

# Does Expanding Health Insurance beyond Formal-Sector Workers Encourage Informality?

Measuring the Impact of Mexico's *Seguro Popular*

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## Abstract

Seguro Popular was introduced in 2002 to provide health insurance to the 50 million Mexicans without Social Security. This paper tests whether the program has had unintended consequences, distorting workers' incentives to operate in the informal sector. The analysis examines the impact of Seguro Popular on disaggregated labor market decisions, taking into account that program coverage depends not only on the individual's employment status, but also that of other household members. The identification strategy relies on the variation in Seguro Popular's rollout across municipalities and time, with the difference-in-difference estimation controlling for household fixed effects. The paper

finds that Seguro Popular lowers formality by 0.4–0.7 percentage points, with adjustments largely occurring within a few years of the program's introduction. Rather than encouraging exit from the formal sector, Seguro Popular is associated with a 3.1 percentage point reduction (a 20 percent decline) in the inflow of workers into formality. Income effects are also apparent, with significantly decreased flows out of unemployment and lower labor force participation. The impact is larger for those with less education, in larger households, and with someone else in the household guaranteeing Social Security coverage. However, workers pay for part of these benefits with lower wages in the informal sector.

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# **Does Expanding Health Insurance beyond Formal-Sector Workers Encourage Informality?: Measuring the Impact of Mexico's *Seguro Popular***

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## 1. Introduction

The existence of large informal labor markets in developing countries has attracted renewed attention because of its connection to poverty, low productivity and lack of social protection. One issue of concern is whether non-contributory programs introduced to address the lack of social safety nets for those not in the formal sector are providing perverse incentives for formal sector workers (Levy, 2008).<sup>2</sup> Formal sector workers could now avoid having to contribute to the formal social security program (SS) by moving to the informal sector while receiving (at least some) services under the new program. We propose to test this hypothesis by looking at the effect of rolling out *Seguro Popular (SP)*, a non-contributory health program for informal households in Mexico from 2000(Q2) - 2009(Q2), using longitudinal data constructed with rotating panels of households.

This issue is of particular relevance in Mexico as other work has shown a relatively high degree of mobility of workers in Mexico between the formal and informal sectors. This feature has often been raised as evidence that formal and informal sectors are not segmented. Maloney and Bosch (2006) show high labor mobility and that entry and separation rates are not significantly different across the two sectors. In addition, the probability of movement between sectors does not depend on the age of the worker, which seemingly contradicts the hypothesis that informal workers queue to get formal jobs. Maloney (1999) finds that wages sometimes increase and sometimes decrease as a worker moves from one sector into the other, and that the substantial flows in both directions persist even in periods of economic expansion. Madrigal and Pagés (2008) and Maloney (2004) find that workers value features of informal work such that it should not necessarily be taken as an inferior or residual employment option. What remains a question is whether the existence of *SP* has motivated some households to stop paying into SS to be able to qualify for free health insurance. Or, alternatively, if the availability of *SP* discouraged workers in the informal sector from seeking formal sector jobs covered by SS.

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<sup>2</sup> In this paper, we consider ‘formal sector workers’ or ‘formal workers’ those individuals who are registered and contributing to Social Security, regardless of the registration or tax status of the enterprise they work for. Thus, workers who are working for a registered firm but who are not themselves contributing to SS, are not considered to be formal sector workers.

The importance of whether the well intended program to expand health care also delivers unintended consequences in the form of disincentives to work for formal firms or to make SS contributions has larger implications on the economy. To the extent that firms in the formal sector are more productive and provide the bulk of the tax revenues (both through VATs and SS contributions), the decline in the share of workers in the formal sector can reduce aggregate productivity and tax revenues. If the disincentive effects are strong enough, they could also jeopardize the sustainability of the social insurance system.

There have been efforts to monitor and quantitatively assess the impact of *SP*. Some papers find very small or no effects (e.g. Azuara and Marinescu, 2011; Barros, 2008; Campos-Vazquez and Knox, 2008), while Bosch et al. (2010) provides aggregate results that *SP* may help explain a decline in formal sector jobs. A paper looking at a different health insurance program in the Federal District also provides suggestive evidence of an effect on participation and wages (Juarez 2008). This paper addresses some of the data limitations, concerns about omitted variables, endogeneity, and institutional features of the SS and *SP* programs that have not been adequately addressed in the existing literature.

This paper makes five contributions to the existing literature. First, this paper exploits the panel nature of the data. The existing papers have either used aggregate data or repeat cross-sections that cannot control for unobserved heterogeneity. Second, we use more disaggregated measures of *SP*. Existing papers fail to capture the full variability of *SP*, either looking at state level or annual measures, when the rollout occurred across municipalities within states over time, with municipality data available quarterly. We also confirm that the rollout of the program was not endogenous to the level or trends of formality within municipalities. Third, we take into account additional institutional features of SS and *SP*, that an individual is covered not only based on their own employment status, but also that of their spouse, and in some cases, their parents. None of the existing studies have considered the joint decisions of members within the same household, that one member's participation in the formal sector or contributions to SS extends coverage of SS to other household members (spouse and dependents). Fourth, other papers have paid little attention to whether the adjustment was a temporary one, i.e. whether there was a level shift in the size of the formal sector, whether the rates of transition in and out of the formal sector themselves

shifted, or whether wages were affected by *SP*. Fifth, most papers have focused on the earliest rollout periods, when the program covered only a small fraction of the population and found little effect of the program. This paper looks at the longer period from 2000 to 2009.

In this paper, we have data on almost 10 million individuals in rotating panels from 2000-09. Individual panel data allows for improved control of individual characteristics and assessing whether *SP* affects labor market dynamics. It also allows us to test whether different subgroups of households or different individuals within the household are more sensitive to *SP*. We can also control for the potential endogeneity of the employment decision based on the decisions of others in the household.

We find that *SP* does impact the decisions of workers, generating a moderate increase in the share of workers in the informal sector. Exploiting variation both over time and across municipalities, the effects of introducing *SP* results in a decline in the share of households with SS of 0.4 to 0.7 percentage points, depending how time effects are controlled for. The results indicate that much of the adjustment occurs within a few years of the introduction of the program, with the effects becoming relatively smaller as the intensity of the program expands. The effect is associated with a 3.1 percentage point reduction (a 20 percent decline) in the inflow of workers into formality rather than an increase of outflows from formality. Thus, while the aggregate net change in the level of formality is not that large, the proportional change in flows into formality is. There is also evidence of important income effects, with the introduction of *SP* associated with fewer transitions out of unemployment and lower rates of entry into the labor force. Our results also suggest higher sensitivity in larger households, as well as among household headed by persons with less than secondary education completed, particularly if the head is female. They also suggest a higher response among individuals who are covered by SS through someone else, or among individuals in a household that are not heads, spouses or dependents of heads. Finally, our results suggest that workers pay for part of the newly acquired *SP* benefits with lower wages in the informal sector.

## **2. Background on *Seguro Popular***

México's Social Security system (SS) only covers about 45 percent of the population, namely formally registered wage workers and their dependents. Self-employed workers are not obliged to

contribute. This has left a large share of the population with limited access to the health care and no access to pensions and other social benefits offered by SS. Moreover a large share of employees in registered firms is not contributing to SS, even if contributions are supposed to be mandatory. To face up to the low coverage of SS, in 2001 the government launched a pilot program, *Seguro Popular (SP)*, to improve health care access to all Mexicans not covered by SS<sup>3</sup>. In 2002, the program was formally established and reached a total of 295,513 households. By 2007 more than five million households had access to the program. The goal is to provide health insurance to everybody lacking SS coverage by 2012. The plan provides different schemes of health insurance, increases investment in health units (hospitals, clinics), expands distribution of medicines, and improves quality of health (including preventative) services.

*SP* is financed by the Federal and States governments. In principle, beneficiaries would pay contributions according to their decile of income, with families included in the lowest two deciles exempted from contributions. However in practice, only 2% of beneficiaries pay to participate in the program (Comisión Nacional de Protection Social y Salud, CNPSS, 2009) despite the fact that 65% of its beneficiaries would be non-poor (Scott, 2006). The only condition for eligibility is not to be a beneficiary of SS. Affiliation of the nuclear family is by household. Coverage extends to the spouse or partner of the affiliate, children under 18, or up to 25 if single and students or economically dependent and living in same dwelling. In addition, parents of the head or spouse 65 or older living in the same house and economically dependent, are also covered. Other members of the household can also be covered, each one with their own individual affiliation. SS coverage is also by household. An important difference however is that SS only covers the spouse and children (up to 16, or up to 25 if incapacitated or studying) of the affiliate. This would suggest that larger households –particularly those with other relatives living in the household – would see *SP* as relatively more attractive and thus could have a higher probability of dropping or not joining SS.

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<sup>3</sup> Prior to the creation of *Seguro Popular*, workers not affiliated to Social Security could obtain health care at either public health clinics run by the Secretaria de Salud (SSA) or at private clinics and hospitals. Public health services were few and of low quality. *Seguro Popular* notably increased the quality and quantity of services provided for the uncovered population.

Another feature of the *SP* program also helps with the identification strategy; the program was launched in 2002, but was rolled out across municipalities over time. This variation in location-time of *SP* coverage provides a cleaner test than a simple before-after comparison.

