

Entry Regulation and Business Start-ups: Evidence from Mexico

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Abstract

We estimate the effect on business start-ups of a program that significantly speeds up firm registration procedures. The program was implemented in Mexico in different municipalities at different dates. Our estimates suggest that new start-ups increased by about 5% per month in eligible industries, and we present evidence that this is a causal effect. Most of the effect is temporary, concentrated in the first 15 months after implementation. The effect is robust to several specifications of the benchmark control group, to the inclusion of different time trends, and to the use of different samples. The estimated effect is much smaller than World Bank estimates for other countries and the effect stated by Mexican authorities.

KEYWORDS: Firm Start-Ups; Regulation; Informal Sector; Program Evaluation

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1 Introduction

Firm creation has been believed to be an important channel of GDP growth at least since Joseph Schumpeter. In addition to expanding the range of products, entry can create more competition, lower prices for consumers, and may lead to better technology adoption. However firm start-ups are limited by several factors, including the burden of complying with government regulations.

Excessive governmental regulations increase the cost of starting a firm in the formal sector, and thus may lead to low rates of firm creation and to high rates of informality. But how burdensome is entry regulation and how much does it limit growth and firm creation? The first part of the question has been addressed by Djankov, La Porta, Lopez-De-Silanes and Shleifer (2002). Based on data from 85 countries they conclude that for an entrepreneur in most countries, legal entry is extremely cumbersome (10 procedures on average), time-consuming (47 days), and expensive (47 percent of per capita GDP). As for the second part of the question, cross country studies tend to find a negative correlation between GDP growth and measures of the burden of firm entry regulation. However they do not fully demonstrate that firm creation is the main transmission channel; furthermore, because of the lack of a counterfactual, it is difficult to establish causality from those studies.¹

Thus the extent to which entry regulation procedures limit firm creation is still an open empirical question, and an essential one for policy. This magnitude is subject to heated debate because of the multiplicity of factors that influence an entrepreneur's decision to start up a formal firm, besides the cost of going through the registration procedures. The tax liability of formal sector firms, for example, may be the main binding constraint to firm creation, and not start-up procedures. It is also possible that entrepreneurs are able to avoid "excessive" regulations through bribes, thus effectively reducing the impact of regulation. Finally, it is often argued that the most important constraint on firm creation in developing countries is the availability of credit or other complementary inputs that are intensively used to start a firm.²

In this paper we answer the following questions: To what extent does a decrease in start-up regulatory costs increase the rate of formal firm creation? Is this effect permanent? Can the size and life span of the new registering firms tell us something about whether they are truly new firms or firms that had been operating informally?

To get at these questions we estimate the effects of a deregulation program, "System of Fast Opening of Firms", (SARE) that took place in Mexico in different locations at different time periods. This program instituted 'one-stop' firm registration offices in some municipalities. These firm registration offices allowed small firms that operate in eligible industries to obtain a license to operate in two days or less and to postpone health and social security inspections for three

¹See for example Djankov, McLiesh and Ramalho (2006a) and Klapper, Laeven and Rajan (2006).

²In spite of the multitude of barriers to formal firm creation, there has been considerable emphasis on the difficulty of complying with all of the regulations required to open a firm in developing countries (see WorldBank (2006), Economist (2004), De Soto (1989), and Easterly (2006). The World Bank has even conditioned aid on reducing these procedures. As a result many countries have implemented reforms designed to simplify the registration process.)

months. SARE significantly reduced the time to obtain the business license: before the program was implemented it took about 30 days to go through the municipal registration procedures and afterwards it took at most 2: thus *the decrease in the delay after SARE was introduced is equivalent to the difference in registration delays between Jamaica and Canada* (according to Djankov et al. (2002)).

The paper reports the results of two different identification strategies: the first compares changes in firm start-ups in municipalities that adopted the program to changes in start-ups in municipalities that did not adopt the program. The second (and preferred) identification strategy compares new firm start-ups in eligible industries to those of non-eligible industries in the same municipality. We prefer this latter strategy because it is a ‘within’ municipality comparison that is robust to some potential problems of selection of municipalities and to municipality specific shocks.

Although the timing of the introduction of the program and the industries to which it applied were not random, we provide some evidence that the implementation was not related to time varying covariates or lagged outcomes. We use different control groups and sources of variation to identify the program’s effect in order to show the robustness of the effect and to run some falsification exercises.

Our preferred estimates imply that the program generated 5% more new formal firms per month in the eligible industries in SARE implementing municipalities. However, this increase in the flow of firm registration appears to be temporary and concentrated in the first 15 months after implementation. The effect is not present for job creation in continuing firms nor is it present for the creation of firms with more than 10 employees. Since the deregulation program does not affect continuing firms and does not apply to large firms, this constitutes additional evidence that the estimated effect is causal.

There have been some previous papers that pose questions related to ours. Bertrand and Kramarz (2002) study the effect of increasing regulatory entry barriers. By using variation across time in the toughness (rejection rates) of the application of zoning restrictions in France, they show that these restrictions have a negative impact on employment growth. Klapper et al. (2006) find that the cost of entry regulation procedures across countries is negatively correlated with the percentage of new firms in an industry. This correlation is stronger for more entry-prone industries. Djankov et al. (2006a) show in cross country regressions that entry regulation procedures are negatively correlated with GDP growth. The main challenge facing these papers is to establish a causal relationship between the regulatory burden and economic outcomes. Bertrand and Kramarz (2002) use the political party composition of the approving board as an instrument for time variation in rejection rates; Klapper et al. (2006) try to control for other business environment variables like financial development, labor regulation and protection of intellectual property; Djankov et al. (2006a) instrument their index of regulatory burden with a legal origin variable and with geographic and cultural variables.

Our paper complements this existing literature by using a more transparent source of variation induced by the staggered implementation of a government program for selected industries, thus

addressing the question of causality in a more direct manner. Another contribution of our paper is to provide for the first time direct causal evidence of the effect of a deregulation program of the type encouraged by the World Bank and implemented by several dozens of countries.

Bruhn (2008) has independently (and simultaneously) evaluated the effects of SARE using household employment data. We believe that our paper is complementary and has several key advantages. First, we use the whole universe of firm registrations from administrative records instead of the employment survey. This not only avoids the interpretation and measurement error issues of the main outcome variable, but also avoids the problem of the lack of representativeness at the municipal level of the Mexican employment survey. Second, we provide a more detailed analysis of the validity of our identification strategy, which includes an analysis of the determinants of program adoption. This analysis leads us to rely more on cross industry comparisons while controlling for municipality trends as opposed to a difference in difference strategy comparing municipalities directly as in Bruhn. Third, our data covers a longer time period, allowing us to study longer term effects of the program and almost three times more municipalities that implemented the program.

Bruhn (2008) has two main advantages: because she uses a household survey she is able to study SARE's effect in the creation of firms that do not register employees with the labor authorities. She also observes the previous occupation of respondents. Our data do not permit us to look at these factors, but do allow us to measure very precisely both the creation of firms with formal sector employees and the creation of formal sector employment. Due to the numerous differences between the two papers, we view our papers as complementary.

The structure of the paper is as follows. Section 2 will describe the program we study and the setting in which it was implemented. Section 3 will describe our data sources and outcome variables. Section 4 will describe and implement our two empirical strategies. Our main results, specification and robustness checks are in Sections 4.2 and 4.3. Section 5 discusses the interpretation of the estimates and presents some caveats. Section 6 concludes.

2 Institutional Setting and Description of the Program

2.1 Regulatory Burden in Mexico and Description of the Program

Mexico ranked in the bottom 69 out of 85 countries in the time to complete procedures according to Djankov et al. (2002), taking 67 days to register a firm. This is higher than Jamaica, Peru, Uruguay, Chile, Argentina, and Brazil. Mexico also has a relatively big informal sector. According to Schneider and Enste (2000) the percentage of GDP produced in the informal sector is between 27% and 49% depending on the method used to measure it. This puts Mexico above Costa Rica, Chile, Argentina, Uruguay, and Venezuela, among others.

Spurred in part by this poor performance, in March 2002 the Federal government in Mexico, through its office of the "Federal Commission of Regulatory Improvement" (COFEMER), implemented a program called "System of Fast Opening of Firms" (SARE for its initials in Spanish) to

reduce the number of administrative procedures and time required to register a firm and to make these procedures more transparent.

