

The Short Term Causal Effects of Seguro Popular on Formal Employment

Nelly Aguilera¹

ANY COMMENTS WELCOME, DO NOT QUOTE

Abstract

Theoretical models conjuncture that in countries where formal and informal labor markets coexist, as is common in many less developed economies, publicly provided health insurance may promote informality. Still, the little empirical evidence on the topic is limited and inconclusive. This paper assesses the causal effect of *Seguro Popular* on formal employment in Mexico. It does so taking advantage of a randomized experiment and longitudinal individual administrative data of participation in formal labor markets. Results show that in the short run the mere presence of *Seguro Popular* has no effects on formal employment nor in the probability of leaving or joining the formal sector.

JEL codes:

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

Introduction

With the introduction of publicly financed health insurance programs in many developing countries there is a concern regarding whether they may reduce the already small formal labor markets (see Levy 2008). The introduction of health insurance financed by general taxes may reduce the valuation that workers attach to health services provided under social-security-type schemes (i.e., based on employer and employee contributions), and thus make them more prone to opt-out of the formal labor market, which is defined as that in which firms pay social security contributions. While

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there is strong theoretical support for these fears (see, for example, Marrufo (2002)), empirical evidence of this phenomenon is still limited.

Barros (2009), using the variation in program intensity across time and space (at the state level) induced by the roll out of the program, found that the program did not have any effect on labor force participation, earnings or formal employment. Azuara and Marinescu (2010), using a similar strategy but at the municipality level, found small effects of *Seguro Popular* on informality, in the order of 1%, and not statistically significant over the whole employed population, although it may have some effects in the less educated (workers with less than nine years of schooling), married workers with children or workers over 34 years old. The authors found no effects on transition probabilities from the formal to the informal sector or vice versa nor on wages.

On the contrary, Bosch and Campos-Vázquez (2010) using a similar identification strategy than Barros (2009) but at the municipality level found *Seguro Popular* negatively affected the creation of formal employment, especially in small-and medium-size firms. According to the authors, had the program not been in place 3.8% more employers, and 2.4% more employees could have been added to the total stocks of employers and employees. Extensive research reveals these to be the only studies to have analyzed the topic.

This paper provides evidence of the causal effect of *Seguro Popular* on the formal employment and probability of entering or leaving the formal market (at the individual level). It takes advantage of the randomized experiment implemented for the evaluation of the impact of *Seguro Popular* on health-related indicators and out-of-pocket spending, and also uses an administrative database that records longitudinal, individual-level information of workers' participation in formal labor markets. As such, this is the

first study on the topic based on a randomized experiment and utilizing longitudinal administrative information at the individual level.

Results suggest that *Seguro Popular* has no impact in the short run on formal employment, or probability of entering or leaving the formal labor market, even in the group that is more sensitive to health services, young women.

The paper is organized as follows: Section II describes *Seguro Popular*; Section III describes the experiment, the databases and the empirical strategy; Section IV presents the results; and, Section V concludes.

I. *Seguro Popular* and social security based health insurance

The Mexican healthcare system was reformed in 2004² with the introduction of the Sistema de Protección Social en Salud (SPSS), also known as *Seguro Popular*. *Seguro Popular* is an insurance scheme that entitles enrollees access to medical and surgical services, as well as drugs and hospitalization, free of cost at the point of service (i.e., there are no copayments and users of public health facilities do not have to pay for services included in the plan).³ The interventions covered by the scheme represent 95% of the disease burden in Mexico (King *et al* 2007). The only requirement to be affiliated with *Seguro Popular* is that persons should not be affiliated with any health insurance provided by social security agencies, the Mexican Institute of Social Security (IMSS), which is the scheme for workers of private companies, or the social security agencies (ISSSTE) for workers of public entities (federal and state workers).

² Formally, *Seguro Popular* started January 1, 2004. In 2002 and 2003 a small pilot of the program was implemented.

³ Before *Seguro Popular* and for the services not covered by *Seguro Popular*, users attending public providers had to pay for services at a subsidized price depending on their socioeconomic condition, the poorest citizens not paying anything. The charges are called “Cuotas de Recuperación”.

