Live or let die: formality, firm survival, and credit access in Colombia

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Abstract

This paper evaluates the impact of a regulatory reform implemented in Colombia that aimed to promote the formalization of new start-ups by cutting down registration costs and taxes and aims to study startup survival determinants. Using a rich combination of administrative records that allow us to follow in time the universe of formal firms in the country, we analyze static and dynamic effects on new formal firm's creation and the quality of the new firms created by testing whether they had a higher probability of survival due to the reform, and whether the firms created faced fewer financial constraints through access to formal credit. We find that the reform increased the number of formal small firms registered by 30%, but we do not find evidence that the newly formalized firms that benefited from the reform had a higher probability of survival, which means that reducing formalization costs could not be enough to improve the quality of the new firms created. We, therefore, consider a statistical model to rationalize the determinants of firm size and firm survival.

JEL classification: O17, J38, O16, C21, C41
Keywords: Startups, entry regulation, firm formality, firm survival, firm size

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1. Introduction

There are multiple factors, influencing entrepreneurs' decisions to create a formal firm or whether to formalize a previously informal startup, such as high entry costs, regulatory barriers, potential to grow, stringency of enforcement, access to capital markets, among others, which makes the impact of narrow formalization policies so ambiguous (Jessen and Kluve, 2021; Ulyssea, 2018; Loayza, 2016). This raises the question of whether cost-reduction policies can have a long-lasting impact on formal firm creation and survival. Additionally, we do not have large body of evidence on the determinants of formal firm survival in a context of high firm informality.

This paper studies the impact of a formalization policy implemented in Colombia in 2010, which combines a reduction of registration costs along with a reduction of taxes for newly registered firms that complied with certain characteristics. We also evaluate the probability of survival of the new formal firms created after the policy throughout time and their access to the credit system. To do so, we use a rich combination of administrative records from the Integrated Record of Contributions to Social Security (PILA), information of Chambers of Commerce, and individual reports of all debtor transactions in Colombia. The methodology implemented is a difference-in-differences design and Cox Hazard survival model. We find that the reform increased the number of formal small firms registered by 30%, but we do not find evidence that the newly formalized firms that benefited from the reform had a higher probability of survival, which means that reducing formalization costs could be not enough to improve the quality of the new firms created. We suggest the mechanism behind our results come from the fact that the newly created firms experienced higher ex-ante heterogeneity, which means these startups mostly had bad business ideas which potentially do not have a technology that is scalable. Therefore, those firms were less suited for growth and more fragile if hit by bad shocks.

Like other developing countries, Colombia has a high prevalence of informal firms and high concentration of workers in self-employment and very small firms. In Colombia approximately 60% of firms are not legally registered as formal entities, between 50% and

60% of workers are informal, and around 95% of these workers are self-employed or work in firms of less than 10 workers (Fernández, 2018; Alfaro et al., 2020). This high prevalence of informal firms in Colombia is worrisome as informal firms are characterized for being smaller in size and exhibit lower productivity than their formal counterparts (Fajnzylber et al., 2007; Meghir et al., 2015; Ulyssea, 2018; Eslava et al., 2019).

Our results provide evidence that reducing entry barriers to the creation for formal firms and lowering some of the taxes levied on the firm, effectively induces an increase in the number of new formal firms by about 30%. Nonetheless, we find that the firms created due to the policy do not have a higher probability of survival in the formal sector. We hypothesize that these firms would probably otherwise not have been created but do so because of the cost-free registration and zero-taxes policy in the first year of creation.

However, we need to better understand why they exit formality as soon as the tax benefits start to decrease. We argue that the mechanism behind our results could come from the fact that small firms in general face ex-ante factors that set them in a path for failure, such as not having good business ideas to start with. Therefore, they get less access to the credit market and grow less, which makes them more prone to exiting formality. We empirically test this hypothesis using the statistical model for employment developed by Sterk et al. (2021), which allows us to decompose the determinants of employment dispersion among firms, between ex-ante heterogeneity and ex-post shocks. We find that for the case of Colombia, ex-ante heterogeneity, which is having good business ideas or a technology that allows for scalability, explains up to 65% of the variance of startup size, while ex-post shocks, which are negative productivity shocks or demand shocks that happen after the firm has been operating, explain 35% of it.

The main contributions of this paper are twofold. First, to add evidence to the literature that studies the effects of formalization policies that lower registration costs for newly formalized firms, but also lower firm taxes for the first few years after firm registration. Second, to study firm survival determinants in a context of high informality such as the one in Colombia. We also contribute to a strand of the literature that explores the relationship between regulation

laws conditional on firm size and productivity, such as Garicano et al. (2016), whose work finds that some productive firms that would have been larger without firm-size dependent regulation, choose to remain below the legal threshold to avoid these costs. In our current scenario, the benefits in terms of tax reduction and elimination of the cost of registration only apply to firms with at most 50 employees.

The rest of the document is organized as follows. The next section describes the literature review. Section 3 presents the institutional details of the policy of interest. Section 4 describes the data used in this study, and sections 5 and 6 discuss our empirical strategy and report our main estimates. Section 7 discusses a potential mechanism and section 8 concludes. The last section summarizes the main findings and concludes.

2. Related literature

Excessive costs of starting a formal firm may lead to low rates of formal firm creation and to high rates of informality (De Soto, 1987 and Djankov et al., 2002). Therefore, many authors have studied different policies that aim to reduce such barriers and costs to entry to the formal sector, but the evidence is mixed (Jessen and Kluven, 2021; Floridi et al., 2020).

Recent literature has focused on evaluating two types of formalization interventions: eliminating barriers to entry and reduction of taxes. The first type of interventions refers to policies aimed at reducing entry barriers either through a reduction in the number of steps a person has to take in order to open a formal business, a reduction in the number of days required to open a business, or a decrease in registration costs. The second type of interventions are related to a reduction in operational costs through lower payroll and corporate taxes.

The evidence on liberalization of entry shows that the implemented interventions have a moderate impact on new firm creation between 5% and 17% (Aghion et al. (2008) and Sharma (2009) for India; Bruhn (2011) and Kaplan et al. (2011) for Mexico; Branstetter et al. (2014) for Portugal; and Cárdenas and Rozo (2009) for Colombia). Additionally,

Branstetter et al. (2014) also find that the newly created firms after the de-regulation program were smaller and less likely to survive in the first two years than firms developed in the absence of the program.

Other authors have found mixed evidence on this point. For example, Ulyssea (2018) found for Brazil that reducing the costs of entering the formal sector has very limited effects on firm formality and no or even negative effects on worker formality, as there is a trade-off between the two margins of formality (firm and worker). In the same line, Bruhn and McKenzie (2013), and Klapper and Love (2010) found negative effects of de-regulation interventions and they argue that the reason for these results could be that the size of the reform was not enough. Yakovlev and Zhuravskaya (2013), whose study the effects of a liberalization reform implemented in Russia, found effects on employment and firm performance, but not on the entry of new firms.

In terms of the evidence on the reduction of firm taxes, Monteiro and Assunção (2006) evaluated the SIMPLES program in Brazil, which was designed to simplify bureaucracy and reduce tax rates for small and micro firms. The authors used firms in non-eligible sectors as a control group to analyze the effect of the program using difference-in-differences strategy and found effects only in one sector: the licensing of retail firms increased 13 percentage points while the other eligible sectors were not affected by the program. Fajnzylber et al. (2011) also evaluated the same program using a Regression Discontinuity Design, exploiting the discontinuity among the formality rates in eligible firms that started their operations after November 1996 and those that started before. They found that the program led to an increase between 6.4% and 7.5% in the number of firms registered as a formal business. Moreover, the program showed an increase in tax compliance and social security contributions.

Rocha et al. (2018) evaluated a different program in Brazil enacted in 2009, which reduced registration costs in the first phase and taxes in the second phase. They found that the first phase did not have a significant effect, but the second phase had a positive and significant effect on formalization, but not on formal firm creation nor survival. Thus, they claimed that only reducing entry costs has no effect on firm informality, whereas reducing taxes,

once registration costs have been eliminated, is effective in reducing firm informality, but the scope of such interventions is limited as the implied formalization elasticity is low and the cost-benefit analysis shows that the program led to net losses in tax revenues.

