

## License to Sell:

### The Effect of Business Registration Reform on Entrepreneurial Activity in Mexico

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## Abstract

This paper studies the effect of business registration regulation on economic activity using micro-level data. The identification strategy exploits the fact that a recent business registration reform in Mexico was introduced in different municipalities at different points in time. Using panel data from the Mexican employment survey, I find that the reform increased the number of registered businesses by 5 percent in eligible industries. This increase was due to former wage earners opening

businesses. Former unregistered business owners were not more likely to register their business after the reform. Moreover, employment in eligible industries went up by 2.8 percent, and people who were previously unemployed or out of the labor force were more likely to work as wage earners after the reform. Finally, the results imply that the competition from new entrants lowered prices by 0.6 percent and decreased the income of incumbent businesses by 3.2 percent.

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This paper—a product of the Finance and Private Sector Team, Development Research Group—is part of a larger effort in the department to understand the impact of business regulation reforms on economic development. Policy Research Working Papers are also posted on the Web at <http://econ.worldbank.org>. The author may be contacted at [mbruhn@worldbank.org](mailto:mbruhn@worldbank.org).

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# License to Sell: The Effect of Business Registration Reform on Entrepreneurial Activity in Mexico

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# 1 Introduction

Business entry regulation varies widely across the world. Djankov et al (2002), find that the number of procedures for registering a business ranged from 2 in Canada to 21 in the Dominican Republic in 1999. Given these large differences, an important question to study is the effect of entry regulation on economic outcomes. Most work on this question has been based on cross-country studies<sup>1</sup>, yet such an approach suffers from endogeneity issues. In particular, cross-country studies cannot rule out that causality goes the other way, such that increases in entry or output lead to reforms. It is also possible that both simpler regulation and higher entry or growth are caused by a third variable. Recent work has also looked at cross-country, cross-industry variation to try to account for endogeneity<sup>2</sup>. However, this approach cannot quantify the overall effect of differences in entry regulation, since all estimates are relative to a benchmark value of the “natural” rates of entry within industries.

This paper uses within country micro-level data to examine the effect of entry regulation on economic outcomes. Specifically, it exploits cross-municipality and cross-time variation in a recent business registration reform in Mexico to measure the effect of this reform on registration, employment, and income, which allows for establishing causality more convincingly than cross-country studies (See Pande and Udry, 2005, for a call to move to research of this type). From a policy perspective, it is also important to study the impact of reforms, since some policy institutions, such as the World Bank, promote simplification of business registration.

The use of micro data also makes it possible to trace out the effects of the reform on the functioning of the product and labor markets. Many economists have argued that barriers to entry harm consumers by raising prices and thwarting employment growth. This paper first examines whether the reform led to the creation of new firms or merely to the registration of existing informal businesses<sup>3</sup>. Having shown that it led to the creation of new businesses, the paper traces out the impact of this increase in competition on consumer prices, incumbents’ income, and employment.

The paper starts by building a simple model describing the expected effects on the product and labor markets when the cost of registration drops. In the model, high cost of registration prevents individuals with medium range ability from opening a formal business. Therefore,

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<sup>1</sup>For example, Loayza, Oviedo, and Servén (2005) and Djankov, McLiesh, and Ramalho (2006) provide evidence that countries with less regulation grow faster.

<sup>2</sup>Klapper, Laeven, and Rajan (2006), as well as Fisman and Sarria-Allende (2004), show that countries with heavier entry regulation have lower firm entry and lower growth in value added in naturally high-entry industries.

<sup>3</sup>Registration of existing informal businesses may still result in an increase in production and efficiency even if no new businesses are created due to the benefits of formality.

the reform leads to increased entry. Depending on the assumption about returns in the informal sector, the increase in entry comes either from informal business owners registering their business or wage earners opening new businesses. The model also predicts that increased entry leads to a decrease in prices and to a decline in income for incumbent businesses.

The paper then tests the predictions of the model using the Mexican reform. The identification strategy used to estimate the effects of the reform relies on the fact that implementation of the business registration reform in Mexico varied across municipalities and across time. The reform was organized by a federal agency, the COFEMER. This agency had to coordinate with municipality governments on implementing the reform since many business registration procedures are set locally in Mexico. COFEMER's goal was to bring the reform to urban municipalities that had the largest volume of economic activity in Mexico. However, due to staff constraints, COFEMER could not implement the reform in all the priority municipalities at the same time. Moreover, they did not specify a particular pattern of implementation within the set of priority municipalities. A number of checks suggest that the order of implementation was indeed exogenous to the outcomes in this set of municipalities.

The reform reduced registration procedures from 8 on average to less than 3. To give a sense of the magnitude of this reform, the reduction corresponds to going from the 30th percentile in registration procedures to the 2nd percentile, or equivalently it corresponds to going from Peru or Pakistan to New Zealand or Australia. It is important to note that only firms in low-risk to society<sup>4</sup> and unregulated industries were eligible for the reform. These eligible industries encompassed approximately 80 percent of pre-reform firms.

The results show that the reform increased the number of registered businesses by 5 percent in eligible industries, supporting the finding of the cross-country literature that lower regulation leads to more entry. The increase in the number of new businesses came exclusively from former wage earners opening businesses. Informal (non-registered) business owners were not more likely to register their business after the reform. The results also show that employment in eligible industries increased by 2.8 percent after the reform. In particular, people who were previously unemployed or out of the labor force were more likely to work as wage earners after the reform.

By increasing competition, the reform benefitted consumers and hurt incumbent businesses. First, using the Mexican CPI as an outcome variable, I find that the reform decreased the price level by 0.6 percent. The fact that the price decline was concentrated among low-risk industries in the non-tradable goods sector indicates that this was due to competition. Second, the income of incumbent registered businesses declined by 3.2 percent. Some of the evidence also suggests

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<sup>4</sup>Low-risk industries are industries that do not present a serious risk to public health, public security, or the environment. An example of a high-risk industry is chemical production.

that the income of previous wage earners decreased after the reform, possibly because business owners passed on the decline in prices to their workers. Interestingly, the results do not show an increase in income for the wage earners who opened a business. One possible explanation is that these new entrepreneurs are still paying off the fixed cost of opening their business during the period of observation. Previous wage earners are observed for a maximum of 4 quarters after the reform. Finally, the income of the previously unemployed and out of the labor force increased after the reform, by about 6 percent.

Overall, this paper illustrates that the effects of a business registration reform are positive and potentially important. These findings match related within-country studies on the impact of reforms. Bertrand and Kramarz (2002) examine what happens to employment growth in the retail trade industry in France after the introduction of zoning regulation which restricts the establishment of large retail stores. Exploiting regional variation in the enforcement of this regulation, they show that stronger deterrence of entry decreased employment growth. Moreover, the World Bank's Doing Business reports include suggestive evidence on the positive impact of business registration reforms. Doing Business in 2005 states that the top 5 reformers in 2003 - Ethiopia, France, Morocco, Slovakia and Turkey - have experienced higher increases in new registrations than the OECD average. Doing Business in 2006 documents that business entry jumped up in several countries by 10 to 42 percent following a reform of registration procedures. The findings of this paper also lend support to the recent attention that policy institutions and governments have paid to business registration reform as a possible vehicle to foster economic development.

Concurrently to this paper, Kaplan, Piedra, and Seira (2006) have investigated the effect of the same business registration reform in Mexico on firm creation. A key difference to my paper is that they use registration and employment data from the Mexican Social Security Institute (IMSS), while my paper uses household data from the Mexican Labor Market Survey. One disadvantage of the IMSS data is that it does not capture registered firms without employees since owners do not typically register themselves with social security<sup>5</sup>. Another disadvantage is that not all registered firms with employees are in the IMSS database since a significant fraction of owners does not register their workers with the IMSS. Consequently, the estimated increase in the number of registered firms in Kaplan, Piedra, and Seira is 7.6 times smaller than the increase in the number of registered firms estimated in this paper. Another difference to Kaplan, Piedra, and Seira is that I am able to provide direct evidence that newly registered firms are not previously existing informal firms, but instead new firms created by former

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<sup>5</sup>According to my data, in the pre-reform period, 40 percent of registered businesses and 79 percent of unregistered businesses had no employees. Among registered firms without employees, less than 0.2 percent of owners were also registered with IMSS.

wage earners. Furthermore, Kaplan, Piedra, and Seira's data does not include information on income. My paper, however, identifies the effect of the reform on the income of different pre-reform occupation groups.

The rest of this paper is organized as follows. Section 2 describes the business registration reform. Section 3 develops a simple occupational choice model which provides the framework for analyzing the effects of the registration reform. Section 4 discusses the identification strategy and Section 5 describes the Mexican employment survey data. Section 6 presents the empirical results. Section 7 concludes.

## 2 The Mexican Rapid Business Opening System Reform

According to Djankov et al (2002), in 1999, the number of procedures required to register a business in Mexico was 15 and the number of days was 67. Both numbers were above the cross-country average (10 and 48 respectively)<sup>6</sup>. Realizing that Mexico had rather heavy regulations by international comparison, in 2000, the Mexican government created the Federal Commission for Improving Regulation (COFEMER), charged with providing information about the state of regulation across Mexico and implementing possible reforms.

The COFEMER suggested a reform to simplify business registration procedures with the goal of stimulating investment and economic growth. Following this proposal, on March 1, 2002, the Mexican government passed a federal law stating that the number of federal procedures required for starting operation of most businesses should be reduced to a maximum of two procedures that could be administered within 72 hours. These two procedures are obtaining a tax payer number and incorporating the business in case it is a corporation. Once a firm starts operating, it has three months to take care of the other federal requirements that may apply, such as registering workers for medical insurance. This reform applied only to non-governmental firms in industries which do not require special permits or concessions and which do not present a serious risk to public health, public security, or the environment. These eligible "low-risk" industries made up 55 percent of all industries and 80 percent of operating firms, typically micro, small or medium size businesses. Another 10 percent of industries were governmental and 35 percent were classified as "high-risk" or regulated. Appendix A lists examples of low-risk and high-risk/regulated industries as classified by the COFEMER. Examples of low-risk industries are commerce and restaurants. Examples of high-risk or regulated industries are chemical production and transportation (including taxis).

However, simplifying federal regulations was not enough, since there were additional state

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<sup>6</sup>The minimum number of procedures (days) was 2 (2) both in Canada and Australia. The maximum number of procedures (days) was 21 in the Dominican Republic (152 in Madagascar). These numbers refer to business registration in the largest city of each country. For Mexico, this is Mexico City.

and municipal procedures required for starting a business. These procedures typically varied from state to state and municipality to municipality. Having simplified federal regulations, the COFEMER then approached state and municipal governments to suggest that they cut down on local regulations and that they implement one-stop-shop centers where entrepreneurs could take care of federal, state, and municipal procedures at the same time. The COFEMER's goal was to create a Rapid Business Opening System (SARE) in Mexico's most populous and economically important urban municipalities in order to quickly reach a large number of people and a large fraction of economic activity with the reform. However, the COFEMER was not able to bring a SARE to all those municipalities at the same time since it had limited resources. There were only four people within the COFEMER working on spreading the reform to local governments. Consequently, the SARE was implemented in different municipalities at different times, starting in May 2002. By September 2006, 103 municipalities had a SARE and another 13 municipalities were in the process of setting up a SARE<sup>7</sup>.

The SARE was successful in simplifying local business registration procedures. Table 1 shows summary statistics for business registration procedures before and after the reform for a sample of 32 municipalities from 17 different states. The averages for the number of days, procedures and office visits required to register a business all decreased significantly, falling from 30.1 to 1.4, from 7.9 to 2.7 and from 4.2 to 1, respectively<sup>8</sup>. The standard deviations of all three measures also became much smaller, implying relatively small differences in business registration procedures across municipalities after the reform.

I have fairly detailed administrative data on licenses issued in 2004 from the registration center in one of the municipalities which adopted the reform in 2003, Guadalajara. Looking at these data provides an insight into what types of businesses registered after the reform. Guadalajara reports that a total of 16,631 businesses were created in 2004, corresponding to an investment of US\$ 90,997,003 and to 21,170 new jobs created. The average investment was thus US\$ 5,471 and the average employment per firm was 1.27. The most frequent business types were video game console rental, computer rental, small grocery stores, clothing stores, home-style food-to-go vendors, and beauty salons.

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<sup>7</sup>There are 2454 counties in Mexico, but 94 percent of the population and 98 percent of economic activity are concentrated in 450 counties. These 450 counties include 99 of the 103 counties which had a SARE by September 2006. The SARE counties contain 33 percent of the population and 47 percent of economic activity. The four counties which are not part of the 450 biggest counties, but have a SARE, implemented the reform when other counties in the same state implemented it.

<sup>8</sup>These pre-reform data are different from the data in Djankov et al since the latter are for Mexico City only. The 32 counties for which COFEMER reports data don't include any county from Mexico City since these counties have not implemented reforms.



### 3 A Simple Occupational Choice Model

To have a framework for analyzing the effects of the business registration reform, I develop a simple occupational choice model. The model generates a division of the population into three occupational groups: wage earners, informal (non-registered) business owners and formal (registered) business owners. An important issue is how we should model informal business owners. There are two different views in the literature. One view is that informal business owners are people who can not be wage earners since their ability is too low. Therefore, they set up a micro firm in the informal sector. Under this view, the informal sector is a residual sector (See for example Loayza, 1994). The other view says that informal business owners have relatively high ability. However, their ability is lower than the ability of formal business owners, and they operate in an unregulated environment since the costs of formality are high (See Levenson and Maloney, 1998, and Maloney, 2004). The latter view is largely consistent with Hernando de Soto's view. He argues, based on anecdotal evidence, that complicated and costly business registration procedures have caused "two-thirds of the world's population [to be] locked out of the global economy: forced to operate outside the rule of law, they have no legal identity, no credit, no capital, and thus no way to prosper."

