What Drives the Effect of Labor Inspections on Formal Labor? The Roles of Information and Punishment

Miguel N. Foguel (IPEA)
Carlos Henrique Corseuil (IPEA)

Resumo

Neste artigo procuramos estimar o impacto de novas ações da inspeção do trabalho visando reduzir o trabalho informal no Brasil. Essas ações combinam um componente de esclarecimento e outro de punição aos empregadores de municípios de pequeno e médio porte no país. Exploramos o fato de a intensidade do componente de punição variar entre municípios para estimar impactos heterogêneos do programa. Os resultados indicam que contratações que regularizam trabalhadores sem registro tendem a aumentar quando a intensidade do componente de punição aumenta. Mas para níveis relativamente baixos deste componente nossas estimativas apontam impactos nulos do programa.

Abstract

This paper aims to evaluate the impact of new actions in labor inspections implemented in Brazil that seek to reduce labor informality. These new actions combines an awareness and a punishment component that are targeted to small and medium municipalities. We explore the fact that the punishment component has different intensities across treated municipalities to identify heterogeneous impacts of the program. We show that hires that regularizes previous unregistered employees increase more in municipalities exposed to higher intensities of the punishment component. But for relatively low levels of this component, our estimates indicate null effects of the intervention.

Palavras chave: inspeção do trabalho, emprego formal.
Keywords: labor inspection, formal employment.
JEL Code: J88, J46, H83.
Área: 13 - Economia do Trabalho

1 Introduction

Labor informality is a widespread phenomenon in developing countries. The share of informally hired workers can be as high as 60% in Latin American countries or even higher in South Asian countries.[1] On one hand, there is wide consensus (see, e.g., Perry et al., 2007) that labor informality can be harmful for workers, the productivity of countries, and government revenues. On the other hand, there is much less consensus on the relative effectiveness of alternative policy instruments to reduce labor informality.

One traditional policy instrument is labor inspections. Broad effects of this instrument have been evaluated by previous studies (e.g., Almeida and Carneiro, 2012, Almeida et al., 2015, Abras et al., 2018). However, knowledge on how labor inspections affects labor informality faces three main obstacles. First, labor inspection practices usually target multiple goals such as formal labor hiring, respect to minimum wage provisions, workplace safety as well as compliance with many items of the labor code. This multiplicity of goals makes it difficult to measure the specific effect of labor inspections on labor informality alone. Second, standard procedures of labor inspections combines distinct components including information campaigns, inspections focused on detecting informality at the extensive and intensive margins, and distance inspections as well as face-to-face inspections. Third, little is known about how different intensities of these components effectively affect labor informality. Few studies attempt to disentangle the relative importance of these distinct features of labor inspections to tackle labor informality.

We contribute to the literature by evaluating a large scale labor inspection program in Brazil - Plano Nacional de Combate à Informalidade dos Trabalhadores Empregados (Plancite) - that was specially targeted to reduce labor informality in small and medium municipalities (SMM) in the country. Apart from having this specific target, the program combines two distinct components. One is the provision of information to through letters and e-mails to employers and a local media campaign on the negative consequences of informality. We refer to this as the *awareness* component. The other component is an increase in the frequency of face-to-face inspections in already tax-registered establishments. This will be referred to as the *punishment* component.

The awareness component of the intervention may impact both formal and informal firms, as the media campaign was implemented in the local radios and newspapers. Though the punishment component is focused on

[1] The figures come from ILOSTAT database (https://www.ilo.org/ilostat) which provides harmonized statistics on informal labor.

formal firms, informal firms may also be impacted as face-to-face inspections in SMM are more likely to be noticed by other firms, including the informal ones. Thus, these two components of Plancite can act on both the intensive and the extensive margins of labor informality (Ulyssea, 2018), that is, the program may affect the share of informal workers in formal firms as well as the share of formal firms. Though we do not attempt to identify the effects of the program on each margin, it is worth keeping in mind that both margins may have been affected by the program.

The program, launched in 2014, was targeted only to municipalities with less than 100 thousand inhabitants and a subset of these municipalities was chosen to receive the intervention. Ideally, we would like to identify the effect of each component separately. However, as both components were always present in the implementation of the intervention, we followed a two-way route. First, we exploit the eligibility cutoff of the program and use a regression discontinuity design (RDD) to identify the program's effect near the population cutoff. Given the local nature of this identification strategy, we are only able to identify the effect of the awareness component for the intensity of the punishment component that prevailed near the cutoff. But for municipalities with population size away from the cutoff the intensity of the punishment component varied. We thus leave aside the RDD framework and instead use a difference-in-differences (DiD) strategy to identify the effect of both components for different intensities of the punishment component.

