

The Effect of Children's Health Insurance on Mother's Labor Supply: Evidence from Mexico¹

Pablo Celhay
Sebastian Martínez
Matías Muñoz²

ABSTRACT

This paper analyzes the impact of offering public health insurance on individual labor supply outcomes in Mexico. Our reduced form estimates show that access to public health insurance increases women's participation in the informal sector by 9 percent. We find that this increase in informality is entirely driven by women entering the labor force, and we find no evidence of workers switching from the formal to the informal sector. We even find some evidence of an increase in formal labor, for specific subgroups of women. We use a novel identification strategy combining: (i) the program's staggered rollout across Mexico's subnational health regions, and (ii) an exogenous increase in take-up caused by the sudden introduction of a date-of-birth eligibility rule for the child-focused insurance program, *Seguro Médico Siglo XXI* (SMSXXI), which is available for children born after December 1st, 2006. We implement a panel fixed effects strategy relying on both sources of identification to provide a causal estimate of the effect of affiliation to public insurance on parent's labor supply. For mechanisms, we provide additional causal evidence showing that child health and intrahousehold responses are the mechanisms behind our positive findings on labor outcomes.

Keywords: Health Insurance; Informality; Female Labor Supply

JEL codes: I13, I15, I38, J21, O12, O17

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² Authors: Celhay: School of Government, Pontificia Universidad Católica de Chile, Avda. Vicuña Mackenna 4860 – Macul, Santiago, Chile, email: pacelhay@uc.cl, Martínez: Inter-American Development Bank, 1300 New York Avenue, NW, Washington DC 20577, smartinez@iadb.org, Muñoz: School of Government, Pontificia Universidad Católica de Chile, Avda. Vicuña Mackenna 4860 – Macul, Santiago, Chile, email: mamuno14@uc.cl

I. Introduction

A classic question in labor economics asks if providing non-contributory programs may have undesirable consequences on individual labor participation (e.g., Gruber and Madrian 1997, Currie and Madrian 1999, Autor and Duggan 2003, and Chetty and Saez 2010). The existence of such programs may discourage individuals from income generating activities related to formal work, as these programs constitute a “safety net” to which people can access without contributing through formal labor taxes. While access to public health care may offer financial protection and improve affiliates’ health, it also encourages informal work, which also means driving people away from social security institutions such as pension funds, effectively crowding out other social protection mechanisms. This issue is particularly pressing for developing countries, where non-contributory programs in their early stages coexist with large informal sectors (Levy and Schady, 2013).

In Mexico, as is the case of other developing economies, health and labor markets are linked through formal employment and social security institutions: being occupied in the formal sector grants access to healthcare insurance among other benefits. In 2002, the government created a *Seguro Popular*, a large public health insurance program available for all individuals without access to Social Security. Later, in 2007, the government further expanded health benefits through the creation of *Seguro Médico Siglo XXI* (SMSXXI), a children-specific insurance designed to cover the costs of interventions for newborns and infants, which are not only expensive but also extremely dependent on adult care. The existence of such a program provides incentives for parents of eligible children to exit the formal sector in favor of informality or reduce the flow into formality, while maintaining health

insurance benefits. It may also affect the margins of participation, as free healthcare may have a similar effect to an increase in non-labor income. Alternatively, access to insurance may increase labor supply through improvements in health, reducing the number of negative health shocks for household members. In particular, SMSXXI's coverage could increase labor supply if it lowers the number of health shocks experienced by children or childcare time necessary from the parents.

While a relevant literature has studied the link between health insurance and labor in developed countries (Garthwaite, Gross and Notowidigdo, 2014, among others³), little is known for countries with no evident employment lock, that is, where public health insurance and a large informal sector provide a clear alternative to formal employment and benefits. Also, it is unclear if free health insurance deters mothers from working (Gruber & Madrian, 2002; Galiani and McEwan, 2012), as positive health effects on children may free up time and change parents' expectations about their child's likelihood of illness, reallocating childcare time to other members or external services (daycare). Thus, the impact of child health insurance on parents' labor supply depends on the interplay between formal and informal labor opportunities, households' time allocation and child health effects. Furthermore, studying the impact of public health insurance on labor supply poses a clear selection problem: individuals who use public health insurance must be outside the formal work sector, and we would also expect them to have a worse labor -related characteristics (i.e. human capital) than their employed counterparts. They are also more likely to have a bad health status than their uninsured, unemployed counterparts.

³ For a review on the case of Medicaid in the U.S.A., see Gruber (2008). Other recent papers for the U.S.A. are: Kolstad and Kowalski (2012, Massachusetts), Baicker et. Al (2014, Oregon), and Dague, DeLeire & Leininger (2017, Wisconsin),

In this paper, we study the effects of offering health insurance through the SMSXXI expansion on individual labor supply, finding that access to the program increases labor participation for women, focused mostly on informal jobs, and with no negative impacts on formal sector work. We also provide evidence that child health impacts and intrahousehold responses on time allocation and daycare are the mechanisms behind these labor decisions. To address the selection problem, we exploit a natural experiment in affiliation to health insurance provoked by the eligibility rule of SMSXXI: only children born after December 1st 2006 are eligible for SMSXXI, and enrollment into this program automatically gives the entire family access to public health insurance. We combine this date of birth rule with the staggered rollout of SMSXXI across municipalities in Mexico to implement a triple difference strategy, identifying the causal effect of providing public health insurance to families on labor force participation, informality and formal sector employment. Our findings show that access to SMSXXI increases labor participation and informality by 3.3 and 1.8 percentage points for mothers of eligible children, respectively. This represents an increase in labor participation and the share of mother's working informally of 9 percent. Notably, in stark contrast with the previous literature on health and labor in Mexico, we find no evidence of a negative effect on formal employment, and even find evidence of an increase in formal work for specific subgroups of women such as the less educated and those with multiple children

We estimate our additional results on child health and household responses using a Regression Discontinuity Design based on the child's date of birth cutoff rule. Our RD results explain the mechanisms behind labor supply decisions: we find that being eligible for SMSXXI improves child health, increases time spent by fathers caring for dependents and increases use of daycare services and family planning,

consistent with women joining the workforce and households deciding to reallocate child rearing time other family members (fathers), and also substituting with external services (daycare), as parents of healthier children are more likely to delegate care of their offspring.

With respect to the existing literature studying health and labor markets in Mexico⁴, the papers that most relate to us are Del Valle (2018), and Conti, Ginja & Narita (2018). The first finds similar results of increased informality for a subset of municipalities, and also studies time use for household members. The second corroborates the impact on informality for the subgroup of low-educated families with children, while also providing an estimate of the value of SP through a household search model. With respect to both of these papers, our second source of identifying variation allows us to control for time-varying characteristics at the municipality level, which include the rollout of SP employed in these papers. This allows us to interpret our results as the impact of SMSXXI providing a new gateway into public health insurance rather than confounding by the parent program's rollout. Also, our source of variation coming from the date of birth rule allows us to explore the causal mechanisms behind our labor supply results. Our work also relates to the literature studying the link between child health and mother's labor supply. Our finding that child health increases female labor force participation is symmetrical with the general

⁴ The most frequently cited works in this literature are: Azuara and Marinescu (2013), and Bosch and Campos-Vasquez (2014), who focus on the formal-informal sector and SP rollout, without accounting for potential health effects. For a comprehensive review of the existing literature, see Bosch, Cobacho & Pages (2012).

conclusion⁵ that child disease and disability decrease the labor supply of mothers, as these are still identified as the primary caregivers for children.

We contribute to the literature on the effects of non-contributive benefits by analyzing one of the largest public health programs in the Americas: as of 2016, the number people affiliated to Seguro Popular was nearly equal to the number of Medicare beneficiaries in the US (54.9 vs 56.8 million, respectively). As several developing countries have recently began operating such schemes (Colombia, Ecuador, Bolivia, Thailand, among others), our results provide a new source of evidence favorable to universal healthcare programs which may partially offset the productivity costs of such schemes. Our study differs from most of the previous literature that focuses on the initial and intermediate rollout stages of SP across municipalities. Our novel identification strategy using two sources of exogenous variation in a panel fixed-effects setting allows us to control for the rollout of SP, separating our results from previous difference-in-difference strategies. We also improve on the existing literature by using a measure of program implementation at the hospital level, rather than municipal enrollment tallies which do not fully reflect having effective access to the insurance's benefits. We are also, to our knowledge, the first paper analyzing health insurance in Mexico to find positive effects in labor supply for both informal and formal employment, a finding that contradicts the previous literature studying the extensive margin of labor supply (Aterido et. al, 2010, among others). Finally, we put together multiple, nationally representative, survey data sources and complement results on labor market outcomes with causal effects on health and intrahousehold mechanisms.

⁵ See Salkever (1990), Wolfe & Hill (1995), Powers (2001), Powers (2003) For a full review, see Browning (1992).

This paper is organized as follows: Section II provides background information for health and labor markets in Mexico; Section III describes the data sources and presents descriptive statistics; Section IV describes our empirical strategy and checks for the validity of identifying assumptions, Section V reports our main results and heterogeneity analysis, Section VI provides a discussion for results, and Section VII concludes.

II. Background

Before the introduction of SP in 2002, about 50 million people in Mexico (50.9 percent of the country's population) had no access to health insurance. Coverage in Mexico was mostly provided by Social Security (SS) institutions⁶, and access to their benefits is gained by being formally employed. This establishes a link between health and labor markets, providing incentives to join the formal sector. This also implied that the unoccupied or the informally employed were left without access to healthcare insurance until SP was instated. Dantés et al. (2011) provide a clear picture of the demand for insurance. The beneficiaries of the Mexican health system can be separated in three different groups: (i) formal employees, who have access to healthcare, pensions and other benefits through their contributions to Social Security institutions, (ii) informal sector workers, self-employed, unemployed and people who do not participate in the labor market; this group has no healthcare insurance from Social Security and commonly seeks attention at public health services provided by the Ministry of Health ("*Secretaria de Salud y Asistencia*" (SSA)), and (iii) high-

⁶ The most common social security institutions providing health services are: (1) Instituto Mexicano del Seguro Social (IMSS), (2) Instituto de Seguridad y Servicios Sociales de los Trabajadores del Estado (ISSSTE), (3) Petróleos Mexicanos (PEMEX), (4) Secretaría de la Defensa (SEDENA) and (5) Secretaría de Marina (SEMA), among others.

earning individuals, whose income allows them to complement SS healthcare with private coverage plans.

In 2003, Mexico's Congress passed a law creating the healthcare institution "*Sistema de Protección Social en Salud*" (SPSS), and its operating arm, SP. The purpose of this policy was to provide health insurance coverage for the 50 million Mexicans excluded from Social Security institutions. SP was rolled out in waves across municipalities, targeting the poorest households and states first. By 2008, SP was already covering 27 million people, approximately half of the uninsured population in Mexico, and was almost entirely rolled out across the country.

On December 1st, 2006, during his inauguration speech, Mexican President Felipe Calderón announced the creation of a children's health insurance for all Mexicans born on or after that day, originally named "*Seguro Médico para una Nueva Generación*". This is a complementary insurance for children under 5 years old, covering the cost of over 150 medical interventions specific to the neonatal and infant period. The program was formally created by the Mexican Congress on February 2007 including the eligibility rule announced by President Calderon: only children born after the date of the program's creation (December 1st, 2006) are eligible for enrollment. The program also demands the child not to be enrolled in a Social Security institution (most commonly through a parent's job).

Geographically, SMSXXI operates in the following manner: hospitals are eligible for reimbursement of covered conditions only once they have been properly certified by the CNPSS (*Comisión Nacional de Protección Social en Salud*, the program's governing body). This implies that SMSXXI's coverage has expanded across Mexico as hospitals have gained certification. In contrast, the eligibility rules have remained completely unchanged: only people who are not part of Social Security may enroll,

and only children born after the creation date and under 5 years of age are eligible for child-specific SMSXXI coverage⁷.

A relevant feature of SMSXXI is that it acts as a gateway for families to enroll into public health insurance. When a child is enrolled into the program, the whole family is also eligible for joining SP, and may do so during the same sign-up process. Program coverage lasts until the child turns 5, at which point he or she becomes part of the regular SP coverage as all other affiliates. It is also important to note that, on the supply side, the program also provides incentives for hospitals to affiliate uninsured children. For every child enrolled for the first time, the federal government assigns a transfer to the state's health budget of 210 Mexican Pesos (about 10.5 USD⁸). This implies that both service providers and potential beneficiaries have incentives to increase enrollment. For reference, during its first year in operation (2007), SMSXXI enrolled a little over eight hundred thousand children, and once it reached nearly full coverage of all children under 5 years old in 2012, the program had about 6.6 million affiliates.

