

The Trade-Offs of Welfare Policies in Labor Markets with Informal Jobs: The Case of the “Seguro Popular” Program in Mexico[†]

By MARIANO BOSCH AND RAYMUNDO M. CAMPOS-VAZQUEZ*

In 2002, the Mexican government began an effort to improve health access to the 50 million uninsured in Mexico, a program known as Seguro Popular (SP). The SP offered virtually free health insurance to informal workers, altering the incentives to operate in the formal economy. We find that the SP program had a negative effect on the number of employers and employees formally registered in small and medium firms (up to 50 employees). Our results suggest that the positive gains of expanding health coverage should be weighed against the implications of the reallocation of labor away from the formal sector. (JEL E26, I13, I18, I38, J46, O15, O17)

Around 60 percent of global workers work in unregulated jobs with no access to basic benefits such as health insurance, workers compensation, death and disability insurance, or retirement pensions.¹ They are normally called the *informal* workers in opposition to the workers covered by *formal* social security programs. However, in the last two decades national governments around the globe have pushed policies to give access to some of the traditional benefits of contributory social security to informal workers at zero or virtually no cost. Today, some 30 middle-income countries are implementing programs which aim to advance the transition to universal health coverage, and many others are considering launching similar programs (Giedion, Alfonso, and Díaz 2013).² The overall impact of these policies has two distinct ramifications. On the one hand, they clearly increase welfare for the uninsured, as they ensure health coverage without financial hardship. On the other, they change the incentives in the labor market and may induce a reallocation of labor from formal jobs, where workers and firms are taxed to obtain health coverage,

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*Bosch: Inter-American Development Bank (IADB), 1300 New York Avenue, Washington, DC 20577 (e-mail: mbosch@iadb.org); Campos-Vazquez: Centro de Estudios Económicos, El Colegio de México (CEE-COLMEX), Camino al Ajusco 20, Col. Pedregal de Santa Teresa, DF, México 10740 (e-mail: rmcampos@colmex.mx). We are very grateful to Eva Arceo, Matias Busso, Gerardo Esquivel, Santiago Levy, Luis Felipe López-Calva, Carmen Pagés, Norbert Schady, Sivan Tamir, and two anonymous referees for very useful comments and suggestions, and we wish to thank seminar and conference participants at El Colegio de México, Inter-American Development Bank, University of California, Berkeley, and the LACEA conference in Chile (2011). All remaining errors are our own.

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¹Pallares-Miralles, Romero, and Whitehouse (2012).

²The expansion of noncontributory pensions in the world mirrors this trend. These pensions aim at providing income for the elderly that were either inactive or informal during their active years and do not qualify for a pension. In the last two decades 51 countries have implemented noncontributory pensions (HelpAge International 2013).

to informal jobs where access to health services is noncontributory or heavily subsidized. In this paper we estimate the extent of this **reallocation effect for one of the most ambitious of these programs—the Seguro Popular**—which aimed at providing free health access to half the population of Mexico (50 million uninsured).

A large body of research in developed economies has analyzed how welfare policies shape the decisions which households and individuals make in the labor market (e.g., Moffitt 2002). Traditionally, this literature has focused on how welfare programs impact labor supply.³ Indeed, simple economic reasoning suggests that subsidies to the poor tend to have negative labor supply effects via an **income effect** if, as typically assumed, leisure is a normal good. In addition, income means testing, which is typically used for the purpose of targeting, imposes an implicit tax on labor earnings. By reducing the price of leisure, this induces a substitution effect away from work, further reducing participation and leading to welfare losses.

In many countries the divide between formal and informal workers adds a crucial dimension to understanding the full impact of welfare policies. In addition to a potential labor supply effect, providing free health access to the informal might create incentives to reallocate away from formal activities as individuals and firms might prefer to hold unofficial, unregistered jobs in order to escape taxation and access noncontributory social assistance (Levy 2008, 2009). This **might** be particularly so when, as in the Mexican case, the targeting mechanism of welfare policies is precisely to not contribute to social security, imposing a 100 percent marginal tax rate on formal earnings.

This reallocation of firms and workers has at least three important implications for welfare. First, it triggers a **loss of taxation revenue for the state in social security contributions and other taxes such as value-added tax (VAT) and corporate tax**. Second, **reallocation toward small-scale informal activities might impact overall long-run productivity growth** as informal firms will be deterred from growing, hampering innovation and productivity growth. And finally, **it might reduce welfare for workers who reallocate in the long run because**, although insured against health shocks, they may become **uninsured against other risks**, such as old-age poverty, disability, or unemployment.

In 2002, the Mexican government embarked on an effort to improve health access to the **50 million uninsured in Mexico**, a program known as Seguro Popular (SP), or *popular health insurance*. By 2010, with virtually universal health coverage achieved, **there were more people covered by the noncontributory SP than by the Mexican Institute of Social Security (IMSS)**, the formal social security. We use administrative data for **1,392 municipalities out of 2,439 municipalities in Mexico** to study the change in the creation of formal jobs during the SP implementation period, between 2000 and 2011. We **exploit the variation generated by the time-staggered entry of municipalities into the program**. The program started as a pilot during 2002 in five states and by the end of 2007 virtually all municipalities in the country had enrolled in the program.

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³See, for instance, Fortin, Lacroix, and Drolet (2004); Lemieux and Milligan (2008); Currie and Madrian (1999); Cutler (2002); and Gruber (2000).

We find evidence that the SP significantly changed the trend in the affiliation to social security of employers and employees in small and medium firms (up to 50 employees), which represent 97 percent of formal firms and 44 percent of formal employment. Nonetheless, we do not find any significant effects for total employment. During 2000–2011, the number of private employers and employees in small and medium firms (up to 50 employees) registered with Mexican social security increased from around 0.75 million to slightly under 0.79 million (5.4 percent) and from 4.05 million to 4.68 million (15.7 percent), respectively. We estimate that had the program not been in place, around 36,000 additional employers and 171,000 additional employees would have formally registered with Mexican social security in these small and medium firms. These represent 4.6 percent and 4 percent of the stock of registered employers and employees, respectively, in firms of less than 50 employees in 2002 when the program began.

This reallocation had measurable costs. On the one hand it generated a loss of revenue to the Mexican social security of at least 0.62 percent of their annual revenue, equivalent to 2 percent of the budget of the SP and 0.01 percent of gross domestic product (GDP). Additionally, the loss of 36,000 registered small and medium firms could have generated a loss of VAT of between 0.08 and 0.32 percent of GDP and a drop between 0.03 and 0.09 percent of GDP. These reallocation losses seem to be in the same order of magnitude as some of the gains associated with expansion of the SP. The available estimates (Aguilera, Miranda, and Velázquez 2012) suggest that the savings in catastrophic health expenditures for the uninsured Mexican households as a result of having SP coverage amounted to between 0.07 and 0.14 percent of GDP.

We are not the first to examine the impact of the SP in the labor market. Early studies such as Barros (2008) and Campos-Vazquez and Knox (2013) find no significant impact of the program in formal employment trends. Azuara and Marinescu (2011) find that the SP increased the share of informality among the unskilled by around 0.9 percentage points (although they do not obtain significant effects using the entire sample). Aterido, Hallward-Driemeier, and Pagés (2011) and Perez-Estrada (2011) use the same data to find that the SP increased the share of informal workers by 0.4 to 1 percentage point depending on the estimation.

While all the above articles rely on household survey data, we exploit actual social security data.⁴ This has several advantages and provides new insights on the effects of welfare policies on the labor market. First, we offer more precise and reliable estimates of the impact of the SP. Household survey data is not representative at the municipality level (the geographical disaggregation at which the SP treatment occurs). This implies that formal employment is measured with substantial error, biasing the estimates downward. This is consistent with the fact that almost all papers in the literature report negative impacts of the SP but those impacts are imprecisely estimated.

Second, we are able to estimate the impact on employer/firm registration. This is important because it establishes that the effect of the SP occurs partially through a

⁴Perhaps the only exception is the work of Aguilera (2011) who employs registry data but for a small set of municipalities finding no impact of the SP on formal employment.

reduction in the number of small firms which are formally registered. This allows us to offer tentative estimates on the potential indirect taxation and output loss derived from the reallocation observed at the firm level.