In sum, we seek the identification of the program's effects associated to the awareness component interacted with different intensities of the punishment component. We believe this makes an important contribution to the literature as previous analysis focus on the punishment component alone and do not explore the potential heterogeneous effects of different intensities of this component.

In fact, our results point that the effectiveness of the program depends on the intensity of the punishment component. On one hand, we show null impacts in the three different outcomes related to formal employment for relatively low intensity of the punishment component. On the other hand, the intervention seems to increase the formalization of unregistered employees for higher levels of the punishment component.

Apart from this Introduction, the paper contains another four sections. In the second section, we provide background information about the program and present the data sources and the variables used in the empirical analysis. The third section is dedicated to discuss the identification strategies and the estimation procedures we use to estimate the program's effects. In the fourth section, we present the results and in the fifth a summary of the findings and conclusions.

2 Background

2.1 The Intervention

Labor inspections in Brazil, as elsewhere, have the mandate to check compliance with the labor code. In Brazil, the labor code is extremely detailed, so labor inspection efforts pulverizes across several dimensions such as the existence unregistered labor relations, misconducts on paid vacations, payment for extra hours, lack of contributions for workers mandatory saving accounts, as well as compliance with many safety conditions. Typically, labor inspections take place either following anonymous requests or through planned actions by labor authorities. On both cases, the bulk of inspections is not focused on labor informality but on these other items.

Within this context, Plancite can be considered the first supply driven initiative of the Brazilian labor authorities specifically targeted to unregistered labor relations, or labor informality. The program, launched in May 2014, originally envisaged multiple components including improvement and homogenization of the information systems, presentations and specific training for inspectors and their supervisors, inspection efforts directed to specific industries with higher informality rates, information to employers, and face-to-face inspections focused on detecting unregistered workers in firms.

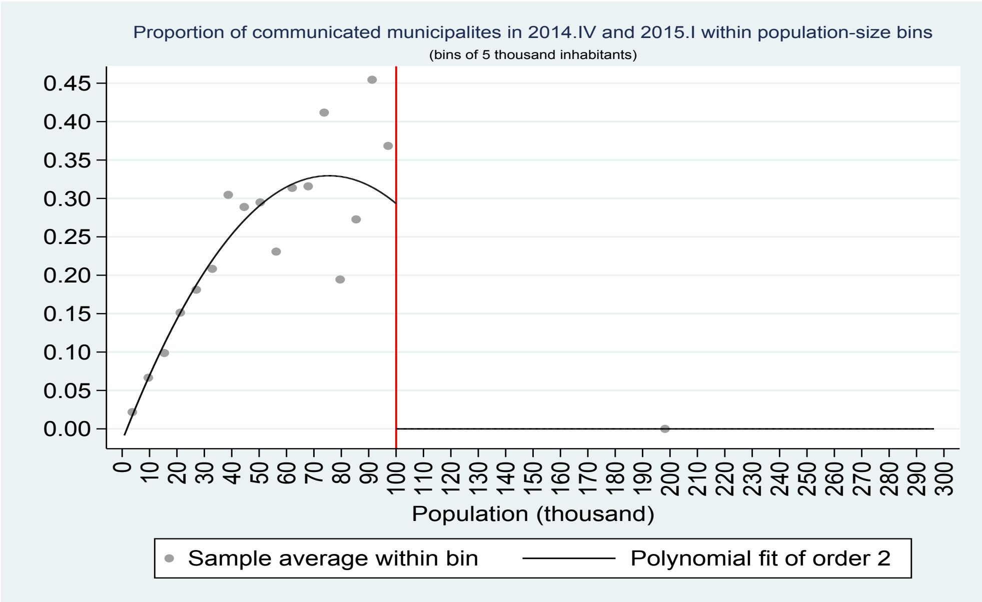
We study the effects of one of the first actions within Plancite that took place between the last quarter of 2014 and the first quarter of 2015. It was restricted to reach municipalities with less than 100 thousand inhabitants and consisted of two of the previously mentioned components: the provision of information on the negative consequences of labor informality and face-to-face inspections.

The first of these components included two different actions. First, letters (or e-mails) were sent to firms or to firms' accountants informing on the potential penalties of unregistered labor relations (including the possibility of losing access to public programs) and emphasizing the importance of registration for the workers (for instance, access to the pension system or to unemployment insurance). The second action consisted on a media campaign in local radios and newspapers divulging the same message of the letters. The second action complements the first one in the sense that it aims circulating the information not only to employers but to other relevant social actors (including the workers themselves) so as to decrease their complacent behavior regarding labor informality. We name this component of the program the *awareness* component.

