Does Formality Improve Firm Performance?
Evidence from a Quasi-experiment in Mexico

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Abstract:

In 2002, Mexico enacted a reform encouraging municipalities to simplify the procedures for registering a business. Earlier papers have shown, that the reform increased formalization; however, these studies lacked data on other firm outcomes. Taking advantage of a rich dataset of micro-enterprises (ENAMIN), this paper studies the effects of reducing registration costs on firm performance. We find that, on average, firm revenues increased more in municipalities that adopted the reform early on relative to municipalities that adopted the reform in future years. Evidence suggests that the main channel driving higher revenues is an increase in physical capital, which allowed for higher labor productivity. We also find that the capital increase is not financed by loans, suggesting that the widespread belief that firms formalize to obtain credit is not supported by the data.

1. Introduction

Reducing informality is regarded as a central issue for economic development by policy makers and scholars alike. However, in spite of the attention that the topic receives, there is limited tight micro-econometric evidence regarding the impacts of formality[1] on firm performance. Does registering a business with government authorities have any effects on firm outcomes? If effects do exist, what are the channels driving them? This paper attempts to address these questions.

The analysis relies on Mexico's survey of micro-enterprises (ENAMIN), which is a special module of Mexico's Labor Force Survey (ENEU/ENOE). The ENAMIN follows the best practices recommended by the ILO to gather reliable information on the informal sector (ILO 2013), and as a result, it is better suited to study the informal sector than other datasets on Mexico. Eight rounds of the survey have been conducted between 1992 and 2012; each covering approximately 15,000 micro and small firms. The data gathers information on firm performance (i.e. profits, revenues, etc.), intermediate outcomes (i.e. assets, access to credit, source of funds used to start the business, etc.), as well as different measures of formality (i.e. registration with government authorities, the type of accounting records, etc.).

Measuring the impacts of formality on firm outcomes is difficult because firms that choose to register may be inherently different from firms that remain informal. Moreover, given that firms weight a number of factors when deciding whether or not to formalize, it is not possible to determine a priori which firms benefit from formality the most.[2] This paper addresses this selection concern by relying on a quasi-experiment. We study the implementation of a federal program in Mexico, which created one-stop-shops to facilitate registration procedures for eligible micro, small, and medium firms. For exogenous reasons, these one stop-stop-shops known as SARE were established in some municipalities earlier than in other comparable municipalities.

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[1] There is not a single definition of formality. For this paper, we define formal firms as those that are registered with any government authority. There is a large literature concerning the nature of informality and its determinants (see Perry et al. (2007) and the citation therein).
[2] More productive firms may be *more likely* to formalize because they tend to expand and be detected by government inspectors. Alternatively, more productive firms may be *less likely* to formalize because they benefit the most from greater flexibility in employment and production decisions.

Identification is attained by comparing municipalities that established a SARE shortly after program inception, relative to municipalities that established a SARE years later.[3] One challenge to our identification strategy is that more effort was exercised to establish a SARE in the municipalities with the highest "economic potential". Nevertheless, our approach remains valid because the agency implementing the project did not have enough resources to simultaneously establish a SARE in all municipalities with similar potential; consequently, comparable municipalities adopted the reform at different times. Further, we control for "potential" relying on the same criteria that was used to classify municipalities at the time the reform was adopted (i.e. municipality rankings based on population and economic variables). Moreover, given that municipality rankings change little over time, we also include municipality fixed effects in our econometric models. After controlling for potential, the choice of whether to open a SARE and the timing of adoption appear to be exogenous: we find no significant pre-treatment differences in economic characteristics between early-adopters and late-adopters (both in levels and in changes).

Earlier papers have already studied the effects of Mexico's SARE program[4]; however, there are important reasons to re-examine this reform. First, we study the impacts of SARE on additional formality dimensions (such as keeping formal books, registering with a business association, etc.). Second, we focus on the long-term impact of the reform, rather than studying short-term impacts. Third, the ENAMIN dataset allows us to study the effects of SARE on different measures of firm's performance (such as revenues and profits); instead, earlier papers considered only a limited number of outcomes (mainly employment, firms' registration, local prices, and individual's earnings). Most importantly, however, unlike earlier papers, we are able to study the mechanisms driving changes in profitability and earnings. Hence, we establish the effects of SARE on a number of intermediate outcomes such as access to credit, capital intensity, amongst other. Lastly, our dataset allows us to measure the effects of the reform both for

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[3] Similar results are obtained when comparing SARE adopters to non-adopters (results available upon request).
[4] See Bruhn (2011; 2013), Kaplan et. al., (2011), as well as number of non-academic reports including COFEMER's own program valuation.

the entire sample of micro-firms and for the sub-sample of micro-firms that existed before the reform was enacted.

While our paper is the first to exploit the SARE program to study the links between formality and firm performance, a growing literature has also studied such links. We re-explore this question for a number of reasons. First of all, there are still few empirical papers on the topic and results have been mixed so far.[5] Hence, it is not yet clear whether any of the results from this literature can be generalized to different settings. Further, we hope to add some clarity as to what may be driving some of the mixed findings obtained thus far (i.e. are different results due to different data or due to different identification strategies?). Moreover, the identification strategy employed in many of the earlier papers relied on un-testable or unrealistic assumptions. For example, Fajnzylber et. al. (2009) assumed that selection is driven only by observables or through a specific functional form. McKenzie and Sakho (2010) assumed that distance to the tax office affects only the registration choice but not firms' tax expenses. Instead, we perform a number of tests to support the validity of our identification assumptions. Further, many of the earlier papers relied on selected samples.[6] Instead, our dataset is representative of micro-firms in major urban areas of Mexico.

We obtain a number of interesting findings. We start by corroborating that reducing the costs of registering a business encourages firms to formalize. We obtain a much larger impact of the SARE reform on formalization than earlier studies (over 15% compared to 5% in Bruhn (2011)). We attribute this difference to the fact that we focus on micro-entrepreneurs rather than focusing on the entire population of Mexico. Micro-entrepreneurs were the intended beneficiary of the reform and they may have responded to the reform more strongly than other groups. Further, our measure of formality (i.e. registering with "any" government authority) is broader than the ones used in earlier papers (i.e. having a legal name or registering a worker for social security). We also find that SARE had significant and robust effects on only few other measures of formality, such as registering with a business association. Instead, effects are not robust for keeping

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[5] For example, while some studies find that formality increases profits for small and micro firms (Fajnzylber et al. 2009), others have reached the opposite conclusion (McKenzie and Sakho 2010).
[6] For example, the sample in McKenzie and Sakho (2010) was obtained from business registries and may over-represent establishments that are more visible and possibly more successful.

any books or keeping formal books. The fact that we do not find any evidence that firms improved book-keeping due to the reform is important; it suggests that other finding in our paper cannot be attributed to better book-keeping as opposed to real effects on firm performance.

We find that the reform had important effects on firm performance. Firms in SARE municipalities experienced an economically large and statistically significant increase in revenues. This result is consistent across a number of different sub-samples and it is also consistent with the most reliable papers in the literature (see Galiani and Melendez (2013) for Colombia). Evidence suggests that the main channel driving higher revenues is an increase in capital, which allowed for higher labor productivity. We also find that the capital increase is not financed by loans; and it may be self-financed. Capital may increase because entrepreneurs may choose two very different investment patters depending on whether their business is formal (and protected by aw) or informal. Though most theoretical models assume that firms formalize to access credit, our finding that formalization has no effect on credit is consistent with the results in other econometric papers on the topic. Results on profits are less robust. We find that formalization increased profits amongst firms that existed before the reform, but not for the overall sample. New firms may be too small to benefit from access to formal institutions, they may have less information about the process of formalization, or they may predict their probability of success less accurately.[7]

We compare our finding to those in earlier papers. Our results are very similar to those in Galiani and Melendez (2013), the only paper to use an experimental approach in Latin America (we use a quasi-experimental approach). Both papers find that formalization has a positive impact on revenues and firm assets, but no effect on access to credit. Indeed our findings are more similar to those of Galiani and Melendez (2013), than to those in Fajnzylber et al. (2009), who use our same dataset but a non-experimental identification strategy (and find much larger impacts). However, unlike Galiani and Melendez (2013) we do find an effect on profits for some sub-samples. Hence our paper suggests, that some of the mixed findings in the literature may occur

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[7] Note, however, that the effects on profits for existing firms disappear when the identification strategy relies on comparing early adopting municipalities to non-adopters (results available upon request).

because the effects of formalization on profits are heterogeneous (i.e. different sub-populations respond differently to the formalization treatment).

The remaining of the paper is organized as follows. Section 2 presents background information, including a discussion of the literature and the SARE program. We also present a conceptual framework. Section 3 describes the data and presents descriptive statistics. Section 4 explains the identification strategy and the econometric approach. Section 5 summarizes our results. Section 6 presents robustness checks. Section 7 provides policy recommendations and concludes.

2. Background

We start this section by summarizing the literature on the costs of registering a business, and the literature on the effects of formality on firm outcomes. Then, we describe the most relevant characteristics of Mexico's SARE Program.

The Cost of Registering a Business

Our paper adds to a growing literature on the micro-level impacts of reducing registration costs on firm formality. The early literature studied this question relying on cross-sectional variation across countries or cross-industry variation within countries.[8] Instead, more recent papers have studied the effects of government programs that reduced registration costs, such as SIMPLES in Brazil[9] and SARE in Mexico. Given that the SARE program is exploited for identification in this paper, we pay particular attention to earlier studies of this reform.