There is a third strand of the literature that finds that reducing barriers to entry to the formal sector is not enough, so many entrepreneurs in developing countries prefer to remain informal (Klapper and Love, 2010; Bruhn and McKenzie, 2013, 2014; Grimm and Paffhausen, 2015; Floridi et al., 2020; Jessen and Kluve, 2021).

In summary, the existing literature on the effects of entry regulation is mixed and depends on the institutional design of the policy itself. Therefore, this study contributes to show new evidence focus on the effects in the business startups and going further in analysis studying the performance in terms of survival and access to the financial system of firms created due to the intervention. None of the previous work studies what determines firm survival after entering the formal sector. We aim to shed light on that matter on this work.

3. Institutional background and the intervention

The Law of Formalization and Employment Generation (Law 1429 of 2010) was enacted by the government of Colombia on December 29, 2010, and it came into force in January 2011. The objective of this law was to promote employment creation and formalization of micro and small businesses in their initial stages. The law was enforced simultaneously in all cities and in equal extent. It introduced changes in several areas related to the liberalization of entry and reduction of taxes of newly formalized firms with less than 50 employees. In the following subsections we present the institutional context in Colombia and describe the regulatory reform implemented in 2010.

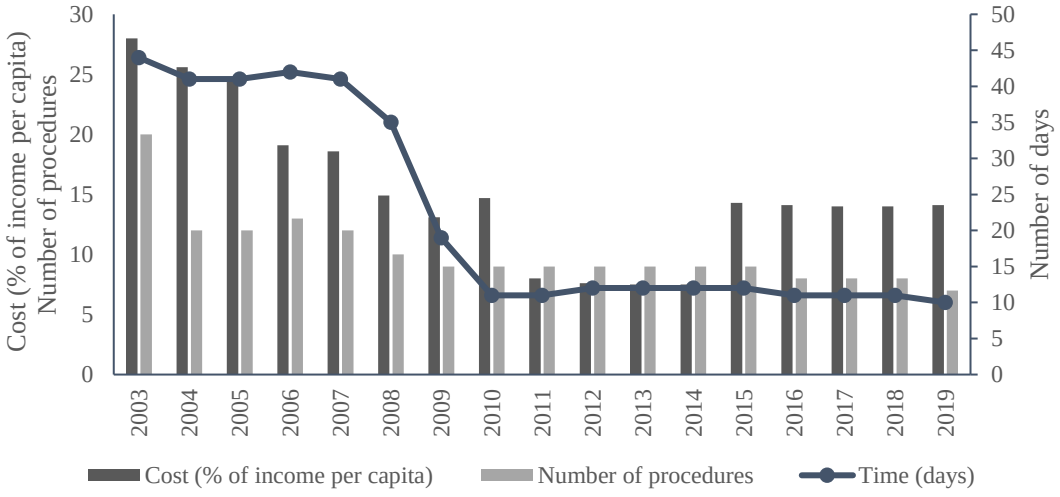
3.1 Colombia a country of firms' regulatory reforms

Prior to 2003, to start a new business in Colombia, in accordance with legal requirements, the entrepreneur had to comply with 19 procedures, which took about 60 days (World Bank,

2004). According to the Doing Business Report 2004, these requirements were excessive compared to other countries, and placed to Colombia as one of the countries with the highest number of procedures to start a business.

Over the period 2000 to 2010, Colombia implemented several reforms to boost its business environment. Figure 1 shows the evolution over time since 2003 of the number of procedures, costs, and days required to open a formal business in Colombia. On the left axis are the costs and procedures and on the right axis are the number of days. The number of procedures decreased quickly from 19 in 2003 to stabilize at around 9 after 2010. The number of required days was 44 in 2003 and decreased at a slower rate than the number of procedures until stabilizing at 11 after 2010. Finally, costs had a different path. They decreased substantially between 2011 and 2014 and increased again after 2015. In general, this behavior shows that the best conditions to open a new formal business took place between 2011 and 2014.

Figure 1. Evolution of regulation measures per year



Notes: The left axis represents a percentage in the case of costs and a number in the case of procedures. The right axis shows the number of days.

Source: Doing Business Database.

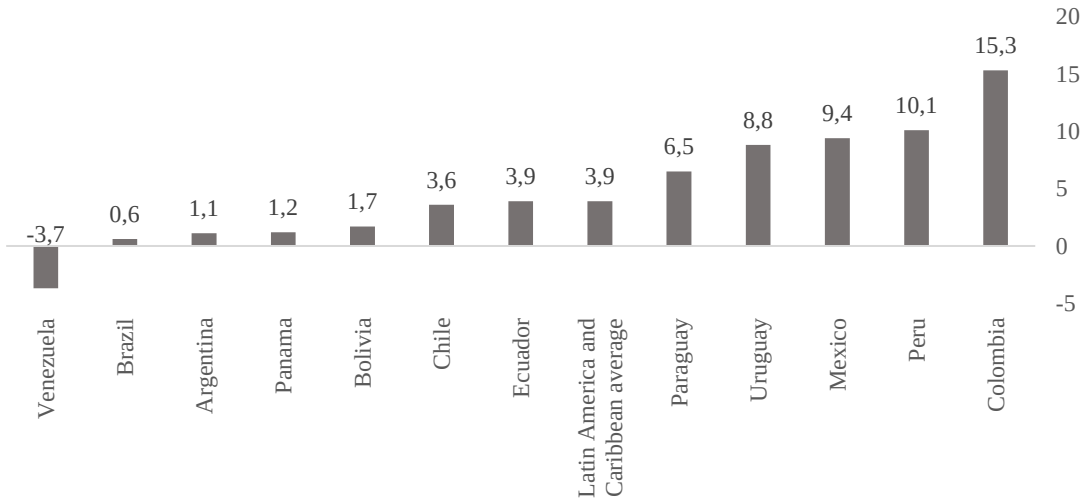
Among the main reforms that have been carried out in Colombia are the implementation of one-stop shops[1] in 2003, which simplified the process for registering formal businesses. Likewise, there was an improvement in the registration process with the introduction of the electronic system for payroll taxes, called the Integrated Record of Contributions to Social Security (PILA in Spanish), which allowed employers and independent workers to pay for social security and other mandatory contributions. It is important to add that in Colombia, both the employer and the employee have to make contributions to social security but it is in the hands of the employer the responsibility to collect the workers portion and make a joint payment in the system. This means that firms act as tax collectors in terms of social security, so that the PILA offers an important simplification of legal processes for firms and workers.

The progress made by Colombia in simplifying the processes for registering formal firms was very remarkable between 2007 and 2009, and it was recognized by the World Bank as one of the top 10 reformers countries (World Bank, 2007, 2008, 2009). In addition, the Doing Business 2013 report, recognizes the progress made by Colombia by making it a study case. According with this report, Colombia was the country that made more progress in regulatory practices than any other Latin American economy since the beginning of the Doing Business reports in 2003 (World Bank, 2012).

Figure 2 shows a comparison in the progress made in regulatory practice among Latin American countries. It shows, on average, an indicator of the progress of each country in narrowing the distance to the best performance achieved by any economy on each Doing Business indicator between 2005 and 2012. Thus, a larger number implies greater advances in terms of regulations. Therefore, by 2013 Colombia was the country in the region with greater progress.

[1] One-stop shops refer to offices that integrate several procedures into one with the aim of simplifying the process of starting a business.

Figure 2. Colombia's performance compared to other countries in Latin America



Notes: The graph shows, on average, an indicator of the progress of each country in narrowing the distance to the best performance achieved by any economy on each Doing Business indicator between 2005 and 2012. Thus, a larger number implies greater advances in terms of regulations.

Source: Doing Business 2013 (World Bank, 2012).

3.2 The intervention: Law 1429 of 2010

The Law 1429 of 2010 focus on three aspects. First, it sought to benefit young individuals under 28 years old with some education who wanted to start their own business by giving them financial aid and training. Second, it offered benefits in the reduction of payroll taxes, and registration and renewal fees to micro and small entrepreneurs who began their economic activity from the enactment of the law until December 31, 2014, when the validity of the law ended.[2] Third, it provided benefits to any entrepreneur who employs a person in a vulnerable situation, such as young employees, displaced people, and employees who earn less than 1.5 minimum wages.