Seguro Popular is financed by: i) means-tested contributions of the insured, with the two poorest deciles paying nothing (called *regimen no contributivo*)⁴; ii) the federal government, which pays a social quota equal of approximately \$230 USD annually per family enrolled (as it does to the IMSS and ISSSTE) plus a contribution per each family equal on average to 1.5 times the social quota (it is higher for poorer states and lower for richer states); and, iii) state governments that contribute for each family with at least the equivalent of an extra 0.5 of the social quota. These numbers imply roughly that per-household spending in *Seguro Popular* equals approximately \$690 USD per year. Given that fact that 97% of the affiliates are registered in the non-contributive regime (SPSS 2010), basically this program is free.

The IMSS, the largest health insurance in the country, serving approximately half of the population, is financed by the social quota of the federal government, as explained above, by a fixed employer and employee payroll contributions equal to \$340 USD, and by a marginal rate of 1.5% that kicks in at wages above the equivalent of three minimum wages (approximately \$4,970 USD yearly). This means that the average worker (who earns the equivalent of around four minimum wages) generates a payroll tax of \$370 USD per year (including the employer part). Added to the social quota, we can determine that IMSS has around \$600 USD per year per family to provide health insurance. IMSS spending in health per family, though, equals \$985 USD⁵. The difference may be explained by the assumption that there may be more than one member of the family affiliated with the program and that reserves may have been used (IMSS 2010).

One feature of the payroll contributions in Mexico that can influence the impact of *Seguro Popular* in formal employment is how the payments for different insurances are

⁴ Recently the non-contributory regime was extended to the poorest four deciles.

⁵ The calculation refers to the health spending (not monetary benefits, such as the maternity subsidy) of the Diseases and Maternity Insurance including the health spending in pensioners.

bundled. All employers have to pay contributions for pensions, health, social services⁶ and the housing program, without the possibility of paying for just one insurance. This means that payroll contributions (employee and employer) in Mexico equal approximately 26% of taxable wages.

Seguro Popular was implemented gradually with the original objective of reaching universal coverage by 2010, now postponed to 2011. The states' participation — the entities in charge of the affiliation — in the program was voluntary and subject to an agreement with the federal government. Although it was planned that the affiliation process should focus first in the poorest regions subject to minimum infrastructure requirements, in reality logistical⁷ and political considerations played a role (Barros 2008).

The rolling over feature of the implementation of *Seguro Popular* has been used as the identification strategy in studies that assess the effect of the program on health outcomes, out-of-pocket spending and labor market outcomes, as was described in the introduction. The rolling over feature of *Seguro Popular* also allowed the implementation of a randomized experiment to assess its effect on health-related outcomes and out-of-pocket spending. This experiment is used in this report to assess the program's effect in labor market outcomes. The description of the experiment is explained in the next section.

⁶ Childcare centers and social facilities, such as theaters and sport centers.

⁷ Families enrolled in *Oportunidades* were enrolled automatically, and regions easy to be reached were also the focus of enrollment.

II. Description of the randomized experiment and databases

II.1 The randomized experiment⁸

In 2005-2006, a randomized experiment was implemented to evaluate the effect of *Seguro Popular* on individual health, financial protection and system effective coverage. Health clusters were defined as the unit of intervention⁹, and aggregate (at health cluster level), and individual results were provided. Different health clusters within the same participant states were matched on several covariates, and one of them was randomly chosen to receive the intervention while the other remained as the control health cluster. In total, 100 health clusters participated in the study. The treatment was defined as “encouragement to affiliate with *Seguro Popular*” across health clusters, which means that authorities in participating states during the period of late August 2005 to April 2006 encouraged affiliation in treatment health clusters via several forms of advertising — radio, television, loudspeakers from cars, knocking on doors, billboards — by setting up a “service and orientation stand” in the health cluster where citizens could register as affiliates, and by bringing the health clinic or hospital above a specified threshold level of human resources, drugs and other infrastructure. While in the control health cluster, there were no additional promotional or advertising efforts to encourage affiliation, no restrictions were placed to any individual to affiliate at any time in any health cluster, either treated or control.

To measure outcomes, a survey of 38,000 households was applied in August–September 2005 (baseline) and in June–July 2006 (follow up) to families randomly selected within the health clusters. It is important to notice that the survey was applied shortly after the initiated treatment (King et al 2007). To estimate the causal effect

⁸ A detailed explanation of the design of the experiment can be found in Harvard University (2005) and King et al (2007).

⁹ A health cluster consists of a health services unit and the population that correspond to it.

under an “encouragement design”, the evaluation team opted for Bayesian models similar to those used in Hirano et al (2000).

