

Does Formality Improve Micro-Firm Performance? Quasi-Experimental Evidence from the Brazilian SIMPLES program

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Abstract: This paper employs Regression Discontinuity methods to identify the impact of the reduction of registration costs and taxes on newly born Brazilian micro firms. The introduction of the SIMPLES program in 1996 provides a quasi-natural experiment that permits us to eliminate many of the endogeneity issues surrounding the impact of registration on firm performance. We find that newly created firms that opt for operating formally employ more paid workers, are more capital intensive and exhibit higher levels of total factor productivity. Increased access to credit and Government provided technical assistance is not responsible for more than a small fraction of those formality effects. Rather, the observed greater willingness of formal firms to operate out of a fixed locale appears to be responsible for a large share of the formality-firm performance link. Further, the impact seems largest on poorly performing firms.

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I. Introduction

Starting with De Soto's (1989) seminal "The Other Path" barriers to participating in governmental institutions, and in particular, the very high costs of registering with the government, have often been seen as largely responsible for the presence of very large informal sectors in developing countries. Since de Soto found that a firm would need to spend 500 years complying with all necessary red tape in Peru, the World Bank Doing Business project has systematically collected data on registration costs for a large set of countries confirming that for most, the burden on firms is, on paper, very onerous.

But this said, establishing the existence of very high registration costs does not, in itself, establish either that this is why firms don't register, or that not registering is a fundamental determinant of average small firm performance. De Soto's telling anecdotes – e.g. the sidewalk vendor who wishes to pay his taxes as a way of securing quasi-property rights to his pitch – do suggest that high costs of formalization may impede informal firms from enforce their property rights, accessing public services and limit their access to markets, thus negatively affecting their performance. However, one must bear in mind that registration costs are only one of the factors that informal firms are likely to consider when assessing whether to enter or not the formal sector. And depending on the importance of other costs and benefits associated to formality, registration costs may not be *the* binding constraint for most informal firms. As argued by Levenson and Maloney (1998), if one accepts that formality broadly construed as participation in the institutions of civil society operates as a normal input in the production function of firms, it is possible that the intrinsic cost structure of many informal enterprises may never, in fact, dictate that they grow large enough to need those institutions.

This view is supported by recent evidence on Mexico by McKenzie and Woodruff's (2006). Using a survey of informal micro firms they show that the vast majority of them give as the principle reason for not being registered, not that it is too expensive or time consuming to do so (respectively 2 and 8 percent of surveyed firms), nor that the costs of operating as registered businesses are too high (4 percent of firms), but that they are too small to make it worth their while (75 percent). This leaves open the

alternative hypothesis that registration costs may, in fact, not be binding for most small firms and are at best a marginal contributor to informality. If this is indeed the case, further incentives may be needed to entice small firms to enter the formal sector, including for instance tax reductions, changes in labor market regulations, increases in government enforcement of regulations, and improvements in private and public services available to formal firms – e.g. credit, contract enforcement, technical assistance, etc.

This paper attempts to answer three questions. First, how much does a fall in registration costs coupled with a reduction and simplification of taxes and social security contributions affect firms' decisions to register (in the sense of obtaining an operating license). Second, how much does registration improve firm performance measured along several dimensions, including employment, capital intensity, and productivity. Third, what are the channels through which such registration gives rise to improved firm performance. For instance, formality offers the firm access to risk pooling mechanisms that may attract more educated paid workers and engage them in a longer relationship with the firm, which in turn makes training and capital goods acquisition more profitable. Formality may be a requirement for access to formal credit markets or Government provided business development services or, as de Paula and Scheinkman (2006) have argued, for subcontracting relations with formal firms.¹ Moreover, to the extent that formality increases the ability of micro-entrepreneurs to establish property rights over their investments, and reduces the risk of being fined by Government inspectors, it creates incentives for operating out of fixed locations rather than in an ambulatory fashion.

Serious endogeneity issues make establishing the effect of formality on firm performance difficult. Regressions of the latter on a formality dummy cannot tell us whether formality is improving performance or, alternatively, whether more productive and better performing firms – e.g. those belonging to entrepreneurs with higher

¹ Note, however, that formality may also reduce firms' flexibility to fire workers in the presence of negative shocks (see for instance Heckman and Pages, 2004). For Brazilian medium and large firms, Almeida and Carneiro (2005) found that stricter enforcement of labor regulation had a negative impact on firm performance. These findings are more in line with Gerxhani's (2004) and Loayza *et al.*'s (2005) view of informality as a tax-evading activity.

managerial ability – became formal to gain access to government and private services. Various approaches have been used to surmount the selection bias issue implicit in the second alternative. Fajnzylber et al.'s (2006) attempt for Mexico employs both matching and traditional control function methods to control for selection into formality using both observables and unobservables. Their results confirm that formality does rise with time in business and size, consistent with it being a normal input, and they find that registering with tax authorities does have an important impact on firms' profits and survival likelihood.

This paper employs an alternative complementary approach, using a natural experiment that allows estimating the impact of formality on firm performance on the basis of an exogenous change in registration costs and tax rates for small firms. To that end, we take advantage of the creation of the *SIMPLES*² system in Brazil, which starting in November 1996 consolidated multiple taxes and social security contributions into a single and reduced tax for eligible small firms. Similar simplified tax and/or registration regimes for small firms have been created over the past decade in Argentina and Mexico. In principle, they constitute natural experiments that offer increased potential for controlling for selection into formality.

In the case of Mexico, Kaplan et al. (2006) show that the creation of one stop shops to facilitate registration procedures for eligible micro, small and medium firms significantly increased the number of new formally registered firms. To identify the effects of this program, called SARE, they adopt a difference in difference approach using firms from non-eligible industries as a control group.³ Kaplan et al. show that the effects of SARE are concentrated in the first 10 months after its implementation, which suggests that the program does not affect the overall rate of firm creation but rather it operates through the formalization of existing informal firms. The magnitude of the

² SIMPLES stands for "Sistema Integrado de Pagamento de Impostos e Contribuções as Microempresas e Empresas de Pequeno Porte"

³ SARE stands for "Sistema de Apertura Rápida de Empresas." It was implemented in selected municipalities and consolidated in single local offices all the federal, state and municipal procedures needed to register a firm, reducing the total duration of the process to at most 48 hours. Kaplan et al. use data both from municipalities where the SARE was actually implemented and, as additional control groups, from other "competing" municipalities which were chosen to participate in SARE but where the program has not yet being launched..

SARE effect, however, is relatively small – between 4 and 8 percent increases in the number of new firms created or about 120 new jobs created per municipality – which suggests that only a small fraction of existing informal firms are enticed to formalized as a result of reduced registration costs.

For Brazil, Monteiro and Assunção (2006) exploit the same administrative simplification and tax reduction program used in the present paper (SIMPLES), applying a difference in difference approach with ineligible firms as a control group. They find that SIMPLES increased formal licensing among retail firms by 13 percentage points, but had no effect on eligible firms from other sectors (construction, manufacturing, transportation and other services). Moreover, using SIMPLES eligibility as an instrumental variable for formality, Monteiro and Assunção show that the latter significantly increases access to credit, and alters the amount and composition of investment towards larger and longer-term projects.

This paper builds on and extends Monteiro and Assunção's (2006) work in several ways. First, finding that much of the control group of firms legally prohibited from using SIMPLES surprisingly do use it anyway, we propose an alternative methodological approach to estimate the impact of SIMPLES on the rate of business licensing of Brazilian micro-firms. Second, using the Regression Discontinuity method, we provide new estimates of the effect of formality on firm performance. Third, we examine the importance of different channels through which registration could affect firm performance.

