

## NOTE

LICENSE TO SELL: THE EFFECT OF BUSINESS REGISTRATION REFORM  
ON ENTREPRENEURIAL ACTIVITY IN MEXICO

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*Abstract*—This paper estimates the economic effects of a recent reform that simplified business entry regulation in Mexico. The reform was introduced in different municipalities at different points in time. Using micro-level data, I find that the reform increased the number of registered businesses by 5%. 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. The reform also increased wage employment by 2.2%. Finally, the results imply that the competition from new entrants decreased the income of incumbent businesses by 3%.

## I. Introduction

**B**USINESS 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, yet such an approach suffers from identification problems, such as reverse causality and omitted variable bias.<sup>1</sup> Recent work has also looked at cross-country, cross-industry variation for an identification strategy.<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 microlevel data to examine the effect of simplifying entry regulation on registration, employment, prices, and income. Specifically, it exploits cross-municipality and cross-time variation in a recent business registration reform in Mexico to measure the effects of this reform, which allows for establishing causality more convincingly than cross-country studies do. The results show that the reform increased the number of registered businesses by 5% in eligible industries, supporting the finding of the cross-country literature that less regulation leads to more entry.

The use of microdata 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 to the registration of existing informal businesses. Having shown that it led to previous wage earners' opening new businesses, rather than infor-

mal businesses registering, the paper traces out the impact of this increase in competition on employment, consumer prices, and incumbents' income. The results show that the fraction of wage earners in eligible industries increased by 2.2%.<sup>3</sup> In particular, people who were previously not employed were more likely to work as wage earners after the reform.

Moreover, by increasing competition, the reform benefited consumers and hurt incumbent businesses. First, the reform decreased the price level by about 1%. Second, the income of incumbent registered businesses declined by 3%. Finally, the income of the previously not employed increased after the reform by about 6%.

Concurrent 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 is that they use registration and employment data from the Social Security Institute (IMSS), while my paper uses household data from the labor market survey. The IMSS data do not capture registered firms without employees since owners do not typically register themselves with social security.<sup>4</sup> Moreover, not all registered firms with employees are in the IMSS database since a significant fraction of owners does not register their workers with IMSS. Consequently, the estimated increase in the number of registered firms in Kaplan et al. is 7.6 times smaller than the increase in the number of registered firms estimated in this paper. Another difference is that I provide direct evidence that newly registered firms are not previously existing informal firms, but instead new firms created by former wage earners. Furthermore, Kaplan et al.'s data do not include information on income. My paper, however, identifies the effect of the reform on the income of different prereform occupation groups.

## II. The Mexican Rapid Business Opening System Reform

The reform consisted in creating a Rapid Business Opening System (SARE) in various municipalities and was organized by the Federal Commission for Improving Regulation (COFEMER). COFEMER had to coordinate with municipality governments on implementing the reform since many business registration procedures are set locally in Mexico. SARE was implemented in different municipalities at different times, starting in May 2002. By September 2006, 103 municipalities had a SARE.<sup>5</sup>

SARE was successful in simplifying local business registration procedures. After reform, 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. The reduction in registration procedures from 8 on

<sup>3</sup> This finding is in line with Bertrand and Kramarz (2002), which shows that French regions with stricter enforcement of a zoning law have lower employment growth in the retail trade industry.

<sup>4</sup> According to my data, in the prereform period, 40% of registered businesses had no employees. Among these firms, less than 0.2% of owners were registered with IMSS.

<sup>5</sup> There are 2,454 municipalities in Mexico, but 94% of the population and 98% of economic activity are concentrated in 450 municipalities. These 450 include 99 of the 103 municipalities that had a SARE by September 2006.

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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.

