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# Labor Drops: Experimental Evidence on the Return to Additional Labor in Microenterprises†

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*A field experiment in Sri Lanka provided wage subsidies to randomly chosen microenterprises to test whether hiring additional labor benefits such firms and whether a short-term subsidy can have a lasting impact on firm employment. Using 12 rounds of surveys to track dynamics 4 years after treatment, we find that firms increased employment during the subsidy period. Treated firms were more likely to survive, but there was no lasting impact on employment and no effect on profitability or sales either during or after the subsidy period. There is some heterogeneity in effects; the subsidies have a more durable effect on manufacturers.* (*JEL* C93, J22, J24, J31, J38, O14, O15)

The modal firm in most developing countries consists of a self-employed entrepreneur with no paid employees. What prevents these firm owners from hiring workers? In the classic complete-markets model of firm size of Lucas (1978), the small firm size is optimal, reflecting poor managerial ability and low productivity. However, a growing body of research in developing economies shows that there are a number of profitable investments households and businesses could make, but do not, for which a one-time subsidy of adoption and learning has lasting impacts. Examples include seasonal migration (Bryan, Chowdhury, and Mobarak 2014), keeping enough change on hand (Beaman, Magruder, and Robinson 2014), using good management practices (Bloom et al. 2013), and adopting a new production technology (Atkin et al. 2017). This raises the question: would more of the self-employed also find it profitable to hire workers than are currently doing so?

One reason that firm owners may not hire workers is they lack information about their own entrepreneurial ability (Jovanovic 1982) and have not had the opportunity to learn whether their business can support an additional worker. Hanna,

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Mullainathan, and Schwartzstein (2014) explain how a feedback loop can arise when individuals have selective attention: a business owner who initially believes an input is unlikely to matter will not pay attention to it or experiment with it and, as a consequence, will not learn whether the input does matter. Alternatively, the failure to hire workers may stem from labor market frictions. For example, training costs coupled with high worker turnover may imply that new workers should pay to work at firms for some initial period, a practice usually ruled out by limited contracting options (Stiglitz 1974).[1] Or, friction arising from imperfect information may make it hard for firms to identify the right match for the job. In all of these cases, once a firm has hired a worker it can be profitable to keep them employed, but firm owners may be reluctant to make the up-front investment in learning, training, or search.

We conduct an experiment to test directly whether hiring additional labor can benefit small firms in Sri Lanka. Previous work providing "capital drops" to microenterprises in Sri Lanka found evidence of capital constraints, but also found that capital alone was not enough to transition firms to hiring workers (de Mel, McKenzie, and Woodruff 2008, 2012). In this paper, we report on an attempt to drop labor into firms by offering microenterprises temporary wage subsidies equivalent to roughly half the wage of an unskilled worker for a period of six months, followed by two additional months at half this rate. In the absence of learning or frictions, a short-term subsidy should increase employment during the subsidy period, but have no lasting impact. In contrast, a temporary subsidy can have long-term impacts on firm size if it enables firm owners to learn about the return to labor in their firm or subsidizes the up-front training and search process.[2]

We find that 24 percent of firms take the subsidy to hire a worker, resulting in an increase in employment in the treated firms during the subsidy period. However, using 12 rounds of survey data to track the dynamics of adjustment for 4 years post-subsidy, we show there is no lasting effect on employment and no effect on firm profitability or sales either during the subsidy period or after. A combination of shedding of workers by treated firms and additional hiring by control firms completely eliminates the employment gap within two years. The only long-term effect in the full sample is that the subsidy increased survival rates, particularly for firms that initially had low capital. When we split the sample by sector of activity, we do find some evidence that the subsidies have a more durable effect among the roughly one-third of the sample in the manufacturing sector. Survey data provide supportive evidence for this finding, indicating that business expansion is easier and search frictions somewhat more evident in this sector.

We use the data generated by the experiment to differentiate between competing views of why small firm owners do not hire more labor and why the subsidy had no lasting impact on employment. A combination of detailed survey data and an

[1] Apprenticeships common in certain labor markets appear to reflect the low, initial marginal product of labor. But as Hardy and McCasland (2015) shows, the efficiency of the apprenticeship solution is compromised by credit constraints and information frictions.

[2] If there are recurrent frictions involved every time a firm needs to replace a worker, then a short-term subsidy can have an effect that persists beyond the end of the subsidy, but gradually fades over time as workers quit or are replaced. We discuss this case later in the text and note it is not consistent with the time path of worker churn that we see.

analysis of heterogeneous treatment effects yields no evidence that owners learn more about their ability to manage workers and suggests that search is not excessively costly for the average firm. Complementary treatments providing either capital or training show that the lack of a long-term effect does not appear to be due to lack of complementary capital or skills. Instead, the estimated return to additional labor during the subsidy period appears similar in magnitude to the subsidy offered, suggesting additional workers bring no more value to the firm than their unsubsidized labor cost. This is consistent with the large spike in worker exits immediately as the subsidy ends and with the fact that these workers do not appear to be moving to better jobs. Taken together, the results are more consistent with a Lucas (1978) viewpoint in which small firms have low marginal returns to labor, than with explanations that suggest that firms are foregoing profitable opportunities to hire workers because of learning or labor market frictions.

This paper contributes to a recent experimental literature on urban labor markets in developing countries.[3] Much of this literature focuses on interventions to help particular job seekers find jobs by directly offering the job seekers wage subsidies (Galasso, Ravallion, and Salvia 2004; Groh et al. 2016; Levinsohn et al. 2014); and/or by trying to improve the search and matching process through transport subsidies and skill certification (Groh et al. 2015; Abel, Burger, and Piraino 2016; Abebe et al. 2016). Some, but not all, of these studies have found modest improvements in formal employment as a result of this assistance, consistent with constraints to workers finding jobs in larger, more formal firms. But none of the studies are designed to provide evidence from the perspective of the firms, and so none show that firms hire more workers through the interventions. Moreover, these studies have not focused on helping workers find employment in microenterprises and have not typically found significant effects on informal employment (McKenzie 2017).

The literature examining labor market frictions from the firm side is much less developed with several recent studies beginning after this paper.[4] Cohen (2016) uses the assumptions implicit in a Cobb-Douglas production function and the adjustments to labor generated in our earlier capital experiment in Sri Lanka to conclude that microenterprises do seem to be constrained in expanding labor when they receive the larger of the two capital shocks in that experiment. Bertrand and Crépon (2016) find that firms with between 5 and 300 employees in South Africa hire more workers when offered labor-law advice that explains to them that firing restrictions are not as burdensome as many firms think, suggesting constraints on labor expansion for SMEs. In work most closely related to ours, Hardy and McCasland (2015) randomly place apprentices with small firms in Ghana and find firms retain this extra labor for at least six months and earn higher profits in doing so. Their context,

[3] There is also a related developed-country literature on the effect of wage subsidies. On the firm side, nonexperimental methods have been used to test whether subsidies increase employment during the period in which they are in place or just crowd out other hiring the firm would do. Dahlberg and Forslund (2005) do find substantial displacement of other workers, but along with Kangasharju (2007), still find that employment increases during the period the subsidy is in place. These studies do not look at what happens when the subsidies end, but on the worker side, Card and Hyslop (2005) finds employment effects for Canadian welfare recipients have completely faded out a year and a half after the subsidy ended.

[4] Levinsohn and Pugatch (2014) writes down a structural search model to estimate how a proposed employer wage subsidy might affect the very high of youth unemployment rate in South Africa.

in which employees typically pay for entry-level positions in order to get trained, differs from the standard labor market contractual form in most developing countries (including Sri Lanka). Moreover, their sample is concentrated in manufacturing with our evidence suggesting wage subsidies have particularly limited effects in retail and services.

The remainder of the paper is structured as follows: Section I outlines different theories of why small firms might be labor constrained and the implications for the impact of a wage subsidy; Section II details the experimental design and intervention; Section III discusses take-up; Section IV provides the results; Section V investigates different mechanisms leading to these results; and Section VI concludes.

## I. Theory: Why Might Small Firms Be Labor Constrained, and How Could a Temporary Subsidy Have Lasting Impacts on Firm Employment?

The most common firm size in many developing countries, including Sri Lanka, is one—an owner with no paid employees. What explains the small size of these firms, and how might we expect a temporary wage subsidy to change this firm size?

### A. *Classic Complete-Markets Model*

Consider first the standard complete-markets model of firm size of Lucas (1978), where differences in employment size among firms facing the same output production technology $f(\cdot)$ reflect differences in their management ability and productivity, $\theta$. A firm facing a wage rate for workers $w$, and an interest rate on capital $r$, will choose capital, $K$ and labor, $L$ to maximize profits $f(\theta, K, L) - wL - rK$. Firms are small and are assumed to be price takers, who can sell all output they produce at a price normalized to one. This yields the familiar first-order conditions in which the optimal levels of capital $(K^*)$ and of labor $(L^*)$ are chosen such that marginal products of labor and capital are equal to the wage rate and interest rate, respectively[5]:

$$f_L(\theta, K^*, L^*) = w, \tag{1}$$

$$f_K(\theta, K^*, L^*) = r. \tag{2}$$

If managerial ability is a complement, rather than a substitute for capital and labor, then in this model firms with zero workers are those with low managerial ability.

Consider a temporary wage subsidy in this model. This lowers the effective wage rate for additional workers from $w$ to $w'$. Resolving the first-order conditions (1) and (2) at this lower wage will result in firms choosing higher levels of employment $L'$ and producing more output and therefore more sales and higher profits in the short run. However, once the subsidy ends, $w$ returns to its previous level, and—so long as $\theta$ is unchanged by the intervention—output, profits, and employment return to their pre-subsidy levels.

[5] For simplicity of exposition, we assume the owner's own labor supply is inelastic here but in our empirical work will also examine the labor-supply response of the owner to our interventions.

### B. *Standard Model with Credit Constraints*

Now consider credit-market constraints, which limit the ability of firm owners to borrow to finance capital investments. Let $A$ be the wealth of the business owner. This wealth can be leveraged in financial markets by some amount $(b - 1)$, with $b \geq 1$ being a measure of borrowing constraints. The capital constraint is then $K \leq bA$. Then the new equilibrium levels of capital, $K^{**}$, and $L^{**}$ solve

$$f_L(\theta, K^{**}, L^{**}) = w, \tag{3}$$

$$f_K(\theta, K^{**}, L^{**}) = r + \lambda b, \tag{4}$$

where $\lambda$ is the Lagrange multiplier on the borrowing constraint. In this setup, equilibrium output and equilibrium capital are lower than in the no constraint case ( $K^{**} < K^{*}$), but $L^{**}$ may be greater than or lower than $L^{*}$ depending on the shape of the production function: firms may substitute capital for labor and end up with more employment than in unconstrained states, or they may find labor less productive without complementary capital and so hire less labor than in unconstrained states.

The wage subsidy treatment should then have a similar impact as in the standard model without constraints, except that the presence of credit constraints may limit the ability of the firm owner to adjust capital upwards to provide the capital needed for additional labor to work with. This would act to reduce the responsiveness of firms to a wage subsidy in the short run. There should again be no long-run impact.[6] An exception to this prediction of no long-run impact may occur if firms face a lower bound of profitability below which they shut down if they can't borrow. The short-term wage subsidy, by temporarily providing a period of higher profits, may allow the firm to survive shocks that would otherwise cause them to close down and thereby remain in business (de Mel et al. 2012).

### C. *Learning and Labor Market Constraints*

The motivation for a wage subsidy instead lies in the idea that there are firms for whom hiring more workers would be beneficial, but who have not done so due to various hiring constraints. If the temporary subsidy helps firms overcome these barriers, a short-term subsidy may then have a lasting employment impact.

