Is Formal Employment Discouraged by the Provision of Free Health Services to the Uninsured ? Evidence From a Natural Experiment in Mexico

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Abstract

This paper analyzes whether the provision of non-contributory health services encourages workers to move away from jobs that pay contributions to social security (formal employment). It takes advantage of the nationwide roll-out of Seguro Popular a large government program that extended health services to households not covered by contributory social security in Mexico to study such labor market costs. Using a difference-in-differences design that exploits the variation generated by this roll-out across municipalities and time, this paper shows that contemporaneous exposure to the program has no impact on formal employment, and that exposure for at least three quarters leads to a small but statistically significant reduction of 0.78 percentage points in the ratio of formal to total employed or a 4.1 percent decrease in the baseline rate. Using two proxies of indirect exposure to Seguro Popular this paper additionally finds that estimates of program impact are not considerably biased as a result of spillover effects, and that the upper-bound estimates of program effects for municipalities that were directly and indirectly exposed at high intensities are only moderately larger (1.5-1.4 percentage points). These findings suggest that the distortions created by the expansion of non-contributory health services in the labor market are small and possibly incapable of offsetting the expected gains in welfare associated with this type of programs.

Keywords: Labor Markets; Health Provision; Informality; Spillover Effects.
JEL codes : I15, I18, I28, I38, J01, O12, O17.

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Introduction

Nearly one-third of the world population has no access to health services, and most of the uncovered are concentrated in developing and middle income countries (ILO, 2008). With-in these countries, over-reliance on contributory social security systems, limited to affiliated workers and their families, results in low coverage rates that are strongly skewed against those at the bottom of the income distribution.

Over the last two decades a number of countries have implemented large scale health reforms, aimed at improving the degree of protection against impoverishing health shocks by increasing coverage.[1] Although, each reform has its own set of specific features, at their core there is some sort of non-contributory program.[2] These can range from those that fully subsidize contributions to social security for targeted groups (e.g. Chile and Turkey), to programs that result in the introduction of a separate non-contributory tier that runs in parallel to contributory social security, (e.g. Mexico, China, India and Indonesia).

This type of interventions could be expected to be welfare improving, directly through their effect on health outcomes and indirectly through the related gains in productivity, human capital accumulation, and poverty reduction. However, there might be negative consequences for the labor market. The provision of quasi-free health services to the uninsured amounts to a reduction in the relative benefits derived from contributory social security, potentially leading more workers to become or remain informal, Levy (2007) and Wagstaff (2007).[3] This could reduce the number of tax payers and raise the total cost of non-contributory health services, while increasing the fraction of the population that will be unable to access the package of benefits that remain bundled with social contributions (e.g. retirement pensions, disability benefits). Furthermore, there might exist a negative link between informality and productivity, which operates through firm's investment in labor training and technology adoption. Such concerns were strongly present in the Mexican policy dialogue regarding Seguro Popular (SP) a program that provides quasi free health services for those not covered by the contributory social security (Levy, 2007, 2008).

The main purpose of this paper is to weigh in on the cost side of this trade-off, by empirically establishing the impact that this type of interventions can have on formal employment. Conceptually, it is unclear whether the parallel provision of non-contributory health services will result in a large reduction of formal employment, as this depends not only on the change of relative incentives but also on the capacity of individuals to respond to them. In a competitive labor market a strong reduction in formal employment may be expected, both as a result of individuals being encouraged to look for informal jobs, as well as by discouraging individuals from seeking formal jobs. By comparison, if the labor market is segmented and workers are unable to choose between sectors, the effects should be significantly smaller. The only likely effect would operate through a reduction in the degree of effort invested in searching for a formal job, plausibly leading to a reduction in the number of transitions towards the formal sector.[4] Hence, the most informative upper bound estimates of program impact would be derived in the case of a country where the labor market is competitive, a large scale intervention has been put in place, and a strong case for the internal validity of the estimates can be made.

[1] A non-exhaustive list of countries that have implemented large scale health reforms that aim to achieve universal health care coverage, include: Brazil, Chile, China, Colombia, India, Indonesia, Israel, Mexico, Peru, Taiwan, Thailand, Turkey.

[2] See OECD (2011) for a more detailed review and Robalino et al. (2010) for an overview of social protection in Latin America.

[3] Although, formality is a multi-dimensional concept, in this article it will be equated with paying contributions to social security, as this is the most relevant dimension for the policy issue at hand.

[4] See Perry et al. (2007) for an overview of the competing theoretical interpretations and their implications.

For all this reasons Mexico provides an almost ideal setting for addressing this question.

In terms of the Mexican labor market there is little evidence in support of segmentation, Maloney (2003) and Maloney and Bosch (2006). More specifically, minimum wages have not been binding for a decade, unions are primarily concerned with preserving employment, wages are extraordinarily flexible during crises, and the patterns of worker transitions between all sectors, formal and informal do not correspond to those of a segmented market.

Regarding the program, in 2004 the Mexican government introduced SP, and rolled it out to more than 42 million affiliates by 2010, making it the largest expansion of non-contributory health services in the Americas. The increase in coverage was accompanied by a significant increase in government health care expenditures and possibly by improvements in quality. According to the presidential report to congress in 2011 cited in Azuara and Marinescu (2011), average non-contributory public health expenditures increased by as much as 50% from 1080 in 2000 to 1620 pesos in 2010. Additionally, it is has been argued that the program not only helped standardized the quality of services across the country, but that it also improved access while increasing the scope of services, Lakin (2010). More specifically, it offered for the first time a guaranteed package of benefits, (i.e., drugs and interventions), that required no out-of-pocket payments at the point of service, and that was deemed capable of covering at least 90% of the disease burden. Positive impacts on the use of health services and expenditures have been document in a series of papers (Knaul et al. (2006), Gakidou et al. (2007), Knox (2008), Barros (2008)) including a randomized control trial that shows that SP was able to sharply reduce both out-of-pocket and catastrophic health care expenditures King et al. (2009).

Interestingly for identification, the progressive roll out of the program at the municipal level created a source of variation across space and time in the provision of SP services, which can be exploited using a difference-in-differences design, where municipalities that received the program at an earlier stage serve as the treatment group and those that received it later serve as the control group. As long as the change in the control group provides an unbiased estimate of the counterfactual I will be able to estimate the causal impact of SP on formal employment. While this claim cannot be tested directly, this paper focuses on a subsample of municipalities for which it is possible to show that pre-intervention time trends between potential treatment and control groups are parallel, making the common trend assumption plausible and thereby allowing me to make a stronger claim of internal validity.

Additionally, in order to address other threats to identification, such as time varying factors that are related to both the timing of SP and formal employment, this paper will lay out the alternative objectives that federal and state authorities could have pursued when deciding program roll out, narrow down the type of confounding factors that are likely to play a significant role, and then take advantage of various data sources, from public finance administrative records to electoral results at the municipal level, in order to directly control for these factors in the difference-in-differences specification.[5]

The empirical strategy deals with two additional issues. The first is expanding the analysis to estimate the effect of lagged exposure to the program. This is important because it is reasonable to assume both that individuals are unlikely to be immediately aware about the availability of SP and/or that the initial valuation of the services is low given their experience with other governments programs. The second issue that the paper addresses is whether municipalities could have been indirectly exposed to SP or its effect on the labor market. Exploring the validity of the stable unit of treatment assumption is particularly interesting in this setting as its violation may not only be a source of bias but also of heterogeneity in program impact. Two proxies of spillover effects help gauge the impact of indirect

[5] A detailed description of the datasets can be found on page 1 of the online appendices.

exposure: distance to the nearest municipality offering SP, and the share of population of neighboring municipalities with direct access to SP.

As such, this paper goes beyond a recent set of papers analyzing the labor market consequences of SP Campos-Vazquez and Knox (2008), Bosch and Campos-Vazquez (2010), Aterido et al. (2011), as well as Azuara and Marinescu (2011). First it contributes to the literature by employing an empirical strategy that pays careful attention to possible violations of internal validity, thereby allowing me to derive causal estimates of both the contemporaneous and lagged impact of SP on formal employment with less stringent assumptions. Second, the role that indirect exposure to SP plays is accounted for, bolstering the reliability of the estimates of program impact, as well as providing interesting insights into the mechanisms that are possibly limiting the negative labor market effects of this type of programs.

Briefly, the main findings are that contemporaneous exposure to SP has no impact on the ratio of formal to total employed.[6] Exposure for at least three quarters leads to a small but statistically significant reduction of 0.78 percentage points or a 4.1 percent decrease in the baseline rate. Estimates controlling for spillover effects are statistically undistinguishable from those previously derived, suggesting that at average levels of indirect exposure estimates of program impact are not considerably biased. Estimates that explore the effect of indirect exposure at the highest levels observed in the sample, indicate in the case of treatment municipalities that the total program effect is only moderately larger, 1.4 or 1.5 percentage points depending on the proxy that is used.

The paper is hence organized as follows: Section 2, provides a brief background on the Mexican health care system. Section 3, describes the identification strategy. Section 4, presents the main set of results and discusses a wide range of robustness checks. Section 5, explores the role of spillover effects, and section 6 concludes.

2 Background

From its onset in 1943 the health care system in Mexico has been segmented by employment status. The majority of private sector workers (and their families) are affiliated with IMSS[7] while public sector employees access their services through ISSSTE.[8] Like in other contributory social security systems, these organizations serve as both social security funds and as health providers. However, in the case of Mexico, affiliates have no guaranteed package of drugs or interventions, they are only able to access hospitals operated by their own fund, and other types benefits, such as pensions or disability benefits, are bundled with health services.[9] In exchange for these set of benefits employers and employees pay a pay-roll tax premium that is subsidized by the government. Workers contributions roughly correspond to 31.5 % of wages, with almost a third of that amount going to health care.