Bruhn (2011) studied the effects of SARE using quarterly data from Mexico's Labor Force Survey (ENE) between 2000 and 2004 (19 quarters in total). Results show that SARE had a significant impact on formalization and employment (5% increase in the stock of registered businesses owners and 2.2% increase in wage employment). However, most of the effect comes exclusively from former salaried workers opening formal firms rather than form existing informal entrepreneurs registering their business. Bruhn (2013)

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[8] See Loayza et. al. (2005) and Djankov et. al. (2006) for papers exploiting cross sectional variation. See Klapper et. al. (2006) and Fisman and Sarria-Allende (2004) for papers exploiting cross industry variation.
[9] Monteiro and Assunsao (2012) study the effects of SIMPLES using a diff-in-diff approach. They find that SIMPLES increased formal licensing for retail firms (by 13 p.p.), but not for firms in other eligible sectors.

examined the impact of SARE separately for existing informal entrepreneurs of different types: (a) those resembling wage workers and (b) those resembling formal entrepreneurs. Only informal entrepreneurs who resemble formal ones were more likely to register their business after SARE. This suggests that only some entrepreneurs are viable while others would prefer to be wage earners. Further, Bruhn (2011) finds that the SARE reform had general equilibrium effects: competition from newly formalized firms lowered prices in treated municipalities, and previously formal businesses experienced lower earnings.

Kaplan et. al. (2011) studied the effects of SARE on business start-ups using monthly administrative data from the social security administration (IMSS) from 1998 to 2006. Relying on a triple-difference framework, they find that SARE led to an increase of 4 to 8% in the flow of newly registered firms in eligible industries. Although the effect is statistically significant, its magnitude is quantitatively small (implying only about 2 to 5 new firms registered and about 12 to 19 new formal jobs per municipality per month). They conclude that firms weight the costs and benefits of becoming formal, with small firms seeing few benefits. Note that the magnitude of the coefficient estimates in Bruhn (2011) and Kaplan et. al. (2011) are not directly comparable because the first focuses on a stock variable, whereas the later focuses on a flow. Adjusting for those differences, Kaplan et. al. (2011) finds smaller effects of SARE on formality than Bruhn (2011). This difference may occur because, by relying on data from the social security, Kaplan et. al. (2011) does not have information on firms without any registered employees (which may be driving much of the effect in Bruhn's dataset).

Formality and Firm Performance

While our paper is the first to exploit the SARE program to study the links between formality and firm performance, the literature has relied on a number of different approaches to address this question. A list of existing papers is provided in Table 4. There are a few experimental papers on the topic. Galiani and Melendez (2013) conducted an experiment that mimicked different public programs aimed at reducing informality in Colombia. Similarly, De Mel et. al. (2013) conducted a field experiment in Sri Lanka, where they reimbursed firms the monetary costs of formalizing. Other papers have relied on a quasi-experimental approach. Fajnzylber et. al. (2011) used a regression

discontinuity design to study Brazil's 1996 introduction of a business tax reduction and simplification scheme (SIMPLES); and it used eligibility to SIMPLES as an instrument for the decision to formalize. Other papers have relied on non-experimental approaches. McKenzie and Sakho (2010) exploited firms' distance to the tax office in Bolivia (conditional on distance to the city center) as an instrument for registration. Fajnzylber et al. (2009) relied on propensity-score matching and control function techniques to control for selection to formality.

Most papers in the literature address three questions. The first question consists of isolating the effects of some variable, usually a treatment indicator, on formality. The main conclusion is that while some interventions are successful at reducing informality, not all interventions are effective. For example, Galiani and Melendez (2013) evaluate different public policies and they obtain different results for each of them. Further, the same intervention may have different impacts in different countries. A policy that is not successful at increasing formalization in Sri Lanka, where entrepreneurs are afraid of formalizing due to land ownership issues, may be successful in Mexico, where land ownership is less of a constraint.

The second question tackled by most papers in the literature is whether formalization has an effect on firm performance. The findings regarding the effects of formality on profits are mixed. While some papers find no statistically significant effects of formalization on profits (Galiani and Melendez 2013; de Mel et al. 2013), other papers some impacts (Fajnzylber et al. 2009; McKenzie and Sakho 2010; Fajnzylber et al. 2011). Further, very few papers study the effects of formalization on alternative measures of performance such as revenues, expenses, etc. (see Galiani and Melendez (2013) for an exception).

Some papers also explore the channels driving any observed changes in firm performance. Fajnzylber et. al. (2011) finds that the channel driving higher profits appears to be a lower cost of contracting labor (allowing better production techniques, a more permanent establishment, etc.). McKenzie and Sakho (2010) find that the channel driving higher profits is an increase in the client base of newly formalized firm, which are able to issue receipts and sell to other formal firms. Theoretical papers have often considered access to credit to be an important benefit of formalization (see, for instance,

Straub 2005); however, most empirical papers in the literature find no effects of formalization on access to credit.

Business Simplification in Mexico

In 2000, the Mexican government created the Federal Commission for Improving Regulation (COFEMER) to address the concern that Mexico's regulations were too heavy by international standards (see Djankov 2002). In 2002, following COFEMER's recommendations, the government passed a federal law simplifying the federal procedures to register eligible, non-governmental, small firms. The number of federal procedures were reduced to at most 2 procedures to be administered within 72 hours. These two procedures were obtaining a taxpayer number and incorporating the business (if a corporation). After starting operations, firms had 3 months to comply with other federal requirements, such as registering workers for medical insurance and social security.

In addition to the federal procedures to open a business, Mexico has state and municipal regulations, which vary across locations. Hence, for the *federal* legislation to be successful, it needed to be accompanied by the simplification of *local* procedures. COFEMER decided to approach municipal governments to suggest that they cut down on local regulations and that they implement one-stop-shops, where entrepreneurs could take care of federal, state, and municipal procedures at the same time.[10] These one-stop-shops were called Rapid Business Opening System (SARE). Municipalities were the main target of the SARE program because many procedures, and ex-post compliance checks occur at that level.[11] Once approached by COFEMER, municipalities could voluntarily decide whether or not to sign a contract. COFEMER would provide the expertise, while municipalities would provide the physical resources and the funds to open, publicize, and

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[10] In addition to speeding-up registration, the program clearly defined the procedures, fees and entities responsible of the registration process, making it more transparent and reducing the likelihood of bribes.
[11] Though the role of states is more limited, COFEMER also approached states to sign contracts with them. Virtually all states have already signed a contract with COFEMER.

continually operate a SARE.[12] Once the SARE became operational, COFEMER would play a supervisory role and would continue to find ways to cut down on regulation.[13]

Since program inception, COFEMER's goal was to create SAREs in Mexico's most populous and economically important municipalities, in order to quickly reach a large share of people and economic activity. COFEMER used a study by Cabrero et al. (2003) to determine which of Mexico's 2,448 municipalities should be targeted for program participation. The study identified 60 major urban centers, based on quality of infrastructure, population, economic activity, and growth potential (these cities are listed in Table 2). These 60 urban centers encompassed 224 municipalities, which were deemed "competitive" and became the intended targets of the program.[14] Note that our dataset consists almost exclusively of municipalities in "competitive" cities because earlier rounds of the ENAMIN survey were conducted only in major urban centers.

In spite of its stated priorities, COFEMER was not able to bring a SARE to all competitive municipalities at the same time due to limited resources.[15] Consequently, SAREs were implemented in comparable municipalities at different times. Importantly, among municipalities of similar potential, COFEMER had no explicit criteria guiding which municipalities to approach first, and the order in which municipalities were approached may be deemed random. This randomness in the selection process was confirmed by Bruhn (2011) based on interviews with several staff members at COFEMER who were in charge of implementing the reform. Further, we control for broad patterns in municipality potential using time-varying municipality characteristics and municipality fixed effects.

Though COFEMER's actions may be deemed random, there are additional features of the SARE adoption process that we need to control for. Table 1 presents statistics on the timing and geographic clustering of SARE adoption during our sample

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[12] Given that richer municipalities may choose to adopt a SARE, we show that there are no pre-reform differences in municipality income between adopters and non-adopters (see Table 6).

[13] COFEMER has recently established the goal of expanding the role of SAREs by integrating them with the tuempresa.gob.mx website. This website is a one-stop-shop to ease compliance with federal business registration regulation.

[14] A municipality ("municipio") is the smallest autonomous entity of the federal system in Mexico. It is typically bigger than a city, but many big cities contain more than two municipalities.

[15] Early on, COFEMER had four people working to spread the reform to local governments (Bruhn 2011).

period.[16] The first row shows the number of municipalities establishing a SARE each year: 209 SAREs were established by the end of 2012. The third row shows the number of States where municipalities adopted a SARE. There is large geographical variation, with SAREs being adopted in virtually all Mexican States. The last row shows that municipalities within a State tended to implement SAREs at the same time.[17] Such clustering occurred because COFEMER approached State governors for help in reaching competitive municipalities. We control for this geographic clustering in SARE adoption by including state-time fixed effects in our regressions.

