Do Entry Regulation and Taxes Hinder Firm Creation and Formalization? Evidence from Brazil*

Rudi Rocha
UFRJ

Gabriel Ulyssea†
PUC-Rio and Ipea

Laísa Rachter
UFRJ

September 8, 2014

Abstract

What are the effects of eliminating registration costs and reducing taxes on firm creation and formalization? We answer this question by estimating the impacts of a large-scale formalization program in Brazil. We explore exogenous variation in access to the program across time, regions and industries using individual panel data. We show that reducing registration costs is not a sufficient condition to induce small informal entrepreneurs to formalize nor to foster the creation of new formal businesses. Our unique empirical setting also allows us to separately identify the impact of reducing taxes once registration costs had already been eliminated. We find modest effects on formalization and none on the creation of new formal businesses.

Key words: Entry regulation, taxes, informality, firm creation.
JEL code: O17, H32, H26.

*We would like to thank Mariano Bosch, Carlos Henrique Corseuil, Claudio Ferraz, Miguel Foguel, David McKenzie, and Joana Monteiro for comments and helpful discussions. We are also thankful to seminar participants at UFRJ, Yale, IDB, PUC-Rio, IPEA, BNDES and the 2013 Meeting of the Brazilian Econometric Society for helpful comments. All remaining errors are ours.

†Corresponding author. Address: Department of Economics, Pontifícia Universidade Católica do Rio de Janeiro, Rua Marquês de São Vicente, 225, Gávea. Rio de Janeiro, RJ, 22451-900, Brasil. Email: ulyssea@econ.puc-rio.br

1 Introduction

Developing countries are often characterized by burdensome regulations, which are perceived as an important obstacle to firm creation, formalization and growth.[1] In order to reduce this regulatory burden, many countries have devoted substantial efforts to reduce the monetary and non-monetary costs of registering a business [Bruhn and McKenzie (2014)]. A growing literature, however, shows little empirical support for the effectiveness of such reforms. Alternatively, the ongoing costs of formality (e.g. tax liabilities) have been increasingly highlighted as a major constraint to firms' performance in these countries [e.g. Kaplan et al. (2011) and De Mel et al. (2013)].[2] If this is the case, tax reductions could in principle lead to improvements in firm-level outcomes, in particular greater firm entry and formalization. However, tax reductions can also imply a substantial fiscal cost, which will be larger the lower their effectiveness in increasing formalization rates.[3] The effectiveness of tax reductions is therefore a key policy parameter, for which we still have very little empirical evidence.

In this paper we answer two specific questions. First, what are the effects of eliminating regulatory entry costs on the creation of formal firms and the formalization of existing informal firms? Second, and most importantly, once registration costs are eliminated, what are the effects of reducing the tax burden on firm creation and formalization? To get to these questions, we estimate the effects of a large-scale formalization program implemented in Brazil, the *Individual Micro-Entrepreneur Program* (henceforth IMP).[4]

Introduced in 2009, the program targeted entrepreneurs with at most one employee, and was designed to reduce formalization costs in two dimensions: (i) entry costs, by lowering monetary and non-monetary registration costs; and (ii) the costs of remaining formal, by reducing monthly taxes and red tape. Even though these two sets of benefits were intended to be introduced simultaneously, we show that in fact the program had two distinct phases. The first phase drastically reduced registration costs but failed to reduce tax rates for most eligible entrepreneurs. This occurred because IMP introduced a monthly lump-sum tax, so only high-income individuals perceived a lower tax rate in comparison to the pre-existing flat rate. It was only in its second phase, when the lump-sum tax is halved, that IMP *de facto* reduced the monetary cost of remaining formal for

[1] For within-country evidence, see for example Besley and Burgess (2004). For comprehensive cross-country evidence, Djankov et al. (2002), Botero et al. (2004), and Djankov et al. (2010).

[2] In Brazil, for example, firms spend on average nearly fifteen times more hours to pay their taxes and face a more than 50% higher tax rate when compared to high-income OECD countries.

[3] Oppositely, in a scenario where tax reductions induce a large increase in formalization rates, the gains from expanding the tax base could more than compensate the revenue losses from reductions in the tax rate.

[4] *Programa do Microempreendedor Individual - MEI*, in Portuguese.

most eligible entrepreneurs. This staggered implementation gives us an unique empirical setting in which different incentives are introduced at different points in time. This feature, coupled with variation in access to the program, enables us to separately identify the effects of reducing registration costs and the costs of remaining formal. As far as we know, this is the first paper to separately identify these channels.

We explore two sources of exogenous variation in access to the program to identify these effects. First, eligibility was defined at the industry level. The list of eligible industries followed the one used by the pre-existing tax framework for micro, small and medium enterprises (known as SIMPLES), which was introduced more than a decade prior to IMP.[5] As there is no indication that the government hand-picked specific industries based on their past performance or observable characteristics, we assume that industry eligibility in IMP was exogenously determined. We perform several empirical tests that increase our confidence on this assumption. Second, firm registration within the program must be made through a web platform, which due to technical restrictions was made progressively available across states. This feature generated variation in access to the program across states and time.

We use these sources of variation in access to the program across time, regions and industries to estimate IMP effects using individual panel data, built from the Brazilian Monthly Employment Survey (PME). The panel structure at the individual level allows us to differentiate formalization of existing informal entrepreneurs from creation of new formal businesses (i.e., transitions of salaried workers into formal entrepreneurship). The heterogeneity in eligibility and access to the program allows for a treatment/control strategy that contrasts the evolution of a given outcome for individuals in different industries and regions to identify the contribution of IMP to the observed changes.

We find that reducing formal sector's entry costs had no effect on formalization of informal entrepreneurs nor on the creation of new formal businesses. This result is observed even amongst high-income informal entrepreneurs, for whom the ongoing costs of formality in IMP were no higher than in SIMPLES. This implies that reducing entry costs is not a sufficient condition for the formalization of small informal entrepreneurs. We only observe effects on registration of informal entrepreneurs when the monetary costs of being formal are substantially reduced. In our preferred specification, the estimates indicate that halving monthly taxes led to a 2.3 percentage points increase in the median entrepreneur's registration rate, from a baseline rate of 27%. Several falsification and robustness tests confirm this result. This average effect is entirely driven

[5]The acronym SIMPLES, in Portuguese, stands for Integrated System for Payment of Taxes and Social Security Contributions of Micro and Small Enterprises. SIMPLES was the pre-existing, and still is the main tax system for micro, small and medium enterprises in Brazil.

by high-income entrepreneurs, which is consistent with the regressive tax rate schedule introduced by IMP. This result is also consistent with the idea that a large fraction of low-productivity informal firms perceive very small or no benefits at all from formality [e.g. Ulyssea (2013)]. Finally, we find no effects of tax reductions on entrepreneurship, as the program did not induce either formal or informal salaried workers to become formal entrepreneurs.

This paper makes two main contributions. First, we show that reducing registration costs is not a sufficient condition for formalization, even when the costs of remaining formal are also reduced (i.e. amongst high-income entrepreneurs). Second, we are able to isolate the impact of reducing the ongoing costs of formality on firm creation and formalization, in a context where registration costs had already been nearly eliminated. This paper thus provides the first attempt to test whether the costs of remaining formal are indeed a relevant constraint to the creation of formal firms, irrespective of registration costs and net of potential confounding effects. In addition, and contrary to most existing studies, we are able to identify whether the observed effects on the creation of formal firms comes from new formal businesses being created or simply informal firms moving into formality. We find that all the effects come from the formalization of the stock of informal firms rather than the creation of new formal businesses.

Several existing studies estimate the effects of reducing registration costs, but do so in settings where the costs of being formal remain unchanged. In a non-experimental setting, Kaplan et al. (2011) and Bruhn (2011) estimate the effects of a program that reduced registration costs in Mexico (SARE) on the creation of formal firms. Bruhn (2011) concludes that the effects come from previously wage workers creating new formal businesses. Oppositely, Kaplan et al. (2011) estimate a very small and transitory impact, and present evidence that the observed effect comes from the formalization of existing informal firms. A second stream of papers relies on experiments and focuses on the formalization of existing informal firms, not being able (by design) to assess the effects on the creation of new formal firms [e.g. De Mel et al. (2013), De Andrade et al. (2013), De Giorgi and Rahman (2013)]. The results indicate very limited or no effects on formalization. Finally, Monteiro and Assunção (2012) and Fajnzylber et al. (2011) evaluate the effects of SIMPLES, which reduced taxes and red tape associated with tax payments but left registration costs unchanged. Both studies explore a cross-section of firms to test whether formality was higher amongst new firms created just before and after the program was introduced, comparing eligible and non-eligible industries. However, as Monteiro and Assunção (2012) recognize, identification concerns leave open the question of whether their results are driven by confounding effects.[6]

[6]The same is true for Fajnzylber et al. (2011), who follow a similar strategy as Monteiro and As-

The remainder of this paper is organized as follows. Section 2 describes the institutional setting, the data, and contains the empirical preliminaries. Section 3 describes our empirical strategy. Section 4 presents and discusses the results, while section 5 presents robustness checks. Section 6 concludes.

2 Institutional Setting and Data

2.1 The Individual Micro Entrepreneur Program

The first major initiative to reduce the tax burden of micro, small and medium enterprises in Brazil dates back to 1996, when the federal government introduced the SIMPLES program. This program created a differentiated tax system, which reduced red tape, consolidated several taxes and social security contributions into a single payment, and reduced the overall tax burden [Monteiro and Assunção (2012)]. Eligibility to the program was defined at the 7-digit industry level and was oriented towards manufacturing, transportation, and other services not subject to specific regulations.[7] Additionally, a cap on annual revenue limited participation of larger firms. Over the years the revenue cap was substantially expanded and the SIMPLES became the relevant institutional framework for most firms in Brazil.