Since the 1990's Mexicans without employment could potentially join IMSS through the family health insurance provision. However, the lack of coverage for pre-existent conditions and the requirement to pay both the employee and the employer contributions meant that inscriptions through this channel are for all purposes negligible.

[6] This zero result is sharply estimated.

[7] By its acronym in Spanish: Instituto Mexicano de Seguro Social

[8] By its acronym in Spanish: Instituto de Seguridad y Servicios Sociales de los Trabajadores del Estado

[9] A more extensive list of services include: life insurance, retirement pensions, disability benefits, housing loans, severance payments and in kind transfers such as sports, cultural facilities and day care services.

Prior to the introduction of SP those not covered by contributory social security, roughly 66% of the employed,[10] relied on health services provided by the ministry of health through its own network of clinics and hospitals. And while qualitative assessments have usually pointed out that the services in this network can best be described as "limited, frequently unavailable and often requiring out-of-pocket payments at the point of service", Lakin (2010). It's hard to draw general conclusions over the quality of services as they are likely to have varied across states, both as a result of the decentralization process in the 1980s that made states responsible for health expenditure in the non-contributory tier, and also because of the skewed distribution of federal health funds which tended to favor states that were generally better off, Lakin (2010).

That said, given the large disparities between the insured and the uninsured in terms of health expenditures and health outcomes, it is likely that even in the most efficient and well funded states the services of contributory social security out performed those of the non-contributory tier. OECD (2005) shows that per-patient expenditure in the non-contributory tier was roughly half of that of IMSS and ISSSTE. Accordingly, the work of Gakidou et al. (2007) documents that while infectious diseases were a minor cause of death for the population covered by the contributory social security tier, they remained prevalent among the uninsured population.

Following a pilot phase, that took place between the fourth quarter of 2002 and the fourth quarter of 2003. The flagship program of the comprehensive overhaul of Mexico's General Health Law, came into effect on January 1^{st} 2004. SP represented a departure from the status-quo, because it achieved three broad objectives. First, it substantially increased federal financing to the non-contributory tier, which grew from US \$2.8 billion in 2000 to US \$5.8 billion in 2007, Lakin (2010). Second, it standardized the quality of services by progressively shifting federal resources to states that tended to be under-financed while exerting additional federal control, by attaching conditions to the use of these new resources. Third, it improved efficiency by creating a national risk pool for particularly expensive interventions.

On the whole, for the uninsured population these changes lead to an overall increase in the access to health services, both as consequence of improved infrastructure but also because of the elimination of any kind of fees at the point of service. Additionally, SP increased substantially the scope of health services by introducing for the first time a guaranteed package of basic services. This package was originally composed of 169 interventions, which go from routine check-ups to third level surgeries, as well as 333 drugs deemed capable of covering 90% of the disease burden in a given region. Over time the package has been continuously upgraded, by 2006 it included 269 medical interventions, and it currently covers 275.[11] In addition, to the basic package affiliates are covered through the catastrophic expenditure fund against illness such as AIDS, childhood cancers, cervical cancer, premature birth and other particularly expensive conditions.

The affiliation process to SP, requires eligible individuals, that is to say, Mexican residents 18 years or older who are not covered by contributory social security, to visit one of the 912 information and affiliation modules, known as MAO's by their acronym in Spanish, there the head of the household will provide some basic documentation, which includes an identification document know as the CURP, as well as proof of residency and the birth certificate of the members of the family that are to be included in the insurance policy. According to the rules of the program, the spouse or partner, sons, biological or adopted until the age of 18 or up to 25 if in school and unmarried, as well as relatives older than 64 who reside in the same dwelling, will be covered by the issued policy.

[10] Calculations from the 2002 Q4 INEGI ENE LFS.
[11] Annual reports by the Comision Nacional de Proteccion Social en Salud.

During the visit to the MAO individuals were also administered a small income evaluation survey, that would be used to determine the premium that families would be required to pay for a SP policy. It was planned that the first two income deciles would receive the program for free while the higher income deciles would have to pay a progressive premium. In practice, however, the program was provided free of cost to the overwhelming majority of affiliates, Scott (2006). This is probably the consequence of using a survey that could be easily manipulated by "field officers trying to meet affiliation targets and by families trying to avoid premium payments.", Lakin (2010), and/or to collective affiliations made by unions, NGO's and other government agencies that were generally exempted from any kind of verification procedure. All in all, while SP may have been envisioned as a health insurance program in practice it operated as a large expansion of non-contributory health care services.

Another important feature of the program is its implementation at the municipal level, and while it would be ideal to have an official record of when and where SP services had been offered,[12] after discussing with senior SP officials, it was clear that the only accurate records available were the number of affiliations by municipality and quarter. In order to recover the sequence in which SP was introduced across municipalities from this administrative records, I define that a municipality has direct access to SP when the number of affiliations is larger than 10, this cut-off point while arbitrary has no bearing on any of the results of this paper which have been reproduced using various cut-off points (e.g., when the affiliation threshold is set at 5 or 1, available upon request).

Using the previous definition, Table 1a below, illustrates how the roll out of the program brakes down by year, while figure 1b highlights that even though the uninsured have the option to affiliate to SP at any point, program take up grew side by side with the introduction of the program at the municipal level. Here it is worth emphasizing that 61% of municipalities received the program between 2004 and 2006 and that during this period SP coverage of the uninsured population sharply increased from 3.6% to 26%. Among those municipalities with direct access to SP the average take up rate of the uninsured population during this same period was in the order of 27%, with municipalities in the top decile reaching coverage rates of up to 88% as detailed in figure 3 of the online appendices.

Finally, in terms of the data sources used a detailed description of the variables and the datasets can be found on page 1 of the online appendices. Briefly, this paper will employ data produced by the Mexican bureau of statistics (INEGI), including the labor force surveys ENE and ENOE, the 2000 census, geo-statistical datasets at the locality, municipal and state level, as well as municipal level records of public finances and health infrastructure. Additionally, data on SP affiliations by municipality and quarter comes from the health and social protection bureau (CNPSS). Electoral results and measures of political competition at the municipal and state level have been originally compiled by the financial group (BANAMEX) and the think tank (CIDAC) but have been later on complemented and updated by myself from various public sources.

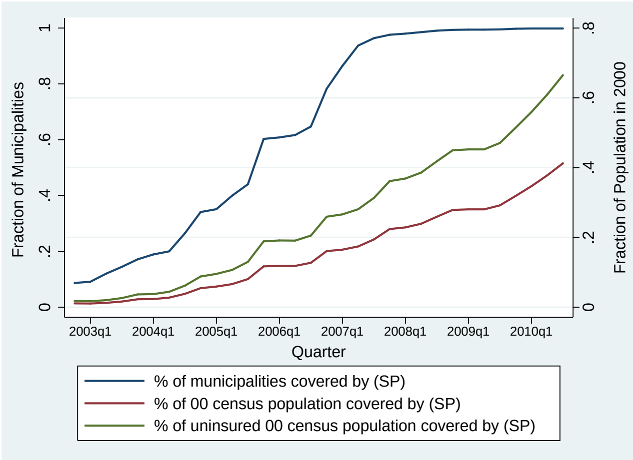
[12] A municipality is a second level administrative division, equivalent to a county in the US.

Figure 1: Geographical Coverage and Program Take up.

(a) Number of Municipalities offering SP by year

Year	No.	Col %	Cum %
2002	213	8.7	8.7
2003	209	8.5	17.2
2004	416	17.0	34.2
2005	643	26.2	60.4
2006	441	18.0	78.4
2007	474	19.3	97.8
2008	43	1.8	99.5
2009	10	0.4	99.9
2010	2	0.1	100.0
Total	2451	100.0	

(b) Program Take Up as Fraction of: Municipalities and Population (Total and Uninsured).



Source: Author's calculations based on administrative records of the CNPSS and the 2000 Census.

3 Identification Strategy

This section addresses the central policy issue of establishing whether SP is capable of reducing formal employment. More specifically, the objective of the paper is that of identifying the average effect of the program on the ratio of formal to total employment in the municipalities in which the uninsured population has been given access to services by SP (i.e., the average impact of treatment on the treated).

This would ideally be done by comparing at the same point in time the ratio of formal to total employment when SP services are offered in a given municipality to the counterfactual (i.e. the ratio of formal to total employment when the only medical services available to the uninsured population are those originally provided by the Ministry of Health). Since this counterfactual cannot be observed, it must be estimated.

A first best would be to have SP randomly assigned across municipalities and then compare the average outcomes for the two groups. However, in the absence of a randomized control trial, a second best is that of using a non experimental method such as the difference-in-differences design.

In this case, it may be possible to estimate the causal impact of SP, by comparing the change in the ratio of formal to total employment before and after the introduction of SP for a group of municipalities that received the program at an early stage (i.e., the treatment group), with the change in the ratio of formal to total employment for a group of municipalities that had not yet received the program (i.e., the control group). Because SP services were progressively rolled out across municipalities during the 2002-2007 period there are many potential "experiment" to exploit. And although the background discussion suggests that it is reasonable to focus on the post 2004 period where the sharp increase in program take up begins, it is still desirable to establish whether a stronger claim of internal validity can be made by further narrowing down the period and sample of municipalities that will be taken into consideration.

In a nutshell, the advantage of this type of design, is that the change in the treatment group allows me to control for time invariant characteristics of municipalities that may be correlated with the timing of SP and formal employment, while the change in the control group accounts for time varying factors that are common to both control and treatment municipalities. Thus, As long as it can be confidently claimed that the change in the control group provides an unbiased estimate of the counterfactual, it is possible to establish a causal link between SP and its impact on the ratio of formal to total employment. This identification assumption cannot be directly tested but supporting evidence can be provided.

