A Helping Hand or the Long Arm of the Law?

Experimental evidence on what governments can do to formalize firms*

Gustavo Henrique de Andrade, *Governo do Estado de Minas Gerais*

Miriam Bruhn, *World Bank*

David McKenzie, *World Bank, BREAD, CEPR and IZA*

Abstract

Many governments have spent much of the past decade trying to extend a helping hand to informal businesses by making it easier and cheaper for them to formalize. Much less effort has been devoted to raising the costs of remaining informal, through increasing enforcement of existing regulations. We conducted a field experiment in Belo Horizonte, Brazil, in order to test which government actions work in getting informal firms to register. Firms were randomized to a control group or one of four treatment groups: the first received information about how to formalize; the second received this information and free registration costs along with the use of an accountant for a year; the third group was assigned to receive an enforcement visit from a municipal inspector; while the fourth group was assigned to have a neighboring firm receive an enforcement visit to see if enforcement has spillovers. We find zero or negative impacts of information and free cost treatments, and a significant but small increase in formalization from inspections. Our LATE estimates of the impact of actually receiving an inspection are much bigger, giving a 21 to 27 percentage point increase in the likelihood of formalizing. The results show most informal firms won't formalize unless forced to do so, suggesting formality offers little private benefit to them, but the tax revenue benefits to the governments of bringing firms of this size into the formal system more than offset the costs of inspections.

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1. Introduction

Spurred by the work of Hernando de Soto (1989), and the World Bank/IFC's *Doing Business* project, governments around the world have spent much of the past decade extending a helping hand to informal businesses by trying to make it cheaper and less burdensome to formalize. Since 2004, 75 percent of countries have adopted at least one reform making it easier to register a business (IFC, 2009). Yet, despite these efforts, the majority of firms in most developing countries remain informal, with studies which have examined the impact of these regulatory reforms finding that much of the action comes from increases in entry of new firms, rather than from formalization of existing firms (e.g. Klapper et al, 2006; Bruhn 2011).[1] Policymakers worry that a large stock of informal firms results in a loss in tax revenue, unfair competition for formal firms, and a culture of informality (Levy, 2008; Perry et al, 2007).

Although policymakers and researchers have devoted attention to reducing the costs of formalizing, much less attention has been given to the issue of increasing the costs of remaining informal. The most obvious way to raise these costs is to use the long arm of the law to increase enforcement of existing regulations. Yet to our knowledge, there is very little empirical evidence on whether enforcement attempts can induce firms to register, or whether they instead cause informal firms to close down. There is a long related literature in developed countries showing that increases in the probability of detection and enforcement lead to a increase in tax compliance, but that other factors such as a household's sense of moral or social obligation are also important (e.g. Alm et al. 1992, Andreoni et al, 1998). Several more recent non-experimental studies in developing countries have found evidence that the degree of enforcement matters for labor informality (Ronconi, 2007; Almeida and Carneiro, 2012), but there is no reliable evidence we are aware of on the impacts of enforcement on firm informality.

We conducted a field experiment together with the State Government of Minas Gerais in the city of Belo Horizonte in Brazil in order to test which government actions work in getting informal firms to register. Brazil began a process of simplification of firm registration in 1996 with the introduction of the SIMPLES tax system which consolidated multiple taxes and contributions into a single payment, and also lowered the tax burden on small firms. Within Minas Gerais, the Minas Fácil service was started in 2005 with the purpose of additionally reducing the number of procedures and time taken to start a

[1] An exception is Mullainathan and Schnabl (2010) who find that formalization of existing informal businesses accounts for about 75 percent of the increase in the number of newly licensed firms in Lima, Peru, after a simplification of municipal licensing. But even then, the numbers involved suggest that the vast majority of informal firms chose not to formalize.

business. Minas Fácil is a one-stop-shop system, where firms obtain municipal, state, and federal tax registrations simultaneously instead of having to request these from separate offices. However, despite these efforts, survey data from 2009 revealed that 72 percent of firms were still informal. As a result, the State Government wanted to test several competing mechanisms for reducing formality.

A listing survey was used to identify potentially informal firms, which were then randomized into four treatment groups and a control group. Survey data revealing a lack of knowledge about how to formalize motivated the first treatment, which was to provide information about how to register by means of a glossy brochure and a dedicated helpline. A second treatment coupled this information with an exemption in the registration fees and free use of mandatory accounting services for a year to test if reducing registration costs would induce formalization. The third treatment randomly assigned municipal inspectors to firms, to see whether increased enforcement would get firms to formalize. The final treatment consists of having a neighboring firm visited by an inspector, to test whether there is a spillover impact of inspection on the formalization behavior of other firms.

We find that efforts to help firms formalize by giving them information, and by reducing the initial cost to zero along with offering a free accountant resulted in an increase in knowledge about the role of accountants in the formalization process, but did not lead to firms formalizing. In fact, it resulted in a small reduction in firms registering through a separate formal category – that of individual entrepreneur – which the information campaign did not target and which had its eligibility criteria relaxed during the course of our study. Moreover, firms which were assigned to either of these two treatments expressed less trust in government in our follow-up survey.

In contrast, assigning firms to receive a visit from a municipal inspector did result in an increase in municipal registration, although the impact was much less than anticipated by the government, with only an additional 3 percent of those assigned to treatment formalizing. This low rate results from the inspectors finding some firms closed, not finding others, and from some firms in our sample already being formal to start with. An instrumental variables estimate suggests that the impact of actually receiving an inspector visit is much higher – resulting in a 21 to 27 percentage point increase in registration. We find no evidence of spillovers on neighboring firms, perhaps due to the relatively low increase in inspections and to many firms saying they do not communicate very much with neighboring businesses. Cost-benefit analysis suggests the revenue benefits to the government of bringing these firms into the formal system are likely to outweigh the costs of the added inspections needed to get them to formalize, suggesting that more inspections of informal firms of this size would be beneficial from a fiscal standpoint.

The most closely related papers to ours are two recent randomized experiments which test the role of “carrots” in inducing informal firms to register. De Mel et al. (2012) find no significant impact of information alone in getting firms to register with the tax authority in urban Sri Lanka, but find many firms are willing to register when offered money to do so, although formalizing does not seem to benefit the performance of most of these firms. Alcázar et al. (2010) and Jaramillo (2009) offered firms in Lima, Peru, information and reimbursement of direct costs in order to encourage municipal registration (which was separate from federal tax registration), and find approximately one-quarter of those treated registered. This larger impact is consistent with municipal registration imposing fewer costs on firms than tax registration, and potentially with municipal enforcement being higher.[2] Our paper builds on these studies by offering information and free cost “carrots” in a context where simplification has recently occurred but in which registration costs and complexity still remain much higher than in the Sri Lankan and Peruvian cases, and by also testing simultaneously the role of “sticks” in the form of inspections.

Other related literature looks at the impact that formalizing has on firms. In addition to the experimental work of de Mel et al (2012), there are several non-experimental studies to examine this question. Fajnzylber et al. (2011) and Monteiro and Assunção (2011) both analyze the impact of the SIMPLES program on firms, finding that firms created after the reform invest more, were larger and more likely to operate in a permanent location than firms created just before the reform. However, it is unclear how much of this is an impact of formalizing versus a difference in the selection of which firms formalize. McKenzie and Sakho (2010) use an instrumental variables strategy in Bolivia based on distance to tax offices 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. Taken together these studies imply that many informal firms would not benefit from becoming formal, and are thus consistent with our results that information and reducing the costs of formalizing are not enough to induce formalization.

The remainder of the paper is structured as follows: Section 2 describes the process of becoming formal as a small firm in Belo Horizonte, the information firms have about the process of formalizing, and what firms see as the costs and benefits of becoming formal. Section 3 describes our interventions and Section 4 outlines the data used to evaluate their impact and the experimental design.

[2] Early results from an information-only intervention designed to encourage registration in Bangladesh also find a zero effect of information (De Giorgi and Rahman, 2013).

Section 5 provides the results of the interventions on the rate of formalization among informal firms, and Section 6 concludes with discussion of implications for both policy and further studies in this area.

2. Context and the Process of Formalizing

Belo Horizonte is the capital city of the state of Minas Gerais in Brazil, and has a city population of almost 2.5 million, with 5.5 million in the official metropolitan area (the third largest in Brazil after São Paulo and Rio de Janeiro). A 2009 survey by the Brazilian statistical agency IBGE along with government records was used by SEBRAE, the government agency for supporting micro and small businesses, to estimate that Belo Horizonte has a total of 561,310 businesses, of which 402,744 were informal (72 percent).

2.1 Registering a microenterprise in Belo Horizonte

The Complementary Federal Law 123 defines micro-enterprises as firms with annual revenues up to R$ 360,000 (US$177,000)[3], provided they are not the subsidiary of another firm. Microenterprises which meet several other conditions – the key ones being they do not have a foreign owner or partner, they are not in certain sectors like financial services, consulting, alcohol or tobacco, or transportation – are eligible to register their businesses formally under a national simplified taxation system called SIMPLES. The SIMPLES regime combines several ongoing tax and contribution payments into a single payment (including employee taxes and ICMS, the State sales tax), but does not simplify the registration process itself. In addition, at the time of beginning our study, enterprises with one or fewer employees and which had R$36,000 or under in annual revenues could instead register as individual microentrepreneurs (MEIs). This was changed after our intervention had begun, with the eligibility threshold being raised to R$60,000 after a law change in September 2011.

