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Entry regulation and business start-ups: Evidence from Mexico ☆

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ABSTRACT

We estimate the effect on business start-ups of a program that significantly speeds up firm registration procedures. The program was implemented in Mexico in different municipalities at different dates. Our estimates suggest that new start-ups increased by about 5% per month in eligible industries, and we present evidence supporting robustness and a causal effect interpretation. Most of the effect is temporary, concentrated in the first 15 months after implementation. The estimated effect is much smaller than World Bank and Mexican authorities claim it is, which suggests attention in business deregulation may be over emphasized.

1. Introduction

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

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

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

In this paper we answer the following questions: To what extent does a decrease in start-up regulatory costs increase the rate of formal

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[1] See for example Djankov et al. (2006a) and Klapper et al. (2006).

[2] 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)). The World Bank has even conditioned aid on reducing these procedures. As a result many countries have implemented reforms designed to simplify the registration process: according to the World Banks Doing Business Report 202 reforms in these lines were introduced between January 2005 and April 2006 in 108 countries.

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firm creation? Is this effect permanent? Can the size and life span of the new registering firms tell us something about whether they are truly new firms or firms that had been operating informally?

To get at these questions we estimate the effects of a deregulation program, "System of Fast Opening of Firms (SARE)", which took place in Mexico in different locations at different time periods. This program instituted 'one-stop' firm registration offices in some municipalities. These firm registration offices allowed small firms that operate in eligible industries to obtain a license to operate in two days or less and to postpone health and social security inspections for three months. SARE significantly reduced the time to obtain the business license: before the program was implemented it took about 30 days to go through the municipal registration procedures and afterwards it took at most 2: *thus the decrease in the delay after SARE was introduced is equivalent to the difference in registration delays between Jamaica and Canada, between Mexico after the reform* versus *Ukraine, or between Mexico before the reform* versus *China* (according to Djankov et al. (2002)).

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

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

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

There have been some previous papers that pose questions related to ours. Bertrand and Kramarz (2002) study the effect of increasing regulatory entry barriers. By using variation across time in the toughness (rejection rates) of the application of zoning restrictions in France, they show that these restrictions have a negative impact on employment growth. Klapper et al. (2006) find that the cost of entry regulation procedures across countries is negatively correlated with the percentage of new firms in an industry. This correlation is stronger for more entry-prone industries. Djankov et al. (2006a) show in cross country regressions that entry regulation procedures are negatively correlated with GDP growth.

The main challenge facing these papers is to establish a causal relationship between the regulatory burden and economic outcomes. Bertrand and Kramarz (2002) use the political party composition of the approving board as an instrument for time variation in rejection rates; Klapper et al. (2006) try to control for other business environment variables like financial development, labor regulation and protection of intellectual property; Djankov et al. (2006a) instrument their index of regulatory burden with a legal origin variable and with geographic and cultural variables.

Our paper complements this existing literature by using a more transparent source of variation induced by the staggered implementation of a government program for selected industries, thus addressing the question of causality in a more direct manner. Another contribution of our paper is to provide for the first time direct causal evidence of the effect of a deregulation program of the type encouraged by the World Bank and implemented by several dozens of countries.[3]

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

2. Institutional setting and description of the program

2.1. Regulatory burden in Mexico and description of the program

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

Spurred in part by this poor performance, in March 2002 the Federal government in Mexico, through its office of the "Federal Commission of Regulatory Improvement" (COFEMER), implemented a program called "System of Fast Opening of Firms" (SARE for its initials in Spanish) to reduce the number of administrative procedures and time required to register a firm and to make these procedures more transparent.

SARE is a Federal level program targeted at municipalities[4] that ensures that micro, small and medium firms that pose no health or environmental risks can register and begin operations in two days, conditional on the eligibility and zoning requirements being met. The program had a substantial impact on the time it takes to register a firm and obtain an operation license. The program not only speeds up registration, but also clearly defines the procedures, fees and identities of the entities involved in the registration process, thus making the procedure more transparent and making it harder for bureaucrats to delay the process in search of bribes.

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

It is important to note that not all firms can register and obtain a license through SARE. The Federal government selected 685 "non-

[3] Bruhn (2011) has independently (and simultaneously) evaluated the effects of SARE using household employment survey data. We believe that our paper is complementary and has several key advantages. Since the papers study the same program and have quite different results we will devote Subsection 5.2 to their comparison.

[4] 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.

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risky" 6-digit industries as eligible for the program using INEGI's classification; that is, only firms in industries that pose little health or environmental threats can register through SARE. The rationale for selecting only these industries is that the officials did not want to reduce oversight for firms in activities prone to accidents or health hazards.

As a result, the retail and services sectors are disproportionately represented in eligible industries. Some examples of eligible industries include: production of metal and wooden furniture, freezing of fruits and vegetables, production of clothes and textiles, drugstores and small supermarkets, video stores and DVD rentals, real estate services, etc. Examples of non-eligible industries include: bars, production of rubber products, hospitals, production of machinery, etc.[5]

Kaplan et al. (2007) report more information about eligibility by industry and how industry eligibility varied across municipalities. Although each municipality was encouraged to select all of these 685 industries, most included only a subset of this list. However, municipalities tended to select the same industries as eligible, mostly copying their lists from other municipalities that already had implemented the program.

Four important features are helpful for our analysis: first, firms that satisfy the eligibility criteria must register through SARE; second, firms cannot register in one municipality and operate in another; third, since the mean number of employees of a firm registering through SARE is 2.6, we believe that these firms are most likely single establishment entities; fourth, there were no other government programs being implemented with a similar location-time profile whose effects we could be attributing to SARE.

2.2. Implementation of SARE

Mexico has 2448 municipalities and 32 States. The Federal government wanted to implement this program first where it could have the greatest impact. It used a study by Cabrero et al. (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 focused its efforts on convincing these municipalities to adopt SARE, but it cannot deny participation to any other municipality. Competitive municipalities form what is commonly known as the "intention-to-treat" group. SARE was supposed to be implemented in all of them by the end of 2006, although this goal was not achieved.

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

Table 1 presents statistics on the timing, geographical variation, and clustering of SARE adoption during our sample period. The first and second rows show the number of municipalities and States that adopted the program in each year. SARE adoption has substantial geographic variation: out of a total of 32 States, SARE was implemented in 31 of them during the sample period. The third row shows that municipalities within a State tended to implement at the same time; for example in 2004 more than a third of implementation happened in the most active State (the State where the President of Mexico came from); in 2005 about one fifth of implementation came from the most active State. The last row counts the number of non-competitive municipalities that implemented the program. Since they were not explicitly invited, they typically implemented later.