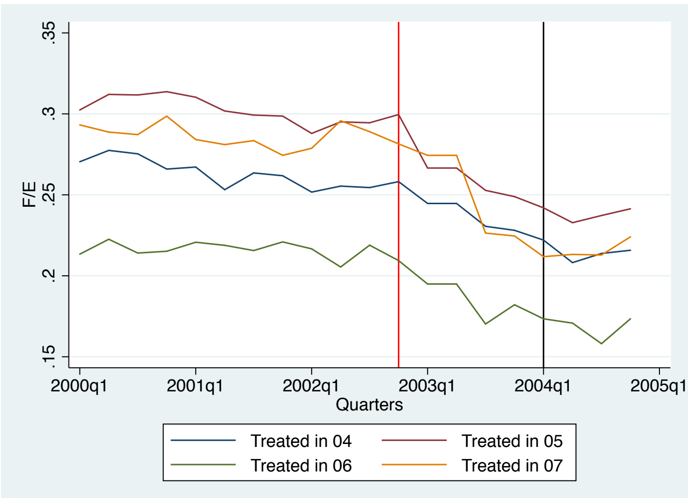
In what follows, I will show that the pre-intervention time trends in the ratio of formal to total employed are parallel between two groups of municipalities that received the program at different stages, potentially providing a treatment and control group for whom it is likely that in the absence of SP their trends would have continued to be parallel in the post-intervention period.

Additionally, in order to deal with other threats to internal validity, such as time-varying unobserved covariates correlated to both the timing of SP and formal employment, a detailed discussion of the determinants of program placement will be made and the most likely confounding factors singled out and controlled for in all specifications. Finally, it will be shown that the group of municipalities for which the strongest claim of internal validity can be made is also the most homogeneous in terms of their economic characteristics at baseline, thereby reducing the likelihood that estimates of program impact could be biased in the event that the effect of SP varies in relation to the characteristics of municipalities

3.1 Pre-Intervention Time Trends

Figure 2 below, provides a first glimpse of how the time trends in the ratio of formal to total employment[13] vary according to the year in which the program was introduced. The first vertical line denotes the quarter in which SP begun operating as a pilot while the second line denotes the time at which the program was officially launched. As can been seen while the time trends between those municipalities that received SP in 2004, 2005 and 2006 appear to be parallel, those of municipalities that received the program in 2007 are clearly distinct.

Figure 2: Formal employment trends in the pre-intervention period by year in which the municipalities received SP.



Source: Author's calculations based on ENE.

[13]One specificity of the Mexican case worth nothing, is that the statistical bureau INEGI moved from the ENE labor force survey (LFS) to the ENOE LFS in the first quarter of 2005. Given that there might be some comparability issues, I will only use data from the ENOE survey for the rest of the paper. However, in this section, I am obliged to use data from the ENE survey which goes back to the first quarter of 2000.

In order, to test whether the pre-intervention time trends are parallel between a first obvious choice of treatment and control group, namely, those municipalities that were going to receive the program in 2005 (the treatment group) and those were it would be introduced in 2006 (the control group). I estimate a simple model that uses only pre-intervention observations (i.e., before the first quarter of 2004). The dependent variable is the ratio of formal to total employment in municipality (m) at quarter (t), on the right hand side the model includes an indicator variable equal to 1 if a given municipality receives SP in 2005 and 0 if it did in 2006, as well as a full set of quarter dummies and their interactions with the indicator variable.

Figure 4a in the online appendix, depicts the trends between these two groups which appear to be parallel. Furthermore, since the coefficients on the interaction terms between the quarter effect and whether a given municipality received SP in 2005, are neither individually nor jointly significant at conventional levels. I am unable to reject the null hypothesis that pre-intervention quarter dummies are the same for both the treatment and the control group, there by bolstering the case of this choice of control and treatment groups.

Additionally, since figure 2 illustrates that the municipalities that received the program in 2004 also seem to be parallel to both groups, it might be feasible to expand the sample of treated municipalities with those that received the program at this earlier stage. In order to assess whether this is the case, I test whether the pre-intervention time trends between the treatment and control group remain parallel, when the treatment group is enlarged by including municipalities that received SP in the different quarters of 2004. Figures 4b to 4e found in the online appendix present the main set of results. The main finding is that as before, in all cases I'm unable to reject the null hypothesis that pre-intervention quarter dummies are the same between each of the enlarged treatment groups and the control (municipalities that received the program in 2006). Thereby, suggesting at least five possible configurations of treatment and control groups where a strong case of internal consistency can be made.

3.2 What factors determined the order in which municipalities received Seguro Popular ?

From what is publicly know about the history of SP as well as from conversations with SP officials, it is clear that state governments played a central role in determining the sequence in which municipalities received the program. Both because they were able to determine the moment in which a state would opt-in to the program, and because state governors had considerable leeway in defining the order in which municipalities would receive the program within their state.

The degree to which the influence of state governments was curtailed by the objectives of the federal government is hard to assess. However, after the introduction in 2004 of agreements of participation between states and the federal government, the capacity of state governments to influence program placement is likely to have been considerably constrained. More specifically, the agreements made federal funds for SP conditional on a set of operational guidelines, which in the case of program placement, clearly established that priority was to be given to the poorest municipalities that satisfied a set of minimum infrastructure requirements.

In terms of the identification strategy the influence of the Federal and the state government on the timing of SP has very different implications. While the targeting condition of the federal government, which depends on baseline characteristics of municipalities, is unlikely to pose a major threat, as the difference in difference design is able to account for time invariant heterogeneity. The fact that state governments could have factored in other considerations when deciding program roll out, does represents a major concern, as time varying unobserved covariates that are correlated with both the introduction of SP and the ratio of formal to total employment could potentially bias the estimates of program impact.

For example, if constituencies are more demanding of elected officials when they experience shocks, as suggested by a number of behavioral models, then state governments would have an incentive to respond to local economic downturns by pushing for the early implementation of government programs such as SP. Alternatively, if state governments deploy SP in order to gain an electoral edge it is reasonable to suspect that they will do so in conjunction with other government programs and/or regulations capable of affecting formal employment. An example of the first, would be the provision of SP in addition to a simultaneous increase in public sector jobs.[14] While an example of the second, would result from municipal governments of political parties, that have a low tolerance for informal employment, reacting to the introduction of SP with legal measures that are likely to crack down on informal workers, such as city ordinances that regulate street trading.

In order to deal with these concerns I take a twofold approach. First, the difference-in-differences specification that will be described in detail in section 3.3 will additionally include a set of time varying controls that directly addresses each of these possibilities. Second, In order to provide supporting evidence with which to rule out that the factors previously outlined are a major source of concern, I am currently working in a discrete-time hazard model that estimates the probability of a municipality receiving SP in a given quarter as function of both time invariant characteristics of municipalities as well as a set of time varying variables related to each of the hypothesis previously described.

For now, I will provide a brief intuition of the type of factors that determined the sequence in which SP was rolled out, by estimating a model that uses the pre-intervention characteristics of municipalities in order to predict the date in which SP was offered for the first time. The details of the model, the variables used, and the results can be found in section 2 of the online appendix.

Using the complete history of program roll out, the main finding is that operational rules seem to have been followed by state governments. It was possible to verify that within states poorer municipalities received the program earlier. And although the measures of baseline medical infrastructure did not seem to play a determinate role, it was nonetheless established that municipalities that were harder to access, as measured by their geographical characteristics and their degree of access to the road network, did in fact receive the program later, tentatively suggesting that minimum infrastructure requirements were respected.

Interestingly, it was also possible to establish that political considerations played a decisive role at both the local (i.e. municipal and state) and Federal level. More specifically, it was shown that the timing of elections[15] party incumbency, the political affiliation of the mayor in relation to that of the governor, and the degree of political competition at baseline are strongly correlated with the timing of SP. Additionally, I am able to confirm the finding that smaller states, in terms of their population, received the program earlier. According to Diaz-Cayeros et al. (2006) this finding is a result of the federal government efforts to showcase states with full coverage of SP.

Furthermore, I am able to show that municipalities with larger total per-capita expenditures, which

[14] State and municipal governments are responsible for the provision of: electricity, water, drainage, security, education, and the maintenance of public areas.

[15] In Mexico every state has its own electoral calendar, municipal elections are held every 3 years and state elections every 6. It is possible to observe elections in every year in the sample.

are financed predominantly through discretionary funds, tend to receive the program earlier. While the result is reversed when I use infrastructure expenditures per capita, which are usually financed with labeled funds. Since discretional funds are more likely to be susceptible to political capture, these findings provide further evidence of the importance of political considerations when determining the role out of SP.

Finally, restricting the sample to municipalities that received the program between 2004 and 2006, which has been previously identified as the group for whom pre-intervention trends are parallel, provides further insights. In this case, it is possible to show that neither the degree of access nor the economic characteristics of municipalities are significant predictors of program placement, thus suggesting that this group is far more homogenous. If it was suspected that the impact of SP on the labor market varied as a function of these characteristics, this result would be important as it makes it less likely that the estimates of program impact could be biased as a result of having no comparable municipalities in the control group or vice-versa.

3.3 The difference-in-differences specification.

In terms of the specification of the model, the arguments previously outlined suggest a very particular choice of treatment and control group, and consequently of the period under analysis. Figure 5 of the online appendix describes in detail the strategy. Ideally, the preferred specification would use municipalities that received SP between 2004 and 2005 as the treatment group and those that received the program in 2006 as the control.