Third, our data covers virtually the whole universe of municipalities with registered formal employment in the country while labor surveys use less than one-third of these municipalities. This is relevant since, as we show below, the effects of the SP are concentrated in small and medium firms and in relatively small municipalities, which tend to be underrepresented in labor surveys or measured with substantial error.

Finally, our data extends to the last quarter of 2011 when the roll-out of the program was completed and all municipalities had been in the program at least four years. This allows us to explore the lag structure of the effects of 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.

The rest of the paper is organized as follows. Section I provides a way of thinking about the divide between formal and informal workers and the potential effects of welfare policies. Section II describes the social security system in Mexico and the reform initiated in 2000. Section III presents the data and provides an overview of the state of the Mexican labor market in the 2000s. Section IV reviews the empirical strategy and studies in detail how the SP was implemented. Section V shows the main results of the paper. Section VI provides our conclusions.

I. Welfare Policies and Informality

Almost a century after the passing of the first social security laws, a large proportion of the labor force in the world is informal. On average, the share of informal over total workers is 12 percent in high-income countries, 35 percent in Europe and Central Asia, 64 percent in East Asia, 65 percent in Latin America, 68 percent in the Middle East, and 91 percent in Africa. In fact, some of these countries seem to be in a stable equilibrium. In Latin America, where long time series of the shares of formal employment are available, countries like Argentina, Mexico, and Brazil have shown very little change in the proportion of formal workers over the last three decades (Rofman and Oliveri 2011).⁵

Theoretically, there are two extreme ways to think about the existence of informal or unregistered jobs in an economy. Traditionally, in early segmentation models proposed by Fields (1975) and Mazumdar (1976), and in the spirit of Harris and Todaro (1970), informal workers are just segmented workers waiting for good, desirable, and scarce formal jobs and whom cannot afford to be unemployed.⁶ In these models, workers desire to be formal but they are unable to find formal jobs. On the other end of the spectrum, in the spirit of two-sector sorting models, Maloney (1999, 2004), Perry et al. (2007), and Levy (2008, 2009) argue that informal workers (especially micro-entrepreneurs and self-employed) self-select optimally into

⁵ Argentina moved from 50.36 percent in 1980 to 51.14 percent in 2010, Brazil from 57.3 percent in 1981 to 55.89 percent in 2009, Mexico from 41.1 percent in 1989 to 37.36 percent in 2010.

⁶ See Fields (2009) for an excellent survey on dual labor markets in developing countries.

informal activities because of better pay, more flexibility, or labor conditions in an environment of low enforcement of labor regulations. In these models, there is a marginal worker or firm which is indifferent between formality and informality.

If, like segmented models suggest, there is no mass of workers or firms in the threshold between formality and informality, such programs will improve welfare for the informal but do little to alter the incentives to operate formally. If, on the other hand, workers and firms are self-selecting into informal activities, the change in relative benefits is likely to impact the cost-benefit analysis of participation in the formal labor market among the marginal worker or firm.

The consensus in the literature (e.g., Fields 2009) is that there is a combination of segmented and self-selected individuals among informal workers. Hence, in the face of change in the relative benefits of informality, we should expect some reallocation since some marginal workers and firms might find it optimal to change sector. However, which of the two actors is predominant, and the extent of this reallocation, is still a matter of debate. Recently, a number of models have analyzed how a variety of policies impact the equilibrium in the share of formal employment in the economy (see, Albrecht, Navarro, and Vroman 2009; and Bosch and Esteban-Pretel 2012). This literature emphasizes two factors that determine the extent of the reallocation of workers and firms across sectors. First, the mass of workers and firms that are at the margin between formality and informality. And second, how incentives change for firms and workers to operate formally, which in turn depends on the generosity of the program and the marginal tax rate that the eligibility criterion imposes on formal earnings.

This reallocation from formal to informal activities has important implications for welfare. Three have been highlighted in the literature (see, Levy and Schady 2013). First, it reduces the tax base and threatens the fiscal balance of the government's social security institutions. The loss of tax revenue might come from different sources—from loss of social security contributions (due to loss of formal workers) to the loss of VAT and corporate tax (due to the loss of registered firms). Second, as Levy (2008) argues, the shift toward informal activities might generate proliferation of low productivity micro-firms, self-employment, distortions in the allocation of labor, lower investment, and ultimately lower growth. For instance, Busso, Fazio, and Levy (2012) estimate that, controlling for size and sector of activity at the six-digit level, Mexican informal firms are substantially less productive than formal ones—the productivity of one peso of capital and labor is between 60 and 88 percent higher in the formal than in the informal sector. Furthermore, Fajnzylber, Maloney, and Montes-Rojas (2011), studying Brazilian firms, find that increases in formality translate into higher productivity as a result of increased access to formal credit by firms, and higher levels of participation in societal institutions like public training efforts and trade associations.

Finally, if employers and employees trade their formal status in order to obtain health services through the SP, they would forgo other important benefits bundled with social security, such as access to a pension scheme. This can be important if workers and firms do not fully optimize when balancing the benefits between formal and informal jobs. The behavioral economics literature suggests that this could be the case, particularly in the case of pensions where default saving mechanisms (as

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those in formal jobs) play a crucial role in generating long-term savings (Choi et al. 2004 and Madrian and Shea 2001). The concern is that reallocated workers in informal jobs without default saving mechanisms will under-save for their old age.

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

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 Mexican Institute of Social Security (Instituto Mexicano de Seguro Social or IMSS) and later (created in 1959) the Institute for Workers' Security and Social Services (Instituto de Seguridad y Servicios Sociales de los Trabajadores del Estado or ISSSTE) for registered private and public employees, respectively⁷. In parallel to IMSS and ISSSTE, the Secretariat for Health and Services (Secretaria de Salud y Asistencia or 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 medical services and medicines. However, due to the overwhelming demand for affordable health care for poor families, a number of programs were designed to provide further access to health care to low-income populations (OECD 2005).⁸ Nonetheless, 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.⁹

In the early 2000s the Federal Government designed the System for Social Protection in Health that 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. This reform implied a 100 percent marginal tax rate on formal earnings. Contribution in either IMSS or ISSSTE automatically disqualified individuals to the SP.

The idea of providing health care to the uninsured was not new. The SSA, and its counterparts at state levels, had been providing assistance programs for over 60 years and substantial portions of public health expenditure were geared toward the uninsured (by 2000, 32 percent of total public health expenditure in Mexico 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. Second, there was a substantial increase in the health budget dedicated to improvement of the service provided by the SSA. Although the budget for SSA had been steadily increasing in the 1990s, the SP gave it an additional boost, increasing

⁷These 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, together employees and their employers pay payroll taxes amounting to roughly 25 percent of salaries excluding other local and federal taxes.

⁸Among others, IMSS Oportunidades and Programa de Seguro de la Familia.

⁹The World Health Organization ranked Mexico 144th out of 191 countries in fairness of health care (WHO 2000) and the Mexican Ministry of Health estimated that two to four 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).

from 0.8 percent of GDP in 2003 to 1.2 percent in 2008. In the same period, the IMSS expenditure declined from 1.7 to 1.5 percent of GDP. Finally, the SP program gave a new sense of entitlement to those uninsured families; the new General Health Law clearly stated 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 which explicitly lists the health interventions to which they are entitled and the health care facilities from which they can demand them (Frenk, Gómez-Dantés, and Knaul 2009).¹⁰ The reform had clear positive impacts. Aguilera, Miranda, and Velázquez (2012) estimated that by 2010, for each \$1 spent in the SP there was a saving of between US \$0.35 and \$0.17 in catastrophic health expenditure of uninsured households. Taken at face value, this estimation implies that the SP reduced catastrophic expenditure of between 0.07 and 0.14 percent of GDP.