The other component, which will be referred to as the *punishment* component, is an increase in the frequency of face-to-face inspections in establishments located in the municipalities that received the awareness component. Only tax-registered establishments were visited by labor inspectors, whose focus should be directed to compliance with registration of all labor relations within the establishments.

In practice, this initiative was implemented in two stages. First, some municipalities with less then 100 thousand inhabitants (according to the 2010 Census) were selected to receive the first component - that is, the sending of the letters and emails to the local employers and the media campaign. Figure 1 shows the proportion of municipalities receiving the awareness component according to municipality size, measured by the population in 2010 census. As it can be seen, the 100 thousand inhabitants threshold was bidding as probability of the awareness campaign drops from around 30% to zero as soon as the threshold is crossed from the left.

Figure 1: Probability of Receiving the Awareness Component



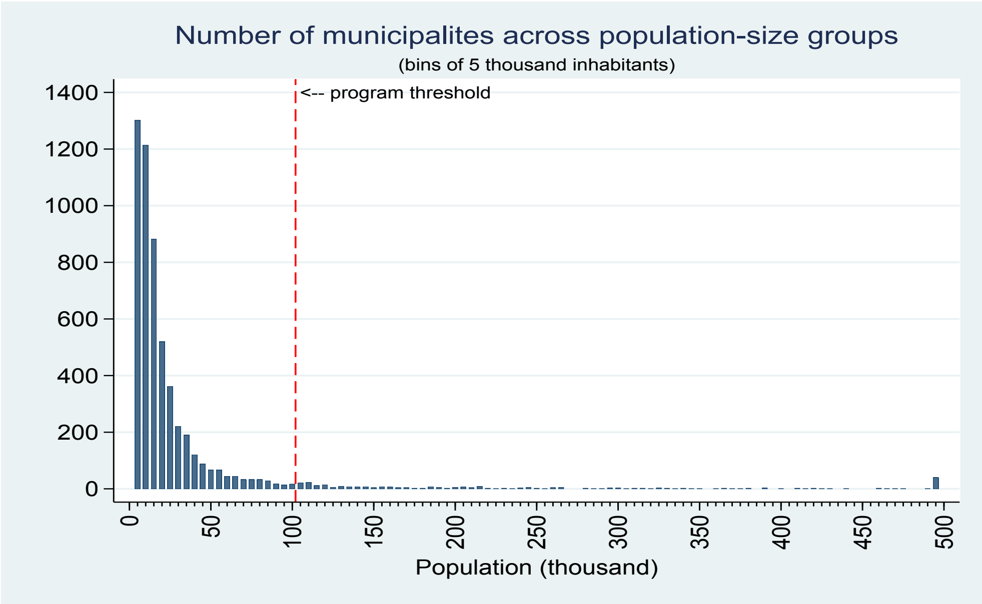
Source: SIT and IBGE.

The low values of the probability of the awareness campaign for the smallest municipalities worth a comment. It is not driven by a small number of treated municipalities but rather by the very high number of municipalities of this size, as can be seen in Figure 2.[2] Figure 2 also helps illustrating that

[2]This heavy tail distribution for population size across municipalities is a stylized fact across countries, as shown for instance in Gabaix, 2009.

there is no bunching of municipalities around either side of the 100 thousands inhabitants threshold.

