

ENTERPRISE RECOVERY FOLLOWING NATURAL DISASTERS*

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Using unique, panel data and a randomised experiment, we assess the effects of relief aid and access to capital on the recovery of Sri Lankan microenterprises following the December 2004 tsunami. Our results show that a lack of access to capital inhibits the recovery process; firms receiving randomly allocated grants recover profit levels almost 2 years before other damaged firms. Access to capital is particularly important for the retail sector; the role of capital in recovery for manufacturing and services sectors may be limited by disruptions in supply chains. Our data show that business recovery is much slower than commonly assumed, underscoring the role targeted aid may play in hastening microenterprise recovery following such disasters.

What seems more typical are the comments appearing six months to a year after a disaster, expressing surprise at the speed with which the community has recovered, and the prosperity that now reigns

(Dacy and Kunreuther, 1969)

A series of catastrophic events in recent years has drawn increased attention of both the public and researchers to the plight of those impacted by natural disasters. The number of natural disasters reported by the press and included in the most comprehensive disaster database is increasing. The Intergovernmental Panel on Climate Change (2007) believes that the frequency of natural disasters is 'likely' to increase as a consequence of global warming. Some argue that the trend results from more complete reporting of disasters;¹ others say any effect of climate change on future trends is uncertain. But there is little disagreement that the impact of natural disasters severely impacts households already living on the margins in low-income countries. The death toll from disasters, for example, is typically much higher in low-income countries (Kahn, 2005).

There is a large literature on how households in developing countries cope with and respond to disasters and aggregate shocks; see Skoufias (2003) for an overview. The poor suffer disproportionately because missing credit and formal insurance markets limit their ability to smooth aggregate shocks. Informal risk-coping strategies that are used to smooth idiosyncratic shocks break down when all members of a risk-sharing

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¹ The trend of an increasing number of disasters shown in the EM-DAT data is likely due in part or in whole to a more complete reporting of disasters. See, for example, the discussion in Strömberg (2007).

group are affected (Morduch, 1999; Lustig, 2000). As a result, transient shocks can have permanent effects, either by reducing the ability of households to provide nutrition or schooling for their children (Ferreira and Schady, 2009; Maccini and Yang, 2009), or by preventing the repurchase of productive assets such as livestock, which are sold to smooth consumption (Carter *et al.*, 2007).

These same market failures that limit the ability of households to recover quickly from disasters are likely to inhibit the recovery of microenterprises. But there has been almost no research on the process of enterprise recovery in developing countries. Even in developed countries, there are only a handful of qualitative case studies. Until recent disasters such as Hurricane Katrina and the Asian tsunami, the conventional wisdom, reflected in the quote from Dacy and Kunreuther's book, was that the economy recovers surprising quickly.²

Based on qualitative interviews with small business owners shortly after Hurricane Katrina, Runyan (2006) concludes that in the US context, insurance is the key to recovery: firms with insurance quickly replace destroyed assets but those without insurance do not, often because business records lost in the flooding and destruction are required to access federal aid. But while about 50% of losses from Hurricane Andrew in Florida and the Northridge earthquake in California were covered by insurance, less than 15% of the losses from the 2004 Asian tsunami were covered (Ferguson, 2006). Moreover, in developing countries micro and small-business owners rarely have insurance to cover natural disasters. In aggregate large government and non-profit aid flows may replace insurance, at least in the largest disasters.³ But do aid flows reach enterprise owners? And is aid alone sufficient to speed the recovery process?

This article provides the first microeconomic study of the recovery of the private sector in a developing country following a major natural disaster. We use firm-level panel data gathered from micro enterprises in southern Sri Lanka following the December 2004 Asian tsunami. In addition to surveys, we implemented a field experiment providing grants to randomly selected enterprises. The grants allow us to assess the importance of capital in the recovery process. This is the focus of the article.

We begin with descriptive data on the extent of losses from the tsunami and sources of funds used to replace or repair damaged assets. The data show that although there was essentially no correlation between damage to business assets and aid flows, the enterprises recovered a surprisingly large portion of damaged assets within three months of the disaster. We then turn to the question of the sufficiency of capital in the recovery process, using data from our randomised experiment. Comparing, first, the effect of grants among firms in and out of the impact zone, we show that, if anything, the capital shocks generated larger increases in profits among firms suffering tsunami damage. Next, we examine whether the grants sped the recovery process. Our counterfactual is the group of firms outside the impact zone that did not receive grants. It is

² Most of the research by economists examining the aftermath of disasters focuses on short-term recovery process, rather than the longer-term rebuilding process (Okuyama, 2003). Analysis of longer-term recovery generally uses aggregate data such as building permits (Dacy and Kunreuther, 1969). Two exceptions using micro-level data are Smith and McCarty (1996), who analyse the demographic changes in southern Dade County, Florida following Hurricane Andrew in 1992, and Dolfman *et al.* (2007) who estimate losses in employment and wages in New Orleans following Katrina.

³ The literature has examined the determinants of the magnitude of international financial flows such as aid response to disasters; see, e.g. Eiseensee and Stromberg (2007) and Yang (2008).

unclear whether capital alone should be sufficient to speed recovery. Where the grants allow only partial recovery of assets, the complementarity of production assets will be important. Moreover, other constraints may also follow from the disaster. For example, supply chains and trading relationships are destroyed, either temporarily or permanently. The time required to rebuild supply chains and re-form relationships depends on the specific sector.⁴ This suggests the impact of the grants may vary across sectors inversely with the importance of supply chains and trading relationships.

Comparing the impacted firms that received grants with those that did not, we find that particularly large impacts of grants in the 24 months following the tsunami. Indeed, the grants appear to have allowed an immediate recovery of the firms relative to the comparison firms. But consistent with the importance of other constraints, we also find that the grants had a significantly larger impact among recipients in the retail sector, and very little effect among those in manufacturing and services. Data limitations and sample size prevent us from unpacking this finding further but the results suggest that cash aid may be particularly effective for retail businesses.

We begin in Section 1 by discussing the extent of damage and the recovery funds provided by insurance and grants. In Section 2, we turn to the process of recovery, discussing a proper comparison sample from which to calibrate recovery. Section 3 is the heart of the article. There, we use the random injections of capital to assess the importance of access to liquid capital – over and above that available from other sources – in the recovery process. Finally, we conclude in Section 4 with a discussion of policies that might hasten the recovery of microenterprises following a major disaster.

1. The Tsunami, Firm Losses and Sources of Recovery Funds

The December 2004 Indian Ocean tsunami produced catastrophic damage along Sri Lanka's eastern and southern coastlines. Official estimates put total deaths in Sri Lanka at more than 35,000. More than half a million people were displaced when their houses were damaged or destroyed. Estimated damage to infrastructure and other assets exceeded \$1.3 billion, around 7% of the country's GDP.⁵ The tsunami's impact was concentrated in a narrow strip along the coast and in the fishing and tourism sectors. Aggregate Sri Lankan GDP fell only by between 0.5% and 1.0% (Jayasuriya *et al.*, 2005). But two-thirds of the island's fishing fleet was destroyed (Asian Development Bank, Japan Bank for International Cooperation and World Bank, 2005) and – in spite of the fact that the arrival of aid workers dampened the blow – hotel bookings fell by 60% on Sri Lanka's southern coast in 2005.

The international response to the disaster was rapid and strong. Governments, international NGOs and the international financial institutions committed more than \$2 billion in relief and recovery funds, equivalent to roughly 10% of GDP. Of the total pledged, \$1.1 billion were dispersed and \$0.6 billion expended within 18 months of the disaster (Government of Sri Lanka, 2006). Of the total aid pledged, \$184 million

⁴ This effect is analogous to the 'disorganisation' effects discussed in the literature on transition economies (Blanchard and Kremer, 1997; Roland and Verdier, 1999).

⁵ This is the figure reported in the EM-DAT database. An assessment by the Asian Development Bank, World Bank and Japanese Bank for International Cooperation estimated losses of roughly \$1.0 billion and replacement costs of \$1.5 billion. The Sri Lankan government estimated recovery costs to be \$1.8 billion.

was categorised as ‘relief’ aid. Of the remainder, the largest amounts were earmarked for recovery of the housing (\$370 million), transportation (\$245 million committed) and water (\$190 million) infrastructure and for livelihood restoration (\$219 million).

1.1. *The Survey Data and Intervention*

We examine the recovery of enterprises from the tsunami using data from a panel survey we conducted with owners of 608 microenterprises along the southern coast of Sri Lanka after the tsunami. The sample includes 205 enterprises suffering damage of business assets during the tsunami. Among the owners suffering no direct damage, 208 were located near enough to the tsunami areas that they faced severe disruptions in their markets, and 195 were located some distance away from impact areas and were unaffected by the tsunami. We refer to the firms suffering damage as ‘directly affected’, those located in the same neighbourhoods as ‘indirectly affected’ and those located further from the coast as ‘unaffected’. We conducted 11 surveys in all, with quarterly surveys from April 2005 to April 2007, and semi-annual surveys in October 2007 and April 2008. The surveys collected data on assets, sales, profits and a variety of other variables. We believe these data are unique in allowing us to examine the recovery process of microenterprises in a low-income country.

The April 2005 microenterprise baseline survey asked owners to detail which assets were damaged or destroyed by the tsunami, and all waves of the survey ask about the repair or replacement of those assets. The July 2005 survey has questions on grants and loans obtained to replace assets. Enterprises in the panel were also subject to random capital injections in May and November 2005. Note that all of the data on asset damage and aid received are self-reported. The small scale enterprises and households which comprise the majority of the samples seldom keep written records of assets or business transactions.