III. Data Sources

For labor supply outcomes, we employ the *Encuesta Nacional de Ocupación y Empleo* (ENOE) for Mexico, a household-level survey in México containing detailed information about employment in a quarter-yearly basis for every household member above 15 years old. We use all quarters between years 2005-2010 for a total of 24 quarters. Our sample consists of all individuals over 15 years old who fulfill the following criteria: (i) have at least one child in their household born between

⁷ This requirement has been confirmed in both the “Acuerdo de Creación” (Creation Agreement) in 2007, and all subsequent versions of versions of the “Reglas de Operación” (Operational Rules) in years 2008 through 2018.

⁸ Calculation based on the exchange rate for May 31st, 2018.

December 1st, 2005 and December 1st, 2007, (ii) said child is the head of household's son or daughter, and (iii) the individual is the head of household or spouse. The first criterion is required to classify each household as eligible to the program according to the age-eligibility threshold. Using the age of the youngest child in the household in each survey we classify a household as eligible to the SMSXXI if the youngest child is born after December 1st of 2006⁹.

In Tables 1 and 2, we provide a brief description of the data for men and women at baseline. In these tables, we define the baseline as year 2006, quarter 4, the last quarter where the eligibility rule for SMSXXI had yet to be put in place. Tables 1 and 2 provide means and standard deviations for control variables such as age, schooling, marriage status, rurality, etc. Summary statistics show that women with a child below 1 year old (eligible) are relatively similar to women with a child between 1 and 2 years old (non-eligible), with respect to observable characteristics. For example, the relevant cohorts of women are about 27 years old, attended at least 8 years of education and a little over half of them are married (57%). Table 2 performs the same comparison for men. At baseline, men are usually around 27 years of age, attended 9 years of schooling and about 60% of them are married.

We use administrative data supplied by the CNPSS to identify the program's rollout across geographical zones of Mexico; specifically, we use the registry of *Declaratorias*, which are reimbursement requests for services provided originating from hospitals which have already signed a collaboration agreement with CNPSS. Data from 2009 to 2016 is automatically gathered and reported by the reimbursement

⁹ For baseline years we assign households the date of birth according to the youngest child in the household, using 2005 as the cutoff date for observations from year 2006, and 2004 as the cutoff date for those coming from 2005 datasets.

request's system operated by CNPSS. The system started operating in 2009, so for years 2007 to 2009, we rely on CNPSS financial registries. These registries indicate which hospitals were reimbursed for their services provided during the first two years of operation (2007-2008). Information coming from the automatic system is kept at the hospital discharge level and includes data on the condition that generated the discharge, hospital admission and discharge dates, the location and the unique identifier of the hospital that provided the service. The financial registries for years 2007-2008 are aggregated at the hospital level and include the hospital state and name. These datasets allow us to establish which hospitals had already signed a collaboration agreement with the CNPSS, as reimbursement is only available for hospitals with such agreements.

Our measure of program access improves on other papers using enrollment figures at the municipality level as treatment indicators: reimbursement requests represent actual SMSXXI coverage, while enrollment only informs that affiliates live in a given place without considering effective access to health benefits.

We use this dataset of reimbursements to identify which areas of the country already had a hospital included in the program for every year of operation. We cross-reference our reimbursements database with the SS list of healthcare provision establishments and reconstruct the history of collaboration agreements for each health jurisdiction¹⁰ in the country. We define a municipality in a health jurisdiction as being treated if at least one of hospital in the jurisdiction had signed such an agreement in a given year. Using this treatment indicator, Figure 1 illustrates how the program was

¹⁰ Health jurisdictions are a sub-state territorial unit used for health policy purposes. They are usually comprised of multiple municipalities but may also have only one. Each one of Mexico's 32 states has at least 3 health jurisdictions, and the most numerous have up to 19 in one state.

phased-in across Mexico for the population under 5 years old from years 2007 to 2016, covering a larger share of the potential population as more jurisdictions were phased-in. Program's rollout was concentrated between 2007 and 2009, covering about 80% of the target population and, by 2014, already reaching 90% of children in Mexico.

Finally, to explore potential mechanisms for our labor supply results, we rely on the third wave of the Mexican Family Life Survey (Mx-FLS III), collected in 2009-2012. This a multi-thematic survey representative of the Mexican population at the national, urban, rural and regional level. Specifically, it includes full information on a household's family relations (parenthood), time use data, reported health status and recent incidence of disease for children. It also specifies the date of birth at the year-month level, allowing us to use the date of birth as a running variable in an RD setting.

IV. Identification Strategy & Empirical Specifications

A. Labor Supply estimation: Panel fixed effects models

To estimate SMSXXI impact on labor supply outcomes, we use variation in program enrollment created by the phased-in implementation over time and across Mexico. Figure 2 shows that there was substantial geographical variation in the first years of program expansion over the country's territory, and that even in 2016 some municipalities remain without access to SMSXXI through a nearby hospital. We combine this geographical rollout with the exogenous variation among cohorts provided by the date-of-birth eligibility rule. We exploit these two sources of identification with a triple difference model. To estimate the effect of SP eligibility

and access on labor supply outcomes, our first model has the following form in equation (1):

$$y_{icjt} = \alpha + \delta Cohort_c * SMSXXI_{jt} + \gamma_1 SMSXXI_{jt} + \gamma_2 (Cohort_c * Y'07_t) \\ + \gamma_3 (Cohort_c * Mun Treat_j) + \phi_j + \phi_c + \phi_t + \phi_{t-T_j} + \varepsilon_{icjt}$$

Where i is an individual, c indexes cohorts, j indexes a municipality, t indexes time in quarter-year, and T_j marks the year when a given municipality j enters the program. In this regression, $Cohort_c$ is equal to one for birth cohorts eligible to the program, i.e. children born between December 1st of year 2006. We limit our sample to children that are eligible and ineligible for one year. In particular in year 2007, eligible cohorts are between 1 – 12 months of age and children in the ineligible group are between 13 – 24 months of age. As time goes by these cohorts become older but the analysis keeps estimating results out of comparison of these cohorts within municipalities. In addition, $SMSXXI_{jt}$ is equal to one if municipality j is in a health jurisdiction that is treated in year t . $Y'07_t$ is a dummy equal to one for year $t=2007$ and onwards, marking program creation. $Mun Treat_j$ indicates if municipality j enters treatment during our analysis period (ever-treated). Finally, the coefficient of interest is δ , the effect of the program roll-out in municipality j for individuals i with eligible children. The effect on labor outcomes, y_{icjt} , is identified using variation in the municipality rollout but also within municipalities, by comparing eligible to ineligible cohorts.

In equation (1), we include fixed effects to control for: (i) time and cohort - invariant differences at the municipality level (ϕ_j), (ii) cohort-specific characteristics (ϕ_c), (iii) time-specific shocks (ϕ_t) common to all municipalities and cohorts, and (iv) fixed effects for time remaining until/after municipality j enters treatment ϕ_{t-T_j} ,

to control for time shock specific to the periods before and after implementing the program.

Equation (1) represents the simplest way to estimate a triple difference in our setting. To allow for an unrestricted effect of the municipality specific characteristics, we define the following model in equation (2):

$$y_{icjt} = \alpha + \delta Cohort_c * SMSXXI_{jt} + \gamma_1 (Cohort_c * Y'07_t) \\ + \phi_{jc} + \phi_{jt} + \phi_{j(t-T_j)} + \varepsilon_{icjt}$$

Here, instead of grouping together all municipalities by treated/non-treated status as in equation (1), we allow for each municipality fixed-effect to interact unrestrictedly with every year-quarter and time to treatment category. These effects allow for heterogeneity in: (i) the base levels of employment for eligible and ineligible cohorts in each municipality (ϕ_{jc}), (ii) the overall employment levels of any municipality in any year-quarter (ϕ_{jt}), and (iii) overall employment levels in any municipality for any period before or after their specific year of entering treatment ($\phi_{j(t-T_j)}$). Including these interacted fixed effects allows us to remove the corresponding interacted dummies in equation (1). In both models the coefficient of interest is δ , which identifies the effect on SMSXXI on eligible individuals with access to the program in their location.

In all models, we cluster standard errors at the municipality-cohort level, which fully determines whether an individual is eligible or not for access to SMSXXI. This follows the recommendation by Abadie et. Al (2016) to allow for correlation within units at the treatment assignment level. Additionally, in the robustness section, we allow for the inclusion of control variables, different levels of interacted fixed effects and also study the subsample of individuals between ages 15-45.

B. Mechanisms: Child Health, Hours of Care and Daycare demand.

To study mechanisms behind labor supply results (child health, time allocation, family planning and daycare use) we implement a Regression Discontinuity Design (RDD), using the third wave of the Mx-FLS survey (years 2009-2012). Since the program was already implemented in 90% of the country's territory by 2009 there is not much variation in geographic rollout to use in the empirical strategy. This is why we only exploit variation in eligibility according to the date of birth criteria. Households with children born on December, 2006 or after are eligible for entering SP by way of their children enrolling through SMSXXI. On the contrary, households with no children born after the date are not affected by this change in policy. We assign values of the running variable Z according to the date of birth of each child. Z takes the values of integer numbers, with $Z=0$ for the children born on December, 2006, the first month eligible for SMSXII. Our main specification for RD analysis takes the following form:

$$y_{iz} = \alpha + f(Z_i) + \tau * \mathbf{1}(Z_i \geq 0) + X_i + \varepsilon_i \quad (3)$$

where y_{iz} represents an outcome for individual i . X_{iz} is a vector of control variables specific to the individual, such as age, sex and educational attainment, $f(\cdot)$ is a smooth function of the running variable, known as the control function in the RD literature. The variable $\mathbf{1}(Z \geq 0)$ is an indicator function that takes the value 1 when the running variable is equal to or higher than the cutoff, that is, when the child of the individual was born on or after the cutoff date. Lastly, ε_{iz} represents the unobserved heterogeneity at the individual level.

The coefficient of interest is τ , which can be interpreted as the local average effect of having an eligible child. Since equation (3) is a reduced form estimation, τ is representative of the Intention to Treat (ITT) estimate, that is, the effect of having access to the program near the cutoff point.

One of the key assumptions of an RD design is that we have correctly specified the control function, $f(Z_i)$. In our main specifications, we employ local polynomial regression estimators as recommended by the literature on regression discontinuity (see Lee & Lemieux (2008), Imbens & Lemieux (2008), Cattaneo, Idrobo & Titiunik (2017), among others). We estimate the model described in equation (3) and compute the estimates using the procedure suggested by Calonico, Cattaneo & Titiunik (hereon called CCT, 2014a)¹¹. Since our running variable is discrete, we cluster standard errors at the running variable level, as suggested by Lee and Card (2008).

In addition to the main results, in the robustness section, we estimate our RD model using a global polynomial of first and second degree¹², restricting the sample within the optimal bandwidth calculated by the CCT procedure, and manually setting the bandwidth to 75 and 125% of the bandwidth chosen by the CCT procedure. These robustness estimates consider both linear and quadratic splines that allow the slope to vary on each side of the cutoff.

Finally, to check for the validity of our RD design, we plot the density of the distribution of individuals around the cutoff point in Figure 3. The figure shows that

¹¹ We implement the procedure in Stata using the accompanying software package *rdrobust*, written by Calonico, Cattaneo & Titiunik (2014b).

¹² We abstain from using higher order polynomials, as their standard errors exhibit undesirable properties and poor coverage (Gelman and Imbens, 2017).

density is similar on both sides of the cutoff point, suggesting no evidence for manipulation or sorting around the date. Also, we have strong reasons to believe that there is no manipulation: first, the announcement of the cutoff date was sudden, as it was set on the same day as program creation was announced; second, our data comes entirely from surveys unrelated to health policy, rather than being an official administrative record, giving no incentive for responders to change their birthday. Also, even when considering administrative records, it would be costly for individuals to manipulate their children's date of birth due to bureaucracy.

