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Are Burdensome Registration Procedures an Important Barrier on Firm Creation? Evidence from Mexico

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Abstract

There has been increasing concern that the difficulty of obtaining firm operation licences in developing countries may decrease firm creation and increase informality. We estimate the effect on new firm creation/registration of a program that speeds up firm registration procedures and makes them more transparent. The program was implemented in Mexico in different municipalities at different dates. Our preferred estimates suggest that new firm registration increased by around 4% in eligible industries. Most of the effect is temporary, being concentrated on the first 10 months after the program is implemented. This suggests that the program's effect may operate through the registration of existing firms instead of through the creation of new ones. We compare the magnitude of our estimates to various benchmarks in order to assess its importance.

KEYWORDS: Firm Creation; Regulation; Informal Sector; Program Evaluation

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1 Introduction

Firm creation is believed to be an important channel of GDP growth. In addition to expanding the range of products, entry can create more competition, lower prices for consumers, and may lead to better technology adoption. Changes in the status of existing firms from informal to formal may also have important effects on GDP growth: it is likely that informal firms have less secure property rights and thus lower than optimal investment and productivity growth, leading to lower profits and value added.[1]

The ability to start a formal firm, however, is limited by several factors including the burden of complying with government regulations. Excessive governmental regulations can provide an incentive to operate in the informal sector. Government regulation also may prevent some entrepreneurs from operating at all since there are inherent disadvantages of operating in the informal sector. The negative correlation between GDP growth and measures of the burden of firm entry regulation that some cross country studies have found could be rationalized by both of these effects[2].

But how burdensome is regulation really? Based on data from 85 countries Djankov, La Porta, Lopez-De-Silanes and Shleifer (2002) find a considerable burden on the entrepreneur looking to register a firm with the appropriate authorities: the average number of procedures required to start a firm around the world is 10, the average number of days is 47, and the official cost of following these procedures for a simple firm is 47 percent of annual per capita income. Djankov et al. (2002) conclude that for an entrepreneur, legal entry is extremely cumbersome, time-consuming, and expensive in most countries. Furthermore, the authors find that "stricter" regulation of entry is associated with sharply higher levels of corruption, and a greater relative size of the unofficial economy. As a result 'high regulation' countries may have low levels of tax collection, a heavy tax burden on formal firms, and 'unfair' competition from informal firms since they do not

[1]In this paper we say that a firm is formal when it is registered with the appropriate government authorities. De Paula and Scheinkman (2006) show that formalization is correlated with a firm's capital-labor ratio or investment per worker, and with higher profits, even after controlling for the quality of the entrepreneur.

[2]See for example Djankov, McLiesh and Ramalho (2006) Klapper, Laeven and Rajan (2006). Djankov et al. (2006) create an index of the burden of regulation based on average country rankings in the World Bank's "Doing Business" indicators. They find that countries that are in the highest (best) quartile of this index grow 2.3 percentage points faster than countries in the lowest (worst) quartile. This effect is more than twice the effect on GDP growth of going from the second quartile to the highest quartile in terms of primary school enrollment. The authors stress that reforms such as a "one-stop shop" for business registration could accelerate GDP growth.

pay taxes.

In this paper we suppose that there are benefits to operating in the formal sector, but firms may be reluctant to register and become formal if registration is too costly. We address two main questions. First, does a decrease firm registration costs lead to a permanent increase in firm registrations? Second, does this effect operate through the registration of existing informal firms or through the creation of truly new firms?

There are without a doubt many factors that influence a firm's decision to become formal besides the time and effort cost of going through the formal registration procedures. The tax liability of formal sector firms, combined with the requirement to comply with health and safety regulations, may be crucial in deterring firm formalization. The most cited potential benefits of being formal involve government protection of property rights, ease of transacting with other firms, and better access to credit for the firm (see Straub (2005)).

Therefore the magnitude of the effect of lower registration costs on firm registration is an empirical issue. If the main reason that firms choose to be informal is the desire to evade taxes, making registration procedures more efficient would likely have little impact. It is also possible that entrepreneurs are able to avoid the "excessive" regulations through bribes, thus effectively reducing the impact of regulation. Finally it is possible that the most important constraint on firm creation is the availability of credit or other complementary inputs; the scarcity of credit may limit firm creation and also negate one of the often cited advantages of becoming formal.

However, in 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)). According to WorldBank (2006), in 2003 the donors in the International Development Association and the United States' Millennium Challenge Account made grant eligibility conditional on performance on the time and cost of business start-up. As a result of the increased concern about burdensome governmental regulations, many countries have implemented reforms designed to simplify the process of registering and opening a firm. According to the World Bank's Doing Business Report 202 reforms in 108 countries were introduced between January 2005 and April 2006.

To estimate the magnitude of the effect of reducing registration procedures we use variation induced by the implementation of a "deregulation" program 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 which operate in eligible industries to obtain a licence to operate in two days or less. Before the program was implemented it took about 30 days to go through the municipal registration procedures, after the program is in place the procedures take two days at most. The change in the delay is equivalent to the difference in delay between Jamaica vs Canada. From this perspective it would seem that the reform improved the business climate substantially, at least for small firms in eligible industries.

Although the timing of introduction of the program and the industries to which it applied was not random, we provide some evidence that implementation may not have been related to time varying covariates or lagged outcomes. We also use different control groups and sources of variation to identify the program's effect, and find that our results are robust to different specifications.

In our analysis, we will use two different identification strategies: the first compares firm registration before and after implementation of the program in municipalities with the program vs. municipalities without it. Our second (and preferred) identification strategy is motivated by a concern that the timing of SARE adoption is systematically related to factors that are also correlated with the time trend of economic conditions in those municipalities. This could happen if for example the government implements in bigger municipalities first and macroeconomic conditions affect them differently. We present some evidence that early adopters are different than late adopters (see Table 10), and that time trends of firm registration for adopting cities may be correlated with the timing of program adoption of those cities (see Figure 5 in Appendix 7.3.)

To avoid relying entirely on cross municipality comparisons our preferred estimates compare the time trend in new business creation for industries affected by SARE to the time trend for exempted industries in the same municipality. This later strategy is a 'within' municipality comparison that is robust to some potential problems of selection of municipalities and municipality specific shocks.

Our preferred estimates imply that the program generated a 4% increase on monthly new firm registration. This increase in the flow of firm registration appears to be temporary and concentrated in the first ten months after implementation, leading us to conjecture that the program mostly affects the existing stock of informal firms and has a negligible effect on the creation of "truly" new firms. 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, these latter two facts bolster our claim that the effects on firms creation that we estimate are the causal effects of the program.

The World Bank and many governments have high expectations for programs of the type we study as promoters of economic development. We therefore pay special attention to the magnitude of the effect we estimate. Using several benchmarks we conclude that the number of firms created and/or formalized by the program was not as large as expected, amounting to at best an incorporation of 0.2% of informal sector workers. We offer some conjectures as to why the effect has been so modest.

There have been some previous papers that pose questions related to ours. The main difficulty of these papers is to establish a causal relationship between the regulatory burden and economic outcomes. Bertrand and Kramarz (2002) study how variation across time in the toughness (rejection rates) of the application of zoning restrictions had an impact on employment growth; they use the political party composition of the approving board as an instrument for time variation in rejection rates. Djankov et al. (2006) instrument their index of regulatory burden with a legal origin variable and with geographic and cultural variables in an attempt to establish causality from regulation to GDP growth.

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. Apart from using a different methodology, our paper has important implications for policy makers today. Although deregulation programs of the type that we study have been in place for some years and pressure for their implementation has caused more than one hundred countries to carry out these reforms, there is scant evidence of their effects and of the determinants of their effectiveness. This paper makes a contribution towards measuring the effect of one of these programs, Mexico's "System of Fast Opening of Firms" (or SARE for its initials in Spanish), which shares many of the characteristics with 'deregulation' programs in other countries.

Miriam Bruhn (2006) has independently (and simultaneously) evaluated the effects of SARE using household data. Since the two papers examine the same program, we devote several paragraphs in Appendix 7.1 to establishing their differences, similarities, and complementarities.

To summarize briefly here, our paper has several key advantages. Our data covers a longer time period, allowing us to study longer term effects of the program. The fact that our data covers a longer time period, combined with the fact the we use census data, allows us to study 59 more SARE implementations than studied by Bruhn (almost 3 times more) and avoid the problems of survey representativeness that she faces. Moreover we use actual registration data instead of survey responses. Finally, we provide a more careful 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. This is important since during this time period Mexico was undergoing a significant economic slowdown that may have affected municipalities differently. As a result of our strategy, in contrast to Bruhn (2006), our preferred estimation strategy shows no evidence of differential trends *prior* to the implementation of SARE.[3]

Bruhn (2006) does have several advantages over our paper. She is able to control for person-level characteristics, including the pre-SARE status of the individual, that is: not employed, employed, owner of a registered business, or owner of a non-registered business. Using this feature of her data she presents evidence that the increase in business registration brought by SARE comes from new business owners who had been wage earners prior to the program. Due to the numerous differences between the two papers, we view our papers as complementary. See Appendix 7.1 for more details.

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 our outcome variables. Section 4 will describe our two empirical strategies, our the main results and perform specification and robustness checks. Section 5 contains a discussion of the magnitude of the effect by comparing it to the size of the informal sector. Section 6 concludes.