In July 2009, the federal government introduced a new tax and business registration framework specifically designed for micro entrepreneurs with up to one employee, the *Individual Micro-Entrepreneur Program* (henceforth IMP).[8] The program had three main goals: to foster entrepreneurship and the creation of new formal business; to increase tax registration and compliance of existing (informal) micro entrepreneurs; and to increase contributions to the social security system. In fact, the program locked these two margins of formalization together: when registering their business through IMP, entrepreneurs were also automatically enrolled in the social security system. IMP substantially reduced formalization costs in two dimensions: (i) registration costs, both monetary and non-monetary (e.g. number of procedures required to register a business); and (ii) taxes and red tape associated with tax payments. Hence, the program reduced both the costs of

sunção (2012) but increase the weight of observations in the proximity of the date that the program is implemented. They also explore a regression discontinuity design that compares firms created around the program's implementation date. This approach, however, leaves open the question of whether the results are driven by confounding time fixed effects.

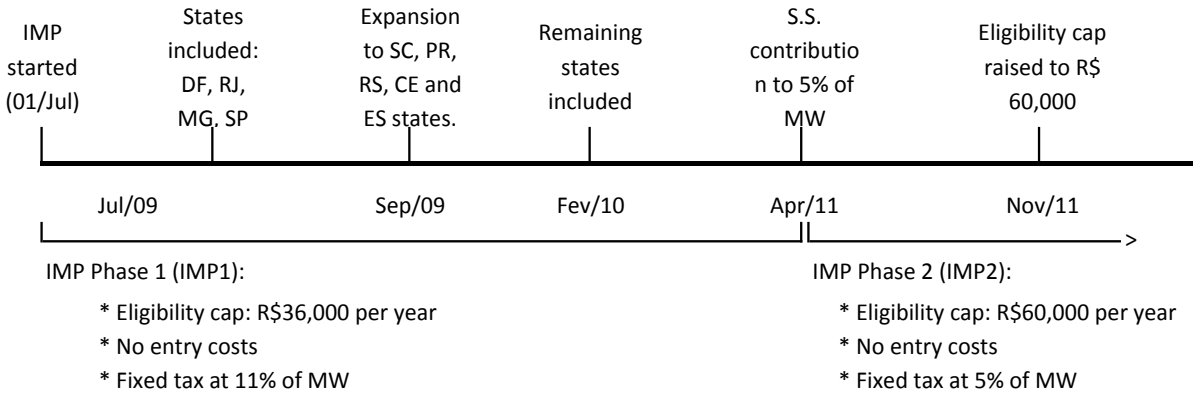
[7]For instance, the SIMPLES eligibility rule excluded the financial sector, real estate companies, developers, cleaning service firms, and activities with regulated occupations, such as physicians, architects, and musicians.

[8]This group corresponds to 89% of all entrepreneurs in Brazil [data from the National Household Survey (PNAD), 2012].

entering the formal sector and the costs of remaining formal.

Even though these two sets of incentives (lower entry costs and taxes) were intended to be introduced simultaneously at the time the program was created, this was not the case. We show that in fact the program had two distinct phases, which we name IMP1 and IMP2. In the first one (IMP1), the program eliminated monetary entry costs and introduced a web platform for online business registration, which consolidates all procedures required by agencies at national and sub-national levels into a few online steps. Due to technical constraints, the online platform was not available to all states simultaneously. There was a 8-month window before all states had access to it, which occurred only in February 2010 (Figure 1).[9] Since firm registration within the program must be made through the web platform – and firms cannot register in one state and operate in another – this generated variation in access to the program across states and time during IMP1.

Figure 1: IMP Implementation Timeline



During IMP1 there was also an attempt to reduce the ongoing costs of formality. Federal taxes were eliminated, while state and municipality taxes were fixed at negligible values. The major tax component became the monthly social security contribution, which was fixed at 11% of the minimum wage and corresponded to more than 90% of total monthly tax expenditures. At the time of implementation, IMP1's monthly tax burden was nearly R$50 (around US$25).[10] By fixing monthly tax expenditures, the

[9] Access was first made available July 1st 2009 for entrepreneurs located in the Federal District; it was then expanded to the states of Minas Gerais, Rio de Janeiro and Sao Paulo in July 24th; to Santa Catarina, Parana, Rio Grande do Sul, Ceara and Espirito Santo in September 2009.

[10] The minimum wage was R$465 at the time of implementation and the corresponding social security

program automatically introduced a regressive tax rate schedule for eligible entrepreneurs. Moreover, the monthly taxes were fixed at high values relatively to eligible entrepreneurs' income. As a consequence, only entrepreneurs at the highest percentiles of the income distribution perceived an actual reduction in their tax rate in comparison to the pre-existing rate under the SIMPLES framework (a fixed rate of around 4%).[11] The tax rate under IMP1 was above 10% for entrepreneurs at the 25th percentile of the earnings distribution. Only for entrepreneurs above the 75th percentile the implied monthly tax rates under IMP1 were similar to, or lower than those of SIMPLES. Figure 2 illustrates this point.

In April 2011 the program entered its second phase (IMP2), when social security contribution was reduced from 11% to 5% of the minimum wage. As shown in Figure 2, it was only then that most eligible entrepreneurs perceived an actual reduction in tax rates in comparison to that under the SIMPLES framework. Hence, it was only in IMP2 that the program actually reduced the costs of remaining formal for most eligible entrepreneurs. Put differently, the first phase (IMP1) reduced entry costs but failed to effectively reduce tax rates. The second phase (IMP2) maintained the benefits from IMP1 and further reduced tax rates, effectively reducing the cost of remaining formal for most eligible entrepreneurs.

2.1.1 Eligibility Criteria

Eligibility to the program is based on three criteria. The first is firm size measured as number of employees, since the program is targeted at entrepreneurs with at most one employee. Second, eligibility is defined by annual revenue, with a cap initially set at R$36,000 per year (around US$18,000), and further expanded to R$60,000 in November 2011. The annual revenue eligibility cap amounts to around 25% of that under the SIMPLES program, which reinforces the fact that the program was targeted at very small businesses.

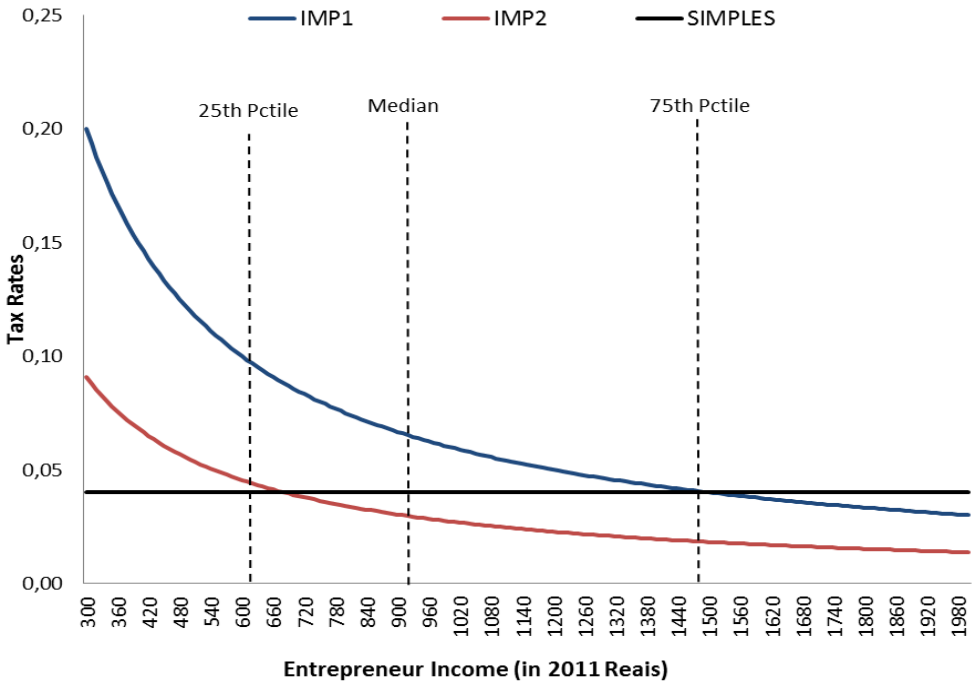
Third, eligibility was defined by economic activity at the 7-digit industry level. IMP's list of eligible industries comprised nearly five hundred 7-digit industries, which in 2009 (just before the program was fully implemented) corresponded to 46.5% of all entrepreneurs in Brazil.[12] The definition of eligible and non-eligible industries followed the previous

contribution was R$51 (approximately US$25.5). State and municipality taxes were fixed at R$1 and R$5 per month, respectively (roughly 50 cents and 2.5 US dollars, respectively). IMP1 thus implied an overall tax burden of either R$52 or R$56, depending on the industry (US$26 or US$28, respectively).

[11] This corresponds to the tax rate alone. Other variable costs, such as social security contribution and accountant fees are excluded from our calculations, since these are optional and therefore depend on entrepreneurs' choice.

[12] According to the National Household Survey (PNAD), our own tabulation. The initial list of eligible

Figure 2: Tax rates under SIMPLES, IMP1 and IMP2



Note: Tax rates under the SIMPLES, IMP1 and IMP2 using income in 2011 Reais. The 25th, median and 75th percentiles refer to the income distribution among entrepreneurs with at most one employee in April 2011, the month of IMP2 implementation. The reduction in tax rates under IMP reflects the reduction of social security contribution from 11% to 5% of the minimum wage in IMP2.

configuration already in place under SIMPLES, which had been introduced more than a decade before IMP implementation. The list of eligible industries was concentrated on manufacturing, transportation, and services not subject to specific regulations. There were minor changes in this list over time, but with no significant variation in the total number of eligible industries (or entrepreneurs) actually observed in the data. We provide descriptive statistics and further details on industry eligibility in the following section.