A first possibility is that firm owners may not know their own type $(\theta)$, as in Jovanovic (1982). Let $\theta^{*}$ be the managerial ability cutoff at which the unconstrained optimum is to hire a worker. Let $\tilde{\theta}$ be the belief a firm owner has about their own ability. If we consider a distribution of initial beliefs about own managerial ability, then all owners with initial beliefs $\tilde{\theta} \geq \theta^{*}$ will have tried hiring a worker before and either found the worker to be productive or not and so kept or released the worker

[6] If the firm is credit constrained and the wage subsidy increases profits in the short run, these profits may be reinvested with a resulting long-term effect. But the upper bound on the additional profits in this case is the amount of the subsidy. In our case, that is 28,000 LKR, while the median (mean) capital stock excluding land and buildings among the firms in our sample is 160,000 (345,000) LKR.

accordingly. The pool of firm owners who have not previously hired a worker will then consist of owners with low actual managerial ability, as well as those with high actual managerial ability who believe they have low ability. The wage subsidy induces some of these owners to take on a worker while the subsidy is in effect. If this enables them to learn their ability type, then some of these firm owners will discover they were incorrect in their beliefs and retain the worker after the subsidy ends. A second possibility stems from labor market frictions. One set of frictions involves identifying, hiring, and firing workers in an environment where firm owners are unsure of worker types. For example, the search and matching theory of Mortensen and Pissarides (1994) features firms with vacancies who find it difficult to match with qualified workers. If small firm owners find it hard to identify good workers they can trust or find it socially or financially costly to fire them if they are bad, then this cost of hiring will deter some firm owners from hiring workers who, if they turn out to be good matches, will increase firm profits. A wage subsidy can reduce these hiring costs and lead firms to take chances on new workers. This increases employment in the short run and, since firms retain workers who are good matches, will also have a lasting impact on employment beyond the subsidy period.

A second set of labor market constraints may arise from the combination of job-specific human capital and either formal or informal minimum wages that prevent untrained workers being paid their low marginal product (or even being charged to learn on-the-job as in the apprenticeship system studied in Hardy and McCasland, 2015). Workers may be less productive in their first few months while they learn the specifics of the job with productivity increasing over time through on-the-job training. For example, one of the firms in our study was a wedding videographer, who said it took two months of training before a new worker could be sent out to film a small wedding by himself. In the standard model previously mentioned, the firm would pay a new worker his or her marginal product so would pay a low (perhaps even zero or negative) wage at the beginning and then a higher wage once productivity increases. However, poverty constraints, minimum wage laws, and social norms may limit the ability of workers to take low initial wages to compensate for their low initial productivity.[7] This imposes the constraint $w \geq m$ on the optimization problem, where $m$ is this lower bound on the wages that can be paid. A short-term subsidy can compensate firms for the low productivity of workers during this training period and for the fixed costs of hiring workers. If the productivity of workers increases during the period wages are subsidized (Bell, Blundell, and Van Reenen 1999), then they may be sufficiently productive after the subsidies end that firms are willing to pay them wage $w \geq m$ and keep them employed.[8]

These constraints may be one-time constraints faced by the firm owner the first time he hires. For example, after hiring a worker, the firm owner learns about his

[7] Fox and Kaul (2017) discusses several reasons why wages do not fall to match the low productivity levels of workers in many developing-country urban labor markets. For example, they note that subsistence living costs, coupled with the cost of transportation to and from work, set a lower bound on wages at which labor supply is perfectly elastic from the viewpoint of individual firms.

[8] Given enough friction in labor markets, firms may be able to recapture initial losses by paying wages below the marginal product of labor after workers become more productive. But movement of workers across firms may prevent this.

$\theta$ and, after having invested in searching for and hiring a worker, learns about how to find and train workers more efficiently. The one-time subsidy should then have permanent impacts on employment. But some of the constraints might be recurring, and be faced by the firm owner each time he wants to hire a new or replacement worker. In the case of recurring frictions, the one-time subsidy will not have a permanent impact as over time workers will quit or need to be replaced, and the same frictions once again occur for their replacements. But neither should the subsidy be accompanied by a sudden spike in worker exits at the end of the subsidy as would be the case in the Lucas (1978) model. Instead, recurring frictions should lead to an impact which lasts beyond the subsidy period and gradually declines over time.

Summing up, this discussion highlights three types of empirical evidence that can help distinguish between the different theories. The first is the overall treatment impact of the subsidy on employment: with no learning or frictions, the subsidy should not have lasting impacts on firm size. The second is to look at worker churn at the end of the subsidy: under the Lucas (1978) model we should see a spike in worker exits immediately following the end of the subsidy, whereas if the subsidy helps overcome labor market frictions, even recurring ones, this same spike should not occur. Thirdly, the theories also offer predictions for which types of firms may respond more or be more likely to retain a worker after the subsidy: if credit constraints bind and capital is needed to make new workers productive, we should see higher initial take-up among wealthier firms; if learning one's type is an issue, we should expect to see: younger firms and those with no previous experience with workers be more likely to retain workers after the subsidy has ended.

## II. Experimental Design and Data Collection

### A. *The Sample*

We aimed to select a random sample of urban microenterprises with two or fewer paid employees, owned by males aged 20 to 45 and operating in nonagricultural sectors. We chose to focus on male-owned enterprises because our previous work with capital grants showed that male-owned businesses appeared to have more growth potential, with female-owned firms facing additional constraints (de Mel, McKenzie, and Woodruff 2008, 2009). We took a random sample of firms, rather than screening on interest in hiring workers, in order to understand whether the average microenterprise is labor constrained.

To attain this sample of firms, we selected Grama Niladhara (GN) divisions within Colombo, Kandy, and the Galle-Matara areas and went door-to-door listing households from a random starting point. The listing collected information on each adult active in the labor force and was used to screen on age, self-employment status, and sector to select firms for our sample. This was then followed by a baseline survey which collected details of the business and the owner. The first phase of this occurred in April 2008 (see online Appendix 1 for a timeline) as part of a larger panel survey that also included other urban areas in Sri Lanka. We then returned in October 2008 and conducted a booster listing exercise and survey in neighboring GNs to attain a larger sample for our intervention, reinterviewing those interviewed in the original sample. After dropping those firms that had closed since the first baseline, this gave a sample of 1,533 firms. Online Appendix 2 provides more details on the sampling methodology.

### B. *The Intervention*

Our main intervention consists of a temporary wage subsidy to firms with the purpose of encouraging owners to hire an additional full-time employee. The April 2009 survey—taken before anyone was made aware of the wage-incentive program—asked for information about each employee currently working at the enterprise. Our sample is mainly single-person enterprises with 81 percent having no paid or unpaid workers at baseline. In early July, we notified those assigned to the wage-incentive treatment that we would pay a flat amount of 4,000 LKR per month for a period of 6 months if they hired an additional employee working at least 30 hours per week and a flat amount of 2,000 LKR per month for a further 2 months. The half-subsidy rate for the last two months was intended to make the transition from subsidized to nonsubsidized labor less drastic with the goal of increasing the likelihood the worker would stay with the firm.

The employee had to be someone living outside the owner's household and could not be an immediate family member (spouse, parents, siblings, and children). Participants were told that payments would start in August 2009 and, regardless of when the worker was hired, end by May 2010. In other words, workers had to be hired by October 1, 2009 to be paid the full amount of the subsidy.[9] The subsidy represents about half of the earnings of a typical unskilled worker. It is also approximately half the minimum wage, which in Sri Lanka is set by Wage Boards and ranged from approximately 7,000 to 8,000 LKR per month during the time of the intervention. Note that the minimum wages only apply to formally registered workers.

Several studies of the impacts of wage subsidies on workers in developing countries have found employers reluctant to register hired workers formally in the Social Security system where they would have to pay labor taxes (e.g., Galasso, Ravallion, and Salvia 2004 and Groh et al. 2016). Since the vast majority of microenterprises in Sri Lanka do not register their workers (de Mel, McKenzie, and Woodruff 2013), we did not make legal registration of workers a requirement of the program. Once we were notified by the participant that a worker had been hired, we sent a research assistant to conduct an interview with the new employee. We also conducted a short interview with the owner focused on the search and hiring process. Research assistants then made frequent unannounced visits to the enterprise to make sure the employee was working with the median firm receiving 21 visits. In cases where we could not confirm after multiple visits that the employee was working full time, payment was withheld that month and the subsidy then removed if no further evidence

[9] Firms therefore had three months to find a worker. In April 2009, in a survey we conducted of 160 owners with 5 or fewer employees, the median owner said it would take 7 days to locate an employee if s/he wanted to hire one; the mean search period was 14 days. Therefore, we do not believe this deadline distorts the optimal search duration, but was used to prevent procrastination and to ensure treatment occurred at a similar time for all firms.

of work was seen on subsequent visits. There was only one case in which we were never able to verify the existence of an employee. Our measures of take-up reported later in this paper are for verified take-up for which payment was made.

In order to determine whether the effectiveness of the wage subsidy differs with the availability of complementary inputs, we also carried out two supplementary interventions. The first was a savings intervention, in which individuals were offered a savings account in which we matched deposits made up to a specified amount. This took place before the wage subsidies started, and the goal of this intervention was to enable firm owners to build up a balance of savings, which they could then use to supplement the worker with any additional capital required to make this worker more productive. The second was a business training intervention, which also took place before the wage subsidies started. Firm owners were offered the ILO's Improve Your Business (IYB) training to allow for the possibility that better business practices are needed in order to be able to successfully employ additional labor. Online Appendix 3 describes these supplementary interventions in more detail, and shows the estimated impacts of these interventions on our main outcomes.

### C. *Randomization and Balance*

After conducting the baseline survey with those in the booster sample, we stratified firms into six strata using geographic region (Colombo, Kandy, or Galle/Matara) and sector (retail or manufacturing/services). Within each stratum, we then randomly assigned 18.7 percent to the control group (286/1,533), 16.3 percent (250/1,533) to get the wage subsidy program alone, 19.3 percent (297/1,533) to get the wage subsidy and the supplementary savings program, 19.3 percent (297/1,533) to get the wage subsidy and the supplementary training program, 7.3 percent (112/1,533) to get the supplementary savings program alone, 9.2 percent (141/1,533) to get the supplementary training program alone, and 9.8 percent (150) to get the supplementary training and savings programs.

Given the number of groups and the irregular sample sizes across groups, it was not possible to stratify the randomization further within strata.[10] In order to improve balance further on a set of key variables likely to be related to business outcomes, we therefore employed a re-randomization procedure. We re-randomized 1,000 times and in each randomization conducted an $F$-test for equality of means across the 7 treatment groups for a set of 13 baseline variables listed in Table 1, including profits, ability, management practices, number of employees, and business assets. One potential pitfall for this approach can arise from outliers, so we also included dummy variables for profits and assets in the top or bottom 5 percent to reduce the possibility that balance on means was disguising large outliers. We then took the maximum $F$-statistic across these 13 variables and then choose the random assignment from among the 1,000 allocations that had the minimum maximum $F$-statistic. In all reported regressions, we control for the baseline measures of these variables

[10] We choose to put more observations in treatment groups where we were concerned that take-up would be more of an issue in order to have sufficient observations in each cell with which to examine intervention take-up.