Studies show that there has been significant uptake of the program, although the impact of *SP* on health outcomes is mixed. King et al. (2007) assessed a sample representing 118,569 households in seven Mexican states, and measured outcomes in a 2005 baseline survey (August, 2005, to September, 2005) and follow-up survey 10 months later (July, 2006, to August, 2006). The treatment consisted of encouragement to enroll in a health-insurance Program and upgraded medical facilities. Participant states also received funds to improve health facilities and to provide medications for services in treated clusters. They estimated intention to treat and complier average causal effects non-parametrically. They find that the Program resources reached the poor. However, the Program did not show some other expected health effects, possibly due to the short duration of treatment (10 months).<sup>4</sup>

Scott (2006) also does an impact evaluation of *SP* using data from 2004 and finds that there was significant uptake. However, he finds that the selection of beneficiaries does not target the poorest states and households as effectively as its stated objectives. This raises the possibility that the program could indeed be affecting the decision of whether to work in the formal or informal sector. If the program successfully targeted those in the lowest income deciles one might expect more limited impact on formality as these households are already significantly less likely to participate in the formal sector. However, this evidence that non-poor households are participating in *SP* would suggest that it is affecting a portion of the population that might well be considering shifts between the formal and informal sectors.

### **3. Literature Review**

There have been several studies that examine the impact on informality of large social programs that may affect the incentive workers have to exit formal jobs. This section first explores the

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<sup>4</sup> Sosa-Rubi et al. (2007) analyzed the cross-sectional 2006 National Health and Nutrition Survey (ENSANUT), focusing on the responses of 3,890 women who delivered babies during 2001-2006. They find that the *SP* program had a robust, significantly positive impact on access to obstetrical services.

recent papers measuring the effect of *SP* on labor market outcomes in Mexico. It then discusses relevant papers which evaluate the impact of similar programs in other countries. Overall, there is suggestive evidence that *SP* may discourage formal employment, but data limitations, endogeneity, and omitted variables has left room for a more rigorous testing of the hypothesis.

Four established working papers are particularly relevant for our study. Two found no effect of *SP* on labor market decisions, while two provide suggestive evidence of an effect – although using aggregate data or data only covering the Federal District.

Campos-Vazquez and Knox (2008) use aggregate data from 33 urban cities from the “Encuesta Nacional de Empleo Urbano” (ENE) during the period 2001-2004. They do not find any effect of *SP* on the rate of formal employment in the municipality. They also conduct a parallel analysis using individual-level data from 136 municipalities during 2002-2004 that also fails to find a significant effect. However, their data is only available for individuals from the poorest deciles, thus it captures a segment of society that already has high levels of informality and where it would be less likely that *SP* would have much effect. Another potential problem is that their period of study may be too premature to find any effect at all.

Parker and Scott (2008) use Rand’s Mexican Family Life Survey 2002 – 2005 panel. They do find a disincentive effect in rural municipalities, with the percentage change relatively large due to the small base, i.e. the absolute magnitude of the change is small. However, they do not find comparable effects in the urban areas. Using aggregate data from the 2000 and 2005 census, they do not find significant effects. This work has only a limited time dimension and is again looking at the early years of the program.

Barros (2008) measures the effect of *SP* on health, consumption, and labor outcomes. He estimates the *SP* effect by using a triple difference equation, taking differences over time, state intensity target (stated objective in 2007 of share of eligible households to enroll), and eligibility (whether no-one in the household holds a formal sector job). He assumes that eligibility in a given state is not correlated with the state’s target intensity.

He uses 4 repeated cross-sections of the national income-expenditure survey ENIGH (2000, 2004, 2005, 2006) which includes labor outcomes and categories of expenditures. ENIGH data has a total of 76,000 household-heads and 156,247 other members. He does not find a significant result on the probability of being in the formal sector, nor does he find an effect on wages. However, Barros does not control for unobserved characteristics as he uses cross section data. The target intensity may also be a poor measure of the actual intention to enroll households in *SP*; the targets are likely to reflect political stances rather than an accurate measure of the availability of *SP* services in an area.

Bosch et al. (2010) look at the effect of *SP* over a longer time period and do find a disincentive effect on formal employment. They use aggregate data at the municipality level from the Instituto Mexicano de Seguro Social (IMSS) from 2001 to 2008. They use a difference in difference approach, comparing the differences in formal jobs outcomes in the municipalities with the program and compare to those without the program. They find evidence of a negative impact of the program on formality at late stages of the program (i.e. after 2007) and found that it is larger in small firms compared to micro firms (with already a high level of informality). However, there are limitations to this work. Their data are aggregated at the municipality level, so they cannot control for individual heterogeneity or aggregation biases.

New working papers do use individual level data to look at the impact of *SP* (Aguilera, 2011; Azuara and Marinescu, 2011; Duval and Smith 2011). However, they only use individual cross-sectional data; they do not look at how individuals' change their employment status over time nor do they control for unobserved heterogeneity. They also do not take into account that individuals' decisions can be affected by the *SS* status of their spouse, nor do they consider both flows into and out of *SS*.<sup>5</sup>

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<sup>5</sup> Duval and Smith (2011) look only at the likelihood of whether informal workers shift from being involuntarily informal to voluntarily informal, but having to use observed household characteristics in making this estimation. Azuara and Marinescu (2011) do look at transitions in employment categories, but only over a single quarter so their results would only capture changes that occurred at the precise moment of the rollout. Otherwise, their results are based on differences between individuals in the cross-section. They also measure *SP* on an annual basis rather than quarterly basis.

One other paper is relevant here too, although it is not focused on *SP* itself. Rather, Juarez, L. (2008) looks at a different program rolled out only in the Federal District (DF) to provide universal free health insurance in 2001. She constructs a difference-in-difference approach by comparing results with 3 neighboring states that did not have the program. The paper's focus is the effect on wages, but she uses the program as an instrument for participation in the formal sector so the first stage results are relevant here.

She uses quarterly individual data from Encuesta Nacional de Empleo 2000-2004 taking the subsample of unskilled 18-60 years old "salaried" women outside of the government sector. She is able to control for a number of individual and household characteristics. While it is a panel data, she does not use panel estimation techniques. Rather, she pooled the cross sections and clustered the standard errors at the individual level. She finds that a woman with at most high school education in DF is 4 to 9.6 percentage points less likely to have a formal job after the policy change.

Juarez (2008) is only testing a single district, and the change is over 1 period, so it makes it harder to exclude the possibility of another trend in the DF contributing to the results. Moreover, at least part of the estimated effects could be driven by migration to covered areas. However, it is certainly suggestive that these types of effects may be at work in the larger *SP* program.

In addition to *Seguro Popular*, two other programs in Argentina and Colombia have been introduced and evaluated for their effects on formal employment. The programs do not share all the same features as *SP*, but again they indicate that it is worth evaluating the potential effect they may have on incentives to work in the formal sector.

Gasparini et al. (2007) assess the impact of a poverty alleviation program, "*Programa Jefes de Hogar*" (PJH), on labor informality in Argentina. The main benefit of the program was a conditional cash-transfer of 150 pesos/month (below average market wage for unskilled workers) to each eligible individual (unemployed household-heads with dependants under 18 who are enrolled in school). Participants were required to perform 20 hours a week of training, school, or subsidized work in a private company for 6 months. However, it is unclear how much these work

conditions were enforced; because the program's target was to alleviate poverty, beneficiaries included poor families even when the household-head or someone else in the household had some labor activity. In addition, as the federal government provided the funds while municipalities were responsible for program implementation –local governments had little incentive to promote formal jobs and reduce beneficiaries or to enforce requirements strictly.

The dataset consisted of two short rotating panels from the national household survey (Encuesta Permanente de Hogares Continua –EPHC) covering 70% of Argentinean urban populations in 28 large urban areas and representing 60% of the population. Individuals are interviewed in two consecutive rounds, left for two other rounds and followed again in the following 2 quarters (rounds). The variables of interest are whether a specific individual transferred to formal employment in period  $t$  to  $t+1$  and whether the individual is a beneficiary of PJH in  $t$ . The authors employ a difference in difference estimation to study whether program beneficiaries are more reluctant to seek/accept formal jobs than otherwise equal counterparts, particularly when the relative value of the transfer with respect to formal wages dropped.

They find some evidence on the informality bias of the PJH in the period when the value of the transfer was relatively high compared to wages in the formal labor market. The effect of the distortion vanished when earnings in registered jobs went substantially up. The results, however, are not robust to all the specifications.

Camacho et al. (2009) measure the effect of a subsidized regime (SR) for health insurance for the poor in Colombia on informality. The period of study is 1992-05. SR was introduced in 1993 and informality increased steadily in the period 1996-2002.

The SR is designed to reach the poor and the unemployed. The SR is financed with a one percent transfer from the formal sector program, CR, and with local and central government funds. The package is less comprehensive than the CR, but unlike the CR, covers every member in an eligible household regardless of their relationship to the household head. Coverage for any additional family member other than spouse and direct dependents in the CR has an additional cost. Thus

there is one additional margin to test for the impact of the program: households with larger extended families may be more affected by SR.

They use the Colombian Household Surveys 1986-2005 (before and after reform); it is a bi-yearly data from 1986 to 1996 and yearly from 1997 to 2005. It is representative of 10 cities and provides repeat cross-sections. They find a 2-5% increase in informality post-SR. The effects of the SR are strongest in households with no vulnerable members and attenuated in household with vulnerable members and also in household without potential extended beneficiaries. They also estimate their results in a panel of poor individuals matched twice with similar results.