2.2 Data

The main data set used in our analysis is the Brazilian Monthly Employment Survey (PME), which covers the six largest metropolitan areas in Brazil. It is a rotating panel with a similar design to that of the Current Population Survey (CPS). It tracks each

industries was edited by Resolution CGSN n.58, April 2009. Marginal changes to the original list were implemented by Resolutions CGSN n.64 (August 2009), n.78 (September 2010), n.94 (November 2011), and n.104 (December 2012).

household for 4 consecutive months, they rest for 8 months, and are then re-interviewed for 4 additional months. We use data from January 2006 to August 2012.

In accordance to the eligibility criterion, we focus on entrepreneurs with at most one employee (including the self-employed). We do not use the criterion based on revenue because the variable reported in the survey is not necessarily the same reported to the tax administration office.[13] The outcome variable is an indicator of whether the individual is an entrepreneur who contributes to the social security system, which is a commonly used measure of formalization in the literature. Moreover, as discussed in the previous section, the program locked together social security contribution and business registration decisions.[14] Hence, contribution to the social security system is a particularly good formality measure in the present context.

We further restrict our sample to the first and fifth interviews of each individual, so they are observed in t and $t + 12$. We include only individuals aged between 21 and 65 in their first interview, as well as those who work more than 20 hours per week, so not to include occasional occupations. We also exclude individuals in agriculture, public sector and domestic activities. Our final sample contains 46,212 observations, which corresponds to 23,106 individuals interviewed twice between January 2006 and August 2012. Table 1 presents the sample's main descriptive statistics.

2.2.1 Eligible and Non-Eligible Industries

Despite its many advantages, the PME only contains information about industry classification at the 2-digit level, while program eligibility is defined at a more disaggregated level. Therefore, we use information on industry classification at the 5-digit level available in the National Household Survey (PNAD) to complement the information available in the PME.[15] The description of how we use the PNAD data to build an improved measure of industry eligibility is provided in the next section.

Table 2 presents summary statistics for entrepreneurs in eligible and non-eligible in-

[13] In the robustness analysis (Section 5) we do use the criterion based on revenue to further restrict our sample to entrepreneurs below the income threshold. In a different robustness check, we expand the sample to entrepreneurs with up to 5 employees. The concern in this case is that many small firms hire a share of their workers without a formal labor contract, which can potentially blur the program's eligibility criteria. The results remain largely unchanged in both exercises.

[14] In Brazil, a firm is formally registered if it holds a CNPJ, which is an acronym for *Cadastro Nacional da Pessoa Jurídica*. This is an identification number for firms that is issued by the federal government.

[15] The National Bureau of Statistics (IBGE) uses two different industry classification systems. One for the National Accounts, which is the same used to define program eligibility (at the 7-digit level), and another for Household Surveys, which includes the PNAD (at the 5-digit level). There is a direct correspondence between these two classification systems that is provided by IBGE. We use this correspondence to link industry classification in the PNAD to the one used to define industry eligibility in the IMP.

Table 1: Descriptive Statistics

	Mean	SD	Min	Max
Contributes to S.S. (share)	0.27	0.44	0	1
Schooling (years)	8.55	4.00	0	15
Log(Wage)	6.90	0.78	3.25	9.31
Hours worked p/week	43.5	11.0	20	112
Age	44.5	10.6	21	66
White (share)	0.54	0.49	0	1
Male (share)	0.63	0.48	0	1
Obs.		46,212		

Source: Authors' own tabulations from the Monthly Employment Survey (PME).The sample correspond to 23,106 entrepreneurs (self-employed and employers with up to one employee, aged between 21 and 65 in the first interview, and who work 20 hours or more per week) interviewed twice over the January 2006 thought August 2012 period.

dustries using PNAD data from 2006 to 2009, just before IMP's full implementation. To obtain these statistics, we restrict the PNAD data to the same metropolitan regions covered by PME, and apply the same filters used to restrict the PME sample. We observe that the main difference between eligible and non-eligible industries regards industry composition, which is explained by the pre-existing eligibility criterion under SIMPLES. Size distribution, log-earnings, age and schooling look very similar, even though in most cases they are statistically different. Taking a step further to refine the comparison between eligible and non-eligible groups, we stack the 2006-2009 PNAD data and run a Logit regression of a dummy indicating whether the entrepreneur is in an eligible industry on a series of observables at the entrepreneur level, conditioning on year and state fixed-effects. We find no association between entrepreneurs' observables and eligibility, except for the industry dummies.[16]

[16]Results omitted and available upon request.

Table 2: Eligible versus Non-Eligible Industries

	Non-Eligible Mean	Eligible Mean	Difference P-value
Size distribution (shares)			
No employees	0.815	0.791	0.000
One employee	0.055	0.051	0.113
2 to 5 employees	0.079	0.098	0.000
6 to 10 employees	0.024	0.031	0.000
11+ employees	0.028	0.029	0.415
Formal firms (has CNPJ)[†]	0.301	0.324	0.019
Contributes to S.S. (share)	0.300	0.314	0.005
Industry Composition			
Service	0.072	0.215	0.000
Commerce	0.456	0.142	0.000
Manufacturing	0.020	0.254	0.000
Transportation	0.053	0.094	0.000
Construction	0.313	0.000	0.000
Log-Earnings	7.0	7.0	0.293
Male (share)	0.732	0.543	0.000
Avg. schooling (years)	8.4	9.2	0.000
White (share)	0.526	0.552	0.000
Age	43.1	42.6	0.000
Hours per week	43.9	43.8	0.512

Notes: Authors' own tabulations from the National Household Surveys (PNAD), stacked 2006–2009. Sample restricted to the same metropolitan regions covered by PME, same additional filters applied.

3 Empirical Strategy

As discussed in the previous section, the PME only provides information about entrepreneurs' industry at the 2-digit level. Thus, we do not observe whether she belongs to an eligible industry, as eligibility is defined at a greater level of disaggregation. The main consequence is that we cannot directly use PME data to apply a standard difference-in-differences estimator. We thus take a different route and define a *potential of treatment intensity* variable, which is the *pre-program* weighted share of eligible industries within each 2-digit industry.[17] Formally, we use the 2009 PNAD micro data to construct the treatment intensity variable for a 2-digit industry s as follows:

[17] This variable is analogous to treatment variables used in different contexts [e.g. Bleakley (2007)].

$$Intense_s = \frac{\sum_{k \in s} (\mathbf{I}\,[\text{Sub-Industry } k \text{ is eligible}] \times N_{ks})}{\sum_{k \in s} N_{ks}} \quad (1)$$

where k indexes sub-industries at the 5-digit level, N_{ks} denotes the number of entrepreneurs that belong to sub-industry k within the 2-digit industry s in 2009.[18] The variable $Intense_s$ is then merged at the industry s level with our monthly panel of entrepreneurs from PME.

Our strategy is to contrast the evolution of a given outcome for individuals located in industries with distinct potential of treatment intensities in order to identify the contribution of IMP to the observed changes. The underlying assumption is that entrepreneurs located in industries with higher potential of treatment are more likely to benefit from the program than those located in industries with a lower potential. This heterogeneity in potential of treatment allows for a treatment/control strategy analogous to a standard difference-in-differences estimator. Our approach will be closer to a standard diff-in-diff the more polarized the distribution of the variable $Intense_s$ is. Indeed, Table 3 shows that $Intense_s$ is a discrete variable (it assumes 15 different values) and is very concentrated around zero and one.

In addition to variation across industries, the staggered implementation of the program's web platform also allows us to explore regional variation in access to the program during its first phase. We thus use a triple interaction between industry ($Intense_s$), time, and region to identify the impacts of IMP1. For IMP2, we rely on the interaction between industry and time.

3.1 Main Specification

In the analysis we consider two sets of outcomes: (i) formalization of informal entrepreneurs; and (ii) creation of new businesses by individuals who were previously formal or informal employees. In order to identify the effects of reducing entry costs (IMP1) and taxes (IMP2), we run two separate regressions using time windows around their start dates – July/2007 and April/2011, respectively. More specifically, for IMP1 we define the sample so that individuals have their first interview at the earliest in August 2008 and at the latest in June 2009 – the month just before IMP1 introduction. For IMP2, we analogously define the time window for the first interview between May 2010 and March

[18] PNADs' reference month is September. IMP was first available in the Federal District in July 2009, though the Federal District is not included in our PME sample of metropolitan regions. IMP was then available in Rio de Janeiro, Minas Gerais and Sao Paulo by August 2009, just a few weeks before the 2009 PNAD's month of reference. Since the share of entrepreneurs in eligible industries within each 2-digit industry remained quite stable between 2008 and 2009, we opt to base our calculations in 2009, which is closer to IMP1 introduction.

Table 3: Variable Intense: Tabulation for Different Samples

Var.	Frequencies for Different Sub-samples			
Intense	Informal Entrepren.	Inf. Employee	Form. Employee	All
0.000	0.219	0.197	0.192	0.193
0.090	0.262	0.167	0.180	0.200
0.126	0.015	0.038	0.058	0.046
0.375	0.015	0.016	0.015	0.016
0.389	0.005	0.009	0.011	0.010
0.604	0.062	0.058	0.067	0.067
0.699	0.015	0.040	0.041	0.033
0.751	0.001	0.009	0.024	0.016
0.822	0.003	0.011	0.017	0.012
0.838	0.056	0.118	0.138	0.119
0.895	0.089	0.051	0.011	0.034
0.907	0.043	0.058	0.036	0.043
0.917	0.002	0.002	0.003	0.003
0.951	0.018	0.037	0.018	0.021
1.000	0.196	0.189	0.188	0.187

Notes: First column contains the values found for the variable $Intense_s = \frac{\sum_{k \in s}(\mathbf{I}[\text{Sub-Industry } k \text{ is eligible}] \times N_{ks})}{\sum_{k \in s} N_{ks}}$. The remainder columns contain the frequencies for each sub-sample used in regressions: informal entrepreneur in the first interview (column 2), informal employee in the first interview (column 3), formal employee in the first interview (column 4), and the entire sample (column 5).