TABLE 1—BALANCE AT BASELINE AND ENDLINE

| | Full sample | | | Normalized | Present in round 12 | | | Normalized |
|---|---|---|---|---|---|---|---|---|
| | Control | Treatment | $p$-value | difference | Control | Treatment | $p$-value | difference |
| *Re-randomized variables* | | | | | | | | |
| Number of paid workers | 0.19 | 0.16 | 0.505 | −0.058 | 0.18 | 0.13 | 0.249 | −0.093 |
| Education (years) | 10.34 | 10.25 | 0.669 | −0.037 | 10.35 | 10.23 | 0.614 | −0.048 |
| Raven test score | 3.30 | 3.34 | 0.856 | 0.016 | 3.29 | 3.26 | 0.868 | −0.011 |
| Digit-span recall score | 6.42 | 6.36 | 0.637 | −0.041 | 6.43 | 6.38 | 0.725 | −0.038 |
| Total assets | 239,107 | 250,499 | 0.751 | 0.028 | 246,770 | 246,924 | 0.793 | 0.000 |
| Total assets < 1,500 LKR | 0.06 | 0.02 | 0.063 | −0.163 | 0.06 | 0.02 | 0.022 | −0.219 |
| Total assets > 935,000 LKR | 0.05 | 0.06 | 0.856 | 0.016 | 0.06 | 0.06 | 0.742 | 0.004 |
| Monthly profits | 13,894 | 14,552 | 0.529 | 0.054 | 13,916 | 14,459 | 0.461 | 0.045 |
| Profit data missing | 0.03 | 0.02 | 0.351 | −0.082 | 0.03 | 0.02 | 0.482 | −0.059 |
| Monthly profits < 2,000 LKR | 0.06 | 0.03 | 0.182 | −0.117 | 0.06 | 0.03 | 0.236 | −0.100 |
| Monthly profits > 30,000 LKR | 0.05 | 0.06 | 0.345 | 0.081 | 0.04 | 0.06 | 0.348 | 0.073 |
| Business practices' score | 8.29 | 8.76 | 0.356 | 0.080 | 8.32 | 8.77 | 0.442 | 0.077 |
| From booster sample | 0.52 | 0.53 | 0.808 | 0.021 | 0.53 | 0.55 | 0.872 | 0.036 |
| *Stratification variables* | | | | | | | | |
| Retail sector | 0.39 | 0.38 | 0.857 | −0.016 | 0.39 | 0.39 | 0.996 | −0.004 |
| Colombo | 0.47 | 0.44 | 0.436 | −0.068 | 0.46 | 0.41 | 0.297 | −0.108 |
| Kandy | 0.47 | 0.48 | 0.893 | 0.012 | 0.48 | 0.50 | 0.772 | 0.036 |
| *Additional variables* | | | | | | | | |
| Any paid worker at baseline | 0.12 | 0.10 | 0.388 | −0.075 | 0.12 | 0.08 | 0.115 | −0.131 |
| Monthly sales | 41,175 | 52,435 | 0.048 | 0.171 | 42,079 | 52,010 | 0.073 | 0.150 |
| Owner's age | 35.43 | 35.16 | 0.644 | −0.040 | 35.34 | 35.39 | 0.984 | 0.008 |
| Business is registered for taxes | 0.31 | 0.32 | 0.827 | 0.019 | 0.31 | 0.32 | 0.942 | 0.005 |
| Weekly hours worked | 57.94 | 59.31 | 0.379 | 0.076 | 58.23 | 58.56 | 0.937 | 0.019 |
| Sample size | 286 | 250 | | | | 262 | 231 | |
| Joint orthogonality $p$-value | | | 0.746 | | | | 0.614 | |

*Note:* Present in round 12 denotes information on whether they have a paid worker available in the last survey round.

and for the full set of strata dummies, which Bruhn and McKenzie (2009) show gives the correct size and power after re-randomizing.

Our main analysis uses the sample of 286 pure control enterprises and 250 enterprises assigned to the wage subsidy treatment alone. Table 1 shows that we achieved balance at baseline on a set of important observable variables: we are unable to reject the null hypothesis that these observables are jointly orthogonal to treatment status ($p = 0.734$). In addition, we follow Imbens and Rubin (2015) in considering the normalized difference $\left(\bar{X}_T - \bar{X}_C\right)/\sqrt{\left(\bar{s}_T{}^2 + \bar{s}_C{}^2\right)/2}$ as a measure of balance, where $\bar{X}_j$ and $\bar{s}_j{}^2$ are the sample mean and variance of the variable for the treatment group ($j = T$) and control group ($j = C$), respectively. These normalized differences provide a scale-invariant measure of the difference in locations and show good balance with the largest differences less than 0.2 standard deviations. Online Appendix 3 also shows balance for the supplementary interventions.

Table 1 helps provide a descriptive picture of the owners of these firms and their businesses. The average owner is 35 years old, has finished 10 years of schooling, and works 58 hours a week in his business. Most firms do not have any paid employees. Only 11 percent having at least one paid worker, and there is an average of only 0.17 paid workers per firm. The businesses are mostly informal (only one-third are registered for tax purposes) with 38 percent in retail (e.g., groceries, hardware, and plastic products), and the remainder split between manufacturing (e.g., tailoring, brasswork, carpentry, and food production, about 33 percent of the sample) and services (e.g., electricians, vehicle repair, haircutting, and transportation, about 29 percent of the sample). In 2008, mean monthly profits were 14,184 LKR (approx. US$130) on 46,434 LKR (approx. US$430) of monthly sales.[11]

### D. *Follow-Up Surveys and Attrition*

After the two rounds of baseline, we conducted twice-yearly surveys every April and October from 2009 through 2012, followed by additional surveys in April 2013 and April 2014. Altogether this provides 12 rounds of data, including 2 to 3 rounds pre-intervention, 2 rounds during the intervention, and then 7 rounds post-intervention covering four years after the subsidy ended. Each survey round collected operating data for the previous month, along with details of worker hiring and other information. Online Appendix 4 describes in more detail how key variables were measured. For firms that closed down, we collected information on the current activities of the owner; where owners could not be interviewed, we attempted to obtain basic information on whether the business still existed and the number of employees through observation and discussions with neighbors and family members.

The multiple rounds of follow-up surveys offer several advantages over standard firm studies, which rely on a single follow-up. First, they enable us to trace out the trajectory of impacts to determine whether the treatment effects vary over time. Second, by pooling together data from multiple waves, we can average out seasonality and increase power (McKenzie 2012). Third, they give us multiple chances to interview firm owners, since owners who may not be available one round may be able to be interviewed in a subsequent round. In order to benefit from all three advantages, we pool together rounds 4 and 5 to capture average effects during the intervention, rounds 6 and 7 to capture average effects in the first year after the subsidy ended, rounds 8 and 9 to capture average effects in the second year after the subsidy ended, and rounds 10, 11, and 12 to capture average effects in years 3 and 4 after the subsidy.

Survey attrition was low for a panel of this length with microenterprises. Round-by-round attrition rates averaged 5.6 percent for whether the business was in operation and 9 percent for whether it had a paid worker (see online Appendix 5). Table 2 provides summary information on data availability by time period and treatment status after we pool together several data rounds as previously described. Data are available for 95 percent of the firms during the intervention period, 95–97 percent in the first year after the subsidy, 92–99 percent in the second year after the subsidy, and 96–98 percent in years 3 to 4 post-subsidy. There is no significant difference in attrition rates by treatment status, except for the second year posttreatment where we have slightly higher data availability for the control group. The last four columns of Table 1 also show that the sample responding to the last survey round remains balanced in terms of observable baseline differences. Given the lack of significant

[11] The exchange rate averaged 108 LKR per USD in 2008, was in the 110–115 range from 2009 to 2011, and then averaged 128 LKR per USD in 2012, 129 in 2013, and 130 in 2014.

Table 2—Data Availability by Treatment Status and Timing

| | Before subsidy | During subsidy | After subsidy Year 1 | Year 2 | Year 3+ |
|---|---|---|---|---|---|
| *Panel A. Data on operating status available* | | | | | |
| Control group | 0.958 | 0.951 | 0.983 | 0.986 | 0.976 |
| Wage subsidy treatment | 0.980 | 0.956 | 0.976 | 0.964 | 0.972 |
| *p*-value | 0.148 | 0.787 | 0.596 | 0.099 | 0.799 |
| *Panel B. Data on having a paid worker available* | | | | | |
| Control group | 0.839 | 0.951 | 0.965 | 0.962 | 0.962 |
| Wage subsidy treatment | 0.888 | 0.956 | 0.948 | 0.920 | 0.956 |
| *p*-value | 0.102 | 0.787 | 0.333 | 0.040 | 0.748 |
| *Panel C. Data on profits available* | | | | | |
| Control group | 0.808 | 0.944 | 0.965 | 0.965 | 0.962 |
| Wage subsidy treatment | 0.832 | 0.956 | 0.944 | 0.920 | 0.956 |
| *p*-value | 0.467 | 0.529 | 0.241 | 0.024 | 0.748 |

*Notes:* Before subsidy refers to round 3 data between baselines and intervention. The *p*-value is from the *t*-test of equality of response rates between control and wage subsidy treatment. Proportions shown indicate that data are available for at least one survey round during the specified timing window. Profits and workers are set to zero for firms, which are closed down.

differences in attrition by either treatment status or observables firm characteristics, we maintain a missing-at-random assumption in our analysis for those attriting.

An important point of context is that the period of our study coincided with a period of rapid general economic growth in Sri Lanka. When we began our study in 2008, per capita GNI (in constant 2011 PPP international dollars) was 7,598.[12] In May 2009, just before our wage subsidy intervention period began, the 25-year civil war ended, and the Sri Lankan economy grew at 8 to 9 percent per year over the 2010 to 2012 period with per capita GNI reaching 10,396 in 2014, the year of our last survey. We are therefore testing the return to additional labor in a growing economy, where firms may be expected to have opportunities to potentially grow.

## III. Take-Up and Who Did They Hire?

### A. *Take-Up*

During the eight months the incentive program was active, 60 of the 250 firms offered only the wage subsidy took it up (24 percent). The take-up rates were not statistically different ($p = 0.622$) in the wage subsidy plus savings (24.2 percent), and for the wage subsidy plus training (21.2 percent) treatment groups, giving a total of 196 firms that used the subsidy for at least 1 month. Conditional on using the subsidy, the median firm used it for 7 out of the 8 possible months and received a total of 24,000 LKR in subsidy. Only 17 percent of those using the subsidy used it for 4 or fewer months, and 68 percent used it for 6 months or more.

[12] Source: World Development Indicators, World Bank.

Table 3—Correlates of Take-Up

| | Wage subsidy-only sample | | | Any wage subsidy treatment | | |
|---|---|---|---|---|---|---|
| Retail | −0.008 | | −0.036 | 0.066 | | 0.037 |
| | (0.069) | | (0.070) | (0.038) | | (0.038) |
| Manufacturing | 0.126 | | 0.105 | 0.120 | | 0.097 |
| | (0.067) | | (0.066) | (0.039) | | (0.039) |
| Colombo | −0.194 | | −0.135 | −0.205 | | −0.156 |
| | (0.093) | | (0.095) | (0.054) | | (0.055) |
| Kandy | −0.069 | | −0.040 | −0.147 | | −0.122 |
| | (0.092) | | (0.093) | (0.054) | | (0.054) |
| Formally registered | 0.004 | | −0.007 | 0.038 | | 0.023 |
| | (0.060) | | (0.058) | (0.031) | | (0.031) |
| Any paid worker at baseline | 0.085 | | 0.044 | 0.014 | | −0.009 |
| | (0.086) | | (0.081) | (0.046) | | (0.045) |
| Above median assets | −0.011 | | −0.031 | 0.026 | | −0.001 |
| | (0.056) | | (0.053) | (0.031) | | (0.030) |
| Firm five years or younger | 0.076 | | 0.073 | 0.033 | | 0.031 |
| | (0.053) | | (0.053) | (0.029) | | (0.030) |
| Owner's education (years) | | 0.020 | 0.016 | | 0.014 | 0.013 |
| | | (0.011) | (0.011) | | (0.006) | (0.006) |
| Baseline business practice index | | 0.011 | 0.010 | | 0.011 | 0.009 |
| | | (0.004) | (0.004) | | (0.002) | (0.003) |
| Owner's age | | −0.002 | 0.000 | | 0.002 | 0.002 |
| | | (0.004) | (0.004) | | (0.002) | (0.002) |
| Sample size | 250 | 250 | 250 | 843 | 844 | 843 |
| Pseudo-$R^2$ | 0.057 | 0.050 | 0.093 | 0.031 | 0.036 | 0.054 |

*Notes:* Coefficients are marginal effects from probit estimation. Robust standard errors are in parentheses.