Following the baseline survey, we randomly selected 117 of the enterprises and gave them either 10,000 LKR (Sri Lankan rupees, about US\$100) or 20,000 LKR (about US\$200). The larger grant is about 80% of the median pre-tsunami capital stock among enterprises suffering some damage. Two-thirds of the grants (78) were for 10,000 LKR, and one-third (39) for 20,000 LKR. Half of the grants were provided in cash and half in-kind. The cash grants were paid directly to the survey respondent by check. For the in-kind grants, the owners selected the items to be purchased. They were accompanied by our research assistants to local stores or markets. Cash treatments were given without restrictions; recipients were told they could purchase anything they wanted with the cash. The treatment was framed as compensation for participating in the panel survey, and enterprise owners were told that they would be eligible to win the grant only once.

The aim of the experiment was to help us understand the role of access to capital in the process of enterprise recovery through the generation of an exogenous shock to capital stock. We describe in detail the results of the experiment among undamaged firms in De Mel *et al.* (2008). Randomisation was done by computer and stratified according to district (Galle, Matara or Kalutara) and extent of tsunami damage (directly affected, indirectly affected, or unaffected). As a result of this stratification, the pre-treatment characteristics of the treatment and control groups are similar to each

other among the subsample of directly affected firms, as they are for the comparison groups of unaffected firms. Online Appendix 1 shows firm characteristics by treatment group and describes the critical survey questions in more detail.⁶

In generating a cash inflow following the disaster, the grants we provided are similar to the checks many firms receive from insurance companies in more advanced countries. An obvious difference is that our grants were not directly linked with losses. Among enterprises suffering tsunami damage, 117 firms were assigned to treatment (57%), with 83 firms assigned to receive treatment after the baseline survey in May 2005 and a further 34 firms assigned to receive treatment after the third survey round in November 2005. This split front-loaded treatments so that more of the randomly allocated aid could reach tsunami victims sooner. Among the 117 treatments, 78 were for 10,000 LKR (39 cash, 38 in-kind), and 39 were for 20,000 LKR (21 cash, 22 in-kind).⁷

Attrition in the data is relatively low. Of the 205 firms in the baseline sample reporting some damage from the tsunami, 186 report profits in round five and 173 in round 11 (83% of the initial sample). However, only 197 firms report profits in the baseline survey, and firms move in and out of the sample. One hundred and thirty-five firms report profits in all 11 rounds and 182 report profits in eight rounds or more.

1.2. *Asset Damage, Aid, and Insurance*

Summary data on the extent of damage, insurance coverage and aid are shown in Table 1 for firms with assets damaged by the tsunami. The Table shows data from the microenterprise panel and also, for comparison, data from a one-off survey of enterprises with between 5 and 50 employees. The larger firm survey was conducted in July 2007 and is described in more detail in the Online Data Appendix. The microenterprise owners report losses of LKR 331,000 – the majority of which (LKR 240,000) was household assets. Business losses averaged LKR 89,700 among the damaged microenterprises. Owners of larger firms (small and medium-sized enterprises or SMEs) report losing an average of slightly more than LKR 4,000,000 in business assets and LKR 600,000 in household assets. Table 1 demonstrates the lack of insurance coverage in both samples. Even among the larger enterprise owners, only 13% of those suffering losses reported having any insurance coverage for business assets, and their policies generally did not cover damage from tsunamis. Even those with insurance reported that only 7.5% of business losses were covered. A similar pattern holds for household assets. Among the larger enterprise owners, only 4.1% said they had any insurance on household assets and only 5.1% of household losses were covered. Among microen-

⁶ Table A1 in the Appendix, and Table 2 in the main article, which is discussed later, show balance on baseline measures both across areas and across treatment groups. Some indicators of pre-tsunami size, measured by recall of the enterprise owners, indicate a lack of balance. We discuss these later in the article.

⁷ Our initial plan was to survey firms for five quarterly waves only. Receipt of further funding enabled us to continue the panel, with four additional quarterly waves collected from July 2006 through April 2007, and tenth and eleventh waves collected in October 2007 and April 2008. In order to compensate firms for the additional burden of staying in the study longer than we had anticipated, we gave 2,500 LKR (~US\$25) in cash to each of the remaining untreated firms after round five of the survey. These treatments are accounted for the analysis.

Table 1
Sources of Recovery Funds for Small Businesses and Households

	Microenterprises (1) Business and Household Assets	SMEs Business Assets
Number suffering damage	197	139
Mean damage (LKR)	LKR 331,800 (2)	LKR 4,020,000
<i>Insurance coverage:</i>		
% of those with loss covered by insurance	0.0%	12.9%
Mean % of losses paid by insurance (conditional on being insured)	NA	7.5%
% of total losses covered by insurance	0.0%	1.7%
<i>Aid received:</i>		
% receiving grant for repair	94.4%	23.7%
% receiving loan for repair	3.5%	25.9%
Mean aid received from government/NGOs (3)	LKR 33,200	LKR 210,100
Mean loan received from government or banks (3)	LKR 49,600	LKR 2,121,500
Mean % losses covered by grants	20.8%	1.2%
Mean % of losses covered by loans	0.4%	13.7%
Correlation of losses and aid received	-0.02	0.02
Correlation of losses and loans received	0.09	0.16
% of all losses covered by insurance, grants or loans	21.3%	16.9%
% of losses replaced/repaid by July/Oct 2007	72.3%	67.8%

Notes. (1) Information on losses and insurance from October 2007 survey. Information on aid and loans from July 2005 survey. (2) The mean loss of business assets reported in the survey was LKR 89,700 and the mean loss of household assets was LKR 242,100. The majority of government aid conditioned on evidence of loss was directed to housing losses. The survey did not separate aid received due to housing and business losses. (3) Mean conditional on reporting some aid.

terprise owners, none of the 176 suffering damage to household assets reported having insurance to cover losses.⁸

Microenterprise owners reported receiving more funds from grants than from insurance, with 94% reporting a grant to cover some part of their damages. Six months after the tsunami, in July 2005, households reported receiving an average of LKR 33,200 in grants from all sources.⁹ This was enough to cover only about 10% of their reported losses. Most of the aid came from government sources, through one of four programmes targeting those who lost family members and household assets.¹⁰

⁸ In contrast, 78% of households suffering damage from hurricane Andrew in Florida had insurance and the insured received average payouts of \$32,000 (Smith and McCarty, 1996). The percentage of insured was probably higher among businesses. Besides providing funds for the recovery of individuals' assets, the \$14 billion dollars of insurance aid following Hurricane Andrew provided a significant boost to the local economy that is lacking in the Sri Lankan case.

⁹ The question asked owners to exclude grants received from our project. But cash aid reported by those who received our grants was roughly 10,000 LKR more than the cash aid reported by those who did not. So these figures may overstate the amount of aid received by typical households.

¹⁰ First, the Sri Lankan government paid surviving family members LKR 15,000 for each person killed by the tsunami, to offset funeral expenses. Second, the government and the World Bank provided four grants of LKR 5,000 distributed to 220,000 households suffering direct damage from the tsunami. Third, the government provided aid to rebuild houses destroyed by the tsunami. Finally, the government and numerous NGOs sponsored cash-for-work programmes, which typically paid workers around 300–350 LKR per day to participate in cleanup and rebuilding activities. As of April 2005, government aid accounted for about 88% of the aid received by microenterprise owners.

For the microenterprise owners, loans were much less common. Fewer than 4% of microenterprise owners reported receiving a loan and these covered less than 1% of losses in aggregate.

Only a quarter of the SME owners reported receiving grants but an equal number reported receiving loans. For the typical SME owner, grants covered almost one-quarter of housing losses but less than 2% of business losses. Loans covered an additional 13.7% of business losses. The SME survey asked how the grants and loans were used. They reported that grants were more likely to cover housing losses and loans were more likely to cover business losses. Combined, however, the reported grants and loans were enough to replace only a small part of the damaged assets – around 1/5th for microenterprise owners and 1/6th for larger firm owners.

Perhaps the most interesting aspect of the reported aid is the lack of correlation between reported losses and reported grants and loans. For SME owners, where we are able to separate business from household aid, the correlation between the reported loss of household assets and the grants (loans) received is 0.70 (0.27). For business assets, on the other hand, there is essentially no correlation between the reported losses and the (very small level of) grants received; the correlation between losses of business assets and loans to replace those assets is 0.16. For microenterprise owners, there is no correlation between the level of losses on the one hand and grants or loans on the other.¹¹ So while overall aid flows into Sri Lanka following the tsunami were large and, while at least some aid there flowed directly to households suffering losses, the data suggest that targeting is particularly poor with regard to damage suffered by business owners.

In sum, the data paint a fairly consistent picture of the process of recovery of business assets. None of the microenterprises and few of the larger firms were insured against losses from the tsunami. Aid flows appear better matched to losses in housing assets, and uncorrelated with losses in business assets. And, the level of aid flows to small scale enterprises was small compared to the size of the losses incurred.

2. Recovery Counterfactual

To understand the recovery of the microenterprises affected by the tsunami, we need to compare the trajectory of the affected firms to a counterfactual trajectory measuring outcomes in the absence of the tsunami. The ideal, of course, would be for the tsunami to have randomly selected enterprises for damage. In any disaster, this is conceptually impossible. However, we believe we have two very good potential comparison groups. The first is a group of enterprises located in the same neighbourhoods as the enterprises damaged by the tsunami, the ‘indirectly affected’ enterprises. The average direct distance to the coast – calculated using GPS location readings – is 760 m for the indirectly affected firms, compared with 410 m for the directly affected firms. There is substantial overlap in the distribution of distances from the ocean, with the median distance among the indirectly affected firms below the 90th percentile of the directly affected sample. Whether the tsunami caused direct damage to firms located very near the coast depended in part on factors such as the shape of the ocean floor and

¹¹ There is no correlation between aid and damage even among the sample of firms not receiving our grants in May 2005.

topography on land. There is little reason to believe that the decisions regarding the locations of the firms – made prior to the tsunami – were influenced by these factors, especially given that the previous major tsunami occurred more than 500 years earlier. In that sense, the damage among the directly and indirectly affected firms comes as close to the ideal of being randomly assigned as we might hope. But there is a potential drawback to this group as a comparison sample. The outcomes of the indirectly affected firms may, for at least some period of time, deviate from the no-tsunami counterfactual because of shifts in demand caused by either the dislocation of their customers or by the reduction in the number of competitors. We examine these concerns as they relate to the appropriateness of these firms as a comparison group in the next subsection.