2011.[19] The main specification for both regressions is given by the following expressions:

$$\begin{aligned} Y_{isrt} &= \beta_1 Post1_{rt} \times Intense_s + \gamma_1 Intense_s + \gamma_2 Post1_{rt} \qquad (2) \\ &+ \gamma_3 \mathbf{I}_t \times Intense_s + \gamma_4 \xi_r \times Intense_s + Z_{isrt} + \phi_i + \epsilon_{isrt} \\ Y_{isrt} &= \beta_2 Post2_t \times Intense_s + \gamma_1 Intense_s + \gamma_2 Post2_t + Z_{isrt} + \phi_i + \epsilon_{isrt} \qquad (3) \end{aligned}$$

where i indexes individuals, s industries, r regions and t time. The Y_{isrt} denotes the outcome variable for entrepreneur i, in industry s, metropolitan region r, at calendar time t. $Post1_{rt}$ is a dummy that indicates whether access to IMP1 through the online platform was available in metropolitan region r at time t; $Post2_t$ is the analogous variable for IMP2, but does not vary across regions; $Intense_s$ denotes the weighted share of entrepreneurs in eligible industries (equation 1); and $\mathbf{I}_t$ is an indicator variable for the implementation dates in the different regions, i.e. $\mathbf{I}_t[t \geq j]$ where $j \in \{Aug09, Sept09, Feb10\}$. The term

[19] In the robustness analysis we include both variables (IMP1 and IMP2) in a single regression and do not impose any time restriction on our sample. The results remains unaltered.

ϕ_i denotes entrepreneurs fixed effects and ϵ_{isrt} is the error term. We include a set of additional controls in the term Z_{isrt}, which is given by:

$$Z_{isrt} = \lambda_t + \xi_r + \delta_s + Trends'_{srt}\theta + X'_{isrt}\gamma$$

where λ_t, ξ_r, δ_s denote time, region and industry fixed effects, respectively.[20] The term X_{isrt} includes time-varying observables at the entrepreneur-level; and $Trends_{srt}$ includes time-varying economic variables at the region-by-industry level and their square, cubic and quartic, as well as a fourth degree polynomial in time interacted with region-by-industry entrepreneur formalization rates in 2006, to control for potential reversion to the mean effects.[21]

The triple-interaction between industry, time and region, $Post1_{rt} \times Intense_s$, is our variable of interest when estimating IMP1 effects. The double-interaction between industry and time, $Post2_t \times Intense_s$, is our variable of interest when estimating the effects of IMP2. The β_1 and β_2 coefficients are thus our parameters of interest.

3.2 Identification

In the experimental ideal, in which entrepreneurs are randomly selected into treatment, β_1 and β_2 identify the variation in a given outcome caused by the introduction of IMP1 or IMP2. In our non-experimental context, however, other unobservable determinants may be correlated with our variables of interest. The inclusion of entrepreneurs fixed-effects, ϕ_i, accounts for part of these potential confounding effects, as they control for non-observable (fixed) characteristics of individuals that may be simultaneously associated with a given outcome and industry choice. However, this is not sufficient to ensure identification. Analogously to a standard difference-in-differences approach, the validity of our strategy relies on two conditions. First, we need to ensure that our estimates are not spuriously driven by economic trends that are specific to industries with a high or low potential of treatment ($Intense_s$). The second condition required for identification is that economic shocks impact entrepreneurs in different industries uniformly throughout the period being considered in the analysis. In particular, eligible industries are prevalent in specific sectors (service and manufacturing) and if these are hit differently by economic shocks, then this could contaminate our estimates.

Regarding the first condition, as discussed in the previous section, industry eligibility

[20] Region fixed-effects are implicit in individuals fixed-effects since migration inter-regions becomes attrition in the PME data.

[21] More specifically, the economic variables are the mean monthly wage, the mean number of hours of work, and the log of the total employment for each region, industry and time.

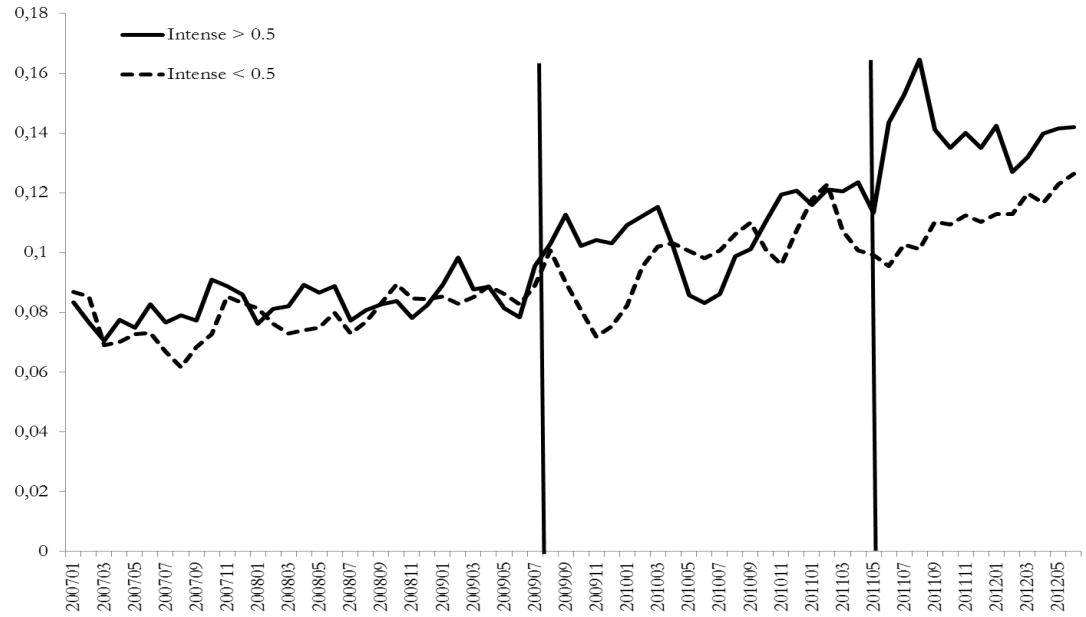
under IMP was based on SIMPLES's list of eligible industries. It was thus defined on a list of industries selected more than a decade prior to IMP. As there is no indication that the government hand-picked specific industries based on their past performance or observable characteristics, we may assume that IMP's industry eligibility was exogenously determined. Figure 3 provides evidence on the validity of this assumption by plotting formality trends before and after policy changes. We compare average transitions into formality for those entrepreneurs who report to be informal in the first interview, and are located in industries with distinct potential of treatment intensities (below and above the median of the variable $Intense$). We observe no systematic difference in pre-trends for these two groups of entrepreneurs. Nevertheless, we do observe a positive shift in transition rates into formality after IMP2's implementation, when taxes are halved.

In order to further assess the validity of the assumption that IMP's industry eligibility was exogenously determined, we use the PNAD data to test whether observables at the entrepreneur level or industries' pre-trends correlate with industry eligibility. We collapse the micro data at the 5-digit industry level, including averages of entrepreneurs' socio-demographic characteristics as well as of other outcomes of interest, such as industries' average wage and formalization rates (social security contribution). We then regress industry eligibility in 2009 (when the program is implemented) against recent variation in formalization rates and wages, as well as other industry-level observables. Table 4 presents the results. We find no significant association between industry eligibility and pre-existing trends in formality and wages. This result increases our confidence in the exogeneity of the industry eligibility criterion.

As for the second condition, it is of particular concern for identifying IMP1 effects because its introduction coincides with the effects of the great recession, which were felt more strongly in Brazil in 2009. For example, if economic recovery within eligible industries was relatively faster (slower) after IMP1, our model would likely overstate (understate) IMP1's true effect on formalization. To address this potential threat to identification, we first control for specific industry-by-region trends in the term $Trends_{srt}$. More importantly, because of the staggered implementation of IMP1 we are able to exploit a triple-interaction between time, region and industry in access to the program, which helps minimize this concern. The identification assumption here is that there is no reason other than IMP1 for the difference of formalization between industries to increase relatively more in regions that were treated earlier when compared to those treated later.[22] As for the IMP2, there is no substantial economic fluctuation around its implementation, which reduces our concerns about the potential influence of heteroge-

[22] A similar triple-difference was implemented in Kaplan et al. (2011), who interacted industry, region and time eligibility to identify the effect of a reduction in entry costs on firm creation in Mexico.

Figure 3: Informal to Formal 12-Month Transition Rates for Entrepreneurs in Industries with Potential of Treatment Below and Above the Median



Note: Authors' own calculation from the Monthly Employment Survey (PME). Annual formalization rates for each month correspond to the share of formal entrepreneurs in the given month that were informal 12 months before. These rates are computed separately for entrepreneurs in industries with low potential of treatment ($Intense_s < Median$) and high potential of treatment ($Intense_s > Median$). The Figure plots the 3-month moving averages of these rates, indexed by the first month used to compute the average. The sample corresponds to self-employed and employers with up to one employee who reported to be informal in the first interview, aged between 21 and 65 in the first interview, and who work 20 hours or more per week.

Table 4: Determinants of Sector Eligibility: Regressions at the Industry Level, data from the 2009 PNAD

Dependent variable: Eligible industry in 2009			
$\Delta_{2009-2008}$(Log Wages)	-0.389		-0.405
	(0.289)		(0.288)
$\Delta_{2009-2008}$(Formality Rate)		-0.110	0.115
		(0.738)	(0.750)
Share Male	-0.180	-0.186	-0.180
	(0.309)	(0.313)	(0.310)
Avg. Schooling (Yrs)	0.070	0.067	0.070
	(0.052)	(0.051)	(0.051)
Share White	-1.207	-1.154	-1.225
	(0.844)	(0.832)	(0.827)
Avg. Age	-0.014	-0.014	-0.014
	(0.034)	(0.035)	(0.034)
Share Service	0.265	0.263	0.264
	(0.250)	(0.249)	(0.251)
Share Manufacturing	0.621	0.605	0.624
	(0.195)***	(0.189)***	(0.187)***
Number of Industries	118	118	118
R-squared	0.287	0.281	0.287

Notes: (i) Standard errors in parentheses; (ii) *** p<0.01, ** p<0.05, * p<0.1; (iii).

neous economic shocks across industries. In this case, the validity of our identification assumption is more straightforward.