The State government created a unit called Minas Fácil in June 2008, in order to simplify the registration process. Registration under this new system involves registering at the federal, state and municipal levels all through a single process (set out in Appendix 1). Many steps in the process are online, and the entire process is estimated to take 7 days for an average firm. The key documents obtained are federal tax registration, evidenced by obtaining a CNPJ (Cadastro Nacional de Pessoas Jurídicas) number; state level registration with the Chamber of Commerce (JUCEMG); and a municipal license (Alvará de Localização e Funcionamento, or ALF). The initial cost of registration is R$236 for a sole proprietorship and R$320 for a limited liability company.

[3] 1 US dollar is approximately 2 Reais during the period of our intervention.

The annual costs of being formal include a sanitary tax (R$53.40-106.76, depending on activity) and the TFLF, a municipal inspection tax (R$77.26) that firms need to pay within 30 days of formalizing and then again at the beginning of each year; and a revenue tax. The revenue tax is a flat rate of R$ 51.65 per month for individuals who qualify as a MEI, and otherwise under SIMPLES ranges from 4% to 8.21% depending on revenue level and industry[4]. In addition, it is mandatory for formal firms with two or more employees to use an accountant, who must prepare cash flow and accounting statements each month, ensure the firm makes the monthly tax payments, and each year submit a form to the federal tax authority. In Belo Horizonte the accountants charge on average R$300 per month for this service. Accountants are not required for MEIs with one or fewer workers and revenue under the MEI revenue threshold.

For the median firm in our baseline survey (described below), which reports having one worker and earning R$30,000 in annual revenues and R$1,200 in monthly profits in our baseline surveys, the annual costs of being registered would therefore be approximately R$750 or 5.2% of their annual income. For a firm at the 75th percentile of reported annual revenues (R$57,000) and monthly profits (R$2,000), with the median of 2 employees, annual costs would therefore be approximately R$6,580 since they would need an accountant, or 27.4% of annual income. This assumes formal firms report their full revenues. Our baseline survey asks firms what percentage of revenues they think other firms report, and the mean (median) is 46% (50%). Firms may also underreport workers, especially if this gets them at the margin where they qualify as an MEI. The annual cost of being formal is thus likely to be 5-25% of annual income, with many firms at the lower end of this range, depending on whether firms report all their revenues, and whether their reported revenue exceeds the threshold needed for an accountant or not.

2.2 Are firm owners well informed about the process of formalizing?

Our baseline survey reveals that most informal firms interviewed lack key information about the process of formalizing, that there is substantial heterogeneity in their beliefs, and that most think formalizing is more time-consuming and costly than it is in reality. Only 46 percent of firm owners claim that they know what is needed to register and only 19 percent knew that the Junta Comercial (Chamber of Commerce) is where firms need to go to register. The mean (median) time firm owners think it takes to register once all documents are provided is 51 days (30 days), whereas in practice the average time

[4] The first R$180,000 in annual revenue is taxed at 4% for firms in commerce, 4.5% for firms in industry, and 6% for firms in services. The next R$180,000 is taxed at 5.47% for firms in commerce, 5.97% for firms in industry, and 8.21% for firms in services. This tax includes a number of taxes including income tax, contributions to social security, and employer pension contributions.

taken is 7 to 9 days. Almost 30 percent have no idea of the cost of registering, and among those who estimate the cost, the mean estimate is R$1,304, and the 90th percentile of R$2,500 is more than 10 times the 10th percentile of estimated cost of R$200. As described above, the actual upfront cost of registering is only R$236, or R$366 if one includes the sanitary and municipal taxes due within 30 days of registering. The mean (median) estimated tax rate is 22 (20) percent, compared to the actual tax rate of 4 to 8 percent.

The baseline survey asked firm owners open-ended questions about what they see as the main benefits and costs of formalizing. The main benefits mentioned were being able to open a bank account in the business name (51%), a better reputation for the business (47%), reduced risk of being fined (44%), ability to get business loans (43%), and being able to sell to other firms which require registration (39%). Only 13 percent said they saw no advantages. The main disadvantages mentioned were the initial costs of registration (62%), having to pay taxes (58%), having to pay for an accountant (34%), and the process of registering taking a lot of time (32%).

The baseline survey also asked firms if they had received an inspection visit from various types of inspectors in the past year. Thirty-two percent of firms responding to the baseline survey had received a visit from the municipal inspector (typically just to check on whether they had paid for a sign outside), 5.5 percent from a state tax inspector, and 3.1 percent from a federal tax inspector. The other main form of inspection was sanitary inspections, which 20.1 percent had received. Only 2 percent report having paid a fine for being informal, with the mean (median) fine being R$2,340 (R$600), with the majority of these fines being made to the municipality. Note though that this data comes from surviving firms, and so may understate the rate of inspection and of fine receipt among all informal firms if many of the firms inspected or fined close down as a result.

3. Interventions

The context is thus one of pervasive informality, despite the introduction of the simplified taxation program SIMPLES and the efforts taken by the Minas Facil unit to streamline the registration process. Given this context, *Descomplicar*, a unit within the state government of Minas Gerais which has the mandate to simplify relations between citizens, firms and the state, worked with the World Bank to test various mechanisms that could be used to induce more firms to formalize under the existing system in place. The focus was on trying to target firms that fell under the eligibility criteria for SIMPLES, which at the time of design was for either revenues in the range R$36,000 to R$240,000, or having two or more workers if revenues were below this.[5]

The following three interventions were designed, along with a fourth, indirect treatment, and a process to test them experimentally (described in the next section).

3.1. Communication Treatment

Given the lack of information many firm owners have about the process, time, and costs of formalizing, the first intervention considered was an information treatment. An attractive and colorful brochure entitled "formalization of enterprises" (see Appendix 2 for example pages) was designed by professional marketing staff. This 18 page brochure included (i) information on the advantages and importance of formalizing, explaining benefits such as availability of lines of credit, ability to participate in tenders and public bids, increased credibility, and compliance with social obligations; (ii) the disadvantages of being informal, including risk of seizure of goods and application of fines, difficulty dealing with medium and large suppliers and customers, limited business growth prospects, inability to practice judicial recovery, and inability to get financing due to lack of accounting records and formal status; (iii) explaining how firms can tell if they qualify as a microenterprise; (iv) discussion of opportunities in business procurement and how simple it is to sell to the Government of Minas Gerais; (v) opportunities for lines of credit for small formal businesses through the state development bank; (vi) the importance of working with an accountant; (vii) how to calculate taxes; (viii) the 10 steps needed to register for SIMPLES (see appendix 1), and a telephone number firms could call for help.

Firms which were selected for this intervention were delivered this brochure in person by a trained interviewer from the survey company Sensus Pesquisa e Consultoria. Descomplicar staff trained these interviewers on the content of the brochures and to deliver an accompanying short speech explaining it. Firms which stated they were formal and could produce a federal taxpayer number (CNPJ) were not given the brochures, while those which stated they were formal but could not document this were still given the brochure.

3.2 Free cost Treatment

The second intervention combined the information brochure given in the communication treatment with an effort to eliminate as many of the costs of registering as possible. As part of this intervention, Descomplicar made an arrangement with its counterparts in the other agencies involved in registration for all registration fees to be waived for the firms selected for this pilot intervention – this

[5] Note the upper threshold was raised to R$360,000 after a law change in late September 2011, while at the same time the revenue threshold for MEI registration was raised from R$36,000 to R$60,000.

included waiving the JUCEMG registration fee and municipal license fees, as well as paying the first year's sanitary tax and municipal inspection fee that are due within 30 days of registering. The fees waived thus amounted to between R$366 and R$504 (US$183-250) depending on the type of firm. In addition, an arrangement was made with the local accountant's association, whereby 50 accountants would be available to provide one year of free accounting services to these firms, which has an effective value of R$3,600 given the prevailing cost of accounting services and the mandate for certain types of firms to use an accountant. Thus firms participating in this treatment who formalize through this offer would pay no initial registration fees, and the only cost of formalizing in the first year would be their SIMPLES taxes. This offer was again delivered in person to firm owners by trained enumerators, with a phone number of a government office firm owners could call for further information or help. Firm owners were given 90 days to take advantage of the offer.

3.3 Inspector Treatment

In addition to informing firm owners about how to register, and making it cheaper for them to do so, the main other instrument governments can use to influence the behavior of informal firms is enforcement. As evidenced by our baseline data, the most common source of enforcement comes from municipal inspectors, which is also the typical pattern in other countries. In countries where municipal registration is separate from tax registration, the result is that many firms tend to be registered with the municipality but not with other levels of government. In Minas Gerais, since the registration is a streamlined process requiring registration at all three levels, this raises the possibility that municipal inspection could also result in full formalization. Note that before Minas Fácil was introduced in 2005, municipal, state, and federal registration processes were not linked in Minas Gerais, so that it was possible for firms which registered pre-reform to have one type of registration, but not the others.

The Prefeitura de Belo Horizonte (PBH) is the authority in charge of municipal inspections within the city of Belo Horizonte. They have approximately 100 inspectors, divided across 9 semi-autonomous subregions, each with their own decision-making processes about which firms to inspect. Typically these municipal inspectors just check whether firms have a permit to display a sign outside the firm (*placa*).