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 (over 50 percent) is classified as informal. 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 by 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 self-employed and/or employers who operate outside the state institutions and obligations, and their employees are not covered by formal labor protection. In our case, the division between formal and informal labor is, nevertheless, very clear: formal workers are those contributing to IMSS and informal workers are not.

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.¹¹ Registration with IMSS entitles the worker to a number of benefits such as health insurance, pension contributions, and 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 2011.

¹⁰ One of the SP goals was to increase health care spending in Mexico by 1 percent of GDP (Knaul and Frenk 2005). The budget of SP has increased tenfold from 2004 to 2009 (the budget was close to US \$3.5 billion in 2009; this amount represents roughly one-half of the total budget to the Ministry of Health). The financing of the SP was co-paid between the Federal Government and the states. For participating states, the Federal Government pledged 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*. 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 US \$60 per family to close to US \$850 per family for rich households. However, according to SP records, 97 percent of families who come from the first two deciles of the income distribution do not pay at all (Secretaría de Salud 2010). 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.

¹¹ This only refers to private workers. A parallel institution for public workers is ISSSTE.

Most of the previous literature on SP has used the labor force surveys for the analysis. However, we argue that the labor force survey is problematic for several reasons. Perhaps the most important concern is that the **labor force survey** (Encuesta Nacional de Empleo or ENE, before 2005; Encuesta Nacional de Ocupación y Empleo or ENOE, after 2005) **is only representative at the state level and not at the municipality level**. This implies that employment at the municipality level is measured with substantial noise compared to administrative records. For instance, the correlation between the growth rate of formal employment captured by IMSS and that captured by the ENE-ENOE between 2000 and 2009 is 0.67 at the state level but only 0.15 at the municipality level. This is important since the effect of the SP is estimated using **within municipality variation**. In addition to this, the labor force survey changed at the end of 2004, from ENE to ENOE, raising some comparability issues.¹²

Unfortunately, we do not have access to the worker histories in the IMSS records. **Instead, we observe the total number of employers and employees affiliated with IMSS in every quarter from 2000 to 2011 as well as worker tabulations by gender, age, and firm size.**¹³ The main **measure of employment used by the IMSS is the total number of permanent workers and temporary urban workers.** 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 percent of affiliation to IMSS). The IMSS only keeps records of around 1,850 municipalities out of the existing 2,439 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 2011 and are left with 1,392 municipalities, which according to the IMSS records constitute 98 percent 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 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 ten.** This number was selected as there were some municipalities that show very low affiliation (zero or

¹² Although the main series (such as total employment and unemployment) are continuous, there is a clear jump in the series in the distribution of employment by firm size. In particular, the data shows a fall in 15 percentage points in the employment in firms of more than 250 employees (from 32 percent in 2004 to 17 percent in 2005); see online Appendix A for details.

¹³ 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 employer's registry if he/she owns firms with different activities. It is also common practice for larger firms to have two or three entries in IMSS for accounting purposes.

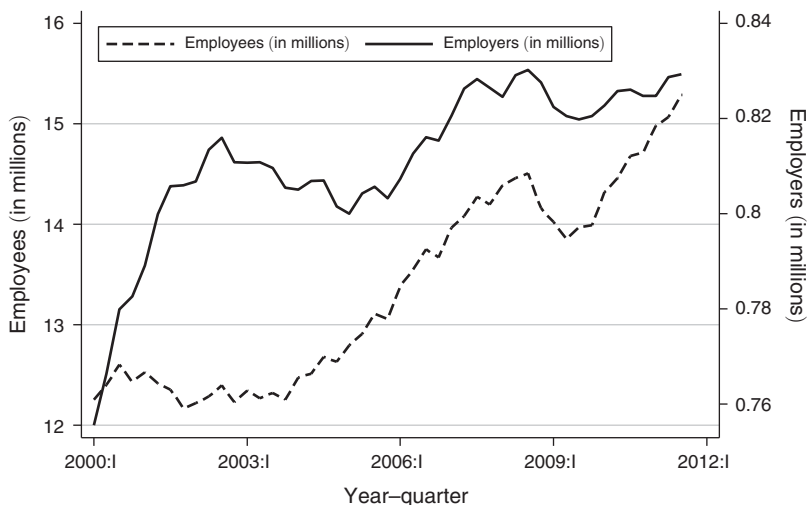


FIGURE 1. NUMBER OF EMPLOYERS AND EMPLOYEES AFFILIATED TO MEXICAN SOCIAL SECURITY (IMSS)

Notes: Number of employers and employees affiliated to IMSS. *All* includes both permanent and temporary workers. The data is drawn from the official records of the IMSS.

one affiliates for several quarters) especially at the start of the program, making it difficult to establish whether the program was operational at that municipality.¹⁴

We further merge this dataset with the 2000 Population census to obtain a series of pretreatment 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. Figure 1 shows the evolution of formal employment during the 2000–2011 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 percent and formal employment creation was almost negligible. Growth resumed in 2004 peaking in the first quarter of 2006 with an annual growth rate of 6.1 percent. 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 percent. 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.

IV. Identification Strategy

We use a difference-in-differences approach to estimate the effect of the SP in Mexico. We take advantage of the roll-out of the program during the period 2002–2007. Our main specification tests whether the affiliation of municipalities with the

¹⁴ We check the robustness of our results by using other definitions of program implementation in later stages of the paper with very similar results.

SP program had any impact on the level of formal employment using data from the first quarter of 2000 to the last quarter of 2011. In order to obtain a proper estimate of this effect we hypothesize that (conditional on some state and municipality employment trends) the evolution of formal employment would have been the same across municipalities in the absence of the SP.

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

$$(1) \quad E_{m,t} = \alpha + \delta P_{m,t} + \sum_{k=-k \min}^{k=k \max} \pi_{j,m,y} 1(\tau_{m,y} = j) \\ + \beta_{x,m} \mathbf{X}_m^{2000} t + \lambda_s t + \lambda_s t^2 + \lambda_s t^3 \\ + \lambda_m + \lambda_t + \varepsilon_{m,t},$$

where $E_{m,t}$ is the log total formal employment registration (employers or employees) in municipality m at time t , λ_m and λ_t are municipality and time fixed effects, $P_{m,t}$ is the log population at municipality m at the time t , and $k \max$ and $k \min$ correspond to the number of years before and after the program. Our identification strategy relies on the assumption that there are no underlying trends in the municipalities correlated with implementation of the SP. To detect the possible existence of these trends, 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 three year pre- and four year post-adoption indicator in batches of four quarters. In particular, we set $\tau_{m,y} = 0$ in the initial year of adoption (meaning the quarter of adoption and the next three quarters), $\tau_{m,y} = 1$ from the fifth to eighth quarter after adoption, $\tau_{m,y} = 2$ from ninth to twelfth quarters after adoption, and so on. We set $\tau_{m,y} = -3$ for all event years less than or equal to -3 and $\tau_{m,y} = 4$ for all event years greater than or equal to four. 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.

The 1,392 municipalities in the sample belong to 32 independent states. This is important because Mexico, like the United States, is a federation of states, each with a certain degree of autonomy, with a constitution, governor, and congress. State-specific policies or macroeconomic factors might induce a spurious correlation between the implementation of the SP in the municipalities which make up 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. In some of our specifications, we saturate the econometric model and include state \times 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 difference-in-differences models (Bertrand, Duflo, and Mullainathan 2004).

Furthermore, we allow the evolution of employment in the formal sector in municipality m to depend on demographic and employment composition, which are municipality-specific characteristics. Indeed, there are particular industries and age groups which 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. 2007). However, we lack quarterly data on such characteristics for the 1,392 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.

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 states¹⁵ 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.¹⁶

Figures 2 and 3 characterize the expansion of the SP. In Figure 2 we plot the municipal coverage of SP over time from the first quarter of 2001 up to the fourth quarter of 2009 when all the municipalities were enrolled in the program (the data is drawn from SP administrative records). Figure 3 shows the number of affiliates of the SP compared with those affiliated with IMSS and other health care providers in the country from 2000 to the first quarter of 2011. 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 percent

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¹⁵ Baja California, Coahuila, Chiapas, Guanajuato Guerrero, Hidalgo, México, Morelos, Oaxaca, Quintana Roo, San Luis Potosí, Sinaloa, Sonora, Tamaulipas, and Zacatecas.

