From Caring to Work: The Labor Market Effects of Noncontributory Health Insurance.

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Abstract

Health reforms in low and middle-income countries usually include the provision of free or subsidized health insurance. In this paper, I examine whether this type of insurance encourages employment by freeing up resources previously used by households to cope with health shocks. To isolate the causal effect of providing free health insurance I use a difference-in-differences design that exploits municipal (county) level variation in the rollout of Mexico's Seguro Popular (SP). My main finding is that SP increases labor supply by retaining workers in the labor force. I propose that this occurs because SP reduces the time burden that dependents in poor health impose on caregivers. Consistent with this channel, I find that the labor supply response triggered by SP is driven by women, in particular those with caregiving responsibilities. Time use estimates provide additional evidence of the mechanism, as they illustrate that the increase in female labor supply is due to the reallocation of time from caregiving tasks at home to work in the labor market. The finding that SP increases labor supply is especially important, because it shows that the provision of subsidized health insurance need not entail an efficiency loss in the labor market. Specifically, I show that the increase in the share of workers in jobs without employer-based health insurance, that is, in informal jobs, is not driven by workers moving to less productive informal jobs, but by informal workers staying in the labor force. Accordingly, back-of-the-envelope calculations suggest that SP has led to a gain of one quarter of one percent of GDP.

Keywords: Health insurance; Crowd-out; Informality; Female labor supply.
JEL codes : I13, I15, I38, J21, J22, O12, O17.

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1 Introduction

Illness is one of the greatest and least predictable shocks to the economic opportunities of the poor. Risk pooling strategies in the form of social safety nets, access to formal insurance markets, and even informal networks to insure against illness are often lacking or completely unavailable to the poor.[1] Accordingly, it has been shown that even when households are able to smooth consumption following a health shock, they do so by employing costly risk-coping strategies. These strategies include depleting savings, accumulating expensive debt, selling durable assets, reducing investments in human capital, and reallocating labor away from productive activities.[2] These responses to risk mean that health shocks among the poor not only reduce their welfare when they occur, but are persistent in the long run.

Given the scope for welfare-improving government intervention,[3] a number of countries have introduced large-scale health reforms over the last three decades that usually entail the provision of free or heavily subsidized health insurance.[4] Despite a body of empirical work suggesting that these types of programs are capable of improving health outcomes, especially among the poor,[5] it is still unclear whether they are welfare improving overall. In particular, concerns over behavioral responses in the labor market, and subsequent efficiency losses, have figured prominently in the policy discussions that surround the introduction of publicly subsidized health insurance, Levy and Schady (2013).

In this paper, I expand on the discussion of the labor market effects of these programs by showing that they are also capable of creating an efficiency-enhancing response in the labor market. Specifically, I show that publicly subsidized health insurance increases labor supply, and that this occurs because insurance reduces exposure to health shocks, thereby enabling households to reallocate resources from costly coping strategies to productive activities.

I provide some of the first evidence of these relationships by recovering causal estimates of the effect that publicly subsidized health insurance has on labor supply, and specifically on the decision to exit or enter employment. I deal with the potential endogeneity of access to health insurance by exploiting the variation created by the municipal-[6] level rollout of Mexico's Seguro Popular (SP). This nationwide program provides free health insurance and better access to health services to those not covered by employer-based schemes. Seguro Popular represents the

[1] Fafchamps and Lund (2003); De Weerdt and Dercon (2006).

[2] See, for example,Gertler and Gruber (2002); Gertler et al. (2009); Mohanan (2013); Islam and Maitra (2012); Wagstaff (2007); Wagstaff and Lindelow (2014); Kochar (1999); Sparrow et al. (2014).

[3] Chetty and Looney (2006); Rosenzweig and Wolpin (1993); Morduch (1999).

[4] For example, Afghanistan, Brazil, Chile, China, Colombia, Dominican Republic, Ghana, India, Indonesia, Israel, Mexico, Peru, Rwanda, Taiwan, Tanzania, Thailand, Turkey, and Vietnam.

[5] See, for example,: Newhouse (1993); Dow et al. (1997); Gruber et al. (2012); Finkelstein et al. (2012).

[6] A municipality is a second-level administrative unit. It is the equivalent of a county in the US. Mexico has 2,455 municipalities.

largest expansion of non-contributory health insurance coverage in the Americas with over 52.6 million enrollees, Knaul et al. (2012).

I use a difference-in-differences design that compares changes in the labor market outcomes of individuals residing in municipalities already reached by the program with individuals in municipalities not yet reached. In order to isolate plausibly exogenous variation in SP rollout, I focus on two years in the middle of the rollout period for which identifying assumptions are likely to hold. In particular, I show that the municipalities reached by the program in 2004 and 2005 had similar pre-program trends, that SP was not anticipated, and that these municipalities were similar at baseline in terms of observables.

I make use of a number of datasets to address the threat posed by unobserved time-varying factors correlated to the introduction of the program and to labor market outcomes. In particular, I control for changes in economic activity at municipal level using the lights-by-night satellite imagery, and draw on recently released municipal administrative records to account for local government expenditure that could potentially confound estimates of SP's impact.

My main finding is that SP increases labor supply by reducing the exit flow from employment, that is, by helping workers to stay employed. This labor supply effect is efficiency enhancing and of a significant economic magnitude. A simple counterfactual simulation suggests that the program led to a gain of as much as one quarter of one percentage point of GDP. I structure my results as follows to show that this finding is consistent with past papers, which conversely posited that SP has led to a loss in efficiency.[7]

First, I consider the argument that SP encourages workers to seek jobs without employer-based health insurance, that is, in the context of Mexico informal jobs.[8] This reallocation of labor is expected to reduce efficiency because informal jobs are often described as less productive.[9] Next, I explore the effect of SP on the share of workers in formal jobs, an aggregated labor market metric that depends on both the decision to seek employment and the choice between formal and informal jobs.

Then, having produced results consistent with the findings of past papers, I use labor market transitions to decompose the effect of SP. I find that whereas SP actually increases the relative size of the informal sector, this effect is not driven by workers moving to less productive informal jobs, but by informal workers being retained in the labor force. This finding is consistent with the idea that healthier and better-off workers on the margin between formal and informal employment are unresponsive to the program, while those most marginalized and excluded from formal employment benefit disproportionately from SP.

[7] See Bosch et al. (2012) for a review of the literature.
[8] The relevant dimension of informality for this article is access to employer-based health insurance.
[9] See, for example, Busso et al. (2012).

I pin down a key version of the proposed mechanism, by taking advantage of the fact that SP is well equipped to improve the health of children and the elderly and that, as revealed by survey data before the introduction of SP, uninsured households spent a large fraction of their time caring for sick dependents. Accordingly, I hypothesize that by reducing the time burden that dependents in poor health impose on caregivers, SP increases their ability to join or remain in the labor market.

I further draw on the fact that this response depends on the division of labor within families in order to narrow down the demographic groups that should be disproportionately affected by SP under the proposed mechanism. My analysis focuses on women, not only because this group bears the brunt of caregiving work in Mexico, but also because, as revealed by survey data, women can be frequently be observed diverting their time away from the labor market in order to respond to family needs.

Given that women with caregiving responsibilities who have not fully specialized in this task are the most likely to respond to SP, I also use their marital status, and household composition, to further identify key groups whose labor supply should be disproportionately affected by the program. Consistent with the proposed mechanism, which I formalize in a simple model, my estimates of heterogeneous effects show that the increased retention of informal workers in the labor force is driven by women and primarily by those with caregiving responsibilities. My time-use estimates further indicate that this occurs because SP enables women to substitute caregiving time at home for time in the labor market.

I also test four alternative mechanisms that could create this pattern in the time-use data. First, I rule out that SP operates by reallocating time among household members by showing that households both reduce the total hours spent caregiving and increase the total hours spent in the labor market. Second, I show that SP has not led to delayed childbearing. Third, I illustrate that SP is unlikely to drive an improvement in the health of the working-age population by reviewing the health literature, and showing that SP does not disproportionately benefit workers in more physically demanding occupations. Fourth, I consider the possibility that SP frees up savings to enable capital-constrained entrepreneurs to start their own business. Specifically, I use transitions within the informal sector to show that, while SP enables women to take on new, profitable activities, this effect is too small to account for the overall impact of the program on the labor market.

This paper contributes to three strands of literature. First, it contributes to the literature that analyzes the economic consequences of health shocks[10] by showing that the provision of health insurance enables households to reallocate time from inefficient risk management strategies towards productive activities.

[10] See the papers listed in footnote 2.

Second, the finding that SP increases female labor supply is important both because it provides a channel for the operation of the program, and also because it highlights a previously undocumented consequence of publicly subsidized health insurance. It thereby connects the literature on the labor market effects of health insurance with a wider body of literature that assesses the role played by economic development in the reduction of gender inequalities.[11]

Third, it contributes to the literature on the labor market effects of social insurance in developing economies by showing that relative increases in the size of the informal sector do not necessarily imply a loss of efficiency.[12] My findings suggest that social insurance in Mexico works primarily by affecting the labor force participation margin, and in particular by reducing the exit flow from employment among those most marginalized. These findings thereby imply that the behavioral response triggered by the incentive structure of SP on the labor market is on net efficiency enhancing.

The rest of this paper is organized as follows. Section 2 presents a brief background to SP and the Mexican health care system. Section 3 discusses the possible impact of SP on the labor market, and develops a model that guides the empirical findings. Section 4 discusses the data and the identification strategy. Section 5 presents the main results. Section 6 investigates the channels through which SP affects employment. Section 7 provides supporting evidence consistent with the identification assumptions. Section 8 concludes.

2 Background to social protection in Mexico

Prior to the introduction of SP, access to health services in Mexico depended primarily on labor market status.[13] The majority of those employed in private sector jobs with employer-based health insurance were covered by IMSS while public sector employees had ISSSTE coverage.[14] These institutions differ from employer-based health insurance schemes in other countries in several respects. First, both IMSS and ISSSTE are public federal institutions financed by a payroll tax.[15] Second, these institutions are vertically integrated, serving the dual role of social security funds and health providers. Third, members are restricted to using their own plan's health providers and have no guaranteed package of services. Fourth, health insurance cannot be subscribed to separately from other welfare benefits such as pensions and disability

[11] See Duflo (2011) for a review of this literature.

[12] See, for example, in Mexico:Aterido et al. (2011a); Azuara and Marinescu (2013); Bosch and Campos-Vazquez (2010). In Colombia: Camacho et al. (2013). In Thailand: Wagstaff and Manachotphong (2012).

[13] Private insurance has always been available, but less than 1% of the population has it, OECD (2005).

[14] Known by its acronym in Spanish: Instituto de Seguridad y Servicios Sociales de los Trabajadores del Estado. The armed forces and the state-run petroleum company have their own funds.

[15] Social security contributions are roughly equivalent to 31.5 % of the average wage, with approximately a third of that amount earmarked for health care, Azuara and Marinescu (2013).

insurance.[16] Fifth, health insurance coverage is automatically extended to the member's nuclear household.[17]

Those not employed or in jobs without employer-based health insurance (i.e. informally employed) had access to some health services through a network of Ministry of Health (MoH) providers. These services, however, were limited both because user fees at the point of service restricted access, and because the system was severely underfunded. In 2000, IMSS expenditures per capita were double those of the MoH network, Lakin (2010).

Not surprisingly, these asymmetries in health service access and quality resulted in large health inequalities between the insured and the uninsured. The Mexican government launched a comprehensive health reform in a move to bridge this gap. Following a pilot phase between October 2002 and December 2003, Seguro Popular (SP) was introduced in January 2004 as the flagship of the new general health law. All Mexican residents not covered by employer-based health insurance were eligible to enroll in the program. The program is provided free of charge to the overwhelming majority of members.[18]

SP represented a departure from the status quo, as it raised public health expenditures from 2.4 to 3.1 percent of GDP between 2000 and 2009. It also reduced inequalities in public health expenditures by redirecting federal transfers, which provided up to 80% of the additional funds. Basically, the law modified the transfer rules to a fixed quota per member, (roughly 12% of an annual minimum wage). As a result, the ratio in per capita public health expenditures between the insured and the uninsured was reduced from 2.1. to 1.2

The program was important for households because it enabled them to transfer risk to the state, and because it improved their access to health services. In particular, the program removed a barrier to access by eliminating the fees at the point of service. It also created an entitlement by introducing the first ever guaranteed package of services. The original package included 84 interventions, ranging from routine check-ups to third level surgeries, and a drug package deemed capable of covering 90% of the disease burden. Over time, the package has been gradually upgraded and now offers 248 interventions and 522 drugs.

In addition to the basic package of services, SP members are also protected by the Catastrophic Health Expenditure Fund. This fund was introduced alongside SP. It provides coverage for 58 high-complexity interventions that range from cancers prevalent in children to HIV/AIDS. The fund enhances the financial protection component of SP, and improves its efficiency by creating

[16] These bundled services include: life insurance, retirement pensions, disability benefits, housing loans, severance payments and in-kind transfers such as sports, cultural facilities and day care services.

[17] Specifically, IMSS extends coverage to: the spouse or partner, children up to the age of 16, or up to 25 years old if they are economically dependent or in school.

[18] It was originally envisioned that members would pay a premium based on a sliding income scale, with the poorest beneficiaries being exempt from contributing. However, the MoH reports that, in practice, less than 1% of members have ever paid for the program.

a national risk pool for these particularly expensive conditions. Given their level of integration and their simultaneous introduction, the following analysis does not distinguish between them. However, they serve to highlight that SP was designed to deal with the dual burden created by the prevalence of both communicable and non-communicable diseases.

3 Theoretical impacts on SP's labor market effects

The introduction of SP is expected to potentially affect two labor market decisions. First, it may influence employed individual's choices between formal and informal jobs, i.e. the formal-informal work margin. Second, it may change the working-age population's decisions to exit or enter the labor market, hereafter called the participation margin.

3.1 SP and the formal-informal work margin

The effect of SP on the formal-informal work margin is often derived using a compensating wage differential model where SP acts as a transfer, Azuara and Marinescu (2013); Bosch and Campos-Vazquez (2010). In these models, the introduction of SP raises the non-monetary benefits associated with informal employment. So, as long as workers value these benefits, the theoretical predictions are that programs like SP should both encourage formal workers to become informal, and discourage informal workers from becoming formal.

Similar predictions can be obtained if SP is interpreted as an insurance product. In this case, the main consideration is that the program will alter the relative price of insurance. The seminal work of Gruber and Madrian (1994) suggests that the introduction of subsidized health insurance reduces the value of jobs with employer-based health insurance. This happens because subsidized health insurance reduces the rents derived from jobs that provide health insurance at a lower price than in the private market.[19] These rents have been shown to be an important determinant of job mobility in the US, but are unlikely to exist in Mexico. Note that as reviewed in section 2, employer-based health insurance in Mexico is financed by mandatory contributions that are the same for all firms and self-employed individuals.[20]

[19] This can occur for a number of reasons: from tax breaks to the capacity of firms to pool risk, see Currie and Madrian (1999) for a review.

[20] Private health insurance is available, but very rare. The OECD (2005) estimates that less than 1% of the population has private insurance. That said, IMSS operating rules may have created these rents for certain sub-groups of workers. For example, self-employed individuals face restrictions with respect to pre-existing conditions when they purchase health insurance directly. Similarly, workers need to contribute for at least 750 weeks (about 14 years) in order to be eligible for health insurance after retirement, OECD (2005); CLC (2010).

3.2 SP and the labor force participation margin

A labor market response to SP along the participation margin can occur through a variety of mechanisms. One key channel is that SP enables households to reallocate resources previously used to cope with health shocks to productive activities. A version of this mechanism, for which I provide empirical evidence in section 6, is that SP reduces the burden of caring for sick dependents, thereby enabling household members to reallocate time from caregiving tasks at home to work in the labor market.

The labor market response generated by SP by means of this mechanism is likely to be of first-order importance for two reasons. First, uninsured households used to rely heavily on household members taking time out to cope with illness. MXFLS health data[21] show that, before the introduction of SP, a typical household with an elderly dependent and two children under the age of seven would spend an average of 12 hours per week seeking health services and caring for sick dependents.[22]

Second, as illustrated by past literature in other settings,[23] the provision of health insurance is likely to generate the largest health gains among dependents in previously uninsured households. This follows from the greater vulnerability of this sub-group of the population, and the fact that prompt medical attention is particularly effective at improving health among these age groups.

In Mexico, as in other countries with an incomplete epidemiological transition, the greater vulnerability of this sub-group of the population stems from two sources. First, much higher exposure to communicable diseases among uninsured households, Knaul et al. (2006). Second, a much higher risk of suffering from communicable diseases for children and the elderly.[24]

Seguro Popular is well equipped to generate health gains with respect to communicable diseases, because prompt medical attention prevents these diseases from becoming chronic, and reduces the likelihood of their dissemination.

Consistent with the idea that SP can give rise to improvements in the health of dependents, experimental evidence finds that SP has increased health care utilization by as much as 40% of the pre-treatment baseline, and that part of this increase is due to the substitution of private providers for SP facilities, Bernal (2014). The crowd-out of private facilities provides additional support to the idea of improvements in health. Firstly, it implies that user fees no longer deter households from seeking preventive health services. Secondly, as measured by clinical guidelines,

[21] The MXFLS is a panel survey similar to the PSID in the US.

[22] Table A2 presents health summary statistics. Table 1 takes the previous estimates to calculate the weekly hours spent caring for sick dependents. Details of this calculation can be found in the appendix.

[23] See Levy and Meltzer (2008) for a review of the literature.

[24] I illustrate this feature with mortality and survey data on infectious diseases in appendix B.

private providers serving the poor in Mexico have been found to be of lower quality than public providers.[25]

Evidence of health gains due to an increase in the use of SP services among dependents comes from studies that exploit the variation generated by the rollout of SP.[26] In particular, Pfutze (2014) finds that SP is responsible for as much as half of the decrease in infant mortality observed in Mexico in the last decade.[27] Alcaraz et al. (2012) finds that, consistent with a health channel, SP has led to considerable improvements in the performance of children in school.[28] Taken together, the findings of this strand of the literature strongly suggest that SP is likely to have had a considerable impact on the health of dependents.

In the following section, I formalize the intuition that, in a setting with missing or imperfect markets for the care of sick children and the elderly, the introduction of SP increases the number of hours spent working in the labor market, and reduces the hours spent caregiving at home.

3.2.1 SP reduces the opportunity cost of employment

Basic Setup

I explore the mechanisms between the provision of government-subsidized health insurance and the standard labor-leisure choice by extending a simple partial equilibrium household model, where households maximize utility,[29] $u(\bar{c}, \bar{\ell}, h)$, with respect to per capita consumption $\bar{c}$, per-capita leisure $\bar{\ell}$, and health h.

Household members have T hours at their disposal. They may choose to sell H_m hours in the labor market at a given wage w or to use them at home, either as leisure or in home production

[25] See, for example, Barber et al. (2007); Bojalil et al. (1998, 2007)

[26] King et al. (2009) provide the first experimental evidence of the effect of SP on health outcomes. The authors fail to find evidence of a general health effect, but do not test the effect of SP on the health outcomes of children or the elderly. Bernal (2014) uses the same experimental variation on a matched dataset of hospital discharges and mortality records. He finds no evidence of SP affecting neonatal mortality, but does not test the impact of the program on infant mortality or the health of the elderly.

[27] The author estimates that SP has reduced infant mortality by as much as 5 per 1,000 live births. Interestingly, Gruber et al. (2012) find that a similar program in Thailand reduced infant mortality by 6.5 per 1,000 births. Bernal (2014) and Pfutze (2014) results on neonatal mortality are not at odds with each other as the experiment does not have the power to look at heterogeneous effects. Moreover, the experiment spans ten months, whereas Pfutze (2014) looks at the effect of SP over a period of several years.