SARE is a Federal level program targeted at municipalities³ that ensures that micro, small and medium firms that pose no health or environmental risks can register and begin operations in two days, conditional on the eligibility and zoning requirement being met. The program had a substantial impact on the time it takes to register a firm and obtain an operation license. The program not only speeds up registration, but also clearly defines the procedures, fees and identities of the entities involved in the registration process, thus making the procedure more transparent and making it harder for bureaucrats to delay the process in search for bribes.

SARE targets municipalities since many procedures and ex-post compliance checks occur at that level. It is operated by the municipalities and each municipality is responsible for publicizing the program and maintaining high standards of efficiency and service. In order to implement the program, interested municipalities voluntarily sign a contract with COFEMER in which COFEMER agrees to provide the expertise and training to the municipality personnel. The municipality, in turn, agrees to provide the personnel, physical space, technology, and funds to implement and continually operate the program. After the signing of the contract, COFEMER officials visit the municipality and remain there until the SARE office is fully operational, with all procedures in place and the objective of registering a firm in two days met. From this point on, COFEMER plays a limited supervisory role, verifying that the standards continue to be met.

It is important to note that not all firms can register and obtain a license through SARE. The Federal government selected 685 “non-risky” 6-digit industries as eligible for the program using INEGI’s classification; that is, only firms in industries that pose little health or environmental threats can register through SARE. 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, the retail and services sectors are disproportionately represented in eligible industries. Some examples of eligible industries include: production of metal and wooden furniture, freezing of fruits and vegetables, production of clothes and textiles, drugstores and small supermarkets, video stores and DVD rentals, real estate services, etc. Examples of non-eligible industries include: bars, production of rubber products, hospitals, production of machinery, etc.⁴ Kaplan, Piedra and Seira (2007) report more information about eligibility by industry and how industry eligibility varied across municipalities. Although each municipality was encouraged to select all of these 685 industries, most included only a subset of this list. However, municipalities tended to select the same industries as eligible, mostly copying their lists from other municipalities that already had implemented the program.

Four important features are helpful 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, since the mean number of employees of a firm registering through SARE is 2.6,

³A 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 two or more municipalities.

⁴For a detailed list please consult COFEMER’s web page at <http://www.cofemer.gob.mx>.

we believe that these firms are most likely 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.

2.2 Implementation of SARE

Mexico has 2448 municipalities and 32 States. The Federal government wanted to implement this program first where it could have the greatest impact. It used a study by Cabrero, Ziccardi and Orihuela (2003) in which 60 major urban centers were identified based on quality of infrastructure, population, economic activity, and growth potential. These centers encompass 224 municipalities which, following COFEMER, we will call “Competitive Municipalities.” The government has focused its efforts on convincing these municipalities to adopt SARE, but it cannot deny participation to any other municipality. Competitive municipalities form what is commonly known as the “intention-to-treat” group. SARE was supposed to be implemented in all of them by the end of 2006, although this goal was not achieved.

The program could not be implemented simultaneously in all locations mostly because of COFEMER’s limited resources, having only 4 employees who traveled to municipalities implementing the program. In our sample period, which extends from January 1998 to March 2006, we observe 93 municipalities implementing SARE, 31 of these are not “Competitive” municipalities, and therefore tend to be smaller.

SARE adoption by year**: Timing, Geographical Breadth and Municipality Clustering					
	2002	2003	2004	2005	2006
Number of municipalities implementing	2	8	28	47	8
Number of States implementing	2	7	15	17	7
Number of municipalities implementing in the most active State*	1	2	10	9	3
Number of non-competitive municipalities implementing	0	0	11	16	4

* The State with more SARE implementing municipalities in a particular year. **Implementation within our sample period.

Table 1: SARE Adoption: Timing, Geographical Breadth and Municipality Clustering

Table 1 presents statistics on the timing, geographical variation, and clustering of SARE adoption during our sample period. The first and second rows show the number of municipalities and States that adopted the program in each year. SARE adoption has substantial geographic variation: out of a total of 32 states, SARE was implemented in 31 of them during the sample period. The third row shows that municipalities within a State tended to implement at the same time; 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); in 2005 about one fifth of

implementation came from the most active State. The last row counts the number of non-competitive municipalities that implemented the program. Since they were not explicitly invited, they typically implemented later.

Large municipalities were explicitly targeted for early program adoption. Table 2 presents summary statistics of Mexican municipalities for a partition of four non-intersecting groups: municipalities with SARE in our sample period, “Competitive” non-SARE municipalities, non-Competitive municipalities without SARE that are geographically adjacent to a SARE municipality, and all others. It shows that SARE municipalities are much bigger in terms of formal employment, new firm creation and population than the other three groups. They also have a higher share of workers in the tertiary sector.

Means and Std. Dev. by Type of Municipality (monthly averages)				
<i>Variable</i>	SARE	Competitive	Adjacent	Other
Formal employment***	61,450 (85,672)	13,213 (23,578)	1,446 (3,040)	1,420 (4,019)
Monthly new jobs by new firms***	409 (478)	99 (188)	16 (49)	15 (71)
Monthly new firms***	111 (118)	26 (48)	4 (9)	4 (11)
Non-exiting Firms***	3,620 (4,367)	786 (1,455)	131 (248)	117 (285)
Population**	1,332,588 (1,356,585)	504,958 (829,175)	120,846 (108,289)	101,024 (115,520)
% Workers in terciary sector**	54% (13%)	53% (12%)	34% (10%)	35% (13%)
Number of establishments*	11,518 (12,573)	4,089 (6,760)	751 (873)	685 (1,019)
Production*	\$2,388 (\$3,071)	\$846 (\$2,346)	\$100 (\$487)	\$59 (\$296)
Number of municipalities	93	142	267	1,008

*As reported in the economic census 2004 (millions of 2004 dollars); ** From the Population Census 2000; *** From our IMSS dataset (averages 1998-2001). Means with Standard Deviations in parenthesis.

Table 2: Summary Statistics by Type of Municipality

Although the government neither randomly selected the municipalities that would implement the program, nor the industries that would be eligible, discussions with COFEMER and municipality officials and the analysis that we present here convinced us that the decision to implement the program was not related to lagged values of our outcome variables nor to their expected future values. Most of the implementation was done where the Federal Government could convince the State governments that there was excessive regulation at the municipality level and by promising to give technical advice and methodology to improve this regulation. The State governors in turn

convinced municipality mayors, which may explain the within State clustering of implementation.

This convincing appears to have been more effective for municipality mayors who belonged to the same party as the President, those who were in the middle of their term, and those from a State where other municipalities were implementing the program. In the first three years of implementation more than 70% of the municipalities were from the President’s party (PAN) at the moment of implementation, while in our whole population of municipalities only about 25% of the municipalities were governed by this party. In our sample period more than 50% of municipalities implement in the mayor’s second year of tenure (municipality mayors have three year terms), which according to officials is because they use the first year for “more pressing issues”.

Since municipalities are autonomous entities, implementation of the program is largely a political issue and may not be related to economic time trends. If this is indeed the case it would strengthen our argument that our estimates are unbiased. Section 4.1 performs an analysis of the determinants of the timing of adoption and confirms that most of the political determinants mentioned above are significant predictors of program adoption, and that past levels of firm and job creation or their changes are not important determinants of adoption.

3 Description of the Data

We will use three sources of data: First, we use data from the Mexican Institute of Statistics, Geography and Informatics (INEGI). These data include municipality demographics from the Mexican Population Census (2000), municipality production data from the Mexican Economic Census (2004) and data about political variables from INEGI’s municipal databases. Second, we use the industry eligibility lists from 86 of the 93 municipalities that implemented the program in our sample period⁵ as well as information on the change in registration time for a subset of these municipalities. Third, we utilize a monthly **census** of all establishments that have employees registered with the Mexican Social Security Institute (IMSS), the equivalent of the US Social Security Administration. That is, we observe the registration of all formal employees in Mexico.

The IMSS data are taken from the last day of each month from January 1998 through March 2006. Registration of all employees is required by law, although not all establishments comply with this law. This lack of compliance means our measure of outcomes is not necessarily the number of new firms, but rather the number of new formally registered firms with at least one formal employee. We discuss in section 5 how our inability to observe firms without any formal employees may affect our analyses.

As a result of the above data issue, we study the effect of SARE on formal employment and on the number of firms with formal employees. Since one of the justifications for programs like SARE has been to increase formal employment, the outcome we study is particularly relevant for policy makers. Furthermore, to the extent that firms may register with the tax authorities but not with

⁵For the 2007 version of the paper we had only 31 of these lists, forcing us to extrapolate eligibility as the union or intersection of these lists. This was a cause of concern because of the induced measurement error. We now eliminated this source of measurement error in this version of the paper, with only small changes in our results.