The second potential comparison group is the ‘unaffected firms’ which are located farther from the coast – 5.2 km on average. These firms are much less likely to have experienced demand shocks after the tsunami; but we might be concerned that the locational choice of microentrepreneurs is not random with respect to damage from the tsunami. Thus, the firms may not be comparable in other ways.

The criteria used to select the sample ensure a high degree of comparability on measured characteristics across all three groups. We sampled firms with no paid employees, no motor vehicles, and with an upper limit on capital stock of LKR 100,000 (about \$1000), excluding land and buildings. We excluded firms operating in agriculture, fishing and professional services. Almost exactly half of the enterprises in all three groups are engaged in retail trade in all of the three areas (51% among directly affected firms, 47% in the indirectly affected area and 53% among the unaffected firms). In both the directly affected and indirectly affected samples, the most common non-retail activities are production of garments, lace, food and coir. Among the unaffected firms, the most common activities outside retail trade include production of garments and food, as well. However, production of bamboo products is also a common activity, while production of lace and coir are not common.

Table 2 compares the capital stock, sales and profits of enterprises in the directly affected, indirectly affected and unaffected sub-samples, using data for March 2005 from the baseline survey and recalled pre-tsunami data. The first column shows data for the 205 firms reporting damage. Twenty-nine per cent report losing all of their assets, excluding land and buildings,¹² while 10% report losing less than one-quarter of their assets and 21% report losing less than half of their assets. Looking first at the March 2005 data, there are no significant differences across the three groups in mean reported capital stock (less land and buildings), or reported machinery and equipment. (See rows 1 and 2.) Rows 3 and 4 show that mean sales and profits are also very similar for the directly affected and indirectly affected firms, but both these measures are almost 50% larger for the unaffected firms, a difference significant at the 1% level.

The March 2005 data reflect the effects of the tsunami. Since recovery implies a return to the pre-tsunami trajectory, a more relevant comparison would be based on pre-tsunami firm sizes. We have no data from surveys conducted before the tsunami. Instead, we rely on reports of damage to assets and recall questions on sales and profits.

¹² We exclude land and building throughout the analysis because the business activity is often carried out at home and separating the value of the assets used in the business from those used for living is very difficult.

Table 2
Comparison of Microenterprises Affected and Not Affected by the Tsunami

	(1)	(2)		(3)	
	Directly Affected*	Indirectly Affected	Sig 1 vs. 2	Unaffected	Sig 1 vs. 3
March 2005 capital, without land & buildings	24,959	25,139	p = 0.98	27,057	p = 0.41
March 2005 capital (machinery and equipment only)	13,962	13,773	p = 0.85	13,755	p = 0.90
March 2005 sales	10,100	9,189	p = 0.51	15,111	p < 0.01
March 2005 profits	3,265	3,117	p = 0.68	4,543	p < 0.01
Pre-tsunami capital without land and buildings†	31,648	24,855	p = 0.01	27,376	p = 0.11
Pre-tsunami capital (machinery and equipment only)	18,738	13,482	p < 0.01	14,073	p = 0.01
November 2004 recalled sales	19,133	11,863	p < 0.01	14,575	p = 0.04
November 2004 recalled profits	5,804	3,986	p < 0.01	4,688	p = 0.02
Sample size	205	199		193	

Notes. Sig denotes significance from a t-test of comparison of means

All figures in Sri Lankan Rupees (LKR). * Includes firms reporting any level of damage. † For firms suffering no damage, December 2004 inventories are not reported in the survey; we use the March 2005 inventories as an estimate for the December 2005 inventories.

These recall data are shown on the bottom half of Table 2. They suggest that prior to the tsunami the directly affected firms were somewhat larger than the indirectly affected and unaffected firms.

We compare the directly and indirectly affected firms (columns 1 and 2), before the tsunami. The directly damaged firms had 27% more invested capital (LKR 31,648 vs LKR 24,855), 61% higher sales (LKR 19,133 vs LKR 11,863) and 45% higher profits (LKR 5,804 vs LKR 3986). All of the differences are significant at the 1% level. The pre-tsunami differences between the directly affected and unaffected firms are somewhat less pronounced, though the former still had 15% more invested capital, 30% higher sales and 25% higher profits, on average. The difference in capital stock is significant only at the 11% level but the differences in sales and profits are each significant at the 5% level. There is substantial overlap in the distributions with, for example, the median invested capital among firms suffering damage being around the 58th percentile of the distribution of either the indirectly affected or unaffected firms.

In measuring the impact of access to capital in the recovery process, we use firm fixed effects regressions which control for any pre-tsunami differences in firms. But because the question of how quickly the damaged firms recover from the tsunami depends on a comparison across firms, this analysis precludes the use of fixed effects. In the recovery regressions, we condition on pre-tsunami size, measured by recalled profits, profits imputed from recalled sales, recalled sales, or capital stock. We show that the results are robust to using any of the measures of pre-tsunami size. In robustness checks, we also use only the sample of damaged firms and compare those suffering damage exceeding the amounts of our grant with those suffering damage less than our grant.

Finally, we look at changes in firm performance between 2005 and 2008 in each of the groups. To provide a sense of what the data show, Figures 1(a–c) depict the trend of real profits, real sales and real invested capital for four subsamples: damaged firms not receiving our grants, damaged firms receiving our grants in May 2005, untreated indirectly affected firms and untreated unaffected firms.¹³ To represent dynamics under more normal conditions, we exclude from the graph the firms in the indirectly and unaffected areas that received grants from us. To make the visual comparison offered by the graphs starker, we exclude damaged firms losing less than half of their assets and those receiving treatments in December 2005. We include both of these groups in the regressions we detail later in the article. A vertical line shows the timing of the grants.

Figure 1a conditions on a measure of pre-tsunami profits imputed from recalled sales, with the mean for each group normalised to 100.¹⁴ Focusing first on the two potential comparison groups, we see that the indirectly affected firms lag behind the unaffected firms for two to three quarters and then track the unaffected firms very closely for the remainder of the sample. This suggests that, in terms of profits, the indirectly affected and unaffected firms are quite similar to one another after a short period of recovery for the indirectly affected firms. Since the indirectly affected firms are located in the same neighbourhoods as the directly affected firms, this gives us some assurance that both the indirectly affected and the unaffected firms are reasonable comparison groups for measuring the recovery process. Also notable in the Figure is the fact that the directly affected firms in the control group lag behind both the untreated indirectly affected and unaffected firms until the end of the three-year period. In most quarters, directly affected firms that received a grant have higher earnings than those that did not, suggesting that the grants helped firms recover.

Figure 1b shows the trajectory of real sales, using recalled November 2004 sales as the base. There is a somewhat more distinct difference in sales growth between the unaffected and indirectly affected firms, with the two not converging until around 30 months after the tsunami. But, as with profits, the grants appear to have had a positive effect on recovery among the enterprises suffering damage. Finally, Figure 1c shows the trajectory of capital stock. Here we benchmark to a measure of the capital stock immediately after the tsunami, based on the reported damage. Again, the indirectly affected and unaffected firms track quite closely apart from a spike in capital stock among the unaffected firms 9 months following the tsunami. Among the damaged firms, the treated firms catch up to the unaffected firms almost immediately, and they track quite closely throughout the period. In the online Appendix, we show that the

¹³ The profits, sales and capital stock underlying the graphs are all deflated to March 2007 values using the Sri Lankan all-island consumer price index (CPI) through September 2007 (wave 10 of our survey). The all-island index was discontinued after November 2007, so we use the rate of inflation indicated by the Colombo CPI between September 2007 and March 2008 to deflate the March 2008 survey data. There is no regional inflation index in Sri Lanka covering the southern region.

¹⁴ The reported pre-tsunami profit data are very noisy, and have low correlations with profits reported in subsequent survey rounds. Based on analysis of the same data used here, De Mel *et al.* (2009) show that firms are able to recall sales level with a greater degree of accuracy. Therefore, we use recalled pre-tsunami sales to impute pre-tsunami profits, using baseline (March 2005) data and a regression of profits on sales, sector and age of the business and gender and education level of the owners. Since the graphs use actual reported profits for each post-tsunami period, this does not affect period-to-period changes in the graphs.