[28] Alcaraz et al. (2012) finds that SP raises primary school student's test scores by 21.5 percent of the standard deviation of scores across municipalities. This is a large effect for a non-targeted intervention.

[29] It is reasonable to expect SP to have a greater impact on household members with considerable caregiving responsibilities and for this in turn to alter their bargaining power. Nonetheless, setting up the model in terms of a unitary household that chooses $\bar{c}, \bar{\ell}h$ makes the model more tractable while providing clear predictions that allow me to distinguish between a reduction in the burden and time reallocation decisions made among household members.

activities, H_a which have a value of $q(H_a)$.[30]

The health production function $h(H_a+\ell, SP, M_p)$ depends on the time that household members spend at home,[31] on the parameter SP, which denotes whether the household has access to the program, and on M_p, which represents other medical inputs to which the household has access. This may range from other social programs to the availability of private doctors. Note that with the introduction of the health production function $h(\cdot)$ in the utility function, leisure and home production decisions are now capable of affecting household utility directly and indirectly through their effect on health.

The functions $u(\cdot)$, $h(\cdot)$, and $q(\cdot)$ are assumed to be twice continuously differentiable, concave, and increasing in each argument. I also assume, in line with the previous reasoning, that inputs in $h(\cdot)$ are substitutes and that $u(\cdot)$ is additively separable.[32]

Households face traditional time and budget constraints and have to decide on the number of hours they will spend on market and home activities, H_m and H_a. The household maximization problem can be stated as follows:[33]

$$\max_{H_m, H_a} = u\left(\frac{H_m w + q(H_a)}{N}, \frac{T - H_a - H_m}{N}, h(T - H_m, SP, P_m)\right) \tag{1}$$

Predictions

Obtaining the first-order conditions of the household's problem and differentiating with respect to SP provides the following predictions:

$$\frac{\partial H_m}{\partial SP} > 0 \qquad \& \qquad \frac{\partial H_a}{\partial SP} < 0 \tag{2}$$

In a nutshell, the model suggests that the introduction of SP decreases the household's need to spend time on the production of health, thereby lowering the opportunity cost of hours spent in the labor market. Accordingly, these conditions imply that the introduction of SP leads to both an increase in the number of hours worked in the labor market and a decrease in the hours that household members spend on home production activities. The underlying assumption driving

[30] Introducing a market for hired labor in home production should not alter the model's predictions since non-separability comes from the inability of household members to substitute time between labor market activities and the production of health.

[31] This implies that leisure and home production are considered to be perfect substitutes.

[32] This assumption can be relaxed. The model's predictions would be the same if it were assumed instead that the value of consumption increases with health and leisure, while the value of leisure decreases with respect to health, that is: $u_{ch} \geq 0$, $u_{c\ell} \geq 0$ and $u_{\ell h} \leq 0$.

[33] Appendix D presents the model's step-by-step solution.

this result is that there is incomplete substitution between market goods and the production of health. This could be due to the perception that hiring non-household members to take care of dependents is risky, making it costly to screen and monitor any individuals who might be hired to take care of dependents when they are sick. Consistent with this interpretation, use of regular paid childcare services is uncommon and expensive in Mexico.[34]

Accounting now for household composition and the division of labor in families yields the following additional predictions. First, in households without dependents, SP should not trigger a labor market response. Second, in households where dependents greatly outnumber caregivers, SP will be less effective at raising labor supply. This could occur in both large and small households, albeit for different reasons. In large households, heavy caregiving demands lead some members to specialize in caregiving. Since these full-time caregivers act as buffers in the event of illness, other household members will be less responsive to SP. Small households with a single caregiver, who also acts as the sole breadwinner, will be less responsive to SP because this individual has a much higher degree of attachment to the labor force. Third, in households where dependents do not outnumber caregivers, members with caregiving responsibilities who do not exclusively specialize in these tasks should benefit disproportionately from SP.

The above predictions are derived under the assumption of a fixed wage rate. This assumption precludes from the model the possibility that SP might have triggered an aggregate labor supply response prompting a reduction in wages. And although my estimates of working hours may be influenced by general equilibrium effects, the existence of these effects simply implies that my estimates are lower bounds of the impact of SP. This occurs because, as long as the substitution effect dominates the income effect, the reduction in wages will lead to a reduction in hours worked.[35]

3.2.2 SP and women's employment decisions

If SP operates by reducing the burden that dependents in poor health impose on caregivers, the program will disproportionately benefit women for three reasons. First, women bear the brunt of caregiving responsibilities in Mexico. MXFLS time use estimates, presented in table A1, reveal not only that a larger fraction of women perform caregiving tasks relative to men (43% in contrast with 15%), but also that they spend more hours per week on these tasks (32

[34] Taking data from the household income and expenditure survey, Calderon (2012) shows that the proportion of households that use paid childcare services is only 3.84%, and the average payment is $MX\$975 \approx US\$75 \approx 70\%$ of the monthly minimum wage. Similarly, among uninsured households, LFS data reveals that less than 1% employ workers to provide caregiving services or help with household chores.

[35] I also assume that general equilibrium effects do not affect control municipalities. Each municipality is likely to operate as a local labor market because less than 3% of survey respondents report moving to a different municipality in order to find a or keep their current job. Moreover, in Del-Valle (2013) I find no evidence of SP having large spillover effects across municipal labor markets.

hours as opposed to 14).

Second, in the absence of SP, households frequently respond to health shocks by reallocating their time. Specifically, labor force survey estimates, presented in figure 1, reveal that family responsibilities are the cause of 44% of all job separation events observed among women.

Third, past literature shows not only that women in developing economies raise their labor market participation in response to other interventions that reduce the cost of caregiving,[36] but also that the labor supply of caregivers is specifically responsive to improvements in the health of dependents. In a closely related paper, Hanna and Oliva (2011) find that air pollution in Mexico City reduces the labor supply because of its effect on health. Their estimates also reveal that pollution disproportionately affects the labor supply of caregivers, a finding that is partly explained by the adverse effect that pollution has on the health of dependents.

Investigating the impact of SP on the labor market response of women is useful, because evidence of a differential effect across genders provides a first indication that SP is operating through the proposed mechanism. Moreover, while it is hard to identify caregivers and the extent of their responsibilities in survey data, women that differ in their marital and motherhood status provide easily identifiable groups that can be used to reformulate the predictions derived in the previous section.

Specifically, I expect to observe that women will respond to SP by increasing the time they spend in the labor market, while simultaneously decreasing the time they spend at home on caregiving tasks. Moreover, I expect the magnitude of these labor market responses to vary in relation to household composition as previously described.

In keeping with the labor economics literature, a first group of interest is single mothers, especially those living in multigenerational households. Since in Mexico, it is common for single mothers to live with their parents, it is important to account for living arrangements that could potentially alter the degree of attachment to the labor force, or the level of caregiving demands made on single mothers. The second group of interest is young women with no biological children of their own, but who may nonetheless be subject to caregiving demands because of the presence of children and elderly individuals in their household.

I discuss a number of possibilities in section 6.2 in order to establish whether SP could disproportionately benefit these groups of women by means of alternative mechanisms: (i) the reallocation of caregiving tasks among household members, (ii) delayed childbearing, (iii) own health effects, and (iv) the freeing up of other types of household resources.

[36] In Mexico, Calderon (2012) shows that the introduction of Estancias Familiares, a government program that provides a 90% subsidy on childcare services, has led to an increase in female labor force participation as well as a reduction in the time spent on childcare. Similar results are found in Colombia where Attanasio and Vera-Hernández (2004) evaluates a subsidized childcare and nutrition program, and in Chile where Contreras et al. (2010) analyzes the extension of the school day.

3.3 The dynamic effects of SP

Regardless of the mechanism through which SP operates, its effect is likely to vary over time both because it is unlikely that individuals will be immediately aware of the availability of SP, and because it is unlikely that their initial valuation of SP services will be high enough to trigger a labor market response. Consistent with this idea, figure 4 illustrates that while average municipal take-up as a fraction of the uninsured population at the time of SP introduction was 23%, it took approximately two years for half of the uninsured to sign up.

Moreover, if the program operates by improving the health of dependents as previously argued, I would expect to find that SP has a delayed impact on the labor market for two reasons. First, the increases in health care inputs occurred roughly within a year of the program's introduction.[37] Second, caregivers are unlikely to perfectly anticipate improvements in the health of dependents. Only after they observe the health benefits of SP over time can we expect them to respond in the labor market.

4 Data and Identification Strategy

4.1 Data

My source of information on the rollout of SP are administrative records from the MoH on the number of SP affiliations by municipality and quarter. The dataset covers all the municipalities in Mexico, between 2002 and 2010. I define municipalities with access to SP as those where the threshold of 10 affiliations has been reached.[38] Figure 2 maps the rollout of SP at national level, helping to visualize the space-time variation used to identify the impact of the program. The rollout of SP does not appear to follow a particular spatial pattern.

The curve in Figure 3 presents the cumulative share of municipalities offering SP for each quarter between 2002 and 2010. The bulk of the rollout occurred between the official lunch of the program in 2004 and 2007 when nearly all of Mexico's municipalities offered the program. Figure 3 also shows the dates and frequencies of the other datasets used in this paper.

To study the effect of SP on the labor market, I use repeated cross-sections from the quarterly labor force surveys (LFS) produced by the Mexican statistical office, INEGI. These include the ENE covering the 2000-2004 period and the ENOE covering the 2005-2010 period. These

[37] Bosch and Campos-Vazquez (2010) show that SP's introduction in a municipality was accompanied by a considerable increase in medical personnel within one year of implementation. Their estimates show that the provision of SP doctors and nurses per 1,000 people rose 5.5 pp. (10%) and 7.7 pp. (12%) respectively within just one year of the program's introduction.

[38] This cut-off has been used by past papers and, while arbitrary, it does not alter the estimates of the impact of SP. Results derived using cut-off points at five and one affiliations are available upon request.

surveys sample 120,000 dwellings per quarter in both urban and rural municipalities.[39] They have a focus and structure similar to that of the Current Population Survey, and they provide information on roughly half of Mexico's municipalities at any given point in time.

Using INEGI's municipal identifiers I then merge the LFS data with four other datasets. The first comes from the MoH, it includes quarterly data on SP affiliations, and yearly data on medical personnel and infrastructure. The second covers detailed yearly records on public expenditures at municipal level. This data is collected and harmonized by INEGI. The third includes detailed records on electoral results at municipal and state level. This dataset was originally collected by Banamex and CIDAC. I have since corrected and updated this dataset based on various public sources. The fourth are high resolution DMSP-OLS satellite composites. This datset is produced by NASA and NOAA and is commonly referred to as the lights-by-night dataset. I use several measures of visible band light to proxy measures of economic activity at municipal-year level.[40]

For the analyses in section 5.1, which looks at the overall effect of SP on the labor market, I aggregate the previous dataset at municipal level, and use it as a pseudo panel at that level. The LFS survey data is not designed to be representative at municipal level, but this should not give cause for concern as long as the extent to which the data is not representative is unrelated to the introduction of SP, and local level unobservable heterogeneity is accounted for. I will provide a number of robustness checks addressing this issue.

Next, in section 5.2, where I examine the effect of SP on labor market flows, I use the dataset at individual level, and construct labor market transitions by taking advantage of the rotating panel embedded in the LFS. This individual level panel enables me to track individuals for up to five quarters. Quarter-to-quarter attrition not related to the panel's rotation is in the order of 4.4%. Attrition does not pose a threat to identification as discussed in detail in section A of the appendix.[41]

An additional problem, which affects both the analysis in section 5.1 and 5.2, is that there are substantial differences in sampling and questionnaire wording between the ENE and the ENOE,[42] Combining these datasets could prompt misleading conclusions, because the variation in labor market outcomes comprises both true variation and seam-caused variation. Moreover, labor market transitions cannot be computed between 2004 and 2005, since individual level identifiers were not designed to be used across surveys. Like Calderon (2012), I will take a conservative approach and overcome this issue by dividing the analysis in two. Evidence

[39] Starting in the third quarter of 2003

[40] appendix B provides details of this calculation. See Henderson et al. (2011) for evidence on the relation between measures of light and economic activity.

[41] Specifically, I show that the probability of attrition is not related to the introduction of SP. Moreover, inverse probability weights and Manski bounds suggest that attrition does not pose a threat to identification.

[42] See INEGI (2009) for a review of the differences between the surveys.

supporting the identifying assumptions is derived using solely the ENE dataset, while the impact of SP is investigated using just the ENOE survey, which covers the post-2005 period.

4.2 Identification Strategy

Isolating plausibly exogenous variation

State governors played a central role in deciding the order in which SP was rolled out at municipal level. First, governors could decide when to sign agreements with the federal government that would grant them access to SP federal funds. Second, even though program guidelines suggested that priority should be given to poorer municipalities with sufficient medical infrastructures, in practice, governors had considerable autonomy in deciding the order in which municipalities received the program.

My conversations with Mexican policymakers suggest that labor supply considerations, and in particular female labor supply consideration, were never factored into program placement decisions. Nonetheless, a major methodological concern that remains is that placement decisions may not be orthogonal to other factors that could affect labor market outcomes. I address this concern in a number of ways, which lead me to believe that the link between SP and labor supply is causal.

I begin by arguing that plausibly exogenous variation can be isolated by focusing on the group of municipalities that received SP in the middle of the rollout period, that is, those municipalities that received the program between 2004 and 2005. A first indication of why the variation created by this group of municipalities can be used to derive an unbiased estimate of the counterfactual is presented in figure 8. The figure plots the average share of pre-program period workers in formal jobs by the year of SP introduction. The figure reveals that those municipalities that received the program between 2004 and 2005 are similar in both levels and trends. While those that received the program in 2006 are similar in trends, but not in levels, and those that received the program in 2007 are neither similar in levels nor trends. Note that illustrating this point with the share of workers in formal jobs is particularly illuminating, because unlike labor market transitions, this metric is easily observable by policymakers, and is of some political importance.

In section 7, I provide further supporting evidence in favor of using the variation created by those municipalities that received SP in the middle of the rollout period. Specifically, I show for this group of municipalities: (i) that there is no correlation between the timing of SP and the time-path of all the other labor market outcomes, (ii) that SP was not anticipated, and (iii) that these municipalities are similar in terms of observables at baseline.

I also address the threat posed by time-varying factors correlated to the timing of SP and

labor market outcomes by introducing two types of controls. First, I consider the possibility, as suggested by a number of behavioral models, that constituencies are more demanding of elected officials when they experience shocks. In this case, governors would have an incentive to respond to local economic downturns by pushing for the early implementation of SP. Since this response could potentially confound estimates of program impact, I control for changes in economic activity at municipal level. These measures are derived from the lights-by-night dataset and the LFS.

Second, governors and mayors preoccupied with gaining or maintaining an electoral edge may have also strategically deployed SP in conjunction with other government programs and/or regulations capable of affecting labor market outcomes. For example, the provision of SP could be accompanied by a simultaneous increase in public sector employment. I account for this type of factor by controlling for various measures of municipal government expenditures, as well as for party dummies for both mayors and governors.

Finally, note that federally funded programs, such as Oportunidades[43] and Estancias Infantiles,[44] are unlikely to confound estimates of program impact. This is due to the fact that, in the period of interest to this paper, these programs were either fully rolled out or not yet introduced.

Aggregate level specification

The variation created by the differential timing in the introduction of SP among the group of municipalities that received SP between 2004 and 2005 is then exploited using a difference-in-differences design. First, I investigate the overall effect of SP on the labor market using common labor market metrics aggregated at municipal level. The main estimating equation is:

$$y_{mt} = \alpha + \delta SP_{mt} + \gamma_t + \theta_m + \beta x_{mt} + \varepsilon_{mt} \tag{3}$$

where y_{mt} is the share of workers with formal jobs in municipality m at time quarter t, SP_{mt} is an indicator variable that takes the value of one starting in the first quarter in which municipality m offers SP, θ_m is a municipal fixed effect, γ_t is a time fixed effect, x_{mt} is a vector of time-varying municipal level covariates,[45] and ε_{mt} is a random error term. This is a standard two-way fixed effects regression, where identification is derived from changes in labor market outcomes correlated to changes in the introduction of SP. This research design allows me to account for both time-invariant characteristics of municipalities, and time-varying characteristics common

[43] A conditional cash transfer first introduced in 1997.

[44] A childcare subsidy introduced in 2007.

[45] A precise definition of the control variables, summary statistics, and a discussion of the characteristics of the sample can be found in appendix B

between treatment and control municipalities. The identifying assumption is therefore that any unobserved time-varying covariates that affect labor market outcomes are uncorrelated with the rollout of SP conditional on the vector x_{mt}.

Accounting for the dynamic impact of SP

Next, I adjust the previous specification in order to disentangle the effect of SP over time. This is important because, as discussed in the section 3, the effect of SP on the labor market is unlikely to be contemporaneous with the introduction of the program. Specifically, I explore the dynamic effects of the program by progressively introducing lagged treatment indicator variables into equation 3. These variables can be denoted as $SP_{m,t+l}$, where l defines the number of quarter lags.

One interesting feature of this adjusted specification, is that the sample of municipalities that can be used to estimate the impact of SP increases as I test longer lags of $SP_{m,t+l}$. This occurs because, among the group of municipalities with plausibly exogenous variation, that is, the group that received SP between 2004 and 2005, only a fraction of municipalities displays within variation in the post-2005 period given the contemporaneous definition of treatment. Since this fraction increases as I use lagged definitions of treatment, the restriction of using only ENOE data becomes less binding, and I gain statistical power with each step. This gain, however, comes at the cost of deriving estimates that are not strictly comparable to each other. For example, while I am able to use 26%, of the rollout when $l = 0$, I can use 43% of the rollout when $l = 4$.

Each of my estimates of $SP_{m,t+l}$ can be interpreted as the average effect of being exposed to SP between l quarters and some end point e. In principle the maximum end point e would be determined by the availability of data. However, estimates for time horizons longer than the duration of the rollout would implicitly rely on the assumption of stationarity. I follow McCrary (2007) and avoid making this assumption, by computing estimates derived from comparing changes in the outcomes of treated municipalities not only to municipalities that have not yet been treated, but also to municipalities that will never be treated.

Note that while every municipality in Mexico received SP it is still possible to create a never treated control group in my sample. This is achieved by restricting the longitudinal dimension so that a group of municipalities that received SP in 2006 operate as a never treated control group. Accordingly, my preferred set of estimates compares changes in the outcomes of treated municipalities both to municipalities that will be treated in the sample, that is, the group that received SP between 2004 and 2005, and to municipalities that will never be treated in the sample, that is, those municipalities that received SP in 2006.

In addition to enabling me to estimate the impact of the program for up to three years without relying on the assumption of stationarity, expanding the control group to include municipalities that received SP in 2006 enables me to increase power, and to reduce researcher degrees of freedom. The latter follows on from the fact that I am not choosing the end point e. This end point is jointly determined by the definition of treatment and by the condition that municipalities that received the program in 2006 act as a never treated control group. Table 2 provides details of the sample used to estimate each lag of the program indicator variable.

Individual level specification

I then test, taking the same identification strategy, the impact of SP on labor market transitions. These transitions are defined as a change in labor market status between one quarter and the next, conditional on the individual being in the initially specified state. Since labor market transitions cannot be calculated for the first period, that is, the first quarter of 2005, the sample is restricted to those municipalities that have within variation given this additional data restriction. See table 2 for details on the sample used to estimate each lag of the program indicator variable.

The dependent variables can be classified as: (i) exit from employment transitions: formal employment to non-employment[46] (FE-NE), and informal employment to non-employment (IE-NE); (ii) entry into employment transitions: non-employment to formal employment (NE-FE), and non-employment to informal employment (IE-NE); and (iii) transitions between formal and informal jobs: formal employment to informal employment (FE-IE), and informal employment to formal employment (IE-FE). The main estimating equation is:

$$y_{imt} = \alpha + \delta SP_{mt} + \gamma_t + \theta_m + \beta_1 x_{mt} + \beta_2 x_{imt} + \varepsilon_{mt} \tag{4}$$

where y_{imt} is one of the previously discussed labor market transitions for individual i in municipality m at time quarter t, the other set of covariates is as previously described, and an additional vector of individual-level covariates x_{imt} is included to increase precision. Standard errors are clustered at municipal level unless otherwise stated. Note that each outcome is estimated on a subsample of individuals that are in a given initial state, and that the precision of the estimates will depend on the number of transitions observed.