Table 1
SARE Adoption: Timing, geographical breadth and municipality clustering.

SARE adoption by year[a]: Timing, geographical breadth and municipality clustering	2002	2003	2004	2005	2006
Number of municipalities implementing	2	8	28	47	8
Number of States implementing	2	7	15	17	7
Number of municipalities implementing in the most active State[b]	1	2	10	9	3
Number of non-competitive municipalities implementing	0	0	11	16	4

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

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

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

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

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

3. Description of the data

We will use three sources of data: First, we use data from the Mexican Institute of Statistics, Geography and Informatics (INEGI). These data include municipality demographics from the 2000 Mexican Population Census, municipality production data from the 2004 Mexican Economic Census and data about political variables from INEGI's municipal databases. Second, we use the industry

[5] For a detailed list please consult COFEMER's web page at http://www.cofemer.gob.mx.

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Table 2
Summary statistics by type of municipality.

Means and Std. Dev. by type of municipality (monthly averages)				
	Variable	SARE	Competitive	Adjacent other
Formal employment[a]	61,450 (85,672)	13,213 (23,578)	1446 (3040)	1420 (4019)
Monthly new jobs by new firms[a]	409 (478)	99 (188)	16 (49)	15 (71)
Monthly new firms[a]	111 (118)	26 (48)	4 (9)	4 (11)
Non-existing firms[a]	3620 (4367)	786 (1455)	131 (248)	117 (285)
Population[b]	1,332,588 (1,356,585)	504,958 (829,175)	120,846 (108,289)	101,024 (115,520)
% Workers in tertiary sector[b]	54% (13%)	53% (12%)	34% (10%)	35% (13%)
Number of establishments[c]	11,518 (12,573)	4089 (6760)	751 (873)	685 (1019)
Production[c]	$2388 ($3071)	$846 ($2346)	$100 ($487)	$59 ($296)
Number of municipalities	93	142	267	1008

Means with Standard Deviations in parenthesis.
[a] From our IMSS dataset (averages 1998–2001).
[b] From the Population Census 2000.
[c] As reported in the economic census 2004 (millions of 2004 dollars).

eligibility lists from 86 of the 93 municipalities that implemented the program in our sample period as well as information on the change in registration time for a subset of these municipalities. Third, we utilize a monthly census of all establishments that have employees registered with the Mexican Social Security Institute (IMSS), the equivalent of the US Social Security Administration. That is, *we observe the registration of all formal employees in Mexico*.

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

As a result our data is ideal to study the effect of SARE on formal employment and on the number of firms with formal employees, as by definition it is total formal employment. Since one of the justifications for programs like SARE has been to increase formal employment, the outcome we study is particularly relevant for policy makers. Furthermore, to the extent that firms may register with the Tax Authorities but not with IMSS, our estimates can be viewed as a lower bound for the total effect on new registrations; however we present evidence suggesting that this lower bound may not be too far from registration with Tax Authorities.

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

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

Since the IMSS definitions of industries are not exactly the same as the INEGI definitions used by COFEMER for eligibility, we had to construct a concordance between these two lists of industries. We manually matched 685 6-digit INEGI industries to 302 4-digit IMSS industries. In principle this may be a cause for concern since some IMSS industries are thus classified as eligible even if not all IMSS employment in that industry is eligible according to the COFEMER-INEGI classification.

Although this could introduce some measurement error, we show in Appendix A that the mismatch was small: only 19 out of the 302 IMSS industries had eligible and ineligible INEGI industries inside. This "ineligible-inside" component represented only 7% of the total of eligible INEGI industries (4% weighting by production). No INEGI industry intersected two distinct IMSS industries.[6]

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

In addition to the concordance of INEGI and IMSS industries, we had to construct a concordance between INEGI and IMSS municipalities. There are 2448 municipalities according to the INEGI classification system, but only 1510 municipalities according to the IMSS classification system. The main difference is that IMSS often aggregates smaller municipalities together into a larger entity. Thus we lose some INEGI municipalities for which we could not find the corresponding IMSS municipality; these are mainly smaller municipalities. We do not lose any SARE municipalities, although we lose 16 Competitive municipalities. Since our main results are estimates of the average treatment on the treated, this small loss of non treated municipalities is of little importance.

4. Empirical strategy and models

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

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

[7] Two recent papers which have similar settings as ours are Athey and Stern (2002) and Galiani et al. (2005).

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Table 3
Discrete duration model of program implementation.

Political variables		Economic time-varying variables		Economic static variables	
Party 1 (official)	0.25** (−2.18)	New firm creation in eligible industries (MA12, detrended)	1.01 (0.53)	Total population (thousands)	1.002 (1.31)
Party 2	0 16*** (−2.83)	New firm creation in non-eligible industries (MA12, detrended)	0.97 (−0.62)	Production ($dollars per capita)	1.00004 (0.39)
Party 3	0.24 (−1.44)	Job creation in eligible industries (MA12, detrended)	1.002 (1.18)	Unemployment (2000 census)	1.20 (0.66)
2nd year of tenure	3.17** (2.31)	Job creation in non-eligible industries (MA12, detrended)	0.99 (−0.53)	Working age population (thousands)	0.99 (−1.02)
3rd year of tenure	2.10 (1.47)	Weibull duration dependence parameter	16.72*** (4.29)	% Employees in tertiary sector	1.04 (0.07)
Number of Mun. in State that implemented	1.26*** (2 97)			% Working age registered at IMSS	0.96 (−0.16)
				Log (State exports)	0.75 (−1.34)
				% Employment in exporting firms	0.19 (−0.58)
				% Workers receiving no income	0.28 (−0.54)
				Municipality government revenues ($ millions of dollars)	1.004 (1.39)

Includes SARE and Competitive Municipalities only. In all tables, we use the notation of *** to denote significance at the 0.01 level. Similarly ** denotes significance at the 0.05 level and * denotes significance at the 0.10 level.

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 as they involve unobserved scenarios. 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 that may be related to future outcomes. Instead we will show that political variables are more important determinants of adoption. This result is important since – as long as the political variables are not correlated with the trends of firm creation – [8] it makes it less likely that time-varying unobserved variables are affecting the trends of firm creation differentially for SARE municipalities or for SARE eligible industries.

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

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

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

4.1. Where is SARE implemented first?

If factors affecting the time trends of firm creation are correlated with variables affecting the date of program adoption, then it is likely that early adopting municipalities would have had a different trend of new firm creation compared to late adopters even in the absence of the program.

If these factors are observed we can simply interact the time trends with these factors – thereby allowing for different control trends – and consistently estimate the effect of the program. However, if they are unobserved, comparing firm creation between early and late adopters before and after the program will give us inconsistent estimates of the true effect of the program. A similar problem holds if we compare eligible vs. non-eligible industries. If the timing of the adoption of the program is correlated with expected changes in the industry composition of new firms, then our estimate of the causal effect will be inconsistent.