Identification of these effects of SR comes from time variation across cities as to when a means test of eligibility for SR was implemented. In their panel estimates, the variation is also across eligibility to SR or not. However, with their data (biannual data for household surveys or two waves for the panel of poor individuals), it appears there is only one round of data difference in the introduction of the program across cities. Thus, most of the identification comes from pre and post comparisons. This makes the results particularly susceptible of being influenced by other changes affecting labor markets that were occurring at the same time, e.g. a broader package of labor market reforms were introduced to make labor markets more flexible and there were external shocks that affected labor demand. The simultaneity of these changes makes it difficult to ascribe all of the effects to the SR program.

The papers reviewed, both in Mexico and outside of Mexico, suggest that social programs that put incentives on informal work can have an effect on the level of informality. However, the papers are not conclusive and there is room to improve the measurement of the effects. The next section describes the data and the identification strategy proposed for this task.

#### **4. Expected Effects of *SP***

*SP* provides health benefits to households provided that the individuals are not in formal jobs or covered by SS through someone else in the household. In that regard, the introduction of *SP* can affect various margins:

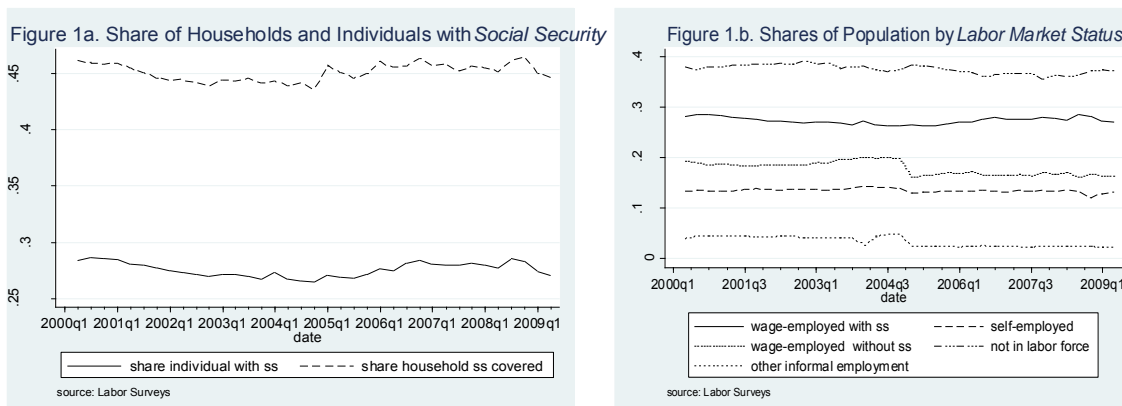
1. *Participating/not participating.* As households spend considerable resources in health care, any benefit that reduces the costs of health care goods and services is tantamount to an increase in income. It is therefore expected that if this effect is important, some workers may reduce their labor supply. Also, transitions out of inactivity may decline.
2. *Unemployment/not participating.* Workers who were previously searching for a job may stop searching or search less intensively when the benefit is made available.
3. *Formal/informal jobs.* In an integrated labor market, that is, in a market where workers have formal and informal jobs available and wages are determined such that workers are indifferent between formal and informal jobs, the possibility of getting *SP* free of charge makes informal jobs more attractive. It is therefore expected that the following occurs: first, some workers leave the formal sector to take jobs in the informal sector. Second, the number of workers who would have normally transited from informal to formal jobs declines, and third, wages in formal sector adjust upwards relative to wages in the informal sector to compensate for the higher difficulty to attract workers in the formal sector.

All in all, the former implies that the introduction of *SP* is expected to be associated with a decline in the supply of labor, a reduction in the outflow from unemployment, an increase in the share of workers in the informal sector, and a raise in the wages of formal relative to wages of informal workers. Notice however, that the ratio of total informal workers to population may increase or decline depending on whether changes in participation dominate the formal/ informal employment margin. The described effects will be more prevalent the more integrated are the formal and informal sectors of the labor market. In particular, in a totally segmented labor market, that is, one in which informal workers are queuing to get formal sector jobs, the introduction of *SP* may not be enough to make informal jobs as attractive as formal ones. In that event, the introduction of *SP* should not affect the incentives to become informal, although it may still entail income effects, which affect the participation/non-participation and the unemployment/non-participation decisions of individuals and households.

## 5. Data

This study uses quarterly data from the National Employment Survey “*Encuesta Nacional de Ocupación y Empleo*” of Mexico, a rotating panel of households. There are two periods of implementation (ENE: 2000-2004) and (ENOE: 2005-2009). It is nationally representative but, strictly speaking, the ENE survey had an adequate frame only for the urban population. The data includes a rotating panel at the individual and household level (2000-2009)<sup>6</sup>.

The data cover almost 10 million individuals from 2000 (Q2) to 2009 (Q2) between 15 and 65 years old<sup>7</sup> in 291 municipalities across the country<sup>8</sup>. We observe whether a specific individual changes *SS* status (provided by formal employment) over consecutive periods. We also observe whether the individual is covered by *SS* through the spouse or directly through his or her job. At the household level we have an average of 100,000 households per period. Figure 1a shows the quarterly trend of the share of individuals with *SS* and households covered by *SS*. Formality exhibits an upward trend more so after the first quarter of 2005. Figure 1b shows the trends of the shares of population by their labor market status. There is a drop in the share of wage employees without *SS* and other informal employment at around the fourth quarter of 2004 but for the most part the shares are stable.

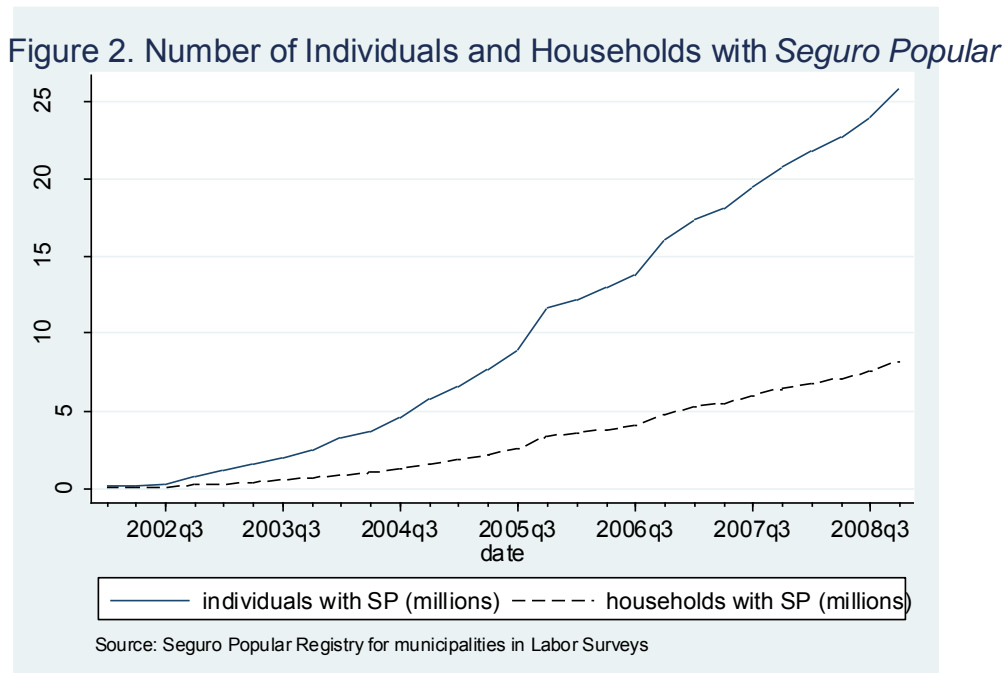


<sup>6</sup> There are three fifths of individuals at the end of 2004 that rollover to ENOE in 2005. However, in view of some methodological changes implemented to the ENOE relative to the ENE which could affect observations overlapping the two panels, we chose to discontinue rollover of individuals into 2005. In our estimates, differences in methodology across the two datasets are accounted for with either a survey dummy or with household fixed effects.

<sup>7</sup> About 370,000 individuals per quarter.

<sup>8</sup> Mexico has 2,456 municipalities in 2009 and the data covers 1,272, however we eliminated municipalities that: 1) do not have at least 50 observations once; 2) do not have at least 30 observations which appear at least 4 consecutive quarters.

To measure *SP* we use the household registry of each entity with records on date of each household membership and location. We match the number of households registered in *SP* by municipality and quarter obtained from the administrative data with the panel data. Figure 2 shows the trend in the number of households and individuals with *SP*.



In turn, figure 3 shows the share of municipalities that have rolled out *SP* using different thresholds (at least 5 households, 2 percent and 5 percent of the population in the municipality covered). The program started being deployed in a few municipalities by 2002 and its coverage went steadily up over the years. By 2005, about 60 percent of the municipalities had at least 5 people enrolled and in 40 percent of the municipalities coverage was above 2 percent of the population. By 2009, the program is present in all municipalities, with more than 5 percent of the population covered.