¹⁶ We interviewed senior officials in charge of the SP. We asked repeatedly 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.

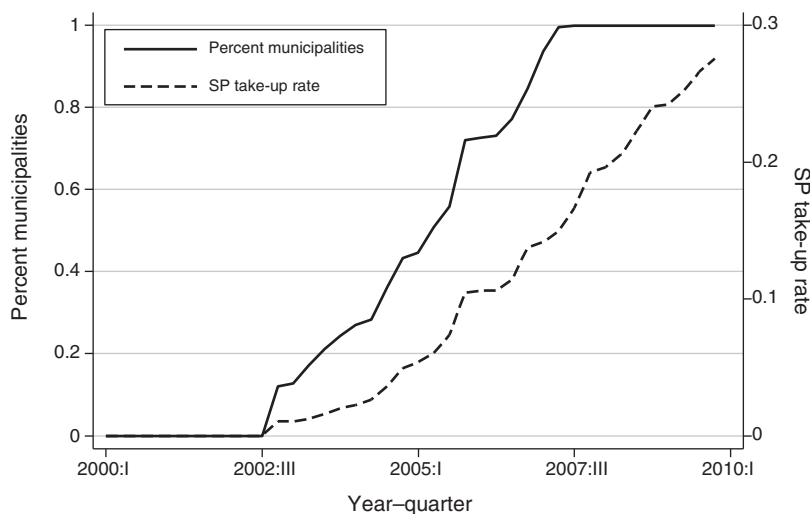


FIGURE 2. SHARE OF COVERED MUNICIPALITIES AND POPULATION: 2000–2009

Notes: The figure shows the share of municipalities treated (left y-axis) and the SP take-up rate as a percentage of total population (right y-axis). Number of beneficiaries obtained from the administrative records of SP and population from the 2000 Population census and 2005 population count.

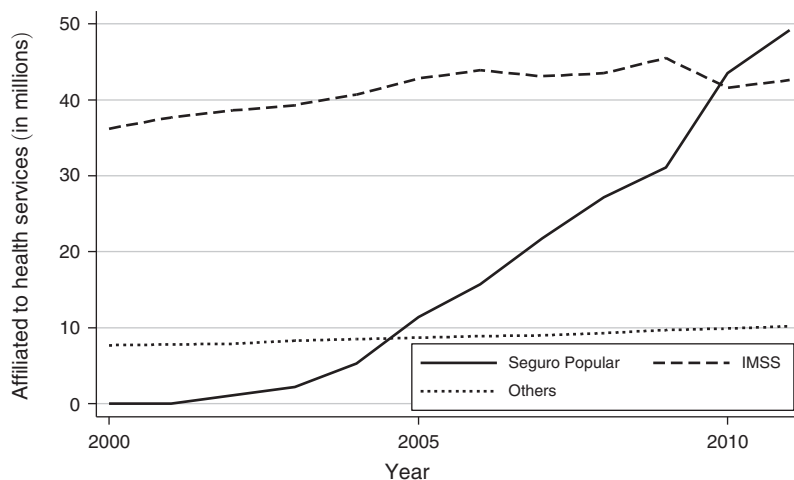


FIGURE 3. NUMBER OF AFFILIATED TO SEGURO POPULAR, IMSS, AND OTHER HEALTH PROVIDERS WITH SP: 2000–2011

Notes: The figure shows the number of individuals affiliated (workers and their families) with the different health services provided. The Seguro Popular, IMSS, and Others (which include public servants and semipublic firms such as Mexico Petroleum Company PEMEX). Figures correspond to the first quarter of every year.

Source: Secretaria de Salud (2011)

of the families in Mexico.¹⁷ 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 and 2010 covering close to 50 million individuals by 2011 replacing IMSS as the largest health care system in the country in terms of number of affiliates.

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 1 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 1, beginning in the third quarter of 2002.¹⁸ 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.¹⁹ 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, Estévez, and Magaloni (2006) who argue that political reasons were at play during the rollout of the SP to municipalities. In particular, they argue that smaller states were given preference to achieve full coverage of the SP in all their 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 1 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 explain 24 percent of the cross-sectional variation in implementation, where the population is the most significant variable. Hence, Table 1 provides no empirical evidence towards targeting of SP in specific municipalities. This is in line with Barros (2008) and Aterido, Hallward-Driemeier, and Pagés (2011) that find that no salient pre-SP labor market conditions are correlated with the implementation of the SP.

¹⁷ Population in the municipality over time is obtained from a simple interpolation using the 2000 Population census and 2005 population count.

¹⁸ 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 United States.

¹⁹ Political party affiliations obtained from CIDAC (2000–2011).

TABLE 1—DETERMINANTS OF MUNICIPALITY AFFILIATION WITH THE SP PROGRAM

	Panel (1)	Post-pilot (2)
log population	−1.320*** (0.281)	−0.513** (0.219)
log state population	2.339** (0.924)	0.959 (0.573)
Share of insured population	1.921 (2.793)	0.746 (2.293)
Urban	0.460 (0.425)	0.434 (0.368)
log median wage	−0.300** (0.143)	−0.117 (0.132)
Years of schooling	−0.497 (0.486)	−0.197 (0.323)
Unemployment rate	3.069 (15.037)	9.438 (17.086)
PRD	3.418 (2.250)	4.092*** (0.806)
PRI	−0.188 (1.752)	0.375 (1.063)
Poverty index (food)	0.040 (0.056)	−0.001 (0.021)
Poverty index (income)	0.029 (0.049)	0.026 (0.022)
Share of aged < 24	−11.754 (7.223)	−5.677 (5.108)
Share of aged > 24 and < 40	11.215 (8.526)	11.419* (5.662)
Share of males	1.451 (5.979)	1.625 (5.374)
Industry shares	YES	YES
Observations	1,392	1,052
R^2	0.244	0.213

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. Political party affiliation obtained from CIDAC (2000–2011). PRI and PRD refer to national parties. PRI stands for Institutional Revolutionary Party and PRD for Democratic Revolutionary Party. Omitted category refers to PAN, which stands for National Action Party. The regressions also include 16 industry variables shares by municipality which we do not report because they are not significant. Robust standard errors in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

V. Results

A. Main Results

Table 2 presents the main results of the paper for the panel municipalities using specification (2). Column 1 uses as a dependent variable the log of the number of all employers registered with IMSS. Columns 2 to 6 use the log of the number of employers registered with IMSS by firm size: single worker firms, small firms

TABLE 2—THE EFFECTS OF THE SP ON NUMBER OF REGISTERED EMPLOYERS BY FIRM SIZE

	All (1)	1 (2)	2–5 (3)	6–50 (4)	51–250 (5)	>250 (6)	<=50 (7)
Three years prior	–0.003 [0.006]	0.001 [0.009]	–0.000 [0.007]	0.000 [0.008]	–0.003 [0.015]	–0.025 [0.022]	–0.001 [0.006]
Two years prior	–0.000 [0.003]	–0.002 [0.004]	0.002 [0.004]	0.002 [0.004]	–0.003 [0.008]	–0.011 [0.012]	0.001 [0.003]
Implementation	–0.007*** [0.002]	–0.010** [0.004]	–0.006* [0.003]	–0.010*** [0.004]	–0.008 [0.010]	–0.008 [0.010]	–0.008*** [0.003]
One year after	–0.014*** [0.004]	–0.016** [0.007]	–0.015** [0.006]	–0.015** [0.006]	–0.013 [0.016]	–0.008 [0.018]	–0.015*** [0.005]
Two years after	–0.022*** [0.006]	–0.024** [0.010]	–0.025*** [0.008]	–0.019** [0.009]	–0.012 [0.022]	–0.018 [0.026]	–0.024*** [0.006]
Three years after	–0.029*** [0.008]	–0.029** [0.012]	–0.033*** [0.010]	–0.030*** [0.011]	–0.015 [0.030]	–0.030 [0.033]	–0.032*** [0.008]
Four years after	–0.044*** [0.010]	–0.038** [0.016]	–0.049*** [0.013]	–0.041*** [0.014]	–0.033 [0.037]	–0.046 [0.041]	–0.046*** [0.010]
Observations	65,424	63,586	63,778	62,386	42,839	24,402	65,360

Notes: The table shows estimates of equation (1). The dependent variable is always the quarterly log of registered employers in municipality m at time t . Column 1 refers to the log of total employers, columns 2–6 refer to the log of registered employers 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 leads and lags of implementation of SP. In particular, each variable takes a value of 1 if the municipality was enrolled in the SP three or less years prior, two years prior, year of implementation, one year after, two years after, three years after, and four or more years after. The estimations are all population weighted and the standard errors are clustered at the municipality level.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

(2–5 workers), medium firms (6–50 workers), large firms (51 to 250 workers), and very large firms (more than 250 workers). Finally, column 7 shows the results of aggregating all small and medium firms (up to 50 employees).

