobs. As we can see in column 2 of Table 8a, having more projects reduces volunteerism. One interpretation is that more aid means villagers devote more time to trying to enhance private gains from that aid. But it could also be a "lobbying effect"—more projects mean individuals waste more time in a social sense, clamoring for more, as we discussed in Section 3. It might reflect an employment effect where more projects mean more temporary employment opportunities, drawing away from volunteerism. We note that in Table 8b, we add a control for the 14% of villages where village hired labor was used to some degree in housing aid provision. As an aid issue, using village paid labor does reduce volunteer days by over 40%. But adding that control leaves the coefficient for number of aid projects unchanged.

The negative effect of having more aid projects on volunteerism is more than offset for a typical count of projects in a village, if the housing provider is a donor-implementer. Clearly the form in which aid is given has a major impact on village public labor inputs. A donor-implementer may free up time and energy for volunteerism, by reducing village squabbling and time monitoring, given mitigation of the agency problem between donors, implementers and contractors.

In addition we control for whether a village has received permanent house aid or not (as opposed to so-called temporary aid) and whether the village has received boat aid (about 69%). The former has no effect while the later has a big negative effect. After the tsunami, villages had a choice of whether to return to fishing and lobby for boat aid, or at least for now to turn to other employment, such as in the aid sector. Fishing villages in Aceh also face the issue of adaptation of new technologies. Much of the fishing has been "primitive", catching reef and other off-shore fish for local consumption. The tsunami heavily devastated the feeding and breeding grounds for these fish and their catch is a tiny fraction of what it used to be. More profitable fishing is using bigger boats capable of over-night fishing far from shore for tuna and shark. To get boat aid requires village investment in lobbying, documentation, and monitoring of boat quality. Boat aid

generates disputes over who disperses aid (village head versus elected leaders of the local fishermen's association) and who gets aid, for example among former owners versus crew. Regardless, villages getting boat aid exhibit less volunteerism. However, as footnoted in the table, for both house and boat aid, the extent of aid provided in boats and housing has no additional effect on volunteerism, controlling for our measure of number of projects in the village. Finally we note that in contrast to public building below, we find no interactive effect between the measure of overall aid and political or social capital measures. That is the effect of aid on public labor inputs is not mediated by politics and social capital.

5.2.3 Politics.

To both column 1 and 2 of Table 8a we add a variety of political controls, as shown in Table 8b. We start with what will be a prime consideration: whether the village had an election post-tsunami. Having an election significantly reduces volunteer days by 25%, an effect that is almost significant at the 5% level. As discussed in Section 3, elections take people's time and involves more lobbying; and elections may heighten conflict and social differences within the village, reducing the incentive to volunteer. We think this is the reason for the election effect on volunteerism. One alternative is that elections bring in new types of village heads who simply desire fewer volunteer days. We lean against this explanation because of the following correlations in the data.

First if the old village head remains in office without an election that has no effect on volunteer days (where we note there 14 villages with a new village head and no election), so this type of leader as opposed to elected or new leaders has no effect on volunteerism. Second, a measure relating to village head social capabilities or preferences, the village head having a high school degree, has no significant impact on volunteerism. The education effect is negative as we expected: more educated village heads may be more separated from the typical villager. However not only is the variable not significant on its own, it is weaker when the election variable is added in; and effects remain completely insignificant with the election variable added on and interacted with education. Measuring education by a scalar roughly proportional to numbers of years of education produces even weaker results. Finally the effect of survival of the pre-tsunami village leader is insignificant. It is also negative for which we have no explanation. We

footnote the fact that once we add in village head survival, we have three correlated measures of survival: mullah, village head, and overall. We explored interactive effects among these three survival rates and concluded that the results as specified in the main part of the table are probably the best we can do for the sample size. But the footnote suggests that there may be some positive interactive effect on volunteerism if both the mullah and traditional village head survive.