In this study, we are specifically interested in the benefits for micro and small entrepreneurs. For purposes of the law, micro and small businesses are those with less than 50 employees; this was defined according to the firm size classification established in Colombia since 2000.[3]

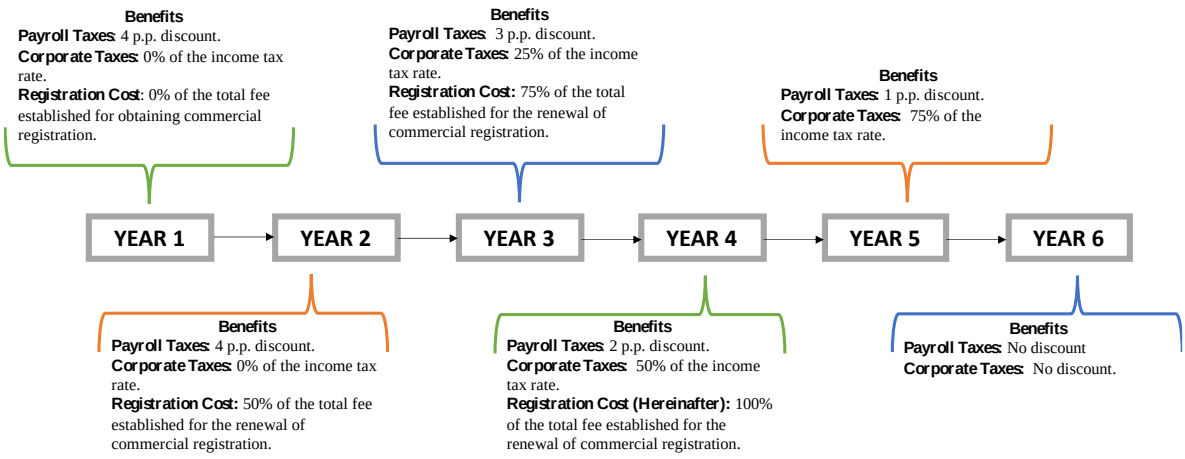
[2] The beginning of the economic activity is understood by the law as the formal registration of the firm in Chamber of Commerce.

[3] Law 590 of 2000 classified firm size in 4 categories: micro (up to 10 employees), small (between 11 and 50 employees), medium (between 51 and 200 employees), and large (more than 200 employees).

Only micro and small businesses first registered as a formal entity between 2011 and 2014 (period in which the law was in force) could access these benefits. Hereafter, we will call them new formal micro or small firms. That is, both new firms and former firms that were informal could access the benefit if they registered during the term of the law.

Specifically, the law states that new formal micro or small firms enjoyed of the benefits in terms of the reduction in payroll taxes in a phased fashion as follows: 0% of the charge for the first two taxable years; 25% in the third taxable year; 50% in the fourth year; 75% in the fifth year; and 100% from the sixth year onwards. In addition, targeted firms had a zero cost of registration at the Chamber of Commerce, which is the definition of firm formality in Colombia, and the benefit on the annual fee established for the renewal of the register also was reduced over time. Therefore, businesses only had to pay 50% of the total fee in the second year of economic activity, 75% the third year, and 100% from the fourth year onwards. For instance, an entrepreneur who went to the Chamber of Commerce in September 2011 to register a new business with less than 50 employees did not have to pay for the registration and the costs of payroll taxes were 0 for this first year. A year later, the entrepreneur paid only 50% of the commercial renewal fee and the costs of payroll taxes were 0 again. The costs continued to increase until reaching 100% by the sixth year. Figure 3 shows the timeline of the benefits of the law. All the benefits last for 6 years, but some of them end earlier than that.

Figure 3. Timeline of the law



Although Decree 545 specified that PILA and Chamber of Commerce should modify their tax collection forms to include the law's beneficiaries by March 2011, it was not until July 2011 that this option was included in the PILA form and then regulated in August through Resolution 3251 of 2011.

4. Data

In this paper, we use three sources of information. First, we use administrative data from PILA, which includes the universe of social security contributions and it is collected on a monthly basis by the Colombian Ministry of Health and Social Protection since 2008. PILA is a census that links information about all formal employers and employees. Therefore, we do not observe informal firms. PILA includes information about health insurance payments, pension contributions, wages, basic characteristics of the employee such as sex and age, and basic characteristics of the firm such as the economic sector and the city of activity.

Using PILA data, we construct an aggregated monthly panel data for the period July 2010 to December 2012 (30 months) with two dimensions: city and firm size categories. We use information for the 23 main cities of Colombia and firm size categories: micro (up to 10 employees), small (between 11 and 50 employees), medium (between 51 and 200 employees), and large (more than 200 employees) firms[4]. We excluded self-employed workers from our analysis because they are not the target of the law. Since we are interested in identifying the effect of reform through a difference in differences approach, we use small-sized firms as our treated group and medium-sized firms as our control groups, so that we focus the statistical analysis on these groups of firms. In section 5, we explain more in detail the selection of these groups. Additionally, we defined periods before and after of the law came into force, where pre-treatment period is defined as 6 months before the law between July 2010 and December 2010[5], and the post-treatment period is the period after the law and

[4] In Colombia the 23 main cities are: Armenia, Barranquilla, Bucaramanga, Bogotá, Cali, Cartagena, Cúcuta, Florencia, Ibagué, Medellín, Manizales, Montería, Neiva, Popayán, Pasto, Pereira, Quibdó, Riohacha, Santa Marta, Sincelejo, Tunja, Valledupar and Villavicencio. As robustness exercise we also construct a panel with more cities using all the cities with Chamber of Commerce.

[5] We found an atypical behavior in the number of registered firms in March 2010. There is an unexplained over-accumulation of new firms, so that we do not use data from before July 2010. However, our results are robust to the inclusion of the first six months of 2010.

takes 24 months between January 2011 and December 2012.[6] Therefore, this data consists of 23 cities by 30 months, which represents 690 observations for each firm size category.

From PILA data we are interested in two outcome variables: the number of new firms per month and the duration in months of these new firms. The first outcome is calculated using the monthly panel data, while for the second outcome, we use individual data from PILA and for each new firm created in the analysis period (between July 2010 and December 2012) is calculated the number of months that the firm remains registered in the PILA. Here we have firm individual data and calculated the duration in months of new firms for 7572 small firms and 680 medium firms.

The second source of administrative data comes from the Chamber of Commerce for the main four cities in Colombia: Barranquilla, Bogota, Cali and Medellín[7], and it includes all the firms formally registered with them for the period of interest.[8] It is important to mention that there are differences in the availability of information for each city, in particular, we do not have information on the constitution date of firm for all cities with which we cannot identify for some firms whether they were operating prior to the registration date. For Barranquilla we observe information about the constitution date, the registration and renewal dates and information about sector, assets and wealth. For Bogota, we only observe the period of registration, the period of renewal and the firm size, but we do not have information about constitution date, assets or related variables. For Cali and Medellín, we have the same information as in Bogota plus sector and assets. These differences in the availability of variables leave us only with the variables of firm's identification number, period of registration, period of renewal and the firm size as common variables in all datasets.

[6] In January 2013 there was a tax reform in Colombia, so that to avoid contamination on the law evaluated in this paper we selected the time period until December 2012.
[7] The main four Chamber of Commerce compress 70% of the total firms registered. We did not have access to information for other Chambers of Commerce.
[8] We cannot merge data from PILA with the information of Chambers of Commerce since PILA does not contain the firm's identification number.

Third, we use the census of all credit transactions which comes from Financial Superintendence of Colombia (*Superfinanciera*)[9] and contains information on the number and amount of credits. We merge this data of credit transactions with the information from Chambers of Commerce by using the firm's identification number with which we have all credit transaction for each firm. Information from the credit system is on a quarterly basis, then we construct a database where we observe the firms registered each quarter from July 2010 to December 2012. This data contains a sample of 4632 small firms and 621 medium firms and the outcome variable of interest a dummy variable which takes the value of 1 if the firm took a new credit and 0 otherwise.

