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The Demand for, and Consequences of, Formalization among Informal Firms in Sri Lanka†

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A field experiment in Sri Lanka provides informal firms incentives to formalize. Information about the registration process and reimbursement of direct costs does not increase registration. Payments equivalent to one-half to one month (alternatively, two months) of the median firm's profits leads to registration of around one-fifth (alternatively, one-half) of firms. Land ownership issues are the most common reason for not registering. Follow-up surveys 15 to 31 months later show higher mean profits, but largely in a few firms that grew rapidly. We find little evidence for other changes in behavior, but formalized firms express more trust in the state. (*JEL* C93, D22, L25, L26, O14)

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The majority of firms in most developing countries are informal. In Sri Lanka, for example, only one-fifth of firms operating without paid workers are registered with any government agency. Even among firms employing paid workers, the majority are unregistered with one or more pertinent agencies. Policymakers are concerned about high levels of informality for two reasons. First, informality is associated with lower tax collection, restricting the government's ability to finance public services (Levy 2008). Second, the coexistence of formal and informal firms means that firms competing in the same industry face different marginal production costs (e.g., labor costs and taxes), leading to an inefficient allocation of resources in the economy (Hsieh and Klenow 2009; Levy 2008). How can informal firms be induced to formalize?

A firm's decision to become formal depends on the costs and benefits of formality. There are two prevailing views of informality, dubbed by Perry et al. (2007) as *exclusion* and *exit*. The exclusion view focuses on the costs of registering. This view is most notably associated with the work of de Soto (1989), who argued that

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burdensome entry regulations prevent small firms from becoming formal. These firms suffer a loss in productivity as a result of remaining informal. The natural policy response is then to remove the burdensome regulations. Spurred in part by the World Bank/IFC *Doing Business* project, governments around the world have in recent years streamlined the process of becoming formal. Indeed, since 2004, 75 percent of the countries included in the *Doing Business* survey have adopted at least one reform making it easier to register a business (IFC 2009).

But is streamlining the registration process sufficient to spur formality? An alternative view, associated with Levy (2008), Maloney (2004) and others, posits that firms rationally exit the formal sector when the benefits provided by formality are outweighed by the costs of being formal. That is, the formality decision is comparable to any other investment decision taken by the firm. Each firm compares its perceived costs of being formal—including initial registration and ongoing costs (e.g., tax payments)—with its perceived benefits of being formal (e.g., a reduced chance of being fined, and access to banks, courts and government contracts). More able firm owners with larger efficient scales rationally become formal as they grow large enough to benefit from the formal institutions of civil society. Smaller, less productive, firms do not find formality desirable because they receive no benefit from access to these formal institutions.[1]

We conduct a field experiment in Sri Lanka (de Mel, McKenzie, and Woodruff 2012) with the goal of generating data to test whether the exclusion or the exit view of informality better reflects reality. Working with a sample of informal firms with 1 to 14 paid employees, we reduce the information costs and increase the monetary benefits of formalizing for a random subset of the sample. The experiment allows us to measure both the latent demand for and the consequences of formalization. We divided the sample into four treatment groups and a control group. The first treatment group was given information about the costs and benefits of, and procedures for, registering their firm with the Divisional Secretariat (DS)—the relevant registration for tax purposes. Additionally, they were reimbursed for the (modest) direct cost of registration if they registered. The second, third, and fourth treatment groups were provided the same information and also offered a payment of 10,000 Sri Lankan rupees (Rs), Rs 20,000, and Rs 40,000 (approximately US $88, $175, and $350, respectively) to register.

We find that providing information and reimbursing the cost of registration did not induce firms to register. In contrast, 17–22 percent of eligible firms registered when offered Rs 10,000 or Rs 20,000, just under half a month's and one month's profits for the median firm, respectively, and 48 percent register when offered Rs 40,000. Few of those who did not register when offered Rs 40,000 registered when given additional time or when offered Rs 80,000.

The experiment sheds light not only on the demand for informality, but also on the nature of barriers to formal registration. An important share of the firms not registering

[1] Levy (2008) discusses the cost-benefit calculus in the context of worker benefits. Firms choose whether to hire formal or informal workers, and may hire some of each. In our setting, owners choose whether or not to register the firm rather than choosing the formality of individual workers. However, the thrust of Levy's argument applies to our setting, even if some of the details—formality is not assumed to affect the productivity of a given worker in Levy's context, while formality may affect the productivity of the firm in ours—differ.

after receiving the largest incentive report that issues related to land tenancy prevented them from doing so. These firms operated with informal leases or agreements—ironically, often on government-owned land—and hence were unable to provide authorities with the required proof of ownership of the land on which the firm operated.

Three follow-up surveys of these same firms were conducted at 15, 22, and 31 months after the intervention, enabling us to examine whether and how the firms benefited from formalization. Firms that formalized are found to have higher profits, although this impact seems largely due to the experiences of a few firms experiencing substantial growth, with the distribution of profits almost identical for treatment and control firms over most of the distribution. Examining the channels through which formalization might benefit firms, we find increased advertising and use of receipt books, but no increases in receipt of government contracts, use of bank accounts or loans, or participation in government programs. Consistent with the effect of formal land titling reported in Di Tella, Galiani, and Schargrodsky (2007), we also find impacts in terms of attitudes. Firms that formalize are more likely to trust local government and agree that paying taxes is a civic duty, but are also more likely to agree that small businesses are taxed too much.

The results provide more support for the exit view than for the exclusion view of informality. The fact that firms remain informal even when the direct costs of registering are reimbursed suggests these firms are not being excluded. Using the reported profit data and the Sri Lankan tax code, we estimate the demand for formality, relating the percentage of firms choosing to register to the present value of tax payments net of the benefit from our one-time payment. We find a steeply downward sloping demand curve, providing support for the view that firms are making rational benefit-cost calculations as Levy and others claim.

This study is the first to experimentally induce informal firms to register for tax purposes.[2] It builds on an existing literature that has largely focused on the impact of entry regulations at both the macro and micro levels and on new firm creation, rather than formalization of existing informal firms. At a cross-country level, countries with more burdensome entry regulations have larger informal sectors (Djankov 2002), and costly entry regulations are also associated with the creation of fewer limited liability companies (Klapper, Laeven, and Rajan 2006).[3] But the endogeneity of regulatory choices complicates any claim of causation based on the cross country patterns. Several micro studies have therefore examined the impact of regulatory reforms. Bruhn (2011) and Kaplan, Piedra, and Seira (2011) study a reform in Mexico which reduced the time required to register at the municipal level from 30 to 2 days for firms operating in specified sectors. Both find some increase in the number of formal firms, although Bruhn concludes this increase is largely coming from new entry rather than formalization of existing firms, while Kaplan, Piedra, and Seira (2011) use different

[2] An unpublished study that took place over a similar time frame to our experiment by Alcázar, Adrade, and Jaramillo (2010) in Lima, Peru also tries to formalize firms through subsidizing the cost of registration, although they focus on municipal-level registration. While their study has very high attrition levels (over half their sample attrits), the preliminary results we have seen suggest the results of our study may generalize to other contexts. We discuss this study in more detail in Section V.

[3] There is also firm-level evidence for an impact of entry regulations. Bertrand and Kramarz (2002) find that entry regulations are associated with significantly slower employment growth among French retail firms.

data to reach the opposite conclusion. Fajnzylber, Maloney, and Montes-Rojas (2011) analyze a simplification program in Brazil and find that the firms that open just after a reform are larger and more likely to operate in a permanent location—a finding which they interpret as evidence that formalization improves firm performance, but could also be the result of selection into firm entry. Monteiro and Assunção (2012) examine the same reform and find that new retail firms created after the reform were more likely to be formal than firms in sectors not affected by the reform. Finally, in one of the few published studies to examine the impact of formalizing on existing informal firms, McKenzie and Sakho (2010) use distance to the registration office as an instrument for registration costs, and find that some firms in Bolivia facing high costs of formalizing would gain on net from registering for taxes, but that other firms would lose from doing so, and so appear to be rationally informal.

The remainder of the paper is set out as follows. Section I describes the process of becoming formal as a small firm in Sri Lanka, and gives evidence as to the extent of formality by firm size. Section II describes our intervention. Section III describes the results on the demand for formalizing, and Section IV describes the consequences of formalizing. Section V discusses the extent to which these results may generalize, and Section VI concludes.

I. Becoming Formal in Sri Lanka

The process of registering a business in Sri Lanka is similar to that in many other developing countries, with multiple levels of registration. Two levels are required of all firms, regardless of size. First, firms are required to obtain a municipal license. Depending on whether the firm is located in a rural, urban, or semi-urban area, this implies registration with the Pradeshiya Saba, Municipal Council or Urban Council (P.S. or UCMC). In some sectors, obtaining this license requires a site visit from a revenue officer and/or a public health inspector, or approval by a police officer and the municipal chairman. Firms must also pay an annual license fee that depends on the sector, but typically ranges from Rs 500 to Rs 5,000. The main benefit of registration at this level is being able to operate without fear of being harassed by local officials, who typically monitor the most visible enterprises since these license fees are an important local revenue source.