Accordingly, the period under analysis should extend from the first quarter of 2004 till the fourth quarter of 2005. In the case of specifications that use a lagged definition of program impact, this basic strategy has to be adjusted in order to account for the fact that some of the original treatment municipalities (i.e. exposed to SP between 2004 and 2005) would have no with-in variation in the period of analysis previously described.[16] This could be accomplished either by dropping these municipalities from the treatment group or by extending the period of analysis by the number of lags being tested (this is equivalent to comparing municipalities exposed to SP for a period longer than the number of lags being tested with municipalities exposed for a shorter period of time.)

For example, the one lag specification would extend the period of analysis to the first quarter of 2006, thus allowing municipalities that received SP in the fourth quarter of 2005 to remain as part of the treatment group. In this case the estimated program impact is derived from the comparison of municipalities exposed to SP for at least one quarter with those never exposed and those exposed for one quarter.

In order to maximize statistical power I have chosen to extend the period of analysis in accordance to the number of lags being tested. However, in order to rule out the posibility that the estimates of program impact could be driven by exposure of the control group to SP, in section 4 it will be shown, that narrowing down the control group to municipalities that have never been exposed to SP, or dropping treatment group municipalities with no with-in variation and using the original period of analysis, leads to estimates of program impact of very similar magnitude, albeit noisier given the smaller sample size.

[16] If in the fourth quarter of 2005, a municipality has been exposed to SP for a number of quarters smaller or equal than the number of lags being tested the SP indicator variable would be zero throughout the period of analysis (i.e., it would be treated as a control municipality).

Another element that the difference in difference specification must take into account is that Mexican labor force surveys (LFS) underwent a considerable transformation during this period. More specifically, in the first quarter of 2005 the ENOE LFS was introduced, and while its substantial revisions to methodology and variable definitions made it far more suitable for international comparisons than the ENE LFS,[17] it created a host of problems for drawing meaningful comparisons across surveys, see INEGI (2009) for a review of the limitations. In the case of this paper, revisions in variable definitions are of particular concern, as this changes could lead to biased estimates of program impact under plausible assumptions.[18]

In order to avoid this potential source of bias, the main results of this paper will be derived using only data and variable definitions from the new LFS ENOE. This choice comes at a relatively small cost as I will still be able to use most of variation created by program roll out in 2004. Figure 6 of the online appendix provides the details for each specification. Succinctly, the main implication of this data constrain is that the period of analysis will now necessarily have to start in the first quarter of 2005, and consequently, that the treatment group has to be narrowed down to those municipalities for whom there is with-in variation in these new time frames.

More specifically, those municipalities that have been exposed to SP in the first quarter of 2005 for a number quarters greater or equal to the number of lags under review, will be dropped from the analysis as they no longer contribute to identifying the impact of the program.[19] For example, in the one lag specification, the period of analysis will end, as before, in the first quarter of 2006 but will now start in the first quarter of 2005. Accordingly, municipalities exposed to the program in 2004 will be dropped from the treatment group, as they no longer contribute to identification. Analogously, specifications that test higher order lags will progressively allow the introduction of municipalities exposed to SP in 2004 into the treatment group, as these municipalities will have with-in variation in their respective windows given the lagged definition of treatment.

This changes in the composition of the treatment group along with the previous discussion on the appropriate period of analysis explain the differences in sample size between each of the specifications that will be tested, note additionally that the pre-intervention time trends have been shown to be parallel for each of these configurations.

Another important specification choice was that of performing the analysis at the municipal level. This decision is partly motivated by the fact that program implementation took place at this level, and more fundamentally, because this type of specification provides a natural way of deriving an upper-bound of program impact, when the effect of SP is expected to be driven by smaller municipalities,[20] as argued by Bosch and Campos-Vazquez (2010). That said, given that controls might perform better at the individual level, section 4 verifies that the results at the municipal and individual level are consistent.

Formally the difference-in-differences model can be specified as a two-way linear regression model, the

[17] This LFS was produced by INEGI between the first quarter of 2004 and the fourth quarter of 2004.

[18] For example, the definition of absent worker without a labor contract (e.g. self employed workers who worked less than one hour in the reference week or who did not earn income from this activity) changed, while they were considered as employed in the ENE they are no longer counted in the ENOE. If I take the relevant definition to be that of the ENE, and assume that some of these workers are absent as a consequence of health shocks that could have been prevented through adequate provision of medical services, or conversely that they are more likely to be absent as a result of being diagnosed. Then it is reasonable to expect that the measurement error in the ratio of formal to total employment derived from ENOE surveys will be (negatively/positively) correlated to SP implementation, potentially leading to a (downward/upward) bias in the estimates of program impact.

[19] This occurs because the program indicator variable would take the value of one throughout the new period of analysis.

[20] In terms of population.

basic setup is given by equation 1 below.

$$\frac{F_{mt}}{E_{mt}} = \alpha + \beta_t + \beta_m + \beta_1 SP_{m,t-L} + \omega X_{mt} + \epsilon_{mt} \ \ ,\forall \ \ m \in M, \ \ t \in T \tag{1}$$

Where the subsample of municipalities (M) and the period of analysis (T) are as previously discussed and as illustrated in figure 6 of the online appendix. A detailed description of variable definitions and their sources can be found in section 1 of the online appendix. Briefly, the left hand side variable $\frac{F_{mt}}{E_{mt}}$ is the ratio of formal to total employment in municipality (m) at quarter (t). On the right hand side, $SP_{m,t-L}$ is a dummy variable that takes the value one if SP is being offered in municipality (m) at quarter (t-L) and is zero otherwise, L represents the number of lags that is being tested, $\{L \in \mathbb{Z} | 0 \leqslant x \leqslant 4\}$. β_m is a fixed effect unique to municipality (m) and β_t is a time effect common to all municipalities in quarter (t). X_{mt} is a vector of control variables that vary across municipalities and time.

Unless otherwise stated the vector X_{mt} is composed of three sets of time varying controls. The first group, accounts for changes in the demographic composition of municipalities, it includes employed population shares of: age (5 groups), educational attainment (4 groups), martial status (6 groups), gender and urban status.

The second group, address the possibility that program allocation responded to economic shocks, it includes employed population shares of labor income (7 groups) as well as the 10th 50th and 90th percentile of labor income in a give municipality.

The third group, controls for both the possibility that SP may have been introduced in conjunction or in replacement of other government programs, and that different political parties (while in office) may have pursued policies and regulations capable of affecting formal employment, it includes: total and infrastructure expenditure per capita, a set of municipal level dummies that take the value of 1 when one of the three main political parties or their alliances (eight groups in total) holds a Mayor's post, and an analogous set of state level dummies for the political affiliation of the governor.

4 The impact of SP on formal employment.

Table 2 below, explores both the contemporaneous and the lagged impact of SP on the ratio of formal to total employment, using the specifications derived from equation 1. The main findings are that there is no evidence of SP having a contemporaneous impact on the ratio of formal to total employment, column 1. And that the coefficients in the three and four lag specifications, columns 4 and 5, indicate that SP leads to a small but statistically significant reduction in formal employment. In particular, the coefficient in column 4, suggests that exposure to SP for at least three quarters is associated with a 0.78 percentage point reduction in the ratio of formal to total employment, which amounts to a 4.1 percent reduction of the baseline rate (the average ratio of formal to total employment in control municipalities in the first quarter of 2005 is 0.187).

In all cases I am able sharply estimate the impact of the program, minimum detectable effect (MDE)[21] calculations, presented on the last row of table 2, suggest that if testing could be carried out recurrently, in 80% of the cases I would be able to reject the null hypothesis of no program effect at the five percent

[21] The MDE is given by $(t_{\alpha/2} + t_{1-\kappa})\sigma_{\hat{\beta}}$ from here on, assuming $\alpha = 0.05$ and $\kappa = 0.8$ implies that $t_{\alpha/2} = 1.96$ and $t_{1-\kappa} = 0.84$, thus the $MDE \approx 2.8\sigma_{\hat{\beta}}$, the standard errors used for this calculations are those clustered at the municipal level.

level, as long as the impact of SP on formal employment was in the order of 1.5 to 1.1 percentage points.

This latter result, however, depends on the assumptions that have been made regarding the term ϵ_{mt} of equation 1, which represents a municipal time varying error that is assumed to be independently distributed of β_m and β_t. In order, to account for the possibility that the error term is correlated across time, for example, because of persistence in regional shocks to labor demand or supply, I allow for an arbitrary covariance structure within municipalities and over time by computing standard errors clustered at the municipal level, they are reported in parentheses in all specifications.

Additionally, since it is also reasonable to suspect that the error term could be correlated through space, standard errors clustered at the state-quarter level are presented in brackets. Last, I provide the most conservative estimates of standard errors by following the work of Cameron et al. (2011) on multi-way clustering which allows for simultaneous clustering at the municipal and state-quarter level, this type of standard errors are reported in crochets. As can be seen from the comparison of these estimates, the main finding is that neither the statistical significance of the estimates of program impact, nor the reliability of the MDE calculations hinge on the assumptions that are made with respect to the standard errors.

In order to further ascertain whether the estimated impact of SP on formal employment is robust, tables 3 and 4 below, presents a number of variations to the specification in which the strongest case for any impact of SP on the labor market can be made, namely, exposure for at least three quarters. These variations and robustness checks can be categorized in 6 groups.

First, columns 1 to 4 of table 3 emphasize the importance of controlling for time varying factors capable of confounding the impact of SP. The main finding is that although the coefficients are statistically indistinguishable from each other across columns, controlling for these factors is important as it leads to different interpretations. More specifically, this can be seen by comparing the coefficient with no controls in column 1 to any of the other coefficients. While in the former a case for a zero result could be made in the latter the larger and statistically significant coefficients suggest that SP was in fact capable of reducing formal employment albeit only slightly.