In Table 1 we present the main descriptive statistics for each dataset built. Panel A and B correspond to the information on the new formal firms created and the duration in formality, while Panel C shows the information on the proportion of firms that acquired new credits. Panel A shows that in the period before the law, the number of new formal small firms created was 12.30 per month on average, while it was 1.25 for medium firms. In the post-treatment period we observe a greater increase in the number of new small-sized firms than new medium-sized firms: 16.92 vs 1.92, respectively. In terms of the duration in formality of new firms, Panel B shows that in the pre-treatment period the average duration for small firms was about 15 months and 19 months for medium firms. In the period after the law, there is a slight increase in the duration in formality of around 3 months for small and medium firms. Finally, Panel C indicates that only 0.9% of targeted firms by the reform (small firms) had a credit in the financial sector before the law was put in place, while after the law around 2% of small firm acquired a new credit.

[9] The Financial Superintendence (*Superfinanciera*) is a government entity in charge of supervising the financial and stock market system, promoting the solvency and discipline in the financial system in Colombia.

Table 1. Descriptive statistics

Variable	Mean pre-treatment	Mean post-treatment	Number of observations
A. New formal firms (monthly)			
Small (between 11 and 50 employees)	12.30	16.96	690
	(0.92)	(3.63)	
Medium (between 51 and 200 employees)	1.25	1.92	690
	(0.32)	(0.62)	
B. Duration (monthly)			
Small (between 11 and 50 employees)	14.77	17.01	7572
	(12.51)	(14.22)	
Medium (between 51 and 200 employees)	19.01	21.52	680
	(13.75)	(14.25)	
C. New credits (quarterly)			
Small (between 11 and 50 employees)	0.009	0.017	4632
	(0.095)	(0.131)	
Medium (between 51 and 200 employees)	0.009	0.050	621
	(0.098)	(0.219)	

Notes: This table shows mean and standard deviation in parenthesis of the outcome variables of interest. Panel A use aggregated monthly panel data from PILA, Panel B use individual firm information from PILA, and Panel C use individual firm information of Chambers of Commerce and Financial Superintendence of Colombia. We use 6 months prior to the beginning of the law as the pre-treatment period, and 24 months after the law as the post-treatment period.

5. Empirical strategy

In this section, we discuss our main empirical strategy. Similar to most studies in the literature (see Floridi et al. (2020) for a detail review of studies), we focus on the effects on new formal firms. At first, we analyze whether or not the law contributed to the creation of new firms. After that, we study their lifespan and the probability of having a credit in the formal financial system.

5.1 Treatment effects

The law of interest affected all the cities simultaneously. Nonetheless, not all firms were eligible for the benefits; only businesses with up to 50 employees first registered as legal entities between 2011 and 2014 were eligible. The aforementioned condition affects micro and small firms according to the legal firm-size classification well-established in Colombia.

As the law targeted small and micro firms as beneficiaries, we use small-sized firms (11 to 50 employees) as the treated group and medium-sized (50 to 200 employees) firms as the

control group. We do a sensitivity analysis using other combinations of firm size and our results are consistent. Unfortunately, we cannot estimate our model using data at the firm level because we do not observe if the firm existed before it appears on PILA (became formal). Thus, this prevents us from using the probability of starting a new formal business as an outcome. This means that difference in differences and discontinuous regression at the firm level are out of reach.

The equation that we estimate can be represented as:

$$y_{cst} = \alpha + \beta PostTreat_{ct} + \theta Treat_{ct} + \gamma_c + \delta_t + \varepsilon_{cst}, \quad (1)$$

where y_{cst} is the number of new formal firms in city *c* of firm size *s* in month *t*; $Treat_{ct}$ is a dummy variable equal to 1 if in city *c* and month *t* the firm size is small (treated group), and 0 if the firm is medium-size; $PostTreat_{st}$ is also a dummy variable which takes the value of 1 if in city *c* and month *t* the firm size is small in a month after December 2010, and 0 before this date. γ_c and δ_t represent city and month fixed effects, respectively, and ε_{cst} is a random error term. Standard errors are clustered at the city and firm size level.

In order to identify time-varying treatment effects of the law, we also estimate a dynamic difference in differences model (Autor, 2003; Borusyak and Jaravel, 2016; de Chaisemartin and D'Haultfoeuille, 2020). The equation in this case is a variant of our baseline model and present the following structure:

$$y_{cst} = \alpha + \sum_{\substack{k=-6 \\ k\neq -1}}^{24} \beta_k I(t = k \cap Treat_{ck} = 1) + \theta Treat_{ct} + \gamma_c + \delta_t + \varepsilon_{cst} \quad (2)$$

where $I(t = k \cap Treat_{ck} = 1)$ are indicator variables equal to 1 for each relative month *k* if in city *c* the firm belong to the group treated (small firms) between -6 (the months before the law came into force) and 24 (the months after the law). We can note that this model consists

in decomposing the average estimated effect for each relative month between -6 and 24. In practice, we omit $k = -1$ ($I(t = -1 \cap Treat_{ck} = 1)$), that is, December 2010, such that all β_k estimates are relative to the month prior to the law came into force.

The main identification assumption in our setting is that prior to the enforcement of the law, there are parallel trends on the outcome of interest between treated and control groups. To confirm the validity of our identification strategy, three tests were undertaken (Angrist and Pischke, 2009; Lechner, 2011; Cunningham, 2021). First, we run the parallel trends test slope, which includes differential linear pre-trends in the dynamic difference in differences model. If the parameter representing the difference in slope between the treatment and control group is not significant, this is evidence that supports the parallel trends assumption. Second, we implement the parallel trends test placebo, which checks in the pre-treatment period if there is a significant "treatment" effect prior to the intervention. If the joint significance test of the coefficients before the intervention are cero is not rejected, this suggests that the parallel trends assumption holds. Third, on estimates of the dynamic difference in differences model in equation (2) is possible to tests visually or statistically the hypothesis that $\beta_{-6} = \ldots = \beta_{-2} = 0$, with which the non-rejection of this hypothesis indicates that the decision to create a new formal firm prior to the law was not correlated with pre-existing trends, which increases confidence in our identification strategy.

5.2 Survival analysis

We would also like to shed some light on the extent that the law increases not only the number of new firms created, but also the likelihood of survival of said firms. For this, we use a Cox Hazard model, where we model the exit probability of a firm from formality in period *t*.[10] We estimate the model with a proportional Hazard of the form (Cameron y Trivedi, 2005):

$$h(t) = h_0(t) \cdot \exp(X'\pi) \tag{3}$$

The regressors in X are defined in equation (1). We keep only firms that were founded

[10] We can only know whether the firm left the formal sector because we no longer observe it in PILA, so that it is not possible to identify whether the firm closed definitively or not.

between the analysis period (July 2010 and December 2012). We have data in PILA until 2016, this means, that a firm created in the last period of analysis (December 2012) could be observed with a maximum of 49 months. In a bid to make fair comparisons between firms started up at different points in time, we keep only firms whose active life span did not exceed 49 months. Then, we estimate the hazard of leaving the formal sector following a similar specification as in our main estimates.[11] We compare treated (small firms) and control (medium firms) groups before and after the implementation of the law.

5.3. Credit access

We are interested in studying the credit behavior of new firms as a mechanism that could explain the lower survival rate of treated firms. We cannot use our sample from PILA given that firm identification numbers are not available to us, therefore we cannot merge this data with the credit system census. Thus, we use our sample from Chamber of Commerce at the firm level. In this case, we test whether firms targeted by the law have a higher probability of financial leverage using a dummy variable of new credits as dependent variable. The estimation strategy follows the same model as equation (1) but the data is at the firm level. Information from the credit system is on a quarterly basis. In this scenario, we estimate the probability that a firm targeted by the law has at least a new credit in its first quarter after its registration as a formal firm. We do a sensitivity analysis changing this time frame from the first quarter up to 12 quarters after registration.