Column 1 of Table 2 suggests that within the first year after the implementation of the program, employer affiliation to IMSS falls by 0.7 percent and by the end of the fourth year the effect reaches 4.4 percent. Importantly, we find virtually zero coefficients for the years before the implementation of the program suggesting a causal interpretation of our results. Columns 2–6 show that the effects are concentrated among small and medium firms. We find significant negative effects of the SP on the registration of firms up to 50 employees, especially in firms between 2–5 employees, where the fall, four years after implementation, reaches 4.9 percent. We do not find significant effects of employer registration in large and very large firms in our sample.

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 one employee category; this would overestimate the effect of the SP for this

TABLE 3—THE EFFECTS OF THE SP ON NUMBER OF REGISTERED EMPLOYEES BY FIRM SIZE

	All (1)	1 (2)	2–5 (3)	6–50 (4)	51–250 (5)	>250 (6)	<=50 (7)
Three years prior	–0.036*** [0.011]	0.001 [0.009]	0.000 [0.008]	–0.005 [0.009]	–0.002 [0.017]	–0.097* [0.049]	–0.005 [0.007]
Two years prior	–0.012** [0.006]	–0.002 [0.004]	0.002 [0.004]	0.000 [0.004]	0.001 [0.009]	–0.047 [0.029]	0.001 [0.004]
Implementation	0.009* [0.005]	–0.010** [0.004]	–0.006* [0.004]	–0.006 [0.004]	–0.007 [0.009]	0.035* [0.020]	–0.005 [0.003]
One year after	0.013 [0.009]	–0.016** [0.007]	–0.016** [0.006]	–0.011 [0.007]	–0.010 [0.015]	0.064* [0.034]	–0.011** [0.006]
Two years after	0.013 [0.012]	–0.024** [0.010]	–0.026*** [0.009]	–0.015 [0.010]	–0.011 [0.023]	0.066 [0.042]	–0.018** [0.008]
Three years after	0.002 [0.015]	–0.029** [0.012]	–0.034*** [0.011]	–0.025** [0.012]	–0.014 [0.031]	0.052 [0.046]	–0.028*** [0.010]
Four years after	–0.013 [0.020]	–0.038** [0.016]	–0.051*** [0.014]	–0.033** [0.016]	–0.039 [0.040]	0.032 [0.054]	–0.040*** [0.013]
Observations	65,424	63,586	63,778	62,386	42,839	24,402	65,360

Notes: The table shows estimates of equation (1). The dependent variable is always the quarterly log of registered employers in municipality m at time t . Column 1 refers to the log of total employers, columns 2–6 refer to the log of registered employers 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 seven rows show different leads and lags of implementation of SP. In particular, each variable takes a value of 1 if the municipality was enrolled in the SP three or less years prior, two years prior, year of implementation, one year after, two years after, three years after, and four or more years after. The estimations are all population weighted and the standard errors are clustered at the municipality level.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

particular firm size group. Similarly, slightly greater firms (6–50 employees) losing employment could be shifted into the 2–5 employee categories making us underestimate the effect of the SP. In order to minimize this effect, we group the employers in firms from 1 up to 50. Our estimates (shown in column 7) do not change. Within the fourth year formal employment registration falls 4.6 percent.

These results indicate a clear negative effect in employer registration among small and medium firms. This is important since the SP was specifically targeted to provide coverage to a mass of informal self-employed and employers of unregistered micro-firms, and hence directly competing with IMSS and impacting employer registration.

Using the same structure, Table 3 studies the impact of the SP for employee registration with IMSS. In this case we do not detect an effect of the SP on total employment. In fact, our estimates for the years after the implementation of the program are preceded by an upward pretrend before the implementation of the program.

However, consistent with the estimates of Table 2, 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 by the end of the fourth year after the implementation of the program fell by 3.8, 5.1, 3.3, and 3.9 percent 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). If we aggregate for firms up to 50 employees (column 7) there is a fall of 4 percent after four years of implementation. In all these cases 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 pretrend in 12 quarters before the implementation of the program. This suggests that formal employment in large firms in municipalities which implemented the SP program first were growing faster than those late implementers. It also suggests that it is the behavior of larger firms which dominates the effects for the overall employee sample.

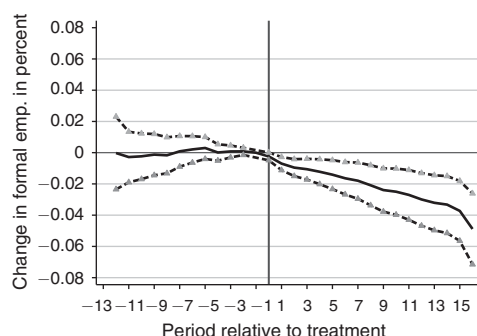
All patterns shown in Tables 2 and 3 are visually confirmed by a series of figures in which we run equation (1) without aggregating the quarters in years. Results are shown in different panels in Figures 4 and 5. In this case, all coefficients are expressed as the percent difference with respect to one quarter before the implementation of the program. Different panels show the results for the number of employers and employment by firm size. In line with Tables 2 and 3, the figures for employers and employees in firms of up to 50 workers show a causal effect of SP on registration of both employers and employees respectively. 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. The exception is the evolution of employment for very large firms which shows a clear pretrend three years before the implementation of the program. In a series of robustness checks below, we include as a control variable employment in large firms to measure the impact such trend may have on the estimated effect for small and medium firms.

In all, our results confirm that the SP had a negative effect on employment registration four years after the implementation of the program of around 4 percent 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.

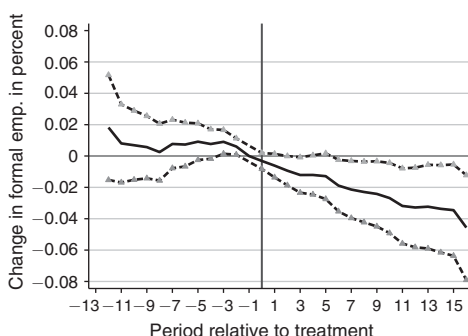
B. Results by Municipal Characteristics

We explore now how our results change across rural/urban areas and with the size of the municipality. We divide our sample of municipalities into four equal groups according to municipality size in the 2000 census. This corresponds to municipality sizes of less than 10,000, between 10,000 and 22,000, between 22,000 and 50,000, and more than 50,000. Columns 1 and 2 of Table 4 show the results for rural/urban status while columns 3–6 show the results by municipality size. We present the results for employers and employees of firms with up to 50 employees.