TABLE 1.—PREREFORM AVERAGES OF OUTCOME VARIABLES

	Early Adopters' Average (Levels) (1)	Late Adopters' Average (Levels) (2)	Difference in Average Levels (3)	Coefficient on Quarter of Implementation (Levels) (4)	Difference in Average Changes (5)	Coefficient on Quarter of Implementation (Changes) (6)
Wage earner dummy	0.5015 (0.5000)	0.4941 (0.5000)	0.0074 (0.0119)	-0.0021 (0.0024)	-0.0125 (0.0079)	0.0022 (0.0016)
Low-risk wage earner dummy	0.2998 (0.4582)	0.2869 (0.4523)	0.0129 (0.0170)	-0.0019 (0.0029)	-0.0106 (0.0091)	0.0016 (0.0018)
High-risk wage earner dummy	0.2017 (0.4013)	0.2072 (0.4053)	-0.0055 (0.0207)	-0.0002 (0.0032)	-0.0020 (0.0078)	0.0005 (0.0014)
Registered business owner dummy	0.0842 (0.2776)	0.0839 (0.2772)	0.0003 (0.0064)	0.0008 (0.0010)	-0.0001 (0.0046)	0.0005 (0.0007)
Low-risk registered owner	0.0737 (0.2612)	0.0734 (0.2608)	0.0003 (0.0058)	0.0007 (0.0010)	0.0011 (0.0044)	0.0001 (0.0008)
High-risk registered owner	0.0105 (0.1019)	0.0105 (0.1019)	0.0000 (0.1019)	0.0001 (0.0002)	-0.0013 (0.0014)	0.0004 (0.0003)
Log monthly income	8.0686 (0.7450)	7.8917 (0.8117)	0.1769** (0.0812)	-0.0298 (0.0197)	-0.0335 (0.0274)	0.0057 (0.0046)

Standard errors in parentheses (clustered at municipality level). Early (late) adopters implemented the reform between May 2002 and March 2004 (April 2004 and December 2004). Data are from 2000 and 2001 ENE. Changes are changes in ENE municipality averages from 2000-IV to 2001-IV. Columns 4 and 6 present the coefficients of a separate regression for each variable (or change in each variable) on quarter of implementation. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%.

average to less than 3 corresponds to going from the 30th percentile in registration procedures to the 2nd percentile, which is equivalent to going from Bangladesh or Kazakhstan to Australia or New Zealand (World Bank, 2006).

SARE applied only to nongovernmental firms in industries that do not require special permits and do not present a serious risk to public health, public security, or the environment. These eligible “low-risk” industries made up 55% of all industries and 80% of operating firms, typically micro, small, or medium-size businesses—for example, commerce and restaurants. Examples of high-risk or regulated industries are chemical production and transportation (including taxis).

Postreform administrative data from one municipality, Guadalajara, show that most of the newly registered business were video game console rental, computer rental, small grocery stores, clothing stores, home-style food-to-go vendors, and beauty salons, with an average investment of US\$5,471 and an average employment per firm of 1.27.

### III. Mexican Employment Survey Data

My main outcome data come from the Mexican National Employment Survey (ENE), the survey that the Mexican government relies on for calculating unemployment statistics and the size of the informal sector. The ENE 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 (nineteen quarters in total). After 2004-IV, the ENE was changed to a new survey, and these data have not been made publicly available.

I construct two of my main outcome variables by creating dummy variables for each person in the sample, indicating whether the person (a) owns a registered business or (b) is a wage earner.<sup>6</sup> I also use monthly income as an outcome variable and the following individual background variables as controls: age, gender, marital status, and education dummies.

<sup>6</sup> Bruhn (2008) includes a detailed description of how I constructed these dummy variables.

Table 1 includes summary statistics for the outcome variables in the prereform period (2000-II to 2001-IV). Half of the people in my sample are wage earners. Another 8.4% are registered business owners. Most registered business owners are in low-risk industries (7.3%), while only 1.1% own registered high-risk businesses.