The model considers two alternative assumptions about returns in the informal sector, which generate occupation divisions corresponding to each of the two views. Both assumptions give the same predictions about the overall effects of the business registration reform. However, the predictions about the effects on wage earners and informal business owners are different across the two models. I test these predictions in Section 6. The reform thus helps to discriminate between the two views described above.

#### 3.1 The Environment

The economy lasts for one period and is populated by a continuum of individuals of mass one. Individuals have strictly increasing preferences over consumption,  $u(c)$ . They have one unit of labor they can supply, and they have to choose between becoming a wage earner, setting up a formal (registered) firm, or setting up an informal (unregistered) firm.

Each individual is endowed with an ability level,  $x$ , drawn from a uniform distribution  $H(x)$  with support  $[0, 1]$ . Wage earners are employed by formal firms and get a wage  $w(x)$ .

Each formal firm employes exactly one worker. The production function for formal firms,  $y(x, x_w)$ , depends on the ability level of the owner,  $x$ , and the ability level of the worker,  $x_w$ . In particular, I assume

$$y = \gamma x + x_w,$$

where  $\gamma > 1$ . Formal firms face a downward sloping demand curve for the good they produce, such that the price of this good is a function,  $P(Y)$ , of the aggregate quantity produced,  $Y$ , where  $\frac{dP(Y)}{dY} < 0$ . Note that the assumption that prices decrease with aggregate output should be true only for non-tradable goods, but not for tradable goods. I assume that the price is measured in terms of a second good that all agents produce at home and that plays no role in the labor market or in firm production. The home good is thus the numeraire.

There is a fixed cost,  $F$ , of registering a formal firm, implying that formal firms' profits are

$$\pi(x, x_w, P(Y), F) = P(Y)[\gamma x + x_w] - w(x_w) - F.$$

Since formal firms are relatively small, they do not consider the impact their output has on aggregate output and the price in their decision to produce.

Profit maximization implies that

$$w'(x_w) = P(Y),$$

such that the marginal increase in wage for an additional unit of ability is equal to the price. This implies that the wage of a worker of ability  $x$ ,  $w(x)$ , is of the form  $P(Y)x - k$ , where is a  $k$  constant that clears the labor market.

Informal business owners produce the same good as formal firms, but they have a different production function,  $y = \mu x$ , where  $0 < \mu < \gamma$ . The assumption that owner's productivity is higher in the formal sector is based on the argument that firms in the formal sector have access to legally enforceable agreements, to government support programs, and to new and lower cost sources of financing (see Jansson and Chalmers, 2001). They also avoid paying government penalties and can expand without fear of being detected. Moreover, McKenzie and Sakho (2007) show that a major benefit of being formal is that it enables firms to issue tax receipts and thereby expand their customer base.

In this model, informal business owners work alone. This fact generally matches my data. In the pre-reform period, 79 percent of informal business owners work alone, another 20 percent have one employee and only 1 percent have more than one employee. In contrast, only 40 percent of formal business owners work alone, 50 percent have one employee and another 10 percent have more than one employee.

Agents choose their occupation by maximizing utility subject to their ability constraint. In this economy, utility maximization is equivalent to income maximization, where the income from the different types of occupations,  $I(x, P(Y), F)$ , is given by

$$I(x, P(Y), F) = \begin{cases} P(Y)\mu x & \text{for informal business owners,} \\ P(Y)x - k & \text{for wage earners,} \\ P(Y)\gamma x + k - F & \text{for formal business owners.} \end{cases}$$

Depending on the size of returns in the informal sector,  $\mu$ , the resulting occupation division matches one of the two different views of informal businesses mentioned above. If returns in the informal sector are low, such that  $\mu < 1$ , then the resulting occupation division is consistent with the residual sector view. If returns in the informal sector are high, such that  $1 < \mu < \gamma$ , then the resulting occupation division matches the De Soto view.

The following subsections discuss the occupation divisions and equilibria under the different assumptions about productivity in the informal sector.

### 3.2 Occupational Choice with a Low Productivity Informal Sector

If  $\mu < 1$ , then income from wage work exceeds income from informal businesses for levels of  $x$  higher than a cutoff,  $\bar{x}_w$ . This threshold level is defined by  $P(Y)\mu\bar{x}_w = P(Y)\bar{x}_w - k$ , and agents with ability lower than  $\bar{x}_w$  become informal business owners.

Agents with ability higher than  $\bar{x}_w$  have to choose between becoming wage earners and formal business owners. Holding the price fixed, a firm's profits are strictly increasing in  $x$ , which implies that only agents with relatively high ability,  $x$ , choose to become formal business owners. In particular, there exists a threshold level,  $\bar{x}$ , such that for values of ability above this level profits exceed the income of wage earners. This threshold level  $\bar{x}$  is defined by  $P(Y)\gamma\bar{x} + k - F = P(Y)\bar{x} - k$ . Given this threshold level, the occupation division is as follows

1. Agents with ability above  $\bar{x}$  become formal business owners.
2. Agents with ability between  $\bar{x}$  and  $\bar{x}_w$  become wage earners.
3. Agents with ability below  $\bar{x}_w$  become informal business owners.

Figure 1 depicts income and the occupation division as a function of ability under the assumption that firms in the informal sector have low productivity.

### 3.3 Equilibrium with a Low Productivity Informal Sector

Aggregate labor demand is given by

$$L(P(Y), F) = \int_{\bar{x}(P(Y), F)}^1 h(x) dx.$$

Labor supply has to equal demand, such that

$$\int_{\bar{x}_w(P(Y))}^{\bar{x}(P(Y),F)} h(x)dx = \int_{\bar{x}(P(Y),F)}^1 h(x)dx.$$

This condition closes the model since it determines the value of the constant  $k$  that clears the labor market.

Aggregate output is given by

$$Y(P(Y), F) = \int_{\bar{x}(P(Y),F)}^1 \gamma x h(x) dx + \int_{\bar{x}_w(P(Y))}^{\bar{x}(P(Y),F)} x h(x) dx + \int_0^{\bar{x}_w(P(Y))} \mu x h(x) dx.$$

Note that both labor demand and aggregate output depend on the price level and the fixed cost of registration.

### 3.4 Occupational Choice with a High Productivity Informal Sector

If  $1 < \mu < \gamma$ , then income from an informal business exceeds the income from wage work. Therefore, the cutoff level of ability  $\bar{x}$  is now the cutoff between formal business owners and informal business owners. This cutoff level,  $\bar{x}$ , is defined by  $P(Y)\gamma\bar{x} + k - F = P(Y)\mu\bar{x}$ .

Note that, under the assumption that  $1 < \mu < \gamma$ , formal firms need to share more of their revenues with their workers than they do when  $\mu < 1$ , such that  $k$  becomes negative in equilibrium. Otherwise, all agents with  $x$  smaller than  $\bar{x}$  would prefer to be informal business owners since  $\mu x > x - k$  would be true for each level of  $x$  if  $k$  were positive. The threshold level,  $\bar{x}_w$ , now represents the cutoff level of ability between workers and informal business owners. This threshold level is given by  $P(Y)\mu\bar{x}_w = P(Y)\bar{x}_w - k$ .

The occupation division is as follows

1. Agents with ability above  $\bar{x}$  become formal business owners.
2. Agents with ability between  $\bar{x}$  and  $\bar{x}_w$  become informal business owners.
3. Agents with ability below  $\bar{x}_w$  become wage earners.

Figure 2 depicts income and the occupation division as a function of ability under the assumption that firms in the informal sector have high productivity.

### 3.5 Equilibrium with a High Productivity Informal Sector

In equilibrium, labor demand is again given by

$$L(P(Y), F) = \int_{\bar{x}(P(Y),F)}^1 h(x)dx.$$

Labor supply has to equal demand, such that

$$\int_0^{\bar{x}_w(P(Y))} h(x)dx = \int_{\bar{x}(P(Y),F)}^1 h(x)dx.$$

This condition again determines the value of the constant  $k$  that clears the labor market.

Aggregate output is now given by

$$Y(P(Y), F) = \int_{\bar{x}(P(Y),F)}^1 \gamma x h(x)dx + \int_{\bar{x}_w(P(Y))}^{\bar{x}(P(Y),F)} \mu x h(x)dx + \int_0^{\bar{x}_w(P(Y))} x h(x)dx.$$

Both labor demand and aggregate output depend on the price level and the fixed cost of registration.

### 3.6 Differences in Occupation Division Across Assumptions

Note that the main differences generated by the different assumptions about productivity in the informal sector lie in the relative income and ability levels of wage earners and informal firms. Under the low productivity assumption, informal businesses earn less than wage earners and are also of lower ability. Under the high productivity assumption, informal businesses earn more than wage earners and are of higher ability.

To gauge which assumption fits the facts better, I look at averages in log monthly income and income residuals for different occupation groups from my pre-reform data. Table 2 reports these statistics for informal (non-registered) business owners, wage earners, and formal (registered) business owners. Registered business owners have the highest income on average (8.32), followed by wage earners (8.01), and finally non-registered businesses (7.52). The income residuals are the residuals of a regression that includes age, education dummies, a gender dummy, and a marital status dummy, as well as time and municipality dummies. The ranking of the average income residuals is the same as the ranking of average income, with registered businesses having the highest income residuals, followed by wage earners, and then non-registered businesses.

The pre-reform data thus suggest that the low productivity assumption is more appropriate, meaning that informal business owners are low-income/low-ability individuals who are in this occupation for the lack of better options. The data do not support the view that informal business owners are medium-range-ability individuals who choose informal production over wage work.

The next subsection analyzes what happens under each assumption when the cost of registration drops. I test the predictions generated by both assumptions in Section 6.

### 3.7 Comparative Statics

The business registration reform corresponds to a reduction in the fixed cost of registration,  $F$ . This decrease in  $F$  has a number of effects in the model. These effects are depicted in Figures 3 and 4, where  $F'$  is the decreased level of  $F$ , such that  $F' < F$ . The first effect under both assumptions is that the profit line for formal firms shifts up, such that potential formal firm profits are higher for each ability level. This means that the threshold level  $\bar{x}$  moves to the left.

As  $\bar{x}$  moves to the left, there is entry into the registered business sector, which increases aggregate output and decreases the price,  $P$  (as mentioned before, the decrease in  $P$  should only be observed in the non-tradables sector). As a result, the profit line of formal firms rotates downward, and  $\bar{x}$  shifts to the right. The wage line also rotates downward, which moves  $\bar{x}$  to the left. Overall,  $\bar{x}$  has to shift to the left. It is not possible for the decrease in  $P$  and the rotation in the profit line to shift  $\bar{x}$  back its original value or to the right. In that case, there would be no entry, or there would be exit, and  $P$  would not decrease in the first place. The decrease in the price implies that incumbent formal businesses see a decline in revenues. The income of informal businesses also declines.

As the number of formal businesses increases, labor demand increases by the same amount. The constant  $k$  then adjusts down to give an incentive to informal business owners to become wage earners. This decline in  $k$  further contributes to the decline in income of formal business owners. The effect on the income of agents who remain wage earners after the reform is ambiguous. The decrease in the price lowers their income, but the decline in  $k$  increases their income.

To summarize, the model predicts that the registration reform should lead to

1. An increase in the fraction of registered businesses.
2. An increase in the number of wage earners and total employment in the formal sector.
3. A decrease in the price level.
4. A decrease in revenues for incumbent formal businesses and for informal businesses.

Under the low productivity assumption, the agents who open a registered business are former wage workers. Therefore, we should observe that

1. Agents who open a registered business are previous wage earners.
2. The wage earners who switch are the ones with the highest ability.

3. Income increases for the wage earners who open a business.

Under the high productivity assumption, it is not wage earners who open registered businesses. Instead, informal business owners register their business. In that case, we should see that

1. Some informal business owners register their businesses.
2. The informal business owners who register are the ones with the highest ability.
3. Income increases for the informal business owners who register their business.

In Section 6, when testing the prediction in the data, I present evidence on the overall changes in the outcome variables after the reform and break down these effects by pre-reform occupation. However, before moving on to the empirical results, I discuss the identification strategy and the data in the following two sections.

## 4 Identification Strategy

This paper uses the cross-municipality and cross-time variation in the implementation of the reform to determine its effect. The data cover all quarters from the second quarter of 2000 to the fourth quarter of 2004. I restrict my sample to the 34 municipalities that adopted the reform by December 2004<sup>9</sup>. This allows me to exploit only variation in the time of adoption, holding the decision to adopt fixed for all municipalities in my sample. One reason for not including municipalities that adopted after December 2004 or that have not yet adopted is that these municipalities become increasingly less comparable to the ones that adopted the reform early<sup>10</sup>. Moreover, the part of the analysis below that looks at the effects of the reform on different pre-reform groups requires me to observe individuals at least once before the reform was implemented.

A list of the municipalities which implemented the reform by December 2004 with respective implementation dates is provided in Appendix B. Figure 5 shows the geographic location of

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<sup>9</sup>In fact, 38 counties adopted the reform by December 2004, but I do not have data for four of them.

<sup>10</sup>Note that Kaplan, Piedra, and Seira (2006) look at a larger set of municipalities in their analysis. Their data set goes up to March 2006, instead of December 2004 as my data does, which allows them to see more municipalities being treated. However, this also implies that the set of municipalities becomes less comparable. In fact, their cross-municipality, cross-time estimates show a negative effect on firm registration while their cross-municipality, cross-time, cross-industry estimates show positive effects. They argue that this is due to the fact that the municipalities that adopted by December 2004 were affected more strongly by an economic slowdown that coincided with the implementation of the reform. There is, however, no direct evidence for this claim since output data at the municipality level is only available every five years in Mexico. These potential economic differences should not affect my analysis since all of my municipalities fall into the set of municipalities that adopted by December 2004. Moreover, as shown in Figure 6, rather than experiencing a slowdown in 2002 when the first municipalities implemented the reform, the Mexican economy actually started to recover from an economic downturn.

these municipalities. The map is coded in the following way. The 17 municipalities which adopted the reform early, between May 2002 and March 2004, are marked by circles. The 17 municipalities which adopted the reform late, between April 2004 and December 2004 are marked by triangles. Both early and late adopters are fairly dispersed throughout Mexico.