6. Results

6.1 Effects on firm formality

Table 2 presents the results of estimates of difference in differences model for new formal firms under different samples. At the bottom of the table we present the parallel trends tests to assess whether groups have different trends in the number of new formal firms prior to the law. We observe that both tests, slope and placebo test, show a fail to reject the null hypothesis, which shows some evidence that the assumption of parallel trends is no violated. Additionally, the visual evidence in Figure 4, where we plot the full set of dynamic difference

[11] In this case, unlike previous estimates, we can use microdata at the firm level because we know the number of months a firm appeared in PILA. Thus, we can estimate the probability of exiting.

in differences estimates (β_k from equation (2)), shows that the estimated coefficients prior to the beginning of the law (period 0) are generally close 0. Using a F-test on the null hypotheses that $\beta_{-6} = \ldots = \beta_{-2} = 0$, we cannot reject this hypothesis (F-stat=1.50, P-value=0.2097). This suggests that in the months leading up to the law, there was no unusual trend in the number of new firms created, which increases confidence on our identification strategy: there is no evidence of differential trends in number of new formal firms across pre-treatment months.

Consider now the estimates of the causal impact of the law on the number of new formal firms in Table 2. Column (1) shows the results of estimating equation (1) using only small firms as treated and medium firms as control for the aggregated sample of the 23 main cities. This is our preferred specification because it restricts the potential differences between the groups making them as similar in composition as possible. We find that the intervention represents an increase of 32% in the number of new firms created. That is, a positive and significant effect of approximately 4 new formal firms per month with respect to the 12.3 formal firms per month created on average in the pre-treatment period.

Table 2. Estimates of difference in differences models for new formal firms

	Y = Number of new formal firms				
	Baseline	56 cities	Size 25-50 vs size 51-75	Micro-small vs medium-large	Chambers of Commerce
	(1)	(2)	(3)	(4)	(5)
PostTreat	3.98***	1.68**	0.96***	24.47**	6.71**
	(1.43)	(0.66)	(0.36)	(9.79)	(2.55)
Treat	11.05***	5.26***	2.36***	84.41***	28.08**
	(2.81)	(1.29)	(0.60)	(23.79)	(9.36)
Observations	1380	3280	1380	1380	240
R-squared	0.63	0.60	0.64	0.56	0.73
Mean dependent variable	12.30	5.74	3.01	85.81	34.02
Parallel trends tests					
Slope: t-stat	0.94	1.29	1.41	-0.42	-0.16
P-value	0.35	0.20	0.16	0.68	0.88
Placebo: F-stat	0.06	0.11	0.60	1.35	0.01
P-value	0.80	0.73	0.44	0.25	0.93

Notes: This table shows the estimates of the equation (1) under different samples. In column (1) are our baseline estimates where we use the 23 main cities of Colombia, and the small firms represent the treated group and the medium firms is the control group. Column (2) is same as column (1) but the sample is composed by the 56 cities of Colombia that have Chamber of Commerce. Column (3) uses firms between 25 to 50 employees as the treated group, while firms between 51 to 75 employees represent the control group. Column (4) uses micro and small firms as treated group, and medium and large firms as control group. Column (5) uses data from the Chamber of Commerce for the four main cities in Colombia: Barranquilla, Bogota, Cali and Medellín, and the treated and control groups are same as column (1). All estimates include city and month fixed effects. Standard errors clustered at the city and firm size level are in parentheses. *p<0.1; **p<0.05; ***p<0.01

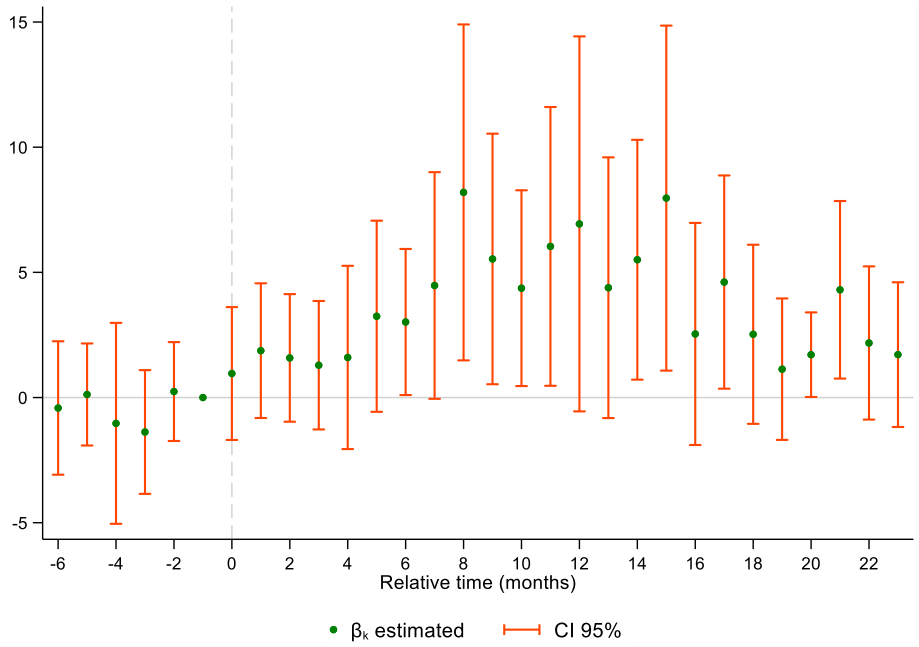
We estimate alternative models to test the robustness of our results under different samples. Column (2) presents the results of the estimation of equation (1) using information for the 56 cities in Colombia that have Chamber of Commerce. Similarly, as in the baseline model, we find that the law represents an increase of 29% in the number of new firms created. In column (3) we estimate the model using again the sample of the 23 main cities, but this time we take the firms that have between 25 and 50 employees as the treated group and whose firms that have between 51 and 75 employees as the control group. With this sample we find again a significant effect of 32% of new firms created after the intervention. In column (4) we use all firms with at least 50 employees (micro and small firms) as treated group and all firms with more than 50 employees (medium and large firms) as the control group. Likewise, we find a positive and significant effect which represents about 28% of new firms created.

Finally, in column (5) we repeat the same exercise as in the baseline model but using data from the Chambers of Commerce. As we already mentioned, in this case we have a limited sample since we only have access to the information for the Chambers of Commerce of Bogota, Medellin, Barranquilla, and Cali. Nonetheless, consistently with previous estimates, we find an increase of about 20% in the number of firms created. The above results indicate that our estimates are robust regardless of the sample we use.[12] Taken together, the results validate our main hypothesis that the Law 1429 from 2010 promoted firm formalization in Colombia.

Turning to the estimates of dynamic difference in differences model, in Figure 4 we plot the coefficients β_k estimated in equation (2). The coefficients prior to the beginning of the law (period 0) are generally not statistically significant, which, as mentioned, suggests that the decision to create a new firm prior to the law was not correlated with pre-existing trends. Coefficients from the first month onwards in Figure 4 show the effects for each month since the beginning of the law.

[12] We carry out the same exercise with different size firm categories and find consistent results in all cases. It is important to note that these exercises of arbitrarily grouping the number of employees to create treated and control groups do not guarantee that we are following the same groups over time. Hence, we prefer using the standard categories established in Colombia since 2000.

Figure 4. Dynamic difference in differences estimates
Number of new formal firms



Notes: This figure plots the estimated coefficients β_k of equation (2) using aggregated monthly panel data from PILA for the 23 main cities in Colombia. Data with seasonal adjustment. Relative year -1 (December 2010) is omitted, so that all estimates are interpreted relative to this year. The estimated include city and month fixed effects. Each point is a relative time parameter estimate, while the vertical lines are the confidence intervals at the 95% level calculated from standard errors clustered at the city and firm size level. The F-statistic for the period prior the law is 1.50 (P-value=0.2097).

An important feature of the law is that despite the fact that it came into effect in January 2011, it was not necessary to register a firm immediately to enjoy the benefits. Any new firm with up to 50 employees registered between January 2011 and December 2014 could benefit from the law. As it can be seen, the effects are especially evident after 8 month, i.e., August 2011. This is not surprising given that PILA only included the option to report beneficiaries of the law in their forms in August 2011. Beneficiaries prior to this date could be underreported. This implies that our results for the first few months could be underestimated. However, we still observe an increasing trend from the beginning, which becomes much more prominent after a few months, and decreases at the end.