3.3 A Simple Framework to Interpret the Estimated Effects

In this section we introduce a very simple model of entrepreneurs' decision to register their business. The goal here is to use this conceptual framework to help us interpreting the effects we identify with the empirical strategy outlined above. Even though the model refers to informal entrepreneurs' decisions to become formal, the main implications are also valid to the analysis of heterogenous employees deciding whether to start a formal business.

Let y denote entrepreneurs productivity and, to make notation simple, assume that entrepreneurs are infinitely lived and have discount rate β.[23] Since our empirical analysis focuses on entrepreneurs with up to one employee, we simply assume that production

[23]It is straightforward to modify this problem to consider entrepreneurs who work for T periods and then retire.

equals entrepreneur's productivity. We normalize the price of the final good to one, so that revenues are given by $r(y) = y$. Entrepreneurs can choose between being formal (IMP1 and IMP2) or informal (INF). Additionally, it is useful to characterize the previous relevant framework for formal entrepreneurs, which was given by the SIMPLES (SIM). The value function of each state can be described as follows:

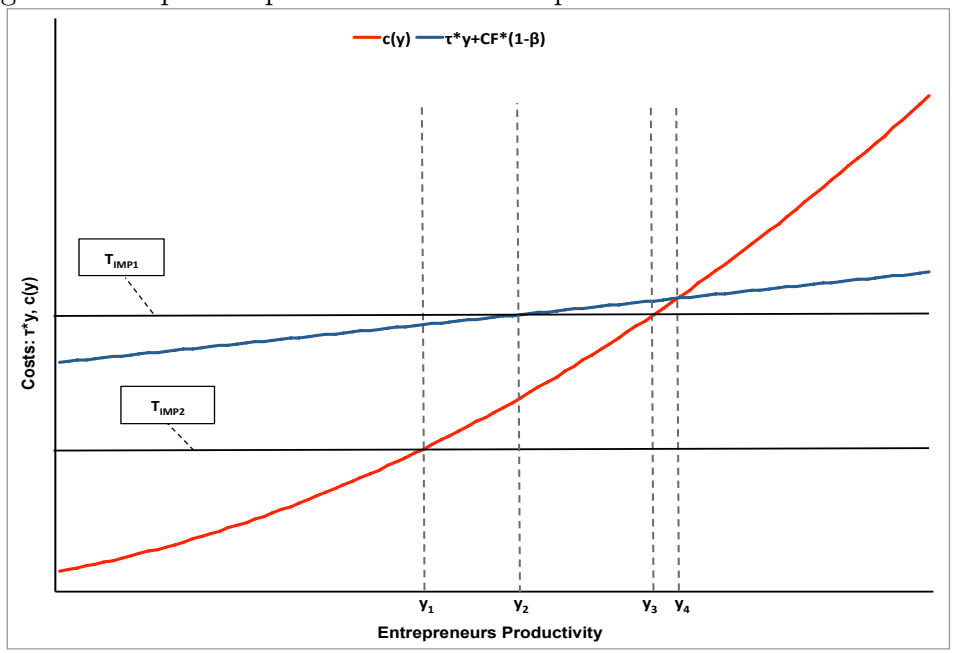
$$\begin{aligned} V^{INF}(y) &= \sum_{t=0}^{\infty} \beta^t \Pi^{INF} = \frac{\Pi^{INF}}{1-\beta} \equiv \frac{y - c(y)}{1-\beta} \\ V^{j}(y) &= \sum_{t=0}^{\infty} \beta^t \Pi^{j} = \frac{\Pi^{j}}{1-\beta} \equiv \frac{y - T_j}{1-\beta}, \quad j = IMP1, IMP2 \\ V^{SIM}(y) &= \sum_{t=0}^{\infty} \beta^t \Pi^{SIM} - C_F = \frac{\Pi^{SIM}}{1-\beta} - C_F \equiv \frac{(1-\tau)\, y}{1-\beta} - C_F \end{aligned}$$

where Π^k denotes monthly profits in state $k = INF, IMP1, IMP2, SIM$; $c(\cdot)$ is a function that summarizes the costs of being informal, where $c', c'' > 0$;[24] T_{IMP1} and T_{IMP2} denote the fixed monthly tax expenditures in IMP1 and IMP2, respectively; τ and C_F denote the monthly tax rate and the registration costs under SIMPLES, respectively.

With this simple structure, it is possible to characterize entrepreneurs' decisions in terms of productivity thresholds, as shown in Figure 4. Previously to the introduction of IMP1 (under the SIMPLES), an entrepreneur would decide to be formal if $V^{SIM}(y) \geq V^{INF}(y) \iff c(y) - \tau y \geq (1-\beta)C_F$, or equivalently if $y \geq y_4$ in Figure 4. After the IMP1 is implemented, entrepreneurs formalize if and only if $V^{IMP1}(y) \geq V^{INF}(y) \iff c(y) \geq T_{IMP1}$, or equivalently iff $y \geq y_3$ in Figure 4. The magnitude of formalization effects after the introduction of IMP1 thus crucially depend on the mass of entrepreneurs with productivity such that $y \in [y_3, y_4]$. For higher values of the fixed monthly tax expenditures (T_{IMP1}), we will have that $y_3 \to y_4$. Moreover, even though IMP1 eliminated entry costs ($C_F = 0$) it raised tax expenditures for a substantial share of entrepreneurs. Due to the regressive structure of IMP's taxation, high-income entrepreneurs are the only ones who perceived a decline in formalization costs under IMP1, which correspond to those whose productivity is such that $\tau y + (1-\beta)C_F \geq T_{IMP1}$, or equivalently $y \geq y_2$ in Figure 4. Finally, under IMP2 the costs of remaining formal, T_{IMP2}, were further reduced, which led to a *de facto* reduction in formalization costs for most eligible entrepreneurs. Under IMP2, entrepreneurs formalize if and only if $V^{IMP2}(y) \geq V^{INF}(y) \iff c(y) \geq T_{IMP2}$, or equivalently iff $y \geq y_1$ in Figure 4.

[24] This cost function can be motivated as the expected cost of being caught by the government, or any other opportunity cost of being informal (e.g. not having property rights, access to credit, among others). This is a common formulation in the literature, see for example de Paula and Scheinkman (2010) and Ulyssea (2013), among others.

Figure 4: Graphic Representation of Entrepreneurs' Decision to Formalize



Note: For this graph, we parameterize $c(y) = ay^2$. The thresholds are determined as follows: (i) $V^{IMP2}(y_1) = V^{INF}(y_1) \iff c(y_1) = T_{IMP2}$; (ii) $\tau y_2 + (1-\beta)C_F = T_{IMP1}$; (iii) $V^{IMP1}(y_3) = V^{INF}(y_3) \iff c(y_3) = T_{IMP1}$; (iv) $V^{SIM}(y_4) = V^{INF}(y_4) \iff c(y_4) - \tau y_4 = (1-\beta)C_F$.

In sum, this simple analysis shows that estimating IMP1's average effect does not identify the effect of reducing either registration costs nor taxes. This is so because the program eliminated entry costs but simultaneously increased tax expenditures for a substantial share of eligible entrepreneurs. Hence, overall formalization costs increased for some entrepreneurs (for those with $y < y_2$ in Figure 4) but decreased for others. The average IMP1 effect will be influenced by both groups, which implies that the net effect captured by the estimator will be ambiguous. Nevertheless, estimating IMP1's effects for different income levels identify the impact of eliminating registration costs, as tax rates remained nearly constant or were slightly reduced for higher income entrepreneurs under IMP1. Thus, finding a positive effect on business registration for higher income informal entrepreneurs can be interpreted as evidence that entry costs are indeed a binding restriction to formalization. Similarly, finding no effect provides evidence that reducing entry costs is not a sufficient condition to induce informal firms to formalize.

Under IMP2 most entrepreneurs perceived an actual reduction in tax rates. Therefore, estimating IMP2's average effect should identify the impact of reducing the costs of remaining formal ($T_{IMP1} \rightarrow T_{IMP2}$), *given that entry costs were already eliminated* ($C_f = 0$). Moreover, if the ongoing costs of being formal are the main factor driving entrepreneurs' decisions, we should observe formalization effects that are increasing in

entrepreneurs' income, as the relative cost of formalization is declining in income under IMP.

4 Results

4.1 Effects on Formalization of Informal Firms

We start by analyzing the effects of reducing entry costs (IMP1) and taxes (IMP2) on the formalization of informal entrepreneurs with up to one employee. Panel A of Table 5 reports IMP1 effects, while Panel B reports IMP2 effects. The first column includes time, industry and region fixed-effects, as well as time-varying observables at the entrepreneur level. Column 2 adds entrepreneur fixed-effects. The third column adds controls for convergence in formalization rates, while the fourth specification adds controls for fluctuations in economic activity. In column 5 we restrict the sample to those entrepreneurs who do not change industries between interviews, and in column 6 we cluster standard errors at the industry level, allowing for unrestricted residual correlation within industries. In all regressions our sample is restricted to entrepreneurs who were informal in their first interview, so that we estimate the effects of either IMP1 or IMP2 on the formalization of the *stock of informal entrepreneurs*.

The results in Panel A (Table 5) show that IMP1 had no statistically significant effect on the formalization of informal entrepreneurs, regardless of the specification used. Oppositely, Panel B shows positive and significant coefficients for all specifications considered. The point estimate remains stable when we include entrepreneurs fixed effects (column 1 to 2) but drops to 0.05 once we control for convergence to the mean effects (column 3). From column 3 through 6, the estimated effect remains nearly unchanged, regardless of the specification used. In our preferred specification, which includes the full set of fixed-effects and controls (column 4), the point estimate is 0.049. Since the average treatment intensity is 0.47, this result implies that IMP2 led to a 2.3 percentage points increase in the average entrepreneur's formalization rate ($= 0.049 \times 0.47$).