The survey includes several questions regarding demographic and social characteristics (at the household and individual level), household characteristics, household expenditure, *Seguro Popular* and general satisfaction, overall health, risk factors, chronic conditions, utilization and quality of health services, social capital and stress and physical examinations. For our topic of interest, the survey includes four questions regarding the family members’ affiliation status — a question that specifically asked each member of the household “whether he is covered by a mandatory health insurance plan” (IMSS, ISSSTE, PEMEX, SEDENA, MARINA, Other), is the direct or indirect beneficiary, or whether he has a voluntary health plan, such as *Seguro Popular* or *IMSS Oportunidades*¹⁰. Regarding occupational status, each household member was asked where they work, which has been the main occupation in the last 12 months, and why he/she is not receiving a salary if, in a previous question, he/she reported working without a wage. Wage amounts were not queried.

An estimate of the impact of *Seguro Popular* in the formal employment could be determined by comparing the before and after answer to the question regarding affiliation. Unfortunately, in the baseline, this question was not answered almost 80% of the time.

A key element for the evaluation, and for the purposes of this research is the matching and allocation of health clusters in treatment and control groups. The evaluation team proceeded to do the matching and the random assignment after defining the catchment areas of health clusters (the people that belong to the health cluster),

¹⁰ *IMSS Oportunidades* (different from the poverty reduction program *Oportunidades*) is a program, implemented in the 1970s, run by IMSS but financed by general revenues, that offers primary and secondary care to rural poor people in some states of the country. It may represent a “competitor” to *Seguro Popular* since they cater to the same population.

gathering information for the catchment area and filtering the health clusters. Clusters were exactly matched on state and whether it is located in a urban or non urban area, using the following covariates as per the Mahalanobis Distance metric (MD). The pair of clusters were selected interactively as those with the smallest MD in the entire state–urban/non urban area set, then the two clusters were removed, and the process was repeated until all clusters in the set were matched. After final approval by authorities in charge of implementing the experiment, the clusters within each pair were randomly assigned to treatment and control groups. Finally, the evaluation team corroborated that covariates were balanced after analyzing the histograms for the most important variables that determine health outcomes: age and sex.

Table 1
Covariates used in the matching process

Covariate	Rural	Urban
Total population	Yes	Yes
Average education	Yes	Yes
Average room per occupant	Yes	Yes
Percent female	Yes	Yes
Percent between 0 and 4 years old	Yes	Yes
Percent under age 18	Yes	Yes
Percent with and without social security rights	Yes	Yes
Percent over 5 who where living in the same locality on both 1995 and 2000	Yes	
Percent disabled	Yes	Yes
Percent over age 15 and illiterate	Yes	Yes
Percent married	Yes	Yes
Percent Catholic	Yes	Yes
Percent employed	Yes	Yes
Percent employed in the primary/secondary/tertiary sector ^{1/}	Yes	Yes
Percent living in households making less than 2 minimum wages	Yes	Yes
Percent of households headed by women	Yes	Yes
Percent over 5 speaking and indigenous language	Yes	
Weight marginalization index	Yes	Yes
Infrastructure variables (beds, doctors offices, physicians, and nurses) ^{2/}	Yes	Yes
Dummy of affiliation of the health center to <i>Seguro Popular</i>	Yes	
Population within 1km without social security	Yes	
Housing infrastructure variables (condition of the walls, roof, plumbing, electricity, and refrigerator)	Yes	Yes
Percent in <i>Oportunidades</i>	Yes	Yes
Index of health infrastructure within 120 minutes of the cluster ^{3/}	Yes	
Percent over age 60		Yes
Percent over age 65		Yes
Percent in IMSS		Yes
Percent living in the AGEB ^{4/} in both 2005 and 2000		Yes
Altitude		Yes

Notes: 1/ For urban clusters only secondary and tertiary sectors; 2/ For the urban case it includes operating rooms; 3/ Of these, four were partially unobserved, and were imputed; 4/ Areas Geo-Estadísticas Básicas (AGEB) are the smallest geographical areas analyzed by national statistics authorities.

This random assignment of health clusters and the intervention of “encouragement to affiliate with *Seguro Popular*” is used to compare formal labor and transition probabilities across treatment and control health clusters before and after the intervention. Under the assumption that persons respond in treatment and control groups equally to interventions that affect labor market outcomes, the difference in difference regression provides an unbiased estimate of the causal effect.