[46] This includes unemployed, discouraged, and inactive workers.

5 Results

5.1 Summarizing the impact of SP on the labor market

I begin by investigating the impact of SP on the share of workers with formal jobs. I use this aggregate labor market metric, because it enables me to both summarize the effect of SP over time, and to benchmark my results against past findings.

Table 4 shows that the estimated effect of SP, up to three years after its introduction, is within the 0.4 to 1 percentage point range of reduction in the share of formal workers documented by past papers.[47] Each of the lagged program indicator variables is tested separately in order to emphasize the idea that their estimation samples are slightly different. The results presented in columns 1 to 3 support the idea that SP had no short-run effects on the share of workers with formal jobs. In contrast, the estimate in column 4 testing exposures to SP longer than three quarters, or of approximately 1 to 2 years, reveals that SP gave rise to a reduction of 0.8 percentage points in the share of workers with formal jobs. This finding is confirmed for exposures of roughly to 2 to 3 years in column 5. The estimates in table 4 are therefore consistent with the idea that, within one year of introduction, SP prompted at most to a modest decrease of 3.5% in the share of workers in formal jobs.[48]

Appendix A shows that these findings are robust to: jointly estimating the lags, excluding the never treated control group, estimating the model using a pooled fractional probit, introducing state quarter fixed effects and other time-varying controls, trimming the sample, and using alternative definitions of the dependent variable.

5.2 SP increases labor supply

Having established the timeframe within which SP is likely to operate on the labor market, and that my estimates are consistent with the findings of past papers, I turn to investigating the effect of SP on labor market flows. My primary interest is to distinguish between: (i) the effect of SP on the decision to enter or exit employment, among the working-age population, and (ii) its effect on the choice between formal and informal jobs, among those already employed.

Since the largest effect of SP on the share of formal workers, a labor market metric influenced by both types of decisions, occurs with program exposures of at least three quarters, the analysis in this section will focus on this timeframe. The following set of results should thus be interpreted as corresponding to an exposure to SP of roughly 1 to 2 years.

[47] See Bosch et al. (2012) for a review of these findings.

[48] The average share of workers in formal jobs in control municipalities during the sample period is 24%.

Table 5 shows that SP increases labor supply by increasing the retention of informal workers in the labor force. It also establishes that the program does not alter the decisions of workers in the margin between formal and informal jobs. Column 1 shows that the probability of transitioning between informal employment and non-employment decreases by 1.3 percentage points after receiving SP. Since the average rate in control municipalities is 16%, this indicates that the effect of the program is to permanently reduce the exit flow from informal employment by 8%. In column 2, I find weak evidence that the program is also capable of increasing entry into informal employment. Specifically, the point estimate is positive, but not statistically significant and roughly a third of the effect that SP has on the exit flow. The previous finding is, however, unsurprising as it is generally harder for government programs to activate workers. Columns 3 and 4 repeat the past exercise with formal employment, and columns 5 and 6 test the impact of the program on the transitions between formal employment and informal employment, and vice versa. In all four cases, the point estimates are small and statistically indistinguishable from zero.

The findings of table 5 remain unchanged when I interpret them using p-values that account for the fact that six outcomes have been tested.[49] Specifically, with the exception of column 1, I am unable to reject the hypothesis that the SP coefficient is equal to zero, when controlling for FDR at q = 0.10

Note that, in my usage, non-employment is comprised by working-age individuals who are either unemployed or are no longer seeking employment. Among those no longer looking for a job, there are those who are immediately available for work and those who are not. I will refer to the first as discouraged and to the second as inactive. Since the labor market response of these groups of individuals is potentially very different, I refine the previous set of results by also testing the effect of SP on the outflows between informal employment and each of these three non-employment labor market states.

Table 6 presents the results of this exercise. Columns 1 and 2 find that the effect of SP on transitions to being unemployed or being a discouraged worker is very small. Column 3, by contrast, shows that SP is able to considerably reduce the flow between informal employment and inactivity, by 1.2 percentage points. Given that there are very few policy options to reactivate this type of individual, the findings of table 6 highlight the potentially efficiency-enhancing effects of SP on the labor market.

[49] See Anderson (2008) for details of the calculation of q-values, that is, p-values that account for the false discovery rate (FDR).

SP leads to an efficiency-enhancing response in the labor market

Taken together, the estimates of tables 4 and 5 enable me to reinterpret the finding that SP gives rise to a relative increase in the level of informal employment. Specifically, I find that the program does not operate by reallocating workers to less productive informal jobs, but by enabling informal workers to stay in the labor force. Hence it does not come at the cost of reducing overall efficiency.

I illustrate the magnitude of the gain in the level of output by performing a simple counterfactual calculation to convert my findings of the effect of SP in the labor market into a GDP metric. I begin by calculating the probability that an informal worker will remain in the labor force after one year.[50] I then use the estimates in table 5 to calculate what the previous probability would be in the absence of SP.[51] Next, I take the difference between the observed and the counterfactual probability to determine the change in the annual probability of remaining in informal employment. For each municipality, I then convert this probability into Mexican pesos. This is done by multiplying the estimate of the change in the annual probability by the number of informal workers and their average annual wage, as observed in the first quarter of 2005.[52] These municipal figures are then summed up.

To account for uncertainty in the estimated regression parameters, I perform the calculation described in the previous paragraph, using coefficients drawn from a normal distribution with mean equal to the estimated coefficient and standard deviation equal to the standard error. This procedure is then repeated 500 times, using a random draw of the coefficients each time.

The resulting simulation suggests that the increase in labor supply triggered by SP had an average value of MXN $22 billion with a standard deviation of MXN $8.8 billion. This figure roughly corresponds to a gain of USD $2.2 billion, or a 0.249 percent gain in Mexico's GDP at 2005 current prices.

Robustness

In appendix A, I show that the estimates on which this back-of-the-envelope calculation is based are robust to: jointly estimating the lags, excluding the never treated control group, and introducing a variety of time-varying controls including state quarter fixed effects. I also show that measurement error in the dependent variable, and quarter-to-quarter attrition are unlikely

[50] The probability of remaining in the labor force after four quarters is given by: $(1 - quaterly\_exit\_rate)^4$

[51] The counterfactual probability of remaining in the labor force after four quarters is given by:$(1 - quaterly\_exit\_rate + SP)^4$.

[52] These estimates do not account for general equilibrium effects. This is reasonable because Azuara and Marinescu (2013) finds that SP had no effect on wages, and because the effect is driven by a group that represents a small share of the labor force.

to lead to biased estimates of the impact of SP. I account for attrition by both weighting and deriving Manski bounds. I further show in this section that weighted and unweighted estimation produces the same set of results once I account for heterogeneity in the impact of SP, a topic to which I turn to next.

6 Channels

6.1 SP enables women to stay employed

The discussion in section 3 states that one of the mechanisms through which SP could increase employment is to free up resources previously used by households to cope with health shocks. Given that Mexican households were potentially spending a large fraction of their time caring for sick dependents, and that women bear the brunt of these responsibilities, I begin by exploring whether SP affected men and women differentially.

Figures 5 and 6 plot the point estimates and the clustered 90% confidence interval of the change in the probability of transitioning between informal employment and inactivity caused by an exposure of at least three-quarters to SP, or of roughly 1 to 2 years.

Figure 5a decomposes the effect of SP between men and women. Specifically, the first coefficient is the estimate of the treatment indicator variable of equation 4 when the sample is restricted to women. The second coefficient is the estimate when the sample is restricted to men. Consistent with the intuition that SP reduced the burden created by sick dependents, figure 5a shows that while SP is effective at helping women stay employed, it does not have an effect on men. The estimated effect of SP for women is a 3.3 percentage point reduction in the probability of transitioning between informal employment and inactivity. Since the average level of the dependent variable in control municipalities for women is 21%, this indicates that the effect of the program is to reduce this exit flow from informal employment by 15%. By contrast, the estimated effect for men finds a smaller and statistically non-significant order of magnitude.[53]

Figure 5b restricts the sample to women and further explores the heterogeneity in the impact of SP for this group. The figure plots the marginal effects derived from four regressions of equation 4, where the program indicator variable in each regression was interacted with one of four sets of covariates that characterize women according to their motherhood status, age, marital status, and relation to the head of the household. The figure shows that the average effect for women conceals considerable differences among subgroups. Specifically, while SP gives both childless

[53] Interacting the program indicator variable of equation 4 with a gender dummy produces marginal effects for men and women of similar magnitude. It also highlights that the effect for women is statistically different to the effect for men. Further testing also reveals that men are not responsive to SP in intensive margin. All of these results are available upon request.

women and mothers considerable help to stay in the labor force, it is younger single women who are the daughters of the head of the household who seem to be disproportionately affected by the introduction of SP.

Women with caregiving responsibilities are more responsive to SP

In order to test whether the previous findings are due to SP disproportionately helping women with caregiving responsibilities, figures 6a and 6b further characterize the effect of the program on mothers and childless women. Note that the status of childless women refers only to giving birth and not to an absence of caregiving responsibilities. In fact, MXFLS data reveals that more than a third of young childless women care for a sibling or older relative.

I begin by exploring the heterogeneous impact of SP among mothers. Since the labor market behavior of this group of women is likely to vary with their marital status,[54] and the intensity of the demands that dependents place on them, I estimate the impact of SP across these two dimensions. I gauge the intensity of the demands by employing an extended dependency ratio. I define dependents as those household members who are either under 14 or over 65 years old. Accordingly, I define caregivers as those between the ages of 14 and 65.

Figure 6a plots the marginal effects derived from estimating equation 4 when the program indicator variable is interacted with marital status.[55] The top part of the figure reports coefficients from a regression where the sample is restricted to mothers residing in households where the dependency ratio is greater than or equal to 1 to 1. The bottom part of the figure repeats the previous exercise, restricting the sample to mothers residing in households where the demands of dependents are less pressing, that is, a dependency ratio strictly below 1 to 1. Note that there are no households without dependents in this figure.

The top part of figure 6a shows that, regardless of marital status, SP does not disproportionately affect mothers in households where demands for caregiving are very large. The lack of an effect among single mothers in this group is not surprising given that this women are also the primary breadwinners and they have a much stronger attachment to the labor force.[56] By contrast, the bottom part of figure 6a shows that SP considerably reduces the probability of transitioning from informal employment to inactivity for single mothers in multigenerational households, -6.3 percentage points. The typical household in this category is a single mother living with her parents.[57]

[54] See Gómez and Campos-Vázquez (2010) for a detailed discussion of the labor market behavior of married and single women in Mexico.

[55] The single category includes: separated, divorced and single women. The married category includes both unregistered civil partnership and registered civil partnerships.

[56] Single mothers who are also the head of the household make 70% of the transitions in this category.

[57] This group of women make 43% of the transitions in this category.

Next, I investigate the impact of SP on the labor market choices of childless women. The top part of figure 6b plots the marginal effects derived from estimating equation 4 on the restricted sample of childless women, and interacting the program indicator variable with the ratio of household dependents. In this case, the ratio is broken up into four categories where the demands of dependents on caregivers progressively increase.[58] The figure shows that SP has a U-shaped effect. Specifically, I find that in households where the dependency ratio is less than 2 to 1, the program does not disproportionately help women stay in the labor force, with effects in the range of -3.2 to -2.5 percent. Next, in households where the dependency ratio is between 2 to 1 and 1 to 1, for example a nuclear household where an older daughter helps care for two younger siblings and an elderly member,[59] I find that the program leads to a threefold decrease in the probability of women transitioning to inactivity, -9.3 percent. Last I find that, in households where the demands of dependents on caregivers are very large, that is, a ratio greater than 1 to 1, the effect of SP is once again around -2.1 percent.

The bottom part of figure 6b repeats the previous exercise, this time interacting the program indicator variable with a variable that denotes their relation to the head of the household. The plotted marginal effects indicate that the bulk of the impact of SP is found among women who are the daughters of the head of the household.

To conclude, SP sharply reduces the exit flow from informal employment to inactivity for women with caregiving responsibilities. This effect, however, is not monotonically increasing in relation to the demands created by dependents. Specifically, in households where there are no dependents, or where there are more dependents than caregivers, I find that the program is less effective at keeping women in employment.

The lack of response to SP for women in households with high dependency ratios is probably due to two polar cases. Larger households might not be affected because they include a household member fully specialized in caregiving who acts as a buffer in the event of illness. In smaller households, where a single member acts as primary caregiver and breadwinner, their greater attachment to the labor force implies that they are less likely to respond to the program along the extensive margin.

Lastly, for households with less dependents than caregivers, I identify two subgroups of women who seem to be particularly responsive to the program: childless women caring for siblings or elderly relatives, and single mothers in multigenerational households.

[58] This is done in order to estimate a more flexible model. The groups are: no dependents, less than or equal to one dependent for every two caregivers, less than or equal to one dependent per caregiver, and more than one dependent per caregiver.

[59] Households with at least one dependent and a daughter of the head of the household ages 14-24 make up 60% of the transitions in this category. In two thirds of these cases the dependent is a child below the age of 7.

SP allows women to reallocate time to the labor market

The predictions of the model, presented in section 3, indicate that if SP operates by reducing the time burden that dependents in poor health impose on caregivers, I should observe both an increase in the time they spend in the labor market and a decrease in the time they spend at home on caregiving activities.

In figure 7, I test whether the time-use pattern is consistent with the previous hypothesis by repeating the figure 6 exercise, this time using as outcomes: weekly hours in the labor market, and weekly hours spent at home on activities related to caring for dependents. The coefficients plotted on the left correspond to hours in the labor market, while the coefficients on the right correspond to time at home.

The results of figure 7a provide support for the idea that women reallocate time to the labor market. In particular, the bottom part of the figure highlights that single mothers living in multigenerational households respond to SP by increasing their labor market hours by 2.8 hours, while reducing the time spent on caregiving activities by 1.5 hours.

Similarly, the top part of figure 7b reveals, that consistent with the findings presented in figure 6b, there is also a U-shaped relationship between SP and the dependency ratio for these outcomes. I find that this relationship is first increasing and then decreasing for the number of hours worked in the labor market, while it is first decreasing and then increasing for the time spent at home. I also find that, once again, the largest effect size occurs for females in households where the dependency ratio is between 2 to 1 and 1 to 1. Specifically, the point estimates suggest that these women increase their weekly hours in the labor market by 3.6 hours while they decrease their weekly hours caring for dependents by 2.1 hours. The bottom part of figure 7b also illustrates that it is the daughters of the head of the household who are most responsive to the program.

On the whole, the pattern of results presented in this section supports the idea that, by releasing women from caregiving tasks at home, SP enables women to stay employed. The estimated effect sizes are of an economically relevant magnitude, and well within the bounds of what the program was expected to achieve in terms of health outcomes. For example, a simple back-of-the-envelope calculation suggests that, for a nuclear household where an older daughter helps care for two younger siblings and an elderly member, access to SP for one to two years leads to a reduction of up to one-sixth of the time previously spent caring for sick dependents, or to roughly half of the reduction observed between 2002 and 2006 in the time spent caring for sick dependents.[60]

[60]To see why this is the case, note that: (i) according to table 1, before the introduction of SP, this type of household spent as much as 12.2 hours a week caring for sick dependents; (ii) that in 2006 this same household would spend only 7.8 hours caring for sick dependents (see table A11), and (iii) that the reduction in time spent caregiving experienced by the older daughter could be as large as 2.1 hours per week.

6.2 Alternative channels.

Reallocation of time among household members

One alternative interpretation to the pattern of results documented in the previous section is that workers with high health care costs avoid early retirement in order to retain their health insurance coverage. This could occur in Mexico because, prior to SP, formal workers aged 60 to 65 needed to contribute for at least 750 weeks (about 14 years) before being eligible for health insurance after retirement, OECD (2005). In this scenario, the introduction of SP would enable these workers to retire, thereby potentially enabling them to take over the caregiving tasks in the household. I rule out that SP operates through this mechanism in two ways. First, I verify that SP does not increase the transitions between formal employment and non-employment for workers aged 54 to 65.[61]

Second, I make the household the unit of analysis, and I test whether SP alters the total hours that the household spends in the labor market or the total hours devoted to caregiving. The intuition behind this test is that if the elderly are as effective at caregiving as the young, then SP would have no effect on total hours when it operates by reallocating household member's time. Alternatively, if SP operates by reducing the burden generated by dependents, we should observe both an increase in total hours spent in the labor market and a decrease in hours spent at home.

Table 7 presents the estimates from a household-level version of equation 4, where the program indicator variable has been interacted with the ratio of household dependents. The main finding, columns 1 and 2, is that, that for household with dependents, for example, single mothers living in multigenerational households, SP leads to both an increase of 4.2 hours in the labor market and a decrease of 2.8 in the time spent at home. In accordance with the previous results, in column 3, I also find that SP leads to an increase in total household earnings of roughly 30 dollars. Note that, while this effect is small and nosily estimated, it lends further support to the idea that the effect of SP on the labor market is welfare improving.

Delayed childbearing

Another way in which SP may have altered the labor market decisions of women is by altering their fertility choices. On the one hand, SP may have incentivized maternity by reducing pregnancy-related risks. The introduction of safe blood and obstetric nurses seems to have considerably reduced the risk of maternal death, Frenk et al. (2012). On the other hand, SP

[61] This result is available upon request. I also verify that SP has no effect on the transition between formal employment and retirement. However, it is commonly argued that workers retire gradually. See Bruce et al. (2000) for a discussion.

added three new contraceptive methods: the sub-dermic implant, the female condom, and emergency contraception. Access to these contraceptive methods could potentially enable women to delay childbearing, thereby partly explaining the SP-related increase in labor force participation.

In order to determine whether this mechanism might have been in play, I test whether SP affected the likelihood of becoming a mother, or the probability of having children. Table 8 presents the estimates derived from equation 4. The dependent variables are: an indicator variable for being a mother in columns 1 to 5, and the number of sons and daughters in columns 6 to 10.

The table shows, that even for women who are at the peak of their reproductive age, SP does not seem to be a primary determinant of their fertility decisions. Specifically, I find that, while by and large the sign of the point estimates are consistent with the idea that SP led to a slight increase in fertility, the estimated effect sizes are small, and not statistically significant. These findings are consistent with those of Bernal (2014), who also fails to find evidence that SP has an effect on fertility using experimental variation. I conclude that delayed childbearing cannot explain the previously documented changes in labor force participation.[62]

Own health effects

SP could also increase labor supply by improving the health of the working-age population. Theoretically, in the standard setting, this is possible because health alters productivity. Thus, as long as we assume that the increase in productivity that may result from SP occurs along the upward-sloping segment of the labor supply, and that people will not increase their consumption of leisure as a result of being in better health, models like Grossman (1972) predict that SP will lead to an increase in labor supply.

Empirically, this mechanism has been hard to pin-down. Experimental evidence from Indonesia suggests that a reduction in access to health services leads to both a downturn in objective health indicators and a sharp decline in female labor force participation, Dow et al. (1997). In contrast, recent experimental work in the US has failed to find evidence of an effect of medicaid on the employment of low-income adults, Baicker et al. (2013).