V. Results

In this section, we present our main results for the effect of SP eligibility on individual's labor supply decisions. First, we present graphical evidence of an increase on program take-up due to the eligibility rule of a child's date of birth. Second, we present estimates based on our panel fixed effects model for the impact of SMSXXI on labor participation and the likelihood of working formally and informally, separating between men and women. Third, for these labor supply outcomes, we also analyze heterogeneity among mothers by levels of education, number of children and marriage status. Fourth, to explore potential mechanisms behind our results, we implement our RDD strategy on the Mx-FLS dataset and test the impact of eligibility on child health status and incidence of disease, daycare usage, parent's time spent caring for dependents, and use of family planning methods. Finally, we present an event study analysis to study the dynamics of SMSXXI's labor supply effects.

A. Main Results

First, we present evidence for the effect of having an eligible child on the probability of the child's parents being enrolled in SP. Figure 4 shows local linear estimates for the distribution of enrollment with respect to the date of birth of an individuals' child. The figure shows that, on average, about 25% of individuals with an eligible child are enrolled in SP. In comparison, only about 17% of individuals with a child born before the cutoff are enrolled in SP, suggesting a significant increase in take-up driven by the eligibility rule.

Table 3 presents the main results on labor supply outcomes using the full sample of eligible parents. Results are interpreted as the effect of being eligible for SP when the program is available at a parent's municipality. We report outcomes for mothers on panel A, and for fathers on panel B. We report estimates of our panel model for the effect of *SMSXXI* access on the probability of labor force participation (columns 1 and 2), having an informal sector job (columns 3 and 4) and having a formal job (5 and 6). For each outcome, we report estimates using the model specified in equation (1), which includes municipality, year-quarter and cohort fixed effects (columns 1, 3, and 5), and we also present the alternative specification with interacted fixed effects (equation (2), columns 2, 4, and 6).

For mothers (panel A), columns 1 and 2 show that access to *SMSXXI* increases the likelihood of participating in the labor force by 3.2 and 3.3 percentage points. Effect size is an increase of 9 to 9.2% with respect to ineligible women at baseline (35.6%, for year 2006). Columns 3 and 4 show a significant effect of increased informal labor among eligible women. The likelihood of working in the informal sector increases by 1.5 to 1.8 p.p., significant at the 95% level. Since 18.5% of ineligible women work informally at baseline, this increase represents an increase between 8.1 to 9.7% on informality with respect to women not affected by the

program. In Columns 5 and 6, results are mixed on the effect of SMSXXI on formal work: our first specification suggests an increase in formal labor (at the 10% level), while the interacted fixed effects models are not statistically significant¹³.

In panel B of Table 1, we report the effect of being eligible in a municipality with access to SMSXXI on fathers' labor decisions. Results suggest a positive effect on labor participation, but these coefficients are relatively small: 98.2% of men at baseline (2006) participate, suggesting an increase of only 0.4 to 0.5%. We find no evidence for an effect of SMSXXI on informal or formal work for men.

In all, the evidence we present confirms a significant effect of SMSXXI eligibility on labor market outcomes for women. Having a child eligible for the program increases labor participation and participation in the informal sector for mothers by 9% with respect to ineligible women. Results are consistent with the idea that providing public health insurance, conditional on not being affiliated to social security, increases the share of women working in the informal sector, as health benefits are now available without the need to have a formal job. Mixed evidence suggests that formal work may also increase, which we claim to be explained by a positive health effect of the insurance on the children of these women (Mechanisms subsection). We also note that we find scant evidence of an impact on men's labor supply, and if anything, our results suggest positive labor participation effects. Taken as a whole, these results suggest that access to public health insurance through a child has a targeted effect on women rather than men.

¹³ In the Robustness Section (Appendix A, table A3), we show that this result varies when looking at the specific subsample of women aged 15-45 years old and for the interacted fixed effects model, providing only mixed evidence for an increase in formal employment.

B. Heterogeneity Analysis for Mothers: Results by Level of Education, Number of Children and Marriage Status

In Table 4, we present a heterogeneity analysis for our labor supply results on mothers. We analyze how labor outcomes differ between groups of women, separating by level of education (less than/with high school or more, panel A), number of children (1 vs 2 or more, panel B) and marriage status (single vs. non-single women, panel C). Estimates in table 4 are obtained using the interacted fixed-effects model (Equation 2).

Panel A disaggregates results for women between less than full high school (columns 1-3) and high school completed or more (columns 4-6). Comparing labor force participation (LF), we observe that women with lower educational levels increase their labor force participation significantly by 4.1 percentage points, while women with higher education exhibit no significant change in participation. The effect on informality is also driven by less educated women, who increase their participation in the informal sector by 2.4 p.p. (at the 95% level). These increases are in the range of an 11 to 18% increase with respect to control (ineligible) women at baseline. Also, eligible women with less education increase their participation in the formal sector by 1.2 p.p., significant at the 10% level. This is consistent with the idea that women with lower educational attainment have more to gain from the informal than the formal labor market, and that a public insurance program does not affect more educated women who have access to social security institutions through formal work.

Next, Panel B disaggregates results among women according to the number of children they have. The effect on labor participation is the same for both groups of women (+3.9 p.p.), and women with more children benefit more from the program by

increasing their informal labor, in line with the idea that women with more household tasks (child-rearing) favor a flexible work arrangement like an informal job.

Finally, Panel C shows heterogeneity for women depending on their marriage status. In this case, effects on participation are almost entirely driven by non-single women, who report a large increase of 5.7 p.p. more likely to participate. Also, the effect on informality is observed more strongly among non-single women (+2.5 p.p.), but single mothers also increase informal labor (+1.3 p.p.). As non-single women can rely on other family members to divide and share household work, we would expect these women to be able to benefit more from the labor market.

In all, the evidence in Table 4 suggests that, in terms of labor participation, access to child health insurance is more beneficial for mothers who have a partner or spouse, lower levels of education and more children. These findings are consistent with the idea that women who can share the time demands of their household can take advantage of labor market opportunities, while women with no children or a partner are not affected by the benefit. In the following section we show that it is indeed the case that children with better health require less attention from their mothers, and that access to the labor market for mothers makes men spend more hours caring or dependents such as children.

C. Mechanisms: Child Health, Hours of Care and Day Care demand

In order to further explore the mechanisms behind our results on women's labor supply, we now turn to supplementary evidence using the third wave of the Mexican Family Life Survey (Mx-FLS). We exploit the discontinuity in eligibility induced by the date of birth rule to implement an RD design, comparing families with children born before and after the cutoff date. We study the impact of SMSXXI

eligibility on child health status, child's incidence of disease, parents' time dedicated to caring for dependents, use of family planning and daycare services.

We present RD results for children in Table 5, and for parents' outcomes in Table 6. For all models, we report the average of the outcome left of the cutoff for comparison (Mean Control), the bandwidth estimated by the CCT procedure in months around the cutoff, and whether covariates were included.

Table 5 reports the impact of eligibility on child reported health (column 1, 5 categories scale, "Very Good" is the max) and incidence of diseases (columns 2-5). Results show that children who are just eligible for the program have a significantly higher probability of reporting their health status as "Very Good". Effects show these children to be 7.2 p.p. more likely to report the best possible health status. Considering that only 11.8% of control children report the best health status, size effect is 61% more likely to report very good health. For incidence of disease, results show that eligible children have a lower probability of flu and diarrhea, flu decreasing by 14 and 4.8 p.p., respectively. For incidence of cough, the effect is negative (-5.8 p.p.) but imprecisely estimated, and for breathing difficulties we find no impact of the program. Taken as a whole, these results provide evidence that SMSXXI eligibility improves reported health for children born after the cutoff date.

In Table 6, we report the impact of SMSXXI eligibility on parents' outcomes. We analyze mother's health status, hours spent caring for dependents, use of family planning methods, and use of daycare services for the child. Column 1 shows no impact of SMSXXI on mother's self-reported health status. Next, columns 2 and 3 show the effect of SP on hours spent caring for dependents, separating between men and women. Results show that men significantly increase time spent caring for dependents in 2.6 hrs., which is a 46.8% increase with respect to fathers whose

children are not eligible for SP. Results for women are insignificant at the usual level. Column 4 shows suggests an increase of 8 percentage points in the likelihood of using family planning (FP) methods, significant at the 10% level. Lastly, in column 5 we report a significant increase in the likelihood of daycare usage for eligible children. RD estimates show that eligibility for SP increases a child's probability of attending daycare by 4.9 percentage points. This effect is large when considering that only 6.8% of control children assist to daycare. In all, supplementary results using the Mx-FLS show that children eligible for SP are: (i) more likely to report very good health status, (ii) less likely to report disease (flu, diarrhea), and (iii) more likely to attend daycare, suggesting that healthier children are more likely to use external care services, freeing up time for mother's to participate in the labor force. On the parents' side, SP eligibility increases time spent by fathers on dependents' care by nearly 50%, and we also find evidence that family planning methods usage increases. We interpret these results as evidence supporting that an intrahousehold mechanism explains our effects on women's labor supply, that is, SP effectively improves children's health, and parents respond accordingly: women take advantage of labor market opportunities in the informal sector, as their children now demand less care, and the time necessary for child rearing is redistributed from mothers to husbands and external services (daycare).

D. Effect Dynamics: Event Study Analysis

To study the dynamics of SMSXXI's effect on labor supply, we present graphical evidence coming from an event study analysis based on our panel fixed effects model. Figures 5 through 7 present the coefficient estimates for each year

before and after program entry, for men and women separately, including the corresponding confidence intervals at the 95% level.

The right panel on Figure 6 shows that there are no effects on men's labor participation, that is, they are precisely estimated zero. The left panel shows that effects on women's participation are not immediate: coefficients are always positive, but they only gain significance a year after program start. More so, effects are twice as large for the third year after start (5 p.p.) and reach a 10 p.p. increase on the fourth year. For informality and formal work, Figures 7 and 8 show that the effects on women only pick up three years into SMSXXI. Estimates for men vary around zero, but they too noisily estimated to be significant.

These dynamics are consistent with a delayed effect of SMSXXI on the labor supply of mothers. As newly born children require the most time-intensive care, the figures suggest that the health effects and intrahousehold responses only kick in after the children have aged enough to be under someone else's care.

VI. Discussion

Our results on labor participation for women suggest a different story than the classic model of labor supply and depart from the literature on SP that emphasizes informality as an undesirable alternative to formal labor. Our findings for the effect of eligibility for SP on the labor supply of women suggest that access to public insurance does in fact increase informal labor, but not entirely at the cost of formal jobs; we find that a larger share of women opting for informality are entering the labor force, rather than switching away from formality. These results imply that the discussion on the effects of SP on informality has failed to account for this flow of women into the labor force, which could partially offset the cost of driving resources away from the

formal sector. More so, the fact that we find no significant effect on men's labor supply suggests that the productivity and tax-base costs of providing a public health insurance program may be smaller than previously thought. In this aspect, our results differ from the paper by Del Valle (2018), whose findings point to women being kept in the informal sector rather than flowing in. We consider this to be related to the additional source of variation in our design, which allows us to control for trend differences between eligible and non-eligible mothers coming from non-treated municipalities.

Second, our results for the effect of SMSXXI on formality give little evidence of a shift from the formal to the informal sector. Notably, for some specifications, we even find evidence of a positive effect in formal employment for women, driven by less educated women (incomplete high school) and non-single women. This finding contradicts the previous literature on public health insurance and labor in Mexico and suggests that previous studies had failed to account for health improvements and the intrahousehold mechanisms we present in our paper. We attribute this to the evidence for positive health effects on children. In a related paper using the same identifying strategy, Celhay et. Al (2018) find evidence for short- and long-term improvements in child health: they find that SMSXXI decreases child mortality during the first month and year of life, and also report an increase in height z-scores for children exposed to the program during their first year of life, eight years after program start. Conti and Ginja (2017) corroborate this decrease in child mortality for poor municipalities. In light of these papers, future work on SP should carefully weigh the labor allocation costs against these health benefits for children.

With respect to the mechanisms explaining heterogeneity between men and women, we turn to the literature on household allocation of time and resources. Our

finding that women who are married and have more children drive results on participation, suggest that the increase in labor force participation comes from households with significant child-rearing obligations who would favor more flexible work arrangements and can reassign time of care to a partner. Supplementary evidence coming from the Mexican Family Life Survey confirms this mechanism: RD estimates suggest that SMSXXI eligibility improves child health and makes them more likely to attend daycare, and that eligible household redistribute time used in child-rearing to fathers.