The matching procedure included several variables that may affect our outcome of interest, formal employment, and indeed some measures of it, which may imply that treatment and control clusters share some labor market characteristics. Nevertheless, given the fact that the matching procedure was not designed to measure labor market outcomes, there may be differences in treatment and control clusters that may bias estimates. To analyze this possibility, baseline characteristics of number of jobs and other covariates at the cluster level across treatment and control clusters were compared, and to correct any difference at baseline that could skew results, regression analysis was performed. The statistics are presented in Section IV.

One final concern has to do with the quality of the intervention. What if, even with the efforts from the evaluation team, local authorities failed to perform the intervention as planned? Table II provides information from the survey suggesting that we can rule out this possibility. As we can see, the population that reported having *Seguro Popular* in treatment areas in the follow-up survey is 45% and only 7% in control areas. The table also shows that affiliation efforts started to have quick effects since there were 13.1% of affiliates in treatment areas already in August–September.¹¹

¹¹ The evaluation team in King et al (2007) reported working under time constraints and there was a lot misunderstanding. In some clusters, health authorities started the affiliation before the application of the survey, thus the number of people with *Seguro Popular* at baseline in treatment areas was higher than the expected zero (see also Harvard University 2005).

Table II
Percentage of persons reported to be affiliated with *Seguro Popular*

	Treatment	Control	Difference
Baseline	0.131 (0.338)	0.035 (0.183)	-0.096***
Follow up	0.451 (0.497)	0.071 (0.258)	-0.379***

Note: Standard errors in parenthesis. *** Statistically significant at 1%.

II.2 Databases

The most important database used in this report is the social security administrative database of the *Sistema de Ahorro para el Retiro* (BDSAR). The database covers the August 1997–February 2006 period. It contains individual information of persons registered at the *Sistema de Ahorro para el Retiro* (SAR). It includes all workers that participate or have participated in the formal sector in Mexico since August 1997. The administrative database contains more than 36 million individual records.

The data contains information on age, sex, employer, pension fund manager who manages the account, investment fund chosen by the affiliate, wage, year of first affiliation with the social security system, and address of affiliate (in the last update, close to 2006). It registers every two months the wage and contributions made to the worker's account, so number of formal workers, and transition probabilities (probability of contributing one period given that in the previous period the worker was not contributing, and probability of not contributing one period given that in the previous period the workers was contributing) can be easily calculated.

The database was refined to correct for multiplicity of registries and temporary workers. A description of the cleaning procedure is explained in Aguila, Aguilera and Velázquez (2008). Nevertheless, even after the refining, of the individuals considered, 15% in treatment and 13% in control areas did not register a salary despite registering

participation, which is clearly a mistake (information from participation comes from one part of the database and information from salaries from another). Since the missing observations represent a small fraction and are more or less uniformly distributed across treatment and control areas, these observations were eliminated.

The second piece of information is the database of the randomized experiment. The identification code of each of the health units used to construct the health clusters was obtained from the database. Using available official data and information from the database of the Mexican mailing system, the addresses of each unit, the zip codes for each health unit, and all the workers in the BDSAR living within the same zip code as the health unit were identified.

The database of information collected includes contribution records, age, sex, wage and density of contribution of all persons who have or have had formal employment since 1997 and live within the same zip code as the treatment and control health units according to the BDSAR.

The notion of health clusters was not used since it was artificially created for the project grouping the notion of AGEB which does relate directly to zip codes. The zip code, nevertheless, defines a smaller geographical area than a health cluster. The zip code captures human settlements, such as neighborhoods. There are approximately 30,000 zip codes in México. Health clusters on the other hand were constructed using a time-to-service focus. In the rural setting a health cluster includes all AGEB¹² in which their residents could reach the health unit in less than 24 hours. Although the documents establish the same criteria (time-to-service) in the urban settings, they do not establish the threshold. Nevertheless, it is sensible to believe that given the fact that not all neighborhoods have a health clinic, a zip code presents a tighter criterion than an urban

¹² The AGEB in the rural setting limits around 11,000 hectares mainly composed of agricultural land. The AGEB in the urban setting refers to a well-defined area of blocks, which can range from 1 to 50. There are, in total, around 50,000 urban AGEB.

health cluster. If treatment affects the formal labor market, it should be felt stronger in the sites where more of the encouragement efforts took place, the population living near a health facility, i.e., in the same zip code, as documented by Gakidou et al (2006). The results are robust given our definition of the geographical area of study.