In the case of SP, while this channel could be at work, it is unlikely that it would be able to account for the overall effect of the program on the labor market for three reasons. First, while recent literature supports the view that SP has improved the health of dependents, there is still no conclusive evidence that SP has had any effect on the health of the working-age

[62]It is possible that longer exposure to SP may lead to changes in fertility decisions. However, corresponding estimates for other lags of SP find no evidence of this effect.

population.[63]

Second, it is hard to reconcile the pattern of the heterogeneous impact of SP in the labor market with differential investments in health inputs. In particular, the differential effect across genders is unlikely to be explained by program features designed to tackle issues exclusive to women. Note that, in addition to the efforts made to reduce maternal mortality, the only other large investment was in the detection and treatment of cervical and breast cancer. These investments are unlikely to explain the labor market response of women who take care of their siblings, both because these illnesses are relatively rare in their age group, and because these efforts were undermined by social prejudice against early detection, Frenk et al. (2012).

Third, I find no evidence to suggest that SP has disproportionately helped workers in occupations and industries that involve strenuous physical working conditions. The intuition behind this test is that, if SP operates by improving the health of workers, then presumably those workers in physically demanding jobs should benefit more from the program. The weakness of this test, however, is that SP may also affect the choice of occupation or industry by enabling workers to take on more physically demanding jobs.

With the previous caveat in mind, table 9 presents estimates for equation 4. The dependent variable is an indicator variable for the transition between informal employment and inactivity. I implement the test by interacting the program indicator variable with a dummy for being in a physically demanding occupation, and in a different regression with a dummy for being in a physically demanding industry. The analysis is performed for both men and women in columns 1 and 2, and for women only in columns 3 and 4.

The interaction terms for the full sample, are small and strongly suggest that SP has not had any differential effect. The interaction terms for the regressions where the sample is restricted to women are larger, but very noisily estimated. This reflects the fact that few women take jobs that are physically strenuous. Since, in all cases, the interaction terms are not statistically significant, I conclude that SP has not benefited disproportionately workers in occupations or industries that are physically demanding.

On the whole, while I cannot rule out that this mechanism might have been at work, these sets of results do suggest that any labor market gains due to health improvements are more likely to be the result of health gains experienced by dependents.

[63] See, for example, King et al. (2009); Barros (2008); Knox (2008); Bernal (2014).

Freeing up of other household resources

Given that one of the aims of SP was to reduce out-of-pocket and catastrophic health care expenditures,[64] it is also possible that the labor market response created by SP resulted partly from the ability of the program to free up household assets. The effect of SP on assets could be sizeable, not only because it has been shown that SP reduces out-of-pocket health expenditures,[65] but also, because the insurance component of SP could allow households to reallocate precautionary savings earmarked for health shocks to other uses.[66]

If these additional resources give rise to an income effect, as suggested by Aterido et al. (2011b), then we should expect this effect to reduce the labor supply.[67] This is important, because it suggests that my labor market estimates provide a lower bound of the effect of SP.

Alternatively, these resources may have also led to an increase in labor supply because they potentially enable capital-constrained entrepreneurs to start their own business. In order to determine whether SP also operates through this mechanism, I examine the effect of the program on labor market transitions between three types of informal jobs: unpaid work, informal salaried work, and independent work. This last category includes both owners of micro-firms and the self-employed.

Women's choices with respect to these three types of jobs in the informal sector may have been altered by SP for two reasons. First, as previously argued, SP has potentially encouraged self-employment by freeing up resources that enable women to set up their own micro-enterprises. Second, SP may also have encouraged work as an informal wage worker, because the reduction in the caregiving burden allows women to take on jobs with less flexible work schedules.

Table 10 presents the estimates for equation 4 where the dependent variables are labor market transitions among these three type of jobs in the informal sector. The sample is restricted to female informal workers. Table 10 provides evidence consistent with both mechanisms. Columns 1 and 2 suggest that SP increases the probability of transitioning to independent work. This increase, however, occurs primarily among those who are initially unpaid workers, 3.6 percentage point increase. While this result is important because it highlights that SP enables households to engage in additional productive activities, it is too small to explain the overall increase in labor supply. Note that unpaid workers make up less than 10% of the women who work in the informal sector.

Moreover, consistent with the idea that SP operates primarily by reducing the burden of caring

[64] households experience catastrophic health care expenditures when they spend more than 30% of their resources on healthcare.

[65] See Barros (2008); King et al. (2009)

[66] This mechanism is documented by Chou et al. (2003), who shows that a similar program in Taiwan produces a 13%. reduction in savings.

[67] This follows on from the assumption that leisure is a normal good.

for dependents, I also find in columns 3 and 4 that SP increases the probability of transitioning to informal salaried work, and that this effect is driven by those who were initially independent workers, 1.9 percentage point increase. Last, in columns 5 and 6, I find no evidence of an effect of SP on the probability of transitioning to unpaid work. On the whole, the estimates in table 10 suggest that SP triggers an increase in female labor supply because it frees up household resources. Among these resources, time seems to have played a key role.

7 Internal validity checks

This section presents several tests that support the validity of the paper's identifying assumptions. The main threat to identification is the correlation between the timing of SP introduction and the time-path of labor market outcomes. I begin by formally testing whether SP is correlated to the share of workers in formal jobs. I estimate a regression of pre-program changes in the share of workers in formal jobs on quarterly indicators of the introduction of SP in a municipality:

$$\triangle y_{mt} = \alpha + \sum_{k \geq t} \delta_k I(SP\ quarter_m = k) + \gamma_t + \varepsilon_{mt} \ \ \forall \ \ t \leq SPquarter_m \tag{5}$$

The dependent variable $\triangle y_{mt}$ is the change in the share of workers in formal jobs in municipality m from quarter $t-1$ to quarter t. The set of dummy variables $SP\ quarter_m = k$ takes the value of 1 in the quarter in which the program was introduced. Quarter fixed effects are denoted as γ_t. The data for this test is derived exclusively from the ENE dataset, and the sample is restricted to those municipalities that received SP between 2004 and 2005. The reference quarter is the first quarter of 2005. SP quarter effects that are jointly significant would indicate that the quarter of introduction is correlated to pre-program changes in the share of workers in formal jobs.

Table 11 reports the results of estimating equation 5. Column 1 shows that the timing of SP is not significantly correlated with pre-program changes in the share of workers in formal jobs, the p-value for the joint test is 0.47. Columns 2 to 8 repeat this exercise with the other labor market outcomes used in the paper. In all cases, I am unable to reject the null hypothesis of the joint test. These findings strongly suggest that pre-program time trends for the labor market outcomes of interest are not correlated with the introduction of SP.

Another threat to internal validity is that SP may have been rolled out in response to sharp changes in labor market outcomes. For example, if policymakers were worried about the effect of SP on informality and chose to conceal the effect by targeting municipalities posting sharp increases in the share of workers in formal jobs, then my estimates would simply reflect reversion

to the mean. I test whether there is any evidence of this type of targeting, or more broadly of SP anticipation effects by estimating the following specification:

$$y_{mt} = \alpha + \sum_{i=1}^{16} \delta_i I(SP\ quarter - i = t)_{mt} + \gamma_t + \theta_m + \beta x_{mt} + \varepsilon_{mt} \quad (6)$$

Where y_{mt} is the share of workers in formal jobs in municipality m at quarter t. The SP dummies take the value of 1 at $q-i$ quarters from the introduction of SP. Available data enables me to estimate the effect up to four years before the introduction of SP. The regression includes the same set of covariates for equation 4.

The results of estimating equation 6 are presented on table 12. The dependent variable in column 1 is the share of workers in formal jobs, the point estimates in this regression are, in all cases, small and statistically indistinguishable from zero. Similar results are found when this exercise is repeated using the other labor market outcomes considered in the paper, columns 2 to 8. Given that, by and large, all point estimates are small and not statistically significant, I conclude that there is no evidence of any systematic anticipation effect. I interpret the combined results of tables 11 and 12 as providing no clear evidence that the identification strategy is biased by the correlation between the timing of SP and labor market outcomes in the pre-program period.

Finally, another potential source of concern is that the impact of SP may not be homogeneous across municipalities, but rather may vary as a function of the characteristics of municipalities. For example, workers in poorer municipalities may benefit disproportionately from the introduction of SP. This could lead to biased estimates if treatment and control municipalities are not comparable in terms of these characteristics. In Del-Valle (2013), I rule out that this could occur by showing that the economic characteristics of municipalities at baseline are unrelated to the sequence in which the program was introduced. There is therefore no evidence that the effect of SP on labor supply could be biased as a result of this issue.

8 Conclusions.

This paper shows that publicly subsidized health insurance increases labor supply, because it frees up resources previously used by households to cope with health shocks. The paper provides some of the first evidence of the existence of this labor supply effect, and of a key version of this mechanism, namely, that SP reduces the time burden of caring for dependents in poor health, thereby enabling households to reallocate time to productive activities.

I deal with the potential endogeneity of access to health insurance by exploiting the variation

generated by the municipal-level rollout of Mexico's Seguro Popular. I exploit this variation using a difference-in-differences design that compares changes in the labor market outcomes of individuals living in municipalities already reached by the program with individuals in municipalities not yet reached. I provide evidence in support of the identifying assumptions, and account for two key time-varying confounders: changes in the level of economic activity, and political targeting of the program.

My main finding is that SP increases labor supply because it reduces the exit flow from informal employment. A number of specification checks support the causal interpretation of these estimates. Consistent with the proposed mechanism, I find that the effect is driven by women with caregiving responsibilities, and that SP enables these women to reallocate time from caregiving tasks at home to work in the labor market.

I also test whether this pattern of results could be caused by four alternative mechanisms. Specifically, I rule out the reallocation of time among household members and delayed childbearing as possible channels. I also provide supporting evidence consistent with the idea that own health effects and the freeing up of savings are not primary SP channels. However, I am unable to fully rule out these channels. These shortcomings highlight the challenge of using labor force survey data to test mechanisms that inherently require health and consumption data. Note that, while this data is available in Mexico, observation frequency and geographic coverage is such that it is not well suited to an identification strategy that exploits quarterly variation in the municipal rollout of SP.

Irrespective of whether there are other channels through which SP increases labor supply, the analysis in this paper draws two important conclusions. First, although it is of paramount importance to determine whether the large-scale provision of subsidized health insurance discourages workers from taking formal jobs, my findings suggest that the provision of health insurance also operates by triggering a behavioral response that affects the decision to participate in the labor market. Accordingly, analysis of social insurance programs could be improved if both the formal-informal work margin, and the labor force participation margin were taken into account when evaluating the impact of these programs.

Second, the design of health insurance could be enhanced by recognizing the way families function. In particular, the potential of health insurance to increase the bargaining power of family members previously overburdened by shock-coping responsibilities is a feature that has not yet been considered in the design of this type of program.

Lastly, although key findings in this paper suggest that programs like SP are likely to be welfare improving, my reduced-form estimates are unable to provide a definitive answer. A stronger stance on welfare could be taken by developing a sufficient statistic that characterizes the welfare gains created by the provision of health insurance. Given that this statistic is likely to depend in

part on reduced-form estimates of the crowd-out effects of publicly subsidized health insurance, the estimates presented in this paper provide a first step in this direction. Accordingly, the purpose of future work will be the derivation of this statistic.

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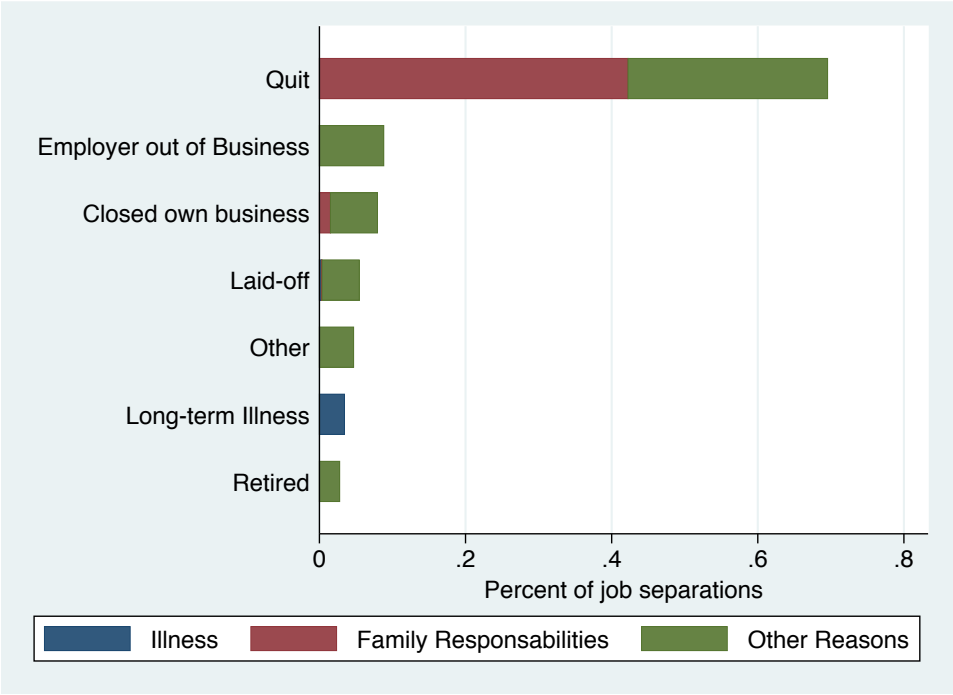
Figures and Tables

Table 1: Health burden per dependent (weekly hours)

	Time ill	Seeking med serv.	Total
Elderly 65 +	5	.2	5.2
Children 0-2	4.2	.1	4.3
Children 3-7	2.3	.1	2.4

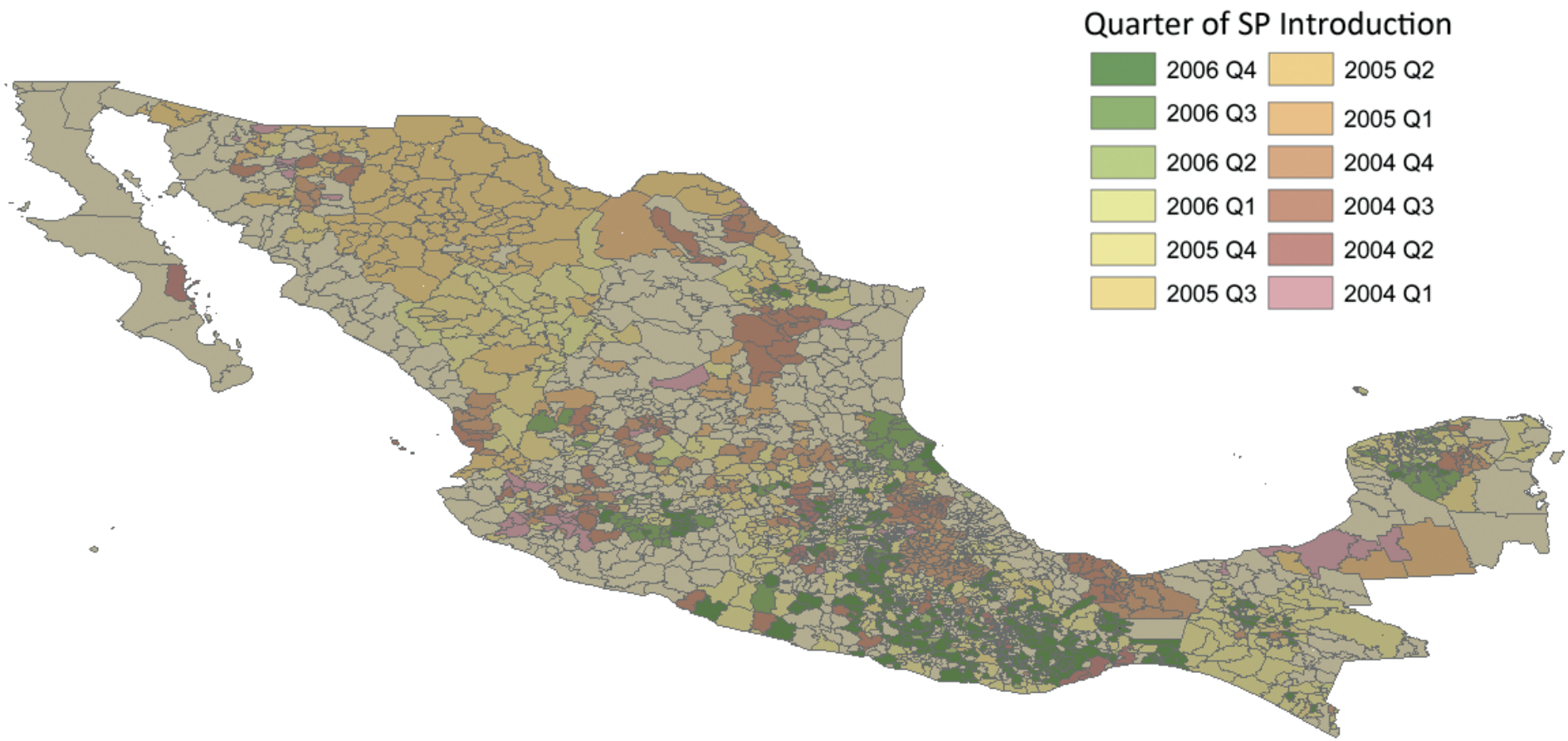
Note: Source: MXFLS 2002

Figure 1: Last Job Separation by type and reason (Women)



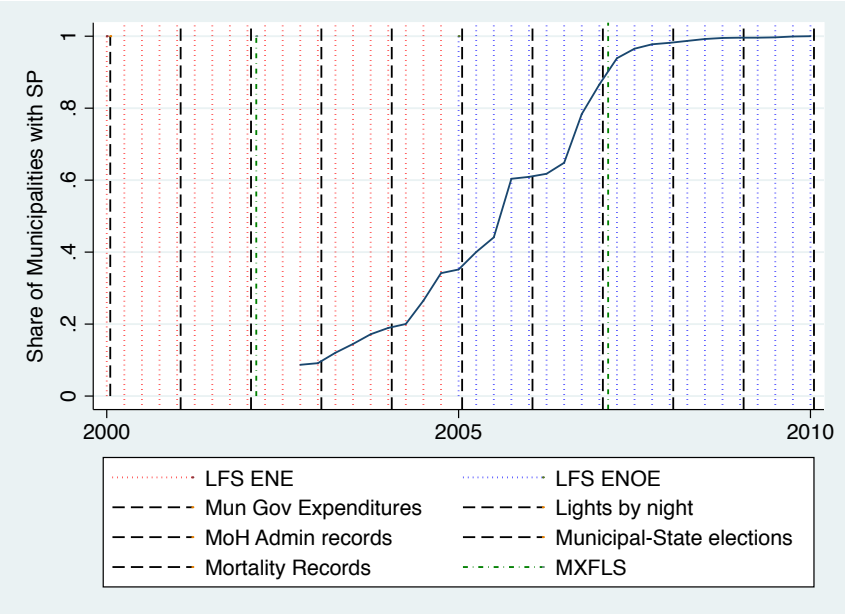
Source: ENOE, Control municipalities in the first quarter of 2005

Figure 2: Municipal Rollout of SP



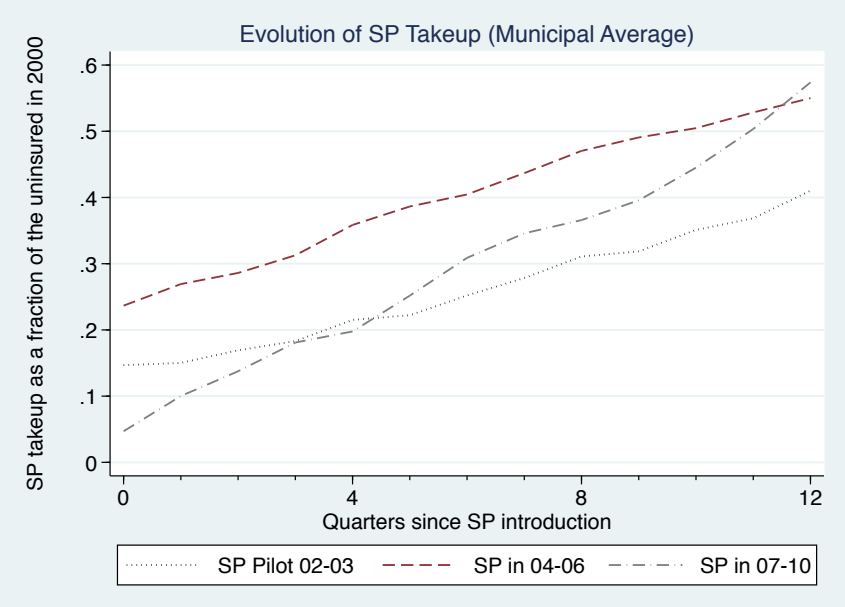
Source: MoH Administrative Records and GIS municipal boundaries from INEGI.
Municipalities in ocher received SP either in the pilot phase 2002-2004, or after 2007.