Second, all firms must register at the division level with the Divisional Secretariat.[4] The one-time registration with the DS establishes the business as a legal entity for tax purposes and provides a Business Registration Certificate (BRC). The BRC serves as legal proof of the enterprise's existence and is needed, for instance, for the firm to be able to sell to government institutions and to larger firms that require formal transaction receipts. The BRC is also needed to open a bank account in the name of the business, and to obtain a loan from most commercial banks. Registration at the DS Division level involves payment of a modest fee, but does not, by itself, imply the

[4]There are four administrative levels in Sri Lanka: Provinces (9), Districts (25), Divisions (324), and Grama Niladaris (GNs, 14,008). Political councils are elected at both the Provincial and local levels. The local councils are called by different names depending on the area they cover, with Municipal Councils in the 18 largest urban areas, Urban Councils in a further 42 urban/semi-urban areas, and Pradeshiya Sabhas in rural areas. Rural Sri Lanka is divided into 270 Pradeshiya Sabhas. Registration of enterprises thus takes place both at the local and divisional level.

firm is liable for taxes. Taxes need to be paid only if annual net profits are in excess of Rs 300,000, and theoretically, are payable regardless of the registration status of the firm. In practice, registration makes the firm more visible to tax authorities, and hence increases the expected tax payments for firms with incomes exceeding this threshold.

Online Appendix 1 provides details on the actual process experienced by firms registering with the DS during our experiment. The process was easier in Colombo, where firm owners were generally not asked to provide any documents other than their national identity card, and could choose to pay an extra Rs 500 express service fee to get their BRC in one day. For the firms in our study the registration process typically involved a total of two days and two visits to the DS. In contrast, firms in Kandy were required to also provide proof of business existence, proof of municipal registration, and a letter of no objection from the property owner. The median firm spent three days getting these other documents processed, and waited six additional days after submitting all documents to receive their BRC.

Larger firms are subject to two additional registrations. Firms with paid workers are required to register these workers with the Ministry of Labour for the Government Social Security Schemes: the Employee's Provident Fund (EPF) and the Employee's Trust Fund (ETF). EPF consists of a monthly payment of 20 percent of the employee's earnings (consisting of a 12 percent employer contribution and an 8 percent employee contribution), and ETF a further 3 percent employer contribution. Formal employers with more than 14 workers also face high severance pay costs if they lay workers off. Finally, firms with revenues exceeding Rs 500,000 per quarter or Rs 1.8 million per year must also register for VAT. The VAT tax rate is 20 percent of value-added for most goods, with producers of some goods paying a lower 10 percent rate and others exempt entirely.

A. *Formality Levels in Practice*

Figure 1 summarizes the percentage of firms that reported being registered with each of the four government entities according to the number of paid employees in the firm. The data come from the baseline of the Sri Lanka Longitudinal Survey of Enterprises (SLLSE), collected by the authors between January and March 2008. The survey contains 2,865 enterprises, and is representative of enterprises in the 31 largest cities and towns (outside the Northern province, which was inaccessible due to civil conflict). A door-to-door listing exercise of households was carried out, to ensure the survey was able to detect both formal and informal firms. The vast majority of enterprises have zero paid workers, and we see that only 23 percent of these nonemployers are registered at the local (P.S./UCMC) level, and only 20 percent are registered at the DS Division level. Less than 1 percent of the nonemployers report being registered for VAT. Registration at all four levels rises quickly with firm size, so that 75 percent of those with 5 paid employees are registered with the P.S. or UCMC and 68 percent with the DS. However, the percent registered then appears to plateau, with approximately 70 to 80 percent registered with the DS for firms with 6 to 20 employees. The SLLSE sample becomes thinner as the number of paid workers grows, making the point estimates more variable at larger firm sizes. Registration of at least some of the workers of the firm with EPF/ETF is less common in firms with

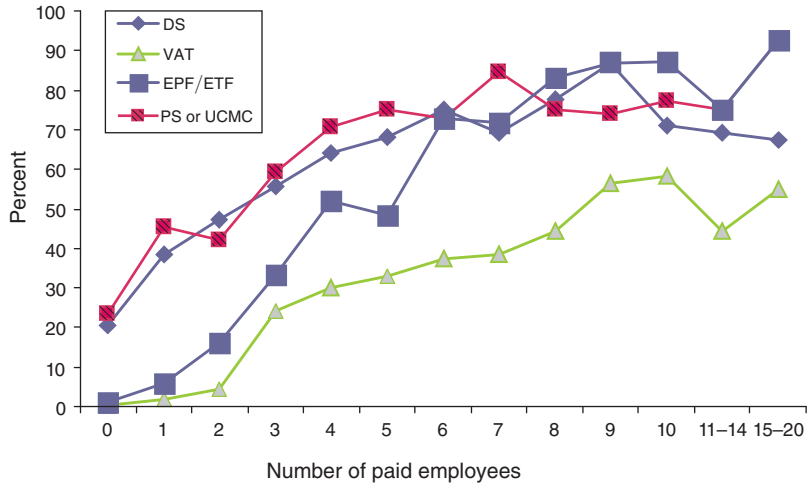

FIGURE 1. PERCENTAGE OF FIRMS REGISTERED WITH DIFFERENT GOVERNMENT ENTITIES BY FIRM SIZE

fewer than five workers, but also increases rapidly with firm size, so that 87 percent of firms with 10 employees have registered at least some of their workers. VAT registration is the least common, but also grows with firm size, so that about half of firms with 10 or more workers are registered for VAT.

Among those firms with 1–10 paid workers who were not registered with any of the four government entities, only 23 percent said they had ever considered registering their business. The majority of the remainder said they felt their businesses were too small to have to register. There is often a presumption that the informal sector faces a high level of de facto fees in terms of fines, penalties, and bribes which arise from operating without a license. However, this does not appear to be the case in Sri Lanka, where fewer than 0.5 percent of unregistered firms with 1–10 paid workers reported paying a fine, penalty, or bribe in the last year because of their unregistered status. Indeed, most informal firms report having little interaction with officials at all. Only 5 percent reported receiving a visit from a Pradeshiya Saba official, 2 percent from a DS official, and 1 percent from a tax official in the past year.

B. *Focusing on DS Registration*

Figure 1 shows that registration at all levels increases markedly with the number of employees. While most firms with one or two workers are entirely unregistered, a majority of firms with ten or more workers are registered with each of the relevant agencies. Our goal is to understand the demand for formality among firms in this size range. We focus on one particular dimension of formalizing, registering with the DS. This DS registration most closely corresponds to the concept of formalizing underlying much of the discussion of formalization in the literature, because it establishes a legal and tax presence, and is the prerequisite for selling to the government and other firms that require registration, as well as for applying for most bank loans.

II. The Experiment

A. *Selection of the Sample*

We chose to carry out our study in the two largest cities in Sri Lanka: Colombo and Kandy. Since the goal of our intervention is to gauge the demand for formality among informal firms, we needed to construct a sample of firms not registered with the DS at the time of the baseline survey. There was no existing sample frame that could be used for this, so we carried out our own screening exercise.[5] We randomly selected five Divisional Secretariat Divisions in each city. In December 2008, we had research assistants go door-to-door in these areas to screen firms, with the goal of surveying approximately 50 unregistered firms in each selected DS. Firms were selected for the baseline survey if they were not registered with the DS, were not in seasonal agriculture or fisheries, had 1–14 paid employees, and had an owner aged 20 to 55 who worked at least 20 hours in the enterprise each week. Few firms of this size are owned by women, and so we chose to restrict the sample to male owners and to businesses jointly owned by a husband and wife. A sixth DS Division was added in Kandy due to difficulty finding enough informal firms that satisfied these criteria. There are lots of informal firms without paid workers, but fewer with paid workers.

An obvious concern in attempting to construct a sample of the unregistered is whether firms will reveal that they are not registered (and hence that they are breaking the law). We used university students to carry out the surveying, and they presented firm owners with a letter from the University of Peradeniya investigator emphasizing that this was a research study. In an environment where informality is commonplace among smaller firms, we believe that most unregistered firms did tell the truth. Indeed, as we will discuss in more detail, our sample contains a nontrivial fraction of firms that were "quasi-registered," but answered that they were not registered, suggesting that there were not strong incentives for firms to pretend to be registered.

The resulting baseline sample consists of 520 firms evenly split between Colombo and Kandy. Although our screening criteria allowed firms to have up to 14 paid employees, in the population there are more firms with 1–5 employees than with 6–14, and a larger percentage of the smaller firms are informal. As a result, 90 percent of firms in the baseline sample have five or fewer paid employees, with a median of three paid employees. Although all of the firms said they were not registered with the DS, 68 percent reported being registered with the municipal council or Pradeshiya Saba, so they are only partially informal. However, only 5 percent have registered any of their workers with EPF/ETF. The firms cover a range of industries, with 44 percent in services (e.g., motor vehicle repair, restaurants), 32 percent in manufacturing (e.g., manufacturing fabricated metal products and glass products), and 23 percent in retail. Mean (median) monthly profit for the firms in our sample was Rs 33,886 (Rs 25,000), approximately US $300 ($220) at market exchange rates at the time of the survey, with a 10–90 percentile range of Rs 9,000–75,000

[5] We decided against using the sample from our previous survey, the SLLSE, since logistically it was infeasible for us to implement the experiment island-wide, and there were too few firms in the SLLSE that were in Kandy and Colombo, and were not part of a separate experiment.