In terms of interpreting the effect of elections on volunteerism as occurring because of village tensions and elections lobbying, Table 8c and 8d point out several facets of the relationship, foremost of which is its robustness to increasingly nuanced specifications. First in Table 8c, the election effect seems to interact with social capital. Villages without an arisan group pre-tsunami that have an election have huge reductions in volunteerism, 74%, while the role of enhanced social capital seems to offset this negative effect, to dampen the generation of conflict prompted by an election. Second, if we break the elections by year, 2005, 2006 and 2007, the effects, whether for elections alone or for elections interacted with arisan group effects are basically the same in each year. This does suggest these election effects are persistent, with the divisions they created persisting over time. Third in Table 8d, we look at the effect of elections which occur in villages where the old head survived versus died. In column 1 it looks like effects are stronger in villages where the head survived and “had to be” removed from office. However as the next column indicates this is mixed in with the non-robust effect of village head survival per se. Controlling for that, the effect on volunteerism of elections and the offsetting effect of arisan groups seems to be similar in villages where the head survived versus died. Finally, in Table 8b, we note in terms of the issue of conflict and energy, having aid allocation decisions made in village meetings reduces volunteerism, although the effect is small and not significant.

In Table 8b we looked at two other issues. One we have already mentioned: the idea that village employment opportunities offered by aid agencies can detract from volunteer days. A common complaint in some villages is that with NGO’s employing village labor, not one wants to do anything “for free”. Second we wanted to see if volunteerism and public capital are substitutes or complements. While this issue needs to

be explored in more detail, the last column of Table 8b suggests no correlation with the public capital measure used in Section 6.

5.2.4 Politics and IV Estimation.

We have treated elections as occurring exogenously. But we know some elections were not prompted by either the death of a village head or exogenously scheduled timing, but by disputes. Disputes themselves could affect volunteerism, biasing the election coefficient downward (more negative), meaning that it may be more that disputes over aid rather than elections per se were inhibiting volunteerism. Given that we experimented with instrumental variable estimation. We used a GMM estimator (Windmeijer and Santos Silva,1997) where we define $v_j \equiv VD_j - \lambda_j = VD_j - \exp[\pi_j]$ and base estimation on the non-linear moment condition

$$E [v_j | Z_j] = 0 . \quad (2)$$

With an endogenous dummy variable, as an instrument, we use the predicted value of the probability of elections from a first stage.

The main issue is how to instrument for elections. In Table 7b we noted that the probability of having had a post-tsunami election is higher when (1) the village head dies (2) the village is on a 5 year elections cycle or (3) it is on a 6 year election cycle. We also note that the probability is higher when (4) the village had a village hall pre-tsunami potentially reflecting a more democratic tradition of assemblage and (5) when the village experienced land ownership disputes, where disputes might increase chances of an election being called. .

Whether the village head survived the tsunami as explained above may well not meet the exclusion restriction. For the rest, on the surface statistically, they look orthogonal to the error term. When added to a Poisson model for volunteer days, the coefficient on the election variable is unchanged (-0.23 as opposed to -0.25). If both the election variable and instruments are correlated with the error term, adding the instruments should have a noticeable effect on the election variable coefficient. It doesn't. However all these instruments are suspect.

Being one of the 69% of villages that historically had a hall may not be independent of any persistent tendencies to volunteerism in the village not captured by

the arisan variable. For the 20% of villages with land disputes, they arose in the early days after the tsunami before permanent aid arrived when villagers engaged in a collective exercise to walk and mark-off landholdings. We could treat these disputes as not aid related and idiosyncratic, an accident of the extent of tsunami destruction of visible markers in the village or greed of 1 family, with no reflection on overall village spirit per se. But that may be a stretch. Finally even for election timing; we can't rule out the possibility for some villages that the prior election cycle was longer in a village and aid disputes prompted an up-dating to the current 5 year national standard.

We tried two experiments. First, we excluded village head survival as either an instrument or covariate and used the remaining four variables as instruments for having a post-tsunami election. They have a first stage F-statistic of 8.5. Second we added village head survived as a covariate and instrumented just with the 5 and 6 year election cycle variables; those instruments have a first stage F-statistic of 8.8. In the first case, the coefficient (s.e.) on having had a post-tsunami election is $-.36 (.36)$; and in the second it is $-.25 (.49)$. Instruments are fairly weak so standard errors are large; and results are not particularly robust (in the second exercise adding village hall pre-tsunami as an instrument doubles the coefficient to $-.52$). Nevertheless the estimated coefficients are not dissimilar to ordinary Poisson estimates of $-.24$.