Figure 3: Correspondence between data and rollout of SP



Source: INEGI, NASA-NOAA, MoH, Banamex-CIDAD.

Figure 4: SP take up rate



Source: Author's calculations based on MoH administrative records and 2000 Census data.

Table 2: Estimation Sample overall labor market effect of SP.

Program Indicator Variable	Interpretation Length of exposure (quarters)	Cross-sectional dimension used Received SP between:	Longitudinal dimension used
SP	1-3	05q2-06q4	05q1-05q4
Lag 1 of SP	2-5	05q1-06q4	05q1-06q1
Lag 2 of SP	3-7	04q4-06q4	05q1-06q2
Lag 3 of SP	4-9	04q3-06q4	05q1-06q3
Lag 4 of SP	5-11	04q2-06q4	05q1-06q4

Note:

Table 3: Estimation Sample Transition Probabilities.

Program Indicator Variable	Interpretation Length of exposure (quarters)	Cross-sectional dimension used Received SP between:	Longitudinal dimension used
SP	1-2	05q3-06q4	05q2-05q4
Lag 1 of SP	2-4	05q2-06q4	05q2-06q1
Lag 2 of SP	3-7	05q1-06q4	05q2-06q2
Lag 3 of SP	4-8	04q4-06q4	05q2-06q3
Lag 4 of SP	5-10	04q3-06q4	05q2-06q4

Note: Since transitions probabilities cannot be calculated for the first period, that is, the first quarter of 2005, the sample is further restricted to those municipalities that have within variation.

Table 4: Overall labor Market effect of SP

Dependent variable: share of formal workers					
	Lag 0	Lag 1	Lag 2	Lag 3	Lag 4
SP	0.0015	-0.0053	-0.0044	-0.0084**	-0.0074*
	(.0055)	(.0054)	(.0046)	(.0039)	(.0038)
Observations	1505	1949	2775	3698	4297
No. of Municipalities	380	413	492	561	573

Note: OLS estimates of equation 3, robust standard errors clustered at municipal at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The unit of observation is the municipality, averages are calculated using all observations of the employed population in the municipal quarter cells defined by table 2. The column titles indicate the lag of the program indicator variable (SP) being tested. SP=1 if the municipality is offering the program. The average level of the dependent variable in control municipalities in the first quarter is 0.24. All estimates are conditional on municipal and quarter fixed effects, shares of age groups, education, marital status, urban residents, gender, income, mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors.

Table 5: Transitions between formal employment, informal employment and non-employment.

	Employment exit and entry				FE-IE work margin	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	IE-NE	NE-IE	FE-NE	NE-FE	IE-FE	FE-IE
Lag 3 of SP	-0.0129**	0.0047	0.0021	0.0019	0.0001	-0.0001
	(0.0054)	(0.0043)	(0.0038)	(0.0016)	(0.0031)	(0.0062)
Observations	154091	202628	98300	202628	154091	98300
No. Municipalities	491	491	467	491	491	467
FDR q-values	0.09	0.56	0.88	0.56	0.96	0.96
Mean Dep. Var (Control Group)	0.164	0.166	0.049	0.011	0.057	0.147

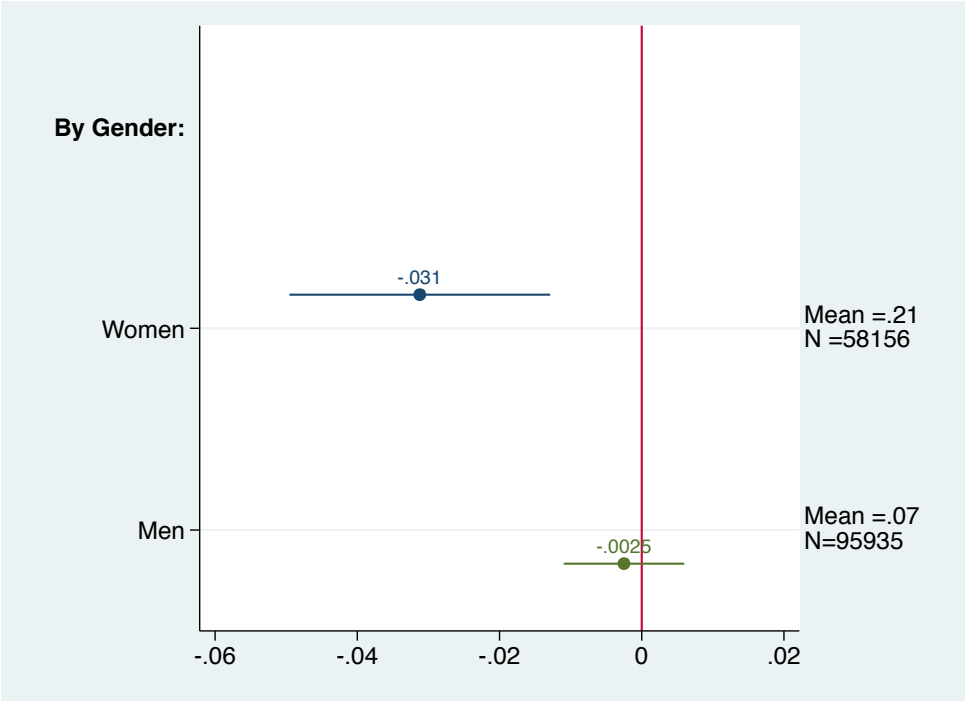
Note: This table presents estimates of equation 4 derived from linear probability models. Regressions are weighted by attrition-adjusted survey weights as described in the data appendix. Robust standard errors clustered at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The unit of observation is the individual. Each regression includes all individuals who (i) are observed in the municipal quarter cells defined by table 3, and (ii) whose labor market status corresponds to the initial status specified in the column title. SP=1 if the municipality is offering the program. All estimates are conditional on municipal and quarter fixed effects, municipal mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Individual level controls include: age in (5 groups), level of education in 4 groups, marital status in 3 groups, urban residency, and gender dummy. The dependent variables are defined as follows: Informal employed to non-employed (IE-NE), non-employed to informal employment (NE-IE), formal employed to non-employed (FE-NE), non-employed to formal employed (NE-FE), Informal employed to formal employed (IE-FE), formal employed to informal employed (FE-IE).

Table 6: Outflow informal employment to non employment

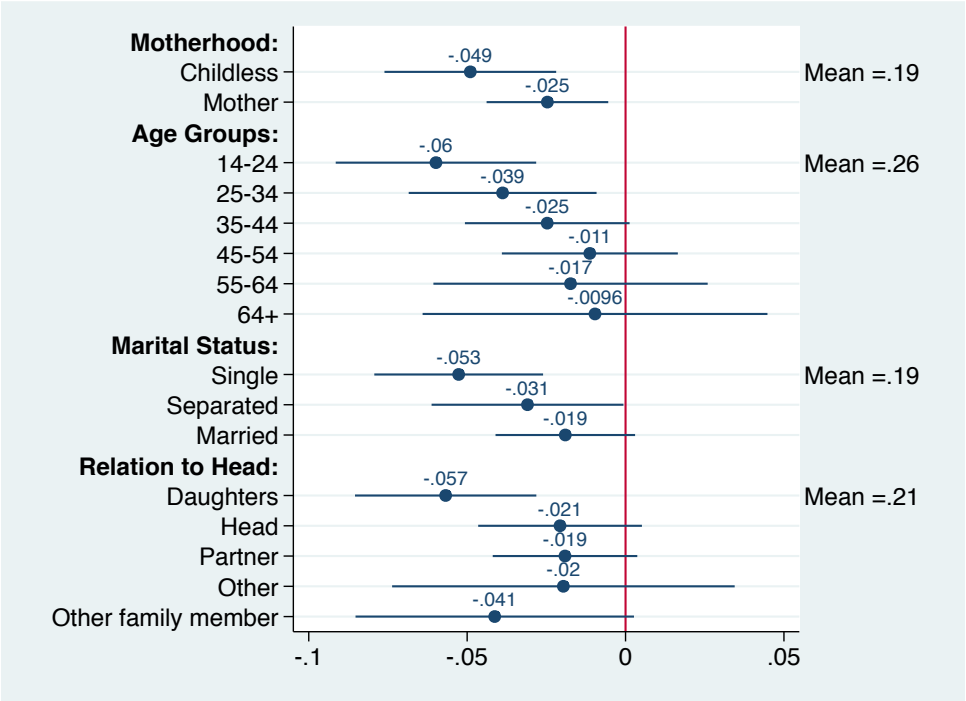
	(1)	(2)	(3)
Dependent Variable:	IE-U	IE-D	IE-IN
Lag 3 of SP	-0.0016	0.0002	-0.0115**
	(0.0020)	(0.0025)	(0.0046)
Observations	154091	154091	154091
No. Municipalities	491	491	491
FDR q-values	0.76	1	0.042
Mean of Dep. Var. control	0.017	0.029	0.118

Note: This table presents estimates of equation 4 derived from linear probability models. Regressions are weighted by attrition-adjusted survey weights as described in the data appendix. Robust standard errors clustered at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The unit of observation is the individual. Each regression includes all individuals who (i) are observed in the municipal quarter cells as defined by table 3, and (ii) whose labor market status corresponds to the initial status specified in the column title. SP=1 if the municipality is offering the program. All estimates are conditional on municipal and quarter fixed effects, municipal mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Individual level controls include: age in (5 groups), level of education in 4 groups, marital status in 3 groups, urban residency, and gender dummy. The dependent variables are defined as follows: informal employment to unemployment (IE-U), informal employment to discouraged workers (IE-D), informal employment to inactive (IE-IN).

Figure 5: The effect of SP on female labor supply



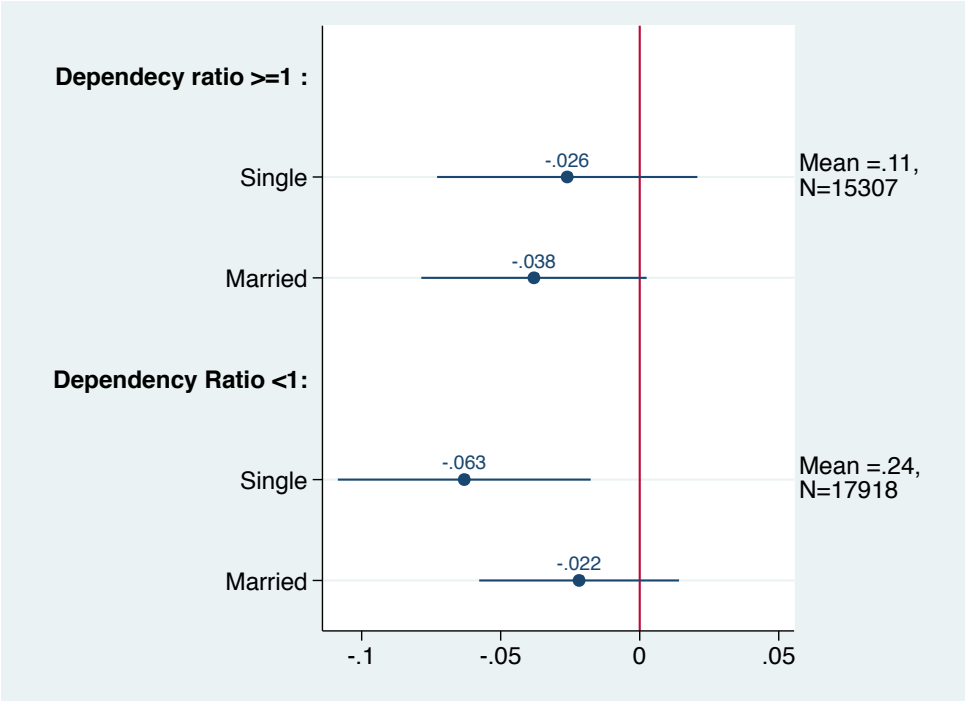
(a) Sample: Women, men



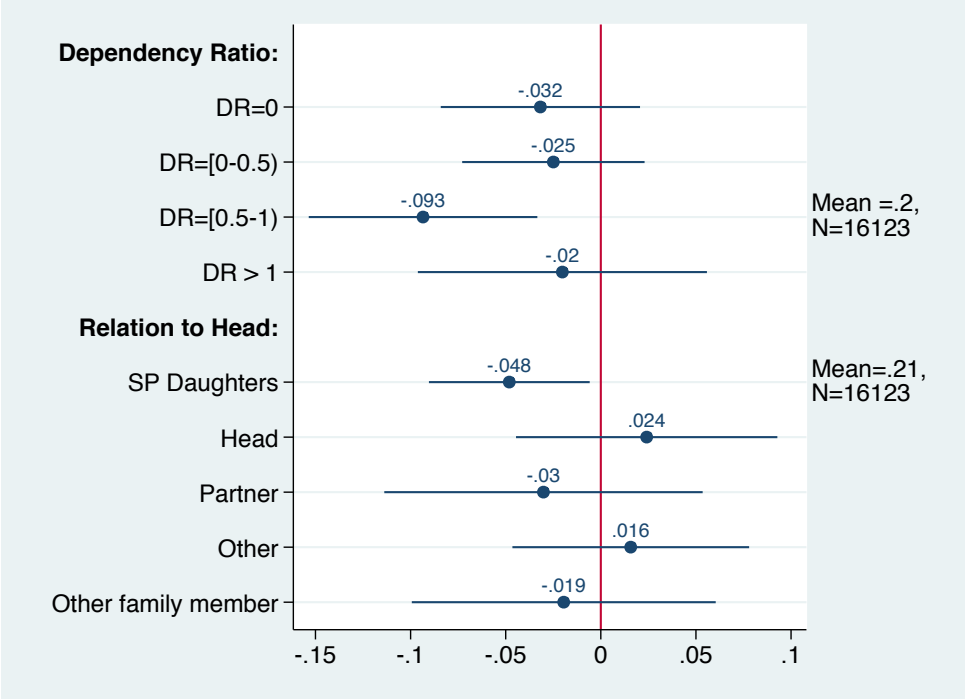
(b) Sample: Women

This figure plots the marginal effect of being exposed for at least three quarters to SP along with municipal level clustered 90% confidence intervals. The dependent variable is an indicator for the transition between informal employment and inactivity. All regressions are linear probability models of equation 4, the sample, weights, and controls are as described in the notes of table 6. Figure 5a plots coefficients from two regressions, the first restricts the sample to women, the second to men. Figure 5b presents estimates from four regressions where the sample is restricted to women. Each set of plotted marginal effects is derived from a regression where the program indicator variable was interacted with the covariate listed (in bold) on the left hand side. The mean of the dependent variable in control municipalities, and the number of observations is presented on the right hand side of the figure.

Figure 6: The effect of SP on the labor supply of female caregivers



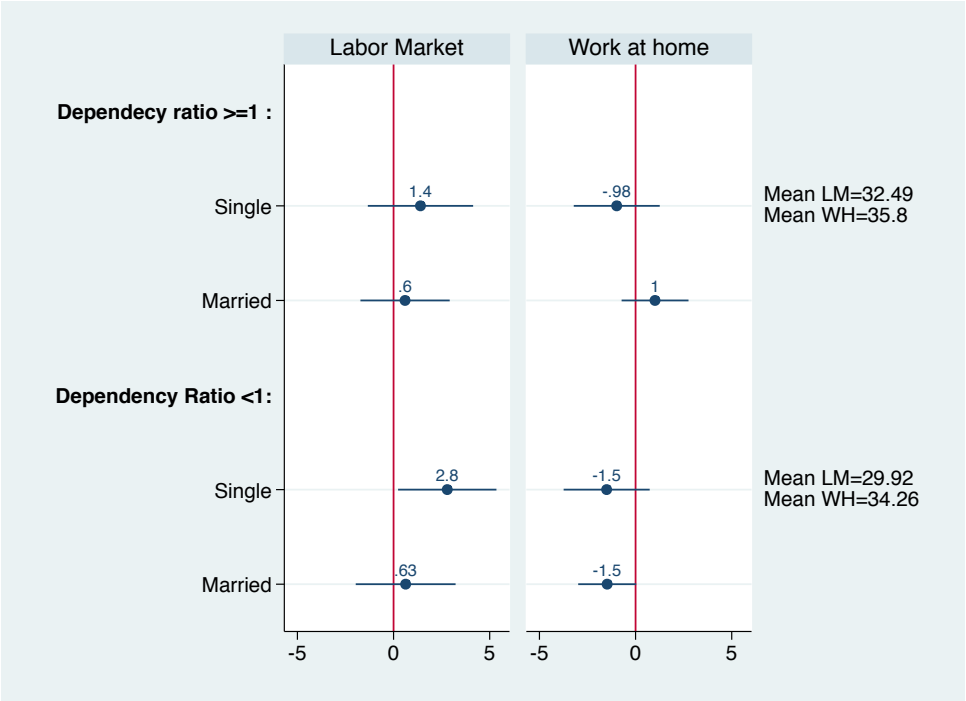
(a) Sample: Mothers



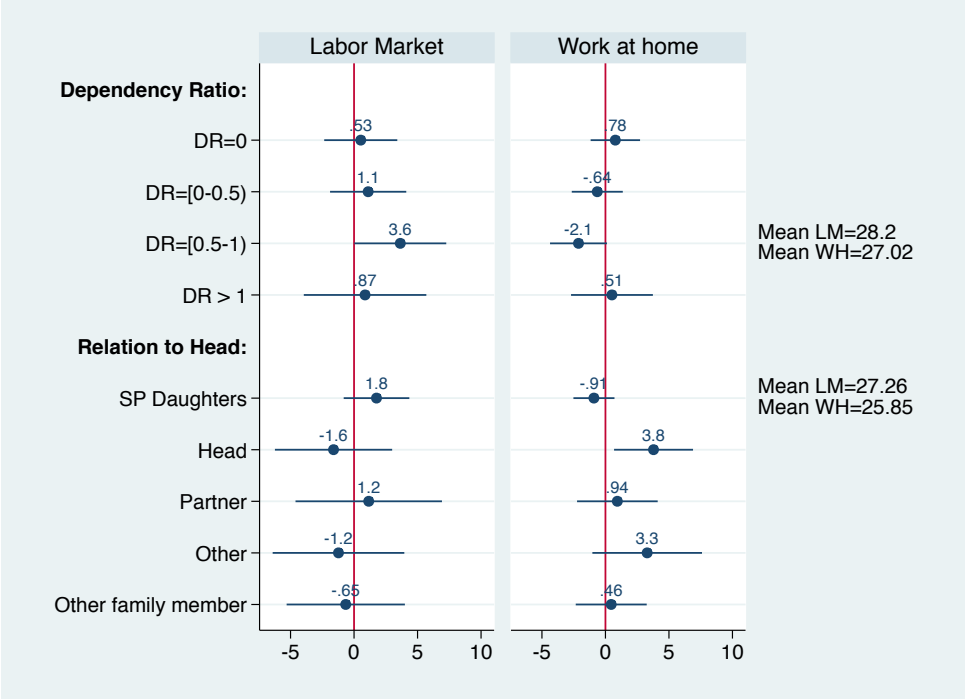
(b) Sample: Childless women

This figure plots the marginal effect of being exposed for at least three quarters to SP along with municipal level clustered 90% confidence intervals. The dependent variable is an indicator for the transition between informal employment and inactivity. All regressions are linear probability models of equation 4, the sample, weights, and controls are as described in the notes of table 6. Figure 6b plots coefficients from two regressions where the sample is restricted to childless women. Each set of plotted marginal effects is derived from a regression where the program indicator variable was interacted with the covariate listed (in bold) on the left hand side. The top part of figure 6a plots marginal effects derived from interacting the program indicator variable with marital status among a sample of mothers in households where the dependency ratio is greater or equal than one. The bottom part of 6a repeats the previous exercise restricting the sample to mothers in household where the dependency ratio is strictly less than one.