Another important aspect of the SARE program is that it only simplified registration procedures in industries which do not present a serious risk to public health, public security, or the environment (and do not require special permits or concessions). The Federal government selected 685 "non- risky" 6-digit industries that were eligible for the program (based on INEGI's CMAP industrial classification).[18] The rationale for selecting only these industries is that the officials did not want to reduce oversight for firms in activities prone to accidents or health hazards. As a result, retail and services sectors (other than transport) were disproportionately represented amongst eligible industries, whereas manufacturing firms were often ineligible.[19] Note further that, although each municipality could choose its own list of eligible industries, municipalities were encouraged to follow the federal classifications. In practice, most municipalities copied their list from the federal classification or from early-adopting municipalities. Our analysis relies only on low-risk industries based on the federal classification.[20]

Many SAREs were established in non-competitive municipalities, even though they were not the intended beneficiaries of the program. In these cases, the municipality approached COFEMER to request a SARE, rather than COFEMER approaching the

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[16] Table 1 updates information in Kaplan et al. (2011). A list of the adopting municipalities and the date of implementation is available in COFEMER's website.

[17] For example, in 2004 more than a third of implementation happened in the most active State: the State where the President of Mexico came from (Kaplan et. al. 2011).

[18] Estimates suggest that the reform applied to 80% of firms and 55% of industries (Bruhn (2011)).

[19] Examples of low-risk industries include commerce and restaurants, real state services, most food manufacturing, production of furniture, textiles manufacturing, etc. Examples of high-risk industries include chemical production, production of rubber products, manufacturing of machinery, transportation, etc.

[20] Most micro-firms belong to low-risk industries. Including high-risk industries in the analysis reduces the magnitude and the significance of coefficient estimates. Estimates are never statistically significant for high risk industries, but this may be due to the much smaller sample.

municipality. By definition, non-competitive municipalities tend to be smaller than competitive municipalities and the program may be less cost-effective in these locations. By law, COFEMER could not deny participation to any municipality interested in the program. The second row of Table 1 shows the number of competitive municipalities that adopted the program each year. We find that the share of non-competitive municipalities adopting the program has increased. While one may worry that non-competitive municipalities adopting a SARE are a self-select group, this is not a concern for our study because our sample is comprised only of competitive municipalities.[21]

It is generally believed that the now 10 year-old SARE program was successful at simplifying local business registration procedures and reducing the time it takes for businesses to register (see Dougherty 2013). COFEMER published data on the costs of registering a business pre and post SARE for the first 32 municipalities that registered. As summarized in Bruhn (2011), the average number of days to register a business fell from 30.1 to 1.4. Similarly, the average number of procedures and number of office visits required to register a business decreased from 7.9 to 2.7 and from 4.2 to 1, respectively. Further, the standard deviations of all three measures became smaller, implying relatively small differences in business registration procedures across municipalities after the reform. Though COFEMER no longer publishes this type of information at the municipality level, the World Bank Doing Business report shows that most Mexican States have reduced the costs to register a business in recent years (which may be due to the SARE program).

Kaplan et. al. (2001) highlight four additional features of the program that are relevant for our analysis. First, firms that satisfy the eligibility criteria must register through SARE; second, firms cannot register in one municipality and operate in another; third, the small size of firms registering through SARE suggest that they are single establishment entities; fourth, there were no other government programs being implemented with a similar location-time profile whose effects we could be attributing to SARE.

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[21] Virtually all municipalities included in the ENAMIN are competitive.

Conceptual Framework

We present a simple conceptual framework in order to show how formalization may affect the performance of micro-firms. The conceptual framework is similar to the model in Evans and Jovanovic (1989) and Venancio (2010). We start by considering the choices of profit-maximizing micro-entrepreneurs who operate in a competitive setting. Entrepreneurs can choose to operate in the formal or the informal sectors.

We first consider the payoffs in the formal sector. We assume that entrepreneurs are heterogeneous in their level of ability, θ_i., which is distributed based on the p.d.f. g(.). Entrepreneur's revenues are a function of their ability, θ_i. and their investment in physical capital, *I*. For simplicity, we assume that the production function takes the form $y_i = \theta_i^{1/2} I^{1/2}$.

Entrepreneurs can finance investment by using their accumulated household savings, s, or by obtaining loans, such that $I = L + s$. The cost of capital is given by *r>0*, and any loans need to be repaid with interest at the end of the period. Formal entrepreneurs also need to pay the fixed costs of formalization F, which can be interpreted as the registration costs including any bribes and regulatory burdens.

Next, we consider the payoffs in the informal sector. Informal entrepreneurs obtain a fixed payoff, *w.* Informal entrepreneurs also get to keep their savings and earn interest, *r,* on them.

Entrepreneurs profits can be summarized as follows:

$$\pi(\theta_i) = \begin{cases} \theta_i^{1/2} I^{1/2} + R(s - I) - F & \text{if formal} \\ w + Rs & \text{if informal} \end{cases}$$

Where *R* equals 1 plus the discount rate*, r.* If *s* is grater than I, the entrepreneur is a net lender, otherwise the entrepreneur is a net borrower.

Entrepreneurs choose the level of investment that maximizes their profits. The level of investment that maximizes profits is given by $I^* = \theta_i / 4R^2$. After plugging in I^* in the above equation, entrepreneur's profit function simplifies to the following:

$$\pi^*(\theta_i) = \begin{cases} \theta_i / 4R + Rs - F & \text{if formal} \\ w + Rs & \text{if informal} \end{cases}$$

Hence, a entrepreneur will choose to be formal as long as the profits from being formal exceed the profits from remaining informal:

$$\theta_i/4R - F > w \quad (1)$$

This equation shows that the decision to formalize depends on one's ability, θ_i, relative to the market earnings of informal entrepreneurs, w. Importantly, the decision to formalize also depends on the level of fixed costs of formalizaion, F.

Let $\hat{\theta}$ be the value of θ that solves the above equation with equality. Hence, we obtain:

$$\hat{\theta} = 4R \ (w+F) \quad\quad (2)$$

Then, the fraction of entrepreneurs who become formal is $1-G(\hat{\theta})$, where G(.) is the c.d.f. for g(.).

We use the conceptual framework discussed above to address two main questions. First, what happens to the level of formalization when the registration costs decrease? We obtain the intuitive result that the lower the fixed costs of formalization, the higher will be the fraction of agents who formalize.

Proof: Equation (2) shows that the entry costs, F, are strictly increasing on $\hat{\theta}$; hence, the lower the entry costs, the lower is the threshold ability that induces entrepreneurs to formalize. In turn, the lower the threshold ability to formalize, the more entrepreneurs that choose to become formal.

The second question is what happens to the level of investment in physical capital (and revenues) when registration costs decrease? We obtain the intuitive result that the lower the registration costs, the higher the level of investment. Further, the higher the investment the higher the revenues.

Proof: In our simple framework, investment depends on whether an entrepreneur is formal or not. In particular, a formal entrepreneur with ability θ_i invests $\theta_i \ / \ 4R^2$, whereas an informal entrepreneur does not invest. We find that a decrease in registration costs increasing the number of formal entrepreneurs (who invest) an decreases the number of informal entrepreneurs (who do not invest). Consequently, aggregate investment goes up. Interestingly, the channel driving higher investment after formalization is not access to credit (there is no relaxation of a borrowing constraint).

Instead, this result follows from the assumption that formal entrepreneurs invest more, which is supported by the data.[22]

Note, further, that firms that formalize due to a reduction in F will be operated by entrepreneurs with relatively low levels of θ_i. These firms will also tend to be small in terms of assets because investment is proportional to θ_i. Hence, unless the increase in formalization is very large, only marginal entrepreneurs will formalize and the increase in profits may be small (if any).[23]

3. Data

The primary dataset used in this paper is multiple rounds of Mexico's survey of micro-enterprises (*Encuesta Nacional de Micronegosios*, ENAMIN). This cross-sectional survey covers a sample of individuals who declare that they are self-employed in a broader labor force survey (ENEU/ENOE). The ENAMIN is restricted to micro-firms with at most 5 workers for all economic sectors except manufacturing, where firms may have up to 15 workers (excluding the owner). Several rounds of the survey were conducted both before the SARE reform (1992, 1994, 1996, 1998, and 2002[24]) and after the reform (2008, 2010 and 2012). Each round includes about 15,000 firms.

In 2005 the ENAMIN became nationally representative (as opposed to an urban survey). Hence, to ensure that the data is comparable across years, we limit the sample to the main urban areas of the country. Table 2 is useful to better understand which municipalities are included in our sample. The table shows the 60 competitive cites that were intended targets for the SARE reform. For each year, the table also shows whether a city was included in the ENAMIN sample. Our dataset includes all the municipalities that adopted a SARE amongst the municipalities located in the cities listed in Table 2.

The ENAMIN allows a relatively precise construction of a wide variety of variables that represent basic firm and entrepreneur characteristics: profits, employment size, capital stock, time in business, and engagement with a wide variety of societal institutions. In addition, by matching the ENAMIN with the labor force survey, it is

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[22] Formal firms may invest more because they no longer need to hide from the tax authorities, and because their assets are protected by formal institutions.
[23] Marginal entrepreneurs are roughly indifferent between formality and informality.
[24] Only 2 municipalitites adopted in 2002.

possible to obtain several personal characteristics of the entrepreneur and his/her household. These include the size of the household and the entrepreneur's gender, age, level of schooling and position in the household (head or not).