The fact that adoption of the reform varied across municipalities and across time makes it possible to control for municipality specific and time specific effects. The effect of the reform is thus identified using cross-municipality differences in the reform dummy over time. The identification strategy is valid as long as the changes in outcome variables over time would be similar across municipalities in the absence of the reform. In particular, the identification strategy may be violated if the implementation of the reform followed a specific pattern in terms of municipality characteristics that are related to changes in outcomes. For example, if the municipalities which experienced a high increase in registered businesses adopted the reform first, I might find an effect of the reform even if there was no effect. To gauge whether or not there was such a specific pattern of implementation, I performed a number of checks.

First, I interviewed several staff members at the COFEMER who are in charge of implementing the reform. They informed me that their goal was to bring the reform first to the urban municipalities that have the largest volume of economic activity in Mexico. However, within this set of municipalities they did not specify a particular pattern of implementation. In fact, they mentioned that all municipality governments they approached were very interested in adopting the reform. As discussed in Section 2, COFEMER was not able to implement the reform in all municipalities simultaneously since they did not have enough personnel.

Second, to check whether the 34 municipalities in my sample are indeed comparable, I examine data from the 1994 and 1999 Economic Census. Panel A of Table 3 presents averages for 1999 log GDP per capita, log economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita split-up by early and late adopters<sup>11</sup>. The comparison of early and late adopters in Column 3 of Table 3 shows that there are no statistically significant differences in terms of the number of economic establishments per capita, fixed assets per capita, and investment per capita. Log GDP per capita is higher on average for early adopters. To further examine the pattern of implementation of the reform, I regress each of the three Census variables on the quarter of implementation. The coefficients on quarter of implementation are reported in Column 4 of Table 3. While there is no statistically significant trend for changes in log employment per capita and log investment per capita, log GDP per capita and fixed assets per capita are higher for municipalities that adopted the reform earlier.

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<sup>11</sup>The GDP, establishment, fixed assets, and investment data come from the 1994 and 1999 Economic Censuses. To convert these numbers into per capita values I use population data from the 1995 Population Count and the 2000 Demographic Census.



The fact that log GDP per capita and fixed assets per capita tend to be higher for municipalities which adopted the reform earlier does not *per se* invalidate the identification strategy since the identification strategy relies on changes over time being similar across municipalities. Thus, Table 3 also compares changes in log GDP per capita, log economic establishments per capita, log fixed assets per capita, and log investment per capita from 1994 to 1999. The averages for early and late adopters are in Columns 1 and 2 in Panel B of Table 3. Columns 3 and 4 show that the changes in the Economic Census variables are not significantly different across early and late adopters and that they do not follow a specific pattern of implementation over time. This adds plausibility to the argument that changes of outcomes in these municipalities would have been similar in the absence of the reform. To further support the identification strategy, in the following section, I illustrate that there is no pattern of adoption related to pre-reform outcome data.

Finally, in the empirical strategy, I take into account that log GDP per capita and log fixed assets per capita differed across early and late adopters in 1999 by controlling for 1999 Economic Census variables interacted with a linear time trend. It is not possible to control for contemporaneous Economic Census variables or Economic Census variables closer to the year of first implementation (2002) since the Economic Census in Mexico is only conducted every five years and there is no higher frequency data on GDP at the municipality level. To explore Mexico's economic situation at the time around the reform in more detail, Figure 6 displays the time series of quarterly real GDP (in 1993 Mexican pesos). The time series shows that the Mexican economy experienced a downturn between 2000 and 2002. Then, GDP started increasing again, and it grew at higher rates from 2004 on. To account for these non-linear patterns and for the possibility that they may differ across municipalities, I re-ran the main regressions of the paper controlling for Economic Census variables interacted with a linear time trend and with time trends of higher powers, up to power 4. The results change only minimally and are not reported in the paper.

## 5 Mexican Employment Survey Data

My main outcome data comes from the Mexican National Employment Survey (ENE). The ENE is the survey that the Mexican government relies on for calculating unemployment statistics and for calculating the size of the informal sector. It has been conducted quarterly since 2000-II and covers a random sample of approximately 150,000 households. Each household remains in the survey for five consecutive quarters. I use data for 2000-II to 2004-IV (19 quarters in total). After 2004-IV, the ENE was changed to a new survey, whose results have not been made publicly available.

I choose the ENE data as my outcome data since it has five important features for this study. First, it covers almost all municipalities that implemented the reform. Second, the quarterly frequency of the data allows me to exploit the staggered implementation of the reform in the identification strategy. Third, the ENE includes detailed questions about a person's economic activity (including self-employment). Fourth, the ENE captures and distinguishes between formal and informal employment and firms. Fifth, the panel structure of the ENE allows me to investigate who is affected by the reform.

It is important to point out that the way the ENE sample is constructed implies that a municipality-year average is not necessarily representative of the municipality in that year. The sample selection procedure randomly selects households at a geographic unit, the AGEB (Basic Geo-Statistical Area), that is smaller than the municipalities in my sample. All AGEBs within a state are first stratified by socioeconomic characteristics. Within each strata, a certain number of AGEBs is chosen at random. Then, households are chosen at random within the AGEB. This procedure implies that it could happen that only some socioeconomic groups get selected in a given municipality in a given year. However, since the strata are randomly chosen, this remains in expectation a random sample of the households in a municipality, so that the estimate should remain unbiased. Moreover, the survey includes several different strata for most of the municipalities in my sample.

An alternative to using ENE data would have been to use administrative data from business registries. This data is, however, not readily available for Mexico since municipalities do not follow a unified system of recording registered firms. Many records are paper records held only at the local registry and are not publicly available. Moreover, some municipalities updated to an electronic system after the reform, suggesting that the reform also impacted record keeping, which may make pre- and post-reform records incomparable. Finally, unlike the ENE, registry data would not include pre-registration information on firms, such as whether they existed in the informal sector.

Since I am looking at labor market outcomes, I keep only individuals of working age (between the ages of 20 and 65) in my sample. I construct three of my main outcome variables by creating dummy variables for each person in the sample, which indicate whether the person a) is employed, b) is a wage earner, and c) owns a registered business<sup>12</sup>. The later dummies encompass subcategories of the earlier dummies, in the sense that everybody who is employed can either be a wage earner or a business owner (I include the self-employed in the category business owners). Business owners can either be registered or unregistered. However, since all dummies are defined for the whole sample, they denote the fraction of all individuals in my

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<sup>12</sup>The Appendix includes a description of how I constructed these dummy variables, paying particular attention to the way business registration is measured in the ENE and explaining some caveats.

sample who fall into each category, not the fraction of people in the preceding category who fall into each subcategory. For example, a value of 0.08 of the registered business owner dummy means that 8 percent of all people own and operate a registered business, not 8 percent of employed individuals.

I create six more dummy variables which split up the employed dummy, the wage earner dummy, and the registered business dummy into low-risk and high-risk registered businesses, using the industry information provided in the ENE and matching it to the low-risk and high-risk/regulated industries as specified by COFEMER. Finally, I also use monthly income as an outcome variable. While the dummy variables are defined for everybody in my sample, income is available only for the gainfully employed and is missing for the unemployed, for individuals who are out of the labor force, and for unpaid workers.

The upper panel of Table 4a includes summary statistics for the outcome variables split up by early and late adopter municipalities, where these groups are defined as in Table 3 with early (late) adopters adopting the reform between May 2002 (April 2004) and March 2004 (December 2004). The data in Table 4a are for the pre-reform period, including only observations between 2000-II and 2001-IV. The averages in Columns 1 and 2 of Table 4a show that 66 percent of the people in my sample are employed<sup>13</sup>. Individuals who are employed fall into three categories - wage earners (who make up 50 percent of the population), registered business owners (8.4 percent) and non-registered business owners (7.9 percent). Most registered business owners are in low-risk/unregulated industries (7.3 percent), with only 1.1 percent of the population owning registered high-risk/regulated businesses.

The third column of Table 4a reports the differences in pre-reform averages for early and late adopters. The differences are small and not statistically significant, except for log monthly income. Income is 17.7 percent higher in early adopter municipalities. However, as Column 4 of Table 4a shows, none of the outcome variables are systematically increasing or decreasing in the quarter of implementation.

As explained above, the required assumption for my estimations to be valid is that the changes of the outcome variables would not have differed systematically between treatment and control municipalities during the treatment period if the reform had not occurred. While this assumption is fundamentally untestable, it is likely to hold if there are no initial systematic differences in trends. To verify this, I report pre-period changes in outcome variables in Table 4b. The changes are calculated as 2000-IV to 2001-IV changes in municipality averages for each

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<sup>13</sup>Labor force participation is about 68 percent. The unemployment rate is at a low 2 percent. Measured unemployment tends to be low in Mexico since individuals count as employed even if they worked only one hour during the past week. There is also a very small fraction of people who I count as being in the labor force who are neither currently employed, nor unemployed, but who have a job lined up that they are planning to start within the next month.

variable. Most average changes in outcome variables are not statistically different between early and late adopter municipalities. They are also not significantly correlated with the quarter of implementation, suggesting that the identification strategy is valid. The one exception is the average change in employment. Municipalities that adopted earlier appear to have smaller increases in overall employment. This would bias the analysis against finding an effect of the reform on overall employment.

I also use a number of individual background variables from the ENE as control variables in my regression. These variables include age, gender, marital status, and education dummies. Summary statistics for these background variables and their pre-period changes are listed in the lower panels of Table 4a and 4b. The background variables are very similar across early and late adopter municipalities. The only statistically significant difference in Table 4a is in the fraction of females, where early adopter municipalities have about one percent fewer females.

## 6 Results

In this section, I put to test the predictions of the model developed above and analyze the effects of the reform on average labor market outcomes. I also break down these effects by pre-reform occupation and examine the impact of the reform on prices.

According to the identification strategy described in Section 4, I obtain the main results by running the following regression with OLS

$$y_{ict} = \alpha + \beta_c + \gamma_t + \delta SARE_{ct} + \pi Z_{ict} + \phi EC_{1999} * t + \varepsilon_{ict},$$

where the subscript  $i$  denotes individuals,  $c$  denotes municipalities, and  $t$  denotes quarters. This regression includes municipality fixed effects,  $\beta_c$ , and quarter fixed effects,  $\gamma_t$ . The variable  $SARE_{ct}$  is the reform dummy and, for each municipality, it is equal to one for the quarter in which the reform was implemented and for all following quarters. I run the regression without and with the individual background variables,  $Z_{ict}$ . When including individual background variables, I also include variables from the 1999 Economic Census ( $EC_{1999}$ ) interacted with a linear time trend,  $t$ . These variables are log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita<sup>14</sup>. Note that I do not include individual dummies or time trends interacted with municipality dummies in my regressions since they take out a lot of the variation in the reform dummy.

The standard errors of the regressions are clustered at the municipality level. As explained in Bertrand, Duflo, and Mullainathan (2004), clustering at the region level helps to prevent the problem that difference-in-difference estimates which use a large number of time periods

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<sup>14</sup>The economic variables are converted to per capita levels using 2000 Demographic Census population data.

severely understate uncorrected standard errors if the outcomes are serially correlated.

## 6.1 Registration

The first prediction of the model above is that the fraction of registered businesses should increase after the reform. Table 5 contains the regressions for the registered business dummy, which denotes the fraction of all people who own a registered business. Both, the specification without controls and the one with individual level and Economic Census controls, show a positive and significant impact of the reform on the number of registered businesses. The increase of 0.32 percentage points is equal to a 3.8 percent increase in registered businesses from the pre-reform level of 8.3 percent.

Columns 3 to 6 of Table 5 show the impact on registered businesses broken down by low-risk and high-risk businesses. Since only the low-risk businesses are eligible for the reform, the increase in registered businesses should only come from low-risk businesses. This is indeed what the results in Columns 3 to 6 confirm. The fraction of low-risk registered businesses increased by 0.37 percentage points (a increase of 5 percent from the pre-reform level of 7.4 percent), while there was no statistically significant change in high-risk registered businesses. Based on the fact that the 34 municipalities in my sample had a total population of 8,285,900 between ages 20 and 65, according to the 2000 Demographic Census, an increase of 0.37 percentage points in low-risk registered businesses corresponds to an increase in 30,678 firms for all 34 municipalities or 902 firms per municipality, on average. The total increase in the number of formal firms is 7.6 times bigger than the increase in formal firms found in Kaplan, Piedra, and Seira (2006), who estimate only 4,029 new formal firms. As explained above, this big difference may be due to the fact that Kaplan, Piedra, and Seira use registration data from the Mexican Social Security Institute (IMSS), which implies that their estimates don't capture firms that have no employees. They do also do not capture firms that obtain an operating license but do not register their workers with the IMSS.

The regression above estimates only the average effect of the reform on registration over the post-treatment period. However, it is also important to check how this effect looks over time. Figures 7 shows a plot of the coefficients of the following regression

$$y_{ict} = \alpha + \beta_c + \gamma_t + \sum \delta_l Quarter_{lc} + \pi Z_{ict} + \varepsilon_{ict},$$

where  $Quarter_l$  is a set of dummy variables for lag and lead quarters relative to the time of implementation in a given municipality. For example,  $Quarter_{-1}$  is equal to one for the last quarter before the reform was implemented. Since the implementation of the reform was phased in over time and since the data set covers quarters 2000-II to 2004-IV only, I do not observe the

outcome data for all lags and leads for all municipalities. For instance, the data set includes information on  $Quarter_{+5}$  only for the eight municipalities that implemented the reform in 2003 or earlier. For  $Quarter_{+6}$  the data cover only six municipalities, and for  $Quarter_{+7}$  and higher only two municipalities. Similarly, for the lags, the data cover all municipalities only for  $Quarter_{-8}$  and higher. For this reason, I limit the regression above to observations that fall between  $Quarter_{-8}$  and  $Quarter_{+6}$ .  $Quarter_{-8}$  is also the omitted quarter in the regression, to which all other quarters are being compared.

