Entry Regulation and Business Start-ups: Evidence from Mexico

David S. Kaplan, Eduardo Piedra and Enrique Seira *

June 2007

Abstract

We estimate the effect on business start-ups of a program that significantly speeds up firm registration procedures. The program was implemented in Mexico in different municipalities at different dates. Our estimates suggest that new start-ups increased by about 4% in eligible industries, and we present evidence that this is a causal effect. Most of the effect is temporary, concentrated in the first 10 months after implementation. The effect is robust to several specifications of the benchmark control group time trends. We find that the program was more effective in municipalities with less corruption and cheaper additional procedures.

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

*We would like to thank Fernando Salas who made us aware of the existence of SARE. Jon Levin and Susan Athey have provided invaluable advice and comments throughout all of the research process. We have also benefited from discussions with Rodrigo Barros, Daniel Bautista, Gustavo Bello, Tim Bresnahan, Ariel Buira, Mierta Capaul, Giacomo DiGiorgi, Liran Einav, Pablo Fajnzylber, Carlos Garcia, Francisco Gil, Alvaro Gonzalez, Jose Antonio Gonzalez, Emeric Henry, Jesus Hurtado, Saumitra Jha, Laura Lombardi, Ernesto Lopez-Cordoba, Bill Maloney, Caralee McLeish, Pedro Miranda, Sriniketh Nagavarapu, Alejandro Ponce, Rita Ramhalo, Aldo Sanchez, Alejandrina Salcedo, Siddharth Sharma, Rosa Maria Vega and Alejandro Werner. SIEPR at Stanford University provided financial support. Enrique Seira: Stanford eseira@stanford.edu, David S. Kaplan: ITAM kaplan@itam.mx, Eduardo Piedra: UT Austin e.piedra-gonzalez@mail.utexas.edu.

1 Introduction

Firm creation has been believed to be an important channel of GDP growth at least since Joseph Schumpeter. In addition to expanding the range of products, entry can create more competition, lower prices for consumers, and may lead to better technology adoption. 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.

The ability to start a 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.[1]

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 in most countries, legal entry is extremely cumbersome, time-consuming, and expensive. 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 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 in firm start-up costs lead to a permanent increase in firm start-ups? Second, does this effect operate through the registration of existing informal firms or through the creation of truly new firms?

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

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)).

The magnitude of the effect of lower registration costs on firm start-ups is therefore 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)).[2] 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 2006 p.13 Doing Business Report "following (firm registration) reform, new entry jumped by 28% in Vietnam, 22% in Romania and 16% in Belgium" and 42% in Serbia and Montenegro."

To estimate the magnitude of the effect of reducing registration procedures we use variation induced by the implementation of a "deregulation" program (SARE) that took place in Mexico in different locations at different time periods. This program instituted 'one-stop' firm registration offices in some municipalities. These firm registration offices allowed small firms that operate in eligible industries to obtain a 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. *The decrease in the delay brought by SARE is equivalent to the difference in delay between Jamaica vs Canada* (according to Djankov et al. (2002)).

Although SARE significantly reduces the time to obtain the business license, SARE does not affect other potentially burdensome procedures. For example, an entrepreneur typically must already have registered with the tax authorities prior to beginning the process. Further, SARE does not affect the various inspections to which formal firms are

[2] 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.

subjected. It may be that firms are particularly vulnerable to the solicitation of bribes during these inspections.

The fact that SARE represents a significant, but less than complete reform of the business climate makes SARE an interesting program to evaluate. The reforms cited in the Doing Business reports have been much more comprehensive and apparently have had dramatic effects. Since we find a much more modest effect on firm registration for the SARE program, one would conjecture that reforms of the business climate are indeed complementary and must be implemented together in order to realize benefits like the ones documented in the Doing Business reports. In fact, we present direct evidence that SARE's effect on firm creation has been bigger in municipalities that appear otherwise to have healthier business climates.

Although the timing of introduction of the program and the industries to which it applied was not random, we provide some evidence that implementation was not related to time varying covariates or lagged outcomes. Instead, political affiliation and the year of tenure of the municipality major are significant predictors of adoption. 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. To our knowledge no program of the type we study has been implemented randomly across cities or regions, probably because a randomized implementation is likely to be politically infeasible. It is therefore likely that evidence on the effectiveness of these programs will always come from non-randomized implementations like the one we study.

In our analysis, we will use two different identification strategies: the first compares firm start-ups 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 may be 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 the program in bigger municipalities first and macroeconomic conditions affect them differently.

To avoid relying entirely on cross municipality comparisons our preferred estimation strategy compares the time trend of 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 start-ups. 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 smaller 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. The size of our estimated effect is only about one quarter of the effect that the Doing Business Report (2006 p.13) documents and about 5% of what the Mexican authorities report as outcome of the program. We offer some conjectures as to why the effect has been modest. In particular, we offer evidence that other barriers to firm creation limit the effectiveness of the deregulation program we study.

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. (2006a) 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. We believe that 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. Due to the numerous differences between the two papers, we view our papers as complementary.

The structure of the paper is as follows. Section 2 will describe the program we study and the setting in which it was implemented. Section 3 will describe our data sources and our outcome variables. Section 4 will describe our two empirical strategies and analyze some determinants of program implementation. Our main results, specification and robustness checks are in Sections 5 and 6. Section 7 presents some conjectures on why the program has not been as effective as expected. Section 8 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 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 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 after filing with the municipality's SARE office. It aims to achieve this objective by consolidating Federal, State and Municipal[3] 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, 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. 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 happened since the program's inception.

