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Supporting Online Material for

One-Time Transfers of Cash or Capital Have Long-Lasting Effects on Microenterprises in Sri Lanka

Suresh de Mel, David McKenzie,* Christopher Woodruff

*To whom correspondence should be addressed. E-mail: dmckenzie@worldbank.org

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SUPPORTING ONLINE MATERIAL

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1. Economic experiments which track outcomes over periods of 4 years or more

This paper is the first we are aware of to track outcomes for firms over a period of longer than 2 and one-half years post-intervention, which allows the opportunity to examine firm survivorship as an important outcome. Most experimental papers in economics look at outcomes over periods of at most 1-2 years, but several recent papers have returned to look at the longer-term effects of health and education interventions. Two studies (17, 19) use the same survey to look at the long-term impacts of a de-worming intervention in Busia, Kenya. They tracked 65% of youth a decade later, and used intensive tracking methods on a sub-sample to obtain a weighted survey response rate of 84% of 5,569 respondents. A third study (20) examines the impacts 4-5 years later of a girl's scholarship program, also in the same region of Kenya. They have follow-up data on 1,385 girls, which is 42% of their initial sample of 3292 students, but use intensive follow-up on a subsample and reweighting to achieve an effective response rate of 79%. Finally, Jensen looks at impacts 3.5 to 4 years after an intervention which provided information on the returns to schooling in the Dominican Republic (21). Of the original sample of 2,250 students, he is able to verify school attendance outcomes for 91% of the students. Together with our 94% re-interview rate at 5.5 years post-treatment, these set of studies show the feasibility of achieving good re-interview rates over longer term horizons than is standard in most existing experiments.

2. Sampling Methodology and Survey Design

The goal of our experiment was to provide a positive shock (in the form of a grant) to the capital stock of firms, and measure the return to this shock. Our target population was low-capital microenterprise owners, those with less than 100,000 Sri Lankan rupees (LKR, about US$1000) in capital, excluding land and buildings. The upper threshold assured that the grants our budget allowed us to provide would result in measurable changes in capital stock. In addition to the capital stock threshold, a microenterprise owner had to fulfill all of the following conditions to be included in our sample:

(a) be self-employed full-time (at least 30 hours per week) outside of agriculture, transportation, fishing and professional services;

(b) be aged between 20 and 65; and,

(c) have no paid employees.

Using the 2001 Sri Lankan Census, we selected 25 Grama Niladhari divisions (GNs) in three Southern and South-Western districts of Sri Lanka: Kalutara, Galle and Matara. A GN is an administrative unit containing on average around 400 households. We used the GN-level data from the Census to select GNs with a high percentage of own-account workers and modest education levels, since these were most likely to yield enterprises with invested capital below the threshold we had set. GNs were also stratified according to the degree of exposure of firms to the December 26, 2004 Indian Ocean tsunami. A door-to-door screening survey of 3361 households in these GNs was then conducted to identify firms whose owners satisfied the criteria listed above. In April 2005, the first wave of the Sri Lanka Microenterprise Survey (SLMS) surveyed the 659 firm owners which the screen identified as meeting these criteria. After reviewing the baseline data, 42 firms were dropped because they exceeded the capital stock threshold, or because a follow-up visit could not verify the existence of the enterprise. This gives a baseline sample of 617 microenterprises. In this paper we exclude the firms which suffered damage to business assets as a result of the tsunami, since recovery of assets damaged by the tsunami might affect returns to capital and lead to different dynamics over the period we look at (28). This leaves 408 firms, of which 197 are run by males and 190 by females. In the remaining 21 firms both husband and wife claim themselves as owner. Given their small number, we drop these dual owner firms. The result is a sample of 387 firms almost evenly split by gender and also across two broad industry categories: retail sales, and manufacturing/services.