III. Results

This section will compare outcomes of interest in urban treatment and control areas before and after the implementation of the intervention. Only urban clusters were chosen because urban communities are more likely to feel any effect on the formal markets. Assuming that the absence of the intervention changes in labor market outcomes would not have been different in treatment and control areas, the difference in difference between these two groups can be interpreted as the causal effect of the program.

Results are presented depending on the unit of analysis chosen. First, formal employment was examined at the zip code level (i.e. how the number of formal employees changed before and after the experiment in the treated and control areas). The experiment included five urban health clusters and their controls. Secondly, the transition probabilities at the individual level were taken into account. Transition probabilities include: i) the probability that a person who is employed in the formal sector at baseline is not employed after the intervention; and, ii) the probability that a person not employed in the formal sector at baseline works in the formal sector after the intervention. In this case, two samples of interest were studied: a full sample and a sample comprised of women between ages 25-35 at baseline who show the highest sensibility to the implementation of a publicly subsidized health insurance program since women within this age bracket regularly use healthcare services and may prefer to

have a more flexible job in the informal sector. The full sample, reported 24,591 individuals in treatment and control areas in the first transition probability, and 30,138 in the second transition probability.

Since the “encouragement to affiliate” efforts started in late August 2005, the baseline is the fourth period of 2005, which covers July and August 2005. Evaluation focused on the effect of this endeavor over the following three consecutive periods. Data limitations did not allow evaluation until the end of the experiment in the second period of 2006. However, not having this last observation should not affect the conclusions in this study, at least those that apply to the short-term.

III.1 Results at the zip code level

Table III presents the basic descriptive statistics of the outcomes and covariates at baseline in the analysis at the zip code level. It includes the following covariates: average wage, measured in terms of the minimum wage; average age; proportion of men in the working population; and, average density of contribution, measured for each individual by the proportion of time the worker has contributed to the system since he/she joined the formal sector or for the last 4 years (contribution rates stabilize after 4 years) (see Aguila, Aguilera and Velázquez (2008)). Age, wage and sex are variables that determine participation in labor markets. The table also includes density of contributions as a measure of the attachment that a person has to the formal labor market. This variable is important because rotation of workers between formal and informal sector is high in Mexico (Levy 2008). Workers always have an option to work within the informal sector, so a higher density of contribution reflects a worker’s preference for the formal sector.

Table III**Means of outcomes of interest and covariates by treatment and control groups**

	Full sample		
	Treatment (1)	Control (2)	Difference (1) - (2)
Formal employment at baseline	2,728	1,490	1,237
	(2,460)	(1,241)	(2,755)
Formal employment 2005.05	2,663	1,450	1,213
	(2,401)	(1,210)	(2,689)
Difference	-64.40	-40.00	-24.40
	(3,438)	(1,733)	(66.7)
Formal employment 2005.06	2,614	1,428	1,186
	(2,357)	(1,193)	(2,642)
Difference	-113.60	-62.20	-51.40
	(3,407)	(1,722)	(113.8)
Formal employment 2006.01	2,530	1,384	1,147
	(2,282)	(1,157)	(2,559)
Difference	-197.20	-106.60	-90.60
	(3,3556)	(1,697.)	(196.6)
Average age	44.37	39.61	4.76
	(5.9)	(1.1)	(6.0)
Wages ^{1/}	1.22	1.52	-0.29
	(0.332)	(0.177)	(0.376)
Proportion of men	0.78	0.64	0.14*
	(0.092)	(0.020)	(0.094)
Density of contributions	57.73	43.90	13.83
	(9.815)	(5.017)	(11.022)

Notes: 1/ Times the minimum wage. Standard errors in parenthesis. ***p<0.01, **p<0.05, *p<0.1

As we can see, at-baseline-treatment and control groups do not statistically differ from each other in the three outcomes analyzed, which is result expected from the matching procedure. Advancing some results, we can see that *Seguro Popular* did not have an effect in formal employment, as measured by the difference in difference. Regarding the covariates, workers in the treatment cluster are older, have lower wages, include more men proportionally, and have higher previous density of contributions, although only the difference in the proportion of men is statistically significant.