It is important to note that not all firms can register and obtain a licence through

[3]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.

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. Some examples of eligible industries include: production of metal and wooden furniture, freezing of fruits and vegetables, production of clothes and textiles, drugstores and small supermarkets, video stores and DVD rentals, real estate services, etc. Examples of non-eligible industries include: bars, production of rubber products, hospitals, production of machinery, etc.[4] 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. There were no other government programs being implemented with a similar location-time profile, that is, there were no government induced policies whose effects we could be attributing to SARE.

2.3 Implementation of SARE

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

[4]For a detailed list please consult COFEMER's web page at http://www.cofemer.gob.mx/portal.asp?seleccionID=22&padreID=10&hijoID$\bar{6}$5#fisicas.

uary 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*	1	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 during our sample period. 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 implemented later.

Large municipalities were explicitly targeted for early program adoption. Table 2 presents summary statistics of Mexican municipalities for a partition of four non-intersecting groups: municipalities with SARE in our sample period, "Competitive" non SARE municipalities, 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. It should be noted that Mexico City did not adopt the program during our sample period.

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.

Means and Std. Dev. by Type of Municipality (monthly averages)

Variable	SARE	Competitive	Adjacent	Other
Employment***	61,450	13,213	1,446	1,420
	(85,672)	(23,578)	(3,040)	(4,019)
Monthly New jobs by new firms***	409	99	16	15
	(478)	(188)	(49)	(71)
Monthly New firms (mean)***	111	26	4	4
	(118)	(48)	(9)	(11)
Non-Exiting Firms***	3,620	786	131	117
	(4,367)	(1,455)	(248)	(285)
Population**	1,332,588	504,958	120,846	101,024
	(1,356,585)	(829,175)	(108,289)	(115,520)
% of Working Population with IMSS**	10%	7%	3%	3%
	(5%)	(8%)	(4%)	(7%)
% Workers in Terciary Sector**	54%	53%	34%	35%
	(13%)	(12%)	(10%)	(13%)
Number of Establishments*	11,518	4,089	751	685
	(12,573)	(6,760)	(873)	(1,019)
Production*	$2,388	$846	$100	$59
	($3,071)	($2,346)	($487)	($296)
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). Means with Standard Deviations in parenthesis.

Table 2: Summary Statistics by Type of Municipality

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 may explain the within State clustering of implementation, although regional shocks could also be an explanation. This convincing appears to have been more effective for municipality mayors who belonged to the same party as the President, those who were in the middle of their term, and those from a state where other municipalities were implementing the program.

In the first three years of implementation more than 70% of the municipalities were from the President's party (PAN) at the moment of implementation. While in our whole population of municipalities only about 25.4% of the municipalities are governed by this party.[5] In our sample period more than 50% of municipalities implement in the mayor's

[5]We counted the number of municipality-months that each party was in power from January 2002 to March 2006. The shares are PAN=25.4%, PRI=53.3%, PRD=16.7%, others=5%. These shares are similar when we look within years.

second year of tenure (municipality mayors have three year terms), and we find that there is clustering of implementation within a State.

Since municipalities are autonomous entities, implementation of the program is largely a political issue. This means that it is extremely difficult randomize implementation with the purpose of evaluation, but more importantly it could imply that implementation is not correlated with the outcomes the program is intended to change. If this is indeed the case then our estimates of the program's effect would be unbiased. Section 4.1 performs an analysis of the determinants of the timing of adoption and confirms that most of the political determinants mentioned above are significant predictions of program adoption, 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).[6]

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). 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 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,

[6]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.

but rather the number of new formally registered 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.[7] 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. FootnoteMeasurementError discusses some concerns with this extrapolation. 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 the Appendix 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 another 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

[7] 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.

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 $\max\left(0, empl_{jt} - empl_{jt-1}\right)$ for establishments in which $empl_{jt-1} > 0$.

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.

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.[8] Alternatively we could use non-eligible industries as controls for eligible industries and compare the difference of firm creation within municipalities across the two sets of industries. In both cases we assume that firm creation in the control municipalities (industries) are good approximations to what would have happened without the program in SARE municipalities (eligible industries). Unfortunately the counterfactual identification assumptions used are inherently not testable. We will use a series of checks to increase our confidence that our identification assumptions are reasonable and that our estimates are close to SARE's true causal effect.

First, in section 4.1 we will show that there is no evidence that municipalities that adopted the program do so because of changes in time varying covariates or lagged outcomes. Instead we will show that political variables are more important determinants of

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

adoption. This result is important since, as long as the political variables are not correlated with the trends of firm creation, it is 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 municipalities (section 5) and comparing across industries (section 6). 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 6.2.

4.1 Where is SARE implemented?

If factors affecting the time trends of firm creation are correlated with variables affecting the decision to adopt the program, then it is likely that adopting municipalities would have had a different trend in new firm creation 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. A similar 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, and this change is different for early and late SARE adopters, then our estimate of the causal effect will not be consistent.