Figure 7: The effect of SP on the time use of female caregivers



(a) Sample: Mothers, informally employed in the previous quarter.



(b) Sample: Childless women, informally employed in the previous quarter.

This figure plots the marginal effect of being exposed for at least three quarters to SP along with municipal level clustered 90% confidence intervals. The dependent variables are: weekly hours working in the labor market, and weekly hours at home in activities related to caregiving. All regressions are OLS estimates of equation 4, the sample, weights, and controls are as described in the notes of table 6. Figure 7b plots coefficients from two regressions where the sample is restricted to childless women. Each set of plotted marginal effects is derived from a regression where the program indicator variable was interacted with the covariate listed (in bold) on the left hand side. The top part of figure 7a plots marginal effects derived from interacting the program indicator variable with marital status among a sample of mothers in households where the dependency ratio is greater or equal than one. The bottom part of 7a repeats the previous exercise restricting the sample to mothers in household where the dependency ratio is strictly less than one.

Table 7: Reallocation of time among household members

Dependent Variable:	(1) Total Household work at home (hours)	(2) Total Household labor market work (hours)	(3) Household monthly earnings (MX pesos)
Lag 3 of SP DR[0-0.5)	-4.25***	2.76**	319.2*
	(0.95)	(1.28)	(166.8)
SP x DR=0	5.81***	-3.58*	-279.3
	(1.13)	(1.82)	(228.6)
SP x DR [0.5-1)	4.67***	-2.58*	-369.0
	(1.17)	(1.37)	(230.0)
SP x DR >1	6.45***	-3.00**	-574.0**
	(1.38)	(1.42)	(236.3)
Observations	99633	99633	99633
No. Municipalities	491	491	491
Mean of Dep var. excluded group	107.2	91.31	5695

Note: This table presents OLS estimates of a household level version of equation 4. The program indicator variable has been interacted with the dependency ratio which has been categorized in four groups. The excluded category are households with a dependency ratio strictly above zero and equal or less than 0.5. Regressions are weighted by attrition-adjusted survey weights as described in the data appendix. Robust standard errors clustered at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. Each regression includes all households where at least one member has been informally employed. All estimates are conditional on municipal and quarter fixed effects, municipal mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Individual level controls have been aggregated at the household level, they include shares of: age in 5 groups, level of education in 4 groups, marital status in 3 groups, urban residency, and gender.

Table 8: The Effect of SP on Fertility

Dependent Variable:	Mother=1					Number of Children				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Lag 3 of SP	0.0038	0.0022	0.0062	0.0072	0.018	0.0560	0.0240	0.0025	-0.0250	0.0140
	(0.0056)	(0.0072)	(0.0103)	(0.0140)	(0.0140)	(0.0466)	(0.0357)	(0.0343)	(0.0502)	(0.0242)
L3SP x Single				-0.0040					0.0401	
				(0.0162)					(0.0558)	
L3SP x Divorced				0.0258					0.1230	
				(0.0268)					(0.1240)	
Sample:	All	Age 15-44	Age 15-34	Age 15-34	Age 15-24	All	Age 15-44	Age 15-34	Age 15-34	Age 15-24
Observations	58156	39016	24886	24886	11641	58143	39008	24879	24879	11637
Clusters	491	488	484	484	454	491	488	484	484	454
Mean Dep Var. Excluded Group	0.707	0.617	0.460	0.895	0.200	2.914	1.909	1.080	2.263	0.332

Note: This table presents estimates of equation 4. Regressions are weighted by attrition-adjusted survey weights as described in the data appendix. Robust standard errors clustered at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The unit of observation is the individual. Each regression includes all women who have been informally employed. All estimates are conditional on municipal and quarter fixed effects. SP=1 if the municipality is offering the program. All estimates are conditional on municipal and quarter fixed effects, municipal mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Individual level controls include: age in (5 groups), level of education in 4 groups, marital status in 3 groups, and urban residency.

Table 9: The impact of SP on workers in strenuous occupations and industries

Dependent variable: transition Informal employment to inactivity.				
Sample:	All		Women	
	(1)	(2)	(3)	(4)
Lag 3 of SP	-0.0135**	-0.0115**	-0.0293***	-0.0286***
	(0.0065)	(0.0058)	(0.0113)	(0.0098)
L3SP x Strenuous occupations	0.0024		-0.0078	
	(0.0063)		(0.0133)	
L3SP x Strenuous industries		-0.0007		-0.0179
		(0.0059)		(0.0165)
Observations	147541	147062	55262	55092
No. Clusters	491	491	490	490
Mean Dep Var excluded group	.1351	.146	.1993	.2068

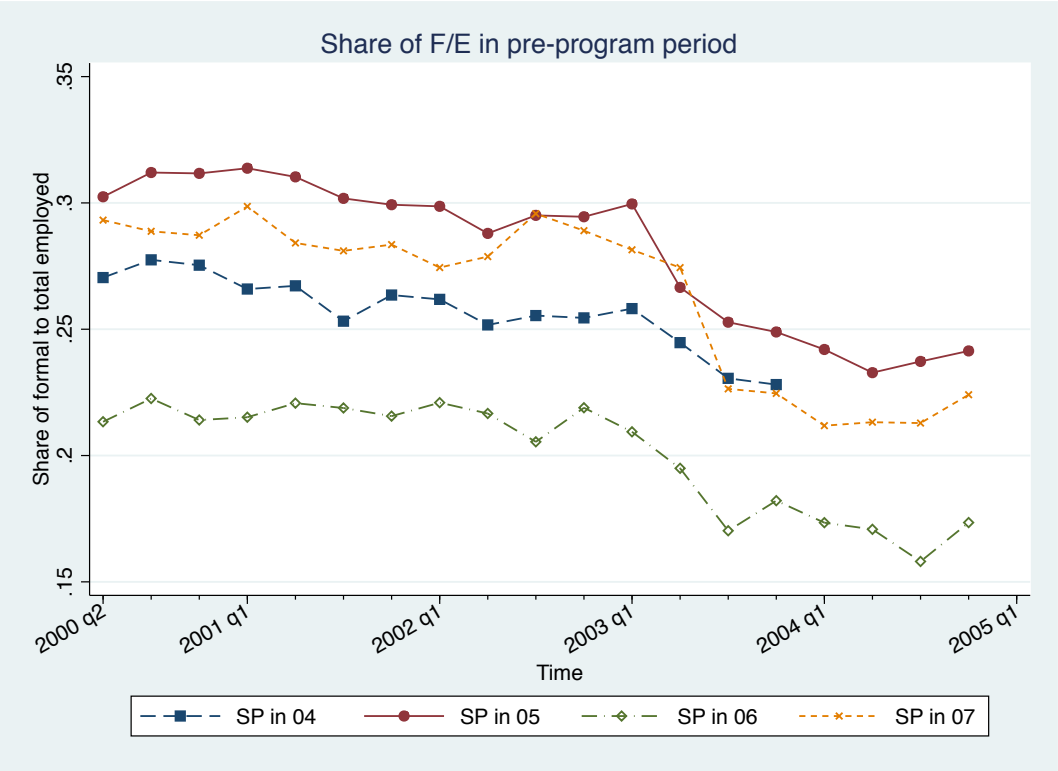
Note: This table presents OLS estimates of equation 4. Regressions are weighted by attrition-adjusted survey weights as described in the data appendix. Robust standard errors clustered at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The unit of observation is the individual. All estimates are conditional on municipal and quarter fixed effects. SP=1 if the municipality is offering the program. All estimates are conditional on municipal and quarter fixed effects, municipal mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Individual level controls include: age in (5 groups), level of education in 4 groups, marital status in 3 groups, urban residency and gender. The definitions of strenuous are as follows, physically demanding occupations: industrial workers, transport operators, personal service workers, security, agriculture. Less demanding occupations: Professional and technical workers, educators, government workers and management, office workers, commerce. Physically demanding industries: agriculture,extractive industries, manufacturing, construction, transport. Less demanding industries: commerce, restaurant and services, professional services, social services, other services, government.

Table 10: The impact of SP on female job to job transitions in the informal sector.

VARIABLES	(1) IE_SE	(2) UW_SE	(3) SE_IE	(4) UW_IE	(5) SE_UW	(6) IE_UW
L3SP	0.00641	0.0359**	0.0184**	0.00114	0.00345	0.00361
	(0.00971)	(0.0151)	(0.00934)	(0.0135)	(0.00934)	(0.00509)
Observations	25414	9748	22994	9748	22994	25414
Clusters	474	455	483	455	483	474
FDR q-values	1	0.120	0.140	1	1	1
Mean of control group	0.0661	0.106	0.0556	0.0583	0.0494	0.0226

Note: This table presents OLS estimates of equation 4. Regressions are weighted by attrition-adjusted survey weights as described in the data appendix. Robust standard errors clustered at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The unit of observation is the individual. All estimates are conditional on municipal and quarter fixed effects. SP=1 if the municipality is offering the program. All estimates are conditional on municipal and quarter fixed effects, municipal mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Individual level controls include: age in (5 groups), level of education in 4 groups, marital status in 3 groups, and urban residency.

Figure 8: Share of formal by year in which SP is introduced



Source: Author's calculations based on ENE LFS.

Table 11: SP roll out and main outcomes in the pre-program period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Stock	LF Participation Margin				F-I work margin		Female
VARIABLES	△ F/E	△ IE-NE	△ IE-FE	△ FE-IE	△ FE-NE	△ NE-IE	△ NE-FE	△ IE-IN
SP in 04q1	-0.00225	0.00316	-0.00160	0.00849	-0.00111	-0.00508	0.00446	-0.00638
	(0.00322)	(0.00884)	(0.00651)	(0.00808)	(0.00132)	(0.00415)	(0.0178)	(0.0121)
SP in 04q2	-0.00738	0.0123	0.00654	0.00140	-0.00164	-0.00223	-0.0109	-0.00180
	(0.00509)	(0.00949)	(0.00792)	(0.00568)	(0.00126)	(0.00417)	(0.0231)	(0.0144)
SP in 04q3	-0.00291	0.00905	0.000674	-0.00570	8.14e-05	-0.00193	0.00460	0.00440
	(0.00221)	(0.00809)	(0.00586)	(0.00415)	(0.000933)	(0.00385)	(0.0166)	(0.0116)
SP in 04q4	-0.00304	0.0116	0.000521	-0.00399	-0.00126	-0.00292	0.0118	0.00669
	(0.00248)	(0.00832)	(0.00640)	(0.00476)	(0.000888)	(0.00385)	(0.0174)	(0.0117)
SP in 05q2	5.42e-06	0.0141*	-0.000972	0.00107	0.00220	-0.00626	0.0123	0.00425
	(0.00236)	(0.00822)	(0.00581)	(0.00466)	(0.00178)	(0.00397)	(0.0167)	(0.0122)
SP in 05q3	-0.00288	0.0116	0.00617	0.00491	-2.74e-05	-0.00502	0.000596	-0.000370
	(0.00238)	(0.00808)	(0.00592)	(0.00433)	(0.000792)	(0.00395)	(0.0175)	(0.0114)
SP in 05q4	-0.00292	0.0104	0.00349	0.00140	-0.000333	-0.00323	-0.00327	0.00159
	(0.00205)	(0.00797)	(0.00557)	(0.00353)	(0.000656)	(0.00379)	(0.0161)	(0.0111)
Observations	5005	3859	3868	3445	3868	3859	3445	3788
No. Clusters	497	478	478	439	478	478	439	470
Pvalue of joint test	.4682	.268	.2652	.1779	.4763	.2337	.3247	.770

Note: Standard errors that allow for clustering at municipal level are reported in parentheses. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels. The observations correspond to the pre-program period for each municipality, it includes all municipalities that received SP between 2004q1 and 2005q4 and that are observed in the ENOE LFS. All regressions include quarter-year dummies, the excluded group is municipalities that received SP in the first quarter of 2005.

Table 12: Ashenfelter Dip

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Stock	LF Participation Margin				F-I work margin		Female
VARIABLES	F/E	IE-NE	NE-IE	FE-NE	NE-FE	IE-FE	FE-IE	IE-IN
1 Quater prior to SP	0.00220	-0.000574	-0.0139	-0.00321	-0.00333	0.00349	-0.00355	0.0100
	(0.00862)	(0.0104)	(0.0109)	(0.00788)	(0.00306)	(0.00648)	(0.0122)	(0.0186)
2 Quaters prior to SP	0.00150	-0.00278	0.00488	0.00574	-0.00455	0.000528	-0.0206*	-0.00227
	(0.00708)	(0.00904)	(0.00691)	(0.00752)	(0.00340)	(0.00534)	(0.0107)	(0.0166)
3 Quaters prior to SP	0.00306	-0.00446	0.00852	0.00613	0.00174	0.00627	-0.0160	-0.0105
	(0.00581)	(0.00675)	(0.00645)	(0.00613)	(0.00182)	(0.00467)	(0.0100)	(0.0131)
4 Quaters prior to SP	-0.00195	0.00543	-0.00645	0.00386	-0.000189	0.00328	-0.0125*	0.00544
	(0.00429)	(0.00604)	(0.00489)	(0.00420)	(0.00154)	(0.00335)	(0.00687)	(0.0123)
7-8 Quaters prior to SP	0.000122	0.00217	0.0105***	-0.00372	0.00127	-0.00192	-0.00593	-0.00186
	(0.00394)	(0.00531)	(0.00400)	(0.00388)	(0.00169)	(0.00275)	(0.00677)	(0.0104)
9-12 Quaters prior to SP	0.000149	-0.000664	0.0140*	-0.00902	0.00293	0.000199	-0.00367	-0.0106
	(0.00571)	(0.00826)	(0.00783)	(0.00571)	(0.00258)	(0.00413)	(0.0107)	(0.0155)
13-16 Quaters prior to SP	-0.00350	-0.00246	0.0225**	-0.0181*	0.00436	-0.00763	-0.00467	-0.0260
	(0.00754)	(0.0134)	(0.0109)	(0.0102)	(0.00433)	(0.00594)	(0.0150)	(0.0235)
Observations	5089	411026	588564	273178	588564	411026	273178	148362
No. Clusters	623	606	606	558	606	606	558	603

Note: Standard errors that allow for clustering at municipal level are reported in parentheses. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels. The observations correspond to the preprogram period (4 years prior to the introduction of SP in each municipality). It includes all municipalities observed in the ENOE LFS that received SP between 2004q1 and 2005q4. All regressions include municipal and quarter-year fixed effects, as well as a set of time varying covariates, at the individual level: age, gender, education attainment, marital status and urban location. And at municipal level: 10th 50th and 90th percentile of household income, total and infrastructure expenditure per capita, and a set of dummies for the political party of the governor and mayor in power at the time. The excluded group is 5-6 quarters prior to the introduction of SP. Column 1 is an OLS regression at the municipal-quarter level. Columns 2 to 7 are sample weighted OLS regressions that use individual level data.

FOR ONLINE PUBLICATION

A Appendix Robustness Checks

A.1 The impact of SP on the share workers in formal jobs

This section addresses various concerns related to the robustness of the estimates present in section 5.1. For ease of presentation these robustness checks focus on the specification that tests exposure to SP for least three quarters. Result for all other lags are very similar and available upon request.

I begin by testing whether the assumptions made on the calculation of standard errors affects the statistical significance of my results. Table A3, column 1 presents standard errors clustered at municipal level, at the state-quarter level, and in both dimension simultaneously.[68] These standard errors range from 0.38 to 0.4 percent, and do not alter the conclusions previously derived. Next in columns 2 and 3 I test the robustness of the specification by excluding the never treated control group from the sample, and jointly estimating the lagged treatment indicator variables. The size of the effect of being exposed to SP for at least three quarters is roughly the same (-0.79 and -0.81 compared to -0.84). Nonetheless, the smaller sample size and the multicollinearity, created by the strong positive correlation between treatment indicator variables, lead to standard errors that are 5-8% larger. In column 4 I also restrict the sample to a balanced panel of municipalities, and in column 5 I estimate a pooled fractional probit on this balanced sample, as suggested by Papke and Wooldridge (2008) for specifications with a fractional response variable. The OLS estimate on this balanced sample and the average partial effect are very similar -0.78 and -0.8 percent respectively.

In Column 6 I address the issue of non-representative municipal averages. Specifically, I follow Magruder (2012) and exclude the top and bottom 5% of municipalities in terms of population. While the smaller sample size reduces the precision of the estimate the effect size is similar -0.73 percent. Thus suggesting that differences between large and small municipalities, for which I have relatively few individuals surveyed do not alter the results. Next in column 7 I define a formal worker as any employed individual in a household were at least one member has direct access to contributory social security. The estimated coefficient of -0.1 percent indicates that SP leads to a reduction in the share of formal workers even when accounting for the possibility that formal workers extend their benefits to family members.

In table A4 columns 1 to 5, I progressively introduce the elements of the x_{mt} vector of time varying covariates. This exercise reveals that the estimated coefficients are between 0.47 (no

[68] See Cameron et al. (2011) for details on this calculation.

covariates) and 0.85 percentage points, well within the effect size range estimated by past papers. Note also that the introduction of these covariates allows me to increase precision, by reducing standard errors by roughly 11%. These two factors explain why I am able to derive, for the first time, estimates of the overall impact of SP that are statistically significant.

Next column 6 adds the lagged vector of time varying covariates x_{mt}, and column 7 controls for the number of medical personnel deployed by the contributory social security system. The latter is done in order to account for the possibility that the quality of medical services offered by contributory social security was adjusted in response to increased competition by SP. In both cases the estimated effect sizes are slightly larger 0.89 and 0.87 percent respectively. Columns 7 to 9 allow the time effects to be specific by state, and by two type of regions. The first region corresponds to Mexico's minimum wage zones, and the second to Aroca et al. (2005) classification of Mexico's zones by their level of economic activity. The estimated effect sizes of SP are less precisely estimated but very similar from 0.53 to 0.77 percent.

A.2 The impact of SP on labor market transitions

In section 5.2 tables 5 and 6 indicated that SP increases labor supply by reducing the flow of workers that exit informal employment towards non-employment, and specifically towards inactivity. Tables A5 to A8 show that this finding is robust to a wide variety of concerns.[69]

In table A5 columns 1 and 2 test the robustness of the specification by excluding from the sample the never treated control group, and by jointly estimating the lagged treatment indicator variables. The effect size of SP is slightly larger (-1.32 and -1.45 compared to -1.12 percent). The coefficients remain statistically significant in both cases, but they are less precisely estimated. In particular in column 2 the smaller sample size and the multicollinearity, created by the strong positive correlation between treatment indicator variables, lead to standard errors that are 22% larger.

Next I address the possibility that informal employment may be misclassified, and that this source of measurement error could be correlated to the introduction of SP. This could occur, for example, if the introduction of SP made the discussion over social security more salient, leading in turn to a change in the rate of misreporting. This concern could be particularly warranted among individuals that receive extended coverage from family members affiliated to contributory social security. In column 3 I restrict the sample to households where neither the head of the households or its partner have access to contributory social security. In column 4 I define informal employment using a concept that is not directly related to contributory

[69] Given that the transition between informal employment and inactivity is more precisely estimated I present the robustness checks exercise for this specification. Robustness checks for the transition between informal employment and non-employment are very similar and available upon request.

social security, namely, whether an individual is working without a written contract. While the smaller sample size leads to less precise estimates, in both cases the point estimates are very similar -0.97 and -1.04 percent respectively. Last in column 5 I exclude from the sample individuals that transition more than once during their five quarter window of observation, as it is likely that these transitions are the result of misclassification. The coefficient is the same as before, albeit less precisely estimated.