(US $79–$658). The median firm had been in business for six years, 80 percent of the firms were more than three years old. Most of the businesses were stand-alone enterprises, with only 8 percent located inside the home.

B. *What Do Firms See as the Costs and Benefits of Formalizing?*

Firm owners were asked open-ended questions about the possible costs and benefits of registering with the DS. The most common perceived cost or disadvantage was having to pay taxes and being more likely to receive visits from tax authorities, mentioned by 46 percent of the owners. The next most common concern, mentioned by 37 percent of the owners, was that the process of registration was burdensome and time consuming. A third common concern was that DS registration would require the firm to pay EPF/ETF, or subject the firm to a greater risk of visit from labor inspectors, which was mentioned by 24 percent of owners.[6] Only 13 percent believed the initial cost of registration was high.

With regard to the benefits of registration, more than half of the owners (58 percent) mentioned being able to get a bank account in the business name or apply for a bank loan. The second most common response to this question (23 percent of owners) was that there is no particular advantage of registering with the DS. Approximately 10 to 15 percent of firms mentioned a number of other advantages, such as being able to operate on a larger and more visible scale, qualifying for participation in government programs, being able to sell to the government and firms that require registration, and having a lower risk of being fined. As with the nationwide sample, less than 1 percent reported actually having had to pay any fines or bribes in the last year for operating informally. So the risk of being fined is already low.

These responses suggest that firms have at least somewhat accurate information about some of the advantages and disadvantages of registering. They have much less knowledge of the specifics of how to register. Only 17 percent knew the correct cost of registering at the DS. The most common response when asked how long it takes to register—given by 22 percent of the owners—was "don't know." The next most common responses were 30 days, 60 days, and 90 days. Firms also lacked knowledge of their income tax responsibilities. Firm owners were asked how much a business owner would have to pay as income tax if their annual income was Rs 100,000. Only 2 percent got the correct answer of 0, with 55 percent saying "don't know." The median answer among those professing to know was Rs 2,000. When asked the same question with respect to an annual income of Rs 400,000, again 50 percent said they didn't know, and 88 percent of those answering with a number gave an answer higher than the true rate of Rs 3,400, with the median answer Rs 8,000.

Finally, firm owners were asked in the baseline a hypothetical question of whether they would register with the DS if someone would pay the fixed cost of registration. Sixty-one percent of firm owners answered yes to this question, suggesting some willingness to formalize if the costs were reduced.

[6] We note that this perception is not accurate—the ETF/EPF registration is separate, and there is no formal communication between the two agencies.

C. *The Intervention*

The baseline survey and hypothetical questions suggest that the informal firms are not perfectly informed about the process of registration or its costs and benefits, and that many express a willingness to register if someone pays the upfront costs. We therefore designed an intervention that provided information about the costs, benefits, and process of registration, and promised to reimburse firms for the direct costs of registering. If the stylized de Soto/*Doing Business* view is true, and firms want to formalize but for the costs, then we should expect to see a large response from this intervention. Likewise, if firms really want to formalize but time-inconsistency means they have never gotten around to it, a limited time offer to reimburse registration costs and a nudge from an outsider might be expected to spur registration. In contrast, if firms have decided not to formalize because they don't think the benefits outweigh the costs, we would expect few firms to be at the margin where just the upfront registration cost alone was enough to change this calculus. Such firms would need additional incentive to register, and so we experiment by seeing how much we need to pay firms to get them to find it worthwhile to formalize.

We therefore designed the following four treatments:

Treatment 1.—Information and Reimbursement. We designed an information brochure in consultation with the chamber of commerce and local Divisional Secretariat which clearly set out the advantages and disadvantages of registering with the DS, and explained the steps required to register. This information brochure was given to firm owners. Trained research assistants were available to answer any questions the firm owner had about how to register. Firm owners were also told that we would pay a reimbursement of Rs 1,000, slightly more than the average level of fees paid to register, if they registered within one month and mailed us a copy of their business registration certificate. This offer was presented in person and written on official project letterhead to increase credibility.

The information brochure was entitled "Could Business Registration Lead to the Success of Your Enterprise?" Issues covered included: what is meant by business registration, the reasons a business should be registered, the reasons some owners do not register, and myths about registration. The brochure also gave details of where to register, and the cost, time, and documents required for registration.

Treatments 2, 3, and 4.—Information and Payments. The other three treatment groups also received the information brochure, but instead of reimbursement owners were offered a larger monetary payment if they registered within one month of the offer. The amounts offered were Rs 10,000, Rs 20,000, and Rs 40,000, respectively (approximately US $88, US $175, and US $350 at the time of treatment). This offer was delivered on a certificate that specified the name of the owner offered the treatment and the last date the registration could be submitted to us to qualify for payment. The certificate came with the signature of Dr. de Mel, and a project seal to increase credibility. We conducted an open house in both Colombo and Kandy, where firm owners could come to ask any questions. The open house also served to reassure the owners that the offer of payment was credible. To receive their payment,

firm owners had to return to the same location during a window of time and present their new business registration certificate along with their national identity card. To guard against false registrations, we required the nature of the business and the address on the BRC to be the same as on the baseline survey. Where the two differed for a legitimate reason, the owner had to inform us in advance. In that case, they received payment only after we had verified the new address.

The payments offered are quite sizable relative to the size of the firms and to the time required to complete the registration process. As noted above, the median profit for the firms in our sample was Rs 25,000, so treatments 2, 3, and 4 were approximately half a month's, one month's, and two months' profits, respectively. A firm earning Rs 25,000 per month faces no income tax, so the payments are also very large relative to the direct cost of formalizing (the roughly Rs 1,000 fee) for these firms. The ninetieth percentile of monthly profits in our data was Rs 75,000, which would require an annual income tax of Rs 33,000 (3.7 percent of income). So our larger treatment exceeds the annual income tax cost of formalizing if they were to report their entire income.[7]

One immediate question is whether firms could easily register to take the payment offered, and then deregister the business to continue operating informally. If this were the case, our experiment would be less informative about the incentives needed to get firms to formalize. However, in practice, this does not appear easy to do. Canceling of business registration can occur if the enterprise ceases to operate. The enterprise needs to notify the DS office within three months of closing the business, and provide certification from the local G.N. official to verify that the business is closed. Therefore, for a firm to accept our payment and then revert back to unregistered status, they would have to actually close their business, and have this verified, before reopening it again.

Our sample size of 520 was randomly assigned by computer into these four treatment groups and a control group as follows. Firms were first stratified by province (Colombo or Kandy), industry (retail, manufacturing, or services), whether or not they had more than two paid employees, and whether or not in the baseline survey they had said they would register if someone were to pay the costs, and had also said they perceived some benefit to registration. Then within each of these 24 strata we sorted firms according to their sales rank and formed matched quintuplets. Where the number of firms in a strata was not perfectly divisible by five, the additional firms were randomly assigned within strata to one of the five treatment groups with equal probability. This method of randomization was based on the recommendations of Bruhn and McKenzie (2009) with the aim of increasing baseline balance and power, given that we only have 102–105 firms in each treatment group. The stratification variables were chosen on the basis that local regulations make the process of registration slightly different in Colombo and Kandy, while the incentives to register were a priori believed to possibly differ by industry, firm size, and self-professed desire to register.

[7] de Mel, McKenzie, and Woodruff (2009) estimate that micro-enterprise owners underestimate reported profits by as much as 30 percent. If firms in our sample are underreporting profits by a similar amount, then the median firm would be liable for annual taxes of Rs 3,000, implying that our smallest payment would compensate for three years of tax payments. If firms underreport profits in the survey, they might also do so in tax reports.

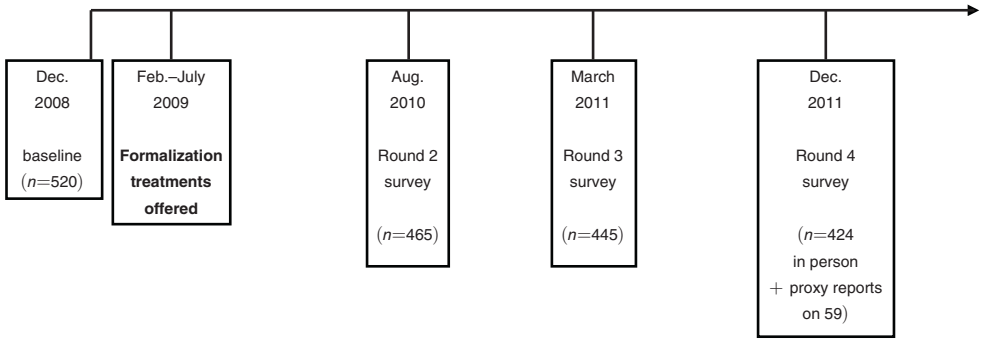

FIGURE 2. TIMELINE

The initial offers were given to treatment groups 1 and 2 in late February/early March 2009 (see timeline in Figure 2). Firms in these two treatment groups were given until the end of March/start of April to provide their business registration certificates and receive payment. Given budget constraints and the desire to map out as much of the demand curve for formality as possible, we waited to decide on the amount to offer treatment groups 3 and 4 until observing the take-up rates of these first groups. Treatment groups 3 and 4 were then given their offers in late April, with a deadline of late May to show their paperwork. However, on May 18 the Sri Lankan Government declared victory in the 25-year civil conflict, leading to a national holiday and making it difficult for paperwork to get processed toward the end of May. We therefore decided to give all four treatment groups an extra month to carry out their registration if they desired, with a new deadline of the end of July 2009. Any firm that had completed its registration between the previous deadline and the new deadline was also eligible to receive the payment.