2 Institutional Setting and Description of the Program

2.1 The Informal Sector and the Regulatory Burden in Mexico

How big is the informal sector in Mexico? Using the ILO definition of informal sector employment[4] the Mexican Statistical Institute (INEGI) reports that, in the year 2000, 23% of the employed population worked in the informal sector. The share of GDP produced by the informal sector is 12.5% (which implies that the informal sector produces less output per worker). Schneider and Enste (2000) report higher numbers: the percentage of GDP produced in the informal sector is between 27% and 49% depending

[3]In Bruhn (2006), one observes a positive trend in the probability of owning a registered business prior to SARE implementation. Although this difference only becomes significant after SARE is implemented, the graphical evidence she presents in figure 6 generates some concern over her estimation strategy.

[4]"According to the International Labour Organization the informal sector is composed of legal, non-agricultural goods and services which are destined to the market and are produced by private unincorporated enterprises, which are part of the household sector". Cited from Garcia-Verdu (2006)

on the method used to measure it. In particular it is higher than in Costa Rica, Chile, Argentina, Uruguay, and Venezuela, among others.

Not only does Mexico have a big informal sector but, consistent with the 'Burdensome Regulation View', Mexico also was among the countries with more lengthy firm registration procedures, ranking 69 out of 85 countries studied by Djankov et al. (2002), taking 67 days to register a firm. This is higher than Jamaica, Peru, Uruguay, Chile, Argentina and Brazil.

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 make these procedures more transparent (in some instances the monetary cost was also reduced).

2.2 Description of the Program

SARE is a Federal level program that ensures "that micro, small and medium firms, which carry no risk for health and environment, can register and open in two days"[5] after filing with the municipality's SARE office. It aims to achieve this objective by consolidating Federal, State and Municipal[6] procedures to register and operate a firm in one municipal office, capping the number of mandatory Federal procedures at only two. SARE requires municipality governments to issue the operation licence in at most 48 hours assuming that industry eligibility and zoning requirements are satisfied. The program effectively permits operation of the firm while postponing federal inspections and requirements for three months after registering with the Federal Tax Authority. SARE 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 is not operated by the Federal government. SARE is operated by the municipalities and each municipality is responsible for publicizing the program and maintaining high standards of efficiency and service. Since municipalities in Mexico can enact regulation about zoning restrictions, firm operation permits, health standards for firms,

[5] "Guias para la Mejora Regulatoria Municipal", COFEMER's web page.

[6] 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 two or more municipalities.

and civil protection issues among others, they have substantial control and influence on firm registration. It is for this reason that municipalities are believed to be the main bottleneck in the process of firm registration. Additionally, since many revision and compliance checks are conducted by municipality governments, it is commonly believed that most corruption related to firm registration occurs at the municipality level. For these reasons, municipalities are the main target of the SARE program.

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.[7] 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. Failure to maintain the standards can result in the public removal of the "SARE" label from the municipality. The municipality also reserves the right to withdraw from SARE at anytime. Neither of these events has ever happened.

It is important to note that not all firms can register and obtain a licence through SARE. The Federal government selected 685 "non-risky" 6-digit industries as eligible for the program, 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 prone to accidents or for firms prone to health hazards. As a result of these criteria, the retail and services sectors are disproportionately represented as eligible industries. Although each municipality was encouraged to select all of these 685 industries, most included only a subset of this list. Despite having considerable autonomy, municipalities tended to select the same industries as eligible, mostly copying their lists from other municipalities that already had implemented the program.

Firms that satisfy the eligibility criteria must register through SARE; unjustified rejections are illegal. More importantly for our purposes, firms cannot register in one municipality and operate in another, thus enabling us to estimate SARE's effect by comparing outcomes in a municipality with SARE to those in municipalities without it. Since the mean number of employees of a firm registering through SARE is 2.6 employees, we believe that firms are most likely single establishment entities. Finally we

[7]COFEMER also provides financial support for some municipalities.

want to emphasize that, to our knowledge, there were no other government programs being implemented with a similar location-time profile. Thus there were no government induced policies whose effects we could be attributing to SARE.

2.3 Implementation of SARE

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. The program could not be implemented simultaneously in all locations mostly because of COFEMER's limited resources. 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. These municipalities are smaller and asked for SARE without COFEMER's encouragement.

SARE adoption by year**: Timing, Geographical Breath and Municipality Clustering

	2002	2003	2004	2005	2006
Municipalities Implementing SARE	2	8	28	47	8
Number of municipalities implementing in the most active State*	0	2	10	9	3
Number of States implementing	2	7	15	17	7
Non Competitive Municipalities	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 Breath and Municipality Clustering

Table 1 presents statistics on the timing, geographical variation, and clustering of SARE adoption. The first row shows the number of municipalities that adopted the program in each year. The second row shows that municipalities within a state tended to implement at the same time; in 2004 more than a third of implementation happened in one state (the state where the President of Mexico came from), for 2005 about one fifth of implementation came from the most active state. The third row of the table depicts the number of states where implementation took place. 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 last row counts the number of non-competitive municipalities that implemented. Since they were not explicitly invited, they typically implement 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, 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 employment, new firm creation and population than the other three groups. They also have a higher share of registered employees and a higher proportion of workers in the tertiary sector.

Means of Selected Variables by Type of Municipality (monthly averages)

Variable	**SARE**	**Competitive**	**Adjacent**	**Other**
Employment***	61,450	13,213	1,446	1,420
Monthly New jobs by new firms***	409	99	16	15
Monthly New firms (mean)***	111	26	4	4
Monthly New firms (median)***	69	6	1	1
Firms that Stay***	3,620	786	131	117
Population**	1,332,588	504,958	120,846	101,024
% of Working Population with IMSS**	10%	7%	3%	3%
% Workers in Terciary Sector**	54%	53%	35%	35%
Number of Establishments*	11,518	4,089	751	685
Production*	$2,388	$846	$100	$59
Number of Municipalities	93	142	267	1,008

*As reported in the economic census 2004 (millons of 2004 dollars); ** From the Population Census 2000; *** From our IMSS dataset (averages 1998-2001)

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 officials, surveys to the 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 expected future values of these. 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 regulation. The state governors in turn convinced municipality mayors. This convincing was 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 at the moment of implementation (in the complete sample period this number is 45%). In our sample period more than 50% of municipalities implement in the mayor's second year of tenure (municipality mayors have three year terms), and we find that there is clustering of implementation within a State.

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 and that past levels of firm and job creation are not important determinants of adoption.

3 Description of the Data

We will use three sources of data: (i) data from the Mexican Institute of Statistics, Geography and Informatics (INEGI); (ii) contracts of the Federal government with 31 of the 93 municipalities that implemented the program; and (iii) proprietary data from the Mexican Social Security Institute (IMSS).[8]

The first source of data is from Mexico's Statistical Institute (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. The second source of data is from the "Federal Commission for Regulatory Improvement" (COFEMER), the Federal government agency in charge of SARE. We collected surveys from COFEMER officials regarding reasons for the different timing of SARE implementation as well as main obstacles for the program's adoption in each municipality. Finally we used contracts between COFEMER and 31 municipalities that have implemented SARE. These contracts contain the lists of eligible industries as well as documentation about the SARE registration procedures.

The third data source is a data set from the Mexican Social Security Institute (IMSS).[9] IMSS is the Mexican equivalent of the US Social Security and one of the main providers of health services for registered employees. In this paper, we use data

[8]We considered using Mexican household data (the ENE). These data, however, are not representative at the municipality level. They also suffer from the problem that one can only observe where a person lives; one cannot observe where the person works or the location of the business the person operates.

[9]These data were obtained through a data-sharing agreement between IMSS and ITAM. All analyses were conducted using a secure server at ITAM. See Castellanos, Garcia-Verdu and Kaplan (2004) and Kaplan, Martinez and Robertson (2005) for more detailed descriptions of the data.

taken from the last day of each month from January 1998 through March 2006. We use a **census** of establishments that have employees registered with the Institute. Registration of all employees is required by law, although it is well known that establishments do not always comply with this law. This means that what we use as our measure of outcomes is not necessarily new firms, but rather the number of new formal firms. Since we observe all registered workers for each establishment, it is straightforward to count the number of employees in each month in each establishment. Although we do not observe much information about the establishments, we do observe the number of employees, their four-digit industry code as well as the municipality in which the establishment operates.

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. One complication is that the municipalities use an industry list from INEGI for the administration of the program, while IMSS manages its own list of industries. We therefore had to construct a concordance between these two lists of industries. Since the INEGI industry classification is more detailed than the IMSS industry classification, we manually matched 685 6-digit INEGI industries to 302 4-digit IMSS industries.

With this concordance in hand, we were able to separate establishments into two groups: those establishments that are eligible for the SARE program and those that are not. Although there is some variation in industry eligibility across municipalities, this variation is not substantial. As mentioned above, we were given access to 31 contracts between COFEMER and municipalities that have implemented SARE. Analyzing these contracts gave us 31 separate industry-eligibility lists. Based on these lists we extrapolate industry eligibility to the remaining 62 SARE municipalities using two definitions of industry eligibility. The first is the "union" of eligible industries. Using this definition, we classify an industry as being eligible if it appears on the eligibility list of at least one of the 31 municipalities. The second is the "intersection" of eligible industries. Using this definition, we declare an industry to be eligible if it appears in all 31 industry-eligibility lists.

There are 30 four-digit industries in our intersection sample and 97 in the union. Our results are quite similar with these two definitions of industry eligibility. Nevertheless, it is important to note that eligible industries include a bigger share of retail and services relative to manufacturing. In particular eligible industries do not include agriculture, construction, and manufacturing that involves chemicals or pollutants. Table 11 in Appendix 2 presents the eligibility status of the 30 IMSS industries with the most firm creation.