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. 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) as well as the mayor's tenure at the time of adoption (the excluded categories are other parties and coalitions and the first year of tenure, respectively); 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 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	1.008	
(MA12, detrended)	(0.45)	
Job Creation	1.0006	
(MA12, detrended)	(1.23)	
New Firm Creation in Eligible		1.01
Industries (MA12, detrended)		(0.53)
New Firm Creation in Non-Eligible		0.97
Industries (MA12, detrended)		(-0.62)
Job Creation in Eligible Industries		1.002
(MA12, detrended)		(1.18)
Job Creation in non-Eligible Industries		0.99
(MA12, detrended)		(-0.53)
Total Population (thousands)	1.001	1.002
	(0.68)	(1.31)
Production	1.00001	1.00004
($dollars per capita)	(0.14)	(0.39)
Unemployment (2000 cenus)	1.26	1.20
	(0.85)	(0.66)
Working Age Population (thousands)	0.99	0.99
	(-0.38)	(-1.02)
% Employees in Tertiary Sector	1.09	1.04
	(0.14)	(0.07)
% Working Age Registered at IMSS	1.17	0.96
	(0.80)	(-0.16)
Log (State Exports)	0.74	0.75
	(-1.44)	(-1.34)
% Employment in Exporting Firms	0.10	0.19
	(-0.84)	(-0.58)
% Workers receiving no income	0.24	0.28
	(-0.62)	(-0.54)
Municipality government revenues	1.004	1.004
($ millions of dollars)	(1.60)	(1.39)

Coefficients are reported in exponentiated form.

Table 3: Where is SARE implemented

The results are consistent with what COFEMER officials told us: mayors in their second year of tenure and those who belong to the party of the President are significantly more likely to adopt the program 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.[9]

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.

There is no strong evidence that locations governed by certain parties where growing faster on average. Political variables are not correlated with firm or job creation the before SARE period: the raw correlation coefficients between firm creation and party dummies are: PAN=0.21, PRI=-0.12, PRD=-0.02 and Others=-0.02. So, while it is true that on average PAN governs municipalities with slightly more firm creation, these municipalities where not growing any faster on average: when we control for municipality fixed effects, the difference in the time trends of firm creation across municipalities governed by different parties are not statistically different from zero. Regarding tenure of the mayor in the job: there is no reason why to expect firm creation to be related to it. This is what we find in the data: the correlation coefficients of firm creation and dummies for year of tenure in the before-SARE period are: 1st year=0.0003, 2nd year=0.001, 3rd year=0.0002. Overall this evidence gives us some confidence that trends in firm start-ups were not correlated with program adoption, and therefore that the identification strategy that we use is less likely to be flawed.

5 SARE's Effect: 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 these two groups of municipalities differ significantly in the level of firm start-ups, it turns out that we can not

[9] The fact that variables such as political party and number of years in office are significant predictors of adoption suggest a possible instrumental-variables approach to estimating the effect of SARE. We indeed have tried this approach using political party and years in office, interacted with cubic time trends, as instruments. Although the instruments were significant in the first stage, the SARE coefficient was always insignificant in the second stage. We never rejected the null hypothesis that SARE adoption was exogenous.

statistically reject them following similar time trends of firm start-up before SARE was in place. We estimate regressions where the dependent variable is firm creation in eligible industries or the difference in firm creation between eligible and non eligible industries, and where explanatory variables include 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, after the program began to be implemented in 2002, a slowdown of the Mexican economy was underway. According to the Mexico's Central Bank this happened partly as a result of the US recession (Banxico (n.d.)). This deceleration was stronger in big municipalities (measured by production or population).[10] Mexico has no time series data at the municipality level, which makes it hard to adequately control directly for the economic trends which are unrelated to SARE. However, we try a battery of controls in the regressions below.

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.

To anticipate results a bit, 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 that 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.

[10]To substantiate this claim we used the Hodrick-Prescott decomposition and plotted the cyclical components of new firm creation for early ($\leq$2004) vs. late ($>$2004) adopters. It shows clearly that the early adopters have stronger cycles. The raw coefficients of variation for new firm creation in the pre-SARE period are 1.06 vs. 0.93 for early vs late adopters respectively, implying that variation with respect to the mean is higher for early adopters. The analogous quantities for total employment are 0.81 vs. 0.65.

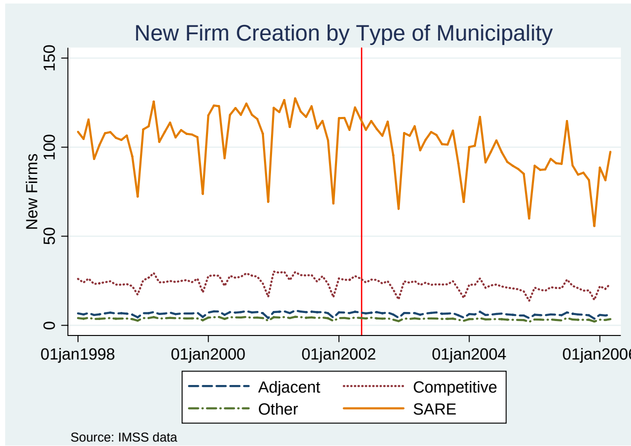

Figure 1: Mean Trends of Monthly Firm Creation by Type of Municipality

In order to understand the intuition behind the identification strategy, let Y_{it} be the log of one plus the number of new firms in eligible industries, where $i = 1$ if the municipality implements SARE and zero otherwise.[11] 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.[12] 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 over 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 munic-

[11]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.

[12]Since we have monthly data, if a SARE is operational in the middle of a month, our $AfterSARE_{it}$ variable only equals one the next month. Thus, for municipalities which adopt at the beginning of the month, SARE's effect may start a few days before being captured by our variable.

ipalities 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).

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 Eligible Firm Creation Across Municipalities: β coefficients

To save on space Table 4 only presents the estimated β coefficient of six specifications (rows) using two different sets of control municipalities (columns).[13] 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} includes measures of the importance of exports and 'maquila' 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 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} includes 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

[13]Full regression results are available from the authors upon request.

