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IN THE LABOR MARKET: THE CASE OF THE  
“SEGURO POPULAR” PROGRAM IN MEXICO**

Mariano Bosch  
Universidad de Alicante

Raymundo M. Campos-Vázquez  
El Colegio de México

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# **The trade-offs of social assistance programs in the labor market: The case of the “Seguro Popular” program in Mexico**

Mariano Bosch\*

Universidad de Alicante

Raymundo M. Campos-Vázquez\*\*

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**Abstract:** In 2002, the Mexican government began a tremendous financial effort to provide health insurance, Seguro Popular (SP), to the 50 million uninsured in Mexico. In doing so, the states and municipalities offered virtually free health insurance to uncovered self-employed and informal salaried workers substantially altering the incentives for workers and firms to operate in the formal economy. We take advantage of the staggered implementation of the program across municipalities to estimate the effects of the SP in the labor market. We find that the SP had a negative effect in the creation of formal jobs, especially in small and medium sized firms. According to our estimates, had the program not been in place, 31.000 more employers and 300.000 new formal jobs should have been registered with Mexican social security. These represent 3.8% and 2.4% of the stock of registered employers and employees in 2002 when the program started.

JEL codes: O12, I18, O15, I38, O17.

Keywords: Social assistance program, Informality, Labor Market, Mexico

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\* Email: [mbosch@ua.es](mailto:mbosch@ua.es), <http://merlin.fae.ua.es/mbosch/>. Address: Universidad de Alicante, Departamento de Fundamentos del Análisis Económico, Campus de San Vicente, 03080, Alicante, Spain. Phone: +34-965-903614. Fax: +34-965-903898

\*\* Email: [rmcampos@colmex.mx](mailto:rmcampos@colmex.mx), <http://raycampos.googlepages.com>. Address: El Colegio de México, Centro de Estudios Económicos, Camino al Ajusco 20, Col. Pedregal de Santa Teresa, 10740, México DF. Phone: +52-55-54493000, ext. 4153. Fax: +52-55-56450464

## **I Introduction**

In 2002, the Mexican government began a tremendous financial effort to provide free health insurance to the 50 million uninsured in Mexico, a program known as Seguro Popular, or Popular Health Insurance (SP). By the fourth quarter of 2009, the program was in place essentially throughout the country covering over 31 million individuals. According to the OECD (2005), the SP entailed an 85% increase in government expenditure per capita for the uninsured sector households. From 2004 to 2009, the expenditures in SP have increased tenfold, from close to 350 million USD to almost 3500 million USD (Secretaria de Salud, 2010). So far the SP has proven to be an effective tool to reduce catastrophic health expenditure, especially among the poor (see Gakidou et al, 2006, Knaul et al. 2006, and Barros, 2008). However, there is yet to be evidence of significant improvements in health for the participants in the program.

One of the main concerns to policy makers is how the SP and other similar social assistance programs might shift the incentives in the labor market. In particular, the SP is designed in such a way that only those self-employed and salaried workers (and their families) not covered by the official Mexican Social Security can enroll in the program. Theoretically, the effects of such programs are ambiguous. If informal workers are just segmented workers queuing for good and desired formal jobs, such welfare improving programs for the informal might alleviate the needs of those in precarious situations, but do little to alter the incentives to participate in the formal institutions. This is the vision of the early segmentation models proposed by Fields (1975) and Mazumdar (1976).<sup>1</sup> If however as Maloney (1999, 2004), Perry et al. (2007) and Levy (2008) argue, informal workers (especially micro-entrepreneurs and self-employed) optimally self-select into informal activities because of better pay, more flexibility or labor conditions, then it is likely that a substantial fraction of the labor force is at the margin between formality and informality. In this case, in the face of improvements in the services available to informal workers we should expect large shifts of the labor force into informal jobs.

The consensus in the literature (see Fields, 2009) is that these two actors (segmented workers and informal entrepreneurs) coexist in the Mexican labor market,

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<sup>1</sup> See Fields (2009) for an excellent survey on dual labor markets in developing countries.

and in general in other developing countries. It is then likely that social assistance programs like the SP change the cost-benefit analysis of participating in the formal labor market among workers and firms. However, we have little empirical evidence on the magnitude of this transfer of labor force from one sector to the other, let alone the consequences for labor productivity, investment and growth.

This paper uses social security data for 1395 municipalities out of 2439 municipalities in Mexico to study the change in the creation of formal jobs during the period of implementation of the SP, between 2000 and 2009. We exploit the variation generated by the time staggered entry of municipalities into the program. The program started as a pilot during 2002 in 5 states and by the end of 2009 virtually all municipalities in the country had enrolled into the program.

We find evidence that the SP significantly changed the trend in the affiliation of employers and employees to the Mexican Social Security. During 2000-2009, the number of private employers and employees registered with Mexican social security increased from 757.000 to 821.000 (8.4%) and from 12.2 millions to 13.9 million (14%), respectively. We estimate that had the program not been in place, around an additional 31.000 employers and 300.000 employees would have registered with Mexican social security. These represent 3.8% and 2.4% of the stock of registered employers and employees in 2002 when the program started.

This change in trends is a concern. First, because social assistance programs, like the SP, are almost fully funded by public monies and hence they are likely to be a substantial burden for the state in the face of large shifts of labor force into this type of program. Second, and perhaps more importantly, as Levy (2008) argues, the shift towards informal activities might generate proliferation of low productivity micro-firms, underinvestment and ultimately lower growth.

We are not the first to examine the impact of the SP in the labor market. Barros (2008), Campos-Vázquez and Knox (2008) find no impact of the program in formal employment trends. We view our paper as complementary evidence. While these rely on household survey data, we exploit actual social security data which covers virtually the whole universe of municipalities with registered formal employment in the country.

Further, our data covers up to the last quarter of 2009 when the roll out of the program had been completed and some municipalities have already been six years in the program. We show below that this is a major advantage since we estimate that the effects of the program in the labor market occur with important lags. Further, we show that the effects of the SP are stronger for small and medium firms and in relatively small municipalities which tend to be underrepresented in labor surveys.

The rest of the paper is organized as follows. Section II describes the social security system in Mexico and the reform initiated in 2000. Section III describes the data and provides an overview of the state of the Mexican labor market in the 2000s. Section IV studies in detail how the SP was implemented. Section V shows the main results of the paper. Section VI provides the conclusion.

## **II Mexico's Health Care System and Reform: The Seguro Popular**

### *Mexico's pre-reform Health System*

Mexico's current health care system was born in 1943. Right from its birth the system was dualistic in nature. Two institutions were created for formal sector workers: the Instituto Mexicano de Seguro Social (IMSS) and later (created in 1959) the Instituto de Seguridad y Servicios Sociales de los Trabajadores del Estado (ISSSTE) for registered private and public employees, respectively. These two institutions operate under mutual systems whereby private and public employed workers (and their families) are entitled to a full spectrum of benefits, not only health care, but other benefits such as pension and disability benefits, housing loans and in the case of dismissal, severance payments. In exchange for these benefits and rights, employees and their employers pay payroll taxes amounting to roughly 25 percent of their salaries excluding other local and federal taxes.<sup>2</sup> Under this system, the workers and their families are not charged for the use of health services and they have access to a wide range of prescription drugs.

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<sup>2</sup> However, the amount paid for a worker who earns twice the minimum wage in Mexico is around 30% as shown in Levy (2009), the difference is due to the upper limit of payroll taxes.

In parallel to IMSS and ISSSTE, the Secretaria de Salud y Asistencia (SSA) was created to serve all individuals outside the formal sector. SSA's main role was purely one of "social assistance". Under this system, the user is charged for the medical services and medicines. However, due to the overwhelming demand for affordable health care for poor families, a number of programs under the umbrella of the SSA were designed to provide access to health care to low income population (OECD, 2005).

By 2000, the inequalities in this system were apparent. Nearly 50 percent of the Mexican population, amounting to 47 million people, was not insured through either IMSS or ISSSTE and were relying on the SSA or private institutions for their health care. The World Health Organization ranked Mexico 144th out of 191 countries in fairness of health care and the Mexican Ministry of Health estimated that 2 to 4 million families, or 10 to 20 percent of the total population, suffered catastrophic and impoverishing health care expenses every year. These families were almost exclusively drawn from the lowest income quintile, and were four times more likely to be uninsured than insured (Knaul and Frenk, 2005).

### *The Reform*

In the early 2000s the Federal Government designed the Sistema de Protección Social en Salud, System for Social Protection in Health (SPS), that through the SSA was aimed at providing affordable health coverage for those not covered by the IMSS or ISSSTE. A key component of this reform was the SP (Popular Health Insurance) program. The goals of SP are three-fold: (1) financial protection for workers in the informal sector, (2) the creation of a culture of prepayment for SP beneficiaries, and (3) a reduction in the number of families that are driven into poverty due to unexpected health shocks (Secretaria de Salud, 2005).

The idea of providing health care to the uninsured was not new. The SSA and its state counterparts have been providing assistance programs for over 60 years and substantial portions of public health expenditure were geared towards the uninsured (by 2000, 32% of total public health expenditure was implemented by the SSA). The novelty of the SP was threefold. First, it offered financial protection by a substantial reduction in the costs of health care for many families. The original package of benefits

for program affiliates gave virtually free access to 169 interventions and 333 drugs, covering 90 percent of the disease burden in Mexico.<sup>3</sup> By 2006, the benefits package was expanded to cover 95 percent of the disease burden. Preliminary studies of the program have shown that it appears to be achieving its stated goals. Gakidou et al. (2006) found that SP affiliates used more health services and were less likely to incur catastrophic health expenses than the uninsured, and Knaul et al. (2006) found a reduction in the deepening of poverty from health spending between 2000 and 2004.

Second, there was a substantial increase in health budget dedicated to the improvement of the service provided by the SSA. Figure 1 shows the health care expenditures for the insured population (IMSS+ISSSTE) and for the uninsured population (SSA) from 1993 to 2008. Although the budget for SSA had been steady increasing in the 1990s, the SP gave it an additional boost, increasing from 0.8 % share of GDP in 2003 to 1.2% in 2008. In the same period the IMSS expenditure declined from 1.7% to 1.5%. This was translated into more health resources being devoted to serve the uninsured population. Figure 2 shows the evolution in the number of medical doctors and nurses dedicated to the insured and the uninsured population. The change in the trend of the resources targeted for the uninsured is notable after 2004, especially in the number of nurses.

Finally, the SP program gave a new sense of entitlement to those uninsured families. The new General Health Law clearly states that SP affiliates will have access to a list of health interventions and respective drugs that has been continuously expanded. In fact, at the moment of affiliation, all families receive a Charter of Rights and Duties that explicitly lists the health interventions to which they are entitled and the health care facilities in which they can demand them (Frenk et. al., 2009).

### **III Data and the Mexican Labor Market in the 2000s**

The Mexican labor market is archetypical of a middle income country. A large share of the labor force is classified as informal or underground unregistered economy.