3. Baseline differences between male and female-owned firms

In Tables 1 and 4 of (11), we compare characteristics of the male- and female-owned microenterprises in our sample. The female owners are on average more educated than the male owners (mean schooling of 9.4 years for females and 8.6 years for males), and come from slightly wealthier households (measured in terms of ownership of durable assets). It is thus not the case that they come from more disadvantaged households on average than male owners. However, their businesses are concentrated in different industries – such as lacework, coir, and food production, versus repair services and some manufacturing for males – but the sample also includes industries such as retail trade and bamboo work which have a relatively balanced gender mix. Women have smaller initial capital stocks and profitability on average as noted in the text. Female businesses are more likely to operate out of the home (74 percent of them do versus 52 percent of male-owned businesses), and to have all of their customers come from within a very narrow (1 km) neighborhood. Since our sample is a random sample of the microenterprises operating in the areas of our study that meet the criteria set out in SOM.2, these differences reflect differences in the scale of firms operated by men and women in Sri Lanka. A final point to note in terms of the setting is that, like much of South Asia and the Middle East, labor force participation rates for women are much lower than that of men, leading to far fewer women being self-employed in the first place: only 7.8 percent of prime-aged females are self-employed, compared to 29.7 percent of prime-aged males (8.

4. Methods

Treatments and Surveying

Human Subjects approval for this study was given by the University of California, San Diego Human Research Protections Program, project number 061050S. "Rebuilding Sri Lankan Microenterprise After the Tsunami." Participants were provided information on the general purpose of the study and signed a written consent form.

Firms were told before the first survey that, as compensation for participating in the survey, we would conduct a random prize drawing, with prizes of cash or inputs and equipment for their business. We randomly selected 124 firms to receive a prize in May 2005, after the first round survey, and a further 104 to receive a prize six months later in November 2005, after the third round survey. Two-thirds of the prizes were for 10,000 LKR, and one-third for 20,000 LKR. In each case, half the prizes were given as unrestricted cash, and half given as materials or equipment for the business, chosen by the owner. The remaining firms were given an unannounced and unanticipated token payment of 2,500 LKR after the fifth round survey as a means to encourage continued participation in the survey.

Firms were then surveyed at quarterly intervals for two years following the baseline (survey waves 2 through 9), and at six-monthly intervals for another year (waves 10 and 11), ending in April 2008. Each survey wave collected a report of business profits from the owner, including any wages they pay to themselves. The profits measure is the profits from the business the owner operates, so if they close a business and start a different one, our profits measure will still capture profits of the new business. We deflate these data by the Sri Lankan consumer price index to calculate real profits. The methodology for measuring these profits is discussed and validated in (30). These data allowed for estimation of the short-term impacts of the grants over the first 2-2.5 years (11, 14). We then returned to re-interview the same firms in June 2010 and December 2010 (waves 12 and 13), to provide the data used in this paper. This enables estimation of the long-term impacts of the grants, over 5.5 years after the first grants were given. Owners who were not operating a business were asked about any wage work they had, as well as the reason for closing their business. We define total labor income as the sum of business profits and income earned from wage work, which is defined for all firms regardless of whether or not they survive, and also consider this as an outcome variable.

Measuring the Impact on Firm Survival

The impact of the treatment on firm survival is estimated via estimating the following linear regression, separately by gender:

$$Closure_i = a + b * Amount_i + \varepsilon_i \quad (1)$$

where *Amount* is the grant firm *i* received, coded in units of 10,000 Rupees (i.e. 0.25, 1 or 2). In studying the short-term impacts of the grants, we found there was insufficient power to reject linearity of the treatment effects in the amount given, and equality of the cash and in-kind forms of the grants (14). The same holds also here, and so we proceed with a single linear treatment variable. The coefficient *b* measures the intent-to-treat effect (the effect of being assigned to receive a grant). Only 7 of those assigned to treatment failed to receive the grants—because they had attrited from the study when the grants were administered—so *b* is also close to the impact of actually receiving the grants.

We then wish to know whether the treatment might also affect which firms survive. To answer this question, we compare the characteristics of the surviving firms in the treatment and control groups. Randomization ensured that baseline characteristics were balanced across treatment status for the initial sample (11, 14). If any treatment effect on business closures (and survey non-response) is random, we should also then see balance on baseline characteristics for the surviving sample. To the extent that we do not, we can infer what types of firms have their survival differentially affected by the treatment. In addition to a comparison of means, in this supporting online material we also construct the cumulative distribution functions of baseline profits for surviving firms in the treatment and control groups separately by gender.