5.2 Individual volunteer days

Besides village level data, we also have a record of days volunteered by the family last month for 544 fishing families in 88 villages, all of whom owned a boat pre-tsunami. In Appendix A, we focus on a version with village fixed effects which covers 420 families in 60 villages; the loss in sample is from villages where no one volunteered in last month or everyone volunteered the same amount (so there is no within village variation), implying for most excluded villages that no volunteer days were called. Thus, with fixed effects, we look at the margin of family participation in villages where volunteer days have been called by the village head. We don't find a lot that is of great interest. Bigger families volunteer more. Those who have received a house on aid volunteer less; those few who have yet to get a house may volunteer as a form of lobbying with the village head to up their chances of getting a house sooner. In contrast, those who get a boat on aid volunteer more; those who don't get a boat are typically

pursuing work outside the village in the aid sector, driving vehicles, doing construction, and working in the service sector focused on services to aid workers. While the sample of villages is small we run also a version with village level covariates to see if the covariates matter at the village level (decision to call volunteer days by the village head) also matter at the family level. That involves two margins—whether the village head called volunteer days and then whether this family participated. For individual level variables, that creates noise, since a family can't participate if the village head didn't call days. As noted in the Appendix, village level covariates with the limited number of villages and weight towards village with high survival rates are not strong, but the key aid, social capital, and political variables have the same sign as in Table 8.

5.3 Volunteer days to rebuild the mosque

A second dimension of volunteerism in the data is whether the village used volunteer days to rebuild the mosque. Almost universally, this is a 0% versus 100% split—no volunteer or all volunteer labor. But there are some villages which report a partial, and so we create a dummy for whether the mosque was rebuilt using over 50% volunteer labor. 26% of villages report the use of this volunteer labor. Looking at volunteer labor to rebuild the mosque involves an unusual phenomenon. Such activity occurred mostly in the 2005 before any permanent aid arrived in the village. Once such aid arrives, it becomes apparent that there are aid agencies willing to pay people to rebuild the mosque or to hire outside workers. Thus the split is whether a village rallied in the months after the tsunami, to focus on rebuilding the mosque (noting volunteer labor is not in evidence in the building of houses or generally other public buildings); or whether people in villages either did nothing or engaged in “work-for-pay” programs designed to give people something to do (clear rubble, drive vehicles, etc.), thus putting aside rebuilding the mosque. For the empirical formulation this has implications. First, including aid variables is pointless—almost by definition more aid programs will imply the village did not proceed with volunteer labor to rebuild the mosque. Second the variables on elections and village decision making on aid allocation also are not relevant—these involve later events. Thus such variables are all insignificant. Apart from whether a village is urban (so there are many nearby alternative inland mosques to visit), the only observable that matters is social capital.

Results are in Table 9. We focus on the sample of 186 villages which had a mosque that was destroyed by the tsunami. What influences the probability is whether the village had an arisan group pre-tsunami, with a marginal effect of 16%. Again this social capital indicator plays a key role. Whether the mullah survived (not shown) has a positive effect; but its coefficient is only .076 and the standard error is .065. The overall survival rate presents a similar situation. We note that in the disaster literature, more trauma (survival) has two opposing effects on immediate village responses—individual depression and a rallying around a common cause by survivors.

6. Public buildings

In this section we make an initial attempt to examine the determinants of village decisions to garner public versus private aid. We examine the ratio of total (non-mosque) public buildings received to the count of houses received in aid. An alternative is to have the dependent variable be just the count of public building and then control for all types of private aid received; the endogeneity issues are obvious and we have no instruments that would reasonably meet the exclusion restriction. So the problem is posed as a choice over the ratio and we ask how this margin is affected by our covariates. For this we look only at villages which have received permanent house aid; but we no longer exclude village with poor counts of post-tsunami households since that variable doesn't affect outcomes and results for the two samples (with and without outliers) are basically the same.

The formulation we use is simply

$$\text{count public bldg-in-aid/houses-in aid} = X_j\beta + \varepsilon_j, \quad (3)$$

which is estimated by OLS. We also did a Tobit specification given about 12% of villages have gotten no public building aid; results are almost the same as OLS. We show results with a basic set of controls in Table 10a. These include the count of households post-tsunami, public buildings destroyed, the social capital variables from Table 8a in column 1; and then, in column 2, we had in the overall aid measures from Table 8a. None of these except being an urban village with nearby substitutes for own public buildings seem to affect the choice between public and private aid and the explanatory power of the model is negligible. Similarly when we add on variables such as the mullah and village survive or not (and those interacted) and political variables on their own from Table 8b,

nothing affects the trade-off. The ratio of public to private aid seems neutral with respect to many observables, perhaps driven by aid agency policies.