II. The SIMPLES Program

In December 1996, the Brazilian Government implemented a new simplified tax system for small firms, the SIMPLES (*Sistema Integrado de Pagamento de Impostos e Contribuições as Microempresas e Empresas de Pequeno Porte*). The new national system consolidates in a single payment all federal taxes and social security contributions

applicable to micro and small enterprises.⁴ Basically, the SIMPLES abridged procedures for the verification and payment of federal, state and municipal taxes. At the Federal level, the system allowed eligible firms to combine six different types of federal taxes and five different social security contributions into a one single monthly payment.⁵ As a result, SIMPLES permitted an overall reduction of up to 8 percent in the tax burden faced by eligible firms (Monteiro and Assunção, 2006). While value added taxes collected at the state and municipal levels – the *Imposto Sobre Circulação de Mercadorias e Prestação de Serviços (ICMS)* and the *Imposto Sobre Serviços (ISS)* – were initially not included in SIMPLES, States and Municipalities can enter into agreements with the Federal Government to transfer to the latter the collection of the corresponding taxes through an increase in the SIMPLES rates.⁶

As for the mechanisms for enrolling in the system, the law established that firms can opt for using SIMPLES either at the time of registering, or in the last weekday of January for those firms already registered under the old system. As in other countries, firms are subject to a series of penalties in case of not having a government-issued license. One important aspect of the new system is that it allowed to substitute a fixed (and relatively low) percentage of total invoicing for the standard payroll contribution, which led to a substantial reduction in labor costs and hence created a strong incentive to hire new employees and/or legalize already existing labor relationships.⁷

⁴ Micro-enterprises are defined as having maximum annual revenues of up to roughly \$100,000 and small enterprises up to \$1,000,000.

⁵ The taxes and contributions covered by SIMPLES are: *Imposto de Renda das Pessoas Jurídicas – IRPJ* (corporate income tax); *Imposto sobre Produtos Industrializados – IPI* (tax on industrialized products); *Imposto sobre a Exportação (Export tax)*; *Imposto sobre a Renda relativo a créditos de aplicações financeiras e ganhos de capital* (tax to profits from financial investments and capital gains); *Imposto sobre a Propriedade Territorial Rural – ITR* (rural property tax); *Contribuição para o PIS/PASEP* (employees' savings programmes); *Contribuição Social sobre o Lucro – CSLL* (social contributions on net profits); *Contribuição para o Financiamento da Seguridade Social – COFINS* (social security contributions); *Contribuição para a Seguridade Social a cargo de Pessoa Jurídica* (employers' social security contributions); and *Contribuição para a Seguridade Social relativa aos Empregados* (social security contributions related to employees) – Gonzalez (2006).

⁶ Ministerio de Fazenda (2006) and Gonzalez (2006)..

⁷ The SIMPLES imposed a contribution based on a fixed percentage of the firms' revenues that is independent on the number of employees and their salaries (González, 2006).

The motivation behind these reductions in direct and indirect taxes was to enable small, unskilled labor-intensive firms to compete more effectively with larger enterprises, for which high tax burdens are more manageable due to scale economies. SIMPLES, however, explicitly excluded all activities that by law require the employment of professionals with regulated occupations. Examples of ineligible activities include the manufacturing of chemical products, machinery and equipment, as well education, health, accounting, insurance and financial services, among others.

III. Data

We employ the Brazilian Survey of the Urban Informal Sector (*Pesquisa Economia Informal Urbana*, ECINF) collected in 1997 and 2003 by the Brazilian Statistical Institute (IBGE, *Instituto Brasileiro de Geografia e Estatística*). This survey is representative of all the urban self-employed and firm owners with at most five paid employees, excluding domestic workers. The stratified sampling design (in two stages) allows studying a population of units which are rare, heterogeneous and hard to detect in standard household surveys. Geographically, it covers all of the 26 Brazilian states, as well as the Federal District, and also each of the 10 Metropolitan Areas (Belém, Fortaleza, Recife, Salvador, Belo Horizonte, Vitória, Rio de Janeiro, São Paulo, Curitiba and Porto Alegre) and the municipality of Goiânia. In each of its two waves, ECINF interviewed roughly 50,000 households among which it found more than 40,000 individuals which reported owning a micro-enterprise.

ECINF allows a relatively precise description of the main firm and entrepreneur characteristics of Brazilian micro-enterprises (e.g. sector, revenues, profits, employment size, capital stocks and time in business). About 28 percent of Brazilian micro-entrepreneurs use their own (or their partners') homes to operate their businesses. Their most frequent sectors of activity are retail trade (26 percent of micro-firms) and personal services (20 percent), followed by construction (15 percent), technical and professional services (11 percent) and manufacturing (11 percent). Respectively 8 and 7 percent of micro-firms belong to the sectors of hotels and restaurants, and transportation.

Most firms are very small both in terms of revenues and employment. Thus, in 1997 the average and median monthly revenues of Brazilian micro-firms were U\$S 1,313 and U\$S 454, respectively. Six years later, accompanying the overall stagnation of the Brazilian economy, average and median micro-firm revenues were even lower, at respectively U\$S 1109 and U\$S 351 (in 1997 prices). As for employment size, pooling the two surveys, we find that 87 percent of all Brazilian micro-firms have no paid employees, and 79 percent have no employees or partners at all. As reported in Table 1, 10 percent of the surveyed micro-firms have one or two paid employees, and only 3 percent have between 3 and 5 paid workers. In those firms with at least one paid employee, roughly 22 percent of all workers are family members, almost two thirds of paid workers are non-registered – *sem carteira assinada* – and only 35 benefit from social security contributions.

ECINF allows investigating the degree of formality of Brazilian micro-enterprises in a number of dimensions. Following Montiero and Assuncao (2006), the definition of formality on which we focus is based on whether the firm has a state or county issued license to operate as a business. Being licensed is a basic legal requirement for operating as a business and also for issuing official invoices for tax purposes. As seen in Table 2, the fraction of licensed micro-firms increased only slightly between 1997 and 2003, from 23.5 to 24.2 percent. Nonetheless, when the sample is restricted to firms with paid employees, a much larger increase is observed during that period, from 30 to 49 percent.

Having a license, however, does not necessarily imply being registered as a micro-enterprise, filling tax declarations, or actually paying taxes. Thus, in both years only about 11 percent of micro-enterprises are registered as such, a fraction that increases to 34 percent for those with paid employees. While a similar share of micro-firms fill tax papers, in 1997 only 7 percent report paying any taxes (17 percent for those with paid employees), and 8 percent pay social security contributions (25 percent for those with paid employees).⁸ This suggests that formality is not an all or nothing decision, but rather one made across several dimensions, each of them involving a different set of costs and

⁸ All of these formality indicators exhibit slight reductions in 2003.

benefits. Arguably important among the benefits, is the possibility of gaining access to credit and participating in trade associations. These broader dimensions of formality are restricted to respectively 5 and 12 percent of firms in 1997 (11 and 25 percent among those with paid employees), and to 6 and 11 percent in 2003 (13 and 23 for firms with paid workers). Access to credit from formal financial institutions (i.e. banks) is however, much less frequent: 2.5 percent overall and 5 percent for those with some paid employees.

As we argued above, one of the main challenges in estimating the impact of formality on firm performance is the possibility that both may be correlated with the entrepreneurs' unobserved managerial ability. In particular, those micro-firm owners that start their business because they have been unable to find other jobs or because their families have been hit by negative external shocks are arguably less likely to have access to good business opportunities that would allow them to stay in business and succeed. Arguably, they are also less likely to incur in the costs associated with formalization.

Some evidence in this respect is presented in Table 3, which shows that individuals that became entrepreneurs to escape from unemployment are found less frequently among the owners of formal businesses (21 percent) than among those with operating licenses (32 percent). Similarly, among licensed formal enterprise owners there are fewer who report having started up to complement their family's income (12 percent of licensed firms and 21 percent of informal ones), and it is more common to find entrepreneurs that mention the search of independence as the main reason to start their business (28 vs. 17 percent among informal firms). On the other hand, as also reported in Table 3, a higher fraction of formal enterprises have plans to expand (45 vs. 37 percent among informal firms) and a lower number intend to abandon their business to search for salaried jobs (6 percent among licensed firms compared to 13 percent for non-licensed ones).

Interestingly, Table 3 also shows that only 1 out of 4 licensed business owners made no attempt at regularizing at the time of starting up, while 85 percent of informal

businesses did not try to regularize their firm when they begun operating. Thus, at least among Brazilian micro-enterprises, the decision of whether to operate formally or informally appears to be made in most cases at the time of starting up.

IV. Econometric Methodology

Estimation Strategy

The introduction of SIMPLES can be seen as an exogenous policy change that significantly altered the incentives to become formal for at least a subset of Brazilian micro firms. In this respect, to the extent that the rules for SIMPLES eligibility were not determined by considerations associated with the expected performance of the corresponding sectors, SIMPLES can be used as a natural experiment to estimate the impact of formality on micro-firm performance, while avoiding possible biases arising from self-selection into formality. Assuming that formality decisions are made at the time of starting up, one way of identifying such treatment effects is by constructing instrumental variables based on a combination of SIMPLES eligibility and an indicator of whether firms were created before or after SIMPLES implementation. When the instrument corresponds to an exogenous change in the environment (i.e. a natural experiment) which is believed to affect the treatment status, a local effect can be estimated by comparing the outcome with and without the change, and comparing it to the change in the treatment status. Such a model permits identifying local average treatment effects (Imbens and Angrist 1994). It is the approach adopted by Monteiro and Assuncao (2006).