IMSS, our estimates can be viewed as a lower bound for the total effect on new registrations, and we will present numbers showing that this lower bound is not far from the total effect.

Since we observe all registered workers for each establishment, it is straightforward to count the number of employees in each month in each establishment and the number of new establishments per month. Although we do not observe much information about the establishments themselves, we do observe each month the number of employees, their four-digit industry code as well as the municipality in which the establishment operates and when it started registering employees.

A crucial part of our identification strategy will be to identify the industries that are eligible for the SARE program and those that are not. It is important to note that eligible industries include a bigger share of retail and services relative to manufacturing.

Since the IMSS definitions of industries are not exactly the same as the INEGI definitions used by COFEMER for eligibility, we had to construct a concordance between these two lists of industries. We manually matched 685 6-digit INEGI industries to 302 4-digit IMSS industries. In principle this may be a cause for concern since some *IMSS* industries are classified as eligible even if not all IMSS employment in that industry is eligible according to COFEMER-INEGI classification. Although this could introduce some measurement error, we show in the Appendix that the mismatch was small: only 19 out of the 302 IMSS industries had eligible and ineligible INEGI industries inside. This “ineligible-inside” component represented only 7% of the total of INEGI industries inside IMSS’s industries classification (4% weighting by production). No INEGI industry intersected two distinct IMSS industries.⁶

Once we had the definitions of eligible industries for 86 municipalities, we aggregated the data at the municipality level for each month, separately for establishments in eligible industries and for establishments not in eligible industries. That is, for a given municipality in a given month, we have two observations: one that aggregates the data for all eligible industries and another that aggregates the data for all non-eligible industries. The three main outcome variables are: (i) The number of new firms in the current month with at least one registered employee this month that did not have any employees in the previous month; (ii) Jobs created by new firms: current employment in firms that did not have any employees in the previous month; (iii) The total number of formal employees in all firms old and new.

In addition to the concordance of INEGI and IMSS industries, we had to construct a concordance between INEGI and IMSS municipalities. There are 2448 municipalities according to the INEGI classification system, but only 1510 municipalities according to the IMSS classification system. The main difference is that IMSS often aggregates smaller municipalities together into a larger entity. Thus we lose some INEGI municipalities for which we could not find the corresponding IMSS municipality; these are mainly smaller municipalities. We do not lose any

⁶We ran the regressions in the paper defining those 19 contaminated IMSS industries as eligible and also as ineligible and results did not change much, which you could expect given the results in Kaplan et al. (2007). We also computed another (continuous) measure of eligibility, assigning to each IMSS industry an eligibility percentage in $[0,1]$, according to the percentage of employment inside it that is eligible according to the COFEMER-INEGI classification. Although results are a bit harder to interpret and not reported here, they are almost identical to those reported in the paper based on a discrete 0/1 measure of eligibility.

SARE municipalities, although we lose 16 Competitive municipalities. Since our main results are estimates of the average treatment on the treated, this small loss of non treated municipalities is of little importance.

4 Empirical Strategy and Models

The main question we want to answer is the following: how big was the effect of SARE on formal firm creation? To answer this question we need to estimate a counterfactual scenario of what firm creation would have been in the absence of the program. This is typically done by selecting a set of “control” municipalities that we expect would mimic the performance that SARE municipalities would have had without SARE.⁷ Alternatively we could use non-eligible industries as controls for eligible industries and compare the difference of firm creation within municipalities across the two sets of industries.

In both cases we assume that firm creation in the control municipalities (industries) are good approximations to what would have happened without the program in SARE municipalities (eligible industries). Unfortunately the counterfactual identification assumptions used are inherently not testable. We will use a series of checks to increase our confidence that our identification assumptions are reasonable and that our estimates are close to SARE’s true causal effect.

First, in section 4.1 we will show that there is no evidence that municipalities that adopted the program do so because of changes in time varying covariates or lagged outcomes. Instead we will show that political variables are more important determinants of adoption. This result is important since, as long as the political variables are not correlated with the trends of firm creation⁸, it makes it less likely that time-varying unobserved variables are affecting the trends of firm creation differentially for SARE municipalities or for SARE eligible industries.

Second, we tested whether the trends of firm creation were parallel and cannot reject the hypothesis that the treatment and control groups had the same time trends before the implementation of the program at the 5% significance level. That is, trends only started to diverge *after* SARE was implemented.

Third, we will use two sources of variation to identify the effect of SARE: comparing across municipalities (section 4.2) and comparing across industries (section 4.3). We obtain two different estimates and argue that if there is any bias in the estimation, the true effect should be between these. For reasons we will discuss later we believe that the estimates comparing across industries are more reliable and we focus mostly on these results in the paper. Fourth, we will report several specification and robustness checks in section 4.3.3.

To reduce possible measurement error, the analysis going forward only uses the 86 municipalities for which we could get lists of eligible industries. Kaplan et al. (2007) use all 93 SARE

⁷Two recent papers which have similar settings as ours are Athey and Stern (2002) and Galiani, Gertler and Schargrodsky (2005).

⁸We find no correlation in the before-SARE period between firm or job creation and these political variables: the raw correlation coefficient is less than 0.02 for any pair. This result also holds if we control for municipality fixed effects and therefore look at changes in firm and job creation.

adoptions with nearly identical results.

4.1 Where is SARE implemented?

If factors affecting the time trends of firm creation are correlated with variables affecting the decision to adopt the program, then it is likely that adopting municipalities would have had a different trend of new firm creation compared to control municipalities even in the absence of the program. If these factors are observed we can simply interact the time trends with these variables, thereby allowing for different control trends and consistently estimating the effect of the program. However, if they are unobserved, comparing firm creation between adopters and our control municipalities before and after the program will give us inconsistent estimates of the true effect of the program. A similar problem holds if we compare eligible vs. non-eligible industries. If the timing of the adoption of the program is correlated with expected changes in the industry composition of new firms, then our estimate of the causal effect will be inconsistent.

Given the above concerns, it is important to analyze the determinants of implementation and to show that time varying covariates do not appear to be related to adoption. The hope is that if time-varying observables are not correlated with implementation, then time-varying unobservables will also be uncorrelated with adoption. We estimate a discrete Weibull duration model of program implementation (as described in Jenkins (1995)) and show that (static) political variables are more important determinants of the timing of adoption than time-varying economic variables.

The political variables we use are: party of the municipality mayor (PRI, PAN, PRD) as well as the mayor’s tenure at the time of adoption (the excluded categories are other parties and coalitions, and the first year of tenure, respectively); to capture the effect of “recommendation” to adopt by the State governor, we also include as a regressor the number of municipalities that have implemented in the State at any given time. The time varying economic variables we use are firm creation and job creation, adjusted for seasonality.⁹ The time constant regressors we use are demographic and economic municipality characteristics: total population (in thousands of individuals), production per capita (in millions of 2000 dollars), unemployment, working age population (in thousands), percentage of the workers in the tertiary sector, percentage of the working age population registered at IMSS, the log of the State’s exports in 2004 dollars, the percentage of employment in exporting firms in the State, the percentage of the workers receiving no income, and gross income (tax revenues plus federal transfers) of the municipality government (in millions of 2000 dollars).

⁹We remove seasonal and level effects that are common to all municipalities from these variables by regressing them on month and municipality fixed effects. We then use the residuals to construct one year moving averages. We use these moving averages as regressors in the duration model. There are at least two reasons for doing this: first, since these variables are highly seasonal and serially correlated we could find a spurious correlation if we use, say, the values of the last month or the last quarter. Secondly, we believe that if there is any relationship at all between economic conditions and implementation, it should operate with a lag. Results are not changed if we do not remove the seasonality of these variables.

Since the program was intended for competitive municipalities, in the estimation we consider only SARE and Competitive (not-yet-SARE) municipalities as the ‘municipalities at risk’,¹⁰ and thus only those are included in the sample for the table. However excluding the non competitive SARE municipalities does not change the qualitative conclusions. Table 3 shows the results. Coefficients are reported in an exponential form so that they can be interpreted as semi-elasticities of the hazard of implementation.