In response to a complaint or other request, the inspectors also can conduct visits which request proof that a firm has a current municipal license (ALF), which expires every five years, as well as check on whether a firm has a CNPJ (tax registration). Firms which are lacking the ALF are then given a notification and given 30 to 45 days to formalize, after which the inspector comes back and if the firm is still lacking municipal registration, fines the firm owner (the fine amount depending on area of the premises)[6] and closes the firm.[7] If firms can prove they are in the process of registering, they can get more time. The inspectors do not have the power to fine firms for not being registered with the state or federal authorities, but can threaten to report un-registered firms to these authorities. In practice this usually does not occur. When a firm applies for a municipal license following the inspection, it should technically also receive the state and federal licenses since all three registration processes are now integrated.

The third intervention consisted of giving these inspectors a list of selected firms to receive a thorough inspection, going beyond the typical *placa* inspection to also request proof of the municipal license (ALF) and checking on whether the firm has a CNPJ.

3.4 Indirect Inspector Treatment

The cost effectiveness of inspection as a means of getting firms to formalize depends in part on whether there are spillover impacts from inspected to non-inspected firms. Our final treatment is therefore an indirect one, whereby a firm does not receive an inspection but firms very closely located to it do receive an inspection. The next section explains our experimental design to measure these spillovers.

4. Data and Experimental Design

In order to experimentally test these interventions, we first needed a sample of informal firms. However, since no recent sample frame of informal firms was available, we had to construct one through a listing exercise. The presence of the inspector treatment added a complication to this listing process for ethical and survey-response reasons. In particular, if a firm owner were interviewed about their formality status, it may not be considered ethical to then use this information to potentially assign an inspector to visit them. Even if it were considered ethical (since the government has a right to ask firm owners about their formality status, and also a right to conduct inspections), we were still concerned that individuals who were interviewed in a baseline survey and then received an inspection may be unwilling to respond to a follow-up. Therefore a listing stage was done which did not involve talking to the firm owner.

[6] Note these fines occur only if the owner fails to respond to the request to formalize after an inspector visit. There are no back taxes or fines for having operated informally before the first inspection visit.

[7] Closing the firm involves the inspector physically shutting the door of the firm, saying the firm is closed, and then coming back for three times to check the firm is still closed.

4.1. Listing Survey

We started with a list of all 2,563 census blocks in Belo Horizonte. Using information from official government lists of registration, as well as a list of partially informal firms (firms who had acquired a license to display a sign outside the firm, but had not registered for a municipal or tax license according to the databases), we found that the number of formal and informal firms were reasonably highly correlated across blocks (0.67), reflecting that some census blocks are residential neighborhoods with few firms and others have more firms. Based on this, we dropped census blocks with below the median number of formal firms (11 formal firms) since these were likely to be mostly residential. This dropped 1,236 blocks. We then also dropped blocks in the top 5 percent of formal firm density, since high density blocks indicated high rise buildings with mainly formal firms and in which surveyors would not be able to enter without permission. We then stratified the remaining 1,260 blocks by sub-district, and randomly selected 600 census blocks to be listed, along with substitutes to be used in case some of the census blocks did not contain any informal firms.

The survey firm Gauss Estatística & Mercado was then hired by the Minas Gerais government through a public procurement process to undertake this listing survey. Listing consisted of enumerators visiting every firm operating out of a fixed building in the census block. It excluded individuals operating informally on the street since our interest was in larger informal firms, and excluded transportation firms since the rules for formalizing are different for them. Enumerators recorded basic information about the firm that could be observed without talking to the firm owner – the full street address, the business sector, the "fantasy name" of the firm (the name on a sign outside the firm if they had one), whether or not the business had a sign, the approximate area in square meters of the premises, and the approximate number of employees in the business. A photo was also taken of the firm to aid in subsequent identification.

Through this process more than 10,000 firms were listed during January and February of 2011 (appendix 3 provides a timeline). They were then matched by Gauss against two databases of formal firms – a database from PBH of 140,628 firms with municipal registration, and a database from JUCEMG (the Chamber of Commerce) of 117,350 firms with state registration. This was used to eliminate the "definitely formal" firms, i.e. firms who appeared on both of these lists, giving a sample of 7,852 listed potentially informal firms in 574 census blocks. In terms of listed sector, 48% were in commerce, 45% in services, only 1% in manufacturing, and 6% undefined. The large number of firms to be matched in a short timeframe, requiring firms to be on both formal lists, and, possibly the inexperience of the survey firm in doing such a matching exercise, meant that, as we will see, a number of formally registered firms remained in this listed sample.

Since part of our design involves looking at spillovers within blocks we wanted to minimize the risk of spillovers across treatment blocks. To do this, we used the address of each listed firm and, from this, obtained the GPS latitude and longitude, and calculated the number of firms in other census blocks that lay within 100 meters of the listed firm in a straight-line. We then examined in detail the 239 census blocks in which at least 10 percent of the firms were close to at least one firm in another census block. We were most concerned with adjacent census blocks where firms on one side of the street that was a block boundary were in one block, and those on the other side of the street were in a different block. We then used an algorithm to reclassify these into new blocks[8], giving a total of 662 geographic blocks. Out of these blocks, 57 contained only one potentially informal firm each, and so we dropped these blocks to end up with a sample of 605 geographic blocks containing 7,795 firms.

4.2 Randomization into Treatments at the Geographic Block Level

We randomized these 605 geographic blocks into three groups: control blocks, communication blocks, and inspector blocks. Randomization was stratified by the 9 sub-districts in Belo Horizonte, and by whether the block had above or below the median firm density (measured by the number of listed informal firms divided by the area of the block).[9] This resulted in 201 blocks being chosen as control blocks, 202 inspector blocks, and 202 communication blocks (Table 1).

4.3 Baseline Survey of Control and Communication Blocks

All of the firms listed in the communication and control blocks were then targeted for a baseline survey which took place between May and August 2011 and was carried out by the same firm (Gauss) as had done the listing. Out of these 5,419 firms, 1,455 were found to be formal (through presentation of documents), in 832 cases the firm had closed and neighbors had said this was a permanent closure, in 871 cases the owner was unable to be contacted on three visits at different times and days, 699 cases the owner said they were too busy and/or refused outright to be interviewed, there were errors in records for 209 firms (such as them being listed twice), and 1,353 firms were interviewed. Thus the

[8] For each street which formed part of more than one block (144 streets in total), we calculated the median street number in each block. We then took the difference in median street numbers across blocks for each street, and for all blocks where this difference was smaller than 250 street numbers, combined the firms that were on the same street but in different blocks into a new block.

[9] Since firm density (area of the block) was not known for the blocks that we had reclassified to avoid having neighboring firms in different blocks, we had three strata within each subdistrict: above median density, below median density, and reclassified block.

interview rate was 25 percent of all listed firms, and 48 percent of non-formal, non-closed firms without listing data errors.

Firms which appear in the baseline survey are almost evenly split between commerce and services, with less than 5 percent in manufacturing. The most common types of firms were hairdressers/salons (20%), bars (14%), automobile mechanics (8%), clothing (4%), and grocery stores (4%), with a wide range of other types of firms such as restaurants, bookstores, photocopying, flower shops, laundromats, and dance and language schools. The average owner is 44 years old and has run the business for 8 years, with 37 percent of the owners being female, and 42 percent having completed high school education or higher. The mean firm has 1.3 employees (not including the owner), reports annual revenues of R$52,000 (US$26,000) and monthly profits of R$2,000 (US$1,000).

4.4 Randomization to Treatment Status at the Individual Level

Given that only one-quarter of the listed firms answered the baseline survey, it was decided to focus the communication treatments just on this subgroup, since there was little point in trying to provide information on the process of formalization to firms which were already formal, closed, or where the owner couldn't be found to talk to. We therefore randomly chose half of the firms which had responded to the baseline survey in each communication block to receive the communication treatment, and the other half to receive the free cost treatment. They would then be directly comparable to the firms in the control block which had answered the baseline survey. This gives a sample of 1,348 firms split up as 689 control firms, 331 communication firms and 328 free cost firms for use in evaluating the effectiveness of the communication and free cost treatments (Table 1).[10]

The first three columns of Table 2 show that randomization succeeded in generating comparable firms across the different treatment groups. The first column shows the control group mean, while the second and third columns show the coefficients on the communication and free cost assignment to treatment dummies in the following regression for firm *i* in geographic block *s*:

$$BaselineVariable_{i,s} = \alpha + \beta Communication_{i,s} + \gamma FreeCost_{i,s} + \sum \delta_s d_s + \varepsilon_{i,s} \quad (1)$$

Where the d_s are dummies for the 27 sub-district*firm density strata used in the block level randomization, and the standard errors are clustered at the block level. The first five rows show variables obtained from the listing survey, while the remainder show variables from the baseline survey. Out of 48 coefficients shown, 5 are significantly different from zero at the 10 percent level, and only one at the 5 percent level, which is in line with what would be expected by pure chance.

[10] Due to data coding issues, seven of the firms which answered the baseline were not assigned to the control, communication or free cost groups, while two firms which weren't in the baseline were still assigned to the communication treatment. We work with the 1,348 observations which were assigned for treatment or control.

In contrast, since the inspector block firms were not interviewed at baseline, randomization for this group required randomizing among all firms originally listed. Our original plan called for assigning half the firms listed in these blocks to receive the inspection treatment, and half to be indirect inspector firms. However, this was not feasible with the existing number of inspectors, and so we instead had to randomly choose only one-quarter to be inspector firms. This resulted in 577 firms getting assigned to the inspector treatment. The remaining firms in this block are then all indirectly inspected, but budget constraints required us to randomly choose 593 of the indirectly inspected firms as the sample we would work with for follow-up surveys. The right control group for these firms consists of listed firms in the control blocks. Recall we already have a sample of 689 firms in the control blocks which had answered the baseline survey. We therefore randomly choose an additional sample of 709 control block firms which had not answered the baseline survey, and assigned them also for follow-up. We then reweight the control group sample to take account of the fact that we have all firms which answered the baseline, and only a sample of those which didn't, in this group.