Table 3 examines the correlates of the take-up decision, building on early analysis presented in de Mel, McKenzie, and Woodruff (2010), which only had take-up data through to November 2009. We conduct probits of the probability of using the wage subsidy voucher for the wage subsidy-only treatment group and for all treatment groups offered the wage subsidy. The first column examines firm characteristics, the second owner characteristics, and the third both together. We see that take-up rates are lower in Colombo than in the southern cities of Galle and Matara with Kandy in between. One possible reason is that wage rates are higher in Colombo, so the flat-rate wage subsidy may cover a lower proportion of the worker's wage there. We find that many firm characteristics have very little predictive power for which firms take up the intervention: there are no significant differences in take-up for those that already had paid workers, for those firms that were formally registered, for firms that had more assets at baseline, or by firm age. We do find differences in take-up by sector with manufacturing firms being more likely to use the subsidy than those in retail or services. The skills of the owner also matter. More highly educated owners and those employing better business practices at baseline are more likely to use the subsidy.

## B. *Who Did They Hire?*

In October 2009, we surveyed both the workers hired under the subsidy program and the employers who hired them. These surveys provide data on the characteristics of the workers and the methods the owners used to find them.[13] The hired workers are 31.5 years of age and have 9.8 years of schooling on average. Close relatives of the owners and those living in the owner's household were not eligible to be hired, but 31.3 percent of hired workers are related to the owner in a more distant way; 15.6 percent are female. Most (83.4 percent) were known to the owner before the hiring, and almost half (48.4 percent) say they live within 1 kilometer of the business. Workers report being paid 1,860 LKR per week with just under one-third of them being paid the subsidy amount or less.[14] They report working just over 50 hours per week on average.

The workers hired through the wage subsidy program appear to be similar in most respects to workers hired at the same time by control-group enterprises and to workers hired after the subsidy period ended by the treatment enterprises themselves. We note that both comparisons are imperfect because of the different circumstances. For example, only 32 percent of those hiring through the subsidy program had any paid employees in April 2009, just before treatment. In contrast, 64 percent of the control-group enterprises hiring over the same period were already employers. With this difference in mind, we find that workers hired by the control group during the 16 months prior to April 2010 are slightly older (33.6 years of age) and less likely to be female (9.4 percent). They are also less likely to be related to the owner (9.4 percent) and are paid a higher wage (3,217 LKR per week); the latter two differences are statistically significant. A second comparison comes from surveys conducted in April 2013 and 2014, where we asked enterprises about workers hired during the previous year. Workers hired later by the subsidy treatment group enterprises are also slightly older (32.6 years), less likely to be female (10.0 percent), less likely to have known the owner previously (71.4 percent), and less likely to live within 1 kilometer of the business (35.7 percent), though none of these differences is statistically significant. The only statistically significant difference is that they are less likely to be related to the owner (10.0 percent). The owners also reported that their search methods were very similar for the employees hired under the subsidy program and in the later period. Asking friends and neighbors for recommendations was by far the most common search method in both periods (used in 50 percent of the subsidy period hires and 85 percent of later hires), followed by asking family members for recommendations (used in 32 percent and 46 percent of the subsidy and later hires, respectively). Arms-length methods (e.g., advertising or posting a notice) were rarely used in either period. Overall, then, the workers hired under the subsidy are similar to those hired outside the program, except that they are somewhat more likely to have some relation to the owner.

[13] Since take-up of the wage subsidy did not differ significantly for the wage subsidy-only treatment compared to those who received the wage subsidy with either savings or training, we use the larger sample of those who received any wage subsidy for this descriptive work.

[14] There is no significant difference in reported hours worked between those paid 1,000 LKR (51.6 hours) per week or less and those paid more than this amount (52.8).

## IV. Results

We first examine whether the wage subsidy changed the survival rates of firms, since, to the extent it did, we need to control for this in examining impacts on employment, profitability, and sales. As noted earlier, our estimation aims to combine the advantages of combining multiple follow-up rounds to increase statistical power with also a desire to explore the trajectory of impacts. Power could potentially be increased further by pooling together the treatment group which received only the wage subsidy with those firms that also received the savings or business training treatments along with the subsidy. We present these pooled regressions in online Appendix 3. This pooled treatment would only give the impact of the "labor drop" provided by the wage subsidy if the other treatments had no impacts. While most of the impacts of these other treatments on our key outcomes are not statistically significant, they are positive in magnitude, and we can reject that the trajectory of the impact on employment is the same from the wage subsidy-only treatment as for the wage subsidy combined with other treatments. As a consequence, we therefore focus on the control group and wage subsidy-only treatment groups to more cleanly reflect how the wage subsidy alone impacts firm outcomes.

We estimate treatment regressions using the following specification for outcome $Y$ for firm $i$ in period $t = 3, \ldots, 12$:

$$(5)\quad Y_{i,t} = \alpha + \beta_1 Treat_i \times Pre_t + \beta_2 Treat_i \times During_t + \beta_3 Treat_i \times Year1_t + \beta_4 Treat_i \times Year2_t + \beta_5 Treat_i \times Year3to4_t + \sum_{s=4}^{12} \delta_s 1(t = s) + \theta' X_i + \varepsilon_{i,t},$$

where *Treat* is a dummy variable for whether they got the wage subsidy treatment or not; *Pre* indicates the pretreatment, post-baselines round 3; *During* indicates the two survey rounds 4 and 5 when the wage subsidy was in effect; *Year*1, *Year*2, and *Year*3*to*4 indicate the survey rounds corresponding to 1 year, 2 years, and 3 to 4 years post-intervention; $1(t = s)$ are a set of survey round time dummies; $X$ is a set of controls for the randomization strata and for the baseline variables used in randomization (Bruhn and McKenzie 2009); and the error term $\varepsilon_{i,t}$ is clustered at the firm level. The baseline controls include the baseline values of many of our key outcomes of interest, making this an Ancova specification, but where the baseline value of the outcome of interest is not in $X$, we also include it as an additional control when available. Our interest is then in the trajectory of treatment effects as given by $\beta_2$ to $\beta_5$. To account for multiple testing across periods, we test the equality $\beta_2 = \beta_3 = \beta_4 = \beta_5$ to test whether the treatment effects are stable and $\beta_2 = \beta_3 = \beta_4 = \beta_5 = 0$ to test whether we can reject that there is no treatment effect after the intervention. Note that $\beta_1$ provides a placebo test, similar to a further balance test, since it uses pre-intervention data.

Table 4—Impact on Firm Survival

| | Sample size | Before subsidy | During subsidy | After subsidy Year 1 | Year 2 | Year 3+ | $p$-value equality | $p$-value all zero |
|---|---|---|---|---|---|---|---|---|
| *Panel A. Self-employed in survey round* | | | | | | | | |
| Assigned to treatment | 5,055 | −0.006 | −0.009 | 0.058 | 0.082 | 0.054 | 0.001 | 0.002 |
| | | (0.023) | (0.018) | (0.021) | (0.025) | (0.027) | | |
| Control mean | | 0.927 | 0.958 | 0.885 | 0.850 | 0.831 | | |
| *Panel B. Self-employed, assuming that firms which close and are never observed again have stayed closed* | | | | | | | | |
| Assigned to treatment | 5,095 | −0.006 | −0.014 | 0.054 | 0.075 | 0.054 | 0.002 | 0.004 |
| | | (0.023) | (0.019) | (0.021) | (0.026) | (0.029) | | |
| Control mean | | 0.927 | 0.958 | 0.885 | 0.847 | 0.815 | | |
| *Panel C. Self-employed, assuming that all attritors are closed* | | | | | | | | |
| Assigned to treatment | 5,360 | 0.015 | −0.006 | 0.057 | 0.067 | 0.059 | 0.012 | 0.016 |
| | | (0.027) | (0.026) | (0.028) | (0.029) | (0.030) | | |
| Control mean | | 0.888 | 0.879 | 0.818 | 0.822 | 0.786 | | |
| *Panel D. The original firm as baseline continues operating* | | | | | | | | |
| Assigned to treatment | 5,060 | 0.012 | −0.007 | 0.066 | 0.079 | 0.074 | 0.003 | 0.008 |
| | | (0.023) | (0.021) | (0.027) | (0.032) | (0.035) | | |
| Control mean | | 0.920 | 0.939 | 0.838 | 0.773 | 0.729 | | |
| *Panel E. Is the owner employed?* | | | | | | | | |
| Assigned to treatment | 5,185 | −0.007 | −0.004 | 0.015 | 0.002 | 0.013 | 0.420 | 0.576 |
| | | (0.023) | (0.009) | (0.012) | (0.019) | (0.021) | | |
| Control mean | | 0.927 | 0.989 | 0.959 | 0.933 | 0.906 | | |

*Notes:* Robust standard errors are in parentheses, clustered at the firm level. All regressions control for randomization strata, variables used for re-randomization, and contain survey round dummies. The $p$-values are to test that the treatment effect is equal in the during, year 1, year 2, and years 3 to 4 periods and that the treatment effect is zero in all four periods.

Note that the treatment effects we estimate are intent-to-treat effects, which is the impact of being offered the wage subsidy. This is the relevant parameter for understanding the policy impact of wage subsidy vouchers. We then turn to estimating the impact of actually hiring an additional worker in Section VC.

### A. *Impact on Survival*

Table 4 examines the impact of the wage subsidy on firm survival. Businesses temporarily close and then reopen again, so our main measure of survival here is defined in terms of whether the owner is self-employed at the time of the survey round and includes the case of the owner shutting down one business and starting another one. Survival rates are reasonably high in the control group: 95.8 percent of firm owners are operating their businesses during the intervention period (1 year after baseline), 88.5 percent 1 year after the intervention, and 83.1 percent 3 to 4 years later. Figure 1 shows graphically the survival pattern round by round and shows a clear widening of the gap between the treatment and control group over time. Panel A of Table 4 shows that there is no significant impact on firm survival during the intervention, but significant impacts in all three time periods afterwards. Those that received the subsidy were 5.4 percentage points more likely to still be self-employed in our last follow-up rounds.

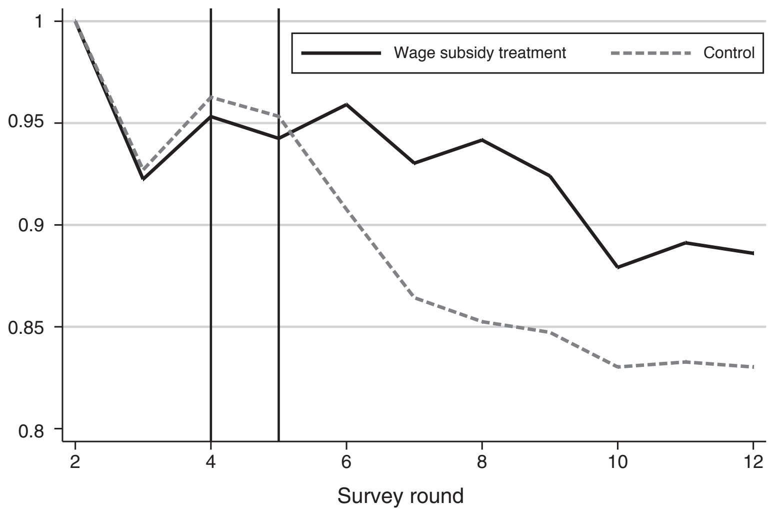


Figure 1. Impact on Firm Survival over Time

*Notes:* The vertical lines indicate the period during which wage subsidy was in effect. Round 2 is the baseline for half the sample and first follow-up for the other half. Firm survival is measured by whether the owner is self-employed.