Figure 2: Population Size Distribution

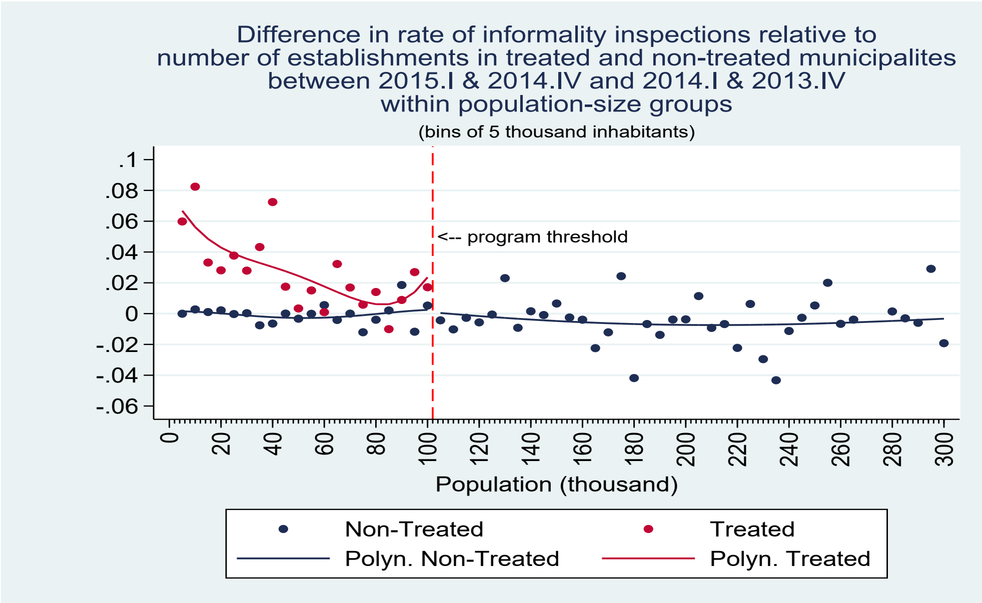


Source: IBGE - 2010 Census.

Following the launching of the awareness campaign, the treatment group for the punishment component was selected, non-randomly, in a second stage as a subset of municipalities within those treated by the awareness component. This selection targeted municipalities with relative higher informality rates. As a result, relatively smaller municipalities were more likely to be visited, as attested by Figure 3, which presents the mean intensity of the punishment component across municipalities. It is important to point out that the increase in the rate of inspections for the treated municipalities near the program's population cutoff was much lower than for smaller municipalities.

Another important point to notice in Figure 3 is that the average values of the punishment component to the right of the 100 thousand threshold are virtually zero. Together with the results shown in Figure 1, this means that the municipalities to the right of the program cutoff were not treated neither by the awareness nor by the punishment component. It is also important to note that untreated municipalities to left of the cutoff did not experience an increase in the intensity of the punishment component.

Figure 3: Intensity of the Punishment Component



Source: SIT and IBGE.

2.2 Outcomes and Data

A natural prediction to be tested is that labor inspections should increase flows into formal employment. That was the objective of the policy makers when they launched the program. This prediction, however, needs more careful discussion. In principle, the risk of non-compliance becomes higher under Plancite. This raises the expected costs of labor, which in theory should decrease labor demand for formal workers. Employers have then to decide how to adjust to these new conditions. Those that already hired informal workers may prefer to regularize the labor status of their workforce since hiring costs have already been incurred, there is higher knowledge about the quality of workers, and training investments may have been made. But the rise in expected labor costs should lead at least a portion of employers either to decrease new formal hires or to increase new formal separations (or both). Less productive or less "risk averse" employers may instead decide to increase their labor demand for informal workers or move entirely to informality.

The data we use allow us to measure two types of hires. The first is the total number of formal hires that take place after the official period of 30 days that employers have to inform the Labor Ministry. This measure is more connected to the formalization of informal workers alluded to above and is typically used by labor inspection managers as a monitoring indicator for the efficacy of their actions to reduce labor informality. The other measure is the total number of hires irrespective of when they were informed to the Labor Ministry. This measure thus includes both new formal hires and the formalization of old hires. Both hiring measures are used to construct some outcomes variables of interest. We also use the total number of formal separations informed by employers to construct another outcome variable.

We use data from four different sources and all share the same municipality code. First, the department of labor inspection in Brazil (SIT) provided us with information related to both components of the treatment to be evaluated. Specifically, they provided the list of all municipalities selected for the program's awareness component as well as a data set with the total number of labor inspections that took place between 2012 and 2015 across all municipalities in Brazil. These pieces of information are used to construct our treatment variables.

The second data source is the Cadastro Geral de Emprego e Desemprego (CAGED), which is an administrative file maintained by the Brazilian Federal Government with records for all formal hiring and separations that take place every month in the country. The information comes from declarations from all establishments that either hire or fire workers at any moment in time. Establishments have 30 days to send the information. After this period, they have to pay a fine and send the information as a regularizing hire. We use the the former to measure new hires and the latter to measure old hires. We also measure the total number of separations. These measures are used to construct our outcome variables.

The third data source is the 2010 demographic Census conducted by the Brazilian Census Bureau (IBGE). It provides the population size for each municipality and also the information necessary to compute the share of informal employment by municipality.

Finally, the fourth source of data is the Relação Anual de Informações Sociais (RAIS), which is another administrative file maintained by the Brazilian Federal Government with annual information on establishments and workers for the whole country. We aggregate the information at municipality level and use RAIS for two purposes. First, we use it to compute the number of establishments at the municipal level that will serve as the denominator of all rates that are used for measuring the outcome variables and some treatment variables. The second usage of RAIS is to construct a set of control variables that are used in some of our estimation procedures. Table 1 lists these variables and compare the average values for treated and non-treated municipalities with population size close to 100 thousand threshold. As it can be seen, there are systematic differences in observable characteristics across the two groups of municipalities. Such differences are suggestive of the presence of non-observable characteristics that may confound the identification of the treatment effects of interest. The next section is dedicated to discuss our identification strategies.