The first three columns of Table 3 then compare the inspector and indirectly inspected firms to this weighted sample of control firms, in terms of the characteristics available from the listing survey. Although this offers far fewer variables for checking balance than is possible for the communication and free cost treatments, the results are still reassuring, with only one out of ten coefficients being significant at the 10 percent level.

4.5 Follow-up surveys

A very short phone survey of firms selected for the communication and free cost treatments was carried out between April 10 and 18, 2012 by Sensus. The purpose of this survey was to follow-up with the firms that had received the communication or free cost content and see whether they had started the process of formalization, their intent to formalize, and whether the information in the brochure had been useful. It was also intended to serve as a last prompt to use the information and free cost offer. This survey was not given to firms that Sensus had not made the treatment offers to, and so was fielded to 464 firms, of which 367 responded (79 percent). In this survey 86 percent of the firms said they remembered receiving the information, and 48 percent had read the brochure after having had it explained in person. Of those who read the brochure, 48 percent said they had learned that there were more potential benefits of formalizing than they had known about, but 42 percent also said they had learned formalization involved more costs than they had known. 57 percent said they had learned the process of formalizing was simpler than they had thought, whereas 18 percent said they learned the process was more complicated than they had thought.

The full follow-up survey consisted of an in-person survey fielded by Sensus between July and September 2012. The target sample size was 3,227 firms consisting of: 328 free cost firms, 331 communication firms, 577 firms assigned to inspectors, 593 indirectly inspected firms, 689 firms in the control blocks which had completed the baseline, and 709 firms in the control blocks which had not completed the baseline (all the firms in the last six columns of Table 1). Three attempts on different days at different times were made to contact these firms. A total of 1802 (56%) of the targeted firms were interviewed, with a further 14 percent closed, 20 percent absent and 10 percent refusing to be interviewed.

To examine whether the attrition rate was significantly different between treatment and control groups we run the following intent-to-treat regression for the sample assigned to the free cost, communication, or control groups which answered the baseline:

$$Attrition_{i,s} = \alpha + \beta Communication_{i,s} + \gamma FreeCost_{i,s} + \sum \delta_s d_s + \varepsilon_{i,s} \qquad (2)$$

And the regression below on the sample listed and assigned to be in the inspector treatment, indirect inspector treatment and selected for follow-up, and control group (using weights as discussed above to account for the fact that the control group is made up of baseline responders plus a random sample of non-responders):

$$Attrition_{i,s} = \alpha + \beta Inspector_{i,s} + \gamma IndirectInspector_{i,s} + \sum \delta_s d_s + \varepsilon_{i,s} \qquad (3)$$

Table 4 shows the results. Panel A shows the results of estimating (2) for the communication versus control blocks, while Panel B shows the results of estimating (3) for the inspector versus control blocks. We see that there is no significant impact of any of the treatments on the likelihood of being interviewed in the follow-up survey, nor in the likelihood of being absent or refusing to participate. The mean response rate is higher in Panel A than Panel B, as is to be expected given that Panel A conditions on being interviewed at baseline. Nevertheless, the follow-up survey was able to interview 50 percent of the control group sample that could not be interviewed at baseline.

While there is no significant treatment impact on the levels of attrition in the follow-up survey, the concern is still that the selection of which firms attrit may differ with treatment status. Tables 2 and 3 examine this possibility by testing for equality in baseline characteristics for the sample interviewed in the follow-up survey. In Table 2 we see the means for the follow-up sample look similar in magnitude to those of the full sample answering the baseline, and we only reject equality of coefficients in 5 out of 48 cases at the 10 percent level, which is consistent with random chance. Table 3 looks at balance on the listing variables and the small number of follow-up survey variables which are likely to be time invariant.

We find only 3 out of 22 coefficients significant at the 10 percent level, and 1 at the 5 percent level, which is again consistent with random chance.

The presence of administrative data (detailed below) allows us to test further whether there is differential attrition in the follow-up survey by the key outcome of interest: whether or not a firm has formalized during our intervention window. We estimate the following regression for firms in the baseline survey assigned to the control, communication, or free cost groups:

$$InterviewedatFollowup_{i,s} = \alpha + \lambda_1 Formalized_{i,s} + \lambda_2 Formalized_{i,s} * Communication_{i,s} + \lambda_3 Formalized_{i,s} * FreeCost_{i,s} + \beta Communication_{i,s} + \gamma FreeCost_{i,s} + \sum \delta_s d_s + \varepsilon_{i,s} \quad (4)$$

Where *InterviewedatFollowup* is an indicator of whether the firm was interviewed in the follow-up survey, and *Formalized* is an indicator of whether they probably formalized during the intervention period according to our administrative data. Then λ_1 measures whether firms in the control group which formalize are more or less likely to answer the follow-up survey than firms which did not formalize, and λ_2 and λ_3 measure whether any differential survey response varies with treatment status. We estimate the analogous equation similarly for the inspector versus control block groups.

Table 5 reports the results. For the baseline sample (who had already been interviewed once before), we cannot reject that whether or not the control group answered the follow-up survey is unrelated to whether or not they formalized during their intervention window. Moreover, likewise we cannot reject that there is no differential survey response with formalization according to treatment status. In the sample used for testing the effects of inspections, we do find that firms in the control group are more likely to answer the follow-up survey if they formalized during the intervention period. Nonetheless, they are not differentially likely to do so if they are assigned to either of the inspector treatments relative to being assigned to the control.

Taken together these results suggest that both the level of attrition and which firms attrit are not affected by treatment status, and so in what follows we estimate treatment effects ignoring attrition, as well as using administrative data to measure the key formalization outcomes free of attrition.

4.6 Administrative Data

We received a list from JUCEMG of all firms which registered with the chamber of commerce in Belo Horizonte between October 25, 2011 (the day the communication treatment started) and September 19, 2012. This list contained the official business name, fantasy name, street address, phone number, and CNPJ of the business, and also contained registrations for MEI status as well as SIMPLES. In addition, we obtained a list from PBH of all firms registering for an ALF license during this same period.

We used a matching algorithm and manual checking to match this to the listing survey data, described in Appendix 4. For each formalization measure, we define "definite" matches as cases where there is sufficient data in both data sources to make it almost certain we are matching the same firm, and "definite or probable matches" to also include cases where it seems very likely to be the same firm, but less data are available for confirming the match. We also construct an overall measure of whether a firm has formalized which measures whether they are a definite or probable match for at least one of these three forms of formalization (MEI, SIMPLES, or ALF).

We cross-checked the accuracy of this matching procedure with two subsamples of data. The first was reports from the inspectors, who recorded the CNPJ and ALF of firms which registered between the inspectors' first visit (when they issued warnings to informal firms) and their second visit (where they came back to see whether firms had formalized or were taking steps towards doing so). Secondly, in November 2012 the survey firm attempted to re-contact firms that were in the follow-up survey and ask them for their CNPJ and ALF numbers and registration dates.[11] Both these checks revealed the matching process to be accurate at matching firms which had formalized – it was only a handful of cases where individuals had registered at different addresses that we had not detected.

5. Results

We begin by discussing implementation of the different interventions, and use our survey data to examine whether this resulted in changes in knowledge or in inspection frequencies. We then examine whether these interventions changed the rate of business closure, and then ultimately, the impact on different aspects of formalization. To estimate treatment effects, we estimate versions of equations (2) and (3) for different outcomes. Since baseline data is not available for the inspector block firms, we do not control for baseline levels of the outcome variable, although the results for the communication versus control blocks are robust to doing so.

A further issue to deal with is that we are examining a number of different outcomes. The tests of significance provided for these outcomes are appropriate if we are interested in a particular outcome, such as whether the communication treatment increases the likelihood of a firm having obtained a municipal license (ALF). However, when looking at the range of outcomes, we need to make adjustments for multiple hypothesis testing here. Two approaches are commonly used in the literature.

[11] The survey firm was only able to re-contact 1166 firms in doing this check (after accidentally inserting a skip pattern which skipped asking the ALF and CNPJ numbers for most firms in the full follow-up survey), and so given this large attrition rate we do not use this data for measuring the formalization outcome, but instead rely on administrative records, only using this data as a cross-check to ensure our administrative matching procedure is reliable.

The first is to aggregate outcomes into indices and test whether the overall impact of the treatment on a family of outcomes is different from zero (e.g. Kling and Liebman, 2004). We use this approach when considering formalization as an outcome, since it is natural to aggregate different types of permits into a single measure of whether a firm has obtained any permit. This approach is less useful when we are interested in the individual outcomes themselves, and so the alternative is to adjust the p-values used to test each individual null hypothesis. We use the Benjamini and Hochberg (1995) correction which controls the false discovery rate within families of outcomes. Fink et al. (2012) provide more detail on this method and the need for its use in analysis of development experiments.

5.1 Implementation of Interventions

The communication treatment took place beginning on October 26, 2011, with up to four attempts made to deliver the brochure to the owner. This resulted in 208 of the 331 firm owners assigned to this treatment receiving the brochure (63%). A further 100 firm owners declared themselves to be formal and so were not given the brochure, only 2 owners outright refused the brochure, while the remainder were either unable to be found or absent.