Discrete Duration Model of Program Implementation					
<i>Political variables</i>		<i>Economic time-varying variables</i>		<i>Economic static variables</i>	
Party 1 (official)	0.25** (-2.18)	New Firm Creation in Eligible Industries (MA12, detrended)	1.01 (0.53)	Total Population (thousands)	1.002 (1.31)
Party 2	0.16*** (-2.83)	New Firm Creation in Non-Eligible Industries (MA12, detrended)	0.97 (-0.62)	Production (\$dollars per capita)	1.00004 (0.39)
Party 3	0.24 (-1.44)	Job Creation in Eligible Industries (MA12, detrended)	1.002 (1.18)	Unemployment (2000 census)	1.20 (0.66)
2nd Year of Tenure	3.17** (2.31)	Job Creation in non-Eligible Industries (MA12, detrended)	0.99 (-0.53)	Working Age Population (thousands)	0.99 (-1.02)
3rd Year of Tenure	2.10 (1.47)	Weibull Duration dependence parameter	16.72*** (4.29)	% Employees in Tertiary Sector	1.04 (0.07)
Number of Mun. in State that implemented	1.26*** (2.97)			% Working Age Registered at IMSS	0.96 (-0.16)
				Log (State Exports)	0.75 (-1.34)
				% Employment in Exporting Firms	0.19 (-0.58)
				% Workers receiving no income	0.28 (-0.54)
				Municipality government revenues (\$ millions of dollars)	1.004 (1.39)

Includes SARE and Competitive Municipalities only. Coefficients are reported in exponentiated form.

Table 3: Discrete Duration Model of Program Implementation

The results are consistent with what COFEMER officials told us: mayors in their second year of tenure and those who belong to the party of the President are significantly more likely to adopt the program. It is also true that the more municipalities that have implemented in a State, the more likely is that another municipality in that State will adopt; this reinforces our belief that

¹⁰Competitive municipalities are just one group that may be “at risk” of implementation, but it is the only a priori clearly identifiable group at risk. Following the suggestions of a referee we experimented with including more municipalities as “controls”. We followed two routes to form groups of municipalities that were at risk. One was to use a propensity score method to form a group of municipalities that are similar to SARE implementers in terms of the distance summarized in the probability of implementing as a function of covariates. The second way was more arbitrary. We included medium sized municipalities geographically close to SARE implementing municipalities but which had not yet implemented SARE. We added between 20 and 50 municipalities this way and the results were qualitatively similar: political variables had a positive and significant impact of the time of adoption and economic time-varying variables were not significant at conventional levels.

the State governor’s pressure on municipality mayors to implement SARE is important and that implementation is less a result of a municipality specific shock.¹¹ Most important for us is the fact that the economic time-varying covariates are not significant (not even jointly) and are small economically.

Overall this evidence confirms that program adoption was not driven by changes in (time-varying) firm or job creation and that political factors were a more important determinant of program adoption, therefore it is less likely that the identification strategy we use is flawed. Kaplan et al. (2007) performed some statistical analyses to show that these political determinants were not correlated with trends in firm or job creation before 2002.

4.2 Comparing Adopting vs non-Adopting Municipalities

4.2.1 Main identification problem

In this section we lay out the main identification problem we face, and estimate the effect of SARE on firm and job creation by comparing early vs. late adopting municipalities. Since this is not our main identification strategy, we are brief in the explanation of the results.

Although early and late adopting municipalities differ significantly in the level of firm start-ups and employment, equality of the pre-SARE time trends for treatment and control groups was not rejected at the 5% significance level. The main identification problem we face however is that after the program began to be implemented in 2002, a slowdown of the Mexican economy was underway.

According to the Mexico’s Central Bank this happened to a large extent as a result of the US recession (Banxico (2002, 2003)). About 3/4 of Mexican exports go to the US, and from October 2000 to January 2002 non-oil exports to the US decreased by 30%. Meanwhile imports of consumption goods decreased by only 15%, suggesting the contraction is more likely to have been an external rather than an internal shock. We obtained data on State’s exports, covering 1994-2003, and we can see that on average 26% of employees in northern States worked in firms that exported, whereas this number is only 17% and 8% for central and southern States respectively. These numbers suggest that this shock particularly affected municipalities in the north of Mexico as well as some of the bigger municipalities in the center that export to the US, since these municipalities are more closely linked economically to the US.

Figure 1 substantiates this claim. It shows the percentage growth rate (yoy) of formal employment for northern States vs. States in the rest of Mexico, by industry eligibility status. The graph is similar if we stratify by size -the employment patterns of larger municipalities are similar to those of northern municipalities. The first fact to note is that northern States seem much more affected by the US 2001 recession. Since 9 of the 10 municipalities that implemented SARE early

¹¹ The fact that variables such as political party and number of years in office are significant predictors of adoption suggests a possible instrumental-variables approach to estimating the effect of SARE. We did not pursue this because the instruments were only marginally significant in the first stage and the SARE coefficients in the second stage were not very robust to different specifications.

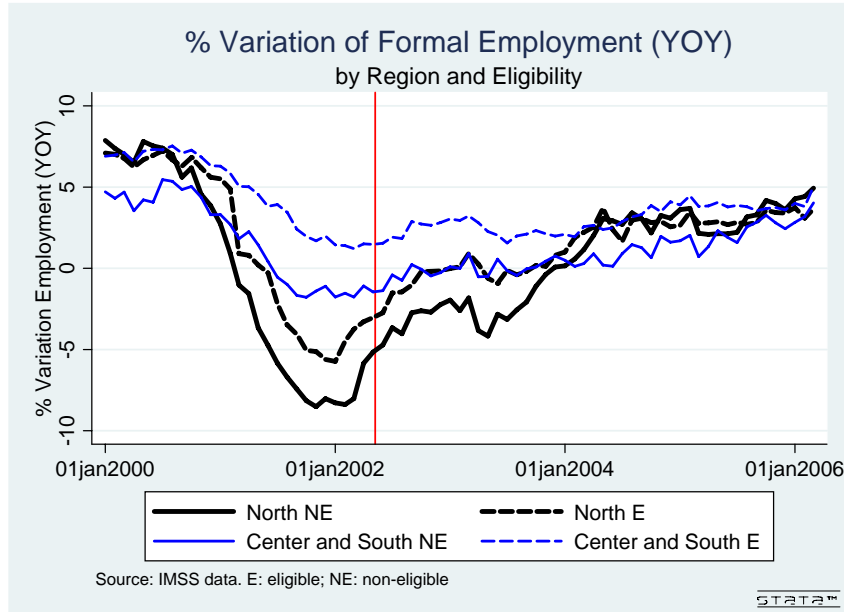


Figure 1: Percentage YOY Growth Employment by Region and Eligibility Status

(in 2002 and 2003) are big ones from the north and the center ¹², comparing early adopting vs late adopting municipalities is problematic: the more pronounced downturn in northern (bigger) municipalities will erroneously be attributed to SARE. Our differences-in-difference estimates would thus *understate* the true effect of the program.

The second important fact to note is that the growth rates of eligible and non-eligible industries seem to be similar within each region, much more similar than growth rates across regions. This is an important motivation for our preferred estimation strategy, which compares across industries within municipalities. Both of these points hold when we use new firm creation instead of employment.

4.2.2 Estimation Strategy 1

Now we proceed to define the object we want to estimate and to present our first estimation strategy. Let Y_{it} denote the outcome variable we want to study (the log of one plus the number of new firms in eligible industries or the number of total or new jobs in eligible industries) where $i = 1$ if the municipality i implements SARE and zero otherwise, and $t = 1$ indicates time after SARE and $t = 0$ time before SARE. Let $SARE=1$ indicate municipalities that implement the program in our sample period.

We want to estimate the effect of SARE (treatment) on SARE implementing municipalities (on the treated): $ATT \equiv E[Y_{1t} - Y_{0t} | SARE = 1]$. Since implementation is staggered across time, we do this by estimating equation 1 by OLS and using the estimate of β as our estimate

¹²In order: Puebla, Los Cabos, Aguascalientes, Guadalajara, Zapopan, Mexicali, Leon, Tlalnepantla, Tehuacan, Oaxaca.

of ATT. This implicitly assumes that the average outcomes of late SARE implementors is an unbiased estimator of the counterfactual outcome Y_{0t} of early SARE implementors, an important but untestable assumption. Consider the following equation:

$$Y_{it} = \alpha_i + X_i' \delta + \gamma_{\varphi(i)t} + \beta \text{AfterSARE}_{it} + \epsilon_{it}. \quad (1)$$

In this equation α_i are municipality fixed effects, $\gamma_{\varphi(i)t}$ are sets of time trends, one for each group of municipalities $\varphi(i)$, and AfterSARE_{it} is our policy variable equal to 1 for municipality i after it implements the program and zero before it does. To save on space Table 4 only presents the estimated β coefficient of specifications with different dependent variables (columns) and samples (rows).¹³ We use a baseline specification and set $X_{it}=1$ and $\varphi(i) =$ all SAREs, thus using only one control group trend.