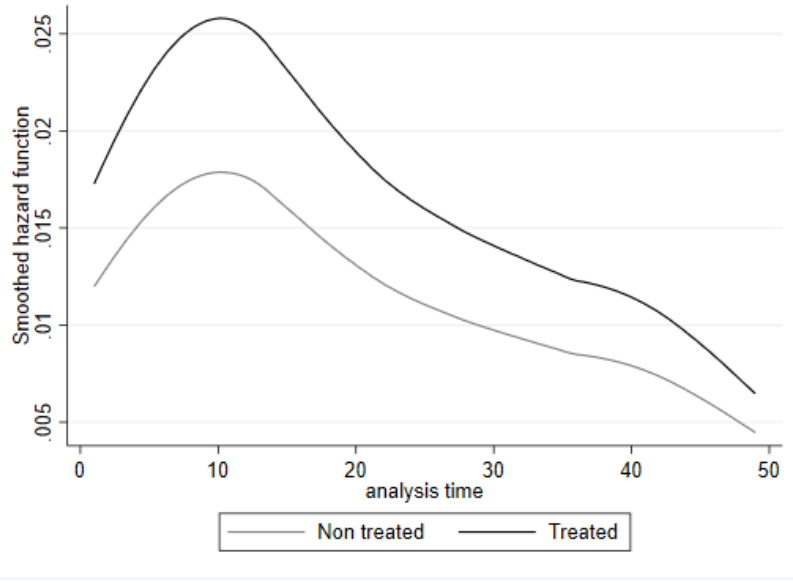
6.2 Survival in formality

The results up to this point show that the law created more formal firms. Now we want to test whether these firms stay open in the market at least as much as medium-sized firms. Regarding the results from the Cox hazard model, Figure 5 shows that treated firms (small

firms created after the implementation of the law) have a greater probability of exit from formality (to informality or stop operation altogether) than non-treated firms for the whole period of analysis. This implies that even with the benefits provided by the law, targeted firms do not have a greater probability of survival than larger firms. The probability of exiting increases in the first 12 months until reaching the maximum and then it starts to decrease.

These results show that even with the benefits received by the treated firms, it did not suffice to help with the survival. This could indicate that the type of firms that were established thanks to the benefits of the law were marginal firms, i.e. firms that otherwise would not have been formed, but which did so given the incentive of cost-free registration. Still, as the benefits were reduced, these firms that were probably not strong enough to stand on their own were more likely to disappear.

Figure 5. Probability of exiting the formal sector

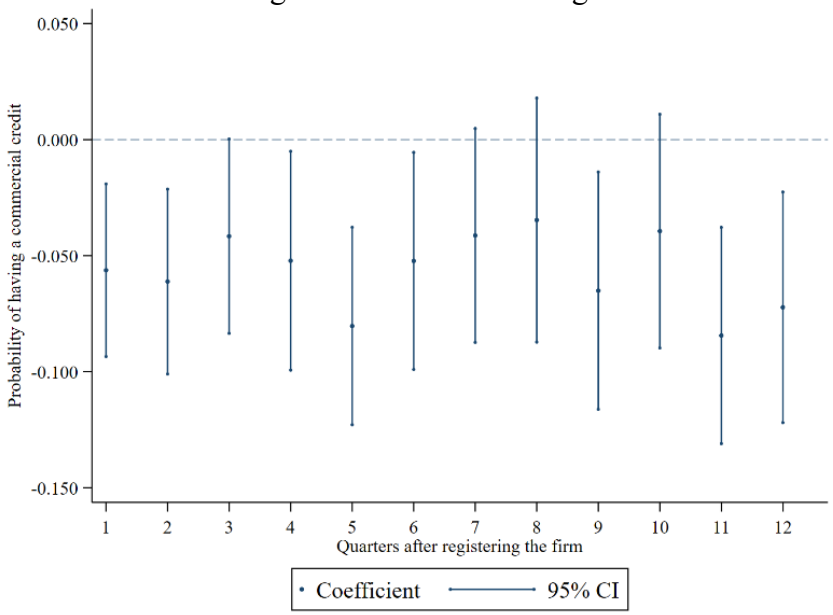


6.3. Credit access

In our last set of results, we test whether the firms created after the law are more likely to have a credit in the formal financial market. Figure 6 plots the coefficients and confidence interval for the estimates of the probability of having a new credit. In general, we find negative effects. That is, new formal firms do not have a greater probability of accessing the financial sector than non-treated firms. In fact, they have a lower probability. This could

explain why targeted firms do not remain open as formal firms longer than control firms despite having benefited from the law.

Figure 6. Financial leverage



These results are in line with the idea that the firms created due to the law are, in general, weaker in nature. They are less likely to get financial leverage through the formal banking system, but we cannot differentiate if these firms do not get credits granted despite having applied to it or if they are simply not trying to get any credit at all as we do not have data on credit applications but on approved credits.

7. Mechanisms

We use Sterk, Sedlacek and Pugsley (2021) statistical model for employment at the firm level to study what drives firm size differences across firms and how important it is ex-ante heterogeneity, understood as if the business idea is good or bad or if there is scalable technology, in contrast to ex-post shocks to demand or productivity, in order to determine growth profiles for startups.

Although we do not have a long time series of employment at the firm level, such as the one used by Sterk, Sedlacek and Pugsley (2021), we constructed a balanced panel of log employment at the firm level from 2008 to 2018 using data from PILA, which is an

administrative source, that allows us to recover the mid-run and long-run employment autocovariance at each firm. Year 2008 is age 0 of the firm as the PILA information system was created in that year and it is not possible to have access to such detailed administrative records from previous periods. Year 2018 is age 9 of the firm. We split the analysis in two cases: a) Only considering small and medium sized firms and b) including all firm sizes.

Figure 7 presents the cross-sectional autocovariance of log employment conditional on age of the firm for the two different samples we constructed. The left figure contains the standard deviations and the right figure the autocorrelations. We use a balanced panel that only include firms that survive the 10-year period of analysis and an unbalanced panel to account for firms that exit.

For the case of only including small and medium sized firms reported in Panel A, the standard deviation analysis shows that, contrary to what Sterk et al. (2021) report for the US scenario, firm exit does not play a role in the cross-sectional employment dispersion by age. In this case, standard deviations are between 0.6 and 0.75, which implies that the differences in firm size, although they grow with firm age, are not as large at young ages, at least when compared to the US. This dispersion does not change when including firms that exit at any point during the period of analysis. When we include all firm sizes, as reported in panel B, we do observe a difference in cross-sectional employment dispersion depending on whether we use the balanced or unbalanced panel. Firms in the balanced panel, exhibited lower log-employment dispersion than when we account for firms that did not survive in the formal sector.

Regarding the right-side figures, they show the autocorrelations of log employment between age a and an earlier age $h<a$. The outer curve closer to the origin, represent $h=0$. As found by Sterk et al. (2021), the autocorrelation declines with age for all h. For panel A, the autocorrelation is higher for the unbalanced panel, which could be caused by the firms that exit. While for the balanced panel, we document lower autocorrelations for all firm ages. For example, the autocorrelation for log employment for ages 0 a 5 is 0.55 and for firms ages 0 and 9 is 0.42. The autocorrelations do not appear to stabilize given the time span we consider.

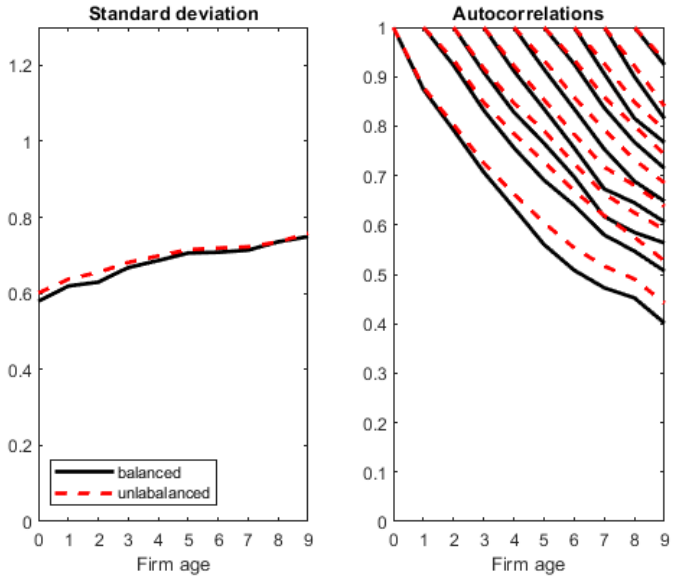
When we look at all firms sample in Panel B, the autocorrelations map for the balanced panel looks similar to the one reported by Sterk et al. (2021), as for firms ages 0 to 10 the

autocorrelation of log employment is 0.52, but we do not have a longer time horizon to see if the autocorrelation curves get flatter.