Second, columns 5 to 11 expand the benchmark specification (i.e. column 4) by including a series of additional controls. More specifically, in column 5, it is corroborated that the empirical strategy is appropriately accounting for time varying factors by estimating a very demanding specification that includes both the contemporaneous and the lagged vector of controls, that is, $X_{m,t}$ and $X_{m,t-L}$, as can be seen the estimate of program impact varies only slightly.

Next in columns 6 and 7, it is assessed whether the impact of SP could be underestimated as result of not taking into account the strategic response of contributory social security providers, (e.g., IMSS could have responded to increased competition by increasing the quality and coverage of services offered to its affiliates). Column 6 includes municipal level, contributory social security medical personnel per capita as a control, while column 7 uses the number of medical units per capita. As can be seen in both cases the coefficients are statistically significant at conventional levels and are of a very similar magnitude to the benchmark specification.

Table 2: Current and Lagged Effect of SP

	(1)	(2)	(3)	(4)	(5)
VARIABLES	F/E	F/E	F/E	F/E	F/E
SP (=1)	0.00147				
	(.0055)				
	[.0048]				
	{.005}				
Lag SP (=1)		-0.00541			
		(.0055)			
		[.0058]			
		{.0058}			
Lag 2 SP (=1)			-0.00355		
			(.0045)		
			[.0049]		
			{.0049}		
Lag 3 SP (=1)				-0.00777	
				(.0039)**	
				[.0038]**	
				{.004}*	
Lag 4 SP (=1)					-0.00699
					(.0038)*
					[.0035]**
					{.0038}*
Observations	1,505	1,949	2,775	3,698	4,297
Number of entmunid	380	413	492	561	573
Num S*Q Clusters	96	120	150	175	208
MDE	.0153	.0153	.0127	.0108	.0107

Note: OLS estimation. Robust standard errors clustered at the municipal level in parentheses, at the state-quarter level in brackets, and in both dimensions through multi-way clustering in crochets, * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is the ratio of formal to total employment. SP is a dummy variable that takes the value of 1 once a SP facility is open and registers at least 10 affiliations in a given municipality and quarter. All regression include municipal and quarter fixed effects as well as a set of time varying controls organized in three categories. Controls I, include: employed population shares of age (5 groups), educational attainment (4 groups), martial status (6 groups), urban and gender. Controls II, include: employed population shares of labor income (7 groups) as well as the 10th 50th and 90th percentile of labor income in a give municipality. Controls III, include: total and infrastructure expenditure per capita, as well as a set of municipal and state level dummies that take the value of 1 when one of the 3 main political parties or their alliances (8 groups in total) holds a Mayor or Governor post. The subsample of municipalities (M) and the period of analysis (T) used in each regression is as described in section 3.3. *The ratio of formal to total employment in control municipalities at baseline (05q1) is 18.7%.*

Additionally, in order to control for changes in the characteristics of employers and the composition of local labor markets, the specification in column 9, includes employed population shares of the size of firms and the type of industry. As before, the estimate of program effect remains statistically significant and is of a similar magnitude.

More generally, the specifications in column 9 and 10, assess whether unobserved time varying factors that affect regions[22] or states equally are capable of driving the results, this is done by including region-quarter and state quarter fixed effects. As can be seen, the introduction of region-quarter dummies has no bearing on the results, while the demanding state-quarter fixed effect specification[23] leads to a smaller SP coefficient (5.8 percentage points) that is not statistically significant at conventional levels. Since changes in the level of economic activity at the state level are a primary source of concern, column 11, introduces a state level electricity consumption index as control. In this case the estimates of program impact are undistinguishable from those of the benchmark specification.

The other types of robustness checks, are presented on table 4. Specifically the third group, columns 1 and 2, assess whether a nonlinear panel data model that recognizes the bounded nature of the dependent variable is able to provide a better approximation to the conditional expectation function. Following Papke and Wooldridge (2008) I employ a pooled fractional probit (PFP) in order to recover the average partial effect of SP. The coefficients are estimated using quasi-maximum likelihood,[24] while the standard errors are derived by bootstrapping. Since the method is only defined for the case of a balanced panel, for purposes of comparison, column 1, presents the OLS fixed effects estimate for this case, (0.72 percentage points). Reassuringly, the (PFP) estimate of program impact is only slightly bigger (0.75 percentage points).

The fourth group, addresses an issue previously discussed, namely, that of ruling out that the estimates of program impact could be under-estimated by the exposure of some control group municipalities to SP. In the benchmark specification, the period analysis runs from the first quarter of 2005 till the third quarter of 2006. Accordingly, the treatment group is composed by the municipalities that received SP between the third quarter of 2004 and the fourth quarter of 2005, while the control group is composed of those municipalities that received the program in 2006. This implies that I am effectively comparing municipalities that have been exposed to SP between (4 and 9 quarters) with those that have been exposed between (0 and 3).

In order to show that the results are not being driven by changes in the control group, the specification on column 3, restricts control group municipalities to those that received SP in fourth quarter of 2006 (i.e., those that have never been exposed to SP). Additionally, the specification in column 4, narrows down the period of analysis (first to fourth quarter of 2005) and drops treatment municipalities that have no with-in variation in this time frame, that is to say, I am now comparing treatment municipalities exposed to SP between (3 and 5 quarters) to the full control group (never exposed given the time frame). Reassuringly, both coefficients are of a similar magnitude to those derived under the benchmark specification, albeit noisier given the smaller sample size. Note additionally, that the specification on column 4, allows me to also rule out that the effect of SP is being driven by treatment municipalities with very prolonged exposure (6 to 9 quarters).

[22]The definition of region that is used comes from the work of Aroca et al. (2005) who identifies three regions with very distinct economic performance. The first region is composed by those states that have a common border with the United States (Baja California, Chihuahua, Coahuila, Nuevo Leon, Sonora and Tamupilas), the second roughly correspond to states in the south (Chiapas, Guerrero, Oaxaca, Puebla, Tlaxcala, Veracruz, Yucatan and Quintana Roo) while the third is made up of the remaining central states.

[23]This specification requires the estimation of 134 additional coefficients.

[24]This specification includes time averages of each regressor instead of municipal fixed effects.

The fifth group of robustness checks deals with the definition of the dependent variable. More precisely, in column 5, I take into account that coverage to contributory social security can be gained indirectly (e.g. through a spouse that has formal employment), by using a broader definition in which any employed member of a household where at least one member has access to contributory social security will be counted as formally employed.[25] As can be seen in column 5, while the coefficient is somewhat larger it is statistically indistinguishable from the one derived from the benchmark model.

Additionally, in order to test whether the loss in information due to aggregation at the municipal level has caused any problems, column 6, presents an analogous individual level specification. The dependent variable is a dummy that take the value of one when an individual is formally employed, the regressors include: a SP indicator variable, municipal and quarter fixed effects, and the same set of controls (defined at the individual level whenever possible). The model is estimated by OLS using sample weights adjusted to give equal weight to every municipality. As can be seen the estimated program effect is statistically significant and slightly larger than the benchmark specification. When unadjusted sample weights are used a similar, albeit smaller coefficient (0.57 percentage points, p-val 0.126) is recovered.

Another possible source of concern is that the (LFS) was not designed to be representative of each municipality, to address this issue the specification on column 7, restricts the sample to municipalities on the top 2/3 of the ratio of (LFS) interviews to total employed population derived from the 2000 census. Alternatively, the specification on column 8, assumes that smaller municipalities are noisier and restricts the sample to those with a population larger than 2500 inhabitants as determined by the 2000 census. In both cases, while the estimates are noisier given the smaller sample size, their magnitude is very similar to that of the benchmark specification.

Finally, In order to bolster the case for a causal interpretation of the estimates of program impact, columns 9 and 10 present a falsification exercise were information on the pre-program period (fourth quarter of 2003 to fourth quarter of 2001) is used to construct a placebo. More precisely, the specification on column 9, tests the effect of SP on placebo data while, the specification in column 10, replicates the benchmark specification restricting the sample to those municipalities that can be observed at both points in time (roughly 1/3 of the sample).

Reassuringly, the placebo specification has the wrong expected sign, while the coefficient in column 10 has the correct sign and is of a similar magnitude as the benchmark specification.[26] The two coefficients are not statistically different from each other at conventional levels (pval-0.164) but this result has to be interpreted taking into account that sample size is much smaller in this exercise.

[25] These members of the household who indirectly receive access, must also comply with the eligibility criteria set by contributory social security

[26] The two specifications in column 9 and 10 were jointly estimated using SURE, then a χ^2 test was performed, the null hypothesis is that the two coefficients are equal.

Table 3: Robustness Checks

	Controls in Steps				Additional Controls						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
VARIABLES	F/E	F/E	F/E	F/E	F/E	F/E	F/E	F/E	F/E	F/E	F/E
Lag 3 SP (=1)	-0.00462	-0.00625*	-0.00792**	-0.00777**	-0.00790*	-0.00802**	-0.00675*	-0.00655**	-0.00709*	-0.00579	-0.00778**
	(0.00399)	(0.00365)	(0.00355)	(0.00386)	(0.00457)	(0.00386)	(0.00376)	(0.00319)	(0.00406)	(0.00441)	(0.00386)
	0.247	0.0876	0.0261	0.0447	0.0847	0.0380	0.0732	0.0403	0.0814	0.190	0.0446
Observations	4,176	4,176	4,174	3,698	2,748	3,698	3,698	3,698	3,698	3,698	3,698
Number of municipalities	608	608	608	561	491	561	561	561	561	561	561
Municipal & Quarter FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls I	.	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls II	.	.	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls III	.	.	.	✓	✓	✓	✓	✓	✓	✓	✓
L. Controls I-III	.	.	.	.	✓	.	.	.	.	.	.
Controls IV	.	.	.	.	.	✓	.	.	.	.	.
Controls V	.	.	.	.	.	.	✓	.	.	.	.
Controls VI	.	.	.	.	.	.	.	✓	.	.	.
Controls VII	.	.	.	.	.	.	.	.	.	.	✓
Region*Quarter FE	.	.	.	.	.	.	.	.	✓	.	.
State*Quarter FE	.	.	.	.	.	.	.	.	.	✓	.