D. *Quasi-Registration and Actually Receiving the Treatment Offer*

Owners of the firms assigned to treatment were asked to confirm their registration status on a follow-up visit in which we made the offer corresponding to their treatment group. We were surprised to learn that a nontrivial number of the firms were already registered with the DS. Recall that all of the firms stated they were not registered with the DS in the baseline survey. A handful of these firms had registered in the three to four months since the baseline survey (this was confirmed by examining the BRC, which shows the date of registration), but the majority of those registered had either misunderstood the question in the baseline survey or were what we term "quasi-registered." The latter refers to a number of cases where the business had been registered in some form, but the registration did not match the current circumstances of the business. For example, in some cases the business had been registered by another family member, who was no longer running it. In other cases the registration had been for another location. The firm thus had a BRC, but it didn't match the firm in all details. This BRC could still presumably be used to access government contracts or in dealing with a bank. We therefore chose not to offer the treatment to firms in this category. We subsequently also revisited the control group firms to determine which of them also fell into this quasi-registered status.

In total, 152 out of the 415 firms that were assigned to 1 of the 4 treatment groups did not receive the registration offer. The majority of the time (106 cases) this was because the firm was already registered or quasi-registered at the time of the baseline survey, as just described. In 14 cases, the business had closed since the baseline survey, in 18 cases, the owner could not be found in the follow-up, and in 14 cases the business had registered on its own between the baseline survey and the intervention. Five of the owners rejected the offer outright. We count these cases as having received the offer. Follow-up visits with the control group revealed 30 firms that were already registered, and a further 12 had closed or moved or couldn't be located.

Table 1 reports summary statistics for the full sample according to assignment to treatment, and also for the subsample that actually received the treatment offer (or was eligible to do so after rechecking in the control group). The randomization succeeded in achieving statistical balance for both the variables explicitly stratified or matched on, and for other key variables. The coefficients of variation are 1.18 for profits and 2.01 for sales, which are in line with or lower than those found in business training experiments around the world. A consequence of the heterogeneity in firm performance is that power to detect differences across treatment groups is lower than it would be if the sample were more homogeneous. We employ several methods to improve power, which we discuss below. With the possible exception of treatment group 1 (the information and reimbursement only group), there is also balance in the share of the group already registered or quasi-registered. As a result, we cannot reject balance for any of the variables in the subsample for which the offers were actually made.

III. The Demand for Formalization

A. *Which Treatments Increased Formalization?*

We estimate both the intention-to-treat effect (effect of being randomized into one of the four different treatment groups), and treatment effect on the treated (effect of being randomized into one of the four different treatment groups and actually being offered the treatment). The intention-to-treat effect is obtained by means of the following regression:

$$(1)\quad \textit{Registered In Intervention Window}_i = \alpha + \beta' \text{TREATMENT}_i + \gamma' \text{STRATA}_i + \varepsilon_i,$$

where TREATMENT_i is a vector of dummies for each of the four treatment groups, and STRATA_i is a vector consisting of dummies for each of the strata used in the randomization. To estimate the treatment effect on the treated, we replace TREATMENT with dummies for actually being offered the treatments, and then instrument these with assignment to treatment.

Table 2 shows the results. Only 1 of the 104 firms in the information and reimbursement group registered during our intervention window, compared to 16 in the Rs 10,000 treatment group, 13 in the Rs 20,000 treatment group, and 30 in the Rs 40,000 treatment group. The large majority of firms registered during the initial month given to them—only eight registered during the extra time provided by the extended July deadline. During the intervention time window, two firms in the

Table 1—Summary Statistics by Treatment Group

	Treatment group 1	Treatment group 2	Treatment group 3	Treatment group 4	Control	*F*-test of equality *p*-value
Panel A. Assignment to treatment						
Variables stratified or matched on						
Retail	0.23	0.22	0.24	0.23	0.24	0.998
Manufacturing	0.34	0.34	0.32	0.31	0.31	0.993
Services	0.43	0.44	0.44	0.46	0.45	0.996
Colombo	0.50	0.50	0.50	0.52	0.49	0.993
More than two paid workers	0.58	0.58	0.60	0.56	0.60	0.971
Says would register and sees benefit to doing so	0.49	0.48	0.48	0.49	0.48	0.999
Sales in last month	218,570	153,184	180,691	160,159	213,310	0.497
	(62,816)	(17,064)	(25,761)	(22,376)	(35,700)	
Median of sales last month	90,000	92,500	100,000	100,000	90,000	
Variables not stratified or matched on						
Doesn't keep records	0.49	0.54	0.55	0.55	0.47	0.649
Years of education of owner	11.07	10.68	10.89	11.43	10.54	0.082
	(0.29)	(0.30)	(0.20)	(0.22)	(0.26)	
Number of paid employees	2.93	3.13	3.10	3.00	3.15	0.905
	(0.16)	(0.23)	(0.19)	(0.22)	(0.20)	
Registered at the Pradeshiya Saba	0.71	0.66	0.71	0.67	0.62	0.572
Profits in last month	29,679	32,822	32,634	36,705	37,585	0.662
	(3,196)	(3,290)	(2,485)	(5,082)	(5,167)	
Given treatment offer (or eligible for it if control)	0.75	0.63	0.63	0.58	0.61	0.070
Sample size	104	104	105	102	105	
Panel B. Given treatment offer (or eligible for it if control)						
Retail	0.19	0.23	0.20	0.19	0.23	0.941
Manufacturing	0.38	0.37	0.38	0.39	0.36	0.997
Services	0.42	0.40	0.42	0.42	0.41	0.998
Colombo	0.42	0.43	0.33	0.39	0.39	0.790
More than two paid workers	0.50	0.48	0.50	0.41	0.53	0.707
Says would register and sees benefit to doing so	0.56	0.45	0.61	0.54	0.59	0.379
Sales in last month	138,985	136,883	146,917	150,559	196,391	0.780
	(28,001)	(21,490)	(25,547)	(31,000)	(41,127)	
Doesn't keep records	0.54	0.60	0.56	0.64	0.52	0.609
Years of education of owner	10.81	10.38	10.65	11.19	10.19	0.241
	(0.36)	(0.41)	(0.28)	(0.29)	(0.37)	
Number of paid employees	2.64	2.98	3.05	2.61	2.77	0.695
	(0.18)	(0.31)	(0.29)	(0.30)	(0.21)	
Registered at the Pradeshiya Saba	0.64	0.60	0.62	0.54	0.59	0.834
Profits in last month	26,449	31,270	30,945	30,759	33,754	0.800
	(3,210)	(4,480)	(3,364)	(4,203)	(7,359)	
Sample size	78	65	66	59	64	

Notes: Treatment group 1 is the information and reimbursement only group. Groups 2, 3, and 4 were offered Rs 10,000, Rs 20,000, and Rs 40,000, respectively. Standard errors for means of continuous variables shown in parentheses.

control group registered. Columns 1 and 2 show the treatment effects as proportions of those in each treatment group, while the TOT effects in columns 3 and 4 are effectively these numbers as a proportion of firms actually offered the treatment. Thus for the information and reimbursement of costs group, there is no significant effect, with more firms from the control group registering. This is a reasonably precise zero

Table 2—Treatment Effects

	Intention-to-treat		Treatment on the treated	
Dependent variable: registered during intervention window	OLS (1)	OLS (2)	IV (3)	IV (4)
Information and reimbursement treatment	−0.00943	−0.0101	−0.0126	−0.0138
	(0.0165)	(0.0254)	(0.0219)	(0.0286)
Rs 10,000 treatment	0.135***	0.134***	0.216***	0.214***
	(0.0380)	(0.0380)	(0.0576)	(0.0515)
Rs 20,000 treatment	0.105***	0.105***	0.167***	0.167***
	(0.0350)	(0.0387)	(0.0534)	(0.0508)
Rs 40,000 treatment	0.275***	0.273***	0.476***	0.471***
	(0.0473)	(0.0453)	(0.0691)	(0.0598)
Strata/quintuplet dummies	No	Yes	No	Yes
Observations	520	520	520	520
R^2	0.102	0.284		
p-values for testing				
Rs 10,000 treatment = 20,000 treatment	0.5320	0.5264	0.4993	0.4470
Rs 10,000 treatment = 40,000 treatment	0.0152	0.0086	0.0021	0.0002

Notes: Robust standard errors in parentheses. Treatment on the treated instruments whether people actually received the offer of this treatment with whether they were assigned to receive this offer.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

effect, with a 95 percent confidence interval for the ITT being (−6 percent, +4 percent). We can therefore reject that information and reimbursement alone results in more than a tiny fraction of firms formalizing.