Measuring the Impact on Firm Profitability

Profit data are notoriously noisy, with much of this variation resulting from genuine seasonality and idiosyncratic firm shocks (31). Figure S3 shows that this noise leads to wide confidence intervals when we consider only round-by-round comparisons of means. We therefore employ several approaches to increase our ability to detect treatment impacts. First, we pool together several waves of survey data, which increases power by averaging out some of this temporary variation (32). Second, in addition to raw real profits, we also consider profits after truncating the top 1 percent, and log profits, which are less sensitive to outliers. We then estimate the following panel data regression equation separately by gender:

$$Profits_{i,t} = \alpha First\ Year_{i,t} + \beta SecondYear_{i,t} + \gamma ThirdYear_{i,t} + \delta FifthYear_{i,t} + \sum_{s=2}^{13} \theta_s \lambda_{s,t} + \mu_i + \varepsilon_{i,t} \quad (2)$$

where $First\ Year_{i,t}$ is the treatment amount firm *i* has received within the last 12 months of time period *t*, $SecondYear_{i,t}$ the amount received 13 to 24 months ago, $ThirdYear_{i,t}$ the amount received 25 to 36 months ago, and $FifthYear_{i,t}$ the amount received 54 to 66 months ago (the period since treatment in the 2010 surveys). The $\lambda_{s,t}$ are period effects that are one when *s=t*, and zero otherwise, and the μ_i are firm individual fixed-effects. Standard errors are clustered at the firm-level. This is estimated for the unbalanced panel, including profits for the survey rounds a firm operates in. The impact of the grants at different intervals after the treatment is then given by α, β, γ, and δ respectively. We test equality of these effects using a standard F-test.

Estimation of equation (2) is done on the sample of surviving firms. To the extent that the treatment affects the characteristics of survivors, this could lead to a bias in the estimated treatment effects. For example, if the treatment causes less successful firms to survive who would otherwise fail without the grant, this will lower mean profits for the treatment group. Since we find little selective effect on which firm survives, we do not believe this to be an important concern in this study. Nevertheless, as a robustness check, we also consider the impact on all labor income from the main job. This is the sum of (truncated) firm profits and wage income for those who are no longer self-employed. This classifies firm profits as zero for those firms who have closed, giving us data for the full sample.

5. Robustness and Additional Detail of Results

Robustness of Survival Results to Survey Attrition

There are 24 firms that were not interviewed in either the June 2010 or December 2010 survey rounds. There are comprised of 2 treated female-owned firms, 6 control female-owned firms, 5 treated male-owned firms, and 11 control male-owned firms. Hence while numbers are small, relatively more of the control group could not be found in either survey. We obtain information on survivorship for these firms through direct observation and proxy reporting, but one may be concerned that this is less accurate. However, most (15/24) of this group who couldn't be located were internal or international migrants, with it being verifiable that their business was no longer operating in the current location. So there are at most 9 individuals (2.3% of our sample) who were not located or declined to be surveyed and for whom we had to rely on neighbor reporting to ascertain whether the business was still in existence. If we drop these 9 firms from our sample, Table S2 shows we still get significant impacts on survival for males, with only a small reduction in the size of the effect (from a 10.9% reduction in closures to 8.8%), and still no impact on survivorship of females.

Is the magnitude of the impact on survival for males plausible?

We estimate that a 10,000 LKR grant reduces the likelihood of a male-owned business closing by 10.9 percentage points relative to the control group (who got 2,500 LKR several months after the treatment group). Since 29 percent of the control group fail, this is a 38 percent drop in the likelihood of failure. We believe an impact of this magnitude is plausible for several reasons. First, it is important to note that the survival rate difference is experienced over 5-6 years – so while this would be a big difference over 1 year, it only requires a difference of just under 2% per year in survival rates from having 15% highly monthly income. Second, although the monthly profits appear to be removed from the firms, on average, capital stock, largely in working capital, is increased by 7500 LKR. This provides a significant additional buffer stock in the business, both in relation to the initial size of 30,000 LKR and in relation to household income. The mean monthly baseline household income for these males is 8,207 LKR, with 5 individuals in the average household. Per person daily income is thus 54 LKR, which is only around 50 cents per day at market exchange rates, and at around $1 per day at Purchasing Power Parity (PPP) exchange rates. These households are thus at, or close to, international poverty lines. At such income levels it is plausible to think people are relatively close to an unexpected shock closing their business. The additional buffer stock allows them to draw capital down for a longer period of time when faced with negative shocks, without pushing capital below the point where the business is not viable. Third, the fact that we find such high returns to capital for males is consistent with them facing tight credit constraints, and thus the amount of liquidity available to deal with shocks on average being small.