VII. Conclusion

We study the implementation of a large public health insurance focused on children conditions in Mexico and find that access to the program affects labor supply. These changes are specific to mothers of eligible children, who increase their labor force participation by 9% to take informal jobs. Our work also points out that the relevant counterfactual for women eligible for public health insurance is not participating in the labor force rather than leaving formality. Having an informal job could be a positive outcome when compared against non-participation. Future work on comparing costs and benefits of non-participation vs. informality would be necessary to fully determine the effects of public health programs on labor market outcomes, rather than only considering labor supply effects.

Additional analyses using an RD design show that this effect is explained by improvements in child health and larger families reallocating time caring for children away from mothers and unto fathers and daycare services. This suggests that improving labor market flexibility for women may be a more appropriate strategy to increase female labor participation in developing countries.

Finally, we show that the effects of SMSXXI on women's labor supply appear around the second year of program effect, consistent with the idea that health improvements for children are not immediate and take time to affect labor outcome. This analysis underscores the importance of designing labor policies that favor mothers during their children's early years.

In general, our results indicate the need to reassess the total effect of public health insurance programs on total productivity and welfare, accounting for a potentially positive effect of SP on female labor market participation through child health and intrahousehold responses. Welfare gains from improving child health through public insurance, coupled with new, underrepresented groups gaining access to the labor market, could partially offset the productivity and tax-base costs associated with a larger informal sector. A more precise assessment of the costs and benefits of universal health care programs could provide valuable policy lessons for developing countries in the early years of operating such schemes (Thailand, Vietnam, Ecuador, Chile, Colombia, among others) or even countries in the process of reevaluating their current social security systems (Costa Rica, Nicaragua).

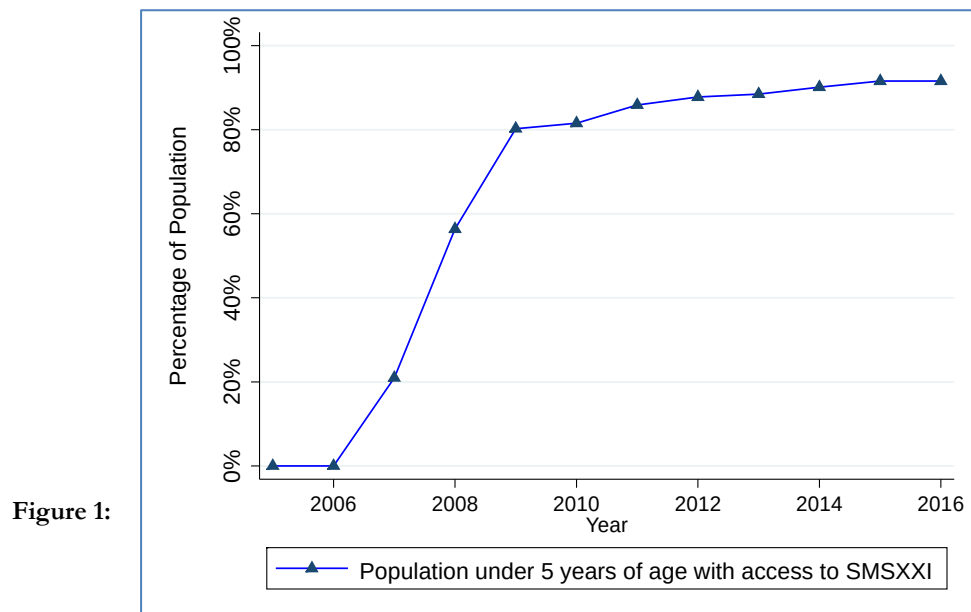
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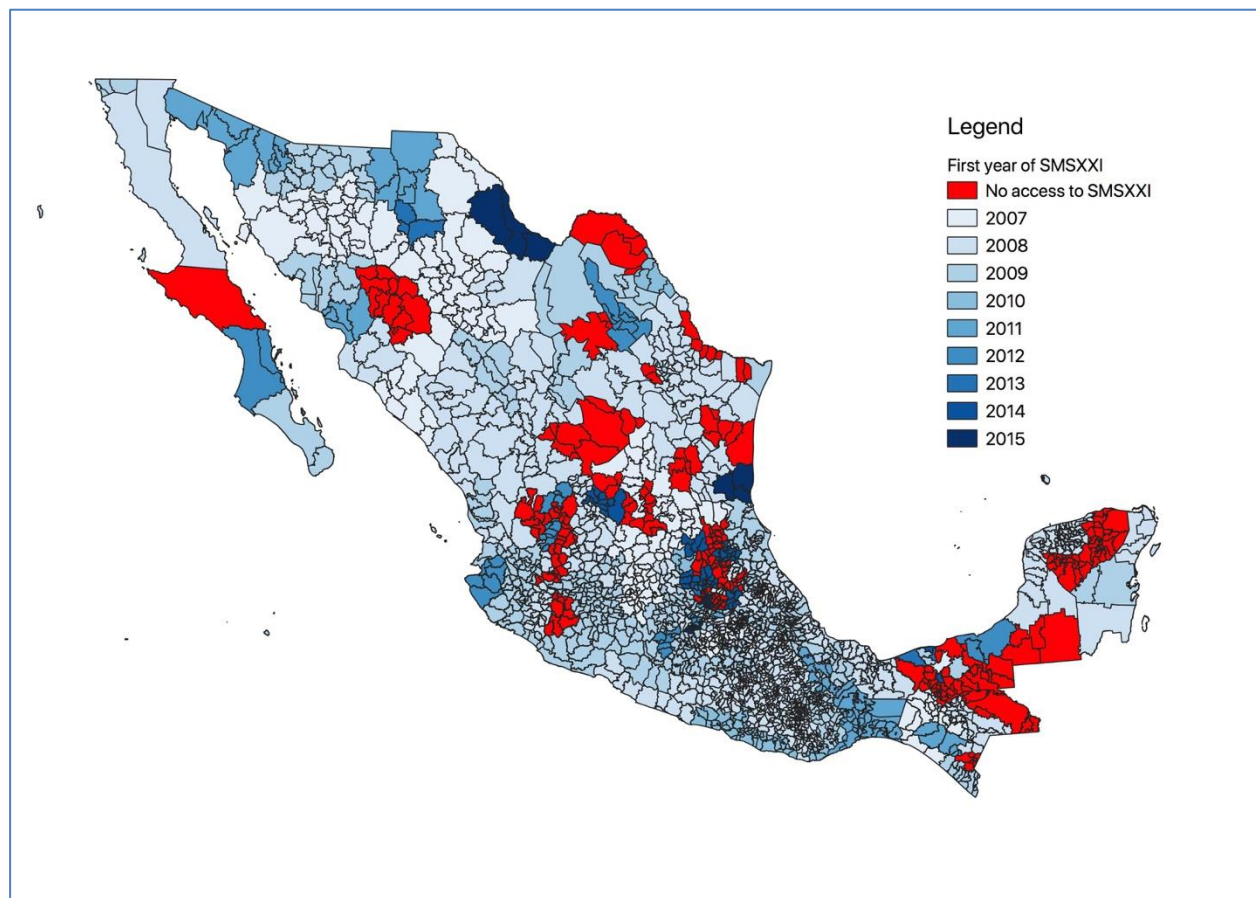
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Figures



**Percentage of child population with access to SMSXXI
in their municipality by year.**

Notes: Author's elaboration based on administrative data supplied by the CNPSS and population statistics from INEGI. Each data point represents the percentage of children under 5 years old who live in a municipality located in a health jurisdiction with access to SMSXXI, as evidenced by the reimbursement requests of a given jurisdictions' hospitals to the CNPSS by year.



**Figure 2: Map of
by year of entry to**

**municipalities in Mexico
SMSXXI**

Notes: Author's administrative data and geographical information provided by INEGI. Colors separate the municipalities according to the year when SMSXXI arrived at the corresponding health jurisdiction. Municipalities in red have yet to receive coverage for the year 2016.

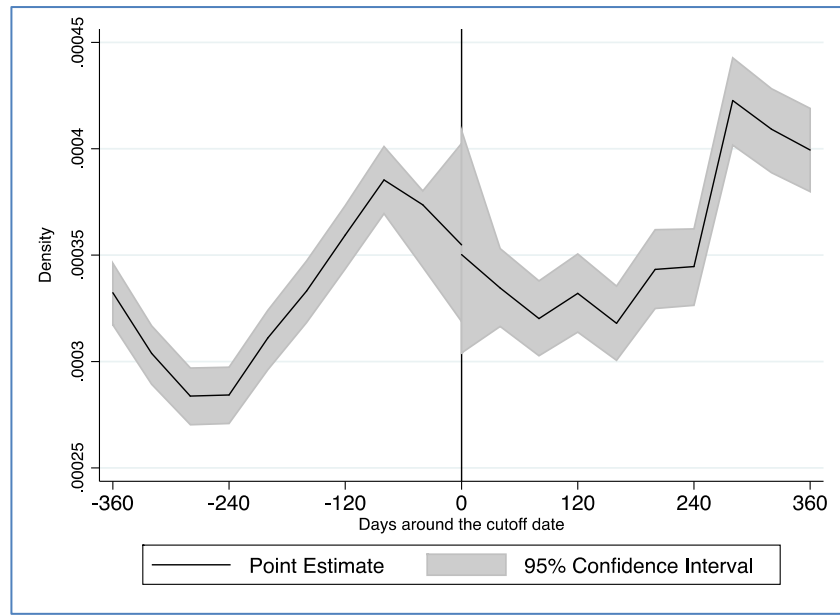
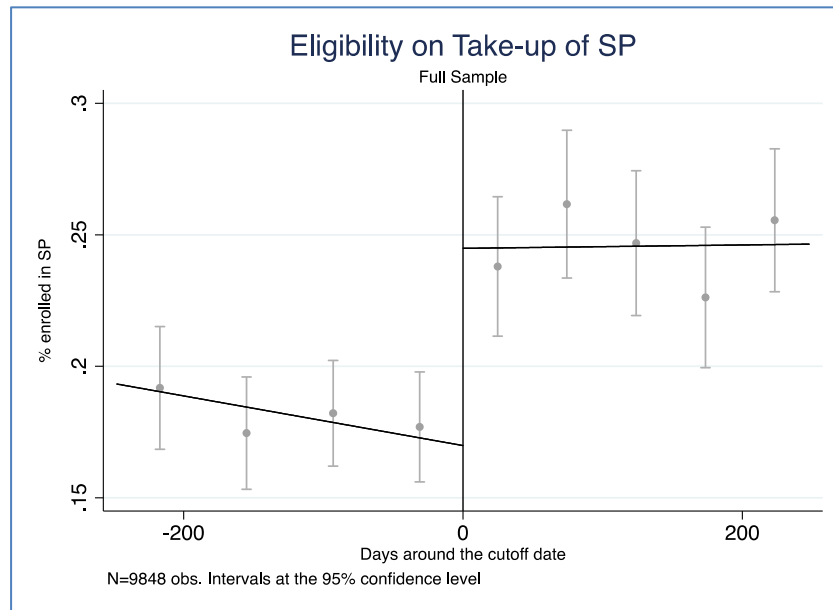


Figure 3: Density plot for the date of birth running variable

Notes: Author's elaboration based on labor data from the ENOE survey collected by INEGI. The vertical axis plots the density at each value of the running variable. Solid line plots the estimated density computed using the local polynomial density estimator by Cattaneo, Jansson & Ma (2017). The estimator is set to be a first-degree polynomial. Shaded area is the confidence interval for the estimated density. The independent variable is the date of birth of a child for the household, normalized around the cutoff date of December 1st, 2006.

Figure 4:
in take-up
Seguro
in



Change
of
Popular

households
with children eligible to the SMSXXI

Notes: Author's elaboration based on the ENOE 2009 survey for the first quarter of the year, as collected by INEGI. The dependent variable is the % of individuals enrolled in Seguro Popular. The independent variable is the child's date of birth. Point estimates represent the average for a two-month interval. Confidence intervals are at the 95% level. Solid line plots a first-degree polynomial approximation for each side of the cutoff. All averages and polynomials are estimated in Stata using the *rdplot* procedure written by Calonico, Cattaneo & Titiunik (2015).