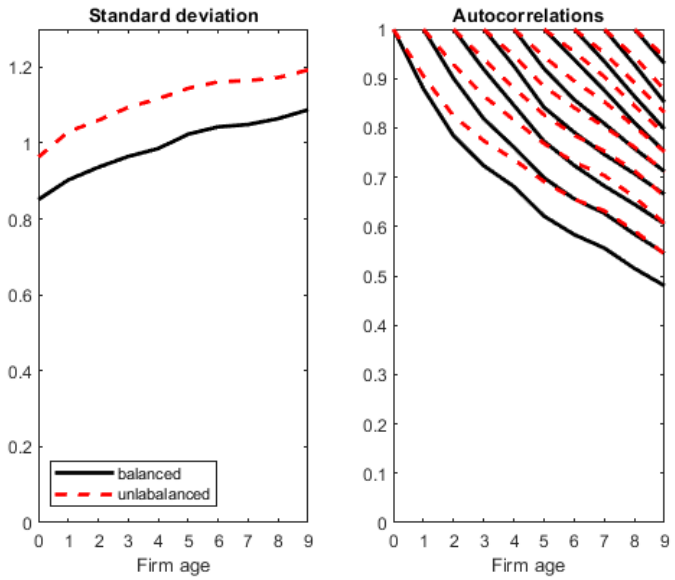
We also observe the empirical fact documented by Sterk et al. (2021) that the autocorrelations are increasing in age, as for Panel A (Panel B) for ages 0 to 4 the autocorrelation is 0.62 (0.65) and for ages 5 to 9 is 0.68 (0.73).

Figure 7. Standard deviations and autocorrelations of log employment by age, 2008-2018.

Panel A. Including only small and medium sized firms.



Panel B. Including all firm sizes.



Note: To determine firm size, we use the mode of the monthly number of employees at the firm for the period of analysis. Balanced refers to a panel of firms that survived at least up to age 9. Unbalanced refers to a panel of all firms, including those that exit.

Using the previous results about autocovariances, we follow the statistical model of employment proposed by Sterk et al. (2021). The baseline model abstracts from endogenous exit, but it combines both the ex-ante heterogeneity and ex-post shocks as determinants of employment.

Let $n_{i,a}$ be the employment level of an individual firm i at age a and consider the following process for this variable:

$$(4) \qquad \ln n_{i,a} = \underbrace{u_{i,a} + v_{i,a}}_{ex\ ante\ component} + \underbrace{w_{i,a} + z_{i,a}}_{ex\ post\ component} ,$$

Where:

$$u_{i,a} = \rho_u u_{i,a-1} + \theta_i , \quad u_{i,-1} \sim iid(\mu_{\bar{u}}, \sigma^2{}_{\bar{u}}), \quad \theta_{i,-1} \sim iid(\mu_\theta, \sigma^2{}_\theta), \quad |\rho_u| \leq 1,$$

$$v_{i,a} = \rho_v v_{i,a-1}, \quad v_{i,-1} \sim iid(\mu_{\bar{v}}, \sigma^2{}_{\bar{v}}), \quad |\rho_v| \leq 1,$$

$$w_{i,a} = \rho_w w_{i,a-1} + \varepsilon_{i,a} , \quad w_{i,-1} = 0, \quad \varepsilon_{i,a} \sim iid(0, \sigma^2{}_\varepsilon), \quad |\rho_w| \leq 1,$$

$$z_{i,a} \sim iid(0, \sigma^2{}_z)$$

In the process above, $\ln {n_{i,a}}^{EXA} = u_{i,a} + v_{i,a}$ captures the ex ante component of employment, where $u_{i,a}$ is a permanent part which converges to a certain level as the firm ages, and $v_{i,a}$is a transitory part which converges to zero. Note that both parts come with their own persistence parameter, ρ_u and ρ_v , respectively, which are common across firms.

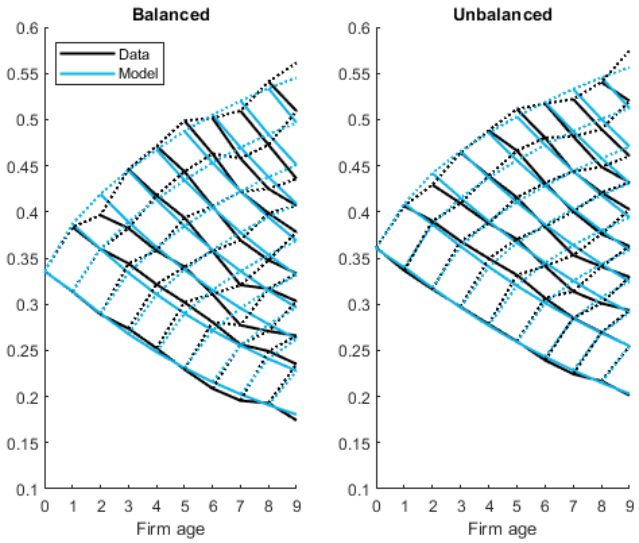
The ex-ante component is governed by three firm-specific constants, which are random and drawn independently just prior to startup, i.e., at age $a = -1$. The constant θ_i determines the firm-specific long-run level of the ex-ante component. The second and third constant, $u_{i,-1}$ and $v_{i,-1}$, represent two firm-specific initial conditions, corresponding to, respectively, the permanent and the transitory part of the ex-ante component

The ex-post shocks enter the model via a second component, $\ln {n_{i,a}}^{EXP} = w_{i,a} + z_{i,a}$. The process for the ex-post component is constructed such that its expected profile is flat and zero so that it does not capture any of the heterogeneity in ex ante profiles. Specifically, $w_{i,a}$ captures persistent ex post shocks and is modeled as an autoregressive process of order one, with i.i.d. innovations given by $\varepsilon_{i,a}$ and a persistence parameter denoted by $|\rho_w| \leq 1$. Notice that this formulation allows $w_{i,a}$ to follow a random walk, in which case each $\varepsilon_{i,a}$ may be interpreted as a growth rate shock. Because the u and v terms are meant to capture the entire ex ante profile, we normalize the initial condition of the persistent ex post shocks to $w_{i,-1} = 0$.

We follow Sterk et al. (2021) estimation method. Figure 4 shows the autocovariance structure in the data and in the estimated model. The figure has the same structure as the right panel of Figure 1, but plots autocovariances rather than autocorrelations (thus combining the information presented in Figure 3). For instance, the bottom solid line shows the autocovariance of employment at a certain age a with employment at age 0. Figure 8 shows that model fit is very good, correctly capturing the convexly declining pattern of the autocovariances in the lag length, given the initial age h, and the concavely increasing pattern in age given the lag length $j > 0$.

Figure 8. Autocovariance matrices: statistical models versus data

Panel A. Only small and medium sized firms.



Panel B. All firm sizes.

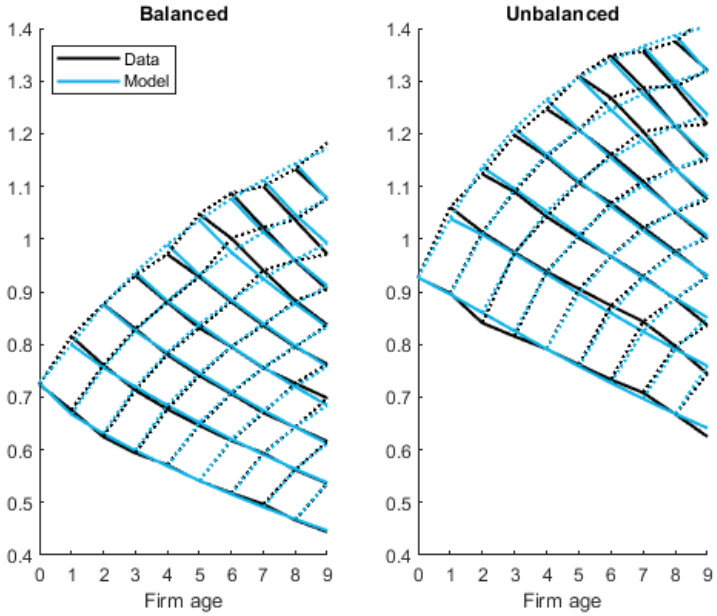


Table 3. Parameter estimates from reduced-form model

Panel A.