Additional Evidence that Grants don't affect which firms survive

Table 2 in the paper shows the means for the treatment and control group surviving firms are not statistically different. SOM Figures S1 and S2 go further and show the entire distribution of baseline profits looks very similar for survivors in the treatment and control groups for both genders. That is, which firms marginally survive does not seem to be related to baseline profitability.

We would expect larger firms to be more likely to survive if we looked across the entire firm size distribution and had small, medium and large firms in our sample. However, our sample is only of microenterprises with baseline capital below 100,000 LKR ($1000). This size range covers a large share of all microenterprises. Among these very small microenterprises, the period-to-period volatility in monthly profits for the same firm is at least as great as the variation in the cross-section across firms (31). Thus the average standard deviation over the 13 periods in monthly profits for a male-owned firm is 3689 LKR, whereas the baseline cross-sectional standard deviation is 3555 LKR. Much of this variation is due to idiosyncratic reasons. Thus, in this range of microenterprises with low levels of profits and capital, it seems the grants do not have differential effects on those with initially lower and those with initially higher levels of profits.

Robustness of Profit Results to Selective Attrition

As the above discussion on attrition and survivorship has shown, we actually have only 9 individuals who we could not in either 2010 survey round locate or verify they had died or migrated. So for local business income, only 2% of our sample could not be traced, with only a small numeric difference in the number of untraceables between the treatment and control groups. The male firms which we cannot locate have marginally lower mean income at baseline than the full male sample (4642 vs 4751). As a bounding exercise, we assume that all the control group firms that we can't locate were actually earning the sample mean profits for a particular round, whereas the treatment group firms we couldn't locate were earning zero. This reduces the coefficient showing the impact of the grants on profits 5-6 years later in column 1 of Table 3 from 1218 to 1150, while significance is retained (p=0.059). Our results therefore seem robust to the concern that the firms we couldn't interview may have been those in which they treatment had least effect.

What are firm owners that closed down doing, and why did they say they closed?

Our surveys asked firm owners what they were doing in June 2010 and December 2010. For those who were in wage work, we asked the reason for going into wage work rather than remaining in business. Table S3 shows what firm owners that closed are doing. Among males, only 34 percent are in wage work, which is balanced between treatment (33.3%) and control (34.8%), with another third either not working or deceased, and the remainder either migrated

(where they may be working for pay) or in an unknown status. Among the female owners, the majority are now doing housework or caring for family members, with only 19% in wage work. This shows that the majority are not closing their businesses to work for wages.

Table S4 then shows why those who are working for wages said that they had closed their business. Only a minority (13% of males and 25% of females) say it is to earn higher income – the most common response for both sexes is that they got a wage job because their business failed, with men also saying they were moving to a more stable working environment.

Finally, we also asked those who are not working what the main reason for closing the business was. For males, the main reasons were business failure (33%) and sickness or health reasons (27%), while for females the main reasons were also business failure (40%) and sickness or health reasons (22%), as well as closing after getting married (22%).

Taken together, this information clearly shows that most owners who close their businesses are not doing it because of an opportunity for higher incomes, but rather because their businesses are failing, and/or they have health or family needs. Further evidence of this is seen by comparing the income from work for owners still running businesses in waves 12 and 13 to the income from work from individuals who closed their businesses. For males receiving the grants, mean monthly work income for those still running their businesses was twice as high as that of the treated males who closed their businesses (7111 LKR vs 3592 LKR), and similarly for control group males monthly work incomes were also almost twice as high for those who didn't close their business as those that did (6394 LKR vs 3482 LKR). In both cases this difference in means is statistically significant at the one percent level. The differences are even larger for females, reflecting their lower likelihood of working if they close the business. Monthly work incomes for treated females average 5612 LKR for those still running their businesses versus 2769 LKR for those who closed their business, and among control group females, average 5208 LKR for those still running their business and 1437 LKR for those who closed their business. This provides further support for our argument that the grants have long-run welfare benefits.