However in Table 10b, once we allow for interactive effects between the overall level of village aid and politics, distinct patterns emerge. To interpret the table we focus on the set relating to whether aid allocation decisions are decided in village meetings. Having aid allocation decided in village meetings leads to more public buildings, but that effect diminishes as the number of aid projects increases. In fact only at low levels of projects (less than 25, for a mean and median of 30) do villages where decisions are made in meetings have a higher ratio of public buildings to private. At the median of aid projects, the total effect for villages where decisions are not made at a village meeting is .0126, while for those where they are made in a village meeting it is lower at .0098. The differential suggests that villages where decisions are not made in a village meeting have 1/10 of a standard deviation higher ratio of public to private aid (the mean and standard deviation are .016 and .028). When the number of aid projects hits 49 (the 90th percentile for aid project counts), villages where decisions are not made in a village meeting have a ½ standard deviation higher ratio of public to private aid buildings. While there seems to be a base positive effect of village “collective” decision making in aid allocation, the effect withers quickly as the number of aid projects rises. In villages with lots of projects, the slant is distinctly towards private aid under “collective” decision making.

The results for having had an election are weaker but the pattern under this form of “greater democracy” is similar. At the median number of projects, villages with and without an election have the same ratio, *ceteris paribus*. By the 90th percentile of villages in numbers of projects, villages with no election have .22 of a standard deviation higher ratio of public to private aid buildings. Correspondingly, in the last row, “non-democratic” villages with the same village head as pre-tsunami with no election have a higher ratio of public aid when the number of aid projects gets large, but estimated effects are smaller than the greater democracy variables. At the 90th percentile for total projects, the effect is .17 of a standard deviation.

7. Summary (To be completed)

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Figure 1. Map of Survey Area