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<sup>3</sup> Although in principle the SP was aimed at providing a culture of co-payment depending on their declared level income. By 2009 only 2 percent of the total affiliates were contributing to the system (Secretaria de Salud, 2010)

Although there is no consensus on what exactly determines the divide between formality and informality, broadly speaking, formal workers are those working in firms licensed with the government and conforming to tax and labor laws, including minimum wage directives, pension and health insurance benefits for employees, workplace standards of safety, etc. Informal workers, on the contrary, are those owners of firms that are largely de-linked from state institutions and obligations and their employees are not covered by formal labor protection.

The best source to measure formal employment is the administrative data from the Mexican Institute of Social Security (IMSS). By law all employers and private sector employees have to be registered with the IMSS.<sup>4</sup> Registration with IMSS entitles the worker to a number of benefits such as health insurance, pension contributions, housing loans, among others. The data employed in this paper relies on the IMSS records for the entire universe of municipalities in Mexico from 2000 to 2009. Unfortunately, we do not have access to the micro-data. Instead, we observe the total number of employers and employees affiliated with IMSS in every quarter from 2000 to 2009 as well as worker tabulations by gender, age and firm size.<sup>5</sup> The main measure of employment used by the IMSS is the total number of permanent workers and temporary urban workers (TPEU). Due to the high degree of seasonality, temporary rural workers are not included in this measure. In any case they constitute less than 0.5% of affiliation to IMSS. The IMSS only keeps records of around 1850 municipalities out of the existing 2439 municipalities in Mexico. This is essentially due to the fact that the IMSS tends to merge smaller municipalities into larger entities for tax purposes. We restrict our sample to those municipalities for which we have entire employment histories from 2000 to 2009 and are left with 1395 municipalities, which according to the IMSS records constitute the 98% of all private formal employment in the country. We call these *panel municipalities*. We also consider a restricted sample of municipalities which implemented the SP only after the *pilot phase* was over (see details below) and was

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<sup>4</sup> This only refers to private workers. A parallel institution for public workers is ISSSTE.

<sup>5</sup> The IMSS keeps two separate registers, one for employers (patrones) and one for employees. According to the Ley de Seguro Social (Social Insurance Law) the employer is required to be registered with IMSS, and register his/her employees. The employer registry refers to the firm (not the establishment). Hence firms with several establishments will still correspond to a single employer. However, the same employer may have several entries in the employers registry if he/she owns firms with different activities and not linked economically.

passed into law in 2004. This effectively removes the 340 municipalities that started to implement the SP in 2002 and 2003. We call this group *post-pilot municipalities*.

We merge this data with the administrative records of SP by municipality. In those records we observe the number of families and individuals affiliated to the SP in each quarter from 2002-2009. We define that the SP is operating in a municipality if the number of individuals affiliated is greater than 10. This minimum figure was selected as there were some municipalities that show very low affiliation (0 or 1 affiliates for several quarters) especially at the start of the program, making it difficult to establish whether the program was operational at that municipality.<sup>6</sup>

We further merge this data set with the 2000 Population Census to obtain a series of pre-treatment municipality characteristics. In particular, we use age and gender profiles, industry shares at the two digit level (16 industries), median income, IMSS coverage, rural/urban status, poverty level and average years of education. Finally, we have access to the annual number of doctors, nurses and clinics (doctor's offices or *consultorios*) employed by the SSA to serve the uninsured population by municipality from 2001 to 2008.<sup>7</sup>

Figure 3 shows the evolution of formal employment during the 2000-2009 period. Several facts merit attention. The early years in our data capture the effects of the 2001-2002 recession when average growth in Mexico was -2.7% and formal employment creation was almost negligible. The recovery, however, was strong and growth resumed in the first quarter of 2006 with an annual growth rate of 6.1%. During this period there was substantial job creation in the formal sector. Overall, the number of workers affiliated with IMSS grew from 12.4 million in the first quarter of 2003 to 14.5 million in the last quarter of 2008, an increase of 17%. The effects of the global recession started to destroy employment in Mexico at a rapid rate at the beginning of 2009. Within four quarters, half a million formal jobs were destroyed.

To put these trends into context we compare our registry data with Mexican survey data from the same period. In the late 1980's, the best reliable information of the

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<sup>6</sup> We check the robustness of our results by using other definitions of program implementation in later stages of the paper with very similar results.

<sup>7</sup> Data obtained online from Dirección General de Información en Salud (2010).

Mexican labor market was the National Survey of Urban Employment (ENEU) which was representative of cities over 100.000 inhabitants. At the turn of the century, the ENEU was systematically complemented to obtain national representativeness to constitute the National Employment Survey (ENE). In 2005 a new survey replaced the ENEU/ENE, the National Survey of Employment and Occupation (ENOE), also representative at the national level. One of the advantages of the survey data is the ability to measure not only the formal sector but also self-employment as well as unregistered wage employment.

Table 1 shows the main aggregates of employment (in millions of workers) for Mexico during the 2000's for the ENE/ENOE survey data and the IMSS registry data for the 2000-2004 and 2005-2009 periods. Several facts merit attention. First, survey data provides a better idea of the structure of the Mexican labor market. Similar to other middle incomes economies, close to a third of employment consists of self-employed workers.<sup>8</sup> The other two thirds are wage workers. Although all wage workers should be registered with the Mexican Institute of Social security, only around 45% of them are.

Second, although the ENE and the ENOE are not strictly comparable (there is a substantial jump in employed population between the 4<sup>th</sup> quarter of 2004 and 1<sup>st</sup> quarter of 2005), they portray similar trends to those found in the IMSS data. According to the ENE, the 2000-2004 period shows an increase in total salaried employment of 8% or 1.9 million workers. However, around 75% of the new salaried employment created was informal (1.4 million vs 0.5 million formal). The 2005-2009 periods captures most of the fast growth in formal employment. According to the ENOE, salaried employment grew by 11% or 2.6 million workers, 1 million of which were affiliated with IMSS. Yet despite the fast job creation during the second part of the 2000's, the share of salaried workers affiliated with IMSS has remained relatively stable at around 43% of total wage employment and 30% of total employment in the country.

#### **IV Implementation and Effects of the SP**

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<sup>8</sup> The self-employed are normally considered part of the informal sector since they lack the benefits and coverage of formal wage employment. Self-employed may choose to pay their contributions to IMSS, however, on average only 2% choose to do so. This does not make self-employment illegal labor in Mexico, since these contributions are not a legal requirement (unlike other similar countries like Brazil).

The SP was implemented in stages across states. Passed into law in 2004 as a modification of the existing General Health Law, the program actually began with a pilot phase in five states in 2002 (Colima, Jalisco, Aguascalientes, Tabasco and Campeche). According to the SSA, these states were chosen initially due “to the capacity of offering the services, large concentration of urban and semi-urban population and the existence of previous benefit programs from the government” (Secretaria de Salud, 2002). In order to start the program in the rest of the 26 states and Mexico City, the Federal Government needed to sign an agreement of participation with each state. However, during 2002 and 2003, 14 other states (Sinaloa, Tamaulipas, Baja California, San Luis Potosí, Sonora, Coahuila, Guanajuato, Zacatecas, Oaxaca, México, Quintana Roo, Guerrero, Hidalgo, Chiapas and Morelos) started to implement the SP without a formal agreement with the Federal Government. According to SP officials, this was possible before 2004 if the municipal government agreed to offer the program. This was still considered by the SSA as the pilot phase. Throughout 2003, 2004 and 2005, all states except for Mexico City (DF) had signed the official agreement with the Federal Government. This agreement included not only the required funds to finance the program but also its rules of operation. The rules of operation state that the program needs to be implemented in localities with high poverty incidence and/or localities with indigenous population, but the localities also needed to have health facilities in close range. Hence, the decision of which municipalities were affiliated first was a decision based on existence of agreements with state governments.<sup>9</sup>

Figures 4 and 5 show the total coverage of SP over time from the first quarter of 2000 up to the fourth quarter of 2009 (the data is drawn from SP administrative records). In the initial years of the program, the number of beneficiaries was low. For example, between 2002 and 2004, around one third of municipalities were enrolled in the program and the number of registered families was around 1.5 million families, representing roughly 6% of the families in Mexico.<sup>10</sup> By 2008, over 7.5 million families and 23 million individuals were affiliated with SP, representing around 30 percent of the total number of families in Mexico. The program expanded rapidly in 2009 covering close to 31 million individuals and close to 10 million families.

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<sup>9</sup> We interviewed senior officials in charge of the SP. We asked repeatedly about how states decided to implement the program. The answer was that each state decided according to its goals, but in general states needed to satisfy the rules of operation of the program.

<sup>10</sup> Population in the municipality over time is obtained from a simple interpolation using the 2000 Population Census and 2005 Population Count.

Figure 6 shows the variation of participation in the program across and within states. The graphs measure the percent of municipalities in each state that are participants in the program. In general, richer and northern states expanded the program to all their municipalities rapidly, while poorer states had more trouble in accomplishing that objective. There is a trade-off between which municipalities to cover; while richer municipalities have health facilities, poorer municipalities in potentially greater need of the program may have a problem in satisfying the criteria for existence of health facilities.

A key issue in our identification strategy relies in the exogeneity of the SP implementation at the municipality level. As mentioned above, states decided to participate in the program and it is not clear how they determined which municipalities to cover first. Table 2 investigates the determinants of implementation more systematically. We compiled characteristics of municipalities from the 2000 census. We use these “pre” characteristics from the 2000 Population Census to predict the date that the municipality joined the SP scheme. The dependent variable is the quarter and year of the municipality’s SP start date, expressed as an index equal to one, beginning in the third quarter of 2002. This analysis is motivated by Hoynes and Schanzenbach (2009) in which they investigate the determinants of early entrance to the Food Stamp program in the U.S. We employ as regressors a comprehensive set of municipality level covariates, such as a municipality’s population, share of IMSS or ISSSTE insured population, unemployment rate, industry shares (not shown) and some state level variables, such as state population, and the political party of the governor.<sup>11</sup> We run the regression for all *panel municipalities* and *post-pilot municipalities* (those that implemented the SP after 2003).

We find that systematically more populated municipalities and those in smaller states (only in the panel municipalities) joined the program at earlier stages. This is consistent with the rules of operation of the SP which require municipalities to have health facilities. This is also consistent with the political economy argument in Diaz-Cayeros et. al. (2006) who argue that political reasons were at play during the rollout of the SP municipalities. In particular, they argue that smaller states were given preference

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<sup>11</sup> Political party affiliations obtained from <http://www.cidac.org>.

to achieve full coverage of the SP in all state municipalities so the Federal Government could claim full coverage before the presidential election of July 2006. Furthermore, there seems to be a correlation between the early implementation in the SP and the affiliation of the state governor in post pilot municipalities.

Table 2 also shows that implementation of the program does not seem to depend on the share of insured population in the municipality. We find municipal average income to have only weak predicative effects in the implementation of the SP for the panel municipalities and none for the post pilot municipalities. None of our 16 industry variables capturing industry shares is significant and thus we do not report them. This is an indication that employment composition at the municipality level was not a major determinant in the rolling out of the SP. In all, we can only explain 25 percent of the cross sectional variation in implementation. Hence, Table 2 provides no empirical evidence towards targeting of SP in specific municipalities.