Balanced

Model	ρ_u	ρ_v	ρ_w	σ_θ	σ_u	σ_v	σ_ε	σ_z	*RMSE*
AR1_GEN	0.170	0.865	0.911	0.290	0.529	0.464	0.288	0.000	0.009
Unbalanced									
Model	ρ_u	ρ_v	ρ_w	σ_θ	σ_u	σ_v	σ_ε	σ_z	*R*MSE
AR1_GEN	0.263	0.900	0.925	0.250	0.445	0.508	0.274	0.000	0.006

Note: Small and medium firms. Annual data

Panel B.

Balanced

Model	ρ_u	ρ_v	ρ_w	σ_θ	σ_u	σ_v	σ_ε	σ_z	*RMSE*
AR1_GEN	0.245	0.943	0.940	0.267	0.866	0.718	0.379	0.087	0.008
Unbalanced									
Model	ρ_u	ρ_v	ρ_w	σ_θ	σ_u	σ_v	σ_ε	σ_z	*R*MSE
AR1_GEN	0.495	0.954	0.936	0.237	0.391	0.866	0.388	0.000	0.010

Note: Small and medium firms. Annual data

Using the model-implied cross-sectional covariance of employment derived by Sterk et al. (2021) presented in Equation 5, we can measure how important are the ex-ante heterogeneity and ex-post shocks for the cross-sectional dispersion in employment.

The covariance of employment of a firm at age a and at age $h = a - j$, where $0 \leq j \leq a$ a is the lag length, can be expressed as:

$$(5) \qquad cov\left[\ln n_{i,a}, \ln n_{i,a-j}\right] = \underbrace{\left(\sum_{k=0}^{a} \rho_u^k\right)\left(\sum_{k=0}^{a-j} \rho_u^k\right)\sigma_\theta^2 + \rho_u^{2(a+1)-j}\sigma^2{}_{\bar{u}} + \rho_v^{2(a+1)-j}\sigma^2{}_{\bar{v}}}_{ex-ante\ components}$$

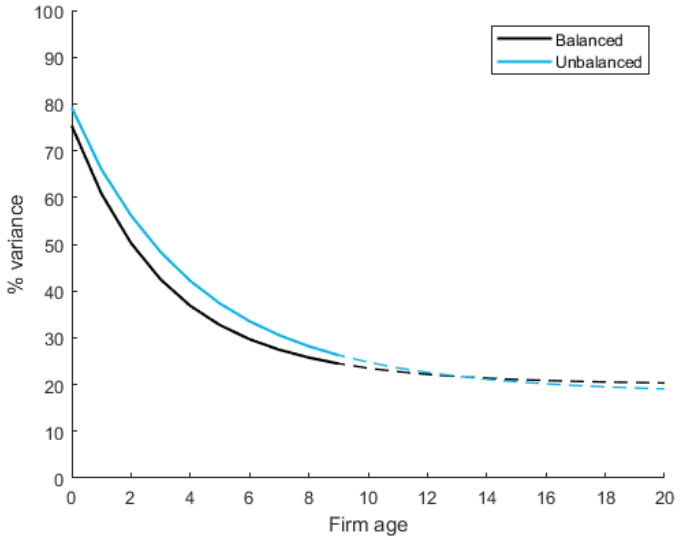
$$+ \underbrace{\sigma_\varepsilon^2 \rho^j \sum_{k=0}^{a-j} \rho_w^{2k} + \sigma_z^2 1_{j=0}}_{ex-post\ components}$$

The authors prove that, when setting the lag *j* to zero, Equation 5 provides a decomposition of the variance of size (log employment), at any given age *a*, into the contributions of the ex-ante and ex-post components. Figure 5 shows the fraction of the total variance in employment that is due to ex-ante heterogeneity. For small and medium sized firms, ex-ante heterogeneity can explain up to 75% of the dispersion in firm size at age 0 of the firm. The remainder is caused by ex-post productivity and/or demand shocks. When we consider all firm sizes, the ex-ante component plays even a more important role explaining up to 79% of the variance in firm size. In both cases, when we use the unbalanced panel, which include firms that exit, the ex-ante heterogeneity becomes more important, which suggests that this could play a determinant role in selective firm exit.

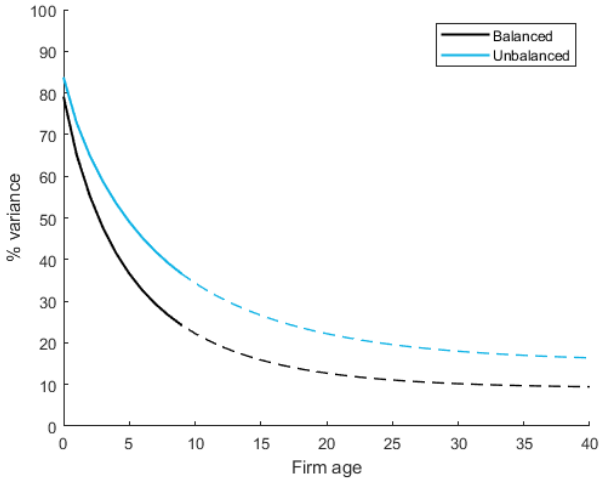
The importance of ex-ante heterogeneity in firm size declines with age of the firm as Figure 9 shows. Thus, at age 10 it explains between 25%-28% of the variance in log employment. For small and medium sized firms, the role of ex-ante heterogeneity stabilizes at 22%. When we use the panel with all firm sizes, we notice in the long-run a large wedge between the balanced and unbalanced panel that can be attributed to survival selection, explained mostly due to the exit of micro firms, which are the ones with the lowest survival rate.

Figure 9. Contribution of ex ante heterogeneity to cross-sectional employment dispersion

Panel A. Only small and medium sized firms.



Panel B. All firm sizes.



Note: Thick lines represent the actual data used for the estimation (ages 0 to 9) and dashed lines represent an extrapolation for firms at age 10-onwards using point-estimates.

8. Conclusion

This paper assesses whether a program aimed to promote firm formality by combining a reduction in entry costs and firm taxes effectively induces an increase in formal firms creation. To accomplish this, we studied a law put in place at the end of 2010 that eliminated registration costs for newly registered firms with less than 50 employees during the first year and also significantly cut down payroll and corporate taxes for the first two years of operation of the firm.

Our results indicate that there was a 30% in new formal firm creation. However, the results show that this policy was not enough to boost sustainable development of the new firms in time. Newly created formal firms have a higher probability of exiting formality, probably when the benefits started to decrease and the cost of operation in the formal sector starts to increase. We also find that targeted firms are less likely of having financial leverage through the formal credit system than those firms created without the benefits of the law, which could explain why these firms remain open in the formal sector for less time than medium-sized firms.

We suggest the mechanism behind our results come from the fact that small firms experience higher ex-ante heterogeneity than larger firms, which means these firms mostly start from bad business ideas, which potentially do not have a technology that is scalable. Therefore, those firms were less suited for growth and more fragile if hit by bad shocks. For our case of study, ex-ante heterogeneity explains up to 65% of the variance of startup size, while ex-post shocks, which are negative productivity shocks or demand shocks that happen after the firm has been operating, explain 35% of it.

Although the effects of the formalization policy we study are larger than what has been found in other contexts in which only registration costs are reduced, it is worrisome that newly formalized firms have lower survival probability than non-treated firms. Thus, we should study if the provision of mentoring to new entrepreneurs, jointly with such formalization policies and better access to the capital market would be a better strategy to promote long-lasting firm formality.

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