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

The political variables we use are: party of the municipality mayor (PRI, PAN, and PRD) as well as the mayor's tenure at the time of adoption (the excluded categories are other parties and coalitions, and the first year of tenure, respectively); to capture the effect of "recommendation" to adopt by the State governor, we also include as a regressor the number of municipalities that have implemented in the State at any given time. The time varying economic variables we use are firm creation and job creation, adjusted for seasonality.[9] The time constant regressors we use are demographic and economic municipality characteristics: total population (in thousands of individuals), production per capita (in millions of 2000 dollars), unemployment rate, 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

[8] 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 for any pair. This result also holds if we control for municipality fixed effects and therefore look at changes in firm and job creation.

[9] 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 remove the seasonality of these variables.

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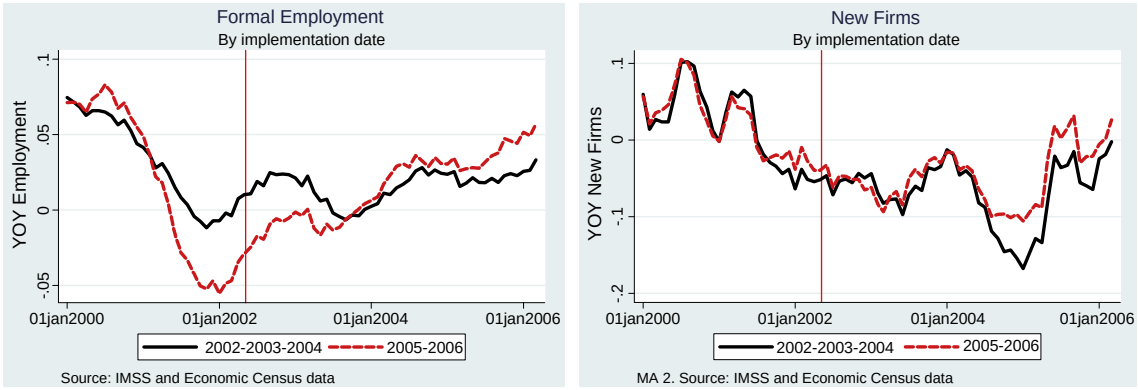

Fig. 1. Year-on-year growth of employment and firm creation by time of implementation.

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',[10] and thus only those are included in the sample for Table 3. However excluding the non competitive SARE municipalities does not change the qualitative conclusions. Table 3 shows the results. Coefficients are reported in an exponential form so that they can be interpreted as semi-elasticities of the hazard of implementation.

The results are consistent with what COFEMER officials told us: mayors in their second year of tenure and those who belong to the party of the President are significantly more likely to adopt the program. It is also true that the more municipalities that have implemented in a State, the more likely it is that another municipality in that State will adopt; this reinforces our belief that the State governor's pressure on municipality mayors to implement SARE is important and that implementation is less a result of a municipality specific shock.[11] Most important for us is the fact that the economic time-varying covariates are not significantly different from zero (not even jointly) at 5% confidence level and are also 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 more important determinants of program adoption; this makes it less likely then that the identification strategy we use is flawed. Kaplan et al. (2007) perform some statistical analysis to show that these political determinants were not correlated with trends in firm or job creation before 2002.

4.2. Comparing adopting vs non-adopting municipalities

4.2.1. Main identification problem

In this section we lay out the main identification problem we face, and estimate the effect of SARE on firm and job creation by comparing early vs. late adopting municipalities. This is not our main identification strategy and we devote just enough space in the paper to aid transparency and to motivate our preferred estimation strategy.

A necessary condition for this strategy to be valid is that pre-SARE time trends for early adopters (treatment group) and late adopters (control group) are parallel. In our data this cannot be statistically rejected at the 5% significance level.[12] However meeting this condition is not sufficient to ensure we estimate the true causal effect of the program: we further need to isolate the change in these time trends as caused by the program versus other causes of time trend changes. Meeting this latter condition is complicated by the fact that just as the program began implementation in 2002, a slowdown of the Mexican economy was underway that may have affected different municipalities differently.

Apparently this differential impact of the economic slowdown across municipalities is indeed what happened. Fig. 1 shows that, although the growth rates are similar before 2001, they begin to differ after 2001, even before SARE. In particular: late adopters suffered a stronger deceleration of growth around 2001 and then a stronger recovery after 2002. This is more marked in total employment changes than firm creation changes[13] since trends move differently for control and treatment groups there is reason to suspect that late adopters don't provide a good counterfactual and therefore the difference-in-difference identification strategy would be invalid.

A complete understanding of the fundamental causes of this discrepancy in trends across municipalities is not necessary for the validity of our preferred identification strategy, but such and understanding may be useful to us to the extent that we can control for these causes in the regression. According to the Mexico's Central

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

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

[12] We interacted a monthly linear and a quadratic time trend 1998–2001 with a treatment and control dummy and their equality cannot be rejected.

[13] In our data employment seems to recover faster than new firm creation. This latter fact may happen since both existing and new firms hire labor during the upturn of the economy. The raw correlation between these two variables in our data during our sample period is 0.86. Firm and employment creation presumably obey different determinants and therefore may have different dynamics (for example entry costs are arguably more important for form creation than for hiring workers at existing firms). Lee and Mukoyama (2008) show that the relationship between total employment and new firm creation is complex, and theoretical predictions across models differ even qualitatively depending on assumptions about the functional form and level of hiring, firing and entry costs. They also show that for US manufacturing, job creation increases more than firm creation during high growth periods and decrease more in low growth periods, which is consistent with our data.

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Table 4
Comparing eligible firm creation across municipalities: β coefficients.

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

SARE's: 86 municipalities, 99 months. Errors clustered at the municipality level. In all tables, we use the notation of *** to denote significance at the 0.01 level. Similarly ** denotes significance at the 0.05 level and * denotes significance at the 0.10 level.

Bank the deceleration was a result of the US recession (Banxico (2002, 2003)). This is a plausible explanation since about 3/4 of Mexican exports go to the US, and from October 2000 to January 2002 non-oil exports to the US decreased by 30%! Meanwhile imports of consumption goods decreased by only 15%, suggesting the contraction is more likely to have been an external rather than an internal shock.

Fig. 4 in Appendix A gives some credence to this explanation showing that there was a marked deceleration of employment growth in 2001–2002, and that it is clearly stronger for municipalities in higher exporting States. The correlation in 2002 between the share of employment in exporting firms and a counter of year of implementation (1, 2, 3 ,4, 5, and 6 representing 2002, 2003, 2004, 2005, 2006, and 2007 respectively) is 0.62; that is: later implementers have more employees working in exporting firms, which may partly explain why they are hit harder and also recover faster with the US recovery.