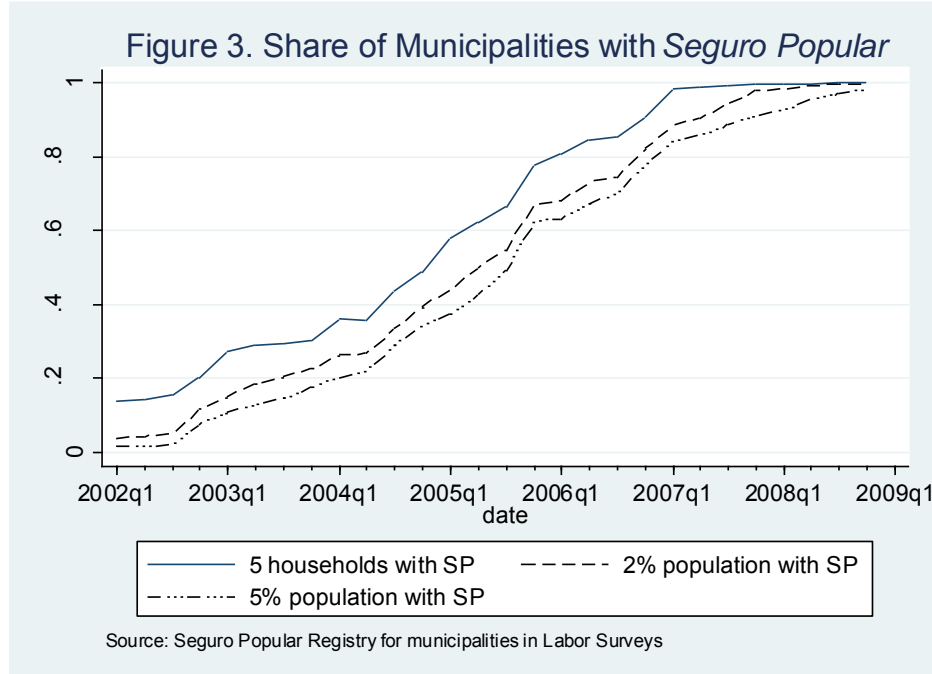


Table 1 shows the summary statistics of the main variables in the analysis. On average 45 percent of the households are covered and 27 of the individuals are directly affiliated by *SS*. Most households have at least 6 people (61 percent). Regarding the shares in different labor market status, in addition to the 27 percent in salaried formal jobs, there are 18 percent of the population in salaried jobs without *SS*, and another 14 percent who are self-employed, 2 percent who are unemployed and 4 percent in other informal jobs. Finally, 38 percent of the population is out of the labor force.

## 6. Identification Strategy

The analysis will examine the impact of *Seguro Popular (SP)* on the probability of a household being covered by *SS* by estimating the following specification.

$$P(SS_{ijt}) = \alpha_i + \lambda Z_t + \beta SP_{jt} + \psi X_{ijt} + \varepsilon_{ijt}, \text{ where } E[\varepsilon_{ijt} | j_t] = 0,$$

where  $i$  identifies the household,  $j$  the municipality, and  $t$  the period, *SS* takes the value of 1 if the household head or the spouse of the head are contributing to *SS* through their jobs,  $Z_t$  is a vector of time dependent variables, and  $X_{ijt}$  is a vector of household characteristics that vary over time.

Finally,  $SP_{jt}$  is defined as a dummy that takes a value of 1 in municipalities and periods in which the number of affiliates to the program is positive.

Panel data enables to control for unobserved time invariant characteristics of the unit making the employment decisions (household). Thus, the average  $SP$  effect is given by  $\beta$  and is estimated by comparing the differences in the probability of formality of households before and after  $SP$  is rolled out across municipalities with and without  $SP$ .

The identification strategy relies on the variation of  $SP$  rollout across municipalities and time and that the difference-in-difference estimation controls for other municipality, time effects. The program was rolled out across the 32 states in Mexico over 4 years, and within states, coverage of municipalities was also rolled out over time (up to 8 years). If  $\beta$  is negative when looking at the probability of formal employment, this would be consistent with  $SP$  having the incentive of encouraging informality.

Our identification strategy could fail if the rollout pattern of  $SP$  coincides with other shocks affecting labor markets. Having multiple locations and multiple years mitigates this concern although it cannot strictly rule it out. To minimize this problem, we control for macro level variables (quarterly GDP and inflation, or alternatively with quarter dummies). We also allow for state specific time trends to capture the important heterogeneity of the Mexican economy.

The analysis also assesses the degree of sensitivity of results to the use of cross sectional estimation by means of the following specification:

$$P(SS_{ijt}) = \varphi_j + \alpha M_t + \mu R_{it} + \lambda Z_t + \beta SP_{jt} + \psi X_{ijt} + \mu_{ijt}, \text{ where } E[\mu_{ijt} | j_t] = 0 \quad (2)$$

Where  $\varphi_j$  denotes a set of municipality dummies  $M_t$  is a dummy that identifies whether an observation is part of the first or the second panel of data, and  $R_{it}$  is a vector of individual characteristics of the household head.

An adult individual can get SS through his or her formal job, through his or her spouse's job or through parents if he or she is a dependant)<sup>9</sup>. The decision of spouses to move sectors based on SS status is likely to be taken at the household level. If it is a joint decision, ignoring the spouse's decision is an omitted variable; but including it introduces endogeneity.

We address this potential concern by estimating the model at the household level. This abstracts from whether one or more members in the household switch between formal and informal jobs to look at the status of the household as a whole. It limits the effects to those switches that affect the household's source of health insurance.

An important concern is that the rollout of *SP* could be correlated with the informality trends in municipalities. This would affect our estimates because the error term would be correlated with our estimate of interest. The results presented in Table 2 however indicate there is little correlation between the share of formality in a given period and municipality and the probability that municipality receives *SP* in the following period. The probability of *SP* being rolled out is measured with a dummy variable that takes a value of 1 when a municipality shows at least one affiliate in the *SP* registry, and zero otherwise. This is true whether the share of formality is lagged one or two years (Columns (1) – (4)). The rate of change of formality (measured as a percentage of households or of individuals in SS) in a municipality is likewise uncorrelated with the timing of *SP*'s rollout (Columns (5) and (6)). These results limit concerns of the possible endogeneity of the deployment of *SP* across municipalities.<sup>10</sup>

SS and *SP* are not perfect substitutes. Contributing to SS also gives right to pension and job security benefits. *SP* only provides for health benefits, which in principle are lower than those granted by SS. However, as previously mentioned, and unlike SS, *SP* provides benefits for all members of the household. Importantly, *SP* is also free. Therefore, the household's decision to choose between *SP* and SS could also be a function of wealth (richer people may opt for private health care even if SS or *SP* is available), the expectations of wages in each sector (formal and

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<sup>9</sup> In Mexico, ss through a formal job covers spouse, parents living in household, and children up to 15, or up to 26 if student or disabled and living in the same house.

<sup>10</sup> There was some discussion among policymakers of targeting the rollout to more populous states first. There is some evidence of this, but controlling for various state and municipality characteristics rather than including municipality fixed effects, the correlation with the size of the formal sector remains insignificant.

informal) and household characteristics, such as age and household size and number of dependents (i.e. having more dependents may increase the need for medical coverage for extended members of the family, as well as increase the willingness to forgo a pension because of higher expectations of being taken care of in older age; also, younger individuals may care less about pensions<sup>11</sup>.) Later in the paper, we test whether the effects of the introduction of *SP* have been different across different types of households.

Households vary in their underlying probability of having someone making SS contributions and thus providing SS coverage. For this table, the household is considered to be covered if either the head or the spouse of the head is making contributions to SS. Table 3 presents the average incidence of SS coverage across household types, averaged across time. As expected, households headed by persons with lower levels of formal education (less than secondary schooling completed) are much less likely to be covered by *SS* than households headed by individuals with higher educational attainment. This is particularly true for households with female heads with less than secondary school completed. In contrast, smaller and larger households have similar rates of coverage.

## 7. Results

Table 4 shows the results of estimating the effect of the rollout of *Seguro Popular* in a given municipality on the probability that a household in that municipality is covered by *SS*, based on models (1) and (2). Columns 1 and 2 report the results of the cross section model, controlling for the characteristics of the household head (gender, age, education) as well as household composition characteristics, such as number of children 5 years old or younger, the number of children between 6 and 13 years old and the number of individuals in the household that are older than 64, and municipality fixed effects. In addition, to control for macro variables and other time varying effects it includes quarterly national GDP, quarterly national inflation and state-specific trends. All specifications are clustered at the municipality level (Bertrand et al., 2004). The first column uses all the individuals in each household, while the second column uses only one observation per household.

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<sup>11</sup> Juarez, L. (2008).

The results strongly suggest a moderate negative effect on informality when *SP* is deployed in a given municipality. The results are very similar across the two columns, and indicate that the introduction of *SP* increases informality by about 0.7 percentage points, which amounts to an additional 1.5 percent of the baseline level.

We next assess the robustness of our results to the introduction of random and individual fixed effects (Columns 3 and 4, respectively), making full use of the longitudinal nature of the data. It should be emphasized that this identification strategy is very demanding, as it is based on comparing the change in the probability of being formal of a *given household* around the introduction of *SP* in a given municipality. Because we can follow households during at most five quarters, our windows of observation around given thresholds are small. The advantage of this methodology is that it ensures that we compare the same households over time and therefore that the results are not due to composition effects resulting from aggregating different households over time—or due to the migration of individuals with a higher probability of being informal toward locations with *SP*. This aspect is particularly important because household surveys in Mexico are not designed to be representative at the municipality level. This implies that cross sectional estimates may suffer from the aggregation at the municipality level of non-representative averages over time, a problem avoided with fixed effects estimates, as they are based on comparing the behavior of given households over time. Given their superior possibilities to control for unobservables and make good use of existing data, most of our results are inferred from estimates that take full advantage of the longitudinal nature of the survey.