Recall that data on operating status are not available for 3 percent of firms in the three-to-four-year period, and these firms may have also closed. In panels B and C of Table 4, we therefore consider two other definitions of survival for robustness. The first assumes that if a firm is surveyed and found to be closed and then attrits from future surveys that it has remained closed. The second measure makes the assumption that all attriting firms are closed. The impact on survival remains of similar magnitude and is still statistically significant using either alternative. Panel D uses an alternative measure of survival, based on McKenzie and Paffhausen (2017), which is to track whether the original firm open at baseline keeps operating. Only 72.9 percent of the original control-group firms are still operating in our last follow-up rounds, and receiving the wage subsidy increases this by 7.4 percentage points. While the owner is more likely to remain self-employed and the original business more likely to stay open, panel E shows that there is no significant impact on whether the owner is employed for pay in any of the periods—there is an offsetting reduction in wage employment.

We discuss possible reasons for the survival impact in Section V, after having seen the impacts of the subsidies on employment, profitability, and sales.

## B. *Impact on Employment*

To account for this impact on survival, we code firms which are closed as having zero employment, zero profits, and zero sales in our analysis. This enables us to examine the full unconditional impact on these outcomes in a way which is not subject to selectivity concerns present in comparing only firms in operation. We later also provide comparisons of treatment and control profits and sales conditional on survival.

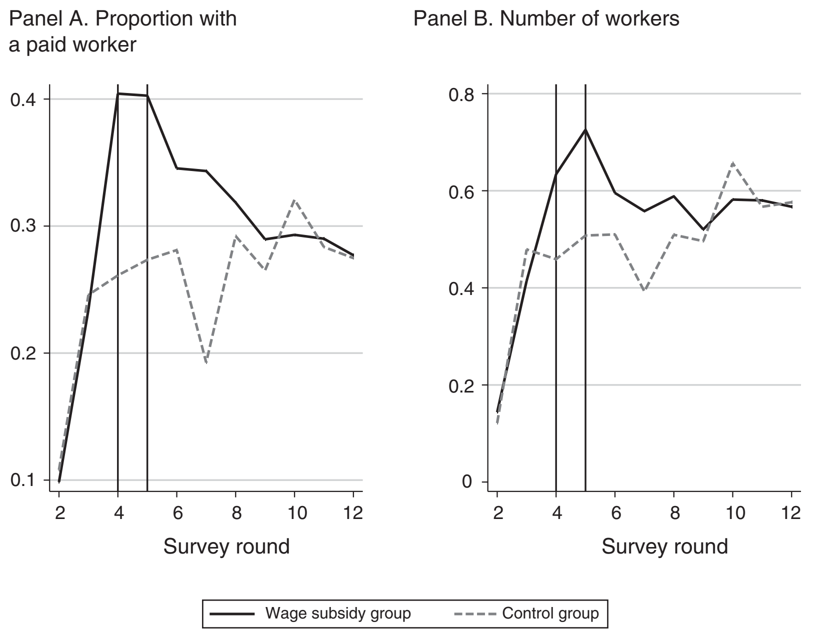


FIGURE 2. IMPACT ON EMPLOYMENT

*Notes:* The vertical lines show the intervention period. The number of workers truncated at five workers. Round 2 is the baseline for half the sample and first follow-up for the other half.

Figure 2 shows the time pattern of whether firms have any paid workers and of the average number of paid workers (truncated at 5 workers, the 99th percentile during the intervention period). We see the treatment and control group have similar employment prior to the intervention, and that the treatment group hires more workers than the control during the intervention period. This gap halves in the year following the intervention but is still noticeable, and then the employment of the two groups looks similar in the last four survey rounds. A further point to note is that employment in the control group is slowly growing over time, so the counterfactual is one in which some firms would be hiring even without the subsidy.

The first two panels of Table 5 examine whether this impact is significant. We see during the subsidy period there is a positive and statistically significant increase in both the likelihood of having any paid workers and in the number of workers hired. The 14 percentage point increase in the likelihood of having any paid workers is relative to a control mean of 27 percent during this time, so the subsidy resulted in a 52 percent increase in the likelihood of having a worker during this period. The impact on the number of paid workers is 0.19 workers, relative to a control mean of 0.48 workers, so again represents a sizeable increase in relative terms. The impact on having at least one paid worker remains positive and significant at 11.1 percentage points in the year after the intervention, but then falls to near zero and is not statistically significant in either the second or third and fourth years. The impact on the number of paid workers is 0.13 workers in the year after the intervention, although this gap is not statistically significant; the gap then falls further over time to near zero and is insignificant in the longer term.

Table 5—Impact on Employment

| | Sample size | Before subsidy | During subsidy | After subsidy Year 1 | After subsidy Year 2 | After subsidy Year 3+ | $p$-value equality | $p$-value all zero |
|---|---|---|---|---|---|---|---|---|
| *Panel A. Number of paid workers* | | | | | | | | |
| Assigned to treatment | 4,879 | −0.073 | 0.194 | 0.126 | 0.051 | −0.024 | 0.057 | 0.038 |
| | | (0.081) | (0.075) | (0.077) | (0.079) | (0.083) | | |
| Control mean | | 0.48 | 0.48 | 0.45 | 0.50 | 0.60 | | |
| *Panel B. Any paid worker* | | | | | | | | |
| Assigned to treatment | 4,879 | −0.012 | 0.138 | 0.111 | 0.028 | −0.005 | 0.000 | 0.000 |
| | | (0.035) | (0.035) | (0.034) | (0.035) | (0.032) | | |
| Control mean | | 0.25 | 0.27 | 0.24 | 0.28 | 0.29 | | |
| *Panel C. Added a worker between survey rounds* | | | | | | | | |
| Assigned to treatment | 4,677 | −0.006 | 0.105 | 0.016 | −0.040 | −0.006 | 0.000 | 0.000 |
| | | (0.036) | (0.024) | (0.020) | (0.020) | (0.017) | | |
| Control mean | | 0.192 | 0.115 | 0.095 | 0.136 | 0.126 | | |
| *Panel D. Subtracted a worker between survey rounds* | | | | | | | | |
| Assigned to treatment | 4,677 | −0.021 | −0.006 | 0.014 | 0.028 | −0.006 | 0.431 | 0.571 |
| | | (0.016) | (0.019) | (0.022) | (0.020) | (0.016) | | |
| Control mean | | 0.050 | 0.113 | 0.138 | 0.095 | 0.107 | | |
| *Panel E. Own hours worked in the business* | | | | | | | | |
| Assigned to treatment | 4,937 | −0.574 | 1.063 | 3.288 | 4.373 | 4.245 | 0.511 | 0.184 |
| | | (2.256) | (1.920) | (1.971) | (2.071) | (1.977) | | |
| Control mean | | 47.3 | 51.3 | 43.8 | 44.6 | 42.6 | | |
| *Panel F. Number of unpaid workers in business* | | | | | | | | |
| Assigned to treatment | 4,840 | −0.043 | 0.012 | −0.025 | 0.023 | 0.004 | 0.631 | 0.756 |
| | | (0.046) | (0.035) | (0.044) | (0.037) | (0.036) | | |
| Control mean | | 0.21 | 0.18 | 0.26 | 0.17 | 0.22 | | |

*Notes:* Robust standard errors are in parentheses, clustered at the firm level. All regressions control for randomization strata, variables used for re-randomization, the baseline value (except for panels C and D on churn), and contain survey round dummies. Regressions are unconditional and assign zero to the outcome for firms not operating. The $p$-values are for testing that the treatment effect is equal in the during, year 1, year 2, and years 3 to 4 periods and that the treatment effect is zero in all four periods.

Figures 3 and 4 delve into the employment changes in more detail by examining the churn in employment. Figure 3 looks at the probability a firm increases or decreases its number of paid workers between each survey round. We see treated firms are more likely to add workers during the intervention period, but less likely than the control group to add workers between rounds 7 and 10. Immediately after the intervention, there is a spike in workers departing from treatment firms: the treatment group reduces the number of workers it has in the six months immediately following the end of the subsidy. Figure 4 examines the change in the number of workers. We see almost all of the action is at the margin of a single worker. As the subsidy begins, the treatment group is more likely to add a worker, and once the subsidy ends, it is more likely to subtract a worker. We see that 78 percent of the control group has no change in worker numbers between rounds, so that approximately one in five control-group firms are adding or subtracting workers from one round to the next. Since this is churn over a six-month period, it suggests that many control-group firms are able to adjust their employment rapidly.

The next two panels of Table 5 examine this churn econometrically. We see a positive and significant impact on the likelihood of adding a worker during the subsidy

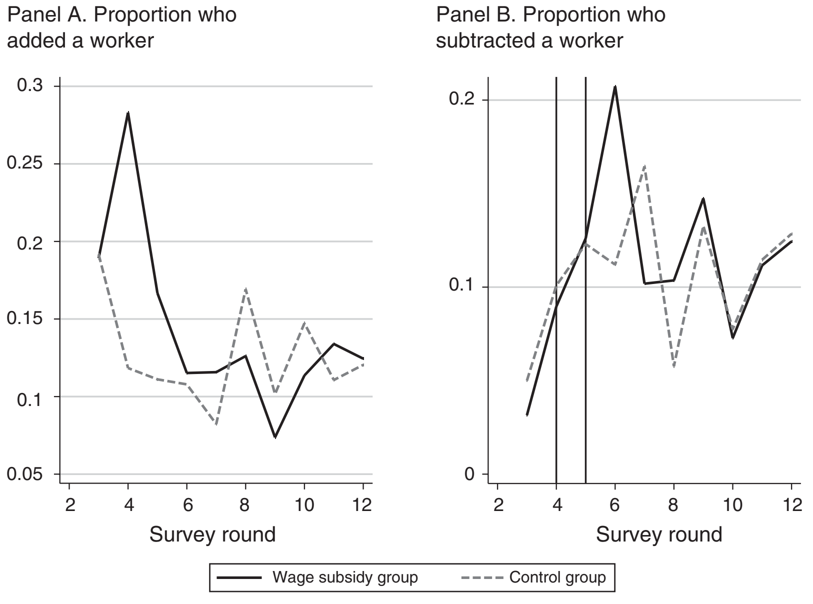


Figure 3. Employment Churn

*Notes:* The vertical lines show the intervention period. Addition and subtraction of workers are defined in terms of changes in the total number of paid workers the firm has between one survey round and the next.

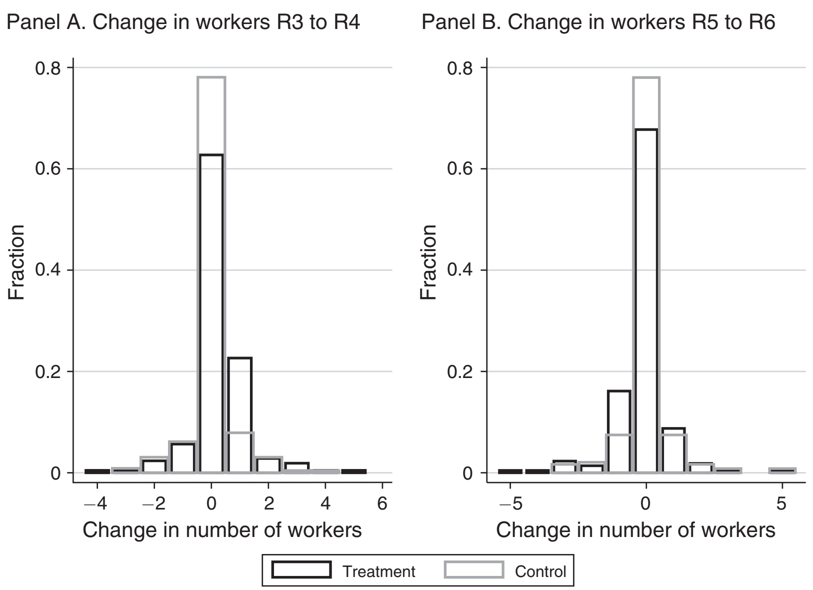


Figure 4. Churn in the Number of Workers

*Notes:* R3 to R4 is the change in the number of workers between the last pre-intervention survey and first survey during the wage subsidy. R5 to R6 is the change in the number of workers between the last survey during the intervention and first survey after the intervention.

period, but a negative and significant impact on the likelihood of adding a worker during the period two years after the intervention. That is, post-subsidy, firms in the treatment group are slower to add workers during a period when firms in the control group are growing. In contrast, the impact on subtracting workers is not as dramatic as seen in Figures 3 and 4 and, while positive immediately after the intervention, is not statistically significant. Graphically, we see that this subtraction effect occurs in the six months immediately after the subsidy ends, and by averaging over the first year, we average in also the lower chance of subtracting a worker between 6 and 12 months post-intervention. Dropping round 7, where there appears to be some seasonality, results in a statistically significant 9.3 percentage point (s.e. 3.4 percentage points) increase in the likelihood treated firms subtract a worker in the first six months after the subsidy.