### IV. 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 2000-II to 2004-IV. I restrict the sample to the 34 municipalities in my data that adopted the reform by December 2004. This allows me to exploit only variation in the time of adoption, holding the decision to adopt fixed. 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>7</sup> Moreover, the part of the analysis that looks at the effects of the reform on different prereform groups requires me to observe individuals at least once before the reform was implemented.

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 off 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. To gauge whether there was such a specific pattern of implementation, I performed a number of checks.

First, I interviewed staff members at the COFEMER who were in charge of implementing the reform. They informed me that their goal was to bring the reform to the urban municipalities with the largest volume of economic activity. However, within this set, they did not spe-

<sup>7</sup> Expanding the sample to sixty municipalities by matching late-adopter or nonadopter municipalities to municipalities that adopted before 2004-IV leads to similar results for all outcome variables.

TABLE 2.—IMPACT OF BUSINESS REGISTRATION REFORM

	Dependent Variable					
	Registered Business Owner Dummy	Low-Risk Registered Business Owner Dummy	High-Risk Registered Business Owner Dummy	Wage Earner Dummy	Low-Risk Wage Earner Dummy	High-Risk Wage Earner Dummy
	(1)	(2)	(3)	(4)	(5)	(6)
Reform dummy (SARE)	0.0031** (0.0015)	0.0037** (0.0014)	−0.0006 (0.0005)	−0.0002 (0.0027)	0.0066** (0.0029)	−0.0068*** (0.0029)
R <sup>2</sup>	0.057	0.047	0.014	0.119	0.078	0.114
Number of observations	1,636,250	1,636,250	1,636,250	1,636,250	1,636,250	1,636,250

Standard errors in parentheses (clustered at municipality level). Regressions include quarter and municipality fixed effects, as well as individual-level and municipal-level control variables. Individual-level control variables are gender, age, marital status, and education dummies. Municipal-level control variables include dummies indicating whether the local party in power was the party of the president (PAN) and a dummy for whether the state and municipal ruling party were both PAN, and census variables interacted with a linear time trend. The census variables are log GDP per capita, log number of economic establishments per 1,000 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%.

cify a particular pattern of implementation. In fact, they mentioned that all local governments they approached were interested in adopting the reform, but COFEMER was not able to implement the reform in all municipalities simultaneously since they did not have enough personnel.

Second, I examine prereform changes in outcome variables. Although the identification assumption of no differential trends in absence of the reform is fundamentally untestable, it is likely to hold if there are no initial systematic differences in trends. Column 5 of table 1 displays the differences in average 2000-IV to 2001-IV changes across early- and late-adopter municipalities. Column 6 reports coefficients from a regression of the average change in each variable on quarter of implementation. The average changes in outcome variables are not statistically different across early- and late-adopter municipalities. They are also not significantly correlated with the quarter of implementation, suggesting that the identification strategy is valid.<sup>8</sup>

## V. Results

I obtain the main results by estimating the following regression with OLS,<sup>9</sup>

$$y_{ict} = \alpha + \beta_c + \gamma_t + \delta SARE_{ct} + \pi Z_{ict} + \varphi EC_{1999} \times t + \lambda POL_{ct} + \varepsilon_{ict},$$

where the subscript  $i$  denotes individuals,  $c$  denotes municipalities, and  $t$  denotes quarters. The 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 1 for the quarter in which the reform was implemented and for all following quarters. Control variables are individual background variables,  $Z_{ict}$ , and 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 1,000 capita, log fixed assets per capita, and log investment per capita. The regressions also include two political dummies,  $POL_{ct}$ . The first one indicates whether the governing party of a municipality was the party of the president (PAN). The second indicates whether both the municipal party and the party of the state governor were the PAN. The standard errors of the regressions are clustered at the municipality level.

<sup>8</sup> Bruhn (2008) also shows that there are no statistically significant differences or patterns in the changes of 1994 to 1999 Economic Census variables. There are some differences in the levels of 1999 Economic Census variables, which is why all the regressions control for these levels interacted with a linear time trend.