Figure 7 shows the coefficients on the lag and lead dummies for the regression with the low-risk registered business owner dummy as the outcome variable. The two lighter colored lines are the 95 percent confidence bands of the estimates. Between  $Quarter_{-8}$  and  $Quarter_{-1}$ , the estimated differences tend to be negative or close to zero. After implementation of the reform, the differences become positive, reflecting the increase in registration. The only exception to the positive differences after implementation is  $Quarter_{+5}$ . As mentioned above, the data set covers fewer municipalities for this lead quarter than for previous lead quarters. The decrease in registration may thus be driven by missing data rather than by a actually reversal of the effect of the reform. Note that the coefficient is much greater for  $Quarter_{+4}$  than for other post-reform quarters. To make sure that the result in the regression in Column 4 of Table 5 is not only driven by  $Quarter_{+4}$ , I rerun the regression without  $Quarter_{+4}$ . The coefficient drops slightly from 0.0037 to 0.0034, and it is significant at the 5 percent level.

To check whether the measured increase of 0.37 percentage points in low-risk registered businesses from Table 5 seems plausible, I use the 2004 administrative data from the business registration center in Guadalajara. First, I calculate how many new businesses the increase of 0.37 percentage points implies for Guadalajara. Given that Guadalajara had a population between the ages of 20 and 65 of 879,019 in 2000, and assuming that the number of additional businesses which came into being due to the reform was equal to the average 0.37 percentage points, this implies that 3,252 new businesses in Guadalajara were created due to the reform. Then, I compare 3,252 to the total number of licenses issued by the registration center in Guadalajara. The center issued 16,613 new licenses in 2004. Given that the reform was implemented in Guadalajara in May 2003, 16,613 is a lower-bound for the total number of businesses created since the reform was implemented. The regression results thus suggest that most of the new licenses issued since the reform in Guadalajara (13361 and up) would have been issued in the absence of the reform as part of the normal turnover in businesses, and that 3,252 of them were issued as a result of the reform.

As mentioned in Section 2, for some municipalities, the COFEMER reports statistics on pre-reform and post-reform registration procedures. These data are available for 27 municipalities

in my sample, and I use it to test whether the effect on registration and income is greater in municipalities with greater reductions in registration procedures. This test corresponds to the following regression

$$y_{ict} = \alpha + \beta_c + \gamma_t + \delta SARE_{ct} + \phi SARE_{ct} * (\Delta Procedures)_c + \pi Z_{ict} + \phi EC_{1999} * t + \varepsilon_{ict},$$

where  $\Delta Procedures$  is the reduction in the number of procedures in municipality  $c$ . Conditional on having the reform, the average reduction in the number of procedures was 5, with a standard deviation of 6. The minimum was 0 and the maximum 22<sup>15</sup>. Column 1 of Table 6 shows the regression results for the registration dummy, where the regression includes individual background variables and Economic Census variables interacted with a linear time trend. As expected, the coefficient on the interaction term of the reform and reduction in procedures is positive and significant, implying that the reform had a bigger effect on registration in municipalities that saw a higher reduction in procedures. For a municipality with the average reduction in procedures (5), the increase in registration was 1.4 percent.

The model from Section 3 predicts that the reform should affect individuals in different pre-reform occupations differently. To test these predictions, I make use of the panel structure of my data. For each individual, I create four dummy variables that specify which of four occupations they held when I first observed them in the pre-reform period. The four possible occupations are registered business owner, non-registered business owner, wage earner, and not employed (unemployed or out of the labor force)<sup>16</sup>. For example, for somebody who was a wage earner in the first period when I observed her, the "past wage earner" dummy is equal to one. I then drop the first period of observation for each person and run the following regression for the remaining periods

$$y_{ioct} = \alpha + \beta_c + \gamma_t + PastOccup_o + \beta_c * PastOccup_o + \gamma_t * PastOccup_o + \delta SARE_{ct} * PastOccup_o + \pi Z_{ioct} + \phi EC_{1999} * t + \varepsilon_{ioct},$$

which includes the reform dummy interacted with all five past occupation dummies. The regression also includes quarter, municipality and past occupation dummies,  $PastOccup_o$ , as well as the interactions of quarter with occupation dummies and time with occupation dum-

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<sup>15</sup>The minimum of 0 only applies to three counties. In these counties, the number of procedures was already low pre-reform. However, the reform was still effective in these counties since it reduced the number of days it takes to register a business in all three counties. In two of the counties the reform also reduced the number of office visits.

<sup>16</sup>In the category "not employed", I also include the small fraction of people who say that they are not currently working, but have a job lined up that they are planning to start within one month.

mies. Finally, it includes the set of individual background characteristics,  $Z_{ioct}$ , and Economic Census variables,  $EC_{1999}$ , interacted with a linear time trend. Individuals who are never observed in the pre-reform period are dropped from this analysis, which makes the sample smaller than the one used to determine the main effects above.

Panel A of Table 7 presents the regression results for this analysis. Column 1 shows that past informal business owners are no more likely to register their businesses after the reform. The effect on past wage earners, on the other hand, is positive and statistically significant at the 15.9 percent level. The coefficient may not be more statistically significant, because, as explained in the previous paragraph, the sample is smaller, lowering power.

Panel B of Table 7 shows the effect on past wage earners, split up by whether they were low-risk or high-risk wage earners. The effect is greater for low-risk wage earners, indicating that individuals who started a low-risk business after the reform were mostly working in low-risk sectors before the reform. Overall, the findings in Column 1 of Table 7 are consistent with the assumption of low productivity of the informal sector in the model in Section 3, and they are inconsistent with the high productivity assumption. The results thus suggest that the low-ability (residual sector) view of informal business owners is more appropriate than the De Soto view.

The model with the low productivity assumption also predicts that the wage earners who open a registered business should be the ones with the highest ability levels<sup>17</sup>. To test this, I would need an estimate of their ability. Unfortunately, the dataset does not include any good proxies for wage earners' ability. The only characteristic in the dataset that is specific to wage earners is whether a wage earner has a written contract or not. About 67.4 percent of wage earners had a written contract in the pre-reform period. In order to examine the effect on wage earners in more detail, Table 8 shows the effect broken down by past wage earners who had a written contract and who did not. The effect is four times bigger for wage earners who did not have a written contract and is statistically significant for this group.

Table 9 examines the effect on different groups of non-registered business owners. For non-registered business owners, the data include information on the number of employees in the firm. This variable is a possible proxy for entrepreneurial ability. Although 79.6 percent of informal business owners work alone, some do have employees or partners, and the ones who do may be the higher ability owners. Panel A of Table 9 reports the effect on non-registered business owners who work alone and on non-registered business owners who have either partners or employees in the business. The coefficients are highly statistically insignificant for both

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<sup>17</sup>A limitation of the model and its predictions is that they are based on the quite restrictive assumption that the ability relevant for wage work is the same as the ability relevant for opening a business. In future work, it would be interesting to estimate a full Roy model of occupational choice to determine whether the people who open a business are the ones with the highest return to opening a business.



groups, implying that none of these groups is more likely to register after the reform. Panel B of Table 9 illustrates that non-registered business owners who operate on fixed premises (representing only 4 percent of informal business owners), as opposed to conducting their business in mobile stands or going door-to-door, are also not more likely to register their businesses after the reform.

## 6.2 Employment

The model from Section 3 also predicts that the reform leads to an increase in the number of wage earners. Table 10 reports the regression for the wage earner dummy. Columns 3 and 4 show that the fraction of wage earners increased in low-risk (eligible) industries. This increase in the fraction of wage earners of 0.64 percentage points corresponds to an increase of about 2 percent over the pre-reform fraction of low-risk wage earners. Dividing the increase in the fraction of wage earners (0.64) by the increase in firms (0.37) gives an average firm size of about 1.7 for newly created firms. This number is much smaller than the average size of new firms calculated in Kaplan, Piedra, and Seira (2006), which is 6.3. Since larger firms may be more likely to register their workers with the IMSS, it is perhaps not surprising that Kaplan, Piedra, and Seira capture larger newly created firms on average.

In the model, overall employment in the formal sector also increases since more agents become formal business owners and wage earners. Table 11 examines the impact of the reform on employment. Employment in low-risk industries went up by 1.24 percentage points, which corresponds to a 2.8 percent increase over the pre-reform low-risk employment level.

Figures 8 and 9 show the effect on wage work and employment in low-risk industries broken down by lag and lead quarters relative to the quarter of implementation. These figures were constructed using the same methodology as for Figure 7. The changes in the fraction of wage earners and employment are close to zero until the quarter of implementation. From *Quarter*<sub>0</sub> or *Quarter*<sub>1</sub> on, both variables are significantly and increasingly higher than before.

Similarly to the fraction of registered businesses, the fraction of wage earners and employment in low-risk industries increased more in municipalities where the reduction in registration procedures was greater, as shown in Columns 2 and 3 of Table 6. In a municipality with the average reduction in procedures (5), the fraction of wage earners increased by 1.2 percent and employment increased by 1.1 percent.

In the model, the increase in wage earners comes from informal business owners. Column 2 in Panel A of Table 7 breaks down the effect on the fraction of wage earners in low-risk industries by pre-reform occupation type. The results show that instead of informal business owners, it was mainly individuals who were previously not employed (the unemployed or out of the labor force) who switched to being low-risk wage earners after the reform. The model could

not have predicted this effect since all agents in the model are employed. In the data, however, about 32 percent of the individuals do not participate in the labor force or are unemployed. The model in this paper is kept simple to emphasize what the reform in Mexico can say about the differing views of informal business owners. In order to capture more of the effects of the reform, the model would have to be more complex. Agents who are not employed could, for example, be introduced into the model if agents had different costs of participating in the labor market.

Another extension of the model for future work is the inclusion of non-eligible industries. In the model, the entire formal business sector is subject to the reform. However, as Tables 10 and 11 show, the overall fraction of wage earners and overall employment did not increase significantly. Instead the increase in wage earners and employment in low-risk (eligible) industries went along with a decrease in wage earners and employment in high-risk (non-eligible) industries. Column 3 in Panel B of Table 7 illustrates that the decrease in wage earners in high-risk industries is due to the fact that former high-risk wage earners are less likely to continue working in these industries after the reform. Instead, they move into low-risk industries, as shown in Column 2. The coefficients in Columns 2 and 3 in Panel A further suggest that all past occupation groups are less likely to be high-risk wage earners after the reform and are more likely to be low-risk wage earners instead. In order to interpret these effects, it is important to point out that the Mexican labor market is quite mobile, with large and symmetric flows among sectors and occupations (Maloney 1997). The reforms seem to have redirected some of these flows from high-risk sectors to low-risk sectors.

### 6.3 Prices

The model above predicts that entry of new registered businesses after the reform decreases prices. My measure of the price level is the Mexican consumer price index (CPI), which is constructed by the Bank of Mexico. Price data are only available at the city level, not at the municipality level. In Mexico, a city can consist of one or several municipalities. I thus convert the price data to the municipality level by assigning each municipality the price index of the city where it is located<sup>18</sup>. CPI data exists only for 20 out of 34 municipalities in the sample since the other 14 municipalities do not fall into cities for which the CPI is calculated. As opposed to the ENE data, which have a quarterly frequency, the price data come at a monthly frequency. The specification used to analyze the effect on the price level is thus

$$\log(CPI_{cm}) = \alpha + \beta_c + \gamma_m + \delta SARE_{cm} + \phi EC_{1999} * t + \varepsilon_{cm},$$

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<sup>18</sup>In my sample, only three counties lie in the same city (Guadalajara, Zapopan and Tlaquepaque in Guadalajara). These three counties are thus assigned the same price data. All other counties have unique observations.

where the CPI varies by municipality and month and the subscript  $m$  stand for months. The base month for the CPI is June 2002.

Table 12 presents the results for the price regression, for two different time spans. Both regressions include the same set of municipalities, which are only the municipalities that are in the ENE sample. Column 1 of Table 12 corresponds to the time span for which I have ENE data. As predicted by the model above, the coefficient on the reform dummy is negative. In the regression in Column 2, the time period covered goes up to May 2006, which is the last month for which I have the CPI data. The negative coefficient on the reform dummy is similar in magnitude and is statistically significant in this larger sample. The decrease in the consumer price level after the reform confirms the argument that lowering barriers to entry benefits consumers.

The coefficients in Table 12 indicate that the reform decreased the log price level by approximately 0.6 percent. To get a sense of the magnitude of the effect, I compare it to the average inflation rate from 2000 to 2001. This average inflation rate, calculated as the increase in the CPI from each month in 2000 to the same month in 2001, was 5.7 percent. The decrease in the log CPI of approximately 0.6 percent due to the reform is a decrease in inflation of approximately 0.6 percent. A back-of-the-envelope calculation thus suggests that the reform decreased inflation to about 5.1 percent from 5.7 percent.

If the decline in prices is indeed due to the reform, then the decline should be present only in industries that are eligible for the reform (low-risk industries). Moreover, the measured decline in prices should come entirely from the non-tradables sector. This is because the estimation strategy compares prices across municipalities. The prices of non-tradables may decrease locally, but the price of tradables should not be affected by an increase in the supply in some municipalities only<sup>19</sup>. Table 13 breaks down the CPI into tradables and non-tradables, by low-risk and high-risk industries. The results indeed show that prices declined significantly only for non-tradables in low-risk industries.