Our preferred estimation triple difference strategy in Section 4.3 will deal with this identification problem by comparing within municipalities and by controlling for export intensity. Instead of proceeding to this estimation we will show the resulting estimates of a difference-in-difference (DID) estimation as a motivation of our more robust estimation strategy.

4.2.2. Estimation strategy 1

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

We want to estimate the effect of SARE (treatment) on SARE implementing municipalities (on the treated): $ATT \equiv E[Y_{1t} - Y_{0t}|SARE=1]$. Since implementation is staggered across time, we do this be estimating Eq. (1) by OLS and using the estimate of β as our estimate of ATT. This implicitly assumes that the average outcomes of late SARE implementers is an unbiased estimator of the counterfactual outcome Y_{0t} of early SARE implementers, an important but untestable assumption. Consider the following equation:

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

In this equation α_i are municipality fixed effects, $\gamma_{\varphi(i)t}$ are sets of time trends, one for each group of municipalities $\varphi(i)$, and AfterSARE_{it} is our policy variable equal to 1 for municipality i after it implements the program and zero before it does. To save space in Table 4 only presents the estimated β coefficient of specifications with different dependent variables (columns) and samples (rows).[14] In Table 4 we use a baseline specification and set $X_{it}=1$ and $\varphi(i)=$ all SAREs, thus using only one control group trend.

The estimates of Eq. (1) reported in Table 4 attribute a 5% decrease of monthly firm creation in eligible industries to the program. Employment is also negatively affected, although the standard errors are bigger. The evidence in Subsection 4.2.1 suggests that this negative coefficient arises because the economic slowdown in the US had larger adverse effects in late implementing municipalities, which are more closely linked to the US and recovered faster after 2002. When we restrict the sample to the more homogeneous group of municipalities adopting before 2005, the "effect" is not different from zero in all variables.

In Kaplan et al. (2007) we estimated more flexible models that allow for different time trends for different groups of control municipalities, i.e., the $\gamma_{\varphi(i)t}$'s of Eq. (1). We do this by interacting monthly time dummies with covariates which we believe proxy for the strength of the above mentioned shock; these include the percentage of the State's GDP made up by exports, total population, a dummy for adopting before or after 2004, and municipality-specific linear time trends. Once we include these different counterfactual control trends the effect of SARE is not statistically different from zero in all specifications for the three dependent variables above. The results are unchanged if we include competitive non adopting municipalities as part of the control group.

Given these results, we believe the DID estimation is not well suited to the estimation of the program's effect. We believe that the additional controls only partially pick up the differential trends for early and late adopters, implying that the DID estimates are underestimates of the true effect. The rest of the paper estimates the effect of SARE by comparing eligible versus non-eligible industries within municipalities.

4.3. Comparing eligible vs. non eligible industries

4.3.1. Estimation strategy 2

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

One important advantage of the strategy is that it allows us to control for the municipality specific (across all industries) slowdown of economic activity after 2002. Since it can be the case that eligible and non-eligible industries have different time trends, what we effectively do is to compare the *rate of change* of the gap of firm and job creation across industries in treated municipalities to the

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

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analogous quantity for the control municipalities, before vs. after implementation. This amounts to using a diff-in-diff-in-diff strategy which can be written as follows:[15]

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

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

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

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

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

In spite of our arguments against the potential selection problem of industries and in favor of the robustness of the triple difference approach, the unbiasedness of estimates are never guaranteed in non-randomized program evaluations. If for reasons unrelated to the program the *gap* in firm creation between eligible and non-eligible industries starts to evolve differently in early vs. late adopters around the time of implementation, our identification strategy will yield an inconsistent estimate of the program's true effect. This could happen, for example, if the US recession was felt less strongly in the eligible industries relative to the non-eligible ones in early adopting municipalities. Note that this alternative story implies more that non-eligible industries being hit harder that eligible ones: there also has to be a bigger trend break in the gap in early adopters relative to later adopters just after each municipality implements the program; this may require a contrived or complex story.[16]

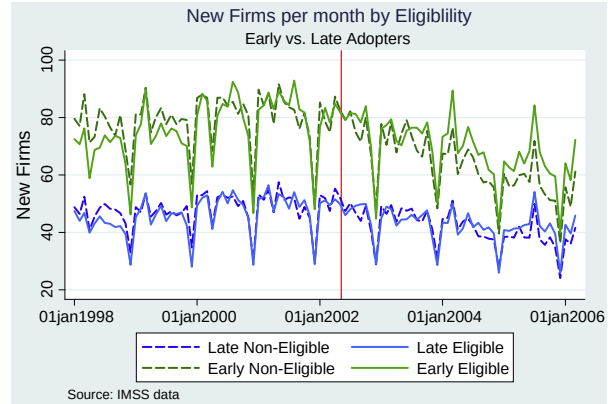

Fig. 2. Mean trends of firm creation by eligibility for early vs. late adopters.

Fig. 2 plots the evolution of firm creation by industry eligibility for early (before or in 2004) and late (after 2004) program adopters that we use in the sample. Importantly, it shows that the time trends of firm creation are very similar for eligible and non-eligible industries.

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

4.3.2. Baseline estimation

We will use the following regression to estimate the effect of SARE that was defined in Eq. (2):

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

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

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

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

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

[16] Note that scale effects, that may arise from municipalities having different sizes, are taken care of in the specification since by using logs we are looking at rates of change.

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Table 5
Means of selected variables for eligible and non-eligible industries.

Summary statistics by type of municipality and industry eligibility (1998–2001)				
	Early adopters		Late adopters	
	Non-eligible	Eligible	Non-eligible	Eligible
Number of monthly new firms	79 (65.9)	75.4 (80.8)	47.9 (42.3)	46.1 (52.1)
Number of monthly new employees in new firms	325.2 (3162)	241.5 (309.2)	200.7 (242.1)	144.9 (188.6)
Number of monthly new employees in all firms	36,792 (44,992.2)	41,252.3 (50,191.3)	30,093.4 (45,115.2)	27,366.5 (37,643.3)
Population	880,011 (750,029)		586,165 (600,870)	
Number of municipalities	37		49	

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

Table 6
SARE's effect.

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

Of the 86 SARE municipalities, 61 are Competitive. In all tables, we use the notation of *** to denote significance at the 0.01 level. Similarly ** denotes significance at the 0.05 level and * denotes significance at the 0.10 level.