Did the grants differentially affect closure rates for poorer vs richer firms?

We construct an index of wealth by taking the first principal component of a set of durable asset indicators owned by the household at baseline (see (11)), and interact this with the treatment effect to see whether the treatment impact is higher for poorer firms. Table S5 shows the result. A higher value of the asset index indicates more wealth. For males the mean value of this index is 0.15, and standard deviation 1.5. The point estimate for the interaction is actually negative, indicating that, if anything, the treatments had bigger impacts on reducing closures for richer owners. However, this interaction has a standard error that is big relative to the point estimate. The 95% confidence interval ranges from -0.074 to +0.038. So we can't rule out some positive interaction, whereby the treatment is benefiting the poor more in terms

of survival. However, we believe the sign and small magnitude of this coefficient certainly does not suggest the main effects are for the poor, which is consistent with Figures S1 and S2.

Would the results differ if loans instead of grants were given?

We find most of the grants, when used in the business, are used for working capital needs like raw materials and inventories, rather than to buy lumpy equipment. This is similar to how many microfinance loans are used, since the short-term repayment periods often deter borrowers from investing in items that take longer to pay off. Moreover, two recent randomized microfinance experiments (3, 4), find no impact of credit on the profitability of female-owned microenterprises over the short-term horizons they look at. Given this and the fact that our grants are typically invested in similar types of business inputs as loans are, we postulate that behavior would not be that different from loans as from our grants (whether in cash or in-kind). Indeed discussions of our results with women's microfinance organization often leads to comments along the lines of "yes, credit alone is not enough which is why we also are now doing training" or "many times women take the loans in their names but invest it in their husband's businesses".

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Figure S1: Baseline Distribution of Profits for Surviving Male Treatment and Control Firms