#### *Financing and the Distribution of Resources*

One of the SP goals was to increase health care spending in Mexico by 1% of GDP (Knaul and Frenk, 2005). The budget of SP has increased tenfold from 2004 to 2009.<sup>12</sup> Moreover, as mentioned previously, the program will eventually cover up to 47 million people, where in 2009 the program covered close to 31 million individuals.

The financing of the SP was co-paid between the Federal Government and the states. For participating states, the Federal Government pledge to transfer per beneficiary household 15 percent of the minimum wage in Mexico City (known as “Cuota Social”) and 1.5 times the “Cuota Social” (known as “Aportación Solidaria”). Simultaneously, state governments needed to fund an additional 0.5 times the “Cuota Social”.<sup>13</sup> The total contribution per beneficiary household is close to 45 percent of the minimum wage in Mexico City. Except for poor households, families are required to pay a specific fee according to income. These annual fees vary from close to 60 USD

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<sup>12</sup> In 2009, the budget was close to \$3500 million USD - this amount represents roughly half of the total budget to the Ministry of Health.

<sup>13</sup> In December 2009, the funding was reformed. Now the “Cuota Social” is in terms of individuals not households. Since then, “Cuota Social” amounts to 3.92% of the minimum wage in Mexico City per affiliate. Since the start of the program, 11% of transfers are saved or spent for catastrophic expenditures or demand changes.

per family to close to 850 USD per family for rich households. However, according to SP records, 97% of families that come from the first two deciles of the income distribution do not pay at all (Secretaria de Salud, 2008). Federal funds constitute between 80 to 90 percent of the funding of the SP, but it is up to the states to distribute the money to the target municipalities.

One important question is whether a municipality's affiliation to the program brought to the municipality visible effects in the quality of the provision of health services. This is not trivial since the funds distributed by the Federal Government were assigned to the states and they were responsible for distributing them among municipalities for the improvement of the service. Hence, in order to test whether SP affected employment, we first need to show that in fact there was a change in the health services provided at the municipality level. In particular, we test whether the implementation of the SP in a municipality increased the number of doctors, nurses and clinics available for the uninsured population. We follow a difference in difference approach to test whether SP affected health inputs at the municipality level using yearly data. We estimate,

$$H_{m,t} = \alpha + \sum_{\kappa=-3}^{\kappa=3} \pi_{j,m,t} 1(\tau_{m,t} = j) + \lambda_m + \lambda_t + \varepsilon_{m,y} \quad (1)$$

where  $H_{m,t}$  is the number of doctors, nurses and clinics per 1,000 people serving the SSA by municipality from 2001 to 2009 and  $\lambda_m$  and  $\lambda_t$  are municipality and year fixed effects. Furthermore,  $\tau_{m,t}$  represents the event of the year, defined so that  $\tau_{m,t} = 0$  the year of adoption of the SP,  $\tau_{m,t} = 1$  one year after adoption,  $\tau_{m,t} = -1$  one year before adoption and so on. We set  $\tau_{m,t} = -3$  for all event years less than or equal to -3 and  $\tau_{m,t} = 3$  for all event years greater than or equal to 3. All coefficients are measured relative to one year before the implementation of the program  $\tau_{m,t} = -1$ , which is the excluded event year. If the program had an effect of the dependent variable we would expect to find no significant trend in the years prior to the implementation of the program and expect to see impacts in the years afterwards.

Table 3 shows the results of this exercise. We present the results for our sample of 1395 panel municipalities and for all available municipalities for which we have health resources data. A few comments merit attention. For both samples there is a clear effect of the implementation of the SP on the number of doctors and nurses but not on the number of clinics. By the third year after implementation the doctors and nurses per 1,000 habitants increase on average by 0.05 and 0.09 respectively. There are no clear pre-trends in any of the samples suggesting that we are capturing the causal effect of the program of the resources available to municipalities to increase the quality of their health services. In all, these results suggest that indeed the municipalities that joined the SP saw increases in the resources allocated to provide health care. To our knowledge, we are the first in documenting a clear change in health services at the municipality level after the introduction of SP.

*Possible Effects of the SP in the Labor Market: Competition with IMSS*

The SP changed the incentives for both employers and employees to participate in the formal institutions. In particular, SP changed the incentives to be registered with IMSS. Under the new health law, the self-employed and salaried workers with no affiliation with IMSS could obtain free health insurance from the SSA at their municipalities. Further, the government transferred a substantial amount of resources to meet the demands of the new entitlement, and as observed in the previous section, these resources translated into more doctors and nurses in treated municipalities. Hence, the key question is how much this hampered job creation in the alternative formal system.

Theoretically, two visions of the labor market in developing countries would provide different conclusions about the effect of such a sharp change in the incentives to participate in the formal labor market. If the self-employed and other informal workers are just segmented from the good and desired formal jobs, such welfare improving programs for informal might alleviate the needs of those in precarious situations, but do little to alter the incentives to participate in the formal market. This is the vision of the early segmentation models proposed by Fields (1975) and Mazumdar (1976). If, however, as Maloney (1999, 2004), Perry et al. (2007) and Levy (2008) argue, informal workers (especially micro-entrepreneurs and self-employed) optimally self-select into informal activities because of better pay, more flexibility or labor conditions, then it is

likely that a substantial fraction of the labor force is at the margin between formality and informality. In this case, in the face of improvements in the services available to informal workers, we should expect large shifts of the labor force into informal jobs.

Reallocation of employment from IMSS registration to the SP depends on two main considerations: willingness to affiliate with SP and ability to circumvent IMSS obligations. The former refers to the employer-employee cost benefit analysis of the services provided by IMSS as compared to the SP. As argued by Levy (2008, 2009), formal benefits in Mexico are bundled together in a package. That is, affiliation to IMSS entitles workers to a number of benefits, not only health insurance, but also a pension plan, and access to housing loans, among other benefits. The degree to which an employer or a worker may shift to the SP for the provision of the health care instead of IMSS depends on the workers attachment to those other benefits. Generally, workers with only sporadic contact to formal institutions through temporary jobs are unlikely to develop a strong attachment with the system. For instance, in order to retain pension rights, the Social Security Law requires at least 25 years of contributions.

An additional consideration in the cost benefit analysis refers to the comparison in the quality of services between SP and IMSS. The quality of the provision of IMSS services is unevenly distributed across municipalities. In particular, smaller rural municipalities have reduced access to IMSS facilities compared to larger urban municipalities and hence workers and firms in these municipalities would be more prone to give away those benefits.

The second main consideration is the enforcement of IMSS law. Even if the SP is available in a particular municipality, employers and employees are still obliged to register with IMSS. Following the theoretical literature in the informal sector, larger firms are easily observed by IMSS authorities while smaller firms face a less serious problem of detection if they are violating IMSS rules.

In sum, workers who are less attached to the formal sector may be more affected by the SP program. In the empirical application below, we explore the employment effects of broad groups defined by gender, age and firm size.

### *Identification Strategy*

We use a differences in differences approach to estimate the effect of the SP in Mexico. We take advantage of the rolling out of the program during the period 2002-2009. Our main specification tests whether the affiliation of municipalities with the SP program had any impact on the level of formal employment from the first quarter of 2000 to the last quarter of 2009. In order to obtain a proper estimate of this effect we hypothesize that (conditional to some state and municipality employment trends) the evolution of formal employment would have been the same across municipalities in the absence of the SP.

The 1395 municipalities in the sample belong to 32 independent states. This is important because Mexico, like the U.S., is a federation of states, each with a certain degree of autonomy, with a Constitution, Governor and Congress. State specific policies or macro economic factors might induce a spurious correlation between the implementation of the SP in the municipalities that make part of that State and trends in employment. To capture such state specific macro shocks we allow for a flexible time trend specification at the state level by employing in our regressions state cubic trends. Further, implementation of the SP varied significantly within states allowing the possibility of identifying the effect of SP based on the differential variation of employment across municipalities in the same State (see figure 6). In some of our specifications, we saturate the econometric model and include state X time fixed ( $\lambda_s \times \lambda_t$ ) effects. We cluster the errors at the municipality level to control for the effects of pervasive serial correlation across time in differences in differences models (Bertrand et. al., 2004).

Furthermore, we allow the evolution of employment in the formal sector in municipality  $m$  to depend on demographic and employment composition municipality specific characteristics. Indeed, there are particular industries and age groups that are more likely to participate in the formal sector. For example, on average the manufacturing sector is more formal. Similarly, young uneducated workers are less likely to participate in formal institutions than prime age educated workers (see Perry et al., 2009). However, we lack quarterly data on such characteristics for the 1395 municipalities in our sample. To control for these compositional effects we include the

term  $X_m t$  which captures a number of municipality level characteristics obtained from the 2000 census multiplied by a time trend. In particular, we allow employment trends to vary with age and gender profiles, industry shares at the two digit level (16 industries), median income, IMSS coverage, rural/urban status, poverty level and average years of education.

In practice, this difference in difference approach can be estimated with the following regression

$$E_{m,t} = \alpha + \delta P_{m,t} + \sum_{\kappa=-3}^{\kappa=3} \pi_{j,m,t} 1(\tau_{m,t} = j) + \beta_{x,m} X_m^{2000} t + \lambda_s t + \lambda_s t^2 + \lambda_s t^3 + \lambda_m + \lambda_t + \varepsilon_{m,t} \quad (2)$$

where  $E_{m,t}$  is the log total formal employment registration (employers or employees) in municipality  $m$  at time  $t$ ,  $\lambda_m$  and  $\lambda_t$  are municipality and time fixed effects,  $P_{m,t}$  is the log population at municipality  $m$  and time  $t$ . As in our estimate of the increase in health resources, our identification strategy relies on the assumption that there are no underlying trends in the municipalities that are correlated with implementation of the SP. For this we allow for a flexible time structure both before and after the implementation of the SP. In this case since we are working with quarterly data and to ease the presentation of the tables we group our 3-year pre/post adoption indicator in batches of 4 quarters. In particular,  $\tau_{m,y} = 0$  in the initial year of adoption (meaning the quarter of adoption and the next three quarters),  $\tau_{m,y} = 1$  from the 5th to 8th quarter after adoption,  $\tau_{m,y} = 2$  from 9th to 12th quarters before adoption and so on. We set  $\tau_{m,y} = -3$  for all event quarters less than or equal to -3 and  $\tau_{m,y} = 3$  for all event quarters greater than or equal to 3. All coefficients are measured relative to one year before the implementation of the program  $\tau_{m,y} = -1$ , which is the excluded category. We further present a series of graphs where we do not group the quarters to better visualize the time pattern of the effect of the program.

## V Results

### *Main Results*

Table 4 presents the main results of the paper for the panel municipalities using specification (2). The robustness tests section includes results for post-pilot municipalities. Column 1 uses as dependent variable the number of employers, column 2 uses total wage employment, columns 3 to 7 use employment by firm size: single worker firms, micro firms (2-5 workers), medium firms (6-50 workers), medium to large firms (50 to 250 workers) and large firms (more than 250 workers). In the robustness tests section we also show that results with state X times fixed effects are very similar to those in Table 4.