In table A5 columns 1 to 5 show that while the introduction of time varying covariates does not alter the estimated effect size, it does slightly improve the precision of the estimates. Column 5 also highlights that clustering the errors at the municipal level, at the state-quarter level, or in both dimension simultaneously does not alter the interpretation of the previous findings. In columns 6 to 9 I show that the findings are robust to controlling for time trends more flexibly. In columns 6 to 8 I allow the time effects to be specific by state and, by two types of regions. The first region corresponds to Mexico's minimum wage zones, and the second to Aroca et al. (2005) classification of Mexico's zones by their level of economic activity. In column 9 I include interactions between time effects and the municipal characteristics that Del-Valle (2013) has shown to be correlated with the rollout of SP. The purpose of this specification is to account for the possibility that municipalities that received SP at an earlier stage experienced differential changes in labor supply for reasons other that the introduction of SP. For example, the program was introduced first in municipalities that were larger in terms of population, by allowing the time effects to depend on the size of population I control for differences in labor supply that are due to early program areas being more populous rather than to the introduction of the program. In all cases the point estimates are very similar and statistically significant, the estimated effect size ranges from 1.03 to 1.25 compared to 1.15 percent.

Another potential issue of concern is the attrition of individuals from the LFS, which is on average 4.4% per quarter. In order to assess the magnitude of this problem in table A7 I estimate the model given by equation 4 with attrition as the outcome variable. In column 1 I restrict the sample to the working population, and in columns to 2 and 3 I restrict the sample to the populations of interest. In all cases the SP coefficient is insignificant and very small. The finding that attrition is not correlated to SP suggest in principle that while attrition may reduce statistical power it will not bias the estimates of program impact. Nonetheless, since it still possible that attritors were selected differently in the treatment and in the control group, in table A8 I present a series of robustness checks.

Column 1 of table A8 assumes that unit nonresponse is random, and multiplies survey weights by the inverse probability of surveying a dwelling across two consecutive quarters. Column 2 takes a more computationally intensive approach and calculates the previous adjustment factor using a very flexible model of selection on observables.[70] Column 3 makes no assumptions about

[70] See the data appendix for details on the calculations of the weights used in columns 1 and 2.

nonresponse, and follows Horowitz and Manski (2000) in order to calculate interval estimates that account for both sampling error and potential nonresponse error. The resulting upper bound is the estimate of the effect of SP if all the attritors in the treatment group would have moved to inactivity and all the attritors in the control group would have moved to informal employment. The lower bound is defined is an analogous way by assuming that attritors in the treatment group remain in informal employment and attritors in control group move to inactivity. Given that the estimates of the effect of SP in columns 1 and 2, -1.19 and -1.04 percent respectively, are within tight Manski bounds, -1.42 to -0.90 percent, I conclude that there is no evidence that the estimated effects of SP could be due to selective attrition.

Last in columns 4 and 5 of table A8 I present estimates of the effect of SP with and without survey weights. Both estimates indicate that the effect of SP is economically large and statistically significant, however, OLS estimates are roughly half a percentage point smaller than WLS estimates, -0.51 compared to -1.01 percent respectively. These differences in the point estimates occur because the impact of SP is heterogenous and this specification is yet to account for those interactions.

Since my primary interest in section 5.2 was to derive estimates of the average impact of SP on labor supply for the Mexican population, I prefer WLS estimates that allow me to get the share of groups that have been oversampled in the survey in line with population shares. Nonetheless, WLS estimates may still differ from the population average partial effect (PAPE) because they do not account for within-group differences in the variance of treatment.[71] In order to estimate the PAPE, in column 6 I employ the "doubly robust" estimator proposed by Robins et al. (1994). The resulting point estimate is 0.93 percent, thus suggesting that WLS estimates provide a reasonable approximation of the average effect of SP on labor supply.

In order to see why unmodeled heterogeneity leads OLS and WLS estimates to identify different averages of the heterogeneous effects. Figure A1 plots the marginal effect of SP on the probability of transitioning between informal employment and inactivity. The top part of the figure plots coefficients of regressions where the sample has been restricted to men. Since SP has no heterogenous impact in this group, as men are by and large unresponsive to the program, I find, as expected, that the WLS and the OLS estimates closely overlap. In addition the bottom part of figure A1 reproduces the results of figure 5b in order to highlight that, in spite of these slight differences in point estimates, the findings of this paper are robust to the choice between weighted and unweighted estimation.

[71] See Solon et al. (2013) for a detailed discussion of this issue.

B Appendix Data

The lights-by-night dataset

I measure economic activity at municipal level using visible band light data captured by the DMSP satellite program. The data provided by NASA and NOAA consist of yearly images at a resolution of 30 arc seconds, or at the equator $\approx$ 0.86 km by 0.86 km grids. This high resolution images of lights from cities and towns provide a light index that ranges between 0-63, with 63 being the brightest. I aggregate this information at municipal level, by overlaying the high resolution images with GIS municipal boundaries created by INEGI. I then calculate for each municipality the average and the standard deviation of the index for that year. I merge the resulting dataset with labor force survey data using INEGI's municipal identifiers.

Sample characteristics

Tables A9 and A10 present means and standard deviations of the key covariates used in the analysis of section 5. The source of the variables in table A9 is the ENOE LFS. The statics are calculated using the cross-section of the second quarter of 2005. The sample has been restricted to municipalities that received SP between the third quarter of 2005 and the fourth quarter of 2006. The statistics can therefore be interpreted as the value of these variables at baseline. All statistics are calculated using sample weights. Columns 1 and 2 restrict the sample to the working-age population. Columns 3 and 4 to the employed, and columns 5 and 6 to those informally employed. In accordance with past papers,[72] I find that informal workers in Mexico tend to be younger, less educated, and less likely to reside in an urban center than the overall employed. There are no differences in terms of their civil status.

Consistent with the analysis of Gómez and Campos-Vázquez (2010) I also find that women are strongly underrepresented in the labor force. Among the informal employed only 36% are women. The bulk of which are mothers. Table A9 also reveals that a substantial fraction of the informally employed is made up of women that benefit disproportionately from SP. Specifically, I find that single mothers living in multigenerational households, and childless women residing in households with dependents, account for 11% of the informally employed, and roughly a third the informally employed women.

Table A10 presents statistics for variables aggregated at municipal level. The sample of municipalities is the same as that of table A9. The sources of information included the ENOE LFS, administrative records collected by the MoH and INEGI, and the lights-by-night dataset. These set of statistics are calculated without weights.

[72] See Perry et al. (2007) for a summary of the demographic characteristics of informal workers.

Table A10, highlights three interesting features of Mexican municipalities. First, income inequality is substantial both between and within municipalities. Consistent with past papers that describe patterns of divergence across growth clusters in Mexico, Aroca et al. (2005), I find that the variation in visible band lights is greater across municipalities than within municipalities. Moreover, I find that even within municipalities inequality is substantial, for example the p90 to p10 ratio of equivalized household income[73] reveals that households in the top decile earn roughly 7 times as much as those in the bottom decile.

Second, I corroborate the idea that the medical infrastructure of the contributory social security tier tends to concentrated in large and densely populated areas. This follows from the observation that unweighted municipal averages of personnel and centers per capita are much smaller than the national average. On the whole these observations bolster the idea that by extending the network of public hospital SP was well posed to improve the health of a underserved population.

Third, while PAN controlled the federal government during the period under analysis, the PRI was firmly in power at both the state and municipal level. These municipal governments had considerable resources at hand, but only about a third of these were being deployed in public work programs that could rapidly alter labor markets.

Weights

The paper employs three types of weights. The first type are sample weights provided by INEGI. In addition to accounting for the probability of sampling INEGI has adjusted these weights to correct for non-response in the cross-section, and to make the estimates compatible with population projections. The non-response correction assumes that dwellings that do not complete the survey are missing at random. Accordingly, their adjustment consists in multiplying the sampling weights of each sampling unit-strata cell by the inverse probability of surveying a dwelling in that cell.

The second type of weights, which are used through out the paper, are constructed in analogous way. In this case I also account for non-response in the longitudinal dimension by multiplying each cell by the inverse probability of surveying a dwelling across two consecutive quarters in that cell. The disadvantage of this correction is that, as in the previous case, this adjustment assumes that dwellings are missing at random. Moreover, since it is relatively rare that the hole household is missing between quarters, this weights are very similar to the ones calculated by INEGI.

[73] Equalized household income is equal to the sum of labor income divided by the square root of household members. I aggregate this variable by calculating the median, the bottom and top decile of each municipality.

The third type of weights, provide an alternative correction for non-response in the longitudinal dimension that relies on the less stringent assumption of selection on observables. Specifically, I calculate inverse probability weights, by estimating the probability of attiring as a function of a flexible model that includes all of the covariates described in the tables notes, and their interactions with the treatment indicator variable. From this estimates a predicted score is calculated and then used to adjust the sampling weights provided by INEGI.

Back-of-the-envelope: caring hours of sick dependents

The back-of-the-envelope calculation presented in table 1 is derived from table A4 which present descriptive statics of the health of the uninsured population. The source of this information is the 2002 wave of the Mexican family life survey. The total time burden created by dependents is calculating by adding the average number of hours that dependents are ill per week and the average time spent seeking health services. I categorize the dependents in three groups: the elderly, those above 65; children below the age of two; and children ages 3 to 7.

The time spent ill is calculated by multiplying the probability of being ill by the number of days spent in bed as a result of this illness. In the case of children under two, I use instead the probability of experiencing diarrhea by the number of days sick from this illness. The time spend seeking health services is calculated by multiplying the time spent waiting and traveling to a health center by the probability of visiting a doctor and the number of visits made.

The evolution of Health in Mexico

Figure A2a plots the fraction of deaths due to infectious diseases by age group. The figure illustrates that like in other countries with an unequal epidemiological transition, children and elderly remain at significantly greater risk of dying from diarrhea, lower respiratory infections, meningitis, and other common infectious diseases. Figure A2a also highlights that over the period in which SP was rolled out, there is a large decrease in the fraction of deaths caused by infectious diseases among children. For example, for children between one month and one year I observe a reduction of roughly 25% in the fraction of deaths attributable to infectious diseases. Note that, as illustrated in figure A2b, the decrease in the fraction of deaths caused by infectious diseases is the result of the death rate attributable to infectious diseases decreasing at faster pace than the death rate from noninfectious diseases. Together these descriptive statistics indicate that while the overall health of children was improving, it was particularly improving with respect to infectious diseases.

These health gains can also be traced in terms of morbidity using health surveys. I use data from the MXFLS because it allows me to observe measures of self-reported health in 2002 and

2006. The main results are summarized in table A13. I find that during this period dependents in previously uninsured households experienced large decreases in both the probability of contracting infectious diseases and the duration of their illnesses. As discussed in MoH (2006) these marked improvements in the health, in particular among children, are likely the result of improvements in water supply that begun in the 1990's, and the introduction of Seguro Popular in 2004.

C Appendix Figures and Tables

Table A1: Caregiving by gender

	Mean	SD
Caregiving by adults, ages 15+ (Last week)		
Women Caregiving=1	0.43	0.5
Men Caregiving=1 (Men)	0.15	0.36
Women Weekly hours	31.6	23.74
Men Weekly hours	14.29	14.57

Note: source: MXFLS Wave 1 2002

Table A2: Dependents Health

	Mean	SD
Health of the elderly, ages 65+ (Last 4 Weeks)		
Serious health problems=1	0.34	0.47
Ill=1	0.14	0.34
Days in bed	6.11	8.16
Flu=1	0.26	0.44
Stomach Ache=1	0.16	0.37
Visit doctor=1	0.21	0.41
Number of visits	1.25	0.73
Hours traveled to health center (one-way)	1.35	4.48
Hours waiting at health center	1.65	3.7
Health of children, ages 0-2 (Last 4 Weeks)		
Flu=1	0.35	0.48
Diarrhea=1	0.2	0.4
Days with Diarrhea	3.6	6.86
Visit doctor=1	0.3	0.46
Number of visits	1.3	0.64
Hours traveled to health center (one-way)	0.59	1.25
Hours waiting at health center	0.9	1.02
Health of children ages, 3-7 (Last 4 Weeks)		
Lost days to illness=1	0.09	0.29
Days in bed	4.31	5.43
Flu=1	0.26	0.44
Diarrhea=1	0.07	0.26
Visit doctor=1	0.17	0.38
Number of visits	1.34	0.85
Hours traveled to health center (one-way)	0.56	0.86
Hours waiting at health center	1.01	1.08

Note: source: MXFLS Wave 1 2002

Table A3: Robustness: the effect of SP on the share of formal workers (Part I)

Dependent variable: share of formal workers							
	Benchmark	Specification Checks		Balanced Panel	PFP (QMLE)	Trimmed Sample	Formal HH
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lag 3 of SP	-0.0084 (0.0039)** [0.0038]* {0.0040}*	-0.0081* (0.0041)	-0.0079* (0.0042)	-0.0078* (0.0040)	-0.00801* (.0042)	-0.0073* (0.0043)	-0.0106** (0.0053)
Observations	3698	2907	2907	3451	3451	3295	3698
No. Municipalities	561	440	440	493	493	500	561

Note: OLS estimates of equation 3 unless otherwise stated, robust standard errors clustered at municipal level in parentheses, at the state-quarter level in brackets, and in both dimensions in crochets * significant at 10%; ** significant at 5%; *** significant at 1%.The unit of observation is the municipality, averages are calculated using all observations of the employed population. SP=1 if the municipality is offering the program. The average level of the dependent variable in control municipalities in the first quarter is 0.24. All estimates are conditional on municipal and quarter fixed effects, shares of age groups, education, marital status, urban residents, gender, income, mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Column 2 and 3 exclude municipalities that received SP in 2006. Column 3 also includes all other lags of SP. Column 4 restricts the sample to a balance panel of municipalities. Column 5 presents the average partial effect of a pooled fraction probit, the model was estimated using quasi maximum likelihood estimation, standard errors are bootstrapped 1000 repetitions. Column 6 excludes the top and bottom 5% of municipalities in terms of population. Column 7 uses a dependent variable where formal worker is defined as any employed individual in a household were at least one member has direct access to contributory social security.

Table A4: Robustness: the effect of SP on the share of formal workers (Part II)

Dependent variable: share of formal workers										
	Controls in Steps				Benchmark	Additional Controls				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Lag 3 of SP	-0.0047	-0.0063	-0.0070*	-0.0085**	-0.0084**	-0.0089*	-0.0087**	-0.0053	-0.0077**	-0.0069*
	(0.0043)	(0.0039)	(0.0040)	(0.0038)	(0.0039)	(0.0046)	(0.0038)	(0.0044)	(0.0039)	(0.0041)
Observations	3698	3698	3698	3698	3698	2748	3698	3698	3698	3698
No. Clusters	561	561	561	561	561	491	561	561	561	561
Demographic	.	✓	✓	✓	✓	✓	✓	✓	✓	✓
Economic Activity I	.	.	✓	✓	✓	✓	✓	✓	✓	✓
Economic Activity II	.	.	.	✓	✓	✓	✓	✓	✓	✓
Electoral & Pub. Finance	.	.	.	.	✓	✓	.	.	.	.
Lagged Controls	.	.	.	.	.	✓	.	.	.	.
Med. Personnel CSS	.	.	.	.	.	.	✓	.	.	.
State x Quarter FE	.	.	.	.	.	.	.	✓	.	.
Region x Quarter FE I	.	.	.	.	.	.	.	.	✓	.
Region x Quarter FE II	.	.	.	.	.	.	.	.	.	✓

Note: OLS estimates of equation 3, robust standard errors clustered at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The unit of observation is the municipality, averages are calculated using all observations of the employed population. SP=1 if the municipality is offering the program. The average level of the dependent variable in control municipalities in the first quarter is 0.24. All estimates are conditional on municipal and quarter fixed effects. Demographic includes: shares of age groups, education, marital status, urban residents, gender. Economic Activity I is shares of population in different income brackets. Economic Activity II includes: mean and sd of visible band lights. Electoral & Pub. Finance includes: total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Med.Personnel CSS refers to the number of medical personnel per 1000 inhabitants deployed in a municipality by the contributory social security. In Region x Quarter FE I the regions correspond to Mexico's minimum wage zones. In Region x Quarter FE II the regions are as defined by Aroca et al. (2005).

Table A5: Robustness: the effect of SP on the transitions between informal employment to inactivity (Part I)

Dependent variable: transition Informal employment to inactivity.					
	Specification Checks		Misclassification		
	(1)	(2)	(3)	(4)	(5)
Lag 3 of SP	-0.0132***	-0.0145***	-0.0097*	-0.0104**	-0.0115***
	(0.0050)	(0.0056)	(0.0055)	(0.0051)	(0.0044)
Observations	121667	121667	112841	75834	151376
No. Municipalities	371	371	491	491	491

Note: This table presents estimates of equation 4 derived from linear probability models. Regressions are weighted by attrition-adjusted survey weights as described in the data appendix. Robust standard errors clustered at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The unit of observation is the individual. Each regression includes all individuals who (i) are observed in the municipal quarter cells as defined by table 3, and (ii) whose initial labor market status is informal employment. SP=1 if the municipality is offering the program. All estimates are conditional on municipal and quarter fixed effects, municipal mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Individual level controls include: age in (5 groups), level of education in 4 groups, marital status in 3 groups, urban residency, and gender dummy. Column 1 and 2 exclude from the sample those municipalities that received SP in 2006. Column 2 additionally includes all other lags of SP in the specification. Column 3 excludes from the sample those individuals residing in households where the head of the household or its partner had access to contributory social security. The dependent variable in column 4 uses a dependent variable where informal employment is defined as having no written contract. Column 5 excludes individuals that transition more than once during the window of observation.

Table A6: Robustness: the effect of SP on the transitions between informal employment to inactivity (Part II)

Dependent variable: Indicator variable for the transition between Informal employment and inactivity.									
	Controls in Steps				Benchmark	Additional Controls			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lag 3 of SP	-0.0119***	-0.0109**	-0.0111**	-0.0112**	-0.0115	-0.0103*	-0.0125**	-0.0105**	-0.0120**
	(0.0045)	(0.0044)	(0.0044)	(0.0045)	(0.0046)**	(0.0055)	(0.0050)	(0.0048)	(0.0048)
					[0.0047]**				
					{0.005}**				
Observations	154091	154091	154091	154091	154091	154091	154091	154091	154091
No. Municipalities	491	491	491	491	491	491	491	491	491
Demographic	.	✓	✓	✓	✓	✓	✓	✓	✓
Economic Activity I	.	.	✓	✓	✓	✓	✓	✓	✓
Economic Activity II	.	.	.	✓	✓	✓	✓	✓	✓
Electoral & Pub. Finances	.	.	.	.	✓	✓	✓	✓	✓
State x Quarter FE	.	.	.	.	.	✓	.	.	.
Region I x Quarter FE	.	.	.	.	.	.	✓	.	.
Region II x Quarter FE	.	.	.	.	.	.	.	✓	.
Mun. Char. x Quarter FE .	.	.	.	.	.	.	.	.	✓

Note: This table presents estimates of equation 4 derived from linear probability models. Regressions are weighted by attrition-adjusted survey weights as described in the data appendix. Robust standard errors clustered at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The unit of observation is the individual. Each regression includes all individuals who (i) are observed in the municipal quarter cells as defined by table 3, and (ii) whose initial labor market status is informal employment.. SP=1 if the municipality is offering the program. All estimates are conditional on municipal and quarter fixed effects. Demographic includes: age in (5 groups), level of education in 4 groups, marital status in 3 groups, urban residency, and gender dummy. Economic Activity I: is the 10th 50th and 90th percentile of the distribution of labor income in the municipality. Economic Activity II includes: mean and sd of municipal visible band lights. Electoral & Pub. Finance includes: total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Region I correspond to Mexico's minimum wage zones. Region II are as defined by Aroca et al. (2005). Municipal Char. include the logarithm of population in the 2000 census, and the log of the municipal area in square kilometers.