Once we had the definitions of eligible industries, 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 on that aggregates the data for all non-eligible industries. We created the following variables:

i) **Number of new establishments**. This variable is the number of firms in the current month with at least one employee that did not have any employees in the previous month.

ii) **Jobs created by new establishments**. This variable is the current employment in firms that did not have any employees in the previous month.

iii) **Jobs created by continuing establishments**. Define $empl_{jt}$ to be employment in establishment j in month t. Jobs created by continuing establishments is $\max(0, empl_{jt} - empl_{jt-1})$.

iv) **Number of exiting establishments**. This variable is the number of firms in the current month with no employees that had at least one employee in the previous month.

In addition to the concordance of INEGI and IMSS industries, we had to construct a concordance between INEGI and IMSS municipalities. According to the INEGI classification there are 2448 municipalities in Mexico, since IMSS has a different method to classify geographical areas we only have 1510 IMSS municipalities. 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 out their corresponding IMSS municipality; these are mainly smaller municipalities. We do not lose any SARE municipalities, although we lose 16 Competitive municipalities. This loss is both unimportant for the results and unavoidable given our data and keys for matching.

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.[10] 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. 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 creation it makes it less likely that time-varying unobserved variables are affecting the trends of firm creation differentially for SARE municipalities. We 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. This holds also if we control for municipality fixed effects. We also test if the trends of firm creation are parallel and cannot reject that treatment and control groups have the same time trends before the implementation of the program.

Second, we will use two sources of variation to identify the effect of SARE: comparing across industries (section 4.3) and comparing across municipalities (section 4.2). 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 we believe that the estimates comparing across industries are more reliable and we focus mostly on these in the paper.

Third, we will calculate our estimates for different definitions of industry eligibility and show that municipalities' slight differences in the choice of eligible industries are not driving the results. Finally, we will report several specification and robustness checks in section 4.5.

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

4.1 Determinants of Program Adoption

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 different time trends compared to control municipalities even in the absence of the program. If these factors are observed we can allow time trends to depend on them and consistently estimate the effect of the program. However, if they are unobserved, comparing firm creation of adopters vs. control municipalities before and after the program will give us inconsistent estimates of the true effect of the program. The same problem holds if we compare eligible vs. non eligible industries: if adoption of the program is correlated with future changes of the industry composition of new firm creation, then our estimate of the causal effect will not be consistent [11].

Given the above concerns, it is therefore informative 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, time-varying unobservables will also be uncorrelated with adoption.[12] We estimate a discrete Weibull duration model of program implementation (as described in Jenkins (1995)) and show that (static) political variables are a more important determinant of the timing of adoption than time-varying economic variables. The political variables we use include: party of the municipality mayor (PRI, PAN, PRD) and his tenure at the time of adoption (the excluded categories are other parties and coalitions and the first year of tenure, respectively); we also include as a regressor the number of municipalities that have implemented in the State at any given time to capture the effect of "recommendation" by the State governor.

The time varying economic variables we use are firm creation and job creation in continuing establishments. We 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

[11] Actually, in our preferred specification we still have consistent estimates as long as the change in industry composition is the same for early vs. late program adopters.

[12] In Appendix 7.3, Figure 5 presents a time series graph of the average of new firm creation for SARE municipalities by year of adoption. The graph shows that bigger municipalities tended to implement first and that on average there is no pre-program trend in new firm registration for any of the groups. Systematic changes in the levels of firm creation before program implementation could influence the estimated effect of the program to the extent to which these are related to unmeasured fundamentals that influence future firm creation. In particular, if municipalities experience a transitory slowdown before program adoption we would overestimate the program's effect.

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 de-trend these variables.

The time constant regressors we use are demographic and economic municipality characteristics from the 2000 Population Census, the 2004 Economic Census, and municipality data bases collected by the Mexican Statistical Institute. These regressors are the following: 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).

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', and thus only those are included in the sample for the table. Table 3 shows the results for two specifications; they differ by whether we include past firm and total job creation separately for eligible and non-eligible industries (specification (2)), or whether we aggregated them (specification (1)). The coefficients are reported in a exponential form so that they can be interpreted as semi-elasticities of the hazard of implementation.

Discrete Duration Model of Progam Implementation

	(1)	(2)
Weibull Duration dependence parameter	16.47***	16.72***
	(4.4)	(4.29)
Party 1 (official)	0.27**	0.25**
	(-2.1)	(-2.18)
Party 2	0.13***	0.16***
	(-3.08)	(-2.83)
Party 3	0.19*	0.24
	(-1.71)	(-1.44)
2nd Year of Tenure	2.9**	3.17**
	(2.17)	(2.31)
3rd Year of Tenure	2.1	2.10
	(1.50)	(1.47)
Number of Mun. in State that implemented	1.26***	1.26***
	(2.98)	(2.97)
New Firm Creation (MA12, detrended)	1.008	
	(0.45)	
Job Creation (MA12, detrended)	1.0006	
	(1.23)	
New Firm Creation in Eligible Industries (MA12, detrended)		1.01
		(0.53)
New Firm Creation in Non-Eligible Industries (MA12, detrended)		0.97
		(-0.62)
Job Creation in Eligible Industries (MA12, detrended)		1.002
		(1.18)
Job Creation in non-Eligible Industries (MA12, detrended)		0.99
		(-0.53)
Total Population (thousands)	1.001	1.002
	(0.68)	(1.31)
Production ($dollars per capita)	1.00001	1.00004
	(0.14)	(0.39)
Unemployment (2000 cenus)	1.26	1.20
	(0.85)	(0.66)
Working Age Population (thousands)	0.99	0.99
	(-0.38)	(-1.02)
% Employees in Tertiary Sector	1.09	1.04
	(0.14)	(0.07)
% Working Age Registered at IMSS	1.17	0.96
	(0.80)	(-0.16)
Log (State Exports)	0.74	0.75
	(-1.44)	(-1.34)
% Employment in Exporting Firms	0.10	0.19
	(-0.84)	(-0.58)
% Workers receiving no income	0.24	0.28
	(-0.62)	(-0.54)
Municipality government revenues ($ millions of dollars)	1.004	1.004
	(1.60)	(1.39)

Coefficients are reported in exponentiated form.

Table 3: Discrete Duration Model of Program Implementation

As expected 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 in both specifications. 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 SARE, reinforcing our belief that SARE is implemented because a recommendation of the governor and not as a result of a municipality specific shock. The demographic variables are not significant.

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. These political variables are not correlated with firm or job creation the before SARE period: the correlation coefficients between firm creation and parties are: PAN=0.21, PRI=-0.10, PRD=-0.02. The correlation coefficients of firm creation and tenure are: 1st year=0.0003, 2nd year=0.001, 3rd year=0.0002. Regressions of firm creation against these political variables including municipality and monthly fixed effects offer no evidence of a correlation different from zero.

4.2 Comparing Adopting vs non-Adopting Municipalities

In this section we estimate the effect of SARE on firm creation in eligible industries by comparing adopting versus non-adopting municipalities. Although this is the more obvious strategy it is not our preferred one since, as table 2 showed, these two groups of municipalities are very different in the levels of the reported variables.

As it is well known, differences in the levels of the outcome variables across treatment and control groups do not invalidate a differences-in-differences approach; however, differences in the time trends of outcomes may. What is typically done in the literature is to test for similar time trends between the control and the treatment group before the treatment. To conduct this test we ran regressions where the dependent variable was firm creation in eligible industries or the difference in firm creation between eligible and non eligible industries, and where explanatory variables included a separate linear time trends for the control group and the treatment group, common monthly fixed effects to control for common seasonality, and municipality fixed effects. Two control-treatment comparisons were used: Competitive (not-yet-SARE) vs SARE municipalities, and early vs late SARE adopters. Equality of the pre-SARE time trends for treatment and control groups was not rejected in any of these four specifications, with p-values greater than 0.5 in all cases.

Although we cannot reject that linear time trends are similar before the implementation of the program, the main identification problem that arises when we compare municipalities is that when 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 partly as a result of the US recession (Banxico (2001, 2002, 2003, 2004, and 2005)). This deceleration was stronger in big municipalities (measured by production or population). We therefore suspect that, despite having similar pre-adoption trends, municipalities that adopted SARE earlier may have experienced differential post-SARE shocks that were unrelated to the program itself. This fact makes comparisons across municipalities problematic: using non-SARE municipalities or late SARE adopters as a control group would *understate* the effect of the program since (early) SARE municipalities are bigger. Figure 1 clearly shows this identification problem.

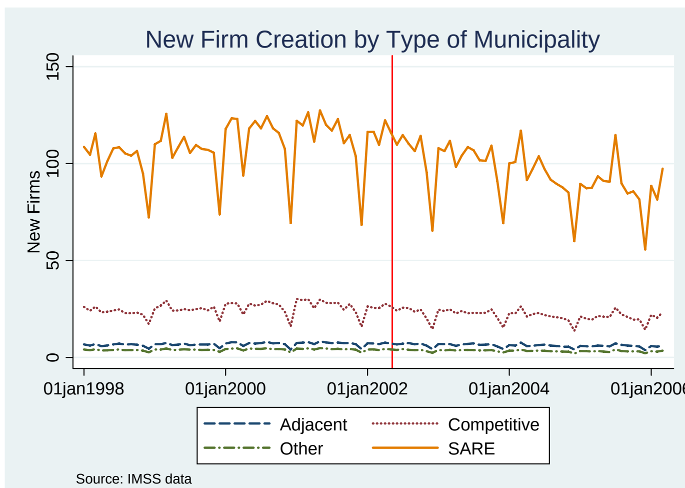

Figure 1: Mean Trends of Monthly Firm Creation by Type of Municipality

A naive differences-in-differences estimate using Competitive municipalities as controls shows that after SARE is implemented there is a decrease of monthly firm creation of about 5%. Our strategy in this section will therefore be to estimate a flexible model which allows for different time trends for different groups of municipalities. We do this by interacting monthly time dummies with covariates which we believe to proxy for the strength of the above mentioned shock. We view the sets of estimates from the strategy in this section as a lower bound on the effect of SARE since we believe that we cannot control for all omitted variables making the downturn stronger for SARE municipalities.