## 6.4 Income

The regression results for the last ENE outcome variable, monthly income, are reported in Table 14. This table displays the effect on log income by past occupation group. The model predicts that the reform should decrease the income of incumbent registered business owners. The results in Column 1 indeed show that nominal income decreased for past registered business owners, by 3.2 percent. Given that the reform led to a decrease in the price level, it is also important to examine the impact of the reform on real income, in order to check whether the

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<sup>19</sup>Even if these counties were big enough to cause the price of tradables to change, no-arbitrage would imply that there should not be any relative price difference for tradables across counties.

decrease in income is solely due to the price effect. Column 3 of Table 14 examines the effect on real income by past occupation group. As mentioned above, for real income, the analysis is restricted to a smaller sample since the CPI is only available for 20 of the 34 municipalities in my main sample<sup>20</sup>. Column 3 of Table 14 thus reports the effect on real income in the small sample. Although the effect on the real income of incumbent registered business owners is slightly smaller than the effect on their nominal income, it is statistically significant.

The model also predicts that the income of informal business owners goes down after the reform. The regressions in Table 14, however, show no statistically significant effect on the income of informal business owners. It may be that informal business owners are not affected by the decline in prices in the same way as formal business owners because the goods they are producing are differentiated from formal sector goods.

The results for the effect on employment in Subsection 6.2 indicate that the previously unemployed or out of the labor force were more likely to work as wage earners after the reform. These switchers should thus also have seen a increase in their income. When aiming to measure this increase, I cannot use log income as the outcome variable as is done in Table 14. Individuals who were initially unemployed or out of the labor force have zero income and thus have to be dropped from a log regression. I choose two different ways of including the zero income observations, which are reported in Table 15. First, Columns 1 through 3 display regressions where the outcome variable is the quadratic root of income. The quadratic root mimics the logarithmic function well for positive numbers. (See Thomas et al, 2003, who choose the quadratic root of income instead of log to include zero and negative yields). Second, Columns 5 through 6 of Table 15 report Tobit regressions. For the Tobit regressions,  $\log(0)$  observations are replaced with the smallest observed log income in the data. This value is then used as the lower bound for the Tobit. The quadratic root and the Tobit regressions show a significant increase in the income of the previously not employed. Nominal income for this group increased by approximately 6 percent.

The regressions in Table 14 and 15 show either no effect or a negative and significant effect on the average income of previous wage earners. The wage earners who opened registered businesses, however, should be earning more. There are several possible explanations for the zero or negative effect. First, wage earners who opened a business may still be paying off the fixed cost of their new business during the period of observation. When analyzing the effect on the income of past wage earners, the regressions include a maximum of four quarters after the reform was implemented. This is due to the fact that the labor market survey keeps

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<sup>20</sup>The second column of Table 12 contains the same nominal income regression as Column 1, but for the smaller sample. The comparison between Columns 1 and 2 illustrates that the effect on nominal income across samples is very similar for all past occupation groups.

individuals only for five quarters. Since the methodology requires one pre-reform period to define pre-reform occupation status, post-reform observations exist for at most four quarters. Second, wage earners who opened businesses may now understate their income (if they think that the statistical institute communicates information to the tax office, which to my knowledge they do not). Third, business owners may have passed on some of the decline in prices to the workers who did not open a new business but remained workers instead, lowering the average income of past wage earners.

Tables 14 and 15 have illustrated the effect on the income of different pre-reform occupation groups. Table 16 displays the effect of the reform on average income. The first column uses log income as the outcome variable. Panel A shows that the reform decreased nominal income by 1.8 percent on average. Panel B reports the nominal income regression for the smaller sample of municipalities for which price data is available, and Panel C reports real income regressions. The effect on log income is negative and significant in all three panels. These regressions thus pick up the effect of the decrease in income for incumbent registered business owners. However, the log regressions in Column 1 exclude zero incomes and may therefore be misspecified. Similarly to the regressions in Table 16, the regressions in Columns 2 and 3 of Table 16 aim at resolving this problem by using the quadratic root of income as an outcome variable and by running a Tobit. The quadratic root regressions no statistically significant change in average income. The Tobit regression in Panel A indicates a positive effect on average income (a 3.1 percent increase). The difference to the log income regressions comes from the fact that, in addition to picking up the decline in income for incumbent formal business owners and for some wage earners, the quadratic root and Tobit regressions pick up the increase in income for the previously not employed.

Finally, Column 4 of Table 16 presents another way of including zero income observations in the regression by looking at log aggregate municipality income per capita as the outcome variable. Aggregate municipality income per capita was constructed by summing up all income observations for a given municipality in a given quarter and then dividing by the number of people observed in that quarter. The aggregate income regression for the whole sample shows a negative but insignificant effect on income. In the regressions for the smaller sample, however, the effect is negative and significant.

Overall, the regressions do not show a robust effect on average income. They are, however, more informative on the income effects of the reform on different occupation groups. Incumbent business owners saw a decline in income (due to increased competition). The previously not employed, on the other hand, benefitted from the reform since some of them found jobs, providing them with positive income.

## 7 Conclusion

This paper uses microeconomic data to analyze the effects of a business registration reform in Mexico on registration, employment, prices, and income. It also traces out the effects of the reform on different pre-reform occupation groups, thereby offering an insight into the channels through which the effects operate.

First, the paper provides evidence that simplifying entry regulation increases the number of registered businesses. After the business registration reform in Mexico, the total number of registered businesses increased by 5 percent in eligible industries. This finding is in line with previous cross-country studies which show that countries with simpler regulation have more business entry. The paper then illustrates that the increase in registered businesses was due to former wage workers opening businesses, and not due to unregistered business owners registering their businesses. This effect is consistent with a model where unregistered business owners are low-ability individuals in a residual sector. The evidence does not support models that are in line with Hernando De Soto's view that informal business owners are medium-range ability individuals who choose a small scale production technology over wage work and who cannot register since registration is too costly and complicated.

The fact that I do not find informal business owners registering their businesses after the reform could also imply that complexity of business registration is not the relevant constraint keeping firms informal. After a firm is registered, it is presumably under more pressure to pay taxes and to comply with labor regulation (even though compliance is far from universal in Mexico, even for registered firms). The associated costs could be so big that changing only registration procedures may not be enough to push informal businesses over the threshold to formality.

This paper also shows that employment in eligible industries increased by 2.8 percent after the reform. In particular, those previously unemployed and out of the labor force were more likely to work as wage earners after the reform as the number of businesses increased. This effect mirrors the cross-country results on output growth, where less complicated regulation is associated with higher growth in output. It also provides evidence that lowering barriers to entry benefits consumers. Another piece of evidence suggesting that consumers benefit from the reform is that prices decreased by 0.6 percent after the reform in Mexico. I attribute this to the fact that new entrants increase total output in a market with a downward sloping demand curve. The price decline was concentrated among low-risk industries in the non-tradable goods sector, which indicates that it was due to competition. The increased competition also decreases income of incumbent firms by 3.2 percent.

Interestingly, this paper does not find an increase in the income of wage earners who opened

businesses. A possible explanation is that these new entrepreneurs were still paying off the fixed cost of opening a business during the time span covered in the data. This paper measures only the short-term effects of the reform, covering 3 quarters on average. For future research, it would be interesting to also measure the longer run effects, and in particular, to examine what happens to the income of new entrepreneurs over several periods.

Overall, the results suggest that promoting simplification of entry regulation is an effective policy for fostering entrepreneurial activity and for making consumers better off by increasing employment opportunities and by lowering prices. The attention that business registration reform has recently received from policy makers thus appears to be warranted.

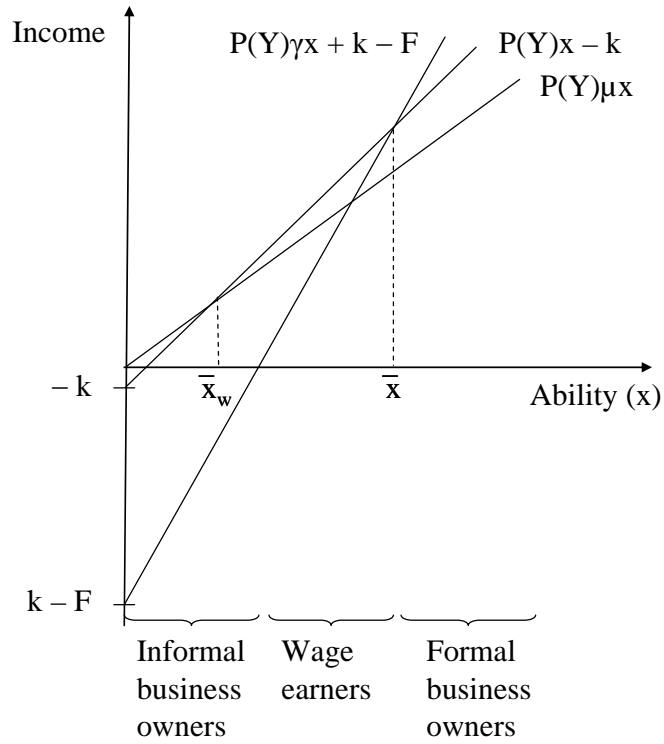
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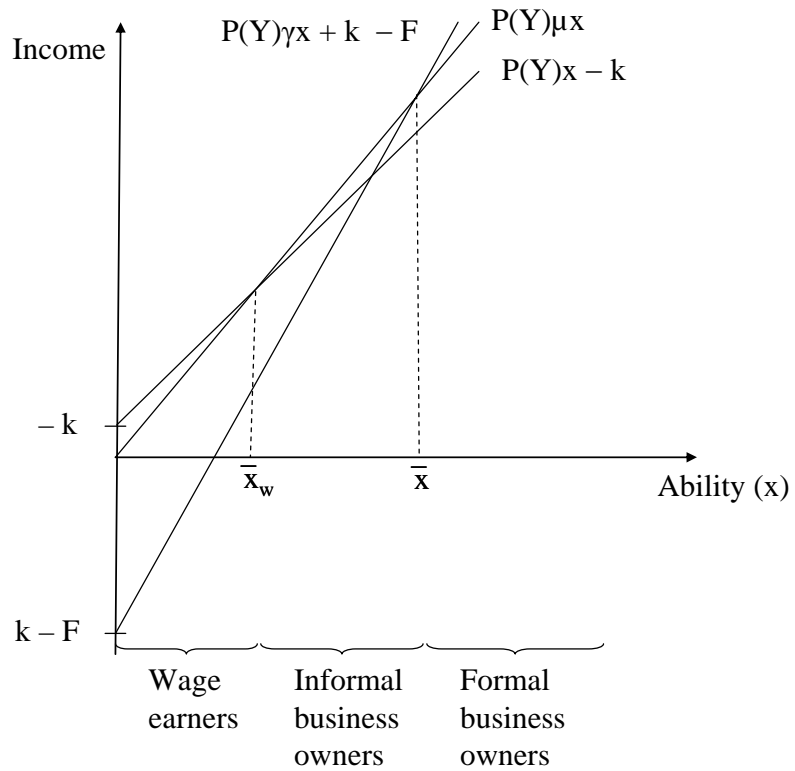
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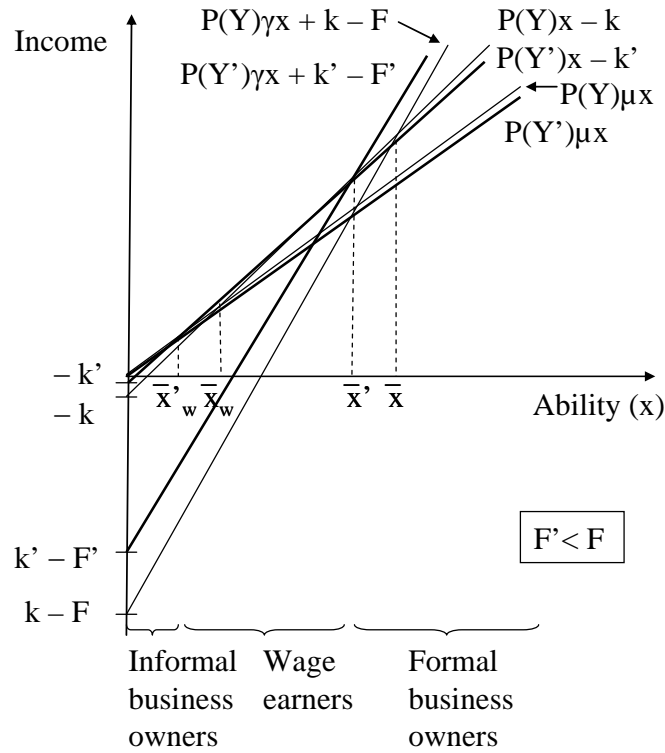
**Figure 1 – Occupation Division with a Low Productivity Informal Sector**



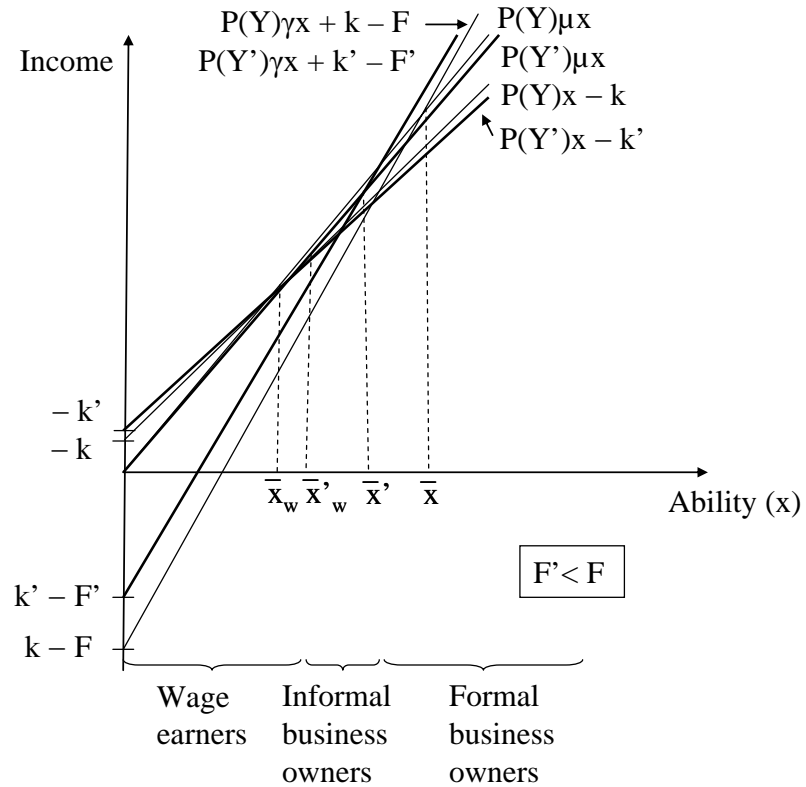
**Figure 2 – Occupation Division with a High Productivity Informal Sector**