Table 1: Descriptive Statistics by Treated and Non-Treated Municipalities

Covariates	Treatment	Control	Difference
High school	0,618	0,624	-0.006*
Young	0,078	0,083	-0.006***
Elderly	0,024	0,027	-0.003***
Commerce	0,469	0,438	0.031***
Services	0,289	0,318	-0.029***
Industry	0,102	0,116	-0.014***
Construction	0,042	0,053	-0.010***
Public	0,004	0,003	0.001***
Others	0,093	0,072	0.022***
North Region	0,192	0,073	0.119***
Northeast Region	0,385	0,215	0.170***
Southeast Region	0,308	0,440	-0.132***
South Region	0,000	0,220	-0.220***
Central-West Region	0,115	0,052	0.063***

Notes: Figures in the first two columns correspond to means of variables.

3 Empirical Strategy

3.1 Identification and Estimation at the Cutoff

As discussed above, Figures 1 and 3 show that the probability of being treated jumps as population size crosses the 100 thousand cutoff. This discontinuity in the probability of being treated can be exploited to (locally) identify the combined effect of the awareness component at the prevailing level of the punishment component in the municipalities with population close to 100 thousand inhabitants. This can be accomplished by using the Regression Discontinuity Design (RDD) strategy. On one hand, as the probability of treatment does not jump from 1 to 0, we are not in the case of the Sharp RDD. On the other hand, the probability stays at 0 to the right of the cutoff, which makes our setting compatible with the partially fuzzy design (Batistin and Rettore, 2008), also known as one-sided compliance.

In such setting, there is no always takers and hence the strategy employed to identify the effect only for compliers in the (fully) Fuzzy RDD settings is capable to identify the effect for treated units. In other words, the average treatment on the treated (ATT) parameter can be identified through the same strategy employed to identify local average treatment effect (LATE) parameter in (fully) Fuzzy RDD settings.

Moreover, Batistin and Rettore (2008) show that such ATT identification in partially fuzzy settings is achieved under the same relative mild conditions required in Sharp RDD settings. Nonetheless, it is worth emphasizing that the identification is still local in the sense that it holds only for the treated municipalities near the 100 thousand cutoff.

Taking c to represent the program's cutoff (100 thousand inhabitants) of the forcing variable (X), our parameter of interest (τ^c) can be identified as:

$$\tau^c = \frac{E[Y|X=c^-] - E[Y|X=c^+]}{Pr[T=1|X=c^-]} \tag{1}$$

where c^- and c^+ denotes the corresponding limits of X to the left and to the right of c, Y is an outcome of interest, and T represents the treatment dummy.

We perform estimation based on the following regression model:

$$Y_m = \alpha + T_m\beta + f(X_m - c) + \epsilon_m, \tag{2}$$

where m indexes municipalities, $f(.)$ is specified as a polynomial function, and ϵ is a mean-zero disturbance term. The parameter of interest is β.

Estimation is based on the robust bias-corrected estimator proposed for Fuzzy RDD by Calonico et al., 2014, and further enhanced by Calonico et al., 2019 to allow the inclusion of covariates in the regression model. The implementation of this estimator requires some decisions concerning the alternative methods for the selection of the bandwidth size, variance estimation, choices for the order of the polynomial function $f(.)$, and shape of kernel function. Our benchmark specification uses the bandwidth size computed according to the minimization method for the mean square error (MSE) proposed by Calonico et al., 2014, an heteroskedasticity-robust plug-in variance estimator, the linear function for $f(.)$, and the triangular kernel. We also computed results using alternative choices for the first three dimensions. Some of these supplementary results will be discussed latter while others were left to an appendix that can be obtained from the authors upon request .

The RDD strategy is widely used for being able to reach a clean identification in settings like ours. Nevertheless, we take two further precautions to avoid the influence of potential confounding factors. First, we apply a first difference transformation to our dependent variables. This aims to remove any systematic imbalance of non-observables that are fixed in time.[3] The second precaution consists of using otherwise relevant covariates, which can also improve the precision of our estimates.

3.2 Identification and Estimation of Heterogeneous Effects of the Punishment Component

In the previous section we discussed the identification of the Plancite's impact for a given level of the punishment component, namely the level prevailing in the treated municipalities near the 100 thousand cutoff. In this section, we discuss the identification of the program's effect as we change the intensity of the punishment component. For this, we leave aside the RDD framework and use a difference-in-differences (DiD) design.