Note: OLS estimation. Robust standard errors clustered at the municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is the ratio of formal to total employment. SP is a dummy variable that takes the value of 1 once a SP facility is open and registers at least 10 affiliations in a given municipality and quarter. Controls I: employed population shares of age (5 groups), educational attainment (4 groups), urban and gender. Controls II: employed population shares of labor income (7 groups) as well as the 10th 50th and 90th percentile of labor income in a give municipality. Controls III: total and infrastructure expenditure per capita, as well as a set of municipal and state level dummies that take the value of 1 when one of the 3 main political parties or their alliances (8 groups in total) holds a Mayor or Governor post. Controls IV: Medical personnel per capita provided by contributory social security. Controls V: Medical units per capita provided by contributory social security. Controls VI: employed population shares of industry (12 groups) and size of firms (7 groups) Controls VIi: An index of electricity consumption at the state level.

Table 4: Robustness Checks II

	Balanced Panel	PFP (QMLE)	Robust Control Group		HH Definition	Individual Lvl.	Representativeness MunAvg.		Placebo	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	F/E	F/E	F/E	F/E	F/E	F/E	F/E	F/E	Placebo	F/E
Lag 3 SP(=1)	-0.00716*	-0.00749*	-0.00601	-0.00826	-0.00982*	-0.00931**	-0.00752	-0.00715*	0.00364	-0.00473
	(0.00400)	[.0043]	(0.00404)	(0.00628)	(0.00521)	(0.00374)	(0.00474)	(0.00392)	(0.00387)	(0.00493)
	0.0742	0.0795	0.137	0.189	0.0601	0.0132	0.113	0.0686	0.346	0.337
Observations	3,451	3,451	3,372	1,048	3,698	463,673	2,412	3,602	1,693	1,693
Clusters	493	493	513	264	561	561	411	544	325	325
χ^2										1.937
Prob > χ^2										0.164

Note: OLS estimation unless stated otherwise. Robust standard errors clustered at the municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable is the ratio of formal to total employment. SP is a dummy variable that takes the value of 1 once a SP facility is open and registers at least 10 affiliations in a given municipality and quarter. All regression include municipal and quarter fixed effects as well as a set of time varying controls organized in three categories. Controls I, include: employed population shares of age (5 groups), educational attainment (4 groups), urban and gender. Controls II, include: employed population shares of labor income (7 groups) as well as the 10th 50th and 90th percentile of labor income in a give municipality. Controls III, include: total and infrastructure expenditure per capita, as well as a set of municipal and state level dummies that take the value of 1 when one of the 3 main political parties or their alliances (8 groups in total) holds a Mayor or Governor post. Column 2, presents a pooled fractional probit model estimated using quasi-maximum likelihood, this specification includes time averages of each regressor instead of municipal fixed effects, the coefficient corresponds to the average partial effect of SP, bootstrap standard errors in brackets. Column 3, restricts the control group to those municipalities that have not been exposed to SP at any point in time. In column 4, The period of analysis spans from the first quarter of 2005 till the fourth quarter of 2005, accordingly treatment group municipalities with no with-in variation are dropped from the sample. In column 5, The ratio of formal to total employed is calculated assuming that all employed members of a household are covered by contributory social security when at least one of the members declares to have access. Column 6, estimates the analogous individual level model, the regression is estimated using sample weights adjusted to give equal weight to every municipality. Column 7, Restricts the sample to municipalities in the top 2/3 of the ratio of interviews to total employment. Column 8, Restricts the sample to municipalities with population over 2500. The placebo specification columns 9 and 10, restricts the sample to municipalities observed at both points in time, the ratio of formal to total employment in column 9 is calculated using pre-program observations (03q4-01q4).

5 Accounting for Spillover Effects

Another area of potential concern regarding the estimates of program impact is related to the relevance of the political boundaries that have been used to designate the treatment and control groups. Intuitively, given the type of intervention and the characteristics of the outcome variable that is the focus of this paper, there are at least three mechanisms through which the introduction of SP in a given municipality could be capable of affecting neighboring municipalities.

The first is related to the extent to which the services of SP were not exclusive to the municipality where the program was introduced. Although, proof of residence was listed as a document required for inscription, the eligibility conditions of SP did not explicitly state that services were limited to residents. Moreover, even if local officials informally decided to implement such a rule, it is unlikely that they could have enforced it, as not only are program guidelines particularly lenient with regards to proof of residence,[27] but as previously mentioned, a large fraction of inscriptions took place through collective affiliations for whom document verification was particularly lax. Thus it cannot yet be ruled out that control group municipalities may have accessed SP services leading me to underestimate the impact of the program.

Additionally, a second effect could be operating through the labor market, here the key idea, as discussed by Petrongolo and Manning (2011), is that local labor markets are not collections of non overlapping administrative units. Therefore, it might very well be the case, either that the introduction of SP affects a local labor market that extends beyond the boundaries of a municipality or that the impact of the program ripples across overlapping labor markets that extend through various municipalities.

For example, it is possible to think of a scenario where residents of a municipality that is the source of the shock (i.e., a SP office is opened) are encouraged by the introduction of the program to reduce their search efforts for formal jobs in both their place of residence as well as in neighboring municipalities. The resulting reduction in competition for formal jobs, all else equal, would make it easier for residents of these neighboring areas to find formal jobs, during the time that it takes for this effect to dissipate (i.e., as candidates from areas that farther away are drawn into labor markets that have become more advantageous).

Another more traditional channel is related to the dissemination of information that makes potential users aware of the introduction of SP, or alternatively that helps them gauge the quality of the new services. As in all other cases, assessing the direction of the bias is difficult because indirect exposure to SP is likely to affect both treatment and control municipalities. However, in this case I am particularly concerned with a scenario in which information over the future introduction of SP makes informal jobs relatively more attractive in control group municipalities, as this would imply that the estimates of section 4 may be underestimating the impact of SP.

Throughout this section my primary focus will not be to disentangle the different channels through which the spillovers effects might be operating, but rather to address these concerns by taking advantage of the idea that the intensity of indirect exposure to SP is likely to be related to the variation in the spatial coverage of the program. More specifically, in what follows I will use two proxies of indirect exposure to SP, in order to explore whether the estimates derived in section 4 are biased as a result of not accounting for spillover effects, as well as to investigate whether the total impact of SP varies in

[27] According to the rules of operation, any errors in the policy, can lead to the termination of services, with the exception of errors related to the proof of residency, in fact families can enroll without presenting proof of residence for a period of up to 90 days.

relation to the degree of indirect exposure.

The first proxy used in the analysis, is road distance from the centroid (i.e., the geometrical center) of the largest urban area in a municipality to the centroid of the nearest urban center of a municipality where SP is being offered in a given quarter.[28] The intuition behind this proxy is that as the program becomes denser and distance to nearest municipality offering SP is reduced, the more likely it becomes that control group municipalities could have had access to SP. Similarly, this reduction in transactional costs (e.g. transportation costs) should increase the degree of awareness over the introduction and benefits of SP, in both treatment and control municipalities. While decreasing[29] the intensity of formal job search performed in control group municipalities by residents of municipalities that have direct access to the program.

That said, since I cannot fully rule out that distance to the nearest municipality offering SP might be picking up factors other than the spillover effects previously outlined. I will additionally employ the population weighted share of neighboring municipalities[30] with direct access to SP, as a second proxy. In this case as the program becomes denser and the share of neighbors with direct access increases, I expect control municipalities to have higher chances of accessing SP services. Furthermore, since the awareness of individuals over the availability of SP or their perception of the quality of the services is likely to be a function of the experience of the people with whom they interact, it is reasonable to expect this proxy to do a good job at capturing this effect in both the control and the treatment group. As before, the intensity of formal job search performed by residents of municipalities with direct access to SP is likely to decrease in control municipalities, at least in short run.

To introduce this proxies into the analysis I modify the difference-in-differences specification presented in equation 1. In the case of distance to the nearest municipality offering SP, equation 2 below, introduces three additional terms: First, $D_{m,t-\lambda}$ which denotes the natural logarithm[31] of road distance to the nearest municipality offering SP for municipality (m) at quarter $(t-\lambda)$, where λ represents the number of lags of indirect exposure to SP $\{\lambda \in \mathbb{Z} | 1 \leqslant x \leqslant 4\}$. Note that this lag is independent from lag (L) found on $SP_{m,t-L}$, this additional flexibility is introduced into the model in order to allow the intensity of indirect exposure to vary over time and/or to account for the possibility that the impact of different spillover effects may become relevant at different points in time. Second, an interaction term between $SP_{m,t-L}$ and $D_{m,t-\lambda}$. Third, an interaction between $SP_{m,t-L}$ and the term UD_m, which denotes the natural logarithm of road distance to the nearest municipality, this latter term is introduced in order to control for the fact that $D_{m,t-\lambda}$ could potentially pick up the heterogeneity in the impact of the program in relation to other factors, for example, the area of municipalities.

$$\begin{aligned}\frac{F_{mt}}{E_{mt}} &= \alpha + \beta_t + \beta_m + \beta_1 SP_{m,t-L} + \beta_2 D_{m,t-\lambda} + \beta_3 SP_{m,t-L} * D_{m,t-\lambda} + \beta_4 SP_{m,t-L} * UD_m \\ &\quad + \omega X_{mt} + \epsilon_{mt}, \ \ \forall\, m \in M, \ \ t \in T \end{aligned} \tag{2}$$

In analogous manner the specification that uses the second proxy, equation 3, includes instead the terms: $SN_{m,t-\lambda}$ which denotes the population weighted share of neighbors with direct access to SP

[28]The largest urban area is defined by population taken from the 2000 census.