<sup>9</sup> Probit regressions give similar results.

### A. Registration and Employment

Column 1 in table 2 shows a positive and significant impact of the reform on the number of registered businesses. Columns 2 and 3 break down the impact on registered businesses by low-risk and high-risk businesses. Since only the low-risk businesses are eligible for the reform, the increase in registered businesses should come only from low-risk businesses. This is indeed what the results in columns 2 and 3 confirm. The fraction of low-risk registered businesses increased by 0.37 percentage points (an increase of 5% from the pre-reform level of 7.4%), while there was no statistically significant change in high-risk registered businesses. The 0.37 percentage point increase in low-risk registered businesses corresponds to an increase in 30,678 firms for all 34 municipalities or 902 firms per municipality, on average.

Columns 5 of table 2 shows that the fraction of wage earners increased by 0.64 percentage points in low-risk industries, corresponding to an increase of about 2% over the prereform fraction of low-risk wage earners. Dividing the increase in 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, which is 6.3. Since larger firms may be more likely to register their workers with IMSS, it is perhaps not surprising that they capture larger newly created firms on average. Note that table 2 does not show an increase in the total fraction of wage earners. Instead, the increase in wage earners in low-risk industries went along with a decrease in wage earners in high-risk industries, suggesting that the reform shifted employment from ineligible to eligible industries.

In order to verify that the estimated effects coincide with the time of the reform, I plot the coefficients of the following regression,

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

where  $Quarter_{lc}$  is a set of dummy variables for lag and lead quarters relative to the time of implementation in a given municipality. I limit this regression to observations that fall between  $Quarter_{-8}$  and  $Quarter_{+6}$  since the staggered implementation of the reform implies that I do not observe the outcomes for all lags and leads.

Figure 1 shows the coefficients on the lag and lead dummies for the low-risk registered business owner dummy as the outcome variable. The two dashed lines are the 95% confidence bands of the estimates. Between quarter  $-8$  and quarter  $-1$ , the estimated differences tend to be negative or close to 0. 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  $+5$ . For this lead quarter, the data set covers fewer municipa-

FIGURE 1.—EFFECT ON LOW-RISK REGISTRATION OVER TIME

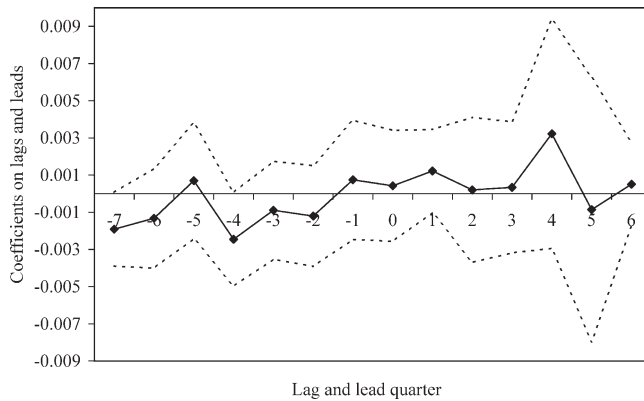
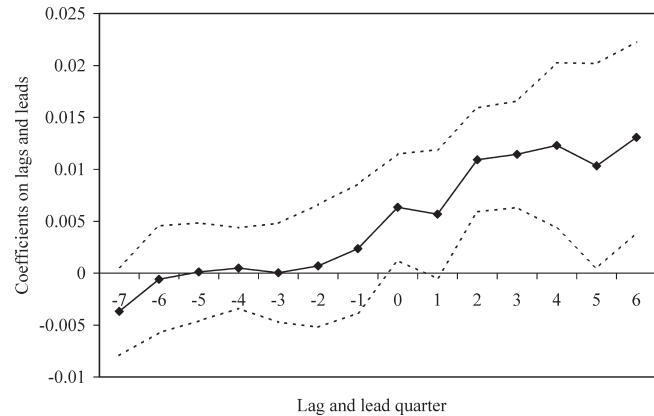


FIGURE 2.—EFFECT ON LOW-RISK WAGE EARNERS OVER TIME



lities than for previous lead quarters, implying that the decrease in registration may be driven by missing data rather than by an actual reversal of the effect of the reform. Figure 2 shows the effect on wage work in low-risk industries broken down by lag and lead quarters. The effect is close to 0 until the quarter of implementation. From quarter 0 or quarter +1 on, the variable is significantly and increasingly higher than before.