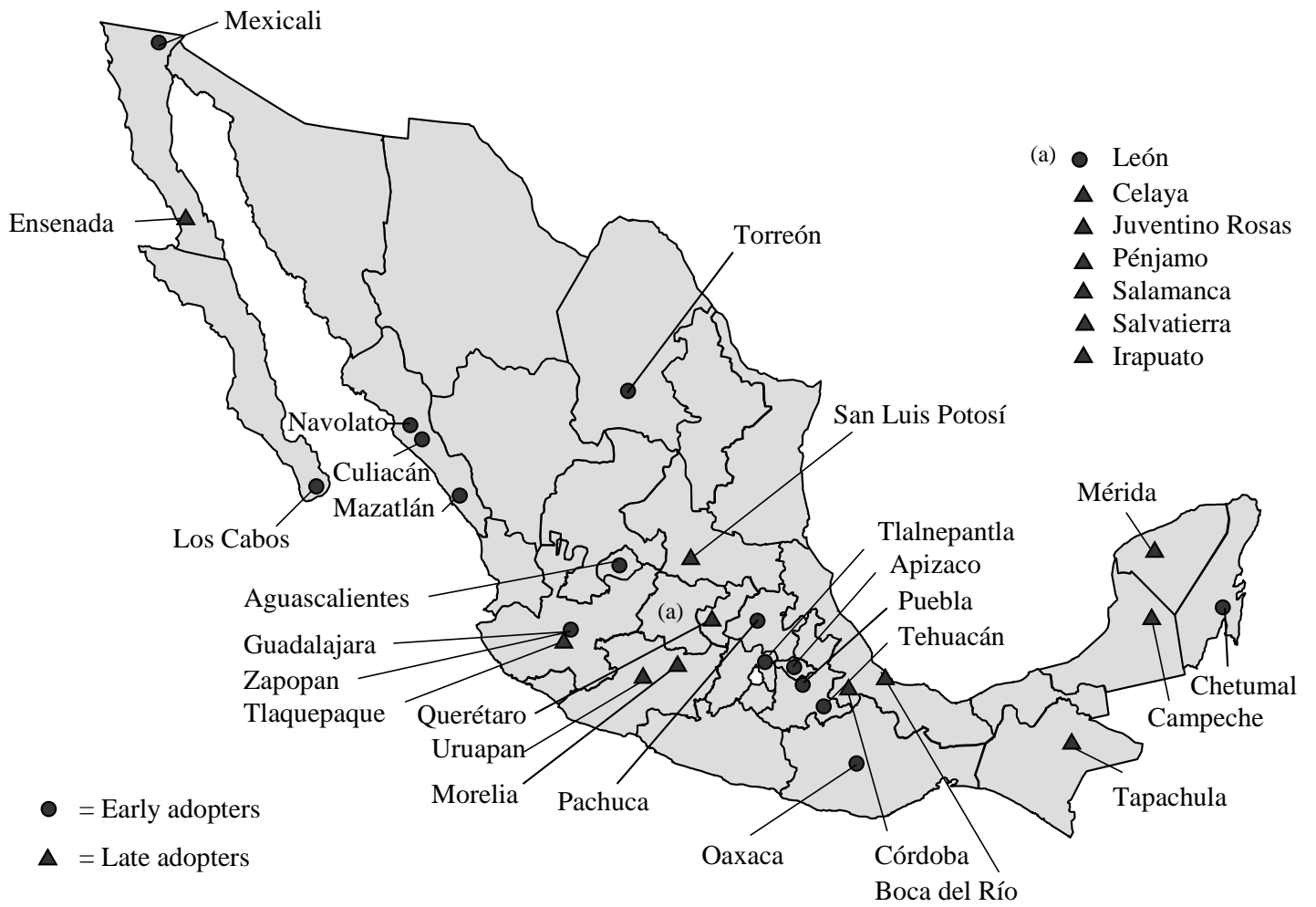
**Figure 3 – Impact of Reform with a Low Productivity Informal Sector**



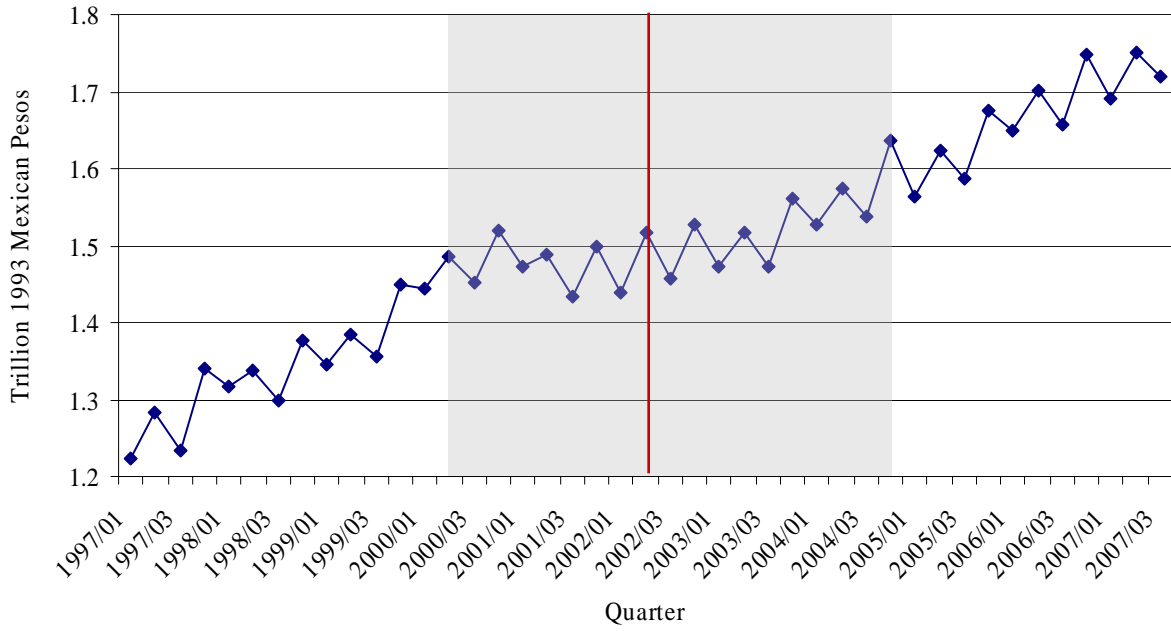
**Figure 4 – Impact of Reform with a High Productivity Informal Sector**



**Figure 5 – Municipalities that Received the Reform by December 2004**

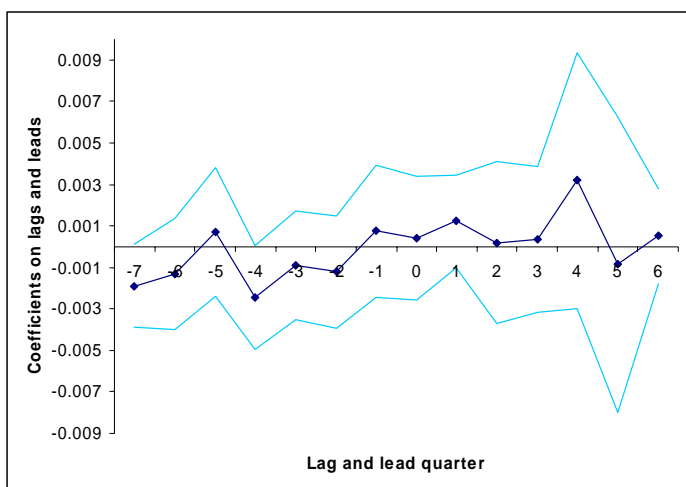


**Figure 6 – Quarterly Real GDP over Time**

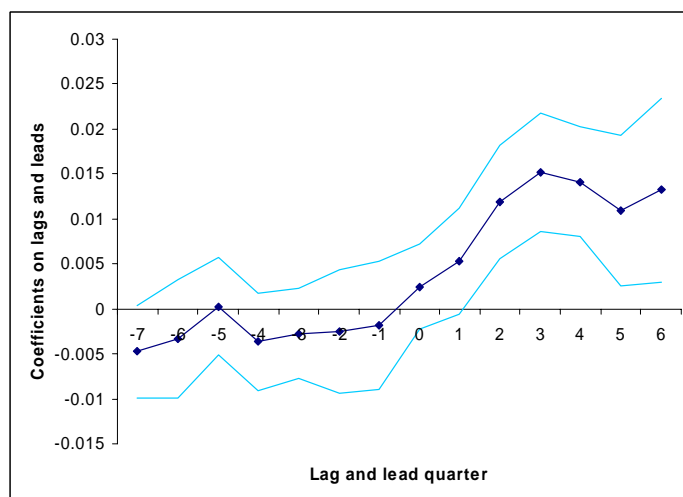


*Note:* The shaded area represents the quarters covered by the data in used in this study. The vertical line marks the quarter when the first municipality implemented the reform.

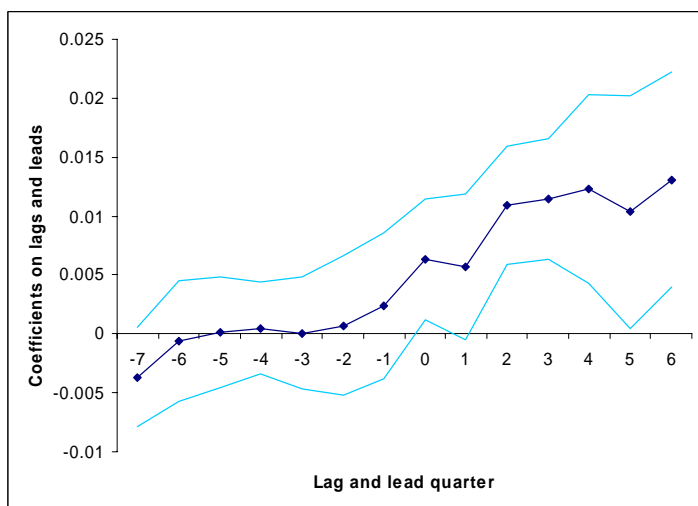
**Figure 7 – Coefficients on Relative Quarter Dummies in Low-Risk Registration Regression**



**Figure 8 – Coefficients on Relative Quarter Dummies in Low-Risk Wage Earner Regression**



**Figure 9 – Coefficients on Relative Quarter Dummies in Low-Risk Employment Regression**



**Table 1: Local Business Registration Procedures in Mexico**

	Average	Std. Dev.	Min	Max
<b>Pre-reform</b>				
Days	30.1	16.7	2	60
Procedures	7.9	7.0	1	27
Office visits	4.2	2.0	1	10
<b>Post-reform</b>				
Days	1.4	0.6	1	3
Procedures	2.7	2.1	1	9
Office visits	1.0	0.2	1	2

*Note:* These data come from the COFEMER website and are based on 32 municipalities from 17 different states. On top of these procedures, most firms need to comply with one or two federal procedures, which can often be done in the same location (this one-stop-shop solution is part of the reform).