Row (1) shows that the estimated effect of the program is an increase in new monthly firm creation of 4.7% (2.5 more firms per month per municipality) when SARE municipalities are the control group, and 9% when we include all other non-SARE competitive municipalities. To the extent that we believe SARE municipalities are better controls for themselves, the first estimate should be preferred. Row (2) reports that the program also caused a monthly increase in employment of 10% (20 employees per month per municipality). This, together with the estimate of firm creation, implies that firms registered after SARE are about twice as big as those registered prior to SARE.

We conjecture, but do not prove, that this change in the size composition of new registered firms is due to the fact that the new registered firms are generally older firms that had been operating (and growing) informally for some time. Another fact consistent with this story is that the exit rate of the new firms in eligible industries is 20% lower after SARE, which could suggest that these were preexisting informal firms, since the exit hazard decreases with age (see Kaplan et al. (2007)). We want to stress the fact that, although it is an important question, we cannot show directly whether the effect on formal firm creation is driven by the registration of preexisting informal firms or truly new firms.[17]

Kaplan et al. (2007) also examine how the effect of the program varies with additional outside SARE procedures which are required to operate a firm. They calculated that a decrease of outside-SARE registration costs of a magnitude equivalent to moving from the 75th percentile to the 25th percentile in this cost distribution[18] is associated with a 35% increase in SARE's effect (say from 4.7% to 6.3%). This calculation has to be taken cautiously, however, since the cost of complementary procedures may proxy for other factors in the municipality that may affect firm creation, like institutional quality.

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

How can we increase our confidence that the estimated effect is causal? If the slower decline of firm creation in eligible industries is due to SARE we would expect this effect not to be present in new job creation in existing firms or new big firm creation, since SARE did not apply to these firms.[19] We would also expect the program to be more effective where it most reduced the time and cost of firm registration. We indeed find strong evidence of these three predictions. Specification (3) shows that SARE is not associated with jobs created in existing firms; specification (4) shows that it did not have an effect on the creation of new firms with more than 10 employees. Finally, Kaplan et al. (2007) also present some evidence that the effect was stronger in municipalities that reduced time and procedures the most, however we only had 46 municipalities in the sample and statistical significance was low (furthermore time reduction may be endogenous).

Another important piece of evidence for causality is that there is no evidence of a pre-SARE trend in the gap between SARE and non-SARE controls (see Fig. 3). Since different municipalities implemented at different times, a placebo effect would need to generate an increase in the gap in each municipality just after implementation. Given the staggered implementation of the program, this explanation seems unlikely. It is difficult to think of why, other than SARE, the widening of gap of firm creation would be different for big vs. small firms or employment creation different for existing firms vs. new firms, just after SARE is implemented.

4.3.3. Robustness checks

As explained above, the unbiasedness of the estimated effect relies on an unbiased estimate of the counterfactual time trends. This subsection presents estimates of SARE's effect using different

[17] However, Bruhn (2011) presents some direct evidence suggesting that the effect of SARE is driven by previous wage earners starting up a business. However, see our discussion of this paper in Subsection 5.2 for a discussion of the Bruhn paper.

[18] As measured by the Doing Business in Mexico report, which amounts to USD$460.

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

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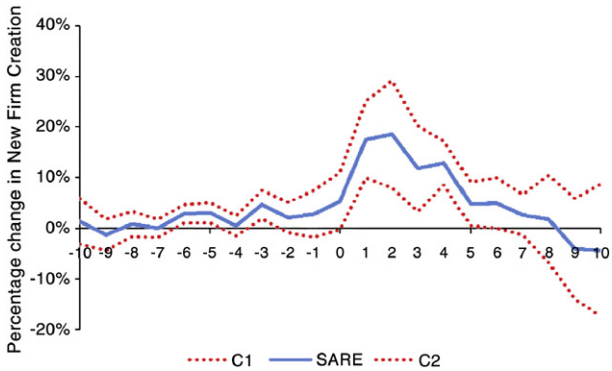

Fig. 3. Firm registration before and after SARE (includes only SARE municipalities).

municipality control time trends: by time of adoption, level of exports, and percentage of firm creation.[20] That is, we are relaxing the implicit assumption of Eq. (3) that non-eligible industries have the same trends across municipalities, by replacing $\varphi AfterSARE_{it}$ by $\varphi_1 AfterSARE_{group1,t}$ and $\varphi_2 AfterSARE_{group2,t}$, etc. By doing this we are effectively making comparisons within the group of early and late implementers, or within municipalities in States with similar level of exports or firm creation. The specifications also include as regressors firm and job creation of geographically adjacent municipalities to which the program did not apply as a control for regional shocks unrelated to SARE, and also the level of exports of the State to which the municipalities belong.

Table 7 presents the estimated effects. If the effect of SARE is spurious, caused by the early adopters having a comparatively larger increase (unrelated to SARE) in the gap between industries than the later adopters, then introducing different benchmark time trends for early and late adopters should eliminate or at least attenuate this effect. It turns out that the effect not only does not disappear, but it is estimated to be bigger.

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

4.3.4. Time profile of effect

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

Fig. 3 investigates the dynamics of firm registration. We estimated a specification in which the effect of SARE is decomposed in quarters before and after implementation by interacting the 'SARE effect' term in Eq. (3) with dummies indicating quarter before and after SARE implementation in the respective municipality. We plot these coefficients in Fig. 3 along with their 5% confidence intervals.[21]

First, it seems that the effect of SARE is temporary, being significantly different from zero from the 1st to the 5th quarter after implementation. Second and importantly, we observe no clear previous trend before SARE: the coefficients are not statistically different from zero. The non existence of a statistically significant prior trend[22] and the steep break from this previous trend is strong evidence that our estimated effect is causal, especially since the program is implemented across different time periods and in different geographical regions.

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

Table 7
Robustness checks: effect against several control group trends.

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

Only SARE municipalities. In all tables, we use the notation of *** to denote significance at the 0.01 level. Similarly ** denotes significance at the 0.05 level and * denotes significance at the 0.10 level.

5. Interpretation and caveats

5.1. Magnitude of the estimated effect

How big or small is the estimated effect of SARE? Can we put the magnitude of our estimates into perspective? Since talking about smallness of largeness of an effect is relative, we will compare it to two benchmarks: what the Mexican government and international organizations claim these (types) of effects are, and to the effect this program can claim to have on informality. Later, we will compare the magnitude of our estimates to those of Bruhn (2011), which also investigates the impact of SARE.