The results for the random and fixed effects model yield very similar coefficients and in both cases about half the ones obtained in the cross sectional model. Thus, the coefficient of *SP* is negative and statistically significant and it indicates a magnitude of the impact of 0.4 percentage points. Nonetheless, the Hausmann test rejects the null hypothesis of equality between random and fixed effects. We therefore only estimate fixed effect models in the remainder of the paper.

We next assess the robustness of our basic results to changes in the specification. Column (1) in Table 5 reports the results of excluding state trends from the FE specification. This yields an

impact of *SP* that is twice as large if included and similar in size to the one obtained with cross sectional estimates. Column (2) in Table 5 is identical to Column (4) Table 4 and is provided only for comparison. Column (3) reports the estimates when instead of controlling for business cycle and other macro variables with quarterly log of GDP and inflation, quarterly time dummies are included. In that case, the size of the coefficient is further reduced and it is no longer significant. Therefore, we cannot exclude the possibility that *SP* has no effect, although this specification maybe too demanding, as it does not use most of the variation of the data.

One possible source of concern is whether our estimates are driven by the launching of a non-contributory pension program during the period of study in the municipality of Mexico City. To the extent that individuals can obtain pensions if informal, informality could have become more attractive due to this program rather than *SP*. We re-estimate our basic results presented in Column 2, (i.e. including state-specific trends) excluding Mexico City from the sample (Col 4) and the results remain unchanged. We also re-estimate our results excluding the last quarter of 2004, a period in which the survey instrument changed and the sample size of the survey was reduced (Col 5) and again find no change in the results.<sup>12</sup>

Finally, to take into account the extensive margin and the fact that the effects may intensify as the number of affiliates increase, we also estimate a quadratic specification with the Log and the Log square of the number of affiliates with our basic macro controls (state trends, Log GDP and inflation). The results are reported in Column 6. The quadratic specification suggests an effect which increases over time until it peaks, to decline somewhat afterwards. It indicates that on average an increase in the number of affiliates of 10 percent leads to a decrease in 0.012 percentage points in formality. At the same time, the number of people in *SP* began at low levels such that the percentage increase in those affiliated with *SP* has risen tremendously over time.

#### *Distance to SP roll out*

To further assess the effects overtime of the introduction of the program, we estimate a third specification which includes a full set of indicator variables that capture the distance to the quarter

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<sup>12</sup> We also exclude the last quarter of 2008 and the data from 2009 to ensure that the results are not being driven by the onset of the financial crisis. The results are robust.

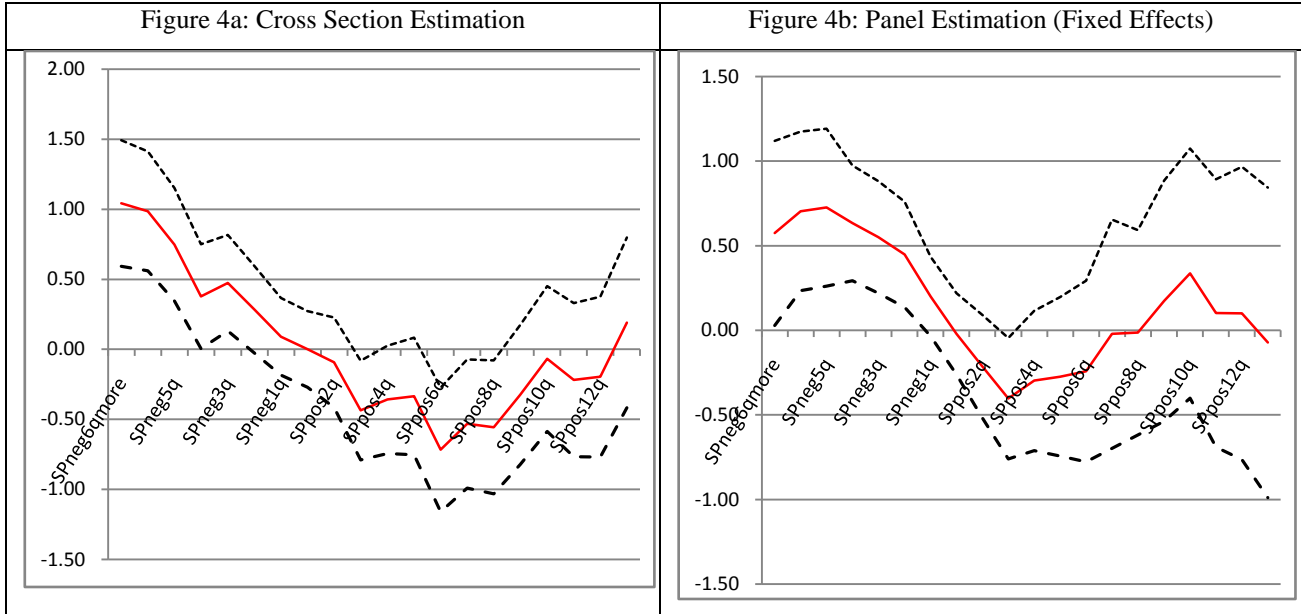
of deployment of the program in a given municipality –defined as the quarter in which the number of *SP* affiliates changes from zero to a positive number – that is:

$$P(SS_{ijt}) = \gamma_i + \lambda Z_t + \sum_{\tau=-\alpha}^{\beta} \phi_{\tau} D_{j\tau} + \psi X_{ijt} + \varepsilon_{ijt}, \text{ where } E[\varepsilon_{ijt}|j_t]=0 \quad (3)$$

where  $D_{j\tau}$  denotes an indicator which takes the value of 1,  $\tau$  quarters after deployment of *SP* in municipality  $j$  and where  $\tau < 0$  denotes number of quarters before the starting time of the program. We also estimate a cross section version of this estimation, in which instead of household fixed effects we control for municipality fixed effects, household type dummies, and an indicator of whether the data belongs to the first or the second panel of the data.

The results, summarized in figures 4a and 4b suggest that the deployment of *SP* brings about a negative impact on formality which would become smaller over time. According to the cross sectional estimates effects would pick 6 to 8 quarters after the introduction of the program, while in the FE estimation, effects would pick 3 quarters after deployment. After a few periods, the effects appear to be no longer significant; however, this may be related to the still short life of the program: only a relatively small fraction of households had access to the program for more than 2 years during the span of our data.

**Figure 4: Impact of Seguro Popular on Formality over Time:**  
Regression Coefficients and 10% Confidence Intervals.



Source: Authors' estimates from *SP* registry and household surveys.

*Intra-household dynamics*

We next explore if, within households, individuals have different incentives to become informal given the availability of *SP* depending on their position in the household. These different dynamics could arise, for example, because coverage of *SS* is extended to eligible dependents of a contributor. Given the current design of *SS*, eligible dependents of a contributor who also contribute obtain exactly the same benefits as if they do not. Therefore, *SS* offers few incentives to contribute as a second earner. The availability of *SP* may make switching to informality more attractive for a second earner if the switcher and her family can get coverage *by SP in addition to* coverage by *SS*, thus maximizing access to different means of protection. In some municipalities, *SP* may provide closer access to clinics or hospitals even if of lower quality, which make it convenient for families to have both and chose depending on needs. Another benefit of *SP* is that it extends health insurance coverage to other members of the household, such as elderly relatives, not covered in *SS*. While in principle, the rules of the program, state that coverage to *SP* requires not being affiliated to *SS* it is unclear whether this condition is enforced in practice.

To assess this, in Table 6 we re-estimate specification (1) at the individual level distinguishing by the relative position that a person occupies within a household, that is, head, spouse of head, son

or daughter of head, and other relatives of head. All estimations include individual fixed effects. Column (1) reports results for all individuals in the sample. Noticeably, the coefficient indicates an effect of *SP* on informality of 0.6 percentage points, almost twice the size of the effect estimated at the household level. This difference suggests that intra-household dynamics are important, but some transitions at the individual level do not translate into changes of the household status on whether they are covered or not covered by *SS*. Column (2) focuses on results for household heads. This coefficient is similar in size to the one estimated at the household level, which suggests that non-heads are more sensitive to *SP*. The results presented in Columns (3) - (6) are consistent with this hypothesis. Spouses are estimated to be more sensitive to the introduction of *SP* than household heads (Column 3), particularly if married to or cohabitating with a household head who is contributing to *SS* (Column 4). We confirm the differences in coefficients provided in Columns (3) and (4) are statistically significant (Column 5). This would be consistent with *SP* and *SS* not being perfect substitutes, but operating as complementary programs in practice for some families. Household members who are neither heads nor dependent of heads show an even higher level of sensitivity, about five times larger than the effects for household heads (Column 6). In contrast, household members covered by *SS* by their parents exhibit a low degree of sensitivity to *SP* (Column 7), consistent with the finding discussed below that younger people and those with fewer dependents are less sensitive to the introduction of *SP*. We also confirm that there are no important differences in response to *SP* within household heads, be they single without dependents (Column 8).