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

groups of municipalities that adopted SARE early (before 2004) or late (on or 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.

6 SARE's Effect: 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 and 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:[15]

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

The dependent variable Y_{itk} is either the log of one plus the number of new firms or the log of one plus the number of jobs created by new firms; i=1 if the municipality i has SARE and is zero if the municipality is a control municipality; t indicates the time after

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

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

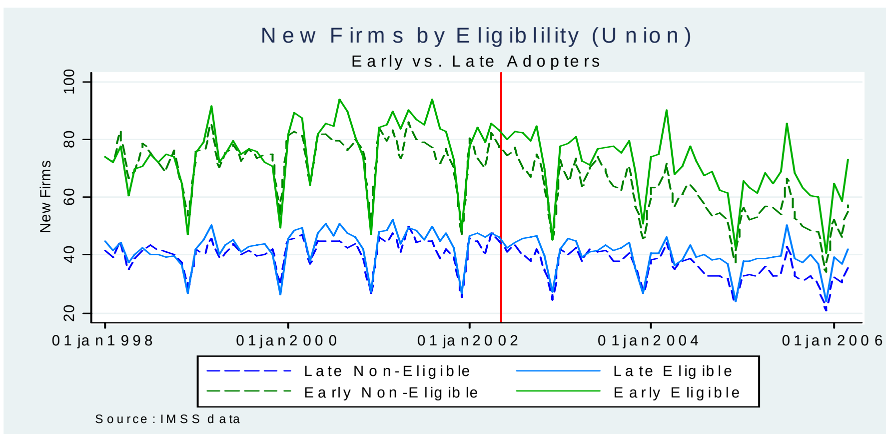

Figure 2: Mean Trends of Firm Creation by Eligibility for SARE and Comptetitive Municipalities. "Union" definition of eligibility is used

Note that this identification assumption is weaker than the assumption used in the simple differences-in-differences analysis, since the triple difference approach is robust to municipality-specific time-varying shocks, as long as they impact eligible and non-eligible industries uniformly. 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 in which this can happen. The best story we could think about involves a) SARE municipalities being especially dependent on 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 6.2 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 biases our results. 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 Employment	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 Employment	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 in the current 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:

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

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 coefficients α_{ik} are fixed effects for each municipality-industry pair. The coefficients γ_t are fixed effects for each month. The remaining regressors are the second order interactions between industry, municipality and time, and some controls.

The parameter ϕ captures the shift of the outcome variable which is common for both types of industries for SARE municipalities after SARE is implemented; this regressor is key to control for the decreasing trend of firm creation in SARE municipalities which 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 6.3, in this section we do not include X_{it} in the regressions.

In Table 6 the estimations will be done for four samples defined by whether the control group includes Competitive municipalities or not, and by the definition of industry eligibility used.[16] To save on space we will only report the estimates for the effect of the program, although the full regression results are available upon request.

Panel A of Table 6 shows eight β estimates from equation 3. The columns define the group used as a control for each of the two definitions of industry eligibility; the rows present the results for different dependent variables: new firms and new jobs in those new firms. 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]

[16] Since we were not able to obtain the list of eligible industries for all municipalities, we were forced to extrapolate to 93 municipalities using the lists of 31 of them. This may induce a measurement error problem which could bias the estimated effect downwards. Since we were assured by COFEMER that municipalities within a State tend to copy the industry eligibility lists of each other, we ran the same regressions extrapolating the "union" definition within each State. The 31 municipalities with lists cover 16 States, which in turn have 47 SARE municipalities. We only used these municipalities in the regressions and the resulting coefficients are quite similar to those reported in the tables. Since we are using fewer municipalities, however, the p-values rise to about 0.19. We take the fact that the estimated coefficients are similar using this state-wide extrapolation method as some evidence against a measurement error problem. However given the smaller statistical significance, we cannot confidently discard the possibility of a downward bias of our estimates due to measurement error.

[17] We found a statistically significant coefficient (not shown in table) on the first order term of the

SARE's Differential Effect on Eligible Industries

Panel A: Start-ups and New Jobs	Only SARE's[a]		SARE's and Competitive[b]	
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)
Panel B: Firm Survival (Cox model)	Only SARE's[c]		SARE's and Competitive[d]	
Coefficient Estimates	Union	Intersection	Union	Intersection
Including size measure	-0.33***	-0.25***	-0.24***	-0.21***
	(-4.48)	(-3.23)	(-2.98)	(-3.42)

(a) 93 Municipalities, 99 months; 9207 obs; (b) 239 municipalities, 99 months = 23661 obs. Errors clustered at the municipality level. (c) Sample: 50,000 new firms in SARE municipalities, 99 months, time at risk: 966,415 months. We report results for the estimated coefficients. (d) 30,000 randomly selected new firms from SARE and competitive municipalities, 99 months. TIme at risk is 584,817.

Table 6: SARE's effect

The effect on job creation by new registered firms is generally 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*. The estimates translate into 12 to 19 more jobs and 2 to 5 more firms per municipality per month.[18]

In Panel B of Table 6 we report an estimate of the effect of SARE on the length of life of start-up firms. To do this we have to work with firm-level information and the computational burden is high. For the models in which only the SARE municipalities are included, we extracted a simple random sample of 50,000 new firms out of the 925,002 new firms that registered with IMSS during our sample period in SARE municipalities (2,073,503 registered in all municipalities).