The Rs 10,000 and Rs 20,000 offers have effects similar to one another (we can't reject equality), with 17 to 22 percent of firms registering in response to the offer. Meanwhile, 48 percent of those offered Rs 40,000 registered in response. The effect of the Rs 40,000 treatment differs from either the Rs 10,000 or Rs 20,000 treatment at standard significance levels. Controlling for the strata dummies increases slightly the precision of the estimates and the share of variation in registration explained by the regression, but does not change the magnitude of the coefficients—as one would expect given baseline balance.

In August 2009, we followed up with all of the 29 firms in the Rs 40,000 treatment group that were offered the payment but chose not to register. We asked why they had not done so. The firms can be divided into three groups. The largest number, 14, said they had started the process of registering, only to learn that because they did not own the land they operated on, they would need to obtain the land owner's permission to register. In many of these cases, the land was owned by the government or a temple, and owners told us that obtaining formal permission was impossible. With this group we therefore made no further offers, concluding that land issues were the reason for lack of registering. Six of the firms told us they were willing to register at Rs 40,000, but had either needed more time or had not understood the offer. We therefore gave them one more month for the offer at Rs 40,000, during which only one business registered. The final group of 9 firms had not completed the registration process because they felt the benefits did not

outweigh the costs, even with the Rs 40,000 payment. We told these firms that we had some money left over from the project, and would increase the offer for a final offer of Rs 80,000. Only two of the nine firms registered when given this higher offer. It therefore seems that these remaining informal firms are resolutely informal, with extra time or extra money not getting many more to register.

B. *Which Owners Formalized?*

Table 3 presents the results of probit estimation of the likelihood of registration for each of the three treatment groups given monetary incentives. This allows us to examine which characteristics of firms are correlated with greater demand for formalization. The base specification in column 1 contains dummies for the Rs 20,000 and Rs 40,000 treatment offers, controls for the variables we stratified on, and an indicator of whether the business operates on public land. We see the demand for formalization is higher in Colombo, where the process is easier, although not significantly so at standard levels ($p = 0.108$), and is 27 percentage points lower for businesses that operate on publicly owned land, an effect that is significant at the 1 percent level. There is no significant difference in demand to formalize by industry, pre-expressed willingness to register, or baseline size in terms of employment or sales, although the standard errors are relatively large for some of these dimensions of heterogeneity.

The remaining columns examine alternative explanations that may enter into a firm owner's decision to formalize. Column 2 tests whether less knowledgeable and less informed owners are more likely to register, perhaps because the information is more useful to them. We see no statistically significant effects, and the point estimate suggests that, if anything, it may be *more* knowledgeable owners who register when given monetary treatments. Column 3 examines factors associated with the likely future costs of formalizing—these costs are expected to be higher for firms with higher profits (that would face more taxes) and expect to have more workers (and thus face labor taxes). We see a negative and significant association with expecting to grow to 15 or more workers, where the costs of formalizing become greater due to labor regulations. Column 4 looks at the role of risk and time preference factors. We find no significant impact of being a hyperbolic discounter or of individuals saying they are risk seeking.[8] The final column uses business assets at baseline as a proxy for wealth, as a test of liquidity constraints. The coefficient is positive and statistically insignificant, suggesting that it is not the case that poorer firms are more likely to respond to the incentives. Overall we view the results as indicating that most of those becoming formal are informed owners who rationally weigh the costs and benefits of formalizing.

[8] Risk preferences are measured on an 11-point scale taken from the German Socioeconomic Survey, which asks "are you generally a person who is fully prepared to take risks or do you try and avoid taking risks." Hyperbolic discounting is measured by asking firms hypothetical questions about how much they would be prepared to take today compared to Rs 10,000 in one month, and similarly for five months versus six months. Twenty percent of the firms in our sample are classified as hyperbolic.

Table 3—Among Firms Offered Money, Which Ones Formalized?
Marginal Effects from Probit Estimation of Registration among Sample Offered
Rs 10,000, Rs 20,000, or Rs 40,000 Treatments

	(1)	(2)	(3)	(4)	(5)
Rs 20,000 treatment	−0.0216	−0.0240	−0.00650	−0.0169	−0.0490
	(0.0886)	(0.0892)	(0.0877)	(0.0886)	(0.0937)
Rs 40,000 treatment	0.307***	0.311***	0.302***	0.311***	0.312***
	(0.0901)	(0.0908)	(0.0917)	(0.0903)	(0.0924)
Colombo	0.132	0.0859	0.143*	0.127	0.162*
	(0.0831)	(0.0859)	(0.0833)	(0.0844)	(0.0914)
Retail	0.00266	0.0370	−0.0204	0.00226	−0.0403
	(0.102)	(0.105)	(0.101)	(0.100)	(0.100)
Manufacturing	−0.0361	−0.0241	−0.0296	−0.0294	−0.105
	(0.0813)	(0.0822)	(0.0816)	(0.0819)	(0.0844)
More than two paid workers	0.0515	0.0867	0.0744	0.0694	0.0112
	(0.0795)	(0.0804)	(0.0805)	(0.0808)	(0.0859)
Says would register and sees benefit to doing so	0.0377	0.0609	0.0444	0.0450	0.0434
	(0.0769)	(0.0779)	(0.0775)	(0.0766)	(0.0794)
log monthly sales in December 2008	−0.0259	−0.0255	0.00358	−0.288	−0.0377
	(0.0421)	(0.0417)	(0.0531)	(0.0415)	(0.0462)
Operate on publicly owned premises	−0.269***	−0.278***	−0.265***	−0.270***	−0.234***
	(0.0744)	(0.0716)	(0.0754)	(0.0737)	(0.0849)
Education of owner (years)		−0.0126			
		(0.0132)			
Digitspan recall of owner		0.0388			
		(0.0303)			
Owner knows cost of registering		0.118			
		(0.0792)			
Expects to have 15 or more employees			−0.156**		
			(0.0748)		
Profits in December 2008 exceed income tax threshold			−0.0805		
			(0.0993)		
Hyperbolic discounter				−0.0422	
				(0.0952)	
Risk seeker				0.0284	
				(0.0185)	
log business assets in December 2008					0.0499
					(0.0305)
Observations	181	181	181	181	163

Note: Robust standard errors in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

C. *A Demand Curve for Formalizing*

With a few more assumptions, we can back out a firm's demand curve for formalization. The main private costs to a firm of formalizing are the upfront registration cost of Rs 1,000 and annual income taxes paid on firm profits.[9] Assuming that firms

[9] In addition, firms also face time costs of registering that we do not include here given uncertainty about what these costs would be for the firms who do not register and the costs involved in fulfilling other requirements needed to register (such as moving to new premises if they are located on publicly owned land).

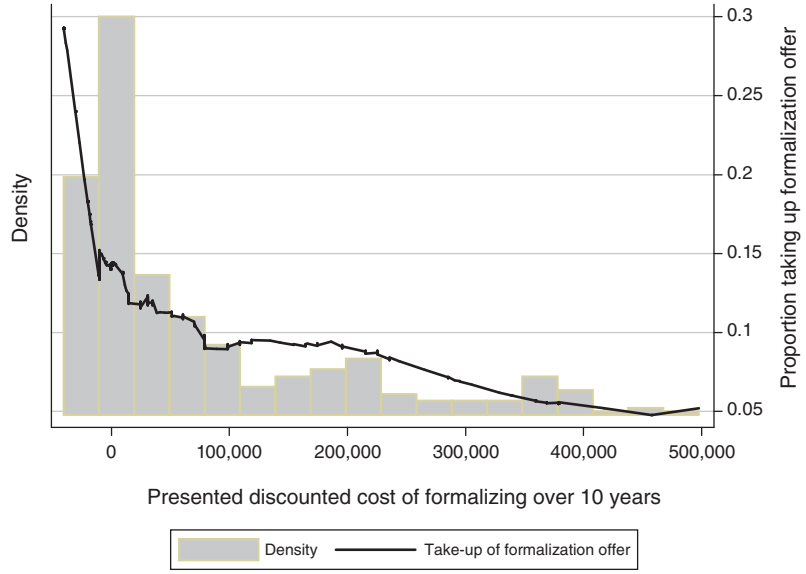

Figure 3. Demand Curve for Formalizing

Notes: Density shows a histogram indicating the underlying distribution of the data. Take-up line is a lowess line of take-up of the formalization offer fitted against the estimated net cost of formalizing. This net cost includes the registration cost and the present discounted value of tax payments over 10 years, assuming a 5 percent discount rate, and 10 percent per year profits growth less the value of any incentive payments offered as a treatment in our experiment.

would report the same amount of profit income to the tax authorities as they report to us, nominal profits grow at 10 percent per year, and firms discount the future at a 5 percent discount rate, we calculate the net present value of 10 years of tax payments. Twenty percent of firms would pay no taxes, 35 percent would pay less over 10 years than our Rs 10,000 treatment, and 48 percent less than our Rs 40,000 treatment. Figure 3 then plots a lowess line of fitted take-up of formalization during our offer period against the net cost of formalizing. The demand curve slopes down, with a relatively steep slope when net costs are negative, and a much flatter slope when net costs are positive. The demand curve can then be viewed as an approximation of the proportion of firms who believe the benefits of registering exceed these costs at each net cost. Given the assumptions required to obtain this curve, we view this as mainly illustrative, but it does suggest that most firms do not view the benefits as exceeding the costs.