This paper, however, adopts an alternative methodology.⁹ Indeed, we interpret the introduction of SIMPLES as giving rise to a regression discontinuity (RD) quasi experiment, with the date of introduction of the new system as the exogenously induced discontinuity. In such a framework, if the instrument corresponds to a tie-breaking experiment based on some continuous set of exogenous variables, the RD approach

⁹ The motivation for discarding this estimation strategy is described below. It is mainly associated to the low covariance between the available instrument and the endogenous firm licensing variable, as well as by the fact that a considerable number of firms from ineligible sectors surveyed in the 2003 ECINF report using the SIMPLES system.

allows to evaluate the impact of the treatment locally around the cut-off point (see Hahn, Todd and Van der Klaauw, 2001 for a discussion and definition and Van der Klaauw, 2002 for an illustrative application). This is the approach adopted in this paper.

In order to illustrate the impact of SIMPLES on the incentives to formalize, Figure 1 plots the percentage of firms licensed against their time in business. At the time the ECINF survey was collected, October 1997, the maximum time in business since the SIMPLES was initiated in December 1996 was 10 months and any firm older than that would have been started under the old system. Controlling for the effect of time and business on the probability of having an operating license, and looking at a window of one month on either side of the date of SIMPLES implementation, we find a significant jump of roughly 10 percent in the registration rate. In fact, with the exception of firms that started at the time of the survey, we see two very different patterns before and after SIMPLES implementation.

Econometric Model

In a general linear regression set-up, the outcome variable Y can be expressed as

$$(1) \quad Y_{i,t} = \alpha D_{i,t} + \delta t_i + \beta X_i + \theta_i + u_{i,t}$$

where i denotes the firm, t denotes time in business, D is a formality indicator, X are exogenous covariates, θ_i is a firm-specific unobserved component and u is an iid error component. In parallel with the program evaluation literature, we will refer to D as a treatment indicator. The parameter of interest is α , the treatment effect. In a naïve OLS framework, estimates of α are likely to be biased because there may exist a positive correlation between firms that receive the treatment and the unobserved component.

As mentioned above, the SIMPLES program introduces a discontinuity in the underlying process driving the decision to become formal that offers information that, in theory, can be exploited in several ways. Denote T as an indicator for whether a firm was created before or after the SIMPLES was implemented, such that $T_i = 1$ if $t_i \leq \bar{t}$ and

$T_i = 0$ otherwise (firms that have been in business for at most \bar{t} months were created after SIMPLES).

In principle, an instrumental variables (IV) approach could exploit T as an instrument for D . Following this line, Monteiro and Assunção (2006) use a difference-in-difference approach to evaluate the effect of this policy change on the likelihood of licensing. They construct an instrumental variable for business registration using the interaction of eligible/non-eligible and before/after indicators, and use it to compute the effect of formality on investment and credit access. There are two concerns with this approach. First, IV estimation is most effective with a considerable covariation of the instrument and the treatment indicator in order to have OLS-level standard errors. In practice, even when interacting the instrument with other firm level but exogenous characteristics, we found large standard errors and considerable instability in our estimates. In this light, it is hard to know how robust Monteiro and Assunção's very high estimates of a 30 percent increase in amounts invested are, only one year after SIMPLES. Second, we found a high degree of participation in the SIMPLES program among firms that were statutorily ineligible leaving us with some doubts about using that as a control group.¹⁰ In practice, this means that we may not be able to control for potential time specific effects.

As an alternative, we follow the quasi-experimental Regression Discontinuity approach (RD). If we assume very similar distributions of characteristics of firms born immediately before and after SIMPLES implementation, the discontinuity that the new system introduces in the factors determining formality (the treatment) can be exploited to provide unbiased estimates of the average treatment effect of the program. There are two main types of RD designs: the *sharp* design and the *fuzzy* design. In the former, the treatment varies in a deterministic way on some observable variable while in the latter,

¹⁰ The 2003 ECINF included a question regarding whether firms that are legally constituted have opted for SIMPLES. In practice, non-eligible firms may enter the system because of misclassification of their activities or low law enforcement. Of a total of non-eligible 335 firms, 185 (55 percent) declare to be in the SIMPLES, while the percentage rises to 80 percent for eligible ones. Unfortunately this determines that the non-eligible group cannot be used as a control group.

the probability of receiving the treatment (i.e. $P[D_i = 1 | t_i = t]$) has a discontinuity at $t_i = \bar{t}$.

As mentioned above, Figure 1 suggests a clear discontinuity in the probability of having an operating license (i.e. of being treated) before and after the implementation of SIMPLES. This, the theory suggests, should have an effect on a number of firm performance measures – the outcome variables Y . Figure 3 and Figure 4 confirm that this is indeed the case, with sizable jumps apparent around the time of SIMPLES implementation, both in the fraction of microfirms reporting fixed investments, and in the amounts invested by those firms.¹¹ Similarly, Figure 5 reports a small increase in the number of paid workers for the firms created around the time of introduction of the SIMPLES program.

A random sub-sample of individuals within a very small interval around the cutoff determined by the implementation of SIMPLES can be thought of as being similar to a randomized experiment. Indeed, those firms have very similar values for time in business and other covariates affecting firm performance, and they are likely to differ only on whether they benefit from the creation of SIMPLES. Estimating the coefficient “ α ” for this interval provides a local treatment effect, and the corresponding inference about the impact of formality is only valid in an arbitrary close (local) interval around the date of SIMPLES introduction. Increasing the interval around the cut-off point increases the number of observations and hence, reduces the variance of the estimators, albeit with the possibility of higher variance. For this reason, observations further away from the cut-off point are generally given a smaller weight in the estimation.

For a fixed value of covariates, a RD design can be implemented to identify the parameter of interest by means of the following estimator (see Hahn, Todd and van der Klaauw, 2001).

¹¹ Figures 3 to 5 are based on the sample of eligible firms surveyed in the 1997 ECINF that were created between 10 months before and 10 months after SIMPLES.

$$(2) \quad \alpha(X = x) = \frac{\lim_{t \downarrow \bar{t}} E[Y | t, X = x] - \lim_{t \uparrow \bar{t}} E[Y | t, X = x]}{\lim_{t \downarrow \bar{t}} E[D | t, X = x] - \lim_{t \uparrow \bar{t}} E[D | t, X = x]}$$

This estimator is similar to a local Wald estimator, that is, an instrumental variables estimator on an arbitrary small interval of the cut-off point, where a before-after indicator is used as instrument. The RD approach requires relatively mild conditions for identification and consistent estimation. In particular, only continuity in the exogenous covariates and unobservables is needed to identify the local effect.

We apply this procedure to estimate the impact of formality on firm performance. Our strategy consists in using matching techniques to locally estimate the numerator and the denominator. The use of this technique is justified under the assumption of random assignment to any side of the cutoff time. We match firms just before the SIMPLES was implemented to firms just after¹², using the set of exogenous covariates X . Then, for each firm i with outcome and treatment values $Y_i(T = 0, X = x)$ and $D_i(T = 0, X = x)$, we construct the counterfactuals $\hat{Y}_i(T = 1, X = x)$ and $\hat{D}_i(T = 1, X = x)$. The difference of the outcome variable can be expressed as:

$$(3) \quad Y_i - \hat{Y}_i = \alpha(D_i - \hat{D}_i) + \delta(t_i - \hat{t}_i) + (\theta_i - \hat{\theta}_i) + (u_{it} - \hat{u}_{it})$$

Differences in the matched outcome can be attributed to differences in the treatment indicators, time in business or unobservable characteristics. If the matched pairs are done not only on observable characteristics, but on $(\theta_i - \hat{\theta}_i) + (u_{it} - \hat{u}_{it})$ as well, $(D_i - \hat{D}_i)$ would represent an exogenous variation in the treatment which can be used to identify the parameter of interest. In principle, there is no reason for expecting significant differences in the average of (θ_i) and (u_{it}) across samples of firms that differ only in the fact that they were created immediately before or after a given date. However, in order to

¹² Since the effect of the SIMPLES may have affected the X distribution, we use the before-SIMPLES subsample to match the after-SIMPLES individuals.

rule out potential endogeneity biases coming from the correlation between $(D_i - \hat{D}_i)$ and the composite error term in (3), the change in the cost of registration after SIMPLES cannot affect the self-selection mechanism into the treatment in terms of θ_i or u_{it} . In other words, conditional on some level of covariates, $(\lim_{t \downarrow \bar{t}} D - \lim_{t \uparrow \bar{t}} D)$ should be independent of (θ, u) , meaning for instance that the impact of SIMPLES on the incentives to register should not depend on unobserved managerial ability. This assumption is weaker than the conditional independence assumption required for selection on observables methods like matching, although of course, the local nature of the parameter to be estimated is also narrower.