We want to identify the effects of heterogeneous levels of the punishment component for municipalities that received both the awareness and the punishment components. To define different levels of the latter component, we compare groups of treated municipalities with different intensities of labor inspections. Thus, for the DiD strategy both treatment and control groups are formed by municipalities that received the awareness component and treatment is defined by higher and lower levels of the punishment component.

To implement the DiD strategy we use quarterly data for our dependent variables from the first quarter of 2012 to the first quarter of 2015. As is standard practice in DiD models, we define dummies for the post-treatment period and for the treated units. The post-treatment dummy assumes value 1 only when the observation refers either to the fourth quarter of 2014 (2014.4) or the first quarter of 2015 (2015.1). As mentioned above, the treatment dummy is defined in different ways so as to capture the heterogeneity in punishment intensity. To be more precise, we define it as $T_m^l = 1(0)$ if municipality m was treated and the rate of inspections in that municipality was above (below) a certain level l. To define these levels we use the distribution of the rate of inspections in treated municipalities in 2014.4 and 2015.1. Based on the percentiles of this distribution we define three levels for the punishment component heterogeneity: the 60^{th} percentile, the 75^{th} percentile, and the 90^{th} percentile.

Let $m = 1, ..., M$ index municipalities and $t = 2012.1, ..., 2015.1$ the time periods (in quarters) available in the data. Denoting the level of the outcome variables by y_{mt}, our DiD model can be specified in the following way:

[3] A similar approach was employed by Lemieux and Milligan, 2008.

$$y_{mt} = \alpha + T^l_m\gamma + Post_{mt}\delta + (T^l_m * Post_{mt})\beta + \theta_m t + \delta_{mq} + \lambda_\tau + \varepsilon_{mt}, \quad (3)$$

where T^l_m is the treatment dummy specified above, $Post_{mt}$ is the aforementioned post-treatment dummy, $\theta_m t$ is a municipality fixed effect interacted with a time trend, δ_{mq} is a municipality-by-quarter ($q = 1, 2, 3, 4$) fixed effect, λ_τ is a year fixed effect, and ε_{mt} is a disturbance term. Our main interest relies on the β parameter, which under some identification hypotheses will inform the differential effect of Plancite across municipalities with different levels of inspection rates.

The usual identification hypothesis in DiD models is that the evolution of the outcome variable of the control group is a good proxy for the evolution of the outcome variable of the treated group in the absence of treatment. In our case, this parallel evolution refers to what is left after taking into consideration the differential trends across municipalities captured by $\theta_m t$ and the within-year seasonal disparities across municipalities absorbed by δ_{mq}. The specification in equation (3) should be rich enough to control for any differential pre-program trends between the treatment and control groups. In the end of the section 4.2 we present the results of a placebo exercise that is intended to test the capacity of our specification to identify the program's effect of interest.

In practice, the model specified in equation (3) can be estimated through:

$$\Delta y_{mt} = Post_{mt}\delta + (T^l_m * Post_{mt})\beta + \theta_m + \lambda_\tau + \Delta\varepsilon_{mt}, \quad (4)$$

where Δ represents the first difference between two quarters separated by one year, that is, between t and $t - 4$. Note that both $Post_{mt}$ and λ_τ are immune to the first difference transformation.

4 Results

4.1 Results for Local Effect at the Cutoff

Table 2 reports the coefficients estimated for our benchmark specification presented in section 3.1. Results are organized in different columns according to the outcome variable of interest and the three rows inform the set of control variables introduced in the regression - namely, no covariates, only the municipal informality rate, and the full set of covariates of Table 1.

Estimates of the Plancite effect on regularizing hiring rates are shown in the first column of Table 2. As discussed in section 2.2, this variable is intended to capture formalization procedures for workers previously hired under informal contracts. Therefore, we should expect positive values for the coefficients in the first column. Though the point estimates across the three rows confirm this expectation only the one in the last row is statistically significant, and this is valid only at the 10% level. It seems then that Plancite was not capable to change the probability of registering informal employees at least for the combination of the awareness component and the intensity of the punishment component prevailing in municipalities near the program's population cutoff.

Results for overall formal hiring and separation rates are reported respectively in the second and third columns of Table 2. As it can be seen, none of the estimates are statistically significant at conventional levels. As for the case of regularizing hires, these results indicate that the intervention did not affect the overall formal labor flows near the population cutoff of the program.

Table 2: Local Effect on Municipalities with Population Size Close to the 100 Thousand Cutoff

Covariates	Regularizing Hiring Rate	Overall Hiring Rate	Overall Separation Rate
No	0.0133	0.00353	0.0262
	(0.00938)	(0.0182)	(0.0203)
Informality rate	0.0149	-0.00162	0.0350
	(0.0103)	(0.0161)	(0.0284)
Full set	0.0155*	0.00273	0.0254
	(0.00922)	(0.0153)	(0.0198)

Note: Estimates from equation (2). See text for definition of variables. Significance levels: *=10%; **=5%; ***=1%.