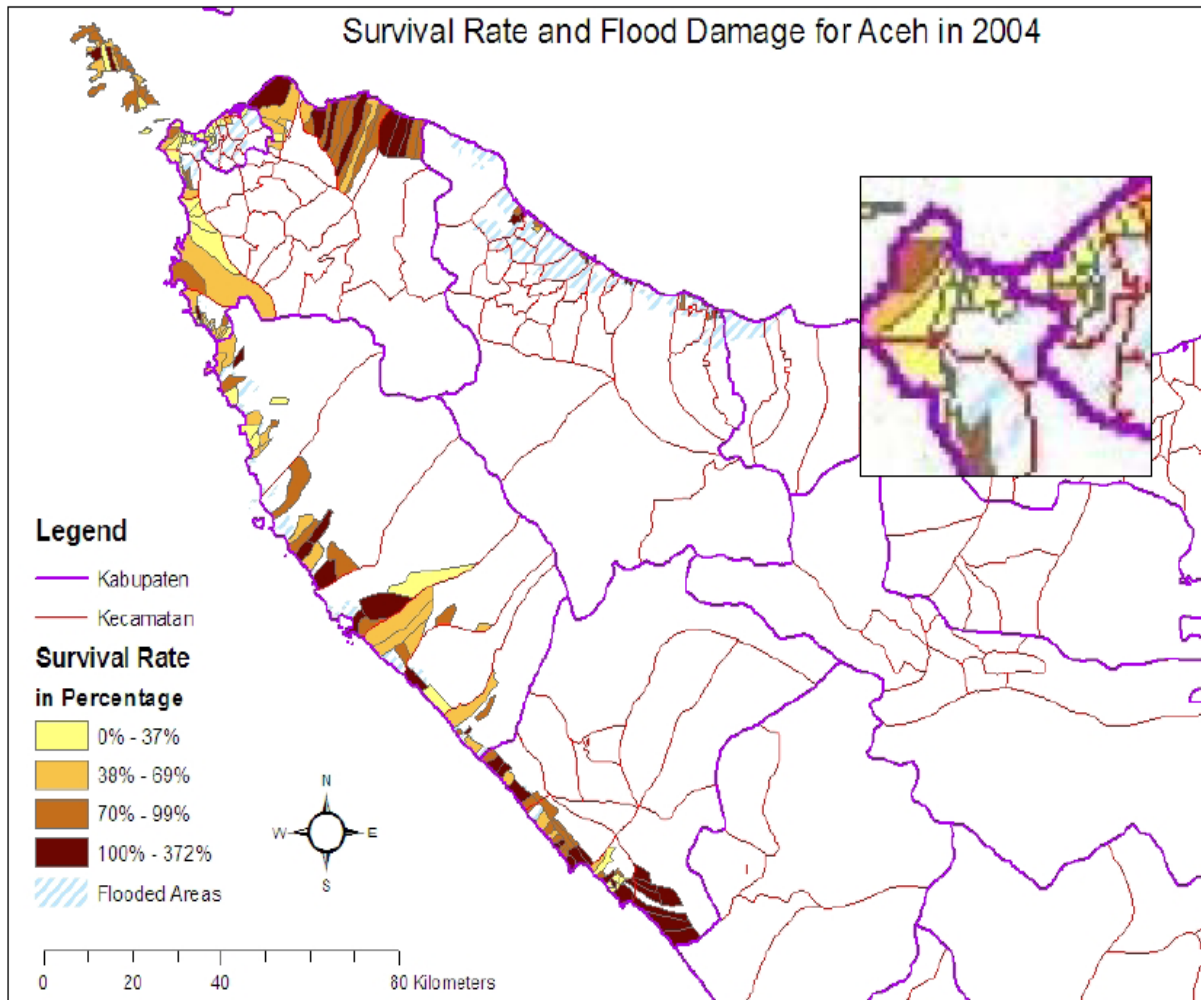


Table 1. Destruction of population and housing

	N=104 ('05 sample)	N=190 ('07 sample)
Survival rates		
Pre-tsunami households ['05 survey, ('07)]	28829 (29702)	58882
Post-tsunami households ['05 survey]	16698	n.a.
Post-tsunami households (PODES) ^a	14775	32628
Survival rate: households^b	58% ['05 survey]	55% [PODES; '07 survey]
Survival rate population ^c	45% (official: 49%)	
Post-tsunami female/male ^d	“.79”	
House aid		
Number of houses survive tsunami ['05 survey ('07)]	2290 (1987)	5299
Survival rate houses	8%	9%
Number of temporary houses built ('07 survey)	3039	6609
Number of permanent houses built ('07 survey)	15506	32194
Replacement rate (base: '05 count of hh's, no house)^e	108% [124% for PODES count]	118%
“Exaggeration”		
2007 number of households claimed [all] ^f	23161	49037
“Rate of increase” since tsunami	39% [57% for PODES count]	50%.

- a. 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 lower counts of households compared to our 2005 survey (Summer and Fall, 2005) may be partly a “9/11 phenomenon”; as time goes on more missing families are discovered.
- b. Column 1 is our 2005 pre (recall) versus post tsunami count. Column 2 is the PODES post-tsunami count over the count recalled in 2007 for village pre-tsunami households.
- c. The official survival rate is the 2006 PODES count divided by the count in 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.).
- d. Villages generally seem to under-count children, and perhaps more so female children, in attempting age-sex breakdowns, so that the numbers used in breakdowns sum to less than totals.
- e. The replacement rate is the number of houses given in aid divided by the number of surviving households (2005 survey) less the number of surviving houses. We also report counts where the number of surviving households is from the PODES in column 1 and the column 2 count is based on that.
- f. The rate of increase in column 1 is the 2007 claims of village households divided by the 2005 claim. For 2007 it is the 2007 claim divided by the 2006 PODES count.

Table 2. Aid Effort—Diversity and Specialization

	N=198
Avg. (median) no. of different NGO’s per village in provision of housing, public buildings, boats, seawalls, mangroves, embankments	7.2 (7)
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 <= 3 NGO’s from our survey	9
No. of villages => 10 NGO’s from our survey	30
Total no. of different NGO’s from our survey	117
Total no. of different housing NGO’s from our survey	57
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 majority housing provider is “donor-implementer”	69
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 damages based on 2005 information), and when housing is destroyed unusual levels of “temporary” housing. We suspect and will confirm in the field that there is a fight with BRR and NGO’s about what is temporary versus permanent.