The ENAMIN has a number of advantages for this study. First, it covers a large number of municipalities that implemented the reform. Second, it covers more indicators of formality than other datasets.[25] Third, ENAMIN is representative of the urban informal sector, and it follows international recommendations for gathering information about this population (ILO 2013). Instead, other datasets, such as Mexico's economic census, excludes mobile workers, many of which are informal.[26] Most importantly, ENAMIN has more detailed information on firm characteristics such access to credit than other datasets.[27]

For our main analysis, we also rely on additional datasets. We use administrative data from COFEMER. This data includes the number of municipalities adopting a SARE, the date of implementation, and information on whether the SARE remains operational. We also use data listing the industries that are eligible for the federal SARE (Catalogo de Giros SARE).[28] We use a list of competitive municipalities based on Cabrero et al. (2003).

We also rely on additional datasets to test for pre-existing differences between early adopting municipalities and late adopters. (1) We use the labor force survey (ENEU/ENOE), which is a quarterly panel (following individuals for 5 quarters). We use data for every two years over 1992-1998 and every year over 2000-2012. To make the population consistent with ENAMIN, we focus exclusively on small entrepreneurs (i.e. employers in small firms and self-employed workers). (2) We rely on economic data from Mexico's population census, which was conducted in 2000.

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[25] For instance, the economic census captures employment informality but not firm informality, and the labor force survey only has information on whether firms have a legal name.

[26] Only about 30% of the business in the ENAMIN were captured in the economic census, and excluded firms differ substantially from those included (see Table 5).

[27] I am not aware of any other dataset that could have been used to more reliably address the question addressed in this paper. For instance, Mexico does not have reliable administrative data from business registries because municipalities do not follow a unified system of recording registered firms.

[28] Though lists of eligible industries in most States are readily available, we do not use this information because federal and state lists are very similar (see Kaplan et. al. 2011).

Descriptive Statistics

Descriptive statistics are presented in Table 5. Column (1) presents averages for all firms in the sample over all available years. Columns (2) and (3) present averages for formal firms and informal firms, respectively. We define formal firms as those that are registered with any government authority; based on this definition, 32% of the firms in the sample are formal. The average firm in the sample is very small with less than one employee (in addition to the owner). The average firm, however, has been operating for 9 years, suggesting that micro-firms represent permanent employment. Firms have monthly profits of close to USD 500, on average (this amount may be considered as owner earnings). Average revenues are USD 1,400, while average expenses are USD 937. Roughly half of the firms pays some type of taxes. Though the average asset level is USD 5,000; the distribution of assets is skewed, and the median asset level is only USD 407. The most widespread source of financing is savings: 62% of firms used savings to start operations, and 30% of firms save to finance investments (based on data up to 1996). By comparison, 16% of firms applied for a loan from any source at any time after starting operations.[29]

The last two columns of Table 5 present mean differences between formal and informal firms. For virtually all variables, the estimated mean differences are large and statistically significant at conventional levels. For example, roughly 70% of formal firms keep accounting books, as compared to only 26% of informal firms. Formal firm profits are roughly twice as large as informal firm profits, on average. Formal firms are also 34 p.p. more likely to pay some form of taxes than informal firms. Further formal firms are 10 p.p. more likely to save to re-invest in their business. It is important to remember, however, that these differences cannot be interpreted as the impact of formalization. Instead, these differences may be explained, to some extent, by the fact firms choosing to formalize differ from informal ones.[30]

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[29] Almost 50% of firms that did not apply for a loan, reported needing one; and over 90% of firms that applied for a loan, reported receiving one. Hence, only firms that are likely to receive a loan may apply for one.

[30] For instance, only the most able entrepreneurs may choose to register their business, in part, due to the expectation that the business will succeed.

4. Empirical Strategy

Identification

In order to reliably estimate the effects of formality on firm performance, we need to rely on some source of exogenous variation. We want to find a variable that affects the registration decision, but is otherwise uncorrelated with outcomes.[31] We exploit the implementation of a reform in Mexico that affected the fixed costs of registering a business in different municipalities at different times (i.e., we exploit c*ross-municipality cross-time variation*). Our approach compares outcomes before and after SARE, between municipalities that adopted the program early, relative to municipalities that did not adopt the reform at the time of comparison, but would adopt it by 2012.[32]

For our identification to be valid, it also requires that the timing of adoption be exogenous. Given that COFEMER could not reach all target municipalities at the same time, we expect municipalities adopting SARE within a narrow time period of each other to be very comparable. Instead, very early-adopters and very late-adopters become increasingly less comparable (based on unconditional comparisons). To address this concern, Bruhn (2011) limited its sample to municipalities that adopted a SARE prior to 2004 (and she presents evidence that within this group, early-adopters and late-adopters are comparable).[33] Rather than truncating our sample, we control for broad patterns in municipality potential in our regressions, and we show that conditioned on potential there are no pre-treatment differences between early and late adopters.

We control for municipality potential by including fixed-effects in our regressions. We follow this strategy because municipality's potential changes little over time (large changes in population and infrastructure require time). Indeed an update of the Cabrero et. al. (2003) report shows that the list of Mexico's competitive cities has not changed much over a few years. As a second approach, we control for an index of municipality potential, which was designed by COFEMER to guide SARE

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[31] Tax changes do not meet this criteria because though they affect firms' registration choices, they also affect production costs. Changes to social insurance or labor regulations also affect production costs.

[32] Relying exclusively on data from municipalities that adopted a SARE by 2012, helps to control for any differences across municipalities that may affect the adoption decision.

[33] This choice may have also been driven by the fact that no additional data was available at the time the paper was written. Our approach allows us to exploit all the data available to date, and to includes more treated municipalities in the analysis.

implementation. The IGI index classifies municipalities into 5 broad groups, with 5 indicating the highest potential. The variables included in the index are shown in Table 3. Note however, that given that the IGI index is available only for one year, it drops out from our regressions when also including municipality fixed effects. [34]

Validity of the Identification Strategy

Although our identification strategy relies on comparing early adopters to late adopters, we start by comparing municipalities adopting SARE to non-adopters. Evidence that adopters and non-adopters are comparable would add even more credibility to the validity of our identification strategy.

Table 6 examines data from the 2000 Population Census for all the municipalities in the ENAMIN dataset. We consider various groups of municipalities: (a) early adopters (adopting between 2002 and 2004); (b) mid-adopters (adopting between 2005 and 2007); (c) late-adopters (adopting after 2008); and (d) non-adopters. Column (1) shows the average characteristics of non-adopting municipalities (i.e. the coefficient for the excluded category in a regression). Columns (2) to (4) show the difference between each group of adopting municipalities and non-adopters (excluded category). The upper panel of Table 6 shows that, based on unconditional comparisons, adopting and non-adopting municipalities are not comparable. However, these results are to be expected due to COFEMER's goal of reaching municipalities with higher potential first. Instead, our identification strategy requires only that municipalities be comparable after conditioning on potential. The bottom panel of Table 6 is similar to the upper panel, but it controls for IGI fixed effects. [35] After controlling for potential using the IGI index, we find virtually no differences between adopters and non-adopters.[36]

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[34] Data on the IGI index is available in a recent COFEMER report (2013). The index measures the importance that COFEMER assigns to creating a SARE in different municipalities. Though the index has been published only recently, it formalizes the criteria guiding COFEMER's objectives since program inception.

[35] Given that only one year of census data is available, we cannot control for potential using municipality fixed effects.

[36] Note that the few instances where statistically significant differences difference do exist do not per se invalidate our identification strategy; the main identifying assumption is that treated and control municipalities would have experienced similar *changes* in outcomes in the absence of the reform.

Table 7 presents additional tests for the validity of our identification strategy using data from the 2000 Census and from the Labor Force Survey. To further examine the pattern of implementation of the reform, we regress each of the variables shown in the previous table on the year (or quarter) of implementation. The first two columns present unconditional comparisons. The last two columns present comparisons controlling for IGI fixed effects. The coefficients on year of implementation are reported in Column (1) and (3). The coefficients on quarter of implementation are reported in Columns (2) and (4). As before, after controlling for potential, there is no statistically significant trend in the timing of adoption for most variables.

While the previous tests are encouraging, we also need to test for pre-treatment differences between early and late adopters using the ENAMIN data (i.e. exploring the same dataset and the same municipalities that will be used in our analysis). Table 8 tests for differences between early and late adopters in levels (using data for years prior to 2002). Whether we control for potential or not, we find little differences between adopters and non-adopters, on average. Table 9 shows the results of additional tests. The first column of Table 9 compares the changes in firm characteristics between adopters and non-adopters during the pre-reform period. The second column of the table regress each firm characteristic on the year of SARE adoption. In most cases, coefficients are not statistically significant suggesting that our approach is valid.[37]

Econometric Model

Our econometric approach relies on OLS regressions. We rely on two main models. While the first model is very parsimonious, the second includes a large number of controls. Our benchmark model is as follows[38]:

$$y_{imt} = \alpha_t + \alpha_m + \delta\, SARE_{mt} + \varepsilon_{imt} \quad (1)$$

where the subscript i denotes firms, m denotes municipalities, and t denotes years. α_t are time fixed effects and α_m are municipality fixed effects. The variable y_{imt} denotes any outcome of interest. For each municipality, $SARE_{mt}$ equals unity for the period in which a

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[37] Further, even though there are a very small number of instances when coefficients are significant, there is no clear pattern. Differences in some variables appear significant for some regressions but not others.
[38] The models are very similar to those in Bruhn (2011).

given municipality adopted a SARE and for all subsequent time periods. The coefficient of interest, δ, compares the mean change in outcomes of interest between municipalities adopting SARE early on relative to late adopters. We attribute any observed differences in firm performance across municipalities to differences in municipalities' average level of formalization.