One problem that may arise in these estimates is that the *SS* contribution status of other members of the household may be endogeneous to an individual's own status if the members of a household make decisions jointly. This might particularly be the case in the regressions for spouses, since their decisions are likely to be made in a simultaneous fashion with household heads. To address this concern, we re-estimate the results presented in Column (5) by instrumental variables. The *SS* status of the head is predicted with the set of variables included in specification (2) and the interaction between *SP* and the *SS* status of the head is predicted with interactions between the variables included in specification (2) and *SP*. The results of this IV approach are presented in Column (9) of Table 6. Results suggest that the status of the head regarding *SS* is a very important predictor of own status for the spouse. In addition, our results confirm that for spouses with

household heads who contribute to SS, the introduction of *SP* acts as an important disincentive for the spouse to contribute.

Overall, the analysis suggests that different individuals have markedly different incentives to switch to *SP* depending on their household situation. Heads of household, whether single or married, are not easily swayed to moving to informality by the availability of a lower quality, but free of charge, health insurance. In contrast, spouses of heads are more likely to switch, particularly if the head is already contributing to SS. We also find evidence that people other than household heads, spouses of heads, or dependents of heads (such as older parents, relatives, cousins, etc) have a higher probability of stopping contributing to SS after the roll out of *SP* than household heads, their dependents or their spouses.

### *Labor flows*

A number of studies have documented the high dynamism of the Mexican labor market, in terms of experiencing large flows in and out of the formal sector and also in and out of the labor market (Maloney, 1999; Pagés and Stampini, 2009).

In such context, an important question is whether the introduction of *SP* leads to a change in labor market dynamics, and in particular, whether the increase in informal employment associated with the introduction of *SP* is related to larger inflows into informality (and out of formal jobs) and/or smaller outflows out of informality.

We address this question estimating the following specification at the individual level:

$$LM_{ijt} = \lambda Z_t + \phi LM_{ijt-1} + \beta SP_{jt} + \delta LM_{ijst-1} * SP_{jt} + \psi X_{ijt} + \gamma SS\_cov_{ijt} + \gamma SP_{jt} * SS\_cov_{ijt} + \varepsilon_{ijt} \quad \text{where } E[\varepsilon_{ijt} | j_t] = 0 \quad (4)$$

that is, we assess how the probability of individual *i*, of being in a given labor market status, in municipality *j* at time *t* ( $LM_{ijt}$ ), is affected by *SP*, conditional on its past labor market status. We consider six possible labor market states: salaried employment with SS, salaried without SS, out of labor force, unemployed, self employed and other types of informal employment such as unpaid

family work. We also control for whether the individual has access to SS through someone else in the household ( $SS\_cov_{ijt}$ ) and for the interaction of this variable with  $SP$ . We estimate each equation for each labor market status separately; there are no efficiency gains from estimating the model jointly because the set of regressors are the same across equations.

The results are presented in Table 7. Column (1) shows a decline in flows from informal salaried to formal salaried employment with the introduction of  $SP$ .<sup>13</sup> The magnitude of the change in flow is substantial, 3.1 percentage points (on a base of 15 percent, so a reduction of 20 percent). We also see a small reduction in transitions from self-employment to formal sector jobs (0.2 percentage points), a sizeable reduction in the transitions from inactivity to formal salaried employment (1.1 percentage points), and an even larger decline in flows from unemployment to formal salaried jobs. These results provide evidence that  $SP$  lowers the incentives of moving to formal wage employment from informal jobs, inactivity or unemployment.

In turn, Columns (2), (3) and (4) assess whether  $SP$  affects inflows into the informal sector. We find evidence of a very small increase in the inflow to salaried informal jobs out of formal employment (0.5 percentage points) and no evidence of an increase in the inflow to self-employment or other informal jobs out of formal salaried jobs as a consequence of  $SP$  rollout. So, while it may be argued that  $SP$  makes informal jobs more attractive, such change has not been sufficient to persuade a large share of formal workers to switch into informality.

There are also some noteworthy effects on the transitions to inactivity (Column 5). The rollout of  $SP$  is associated with an increase in the inflows to inactivity from formal salaried employment (0.5 percentage points). The increase in inflows to inactivity is particularly evident among self-employed workers (2.3 p.p.) and workers in other informal jobs (1.8 p.p.). In addition,  $SP$  is associated with an increase in the share of workers who remain in inactivity one period after (1 p.p.). We also find an increase in the persistence of unemployment and a decline in all unemployment outflows as a result of the  $SP$  suggesting that it reduces workers incentives to get out of unemployment and into jobs.

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<sup>13</sup> Calculated as the difference between the coefficient on  $SP*Wage$  employed without SS and the coefficient on  $SP$  i.e.  $0.084-0.053=0.031$ .

All in all, the evidence indicates that the reduction in formality has more to do with a decline of transitions into formality than an increase in the outflow from formality. This is consistent with *SP* having reduced the incentives of workers to actively search for formal jobs, while not providing incentives strong enough to dissuade many formal workers to switch into informality. The results also suggest that *SP* is associated with some significant income effects, reducing the incentives of people to be employed or actively search for jobs.

#### *Effects across types of households*

As indicated before, *SP* and *SS* are not direct substitutes. While *SS* offers a better health care package, and some additional benefits (most notably access to post-retirement income in the form of a pension, whose amount varies depending on the number of contributions), *SS* requires contributing a fairly large share of income to the program. Instead, *SP* offers only health benefits, but it is free of charge and covers more members of a household. Given these characteristics it is expected that the introduction of *SP* would reduce incentives to participate in *SS* differently across types of households, depending on income, size of the household, or other characteristics. In order to test this prediction, we run our basic estimates at the household level, for different types of households. Results are presented in Table 8.

When separating according the education and education and gender of the household head, we find the effects to be more important for households headed by individuals with lower levels of education, and this is true both for male and female headed households. As in Juarez (2008) we find the effects to be larger for less educated women than for males.

By age, the effect of *SP* is measured to be lower across households headed by younger and older people and stronger for those headed by middle-age people. The lower reaction of older workers it is not surprising considering that older workers value more pensions which are only provided in the formal sector. In addition, they consume more health care and therefore more likely to weight on the quality of the package. On their part, younger workers tend to be healthier and overall less concerned with health insurance of any kind.

The evidence also indicates that larger households react quite differently to the introduction of *SP* than smaller ones. For households of more than 5 members, the introduction of *SP* reduces the probability of formality while the effect is not apparent for smaller households, which confirms that *SP* is more valuable to households in which the program allows the extension of coverage to more members.

### *Effects on Wages*

Lastly, we explore whether wages adjust to the introduction of *SP*. Using the subsample of people who remain in paid employment from one quarter to the next, we estimate the following specification:

$$\Delta \log w_{ijt} = \beta SP_{jt} + \gamma_{IF} D_{IFijt} + \gamma_{FI} D_{FIijt} + \gamma_{II} D_{IIijt} + \phi_{IF} SP_{jt} * D_{IFijt} + \phi_{FI} SP_{jt} * D_{FIijt} + \phi_{II} SP_{jt} * D_{IIijt} + \mu_{ijt} \quad (5)$$

Where  $\Delta \log w_{ijt}$  denotes the log difference of wages from period  $t-1$  to period  $t$ ;  $D_{MKijt}$  denotes a set of dummies indicating the transit from state  $M$  in  $t-1$  to state  $K$  in quarter  $t$ , and where the states possible are: salaried with  $SS$ ,  $F$  and working in the informal sector,  $I$  (salaried informal, self-employed, other informal) and the possible transitions are four:  $FF$ ,  $FI$ ,  $IF$  and  $II$ . This specification assesses the wage gains or losses associated with changing states and whether those gains or losses have been on average modified by the introduction of *SP*.

The results are reported in Table 9. Focusing first on wage gains or losses prior to the introduction of *SP*, we measure that relative to the omitted transition (remaining in the formal sector), transiting from an informal to a formal job is associated with a further 1.5 percentage point (10 percent) increase in wages, while transiting from a formal to an informal job is associated with a 1.4 percentage point decline. In contrast, remaining in the informal sector leads to a minor wage decline of 0.2 percentage point relative to remaining in the formal sector.

As discussed, in an integrated labor market the introduction of *SP* should have led to a decline in wages in informal jobs and an increase of wages in formal jobs to compensate for an increase in fringe benefits in the informal sector. The interaction with *SP* is only statistically significant for those who remain in the informal sector and suggest a wage decline relative to those who remain

in a formal job. This is consistent with the transition results: *SP* discouraged some people from exiting from the informal sector and transiting to formal jobs, remaining instead in the informal sector. In turn, wages in the informal sector declined somewhat relative to those in the formal sector.

All in all, the results indicate that the introduction of *SP* encouraged a moderate increase in informality, which increased faster as the program got better known and then declined somewhat. At the individual level, the effects mostly came from reduced flows into formal jobs, rather than through higher outflows from formality. These effects are more pronounced for individuals who are covered by *SS* through someone else in the household or are not heads, spouses of heads, or dependents of heads. At the household level, households headed by low skilled household heads (particularly if female) or large households show a higher propensity to stop contributing to *SS* with the introduction of *SP*.

The results also suggest that informal workers paid partly for this benefit through a moderate reduction in their wages relative to those of formal workers.

Finally, our estimates also reveal that the provision of *SP* discouraged some workers from participating in the labor market and from actively searching for jobs. Such effects are given by the reduction of outflows from inactivity, and also by an increase in inflows to inactivity, particularly from some types of informal sector jobs. They are also noticeable in the reduction of unemployment outflows to all types of jobs.