The mean life of a firm in our sample is 19 months and 37,215 of the 50,000 start-up firms exit during our sample period. The hazard of exit is decreasing for both eligible and non eligible industries as shown in Figure 4 in the Appendix, confirming in our sample a well established fact in the economic's literature (see for example Dunne, Roberts and Samuelson (1988)): older firms are less likely to exit even conditional on having reached that age.

For the hazard models in which we include SARE municipalities and competitive munic-

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.

[18] To give a sense of the rest of the coefficients we report them here along with their standard errors for the regression of new firms in "only SARE's" for the union definition, although their magnitudes are consistent across specifications: ϕ = -.09 (-3.55); θ =-.37 (-15.26); the linear, quadratic and cube terms of σ are the following respectively: .004 (2.24), -.00006 (-1.52), 4.730e-07 (1.70).

ipalities, we extracted a simple random sample of 30,000 start-up firms from a population of 1,284,920. We used fewer firms because the hazard models needed to include more than twice as many municipality dummies. The descriptive statistics for these 30,000 start-up firms are similar to those reported in the previous paragraph.

If SARE's main effect is through the formalization of already existing firms, we would expect that the probability of firm survival in eligible industries changes after SARE is implemented. In particular, we would expect that they live longer after SARE is implemented since the apparently new firms are really older firms that have now decided to register with the Social Security. Since older firms have lower exit hazards, the "new" firms formalized as a result of SARE would have exit hazard rates closer to those of older firms, and therefore live longer than the typical newly-registered firm. However a finding like this one will by no means show that the registration increase is driven by registering informal firms.

We estimated a Cox duration model with proportional hazard $h(t) = h_0(t) \cdot exp(X'\pi)$. The specification we use for the regressors $X'\pi$ is given by equation 3. Again our coefficient if interest is β: it measures the relative change in the hazard of exit of eligible vs. non eligible industries after SARE is implemented. Since we have already shown that new start-ups have more employees, an additional control variable included in the duration model is the number of workers in the firm in each month, in this sense we are looking at the effect on SARE conditional on firm size. Again we cluster standard errors at the municipality level.

The coefficient estimate for the union definition, using only SARE municipalities, implies that after SARE is implemented, new start-up eligible firms increase their lives by 28% ([exp(-0.33)-1]*100) relative to their non-eligible counterparts (22% for the intersection definition). We interpret this evidence as being consistent with the hypothesis that at least some of SARE's effect works through the formalization of firms that had already been operating prior to the program's implementation. The results are similar when using the 30,000 firm sample that includes non-SARE competitive municipalities as controls. The point estimates, presented in the right side of Panel B, are a bit smaller but the qualitative result is unchanged. Once again, we interpret the fact that post-SARE start ups in eligible industries tend to live longer as evidence that these start ups were really existing firms that had been operating formally.

The fact that the estimated effects are very similar for both definitions of eligibility, both for the models of firm creation and for the duration models, 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.

We now return to the estimates on firms creation. The estimated effect of SARE on firm creation 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. These data do not exist and we are therefore unable to answer this important question.

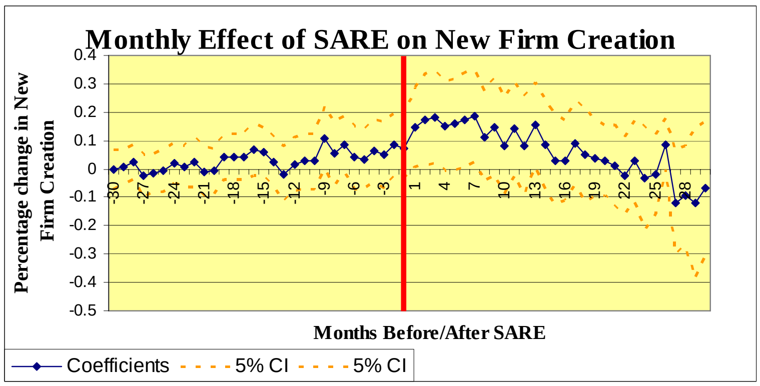

Figure 3: Firm Registration Before and After SARE (Includes only SARE municipalities using the "union" definition)

Figure 3 investigates the dynamics of firm registration. 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. One can see that the effect of SARE is temporary, being more important from the 3rd to the 10th month after its implementation. 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.[19] We would like to stress the 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.

There are many potential explanations for this temporary increase in firm registration. The following is a partial list of these explanations: a) an existing stock of existing informal firms or entrepreneurs decide to register or create new firms once registration costs decrease,

[19]There are several reasons to expect either a decreasing or an increasing trend very close to adoption: (a) in the two or three months before implementation COFEMER officials are around evaluating the municipality procedures and we should expect an increase in speed of registration and a reduction on the backlog and thus an increase in registration; (b) there could be some media coverage of the fact that it is easier to register firms, thus increasing the demand for formal licences; (c) some potential clients could withhold their application for a few weeks until SARE is operational, thus decreasing registration before SARE.

and this shows up as an jump in the flow of registration. b) as new firms enter and rents are competed away the rate of entry slows down. c) 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. d) The effect is driven by the changing set of municipalities acting as "controls" as time passes: we have data 10, 20 and 30 months after implementation for 53, 20 and 8 SAREs respectively.

6.1 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 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.

Price Level Effects of SARE

	Dif-in-Dif		Dif-in-Dif-in-Diff	
Dep Var: log(CPI)	46 municipalities	29 SARE municipalities	46 municipalities	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 two columns on the right compare changes across eligible vs non-eligible industries using specification 3.[20] 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. The results in the four specifications show that SARE had *no* significant effect on prices.