In sum, we interpret the evidence on registration as providing support for the exit view of informality supported by Levy and others over the exclusion view of de Soto. The fact that there is no effect of providing information and reimbursement of direct costs, even in Colombo, where the registration process is very streamlined, suggests that most informality in this environment is not driven by costs of registering a firm. A demand curve that falls very steeply around the zero net cost point also suggests that few firms perceive large positive benefits of being formal. However, we do find support for the de Soto view in a minority of firms that face legal barriers

to registration because, for example, they operate on public property. These firms may be thought of as excluded from the formal sector in the de Soto sense.

IV. What Are the Consequences of Formalizing?

We carried out three follow-up surveys to measure the impact of formalizing on firms (Figure 2). The first follow-up was in August 2010, corresponding to a period of between 12 and 18 months after firms were induced to register. We were able to reinterview 465 of the original 520 firms (89 percent), with the most common reasons for attrition being not being able to find the owner (20 out of the 55 cases), the owner being abroad (9 out of 55 cases), and firm owners refusing to be reinterviewed (9 out of 55 cases). We cannot reject the null hypothesis that attrition is unrelated to treatment status at conventional significance levels ($p = 0.17$). The second follow-up survey was conducted in March 2011, at a period averaging 22 months after the start of the intervention. We reached 445 firms in this round, with attrition again unrelated to treatment status ($p = 0.35$). The final follow-up survey was conducted in December 2011, an average of 31 months after treatment. In this last survey, 424 firms were interviewed with attrition again unrelated to treatment status ($p = 0.40$). In addition, proxy reports were used to determine whether the business was still open for a further 59 firms. The remaining 37 firm owners were unable to be located, largely due to their having moved out of the study areas.

The follow-up surveys collected data on both the intermediate channels through which formalization is often hypothesized to increase profitability, as well as firm profits, sales, capital stock, and employment. Our key outcome of profitability is measured via a direct question, following years of study of how to collect profits from firms in Sri Lanka summarized in de Mel, McKenzie, and Woodruff (2009).[10]

Table 4 examines whether any of the treatments is significantly associated with firm survival in the fourth round survey, or with reporting profits in any survey round. It does this by estimating equation (1) with survival or reporting profits as the dependent variable. In all cases, we find no significant relationship between treatment and the likelihood of survival or reporting profits in our follow-up surveys. Moreover, online Appendix Table 2 shows that treatment does not appear to be associated with which firms survive. The online Appendix reports the same characteristics as in Table 1, and shows balance on baseline characteristics for the firms surviving to the final survey. As a result we ignore attrition in what follows.

A. *What Do Firms Say the Consequences of Formalizing Are?*

The August 2010 survey asked the firms that formalized as a result of our intervention how they had benefited from being formal. The most common response,

[10] A potential concern one might have is whether firms directly count the cash given to them as "profits." Our measure of profits is based on total income earned in the past month after paying all expenses. This does not include cash on hand or money in a bank account and, since our direct question asks about the last month, would not include any money the firm earned as a windfall payment from registering. Moreover, the fact that the treatment impact on profits does not change significantly over the three follow-up rounds also helps rule out a temporary blip due to firm owners counting the treatment amount as profits in the first follow-up wave.

Table 4—Does Treatment Status Predict Survival or Reporting Profits in Survey?

	In business at R4 (1)	Report profits in R2 (2)	Report profits in R3 (3)	Report profits in R4 (4)
Information and reimbursement treatment	0.000653	0.0556	0.0266	0.0531
	(0.0408)	(0.0569)	(0.0543)	(0.0618)
Rs 10,000 treatment	0.00231	0.00747	−0.0600	−0.0719
	(0.0428)	(0.0567)	(0.0592)	(0.0682)
Rs 20,000 treatment	0.0203	0.0190	0.0667	0.0571
	(0.0399)	(0.0559)	(0.0517)	(0.0632)
Rs 40,000 treatment	0.00114	0.0425	−0.0462	−0.0140
	(0.0428)	(0.0572)	(0.0591)	(0.0649)
Observations	483	520	520	520
F-test *p*-value that jointly zero	0.9849	0.8262	0.1049	0.2751

Notes: All regressions include controls for randomization strata. Robust standard errors in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

given by 36 percent of firms, was that they had yet to see any benefit from registering. The next most common response, given by 20 percent of firms, was an answer related to improvements in the image of the business. This encompasses answers like "it is good publicity," "customers trust the business more," and "social validity." Other common responses refer to feeling more secure and protected (12 percent) and to being able to use the registration to help obtain business loans (10 percent). Very few firms claimed to have obtained a loan or to have received a government contract as a result of formalizing.

B. *Econometric Estimation of the Consequences of Formalizing*

We use the follow-up data to estimate the impact of formalizing on firm outcomes, intermediate channels, and attitudes of firm owners. For outcome Y and firm i in randomization strata s, we estimate

$$Y_{i,t} = \alpha_{i,s} + \beta Formal_i + \gamma Y_{i,0} + \sum \delta_t + \varepsilon_{i,t}, \tag{2}$$

where $\alpha_{i,s}$ are randomization strata fixed effects; $Y_{i,0}$ is the baseline value of the dependent variable; and δ_t are survey wave effects. Our main object of interest is in estimating β, the causal impact of becoming formal (defined in terms of being registered with the DS) on the outcome of interest.

A key concern given our limited take-up of the formalization treatments is whether we have the power to detect impacts of formalizing, especially on outcomes like sales and profits, which are relatively noisy. We employ several methods to try and maximize power. First, we use stratified randomization and include randomization strata dummies to boost power relative to a pure random draw. Second, the ANCOVA specification we employ, which includes the lagged dependent variable as a regressor, increases power relative to difference-in-differences or analysis using only the follow-up data, and helps control for any selective attrition based

on the outcome of interest.[11] Third, we pool together all rounds of follow-up data (McKenzie 2012). Online Appendix Table 3 shows we cannot reject equality of impacts on profits across our three follow-up rounds ($p > 0.90$). Fourth, for the key outcomes of firm profits and firm sales, the March and December 2011 survey asked for data on each of the past three months. We then run the specification in (2) with seven months of observations per firm for these outcomes. Finally, to counter the possibility that low power is driven by a few outliers, we also consider truncated profits and sales as outcomes.

Standard errors are clustered at the firm level. We instrument for formalization with three variables indicating assignment to the Rs 10,000, Rs 20,000, and Rs 40,000 treatments, respectively. Since information and reimbursement only did not lead to changes in registration, this treatment does not serve as an instrument, and we drop this group from this part of the analysis.

The treatments are valid instruments for being formal under the assumption that they affect the outcomes of interest only through changing registration status, and not through any other channel. An obvious concern with this assumption is that the grants given to the firm owner may have had independent impacts on the business through alleviating credit constraints. If such impacts are positive, using the treatment assignment as an instrument will overstate the gains to formalizing, providing us with an upper bound of the consequences of formalizing.

An alternative approach is to control for capital stock in regression (2), and thereby attempt to identify the impact of formalizing through channels other than changing capital stock. We use log capital stock as the control, given the skewness of this variable. This will net out any impact of the grants on capital stock. But the inclusion of capital stock as a control also removes the effect of changes in capital stock that come from formalizing. If formalizing increases access to credit, for example, firms might invest more. This specification should therefore serve as a lower bound for the impact of formalizing. For this approach to be valid, we have to assume that the cash grant affects profits directly only through investments in capital,[12] and then make what Imai et al. (2011) refer to as a sequential ignorability assumption. In our context, this amounts to assuming that—conditional on treatment assignment, the lagged dependent variable, and the strata randomization controls—capital stock is independent of the outcome of interest. Given the rich set of controls used here, this appears reasonable.

C. *Impact on Major Firm Outcomes*

Table 5A presents the results of estimating equation (2) for the key outcomes of firm profits, sales, employment, and capital stock. Consider first the impact on firm profits. Column 1 shows an upper bound estimate of a Rs 13,706 increase in monthly profits from formalizing, which is large relative to the mean profits in the

[11] Recall that we can not reject that attrition is unrelated to treatment status.

[12] Our measure of capital stock includes inventories and raw materials. Given that few firms hire workers, and that firms do not hire consultants or pay for skills training, investments in capital seems the most likely channel for cash to directly influence profits.