The local average treatment effect in (2) is conditional on some level of covariates. In order to estimate the unconditional effect we run a local weighted regression of $(Y_i - \hat{Y}_i)$ on $(D_i - \hat{D}_i), (t_i - \hat{t}_i)$ and X_i , where a kernel weighting scheme is used to increase the sampling importance of observations closer to the cut-off point. Therefore the regression model considered is:

$$(4) \quad Y_i - \hat{Y}_i = \alpha(D_i - \hat{D}_i) + \delta(t_i - \hat{t}_i) + \gamma X_i + \varepsilon_i$$

The estimated coefficient α is the effect of the exogenous change in the treatment status, $(D_i - \hat{D}_i)$, on the outcome of interest, that is not related to differences in time in business nor other observable covariates.

An additional concern is that the treatment may be correlated with some covariates not in X , which are potentially affected by the treatment. A typical example of those is labor and capital. On the one hand, bigger firms have a higher likelihood of becoming formal to ensure property rights over the capital stock and they also have higher likelihood of being detected by the tax authority. On the other hand, the treatment may influence positively the firm's size. We will denote these covariates as Z . In order to control for those variables we add Z to the regression model (4) to obtain:

$$(5) \quad Y_i - \hat{Y}_i = \alpha(D_i - \hat{D}_i) + \delta(t_i - \hat{t}_i) + \gamma X_i + \eta Z_i + \varepsilon_i$$

As in Fajnzylber et al. (2006) by adding Z to the model we disentangle the potential effect of those covariates on the outcome of interest. In this case, provided that Z may contain the effect of having a license, we are estimating a *lower bound* on the treatment effect. That is, it captures the treatment effect not related to Z. For example, when estimating the impact of formality on revenues, by conditioning on labor and capital, α is interpreted as the effect of becoming formal on the firm's Total Factor Productivity. Following this line of reasoning, if the model is estimated without Z (i.e. eq.(4)), we have an *upper bound*, because it also includes the *indirect* effect coming through Z.

We focus on relatively new firms on either side of the cut-off point, and assume that the decision to register is made at the time of starting the business or soon after – as suggested by the 2003 ECINF (see the above discussion of the right lower panel of table 3). Note, however, that Figure 1 shows that the average level of registration of firms created more than 6 months after SIMPLES implementation was not higher than among older firms which started before SIMPLES. Figure 1 replicates Figure 1 using the 2003 ECINF survey, focusing on the sample of firms that were created in the 10 months before or after the December 1996 implementation of SIMPLES. Despite a high variance in registration rates on either side of the November 1996-January 1997 window, those firms born immediately before or after SIMPLES was introduced exhibit the highest levels of licensing. This suggests, first, that formality decisions are, in fact, made at the time of starting up or soon after. Second, the fact that there is a break, but that the difference rapidly dissipates on either side suggests that part of what we may be picking up is the result of a government advertising campaign together with a temporary increase in enforcement intended to promote the new system.

In sum, our strategy constructs the difference in the outcome for firms established immediately before and immediately after the implementation of SIMPLES. This means that we really are not looking at the impact of formalization on existing informal firms,

but rather asking “if this particular informal firm were reborn under the SIMPLES regime, how would its likelihood of registering be increase, and how would its performance vary as a result of registration?”

We also employ Quantile analysis on the matched counterfactuals to examine the distribution of outcome differences with respect to formalization. The error term in equations (4) and (5) provides information about the nature of the firms’ unobservables. In particular, high (low) quantiles of the distribution are associated with unusually high (low) values of the composite error term in eq. (3), i.e. $(\theta_i - \hat{\theta}_i) + (u_{it} - \hat{u}_{it})$. Since for fixed covariates the constructed counterfactual is also fixed, the estimated quantiles correspond to those in the error term of equation (1).

Identifying the channels through which formalization improves performance and who is most affected?

Registering the firm itself will not obviously lead directly to better performance and hence we are interested in what other dimensions of formality – for instance, access to formal credit markets – registration makes possible. Using the same arguments, we can argue that if there is a change in access to credit, it is not being driven by unobserved heterogeneity, and hence the same approach can be used to decompose the effect of licensing. To see this, let C be an indicator variable representing participation in credit or other markets potentially associated with formality status. Consider also a simple extension of the post matching regression model. First, we can model the direct impact of licensing on C as:

$$C_i - \hat{C}_i = \alpha_{D1}(D_i - \hat{D}_i) + \delta_1(t_i - \hat{t}_i) + \gamma_1 X_i + \varepsilon_{1i}$$

Second, the total impact on firm performance of both whatever direct impact and subsidiary impact is:

$$Y_i - \hat{Y}_i = \alpha_{D2}(D_i - \hat{D}_i) + \alpha_{C2}(C_i - \hat{C}_i) + \alpha_{DC2}(D_i - \hat{D}_i)(C_i - \hat{C}_i) + \delta_2(t_i - \hat{t}_i) + \gamma_2 X_i + \varepsilon_{2i}$$

Define the impact of an exogenous change in ΔD associated with this particular channel as:

$$(6) \quad \Delta Y = (\alpha_{C2}\alpha_{D1} + \alpha_{DC2}\alpha_{D1}\Delta D)\Delta D$$

We consider the proportion of the change in outcome due to this channel by considering a 10 percent increase in D, obtained by considering the ratio (6)/($\alpha*\Delta D$) with $\Delta D=0.1$ and α obtained from equations (4) or(5) In section V, we calculate this for several different possible channels.

V. Results

We report both OLS and RD estimates of the impact of formality on firm performance. In both cases we only use the sample of SIMPLES eligible firms created between 3 and 20 months before the 1997 ECINF survey.¹³ For the purpose of OLS estimation we employ the complete sample of firms that begun operations during that period. For the RD estimates, we use only the pre-SIMPLES sample and then match those firms to counterfactual similar firms created after SIMPLES, using a set of exogenous covariates (X). A description of the matching procedure and its quality is provided in the Appendix.¹⁴ Using the matched set, we then estimate equations (4) and (5).

¹³ Inspection of figures 1 and 2 suggests that firms created in the month preceding SIMPLES implementation (November 1996) may have been affected by the program – which is reasonable if firms go about registering during the month that follows the start of their operations. Thus, we assume that the break created by SIMPLES in the incentives for micro-firm formalization occurs in November 1996 – time in business variable of at least 11 months in the October 1997 ECINF survey.. As a result, the choice of a sample of firms with between 3 and 20 months in business amounts to considering firms created between 9 month before and 9 months after the relevant regression discontinuity.

¹⁴ The set of exogenous covariates include variables that represent sector of activity, the entrepreneur’s age, gender and level of education, as well as the size of his/her household and an indicator for home ownership. Differences in means tests reported in the appendix show that after the matching the groups of sampled firms created before and after the SIMPLES break are not significantly different from each other in any of the above dimensions.

Table 4 presents the results for both methods for two sets of performance variables. The first set is composed by objective measures of current firm performance: total firm revenues, a binary variable capturing whether the firm invests in fixed capital or not, a continuous variable capturing the amount invested and another capturing the level of investment conditional on reporting some fixed capital investment, the number of employees, the number of paid employees, the fixed capital stock, a binary variable capturing whether credit was received or not, an indicator of access to governmental programs for micro-firms (i.e. technical assistance), and a binary variable capturing whether the firm has a fixed location. The second set of outcomes includes two variables capturing (subjective) future prospects. The first is a binary variable capturing whether or not the firm plans to expand. The second is an index representing the entrepreneur's degree of pessimism with regard to his future business activities: 1 for plans to expand, 2 for remaining the same, 3 for changing activities but remaining and entrepreneur and 4 for looking for salaried work.