To analyze the effect of *Seguro Popular* on outcomes of interest, controlling for covariates, a regression procedure was necessary. The regression performed was the following (only between baseline and the last observation)

$$Y_{kt} = \alpha + X\beta + \delta_1 treat + \delta_2 period_{2006.01} + \delta_3 (treat \times period_{2006.01}) + v_{kt}$$

Y_{kt} is the number of formal employees at time t of cluster k . $treat$ is a dummy variable indicating that the cluster belongs to the treatment group and $period$ is a dummy variable indicating that the observation belongs to the 2006.01 period. The parameter of interest is δ_3 , which measures the difference in difference effect of *Seguro Popular* on the outcomes of interest. X are control variables: average age, average wage, proportion of men and average density of contribution. Table IV shows the estimates.

Table IV
Regression results at the zip code level, 2005.05–2006.01

Treatment*time	-90.60	-226.13
	(3760.382)	(3369.416)
Treatment	1,237.40	4,625.27
	(2755.431)	(3118.904)
Time	-106.60	201.95
	(1697.016)	(2406.815)
Age		397.89
		(267.6956)
Wage		7,570.94
		(6958.536)
Male		-14,057.77
		(16855.72)
Contribution density (last 4 years)		-83.30
		(192.096)
Constant	1,490.20	-13,085.57
	(1241.054)	(19893.32)
R-squared	0.02	0.41
Obs	20	20

Note: Standard errors in parenthesis. ***p<0.01, **p<0.05, *p<0.1

Corroborating the data in Table III, *Seguro Popular* did not have any effect in the number of formal employees. When covariates were included the estimate increased in magnitude, but still is not statistically significant. In this regression none of the covariates is statistically significant, although the R-squared is high. Next, analysis of data at the individual level.

III.2 Results at individual level

Tables V and VI show the descriptive statistics of the outcomes and covariates at baseline in the individual analysis for the full sample and the sample comprised of young women (aged 25–35). The same covariates as in the zip code level analysis were applied at the individual level.

Table V
Means of outcomes of interest and covariates by treatment and control groups, individual data of those employed at baseline

	Full sample			Women 25-35		
	Treatment	Control	Difference	Treatment	Control	Difference
Age	37.71 (0.0893)	39.09 (0.1225)	-1.38*** (0.1510)	29.92 (0.0682)	30.44 (0.09294)	-0.52*** (0.1159)
Male	0.64 (0.0041)	0.64 (0.0055)	0.00 (0.0069)			
Wage	3.73 (0.0283)	4.21 (0.0440)	-0.47*** (0.0502)	3.13 (0.0496)	3.48 (0.0874)	-0.35*** (0.0933)
Density	88.75 (0.1649)	90.19 (0.2126)	-1.43*** (0.2729)	85.44 (0.4710)	85.60 (0.6650)	-0.16 (0.8100)
Employed in 2005.05	0.96 (0.0017)	0.96 (0.0023)	0.00 (0.0029)	0.95 (0.0046)	0.95 (0.0064)	0.00 (0.0079)
Employed in 2005.06	0.93 (0.0022)	0.93 (0.0029)	-0.01* (0.0017)	0.92 (0.0060)	0.93 (0.0077)	-0.013* (0.0100)
Employed in 2006.01	0.90 (0.0026)	0.90 (0.0034)	-0.01* (0.0043)	0.88 (0.0072)	0.89 (0.0094)	-0.01 (0.0120)
Obs.	13,638	7,451		2,060	1,074	

Note: Standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1

Table VI
Means of outcomes of interest and covariates by treatment and control groups,
individual data for those not employed at baseline

	Full sample			Women 25-35		
	Treatment	Control	Difference	Treatment	Control	Difference
Age	38.72 (0.0916)	40.78 (0.1328)	-2.06*** (0.1600)	29.96 (0.0473)	30.38 (0.0679)	-0.42*** (0.0837)
Male	0.57 (0.0035)	0.59 (0.0049)	-0.02*** (0.0060)			
Density	20.68 (0.1880)	19.79 (0.2679)	0.88*** (0.3265)	19.17 (0.3887)	18.78 (0.5820)	0.39 (0.6966)
Employed in 2005.05	0.04 (0.0014)	0.03 (0.0018)	0.001*** (0.0023)	0.03 (0.0028)	0.03 (0.0038)	0.00 (0.0049)
Employed in 2005.06	0.06 (0.0017)	0.05 (0.0023)	0.01*** (0.0029)	0.05 (0.0035)	0.06 (0.0053)	-0.00 (0.0063)
Employed in 2006.01	0.07 (0.0018)	0.07 (0.0025)	0.01** (0.0031)	0.07 (0.0038)	0.07 (0.0056)	0.00 (0.0068)
Obs.	20,089	10,049		4,237	1,937	

Note: Standard errors in parenthesis ***p<0.01, **p<0.05, *p<0.1

When individual data is analyzed, small but statistically significant differences are found between the treatment and control areas. In both samples of employed workers at baseline (full and young women), workers in treatment areas were a bit younger, had lower earnings, and had a slightly lower density of contribution in previous years. There is no difference in the composition of sex in the full sample.