The final two panels of Table 5 consider whether the change in paid workers is changing the other two labor inputs in the business: the owner's own time and unpaid labor. At baseline, only 9.3 percent of firms had any unpaid workers. On average, firms have only 0.2 unpaid workers during our study period, and treatment has no significant impact on this number during any of the follow-up periods. The point estimates on own hours are positive, but small and not statistically significant during the intervention. The positive effect is significant for the follow-up periods, and reflects the greater survival of firms at this stage.[15] Taken together, these results show that the subsidized labor is not substituting for other types of labor the business is already using, but represents a net increase in labor input during the subsidy period.

Online Appendix 6 explores the extent to which the wage subsidy changes which firms have workers. The evidence suggests that the new firms induced to hire an employee because of the subsidy, but who would not have hired one if they had been in the control group, are smaller and less profitable firms and less likely to be found in Colombo. There is little selectivity on owner's characteristics. Conditional on hiring a worker, those in Colombo are more likely to have kept the worker on, with no significant differences in other firm or owner characteristics. By the time of the last survey, when the proportion of firms with employees is similar in both groups, some of these lower profitability treated firms no longer employ a worker, while some of the lower profitability control firms have started to employ one. The result is that baseline profit levels and other firm characteristics are similar for the sample of treatment and control firms with workers at the time of the last survey. The one remaining difference is geographic—a smaller share of the treated firms with employees is in Colombo compared to the control group.

## C. *Impact on Profitability and Sales*

We next examine how the wage subsidy and additional labor affected business profits and sales. There are two important issues that affect measurement of the treatment effect on these outcomes. The first, as previously discussed, is the treatment

[15] Conditional on being in business, treated firm owners are working an average of 1.07 more hours per week in the post-intervention period, which is not statistically significant ($p = 0.402$).

impact on business survival post-intervention. We consider both unconditional profits and sales (where firms not in operation are coded as having zero profits and zero sales) and conditional measures (conditioning on the business operating). Comparing outcomes for treated and control firms will only yield unbiased estimates of the impact on conditional profits and sales if survival is not selective on characteristics that predict these outcomes. We cannot reject that survival is independent of baseline characteristics (a joint test of orthogonality for the sample still self-employed in round 12 has $p$-value 0.568), so this may be a reasonable assumption in our case. Otherwise, the conditional regressions should be viewed as descriptive, rather than causal. Second, both profits and sales have long right tails: the 99th percentile conditional on operating is 6 to 8 times the mean and kurtosis values of 14 to 20 (compared to 3 for a normal distribution). We use two approaches to reduce the dependence on the top tail. The first is to consider transformations which place less weight on the top tail: the inverse hyperbolic sine transformation of unconditional values and the log transformation of conditional values. The second is to estimate quantile regressions.[16]

Table 6 presents the estimated treatment effects on these different measures, while Figures 5 and 6 graph the quantile treatment effects for round 4 and round 12 (during the intervention and in our last follow-up round four years later). For profits, we see no significant effect during the intervention period for any of the four measures, and the quantile treatment effects on both conditional and unconditional profits are fairly constant and also not significant across the distribution. The 95 percent confidence interval for the OLS treatment effect for unconditional profits is (−1,749, 3,104), while for the fiftieth percentile it is (−992, 3,334). This is relative to a control mean of 16,603 LKR so represents a range of −11 percent to +19 percent. We cannot reject the hypothesis that all treatment effects are jointly zero for three out of four measures of profits in Table 6. The exception is the inverse hyperbolic sine transformation. This shows a positive treatment effect, which arises from the survival effect at the bottom of the distribution, with positive treatment effects for lower quantiles. Conditional on operating, this effect is not significant.

For sales, we likewise see no significant treatment effect during the intervention using any of the four measures, nor using the quantile treatment effects. The 95 percent confidence interval for unconditional sales is (−8,371, 19,593), or (−14 percent, +32 percent) relative to the control mean. Like profits, the inverse hyperbolic sine transformation of sales shows a significant treatment effect in the post-intervention period, reflecting the impact on survival. We find a significant effect on unconditional sales in year two and on log sales in year one post-intervention, but in both cases cannot reject that all treatment effects are jointly zero.

### D. *Return to Labor*

We now can combine the impact on the number of workers with the impact on profits to obtain an estimate of the return to additional labor in these microenterprises.

[16] We control for the same baseline controls in the quantile regressions as in the OLS treatment regressions.

Table 6—Impact on Profits and Sales

| | Sample size | Before subsidy | During subsidy | After subsidy Year 1 | Year 2 | Year 3+ | $p$-value equality | $p$-value all zero |
|---|---|---|---|---|---|---|---|---|
| *Panel A. Unconditional profits (truncated at ninety-ninth percentile)* | | | | | | | | |
| Assigned to treatment | 4,692 | 873 | 6,786 | 1,906 | 2,110 | 1,431 | 0.727 | 0.478 |
| | | (1,479) | (1,235) | (1,150) | (1,445) | (1,175) | | |
| Control mean | | 14,572 | 16,603 | 16,492 | 18,534 | 17,808 | | |
| *Panel B. Inverse hyperbolic sine of profits* | | | | | | | | |
| Assigned to treatment | 4,692 | −0.028 | 0.193 | 0.610 | 0.842 | 0.601 | 0.145 | 0.048 |
| | | (0.282) | (0.215) | (0.242) | (0.286) | (0.297) | | |
| Control mean | | 9.21 | 9.38 | 8.94 | 8.77 | 8.54 | | |
| *Panel C. Profits conditional on business operating (truncated at ninety-ninth percentile)* | | | | | | | | |
| Assigned to treatment | 4,197 | 1,091 | 871 | 1,008 | 698 | 530 | 0.988 | 0.931 |
| | | (1,530) | (1,258) | (1,165) | (1,489) | (1,213) | | |
| Control mean | | 15,954 | 17,356 | 18,658 | 21,975 | 21,588 | | |
| *Panel D. log profits conditional on business operating* | | | | | | | | |
| Assigned to treatment | 4,158 | 0.072 | 0.042 | 0.056 | 0.010 | 0.002 | 0.837 | 0.867 |
| | | (0.070) | (0.057) | (0.059) | (0.061) | (0.056) | | |
| Control mean | | 9.39 | 9.45 | 9.55 | 9.71 | 9.72 | | |
| *Panel E. Unconditional sales (truncated at ninety-ninth percentile)* | | | | | | | | |
| Assigned to treatment | 4,784 | −3,795 | 5,611 | 7,290 | 17,699 | 7,172 | 0.373 | 0.343 |
| | | (7,283) | (7,117) | (7,718) | (8,833) | (9,349) | | |
| Control mean | | 51,783 | 60,638 | 61,385 | 69,015 | 72,723 | | |
| *Panel F. Inverse hyperbolic sine of sales* | | | | | | | | |
| Assigned to treatment | 4,784 | −0.046 | 0.190 | 0.760 | 0.977 | 0.635 | 0.059 | 0.016 |
| | | (0.298) | (0.235) | (0.251) | (0.309) | (0.321) | | |
| Control mean | | 10.16 | 10.33 | 9.87 | 9.63 | 9.45 | | |
| *Panel G. Sales conditional on business operating (truncated at ninety-ninth percentile)* | | | | | | | | |
| Assigned to treatment | 4,284 | −4,000 | 6,809 | 4,651 | 14,295 | 4,925 | 0.553 | 0.541 |
| | | (7,769) | (7,497) | (8,282) | (9,561) | (10,759) | | |
| Control mean | | 56,577 | 63,361 | 69,061 | 81,830 | 88,142 | | |
| *Panel H. log sales conditional on business operating* | | | | | | | | |
| Assigned to treatment | 4,263 | −0.003 | 0.103 | 0.135 | 0.091 | 0.066 | 0.829 | 0.485 |
| | | (0.087) | (0.074) | (0.079) | (0.077) | (0.081) | | |
| Control mean | | 10.41 | 10.44 | 10.49 | 10.73 | 10.73 | | |
| *Panel I. Total income from all work (truncated at the ninety-ninth percentile)* | | | | | | | | |
| Assigned to treatment | 4,485 | 775 | 805 | 1,735 | 897 | 486 | 0.824 | 0.762 |
| | | (1,422) | (1,249) | (1,274) | (1,598) | (1,198) | | |
| Control mean | | 14,524 | 17,144 | 17,984 | 20,249 | 19,128 | | |

*Notes:* Robust standard errors are in parentheses, clustered at the firm level. All regressions control for randomization strata, variables used for re-randomization, the baseline value, and contain survey round dummies. The $p$-values are for testing that the treatment effect is equal in the during, year 1, year 2, and years 3 to 4 periods and that the treatment effect is zero in all four periods. The total income from all work includes business profits and any earnings from paid wage and casual labor.

For comparison, we begin by using the pooled cross sections from the control-group sample to estimate for firm $i$ in periods $t = 2, \ldots, 12$:

$$(6) \qquad Profits_{i,t} = \alpha + \beta_1 \times L_{i,t} + \sum_{s=3}^{12} \delta_s 1(t = s) + \theta' X_i + \varepsilon_{i,t},$$

where $L$ is the number of paid workers, and we control for time dummies, the variables used to form strata for randomizing, and the baseline variables used in

TABLE 7—RETURN ON LABOR

| | Level of profits | | | | log of profits | | |
|---|---|---|---|---|---|---|---|
| | Associations in control group | | Treatment IV estimates | | Associations in control group | | |
| | Cross section (1) | Panel data (2) | Unconditional profits (3) | Conditional profits (4) | Cross section (5) | Panel data (6) | IV treatment effect (7) |
| Number of paid workers | 6,214 | 4,903 | 2,586 | 3,270 | 0.198 | 0.127 | 0.131 |
| | (748) | (696) | (6,358) | (5,974) | (0.021) | (0.023) | (0.295) |
| Sample size | 2,670 | 2,670 | 959 | 913 | 2,320 | 2,320 | 892 |

*Notes:* Robust standard errors are in parentheses, clustered at the firm level. Regressions control for time fixed effects, randomization strata, and controls used in re-randomization. Columns 1, 2, 5, and 6 use control group only. Columns 3, 4, and 7 use wage subsidy only and control groups. The IV estimates instrument the number of paid workers with assignment to the wage subsidy treatment.