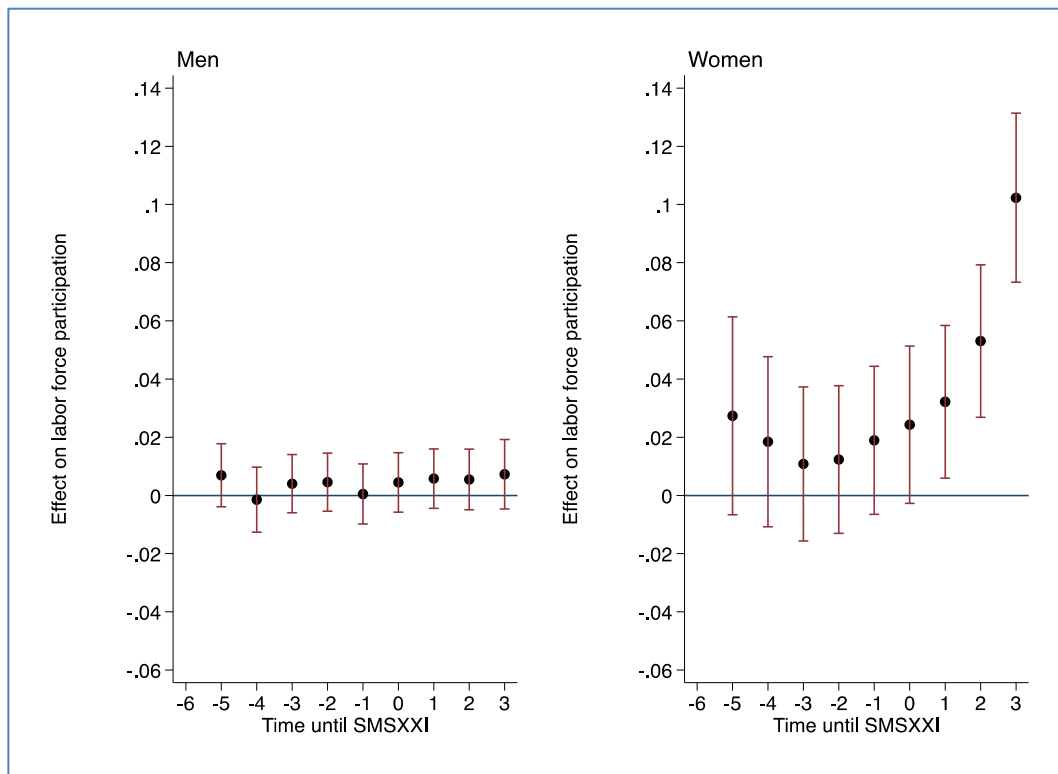


Figure 5: Event Study Analysis for labor force participation – Men and Women

Notes: Figure presents an event study for the effect of SMSXXI. The vertical axis represents the change in the percentage of individuals participating in the labor force. The independent variable is the time until/after treatment in years. Each data point is the coefficient estimator for the effect at given time. Confidence intervals are at the 95% level.

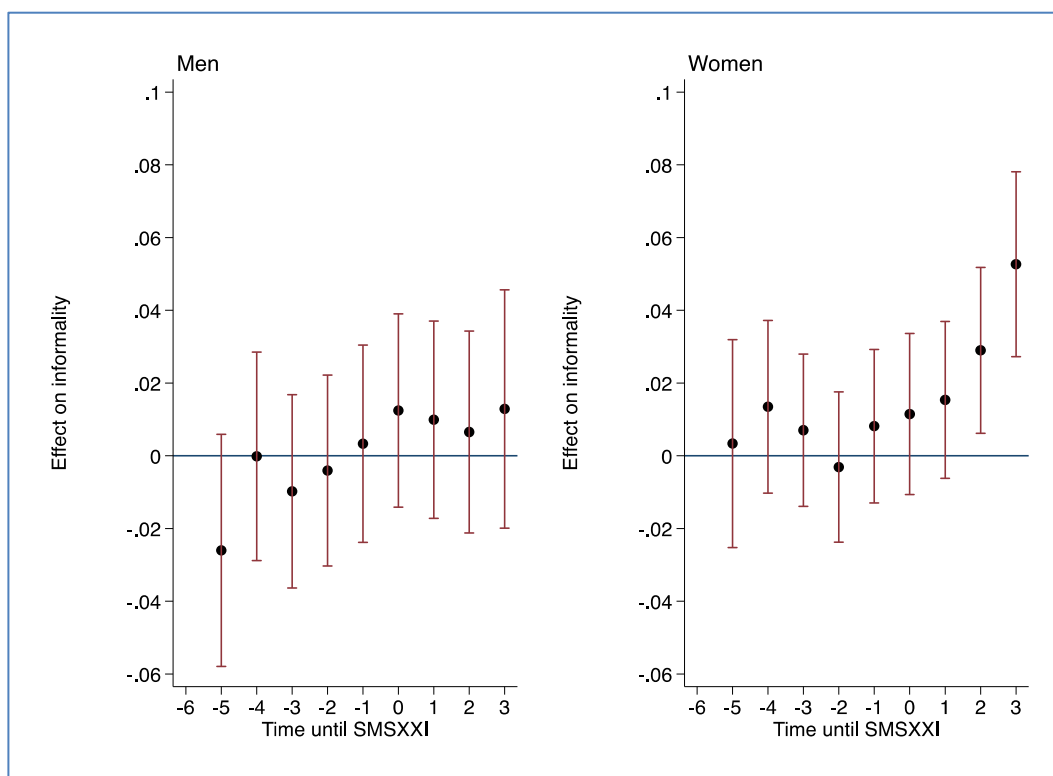


Figure 6: Event Study Analysis for Informality – Men and Women

Notes: Figure presents an event study for the effect of SMSXXI. The vertical axis represents the change in the percentage of individuals working in the informal sector. The independent variable is the time until/after treatment in years. Each data point is the coefficient estimator for the effect at given time. Confidence intervals are at the 95% level.

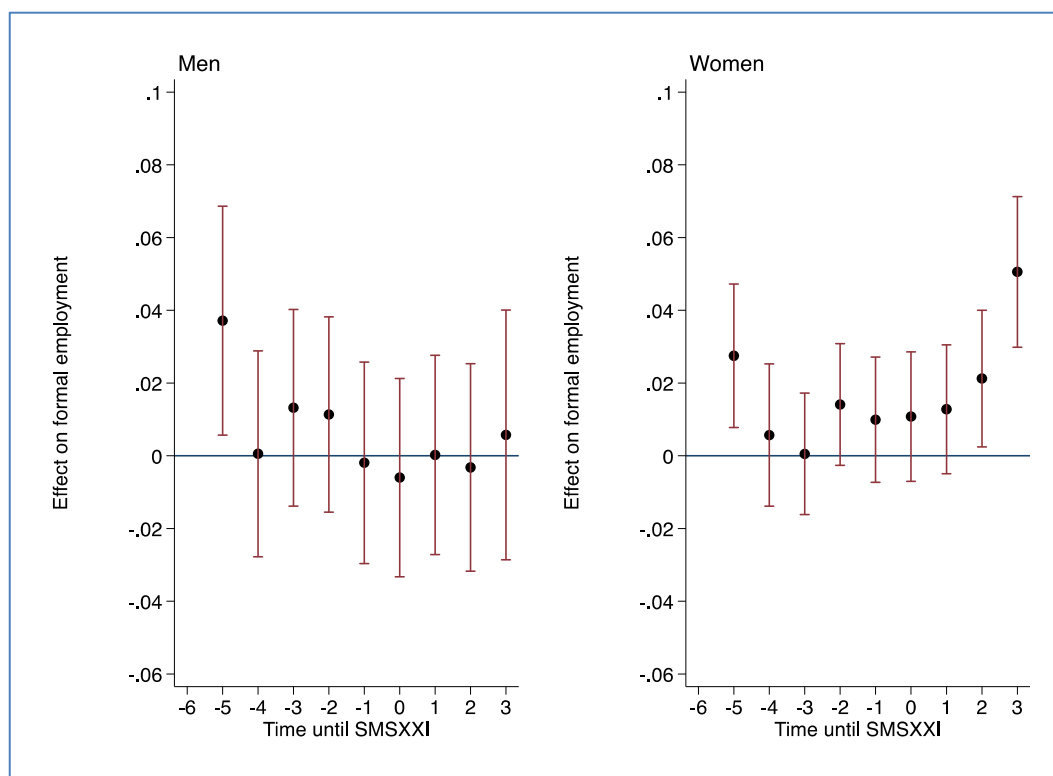


Figure 7: Event Study Analysis for formality – Men and Women

Notes: Figure presents an event study for the effect of SMSXXI. The vertical axis represents the change in the percentage of individuals working in the formal sector. The independent variable is the time until/after treatment in years. Each data point is the coefficient estimator for the effect at given time. Confidence intervals are at the 95% level.

Tables

Table 1: Descriptive Statistics for Women at baseline

Covariate	Eligible			Non-eligible		
	Mean	Std. Deviation	N	Mean	Std. Deviation	N
Age	27.393	9.539	5,010	27.828	10.027	5,698
Read & write (Yes=1)	0.963	0.189	5,010	0.959	0.197	5,698
Years of schooling	9.020	5.428	5,010	8.770	5.007	5,698
N° of Children	1.962	2.190	5,010	2.226	1.889	5,698
Married (Yes=1)	0.571	0.495	5,010	0.570	0.495	5,698
Size of Household	4.944	1.734	5,010	5.000	1.838	5,698
N° of women in HH	2.525	1.228	5,010	2.623	1.302	5,698
N° of 15 y.o. in household	2.494	1.273	5,010	2.515	1.349	5,698
Female Headed Household (Yes=1)	0.094	0.292	5,010	0.103	0.304	5,698
Lives in Distrito Federal (Yes=1)	0.021	0.145	5,010	0.021	0.142	5,698
Rural HH (Yes=1)	0.202	0.401	5,010	0.224	0.417	5,698
Attended higher education (Yes=1)	0.185	0.388	5,010	0.182	0.386	5,698

Note: This table presents descriptive statistics for women in our sample, for the year 2006 quarter 4 (right before program start). We consider all heads of household or their spouses who have a child below 2 years of age. We separate the sample between eligible and non-eligible women, according to whether their children are 1 year old or less (eligible) or between 1 and 2 years old (non-eligible).

Table 2: Descriptive Statistics for Men at baseline

Covariate	Eligible			Non-eligible		
	Mean	Std. Deviation	N	Mean	Std. Deviation	N
Age	29.323	9.554	4,617	29.838	10.295	5,102
Read & write (Yes=1)	0.974	0.160	4,617	0.969	0.174	5,102
Years of schooling	9.213	5.138	4,617	9.122	5.956	5,102
Married (Yes=1)	0.602	0.490	4,617	0.605	0.489	5,102
Size of Household	4.991	1.756	4,617	5.031	1.838	5,102
N° of women in HH	2.363	1.124	4,617	2.412	1.195	5,102
N° of 15 y.o. in household	2.517	1.308	4,617	2.505	1.353	5,102
Female Headed Household (Yes=1)	0.047	0.212	4,617	0.044	0.206	5,102
Lives in Distrito Federal (Yes=1)	0.017	0.130	4,617	0.019	0.138	5,102
Rural HH (Yes=1)	0.205	0.404	4,617	0.219	0.414	5,102
Attended higher education (Yes=1)	0.183	0.387	4,617	0.173	0.378	5,102

Note: This table presents descriptive statistics for men in our sample, for the year 2006 quarter 4 (right before program start). We consider all heads of household or their spouses who have a child below 2 years of age. We separate the sample between eligible and non-eligible men, according to whether their children are 1 year old or less (eligible) or between 1 and 2 years old (non-eligible).