Table 5A—Effect of Formalizing on Firm Outcomes

	Monthly profits	Truncated profits (99th)	Truncated profits (95th)	Log profits	Monthly sales	Truncated sales (99th)
Full effect (*upper bound*)						
Registered with the DS	13,706*	10,834	5,923	0.357*	122,295	99,073
	(8,241)	(6,847)	(4,774)	(0.202)	(85,196)	(72,440)
Effect after controlling for log capital stock (*lower bound*)						
Registered with the DS	8,996	7,054	2,544	0.168	61,868	44,438
	(8,074)	(6,474)	(4,462)	(0.187)	(85,602)	(71,944)
Observations	2,181	2,181	2,181	2,123	2,139	2,139
Mean for control group in sample	30,537	28,662	25,048	9.87	237,185	211,399

	Log sales	Number of paid workers	Recruited a new worker	Log capital stock
Full effect (*upper bound*)				
Registered with the DS	0.460	0.525	0.102	0.396
	(0.319)	(0.426)	(0.0902)	(0.258)
Effect after controlling for log capital stock (*lower bound*)				
Registered with the DS	0.174	0.431	0.106	N/A
	(0.298)	(0.437)	(0.0925)	
Observations	2,088	1,017	1,017	1,009
Mean for control group in sample	11.42	2.35	0.36	12.278

Notes: Registration with the DS instrumented with offer of the Rs 10,000, Rs 20,000, or Rs 40,000 registration treatments. Information only treatment group excluded from these regressions. All regressions include controls for randomization strata and for the baseline value of the dependent variable. Robust standard errors in parentheses, clustered at the firm level.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

control group of Rs 30,537, and significant at the 10 percent level. The lower bound estimate, controlling for capital stock, is one-third smaller (8,996), and not significant at standard levels ($p = 0.265$). These point estimates are sizable relative to both the upfront costs of registering (Rs 1,000) and the annual income taxes for a typical firm of this level (Rs 3,000). They are also sizable relative to the incentives provided to register. Columns 2 and 3 show the results of truncating profits at the ninety-ninth and ninety-fifth percentiles, respectively. The latter more than halves the point estimate of the impact on profits. In contrast, log profits are also significant (column 4). Columns 5–7 show large positive coefficients for sales, which are not statistically significant in levels, and which halve when we control for capital stock. The remaining columns show positive, but insignificant, impacts on employment and capital stock. It should be noted that the standard errors on the sales, employment, and capital stocks imply that we only have power to rule out very large improvements (or declines) from formalization. For example, the 95 percent confidence interval for the impact on profits truncated at the ninety-fifth percentile level after controlling for capital stock is (−6,201, +11,289), which encompasses a 24.7 percent decline and a 45.0 percent increase in profits.

To see where this statistically significant increase in profits is coming from, Figure 4 plots the cumulative distribution function of profits in our final round

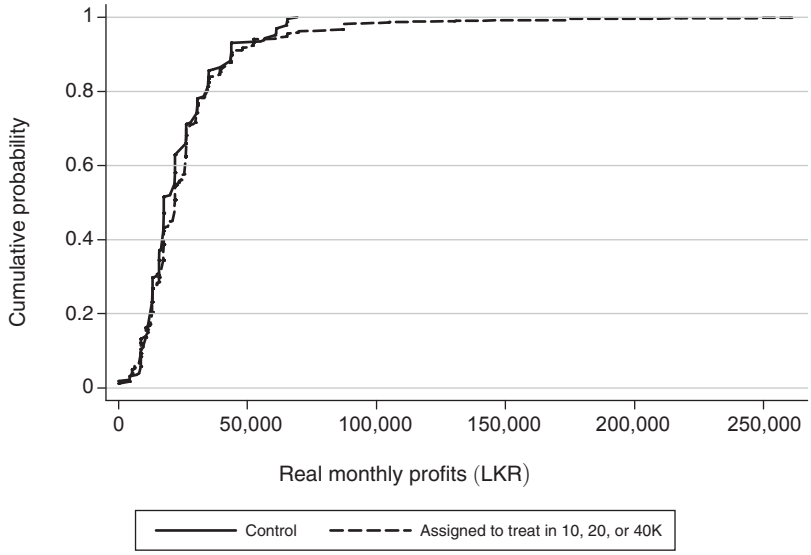

FIGURE 4. DISTRIBUTION OF FINAL ROUND REAL PROFITS BY TREATMENT STATUS

survey for the combined monetary treatment groups and for the control group. The distributions are extremely similar to one another right up until the very top tail, where they diverge. The significant mean effect is thus being driven by this handful of firms which formalized and experienced large increases in profits, whereas for most of the distribution the treatment does not influence profits.

An obvious question is whether these large profits in the upper tail represent genuine changes or measurement error and/or idiosyncratic shocks for a few firms. To address this question, in June 2011 we conducted open-ended discussions with a set of firms experiencing large increases in profits after registering. We wanted to see whether they had taken specific actions following registration and whether they appeared to benefit from doing so. Although small in number, these more detailed case studies provide support for the idea that a few firms did benefit substantially from formalizing. For example, two of the firms were in the vehicle repair business—one automobiles and one autorickshaws. Both said that an important consequence of registration was the ability to become an official parts distributor for an auto parts manufacturer. Previously, they had purchased parts from another dealer, i.e., at higher than the wholesale price. Both had also undertaken expansions of the physical facilities, with one adding an auto lift and a customer waiting room, while increasing employment from two to eight workers. A saw mill that registered said the key was to be able to put the forest service stamp on the receipts he issued. The stamp allows customers to transport the wood across municipal boundaries without obtaining further permissions. His estimate was that he had previously lost 25 percent of sales to other mills that could provide this stamp. Finally, a grocery store and tea (snack) shop had used the license to obtain a loan to purchase a delivery truck. The truck was used in the business, but also leased out. On his own, the owner had gone to the health department to request a health inspection for his tea shop. He was intent on obtaining a health sticker he could display, which would allow him to open

a bakery. A final case was of a fish market vendor, who had used the Rs 40,000 grant to buy a freezer which he used to increase his stock. The owner also used his registration to print a receipt book to give receipts to restaurants.

In all these cases, there were clear changes in the business operation, and in the attitude and vision of the owners. Given the small number of cases we are examining, it is possible that these tail events are just the result of luck, which owners ex post attribute instead to formalizing. We therefore view this as only suggestive, but do at least believe this growth at the top tail of the distribution is genuine and not measurement error. But such firms were rare. Most firms that had formalized saw no such improvements.

D. *Impact on Mediating Channels*

The above analysis suggests that formalization has not had significant impacts on key outcomes for most firms that formalized, but that it has helped a handful of firms grow substantially. Our follow-up surveys span a period of 15 to 31 months after treatment. This should be a sufficient period to detect effects for most firms. Nevertheless, one concern is that the standard errors in Table 5A are relatively large, reflecting relatively low power to detect effects. The CDF in Figure 4 is comforting in this regard, as it shows strikingly similar distributions of profits for treatment and control for most of the distribution. However, a further approach to determining whether formalization benefits firms is to look for evidence on the mediating channels through which advocates claim formalization works. Impacts through these intermediate channels may materialize more quickly. The intermediate channels also suffer from less variation, enabling more precision.

Table 5B presents the impact of formalizing on channels through which formalization might be expected to influence firm outcomes. The assumption that the grants do not independently affect these channels is more plausible here than for profits or capital stock, and consistent with this, we find that the upper and lower bounds are quite close to one another. We therefore just present the upper bound estimates, which do not control for capital stock.

The results show little evidence that formalizing has impacts on the main channels through which formalization is often hypothesized to work. We find no significant effect on relationships with the financial sector (applying for business or personal loans or having a business bank account), or on relationships with the government (having an electricity connection in the business name, applying for a government contract, making sales to the government, or participating in any government SME program). Moreover, the point estimates on most of these variables are close to zero, with relatively small confidence intervals in most cases. The point estimates also suggest businesses are not any more likely to pay taxes, and the point estimate is actually negative on the amount of taxes paid. We do not find any evidence that businesses change location after formalizing.

The only intermediate channels that show significant impacts are the use of receipt books ($p = 0.007$) and an increase in advertising ($p = 0.003$). Both outcomes retain significance at standard levels, even if we conservatively control for multiple hypothesis testing of 14 intermediate channels by multiplying each by 14 to

Table 5B—Effect of Formalizing on Different Channels

	Paid taxes	Amount of taxes paid	Formal accounting	Has receipt book	Business bank a/c.	Applied for business loan	Applied for personal loan
Registered with the DS	−0.0643	−8,865	−0.103	0.352**	0.0239	−0.00421	−0.0451
	(0.142)	(6,670)	(0.0763)	(0.130)	(0.0812)	(0.0770)	(0.0690)
Lag included	No	No	Yes	No	Yes	No	No
Observations	1,036	1,036	1,016	1,049	1,059	1,059	724
Survey rounds question asked	R2, R3, R4	R2, R3, R4	R1–R4	R2, R3, R4	R1–R4	R2, R3, R4	R2, R3
Mean for control group in sample	0.66	6,800	0.141	0.31	0.14	0.10	0.056

	Share of sales made to government (%)	Electric connection in bus. name	Applied for govt. contract	Participate in govt. SME program	Advertised in last six months	Business has clear and visible sign	Changed location
Registered with the DS	3.543	−0.152	0.000453	0.0535	0.261***	−0.0895	−0.0504
	(2.285)	(0.116)	(0.0540)	(0.0449)	(0.0892)	(0.130)	(0.0870)
Lag included	No	No	No	No	Yes	Yes	No
Observations	1,020	724	724	724	1,036	1,030	1,016
Survey rounds question asked	R2, R3, R4	R2, R3	R2, R3	R2, R3	R1–R4	R1–R4	R2, R3, R4
Mean for control group in sample	0.96	0.40	0.022	0.033	0.16	0.56	0.18

Notes: Registration with the DS instrumented with offer of the Rs 10,000, Rs 20,000, or Rs 40,000 registration treatments. Information only treatment group excluded from these regressions. All regressions include controls for randomization strata, and for the lagged dependent variable if it is collected. Robust standard errors in parentheses, clustered at the firm level.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

obtain Bonferroni p-values. This is consistent with the nonexperimental evidence in McKenzie and Sakho (2010) who suggest the main effect of formalizing in Bolivia is to expand sales through increasing the use of receipts. However, the evidence from Figure 4 and Table 5A suggests that for most firms this channel has not yet resulted in significantly higher profitability.