As mentioned in section 3.1, the implementation of our estimation procedure requires some choices regarding the functional form of the (local) regression function, the variance estimation procedure, and the objective function to be minimized to choose the optimal bandwidth size. In Table 3 we check the robustness of the estimates for the effect on the regularizing hiring rate to these choices for the specification that uses the full set of control variables. In the first column we reproduce the value estimated for our benchmark specification. In the next three columns we present estimates for the same parameter varying only one of the three dimensions listed above. In the second column, we just change the variance estimation procedure using a nearest neighborhood variance estimation method (NN). In the third column, the bandwidth size is based on the minimization of an alternative objective function that allows for faster coverage error decay rates (CER). Lastly, in the fourth column, we use a quadratic polynomial in the regression function.

Table 3: Effect on Regularizing Hires Under Alternative Specifications

	MSE OLP (1) Var.(HC0)	MSE OLP (1) Var.(NN)	CER OLP (1) Var.(HC0)	MSE OLP (2) Var.(HC0)
Regularing Hiring	0.0155*	0.0153	0.0167	0.0178
	(0.00922)	(0.00979)	(0.0110)	(0.0114)
Observations Left	127	133	43	178
Observations Right	81	81	55	89
Bandwidth	27.7612	28.8470	13.8452	36.3302

Note: Columns refer to different specifications explained in the text.
Significance levels: *=10%; **=5%; ***=1%.

The main point to be observed in Table 3 is that the marginally significant estimate in first column is not robust to the alternative specifications. Notice that this result comes from estimations conducted for different number of observations and bandwidth sizes, which are shown in the bottom part of the table. Analogous robustness exercises were conducted for the overall hiring and separation rates. For both cases, the estimates were not statistically significant for all alternative specifications. The bottom line of these robustness exercises is that Plancite did not seem to have increased the formalization of labor contracts at least for the configuration of the awareness and punishment components in the municipalities with population around 100 thousands inhabitants.

4.2 Heterogeneity Across Intensity Levels for the Punishment Component

It is important to bear in mind that the results presented in the previous subsection are specific to a certain configuration of the program, in particular to intensity of the punishment component. In this section, we check whether results vary as we consider different levels of this component.

To be more specific we estimate the effect for distinct levels of inspection rates across the municipalities that were treated by both components of the program. This is implemented using the difference-in-differences specification presented in section 3.2. Municipalities are grouped as being subjected to either higher or lower rates of inspection according to whether the municipal inspection rate was respectively above or below some specific percentiles (60^{th}, 75^{th}, and 90^{th}) of the distribution of inspection rates in treated municipalities in 2014.4 and 2015.1. The corresponding differential impact of Plancite according to each of these thresholds of the punishment component are reported in each row of Table 4.

One of the main results that comes out from this table is that the rate of regularizing hires increases relatively more for municipalities that were exposed to higher rates of inspections. Moreover, the effect size increases for higher thresholds that separate the municipalities: the effect for the 75^{th} threshold percentile is 30% higher than for the 30^{th}'s threshold and the differential impact between the 90^{th}'s and the 75^{th}'s thresholds is almost 70%. These results indicate that as the punishment component becomes more intense, the employers in small and medium size treated municipalities react accordingly formalizing higher shares of their workforce.

Table 4: Relative Effect on Municipalities with Higher Inspection Rates

Treatment dummy	Regularizing Hiring Rate	Overall Hiring Rate	Overall Separation Rate
Above 60^{th} percentile	0.00323**	0.00602**	0.00386
	(0.00125)	(0.00286)	(0.00326)
Above 75^{th} percentile	0.00420***	0.00198	0.00116
	(0.00138)	(0.00350)	(0.00391)
Above 90^{th} percentile	0.00703***	0.000345	0.00164
	(0.00187)	(0.00442)	(0.00722)

Note: Estimates from equation (3). See text for definition of variables. Significance levels: *=10%; **=5%; ***=1%.

Table 4 also shows that such significant and robust results for regularizing hires are not observed for the other two outcome variables reported in the last two columns of the table. Indeed, apart from one statistically significant (at the 5% level) estimate for the overall hiring rate, all other estimates are not significant on statistical grounds for both variables. Thus, increasing the intensity of inspection rates does not seem to change the decision of employers regarding overall formal hiring and separations flows in treated municipalities of small and medium sizes.