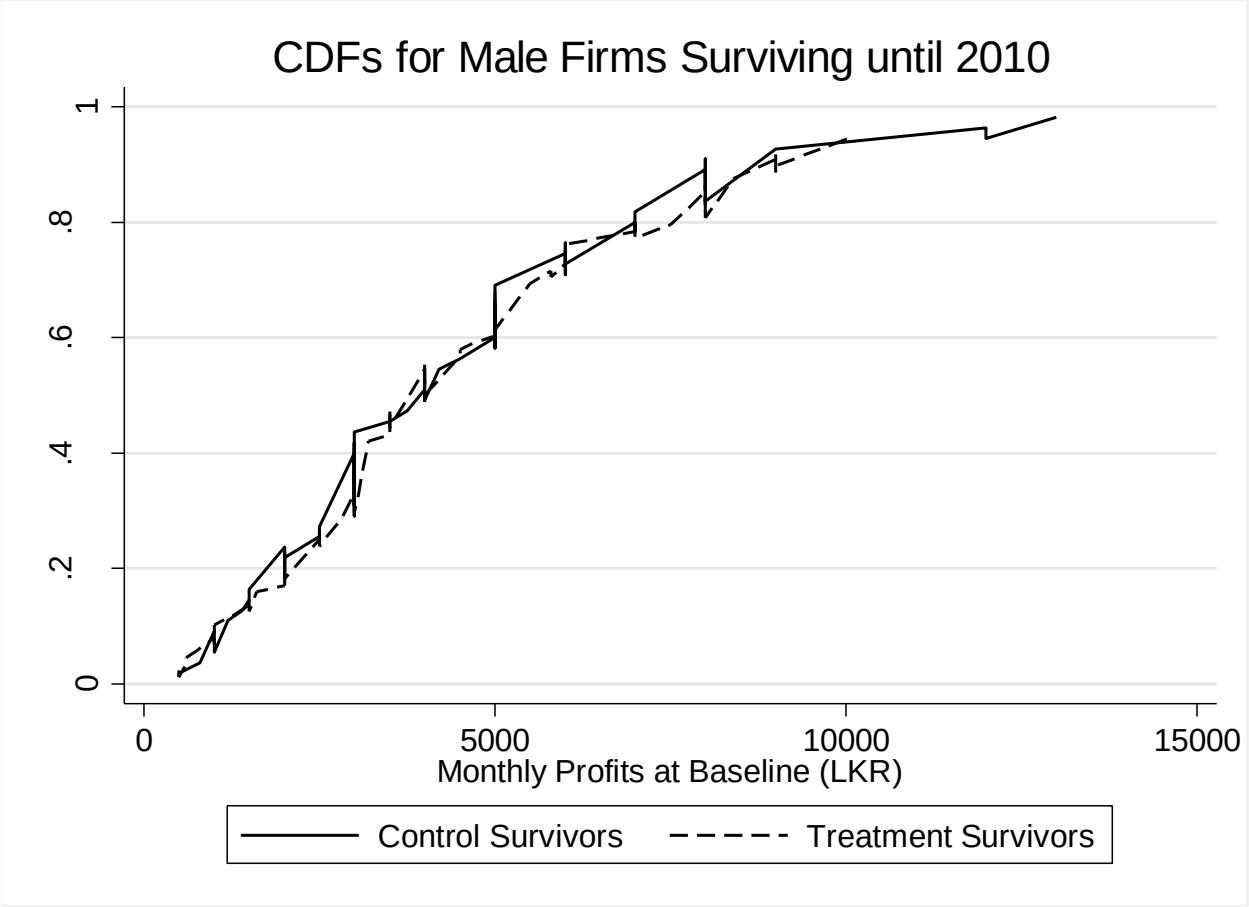


Figure S2: Baseline Distribution of Profits for Surviving Female-owned Treatment and Control Firms