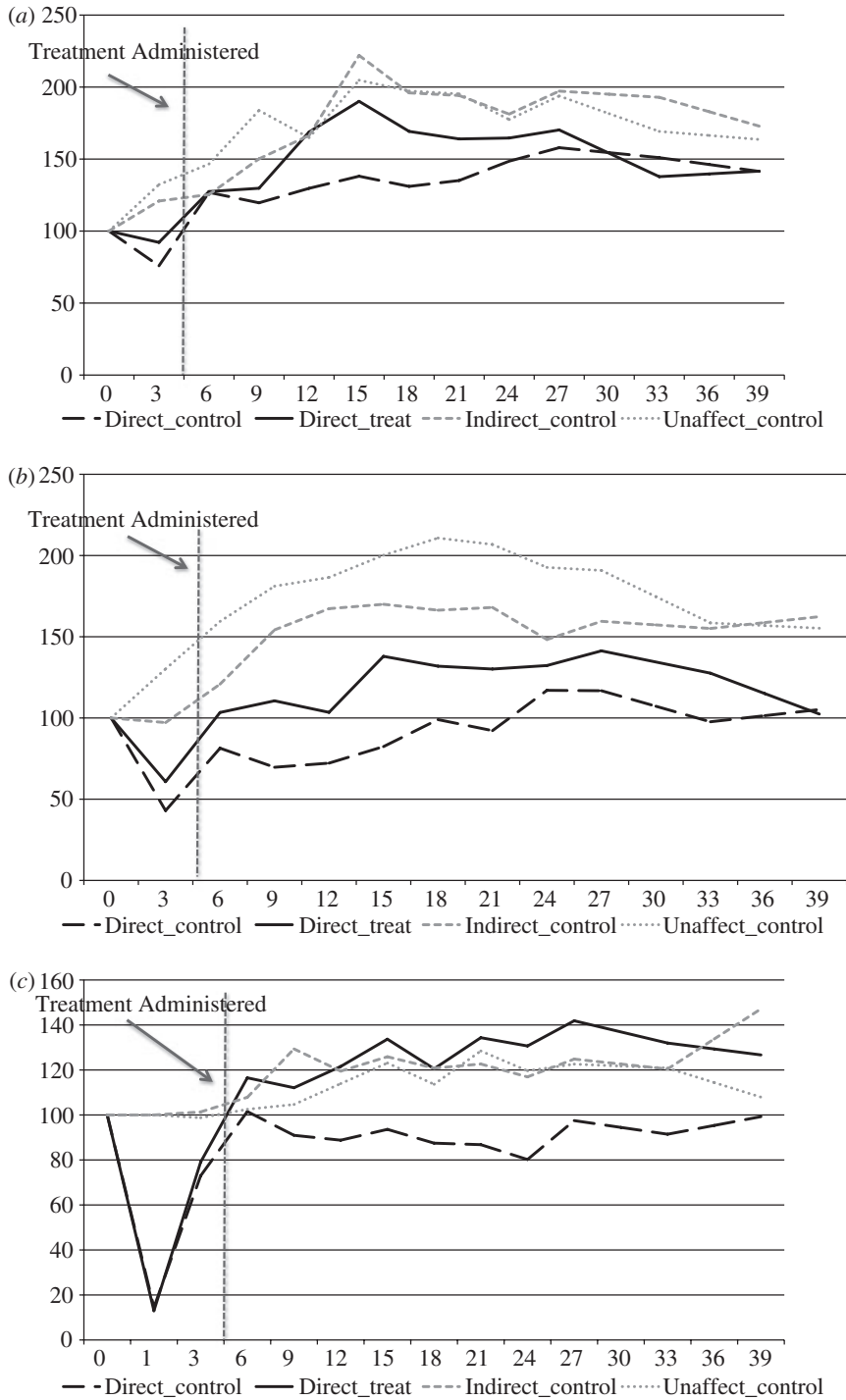


Fig. 1. (a) Profits as a Percentage of Pre-tsunami Imputed Profits. (b) Revenues as a Percentage of Pre-tsunami Revenues. (c) Capital Stock as a Percentage of Pre-tsunami Imputed Capital Stock

same three graphs exhibit similar patterns using medians rather than means, and also using means truncated at the 99th percentile.

Notice that while the damaged firms lag behind their undamaged, untreated firms for some period of time, they catch up to their baseline levels within two or three quarters of the tsunami. We note several caveats to the rapid recovery story. First, the median firm recovers only to about 80% of its pre-tsunami capital stock (see Appendix Figure A1c), suggesting the aggregate recovery is driven by rapid (and temporary) recovery of a limited number of firms. Second, we use an island-wide CPI to deflate all of the operating data to March 2005 prices. Because of the large inflow of resources from outside the area, it is likely that prices rose more rapidly closer to the tsunami affected areas. If so, our inflation-adjusted data will be overstated. Finally, GDP per capita grew by about 6% per year during the three years represented on the graph. While we do not have reliable data on wage rates, we expect that average wages and average earnings of microenterprise owners, increased at a similar pace during this period. For all of these reasons, we believe it is more appropriate to focus on differences with the comparison firms, rather than differences with reported pre-tsunami levels.

In sum, the indirectly affected firms are, on average, somewhat smaller in pre-tsunami size than the directly affected firms. But in the sector of activity and location, the two samples are very closely matched. The unaffected sample differs from the indirectly affected sample somewhat in the detailed non-retail activities, but the enterprises in the two samples are very similar in pre-tsunami size and in the trajectory of profits and capital stock within 6–9 months following the tsunami, though differences in sales appear to endure for a longer period. We conclude that both the indirectly affected and unaffected firms are good, if imperfect, comparison samples. In the interest of statistical power, we combine them for the main regression results reported in the article. With each set of results, we discuss how the results would differ if we used only the unaffected firms as the comparison sample.

3. The Value of Additional Capital

The random allocation of the grants allows us to examine the extent to which restrictions on capital limit recovery. We do this in two ways. First, we measure the effect of the grants on profits in the tsunami damage zone, in comparison with the effect of the grants outside the damage zone. Next, we examine the role of the grants in accelerating recovery across time. We do this by using the undamaged, untreated firms as a counterfactual against which to measure the progress of both the treated and untreated firms that suffered damage.

3.1. *Recovery in the Absence of Grants*

Before turning to the role of the grants, we note that Figure 1c shows a surprisingly rapid recovery to pre-tsunami capital levels even among damaged firms in our control group. The recovery is especially surprising given the lack of insurance and targeted aid flows discussed in Section 2. Where did the enterprises obtain funds to repair or replace assets damaged by the tsunami? The survey we conducted 3 months after the tsunami allows us to provide some insight on this question.

Among the 205 firms suffering tsunami damage, 168 (82%) repaired or replaced some enterprise assets within 3 months of the tsunami. On average, owners who suffered damage reported spending LKR 11,100 on enterprise asset recovery in the 3 months following the tsunami. While the average expenditure is more than twice the average pre-tsunami monthly enterprise income, it is only about 15% of the assets lost or damaged by the tsunami. What were the sources of funds that enterprises used to replace lost or damaged equipment? On average, only one-fifth of the resources (20%) came from grants or loans from tsunami relief agencies. Just over half of the funds spent in the first 3 months came from own savings (51%). An additional 15% were obtained through loans from family members (9%) or friends (6%). The remaining 14% were spread among credit from suppliers (6%), loans from microfinance organisations (2%), moneylenders (2%), banks (less than 1%), remittances from relatives abroad (1%) and other sources. The largest spenders relied more heavily on loans from family members and credit from suppliers, while those spending the least relied more on own savings. The enterprises showed surprising access to capital following the tsunami, particularly given the evidence suggesting that severe capital constraints are commonplace in Sri Lanka under normal circumstances (De Mel *et al.*, 2008).

3.2. How Much Did the Grants Increase Capital and Income?

We begin by estimating the mean impact of the grants on real profits of tsunami-affected firms, via the following fixed effects regression for firm i in period t :

$$\begin{aligned} \text{PROFITS}_{i,t} = & \alpha + \beta \text{AMOUNT}_{i,t} + \lambda \text{AMOUNT}_{i,t} \times \text{PROPORTION_DAMAGED}_i \\ & + \sum_{h=2}^{11} \omega_h \delta_h + \sum_{h=2}^{11} \psi_h \delta_h \times \text{PROPORTION_DAMAGED}_i + \mu_i + \varepsilon_{i,t} \end{aligned} \quad (1)$$

where $\text{AMOUNT}_{i,t}$ is an indicator of the amount of treatment received by firm i at time t . One-third of the enterprises suffering any damage report losing all of their assets, and half report losing 85% or more of their assets. So in discussing the results, we will generally compare undamaged enterprises with those losing all of their assets. The δ_h are wave dummies, which are allowed to vary for damaged and undamaged firms.

Treatments are coded 0, 100, or 200 in the regressions so the coefficient shows the increase in real profits in rupees from a LKR 100 treatment.¹⁵ Thus, the coefficients can be interpreted as the percentage return on the treatment. In most of the regressions we use the amount of the treatment as the independent variable and so are measuring the intention to treat. As not all of the grants found their way into the enterprise, this may differ from the return to incremental investments. The intention to treat seems the more policy-relevant effect in the disaster recovery context.

The first column of Table 3 shows the effect of the capital grant on real profits based on estimating (1), assuming returns which are linear in amount and the same for cash or in-kind grants. Allowing the return to the LKR 10,000 grant to differ from the

¹⁵ Firms receiving LKR 2,500 after round five will thus have AMOUNT of 25 in rounds six through 11 (and 0 before this).

Table 3
Returns to Grants to Enterprises According to Tsunami Damage

	(1)	(2)	(3)	(4)	(5)
	Real Profit	Real Sales	Capital Stock	Owner Hours	Real Profit
	FE	FE	FE	FE	IV FE
Grant amount	5.27** (2.09)	37.66*** (11.82)	89.42*** (19.22)	1.39 (1.16)	
Grant amount × proportion of assets damaged	5.93 (6.27)	-15.49 (23.35)	12.52 (49.80)	-0.15 (2.37)	
Capital stock					0.062** (0.024)
Capital stock × proportion of assets damaged					0.076 (0.087)
Observations	5,877	5,942	5,619	6,057	5,484
Number of enterprises	597	597	597	597	581

Notes. Robust standard errors clustered at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5% and 1% levels respectively. Coefficients represent the increase in the dependent variable resulting from a LKR 100 grant. Profits are measured monthly and hours are measured weekly. The calculation of the proportion of assets damaged by the tsunami excludes investments in land and buildings. All regressions also include survey wave effects, and wave effects interacted with the proportion of non-land assets damaged by the tsunami. The regression in column 5 is an instrumental variables fixed effect specification using the treatments as an instrument for changes in capital stock. FE denotes firm-level fixed effects.

return to the LKR 20,000 grant suggests diminishing returns among both damaged and undamaged firms but in neither case are the returns significantly different from linear. Similarly, the return to the in-kind grant is about double the return to the cash grant in the directly affected area and half the return to the cash grant in the other areas. But, again, in neither case is the difference in returns statistically significant. Finally, the specification in column 1 also assumes that the effect of the grants among tsunami-affected enterprises is linear in the proportion of the assets damaged. The linear fit is not improved upon when we allow for nonlinearity in the proportion of assets damaged by including a dummy variable indicating that more than half of the non-land assets were lost.¹⁶ Hence, for simplicity, we constrain the returns to be linear in amount, identical for cash and in-kind grants, and linear in the proportion of assets damaged.