For 27 municipalities in my sample, COFEMER reports statistics on prereform and postreform registration procedures. I use this information to test whether the effects are greater in municipalities with greater reductions in registration procedures. Column 1 of table 3 shows that the reform had a bigger effect on registration in municipalities that saw a higher reduction in procedures. The fraction of wage earners in low-risk industries also increased more in municipalities where the reduction in registration procedures was greater (column 2).

Next, I examine whether the increase in registered businesses comes from new business creation for informal businesses registering by making use of the panel structure of the data. For each individual, I create four dummy variables specifying which occupation they held when I first observed them in the prereform period. The four possible occupations are registered business owner, nonregistered business owner, wage earner, and not employed (unemployed or out of the labor force). I then drop the first period of observation for each person and use the remaining data in a regression that includes the reform dummy interacted with all four past occupation dummies. The regression also includes quarter, municipality, and past occupation dummies, as well as the interactions of quarter and municipality dummies with occupation dummies. It also includes the individual control variables interacted with occupation dummies.

Table 4 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, however, is positive and statistically significant. Column 2 breaks down the effect on the fraction of wage earners in low-risk industries by prereform occupation type. The results show that individuals who were previously not employed switched to being low-risk wage earners after the reform. Moreover, columns 2 and 3 together indicate that previous wage earners moved out of high-risk industries and into low-risk industries.

### B. Prices and Income

This section first examines the effect of the reform on the Mexican consumer price index (CPI), constructed by the Bank of Mexico. Since price data are available only at the city level, I convert the price data to

TABLE 3.—IMPACT OF REFORM BY REDUCTION IN NUMBER OF PROCEDURES

	Dependent Variable:	
	Low-Risk Registered Business Owner Dummy	Low-Risk Wage Earner Dummy
	(1)	(2)
Reform dummy (SARE)	0.0015 (0.0019)	-0.0015 (0.0036)
Reform Dummy × Reduction in Procedures	0.0004** (0.0002)	0.0011*** (0.0003)
$R^2$	0.048	0.076
Number of observations	1,392,477	1,392,477

Standard errors in parentheses (clustered at municipality level). Regressions include quarter and municipality fixed effects. They also include 1999 municipality Economic Census variables interacted with a linear time trend, as well as political party dummies and individual background variables. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%.

the municipality level by assigning each municipality the price index of the city where it is located.<sup>10</sup> CPI data exist for only 20 municipalities in my sample. As opposed to the ENE data, which have a quarterly frequency, the price data come at a monthly frequency. The results in table 5 suggest that the reform had a negative and statistically significant effect on prices. Columns 2 and 3 break down the CPI into low-risk and high-risk industries, showing that prices declined by about 1% in low-risk industries. This effect may seem large, but it is in line with the previous literature. Bresnahan and Reiss (1991) find that product prices fall by about 8% when going from one or two firms in the market to between three and five firms. While the 8% estimate is for small towns in the United States, the effect might arguably be similar within local neighborhoods in Mexican cities. The administrative data from the municipality of Guadalajara indicates that many of the firms that opened after the reform were small neighborhood stores, which presumably compete with a handful of other stores in the neighborhood. Moreover, many of the new firms were in the food and services industries, which together make up about 30% of the CPI.

Second, table 4 displays the effect of the reform on real income by past occupation group. Column 5 shows a decline in real income for past registered business owners by 3%, although this effect is only

<sup>10</sup> In my sample, only three municipalities lie in the same city (Guadalajara, Zapopan, and Tlaquepaque in the City of Guadalajara). These three municipalities are thus assigned the same price data. All other municipalities have unique observations.