Let us start with the last one. Since we will argue that the effect of the program is fairly modest, we will use an optimistically large estimate to make our case stronger. We use the biggest point estimate of SARE's effect on jobs created in new firms, which translates to 19 more jobs created in an average municipality. Let us also assume that SARE's effect is constant and lasts for 2 years (also an inflated quantity given our results). In this case the number of jobs created by SARE would be 42,408 ($=19\times93\times24$). This number is about 0.2% of the total number of informal employees in SARE municipalities we study according to the 2000 Population Census. The program is apparently not a big success as a tool to fight informality, measured against the expectation of the influential work of De Soto (1989).[23]

What does the World Bank and Mexican government say the effect is? The World Bank Doing Business Report estimated that the effect of

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

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

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

[23] INEGI's Economic Census 2004 estimates that there were more than four million firm establishments in the country with less than 2.5 million of firms registered with the authorities. When compared to these numbers it seems that SARE had a limited impact.

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these types of programs on firm creation is higher than 20% in several developing countries. Their estimates, however, rely on before/after comparisons and lack an appropriate control group, our estimates are about 3 to 4 times smaller. Our estimates are also substantially smaller than what the Mexican government reports. According to COFEMER, 75,168 new firms, 194,577 new jobs, and around 1200 million dollars of new investment had been created through SARE as of July 2006. COFEMER implies that these registrations were caused by SARE.[24] According to our estimates, the counterfactual number of firms created by SARE is closer to 4029 (1343 municipality months multiplied by 3 firms per month), which is about 20 times less!

Can this relatively small estimated effect arise because results take longer to materialize or because the program was implemented wrongly? We think it is hard to argue that there has not been enough time for these reforms to have their effect or that the program constituted a small change in procedures. First, the data captures firm start-ups up to almost four years after implementation for some municipalities, this should be enough time to see an effect. More importantly, the effect we estimate shows up immediately and only lasts for a bit more than one year. Second, recall that the time reduction caused by SARE is on average 28 days which, as mentioned earlier, is *comparable to the difference in delay between Jamaica and Canada, of that between China and Mexico before SARE*. This is a substantial reduction and programs of this kind will likely be similarly implemented.

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

We report here a back of the envelope calculation to measure value added by average small firms in eligible industries in municipalities where SARE is implemented. The calculation of value added requires adding up the payments to factors of production: land, labor, capital and entrepreneurship. We use the 2002 micro firms survey conducted in Mexico (ENAMIN),[25] which provides data on payments to land (rent), labor (wages) and entrepreneurship (profits) to calculate this.[26]

We calculate that the value added by the average small firm operating in an eligible industry in a municipality where SARE is operating, amounts to $5078 Mexican pesos per month. Considering that the effect of SARE lasts 15 months, and that on average five of the new firms created per month in eligible industries in 93 SARE sample municipalities can be attributed to the existence of SARE, the benefit from this program in our sample period is 35.4 millions of pesos ($=93\times5\times15\times\$5078$).[27]

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

This implies that the benefit of the program is almost 6 times higher than its cost, thus even if we overestimated the benefits relative to cost by a factor of 6 times, it is worth it to implement SARE.

5.2. Comparison with Bruhn

Bruhn (2011)[28] uses data from household surveys in Mexico to estimate the causal effect of this same program (SARE) on new firm creation and other outcomes. Both papers conclude that SARE had an effect of 4% to 5% on firm registration; however the effect of Bruhn is on the stock of existing firms, which she translates into an average of 902 new registered establishments per municipality on all exposure period. In contrast, our estimates translate into 2 to 5 new registered establishments per municipality per month, that is, at most 15 per quarter.

To make a meaningful comparison of magnitudes between the two papers, we have to – as much as possible – normalize the estimates to the same time units, since the length of exposure to the treatment is different in the two papers and thus the estimates translate into accumulated effects differently. Since we have "per month of exposure" effects and she has "per-municipality-average-exposure" effects, we calculate the average number of firms created per municipality per quarter in both papers. Bruhn finds that a total of 30,678 firms were created due to SARE [see page 18 of Bruhn (2008)], in a total of 122 municipality-quarters of exposure (see Appendix B Bruhn, 2008 to calculate months of exposure). These results imply that 251 firms were created per quarter in the average municipality-exposure unit. This number is about 16 to 28 times higher than the 9 to 15 firms per municipality per quarter that we find.[29]

Although a comparison of the magnitudes in the two papers is important, we first compare the magnitudes of the Bruhn estimates to the administrative records of the SARE program. In order to understand the motivation behind this comparison, recall first that firms that meet the requirements of SARE are required to register through SARE. The number of firms registered through SARE is therefore an upper bound on the causal effect of SARE. As we will show below, it appears that Bruhn's estimates exceed this upper bound.

We start by simply showing plain numbers without adjustments. Bruhn estimates that, in her sample period from May 2002 to December 2004, 30,678 firms were created because of SARE. Unfortunately we do not have administrative records from SARE from December 2004. We do, however, have data from COFEMER from July 2006, which tell us that a

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

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

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

[27] We would have to add also the cost savings from shorter waiting periods, but we have little information about the opportunity cost of time for the sample of SARE applicants, and even omitting this the benefit turns out to be greater than the cost in this simple calculation.

[28] We would like to thank the referees and editor for strongly encouraging us to have the more detailed and direct discussion of Bruhns results presented in this Section 5.2.

[29] We re-estimated the main regressions in our paper using only Bruhn's municipalities and time periods. The point estimates are typically smaller by 10%–15% points, and the statistical significance decreases somewhat, but this may be due to the fact that restricting our sample to the municipalities used in the Bruhn paper reduces our sample size by nearly two thirds.

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total of 75,168 firms were registered through SARE in Bruhn's 34 municipalities. To make the most extreme assumption in favor of Bruhn's estimates, suppose all firm registrations in the SARE program occurred during her period of analysis (that is, that not a single firm was registered in the SARE program from January 2005 to July 2006). Even under this extreme assumption, Bruhn's estimates would imply that 40% of total registrations through the SARE program would not have occurred in absence of SARE, which translates to a 67% increase in firm registrations due to SARE!

However, the above assumption clearly underestimates the size of Bruhn's estimates in comparison to administrative records. To make a more reasonable extrapolation of Bruhn's results, we extend her estimated firm creation from the period of May 2002 to December 2004 (where Bruhn's period of analysis ends) to July 2006, where the stock of registration was measured at COFEMER. There are 318 SARE-quarters from May 2002 to July 2006 for the 34 municipalities that she uses, and assuming a constant effect, Bruhn's estimate would imply a total of $251 \times 318 = 79{,}818$ firms created. This is more than the 75,168 firms that COFEMER reports as registration through SARE for Bruhn's municipalities over the May 2002 to July 2006 period.