Table 4 column 1 suggests that within the first year after the implementation of the program, employer affiliation to IMSS falls by 0.7%. By the end of the third year the effect reaches 3.7%. Importantly, we find virtually 0 coefficients for the years before the implementation of the program suggesting a causal interpretation of our results. Although we lack firm size data for employers, IMSS aggregate data suggest that vast majority of the employers registered with IMSS (around 90%) own firms of less than 50 workers. Hence, the movements we observe here are likely to be those of small and medium firm employers. This is important because the SP was specifically targeted to provide coverage to a mass of informal self-employed and owners of unregistered micro-firms, and hence directly competing with IMSS and impacting owner registration. Our results suggest that owner registration in the IMSS fell around 3.8% as a consequence of the program.

The next columns in Table 4 study the impact of the SP for wage employment registration with IMSS. Column (2) shows the effect for the total number of workers in the sample. For our panel municipalities, we detect negative effects after the implementation of the SP, however they are never significant at the conventional levels and they seem to be preceded by an upward pre-trend before the implementation of the program.

However, the split by firm size reveals a significant and systematic negative effect of the SP in the creation of formal employment for small and medium firms. In particular, the registration of employees with IMSS in small and medium firms (1-250) by the end of the third year after the implementation of the program, fell by 3.8%, 4.2%,

3.3% and 3.7% for firms of 1, 2-5, 6-50 and 51-250 employees respectively (although the latter is estimated with low precision and it is not significant). Again, we do not observe any pretreatment trends in any of our results. Note that as we increase the firm size, the number of observations is reduced as not all municipalities have large firms within their boundaries.

For larger firms (over 250), most of the point estimates are not significant, indicating no effect of SP. However, they suggest there is an increasing pre-trend in 12 quarters before the implementation of the program. This suggests that large firms in municipalities that implemented the SP program first were growing faster than those late implementers. It also suggests that it is the behavior of larger firms that dominates the effects for the overall sample.

All patterns shown in Table 4 are visually confirmed by a series of figures in which we run equation (2) without aggregating the quarters in years. Results are shown in different panels in Figure 7. In this case, all coefficients are expressed as the percent difference with respect to 1 quarter before the implementation of the program. Different panels show the results for the number of owners and employment by firm size. In line with Table 4, the figures for employers and employees in firms of less than 250 workers show a causal effect of SP on employment. The remarkable feature of these figures is the flat trend up to three years before the implementation of the program and then the steady but continuous decline in the creation of formal jobs after the implementation of the program. We view this as strong evidence for the validity of our identification strategy. Any possible confounding factor would have to very closely mimic the timing of the implementation of the SP across municipalities in order to generate similar time profiles.

Due to the characteristics of our data, firms may increase or reduce their size endogenously due to the effect of the SP policy. This may be problematic because it may induce some of the employment shifts we observe within firm sizes. For instance, if firms between 2-5 employees were losing employment due to the effect of the SP program in a particular quarter, some of those firms would be shifted to the 1 employee category; this would over estimate the effect of the SP for this particular firm size group. Similarly, slightly greater firms (6-50 employees) losing employment could be

shifted into the 2-5 employee categories making us under estimate the effect of the SP. In order to minimize this effect, we include results following larger categorization of firms. In particular, we use two alternative aggregations. We group the employment of firms from 1 up to 50 employees and from 1 up to 250 employees. We present the results of this aggregation in figure 8 and throughout the rest of our results. Our estimates do not change. Within the third year formal employment registration falls around 4%.

In all, our results confirm that the SP had a negative effect on employment registration between 3.5% and 4% for both employers and employees in small and medium firms. The results are compatible with the idea that less visible firms (smaller firms) which can more easily avoid monitoring from the government will be more likely to reallocate labor from formal to informal contracts.

#### *Results by Age and Gender*

We now explore how our results change according to the gender or age of the worker. Table 5 shows the results for the effect of SP on employment by gender and age groups according to firm size. For presentational purposes we only report the coefficient capturing the effects of the program after three years. Panel A includes the results for all workers and age groups by firm size. Panels B and C show the same results for males and females respectively.

Two facts merit attention from this exercise. First, results show that even by age groups or by gender, the negative effect of SP in employment is restricted to small and medium firms. Second, the effect is stronger for younger workers especially for females, but especially so for younger women in small firms in which employment registration falls by up to 5%. This is consistent with the fact that those workers who are less attached to the labor market would be more prone to shifts towards informal employment. In particular, young women in their reproductive years tend to move more in and out of the labor force and possibly have lower attachment with IMSS benefits system.

#### *Results by Municipality Characteristics*

We explore now how our results change across rural/urban areas and with the size of the municipality. We divide our sample of municipalities in three equal groups according to municipality size in the 2000 Census. Table 6 columns 1 and 2 show the results for rural/urban status, while columns 3 to 5 show the results by municipality size. We present the results for employers (Panel A) and employees in firms with less than 50 employees (panel B) and in firms with less than 250 employees (panel C), although this last results could be misleading since small/rural municipalities may not have firms bigger than 50 employees. The effects of the SP appear stronger in rural and small municipalities. In particular the number of registered employers fell by 5.5% in rural municipalities compared to 3.3% in urban municipalities. Similar results are found for employment in firms with less than 50 employees. However, the estimates are not as precise as when using the full sample.

#### *Robustness checks*

We run a number of robustness checks to our main specifications. Table 7 Panel A reports the main effects found in Table 4, while Panels B to H report estimates for different specifications.

Panel B shows the results in which the sample is restricted to the 1055 post pilot municipalities instead of the 1395 panel municipalities. SP take up rates grew slowly in the pilot period of implementation (2002-2004). Panel B shows stronger effects and weaker pre-tend effects. The result is consistent with the fact that expenditures in health and take up rates increased only after SP was passed into law as a modification of the General Health law.

Panel C shows the unweighted results (without population weights). The negative effects of the SP are substantially higher. For example, number of employers decreased 4.6% after 3 years of the program, while the main results show a decline of 3.8%. This is due to the effects of SP which are more notable in smaller municipalities, hence in a specification with no weights, smaller municipalities get heavier weights.

Panel D includes results with a more saturated control specification at the state level. The main results include state cubic trends. That specification assumes that state cubic trends are enough to control for possible unobserved components correlated with the implementation of SP. A more flexible control function is to include state X time fixed effects. In this sense, the effect of SP is identified from the variation within states. The coefficients in this specification are very similar to the main results.

Panel E shows the results with a slightly different definition of the treatment. In the main results, we define treatment if the municipality has more than ten beneficiaries. In the pilot period of SP, we observe that some municipalities had less than ten beneficiaries in one quarter but then in the following quarter the municipality reported zero beneficiaries. To check the robustness of the assumption, we use the original treatment variable as we observe it from the administrative records. However, we assume that from the quarter the municipality is treated, the municipality is always treated, even if the registered number of beneficiaries returns to 0. Panel E shows the results remain unaffected by this change.

Panel F includes many more controls than the main regression. In the main regression, we include observable characteristic trends using the 2000 Population Census. In particular, we employ age and gender profiles, industry shares at the two digit level (16 industries), median income, IMSS coverage, rural/urban status, poverty level and average years of education. In order to assure the causality of the estimate, we include a more flexible control function in observable characteristics. We allow a cubic polynomial in the log of population and include square trends of all the above variables plus interactions among them. The results are fairly similar to those found in the main specification.

Panel G restricts the sample to the period 2001-2008. Figure 3 shows that employment growth was low in the period 2000-2002, and even negative in the year 2009. If the crisis of the late 2008 and 2009 is affecting differently municipalities with SP that implemented earlier than those late adopters, it may affect the results. However, we find that results are similar to the main specification. Moreover, it may be considered that SP affects even more negatively employment.

Finally, in the spirit of Card (1992), we assume that employment in large firms is reasonably unaffected by the introduction of the program (as observed in results from column 6). In this case, we can use the employment of firms over 250 employees as an additional time varying control at the municipality level. The assumption is that all unobserved factors of employment at the municipality level and correlated with SP are captured by the employment of large firms. Panel H shows the effects of SP once controlling for employment in large firms.<sup>14</sup> Since not all municipalities have large firms and hence our remaining sample is reduced to the largest municipalities, we show the same regression with and without the inclusion of employment in large firms. In sum, Panel H shows the results for the main specification using the restricted sample with positive values in employment for large firms and results for the specification that controls for employment in large firms. In order to save space for presentation purposes, we do not include the pre-trend effects given that most of the effects are similar to those previously found. We can conclude two different things from these results. First, in this sample of municipalities the effect of the SP is substantially muted. Again, this comes as no surprise since we have established that the effect was lower in larger urban municipalities. Second, the effect of SP is unaffected by the inclusion of the control variable of employment in large firms.

### *Quantitative Effects of the SP*

We now turn to the quantitative question of how much formal employment would have been created had the SP not been implemented. We use estimations in Table 4 to calculate the counterfactual trends in formal employment creation. In particular we subtract from the actual series the effect of the SP obtained in Table 4 for each firm size category. Table 8 presents the results for the subsamples for which we can confidently establish a causal interpretation of our results.

The first column shows the actual changes of the log employment for each one of our subsamples. This shows the coefficient of a regression of the corresponding employment variable on a time trend. Similarly, we regress the counterfactual series on

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<sup>14</sup> The coefficient on large firms is only significant for the case of number of employers, total employment, and firms with less than 50 employees.

a time trend to obtain the coefficients in column (2) of Table 8. According to this estimation, for the period 2000 to 2009, employers' registration grew 0.24% per quarter. We estimate that employer's registration should have increased by 0.35% per quarter. Further, for firms below 250 employees, employment growth was 0.51% per quarter and we estimate that it should have been 0.6%. The last column in Table 4 shows the raw difference between the actual series and the counterfactual series aggregated at the country level. We estimate that around 31.000 more employers and 300.000 more employees should have been registered with IMSS. These are sizeable effects. To put these numbers in perspective, they correspond to the 3.8% and 2.4% of the stock of registered employers and employees in 2002 when the program started. As a final benchmark we compare with the actual increase in employer/employee registration for the period. During the 2000-2009 periods, 64.000 employers registered with IMSS, implying that employer registration, based on our estimates, should have increased by 95.000, representing an additional 48%. Similarly, employee registration increased by 1.7 million workers. Our estimates suggest that it should have been increased by 2 million workers, an additional 17.6%.

## **VI Discussion and Concluding remarks**

This paper analyzes the effect of the SP in formal employment trends in Mexico. We find that the introduction of the program significantly shifted the trends in formal employment creation by 2009. According to our estimates, between 2000 and 2009, an additional 31.000 employers and 300.000 employees should have been registered with IMSS.

Although previous studies have failed to find significant effects of the SP in the labor market, we do not see them in direct conflict with the results presented in the paper. Our results show that the strongest effects of the SP were found in small firms and in small municipalities and these are underrepresented in labor market surveys. Our estimates only show modest effects in large urban labor markets. Our extended view of the Mexican labor market is probably better suited to find these effects.