In both the OLS and RD contexts, we gradually add additional covariates which are potentially affected by the treatment, namely the number of employees and the log of fixed capital stock. In this way we make a first cut at isolating the impact of the treatment. For instance, the results in the first row of table 4 indicate that much of the rise in revenues appears due to increases in capital stocks and, to a lesser degree, to increased employment. That said there is nonetheless an apparent impact through Total Factor Productivity as well. In almost every case we observe a reduction in the estimated effects of formality as labor and capital are included in the model, suggesting that a large part of its impact operates through increases in firm size.

For many of the indicators of performance, there is significant difference between the OLS and RD estimators suggesting the latter is, in fact, redressing important endogeneity issues. Hence, in what follows we discuss only the RD estimates. This is particularly the case with the effect of formality on the amounts invested in fixed capital conditional on some investment, which goes from strongly significant in OLS to insignificant and small in the RD estimates. For most other performance measures, there

is often a fall in magnitudes of around half but the coefficient, at least in the case without conditioning on labor and capital, remains significant and with the expected sign.

Both the OLS and the RD estimates suggest a significant impact of obtaining a license on firm revenues. The lower bound estimate shows that firms which operate with a license have revenues that are at least 13 percent higher. In fact, the upper bound estimate appears much larger, with an increase of over 40 percent in revenues. The comparison of columns (3) to (5) suggests that roughly 25 percent of that effect is due to an increase in employment size and about one half to an apparent increase in capital stocks – the remaining 13 percent estimate in column (5) would correspond to an increase in TFP. Again, the interpretation is not that existing firms change their levels of investment and productivity, but rather that if those firms were to be reborn under the SIMPLES regime, they would take a different trajectory involving a higher probability of formality, and the latter would be associated with higher investments and revenues. This will become clearer below when we discuss the channels through which registration affects firm performance.

Differently from Monteiro and Assuncao's, our RD estimates suggest that formality significantly increases the propensity to invest in capital goods – at least when capital stocks are not controlled for – but it does not affect the magnitude of investments by firms already investing. There is also a significant effect on employment size – both on paid and total employment – and on fixed capital stocks. Thus, an exogenous shift in a firm's status at the time of starting up, from informality to operating with a Government license, is associated with increases of roughly 20 and 40 percent, respectively in total employment and fixed capital stock. We also find a significant effect on paid labor, which persists even when we condition on capital stocks and total employees – paid or unpaid – suggesting a substitution toward paid labor.

There is a marginally significant effect on access to credit which, oddly, does not diminish significantly when capital is included suggesting that the effect is not working through fixed capital accumulation but perhaps working capital. Formality, however,

does not seem to affect, at least in the case of Brazil, micro-firm access to targeted Government programs – e.g. training and technical assistance. It does, nonetheless, have very significant effect on the likelihood of having a fixed location to run the business. Note that this result persists even when firm employment and capital stocks are controlled for, and it is arguably not related to the entrepreneur’s wealth – we do control for human capital and home ownership. It thus appears that beyond its effect on firm size, formality may also have an impact on the time horizon and the overall business strategy of micro-enterprise owners.

With regard to the effects of formality on the degree of optimism of entrepreneurs, we find that business expansion prospects increase with registration, even when employment is held constant. However, this higher optimism found among formal firms disappears when capital stocks are controlled for, which may be interpreted as reflecting the possibility that formal firms could be born closer to their steady state size, and would thus be less likely to expand further.

Using quantile regression techniques, Table 5 allows looking in more detail at the impact of formality on the revenues of firms located in different parts of the distribution of firm size and productivity. The results suggest that having access to an operating license has a much larger effect on firms that due to either unobserved characteristics of their managers or random negative shocks exhibit lower revenues. Thus, formal firms in the 10th and 20th quantiles, those with the lowest revenues after controlling for the variables used to match pre- and post-SIMPLES firms, exhibit revenues that are about 46 percent higher than those of similar informal firms. The same comparison in the 90th quantile yields an estimated impact of formality of only 36 percent. The differences across lower and upper quantiles are even more dramatic when employment is controlled for, suggesting that much of the differential impact comes through a larger effect of formality on the capital stocks of low performing firms.

Channels through which the impact of registration operates

Looking again only at the impact of formality on revenues, table 6 presents estimates of the share of that impact that is related to the effect that formality could potentially have on access to credit markets, Government technical assistance, and the use of fixed business location. As reported above, formality does have an effect on both credit and the use of a fixed location: the estimated α_{D1} is significant at the 5 percent level in the first case and at 10 percent in the second. When we condition on labor and then capital, credit access becomes insignificant, but interestingly, the magnitude falls only marginally, suggesting that its impact is not, especially through fixed capital accumulation. In any case, our estimates suggest that only a very small fraction of the effect of formality on revenues – at most 5 percent of the total effect when firm size is not controlled for – operates through higher access to credit. Similarly, a maximum of 1.6 percent of the formality-related change in revenues can be linked to a higher access to government assistance programs.

Having a business location, however, appears to affect the accumulation of labor, capital and knowledge. The α_{D1} coefficient estimate retains its significance as we progressively add labor and capital, although the magnitude falls at each step and most particularly when we add capital. This suggests that the larger likelihood of having a fixed place for operating a formal business affects mainly the firm's size, but also its productivity and most likely its whole business model – e.g. whether ambulatory hawker or fixed merchant. In fact, taking the estimated parameters and calculating what fraction of the total change in revenues can be explained through this channel leaves having a permanent place of business as the dominant effect, accounting for almost 50 percent of the change in revenues and over 30 percent of the higher TFP associated with formal business operations.

Conclusions

Most micro-enterprises in the developing world operate informally. They lack legally mandated operating licenses and seldom pay taxes and social security contributions. To the extent that informality creates obstacles for the enforcement of

property rights, and limits access to markets and public services, it could arguably curtail the ability of micro-enterprises to exploit their human and physical resources more effectively. Not surprisingly, efforts to facilitate the formalization of small businesses have become very popular among development practitioners, often with a focus on reducing the time and cost required to obtain government permits and licenses.

Whether the recent emphasis on reducing registration costs is or not appropriate, or sufficient, is not the main question that this paper has intended to address. It is well known that registration costs are only one among many other factors that may affect formality decisions – e.g. tax rates and levels of enforcement, availability of alternative forms of social protection, the quality of services available to formal firms, etc. The question on which this paper has focused is whether and by what measure micro-firm performance could potentially increase if the appropriate incentives were created for enticing small entrepreneurs to register their businesses. In fact, we have addressed this question by taking advantage of a 1996 Brazilian program – the SIMPLES – which combined the simplification of registration procedures with a considerable reduction of the tax burden carried by micro and small firms, through the consolidation of various taxes and social security contributions in a unique and lower payment.

We have not intended to provide definite answers on whether the SIMPLES program was successful in generating a permanent reduction in the size of the Brazilian informal sector. While our data suggests that a positive answer to this question is debatable, by exploiting a survey of Brazilian micro-enterprises collected less than a year after the launch of SIMPLES, we have established the presence of at least a temporary increase in registration rates around the time of implementation of the program. Using a Regression Discontinuity approach, we have exploited that exogenous jump in registration rates to identify the impact of increased formality levels on the performance of newly born Brazilian micro firms. Thus, by exploring the quasi- natural experiment provided by the implementation of SIMPLES, we have been able to eliminate many of the endogeneity issues surrounding the impact of registration on firm performance – e.g.

the bias created by the effect of unobserved managerial ability on both formality and firm performance.

Our results suggest that formality is indeed associated with a higher use of paid labor, with higher levels of capital intensity and with increased total factor productivity. Moreover, we have shown that at least in the Brazilian context, increased access to credit markets and Government provided technical assistance respond for only a small fraction of the revenue increasing effects associated with formality. On the other hand, our results indicate that the observed greater willingness of formal firms to operate out of a fixed locale appears to be responsible for as much as 50 percent of the increase in revenues and for a third of the TFP increase observed among formal firms. Overall, the effects of formality appear largest on poorly performing firms.

To be sure, the income and employment generating potential of the micro-firm sector is quite limited anyway, given the low levels of human and physical capital of the majority of micro-entrepreneurs in developing countries. However, what this paper has demonstrated is that efforts to improve the incentives for the formalization of micro-firms are well justified, as the evidence indicates that when faced with increased incentives for operating formally micro-entrepreneurs appear to improve the efficiency with which they employ their scarce resources.