We also estimate a richer model which is shown below:

$$y_{imt} = \alpha_t + \alpha_m + \alpha_{s\text{-}t} + \alpha_{s\text{-}t} + \delta\, SARE_{mt} + \beta X_{imt} + \varepsilon_{imt} \quad (2)$$

$\alpha_{s\text{-}t}$ are state-time fixed effects (i.e. state fixed effects interacted with a linear time trend);[39] $\alpha_{s\text{-}t}$ are industry-time fixed effects (i.e. industry fixed effects interacted with a linear time trend). X includes a number of controls such as predetermined variables interacted with a linear time trend, firm characteristics, and time-varying municipality characteristics. Predetermined variables include: (a) the average firm revenues and costs in each municipality prior to the reform, (b) the average number of days it took a firm in each state to open a business in 1998, and (c) the average population size in each municipality in 2000. Firm level controls include the year a firm was created, and whether a firm self-classifies as "self-employed" or "owner of a small business". Municipal level controls include political variables such as the party of the mayor in each municipality (i.e. whether the mayor belongs to the PRI, PAN or other party). Other plausible models are discussed in the robustness checks section.

Standard errors are clustered at the municipality level. Clustering standard errors allows to attain reliable inference in the presence of serial correlation, which is likely to occur in diff-in-diff models with several time periods (see Bertrand et al. 2004).

An alternative econometric approach would be to rely on instrumental variables (IV) to estimate the effects of formality on firm performance.[40] However, the IV approach would not be valid id SARE had general equilibrium effects.[41] It is not clear whether the SARE reform had any general equilibrium effects. While Bruhn (2011) finds

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[39] Controlling for state fixed effects is important because municipalities in the same state tended to adopt SARE at similar times (i.e. geographic clustering of SARE adoption). Given that state does vary over time, we interact the state fixed effects with a linear time trend.

[40] We could use SARE adoption as an instrument for formality.

[41] It would attribute any observed changes in firm performance to firms that formalize, ignoring any effects of SARE on untreated firms (such as changes in the degree of competition faced by firms that did not react to the SARE reform).

evidence of changes in prices in treated municipalities; Kaplan et. al. (2011) finds different results. Nevertheless, to ensure the reliability of our results, we choose to rely on OLS estimates rather than IV estimates.

5. Results

We discuss the effects of the SARE reform on (a) formality, (b) firm performance, and (c) the potential channels driving changes in performance. Models are estimated using two different samples: the first sample includes firms regardless of when they started operations; the second sample only includes firms were created prior to the SARE reform (i.e. before 2002).[42] For each sample, we present results for the two different models discussed in the previous section.

Effects on Formality

Table 10 presents estimates for the effects of the SARE reform on firm registration (with any government authority). Results suggest that SARE increased formality between 4 and 6 p.p., and estimates are statistically significant at conventional levels for both samples (for our preferred model). If we consider that only 32% of firms were registered before the reform, our estimates imply an 18% increase in formalization.[43] Such estimates are much larger than those obtained in Bruhn (2011) and Kaplan et. al. (2011). Our results may be much larger because we study only micro-entrepreneurs as opposed to the entire Mexican population, and one would expect micro-entrepreneurs to respond more strongly.[44] Further, Bruhn and Kaplan et. al., study whether firms registered with either the tax agency or the social security agency, respectively. If we believe that some firms may register with one agency but not the other, then one would expect our estimates to be larger.

One contribution of our paper is that we are able to estimate the effects of SARE on more than one measure of formality. Table 11 presents estimates for the effects of SARE on various indicators of formality: (a) a dummy indicating whether a firm is

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[42] Results for each of these samples are labeled "All" and "Existing", respectively.
[43] Percentage increase in formalization is calculated as (38-32)/32*100.
[44] Note that our results are unbiased only for the population of micro-entrepreneurs and they do not provide information about the overall effects of SARE for the population at large.

registered with a business association, (b) a dummy indicating if it keeps any books, and (c) a dummy indicating if it keeps formal books with the help of an accountant. Results suggest that SARE increased the share of firms that register with a business association by between 1 and 4 p.p. (estimates are statistically significant at conventional levels). Instead, SARE had no effect on book-keeping. This later result is particularly important because it suggest that any effects of SARE on firm performance may not be driven by improvement in book-keeping, and they may represent real changes in firm behavior.[45]

Another interesting aspect of our results is that SARE increased formalization for both of our samples (including the sample of firms created prior to 2002). Instead, some of the earlier papers in the literature attributed any impacts of reforms reducing the costs of registration to new entrepreneurs opening a formal business (see for instance Fajnzylber et. al. (2011)). However, given that we have data on when firms were created (which is generally not available), we can answer this question more reliably than earlier papers.[46] We also notice that our results on formalization are very similar for the sample of "all" firms and the sample of "existing firms". This similarity may be explained because a very large share of Mexican micro-firms are relatively old and they were created prior to 2002.

Effects on Firm Performance

Table 12 presents estimates of the effects of SARE on different measures of firm performance, such as monthly profits, revenues and expenses. Earlier studies of the SARE reform did not have access to such detail firm level information. The table suggests that firms in SARE municipalities experienced an economically large and statistically significant increase in revenues (at conventional levels of significance). This result is consistent for all samples shown in the table (as well as samples not shown in this paper, such as comparing SARE adopting municipalities to non-adopters). This is also consistent with the findings in Galiani and Melendez (2013) for Colombia.

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[45] We find no robust effect on other less direct measures of formality, such as registering with the health authority, and these results are not presented for brevity. The impact on less direct measures of formality may be small and it is possible that our dataset may not be large enough to capture these changes robustly.
[46] For instance, Bruhn (2011) could follow firms only for 5 quarters (the Mexican LFS only follows individuals for that amount of time), any firm formalizing after that time would not be considered.

Table 12 suggests that the effects of SARE on firm costs and profits are somewhat mixed. For these latter variables, the magnitude and significance of our estimates vary depending on the sample used, the controls included, and whether variables are measured in levels or logarithms. The differences across samples seem important. While we find no increase in profits for the sample including all firms, profits may have increased by as much as 20% for existing firms.[47] Such differences may occur because existing entrepreneurs know their level of ability more precisely and can benefit more from the reform.

There are plausible explanations for the lack of an effect on profits for some sub-samples. As suggested by the conceptual framework, a small reduction in the cost of formalization encourages formalization only for entrepreneurs who are nearly indifferent between registering and remaining informal. Hence, unless the costs of formalization are dramatically reduced (starting from very high costs), the benefits observed by entrepreneurs may be small and difficult to measure precisely (large standard errors). Further, not all the entrepreneurs that choose to formalize may benefit. For instance, if entrepreneurs do not know their level of ability precisely, they may choose to formalize even if they would have been better off by remaining informal.

Our overall conclusions are more optimistic than earlier papers, which often finds no effects of formalization on firm performance (see for instance De Mel et. al. (2013)). Based on the large and robust effect that SARE had on revenues (and the somewhat mixed effects on profits), we conclude that micro-firms may benefit from formalization, and that reforms reducing the costs of registration are useful.

Potential Channels

What are the channels driving the observed increase in revenues (and to some extent profits)? According to the conceptual framework, we can expect aggregate investment to increase after a reduction in the costs of formalization, as occurred due to SARE. Table 13 presents estimates of the effects of SARE on different investment

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[47] The lack of impact on profits for the sample of all firms hides the fact that SARE did have a large and statistically significant effect on revenues, and a somewhat smaller effect on expenses. It is also important to note that whether the effects on revenues is larger than the effect on expenses varies depending on the municipalities used in the analysis. For instance, when comparing treated and non-treated municipalities, the increase in firm expenses is almost as large as the effect on revenues.

proxies: (a) a dummy variable equal to unity for firms that are equipped with assets; (b) assets measured in logarithms, and (c) assets measured in levels. Based on these variables, we find that SARE increased the likelihood of holding assets and the average value of the assets held by firms. Hence, one may conclude that, as predicted by the conceptual framework, firms in municipalities adopting a SARE early on may have invested more relative to firms in other municipalities. Further, the magnitude of coefficient estimates is very similar based on all models.

Table 13 also shows the effects of SARE on employment. We find that SARE had no effect on the number of firm workers, whether paid or unpaid. In fact, given that SARE did not simplify the costs for registering workers with social security, we expect to observe little impact on employment.[48] **Error! Reference source not found.** shows the effects of SARE on labor productivity and capital intensity. We find that labor productivity increased. Workers in firms with more assets may be able to better use their time. Capital intensity also increased. This is not surprising given that we observed a significant increase in capital but little effect on the number of workers.

Another important question is how firms finance the observed increase in assets. One possibility, is that firms that formalize have more access to credit, allowing them to afford buying more assets. In other words, credit constrained firms may experience a relaxation of their borrowing constraint, because banks may be more wiling to lend to formal firms.[49] Another possibility, is that entrepreneurs that choose to become formal may choose a different investment strategy than informal firms, regardless of how they finance that investment.

Table 14 shows the effect of SARE on access to credit. We have information on both credit demand (whether a firm applies for a loan) and supply (whether the bank grants a loan, conditional on a firm applying). We also have information on the reasons for not applying to a loan. We find no effect of SARE on any of these variables. This finding is not surprising because of the extremely low access to credit for Mexican small firms. Indeed, as shown in the descriptive statistics, only 16% of firms applied for a loan

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[48] Most of the firms that formalize may have no employees in addition to the owner. The costs of registering the firm owner for social security may not be as high, because the entrepreneur benefits from social security in exchange of its contributions.