The free cost treatment took place in February 2012, once arrangements had been finalized with both the government agencies that would waive the costs, and with the accountants' association. We realized that many of the firms claiming to be formal in the communication treatment may have possessed one document (either a federal, state for municipal registration), but not have SIMPLES, i.e. all types of required registrations, and so the instructions were to give the offer to all firms without SIMPLES, explaining it was also valid to take them from partially formal to fully formal. As a result, this offer was delivered to 255 out of the 328 firms assigned to this treatment (78%). Take-up of the offer was incredibly low: one month after the offer our partner government agency and hotline had received just 5 calls and 2 visits; and three months after the offer, only 10 to 15 people had called and one had started the formalization process. Ultimately only one firm in this treatment group took-up the offer to formalize and use one of the free accountants.

The inspector treatment began in December 2011 and lasted through April 2012. Of the 577 firms identified for inspection, the inspectors said they were able to locate 530, of which 387 firms were open. Among these 387 firms, 170 were found to have a municipal license, although some of these had expired and others were using more space than licensed for or had other infractions. The inspectors notified 269 firms that they were operating without the proper licenses. The inspectors then reported that in their follow-up visits, 143 firms were closed and 88 firms were in the process of formalizing. Their final report to us showed 17 of the notified firms had now produced a valid municipal license, 4 a license as a microentrepreneur, and the rest were still classified as in process.

5.2 Impacts on Knowledge and Inspection Likelihood

The first part of Table 6 examines whether the treatments changed the knowledge individuals have about the process of formalization. We obtain these treatment impacts through estimating equations (2) and (3), with different knowledge measures as the outcome of interest. We see first that approximately 60 percent of the control group firms claim to know the steps required to fully register a business, and that none of the treatments has a statistically significant impact on this outcome. This high rate of self-assessed knowledge contrasts dramatically with objective measures of knowledge – firm owners were asked the cost of registering and the tax rate faced by firms that register. As in the baseline survey, knowledge of these is very low, with only 3 percent of the control group giving an answer in the right range for the cost, and only 4 percent in the right range for knowing the tax rate. The communication and free cost treatments have no significant impact on this, whilst the marginally significant impacts of the inspector treatment on knowledge of the tax rate, and of the indirect inspector treatment on knowledge of the cost of registration are not significant once a correction is made for multiple hypothesis testing.

We find stronger impacts on whether individuals claim to use an accountant (a requirement of being formal for most firms) and whether they know the cost of an accountant. The free cost treatment raises the likelihood of claiming to use an accountant by 11.6 percentage points (p=0.014) and the likelihood of knowing the cost of an accountant by 11.8 percentage points (p=0.0002). The communication treatment has a similar magnitude impact on claiming to use an accountant (p=0.022). However, applying the Benjamini-Hochberg (1995) false discovery correction procedure to the 10 information and knowledge estimates results in only finding a significant impact of the free cost treatment on knowing how much an accountant costs. Firms assigned to the inspector treatment are also significantly more likely to know the cost of the accountant, even after controlling for false discoveries. Taken together, these results suggest little impact of the treatments on precise details of the formalization process like costs and tax rates, but that firm owners did gain information about the role and cost of accountants in this process.

The second part of Table 6 examines a second family of outcomes relating to inspector visits. Approximately 47 percent of firms in the control blocks report having been visited by a municipal inspector in the past year. This increases by 13.5 percentage points for the firms assigned to the inspector treatment (p=0.0002), which is consistent with the inspector treatment increasing the likelihood of getting an inspection. Note however that the combination of firms being closed or unable to be located by the inspectors, coupled with the fact that some would have received an inspection anyway, means that this difference between treatment and control groups is much less than 100 percentage points. The inspector treatment firms are marginally more likely to say they got information on how to formalize from an inspector (not significant after adjustment for multiple testing), and are no more likely to be notified or fined than control firms.

The indirectly inspected firms are no more likely to report having seen or heard that a neighboring firm had received an inspection in the past 12 months. This may result from the fact that inspections occur to some extent anyway, and that firm owners do not always communicate amongst each other – 35 percent of firm owners say they don't talk at all to other firm owners about business matters. Additional support for the view that many firms just don't notice inspections in neighboring firms comes from the fact that twice as many firms report having been inspected themselves than report having seen a neighboring firm inspected. These findings imply that we should expect the spillover impact from inspecting the inspector group firms on the indirectly inspected firms to be minimal in terms of formalization.

5.3 Impacts on Firm Survival

La Porta and Shleifer (2008) note that a competing view to the De Soto/Doing Business view of the informal economy as home to potentially productive entrepreneurs held back by regulatory barriers is the dual economy view associated with Tokman (1992) and Rauch (1991). Under this view, the informal sector is a source of subsistence livelihoods for individuals with relatively low levels of human capital, and any increase in firm value that these owners would be able to generate by formalizing would not be large enough to offset the additional costs of taxes and other regulatory requirements. The result would be that enforcing formalization may cause these firms to shut down, since they cannot afford to operate formally.

Table 4 examines this possibility by looking at the impact of the different treatments on firm closure. Firm closure is measured by whether the firm is observed to be closed at the time of the follow-up survey, coupled with information obtained from neighboring businesses. Between 14 and 16 percent of the control group is verified as closed at follow-up, with none of the treatments having any sizeable or significant impact on this closure rate. In particular, it is not the case that firms which received enforcement through the inspector treatment are more likely to have closed down.

5.4 Impact on Formalization

Table 7 turns to the main outcome of interest, whether or not the treatments succeeded in getting firms to formalize. We use the administrative data to measure formalization, since this offers impacts without attrition, and offers substantially larger samples to measure impacts, offering the most power.[12] The first few columns compare the control firms to the free cost and communication firms. We see that assigning firms to the free cost treatment have a strongly significant negative impact on the likelihood of MEI registration (p=0.008 for definite matches, p=0.010 for probable matches). One possible explanation for this is that the free cost and communication interventions explained how to register for SIMPLES, but not how to register as a MEI. The free cost treatment also emphasized the need to have an accountant if you formalize, which is not required for MEIs. The unexpected policy change right after we launched this intervention increased the revenue thresholds under which firms could register as MEIs. It is plausible that firms receiving the communication and free cost intervention were less aware of this policy change given the information about the need to register for SIMPLES and get an accountant that they were given in person, causing them to decide not to register at all. The point estimates are also negative for the communication treatment, and testing for equality of treatment effects, we cannot reject that the communication only treatment has the same negative impact as the free cost – but neither can we reject a zero treatment effect for communication only.

We see no significant impacts of the free cost or communication treatments on registering for SIMPLES or obtaining an ALF license. Pooling together all the three measures[13], we see that 8-10 percent of the control group that was interviewed at baseline got some form of formal status after our interventions began, and that this was approximately 3 percentage points lower for the free cost group. Given the size of the free cost group, this equates to approximately 10 firms not formalizing that would have in the absence of this intervention.

Turning to the last three columns, which consider the inspector versus control block comparisons, we see that assignment to the inspector treatment group leads to a strongly significant 2 to 3 percentage point increase in the likelihood of obtaining an ALF. Recall that the ALF license is the

[12] A last minute change in question placement by the survey firm led to a skip pattern skipping the detailed formalization questions for many firms in the follow-up survey. An attempt to re-contact these firms to obtain this extra information only obtained this data for 71 percent of the follow-up survey sample, with this response unbalanced by treatment status. Since the follow-up survey already had relatively high attrition, the end result is that we only have survey measures of formalization for 35 to 50 percent of the assigned sample, depending on treatment group. We used the data collected to cross-check the administrative matching process, but otherwise do not use this data.

[13] Recall that since the formalization measures naturally aggregate, we consider impacts on this aggregate to deal with concerns with multiple hypothesis testing.

only one the municipal inspectors are legally able to enforce. Under the one-stop shop for registration, we expected that firms would register and obtain SIMPLES and an ALF all at once. However, if firms had already obtained a CNPJ, or if they had previously had an ALF which had expired (they are valid for 5 years), firms could just register and get an ALF only. It therefore appears that this extra formalization was by firms that were already partially formal. There are small and insignificant impacts of inspections on the other forms of formalization, and the overall impact of formalizing is thus coming from the ALF registrations. We find a significant overall impact of between 2 and 4 percentage points - which is equivalent to between 11 and 22 extra firms formalizing out of the 577 assigned to the inspector group.

In contrast, we find a rather precise zero effect of the indirect inspector treatment on the likelihood of formalizing. The 95 percent confidence for the treatment effect on "definitely or probably got any type of formal status" is [-0.020, +0.013]. This is consistent with the evidence in Table 6 that the firms in this treatment group did not notice any increase in inspections of neighboring firms.

5.5 IV Estimates of the Impact of Being Inspected

Whilst we find a significant impact of being assigned to receive an inspector visit on obtaining an ALF license, the effect of 3 percentage points is very small. However, there are several reasons for this small effect: many of the firms were closed or could not be found by the inspectors, some firms were already formal, and some firms would have already been inspected anyway. To estimate the causal impact of being inspected on formality, we therefore run the following instrumental variables regression:

$$Formalize_{i,s} = \alpha + \beta ReceiveMunicipalInspection_{i,s} + \sum \delta_s d_s + \varepsilon_{i,s} \qquad (5)$$

Where we instrument the follow-up survey report of whether the firm had received a municipal inspection in the past year with assignment to the inspector treatment group. We estimate this equation using the follow-up survey data for the control group and inspector group only. We consider both ALF registration, which is the registration form most closely tied to municipal inspection, as well as our overall measure of formalizing.