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Table 1: Size Distribution and Employment Composition of Brazilian Micro-Enterprises

# Paid Employees	Share of Micro-firm Sector (%)	Family workers/ Total workers (%)	Informal workers / Paid workers (%)	Pay Social Security (%)
0	86.9	4.2	-	8.0
1	7.3	22.1	71.8	27.8
2	2.9	22.9	61.3	37.3
3	1.5	23.4	51.2	49.5
4	0.9	21.6	43.8	52.6
5	0.5	15.9	45.7	49.0

Notes: Pooled ECINF 1997 and 2003. Entrepreneurs at least 20 years old.

Table 2: Formality Indicators

	1997	2003	1997	2003
	All		With paid employees	
License to operate	23.5	24.2	31.0	49.2
Micro-firm Registration	11.6	11.2	34.0	34.4
Filled Tax Forms	11.4	10.6	35.1	32.2
Paid Taxes	6.8	6.1	17.1	15.6
Informal / Paid workers	-	-	63.3	65.0
Paid Social Security	8.1	7.1	25.1	24.4
Access to Credit	5.2	6.3	11.4	13.1
Participation in Guilds	12.4	11.4	24.6	23.0

Notes: Pooled ECINF 1997 and 2003. Entrepreneurs at least 20 years old.

Table 3: Reasons for Starting-up, Firm Prospects and Firm Licensing

Main reason to start a micro-firm	% firms (with License)	% firms (without License)	% firms	Plans for Future	% firms (with License)	% firms (without License)	% firms
Didn't find a job	20.9	32.2	29.6	Expand	45.5	36.6	38.7
Profitable business	2.2	1.2	1.5	Same level	31.2	31.2	31.2
Flexible hours	1.6	2.3	2.1	Change activity, remain independent	9.2	9.5	9.5
Be independent	27.8	17.1	19.6	Find a salaried job	6.5	13.4	11.8
Family tradition	11.0	8.1	8.7	Don't know	7.6	9.3	8.9
To help family income	12.2	20.8	18.8	Difficulties to regularize when starting-up? (2003)	% firms (with License)	% firms (without License)	% firms
Accumulated experience	10.7	8.7	9.2	Yes	18.0	5.1	8.2
Make good deal	10.7	7.6	8.3	No	57.4	10.4	21.7
As a secondary job	2.5	2.1	2.2	Didn't Try	24.7	84.5	70.1

Notes: Pooled ECINF 1997 and 2003 (except for "Difficulties to regularize?" from 2003). Sampled restricted to entrepreneurs aged at least 20.

Table 4: The Impact of Formality on Firm Performance, OLS and RD Estimates

	OLS			RD		
	NO	YES	YES	NO	YES	YES
Labor Capital	NO	NO	YES	NO	NO	YES
<i>Current Performance</i>						
Revenues	0.602 (0.036)	0.428 (0.430)	0.157 (0.032)	0.452 (0.054)	0.347 (0.053)	0.128 (0.054)
Fixed Capital Investment	0.081 (0.013)	0.057 (0.013)	0.014 (0.016)	0.038 (0.019)	0.027 (0.023)	0.008 (0.019)
Amount Invested	0.699 (0.086)	0.511 (0.081)	0.099 (0.053)	0.340 (0.156)	0.324 (0.150)	0.095 (0.135)
Amount Invested (if >0)	0.699 (0.086)	0.511 (0.081)	0.099 (0.053)	0.079 (0.277)	0.149 (0.247)	-0.146 (0.185)
Employees	0.318 (0.028)	-	-	0.195 (0.051)	-	-
Paid Employees	0.225 (0.021)	0.029 (0.012)	0.040 (0.015)	0.152 (0.041)	0.034 (0.024)	0.051 (0.025)
Fixed Capital Stock	0.741 (0.056)	0.579 (0.053)	-	0.464 (0.107)	0.395 (0.105)	-
Access to Credit	0.019 (0.007)	0.013 (0.007)	-0.003 (0.008)	0.032 (0.017)	0.029 (0.017)	0.025 (0.021)
Fixed Location	0.253 (0.011)	0.220 (0.011)	0.163 (0.023)	0.208 (0.018)	0.187 (0.023)	0.126 (0.021)
Access to Gov. Programs	0.0066 (0.0043)	0.0047 (0.0044)	0.0038 (0.0054)	0.014 (0.011)	0.014 (0.010)	0.009 (0.014)
<i>Future Prospects</i>						
Expand?	0.099 (0.015)	0.081 (0.014)	0.033 (0.017)	0.073 (0.030)	0.064 (0.027)	0.026 (0.034)
Pessimism Index *	-0.273 (0.040)	-0.224 (0.040)	-0.074 (0.045)	-0.200 (0.064)	-0.177 (0.086)	-0.080 (0.090)

Notes: Standard errors in parenthesis. ECINF 1997. SIMPLES-eligible micro-firms with at most 20 months of time in business and entrepreneurs at least 20 years old and without a College degree (or equivalent). * 1 Expand; 2 Same; 3 Change activities but remain independent; 4 Find a salaried job. For RD, bootstrapping standard errors using 200 samples with replacement.

Table 5: The Impact of Formality on Firm Revenues, Quantile Regression Estimates

Labor Capital Quantile	Quantile Regression			RD Quantile		
	NO	YES	YES	NO	YES	YES
	NO	NO	YES	NO	NO	YES
10	0.581 (0.067)	0.472 (0.062)	0.164 (0.065)	0.441 (0.126)	0.340 (0.126)	0.034 (0.147)
20	0.635 (0.047)	0.481 (0.044)	0.186 (0.039)	0.472 (0.109)	0.377 (0.128)	0.169 (0.108)
30	0.658 (0.044)	0.479 (0.047)	0.136 (0.036)	0.436 (0.106)	0.334 (0.078)	0.075 (0.075)
40	0.605 (0.035)	0.455 (0.040)	0.145 (0.036)	0.404 (0.097)	0.330 (0.087)	0.074 (0.074)
50	0.584 (0.048)	0.406 (0.032)	0.160 (0.037)	0.391 (0.082)	0.304 (0.086)	0.096 (0.055)
60	0.592 (0.042)	0.404 (0.038)	0.126 (0.040)	0.379 (0.095)	0.309 (0.078)	0.057 (0.078)
70	0.560 (0.048)	0.388 (0.034)	0.157 (0.041)	0.396 (0.082)	0.314 (0.094)	0.079 (0.078)
80	0.545 (0.053)	0.374 (0.040)	0.109 (0.046)	0.415 (0.114)	0.240 (0.102)	0.057 (0.066)
90	0.533 (0.067)	0.304 (0.048)	0.095 (0.043)	0.357 (0.126)	0.173 (0.105)	0.095 (0.096)

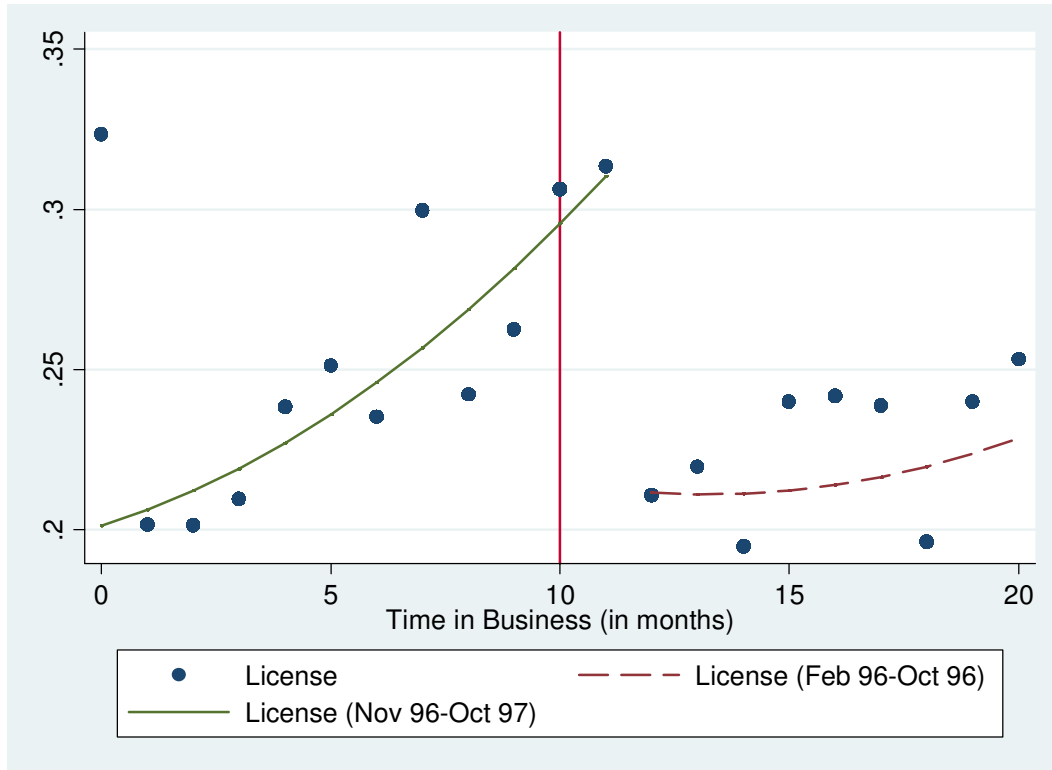
Notes: Standard errors in parenthesis. ECINF 1997. SIMPLES-eligible micro-firms with at most 20 months of time in business and entrepreneurs at least 20 years old and without a College degree (or equivalent). Dependent variable is log of total revenues. For RD, bootstrapping standard errors using 200 samples with replacement.