[49] There is a large literature suggesting that one important reason for informal firms to formalize is to access credit (see for instance Straub (2005)).

after starting operations. We take additional steps to make sure that results are not driven by access to credit. In particular, we re-estimate the effect of SARE on firm assets but excluding all firms that have ever applied or received a loan. These latter group of firms includes firms that may finance most of their investment via savings. Interestingly, the effects of SARE on savers is roughly the same as the effects of SARE on the overall sample.

6. Robustness Checks

We estimate a number of different models to test the robustness of our results. These models include more controls than the first model discussed in the main text of the paper, but are more parsimonious than the second model discussed in the paper. Model (1) controls for year fixed effects, municipality fixed effect, and firm level controls. Firm level controls include the year the firm was created, and whether the owner views himself as "self employed" or "owner of a small business". Model (2) adds state fixed effects interacted with a linear time trend. Model (3) adds predetermined variables interacted with a time trend. Model (4) adds municipality level control (particularly, political variables). Model (5) adds industry fixed effects interacted with a time trend. Table 15 and Table 16 presents results from the estimation of the above models for the sample of all firms and the sample of existing firms. The coefficients presented in these tables are very similar to those presented earlier in the paper, supporting the validity of our results.

7. Conclusion and Policy Recommendations

This paper evaluated the effects of a 2002 reform in Mexico, which simplified the costs of registering a business. We corroborate the findings in earlier papers that the reform increased formalization. We also find that, on average, firm revenues increased more in municipalities that adopted the reform early on relative to municipalities that adopted the reform in future years. The main channel driving higher revenues is an increase in physical capital, which allowed for higher labor productivity. We also find that the capital increase is not financed by loans, suggesting that the widespread belief that firms formalize to obtain credit is not supported by the data.

Overall our results are more optimistic than those in earlier papers. Our estimates of the impacts of the reform on formalization is much larger than in Bruhn (2011) and Kaplan et. al. (2011). Further, unlike some recent papers in the literature, we find that the reform had positive impacts on profits (at least for some groups). We conclude that, when adequately designed and implemented, a reform that reduces the costs of registering a business may be desirable. While the magnitude of some of our coefficient estimates may seem large (i.e. a 50% increase in assets). It is important to remember that the results apply only to the population of micro-firms. A large increase in the assets of micro-firms may still be very small relative to the aggregate stock of physical in a municipality.

Tables

Table 1: Entities Implementing a SARE

	Number of Entities Implementing										
	02	03	04	05	06	07	08	09	10	11	12
Timing											
Municipalities	2	9	28	46	25	26	1	28	19	19	6
Metro municipalities	2	9	20	34	13	7	0	11	9	6	4
Clustering											
States	2	7	15	17	13	10	1	10	8	5	3
Munis. in most active state	2	4	10	9	8	6	1	6	7	12	6

Note: The first row shows the number of municipalities adopting SARE each year. The second row shows the number of competitive municipalities adopting SARE each year. The third row shows the number of states where at least one municipality adopted SARE. The fourth column show the number of municipalities that adopted SARE in the state with the most SARE adoptions.

Table 2: Competitive Cities

		1992	1994	1996	1998-02	2008-12			1992	1994	1996	1998-02	2008-12
1	Queretaro		x	x	x	x		Mazatlan					
2	Monterrey	x	x	x	x	x	21	Villahermosa	x	x	x	x	x
3	Chihuahua	x	x	x	x	x	22	Merida	x	x	x	x	x
4	Mexico City	x	x	x	x	x	23	Pachuca			x	x	x
5	Saltillo	x	x	x	x	x		Celaya		x	x	x	x
	Mexicali				x	x		Ensenada					
6	Toluca	x	x	x	x	x		Ciudad Obregon					
7	Tijuana	x	x	x	x	x		Coatzacoalcos	x	x	x	x	x
	Ciudad Juarez	x	x	x	x	x		Tehuacan					
8	Cancun			x	x	x	24	Colima	x	x	x	x	x
9	Guadalajara	x	x	x	x	x		Irapuato		x	x	x	x
10	Tampico	x	x	x	x	x		Los Mochis					
11	Aguascalientes	x	x	x	x	x	25	Zacatecas	x	x	x	x	x
12	San Luis	x	x	x	x	x		Ciudad del Carmen			x	x	x
	Los Cabos						26	La Paz			x	x	x
13	Hermosillo	x	x	x	x	x	27	Tepic	x	x	x	x	x
14	Puebla	x	x	x	x	x		Ciudad Victoria					x
	Torreon	x	x	x	x	x		Cuautla					
15	Cuernavaca	x	x	x	x	x	28	Oaxaca	x	x	x	x	x
	Monclova		x	x	x	x	29	Acapulco	x	x	x	x	x
	Nuevo Laredo	x	x	x	x	x	30	Campeche	x	x	x	x	x
	Puerto Vallarta							Xalapa					
	Reynosa						31	Tlaxcala		x	x	x	x
16	Leon	x	x	x	x	x		Uruapan					
	Matamoros	x	x	x	x			Orizaba	x	x	x	x	x
17	Culiacan	x	x	x	x	x		Minatitlan					
18	Durango	x	x	x	x	x		Chetumal					x
19	Morelia	x	x	x	x	x	32	Tuxtla Gutierrez	x	x	x	x	x
20	Veracruz	x	x	x	x	x		Poza Rica					
	Manzanillo	x	x	x	x	x		Chilpancingo					

Source: Cabrero et al. (2003), and ENAMIN documentation.

Notes: The table list the 60 Mexican cities that were identified as "competitive" in Cabrero et al. (2003). The 32 cities highlighted are the cities in the Mexican Labor Force Survey for which the survey is representative. The "x" symbol in the table indicates whether a given city was included in the ENAMIN sample in a given year.

Table 3: Variables Included in the IGI

Variable (and weight)	Source
Population (2010) - 10%	Population Census 2010 (INEGI)
Economically Active Population (2010) - 10%	
Population (2015) - 10%	Projections based on population growth rate over 2000-2010
Municipal GDP in US dollars - 30%	INEGI data, converted to dollars using annual average of the fix exchange rate of the Central Bank of Mexico and converted to PPP using WDI-2009.
Businesses eligible for SARE - 30%	National Statistical Directory of Economic Units 2009 (*Directorio Estadístico Nacional de Unidades Económicas*, DENUE) Based on the number of eligible (i.e. low-risk) industries.
Manufacturing Businesses - 5%	
Other Businesses - 5%	

Source: COFEMER
Notes: Other businesses refer to the number of businesses other than manufacturing.

Table 4: Literature Review

Authors	Description
Galiani and Melendez (2013)	Field experiment in Colombia reduced the costs of formalization
De Mel et. al. (2013)	Field experiment in Sri Lanka: *(a)* 1st group received refund of registration costs. *(b)* 2nd group received also a cash grant
Fajnzylber et. al. (2011)	RDD studying introduction Brazil's SIMPLES, which reduced and simplified business taxes. SIMPLES eligibility used as IV for decision to formalize.
McKenzie and Sakho (2010)	Exploited firms' distance to the tax office (conditional on distance to the city center) as an instrument for registration, amongst Bolivian firms.
Venancio (2010)	Studied a business registration reform that took place in 2005 in Portugal.
Fajnzylber et. al. (2009)	Use ENAMIN data for Mexico. Relied on propensity-score matching and control function techniques to control for selection to formality.

Table 5: Descriptive Statistics

	All (1)	Formal (2)	Informal (3)	Difference: (2)-(3) Coeff.	SE
Formality					
Registered with Any Gov. Authority	32%	100%	0%		
Registered with Business Assoc.	13%	35%	3%	0.33***	(0.002)
Registered with Health Dept.	5%	14%	0%	0.15***	(0.002)
Keeps any Books	41%	71%	26%	0.46***	(0.003)
Keeps Formal Books	21%	45%	9%	0.37***	(0.003)
Characteristics					
Total Workers	0.94	1.37	0.67	0.78***	(0.013)
Salaried Workers	0.57	0.78	0.43	0.40***	(0.010)
Non-salaried workers	0.55	0.82	0.36	0.55***	(0.014)
Age (years)	9.36	10.67	8.73	2.02***	(0.066)
Catured Census	33%	61%	18%	0.42***	(0.004)
Performance					
Profits $	488	755	363	325.66***	(13.2)
Revenues $	1,400	2,382	931	1,391.81***	(28.9)
Expenses $	937	1,684	572	1,111.66***	(31.2)
Taxation					
P(Tax Expenses>0)	52%	74%	41%	0.34***	(0.003)
Tax Expenses $	191	416	72	326.11***	(14.517)
Assets					
Has Premises	52%	53%	51%	0.02***	(0.005)
P(Assets>0)	82%	94%	76%	0.16***	(0.002)
Assets $	5,457	11,853	2,508	9,746.39***	(220.9)
Initial Financing					
Bank	2%	2%	1%	0.01***	(0.001)
Savings	62%	58%	64%	-0.07***	(0.003)
Other	36%	40%	34%	0.06***	(0.003)
Other Financing					
Saves to Invest	28%	35%	23%	0.10***	(0.005)
Applied for a Loan	16%	23%	13%	0.12***	(0.002)
Needed Loan (Share of Not Applied)	46%	50%	44%	0.05***	(0.004)
Received Loan (Share of Applied)	92%	92%	92%	0.01**	(0.004)
Bank Loan (Share of Receied Loan)	16%	18%	15%	0.07	(0.068)

Notes: Based on all rounds of data available. For most variables the number of observations is 117,259. Data on whether the firm saves to invest is only available over 1992-96. All monetary values are expressed in Real US Dollars of 2012.