**Table 2: Pre-Reform Income by Occupation**

	Non-registered business owners	Wage workers	Registered business owners
Avg. pre-reform income (in logs)	7.519	8.010	8.324
Avg. pre-reform income residual	-0.276	0.015	0.189

**Table 3: Municipality Characteristics Economic Census**

	Early adopters average	Late adopters average	Difference in averages	Coefficient on quarter of implementation
	(1)	(2)	(3)	(4)
<b>Panel A: Levels 1999</b>				
Log GDP per capita (value added)	9.546 (0.578)	9.145 (0.707)	0.401* (0.221)	-0.1046** (0.039)
Log establishments per capita	3.509 (0.436)	3.438 (0.239)	0.071 (0.121)	-0.013 (0.017)
Log fixed assets per capita	9.637 (0.615)	9.376 (0.924)	0.261 (0.269)	-0.0966** (0.046)
Log investment per capita	6.833 (0.725)	6.733 (1.042)	0.100 (0.308)	-0.080 (0.049)
<b>Panel B: Changes 1994 - 1999</b>				
Log GDP per capita (value added)	1.112 (0.232)	1.106 (0.286)	0.006 (0.089)	0.001 (0.016)
Log establishments per capita	0.112 (0.103)	0.145 (0.094)	-0.033 (0.034)	0.010 (0.006)
Log fixed assets per capita	0.783 (0.669)	1.053 (0.343)	-0.270 (0.182)	0.020 (0.021)
Log investment per capita	0.827 (0.754)	1.123 (0.963)	-0.296 (0.297)	0.059 (0.069)
Observations	17	17	34	34

*Note:* Standard errors in parentheses. I define early adopters as the municipalities which implemented the reform between May 2002 and March 2004. Late adopters are municipalities which adopted the reform between April 2004 and December 2004, when my dataset ends. Economic data come from the 1994 and 1999 Economic Census and are converted to per capita levels using data from the 1995 Population Count and the 2000 Demographic Census. Establishments are measured per 1000 inhabitants. Changes are defined as  $[Y(1999)-Y(1994)]$ . Column 4 presents the coefficients of a separate regression for each variable or change on quarter of implementation. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 4a: Pre-Reform Averages of Individual Level Variables**

	Early adopters average	Late adopters average	Difference in averages	Coefficient on quarter of implementation
	(1)	(2)	(3)	(4)
<b>Outcome Variables</b>				
Employed dummy	0.6617 (0.4731)	0.6605 (0.4735)	0.0012 (0.0118)	0.0002 (0.0025)
Low-risk employed dummy	0.4463 (0.4971)	0.4402 (0.4964)	0.0062 (0.0211)	0.0003 (0.004)
High-risk employed dummy	0.2154 (0.4111)	0.2203 (0.4145)	-0.0049 (0.0208)	-0.0001 (0.0032)
Wage earner dummy	0.5014 (0.5000)	0.4941 (0.5000)	0.0074 (0.0119)	-0.0021 (0.0024)
Low-risk wage earner dummy	0.2998 (0.4582)	0.2869 (0.4523)	0.0129 (0.0170)	-0.0019 (0.0029)
High-risk wage earner dummy	0.2017 (0.4012)	0.2072 (0.4053)	-0.0055 (0.0207)	-0.0002 (0.0032)
Registered business owner dummy	0.0842 (0.2776)	0.0839 (0.2772)	0.0003 (0.0064)	0.0008 (0.0010)
Low-risk registered owner	0.0737 (0.2612)	0.0734 (0.2608)	0.0003 (0.0058)	0.0007 (0.0010)
High-risk registered owner	0.0105 (0.1019)	0.0105 (0.1019)	0.0000 (0.0019)	0.0001 (0.0002)
Log monthly income	8.0686 (0.7450)	7.8917 (0.8117)	0.1769** (0.0812)	-0.0298 (0.0197)
<b>Background Variables</b>				
Primary education dummy	0.2286 (0.4199)	0.2265 (0.4186)	0.0020 (0.0141)	-0.0007 (0.0021)
Secondary education dummy	0.2287 (0.4200)	0.2289 (0.4201)	-0.0001 (0.0127)	-0.0004 (0.0018)
High school education dummy	0.2416 (0.4280)	0.2348 (0.4239)	0.0067 (0.0167)	-0.0025 (0.0024)
University education dummy	0.1475 (0.3546)	0.1371 (0.3440)	0.0104 (0.0148)	-0.0017 (0.002)
Female dummy	0.5310 (0.4990)	0.5432 (0.4981)	-0.0121** (0.0052)	0.0018 (0.0014)
Age	36.9033 (11.9884)	37.1139 (12.0612)	-0.2106 (0.1598)	0.0305 (0.0221)
Married dummy	0.6694 (0.4704)	0.6848 (0.4646)	-0.0154 (0.0108)	0.0041** (0.002)

*Note:* Standard errors in parentheses (clustered at municipality level). Data is from 2000 & 2001 ENE. The employed include wage earners and self-employed/business owners. Column 4 presents the coefficients of a separate regression for each variable on quarter of implementation. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%



**Table 4b: Pre-Reform Yearly Changes in Individual Level Variables**

	Early adopters average	Late adopters average	Difference in averages	Coefficient on quarter of implementation
	(1)	(2)	(3)	(4)
<b>Outcome Variables</b>				
Employed change	-0.0045 (0.0220)	0.0148 (0.0331)	-0.0193* (0.0097)	0.0046*** (0.0017)
Low-risk employed change	0.0047 (0.0236)	0.0201 (0.0452)	-0.0154 (0.0124)	0.0035 (0.0022)
High-risk employed change	-0.0092 (0.0130)	-0.0053 (0.0282)	-0.0040 (0.0075)	0.0011 (0.0013)
Wage earner change	-0.0082 (0.0170)	0.0044 (0.0279)	-0.0126 (0.0079)	0.0022 (0.0016)
Low-risk wage earner change	-0.0006 (0.0229)	0.0100 (0.0296)	-0.0106 (0.0091)	0.0016 (0.0018)
High-risk wage earner change	-0.0076 (0.0140)	-0.0056 (0.0289)	-0.0020 (0.0078)	0.0005 (0.0014)
Registered business owner change	0.0016 (0.0145)	0.0017 (0.0124)	-0.0001 (0.0046)	0.0005 (0.0007)
Low-risk registered owner change	0.0025 (0.0147)	0.0014 (0.0105)	0.0012 (0.0044)	0.0001 (0.0008)
High-risk registered owner change	-0.0010 (0.0047)	0.0003 (0.0035)	-0.0013 (0.0014)	0.0004 (0.0003)
Log monthly income change	0.1044 (0.0455)	0.1379 (0.1034)	-0.0334 (0.0274)	0.0057 (0.0046)
<b>Background Variables</b>				
Primary education change	0.0029 (0.0176)	-0.0102 (0.0177)	0.0131** (0.0061)	-0.0013 (0.0010)
Secondary education change	0.0086 (0.0217)	0.0032 (0.0166)	0.0053 (0.0066)	-0.0013 (0.0012)
High school education change	-0.0037 (0.0182)	0.0144 (0.0190)	-0.0181*** (0.0064)	0.0017 (0.0011)
University education change	0.0018 (0.0183)	0.0023 (0.0116)	-0.0005 (0.0053)	0.0001 (0.0009)
Female change	0.0036 (0.0157)	-0.0081 (0.0199)	0.0117* (0.0062)	-0.0029** (0.0013)
Age change	0.3067 (0.4665)	-0.0508 (0.7032)	0.3575* (0.2047)	-0.0244 (0.0453)
Married change	-0.0069 (0.0240)	-0.0049 (0.0175)	-0.0020 (0.0072)	0.0001 (0.0012)

*Note:* Standard errors in parentheses. Changes are changes in ENE municipality averages from 2000-IV to 2001-IV. Column 4 presents the coefficients of a separate regression for each variable on quarter of implementation.

Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 5: Impact of Business Registration Reform on Registration**

	Dependent variable:					
	Registered business owner dummy		Low-risk registered business owner dummy		High-risk registered business owner dummy	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform dummy (SARE)	0.0036** (0.0015)	0.0032** (0.0014)	0.0042*** (0.0014)	0.0037*** (0.0013)	-0.0006 (0.0004)	-0.0006 (0.0004)
Primary education dummy		0.0314*** (0.0023)		0.0249*** (0.0021)		0.0065*** (0.0006)
Secondary education dummy		0.0475*** (0.0031)		0.0395*** (0.0029)		0.0081*** (0.0009)
High school education dummy		0.0556*** (0.0033)		0.0478*** (0.0030)		0.0078*** (0.0008)
University education dummy		0.1170*** (0.0067)		0.1124*** (0.0063)		0.0046*** (0.0011)
Female dummy		-0.0829*** (0.0036)		-0.0636*** (0.0033)		-0.0193*** (0.0018)
Age		0.0031*** (0.0001)		0.0027*** (0.0001)		0.0004*** (0.0000)
Married dummy		0.0257*** (0.0013)		0.0204*** (0.0011)		0.0053*** (0.0005)
Census variables*time trend	No	Yes	No	Yes	No	Yes
R-squared	0.003	0.057	0.003	0.047	0.001	0.014
No. of observations	1,636,225	1,636,225	1,636,225	1,636,225	1,636,225	1,636,225

*Note:* Standard errors in parentheses (clustered at municipality level). Regressions include quarter and municipality fixed effects. Census variables are log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita from the 1999 Economic Census, converted to per capita levels using population data from the 2000 Demographic Census. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 6: Impact of Reform By Reduction in Number of Procedures**

	Dependent variable:		
	Low-risk registered business owner dummy	Low-risk wage earner dummy	Low-risk employed dummy
	(1)	(2)	(3)
Reform dummy (SARE)	0.0021 (0.0020)	0.0006 (0.0038)	0.0045 (0.0058)
Reform dummy*Reduct. in procedures	0.0002* (0.0001)	0.0007** (0.0002)	0.0010*** (0.0003)
R-squared	0.048	0.076	0.095
No. of observations	1,392,457	1,392,457	1,392,457

*Note:* Standard errors in parentheses (clustered at municipality level). Regressions include quarter and municipality fixed effects. They also include 1999 municipality ln GDP per capita, ln number of establishments per capita, ln fixed assets per capita, and ln investment per capita, all interacted with a linear time trend, as well as individual background variables. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 7: Impact of Reform by Pre-Reform Occupation**

	Dependent variable:		
	Low-risk registered business owner dummy	Low-risk wage earner dummy	High-risk wage earner dummy
	(1)	(2)	(3)
<b>Panel A: Impact on all groups</b>			
SARE*Past registered owner	0.0053 (0.0086)	0.0104* (0.0057)	-0.0043 (0.0061)
SARE*Past non-registered owner	-0.0004 (0.0066)	0.0031 (0.0089)	-0.0062** (0.0026)
SARE*Past wage earner	0.0021 (0.0014)	0.0051 (0.0045)	-0.0125** (0.0049)
SARE*Past not employed	0.0000 (0.0017)	0.0124*** (0.0028)	-0.0022 (0.0028)
R-squared	0.347	0.198	0.252
No. of observations	1,051,274	1,051,274	1,051,274
<b>Panel B: Impact on wage earners</b>			
SARE*Past low-risk wage earner	0.0026 (0.0020)	-0.0013 (0.0056)	-0.0057* (0.0031)
SARE*Past high-risk wage earner	0.0016 (0.0015)	0.0067** (0.0032)	-0.0123** (0.0059)
R-squared	0.018	0.400	0.595
No. of observations	520,804	520,804	520,804

*Note:* Standard errors in parentheses (clustered at municipality level). Past occupation variables are pre-reform occupations when the person was first observed. The past not employed include the unemployed, people who are not in the labor force, and the small fraction of people who say that they are not currently working, but have a job lined up that they are planning to start within one month. Regressions include quarter and municipality fixed effects, as well as dummies for occupation interacted with quarter and municipality fixed effects, and individual background variables. They also include 1999 municipality log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita, all interacted with a linear time trend. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 8: Impact on Wage Earners**

	Dependent variable: Low-risk registered business owner dummy
Reform dummy*Wage earner has a written contract	0.0013 (0.0011)
Reform dummy*Wage earner has no written contract	0.0054* (0.0029)
R-squared	0.022
No. of observations	520,804

*Note:* Standard errors in parentheses (clustered at municipality level). Sample includes past wage earners only. Written contract variables are pre-reform characteristics when the person was first observed. Regressions include quarter and municipality fixed effects, as well as dummies for firm characteristics interacted with quarter and municipality fixed effects, and individual background variables. They also include 1999 municipality log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita, all interacted with a linear time trend. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 9: Impact on Non-Registered Business Owners**

	Dependent variable: Low-risk registered business owner dummy
<b>Panel A: Impact by firm size</b>	
Reform dummy*Owner is alone in firm	-0.0025 (0.0083)
Reform dummy*Owner is not alone (has partners and/or workers)	0.0050 (0.0439)
R-squared	0.044
No. of observations	85,691
<b>Panel B: Impact by type of premises</b>	
Reform dummy*Fixed premises	-0.0056 (0.0307)
Reform dummy*Mobile premises (street stand or door-to-door)	-0.0036 (0.0067)
R-squared	0.062
No. of observations	78,518

*Note:* Standard errors in parentheses (clustered at municipality level). Sample includes past non-registered business owners only. Firm size and type of premises variables are pre-reform characteristics when the person was first observed. Regressions include quarter and municipality fixed effects, as well as dummies for firm characteristics interacted with quarter and municipality fixed effects, and individual background variables. They also include 1999 municipality log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita, all interacted with a linear time trend. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 10: Impact of Business Registration Reform on Wage Work**

	Dependent variable:					
	Wage earner dummy		Low-risk wage earner dummy		High-risk wage earner dummy	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform dummy (SARE)	-0.0020 (0.0027)	-0.0006 (0.0023)	0.0051 (0.0030)	0.0064** (0.0027)	-0.0071* (0.0035)	-0.0070*** (0.0021)
Primary education dummy		0.0563*** (0.0040)		0.0034 (0.0032)		0.0529*** (0.0041)
Secondary education dummy		0.1182*** (0.0066)		-0.0096** (0.0046)		0.1278*** (0.0075)
High school education dummy		0.1006*** (0.0080)		-0.0892*** (0.0064)		0.1898*** (0.0083)
University education dummy		0.2356*** (0.0112)		-0.1395*** (0.0087)		0.3751*** (0.0151)
Female dummy		-0.2509*** (0.0093)		-0.1447*** (0.0104)		-0.1062*** (0.0075)
Age		-0.0047*** (0.0003)		-0.0061*** (0.0002)		0.0013*** (0.0003)
Married dummy		-0.0808*** (0.0074)		-0.0955*** (0.0060)		0.0147*** (0.0034)
Census variables*time trend	No	Yes	No	Yes	No	Yes
R-squared	0.003	0.119	0.007	0.078	0.011	0.113
No. of observations	1,636,225	1,636,225	1,636,225	1,636,225	1,636,225	1,636,225

*Note:* Standard errors in parentheses (clustered at municipality level). Regressions include quarter and municipality fixed effects. Census variables are log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita from the 1999 Economic Census, converted to per capita levels using population data from the 2000 Demographic Census. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 11: Impact of Business Registration Reform on Employment**