In order to understand the intuition behind the identification strategy, let Y_{it} be the log of one plus the number of new firms, where $i = 1$ if the municipality implements

SARE and zero otherwise.[13] Let t index time and M=1 indicate municipalities with the program. We want to estimate the treatment effect on the treated given by the following expression: $ATT \equiv E[Y_{1t} - Y_{0t}|M = 1]$. If SARE municipalities without the program would have had the same mean outcomes as the control municipalities, then β in equation 1 estimates the ATT.

$$Y_{it} = \alpha_i + X_i'\gamma_t + \beta AfterSARE_{it} + \epsilon_{it} \tag{1}$$

In this equation α_i are municipality fixed effects, γ_t are month fixed effects, and $AfterSARE_{it}$ is equal to 1 for municipality i after the program is implemented in that municipality and zero before implementation. In all (but the first) specifications reported in table 4 in which we use competitive municipalities as a control group, Xit includes a dummy that identifies SARE municipalities. Therefore we allow SARE municipalities to have a different time trend even before program implementation. We will estimate different specifications by changing the controls in Xit, that is, by changing the benchmark time trends against which the program's effect is measured.

The error in equation 1 could be correlated across time within a municipality, which would happen if economic conditions are persistent. It can also be the case that economic conditions within a state are similar, inducing correlation of the error terms across municipalities within a state. To avoid erroneous conclusions from biases of the standard errors, in all regressions of this paper we report standard errors clustered at the municipality level (we also clustered at the state-year level and the results were unchanged).

Table 4 presents the estimated β coefficient of six specifications (rows) using two different sets of control municipalities (columns). In specification (1) we set X_{it}=1. We can see that naively using common month dummies and municipality fixed effects does not control for the declining economic activity and confounds the effect of SARE with the economic slowdown of big municipalities.[14] In specification (3) X_{it} include measures of the importance of exports and 'maquila'[15] in each of the States to which the municipality belongs. For data availability reasons these variables are defined at the State level, not at the municipality level. Three measures where used: the percentage of the State's GDP made up by exports, the log of total trade in dollars, and the percentage

[13]In some municipalities in some months, the number of new firms is zero. For this reason we take as the dependent variable the log of one plus the number of new firms.

[14]This happens also for non-eligible industries as well as for all firm sizes.

[15]A maquila is a factory that imports materials and equipment on a duty-free and tariff-free basis for assembly or manufacturing and then re-exports the assembled product, usually back to the originating country.

SARE's Effect on New Firm Creation (Comparing Municipalities)		
Specification	(a) Only SARE's	(b) SARE's and Competitive
(1) Naïve	-0.05**	-.08***
	(-2.42)	(-2.98)
(2) Separate Time Trends for SARE's	-------	-0.05**
		(-2.23)
(3) Exporting Controls	-0.05**	-0.05**
	(-2.24)	(-2.14)
(4) Population Controls	-0.03	-0.03
	(-1.22)	(-1.26)
(5) Time Trends by Year of Adoption	-0.04	-0.04
	(-1.16)	(-1.18)
(6) Linear Time Trends for each SARE Municipality	-0.003	-0.01
	(-0.18)	(-0.56)

(a) SAREs: 93 Municipalities, 99 months; 9207 obs; (b) SARE's and Competive: 239 municipalities, 99 months = 23661 obs. Errors clustered at the municipality level.

Table 4: Comparing Firm Creation Across Municipalities: β coefficients

of exports made up of 'maquila' exports. These interactions were significantly different from zero but not very effective controls for the decline in economic activity of SARE municipalities since the 'After SARE' coefficient is unchanged. This may be due to the fact that municipalities within a State are very heterogeneous, making State level variables uninformative at the municipality level.

In specification (4) X_{it} include total population and the percentage of the working population in the tertiary sector. When we use these regressors the estimated negative effect of SARE is no longer significantly different from zero. In specification (5) we formed groups of municipalities that adopted SARE early (before 2004) or late (after 2004) and assigned a different set of monthly dummy variables for each of these two groups. In this case we estimate SARE's effect to be indistinguishable from zero. Finally in specification (6) we include a municipality-specific linear time trend for adopting municipalities. Analogous to a regression discontinuity design, the effect of SARE is identified as the break from that linear trend. We again estimate a zero effect of SARE.

Overall, once we include interactions of time trends with covariates that control for some of the heterogeneity between treatment and control municipalities, the effect of SARE is not different from zero. As we discussed at the beginning of the section we view this estimate as a lower bound and we devote the rest of the paper to estimating the effect of SARE by comparing eligible versus non eligible industries instead.

4.3 Comparing Eligible vs. Non Eligible Industries

Given the difficulty of finding a good control group for SARE municipalities, we decided to use comparisons *within* SARE municipalities across eligible vs. non-eligible industries to estimate the program's effect on firm creation. 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 SARE 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.

This strategy allows us to control for the general (across all industries) slowdown of economic activity after 2002, which was particularly pronounced in the larger municipalities that tended to adopt SARE. Since it can be the case that eligible and non-eligible industries have different time trends, what we effectively do is to compare the gap of firm creation across industries in treated municipalities to the analogous quantity for the control municipalities. This amounts to using a diff-in-diff-in-diff strategy which can be written as follows:[16]

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

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 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 (recall that all industries have been aggregated in one of those two categories). The first square bracket contains the difference of firm (or job) creation in eligible vs. non eligible industries in SARE municipalities. The second square bracket contains the same quantity for control municipalities.

Effectively, our empirical strategy will attribute the relative increase in the gap of firm creation between eligible versus non-eligible industries to the SARE 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 recently adopting municipalities than for the controls (late adopters or competitive non-SARE municipalities). Figure 2 shows the evolution of firm creation by industry

[16]For a paper that also uses this 3rd difference approach see Gruber (1994).

eligibility for early (before or in 2004) and late (after 2004) SARE adopters.

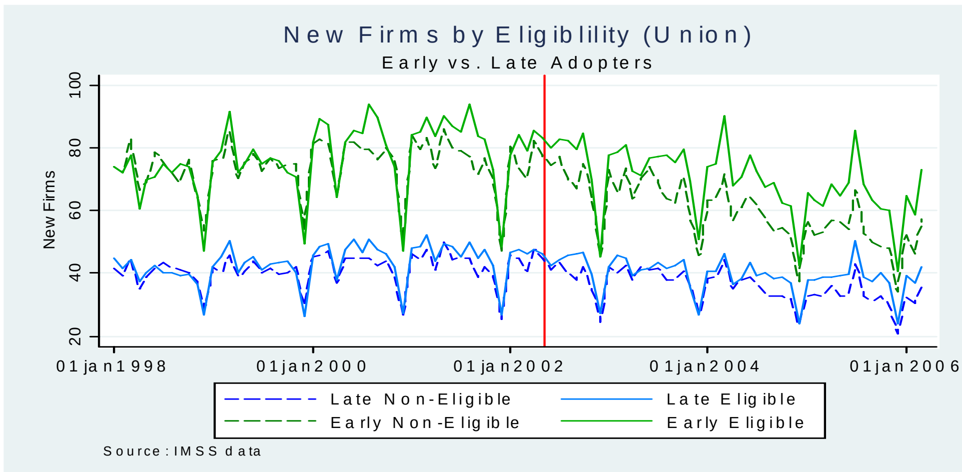

Figure 2: Mean Trends of Firm Creation by Eligibility for SARE and Comptetitive Municipalities. "Union" definition of eligibility is used

Note that our identification assumption is weaker than the assumption used in the simple differences-in-differences analysis, since ours is robust to municipality-specific time-varying shocks, as long as they impact eligible and non-eligible industries symmetrically. Because of this robustness, it reduces the possible bias introduced by the endogenous selection of municipalities.

However, as with any non-randomized program evaluation, the unbiasedness of the estimates is never guaranteed. If the *gap* in firm creation between eligible and non-eligible industries evolves differently in SARE and non-SARE municipalities, our identification strategy will yield and inconsistent estimate. There are plausible stories where this can happen. The best story we could think about involves a) SARE municipalities being especially dependent to the US economy, b) eligible industries being composed mostly of retail and service non tradeables, and c) the timing of adoption to be such that more dependent municipalities implemented first. If this is the case, the downturn of US activity could hit non-eligible industries harder in the early SARE municipalities, with implementation happening just when the effect becomes stronger, thus widening the gap between industries more in the first SARE municipalities.

In section 4.5 we present some evidence showing that there is no before-SARE trend in the gap of firm creation and that the effect seems to be present only in new creation of small firms and not on employment in existing firms, making it less likely that this alternative story explains the data. The fact that our results are robust to the definition of industry eligibility is also encouraging since the effect is less likely to be an artifact of some industry outliers. In any case, if this alternative story is true our estimates would *overestimate* SARE's positive effect. Since we will later argue that SARE's effect is not big anyway in spite of our possible overestimation, this alternative story strengthens our conclusion.