Table 8 displays the results. We see the point estimates range from 0.214 to 0.265, so that receiving an inspection results in a 21 to 27 percentage point increase in the likelihood of formalizing. The statistical significance is greatest for ALF registration, where the p-value is 0.108 for definite registration, and 0.051 for definite or probable registration. This is the impact on the group of firms who answer the follow-up survey. It therefore removes firms which had closed or which can't be found easily, but still includes some firms which were actually already formally registered. Our estimate is thus a lower bound for the impact that inspections would have on formalization of informal firms.

The estimated cost of an inspection is R$64.34, which is based on an estimated inspection taking 56 minutes per visit plus 17 minutes of travel time (estimates provided by PBH). The inspectors visited 387 firms (the rest being closed or not found), so the total cost of inspections is estimated at R$24,900.Taking our estimated impact of 11 to 22 more firms formalizing, the cost per firm formalized is R$1132-2264. Our IV estimates suggest that since many of these inspections would have occurred anyway, approximately four additional inspections are required to get one firm to register for an ALF license, so the estimated cost of formalizing one firm is approximately R$256.

Annual tax revenue is R$620 for a MEI, and based on 4 percent revenue tax on the average revenue of R$57,000 for newly formalized firms with an ALF, is R$2280. Firms report that firms like theirs typically only report only half their revenues, which would take the SIMPLES annual tax take down to R$1140. So the cost of formalizing a firm in our experiment via inspections appears that it would be gained back within the first year of tax payments (or more than gained back if we consider only the marginal visits under the IV estimation), with subsequent years of tax payments then a net gain for the government.

However, while total government revenues appear to warrant bringing firms into the formal system via inspections, the municipality (paying for the municipal inspectors) only gets a share of the revenue, with the remainder going to the state and federal governments. Apart from the annual inspection tax, under SIMPLES municipalities only directly get tax revenue from service firms[14]: one component of the SIMPLES tax, called the ISS, is 2% of revenues on the first R$180,000 of revenues, and 2.79% after that. So the municipality would gain approximately R$570 per year from formalizing a service firm. On this basis, it still appears it would be worth the municipalities attempting more inspections to formalize more service firms of the size in our sample, particularly if this inspection effort can be targeted at firms that wouldn't otherwise be inspected.

5.6 Comparing Actual Impacts to Expectations of Treatment Impacts

A standard question with impact evaluations is whether they deliver new knowledge or merely formally confirm the beliefs that policymakers already have (Groh et al, 2012). In order to measure whether the results differ from what was anticipated, in January 2012 (before any results were known) we elicited the expectations of the Descomplicar team as to what they thought the impacts of the

[14] Municipalities also gets part of the taxes that go to federal and state governments back indirectly through transfers from these levels of government back to the municipality, but we ignore this indirect component here which is based on a complicated revenue-sharing procedure that doesn't just depend on municipal tax takes.

different treatments would be. Their team expected that 4 percent of the control group would register for SIMPLES between the baseline and follow-up surveys. We see from Table 7 that this is an overestimate of the SIMPLES registration rate, but given the change in MEI requirements, in line with the combined SIMPLES and MEI registration level.

They then expected the communication only group to double this rate, so that 8 percent would register, that the free cost treatment would lead to 15 percent registering, and that the inspector treatment would lead to 25 percent registering. They did not expect there to be any indirect inspector effect, and so expected only 4 percent of the untreated firms in the inspector blocks would register. The zero or negative impacts of the communication and free cost treatments therefore are a surprise. The overall impact of the inspector treatment is much lower than expected, but is in line with the IV estimates, suggesting the Descomplicar team have a reasonable sense of what to expect when an inspection actually occurs, but may have overestimated the amount of new inspections that would take place. Their expectation of a lack of impact for the indirect inspector treatment was also accurate.

5.7 Impacts on Trust and Attitudes Towards Government

De Mel et al. (2012) offer Sri Lankan firms monetary payments to get them to formalize, and find that one outcome of formalization is that firms have more trust in local government. In their case formalization is much cheaper and quicker than firms had believed, and so they note that one possible reason for this increase in trust was that firms experienced better services from the government than they had expected, while an alternative could be that they were less afraid of being shut down after registering.

Our follow-up survey asked firms about their trust and views of government. Individuals were asked on scale of 1 to 10 how much they trust different actors, where 10 denoted most trust and 1 least trust; as well as a question on whether they believe government acts in the interests of the people or in its own interest. Table 9 reports the results of estimating the impacts of our treatment assignments on these outcomes. We see a strong contrast to the results in Sri Lanka: the attempts at formalization in Belo Horizonte appear to have generally worsened trust in government. The results for the communication and free cost treatments all remain significant after controlling the false discovery rate at α=0.10, while the impact of the free cost treatment on believing the government acts in its own interests is also still significant when controlling the false discovery rate at α=0.05.

The impacts are not that large – a reduction of 0.3 to 0.5 points on a 10-point trust scale, which represents a 0.1 to 0.15 change; and an increase of 4 to 10 percentage points in the likelihood firm owners think the government acts in its own interest rather than in the interests of the people. Nevertheless, in an environment of widespread informality, efforts to reach out to particular firms and bring them into the formal system by either carrots or sticks may run the risk of increasing distrust in government if firm owners do not see any benefits from being brought into this formal system. The distrust effect is more significant for information and free cost efforts than for the inspection treatment – possibly because a government initiative that individually invited firms to register is different from usual activities and appears to have aroused suspicion.

6. Conclusion

Despite reforms which make it faster and simpler for informal firms in Brazil to register, the majority of firms remain informal. While simply paying firms to formalize has been found to be have big impacts on formalization rates in Sri Lanka, this is unlikely to be on the policy menu for most governments. Instead there are a range of carrots and sticks that governments can use to attempt to bring firms into the formal sector. Our experiment tests some of the most common ones – informing firms, making it cheaper for them to register, and increasing enforcement of rules. Our findings suggest sticks rather than carrots seem more effective at getting firms to formalize, but also show limits to this approach.

The process of registering in Belo Horizonte still requires more steps and complications than in a number of other countries that have pursued entry reforms. Moreover, in addition to facing taxes, firms which do register face a relatively large cost in terms of the need to hire an accountant. Faced with these costs of *being* formal, it appears few informal firms want to formalize unless they are forced to do so by enforcement. We are unable to measure whether firms benefit from being forced to formalize, since the number of firms induced to formalize is too small, and our follow-up survey also suffered from high item non-response on sales and profits questions. But evidence from other countries (McKenzie and Sakho, 2010; de Mel et al, 2012) suggests that while some informal firms benefit from formalizing, the majority appear not to. Being informal is thus likely to be privately optimal for many firms.

This suggests three directions for government policy. The first is to re-consider where it is desirable to even try and attempt to bring these firms into the formal sector. It may not make much sense for the smallest firms, but given the limited tax base and that firm owners with revenues in the range which qualifies for SIMPLES are likely to be at least in the middle of the income distribution, there may be a public benefit to formalizing these firms even if there is no private benefit. Our results suggest the fiscal benefits of bringing service firms of the average size in our study into the formal sector would outweigh the costs of the inspections needed to make this happen. The second avenue for policy is then to simplify further the ease of formalizing, and more importantly, to revisit the need for an accountant which dramatically increases the cost of being formal. Efforts to link formality with access to government programs and bank financing might help induce some firms to register, but many firms will not benefit from such approaches. Improved enforcement is thus the third part of policy efforts- our research shows enforcement can induce formalization, but that there are limits. Rather than having separate inspectors for different forms of registration, having municipal inspectors able to enforce municipal, state, and federal registration should have stronger impacts. And given that many of the firms the inspectors said they had closed down were open again at the time of our follow-up survey, there appears to be scope for improving the degree of enforcement that inspection actually entails. Combining enforcement with carrots may offer the greatest impact, since firms may be far more receptive to information and lower costs of registering when they have an enforcement incentive to register.

If enforcement appears promising as a way to bring larger informal firms into the formal sector, our study also shows the difficulties for researchers of evaluating such efforts. It does not seem ethically possible for researchers to survey firms about their formality status and then use this information to determine whether they are selected for enforcement. The approach pursued in this paper of listing firms without surveying their owners provides one approach, but at the cost of including some already formal firms and some closed firms or hard to interview firms in the study. One alternative would be to conduct a baseline survey to identify which firms are informal and willing to be interviewed, and then for the government to randomly assign all geographic blocks throughout the city to treatment or control status without reference to this baseline information. A downside of this approach is that it would make it harder to measure treatment spillovers, since one could not assign firms to different treatment status within blocks, although potentially the use of GPS coordinates (Gibson and McKenzie, 2007) on each firm could allow some measurement of spillovers.

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Appendix 1: Registration Process in Belo Horizonte

A firm wishing to formally register must undertake the following steps:

Step 1: Access the Chamber of Commerce (JUCEMG) website (www.jucemg.mg.gov.br) and fill out a Consulta de Viabilidade (feasibility consultation). The main purpose of this step is to find out if the desired business name is available and if they can open their firm at the desired address. This step will also show the licenses needed and the costs involved, based on the main activity of the business.