Table 3: Impact of SMSXXI on Labor Supply choices of Mothers and Fathers

	(1)	(2)	(3)	(4)	(5)	(6)
	LFP	LFP	Informality	Informality	Formal	Formal
Panel A: Women						
SMSXXI	0.032*** (0.010)	0.033*** (0.008)	0.015** (0.007)	0.018*** (0.006)	0.014* (0.008)	0.009 (0.007)
Obs	187058	181944	187075	181961	187075	181961
Adjusted R ²	0.061	0.045	0.041	0.040	0.055	0.010
Panel B: Men						
SMSXXI	0.005** (0.003)	0.004* (0.002)	-0.002 (0.010)	-0.010 (0.009)	0.010 (0.010)	0.014 (0.009)
Obs	172252	166723	172265	166736	172265	166806
Adjusted R ²	0.018	0.049	0.143	0.126	0.150	0.128
Mun FE	Y	Y	Y	Y	Y	Y
YQ FE	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y
Mun/YQ FE	N	N	N	N	N	N
Mun/Time until FE	N	N	N	N	N	N
Cohort/Mun FE	N	Y	N	Y	N	Y

Note: This table presents estimates for the effect of eligibility of SP on labor supply outcomes, separating between men and women. We implement our panel fixed effects models. Coefficient of interest is ITT (intention to treat), which represents the effect of having a child eligible for SMSXXI in a municipality with access to the program through its respective health jurisdiction. In models 1, 3 and 5 we include fixed effects at the municipality, quarter and cohort level. In models 2, 4 and we include fixed effects for each cohort-municipality pair, control variables and a linear RDD control function. We cluster standard errors at the program assignment (cohort-municipality) level, as suggested by Abadie et. Al (2017). Standard errors in parentheses. * <0.1 , ** <0.05 , *** <0.01

Table 4: Heterogenous Impact of SMSXXI on Labor supply decisions for Mothers

	(1)	(2)	(3)	(4)	(5)	(6)
A. Education	Less than High School			High School completed or more		
	LFP	Informality	Formal	LFP	Informality	Formal
SMSXXI	0.041*** (0.009)	0.024*** (0.008)	0.012* (0.006)	0.002 (0.017)	0.013 (0.012)	-0.016 (0.017)
Obs	125483	125502	125502	50951	50951	50951
Adjusted R2	0.041	0.045	-0.019	0.026	0.027	0.012
B. N° of Children	Two or more Children			One Child		
	LFP	Informality	Formal	LFP	Informality	Formal
SMSXXI	0.039*** (0.011)	0.017** (0.008)	0.015 (0.010)	0.039** (0.016)	0.013 (0.012)	0.019 (0.016)
Obs	130113	130126	130126	44914	44919	44919
Adjusted R2	0.044	0.042	0.002	0.030	0.008	-0.001
C. Marriage Status	Single Mothers			Non-single Mothers		
	LFP	Informality	Formal	LFP	Informality	Formal
SMSXXI	0.018 (0.012)	0.013* (0.008)	0.001 (0.010)	0.057*** (0.015)	0.025** (0.013)	0.025* (0.015)
Obs	123886	123900	123900	50717	50723	50723
Adjusted R2	0.041	0.043	0.006	0.044	0.022	-0.002

Note: This table presents estimates for the effect of eligibility of SP on labor supply outcomes, separating women according to their level of education, number of children and marriage status. Outcomes are labor force participation (LF), informal sector work (IN) and formal sector work (FO). We implement our panel model with interacted fixed effects (Equation 2). Coefficient of interest is SMSXXI (intention to treat), which represents the effect of having a child eligible for SMSXXI in a municipality with access to the program through its respective health jurisdiction. We cluster standard errors at the program assignment (cohort-municipality) level, as suggested by Abadie et. Al (2017). Standard errors in parentheses. * <0.1 , ** <0.05 , *** <0.01

Table 5: Impact of SMSXXI on child health

Panel A: Child Health	Incidence of disease in last 4 weeks				
	Reported Health Status (Very Good=1)	Flu	Cough	Diarrhea	Breathing Difficulties
	(1)	(2)	(3)	(4)	(5)
Eligible	0.072*** (0.017)	-0.146*** (0.055)	-0.058 (0.053)	-0.048* (0.025)	-0.007 (0.019)
Mean Control	0.118	0.372	0.332	0.061	0.058
Bandwidth (CCT)	11	11	15	10	15
Covariates	Y	Y	Y	Y	Y
N	1400	1400	1747	1296	1747

Note: This table reports RDD estimates for the effect of being eligible on a child's reported health status (Very Good=1) and incidence of disease during the last 4 weeks (1=Yes). We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Standard errors in parenthesis. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). * <0.1 , ** <0.05 , *** <0.01

Table 6: Impact of SMSXXI on parents' decisions and outcomes

Outcomes	Hours Caring for Dependents				
	Mother's reported health status (Very Good = 1)			Use of FP Methods	Child attends daycare
	(1)	Mother (2)	Father (3)	(4)	(5)
Eligible	0.051 (0.035)	1.295 (3.013)	2.656** (1.116)	0.080* (0.041)	0.049** (0.024)
Mean Control	0.090	29.755	5.762	0.681	0.068
Bandwidth (CCT)	20	11	10	13	10
Covariates	Y	Y	Y	Y	Y
N	2667	864	563	935	1282

Note: This table reports RDD estimates for the effect of being eligible on mother's health status, hours spent caring for dependents, use of family planning methods (1=Yes), use of external care services for children (Yes=1). We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Standard errors in parenthesis. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). * <0.1 , ** <0.05 , *** <0.01

Appendix A: Robustness Checks for Triple Differences and Regression Discontinuity

Estimates

A. Triple Differences: Labor Supply outcomes

To estimate the effect of SP eligibility and access on labor supply outcomes, our first model has the following form:

$$y_{icjt} = \alpha + \delta Cohort_c * SMSXXI_{jt} + \gamma_1 SMSXXI_{jt} + \gamma_2 (Cohort_c * Y'07_t) + \gamma_3 (Cohort_c * MunTreat_j) + \phi_j + \phi_c + \phi_t + \phi_{t-T_j} + \varepsilon_{icjt}$$

Where i is an individual, c indexes cohorts, j indexes a municipality, t indexes time in quarter-year, and T_j marks the year when a given municipality j enters the program. In this regression, $Cohort_c$ is equal to one for birth cohorts eligible to the program, i.e. children born between December 1st of year 2006. We limit our sample to children that are eligible and ineligible for one year. In particular in year 2007, eligible cohorts are between 1 – 12 months of age and children in the ineligible group are between 13 – 24 months of age. As time goes by these cohorts become older but the analysis keeps estimating results out of comparison of these cohorts within municipalities. In addition, $SMSXXI_{jt}$ is equal to one if municipality j is in a health jurisdiction that is treated in year t . $Y'07_t$ is a dummy equal to one for year $t=2007$ and onwards, marking program creation. $MunTreat_j$ indicates if municipality j enters treatment during our analysis period (ever-treated). Finally, the coefficient of interest is δ , the effect of the program roll-out in municipality j for individuals i with eligible children. The effect on labor outcomes, y_{icjt} , is identified using variation in the municipality roll out but also within treated municipalities, by comparing eligible to ineligible cohorts.

In equation (1), we include fixed effects to control for: (i) time and cohort -invariant differences at the municipality level (ϕ_j), (ii) cohort-specific characteristics (ϕ_c), (iii) time-specific shocks (ϕ_t) common to all municipalities and cohorts, and (iv) fixed effects for time remaining until/after municipality j enters treatment ϕ_{t-T_j} , to control for time shock specific to the periods before and after implementing the program.

Equation (1) represents the simplest way to estimate a triple difference in our setting. For our labor outcomes separated by sex, in tables A1 through A6, we present this is model in column (1). As robustness checks, in the following two columns we restrict: (i) the sample to individuals aged 15-45, and (ii) allow for the inclusion of both individual and household level covariates.

To allow for an unrestricted effect of the municipality characteristics, we define the following regressions:

$$y_{icjt} = \alpha + \delta Cohort_c * SMSXXI_{jt} + \gamma_1(Cohort_c * Y'07_t) + \gamma_2(Cohort_c * Mun Treat_j) + \phi_{jt} + \phi_{j(t-T_j)} + \varepsilon_{icjt}$$

$$y_{icjt} = \alpha + \delta Cohort_c * SMSXXI_{jt} + \gamma_1(Cohort_c * Y'07_t) + \phi_{jc} + \phi_{jt} + \phi_{j(t-T_j)} + \varepsilon_{icjt}$$

In equation (2), instead of grouping together all municipalities by treated/non-treated status as in equation (1), we allow for each municipality fixed-effect to interact unrestrictedly with every year-quarter and time to treatment category. These effects allow for heterogeneity in: (i) the overall employment levels of any municipality in any year-quarter (ϕ_{jt}), and (ii) overall employment levels in any municipality for any period before or after their specific year of entering treatment ($\phi_{j(t-T_j)}$). Furthermore, in equation (3) we also allow the municipality fixed effect to vary for eligible and non-eligible individuals (ϕ_{jc}), controlling for different base levels of employment for both cohorts in

each municipality. Including these interacted fixed effects allows us to remove the corresponding interacted dummies in equation (1). In both models the coefficient of interest is δ , which identifies the effect on SMSXXI on eligible individuals with access to the program in their location. In tables A1 to A6, we present results for equation (2) on column (4), and for equation (3) on column (5). As robustness checks on equation (3), we allow for the inclusion of covariates in column (6) and consider only individuals aged 15-45 y.o. in column (7).

Additionally, to check for the assumption of no differences in trends before the start of the program, in column (8) we implement a falsification exercise where we manually set the onset of SMSXXI 1 year before the actual start for each municipality and estimate the model for the pre-treatment years (2005-2006).

Tables A7-A15 present similar robustness exercises for our heterogeneity estimates. In these tables, columns (1-3) and (6-8) present estimates for the model specified in equation (1). Columns (4) and (9) present the model from equation (2), and columns (5) and (10) the model from equation (3). Finally, in all models (A1-A15), we cluster standard errors at the municipality-cohort level, which fully determines whether an individual is eligible or not for access to SMSXXI. This follows the recommendation by Abadie et. Al (2017) to allow for correlation within units at the treatment assignment level.

B. Regression Discontinuity Design: Time allocation, child health, family planning and daycare use

For mechanisms, we implement a Regression Discontinuity Design (RDD). Our main specification for RD analysis takes the following form (equation 4):

$$y_{iz} = \alpha + f(Z_i) + \tau * \mathbf{1}(Z_i \geq 0) + X_i + \varepsilon_i \quad (4)$$

where y_{iz} represents an outcome for individual i . X_i is a vector of control variables specific to the individual, such as age, sex and educational attainment, $f(\cdot)$ is a smooth function of the running variable, known as the control function in the RD literature. The variable $\mathbf{1}(Z \geq 0)$ is an indicator function that takes the value 1 when the running variable is equal to or higher than the cutoff, that is, when the child of the individual was born on or after the cutoff date. Lastly, ε_{iz} represents the unobserved heterogeneity at the individual level. The coefficient of interest is τ , which can be interpreted as the local average effect of having an eligible child. Since equation (4) is a reduced form estimation, τ is representative of the Intention to Treat (ITT) estimate, that is, the effect of having access to the program near the cutoff point.

One of the key assumptions of an RD design is that we have correctly specified the control function, $f(Z_i)$. We employ local polynomial regression estimators as recommended by the literature on regression discontinuity (see Lee & Lemieux (2008), Imbens & Lemieux (2008), Cattaneo, Idrobo & Titiunik (2017), among others). We estimate the model described in equation (3) and compute the estimates using the procedure suggested by Calonico, Cattaneo & Titiunik (hereon called CCT, 2014a)¹⁴. Since our running variable is discrete, we cluster standard errors at the running variable level, as suggested by Lee and Card (2008).

In the robustness tables (A16-A24), we specify the following robustness checks: for each outcome presented in all tables, column (1) reports an RD specification with a first-degree local polynomial as the control function, column (2) estimates the impact with a bandwidth equal to 75%

¹⁴ We implement the procedure in Stata using the accompanying software package *rdrobust*, written by Calonico, Cattaneo & Titiunik (2014b).

of the one chosen by CCT procedure in column 81), column (3) performs the same exercise with a bandwidth that is 12% of column (1), column (4) allows for a second-degree control function, and column (5) is equal to column (1) but also includes control variables in the estimation. All estimates allow the slope of the control function to vary on each side of the cutoff. In all robustness estimations, we allow for correlation at the running variable level, following the recommendation by Card and Lee (2008) for discrete variables settings. For all estimates we report the mean at the control side of the cutoff, the bandwidth used and the polynomial order of the control function.