This is simply not possible if COFEMER measured administrative registrations correctly and if the effect is constant. As we mentioned earlier, it is easier to reconcile Bruhn's results with administrative data from the SARE program itself if the effects on firm registration are immediate, say entirely in the first quarter after implementation. Fig. 1 from Bruhn (2011), however, suggests that the largest effect of SARE occurs four quarters after implementation. We therefore believe that the comparison we are making between Bruhn's results and the administrative records from the program itself is not unfair to Bruhn's paper.

It is also worthwhile to point out that Bruhn's estimate of a net creation of 902 firms per quarter would be a lower bound for an estimate of the gross firm creation that COFEMER has as new firm registration. Since the hazard rate for firm exit is high for new entrants, the difference between net firm creation and gross firm creation could be significant. Interpreted in this light, the magnitude of Bruhn's estimate in comparison with administrative records seems even larger.

For all of the reasons explained above, we believe that the estimates from Bruhn's paper are implausibly large. We now turn to a more direct comparison between the results in the two papers. As Bruhn notes, our data do not include new firms that do not have employees or have chosen not to register them with social security. Such a firm would not appear as a new firm in the IMSS data used in our paper, but could appear in her household survey data. Along these lines, we want to highlight that even if the above explanation for the difference in our papers is correct, *we are correct to claim that SARE has not had a large impact on the creation of formal sector jobs and new firm start-ups with formal sector jobs*. We therefore note that, at worst, we are studying the effect of SARE on formal workers and on firms with formal workers, which is a crucially important outcome to analyze. SARE was partly intended to reduce informality, and job informality is a key outcome of tremendous interest to government and society. Informal employees have little access to insurance and medical services, are less productive than their formal counterparts, and are more likely to work in firms that do not pay taxes. We believe that studying registration with labor authorities is as important as studying registration with any other governmental agency like the tax agency.

Having said this, lack of registration with IMSS versus say the Tax Authority (SAT)[30] is unlikely to explain the large difference in magnitudes between the two papers. We were able to obtain some administrative numbers on this issue from a match of the IMSS and SAT datasets. It turns out that we may be missing at most 45% that are registered with SAT and not IMSS.[31]

Assuming registration happens uniformly in the intersection and non intersection of the data, this would imply an effect of 6.8% of SARE ($= 4.7\% \times [100\% + 45\%]$) instead of the 4.7% we estimated, which is still several times less than what the World Bank, Mexican authorities, and Bruhn (2011) report. Note further that if Bruhn is capturing firms that are not registered with SAT but are registered with other authorities then these registrations could not be due to SARE because registration with SAT is a mandatory step of the SARE program. Fig. 5 in the Appendix shows also that the size distribution of new firms in the IMSS dataset is not very different from that in the Economic Census 1999, so IMSS does not seem to be missing particularly small firms with greater likelihood.

If non-reporting to IMSS due to firms not having employees is not the explanation for the huge difference, then what else might explain the difference? We believe there are serious identification problems in Bruhn's paper. Bruhn's data consist of a sample of households from the employment surveys (ENE). This survey has the very grave problem that it does not unambiguously identify formal registered firms, our main outcome variable. Bruhn defines a formal firm as an employed person who said they were self employed or the boss at their job, and whose business had a name[32]. Furthermore, as Bruhn (2008) notes in the Appendix, the instructions say that anybody with a professional license (e.g. a bachelor's degree in law or an economic's degree) who has an unregistered business should be classified as a registered formal business.

To the extent that owners of informal firms might be miscoded as owners of formal firms (highly likely given that most informal firms 'have a name' and given that people tend to avoid saying that they are illegally informal), Bruhn will be picking up a shift to informal self employment as a result of the documented negative economic shock that disproportionately affected the early SARE adopters. The evidence for this differential shock is large and strong as documented in Subsection 4.2.1. We therefore propose the following alternative explanation of her findings. A fraction of previous wage earners who lost their jobs as a result of the US-induced deceleration opened a small stand with 'a name', thus being classified in Bruhn (2011) as SARE induced entrepreneurs. Indeed, Bruhn shows that when split by low-risk industries (mostly services) and high-risk industries (mostly manufacturing) the increase of 5% in employment in low risk industries is exactly compensated by a proportional decrease in employment in high-risk industries. That is: employment moved from industry/manufacturing and toward services right in the middle Mexico's US induced deceleration. This result is expected since the US effect is mostly felt in tradable goods.

[30] Even firms with no employees have to register with SAT to pay taxes, so that it captures formality in a broader sense. SAT is also likely to be the government agency that pursues and punishes most severely non-registration since taxes depend on compliance with SAT.

[31] This figure does not imply a lower bound of 45% on the number of new firm registrations due to SARE that are not observed in the IMSS database. It could be the case that most firm creation due to SARE happens for the 45% of firms that are not in IMSS, implying that our results using IMSS data understate the effect of SARE on total registrations by more than 45%. We tried to address this issue by calculating whether the intersection between the SAT versus IMSS data changed significantly after SARE was implemented. If SARE affected mostly registrations with SAT then – holding all else constant – the intersection between the two datasets should be smaller after SARE than before SARE. We found similar% of intersection in 2003 and 2007. However due to matching inaccuracy and the possibility of other things affecting the intersections of these datasets through time, this is only suggestive evidence.

[32] The literal question is "What is the name of the business in which you worked in last week?". The instructions to the interviewer in a 200 page manual say it should be clarified that the name means "the name with which the firm was registered" but it does not say with which Agency. Assuming these instructions are always followed and always understood by the respondents and interviewers, a firm registered with any governmental entity would qualify as formal under this definition, thus if you register with for example the directory SIEM you are classified as formal, overstating thus tax or social security formality. Furthermore one wonders whether coding errors might be common in this question. In the United States, for example, there are well known coding errors for occupation in the Consumer Population Survey Polivka and Rothgeb (1993).

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Besides the supporting evidence reported in Subsection 4.2.1, this alternative explanation matches most of Bruhn's findings. First, it could explain why former wage earners – and not for example people out of the labor force or previous firm owners – became entrepreneurs: they lost their jobs and became self employed or bosses in businesses with names.[33]

Second, it could explain why these entrepreneurs earned on average less as entrepreneurs than as previous wage workers. Under our alternative explanation, these transitions were involuntary changes, not preferences revealed by choice.[34]

Finally, the differential macroeconomic slowdown also can explain the huge decrease of 3.2% in profits of incumbents and 0.6% decrease in the economy's price level! Note that an explanation of these magnitudes most likely requires a large macro shock; they are unlikely to arise across the board and at such a high level due to the second order competition effects created by the entry induced by an small program.

In the face of these facts we conclude that the most likely and parsimonious explanation for the difference in magnitudes is that in comparison to Bruhn this paper avoids the identification problem laid out in Subsection 4.2.1, and uses better data to study formal employment and firms with formal employment (our data is virtually error free by definition of formal employment).