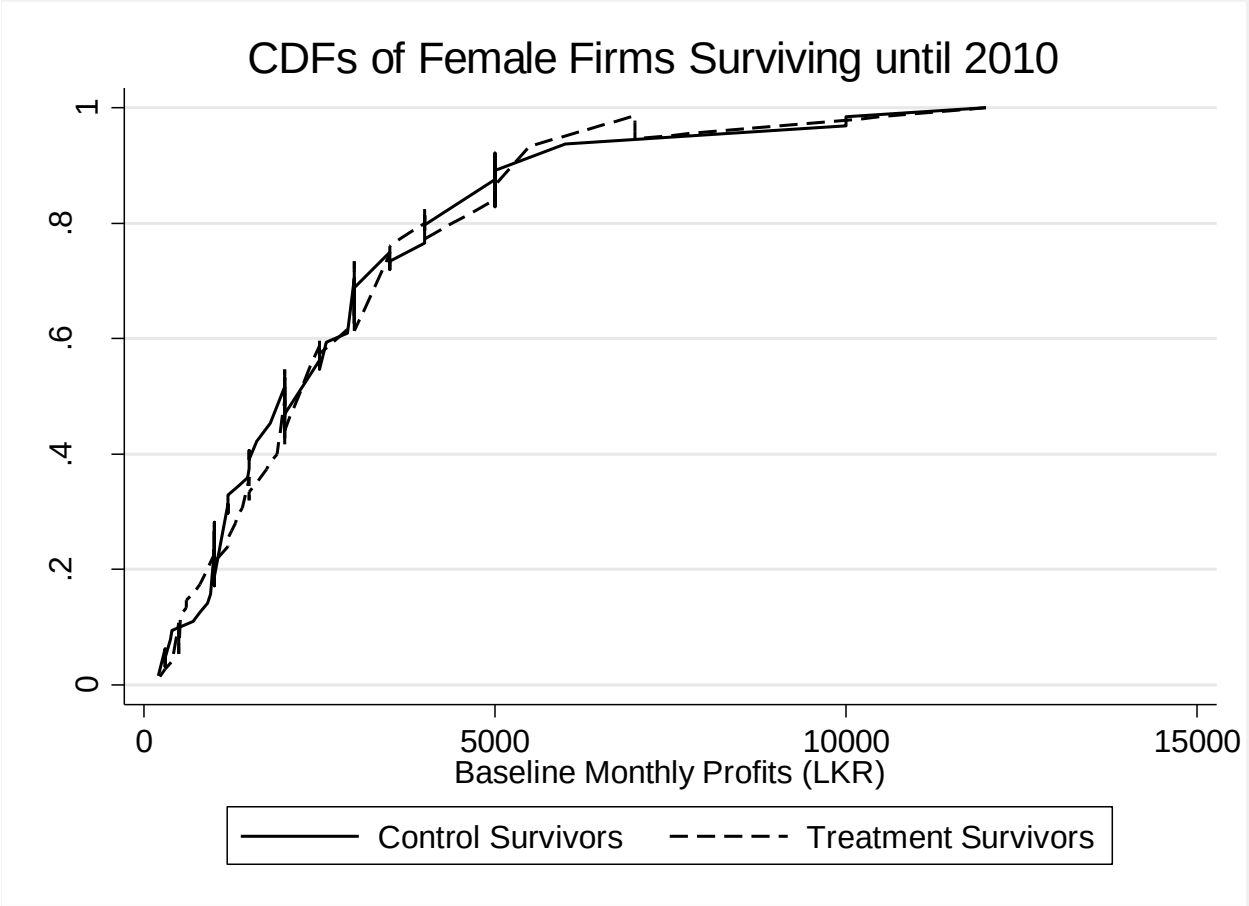
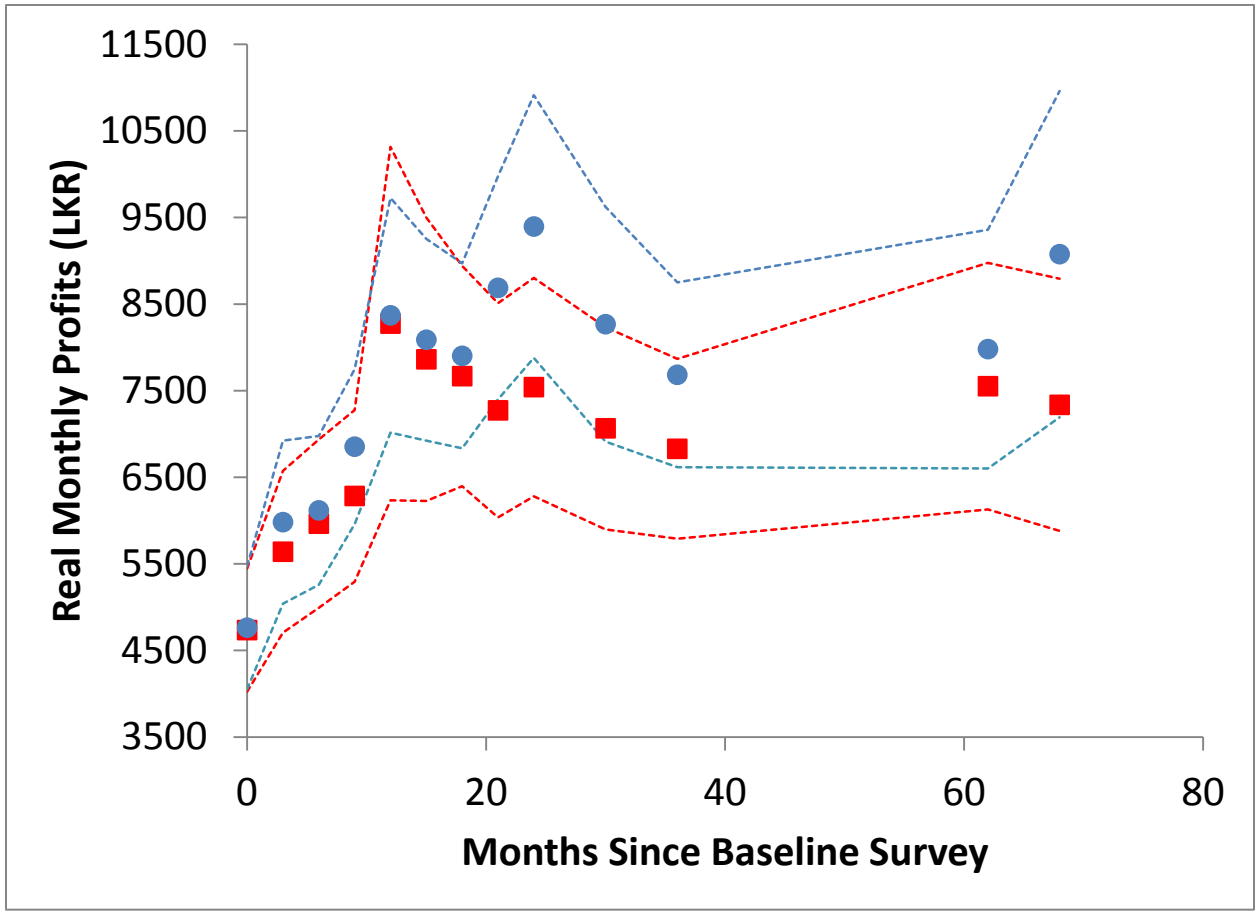