While we find that the measured return on the treatment amongst firms losing all of their assets is double that in the undamaged areas – 11.20% per month vs 5.27% per month – this difference is not statistically significant. Note that because we use a fixed effects specification, the comparison group of undamaged, untreated firms only impacts the estimated returns for damaged firms through the estimation of the wave fixed effects. If we use only the unaffected firms only – that is, if we exclude the indirectly affected firms from the sample – the measured return from the grant increases slightly among the firms suffering a total loss of assets to 11.8% per month.

¹⁶ The regression results are shown in the Appendix on Table A2. We also tested for threshold effects by using a dummy variable to indicate damage and then excluding from the sample firms losing less than 100%, 70% or 50% of their assets. These regressions, reported in Table A3 of the Appendix, show qualitatively similar effects.

Interpreting the results on profits raises two concerns that merit some discussion. First, most of the firms in the sample do not keep written accounts. Our measure of profits comes from the response to the question: What was the total income the business earned during March after paying all expenses including wages of employees, but not including any income you paid yourself. That is, what were the profits of your business during March?

In De Mel *et al.* (2009), we discuss alternative measures of profits and, using data from additional experiments and surveys, show that this is likely to be the most reliable measure of income generated by the business. The second concern is that firms may treat our grants as free inputs. If they do, profits may increase mechanically because the revenues generated by the grants are not associated with any expenses. As we discuss in De Mel *et al.* (2008), we do not believe this is a concern for several reasons. First, the majority of the grants were used to purchase inputs and inventories. Firms turn over inventories more than once per month on average. Therefore, inputs purchased with our grant would typically have been sold and repurchased before the subsequent survey round. Moreover, the grant yields higher profits even several quarters after the treatment, when the inputs purchased with grant would have been sold and repurchased many times.

Column 2 of Table 3 reports the results of estimating (1) using revenues rather than profits. The results are qualitatively similar, though in this case the grants have an insignificantly lower impact on revenues of damaged firms compared with those not damaged. Using capital stock as the dependent variable (column 3) provides an estimate of the average amount of the grant invested in the enterprise over all of the post-grant survey rounds. We find that about 90% of the grants remain invested in the enterprises, with this amount similar for both damaged and undamaged firms. In column 4, we show that the grants had no effect on the labour effort of the owner, measured by the number of hours worked in a week, among either damaged or undamaged enterprises.¹⁷ Finally, we report the results of an instrumental variables (IV) regression using the grant as an instrument for changes in capital stock. Since the grants do not affect hours worked, the IV regression in column 5 can be interpreted as a marginal return on the incremental capital. Consistent with the ITT results reported above, the measured returns are 13% per month among the most affected firms – though large standard errors imply the return is not significantly different from zero – and 6.7% among firms not directly affected.

The intention-to-treat effects measure the private impact to an individual micro-enterprise of being given our grant. This relies on the stable unit treatment value assumption, which in practice requires that the potential outcomes for a firm are independent of the treatment status of any other firm. A potential threat to the validity of this assumption would be if any gain in profits to treated firms comes as a result of these treated firms taking customers from the untreated firms. We calculate for each wave the number of treated firms in the same industry within 500 m of each firm and following De Mel *et al.* (2008, p. 1361) test for the significance of these spillovers. For

¹⁷ We find no significant effect of the grants on hours worked in the enterprise by family members or non-family members, either among damaged or undamaged firms. For damaged firms, the measured effects are negative, but highly insignificant.

the directly affected firms this spillover term is insignificant ($p = 0.25$) and the coefficient is positive, so that there is no evidence of negative spillovers on untreated firms. These results are shown in Appendix Table A4.

The regressions in Table 3 estimate the average returns to the grants over the entire post-tsunami period. We might expect the effect of the grant to vary with the passage of time after the tsunami.¹⁸ More precisely, we might expect the returns to be highest in the period immediately after the tsunami, because that is the time when capital is likely to be scarcest. As the directly affected firms not receiving grants recover over time, the effect of the grants should diminish. We test this in Table 4 by allowing the impact of the grant on profits and capital investment to vary over time. We do this by estimating regressions of the following form:

$$\begin{aligned} \text{PROFITS}_{i,t} = & \alpha + \sum_{v=1}^3 \beta_v \text{AMOUNT}_{i,t} \times Y_v \\ & + \sum_{v=1}^3 \lambda_v \text{AMOUNT}_{i,t} \times \text{PROPORTION_DAMAGED}_i * Y_v \\ & + \sum_{h=2}^{11} \omega_h \delta_h + \sum_{h=2}^{11} \psi_h \delta_h \times \text{PROPORTION_DAMAGED}_i + \mu_i + \varepsilon_{i,t} \quad (2) \end{aligned}$$

where v indicates the period 1 year (survey waves one through four), 2 years (waves 5 through 8), or 3 or more years (waves 9 through 11) following the tsunami. Thus, returns are allowed to vary by time period and whether or not the firm suffered damage from the tsunami. As before, the effect of the grant is allowed to vary with the proportion of assets damaged by the tsunami.

Columns 1 through 3 of Table 4 report the time-varying effect of the grants on profits, revenues and capital stock, respectively. Column 4 reports hours worked and column 5 an IV regression using the grant as an instrument for capital stock. With respect to profits, we find that the measured impact of the grant is larger for the damaged firms for 2 years after the tsunami and smaller in the third year compared with the undamaged firms. However, none of these differences are statistically significant. The difference between the first 2 years is not statistically significant but the difference between the second and third years is significant at the 5% level. For sales, we cannot rule out the possibility that the grants had no effect on sales among enterprises losing all of their assets. For hours worked by the owner (column 3), we find a significant effect only among the undamaged firms, and then only during the first year after the tsunami. In unreported regressions, we do find that the grants are associated with a significant decrease in the number of hours worked by members of the owner's family in the enterprise, only during the second year following the tsunami.

The patterns from columns 1 and 3 also hold in the IV regression in column four, with returns first rising and then falling among the directly affected firms. The IV regression indicates that the return on capital invested in enterprises that suffered a

¹⁸ The returns might also vary with the length of time after the grant is received as well. De Mel *et al.* (2008) show that there is no such 'time since treatment' effect among the sample of undamaged firms. Similarly, we find no significant time since treatment effect in the sample of damaged enterprises.

Table 4
Allowing the Returns to the Grants to Vary with Time Since Tsunami

	(1)	(2)	(3)	(4)	(5)
	Real Profit	Real Sales	Capital Stock	Owner Hours	Real Profit
	FE	FE	FE	FE	IV FE
Grant amount × within 1 year of tsunami	5.74*** (2.08)	28.61*** (11.02)	76.03*** (15.72)	3.16** (1.23)	
Grant amount × 1 to 2 years after tsunami	3.69 (2.90)	37.16*** (14.55)	109.92*** (25.08)	0.13 (1.44)	
Grant amount × more than 2 years after tsunami	6.95** (3.34)	48.26*** (17.67)	92.69** (36.59)	-2.03 (1.62)	
Grant amount × proportion assets damaged × within 1 year of tsunami	4.15 (5.88)	-19.47 (25.79)	26.17 (49.58)	-2.58 (2.61)	
Grant amount × proportion assets damaged × 1 to 2 years after tsunami	12.79 (8.93)	-52.62 (34.87)	-6.87 (48.93)	1.57 (2.82)	
Grant amount × proportion assets damaged × more than 2 years after tsunami	-2.28 (7.93)	-65.09* (36.34)	8.67 (78.91)	4.51 (3.64)	
Capital stock × within 1 year of tsunami					0.095** (0.040)
Capital stock × 1 to 2 years after tsunami					0.045* (0.025)
Capital stock × more than 2 years after tsunami					0.0807*** (0.029)
Capital stock × proportion assets damaged × within 1 year of tsunami					0.031 (0.080)
Capital stock × proportion assets damaged × 1 to 2 years after tsunami					0.171** (0.068)
Capital stock × proportion assets damaged × more than 2 years after tsunami					0.017 (0.068)
Observations	5,877	5,942	5,619	6,057	5,499
Number of enterprises	597	597	597	597	596

Notes. Robust standard errors clustered at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5% and 1% levels respectively. Coefficients represent the increase in the dependent variable resulting from a LKR 100 grant. Profits are measured monthly and hours are measured weekly. The calculation of the proportion of assets damaged by the tsunami excludes investments in land and buildings. Within 1 year of the tsunami includes the first 4 survey waves; 1–2 years after the tsunami includes waves 5 through 8; more than 2 years after the tsunami includes waves 9 through 11. The regressions also include wave effects and wave effects interacted with a dummy variable indicating the proportion of assets damaged by the tsunami. FE denotes fixed effects. The regression in column 5 is an instrumental variables fixed effect specification using the treatments as an instrument for changes in capital stock.

total loss of assets is 12.6% per month during the first year following the tsunami and 21.6% during the second year. Given the decrease in unpaid family labour working in the enterprise during the second year, this might understate the marginal returns to investments during that year. With regard to capital stock, we find very little difference across years, for either damaged or undamaged firms. Among firms losing all of their assets, very close to the total grant shows up in capital stock in each of the three years.