E. *Impacts on Attitudes*

The tea shop owner who contacted the health department about an inspection raises the question of whether formalization may have changed attitudes about the government and market regulations more generally. Di Tella, Galiani and Schargrodsky (2007) study formalization in a different domain—formal titles among land squatters—and find very large effects of obtaining formal property title on beliefs that favor the workings of the market. Motivated partly by this, in the first follow-up survey we asked a number of questions on trust and attitudes that enable us to examine the impact of formalizing on the attitudes that firm owners have toward government, the courts and police, and taxation.

Table 5C—Effect of Formalizing on Attitudes in August 2010 Follow-up Survey

	Trust in provincial government	Trust in municipal government	Trust in national government	Trust in the courts	Trust in the police	Confident in police & courts to settle bus. dispute
Registered with the DS	0.634***	0.559**	0.227	−0.0387	−0.0993	0.0267
	(0.234)	(0.234)	(0.221)	(0.188)	(0.238)	(0.203)
Lag included	Yes	Yes	Yes	Yes	Yes	No
Observations	369	369	369	369	369	369
Mean for control group	0.360	0.310	0.620	0.811	0.567	0.744

	Agrees being good citizen means paying taxes	Thinks govt. charges business too much tax	Feels govt. spends revenue on important items
Registered with the DS	0.511**	0.517**	−0.0410
	(0.188)	(0.238)	(0.232)
Lag included	No	No	No
Observations	369	369	369
Mean for control group	0.767	0.444	0.378

Notes: Registration with the DS instrumented with offer of the Rs 10,000, Rs 20,000, or Rs 40,000 registration treatments. Information only treatment group excluded from these regressions. All regressions include controls for randomization strata, and for the lagged dependent variable if it was collected. Robust standard errors in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 5C shows the results. We find very strong positive effects of formalizing on trust in the provincial government and trust in the municipal government, the two levels of government that business owners interact with during the registration process. The p-value on trust in the provincial government is 0.007, so it remains significant even after controlling for multiple hypothesis testing over the nine outcomes in the table. In contrast, we find no significant impact on trust in the national government, trust in the courts, or trust in the police, and formalizing does not make firms any more likely to be confident in the police and courts to resolve business disputes.

One interpretation of the increase in trust is that those firms that formalized had to deal with the DS and municipal governments in the registration process, and may have been surprised to find the process less burdensome and less subject to bribes than they had imagined. That is, the *process* of formalizing might be the lever for attitude change, by demonstrating that these levels of government can be trusted. An alternative potential explanation is that the change in attitudes is a consequence of being formalized. That is, if firm owners no longer worry about the provincial and municipal governments shutting them down for lack of compliance, they may be more trusting of these levels of government.

We also see formalizing leads to changes in attitudes toward taxes. Firms that formalize are much more likely to agree that part of being a good citizen means paying taxes. However, they are no more likely to think the government spends its revenue on items important to these firm owners, and also significantly more likely to agree that the government charges businesses too much in terms of taxes. Recall

that actual taxes are quite low for most businesses in this sample, so this perception contrasts with the reality facing most firms.

V. Discussion and External Validity

Our baseline sample was a random sample of informal firms in the two largest cities in Sri Lanka. As with all microeconomic studies, there is a question of external validity. We present evidence here to suggest that our results from Sri Lanka are likely informative of constraints to formalizing firms in other countries.

First, we note that the pattern of informality with regard to firm size, and what firms say are the potential advantages and disadvantages of formalizing are similar to those we see in other countries for which data exist. Online Appendix Figure 1 compares municipal government and tax authority registration rates by firm size in Sri Lanka, Bangladesh, and Mexico. In all three countries, the data are representative of firms in the largest urban areas.[13] Sri Lanka has tax registration rates that lie between those in Mexico and Bangladesh. The figure shows that in these other countries, registration also typically occurs over the size range considered in our study—not with the smallest microenterprises.

The Bangladesh survey asked firm owners what they saw as the main advantages of being formal. Owners give responses that are similar to those in the Sri Lanka survey: links to bank financing, better reputation for the business, a lower chance of being fined, and the ability to operate visibly at a large scale without fear of being caught. Smaller and more informal firms in Bangladesh are more likely to say they see no potential benefits and all firms say the main disadvantages were paying taxes and having to deal with the cost and process of registering (McKenzie 2010). These same channels also appear in discussions of the costs and benefits of formalizing in different Latin American countries (Perry et al. 2007; World Bank 2009).

As a result, it seems reasonable to believe that our results are informative outside Sri Lanka about the number of firms at the margin that will be induced to formalize by relatively small changes in the costs and benefits, and also about the characteristics of those firms. Our results suggest that taking the costs of registering from the levels in Sri Lanka to zero induces few firms to formalize. However, increasing the benefits further (in our case by paying firms) induces more firms to formalize. This is consistent with recent cross-country panel data, in which Klapper and Love (2010) find that only changes in business environment reforms, which involve more than a 40 percent reduction in costs, are associated with changes in firm entry.[14]

[13] The Sri Lankan data come from the Sri Lankan Longitudinal Survey of Entrepreneurs, which draws a random sample of 2,255 firms from household listings in 31 cities outside the northern province. In Mexico, the data are from the 2002 version of the National Microenterprise survey, conducted in urban areas with a sample drawn from a household-based nationally representative labor survey. The Bangladesh survey data come from a census of 55,817 firms in randomly selected sampling areas from 19 districts conducted by the World Bank in 2009–2010.

[14] Our results are also consistent with evidence from an unpublished parallel experiment in Lima, Peru, which randomly encouraged firms to obtain a municipal license (Jaramillo 2009; Alcázar, Adrade, and Jaramillo 2010). About one-quarter of firms offered information and reimbursement of direct costs obtained the municipal license. One-third of those who didn't register reported problems with other regulations, such as zoning, consistent with our finding that land issues prevent many firms from registering. A follow-up survey 18 months after baseline failed to find any significant impact of registration obtaining the municipal license on firm size, access to credit, or profitability, but their power to detect an effect is low. Moreover, since banks and the government typically require a tax

VI. Conclusions

Prior to the intervention, owners of unregistered firms were either ignorant of, or vastly overestimated the costs of registration. We might therefore have expected that simply informing firms about the costs of registration would be sufficient to induce registration. Such a finding would have been consistent with the view, often attributed to de Soto, of informal firms being excluded from the formal sector by the perception, if not the reality, of high entry barriers. But in fact we find that information and reimbursement for the modest direct costs do not result in any increase in registration. Instead, registration is spurred only when the information is combined with incentive payments.

The incentive required to induce registration is modest compared to reported profit levels of the sample enterprises. A payment of two month's profit is sufficient to induce half of the firms to register. The willingness of firms to register for a modest payment suggests that they perceive modest costs but even more modest benefits from being formal. The relatively high rates of registration induced by these payments are consistent with firms taking a rational cost-benefit approach to the decision to formalize, as suggested by Levy (2008). Among those not registering after receiving an offer of this magnitude, more than half took some steps toward registering. They stopped only when learning that issues of land ownership would prevent them from registering without paying significant fees to landlords, temples, or the government. Thus, in the absence of land issues, the net costs of formality appear to be modest for almost all of the informal firms represented by our sample—though the minority of firms affected by land issues may be seen as excluded from the formal sector.

The net benefit of formality also appears modest for most firms. While we do find an increase in profits after formalization, the mean treatment effect seems to be driven by a successful upper tail. Firms that formalize begin advertising and using receipt books more often, but don't appear to get the more touted benefits of formalizing, such as increased access to credit, obtaining government contracts, or participating in government programs. Most firms seem therefore to be rationally refraining from formalizing, while a few seem to be suboptimally informal.

This finding is important for two reasons. First, while governments clearly should not mimic our experiment with a policy of direct payments, the results do suggest that modest increases in the perceived benefits of being formal could be expected to dramatically increase the demand to formalize among firms currently operating informally. Whether tax collections increase enough to pay for these increased benefits (or, alternatively to pay for increased enforcement efforts) is beyond the scope of this paper. Our data do suggest, though, that given the current tax code, the additional tax collections would not be large among this sample of firms. This, combined with the fact that the spillovers to growth are modest, suggests that near-term gains to the government of increased formality are limited. Second, despite the pervasive interest of governments around the world in trying to increase the size of their formal sector, our results overall suggest little in the way of pent-up demand to become

license rather than municipal license as proof of formality in many countries, there may be less ex ante reason to expect a positive impact on firms of municipal registration.

formal among existing firms. Nevertheless, our results do show an increase in trust in the government as a result of formalizing. It is possible that formalizing a large number of firms would offer broader benefits to society in terms of trust.

Finally, it should be noted that our study measures the benefits of formalizing for firms already in business. There may well be benefits from the simplification of registration at the extensive margin, if high-ability entrepreneurs not currently operating a business are induced to enter. Measuring such impact remains a key area for future research.

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