Table 6: The Impact of Formality on Firm Performance through Access to Credit, Government Programs, and a Fixed Business Location

	OLS			RD		
Labor	NO	YES	YES	NO	YES	YES
Capital	NO	NO	YES	NO	NO	YES
Credit						
α_{D1}	0.019 (0.007)	0.013 (0.007)	-0.003 (0.008)	0.032 (0.017)	0.029 (0.017)	0.025 (0.021)
α_{D2}	0.619 (0.037)	0.440 (0.033)	0.159 (0.033)	0.398 (0.081)	0.338 (0.074)	0.107 (0.065)
α_{C2}	0.703 (0.076)	0.492 (0.068)	0.226 (0.067)	0.691 (0.108)	0.427 (0.082)	0.099 (0.081)
α_{DC2}	-0.377 (0.131)	-0.227 (0.118)	-0.018 (0.115)	-0.575 (0.277)	-0.344 (0.217)	-0.182 (0.203)
% explained	2.10%	1.43%	-0.43%	4.48%	3.27%	1.58%
Gov. Programs						
α_{D1}	0.0066 (0.0043)	0.0047 (0.0044)	0.0038 (0.0054)	0.014 (0.011)	0.014 (0.010)	0.009 (0.014)
α_{D2}	0.603 (0.036)	0.425 (0.033)	0.151 (0.032)	0.405 (0.081)	0.308 (0.065)	0.093 (0.063)
α_{C2}	0.512 (0.125)	0.297 (0.112)	0.072 (0.109)	0.556 (0.239)	0.463 (0.196)	0.127 (0.165)
α_{DC2}	-0.132 (0.223)	0.074 (0.204)	0.184 (0.182)	-0.315 (0.359)	0.002 (0.344)	0.167 (0.237)
% explained	0.55%	0.33%	0.22%	1.62%	1.87%	1.01%
Fixed Business Location						
α_{D1}	0.253 (0.011)	0.220 (0.011)	0.163 (0.023)	0.208 (0.018)	0.187 (0.023)	0.126 (0.021)
α_{D2}	0.428 (0.045)	0.357 (0.041)	0.144 (0.040)	0.263 (0.061)	0.243 (0.059)	0.079 (0.065)
α_{C2}	1.061 (0.045)	0.751 (0.042)	0.391 (0.043)	1.117 (0.069)	0.760 (0.056)	0.360 (0.069)
α_{DC2}	-0.211 (0.073)	-0.215 (0.067)	-0.121 (0.065)	-0.445 (0.157)	-0.289 (0.113)	-0.216 (0.133)
% explained	43.69%	37.47%	39.41%	49.36%	39.38%	33.33%

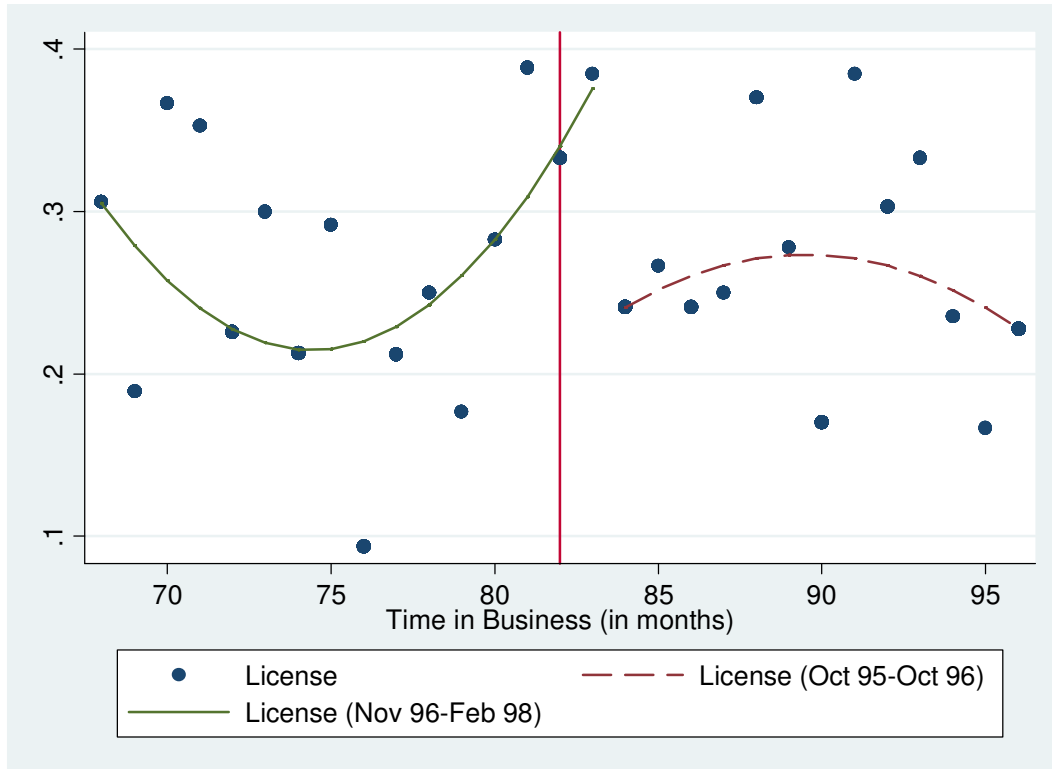
Notes: Standard errors in parenthesis. ECINF 1997. SIMPLES-eligible micro-firms with at most 20 months of time in business and entrepreneurs at least 20 years old and without a College degree (or equivalent). Effects on log of total revenues. For RD, bootstrapping standard errors using 200 samples with replacement.

Figure 1: Proportion of registered firms and time in business (eligible firms surveyed in the 1997 ECINF that were created between 10 months before and 10 months after SIMPLES)



Note: Authors calculations using ECINF 1997, Feb 1996-Oct 1997.