Table 6: Comparisons between different types of municipalities

| | Non-adopters | Difference relative to non-adopters | | |
		Early adopters (02-04)	Mid adopters (05-07)	Late adopters (08-12)
Raw Differences				
Population [a]	94,000***	438,834***	197,488***	33,870
	(13,870)	(72,724)	(39,146)	(30,392)
Economically active pop (as a share of pop) [a]	0.35***	0.03***	0.03***	0.01**
	(0.00)	(0.01)	(0.01)	(0.01)
Workers in tertiary sector (as a share of occupied pop) [a]	0.49***	0.11***	0.07***	0.04*
	(0.01)	(0.02)	(0.02)	(0.03)
Population with IMSS health insurance (as share of pop) [a]	0.29***	0.14***	0.15***	0.02
	(0.01)	(0.02)	(0.02)	(0.02)
Log (Municipal Income per Capita) [b]	6.596***	0.325***	0.329***	0.0969
	(0.04)	(0.07)	(0.07)	(0.10)
Differences Whithin IGIR				
Population [a]	169,748***	77,397	-40,616	-65,465**
	(15,944)	(64,724)	(37,515)	(15,944)
Economically active pop (as a share of pop) [a]	0.36***	-0.01	0.00	0.00
	(0.00)	(0.01)	(0.01)	(0.01)
Workers in tertiary sector (as a share of occupied pop) [a]	0.52***	-0.04**	-0.05**	-0.01
	(0.01)	(0.02)	(0.02)	(0.02)
Population with IMSS health insurance (as share of pop) [a]	0.31***	0.03	0.06***	-0.03
	(0.01)	(0.03)	(0.02)	(0.02)
Log (Municipal Income per Capita) [b]	6.647***	0.0872	0.156**	0.0615
	(0.04)	(0.08)	(0.08)	(0.11)

Source: [a] Data from Population Census 2000, based on 357 municipalities. [b] Data from INEGI's database of municipal finance.
Note: Includes only competitive municipalities. The table shows municipal averages. Includes metro area fixed effects.

Table 7: Patterns in the timing of adoption

	Raw		Whithin Groups	
	(a) SARE year	*(b)* SARE quarter	*(a)* SARE year	*(b)* SARE quarter
[a] Pop Census (N=115)				
Population (Ths)	-54.83***	-13.99***	-50.68**	-16.54**
	(11)	(3)	(25)	(6)
Economically active pop (as a share of pop)	-0.003**	-0.001***	0.000	0.000
	(0.00)	0.00	(0.00)	(0.00)
Workers in tertiary sector (as a share of occupied pop)	-0.010**	-0.002**	-0.012	-0.003
	(0.00)	(0.00)	(0.01)	(0.00)
Population with IMSS health insurance (as share of pop)	-0.020***	-0.005***	-0.002	-0.002
	(0.00)	(0.00)	(0.01)	(0.00)
[b] LFS (N=81)				
All Sectors: Formality	-0.007*	-0.00185*	0.003	0.00053
	(0.00)	(0.00)	(0.01)	(0.00)
All Sectors: Change in Formality	-0.005	-0.00119	-0.001	0.00004
	(0.01)	(0.00)	(0.01)	(0.00)
Low-risk: Formality	-0.004	-0.00106	0.005	0.00126
	(0.00)	(0.00)	(0.01)	(0.00)
Low-risk: Change in Formality	-0.007	-0.00157	0.008	0.00256
	(0.01)	(0.00)	(0.01)	(0.00)
High risk: Formality	-0.017**	-0.00456**	-0.012	-0.00376
	(0.01)	(0.00)	(0.01)	(0.00)
High risk: Change in Formality	0.007	0.00171	-0.011	-0.00256
	(0.01)	(0.00)	(0.02)	(0.01)

Note: Includes only SARE municipalities in metro areas. The table shows municipal averages. [a] Data from Population Census 2000, based on 115 municipalities. [b] Data from LFS 2000, based on 81 municipalities.

Table 8: Differences between Early and Late Adopters, Levels

	Raw			Controlling for Potential		
	Early	*Late*	*Dif.*	*Early*	*Late*	*Dif.*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
(a) P(Formal=1)	0.270	0.323	-0.053	0.270	0.332	-0.063
			(0.048)			(0.047)
(b) Reg. Assoc.	0.115	0.123	-0.008	0.114	0.126	-0.012
			(0.018)			(0.018)
(c) Books	0.564	0.573	-0.009	0.564	0.576	-0.012
			(0.031)			(0.031)
(d) Formal Books	0.304	0.292	0.012	0.304	0.299	0.005
			(0.033)			(0.032)
(e) Profit (log)	7.675	7.601	0.073	7.665	7.612	0.052
			(0.140)			(0.143)
(f) Profit ($)	570.671	419.056	151.615	563.721	423.445	140.276
			(149.315)			(154.312)
(g) Revenues (log)	8.772	8.663	0.110	8.765	8.671	0.094
			(0.092)			(0.099)
(i) Revenues ($)	1736.669	1399.557	337.112	1723.583	1424.438	299.145
			(219.628)			(225.530)
(j) Expenses (log)	7.636	7.111	0.526**	7.631	7.120	0.511*
			(0.250)			(0.258)
(k) Expenses ($)	1183.604	969.154	214.450	1179.287	994.918	184.369
			(154.984)			(154.663)
(l) P(Assets>0)	0.899	0.906	-0.007	0.899	0.907	-0.008
			(0.013)			(0.013)
(m) Assets (log)	8.192	8.084	0.107	8.194	8.100	0.095
			(0.278)			(0.279)
(n) Assets ($)	6480.380	5919.522	560.858	6530.794	6283.616	247.178
			(1,078.480)			(1,041.733)
(o) Applied Loan	0.093	0.064	0.030**	0.094	0.059	0.035**
			(0.013)			(0.014)
(q) Received Loan	0.913	0.885	0.028	0.915	0.887	0.028
			(0.034)			(0.032)
(r) Needed Loan	0.668	0.642	0.026	0.667	0.651	0.016
			(0.069)			(0.071)
(s) L. Productivity	8.498	8.417	0.080	8.491	8.419	0.072
			(0.085)			(0.093)
(t) Cap. Int. (log)	7.669	7.592	0.077	7.672	7.593	0.079
			(0.199)			(0.200)

Table 9: Differences between Early and Late Adopters, Changes

	Changes (1)	*SAREyear* (2)
(a) P(Formal=1)	0.018	-0.000
	(0.046)	(0.008)
(b) Reg. Assoc.	-0.040	0.002
	(0.049)	(0.004)
(c) Books	-0.093*	-0.005
	(0.050)	(0.006)
(d) Formal Books	-0.010	-0.004
	(0.040)	(0.005)
(e) Profit (log)	-0.094	-0.028
	(0.161)	(0.021)
(f) Profit ($)	-943.795	-44.181
	(748.201)	(36.615)
(g) Revenues (log)	-0.122	-0.040**
	(0.185)	(0.019)
(h) Revenues ($)	-1,095.104	-57.088
	(824.491)	(49.628)
(i) Expenses (log)	-0.320	-0.105***
	(0.499)	(0.037)
(j) Expenses ($)	-244.344	-8.990
	(260.025)	(30.478)
(k) P(Assets>0)	0.022**	0.001
	(0.009)	(0.002)
(l) Assets (log)	0.101	-0.064
	(0.328)	(0.048)
(m) Assets ($)	-1,404.793	-126.399
	(4,644.661)	(233.274)
(n) Applied Loan	-0.086**	-0.004
	(0.041)	(0.003)
(o) Received Loan	-0.183	-0.000
	(0.134)	(0.005)
(p) Needed Loan	-0.117	-0.003
	(0.086)	(0.009)
(q) L. Productivity	-0.078	-0.036**
	(0.206)	(0.016)
(r) Cap. Int. (log)	0.186	-0.056
	(0.212)	(0.036)

Table 10: Registration with Government Authorities

	All Firms		Existing Firms	
	Model 1	*Model 2*	*Model 1*	*Model 2*
	(1)	*(2)*	*(3)*	*(4)*
SARE	0.038*	0.058***	0.027	0.064**
	(0.022)	(0.020)	(0.025)	(0.026)

Notes: The table reports the coefficient and standard errors for a treatment dummy, based on different models. For all models, the dependent variable is a dummy equal to one for firms that have registered with any government authority. All standard errors are clustered at the municipality level. All models include year fixed effects. Model 1 controls for municipality fixed effects. Model 2 controls for municipality-industry fixed effects, as well as a number of other control variables.

Table 11: Other Indicators of Formality

	All Firms		Existing Firms	
	Model 1	*Model 2*	*Model 1*	*Model 2*
	(1)	*(2)*	*(3)*	*(4)*
(a) Reg. Assoc.	0.012	0.024*	0.011	0.036**
	(0.018)	(0.013)	(0.022)	(0.017)
(b) Books	-0.021	0.007	-0.028	0.012
	(0.030)	(0.030)	(0.029)	(0.035)
(c) Formal Books	0.000	0.010	-0.001	0.024
	(0.021)	(0.016)	(0.020)	(0.020)

Notes: The table reports the coefficient and standard errors for a treatment dummy, based on different models. Each row shows results from a separate regression, where the dependent variable is listed. All standard errors are clustered at the municipality level. All models include year fixed effects. Model 1 controls for municipality fixed effects. Model 2 controls for municipality-industry fixed effects, as well as a number of other control variables.