Summary Statistics by Type of Municipality and Industry (1998-2000)

Industry	Variable	Adjacent	Competitive	Other	SARE
Non-Eligible	New Firms	3	11	2	49
	New Jobs by New Firms	16	49	9	213
	Current Employemnt	1,686	5,265	718	28,055
	Non-Exiting Firms	77	228	40	1,122
Eligible	New Firms	3	12	2	53
	New Jobs by New Firms	9	36	4	174
	Current Employemnt	1,532	6,257	646	33,490
	Non-Exiting Firms	141	534	76	2,542

Sample period 1998-2000. We use the union definition of eligibility.

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

Before proceeding to the estimation, Table 5 shows some summary statistics comparing eligible and non-eligible industries in SARE municipalities. It shows that eligible industries are slightly bigger in terms of new firm creation, current total employment and the number of stable (non-exiting) firms (firms that had registered employees last month and still have this month). They are a bit smaller in terms of jobs created by new firms, implying that the average new firm in non-eligible industries has about 4 employees compared to 3 for eligible ones.

We will use the following regression to estimate the effect of SARE:

$$Y_{ikt} = \alpha_i + \gamma_t + \theta_k + \beta AfterSARE_{it} * I_k + \phi AfterSARE_{it} + \sigma(t) * I_k + \delta SARE_i * I_k + \lambda X_{it} + \epsilon_{itk} \tag{3}$$

The effect of SARE is captured by β which is the coefficient of the interaction of the eligible industry dummy, the 'after implementation' dummy and the SARE municipality dummy. This estimates the effect defined in equation 2. The parameters α, γ and θ are municipality, month and industry fixed effects. The remaining regressors are the second order interactions between industry, municipality and time, and some controls.

The parameter ϕ captures the time trend 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 occurs around 2002. The term $\sigma(t)$ 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 (SARE municipalities before implementation and non-SARE 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_{it} will be explored on Section 4.6, in this section we do not include X_{it} in the regressions.

The OLS estimation will be done for four samples defined by whether the control group includes Competitive municipalities and by the definition of industry eligibility used.

SARE's Differential Effect on Eligible Industries

	Only SARE's*		SARE's and Competitive**	
Dependent Variable	Union	Intersection	Union	Intersection
log (New Jobs in New Firms)	0.08**	0.07*	0.11***	0.08**
	(2.2)	(1.8)	(3.02)	(2.2)
log (New Firms)	0.04**	0.05**	0.09***	0.09***
	(2.08)	(2.2)	(4.09)	(4.9)

*93 Municipalities, 99 months; 9207 obs; ** 239 municipalities, 99 months = 23661 obs. Errors clustered at the municipality level.

Table 6: SARE's effect

Table 6 shows eight β estimates from equation 3. The columns define the group used as control for each of the two definitions of industry eligibility; the rows present the results for different dependent variables. All the estimated coefficients imply that SARE had a positive and statistically significant effect. This effect is robust to the definition of industry eligibility.

In the sample that uses only SARE municipalities the effect of the program is a 4% to 5% increase in firm creation. In this sample the implicit control group is late SARE adopters. When we include all Competitive municipalities as controls the estimated effect is 9%.[17] The effect on job creation by new registered firms is twice as big: an increase of 8% to 11%. This is important since it implies that after the program is implemented the new firms being registered are bigger. We take this as evidence

[17] We found a statistically significant coefficient (not shown in table) on the first order term of the relative industry time trends (the σ function) of about 0.004 more new firms per month. We believe that this non-SARE increase in the gap is not big enough to cast doubt on our estimate.

that SARE is registering existing (bigger) unregistered (informal) firms. The estimates translate into 12 to 19 more jobs and 2 to 5 more firms per municipality per month.

The fact that the estimated effect is very similar for both definitions of eligibility gives us more confidence that municipalities are not selecting which industries to include in their program based on expectations of their growth and that this selection is not driving our results.

The estimated SARE effect seems big if it can be sustained permanently, especially considering that the country's GDP has grown close to 2% from 1988 to 2003. But, is this a permanent increase in the rate of firm creation or just a once and for all shift of the stock of informal firms now being registered? Answering this question would require accurate time series measures of the size of the informal sector at the municipality level.[18] These data do not exist, but we think that looking at the dynamics of the increase in firm registration may provide suggesting evidence. In particular, observing a hump in firm registration after implementation of the program would suggest that a stock of existing informal firms is the main channel of the program's effect. If the program mainly affected "true" firm entry, we would expect the program's effect to be small in the beginning and get stronger over time.

This hump could also be due to the existence of a stock of entrepreneurs who decide to create new firms once registration costs decrease. Although this is possible, we think it is less plausible since we think that the cost of registering features less prominently in the cost benefit analysis of an entrepreneur deciding to open a firm. Another explanation for a hump could be that the program was better publicized when it was first implemented. Although we have no data on advertising, officials tell us that the marketing effort was minimal and we corroborate this with a survey presented in Appendix 7.4

To investigate these dynamics we estimated an specification where the effect of SARE is decomposed in months before and after implementation by interacting the 'SARE effect' term in equation 3 with monthly dummies. We plot these coefficients in Figure 3 along with their 5% confidence intervals.

Figure 3 shows that the effect of SARE is temporary, being more important from the 3rd to the 10th month after its implementation.[19] The relative sizes of the coefficients seem sensible, and more importantly, we observe no clear previous trend before SARE: the coefficients are not statistically different from zero.[20] We would like to stress the

[18] We tried looking at tax collection from small firms but the Mexican Treasury does not have these data at the municipality level.

[19] We have data 10, 20 and 30 months after implementation for 53, 20 and 8 SAREs respectively.

[20] There are several reasons to expect either a decreasing or an increasing trend very close to adoption:

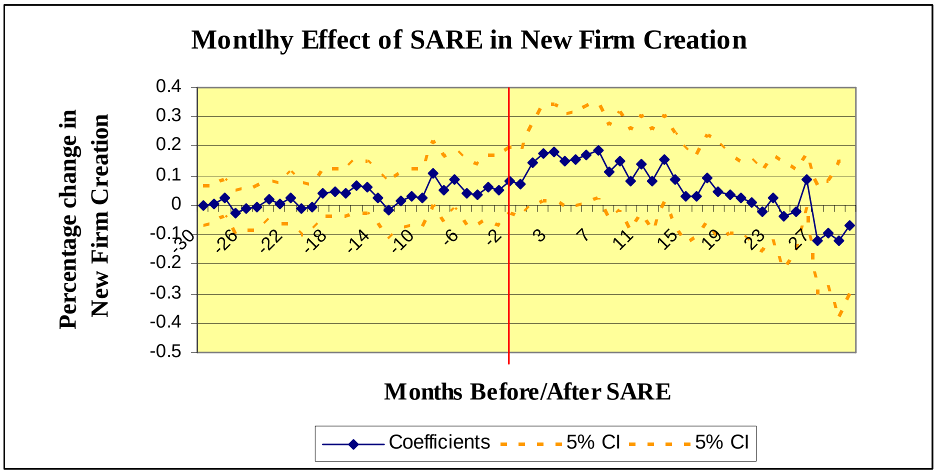

Figure 3: Firm Registration Before and After SARE (Includes only SARE municipalities using the "union" definition)

fact that the non existence of a prior trend is strong evidence that our estimated effect is not the result of a general difference in time trends across industries unrelated to SARE, especially since the program is implemented across all the sample period in different geographical regions.

Although we cannot prove that the main channel through which the program operates is through registering existing informal firms, we believe that the fact that registering firms are bigger after the program is in place, together with the fact that registration increases sharply in the first few months only is very suggestive that this hypothesis is correct.

4.4 Effect on Competition: Price Level

Although we estimate the extra number of registered firms in a SARE municipality to be at most five per month, in this section we inquire if SARE has effects in competition as reflected in the price level. Bruhn (2006) estimated that SARE decreased the price level by approximately 1% using the same price data that we have but including fewer

(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 licences; (c) some potential clients could withhold their application for a few weeks until SARE is operational, thus decreasing registration before SARE.

municipalities. Table 7 reports the estimated SARE effect on the log price level for four regressions: the two columns on the left compare the price level across municipalities before and after SARE using specification 1. The two columns on the right compare changes across eligible vs non-eligible industries using specification 3. [21] Banco de Mexico only reports prices for 46 cities, 29 of which had implemented SARE during our sample period. We report results using both: only SARE municipalities and using all cities which includes non-SARE cities as controls.

Price Level Effects of SARE

	Dif-in-Dif		Dif-in-Dif-in-Diff	
Dep Var: log(CPI)	46 municpalities	29 SARE municipalities	46 municpalities	29 SARE municipalities
After SARE	-0.003 (-0.56)	-0.001 (-0.14)	-0.008 (-1.07)	-0.007 (-1.08)
Number of observations	4,554	2,871	9,108	5,742
R-squared	0.99	0.99	0.97	0.98

Notes: both models use data from January 1998 through March 2006. Models also include dummies for each the 99 months during this period. There is disaggregated price data for 46 cities which include 29 SARE's. Industry classification: Eligible: Food, clothing, shoes, clothing accessories and tailoring, furniture and domestic appliances, cleaning accessories, personal health products, recreation. Non Eligible: Alcoholic drinks and tobacco, cost of housing, electricity and fuels, other services related to housing, health care, public transport, private transport, education.