Table 3. Public Buildings

	Total pre-tsunami	Total destroyed	aid	No. villages: facility now / none before	No. villages: facility now / had before and destroyed^a	No. villages: got facility/ had before & no destroy
Mosques	395	325	243	0 / 2	127 / 144	7 / 23
Public bldgs	501	463	389	n.a.	n.a.	n.a.
Village hall	136	130	84	29 / 63	83 / 130	1 / 6
State elem. school	118	107	88	2 / 80	84 / 107	4 / 10
Dayah	71	63	50	3 / 126	43 / 63	4 / 8
Health facility	112	106	76	28 / 86	74 / 106	2 / 5
Fishermen’s hall	64	57	21	11 / 135	19 / 57	2 / 7

a. Rows for the break-down of public buildings cover all villages (sum of denominators in last 3 columns is 199 or slightly less if there are missing values). For mosques, villages may have multiple mosques to start and not all are destroyed.

Table 4. Social-cultural activities: social capital

	Pre-tsunami	Post-tsunami	Sample	t-stat. on differences
Arisan group exists	136	123	199 villages	1.4
Belong to arisan group	91	74	387 h.h.'s in 62 villages surveyed in 2005 and 2007	1.5
Belong to arisan group (sub-sample)	51	34	177 h.h.'s; wife survive '05; adult female present in 07	2.1
Quran recitation group exists	176	171	199 villages	.7
Attend Quran recitation at least once a week	339	266	387 h.h.'s surveyed in 2005 and 2007	6.5
Attend Quran recitation (sub-sample)	159	147	177 h.h.'s; wife survive '05; adult female present in 07	1.9

Table 5. Participation rates in meetings

	Pre-tsunami	Post-tsunami	Sample	t-stat. on difference
Usually attend Friday fishermen's meeting	61%	68%	Boat owners: 298 in '05; 207 in '07	-1.6
Average [median] no. representatives in higher levels of PL	1.32 [1]	3.07 [1.5]	102 Villages surveyed in '05 and '07	-3.6 [-4.2]
Attend last village hall meeting	n.a.	78%	387 h.h.'s	n.a.

Table 6. Volunteerism

	Pre-tsunami	Post-tsunami	Sample	t-stat. on differences
Regular volunteer days	199	124	199 villages	11
Average [median] volunteer days per month, if have days	2.66 [3]	2.13 [1]	Pre-tsunami number based on reporting of 193 villages	3.5 [3.4]
Households	n.a.	187	387 h.h.'s surveyed in '05 and '07	n.a.

Table 7a. Post-tsunami political institutions

	Out of 199 villages	Notes
Villages with elections since 2004	130	37 no election after 1999; 53 village head not survive (46 have election)
Pre-tsunami village head survived and in office	71	55 with no post-tsunami election
Pre-tsunami village head survived, not in office	75	16 of 84 who faced post-tsunami election were re-elected
In last election, report more than 1 candidate in last round	160	Of 37 with no election since '99; 11 have no report.
Changed election method since 2004	54	42 had election after 2004
Election by direct vote (secret ballot)	155	78%; (85% for those who change selection method since tsunami)
Have a tuhaput (council of elders)	172	Virtually the same pre-tsunami

Table 7b. Probit: Village has election since 2004 (marginal effects)

Ln (no. households post tsunami)	.011 (.053)
Coastal village	-.028 (.083)
Village in urban area	-.213* (.124)
Survival rate (households post to pre- tsunami)	-.058 (.081)
Village had pre-tsunami arisan group	.045 (.087)
Mullah survive tsunami	.043 (.083)
Village head survive	-.293** (.070)
5 year election cycle	.289** (.081)
6 year election cycle	.367** (.079)
N	182
Pseudo Rsq	.05

Table 7c. Education of village heads, 2007

	Old village head in office			New village head			t-stat. on overall
	Over-all	No elect.	Re-elect	Over-all	Old head survive ^a	Old VH die ^b	
Proportion heads with high school or more	.41	.42	.38	.72	.68	.77	-4.3
N	71	55	16	128	75	53	199

a. Of these, 7 villages have no election (86% have high school); 68 have elections (66% with high school).

b. Of these, 7 villages have no election (71% have high school); 46 have elections (78% with high school).