## **8. Conclusions**

The question of whether social protection programs distort incentives to encourage workers to be in the informal sector is of considerable interest to current policy makers. There is a worthy commitment to provide expanded health care coverage, but a concern not to provide disincentives to work in the formal sector. Providing evidence on the extent to which such incentives appear to be operating is an important contribution. Other papers looking at this question have provided

mixed results. Our results, controlling for decisions at the household level – given that coverage in *SS* varies at the household level<sup>14</sup> – and for unobserved household heterogeneity provide the best test of whether such effects appear important. We find significant and robust evidence that such disincentives are at play, and that they are stronger in larger households, in households with less education and for workers who are covered by *SS* through someone else. The magnitude of the effects is on the order of 0.4-0.7 percentage points decline in the size of the formal sector, with much of the adjustment happening upfront (over the initial 2 years). Rather than formal workers moving into formality, such effects are driven by a reduction of flows out of the informal sector into formal jobs.

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<sup>14</sup> Strictly for the spouses and their dependent children.

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**Table 1. Summary Statistics of the Main Variables**

Variable	N	Mean	S.D.	Min	Max
<i>Households</i>					
Covered by Social Security	3,230,194	0.45	0.50	0	1
Number of children 0-5	3,235,495	0.48	0.73	0	11
Number of children 6-13	3,235,495	0.71	0.92	0	10
Number of adults 66 or older	3,235,495	0.08	0.35	0	18
large household (more than 5)	3,235,495	0.61	0.49	0	1
<i>Individuals</i>					
Social Security	9,900,477	0.27	0.44	0	1
Male	9,912,656	0.47	0.50	0	1
Age	9,912,656	33.34	13.76	14	65
Education: secondary or more	9,912,020	0.15	0.35	0	1
Wage-employed with social security	9,912,656	0.27	0.44	0	1
Wage-employed without social security	9,912,656	0.18	0.38	0	1
Self-employed	9,912,656	0.14	0.34	0	1
Other informal employment	9,912,656	0.04	0.19	0	1
Not in the labor market	9,912,656	0.38	0.49	0	1
Unemployed	9,912,656	0.02	0.14	0	1
Working in small firm 1-5 employees	8,230,081	0.24	0.42	0	1
Working in medium firm 6-50 employees	8,230,081	0.15	0.35	0	1
Working in large firm more than 50 employees	8,230,081	0.26	0.44	0	1

Source: Author's calculations based on Mexico Labor Survey 2000q2-2009q2.

**Table 2. Rollout of *Seguro Popular***

	(1)	(2)	(3)	(4)	(5)	(6)
Sample			Municipality and period			
Dependent Variable	<i>SP</i>	<i>SP</i>	<i>SP</i>	<i>SP</i>	<i>SP</i>	<i>SP</i>
Share ss coverage (%HH) 1 year lagged	-0.01536 [0.11415]					
share ss (%individuals) 1 year lagged		-0.00188 [0.17790]				
Share ss coverage (%HH) 2 years lagged			0.01189 [0.12975]			
Share ss (%individuals) 2 years lagged				0.01721 [0.21118]		
Growth of share ss coverage (%HH)- 2 years					0.00004 [0.00003]	
Growth of share ss (%individuals)- 2 years						0.00005 [0.00006]
quarter time effects	yes	yes	yes	yes	yes	yes
municipality dummies	yes	yes	yes	yes	yes	yes
Constant	0.58189 [0.04552]**	0.57710 [0.04432]**	0.56429 [0.05104]**	0.56463 [0.05181]**	0.56816 [0.02431]**	0.56820 [0.02431]**
Observations	8740	8740	7576	7576	7524	7524
R-squared	0.67	0.67	0.56	0.56	0.56	0.56

Standard errors clustered at the municipality level

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

Source: Author's calculations based on Seguro Popular Registry for dependent variable and on Mexico Labour Surveys

**Table 3: Percent of Households Covered by Social Security**

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Household head (HH) with less than secondary education completed	40%
HH with secondary completed or more education	55%
HH male with less than secondary education completed	40%
HH female with less than secondary education completed	36%
HH <30 years old	52%
HH 30-49 years old	51%
HH 50-65 years old	29%
Large Household ( $\geq 5$ individuals)	45%
Small Household ( $< 5$ individuals)	43%

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**Table 4. Impact of Seguro Popular on Formality<sup>1</sup>: No-Panel versus Panel**

	(1)	(2)	(3)	(4)
	Cross-Section	Cross-Section (1 obs. Per person)	Random Effects <sup>2</sup>	Household Fixed Effects <sup>2</sup>
<b>SP</b>	<b>-0.00670</b>	<b>-0.00724</b>	<b>-0.00367</b>	<b>-0.00358</b>
	<b>[0.00315]**</b>	<b>[0.00326]**</b>	<b>[0.00144]**</b>	<b>[0.00136]***</b>
Male head	0.13200	0.12969	0.13182	
	[0.00397]***	[0.00378]***	[0.00402]***	
Age of head	-0.00570	-0.00512	-0.00525	
	[0.00033]***	[0.00034]***	[0.00033]***	
head with primary	0.08876	0.09258	0.08357	
	[0.00313]***	[0.00323]***	[0.00279]***	
head with less than secondary	0.21814	0.22236	0.20210	
	[0.00596]***	[0.00626]***	[0.00560]***	
head with secondary	0.24393	0.24055	0.23033	
	[0.00793]***	[0.00818]***	[0.00755]***	
head with terciary	0.35490	0.35925	0.33781	
	[0.01156]***	[0.01142]***	[0.01054]***	
N.children 0-5	-0.00984	-0.00628	-0.00327	0.00228
	[0.00104]***	[0.00125]***	[0.00085]***	[0.00088]***
N. children 6-13	0.00261	0.00525	0.00290	0.00157
	[0.00096]***	[0.00104]***	[0.00078]***	[0.00085]*
N. elderly +66	-0.01313	-0.00157	-0.01051	-0.00860
	[0.00129]***	[0.00209]***	[0.00073]***	[0.00071]***
Municipality dummy	yes	yes	yes	no
Survey dummy	yes	yes	yes	no
Controls	yes	yes	yes	yes
Constant	0.27522	0.40307	0.42945	0.43052
	[0.08990]***	[0.11014]***	[0.05168]***	[0.05002]***
Observations	3230029	926827	3230029	3230194
R-squared	0.15	0.14	0.0010	0.0008
Number of id_i			929339	929364

<sup>1</sup> Household level: A formal household is one that is covered by Social Security either by the job of the household head or spouse of head.

Controls include: quarterly log GDP, quarterly inflation, and a state trend

Standard errors clustered at the municipality level in brackets

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

<sup>2</sup> Hausman test for H0: "difference in coefficients -fixed effects (FE) versus random effects- not systematic" yields a Chi2=969; Prob>chi2 = 0.0000. The null H0 is rejected.

FE will be used in the rest of the analysis.

**Table 5. Impact of Seguro Popular on Formality<sup>1</sup>: Extensions. Fixed Effects Estimation**

	(1)	(2)	(3)	(4)	(5)	(6)
				excluding DF	excluding 2004q4	
<b>SP</b>	<b>-0.00654</b>	<b>-0.00358</b>	<b>-0.00084</b>	<b>-0.00378</b>	<b>-0.00376</b>	
	[0.00128]***	[0.00136]***	[0.00143]	[0.00136]***	[0.00140]***	
logSP						<b>-0.00123</b>
						[0.00050]**
logSPsq						<b>0.00011</b>
						[0.00005]**
n.children 0-5	0.00264	0.00228	0.00228	0.00219	0.00218	0.00228
	[0.00089]***	[0.00088]***	[0.00088]***	[0.00089]**	[0.00087]**	[0.00088]***
n. children 6-13	0.0016919	0.00157	0.00155	0.00154	0.00168	0.00157
	[0.00085]**	[0.00085]*	[0.00085]*	[0.00086]*	[0.00087]*	[0.00085]*
n. elderly +66	-0.0094255	-0.00860	-0.00871	-0.00843	-0.00859	-0.00860
	[0.00072]***	[0.00071]***	[0.00070]***	[0.00072]***	[0.00071]***	[0.00071]***
Controls	yes	yes	no	yes	yes	yes
State trends	no	yes	no	yes	yes	yes
Quarterly time effects	no	no	yes	no	no	no
Constant	1.10744	0.43052	0.45948	0.42690	0.42253	0.44150
	[0.04275]***	[0.05002]***	[0.00181]***	[0.05069]***	[0.05121]***	[0.05064]***
Observations	3230194	3230194	3230194	3146463	3168847	3230194
Number of id_i	929364	929364	929364	906008	923643	929364
R-squared	0.0005	0.0008	0.00086	0.00076	0.00077	0.00077

<sup>1</sup> Household level fixed effects (FE)

Controls include: state trend, quarterly log GDP, quarterly inflation

Standard errors clustered at the municipality level in brackets

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Column (2) will be the basic specification used in the reminding of the analysis