As discussed in Section 3.3, the estimates of IMP1 average effects do not identify the impact on formalization of a reduction in registration costs nor in taxes. On the other hand, IMP2 does identify the effect of reducing taxes on formalization, given that entry costs had been previously eliminated. In order to isolate the effects of reducing entry costs and taxes, we explore heterogeneous effects across entrepreneurs' income levels. In particular, if reducing entry costs is not a sufficient condition to induce firms to formalize, then we should find no effect from IMP1 on higher-income entrepreneurs. Similarly, if all formalization effects observed in IMP2 come from lowering the costs of remaining formal,

Table 5: Effects on formalization of informal entrepreneurs

	Dependent Variable: Formal Entrepreneur (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
IMP1	-0.057	-0.012	-0.024	-0.027	-0.033	-0.033
	(0.049)	(0.051)	(0.051)	(0.051)	(0.058)	(0.061)
Number of individuals	2,702	2,702	2,702	2,702	2,217	2,217
R-squared	0.126	0.165	0.205	0.210	0.198	0.198
			Panel B			
IMP2	0.078	0.079	0.050	0.049	0.049	0.049
	(0.019)***	(0.020)***	(0.020)**	(0.020)**	(0.021)**	(0.027)*
Number of individuals	2,336	2,336	2,336	2,336	1,916	1,916
R-squared	0.140	0.203	0.231	0.234	0.231	0.231
Time, region and industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Entrepreneur FE	No	Yes	Yes	Yes	Yes	Yes
Control convergence	No	No	Yes	Yes	Yes	Yes
Control economic fluctuation	No	No	No	Yes	Yes	Yes
Restrict to same industry	No	No	No	No	Yes	Yes
Cluster at industry-level	No	No	No	No	No	Yes

Notes: Significance *** p<0.01, ** p<0.05, * p<0.1. IMP1 corresponds to the estimated coefficient $\hat{\beta}_1$ of the interaction $Post1_{rt} \times Intense_s$ in (2). It corresponds to the effect of the program's first phase (IMP1), which reduced entry costs but failed to effectively reduce tax rates. IMP2 corresponds to the estimated coefficient $\hat{\beta}_2$ of the interaction $Post2_t \times Intense_s$ in (3). It corresponds to the effect of the program's second phase (IMP2), which maintained the benefits from IMP1 and effectively reduced the tax rate for most eligible entrepreneurs. All regressions control for time, region, and industry fixed effects. The different specifications are the following: (1) time, industry and region fixed effects, and time-varying observables at the entrepreneur level; (2) adds entrepreneur fixed effects; (3) adds controls for convergence in formalization rates; (4) controls for fluctuations in economic activity; (5) we restrict the sample to those entrepreneurs who do not change industries between interviews; (6) clustered standard errors at the industry level, allowing for unrestricted residual correlation within industries.

the effects across income levels should follow the pattern implied by IMP's tax structure. That is, we should observe stronger formalization effects for upper income entrepreneurs and lower or zero effects for lower income ones.

To examine these hypotheses, we divide the sample into quartiles of entrepreneurs' annual income and estimate our preferred specification for each quartile.[25] Table 6 presents the results. We observe in Panel A that IMP1 did not have any significant effect on formalization, irrespective of the income quartile considered. In particular, we observe

[25]The benchmark specification corresponds to the fourth column of Table 5.

no effect for entrepreneurs in higher quartiles, for whom tax rates remained roughly constant or were reduced. This result thus implies that reducing entry costs is not a sufficient condition to induce firms to register. If it was, we should observe positive effects of IMP1 on formalization amongst higher income entrepreneurs.

Table 6: Effects on Formalization of Informal Entrepreneurs: Heterogeneity by Income Level

	Dependent Variable: Formal Entrepreneur (0/1)				
	(1)	(2)	(3)	(4)	(5)
			Panel A		
IMP1	0.045	0.025	-0.183	-0.145	-0.111
	(0.039)	(0.057)	(0.119)	(0.219)	(0.221)
Number of individuals	2,702	673	674	674	681
R-squared	0.230	0.232	0.243	0.393	0.394
			Panel B		
IMP2	0.049	0.024	0.036	0.076	0.115
	(0.020)**	(0.031)	(0.034)	(0.041)*	(0.056)**
Number of individuals	2,336	585	582	580	589
R-squared	0.234	0.235	0.236	0.342	0.403
Sample	Benchmark	1st Income Quartile	2nd Income Quartile	3rd Income Quartile	4th Income Quartile

Notes: Significance *** p<0.01, ** p<0.05, * p<0.1. IMP1 corresponds to the estimated effect of the program's first phase (IMP1), which reduced entry costs but failed to effectively reduce tax rates. IMP2 corresponds to the estimated effect of the program's second phase (IMP2), which maintained the benefits from IMP1 and effectively reduced the tax rate for most eligible entrepreneurs. The benchmark specification corresponds to the fifth column in Table 5: time, industry, region and entrepreneur fixed effects; time-varying observables at the entrepreneur level; controls for convergence in formalization rates; controls for fluctuations in economic activity; sample restricted to those who never change industries.

Turning to IMP2 (Panel B in Table 6), we find that formalization effects vary substantially across income levels, and in the expected direction. The results show that the positive average effect (first column) is entirely driven by entrepreneurs in upper income quartiles. The point estimate increases monotonically as we move from the lowest to the highest income quartile, although the effects for those below the median are not statistically significant. In Table 7 we compute the mean effect on formalization rates for each income quartile. As expected, the effects for the 3rd and 4th quartiles are higher than

the average increase of 2.3 p.p., specially for the fourth quartile, with an increase of 5.9 p.p. in the average formalization rate. As shown in Table 7, however, the proportional effect is somewhat similar, as the baseline formalization rates across quartiles are also increasing in income.

Table 7: Effects on formalization of informal entrepreneurs by income quartiles (in p.p.)

	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
β_k	0.024	0.036	0.076 *	0.115**
$E\left[Intense_{si} \mid i \in \text{Quartile } k\right] \times 100$	55.3	44.2	43.9	51.2
$\text{Effect}_k = \beta_k \times E\left[Intense_{si} \mid i \in \text{Quartile } k\right]$	1.3	1.6	3.3	5.9
Share of Formal	13.1	20.1	34.0	55.3
Mean Effect: $\beta \times E\left[Intense_{si}\right]$		2.3		

Notes: Significance *** p<0.01, ** p<0.05, * p<0.1. The i indexes individuals and k income quartiles; β_k denotes the estimated coefficients by income quartile k in Table 6; β denotes the estimated coefficient for the entire sample, in the 5th column of Table 5.

4.2 Transitions into Entrepreneurship

We now examine whether the program fostered the creation of small businesses by increasing transitions from employment (both formal and informal) into entrepreneurship. More specifically, we test whether IMP induced formal and informal salaried workers to become formal micro entrepreneurs (with up to one employee).[26] Once again we focus on our benchmark specification, and restrict the sample to individuals who report being either formal or informal employees in the first interview. Table 8 shows that neither reducing entry costs (IMP1) nor taxes (IMP2) had any statistically significant effect on transitions into entrepreneurship. The same is true when we estimate the effects by income quartiles.

This result indicates that the sharp reduction in the costs of being a formal entrepreneur was not a sufficiently strong incentive to induce individuals to move out of employment and start a formal business. The results also point against a potential unintended consequence of the program: in order to avoid the costs from labor regulations, larger firms could in principle substitute a formal labor contract for a firm-to-firm contract with the same employee who would now be a "micro entrepreneur". This effect

[26]Because of the way our treatment variable is constructed, we only examine within-industry (at the 2-digit level) transitions. For the same reason, we do not analyze transitions out of unemployment and into entrepreneurship. We need individuals' industry affiliation in their first interview in order to construct the treatment variable, which cannot be done for the unemployment.

Table 8: Effects on Firm Creation by Income Level

	Dependent Variable: Formal Entrepreneur (0/1)				
	(1)	(2)	(3)	(4)	(5)
	Panel A - Formal Employee in 1st Interview				
IMP1	0.012	-0.002	0.011	0.038	0.046
	(0.008)	(0.003)	(0.008)	(0.050)	(0.024)*
Number of individuals	9,458	2,364	2,351	2,353	2,390
R-squared	0.031	0.046	0.076	0.077	0.112
	Panel B - Formal Employee in 1st Interview				
IMP2	-0.003	-0.002	-0.000	-0.000	-0.002
	(0.003)	(0.004)	(0.004)	(0.004)	(0.007)
Number of individuals	10,084	2,510	2,516	2,510	2,548
R-squared	0.022	0.032	0.043	0.065	0.071
	Panel C - Informal Employee in 1st Interview				
IMP1	0.034	0.009	-0.137	0.245	0.130
	(0.036)	(0.012)	(0.113)	(0.158)	(0.100)
Number of individuals	2,103	523	522	523	535
R-squared	0.111	0.359	0.174	0.296	0.299
	Panel D - Informal Employee in 1st Interview				
IMP2	-0.024	0.017	-0.034	-0.023	-0.086
	(0.018)	(0.011)	(0.022)	(0.034)	(0.056)
Number of individuals	1,908	477	473	475	483
R-squared	0.138	0.250	0.217	0.255	0.342
Sample	Benchmark	1st Income Quartile	2nd Income Quartile	3rd Income Quartile	4th Income Quartile

Notes: Significance *** p<0.01, ** p<0.05, * p<0.1. IMP1 corresponds to the estimated coefficient $\hat{\beta}_1$ of the interaction $Post1_{rt} \times Intense_s$ in (2). It corresponds to the effect of the program's first phase (IMP1), which reduced entry costs but failed to effectively reduce tax rates. IMP2 corresponds to the estimated coefficient $\hat{\beta}_2$ of the interaction $Post2_t \times Intense_s$ in (3). It corresponds to the effect of the program's second phase (IMP2), which maintained the benefits from IMP1 and effectively reduced the tax rate for most eligible entrepreneurs. The benchmark specification corresponds to the fourth column of Table 5: time, industry, region and entrepreneur fixed effects; time-varying observables at the entrepreneur level; controls for convergence in formalization rates; controls for fluctuations in economic activity.

would show up in the data as transitions from employment to entrepreneurship, and we would not be able to separate these competing stories. We find no evidence in support of either interpretation.