Figure S3: Mean Profits by Survey Round for Males with 95% Pointwise Confidence Bands



Mean real monthly profits for control firms (red) and treatment firms (blue) are shown, along with 95% pointwise confidence intervals. The regressions pool together multiple rounds and control for firm fixed effects in order to increase statistical power beyond that possible for round-by-round comparisons of means.

Table S1: Results pooling male- and female-owned firms and including jointly owned firms

Treatment impacts on outcomes in 2010 survey data.

	Survival Results		Profitability and Income			
	Closed	Reports Profits	Real Profits	Truncated Real Profits	Log Real Profits	Total Labor Income
	(1)	(2)				
Treatment Amount (in 10,000s of LKR)	-0.051	0.050				
	(0.033)	(0.031)				
Amount*First year since grant			527.6***	537.1***	0.109***	570.0***
			(199.6)	(190.5)	(0.0376)	(193.8)
Amount*Second year since grant			536.5*	433.8	0.0693	484.8*
			(293.9)	(263.3)	(0.0448)	(269.0)
Amount*third year since grant			550.4*	492.6*	0.0546	516.2*
			(295.9)	(275.4)	(0.0560)	(284.8)
Amount*five to six years since grant			814.4*	562.6*	0.0600	613.3*
			(419.8)	(335.8)	(0.0544)	(345.8)
Mean for Control Group	0.28	0.76	5280	5186	8.14	4969
Number of Observations	408	408	4,582	4,582	4,562	4,804

Notes:

Huber-White standard errors in parentheses, clustered at the firm level for profitability and income outcomes

*, ** and *** denote significance at the 10, 5 and 1% levels respectively.

Truncated profits truncate the top 1 percent.

Table S2: Robustness of survival results to excluding firms who refused or could not be located

Dependent Variable: Firm has closed by 2010 survey rounds

	Males		Females	
	Result in paper	Excluding proxy reported	Result in paper	Excluding proxy reported
	(1)	(2)	(3)	(4)
Treatment Amount (in 10,000s of LKR)	-0.109***	-0.088**	0.025	0.042
	(0.040)	(0.039)	(0.056)	(0.055)
Proportion of control group closing	0.292	0.247	0.256	0.239
Number of Firms	197	190	190	188

Notes:

Huber-White standard errors in parentheses.

*, ** and *** denote significance at the 10, 5 and 1% levels respectively.

Table S3: What are firm owners that closed doing?(%)

	Males	Females
Wage work	34.1	19.0
Looking for work	13.6	4.8
Housework/caring for family members	15.9	57.1
Migrant Abroad	9.1	4.8
Internal migrant	6.8	9.5
Deceased	4.5	0.0
Unknown	15.9	4.8

Tabulated from Sri Lanka Microenterprise Survey interviews with owners of firms which closed.

Table S4: What are the main reasons for going into wage work? (up to 2 selected) (%)

	Males	Females
Higher salary	13	25
More stable working environment	60	25
Less stress	7	25
Business failed	67	75
Better working hours	0	0
Prospects for future wage growth	0	0
Marriage	0	0
Easier to take care of family matters with job	0	0

Tabulated from Sri Lanka Microenterprise Survey interviews with owners of firms which closed and whose owners went into wage work.

Table S5: Did the grants differentially affect closure rates for the wealthy male-owners?

Dependent variable: Firm closed by 2010 surveys

Treatment Amount	-0.103**
	(0.0401)
Treatment Amount*Asset Index	-0.0181
	(0.0284)
Asset Index	0.0362
	(0.0347)
Observations	197

Notes:

Huber-White standard errors in parentheses.

*, ** and *** denote significance at the 10, 5 and 1% levels respectively.

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