[20]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.

6.2 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.[21] 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.

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

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 start-up 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: no effect.

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

[21] 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.

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 not 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.

6.3 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.[22] 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 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 are 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.

What we do in Panel B is to see if the effect of the program is stronger in municipalities that had lower firm start-ups before 2002 or a higher percentage of informal workers, and whether the effect is different for "competitive" municipalities. 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 6 since bigger municipalities

[22]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.

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**	0.054**
	(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)

(3) Separate regressions for Competitive and Non-Competitive SARE municipalities

	Competitive	Non- Competitive
log (New Firms)	0.042*	0.035
	(1.73)	(0.94)

Regresions include only SARE municipalities and uses the union definition. There are 62 competitive SARE municipalties are and 31 non-competitive SAREs.

Table 9: Robustness of SARE's Effect

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 (that is, registered with 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.

The third specification looks at the effect of SARE separately for competitive vs. non-competitive municipalities. Recall that competitive municipalities where explicitly invited by the federal government whereas non-competitive municipalities were not. In section 4.1 we argued and showed some evidence that the selection of municipalities by the federal government and the timing of SARE adoption is likely to be uncorrelated to the outcome variable since it was driven mostly by political reasons. If this is true then we would expect that the effect of SARE to be similar for competitive and non competitive municipalities. This is indeed what we find: the effect is 4.2% in competitive municipalities and 3.5% in non-competitive ones. However in the sample of non competitive SARE adopters, which contains only 31 of them, the effect is not statistically significant at conventional levels.

6.3.1 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 new firm start-ups and the desire to implement these programs in many countries. Our estimates are of course estimates of the Treatment Effect on the Treated, 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.

7 When is SARE More Effective?

The estimated effect of SARE is much smaller than the 20% increase in start-ups that the Doing Business Report documents for other countries who have done reforms along the lines of SARE.[23] It is also much smaller than what the Mexican government reports. According to COFEMER, 75,168 new firms, 194,577 new jobs, and around 1,200 million dollars of new investment had been created through SARE as of July 2006.[24] According

[23] Their results are based only on a before/after comparisons and does not control for time trends.

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

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.[25]

It is hard to argue that there has not been enough time for these reforms to have their effect. The data captures firm start-ups up to almost four years after implementation for some municipalities. This should be enough time to see an effect. More importantly, the effect we estimate shows up immediately and only last for about a year. The interesting question thus is why is the effect smaller than expected? In this section we provide some informed discussion about it and attempt to shed some light on this important issue.

While it is true that SARE significantly reduced the time and number of municipal procedures. Recall that the time reduction caused by SARE is on average 28 days which, as mentioned earlier, is **comparable to the difference in delay between Jamaica and Canada**. However it is still the case that some important procedures must be performed outside the SARE offices. [26]

The Doing Business Group conducted a survey in 32 Mexican municipalities in 2006 where the time and cost of the above procedures was quantified; 26 of these municipalities adopted SARE during our sample period. They sent surveys to an average of three law firms per city and the city government in which the time and cost of these and other procedures was ascertained.[27] It turns out that the cost and timeliness to complete these procedures excluding obtaining an operation licence (SARE's procedure) is not negligible and varies widely across cities: the average cost is around $980 dollars with a standard deviation of $390; they take on average 23 days to complete with a standard deviation of 11. Variation in the number of cost and days to perform these procedures is therefore non negligible. Although there are additional procedures which SARE left untouched, we want to emphasize that SARE *did* reduce the administrative burden of registration to a great

[25]Back of the envelope calculations reported in previous versions of the paper suggest that SARE would only decrease by 0.2% the size of the informal sector.

[26]Before going to SARE the entrepreneur has to 1) register the name of the company with the Ministry of Foreign Affairs, 2) legalize the company's statutes (in case it is a limited liability company), 3) register statutes with the public registry of commerce, 4) register with the Federal Taxpayers Registry (RFC). After obtaining the operation licence with SARE the entrepreneur still has to 5) register with the Social Security (IMSS); 6) register to pay payroll tax, 7) register with the System of Business Information (SIEM) and 8) register with the Mexican Statistical Institute (INEGI).

[27]The survey requests its respondents to answer questions with respect to a particular kind of model company: a domestically owned limited liability company with 5 owners and start up capital of 10 time the GDP of the respective state. To the extent that costs are similar for different types of companies within a city this information should be useful. For more information on their methodology, data and results of the survey consult the World Bank's Doing Business web site.

extent cutting the delays more than half. Therefore the small estimated effect of SARE can not be attributed to a weak reform.

Since these additional procedures are complements to SARE we should expect SARE to have a smaller effect where the cost of the additional procedures is greater. In other words, SARE decreases the total burden by a smaller proportion in locations where the burden of the other procedures is high. To test this hypothesis we interact the SARE effect coefficient with the cost of these extra procedures.

Possible Explanations for the Small SARE effect

	Main Effect	Interaction
Cost of other Procedures[a]	0.02***	-0.03***
	(3.53)	(-3.01)
Corruption in Zoning Restriction[b]	0.021**	-0.016*
	(2.70)	(-1.82)

[a] Includes the 26 SARE municipalities for which the *Doing Business in Mexico Report* provides information. [b] To have a consistent sample, we imputed the State values to the 26 municipalities that have Doing Business data and used only those municipalities.