The results are very similar when we use only the unaffected firms as a comparison group. The measured effect of the grant on profits of the directly affected firms increases slightly to 10.7% during the first year and falls very slightly to 16.2% during the 2nd year, but the statistical significance of the differences across groups and years remains unchanged.

3.3. *Did the Grants Enable Damaged Enterprises to Recover?*

The results in Tables 3 and 4 suggest that the difference in business outcomes between the treatment and control groups is larger for damaged firms than for undamaged firms. However, while this tells us the grants are causing some recovery of profit levels and capital stock for treated damaged firms, it only provides a relative comparison – it does not tell us the extent of the recovery achieved. Recovery here means not just getting back to their pre-tsunami levels but getting back to where the firms would have been had the tsunami not occurred. This counterfactual is provided by using the set of untreated firms in both the indirectly affected and unaffected areas. We will now compare the damaged firms receiving and not receiving grants to this counterfactual. We estimate a regression similar to (2) above. However, because the question of recovery is inherently a cross-firm comparison, we use random effects regressions which condition on a measure of pre-tsunami firm size. We also constrain the wave effects to be the same for the directly affected and other firms:

$$\begin{aligned} \text{PROFITS}_{i,t} = & \alpha + \sum_{v=1}^3 \theta_v \text{PROPORTION_DAMAGED}_i \times Y_v \\ & + \sum_{v=1}^3 \beta_v \text{AMOUNT}_{i,t} \times \text{PROPORTION_DAMAGED}_i \times Y_v \\ & + \gamma \text{SIZE}_{i,t=0} + \sum_{h=2}^{11} \omega_h \delta_h + \varepsilon_{i,t} \end{aligned} \quad (3)$$

where $\text{SIZE}_{i,t=0}$ is pre-tsunami size measured by recalled profits, imputed profits, or December 2004 capital stock excluding land, buildings and inventories. The parameter θ_v then measures the extent to which untreated damaged firms have not recovered relative to the untreated undamaged firms in year v , while β_v measures the extent to which the treatments help damaged firms to recover. Complete recovery of treated damaged firms therefore occurs if $\theta_v + \beta_v = 0$, while complete recovery of untreated firms requires $\theta_v = 0$.

Table 5 reports the results of estimating (3) for real profits, sales and capital stock. For profits, we show results using both recalled November 2004 profit levels (column 1) and the November 2004 profit levels imputed from recalled November 2004 sales (column 2). In either case, the regressions indicate that the untreated firms losing all of their assets suffered a fall in profits of between LKR 734 and LKR 1252 per month during the year after the tsunami, and between LKR 1055 and LKR 1743 during the second year. However, this gap closes markedly in the third year and we cannot reject completely recovery of profit levels by this time using either measure of pre-tsunami size. The size of the estimated effects of the grants are similar in magnitude (and of opposite sign) to these losses during the first 2 years. That is, on average the grants allowed the damaged firms to recover within the first year, in that damaged firms receiving a grant had profits relative to pre-tsunami size which were almost identical to that of undamaged firms not receiving a grant. We find a similar effect for sales: sales fall significantly the year after the tsunami among the untreated firms suffering damage, and the grant offsets the drop almost entirely. Both the tsunami damage and

Table 5
Effect of Grants on Recovery of Enterprises Damaged by the Tsunami

	(1)	(2)	(3)	(4)	(5)	(6)
	Real Profit	Real Profit	Real Sales	Capital Stock	Real Profit	Capital Stock
	RE	RE	RE	RE	RE	RE
	All Firms	All Firms	All Firms	All Firms	Directly Affected and Unaffected Only	Directly Affected and Unaffected Only
Proportion assets damaged × within 1 year of tsunami	-7.34* (3.76)	-12.52*** (4.47)	-68.36*** (18.76)	-100.22*** (32.39)	-14.90** (5.87)	-96.10*** (35.99)
Proportion assets damaged × 1 to 2 years after tsunami	-10.55* (6.30)	-17.43*** (5.87)	-30.71 (36.97)	-118.45*** (43.99)	-16.36** (6.42)	-141.55*** (54.14)
Proportion assets damaged × more than 2 years after tsunami	3.61 (7.07)	-3.80 (6.87)	32.92 (43.23)	-50.19 (95.99)	-2.64 (7.41)	-56.96 (109.12)
Grant amount × proportion assets damaged × within 1 year of tsunami	8.60** (4.07)	10.24*** (3.60)	54.12** (22.72)	92.74*** (35.14)	9.26*** (3.76)	94.71*** (36.18)
Grant amount × proportion assets damaged × 1 to 2 years after tsunami	13.25** (6.72)	17.64*** (6.85)	7.15 (28.10)	98.11*** (32.19)	17.35*** (6.95)	99.47*** (32.68)
Grant amount × proportion assets damaged × more than 2 years after tsunami	2.03 (5.63)	6.39 (5.55)	-3.94 (29.22)	90.95 (60.84)	6.04 (5.61)	92.74 (61.27)
November 2004 profits	0.466*** (0.040)					
November 2004 sales			0.631*** (0.175)			
November 2004 profits imputed from November 2004 sales		1.226*** (0.105)			1.140*** (0.117)	
December 2004 capital stock (excl. land and buildings)				1.070*** (0.145)		1.026*** (0.168)
Observations	3,612	3,455	3,601	3,427	2,667	2,604
Number of enterprises	374	350	363	374	269	285

Notes. Robust standard errors clustered at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5% and 1% levels respectively. Coefficients represent the increase in the dependent variable resulting from a LKR 100 grant. Profits are measured monthly. The base group includes only untreated (control) firms in the indirectly affected and unaffected areas. December capital stock is calculated from reported damage, and sales and purchases of assets between December 2004 and the baseline survey. The regressions also include wave effects. RE denotes random effects.

the grant have smaller (and insignificant) effects on sales in the second year after the tsunami.

Column 4 shows the effect of the grants on recovery of capital stock, conditioning on the December 2004 pre-tsunami capital stock level. The pattern is similar to that for profits. The untreated damaged firms fall behind for 2 years following the tsunami before catching up. The grants enabled immediate recovery of capital stock, on average, relative to the comparison group of untreated, undamaged firms.

We also ran regressions allowing the effect of the grant to vary by quarter. In Figure 2*a* we plot the coefficients for the untreated and treated firms whose assets were fully destroyed by the tsunami. The profits of treated firms are not significantly different from the comparison sample in any quarter following treatment. However, the untreated, damaged firms have profits which are lower than the comparison sample (denoted by an 'X' in the Figure) for most of the first 21 months following the tsunami.¹⁹ The profits of the untreated firms differ from those of the treated firms (denoted by a '+' in the Figure) in each of the surveys conducted 12 months to 21 months following the tsunami, and the survey conducted 27 months after the tsunami. Figures 2*b* and *c* show the same tsunami and treatment effects for sales (Figure 2*b*) and capital stock (Figure 2*c*). As with profits, the firms receiving grants never differ significantly from the undamaged firms. Among the untreated firms suffering damage, sales appears to recover most quickly, while capital stock remains significantly different from the undamaged firms for a full 24 months following the tsunami.

We can use the coefficients from these regressions to make a rough calculation of the return to the grants over the 2-years where the grants have a significant effect on recovery. To do this, we assume that each of the differences in profits we measure applies equally to each of the 3 months between surveys. We find the treatment led to a cumulative increase in profits of LKR 28,520 between June 2005 (the quarter after the treatment) and March 2007. This represents nearly a quadrupling of the initial grant over a period of less than 2 years. Moreover, we cannot rule out the possibility that the grant had effects beyond 2 years, so we think of this as a lower bound of the treatment effect.

Because the recovery regressions are based on cross-firm comparisons, the results may be more sensitive to the choice of comparison sample. Columns 5 and 6 of Table 5 repeat the regressions in columns 2 and 4 using only the unaffected untreated firms as the comparison sample. The narrower comparison sample results in qualitatively similar results. We see that in the case of profits, the damaged firms fell further behind, and – in the year following the tsunami – the point estimates suggest that the average impact of the grants was not sufficient for damaged firms to catch up. However, we still cannot reject full recovery for the treated damaged firms within the first year. For capital stock (column 6), the change in the comparison group has a smaller effect on the magnitude of the coefficients (compare column 6 with column 4).

¹⁹ The only exception comes from the survey gathering data for June 2005, 6 months after the tsunami. Note that there are no treated firms 3 months after the tsunami, because the baseline survey was administered just at that time.