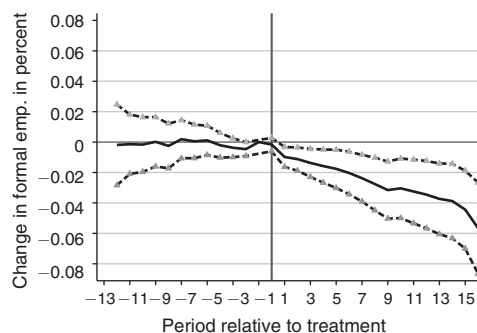
Panel A. Total number of employers



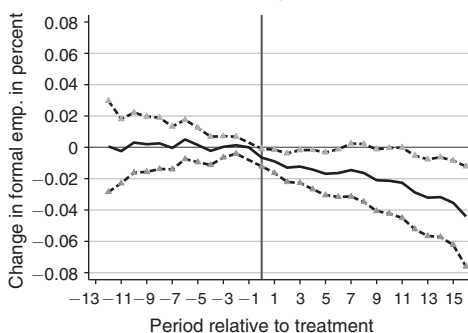
Panel B. Firm size: 1 employee



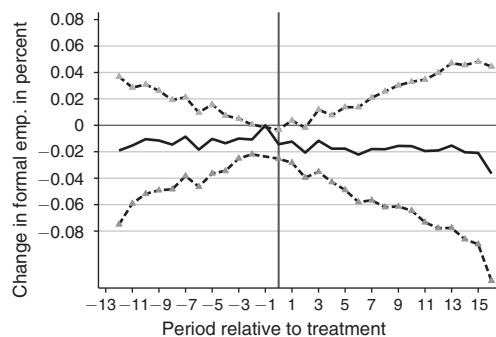
Panel C. Firm size: 25 employees



Panel D. Firm size: 6–50 employees



Panel E. Firm size: 51–250 employees



Panel F. Firm size: 250+ employees

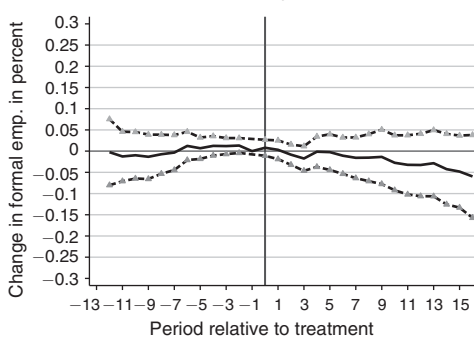


FIGURE 4. EVENT STUDY: NUMBER OF EMPLOYERS BY FIRM SIZE

Notes: Different panels show different dependent variables. Panel A refers to (log) number of employers and rest of the figures refer to (log) number of employers 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 16 , respectively, are set to 1. Omitted category is one 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 percent confidence intervals.

The effects of the SP appear stronger in rural and small municipalities. The point estimates suggest that the SP decreased the number of registered employers for all firm sizes below 50 employees by twice as much in rural municipalities compared

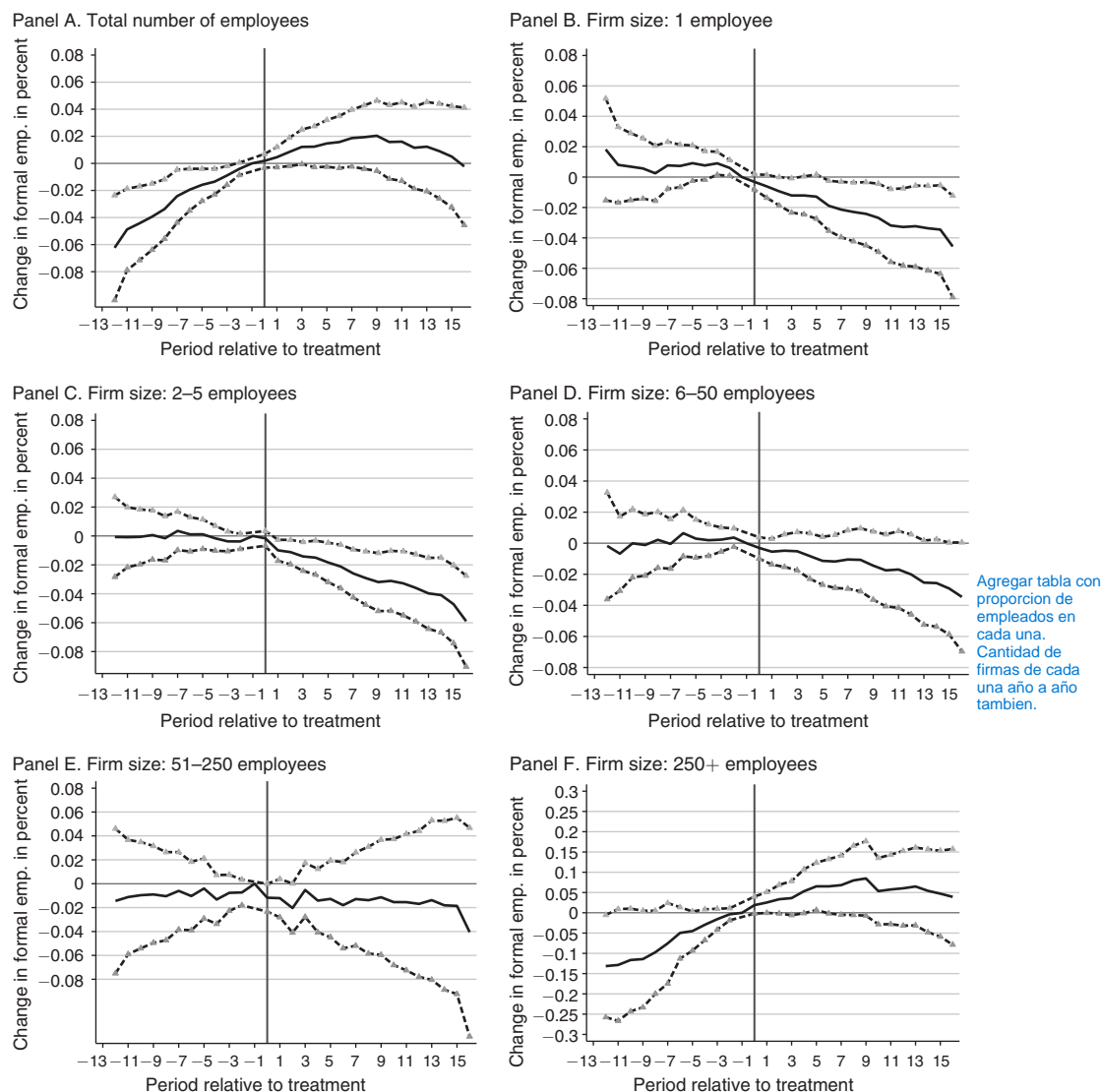


FIGURE 5. EVENT STUDY: NUMBER OF EMPLOYEES BY FIRM SIZE

Notes: Different panels show different dependent variables. Panel A refers to (log) number of employees and rest of the figures refer to (log) number of employees 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 16, respectively, are set to 1. Omitted category is one 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 percent confidence intervals.

to urban municipalities, although the estimates are far more precise in urban areas where most of the formal employment is concentrated.

Furthermore, columns 3 to 6 shows that larger municipalities were less affected. In fact, the effects are stronger in municipalities in the second and third quartile in

TABLE 4—THE EFFECTS OF THE SP BY MUNICIPALITY SIZE

	Rural (1)	Urban (2)	[1,000– 10,000] (3)	[10,000– 22,000] (4)	[22,000– 50,000] (5)	[>50,000] (6)
<i>Panel A. Employers (<=50)</i>						
Three years prior	−0.020 [0.017]	0.002 [0.007]	−0.022 [0.000]	0.020 [0.023]	−0.002 [0.018]	0.006 [0.006]
Four years after	−0.081** [0.032]	−0.036*** [0.010]	−0.071 [0.062]	−0.089** [0.045]	−0.062** [0.031]	−0.034*** [0.011]
<i>Panel B. Employees (<=50)</i>						
Three years prior	−0.026 [0.022]	0.002 [0.008]	−0.041 [0.000]	0.011 [0.026]	0.001 [0.026]	0.005 [0.007]
Four years after	−0.073* [0.043]	−0.036*** [0.012]	−0.120 [0.083]	−0.038 [0.061]	−0.091** [0.043]	−0.025** [0.013]

Notes: The table shows estimates of equation (1) by municipality characteristics (rural/urban) and municipality 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. Entries report the coefficients of both the lead and lag of implementation of SP with respect to four or more years before and after respectively. The estimations are all population weighted and the standard errors are clustered at the municipality level.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

the municipality size distribution. This suggests that the effect of the SP was more intense in smaller municipalities but with enough formal employment to be able to show significant trend changes. Similar results are found for employees in firms with less than 50 employees.