Table 8a. Volunteer days per month called by village head

	Social Capital	Overall aid^{a,b}
Ln (no. households post tsunami)	-.0020 (.119)	.035 (.129)
Coastal village	.011 (.178)	-.049 (.170)
Village in urban area	.141 (.223)	.131 (.221)
“Social capital” indicators		
Survival rate (households post to pre- tsunami)	.246* (.135)	.289** (.121)
Village had pre-tsunami arisan group	.523** (.173)	.401** (.170)
Mullah survive tsunami	.294* (.167)	.242 (.159)
Aid level and form		
Official number of aid projects in village		-.010* (.0061)
Majority housing provider is donor-implementer		.401** (.138)
Received house aid		.279 (.288)
Received boat aid		-.267* (.136)
N	182	180
Pseudo Rsq	.05	.08

a. For N= 194 with controls for whether a village suffered any house damages and, if so, if it has not received house aid (both insignificant), the coefficients (s.e.'s) for covariates in order as listed in column 2 are -.22 (.13); .02 (.17); .25 (.24); .33 (.12); .43 (.16); .08 (.15); -.009 (.005); .35 (.14); .42 (.29); and -.28 (.14).

b. To column 2 if we add excess house aid (count of houses-in-aid/post-tsun h.h. -1) and excess boat aid (count of boats-in-aid/ pretsunami boats -1) the coefficients (s.e.'s) are respectively -.105 (.159) and .017 (.127).

Table 8b. Volunteer days, politics

(Political factors added to columns 1 and 2 of Table 8a)

Added to column 1 of Table 8a					Added to column 2 of Table 8a	
Village election since '04	VH survive in office, no elect since '04	V. meet decide house allocation	Village head survive ^a	Village head high school or more ^b	House aid hires village labor ^c	Public bldg aid/ house aid ^d
-.243* (.139)	.024 (.160)	-.058 (.142)	-.163 (.177)	-.157 (.211)	-.444** (.183)	1.48 (5.47)

a. When we add an interactive term, the coefficients (s.e.'s) on VH survive, mullah survive and both survive are respectively -.71** (.26), -.21 (.29) and .84** (.36). From that, it looks like VH survival per se is bad and the effect of the mullah surviving is to offset that. But we could think of no explanation for this. The main worry is that we have three correlated survival variables, as well as a somewhat small cell size for only mullah surviving (19). So we tried interacting all three survival rates. The best specification we came up with was survival rate [.73** (.33)]; VH survive [-.30** (.41)]; Mullah survive [-.14 (.28)]; VH survive*mullah survive [.79** (.36)]; and survival rate* VH survive [-.53 (.34)]. An interpretation is that village survival rates are most important in villages where the village head did not survive; and having both the mullah and village head survive is very helpful for fostering volunteer days. But the sample size really doesn't allow us to nail interactive effects among such correlated variables.

b. A linear variable mimicking years of education has coefficient (s.e.) -.035 (.044). Adding the election since '04 variable leaves the result unchanged.

c. In the second row the coefficients (s.e.'s) on number of aid projects and donor-implementer are respectively -.011* (.062) and .415** (.135), unchanged by this added control

d. Drops one outlier so sample size is 179. The measure does not include mosques and fishermen's halls (that coefficient (s.e.) is (.550 (3.89))).

Table 8c Volunteer days, Election timing and arisan group

(added to column 1 of Table 8a)

	1 (base case)	2	3	4
Arisan group pre-tsunami	.540** (.167)	.195 (.261)	.545** (.167)	.189 (.261)
Village elect. since '04	-.243* (.139)	-.743** (.269)		
Village elect. in 2005			-.271 (.218)	-1.11** (.517)
Village elect. in 2006			-.293 (.195)	-.838** (.402)
Village elect. in 2007			-.197 (.162)	-.601** (.290)
Arisan group* Village elect. since '04		.644** (.320)		
Arisan group* Village elect. in 2005				1.05* (.569)
Arisan group* Village elect. in 2006				.673 (.455)
Arisan group* Village elect. in 2007				.541 (.361)

Table 8d Volunteer days, Election timing and arisan group
(added to column 1 of Table 8a)

	1	2
Arisan group pre-tsunami	.532** (.167)	.232 (.236)
Village head survive tsunami		-.749** (.381)
Village head survive* elect. since '04	-.291** (.144)	-.744** (.271)
Village head die* elect. since '04	-.141 (1.98)	-1.15** (.451)
Arisan group* Village head survive* elect. since '04		.651** (.324)
Arisan group* Village head die* elect. since '04		.425 (.382)