Table 7: Effect of SARE on Consumer Prices

The results in the four specifications show that SARE had *no* significant effect on prices. We are not certain about why our results differ from Bruhn (2006), but at a minimum we have shown that Bruhn (2006) CPI results are not robust. Part of the reason she may find a negative effect is that prices may adjust downwards during this period when Mexico was undergoing an economic slowdown, which as we showed seemed to hit harder the bigger (SARE) municipalities. She is also restricting her sample to 20 SAREs whereas we are including all 29 of them.[22] Figure 4 decomposes the estimated coefficients by months before and after SARE for the sample that uses only the 29 SARE

[21] We classified 8 industries as eligible for SARE and the remaining 8 as non-eligible, making them as compatible as possible with our previous definition. Table 7 lists our classification

[22] Our classification is as follows: 17 NON SARE's: Acapulco, Acunia, Cordoba, DF, Huatabampo, Iguala Jacona, Jimenez, Matamoros, Monclova, San Andres Tuxtla, Tehuantepec, Tepic, Tlaxcala, Toluca, Tulancingo, Veracruz. 29 SARE's: Aguascalientes, Campeche, Chetumal, Chihuahua, Colima, Cortazar, Cuernavaca, Culiacan, Durango Fresnillo, Guadalajara, Hermosillo, Juarez, La paz, Leon, Merida, Mexicali, Monterrey, Morelia, Torreon, Villahermosa, Oaxaca, Puebla, Queretaro, San Luis, Tampico, Tapachula, Tepatitlan, Tijuana.

municipalities (graphs for the other specifications are qualitatively similar).

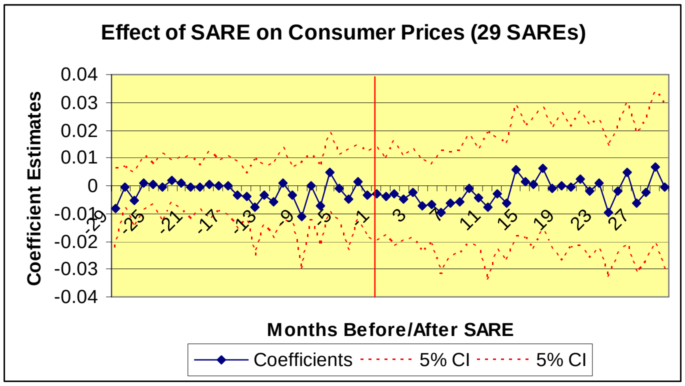

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Figure 4: Effect of SARE on Consumer Prices (Dif-in-Dif-in-Dif coefficients)

Overall, we find no effect on the price level. This is consistent with the small number of registered firms we estimate as SARE's effect.

4.5 Specification Checks

Are we really identifying a causal effect? 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.[23] We would also expect the program to be more effective where it most reduced the time and cost of firm registration. We provide some evidence of these below.

Table 8 shows the estimated "SARE effect" on different dependent variables using specification 3. In the first row of Panel A the dependent variable is the log of job creation *in existing firms*, that is, firms that already had registered employees in the prior month. The estimates show that job creation in already registered firms does *not* increase significantly after SARE implementation. The second row of Panel A estimates the same equation 3 but using only registration of firms with more than 10 employees as the dependent variable. We expect SARE to have a smaller effect when looking at bigger firms since it is less likely that such a firm could be registered through the program. This is indeed what we find: there is not effect on the creation of bigger firms.

[23] Jobs created in continuing firms is about five times bigger than jobs created by new firms. Regarding the contribution to new firm creation by sizes: approximately 85% of new firms are smaller than five employees. Recall that SARE is aimed at small firms: most SARE's have restrictions on the maximum number of square meters of the new locale.

Panel A: SARE's Effect on Different Economic Outcomes

Dependent Variable	Union	Intersection
(1) log (New Jobs in Old Firms)	0.04	0.05
	(1.2)	(1.3)
(2) log (New Firms)	0.003	0.04
# employees > 10	(0.09)	(1.3)

Panel B: SARE's Effect by size of the change brought by SARE*

Dependent Variable	Number of days difference		Number of procedures difference	
	<23	>=23	<3.5	>=3.5
log (New Firms)	0.016	0.03	0.02	0.04
	(0.05)	(0.88)	(0.69)	(1.03)

*Includes only SARE municipality and uses the union definition.

Table 8: Specification Checks: SARE's effect on Big Firm Creation and Jobs on Existing Firms and Effect by Size of change of Practices

Another check that would increase our confidence that the estimated effect is causal would be to test if it is bigger where SARE changed existing practices the most; this is what we do in Panel B. We obtained information from COFEMER on the number of procedures and time it took to register a firm before and after SARE. Information was available for 41 of the 93 SARE municipalities. In these municipalities it took an average of 28 days to register a firm and obtain the operation licence (with a minimum of 2 and a maximum of 60 days). It also took an average of 6 procedures (with a minimum of 1 and a maximum of 7). We divided the sample of 41 municipalities according to the median change in these variables before and after SARE. The median difference of the number of procedures is 3.5 and the median difference of the number of days is 23. We ran a regression to estimate equation 3 for municipalities above the median and another for those below the median for both of our measures. The results appear in Panel B.

The estimates are not statistically significant at the 10% level, maybe due to the much smaller size of the sample, but the size of the SARE effect is twice as big where the burden was reduced more, and the t-statistic is also much higher. This implies that the gap of firm creation in eligible relative to non eligible industries increased more after the program's implementation for those municipalities where SARE had a bigger effect on the burden of regulation.

This evidence increases our confidence that our estimated SARE effect is capturing general economic activity unrelated to the program, since this general economic activity not only would have to happen just after the program is implemented, but also be concentrated on jobs in *new* firms and *not* on jobs in existing firms. It is hard to come up with an alternative story that matches all these facts. Based on this evidence we think that our causal interpretation is supported by the data.

4.6 Additional Controls and Caveats

We believe we have gathered substantial evidence that our estimated effect is causal. Here we show that SARE's effect is robust to the inclusion of monthly measures of economic activity at the municipality level and to the inclusion of different time-industry trends. In particular we will allow for a different set of time trends for municipalities with similar time of adoption, similar percentages of firm creation in eligible vs. non-eligible industries, and similar dependence on trade. In this case identification comes from deviations from these group specific trends after the municipality implements the program. In other words, the counterfactual time trends are allowed to depend on observable municipality characteristics. It will turn out that the estimated effect is robust to which of these different counterfactual time trends we use to benchmark SARE's effect.

Panel A of table 9 presents estimates of SARE's effect for four specifications. We use equation 3 but introduce extra controls as explained below. Our results are unchanged in all of these alternatives.

In specification (1) we included three monthly municipality-industry level regressors: job creation in existing firms, the number of firms that shut down, and average new firm creation in adjacent municipalities.[24] A general decline in economic activity in the municipality or in the neighboring region unrelated to SARE should be captured by these regressors. Additionally we introduce 99 monthly dummies interacted with the eligible industries dummy and also monthly dummies interactions with the "AfterSARE" regressor to allow for a differential effect of economic activity across time. The estimated effect is now 0.033 instead of 0.04.

For specification (2) we allowed those municipalities who implemented the program early (in 2002, 2003 or 2004) to have a different industry time trend than the late adopters (2005 and 2006) by creating two different sets of monthly dummies for these groups and interacting them with an industry eligibility dummy. If the effect of SARE is a spurious

[24]For each SARE we identified its neighboring municipalities and associated to that SARE the average firm creation by month in it neighboring municipalities. SARE municipalities have 4 adjacent municipalities on average.

effect caused by the bigger early adopters having a larger increase in the gap between industries than the later adopters controls, then introducing different benchmark time trends for these should eliminate this effect. It turns out that the effect not only does not disappear, but it is estimated to be bigger.

In specification (3), to 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 allow each quartile group to have its own set of monthly dummies. Finally in specification (4) we introduced a time dummies*industry*trade interaction, effectively allowing time trends of firm creation in each industry type to differ by the level of dollar exports of the state, the percentage of employees working in exporting companies, and the percentage of exports made by maquila. If changes in the industry composition of firm creation is driven by a slowdown of the US economy, then allowing for high trade States to have different trends, or different trends by the previous share of firms in eligible industries, will eliminate the effect attributed to SARE since the timing of implementation per se should not have an impact when measured against the benchmark of municipalities with a similar industry composition. In all specifications the effect is still present.

Ideally we would like to see if its effect is bigger where there is more availability of credit, less tax burden of becoming formal, and higher benefit of being formal through, say, higher importance of backward/forward linkages. Unfortunately there is not much variation in those margins since the same banks with the same policies operate in these SARE municipalities and the value added tax and income tax are set nationally.

What we do in Panel B is to see if the effect of the program is stronger in municipalities that had lower firm registration before 2002 or a higher percentage of informal workers. The first specification estimates the effect of SARE by quartiles of average (1998-2001) firm creation. It shows that the program had a bigger impact on municipalities that had lower pre-SARE firm creation. One explanation for this finding is that these municipalities had low firm creation in the first place because of the high registration burden. Note that this does not square well with the alternative 'decrease in exports' story alluded in Section 4.3 since bigger municipalities are the ones who export the most.

The second specification looks for an interaction of the effect of SARE and the percentage of the working population not enrolled with the labor authorities (in IMSS). We expected to find that SARE's effect is bigger in municipalities with lower enrollment (more informality) but we did not find a significant interaction.