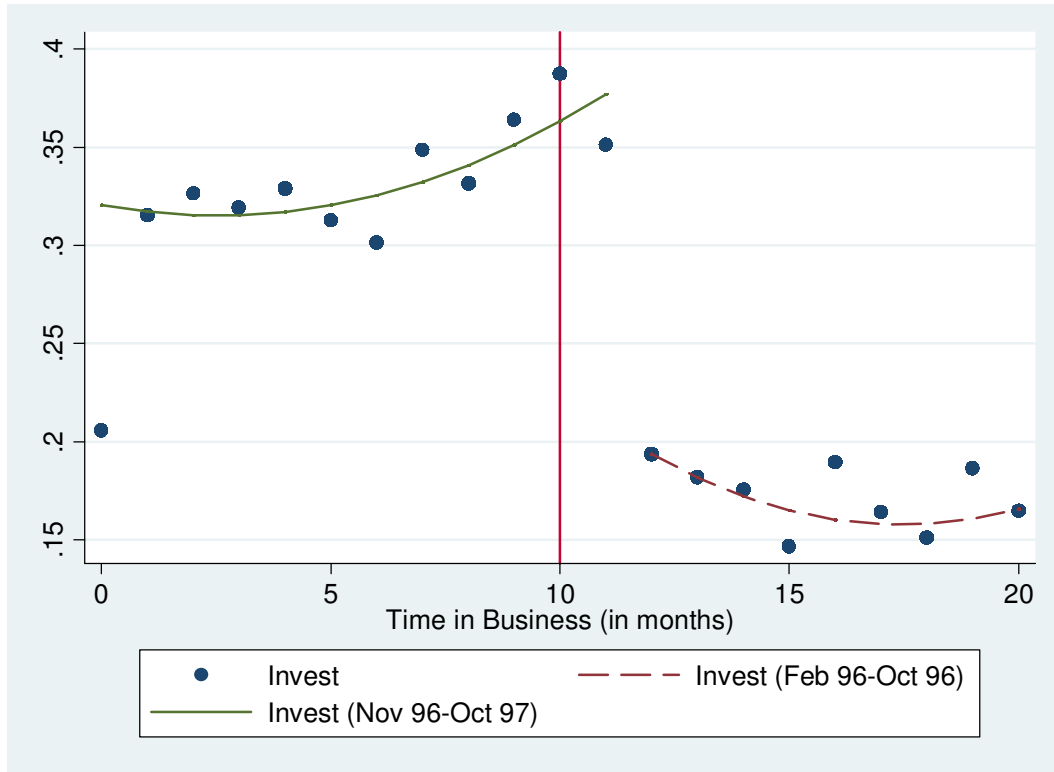
Figure 2: Proportion of registered firms and time in business (eligible firms surveyed in the 2003 ECINF that were created between 10 months before and 10 months after SIMPLES)



Title: Proportion of firms with license (2003)

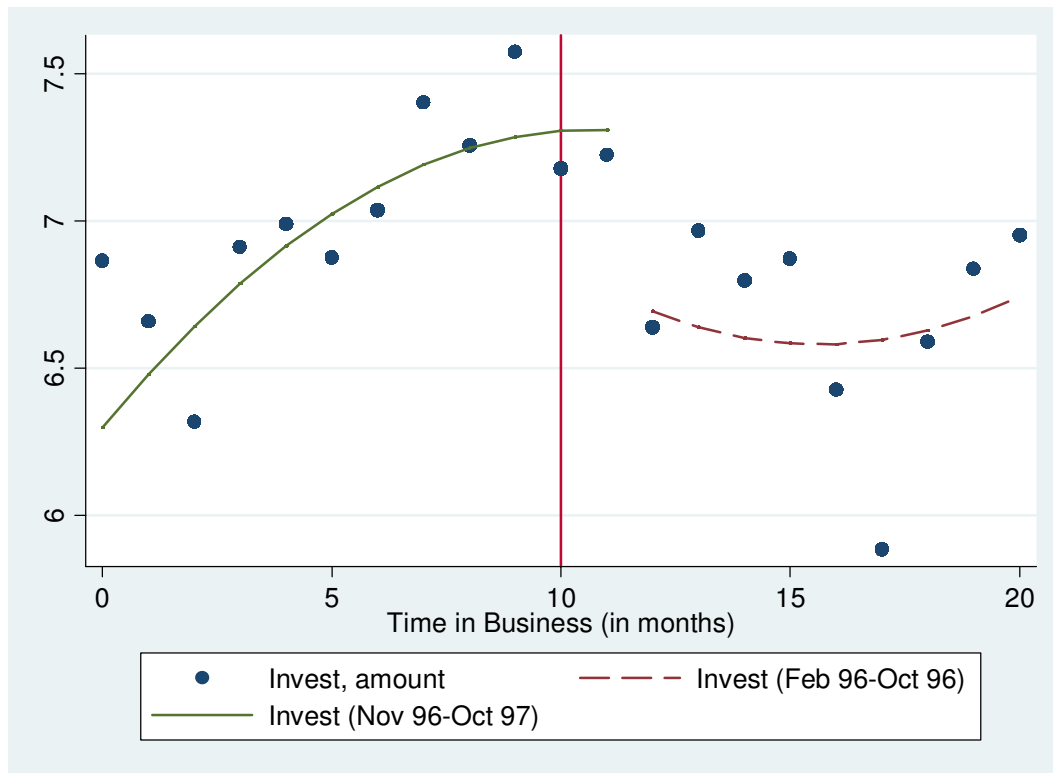
Notes: Authors calculations using ECINF 2003 Oct 1995-Feb 1998. Eligible firms only.

Figure 3: Proportion of firms that invest and time in business (eligible firms surveyed in the 1997 ECINF that were created between 10 months before and 10 months after SIMPLES)



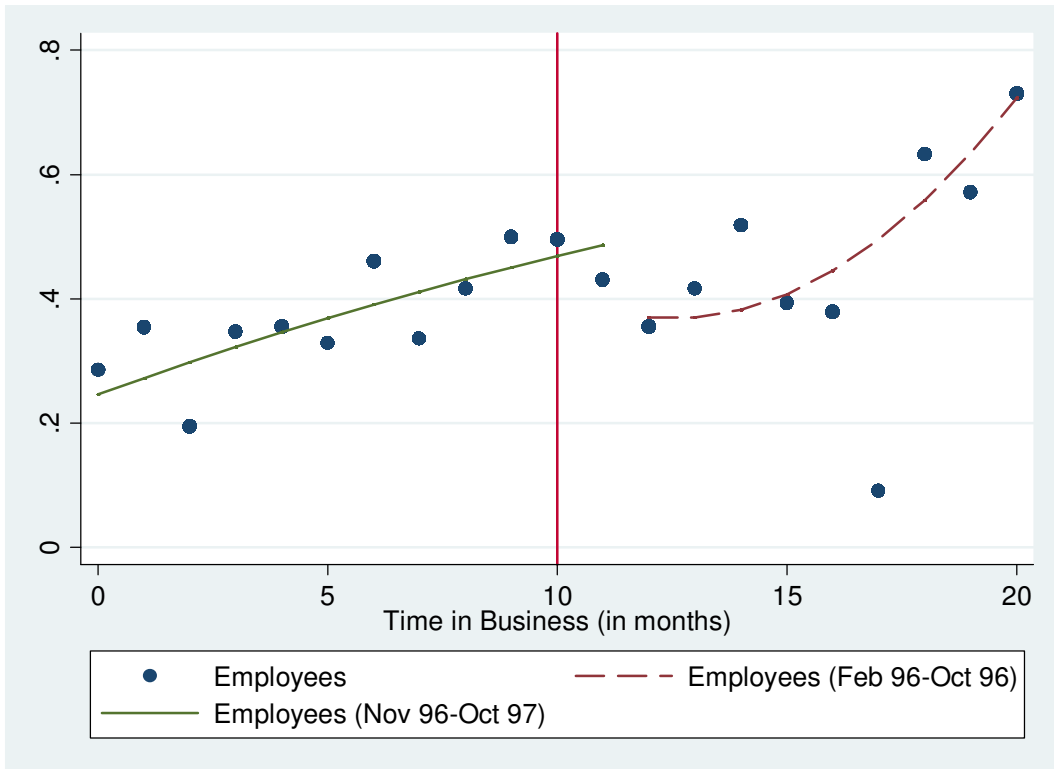
Notes: Authors calculations using ECINF 1997, Feb 1996-Oct 1997. Eligible firms only.

Figure 4: Amount invested (in logs) and time in business (eligible firms surveyed in the 1997 ECINF that were created between 10 months before and 10 months after SIMPLES)



Notes: Authors calculations using ECINF 1997, Feb 1996-Oct 1997. Eligible firms only.

Figure 5: Number of paid employees and time in business (eligible firms surveyed in the 1997 ECINF that were created between 10 months before and 10 months after SIMPLES).



Notes: Authors calculations using ECINF 1997, Feb 1996-Oct 1997. Eligible firms only.

Appendix: Matching Quality Indicators

	Unmatched	Matched
Gender	0.000	0.154
<i>Education</i>		
First Level incomplete	0.887	0.519
First level complete	0.744	0.162
Second level incomplete	0.115	0.357
Second level complete	0.180	0.496
College incomplete	0.832	0.361
Age	0.001	0.690
Npers	0.364	0.155
Own House	0.281	0.977
<i>Industry</i>		
Construction	0.009	0.470
Retail Trade	0.377	0.747
Hotels and Rest	0.008	0.371
Transportation	0.145	0.761
Services	0.335	0.292

Notes: t-test for differences in means, p-values. ECINF 1997. SIMPLES-eligible micro-firms with at most 20 months of time in business and entrepreneurs at least 20 years old and without a College degree (or equivalent).

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