However, even taking our estimates at face value, we do not claim that the SP was an unwise economic policy. Several remarks are in order to interpret our results.

The SP has provided access to health care to millions of Mexicans. In this sense, the program seems to have a clear social- welfare-improving effect since now more workers (and their families) have access to health coverage. The evidence collected so far suggest that this has dramatically reduced the catastrophic expenditure in health according to Gakidou et al (2006), although important impacts in health status are yet to be confirmed by the data.

What this paper shows is that the implementation of the SP also generated a non-trivial reallocation of workers from formality into informality. Hence the possible gains of health coverage have to be weighted against the implications of this reallocation of labor. We note several possible welfare effects.

First, as Levy (2008) notes, formal benefits are bundled. The worker contributing to the IMSS has access not only to health insurance but also to pension and disability benefits, housing loans and severance payments among other benefits. Informal benefits occur through assistance programs like the SP and they do not constitute a comprehensive set of insurance arrangements. In a sense, this reallocation implies that fewer workers will have access to protection and social insurance. Hence, if the social planner is concerned about overall worker protection it reduces the overall social welfare.

Second, we have shown that the effects of the SP were stronger for newcomers into the labor market, especially young women. This has important implications in the long run. We have argued throughout the paper that formality has important benefits for the worker. However, firms and workers bear the cost of formality today while most of the benefits accrue in the future (that is especially the case for the pension and disability benefits). However, these are only available to the worker if there is a permanent attachment to formal employment (the pension system in Mexico requires 25 years of contributions). If new entrants do not get attached with formality, this could pose serious problems to productivity and development in the future.

In all, the general equilibrium effects of the policy will prove determinant to assess the convenience or not of such programs and should be thoroughly investigated.

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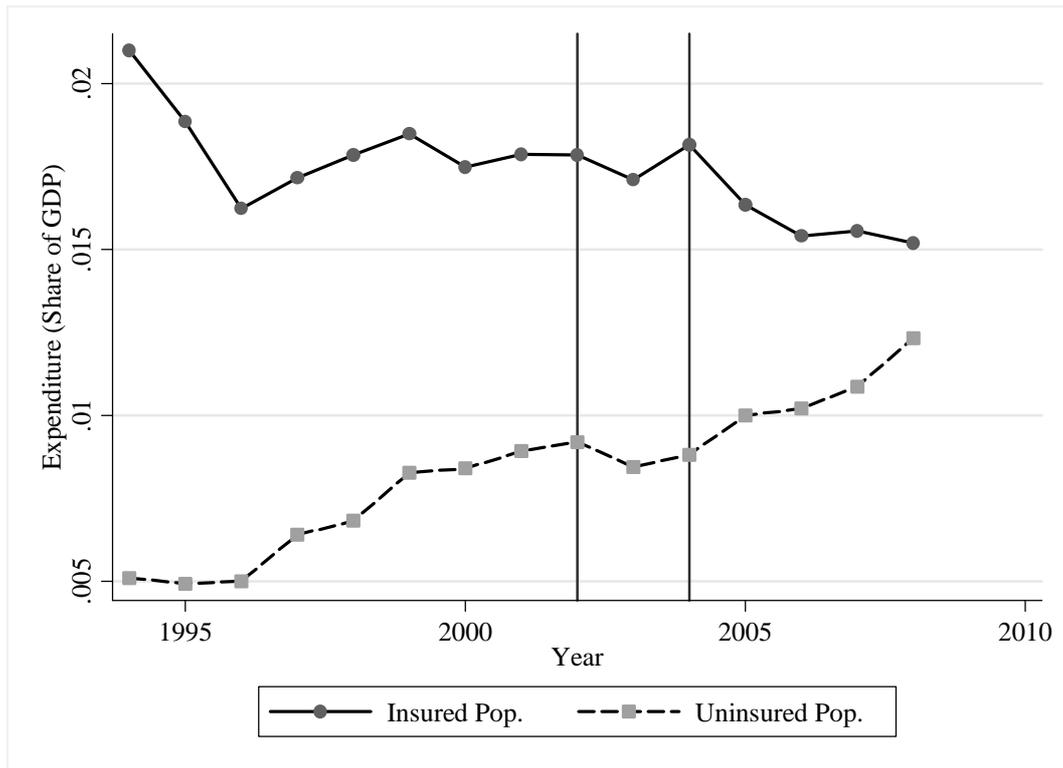
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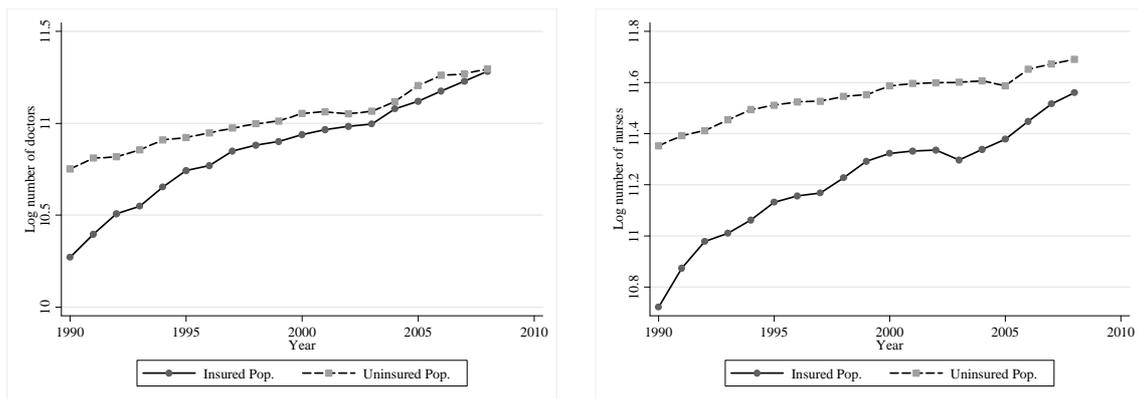
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Figure 1. Health Expenditures for the Insured and Uninsured Population: 1993-2008



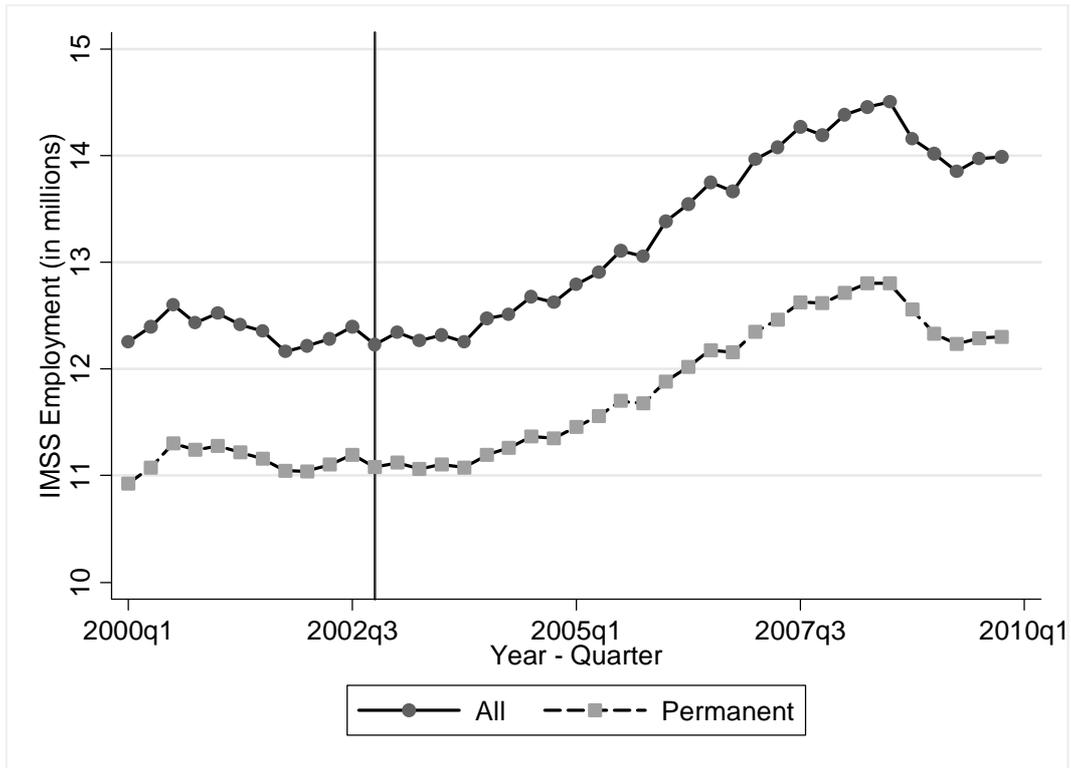
Notes: Health expenditures as a share of GDP for the insured population (IMSS, ISSSTE, etc) and uninsured population. Data obtained from Dirección General de Información en Salud (2010)

Figure 2: Doctors and Nurses for the Insured and Uninsured Population: 1990-2008  
 A. Medical Doctors  
 B. Nurses



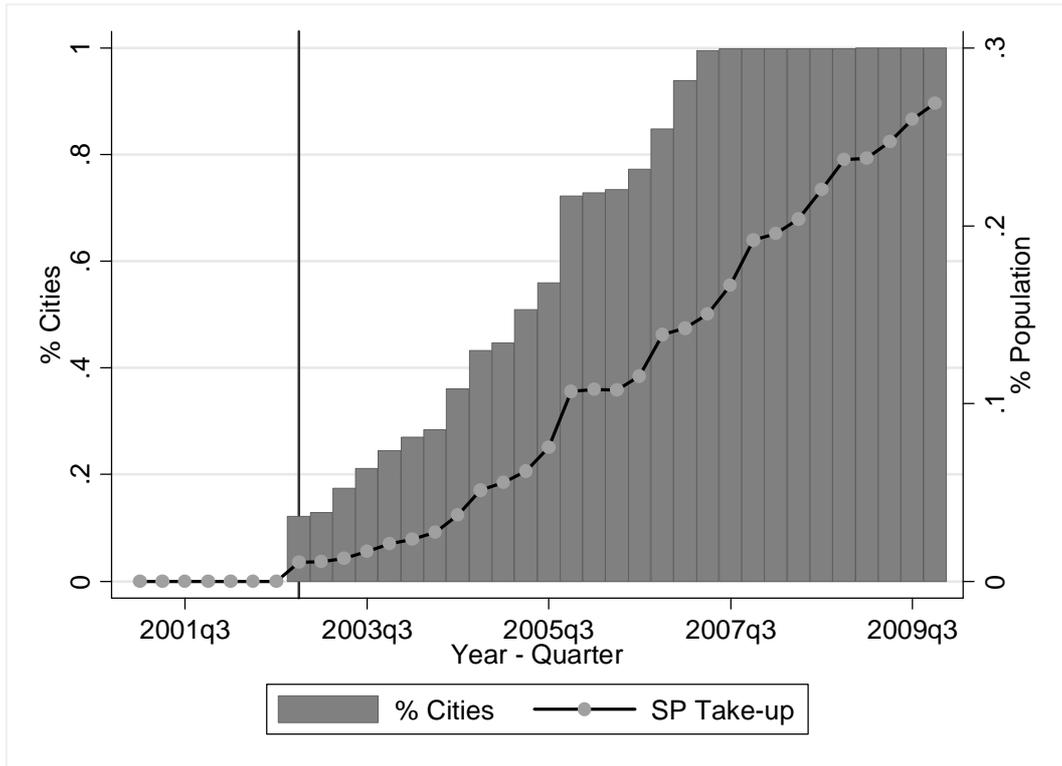
Notes: Registered medical doctors and nurses for the insured and uninsured population. Data obtained from Dirección General de Información en Salud (2010)