Panel A: Robustness Checks

Specification	Union	Intersection
(1) Additional Controls	0.03*	0.041*
	(1.74)	(1.97)
(2) Separate Industry Trends Interactions for Early and Late Adopters	0.08***	0.09***
	(3.04)	(3.23)
(3) Industry Trends Interacted with Trade Variables	0.07*	0.04*
	(1.7)	(1.68)
(4) Monthly dummies interacted with Industry	.039**	0.54**
	(1.97)	(2.38)

Panel B: SARE's Effect Heterogeneity

(1) By quartiles of New Firms	1st	2nd	3rd	4th
log (New Firms)	0.10*	0.09*	0.0001	-0.01
	(1.86)	(1.74)	(1.36)	(-0.26)

(2) Interacted with the proportion of the population with IMSS

	Main Effect	Interaction
log (New Firms)	0.06*	-0.005
	(2.28)	(-0.55)

*Includes only SARE municipality and uses the union definition.

Table 9: Robustness of SARE's Effect

4.6.1 Caveats

One general criticism of the differences-in-differences methodology is that treatment may affect the non-treated units through general equilibrium effects. In our case general equilibrium effects could lead to both under and over-estimation of the program's effect. For example: a) firm creation in non eligible industries may increase, say, from spill over effects of the eligible industries using inputs from the non-eligible sector, leading to a decrease in the gap of firm creation and thus an underestimate of the SARE's effect; or b) firm creation may decrease in non eligible industries because, by making it easier for firms to register, SARE may creates more competition from close (eligible) products, leading to an overestimation of the effect.

Although we cannot rule out any of the general equilibrium critiques (nor can most published papers) we think that their effects are likely to be negligible. The story in part b) is very unlikely to hold since industry eligibility is defined at the 4 digit level (in the IMSS data) and therefore encompasses almost all kinds of similar industries. This makes it relatively hard to find substitute products in both eligible and non eligible industries.

Against the story in part a), we can only point out that the program is not very big and its effect was estimated to be temporary. Recall that the main eligible industries are in retail and services: it is hard to imagine that having a few more stores will have important backward (supply) linkages spurring the creation of risky manufacturing firms like chemicals or highly polluting industries.

Concerns about the external validity of our estimates are obviously very important, especially given our very limited knowledge of the determinants of informality and the desire to implement these programs in many countries. Our estimates are of course estimates of the Treatment Effect on the Treated, and because we believe that many factors could 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.

5 How Big should the Coefficient be to be Important?

Ideally if the program was easily replicable and if the necessary information was available this section would contain a cost-benefit analysis of SARE. Unfortunately we do not have adequate information to do this. We would not only have to take into account wages of the SARE and COFEMER employees (there are between 100 and 150 of them) but also the opportunity cost of government's attention, which we believe could be substantial. Moreover and more importantly, we would also have to calculate the benefit of having a new firm in the market in the case of new entry and the benefit of having an informal firm become formal in the case the firm already existed in the informal sector. This is indeed a very hard task.

Additionally, even if we could do a cost-benefit analysis, we would still like to have an idea of whether or not programs like SARE can have a substantial effect in the stock of existing firms and on the relative size of the informal sector of a country. In this section, we assess the magnitude of the estimated effect of SARE by comparing it to a) the size of the informal sector that could potentially be incorporated, and b) the expectations and numbers reported by COFEMER as results of SARE. The calculations are quick and dirty, but we believe that they are informative.

To anticipate results, we conclude that SARE has had a relatively modest effect on firm and job creation. This does not imply that the program is not cost effective, nor does it imply that there could not be a substantially bigger effect if the implementation of the program included better advertising and complementary incentives like access to credit and a decreased tax burden for those who register[25]. It does imply however that SARE will not change the relative size of the informal sector in a palpable way.

Since we will argue that the effect of the program in fairly modest, we will use an optimistically large estimate to make our case stronger. We use the biggest point estimate of SARE's effect on jobs created in new firms, which translates to 19 more jobs created in an average municipality (we discussed above that if the estimate is biased it is likely to be upwards). Let us also assume that SARE's effect is constant and lasts for 2 years (also an inflated quantity since SARE has a significant effect only for about 10 months). In this case the number of jobs created by SARE would be 42,408 (=19*93*24). This number is *about 0.2% of the total number of informal employees in SARE municipalities*. We therefore think it is safe to conclude that SARE has not had a significant impact on reducing informality in Mexico.

As a benchmark for the expectations that the Mexican authorities have for this program, we report some statistics that are cited as the results of the SARE program. 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.[26] However these figures include firms that have been registering through SARE that would have registered with the appropriate authorities even in the absence of the program. 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 *5% of the number the authorities report*. Our counterfactual estimate of number of jobs created by new firms is 25,517 (1,343 municipality-months*19 firms per month), which is 13% of the official number. Based on these magnitudes we believe that SARE has had very limited effect on reducing the size of the informal sector, and that its effect is more modest than what many influential deregulation enthusiasts would expect.

6 Conclusion

Policy makers in the last five years have invested considerable effort (202 "Doing Business" reforms in 108 countries were introduced between January 2005 and April 2006) in decreasing the number of procedures and in reducing the time to register a firm. The expectations for these reforms include increased firm registration, decreased

[25] Appendix 3 presents evidence that the program was not well publicized.

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

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 licence can in fact lead to increased firm creation in the formal sector. We also find, however, that the effects of the program we studied were temporary and of a relatively modest magnitude. Since the costs of the program have also been quite low, the program may well have been cost effective. However, we believe the program has not lived up to the lofty expectations of some.

We conclude by mentioning that the small estimated effect may indicate that burdensome regulations may not be the most important barrier to firm creation or firm formalization. To the extent that a poorly functioning tax system or a poorly functioning credit market may also be important, additional (possibly complementary) policies addressing these problems should be considered in addition to reform programs like the one we studied.

7 Appendices

7.1 Appendix 1: Comparison of our paper with Bruhn's paper

Differences in Data: Bruhn (2006) uses publicly available data from Mexico's employment survey (ENE). We use a proprietary census of all registered employees/employers in Mexico which comes from the Social Security agency in Mexico (IMSS). The IMSS data set has several advantages: first, it avoids problems of survey misreporting: not being formally registered is illegal and there is reason to believe that, when surveyed, some people will falsely say they are registered. Second, IMSS data, being a census, does not have issues of representativeness at the level of the municipality; the ENE survey is not designed to be representative at the (municipality) level the SARE program was implemented: different socioeconomic strata are selected from different municipalities. If misreporting is correlated with socioeconomic characteristics then differences in reported business registration across municipalities will partly reflect strata selection. Furthermore, to the extent to which economic conditions affect different strata differently, changes in economic conditions will affect the estimated SARE effect.

Third, instead of using a survey proxy of registration we use registration itself. Fourth, the ENE data is only available up to 2004. This forces the researcher to focus on only 34 municipalities and to look at very short term outcomes of at most one year after implementation. This is problematic since in our paper we estimate the effect of the program to be only temporary and document that the adopters of the first two years are much bigger and have different registration trends than late adopters. In contrast, we have data through March 2006 which enables us to examine the effect in 93 municipalities that adopted SARE in our sample period. For some municipalities we observe outcomes three years after implementation and are thus able to look at longer term outcomes.

The advantage of using ENE data is that it contains information at the worker level, like income and type of work performed, which can serve as controls or outcome variables. Bruhn (2006) is therefore able to directly look at transitions from wage employment to registered business entrepreneurs. She uses these features of the data to test some predictions of her theoretical model and finds that the increase in the fraction of registered businesses comes from previous wage earners and not from existing informal business owners.

Differences in Estimation Strategy: First, since we document that early adopters are different in levels of some important economic variables and different in the trends of employee registration, our main identification strategy is to compare firm registration across eligible and non-eligible industries and avoid relying in direct comparisons of firm registration across municipalities. This is especially important since there was a slow-down of the Mexican economy starting in 2001 which hit bigger municipalities harder. Second, given that selection of municipalities and eligible industries was not random, we perform a detailed analysis of the determinants of the timing of adoption and show that time varying covariates are not related to adoption, and that 'political' variables that were not correlated with firm registration are powerful predictors of implementation. Concerned that changing macroeconomic conditions may be driving the results, we devote a section to specification and robustness checks that supports our causal interpretation: we show that the effect is not present in the creation of bigger firms or in employment growth in existing firms. The effect is robust to controlling for economic activity in neighboring municipalities, job creation in existing firms, firm shutdowns and allowing for different time trends by size of the municipality, time of implementation and dependence on trade. We also experimented with matching estimators but the results are not reported in the paper.

Differences in Results: Both papers conclude that SARE had an effect of 4% to 5% on firm registration; however the effect of Bruhn is on the stock of existing firms, which she translates into an average of 999 new registered establishments per municipality per quarter. In contrast, our estimates translate into 2 to 5 new registered establishments per municipality per month, that is, at most 15 per quarter.