[29]It is also possible competition for formal jobs increases as candidates are drawn from municipalities that are farther away from the source.

[30]Neighboring municipalities are defined as those that have a common border. Note that the influence of the neighboring municipality does not depend on the length of the border but rather on its population, for further details about the calculation please refer to the dataset appendix.

[31]This transformation is performed in order to minimize the impact of outliers.

for municipality (m) at quarter (t-λ). The interaction term between $SP_{m,t-L}$ and $SN_{m,t-\lambda}$ and the interaction term between $SP_{m,t-L}$ and the term $NPOP_m$, which denotes the natural logarithm of the total population of neighboring municipalities, this latter term is included in order to account for the fact that the term $SN_{m,t-\lambda}$ could potentially capture the heterogeneity in the impact of the program in relation to factors, such as the size of the labor market of neighboring municipalities.

$$\frac{F_{mt}}{E_{mt}} = \alpha + \beta_t + \beta_m + \beta_1 SP_{m,t-L} + \beta_2 SN_{m,t-\lambda} + \beta_3 SP_{m,t-L} * SN_{m,t-\lambda} + \beta_4 SP_{m,t-L} * NPOP_m + \omega X_{mt} + \epsilon_{mt}, \ \forall \ m \in M, \ t \in T \quad (3)$$

The other variables in equations 2 and 3, as well as the sample of municipalities included (M) and the time frame (T) are the same as those of section 4. The intuition behind this new specifications, is that of assessing the impact of SP on formal employment when the total impact of the program is allowed to depend on both direct and indirect exposure to SP. Accordingly, the focus of this paper shifts toward the marginal effect of SP, which in the case of the first proxy, is given by the expression $\beta_1 + \beta_3 * D_{m,t-\lambda} + \beta_4 * UD_m$ or equivalently by β_1 once the variables $D_{m,t-\lambda}$ and UD_m are centered at a distance that is relevant for the analysis.

Since I would like to derive standard errors under various assumptions this latter alternative will prove to be far more tractable. In sum, the coefficients of interest in this section will be β_1 which can now be interpreted as the impact of SP treatment conditional on the degree of indirect exposure (i.e., the value at which the interaction terms are centered), and the coefficient $D_{m,t-\lambda}$ in equation 2 and $SN_{m,t-\lambda}$ in equation 3, as they allow me to gauge whether indirect exposure to SP mattered in control municipalities. Finally, it is important to note, that this analysis is based on an identification assumption that is far more stringent, namely, that pre-intervention time trends between treatment and control municipalities are parallel conditional on the proxy that is being used.

Given that the results of section 4 suggest that the largest reduction in formal employment is associated with an exposure of at least three quarters to SP, table 5 below, explores whether these estimate of program impact may have been considerably biased as a result of spillover effects. This is done by comparing the coefficient of the benchmark model with the ones derived from equations 2 and 3 when the interaction terms are centered at their respective averages. Each panel tests a different proxy of indirect exposure, column 1 reproduces the result from section 4, while columns 2 to 5 investigate the impact for different lags (λ) of indirect exposure to SP.

The main findings, are that regardless of the proxy used, the coefficients of SP impact at average spillover levels are of a very similar magnitude to that of the benchmark model, and that in all cases these coefficients are statistically undistinguishable[32] from the benchmark coefficient.[33] Moreover, consistent with the idea that the results of section 4 are not overestimating the impact of SP, I am also unable to provide any evidence of indirect exposure to SP having an effect on control group municipalities. In order to rule out that these results are specific to municipalities that have been directly exposed to SP for at least three quarters, tables 15 and 16 of the online appendix, replicate this analysis for each specification discussed in section 4. The results are analogous in all cases.

[32]This is accomplished by jointly estimating the different specifications using SUR and then performing χ^2 tests where the null hypothesis is that the coefficients are equal.

[33]Additionally testing was carried out using both of this proxies simultaneously, this is done by including in equation 3 the triple interaction term $\beta_3 SP_{m,t-L} * SN_{m,t-\lambda} * D_{m,t-\lambda}$ in addition to all the secondary terms that are required. The results strongly mirror those reported here and are available upon request.

Table 5: Estimates of program impact at average levels of indirect exposure.

VARIABLES	(1) F/E	(2) F/E ($\lambda = 1$)	(3) F/E ($\lambda = 2$)	(4) F/E ($\lambda = 3$)	(5) F/E F($\lambda = 4$)
Panel 1					
Lag 3 SP(=1)	-0.00777**	-0.00863	-0.00553	-0.00598	-0.00657
	(0.00384)	(0.00652)	(0.00694)	(0.00701)	(0.00417)
$D_{m,t-\lambda}$ (C at mean)		0.00215	0.00144	0.00360	0.00251
		(0.00348)	(0.00309)	(0.00294)	(0.00328)
$SP_{m,t-3}$ x $D_{m,t-\lambda}$ (C at mean)		-0.00328	0.000617	-0.00248	-0.00211
		(0.00832)	(0.00757)	(0.00668)	(0.00369)
$SP_{m,t-3}$ x UD_m (C at mean)		0.00768	0.00408	0.00603	0.00592
		(0.00881)	(0.00814)	(0.00701)	(0.00396)
Observations	3,698	3,698	3,698	3,698	3,698
Clusters	561	561	561	561	561
χ^2 eq(1)-eq(λ+1)		0.0267	0.150	0.0946	0.473
Prob > χ^2		0.870	0.698	0.758	0.492
Mean $D_{m,\ t-\lambda}$		42.97	51.08	61.47	72.18
Panel 2					
Lag 3 SP(=1)	-0.00777**	-0.00619	-0.00563	-0.00583	-0.00520
	(0.00384)	(0.00523)	(0.00514)	(0.00533)	(0.00440)
$SN_{m,t-\lambda}$ (C at mean)		-0.00221	0.00335	0.000514	-0.00123
		(0.00726)	(0.00677)	(0.00705)	(0.00885)
$SP_{m,t-3}$ x $SN_{m,t-\lambda}$ (C at mean)		0.00196	-0.00181	-8.23e-05	-0.00127
		(0.0124)	(0.0110)	(0.0110)	(0.00903)
$SP_{m,t-3}$ x $NPOP_m$ (C at mean)		0.00346	0.00355	0.00351	0.00351
		(0.00333)	(0.00335)	(0.00336)	(0.00338)
Observations	3,698	3,698	3,698	3,698	3,698
Clusters	561	561	561	561	561
χ^2 eq(1)-eq(λ+1)		0.179	0.325	0.229	1.201
Prob > χ^2		0.672	0.569	0.632	0.273
Mean $SN_{m,\ t-\lambda}$		0.604	0.527	0.438	0.349

Note: SUR estimation, robust standard errors clustered at the municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include municipal and quarter fixed effects as well as time varying controls I-III as described in section 4. The lag 3 of SP coefficient is the marginal effect of the program at average levels of indirect exposure. $D_{m,t-\lambda}$ is the natural logarithm of road distance to the nearest municipality offering SP. UD_m, is natural logarithm of road distance to the nearest municipality. $SN_{m,t-\lambda}$ is the population weighted share of neighbors with direct access to SP. $NPOP_m$, is the natural logarithm of the population of neighboring municipalities. In all cases these covariates have been centered at their respective sample averages.

Turning to the question of whether the effect of the program varies in relation to the degree of indirect exposure, is particularly interesting because it allows me to further bound the effect of SP on formal employment. This is done by estimating the impact of SP on municipalities that had direct access to SP and that additionally were indirectly exposed at very high intensities. More specifically, since it is reasonable to conjecture that the intensity of spillover effects is inversely related to distance to the nearest municipality offering SP and directly related with the population weighted share of neighbors,[34] it is possible to derive meaningful upper-bound estimates of program effect by examining the cases in which the marginal effect is calculated at the smallest $D_{m,t}$ and the largest $SN_{m,t}$ observed in the sample.

However, before proceeding, assumptions on the timing of direct and indirect exposure to SP are also required. Although I suspected that indirect exposure is likely to be more relevant when it precedes direct exposure, without strong priors, the most generous assumptions for the calculation of an upper-bound would be to test every possible lag combination (direct and indirect, L, λ).[35] This of course implies testing a large number of hypotheses and therefore raises the issue of accounting for the false discovery rate (FDR). In order to address this issue I will follow Anderson (2008) and calculate both FDR and "sharpened" FDR adjusted p-values, from here on q-values, as described in Benjamini and Hochberg (1995) and in Benjamini et al. (2006).

Table 6 below, calculates the marginal effect of SP for every possible lag combination when $D_{m,t}$ and UD_m are centered at 10km. Each entry is from a separate OLS regression of equation 2, the panels vary the timing of direct exposure to SP (lag L), while the columns correspond to a different lag of indirect exposure to SP (lag λ). The main finding is presented in panel 3 column 4, where the largest and most sharply estimated total program effect is found (1.5 percentage points), q-values 0.12 and 0.14.[36] Moreover, the results of table 6 also seem to suggest that prolonged indirect exposure to SP matters most in municipalities that have been directly exposed to SP for shorter periods of time, however, in all cases I am unable to reject the null hypothesis of no program effect when controlling FDR at q = 0.10.

In analogous manner, table 17 in the online appendix, calculates the marginal effect of SP when $SN_{m,t}$ is centered at a share of 0.85 and $NPOP_m$ at the median of the sample.[37] Consistent with the previous set of results, the point estimate of panel 3 column 4, suggests a program effect in the order of (1.4 percentage points), q-values 0.15 and 0.17.[38] While the estimates of panels (1-3) in columns 4 and 5 lend further support to the idea that prolonged exposure matters most when it precedes direct exposure,[39] as in the previous case no result rejects when controlling FDR at q = 0.10.