Tables

Table A1: Impact of SMSXXI on Women's Labor Force Participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	LFP	LFP	LFP	LFP	LFP	LFP	LFP	Falsification
SMSXXI	0.032*** (0.010)	0.032*** (0.009)	0.029*** (0.010)	0.034*** (0.008)	0.033*** (0.008)	0.032*** (0.007)	0.031*** (0.008)	-0.009 (0.012)
Obs	187058	187056	185400	181995	181944	181942	180215	70126
Adjusted R2	0.061	0.145	0.061	0.047	0.045	0.134	0.045	0.053
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	Y	N	N
Age 15-45	N	N	Y	N	N	N	Y	N
Mun/Time FE	N	N	N	Y	Y	Y	Y	N
Mun/YQ FE	N	N	N	Y	Y	Y	Y	N
Cohort/Mun FE	N	N	N	N	Y	Y	Y	N

Notes: This table presents robustness estimates for the impact of SMSXXI on labor force participation for women. In columns (1), (2) and (3) we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In column (4), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In column (5) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A2: Impact of SMSXXI on Women's Informal Labor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Informality	Informality	Informality	Informality	Informality	Informality	Informality	Falsification
SMSXXI	0.015** (0.007)	0.016** (0.007)	0.014** (0.007)	0.017*** (0.006)	0.018*** (0.006)	0.018*** (0.006)	0.018*** (0.006)	-0.001 (0.009)
Obs	187075	187056	185400	182012	181961	181942	180215	70131
Adjusted R2	0.041	0.053	0.041	0.040	0.040	0.052	0.040	0.043
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y
Year-Q FE	Y	Y	Y	Y	Y	Y	Y	Y
Time until Treat FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	N	N	Y	N	N
Age 15-45	N	N	Y	N	N	N	Y	N
Mun - Time FE	N	N	N	Y	Y	Y	Y	N
Mun - YQ FE	N	N	N	Y	Y	Y	Y	N
Cohort Mun. FE	N	N	N	N	Y	Y	Y	N

Notes: This table presents robustness estimates for the impact of SMSXXI on informal work for women. In columns (1), (2) and (3) we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In column (4), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In column (5) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A3: Impact of SMSXXI on Women's Formal Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Falsification
SMSXXI	0.014*	0.015**	0.013	0.014*	0.009	0.009	0.008	-0.007
	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)	(0.006)	(0.007)	(0.009)
Obs	187075	187056	185400	182012	181961	181942	180215	70131
Adjusted R2	0.055	0.203	0.056	0.014	0.010	0.165	0.009	0.052
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y
Year-Q FE	Y	Y	Y	Y	Y	Y	Y	Y
Time until Treat FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	N	N	Y	N	N
Age 15-45	N	N	Y	N	N	N	Y	N
Mun - Time FE	N	N	N	Y	Y	Y	Y	N
Mun - YQ FE	N	N	N	Y	Y	Y	Y	N
Cohort Mun. FE	N	N	N	N	Y	Y	Y	N

Notes: This table presents robustness estimates for the impact of SMSXXI on formal work for women. In columns (1), (2) and (3) we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In column (4), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In column (5) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A4: Impact of SMSXXI on Men's Labor Force Participation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(6)
	LFP	LFP	LFP	LFP	LFP	LFP	LFP	Falsification
SMSXXI	0.005** (0.003)	0.005* (0.003)	0.003 (0.002)	0.005** (0.002)	0.004* (0.002)	0.003 (0.002)	0.001 (0.002)	-0.002 (0.003)
Obs	172252	172249	162783	166793	166723	166719	156988	64729
Adjusted R2	0.018	0.024	0.020	0.051	0.049	0.055	0.055	0.020
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	Y	N	N
Age 15-45	N	N	Y	N	N	N	Y	N
Mun/Time FE	N	N	N	Y	Y	Y	Y	N
Mun/YQ FE	N	N	N	Y	Y	Y	Y	N
Cohort/Mun FE	N	N	N	N	Y	Y	Y	N

Notes: This table presents robustness estimates for the impact of SMSXXI on labor force participation for men. In columns (1), (2) and (3) we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In column (4), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In column (5) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A5: Impact of SMSXXI on Men's Informal Labor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Informality	Informality	Informality	Informality	Informality	Informality	Informality	Falsification
SMSXXI	-0.002 (0.010)	0.003 (0.010)	0.002 (0.010)	-0.004 (0.009)	-0.010 (0.009)	-0.004 (0.009)	-0.006 (0.009)	0.007 (0.012)
Obs	172265	172249	162783	166806	166736	166719	156988	64735
Adjusted R2	0.143	0.210	0.143	0.128	0.126	0.194	0.126	0.157
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	Y	N	N
Age 15-45	N	N	Y	N	N	N	Y	N
Mun/Time FE	N	N	N	Y	Y	Y	Y	N
Mun/YQ FE	N	N	N	Y	Y	Y	Y	N
Cohort/Mun FE	N	N	N	N	Y	Y	Y	N

Notes: This table presents robustness estimates for the impact of SMSXXI on informal work for men. In columns (1), (2) and (3) we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In column (4), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In column (5) we present the main estimates of the paper, adding municipality-cohort fixed

effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A6: Impact of SMSXXI on Men's Formal Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Falsification
SMSXXI	0.010 (0.010)	0.005 (0.010)	0.004 (0.010)	0.011 (0.009)	0.014 (0.009)	0.007 (0.009)	0.007 (0.009)	-0.007 (0.012)
Obs	172265	172249	162783	166806	166736	166719	156988	64735
Adjusted R2	0.150	0.223	0.151	0.128	0.127	0.201	0.127	0.162
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	Y	N	N
Age 15-45	N	N	Y	N	N	N	Y	N
Mun/Time FE	N	N	N	Y	Y	Y	Y	N
Mun/YQ FE	N	N	N	Y	Y	Y	Y	N
Cohort/Mun FE	N	N	N	N	Y	Y	Y	N

Notes: This table presents robustness estimates for the impact of SMSXXI on formal work for men. In columns (1), (2) and (3) we implement our

triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In column (4), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In column (5) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A7: Heterogeneous Impact of SMSXXI on Women's LFP by level of education

	Less than High School					High School or more completed				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	LFP	LFP	LFP	LFP	LFP	LFP	LFP	LFP	LFP	LFP
SMSXXI	0.032*** (0.010)	0.033*** (0.010)	0.028*** (0.010)	0.055*** (0.008)	0.041*** (0.009)	0.020 (0.019)	0.025 (0.018)	0.021 (0.020)	0.025 (0.016)	0.002 (0.017)
Obs	131009	131007	129667	125542	125483	56011	56011	55692	51047	50951
Adjusted R2	0.059	0.099	0.059	0.044	0.041	0.053	0.118	0.053	0.029	0.026
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	N	Y	N	N	N
Age 15-45	N	N	Y	N	N	N	N	Y	N	N
Mun/Time FE	N	N	N	Y	Y	N	N	N	Y	Y

Mun/YQ FE	N	N	N	Y	Y	N	N	N	Y	Y
Cohort/Mun FE	N	N	N	N	Y	N	N	N	N	Y

Notes: This table presents robustness estimates for the impact of SMSXXI on labor force participation for women, separating them by their educational attainment. In columns (1-3) and (6-8), we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In columns (4) and (8), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In columns (5) and (10) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A8: Heterogeneous Impact of SMSXXI on Women's Informality by level of education

	Less than High School					High School or more completed				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Informal	Informal	Informal	Informal	Informal	Informal	Informal	Informal	Informal	Informal
SMSXXI	0.017** (0.008)	0.018** (0.008)	0.015* (0.008)	0.034*** (0.008)	0.024*** (0.008)	0.009 (0.013)	0.003 (0.013)	0.009 (0.013)	0.018 (0.011)	0.013 (0.012)
Obs	131026	131007	129667	125561	125502	56011	56011	55692	51047	50951
Adjusted R2	0.052	0.068	0.052	0.046	0.045	0.044	0.049	0.044	0.029	0.027
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Covariates	N	Y	N	N	N	N	Y	N	N	N
Age 15-45	N	N	Y	N	N	N	N	Y	N	N
Mun/Time FE	N	N	N	Y	Y	N	N	N	Y	Y
Mun/YQ FE	N	N	N	Y	Y	N	N	N	Y	Y
Cohort/Mun FE	N	N	N	N	Y	N	N	N	N	Y

Notes: This table presents robustness estimates for the impact of SMSXXI on informal work for women, separating them by their educational attainment. In columns (1-3) and (6-8), we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In columns (4) and (8), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In columns (5) and (10) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A9: Heterogeneous Impact of SMSXXI on Women's formal employment by level of education

	Less than High School					High School or more completed				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Formal
SMSXXI	0.013** (0.006)	0.013** (0.006)	0.011* (0.006)	0.017*** (0.005)	0.012* (0.006)	0.008 (0.020)	0.021 (0.018)	0.009 (0.020)	0.005 (0.017)	-0.016 (0.017)
Obs	131026	131007	129667	125561	125502	56011	56011	55692	51047	50951
Adjusted R2	0.044	0.081	0.044	-0.010	-0.019	0.048	0.141	0.048	0.014	0.012
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	N	Y	N	N	N
Age 15-45	N	N	Y	N	N	N	N	Y	N	N
Mun/Time FE	N	N	N	Y	Y	N	N	N	Y	Y
Mun/YQ FE	N	N	N	Y	Y	N	N	N	Y	Y
Cohort/Mun FE	N	N	N	N	Y	N	N	N	N	Y

Notes: This table presents robustness estimates for the impact of SMSXXI on formal work for women, separating them by their educational attainment. In columns (1-3) and (6-8), we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In columns (4) and (8), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In columns (5) and (10) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A10: Heterogeneous Impact of SMSXXI on Women's LFP by N° of children

	Two or More Children						One Child			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	LFP	LFP	LFP	LFP	LFP	LFP	LFP	LFP	LFP	LFP
SMSXXI	0.028** (0.011)	0.027*** (0.010)	0.025** (0.011)	0.055*** (0.009)	0.039*** (0.011)	0.042** (0.018)	0.034** (0.015)	0.042** (0.018)	0.054*** (0.014)	0.039** (0.016)
Obs	135993	135993	134534	130187	130113	51023	51021	50824	45052	44914
Adjusted R2	0.063	0.135	0.063	0.046	0.044	0.078	0.197	0.077	0.041	0.030
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	N	Y	N	N	N
Age 15-45	N	N	Y	N	N	N	N	Y	N	N
Mun/Time FE	N	N	N	Y	Y	N	N	N	Y	Y
Mun/YQ FE	N	N	N	Y	Y	N	N	N	Y	Y
Cohort/Mun FE	N	N	N	N	Y	N	N	N	N	Y

Notes: This table presents robustness estimates for the impact of SMSXXI on labor force participation for women, separating them by their number of children. In columns (1-3) and (6-8), we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In columns (4) and (8), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In columns (5) and (10) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A11: Heterogeneous Impact of SMSXXI on Women's Informality by N° of children

	Two or More Children						One Child			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Informal	Informal	Informal	Informal	Informal	Informal	Informal	Informal	Informal	Informal
SMSXXI	0.013 (0.009)	0.016* (0.009)	0.012 (0.009)	0.029*** (0.008)	0.017** (0.008)	0.017 (0.012)	0.014 (0.012)	0.018 (0.012)	0.032*** (0.011)	0.013 (0.012)
Obs	136006	135993	134534	130200	130126	51027	51021	50824	45057	44919
Adjusted R2	0.046	0.059	0.046	0.042	0.042	0.046	0.048	0.046	0.018	0.008
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	N	Y	N	N	N
Age 15-45	N	N	Y	N	N	N	N	Y	N	N
Mun/Time FE	N	N	N	Y	Y	N	N	N	Y	Y
Mun/YQ FE	N	N	N	Y	Y	N	N	N	Y	Y
Cohort/Mun FE	N	N	N	N	Y	N	N	N	N	Y

Notes: This table presents robustness estimates for the impact of SMSXXI on informal work for women, separating them by their number of children. In columns (1-3) and (6-8), we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In columns (4) and (8), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In columns (5) and (10) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A12: Heterogeneous Impact of SMSXXI on Women's Formal employment by N° of children