In the sample of persons not employed in the formal sector at baseline, persons in the treatment group were a little bit younger, had a higher density of contribution and the sample is comprised of 57% men, while in the control group 59%. In this case, by construction there are no registries of wages at baseline.

Regarding the outcomes of interest in the first transition probability, we observe that two months after the implementation of the program 4% of the workers left the formal sector, four months later 7% had already left formal employment and six months

later 10% of the employed had left. Conversely, of those without a formal job at baseline, two months later around 4% joined the formal sector, and four and six months later around 5 and 7% had joined the formal sector.

While this data demonstrates the high rotation of the Mexican labor market, the small difference between the outcomes in the treatment and control areas show that if *Seguro Popular* is affecting individual decisions to join or leave the formal labor market the effect is rather small. To account for the possible influence of covariates, the following regression (only between baseline and the last observation) needs to be performed:

$$Y_{ikt} = \alpha + X_{it}\beta + \delta_1 treat + \delta_2 period_{2006.01} + \delta_3 (treat \times period_{2006.01}) + v_{ikt}$$

Where Y_{ikt} is a dummy variable indicating if person i works in the formal sector in area k at time $t=2005.04$ and 2006.01 . $treat$ is a dummy variable indicating the cluster belongs to the treatment group and $period$ is a dummy variable indicating that the observation belongs to the 2006.01 period. The parameter of interest is δ_3 , which measures the difference in difference effect of *Seguro Popular* on the outcomes of interest. X are control variables at baseline: age, wage, sex (only in the full sample) and previous density of contributions. Tables VII and VIII show the estimates regarding probability of leaving and joining the system for the full sample and for the case of young women (25–35 years of age), respectively.

Table VII
Regression results at the individual level, full sample

	Employed at baseline		Not employed at baseline	
	(1)	(2)	(3)	(4)
Treatment	-1.14e-14* (6.38e-15)	0.007*** (0.8935)	-2.35e-14*** (6.08e-15)	-0.003*** (0.0004)
Time	-0.096*** (0.0034)	-0.092*** (0.0033)	0.066*** (0.0025)	0.066*** (0.0024)
Treatment* time	-0.008* (0.0043)	-0.007* (0.0042)	0.008** (0.0031)	0.008** (0.0031)
Age		0.000 (0.0001)		0.001*** (0.00004)
Male		-0.008*** (0.0021)		0.012*** (0.0015)
Wage		0.0140*** (0.0003)		
Contribution density (last 4 years)		0.0001*** (0.0001)		0.001*** (0.00004)
Constant	1.000*** (5.16e-15)	0.894*** (0.0068)	2.10e-14*** (4.84e-14)	0.012*** (0.0020)
R-squared	0.053	0.112	0.037	0.063
Obs	42,178	42,178	60,276	60,276

Note: Robust standard error in parenthesis. ***p<0.01, **p<0.05, *p<0.1

Table VIII
Regression results at the zip code level, young women

	Employed at baseline		Not employed at baseline	
	(1)	(2)	(3)	(4)
Treatment	-4.02e-14*** (1.68e-15)	0.008*** (0.0027)	-1.92e-14*** (1.20e-15)	-0.001 (0.0007)
Time	-0.107*** (0.0094)	-0.100*** (0.0089)	0.066*** (0.0056)	0.066*** (0.0056)
Treatment* time	-0.013 (0.0119)	-0.012 (0.0114)	0.000 (0.0068)	0.000 (0.0067)
Age		-0.003*** (0.0009)		-0.001 (0.0005)
Wage		0.027*** (0.0017)		
Contribution density (last 4 years)		0.000*** (0.0001)		0.001*** (0.0001)
Constant	1.000*** (1.13e-15)	0.976*** (0.0287)	2.06e-14*** (8.60e-16)	0.008 (0.0162)
R-squared	0.062	0.146	0.034	0.049
Obs	6,268	6,268	12,348	12,348

Note: Robust standard error in parenthesis. ***p<0.01, **p<0.05, *p<0.1

As we can see, *Seguro Popular* implies a slightly lower probability of continuing in the formal sector and a higher probability of joining the formal sector, contrary to expectations. The effects are higher in the case of young women when we consider employment at baseline although effects are not statistically significant. There are no effects in the case of young women when we restrict the sample to those not in the formal sector at baseline.