SARE's Effect on firm and job creation (DID strategy)						
	New Firms		New Jobs			
	Eligible	Non-eligible	In all firms		In new firms	
			Eligible	Non-eligible	Eligible	Non-eligible
All SARE municipalities	-0.043** (-2.17)	-0.084*** (-3.07)	-0.005 (-0.20)	-0.03 (-1.45)	-0.02 (-0.59)	-0.08*** (-2.72)
SARE municipalities implementing before 2005	0.01 (0.52)	0.02 (0.63)	0.02 (1.06)	-0.03 (-0.93)	0.07 (0.74)	0.07 (0.57)

SARE's: 86 municipalities, 99 months. Errors clustered.

Table 4: Comparing Eligible Firm Creation Across Municipalities: β coefficients

The estimates of equation 1 reported in Table 4 attribute a 5% decrease in monthly firm creation in eligible industries to the program. As we explained earlier, we do not believe this 5% decrease is the true effect of the program. Rather, this negative coefficient arises because the economic slowdown in the US had larger adverse effects in the bigger and northern municipalities that are more closely linked to the US. Unfortunately, these larger and northern municipalities tend to be early SARE adopters. Consistent with this explanation, non-eligible industries were also affected by the downturn, and when we restrict the sample to the more homogeneous group of municipalities adopting before 2004, the effect is not different from zero.¹⁴

The effect on jobs in eligible industries is not statistically different from zero, implying that employment is less sensitive to the downturn than firm creation. The estimates show larger decreases in firm creation in non-eligible industries than in eligible industries, which suggests that a possible positive effect of the SARE program is being masked by the effects of the US recession.

¹³Full regression results are available from the authors upon request. In all regressions in this paper we report standard errors clustered at the municipality level. We also clustered at the State-year level and results were unchanged.

¹⁴We would like to thank a referee for this suggestion.

The effect of the US recession may erroneously be absorbed in β because late SARE implementers are not a good control group for early implementers. In Kaplan et al. (2007), we estimated flexible models that allow for different time trends for different groups of control municipalities, i.e. the $\gamma_{\varphi(i)t}$'s of equation 1. We do this by interacting monthly time dummies with covariates which we believe proxy for the strength of the above mentioned shock; these include the percentage of the State's GDP made up by exports, total population, a dummy for adopting before or after 2004, and municipality-specific linear time trends.

Overall, once we include different counterfactual control trends, the effect of SARE is not statistically different from zero. The results are unchanged if we include competitive non adopting municipalities as part of the control group. We view this differences-in-differences estimate as a lower bound on the effect of the program, since we believe that we cannot control for all omitted variables making the downturn stronger for early implementing municipalities. Unfortunately there are no economic time series variables at the level of the municipality in Mexico. The rest of the paper estimates the effect of SARE by comparing eligible versus non-eligible industries within municipalities.

4.3 Comparing Eligible vs. Non Eligible Industries

4.3.1 Estimation Strategy 2

Given the difficulty of finding a good control group for SARE municipalities, we decided to use comparisons *within* SARE municipalities across eligible and non-eligible industries to estimate the program's effect on firm creation, effectively making each municipality a control for itself. The basic idea is that since only certain industries are eligible to register through the program, the program's effect should only be present in these industries. If the program is effective we expect the economic decline in eligible industries to be smaller than that in non-eligible ones just after the program is implemented, thus increasing the gap of firm creation across industries. We will call the difference of firm or job creation in eligible vs. non-eligible industries the *gap*.

This strategy allows us to control for the general (across all industries) slowdown of economic activity after 2002. Since it can be the case that eligible and non-eligible industries have different time trends, what we effectively do is to compare the *rate of change* of the gap of firm and job creation across industries in treated municipalities to the analogous quantity for the control municipalities, before vs. after implementation. This amounts to using a diff-in-diff-in-diff strategy which can be written as follows:¹⁵

$$\beta \equiv \underbrace{[(\overline{Y_{1t1}} - \overline{Y_{0t'1}}) - (\overline{Y_{1t0}} - \overline{Y_{0t'0}})]}_{SARE} - \underbrace{[(\overline{Y_{0t1}} - \overline{Y_{0t'1}}) - (\overline{Y_{0t0}} - \overline{Y_{0t'0}})]}_{Controls} \quad (2)$$

The dependent variable Y_{itk} is either the log of one plus the number of new firms or the log of one plus the number of jobs created by new firms; $i=1$ if the municipality i has SARE and is

¹⁵For a paper that also uses a 3rd difference approach see Gruber (1994).

zero if the municipality is a control municipality; t indicates the time after SARE implementation and t' the before implementation period; $k = 1$ if the industry is eligible and zero otherwise. The first square bracket contains the difference of firm (or job) creation in eligible vs. non-eligible industries in SARE municipalities before and after SARE. The second square bracket contains the same quantity for control municipalities. Note that the expression involves counterfactuals which have to be estimated.

Effectively, our empirical strategy will attribute the relative increase in the gap of firm creation between eligible versus non-eligible industries to the program if the gap widens by more in an adopting municipality compared to the widening in a control municipality after the program is implemented. Thus the identification assumption is that there is no reason other than SARE for this gap between industries to increase more for adopting municipalities than for the controls (late adopters or competitive non-SARE municipalities) at the time of implementation.

Note that this identification assumption is weaker than the assumption used in the simple differences-in-differences analysis, since the triple difference approach is robust to municipality-specific time-varying shocks, as long as they impact eligible and non-eligible industries uniformly. By comparing within municipalities the strategy reduces the possible bias introduced by the endogenous selection of municipalities.

One possible problem could arise if municipalities selected as eligible the industries that they expected would grow more in the future. We believe this is not the case. The selection of eligible industries was quite homogeneous across municipalities and municipalities did not have an incentive to make the program appear successful since SARE is a Federal program. More importantly, in order to mimic the effect of SARE it would not have been sufficient for municipalities to choose as eligible those industries that would grow in the future. The implementation of the program would have had to be such that this expected growth materialized at that specific date. Furthermore Kaplan et al. (2007) showed that results did not change much by extrapolating eligibility choices based on the eligibility criteria from a subset of municipalities.

In spite of our arguments against the potential selection problem of industries and in favor of the robustness of the triple difference approach, the unbiasedness of estimates are never guaranteed in non-randomized program evaluations. If for reasons unrelated to the program the *gap* in firm creation between eligible and non-eligible industries starts to evolve differently in early vs. late adopters around the time of implementation, our identification strategy will yield an inconsistent estimate of the program's true effect. This could happen, for example, if the US recession was felt less strongly in the eligible industries relative to the non-eligible ones in early adopting municipalities. Note that this alternative story has to involve not only that non-eligible industries are hit harder, there has to be a *bigger trend break in the gap* in early adopters relative to later adopters just after each municipality implements the program; this may require a contrived story.¹⁶

Figure 2 plots the evolution of firm creation by industry eligibility for early (before or in 2004)

¹⁶Note that scale effects, that may arise from municipalities having different sizes, are taken care of in the specification since by using logs we are looking at rates of change. We thank a referee for this point.

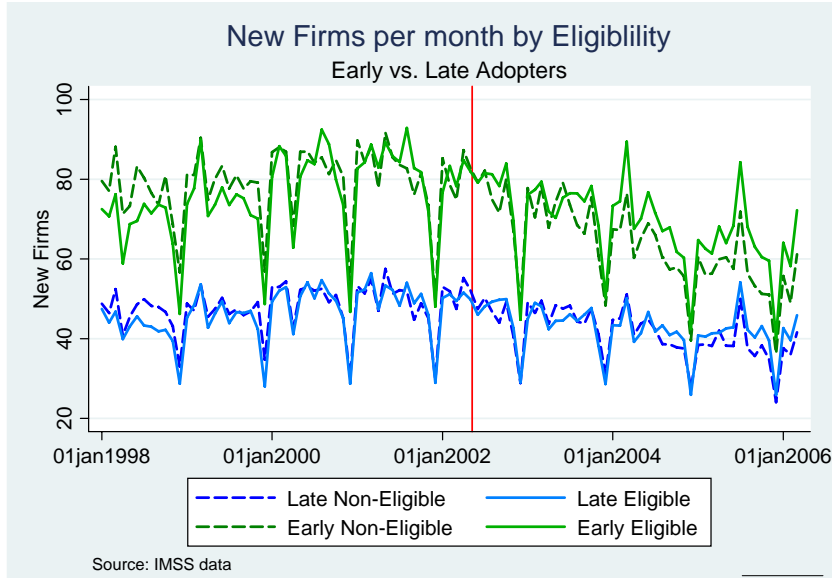


Figure 2: Mean Trends of Firm Creation by Eligibility for early vs. late adopters

and late (after 2004) program adopters that we use in the sample. Importantly, it shows that the time trends of firm creation are very similar for eligible and non-eligible industries.