Table 10: Some Possible Determinants of the Effect of SARE

To get a measure of relative cost of performing the non-SARE procedures we divided the this cost by the cost in the most expensive municipality. So our measure of additional cost is between 0 and 1. It has a mean of 0.47 and a standard deviation of 0.18. As the first line of table 10 shows the interaction is negative: SARE has a lower effect in those municipalities where the burden of *other* registration procedures is high. A decrease of the extra cost from the 75th percentile to the 25th percentile (that is, by about $460 dlls, 0.23 in our relative cost measure), is associated with an increase of SARE's effect of 35%.[28]

One important cost of being registered is complying with a series of health and safety regulations. Anecdotal evidence indicates that at this stage bureaucrats try to find any minor non-compliance in order to extract bribes. SARE delays these revisions for three months but does not eliminate them. If bribe extraction is high, entrepreneurs will be reluctant to register and make their location known to inspectors. Unfortunately the Doing Business indicators do not include a measure of the pervasiveness of corruption at this stage.

However, Transparency International has conducted a survey in Mexico on the corruption involved in asking for a zoning permit ("uso de suelo").[29] In order to obtain the operational licence, the firm must first obtain a zoning permit that demonstrates that the

[28] Using the number of days we also get significant and qualitatively similar results.

[29] Their survey is not representative at the municipal level. The data are reported at the State level, however the main cities, which are typically the SARE cities, are much more represented. Because of this results should be treated with caution. For more information see their web page.

firm complies with all zoning restrictions. Transparency International reports the percentage of the time that the interviewed households reported giving a bribe when asking for this zoning permit. It turns out that the national average for 2001 (for about 15,000 households) is 10.1%.

We expect SARE to have a smaller effect in States with more corruption in the pre-requirements for using SARE. We interact the variable measuring the percentage of bribes regarding "uso de suelo" in a State with the SARE effect variable. Results are reported in the second row of Table 10. The interaction shows that SARE is less effective in States with more "corrupt" zoning permit procedures: one standard deviation increase in the percent of bribes (6%) decreases the effect of SARE from 2% to 1%.

We have discussed how different costs may be reducing the effect of SARE on new firm start-ups. Additionally the benefits of registering with the government authorities may be low. The most often mentioned benefit is access to credit. However, given that collateral is hard to register and verify, credit may be hard to obtain. The Doing Business Report on Mexico calculated an index of the ease of creating and registering collateral that intends to measure "the degree to which collateral and bankruptcy laws facilitate lending". The index ranges from 0 to 10 with higher scores indicating that existing laws promote the extension of credit. The report also provides information about the cost and time it takes to register collateral. We interacted each of these variables with the SARE effect variable and found the interaction to be statistically not different from zero.

The above regressions do not establish any causal effects; furthermore we have no strategy to separately identify the effect of corruption from, say, the effect of extra costs. Besides the small sample size, there may be problems of omitted variables (e.g. institutional characteristics) that are correlated with corruption, extra registration costs, ease to register collateral and SARE effectiveness. However the correlations presented are useful to have an more informed discussion. We think that additional procedures, corruption and omitted factors closely correlated with them can be interpreted as measures of complementary institutional quality. And the picture that emerges from the above regressions is that they can hinder the effectiveness of SARE.

To gain a better understanding of the barriers of starting and managing a small firm in Mexico we used the National Survey to Small Firms (ENAMIN) produced by Mexico's Statistical Institute (INEGI). This survey is conducted at the households in 45 main urban areas in Mexico, and has a target population of firms with less than 6 employees (or 16 if the firm is in the manufacturing sector).[30] We use the latest (2002) survey which contains 11,306 respondents.

The firm owners were asked about how they financed their start-up firm; 40% said it

[30]For more information about this survey consult www.inegi.gob.

was from personal savings, 11.4% said friends lent them, 20.5% said they did not need money to start, and 1.5% used trade credit. Only 0.5% of these entrepreneurs borrowed from commercial banks. This may mean that access to credit for small firms is difficult, but it may also mean that the demand for bank credit is low. If it were the former then we should expect credit to be a big problem in the operation of a small business. These entrepreneurs were asked which is the main problem facing their business; 49.4% replied that it was lack of customers or strong competition, 12.5% said they had no problems, *3.8% said that credit was a problem, and only 1% said government authorities were a problem.* Even though firms in the sample are a selection of firms that could start-up a business, the proportion of entrepreneurs who report problems of access to credit or dealing with authorities is surprisingly low.

The fact that entrepreneurs say that competition and lack of customers is their main problem suggests that we have to take seriously the possibility that human capital or entrepreneurial ability could play a significant role in limiting the effect of programs like SARE. The surveyed entrepreneurs may not be good at developing and marketing new ideas. Djankov, Quian, Ronald and Zhuravskaya (2006b) provide some evidence that entrepreneurs differ significantly from non-entrepreneurs in their attitudes towards risk and work-leisure preferences, as well as in their social environment. Another possibility to consider is the stage of development on the demand side: consumers in less developed countries may spend a big part of their income on basic needs, which may limit the sale of some new products or services.

Still another possible explanation for the small estimated effect of SARE is that not all new firms register in our data set, that is, with the labor authorities. A requirement for starting the SARE registration procedure is to register with the tax authorities, and only 3 months after obtaining the operation licence through SARE is the entrepreneur required to register with the labor authorities. It is possible that they illegally skip this later requirement. But to what extent can this happen?