Figure 3: Number of workers affiliated to Mexican Social Security (IMSS)



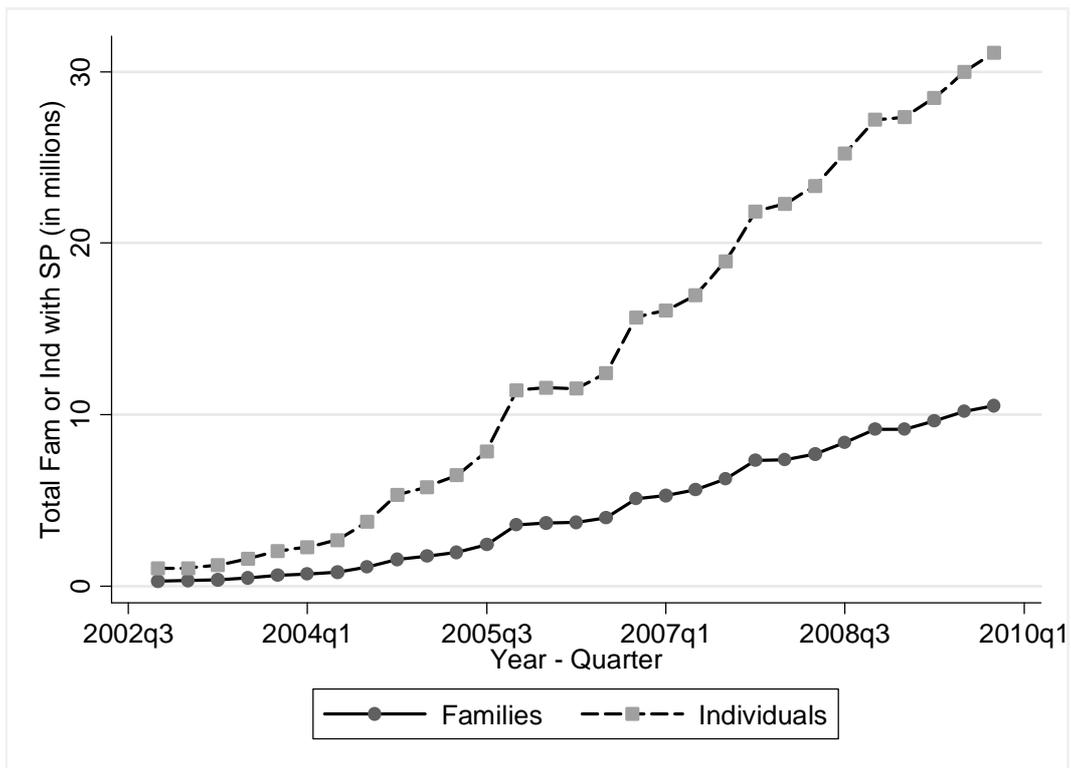
Notes: Number of workers affiliate to IMSS. It includes both permanent and temporary workers. The data is drawn from the official records of the IMSS.

Figure 4. Share of covered municipalities and population: 2000-2009



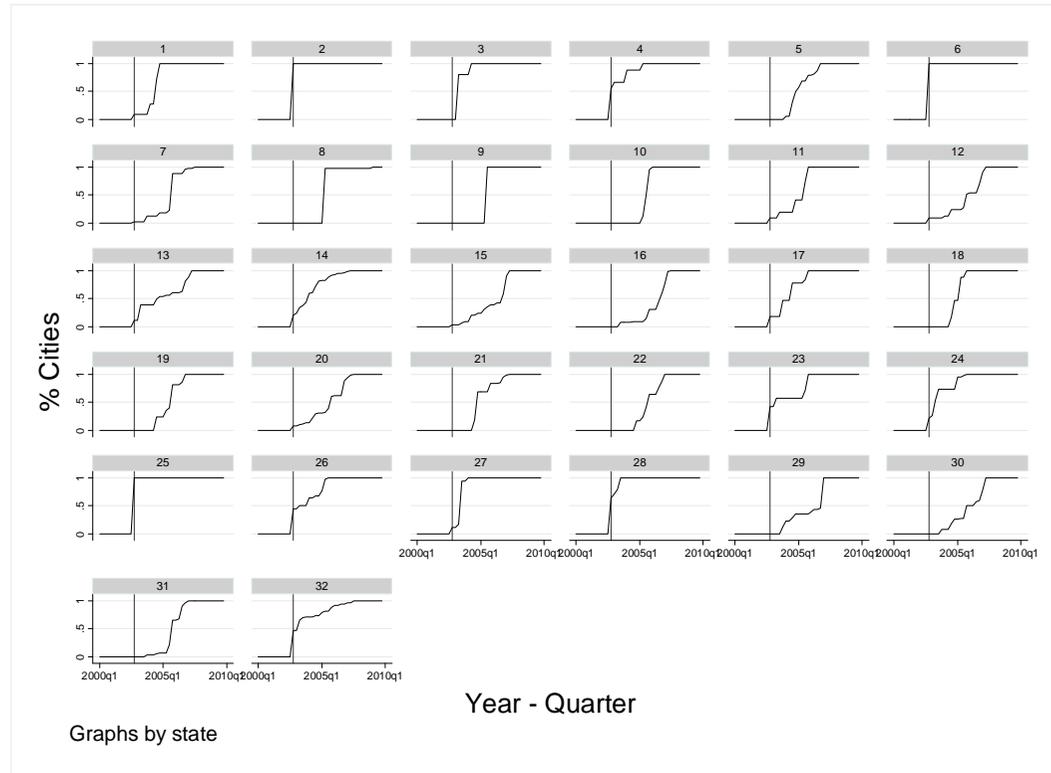
Notes: The figure shows the share of municipalities (cities) treated (left y axis) and the SP take-up rate (right y axis). Number of beneficiaries obtained from the administrative records of SP and population from the 2000 Population Census and 2005 Population Count.

Figure 5: Number individuals and households affiliated with SP: 2002-2009



Notes: The figure shows the number of individuals and households registered with the SP according to the administrative records of SP 2002-2009, including children and dependant.

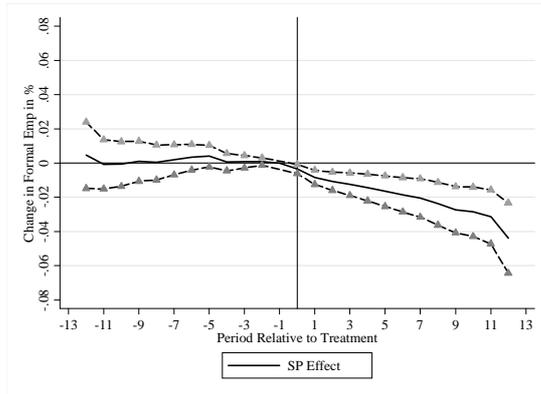
Figure 6: Share of Municipalities covered by the SP by State: 2001-2008



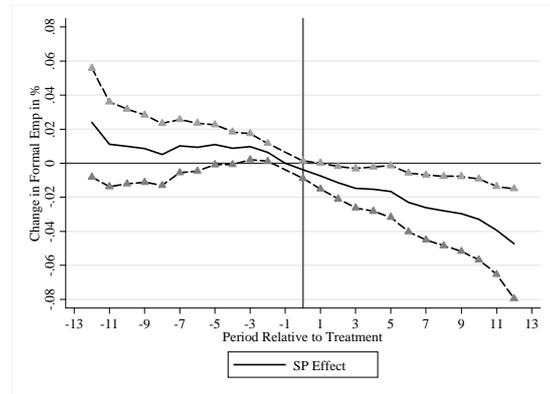
Notes: Share of municipalities covered by the SP. State codes are: 01 Aguascalientes, 02 Baja California, 03 Baja California Sur, 04 Campeche, 05 Coahuila, 06 Colima, 07 Chiapas, 8 Chihuahua, 09 Distrito Federal, 10 Durango, 11 Guanajuato, 12 Guerrero, 13 Hidalgo, 14 Jalisco, 15 México, 16 Michoacán, 17 Morelos, 18 Nayarit, 19 Nuevo León, 20 Oaxaca, 21 Puebla, 22 Querétaro, 23 Quintana Roo, 24 San Luis Potosí, 25 Sinaloa, 26 Sonora, 27 Tabasco, 28 Tamaulipas, 29 Tlaxcala, 30 Veracruz, 31 Yucatán, 32 Zacateca