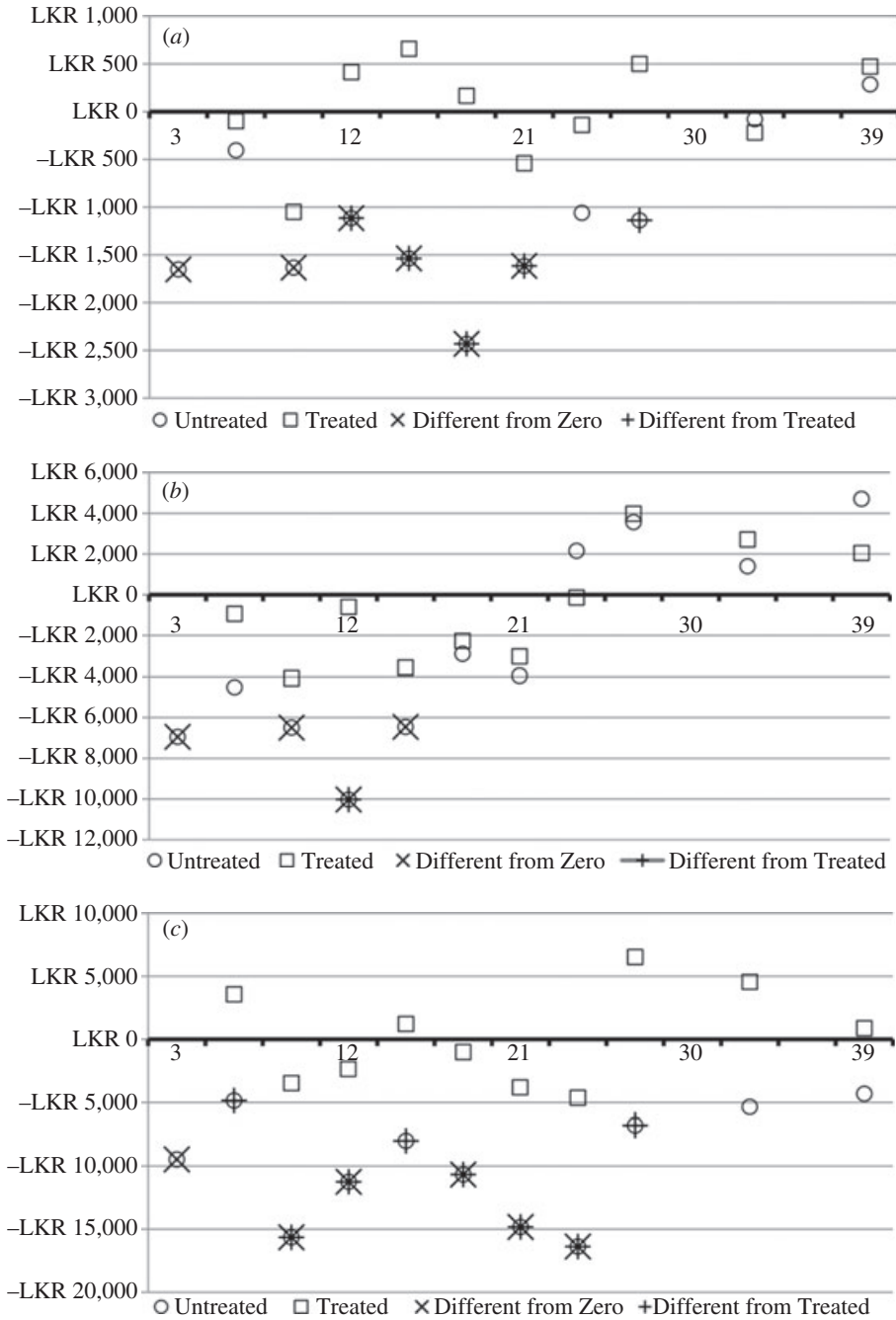


Fig. 2. (a) Recovery of Profit Levels. (b) Recovery of Sales. (c) Recovery of Capital Stock
 Notes. Estimates from random effects regressions using including comparison group of firms not suffering damage from the tsunami, and including a measure of pre-tsunami size based profit (Panel a), sales (Panel b), and capital stock (Panel c).

3.4. Comparing Retail and Non-retail Firms

Aid efforts following the tsunami focused on rebuilding supply chains in industry located along the coast. If capital is sufficient to accelerate enterprise recovery following disasters, then perhaps this focus is misplaced. Our sample is too small to unpack all of the alternative reasons for delays in the recovery process. We will, however, examine heterogeneity in one important dimension, separating enterprises in the retail sector from those in manufacturing and services. Retailers typically sell goods produced outside the area to a large number of customers in arm's length transactions. Those in services and manufacturing typically have fewer, more intense trading relationships and often work with locally procured inputs. Particularly in manufacturing, supply chains are more complex. In some cases, the tsunami disrupted the entirety of the supply chain – for example, in the coir industry, the pits used to soak the coconut husks were filled with debris that took many months to clear. Differences between the retail and non-retail sectors in the effect of capital on recovery of enterprises may suggest that access to capital is sufficient to speed recovery in some circumstances but not others. We caution that we did not stratify treatment on sector of activity, and hence the analysis in this Section should be treated as exploratory and the results as suggestive.

We investigate how the impact of the grants varies across sectors by estimating the following fixed effects regression in which the treatment variable and wave dummies are each interacted with sector or trading partner characteristics of the firm:

$$\begin{aligned} \text{PROFITS}_{i,t} = & \alpha + \sum_{S=R,M} \beta_S \text{AMOUNT}_{i,t} \\ & + \sum_{S=R,M} \lambda_S \text{AMOUNT}_{i,t} \times \text{PROPORTION_DAMAGED}_i \\ & + \sum_{h=2}^{11} \omega_h \delta_h + \sum_{h=2}^{11} \psi_h \delta_h \times \text{PROPORTION_DAMAGED}_i + \mu_i + \varepsilon_{i,t} \quad (4) \end{aligned}$$

where S indicates the sector, retail or manufacturing and services and the other variables are as defined above. This specification constrains the wave effects to be identical in the retail and non-retail sectors, an assumption we relax shortly.

The first two columns of Table 6 repeat the regressions in columns 1 and 3 of Table 3, except that we allow the effect of the grants to differ in the retail and non-retail sectors.²⁰ The results are stark. Among firms suffering a total loss of assets, the measured effect of the grant is actually negative (but highly insignificant) for the non-retail firms suffering damage, and just less than 20% of the grant amount per month for the retail firms. The difference in returns across sectors is significant at the 1% level. Among firms suffering no damage, the grant has a much more modest, and insignificant, effect on the relative return to retail firms. Among the damaged firms, a larger portion of the grant also makes its way into the enterprise in the retail sector: LKR 13,400 for the retail firms vs. LKR 6,770 for the non-retail firms, though the gap

²⁰ We combine manufacturing and services because the sample is not large enough to split them. A larger sample might uncover differences between these sectors as well. We do not report IV regressions because we experienced convergence issues in the smaller subsamples.

Table 6
Returns to Grants to Enterprises by Sector of Activity

	(1)	(2)	(3)	(4)	(5)	(6)
	Real Profit	Capital Stock	Real Profit	Real Profit	Capital Stock	Capital Stock
	FE	FE	FE	FE	FE	FE
	All firms	All Firms	Trade	Man/Serv	Trade	Man/Serv
Grant amount	3.28 (2.95)	39.31** (16.50)				
Grant amount × proportion assets damaged	-7.34 (6.07)	28.39 (62.23)				
Grant amount × retail sector	3.68 (3.36)	96.48*** (31.27)				
Grant amount × proportion assets damaged × retail sector	23.01** (9.32)	-29.7 (112.93)				
Grant amount × within 1 year of tsunami			4.53 (2.92)	6.42** (3.00)	98.69*** (27.07)	49.76*** (13.84)
Grant amount × 1 to 2 years after tsunami			2.87 (4.46)	3.95 (3.81)	174.28*** (45.01)	46.88*** (17.64)
Grant amount × more than 2 years after tsunami			7.55 (5.10)	6.27 (4.52)	114.57* (69.11)	64.27** (26.07)
Grant amount × proportion assets damaged × within 1 year of tsunami			19.55** (8.61)	-13.16** (5.34)	-5.16 (76.68)	50.92 (59.44)
Grant amount × proportion assets damaged × 1 to 2 years after tsunami			23.65* (13.38)	-1.36 (10.59)	-70.14 (69.11)	49.43 (63.28)
Grant amount × proportion assets damaged × more than 2 years after tsunami			0.90 (12.85)	-9.23 (7.89)	-44.71 (145.76)	51.52 (75.87)
Observations	5,877	5,619	2,904	2,973	2,679	2,940
R-squared	0.062	0.099	0.08	0.049	0.116	0.099
Number of enterprises	597	597	298	299	298	299

Notes. Robust standard errors clustered at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5% and 1% levels respectively. Coefficients represent the increase in the dependent variable resulting from a LKR 100 grant. Profits are measured monthly and hours are measured weekly. Within 1 year of the tsunami includes the first 4 survey waves; 1 to 2 years after the tsunami includes waves 5 to 8; more than 2 years after the tsunami includes waves 9 to 11. The regressions also include wave effects and wave effects interacted with the proportion of assets damaged by the tsunami. FE denotes fixed effects.

between the sectors is not statistically significant. Among the firms not suffering damage, the measured returns are higher in the retail sector, but not significantly so and the effect of the grant on invested capital is significantly higher in the retail sector.

The next four columns allow the effect of the grant to vary with time. In order to limit the number of interaction terms, we separately estimate (2) for the retail and non-retail sectors. Among the directly affected firms, we find particularly large effects on retail sector profits during the first 2 years following the tsunami, and significantly ($p < 0.01$) smaller effects more than 2 years after the tsunami. We find no significant effect of the grant on profits of the damaged, non-retail, firms in any period following the tsunami, and no significant differences in effects across time. Curiously, however, the capital

stock regressions suggest that among the damaged firms, manufacturers invest as much of the grant as retailers.²¹ With regards to the undamaged firms, we find no differences in the effect of the grant on profits either across sectors or across years within a sector. Undamaged retailers do appear to invest more of the grant than non-retailers.

Table 7 repeats the recovery regressions (3) with the sample split by retail and non-retail. With regard to profits, the untreated damaged firms in the retail sector fall further and more significantly behind than those in the manufacturing sector, and the grant almost exactly offsets the losses. Looking at capital stock (columns 3 and 4), the grants offset any effect of the tsunami in both the retail and non-retail sectors.

These results indicate substantial and significant differences in the impact of the grants across the two sectors. In the manufacturing and services sectors, where supply chain and customer/supplier relations issues are likely to be more important, the grant led to recovery of capital stock but had no effect on profit levels following the tsunami. In contrast, among firms in the retail sector, the grants led to rapid recovery of capital stock and very large increases in profits.