Before proceeding to the estimation, Table 5 shows some summary statistics comparing eligible and non-eligible industries in early vs. late implementing municipalities. Within municipalities, eligible and non-eligible industries have similar averages of new firm creation and somewhat higher average employment creation in new firms, implying that the average new firm in non-eligible industries has about 4 employees compared to 3 for eligible ones. Across municipalities, early adopters are bigger in all variables reported, however the gap is not significantly different.

4.3.2 Baseline estimation

We will use the following regression to estimate the effect of SARE that was defined in equation 2:

$$Y_{ikt} = \alpha_{ik} + \gamma_t + \beta \text{AfterSARE}_{it} * I_k + \phi \text{AfterSARE}_{it} + \sigma(t) * I_k + \delta \text{SARE}_i * I_k + \lambda X_{itk} + \epsilon_{itk} \quad (3)$$

The effect of SARE is captured by β which is the coefficient of the interaction of the eligible industry dummy and the ‘after implementation’ dummy. It estimates the effect defined in equation 2. The coefficients α_{ik} are fixed effects for each municipality-industry pair. The coefficients γ_t are 99 fixed effects for each month of each year. The remaining independent variables are the second order interactions between industry, municipality and time.

The parameter ϕ captures the shift of the outcome variable which is common for both types of industries for SARE municipalities after SARE is implemented; this regressor is key to control for the decreasing trend of firm creation in SARE municipalities which we documented in section

Summary Statistics by Type of Municipality and Industry Eligibility (1998-2001)				
	Early adopters		Late adopters	
	Non-eligible	Eligible	Non-eligible	Eligible
Number of monthly new firms	79 (65.9)	75.4 (80.8)	47.9 (42.3)	46.1 (52.1)
Number of monthly new employees in new firms	325.2 (316.2)	241.5 (309.2)	200.7 (242.1)	144.9 (188.6)
Number of monthly new employees in all firms	36,792 (44,992.2)	41,252.3 (50,191.3)	30,093.4 (45,115.2)	27,366.5 (37,643.3)
Population	880,011 (750,029)		586,165 (600,870)	
Number of municipalities	37		49	

Means and (sd). Early adopters=adopted before 2005.

Table 5: Means of selected variables for Eligible and Non-Eligible Industries

4.2. The term $\sigma(t) * I_k$ is a third degree polynomial of time interacted with the eligible industry dummy, it captures the time trend differences for eligible relative to non-eligible industries which is common to all municipalities; the parameter δ estimates the average difference in levels of firm creation (job creation) of eligible industries in SARE municipalities. Different specifications for X_{itk} will be explored in Section 4.3.3, in this section we do not include X_{itk} in the regressions.

Table 6 reports β estimates of equation 3 for different outcome variables and samples of municipalities (i.e. different control groups).

Row (1) shows that the estimated effect of the program is an increase in new monthly firm creation of 4.7% (2.5 more firms per month per municipality) when SARE municipalities are the control group, and 9% when we include all other non-SARE competitive municipalities. To the extent that we believe SARE municipalities are better controls for themselves, the first estimate should be preferred. Row (2) reports that the program also caused a monthly increase in employment of 10% (20 employees per month per municipality). This, together with the estimate of firm creation, implies that firms registered after SARE are about twice as big as those registered prior to SARE. We conjecture, but do not prove, that this change in the size composition of new registered firms is due to the fact that the new registered firms are generally older firms that had been operating (and growing) informally for some time. Another fact consistent with this story is that the exit rate of the new firms in eligible industries is 20% lower after SARE, which could suggest that this were preexisting informal firms, since the exit hazard decreases with age (see Kaplan et al. (2007)). We want to stress the fact that, although it is an important question, we cannot show whether the effect is driven by the registration of preexisting informal firms or truly new firms.¹⁷

¹⁷However, Brunh (2006) presents some direct evidence suggesting that the effect of SARE is driven by previously unemployed persons starting up a business.

SARE's Effect on firm and job creation (DIDID strategy)			
Dep. Variable	Municipalities in sample		
	SARE only (86 muns.)	SARE union Competitive (212 muns.)	SARE intersection Competitive (55 muns.)
(1) New Firms	0.047** (2.35)	0.09*** (4.45)	0.044* (1.83)
(2) New Jobs in new firms	0.095** (2.41)	0.11*** (2.99)	0.084* (1.72)
(3) New Jobs in existing firms	0.018 (0.55)	-0.001 (-0.03)	0.041 (1.07)
(4) New Firms with more than 10 employees	0.002 (0.07)	0.003 (0.09)	0.001 (0.03)

Of the 86 SARE municipalities, 61 are Competitive.

Table 6: SARE's effect

Are the magnitudes of our estimates reasonable? This is a difficult question for two reasons. First, it is difficult to know how many entrepreneurs are truly at the margin (that is, indifferent) between starting a new firm or not. Second, it is difficult to know whether a large stock of informal firms may exist that might be on the margin between registering or not. If most of the SARE induced registrations come from a stock of previously existing informal firms or a stock of entrepreneurs, we would expect an accelerated increase in the number of registrations when the program is implemented, then a slowdown in registrations after firms from this stock have registered. This pattern is indeed what we find in section 4.3.4.

How can we increase our confidence that the estimated effect is causal? If the slower decline of firm creation in eligible industries is due to SARE we would expect this effect not to be present in new job creation in *existing* firms or new *big* firm creation, since SARE did not apply to these firms.¹⁸ We would also expect the program to be more effective where it most reduced the time and cost of firm registration. We indeed find strong evidence of these. Specification (3) shows that for the three samples that SARE is not associated with jobs created in existing firms; specification (4) shows that it did not have an effect on the creation of new firms with more than 10 employees.

Another important piece of evidence for causality is that there is no evidence of a pre-SARE trend in the gap between SARE and non-SARE controls (see figure 3). Since different municipalities implemented at different times, a placebo effect would need to generate an increase in the gap in each municipality *just after implementation*. Given the staggered implementation of the program, this explanation seems unlikely.

¹⁸Recall that SARE is aimed at small firms: most SARE's have restrictions on the maximum number of square meters of the new locale.

Finally, Kaplan et al. (2007) also present some evidence that the effect was stronger in municipalities that reduced time and procedures the most, however we only had 46 municipalities in the sample and statistical significance was low (furthermore time reduction may be endogenous).

It is difficult to think of why, other than SARE, the widening of gap of firm creation would be different for big vs. small firms or employment creation different for existing firms vs. new firms, just after SARE is implemented. Especially since the average size and age of firms in eligible industries is similar to that of non-eligible ones.

4.3.3 Robustness checks

As explained above, the unbiasedness of the estimated effect relies on an unbiased estimate of the counterfactual time trends, which highlights the importance of the time trends in the control municipalities. This subsection presents estimates of SARE's effect using different municipality control time trends: by time of adoption, level of exports, and percentage of firm creation in eligible industries.¹⁹

That is, we are relaxing the implicit assumption of equation 3 that non-eligible industries have the same trends across municipalities, by replacing $\phi AfterSARE_{it}$ by $\phi_1 AfterSARE_{group1,t}$ and $\phi_2 AfterSARE_{group2,t}$, etc. By doing this we are effectively making comparisons within the group of early and late implementers, or within municipalities in States with similar level of exports or non-eligible firm creation. The specifications also include as regressors firm and job creation of geographically adjacent municipalities to which the program did not apply, this is a control for regional shocks unrelated to SARE.

Robustness checks (DIDID)	
<i>Specification</i>	New Firms
(1) Separate municipality group trends for early and late adopters	0.076** (3.18)
(2) Separate municipality group trends by quartile of exports in state	0.086* (1.77)
(3) Separate municipality group trend by quartiles of the % of firm creation in non-eligible industries	0.047** (2.34)
Only SARE municipalities	

Table 7: Robustness Checks: effect against several control group trends

Table 7 presents the estimated effects. If the effect of SARE is spurious, caused by the early adopters having a comparatively larger increase (unrelated to SARE) in the gap between

¹⁹To compute a measure of industry composition, for each municipality we calculated the percentage of firm creation (from 1998 to 2001) that happened in eligible industries. We then identified municipalities by quartiles of this measure and allowed each quartile group to have its own set of monthly dummies.

industries than the later adopters, then introducing different benchmark time trends for early and late adopters should eliminate or at least attenuate this effect. It turns out that the effect not only does not disappear, but it is estimated to be bigger.