Table 12: Effects on Profits, Revenues, and Expenses

	All Firms		Existing Firms	
	Model 1	*Model 2*	*Model 1*	*Model 2*
	(1)	*(2)*	*(3)*	*(4)*
(a) Profit (log)	0.165*	0.138	0.211**	0.210**
	(0.099)	(0.087)	(0.106)	(0.100)
(b) Revenues (log)	0.185**	0.148**	0.167**	0.158*
	(0.074)	(0.060)	(0.069)	(0.082)
(c) Expenses (log)	-0.006	0.071	-0.027	0.088
	(0.136)	(0.107)	(0.118)	(0.145)
(d) Profit ($)	320.926**	212.892**	244.588*	248.105**
	(130.529)	(81.653)	(130.226)	(107.820)
(e) Revenues ($)	684.795**	584.226***	533.981**	604.856**
	(282.576)	(199.456)	(252.696)	(303.533)
(f) Expenses ($)	226.928	273.507**	171.006	275.623
	(156.134)	(133.562)	(134.577)	(201.554)

Notes: The table reports the coefficient and standard errors for a treatment dummy, based on different models. Each row shows results from a separate regression, where the dependent variable is listed. All standard errors are clustered at the municipality level. All models include year fixed effects. Model 1 controls for municipality fixed effects. Model 2 controls for municipality-industry fixed effects, as well as a number of other control variables.

Table 13: Effects on Capital and Labor

	All Firms		Existing Firms	
	Model 1	*Model 2*	*Model 1*	*Model 2*
	(1)	*(2)*	*(3)*	*(4)*
(a) P(Assets>0)	0.062***	0.032**	0.042**	0.031*
	(0.016)	(0.012)	(0.017)	(0.018)
(b) Assets (log)	0.751***	0.449***	0.568***	0.441**
	(0.204)	(0.129)	(0.200)	(0.202)
(c) Assets ($)	2,109.381**	1,235.752	2,030.177*	1,127.736
	(1,042.577)	(763.793)	(1,087.663)	(908.934)
(d) Workers - total	0.027	0.038	0.036	-0.005
	(0.073)	(0.068)	(0.092)	(0.078)
(e) Workers - paid	0.050	0.051	0.088	0.072
	(0.065)	(0.047)	(0.091)	(0.091)
(f) Workers - unp.	-0.014	0.027	-0.022	-0.027
	(0.060)	(0.069)	(0.066)	(0.069)

Notes: The table reports the coefficient and standard errors for a treatment dummy, based on different models. Each row shows results from a separate regression, where the dependent variable is listed. All standard errors are clustered at the municipality level. All models include year fixed effects. Model 1 controls for municipality fixed effects. Model 2 controls for municipality-industry fixed effects, as well as a number of other control variables.

Table 14: Effects on Labor Productivity and Capital Intensity

	All Firms		Existing Firms	
	Model 1	*Model 2*	*Model 1*	*Model 2*
	(1)	*(2)*	*(3)*	*(4)*
(a) L. Productivity	0.229**	0.241***	0.320***	0.317***
	(0.093)	(0.089)	(0.097)	(0.110)
(b) Cap. Int. (log)	0.372*	0.282	0.333	0.117
	(0.216)	(0.175)	(0.223)	(0.241)

Notes: The table reports the coefficient and standard errors for a treatment dummy, based on different models. Each row shows results from a separate regression, where the dependent variable is listed. All standard errors are clustered at the municipality level. All models include year fixed effects. Model 1 controls for municipality fixed effects. Model 2 controls for municipality-industry fixed effects, as well as a number of other control variables.

Table 14: Effects on Credit

	All Firms		Existing Firms	
	Model 1	*Model 2*	*Model 1*	*Model 2*
	(1)	*(2)*	*(3)*	*(4)*
(a) Applied Loan	-0.003	-0.005	0.001	0.009
	(0.022)	(0.011)	(0.024)	(0.016)
(b) Received Loan	-0.018	0.009	-0.018	0.003
	(0.022)	(0.027)	(0.024)	(0.042)
(c) Needed Loan	0.029	0.037	0.016	0.056
	(0.034)	(0.042)	(0.038)	(0.049)
(d) P(Assets>0)	0.064***	0.038***	0.046**	0.047**
	(0.018)	(0.014)	(0.020)	(0.019)
(e) Assets (log)	0.752***	0.487***	0.614***	0.568***
	(0.223)	(0.131)	(0.232)	(0.211)
(f) Assets ($)	1,977.100*	1,126.973	2,035.504*	1,186.025
	(1,132.342)	(834.804)	(1,214.454)	(959.219)

Notes: The table reports the coefficient and standard errors for a treatment dummy, based on different models. Each row shows results from a separate regression, where the dependent variable is listed. All standard errors are clustered at the municipality level. All models include year fixed effects. Model 1 controls for municipality fixed effects. Model 2 controls for municipality-industry fixed effects, as well as a number of other control variables.

Table 15: Additional Models, All Firms

	Models							
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
(a) P(Formal=1)	0.03	0.04*	0.04*	0.05**	0.04*	0.06***	0.06***	0.06***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
(b) Reg. Assoc.	0.01	0.02	0.02	0.02*	0.01	0.02*	0.02*	0.03*
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
(c) Profit (log)	0.14	0.13	0.13	0.13	0.14	0.14	0.14*	0.14
	(0.09)	(0.10)	(0.09)	(0.09)	(0.10)	(0.10)	(0.09)	(0.09)
(d) Revenues (log)	0.17***	0.10	0.11	0.11	0.20***	0.15***	0.15**	0.16**
	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)	(0.06)
(e) Expenses (log)	-0.01	-0.04	-0.05	-0.06	0.11	0.09	0.09	0.09
	(0.13)	(0.11)	(0.12)	(0.12)	(0.11)	(0.10)	(0.10)	(0.11)
(f) Assets (log)	0.69***	0.46**	0.47***	0.49***	0.66***	0.50***	0.51***	0.52***
	(0.20)	(0.17)	(0.16)	(0.15)	(0.20)	(0.16)	(0.13)	(0.13)
(g) L. Productivity	0.22**	0.17*	0.17*	0.17*	0.24***	0.24**	0.23**	0.23**
	(0.09)	(0.10)	(0.10)	(0.10)	(0.09)	(0.09)	(0.09)	(0.09)
(h) Cap. Int. (log)	0.35	0.32*	0.33*	0.33*	0.30	0.31*	0.31*	0.32*
	(0.22)	(0.17)	(0.17)	(0.17)	(0.22)	(0.18)	(0.18)	(0.17)

Notes: The table reports the coefficient and standard errors for a treatment dummy, based on different models. Each row shows results from a separate regression, where the dependent variable is listed. All standard errors are clustered at the municipality level. Model 1 to 4 include municipality fixed effects. Models 1 and 5 control for state-year fixed effects, firm age, and owner calssification. Models 2 and 6 add industry-year fixed effects to the previous controls. Models 3 and 7 add interactions of pre-determined variables and year. Models 4 and 8 add political variables.

Table 16: Additional Models, Existing Firms

					Models			
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
(a) P(Formal=1)	0.02	0.04	0.04	0.05	0.03	0.06**	0.06**	0.07**
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
(b) Reg. Assoc.	0.01	0.03	0.03	0.03	0.01	0.04**	0.04**	0.04**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
(c) Profit (log)	0.20*	0.23**	0.23***	0.23***	0.18	0.21*	0.21**	0.21**
	(0.10)	(0.09)	(0.08)	(0.08)	(0.11)	(0.11)	(0.10)	(0.10)
(d) Revenues (log)	0.15**	0.12	0.12	0.12	0.18**	0.17**	0.17*	0.17*
	(0.06)	(0.08)	(0.08)	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)
(e) Expenses (log)	-0.03	-0.07	-0.08	-0.08	0.11	0.12	0.12	0.11
	(0.12)	(0.13)	(0.14)	(0.14)	(0.11)	(0.14)	(0.14)	(0.15)
(f) Assets (log)	0.51**	0.38*	0.39**	0.41**	0.54**	0.48**	0.48**	0.50***
	(0.22)	(0.20)	(0.18)	(0.17)	(0.22)	(0.22)	(0.19)	(0.19)
(g) L. Productivity	0.30***	0.29**	0.27**	0.27**	0.32***	0.33***	0.32***	0.31***
	(0.10)	(0.11)	(0.11)	(0.10)	(0.10)	(0.12)	(0.11)	(0.11)
(h) Cap. Int. (log)	0.26	0.19	0.17	0.18	0.22	0.16	0.14	0.14
	(0.24)	(0.23)	(0.23)	(0.23)	(0.23)	(0.24)	(0.23)	(0.23)

Notes: The table reports the coefficient and standard errors for a treatment dummy, based on different models. Each row shows results from a separate regression, where the dependent variable is listed. All standard errors are clustered at the municipality level. Model 1 to 4 include municipality fixed effects. Models 1 and 5 control for state-year fixed effects, firm age, and owner calssification. Models 2 and 6 add industry-year fixed effects to the previous controls. Models 3 and 7 add interactions of pre-determined variables and year. Models 4 and 8 add political variables.

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