Finally, Table A5 in the Appendix shows the effect for the profits regressions of truncating the sample at the 99th percentile. Comparisons with the results on Tables 3, 5, and 6, as described in the notes to Appendix Table A5, suggest that the truncation has only a modest effect on the magnitude of the results.

3.5. *Using Variation Within the Tsunami-Affected Area*

Though we showed above that the enterprises in the tsunami-affected area were similar to those outside the affected area, the tsunami damage is clearly not random. In a final exercise, we explore variation of the treatment effect within the sample of firms suffering some damage. The results from this subsample are interesting because within the affected zone, the extent of damage was determined mainly by nuances of topology. Indeed, among the subsample experiencing some damage, the correlation between the proportion of assets damaged and the distance to the coast is actually positive, though it is small and not statistically significant (0.09, $p = 0.18$). In contrast, for the full sample, this correlation is strongly negative (-0.34 , $p < 0.001$). Hence, even the extent of damage within this sample is arguably random.

We split this sample into the group of firms for which our grant covered all of the assets reported lost in the tsunami and those for which the grant was not enough to replace all of the assets lost. About one-third of the sample of damaged firms falls in the first group and two-thirds in the second. Given that even the untreated firms were able to find resources to recover some part of their capital fairly quickly (see Figure 1*b*), we expect the marginal capital provided by the grant will have a larger effect on the firms whose damage exceeded the amount of the grant. Using a specification similar to (3), we find results consistent with this expectation. Among firms suffering damage exceeding the grant, profits increased by 21.5% of the grant amount in the second year following the tsunami; the grant increased profits by only 5.3% during that year among

²¹ The differences across sectors change in columns 5 and 6 compared with column 2. This results from not including sector-specific wave effects in column 2. In the separate regressions in columns 5 and 6, the wave effects are allowed to differ by sector.

Table 7
Effect of Grants on Recovery of Enterprises by Sector of Activity

	(1)	(2)	(3)	(4)
	Real Profit	Real Profit	Capital Stock	Capital Stock
	RE	RE	RE	RE
	Retail Trade	Manuf/serv	Retail Trade	Manuf/serv
Proportion assets damaged × within 1 year of tsunami	-16.82** (8.10)	-5.24 (4.35)	-85.06 (52.07)	-112.84*** (37.90)
Proportion assets damaged × 1 to 2 years after tsunami	-23.80** (10.31)	-8.45 (6.49)	-128.64 (80.77)	-107.11** (42.52)
Proportion assets damaged × more than 2 years after tsunami	4.07 (12.18)	-5.82 (7.34)	65.71 (206.96)	-124.46 (52.38)
Grant amount × proportion assets damaged × within 1 year of tsunami	17.48*** (5.76)	1.73 (3.13)	85.23 (55.13)	88.92** (38.77)
Grant amount × proportion assets damaged × 1 to 2 years after tsunami	24.10** (9.48)	9.94 (9.35)	102.29** (45.70)	85.69** (42.37)
Grant amount × proportion assets damaged × more than 2 years after tsunami	4.20 (8.69)	5.70 (6.48)	46.8 (119.42)	108.72** (52.85)
Pre-tsunami size (see notes)	1.032*** (0.147)	1.345*** (0.131)	0.958*** (0.261)	1.188*** (0.103)
Observations	1,655	1,800	1,585	1,842
Number of enterprises	168	182	183	191

Notes. Robust standard errors clustered at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5% and 1% levels respectively. Coefficients represent the increase in the dependent variable resulting from a LKR 100 grant. Profits are measured monthly. The base group includes only untreated (control) firms in the indirectly affected and unaffected areas. The first two columns include November 2004 profits inputted from recalled sales and other variables, as described in the text. Columns 3 and 4 include the December 2004 capital stock excluding land, buildings and inventories calculated from reported damage, and sales and purchases of assets between December 2004 and the baseline survey. The regressions also include wave effects. RE denotes random effects.

those with damages less than the grant amount. The gap in the third year is of nearly equal size. In spite of the small sample, the gap is statistically significant at the 5% level in both the second and third years following the tsunami.

Of course, most of the untreated firms suffered damage in excess of the LKR 2,500 payment we made to them after the fifth wave. Hence, the untreated firms dominate the group for which damage exceeds the grant amount. This suggests the possibility that the regression in the first column of Table 8 merely reflects the variance generated by treatment vs. control. The regression in the second column shows this is not the case. Here we exclude the untreated firms, so that the difference between the grant exceeding or falling short of the damage amount is driven entirely by the extent of damage and the (randomly determined) size of the grant. We find gaps between the two groups of very similar magnitude to those reported in column 1.

4. Conclusions

Large natural disasters on the scale of the 2004 Asian tsunami leave complete chaos in their wake. The flow of relief and recovery aid following these disasters is often very large. In Sri Lanka, we find that the aid flow to households for the purpose of

Table 8
Effect of Damage Exceeding Grant Amount

	(1)	(2)
	Real Profit	Real Profit
	RE	RE
	Damaged Firms	Damaged Treated
Grant amount * within 1 year of tsunami	5.05 (4.01)	7.51 (5.03)
Grant amount * 1 to 2 years after tsunami	5.34 (3.95)	2.79 (9.10)
Grant amount * more than 2 years after tsunami	-0.95 (4.28)	-9.53 (9.28)
Grant amount * Damage > grant amount * within 1 year of tsunami	3.62 (5.18)	2.89 (5.11)
Grant amount * Damage > grant amount * 1 to 2 years after tsunami	16.11** (7.41)	15.60** (7.40)
Grant amount * Damage > grant amount * more than 2 years after tsunami	16.30** (6.41)	15.85** (6.38)
Pre-tsunami imputed profits	1.097*** (0.120)	1.206*** (0.176)
Observations	1,977	1,138
Number of enterprises	198	114

Notes. Robust standard errors clustered at the firm level in parentheses. *, **, and *** indicate significance at the 10%, 5% and 1% levels respectively. Coefficients represent the increase in the dependent variable resulting from a LKR 100 grant. Profits are measured monthly. Within 1 year of the tsunami includes the first 4 survey waves; 1–2 years after the tsunami includes waves 5 through 8; more than 2 years after the tsunami includes waves 9 through 11. The regressions also include wave effects and wave effects interacted with a dummy variable indicating the proportion of assets damaged by the tsunami. RE denotes random effects.

recovering household assets damaged by the tsunami was large and relatively well targeted, with a positively correlation between the damage suffered by the household and the aid received directly by the household. In contrast, the aid flow to enterprises was small and not well targeted, with no correlation between reported damage and aid receipts. Would better targeting of aid to microenterprises speed the recovery? The answer to this question is not obvious, because for firms, the destruction extends beyond assets to the destruction of trading relationships, supply chains and infrastructure necessary to carry out business normally. In this setting, it is not clear when and whether capital alone will aid in the recovery of enterprises.

Using the random allocation of cash grants to enterprises, we show that providing additional capital is sufficient to speed recovery. Incremental capital invested in the business yields very high rates of return – around 5% per month among firms on the roughly two-thirds of our grants which were invested in the typical recipient firms damaged by the tsunami. These returns to capital are larger than in inland areas less affected by the tsunami. However, we also find differences in the effect of the grants provided to firms in different sectors. For retailers, the effects were much larger than the sample average, while we struggle to find any significant effect of the grants among firms in the manufacturing and services sectors. The high returns, of course, provided an incentive to invest in the enterprises during the recovery period. The data also show that the recovery was more rapid among those operating in the retail sector than

among those in manufacturing or services. One explanation for the difference is the greater importance for non-retailers of specific relationships with trading partners. Higher levels of complementarity among production assets, and more complex supply chains may also play a role in explaining the sectoral differences.

These data come from a single location following a single disaster. But we believe that many of the findings from the tsunami experience likely have some applicability to the recovery of firms from other disasters, especially those arising from infrequent natural phenomena such as tsunamis and earthquakes. The majority of microenterprises are likely to be uninsured. The general pattern of large aid flows and poor targeting toward business recovery likewise appears generalisable, as does the change in demand and supply chain disruption leading to faster recovery for retail than manufacturing.

These data provide some guidance on how that aid might be more effectively administered. We interpret the data as supporting the use of cash grants in disaster recovery, but only in limited cases. Grants to firms in retail trade stimulate more rapid recovery of these enterprises. The data also support a greater use of cash aid in household recovery. The spending by households in local shops provides a stimulus that is lacking when goods are delivered directly into affected areas. Finally, we believe the experiment has demonstrated the ability of random grants to generate knowledge about how to increase the speed with which small enterprises recover. If global warming leads to more frequent and more severe water-related disasters, as climate change experts predict, this knowledge will be increasingly valuable in hastening recovery from the growing devastation.

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Additional Supporting information may be found in the online version of this article:

Appendix 1. Description of the Sample and Measurement of Key Variables.

Table A1. Balance on Treatment.

Table A2. Allowing Returns to Vary by Grant Size and Form.

Table A3. Sensitivity to Cutoffs in Percentage Damaged.

Table A4. Are there Treatment Spillovers?

Table A5. Effect of Trimming Sample.

Figure A1. (a) Profits as a Percentage of Imputed Pre-tsunami Profits (Median); (b) Sales as a Percentage of Pre-tsunami Sales (Median); (c) Capital Stock as a Percentage of Pre-tsunami Capital Stock (Median).

Figure A2. (a) Profits as a Percentage of Imputed Pre-tsunami Profits (Truncated); (b) Sales as a Percentage of Pre-tsunami Sales (Truncated); (c) Capital Stock as Percentage of Pre-tsunami Capital Stock (Truncated).

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