Regarding the covariates, age has a negligible effect in permanence in the formal sector or out of the formal sector in both samples. Being male reduces the probability of staying in the labor market and increases the probability of staying out of the labor market. The higher the wage the more probable it is for workers to stay in the formal market for both samples, full and young women. Finally, the density of contribution in the past periods also has no effect on outcomes in the two samples.

III.3 Robustness checks

Even if we trust the randomization of our data, two more analyses are necessary to rule out a possible event that could skew the estimate. The identification assumption of the study's results is that in the absence of *Seguro Popular* labor market outcomes in both types of communities those treated and those not treated, behave the same way. In Panel A and Panel B of Table IX we show the difference in difference estimate of the outcomes of interest in the six-month period before the experiment took place, 2005.01 to 2005.04, and in the period 2003.3 to 2003.06 just before *Seguro Popular* was implemented. Results indicate a not-statistically-significant difference between treatment and control areas in the three periods.

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Table IX
Formal employment in different periods at the zip code level

	Treatment	Control	Difference
Experiment of interest			
2005.04	2,728	1,490.20	1,237.40
	(2,460)	(1,241)	(2,755)
2006.01	2,530	1,384	1,147
	(2,282)	(1,157)	(2,559)
Difference	-197.20	-106.60	-90.60
	(3,356)	(1,697)	(196.6)
Panel A			
2005.01	3,058	1,636	1,422
	(2,753)	(1,363)	(3,072)
2005.04	2,728	1,490	1,237
	(2,460)	(1,241)	(2,755)
Difference	-330.40	-145.80	-184.60
	(3,692)	(1,844)	(317.3)
Panel B			
2003.03	3,045	1,682	1,363
	(2,742)	(1,399)	(3,078)
2003.06	3,105	1,685	1,421
	(2,798)	(1,406)	(3,132)
Difference	60.20	2.60	57.60
	(3,918)	(1,983)	(57.0)

Note: ***p<0.01, **p<0.05, *p<0.1

Conclusions

Understanding the impact of publicly subsidized health insurance programs on formal labor markets is very important not only to redefine how healthcare systems should be financed, but to assess the overall social protection programs, especially in countries where informality is an extended phenomenon.

This paper has argued, using a randomized experiment and an administrative database that records longitudinal individual participation in the formal labor market, that the presence of *Seguro Popular* is not affecting formal employment in the short run. Although this finding is in line with other empirical studies, it is contrary to the

predictions of some views of some theoretical models. What possible explanation is there for this contradiction?

A reasonable argument, consistent with the findings in this report, and those in Azuara and Marinescu (2010) is that people have a long-term (implicit) contract with their healthcare. An individual may want to change his or her healthcare provider only after he or she has had a bad experience or has been referred to another system by close related persons, which offers a better service (ex: some medications that are not available from his or her current provider). Anyone can affiliate themselves with IMSS or *Seguro Popular* at any time because of the high rotation in the labor markets and because several loopholes in the entry procedures of the systems (ex: not requiring reporting of preexisting conditions). In this view, the mere existence of the program does not affect workers' behavior.

If this is the case, two implications arise. First, there may be no effects in the short run — since persons do not have enough information about the quality of service provided under *Seguro Popular* — but there may be long-term effects. This is consistent with the finding of Bosch and Campos-Vázquez (2010). Second, even in the long run, if individuals perceive that services provided under *Seguro Popular* are not of good quality, they may prefer to stay in the formal sector and have access to the IMSS healthcare services, as argued by Barros (2009). If this argument is true, as more resources per capita are devoted to *Seguro Popular* and as more services are covered as has been happening, the quality between the two schemes converge and it is expected that formal employment may be hindered by the program. Still, to confirm this hypothesis more research is needed.

Another possibility is that workers affiliated with IMSS value other benefits of the scheme, such as the pensions — retirement, disability or work risk — maternity

benefits, or the possibility to get a mortgage, and thus, are willing to stay in the formal employment despite the possibility to affiliate with *Seguro Popular* at no cost. If this is true, no effect should be observed in the short and in the long run. This hypothesis requires further research.

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