	Two or More Children						One Child			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Formal
SMSXXI	0.013 (0.010)	0.010 (0.008)	0.012 (0.010)	0.022** (0.008)	0.015 (0.010)	0.022 (0.016)	0.019 (0.014)	0.021 (0.016)	0.018 (0.014)	0.019 (0.016)
Obs	136006	135993	134534	130200	130126	51027	51021	50824	45057	44919
Adjusted R2	0.057	0.191	0.058	0.007	0.002	0.065	0.232	0.065	0.012	-0.001
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	N	Y	N	N	N
Age 15-45	N	N	Y	N	N	N	N	Y	N	N
Mun/Time FE	N	N	N	Y	Y	N	N	N	Y	Y
Mun/YQ FE	N	N	N	Y	Y	N	N	N	Y	Y
Cohort/Mun FE	N	N	N	N	Y	N	N	N	N	Y

Notes: This table presents robustness estimates for the impact of SMSXXI on formal work for women, separating them by their number of children. In columns (1-3) and (6-8), we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In columns (4) and (8), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In columns (5) and (10) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A13: Heterogeneous Impact of SMSXXI on Women's LFP by Marriage Status

	Single Mothers					Non-Single Mothers				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	LFP	LFP	LFP	LFP	LFP	LFP	LFP	LFP	LFP	LFP
SMSXXI	0.021*	0.024**	0.020	0.038***	0.018	0.054***	0.049***	0.051***	0.071***	0.057***
	(0.012)	(0.011)	(0.012)	(0.010)	(0.012)	(0.014)	(0.013)	(0.014)	(0.013)	(0.015)
Obs	130043	130041	128973	123971	123886	56976	56976	56387	50847	50717
Adjusted R2	0.064	0.137	0.063	0.044	0.041	0.084	0.208	0.084	0.050	0.044
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	N	Y	N	N	N
Age 15-45	N	N	Y	N	N	N	N	Y	N	N
Mun/Time FE	N	N	N	Y	Y	N	N	N	Y	Y
Mun/YQ FE	N	N	N	Y	Y	N	N	N	Y	Y
Cohort/Mun FE	N	N	N	N	Y	N	N	N	N	Y

Notes: This table presents robustness estimates for the impact of SMSXXI on labor force participation for women, separating them by single or non-single (marriage status). In columns (1-3) and (6-8), we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In columns (4) and (8), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In columns (5) and (10) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A14: Heterogeneous Impact of SMSXXI on Women's Informality by Marriage Status

	Single Mothers					Non-Single Mothers				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Informal	Informal	Informal	Informal	Informal	Informal	Informal	Informal	Informal	Informal
SMSXXI	0.014 (0.008)	0.015* (0.008)	0.012 (0.009)	0.029*** (0.008)	0.013* (0.008)	0.021* (0.013)	0.018 (0.013)	0.022* (0.013)	0.030*** (0.012)	0.025** (0.013)
Obs	130057	130041	128973	123985	123900	56979	56976	56387	50853	50723
Adjusted R2	0.047	0.051	0.047	0.044	0.043	0.054	0.085	0.054	0.029	0.022
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	N	Y	N	N	N
Age 15-45	N	N	Y	N	N	N	N	Y	N	N
Mun/Time FE	N	N	N	Y	Y	N	N	N	Y	Y
Mun/YQ FE	N	N	N	Y	Y	N	N	N	Y	Y
Cohort/Mun FE	N	N	N	N	Y	N	N	N	N	Y

Notes: This table presents robustness estimates for the impact of SMSXXI on informal work for women, separating them by single or non-single (marriage status). In columns (1-3) and (6-8), we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In columns (4) and (8), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In columns (5) and (10) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A15: Heterogeneous Impact of SMSXXI on Women's Formal Employment by Marriage Status

	Single Mothers					Non-Single Mothers				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Formal	Formal
SMSXXI	0.007 (0.010)	0.008 (0.009)	0.007 (0.010)	0.005 (0.009)	0.001 (0.010)	0.028** (0.013)	0.027** (0.011)	0.024* (0.013)	0.035*** (0.012)	0.025* (0.015)
Obs	130057	130041	128973	123985	123900	56979	56976	56387	50853	50723
Adjusted R2	0.061	0.206	0.061	0.012	0.006	0.068	0.217	0.068	0.007	-0.002
Mun. FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year - Q FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Cohort FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Time until FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	N	Y	N	N	N	N	Y	N	N	N
Age 15-45	N	N	Y	N	N	N	N	Y	N	N
Mun/Time FE	N	N	N	Y	Y	N	N	N	Y	Y
Mun/YQ FE	N	N	N	Y	Y	N	N	N	Y	Y
Cohort/Mun FE	N	N	N	N	Y	N	N	N	N	Y

Notes: This table presents robustness estimates for the impact of SMSXXI on formal work for women, separating them by single or non-single (marriage status). In columns (1-3) and (6-8), we implement our triple difference model with fixed effects for municipalities, year-quarter, cohort and time until entering treatment, as specified in Equation (1) in Appendix A. In columns (4) and (8), we implement our triple difference interacting the year-quarter and time until fixed effects with each municipality category, as specified in Equation (2) of the Appendix. In columns (5) and (10) we present the main estimates of the paper, adding municipality-cohort fixed effects to the previous model, as specified in Equation (3). For all models, we cluster standard errors at the municipality-cohort level, following the indication by Abadie et. al (2017) to allow for correlation at the program assignment level when estimating treatment effects. Standard errors are presented in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A16: Impact of SMSXXI on time spent by parents caring for dependents

Model	P1	75%BW	125% BW	P2	Main
	(1)	(2)	(3)	(4)	(5)
Panel 1: Women					
Eligible	1.632 (3.089)	0.846 (3.110)	1.420 (2.840)	2.041 (3.347)	1.295 (3.013)
Mean Control	29.755	30.628	30.532	29.394	29.755
Bandwidth (CCT)	11	8	14	17	11
Polynomial Order	1	1	1	2	1
N	868	685	1115	1446	864
Panel 2: Men					
Eligible	2.656** (1.334)	3.104* (1.705)	3.075** (1.233)	2.977** (1.400)	2.656** (1.116)
Mean Control	5.762	5.279	5.575	5.458	5.762
Bandwidth (CCT)	10	8	13	18	10
Polynomial Order	1	1	1	2	1
N	569	400	680	955	563

Note: This table reports RDD estimates for the effect of being eligible for SMSXXI on hours spent caring for dependents. We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). Standard errors in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A17: Impact of SMSXXI on Mother's self-reported health status (1=Very good)

Model	P1	75%BW	125% BW	P2	Main
	(1)	(2)	(3)	(4)	(5)
Eligible	0.051 (0.034)	0.080* (0.041)	0.062* (0.034)	0.072* (0.043)	0.051 (0.035)
Mean Control	0.088	0.086	0.082	0.088	0.090
Bandwidth (CCT)	21	16	26	18	20
Polynomial Order	1	1	1	2	1
N	2849	2102	3813	2403	2667

Note: This table reports RDD estimates for the effect of being eligible for SMSXXI on mothers' self-reported health status being declared as very good (=1). We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). Standard errors in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A18: Impact of SMSXXI on use of family planning methods

Model	P1	75%BW	125% BW	P2	Main
	(1)	(2)	(3)	(4)	(5)
Eligible	0.057 (0.046)	0.044 (0.050)	0.068 (0.049)	0.059 (0.050)	0.080* (0.041)
Mean Control	0.676	0.678	0.668	0.666	0.681
Bandwidth (CCT)	14	11	18	23	13
Polynomial Order	1	1	1	2	1
N	1011	743	1234	1617	935

Note: This table reports RDD estimates for the effect of being eligible for SMSXXI on use of family planning methods (=1). We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). Standard errors in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A19: Impact of SMSXXI on child's reported health status

Model	P1	75%BW	125% BW	P2	Main
	(1)	(2)	(3)	(4)	(5)
Eligible	0.065*** (0.018)	0.079*** (0.021)	0.079*** (0.016)	0.088*** (0.017)	0.072*** (0.017)
Mean Control	0.118	0.117	0.129	0.118	0.118
Bandwidth (CCT)	12	9	15	12	11
Polynomial Order	1	1	1	2	1
N	1400	1012	1748	1400	1400

Note: This table reports RDD estimates for the effect of being eligible for SMSXXI on a child's health status being reported as very good (=1). We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). Standard errors in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A20: Impact of SMSXXI on child's incidence of flu in last 4 weeks

Model	P1	75%BW	125% BW	P2	Main
	(1)	(2)	(3)	(4)	(5)
Eligible	-0.146*** (0.055)	-0.014 (0.049)	-0.133* (0.075)	-0.138* (0.076)	-0.146*** (0.055)
Mean Control	0.372	0.381	0.375	0.370	0.372
Bandwidth (CCT)	11	8	14	17	11
Polynomial Order	1	1	1	2	1
N	1400	1012	1628	1991	1400

Note: This table reports RDD estimates for the effect of being eligible on a child 's incidence of flu in the last 4 weeks (Yes=1). We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Polynomial order displays the degree of the control function used for estimates. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). Standard errors in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A21: Impact of SMSXXI on child's incidence of cough in last 4 weeks

Model	P1	75%BW	125% BW	P2	Main
	(1)	(2)	(3)	(4)	(5)
Eligible	-0.060 (0.054)	0.034 (0.056)	-0.054 (0.057)	-0.060 (0.061)	-0.058 (0.053)
Mean Control	0.332	0.321	0.331	0.331	0.332
Bandwidth (CCT)	15	11	18	18	15
Polynomial Order	1	1	1	2	1
N	1747	1400	2259	2259	1747

Note: This table reports RDD estimates for the effect of being eligible on a child 's incidence of cough problems in the last 4 weeks (Yes=1). We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Polynomial order displays the degree of the control function used for estimates. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). Standard errors in parenthesis. *<0.1, **<0.05, ***<0.01

Table A22: Impact of SMSXXI on child's incidence of breathing difficulties in last 4 weeks

Model	P1	75%BW	125% BW	P2	Main
	(1)	(2)	(3)	(4)	(5)
Eligible	-0.010 (0.020)	0.024 (0.022)	-0.002 (0.020)	-0.000 (0.024)	-0.007 (0.019)
Mean Control	0.060	0.057	0.062	0.060	0.058
Bandwidth (CCT)	15	11	19	18	15
Polynomial Order	1	1	1	2	1
N	1864	1400	2375	2130	1747

Note: This table reports RDD estimates for the effect of being eligible on a child 's incidence of breathing difficulties in the last 4 weeks (Yes=1). We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Polynomial order displays the degree of the control function used for estimates. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). Standard errors in parenthesis. *<0.1, **<0.05, ***<0.01

Table A23: Impact of SMSXXI on child's incidence of diarrhea in last 4 weeks

Model	P1	75%BW	125% BW	P2	Main
	(1)	(2)	(3)	(4)	(5)
Eligible	-0.048*	-0.037	-0.051*	-0.050*	-0.048*
	(0.024)	(0.029)	(0.027)	(0.029)	(0.025)
Mean Control	0.061	0.061	0.060	0.052	0.061
Bandwidth (CCT)	10	8	13	17	10
Polynomial Order	1	1	1	2	1
N	1296	897	1522	2130	1296

Note: This table reports RDD estimates for the effect of being eligible on a child 's incidence of diarrhea in the last 4 weeks (Yes=1). We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Polynomial order displays the degree of the control function used for estimates. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). Standard errors in parenthesis. * <0.1 , ** <0.05 , *** <0.01

Table A24: Impact of SMSXXI on child's daycare use

Model	P1	75%BW	125% BW	P2	Main
	(1)	(2)	(3)	(4)	(5)
Eligible	0.039* (0.024)	0.001 (0.027)	0.034* (0.019)	0.035* (0.021)	0.049** (0.024)
Mean Control	0.068	0.068	0.075	0.074	0.068
Bandwidth (CCT)	11	8	13	13	10
Polynomial Order	1	1	1	2	1
N	1282	889	1612	1506	1282

Note: This table reports RDD estimates for the effect of being eligible on a whether a child attends daycare (Yes=1). We choose the bandwidth using the bandwidth selection procedure described in Calonico, Cattaneo & Titiunik (2014). We also report the average of the control group calculated between the bandwidth and the cutoff. Bandwidth is reported in days above (below) the cutoff date. Polynomial order displays the degree of the control function used for estimates. Errors are clustered at the running variable level, as suggested by Lee and Card (2008). Standard errors in parenthesis. * <0.1 , ** <0.05 , *** <0.01