4.3 Discussion

In Section 3.3 we argued that estimating IMP1 average effects does not allow us to test whether reducing entry costs is a sufficient condition to induce firms to formalize. Due to the regressive structure of IMP taxation, however, the tax rates for upper income entrepreneurs remained roughly constant or decreased slightly in IMP1. Thus, if registration costs alone are a binding restriction to formalization, we should observe formalization effects in the upper income quartiles, in particular the fourth. As we do not observe formalization effects in any income quartile, our results indicate that reducing entry costs is not a sufficient condition to induce informal firms to formalize.

In IMP2 the ongoing costs of being formal were *de facto* reduced for most eligible entrepreneurs and therefore the average effect captures the impacts of reducing taxes, *given that entry costs were already set to zero*. Our results indicate that reducing the costs of *remaining formal* does have positive effects on informal entrepreneurs' decision to formalize. The effects are stronger for entrepreneurs in upper income quartiles, who perceived the highest reductions in tax rates. Nevertheless, we do not find any impact on firm creation.

Given that the program introduced strong incentives to formalization, the estimated effects are arguably low in magnitude. IMP1 brought registration costs to nearly zero while IMP2 established very low monthly tax expenditures. Moreover, it is important to mention that IMP2 introduced a *de facto* subsidy to social security contribution, since it remained at 11% of the minimum wage for self-employed and entrepreneurs with up to one employee outside IMP.

The natural question is thus why small business owners do not formalize in a larger scale. An increasingly frequent argument in the literature is that the perceived benefits of formalization are very low for small-scale entrepreneurs [e.g. Bruhn and McKenzie (2014)]. We assess this hypothesis in our context by directly estimating IMP's effects on entrepreneurs' income. This exercise corresponds to a reduced-form estimation of the effect of formalization on income. We repeat the same series of specifications as in Table 5, but use as dependent variable the log of entrepreneurs' income. As Table 9 shows, we do not find any significant effect of IMP1 nor IMP2 on income. This is consistent with perceived benefits from formalization being low for small firms.

These results are also consistent with and provide further insights to the findings of

Table 9: Effects on Income

	Dependent Variable: Log(Income)					
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
IMP1	-0.405	-0.142	-0.106	-0.040	0.109	0.109
	(0.239)*	(0.279)	(0.280)	(0.280)	(0.347)	(0.313)
Number of individuals	2,702	2,702	2,702	2,702	2,217	2,217
R-squared	0.071	0.040	0.043	0.049	0.030	0.030
			Panel B			
IMP2	0.122	0.100	0.071	0.069	0.086	0.086
	(0.106)	(0.103)	(0.101)	(0.104)	(0.110)	(0.081)
Number of individuals	2,336	2,336	2,336	2,336	1,916	1,916
R-squared	0.076	0.055	0.059	0.062	0.050	0.050
Time, region and industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Entrepreneur FE	No	Yes	Yes	Yes	Yes	Yes
Control convergence	No	No	Yes	Yes	Yes	Yes
Control economic fluctuation	No	No	No	Yes	Yes	Yes
Restrict to same industry	No	No	No	No	Yes	Yes
Cluster at industry-level	No	No	No	No	No	Yes

Notes: Significance *** p<0.01, ** p<0.05, * p<0.1. IMP1 corresponds to the estimated effect of the program's first phase (IMP1), which reduced entry costs but failed to effectively reduce tax rates. IMP2 corresponds to the estimated effect of the program's second phase (IMP2), which maintained the benefits from IMP1 and effectively reduced the tax rate for most eligible entrepreneurs. All regressions control for time, region, and industry fixed effects. The different specifications are the following: (1) time, industry and region fixed effects, and time-varying observables at the entrepreneur level; (2) adds entrepreneur fixed effects; (3) adds controls for convergence in formalization rates; (4) controls for fluctuations in economic activity; (5) we restrict the sample to those entrepreneurs who do not change industries between interviews; (6) clustered standard errors at the industry level, allowing for unrestricted residual correlation within industries.

De Mel et al. (2013). In their experimental setting, the authors find that eliminating formal sector's entry costs alone does not induce firms to formalize; only when provided with substantial monetary payments some entrepreneurs decide to register their business. Accordingly, our results indicate that the main restriction to formalization are not entry costs, but the costs of remaining formal. Hence, it is natural that entrepreneurs would require to be compensated beyond entry costs in order to formalize. Moreover, since red tape associated with tax payments was already eliminated under IMP1, our results show that the monetary costs are an important restriction to formalization.

Finally, we observe no effect of IMP1 and IMP2 on firm creation. This result indicates that a substantial reduction in formalization costs – both in the costs of entering the formal sector as well as in the costs of staying formal – is not a sufficient condition to foster the creation of new businesses. It is worth noting that before IMP these individuals already had the option to become informal entrepreneurs but chose to be employees. If formal and informal entrepreneurship are close substitutes for these individuals, then reducing the costs of formal entrepreneurship is indeed unlikely to have any effect on occupational choice.

5 Robustness Checks

We start our robustness analysis by performing falsification tests. For that, we artificially change the timing of IMP implementation, considering lags of one, two and three years relatively to the actual start date. For each false implementation date, we re-define the time window accordingly, using the same procedure as in our benchmark model. For example, in the first column in Table 10, Panel A, we present the coefficient of a triple-interaction between the potential of treatment intensity, time and region eligibility lagged one year relatively to IMP1's starting date. Panel B reports analogous estimates for IMP2, although in this case we do not have a clear placebo test since the coefficients are also partially capturing the effects of IMP1 over 2010. The second and third columns report the results from analogous regressions considering two and three years lags, respectively.[27] The higher-order lags allow for a true placebo test for IMP2, as the relevant time window in both cases is defined before the actual start date of the program. As Table 10 shows, we find no statistically significant effect in any of these regressions. Thus, there is no association between our variables of interest and formalization rates before the actual IMP implementation. This result reduces our concerns about the potential influence of confounding effects driven by unobserved trends, or even by heterogeneity in economic shocks and their consequences across industries and regions.

Table 11 provides an additional test for the potential influence of unobservable trends in our results. We repeat the same sequence of specifications as in Table 5, but now simultaneously include the variables IMP1 and IMP2 in the regressions, and do not impose any time restriction to our sample. We thus use the full sample of entrepreneurs with up to one employee, who were informal in their first interview, and who were interviewed twice over the 2006-2012 period. This specification allows us to capture the influence of time-trends with more precision, as we are able to explore the entire sample (6 years of monthly

[27] We are not able to build the placebo for IMP1 lagged three years because our sample begins in January 2006.

Table 10: Placebo Tests on Formalization of Informal Entrepreneurs

	Dep Var: Formal Entrepreneur (0/1)		
	(1) Lag 1 Year	(2) Lag 2 Years	(3) Lag 3 Years
		Panel A	
IMP1	0.032	-0.068	-
	(0.029)	(0.044)	-
Number of individuals	2,721	2,778	-
R-squared	0.187	0.178	-
		Panel B	
IMP2	-0.003	-0.003	0.020
	(0.017)	(0.015)	(0.016)
Number of individuals	2,556	2,792	2,698
R-squared	0.213	0.218	0.194

Notes: Significance *** p<0.01, ** p<0.05, * p<0.1. Regressions include time, industry, region and entrepreneur fixed effects; time-varying observables at the entrepreneur level; controls for convergence in formalization rates; and controls for fluctuations in economic activity.

data) rather than a time window around the program's implementation date. It also tests whether our results are robust to the way specify our sample in the main regressions. As Table 11 shows, the results display a similar pattern: we find no relationship between IMP1 and formalization, while IMP2 effects remain positive albeit slightly smaller in magnitude.

We also examine whether the results change if we relax the constraint of only looking at entrepreneurs with at most one employee. Even though this is a (potentially) sharp eligibility criterion, many small formal firms hire informal workers [see Ulyssea (2013)], who are invisible to the government. It is therefore possible that firms registered in IMP have more than one employee if they hire informal workers as well.[28] We run the same specification as in Table 5, but considering entrepreneurs with at most five employees. As Table 12 shows, the same patterns arise but the point estimates are slightly larger than in our main results. In the preferred specification the coefficient increases to 0.053

[28]Unfortunately the PME data set is not matched employer-employee, and therefore we cannot identify how many formal and informal workers each firm hires.