Step 2: Once a positive response is received, access the Receita Federal do Brasil - RFB (Federal tax service) website (https://www14.receita.fazenda.gov.br/cadsincnac/inicioAction.do) - CadSinc (Synchronized Database) and fill out the Coleta WEB. CadSinc brings together the cadastral (land ownership) procedures of agencies and entities involved in the process of formalization and legalization of companies.

Step 3: After receiving the response from step 2, print the DBE (Document Basic Input).

Step 4: Go to the website of JUCEMG (www.jucemg.mg.gov.br) and click on Portal de Serviços (Serviços WEB/Integrador). Click on the link Integrador and then the link Gerar Novo FCN/REMP. This step will integrate the Consulta de Viabilidade data with the CadSinc data and generate the documents that will be the base of the Contrato Social (state registration for limited liability companies (LLC)) or REMP (state registration for individuals).

Step 5: Click on DAE and print it. The DAE is the document with the amount the entrepreneur should pay to continue the formalization process. All the steps up to now are online and free of charge. If the firm is a LLC the entrepreneur will pay R$ 165.53 and if the firm is a sole proprietorship the entrepreneur will pay R$ 82.11.

Step 6: Deliver all documents to the Minas Fácil office in Belo Horizonte, including a document evidencing the payment of the DAE, in person. In this step JUCEMG will check and validate all documentation delivered.

Step 7: Look on the website of JUCEMG (www.jucemg.mg.gov.br) for a decision on the application for registration of the firm under the link Consulta de Protocolo. If the request was approved, go to the Minas Fácil office to pick up 2 authenticated copies of the REMP or Contrato Social and the micro or small enterprise declaration, in person.

Step 8: Go to the RFB (IRS) website to print the CNPJ (federal tax registration), and to get the Inscrição Estadual (State Tax Registration) and Inscrição Municipal (Municipal Tax Registration).

Step 9: Go to the municipal website (http://www.pbh.gov.br) to get the Alvará de Localização e Funcionamento (Municipal License). This costs R$ 154.60.

Step 10: Go to the JUCEMG website, under the link **Consulta de Protocolo**, to consult the need for inclusion in other entities, as well as licenses. Some businesses will also need a license from the Fire Department, the Health Division and/or the Environment Division.

Appendix 2: Example pages from Communication Treatment

Formalização de empresas

As vantagens e a importância da formalização de empresas

O primeiro impulso do novo empreendedor geralmente é manter o negócio na informalidade. Isso se deve muitas vezes ao desconhecimento do processo de formalização ou às dificuldades com a burocracia, impostos e outros encargos.

No entanto, o que muita gente não sabe é que a informalidade pode dificultar o crescimento da empresa. Enquanto isso, a legalidade facilita a credibilidade do negócio, o relacionamento com clientes e fornecedores, a adequação dos controles internos e o aumento da produção, das vendas e do lucro.

O registro é o caminho para se formalizar uma empresa. É por meio da inscrição do empreendedor nos órgãos competentes que seu negócio obtém uma "certidão de nascimento". Uma vez legalizada, a empresa passa a usufruir de muitos benefícios:

- Disponibilidade de linha de crédito em instituições financeiras públicas ou privadas;
- Possibilidade de expansão no mercado interno e também em outros países;

Appendix 3: Timeline

January-February 2011: Listing Survey Carried out

May-August 2011: Baseline Survey of firms in Control and Communication blocks

October 26, 2011-February 2012: Firms in Communication groups given information brochures, and those in free cost group also get free cost offer

December 2011-April 2012: Inspectors visit firms selected for Inspection treatment

April 2012: Follow-up survey by phone of firms in communication and free cost treatments

July-September 2012: Final round in-person follow-up survey.

Appendix 4: Matching Listing Data to the Administrative Data

In order to ensure that any selective response to surveys was not affecting our ability to match a firm to the registration data, we use only information contained in the listing data to match firms. The listing data contained the street address of the firm, the fantasy name of the firm if it had one, the neighborhood (barrio), the activity the firm undertook, and in cases where a phone number was visible from outside the firm, a phone number. The corresponding information in the administrative data differed slightly for the three different types of registration (SIMPLES, MEI, and ALF), so we use slightly different criteria for each one.

Matching SIMPLES

Firms registering for SIMPLES provided (i) their street name and number, (ii) their fantasy name, and (iii) their activity, as well as a phone number (which could be that of their accountant or a personal phone number instead of the business phone number). We therefore matched on (i) to (iii), using the phone number where available in both data sets as an additional check. We consider it to be a **definite match** if the listing data matches the administrative data on all three criteria, or if it matches on street and number plus activity and there is no fantasy name, or if (ii) and (iii) match and the street is the same and street number is only different by one digit. We consider it a **definite or probable match** if it is a definite match, or if it matches on street and number and the listing omits the fantasy name and activity, or if it matches on two criteria but the third is different. In this case, we only consider a match on fantasy name if it is unique and not generally descriptive (e.g. we would not consider the fantasy name "bar" as grounds for a match) and there is only one occurrence of that fantasy name in the entire administrative data.

Matching MEIs

Firms which register as a MEI do not provide a fantasy name in their registration, so the matching could only be on street name and number, and activity (and sometimes phone number). A **definite match** matched on both these. A **definite or probable match** is a definite match, or any one of the following: a match on street name and number, with a very similar activity (e.g. "bar" and "comercio varejista de bebidas"); an exact match on street name and number, with no name or activity data in the listing; or a match on phone number, street name, and activity, with a different street number.

Matching ALF registrations

The ALF administrative data also does not contain a fantasy name, so the same criteria were used as for matching MEIs.

Table 1: Treatment Assignment

	Number of Blocks	Number of Firms Listed	Firms Surveyed at Baseline	Randomization Among Baseline Firms			Random Sample of Listed firms not in Baseline		
				Control	Communication	Free Cost	Control	Inspector	Indirect Inspector
Control Block	201	2810	689	689	0	0	709	0	0
Communication Block	202	2609	659	0	331	328	0	0	0
Inspector Block	202	2376	0	0	0	0	0	577	593

Table 2: Confirming Randomization for Control Vs Communication Blocks

	Full Sample in Baseline			Sample Interviewed at Follow-up		
	Control Mean	Free Cost Difference	Communication Difference	Control Mean	Free Cost Difference	Communication Difference
Listing Variables						
In commerce	0.46	0.0254	-0.0316	0.45	0.0424	-0.0648
In services	0.50	-0.0108	0.0199	0.51	-0.0112	0.0584
Has a sign outside	0.34	-0.0104	0.0102	0.35	-0.0244	0.0355
Num. Employees	0.99	0.0511	-0.0533	1.05	0.0766	-0.166*
Area (square meters)	43.8	7.293	1.886	41.7	7.735	7.765
Baseline Variables						
Owner is Female	0.37	-0.00542	0.0307	0.37	-0.0386	0.01000
Owner's Age	44.3	0.172	-0.175	44.3	-0.422	-0.558
Owner has primary or lower education	0.17	0.00315	0.00641	0.17	0.0171	0.00751
Owner has completed high school	0.42	-0.0319	-0.00110	0.42	-0.0356	0.0365
Owner is married	0.58	-0.00277	-0.0291	0.60	-0.0353	0.0103
Age of Business (Years)	7.82	0.309	0.150	8.18	-0.255	0.112
Hours owner works per week	54.1	-1.565	-1.244	54.6	-1.162	-2.767*
Total Number of Employees	1.14	0.154	0.201	1.19	0.200	0.239
Keeps Business Records	0.32	0.0574*	0.0728**	0.31	0.00181	0.0675
Annual Revenue (Reais)	48595	3,637	12,299	48411	6,002	21,644
Monthly Profits (Reais)	1797	159.3	824.3*	1879	107.5	1,264*
Owned Capital Stock (Reais)	46319	8,243	13,486	48746	12,875	31,059
Claims to have SIMPLES but no proof	0.15	0.0132	0.00256	0.15	-0.00697	-0.0189
Claims to be registered with JUCEMG	0.13	-0.00379	-0.00268	0.12	-0.0216	0.00295
Claims to have a CNPJ	0.23	-0.0164	-0.0108	0.24	-0.0757*	-0.0281
Claims to have an ALF	0.24	0.0419	0.0515	0.26	-0.000496	0.0504
Visited by municipal inspector in past year	0.33	-0.0539*	-0.0272	0.36	-0.0592	-0.0288
Visited by state inspector in past year	0.06	-0.00700	-0.0287*	0.06	-0.00870	-0.0374*
Visited by federal tax inspector in past year	0.03	0.00692	0.00380	0.03	0.00147	0.00595
Sample Size	689	328	331	436	192	198

Notes: *, **, and *** indicate statistically different from control mean at the 10, 5 and 1% levels respectively, after controlling for randomization strata and clustering at the block level.

Table 3: Confirming Randomization for Control Vs Inspector Blocks

	Full Sample in Listing			Sample Interviewed at Follow-up		
	Control Mean	Inspector Assigned Difference	Control in Inspector Block Difference	Control Mean	Inspector Assigned Difference	Indirectly Inspected Difference
Listing Variables						
In commerce	0.47	0.0230	-0.000767	0.46	0.0288	0.00567
In services	0.45	-0.0248	-0.00635	0.47	-0.0278	-0.0162
Has a sign outside	0.39	0.0382	0.0407	0.40	-0.00622	0.0324
Num. Employees	1.09	-0.107	0.191*	1.24	-0.192*	0.171
Area (square meters)	60.4	3.932	0.682	58.6	1.208	-7.665
Time-invariant Variables from Follow-up Survey						
Owner is Female				0.42	-0.00978	0.00941
Owner's Age				42.4	0.162	0.855
Owner has primary or lower education				0.09	-0.0104	0.00498
Owner has completed high school				0.49	0.0271	0.0851**
Father of owner has at most primary				0.32	-0.0579*	0.00551
Father of owner completed high school				0.14	0.0125	-0.00858
Sample Size	1398	577	593	788	329	295

Notes: *, **, and *** indicate statistically different from control mean at the 10, 5 and 1% levels respectively, after controlling for randomization strata and clustering at the block level. Sampling weights used to account for uneven sampling probabilities.