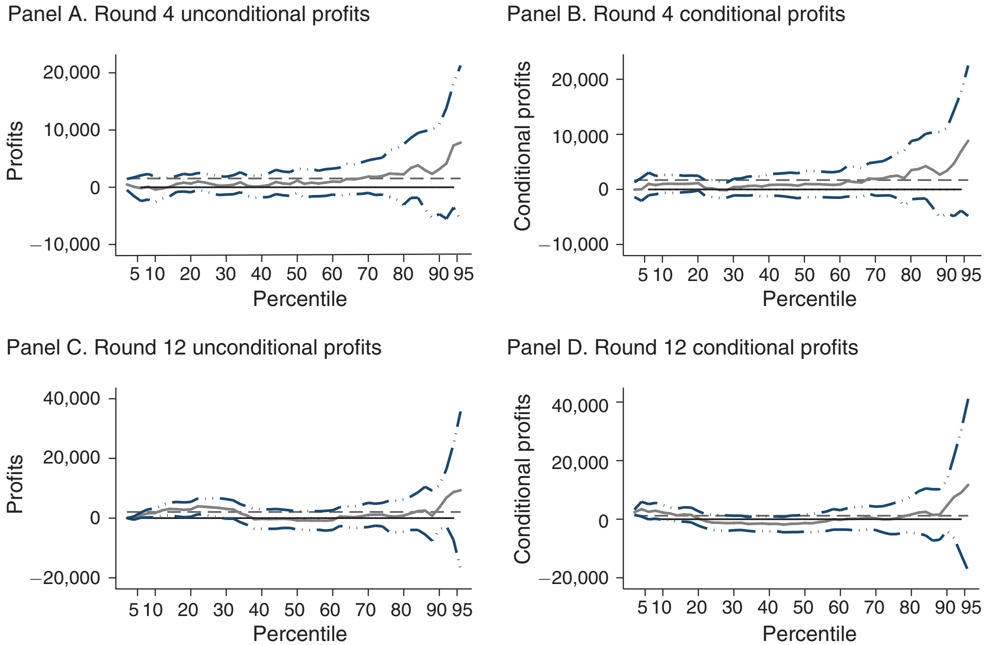


FIGURE 5. QUANTILE TREATMENT EFFECTS ON PROFITS IN ROUND 4 (*during intervention*) AND ROUND 12 (*four years later*)

*Notes:* The solid line shows quantile treatment effects with a 95 percent confidence interval around it. The dashed line indicates the OLS treatment effect. The quantile regressions control for baseline profits, randomization strata, and the set of re-randomized variables. Unconditional profits include zeros for firms not operating; conditional profits are conditional on the business still operating.

re-randomization as before. The standard errors are clustered at the firm level. Column 1 of Table 7 shows this estimate using the unconditional level of profits as the outcome, while column 5 uses log profits (conditional on operating). We see an additional worker is significantly associated with higher profits with firms with one more worker earning 6,214 LKR per week more profits or 22 percent higher profits.

The standard concern with such an estimate is that there are unobserved features of the firm that are correlated with both profitability and how many workers the

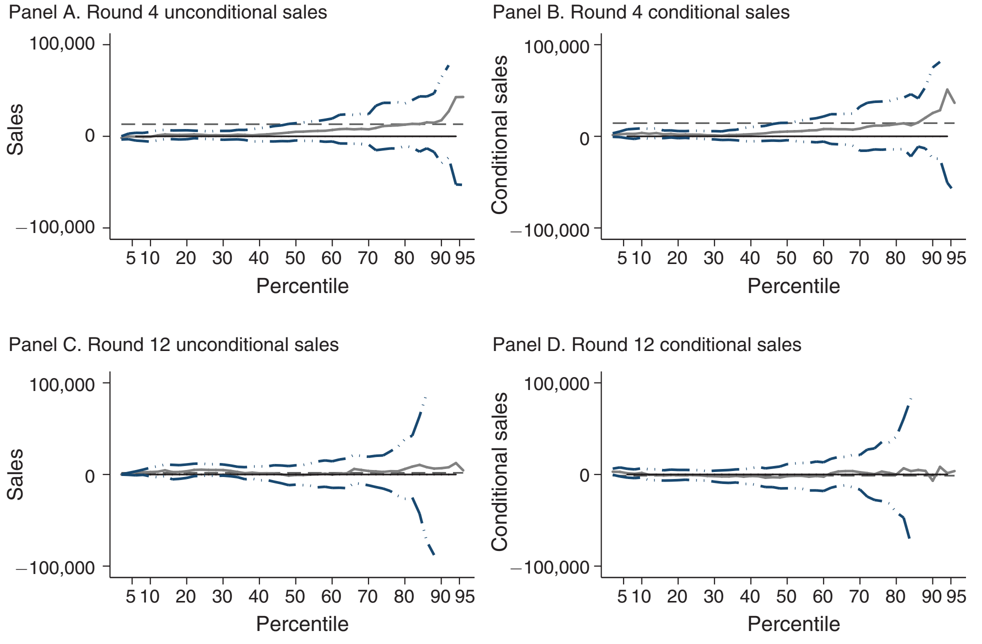


Figure 6. Quantile Treatment Effects on Sales in Round 4 (*during intervention*) and Round 12 (*four years later*)

*Notes:* The solid line shows quantile treatment effects with a 95 percent confidence interval around it. The dashed line indicates the OLS treatment effect. Quantile regressions control for baseline profits, randomization strata, and the set of re-randomized variables. Unconditional profits include zeros for firms not operating; conditional profits are conditional on the business still operating.

firm hires. For example, more productive firms may earn more and use more labor. We can control for time-invariant unobserved firm characteristics by adding firm fixed effects to equation (6). Columns 2 and 6 of Table 7 do this. Adding these fixed effects reduces the magnitude of the coefficient on labor to 4,903 LKR or 14 percent, which is still statistically significant. However, this will still overstate the return to labor if there are time-varying unobservables, which lead firms to both hire more workers and be more profitable: for example, a positive demand shock may cause the firm to be able to sell more and hire more workers to meet this demand.

Our experiment provides an estimate of the causal effect of hiring an additional worker. Since the intervention period is the only period during which we see a significant increase in the number of workers, we estimate equation (6) only during the intervention period ($t = 4$ and 5) and instrument $L$ with assignment to the wage subsidy treatment. Columns 3, 4, and 7 of Table 7 report the IV estimates under the assumption that the wage subsidy only affects profits through the additional labor it induces. A potential threat to this exclusion restriction would be firms adjusting capital stock along with labor. Online Appendix 7 shows that there was no significant treatment impact on capital stock during the intervention period, and the magnitude of the point estimates suggest capital can explain only a small fraction of the observed profit change.

The point estimates of the marginal return to labor from this IV estimation are smaller than the fixed effects estimates in levels and similar in size to the fixed effects estimates in logs; however, the standard errors are much larger. The point estimates suggest a return of 2,600 to 3,300 LKR per month per additional worker hired. Recall that the wage subsidy was 4,000 LKR per month during this time and is not included in the measure of profit being reported by the owner. This point estimate would suggest that the net return to keeping these workers is their current wages would be negative once the subsidy was removed. This is consistent with many firms stopping employing workers once the subsidy ends. However, we acknowledge that the large standard errors imply that there is considerable imprecision in our estimated return to labor.[17]

## V. Mechanisms

The wage subsidies induced firms to hire additional workers during the intervention period, but did not have a lasting impact on employment, nor a significant impact on profits and sales. They did however increase survival rates of firms. We examine possible explanations for these impacts in this section.

### A. *Heterogeneity of Treatment Effects*

Our theory suggests several possible channels through which a short-term subsidy may have a lasting impact on employment. We examine the heterogeneity of treatment effects to provide some insights into whether these different channels appear to be operating in practice. To do this, we add interactions (one at a time) between the treatment variables and baseline covariates $X$ to equation (5), along with controlling for the baseline value of $X$, and interactions between $X$ and the survey round effects. Since our sample sizes become small once we split the sample, we pool together all posttreatment survey rounds to show heterogeneity in the posttreatment impact averaged over all four years after the subsidy. The results are presented in Table 8, which tests for heterogeneity in the impact on the number of paid workers for different $X$s.

If the subsidy allows firm owners to learn about their management type, $\theta$, then we would expect the retention impacts to be greater for younger firms (where the owners have had less time to learn their type) and for firms whose owners have never hired a worker before. The first two columns of Table 8 show that there is no significant heterogeneity with respect to either variable. Moreover, the coefficients indicate that that those who had previously hired workers were more likely to hire and retain workers in response to the intervention, which is the opposite sign to what the learning-about-type theory would predict.

[17] If we are willing to further assume that the savings and training treatments did not affect profits other than through the number of employees, then we could use all three wage subsidy treatments as instruments. Doing so gives an estimated return on labor of 823 LKR (s.e. 3,564) using levels of profits and 0.125 (s.e. 0.157) using log profits. That is, it leads to lower point estimates. However, these additional exclusion restrictions seem less likely to hold (for example, training led to an increase in business practices, which could independently affect profits).

Table 8—Heterogeneity in the Impact of the Wage Subsidy on the Number of Employees

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Before subsidy | −0.175 | −0.066 | −0.127 | −0.072 | −0.112 | −0.142 |
| | (0.112) | (0.061) | (0.145) | (0.111) | (0.123) | (0.081) |
| During subsidy | 0.105 | 0.170 | −0.087 | 0.143 | 0.154 | 0.091 |
| | (0.106) | (0.069) | (0.128) | (0.095) | (0.115) | (0.081) |
| After subsidy | 0.028 | 0.009 | −0.096 | −0.027 | −0.017 | −0.150 |
| | (0.091) | (0.075) | (0.129) | (0.100) | (0.117) | (0.078) |
| Interaction with: | Young firm | Previously hired | Business practices | Above median capital | Low household wealth | Manufacturing sector |
| Before subsidy × interaction | 0.223 | 0.040 | 0.007 | 0.012 | 0.059 | 0.201 |
| | (0.157) | (0.255) | (0.014) | (0.165) | (0.162) | (0.190) |
| During subsidy × interaction | 0.197 | 0.183 | 0.032 | 0.112 | 0.063 | 0.293 |
| | (0.150) | (0.228) | (0.014) | (0.152) | (0.150) | (0.167) |
| After subsidy × interaction | 0.024 | 0.168 | 0.016 | 0.150 | 0.094 | 0.538 |
| | (0.145) | (0.196) | (0.014) | (0.142) | (0.143) | (0.157) |
| Sample size | 4,879 | 4,879 | 4,879 | 4,879 | 4,879 | 4,879 |

*Notes:* Each column shows the treatment impact and treatment impact interacted with a specified covariate for the outcome of the number of paid workers in the firm. Young firms are less than five years old at baseline; previously hired denotes the firm having hired a worker at some point prior to the experiment; business practices is an index of different business practices employed at baseline; above median capital denotes the firm has above the median capital stock at baseline; low household wealth is below the median on a principal component of baseline household assets; manufacturing denotes that the firm is in the manufacturing sector. All regressions control for the baseline value of the interacting variable, interaction between this variable and each survey round, as well as for randomization strata, variables used for re-randomization, and survey round fixed effects. Robust standard errors are in parentheses, clustered at the firm level.

Column 3 of Table 8 examines heterogeneity with respect to baseline business practices. Consistent with the higher take-up seen in Table 3, we see that firm owners with better management practices were more likely to hire workers in response to the subsidy during the intervention. A 1 standard deviation improvement in baseline practices (5.9) is associated with a 0.19 additional worker increase during the intervention, which is significant at the 5 percent level. However, like the aggregate effect, this effect dissipates over time, and there is no long-term effect. Similarly, online Appendix Table 3.4 shows that firms receiving business training along with the wage subsidy increased workers more than those receiving the wage subsidy alone and that the impact persisted longer, but the difference was insignificant by our last survey rounds. Our wage subsidy required firms to hire a full-time worker, so the margin for adjusting labor was lumpy. Recalling that around 90 percent of enterprises hire zero paid workers at baseline, we might expect that better managed firms should be closer to the threshold of hiring a first worker so may have responded more. An alternative explanation is that firm owners with higher management ability could better understand the short-term gains possible through the subsidy.

Columns 4 and 5 of Table 8 examine heterogeneity with respect to wealth. If firms need capital to make new workers productive, then the theory predicts that treatment effects should be larger for firms that have more access to capital. We see no significant heterogeneity with respect to either baseline capital stock or household wealth at baseline. The signs of the coefficients are also not consistent across the two measures with column 4 suggesting larger impacts for those with more capital, versus column 5 suggesting larger impacts for poorer firm owners. Online Appendix Table 3.4 does show that the impact of the wage subsidy lasts longer for firms that also received the savings treatment, but this impact is also insignificant in the last survey rounds.