Table 9. Volunteer labor to rebuild the mosque (probit for village volunteer labor comprising more than 50% of all labor used): Marginal effects

	Social Capital
Ln (no. households post tsunami)	-.018 (.046)
Coastal village	.011 (.076)
Village in urban area	-.178* (.071)
“Social capital” indicators	
Survival rate (households post to pre- tsunami)	.087 (.078)
Village had pre-tsunami arisan group	.162** (.061)
N	182

Table 10a. Count public-housings-in-aid/ count houses-in-aid (OLS) ^a

	Social	Overall Aid
Ln (no. households post tsunami PODES06)	-.00014 (.0016)	-.0016 (.0017)
Public buildings lost	-.0024 (.0038)	-.0023 (.0034)
Coastal village	-.0099 (.0083)	-.011 (.010)
Village in urban area	-.0061* (.0031)	-.0043 (.0029)
Survival rate (households post (PODES) to pre-tsunami (P4B))	.0046 (.005)	.0055 (.0047)
Pre-tsunami arisan group	.0014 (.043)	.00026 (.0038)
Official number of aid projects in village		.00014 (.00012)
Majority housing provider is donor-implementer		.0043 (.0052)
N	180	178
Rsq	.045	.054

a. For these samples if we replace the first two covariates in columns 1 and 2 by the ratio of public buildings destroyed to post-tsunami households, we get a significant negative coefficient (and a significant F-statistic on the whole relationship). However if we drop two observations with outliers on that covariate the coefficient becomes completely insignificant. Similarly adding in a control for the ratio of pre-tsunami public buildings to pre-tsunami households yields an insignificant positive coefficient.

Table 10b Count Public-bldg-in-aid/count houses-in-aid (Table 12a, col 2 formulation) ^b

Official count aid projects	Election since '05	Election since '05 * off. count aid proj.	V. Meet decide house alloc.	V. M. decide house * off. count aid proj.	VH survive in office, no elect since '04	VH surv. in office, no elect.* off. count aid proj.
.00032* (.00017)	.011 (.0067)	-.00035* (.00019)				
.00042** (.00014)			.014** (.0072)	-.00056** (.00019)		
-.000046 (.00012)					-.015** (.0072)	.00044** (.00019)

b. The regressions in rows 1, 2, and 3 have p-values on the F-statistic [Rsq] of respectively .31 [.061], .066 [.077] and .23 [.065] . If we strip off all the covariates from Table 12a except coastal village, urban village, and majority housing provider is donor-implementer the three p- values become .181, .024, and .088.

Appendix: Volunteerism from family data

Table A presents the basic results with village fixed effects. Family size and receipt of aid have significant effects, albeit with opposite effects for house and boat aid. But many things have little or imprecisely measured effects: working only part time; having a larger boat pre-tsunami and potentially high fishing ability which might detract from volunteerism; and having more education of the household head.

For the full sample of 543 pre-tsunami boat owning families in 87 villages, for village level covariates controlling for the household covariates in column 1, the coefficients (s.e.'s) for village level variables are: ln (no. h.h.'s post-tsunami) [-.15 (.13)], coastal [-.17 (.19)], urban [.23 (.21)], survival rate [-.47 (.21)], artisan group [.17 (.19)], no. aid projects [-.0012 (.006)], donor-implementer [.23 (.27)], and election since '05 [-.11 (.18)]. Note such a model captures both the effect of covariates on a village head calling volunteer days and then people participating. With the reduced sample size and no weighting for village sampling, all village level covariates are insignificant, but they have the same sign as in Table 8 except for survival rate and number of households. However the overall survival rate in this sample is 80% compared to 55% in the general sample.

Table A. Family volunteer days: Poisson

Household size	.161** (.038)	.159** (.036)
Received house on aid	-.546* (.299)	-.502 (.313)
Own a boat currently	.271 (.190)	.260 (.187)
Received boat on aid	.369** (.176)	.356** (.177)
Not work full time		.184 (.154)
Owned large boat (9+ meters)		-.127 (.277)
Junior high school education		.073 (.191)
High school or more		-.264 (.235)
Village fixed effects	Yes	Yes
N (60 villages/clusters)	420	420