Table 4: Attrition rates and Business Closure Impacts by Treatment Group

Panel A: Control Vs Communication Blocks

	Control Mean	Free Cost Difference	Communication Difference
Interviewed at Follow-up	0.636	-0.0428	-0.0274
		(0.0396)	(0.0353)
Closed at Follow-up	0.142	0.0139	0.0297
		(0.0268)	(0.0277)
Refused or Absent at Follow-up	0.238	0.0321	0.00105
		(0.0332)	(0.0274)
Sample Size	685	329	328

Panel B: Control Vs Inspector Blocks

	Control Mean	Inspector Assigned Difference	Indirectly Inspected Difference
Interviewed at Follow-up	0.537	0.0354	-0.0332
		(0.0247)	(0.0278)
Closed at Follow-up	0.166	-0.0184	-0.00850
		(0.0188)	(0.0209)
Refused or Absent at Follow-up	0.316	-0.0241	0.0326
		(0.0253)	(0.0257)
Sample Size	1383	577	593

Notes: Standard errors in parentheses, clustered at the block level. *, **, and *** indicate statistically different from control mean at the 10, 5 and 1 percent levels respectively, after controlling for randomization strata. Sampling weights are used in Panel B to account for uneven sampling probabilities.

Table 5: Was there differential attrition by formalization action?

Dependent Variable: Interviewed in Endline Survey

	(1)	(2)
Probably formalized in intervention window	0.0115	0.152***
	(0.0557)	(0.0553)
Formalized*Assigned to Free Cost Treatment	-0.0928	
	(0.108)	
Formalized*Assigned to Communication Treatment	-0.0512	
	(0.120)	
Formalized*Assigned to Get Inspector		-0.0489
		(0.0831)
Formalized*Assigned to Indirect Inspector		0.0172
		(0.0914)
Sample Size	1,341	2,547

Notes: Robust standard errors in parentheses, clustered at block level.
*, **, and *** indicate significance at the 10, 5 and 1 percent levels respectively.
Regressions also control for randomization strata, and for treatment group assignment and are for the sample chosen to attempt to survey at follow-up
Sampling weights used in column (2).

Table 6: Impacts on Knowledge about Formalization and Inspections

	Communication vs Control Blocks				Inspector vs Control Blocks			
	Sample Size	Control Mean	Free Cost Difference	Communication Difference	Sample Size	Control Mean	Inspector Assigned Difference	Indirectly Inspected Difference
Information and Knowledge								
Claims to know procedures for formalizing	817	0.59	0.00415	0.0303	1,393	0.62	-0.00871	0.0458
			(0.0432)	(0.0380)			(0.0349)	(0.0318)
Knows cost of registration is between R$200 and R$400	817	0.03	-0.0111	0.00734	1,393	0.03	0.00170	-0.0197*
			(0.0117)	(0.0159)			(0.0130)	(0.0103)
Knows tax rate if registered is between 4% and 8%	817	0.04	0.0213	0.0120	1,393	0.04	0.0272*	0.00866
			(0.0201)	(0.0184)			(0.0164)	(0.0150)
Claims to use an accountant	817	0.27	0.116**	0.108**	1,393	0.48	0.0658*	-0.00847
			(0.0470)	(0.0470)			(0.0348)	(0.0391)
Knows cost of an accountant is R$200-R$400/month	817	0.10	**0.118*****	0.0406	1,393	0.18	**0.0907*****	0.0562*
			(0.0312)	(0.0300)			(0.0312)	(0.0307)
Inspections								
Municipal inspector visit in past year	817	0.42	0.0346	0.0709	1,393	0.47	**0.135*****	0.0465
			(0.0467)	(0.0438)			(0.0367)	(0.0392)
Received information on how to formalize from any inspector	817	0.14	-0.0404	-0.00387	1,393	0.12	0.0517*	0.00272
			(0.0262)	(0.0318)			(0.0275)	(0.0267)
Reports a neighboring firm inspected in past year	817	0.19	0.00429	-0.0231	1,393	0.19	0.000869	0.0355
			(0.0370)	(0.0372)			(0.0295)	(0.0326)
Was notified or fined by an inspector in past year	817	0.10	-0.0223	-0.0292	1,393	0.10	0.0324	0.0306
			(0.0264)	(0.0257)			(0.0248)	(0.0245)

Notes: Standard errors in parentheses, clustered at the block level. *, **, and *** indicate statistically different from control mean at the 10, 5 and 1 percent levels respectively, after controlling for randomization strata. Sampling weights are used for the Inspector vs Control blocks comparisons.
Coefficients in **bold** remain significant applying the Benjamini-Hochberg (1995) procedure within a family of outcomes to control false discoveries.

Table 7: Impacts on Formality

	Communication vs Control Blocks				Inspector vs Control Blocks			
	Sample Size	Control Mean	Free Cost Difference	Communication Difference	Sample Size	Control Mean	Inspector Assigned Difference	Indirectly Inspected Difference
Administrative data measures of formalizing after interventions began								
Definite match for SIMPLES	1346	0.007	0.00618	-0.00300	5186	0.006	0.00390	-0.00144
			(0.00662)	(0.00446)			(0.00443)	(0.00229)
Definite or probable match for SIMPLES	1346	0.015	0.00177	-0.00706	5186	0.014	0.00421	0.00102
			(0.00773)	(0.00761)			(0.00586)	(0.00384)
Definite match for MEI	1346	0.060	-0.0349***	-0.0155	5186	0.026	0.00313	-0.00557
			(0.0131)	(0.0139)			(0.00826)	(0.00469)
Definite or probable match for MEI	1346	0.067	-0.0370***	-0.0143	5186	0.033	0.0138	-0.00339
			(0.0142)	(0.0161)			(0.0106)	(0.00561)
Definite match for ALF	1346	0.030	0.00311	0.000227	5186	0.032	0.0218**	-0.00540
			(0.0113)	(0.0117)			(0.0110)	(0.00535)
Definite or probable match for ALF	1346	0.041	-0.000335	-0.00597	5186	0.041	0.0327***	0.00206
			(0.0125)	(0.0125)			(0.0124)	(0.00653)
Definitely got any type of formal status	1346	0.083	-0.0281*	-0.0120	5186	0.056	0.0245*	-0.0100
			(0.0159)	(0.0174)			(0.130)	(0.0068)
Definitely or probably got any type of formal status	1346	0.104	-0.0350*	-0.0182	5186	0.075	0.0392***	-0.0034
			(0.0183)	(0.0209)			(0.0149)	(0.0083)

Notes: Standard errors in parentheses, clustered at the block level. *, **, and *** indicate statistically different from control mean at the 10, 5 and 1 percent levels respectively, after controlling for randomization strata. Sampling weights are used for the Inspector vs Control blocks comparisons.

Table 8: Instrumental Variable Estimates of the Impact of an ALF inspection on Formalization
Dependent variables all for formalizing after intervention started

	Definitely Got ALF	Definitely or Probably got ALF	Definitely Formalized	Definitely or Probably Formalized
Reports receiving an ALF inspection	0.204	0.265*	0.214	0.222
	(0.127)	(0.136)	(0.148)	(0.157)
Observations	1,100	1,100	1,100	1,100

Notes: Robust standard errors in parentheses, clustered at block level.
*, **, and *** indicate significance at the 10, 5 and 1 percent levels respectively.
Regressions also control for randomization strata, and are only for the control group and firms assigned to receive inspectors. Assignment to receive an inspector is used as an instrument for receiving an inspection. Table 6 provides this first stage.

Table 9: Impacts on Trust and Views of Government

	Communication vs Control Blocks				Inspector vs Control Blocks			
	Sample Size	Control Mean	Free Cost Difference	Communication Difference	Sample Size	Control Mean	Inspector Assigned Difference	Indirectly Inspected Difference
Trust state Governor	805	4.89	-0.304	**-0.602****	1,374	4.81	-0.376*	-0.524**
			(0.296)	(0.288)			(0.223)	(0.238)
Trust state officials	799	4.28	**-0.584****	**-0.566***	1,362	4.00	0.124	-0.0686
			(0.297)	(0.300)			(0.229)	(0.218)
Trust state and municipal inspectors	793	4.43	-0.263	**-0.585****	1,360	4.23	0.0695	-0.167
			(0.280)	(0.233)			(0.236)	(0.208)
Believe people in govt. act in own interests	733	0.77	**0.106*****	**0.0848****	1,238	0.80	0.0569**	0.0400
rather than interests of the people			(0.0355)	(0.0346)			(0.0282)	(0.0292)

Notes: Standard errors in parentheses, clustered at the block level. *, **, and *** indicate statistically different from control mean at the 10, 5 and 1 percent levels respectively, after controlling for randomization strata. Sampling weights are used for the Inspector vs Control blocks comparisons.

Coefficients in **bold** remain significant applying the Benjamini-Hochberg (1995) procedure within a family of outcomes to control false discoveries.