C. Robustness Checks

We run a number of robustness checks to our main specifications. Again, we focus on our results for registered employers and employees in firms of up to 50 employees. Panel A of Table 5 reproduces the main effects found in Tables 2 and 3 without any controls other than the state cubic trends. The results are similar albeit with slightly lower impact of the SP, 3.8 percent after four years for employers.

Panel B shows the results in which the sample is restricted to the 1,052 post-pilot municipalities instead of the 1,392 panel municipalities. SP take-up rates grew slowly in the pilot period of implementation (2002–2004). Panel B shows stronger effects and weaker pretrend 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 5.8 percent after four years of the program, while the main results show a decline of 4.6 percent. 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.

TABLE 5—ROBUSTNESS CHECKS

	Employers ≤50 (1)	Employees ≤50 (2)
<i>Panel A. No controls</i>		
Three years prior	−0.010 [0.008]	0.015* [0.006]
Four years after	−0.038*** [0.011]	−0.033** [0.014]
<i>Panel B. Post-pilot</i>		
Three years prior	0.005 [0.007]	−0.008 [0.009]
Four years after	−0.067*** [0.019]	−0.062*** [0.023]
<i>Panel C. Unweighted results</i>		
Three years prior	−0.016 [0.011]	−0.021 [0.015]
Four years after	−0.058*** [0.021]	−0.058* [0.030]
<i>Panel D. State × time</i>		
Three years prior	0.001 [0.007]	−0.007 [0.009]
Four years after	−0.054*** [0.012]	−0.048*** [0.015]
<i>Panel E. Original treatment</i>		
Three years prior	−0.007 [0.007]	−0.011 [0.008]
Four years after	−0.045*** [0.010]	−0.042*** [0.013]
<i>Panel F. ENE-ENOE municipalities</i>		
Three years prior	0.008 [0.007]	0.008 [0.008]
Four years after	−0.033*** [0.011]	−0.033** [0.014]
<i>Panel G. Very large firms as controls</i>		
Three years prior	0.001 [0.006]	−0.002 [0.006]
Four years after	−0.029*** [0.011]	−0.022* [0.012]
<i>w/control</i>		
Three years prior	0.002 [0.006]	−0.000 [0.007]
Four years after	−0.029*** [0.011]	−0.023* [0.012]

Notes: The table shows estimates of equation (1) for numbers of employers and employment for firms up to 50 employees. All regressions (except for panel A) 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 × period fixed effects instead). Entries report the coefficients of both the lead and lag of implementation of SP with respect to four 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 the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Panel D of Table 5 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 \times 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 of Table 5 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 zero. Panel E shows the results remain unaffected by this change.²⁰

For the sake of comparability with similar studies using the Mexican labor survey ENE-ENOE,²¹ panel F restricts the sample to those municipalities available in the ENE-ENOE. We find that the effect of the SP for these municipalities is around 30 percent smaller but still highly significant (3.3 as opposed to 4.6 for employers). This is expected since the smaller municipalities are underrepresented in the ENE-ENOE dataset.

Finally, in the spirit of Card (1992), we 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. Since not all municipalities have large firms, for this exercise our sample is reduced to only those municipalities with at least 1 firm over 250. Thus, 470 municipalities out of 1,392 remain. To study the impact of this additional control we first show the main regression without the employment of large firms as a control. This is shown in the first row of panel G in Table 5. In this sample of municipalities, the effect of the SP is muted (2.9 percent after four years), consistent with the fact that these are larger municipalities. We then include the employment of large firms as an additional control to find that the estimate on the effect of the SP for small and medium firms remains unaffected.

D. 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 Tables 2 and 3 to calculate the counterfactual trends in formal employment creation. Our estimates suggest that the SP had a causal effect in firms of up to 50 employees, for

²⁰ Although not shown in Table 5, we also included many more controls than in the main regression. We allow a cubic polynomial in the log of population and include square trends plus interactions among the original control variables. The results are fairly similar to those found in the main specification.

²¹ See Campos-Vazquez and Knox (2013); Azuara and Marinescu (2011); Aterido, Hallward-Driemeier, and Pagés (2011); and Perez-Estrada (2011).

TABLE 6—QUANTITATIVE EFFECTS OF THE SP

	Actual (1)	Counterfactual (2)	Employers/jobs lost by 2011 (3)
Number of employers (≤ 50)	0.0021*** [0.001]	0.0033*** [0.001]	36,000
Number of employees (≤ 50)	0.0036*** [0.001]	0.0046*** [0.001]	171,000

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 who would have registered in the absence of the SP. The actual trends are 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 (1). We then regress that counterfactual series on a time trend. The raw difference (in number of employers and employees) between the actual and the counterfactual series is plotted in column 3. The standard errors are clustered at the municipality level. First row is the main specification for municipalities with positive employment in large firms, third row includes control variable.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

both employer and employee registration. Accordingly, we use the estimations in Tables 2 and 3 to subtract from the original series the effect of the SP. Table 6 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 of employer and employee registration in small and medium firms. This shows the coefficient of a regression of the corresponding employment variable on a time trend. Similarly, we regress the counterfactual series on a time trend to obtain the coefficients in column 2 of Table 6. According to this estimation, for the period 2000 to 2011, employers' registration in firms of less than 50 employees grew 0.21 percent per quarter. We estimate that employer's registration should have increased by 0.33 percent per quarter. The last column in Table 3 shows the raw difference between the actual series and the counterfactual series in terms of number of jobs. We estimate that around 36,000 more employers and 171,000 more employees should have been registered with IMSS in firms of less than 50 employees.

To put these numbers in perspective, they correspond to the 4.6 and 4 percent of the stock of registered employers and employees in firms of less than 50 employees in 2002 when the program started. During the 2000–2011 period, the number of private employers and employees in small and medium firms registered with Mexican social security increased from around 0.75 million to slightly under 0.79 million (5.4 percent) and from 4.05 million to 4.68 million (15.7 percent), respectively. Our results suggest that in the absence of the SP, the increments should have been 89 and 26 percent larger, respectively.

How do our estimates compare to similar studies using the ENE-ENOE? Most studies report an increase in the share of informal employment (over total employment) between 0.4 and 1 percentage points. According to the 2010 Mexican census there were around 40 million workers. An increase in informal employment of around 0.4 to 1 percentage point (assuming there are no overall employment effects) corresponds to a loss of formal employment between 160,000 and 400,000 jobs.

TABLE 7—FIRM CREATION 1998–2010: COMPARISON IMSS, ECONOMIC CENSUS, AND POPULATION CENSUS

	Number of firms/employers (in millions)					Growth rates		
	1998	2000	2003	2008	2010	1998–2003	2003–2008	2000–2010
<i>Panel A. IMSS</i>								
1–5	0.50	0.54	0.57	0.57	0.56	14.03%	–0.70%	3.13%
1–50	0.68	0.75	0.78	0.79	0.79	13.62%	1.68%	5.23%
Total	0.71	0.78	0.81	0.83	0.83	13.46%	2.72%	6.15%
<i>Panel B. Economic census</i>								
1–5	2.55	—	2.70	3.33	—	5.88%	23.33%	—
1–50	2.78	—	2.97	3.7	—	6.83%	24.58%	—
Total	2.80	—	3.01	3.74	—	7.50%	24.25%	—
<i>Panel C. Population census</i>								
Self-employed	—	8.10	—	—	10.33	—	—	27.53%
Employers	—	0.89	—	—	1.23	—	—	38.20%
Total	—	8.99	—	—	11.56	—	—	28.59%

Notes: The table shows the actual number of firms/employers captured in the IMSS data, the Mexican Economic census, and the Mexican Population census for various years between 1998 and 2010. The IMSS data captures the number of registered employers. The economic census captures the number of firms (registered and unregistered) that have an establishment. The population census registers the self-reported labor status of individuals.