Table 11: Effects on Formalization of Informal Entrepreneurs: Full sample

	Dependent Variable: Formal Entrepreneur (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
IMP1	-0.019	-0.020	-0.013	-0.014	-0.037	-0.037
	(0.022)	(0.033)	(0.033)	(0.032)	(0.035)	(0.037)
IMP2	0.029	0.058	0.045	0.034	0.044	0.044
	(0.014)**	(0.017)***	(0.017)***	(0.018)*	(0.021)**	(0.020)**
Number of individuals	16,944	16,944	16,943	16,943	13,835	13,835
R-squared	0.067	0.134	0.152	0.169	0.189	0.189
Time, region and industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Entrepreneur FE	No	Yes	Yes	Yes	Yes	Yes
Control convergence	No	No	Yes	Yes	Yes	Yes
Control economic fluctuation	No	No	No	Yes	Yes	Yes
Restrict to same industry	No	No	No	No	Yes	Yes
Cluster at industry-level	No	No	No	No	No	Yes

Notes: Significance *** p<0.01, ** p<0.05, * p<0.1. IMP1 corresponds to the estimated effect of the program's first phase (IMP1), which reduced entry costs but failed to effectively reduce tax rates. IMP2 corresponds to the estimated effect of the program's second phase (IMP2), which maintained the benefits from IMP1 and effectively reduced the tax rate for most eligible entrepreneurs. All regressions control for time, region, and industry fixed effects. The different specifications are the following: (1) time, industry and region fixed effects, and time-varying observables at the entrepreneur level; (2) adds entrepreneur fixed effects; (3) adds controls for convergence in formalization rates; (4) controls for fluctuations in economic activity; (5) we restrict the sample to those entrepreneurs who do not change industries between interviews; (6) clustered standard errors at the industry level, allowing for unrestricted residual correlation within industries.

against 0.049 observed in our benchmark sample.

Finally, we only look at entrepreneurs with annual income below the eligibility threshold (below R$36,000 up to November 2011, and below R$60,000 after that). Again, we should not expect the income threshold to be binding since firms may register in IMP but easily under-report earnings. We follow the same specification as in Table 5, but only considering entrepreneurs below the income threshold. Table 13 presents the results. The same patterns arise but the point estimates are now slightly smaller than in our main results. In the preferred specification the coefficient decreases to 0.046 against 0.049 observed in our benchmark sample. This is expected once we excluded from the sample entrepreneurs from upper income quartiles, those who benefited the most from the IMP's regressive tax scheme.

Table 12: Effects on Formalization of Informal Entrepreneurs: Entrepreneurs with up to 5 Employees

	Dependent Variable: Formal Entrepreneur (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
IMP1	-0.080	-0.054	-0.058	-0.061	-0.065	-0.065
	(0.054)	(0.058)	(0.056)	(0.056)	(0.066)	(0.070)
Number of individuals	2,922	2,922	2,922	2,922	2,396	2,396
R-squared	0.161	0.187	0.230	0.235	0.231	0.231
			Panel B			
IMP2	0.085	0.088	0.055	0.053	0.055	0.055
	(0.019)***	(0.020)***	(0.020)***	(0.020)***	(0.021)**	(0.025)**
Number of individuals	2,516	2,516	2,516	2,516	2,057	2,057
R-squared	0.166	0.229	0.258	0.261	0.251	0.251
Time, region and industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Entrepreneur FE	No	Yes	Yes	Yes	Yes	Yes
Control convergence	No	No	Yes	Yes	Yes	Yes
Control economic fluctuation	No	No	No	Yes	Yes	Yes
Restrict to same industry	No	No	No	No	Yes	Yes
Cluster at industry-level	No	No	No	No	No	Yes

Notes: Significance *** p<0.01, ** p<0.05, * p<0.1. IMP1 corresponds to the estimated effect of the program's first phase (IMP1), which reduced entry costs but failed to effectively reduce tax rates. IMP2 corresponds to the estimated effect of the program's second phase (IMP2), which maintained the benefits from IMP1 and effectively reduced the tax rate for most eligible entrepreneurs. All regressions control for time, region, and industry fixed effects. The different specifications are the following: (1) time, industry and region fixed effects, and time-varying observables at the entrepreneur level; (2) adds entrepreneur fixed effects; (3) adds controls for convergence in formalization rates; (4) controls for fluctuations in economic activity; (5) we restrict the sample to those entrepreneurs who do not change industries between interviews; (6) clustered standard errors at the industry level, allowing for unrestricted residual correlation within industries.

Table 13: Effects on Formalization of Informal Entrepreneurs – Eligibility Criterion using Annual Income

	Dependent Variable: Formal Entrepreneur (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A			
IMP1	-0.083	-0.041	-0.047	-0.051	-0.057	-0.057
	(0.049)*	(0.050)	(0.050)	(0.049)	(0.057)	(0.058)
Observations	5,122	5,122	5,120	5,120	4,194	4,194
R-squared	0.107	0.151	0.178	0.182	0.169	0.169
			Panel B			
IMP2	0.064	0.070	0.047	0.046	0.042	0.042
	(0.018)***	(0.019)***	(0.019)**	(0.020)**	(0.021)**	(0.026)
Observations	4,397	4,397	4,397	4,397	3,587	3,587
R-squared	0.132	0.191	0.210	0.214	0.199	0.199
Time, region and industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Entrepreneur FE	No	Yes	Yes	Yes	Yes	Yes
Control convergence	No	No	Yes	Yes	Yes	Yes
Control economic fluctuation	No	No	No	Yes	Yes	Yes
Restrict to same industry	No	No	No	No	Yes	Yes
Cluster at industry-level	No	No	No	No	No	Yes

Notes: Significance *** p<0.01, ** p<0.05, * p<0.1. IMP1 corresponds to the estimated effect of the program's first phase (IMP1), which reduced entry costs but failed to effectively reduce tax rates. IMP2 corresponds to the estimated effect of the program's second phase (IMP2), which maintained the benefits from IMP1 and effectively reduced the tax rate for most eligible entrepreneurs. All regressions control for time, region, and industry fixed effects. The different specifications are the following: (1) time, industry and region fixed effects, and time-varying observables at the entrepreneur level; (2) adds entrepreneur fixed effects; (3) adds controls for convergence in formalization rates; (4) controls for fluctuations in economic activity; (5) we restrict the sample to those entrepreneurs who do not change industries between interviews; (6) clustered standard errors at the industry level, allowing for unrestricted residual correlation within industries.

6 Final Remarks

In this paper we test whether entry regulation and taxes hinder firm creation and formalization in developing countries. To this end, we estimate the effects of a large scale formalization program in Brazil, the *Individual Micro-Entrepreneur Program* (IMP). We build a panel of entrepreneurs to exploit time, inter-industry and inter-region exogenous variations in access to the program to separately identify the causal effect of reducing registration costs and taxes on formal firm creation. Contrary to most of the literature, we are able to discriminate whether the effects come from new formal businesses being created or simply informal firms moving into formality.

Taken together, our results indicate that registration costs and taxes are not the main barriers to firm creation nor to formalization of small informal businesses. We show that reducing registration costs is not a sufficient condition to induce small informal entrepreneurs to formalize nor to spur the creation of new formal businesses. We are also able to identify the impact of reducing the ongoing costs of formality in a context where registration costs had already been nearly eliminated. The results show that substantially reducing the tax burden for small formal businesses has modest effects on the formalization of existing informal firms and none on the creation of new formal businesses.

Our results are thus consistent with the view that a large fraction of low productivity informal firms perceive very small or no benefits at all from formality. Therefore, they do not choose to enter the formal sector even when formalization costs are substantially reduced. Indeed, a large fraction of firms in developing countries have only one or two employees,[29] and informality rates are highest among these firms. Thus, from a policy stand point, our results imply that policies aimed at reducing the costs of formality for small firms are likely to have limited effects on formalization rates in developing countries.

[29]In Brazil, entrepreneurs with at most one employee correspond to 89% of all entrepreneurs [data from the 2012 National Household Survey (PNAD), our own tabulation]. Bruhn and McKenzie (2014) provide evidence for other developing countries.

References

Besley, T. and R. Burgess (2004). Can labor regulation hinder economic performance? evidence from india. *The Quarterly Journal of Economics 119*(1), pp. 91–134.

Bleakley, H. (2007). Disease and development: Evidence from hookworm eradication in the american south. *The Quarterly Journal of Economics 122*(1), 73–117.

Botero, J. C., S. Djankov, R. L. Porta, F. Lopez-de Silanes, and A. Shleifer (2004). The regulation of labor*. *The Quarterly Journal of Economics 119*(4), 1339–1382.

Bruhn, M. (2011). License to sell: The effect of business registration reform on entrepreneurial activity in mexico. *Review of Economics and Statistics 93*(1), 382–386.

Bruhn, M. and D. McKenzie (2014). Entry regulation and the formalization of microenterprises in developing countries. *The World Bank Research Observer 29*(2), 186–201.

De Andrade, G. H., M. Bruhn, and D. McKenzie (2013). A helping hand or the long arm of the law? experimental evidence on what governments can do to formalize firms. Mimeo.

De Giorgi, G. and A. Rahman (2013). Sme's registration: Evidence from an rct in bangladesh. *Economics Letters 120*(3), 573 – 578.

De Mel, S., D. McKenzie, and C. Woodruff (2013). The demand for, and consequences of, formalization among informal firms in sri lanka. *American Economic Journal: Applied Economics 5*(2), 122–50.

de Paula, A. and J. A. Scheinkman (2010). Value-added taxes, chain effects, and informality. *American Economic Journal: Macroeconomics 2*(4), 195–221.

Djankov, S., T. Ganser, C. McLiesh, R. Ramalho, and A. Shleifer (2010). The effect of corporate taxes on investment and entrepreneurship. *American Economic Journal: Macroeconomics 3*(2), 31–64.

Djankov, S., R. L. Porta, F. Lopez-de Silanes, and A. Shleifer (2002). The regulation of entry. *The Quarterly Journal of Economics 117*(1), pp. 1–37.

Fajnzylber, P., W. F. Maloney, and G. V. Montes-Rojas (2011). Does formality improve micro-firm performance? evidence from the brazilian simples program. *Journal of Development Economics 94*(2), 262 – 276.

Kaplan, D. S., E. Piedra, and E. Seira (2011). Entry regulation and business start-ups: Evidence from mexico. *Journal of Public Economics 95*(11), 1501–1515.

Monteiro, J. and J. J. Assunção (2012). Coming out of the shadows? estimating the impact of bureaucracy simplification and tax cut on formality in brazilian microenterprises. *Journal of Development Economics 99*(1), 105–115.

Ulyssea, G. (2013). Firms, informality and welfare. University of Chicago, mimeo.