**Table 6. Impact of Seguro Popular on Formality: Individual versus Household. Fixed Effects Estimation**

Dependent Variable	(1) SS	(2) SS	(3) SS	(4) SS	(5) SS	(6) SS	(7) SS	(8) SS	(9) SS
	All individuals	Household heads (HH)	Spouses of HH	Spouses of HH if HH is contributing.	Spouses	Members of households other than heads, spouses and their dependents	Children of HH covered by their parents	Single HH (without dependents)	Spouses- IV ESTIMATION
Sample									
<b>SP</b>	<b>-0.00608</b>	<b>-0.00296</b>	<b>-0.00443</b>	<b>-0.00708</b>	<b>-0.00352</b>	<b>-0.01534</b>	<b>0.00122</b>	<b>-0.00268</b>	<b>0.00894</b>
	[0.00105]***	[0.00110]***	[0.00119]***	[0.00169]***	[0.00133]***	[0.00184]***	[0.00136]	[0.00225]	[0.00332]***
SS head					0.03550				0.50681
					[0.024937]***				[0.04708]***
<b>SP*SS head</b>					<b>-0.00256</b>				<b>-0.02806</b>
					[0.0015046]**				[0.00736]***
HH controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Constant	0.27450	0.36861	0.22923	0.19499	0.22655	0.46580	-0.21307	0.15998	-0.02793
	[0.04160]***	[0.05171]***	[0.04559]***	[0.07791]**	[0.04598]***	[0.06221]***	[0.05776]***	[0.08191]*	[0.05257]
Observations	6076859	3214851	2501360	1019940	2365615	2778521	667772	849414	2365429
Number of id_i	2098150	935480	723184	359412	689813	906720	250467	274580	689780
R-squared	0.00026	0.00051	0.00052	0.0008	0.0029	0.00094	0.0022	0.0004	0.001

Robust standard errors in clustered at the municipality level in brackets

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

HH controls include number of children age 5 or younger, a number of children ages 6-12, and number of adults older than 65

Controls include: state trend, quarterly log GDP, quarterly inflation

**Table 7: Effects of SP on Transitions**

Dependent Variable	(1) wage- employed with social security (t)	(2) wage- employed without social security (t)	(3) Self- employment (t)	(4) Other informal employment (t)	(5) Inactivity (t)	(6) Unemployed (t)
<b>SP</b>	<b>0.05341</b>	<b>0.04829</b>	<b>0.06671</b>	<b>0.08119</b>	<b>0.01033</b>	<b>0.08593</b>
	[0.00488]***	[0.00765]***	[0.00458]***	[0.00789]***	[0.00244]***	[0.00605]***
SS_covered	0.00137	-0.03883	-0.01242	-0.05060	0.10984	-0.00931
	[0.00201]	[0.00131]***	[0.00082]***	[0.00103]***	[0.00176]***	[0.00046]***
<b>SS_covered*SP</b>	<b>-0.00350</b>	<b>-0.00426</b>	<b>0.00598</b>	<b>0.01646</b>	<b>-0.01249</b>	<b>-0.00400</b>
	[0.00156]**	[0.00119]**	[0.00085]**	[0.00080]**	[0.00161]**	[0.00048]**
wage-employed with social security (t-1)		-0.41444	-0.49790	-0.16099	-0.67201	0.00132
		[0.00794]***	[0.00467]***	[0.00573]***	[0.00365]***	[0.00137]
<b>SP*wage-employed with social security (t-1)</b>		<b>-0.04354</b>	<b>-0.07182</b>	<b>-0.08064</b>	<b>-0.00594</b>	<b>-0.08889</b>
		[0.00686]**	[0.00581]**	[0.00829]**	[0.00331]*	[0.00615]**
wage-employed without social security (t-1)	-0.52258		-0.39516	-0.14022	-0.57772	0.00647
	[0.00526]***		[0.00361]***	[0.00493]***	[0.00376]***	[0.00114]***
<b>SP*wage-employed without social security (t-1)</b>	<b>-0.08418</b>		<b>-0.07991</b>	<b>-0.08721</b>	<b>-0.01459</b>	<b>-0.08094</b>
	[0.00613]**		[0.00576]**	[0.00808]**	[0.00377]**	[0.00573]**
self-employed (t-1)	-0.68054	-0.39553		-0.14970	-0.61109	-0.00178
	[0.00485]***	[0.00745]**		[0.00526]**	[0.00295]**	[0.00131]
<b>SP*self-employed (t-1)</b>	<b>-0.05572</b>	<b>-0.03848</b>		<b>-0.07260</b>	<b>0.01313</b>	<b>-0.08514</b>
	[0.00544]**	[0.00658]**		[0.00799]**	[0.00229]**	[0.00588]**
Other informal employment (t-1)	-0.69332	-0.41982	-0.39876		-0.48076	-0.00820
	[0.00448]***	[0.00839]***	[0.00413]***		[0.00417]***	[0.00143]***
<b>SP*Other informal employment (t-1)</b>	<b>-0.03988</b>	<b>-0.03186</b>	<b>-0.04750</b>		<b>0.00848</b>	<b>-0.08567</b>
	[0.00469]**	[0.00728]**	[0.00530]**		[0.00431]*	[0.00591]**
Inactivity (t-1)	-0.65464	-0.42372	-0.43325	-0.15418		0.00011
	[0.00395]***	[0.00783]***	[0.00319]***	[0.00482]***		[0.00128]
<b>SP*Inactivity (t-1)</b>	<b>-0.06474</b>	<b>-0.04993</b>	<b>-0.06729</b>	<b>-0.07879</b>		<b>-0.08072</b>
	[0.00490]**	[0.00730]**	[0.00486]**	[0.00772]**		[0.00582]**
unemployed (t-1)	-0.26860	-0.10345	-0.14093	-0.04636	-0.04371	
	[0.00692]***	[0.00582]***	[0.00389]***	[0.00314]**	[0.00542]***	
<b>SP*unemployed (t-1)</b>	<b>-0.25642</b>	<b>-0.17483</b>	<b>-0.25960</b>	<b>-0.15486</b>	<b>-0.30345</b>	
	[0.00976]**	[0.00829]**	[0.00909]**	[0.00923]**	[0.00904]**	
HH controls	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Constant	0.67001	0.54757	0.57131	0.43996	1.31916	0.16385
	[0.04636]***	[0.06620]***	[0.03234]***	[0.03154]***	[0.05022]***	[0.02648]***
Observations	7005475	7005475	7005475	7005475	7005475	7005475
Number of id_i	0.56	0.24	0.37	0.08	0.54	0.02
R-squared						

Controls include: state trend, quarterly log GDP, quarterly inflation

Standard errors clustered at the municipality level in brackets

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 8: Effects by type of household according to household characteristics Fixed Effect Estimation.<sup>1</sup>**

	SP	[se]	HH controls	Controls	Constant	[se]	Obs.	N. of households	R-sq.
(1) Male household head	-0.00360	[0.00142]**	yes	yes	0.47213	[0.05752]***	2506619	722459	0.0006
(2) Female household head	-0.00378	[0.00284]	yes	yes	0.31452	[0.09884]***	723575	209264	0.002
(3) HH with less than secondary completed	-0.00541	[0.00163]***	yes	yes	0.35269	[0.06181]***	2186504	629494	0.0009
(4) HH with secondary completed or more	0.00204	[0.00180]	yes	yes	0.59307	[0.09127]***	1043690	311432	0.0007
(5) Male HH with less than secondary	-0.00486	[0.00179]***	yes	yes	0.37516	[0.07118]***	1661323	480085	0.0007
(6) Female HH with less than secondary	-0.00721	[0.00324]**	yes	yes	0.28431	[0.11956]**	525181	150795	0.0023
(7) HH less than 30 years old	0.00039	[0.00248]	yes	yes	0.55318	[0.15428]***	591366	203882	0.0008
(8) HH 30-49 years old	-0.00586	[0.00179]***	yes	yes	0.47311	[0.06594]***	1818573	512787	0.0007
(9) HH 50 or more years old	-0.00164	[0.00190]	yes	yes	0.31448	[0.07636]***	820255	221431	0.0014
(10) Large Household	-0.00363	[0.00143]**	yes	yes	0.43989	[0.05066]***	2832985	673640	0.0008
(11) Small Household	-0.00130	[0.00521]	yes	yes	0.24806	[0.23519]	397209	255724	0.0010

<sup>1</sup> Household level

HH controls include: number of children 0-5 years old, number of children 6-13 years old, number of adults 66 or older

Controls include: state trend, quarterly log GDP, quarterly inflation

Standard errors clustered at the municipality level in brackets

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

**Table 9: Effects of *SP* on Wages**

	(1)
	$\log w(t) - \log w(t-1)$
<i>SP</i>	0.00148 [0.00195]
D_IF	0.01466 [0.00853]*
<i>SP</i> *D_IF	-0.00331 [0.00470]
D_FI	-0.01397 [0.00858] <sup>+</sup>
<i>SP</i> *D_FI	-0.00304 [0.00463]
D_II	-0.00179 [0.00109] <sup>+</sup>
<i>SP</i> *D_II	<b>-0.00302</b> <b>[0.00146]**</b>
Individual and Household Head Controls	yes
Macro Controls	yes
Constant	0.14238 [0.07560]*
Observations	2843538
R-squared	0.0004

Robust standard errors clustered at the municipality level in brackets

+ significant at 10.35%; \* significant at 10%; \*\* significant at 5%

Household head controls include: number of children 0-5 years old, number of children 6-13 years old, number of adults 66 or older

Macro controls include: state trend, quarterly log GDP, quarterly inflation