To the extent that Bruhn's findings are dominated by firms that do not have salaried employees, one simple explanation for the differences between our papers is that firms without salaried employees will not register with the Social Security Institute (IMSS). But the difference seems too high to be attributed solely to this. Since the start of the program to June 2006 there have been about 318 SARE-quarters for the 34 municipalities which Bruhn uses. Assuming a constant effect, her estimates imply that 317,682 (=999*318) firms have been registered as a result of SARE. This is 5 times more than the 63,011 firms that COFEMER reports as registration through SARE for those municipalities in that time period[27]. A possible explanation for her big estimate is that during this period of growing unemployment, some previously employed workers became unemployed and declared themselves as self-employed and registered at the time of the ENE survey. This explanation is consistent with Bruhn's result that the only individuals

[27] If we use 10 months as the SARE effect period, this implies that there were 113 SARE-effect quarters, which translates into 113,219 new firms. This is still close to twice the registration that SARE offices had.

who switch to being registered business owners are the ones who previously were wage earners, and not previously non registered businesses.

A second difference is that we examine longer term outcomes and find the effect to be present only in the first 10 months after the program is implemented. Thirdly, Bruhn finds that the effect is driven by wage workers opening firms, whereas we find suggestive evidence that it is the registration of existing firms that is driving the results. Fourthly, we find no effect in the price level, regardless of whether we compare across municipalities with and without SARE, or across eligible vs. non-eligible industries.

Overall we think the papers complement each other: Bruhn (2006) is able to look at more outcomes of SARE and control for individual characteristics. Our paper is able to look at longer term outcomes that incorporate substantially more SARE observations and uses a data set of actual registration instead of survey responses. We conduct a more detailed analysis of the identification assumptions and use an identification strategy that has cleaner pre-SARE trends and is more robust by relying less on municipality comparisons. Furthermore we carry out a survey that points to possible explanations for the lower than expected effect of SARE.

7.2 Appendix 2: A Typical SARE Procedure

Although the details differ across municipalities, a typical [28] SARE procedure is as follows:

1. *Requirements and Application Form:* The user goes to the SARE office and presents the requirements: i) Application Form (Formato Unico de Apertura) indicating name, location of firm, Tax Registration Number (RFC), type of industry, square meter size of the land used, number of employees, and initial investment (the later two only to keep internal statistics); ii) Tax Registration Number[29]; iii) proof of notarization of company's deeds in case it is a limited liability company; iv) proof of address of firm's location; v) proof of ownership; and in some cases: vi) proof of payment of local property tax and vii) proof of zoning restriction compliance

[28] SARE requirements and procedures vary by municipalities. Also municipalities differ widely in the technology they use, for example: some check zoning restrictions electronically in the SARE office while other request this to another office which does it manually looking at paper maps; some have agreements with the treasury to process the Tax Registration Number (RFC) while most take this as a pre-requisite.

[29] Some municipalities do not require it since they have agreements with the Federal Tax Agency that allow them to process it in the SARE office

("uso de suelo").[30]

2. *Eligibility:* The SARE officer checks that the firm satisfies the industry and square meters size restrictions of the program. If it does then it gives the payment form for the "Operation Licence" for the user to pay (most likely in another office XXX). The user typically has 48 hours to make payment and to pick up the license.

3. *Check-ups and Revisions:* Within the next 48 hours the SARE office sends a request to the office of "Urban Development" (Desarrollo Urbano) to check the zoning restrictions that apply in the firm's location. Some SARE offices do this within the SARE office. Also during this time, some municipalities physically send an employee to the registered address to check if the activity of the potential firm coincides with the industry the user listed on the application.[31]

4. *Payment, Awarding of Licence and Ex-post Revisions:* The user comes in the specified time and day with the proof of payment, receives the "Licence of Operation", and schedules revision visits with "Civil Protection", "Ecology" and "Health" municipal agencies that check that minimum public safety (fire extinguishers, emergency exits, etc.) and hygiene standards are met. A list of potential fines in case of non-compliance with the standards is given as well, and the client signs an agreement letter.

7.3 Appendix 3: Additional Graphs and Tables

The following figure and table provide evidence that early SARE adopters are somewhat different than late adopters. Figure 5 presents trends of firm creation by year of adoption group. Table 10 shows means for selected variables by year of adoption group:

[30] In many municipalities the latter is not a requirement but part of the SARE office verification procedures.

[31] It is believed by some COFEMER officials that this is the process where most bribe extraction happens and the less useful one since firms can disguise the activity of the firm in this pre-opening stage.

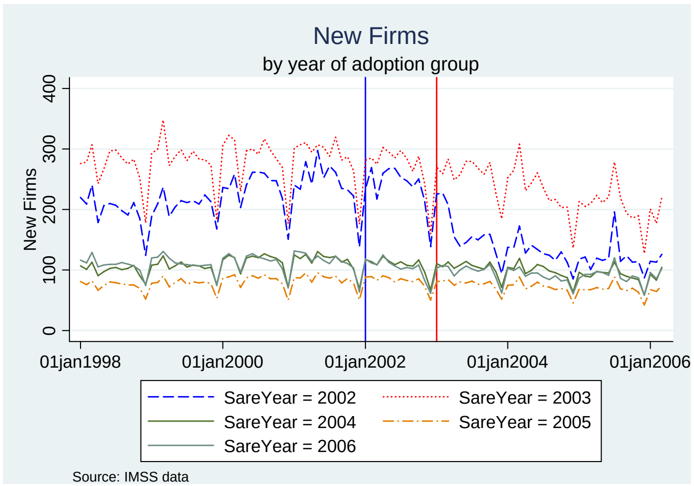

Figure 5: New Firm Creation in SARE Municipalities by year of Adoption

Means of Selected Variables by Year of SARE implementation

Variable	**2002**	**2003**	**2004**	**2005**	**2006**
Employment (registered) ***	119,968	176,484	44,251	45,175	63,128
Monthly New firms***	220	280	208	79	94
Firms that Stay***	7,702	10,109	3,205	2,593	2,959
Population**	2,904,770	3,197,003	1,218,338	963,112	1,182,377
Production*	3,893	5,737	1,949	1,941	1,888
Municipalities Implementing SARE	2	8	28	47	8

*As reported in the economic census 2004 (millons of 2004 dollars); ** From the Population Census 2000; *** From our IMSS dataset (averages 1998-2001)

Table 10: Means of Selected Variables by Year of SARE implementation

Table 11 shows the names and industry eligibility status of the 30 biggest IMSS industries in terms of firm creation during our sample period. It gives an idea of the level of aggregation with which IMSS classifies industries.

The 30 biggest* IMSS industries by Eligiblity Status

Industry Description	Intersection	Union	Non Eligible
Construction of building (except public works)	0	0	1
Profesional and technical services	1	1	0
Preparation of food and food services	1	1	0
Retail stores of food and brevages	0	1	0
Mechanic, services of repair of vehicles	0	0	1
Load transport	0	1	0
Retail stores of clothes and clothing accessories	0	1	0
Passenger transport	0	1	0
Plumbing, electricity and air conditioning instalations	0	1	0
Medical services	0	0	0
Other services of remodeling, instalation or finishings of construction	0	1	0
Purchase/Sales of material for construction	0	1	0
Agriculture	0	0	1
Manufacturing of products using cereals	0	1	0
Retail stores of personal use items	1	1	0
Academic, training and cultural difusion services	1	1	0
Purchase/Sale of food, drinks (with transport)	1	1	0
Beauty saloons and hairdressers	1	1	0
Editoral, printing, bookbinding industries and connected activities	0	1	0
Manufacturing of wooden furniture and their parts	0	1	0
Purchase/Sale of food, drinks (with transport)	1	1	0
Purchase/Sale of computing equipment, with instalation	0	1	0
Manufacturing of doors, windows, ironworks	0	0	1
Instalation of windows, ironworks, and glass	0	1	0
Manufacturing of metalic products using machinery	0	0	1
Purchase/Sales of transport equipment, parts, accessories.	1	1	0
Cleaning services with motorized machinery	0	1	0
Clothing made to measure	1	1	0
Purchase/Sales of construction material, no transport	0	1	0
Retail stores of paper, stationer's shop, office supplies	1	1	0

* industries with the most firm registration in our sample period

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Table 11: The 30 industries with more firm creation and their eligibility status

7.4 Appendix 4: Survey of Lawyers in Mexico

As a part of the local level "The Doing Business Report" in Mexico, the World Bank conducted surveys at 31 different cities in Mexico. The surveys were sent to an average of 3 law firms and one government official per city. We were permitted to include 6 questions about SARE in those surveys which intended to measure if the program was well known, and the changes in the efficiency of practices and cost to register a firm due to SARE.[32] Only 27 of the surveyed cities had SARE at the time of the survey.

The main results were as follows: 40% of the lawyers interviewed in SARE municipalities did not know about the existence of the program in their municipality.[33] The correlation between knowing that SARE exist in the municipality and years since implementation is -0.01 and it is not significantly different from zero. This can be interpreted as evidence that publicity is not likely to be higher during implementation and thus may not be the reason we observe a hump in firm creation after implementation.

Regarding changes in practices: the number of procedures decreased from 6.8 to 3.6 and the number of days from 31 to 2, consistent with what COFEMER reports. The cost decreased on average from 470 dollars to 360 dollars.[34]

Our conclusion from this small survey is that SARE is not well known. Thus, it may be that SARE has a small effect compared to the size of the informal sector because it has not been adequately publicized in the municipalities. Some municipality officials we surveyed also felt that a better marketing campaign could be very useful.

[32] The survey questions and results are available upon request.

[33] If this was entirely due to a bad governmental marketing campaign we should tend to see that either all lawyers know or all of them do not. This is not what we find: for in 10% of the municipalities no lawyer knew, for 12% one out of 3 knew, for 52% of the municipalities 2 out of 3 lawyers knew and in 26% of the municipalities with SARE all lawyers knew.

[34] If lawyers differed on their report within a municipality we used the median.

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