Figure 7: Event Study: Number of owners and Employment by Firm Size

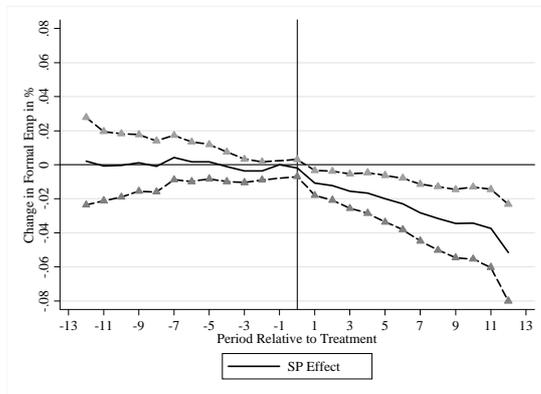
A. Number of Owners



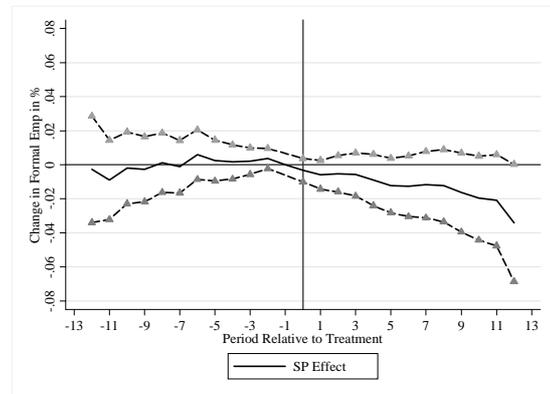
B. Firm size: 1 employee



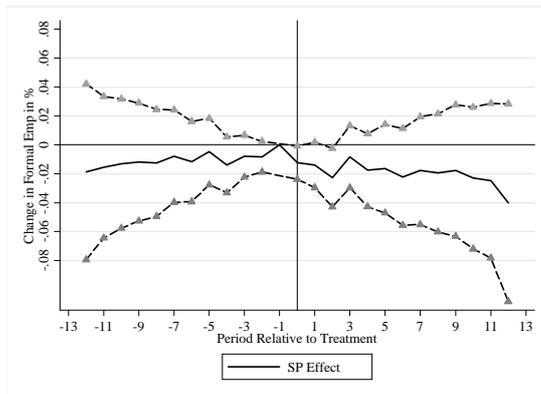
C. Firm size: 2-5 employees



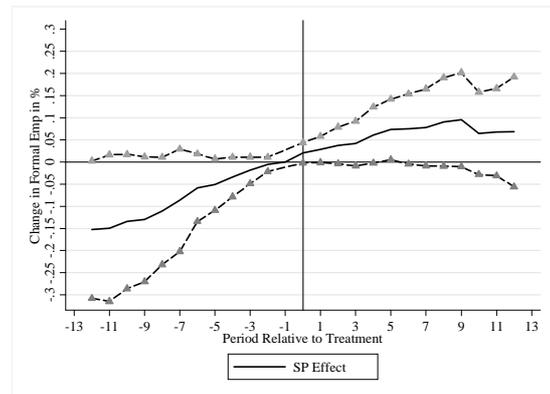
D. Firm size: 6-50 employees



E. Firm size: 51-250 employees

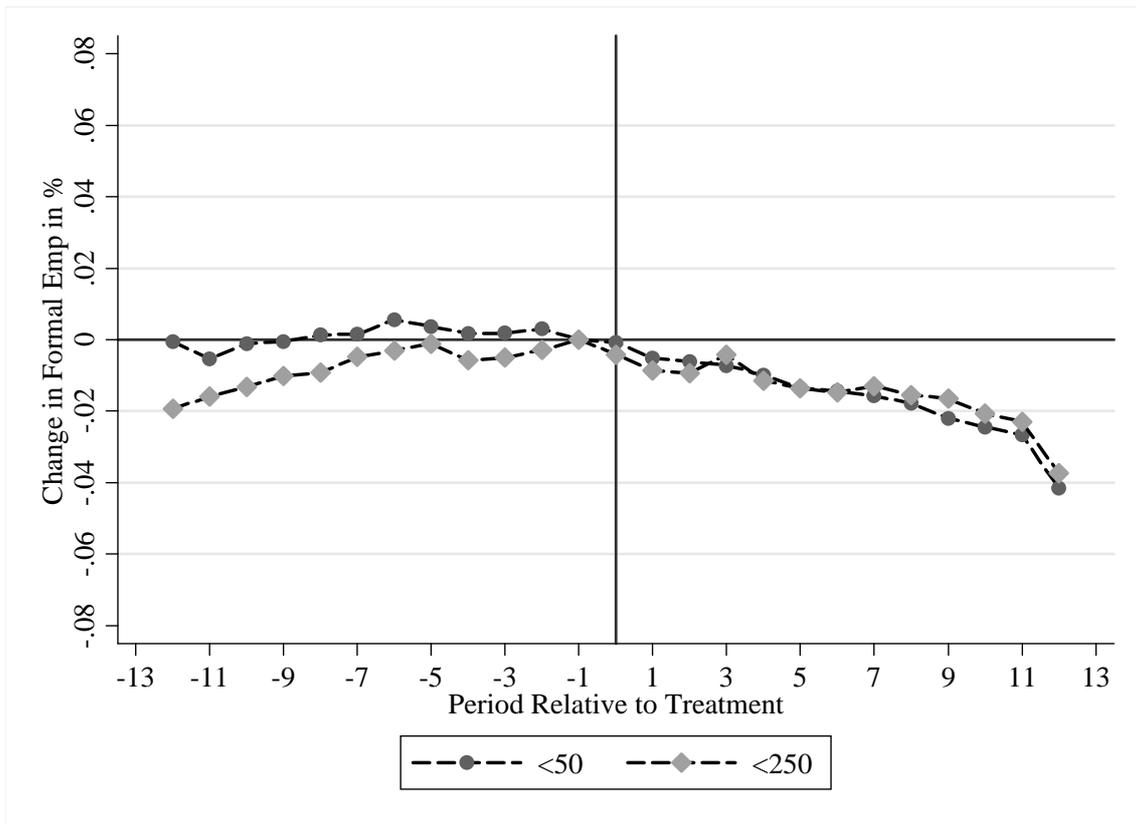


F. Firm size: 250+ employees



Notes: Different panels show different dependent variables. Panel A refers to (log) number of owners and rest of the figures refer to (log) total employment in each type of firm. Solid line represents the coefficients of dummy variables for each quarter before and after treatment in an event study analysis as in specification (2). All treatment periods before and after period -12 and 12 respectively are set to 1. Omitted category is 1 quarter before treatment, hence all coefficients are interpreted with respect to period -1. Regressions include state cubic trends, observable characteristics trends using information from the 2000 Population Census, municipality and period fixed effects. Robust and cluster standard errors at the municipality level. Dashed lines represent 95% confidence intervals.

Figure 8: Event Study: Number of owners and Employment by Firm Size



Notes Different lines represents the coefficients of dummy variables for each quarter before and after treatment in an event study analysis as in specification (2). We plot number the results for log (number of employers). All treatment periods before and after period -12 and 12 respectively are set to 1. Omitted category is 1 quarter before treatment, hence all coefficients are interpreted with respect to period -1. Regressions include state cubic trends, observable characteristics trends using information from the 2000 Population Census, municipality and period fixed effects. Robust and cluster standard errors at the municipality level. Dashed lines represent 95% confidence intervals.

**Table 1: The Mexican Labor Market in the 2000s survey and Registry data.**

Survey Data	ENE				ENOE			
	2000	2004	Diff		2005	2009	Diff	
Total	35.58	38.6	3.02	8%	35.6	39	3.4	10%
Employers	1.69	1.7	0.01	1%	1.89	2	0.11	6%
Self-Employed	9.49	10.6	1.11	12%	9.5	10.3	0.8	8%
Salaried workers	24.4	26.3	1.9	8%	24.2	26.8	2.6	11%
NO IMSS	13.3	14.7	1.4	11%	13.8	15.4	1.6	12%
IMSS	11.1	11.6	0.5	5%	10.4	11.4	1	10%
<b>Registry data</b>								
Employers	0.76	0.8	0.04	6%	0.8	0.82	0.02	3%
Salaried workers								
IMSS	12.4	12.6	0.2	2%	12.8	14	1.2	9%

Notes: ENE refers to Encuesta Nacional de Empleo and ENOE refers to Encuesta Nacional de Ocupacion y Empleo, both Labor Force Surveys. Employers refers to Owners of firms, Registry data refers to IMSS data. Columns are in millions of workers.

**Table 2: Determinants of Municipality affiliation with the SP Program**

	Panel (1)	Post-Pilot (2)
Log Population	-1.227*** [0.2908]	-0.474** [0.2268]
Log State Population	2.394** [1.0112]	0.853 [0.6437]
Share of Insured population	2.046 [2.7754]	0.623 [2.2844]
Urban	0.424 [0.4144]	0.437 [0.3581]
Log Median Wage	-0.278* [0.1460]	-0.103 [0.1370]
Years of Schooling	-0.477 [0.4818]	-0.208 [0.3298]
Unemployment Rate	3.514 [14.8027]	10.365 [17.0067]
PRD	3.422 [2.2721]	4.118*** [0.7976]
PRI	-0.436 [1.8575]	0.34 [1.1034]
Poverty Index (Food)	0.037 [0.0536]	-0.003 [0.0208]
Poverty Index (Income)	0.025 [0.0497]	0.024 [0.0230]
Share of aged<24	-11.792 [7.0324]	-5.652 [5.1003]
Share of aged>24&<40	10.383 [8.4473]	11.156* [5.6687]
Share of Males	2.438 [5.8443]	1.56 [5.3980]
Industry shares	YES	YES
Observations	1395	1055
R2	0.2449	0.2072

Notes: Each column shows a regression where the dependent variable is an index indicating the quarter and year of the start of the SP in a municipality. Explanatory variables are drawn from the 2000 Population Census. The regressions also include 16 industry variables shares by municipality which we do not report because they are not significant. Political party affiliation obtained from <http://www.cidac.org/>. Robust standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 3: The effects of the SP on Health resources**

	Doctors		Nurses		Clinics	
	All	Panel Sample	All	Panel Sample	All	Panel Sample
3 yr prior	-0.005 [0.011]	0.022* [0.012]	-0.022 [0.014]	0.020 [0.020]	-0.010 [0.008]	0.010 [0.006]
2 yr prior	-0.001 [0.006]	0.007 [0.006]	-0.015* [0.009]	0.006 [0.012]	0.001 [0.004]	0.003 [0.003]
Implementation	0.020*** [0.007]	0.014** [0.007]	0.022** [0.009]	0.016* [0.010]	0.002 [0.004]	0.008** [0.004]
1 yr after	0.055*** [0.012]	0.039*** [0.014]	0.071*** [0.015]	0.056*** [0.019]	0.004 [0.008]	0.008 [0.006]
2 yr after	0.076*** [0.016]	0.037** [0.018]	0.107*** [0.021]	0.068*** [0.023]	0.002 [0.012]	0.007 [0.008]
3 yr after	0.098*** [0.022]	0.053** [0.024]	0.135*** [0.030]	0.096*** [0.035]	-0.004 [0.017]	0.013 [0.011]
Observations	21951	12555	21951	12555	21951	12555

Notes: The table shows estimates of equation (1). Columns “All” use all municipalities in Mexico, Columns “Panel Sample” use the working sample of 1395 municipalities. The dependent variable is in terms of 1,000 habitants in municipality  $m$  at time  $t$ . All regressions control for municipality and period fixed effects. The six rows show different lags of implementation of SP. In particular, each variable takes a value of 1 if the municipality was enrolled in the SP 3 or less years prior, 2 years prior, year of implementation, 1 year after, 2 years after, and 3 or more years after. The estimations are all population weighted and the standard errors are clustered at the municipality level. \* Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

**Table 4: The effects of the SP on Number of Owners, Total Employment and Employment by Firm Size**

	Employers (1)	All (2)	1 (3)	2-5 (4)	6-50 (5)	51-250 (6)	>250 (7)
3 yr prior	-0.0027 [0.0053]	-0.0308*** [0.0115]	0.0022 [0.0083]	-0.0001 [0.0070]	-0.0061 [0.0081]	-0.0041 [0.0168]	-0.107* [0.0579]
2 yr prior	0.0002 [0.0023]	-0.0099 [0.0059]	-0.0011 [0.0042]	0.0023 [0.0037]	-2.98E-05 [0.0043]	0.0003 [0.0089]	-0.0517 [0.0330]
Implementation	-0.0080*** [0.0025]	0.0055 [0.0049]	-0.0113*** [0.0043]	-0.0065* [0.0036]	-0.0062 [0.0044]	-0.0084 [0.0081]	0.0377 [0.0233]
1 yr after	-0.0160*** [0.0046]	0.0065 [0.0093]	-0.0187** [0.0074]	-0.0167*** [0.0064]	-0.0120 [0.0077]	-0.0139 [0.0141]	0.0693* [0.0395]
2 yr after	-0.0249*** [0.0066]	0.0018 [0.0131]	-0.0278*** [0.0103]	-0.0277*** [0.0092]	-0.0172 [0.0108]	-0.0176 [0.0207]	0.0722 [0.0488]
3 yr after	-0.0379*** [0.0096]	-0.0182 [0.0194]	-0.0377** [0.0150]	-0.0427*** [0.0129]	-0.0330** [0.0163]	-0.0372 [0.0305]	0.0553 [0.0589]
Observations	55800	55800	54142	54386	53064	36337	20636