	Dependent variable:					
	Employed dummy		Low-risk employed dummy		High-risk employed dummy	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform dummy (SARE)	0.0035 (0.0038)	0.0049 (0.0034)	0.011** (0.0045)	0.0124*** (0.0038)	-0.0075** (0.0035)	-0.0075*** (0.0021)
Primary education dummy		0.0498*** (0.0035)		-0.0090* (0.0052)		0.0588*** (0.0041)
Secondary education dummy		0.0916*** (0.0043)		-0.0423*** (0.0089)		0.1339*** (0.0075)
High school education dummy		0.0578*** (0.0072)		-0.1373*** (0.0111)		0.1951*** (0.0078)
University education dummy		0.2171*** (0.0061)		-0.1586*** (0.0151)		0.3758*** (0.0147)
Female dummy		-0.3958*** (0.0109)		-0.2653*** (0.0133)		-0.1305*** (0.0075)
Age		-0.0002*** (0.0003)		-0.0020*** (0.0002)		0.0018*** (0.0003)
Married dummy		-0.0578*** (0.0069)		-0.0781*** (0.0058)		0.0203*** (0.0033)
Census variables*time trend	No	Yes	No	Yes	No	Yes
R-squared	0.003	0.206	0.008	0.094	0.010	0.117
No. of observations	1,636,225	1,636,225	1,636,225	1,636,225	1,636,225	1,636,225

*Note:* Standard errors in parentheses (clustered at municipality level). Regressions include quarter and municipality fixed effects. Census variables are log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita from the 1999 Economic Census, converted to per capita levels using population data from the 2000 Demographic Census. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 12: Impact of Reform on Prices**

	Dependent variable: Log consumer price index	
	(1)	(2)
Reform dummy (SARE)	-0.0056 (0.0043)	-0.0058** (0.0027)
Time span	Apr 00 - Dec 04	Apr 00 - May 06
R-squared	0.980	0.983
No. of observations	1,140	1,480

*Note:* Standard errors in parentheses (clustered at municipality level). Regressions include month and municipality fixed effects, as well as 1999 municipality log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita, all interacted with a linear time trend. Regressions are for 20 municipalities only since the price index is only available for 20 of the 34 municipalities in the main sample. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 13: Impact of Reform on Prices by Subcategory**

Dependent variable: Log Price index for	Coefficient on reform dummy (SARE)
Non-tradables in low-risk industries	-0.0096** (0.0040)
Non-tradables in high-risk industries	-0.0062 (0.0089)
Tradables in low-risk industries	-0.0039 (0.0034)
Tradables in high-risk industries	0.0014 (0.0036)

*Note:* Standard errors in parentheses (clustered at municipality level). Regressions include month and municipality fixed effects, as well as 1999 municipality log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita, all interacted with a linear time trend. Regressions are for 20 municipalities only since the price index is only available for 20 of the 34 municipalities in the main sample. Regressions are for the April 2000 to May 2006 time span and include 1,480 observations. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%



**Table 14: Impact on Income by Pre-Reform Occupation**

	Dependent variable:		
	Log income		
	Nominal		Real
	(1)	(2)	(3)
SARE*Past registered owner	-0.0317*	-0.0387**	-0.0353**
	(0.0180)	(0.0172)	(0.0165)
SARE*Past non-regist. owner	-0.0177	-0.0062	-0.0038
	(0.0211)	(0.0213)	(0.0212)
SARE*Past wage earner	-0.0047	-0.0065	-0.0035
	(0.0082)	(0.0074)	(0.0082)
R-squared	0.376	0.382	0.376
No. of municipalities	34	20	20
No. of observations	570,154	419,709	419,709

*Note:* Standard errors in parentheses (clustered at municipality level). Past occupation variables are pre-reform occupations when the person was first observed. The regressions drop all individuals who were initially not employed since their past income is not observed. Regressions include quarter and municipality fixed effects, as well as dummies for firm characteristics interacted with quarter and municipality fixed effects, and individual background variables. They also include 1999 municipality log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita, all interacted with a linear time trend. Column 1 includes all 34 municipalities from the main sample. Columns 2 and 3 include only the 20 municipalities for which the price index is available. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 15: Impact on Income by Pre-Reform Occupation**

	Dependent variable:					
	Quadratic root of income			Log income (Tobit)		
	Nominal		Real	Nominal		Real
	(1)	(2)	(3)	(4)	(5)	(6)
SARE*Past registered owner	-0.0568 (0.0561)	-0.0405 (0.0505)	-0.0334 (0.0459)	-0.0326 (0.0329)	-0.0103 (0.0366)	-0.0035 (0.0537)
SARE*Past non-regist. owner	0.0449 (0.0570)	0.0506 (0.0471)	0.0512 (0.0479)	0.0405 (0.0339)	0.0421 (0.0380)	0.0595 (0.0563)
SARE*Past wage earner	-0.0443 (0.0271)	-0.0519** (0.0238)	-0.0450* (0.0229)	-0.0354*** (0.0138)	-0.0371** (0.0154)	-0.0368 (0.0227)
SARE*Past not employed	0.0656** (0.0322)	0.0548 (0.0344)	0.0554 (0.0339)	0.0571*** (0.0160)	0.0495*** (0.0179)	0.1508*** (0.0343)
R-squared	0.545	0.546	0.546	0.155	0.156	0.175
No. of counties	34	20	20	34	20	20
No. of observations	1,009,174	733,669	733,669	1,009,174	733,669	733,669

*Note:* Standard errors in parentheses (clustered at municipality level). Past occupation variables are pre-reform occupations when the person was first observed. The past not employed include the unemployed, people who are not in the labor force, and the small fraction of people who say that they are not currently working, but have a job lined up that they are planning to start within one month. Regressions include quarter and municipality fixed effects, as well as dummies for occupation interacted with quarter and municipality fixed effects and individual background variables. They also include 1999 municipality log GDP per capita, log number of economic establishments per 1000 capita, log fixed assets per capita, and log investment per capita, all interacted with a linear time trend. Columns 1 and 4 include all 34 municipalities from the main sample. Columns 2, 3, 5, and 6 include only the 20 municipalities for which the price index is available. In the Tobit regressions, log(0) observations are replaced with the value of the lowest log income in the data. This value is also used as the lower bound for the Tobit.

Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Table 16: Impact of Business Registration Reform on Income**

	Dependent variable:			
	Log income	Quadratic root of income	Log income (Tobit)	Log agg. municipality income per capita
	(1)	(2)	(3)	(4)
<b>Panel A: Nominal income</b>				
Reform dummy (SARE)	-0.0180* (0.0090)	0.0039 (0.0338)	0.0309* (0.0181)	-0.0167 (0.0230)
R-squared	0.355	0.288	0.060	0.932
No. of municipalities	34	34	34	34
No. of observations	974,146	1,565,720	1,565,720	645
<b>Panel B: Nominal income</b>				
Reform dummy (SARE)	-0.0200** (0.0088)	-0.0241 (0.0371)	0.0015 (0.0206)	-0.0454* 0.0225
R-squared	0.360	0.282	0.058	0.921
No. of municipalities	20	20	20	20
No. of observations	731,302	1,165,522	1,165,522	380
<b>Panel C: Real income</b>				
Reform dummy (SARE)	-0.0169* (0.0091)	-0.0165 (0.0346)	-0.0027 (0.0128)	-0.0426* (0.0215)
R-squared	0.352	0.282	0.054	0.912
No. of municipalities	20	20	20	20
No. of observations	731,302	1,165,522	1,165,522	380

*Note:* Standard errors in parentheses (clustered at municipality level). Regressions include quarter and municipality fixed effects. They also include 1999 municipality ln GDP per capita, ln number of establishments per capita, ln fixed assets per capita, and ln investment per capita, all interacted with a linear time trend, as well as individual background variables. Panel A includes all 34 municipalities from my main sample. Panels B and C include only the 20 municipalities for which the price index is available. In the Tobit regressions, log(0) observations are replaced with the value of the lowest log income in the data. This value is also used as the lower bound for the Tobit. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%

**Appendix A. Examples of “Low-Risk to Society” and “High-Risk to Society”/Regulated Industries**

<b>Low-Risk to Society Industries</b>	<b>High-Risk to Society/Regulated Industries</b>
Most agricultural activities	Mineral extraction
	Electricity, water, and gas
Food production	Production of alcoholic beverages
Textile production	Paper production
IT production	Oil production
	Chemical production
	Plastic production
	Metal production
	Machinery production
	Vehicle production
Wholesale commerce	
Retail commerce	
	Transportation (including taxis)
Real estate	Financial services
Professional services	
Hotels and restaurants	

## Appendix B. Reform Municipalities by Implementation Date

<b>Municipality</b>	<b>State</b>	<b>Reform Date</b>
Puebla	Puebla	8-May-2002
Los Cabos	Baja California Sur	16-Oct-2002
Aguascalientes	Aguascalientes	10-Apr-2003
Guadalajara	Jalisco	28-May-2003
Zapopan	Jalisco	28-May-2003
Mexicali	Baja California	2-Jun-2003
León	Guanajuato	1-Jul-2003
Tlalnepantla	Estado de México	8-Sep-2003
Tehuacán	Puebla	1-Oct-2003
Oaxaca	Oaxaca	8-Dec-2003
Torreón	Coahuila	7-Jan-2004
Pachuca	Hidalgo	15-Jan-2004
Chetumal	Quintana Roo	2-Feb-2004
Apizaco	Tlaxcala	4-Feb-2004
Culiacán	Sinaloa	24-Mar-2004
Mazatlán	Sinaloa	24-Mar-2004
Navolato	Sinaloa	24-Mar-2004
Mérida	Yucatán	14-Apr-2004
Celaya	Guanajuato	2-Jul-2004
Ensenada	Baja California	9-Jul-2004
Tapachula	Chiapas	12-Aug-2004
Campeche	Campeche	20-Aug-2004
Querétaro	Querétaro	24-Aug-2004
San Luis Potosí	San Luis Potosí	14-Sep-2004
Morelia	Michoacán	4-Oct-2004
Uruapan	Michoacán	4-Oct-2004
Cortazar	Guanajuato	19-Oct-2004
Jaral del Progreso	Guanajuato	19-Oct-2004
Juventino Rosas	Guanajuato	19-Oct-2004
Pénjamo	Guanajuato	19-Oct-2004
Salamanca	Guanajuato	19-Oct-2004
Salvatierra	Guanajuato	19-Oct-2004
San José Iturbide	Guanajuato	19-Oct-2004
Silao	Guanajuato	19-Oct-2004
Córdoba	Veracruz	23-Nov-2004
Tlaquepaque	Jalisco	30-Nov-2004
Irapuato	Guanajuato	7-Dec-2004
Boca del Río	Veracruz	16-Dec-2004

Shaded municipalities are not represented in the data set I am using.

## Appendix C. Definition of Variables

Employed Dummy: The employed dummy is equal to one for everybody who

1. Worked for pay for a least one hour during the past week, or
2. Was on paid vacation, leave or on strike during the past week, or
3. Didn't work during the past week for other reasons, but has a job and will return to work within one month, or
4. Helped somebody else without pay in their business.

This and all other dummy variables are zero for everybody else in my sample, which are people between 20 and 65 years old.

Wage Earner Dummy: This dummy is equal to one for people who

1. Are employed according to the definition above, and
2. Who chose the option of "worker" or "member of a cooperative" as the answer to the question about which position they held in their main job.

Registered Business Dummy: This dummy is equal to one for people who

1. Are employed according to the definition above, and
2. Chose the option of "boss" or "self-employed" as the answer to the question about which position they held in their main job, and
3. Are classified into category 2 or 4 of the survey question "What is the name of your business?" The instructions to interviewers specify that the word "name" here refers to the legal name under which the business is registered with the authorities. After asking this question, the interviewer has to further classify the business into one of the following categories 1) state-owned company, 2) industrial, commercial or services chain, 3) cooperative or trade union, 4) any other economic entity with name and/or register, 5) business owner or self-employed whose business does not have any (legal) name and/or register, 6) worker in a business without (legal) name and/or register, 7) worker in the US. All "bosses" or "self-employed" in my sample have values 2, 4, 5, or 7 for this question. I drop individuals with value 7. For category 4, the instructions tell the interviewer that any type of official register is sufficient, such as federal tax register, health ministry register, or municipal government register. Since the SARE reform simplifies obtaining a municipal license, I would ideally like to know whether the firm has a municipal register only. Firms need to register with the federal tax authority first before registering for a municipal license, implying that this measure may overstate the number of firms registered with the municipal government. Other types of registers, such as the health ministry register typically come after registration with the municipal authorities, meaning that these types of registers capture what is relevant for this study. An additional caveat is that the instructions also specify that anybody with a professional license who has an unregistered business should be classified as registered here. Note that neither tax registration nor professional licensing should change due to the reform (unless a businesses gets registered for taxes since the owner also wants to register it for a municipal license), meaning that the estimated effect of the reform on the number of registered businesses should only come from additional businesses with municipal licenses.

Only 53 percent of people in my sample who comply with criteria 1 and 2 also comply with criterion 3. The others are self-employed without being registered. Most of these people are engaged in small commerce, food preparation, repair work or construction. They work roadside without a fixed locality, in the home of their clients, or in their own home.

A concern is that people may not truthfully report whether their business is registered with the authorities. To check whether the fraction of individuals who report having a registered business seems reasonable, I compare the ENE number to the number of economic establishments per population of age 20-65, calculated from the 1999 Economic Census and the 2000 Demographic Census. For the 34 municipalities in my sample, the total number of establishments is 553,235 and the total population 20-65 is 8,285,900, implying a ratio of 0.067. This number is not far from the mean of the registered business dummy in my ENE sample (0.084). The Census number may be lower since the Economic Census only includes businesses that have a fixed establishment. While the majority of registered businesses fall into this category, there are also some who operate in vehicles (such as taxi drivers), who work from home or in the home of their clients.

Low-Risk (High-Risk) Registered Businesses Dummy: The low-risk (high-risk) dummy is equal to one for individuals for whom the registered business dummy is equal to one and who are in a low-risk (high-risk) industry. Appendix B describes the division of industries into low-risk and high-risk.

Income: This variable is the monthly income from the main job held in the past week. It is zero for the people who work without pay, who are unemployed, or who are out of the labor force.

Schooling Dummies: These dummies refer to the highest completed level of schooling.