TABLE 4.—IMPACT OF REFORM BY PREREFORM OCCUPATION

	Dependent Variable:				
	Low-Risk Registered Business Owner Dummy	Low-Risk Wage Earner Dummy	High-Risk Wage Earner Dummy	Log Real Income (OLS)	Fourth Root of Real Income
	(1)	(2)	(3)	(4)	(5)
SARE × Past Registered Owner	0.0021 (0.0082)	0.0075 (0.0058)	0.0030 (0.0047)	-0.0296 (0.0201)	-0.0431 (0.0453)
SARE × Past Nonregistered Owner	0.0005 (0.0064)	0.0021 (0.0082)	-0.0048 (0.0029)	0.0153 (0.0236)	0.0808 (0.0516)
SARE × Past Wage Earner	0.0025* (0.0015)	0.0083* (0.0043)	-0.0148*** (0.0043)	-0.0021 (0.0075)	-0.0418 (0.0281)
SARE × Past Not Employed	-0.0004 (0.0016)	0.0095*** (0.0028)	0.0014 (0.0022)	—	-0.0590* (0.0302)
$R^2$	0.352	0.211	0.284	0.387	0.555
Number of observations	1,051,295	1,051,295	1,051,295	419,718	733,679

Standard errors in parentheses (clustered at municipality level). Past occupation variables are prereform occupations when the person was first observed. The past not employed include the unemployed and people who are not in the labor force. The regressions in column 4 drop all individuals who were initially not employed since their past income is zero. Columns 4 and 5 include only the twenty municipalities for which the price index is available. Regressions include quarter, municipality, and past occupation dummies, as well as the interactions of quarter and municipality dummies with occupation dummies. They also include individual control variables interacted with occupation dummies, 1999 municipality Economic Census variables interacted with a linear time trend, and political party dummies. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%.

TABLE 5.—IMPACT OF REFORM ON PRICES

	Dependent Variable:		
	Log Consumer Price Index	Log CPI for Low-Risk Industries	Log CPI for High-Risk Industries
	(1)	(2)	(3)
Reform dummy (SARE)	-0.0067* (0.0037)	-0.0104** (0.0045)	-0.0047 (0.0042)
$R^2$	0.980	0.961	0.951
Number of observations	1,140	1,140	1,140

Standard errors in parentheses (clustered at municipality level). Regressions include month and municipality fixed effects, 1999 Economic Census variables interacted with a linear time trend, as well as political party dummies. Regressions cover 57 months for the twenty municipalities for which the price index is available. Significance levels: \* 10%, \*\* 5%, \*\*\* 1%.

marginally significant (at the 15.8% level), possibly due to the smaller sample size that results from using only municipalities with CPI data.

The results above indicate that the previously not employed were more likely to work as wage earners after the reform. These switchers should have seen an increase in their income. However, individuals who were initially not employed have no income and are thus dropped from the log regressions in column 5. Column 6 includes the no income observations by using the quadratic root of income as the outcome variable. The quadratic root mimics the logarithmic function well for positive numbers (see Thomas et al., 2003). The regression in column 6 shows a significant increase in the income of the previously not employed, of about 6%.

## VI. Conclusion

This paper uses microeconomic data to provide evidence that simplifying entry regulation increases the number of registered businesses. It shows that after a recent business registration reform in Mexico, the total number of registered businesses increased by 5% in eligible industries. This increase in registered businesses was due to former wage workers' opening businesses, and not due to unregistered business owners' registering their businesses.

The paper also shows that the fraction of wage earners in eligible industries increased by 2.2% after the reform. This effect mirrors the cross-country results on output growth, where less complicated regulation is associated with higher growth in output. Moreover, prices decreased by 1% after the reform.

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 lowering prices.

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