Notes: The table shows estimates of equation (2). The dependent variable is always the quarterly log of formal employment/owners in municipality  $m$  at time  $t$ . Column 1 refers to the log of total owners, Column 2 refers to the log of total employment, Columns 3-7 refer to the log of employment by firm size. All regressions control for the level of population by municipality and trends of observable characteristics using the 2000 Population Census (Poverty, Population shares of gender and age, industry shares, uninsured share, median wage). All regressions also include municipality and period fixed effects plus state cubic trends. The six rows show different lags of implementation of SP. In particular, each variable takes a value of 1 if the municipality was enrolled in the SP 3 or less years prior, 2 years prior, year of implementation, 1 year after, 2 years after, and 3 or more years after. The estimations are all population weighted and the standard errors are clustered at the municipality level. \* Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

**Table 5: The effects of the SP by Gender, Age and Firm Size**

	Firm Size					
	1 (1)	2-5 (2)	6-50 (3)	51-250 (4)	>250 (5)	<250 (6)
<b>A. Full Sample</b>						
All	-0.038** [0.015]	-0.043*** [0.013]	-0.033** [0.0163]	-0.037 [0.031]	0.055 [0.059]	-0.038** [0.017]
15-29	-0.032 [0.023]	-0.049*** [0.017]	-0.032 [0.021]	-0.049 [0.037]	0.053 [0.076]	-0.046** [0.021]
30-49	-0.041** [0.018]	-0.039*** [0.015]	-0.032** [0.016]	-0.036 [0.031]	0.0620 [0.055]	-0.036** [0.016]
>=50	-0.036 [0.024]	-0.035** [0.018]	-0.039** [0.018]	0.043 [0.039]	0.096* [0.058]	-0.028* [0.017]
<b>B. Males</b>						
All	-0.051*** [0.017]	-0.044*** [0.014]	-0.039** [0.018]	-0.0073 [0.031]	0.094 [0.075]	-0.029* [0.017]
15-29	-0.069** [0.030]	-0.035* [0.019]	-0.036 [0.023]	-0.018 [0.037]	0.098 [0.091]	-0.033 [0.022]
30-49	-0.042** [0.021]	-0.042*** [0.016]	-0.037** [0.017]	-0.012 [0.033]	0.104 [0.071]	-0.032* [0.017]
>=50	-0.042 [0.026]	-0.039** [0.018]	-0.042** [0.020]	0.049 [0.041]	0.133* [0.069]	-0.028 [0.018]
<b>C. Females</b>						
All	-0.020 [0.019]	-0.039** [0.016]	-0.021 [0.022]	-0.104** [0.045]	0.001 [0.069]	-0.043** [0.019]
15-29	0.004 [0.028]	-0.064*** [0.022]	-0.005 [0.025]	-0.117** [0.052]	0.019 [0.085]	-0.049** [0.024]
30-49	-0.059** [0.026]	-0.031* [0.018]	-0.011 [0.020]	-0.080* [0.043]	0.007 [0.061]	-0.035** [0.017]
>=50	-0.008 [0.037]	-0.016 [0.027]	-0.009 [0.024]	-0.001 [0.045]	-0.033 [0.066]	0.002 [0.021]

Notes: The table shows estimates of equation (2) by Gender, Age and Firm Size. Each entry is a separate regression, columns show estimates by firms size and rows show estimates by gender and age groups. All regressions control for the level of population by municipality and trends of observable characteristics using the 2000 Population Census (Poverty, Population shares of gender and age, industry shares, uninsured share, median wage). All regressions also include municipality and period fixed effects plus state cubic trends. Each entry reports the coefficient of the lag of implementation of SP with respect to 3 or more years after. The estimations are all population weighted and the standard errors are clustered at the municipality level. \* Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

**Table 6: The effects of the SP by Municipality Size**

	Rural (1)	Urban (2)	Small (3)	Medium (4)	Large (5)
<b>A. Employers</b>	-0.055** [0.027]	-0.031*** [0.010]	-0.042 [0.038]	-0.046 [0.030]	-0.032*** [0.011]
<b>B. Employees (&lt;50)</b>	-0.068 [0.042]	-0.033*** [0.012]	-0.054 [0.063]	-0.083* [0.044]	-0.026** [0.013]
<b>C. Employees (&lt;250)</b>	-0.014 [0.050]	-0.050*** [0.018]	-0.000 [0.091]	-0.043 [0.061]	-0.032* [0.018]

Notes: The table shows estimates of equation (2) by municipality characteristics (Rural/Urban) and municipality size. Each entry is a separate regression, columns show estimates by firms size and rows show estimates by employer/employee groups. All regressions control for the level of population by municipality and trends of observable characteristics using the 2000 Population Census (Poverty, Population shares of gender and age, industry shares, uninsured share, median wage). All regressions also include municipality and period fixed effects plus state cubic trends. Each entry reports the coefficient of the lag of implementation of SP with respect to 3 or more years after. The estimations are all population weighted and the standard errors are clustered at the municipality level. \* Significant at 10%;

\*\* significant at 5%; \*\*\* significant at 1%.

**Table 7: Robustness Checks**

	Employers (1)	All (2)	2-5 (3)	<50 (4)	<250 (5)	>250 (6)
<b>A. Main</b>						
3 yr prior	-0.003 [0.005]	-0.031*** [0.011]	0.000 [0.007]	-0.005 [0.007]	-0.009 [0.009]	-0.107* [0.058]
3 yr after	-0.038*** [0.010]	-0.018 [0.019]	-0.043*** [0.013]	-0.039*** [0.013]	-0.038** [0.017]	0.055 [0.059]
<b>B. Post-Pilot</b>						
3 yr prior	0.006 [0.006]	-0.020* [0.011]	0.009 [0.009]	-0.006 [0.008]	-0.004 [0.011]	-0.017 [0.028]
3 yr after	-0.054*** [0.018]	-0.054 [0.037]	-0.064*** [0.023]	-0.052** [0.022]	-0.058* [0.030]	-0.042 [0.085]
<b>C. Unweighted results</b>						
3 yr prior	-0.011 [0.008]	-0.022 [0.018]	-0.002 [0.013]	-0.024* [0.014]	-0.017 [0.020]	-0.043 [0.028]
3 yr after	-0.046*** [0.018]	-0.058 [0.039]	-0.077*** [0.026]	-0.055* [0.028]	-0.039 [0.038]	-0.082 [0.055]
<b>D. State X Time Fixed Effects</b>						
3 yr prior	0.002 [0.006]	-0.033** [0.013]	0.001 [0.008]	-0.004 [0.008]	-0.012 [0.010]	-0.117 [0.071]
3 yr after	-0.042*** [0.011]	-0.022 [0.023]	-0.050*** [0.015]	-0.043*** [0.015]	-0.040** [0.020]	0.048 [0.076]
<b>E. Original Treatment</b>						
3 yr prior	-0.006 [0.005]	-0.035*** [0.011]	-0.006 [0.007]	-0.008 [0.007]	-0.013 [0.009]	-0.116** [0.057]
3 yr after	-0.039*** [0.009]	-0.003 [0.019]	-0.041*** [0.013]	-0.042*** [0.012]	-0.039** [0.016]	0.090 [0.056]
<b>F. More X's</b>						
3 yr prior	-0.002 [0.005]	-0.027** [0.011]	0.002 [0.007]	-0.002 [0.006]	-0.008 [0.008]	-0.113** [0.051]
3 yr after	-0.043*** [0.014]	-0.032*** [0.009]	-0.036*** [0.013]	-0.042*** [0.013]	-0.036** [0.016]	0.068* [0.041]
<b>G. Period 2001-2008</b>						
3 yr prior	0.002 [0.005]	-0.021** [0.010]	0.006 [0.007]	-0.004 [0.007]	-0.003 [0.009]	-0.076** [0.036]
3 yr after	-0.040*** [0.010]	-0.035* [0.020]	-0.051*** [0.014]	-0.043*** [0.014]	-0.050*** [0.017]	0.017 [0.047]
<b>H. Using firms &gt;250 as controls<sup>a</sup></b>						
3 yr after	-0.023** [0.010]	-0.001 [0.017]	-0.029** [0.013]	-0.019* [0.011]	-0.020 [0.015]	-
Main						
3 yr after	-0.024** [0.010]	-0.018 [0.014]	-0.029** [0.013]	-0.020* [0.011]	-0.019 [0.015]	-
w/ control						-

Notes: The table shows estimates of equation (2) for numbers of owners and employment by firm size. All regressions control for the level of population by municipality and trends of observable characteristics using the 2000 Population Census (Poverty, Population shares of gender and age, industry shares, uninsured share, median wage). All regressions also include municipality and period fixed effects plus state cubic trends (Panel D includes state X period fixed effects instead). Entries report the coefficients of both the lead and lag of implementation of SP with respect to 3 or more years before and after respectively (unless specified otherwise). The estimations are all population weighted (not Panel C) and the standard errors are clustered at the municipality level. \* Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. <sup>a</sup> first row is the main specification for municipalities with positive employment in large firms, third row includes control variable.

**Table 8: Quantitative Effects of the SP**

	Actual	Counterfactual	Employers/Jobs
<b>Number of Employers</b>	<b>0.0024***</b> (0.0005)	<b>0.0035***</b> (0.0003)	<b>30798</b>
<b>Size of the Firm</b>			
1	0.0031*** (0.0005)	0.0043*** (0.0003)	9010
2-5	0.0013** (0.0006)	0.0025*** (0.0007)	41162
6-50	0.0046*** (0.0006)	0.0053*** (0.0004)	107816
<50	<b>0.0036***</b> <b>(0.0006)</b>	<b>0.0046***</b> <b>(0.0003)</b>	<b>172925</b>
51-250	0.0079*** (0.0006)	0.0089*** (0.0009)	125069
<250	<b>0.0051***</b> <b>(0.0005)</b>	<b>0.0060***</b> <b>(0.0004)</b>	<b>297101</b>

Notes: The table shows actual and counterfactual trends of registration of employers and employees to IMSS for different firm sizes. It also shows the actual number of employers and employees that would have registered in the absence of the SP. The actual trends is estimated in a regression of the respective variable on a time trend. The counterfactual series is obtained by subtracting to the actual series the effect of the SP estimated in equation (2). We then regress that counterfactual series on a time trends. The raw difference (in number of employers and employees) between the actual and the counterfactual series is plot in column 3. The standard errors are clustered at the municipality level. \* Significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. <sup>a</sup> first row is the main specification for municipalities with positive employment in large firms, third row includes control variable.