5.3. Other barriers to formal firm entry

Why would the effect be much lower than expected by authorities, some economist like De Soto and institutions like the World Bank? We believe that procedures to register a firm may not be the main barrier to opening a formal firm. Two of the other main candidates in the literature are taxes and lack of access to credit; another basic one is just scarcity of marketable ideas.

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

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

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

5.4. Statistical validity

Now we turn to the statistical or internal validity of our estimates. In non-experimental evaluations it is hard to prove conclusively that the results are not being driven by some omitted factor. We showed, however, three important pieces of evidence which in our view greatly minimize the risk of confounding the causal effect. First, we showed that selection based on time varying observables (and then hopefully also in unobservables) was not very likely. Second, we showed that the estimated effect is not present for samples in which it should not be. Finally, we documented a trend break when the program was implemented.

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

6. Conclusion

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

This paper presents evidence that reducing the costs of obtaining an operation license can in fact lead to increased formal firm creation. We also find, however, that the effects of the program we studied were temporary and of a smaller magnitude than those reported by authorities and the World Bank and hardly will decrease informality or spur large growth. In spite of this the program seems to have been cost effective.

We conclude by mentioning that burdensome registration regulations may not be the most important barrier to firm creation or firm formalization. In our view, the cost of paying taxes, the scarcity of marketable ideas, and the small benefits of being formal (i.e. Mexico has low access to credit and high costs of enforcing contracts in court) are far more important determinants of firm creation and formalization. Programs that attack single aspects of the problem will most likely have a small effect on informality and firm creation, and therefore on growth.

Appendix A

Appendix A.1. Evolution of employment and firm creation by exporting intensity

Fig. 4 shows the year-on-year (yoy) growth of total formal employment and new firms, by level of exporting. That is, we ranked States by the percentage of total labor force employed in exporting firms in 2002 and partitioned them in 3 groups of (almost) equal sizes (we obtain similar results if we use total exports to rank). Municipalities were labeled as Low, Medium, and High exporting depending to the group of States to which they belonged.[35] Looking at total employment it is clear that there was a marked deceleration of employment growth in 2001–2002, and that it is clearly stronger for

[33] Bruhn interprets this as evidence that SARE had an effect of new firm creation and not on the formalization of existing firms.

[34] Bruhn (2011) attributes this decrease to people paying a fixed cost, but the survey question asks what is your income not income minus fixed cost. We therefore believe that a more reasonable interpretation is that they really earn less.

[35] There is no data of exporting at the municipality level, so we are forced to group at the State level.

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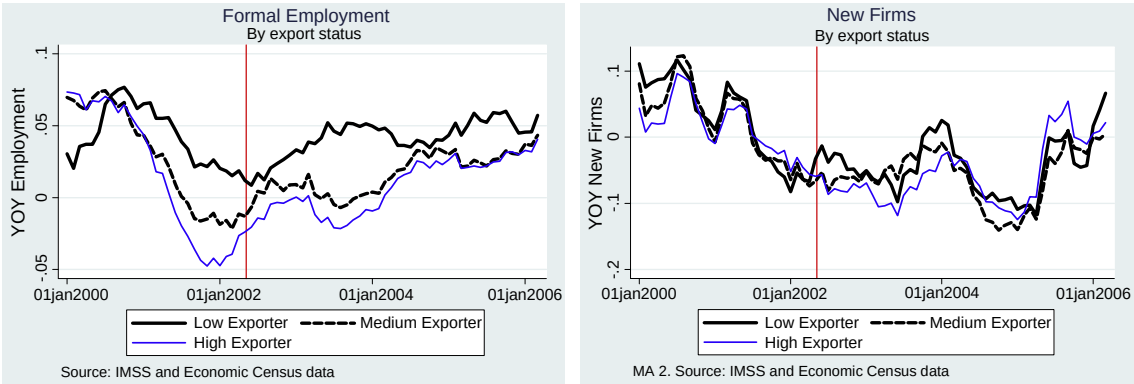

Fig. 4. Percentage YOY growth employment and firm creation by export of state.

Table 8
Description of match IMSS-INEGI industries.

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

municipalities in higher exporting States. There is also a marked decrease in new firm creation, although the monotonicity with respect to exports is not as clear.

Appendix A.2. Description of IMSS-INEGI industry match

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

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

Table 8 reports the calculations we did in order ascertain the potential impact these contaminated industries may have on our results. Column (b) shows the number of non-eligible INEGI industries inside each IMSS industry. Columns (c) and (d) translate these IMSS numbers into INEGI industry numbers, so as to weight each IMSS industry by a "contamination" factor. Column (e) calculates the fraction of non-eligible to eligible in these IMSS industries, and column (f) calculates the number of INEGI industries in these contaminated IMSS industries as a fraction of all matched INEGI industries in order to weight the IMSS industries in the sum.[36]

Appendix A.3. Firm size distribution: comparing IMSS data to Census data

To shed some light into the extent to which IMSS data is missing small or zero employee firms we conducted a comparison of the firm size distribution of IMSS in firms with less than 1 year versus 1999 Economic Census (EC). The EC is done face to face covering the whole country (localities with less than 2500 inhabitants receive a different treatment) and so it captures micro businesses and miniscule shops.

Note fist that this is not a clean comparison: to the extent that firms are multi establishment, we would be underestimating the size of firms in the Census data (unfortunately there is no way to match at the firm level). In spite of this possible underestimation, newly created IMSS firms are not much bigger than those of the whole country, using 0–10 employees as denominator: 0.4% of firms in EC have zero employees, 52% have 1 employee vs. 48% at IMSS, etc. So, IMSS does not seem to be relatively missing a huge amount of small firms. We could however miss them absolutely: i.e. even if the distribution is similar IMSS is missing all of them in a large proportion. The evidence presented in Section 5.2, however, suggests that this

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

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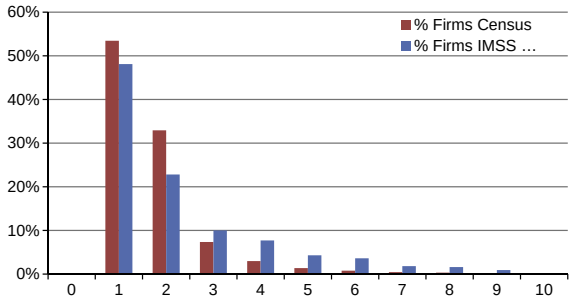

Fig. 5. Distribution of new firm sizes for firms with less than 1 yr of age: IMSS vs Census.

hypothesis is unlikely to explain the differences between the two papers.

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