Finally, in order to test the sensitivity of these upper-bounds to the degree of indirect exposure. Table 18 and 19 in the online appendix, calculate the marginal effect of SP holding the choice of lags constant while allowing the proxies of indirect exposure to vary ($D_{m,t}$ 7-100 km, $SN_{m,t}$ 0.95-0.25). The main finding is that in both cases the slightly larger program effects are relevant for less than 1/5 of the municipal-quarter observations and that weak indirect exposure (i.e., high $D_{m,t}$, low $SN_{m,t}$) is associated with s smaller, but never statistically significant positive effects of SP on formal employment.

[34] This is not necessarily the case for the labor market spillover effect.

[35] There are 25 possible combinations of lags for each proxy.

[36] This result is relevant for 255 municipal quarter-observations, 225 in treatment and 30 in control.

[37] The median is used because a few municipalities have very large neighbors, that said, centering with the mean of total neighbors population produces similar results.

[38] This result is relevant for 536 municipal quarter-observations, 505 in treatment and 31 in control.

[39] The results on panel 1 columns 3 and 4 estimates a program effects as large as 2.3 percentage points, however, they are not emphasized because there are only 26 municipal-quarter observations for whom this result is relevant.

Table 6: Upper-bounds of program impact
(Road distance to nearest municipality offering SP)

VARIABLES	(1) F/E ($\lambda = 0$)	(2) F/E ($\lambda = 1$)	(3) F/E ($\lambda = 2$)	(4) F/E 3 ($\lambda = 3$)	(5) F/E F($\lambda = 4$)
Panel 1					
SP (=1)	0.00278	-0.00275	0.000682	-0.00255	-0.00374
	(0.00659)	(0.00838)	(0.00826)	(0.00843)	(0.00856)
	0.674	0.743	0.934	0.762	0.662
Observations	1,501	1,501	1,501	1,501	1,501
Number of entmunid	379	379	379	379	379
FDR q-values BH95	0.766	0.795	0.935	0.795	0.766
FDR q-values BKY06	0.582	0.586	0.698	0.586	0.582
Panel 2					
Lag SP (=1)	-0.00797	-0.00647	-0.00542	-0.0149*	-0.00971
	(0.00631)	(0.00688)	(0.00765)	(0.00815)	(0.00881)
	0.207	0.348	0.479	0.0689	0.271
Observations	1,943	1,943	1,943	1,943	1,943
Number of entmunid	411	411	411	411	411
FDR q-values BH95	0.370	0.458	0.599	0.192	0.418
FDR q-values BKY06	0.285	0.345	0.453	0.150	0.335
Panel 3					
Lag 2 SP (=1)	-0.00840	-0.00790	-0.0102*	-0.0150**	-0.0145**
	(0.00526)	(0.00512)	(0.00600)	(0.00668)	(0.00695)
	0.111	0.123	0.0881	0.0255	0.0372
Observations	2,767	2,767	2,767	2,767	2,767
Number of entmunid	490	490	490	490	490
FDR q-values BH95	0.253	0.258	0.221	0.121	0.121
FDR q-values BKY06	0.207	0.212	0.177	0.137	0.137
Panel 4					
Lag 3 SP (=1)	-0.0101**	-0.00932**	-0.00944**	-0.00577	-0.00661
	(0.00430)	(0.00431)	(0.00434)	(0.00527)	(0.00616)
	0.0190	0.0311	0.0300	0.274	0.284
Observations	3,688	3,688	3,688	3,688	3,688
Number of entmunid	559	559	559	559	559
FDR q-values BH95	0.121	0.121	0.121	0.418	0.418
FDR q-values BKY06	0.137	0.137	0.137	0.335	0.335
Panel 5					
Lag 4 SP (=1)	-0.00924**	-0.00984**	-0.00896**	-0.00582	-0.00519
	(0.00433)	(0.00434)	(0.00432)	(0.00433)	(0.00503)
	0.0333	0.0237	0.0384	0.179	0.303
Observations	4,285	4,285	4,285	4,285	4,285
Number of entmunid	571	571	571	571	571
FDR q-values BH95	0.121	0.121	0.121	0.345	0.421
FDR q-values BKY06	0.137	0.137	0.137	0.283	0.337

Note: Each entry is from a separate OLS estimation of equation 2. Robust standard errors clustered at the municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include municipal and quarter fixed effects as well as time varying controls I-III as described in section 4. In all cases $D_{m,t-\lambda}$ and UD_m have been centered at 10 km. Two types of p-values adjusted to control for the false discovery rate are presented in the last two rows of each panel.

6 Conclusions

In 2002 the Mexican government began implementing Seguro Popular, an ambitious program that was aimed at rapidly extending health coverage to roughly 50% of the population that had so far remained uninsured. In the context of the Mexican health care system, the introduction of Seguro Popular amounted to a large expansion of non-contributory health care services which run in parallel to the countries contributory social security system. Despite the expected improvements in health outcomes of millions of uninsured workers, and the well documented reduction in catastrophic health care expenditures, it has often being argued that the downside of this type of intervention occurs in the labor market, where it is expected to encourage the reallocation of workers away from the formal sector.

To evaluate the cost side of the trade-off between the large scale provision of free medical health services to the uninsured and formal employment, this paper uses a difference-in-differences design that takes advantage of the variation in the provision of Seguro Popular services, created by the progressive roll out of the program across municipalities.

The main findings are that contemporaneous exposure to the program has no impact on the ratio of formal to total employment while exposure to the program for at least three quarters is associated with a small but statistically significant reduction of 0.78 percentage points in the ratio of formal to total employment. These results are of particular interest as a number of factors support the causal interpretation of this relationship. First, pre-intervention trends between the treatment and the control group are parallel. Second, time varying factors likely to confound the estimates of program impact are controlled for in all specifications. Third, the falsification exercise fails to provide any evidence of program impact when the placebo pre-intervention period is used. Fourth, it has been shown that estimates of program impact are not underestimated as a result of spillover effects.

Additionally, in order to further bound the impact of Seguro Popular, this paper looks at the effect of the program in municipalities that had direct access to SP and that additionally were indirectly exposed high intensities. Interestingly, in spite of the strong assumptions underlying this estimates[40] they are only moderately larger with program impact peaking at 1.5 or 1.4 percentage points depending on the proxy that is used.

On the whole, the results of this paper indicate that Seguro Popular had a small distortionary effect on the labor market. However, it must be emphasized that this does not imply that the program is not welfare improving as the costs in the labor market must be balanced against the benefits that Seguro Popular creates in other dimensions of welfare.

Work currently underway on labor market transitions, will attempt to provide and even clearer narrative of the welfare implications of Seguro Popular in the labor market, by providing qualifiers for these findings in at least two areas. First, it will establish whether the reduction in formal employment is predominantly due to entry or retention of new informal workers or whether it is being driven by current workers switching between sectors. Second, since the creation or loss of certain types of jobs is likely to have different effects on outcomes such as tax collection, it is important to characterize the type of formal jobs that are being lost or informal jobs that are being created as a result of the program.

In terms of the broader policy debate, the findings of this paper are mid-way between those of other

[40] These estimates not only allowed for the strongest degree of indirect exposure observed in the sample but they additionally use the most favorable assumptions regarding the combination of lags for direct and indirect exposure to the program.

empirical work. Thus independently of the type of assumptions that are made, there is no evidence of the largest expansion of non-contributory health services in the Americas having any substantial effect on formal employment. Thereby raising the question of what are the factors that have contributed to limiting its potentially negative effect on the labor market.

In this respect it has been previously suggested that the improvements in the quality of SP services over the status quo were too small to encourage the creation or retention of informal jobs, which seems unlikely given the amount of federal resources that the program was mobilizing. And that Seguro Popular services only represent a small proportion of the overall non-monetary benefits associated with informal jobs, which is something that is hard to gauge empirically.

In addition to these arguments, the findings of this paper are broadly in line with two complementary explanations that merit further research. The First, is that individuals may have a low valuation of Seguro Popular services regardless of their actual quality, both because the bulk of the benefits which are likely to occur in the future may be strongly discounted, and because the factors that drive perceived improvements in the quality of health services, such as waiting times or the time spent with the physician, are hard to gauge by potential new users, who are only likely to react to the program as they learn from their own experience and that of others. This intuition is consistent with the findings of lagged program impact, as well as with stronger effects for treatment municipalities whose neighbors had also been exposed to the program.

A second complementary explanation comes from the recent work of Duflo (2012) and Devoto et al. (2011), who argue that even small barriers may be capable of drastically reducing program take-up. Since Seguro Popular is not a default option but a choice that was not accompanied with an appropriate intervention to raise awareness (e.g. in the case of Chile Solidario social workers have been shown to be instrumental in encouraging program take-up Galasso (2011)). It may be the case that the registration procedure and the requirement to visit an affiliation module worked as a strong deterrent. Because distance and the transactional costs of accessing the services are strongly related, the finding of larger program effects in municipalities that were in close proximity to other municipalities that offered Seguro Popular is consistent with the idea that administrative barriers may have played a role in limiting program take-up.

Finally, it is worth highlighting that a strong case for the external validity of these findings can be made on the grounds that the effect of this type of intervention are likely to be smaller in countries where the degree of labor market segmentation is higher than that of Mexico. Thus, while it may still be inefficient to have a two tier social security system for a number of reasons. Policy makers on the road to universal coverage would be ill advised not to pursue a temporary expansion of non-contributory health services because of its potentially negative effects on formal employment.

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