In order to achieve the 20%-plus increase in firm start-ups that the World Bank reports for other countries, more than three quarters of the firms that register with the tax authorities would have to avoid registration with the labor authorities. This seems highly unlikely. To substantiate this claim we used the ENAMIN survey again. It asks two separate questions on whether the firm is registered with the tax and/or the labor authorities. It turns out that only 28% of the firms registered with the tax authorities said they are not registered with the labor authorities. If we believe this number SARE's effect would maybe be 5% of 6% instead of 4%, that is, almost not change. Thus we believe that this is not the right explanation for the small SARE effect.

The discussion in this section can only be suggestive. As mentioned above, the sample of municipalities used is small and, more importantly, there may be omitted variables driving some of the reported correlations. However we have raised issues that should matter for policy makers trying to implement this type of programs: the burden of additional procedures, taxes and the overall institutional environment will have an impact on their effectiveness.

8 Conclusion

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

This paper presents evidence that reducing the costs of obtaining an operation licence can in fact lead to increased formal firm creation. We also find, however, that the effects of the program we studied were temporary and of a 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 we study has not lived up to the lofty expectations of some. We conjecture that this is due in part to the burden of complementary procedures and overall institutional quality. A more inclusive program could have a much bigger effect on firm start-ups.

We conclude by mentioning that burdensome *registration* regulations may not be the only important barrier to firm creation or firm formalization. Instead the cost of paying taxes may still outweigh the benefits of registering, especially since credit was scarce during this period and the cost of enforcing contracts is still high in Mexico. We also pointed to the fact that publicity about the program may be important. To the extent that a poorly functioning tax system, a poorly functioning credit market, corruption, or other institutions are relevant, additional (possibly complementary) policies addressing these problems should be considered in addition to reform programs like the one we studied.

9 Appendix: Additional Tables

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

.

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

Kernel estimates of the Exit Hazard function by Industry Eligibility.

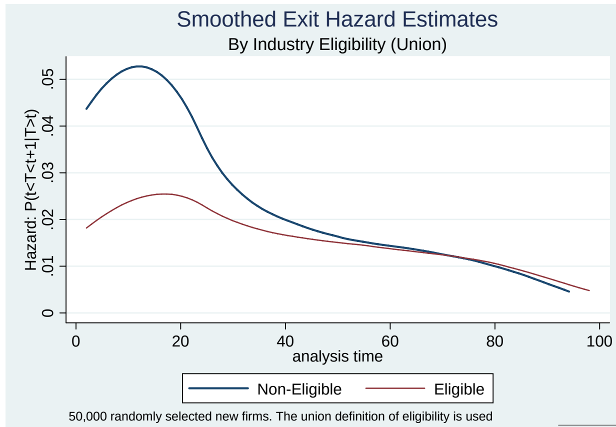

Figure 4: Exit Hazard by Industry Eligibility. SARE municipalities using the “union” definition

10 Bibliography

References

Athey, S. and S. Stern, “The Impact of Information Technology on Emergency Health Care Outcomes,” *RAND Journal of Economics*, 2002, *33* (3), 399–432.

Banxico, “Resumen Informe Anual,” *Webpage: www.banxico.org.mx.*

Bertrand, M. and F. Kramarz, “Does Entry Regulation Hinder Job Creation? Evidence from the French Retail Industry,” *Quarterly Journal of Economics*, 2002, *17.*

Bruhn, M., “License to Sell: The Effect of Business Registration Reform on Entreprenurial Activity in Mexico,” *Job Market Paper MIT*, 2006.

Cabrero, E., A. Ziccardi, and I. Orihuela, “Ciudades competitivas - ciudades cooperativas: conceptos claves y construccion de un indice para ciudades mexicanas,” *Documento de Trabajo CIDE*, 2003, *139.*

Djankov, S., C. McLiesh, and R. Ramalho, “Regulation and Growth,” *Economic Letters*, 2006, *92.*

——, **R. La Porta, F Lopez-De-Silanes, and A. Shleifer**, “The Regulation of Entry,” *Quarterly Journal of Economics*, Feb 2002, *CXVII* (1), 1–37.

——— **, Y. Quian, G. Ronald, and E. Zhuravskaya**, "Who Are China's Enterpreneurs?," *American Economic Review*, May 2006, *96* (2), 348–352.

Dunne, T., M. Roberts, and L. Samuelson, "Patterns of Firm Entry and Exit in US Manufacturing Industries," *RAND Journal of Economics*, 1988, *19* (4), 495–515.

Easterly, William, "The White's Man Burden," *Penguin Press*, 2006, p. 111.

Economist, The, "Measure First, Then Cut," Sept 11th 2004, p. 71.

Galiani, S., P. Gertler, and E. Schargrodsky, "Water for Life: The Impact of the Privatization of Water Services on Child Mortality," *Journal of Political Economy*, 2005, *113.*

Gruber, J., "The Incidence of Mandated Maternity Benefits," *American Economic Review*, Jun 1994, *84* (3), 622–641.

Jenkins, S., "Easy Estimation Methods for Discrete-Time Duration Models," *Oxford Bulletin of Economics and Statistics*, 1995, *57.*

Klapper, L., L. Laeven, and R. Rajan, "Entry regulation as a barrier to entrepreneurship," *Journal of Financial Economics*, 2006, *82* (3), 591–629.

Schneider, F. and D. Enste, "Shadow Economies: Size, Causes, and Consequences," *Journal of Economic Literature*, 2000, *XXXVIII.*

Soto, H. De, *The Other Path*, Harper and Row: New York, 1989.

Straub, S., "Informal Sector: The Credit Market Channel," *Journal of Development Economics*, 2005, *78.*

WorldBank, *Doing Business 2006*, World Bank, 2006.