It should be pointed out that all our results are for the short term. Results may change in the longer run depending on whether employers perceive that the changes brought about by Plancite - specially the punishment component - are permanent or transitory.

The results shown in Table 4 relies on the identification hypothesis that the evolution of our outcome variables for the group with lower inspection rates is a good proxy for the counterfactual evolution of the outcome variables for the group with higher inspection rates. In order to access the plausibility of this hypothesis, we conducted a placebo exercise where we estimated the parameters of interest using the same model specifications but pretending that the launching of Plancite occurred one year before its actual launching. One should expect the estimation of null effects as there was no program in reality. As shown in Table 5, this expected result is confirmed as none of the estimates are statistically significant.

Table 5: Placebo Exercise for Relative Effect on Municipalities with Higher Inspection Rates

Treatment dummy	Regularizing Hiring Rate	Overall Hiring Rate	Overall Separation Rate
Above 60^{th} percentile	-0.00226	-0.000649	-0.00311
	(0.00139)	(0.00446)	(0.00403)
Above 75^{th} percentile	-0.00137	-0.00261	-0.00542
	(0.00158)	(0.00626)	(0.00461)
Above 90^{th} percentile	-0.00314	0.00472	0.00304
	(0.00275)	(0.00782)	(0.00542)

Note: Estimates from equation (3). See text for definition of variables. Significance levels: *=10%; **=5%; ***=1%.

5 Summary and Conclusion

The aim of this study was to evaluate a policy experiment implemented in Brazil to reduce labor informality through new actions in labor inspection. These new actions were consolidated in a program called Plancite, and our evaluation focus on a specific new action targeted to small and medium size municipalities in the country. This initiative combined two components. The first consists in providing information on the negative consequences of informality for employers, workers, and for society in general. The information was channeled through letters and e-mails sent to employers and also divulged to the population in general by local media. The other component is an increase in the frequency of face-to-face inspections in the establishments located in a subset of municipalities treated by the first component. We call the first component the *awareness* component and the second the *punishment* component.

We explore the fact that the punishment component had different intensities across treated municipalities to identify potential heterogeneous impacts of the program. In order to pin down this relative impact, we firstly exploited a discontinuity design in the eligibility rule of the program to identify its impact for a certain level of the punishment component. For a relatively low level of this component, our results indicate that the intervention had no effect on the formalization of previously unregistered employees. However, employing a second identification strategy that compares treated municipalities exposed to distinct levels of the punishment component, we show that hires regularizing previously unregistered employees increase in municipalities submitted to higher intensities of the punishment component. Our results also indicate that the effect size is monotonically related to the punishment component intensity, that is, the higher the increase in the rate of face-to-face inspections, the higher the estimated effect.

We also investigated the program's effect on overall formal hiring and separations. For these outcomes, we do not find any robust effect of the intervention irrespective of the intensity in the punishment component. All our results are robust to many alternative specifications of the estimation procedures.

One limitation of our results is that they are restricted to capture the program's effects only for the short run. In future work, we intend to extend the results for longer time horizons.

References

Abras, A., Almeida, R., Carneiro, P., and Corseuil, C. (2018). Enforcement of labor regulations and job flows: evidence from brazilian cities. *IZA Journal of Development and Migration*, 24(8):1–19.

Almeida, R. and Carneiro, P. (2012). Enforcement of labor regulation and informality. *American Economic Journal: Applied Economics*, 4(3):64–89.

Almeida, R., Carneiro, P., and Narita, R. (2015). Producing higher quality jobs: Enforcement of mandated benefits across brazilian cities between 1996-2007.

Batistin, E. and Rettore, E. (2008). Ineligibles and eligibles non-participants as a double comparison-group in regression discontinuity designs. *Journal of Econometrics*, 142:715–730.

Calonico, S., Cattaneo, M., Farrelland, M., and Titiunik, R. (2019). Regression discontinuity designs using covariates. *Review of Economics and Statistics*, 101(3):442–451.

Calonico, S., Cattaneo, M., and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression discontinuity designs. *Econometrica*, 82(6):2295–2326.

Gabaix, X. (2009). "power laws in economics and finance. *Annual Review of Economics*, 1(1):255–93.

Lemieux, T. and Milligan, K. (2008). Incentive effects of social assistance: A regression discontinuity approach. *Journal of Econometrics*, 142:807–828.

Perry, G., Maloney, W., Arias, O., Fajnzylber, P., Mason, A., and Saavedra-Chanduvi, J. (2007). *Informality, Social Protection, and Antipoverty Policies*. Informality: Exit and Exclusion. The World Bank.

Ulyssea, G. (2018). Firms, informality and development: Theory and evidence from brazil. *American Economic Review*, 108(8):2015–2047.