To the extent that early implementers constitute a better measure of counterfactual firm creation for early SARE municipalities, and analogously for late adopters, the estimate of specification (1) in this table may be closer to the true effect. An analogous argument applies for specification (2) and (3). Unfortunately there is no way to test which group provides a better counterfactual, but we believe is safe to say that the effect lies within the estimates of tables 7 and 6.

4.3.4 Time profile of effect

We believe we have presented convincing evidence that the estimated effect is causal, it varies between 4% and 8% depending the control group chosen. An important question we address now is the following: is the effect on the firm creation permanent or temporary? This question is important for at least two reasons: for measuring the sheer effect of the program, and for gaining insight into what may be driving the program’s effect.

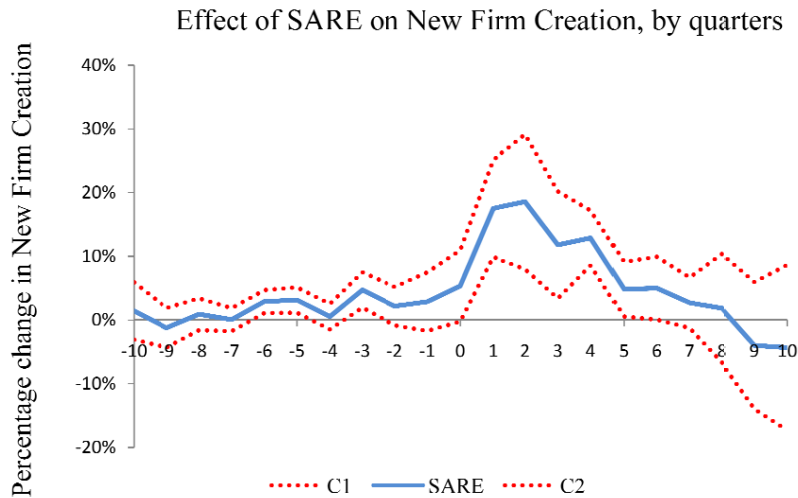


Figure 3: Firm Registration Before and After SARE (Includes only SARE municipalities)

Figure 3 investigates the dynamics of firm registration. We estimated a specification in which the effect of SARE is decomposed in quarters before and after implementation by interacting the ‘SARE effect’ term in equation 3 with dummies indicating quarter before and after SARE implementation in the respective municipality. We plot these coefficients in Figure 3 along with their 5% confidence intervals.²⁰

²⁰We estimate 14 quarters before and after implementation, but report only 10 after and 10 before. Note that the model is highly parameterized already and precision of the estimates may suffer by including more regressors (R squares are around 0.93)

First, it seems that the effect of SARE is temporary, being significantly different from zero from the 1st to the 5th quarter after implementation. Second and importantly, we observe no clear previous trend before SARE: the coefficients are not statistically different from zero. The non existence of a statistically significant prior trend²¹ and the steep break from this previous trend is strong evidence that our estimated effect is causal, especially since the program is implemented across the entire sample period in different geographical regions.

There are many potential explanations for this temporary increase in firm registration. For example, it could be due to an existing *stock* of informal firms (or formal entrepreneurs at the margin of starting up a firm) now registering (or creating new firms) once registration costs decrease; this would show up as a jump in the flow of registration. Alternatively, the program could have been better publicized when it was first implemented.

5 Interpretation and caveats

5.1 Magnitude of the estimated effect

How big or small is the estimated effect of SARE? Can we put the magnitude of our estimates into perspective? Note that the stock of informally operating firms that might be induced to register could be quite large. INEGI's Economic Census 2004 estimates that there were more than four million firm establishments in the country. Less than 2.5 million of firms were registered with the authorities and when compared to these number it seems that SARE has a limited impact.

We provide two benchmarks with which to compare the magnitude of our estimates: First, the World Bank Doing Business Report estimated that the effect of these types of programs on firm creation is higher than 20% in several developing countries. Their estimates, however, rely on before/after comparisons and lack an appropriate control group. Importantly our more careful estimates are about 3 to 4 times smaller! Second, our estimates are also substantially smaller than what the Mexican government reports. According to COFEMER, 75,168 new firms, 194,577 new jobs, and around 1,200 million dollars of new investment had been created *through* SARE as of July 2006. COFEMER implies that these registrations were caused by SARE.²² According to our estimates, the counterfactual number of firms created by SARE is closer to 4,029 (1,343 municipality-months*3 firms per month), which is about 20 times less! Back of the envelope calculations reported in previous versions (and available upon request) of the paper suggest that

²¹There is a slight upward but insignificant trend one quarter before adoption. The size of the trend break at the time of SARE implementation makes us comfortable of the validity of our identification strategy. There are several reasons to expect either a decreasing or an increasing trend close before adoption: (a) in the two or three months before implementation COFEMER officials are around evaluating the municipality procedures and we should expect an increase in speed of registration and a reduction on the backlog and thus an increase in registration; (b) there could be some media coverage of the fact that it is easier to register firms, thus increasing the demand for formal licenses and registrations; or (c) some potential clients could withhold their applications for a few weeks until SARE is operational, thus decreasing registration before SARE.

²² Statistics obtained from COFEMER's web page at <http://www.cofemer.gob.mx/portal.asp?seleccionID=66&padreID=10&hijoID=22>.

SARE would only decrease the size of the informal sector by 0.2%. The program is apparently not a big success as a tool to fight informality, measured against the expectation of the influential work of De Soto (1989).

It is hard to argue that there has not been enough time for these reforms to have their effect or that the program constituted a small change in procedures. First, the data captures firm start-ups up to almost four years after implementation for some municipalities, this should be enough time to see an effect. More importantly, the effect we estimate shows up immediately and only lasts for a bit more than one year. Second, recall that the time reduction caused by SARE is on average 28 days which, as mentioned earlier, is **comparable to the difference in delay between Jamaica and Canada**. This is a substantial reduction and programs of this kind will likely be similar. However, it is true that there are still some key procedures that cannot be done in the SARE offices.

To what extent are we missing firms that because of SARE registered with the tax authorities (SAT) and not with labor authorities (IMSS) which is our data source? This is a legitimate question since SAT is known to be more aggressive in pursuing evaders than IMSS. Fortunately, after a great effort we were able to obtain some administrative numbers on this from a match of the IMSS and SAT datasets, which are more reliable than numbers from existing surveys. It turns out that about 60% of firms (personas morales) and 48% of self employed entrepreneurs/workers (personas fisicas) that are registered with SAT could be matched with the IMSS dataset. This is surely a lower bound since there were some matching problems using the tax identification code (RFC) and several IMSS registries could not be matched.

Weighting the above numbers across personas fisicas and personas morales by the their proportion of workers registered in SAT, we conclude that we may be missing at most 45% of registered with SAT and not IMSS. This could imply an effect of 6.8% of SARE(=4.7% x [100%+45%]) instead of 4.7%, still much less than what the World Bank and Mexican authorities report. In any case, we can still say confidently that we are estimating the effect of the program on formal employment and on new firms with formal employees. In our view this is a significant contribution, since registration to formalize employment which gives access to social security and health care is as important as registration to pay taxes.

Having said that the effect of the program is likely to be below what the World Bank and authorities report, even a program with a relatively small effect may be worth pursuing if the benefit is larger than the cost of implementing it. Thus performing a cost-benefit analysis would be useful. However a good benefit analysis would have to measure the increase in welfare brought about by a new firm, which depends, for example, on the extent to which the good or service sold is new or not, and on the business stealing effect. Appropriate quantification of this involves knowing the exact products sold/produced and an estimation of demand which is clearly outside the scope of the paper and would require more precise information than what we currently have.

We report here a back of the envelope calculation to measure value added by average small firms in eligible industries in municipalities where SARE is implemented. The calculation of value added requires adding up the payments to factors of production: land, labor, capital and

entrepreneurship. We use the 2002 micro firms survey conducted in Mexico (ENAMIN)²³, which provides data on payments to land (rent), labor (wages) and entrepreneurship (profits) to calculate this.²⁴

We calculate that the value added by the average small firm operating in an eligible industry in a municipality where SARE is operating, amounts to \$5,078 mexican pesos per month. Considering that the effect of SARE lasts 15 months, and that on average five of the new firms created per month in eligible industries in 93 SARE sample municipalities can be attributed to the existence of SARE, the benefit from this program is 35.4 millions of pesos ($=93*5*15*\$5,078$).