Finally, column 6 of Table 8 examines whether the impacts differ for firms in the manufacturing sector, 34 percent of our sample. This analysis by sector was not something the experiment was ex ante designed to study, but this heterogeneity analysis is motivated by three factors: the higher take-up rate of the subsidy for manufacturing seen in Table 3; the finding of Hardy and McCasland (2015) that a wage subsidy had an impact in their setting, which focused on manufacturing firms; and the helpful suggestion of a referee. We find evidence that the wage subsidy had a larger impact on manufacturing firms than those in retail or services. This is particularly true after the intervention, where the average treatment effect is 0.5 workers more for manufacturing than the other sectors ($p = 0.001$).[18] In online Appendix 8, we also examine the heterogeneity with manufacturing in the impact of the wage subsidy on other key firm outcomes. The point estimates suggest that wage subsidy had greater post-intervention impact on profits in manufacturing than in other sectors, though the effect is not significant.

## B. *Survey Evidence*

The survey data provide additional information to help interpret the results in Tables 5 through 8. We use these data to understand both why there was no lasting effect of the subsidy in the full sample and why firms in the manufacturing sector appear to have responded more robustly to the subsidies. We begin by looking at responses as they relate to key aspects of the heterogeneity examined in Table 8:

- *Learning about type and managerial ability*: Among those with a worker hired under the program who left, we asked which among several reasons the worker left. One option was that the owner had come to realize he was not able to properly manage the worker. Across several rounds and more than 30 such cases, this reason was only ever given once. In October 2009, we asked owners who were eligible for the subsidy but had not hired a worker whether any of 12 reasons for not hiring were important in their case. Only 13.3 percent of the owners said concern about their ability to manage an employee was a reason for not hiring. The far more common responses for not hiring related to a lack of demand for labor, including that the additional employee would not be profitable (43.4 percent), that the enterprise does not require an additional employee (26.5 percent), and that the subsidy is not large enough (13.3 percent). Together, these data suggest that learning about managerial ability is not a central issue among these firms.

[18] This therefore remains significant ($p = 0.006$) even when applying a conservative Bonferroni correction of multiple testing across the six dimensions of heterogeneity examined in this table.

- *Importance of access to capital*: The evidence on the interaction between labor and capital constraints is somewhat mixed. First, we have noted that the hiring response was stronger and somewhat more durable when the wage subsidy was combined with a savings intervention. Second, survey data show that manufacturers require less capital for new workers to be profitable, which may contribute to the more robust and enduring response by manufacturers. Manufacturers report that they could make an additional employee fully productive with 41,000 LKR in additional capital, compared to more than 100,000 LKR required by firms in other sectors. Moreover, without any additional capital investment, manufacturers say an additional employee would increase profits by 11,000 LKR, compared to 8,000 LKR in other sectors. Both of these differences are significant at the 1 percent level.[19] However, two-thirds of firms eligible for the wage subsidy (75 percent of manufacturers and 60 percent of those in other sectors) said they could make an additional employee fully productive with an investment of a month's profit or less. Though suggesting that a lack of capital may play a role in some cases, these data suggest that capital constraints are not likely a major reason for failing to take up for the majority of firms.

The survey data also yield some information on several other constraints suggested by theory:

- *Newly hired employees have lower productivity*: The subsidy may induce firms to hire when they are otherwise unable to pay a wage that reflects the initial low marginal productivity of workers. In April 2010, we asked owners how long they thought it would take a hired worker to become fully productive. The median response was 1 month and the mean 4.1 months; 86 percent said the period would be 6 months or shorter, suggesting that the subsidy was long enough to fully cover the learning period for the majority of the sample. In October 2011, we asked owners how long it took for the most recent worker they had hired (if there was one) to be profitable for the enterprise. Half said the employees were profitable right away, and another 38 percent were profitable within the first month. Hence, the survey data do not suggest that the time taken to integrate an employee is long enough to explain the patterns we see in the data.
- *The relevance of search costs:* The October 2009 survey, referenced earlier, also asked the subsidy-group owners about the role of search costs in the decision not to hire workers. Overall, only 12 percent of the wage subsidy group said that a reason for hiring was that they had not found the right employee, though this response was twice as likely among manufacturers (19 percent, compared to 10 percent in other sectors). These data suggest that search costs

[19] These comparisons use responses of all firms eligible for the wage subsidy, including those receiving other treatments. The comparisons using the voucher-only sample are qualitatively similar.

may be more important among manufacturing firms.[20] That search costs are important for only a small percentage of owners of small firms is consistent with the fact that small enterprise owners report that most workers they hire do relatively routine, physical tasks.

- *Recurring search costs?* If the search/training costs being subsidized are incurred every time a worker is hired, then owners may not find it profitable to rehire once the original worker leaves. Indeed, we find that only about one-third of owners rehire a worker after the subsidized hire separates from the enterprise. Since this excess separation occurs after the subsidy period, the failure of the majority of owners to rehire is consistent either with the marginal product of labor lying between the subsidized wage rate and the market wage rate or with recurring hiring costs. However, the timing of the largest spike in separations occurs in the six months immediately following the end of the subsidy is arguably most consistent with the marginal product of the worker falling between the subsidized and unsubsidized wage. We also find that the strongest correlate with retaining a worker after the subsidy period is the wage paid to the worker during the subsidy period. Among workers paid 2,000 LKR per week or less,[21] almost two-thirds (63 percent) no longer worked at the enterprise 6 months after the subsidy ended. In contrast, among workers paid more than 2,000 LKR per week, 67 percent were still employed by the enterprise 6 months after the subsidy ended.[22] Consistent with the higher retention rates among manufacturers, workers hired by manufacturers were paid around 600 LKR more per week than firms in other sectors. Moreover, owners report that among the separated low-wage workers, the vast majority (78 percent) were either unemployed or working as casual workers, suggesting the majority of these workers were not lured away by better offers, but instead had marginal products less than the market wage. In contrast, among the separated high-wage workers, the majority (six out of eight) were either in other wage work, self-employed, or working overseas. This suggests that the higher wage workers hired by around 10 percent of the eligible firms had marginal products that exceeded the market wage rate. In contrast, the majority of workers hired through the subsidy program appear to have marginal products lower than unsubsidized market wages.

In sum, we find little evidence that the subsidy works by leading owners to learn either their managerial ability or the marginal product of labor. Survey data also suggest that search costs are modest, in part because most hired workers perform physical, low-skill tasks. However, there is some evidence that search costs are more

[20] This conclusion is supported by data from a survey of owners of small- and medium-sized businesses conducted around the same time. In April 2009, among 160 owners with 5 or fewer employees, the median owner said it would take 7 days to locate an employee if s/he wanted to hire one; the mean search period was 14 days. The SME survey was conducted in urban areas throughout Sri Lanka, but a majority of respondents come from the same urban areas in which we conducted the experiment. The survey is described in online Appendix 4.

[21] The 2,000 per week rate is around the sixtieth percentile among workers hired through the subsidy program, but is approximately the twenty-fifth percentile of wages of workers employed in control-group enterprises at the start of the subsidy period (October 2009).

[22] The difference in separation rates between lower and higher paid workers is significant at the 0.04 level.

significant and capital constraints less so among manufacturers. Finally, the timing of the excess separations, coming immediately after the end of the subsidies, is consistent with the marginal product of labor falling between the subsidized and market wage rates.

### C. *Why Was There a Survival Impact?*

The only long-term impact of the wage subsidies on the average firm appears to be on firm survival, with no impact on firm sales or profitability. Moreover, panel E of Table 4 and panel I of Table 6 show that this increase in firm survival is not accompanied by higher chances of the individual being employed, nor in higher take-home earnings from all work. This suggests there is no long-run income or employment benefit to the owners of keeping their firms alive.

This suggests that the survival impact is not coming through relieving labor market constraints on firms. Instead, the most likely explanation for the survival effect appears to be that the subsidy provided firms with extra profits during the intervention period, and this small amount of additional capital allowed firms to survive shocks that would otherwise shut them down. De Mel, McKenzie, and Woodruff (2012) show one-time grants of 10,000 and 20,000 SLK helped small firms survive. If workers earn their marginal product, then the maximum subsidy for a firm was 28,000 SLK. These firms are larger on average than those in de Mel, McKenzie, and Woodruff (2012), so this amount is equivalent to less than two months' baseline profits. If it were to have an effect, it should therefore be for the smallest firms.[23] Online Appendix 9 provides suggestive evidence that the survival effect is larger for firms with below median baseline capital. However, even for the owners of these lower capital firms, increased firm survival is not accompanied by higher odds of employment or greater work income. Note that the additional survival amounts to only 12 firms (5 percent of 250), so sample size limits what else we can say about this group.

## VI. Discussion and Conclusions

Microenterprises in developing countries face many potential constraints to growth, and understanding which of these constraints are binding is crucial for designing policies to address them. Moreover, as firms with fewer than five employees are the source of livelihoods for the majority of urban workers in most low- and middle-income countries, binding constraints to their growth may have important aggregate effects on economies. This is particularly true with regard to employment growth. Compared with a larger literature on capital and entrepreneurship training, there is a paucity of research on the functioning of the informal labor markets most

[23] Our savings intervention also provides an additional test of the role of additional capital on firm survival. The size of the grant received by the median firm in this treatment was 5,000 LKR, which is similar to the average amount received by those offered the wage subsidy of 5,116 LKR. The key difference is that the wage subsidy treatment had much lower take-up, but those who did take it up received a median of 24,000 LKR. Online Appendix 3 shows that the savings treatment resulted in a positive, but statistically insignificant impact on firm survival and that we cannot reject that the savings treatment effect equals the wage subsidy treatment impact for survival.

relevant to these firms. We use a shock to wages, implemented through temporary subsidies offered when enterprises hire new workers, to measure whether a lack of labor is a key constraint to small firm growth.

The effects of the subsidy on the average firm are more consistent with well-functioning neoclassical labor markets than they are with learning, search, and training frictions leading firms to suboptimally underinvest in workers. Firms respond to the subsidy by hiring additional workers, but the excess hiring dissipates completely within a year or so of the removal of the subsidies. Complementary experiments loosening credit constraints and providing entrepreneurship training do not change the effects of the temporary subsidies. We conclude that hiring labor is not a profitable investment that the average firm should be undertaking. This does not mean that there are not some firms that face frictions, just that they are a minority. Indeed, we find some suggestive evidence that firms in the manufacturing sector may face more frictions and are likely to keep workers hired with a wage subsidy for longer. These results suggest that more research focusing on manufacturing firms may be merited.

An obvious question is how much the results from this experiment generalize to the broader development context. We suspect that many characteristics of this labor market hold in most urban areas in developing countries. Workers remain unregistered and hiring is generally unregulated. The work performed by employees in small enterprises involves relatively more brawn and less creative energy. Employees are hired from local areas, and there is usually a low degree of separation between employer and employee before hiring. There are other aspects of the context which are likely less usual. Our experiment coincided with the end of a long civil war in Sri Lanka and a period of rapid growth. Compared with other low- and middle-income countries, our samples of both microentrepreneurs and wage workers show relatively high levels of generalized trust.[24] How much these more distinctive features of the context drive the results is difficult to say. We would hope this might be resolved by future work examining labor market frictions in different contexts.

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[24] In April 2009 surveys, 32 percent of wage workers and 27 percent of entrepreneurs said that "most people can be trusted." Though this may seem low, it compares with rates in the general population in most sub-Saharan African and Latin American countries, where less than 20 percent of the population responds similarly. (Calculations by authors using WVS data downloaded from http://www.worldvaluessurvey.org.)

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