However, unlike those papers, our work highlights negative effects on employer registration in small and medium firms. The results resonate with known firm creation trends in Mexico during this period. We compare the IMSS data²² to two other sources of firm creation in Mexico, the Economic census and the Population census. The Mexican Economic census gathers data for all registered (formal) and unregistered (informal) firms in the country with a permanent establishment every five years (1998, 2003, and 2008). Further, the Population census gathers data on the self-reported labor status of all Mexicans every ten years (2000 and 2010). We focus on the trends of those who claim to be either self-employed or employers who potentially could be registered at IMSS as an employer. Table 7 captures the trends in the number of firms/employers during the 1998–2010 period in all three data sources. According to the Economic census, from 1998 to 2003 (before the implementation of the SP) the stock of formal and informal firms grew 7.5 percent (from 2.8 to 3.01 million). Virtually all the growth is due to the dynamics of firms up to 50 employees which amount to 99 percent of all firms in Mexico. During the same period the IMSS data suggest a growth rate of registered firms of 13.5 percent indicating that during the first part of the decade there was an increase in the share of registered firms (from 25.5 to 26.8 percent). During 2003–2008, the period encompassing the implementation of the SP, there was a large creation of firms. According to the Economic census, the stock of firms increased by 24.25 percent. The IMSS data, however, only shows a pyrrhic increase of 2.72 percent, decreasing the share of registered firms to 22.2 percent. This trend is particularly salient for the very small firms (one to five employees), which (according to the Economic census) experienced a 23.3 percent increase (over 630,000 new firms were created).

²²For which there is aggregated data dating back to 1998.

However, the IMSS registry shows a virtually zero increase in the stock of this type of firms.

Similarly, data from the Population census reinforces the idea that there was a very steep increase in the creation of firms. From 2000 to 2010 there was a 38.7 percent increase in the number of employers and a 27.5 percent increase in the number of self-employed.²³ However, the number of registered employers during the decade only increased by 6.15 percent. In all, these figures suggest a dramatic slowdown in the registration of small and medium formal firms in Mexico in the second part of the 2000s. We argue that part of that slowdown is explained by the implementation of the SP.

E. The Cost of Reallocating Workers and Firms to the Informal Sector

This section quantifies some of the costs of this reallocation of firms and workers to the informal sector. Some reliable cost estimates can be obtained under reasonable assumptions. This is the case for the social security contributions lost due to the reduction in the number of formal employees or the VAT loss for the reduction in the number of formal firms. For others, such as the output loss due to the reallocation of firms to informal activities, we can offer an illustration of the order of magnitude under the assumptions that as normally estimated, formal firms are more productive than informal firms.

The loss of social security contributions is relatively straightforward to calculate given the wage distribution of the reallocated workers. Panel A of Table 8 shows the loss of revenue under three different assumptions about that wage distribution. The most conservative assumes that all workers displaced were earning the minimum wage (MW). In the other end we assume that displaced workers were randomly selected from the actual wage distribution of formal workers. The cost is estimated to be between 0.01 to 0.04 percent of GDP which translates into 0.62 and 2.2 percent of total revenue of social security.

From the 2008 Economic census, we obtain the value-added of firms of less than 50 employees which have at least one formal worker. They represent 13 percent of total value-added in the census. VAT in Mexico ranges from 0 to 16 percent, depending on the product. Levy (2008) estimates that the average VAT rate is around 10 percent. The VAT loss is then obtained by multiplying this rate with the potential value-added lost. Given that compliance with VAT is bound to be low, especially for firms in the margin between formality and informality, we then estimate the VAT loss under three different assumptions of compliance: 100 percent, 50 percent, and 25 percent. In the most conservative estimate (where compliance among these firms is 25 percent), the revenue lost is 0.08 percent of GDP (panel B of Table 8).

Finally, Busso, Fazio, and Levy (2012) estimate that there is a substantial productivity difference between formal and informal firms. Even after controlling for a wide set of covariates the authors find a productivity gap of between 60 and

²³ Unfortunately, the Population census does not gather data on the size of firms and hence it is impossible to compare just the creation of small and medium firms, although as established before, 99 percent of the firms in Mexico have up to 50 employees.

TABLE 8—COST AND BENEFITS OF THE SP TAX AND OUTPUT LOSS DUE TO SP

	Type of workers displaced		
	MW	<3MW (with wage structure as of 2011)	All (with wage structure as of 2011)
<i>Panel A. IMSS contributions loss (percent GDP)</i>			
Total	0.01%	0.01%	0.04%
Previous compliance			
	100%	50%	25%
<i>Panel B. VAT loss (percent GDP)</i>			
1–5	0.06%	0.03%	0.01%
6–10	0.06%	0.03%	0.01%
11–50	0.20%	0.10%	0.05%
Total	0.32%	0.16%	0.08%
Productivity gap			
	Busso, Fazio, and Levy (2012)	50%	20%
<i>Panel C. (Percent GDP)</i>			
1–5	0.02%	0.01%	0.00%
6–10	0.01%	0.01%	0.00%
11–50	0.06%	0.04%	0.02%
Total	0.09%	0.06%	0.03%

Notes: The table shows the tax and output loss of the reallocation of 171,000 formal jobs and 36,000 formal firms. For details see online Appendix B.

88 percent. Table 8 (panel C of Table 8) illustrates the output loss derived from the reallocation of 36,000 small and medium firms under three different assumptions about the productivity loss (see online Appendix B for details). Output loss ranges from 0.03 to 0.09 percent of GDP. This estimate should be interpreted with caution since, as Busso, Fazio, and Levy (2012) argue, estimates of the productivity gap between formal and informal firms are not causal.

VI. Conclusion

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 among small and medium firms. According to our estimates, in the absence of the SP between 2000 and 2011, an additional 36,000 employers and 171,000 employees should have been registered with IMSS in firms with less than 50 employees. These represent 4.6 and 4 percent of the stock of registered employers and employees in firms of less than 50 employees in 2002 when the program started.

However, the welfare effects of a policy like the SP are ambiguous. On the positive side, the SP has improved access to health care to millions of Mexicans. In this sense, the program has a clear social-welfare-improving effect since now more workers (and their families) have access to health coverage. The evidence collected so far suggests that this has dramatically reduced the catastrophic expenditure in

health according to Gakidou et al. (2006). The estimated gain from reducing catastrophic health expenditure is estimated to be between 0.07 and 0.14 percent of GDP. Although important impacts in health status are yet to be confirmed by the data, improvements in health may generate gains in productivity, human capital accumulation, and ultimately economic growth (Bloom and Canning 2000). A full welfare analysis of the SP should take into account these gains.

What this paper shows is that, like many public policies, there is a trade-off between competing objectives. According to the results in our paper, the implementation of the SP generated a reallocation of workers away from formality. Hence the possible gains of health coverage have to be weighed against the implications of this reallocation of labor. The reallocation has three main negative welfare effects, specifically loss of revenue, loss of additional benefits of formality for the workers, and productivity losses. We quantify the loss of revenue to be between 0.08 and 0.36 percent of GDP and the output loss in the order of 0.03 and 0.09 percent due to the reallocation of firms away from the formal sector.

In general, welfare gains from providing access to health care to more than half of the population could outweigh the cost of any distortions we find in the labor market, at least in the short run. However, the larger question from the policy perspective is whether achieving universal health coverage by creating a parallel system for the uninsured is the best pathway to reach universal health coverage in a middle-income country with a large uncovered population. This is of foremost importance in a time when there is a general trend in developing countries of closing the social security coverage gaps by providing specific noncontributory programs. On the health side, Thailand, Vietnam, Ecuador, Bolivia, and Colombia, among others, had implemented similar systems to Mexico's Seguro Popular. Furthermore, in the last two decades numerous countries have implemented noncontributory pension systems due to the lack of coverage of the contributory system. Closing the health coverage gap with policies like the SP may exacerbate the need for other noncontributory policies in other areas, which would further fragment the provision of social protection.

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