As for the cost of the program, we must consider both the costs incurred by the Federal Commission for Regulatory Improvement (COFEMER) and by the municipalities implementing the program. On the side of COFEMER, this commission had four employees working on SARE, whose average monthly wage was \$30,000, totaling \$120,000 per month. On the side of municipalities, around half of them (say 46) bought computers to manage SARE while others used existing equipment. Assuming that the average computer required to manage SARE cost \$10,000 pesos, the total cost of this equipment was \$460,000. Besides this, no other cost can be attributed to SARE at the municipality level since the program was operated on premises already in use by the municipality, and with employees already working for the municipality. Thus, the total cost of implementing the SARE during the 46-months period analyzed, is 5.9 millions of pesos. This implies that the benefit of the program is almost 6 times higher than its cost, thus even if we overestimated the the benefits relative to cost by a factor of 6 times, it is worth it to implement SARE.

Kaplan et al. (2007) also examine how the effect of the program varies with additional outside SARE procedures which are required to operate a firm. They calculated that a decrease of outside-SARE registration costs of a magnitude equivalent to moving from the 75th percentile to the 25th percentile in this cost distribution²⁵ is associated with a 35% increase in SARE's effect (say from 4.7% to 6.3%). This calculation has to be taken cautiously, however, since the cost of complementary procedures may proxy for other factors in the municipality that may affect firm creation, like institutional quality.

5.2 Other barriers to formal firm entry

We believe that procedures to register a firm may not be the main barrier to open a formal firm. Two of the other main candidates in the literature are tax avoidance and lack of access to

²³This survey is conducted by INEGI at the households in 45 main urban areas in Mexico, and has a target population of firms with fewer than 6 employees (or 16 if the firm is in the manufacturing sector). We use the latest (2002) survey which contains 11,306 respondents.

²⁴Although the survey does not provide a direct measure of payments to capital, it provides information on outstanding debt. We actually calculate a floor of payments to capital for two reasons. First, we are not imputing rent payments on the ownership of machinery and equipment. Second, since the micro-enterprises created through SARE are not riskless projects, their payments to capital should be higher than that of CETES 28, which is the interest rate we are using to impute interest payments of outstanding debt.

²⁵As measured by the Doing Business in Mexico report, which amounts to USD\$460

credit. To gain a better understanding of the barriers of starting and managing a small firm in Mexico we use again the ENAMIN 2002. In this survey firm owners were asked to report the main obstacle for their business; 49.4% replied that it was lack of customers or strong competition, 12.5% said they had no problems, *only 3.8% said that credit was a problem, and only 1% said government authorities were a problem.* Even though firms in the ENAMIN sample a selection of firms that are operating and therefore successfully entered, the proportion of entrepreneurs who report problems of access to credit or dealing with authorities is surprisingly low.

Firm owners were also asked about how they financed their start-up firm; 40% said it was from personal savings, 11.4% said friends lent them the necessary funds, 20.5% said they did not need money to start, and 1.5% used trade credit. Only 0.5% of these entrepreneurs borrowed from commercial banks. These results may imply that access to credit for small firms is difficult, but it may also imply that the demand for bank credit is low.

The fact that entrepreneurs say that competition and lack of customers are their main problems suggests that we have to take seriously the possibility that human capital or entrepreneurial ability could play a significant role in limiting the effect of programs like SARE. Djankov, Quian, Ronald and Zhuravskaya (2006b) provide some evidence that entrepreneurs differ significantly from non-entrepreneurs in their attitudes towards risk and work-leisure preferences, as well as in their social environment.

5.3 Statistical Validity

Now we turn to the statistical or internal validity of our estimates. The biggest concern of any non-randomized program evaluation is whether it is identifying the causal effect of the program. There is never a way to prove conclusively that the results are not being driven by some omitted factor.

We showed, however, three important pieces of evidence which in our view greatly minimizes the risk of confounding the causal effect. First, we showed that selection based on time varying observables (and maybe unobservables) was not very likely. Second, we showed that the estimated effect is not present for samples in which it should not be. Finally, we documented a strong departure of the firm creation trend beginning from the time the program was implemented.

Concerns about the external validity of our estimates are obviously important as well, especially given our very limited knowledge of the determinants of new firm start-ups and the desire to implement these programs in many countries. Our estimates are of course estimates of the Treatment Effect on the Treated. Since we present evidence in Kaplan et al. (2007) that baseline factors seem to influence the program's effect, we cannot claim that the results of this program carry over to any other country or similar type of procedure. This is a concern that can only be overcome by studying these types of programs in other settings. Another often cited issue is the influence of general equilibrium effects, which may bias the magnitude of the estimate. In Kaplan et al. (2007) we argued why this should not be a big concern for us.

6 Conclusion

Policy makers around the world are investing considerable effort in decreasing the number of procedures and the time to register a firm. The expectations for these reforms include increased firm start-ups, decreased informality, and increased tax revenues. Nevertheless, there has been scant evidence on their effectiveness.

This paper presents evidence that reducing the costs of obtaining an operation license can in fact lead to increased formal firm creation. We also find, however, that the effects of the program we studied were temporary and of a smaller magnitude than those reported by authorities and the World Bank. Since the costs of the program have also been quite low, the program may well have been cost effective.

We conclude by mentioning that burdensome registration regulations may not be the most important barrier to firm creation or firm formalization. The cost of paying taxes, the small benefits of being formal (i.e. Mexico has low access to credit and high costs of enforcing contracts in court), and the activities in which informal firms operate (mostly street vendors and services like hairdressers, mechanics, small stores, and the like) make formal firm creation a multidimensional problem. Programs that attack single aspects of the problem will most likely have a small effect on informality and firm creation, and therefore on growth.

7 Appendix: Description of IMSS-INEGI industry match

Table 8 describes the IMSS-INEGI industry match with an aim to show that measurement error is not substantial. The unit of observation is an IMSS 4-digit industry. Of the IMSS industries that have at least one eligible INEGI industry inside, and are thus classified as eligible in the paper, we report how many also have non-eligible INEGI industries; we label these as “contaminated”, and show that weighting by the number of INEGI industries we have only 7.4% of contamination which should not attenuate the estimated effect of the program much.

Column (a) shows the number of IMSS industries with at least one eligible and one non-eligible INEGI industry. We can see that 80 IMSS (eligible) industries are “fully covered” in the sense that there are no ineligible INEGI industries inside; 19 IMSS of industries are contaminated, in the sense that they have eligible and non-eligible INEGI industries inside; and 203 (=302-99) IMSS industries have only non-eligible INEGI industries inside, thus 19 out of 302 IMSS industries are contaminated.

Table 8 reports the calculations we did in order ascertain the potential impact these contaminated industries may have on our results. Column (b) shows the number of non-eligible INEGI industries inside each IMSS industry. Columns (c) and (d) translate these IMSS numbers into INEGI industry numbers, so as to weight each IMSS industry by a “contamination” factor. Column (e) calculates the fraction of non-eligible to eligible in these IMSS industries, and column (f) calculates the number of INEGI industries in these contaminated IMSS industries as a fraction of all matched INEGI industries in order to weight the IMSS industries in the sum.²⁶

(a) Number of contaminated and some non contaminated IMSS Industries with at least one eligible INEGI industry inside	(b) Number of non-eligible CMAP sectors inside contaminated IMSS industries	(c) Total non-eligible INEGI industries ((a)*(b)) inside contaminated IMSS industries	(d) Total eligible INEGI industries inside these IMSS industries	(e) Contamination percentage = Fraction of Non eligible to eligible in these IMSS industries= c / (c+d)	(f) Weight of these IMSS industries using INEGI industries=(c)+(d) / Total number of INEGI matched industries	(g) Weighted contamination=(e) * (f)
1	9	9	4	69.2%	0.0219	0.0152
0	8	0	0	0.0%	0.0000	0.0000
0	7	0	0	0.0%	0.0000	0.0000
1	6	6	27	18.2%	0.0556	0.0101
1	5	5	1	83.3%	0.0101	0.0084
0	4	0	0	0.0%	0.0000	0.0000
1	3	3	2	60.0%	0.0084	0.0051
6	2	12	25	32.4%	0.0623	0.0202
9	1	9	20	31.0%	0.0488	0.0152
80 (non contaminated)	0	0	471	0.0%	0.7929	0.0000
Total Weighted Contamination %						7.4%

Table 8: Description of match IMSS-INEGI industries

²⁶Only 550 out of 685 eligible INEGI industries could be matched to IMSS industries, however they represent more than 90% of production in eligible INEGI industries.

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