Table A7: The effect of SP on attrition

Dependent variable: attrition indicator.			
	Working Age Population	Sample: Informal workers	Sample: Female informal workers
	(1)	(2)	(3)
L3SP	-0.0006	0.0001	0.0003
	(0.0015)	(0.0024)	(0.0031)
Observations	697,327	154,091	58,156
No of Municipalities	491	491	491

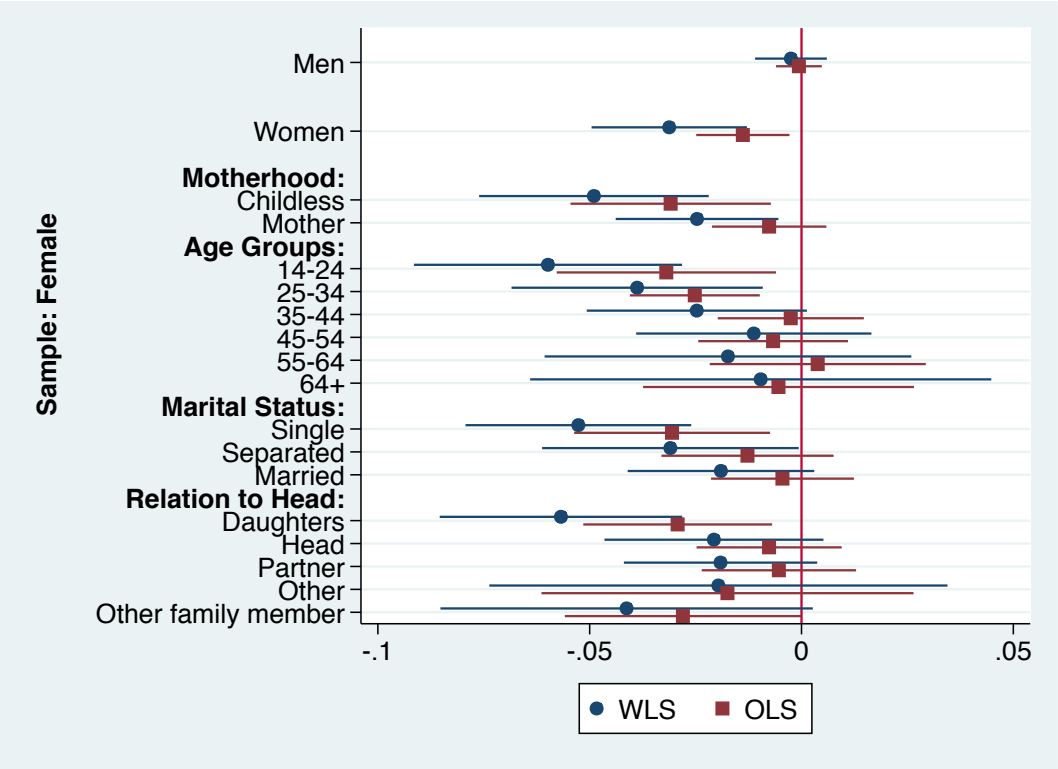
Note: All regression are linear probability models of equation 4. Standard errors that allow for clustering at municipal level are reported in parentheses. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels. Data includes all municipal quarter cells used to produce the estimates of section 5.2. Column 1 restricts the sample to the working age population. Column 2 restricts the sample to individuals that reported being informal workers in the previous quarter. Column 3 further restricts the sample to women. All estimates are conditional on municipal and quarter fixed effects, municipal mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Individual level controls include: age in (5 groups), level of education in 4 groups, marital status in 3 groups, urban residency, and gender dummy.

Table A8: Robustness: the effect of SP on the transitions between informal employment to inactivity (Part III)

Dependent variable: transition between informal employment and inactivity						
	Attrition adjusted survey wgt	Inverse Probability adjusted survey wgt	Manski Bounds	Survey wgt WLS	Unweighted OLS	Doubly robust
	(1)	(2)	(3)	(4)	(5)	(6)
Lag 3 of SP	-0.0119***	-0.0104**		-0.0101**	-0.0052*	-0.0093*
	(0.0045)	(0.0041)		(0.0043)	(0.0029)	(0.0053)
Upper Bound			-0.0142			
Lower Bound			-0.0090			
Observations	154091	153326	159618	154091	154091	152112
No. of Municipalities	491	491	491	491	491	484

Note: This table presents estimates of equation 4 derived from linear probability models. Robust standard errors clustered at municipal level in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. The unit of observation is the individual. Each regression includes all individuals who (i) are observed in the municipal quarter cells as defined by table 3, and (ii) whose initial labor market status is informal employment. SP=1 if the municipality is offering the program. Estimates on columns 1 to 3 are conditional on municipal and quarter fixed effects. Estimates on columns 4 to 6 are additionally conditional on: municipal mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Individual level controls include: age in (5 groups), level of education in 4 groups, marital status in 3 groups, urban residency, and gender dummy. The regression are weighted as specified in the column title, details on the construction of the weights can be found in the data appendix.

Figure A1: Comparing weighted and unweighted estimates



This figure plots the marginal effect of being exposed for at least three quarters to SP along with municipal level clustered 90% confidence intervals. The dependent variable is an indicator for the transition between informal employment and inactivity. All regressions are linear probability models of equation 4 estimates are weighted by attrition-adjusted survey weights as described in the data appendix, estimates in blue are unweighted. All estimates are conditional on municipal and quarter fixed effects, municipal mean and sd of visible band lights, total and infrastructure expenditures per capita by municipal governments, and party dummies for Governors and Mayors. Individual level controls include: age in (5 groups), level of education in 4 groups, marital status in 3 groups, urban residency, and gender dummy. The top part of the figure plots coefficients from two regressions, the first restricts the sample to men, the second to women. The bottom part presents estimates from four regressions where the sample is restricted to women. Each set of plotted marginal effects is derived from a regression where the program indicator variable was interacted with the covariate listed (in bold) on the left hand side.

Table A9: Descriptive Statistics Individual Level Variables

	Working Age		Employed		Informally Emp.	
	mean	sd	mean	sd	mean	sd
Age groups:						
14-24	0.29	0.45	0.20	0.40	0.22	0.41
25-34	0.21	0.40	0.25	0.43	0.20	0.40
35-44	0.18	0.39	0.24	0.42	0.21	0.40
45-54	0.14	0.35	0.17	0.38	0.17	0.37
54-64	0.09	0.29	0.09	0.28	0.10	0.29
>64	0.10	0.29	0.05	0.22	0.11	0.31
Educational attainment:						
Incomplete Primary	0.25	0.44	0.22	0.41	0.30	0.46
Complete Primary	0.26	0.44	0.22	0.42	0.27	0.44
Secondary	0.28	0.45	0.31	0.46	0.27	0.44
Terciary	0.20	0.40	0.25	0.43	0.16	0.36
Civil status:						
Married	0.39	0.49	0.32	0.47	0.31	0.46
Divorced	0.09	0.28	0.08	0.27	0.09	0.28
Single	0.53	0.50	0.60	0.49	0.61	0.49
Resides in urban area	0.77	0.42	0.78	0.41	0.70	0.46
Women	0.53	0.50	0.37	0.48	0.36	0.48
Mother	0.34	0.47	0.25	0.43	0.25	0.43
Single Mothers in multigenerational households	0.05	0.22	0.05	0.23	0.05	0.21
Childless women in household with dependents	0.13	0.34	0.08	0.26	0.06	0.25

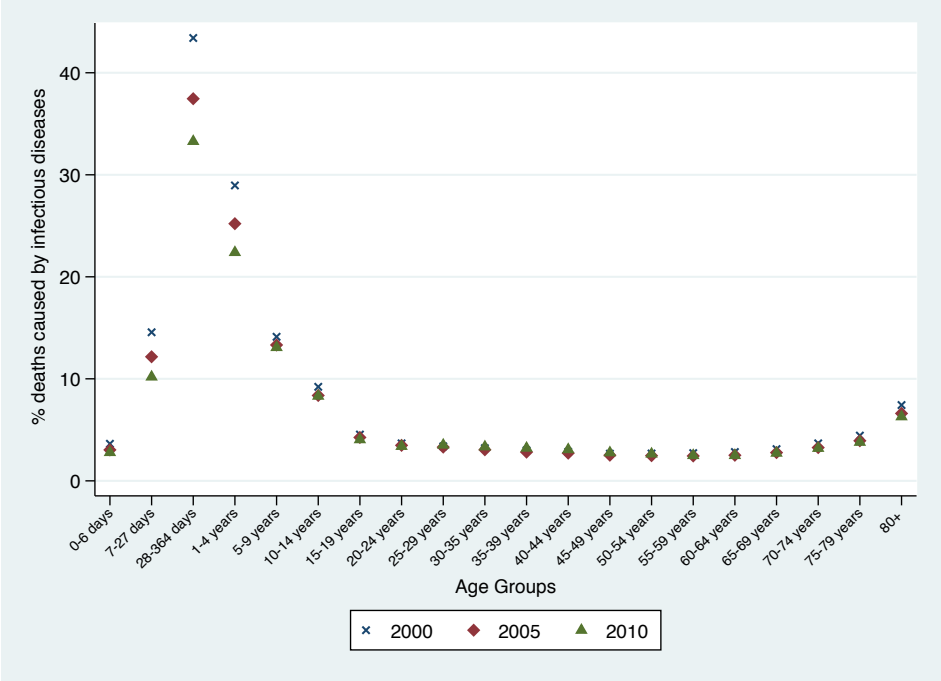
This table shows means and standard deviations for the groups defined in the column titles. The source of the data is the the ENOE survey, all variables are measured in the second quarter of 2005. The set of municipalities is restricted to those used in section 5. None of the municipalities in this sample is exposed to SP.

Table A10: Descriptive Statistics Municipal Level Variables

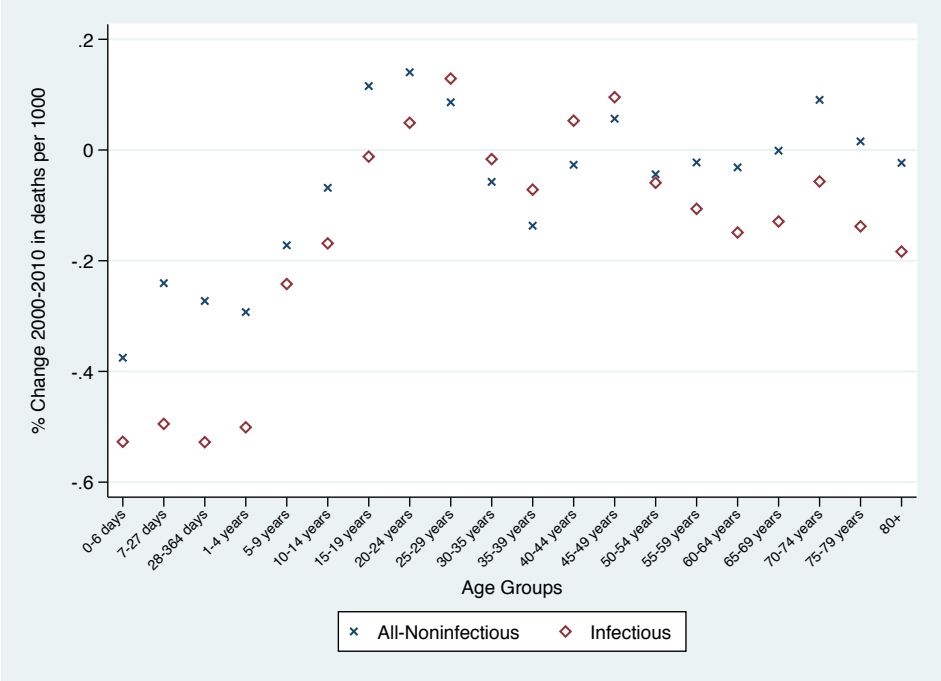
	mean	sd
Visible band digital number 0-63:		
Mean muncipal light	4.94	10.91
Standard deviation of municipal light	3.26	4.21
Party of mayor:		
PRI	0.37	0.48
PAN	0.18	0.39
PRD	0.18	0.38
No data	0.22	0.42
Party of governor:		
PRI	0.61	0.49
PAN	0.17	0.37
PRD	0.13	0.33
IMSS medical supply per 1000 people:		
Personnel	0.12	0.34
Centers	0.03	0.07
Equivalized household income $ MX:		
bottom decile	770.76	568.13
median	2,096.55	1,262.61
top decile	5,257.11	3,771.01
Muncipal goverment expenditures per capita $ MX :		
Total	2,028.13	1,329.65
Infrastructure	654.67	423.33

This table shows means and standard deviations of key municipal level variables. All variables are measured in the first quarter of 2005. The municipalities are restricted to those used in section 5. None of the municipalities in this sample is exposed to SP.

Figure A2: Evolution of Deaths from Diarrhea, lower respiratory infections, meningitis, and other common infectious diseases.



(a) Evolution of the percent of infectious deaths by age group



(b) % Change 2000-2010 in the death rate by age group and cause

Source: MoH mortality records.

Table A11: Health burden per dependent
(weekly hours)

	Time ill	Seeking med serv.	Total
Elderly 65 +	4.7	.1	4.8
Children 0-2	1.7	.1	1.8
Children 3-7	1.2	0	1.2

Note: Source: MXFLS 2006

Table A13: Illness MXFLS 2002 and 2006

	Mean 2002	Mean 2006	% △ 2002-2006
Health of the elderly, ages 65+ (Last 4 Weeks)			
Ill=1	0.14	0.14	-0.45
Days in Bed	6.11	5.76	-5.70
Flu=1	0.26	0.21	-18.18
Stomache Ache	0.16	0.15	-9.46
Health of children, ages 0-2 (Last 4 Weeks)			
Flu=1	0.35	0.24	-30.64
Diarrhea=1	0.20	0.08	-57.08
Days with Diarrhea	3.60	3.37	-6.37
Health of children, ages 0-7 (Last 4 Weeks)			
Ill=1	0.09	0.05	-44.82
Days in Bed	4.72	3.11	-34.03
Flu=1	0.27	0.15	-44.54
Diarrhea=1	0.07	0.06	-15.61

Note: source: MXFLS 2002 and 2006

D Appendix Model

The household's utility function is given by:

$$u(\bar{c}, \bar{\ell}, h) \tag{7}$$

Household face the following set of constrains:

$$\begin{aligned} h &= h(H_a + \ell, SP, M_p) \\ pc &= wH_m + q(H_a) \\ T &= \ell + H_m + H_a \\ H_m, H_a, \ell, c &\geq 0 \end{aligned} \tag{8}$$

Normalizing the price of consumption to 1 and introducing the constrains in the utility function, the household maximization problem can be written as:

$$\max_{H_m, H_a} = u(\frac{H_m w + q(H_a)}{N}, \frac{T - H_a - H_m}{N}, h(T - H_m, SP, P_m)) \tag{9}$$

Household decide on the number of hours they want spend in the labor market and the hours that they will devote to home production, the first-order conditions are given by:

$$u_{H_m} = \frac{w\, u_{\bar{c}}}{N} - \frac{u_{\bar{\ell}}}{N} - u_h h_{H_m} = 0 \tag{10}$$

$$u_{H_a} = q_{h_a} u_{\bar{c}} - u_{\bar{\ell}} = 0 \tag{11}$$

Equation 10 states that in the optimum households will equate the marginal utility of leisure with the value of spending an additional hour either working in the market or producing health. Equation 11 similarly states that households will equate the marginal utility of leisure with values of an additional hour spent on home production activities.

Differentiating each of the first-order conditions with respect to SP and solving for $\frac{\partial H_m}{\partial SP}$ and $\frac{\partial H_a}{\partial SP}$ provides the following predictions under the assumption of additive separability for both $u(\cdot)$ and $h(\cdot)$:

$$\frac{\partial H_m}{\partial SP} = \frac{h_a\, h_{sp}\, u_{hh}(N^3 q_{aa}\, u_c + N^2\, q_a^2 u_{cc} + N^2\, u_{ll})}{\begin{array}{c} -2q_a w u_{cc} u_{ll} + q_a^2 u_{cc} u_{ll} + w_a^2 u_{cc} u_{ll} + N q_{aa} u_c (u_{ll} + u_{cc} w^2 + \\ N^2 (a_a^2 u_{cc} + u_{ll})(h_a^2 u_{hh} + h_{aa} u_s) + N^3 q_{aa}\, u_c (h_a^2\, u_{hh} + h_{aa}\, u_s) \end{array}} > 0 \tag{12}$$

Given the assumption made on functions $u(\cdot)$, $h(\cdot)$ and $q(\cdot)$ it is easy to see that the numerator of equation 12 is positive. It is also possible to verify that the denominator is positive, by noticing

that if $2qw < w^2 + q^2$ then it will also be the case that $2q_a w u_{cc} u_{ll} < q_a^2 u_{cc} u_{ll} + w_a^2 u_{cc} u_{ll}$. Since all of the terms being added are positive with the exception of the first one which is necessarily smaller than terms two and three we can concluded that denominator is also positive.

$$\frac{\partial H_a}{\partial SP} = \frac{-h_a h_{sp} u_{hh}(N^2 u_{ll} + N^2 q_a u_{cc} w)}{\begin{array}{c} -2q_a w u_{cc} u_{ll} + q_a^2 u_{cc} u_{ll} + w_a^2 u_{cc} u_{ll} + N q_{aa} u_c (u_{ll} + u_{cc} w^2 + \\ N^2 (a_a^2 u_{cc} + u_{ll})(h_a^2 u_{hh} + h_{aa} u_s) + N^3 q_{aa}\, u_c (h_a^2\, u_{hh} + h_{aa}\, u_s) \end{array}} < 0 \tag{13}$$

In the case of equation 13 the denominator is the same as before, however, the numerator has a negative sing, thus implying that SP reduces the hours spent in home production. In order to relax the constrains imposed on the partial cross-derivatives equations 14 and 15 repeat the previous exercise while allowing the inputs of function $h(\cdot)$ to be substitutes.

$$\frac{\partial H_m}{\partial SP} = \frac{(h_{asp} u_s + h_a\, h_{sp}\, u_{hh})(N^3 q_{aa}\, u_c + N^2\, q_a^2 u_{cc} + N^2\, u_{ll})}{\begin{array}{c} -2q_a w u_{cc} u_{ll} + q_a^2 u_{cc} u_{ll} + w_a^2 u_{cc} u_{ll} + N q_{aa} u_c (u_{ll} + u_{cc} w^2 + \\ N^2 (a_a^2 u_{cc} + u_{ll})(h_a^2 u_{hh} + h_{aa} u_s) + N^3 q_{aa}\, u_c (h_a^2\, u_{hh} + h_{aa}\, u_s) \end{array}} > 0 \tag{14}$$

It can be seen that the sign are not only the same as before but that by allowing the value of time spent in health production to decrease with the introduction of SP the effect on hours worked and time spent in home production is bigger than that those of equations 12 and 13.

$$\frac{\partial H_a}{\partial SP} = \frac{-(h_{asp} u_s + h_a h_{sp} u_{hh})(N^2 u_{ll} + N^2 q_a u_{cc} w)}{\begin{array}{c} -2q_a w u_{cc} u_{ll} + q_a^2 u_{cc} u_{ll} + w_a^2 u_{cc} u_{ll} + N q_{aa} u_c (u_{ll} + u_{cc} w^2 + \\ N^2 (a_a^2 u_{cc} + u_{ll})(h_a^2 u_{hh} + h_{aa} u_s) + N^3 q_{aa}\, u_c (h_a^2\, u_{hh} + h_{aa}\, u_s) \end{array}} < 0 \tag{15}$$